

missing are nationalism in the EU, racism in the United States, language in Canada, citizenship in Germany, and separatism in Spain). I think we can chalk up most of these faults to the book's forced friendliness to economic political economy (thus, strange constructions like "many black families find it rational to migrate to urban areas either in the North or in the West" [p. 113], and southern paternalism described as a "peculiar internal equilibrium" [p. 116]). Finally, while Beramendi's take on geographical mobility—that it can spur redistribution between regions when rich and poor voters in rich regions fear an invasion of the poor from poor regions—is interesting and provocative, the measurement seems highly problematic. Mobility within a political unit is used to proxy mobility between political units (pp. 80, 114–15, 161), but we know these are very different. The use of outward mobility from a political unit (p. 100) is not much better, since we do not know from the emigration rate the destination of the migrants. Surely mobility is better measured as relational (dyadic or triadic over short periods of time), not as an attribute of political unions.

Faults aside, this book has arrived at the right historical moment, as the debate over the future of the EU, the convulsive Arab Spring, and even the current ballot initiative to create six Californias seem to destabilize fiscal institutions, representative structures, and economic geography. EU egalitarians might infer that they should advance centripetal representation, if redistribution is to grow from its currently tiny (< 1%) share of EU GDP. EU antiegalitarians might infer that they should resist strongly any such advance and preserve the near irrelevance of the European Parliament. The strength of these inferences suggests how much *The Political Geography of Inequality* should change how we think about redistribution in complex political unions.

Corruption and Reform in India: Public Services in the Digital Age. By Jennifer Bussell. Cambridge: Cambridge University Press, 2012. 346p. \$103.00 cloth, \$34.99 paper.
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— John Echeverri-Gent, *University of Virginia*

E-governance, the incorporation of information technologies into public administration, has been widely hailed as an important means of enhancing government responsiveness and accountability. Yet recent studies have shown wide variation in the impact of e-governance reforms. Jennifer Bussell employs a sophisticated multi-method research design to investigate the variable impact of e-governance reforms. Her subnational comparisons across 20 Indian states; structured comparisons of India, South Africa, and Brazil; and cross-national quantitative analysis of more than a hundred countries each finds that the pattern of corruption in a country is an important factor

shaping policy reforms. While other scholarship has shown the impact of corruption on public policy reform, Bussell's analysis makes an important contribution by differentiating the distinctive impact of petty and grand corruption.

Subnational comparisons across Indian states comprise the bulk—five of nine chapters—of Bussell's analysis. The author uses qualitative and quantitative analysis to examine the reforms' social impact, timing, and scope, as well as governance structure and scale. Throughout India, e-governance has been introduced primarily in the form of one-stop, computerized service centers where citizens can go to access services from a range of government departments. Using both survey and experimental instruments to investigate the social impact of service centers in the state of Karnataka, Bussell finds that e-governance, on average, reduces the bribes that citizens must pay and more generally the costs of accessing services.

Many analysts suggest that the level of economic development and the intensity of political competition are associated with the introduction of e-governance. However, using an event-history model to analyze the experience across Indian states, Bussell finds no significant relationship between the incidence of e-governance and variables for development, governance capacity, and political competition. Instead, she finds that the level of petty corruption, or the extraction of bribes in return for government service provision, is significantly and robustly associated with a reduced likelihood of reforms.

There is a considerable variation in the number of services provided by e-governance centers in India. Surprisingly, centers in one of India's poorest and least developed states, Chhattisgarh, provide seven times more services than centers in the affluent national capital of Delhi. Bussell finds that the level of petty corruption reduces the number of services provided, and it is associated with an even greater reduction in the number of services with a high potential for corruption. Furthermore, her illuminating case studies in seven states show that political leaders target the services provided by the centers to benefit their political supporters.

Coalition governments also affect the incidence and scope of the services provided by the e-governance centers. Coalition rule is associated with statistically significant delays in the introduction of e-governance reforms. When coalition leaders implement these reforms, they limit the costs to key coalition partners by excluding services from departments under the partners' control, even as they attempt to realize the electoral benefits of implementing e-governance reforms by providing a greater number of services from departments under their own party's control.

Bussell's analysis shows that grand corruption—when officials involved with procurement or development programs extract bribes from service providers—affects the governance structure of the computerized centers.

Politicians in states with high levels of grand corruption prefer to subcontract e-governance centers to a single private company because it enables top political officials to collect a larger bribe per transaction. Politicians in states with low levels of grand corruption are more inclined to resort to state ownership of e-governance centers. States that view e-governance reforms as an opportunity to promote development prefer to subcontract centers to individual entrepreneurs. Petty corruption interacts with the governance structure to affect the dynamics of e-governance implementation. When states with low levels of grand and petty corruption partner with entrepreneurs, citizens receive the broadest scope of services and the most beneficial outcomes. In states with high grand and petty corruption, political leaders are likely to contract with single companies and provide only limited services. This results in a limited number of poorly functioning centers that offer few services. States with high levels of grand corruption but low levels of petty corruption produce a limited number of company-owned centers that provide extensive services, especially to the government's supporters. In states with low grand corruption but high petty corruption, the centers are publicly owned or run by entrepreneurs, but they offer few services and their operators struggle to maintain their center's viability.

Comparing South Africa and Brazil with India affords the opportunity to examine greater variation in political competition. In India, while relatively high levels of political competition encourage national level reforms, high levels of petty corruption result in only a limited scope of service provision. South Africa has less political competition at the national level but lower petty corruption. There are some national-level reforms, but the state has yet to make e-governance an important policy priority. Brazil has relatively high levels of political competition and low levels of petty corruption. E-governance reforms are more comprehensive than in India or South Africa. Bussell's large-n, cross-national analysis offers further support for the impact of patterns of corruption on e-governance, though it provides only limited, mixed support for the consequences of political competition.

The strength of Bussell's book is its careful empirical analysis. At both the subnational and cross-national levels, the author tests her arguments with quantitative analysis highlighting their plausibility across cases and with detailed case studies investigating the validity of the causal mechanisms that she posits. Bussell addresses the difficult challenge of providing valid measures for petty and grand corruption in India with ingenuity, and her measures, while not perfect, are reasonable proxies. Her findings about the distinctive impact of grand and petty corruption in shaping e-governance reforms are robust and have extensive implications for the study of policy reform.

This study has interesting implications for the politics of public goods. They deserve further development. Indian

politicians' transformation of e-governance from a public good to a targeted club good means that in many cases, the provision of public goods requires more than overcoming the free-rider problem. It also requires that political leaders be motivated not to convert public goods into exclusive club goods for the benefit of their supporters.

The book raises unanswered questions about the impact of political competition. Bussell, like other analysts, hypothesizes that political competition often motivates administrative reform. However, in India—where rules for campaign finance encourage illicit funding, the legal system rarely prosecutes corrupt officials, and politicians punitively sanction government officials if they do not bend to their will—political competition discourages the initiation of reforms and limits their benefits by elevating the importance of petty corruption as a valuable source of campaign finance. It is no wonder that Bussell finds mixed support for political competition in her econometric analysis. Her study shows that we need to better specify the mechanisms through which institutional conditions shape the impact of political competition.

Corruption and Reform in India is valuable not only for its novel findings but also for the questions that it raises. It should be widely read by scholars of comparative politics and public administration.

The Politics of Memory in Chile: From Pinochet to Bachelet. Edited by Cath Collins, Katherine Hite, and Alfredo Joignant. Boulder, CO: Lynne Rienner Publishers, 2013. 279p. \$69.95. doi:10.1017/S1537592714002539

— Michelle D. Bonner, *University of Victoria*

In *The Politics of Memory in Chile*, editors Cath Collins, Katherine Hite, and Alfredo Joignant ask: How does memory of the recent past shape Chile's political community today? Chile is often touted as a model of transition from authoritarianism. After suffering a long and brutal military rule under General Augusto Pinochet (1973–1990), it held a truth commission, trials, offered reparations, and has seen hundreds of memorial sites built. Yet, how to remember the past remains an area of important political debate and division. This debate offers insights that may be useful for other countries struggling with a traumatic past and makes an important contribution to the large literature on transitional justice (such as work by Kathryn Sikkink, Leigh A. Payne, and Elizabeth Jelin).

The editors answer their question through a rich exploration of how collective memory of the recent past has been debated and transformed since the return of electoral democracy (1990–2010). Each chapter explores memory in a different way: a turning point (Pinochet's 1998 arrest in London), trials, the history of torture, memorials, Pinochet's funeral, and public opinion polls. By retracing events from different perspectives the reader