ARAB UPRISINGS

New Opportunities for Political Science

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The Project on Middle East Political Science

The Project on Middle East Political Science (POMEPS) is a collaborative network which aims to increase the impact of political scientists specializing in the study of the Middle East in the public sphere and in the academic community. POMEPS, directed by Marc Lynch, is based at the Institute for Middle East Studies at the George Washington University and is supported by the Carnegie Corporation and the Social Science Research Council. It is a co-sponsor of the Middle East Channel (http://mideast.foreignpolicy.com). For more information, see http://www.pomeps.org.
The uprisings that swept the Arab world following the fall of Tunisia’s President Zine el-Abidine Ben Ali in January 2011 represented a stunning moment in the region’s political history. For political scientists specializing in the region, the events of the last year and a half represented an exhilarating moment of potential change but also an important opportunity to develop new research questions, engage in new comparisons, and exploit new data and information. The Arab uprisings challenged long-held theories dominant in the field, particularly about the resilience of authoritarian regimes, while opening up entirely new areas of legitimate social scientific inquiry.

The Project on Middle East Political Science (POMEPS) was created in 2010 in part to build the capacity of Middle East experts to engage and inform policy-makers, the public sphere, and other political scientists about the region. On May 29 to 30, 2012, POMEPS convened a group of leading political scientists who specialize in the Middle East for its third annual conference at George Washington University to discuss the opportunities and challenges that the Arab uprisings pose to the subfield. Participants were asked:

“What new and innovative research questions do you think have become particularly urgent, feasible, or relevant? How would those research questions fit into wider debates in the field of political science?”

This special POMEPS Briefing collects nearly two dozen of the memos written for the conference. The authors are all academic political scientists and Middle East specialists who speak Arabic and have lived in and studied Arab countries for extended periods. They include scholars at all career levels, from senior faculty at top universities to advanced graduate students still writing their dissertations. The memos reflect on a wide range of debates and paradigms within political science, and taken together lay out an impressive set of marching orders for the subfield. Graduate students looking for dissertation topics and junior faculty looking for articles that might make a big splash take note.

There is widespread agreement that, as Jillian Schwedler puts it “it’s just too early to really make substantive headway of the sort that would allow us to either challenge or support existing theories of revolutions and regime change.” At the 2011 POMEPS annual conference, held at the height of the still-surging Arab Spring, political scientists long keenly attuned to the machinations of Arab authoritarian regimes had warned about exaggerated expectations for change. Theories hastily put forward at the height of the Egyptian revolution about inevitable, irreversible change looked foolish within months as the military regime clawed back power, Islamists surged at the polls, and activist groups struggled to adapt. The rise of sectarianism, driven in part by the ugly developments in Bahrain and Syria, divided momentarily united Arab publics, while the descent into violent stalemate in several countries deflated outsized expectations. But one easily forgotten change should not be underestimated, as Nathan Brown eloquently argues in his memo, “politics — in the sense of public discussion and contestation about issues of common concern — now unmistakably exists.”
Eighteen months on, the field is better positioned to ask the right questions and to capture
both broad trends and significant variation across and within cases. The questions raised
by the Arab uprisings are not parochial. They go to the heart of the most important and
relevant debates in the social sciences, to say nothing about foreign policy and the broader
public. Appropriate caution about leaping to conclusions should not prevent scholars from
grappling with these developments head-on. Area experts with deep knowledge of the
Middle East cannot cede the field to those who lack such background. But they also cannot
simply assume that their expertise will grant them a privileged voice in public or scholarly
debate. More than ever before, this is a moment for political scientists specializing in the
Middle East to prove that particular expertise makes a real difference. Good articles are
beginning to appear in leading academic journals, and more are in the pipeline — but there is
clearly far more to be done.

Surveying the emerging region today reveals an uneasy mix of change and continuity —
which may be politically frustrating, but is producing the kind of variation that should
allow political scientists to gain purchase on crucial questions. Mobilization has receded in
many places, but remains real and vibrant in others. Some regimes have fallen but others
have proven resilient. Some countries have been consumed by violence, while others have
avoided such a trajectory. Islamists have leapt into the political arena, performing better in
some countries than others. Few Arab countries seem to be following the “transitions to
democracy” template familiar to generations of graduate students. Public opinion surveys
have proliferated, but have done poorly in predicting electoral outcomes.

What are the key questions emerged at the POMEPS conference, then? There is obviously
a great deal of research to be done to explain the variation in regime survival. The fall of
long-sitting leaders such as Ben Ali, Egypt’s Hosni Mubarak, Yemen’s Ali Abdullah Saleh
and Libya’s Muammar al-Qaddafi each took a different path — and should Syria’s Bashar
al-Assad be next, this would be yet another distinct course. Meanwhile, other regimes that
might a priori have appeared to be in line for serious trouble survived. Even where leaders
have fallen, the continuity with the old regime in some cases seems dramatic (Egypt, Yemen)
compared with others (Libya, Tunisia). Explaining this variation in regime survival will likely
be a major preoccupation of the field in the coming years.

One common answer has been particularly contentious: monarchy. Is there a monarchical
exception, or some reason to believe that monarchies are more resilient in the face of popular
grievances? For some, the answer is obvious: none of the fallen regimes were monarchies,
while non-monarchies have struggled or fallen at historic rates. As Michael Herb argues,
“the regimes most seriously affected by the Arab Spring were not monarchies, with the
exception of Bahrain.” But others are far more skeptical that monarchy makes the difference.
After all, Gulf monarchies such as Bahrain, Kuwait, and Oman all experienced significant
mobilization, as did non-oil monarchies such as Jordan and Morocco, which gives lie to any
sense of their greater legitimacy. The significance of monarchy in regime stability should be a
vibrant debate in academic journals in the coming years.
The real significance of the rentier state is emerging as another extremely interesting area of debate. If monarchy does not provide the answer to regime resilience, what about oil wealth and the ability to distribute patronage as an important buffer to regime collapse? For Glenn Robinson, the verdict is clear, “While oil-poor Arab countries have been riven by turmoil, the hydrocarbon rich countries (enjoying high oil and gas prices throughout 2011) have suffered relatively little turmoil by comparison, and have used their significant rents to placate most potential dissent.” David Waldner, by contrast, warns that “our understanding of the political economy of the Middle East has for too long rested, somewhat complacently, on a relatively vague notion of the rentier state.” Gwenn Okruhlik further notes, “the oft-utilized rentier framework vastly overstated economic determinism. In reality, money does not spend itself.” Oil rich Libya, for example, did not find itself particularly protected from popular challenge. Peter Moore points to variation in the fiscal capacity of states as a crucial variable, while others point to ethnic or family dominated regimes. And, asks Shana Marshall, what of the financial interests of the militaries themselves, about which far more is now publicly known than ever before?

Several participants point to questions about the real political impact of social welfare provision in the region as key emerging questions. Melani Cammett directs attention to how little is known empirically about the implementation and effects of social welfare programs. Eleanor Gao asks the pertinent empirical question of “whether governments can purchase loyalty through increasing public sector employment and salaries, reducing taxes, and augmenting food subsidies.” For David Patel, shifts in existing patronage networks in the face of crisis — including, he notes, global financial crisis and pressures toward austerity as well as popular mobilization — may well be more important than the macro questions of regime survival.

The wave of mass mobilization is obviously another primary area for new political science research. Scholars are already doing important work on the protest movements in various countries, unpacking the role of different actors (youth, internet activists, labor unions, political parties, and so forth) and different political contexts. There is a vibrant debate unfolding about the micro-level foundations and mechanisms driving the sudden explosion of mass mobilization that will be of obvious relevance to the broader political science literature. Some focus on the revelation of private information and updated expectations of success (an argument I developed here). Rex Brynen notes, for instance, that, “compliance with authoritarian rule was sustained, in part, by a regime’s ability to project spectacular omnipotence. Populations, for the most part, genuinely believed that resistance was futile. What the Tunisian revolution did, of course, was to shatter that perception. The consequent demonstration effect then led other Arab populations to reevaluate both the power of popular protest and the strength of regimes.”

Others, such as Wendy Pearlman, focus on the role of emotions in fueling protest. And still others focus on the dynamic relationship between repression and protest. Eva Bellin argues, “Syria dramatically challenges [Mark] Lichbach’s analysis given the persistent mobilization
of protest even in the face of the state’s use of consistent lethal force against the protesters.” What is the “right” level of repression, that which keeps subjects in line without triggering a cascade of outraged protest?

Several of the memos urge scholars to look beyond the immediate action for deeper causes. Adria Lawrence urges more historical comparisons, noting that this is not the first time such protest waves have caught the outside world unprepared: “in the mid twentieth century, colonial powers were shocked when their subjects took to the streets to demand independence.” Others, such as David Patel and Quinn Mecham, want more extra-regional comparisons. Jillian Schwedler urges attention to “in-case variation. As far as I can tell, we know a lot about what has and is happening in urban centers, and little about the rural mobilizations.” Janine Clark calls on scholars to pay attention to “slow change: the gradual social, economic and political changes at the local level that underlie rapid political change and make it possible.” She also warns us to “look beyond the capitals and large urban cities of the region and pay attention to the region’s peripheries: the rural areas, small towns and small cities with relatively little national economic significance.”

The regional dimension of the protests and the elicited regime responses should force greater attention to the oft-neglected international relations literature. Curtis Ryan argues forcefully “the outcome of almost every case within the Arab uprisings has turned at least in part on the actions and decisions of external powers.” This is a problem for a comparative literature, which tends to focus on domestic variables and treat each country as a discrete case. As Gwenn Okruhlik points out, “these were not coincidentally simultaneous parallel revolts but somehow a single collective phenomenon.” Sheila Carapico notes “the sharing of slogans in classical Arabic like al-sha’ab yuridh isqat al-nizam and Irhal invite us to think again about the terrain of an Arab ‘region’ which, for all its diversities and contradictions, has more coherence than either the Middle East or the Muslim world.” My memo points to the role of the media, both broadcast and online, in disseminating information, framing the uprisings, and fueling the protests.

These are only some of the many research agendas that unfold in the memos to follow. They lay out a rich overview of the current thinking of a cross-section of leading scholars who are deeply engaged in thinking about how the Arab uprisings should change the scholarly field. This is an exciting time for scholars, a time for theoretical creativity and empirical. It is a time when our ideas can actually matter for shaping policy, for informing public debate, and for addressing the mainstream of the field of political science. It should not be missed. This special POMEPS Briefing represents our modest effort to help the field seize these opportunities.

– Marc Lynch, Director of the Project on Middle East Political Science
Research Opportunities Post Arab Spring: Mobilization and Contentious Politics

By Eva Bellin, May 22, 2012

In the wake of the Arab Spring four issues related to the dynamics of political mobilization and contentious politics appear to me as important, feasible, and rich opportunities for comparative study through the exploration of Arab cases. The four issues are as follows:

The effectiveness of non-violent mobilization

The past year and a half has seen multiple cases of non-violent mobilization in the name of reform and/or regime change in the region. In some cases non-violent mobilization has proven effective. In others it has not. What explains this variation in effectiveness? Is it a consequence of the character and scope of the mobilization? The character of the regime? The timing of the mobilization? What does this say about the terms governing the effectiveness of non-violent mobilization more generally?

The relationship between repression and mobilization

There has long been a debate about whether the embrace of repressive tactics by the state to quell popular protest is more likely to enflame or extinguish such mobilization. The relationship is not necessarily linear. Even extremely violent repression by the state can have contrary impact. To the extent that protesters are “rational calculators” extreme repression is likely to depress participation since violent repressive tactics dramatically heightens the cost of participation. To the extent that protesters are responding to more emotional or ideological motivations extreme repression is likely to increase participation since violent repressive tactics heightens popular sentiment of outrage with the state. Mark Lichbach has argued persuasively that there is a curvilinear relationship between repression and mobilization. However he has also argued that where the state applies consistent lethal force against protesters, mobilization generally declines. The case of Syria dramatically challenges Lichbach’s analysis given the persistent mobilization of protest even in the face of the state’s use of consistent lethal force against the protesters. The puzzle of Syria’s experience begs for study.

The relative role of emotion vs. rationality in explaining the mobilization of protest

The dynamics of protest witnessed cannot be fully accounted for by a rational calculation of interest. Clearly emotions such as outrage, solidarity, hope/optimism played a role in the level of mobilization witnessed in Tunisia, Egypt, Bahrain, etc. Using within case and cross case comparison what do the Arab spring cases say about the relative role of emotion vs. rationality in the mobilization of protest?

Interactive effect of mobilization tactics and regime response

The events of the past year make clear the contingent nature of protest politics and especially the interactive effect of mobilization tactics and regime response. Using within case and cross-case comparison, is there a way to systematize how this interaction plays out?

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Politician-Citizen Linkages and Representation in Authoritarian and Transitional States

By Lindsay Benstead, June 11, 2012

The Arab Spring — for all the debates about how it should be understood — offers unparalleled potential for scholars of Middle East politics to extend, develop, and test comparative theories both within the Arab world, as well as cross-regionally. An area of research that is particularly significant and feasible is the politics of transition; in this memo I pay particular attention to theoretical debates about why politician-citizen linkages vary across space and time and how this comes to bear on the extent of accountability and the quality of representation for different segments of society in authoritarian and transitional regimes. Yet, understanding the politics of authoritarian and transitional regimes remains critically important — as does explaining why transition democratization occurs.

The changing political environment is also providing a watershed in survey research. This is occurring as a result not only of the increasing number of polls that are being conducted in the Arab world, but also, and perhaps more importantly, as a result of the increased interest in regional collaboration among Arab-world based scholars, which has the potential to facilitate a giant leap forward in survey research capacity. Organizations seeking to advance research capacity in the Arab world should focus on funding travel and research costs for scholars in the Arab world, making it possible for them to train other Arab researchers.

Politician-Citizen Linkages and Accountability

The post Arab spring elections provide a unique opportunity to develop and test theories about the politics of transition and to ask how and why critical political outcomes change or remain constant across time and space. One urgent question is how and why regime change—whether regime structure or degree of inclusiveness—shapes what parties and politicians provide for citizens and, thus, the overall quality of representation or extent of accountability in the political system. Equally, what factors explain variation in these outcomes across a class of authoritarian or transitional regimes? Do these answers depend on particular social structure, regime type, or economic features of the country?

Literature in comparative politics makes particular predictions about the relationship between political, social, and economic variables and mixes of programmatic and clientelistic benefits in a political system (See, e.g., Lust-Okar, 2006; Malesky & Schuler, 2010; Piattoni, 2001). This literature employs both spatial comparisons — across country cases with different values on one or more explanatory variable — as well as to temporal comparisons — a single case over time as change in as an explanatory variable (in this case regime change) occurs. However, a great deal of theoretical development, extension, and testing is needed, both in countries that are well-studied, as well as in cases which are theoretically-relevant, but neglected, including most in the Arab world. Kitschelt and Wilkinson (2007) argue, for example, that the mix of programmatic and clientelistic benefits provided in a political system depends on the level of party competition, ethnocultural make-up of the society, and level of economic development. This implies that as political systems become more competitive, more programmatic and less clientelistic benefits will be provided. It also suggests that particular characteristics of political regimes, social structure, and economies shape politician-citizen linkages and, thus, explain variation in the degree of accountability in the political system.

Kitschelt and Wilkinson’s (2007) theoretical framework is useful for theory-building and testing about the factors that shape these outcomes in the Arab world. It also invites theoretical innovation, for example surrounding the question of whether and why some groups or interests benefit disproportionally from personalistic benefits and how these linkages shape outcomes such as the durability of tribal identity and the quality of representation of groups such as women or ethnic minorities, all questions I address in my book manuscript on Morocco, Algeria, and Tunisia. Van de Walle (2007) argues with regard to sub-Saharan
Africa that democratic political transition transformed clientelistic goods provision from narrow elites to kin and family, through social upscaling, to groups based on sectoral or class interests. To what extent will politicians serve wider constituencies—though club goods and programmatic benefits—as party competition increases in the Arab world?

**New Opportunities in Survey Research**

These questions can increasingly be answered as a result of the improving ability of U.S. and Arab world-based scholars to conduct studies in countries that were previously closed to social research (e.g. Tunisia and Libya) or in which it was difficult to obtain access (e.g. Egypt). While these openings provide possibilities not only to test theories about the politics and dynamics of transition, they also provide the possibility—during a brief window of opportunity—to collect retrospective data about linkages during the previous regime where this data does not exist. Asking some questions about behaviors or attitudes before regime change, while imperfect, provides a baseline for comparison in those countries for which data are unavailable. To the greatest extent possible, surveys should include not only attitudinal questions, but also through measuring behaviors (e.g. voting, receiving a gift for one’s vote, getting help with a personal problem from an official, etc.)

As scholars continue to advance survey research in the Arab world, the development of new instruments requires care — first, to ensure that measures included are both well operationalized, as well as well suited to test theoretical suppositions. This presents a number of challenges, but most importantly, it raises tensions between the use of similar questions — to facilitate comparisons across time and space—and the development of new indicators that respond to the needs of theory-testing within a particular context. Advances in sampling procedures — use of random within household selection — are also important, as is continued innovation in methodology (e.g. framing or list experiments, tests of mode or interviewer effects), given its potential to add to theoretical knowledge of key social and political processes. Capacity-building in survey research is critically needed in the region. Yet, the single most effective way to increase capacity is to facilitate collaboration between scholars based in different Arab countries and between these scholars and U.S.-based researchers. Arab scholars who are already skilled at conducting survey research are well placed to build the capacity of researchers with less experience and to train interviewer teams. Yet, Arab scholars in some countries have very limited funds to participate in these activities. Organizations seeking to advance research capacity in the Arab world should focus on funding travel and research costs for Arab-based scholars in order to facilitate their participation in project with scholars in other Arab countries and to allow them to share best practices with research teams in other countries.

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**References**


The Difficult Rebirth of Arab Politics

By Nathan J. Brown, May 4, 2012

The political changes in the Arab world are likely to open new avenues of inquiry; they have also heightened attention (even from scholars previously uninterested in the region) in domestic politics of Arab societies and will likely inspire more extensive comparative work.

At this point, politics is confusing and political systems within the region seem to be veering in some sharply different directions. But there is one broad theme which seems to cut across the region: politics — in the sense of public discussion and contestation about issues of common concern — now unmistakably exists.

When I first journeyed to Arab societies in the early 1980s as a doctoral student of political science, one thing struck me in Egypt (where I spent the most time) as well as Jordan and Syria (which I briefly visited): nobody seemed interested or felt comfortable speaking about politics. A combination of nervousness and disinterest inhibited or even prevented most conversations. Public places were devoid of political discussions: the largest public squares in major cities showed no signs of assemblies for political purposes; all coffeehouses and restaurants seemed to have televisions that showed only sports. If one picked up a newspaper, one read about official comings-and-goings but precious little politics; if one watched the television news, such official comings-and-goings seemed to dominate half the broadcast.

That no longer holds. Politics has edged out even sports in personal conversations and coffeehouses; public squares have filled with demonstrators in some places; newspapers are filled with (and occasionally even invent) news; and political talk fills the airwaves, attracting higher ratings than sitcoms, soap operas, and even sports.

Arab politics — in the sense of discussion and contestation about public affairs — has been reborn. And it is pursued, sustained, and developed in numerous structures, institutions, and practices.

Many of the changes have already been noted in various settings. But our tools to understand such a development are very much shaped by normative concerns — that are lofty and even inspiring, but also ones that can edge out more practical understandings. Political scientists have shown great interest in exploring public discussions about politics: how can we reason together, despite our differences in outlook and values, and come to common decisions about public matters? How can we structure public discussions to encourage deliberation rather than mere horse-trading, bargaining, sloganeering, threats, and appeals to passion rather than reason. How can we make sure that all citizens have access to — and the ability to participate in — such discussions?

Yet politics, even in the sense that I use the term here in a manner that centers on public discussion, is rarely so lofty. Public discussions take place to be sure, but they are emotional, manipulative, passionate, and edge into disrespect, prejudice, and even threats. And not only are certain forms of argumentation privileged in the actually existing public spheres, but certain participants have privileged entry as well: in a television talk show or a public rally, only a few voices speak. Class, status, and power do not disappear when one argues in public. But much of how they matter depends on how publicity is structured, molded, and used.

It is instructive here to turn to the confusing guidance of Jürgen Habermas, one of the most sophisticated normative theorists of the politics of the public sphere. He generally has done so in almost prose so difficult to penetrate it is never quite clear if it has been translated or is still in German: “I think an empirically meaningful approach to our selective and even colonized forms of public communication is to see how they work within certain procedural dimensions of formal inclusion, of the degree of political participation, of the quality of discussion, of the range of issues, and, finally and most important, of
how the presuppositions of those public debates are really institutionalized.”

Immediately after making this comment, Habermas explained himself in uncharacteristically earthy terms: “Of course, I too look at American television. When I see debates between presidential candidates, I get sick.” Real politics in real public space can be nauseating. But, as Habermas hastens to add, “we at least have to explain why we get sick...”

Arab politics in the sense of public discussion and contestation of issues takes place in old and new media, public spaces, and semi-private gatherings. We now have unprecedented opportunities to explore actually existing public spheres — not simply to celebrate them but to understand which voices and what kind of arguments they privilege and which ones they exclude and marginalize; how they reflect differences in class and power and how they allow such differences to be contested; how much they affect policy outcomes and how much they are affected, guided, or controlled by existing authorities.

If we take on the task of exploring Arab politics in this sense, I may be at risk of exaggerating the novelty of the subject in three ways. First, the contrast between the current day and past decades, while strong, is certainly not absolute. And the birth of Arab politics was a trend discernable for more than a decade; it was not a sudden invention of January 2011. Second, scholars of the public sphere, while sometimes inspired by normative concerns, have never been blind to wealth and power. And some (such as historians of print media) have shown us how to understand how such institutions of publicity actually operate. Third, several leading scholars of the Arab world (and I need not name names here since they are well known) have already begun the exploration I propose.

So much of the groundwork has already been laid. All the more reason to get started.

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Arab Uprisings and the Study of Middle East Politics:
Semi-Random Observations in Search of a Witty Subtitle

By Rex Brynen, May 23, 2012

While we can endlessly debate who was most right (or least wrong) about the emergence of mass, regime-toppling protest movements in the Arab world in 2011-12, it remains the case that few if any analysts in either academic political science or government diplomatic and intelligence agencies expected such a widespread challenge to the authoritarian status quo to emerge in the region so fast, and with such dramatic effects. Within the field, explaining authoritarian persistence and adaptation were the predominant themes of the pre-Mohamed Bouazizi literature, even for those of us who had long held out the possibility of future political transitions and transformations.

 Appropriately enough, scholars have already begun to address what it is we might have missed (Gause 2011, Bellin 2012). Clearly too little attention was paid in the past to civil-military relations, which have proven to be critical
in almost every case. Too much emphasis may have been placed on the stabilizing effects of neopatrimonialism, rigged elections, and crony capitalism. Perhaps too little emphasis was placed on growing networks of contentious politics, on everyday forms of opposition, and on the enabling effects of information and communication technologies. Critical self-reflection, both as individuals and as a field, is undeniably a good and useful thing, contributing to refined approaches and better insights into the politics of the Middle East.

However, there is also the risk that we learn the wrong lessons from the “Arab Spring.” There are a variety of reasons why we might do so. It might be a function of an emerging consensus about what happened, why, and when that — while intuitive and comfortable — is also somewhat post hoc in its rationalizations. It might be due to academic fad, fashion, and the tendency of scholarship to look for vindication of cherished models rather than genuine black swans that require paradigmatic rethinking. It might be because of the effects of data availability and deficiencies, which facilitate some sorts of explanations more than others.

Given that, I offer — on the admitted basis on no clear methodology whatsoever — some thoughts on where we might be at risk of misreading the Arab Spring. In doing so, this memo is not intended as a critique of very recent work on very recent events, but rather, cautionary suggestions of what methodological and theoretical potholes we might want to avoid in the future.

**Ad hoc economic explanations.** It is deceptively easy to link the emergence of popular protests with snapshot data on unemployment or poverty rates, food prices, or social inequality. However, it is also unconvincing. Poverty data is rarely comparable from year to year or country to country for methodological reasons. In many cases unemployment rates (including youth unemployment), inequality, and inflation had remained relatively unchanged in much of MENA for years. Food prices did increase, but less so than in the past, and with only modest effects on household expenditure in many cases. This is certainly not to say that economic factors were irrelevant in sparking protests, but it is to suggest that demonstrating the precise connections will require greater sophistication than seen in many accounts to date.

Retrospectively rendering Tunisia into Tahrir. In some ways the initial protest movement in Tunisia — the spark that undeniably lit the tinder of the Arab uprisings — does not fit well with the emerging popular and academic narrative of 2011, a collective memory that has been heavily influenced by subsequent events in Egypt. Certainly ICTs played an important role, but the actual pace at which demonstrations travelled from Sidi Bouzid to Tunis was remarkably slow (less than 2km per hour, slower than pre-internet protests in Russia in 1905). Certainly unemployed youth played an essential role, but what was also important about the first large demonstrations in Tunis was the active organizational participation of the UGTT, the lawyers’ syndicate, and other rather traditional (and middle-aged) organizations, many of which had previously been considered as clients of the neo-Destourian regime. Without the support of the unions and middle classes, and without the quasi-coup against Ben Ali by the military, it is not clear that the Tunisian revolution would have happened at all.

**Underestimating the role of perception.** To my mind, perhaps the greatest insight into the Arab Spring is offered by a book that was not about regime change at all, but rather regime durability: Lisa Wedeen’s *Ambiguities of Domination* (1999). As her work showed, compliance with authoritarian rule was sustained, in part, by a regime’s ability to project spectacular omnipotence. Populations, for the most part, genuinely believed that resistance was futile.

What the Tunisian revolution did, of course, was to shatter that perception. The consequent demonstration effect then led other Arab populations to reevaluate both the power of popular protest and the strength of regimes.

Of course, attributing important causal weight to perceptual change presents challenges. It is hard to measure, and even harder in authoritarian societies. As Charles Kurzman (2004) noted with regard to Iran,
participants themselves often fail to anticipate their own perceptual shifts, making it even harder for an outsider to theorize or predict them. Nonetheless, there is a significant scholarly literature on demonstration effects, fads, informational cascades, and the role of signaling in collective action — much of it rooted in economics and rational choice political science — which might be useful here. Whether or no one accepts all of its theoretical presuppositions, it is certainly worth mining for potential insight and inspiration.

They’re not dead yet. It is worth remembering that the majority of authoritarian regimes that were in place in December 2010 are still there now, and many seem likely to be in place for many Decembers to come. Explaining their resilience remains just as important as explaining regime transition in Tunisia, Egypt, Libya, Yemen, and perhaps elsewhere. In particular, the striking persistence of monarchies — an issue that few of our comparativist colleagues care about given the scarcity of proper throw-the-dissidents-in-the-dungeon type monarchies outside the Middle East — deserves further discussion.

Hopefully in moving forward we can also put behind us the notion that the study of authoritarian persistence and the study of post-authoritarian transition are somehow at odds at each other — a “great taste/less filling” debate that we had seen emerge in some of the literature in the decade prior to today’s transformations. On the contrary, if the events of the past year and a half have highlighted anything, it is that the most productive analysis is that which addresses both.

_ Rex Brynen is a professor of political science at McGill University._

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**References**


The Politics of Economic and Social Change in the MENA

By Melani Cammett, May 2012

The political economy of development in the Middle East/North Africa has received less attention in political science scholarship on the region to date but, as the Arab revolutions have demonstrated, some of the most pressing and vexing problems facing the MENA region are economic in nature. These include factors that have potentially hampered growth such as low industrial development and private sector investment as well as the mismatch between educational systems and labor market needs, among other issues. The dynamics of protest and political change complicate state efforts to address these problems because of the temporal disjuncture in citizen expectations for immediate improvements in standards of living and the longer time horizons needed to bring about real change. According to neoliberal policy prescriptions, state responses to immediate popular demands, such as the extension of food subsidies, wage increases and the expansion of public sector employment, exacerbate the very problems that have contributed to economic underdevelopment in the region.

The politics of economic change present a rich research agenda for MENA specialists and, given that the region's economic problems and responses to them are fundamentally political, political scientists are well equipped to take on relevant questions. In this brief memo, I propose a few general areas of research, where relevant noting how some of my current projects directly or indirectly address these questions.

The record of economic reform and economic policy orientations

Surprisingly little is known about the actual trajectories of economic adjustment since the 1990s. Some studies present general overviews of liberalization and privatization, particularly in the "star pupil" countries such as Morocco, Tunisia, Jordan and Egypt. But the actual empirical record is not well researched in part due to lack of data, some of which may be more available now (wishful thinking??). Baseline empirical information on the track records of economic and social policy reform in the region would lay the groundwork for research that contributes to scholarly research on the politics of economic reform beyond the region. More detailed knowledge about economic change in the MENA since the 1980s would undoubtedly present puzzles based on cross-national, subnational and sectoral variation in economic retrenchment. This research agenda also has great policy relevance. In light of the Arab Revolutions, the question of what kinds of development policies MENA states should adopt is all the more urgent. Many arguments I have encountered in the media and at conferences presume an efficiency-equity tradeoff in whatever policies MENA governments adopt, despite the fact that economists debate the assumed tradeoff. Political scientists with expertise on the political economy of development of specific countries in the region should engage more directly with debates about appropriate economic policies for the MENA diverse economies.

Human security and welfare regimes

In addition to broad trajectories of economic adjustment, we know very little about how MENA welfare regimes have evolved since their establishment in the initial phases of post-independence state-building and during the alleged period of retrenchment in the 1980s onward. In the non-oil economies, what “entitlements” were extended to which groups and how, if at all, were they cut back during the reform period? In the Gulf oil economies, how have declining oil resources (or the prospect thereof), among other factors, compelled governments to renegotiate social contracts? What intra-regional patterns of the renegotiation of social contracts have emerged in the region? I am currently working on a paper on comparative welfare regimes in the MENA. Likely focused on the non-oil economies, the paper aims to explain variation
in welfare regime development in the post-independence period and distinct patterns of reform since the 1980s.

**Historical legacies and paths of economic development/underdevelopment in the MENA**

In the past decade, a rich body of research on the very long-run roots of economic development has emerged in economics and political science. To my knowledge, Timur Kuran is the only social scientist specializing in the region to directly engage this research (i.e., from a very long-term perspective). Kuran has made enormous contributions to the study of development in the region but his arguments are subject to criticisms, as Goldstone elaborates (and I argue in a slightly different way in a forthcoming review essay). “Islamic institutions” per se may not adequately explain the relative lack of industrial and private sector development in the region. Rather, as the work of economic historians suggests, Ottoman state institutions and practices, among other factors, are more likely causes of the suboptimal institutional equilibria highlighted in Kuran’s work. Furthermore, the focus on Islamic institutions neglects temporal and geographic variation in actual implementation and lived experiences. These broad critiques are a good start, but we need empirically grounded, comparative analyses of long-run institutional development across the region, incorporating systematic attention to colonial and Ottoman legacies and their interplay with post-colonial practices in specific places in the region. This research would be make an important scholarly contribution to an exciting literature and relates to current debates about development policy in the MENA.

**The Politics of Resource Management**

Resource scarcity is among the most urgent issues facing the region and yet few political scientists address this topic. The politics of resource management (i.e., water, not just oil) and food insecurity present promising and policy-relevant research agendas. How do different countries and subnational communities manage their resources? How have governments addressed these issues and what accounts for varied policy responses? What explains variable outcomes in resource management?

**Islamist governance**

Islamists have played a role in social safety networks for decades in many MENA countries. In the aftermath of the Arab Revolutions, they have more freedom to organize and now even control parliaments following relatively free elections in Egypt and Tunisia. Thus, they play an increasingly direct role in crafting economic and social policy. How can we characterize the positions of Islamists on economic and social policy? How and why do they vary across different Islamist organizations both within and across countries? What policies and programs do they support and implement and what are their effects on well-being? My new research project, entitled “Is there an Islamist Governance Advantage?”, addresses these types of questions. Many journalistic and scholarly accounts allege that Islamists provide social welfare and public goods exceptionally well, often better than states. First, is this true? Second, if so, why? Third, what is the impact of Islamist social service provision on the political behavior of beneficiaries, their families, and residents in catchment areas? Does it “buy” support and induce citizens to vote or engage in other forms of political mobilization organized by the provider or its Islamist parent organization?

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An Arab Region

By Sheila Carapico, May 4, 2012

The Arab uprisings are an invitation to scholars publishing in English to refresh, re-conceptualize, and maybe revitalize the notion of the Arab region as a unit of analysis; and then, perhaps, to disaggregate it into its component sub-systems.

Such an approach is hardly unprecedented and may seem too obvious to warrant explicit attention. In addition to Raphael Patai’s mostly discredited psycho-cultural study of The Arab Mind we draw on Albert Hourani’s masterful histories of the Arab peoples and liberal Arabic thought and Samir Amin’s still-relevant political economy of the Arab Nation. More recently Marc Lynch’s study of Arabic media publics, Nathan Brown’s work on Arab constitutions and law, and Roger Owen’s new book on Arab presidents-for-life, among others, deal with the commonalities of a region that is something more than the sum of its national components.

Usually, during the past couple of decades we in the field and others outside it talk and write about the Middle East and North Africa (MENA) or, alternately, the Islamic world. None of these is more or less “real” than the other; surely they are all constructs or frames that convey certain connotations and obscure others. The “Middle East” is easy to critique (“middle of what? East of where?” as a Canadian art curator at AUC quipped) but it is a geographic descriptor of a ‘theater’ of international relations where nowadays the key actors are Israel, Iran, and the US. When we find ourselves saying “in the MENA region, with the exception of Israel, Iran, and Turkey...,” we should know that the MENA construct is impairing rather than facilitating analysis. Likewise, since Huntington’s Clash of Civilizations and 9/11 many scholars, pundits, and policy makers sought generalizations about the vast, diverse, non-contiguous “Muslim world.” This is not wrong, but obviously it privileges religion rather than politics as conventionally understood. Each of these two common frames — the Middle East and the Islamic world – is useful for some kinds of analysis but tends to obscure the commonalities and interconnections of the lands bound by a lingua franca, shared histories, and economic linkages. As Beshara Doumani contends, “Arab studies” has been a hard sell in American academia.

The popular mobilizations that began in rural Tunisia and spread throughout the region, notably with the sharing of slogans in classical Arabic like al-sha’ab yuridh isqat al-nizam and Irhal invite us to think again about the terrain of an Arab ‘region’ which, for all its diversities and contradictions, has more coherence than either the Middle East or the Muslim world. The “Arab Spring” has put the word “Arab” back in English usage, although of course it was never missing in Arabic. Frustrations and protests had been mounting in Yemen and Egypt for years, and living in Cairo or Sanaa it was not so difficult to imagine upheaval. What no one anticipated or could have anticipated was the concurrent explosion of mass expression in Tunisia, Libya, Egypt, Syria, Yemen, Bahrain, and other countries. “The Arab Spring” is a strange phrase in some ways but it usefully conveys the extent to which these were not coincidentally simultaneous parallel revolts but somehow a single collective phenomenon. As we instinctively apply the tools of comparative politics and our knowledge of the backstory in individual countries we have difficulty putting flesh on the abstract notion of “diffusion” to explain this synchronicity.

It seems to me that there is a region, or regional system, that is more than the comparative sum of its nation-state polities or the quasi-multilateralism of the Arab League. Perhaps it is useful to conceptualize it as a linguistic-legal-cultural ‘layer’ between the domestic and the global, and thus, methodologically, as a level of analysis between comparative politics and international relations. Within the Arabophone zone, so to speak, public discourses and political viewpoints are shared (or sometimes not) in verses from classical, modern, and very 21st century poetry, literature, legal texts, party platforms, musical performances, jokes, and entertainment — as the uprisings
demonstrated, even as each also had national and local idioms as well.

On a more political level we really need to understand the collective, inter-connected reactions of Saudi Arabia and other Gulf monarchies and billionaires to events across the region and in individual instances (Libya, Egypt, Bahrain, Yemen, Syria) including secretive financial channels as well as official policy positions and backing for some parties against others. The opulent ‘one percent’ of Arabs is convinced that what happens on streets as far away as Tunisia matters to them directly. The region is a system in the sense that changing events and circumstances in one part seems to affect the whole in complex ways that invite new levels of scholarly investigation.

In this regard it may also be useful to recognize that the Arab region has distinctive sub-topographies or political ecosystems each with distinguishable food, dress, architecture, dialects, religious practices, and so forth. North Africa, including Egypt and Libya as well as the three Maghrib countries, is one, where three of the five changed leaders this year. The Levant or Fertile Crescent is trickier because geographically it includes Israel and the Occupied Territories, although the commonalities of Lebanon, Syria, and Iraq are worthy of investigation. I have previously argued that the Arabian Peninsula is a sub-unit of interconnected parts that includes the tidy super-modern affluent cities of eastern Arabia as well as the messy upheavals of impoverished Yemen, whereas ‘the Gulf’ is a geo-strategic designation based on where the oil and gas resources and American firepower are concentrated.

Seeing these different pieces in terms of interlaced histories, legal systems, media publics, class relations, and politics might help future researchers to understand more fully the ways in which the revolts of 2011/12 and their denouements (like what happens next in Syria, Egypt, Yemen, elsewhere) were not isolated endogenous events in the domain of comparative politics but a regional phenomenon with interspersed but inter-related zones of calm and chaos.

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A Call for the Quotidian, the Local, the Peripheries and a Spatial Dimension

By Janine A. Clark, May 21, 2012

While political scientists have been very good at understanding and theorizing regime stability and, at the other end of the spectrum, rapid political change, recent events in the Arab world highlight the need to pursue further questions regarding slow change: the gradual social, economic and political changes at the local level that underlie rapid political change and make it possible. The Arab Spring also demonstrates the need for political scientists to look beyond the capitals and large urban cities of the region and pay attention to the region’s peripheries: the rural areas, small towns and small cities with relatively little national economic significance. To wit, the Tunisia Arab Spring was triggered by an event over 200 kilometers from the capital city. Similarly, the Jordanian Arab Spring was triggered by protests in an even smaller town than Sidibouzid in Tunisia, that of Dhiban with a population...
of only 15,000 and located approximately 70 kilometers from the capital, Amman. And in the other countries of the region, the participation of the peripheries – long assumed to be under the control of regime-supportive local elites — was critical for the uprisings’ spread, sustainability and success in bringing down their respective leaderships. This memo lays out three directions for research by which we can address these lacunae in the literature and focus on gradual changes at the local level and in the peripheries as well as their significance for regime stability.

The three research directions for capturing change at the local and in the peripheries can be conceptualized as falling along two inter-intersecting axes. The first direction for research falls along the vertical axis and involves top-down questions regarding how central strategies and the policies they produce are implemented at the local level, how local elites adapt to them and shape them, and their political impact upon the local distribution of power. The second direction for research falls along the horizontal axis and involves questions regarding the two-way economic and social exchanges or interactions that occur between large urban areas and the rural and small town periphery and, importantly, their political effects. The third direction for research falls at the intersection of the vertical and the horizontal.

Beginning with the vertical axis, with few exceptions, political scientists have conducted very little research on the implementation and impact of state strategies and national policies at the local level. What new unintended actors emerge, for example, as a result of various strategies of adaptation at the local level? To what extent have they a significant impact on the local distribution of power? What significance does this change have in the distribution of power at the local level have for regime stability? Certainly, in my own research on decentralization, I have found that, while decentralization has been implemented in only a highly limited sense (and has largely been dismissed given the limited devolution of power), it has nonetheless brought about politically significant changes at the local level. In Morocco, for example, decentralization has granted local municipalities a variety of new responsibilities — the most important of which are responsibilities for local development, including the creation of development projects, such as Public-Private Partnerships (PPPs), in order to create employment and generate income, as well as the inclusion of civil society in the decision-making process. Local governments in the periphery regularly send out tenders, small and large, engaging with private contractors and entering PPPs. While the new powers granted to local governments are often viewed as part of a larger regime strategy of adaptation and control, they create, irrespective of their size, new local political-economies — local economies that may not be reinforcing the power of regime-supportive local elites.

On the horizontal axis, the peripheries of the region have been undergoing significant social and economic changes, particularly in terms of urbanization, with the growth of rural towns and the creation and/or expansion of new industries, such as construction. While there can be no doubt that the MENA region as a whole is becoming less rural and more urban, rural migration does not flow strictly to the capitals or even to major cities of the region. Thus one fruitful direction for research would be to understand better what economists, urban planners and geographers refer to as the peri-urban interface (in recognition of the fact that urban cores and peripheries are not two separate zones and cannot be treated as such) and the political significance of the two-way exchanges between the center and the periphery that make up this space. While there are many definitions and conceptualizations of the terms, the peri-urban interface is understood as the interactions or exchanges inside the space it contains or the space itself. While geographers, environmentalists and urban planners, for example, have conducted significant work on the mechanisms, functioning and nature of the peri-urban exchanges, little work has been conducted by political scientists on their political significance. Researchers have noted the under-evaluation of the peri-urban interface as a politics of place where multiple interest groups with particular preferences struggle to impose particular meanings on homes, workplaces, communities, and natural and built environments.
This second direction for research thus indicates a need for political scientists to focus also on spatial issues in our pursuit to better understand quotidian change at the local level. Examining the peri-urban periphery — the new actors and ideas that emerge from these exchanges — is particularly suited towards capturing and understanding gradual political change and the dynamics that underlie that change. That the peri-urban interface played a role in the success of the Tunisian revolution is evident and goes beyond the exchanges created via Facebook and other forms of social media. There is emerging evidence, for example, that university students moving between Tunis and their hometowns in the periphery during their January vacation played an important role in the spreading of the protests. For the first time the MENA region is experiencing significant reverse migration, as urbanites move to the peripheries in order to access more affordable housing and commute to the capital cores. What impact do they have on local politics? How do local parties adapt to their presence? What new political forces arise as a result of these new populations? While urban to rural migration is considered one of the most important modes of migration existing in developing countries today, researchers have noted the lack of attention to the urban-rural drift and its political consequences. Similarly, as peripheral towns grow, national NGOs are increasingly establishing branches in the peripheries. These phenomena warrant significant questions: what new ideas do they bring and, conversely, how does the local impact the national? The peri-urban interface allows us to bring new actors and new ideas to the fore and question how local elites respond to them, the success to which local elites are able to adapt to these changes, whether new elites emerge, and the impact these political dynamics have on regime stability, sustainability and change. In a field that has, until recently, been focused on stability, a re-alignment toward the peri-urban interface offers the opportunity to examine the often quiet changes that underlie and potentially undermine the presumed stability.

Finally, the third possible direction for research — one which is located at the intersection of the vertical and the horizontal axes — to better capture the slow “pressure-cooker” changes at the local level that can lead to regime change, is a renewed focus on the nature and dynamics of patron-clientelism. Regime-supportive local elites maintain control through a web of patron-clientele ties that are both horizontal and vertical, involving national level state and political figures. Given economic restructuring and the on-going global recession and, largely as a consequence of this, the re-structuring that has occurred as states have implemented various versions and degrees of decentralization, how have patron-clientele ties changed? What “old” patron have disappeared and new ones arisen? What is the structure of patron-clientelism? Can it still be considered as a relatively direct flow from top to bottom? What impact are these changes having for the distribution of power at the local level? Stated differently, under which conditions do local elites no longer feel that the ties to the authoritarian leader are worth it? Under which conditions do patron-clientele ties no longer buy political loyalty?

By bringing in the local, the periphery and the spatial, we can better understand how gradual and quotidian socio-economic and political shifts can pave the way for larger national change. These suggested directions for research represent a much larger largely unexplored number of avenues for research to better understand slow change at local level and in the peripheries. They furthermore allow us to contribute to several theoretical areas, including comparative politics, social movement theory, studies of neo-liberalism, migration theory and, potentially, historical institutionalism and theories of path dependency.

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Policing, Polling Booths, and Public Policy: The Arab Uprisings and Political Science

By Jill Crystal, May 1, 2012

Policing and polling booths both present research questions that are particularly relevant in light of the events of the last year because they involve the practical mechanics of facilitating or subverting transitions and will play a central role in determining the relative success of authoritarian and democratic forces. In disciplinary terms, they range from the center (polls) to the periphery (police) of political science. The third area, public policy, is one that may take on increasing relevance as a mobilized public holds governments (elected and unelected) accountable for the services they provide.

1. Police

Police play a pivotal role in transitions and pose a particular challenge to rulers during any period of rapid change (democratic or otherwise). If leaders, new or old, are to survive a period of popular mobilization they will need a reliable, effective police force—in other words, one comprised of experienced officers. The temptation to retain the existing system of politicized police, trained to monitor and crush dissent, will be great. Even newly elected leaders may resort to existing forces, ones inclined to force, to hold power during the transition, just as the early republican regimes once retained essentially colonial place forces for the same reason.

If more democratic governments emerge, these same police forces will be ill-suited to their maintenance. They are associated with the old regime and its worst abuses and thus despised by the population. They will need to be radically reconstituted if they are to become a force devoted to protecting public order and preventing crime rather than protecting a particular regime and a particular order.

Most Middle East states have three types of police, each requiring a different approach. The first line of policing everywhere is the regular urban police trained primarily to handle traffic violations, criminal investigations, and the other demands of everyday policing. While not tasked primarily with political policing, they are nonetheless the first on the scene when demonstrations and public protest occur. Even before the uprisings their status was low because of the role they too played in policing dissent, because of often rampant corruption, and because (in some countries) they often comprise men rejected by the military. They are unlikely to willingly embrace reform, yet any reform imposed upon a resistant force will be less effective than one organized internally. The second category is the security sector housed in the Interior ministry and/or military. These forces will require more radical reform or even elimination. Finally, throughout the region the military engages in a degree of domestic policing, including political policing. A democratic transition requires tasking the military with the primary mission of external defense, a transition that will need to be negotiated in those countries where the military is actively involved in economic enterprises.

The study of policing transitions offers us as a discipline an opportunity to reclaim criminal justice from sociology where it has no business being. Not only is policing and coercion at the center of what states do but, normatively, transitions are important precisely because democratic institutions offer a check on political policing.

2. Polling stations and Rigged Elections

The days of Kim Jong Un elections are over (although the days of CIA agents arriving with bags of cash may be about to return). If elections are held with more secure technology and in the presence of outside observers, manipulating their outcomes will require a more nuanced approach than elections of the past. Unpopular leaders
who seek to retain power will need to update their skills. While this is especially true in Tunisia and Egypt, where many citizens expect to see something approaching free and fair elections, it is also relevant to countries where heads of state are not elected, but elections are held. In fact, these countries offer some useful strategies for undermining effective elections (e.g., holding free and open elections for largely unimportant bodies, such as Doha’s municipal council).

Election day itself is the final day of a long process. The pre-election phase involves such elements as crafting election laws that affect voter turnout, redistricting and gerrymandering, voter suppression, clamping down selectively on voter fraud, and shaping the news, all in ways that favor a particular party or candidate. These pre-election manipulations can be done largely legally, but they require a new skill set.

Election day involves a mix of legal and illegal strategies. A legal strategy, for example, might be inviting in multiple election monitors, then focusing on their areas of disagreement. Illegal activities however must now masquerade as incompetence (e.g., long lines, broken machines, missing material in opposition districts). Outright ballot fraud should be confined to a few swing districts where the manipulated vote is at least credible. Subverting a democratic process is still possible, but in the new environment in the region, it will require more agility and effort.

External actors will also be part of the process. The US already is; regional actors will follow. For them, the outcome is more important than the process. If the Gulf is any indication, whether a state is more (e.g., Kuwait) or less (Bahrain) democratic is far less important to Iran or Saudi Arabia than whether the government allies with it politically.

From a disciplinary perspective, the attractiveness of studying election rigging is that it can be approached from a genuinely comparative perspective, drawing on and contributing to election studies throughout the world. Everywhere elections are held, from North Korea to Norway, candidates (and especially incumbents, particularly unpopular ones) must devise strategies, both legal and illegal, for undermining the fairness of the process.

3. Public policy

Should the old order fail in its efforts to fully rig elections and retain political police, then it will have to face the possibility of more elections and the potential ouster of incumbents. Over time, elected policy makers will learn to insulate themselves from much public scrutiny if they are to survive, but in the short-term, a mobilized public may demand a degree of accountability to which even well-entrenched democracies are rarely exposed.

In this environment, to survive reelection, regimes throughout the Middle East will need to be particularly attentive to delivering services to a newly mobilized public under conditions of sometimes severe economic constraint. Interest politics may initially take a more central role as older (e.g., trade unions) and newly mobilized publics demand accountability. If schools lack books, if hospitals lack medicine, if the mines in District Twelve lack proper safety protocols, elected officials will now be held publicly accountable. The mundane politics of collecting the trash, paying the teachers, and putting out fires, not to mention the huge tasks of creating jobs and balancing budgets, will move to a more central position politically.

4. Pedagogy

This is not a research agenda, but for those of us teaching the Middle East (and perhaps offering new courses on the Arab uprisings), forums (e.g., Jadaliyya’s Pedagogy Section) for sharing thoughts and best practices in teaching this topic would be very welcome.

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Research Opportunities in Political Communication

By Emma Deputy, May 8, 2012

In this memo, I will outline what I believe to be one of the most interesting and potentially fruitful research opportunities in political communication. Then I will discuss potential pitfalls that scholars must take into account before conducting this type of research. Finally, how the study of political communication has shaped my experiences as a graduate student and what I hope it can provide for others in my shoes.

**Corpus Linguistics as an Avenue for Future Research**

In my experiences, it seems that interpretive methodology is one of the most misunderstood tools in political science today. I define interpretive political methodology as the use of methods borrowed from linguistics to understand political phenomena. One reason that interpretivism is commonly misunderstood is that we frequently focus on the qualitative versus quantitative methodology debate. This debate has produced superficial stereotypes, which I believe are responsible for completely obscuring the existence of quantitative interpretive methods. Albeit, I persistently argue that interpretivism occupies a unique strategic position that can provide us with varied tools to further enhance our understanding of political communication. One of these tools is corpus linguistics. Roughly speaking, corpus linguistics is the annotation of texts, collected by a researcher, through observational means. This practice creates a “corpus” that can be analyzed, including through the use of statistical analysis. I think that the most fruitful first step for advancing the study of political communication would be through annotating particular words or phrases and analyzing how frequently they are used. Although computational and mathematical linguistics also lend themselves to quantitative analysis in a similar manner, I consider this method as part of corpus linguistics because computation and mathematical linguistics usually presuppose the existence of a corpus when undertaking their analysis; additionally, the construction of said corpus is an essential element for advancing the study of political communication.

**Potential Pitfalls**

While corpus linguistics is statistical, and will initially appear attractive to those who bemoan the lack of quantitative data in M.E. Polisci - I want to stress that this method works better when it is combined with other interpretive methods. Just as theoretical or formal linguists have training in sociolinguistics, those who wish to engage in quantitative interpretation (conducting frequency counts) should begin their analysis with other interpretive methods including: content analysis¹, discourse analysis, semiotics or ordinary language analysis. I believe that qualitative interpretive methods will be essential in the success of most studies that attempt to use corpus linguistics to understand political communication. The reason for this is that neither politicians, nor those on the “Arab Street,” are completely blunt when discussing politics. Hence, analysis that doesn’t take this into account runs a high risk of concluding that people “don’t speak frankly about politics.” While it would be hard to argue with such a finding, it leaves out what I believe to be the more interesting question of “how does political communication function,” unanswered.

Supplementary interpretive methods can help us to solve this problem. By engaging in other types of interpretive analysis before we begin our annotation to help us understand how to find the “politics” in “political communication” (ex. we can annotate polite rhetoric in a corpus of diplomatic correspondence rather than overtly political and sensitive words, such as nuclear).

¹ I am referring to content analysis as the analysis of the senders intent, the message and receivers perception.
Growth Opportunities

I believe that the study of political communication should become a more prominent topic in M.E. Polisci. Three places where there are wonderful growth opportunities are: the public sphere, media production and official rhetoric.

Additionally, I think that seizing these opportunities will also provide us with the opportunity to work with less conventional research materials, which is something that is sorely needed in the field today. By engaging in a method where data has to be collected observationally (i.e. this should not be done on interview data, questionnaires or anything where the researcher could have applied a treatment) we can expand the pool of data being used to understand politics in the Middle East. As a grad student, I hope that this method can provide an alternative approach and chip away at the expectation that a dissertation in M.E. Polisci should consist of a secondary literature review, followed by research based on elite interviews and potentially some statistical analysis of WVS data. As someone interested in interpretive methods, I hope that this approach can expand our understanding of political practices, and that this approach can help to determine future research questions and the instruments needed to answer them.

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Youth Movements and Post-Islamism in the Gulf

By Kristin Smith Diwan, May 14, 2012

The past year of citizen uprisings throughout the Middle East has confirmed the observation of many social scientists of the emergence of “post-Islamist” tendencies, particularly among youth groups. While Islamists continue to predominate in those countries where elections have been held, notably in Egypt and Tunisia, a significant sector of youth active in Islamist movements have found common cause with liberal youth activists in pushing for greater citizenship rights and for the realization of a civil state.

While the Arab Gulf has played a less prominent role in the Arab uprisings - with the notable exception of Bahrain — the phenomenon of post-Islamism has not been absent in this sub-region. Youth movements in Kuwait adopted post-Islamist themes in their successful campaign to oust the Prime Minister Sheikh Nasser al-Muhammed al-Sabah. Saudi Arabia has likewise seen the evolution of prominent leaders of the reviverist Sahwa movement who now find common cause with more liberal human rights activists over issues of civil liberties and political prisoners. The importance of these movements resides precisely in this ability to traverse the divide preventing liberal and Islamist activists from working together to press for national reform: to impose more representative government and accountable governance on Gulf monarchies.

In prior meetings with youth activists in these countries I have heard many of the same Islamist and political thinkers mentioned as influential in the development of this evolving post-Islamist thought. Many, although not all, of these thinkers emerged from the Muslim Brotherhood or Brotherhood-related movements like the Sahwa in Saudi Arabia. However, their outreach has been distinctive from traditional Brotherhood organization in
some significant ways. First, in ideology and presentation, these thinkers display much more openness to pluralism and to the concept of a civil — not Islamist — state. Second, their appeal is transnational and clearly directed at capturing the youth. In making their appeal, many of these thinkers have made use of business-management discourse and techniques: emphasizing self-help and motivation; encouraging practical, project-based goals; and making ample use of both mainstream and new media in their appeals. Interestingly, several of these thinkers live in exile and are based out of Qatar which is seeking to gain influence amongst these youthful publics. One important question to ask, then, is what is the role in these new transnational post-Islamist movements in interstate competition — notably between Saudi Arabia and Qatar — in influencing the post Arab Spring landscape?

Literature Review

The term "post-Islamism" has taken on different meanings in the hands of different scholars. For many the term was used to describe a condition: the decline of jihadi movements, or alternatively the retrenchment of the ambitions of political Islam towards a more individualistic posture (Kepel 2002; Roy 2004). However, post Islamism as a process has been most fully examined by Asef Bayat in his comparative study of the transformation of Islamic movements in Iran and Egypt (Bayat 2007).

In the past decade, the French scholar Stephane Lacroix has identified the glimmerings of a post-Islamist movement in Saudi Arabia (LaCroix 2004). While admittedly bearing significant differences, the struggles of movement leaders working within the Saudi state religious bureaucracy holds tantalizing similarities to the Iranian case described by Bayat. While failing to coalesce as a hegemonic movement, the liberal-Islamists described by Lacroix were able to organize several petitions to the King in co-ordination with liberal movements, before retreating into the arms of the state after the occurrence of terrorist attacks in the country by Al-Qaeda in the Arabian Peninsula (AQAP). Nonetheless, the Saudi scholar Saud Sarhan, has recently noted the re-emergence of this trend through the influence among the youth of a dissident sheikh of the Sahwa movement, Muhammed al-Ahmari, who is currently residing in exile in Qatar (Sarhan 2009).

In the past two years I have observed a post-Islamist turn in Kuwait, emerging through the interaction of liberal activists and frustrated Muslim Brotherhood youth (Diwan 2011). This occurrence parallels a similar dynamic observed in Egypt through the rise of the liberal Kefaya movement in 2005, and subsequently coming together in the eruption of the Tahrir Square protests that led to the ousting of the Mubarak government (Anani 2010; Wickham 2011). In both cases Muslim Brotherhood youth were galvanized through the street activism of their liberal peers and their mounting frustration with the rigid hierarchical structure of the Muslim Brotherhood organization.

These developments present the opportunity to extend Bayat’s thesis in the post-Arab Spring era through an analysis of parallel trends in the Gulf. In the case of the Gulf, I hypothesize that liberal-Islamist movements show the potential to coalesce into a forceful movement for change through their interaction with transnational sheikhs based in Qatar. The dynamics in the Gulf, then, show the potential to push the theory of post-Islamism further through an exploration of the potential to overcome the limitations of its state-society dynamics through state sponsorship — or the instrumentalization of liberal-Islamist movements in the competition for regional hegemony in the post-Arab Spring.

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Economic Underpinnings to the Arab Spring

By Eleanor Gao, May 1, 2012

When Mohammed Bouazizi turned to self-immolation in the town of Sidi Bouzid in Tunisia, he was not only protesting corrupt practices but the lack of economic opportunities in his country. As a street vendor selling fruits and vegetables, he had already been repeatedly harassed by police officials but continued with his work because he had no other employment options. He had already tried to join the army and applied for several other jobs but had been refused for all of them. In the town of Sidi Bouzid, the unemployment rate was reportedly as high as 30 percent.

Bouazizi’s own lack of economic opportunity and the centrality of economic grievances among protestors during the Arab Spring suggest that these issues may have played a part in inciting protests. It may be worth
investigating in future research projects whether there were any economic underpinnings to the protests in the Arab Spring. Did countries that experience protests suffer from worse economic predicaments and if so, what aspect of a country’s economic development is most important in understanding whether or not protests took place? We know already that neither the poorest nor the richest of the countries in the Arab world (in terms of GDP) experienced uprisings but that most protests occurred in the middle income countries. This suggests that it is not the absolute level of economic growth that matters but perhaps other factors such as economic inequality, unemployment level especially of certain sectors of society, or the lack of food security.

For instance, despite an annual growth rate of 5 or 6 percent, Egypt, Syria, Tunisia, and Libya experienced widespread demonstrations. Were there higher levels of inequality between the wealthy and the rich in these countries? Similarly, the presence of a youth population bulge coupled with high unemployment rates amongst this demographic sector meant an entire generation of dissatisfied citizens. With regard to food security, some scholars have noted that the recent protests coincided with a rise in global food prices. As the Middle East and North Africa imports more food per capita than most regions and also have high levels of population growth, these countries are particularly sensitive to hikes in food prices. However, these have also been persistent problems and so it is important to determine whether economic matters were particularly exacerbated at the time of the Arab Spring.

Perhaps the main issue at stake is not economic development at all but how government resources are used. One question is whether governments can purchase loyalty through increasing public sector employment and salaries, reducing taxes, and augmenting food subsidies. We know that some of the wealthiest countries in the Arab world such as Saudi Arabia, Qatar, the United Arab Emirates, and Kuwait experienced relatively few if any protests. These oil producing states have been able to provide their citizens with comfortable economic conditions. But does this practice extend to other states in the Arab world which are not ostensibly so wealthy and does this strategy discourage citizens from demonstrating? If a high percentage of citizens are employed by the state and benefit from generous subsidies on staple products are they more reluctant to challenge authorities for fear that they will lose their employment as well as benefits?

In Jordan, protests over the removal of subsidies led King Hussein to liberalize politically. In a similar vein does a thriving private sector free citizens to express their opinion? If citizens are not dependent on the state for their economic welfare, will they be more forceful in demanding reforms? We also have to keep in mind though that the existence of a vibrant private sector may also discourage protests because of the potential instability and disruption it will create for businesses.

On a similar note, it may make sense to examine whether quality of governance impacts the duration or intensity of protests or whether protestors demanded a new regime or reforms. Aside from the level of democracy and degree of repressiveness which are often used to assess whether there are reasons for grievance, we should also investigate whether a government’s provision of social services such as the existence of a national health care system, access to decent education, and investment in public infrastructure affects level of grievances. Were the countries that managed to avoid revolution or civil war the same countries where robust public services are in place?

Contextually, these questions concern the role of grievance in social mobilization. Much of this literature emphasizes relative not absolute levels of grievance and for the likelihood of protesting, on the individual level, to be based on whether or not they are embedded in social networks. But when do grievances reach a critical level? Similarly, these questions also touch upon the issue of rentier states and whether subsidies and economic benefits can be used to purchase citizen loyalty and discourage mobilization for political grievances.

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The Arab Spring and Political Science

By Michael Herb, May 23, 2012

There are lots of interesting questions raised by the Arab Spring: I will focus on three.

Diffusion
Political scientists who study the region have not talked a lot about diffusion in the past. But it is hard to make any sense of the Arab Spring without a theory of diffusion. There were, of course, underlying structural factors that help to explain the protests experienced by these regimes. But they had been quite stable for decades before the spring of 2011, and the timing and character of what happened in these countries must take into account the independent causal power of diffusion.2

The utility of repression
Up until 2011, repression seemed to be a reasonably reliable strategy for Middle Eastern regimes. Many employed repression liberally and, while this had costs, those costs did not often include losing power. The spring to 2011, however, did not turn out well for rulers whose initial responses to protests were heavy on repression and light on concessions. Qaddafi met protests with furious violence, and wound up in a civil war. The king of Morocco met protests with a speech that responded to the demands of protesters, and prevented the further spread of protests.

There are also compelling questions generated by citizens’ responses to repression. I am particularly struck by the persistence of protest against the Syrian regime despite the very high costs of dissent. The issue of repression, and responses to it, seems to be an issue that could benefit from attention from political scientists who study the region.3

Regime type and regime vulnerability
One of the central questions of the Arab Spring is why some regimes proved vulnerable to the wave of protests triggered by the fall of Ben Ali, and why some did not. A number of observers have pointed to regime type, and in particular to monarchism: the regimes most seriously affected by the Arab Spring were not monarchies, with the exception of Bahrain. Other regime types might also be important in explaining the pattern of the spread of the Arab Spring, particularly the sort of regimes that prominently featured a president. Arab public opinion in the spring of 2011 was arguably much more hostile to presidents that friendly to monarchs: very few regimes that had a strong president (or president-like figure, such as Qaddafi) survived the spring unscathed.

This argument, however, relies upon (1) a coherent theory of regimes and (2) a plausible causal mechanism explaining why monarchies are different from other sorts of regimes. Some of the monarchies were beginning to look a lot like monarchies before the Arab Spring, and the “presidential” regimes were in some ways a disparate lot. If regime type is important, there needs to be clear distinctions between regimes. We also need a specific causal mechanism explaining why monarchs were less vulnerable than presidents. It would, of course, be best if this causal mechanism did not rely on fuzzy references to legitimacy.

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This demonstration of the power of diffusion suggests that we pay attention to other processes of diffusion in the region, especially those that happen much more slowly and thus are not so clearly a result of diffusion. The grooming of sons by presidents had something to do with cross-border imitation. The primacy of the president in a number of Arab political systems (before 2011) also owes something to diffusion. The institutional similarities among Gulf monarchies result, in part, from cross-border learning.
Twenty questions about the Arab Spring

By Amaney Jamal, May 22, 2012

The Arab Uprisings raise a series of issues that speak to multiple debates. This memo will highlight some of the most salient questions about the Arab Spring. The queries below revolve around ongoing (some new and some old) empirical and theoretical debates.

1) What were the factors that led to the Arab protests movements? Were they structural or agential factors? How about international?

2) We are told that there are more differences than similarities in Arab Spring protest patterns? Can we then talk about an Arab Spring as a “unitary” phenomenon?

3) Is there room for different classification schemes:
   a. Regime change?
   b. Democratization?
   c. Authoritarian consolidation/entrenchment?
   d. Hybrid regime?
   e. Populism anew?

4) Where we didn’t see protest: What can we say about these regimes?
   a. Rentier States
   b. Aid Dependent Entities (?)
   c. PNA
   d. Jordan
   e. Iraq
   f. Morocco

5) Social media and its role?

6) Satellite news and its effects.

7) Role of militaries in sustaining authoritarian rule.

8) Nationalist legacies and what role did they play? (Protests in nationalist states).

9) What role did economic vs. political grievances play? (And how might this affect the future course of citizen participation/engagement)?

10) What of Qatar?

11) What of the Salafists?

12) How homogenous is the Muslim Brotherhood?

13) How does one develop/institutionalize political parties/political party systems?

14) Is this the beginning of a revolutionary tide?

15) How do we study mass mobilization? Are there new tools other than public opinion and civil society?

16) How might we create an index of societal grievances that captures the nuances on the ground in the Arab World? Is there room for an analytical framework that captures overlapping grievances? Would this be a good way to think about “tipping” potential?

17) The most dominant explanations explaining authoritarian persistence revolve around:
   a. Oil
   b. Repressive capacity
   c. Islam
   Have these factors changed?

18) What about the political economy of the region:
   a. Post-structural adjustment?
   b. Increase in price of oil?
   c. FDI?
   d. Globalization?

19) What do theories of transition tell us about the particular historical juncture that now confronts the region? What are the prospects for movement towards democracy? What factors will matter?

20) What of economic development? Under these circumstances how likely are we to see successful economies in the region?

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Approach and substance in Middle East political science

By Manal A. Jamal, May 17, 2012

Events of the past 18 months necessarily require that as Middle East political scientists we reassess the state of the field. Though, it is too early to definitively ascertain the scope of new research opportunities, a few issues/suggestions might help us expand the parameters of our current discussions.

My comments can be summarized in two broad categories pertaining to approach and substance.

Approach

1. To move away from the “defensive mode” to which we have often been pushed:

   - Because of the persistent misunderstanding of the region, our work has sometimes tended to be defensive, justifying certain practices, and presenting more palatable understandings and portrayals to Western audiences. I am wondering if it is time to abandon this approach in our research.

2. To engage in more cross-regional research with the objective of firmly situating Middle East politics in broader comparative politics debates:

   - Current developments lend themselves to cross-regional research, and require moving away from modes of research which reify the “exceptionality of the region.” My objectives here are both epistemological in terms of how to better understand the region, and also political.

Substance

1. Program of systematic, theoretical engagement of the democratic transitions and consolidation literature, again with the objective of placing Middle East politics as central to these debates, and not simply as an exercise of hypothesis testing:

2. Relating to this body of literature, I believe we have much to gain and contribute pertaining to the economic determinants of transition, especially relating to the role of business classes, or more generally the ‘bourgeoisie.’ In the broader comparative literature, as well as in key works of Middle East politics, scholars have argued that the role of the “bourgeoisie” is critical to any democratization process. Cursory evaluation of both Egypt and Syria suggests otherwise. Moreover, it is timely to investigate in more detail the relationship between neo-liberal economic reform and economic crises and unfolding developments. In the next few years, there will also be increased opportunity to explore emerging conceptions of hybrid regimes, and how these may diverge from existing understandings.

2. More in-depth examination of different social movements, especially focusing on developments in non-political centers:

   - Departing from earlier work, it is incumbent that this research is non-elite driven, and though not necessarily ethnographic, there is much to gain from in-depth fieldwork. Currently, for example, there are serious gaps in our understanding of different movements, such as:

   - Student groups at non-university levels, especially at the secondary and preparatory levels, and a better grasp of their relationships to broader political groups such as political parties, university student groups etc...

   - Youth movements more broadly: Much of the recent literature (and research programs) on the youth in the region has focused on the youth bulge, the need to provide employment as a way to avert radicalization and chances that this
youth might join terrorist networks. Here we are pleasantly surprised that this youth could organize "liberal revolutions." These current developments require us to reassess the very points or departure that have grounded our work on youth thus far.

- Salafist movements, and factors which account for their appeal: For example, cursory research conducted on Salafists in Egypt by Yasmine Moataz Ahmed, a PhD candidate in social anthropology at Cambridge University, indicates that their followers are drawn to them because of their more "open," sometimes more "liberal," and less rigid interpretations of Islam. Followers believe that by embracing the way of the prophet, they are in fact adopting more flexible approaches to Islam. Obviously, these findings appear almost counter-intuitive and remind us of the serious gaps in our understanding of Salafists and their supporters.

- Local and village councils: It is critical that we orient our research to non-urban centers. The world of social movements looks starkly different in non-political centers.

Give recent availability and access to official Iraqi state archives, there is now ample opportunity to learn about the organization of the Baath at sub-regional levels.

- It is also timely that we re-examine how we have approached the study of different social movements. Very importantly, we may want to reconsider how we have understood the relationship between social movements and civil society. Similar to Mamdani’s earlier criticism of civil society debates in South Africa, the discussion to date in Middle East circles has often been more programmatic and ideological, rather than historical and analytical. Moreover, we have obviously over-emphasized and over-focused on Islamists at the expense of other movements. This may also be an opportune time to focus more intently on emergent social movement coalitions, and factors which shape these coalitions etc…

3. At a later date, we may want to re-examine the literature on revolution, especially focusing on notions of “leadership” — I think there are important theoretical contributions to be made pertaining to this dimension.

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The Arab Spring and New Approaches to Social Movement Research

By Adria Lawrence, May 23, 2012

The events of the Arab Spring have not rendered existing investigations into mechanisms of authoritarian rule or problems of political economy obsolete, but they have pointed to new opportunities for scholars with expertise on the Arab world. Two of those opportunities concern the way that we approach the study of politics in the Arab world. In what follows, I discuss how the Arab spring can shape the cases we study and the methods we use to study them.

Rethinking variation

Scholars of the social movements in the Arab world have an opportunity to widen their empirical analyses to different time periods and cases. I begin by suggesting that history can provide useful comparisons. The mass protests of 2011 only appear unprecedented if one is focused on the most recent decades. The exceptionalism literature has posited that Arab populations are unusually apathetic and disengaged from politics; such descriptions were heard about Egypt right up until Egyptians flooded Tahrir square. The events that occurred across the region challenge claims about exceptionalism; even where regimes were not overthrown, people took to the streets to demand change in the majority of cases. The extent of mobilization across the region stunned observers.

Yet the exceptionalism argument was always a bit of a straw man, dependent on untested cultural propositions or unexplained large-n correlations. In fact, there is a history of resistance in the Arab world, much of it to what was perceived as outside dominance. Populations acted collectively against foreign invasion, to bring down colonial regimes, and in defense of Palestine. Throughout the 20th century, they also engaged in religious revival movements, joined religious brotherhoods, and formed leftist parties. By pointing to this history, my intent is not to diminish the current wave of protest, but to suggest that there is a historical precedent that may be useful to scholars in several ways.

First, studying past social movements can help illuminate puzzles about current movements. For instance, one of the main reactions to the Arab Spring has been surprise, the sense of “why didn't we see this coming.” This surprise is in some sense unsurprising, as a key feature of waves of contention appears to be their unpredictability.4 In the Arab world, these protests were not the first to come as a surprise. In the mid twentieth century, colonial powers were shocked when their subjects took to the streets to demand independence. In the French empire in particular, a wave of protest favoring regime change erupted during World War II. The protest did not begin when France fell to Germany in 1940, but later in the war when the French colonies were directly affected. In the French colonies where Allied or Axis forces invaded, nationalist mobilization followed.5 Yet on the eve of nationalist protest, the French believed their empire was secure and failed to predict widespread protest. The French thought Algeria, for instance, was and would remain French. They had ruled Algeria for over a century and expected French rule to last. Only in retrospect do the nationalist protests against colonial rulers appear inevitable; in a few years time, the anti-Mubarak and anti-Ben Ali protests will, in all likelihood, be likewise proclaimed unavoidable. Recognizing that these patterns have occurred before can be helpful in identifying the conditions that trigger and precede contentious action. During the mid twentieth century, disruptions in individual countries due to wartime invasion were highly consequential; other kinds of local and international disruptions may similarly have provided an environment that facilitated protest in 2011. By looking back in time, scholars can expand their case list and uncover patterns that can help account for onset of mass protest.


5 Adria Lawrence, Imperial Rule and the Politics of Nationalism: Anti-Colonial Protest in the French Empire (Book Manuscript, 2012).
Looking at historical cases is not just useful for increasing the number of observation and suggesting hypotheses in general, it may also help us understand the eruption of the Arab Spring protests in particular. Current mobilization may be tied to past mobilization in ways that are easy to miss. In interviews with members of Morocco’s February 20th movement from fall 2011 to spring 2012, I found that core activists were linked to past mobilization via their families. Those who began the protests in Morocco came from families that had challenged the monarchy in the past, and often suffered repression from the regime. The ability and impetus to mobilize may stem from past experience and examples, not just from seeing what happens in other countries.

The Arab Spring not only prompts scholars to look back in time, but also to look outside the region. The opportunities for comparative research with the former Soviet states comes to mind. In addition, one of the features of the Arab spring is that monarchies appear to have fared far better than presidential republics. The relationship may be spurious, however, if factors like oil explain why monarchies were relatively more stable. Testing the relationship requires gathering external cases; there are simply too few cases in the Arab spring to evaluate the argument. Engaging in comparative research that looks over time and outside the region is a fruitful way to evaluate competing explanations for protest.

**Qualitative and Multi-method Approaches**

The Arab spring has encouraged engagement with comparative scholars in other regions; political scientists and the outside community have followed the events with interest. The increased attention to the region provides an opportunity for scholars to call attention to their work. It also invites attention to particular types of approaches. The large-N cross national work on democratization, oil rents, and violence in the region that have appeared in many top journals have added to our knowledge of how the region compares with the rest of the world, but have little to say about when and why changes occur, or what leads people to organize collectively. Answering these questions requires engagement with non-elites. Some of this work may be large-N (via new opportunities for public opinion research), some of it may be experimental, but much of it is likely to include qualitative methods. Scholars using qualitative methods such as archival research or field work can take advantage of the increased disciplinary attention to the Arab world to re-emphasize the contribution of qualitative methods to understanding and explaining social movement activity in the Arab Spring. Field research may be the best way to get at participation and non-participation, key questions in the social movement literature.

At a point when interest in the region is elevated, scholars can make arguments about the usefulness of qualitative and mixed methods. They can also point out the way that these methods have changed in recent years, becoming more sophisticated and attuned to criticisms and drawbacks of the methods that have come from the quantitative and experimental approaches.

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Turning to the Local: Potential Opportunities for Research, Scholarship and Programming on Development

By Ellen Lust, May 22, 2012

It is difficult to predict exactly what (undoubtedly varied) long-term effects the Arab uprisings will have across the region, but without question, the uprisings have highlighted the gaps between citizens’ expectations and their lived realities, national affluence and the distribution of resources, and the apparent resilience and strength of regimes and their inability to govern effectively. The problems that sparked the uprisings — poverty, unemployment, poor education and health services, disaffected youth, etc. — will receive increased attention. And, perhaps most importantly, governments that previously recognized these problems but failed to recognize their urgency have new incentives to improve governance.

This provides new opportunities for research and scholarship on the Arab world aimed at understanding issues of local development and human security. Of course, the study of these issues is not entirely new. To name just a few: Melani Cammett, Janine Clark and Ella Gao have done impressive work on local service provision; Asaf Bayat and Diane Singerman drew attention to politics at the most local level; and Jamil Jreisat has long made inroads into understanding public administration and governance issues.

Yet, despite attention to welfare provision and local governance, the Arab world has not been a particularly conducive region for studying these issues. It’s not surprising nor coincidence that the greatest advances in studying these issues have come from studies in sub-Saharan Africa, Southeast Asia, and even in China. These are regions in which central governments have incentives to improve local governance, and development agencies have had more extensive programming. There is also greater decentralization and deconcentration — and greater variation in arrangements — which make fertile ground for scholarship.

There are several possible arenas of research. One is to look at specific populations (e.g., youth, women, minorities, the poor and other special or ‘at risk’ populations, as well as new or emerging economic and political elites); this is somewhat well-trodden ground with regard to women, but less so for minorities, poor and youth. A second option is to focus on spatial issues (e.g., urban transformation, rural development, underprivileged regions, etc.), examining patterns of growth, security, service provision (e.g., health, education, welfare), and governance that result from transformations. Here, we find that work to date has focused more on urbanization than rural development. A third possibility focuses on substantive issue areas (e.g., local service delivery — with foci on educational reform, health, food, etc.; forms and varieties of community engagement and volunteerism; formal and informal political participation; social and economic transformations, including issues of changing business-state relations, labor, etc.; social accountability and governance; and crime, violence and security sector reform).

The point is not that these arenas are entirely new territory, but that they are relatively understudied and that (with some important exceptions), focus has been more on evaluating the conditions that exist than in explaining causes and — where underlying factors are examined — focusing more on socio-economic and cultural explanations than on political dynamics. Many questions remain regarding the interaction between formal and informal institutions, the roles of emerging vs. established elites, the effectiveness of different local governing structures, and the nature of central vs. local political incentives for service provision.

Research in this area is likely to be pushed significantly by international (donor) agendas, which have tended recently to focus on women, youth, security and other specific/isolated areas. The “piece-meal” approach
reflects the heightened concern and apparent relevance of individual “pieces” of development. Yet, it also is somewhat misleading — belying the interconnected relationships between artificially separated “development areas.” (See Timothy Mitchell, of course.) Research may be particularly useful when it examines these intersections, paying attention to the broader governance environment.

Such research may be especially useful by providing a lens through which to address questions about macro-level change. Understanding participation and accountability at the local level sheds light on the possibilities for democratization and democratic practices more broadly — especially as changes in participation spur and are spurred by changing public expectations. Changes in the relative influence of “informal” (or better, social) and formal (political) institutions give us a better understanding of the changing reach of the state. These are not necessarily “new” areas of interest — and we may want to contest/debate whether it is useful and appropriate to emphasize democratization and state strength in the post-rupture Arab world, but they are likely to receive renewed attention in the coming years.

Indeed, many questions remain in moving such an agenda forward. It is useful to consider which populations, sectors, or processes should receive priority. The relevance and appropriateness of research foci varies across countries, and in light of the changes that have and haven't taken place in the region, we may want to consider where studies are most feasible and which comparisons are best suited. (That is, are previous comparisons — often by regime type, resource endowments, etc. — still the most relevant?) Finally, we can consider which areas of study lend both substantive and theoretical leverage. What addresses relevant policymaking questions and also enhances our broader understanding of political process and institutions?

In short, turning greater attention to the local can strengthen our understanding of both local governance and macro-level changes. That these questions have been understudied in the MENA is understandable given the political constraints on research in much of the region. Yet, this is likely to change as central authorities — whether following ruptures or responding to the demonstrated possibility of further pressures — attempt to improve local conditions. It’s an opportunity for establishing better governance and service provision in the region, and for exciting new streams of scholarship.

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New Arab Media Research Opportunities and Agendas

By Marc Lynch, May 15, 2012

The Arab uprisings have featured an outsized role for various forms of media, from television to the internet and mobile phones. This promises to galvanize an already rapidly developing research agenda by offering important new questions, rich new sources of data, and the opportunity to test specific hypotheses. Thus far, however, new research in these areas has been dominated by journalists or by internet-studies generalists who often over-emphasize the significance of their favored online platforms at the expense of broader political structures and processes. There has been far more data analysis thus far than there has been innovative theory about the role of the new media. It is vital that specialists in Arab political communication step up to better contextualize the role
of media in Arab politics, and to exploit the flood of new evidence and information. This memo lays out some preliminary thoughts on the nature of this new data, and a number of hypotheses which warrant further study.

The wealth of new evidence available on the internet should not only transform Arab media studies, it should be integrated into a wide range of research programs. Twitter, Facebook, YouTube and other social media platforms offer enormous quantities of publicly available data which can be accessed to varying degrees. This data has been most often used to track information flows, for instance through linking and retweeting patterns or through analysis of the quantity and rhythms of particular phrases and hashtags (Lotan, et al; Howard, et al). A George Washington University team (including me) was the first to use bit.ly link-shortening data to track the consumption of online content (Aday, et al). Some researchers have gone so far as to attempt to predict protest outbreaks through spikes in particular protest or dissent-oriented hashtags (Howard, et al). Other important potential uses remain less developed, such as sentiment analysis of Facebook or Twitter postings and comments (which might be used to evaluate expectations or attitudes in real time) and linkages between online social media and mass media content. Facebook postings, blogs and other social media could be used in historiographical fashion as online diaries rather than as large-n data sources. There are many live-blogs and daily Storify collections, along with YouTube videos and Flickr images, which collect useful content. And the online archives of news coverage and op-eds which almost all Arabic newspapers now maintain represent an outstanding resource.

These data present unprecedented opportunities. But in all forms, researchers must be extremely careful about systematic bias in the data sources: Twitter, for instance, is likely the best suited data set for Big Data analysis since it is a self-contained universe in a research friendly format, but is among the least-often used social media platforms in countries such as Egypt (barely one percent of online Egyptians used Twitter, according to a recent survey — to say nothing of Egyptians without internet access). Facebook is far more popular, but much harder to access systematically for research purposes. This is not necessarily a fatal flaw, since the relevant population is defined by the question: if one’s question is about online activism, then online activists are a legitimate population to study. But if the goal is to generalize to mass publics, then caveat emptor. Unfortunately, the tendency to date is for researchers to acknowledge these limitations…and then to proceed with the data analysis nevertheless.

The fervor for internet studies has tended to distract attention away from broadcast media, which based on audience alone must still count as the most influential media form. I have argued, along with others, that Al Jazeera played a crucial role in unifying the Arab public sphere over the preceding decade (Lynch 2006, 2012) and in spreading both a pan-Arab identification and a discourse of dissent. The iconic image of the Arab uprisings, in my view, is the Al Jazeera split screen showing simultaneous, identical protests in multiple Arab countries. But broadcast media tends to be less amenable to systematic quantitative analysis, and after all this time we still lack for the most part even rudimentary systematic content analysis, audience research, or careful tracing of impact on political attitudes or behavior.

There are plenty of important questions to explore here. There is a widespread view, for instance, that al-Jazeera has lost credibility over the last year through its advocacy on Libya and Syria, ignoring Bahrain and closer alignment with Qatari foreign policy — but nobody has yet produced systematic evidence of that alignment, of its coverage of these cases, or of its lost credibility. Another important question would be the relative weight of international, transnational, and local television: have local television stations risen in some cases to challenge Al Jazeera and the other satellite television stations? Have international stations such as America’s al-Hurra, Russia’s RT, the UK’s BBC Arabic had any demonstrable impact?

The most obvious new area for research would be on the Arab uprisings themselves, attempting to parse out the causal impact of various media forms on protest mobilization, regime responses, violence, or democratic transitions. It has been widely argued that
social media radically reduce the transaction costs for protest organizers, allow for the more efficient spread of information, help to reveal previously private information about political attitudes, and allow the construction of ties of solidarity previously blocked by authoritarian regimes (Lynch 2012). It is worth considering the differential impact of these media on mobilization vs. democratization, on protest organization vs. formation of civil society and parties, and the different uses and impacts of the media on activists vs. mass publics. It should not perhaps be surprising that groups able to mobilize to punch above their weight struggle to replicate their success in mass electoral politics.

Research might also look at the structural effects of changing media environments, such as the impact of increased but unregulated media freedom (Libya) or the maintenance of control over state media during a transition (Egypt). How do these media affect perceptions of security, of the legitimacy of the transition, or of emergent political forces?

Another obvious research area for the Arab media is its role in the diffusion of protest across borders (Patel, et al). It is possible to imagine systematic tests of various hypotheses here: can we trace the original appearance of particular protest forms or slogans, and their diffusion across different cases? Can we demonstrate the conditions under which protest in one country are inspired or deflated by events in other Arab countries rather than by events on the ground? And what explains different patterns of attention to these cases — why do protests or videos from one country gain traction with international audiences while others remain obscure?

There is also the strategic dimension to these media, both within cases and across the region. Have media such as Al Jazeera and Al Arabiya proven to be an effective tool for regional soft power? While data remains anecdotal, there are fascinating questions to be asked about the media framing war between the Gulf media and the Syrian regime media, and their differential reception inside of Syria and in the broader Arab world. Another worthwhile question would be about the efforts by regimes to discourage coverage of a conflict, such as the Gulf media treatment of Bahrain: do these succeed or are they overcome by social media attention? My current research project focuses on how the ubiquity of videos of violence from Syria and around the region affects political norms and behavior: does the availability of these videos make intervention more likely or encourage the development of a norm against violence? How do questions about the credibility of these videos and their strategic use as a form of political warfare affect their reception or their impact?

In short, the availability of both unprecedented new data sources and a wealth of intriguing hypotheses without obvious answers should lead to a flourishing of the field of Arab media studies. But this will require deeper engagement between area experts and general media scholars, a willingness and ability to exploit these new data, and far more attention to theory. If the new media is helping to constitute a new Arab public sphere, what are its key characteristics and dynamics? What is the relationship between transnational networks and national publics? Are some groups differentially empowered by the new media, or will the new media over time tend to reflect some “real” balance of social and political power? This memo has raised many more questions than answers — which is, hopefully, a good sign for an emergent research program.

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Select References


Measuring the Military? Obstacles & Resources for Studying the Armed Forces Amidst the Uprising in Egypt

By Shana Marshall, May 17, 2012

Theorizing about the military and security apparatus of any state is a daunting task, but this is especially true in Egypt, where restrictions on media coverage and the absence of official documents have produced a virtual information blackout. The result has been the persistence of vague estimates regarding the Egyptian military’s “share” of the economy and a general disagreement over how to measure its institutional impact on domestic politics. But the recent uprisings have not only made clear that the Egyptian military does indeed play a critical role in the domestic economy and the political sphere, it has also generated new opportunities to gather data on the army’s economic footprint.

Since the Supreme Council of the Armed Forces (SCAF) has taken over as Egypt’s interim ruling authority, the military’s revolutionary role has gone from being “one hand” with the protestors to being the focus of intense scrutiny regarding its economic operations. What was once a taboo subject seen as parenthetical to the rampant corruption and fiscal failures of the Mubarak government has become the focus of countless articles in the Egyptian
press; hundreds of entries by prominent Egyptian bloggers and ordinary networked citizens; a series of twitter hashtags; activism directly targeting public disclosure of the military’s business enterprises; revelations from disenchanted mid-ranking officers; and (in response to these phenomena) periodic disclosures and appeals for solidarity from the military’s own economic managers.

This reinvigorated scrutiny of the military’s economic empire has come at a time when many of the military’s assets are easier to identify because new information sources are available. Although area scholars have made good use of traditional sources of information on the army’s economic operations — notably U.S. Government documents — these paint an incomplete picture, largely due to the military’s successful efforts to diversify its economic portfolio beyond its traditional state-owned manufacturing and agricultural operations. Fortunately, these same expansionary efforts have produced a range of new information sources on the economic operations of the armed forces. These include literature from trade associations representing the energy industry, manufacturing, agriculture, petrochemicals, and other large sectors where the military has investments and joint operations; press releases and annual reports issued by individual private sector firms and foreign conglomerates doing business with military subsidiaries; fee-based business intelligence publications; reports by regional financial institutions; literature produced by professional public relations firms; and online employment history profiles.

However, the same diversification drive that has led to a proliferation of available data points on military businesses has also made it more difficult to categorize these economic assets in terms of their contribution to institutional power, since they are more widely dispersed across sectors and based on more varied ownership models (including joint ventures, minority stakes, public-private partnerships, etc). On what basis do we classify a discrete economic venture as a “military business?” Is it enough to demonstrate that the board of directors is composed of active (or retired) generals? What about operations in which the ministry of military production has only small minority shareholdings? What threshold constitutes meaningful ownership? While we should not devote all our time to developing a comprehensive quantitative measure of the military’s economic empire (which in all likelihood would be inaccurate anyway), attempting to answer such questions will help us to better conceptualize Egypt’s military economy. This would include getting answers to questions like: Who makes decisions about how to dispose of the military’s economic assets? and, How does the private “officer economy” contribute to the military’s institutional political influence?

Those with expertise in studying political economy and/or the Egyptian army should start by identifying and collecting these new potential information sources and examining how scholars might tease out theoretical implications based on their contents. Combining these externally generated sources of information with on-the-ground efforts by activists to document the military’s presence in a variety of Egyptian firms may bring us closer to a full understanding of the army’s economic empire. The momentum of anti-SCAF activism may prove to be an important driver of new data production — but it may also be ephemeral, since many anecdotal reports and collections of images and documents may disappear as activists and journalists move on to cover new events and focus on new agents of Egyptian politics.

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New methodological opportunities for political science research

By Quinn Mecham, May 1, 2012

New political dynamism in the Middle East has created an enormous number of research opportunities; given the comparatively low numbers of well-trained, active political scientists doing social science research on the Middle East, I believe it will take a decade or more to understand the political processes that are currently unfolding. Likewise, I believe that we are in for many political surprises over the next decade that will continue to attract new ideas and shift us in other directions. Political changes in the Middle East have created new methodological opportunities for political science research because of the availability of new data, as well as a number of new topics of inquiry. Even some of the standard research subjects in our field (e.g., authoritarianism) will require substantive revision and debate. I have divided this memo into five new methodological opportunities, and ten new (or revised) topics worthy of focus, below.

New Methodological Opportunities

Variation! — From a social science perspective, recent political changes have created new and fascinating variation across cases (country, organization, political event) in domestic political environments known for their comparative stability. New variation in regimes, political mobilization, electoral outcomes, and international relations gives us additional leverage to credibly test arguments. This includes well-known political science arguments that emerged from research in historically more data-rich environments (such as Europe and the Americas).

Opinion Polling — In a number of countries where we knew little about public opinion, credible opinion polls can now be taken. This requires substantial resources, but it will be fascinating to see what we learn from statistically credible polling that we previously only suspected or even got wrong.

Voting Behavior — In countries that have recently held (more free and transparent) elections, we can observe voting behavior without the same level of noise from regime interference.

Network Analysis — We have more evidence from social media about interpersonal connections and how those connections shape political outcomes than ever before. There are huge data opportunities on social organization here that can be taken advantage of with creative research designs.

Ethnography — Though ethnography has been one of the staples of good political science research in the region, a number of places have been virtually untouched because of highly constrained research environments that prohibited assessment of important political topics. It is time to do much stronger research on the domestic politics of Libya, Tunisia, and Iraq than we have previously done.

New (and Revised) Topics

Authoritarian Governance — Authoritarian regimes are best studied in the face of challenge, because that is when governance strategies are most clearly negotiated and revealed. It doesn't appear that this topic will become irrelevant anytime soon, although the costs of maintaining these regimes have become increasingly clear.

State Failure — Attempts at reconstructing weak states like Libya and Yemen will be ongoing for some time, but both states have lost a monopoly over the use of force and it will be very hard for them to regain it.

Social Movements — Youth, Islamist, labor, and other types of social movements in the Middle East have been extraordinarily active and there is a wealth of new data for understanding how they operate and when they are successful.
Information and its Dissemination — One of the early stories of popular revolution has been the diffusion of information both through traditional media and social media. How information aids in coordination, framing, and solving collective action problems is a big story that we need to tell well.

Liberal Reform — Leaders of the Middle East have recently adopted a number of liberal political reforms, both proactively and reactively. These reforms change the rules of the political game, and new rules lead to new political experiments, new actors, and new processes that are all poorly understood in newly reforming countries.

Constitutional Design — Constitutions are negotiated documents that have historically been ignored or been deemed comparatively irrelevant to understanding de facto political processes. As constitutions become the site for high politics in places like Tunisia, Libya, and Egypt, they will become both interesting and consequential.

Political Parties — Every country that has prepared for a new “founding” election has seen an explosion in the number of political parties. What parties emerge and what parties succeed? How do they distinguish themselves from one another and formulate political strategies?

Civil Society — Though this was a major focus of the field in the 1990s, it should become one once again. As civil society groups find new spaces in which to operate, they will matter for political outcomes, especially in urban environments like Tunis and Cairo.

International Relations — Old relationships within the Middle East are being upended and are now in a state of considerable revision. How has Turkey been forced to change its international relations in the face of uprisings against Libya and Syria? How do the dynamics between Saudi Arabia and Iran change as a result of Bahrain’s uprising? Why do Middle Eastern countries support uprisings in some cases and condemn them in others?

International Organizations — The Arab League and even the GCC suddenly matter in enabling political outcomes. What do dynamics of Arab League members say about the potential for international pressure to impact domestic political outcomes in a place like Syria? How critical was the GCC plan to President Saleh’s resignation in Yemen. Why was a NATO coalition formed quickly and consequentially in Libya, but not elsewhere?

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Understanding political economies of the 2011 uprisings

By Pete W. Moore, May 23, 2012

Prominent among the grievances of the 2011 Arab uprisings have been socio-economic. But these complaints are not unprecedented. While new forms of mass politics have come to the fore in a number of countries, the uprisings of 2011 should not be analyzed as singular events. Protests infused with socio-economic grievance and responded to by force from central authorities have a long history in the region. Nevertheless, the temptation to provide a “why now” economic explanation has generated three seemingly opposed arguments. One set of arguments ascribes the uprisings’ start in Egypt and Tunisia to economic development, or modernization more
generally with the attendant health and literacy gains. A second argument credits the strangulation of economic freedom as generating uprising or in the words of World Bank President Robert Zoellick, “The late Mr. Bouazizi was basically driven to burn himself alive because he was harassed with red tape.” A third set of critical responses casts a pox on both houses emphasizing the failure of neoliberal policies and increases in inequality and poverty as driving protest. It’s quite likely that future scholarship will adopt variants of these arguments to debate the economics of 2011. However, I want to caution against this temptation. For one thing, a cursory review of the economic data finds ample evidence for each of these claims. The Arab world has experienced economic growth, increased health and educational achievement, income inequality has leveled off (or at least not worsened); however country specific indicators of poverty, related social indicators, and perceptions of inequality has worsened, particularly during the region’s neoliberal period.

I argue that the attempt to find an economic smoking gun to explain the uprisings is likely a dead end because these arguments confuse symptoms with causes. What binds the Arab world in political economy terms and shapes the socio-economic grievances of 2011 are fiscally underdeveloped states with uneven administrative capacities. The historical inability and unwillingness of Arab rulers to extract and redistribute resources domestically has wide reaching implications for understanding the path to 2011 and after. Well before the uprisings, fiscal weakness evolved — to varying degrees — into strategies of rule for many regimes; purchasing political security with decades of boom and bust economic growth but little development. Weakness here should not be taken as uniform, rather it varies across and within particular country settings. In the last two decades, Arab states have attempted to compensate for the side effects of fiscal weaknesses with measures that have underpinned the socio-economic grievances and dynamics of 2011. Seen from this vantage, informal markets and corruption did not infirm states, rather weak states fostered the conditions for grey market growth. And while some Arab regimes made promises that later feed unmet expectations, it is state weakness which impairs the ability to meet those expectations. The rapid spread of protests, particularly the role of organized labor, is in part linked to decades of intra-regional labor migration, itself a coping tactic of fiscal weakness. Protests of corruption, inequality, red tape, and labor insecurity therefore share a common historical source, fiscal underdevelopment. There are periodic exceptions to Arab fiscal weakness, but like much of Africa, Latin America, as well as parts of Asia, fiscal weakness is the institutionally sticky norm and does not change easily.

A second claim here is to suggest inclusion of some of the tools of fiscal sociology as an important alternative to diagnosing the sources of fiscal weakness and linking them to issues of current political economy importance. Joseph Schumpeter summarized fiscal sociology as the study of how a state collects its revenue influences its basic politics. Attempts by social scientists to explore the relationship between taxes and the Arab state have been limited. In particular, political scientists studying the Arab world employed an abbreviated form of Schumpeter’s formulation emphasizing ecological factors like oil (or other rents) and war to explain Arab state weakness generally and fiscal impairment specifically. Dependence on oil forestalls domestic extraction and hobbles administrative capacity; war retards institutional development and enhances reliance on outside patrons. Understanding the 2011 uprisings requires a return to a more comprehensive and critical political economy. Fiscal sociology offers an advantage in these respects as it recognizes state capacity is relational and not simply a uniform condition which terms like autonomy, strength, or weakness often convey. In other words, sociological approaches call for a broader consideration of “the political economy” which makes room for “the complex social interactions and institutional and historical contexts that link state and society in ways that shape fiscal policies and their effects.” In critical terms, future research could unpack the historical and institutional evolution of taxation, labor rights, and property rights as important elements of fiscal politics; how responses to these conditions in the last several decades laid the
groundwork for 2011; and how these dynamics have been shaped by changes at the regional and global levels. However, scholars would be well warned to treat this aggregate data (produced by fiscally and administratively weak states) more critically than in the past. Extraction and distribution data is riddled with error and uneven measurement, problems that should not mean rejecting the data but rather learning what the errors tell us.

A final take home claim is that if the fiscal weakness diagnosis is accurate, then important implications for post-2011 transitions follow. Because Jordan, Algeria, Morocco, and Palestine suffer similar effects of fiscal weakness as Egypt, Yemen, and Syria, the uprisings of 2011 are likely not spent. On the other hand, for those countries which appear to have entered an era of mass mobilized politics, continued fiscal weakness is likely to spawn impediments for political transitions. New political leaders can be expected to emphasize ways to turn back on the external revenue faucet and delay politically threatening institutional restructuring. The speed of political demands by a newly empowered citizenry will likely outstrip the pace of economic readjustment and reform. Fears of precocious democratization will strengthen the old guard and caution against “radical” change. External powers having nurtured weak Arab states for decades are unlikely to shift tactics and are more likely to double down; thereby lessening the symptoms but leaving intact fiscal weakness. The least costly and most attractive option will remain adherence to a strategy of weak fiscal rule as long as international legitimacy guarantees access to rents.

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Re-thinking the Politics of Distribution: Lessons from the Arab Uprisings and the Lack Thereof

By Gwenn Okruhlik, May 2012

The Arab uprisings, and the relative lack thereof (not to dismiss the many protests that have occurred), in the states of the Gulf Cooperation Council (GCC) confirm the importance of distribution and prompts political scientists to re-think the subject. The oft-utilized rentier framework vastly overstated economic determinism. In reality, money does not spend itself. Choices are made by state authorities about who gets how much when.8 We know that a legacy of biased choices can fuel popular dissent, even “rising up” against the state. We need to systematically integrate the politics of distribution with the resilience of authoritarians in crisis. I think that particularly fruitful avenues might incorporate ideas about state building, sectarian politics, labor, citizenship and the construction of narrative. This is not just a domestic question; rather it concerns cross-border politics.

Distributive politics are resurgent across much of the Peninsula. Oil and gas rents are strategically distributed to inhibit or squash the encroachment of uprisings. For example, there are massive housing schemes in Saudi Arabia and Bahrain; a 60 percent salary increase overnight in Qatar; food subsidies and outright grants to all Kuwaiti

citizens; debt absolution and wage increases in the United Arab Emirates (UAE), and in Oman, unemployment grants, student stipends and 50,000 jobs. All of this provokes a curious sidebar: what if the Arab uprisings had happened in, for example, 1998 when oil hovered around $10 per barrel and sovereign wealth funds were not yet fully developed? Would the region look different?

The uprisings offer us an opportunity to ask new questions. The obvious one is about how uprisings (and the lack thereof) vary between distributive and non-distributive states – between the haves and have not’s (e.g., Egypt and the UAE). Less obvious is to inquire about differences between the distributive states and how that affects political discourse (Saudi Arabia and Qatar); the circulation of rents among the distributive states (from Saudi Arabia, Qatar and the UAE to Bahrain and Oman), and rent flows outward from distributive states to allied non-distributive states (from the GCC to Jordan and Morocco). The diffusion and circulation of wealth takes various forms. In the last year or so, there has been ample discussion of the diffusion of protest. There is also a diffusion of repression, money, narration and sectarian identity.

Since distributive politics involve more than material commodities, I am interested in the politics of strategic naturalization and citizenship, especially as it relates to sect. Bahrain has naturalized many Baloochis, Syrians and Jordanians who are Sunni to counterbalance the Shi’a majority. Saudi Arabia naturalizes Sunni populations to balance the Ismaili population in the south. Kuwait has naturalized thousands of Sunni Saudis. The UAE is likely to naturalize Sunni bidoon. There is heightened regional and international flow of people across borders for explicitly political, sectarian reasons. As well, there are interesting dynamics between sect, state and large foreign worker populations. The state plays migrant laborers and sect off each other. This is especially evident in Bahrain.

I recently returned from fieldwork in Bahrain, Kuwait and Qatar and was struck, in the first two states, by the construction and diffusion of a new narrative made evident in the display of flags and portraits of rulers. Neighborhood demographics matter. Residents hang portraits of Sheikh Hamad al Khalifah and King Abdullah al Saud side by side, or Amir Sabah al Sabah next to King Abdullah. I found the flags of Saudi Arabia, the UAE and Bahrain flown together in Manama on the eve of the anniversary of Lulu Roundabout.

While observers long analyzed distributive politics and sect vis a vis individual sovereign states on the Peninsula, some of the regimes are trying to re-frame this issue to be sect vis a vis the Gulf Cooperation Council. Supra-territorality is evident in the speeches of senior Princes, in the GCC response to the uprising in Bahrain and in a thinly veiled suggestion of an extra-territorial response to uprisings in the Eastern Province of Saudi Arabia (though the plan for a Union has now been postponed).

These observations from the legacy of the Arab uprisings in the Gulf invite political scientists to step aside from rentierism to the study of the politics of distributive states thereby eschewing economic determinism in favor of strategic choice. Such a focus allows us to examine politics both from the bottom up and the top down; that is, the political economy of the origins of protest and of authoritarian resilience. Further, distributive goods are not just monetary. If we insert the more ambiguous but critical concepts of narratives, rights, citizenship and belonging into a distributive framework, it will yield rich analyses. Finally, though state strategic choices in the Gulf often reflect sectarian politics, comparison and extrapolation to other contexts may incorporate identities such as gender, ethnicity or region.

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An opportunity to revisit the political culture baby that drowned in the bathwater

By David Siddhartha Patel, May 8, 2012

American troops and analysts who served in Iraq often describe it as a “tribal society.” Based on their experiences (and decades-old books by Gertrude Bell…), this inference might seem reasonable. Those who served, particularly after 2006 in the co-called Sunni Triangle, interacted with tribal leaders who were influential in the sense that they often could get members of their tribe to do what they asked them to do. Many officers say they worked with sheikhs because they were powerful, but few understood that those sheikhs were powerful precisely because U.S. forces worked with them. Calling Iraq a “tribal society” describes an equilibrium in which tribal sheikhs lead and tribesmen follow. But, what sustained this equilibrium of tribal (or, more broadly, cultural) authority? Did Iraqi tribesmen obey their sheikh because of deep cultural values and orientations or primordial attachments? Did, as the late Ali al-Wardi might have argued, Iraqis turn to traditional society and its values as state authority weakened and times grew rough?

By mid-2007, U.S. forces had formed alliances with sheikhs to reduce U.S. casualties, divide insurgents, and limit the area in which al-Qaeda in the Land of the Two Rivers could operate. Local security and reconstruction projects were sub-contracted to sheikhs as part of the implementation of the Bush administration’s “New Way Forward” plan. In effect, U.S. officers picked a sheikh (or a purported “sheikh” volunteered) and allowed him to be Don Corleone in a designated area in exchange for reducing attacks and keeping al-Qaeda out. Tribesmen in such areas listened to U.S.-supported sheikhs because those sheikhs had been empowered to provide jobs, guarantee the delivery of crucial goods (e.g., fuel, RO water), and “protect” (i.e., monopolize extortion in) the area.

If those same U.S. troops had visited Iraq before the U.S. provided sheikhs these powers, they would have found that tribal “leaders” had little ability to get tribesmen to do what they wanted.9 Numerous sheikhs reinvented themselves as manpower contractors after the U.S.-led invasion. One sheikh from al-Amarah told me that he could deliver 5000 working age men if I helped get him a contract. It was obvious, though, that he could not get those 5000 men to do anything if he did not have a contract. He was still a sheikh (in this case, the head of a fakhith), but his position had long been largely ceremonial, and his most important function seemed to be attending wakes (“Fat-ha,” a very big deal in Southern Iraq…). The authority enjoyed by cultural authorities can vary. I interviewed many Iraqis who could name the sheikhs of their tribe and clan but said they would not obey if told to do something, such as vote for a specific candidate. Many tribesmen told me they only saw their sheikhs once or twice a year at wakes and weddings.

What does this have to do with the Arab Spring and the political science field? I see several things occurring in parallel. First, the Arab uprisings are heightened expectations, particularly among the youth, that government will be less venal. At the same time, many of the long-standing access points to state resources in Arab societies will change in the coming years, or, at least, those who control such access points will change. Long-standing party elites and supporters of embattled regimes may no longer be reliable ways to exert influence or access patronage resources, such as jobs in SOEs, public sector jobs, kickbacks, and administrative resources of the state. Networks that individuals have relied on to access such resources will no longer “work” like they did in the past. Individuals will search for new avenues or, perhaps,  

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9 Although, knowing that tribesmen would not follow, sheikhs would not attempt to lead. They would not issue orders that would be disobeyed. It is important to understand the off-the-equilibrium-path beliefs that support what we observe.
diversify their networks in a period of uncertainty. Finally, this is all occurring during a period of global austerity. The financial crisis of 2008-09 (and, perhaps 2011-?) will shape many of the central questions of political science for the next decade. Instead of “who gets what?” our focus will often be on “who loses less?”

I expect to see in many places the resurgence of what might be called “traditional” forms of social organization — such as tribes and kinship networks, guild-like cliques, and religious communities — because they can provide means to target the distribution of resources to individuals on a limiting set of criteria. As a theoretical starting-point, I look to the work of Kanchan Chandra on the informational advantages of ethnic favoritism in patronage democracies and Dan Posner on why some ethnic cleavages but not others become politically salient. I am not suggesting that these forms of social organization are substitutes or rivals to the state, which is how they are often studied. Instead, under certain conditions, I argue they have and increasingly will thrive under or as part of the state, perhaps in places where they have not been considered relevant for decades. This newer literature, largely unexplored in the Middle East, sees ethnic groups as political coalitions in which membership criteria is useful to limit access to spoils that successful mobilization brings. People turn to “ethnic” identities because they are useful for them as individuals, not because of some institutional legacy, a taste for ethnic favoritism, the inherent strength of ties, or manipulation by elites.

Positivist-oriented political scientists largely abandoned the study of political culture long ago, but the discipline is slowly reengaging culture. As changes in the Arab world provide numerous opportunities to develop, test, and refine intuitions and theory, we scholars of the region have an opportunity to unpack the micro-foundations of cultural authority. The equilibrium-based analysis of Iraqi tribes that I provide above suggests that we can deductively restrict the conditions under which cultural authorities will indeed have authority. More broadly, we can deductively restrict the condition under which traditional institutions flourish or diminish. I see this line of work as very different than established research programs, such as those focus on civic culture (a la Almond & Verba), national character studies, or values and orientations (e.g., Inglehart, much of the work based on Arab Barometer data). I see this line of work as contributing to an emerging rational choice approach to culture, although there are other directions it can take. I suspect the most cogent work will combine formal theory and ethnographic understanding, approaches that I believe have a natural affinity which is often overlooked.

Regardless of whether we engage with culture, others will. We should anticipate a resurgence in the popularity of cultural explanations based upon values and primordial identities from think tanks, journalists, and some scholars to account for the electoral successes of Islamists, the relative lack of protests in Gulf monarchies, violence in Syria, and the failure of democracy to instantly emerge whenever a dictator is overthrown. We political scientists now have methods, approaches, and access that can help us analyze cultural institutions without resorting to simplistic tropes or a value/orientation definition of political culture. We can and should take up the challenge.

Michael Hudson and Lisa Anderson published an influential exchange seventeen years ago about the egregious use and abuse of political culture concepts to explain Arab politics.10 Anderson raised several concerns about work on political culture at the time, including: 1) it rarely ascribes to professional norms of testing and verification; 2) it displays a marked willingness to make categorical statements without regard to evidence; and 3) it largely attempts to account for something absent (democracy) with something present (“culture”). It strikes me that none of these concerns characterize this new wave of rational choice work on culture. For several

reasons, not the least of which was our fixation on durable authoritarianism as a dependent variable, the political culture baby was indeed thrown out with the Orientalist bathwater. In the meanwhile, though, the baby — long estranged from Middle Eastern studies — has grown into an adolescent. We should invite the kid back over.

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Middle East political science research during transition

By Wendy Pearlman, May 8, 2012

Events since 2011 posit many new questions for political scientists specializing in the Middle East and North Africa. Among these are the tasks of explaining variation in the character, duration, demands, and outcomes of protest across countries; the conditions under which some breakdown in an authoritarian regime results in consolidated democracy or another kind of rule; the roles of elections, popular mobilization, and constitution-writing in shaping struggles over a polity’s future; how the political projects and constituencies of opposition groups change as they become governing parties; the links between civil-military relations and transition outcomes; the influence of regional and international pressures on a domestic political transformation; and the relationship between transitions and varied economic interests, including those of business elites, rural peripheries, the urban poor, organized labor, and elements of the former regime. These are promising topics for research because they not only are of pressing concern for the people of the Middle East, but also address long-standing concerns in political science at large.

One challenge we face is how to formulate bounded research questions such that we can move forward in gathering data and making inferences that are not dependent on the still uncertain outcomes of on-going developments. While neither the phase of protest nor of regime transition is complete, it does not seem viable to wait until it is before we engage in research on the region’s changed circumstances. Yet how can we study something that is still unfolding?

I have no answers on this challenge, but a few points come to mind. First, we can take cues from how scholars have framed debates and investigations on relevant cases beyond the Middle East. Doing so extends an opportunity to engage deeply with accumulated knowledge and to integrate scholarship on the Middle East more fully into comparative politics and other subfields. In this respect, research on democratic transitions is an obvious frame of reference for us. That work can give us ideas about what questions we can ask today, as well as numerous insights to consider in our answers. At the same time, research on newly transitional cases in the Middle East can test, challenge, and expand that literature in new ways. Some of the categories and theories from classic works on transitions are taken as nearly universally applicable, even though they are heavily informed by cases from Latin American and Southern and Eastern Europe. Our new work can subject their conclusions to new tests, and perhaps produce an analytical vocabulary for examining transitions that is truly novel.
Second, we should continue to explore arguments findings about Middle East politics from before 2011, but perhaps use them to ask new questions. For example, a wealth of research traced how survival strategies pursued by the region’s authoritarian leaders gave rise to particular political practices and institutions. These included certain patterns in elections, government-oppositions relations, civil society, political-business networks, etc. In some countries, the uprisings illustrated the limits of these political forms as strategies for insulating incumbents from viable challenges. Yet do they necessarily indicate the limits of the durability of those political forms. We can continue to investigate the degree to which they remain, what functions and interests they serve in evolving contexts, and what their persistence might indicate about structures of politics in the Middle East and North Africa that are more enduring than the rise and fall of particular regime elites (or, if it should become the case, even regimes). At the same time, we might want to decouple investigations of those topics from the overriding puzzle of authoritarian persistence. This is clearly not because authoritarianism is over in the Middle East. Rather, the dominance of that question may have limited our imaginations in ways that we were not aware. We can embrace 2011 as an opportunity to open multiple new lines of investigation that diversify our field, without throwing the baby of accumulated knowledge out with the bathwater of Ben Ali, Qadhafi, and co.

Finally, I think it is worthwhile to (continue to) carry out research that takes the very emergence and spread of the uprisings as an outcome to be explained — though this should in no way become a new hegemonic question. Our field long puzzled over the coexistence in most Arab states of pervasive discontent with authoritarianism on the one hand, and authoritarian durability, on the other. We developed a range of compelling explanations for the indefinite stability of a seemingly unstable status quo: to borrow from Waterbury (1970: 61) on Morocco, some societies’ being “ever on the verge of an explosion that never occurs.” What explains the arrival of that explosion, in countries where it did finally occur, in 2011? How can we understand the sudden and sweeping mobilization of popular protest under repressive circumstances? What were the processes that brought people to participate in collective challenges to autocratic rule, even when dangers remained high and benefits uncertain?

In developing answers to these questions, we need not get caught up in “what we missed” debates. But it is worth asking if there were developments that we noted prior to 2011 but failed to interpret correctly. This seems particularly the case with regards to trends in opposition activity. For example, scholars did not fail to document street protests, labor activism, and the work of new opposition parties and movements in Egypt over the course of the decade leading to 2011. However, it seemed logical to most of us to view those events as compatible with authoritarian persistence more than harbingers of mass mobilization that could force the president from office. As we move forward, it might be fruitful to think not only about the empirical phenomenon upon which we should focus with new vigilance, but also about the analytical assumptions — about structure and agency, about predictability and contingency, about continuity and change — with which we seek to understand them.

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Scholars of the Middle East have spent much of the last 18 months reflecting on the inability of existing theories to explain and/or predict the wave of popular, anti-regime uprisings that took place in Tunisia, Egypt, Libya, Morocco, Jordan, Bahrain, Syria, and Yemen. However, scholars should not too be too hasty in dismissing existing work on authoritarianism, patron-client relations, sectarianism, and state formation in the Middle East. This literature served us well in the past, and many of its insights continue to apply to countries whose status quo remains unchanged. Further, existing approaches may also elucidate cases where political trajectories have shot off in new and unexpected directions.

At the same time, general theories or cross-regional comparison may improve our understanding of these new developments, and allow Middle East specialists to enter more mainstream dialogues in political science. It is important that the field does this graciously. The Middle East subfield of comparative politics has long been isolated, largely because its enduring authoritarianism, patrimonial forms of political organization, and lack of viable social movements outside of Islamist parties rendered it unique from other regions. The Arab Spring, by prompting new questions about social movements, revolutions, and attempts at democratization, now introduces new research questions to the area of Middle East Studies — yet these are old questions for many generalists and scholars of other regions. Just as Middle East scholars will be tempted to draw upon these older theories, adjusting them for any necessary regional or country-specific contexts, so will non-Middle East specialists be eager to seize upon Arab countries as they seek to maximize the external validity of their work. If Middle East specialists seek to enter debates with mainstream political science (and indeed, some do not), it is important to accommodate these newcomers, share our knowledge, and correct any overgeneralizations, historical inaccuracies, or poor inferences — and hope that they would do the same with us as we venture into unfamiliar territory. Instead, however, some Middle East specialists have accused these scholars of “academic tourism” and “Orientalism (Abaza 2011).” This is not a good start.

The Big Question

Q.1. What explains the occurrence of mass regime opposition across multiple Arab societies within such a short period of time?

Regional specialists have long known that Arab publics harbored a variety of socioeconomic grievances against the authoritarian regimes that governed them. These included the outpacing of salary increases by inflation, retrenchment of consumer subsidies, privatization, poor public services, corruption, crony capitalism, and a slowdown in public sector hiring that did not match private sector growth — much of which was disproportionately borne by the youth bulge of the 1980s. At the same time, however, the predominant narrative in the study of Middle East politics came to be that of “durable authoritarianism,” which revolved around top-down approaches that, aside from its consideration of key elites (Brownlee 2007; Stacher 2012), largely conceptualized Arab societies as politically demobilized, collectively disorganized actors that could be either placated by state largesse and/or intimidated and crushed by the state’s coercive apparatus (Ross 2001; Bellin 2004; Lust-Okar 2004; Heydemann 2007; Richter 2007; Yom and al-Momani 2008; Peters and Moore 2009). This was not an unreasonable assumption, given the disappointing momentum of protests outside of industrial areas. In Egypt, for instance, the first and second April 6th general strikes were a tremendous disappointment, and as late as Summer 2010 the Central Security Forces were still able to neatly clear out protests in solidarity with Khalid Said, the young Alexandrian who was brutally killed...
by police in June 2010 and who would later become an important popular symbol.

What the current theoretical lexicon lacks is a way to explain the suddenness with which Arab publics were brought to life, and in such tight geographical sequence, following the self-immolation of Mohammed Bouazizi in December 2010. What accounts for the sudden willingness of millions of Arabs, who live in a variety of different local, national, and regional contexts, to hit the street — all within the period of two months? Existing institutionalist approaches are unlikely to offer satisfactory answers. This may mean an opening for approaches that have not been central in the past, including those that deal with individual and group psychology, pan-Arab identity, and public spheres (Wedeen 1999, 2008; Lynch 1999, 2007). Yet we should be cautious not to flop from one approach to another, from institutionalism to constructivism, based on the dependent variable at hand. An approach that could capture both top-down regime/state dynamics and bottom-up social mobilization would be most welcome.

New Questions

Q.2. What explains the diversity of regime responses to mass protest?

Q.3. What dynamics shaped the nature of the cross-class coalitions that dislodged regimes, and what dynamics shape their future post-regime?

Q.4. Did the tactics of the protesters themselves, specifically the decision to use non-violence or the decision to use violence, affect each country’s respective outcome?

The developments of the past eighteen months include a number of new dependent variables that ought to be explained. The first is the diversity of regime responses, namely reform and no exit, non-violent exit, or violent retaliation. Here the existing regional specialist literature provides a foundation. Eva Bellin’s work on durable authoritarianism (2004, 2012), for instance, highlighted the importance of military professionalism to regime durability, arguing that militaries that were closely bound to the incumbent political leadership through material or sectarian ties were more likely to repress opposition. This argument alone tells us a great deal about regime responses to protests in Egypt and Syria. Egypt has a relatively professional military that increasingly saw its institutional interests as separate from that of Mubarak. Syria’s military consists primarily of the Alawite minority sect that has governed Syria since 1970 — an important part of Hafez al-Assad’s “coup-proofing” strategy (Quinlivan 1999). Yet there is also relevance to general scholarship. Studies of Syria and Libya, for instance, might inform the larger debate about the role of ethnicity versus state weakness in fomenting civil war (Fearon and Laitin 2003, 2007).

A second outcome, cross-class coalitions and their trajectories before and after regime exit, has a weaker basis in the Middle East literature. The mass overthrow of an authoritarian regime has little precedent in the region outside of the 1979 Iranian Revolution. The last time that the Middle East even saw crowds that were this large was in the 1950s, arguably before social classes had consolidated into somewhat self-aware bodies and also long before scholars began developing more sophisticated theories of social movements and revolutions. As such, it may be useful to draw on ideas from generalist and non-Middle East scholarship to understand the patterns of cross-class mobilization that occurred during and after the Arab Spring protests (Moore 1968; Huntington 1968; Skocpol 1979; Arjomand 1989). Yet any attempts to dialogue with this scholarship will surely have their limits, particularly due to the novelty of some social classes in the Arab Middle East (i.e. a “state bourgeoisie,” crony capitalists, and elements of the informal sector).

Finally, we might consider the specific tactics that the protesters used in different countries as causal forces in themselves. Libya may conform to Chenoweth and Stephan’s (2011) claim that violent resistance works best in cases where the opposition can find external patrons, while Egypt and Tunisia seem to support their claim that non-violence resistance “works” by courting regime moderates, providing opposition leaders with more tactical choices,
and allowing the opposition to pose as a negotiating partner. Specialists on these countries, however, could certainly comment in-depth as to whether or not they believe this choice of tactics was a “choice” at all.

**Old Questions**

Q.5. *What accounts for the durability of authoritarian regimes in the Middle East?*

Q.6. *To what extent is the United States willing to supply regional clients with diplomatic and material support?*

These two questions have dominated past scholarship of the Middle East and remain highly salient. The decisions that some regimes made during the Arab Spring reveal that several well-known logics still operate in some countries. The lavishing of state-mediated material benefits on the societies of the Arab Gulf states reveals that the oil-fueled, distributive model is still alive and well. In Jordan, old strategies of distribution, meaningless political reform, and sectarian divide-and-rule successfully put down the beginnings of popular protest. And in Morocco, a sudden referendum over constitutional reforms (which do not touch the core of monarchical power) also seems to have done the trick. In Syria, massive coercion, sectarian allegiances, and a lack of external intervention may still see the Assad regime through, just as it did in Hama in 1982.

The degree to which sectarian allegiances held the Assad regime together has become even more apparent in the bloodshed of the past year.

The Arab Spring has also compelled the United States to reveal the strength of its patron-client ties in the region, more specifically, how much it values some regimes relative to others. Until February of last year, U.S. security architecture in the Arab Middle East was heavily predicated upon the provision of material and diplomatic support to authoritarian regimes, which claimed their indispensability to U.S. national interests and continually alluded to the potential for an Islamist takeover. Yet with Ben Ali exiled and Mubarak on the brink, it became apparent that opposing (or not supporting) the Arab street would be costly for the United States, which could “end up on the wrong side of history.” The United States was relatively quick to throw Mubarak under the bus (although this was likely done with the knowledge that the Egyptian military could still serve as a pro-U.S. actor). Yet the U.S. has not opposed the regional orchestrations of its other important Arab ally, Saudi Arabia, whose military is intimately tied to the Saudi royal family. As such, the Saudi-backed massacre in Bahrain has largely escaped U.S. condemnation.

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The Arab Spring and the Future of Violence Studies

By Dina Rashed, May 23, 2012

In this memo I focus on prospects for the study of violence in political science suggesting some areas where I hope further examination will be pursued. Studies within the field had produced valuable insights into our knowledge with regards to interstate wars, insurgencies, civil wars and terrorism. Newer research is moving now in another direction, one that pays attention to criminal violence, criminal wars, and dynamics of illicit networks within and across national borders. This emerging literature also focuses on forms of policing and the kinds of relationships that bind the state and non-state armed actors. In that regard, I think the dynamics of contentious politics in the Arab world can help produce new knowledge given the change in actors’ positions and mechanisms of protest. New inquiries need to pay attention to the relationship between religion and violence and/or criminality, spaces of contestation, and new mechanisms of policing and control.

The rise of Islamists to formal political authority in several Arab states means that forms of state and opposition violence will be revisited. In a sense, this trading of places makes the relationship between religiosity and state violence, as well as religiosity and criminality more salient and begs some important questions such as:

1. How would the integration of previously-excluded and criminalized groups into the state’s institutions of force (militaries and police forces) impact formal and informal policing cultures?

2. What new forms of state authority are bound to be projected in light of these changes?

3. Given that Islamists groups do not subscribe to one and the same religious framework, how does the Salafi-Muslim Brotherhood-Sufi-Shiite divide complicate relationships of control and/or violence among religious groups, and between these groups and the state?

4. Would different religious interpretations and practices lead to differentiated forms of criminality? And what would this mean for extant networks of illegal/illicit trade and criminal activities?

The change in actors’ identities is not limited to Islamists new political roles; dynamics of protests underscore the important role of an emerging non-Islamist opposition as well. The inability of traditional political parties to mobilize non-Islamist groups has given way to the development of new secular opposition groups. There is no doubt that closer examination of such groups enriches studies of social movement and political organization, however what is important for violence studies is how these groups interact with state and non-state armed actors. The current protests show that some of these new secular groups do not shy away from violently challenging the state and its coercive apparatus. Soccer fans — or Ultras as they are referred to in Egypt — have been at the forefront of violent protests against the state and the regime, and some of their members have called for establishing private militias. Whereas the Arab state aimed at controlling labor’s discontent and violence in the 1960s and 1970s, and Islamists’ in the 1980s, 1990s, and 2000s, it may direct its future efforts towards secular, radical and sometimes violent sports fans. In a region where the youth constitutes at least half of the population and where soccer has traditionally been the only democratic game in town, mobilizing through stadiums may be a real threat to stability. Authorities in the Gulf monarchies, Saudi Arabia in particular, have issued recently warnings against what they called irresponsible behavior of sports fans. Future research need to closely probe the forms of violence that sports may bring about and how such forms of violence co-exist with other forms of political and/or criminal
violence. Studies may also investigate how these new secular groups alter spaces of contestation, shifting state-society violent confrontations from industrial complexes and radical mosques to soccer pitches and public squares.

Finally, I think the Arab Spring offers an opportunity to examine violence in light of the growing judicialization of politics. Discourses on human rights and legality are bound to have an impact on two main issues in transitioning societies: transitional justice and policing.

Transitional justice efforts in Egypt, Tunisia and to some extent in Libya and Yemen are following a constitutional legal course. Such path puts limits on state violence against fallen regimes. At the same time, the salience of human rights framework in an era of international economic assistance and external accountability may put limits on formal state violence and prompt authorities to informally rely on non-state armed actors as violent entrepreneurs,11

11 The judicialization of politics may put limits on some forms of state violence in authoritarian as well as transitioning regimes. In my dissertation research, which focuses on civil-military-police relations in authoritarian regimes (Egypt, Algeria and Mexico), I discuss the reliance of the Mubarak ruling elite on thugs and criminal networks over the last decade. I argue that such practice emerged in light of increasing judicialization of politics and the relative independence of judiciary which threatened criminalization of regime violence.

and/or formally repress through court rulings.

Future scholarship on political violence is definitely not limited to the above-mentioned areas, however, in this memo, I tried to discuss briefly some new approaches beyond the narrow emphasis on civil wars, insurgencies and terrorism. I think future research on violence will benefit from expanding its scope as it converges with religious studies, criminology and law. Studying violence in light of the unfolding events of the Arab Spring may be a good starting point.

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The Arab Spring has decisively settled one political economy debate, shed valuable light on a second debate, and raised in importance several other political economy issues that political scientists focusing on the Middle East and North Africa (MENA) region may wish to study in greater depth. I’ll briefly treat each of these three issues in turn.

A Debate Settled: Oil Matters.

Theda Skocpol did not invent the “rentier state” model, but she did more than anyone else to popularize it in the wake of the 1979 revolution in Iran (Skocpol 1982). Skocpol was attempting to explain fundamental state weakness when all the outward appearances of a strong state were in place: the coercive apparatus of the Shah’s Iran was large, well equipped and intact; the army had not been defeated in a foreign war; the secret police (SAVAK) was feared and thought to be effective; and the rapid increase in oil prices from 1971 to 1974 had brought enormous wealth to the regime. Skocpol, and many political scientists who followed this line of argument, used the rentier state concept to suggest that such an oil-based political economy created a Paper Tiger, fierce on the face of it, but quite weak in reality given the erosion of state-society relations. Oil states were quite vulnerable to upheaval and instability, particularly during sustained interruption of flows of international rents to a state’s treasury.

Other scholars were less convinced. F. Gregory Gause III argued that oil states in the Gulf were so empowered because of oil rents that they were effectively able to mold and shape powerful social actors, such as tribes and Islam (Gause 1994). Oil rents decisively shifted the balance of power between state and society in the state’s direction. As long as the rents flowed, Gause argued, this political economy produced greatly enhanced regime stability. Large-n statistical models done by other scholars also seemed to back up this argument.

The Arab Spring has settled this question, and Gause and his cohorts can claim victory. While oil-poor Arab countries have been riven by turmoil, the hydrocarbon rich countries (enjoying high oil and gas prices throughout 2011) have suffered relatively little turmoil by comparison, and have used their significant rents to placate most potential dissent. The oil-poor Arab countries of Tunisia, Egypt and Yemen have witnessed regime decapitation, and perhaps even social revolution in the making in Tunisia. Oil-poor Syria has seen the greatest violence to date, and may also end with regime change. Bahrain has a bit more oil and a lot fewer people than these other four states, but essentially ceased being a significant oil exporter two decades ago. It likewise experienced major turmoil, bringing in Saudi and other Gulf troops to suppress popular uprisings. By contrast, hydrocarbon-rich states such as Saudi Arabia, the UAE, Qatar, Kuwait and even Oman have witnessed relatively little turmoil, notwithstanding some minor protests in the Shi’a areas of Saudi and some limited disturbances in Oman.

In an iconic move, Saudi Arabia captured the essence of what these states are able to do to coopt dissent when it pledged an additional $130 billion to various domestic social programs in the early weeks of the Arab Spring.

The only serious exception to this pattern is Libya, a relatively major oil exporter that experienced armed regime change. But here, NATO intervention was decisive. The Qaddafi regime was on the cusp on retaking Benghazi (likely at the cost of hundreds, perhaps thousands, of lives) when the NATO bombs began to fall. Without NATO intervention, the Qaddafi regime surely survives, with a loss of life likely similar to Egypt’s upheaval.

Oil matters; the more hydrocarbon rents a regime has at its disposal, the more likely it can survive periods of dissent. This is a structural factor, so does not eliminate the possibility that a regime can stupidly squander those rents as a matter of policy and thus invite opposition. They just have a lot more room for error than non-oil states.
A Debate Clarified: Disentangling Rentier States from Oil States.

The Arab Spring allows political scientists the opportunity to disentangle two concepts that have been improperly conflated: oil states and rentier states. Put simply, all oil states are rentier states, but not all rentier states are oil states. By this, I do not mean the valuable point that there are other forms of rent besides oil, and in particular strategic rents. Laurie Brand made this point about strategic rents and Jordan’s search for ‘budget security’ years ago (Brand 1995).

By the standard definition of a rentier state — where a disproportionate share (typically at least 50 percent, often 80 to 90 percent) of annual budgetary revenues come from international rents — poor rentier states have suffered tremendous upheaval while rich rentier states (what I am calling oil states here) have not. Yemen is not a rich state, but at their height in 2006, over 90 percent of government budget revenues came from the sale of oil on the international market. Today, that figure is closer to 70 percent, but add in strategic rents from the Gulf and the United States, and Yemen is very close to being a “pure” rentier state. This same analysis applies to other poor rentier states, where the absolute size of the rents is a far greater predictor of a state’s stability/instability than the mere fact of its “rentierness.” About half of Jordan’s budget revenues come from strategic rents; the Palestinian Authority (PA) is a rentier “state” in that something like 90 percent of its budget revenues come from strategic rents and tax transfers from Israel right into the PA treasury (which act the same as traditional rents); Bahrain does not export a lot of oil anymore, but the little that it does still accounts for about 60 percent of all budgetary revenues. Economic data on Syria is unreliable, but it is estimated that in 2010 about a quarter of Syria’s budget revenues came from the sale of oil, a figure that likely used to be higher.

Poor rentier states do not share enhanced political stability with their oil-rich rentier neighbors, but do share many other characteristics, including lumpy institutionalism, personalized authoritarianism (until recently?), weak state-society relations, and a tendency to divert politics to cultural ‘wedge’ issues.

The Arab Spring should sharpen our distinctions among different types of rentier states, and, at the least, compel us to differentiate between significant oil states and their poor rentier cousins.

Emergent Political Economy Debates.

The Arab Spring ought to open up new or underutilized avenues of inquiry as well. Three issues really stand out in this regard. The first is corruption, and specifically ‘grand corruption.’ Complaints about regime corruption were nearly universal during the events of 2011, and there are many analytical accounts that mention corruption issues in passing. Corruption has gotten the attention of international civil society (e.g., Transparency International), and the U.S. Government conducts corruption assessments as part of its system of aid (full disclosure: I co-authored two of these corruption assessments, on Yemen and Jordan). But try doing a literature review of serious and systematic academic work on corruption in MENA and you will find precious little since an essay by John Waterbury decades ago (Waterbury 1976). The political economy of corruption in MENA is simply begging to be engaged seriously.

The political economy (and not just the politics) of the information revolution in MENA is more developed, but still in its infancy. Marc Lynch has done more than anyone to bring our attention to the impacts of the information revolution, and certainly the Arab Spring has provided an exclamation point to his work (Lynch 1999, 2007). The tactical uses of information by social movements and their activists is an important area for study, although I would suggest the strategic, structural changes in information flows since 1996 are far more critical. When MENA states lost the monopoly on the flow of information (even while retaining the commanding heights of information), authoritarianism just got harder to sustain. Not impossible, just harder. The political economy of information in MENA is rich with unstudied questions and unexamined puzzles.
The politics and, to some degree, the political economy, of water has been more studied by MENA political scientists, but much more can be done. Climate change, in addition to free rider water use, is making water scarcer and more politically problematic. Conflict in Darfur has been based, in part, on climate change, and internal migrations and conflict over water in Yemen, Jordan and Syria has already begun. This is an issue that has not only intellectual appeal, but also serious, real world policy implications.

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References


New Opportunities for Political Science: IR Theory

By Curtis Ryan, May 3, 2012

Most research in Middle East political science focuses on various aspects of comparative politics, while very little is couched in the study of international relations. That can be seen as a major deficiency in the literature (notwithstanding the importance of comparative politics) especially given the demonstrated importance of external factors in affecting the outcomes of Arab revolutions.

While every one of the Arab revolutions was rooted in domestic causes, they were also deeply influenced by the events in other Arab states and the specific actions of outside powers, including:

- Saudi Arabia giving asylum to a fleeing Ben ‘Ali in Tunisia,

- The United States using connections to the Egyptian military in an attempt to thwart a “Tiananmen Square on the Nile” bloodbath in Egypt,

- NATO intervention helping to topple the Qaddafi regime in Libya,

- Gulf Cooperation Council (GCC) counter-revolutionary intervention in Bahrain,
• External mediation by GCC states to arrange a Saleh exit in Yemen,

• And now current regional and global debates raging over intervention in Syria.

In short, the outcome of almost every case within the Arab uprisings has turned at least in part on the actions and decisions of external powers.

The dramatic changes across the Middle East regional system suggest a need to bring international relations (IR) back to the study of the region, not as a replacement, but as a complement to the rich traditions of research on the comparative politics of the region. Yet historically, research within Middle East political science has focused almost entirely on comparative politics, rather than IR.

IR theory has perhaps not helped matters by often having little grounding in the field research norms of comparative politics and Middle East studies. IR has also been relatively narrow in focus even when it does make an appearance, focusing on the Arab-Israeli conflict, the foreign policy decision-making of a particular state, or the roles of outside powers. Yet what is often frustrating and even maddening to scholars from other disciplines or subfields is the tendency of IR theory debates to turn not on substance, but rather on perceived gains and losses between vast competing paradigms. In a conversation on whether international relations theory has any relevance to understanding Middle East regional politics — at an earlier POMEPS meeting — one IR scholar noted that too often IR theory debates boil down to “what team you are on” and whose “team” seems to be winning.

Second, I would argue that understanding the IR of the Middle East requires an understanding of inter-Arab relations. While our existing studies of Israel, Iran, and Turkey are important, the fact remains that most states in the region are Arab. Most interactions are between Arab states, and hence most of the international relations of the region are in fact inter-Arab relations. Yet we still have little research on this subject. Much like the topic of civil-military relations, when one brings up the topic of inter-Arab relations, the obligatory citations are usually decades old. But inter-Arab relations didn’t end with the death of Nasser, and have in fact been vital to the outcomes of the Arab uprisings.

But understanding the international relations of the region, including inter-Arab political dynamics, will require analysts to draw on the insights of more than one of the IR paradigms. It would be fruitful, for example, to consider traditional Realist concerns with security dilemmas, alliances, and balances of power, while also drawing on Constructivist insights regarding the demonstrable importance of ideas and identities (including Pan-Arabism), as well as changing regional norms (including those regarding sovereignty, violence, and intervention). But overall, a more complete understanding of the region will, of course, require greater linkages between both comparative politics and international relations, with
emphasis on how each informs the other.

The Arab uprisings therefore provide political scientists the opportunity to reassess theories and perspectives, and to open new avenues of inquiry. In terms of research, several topics strike me as particularly important and interesting, and each has the potential to cross the lines of subfields, paradigms, and disciplines. Key topics and questions might then include:

1. Explaining alliances within inter-Arab relations, and especially the use of the Gulf Cooperation Council as an instrument for counter-revolution, and to what extent this is part of a new Arab cold war.

2. Explaining the persistence of monarchy even in the midst of the Arab uprisings: should their resilience be seen as rooted in domestic political economy or in the right constellation of external allies? And is this phenomenon merely fleeting, before the monarchies too begin to fall?

3. Exploring the links between statist theories of authoritarian persistence and more societally-based perspectives such as civil society and social movement theories. As societies mobilize and run headlong into authoritarian states, which aspects of these broader theories hold up, and which require major modifications?

4. Returning to the long-neglected study of civil-military relations, since military decisions have been crucial to almost every outcome so far in the Arab revolutions.

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What should political scientists be doing?

By Jillian Schwedler, May 21, 2012

The Arab uprisings have, unsurprisingly, shifted the attention of many scholars to two bodies of literature: political transitions, and social movements/revolutions. Each of these analytic frameworks have much to offer, particularly the well-developed literature on social movements and revolutions, and some political scientists will do well to see how the cases from the Arab uprisings — of failed and successful revolutions, and of failed and successful transitions — fit or challenge these existing models.

But while I believe that these frameworks will provide some frameworks for understanding the dynamics of individual cases, I don't believe they will raise many surprising insights or theoretical innovations. This is because the transitions literature and the revolutions literature both tend — with notable exceptions — to cluster countries into types — of regimes, of revolutions, of social movements, of successes and failures, and so on. Many overall assessments of the uprisings focus on sorting the individual cases into different columns: Egypt and Tunisia are examples of W, Jordan and Morocco are examples of X, while Syria, Libya and Yemen are examples of Y, Algeria is an outlier, the Gulf monarchies are something else entirely, except for Bahrain, which is a different kind of outlier, and so on.

These sorts of exercises, while interesting, are going to be of only limited use for us analytically, at least at this time. Part of the reason is because we simply don't know what we are looking at yet, not in a macro, big-picture sense. One thing we know definitively from social movement theory and the
literature on revolutions is that a successful revolution — a popularly based movement that brings about some change in political leadership — does not necessarily (or even often) lead to a regime change as scholars would define it (a fundamental overturning of an economic, political, or social system). The literature tells us that at least some old institutions and alliances almost reemerge (if they even appear to go away), and that what the new regimes will look like often takes several years to know for sure (think of the Iranian revolution as an example). So while thinking comparatively in terms of which states have had revolutions, which have not, and what is a real revolution anyway, and so on, may be interesting, but it is just too early to really make substantive headway of the sort that would allow us to either challenge or support existing theories of revolutions and regime change. I want to stress that these are important questions, but not ones that I think we can answer fruitfully from our current vantage point. Nor should they occupy all of our attention.

From my perspective, one of the main problems with the regime-type and other state-as-unit forms of comparison at any stage is that they fail to capture really interesting dynamics within cases, including in-case variation. As far as I can tell, we know a lot about what has and is happening in urban centers, and little about the rural or small town mobilizations other than “it started in town X and spread to towns Y and Z before reaching the capital.” I think we might raise some interesting insights if we think about diffusion across a single country. Why do some mobilizations being in major urban centers while others begin in smaller towns or rural areas? How and why do protests spread? How do regimes respond across space? Are state responses similar in different places? The objective is not just to compare across cases, such as Syria vs. Tunisia, but to think about variation within a case. Have the dynamics of mobilization and repression in Yemen been similar around Change Square in Sanaa as they have been in Taiz and Ibb? In Egypt, were the dynamics around Tahrir similar to those in Suez? We haven’t really explored these questions in detail, but I believe it would be a very productive place to focus attention, particularly since the “end points” of most of these mobilizations are far from clear (and that is typical to mass mobilization: think of the French Revolution).

We might also 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. And we can think about variation over time. How have mobilizations and police responses varied, for example, across Egypt, and over the course of the past year? I think of these questions as examining micro-processes and micro-practices, rather than sorting countries according to a particular type.

A final problem is that we need to think more creatively about connections across cases, and not only questions of diffusion of protests (which is a great question, but not the only one). Patterns of repression also diffuse. But even more, I think that we can fruitfully theorize, for example, about the spread of notions of legal accountability and how discourses (by regimes as well as their challengers) adopt new tropes, draw references to similar justifications, and so on. This can include a simple slogan that spreads “The People Want the Fall of the Regime!” to various references to “occupy,” to patterns of resistance focused around the reclaiming of public space (from occupy to the public art of the uprisings). We can think about the political economy of security and surveillance paradigms, and how they are connected to foreign aid, to notions of modernity and cosmopolitanism, to neoliberal economic projects, to which states (or non-state groups) 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? I think we have lots of really rich material that would raise some exciting questions, should we push our research in these directions.

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An Agenda for a Post-Arab Spring Political Economy of the Middle East

By David Waldner, May 23, 2012

Political uprisings across the Arab world have disrupted the political-economic status quo. This memo suggests some ways to disrupt the intellectual status quo as well. It is my contention that our understanding of the political economy of the Middle East has for too long rested, somewhat complacently, on a relatively vague notion of the rentier state. Indeed, I have often heard scholars of the Middle East complain that an idea that originated in studies of the Middle East has been hijacked by non-area experts. Almost two decades ago, I argued at a MESA panel that the rentier state paradigm was theoretically inadequate and empirically unjustified. I contend below that despite the flood of research studying the effects of petroleum rents on political regimes, economic growth, and civil wars, my original claim remains correct. I then suggest three areas that require sustained attention: institutional origins, distributive coalitions, and the political economy of collective action. I write this memo as a scholar whose training and early years were spent as a specialist in Middle East political economy (Waldner 1999) but whose research and teaching focus has not been Middle East focused for over 10 years.

1 Rentier States

Sachs & Warner (1995) pioneered the cross-national study of the growth effects of resource dependence; Ross (2001) estimated the first quantitative models of the democracy-inhibiting effects of resource rents; Collier & Hoefllner (2004) extended the study to the onset of civil wars. Waldner & Smith (2013) review and critically assess the dozens of studies that followed. Despite abundant empirical support, these studies have been plagued by persistent and compelling criticisms of measurement error, endogeneity bias, and model misspecification.12 Herb (2005) astutely noted that measurement error generated incorrect counterfactuals. Too often, models exploited cross-sectional variation at the expense of the longitudinal variation that would be necessary for unbiased causal inference; models and date that correct this error find no rentier effect (Lederman & Maloney 2007, Haber & Menaldo 2011). Indeed, the best historical work suggests a powerful growth-inducing effect of resource rents for 19th century developers like the United States (Wright & Czelusta 2004). The rentier paradigm suffers from seemingly incorrigible theoretical defects as well. To explain political and economic outcomes, rentier theory requires that state institutions be endogenous to resource rents, yet abundant theory and evidence demonstrates that state institutions are largely exogenous to resource rents. Karl (1997) achieves the unenviable position of making both claims in the same study. Perhaps the best that can be said for the paradigm is that the effects of resource rents interact with pre-existing institutional and coalitional variables (Dunning 2008, Smith 2007). But this position, if tenable, directs our attention to these pre-existing institutional and coalitional variables as important foci of research.

2 Institutional Origins

Political scientists and economists routinely attribute outcomes such as economic growth, democratic stability, and authoritarian durability to institutions such as private property and the rule of law, constitutional arrangements, and authoritarian elections. These valuable studies confront two limitations. First, to attribute outcomes to institutions is to defer explanatory adequacy, immediately raising questions about institutional origins: why does a given country have growth-inducing versus growth inhibiting institutions? Second, for institutions to be valid treatments, we must either randomize their distribution or show that assignment is independent of outcomes given...

12 But see Ross (2012) for cogent responses to these critiques.
pre-treatment covariates. Both issues direct us to model institutional origins. The topic should be of considerable importance to scholars of the Middle East who have invested heavily in institutional arguments over the past decade. The standard model of post-colonial institutions is derived from studies of colonial institutions in Latin America (Sokoloff & Engerman 2000, Acemoglu, Johnson & Robinson 2001). Acemoglu, Johnson & Robinson (2003) attempt to adapt the model to explain African political and economic institutions; the results are not promising. Area experts should play a critical role in either developing, modifying, or displacing the standard model.

3 Coalitions

One consequence of the field's reliance on the rentier paradigm is that the vague notion of a “distributive state” has too often displaced careful description and analysis of specific distributive coalitions. The value of such analyses has been well established in studies of other global regions. Lipton (1977) and Bates (1981), for example, pioneered the study of “urban bias,” or the implications for public policy and growth trajectories of urban-centric distributive coalitions. As the essays in Ndulu et al. (2008) attest, the theme has not lost any relevance over the years. Waldner (forthcoming) estimates hazard rates for democracies and dictatorships and finds that rural-incorporating regimes face a risk of regime failure approximately one-sixth of non-incorporating regimes. Some of the standard models of coalitional dynamics, e.g., Bueno de Mesquita et al. (2005), will undoubtedly not pass scrutiny with area experts, leading to their modification of displacement. Yet we know too little about the political economy of income distribution in the contemporary Middle East.13 This omission is especially troubling given the prominence of models linking regime dynamics to income inequality (Boix 2003, Acemoglu & Robinson 2006). Here too, studies by area experts will play a critical role in the critical evaluation of these models (Smith 2008).

4 Collective Action

Revolutions often yield new insights into the dynamics of collective action. The Revolutions of 1989, for example, generated models of preference falsification and information cascades (Kuran 1991, Lohmann 1994). In addition to applying, extending, modifying, or discarding these models, we scholars of the Arab Spring would profit from some new approaches to collective action. One line of theorizing draws on theories of normative and affective commitments (Petersen 2001, Wood 2004). As Wood's formal model attests, these new theoretical approaches do not preclude rational-actor models. Furthermore, as Medina (2007) demonstrates, the method of stability sets permits us to derive the probabilities of coordination from the game's payoffs. In other words, none of the newer models of collective action preclude a structural, political-economic approach. Always keeping in mind Kurzman's (2004) admonition that social scientists earn a living by constructing post-hoc explanations of unexpected and inexplicable events, scholars of the Arab Spring should exploit these models ruthlessly as we attempt to understand revolutionary dynamics.

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References


