What is the rule of law? How should we measure its various aspects? What explains the way in which it manifests in different locations and changes over time? The contributors to this symposium tackle these questions. They offer a number of important insights and suggest productive avenues for future research. Just as Pasquale Pasquino opens the issue with remarks reflecting the interests of a political theorist focusing on constitutional theory, my remarks reflect the interests of a political scientist focusing on institutional theory. This orientation no doubt colors what I take to be a fruitful question to ask and what I take to be a productive answer. With this caveat in mind, my goal is modest. I hope to suggest a few ways in which the conversation started by the authors of this excellent symposium might be extended.

The symposium is framed around two readily apparent concerns in the literature. The first is that the concept is contested (Waldron, 2002). There are fundamental disagreements about what the rule of law means. The second is that empirical scholarship has largely taken place at the macro level, without serious consideration of underlying micro-mechanisms (McCubbins, Rodriguez, and Weingast, 2010). The first concern is far from problematic, at least for empirically oriented institutionalists. In addition to being irresolvable precisely because this is the nature of a contested concept, the contest is not undermining dialogue or intellectual progress, as evidenced by the authors’ own research. The second concern ever so slightly mischaracterizes the challenges and opportunities that rule-of-law scholars confront. The key implication emerging from the essays is not so much that we need more micro-level modeling. The key implication is that we need more modeling, of any kind.

The first three essays in the symposium reflect well the contested nature of the rule of law. They also remind us that empirical research can proceed without too much trouble even when scholars do not agree precisely about what the rule of law means. Møller and Skaaning highlight four salient elements of the rule of law. The first element, the “core” of the rule of law, or the shape of rules, deals with features of the law that make it possible for subjects to follow it (e.g., generality, prospectiveness, clarity, etc). The second element, what they refer to as the sanctions of law, or “control,” ensures that even the sovereign is constrained in some way by the law. The third element, the source of law, or “consent,” adds a minimally democratic concept to the rule of law. On this account, the source of law must be grounded in an electoral environment in which leaders compete for power. The final element is the “content” of law, which can be negative in the sense of the typical set of liberal rights and liberties (e.g.,
property, speech, a fair trial) or positive, in the sense of economic and social rights (e.g., housing, education).

With the elements summarized, Møller and Skaaning propose two rule-of-law concepts. They organize the elements into a meaningful hierarchical typology based on both classical rule-of-law thought and their own work on democratization. They then ask whether these hierarchies characterize well the way that elements of the rule of law are distributed across states around the world. Importantly, the hierarchies imply particular empirical configurations, which appear to follow from a theoretical argument about the process by which the rule of law is constructed. For example, their take on classical rule-of-law thought is that positive content follows consent, which follows negative content, which follows control, which follows the core. The alternative hierarchy implies that positive content follows control, which follows the core, which follows negative content, which follows consent. Empirically, they find that both hierarchies explain the clustering of state outcomes on measures of the separation of powers, judicial independence, freedom of expression, property rights, free and fair elections, and social rights better than a null model, in which elements are distributed randomly across states; however, the hierarchy derived from classical theory does not fit the data as well as their alternative.

Møller and Skaaning’s elements appear in the Carlin and Bergman papers, but the precise concepts at play are distinct. Carlin, adopting Shklar’s (1998) conception, adds to their list the requirement that citizens are free from fear of violence, i.e., the rule of law also implies an orderly society. He subtracts “consent,” so that the democratic element remains a distinct concept. Carlin does not attempt to construct a hierarchy of rule-of-law elements either. Instead, he simply envisions the rule of law as manifesting in three kinds of ways—in the way that arbitrary government is constrained, in the way that citizens are treated equally and fairly under the law, and in the way that citizens perceive their safety from violence. Likewise, Bergman creates no hierarchy and he subtracts the “consent” element, holding that the rule of law exists when “the vast majority of citizens accept to be ruled most of the time by binding and general norms that have a high probability of compliance” (Bergman, in this issue, p. 175).

So the authors do not precisely agree about the content of the concept or its structure. One paper envisions a hierarchy, two do not. One envisions democracy as a core component of the rule of law, two do not. All three papers differ in the extent to which they “thicken” the rule-of-law concept. That said, on the whole, the authors are very much talking about a large set of similar traits. In so far as they attempt to validate new measures by demonstrating a high correlation with existing measures derived from distinct concepts, the problem is admittedly alarming. But if the ultimate goal is

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1 To be fair, it is somewhat unclear if the hierarchies derive from a theory that suggests a temporal process. Perhaps the better way to describe their claim is that a state is simply no less likely to have control as positive content, no less likely to have negative content as control, no less likely to have the core as control, no less likely to have negative content as the core, and no less likely to have consent as negative content. The precise theoretical underpinning for this claim is provided elsewhere (Møller and Skaaning, 2011).
to say something about how elements of the rule of law might be constructed or how they emerge on their own, then the differences over particulars are not shutting down a productive exchange.

Møller and Skaaning's findings suggest a clustering of rule-of-law elements. States with strong protections for social and economic rights, for example, are also likely to protect property and constrain their governments. Carlin explicitly builds on this suggestion. Accepting that the rule of law is multidimensional, Carlin asks whether distinct elements cohere. The core contribution of the Carlin paper is the application of a cluster analytic model to the measurement of the rule law, a particularly appropriate technique given his goal. He finds that states can be partitioned fruitfully into five types of societies: 1) states that have all of the basic attributes of the rule of law under Shklar's definition, 2) states that are relatively peaceful but perform less well on the other dimensions relative to type 1, 3) states that are orderly but perform very badly on the other dimensions, 4) states that are perform poorly on all dimensions, but where violent conflict is relatively minor, and 5) states beset by complete disorder, typically associated with severe civil conflict. Having separated democracy from the rule of law, Carlin is now in a position to ask about the ways in which democracy relates to the clusters he uncovers. Unsurprisingly, full-rule-of-law states are also highly democratic.

But Carlin's finding reflects well a key insight of Møller and Skaaning—that "consent" generally precedes "control" and "content." Likewise, both scholars find that there are highly democratic states that nevertheless fail to protect rights, positive or negative. The point here is that despite a core difference of opinion about how to conceptualize the rule of law, empirical results across the two papers reinforce each other. They certainly do not answer why we see this covariation, but we cannot conclude that the conceptual contest is undermining either dialogue or intellectual progress.

The validity of Carlin's results depends, of course, on the indicators that he uses to reveal underlying rule-of-law traits. Bergman suggests that we need to pay closer attention to compliance with rules, and that we more validly measure compliance with behavioral indicators. And if we are going to use perceptions (as in cross-national expert surveys), we need to be careful about the perceptions we sample. Victimization surveys are likely more valid indicators of crime than are official reports. Household surveys that ask about experiences with corruption are likely more valid indicators of corruption than cross-national expert surveys. Clearly, these concerns hold just as strongly for Møller and Skaaning as they do for Carlin, or for any empirically oriented scholar. The point is really quite simple. There is nothing about the conceptual differences that prevents a productive conversation across all three papers about core measurement concerns. Have we measured negative content validly? You do not have to agree that democracy is part of the rule of law to answer that question.

The core challenges and opportunities that rule-of-law scholars confront are not exactly conceptual. This is not to say that we can afford to ignore careful conceptualization. We cannot. I just mean to say that our goal ought not to be developing and
then coordinating on a single, unifying rule-of-law concept. We can afford multiple concepts. What we cannot afford is an inattention to theory. The theoretical challenges begin with measurement. Nearly every element of the rule of law, when expressed in its purest form, is latent. Are leaders constrained from arbitrary governance? Do leaders really depend on citizen support? Does a state really respect free speech or provide fair trials? For this reason, we confront immediately the challenge of identifying likely manifestations of these concepts. Observing elections is not enough. We have to have a theory about what a fair election looks like to infer fairness from the elections that we observe. And of course, there is a wide array of statistical techniques applicable to latent concepts, each of which derives from a particular model about how latent concepts reveal themselves. The World Bank’s rule-of-law indicator derives from one such model. Carlin’s model reflects another. The challenge in each case is to use what we believe theoretically about the way elements of the rule of law manifest and change to simplify our inferences from the observable to the unobservable. Clearly, many scholars of the rule of law understand this point, as it is reflected in their approach to measurement, but we are far from solving every measurement-modeling challenge, and my hope is that rule-of-law research will continue to inform those challenges.

Theoretical challenges extend beyond measurement, of course. The papers in this symposium raise a number of intriguing questions. Consider Møller and Skaaning. They find that states perform at least as well on negative content than they do on control. What political process explains such a finding? Of course, even autocrats have good reasons to constrain their predatory instincts, and they may even be willing to allow freer political processes to render economic promises credible (e.g., Moustafa, 2007). That said, not every autocrat makes this choice. An important challenge here is to identify the conditions under which autocrats provide negative content. What do they risk by doing so?

Recall that the Møller and Skaaning findings imply that negative content is possible without an independent judiciary or a separation-of-powers system. They do not necessarily imply that negative content is possible without state constraint. Indeed, it strains the concept of negative content if it is possible for a leader to both be unconstrained from arbitrary choices and simultaneously constrained by the law to protect particular rights. Møller and Skaaning’s leaders who provide negative content must be constrained—they are just not constrained by separating the powers of government. One plausible explanation then is that elections or relevant publics themselves do the constraining. This is consistent with Bergman’s account of the rule of law, which derives Weingast (1997). In so far as citizens coordinate on a belief about appropriate state behavior, then perhaps mobilization via ballots or public protest might be sufficient. Carlin’s results deepen the issue, though. Plenty of democratic states fail to provide the rule of law. Thus, we are encouraged to ask why democratically accountable leaders often fail to provide elements of the rule of law. If the answer is, again, a failure to coordinate, then we have just pushed the question back one degree.
Weingast’s answer depends on a focal solution provided by constitutional moments. An alternative is democratic majorities are comfortable relaxing some elements of the rule of law (e.g., equal protection or due process) to enhance others (e.g., perceptions about social order). This might be a central concern in democracies characterized by social unrest or otherwise under threat. In the end, we are left with open questions about the development of the rule of law across regime types.

At the macro level we ask questions like “why do leaders provide the rule of law,” as if they could simply go out to the regime store, purchase some rule of law, and install it. I agree with Sarsfield and Carlin that macro-research can benefit from micro-level insights, which help put flesh on our macro claims about rule-of-law construction. Still, there is much to do at the micro level. For this reason, the Ríos-Figueroa and Sarsfield papers are welcome additions to the symposium. Both remind us that macro-level claims depend on individual-level incentives. If corruption depends on shared expectations about a wide array of connected activities, as is true in Sarsfield’s account of traffic policing in Mexico City, then reforms must target these expectations. And given the complexity of the problem, it may not be possible to engineer the change absent a massive shock to the system, one that might be politically infeasible. Importantly, Ríos-Figueroa reminds us that reforms designed to advance one purpose can undermine another. Specifically, increasing lower-court-judge independence from their superiors might pave the way for doctrinal experimentation and change (Hilbink, 2007), but it might do so at the expense of increasing incentives for corruption.

As persuasive as these papers are, I wish that the authors had gone further. Ríos-Figueroa seeks to understand how broad constitutional choices about the structure of the justice system might influence corruption. He focuses on the location of the public prosecutor’s office and the external and internal independence of the judiciary, finding that corruption is lowered when prosecutors are not directly dependent on the chief executive and when judges enjoy less internal independence. The empirical evidence is suggestive, but it is suggestive of a very weak effect. But what of the model that suggests that internal independence creates incentives for greater corruption? The idea is that the less lower-court judges are monitored and reviewed by their superiors, the stronger the incentives are for corrupt behavior. Yet suppose we have a judiciary with very low internal independence and a highly corrupt high court. Why would the high court’s monitoring and review authority not induce greater corruption in such a context? Might stronger ties between the high court and the lower courts in the judiciary enhance the incentives for high-court corruption, thus reinforcing the problem, especially when the members of the high court are particularly prone to corruption? Sarsfield stresses that, at the end of the day, bribes are offered and received by actual individuals. That insight could inform well the kind of theoretical argument Ríos-Figueroa develops. To my mind, it is not possible to fully separate a theory of institutions from a theory of behavior.

As Ríos-Figueroa himself has written, institutions can have very different effects in different contexts (Pozas-Loyo and Ríos-Figueroa, 2006). And the same institution
can create incentives for two completely different behaviors in the same context (Helmke and Staton, 2011). To know what to expect from an institutional intervention, we need to say something about what behavior we would see without the institution. I see a great imbalance between the amount of data we have on institutions allegedly connected to the rule of law and the amount of careful theory that should guide the way we interpret empirical findings. Ríos-Figueroa and Sarsfield's essays take us in the right direction. I believe the next step is to combine their core insights.

Before reading a single word of the essays, my sense was that despite the considerable ink that has been spilled over conceptualizing and measuring the rule of law and despite the considerable resources that have been devoted to promoting it globally (e.g., Carothers, 2006), there was still so much left to do, so many open questions to pursue, and puzzles to resolve. Collectively, the authors' research pushes us to "get on with it." If we do not like existing measures of the rule of law, if we think they miss important aspects or conflate distinct concepts, then propose a new measure, a new method if necessary. The rule of law may be a contested concept, but this is not to say that it is unredeemably vague—that we have can never meaningfully communicate about it. For an empirically oriented scholar, the conceptual contest merely suggests that there are reasonable conceptual alternatives, and that the choice ought to follow from particular theoretical goals or research challenges. By suggesting a variety of ways of conceptualizing and measuring the rule of law, the essays focus our attention on the challenge of modeling the rule of law and its effects. The productive path forward for institutional researchers does not lie in continued debate over the meaning of the rule of law. The productive way forward is theoretical. Quite obviously, to understand its emergence, its change and its effects on human welfare, we need a theoretical model. But this is no less true if our goals only involve questions of description. Precisely because so many features of so many conceptual variants of the rule of law are unobservable, even to measure it well, we need theory. In a number of ways, the papers already reflect the kind of theoretically focused research that we need.

REFERENCES


