argues that the apology helped publicly to justify, and therefore to underpin and sustain, the Canadian state’s commitment to Aboriginal self-government.

This reviewer, although a supporter of political apology, has some doubts about the book’s relatively straightforward linkage between apologies and group-conscious responses to injustice. Concerning Canada, for example, a skeptic could argue that whatever positive state action on Aboriginal claims there has been reflects the combined impact of disruptive collective action and particular court rulings favoring Aboriginal plaintiffs. Nobles is careful to stress the modest role of apology as one factor among many. Still, one wonders whether more might have been done here to disentangle the impact of apology from that of other factors.

Nobles provides evidence indicating broad Canadian support for the 1998 apology and an apparent ensuing societal willingness to bear some of the costs of historical reckoning. However, subsequent events not only suggest such support may be fleeting but, more importantly for Nobles’ thesis, that, with the growing normalization of political apology, the incidence of manipulative or cynical apologies may increase. For example, in June 2008 (after The Politics of Official Apologies was written), Canada’s newly elected Conservative government issued a new residential schools apology to Aboriginal peoples, one far more fulsome than the 1998 instance discussed by Nobles. Yet the same government had also just scrapped the $5 billion CAN Kelowna Accord, a “Marshall plan” agreement for Aboriginal social policy reached three years earlier by the Liberals. The point is not to fault Nobles for lacking clairvoyance. It is rather that the Conservative apology may reflect a phenomenon noted by social memory scholar Jeffrey K. Olick (States of Memory [Durham NC: Duke University Press, 2003]): the skilful use of ritually correct apologetic performances by right-wing regimes that have learned the limitations of nakedly reactionary styles and rhetoric. As it stands, the membership theory of apologies seems insufficiently attuned to this possibility.

It would also seem important to distinguish between the robustness of various instances of apology. If one counts equally all admissions of past wrongdoing as “apologies” — as this book seems to do — then what is there to say when a group dissatisfied by some minimalist gesture of pseudo-contrition gets portrayed as insatiably radical? More theoretically speaking, the membership theory would surely be strengthened if it could show a positive relationship between the robustness of a given apology and the extent to which the subsequent relevant policies are group conscious and injustice responsive. Alternatively, it would seem advisable in an expanded or amended theory to address the sort of phenomenon that we appear to see in the contemporary Canadian case: increased apologetic robustness coupled with diminished responsiveness to injustice.

Finally, because the book focuses so heavily on apologies to Indigenous peoples, The Politics of Official Apologies might have done more to address the political specificity of apologies in Indigenous-settler contexts. If, as Nobles cogently argues, apologies are sought to alter the terms of national membership, then Indigenous visions of membership would seem pertinent. For example, do groups in full confrontation with settler-state citizenship display a correspondingly negligible interest in apologies, or do groups seeking to revise the terms of membership value apologies no matter the nature of the desired revisions? The book’s relatively brief discussions of African Americans and (non)apologies might have asked similarly whether apology is a more important objective for organizations with integrationist as opposed to nationalist agendas. The point is that thinking about apologies and membership requires thinking about the membership visions of particular apology-seeking groups.

The Politics of Official Apologies is an excellent book that stakes out important ground. The criticisms outlined here are themselves testimony to the fruitfulness of Nobles’ membership framework, emerging “from it,” rather than “against it” as it were. The book is careful and nonpolemical; it does an excellent job of situating the phenomenon of apology in the wider field of redress politics; and it navigates admirably and sensitively through the specific details and histories of the relevant cases. Sophisticated and scholarly, but also suitable for course use with graduate students and senior undergraduates, it should be on the bookshelves of every researcher working on questions of historic injustice.

Matt James, University of Victoria


In this well-written, accessible, and superbly researched book, Meguid offers a new framework for explaining what she calls niche party success and failure. She argues that existing institutional and
sociological theories do not explain the rise and fall of niche party electoral performance. Institutions are typically constant and sociological explanations (such as presence of immigrants, value orientation, and economic prosperity) often predict the opposite outcome than what is observed. Meguid, thus, brings the party back into the discussion and argues that it is mainstream party strategy that determines niche party success.

While a handful of scholars have used the term “niche party” to describe the set of parties that are not the main players in a given political system, many of which have emerged over the last 30 years, the party families which are included in this category vary across studies. This typology can include communists and other extreme leftists, greens, regional and ethnically based parties, and radical right parties. Meguid, however, narrows this definition to include only those parties that (1) reject the traditional class-based orientation of politics, (2) raise issues that are outside of left-right political divisions, and (3) focus on a more limited set of issues. Given these restrictions, the niche group includes three party families—greens, ethnorough case study discussions. One of the few weaknesses of this study is that Meguid does not explain how she defines success and failure.

In a later chapter, Meguid extends her analysis and demonstrates her theory’s portability outside of the West European plurality systems of the United Kingdom and France. First, she briefly discusses three additional cases, which are compared to the above three parties and form party family pairs (the British National Front, Breton Democratic Union, and French Green Party). Second, she applies her theory to explaining the electoral strengthening of the U.S. Green Party and electoral decline of the Australian One Nation Party.

One of the few weaknesses of this study is that Meguid does not explain how she defines success and
failure. Does success correlate with a specific vote share? Or is it related to issue ownership or salience? This is not clear and one could envision how it could affect the overall results. The analysis would benefit from clarifying these concepts so central to the analysis.

Nonetheless, Meguid’s book significantly advances our understanding of the relationship between mainstream and niche parties, niche party success and failure, and party competition between unequals and nonproximal parties. This review has attempted to summarize Meguid’s key theory and findings, though it is hard to do justice to such a rich book in so few words. Future work should build on her theory and findings and use this framework for analyzing niche party electoral performance under differing institutional settings.

Jae-Jae Spoon, University of Iowa


Why do ruling parties limit their own power by enacting reforms that create independent judiciaries? Jodi Finkel argues that the main reason lies in the ruling parties’ self-interest: they empower courts when they believe they are unlikely to maintain political power in order to limit the future government. The insurance logic of judicial empowerment has been explored before by other authors (e.g., Ginsburg 2003; Ran Hirschl 2004), including Finkel herself in previous work. The main contribution of her new book is a nice theoretical distinction between the initiation period, when constitutional changes are made, and the statutory period, when the implementing legislation is enacted. The argument is that the insurance logic functions best at the implementation phase. Ruling parties are more likely to agree to constitutional changes that empower courts because at that stage the costs are neither immediate nor certain. At the implementation period, however, the real costs are felt and “presidents who enjoy a congressional majority can undermine increases in judicial power that were agreed on at initiation” (13). Thus, the likelihood of meaningful implementation increases as the ruling party’s probability of reelection declines.

Jodi Finkel makes her case based on three detailed case studies of the judicial reforms carried out during the 1990s in Argentina, Peru, and Mexico. The rationale for case selection is that these countries allow controlling for two common alternative explanations: that judicial reforms are a response to societal groups pressing for a more effective judiciary and that they were undertaken to ensure successful economic reform. Finkel argues convincingly that the role of civil society in the three countries was not determinant and that the economic argument alone cannot explain the resulting variation in institutional power in reform judiciaries. In addition, the cases provide variation on the outcome of reform, with Mexico going furthest in granting real power to the judiciary, Argentina delaying five years the implementation of reform, and Peru where executive interference eviscerated the reforms shortly after they were undertaken (14).

The logic of the insurance model makes sense, as does Finkel’s distinction between initiation and implementation of judicial reform. The main arguments and examples in the book prompt interesting theoretical questions regarding judicial empowerment. I will focus on two of them. The first concerns the distinction between two stages of reform and the knowledge actors are assumed to have at these two stages. The second one is about the empirical tests of the insurance model.

According to the argument, at the constitutional stage the ruling party bargains with the opposition thinking that in the next implementing stage it will retain power to make ordinary laws and thus will be able to impede or at least delay the costs of empowering courts. But the opposition is focused in the first stage. Why in this account is the ruling party the only one with an eye on the future during the constitutional bargain? Arguably, the opposition also bargains foreseeing what will likely happen at the next stage and could, for instance, demand to include more safeguards in the constitution instead of leaving them for ordinary legislation. If the constitutional change is done via amendment, for instance, the constitution makers know the distribution of seats in the ordinary legislature. In constituent assemblies the future composition of the legislature would be more difficult to foresee because it will be defined in future elections. In any case, a rational political opposition is likely to demand greater guarantees of judicial empowerment to be included in the constitution (or at least supermajority requirements for passing the implementing legislation) if they know that they will not be able to stop ordinary legislation that undoes the constitutional changes. To be fair, Finkel’s discussion of the Argentinean case implies this intertemporal rational calculation by opposition parties (52, 53) even though it is absent in the theoretical account.