

A missing guardrail: The failure of protest risk to deter election manipulation in non-democracies

Abstract

The risk of mass protest figures prominently in many models of election manipulation, as a deterrent to tampering with elections. Yet election manipulation is common outside liberal democracies, while protest is rare (and successful protest rarer still). Using count data on protest events from 647 elections from 1990 to 2012, this paper argues that the risk to incumbents has been overstated in prior work for two reasons. First, information revealed by election fraud is ambiguous; second, any such information quickly decays once protests do begin. The results show that the level of fraud has no bearing on the number of protests after an election, and is associated with protest initiation only in rare circumstances. This pessimistic finding implies that democracies are more fragile, and non-democracies more robust, than prior work predicts.

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On August 9, 2020, Belarus held an election in which President Aleksander Lukashenko ran for a sixth term. The official results credited Lukashenko with over 80% of the vote, a result which was regarded by observers as wildly inflated ([Tut.by 2020](#)). In response, hundreds of thousands of Belarusians took to the streets over the course of several months. Though the protests put significant strain on the regime, a harsh campaign of repression ultimately quelled the protests and President Lukashenko was inaugurated into a new five-year term. The Belarusian case offers a useful illustration of a central puzzle in the study of election manipulation: why did the risk of costly protest fail to deter the incumbent from engaging in such widespread fraud? And why did the regime use fraud to claim such an implausibly high margin of victory?

Events like those in Belarus are puzzling because many theories of election manipulation expect that incumbents will work hard to avoid them. A large, sustained protest movement creates a moment of heightened risk for the regime; the uncertainty of the outcome creates opportunities for elite defection, splits in the security services, and other negative outcomes. On some occasions, like Ukraine’s Orange Revolution or the People Power Revolution in the Philippines, these protests can result in the fall of an incumbent and a political opening. For this reason, many scholars—especially those working in the formal modeling tradition—have identified the risk that election manipulation will spark protest as a major deterrent to regimes working to engineer election outcomes. Most often, though, non-democratic states can rely on superior resources, elite solidarity, and repressive capacity to quash electoral protests. How threatening, then, are electoral protests really, and can that threat constrain electoral manipulation by governments?

In this paper, I argue that protest risk rarely serves as a deterrent to election manipulation for two reasons. First, election fraud is carried out both by strong, cohesive ruling parties, and by weakened parties seeking to cling to power. The ambiguity of the electoral environment in a non-democracy makes it difficult for opposition actors to correctly distinguish between the two. As a result, we observe a high proportion of ‘failed’ protest

movements, and—most likely—instances where weak governments go un-protested. Second, if electoral protests do materialize, their success or failure quickly becomes untethered from the severity of manipulation itself. Instead, the noisy election-day signal of regime strength is replaced by much clearer signals in the contest with protesters. Consequently, there is little penalty for severe manipulation in terms of protest risk.

Testing this proposition requires looking beyond binary measures of protest, and toward measures of its scope. Research on the risk posed to non-democratic governments by electoral protest often acknowledges—whether explicitly or implicitly—the importance of broad-based protest campaigns, rather than one-off events (Bunce and Wolchik 2006; Bunce and Wolchik 2010; Carnaghan 2016; D’Anieri 2006; Tucker 2007). As Brancati (2016) finds, approximately two-thirds of pro-democracy electoral protests ended within three days, posing little threat to incumbents. This implicit assumption has largely been overlooked in theories and empirical studies of the relationship between election manipulation and protest, however. In most cases, the risk of protest has both been conceptualized and operationalized as a binary outcome: protests occur or they do not. This approach obscures the importance of the number of electoral protests in a ruling party’s calculation of the risk involved in manipulation an election. Small, short-lived protests may be easily contained by a well-resourced government, while a long-lasting movement with a large number of protest events poses a more significant challenge.

Using cross-national data on 647 elections from 1990 to 2012 where incumbents claimed victory, combining data from NELDA (Susan D. Hyde and Marinov 2012), V-Dem (Coppedge et al. 2021), and the Mass Mobilization dataset (Clark and Regan 2021), this paper finds little support for the hypothesis that more fraudulent elections are associated with a higher risk that any post-election protest event occurs. Very highly fraudulent elections are associated with increased risk of protest initiation when incumbent vote-shares are small, in keeping with the formal-modeling literature, which predicts that overreach in the form of election fraud indicates government weakness and makes opposition groups confident of win-

ning a conflict in the streets. And, in keeping with the social-grievance literature, fraud is associated with protest initiation in societies with worse public sector corruption. However, these effects are largely confined to extreme scenarios, and there is no statistically significant association between the level of election fraud and the number of protest events that occur. This suggests a more complex relationship between election fraud and protest, undermining the argument that protest risk is a major deterrent to electoral malfeasance.

Since fraud is not associated with the overall number of events in a protest wave, the risk to governments has likely been overemphasized. That is, even if protests do break out in response to large-scale fraud, the government has many tools it may deploy to curtail them. Once protests begin, the noisy signal of regime strength provided by the election is replaced by a very clear signal: the force of the regime’s response to the emerging protest movement in contest with the mobilizational capacity of the opposition. To the extent that larger protest waves are more damaging to the government than small ones, protest risk does not appear to be a substantial constraint on the severity of election fraud. Troublingly, these findings indicate that ruling parties should follow an ‘all-in’ strategy on election manipulation. More manipulation is not associated with a larger number of protests, but a large margin of victory is associated with a lower risk of protest initiation. These results help explain both the relative paucity of electoral protests, the rarity of their success in overturning incumbent regimes, and governments’ attempts to claim implausibly high margins of victory.

1 Literature review

Broadly speaking, there are thought to be two mechanisms by which election manipulation leads to protest. The first is through revealing information about the strength of the government or ruling party. This information, in turn, feeds into strategic decision-making by opposition groups. This logic is often seen in formal models ([Chernykh and Svolik- 2015](#); [Egorov and Sonin 2021](#)). Several prominent models of electoral manipulation argue that

the risk of mass post-election protest helps restrain incumbents who might otherwise prefer larger levels of manipulation (Birch 2011; Fearon 2011a; Little 2012; Little, Tucker, and LaGatta 2015; Luo and Rozenas 2020; Magaloni 2010; Rozenas 2016). The risk of mass protest in these models is assumed to constitute a significant guardrail for election integrity in democracies, and a significant challenge for ruling parties in non-democracies. Building on this logic, researchers have proposed that third-party institutions—like election monitors (Daxecker 2012; S. Hyde and Marinov 2014; Little 2012), civil society groups (Birch and Van Ham 2017), and courts (Chernykh and Svolik 2015)—can reduce manipulation and increase protest risk by revealing information about electoral malfeasance.

Despite this consensus in the formal literature, there is relatively little empirical evidence to support the claim that manipulation per se indicates weakness, and thus increases protest risk. For example, looking at the 2011 election and protest cycle in Russia, Ananyev and Poyker (2022) show that post-election protest was not associated with the severity of fraud using several measures.¹ In a cross-national study, Rozenas (2016) finds that governments facing pre-election protests manipulate less severely; however, the dependent variables for that study are largely defined by the structural conditions of the election (i.e. whether opponents are allowed to run, whether the media environment is pro-regime), rather than by the election-day manipulation that usually figures in theories of election integrity and protest.

On the other hand, Simpser (2011) shows support for the alternative view that excessive manipulation can induce cooperation from opposition groups, elites, and other actors by revealing information about regime strength. Similarly, the type of manipulation employed can convey information about the ruling party’s capacity to mobilize, with costlier forms of manipulation associated with less protest (Harvey and Mukherjee 2020). In this way, large-scale manipulation can contribute to a ruling party’s ability to produce a large, stabilizing majority vote-share (Magaloni 2006). Similarly, Luo and Rozenas (2020) posit that ex post

¹Though, it should be noted that Lankina and Skovoroda (2017) show that a different measure of fraud was associated with post-election protest in the subsequent presidential election.

fraud is more likely to result in protest than ex ante manipulation, though this stands in contrast with the finding that parties are more likely to protest elections after ex ante changes to the rules ([Chernykh 2014](#)). In sum, it is likely that manipulation *may* signal regime weakness, but empirical works suggests that this is more likely when margins of victory are narrow and when falsification is a major tool.

Without disputing that election manipulation conveys information, a second school of thought has emphasized the ways in which biased elections may predispose individuals to engage in protest ([Kuntz and Mark R. Thompson 2009](#)). In this view, election manipulation acts as a shared grievance among individuals who prefer to see the ruling party lose—especially when it appears that fraud may have been decisive ([Kuntz and Mark R. Thompson 2009](#)). Belief in a stolen election is a particularly motivating kind of grievance, because it acts to focus all the diverse personal grievances individuals may have against the regime into one moment of opportunity for action ([Tucker 2007](#)), possibly initiating a protest cascade ([Kuran 1991](#)). This conception is closely connected to the idea that manipulated elections carry legitimacy costs for the ruling party ([Birch 2011](#)).

For example, ballot-stuffing and voter coercion have been shown to generate stronger feelings of anger than pre-election manipulation of ballot access ([Szakonyi 2021](#)), and access to information about manipulation appears to reduce support for the ruling party ([Reuter and Szakonyi 2021](#)), increase support for protest and reduce the perceived legitimacy of the regime ([Williamson 2021](#)). This argument has found support in studies of the relationship between economic grievances, manipulated elections, and protest ([Brancati 2016](#); [Rød 2019](#)).

Subjective, grievance-based approaches hinge more on citizens’ beliefs about fraud than on the actual level of malfeasance. A study of individuals in Nigeria found that individuals’ subjective perceptions of fraud makes them more likely to participate in protest, not more objective measures ([Daxecker, Salvatore, and Ruggeri 2019](#)), which suggests that emotions, moral condemnation, or grievances take precedence over strategic calculations for individuals. Subjective belief in fraud was also shown to be correlated with approval of mass protests

after Russia’s 2011 election ([Chaisty and Whitefield 2013](#)). Furthermore, simply being on the losing side in an election is associated with reduced belief that the election was fair ([Cantú and García-Ponce 2015](#)), and with increased support for electoral protest ([Anderson and Mendes 2006](#)). These studies suggest that electoral protest can occur at low or high levels of fraud, contingent on other factors.

The strategic and social-psychological explanations are, of course, interconnected. As Schedler writes, “Grievances provide moral justifications for collective protest: we need to do something. Opportunities provide strategic justifications: we can achieve something” ([Schedler 2013, 304](#)). Several empirical studies have, accordingly, found links between election manipulation and binary measures of protest ([Brancati 2016](#); [Harvey and Mukherjee 2020](#); [S. D. Hyde and Marinov 2014](#); [Rød 2019](#)).

The literature thus expects that manipulated elections increase the risk of protest by 1) revealing regime weakness, especially when the margin of victory is low, and 2) exacerbating existing grievances and upsetting citizens’ sense of procedural fairness. In turn, governments should be reluctant to manipulate. However, elections in non-democracies are *generally* risky for incumbents, regardless of how free and fair they may be, as they create an opportunity for opposition collective action ([Knutsen, Nygård, and Wig 2017](#)) and reveal information ([Pop-Eleches and Robertson 2015](#)). Furthermore, electoral protest is relatively rare; even in cases where it does occur, most efforts to overturn fraudulent election results through protest have failed ([Kalandadze and Orenstein 2009](#)). The apparent failure of protest risk to deter election fraud underlines our limited understanding of non-democratic ruling parties’ decision-making. Does the extent of fraud, in fact, make more numerous (and thus more costly) protest more likely?

2 Theory

As noted above, much of the formal literature on election manipulation assumes that the risk of protest serves as a deterrent to election manipulation. Given that pre-election protest is likely to be a strong predictor of post-election protest—by indicating strong social networks (Mateo 2022), pre-existing opposition groups and civil society capacity (Bunce and Wolchik 2006), and/or popular grievances (Brancati 2016)—the deterrent logic suggests that non-democratic governments will pull back on election-day fraud when the pre-election environment is characterized by protest (Rozenas 2016). This implies the following hypothesis.

H1: Pre-election protest will be associated with lower levels of election-day manipulation.

In addition, the logic of deterrence suggests that increasingly severe election fraud should make post-election more likely, either unconditionally or when the incumbent’s margin of victory is low. More extensive election-day manipulation, in these models, is generally taken as a signal of regime unpopularity. This in turn makes citizens (Egorov and Sonin 2021; Little, Tucker, and LaGatta 2015) or opposition parties (Luo and Rozenas 2020; Magaloni 2010; Rozenas 2016) more likely to stage protests.

H2: Post-election protest will be more likely to occur when fraud is severe.

H3: More severe election fraud is associated with a higher likelihood of protest when winning-party vote-share is low.

Previous research has also suggested that a fraudulent election is more likely to trigger protest when social grievances are high (Brancati 2016; Tucker 2007).

H4: Post-election protest will be more likely to occur when fraud is severe and economic and social grievances are elevated.

However, there are reasons to doubt the veracity of the deterrence logic. Little (2012) argues that citizens—knowing the regime is capable of manipulating the results—discount an expected level of fraud from the results when deciding whether or not to protest. The severity of fraud would thus be untethered from protest risk; what matters is citizens’ underlying estimate of the regime’s strength. Election fraud can itself be a signal of strength, rather than weakness. Fraud is not solely a weapon of floundering incumbents—in fact, Seeberg (2019) finds that higher-capacity states are weakly associated with more election fraud. Rather than acting out of desperation, strong ruling parties may engage in election fraud in order to pad their margins and secure cooperation from other actors (Simpser 2013) or because of a bandwagoning effect among front-line agents eager to please an incumbent patron (Rundlett and Svolik 2016). Fraud is thus an ambiguous signal of regime strength, which cannot conclusively guide strategic decisions to protest.

With some exceptions, such as the study by Lankina and Skovoroda (2017), most research on election fraud and protest treats the latter as a binary outcome. Such an approach does not consider how the protest environment might evolve after the election. Here I argue that, once protests begin, the extent of electoral fraud is unlikely to affect their scope. The initial emergence of protest triggers a response by the government and ruling party that can take the form of repression, concessions, or toleration (Bishara 2015; Frantz and Kendall-Taylor 2014; Hummel 2019; Lipsky 1968; Rasler 1996; Tilly 1978). That election dates are known in advance creates opportunities for opposition mobilization; anticipating this, regimes can more easily engage in pre-emptive repression (Bhasin and Gandhi 2013; Sullivan 2016; Truex 2019) —Knutsen et al (2017) argue that regimes become more effective at these sorts of behaviors over time.

In many cases, the government doubles down on repression, calling upon the security services to disperse protests, arrest leaders, and perhaps employ harsher methods including disappearance, torture, and execution (Davenport 2007; Escribà-Folch 2013). Though such crackdowns risk provoking a backlash, they are aimed at demobilizing the opposition

by increasing the costs of participation in overt dissent (Vogel 2022). In other cases, the government may attempt to accommodate the opposition, by making concessions on policy or institutions (Hafner-Burton, Hyde, and Jablonski 2016; Moore 2000; Piven and Cloward 1979). Alternatively, concessions may be aimed at the more moderate public, in order to keep them on the sidelines of the conflict (Hummel 2019).

What results is a test of strength between the anti-incumbent protest coalition and the regime with its supporters (McAdam, Tarrow, and Tilly 2001; Tilly 2010). In essence, as Beaulieu (Beaulieu 2014) notes, the onset of electoral protest marks a case of bargaining failure. Just as in the international context, the ensuing conflict acts to reveal information on the strength and resolve of the two sides. In such a contest, the incumbent enjoys multiple advantages, though it may lack the support of a majority of the population. It controls the resources of the state, and can call upon the support of those elites who benefit from the status quo. High-capacity states, as a result, are more likely to be able to engage in successful repression (Tarrow 2011). Though a biased election may temporarily allow diverse opposition groups to set aside their differences (Trejo 2014), this internal diversity can enable the incumbent to divide the opposition with selective repression or concessions. While the regime may have its own splits—between reformists and hardliners, for example—the emergence of protest can act to solidify support for the ruling party among its supporters.

In this struggle between the potential protest coalition and the incumbent government, protesters and their possible allies behave as “passionate economists” (Zomer, Leach, and Spears 2012)—they are motivated both by emotional responses to grievance, and by a more rational calculation of individual-level risk and group-level efficacy. What is the likelihood that protest action can help resolve the grievance, and does the benefit of participating outweigh the risk of personal consequences to the individual for participating? A fraudulent election may heighten citizens’ grievances and generate uncertainty around the underlying popularity of the regime, creating a moment of elevated protest risk. Crucially, though, this evaluation is dynamic and can be updated over time in response to new information

and new events (Zomeran, Leach, and Spears 2012). If protest movements have sufficient numbers and organizational resources to put the regime on the back foot, this can lead to more numerous protests as individuals' sense of group efficacy grows and their sense of risk declines (Chenoweth and Stephan 2011). On the other hand, if the regime is well-resourced, capable, and unified, its actions to end a protest movement (including repression, concessions, and co-optation) can lead to a negative re-evaluation of the movement's odds of success. This does not, of course, eliminate citizens' sense of grievance. Instead, it leads them to abandon 'approach-oriented' forms of coping, to avoidance-oriented forms: demobilization and disengagement (Young 2019; Zomeran et al. 2004). For example, while ruling-party supporters in Russia disapprove of election fraud (Reuter and Szakonyi 2021), they are more likely to withdraw from politics after exposure to electoral protest than to support the opposition (Tertychnaya 2020).

Finally, protest waves can have self-defeating and regime-sustaining effects. When unexpected post-election protests occur, trust in the government can increase, especially among opposition supporters (Frye and Borisova 2019). As the number of protests increases, support for the protesters' aims may fall; this effect is even larger for those who consume pro-government media (Tertychnaya and Lankina 2020). These empirical results are in line with models proposed by Lohmann (1994) and Shadmehr and Bernhardt (2011); they argue that when some citizens are too predisposed to protest, others become more inclined to stay home—dooming the first group's efforts to failure. These effects give reason to doubt that protest risk serves as a guardrail against election manipulation; rather, post-election protests may burn themselves out while leaving the public more approving of the government.

In summary, fraudulent elections are not likely to pose a significant protest risk to incumbents for two reasons. First, protest initiation is unlikely even in fraudulent elections because it is a noisy signal of regime strength. Weak and strong governments alike may engage in election manipulation (Simpser 2013), and acts of manipulation can be interpreted either as signs of strength or weakness (Pop-Eleches and Robertson 2015). Second, even if a

protest movement begins after the election, any useful information provided by the level of election manipulation quickly decays and is replaced by signals from the protests themselves (Lohmann 1994). Protesters, bystanders, and elites can update their estimation of the relative strength of the protest coalition and the incumbent government by observing the size of crowds, the number of protests, and the government’s reaction. Since the level of manipulation will no longer serve as an important indicator of regime strength once a challenge is taken to the streets, the number of protests will instead be related to other indicators of opposition and incumbent capacity. As identified in prior research, these factors include the previous repressive history of the government (Davenport et al. 2019), the institutionalization of opposition parties (Trejo 2014), the margin of victory in the election (Simpser 2013), and whether or not the chief executive faces term limits (Hale 2014). Generally speaking, these structural variables indicate aspects of the political opportunity structure that make protest more likely to occur (Meyer 2004).

H5: Once protests begin, the severity of election fraud will not be correlated with the number of protest events.

3 Data and methods

These hypotheses are tested using data from three datasets: NELDA (S. D. Hyde and Marinov 2012), V-Dem version 11.1 (Coppedge et al. 2021), and the Mass Mobilization Project dataset (Clark and Regan 2021). The unit of observation for this study is an election period as recorded in NELDA. This framework prevents selecting on the dependent variable, by capturing both protested and non-protested elections. NELDA also includes a variable indicating elections in which incumbents lost; as this study is focused on protest against ostensibly victorious incumbents, I use this variable to filter out elections which the opposition won outright. I further limit the sample to regimes coded as electoral autocracies or electoral democracies in V-Dem. This excludes closed autocracies (where no national elec-

tions are held) and liberal democracies (where electoral integrity and rule of law are both high).² Finally, the beginning of the date range is limited by the availability of data on protest from the Mass Mobilization dataset, as discussed below, which begins in 1990. The inclusion of a control variable from the Latent Judicial Independence dataset (Linzer and Staton 2015) limits the end of the time range to 2012. The resulting dataset includes 647 total election-period observations across 109 countries.

The Mass Mobilization dataset, from which study’s measures of protest are drawn, attempts to record all anti-government protests drawing 50 or more participants. The data is in event-day format. I divide this data into a pre-election period (180 days prior to an election) to capture the number and size of pre-election protest, and a post-election period (90 days after the election) for the dependent variables. I include all protest events in the post-election period, rather than filtering based on key terms like “election” or “vote,” since the conduct and outcome of the election may spark protests that do not explicitly refer back to the election in protesters’ demands. This is an extension of both the strategic and grievance logics of post-election protest. An electoral outcome that signals weakness for the incumbent regime may lead to strategic protests by outsider groups pressing their demands, even if they do not specifically focus on the integrity of the election. Likewise, disaffected groups may use the election as a focal point for their socioeconomic demands, irrespective of the nature of the election. Excluding such cases would like bias the results as a consequence.

Two variables are derived from the Mass Mobilization data. First, I collect the *median size* of post-election protest. The Mass Mobilization data records protest size using six categories.³ I then take the median size of the pre-election set of protests per election period; the result is an ordered categorical variable. Second, I capture the *number of protests* by taking the sum number of events recorded in the pre- and post-election data. The result is a count

²The specific variable used is ‘v2x_regime’ from V-Dem. This variable codes an electoral democracy, in part, if the country scores above a 2 on the measure of free and fair elections (‘v2elfrfair_osp’). This still permits election fraud to be detected in the electoral democracies; v2elfrfair can take on a value of 3 when “deficiencies and some degree of fraud and irregularities” occur.

³This are: 50-99 participants, 100-999, 1,000-1,999, 2,000-4,999, 5,000-9,999, and greater than 10,000 participants.

	Unique (#)	Missing (%)	Mean	SD	Min	Median	Max	
Judicial independence	281	0	0.4	0.3	0.0	0.4	0.9	
Legislative constraints	282	4	0.4	0.3	0.0	0.4	0.9	
Alternative information	326	0	0.6	0.3	0.0	0.7	0.9	
GDP growth rate	254	1	0.1	0.4	-0.4	0.0	5.9	
Civil society openness	283	0	0.7	1.2	-2.7	0.9	3.5	
Public sector corruption	237	0	-1.0	1.1	-3.1	-1.1	3.8	
National party organization	246	0	0.7	0.9	-2.3	0.7	3.3	
Election fraud	423	1	0.7	1.1	-2.3	0.7	3.2	
Incumbent term-limited	3	4	0.1	0.3	0.0	0.0	1.0	
Physical integrity index	291	0	0.5	0.3	0.0	0.6	1.0	
Urbanization	463	0	49.8	20.1	9.2	51.2	100.0	
Rentier state	3	5	0.3	0.5	0.0	0.0	1.0	
Tax state	3	5	0.5	0.5	0.0	1.0	1.0	
Incumbent vote-share	407	15	0.6	0.2	0.1	0.5	1.0	
Median pre-election protest size	8	1	1.1	1.5	0.0	0.0	6.0	
Off-schedule election	2	0	0.3	0.5	0.0	0.0	1.0	

variable ranging from zero to 435 for the pre-election period and 0 to 385 for the post-election period. The large majority of elections experience no post-election protest: in 504 of the 647 elections, no protests are recorded. Figure 1 reports the distribution of protest counts across the dataset, as well as winners' vote-shares.

This distribution is in line with the theoretical proposition that two data-generating processes are at work: the factors that lead to the occurrence of protest may not be the same as those that sustain or intensify it. Consequently, I model the results using negative binomial hurdle regressions. Hurdle models are two-part regressions in which the variation in the dependent variable is assumed to follow two distributions: the probability of obtaining a zero or non-zero outcome is modeled as binomial logic function, while the distribution of the non-zero count variable is modeled (in this case) using the negative binomial function. The logistic portion of the model identifies the factors that make *any protest* more likely to occur; the negative binomial regressions to model the count of protests given that protest has begun.

Each model includes the explanatory variables proposed above. First, three proxy mea-

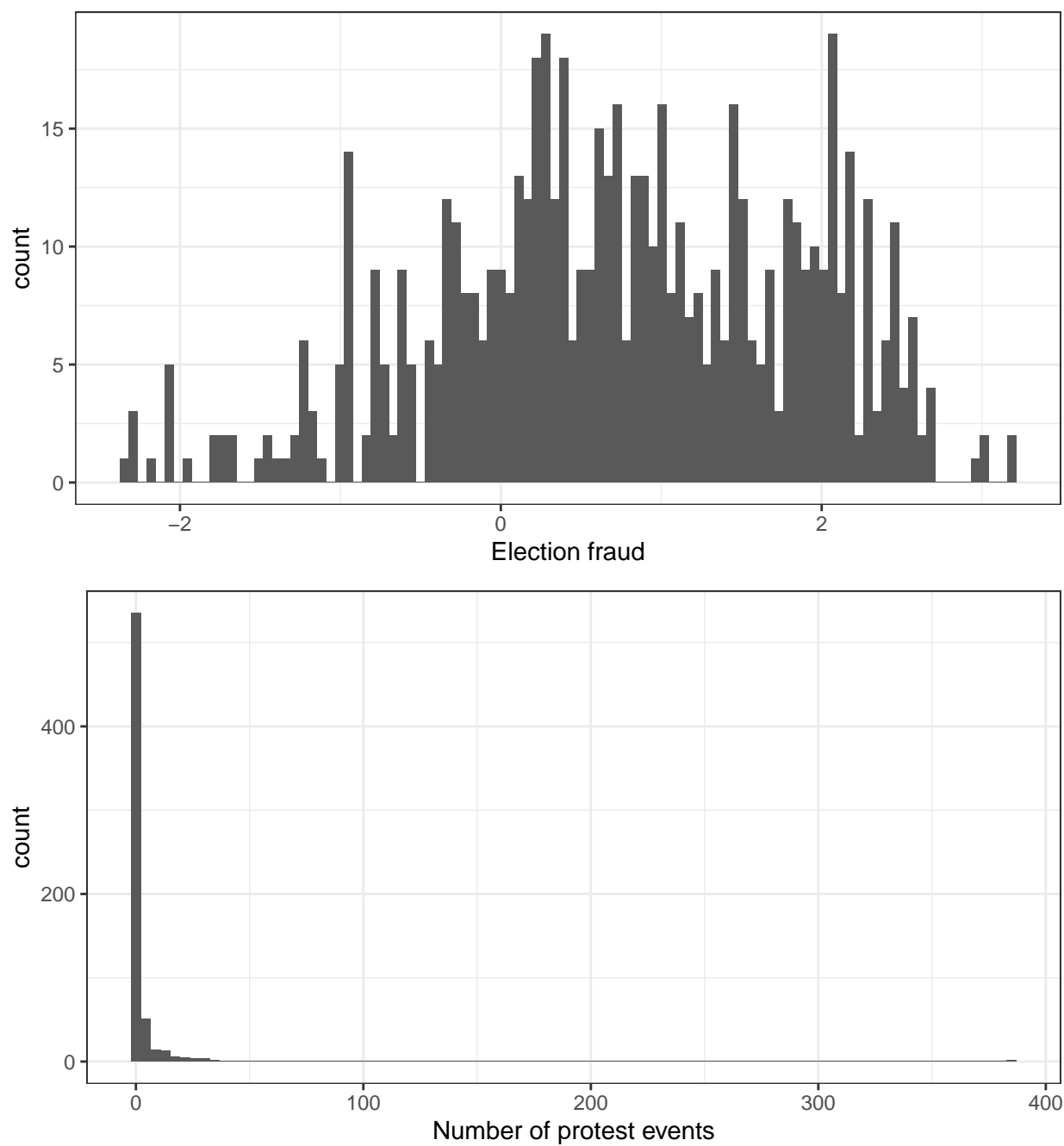


Figure 1: Distributions of election fraud and post-election protest

asures of economic grievance include an indicator of *public sector corruption* and the *lagged GDP growth rate*, both taken from V-Dem, and a binary measure of *economic crisis* from the NELDA dataset. Higher levels of corruption, negative GDP growth rates, and a positive value for *economic crisis* should each be expected to increase citizens’ sense of grievance. To capture the *severity of election fraud*, I use the V-Dem measure of intentional election irregularities, a category which includes “double IDs, intentional lack of voting materials, ballot-stuffing, misreporting of votes, and false collation of votes.” I operationalize electoral manipulation using this variable, rather than aggregate measures of election integrity, because fraud of this kind is thought to be most likely to trigger protest (Linebarger and Salehyan 2020)—it is often incumbents’ tool of last resort (Sjoberg 2016), it can fail to signal strength (Harvey and Mukherjee 2020), and it can be more damaging to legitimacy than pre-election manipulation (Birch 2011; Szakonyi 2021). This measure of election integrity should thus pose a strong test of the protest-oriented model. Finally, the winning party’s *vote-share* is also taken from the V-Dem dataset.⁴

Several control variables are included, which could be common causes for both the severity of election manipulation and protest. To help control for the underlying mobilizational capacity, which likely affects incumbents’ decision to manipulate and the latent risk of protest, I include lagged indicators of government *control over civil society* (the ‘v2cseeorgs’ variable from V-Dem) and the proportion of national political parties with *permanent party organizations* (V-Dem’s ‘v2psorgs’). Likewise, the ability of the state to engage in repression is likely to embolden the ruling party and deter protests; I thus control for *physical integrity* using the ‘physical violence index’ from V-Dem lagged one year. Higher values for this variable indicate greater freedom from torture and political killing. Since intense repression may itself become a grievance that triggers further protest, I also include the variable’s square term.

Next, all models include three measures of the degree of political openness in the country. The first of these is an indicator variable for *electoral democracy*; the reference category is

⁴This variable has considerable missingness, which I reduced in part by updating missing values in V-Dem using data from Wikipedia and Psephos.

electoral autocracy. The second and third are *judicial constraints* and *legislative constraints* on the executive, V-Dem variables which captures the extent to which the judicial and legislative bodies in the country act independently in practice to limit executive power. Since urban areas can make electoral manipulation more difficult (Larreguy, Marshall, and Querubin 2016; Ziblatt 2009) while also making protest more likely (Wallace 2013), I also include data on *urbanization* taken from the United Nations (Department of Economic and Social Affairs (Population Division) 2019).

Finally, several binary control variables are included. First, a variable from NELDA is used to indicate *presidential* elections, since these may include both higher levels of manipulation (Simpser 2011) and a greater risk of protest. Next, a variable from NELDA indicates if the chief executive is *term limited* during the election at hand. This variable is included since incumbents running up against constitutional term limits can pose major challenges for the unity of the regime, possibly leading to fractured manipulation efforts and post-election protest (Hale 2014). Finally, I include dummy variables that provide a categorical evaluation of state capacity. These are taken from the ordinal version of the V-Dem variable “state source of fiscal revenue” (v2stfisccap). If the V-Dem data codes a state as not capable of raising revenue or primarily relying on loans and foreign aid, I code it as a *weak state*. If the state is described as primarily relying on resource rents and expropriation, I code it is a *rentier state*. States that rely primarily on taxes on property or economic transactions are coded as *tax states*. This distinction is relevant since access to natural resource rents may both make protest less likely and repression more effective (Girod, Stewart, and Walters 2018). Lastly, since strong non-democratic governments may be able to adjust the electoral calendar strategically (to affecting both election manipulation and protest risk), I include a variable from NELDA that indicates *off-schedule elections*. Figure 2 shows summary statistics for all of these variables. To address concerns about multicollinearity, I present models in the appendix (page 25) that exclude control variables associated with pre-election protest.

4 Results

4.1 Pre-election protest and election-day fraud

The results of these models are not promising for argument that protest risk deters election manipulation. First, while Hypothesis 1 is supported in Model 1 in Table 1—the coefficient on *pre-election protest* is negative and significant—the substantive effect of the relationship is minuscule. The model predicts that each additional pre-election protest event is associated with a reduction in the fraud measure of 0.007. This is a very small effect, given that the dependent variable ranges from -2.329 to 3.201. A very large increase in the degree of protest, from zero to the ninetieth percentile (15 events) is associated with a 0.105 reduction in election-day fraud. This represents less than ten percent of a standard deviation of the election fraud variable. So, while pre-election protest (an indicator of post-election protest risk) is indeed associated with reduced election-day fraud, this reduction is only likely to be noticeable in extreme circumstances. For example, pre-election protest in the 99th percentile is associated with a reduction of 0.53256, a bit less than half of a standard deviation. This suggests that the level of election manipulation is not highly responsive to changes in the pre-election protest environment.

Table 1: Random-intercept multilevel model of election-day fraud

	Model 1
(Intercept)	2.252*** (0.218)
Judicial independence (lag)	−1.557*** (0.286)
Leg. constraints (lag)	0.785** (0.282)
GDP growth rate (lag)	0.114 (0.077)
Alternative info. (lag)	0.010 (0.341)
Civil soc. openness (lag)	0.091 (0.066)
Economic crisis	0.050 (0.055)
National party organization (lag)	−0.110+ (0.057)
Presidential election	0.068 (0.046)
Incumbent term-limited	0.064 (0.089)
Liberal democracy index (lag)	−3.077*** (0.559)
Urbanization	−0.009** (0.003)
Rentier state	0.103 (0.127)
Tax state	0.250+ (0.135)
Number of pre-election protests	−0.006** (0.002)
Median size of pre-election protests	0.009 (0.018)
Off-schedule election	−0.176*** (0.050)
SD (Intercept stateid)	0.559
SD (Observations)	0.446
Num.Obs.	533
AIC	949.6
BIC	1030.9
ICC	0.6

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.2 Election-day fraud and protest risk

The second set of models also challenge the conventional wisdom that more severe electoral manipulation is associated with an increased risk of anti-government protest, except under extraordinary circumstances. Beginning with the binary portion of the hurdle models, which model the likelihood of any protest occurring after the election, the standalone coefficient for election fraud in Model 2 is not statistically significant. Model 3 interacts election fraud with the incumbent's official vote-share; here we do observe statistically significant relationships at the $p < .05$ level. However, as the marginal effects plot in Figure 2 shows, this relationship is only statistically significant when comparing extreme scenarios.

While more fraudulent elections are associated with an increasing risk of protest as margins of victory shrink, this increase is not statistically significant for typical values of manipulation. In the bottom panel of Figure 2, typical values for low and high levels of fraud are used (one standard deviation below and above the mean, respectively). In this lower panel, the increased risk of protest in the high-fraud scenario is not statistically distinguishable from the low-fraud scenario. It is only when comparing extreme values, as in the top panel where the minimum and maximum values for election fraud are used, that the increase in risk becomes significant. It is also worth noting that the inclusion of the interaction term does not improve the fit of the model compared to the baseline Model 2. In summary, it appears that though the strategic logic of protest can be detected—protest appears more likely when fraud is high and winning margins low—this pattern only emerges in unusual scenarios. This finding, where the relationship between fraud and protest is only visible at the margins, is similar to that observed in Model 1 above.

A similar, but somewhat stronger effect, can also be seen in the relationship between fraud, public sector corruption, and protest. As shown in Figure 4, higher levels of fraud are associated with higher risk of protest when corruption is high. This effect is not statistically significant for typical values of fraud (one SD above and below the mean), but quickly becomes so when comparing more disparate levels of fraud. At the maximum values for

fraud, as the top panel shows, the difference in risk is significant even for modestly elevated levels of corruption. This is at least partially in line with the predictions of the literature following Tucker (2007), though the relationship is not significant when comparing more typical levels of fraud. Finally, there is no significant effect for *economic crisis*, at least under the typical covariate values used to create the marginal effects plot in Figure 3.

Table 2: Binary portion of hurdle models of post-election protest

	Model 2	Model 3	Model 4	Model 5
Intercept	−3.457** (1.222)	−4.096** (1.279)	−3.205*** (0.926)	−2.647** (0.902)
Judicial independence (lag)	2.069* (0.986)	2.160* (0.985)	0.661 (0.797)	1.076 (0.794)
Leg. constraints (lag)	0.156 (0.854)	0.156 (0.843)	1.088 (0.775)	0.892 (0.761)
GDP growth rate (lag)	2.483+ (1.394)	2.599+ (1.421)		
Alternative info. (lag)	−0.357 (1.379)	−0.137 (1.379)	−0.179 (1.310)	−0.095 (1.292)
Public sector corruption (lag)	0.069 (0.161)	0.004 (0.167)	0.071 (0.153)	−0.157 (0.174)
Civil soc. openness (lag)	0.128 (0.239)	0.102 (0.240)	0.062 (0.229)	−0.019 (0.224)
Economic crisis	−0.090 (0.294)	−0.143 (0.296)	0.352 (0.316)	
National party organization (lag)	−0.030 (0.189)	0.012 (0.191)	−0.028 (0.171)	−0.016 (0.175)
Election fraud	0.339+ (0.205)	0.899* (0.394)	0.233 (0.193)	0.353+ (0.200)
Incumbent vote-share	−0.431 (0.801)	0.745 (1.063)		
Presidential election	−0.143 (0.301)	−0.148 (0.301)	−0.189 (0.263)	−0.191 (0.260)
Incumbent term-limited	−0.037 (0.463)	−0.108 (0.467)	0.113 (0.438)	0.152 (0.437)
Physical integrity (lag)	4.116 (3.042)	3.571 (3.062)	4.566+ (2.658)	2.032 (2.676)
Physical integrity (lag), squared	−4.852+ (2.635)	−4.547+ (2.642)	−5.414* (2.335)	−2.897 (2.346)
Urbanization	0.008 (0.008)	0.009 (0.008)	0.005 (0.007)	0.007 (0.007)
Rentier state	−0.490 (0.455)	−0.562 (0.463)	−0.646 (0.396)	−0.838* (0.395)
Tax state	0.051 (0.422)	0.036 (0.424)	0.124 (0.353)	−0.091 (0.359)
Number of pre-election protests	0.046*** (0.011)	0.046*** (0.011)	0.050*** (0.011)	0.052*** (0.011)
Off-schedule election	0.469 (0.291)	0.460 (0.292)	0.354 (0.259)	0.228 (0.256)

Election fraud:winner.share.upd		-1.094+		
		(0.656)		
Economic crisis:Election fraud			-0.626*	
			(0.269)	
Public sector corruption (lag):Election fraud				0.316**
				(0.113)
Num.Obs.	462	462	546	559
AIC	1137.0	1137.5	1320.7	1348.8
BIC	1306.6	1315.3	1488.5	1508.8
RMSE	25.72	24.69	63.18	31.01
+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001				

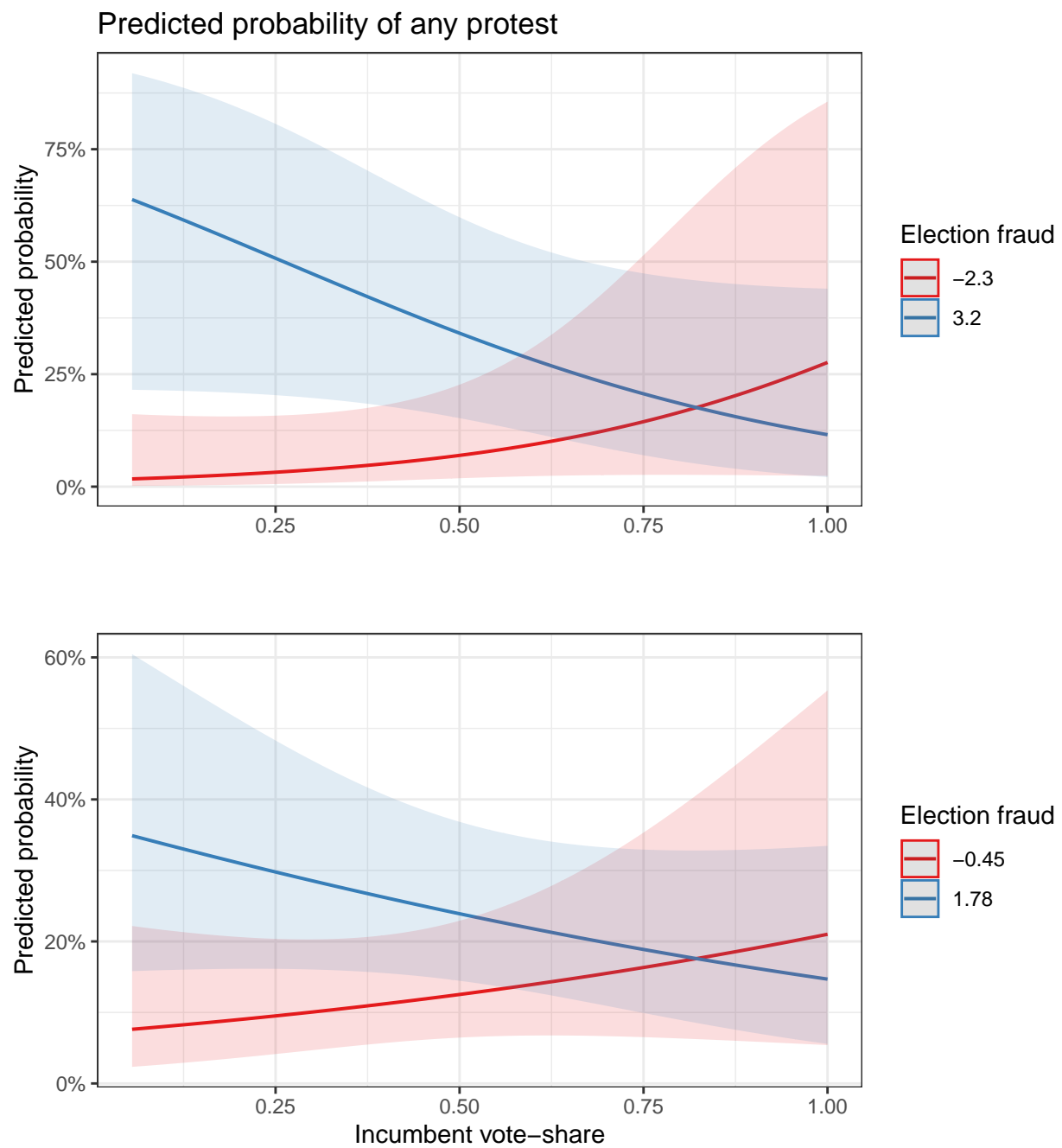


Figure 2: Fraud, winning vote-share, and probability of protest initiation

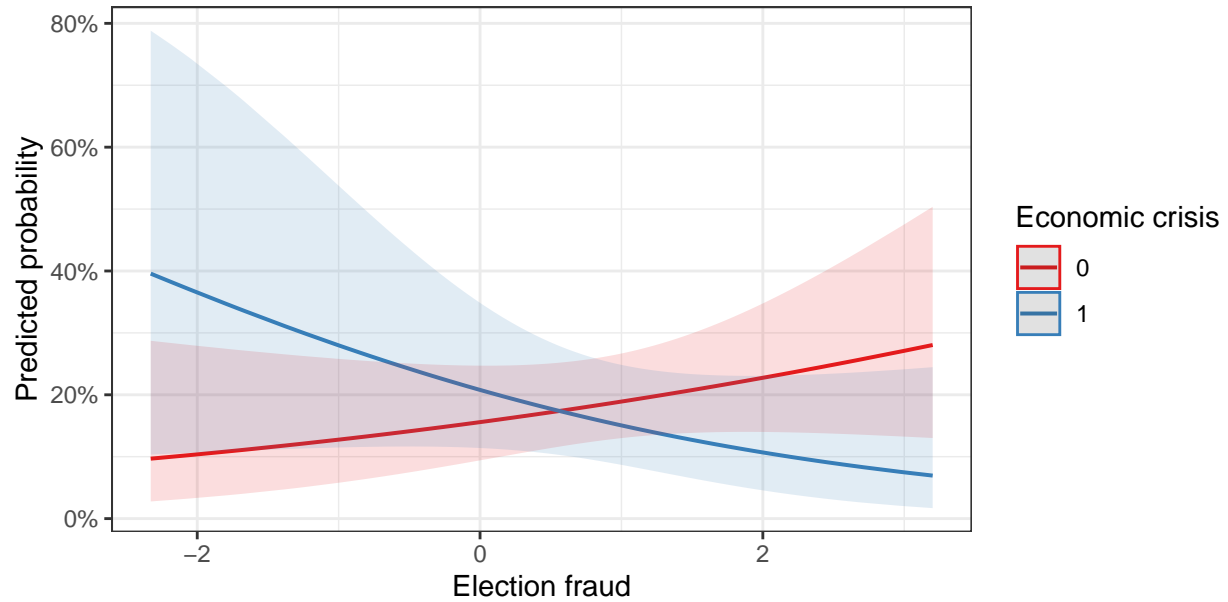
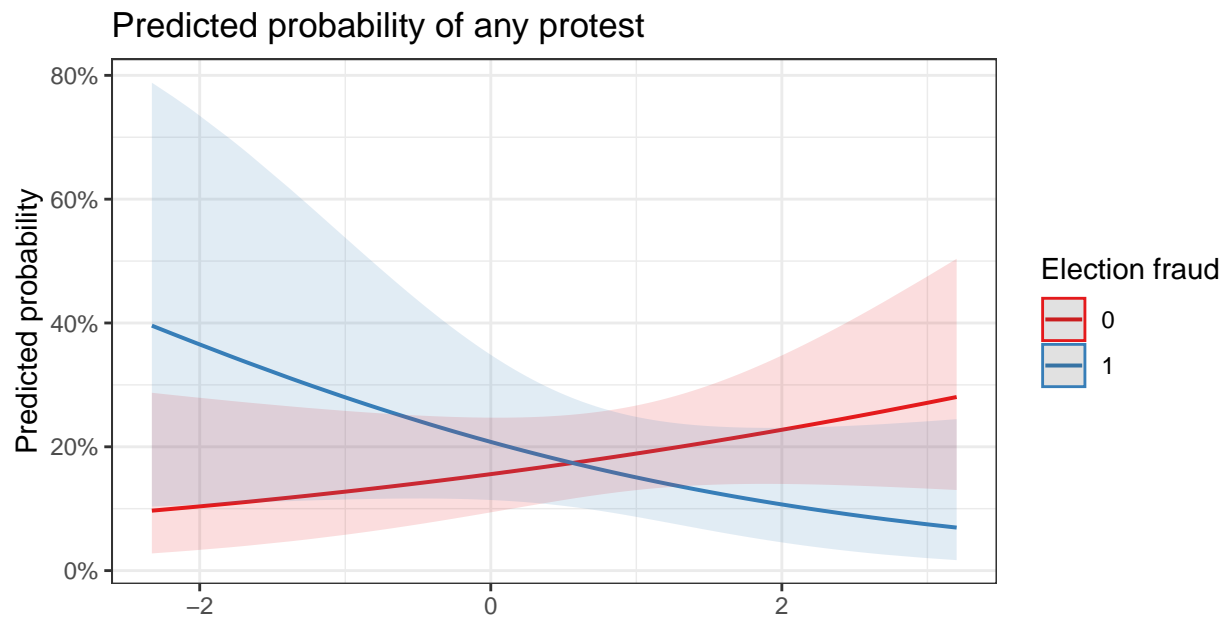


Figure 3: Fraud, economic crisis, and probability of protest initiation

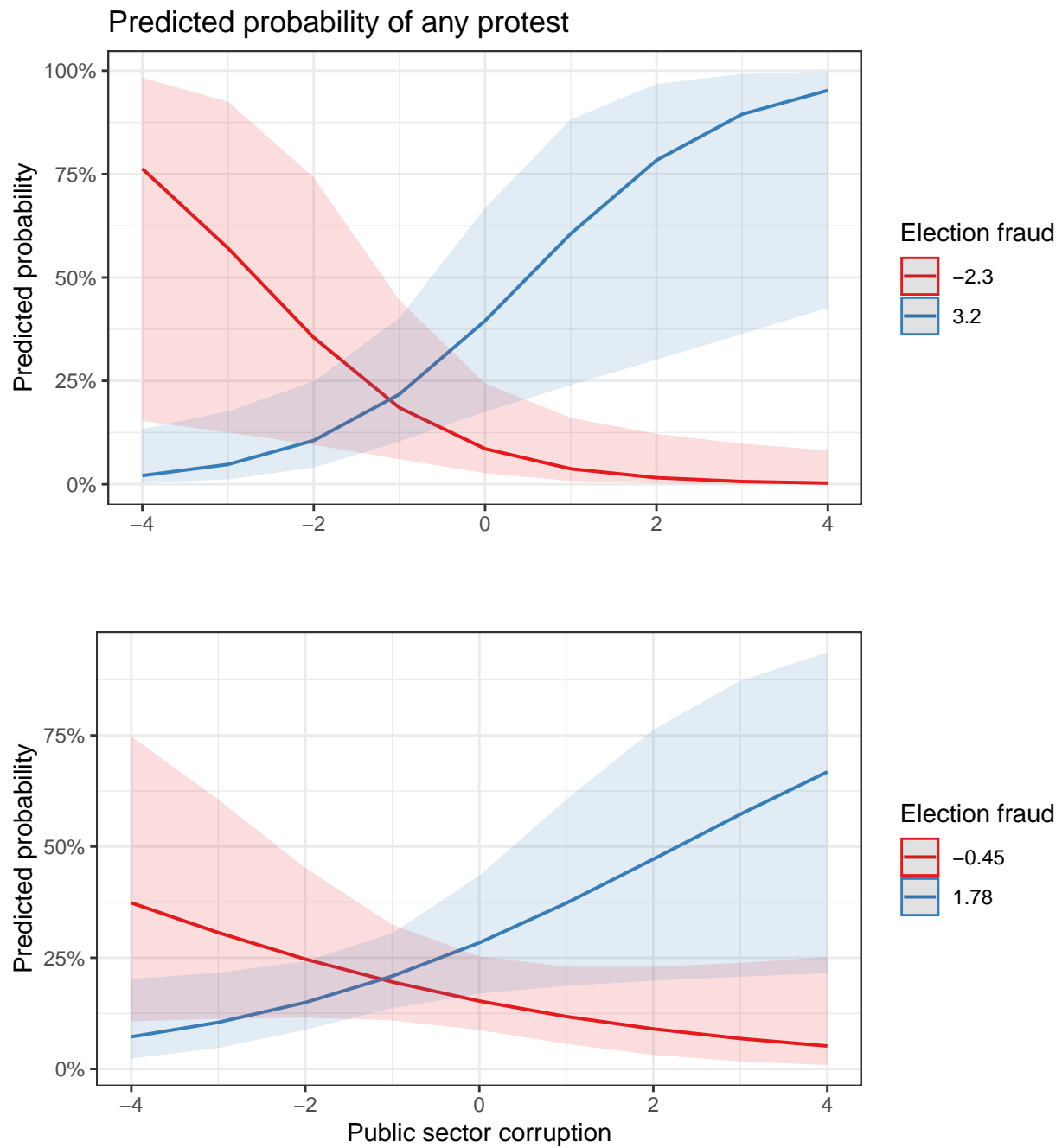


Figure 4: Fraud, public sector corruption, and probability of protest initiation

4.3 Election-day fraud and number of post-election protests

Turning to the models of number of protest events shown in Table 3, Model 2—which includes no interaction terms—shows no significant relationship between fraud and protest in the count portions of the hurdle regression. Similarly, none of the three models that include interaction terms yield a significant relationship between fraud, the conditional variable, and number of protest events. Model 3 shows no significant interaction effect for fraud and winning vote-share on quantity of protest. Figure 5 shows the marginal effect of election fraud at varying levels of winning vote-share, holding other variables constant at their means (continuous variables) or modes (categorical variables). The high and low levels of fraud in the plot represent one standard deviation above and below the mean, respectively. As the figure shows, the predicted number of post-election protests is somewhat higher in the high fraud condition when winning margin is low, but this relationship is not statistically significant.

Neither economic crises nor public sector corruption is associated with a significant marginal effect for typical levels of election fraud on protest quantity, as Figures 6 and 7 show.⁵ In fact, more fraudulent elections appear to be associated with *fewer* incidents of protest during periods of economic crisis, though this distinction is not statistically significant at the $p < .05$ level.

Altogether, the argument that election fraud increases the risk of protest initiation is mixed. Hypothesis 1 is weakly supported: pre-election protest is associated with reduced election-day fraud, but the substantive effect of this relationship is minimal. Hypothesis 2 is also weakly supported; there is a significant and sizable association between election fraud and protest initiation when the ruling party's winning margin is low, but only at extremely low and high values of fraud. Support for Hypothesis 3 is mixed—fraud severity is not associated with increased protest risk during economic crises, though there is a significant

⁵Figures demonstrating minimum and maximum values for fraud are shown in the appendix (page 5); they also indicate no significant relationship.

and sizable increase in the marginal effect as public sector corruption increases. Election fraud appears to be associated with a higher risk of protest initiation, but generally in unusual scenarios—when comparing a highly fraudulent to a largely clean elections when the incumbent’s margin of victory is quite low, for example—or with meager substantive effect.

However, even this mixed finding likely presents too optimistic a picture for the protest-deterrent model of election. fraud. The count results from the hurdle regressions show no effect for election fraud across any of the models, alone or in interaction. Elections that were more fraudulent are not associated with more numerous protest. Even when protests are initiated—a relatively rare event in the first place—the size of a protest wave is not influenced by the degree of fraud perpetrated by the ruling party. In turn, this suggests two pessimistic interpretations of fraud-protest dynamic. First, protest may simply be less damaging to ruling parties than is regularly assumed in theories of election manipulation. Faced with a nascent protest movement, ruling parties are generally able to contain, co-opt, or repress them—regardless of the severity of election fraud. An even more troubling implication would suggest that the protest-manipulation logic, rather than serving as a deterrent, creates perverse incentives to manipulate as much as possible. If ruling parties do indeed seek to avoid protest, but manipulating heavily does not come with a cost in the number of post-election protests, ruling parties have an incentive fraudulently inflate their official margins of victory in order to reduce the risk of protest initiation.

Table 3: Hurdle models of post-election protest (counts)

	Model 2	Model 3	Model 4	Model 5
Intercept	3.216* (1.483)	2.875+ (1.550)	1.302 (1.219)	1.477 (1.402)
Judicial independence (lag)	−0.931 (1.142)	−1.108 (1.184)	−1.407 (0.877)	−0.980 (0.996)
Leg. constraints (lag)	2.144* (0.850)	2.182* (0.855)	2.666*** (0.742)	2.571** (0.822)
Alternative info. (lag)	−1.228	−1.189	−2.184	−1.987

	(1.435)	(1.446)	(1.355)	(1.520)
GDP growth rate (lag)	−1.133 (2.065)	−0.669 (2.130)		
Public sector corruption (lag)	−0.241 (0.191)	−0.241 (0.191)	−0.337+ (0.181)	−0.354 (0.234)
Civil soc. openness (lag)	0.132 (0.271)	0.163 (0.275)	0.062 (0.263)	−0.009 (0.298)
Economic crisis	0.234 (0.363)	0.233 (0.362)	0.724+ (0.384)	
National party organization (lag)	0.409* (0.187)	0.376* (0.190)	0.198 (0.140)	0.203 (0.160)
Election fraud	0.207 (0.249)	0.647 (0.614)	0.018 (0.225)	0.006 (0.318)
Incumbent vote-share	−0.885 (1.093)	−0.279 (1.331)		
Presidential election	0.362 (0.348)	0.268 (0.370)	0.561+ (0.317)	0.665+ (0.370)
Incumbent term-limited	−0.826+ (0.481)	−0.814+ (0.478)	−0.648 (0.465)	−0.969+ (0.518)
Physical integrity (lag)	−5.681 (4.228)	−5.888 (4.244)	−0.346 (3.517)	−2.335 (3.891)
Physical integrity (lag), squared	4.388 (3.618)	4.503 (3.630)	0.438 (3.064)	3.111 (3.307)
Urbanization	0.012 (0.008)	0.015+ (0.009)	0.008 (0.007)	0.008 (0.008)
Rentier state	−1.045+ (0.607)	−1.047+ (0.608)	−0.242 (0.510)	−0.676 (0.517)
Tax state	−0.279 (0.539)	−0.171 (0.553)	0.614 (0.411)	0.530 (0.458)
Number of pre-election protests	0.010*** (0.003)	0.010*** (0.003)	0.008** (0.003)	0.009** (0.003)
Off-schedule election	0.298 (0.357)	0.324 (0.359)	0.260 (0.345)	0.255 (0.356)
Election fraud:winner.share.upd		−0.888 (1.126)		
Economic crisis:Election fraud			−0.755+ (0.421)	
Public sector corruption (lag):Election fraud				0.053 (0.211)
Num.Obs.	462	462	546	559
AIC	1137.0	1137.5	1320.7	1348.8
BIC	1306.6	1315.3	1488.5	1508.8
RMSE	25.72	24.69	63.18	31.01

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

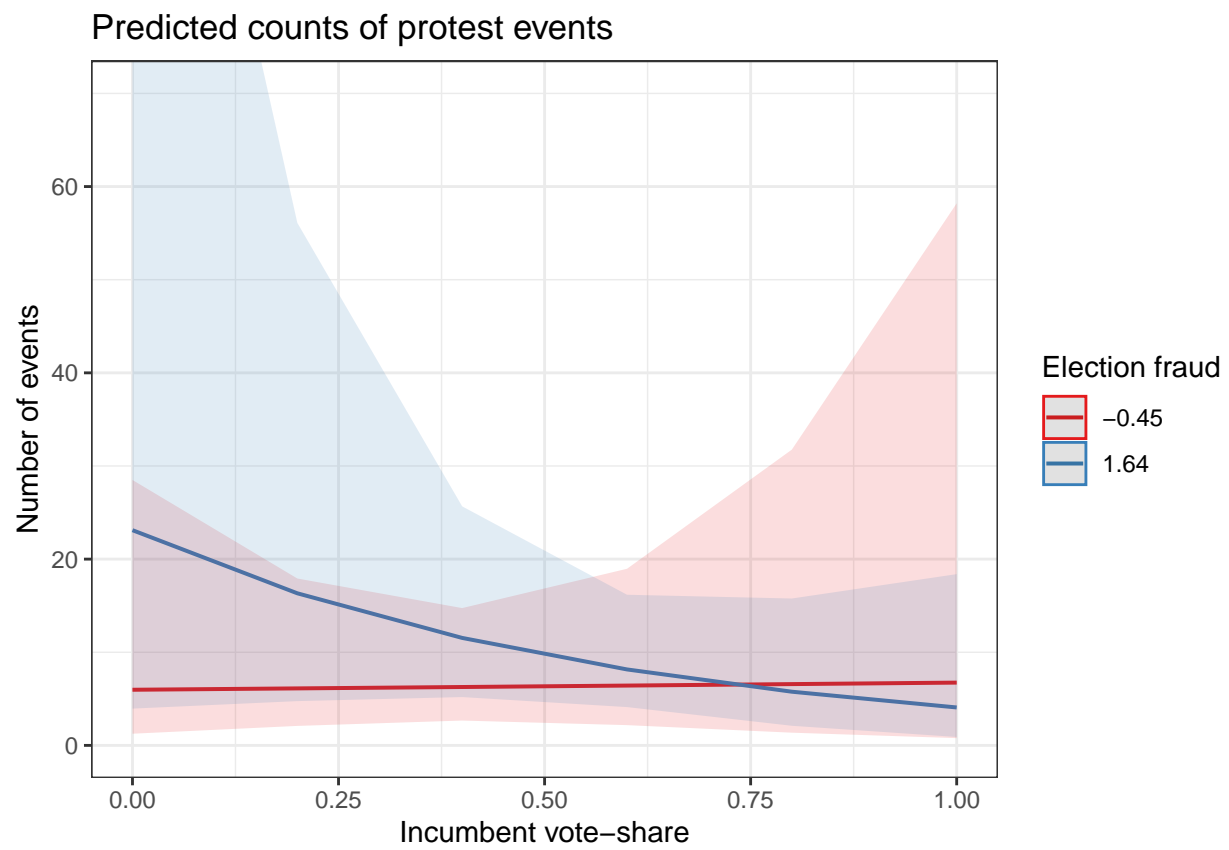


Figure 5: Fraud, incumbent vote-share, and number of protest events

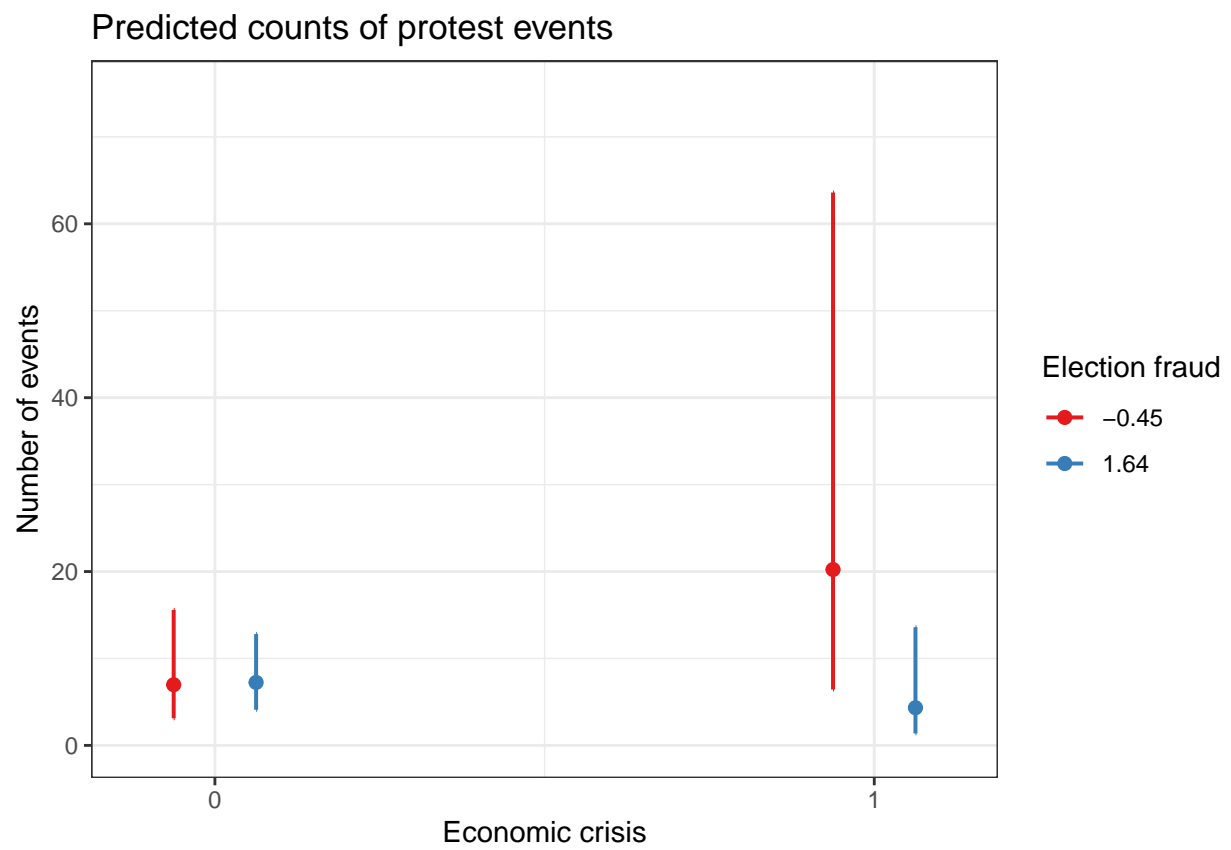


Figure 6: Fraud, economic crisis, and number of protest events

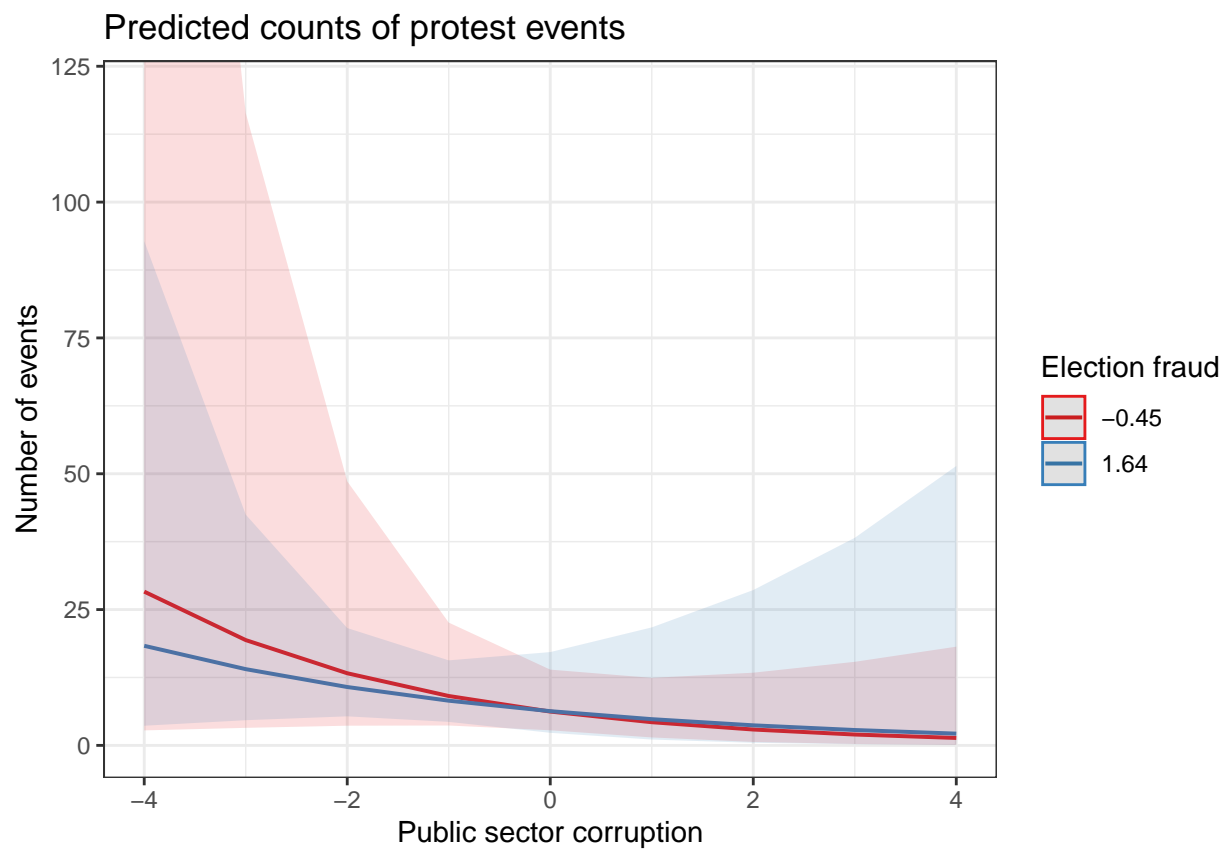


Figure 7: Fraud, public sector corruption, and number of protest events

5 Discussion

Altogether, the findings challenge the idea that the fear of protest will deter election-manipulation by ruling parties. To the extent that fraud is a signal of incumbent strength, it is a noisy and unreliable one. When an incumbent party wins by a narrow margin in a highly fraudulent election, the likelihood of protest initiation is substantially higher than when it wins more cleanly: roughly a 50 to 60% chance in the former case versus 5 to 10% in the latter as shown in Figure 2. However, since even strong incumbents commit fraud, the decision to take to the streets is often a losing gamble—anticipating a weak incumbent, they instead find one that is capable of repressing, coopting, or conciliating an emerging protest movement. Indeed, while there are 143 cases of protest out of the 647 elections in this dataset, only 9 elections resulted in either the incumbent being replaced or the election being re-run due to mass protests (according to NELDA).⁶ This suggests a ‘success rate’ for post-election protest of about six percent *conditional on protest initiation*, and about one percent for all elections in electoral authoritarian and electoral democratic regimes.

The results indicate that we should be cautious when building and interpreting theories of non-democratic politics that emphasize the risk of mass protest as a trip-wire to be avoided, and add empirical evidence to Przeworski’s (2022) critique of such models. The rarity of post-election protest, especially successful protest, suggest that such events are highly contingent; they hinge on a multitude of factors that go beyond the regime’s efforts to bias election outcomes. Election fraud is a complex phenomenon with multiple drivers, including resource costs, signaling, information-gathering, principal-agent dynamics, and more; these findings indicate that protest risk is not a significant factor under most circumstances.

This has several implications for understanding election manipulation and regime resilience. By overlooking the poor quality of election fraud as a signal, or by treating protest primarily as a binary event, the existing literature on electoral protest has overemphasized

⁶This information is determined using the NELDA variables NELDA37 and NELDA41. The cases of successful protest are: Ukraine 2004, Azerbaijan 2005, Georgia 2003, Kyrgyzstan 2005, Bulgaria 1990, Bangladesh 1996, Mali 1997, Peru 2000, and Cote d’Ivoire 2000.

the risk to incumbents of engaging in electoral manipulation. This results presented here show that fraud has no bearing on how numerous protest events are, and only little influence on protest initiation. While pre-election protest is associated with reduced fraud, the substantive size of this relationship is very small. Moreover, contrary to some prior research, the risk of a large protest wave does not appear to be heightened by poor economic conditions ([Brancati 2014](#)) or intensive corruption in the public sector. As a result, circumstances that are to some extent outside of leaders' control, like a worsening macroeconomic climate, do not appear to make engaging in fraud any riskier. Instead, the conditions that are correlated with more damaging protest are generally institutional factors, which are either more amenable to control or can be anticipated by non-democratic incumbents. Legislative constraints, an important predictor of the number of protests in all four models, can be adjusted through institutional reforms and cooptation of the opposition over time. In other words, election fraud only has a minor cost in terms of protest risk, a risk which can be further blunted to the extent that elections can often be held on incumbents' terms.

These are pessimistic findings for proponents of democracy. That electoral protest is less risky than is often assumed, in turn, implies that governments and ruling parties have a freer hand to engage in electoral manipulation than many models posit. Since the severity of fraud has no apparent bearing on whether or not a major protest wave will emerge as a serious threat to the regime, as shown in Figure 3, government have an incentive to manipulate as much as possible to secure a large winning vote-share. This perverse incentive undermines the case that democracy, and fair elections generally, are a self-enforcing equilibrium backed up by the risk of citizen protest ([Fearon 2011b](#)).

One objection to this framing may be that, if the level of manipulation and the risk of protest are endogenous to one another, perhaps the null result found for the number of protests is to be expected. That is, if incumbents choose a level of manipulation calculated to avoid large-scale protest—sometimes high and sometimes low, conditional on contextual factors—no correlation between fraud and protest quantity would be found. This paper

does not dispute that decisions about manipulation and protest may be endogenous to one another, but it does argue that the resulting equilibrium favors governments. In the dataset used here, only ten elections in four countries are coded as having an *intentional irregularities* score of less than -2 (indicating extremely fraud-free elections). By contrast, 103 elections in 27 countries score higher than 2 (indicating highly fraudulent elections). The frequency of fraud, the rarity of protest, and the finding that fraud severity is not correlated with the scope of protest events all point to the same conclusion: protest risk is not a substantial constraint on election fraud in non-democracies. Rather, the constraints on manipulation are more likely to be driven by factors like resource availability (Greene 2007), principal-agent problems (Rundlett and Svolik 2016), and legal risks to manipulators (Harvey 2022).

These results are consistent with different specifications presented in an online appendix. In particular, the main results are supported when data from the Electoral Contention and Violence (ECAV) dataset (Daxecker, Amicarelli, and Jung 2019) are used in place of the Mass Mobilization data (appendix page 1), and when a measure of overall election integrity is used in place of the measure of election fraud (appendix page 15).

6 Conclusion

Protest risk is often considered to be a major risk to incumbent governments aiming to rig elections. This risk has a central role in several prominent models of electoral manipulation, serving to deter manipulation and to incentivize the toleration of election monitors and other restraints on the ruling party. However, I argue that such models overlook the ambiguity inherent in election manipulation efforts, as well as the quick decay of even such limited information once protests begin. Empirical research on the subject has largely treated protest as a binary variable, an approach which obscures the distinction between small, one-off protests and much more costly protest waves.

This paper addresses these concerns by testing the relationship between election fraud,

the risk that any protest occurs, and the number of protest events that occur after an election. Drawing on data from the V-Dem, NELDA, and Mass Mobilization datasets, it uses cross-national data on 647 elections to show that—while there is an elevated risk of some protest occurring in a fraudulent election with narrow margins of victory or in highly corrupt societies—election fraud has no significant relationship with the number of protests that occur in a protest wave. These results imply that protest risk should not be considered a central deterrent to election fraud. Instead, most fraudulent elections go un-protested, and most post-election protests are strategic gambles gone wrong. The initiation of protest more often reveals the incumbent to be strong enough to manage protest through repression, cooptation, or other concessions. These findings indicate that, rather than being deterred by protest risk, governments have an incentive to manipulate as much as possible in order to achieve a large official vote-share. The evidence suggests that democratic breakthroughs in non-democratic elections are rare and unlikely to be driven by the severity of manipulation—a gloomy picture for supporters of democratization—and that researchers should consider other mechanisms beyond mass protest in their models of electoral manipulation.

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