As the quality of democracy declines around the world—including in democracies like the United States and Poland—the role and function of civil society organizations become even more important to understand. We know from decades of research on democratic development that organized civil society is one of the few spaces that can potentially promote, strengthen, and advocate for more robust democratic systems. Further, since the 1980s, state services and budgets have been cut in many European and North American nations. Nonprofit and public benefit (or charity) organizations are often called on to pick up the slack. At the same time, citizens around the world are increasingly less engaged in the organized sphere and more distrustful of politicians and governments. To understand the complex interplay of these issues, it is important to investigate the relationship between civil society and the state in complicated political and economic environments.

Nicole Bolleyer’s well-organized book addresses one void in our understanding of these issues: the regulatory framework that governs political parties, interest groups, and public benefit organizations. She reminds us that state regulation of the voluntary sector is important in that it defines what is possible in the civic sphere. In other words, the legal framework that regulates these organizations will determine if they can improve the quality of democracy and effectively provide services to citizens amid fiscally constrained budgets. In this extremely comprehensive and thorough tome, the author is able to view these governing environments across organizations and across countries, with a focus on Southern and Western Europe, as well as the United States, Canada, Australia, and New Zealand. The mere scope of her analysis, combined with a detailed documentation of the codes that regulate the civic sector, is an impressive contribution to our understanding of civil society in established democracies.

One of the book’s most important contributions is the meticulous descriptions of the legal frameworks that regulate the three kinds of organizations in the 19 “long-standing” democracies. Bolleyer’s thorough review of these frameworks, including the kind of monitoring agency that oversees the work, harks back to the origins of comparative political studies. As such, the book is a valuable addition to the more current neo-institutionalist literature, as well as the growing field of comparative legal studies.

In undertaking this careful analysis, Bolleyer makes two observations. First, by moving beyond studies that focus on one kind of organization, she is able to employ a “legal inclusive” framework, which explores how the same regulation applies to different kinds of organizations simultaneously. This allows her to uncover previously hidden complexities in the legal code that affect the ability of organizations both to form and operate. For example, research on political parties might focus on political party law, when, in fact, in many of these nations’ political parties are a kind of nonprofit organization. Focusing on party-specific rules misses some of the ways that states regulate these organizations under the larger umbrella of nonprofit law. Importantly, this overlap in civil society organization (CSO) law is often unintentional and perhaps not even understood by lawmakers themselves.

The second observation is that legal frameworks differ according to the distinct phases in the life of an organization. Bolleyer breaks these phases into three categories: formation, operation, and dissolution. She is then able to closely examine laws in light of each phase, demonstrating that regulating the operations of one particular kind of nonprofit organization—for example, lobby organizations—will have unexpected effects on other kinds of organizations like public benefit organizations.

The implications of these findings are important and unexplored; it would be interesting to think more about how these unintended webs of regulation are affecting the ability of these organizations to undertake their work, whether it is in the guise of service provision or political advocacy. Further, to what extent can or should these findings be shared with those making the regulations that are having these unintentional effects? Can or should lawmakers be alerted to these overlaps, or are the overall effects of her observations relatively benign?
Next, Bolleyer moves on to the cross-national analysis of legal frameworks to detect patterns of “legal disposition” over time. She asks: Do countries tend to be more or less constraining (or permissive) when regulating CSOs? The analysis of the laws allows Bolleyer to develop a complex Legal Regulation Index, which scores the 19 countries and then ranks them from most constrained to most permissive (p. 173). The index is founded on impressive logic and precision, an undertaking made possible by the careful attention to detail and the innate complexities that define this work.

Interestingly, the results of this index show that some Northern European countries (Denmark, Sweden, Switzerland, and Norway) have more permissive legal environments, whereas Ireland, Canada, the United Kingdom, and the United States place more constraints on the civil society sector. To a casual observer, this may seem counterintuitive. The countries with more permissive environments tend to have more state oversight and involvement in many spheres, such as environmental and social services, than countries like the United States and the United Kingdom, which have slowly eroded regulatory frameworks in many areas (especially financial).

To better understand the index scores, Bolleyer asks why some countries have more or less constraining legal environments. In other words, she explores the conditioning factors that determine the nature of the environment, which allows her to explain these counterintuitive results. Bolleyer develops a typology of five conditions: legitimizing (experiences in democratic stability and the nature of the voluntary sector regime), reinforcing (the nature of the legal system), and facilitating (the decision-making and administrative capacities of the regime). Using this typology, she employs qualitative comparative analysis (QCA) to determine which conditions are either necessary or sufficient for the observed outcomes (p. 190).

Again, the meticulous attention to detail leads to some interesting findings. As would be expected, no condition alone can explain the observed outcomes; rather she identifies combinations of conditions that explain more constrained and permissive regulatory frameworks. She determines that several “paths” toward these outcomes exist and that the combination of legitimizing conditions (experience with democracy) and reinforcing conditions (legal family) is particularly important in explaining the observed outcomes. Specifically, experiences with democratic instability and corporatist voluntary sector regimes—read postwar Germany—will lead to more constraining regulatory environments for the voluntary sector.

This part of the analysis is the boldest and most creative yet, at the same time, the least satisfying. To understand these paths, Bolleyer is contributing to our understanding of the underlying environments that explain legal norms in general, moving beyond the three specific kinds of organizations. However, the framework might serve the goal of theory development more effectively with more parsimony. To truly develop a mid-range theory of state regulations of civil society organizations in long-established democracies, a slightly more concise framework may garner more analytical leverage.

The issue of theory development leads to another question about the case selection and the generalizability of the findings. Bolleyer narrows the country cases by focusing on stable democratic systems since World War II (p. 47). This limits the cases to mostly EU cases. As someone who writes about Latin America and the developing world, I immediately wondered why she chose this particular focus. Would expanding the geographic scope to stable democracies not in North American or European environments (Costa Rica) and since the postwar independence movement (India) provide additional analytic leverage? Would the finding about democratic instability as an important factor in explaining the outcome be better understood if countries with actual experience with instability (Chile) were evaluated? What else might we learn about the state-civil society relationship by exploring these cases?

It would also be interesting to explore the implications of these paths. How does a constraining path affect the work of political parties in places like France and Germany? Are they still able to channel citizens’ political demands? Can public interest organizations represent their members in the same way or provide effective services? In other words, how do these paths affect the quality of democracy and service provision?

In sum, the book provides a variety of empirical and theoretical contributions about many unexplored and previously hidden ways that the state and civil society interact in established democracies. Bolleyer’s careful logic and evidence about three kinds of organizations in 19 countries are exemplary. Her use of QCA can also provide a model to new scholars who are interested in this particular methodological tool. Overall, I recommend this book for scholars who are interested in best understanding civil society, including political parties, in democratic nations.

Response to Stephanie McNulty’s review of The State and Civil Society: Regulating Interest Groups, Parties, and Public Benefit Organizations in Contemporary Democracies
doi:10.1017/S1537592719003694

— Nicole Bolleyer

It is a pleasure to respond to Professor McNulty’s excellent comments. With only limited space to answer
to a multitude of important points, I would like to focus on the (in my view) three most fundamental ones.

I start with the comment that caught me by surprise. McNulty asks whether a more parsimonious framework would have helped me to “truly develop a mid-range theory of state regulation of civil society organizations in long-established democracies.” My expectation was to be criticized, if anything, for the opposite, given that we have (with few exceptions) predominantly very case-oriented research on civil society regulation focusing on single countries or an “N” smaller than 19 democracies. My framework formulates theoretical expectations on five macro conditions derived from research cutting across literatures in politics, sociology, and law. Only three of them (in different configurations) form part of three main “paths” shaping democracies’ dispositions toward either an (overall) more permissive or more constraining approach to civil society regulation. These paths—experiences of democratic (dis)continuity and (in)stability, voluntary sector type, and legal family—affect the nature of legal regulation of political parties, interest groups, and of public benefit organizations. Asking for more parsimony suggests that at least one of these three could be removed without diminishing the ability of the approach to account for the variation in legal permissiveness or restrictiveness across the 19 democracies covered. In empirical terms, the analysis clearly suggests that it is configurations of (respectively, two of three) conditions rather than any single one that grants the best grasp of the legal differences found. In theoretical terms, there is no general yardstick for when any theory is “parsimonious enough” without consideration of the phenomenon it is developed to account for. The question then becomes whether we would understand more or less about long-established democracies’ legal dispositions toward or against legally constraining organized civil society by focusing from the outset (in terms of theoretical framework) on less. Based on earlier works inspiring my study and the research on which that framework is based, it is probably no surprise that my answer to this question would be no.

Furthermore, McNulty rightly queries whether the approach could be more widely applied to democracies outside Europe and beyond the long-lived democracies that are considered fully consolidated: my answer to this would be, in principle, yes. For instance, the expectation that governments heavily subsidizing voluntary organizations to provide public services are more inclined and legitimized to regulate them in a constraining fashion than those governments that do not is likely to apply more broadly. So are the expectations regarding democracies’ “defensive” legislative responses to internally triggered threats to the democratic system. That said, some of the classifications used from which central arguments were derived, such as types of voluntary sector regimes or legal family, were developed with the long-lived European democracies (plus the United States) as central reference points. A wider application would pose the challenge that cases are more likely to cut across the “templates” to which theoretical expectations about legal dispositions toward civil society tend to be tied. Mechanisms expected to affect legal dispositions would have to be operationalized across systems without being able to fall back on “standard classifications.” Though predominantly a challenge of operationalization, it is one with possibly far-reaching implications for testing the theoretical expectations in a methodologically sound fashion on a broader scale.

Related to this point, McNulty also asks what we could learn from a broader application. Let me briefly respond to one specific comment related to this important question, which suggested that I look at countries with “actual experience with instability” such as Chile. I do not think democratic instability in Chile is more genuine or telling than instability experienced by various European countries over the last century (which included democratic breakdowns). However, where I would expect a broadening of the scope to newer democracies to lead to potentially different findings from my current study is related to differences in states’ administrative capacity. This factor is theorized in my framework as an enabling facilitating condition but did not play an important role empirically. In newer democracies, administrative capacity might gain relevance to account for whether democracies tend to apply constraining legislation to civil society actors, because such regulation might be—given greater resistance—costly to implement. Broadening the scope to newer systems and thereby enhancing differences in administrative capacity are likely to make issues related to the implementation of legislation generally more crucial for understanding what type of legislation we are likely to find. This, of course, links to the important lessons of McNulty’s own study on the implementation of legal reforms in developing countries, where policy implementation is a much bigger hurdle than in the long-lived European and North American democracies.


— Nicole Bolleyer, University of Exeter
n.bolleyer@exeter.ac.uk

Stephanie L. McNulty’s book, Democracy from Above? The Unfulfilled Promise of Nationally Mandated Participatory Reforms, asks whether national politicians can effectively mandate subnational participation, with beneficial effects for democracy and governance, such as expanded participation, increased government responsiveness, and reduced corruption. This question is highly salient not
only for those developing countries covered in this excellent book but also for representative democracy generally, in which citizen disengagement and alienation are increasingly widespread. No doubt, the search for measures able to reinvigorate representative democratic institutions is of crucial importance for academics and practitioners alike.

Although the in-depth case studies in the book focus on Latin America, nationally mandated participatory reforms are by no means a “regional” phenomenon, as the cross-national assessment at the start of the book demonstrates. Indeed, it is a pity that in-depth research on the 14 reform cases not covered in the three main case studies, including countries like India or Rwanda, did not make it into the book (although the author does provide a link to an online appendix that any reader should consult).

The findings of this important study are as insightful as they are sobering. As McNulty points out, instead of remedying pitfalls of representative institutions, participatory reforms embedded in them often replicate rather than solve existing problems. Unequal participation, a weak civil society, clientelism, and corruption are central expressions of the “broken promise” of participatory reform. Although some of these expressions are particularly symptomatic for the developing world, this book is essential reading for anyone who wants to understand why reforms that might look good on paper do not deliver, even when passed with the best of intentions. That is not to say reforms did not make a difference at all. Yet while widening participation numerically, they did less to overcome inequalities between those who tend to participate and those who tend to do so less, or to change problems fundamentally plaguing the political system such as clientelism or corruption.

McNulty’s book also raises some questions, as any challenging study will do. The book starts with a cross-national analysis of nationally mandated participatory reforms in subnational governments introduced in developing democracies. The scope conditions chosen to identify relevant countries to be considered are convincingly rationalized: 142 (economically) developing countries were included if they had a decentralized state structure and adopted relevant participatory reforms. Covering the period 1985–2015, 17 countries qualified. However, given the fundamental focus of the study of whether and how legal norms are implemented in practice and which effects they have on democratic governance, I had expected the discussion of two systemic properties that seem to be crucial preconditions to reasonably expect beneficial democratic effects of (any) participatory reform in any country. What about minimum levels of rule of law or democracy (for which various indicators are available) to focus on environments in which participatory reforms have some chance to be implemented? The literature on elections in authoritarian and hybrid regimes (for example, Andreas Schedler, Electoral Authoritarianism: The Dynamics of Unfree Competition, 2006) has stressed that “democratic practice” is only likely to result from the formal requirement to run regular elections when basic rights and protections are guaranteed to those participating in them as well. In unfavorable contexts, elections might be used as mechanisms of control, just as participatory procedures might be used for practices of clientelism and corruption. A more detailed consideration of these systemic scope conditions might have allowed for a discussion regarding the type of political system in which the “promise” attached to participatory reforms is more or less realistic.

McNulty focuses on participatory legal frameworks enshrined in national law and implemented widely across subnational governments that “must give actors meaningful decision-making power” (pp. 20–21). My initial reading of the latter specification was that either individual citizens or civil society organizations through these reforms have been (formally) granted the power to make some binding decisions that are final. If decisions can be modified by elites afterward, their impact is ex ante curtailed. This is even more the case if procedures are consultative (for different methods of public consultation, see Helena Catt and Michael Murphy, “What Voice for the People? Categorising Methods of Public Consultation,” Australian Journal of Political Science 38 (3), 2003). However, in the descriptions of the different institutional designs, the National Public Policy Conferences in Brazil, for instance, seem to be a consultative mechanism directed toward giving recommendations rather than actual decision making (pp. 43–44). Later on in the case studies, civil society participation through Peru’s regional and local coordination councils in subnational planning processes is characterized as ineffective because of its “non-binding” character (p. 134). This is not to downplay “voice mechanisms” that, strictly speaking, do not grant decision-making power, but whether the mechanisms studied do one or the other is of central importance. On the one hand, regional and local political elites have less incentive to undermine procedures if they are not bound by the input received. On the other, the potential to generate democratic effects and the incentive to participate on behalf of groups are likely to be weaker in consultative procedures. Finally, the type of input by citizens or civil society that is legally required provides the yardstick to evaluate whether legal requirements have “only” been implemented ineffectively or whether they have been violated outright.

More detail on the specificities of the legal designs would have been desirable to get a better sense of what difference different legislation could have made. One mechanism explicitly recommended is quotas to enhance participation of marginalized actors (p. 172). In addition, as McNulty persuasively points out, what is the use of
mandating civil society participation in Peru if registration as a civil society organization is so demanding that it restricts access to the process before it has even begun? Hence, legal access to the participatory process is crucial (pp. 162–63). Still, I would have liked to hear more about which of the designs systematized at the start of the study might work better under which conditions. Early on, McNulty presents six institutional designs that governments have adopted in the 17 countries covered, such as participatory budgeting laws or citizen or civil society councils (pp. 23, 40). The study analyzes instances of five of the six designs in three in-depth case studies of reforms in Bolivia, Guatemala, and Peru, putting her in an excellent position to assess past reform choices and future options. Intuitively, the “national council system” and even more so the “fourth branch of government” design seem to affect the state structure much more fundamentally than the other designs, because they constitute multilevel governance structures spanning national and subnational levels rather than focusing on lower levels only (pp. 40–44).

But given the overall findings of this study these “design templates” might be of little relevance. The real problem seems to be rule implementation: rules on paper are not put into practice properly, be this due to lack of training or resources or manipulation by subnational elites in the absence of effective oversight. Strikingly, this is the case even though “an implementation mechanism does exist in every case included in the analysis” and “time and effort are being dedicated to implementing these reforms” (p. 21). So maybe the content of rules and formal implementation structures are less central than legal reformers might hope. McNulty stresses the need for support by participants on both the national and subnational levels to assure effective reform implementation (p. 162). That, of course, is a rare constellation in any national levels to assure effective reform implementation support by participants on both the national and subnational levels rather than focusing on lower levels only (pp. 40–44).

And “financial resources” (p. 20), which seems clear enough. However, cross-national research on decentralization stresses the vast variation in competence and resource allocation implemented in different decentralization reforms. Some reforms only decentralize powers to implement national policies; others decentralize considerable decision-making power but too few resources to deliver the respective policies (see Daniel Treisman, The Architecture of Government: Rethinking Political Decentralization, 2007). If reforms are inefficient, how do we know this feature is linked to the specific participatory mechanism rather than more fundamental problems in state structures? Depending on the type of institutional setting considered, it could be either, which is of crucial importance for whether future reform of participatory procedures themselves is likely to improve matters or whether much more fundamental reforms are necessary to enhance subnational democracy and governance.

This research has received funding from the European Research Council under the European Union’s Seventh Framework Programme (FP7/2007–2013) / ERC grant agreement n° 335890 STATORG. This support is gratefully acknowledged.

Response to Nicole Bolleyer’s review of Democracy from Above? The Unfulfilled Promise of Nationally Mandated Participatory Reforms
doi:10.1017/S1537592719003797

— Stephanie L. McNulty

I am grateful to Nicole Bolleyer for her praise and thoughtful critique of my book, Democracy from Above? Both of our books ask important questions about how national legal frameworks can condition, constrain, and motivate citizen participation in democratic systems. These questions are especially vital to explore now as democracy is increasingly under attack.

It is rare to have this opportunity to reflect on Bolleyer’s commentary about the logic and evidence in my book. She notes that a discussion of systemic properties, such as rule of law or levels of democracy, would improve my analysis of why some nationally mandated participatory reforms—the focus of my book—do not live up to their promise. This is something that I certainly considered when I began writing. However, at the time, there were data limitations that did not allow me to explore additional independent variables in this particular analysis. Specifically, large datasets that included all nations and went back to the 1980s did not yet exist. I believe that testing the effect of many of the systemic conditions on the reforms’ outputs is best undertaken in a large-N statistical analysis. Fortunately, better databases, such as the Varieties of Democracy project, are now available; therefore I
am currently working on a quantitative analysis to complement the largely qualitative analysis in this book.

*Democracy from Above?* explores six kinds of participatory institutions that engage new actors in public policy decision making. Bolleyer asks: Can we determine which design has the most impact on democratic governance in these countries? She is correct that the most potential might lie in the national council or fourth branch design. However, my theoretical framework is based on the finding that the impact of all of these reforms, including the national-level participatory systems, will rest on several intervening variables: the nature of the design, civil society, and political support. In other words, in an environment with weak civil society and political support, such as Guatemala, the systemic participatory design will not have its purported effects; in an environment with stronger civil society and political support, such as Brazil, it may. Thus, the conditioning factors are so important that they can mitigate results for all of the institutional designs in ways that prevent or allow results. This makes it difficult to state with certainty which design will have the most impact over time.

Bolleyer also notes that it is hard to decouple nationally mandated participatory reform from the larger decentralization reform, a question that reflects her interest in legal inclusivity. In this particular study, I hold the larger decentralized state structure relatively constant in an effort to simplify the research design. This oversimplifies the real world, of course, and her point offers a fascinating line of research for future work on this issue.

Combined, the books explore the nature of civil society–state relations in almost the entire globe, Bolleyer focusing on the developed world (or the Global North) and my own work focusing on developing nations (or the Global South). Reading the books together demonstrates how research on these two different parts of the world will lead to different research questions, data collection, and methodological choices. Researching established democracies, for example, allows us to hold more factors constant and gather data in a way that is not always possible when undertaking a cross-national analysis of countries in the developing world. I enjoyed juxtaposing our common intellectual interests and realizing how much the distinct locations condition the nature of our analytic endeavors.