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Methods of Election Manipulation and the Likelihood of Post-Election Protest

Cole J. Harvey[1] and Paula Mukherjee[2*]

[1]Cole J. Harvey, Department of Political Science, UNC-Chapel Hill, North Carolina, US, and
[2]Paula Mukherjee, Department of Political Science, UNC-Chapel Hill, North Carolina, US
*Corresponding author. Email: paulamukherjee@gmail.com

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Abstract

The risk of popular protest is one of the few deterrents against election manipulation in authoritarian regimes and unconsolidated democracies, but why are some fraudulent elections met with popular protest while others are not? We use data from elections in 108 countries, from 1980 to 2004, to show that the regime's choice of election manipulation tactics affects the likelihood of post-election protest. Leaders signal their strength and resources by manipulating elections, but some manipulation tactics send stronger signals than others. We find that opposition groups are more likely to protest when relatively cheap administrative fraud is employed, but not when more costly forms of manipulation – extra-legal mobilization and voter intimidation – are used. This study demonstrates the importance of accounting for variation in electoral manipulation tactics, and the information communicated by those tactics, in explaining post-election protest and the stability of electoral authoritarian and newly democratic regimes.

Keywords: elections; vote fraud; protest; post-electoral violence; voter intimidation

Beginning in late November 2004, tens of thousands of protesters took to the central square in Kiev, Ukraine, to challenge the fraudulent electoral victory of Viktor Yanukovych; the Orange Revolution ultimately led to the annulment of the original results and victory for the opposition candidate. Sixteen years earlier in Mexico, as many as 250,000 people marched on the capital's main square to protest the results of a presidential election in which a suspicious last-minute computer crash appeared to throw the results against the challenger, Cuauhtémoc Cárdenas. These mass protests bolstered opposition parties that would eventually unseat the ruling party. Such dramatic examples are rare, however. Most manipulated elections in hybrid or authoritarian regimes pass by with little, if any, post-election protest. This article investigates whether the mix of election-manipulation tactics that governments employ affects the probability of protests challenging the results. In particular, we argue that manipulation tactics that involve directly influencing voters (such as vote-buying and voter intimidation) send strong signals about the

organizational and financial strength of the incumbents. As a result, extra-legal voter mobilization or intimidation efforts can deter protest by making it look less likely to succeed. By contrast, administrative fraud of the kind seen in Mexico in 1988 and in Western Ukraine in 2004 communicates less information about organizational capacity, making protest more likely.

We consider three broad categories of electoral manipulation: administrative fraud, extra-legal voter mobilization and voter intimidation. Administrative fraud is committed by election officials and includes tactics such as vote padding, ballot stuffing and tampering with ballots. In contrast, extra-legal voter mobilization involves direct contact with voters in the form of vote-buying, patronage, multiple voting and similar tactics. Similarly, voter intimidation involves pressuring voters through party representatives, supervisors or the security services. The three families of manipulation tactics require different investments of resources by leaders. Administrative manipulation is relatively cost effective and easier for leaders to control, since it relies directly on employees of the state and does not involve voters. Mobilization and intimidation rely on material rewards or political or legal pressure, all of which require the construction and monitoring of complex patronage networks of voters and agents. As a result, extra-legal mobilization and voter intimidation are costlier signals of the capacity of the ruling party than administrative manipulation. We predict, consequently, that opposition parties and activists are more likely to protest elections when leaders rely on administrative manipulation (especially in the absence of other more resource-intensive forms of manipulation). Our findings in support of these hypotheses are robust to different model specifications and data sets, including after statistical matching to approximate the benefits of random assignment to treatment and control groups for observational data.

This article contributes to a growing literature on election manipulation in two main ways. First, much existing research focuses on the role 'electoral manipulation' as a general category may have in sparking post-election protest (Fearon 2011; Little 2012; Magaloni 2010; Tucker 2007). By acknowledging the possibility that different manipulation tactics, with their varying costs and benefits, may affect protest in different ways, this article contributes to recent attempts to disaggregate the causes and effects of different electoral manipulation methods (Fortin-Rittberger 2014; Harvey 2016). Second, most existing research focuses either on the role of authoritarian leaders in committing election fraud (Howard and Roessler 2006; Lehoucq 2003; Ziblatt 2009) or on the capacity of opposition groups to mobilize (Bunce and Wolchik 2010; Magaloni 2010) against it. We help bridge these two explanations by considering how the incumbent's choice of manipulation tactic can influence opposition groups' evaluation of the strength of the incumbent.

The article proceeds as follows. In the next section, we situate our research question within the literature on authoritarian institutional manipulation, electoral fraud, collective action and protest. Then, we develop a theory of electoral manipulation tactics and post-electoral protest. Next, we test the theory using a series of empirical tests to assess the impact of administrative manipulation and extra-legal voter mobilization on post-electoral protest. We conclude with a summary and discuss the implications of our findings.

Elections, electoral manipulation and protest

Many authoritarian leaders responded to post-Cold War pressures by legalizing opposition parties and allowing multiparty elections. They did so, however, while manipulating the process in order to maintain their hold on power (Howard and Roessler 2006). These hybrid regimes use formally democratic political institutions to mask and legitimate authoritarian domination (Carothers 2002; Diamond 2002; Diamond et al. 1989; Schedler 2002). While generally stable, when such regimes do collapse, they are more likely to shift towards democracy than towards renewed authoritarianism (Brownlee 2009); as a result, mass electoral protest creates significant risks for incumbents.

The manipulation of elections is one of several ways in which the leaders of hybrid regimes work to assert control over formally representative institutions, bureaucracies and other political actors (Gehlbach and Simpser 2015; Schedler 2002). As authoritarian elections have become increasingly common, researchers have devoted considerable attention to understanding the roles that elections and political parties perform in settings where those institutions do not directly determine the fates of incumbents. This growing area of study has generally concluded that authoritarian elections are generally beneficial for incumbents, but also carry some risk. Authoritarian elections have been shown to benefit rulers in a variety of ways, including co-optation of the opposition (Gandhi and Przeworski 2006, 2007), information-gathering and managing elite disagreements (Blaydes 2011; Brownlee 2007; Lust-Okar 2006; Magaloni 2006; Reuter and Robertson 2015). Yet, these institutions also allow for the possibility of pro-opposition contestation. Authoritarian leaders face a dilemma: they set up liberal-democratic institutions to help prop up their regimes and ensure their survival, but these same institutions can become sites of resistance.

While elections and other representative institutions can serve as sites for opposition resistance, collective action problems often block popular mobilization. Many ordinary citizens living in a regime where citizens are unable to place appropriate limits on state power (Weingast 1997) would benefit from joining together in resistance against the government, but people falsify their preferences (Kuran 1989), 'shirk' and tolerate political abuses. Individuals do not cooperate and join in protest because they believe the cost of participating is too high and the likelihood of success is too low. In addition, structural incentives to not share information lead to a belief that the size of the population willing to protest is smaller than it is.

Manipulated elections can provide a solution to this collective action problem (Tucker 2007). People normally face abusive actions on an individual level and have to decide to react alone. Electoral manipulation differs from day-to-day violations because a large group of citizens experiences the same abusive act simultaneously; as a result, people believe they are less likely to be individually punished as the number of protesters increases. In addition to lowering the cost of participation, electoral manipulation increases beliefs in the likelihood of success. As Joshua Tucker (2007) notes, electoral fraud increases hopes about success partly because a mass post-election protest will have a louder voice than any individual acts of resistance or protest. By reducing the cost of participation and increasing

beliefs about the likelihood of success, electoral manipulation provides a clear focal point for action (Chong 1991; Schelling 1960; Weingast 1997).

Previous research on election monitoring has consistently shown that monitors' exposure of electoral manipulation can increase the risk of post-election protest. However, earlier work has generally focused on how monitoring can reveal information on ruling party popularity, or on citizens' shared grievances when elections are manipulated, and has largely not considered the varying impact that different forms of electoral manipulation may have on protest. For example, Susan Hyde and Nikolay Marinov (2014) show that when credible election monitors issue an overall negative report on the quality of an election, citizens are more likely to overcome coordination problems and engage in protest; Ursula Daxecker (2012) finds a similar effect of negative observer reports on post-election violence. In a related argument, Andrew Little et al. (2015) claim that allegations of large amounts of electoral manipulation make protest more likely, by indicating the ruling party's popularity is limited and suggesting potential weakness in the regime.

Our argument builds on this literature by showing that grievances and incumbent popularity are only part of the picture. As Alberto Simpser (2013) shows, manipulated elections can communicate information about the organizational capacity, resources and expected durability of the government and ruling party. Governments engage in excessive and blatant manipulation efforts in order to convince other actors – bureaucrats, the armed forces and security services, opposition parties and citizens – that the government has the resources and capacity to remain in power for the foreseeable future. This view has antecedents in findings which show that authoritarian governments aim for supermajority victories (Magaloni 2006) and that the ability to engage in manipulation attracts ambitious, talented politicians to the ruling party (Greene 2007). However, neither this body of work nor the literature on election observation investigates the possibility that different manipulation tactics might communicate different information about resources and capacity, and might affect the probability of protest differently as a result.

In particular, a sizeable body of research shows that protest – especially electoral protest – is a tool strategically utilized by political parties and social movement organizers. Graeme Robertson (2010) finds that regional elites in Russia activated patronage networks to stage protests against the central government in the 1990s, while the Kremlin used the same tactics to stage pro-government rallies throughout Vladimir Putin's rule. The Colour Revolutions in Serbia, Georgia, Ukraine and Kyrgyzstan were characterized by well-organized, experienced social movement organizations that laid the groundwork for post-election protest and formed links with unified opposition groups that called for post-election protests (Beissinger 2007). Similarly, Valerie Bunce and Sharon Wolchik (2010) argue that, far from being spontaneous outpourings of discontent, major post-election protests are carefully planned and organized by leaders and activists in parties and social activist groups. The introduction of authoritarian elections creates an opportunity for a strategic alliance between activist groups and political parties, which can work together to stage post-election mobilizations (Trejo 2014). Massive protests only take hold once a dedicated core of party and group activists have decided to make a sustained push against incumbents (Kuran 1989). This element of strategic

decision-making in electoral protest suggests that – though all types of electoral manipulation may generate grievances – some forms may deter protest by convincing opposition groups that the ruling party has the resources and networks necessary to effectively counter-mobilize.

Theory and hypotheses: signals from election manipulation tactics

The circumstances under which mass protests challenge election fraud can inform us about how electoral manipulation works as an authoritarian tool. The risk that electoral manipulation might spark popular protest by citizens and opposition parties is thought to be one deterrent in electoral authoritarian states (Fearon 2011; Magaloni 2010; Tucker 2007). However, leaders have at their disposal many different tactics with which to manipulate elections (Schedler 2002). These tactics vary in how strongly they signal regime resource and organizational capacity. Extra-legal voter mobilization with material rewards (Lehoucq and Molina 2002) and intimidation through workplace pressure (Frye et al. 2014) or physical violence (Collier and Vicente 2012; Fortin-Rittberger 2014) require investment in networks of brokers and clients, to monitor voters, distribute rewards and impose punishments. By contrast, administrative manipulation involves relatively few people and produces publicly observable results. Direct manipulation of the results using agents in the electoral administration is thus relatively cheap and easy for leaders to monitor. This theory suggests that simply engaging in electoral manipulation is not just a signal of regime weakness, since even popular and secure incumbents regularly resort to manipulation to demonstrate their strength. However, because administrative manipulation is less costly and thus a less effective signal of resource strength, heavy reliance on that tactic is more likely to lead opposition parties and activists to challenge the elections in the streets.

While administrative fraud is not a direct proxy for regime weakness, it is more commonly employed in weaker states (Fortin-Rittberger 2014). It relies primarily on election commissioners who can falsify election protocols or adjust vote totals as needed. They can also stuff ballot boxes themselves or turn a blind eye when others do so. In authoritarian countries, coercing or co-opting precinct officials is relatively straightforward; electoral commissions can be dominated by the ruling party, facilitating large-scale manipulation (Bader 2012; Calingaert 2006; Kovalov 2014). Judicial punishment for electoral malfeasance is likely to be rare where the ruling party controls the courts (Magaloni 2010; Popova 2006). Under such conditions, large numbers of pro-government votes can be manufactured (or opposition votes discarded) at relatively low cost. At higher levels of aggregation, this manipulation process becomes more efficient and less costly in administrative resources. That is, wholesale manipulation at the level of the central election commission requires the complicity of many fewer people than 'retail' manipulation at the level of the precinct.

However, even at the retail level, administrative manipulation is often more cost effective than extra-legal mobilization or voter intimidation efforts. Administrative manipulation relies on the cooperation of easily monitored officials on the state payroll. The number of individuals needed to carry out administrative fraud of a given size is relatively small compared with that needed to carry out extra-legal

mobilization or intimidation efforts of equivalent size. For example, election administrators in Russia have been observed adding hundreds or even thousands of votes to the ruling party's total in a single precinct (Golos 2012). An equivalent vote-buying effort would require paying those hundreds or thousands of voters, as well as the dozens of brokers who each take their own cut. Those brokers and voters also complicate the principal–agent problems associated with election manipulation (Rundlett and Svolik 2016); voters, not just brokers, may fail to follow through with their instructions. Furthermore, some administrative tactics, such as failing to invest in up-to-date voter registration lists or to provide a sufficient number of polling places for residents, may end up being less costly than running a clean election (Birch 2011).

Leaders use extra-legal mobilization to influence voters directly in several ways. Political machines can make payments directly to individuals in exchange for their support (Nichter 2008; Stokes 2005), pay opposition voters to abstain (Gans-Morse et al. 2014), hire voters to vote multiple times, and more. In any case, voters must be rewarded for behaving correctly. Consequently, extra-legal mobilization of any kind requires the creation of patronage networks whereby patrons can monitor clients' behaviour and punish or reward them appropriately. These networks are usually mediated by layers of brokers, with individuals at each level responsible for overseeing a larger number of actors at the level below (Auyero 2007; Holzner 2007; Knoke 1990; Lazar 2007). These pyramidal networks are inherently vulnerable to principal–agent problems, as brokers and clients both face incentives to shirk their responsibilities when monitoring is imperfect (Kitschelt and Wilkinson 2007).

The same sorts of networks may be used to engage in voter intimidation. Voter intimidation has been known to be carried out by organized militias in Nigeria (Bratton 2008), hired thugs and party supporters in Zimbabwe (Collier and Vicente 2012) and employers and supervisors in Russia (Frye et al. 2014). In a series of case studies in Argentina, Javier Auyero et al. (2009) demonstrate that patrons can activate their patronage networks in electoral and non-electoral contexts, mobilizing clients in violent and non-violent political activity. Similar results have been shown in Indonesia (Kalyvas 2003) and India (Wilkinson 2006). As Auyero et al. (2009: 12) write, 'Well-functioning patronage networks can be purposively activated to conduct politics by other collective (and sometimes violent) means'.

Large-scale election manipulation efforts that rely on these networks are complex and costly as a result. At every level of the network, brokers must be prevented from diverting resources for their own private gain and clients must be monitored (Kitschelt and Wilkinson 2007). One study of vote-buying in a district election in Taiwan illustrates the costliness of such efforts. Chin-Shou Wang and Charles Kurzman (2007) found that at least 45% of voters who had received a payment from the Kuomintang voted for a different candidate. Motivating large numbers of voters to support the ruling party in one district in that election could cost as much as $4 million, not including necessary payments to brokers (Wang and Kurzman 2007). A survey of Nigerian voters found similar problems with defection by voters who had been targeted by vote-buying and by intimidation efforts (Bratton 2008). This problem does not diminish with scale: a study of Costa Rica's elections in the first half of the 20th century found that a major vote-buying effort in a presidential

election could consume as much as 20% of a candidate's budget (Lehoucq 2007; Lehoucq and Molina 2002). Persistent principal–agent problems and defections mean that extra-legal mobilization requires patrons to devote considerable resources if they are to rely on this method to influence an election. Patronage networks are not necessarily a function of state capacity per se; they may also be parastatal, with incumbents making use of partisan or civil society organizations, political machines with local knowledge, corporate groups and/or coercive elements of the state (Hale 2014; Stokes et al. 2013). Thus, not all incumbents are capable of using extra-legal mobilization; the decision to use any particular strategy or set of strategies is shaped by the availability of resources and networks that can deploy them.

Political principals absorb local patronage networks in order to prevent local machines from agitating against the incumbent (Hale 2014; Robertson 2010). The patronage networks used to mobilize and pressure voters at election time can be directed to demobilize clients in times of protest, or to mobilize clients in defence of the regime. By building or co-opting local patronage networks, incumbents increase the difficulty and risk of opposition mobilization. Cole Harvey (2016) indirectly tests the argument by showing that mobilizational forms of manipulation are more common in competitive areas, while administrative fraud is more common in pro-regime strongholds. We offer a more direct (and cross-national) test, by showing that post-election protest is more likely when administrative fraud, rather than voter-mobilizing efforts, is used. Harvey's findings also help rule out the potentially confounding explanation that elevated competitiveness increases the likelihood of protest. Since increased competitiveness has been shown to be associated with voter intimidation and vote-buying, rather than with fraud, our argument that administrative fraud causes protest should not be spuriously correlated with high competitiveness.

We do not claim that administrative fraud is costless, either materially or in terms of legitimacy, for ruling parties. Additionally, even relatively cheap and less observable forms of manipulation like administrative fraud may produce benefits by inducing bureaucratic compliance with the regime (Gehlbach and Simpser 2015). However, we follow Carolien van Ham and Staffan Lindberg (2015) in arguing that manipulation in electoral administration is generally cheaper in operational terms than voter pressure, which is generally cheaper than widespread vote-buying.

Consequently, we argue that administrative fraud, like all forms of electoral manipulation, creates grievances for voters. However, administrative fraud provides little information on the incumbent regime's capacity to marshal financial and organizational resources. By contrast, the ability to manipulate large numbers of voters on election day indicates deep pockets and a deep bench of allies. Extra-legal voter mobilization and intimidation efforts are just as contrary to the spirit of clean elections as administrative malfeasance but communicate different information about the regime's resources. They are indicators of the capacity and stability of pro-incumbent patronage networks that can also be deployed outside the electoral arena. This argument complements the finding that pre-election manipulation of the electoral laws increases the likelihood of opposition protest (Chernykh 2014) because adjustment of the electoral laws communicates little

information about the strength of regime supporters other than the government's ability to control the legislative process.

Finally, we are interested in examining the interaction between administrative fraud and vote-buying/voter intimidation. In many cases, ruling parties deploy a mix of tactics to influence the election result (Gans-Morse et al. 2014; Harvey 2016; Van Ham and Lindberg 2015). A high degree of administrative fraud may not signal ruling party weakness if it is accompanied by a strong extra-legal mobilization effort, for example. If, as we argue, administrative fraud communicates little information, the probability of protest should increase most dramatically as administrative fraud grows, especially in the absence of extra-legal mobilization or voter intimidation. We test the following four hypotheses.

Hypothesis 1: *Increases in extra-legal voter mobilization will not significantly increase the probability of post-election protest.*

Hypothesis 2: *Increases in voter intimidation will not significantly increase the probability of post-election protest.*

Hypothesis 3: *Increases in administrative fraud will be associated with a significantly higher probability of post-election protest.*

Hypothesis 4: (Interaction) *The positive relationship between administrative fraud and the probability of protest will decrease as extra-legal mobilization or intimidation increase.*

Data, methods and results

To test these hypotheses, we combine existing data to create a data set of the type and severity of electoral manipulation, underlying economic and political conditions, and protest. There are two major sources for our data set. First, the National Elections Across Democracy and Autocracy (NELDA) data set provides data on post-election protest, our dependent variable, and several controls. With the exception of Western countries, NELDA includes data on elections in all independent countries with a population above 0.5 million, from 1945 to 2012. A number of primary sources were used to code NELDA variables, including academic election databases (such as the Lijphart Elections Archive), newspaper databases (such as Lexis-Nexis and Pro-Quest), US government databases (such as the US State Department Library of Congress Country Reports) and databases from intergovernmental and non-governmental organizations. The authors supplemented these sources with country case histories when needed (Hyde and Marinov 2012).

Second, the Data on International Election Monitoring (DIEM) data set (Kelley 2012) provides the measures of electoral manipulation tactics that serve as explanatory variables in our models. This data set covers countries that accepted election monitors, including new democracies (e.g. Bulgaria), competitive authoritarian regimes (e.g. Russia) and some highly authoritarian systems (e.g. Uzbekistan). Political context is measured using data from the Polity IV data set (Marshall et al. 2014). Finally, additional economic data are taken from the World Bank (World Bank 2014).

The combined data set includes 593 observations of elections and protest from 1980 to 2004. While there are 593 observation reports in our overall data set, there are only 277 unique elections. Duplicate elections exist where multiple monitoring organizations issued reports on the same election. We eliminate duplicate observations by taking the highest (that is, the worst) score reported by any observer mission for each DIEM variable. This approach has the benefit of incorporating information from multiple observer missions. It also helps compensate for a downward bias that has been found in observation mission reports, as observer organizations often face incentives to under-report allegations of manipulation (Kelley 2012).

We pre-process the data using two complementary approaches, in addition to robustness checks presented in the online appendix, before modelling the occurrence of protest. First, we use multiple imputation to help correct for the potential bias associated with non-random missing data. Separately, we employ a statistical matching technique that helps reduce bias and model dependence, allowing us to better estimate the effect of each electoral manipulation tactic on protest. Finally, as a robustness check, we also analyse the raw data. As a further robustness check, we also test our hypotheses using data from the V-Dem project (Coppedge et al. 2017).[1] All of these approaches are supportive of our theory.[2]

Dependent variable

The dependent variable for this study, *post-election protest*, comes from NELDA (Hyde and Marinov 2012) and records whether protests or riots occurred after the election. The codebook states that these protests must be 'at least somewhat related to the handling or outcome of the election' in order to be counted. Because this is a binary variable, we employ logit models to test our hypotheses.

Explanatory variables

Our predictions are tested using three variables from election observation mission data coded in DIEM. The first explanatory variable, *administrative fraud*, is a measure of problems in vote processing: ballot stuffing, tampering with ballots and falsification. It captures the kind of electoral malfeasance that affects results directly but does not involve voters or signal the strength of patronage networks. The second explanatory variable, *extra-legal voter mobilization*, is operationalized as a measure of problems such as multiple voting by individuals, voter impersonation, vote-buying and the distribution of favours by political parties. The third explanatory variable, *intimidation*, covers a variety of behaviours, including political pressure on voters, extraordinary tax inspections, administrative fines, intimidation of candidates, arrest of voters and martial law. Both extra-legal mobilization and intimidation measure the kinds of manipulation that involve extensive organizational networks, resource distribution and the mobilization of actual voters. All three variables are categorical and measured on a scale from 0 (no problems) to 3 (major problems).

Since these DIEM variables are drawn from election-observer reports, our data set excludes elections in which no monitors were present. Excluding unmonitored elections is unlikely to bias our results for several reasons. First, over the time

period we study, election monitoring rapidly became entrenched as an international norm (Kelley 2012), even among states which intend to engage in manipulation (Hyde 2011). The set of monitored elections encompasses a wide variety of total elections, as a result. Second, since we include only monitored elections, we hold constant any effect monitors may have on the likelihood of post-election protest by publicizing violations (Daxecker 2012), on the type of manipulation employed by governments and political parties seeking to avoid exposure (Simpser and Donno 2012; Sjoberg 2013) and on the long-term incentives for leaders to hold democratic elections (Hyde and Marinov 2014). Finally, it does not appear that observers were more able to detect some forms of manipulation than others. The median value for administrative fraud, extra-legal mobilization and voter intimidation is 1, and the mean values are 1.1, 0.83 and 0.97, respectively. A comparison of the scatter plot of observations of fraud and intimidation (Figure 1) and the scatter plot of observations of fraud and voter pressure (Figure 2) illustrates this variation.

Control variables

We control for several other factors that may affect protest. All of these variables are drawn from DIEM and range from 0 to 3. First, we control for overall *pre-election cheating*, which evaluates pre-election abuse of public funds, campaign freedom, media openness and intimidation by public authorities. These sorts of manipulation often draw less international condemnation and may be less likely to spark mass protest (Birch 2011), though Svitlana Chernykh (2014) finds pre-election manipulation of electoral rules was positively associated with post-election protest in post-communist countries. In addition, we control for *pre-election violence*, which could easily be correlated with post-election riots and protests. Pre-election violence is conceptually distinct from voter intimidation and is coded separately in DIEM for several reasons. The measure of pre-election violence captures the observed level of violence and includes behaviours such as weapons

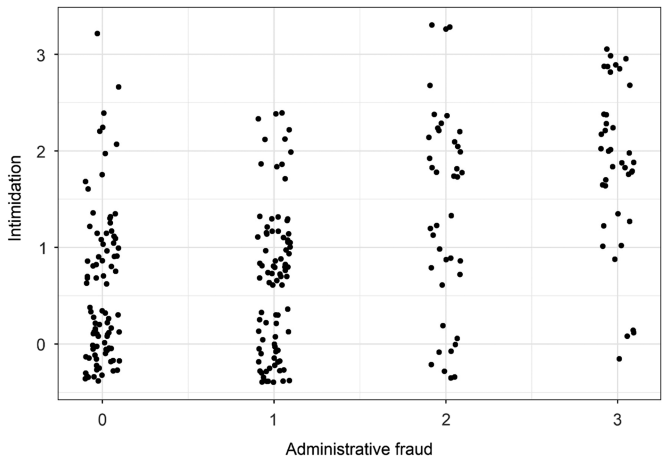

Figure 1. Scatter plot for fraud and intimidation (jittered)

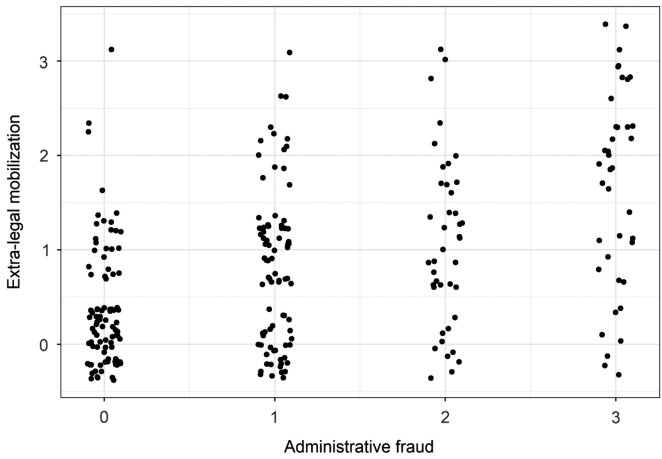

Figure 2. Scatter plot for fraud and mobilization (jittered)

use, assault and murder, while voter intimidation behaviours include political pressure, excessive inspections and fines and arrest or detention. Furthermore, the two variables do not represent the same costly network because the measure of pre-election violence does not distinguish between state-sanctioned violence and violence perpetrated by regime opponents. While violence perpetrated by the state would arguably capture the regime's ability to signal dominance, violence committed by regime opponents may instead demonstrate the fragility of the regime. Consequently, the measure of pre-election violence captures instability, which may open the space for greater post-election protest (Kuran 1989) independent of electoral manipulation. As a robustness check, we include an alternative measure of pre-election violence in the online appendix.

Next, we control for characteristics of the election itself. A binary variable drawn from NELDA records whether the election resulted in an *increase in the opposition's vote share* compared with the previous election. An opposition gain might signal to opposition parties and voters that protest is likely be successful due to weakening support for the ruling party among voters or elites (Hale 2014). We control for *transitional elections*, or the first election following the collapse of an authoritarian regime, using a binary variable since protest may be more likely in such uncertain contexts. Similarly, we use a binary measure of *protest in the previous election* to control for possible temporal dependence in the occurrence of electoral protest. Lastly, we include a dummy variable for *executive elections* because incentives for manipulation and protest may differ when control of the executive is at stake (Simpser 2013). These measures are all taken from NELDA.

We also use data from Polity IV to measure the overall level of repression outside the context of the particular election. In order to avoid collinearity with our explanatory measures of electoral integrity, we selected the *competitiveness of participation*, a subcomponent of Polity. This categorical variable measures 'the extent to which alternative preferences for policy and leadership can be pursued in the political arena' and ranges from 1 to 5, corresponding to the following

categories: repressed (no opposition activity permitted), suppressed (systematically limited competition), factional (competition among ethnic or parochial factions), transitional and competitive (regular, institutionalized competition). Since these are distinct categories, we treat them as categorical variables in our model.

The final set of control variables measure economic conditions that may give rise to political unrest. All economic control variables are measured for the year before the election. These are: *GDP per capita* (*logged*), *unemployment* and *election-year inflation* (*logged*). We take the natural logarithm of GDP per capita and election-year inflation because these variables are non-normally distributed in their raw state and heavily influenced by distant outliers. Furthermore, we include both logged inflation and its square in our models, in the understanding that moderate inflation can be a sign of economic growth, while runaway inflation can be a source of instability. See Table 1 for information on these variables.

Pre-processing: multiple imputation

To make use of as much data as possible given the modest size of the data set, we use multiple imputation to estimate values for missing data. Without multiple imputation, almost half of the data – 114 observations – are lost due to missingness; after imputation, we can make use of all 270 elections. See the online appendix for a discussion on our use of multiple imputation for time-series cross-sectional data.

Table 2 shows the results from analysis of the multiple imputation data. All three models presented here include our main variable of interest, administrative fraud. Model 1 includes all three manipulation variables, with no interactions. Models 2 and 3 include interactions of administrative fraud with vote-buying and voter intimidation, respectively. The results of the model without interaction terms

Table 1. Descriptive Statistics

Statistic	*N*	Mean	St. dev.	Min	Max
Post-election protest	277	0.18	0.39	0	1
Pre-election cheating	265	1.46	1.01	0	3
Pre-election violence	266	1.01	1.14	0	3
Administrative fraud	264	1.11	1.05	0	3
Extra-legal mobilization	263	0.83	0.89	0	3
Intimidation	264	0.97	0.92	0	3
PARCOMP – Suppressed	277	0.12	0.33	0	1
PARCOMP – Factional	277	0.34	0.48	0	1
PARCOMP – Transitional	277	0.35	0.48	0	1
PARCOMP – Competitive	277	0.08	0.27	0	1
Opposition vote gain	259	0.48	0.50	0	1
Transitional election	277	0.13	0.33	0	1
Prior election protest	264	0.18	0.38	0	1
Executive election	277	0.48	0.50	0	1
Unemployment (lagged)	241	10.83	7.28	0.60	39.30
Log GDP per capita (lagged)	258	6.95	1.01	4.28	10.59
Log inflation (lagged)	223	2.50	1.74	3.00	8.46
Year	277	N/A	N/A	1980	2004

Table 2. Models Using Imputed Data

	Post-election protests		
	(1)	(2)	(3)
Intercept	−3.08	−3.6	−3.77
	(2.02)	(2.04)	(2.14)
Administrative fraud	0.46*	0.77*	1.19*
	(0.2)	(0.29)	(0.36)
Extra-legal mobilization	0.18	0.7	
	(0.23)	(0.4)	
Admin. fraud: Mobilization		−0.29	
		(0.19)	
Intimidation	−0.01		0.91
	(0.27)		(0.45)
Admin. fraud: Intimidation			−0.49
			(0.2)
Opposition vote gain	−0.59	−0.58	−0.64
	(0.45)	(0.45)	(0.46)
Transitional election	−0.14	−0.1	−0.05
	(0.59)	(0.57)	(0.59)
Pre-election cheating	−0.21	−0.25	−0.29
	(0.26)	(0.26)	(0.27)
PARCOMP – Suppressed	0.77	0.81	0.88
	(0.74)	(0.74)	(0.74)
PARCOMP – Factional	0.61	0.56	0.46
	(0.65)	(0.65)	(0.66)
PARCOMP – Transitional	−0.82	−0.9	−0.89
	(0.68)	(0.69)	(0.7)
PARCOMP – Competitive	−15.8	−15.95	−16.14
	(824.88)	(935.17)	(1145.38)
Prior election protest	0.66	0.72	0.7
	(0.49)	(0.48)	(0.48)
Executive election	0.24	0.32	0.35
	(0.38)	(0.39)	(0.39)
Pre-election violence	0.31	0.27	0.25
	(0.19)	(0.19)	(0.19)
Unemployment (lagged)	0.02	0.02	0.03
	(0.03)	(0.03)	(0.03)
Log GDP per capita (lagged)	0.2	0.21	0.18
	(0.26)	(0.26)	(0.26)
Log inflation (lagged)	−0.27	−0.25	−0.21
	(0.2)	(0.2)	(0.21)
Log inflation squared (lagged)	−0.04	−0.04	−0.05
	0.04	0.04	0.04
Observations		277	

Note: * $p < 0.05$.

show that administrative fraud is positively and significantly associated with post-election protest, while there is no statistically significant relationship between protest and extra-legal mobilization or intimidation. The models with interaction effects show that administrative fraud is associated with a higher risk of protest, especially when vote-buying and intimidation efforts are limited. Figures shown in the online appendix represent graphically the individual results for each of the five imputed data sets, as well as the interaction effects in Models 2 and 3.

Pre-processing: statistical matching

Statistical matching techniques attempt to approximate the benefits of random assignment to treatment and control groups for observational data (where random assignment is not possible). In our case, matching is used to isolate the independent effect of each treatment variable on the probability of protest. We use nearest-neighbour matching with Mahalanobis distance, the most common form of multivariate matching (Sekhon 2009), to pre-process the data before running the logistic regressions. Matching is an important step in our research design because the severity of administrative manipulation may correlate with the severity of extra-legal mobilization and intimidation; see the online appendix for more detail on our use of matching.[3]

In order to determine the independent effect of administrative fraud, controlling for extra-legal mobilization and intimidation, we present five different models of post-election protest using matched data. In the first, we designate administrative fraud as the treatment variable, and use measures of mobilization and intimidation as controls after matching. In the second and third approaches, extra-legal mobilization and intimidation serve as treatment variables (respectively), while administrative fraud is used as a post-matching control. In the final two models, we interact the binary administrative fraud treatment with the other two forms of manipulation. Since our treatment variables have four categories by default, we first collapse each one into a binary variable. For each treatment variable, we marked elections that received a 0 or 1 (no problems or minor problems) as untreated, while elections that received a 2 or 3 (moderate or major problems) were considered treated.

We matched treatment and control groups along all pre-treatment explanatory and control variables, with the exception of opposition vote-gain, transitional election, suppressed participation, factional participation and competitive participation. The first of these is excluded because it is potentially a post-treatment variable, while the remaining four variables are not matched because they are unevenly proportioned binary variables. As a result, treatment and control groups were matched using GDP per capita, unemployment, executive elections, pre-election cheating, pre-election violence, extra-legal mobilization and voter intimidation.[4]

This technique finds a match for each of the 66 treated observations. Matching was done with replacement, so that an untreated observation can serve as the control for more than one treated observation, if there is no closer unmatched observation. Observations are weighted in the analysis models to account for this (Ho et al. 2007). Tables provided in the online appendix show that balance improves for matched variables, as the difference between the mean values of the treatment and control groups on those variables declines after matching. Most importantly, balance along the extra-legal mobilization and intimidation variables improves by 58% and 46% after matching, respectively (Model 4).

Ho et al. (2007: 223) also advocate that, after matching, researchers should employ the same parametric analysis they would have used in the absence of pre-processing. In our case, we use a logit regression model that includes the treatment variable as well as the controls provided above. In this instance, there are only

66 treated observations marked as treated, and 38 matched controls; 80 control observations are pruned. Though the number of observations is much reduced, the matching process helps reduce bias without sacrificing much in terms of variance (Ho et al. 2007; Smith 1997).

Results from matched data

Despite the fact that matching the data results in pruning roughly half of the observations (and requires collapsing a four-level categorical variable into binary treatments), analysis of the matched data supports the unmatched results. Table 3 and Figures 3 to 5 present the results of the matched data. Models 4 to 6 use the binary administrative fraud variable as the treatment. To test for the possibility that the other forms of manipulation do not have their own independent effects on protest, Models 7 and 8 use binary mobilization and intimidation variables as treatments. In all five models the most competitive category of PARCOMP is excluded, due to the very small number of such observations in the matched data sets.

The administrative fraud treatment condition variable has a positive and significant relationship with protest in Model 4, where it is a standalone variable. In Models 5 and 6, the relationship between the treatment and the dependent variable must be interpreted at various levels of extra-legal mobilization and intimidation, respectively, due to the interaction effects in the models. Figures 3 and 4 illustrate these interaction effects. Though both cases show some evidence in support of Hypothesis 4, the results from Model 5 are stronger. They show that the relationship between administrative fraud and protest is significantly positive when extra-legal mobilization is non-existent or rare; the relationship is decreasing as the more muscular forms of electoral manipulation become more pervasive. The results from Model 6 are less supportive, but still indicate that there is a significant and positive effect for administrative fraud when intimidation takes on a value of 1. We consider these results to be supportive of Hypothesis 4 overall, given the challenges of condensing a four-point treatment variable into a binary variable, and sacrificing some variance during the matching procedure by pruning observations. That the results for the standalone and interaction models are largely consistent with the previous models, despite the limitations of the data, increases confidence that administrative fraud has an independent positive effect on protest, which can be limited by employing more resource-intensive forms of manipulation.

That extra-legal mobilization and intimidation show no effect on protest when used as control variables is not surprising, since in these models we have attempted to balance those variables across treatment and control groups. In order to apply a stronger test of our hypothesis, we also use these variables as matched treatments (balancing administrative fraud across treatment and control groups, along with the other control variables).

In Model 7, extra-legal mobilization is the treatment variable along which observations are matched. After balancing control variables, there is no significant difference in the probability of protest in elections where extra-legal mobilization was moderate or severe compared with elections where such mobilization was

Table 3. Models Using Matched Data

	Post-election protests				
	(4)	(5)	(6)	(7)	(8)
Admin. fraud (treatment)	2.60**	4.66**	1.88		
	(1.08)	(2.28)	(1.54)		
Extra-legal mob. (control)	0.13	1.73*			0.05
	(0.36)	(1.02)			(0.30)
Intimidation (control)	0.15		0.11	0.52	
	(0.43)		(1.17)	(0.50)	
Admin. fraud treatment: Mobilization		1.69			
		(1.07)			
Admin. fraud treatment: Intimidation			0.06		
			(1.20)		
Extra-legal mob. (treatment)				0.09	
				(0.66)	
Intimidation (treatment)					0.33
					(0.64)
Administrative fraud (control)				0.91**	0.49
				(0.43)	(0.31)
Log GDP per capita (lag)	1.03**	1.01**	0.56	0.03	0.88**
	(0.50)	(0.49)	(0.43)	(0.56)	(0.42)
Pre-election cheating	0.06	0.04	0.31	0.04	0.11
	(0.49)	(0.48)	(0.46)	(0.46)	(0.42)
Pre-election violence	0.32	0.28	0.27	0.07	0.44*
	(0.29)	(0.28)	(0.27)	(0.30)	(0.26)
PARCOMP – Suppressed	0.02	0.49	0.05	0.26	1.12
	(1.26)	(1.19)	(1.20)	(1.24)	(1.15)
PARCOMP – Factional	0.47	0.23	0.63	0.27	0.72
	(1.21)	(1.16)	(1.20)	(1.29)	(1.05)
PARCOMP – Transitional	2.06	1.74	1.68	1.11	1.31
	(1.29)	(1.24)	(1.20)	(1.31)	(1.17)
Prior election protest	0.59	0.48	0.37	0.24	0.08
	(0.75)	(0.72)	(0.70)	(0.77)	(0.62)
Executive election	0.83	1.10*	1.13*	0.20	0.99*
	(0.63)	(0.63)	(0.62)	(0.65)	(0.57)
Transitional election	0.66	0.43	0.37	0.34	1.31
	(1.24)	(1.18)	(1.29)	(1.19)	(0.95)
Opposition vote gain	0.44	0.27	0.39	1.08	0.47
	(0.82)	(0.79)	(0.77)	(0.97)	(0.71)
Unemployment (lagged)	0.07	0.05	0.06	0.02	0.03
	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)
Constant	11.33***	13.46***	7.77**	1.67	9.70***
	(3.98)	(4.27)	(3.57)	(4.12)	(3.32)
N	104	102	106	74	105
AIC	110.80	114.55	122.19	108.70	128.64

Note: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

isolated or non-existent. The same pattern holds true in Model 8: voter intimidation has no effect as a treatment variable. As Table 3 shows, the administrative fraud treatment variable has a positive and significant effect, while mobilization and intimidation are not significant either as treatment or control variables. By comparing matched samples in this way, we help isolate the causal effect of administrative manipulation from that of extra-legal mobilization or voter intimidation. These results support Hypotheses 1 to 3.

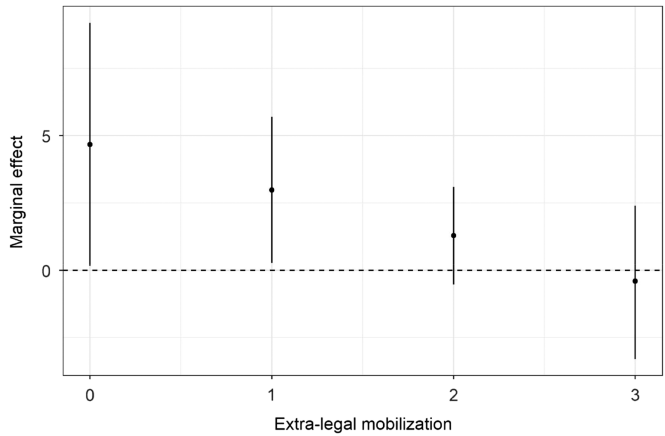

Figure 3. Marginal Effect of Administrative Fraud Treatment on Protest, Conditional on Extra-Legal Mobilization (95% confidence intervals)

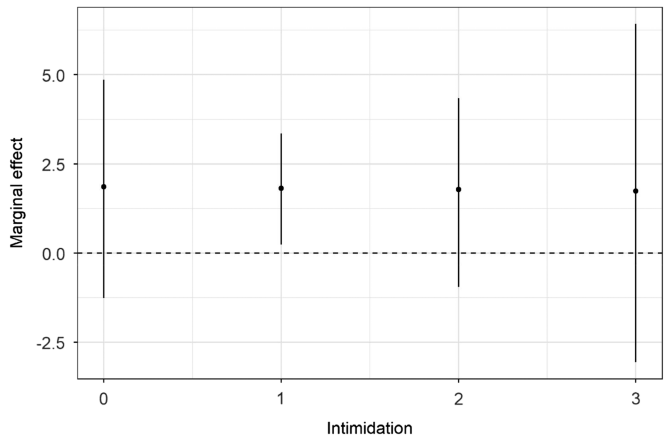

Figure 4. Marginal Effect of Administrative Fraud Treatment on Protest, Conditional on Intimidation (95% confidence intervals)

In order to better display the substantive relevance of this result, Figure 5 shows the predicted probability of protest when the administrative fraud treatment is applied, compared to the control condition (Model 4). This analysis draws on the matched data and holds the control variables constant at their medians (for categorical variables) or means (for continuous variables) and assumes an executive (or general) election. In particular, extra-legal mobilization and voter intimidation were assumed to be minor. Though the confidence intervals on the predicted probabilities are wide, the point estimates reveal the substantive importance of administrative fraud in provoking protest. Under these conditions, in the absence of administrative fraud, the predicted probability of protest is approximately 5%, and the lower limit of the confidence interval is essentially 0. The predicted probability of protest increases dramatically when administrative fraud is employed, rising to 40%.

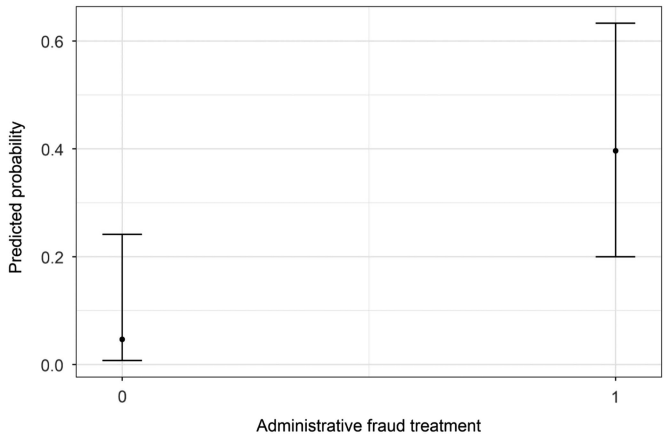

Figure 5. Probability of Post-Election Protest (with 90% confidence intervals)

Discussion and robustness checks

Both the multiple imputation and matching approaches confirm our hypotheses, as does analysis of the raw data (in the online appendix). These tests are complementary: multiple imputation compensates for a sizeable amount of non-random missing data, while statistical matching draws on fewer observations but is better able to rule out potential effects of mobilization and intimidation. The size of the effect under the matching procedure differs from that found in the unmatched data due to the shift from categorical to binary explanatory variables, but in all cases the effect is substantively meaningful.

These results are supportive of the broader signalling theory of election manipulation (Simpser 2013). However, our approach shows that the method of manipulation – not just the outcome – is important. Extra-legal mobilization on a large scale requires a significant investment of resources and shows the ability of the incumbent to mobilize people in support of the regime. By contrast, administrative fraud indicates control over the electoral apparatus but carries little information about the resources the incumbent can deploy or the regime's ability to influence ordinary citizens. These signals then influence the behaviour of other political actors – in this case, by influencing the likelihood that citizens and opposition activists will engage in anti-regime protests.

An alternative explanation for these findings could be that extra-legal mobilization transfers some benefits to clients (via vote-buying, for example), which reduces voters' inclination to protest. This explanation does not involve signalling; instead, voters satisfied by the benefits of patronage may be unmotivated to protest and unwilling to overcome the collective action problem. We find this interpretation unlikely to be correct for two reasons. First, it is indeed possible that those who receive clientelistic benefits will be more supportive of the party that pays them and less likely to protest against that party. However, vote-buyers have no incentive to target opponents of their party (Stokes et al. 2013) and are unlikely to target those with strong positive attitudes towards democracy (Carlin and Moseley 2015). These voters – opposition party supporters with attachments to

democracy – are more likely to be the constituents of a post-election protest, even if those who received targeted benefits are satisfied. Second, our findings also hold for voter intimidation, which would be harder to square with a hypothesis in which non-administrative forms of electoral manipulation provide utility to voters.

We provide further robustness checks in the online appendix. These include analyses of the raw data (without imputation or matching procedures) and of a larger data set drawn from V-Dem (Coppedge et al. 2017). In all cases, the results are supportive of our hypotheses.

These results add a new dimension to Little's argument (2012) that citizens discount electoral manipulation when evaluating the incumbent regime's vulnerability to protest. They imply that citizens more readily discount administrative fraud, but they can only partially discount mobilizational techniques since the latter demonstrate real sources of strength for the ruling party. Second, our results indicate that regimes are most vulnerable to post-election instability when they apply administrative fraud in the absence of a sizeable mobilization effort.

Conclusion

By considering the impact of different manipulation tactics on post-election protest, this article deepens our understanding of the role that electoral manipulation plays in maintaining (or threatening) authoritarian stability. We have argued that taking manipulation tactics into account helps explain why citizens sometimes decide to protest fraudulent elections, but often do not. Our theory builds upon and expands the signalling model of election manipulation (Simpser 2013), in which non-democratic leaders manipulate elections in order to display their organizational strength and material resources. However, our contribution shows that the form that manipulation efforts take is important, in addition to the overall level of malfeasance.

Manipulated elections create an opportunity for protest or other forms of collective action by the opposition – an opportunity that opposition groups may seize if they are organized and united. We show that this is only part of the story. Factors such as the opposition vote-share and the severity and type of manipulation send signals about the organizational capacity of the government; these signals can be read by voters, opposition activists and wavering elites when deciding whether to join in an anti-regime action. This study demonstrates that post-election protest is most likely when the government's reach exceeds its grasp; that is, when it seeks to influence the results of the election without demonstrating sufficient organizational muscle and resource capacity.

We identify three categories of manipulation tactics: administrative fraud, extra-legal voter mobilization and voter intimidation. When the government alters the results of the election using administrative tools, it provides only limited information on the organizational capacity of the incumbent to opposition parties and potential protesters. Such an approach communicates that the incumbents needed administrative fraud in order to win the election but may lack the organizational support to counteract opposition protest. When elections are manipulated using extra-legal mobilization or intimidation, however, opposition leaders and potential protesters face a different calculus. When the regime has demonstrated its

mobilizational capacity in the election, taking to the streets may appear more likely to lead to violent confrontation with the security forces or their proxies (Daxecker 2012), or to clientelistic penalties for those who take part. For this reason, protest is less likely in the context of such comprehensive manipulation than when administrative fraud alone is employed.

Support for these hypotheses is robust across different data sets, pre-processing techniques and model specifications. Variation in the effect of manipulation tactics on protest is detectable even when controlling for numerous other factors known to influence protest, including economic and political factors. These findings provide support for our theory that citizens and opposition parties respond to the signals sent by manipulation tactics when they decide whether to protest a fraudulent election. Consequently, we argue that the mix of tactics that incumbents use to manipulate elections is an important source of post-election protest, an understanding which helps explain why some fraudulent elections lead to major social upheavals while others do not.

These results improve our understanding of electoral manipulation and post-election protest. They lend support to the theory that election manipulation is as much about influencing the broader political environment as it is about winning elections. We also contribute to a literature on protest against manipulated elections which emphasizes factors such as opposition unity and the resources available to the opposition. To this, we add that opposition groups weigh their chances in part by observing the type and severity of manipulation employed. A government that has manipulated an election by mobilizing large numbers of voters in an extra-legal way, or by deploying proxies to intimidate opposition voters, has displayed considerable patronage resources. The financial resources, organizational capacity and local knowledge necessary to manipulate the election could also be deployed to counteract opposition protests. The revelation of this information about the strength of the incumbent regime makes opposition groups less likely to protest, compared with elections in which incumbents rely more heavily on cheaper administrative fraud. Future work should build on this analysis in several ways. The collection of data on the size of post-election protests would allow for a more nuanced evaluation of the effects of different election manipulation tactics on a continuous, rather than binary, measure of protest. Future work may help identify why and when incumbents choose administrative fraud, or a mixture of administrative fraud and mobilizational techniques, instead of relying solely on costlier signals of organizational capacity.

Supplementary information. To view the supplementary material for this article, please go to: https://doi.org/10.1017/gov.2018.38

Notes

1 Using V-Dem data allows us to increase the number of observation-years for measures of our explanatory variables. However, this comes at the cost of possible measurement error, since the data are drawn from expert coders rather than directly from election observation mission reports as in DIEM. Consequently, we rely on the DIEM data in our main analysis and use the V-Dem results as supporting evidence.
2 It would be possible to combine matching with imputation: Hill (2004) proposes two methods for doing so. However, we do not do so in this case, in an attempt to limit complexity of the model.

3 For example, the Pearson's correlation coefficient between administrative fraud and extra-legal mobilization before matching is 0.50. After matching, the correlation coefficient between the two raw variables is 0.28, and the correlation coefficient between the binary administrative fraud treatment variable and extra-legal mobilization is 0.20.

4 We exclude inflation from the matching procedure and analysis due to the high degree of missingness for that variable. Excluding this variable allows us to increase the already small number of complete cases by 16% (from 160 to 184 cases). We judge that this increase in sample size outweighs possible omitted variable bias, especially as we continue to include variables for GDP growth and unemployment.

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