



Research Article

# Learning and the diffusion of regime contention in the Arab Spring

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## Abstract

The Arab Spring has become a prominent example of the spread of cross-national regime contention. It is widely accepted that successful regime contention (in Tunisia and Egypt) triggered protests in other countries. Both scholars and other observers have suggested that protesters learned from successful regime contention. Thus far, available evidence has been either anecdotal or qualitative. This article provides a quantitative analysis of the diffusion of regime contention in 20 countries in the Middle East and North Africa (MENA) region between December 2010 and April 2011. Results show that protests that led to a regime change were much more likely to be imitated elsewhere, but especially in the first couple of weeks and without taking the specificity of the context into account. Our analysis thus suggests that regime contention diffused through bounded learning during the Arab Spring.

## Keywords

Arab Spring, democratization, diffusion

## Introduction

The Arab Spring, the wave of protests that has swept the Middle East and North Africa (MENA) since December 2010, has come to epitomize the cross-national diffusion of regime contention (Solingen, 2012). Scholars and journalists quickly adopted the domino metaphor to describe the successive onset of revolts in the MENA region. There is a consensus that the successful protests in Tunisia and then Egypt set in motion a process of regime change by sending a powerful signal to protesters in other countries, who were inspired by what their Tunisian and Egyptian peers achieved (Hale, 2013; Herb, 2014; Lynch, 2013; Saideman, 2012). That is, the diffusion of regime contention in the Arab Spring seems to have been driven by “demonstration effects” (Hale, 2013; Lynch et al., 2014; Patel et al., 2014; Saideman, 2012) or, in other words, learning (Weyland, 2012). In this respect, the Arab Spring is similar to other revolutionary waves (Hale, 2013; Patel et al., 2014; Weyland, 2009, 2010).

Our contribution provides a quantitative test of the argument that learning explains the cross-national diffusion of protests in the Arab Spring, focusing on the mobilization phase of the uprisings (Lynch, 2014a: 3). The argument has been made many times using anecdotal or qualitative

evidence. The nature of the phenomenon, however, also requires quantitative analysis: as Hale (2013: 334) warns, “the very sense of drama that makes the idea of regime change cascades compelling can put scholars at risk of overemphasizing superficially prominent causes”. A quantitative analysis helps to provide a colder examination of the argument. We also develop hypotheses on “demonstration effects” that allow us to examine the specific nature of the learning process, and in particular to assess whether it is rational or bounded. Weyland (2012) argues that protesters relied on cognitive shortcuts such as availability and representativeness, which led them to extrapolate the experience of successful protests and overestimate the likelihood of similar outcomes in their local context. If that is the case, successful protests should have been imitated quickly, based on a superficial assessment of their outcomes, and

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without careful consideration of whether they can be replicated in a different context. These hypotheses are consistent with qualitative evidence (Lynch, 2013, 2014a; Weyland, 2012), but they have not been subjected to quantitative tests.

This article offers one of the few statistical analyses of the Arab Spring and the first quantitative analysis of demonstration effects in this context. Lynch et al. (2014) use Twitter data to show how Syrian protests were at first directly inspired by prominent counterparts in the region, not just by their neighbors, but how the protests later lost this focus. Brancati (2014) includes the Arab Spring in a broader sample of protests and focuses on their economic determinants instead of diffusion. Danneman and Ritter (2014) study the diffusion of preemptive repression as a reaction to conflict in neighboring countries, but their analysis does not include the Arab Spring. Finally, Hussain and Howard (2013) use fuzzy-set-QCA to examine the role of mobile phone use in the protests without an explicit diffusion angle.

Our statistical analysis, relying on a dyadic analysis of 20 MENA countries between December 2010 and April 2011, finds that successful protests, those that led to a regime change defined as the replacement of a dictator (Brownlee et al., 2013: 36), were more likely to be imitated in other countries, but especially within the first couple of weeks, well before a meaningful assessment of their outcomes was possible. Moreover, the imitation of protests did not depend on geographic proximity, nor on the comparability of political and social structures. These findings confirm that protests diffused and that protesters learned from the experience of their peers in other countries, but also that the lessons they drew were biased by cognitive shortcuts.

### *Rational and bounded learning during the Arab Spring*

Prevailing explanations for the onset of protests argue that economic and political grievances lead people start challenging their political regime (Brancati, 2014). Scholars of the Arab Spring have emphasized such explanations. Dalacoura (2012: 66), for example, argues that an “explosive mix of socio-economic problems and widespread and deepening political grievances constituted a common causal thread behind all the uprisings”. Prior to 2011, the MENA countries were afflicted by many problems that laid the ground for protests, such as high unemployment among educated youth, rampant corruption, social inequalities, deteriorating economic conditions, and increases in food prices. Several studies emphasized that economic grievances are particularly dangerous when a large part of the population are young adults (Hvistendahl, 2011; LaGraffe, 2012; Nordås and Davenport, 2013). A so-called “youth bulge” was a common characteristic in the affected countries, as the proportion of young adults (15–29 years old) was, for

example, 38% in Tunisia and Bahrain, 43% in Egypt, and 53% in Yemen (Hvistendahl, 2011: 552). Finally, Brownlee et al. (2013, 2015) emphasize the absence of oil wealth and of hereditary succession not for the emergence of uprisings, but for regime change conditional on protests. These economic and demographic factors explain where protests might arise, but not their timing. Weyland (2012: 919) argues convincingly that economic and demographic determinants are “necessary conditions for widespread contention”, but they do not explain why protests unfolded at this particular time (Dalacoura, 2012). Brownlee et al. (2015: 42) “find no structural preconditions for the *emergence* of uprisings” (original emphasis).

To explain the emergence of protests in the Arab Spring, we focus on learning arguments that have been developed in the recent diffusion literature (Gilardi, 2012; Simmons et al., 2006)<sup>1</sup> and that are often referred to as “demonstration effects” in research on the Arab Spring (Hale, 2013; Saideman, 2012). Thus, we shift the focus from which characteristics make protests more likely in a given country (Brancati, 2014) to how prominent events in the region shaped their spread. To explain the diffusion of regime contention, (Weyland, 2009, 2012) identifies two mechanisms: rational and bounded learning. Both assume that protesters observe events in other countries and use them to update their evaluation of the costs and benefits of protesting in their own country; in particular, the prospects for success (Lynch, 2014a: 8). The key difference is that rational learning assumes full rationality, while bounded learning implies reliance on cognitive shortcuts that can lead to inaccurate assessments.

Rational learning assumes that actors maximize their utility in a fully rational way and take uncertainty and imperfect information into account accurately. Decisions are a function of systematic and unbiased cost–benefit calculations. In the case of regime contention, protesters are expected to challenge their government if they conclude that the costs of the status quo exceed the risks of protesting (Buenrostro et al., 2007; Weyland, 2009). One way to assess the risks of protesting is the past behavior of the regime, and another is the nature and number of uprisings in other countries. Protests elsewhere reveal how strong the ruling regimes and discontent within societies are, and how violently the ruling regimes react. This information signals the mobilization potential and odds of success in the local context, which, if positive in the view of the protesters, can trigger “spiraling contestation” (Weyland, 2009: 399). In short, the rational framework suggests that protesters update their belief about the costs of contention and the chances of success using new information from other countries. Buenrostro et al. (2007: 354) argue that the probability of being successful is “one of the key factors influencing potential protesters’ decision to take action” (see also Koopmans, 2004). Successful regime contentions in other countries show that regimes are vulnerable. However,

contentions against repressive regimes are risky, given that the chances of success are meager and the costs of failure significant. Therefore, according to the rational learning argument, protesters evaluate thoroughly whether their opportunity structure is comparable to that in the country in which they observe successful protests. This framework suggests that protesters assess information systematically using a process similar to a Bayesian updating, and that foreign examples are more valuable to protestors when they believe these foreign examples' political, institutional, and socio-economic conditions are similar to conditions in their own country. In this Bayesian updating framework, the quantity and quality of available evidence are taken into account carefully (Meseguer, 2009).

By contrast, bounded learning emphasizes the tendency of protesters to act based on cognitive shortcuts. People do not process information fully rationally. Instead, they are influenced by information that strikes their attention because of its vividness. They rely on two heuristics: availability and representativeness (Kahneman et al., 2002; Tversky and Kahneman, 1974, 1981). The availability heuristic determines which information people pay attention to. In our context, it implies that potential protesters are influenced by dramatic and remarkable events, to which they attribute disproportionate weight. The representativeness heuristic affects how information is processed. Protesters can be expected to make distorted inferences by overestimating the relevance of the focal event to their own situation.

Weyland (2009, 2010, 2012) relies on this theory to study the diffusion of regime contention, including in the Arab Spring. He argues that a dramatic event leading to a successful regime overthrow can quickly trigger a wave of regime contentions. Potential protesters believe that their chances of success are similar to those of successful front-runner countries. After a successful protest, they infer that their regime is also weak, that internal discontent is also sufficiently widespread, and that the regime's security forces will also behave as they did in the protesting nation. These inferences are made regardless of differences in the political regimes or the demographic structure between the two countries. Potential protesters are inspired by dramatic events and believe that they are representative of their own situation. However, often their focus on "logically irrelevant similarities and patterns of correspondence" (Weyland, 2010: 1153) will lead them to the wrong conclusions. Weyland (2012) finds evidence of bounded learning in the case of the Arab Spring. In line with the availability heuristics, the stunning events in Tunisia and Egypt attracted enormous attention and were emulated by protesters in other countries. Protesters acted based on rumors and narratives, not balanced information, and believed that the Tunisian and Egyptian events were representative for their own country, even in contexts radically different such as the Gulf monarchies (Lynch, 2013: 83, 104). These

cognitive biases were driven by hope: protesters wanted to imitate the accomplishments in Tunisia and Egypt by using "the same watchwords, slogans, and symbols" (Weyland, 2012: 926). The "day of rage", for example, a phrase coined by Egyptian protest movements, was subsequently taken up by protesters in other MENA countries. The signals coming from other countries were not analyzed carefully. Instead, "[t]he comparison to Egypt and Tunisia exaggerated the real prospects for victory" in the eyes of "hopeful but naive protestors" (Lynch, 2013: 103). As Lynch (2013: 99) writes vividly, "in those delirious moments, their victory [. . .] seemed unstoppable".

Several qualitative studies provide empirical evidence for the relevance of bounded learning in the Arab Spring (Cooley and Nexon, 2011; Dalacoura, 2012; Herb, 2014; Heydemann and Leenders, 2011; Lutterbeck, 2012; Lynch, 2013; Weyland, 2012). However, to the best of our knowledge, there has been no quantitative analysis of demonstration effects or learning during the Arab Spring.

We rely on two factors for distinguishing between bounded and rational learning: the speed of diffusion and the relevance of political and socio-economic similarities. First, the bounded learning framework suggests that potential protesters imitate successful protests quickly, without waiting to observe their ultimate outcome. The speed of mobilization was a distinctive characteristic of the uprisings (Lynch, 2014a: 4). Accordingly, the first hypothesis is:

*H1: Successful protests are emulated quickly.*

Second, we consider whether similarities and differences between countries matter. The rational learning framework suggests that protesters take factors such as differences in regime type (Herb, 2014) and socio-demographic structures into account, pondering whether the situation in the triggering country is comparable to their own situation. By contrast, the representativeness heuristic of bounded learning suggests that protesters believe that the stunning events are representative despite fundamental differences in regime types, population structures, country sizes, and the involvement of extra-regional powers (Anderson, 2011; Fjelde, 2010; Goldstone, 2011; Heydemann and Leenders, 2011; Lutterbeck, 2012; Lynch, 2013; Weyland, 2012). Accordingly, our second hypothesis is:

*H2: Successful protests are emulated regardless of political or socio-demographic differences.*

## Data and methods

We study the spread of protests across the 20 countries of the MENA region from December 2010 to April 2011 and show in the appendix robust results for a smaller sample of 17 countries.<sup>2</sup> Because the protests unfolded quickly (within 14 weeks), we use weeks as time units. We define

**Table 1.** List of MENA countries with protests during the Arab Spring. Success indicates that the protest led to a regime change. See the online appendix for analyses using alternative specifications of the sample and success.

Country	Date of protest	Success
Tunisia	19 December 2010	Yes
Algeria	7 January 2011	No
Jordan	15 January 2011	No
Egypt	17 January 2011	Yes
Oman	17 January 2011	No
Yemen	23 January 2011	No
Lebanon	25 January 2011	No
Saudi Arabia	28 January 2011	No
Sudan	30 January 2011	No
Bahrain	14 February 2011	No
Iran	14 February 2011	No
Libya	16 February 2011	No
Morocco	20 February 2011	No
Iraq	25 February 2011	No
Syria	18 March 2011	No

protests as episodic, public, and collective events that involve people directing social, economic, or political claims at a government (Rasler, 1996: 137). To code where and when protests occurred, we use “hard” information on the protests’ place, time, and form of action (Koopmans and Rucht, 2002: 238) collected by the Uppsala Conflict Data Program (UCDP).<sup>3</sup> Table 1 lists the dates of protests in the MENA region and indicates whether they led to a regime change, defined as the replacement of a dictator (Brownlee et al., 2013: 36).<sup>4</sup>

To test the hypotheses, we rely on the directed dyadic approach: a standard method in diffusion research (Boehmke, 2009; Gilardi, 2010; Gilardi and Fuglister, 2008; Volden, 2006). Units of analysis are dyads, or pairs of countries, whereby the first country (*i*) is the potential receiver of protests and the second country (*j*) is the potential sender. The dependent variable is coded 1 if protests take place in the first country after they have already taken place in the second country, and 0 otherwise. To test the temporal hypothesis, we construct five variations of this variable using different time frames, between 1 and 5 weeks, and we compare the analysis of the different time frames. The shorter the time frame, the faster are protests emulated. This dependent variable does not code imitation directly. It is possible that the sequence of protests is not due to cross-national influences. Thus, this type of dependent variable is often labeled “potential imitation”. The point of the analysis is to see whether potential imitation was consistent with the hypothesized diffusion process. The advantage of the dyadic approach is that it allows to examine the influence of specific examples, which is at the core of the idea of learning or demonstration effects. Based on their formal model of policy learning, Volden et al. (2008:

327–328) note that “scholarship providing evidence of a greater likelihood of policy adoption given earlier effectiveness elsewhere would lend convincing support to the concept of learning-based policy diffusion”. Considering the outcomes of protests instead of their mere occurrence is crucial, and the dyadic setup offers an effective way to include this piece of information in the statistical model.

We focus on three main explanatory variables, namely, the success of protests in the sender, political and socio-economic differences between sender and receiver, and political and socio-economic characteristics of the receiver.

*Success of protests in sender country (j).* We code a protest as successful if it was followed by a regime change, defined as the replacement of a dictator (Brownlee et al., 2013: 36). As we did for the dependent variable, we construct five versions of this variable by extending the time frame from 1–5 weeks. This setup allows us to gauge whether successful examples in the sender country increase the probability of potential imitation in the receiver country, comparing shorter and longer time frames. We expect the coefficient of these variables to be positive but decreasing as the time span increases. As alternative and broader definitions of success, we report robust findings when we include cases with large-scale mobilization and sustained protests, Bahrain, Yemen, and Libya (see Table 4 of the online appendix), and when we use data from Hussain and Howard (2013), which assign high success values to Tunisia, Egypt, Yemen, and Libya (see Table 5 of the online appendix).

*Differences between receiver country (i) and sender country (j).* We code the absolute and the directed differences between the political regimes of the sender and receiver countries using their Polity IV scores and two alternative measures. The Polity IV index ranks countries on a 21-point scale from autocracies to fully institutionalized democracies. The absolute differences gauges whether “potential imitation” is more likely the more similar the regimes are; while the directed differences tests whether protests in autocracies are perceived as strong signals of non-repression. The larger the Polity IV score of the receiver country compared with the sender ( $\text{Polity IV}_i - \text{Polity IV}_j$ ), the larger the resulting signal that protests will not be repressed. The Polity IV has been widely used but also criticized (Hadenius and Teorell, 2005). As a first alternative, we rely on the State Fragility Index, a five point-scale indicator of state repression “closely associated with ( ) (a state’s) systemic resilience in [ . . . ] responding effectively to challenges and crises” (Marshall and Cole, 2011: 7). As a second alternative, we construct a dummy variable using a regime typology based on the data from Hadenius and Teorell (2007), which distinguishes between democracy, multi-party autocracy, military regime, monarchy, and others (Fjelde, 2010).<sup>5</sup> The robust findings of the alternative regime type measures are reported in Tables 7 and 8 of the online appendix. Following our second hypotheses,



these absolute and directed differences should be irrelevant to the diffusion of protests. Thus, we expect the coefficients of these variables to be indistinguishable from zero. As additional measures of similarity, we consider absolute differences in population size, share of urban population, and ethnic and religious compositions. Again, our second hypothesis states that these differences do not matter for protest diffusion. Data for population size and share of the urban population come from the Correlates of War project (Singer et al., 1972), while the data on the religious and ethnic compositions are taken from the fractionalization dataset (Alesina et al., 2003).

*Domestic characteristics of the receiver country (i).* Finally, we include data on the socio-economic characteristics of the receiver country. First, we include GDP per capita to account for economic conditions. We expect a negative relationship between this variable and the imitation of protests. Second, we include the proportion of young adults (15–19 years old) to see whether a “youth bulge” increases the probability of imitation. Third, we include a democracy dummy using data from Hadenius and Teorell (2007). We expect that imitation is more likely in democracies. Finally, we control for oil and gas rents (as a percentage of GDP). More descriptive information on the data is available in Table 3 of the online appendix.

There are three methodological issues in directed dyadic analyses (Boehmke, 2009; Gilardi and Fuglister, 2008). First, for many of the country dyads the dependent variable must be 0 due to how the variable is constructed. This is the case when neither country in a dyad experienced protests during the investigated time period, or when protests already occurred in both countries. As suggested by Boehmke (2009), we exclude these dyads from the analysis, which decreases the number of dyads from 5320 to 1046. Second, to control for time dependence, we include time, time squared, and time cubed (Carter and Signorino, 2010). Third, the data structure induces complex dependencies among observations that need to be taken into account. We compute classic standard errors but the results are robust to adjusting standard errors for clustering on dyads or on country *i* (see Table 10 of the online appendix).

## Findings

Table 2 shows the estimates of five models. The main variable of interest is “success in country *j*,” which we expect to be positively correlated with the probability that country *i* imitates the protests that occurred in country *j*. To investigate how this relationship changes over time, we consider protests that occurred in the past week (Model 1), in the past 2 weeks (Model 2), in the past 3 weeks (Model 3), in the past 4 weeks (Model 4), and in the past 5 weeks (Model 5). In each model, the dependent variable is recoded accordingly. Following H1, we expect that the effect of successful examples diminishes over time. Thus, estimates of “success

in *j*” should decrease in size and significance from Model 1 to Model 5.

The results support the hypothesis that successful protests are emulated rather quickly. The positive effects of successful protests in country *j* on “potential imitation” in country *i* are large and statistically significant in Model 1 and Model 2, which consider events in the past one and two weeks, while they decrease with the longer time frame in Models 3–5. Figure 1 plots the effects of successful uprisings in country *j* on the likelihood that the protests are imitated by country *i* in different time frames. In the first week, the odds of protest imitation are ten times larger if the protests were successful than if they were not. Then, the effect decreases. After 4 weeks, it remains positive but is significantly smaller. Overall, the pattern is consistent with the argument that protests spread quickly by bounded learning.

These findings suggest that protesters rushed into protesting after they observed that an uprising against the regime made it more vulnerable and could lead to its overthrow. This is consistent with qualitative evidence showing that the successful protests in Tunisia and Egypt galvanized and inspired protesters in other MENA countries (Dalacoura, 2012; Heydemann and Leenders, 2011; Lutterbeck, 2012; Weyland, 2012). The decreasing influence of successful examples provides empirical evidence that the dynamics in the Arab Spring are quite accurately explained by the heuristics of availability and representativeness.

The results for the relational variables lend further support to the plausibility of the representativeness heuristics (H2). Overall, political and demographic similarities between country *i* and country *j* do not influence the probability of imitation. Protesters are not more likely to imitate uprisings in countries ruled by political regimes similar to their own, of similar size, or with comparable ethnic and religious populations (maybe with the exception of similarity in urban compositions, although the findings for that variable are not consistent across all models). Not even geographic proximity seems to matter: protesters were not more likely to imitate events that occurred in neighboring countries. This finding is consistent with Lynch et al. (2014), which shows that social media discussions of the Syrian conflict referenced Egypt and Tunisia rather than Syria’s neighbors, and with the argument that the emergence of a new Arab public sphere played a key role in creating a shared narrative across the region (Lynch, 2013, 2014b).

Finally, the findings for domestic characteristics of country *i* are largely in line with previous analyses: the larger the youth bulge and the worse the economic conditions (measured as GDP per capita), the more likely a country is to imitate protests. Particularly, the youth bulge has been identified in the literature as an important predictor for the likelihood of political protests (Hvistendahl, 2011; LaGraffe, 2012; Nordås and Davenport, 2013). However, the democracy

**Table 2.** Logit estimates predicting “potential imitation,” with standard errors in parentheses. Intercept and  $t$ ,  $t^2$ , and  $t^3$  are included, but not shown.

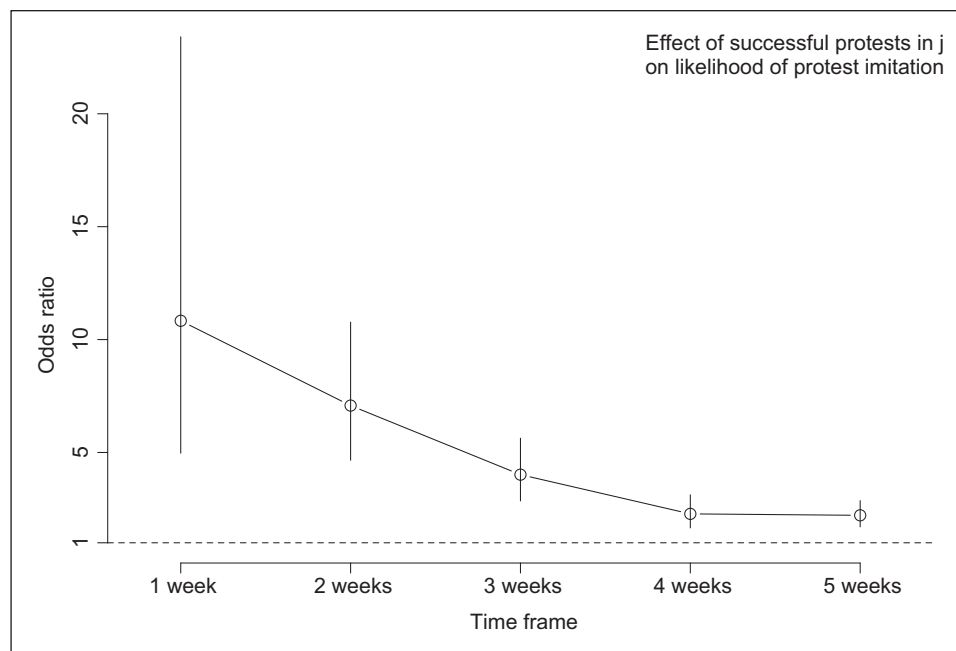
	Model 1	Model 2	Model 3	Model 4	Model 5
Success in country $j$					
Past week	2.375*** (0.634)				
Past 2 weeks		1.957*** (0.462)			
Past 3 weeks			1.388*** (0.418)		
Past 4 weeks				0.824** (0.401)	
Past 5 weeks					0.795** (0.362)
Relational variables ( $ij$ )					
Neighbors	0.580 (0.631)	0.601 (0.439)	0.519 (0.374)	0.295 (0.356)	0.098 (0.340)
Polity IV $\Delta_{ij}$	-0.090 (0.078)	-0.005 (0.050)	0.013 (0.041)	0.005 (0.039)	-0.038 (0.037)
Polity IV $_i$ – Polity IV $_j$	0.003 (0.057)	0.022 (0.039)	-0.015 (0.032)	-0.001 (0.030)	-0.005 (0.029)
Population $\Delta_{ij}$	0.022 (0.021)	0.025 (0.017)	0.023 (0.014)	0.016 (0.014)	0.013 (0.013)
Urban population $\Delta_{ij}$	0.029 (0.018)	0.018 (0.013)	0.017 (0.011)	0.025** (0.010)	0.021** (0.010)
Religion $\Delta_{ij}$	1.742 (1.239)	0.455 (0.853)	0.625 (0.716)	0.617 (0.659)	0.650 (0.617)
Ethnicity $\Delta_{ij}$	-0.637 (1.351)	-0.997 (1.031)	-0.771 (0.873)	-0.263 (0.806)	-0.343 (0.758)
Characteristics of country $i$					
Youth bulge	0.141* (0.078)	0.128** (0.056)	0.085* (0.049)	0.067 (0.047)	0.021 (0.046)
GDP per capita	-0.476 (0.305)	-0.432** (0.218)	-0.561*** (0.192)	-0.678*** (0.189)	-0.825*** (0.187)
Democracy dummy	-0.579 (0.793)	-0.187 (0.589)	-0.089 (0.516)	-0.308 (0.483)	-0.271 (0.446)
Oil and gas rents	-0.014 (0.021)	-0.009 (0.015)	-0.003 (0.012)	-0.000 (0.012)	0.013 (0.011)
Akaike information criterion	192.260	307.881	396.293	451.153	503.194
Bayesian information criterion	271.504	387.125	475.537	530.397	582.438
Log likelihood	-80.130	-137.941	-182.146	-209.577	-235.597
Number of observations	1046	1046	1046	1046	1046

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

dummy and oil and gas rents are not systematic predictors of imitation. While some domestic characteristics are important variables for gauging the risk potential for protest imitation, they cannot explain the timing of such events. This finding is consistent with the argument that while structural factors are of great importance to explain the outcomes of protests, they do not matter for their emergence (Brownlee et al., 2013, 2015). Instead, the strong, immediate demonstration effect of successful regime changes

prompted protesters to “[jump] to the conclusion that they could successfully challenge their own autocrats” (Weyland, 2012: 927).

The additional analyses reported in the online appendix show that these findings are robust across many alternative specifications of the statistical models, including broader definitions of success, different samples, other regime type measures, additional control variables, and adjusting standard errors for clustering on dyads.



**Figure 1.** Estimates of the increase in predicted probability with 95% confidence interval if prior protests in country  $j$  were successful compared with no success. Shown are the results for Models 1–5 based on the estimates reported in Table 2.

## Conclusion

We have provided quantitative evidence that a learning process drove the wave of uprisings in the Arab Spring. This was a learning process in which successful protests in Tunisia and Egypt spurred similar actions in other countries before the outcomes of the protests were clear, and regardless of differences in political and social conditions. If they were successful, protests were about 10 times more likely to be imitated a week after they occurred than if they were not successful, 7 times more likely after 2 weeks, 4 times more likely after 3 weeks, and twice as likely after then. The quickly diminishing demonstration effect of successful protests fits well with arguments about the role of cognitive shortcuts, such as availability and representativeness, in the diffusion of regime contention in the Arab Spring (Weyland, 2012) and beyond (Weyland, 2009, 2010).

This article is one of the few statistical analyses of the Arab Spring and the first quantitative analysis of demonstration effects in this context. While qualitative studies have documented how the mechanism of bounded learning unfolded in various countries with detailed case-specific information about the protests, this quantitative investigation confirms that the general diffusion dynamic in the Arab Spring followed the pattern suggested by the bounded learning framework. In that sense, this study complements the qualitative findings in the literature and hopefully provides a fruitful starting point for further systematic quantitative analyses of bounded learning.

This study also contributes to explaining why the democratization trajectory of MENA countries has been much less steady than initially hoped. After the governments of Tunisia and Egypt were overthrown, no other protest in the MENA region led to a regime change. Libya was the only notable exception: Muammar Gaddafi was killed on 20 October 2011, after the UN Security Council declared a no-fly zone and NATO enforced it, which helped the dissidents of the National Transition Council in their fight against the regime. Four years after the Arab Spring, violence and protests are ongoing in many countries of the regions. Protesters were inspired in January and February of 2011 by a few striking events to attempt enduring political change, and they started an unstable transition process as a result.

We have focused on the role of bounded learning in the diffusion of regime contention. However, not only protesters, but also authoritarian rulers can take cues from events in other countries to engage in “diffusion-proofing” (Danneman and Ritter, 2014; Heydemann and Leenders, 2011, 2014; Koesel and Bunce, 2013; Lynch, 2013; Weyland, 2010). The interaction between protesters and political authorities, the “dissent–repression nexus”, is of great importance to the processes we have examined here, as well (Davenport and Moore, 2012). Moreover, our analysis could not account for the role of political elites in the diffusion of contention itself (Hale, 2013; Mekouar, 2014). Clearly, these aspects are crucial for understanding why many reform attempts failed. While they are outside of the scope of this project, they remain important questions for future research.

In conclusion, during the Arab Spring successful protests diffused quickly regardless of differences in social or political conditions, consistent with theories of bounded learning. Unfortunately, four years later, the hopes of protesters clearly appear exaggerated, which further validates the bounded learning perspective.

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### Conflict of interest statement

The authors have no conflicts of interest to declare.

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### Supplementary material

The online appendix is available at: <http://rap.sagepub.com/content/by/supplemental-data>.

The replication archive can be downloaded here: <http://fabriziogilardi.org/resources/data/Bamert-Gilardi-Wasserfallen-RAP-replication.zip>

### Notes

- 1 As is conventional in the literature, we focus on the analysis of the diffusion process without explaining the occurrence of the first event (that is, in this study, the protests in Tunisia).
- 2 The main models include: Algeria, Bahrain, Egypt, Iran, Iraq, Israel, Jordan, Kuwait, Lebanon, Libya, Morocco, Oman, Qatar, Saudi Arabia, Syria, Sudan, Tunisia, Turkey, the United Arab Emirates, and Yemen. As robustness check, we exclude Iran, Israel, and Turkey from the sample (see Table 6 in the online appendix).
- 3 We double-checked the data with reporting from *The Guardian*.
- 4 Israel, Kuwait, Qatar, Turkey, and the United Arab Emirates are the countries of our sample without protests
- 5 For this variable, we construct a dummy variable, coding 1 if the countries *i* and *j* are in the same regime category (and 0 otherwise). The only exception are the two dyads with Iran and Libya, which both fall into the categories "others". As both regimes are quite distinct, we code these dyads as 0.

### Supplementary material

The online appendix is available at: <http://rap.sagepub.com/content/by/supplemental-data>

Replication material is available at: <http://thedata.harvard.edu/dvn/dv/researchandpolitics>

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