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# CONSTITUTIONS

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## I. Two Types of Constitution; Two Strains of Research

Although constitutional scholarship has long been predominantly normative in character, that has certainly not been for a lack of interesting empirical questions. To name but a few: Why do some countries adopt entrenched formal constitutions while others do not? What are the practical consequences of adopting a formal constitution, or of choosing one set of political institutions over another? Is there a magic combination of adoption procedures and substantive provisions that renders formal constitution-making an effective instrument for achieving social, political,

[1] The author is grateful to the editors of this volume, and to Josh Fischman, Paul Gribble, Ran Hirschl, Anne Law, Miguel Schor, and Mila Versteeg, for suggestions that have improved this Chapter in a myriad of ways.

and economic goals? What influences the evolution of a country's de jure or de facto constitution? Do constitutions exhibit recurring patterns over space and time, and if so, what explains those patterns? Yet before these topics can even be broached, there are two broad and recurring questions with which those interested in the empirical study of constitutions must grapple. The first might initially appear to be definitional but in fact concerns the appropriate scope and content of the research agenda: what exactly are we studying when we study constitutions? The second is methodological: what methods are appropriate and feasible when it comes to the empirical study of constitutions?

Empirical research on constitutions addresses two different phenomena that both go by the name of "constitution." The distinction at issue has been expressed in a variety of ways, each of which has its own shadings and nuances, but the underlying divide is between de jure, written, codified, or formal constitutions ("large-c" constitutions), on the one hand, and de facto, unwritten, uncodified, or informal constitutions ("small-c" constitutions), on the other. A large-c constitution is a legal document, or set of legal documents, that (1) proclaims its own status as supreme or fundamental law, (2) purports to dictate the structure, contours, and powers of the state, and (3) may also be formally entrenched, in the sense of being harder to amend or repeal than other laws.

A small-c constitution, by contrast, consists of the body of rules, practices, and understandings, written or unwritten, that *actually* determines who holds what kind of power, under what conditions, and subject to what limits. Long before the first large-c constitutions came into being in the late eighteenth century, Aristotle used the term "constitution" in this small-c sense to describe the actual organization of a city-state. Small-c constitutions can include not only treaties and statutes that lack formal constitutional status, but also unwritten conventions that lack formal legal status of any kind (Dicey, 1915; Palmer, 2006). A small-c constitution is likely to depart from, and indeed may even contradict, its large-c counterpart in at least some respects. To use an extreme example, although Thailand's large-c constitution purports to specify the conditions under which the government can be removed from power or the constitution itself can be altered, it is tantamount to a small-c constitutional rule that both the government and the large-c constitution may be swept aside via a coup d'état, provided that the coup leaders obtain the blessing of the monarchy, refrain from harming civilians, and hold elections thereafter (Elkins et al., 2009: 190–1).

It is common for scholars to refer to "constitutions" without specifying which of the two phenomena they have in mind. This terminological confusion reflects not only a lack of conceptual clarity, but also a disciplinary divide. On the whole, social scientists have generally been more interested in actual as opposed to formal arrangements and thus have tended to focus on small-c constitutions, whereas legal scholars have been more inclined to equate the idea of a constitution with a formal legal document, and to pay greater heed to such documents. Failure to distinguish between the two types of constitution, however, begs analytical confusion. Because a jurisdiction's small-c and large-c constitutions are unlikely to coincide perfectly, they cannot be discussed interchangeably.

The literature on small-c constitutions is immense. This is because much of the literature in the fields of comparative politics and political economy can be characterized as empirical research on small-c constitutions, whether or not it explicitly labels itself as such. Common themes of this literature include the causes and consequences of different forms and structures of government, such as democracy as opposed to dictatorship, presidential as opposed to parliamentary regimes, and proportional as opposed to majoritarian electoral systems. Scholars have asked, for example, whether democracies are more likely to prosper (e.g., Przeworski and Limongi, 1993) or wage war (e.g., Reiter and Stam, 2002) than autocracies, and which type of democracy—presidential or parliamentary—is likely to last longer (e.g., Shugart and Carey, 1992) or lead to lower levels of government spending (e.g., Persson and Tabellini, 2005). Reflecting the fact that it is produced mostly by social scientists, this body of work frequently involves quantitative and statistical analysis.

The empirical literature on large-c constitutions, by contrast, is not nearly as extensive. To say that our knowledge does not match our aspirations in this domain would be an understatement. Large-c constitutionalism was born of the Enlightenment belief that the act of making reasoned, explicit commitments in written form would enable societies to establish better forms of government. Large-c constitutions have since become ubiquitous instruments of modern government: 90% of countries possess some document or collection of documents that advertises its own status as higher ("constitutional," or "fundamental," or "basic") law (Elkins et al., 2009: 49). In that same spirit of improving the technology of governance, scholars have sought to understand whether and how large-c constitutions can be designed and employed to achieve such fundamental and challenging goals as economic prosperity (e.g., Voigt, 1997), political stability (e.g., Lutz, 2006), respect for rights (e.g., Cross, 1999; Davenport, 1996; Keith, 2002; Lutz, 2006), lasting democracy (e.g., Elster, 1991), and peaceful coexistence among potentially warring factions of society (e.g., Choudhry, 2008; Horowitz, 2008; Lijphart, 2008). Efforts to devise large-c constitutional solutions to such challenges have focused on identifying, on the basis of a combination of theory and experience, the best answers to the interrelated questions of process and content: what should the content of the constitution be, and what is the process by which the content of the constitution should be determined?

For a variety of reasons, however, relatively little is known with confidence about how large-c constitutions can most effectively be used to achieve these goals or, indeed, whether they are effective at all. Much of the empirical work that does exist consists of case studies or small-n comparisons which, although valuable, have their limitations. Even at a purely descriptive level, surprisingly little was known until recently about the characteristics of the existing universe of large-c constitutions. There has been no convenient way of answering such basic questions as what proportion of the world's constitutions contain a right to education or executive term limits. The emergence of new data sets on the adoption and content of the world's written constitutions, at least some of which may be made available to the rest of the scholarly community in the near future, is now enabling quantitative researchers to tackle the full panoply of such questions (e.g., Elkins et al., 2009; Widner, 2008; Goderis and Versteeg, 2009). We now know, for example, what the average lifespan of a large-c constitution is (19 years), and what factors predict greater constitutional longevity (for example, the inclusion of a broad range of relevant actors in the constitution-making process, a relatively high degree of substantive specificity in the constitutional document itself, and amendment mechanisms that are neither too difficult nor too easy to invoke) (Elkins et al., 2009). An important next step will be the collection of thorough cross-national time-series data on how courts interpret large-c constitutions. Creation of a data set of such scope is a formidable undertaking but will make it possible to accurately describe and analyze the substance and development of constitutional law at a global level.

The growth of quantitative empiricism in the field of large-c constitutional scholarship comes not a moment too soon. Witness the question posed—and the answers offered—by a recently published symposium: "What, If Anything, Do We Know about Constitutional Design?" The answer appears to be, for the time being, not much (Levinson, 2009: 1265, 1271). The field remains a young one: as recently as 1995, it was ruefully observed that there existed "no body of literature that deals with the constitution-making process in a positive, explanatory perspective," and not "a single book or even article that considers the process of constitution-making . . . as a distinctive object of positive analysis" (Elster, 1995: 364). With respect to the specific problem of constitution-making for divided societies, there remains no "definitive or uniformly accepted answer" to the question of either process or content: we know neither what process of constitution-making is "most apt to produce the best configuration of institutions," nor what the "best configuration of institutions" happens to be (Horowitz, 2008: 1213). Likewise, although the judicial use of foreign and international legal materials in constitutional cases has prompted a good deal of qualitative empirical discussion (e.g., Choudhry, 2008; Law, 2005a), an accurate global picture of the scope and degree of large-c constitutional "borrowing," "migration," and "diffusion" awaits comprehensive quantitative analysis of a type that is only beginning to occur (Elkins et al., 2009: 24–6). What Ginsburg et al. (2009) say about the state of the empirical literature on constitution-making in particular might well be said of the empirical literature on large-c constitutions as a whole: "In general, scholars have been far better at generating hypotheses . . . than at testing them. Individual case studies have provided some insights, but large-n work has been hindered by a lack of data and by a need for conceptual refinement" (p. 219).

# II. Do Constitutions Matter?

Certain empirical questions about large-c constitutions have profound implications for much of the normative literature. First, to what extent and why do large-c and small-c constitutions diverge from one another? Second, what impact, if any, does a large-c constitution have on its small-c counterpart? There are many reasons why the two types of constitution may diverge. A large-c constitution may be partly or wholly an aspirational or expressive document, in which case we would expect at least some gap to exist between text and reality. It may be insincere, as in the case of an authoritarian regime that adopts an expansive bill of rights for ulterior motives that have more to do with, say, securing the acceptance of the international community (Meyer et al., 1997) or attracting foreign capital and skilled workers (Law, 2008) than protecting the freedom of its citizens. Or it may simply be ineffective: a constitution framed with sincere intentions and high hopes may fail to have much impact on actual practice.

The possibility that large-c constitutions may lack practical impact strikes at the heart of the literature on constitution-making and constitutional design. What is the point of debating, for example, what rights to include in a constitution if the inclusion of those rights has no practical consequence? Of what value is it to know that an authoritarian regime promises in a written constitutional document to respect judicial independence or personal freedom? How can it be meaningful to discuss how best to write a constitution that mitigates conflict in divided societies, if constitutions have no effect? Why is it helpful to know the factors that predict the endurance of large-c constitutions if large-c constitutions neither reflect nor shape actual practice?

Scholarship aimed at identifying the best ways to devise or write a large-c constitution embodies the assumption that *constitutions matter*: that is, large-c constitutions are not merely shaped by, but also shape, political, economic, and social life. If one assumes that large-c constitutions do in fact affect the prospects of achieving such mammoth and elusive goals as peace and prosperity, then the stakes involved in the empirical study of large-c constitutions are exceedingly high. It has been asserted, for example, that a "majority" of the world's constitutions not only purport to limit government power, but are also "reasonably or fully operative" in practice (Breslin, 2009: 27). Upon critical reflection, however, it may seem like something of a lawyer's dream to suppose that the mere drafting and adoption of a legal document that typically lacks any plausible prospect of third-party enforcement can nevertheless define the machinery of government and shape the welfare of a nation.

The evidence that large-c constitutionalism promotes human flourishing, for example, is hardly conclusive. As Hirschl observes, if one uses common measures of democracy and human development as the yardstick, the world's most successful nations do not appear to be characterized or distinguished by the presence of robust, "American-style written constitutionalism, active judicial review, or culturally engrained constitutional sanctity" (2009: 1357). The drivers of success would appear instead to include a combination of exogenous resource constraints (such as population size) and small-c constitutional factors (such as stable electoral processes, the existence of a large middle class, a healthy civil society, and a developed market economy characterized by high levels of public investment in science, education, and health care). By contrast, the impact of large-c constitutional variance would appear to be "[q]uite negligible" (p. 1360).

The empirical literature on the large-c constitutional protection of individual rights, in particular, is not very encouraging. A scholarly preoccupation with rights has defined the agenda of comparative constitutional law and spawned a vast literature on what rights large-c constitutions should contain and how they ought to be enforced (Choudhry, 2008). Much of this literature is normative in character and proceeds on the assumption that the inclusion of particular rights in large-c constitutions has an impact on actual government behavior. Considerably less attention has been given, however, to the question of whether this assumption is empirically justified. The existence of large-c constitutional rights is obviously neither a necessary nor sufficient condition for the observance of such rights in practice; compare, for example, the United Kingdom, which has long respected a variety of rights in the absence of a large-c constitution, with the former Soviet Union, which guaranteed a variety of rights that were routinely abused. Even the architects of the world's most enduring and best-known bill of rights appear to have held out only modest hope for such "parchment barriers" (Madison, 1789); the framers of the United States Constitution allowed merely that that the inclusion of specific rights provisions would not be "altogether useless" (Hamilton, 1789).

Scholars have struggled mightily, but with decidedly mixed results, to conclude that the existence of written rights guarantees leads to greater respect for rights in practice. The quantitative literature that does exist paints, on the whole, a discouraging picture of the efficacy of such provisions (e.g., Blasi and Cingranelli, 1996; Cross, 1999; Davenport, 1996; Hirschl, 2008; Keith, 2002; Keith et al., 2009; Pritchard, 1986).[2] Davenport (1996), for example, reports that the existence of constitutional provisions governing freedom of the press, martial law, and the declaration of states of emergency is associated with lower levels of political repression, but guarantees of freedom of speech (as opposed to freedom of the press) and of the freedom to unionize and strike lack such impact. Nevertheless, he arrives at the conclusion that "constitutions do matter" (p. 648). Likewise, a study by Cross

[2] A parallel debate over the actual impact of written rights guarantees can be found in the literature on human rights treaties (e.g., Hafner-Burton and Tsutsui, 2007; Simmons, 2009).

(1999) of the determinants of government behavior in the specific area of search and seizure finds that, by itself, the presence of an express constitutional prohibition against unreasonable search and seizure has no significant impact on actual practice.[3] Neither, for that matter, do such "institutional variables" as federalism and separation of powers. Yet he emphasizes in conclusion that "certain legal variables are important determinants of human rights protection" (p. 97).

Indeed, a number of studies have found a negative relationship between formal rights protection and actual rights observance (Boli-Bennett, 1976; Keith, 2002; Pritchard, 1986). For example, Keith (2002) evaluates the impact of various formal constitutional provisions on the incidence of severe rights violations such as torture, political imprisonment, kidnapping, and murder. Controlling for such variables as the extent to which the country in question is democratic, she finds that formal constitutional guarantees of freedom of speech, religion, association, and assembly are not significant predictors of respect for rights. Rather, constitutional guarantees of freedom of the press and habeas corpus are actually correlated to a statistically significant degree with a *higher* incidence of severe rights abuse. And in an especially ironic twist, constitutional bans on torture are associated with a higher incidence of torture. The only provisions that her analysis suggests are associated in a statistically significant way with increased respect for rights are due process guarantees of a fair and public trial (2002: 128). Although Keith rejects her own findings as to the negative impact of certain constitutional guarantees on actual respect for rights on the grounds that they point in the "wrong direction" and lack "theoretical justification" (pp. 127, 134), a subsequent analysis by Keith et al. (2009) incorporating a host of additional variables merely confirms these supposedly implausible results.[4]

Does this sort of evidence regarding the impact of written rights guarantees warrant skepticism about the efficacy of large-c constitutions more generally? It might be argued that rights guarantees are more prone to failure than other types of large-c constitutional provisions. The success of such guarantees might be dependent upon somewhat demanding institutional and environmental conditions, such



[3] Cross's findings suggest that the presence of an express constitutional prohibition against unreasonable search and seizure may have an impact on actual practice, but only in countries characterized by a lack of judicial independence: he notes that higher levels of judicial independence are associated with greater levels of actual freedom from unreasonable search and seizure, but this relationship emerges only in the absence of a constitutional provision.

[4] As the authors of the 2009 study acknowledge, their decision to use one-tailed statistical tests—which embody the assumption that written constitutional protections can only have the effect of *decreasing* rights abuse—ensured, as a mathematical matter, that any findings contrary to this assumption would not be statistically significant (p. 657). Had they used substantively agnostic two-tailed tests, as Keith (2002) did, they might have reported precisely the same result as the earlier study—namely, that constitutional provisions on torture, habeas corpus, and freedom of speech have a negative *and* statistically significant impact on respect for rights.

as the existence of judicial review by independent courts (Goderis and Versteeg, 2009; La Porta et al., 2004) and a legal profession organized in a manner that encourages and sustains rights advocacy (Epp, 1998). Another problem is that such guarantees are not always sincere. For various reasons, it has become de rigueur for even the most tyrannical of regimes to recite in their large-c constitutions a litany of constitutional rights sufficient to please the most ardent idealist (Alston, 1999).

By contrast, government actors may have both less incentive and less ability to ignore constitutional provisions pertaining to institutional design than individual rights. A would-be dictator may stand to gain much more by violating the rights of political dissidents, for example, than by converting a presidential system into a parliamentary one, or vice versa. He or she may also find it much easier to persecute dissidents than to abolish a rival government institution, such as the legislature, that is likely to possess greater ability to fend for itself than a handful of malcontents. There are also many reasons, however, why actual institutional arrangements might not correspond with formal constitutional provisions. To name just a few, the resources to create the constitutionally ordained institutions may be lacking, or the officials responsible for implementing them may lack the desire to do so (Blasi and Cingranelli, 1996: 228), or the arrangement may prove so unpopular that it persists only in form and not in practice (such as the legislative override provision of the Canadian Charter of Rights and Freedoms, or the U.S. Constitution's allocation of responsibility for selecting the President to the Electoral College).

It is not possible to draw any firm conclusions from the scant existing literature about the relative efficacy of other types of large-c constitutional provisions. Elkins et al. (2009) suggest that, as a general matter, "the functioning of important political institutions is described fairly accurately in constitutions, but the extent to which rights provisions are implemented in practice varies dramatically across countries, with some countries promising more than they deliver and others delivering more than they promise" (p. 55). In support of this assessment, they cite statistical evidence showing the existence of a much closer relationship between constitutional text and actual practice when it comes to the scope of legislative power than when individual rights are at stake. In a similar vein, Blasi and Cingranelli (1996) find that constitutional guarantees of federalism correlate strongly with actual government decentralization. Even the relatively modest claim that large-c constitutions tend to reflect the actual operation of a country's political institutions, however, may still be too broad. Consider, for example, the power and independence of the judiciary. Two-thirds of the world's large-c constitutions contain some form of explicit protection for judicial independence, and that proportion has only been rising over time (Constitutional Design Group, 2008). Yet the existence of formal constitutional guarantees of judicial independence appears to be poorly correlated with actual respect for judicial independence in practice: once one controls for such factors as wealth and education, de jure judicial independence ceases to be a meaningful predictor of de facto judicial independence (Feld and Voigt, 2006: 267).[5]

None of this is to suggest that large-c constitutions never succeed. The problem is, rather, that that we know little about the conditions under which large-c constitutionalism succeeds, in the sense of either defining actual practice or improving social welfare. It is not simply the case, for example, that the success of large-c constitutionalism is limited to long-established democracies or to certain geographic regions, as we know from countries such as South Africa and Taiwan. The most that can be said with confidence is that there is a continuing need for a "complete evaluation of the relationship between formal constitutional provisions and constitutional practice" (Elkins et al., 2009: 55). The challenge is to identify which factors determine large-c constitutional success, to what extent, and in what combination—and, ultimately, to do so in a way that holds true not simply for a handful of countries that have received close scrutiny, but on a global basis as well.



## III. Judicialization and Constitutionalization

There is one area in which scholars have made significant progress at understanding and explaining how and why large-c constitutions become effective. The growing body of work on the judicialization and constitutionalization of politics is a highlight of the empirical literature on constitutions more generally. Although the uses and definitions of the terms "judicialization" and "constitutionalization" sometimes overlap, "judicialization" has mainly been used to refer to (1) the expansion of the policy-making role of courts, at the expense of other actors and institutions; and (2) "the spread of judicial decision-making methods outside the judicial province proper," such as the use of adjudication as a policy-making mechanism and the deployment of legal concepts and arguments by non-judicial actors (Vallinder, 1995: 13). "Constitutionalization," in turn, refers to the process by which a body of formal law becomes an effective source of limits on state power and government actors. The legal regime at issue need not be, and often is not, explicitly or formally constitutional in character: one can speak, for example, of

[5] An earlier study by Blasi and Cingranelli (1996) of a smaller cross-section of countries over a single year found a positive correlation between de jure and de facto judicial independence but did not control for any other variables.

the constitutionalization of international law (Dunoff and Trachtman, 2009) or the European Union treaty regime (Stone Sweet, 2004), even in the absence of any legal instrument that is formally denominated as constitutional.[6] Where a large-c constitution does exist, however, the term can logically be used to describe the process by which the large-c constitution becomes effective as a practical matter or, in other words, becomes the small-c constitution.

Judicialization and constitutionalization are heavily symbiotic. In order for judges to decide a policy question, that question must be characterized as a legal question. But if judicial decision-making is to trump or displace policy-making by other actors, it helps if the question to be decided is not merely legal, but constitutional in character. Perhaps the most obvious and important way in which courts claim and exercise exclusive authority over policy questions is by acquiring and exercising the power of judicial review. As Stone Sweet puts it, "the judiciary's share of total governmental authority and influence varies with the degree to which it possesses and exercises the power to review the lawfulness of activity, public and private" (1999: 163). Large-c constitutional enforcement is the primary vehicle for the expansion of judicial power, but the expansion of judicial power, in turn, makes the large-c constitution more efficacious. That is, judicialization and constitutionalization form a virtuous circle: by enforcing constitutions, judges acquire power; by exercising their power, judges give effect to constitutions. To understand how and why judicialization occurs, therefore, is also to understand one way in which large-c constitutions become effective.

On the subject of judicialization, there is widespread agreement on two points. The first point is that judicialization is occurring around the world with increasing frequency and intensity, which bodes well for the ability of large-c constitutions to gain traction (e.g., Hirschl, 2004; Shapiro and Stone Sweet, 2002). The second point, which commands even stronger agreement, is that judicialization often occurs with the acquiescence or encouragement of powerful political actors, who frequently have self-serving reasons to refrain from making policy themselves, to empower courts to make policy instead, and to avail themselves of judicial fora (e.g., Ginsburg, 2003; Graber, 1993; Hirschl, 2004; Stone Sweet, 1999, 2004; Whittington, 2007). The "mantra" of this literature, as Graber puts it, is that "judicial review is politically constructed": "Elected officials provide vital political foundations for judicial power by creating constitutional courts, vesting those courts with jurisdiction over constitutional questions, staffing those courts with judges prone to

[6] A proposed "Constitutional Treaty" would have explicitly repackaged and redenominated the EU's treaty structure as "constitutional" in name as well as substance. The rejection of that treaty by French and Dutch voters did little to halt the constitutionalization of EU law, however, as the vast majority of its contents were simply transferred over to a more innocuous-sounding "Reform Treaty," better known as the Treaty of Lisbon, which did secure ratification. The result is a body of law that operates increasingly as a de facto or small-c European constitution but may owe its growing reach in part to the fact that it disclaims any large-c constitutional status.

exercising judicial power, assisting or initiating litigation aimed at having those courts declare laws unconstitutional, and passing legislation that encourages justices to make public policy in the guise of statutory or constitutional interpretation" (Graber, 2005: 427–8, 446).

This insight has profound implications that normative constitutional theory has yet to fully absorb. Combined with a substantial body of research documenting a relatively close relationship between public opinion and judicial behavior (e.g., Peretti, 2005), it is part of a multipronged empirical assault on the core premise of normative constitutional theory that judicial review of the constitutionality of the acts of elected officials poses a "counter-majoritarian dilemma" (Bickel, 1986). Constitutional theorists have sought for decades to reconcile the supposedly counter-majoritarian nature of judicial review with the idea of democracy by prescribing conditions for the legitimate exercise of judicial review. It becomes harder to argue, however, that judicial review lacks majoritarian legitimacy, or that restrictions upon the policy-making domain of the courts can solve the counter-majoritarian dilemma, if the elected officials who supposedly represent the will of the majority are the ones responsible for empowering the supposedly counter-majoritarian courts.

Scholars have identified a variety of reasons for which other political actors might permit or even encourage courts to give effect to large-c constitutional rules. Elected officials may be anxious to "defer" to the judiciary on controversial issues that have the potential to split existing political coalitions, and to "avoid responsibility" for "tough decisions" that are likely to expose them to substantial criticism, no matter what position they take: on this view, the U.S. Supreme Court's constitutional decisions on such politically sensitive topics as slavery and abortion are more accurately characterized as "non-majoritarian" than "counter-majoritarian" (Graber, 1993: 37). Stalemate among other political actors can create both an opportunity and a need for courts to fill what might otherwise become a policy vacuum (e.g., Ginsburg, 2010). A governing party or set of elites that fears or anticipates that it may soon lose power may seek to blunt the impact of a change in control and protect itself from radical changes by an opposition government by entrenching certain policies in constitutional form and empowering the courts to enforce them (Ginsburg, 2003; Hirschl, 2004). Judicial enforcement of constitutional rules can help governments to achieve certain otherwise elusive goals by making credible commitments: effective constitutional protection against uncompensated takings, for example, may enhance the credibility of a government's promises to repay its debts and thereby lower the cost of sovereign borrowing (North and Weingast, 1989; Law, 2005b). The ability of courts to successfully generate constitutionalization is likely also to depend upon institutional features of the judicial and legal system, such as a docket that affords courts adequate opportunity to make policy, "a minimally robust conception of precedent" (Keller and Stone Sweet, 2008: 8; Stone Sweet, 2004: 35), and a "support structure" consisting of the human and financial resources necessary for sustained constitutional litigation (Epp, 1998: 3, 5).

## IV. Methodological Challenges

The methodological challenges that characterize the study of constitutions are endemic to social science as a whole, but some happen to be especially pronounced in this context. The two overarching obstacles to empirical research on constitutions in general, and to reliable causal inference in particular, are inadequate data and causal complexity.

(1) *Data inadequacy*. Empirical data on constitutions are prone to inadequacy in both quantity and quality. With respect to quantity, the number of cases available for meaningful comparison and analysis may be quite low depending upon the research question. Many of the phenomena that interest scholars, such as constitution-making, are relatively rare events. There are fewer than 200 countries currently in existence, and the total number of large-c constitutions that have ever existed—the vast majority of which are no longer in effect—falls shy of 1,000 (Ginsburg et al., 2009: 6, 226). Thus, even if one wishes to focus upon a relatively broad phenomenon, such as constitution-making in new democracies over the last century, the slice of data available for analysis may not be especially large. Even less data will be available on more specific phenomena, such as constitution-making in the United States at the federal level (one observation) or amendment of the Japanese constitution (zero observations). The quality of the data that scholars can hope to employ, meanwhile, is constrained by the sheer difficulty of measuring constitutional phenomena. Many of the phenomena that are of greatest interest, such as judicial independence (Feld and Voigt, 2006; La Porta et al., 2004) or respect for human rights (Hafner-Burton and Ron, 2009), also happen to be multifaceted, ill-defined, hard to quantify, or costly to measure.

(2) *Causal complexity*. Constitutions are complex phenomena with a host of potential causes and effects that can interact or conflict with one another and evolve over time in ways that are difficult to predict. It is a daunting task to identify all of the variables that are relevant to, say, respect for human rights, much less to determine what importance to assign to each of them. The underlying causal mechanisms and chains of causation are also difficult to parse: even if a correlation between two variables reflects a causal relationship, that relationship itself may be attenuated or conditional upon other factors that may be difficult to identify without in-depth examination. And as scholars have long recognized, the problems of causal

complexity and inadequate data merely aggravate one another. Even if one somehow manages to identify and parse all relevant casual factors, a lack of data makes it difficult to isolate the impact of any particular factor: one cannot control for an abundance of factors when there are only a few cases to analyze—or, in the context of qualitative research, perhaps just a single case.

Path dependence also poses complications for empirical scholars interested in questions of causation. Certain phenomena, such as those surrounding the development of political institutions, can in a sense be described as causing themselves: they involve self-reinforcing dynamics and generate positive feedback loops (Pierson, 2004). Path dependence may help to explain why, for example, one of the best predictors of future constitutional longevity is past constitutional longevity: controlling for a host of other variables, Elkins et al. (2009) find that the longer a constitution has already lasted, the longer it is likely to endure into the future. It is not easy, however, to model such processes and dynamics using traditional statistical tools.

Another vexing problem for quantitative researchers in particular is that of bidirectional causation, or endogeneity, wherein the independent variable is both a cause and a consequence of the dependent variable. Large-c constitutions may be intended to structure political, social, and economic arrangements, but they are also the products of the very arrangements that they are supposed to shape. Even if they shape their environment, the environment is likely also to shape them, with the result that it becomes very difficult to say which is influencing which, or to what extent. Endogeneity is of particular concern for quantitative scholarship because the regression models on which such work generally relies assume unidirectional causation. Although techniques for dealing with endogeneity do exist, those techniques are not always feasible and can require considerable ingenuity to execute.[7]

To illustrate some of the challenges surrounding causal inference in the face of complexity, consider the finding of Keith et al. (2009) that the existence of constitutional guarantees of a fair and public trial is correlated with a lower incidence of repression. As the authors observe, it is effectively impossible to know what causal inference to draw from such a finding: "We cannot infer for certain that it is the adoption of the constitutional provisions that causes the reduction in repression. Perhaps governments adopt constitutional protections when they are already predisposed to enforce them and not to abuse personal integrity. Since we (and everyone else, so far as we are aware) lack indicators of governmental predispositions, we are not, ultimately, in a position to resolve this causal conundrum" (p. 646). Likewise, even if we know that the existence of a formal right to a fair and public trial causes less repression, it would remain necessary to explain why the existence of

[7] One such technique is to identify a source of variation in the explanatory variable that is unaffected by the dependent variable, and to substitute this instrumental variable for the original explanatory variable (King et al., 1994: 187–95).

such a guarantee has such an effect, and to identify what conditions must be satisfied in order for the effect to occur. It might be that the effect of the right is conditional upon the existence of a competent criminal defense bar or the capacity of ordinary courts to exercise jurisdiction over politically sensitive cases, to name just a couple of possibilities.

Neither qualitative nor quantitative research, by itself, is likely to meet all of these challenges. The debate among methodologists over the relative merits of qualitative and quantitative approaches is a lively one (e.g., George and Bennett, 2005; King et al., 1994). It seems safe to say, however, that a combination of approaches—quantitative and qualitative, established and innovative—will be necessary if scholars are to gain traction on the many difficult empirical questions surrounding constitutions (large-c and small-c alike), constitutionalization, and constitutionalism. Qualitative methods, such as the case study approach (wherein the researcher explores a single instance of a phenomenon in depth or compares a small number of such instances), are widely thought to possess advantages when it comes to building theories and developing explanations of empirical relationships, whereas quantitative methods, such as regression models, excel at identifying and verifying the existence of empirical relationships, and at assigning the appropriate weight or significance to different factors. Understood in this way, the two approaches are clearly complementary rather than substitutes for one another.

Given the nature and severity of the methodological challenges that characterize the study of constitutions—not to mention the paucity of quantitative skills among constitutional law scholars—it is perhaps not surprising that case studies have thus far dominated the fields of comparative constitutional law and politics. This state of the literature, however, is not entirely healthy. First, whatever the full potential of the case study method may be, that potential remains largely unrealized by legal scholars. Although there are, of course, examples of highly systematic work in this vein,[8] the manner in which comparative constitutional scholars have tended to conduct case studies has been faulted for its inattention to "basic methodological principles of controlled comparison, research design, and case selection" that would permit meaningful causal inference (Hirschl, 2008: 39).

Second, other types of empirical scholarship are needed to compensate for the inherent limitations of small-n qualitative research. Even if one selects cases, constructs theories, and devises empirically testable hypotheses with the utmost care and ingenuity, it remains intrinsically difficult to draw reliable causal inferences solely on the basis of qualitative research involving a small number of cases (King et al., 1994). Thus, absent the development of a more extensive body of quantitative

[8] A conspicuous example is the collection of "structured-focused comparisons" edited by Keller and Stone Sweet (2008) on the subject of the constitutionalization of the European Convention of Human Rights: the volume consists of a series of comparisons of different pairs of European countries, wherein each comparison is "structured" and "focused" in such a way that it can itself be compared with the other comparisons in the volume.

research, empirical constitutional scholars are likely to continue to prove "better at generating hypotheses . . . than at testing them" (Ginsburg et al., 2009: 219). There is also some risk that overreliance on qualitative research can yield a systematically biased understanding of the world. Hafner-Burton and Ron (2009) note, for example, that case studies have generally painted a rosier picture of the efficacy of international human rights instruments than statistical studies. It is not difficult to see how case studies conducted by constitutional scholars regarding, say, the impact of large-c constitutions or written rights guarantees might exhibit a similar tendency. Qualitative methods entail a plethora of subjective judgments that lack transparency and "are inherently imprecise and subject to unconscious biases" (King et al., 1994: 152), and if there is any unconscious bias that constitutional scholars might be expected to share, it is that constitutions matter. This is not to say that legal scholars ought to forsake qualitative methods. The point is, rather, that the cause of empirical constitutional scholarship is better served by methodological pluralism than by disproportionate reliance upon a specific methodology.



## V. Methodological Possibilities: Can Constitutions Be Computer-Simulated?

Tackling the problems of data inadequacy and causal complexity calls for scholars not only to combine familiar techniques, but also to pursue creative, unorthodox new approaches. There are two approaches in particular that are widely employed in other contexts and hold considerable promise in theory but have thus far seen little actual use in the literature on constitutions. The first is to conduct experiments. Not only do experiments generate original data, but the resulting data and findings are of a quality that one rarely encounters in social science research; the random assignment of subjects to control and experimental groups, in particular, ensures that any systematic differences in outcomes between the two groups of subjects are the result of the experimental treatment and not of other confounding variables. In some cases, it may be possible to substitute laboratory experiments for real-world experiments: one could, for example, observe the types of constitutional arrangements that human subjects in a laboratory setting actually reach under different procedural rules (Voigt, 1997: 20–2). Unfortunately, the prospects for genuine constitutional experimentation on a random sample of actual countries are effectively non-existent.

The second approach is to employ simulations. Improvements in computing power have expanded both the range of what can be accomplished using simulation-based techniques and the availability of such techniques to the research community. For example, Bayesian Markov Chain Monte Carlo methods that use simulation have become an accepted part of the social science toolkit for analyzing data that is too porous for conventional regression analysis (Jackman, 2000). It is neither possible nor necessary to explain these highly technical methods here. Suffice it to say, instead, that computer simulation techniques are already more ubiquitous in empirical scholarship than may generally be realized, and that software advances render it increasingly unnecessary to possess a technical understanding of such methods in order to make use of them.

A more ambitious form of simulation that may hold particular promise for the study of constitutional phenomena is agent-based modeling (ABM). An agent-based model consists of a simulated environment in which virtual actors, or "agents," are programmed to interact with and respond to one another. The fruits of their interaction are cumulative: the results of one round of interaction become the point of departure for the next round. Computational simulation of these interactions enables the modeler to observe not only the eventual outcome after many iterations, but also the dynamics that produce the outcome.

The strengths of ABM align nicely with a number of the challenges involved in studying constitutions. Constitutions are challenging phenomena to model because, inter alia, they generate, and are generated by, complex and recurring interactions among large numbers of actors who do not necessarily make fully rational decisions, and whose choices both reflect and generate path dependence. One advantage of ABM is that it can cope with a much greater degree of complexity than the usual tools of formal modeling: whereas the outcome of a game with a large number of players can be prohibitively difficult to deduce mathematically, ABM employs raw computing power to simulate the entire game. Another advantage is that, unlike a typical game-theoretic model, an agent-based model need not assume that actors make decisions in a highly rational manner by assigning payoffs and probabilities to possible outcomes, then engaging in backwards induction so as to maximize the expected payoff. Instead, agents can be programmed to react to one another in ways that reflect limited cognitive capacity and carry the potential for unwanted consequences (De Marchi and Page, 2008). Finally, ABM is especially well suited to modeling systems that exhibit path dependence and other "emergent properties, that is, properties arising from the interactions of the agents that cannot be deduced simply by aggregating the properties of the agents. When the interaction of the agents is contingent on past experience, and especially when the agents continually adapt to that experience, … ABM might be the only practical method of analysis" (Axelrod and Tesfatsion, 2006: 1649).

Consider two relatively simple examples of how ABM might be used to shed light on questions of interest to constitutional scholars. One such question is that of constitutional convergence: are countries adopting increasingly similar rules and practices in the area of constitutional rights? If the focus is on small-c constitutions, an obvious approach would be to compile quantitative measures of actual rights observance across countries and determine whether there is an aggregate trend or the variance between countries is decreasing over time (Law, 2008). If the focus is instead on large-c constitutions, a conventional quantitative approach might be to compute a measure of textual similarity among constitutions over time (Elkins et al., 2009: 24–5). But one might also use ABM to explore the conditions under which convergence occurs.

Axelrod's (1997) agent-based model of *cultural* convergence, for example, could be treated as a template for modeling *constitutional* convergence. The premise of the model is simple: the more similar that two countries already happen to be, the more likely that they will interact with and thus influence one another. The model takes the form of a virtual world of countries that are evenly distributed over a square grid. Each country's culture/constitution comprises five attributes, each of which can assume up to ten different values, with the result that a culture/constitution "can be described as a list of five digits, such as 8, 7, 2, 5, and 4" (p. 208). At the outset, each country's culture/constitution is a series of random digits. The simulation begins with the random selection of a country and one of its neighbors for "interaction." The likelihood that one country will influence the other depends upon how "compatible" their cultures/constitutions already happen to be. To be specific, the probability of influence is equal to the proportion of attributes that they already share in common: for example, a country with a culture/constitution of (8, 2, 2, 2, 2) has a 20% chance of influencing a neighbor that possesses a culture/constitution of (8, 3, 3, 3, 3) because the two countries already share 20% of their attributes in common. If influence occurs, then one of the second country's remaining unique attributes changes to match that of the first country: in this case, the second agent's culture/constitution (8, 3, 3, 3, 3) might become (8, 3, 3, 3, 2).

If this two-step process is allowed to repeat itself, the eventual result is a very high degree of convergence. Assuming an initial population of 100 unique cultures/constitutions, each of which can vary 10,000 ways, the model tends to stabilize at a total of three cultural/constitutional paradigms, one of which dwarfs the others. Indeed, 14% of the time that the simulation was conducted, a single paradigm ultimately dominated the entire world, whereas only 10% of the time did the simulation produce six or more stable paradigms. Meanwhile, the few paradigms that do survive are completely polarized, in the sense that they share nothing in common: convergence ceases to occur because the simulation reaches a point at which "every pair of neighboring sites has cultures [or constitutions] that are either identical or completely different" (p. 211).

This model, which is simple even by the standards of ABM, does not purport to offer a highly accurate depiction of actual interaction among countries. It does, however, illustrate the characteristics and potential of ABM as a tool for studying constitutional phenomena. Like formal modeling, ABM is used to model interaction among competing actors, and to test in rigorous and unambiguous terms the logical consequences of a certain set of assumptions about how those actors behave. By capturing the effects of path dependence, however, even the simplest of models can shed new light on how the operation of a single dynamic can, over time, generate nonintuitive outcomes. In this case, an elementary model of convergence illustrates (1) how little influence countries need have over one another in order for a high degree of convergence to occur, (2) how fragile diversity can be in the face of even mild tendencies toward convergence, and (3) how convergence can culminate in extreme polarization with potentially grim practical implications, as in the form of a conflict between, say, a secular liberal democratic bloc and a religious fundamentalist bloc that share no common ground.

In a more elaborate example of ABM, Kollman et al. (1997) tackle a pair of questions that ought to be of considerable interest to constitutional design scholars: what type of democratic institution encourages the adoption of welfare-maximizing government policies, and what is the impact of federalism on the capacity of different types of institutions to maximize citizen welfare? The agents in this model are citizens and governments. The model assumes that citizens have randomly assigned preferences over a particular set of policies and can move freely between jurisdictions, and that each jurisdiction has a particular institutional mechanism for aggregating citizen preferences and turning them into policy. Once policies in a jurisdiction are adjusted to reflect the preferences of its current residents, everyone is once again given a chance to move to a new jurisdiction. These interactions are then simulated repeatedly in order to evaluate the ability of three types of institutions to maximize utility per capita: "democratic referenda," meaning simple majority rule on an issue-by-issue basis; "direct competition," meaning "winner-take-all plurality voting among parties advocating different platforms"; and proportional representation, under which parties receive legislative seats in proportion to their share of their popular vote (pp. 980–1). The authors find that the ideal choice of institution depends upon, inter alia, the ability of citizens to move between jurisdictions offering different policies. In a world where citizens cannot move, government by referendum maximizes aggregate utility, followed by two-party electoral competition (either in the form of a winner-take-all or proportional representation system) and, lastly, proportional representation systems with more than two parties. If preferences are heterogeneous and citizens can move, however, the ranking of the various institutions is entirely reversed: proportional representation is best, whereas rule by referendum is worst. This reversal occurs because policy-making in jurisdictions with proportional representation is highly responsive to minor shifts in the preferences of the citizenry, which occur frequently when migration is possible.

As intriguing as computer simulation and laboratory experimentation may be, such approaches beg the question of whether one can generalize from findings generated in a simplified, hypothetical world to the vastly more complex operation of the real world. It may seem highly unrealistic that the workings of a country or constitution could ever be simulated with sufficient accuracy, either on a computer or in a laboratory setting, to permit any confidence in the results. Yet it would be a mistake not to pursue such approaches. First, a simulation need not capture the entirety of a phenomenon with complete accuracy in order to be of value to researchers. Much can be learned by devising and testing deliberately simplified models that yield clear predictions about the impact of specific dynamics. It may also be possible to break down complex phenomena into component parts that can individually be modeled with greater accuracy.

Second, the relevance and utility of computer simulations are certain only to improve with time. As computing power continues to advance, the accuracy and sophistication of agent-based models and substantive other forms of simulation will increasingly be limited only by the creativity and expertise of those who design them. Of ABM in particular, it might be doubted whether pre-programmed agents can adequately simulate actual human decision-making. Yet there is an obvious solution—namely, to combine the best aspects of computer simulation and live experimentation by incorporating actual human decision-makers into the simulations. Scholars in other fields, such as experimental economics, are already evaluating the use of social and economic behavior in virtual reality environments as a research proxy for real-world behavior (e.g., Friedman et al., 2007). The existence of virtual reality environments and video games in which people can lead entire lives through alter egos ("Second Life," "The Sims") and govern major cities ("SimCity") suggests that plausible simulations of constitution-making and constitutional politics are within reach. And the popularity of such programs demonstrates that human subjects would gladly volunteer, if not pay, to play assigned roles in a "SimGovernment" or "SimConstitution" simulation against one another online. If multiplayer computer tournaments can be used to test strategies for playing the Prisoner's Dilemma (Axelrod, 1984), surely they can also be used to evaluate, for example, the consequences of different strategies for making constitutions.

Third, and perhaps most importantly, it makes little sense to rule out novel approaches in the absence of any demonstrably superior alternative. Skeptics might fairly question whether the design of political and legal institutions and, by extension, the fate of entire nations should be entrusted to emerging technologies that resemble outsized video games. Yet the reality is that we already entrust matters of life and death to virtual reality. Scholars with an interest in constitutions are by no means the only ones who must study rare events of enormous consequence that are too costly or unthinkable to rehearse in real life. When it comes to evaluating the battlefield use of tactical nuclear weapons, for example, or the mid-air disintegration of fully loaded passenger jets, computer simulations are not so much a matter of choice as of necessity. It is unclear why the same tools cannot or should not be applied to the machinery of government. To reject computer simulation or laboratory experimentation on the ground that they may not solve every methodological challenge at one stroke or improve in every respect on existing tools is to allow the perfect to become the enemy of the good.

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