Comparing the Arab Uprisings: On Comparative Politics

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Abstract

How have scholars working in the political science subfield of comparative politics approached the Arab uprisings in their analyses? Two dominant trends have been to explore the uprisings through the literatures on robust authoritarianism and on social movements. While each of these has produced rich and lively debates, scholars of Middle East politics have mostly drawn comparisons at the national level: for example, explaining variation between those state that experienced uprisings and those that did not, or between those uprisings that turned violent and those that did not. I suggest that adopting “states” and “movements” as objects of analysis can obscure some of the more unique dynamics of the uprisings—dynamics that might be leveraged in contributing new ideas to broader theoretical debates. I illustrate the ways in which research designs that focus on identifying and explaining variation between and across cases tend to assume discrete objects of study (i.e., regimes and movements) in ways that obscure other fascinating processes and practices at both the micro-level and in terms of the complex interconnections across states and regions. This focus has in turn led to a deficit in studies of in-case variation: how mobilization and state repression varied, for example, between Cairo, Alexandria, the Suez, rural regions, and other locations outside of Tahrir Square. Finally, I applaud and encourage the continuation of the lively and open debates within the field about the strengths and weaknesses of our earlier scholarship and the potential of various future research agendas.

Keywords

Arab uprisings, authoritarianism, contentious politics, revolution, methodology, comparative politics, research design, variation

As spectacular political events do, the Arab uprisings have challenged and inspired social scientists in a variety of ways. In the months and now years since the outbreak of protests in Tunisia in 2010, tens of books, dozens of articles, and hundreds of papers and blog posts penned by scholars of the Middle East and North Africa have put the uprisings at the center of analyses of the region. Just as the collapse of the Soviet Union and its ripple effects have forever changed discussions of Russia and the former Soviet states, so will the Arab uprisings be a part of most conversations about the Middle East for many years to come. Everyone wants to know why the uprisings happened when they did, in the places where they did, and what it will mean in both the near and the distant future. And few topics are
unconnected, as the uprisings were sometimes as much about regional power struggles and the politics of identity that they invoke (and evoke) as they were about the familiar topics of repression and economic hardships within individual states.

How have scholars working in the political science subfield of comparative politics approached the Arab uprisings in their analyses? One of the early trends in these new debates might be described as a reckoning: scholars asked themselves hard questions about why most of their analyses did not anticipate the uprisings or at least the timing of the uprisings. In particular, scholars who had been engaged in one of the most dominant debates over the previous decade—that of “robust authoritarianism”—now questioned their earlier assumptions that the potential for serious challenges to state power were extremely unlikely. With their focus on how these repressive and autocratic regimes had managed to survive while other regions experienced “waves of democratization,” had they unwittingly missed significant signs of regime vulnerabilities?

A second trend that has gained prominence among scholars of the region has been to leverage the rich literature on social movement and contentious politics to understand the dynamics of each of the uprisings. The uprisings were massive mobilizations in highly repressive contexts, so it seems that they must have something to contribute to the literature on social movements and revolutions. At the very least, social movement literature seemed to offer an anecdote to the authoritarianism literature, providing the theoretical framework for understand how the uprisings emerged and evolved in a variety of distinct settings. Of course many scholars of the Middle East had for decades been utilizing these frameworks in their studies of the region, but the uprisings offered an opportunity to test extant theories anew against the large-scale mobilizations of 2011.

While both of these trends among regional specialists have produced rich and lively debates, scholars have mostly drawn comparisons at the national level. This focus has in turn led to a deficit in both studies of in-case variations and of localized micro-processes. I argue that while the aforementioned dominant trends in scholarship should not be abandoned, we may well be missing a major opportunity for building comparative theory if we do not expand our studies to encompass a broader range of approaches and topics. In particular, I fear that while the application of existing theories may well illuminate some dynamics of the uprising, these studies will do little to advance our broader knowledge of political processes and practices locally as well as globally. In this article, I first examine these dominant approaches to studying the uprisings—robust authoritarianism and social movement theory—and illustrate how the resulting analyses will likely be of interest in illuminating those cases but not in advancing theoretical debates broader than those specific to Middle East politics. I suggest that prioritizing “states” and “movements” as objects of analysis can obscure some of the more unique dynamics of the uprisings—dynamics that might be leveraged in contributing new ideas to broader theoretical debates. Finally, I suggest some possibilities for research that
might create these new opportunities for scholars of the region to locate themselves centrally in cutting-edge debates about comparative politics.

State Repression and State-to-State Comparisons

It is commonly thought that scholars are reluctant to acknowledge when their theories or analyses “fail.” Whether for reasons of professional advancement or ego, there are few incentives in academia for scholars to admit when one has been wrong. The Arab uprisings, however, sparked thoughtful, honest, and serious debate among political scientists who had advanced theories of robust authoritarianism in the Arab world and thus had not anticipated the timing or scope of the uprisings and their rapid diffusion. The literature on robust authoritarianism had emerged over the decade prior to the start of the uprisings in December 2010. In many ways, it had already embodied a practice of self-reflection on the “failure” of the highly optimistic literature of the 1990s. Those earlier debates emphasized the relationship between civil society and democratization in the wake of several significant political openings in the region and, in some cases, posited highly optimistic trajectories.¹ The authoritarianism literature did not converge on a single explanation, but scholars posited a variety of factors to understand the apparent puzzle of why the Middle East was resistant to democratization trends spreading elsewhere in the world.²

F. Gregory Gause was among the first to offer a serious reflection on studies of Middle East politics and what they did or did not get right about the Arab uprisings.³ Eva Bellin reflected directly on the strengths and weaknesses of her 2004 article on

robust authoritarianism. My own reaction to the debates critiquing the authoritarianism literature was to be cautious in declaring its failure. Revolutions seldom bring about a wholesale elimination of pre-revolutionary repressive institutions and tactics: the post-revolutionary contexts typically see at least some segments of the pre-revolutionary power structure reemerge in barely disguised forms (if disguised at all). In this way, I felt that the critiques suggesting that some of these regimes were perhaps not as robust as the scholarship suggested were premature, the impact of the uprisings notwithstanding.

This literature was and continues to be insightful for our understanding of Middle East regimes, and its attention to the institutions and practices of repression will not decline in relevance any time soon. My own critique of this literature is not that it failed to predict the uprisings, but that its focus on regimes as the object of analysis comes at a significant cost. I will return to this point below.

Social Movement Theories and Movement-to-Movement comparisons

While the scholars of robust authoritarianism were critiqued for failing to anticipate the uprisings, another group of scholars had been studying and documenting social movements and contentious politics in the region for more than a decade. The literature on social movements in particular demonstrated that, contrary to some portrayals of strong and repressive regimes, the region was one in which a wide range of opposition groups were making claims against those in power (politically and economically) through a variety of tactics. Thus scholars studied protests, marches, and strikes; art, music, and social media; and numerous legal and quasi-legal political parties pushing claims on the floors of parliaments that the regimes never intended to have any role in governance. They studied Islamist movements, labor movements, and student activism on campuses.

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4 Eve Bellin, ”Reconsidering the Robustness of Authoritarianism in the Middle East: Lessons from the Arab Spring,” Comparative Politics 44, no. 2 (January 2012):127-149.
5 I have made this point previously, in Jillian Schwedler, “What should political scientists be doing?” in Marc Lynch, ed., The Arab Uprisings: New Opportunities for Political Science, POMEPS Studies 1, Project on Middle East Political Science, George Washington University.


7 The literature on contentious politics can be understood as having evolved out of the literature on social movement studies. While there are important distinctions, the portion of the current literature that has been most applied to the uprisings has been that relating to the study of social movements.

Critics argued that this literature, like the optimism of the civil society debates of the 1990s, exaggerated the strength of social movements just as the robust authoritarianism literature exaggerated the strength of the regimes. But both strong regimes and strong challenges to them can be present simultaneously. The question central to the social movement literature, therefore, has been to identify under what sets of conditions a range of contentious claims can escalate in ways that challenge the status quo, even when most (or all) of those previous efforts appear to have “failed.”

In this way, scholars of social movements and contentious politics in the Middle East did not anticipate the precise timing of the uprisings, but many of them had documented increases in the scope and scale of contentious episodes over the past decade. Like the scholars of robust authoritarianism, however, these scholars have tended to construct variation-finding comparisons across cases, even as the “objects” of comparison were movements, parties, or revolutions, rather than regimes. It is to this trend in case-study comparisons that I now turn.

The Trouble with Comparing

Comparativists like to compare things, it's what we have been trained to do. We compare whatever objects we study with other seemingly similar or different objects, and the resulting analysis aims to explain whatever similarities and differences we identify. But what does that mean? Many of us answered questions in our doctoral qualifying exams about whether comparative politics was a subfield, an approach, or a methodology, so we know the subfield has debated its methods and epistemologies almost since its beginning. In recent years, the full range of comparative methodologies has given way to a preference for variation-finding analyses. For advocates of this approach, identifying and explaining variation is the best method for leveraging insights. It is largely based on a positivist epistemology and is strongly advocated in the (in)famous volume known in political science as KKV (the acronym of the three authors’ last names: King, Keohane, and Verba). Positivism, we should recall, is not a single approach even within the human

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10 See, for example, the four distinct models of comparison detailed in Charles Tilly, Big Structures, Large Processes, Huge Comparisons (New York: Russell Sage Foundation, 1989), of which variation-finding is only one approach to comparative research design.

sciences, but KKV advocates positivism both for the use of causal “scientific inference” in social sciences and as a single research methodology aimed at explaining variation. The impact of KKV on the discipline of political science has been huge, including in the study of Middle East politics.

This is not to suggest that explaining variation is necessarily “bad.” We see a variation across cases, and we want to know why. Following the outbreak of the Arab uprisings, questions about variations have become central to debates about Middle East politics. For example, why have some states had major uprisings while others have not? Why were some uprisings successful while others failed? Why did some become violent while others did not? It is common to observe that the cases in which uprisings seriously challenged their regimes—what we might call the positive cases—are Tunisia, Egypt, Syria, Libya, Yemen, and Bahrain. Among these, only Bahrain is a monarchy, an observation that has led many scholars to explore this apparent “monarchical exceptionalism”. As a research design, we identify variation in where the uprisings occurred and then sort those states into positive and negative cases. A key conceptual question in this undertaking is therefore “What is X a case of?” Once that question is answered for each in the universe of cases (whether that universe is states with uprisings, states in the Middle East, states in the Arab world, or another criterion), the detailed analysis can begin.

The emphasis on explaining variation across a universe of cases has led to at least three significant weaknesses in our analyses that are not routinely recognized. First, sorting states into “case of” columns can obscure significant dynamics while privileging others. For example, sorting states during the Arab uprisings into those that saw massive uprisings and those that did not implies that in the latter (negative) cases, virtually no groups mobilized as the uprisings spread. If they saw no massive protests, then the uprisings did not “diffuse” to those states. Yet one has to scratch only slightly deeper to see that this was not the case. Of course, few scholars have stated outright that the “negative” cases saw no protests at all. But the majority of books and articles focus on Tunisia, Egypt, Yemen, Syria, Libya, and Bahrain, strongly if unintentionally implying that for other states, any protests that did emerge did not have much impact. Yet no state in the region was unaffected,

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14 Two studies that do focus on a wide range of states in the region are as follows: Lina Khatib and Ellen Lust, Taking to the Streets: The Transformation of Arab Activism (Baltimore: Johns Hopkins University Press, 2014); Paul Amar and Vijay Prashad, eds., Dispatches from the Arab Spring: Understanding the New Middle East (Minneapolis: University of Minnesota Press, 2013). Full disclosure: I authored a chapter in the latter volume.
and certainly not the monarchies. The kings of Jordan and Morocco, for example, nervously sought to get ahead of the uprisings by introducing limited reforms to expand popular participation. Saudi Arabia reportedly spent $2 billion in the first half of 2011 to shore up opposition to any protests—not a move undertaken by a regime feeling little pressure. These monarchies may have weathered the uprisings fairly well thus far, but the trend of focusing on the “big” cases has led to a dearth of detailed studies on the effects of the uprisings in cases other than the six mentioned above.

A second way in which cross-case comparisons can obscure crucial dynamics is by treating these objects of study as if they were unconnected either to each other or to processes originating outside of the region. The literature on diffusion does address interconnections, but it is concerned with the spread of the protests themselves to other “cases” (i.e., states) within the region. Yet regimes and economic practices in each state are also deeply connected to other states in the region as well as outside the region. Global flows of capital and security technologies, for example, make possible the kinds of repression that are imagined to be specific to each state and yet are the result of complex interconnections across states and regions. “Country reports” published by agencies as varied as the US Department of State, the Central Intelligence Agency, Freedom House, and other international human rights organizations also reinforce the perception—again likely unintentionally—that states are islands unto themselves. Scholars clearly know such conclusions to be spurious, but our comparative methodologies often reproduce the suggestion of isolation through the ways in which we utilize the variation-finding approach. “Why here but not there?” is a question whose starting assumption is often one of discrete, disconnected cases.

A third limitation of cross-case comparisons is that in-case variation and micro-processes are less valued due to their inability to solve the larger “puzzles.” Even worse, detailed case studies themselves are sometimes disparaged as producing empirical data but not theoretical insights. Yet some of the classics works in the cannon of comparative politics would, in this view, have limited value given their focus on detailed single case studies at the sub-national level (e.g., E.P. Thompson’s study of the English working class and James Scott’s work on everyday resistance of laborers, to name just two). Of course paired and multi-case studies have also been around since the emergence of comparative politics as a subfield following World War II, but with the publication of KKV, new generations of doctoral students are encouraged, even pressured, to include at least two cases in their analyses—not only because variation-finding analyses are treated as the sine qua non of what good

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comparativists do (and the more cases the better!), but also to increase one’s chance of obtaining a position at a top research university. And, publishers increasingly prefer more cases because they multiply the potential audience for a title. This pressure has been felt in Middle East politics despite the difficulties of obtaining sufficient knowledge even of a single case to produce a solid dissertation (and not to mention the time it takes to master even one of the difficult languages and dialects of the region). Creating a research design around two or more case studies can mean two or more years in the field—so time-to-degree is longer—but the result is a product that can be readily identified (or defended, literally) as a work of comparative politics.

All of this matters to the production of knowledge about the Middle East (as any part of the world) as a whole new generation of doctoral students enter the field in the post-uprising period and the “puzzle” of explaining the uprisings and their variations looms large as research topics are chosen. Apparent patterns of variation across cases—that is, across states, movements, or uprisings—are taken to be so self-evident as to demand most of our attention. Indeed, this pattern of focusing on only certain objects of analysis has serious implications for the depth of our knowledge and thus the reliability of our insights. For example, we know a great deal about how the uprisings unfolded in urban centers, and far less about the rural or small town mobilizations except to the extent that we track where they begin and when they spread. Egypt is not Tahrir Square, except symbolically, but what do we know about patterns of mobilization and repression in Alexandria or in Suez, let alone in smaller towns and rural areas? Did the rural poor protest at all? If so, did they do so locally, or seek to relocate to join larger protests in more urban centers? Much of this information is known and knowable, but has seldom been incorporated into our analyses, certainly not systematically in the subfield of comparative politics within the broader discipline of political science.

The field of social movement studies may be more open to single case studies than the mainstream of political science, but it often retains a tendency to compare the “life cycle” of the different movements, revolutions, uprisings, etc. That is, we ask questions about the trajectory of different movements, whether and under what circumstances they shift from peaceful to violent means of protest, how they use various resources (e.g., social media, cell phones, networks, and so on), and who joins or defects, at what stage, and why. While these are questions that scholars certainly want to answer, the location of a movement or uprising as the primary unit of analysis again obscures peripheral dynamics and variations across space and arenas within a “single” mobilization. Indeed, as detailed studies of the Iranian revolution have ably demonstrated, that revolution can hardly be called a single movement until perhaps even the last months of 1978 when the regime closed

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virtually every venue for mobilization except for the mosque.19 My critique is not to disparage the study of movements such as the Arab uprisings, but rather a plea that we recognize that our discursive construction of movements (or regimes) as objects of analysis is just that: a discursive construct. This matters for comparative analysis because if our core objective is to draw insights (or theories) from patterns across cases, we need to recognize our choice in selecting certain “objects”—states, uprisings, movements—as worthy of study has significant implications for which patterns we see and thus which conclusions we draw. Whether our desire to identify patterns is driven by professional practices (making a name for oneself) or normative concerns (understanding bad things in the hope of making them better), it behooves us to “see” our objects of comparative study in different ways in order to understand those patterns in the most fine-grained manner possible. This is equally true if we aspire to mid-level theorizing or the identification of grand covering laws (nomothetic theory), or if we aim to leverage micro-details and interpretive methods to simply get a single story right.

An additional potential of in-depth case studies is to focus on micro-politics and practices—significantly narrowing the scope of what we seek to explain. Particularly since the “outcome” of most of the uprisings remains unknowable—these are revolutions still in progress—attention directed at the sub-state level can generate new insights by illuminating patterns, processes, and practices that are less evident at the macro level. For example, we might ask how new alliances emerge and old ones are challenged in different ways across a single case, across class, across neighborhoods, and so on, rather than emphasizing nation-level patterns alone. Have new actors emerged over the course of the uprisings, and in what spaces? How have mobilizations and police responses varied, for example, across Yemen, and over time?

In my current work on Jordan, I have been examining patterns in protest and policing activities spatially, which has enabled me to see patterns of repression and exclusion that are not immediately evident at the state level. For example, police responses have varied across neighborhoods depending on not only who is protesting and what claims are being made (e.g., whether protestors are demanding the fall of the regime, democratic reforms, economic justice, the cancellation of the peace treaty with Israel, and so on), but where the protests are taking place. This lens has in turn drawn my attention to the massive impact of neoliberal economic reforms on local politics not only in terms of increased exclusions,20 but also in the sense that new built environments are fundamentally altering the landscape in ways that foreclose the possibilities for certain kinds of more egalitarian politics. Gated communities and locations of elite consumption exacerbate economic exclusions by


making them more easily recognizable, creating a hyper-elite culture alienated and physically separated from the (increasingly unemployed) general population. The insights of such studies of micro-politics can still be leveraged for more macro-analyses, however. Given that similar projects of economic reform and urban redevelopment have been unfolding across the region, a set of detailed studies of the local effects of these projects could illuminate shifting patterns and practices that we are simply not able to see in our studies of the Arab uprisings that prioritize obvious variations across states.

Finally, we might also develop comparative studies that think creatively about connections across cases, and not only about questions of diffusion of protests (a great question, but not the only one). Patterns of repression also diffuse. But even more, we can explore, for example, the spread of notions of legal accountability and how discourses (created and used by regimes as well as their challengers) adopt new tropes, draw references to similar justifications, and so on. We can think about the political economy of hyper-securitization and surveillance technologies, and how they are connected to foreign aid, to notions of modernity and cosmopolitanism, to neoliberal economic projects, and to which regimes and non-state actors are training which security agencies in which countries. We can also think about how certain economic paradigms remain unchallenged (or do they?) across cases, and why that is the case. What are the precise ways in which economic dimensions of grievances are being shut out of the debates about “transitions”? What role are “experts” (like political scientists, but also aid agencies) playing in advancing certain models of what is happening and what are the possibilities of what might happen?

Conclusion

The Arab uprisings have drawn the attention of non-regional specialists across the discipline of political science, but as regional specialists we need to be more than repositories of necessary local knowledge or appliers of existing theories. To contribute more centrally to the subfield of comparative politics, we should try to be less constrained by the orthodoxy of KKV-styled variation-finding research designs. To be sure, scholars of Middle East politics are already engaged in debates about what we should be doing.21 We are faced with a rare opportunity to rethink how we understand politics in the region while we also have (for the moment) the broader attention of our sub-discipline and even parts of the larger discipline.

In this article, I have sought to highlight the dominant frames currently used in comparative politics for analyzing the Arab uprisings and to draw attention to some

21 POMEPS, New Opportunities for Political Science, POMEPS Studies 1, Project on Middle East Political Science (Washington D.C.: George Washington University, 2012); idem; Reflections on the Arab Uprisings. POMEPS Studies 10, Project on Middle East Political Science (Washington D.C.: George Washington University, 2014).
of the weaknesses of those approaches. I wish to emphasize that I am not advocating for the abandonment of studies of regimes or movements, nor do I think that those topics will become irrelevant in the near future. Rather, I hope to have drawn attention to the ways in which the particular research design of variation-finding studies tends to assume discrete objects of study (regimes, movements) in ways that may obscure other fascinating processes and practices at both the micro-level and in terms of the complex interconnections across states and regions.

Perhaps most important of all is that these varied debates—among scholars with different objects of studies, methodologies, and sometimes even diametrically opposed epistemological commitments—should continue to be inclusive. Scholars of Middle East politics have been exemplary in this regard, with quantitative positivists and ethnographers engaging with each other routinely, striving to understand research questions and methodologies on each author’s own terms. This practice is sadly atypical for much of political science. My comments and suggestions here—which at times might appear to some as approaching a rant—are intended to be a provocative contribution to an already lively debate. I am deeply grateful for the generosity with which my colleagues in Middle East politics have received and critiqued my own interventions. As we work to improve our own analyses and expand our understanding of the region and of politics in general, I strive to advance the norms of generosity and respect that already characterizes our lively and passionate disagreements.