**Appendix: Methods of Election Manipulation and the Likelihood of Post-Election Protest**

*Government and Opposition*

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1 Analysis using V-Dem Data

In order to increase the geographic and temporal scope of our observations, we draw on data from V-Dem as a further test of our hypotheses (Coppedge et al. 2017). While our primary dataset has observations ranging from 1980 to 2004, measurements of election manipulation have been coded by the V-Dem Project’s experts going back to 1945. Likewise, our V-Dem dataset includes observations from 123 countries, while our main dataset includes 104. We believe the data presented in the main text, using DIEM (Kelley 2012), is best suited to our research question due to the fact that the coding in that dataset is drawn directly from election-monitoring reports. However, the larger V-Dem dataset allows us to test our theory on additional, out-of-sample data. The results are supportive of the hypotheses discussed in the main text. In this section, we describe the specifics of the V-Dem data and analysis, and present the results.

First, since our theory is targeted toward authoritarian, hybrid, and unconsolidated democratic regimes, we exclude country-cases with a Polity score greater than eight from our V-Dem sample. We then merge the V-Dem data with the NELDA dataset, which also ranges from 1945 to 2012, based on the unique NELDA identifier for each country-election observation. This yields a combined dataset with 815 unique observations. As in the primary analysis, our dependent variable on post-election protest is drawn from NELDA, along with the election-level control variables transitional election, opposition vote gain, and prior election protest.

V-Dem provides indicators of three types of electoral manipulation, as coded by country experts: voting irregularities, vote-buying, and intimidation. The variable voting irregularities is a measure of generally administrative forms of manipulation, including “intentional lack of voting materials, ballot-stuffing, misreporting of votes, and false collation of votes” (Coppedge et al. 2017: 95). As such, we consider it a useful complement to our administrative fraud variable in the main text. In the original V-Dem coding scheme, higher scores on these variables indicate a cleaner election; in order to make interpreting the results easier, we have inverted the scores so that higher values for voting irregularities, for example, indicates a more severe effort to tamper with the election.

The other control variables we include in the V-Dem models are similar to those in the main text. We include the categorical competitiveness of political participation variable from Polity (PARCOMP), whether or not the observation is of an executive election, and logged GDP per capita. Lastly, we include a measure of election-related violence that is not conducted by government or ruling party agents. This helps us control for overall instability around the election, which could affect protest risk, without inadvertently capturing additional efforts by the government to harass or intimidate opposition actors.

As in the main text, we use logit models to analyze the data, since the dependent variable is binary. However, since in this case we have considerably more observations per country, we use multilevel logit models with intercepts that are allowed to vary by country. This partial-pooling approach helps account for variation across cases and within cases over time (Gelman and Hill 2006). The results of three models are presented below: the first tests the relationship between voting irregularities (that is, administrative fraud) and protest as a standalone variable, while the latter two include interaction terms with the other two forms of election manipulation.

Table 1: Multilevel logit models of post-election protest, V- Dem data

|  |  |  |  |
| --- | --- | --- | --- |
|  | Post-election protest | | |
|  | (1) | (2) | (3) |
|  |  |  |  |
| Voting irregularities | 0.39\*\* | 0.45\*\* | 0.60\*\*\* |
|  | (0.17) | (0.17) | (0.19) |
| Vote-buying | −0.10 | 0.07 | −0.10 |
|  | (0.15) | (0.19) | (0.14) |
| Intimidation | 0.02 | 0.05 | 0.26 |
|  | (0.16) | (0.16) | (0.20) |
| Opposition vote gain | −0.11 | −0.13 | −0.06 |
|  | (0.25) | (0.25) | (0.25) |
| Transitional election | −0.72\* | −0.70\* | −0.75\* |
|  | (0.41) | (0.41) | (0.41) |
| PARCOMP —Unregulated | 1.23 | -1.13 | -0.87 |
|  | (1.04) | (1.08) | (1.10) |
| PARCOMP —Repressed | −0.75 | −0.96 | −0.97 |
|  | (1.14) | (1.18) | (1.17) |
| PARCOMP —Suppressed | −0.33 | −0.43 | −0.65 |
|  | (0.98) | (1.03) | (1.04) |
| PARCOMP —Factional | −0.23 | −0.36 | −0.57 |
|  | (0.95) | (1.00) | (1.02) |
| PARCOMP —Transitional | −0.29 | −0.43 | −0.64 |
|  | (0.97) | (1.02) | (1.04) |
| Prior election protests | 1.65\*\*\* | 1.64\*\*\* | 1.66\*\*\* |
|  | (0.24) | (0.24) | (0.24) |
| Executive election | 0.63\*\*\* | 0.63\*\*\* | 0.68\*\*\* |
|  | (0.23) | (0.23) | (0.23) |
| Election-related violence | −0.44\*\*\* | −0.45\*\*\* | −0.39\*\*\* |
|  | (0.12) | (0.12) | (0.12) |

|  |  |  |  |
| --- | --- | --- | --- |
| Log GDP per capita | 0.17 | 0.21 | 0.21 |
|  | (0.15) | (0.15) | (0.15) |
| Voting irregularities:Vote-buying |  | −0.15 |  |
|  |  | (0.10) |  |
| Voting irregularities:Intimidation |  |  | −0.26\*\* |
|  |  |  | (0.12) |
| Constant | −3.44\*\* | −3.61\*\* | −3.44\*\* |
|  | (1.55) | (1.58) | (1.56) |
|  |  |  |  |
| Observations |  | 644 |  |
| Groups |  | 108 |  |
| BIC | 665.07 | 669.42 | 666.05 |

\*p < .1; \*\*p < .05; \*\*\*p < .01

As Model 1 in Table 1 shows, voting irregularities has a positive and significant relationship with electoral protest, while vote-buying and electoral intimidation do not. Furthermore, Models 2 and 3 show that as vote-buying and intimidation increase in severity, the risk of protest associated with administrative forms of manipulation declines. Figures 7 and 8 illustrate this relationship graphically. In both cases, increases in voting irregularities are associated with an increased risk of protest when vote-buying or intimidation are rare or non-existent. However, as these more resource-intensive forms of manipulation increase in severity, the marginal effect of administrative forms of manipulation on protest declines until it is statistically indistinct from zero. As a result, the V-Dem data support Hypotheses 1-4 in addition to the narrower dataset used in the main text.

2 Multiple Imputation

The bulk of the missing observations in the dataset are attributable to the economic variables: unemployment, inflation, GDP growth, and GDP per capita. These values are more likely to be missing for developing countries. Dropping those observations can lead to biased parameter estimates because the missingness is not random (King et al. 2001). Because our data include multiple elections over time and across countries, we implement the multiple imputation procedure for time-series cross-sectional data proposed by Honaker and King (2010).

To reduce the risk that an outlier in the imputation process might drive the results, we created five distinct imputed datasets (Rubin 2004). Combining the results from all five datasets according the procedure laid out by Rubin (2004) accounts for variance within each estimate as well as across the imputed datasets, and avoids cherry-picking results from a favored dataset.

Here, we show the results of models relying on the individual imputed datasets, and for inter- actions of administrative fraud, mobilization, and intimidation using the imputed data. Figure 4 shows the results of the five individual imputed datasets (rather than the pooled coefficients and standard errors presented in the main text). In Figure 5 the models include extra-legal mobilization as a control variable, while voter intimidation is employed in Figure 6. The figures show that administrative fraud is positively and significantly associated with post-election protest, while extra-legal mobilization and voter-intimidation are not.

Figures 5 and 6 are produced using the Zelig statistical software program (Choirat et al. 2016; Imai et al. 2008), and show predicted and expected values of post-election protest under varying conditions, in order to demonstrate the interaction effect using the imputed data. In both cases, continuous control variables were held at their means and categorical variables at their medians. The variable administrative fraud is set to its maximum value (severe). To interpret the figures, condition X refers to values for the dependent variable when extra-legal mobilization or intimidation are held at zero (non-existent), while condition X1 refers to the case where mobilization or intimidation have been set to a value of 3 (severe). In Figure 5, for example, the distribution of expected values for post-election protest when extra-legal mobilization is severe is considerably negative compared to the base condition (note that the mean first difference is negative). This pattern is even more noticeable in Figure 6. For that model, the predicted probability of protest when administrative fraud is severe but electoral intimidation is non-existent is sixty percent. However, when intimidation is severe, but risk of protest falls to thirty percent. Likewise, the distribution of first differences in expected values between the two conditions is almost entirely negative.

3 Statistical Matching

Statistical matching is used to isolate the independent effect of each treatment variable on the probability of protest. The division of the treatment variables into binary treatments is theoretically justifiable based on the coding procedure used in the DIEM dataset (Kelley 2011). Minor problems were coded when election observation reports noted a problem in passing, using words like rare, uncommon, unusual, exceptional, and so on. By contrast, elections were coded as experiencing moderate problems if observation mission reports described violations that were “considerable” or “not uncommon” —somewhere between “negligible” and “egregious”. We consider that such activities do have a signaling effect and may affect the outcome of the election, especially given the incentives that election monitors may have to downplay their findings (Kelley 2012). Together with elections coded as having major problems, we mark such elections as treated.[[1]](#endnote-1)

Ho et al. (2007: 216) note that “[a]ll variables. . . that would have been included in a parametric model without pre-processing should be included in the matching procedure”, with the exception of variables that are themselves affected by the treatment (see also Stuart (2010)). They also recommend that binary variables with a large proportion of one outcome be excluded. We follow these guidelines by matching treatment and control groups along all 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 has the potential to be affected by the level and type of manipulation employed, 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, inflation, executive elections, ‘transitional’ participation, pre-election cheating, pre-election violence, extra-legal mobilization, and voter intimidation. Table 2 presents the percent improvement in the difference- in-means between the treatment and control groups after matching compared to the un-processed data, for Model 4 (shown below). Equivalent tables for the remaining models are available in the following section.

Table 2: Percentage Improvement in balance after matching (Model 4)

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Mean Diff. | eQQ Med | eQQ Mean | eQQ Max |
| Log GDP per capita (lagged) | 70.66 | 42.61 | 44.34 | 76.49 |
| Pre-election cheating | 74.95 | 100 | 47.27 | 50 |
| Pre-election violence | 77.29 | 100 | 56.58 | 50 |
| PARCOMP-Transitional | 100 | 0.00 | 100 | 100 |
| Prior election protest | 56.93 | 0.00 | 56.58 | 0.00 |
| Executive election | 77.05 | -30 | -3.56 | 24.19 |
| Unemployment (lagged) | 58.27 | 100 | 46.83 | 50 |
| Extra-legal mobilization | 46.38 | 0.00 | 45.56 | 50 |
|  | Treatment | Control |  |  |
| All | 66 | 118 |  |  |
| Matched | 66 | 38 |  |  |
| Unmatched | 0 | 80 |  |  |

As the table shows, this technique finds one match for each of the 62 treated observations, and prunes 40 control observations. Balance improves for most 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 variables improves by twenty-seven percent after matching. Improvement is similar in size for the other models which used matched data.

Tables 3 through 6 show the variables used to match treatment and control observations for the remaining models presented in Table 4 of the main text, and how the matching procedure improves balance between treatment and control groups across those variables. Matching improves balance consistently for all models except for executive, for which the results are mixed. In particular, balance is improved for the manipulation-related control variables extra-legal mobilization and election-day intimidation.

Table 3: Percent balance improvement, Model 5 (main text)

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Mean Diff. | eQQ Med | eQQ Mean | eQQ Max |
| Log GDP per capita (lagged) | 79.19 | 45.44 | 47.37 | 73.42 |
| Pre-election cheating | 74.95 | 100 | 54.17 | 50 |
| Pre-election violence | 67.19 | 100 | 54.17 | 50 |
| PARCOMP-Transitional | 100 | 0.00 | 8.33 | 0.00 |
| Prior election protest | 65.55 | 0.00 | 23.61 | 0.00 |
| Executive election | 18.06 | 0.00 | 38.89 | 0.00 |
| Unemployment (lagged) | 9.49 | -30 | -4.63 | 50 |
| Extra-legal mobilization | 70.79 | 100 | 47.62 | 50 |
|  | Treatment | Control |  |  |
| All | 66 | 118 |  |  |
| Matched | 66 | 36 |  |  |
| Unmatched | 0 | 82 |  |  |

Table 4: Percent balance improvement, Model 6 (main text)

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Mean Diff. | eQQ Med | eQQ Mean | eQQ Max |
| Log GDP per capita (lagged) | 70.30 | 54.28 | 45.30 | 73.42 |
| Pre-election cheating | 58.85 | 100 | 46.96 | 50 |
| Pre-election violence | 49.53 | 100 | 42.25 | 50 |
| PARCOMP-Transitional | 49.14 | 0.00 | 100 | 100 |
| Prior election protest | 74.16 | 0.00 | 31.25 | 0.00 |
| Executive election | 100 | 0.00 | 100 | 100 |
| Unemployment (lagged) | 51.72 | -100 | -39.7 | 0.00 |
| Extra-legal mobilization | 41.91 | 0.00 | 38.43 | 50 |
|  | Treatment | Control |  |  |
| All | 66 | 118 |  |  |
| Matched | 66 | 40 |  |  |
| Unmatched | 0 | 78 |  |  |

Table 5: Percent balance improvement, Model 7 (main text)

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Mean Diff. | eQQ Med | eQQ Mean | eQQ Max |
| Log GDP per capita (lagged) | 44.95 | 20.14 | 22.17 | 63.36 |
| Pre-election cheating | 62.26 | 100 | 55.10 | 50 |
| Pre-election violence | 52.26 | 100 | 62.98 | 50 |
| PARCOMP-Transitional | 100 | 0.00 | 34.37 | 0.00 |
| Prior election protest | 74.31 | 0.00 | 45.31 | 0.00 |
| Executive election | 41.43 | -8.11 | 20.84 | 14.74 |
| Unemployment (lagged) | 82.50 | 100 | 70.83 | 50 |
| Extra-legal mobilization | 74.65 | 100 | 66.42 | 50 |
|  | Treatment | Control |  |  |
| All | 42 | 142 |  |  |
| Matched | 42 | 32 |  |  |
| Unmatched | 0 | 110 |  |  |

4 Raw Data

The models in the main paper rely on data that has been pre-processed using multiple imputation or statistical matching. As a robustness check, we include analyses of the raw data here. Table 7 presents the results of this analysis. In it, Model 1 includes administrative fraud and control variables, but no interaction terms. Models 2 and 3 include interaction terms for administrative fraud and extra-legal mobilization or intimidation, respectively.

Table 6: Percent balance improvement, Model 8 (main text)

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Mean Diff. | eQQ Med | eQQ Mean | eQQ Max |
| Log GDP per capita (lagged) | 92.55 | 61.02 | 52.92 | 74.65 |
| Pre-election cheating | 50.71 | 100 | 48.31 | 50 |
| Pre-election violence | 65.67 | 100 | 58.45 | 50 |
| PARCOMP-Transitional | 39.80 | 0.00 | 42.33 | 0.00 |
| Prior election protest | 64.50 | 0.00 | 40.63 | 0.00 |
| Executive election | 26.88 | 0.00 | 19.90 | 0.00 |
| Unemployment (lagged) | 55.10 | 100 | 47.79 | 50 |
| Extra-legal mobilization | 52.20 | 0.00 | 51.94 | 50 |
|  | Treatment | Control |  |  |
| All | 62 | 122 |  |  |
| Matched | 62 | 43 |  |  |
| Unmatched | 0 | 79 |  |  |

Taken together, the three models confirm the results presented in the main text. The raw data models show that administrative fraud has a clear positive effect on the probability of post-election protest, while extra-legal mobilization and intimidation do not. Since Models 2 and 3 include interaction terms, we also present predicted probability plots to show the effect of administrative fraud as levels of intimidation and mobilization shift. In both cases, control variables were held at their means (for continuous variables) or medians (for categorical variables). For visual clarity, probabilities and confidence intervals are shown only for the minimum and maximum values of extra-legal mobilization and intimidation. Figure 2 shows the predicted probabilities drawn from Model 2. As the figure illustrates, the probability of protest increases with the severity of administrative fraud when extra-legal mobilization is non-existent, while holding steady when mobilization is severe. The same pattern can be seen in Figure 3 with regard to intimidation. These results help confirm the finding that administrative fraud increases the risk of protest, while mobilization and intimidation efforts do not, and may even reduce the risk in interaction.

|  |  |  |  |
| --- | --- | --- | --- |
| Table 7: Analysis of raw data using allegations of manipulation | | | |
|  | *Dependent variable:* | | |
|  |  | | |
|  | Post-election protests | | |
|  | (1) | (2) | (3) |
|  | | | |
|  |  |  |  |
| Administrative fraud | 0.737\*\* | 1.006\*\* | 1.063\*\* |
|  | (0.308) | (0.426) | (0.481) |
| Extra-legal mobilization | -0.139 | 0.369 |  |
|  | (0.329) | (0.613) |  |
| Intimidation | -0.063 |  | 0.403 |
|  | (0.351) |  | (0.624) |
| Opposition vote-gain | -0.464 | -0.516 | -0.641 |
|  | (0.638) | (0.638) | (0.627) |
|  |  |  |  |
| Transitional election | 0.324 | 0.293 | 0.384 |
|  | (0.888) | (0.834) | (0.881) |
|  |  |  |  |
| Pre-election cheating | -0.311 | -0.420 | -0.402 |
|  | (0.406) | (0.401) | (0.410) |
|  |  |  |  |
| Prior election protests | 0.543 | 0.605 | 0.471 |
|  | (0.651) | (0.636) | (0.632) |
|  |  |  |  |
|  |  |  |  |
| PARCOMP—Suppressed | 1.172 | 1.162 | 1.063 |
|  | (1.123) | (1.112) | (1.097) |
|  |  |  |  |
| PARCOMP—Factional | 0.820 | 0.730 | 0.592 |
|  | (0.896) | (0.908) | (0.914) |
|  |  |  |  |
| PARCOMP—Transitional | -0.723 | -0.859 | -0.886 |
|  | (0.909) | (0.924) | (0.923) |
|  |  |  |  |
| Executive election | 0.710 | 0.755 | 0.793 |
|  | (0.528) | (0.531) | (0.535) |
|  |  |  |  |
| Pre-election violence | 0.621\*\* | 0.571\*\* | 0.561\*\* |
|  | (0.259) | (0.264) | (0.260) |
|  |  |  |  |
| Unemployment (lagged) | -0.003 | -0.003 | 0.003 |
|  | (0.042) | (0.042) | (0.042) |
|  |  |  |  |
| Log GDP per capita (lagged) | 0.130 | 0.176 | 0.172 |
|  | (0.321) | (0.324) | (0.325) |
|  |  |  |  |
|  |  |  |  |
| Log inflation (lagged) | -0.291 | -0.301 | -0.259 |
|  | (0.250) | (0.247) | (0.261) |
|  |  |  |  |
| Log inflation squared (lagged) | -0.049 | -0.053 | -0.055 |
|  | (0.066) | (0.068) | (0.069) |
|  |  |  |  |
| Admin. fraud : Extra-legal mob. |  | -0.259 |  |
|  |  | (0.264) |  |
|  |  |  |  |
| Admin. fraud : Intimidation |  |  | -0.259 |
|  |  |  | (0.270) |
|  |  |  |  |
| Constant | -3.251 | -3.734 | -3.819 |
|  | (2.720) | (2.769) | (2.805) |
|  |  |  |  |
|  | | | |
| Observations | 157 | 157 | 157 |
| Log Likelihood | -55.179 | -54.714 | -54.806 |
| Akaike Inf. Crit. | 144.358 | 143.429 | 143.612 |
|  | | | |
| *Note:* | \*p<0.1; \*\*p<0.05; \*\*\*p<0.01 | | |

5 Alternate Measure of Pre-Election Instability

Our primary measure of pre-election unrest, which is used as a control variable, is taken from the Dataset on International Election Monitoring (Kelley 2012). It is a categorical variable, and codes the degree of pre-election violence identified by election monitors; the four levels of the variable are major, moderate, minor, or no problems. It is coded separately from government- sponsored intimidation, which is one of our explanatory variables. However, it is possible that some use of government election-tampering networks might be picked up by this variable, potentially affecting the results of our main models.

As a robustness check, we replace the variable pre-election violence with a different variable derived from the UCDP Georeferenced Event Dataset, version 18.1 (Sundberg and Melander 2013). The UCDP dataset identifies the time and place of incidents in which armed force was used “by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death” (Croicu and Sundberg 2017). To construct a measure of pre-election instability, we identified all violent events in the UCDP dataset that took place up to six months before an election in our dataset. The number of such events was then totaled for each election-year. The variable, *pre-election conflict events*, ranges from zero to 361; because most countries in our dataset do not experience violent conflict prior to the election, the median value for the variable is zero. Table 8 reports the results of our base models using the imputed dataset, where *pre-election conflict events* replaces the control variable for pre-election conflict used in other models. As the table shows, this robustness check does not affect our findings. In both models, administrative fraud remains positively associated with post-election protest at the .05 significance level or better, while costlier forms of electoral manipulation (extra-legal mobilization and intimidation) are not. As in the other models, the control variable for pre-election violence is not statistically significant.

Figure 1: Predicted probability of protest (Model 5)

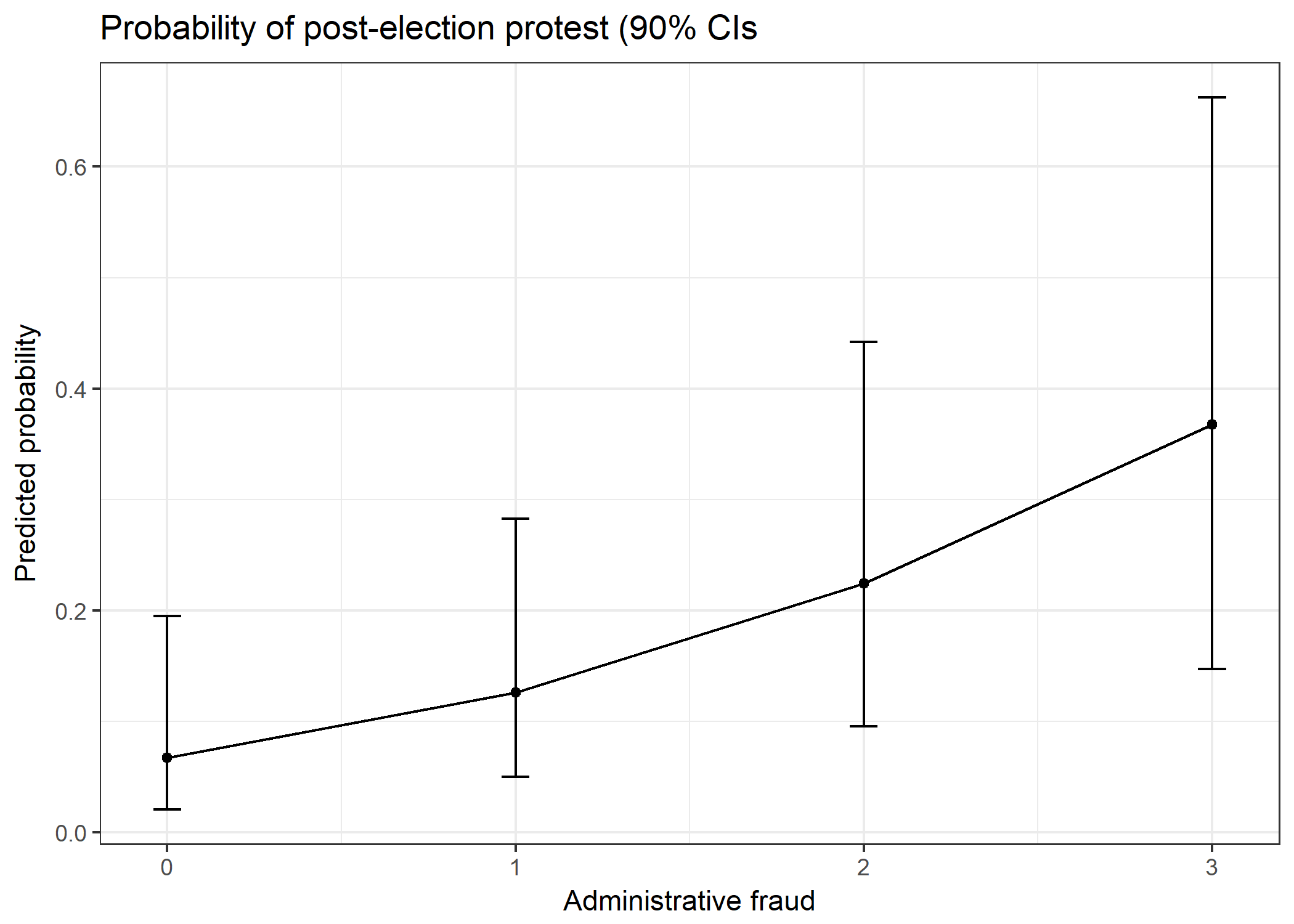


Table 8: Alternative control measure of pre-election instability

|  |
| --- |
|  |
|  | Dependent variable | |
|  | *Post-election protest* | |
|  | (4) | (5) |
| Intercept | -2.998 | -2.813 |
|  | (2.015) | (2.003) |
| Administrative fraud | 0.525\* | 0.569\*\* |
|  | (0.204) | (0.211) |
| Extra-legal mobilization | 0.254 |  |
|  | (0.232) |  |
| Intimidation |  | 0.071 |
|  |  | (0.245) |
| Pre-election conflict events | -0.006 | -0.005 |
|  | (0.006) | (0.006) |
| Opposition vote gain | -0.585 | -0.534 |
|  | (0.499) | (0.496) |
| Transitional election | -0.419 | -0.431 |
|  | (0.6) | (0.608) |
| Pre-election cheating | -0.073 | -0.042 |
|  | (0.233) | (0.240) |
| PARCOMP - Suppressed | 0.436 | 0.460 |
|  | (0.744) | (0.738) |
| PARCOMP - Factional | 0.412 | 0.400 |
|  | (0.634) | (0.630) |
| PARCOMP - Transitional | -0.919 | -0.913 |
|  | (0.669) | (0.670) |
| PARCOMP - Competitive | -15.83 | -15.845 |
|  | (847) | (847) |
| Prior election protest | 0.482 | 0.550 |
|  | (0.476) | (0.477) |
| Executive election | 0.242 | 0.245 |
|  | (0.396) | (0.396) |
| Unemployment (lagged) | 0.026 | 0.024 |
|  | (0.029) | (0.029) |
| Log GDP per capita (lagged) | 0.186 | 0.167 |
|  | (0.254) | (0.252) |
| Log inflation (lagged) | -0.185 | -0.187 |
|  | (0.196) | (0.198) |
| Log inflation squared (lagged) | -0.039 | -0.039 |
|  | (0.04) | (0.041) |
| \*p < .05; \*\* p < .01 | | |

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1. An alternative approach, coding an election as treated if any election manipulation at all was observed, yields substantively similar results. However, this method produces far more treatment observations than controls. [↑](#endnote-ref-1)