in interest intermediation, and the joint shifts in the macroeconomic regime and the ideational background of policy making.

Chapters 3–6 study key institutions in the construction of the complementarities that have come to define Germany as a CME. Niccolo Durazzi and Chiara Benassi explore the changes in skill formation in Germany (crucial to the typology of capitalism), from vocational training to higher education (chapter 3). This change is guided by the intervention both of the state and of larger firms. Ute Klammer, in chapter 4, takes up the topic of pension reform, documenting the withdrawal of the state and the stepping-up of private companies, which she argues lead to higher inequality. Chapters 5 (by Benjamin Braun and Richard Deeg) and 6 (by Thomas Haipeter) shift focus to the financial sector to show that the export growth model erodes banks' hegemonic position. Because nonfinancial corporations can now finance investment from retained profits, the power of banks vis-à-vis German industry is undermined—crippling another important pillar of this CME. Chapter 6 challenges the concept of financial capitalism, used to characterize contemporary pressures on industrial relations and working conditions. Haipeter finds that the concept is lacking explanatory power both because of the crowding-out of other explanatory factors and the erasure of agency and social interaction it implies.

Chapters 7 and 8 deal with industrial relations, whereas chapter 9 focuses on the contradictory economic paradigm of the Social-Democratic Party of Germany (SPD). Stephen J. Silvia studies the so far unsuccessful efforts of German unions to export their power beyond borders (chapter 7). Leveraging five case studies across countries and sectors, the author finds that global supply chains are an obstacle for the organization of workers. Martin Behrens and Heiner Dribbusch look at employers' hostility toward works councils in chapter 8. Although in theory workers' representation at the workplace is a fundamental part of the German model of labor relations, the authors uncover a great deal of resistance from employers. This resistance, moreover, is evenly distributed among the strong manufacturing sector and the weaker service sectors (pp. 141–48). These two chapters indicate that the German model is bleeding at its manufacturing core, rather than being undermined from the services in the margins—a counterpoint to accounts that stress dualization, instead of liberalization (e.g., Anke Hassel, "The Paradox of Liberalization—Understanding Dualism and the Recovery of the German Political Economy," *British Journal of Industrial Relations*, 52 [1], 2014, and Kathleen Thelen, *Varieties of Liberalization and the New Politics of Social Solidarity*, 2014). In chapter 9, Björn Bremer focuses on the SPD and its inability to create an alternative to fiscal orthodoxy. The author documents a party divided between a left wing that still believes in Keynesianism policies and a right wing overly influenced by supply-side economics (pp. 156–60).

Chapter 10 considers the role of ideology and ideas, whereas chapters 11 and 12 take up a macroeconomic perspective to understand outcomes in income inequality and in Germany's current account, respectively. Briggite Young challenges ordoliberalism as the explanation for Germany's economic success and the European response to the crisis (chapter 10). By juxtaposing the basic framework and principles of ordoliberalism with actual policy making, she claims that ideas carry little explanatory power for the country's economic growth—which she instead attributes to the euro and Germany's growth model (pp. 183–86). Jan Behringer, Nikolaus Kowall, Thomas Theobald and Till van Treeck document in chapter 11 that inequality in the country is higher than that recorded by household surveys and that it in turn contributes to macro instability. Wade Jacoby, finally, explains the causes of the large trade surplus in Germany in chapter 12. He then shows not only its problematic character but also how German elites have "normalized and apologized" for its consequences (pp. 215–18).

Although extremely valuable on their own, the different chapters could have followed the theoretical contributions sketched in the introduction more closely and engaged with each other's insights for a more compelling overall achievement. Notably, the reader is left to wonder how the conclusion that ordoliberalism "does not matter" squares with the account of the ideational divisions of the SPD and how inequality, in turn, is connected to the resistance to unions within workplaces.

Although they focus on one country, both books deal with pressing questions in the field: the agents and mechanisms behind institution building, continuity and change in political economies, and the outcomes of these formations on growth and inequality. There is a disagreement on this last point: whereas the authors of the edited volume emphasize increasing inequality, Manow acknowledges it but highlights the stronger growth achieved with respect to the counterfactual of a Germany without reforms. Yet "no reforms" is not the only counterfactual at play, as the edited volume suggests by providing a more granular account of change, including proposals less committed to marketization and corporate political clout. Their contrasting conclusions notwithstanding, the books touch on the question that both political science and politics have been debating since the golden age of capitalism ended: What are the alternatives?

The Veil of Participation: Citizens and Political Parties in Constitution-Making Processes. By Alexander Hudson. Cambridge: Cambridge University Press, 2021. 224p. $110.00 cloth.
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Recent years have seen the publication of many high-quality works in both political science and law on the process of constitution making, which was once a very

understudied field. Nonetheless, key questions remain unanswered, and we still know far too little about the impact of constitution-making processes on important outcomes. Perhaps most troublingly, this creates an enormous gap between the recommendations that advisers and NGOs offer as gospel on the ground and the state of social science findings.

Alexander Hudson's new book, *The Veil of Participation: Citizens and Political Parties in Constitution-Making Processes*, tackles one of the most important—and difficult—gaps: the impact of popular participation during constitution making. The call for extensive popular participation is probably the most ubiquitous recommendation of transnational policy makers, and it has even been described as perhaps the only international law norm bearing on constitution-making processes. But the meaning of participation is ambiguous. Participation comes in many different forms, some likely much deeper than others. Popular referenda at the beginning and end of the constitution-making process, education campaigns, and various methods (both in-person and virtual) to solicit input have all been described as participatory (see Abrak Saati, *The Participation Myth: Outcomes of Participatory Constitution Building Processes on Democracy*, 2015).

Moreover, the state of the social science on this question is murky. Whereas some work finds evidence that more popular participation increases levels of democracy (see, e.g., Todd A. Eisenstadt, A. Carl Levan, and Tofigh Maboudi, *Constituents before Assembly: Participation, Deliberation, and Representation in the Crafting of New Constitutions*, 2017), other work casts doubt on such a conclusion and instead pinpoints pluralistic agreements between competing elites as the key for democratic outcomes (see, e.g., Gabriel A. Negretto and Mariano Sánchez-Talanquer, "Constitutional Origins and Liberal Democracy: A Global Analysis, 1900–2015," *American Political Science Review*, 115[2], 2021). Against this backdrop, *The Veil of Participation* takes a novel approach. It asks not *whether* popular participation makes a difference in outcomes, but *when* it might matter.

The answer the book gives is that success depends on the strength of the parties involved in constitution making. Strong parties create few openings for public participation to influence the constitution; they will instead draw on their own platforms and internal discussions to shape the constitutional product. Weak parties offer more opportunities because representatives will approach the process with fewer preconceived ideas.

The evidence includes detailed, chapter-length case studies of three relatively recent processes: South Africa's 1996 constitution, written largely by the dominant African National Congress (ANC); Brazil's constitution in 1988, which was drafted by a conglomeration of relatively weak, patronage-based parties; and Iceland's failed 2012–13 replacement process, which unusually was drafted by an assembly that essentially excluded the existing parties altogether. All three processes included robust and creative efforts to solicit popular input. Yet the Icelandic experience, by far, is the one where popular participation appears to have most shaped the final product, influencing an assembly of relative political outsiders. Even there, fewer than 10% of the suggestions collected via novel use of tools like Facebook ended up in the final text.

Hudson also carries out a quantitative analysis of all constitution-making processes between 1974 and 2014. Because it is nearly impossible to gather direct, large-N data on the impact of popular proposals on the final text, the book uses as a reasonable (if debatable) proxy the number of novel rights included in the constitutional text. The theory is that, because popular participation seems to mainly aim at adding rights provisions, successful participation should show up as an increase in the number of uncommon constitutional rights that are not part of the generic core almost inevitably found in texts. The model finds strong significance for an interaction term between levels of popular participation and the strength of parties. Where parties are one standard deviation stronger than the mean, moving from the lowest to the highest level of participation adds only one-half of a novel constitutional right. Where parties are one standard deviation weaker than the mean, making the same move in terms of increasing participation would be expected to add four (from two to six) additional novel constitutional rights.

The careful, multimethod approach taken by Hudson provides convincing support for the core argument and sheds light on the limited conditions under which popular participation will influence the constitutional text. Moreover, the findings sharpen an important trade-off: those conditions that are most conducive to effective popular engagement may also be those where the constitution-making process may be less successful in achieving other important ends. Take Iceland, where popular input played a relatively major role in shaping the constitutional text precisely because the assembly effectively excluded representatives of the parties. In the end, this design contributed to the failure of the process to produce a new constitution: the excluded political elites blocked its promulgation.

There is thus a complex trade-off between the inter-elite bargaining that might be necessary to stabilize a new constitution and popular participation. Absent more clarity about the contours of this trade-off, the ubiquitous policy recommendation in favor of a highly participatory process seems problematic. At minimum, other dimensions of constitution-making processes almost certainly matter more. More strongly, pushing for high levels of participation could in some contexts undermine those dimensions.

The most important limitation of the analysis in *The Veil of Participation* is explicitly noted by the author: it measures the impact of participation in only one way, by looking at how popular input shapes text. There are good

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reasons in terms of tractability for such a focus. And the findings provide a dose of much-needed realism. But the book's focus may undersell the value of participation by highlighting only one route through which it may make a difference.

There are, as Hudson acknowledges, other possible routes. Popular involvement may influence elite bargaining in different ways, both positive and negative; for example, forcing parties to stick to deals or preventing hard bargaining from occurring in the first place (see Jon Elster, "Forces and Mechanisms in the Constitution-Making Process," *Duke Law Journal*, 45[2], 1995). Moreover, a highly participatory process may increase the legitimacy of the final constitution, even if it does not exercise direct influence over the text, by increasing popular buy-in. This may in turn increase constitutional durability and political stability.

Hudson argues that the legitimizing impact of popular participation may rest on a kind of sham. As he puts it, there is a "worry that gains in the perception of the sociological legitimacy of a constitution are based on false statements on the part of constitution makers and inaccurate judgments on the part of the public" (p. 181). This framing is provocative, albeit perhaps overstated: sham-like participation processes certainly exist, during constitution making and elsewhere, but they should not be assumed just because they do not directly influence the final product. Take South Africa, which in Hudson's analysis represents a paradigm case of widespread, invited popular involvement whose impact is very difficult to trace in the final constitutional text. Almost all the text emerged, instead, from bargaining between the ANC and its rivals. But the robust process of popular involvement still seems like a significant net-plus: it increased education and engagement with the text, and the ANC itself had substantial legitimacy as the driving force in post-apartheid South Africa.

Scholars and students of constitution making, as well as other forms of popular participation in politics, will benefit greatly from Hudson's excellent book, both in the answers it gives and the new questions it asks.

Colonial Institutions and Civil War: Indirect Rule and Maoist Insurgency in India. By Shivaji Mukherjee. New York: Cambridge University Press, 2021. 392p. $99.99 cloth.
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In recent decades, scholars of civil war have begun to dig deeper into the history behind contemporary conflicts using new datasets and sophisticated quantitative analyses. Many of these studies have linked the origins of civil war in states across South America, Africa, and Asia to the era of European colonialism.

Shivaji Mukherjee's *Colonial Institutions and Civil War: Indirect Rule and Maoist Insurgency in India* is a new entry into this literature, tackling the important case of the world's largest colony and its long-running Maoist insurgency. This conflict was once considered India's gravest internal security threat, having taken place from 2005–12 in as many as 30% of the country's districts (p. 6). Mukherjee's central argument is that "different forms of colonial indirect rule…created long-term persistent and path-dependent effects conducive to leftist ideological insurgency in India" (p. 10). Specifically, indirect colonial rule led to low state capacity and development and the political exclusion of indigenous groups. This created grievances among the population and opportunities for rebels.

One of Mukherjee's central aims is to develop a "more fine-grained typology of different types of indirect rule" (p. 19). He differentiates between *formal* indirect rule in the "princely states," areas that remained under the control of native rulers, and *informal* indirect rule in British areas that came under *zamindari* (landlord) tenure. Direct rule, which is not dichotomized, occurs in British areas under *ryotwari* (cultivator) tenure. Rather than code all British areas as direct rule and all princely states as indirect rule, as many scholars do, Mukherjee argues that indirect rule occurs through native rulers, whether they are princes or landlords. Using a variety of qualitative and quantitative methods, Mukherjee then shows that indirect rule(s) created the grievances exploited by Maoist rebels in both the northern epicenter of the conflict (landlord areas in Bengal, Bihar, and Jharkhand) and the south-central epicenter (princely states in Andhra Pradesh, Chhattisgarh, etc.). At the end of the book, he suggests that this theory can generalize to neighboring cases in Burma and Pakistan.

Mukherjee's work offers some real advancements to the study of colonialism and civil war. First, he is right to move beyond the binary direct/indirect rule conceptualization and coding used by many previous scholars. Chapter 6 illustrates the full promise of doing so, as he builds on the work of historian Barbara Ramusack to code five distinct kinds of princely states in his quantitative analysis. To deal with selection issues—why certain areas came under colonial rule whereas others did not—Mukherjee develops a new and plausible instrumental variable for colonial indirect rule in India based on the timing of European wars, which reduced the willingness of the British to annex princely states (pp. 136–37).

The best part of the book is Mukherjee's extensive fieldwork in India—in conflict zones, no less—and his case study chapters are richly detailed. In chapters 7 and 8, for example, he draws on a unique blend of archival documents, state legislative assembly data, and data on Maoist influence at polling stations to test his theories at the subdistrict level in two case studies from the southern