

Political Uncertainty and Firm Investment: Project-level Evidence from M&A Activity*

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December 29, 2017

Abstract

We estimate the real effect of political uncertainty on firm investment decisions by examining how U.S. gubernatorial elections affect state-level merger and acquisition activity. The number of acquisitions made by firms headquartered in states with elections decreases by 9% in the second half of an election year. The number of deals targeting firms in election states decreases by 9.6%. Political uncertainty's importance depends on acquirer and deal characteristics, with a larger impact on small deals, financially-constrained firms, and firms with low cash flows and cash holdings. Finally, serial acquirers shift acquisition timing to avoid political uncertainty.

Keywords: investment, political uncertainty, gubernatorial elections, mergers and acquisitions

JEL Classification: G34, G38, H70

*We thank seminar participants at Tulane University for helpful comments and suggestions.

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1. Introduction

We find political uncertainty affects both the level of acquisition activity and the selection of targets and show these effects are stronger for a series of deal and acquirer characteristics. Investigating how political uncertainty affects mergers and acquisitions (M&As) is important for two reasons. First, mergers constitute a large fraction of corporate investment—in 2015, the total deal value of U.S. M&As was just over \$2 trillion,¹ larger than total capital expenditures of \$1.64 trillion²—so establishing the effects of political uncertainty on merger activity is relevant to the current debate on the costs of political uncertainty to U.S. firms.³ We find a 9% decrease in announcements of deals with acquirers headquartered in election states in the quarters around U.S. gubernatorial elections. This constitutes a larger decrease in investment than the 5% decline in capital expenditures [Julio and Yook \(2012\)](#) and [Jens \(2017\)](#) show around elections. Second, merger data are effectively project-level data, so examining merger activity around elections allows us to answer questions about how political uncertainty affects project selection. These project-level questions are unanswerable in studies using aggregate quarterly data as a measure of firm investment ([Julio and Yook, 2012](#); [Gulen and Ion, 2016](#); [Jens, 2017](#); [Atanassov et al., 2016](#)) because individual projects cannot be identified from such data and studies using uncertainty proxies that affect targets and acquirers simultaneously ([Bhagwat, Dam, and Harford, 2016](#); [Bonaime, Gulen, and Ion, 2017](#); [Nguyen and Phan, 2017](#)). Our research design allows us to isolate the effects of political uncertainty in the states of headquarters of targets, and we find acquirers are approximately 9.6% less likely to choose a target headquartered in a state with an upcoming gubernatorial election than a target from a non-election state. This result also furthers our understanding of the costs of political uncertainty by demonstrating for the first time in this literature that

¹S&P Global.

²Estimated investment of non-farm businesses in new and used structures and equipment for 2015.

³While many papers in this literature find a decrease in firm investment in times of greater uncertainty, [Atanassov, Julio, and Leng \(2016\)](#) show the opposite effect in research and development (R&D) expenditures. Additionally, several papers, including [Jens \(2017\)](#), show a rebound in investment after uncertainty is resolved, raising the question of whether there is any loss of investment due to uncertainty or if uncertainty merely shifts investment through time.

uncertainty outside a firm's state of headquarters can affect a firm's investment. Taken together, our results contribute to the understanding of how political uncertainty affects firms' real activities.

We find political uncertainty related to gubernatorial elections decreases both the number of acquirers and targets in a state. We use a difference-in-difference (DD) framework to compare the number of acquirers and targets headquartered in states about to elect a governor with the number of acquirers and targets headquartered in non-election states. We find 9% fewer acquisition announcements by firms headquartered in election states, relative to non-election states. Additionally, there are 9.6% fewer targets in election states, relative to non-election states. Using gubernatorial elections as a measure of political uncertainty allows us to separately identify the effects of political uncertainty on targets and acquirers. We find high political uncertainty within a state causes firms to be less likely to engage in acquisitions and a decrease in the number of firms selected as targets.

We incorporate the type of political uncertainty caused by gubernatorial elections into a model of an acquisition decision to make predictions about which acquirer and target attributes result in deals being particularly susceptible to political uncertainty and find empirical support for each of these predictions. We show the decrease in merger activity in election years is strongest in smaller deals. Acquirers are 11.38% less likely to announce deals less than \$250 million in election years. There is approximately a 15% decline in the number of targets worth less than \$250 million in election years. In contrast, acquirer or target activity for deals above \$250 million are unaffected by political uncertainty related to elections. These results are consistent with our predictions that smaller projects, which are closer to the margin in profitability, are more likely to be forgone in uncertain times.

A second set of results finds financially constrained firms are less likely to announce acquisitions in election years. Firms without bond ratings are less likely to announce acquisitions in election years than rated firms, but the difference is not significant. Additionally, we find acquirers without bond ratings are seven-and-a-half times more likely to select a

target outside of an election state than rated firms. Our results suggest that particularly in terms of project choice, financially constrained firms are more affected by higher political uncertainty.

Along a similar vein, we examine whether firms with lower average cash holdings and cash flows are more affected by political uncertainty. Firms with low average cash flow or holdings are less likely to engage in an acquisition when political uncertainty is high than firms with high average cash flow or holdings. Generally, these results are consistent with results for subsamples formed on bond ratings; firms that are less able to weather bad project outcomes are more affected by higher political uncertainty.

Our model well explains how subsample of acquirers and targets are more susceptible to political uncertainty, but its predictability is limited as a static model does not address dynamic investment decisions. Results in the literature show significant differences between serial and non-serial acquirers (Ismail, 2008) and dynamic aspects to decision making by serial acquirers (Aktas, De Bodt, and Roll, 2011). It may be the case that serial acquirers are more adept at handling political uncertainty in acquisition decisions than are non-serial acquirers given their experience. Therefore, we examine how political uncertainty affects serial acquirers, who acquirer more than five targets in a three year period (Fuller, Netter, and Stegemoller, 2002), separately from non-serial acquirers.

We find that serial acquirers experience a much larger decline in acquisition announcements around elections, relative to non-serial acquirers, but offset this decline with an increase in announcements at the beginning of the election year, so non-serial acquirers have a larger election year decrease in acquisition announcements than serial acquirers. One to three quarters before elections, there is an increase of 19.68% in announcements by serial acquirers headquartered in election states. This increase is followed by a 28.38% decline in announcements around elections. The cumulative year change in announcements by serial acquirers is 9.37%. In contrast, there is no increase in announcements by non-serial acquirers and a cumulative decline of approximately 14.7%. Serial acquirers have the greater decline

in announcements around elections, relative to non-serial acquirers but this decline is, in part, made up within the election year by an increase in announcements before the spike in uncertainty related to elections.

Serial acquirers are less likely to acquire targets in election states than are non-serial acquirers. Non-serial acquirers announce approximately 8% fewer deals for targets headquartered in election states relative to non-election states. Serial acquirers acquire just over 30% fewer targets from election states. Our results suggest that serial acquirers are more sensitive to political uncertainty in target states than are non-serial acquirers.

Our results are robust to the inclusion of alternate uncertainty proxies. We re-estimate our main results including the VIX and economic policy uncertainty (EPU) indexes, which [Bhagwat et al. \(2016\)](#) and [Nguyen and Phan \(2017\)](#) and [Bonaime et al. \(2017\)](#), respectively, use to ensure our results are not subsumed by other uncertainty proxies. Our results are basically unchanged by the inclusion of these proxies, which is not surprising because a strength of the DD framework is identification through comparing merger activity in election and non-election states. The VIX and EPU index rely on identification of time-series variation in merger activity and uncertainty.

Our paper contributes to two areas of research. First, we add to the growing literature on the real effects of political uncertainty. A number of papers show that political uncertainty affects firms' real activities.⁴ Our focus is distinct from other papers examining how political uncertainty affects M&As. [Nguyen and Phan \(2017\)](#) show how political uncertainty affects characteristics related to the successful completion of mergers, including time to completion, payment type, bid premiums, acquirer and target cumulative abnormal returns (CARs), and long-term stock and operating performance. We do not examine merger outcome variables; we examine how political uncertainty affects firms' acquisition decisions. [Bonaime et al. \(2017\)](#) document how political uncertainty affects the start of merger waves and how

⁴See, for example, [Julio and Yook \(2012\)](#), [Gulen and Ion \(2016\)](#), [Jens \(2017\)](#), [Bonaime et al. \(2017\)](#), [Nguyen and Phan \(2017\)](#), [Atanassov et al. \(2016\)](#), [Cao, Li, and Liu \(2016\)](#), [Çolak, Durnev, and Qian \(2017\)](#), [Ben-Nasr, Bouslimi, and Zhong \(2017\)](#), [Buchanan, Cao, Liljebloom, and Wehrich \(2017\)](#), [Huang, Wu, Yu, and Zhang \(2015\)](#), and [Jens and Page \(2017\)](#).

characteristics of acquirers and targets related to real options—for example, less reversible deals—are associated with how political uncertainty affects merger activity. We motivate our empirical predictions using a model that does not incorporate real options and show how firm and acquirer characteristics [Bonaime et al. \(2017\)](#) do not examine change how susceptible a deal is to increased political uncertainty.

Additionally, our empirical strategy allows us to differentiate between the effects of political uncertainty on targets and acquirers, providing some of the first project-level evidence on how political uncertainty affects firm investment. Both [Nguyen and Phan \(2017\)](#) and [Bonaime et al. \(2017\)](#) demonstrate a decline in the propensity of firms to announce acquisitions in election years, which is consistent with our results showing a decrease in election-year acquirer announcements, relative to non-election-year announcements. However, our gubernatorial elections framework allows us to demonstrate the decline in acquisition activity is related to uncertainty affecting acquirers, an increase in political uncertainty from an upcoming election in the state of headquarters of the acquirer. We are also able to show that political uncertainty in a state affects the number of targets selected from that state. [Nguyen and Phan \(2017\)](#) and [Bonaime et al. \(2017\)](#) use the economic policy uncertainty (EPU) index to proxy for uncertainty, so they are unable to differentiate between changes in acquisition activity due to uncertainty affecting the acquirer or target.

The second area of literature to which we contribute is the relatively small literature on how any uncertainty affects M&As. Relative to other types of firm investment,⁵ the effects of uncertainty on mergers is less studied. [Bhagwat et al. \(2016\)](#) results provides some of the first evidence of how uncertainty, proxied by VIX, is correlated with acquisition activity. Our results provide an interesting contrast to those in [Bhagwat et al. \(2016\)](#). For example, whereas the [Bhagwat et al. \(2016\)](#) are concentrated in deals for large (greater than \$250 million) deals for public targets, our results are primarily in smaller deals, as is predicted by our model, and in private targets, which tend to be smaller, on average, than public targets.

⁵See [Bloom \(2009\)](#) for an overview.

One paper cannot fully show how political uncertainty related to elections affects merger activity and reconcile how different types of uncertainty affect merger activity. However, discussing differences between the results here and those in [Bhagwat et al. \(2016\)](#) highlight that there is still more work to be done in terms of understanding how M&As are affected by political and broader types of uncertainty.

The paper proceeds as follows. In Section 2, we develop our model and hypotheses. In Sections 3 and 4, we discuss our data and variables and how our empirical design results in identification, respectively. In Section 5, we present our results, and in Section 6, we conclude.

2. Model and hypothesis development

To develop intuition for how political uncertainty can affect M&A activity, we describe a simple model of a firm's acquisition decision. We then introduce the kind of political uncertainty created by elections into the model and propose hypotheses about how political uncertainty affects M&A activity based on comparative statics.

2.1. Baseline model

The model has two stages. In the first stage, the firm evaluates an acquisition and decides whether or not to pursue the project. In the second stage, the firm earns its final cash flow.

The firm has a baseline cash flow, X . If, in the first stage, the firm decides not to pursue the acquisition, the firm realizes cash flow X in the second stage. As X represents firm value and future cash flows without undertaking the acquisition, real-world analogues to X are, for example, firm size and profitability. We assume that X is a known, constant value. The intuition remains the same if uncertainty affects the future value of the firm as well as the acquisition decision, but we make this assumption to keep our focus on how uncertainty affects the acquisition decision.

If the firm accepts the project, it pays a cost to do so but can receive cash flows in addition to X . The potential deal has four characteristics: an acquisition cost (α), a success of probability (p), and a payoff that has riskless (π) and risky (λ) components. An acquirer pays cost α to pursue the acquisition, which represents a number of possible frictions acquiring firms face, including: the cost of integrating the new firm's employees, any costs or requirements imposed by regulators, and payments to the target firm's shareholders. The riskless component of the acquisition payout (π) represents the target's fixed assets and any other value that transfers easily to the acquirer. The acquirer does not know in the first stage whether a deal will be profitable, so the risky portion of the payoff (λ) models the acquisition's risk. For simplicity, we model an acquisition as being either successful, producing income of $\pi + \lambda$, or unsuccessful, producing income of $\pi - \lambda$. To make the problem interesting, we assume the second stage cash flow for an unsuccessful merger is negative ($X + \pi - \lambda - \alpha < 0$), otherwise the acquisition comes with no risk to the acquiring firm. The acquisition is successful with probability p , and unsuccessful with probability $1 - p$.

If the project is not successful, there is a penalty, χ , for negative cash flow. A firm can recapitalize negative cash flow using external financing, but external financing is costly, so each dollar raised externally costs the firm $\chi \geq 1$ dollars. A higher value of χ indicates a firm faces greater financing constraints, but could also represent any other penalties associated with a failed acquisition, for example, reputation costs.

The acquiring firm's first stage expected value (A) if it accepts the acquisition is:

$$A = (1 - p)(X + \pi - \lambda - \alpha)\chi + p(X + \pi + \lambda - \alpha). \quad (1)$$

If the firm does not acquire the target, it receives the baseline cash flow, X . The firm acquires the target if $A \geq X$.

2.2. Political uncertainty and the acquisition decision

Political uncertainty matters to firm decisions because politicians can alter the environment in which the firm operates. [Pástor and Veronesi \(2013\)](#) discuss political uncertainty as uncertainty about future government policies. Gubernatorial elections capture a change in political uncertainty because uncertainty about election outcomes results in uncertainty about the likelihood of future policy changes within a state.⁶ We introduce the type of political uncertainty measured by elections into our model to show how political uncertainty affects firms' acquisition decisions.

Political uncertainty is a lack of knowledge about the future political environment and how it will affect an acquisition's profitability. Assume there are two possible future political regimes: a favorable regime and an unfavorable regime. The favorable regime creates an economic environment that positively affects the acquisition's profitability, while the unfavorable regime negatively affects the acquisition's profitability. In terms of our model, the favorable regime makes a successful acquisition outcome more likely. Hence, we model political uncertainty as a lack of knowledge about the likelihood of a successful acquisition (p). We assume that the true p is unknown before the election, with $p \in \{H, L\}$ and $0 \leq L < H \leq 1$. If, after the election, $p = H$, then the regime is favorable for the acquisition, otherwise it is unfavorable. Before the election, the firm believes the favorable outcome occurs with probability ρ and the unfavorable outcome with probability $1 - \rho$, with $\rho \in [0.5, 1]$.⁷ Here ρ is our measure of political uncertainty; it maps to beliefs about the likely winner of an election and the ultimate economic environment the winner will create. If $\rho = 1$, the firm know the future outcome with certainty. For example, an election in which a popular incumbent governor is running for re-election likely has a value of ρ close to 1. If $\rho = 0.5$, then the firm

⁶A number of researchers, including [Kelly, Pástor, and Veronesi \(2016\)](#), use elections to capture exogenous variation in political uncertainty. See [Jens \(2017\)](#) for a discussion of the appropriateness of using gubernatorial elections to measure political uncertainty and the importance of the election of a governor to a state's political environment.

⁷We focus on this range for ρ because, as ρ approaches 0, certainty increases for the unfavorable outcome. We assume the firm is unlikely to pursue the acquisition in an unfavorable environment, otherwise the uncertainty is of little consequence for the acquisition decision.

is maximally uncertain about the future economic environment. If there is no incumbent running, and the political polls have a race that is too close to call, then ρ is close to 0.5; the firm believes that the future being favorable or unfavorable happens with equal likelihood. Most gubernatorial elections would have a value of ρ between these two extremes.

We incorporate political uncertainty into Eq. 1 by defining the pre-election probability of a successful acquisition given uncertainty as:

$$p(\rho) = L + (H - L)\rho. \quad (2)$$

For a given level of uncertainty about the future political regime, the firm perceives its value if it pursues the acquisition to be:

$$A(\rho) = (1 - p(\rho))(X + \pi - \lambda - \alpha)\chi + p(\rho)(X + \pi + \lambda - \alpha). \quad (3)$$

The firm acquires the target if:

$$A(\rho) \geq X, \quad (4)$$

which is the same as saying the firm pursues the the acquisition if it believes doing so increases firm value.

2.3. Model predictions and hypotheses

Fig. 1 presents comparative statics for the model, which we use to formulate hypotheses about how political uncertainty affects merger activity. In each panel, we plot a black solid line indicating the decision cutoff, below which $A(\rho) < X$ and the firm does not accept the project. The x-axis for panel A (top left) is uncertainty, and the panel shows how acquisition activity is affected by higher uncertainty. In panels B through F, we plot the value of the firm ($A(\rho)$) while varying a different model parameter along each x-axis. We present comparative statics for high and low levels of political uncertainty to show how acquisition decisions is

affected by increased uncertainty accompanying gubernatorial elections.

Panel A of Fig. 1 presents the comparative static with respect to certainty, ρ . As we expect given the findings in [Bonaime et al. \(2017\)](#) and [Nguyen and Phan \(2017\)](#), political uncertainty decreases the acquisition's value. The more uncertainty that exists, the less likely it is that a given acquisition takes place. The model predicts that, all else equal, merger and acquisition activity is lower when political uncertainty is high.

Panel B of Fig. 1 presents the comparative static with respect to the acquirer's base cash flow, X . As mentioned above, we can think of X as representing either firm size or profitability. We must be careful when considering the effect X has on acquisitions, as it is also the right hand side of the acquisition decision in Eq. 4. We can show that acquisition value grows faster than X as long as $\chi > 1$, meaning that, as long as there is a cost associated with an unsuccessful acquisition, a larger or more profitable firm is more likely to pursue a given acquisition than a less profitable firm. Again, as long as $\chi > 1$, the effect of firm size and profitability is larger when there is more political uncertainty. In other words, smaller and more marginally-profitable firms are more likely to forego an acquisition in the face of increased political uncertainty. The number of acquisitions by small, less-profitable firms should thus decrease more than the number of acquisitions by larger, profitable firms in times of high political uncertainty.

Panel C of Fig. 1 presents the comparative static with respect to the negative cash flow penalty, χ . This penalty, which represents the firm's financing costs, decreases the value of an acquisition because the firm pays extra to recapitalize after the failure. Hence, firms less able to withstand negative outcomes are less likely to pursue a given acquisition. High uncertainty has a larger negative effect on firms with a high cash flow penalty. In essence, the cash flow penalty is a measure of how expensive it is for a firm to raise money if the acquisition goes poorly. Acquisitions in which the acquiring firm has high external financing costs or is more likely to need external financing should be more affected by uncertainty than other acquisitions.

Panels D and E of Fig. 1 presents the comparative static with respect to the acquisition's riskless value, π , and cost, α . Holding everything else constant, an acquisition is more valuable when its guaranteed cash flows are high (π) and less valuable when the project costs more (α). Uncertainty magnifies both of these effects, so, with high uncertainty, acquisitions for target firms with higher proportions of risky cash flows and acquisitions with high costs are less likely to occur. Note that the comparative static for $\pi - \alpha$ looks exactly like Panel D, so the predictions are the same for this difference. The model thus predicts that more marginal acquisitions are less likely to occur, and we expect smaller deals to be more affected by political uncertainty than large deals. Also, acquisition cost measures not only the amount paid for a target firm, but all costs associated with the merger. Firms that are better able to integrate an acquisition into the firm, all else equal, face lower acquisition costs, and should be less affected by political uncertainty.

Panel F of Fig. 1 presents the comparative static with respect to the acquisition's risky payout, λ . This panel assumes that the acquirer's negative cash flow penalty is not too large in relation to the acquisition's success probability, $p(\rho) > \frac{\chi}{\chi+1}$, as the slope is otherwise negative. As $p(\rho)$ depends on the level of uncertainty, it is possible for uncertainty to be so high that a positive slope is not possible. The model predicts acquirer's value risky deals more than non-risky deals when uncertainty is high, but not too high.

Despite its simplicity, the model provides many predictions for how political uncertainty affects firms. We expect firms to make fewer acquisitions in the face of political uncertainty, and that the types of acquisitions and acquirers will differ from those in periods without political uncertainty. We use these predictions to inform our empirical tests.

2.4. Limitations of the model and serial acquirers

Our model is a two-period model and is not dynamic, which limits the acquisitions for which the model is appropriate. Studies recognize that serial acquirers, who engage in multiple acquisitions in a short time frame, differ in types of acquisitions and targets from non-serial

acquirers (Ismail, 2008). We examine serial and non-serial acquirers separately to determine whether political uncertainty affects each group disparately because our model may not result in appropriate predictions for serial acquirers, who are likely making more dynamic decisions.

The literature examines serial acquirers separately from non-serial acquirers because of differences between the two groups. Ismail (2008) shows that serial acquirers receive lower returns from acquisitions than single acquirers. This return difference is explained in part by the differences between the two acquirer types and their targets. Aktas et al. (2011) argue that serial acquirer returns may be the result of CEO learning from acquisitions, with rational CEOs changing their bidding behavior as they learn more from previous acquisitions. Such dynamic behavior does not fit well within our static model.

Our model is relatively simple and does not incorporate dynamic behavior, such as learning, limiting our ability to make predictions about how political uncertainty affects serial acquirers. The dynamic nature of serial acquirers' decision making may be important to how they react to political uncertainty. If, for example, serial acquirers are learning with each acquisition, then firms that regularly make acquisitions may be more aware of, and hence more sensitive to, political uncertainty. We cannot make any predictions about whether serial acquirers will be more or less affected by political uncertainty using our two-stage model, but we empirically explore whether there are differences in susceptibility to political uncertainty between the two samples.

3. Data and variables

3.1. Measuring political uncertainty

Our sample includes 422 gubernatorial elections—all regular gubernatorial elections from 1982 to 2014. There were four special elections during the sample period: Utah (2010), West Virginia (2011), Wisconsin (2012), and California (2013). All are excluded from the

sample because the elections were planned quickly, so there was not enough time pre-election for firms to observe and react to political uncertainty created by the elections. Additionally, we include all gubernatorial elections from Louisiana. Louisiana has a unique gubernatorial election cycle, with an open primary followed by a run-off election if no one candidate obtains a majority vote. However, the timing of the open primary and run-off elections are such that our definitions of pre- and post-election period are appropriate for all elections in the sample, including those in Louisiana. Further discussion of these special cases is included in the appendix, and alternate sample constructions, which have no impact on our results, are considered in the robustness section.

We create variables using gubernatorial and presidential election data from Congressional Quarterly (CQ) Press. *Election* is a binary variable equal to one if a state elects a governor in a year, and *pres. election* is a binary variable equal to one if a presidential election occurs in a year. Presidential elections are a competing source of political uncertainty, so we control for them in our empirical tests.⁸

3.2. Macroeconomic control variables

State unemployment (*unemp*) and change in state GDP (*changeGDP*) are used to control for economic conditions within a state. Monthly state unemployment data are from the Bureau of Labor Statistics (BLS), so we use a quarterly average of state unemployment as a control. We compute change in state GDP from annual state GDP levels, available from the Bureau of Economic Analysis (BEA). Because state GDP data consistent with earlier GDP data are not available after 2014, we limit our sample to M&A activity through 2014.

⁸See [Jens and Page \(2017\)](#) for a discussion of why Senate and other congressional elections are less important to control for in this setting.

3.3. *Merger activity variables*

Merger and activity data are from the Thomson Reuters SDC Platinum database (SDC). We begin with all domestic acquisitions for the period 1982 to 2014 in which a publicly-traded firm buys another publicly-traded or private firm. To be included in the sample, a deal must be classified as: an acquisition, an acquisition of assets, an acquisition of majority interest, or a merger. We exclude the following transaction types: leveraged buyouts, spinoffs, recapitalizations, self-tenders, exchange offers, repurchases, minority stake purchases, acquisitions of remaining interest, and privatizations. We drop any deals missing information on deal number. Although it is common to limit the deals in the sample by size—see, for example, [Fuller et al. \(2002\)](#), who only include deals over \$1 million in their sample—we do not impose any arbitrary requirements on deal size. To ensure our results are not driven by the smallest mergers, we estimate the effects of political uncertainty on merger activity measured as both size and number of deals. For inclusion in our sample, we require acquirer state of headquarters, target state of headquarters, and announcement date, as these variables are necessary for the creation of our state-level M&A activity measures.

Our sample begins in 1982. Because of concerns about data quality prior to 1990, [Bhagwat et al. \(2016\)](#) limit their sample to deals announced after 1989. To preserve sample size, we include deals from 1982 to 1989 in our sample. Our results are robust to starting our sample in 1990, rather than 1982.

We focus only on acquisitions of private and public targets and omit acquisitions of subsidiary targets from the sample. Subsidiary targets present so differently from private and public targets that incentives to postpone acquisition of subsidiary targets during times of greater uncertainty may differ in substantial ways from the incentives concerning public and private target acquisitions. Results for analysis of subsidiary targets are available upon request, but are best left to a separate study with a focus on only subsidiary acquisitions.

To examine the effects of political uncertainty on merger activity, we create quarterly state-level M&A activity variables. We link acquirers and targets to elections based on their

state of headquarters reported in SDC. We follow [Bhagwat et al. \(2016\)](#) and identify the timing of the merger using the announcement date because firms are most susceptible to policy changes during the time period between the announcement and effective dates. To create our dependent variables, we calculate the total number of acquirers, targets, private targets, and public targets for each state-quarter. Our final sample includes 6,400 state-quarter observations.

Our results are robust to studying monthly, rather than quarterly, state merger activity. Many studies on the effects of political uncertainty on firm activities are limited to observing quarterly activity by data availability from firm financial statements. However, merger data can be aggregated by the month, rather than the quarter, of announcement date, providing a finer look at changes in activity around elections. [Bhagwat et al. \(2016\)](#), for example, study monthly merger activity in the U.S. However, for many states in the U.S., monthly merger activity is zero. To use gubernatorial elections and a difference-in-difference framework to study the effects of political uncertainty on merger activity, we need a sufficiently large cross-section of states with measurable merger activity. Therefore, we aggregate our data quarterly, rather than monthly. Our results are unchanged using monthly data, and monthly results are presented in the Robustness section (Fig. 10).

Table 1 shows the total number of acquirers and targets for each state during our sample period. There is sufficient variation in quarterly state merger activity to include all states in our main set of analyses. However, in subsample analyses, the activity of some states can be perfectly predicted with state fixed effects. Therefore, we exclude seventeen states with low activity from our subsamples, which are indicated in the table with asterisks. These states tend to have elections in even years, which are the most common gubernatorial election cycles, so their removal does not eliminate any election cycle from the sample and leaves sufficient variation for identification of the effects of political uncertainty. We estimate subsample results without state fixed effects, including observations from these states, and find effects much larger than those presented, so the results presented are our most conservative

estimates.

Table 2 presents summary statistics for our merger activity variables, as well as for *election* and macroeconomic controls. On average, there are just under five mergers announced per state-quarter in the sample, with a median of two. Generally, the averages for the merger activity variables are larger than the medians, which is expected given the distribution of merger activity amongst states in Table 1.

Table 2 presents summary statistics for serial and non-serial acquirers separately. Serial acquirers are defined as acquirers who make more than five acquisitions in a three-year period (Fuller et al., 2002). Because our model is not dynamic, it is better suited to explaining the activity of non-serial acquirers, who tend to make only one or two acquisitions in our sample period. Therefore, we analyze each subsample separately, and Table 2 shows the summary statistics for the dependent variables in these subsample analyses. There are more acquisitions by non-serial acquirers in the sample. Generally, differences between the two samples are consistent with our expectations that serial and non-serial acquirers make different acquisition decisions and supports our decision to examine both samples separately.

The distribution of our merger activity variables influences our choice of an empirical model. Because state-level merger activity is so low, even after aggregating at the quarterly level, our data can only be properly modeled as discrete data. Additionally, the 25th percentile for all merger activity variables is zero and the median for all merger activity variables for serial acquirers is zero. Thus, we elect to use a two-stage model, a hurdle Poisson model, that will accommodate both the large number of zeros and discrete quality of our data.

4. Empirical specification and identification

4.1. Challenges to identification

We face two main hurdles to studying the effects of political uncertainty on M&A activity. First, we must isolate a source of political uncertainty and identify its effects. Second, we

must address the selection issue inherent in examining M&As. We discuss our answers to each problem in turn.

We use gubernatorial elections as our measure of political uncertainty. [Jens \(2017\)](#) presents the challenges to identification in the political uncertainty literature, which include separating the effects of political uncertainty from the political or business cycle and measuring political, rather than general economic, uncertainty. Although national elections could coincidentally line up with the business cycle, gubernatorial elections are staggered across states in different years. While political uncertainty is frequently correlated with economic uncertainty ([Kelly et al., 2016](#)), the timing of pre-scheduled elections is widely considered to be exogenous.⁹ Provided that firms are not self-selecting into states because of election timing, which is unlikely, our election variables should capture only the effects of increased pre-election political uncertainty.

Although elections are arguably exogenous, because we are examining M&A activity, a natural concern is endogeneity caused by self-selection—only some firms choose to engage in mergers, and this gives rise to the possibility of bias introduced by omitted variables. Consider a hypothetical estimation in which we estimate how political uncertainty affects the propensity of a firm to engage in a merger. Our dependent variable would equal one if a firm engages in a merger in a period and zero otherwise. We would then regress this dependent variable on explanatory variables known to affect a firm’s propensity to engage in a merger and our measure of political uncertainty, an election variable. If we do not perfectly model the decision to engage in a merger—if variables are omitted from the model—we run the risk that our coefficients would be biased. Given the substantial differences between acquiring and non-acquiring firms, the likelihood of omitted variables and thus the risk of biased coefficients is high. If elections are truly exogenous, omitted variables are not problematic for us; if elections are not correlated with any variables, omitted or otherwise, our coefficient of interest is not biased. However, particularly in firm-level estimations,

⁹See, for example, [Julio and Yook \(2012\)](#), [Julio and Yook \(2014\)](#), [Kelly et al. \(2016\)](#), [Gao and Qi \(2013\)](#), [Çolak et al. \(2017\)](#), and [Jens \(2017\)](#).

it is difficult to tell all possible stories and show that elections are uncorrelated with all possible omitted variables. To mitigate these potential concerns, we focus on state-level tests, avoiding estimations with clear selection issues.

Although we argue that the selection issue inherent in studying M&As is not problematic for us because elections are exogenous, we minimize concerns about endogeneity by comparing mergers in election and non-election states. Our treated sample is deals in states with gubernatorial elections in a given year; our control sample is deals in non-election states. We control for pre-existing differences in state-level macroeconomic variables likely to affect aggregate acquisition activity. Any differences in state-level M&A activity should then be driven by changing political uncertainty before elections. In structuring our tests in this way, we effectively side-step any concerns about self-selection and omitted variables.

Figure 2 shows parallel trends in average acquisition activity in election and non-election states leading up to elections, followed by a clear drop in acquisition activity in the quarter before and quarter of elections. In a difference-in-difference framework, it is important to show parallel trends in the treated and control samples leading up to the event, whereupon there is a clear change in the treated, but not the control sample. We define our treated sample as any state-quarter observations in a gubernatorial election year or the year after an election. We define our control sample as any state quarter observations in the year before or two years before a gubernatorial election. Thus, states cycle in and out of the treated and control samples over the course of our estimation.

Figure 2 plots the average number of acquirers (top left), targets (bottom left), private targets (top right), and public targets (bottom right) headquartered in states in the treated (blue boxes) and control (red dots) samples around the timing of elections. For acquirers and target in non-election years, merger activity shows an upward trend over the course of the year. The election trend follows this upward trend for the first and second quarters of the year (two and three quarters before the election), but drops down sharply in the quarter before the election, which is indicated by the vertical dashed line. This pattern is

also shown clearly in the sample of private targets, which comprise a large portion of the total targets sample. The public targets figures suggests a decline in election-year merger activity, but the change is slight. Because the standard deviations of the merger activity variables are so high (Table 2), none of the differences in election and non-election year samples are statistically different from each other. However, the figure strongly suggests increased political uncertainty before elections decrease merger activity.

4.2. Empirical specification

We face three issues in choosing an empirical specification to test our hypotheses: the count nature of the data, an excess of zero counts, and the need to control for state-level unobserved heterogeneity. Traditional linear regression is inappropriate in our setting, as the dependent variable is the count of merger announcements in a state-quarter. A linear regression may predict non-integer and negative outcomes, making its estimates suspect. A better approach is to use a count model, such as Poisson regression. Our summary statistics in Table 2 show that our data has an excessive number of zero outcomes for a Poisson regression. While the zero-inflated Poisson model of Lambert (1992) can handle excess zero counts, it is inappropriate in a panel setting with time-invariant fixed effects. We expect state-level unobserved heterogeneity to be important for explaining merger activity, as states differ in the amount and types of firms that may be acquirers or targets, so we must use an empirical model that allows us to use state fixed effects.

Majo and van Soest (2011) and Gillingham and Tsvetanov (2014) address the problem of estimating a count model with fixed effects and an excess of zero counts, relying on the hurdle model of Mullahy (1986). A hurdle model treats the underlying data as the outcome from a two stage process. The first stage determines whether the outcome realization is zero or positive. A positive realization implies that some “hurdle” has been crossed. A truncated count model produces the actual count if it is greater than zero. The two stages are functionally independent, so as long as the models for each stage can estimate fixed

effects, the overall estimation can as well. The complete distribution of count Y is given by:

$$P(Y = y) = \begin{cases} f_1(0) & \text{if } y = 0, \\ (1 - f_1(0))f_2(y) & \text{if } y > 0. \end{cases} \quad (5)$$

We use a logistic regression for the first stage of our regression, and a zero-truncated Poisson for the second stage. For state i in year and quarter t and q , we estimate the first stage of the hurdle model as:

$$P(Y_{itq} > 0 | \mu_{itq}) = \frac{e^{\mu_{itq}}}{1 + e^{\mu_{itq}}}, \quad (6)$$

and the second stage as:

$$f_2(y | \lambda_{itq}) = \frac{\lambda_{itq}^y}{y!(e^{\lambda_{itq}} - 1)}. \quad (7)$$

The variables included in μ_{itq} and λ_{itq} depend on the quarter for which we estimate the political uncertainty effect. For example, when we estimate the political uncertainty effect for the count of target firms in the third quarter, we estimate:

$$\mu_{itq} = \alpha_i^1 + \alpha_t^1 + \beta_1^1 \text{election} + \beta_2^1 \text{Q3} + \beta_3^1 \text{election} \times \text{Q3} + \delta^1 \text{controls} + u_{itq}^1, \quad (8)$$

and

$$\ln \lambda_{itq} = \alpha_i^2 + \alpha_t^2 + \beta_1^2 \text{election} + \beta_2^2 \text{Q3} + \beta_3^2 \text{election} \times \text{Q3} + \delta^2 \text{controls} + u_{itq}^2, \quad (9)$$

in which α_i^s and α_t^s are fixed effects for the state and year for each stage s , *election* is a dummy variable for election year in a target firm headquarters state, *Q3* is a dummy variable for the third quarter, and *controls* includes dummy variables for the other quarters, a dummy variable for presidential election years (also interacted with *Q3*), state unemployment, and change in state GDP. Our control variables proxy for the economic environment in the state,

and for the competing source of political uncertainty that comes from presidential elections.

The parameters of interest from (8) and (9) are β_1^1 , β_3^1 , β_1^2 , and β_3^2 , which capture the total effect of the election in the third quarter. We estimate these equations for each quarter of the election year, as well as the year following, to see how mergers and acquisitions react as political uncertainty approaches and after it is resolved.

5. Results

5.1. *Political uncertainty and state-level merger activity*

The results in Table 3 show a decline in merger activity in the quarter before and quarter of elections, consistent with a decline in political uncertainty dampening merger activity. We examine differences between election and non-election state merger activity and find lower activity in election states, relative to non-election states. We find a decline of approximately 9% in acquirer announcements in election states, larger than the 5% estimated decline in capital expenditures Jens (2017) shows before gubernatorial elections. We also find nearly a 10% decrease in target activity around elections, providing the first evidence that political uncertainty can affect project selection and that political uncertainty from outside the state of headquarters of the firm can affect a firm’s investment.

In the first three columns of Table 3, we present coefficient estimates and average marginal effects of elections on acquirers. We present two sets of coefficient estimates. The coefficient estimates are combinations of the coefficients on *election* and *election* and the quarter interaction (indicated by the row), giving the total marginal effect of an election in a quarter on acquisition activity. The first model, in the “hurdle” column, is a logit model and models the propensity of the number of acquirers in a state-quarter to be greater than zero. A negative coefficient indicates observations are more likely to be zero, signifying a decline in merger activity for a quarter. The second model, in the “count” column, is a truncated Poisson model and models how many acquisition announcements we observe. Thus, a negative coefficient

means an election year is associated with a lower number of acquisition in a quarter.

We find a decrease in acquirer activity that comes primarily from a decrease in the number of announced acquisitions, rather than an increased propensity of zero announced acquisitions. Our estimated results show that there are no statistical differences between election and non-election years in terms of whether any acquirers announce deals. However, there are negative and significant estimates for the truncated Poisson model the quarter before, quarter of, and two quarters after for the count model, so election-year acquirer activity is lower relative to non-election-year acquirer activity in those quarters. Our results show that firms headquartered in election states are less likely to become acquirers around gubernatorial elections, which is consistent with our model’s prediction that firms will become less likely to engage in acquisitions in times of greater uncertainty.

It is difficult to discuss the economic magnitude of effects with coefficients from nonlinear models, so marginal effects are presented in the third column for the acquirer activity level estimation. To calculate marginal effects, we hold all control variables equal to their sample means and calculate the average deal activity predicted by the model when *election* and the interaction between *election* and quarter are equal to one and then repeat the calculation with the election variables equal to zero. The marginal effect is the difference between the model predictions when election is equal to one or zero. The average marginal effect for the quarter directly preceding the election is -0.128 . This marginal effect can be compared with the sample average in Table 2 of 4.84. The model indicates a 2.64% decline in the number of acquirers per state in election years. This estimate is greater for the quarter of the election, just under 4%. These estimates can be combined for a 6.53% decline in number of acquirers before elections.¹⁰ Including the following quarter’s decline estimates 9.01% lower acquirer announcements in election states, relative to non-election states.

By three quarters following the election (estimates presented in the last row, labeled +3), there are no statistical differences between election and non-election year acquirer activity.

¹⁰Cumulative effects are calculated as geometric averages of marginal effects scaled by sample averages.

However, the number of acquirers in election years remains low one and two quarters following elections. Lower merger activity following elections is consistent with the results in [Bonaime et al. \(2017\)](#), who find spikes in policy uncertainty depress merger activity for a time. From the quarter before to two quarters after the election, we find 9% lower merger announcements from acquirers headquartered in election states than announcements from acquirers in non-election states.

Table 3 also shows that targets in election states are less likely to be acquired than targets headquartered in non-election states. In the quarter before and quarter of elections, we calculate marginal effects of -0.182 and -0.248 . Relative to the average target activity of 4.84 (Table 2), this constitutes an 8.69% cumulative decline in the number of targets in election states (9.63% including the following quarter's decline). Our estimated decline in private target activity for the quarter before and quarter of the election is of similar magnitude, 8.43% (9.06% including the following quarter's decline). Most of the targets in the sample are private targets, so it is not surprising the results in the whole target sample and private target subsample are similar.

Our ability to distinguish between the effects of uncertainty on acquirer and target activity sets this study apart from several others in the literature. [Bhagwat et al. \(2016\)](#) use the VIX as a proxy for uncertainty, and [Nguyen and Phan \(2017\)](#) and [Bonaime et al. \(2017\)](#) measure policy uncertainty with the EPU index. Because these indexes are constructed at a country-level, none of these studies are able to differentiate between the effects of uncertainty on targets from the effects of uncertainty on acquirers. Using gubernatorial elections as a source of political uncertainty allows us to show that uncertainty affecting either targets or acquirers results in dampened M&A activity. Political uncertainty causes fewer announcements by acquirers headquartered in election states. Political uncertainty causes fewer firms to be selected as targets around elections in the state of headquarters of these firms. In the results in Table 3, we are able to demonstrate a decrease in M&A activity caused by political uncertainty through a clearer channel than has been seen previously.

Fig. 3 plots the marginal effects shown in Table 3. The figure gives a visual depiction of the decline in acquirer and target activity pre-elections. For our subsample tests in the following section, we do not report coefficients from hurdle model estimates. Rather, we present plotted marginal effects for ease in interpretation and sample comparison. Marginal effects in subsamples plots are directly comparable to Fig. 3 and all calculations follow the procedures we describe in this section.

5.2. *Subsample analyses*

To test predictions from our model about how political uncertainty will affect aggregate merger activity, we re-estimate the results from Table 3 on subsamples based on acquirer and target characteristics. We find results consistent with predictions from our model. The effects of political uncertainty are greater for smaller deals, deals with financially constrained acquirers, and firms with lower profitability and cash holdings.

Fig. 4 shows marginal effects estimated for subsamples of acquisitions split on deal size. We present marginal effects for acquirer (top panels) and target (bottom panels) activity for small and large deals, defined as deals below and above \$250 million (left panels) and \$75 million, respectively.¹¹ We do not show results for public and private targets for these, or any of our subsample, analyses because the private target activity changes similarly to all target activity (see Fig. 3) and public target activity is not greatly affected by political uncertainty, so showing variation in the effect is not as interesting.

The results in Fig. 4 confirm our model predictions that smaller projects are not undertaken when political uncertainty is high. Our model predicts that, as uncertainty rises, firms will forego smaller projects and projects with marginal profitability. Our results show that acquirers in election states are no less likely to announce deals greater than \$250 million than acquirers in non-election states (top left panel); we estimate a small and insignificant cumulative change (0.14%) in number of announcements by acquirers headquartered in elec-

¹¹We follow Bhagwat et al. (2016) and use \$250 and \$75 million as size cutoffs, to increase comparability between the two sets of results.

tion states for deals greater than \$250 million. However, there is a large drop in acquirer announcements for deals less than \$250 million, -11.38% . This decline is greater than the 9.01% decline seen in the whole sample (Table 3). There is no significant change in the announcement of deals for targets greater than \$250 million in size headquartered in election states (bottom left panel). Announcements for deals with targets less than \$250 million in size are over 15% less likely before elections, relative to announcements in non-election years. Thus, political uncertainty causes acquirers to be less likely to acquire small targets and acquirers to avoid small targets headquartered in election states.

The right panels in Fig. 4 show the same intuition for deals above and below \$75 million, demonstrating the effect is robust to different definitions of small and large deals.

In Fig. 5 we test our model prediction that financially constrained firms are less likely to announce acquisitions in times of greater uncertainty. Financially constrained firms are more susceptible to political uncertainty because they are less likely to be able to withstand the effects of a poor acquisition outcome. We follow Riddick and Whited (2009) and use whether an acquirer had a bond rating at the time of the announcement to proxy to financial constraints. Bond rating data are from Compustat.

The results in Fig. 5 show that unrated firms are less likely to announce acquisitions around elections. The top panel shows that both rated and unrated acquirers are less likely to announce deals in the quarter of elections. However, the marginal effect of elections is nearly twice as large on firms without bond ratings.¹² The difference between marginal effects is not statistically significant.

Acquirers without bond ratings are much less likely to announce deals for targets headquartered in election states. The marginal effect of elections on targets for deals with financially constrained acquirers in the quarter of the election is -0.383 , over seven-and-a-half times the marginal effect of -0.051 estimated for unconstrained acquirers. Taken together, the results in Fig. 5 strongly suggest that firms that are financially constrained and less

¹²Tables with coefficient and marginal effect estimations are available upon request.

likely to be able to withstand a negative outcome from an acquisition more susceptible to the effects of political uncertainty—these firms announce fewer acquisitions in election years and are much less likely to acquire a target from a state with an upcoming election.

Similarly, we test whether acquirers with high average cash flows and cash holdings are less likely to announce deals before elections in their state of headquarters and announce deals for targets headquartered in election states. To split the sample, we calculate rolling three-year averages of firm operating cash flow and cash scaled by assets.¹³ Firms with high average cash flow and holdings are above the sample median for these variables.

The results in Fig. 6 show acquisition announcements by firms with high mean cash holdings and flows are more affected by political uncertainty than firms with low cash holdings and flows. Marginal effects plotted in the top panel show a greater decline in announcement activity by acquirers in election states with low cash holdings and cash flows headquartered, relative to acquirers with high cash holdings and cash flows. There is less of a clear difference in the effect of elections on targets. Generally, acquirers are less likely to acquire targets in election years. The trend is stronger for firms with lower cash holdings and cash flows, but not significantly stronger. The intuition behind these results is similar to the intuition for the results in Fig. 5. Firms with more cash and higher average high flows are more able to withstand negative outcomes from acquisitions.

5.3. *Serial and non-serial acquirers*

We find acquisitions by serial and non-serial acquirers are affected differently by political uncertainty. Results in the previous section are consistent with the model of acquisition activity we present in the Model section of this paper. However, the model is not dynamic and was not intended to explain the acquisition activity of firms that make multiple serial acquisitions. Therefore, we re-estimate our model on serial—acquirers that acquire more than five targets in a three year period (Fuller et al., 2002)—and non-serial acquirers and

¹³Acquirer financial data are from Compustat quarterly files.

find both samples decrease acquisition activity before elections, but serial acquirers have a much greater relative decline in activity that is off-set by an increase in acquisitions before uncertainty spikes.

We find higher levels of announcements by serial acquirers headquartered in election states, relative to non-election states in the months leading up to elections, followed by a sharp drop off in announcement activity. Table 4 shows coefficient estimates and marginal effects for acquisition activity around elections for serial (top panel) and non-serial acquirers (bottom panel). The number of serial acquirers in in election states significantly increases one to three quarters before elections, a cumulative increase of 19.68% in announcements by acquirers.¹⁴ From the quarter before the election to two quarters after the election, there is a 28.38% decline in announcement activity by serial acquirers. The cumulative effect over the year is -9.37% . There is a large decline in announcements right around elections that is offset by an increase in announcements in the first half of election years. Taken together, these results suggest that serial acquirers shift acquisition activity through time, away from elections, to avoid exposure to political uncertainty.

In contrast, there is no evidence that non-serial acquirers increase activity leading up to elections. The bottom panel of Table 4 shows estimates for the effects of elections on announcement activity by non-serial acquirers headquartered in election states. There is no significant change in announcement activity by non-serial acquirers until the quarter before the election, in which there is a sharp decline in activity. The marginal effect of -0.338 , relative to the sample average of 3.56, suggests a 9.5% decline in announcements by serial acquirers headquartered in election states. The estimate in the election quarter is not quite statistically insignificant, with a t-statistic of 1.58 for the truncated Poisson model. However, the marginal effect for this period is also large, constituting an almost 5% decline in activity. The cumulative year change in announcements by non-serial acquirers is -14.69% , which is greater than the estimate for serial acquirers (-9.37%). Although non-serial acquirers have a

¹⁴Cumulative effects calculated using the sample averages presented in Table 2.

smaller and generally less statistically significant decline in announcements around elections than do serial acquirers, the cumulative year decline is greater because non-serial acquirers do not increase their acquisition activity in advance of elections like serial acquirers.

Table 4 also shows how targets selected by serial and non-serial acquirers change during election years. Estimates in the top panel show serial acquirers are 19.2% less likely to acquirer any target headquartered in an election state in the quarter of an election. For private targets, that estimate is approximately 19.5%. Generally, we have not seen a statistically significant decline in public targets (see, for example, Table 3). However, there is a significant decline in acquisitions of public targets headquartered in election states by serial acquirers. We estimate a marginal effect of -0.023 and -0.054 for public targets in the quarter before and quarter of elections, respectively. These marginal effects are smaller relative to the estimated effects for private targets, but there are, on average, fewer public targets than private targets. Relative to the average number of public targets, 0.32, these marginal effects suggest a 22.85% decline in acquisitions of public targets by serial acquirers before elections. Taken together, the target results in the top panel of Table 4 suggest serial acquirers sharply decrease their acquisitions of both public and private targets before elections. Generally, the pre-election increase in serial acquisitions seems to be limited to an increase in the acquisition of private targets, which comprise a larger percentage of serial acquirers' targets in the overall sample (Table 3).

Estimates in the lower panel of Table 4 show non-serial acquirers are also less likely to acquire targets in states with upcoming elections. This effect is strongest in private targets, with a 12.44% cumulative decline in acquisitions of private targets in the quarter before and quarter of the election. There is no statistically significant decline in acquisitions of public targets headquartered in election states. The marginal effects suggest a decline, but none of the estimates are significant.

Fig. 7 plots the marginal effects on acquirer and target activity from Table 4 and clearly shows the difference between the behavior of serial and non-serial acquirers around elections.

Serial acquirers show a sharp trend upward in announcements by acquirers headquartered in election states two quarters before elections before declining in the quarter of the election. In contrast, non-serial acquirers do not react to elections until the quarter before, showing only a decline in activity coinciding with the election. Both subsamples of acquirers are less likely to acquirer targets in election states, with estimates of marginal effects of election years on target activity dropping in the quarter before and quarter of elections before returning to zero. These results suggest that targets are affected similarly by uncertainty in their state of headquarters, regardless of the type of acquirer. However, uncertainty in the state of headquarters of the acquirer affects different types of acquirers disparately.

5.4. *Robustness*

5.4.1. *Alternate measures of uncertainty*

There are several other papers examining the effects of different types of uncertainty on merger activity, so we ensure our results are robust to the inclusion of their uncertainty proxies. [Bonaime et al. \(2017\)](#) and [Nguyen and Phan \(2017\)](#) use the EPU index, which measures policy uncertainty, and generally find merger activity is lower when the EPU is high. [Bhagwat et al. \(2016\)](#) use VIX to proxy for uncertainty and also find lower levels of acquisition activity when uncertainty is high. Our results are unchanged when either of these measures is included in our estimation.

Because the EPU and VIX both measure only time series changes in uncertainty. The EPU index from [Baker, Bloom, and Davis \(2016\)](#) uses a count of words and phrases from ten large newspapers to capture policy uncertainty.¹⁵ The VIX is an estimate of implied market volatility, calculated using S&P 500 Index option quotes.¹⁶ [Bonaime et al. \(2017\)](#), [Nguyen and Phan \(2017\)](#), and [Bhagwat et al. \(2016\)](#) use these indexes to explain time-series variation in merger activity.

¹⁵EPU data are available from Nick Bloom's website, policyuncertainty.com.

¹⁶VIX data are available from the Chicago Board Options Exchange (CBOE) website, cboe.com.

We expect our results will not be changed by the inclusion of these indexes in our estimation because our identification strategy relies on exploiting cross-sectional variation in state-level uncertainty and M&A activity. Gubernatorial elections are held in at least two states every year, allowing us to compare merger activity in election and non-election states through time. All firms are exposed to changes in the EPU and VIX simultaneously. Thus, we would only expect our results to be affected by including the EPU or VIX in our estimations if firms in some election cycles are more or less susceptible to the kinds of uncertainty measured by the EPU and VIX.

Fig. 8 plots marginal effects for a re-estimation of our main model (Table 3) including the EPU index as a control variable. In the left panels, we calculate the marginal effects of elections on acquirer (top) and target (bottom) activity when the EPU is held at its average (blue squares) or one standard deviation above its average (red dots), with all other variables held at their sample averages (the same procedure described for Table 3). Our marginal effects are barely changed by the inclusion of the EPU index, so we conclude our results are not driven by the policy uncertainty captured by the EPU.

In the right panels of Fig. 8, we calculate marginal effects of the EPU index. We find the average number of acquirers and targets predicted by the model when the EPU is held at its average and one standard deviation above its average. The difference between the two predictions is the marginal effect of the EPU index. We perform this calculation with election and our election and quarter interaction equal to zero (blue boxes) and one (red dots).

In contrast with our election results, which are unchanged by the inclusion of the EPU index, the marginal effects of the EPU index are affected by the occurrence of elections. Two quarters before elections, the EPU has a smaller effect on the number of acquirers and targets in non-election years than in election years. In the quarter of elections, the inclusion of elections dampens the estimated effect to the EPU index. These results suggest that the policy uncertainty measured by the EPU index may, in part, be explained by

the political uncertainty inherent in election cycles. However, controlling for the policy uncertainty measured by the EPU is not important in a study of the effects of the political uncertainty captured by elections on firm acquisition activity.

Fig. 9 shows a similar result when the VIX is included in our estimations to the EPU results in Fig. 8. The panels to the left show no change in our estimated election effect when the VIX is held at its average (blue squares) or one standard deviation above its average (red dots). However, elections tend to dampen the effect of the VIX on acquirer and target activity (right panels). In particular, elections result in the VIX having no significant affect on merger activity (coefficient estimates are slightly positive). Taken together, these results suggest that political uncertainty associated with elections is a more important driver of variation in merger activity than is variation in the VIX or EPU indexes.

5.4.2. Monthly merger activity estimates

In our main specification, we test whether political uncertainty affects merger activity with merger activity aggregated at the state-quarter level. Our results are robust to aggregating at the state-month level. Fig. 10 presents marginal effects for elections on monthly state merger activity. The results confirm our earlier findings (Table 3) and show a clear drop in activity leading up to the election (indicated by a vertical dashed line). We do not report a table of the estimated coefficients we use to calculate these marginal effects, but we find a statistically significant decline in acquirer and target activity accompanying elections; results are available upon request. Following the election, estimated marginal effects cluster around zero, but are noisy. We prefer quarterly aggregated merger activity levels due to the large number of zeros in state-month merger activity, but the conclusions are the same using either data aggregation.

6. Conclusion

In this paper we examine the role political uncertainty plays in M&A activity. Using a simple model of the acquisition decision when political uncertainty exists, we make predictions about the negative impact political uncertainty has on M&A activity, and how the types of acquirers and deals change in response. Using U.S. state gubernatorial elections as our proxy for political uncertainty, we test the model's predictions by testing whether the number of deals done in a state-year-quarter differs around elections. We find that in the last six months of an election year, the number of acquirers making deals decreases by over 9% from non-election years, and that the number of targets also decreases by approximately 9.6%. We show that the negative impact is greater for financially-constrained acquirers, less-profitable acquirers, private targets, and small deals, all consistent with model predictions. Additionally, we show that serial acquirers shift their M&A activities away from election quarters.

The above results are novel in the literature. Thanks to our setting, we are able to show that project-level political uncertainty, as captured by target firms facing political uncertainty, affects firm investment decisions. The magnitude of this effect depends on characteristics of both the acquirer and the target. Our results are economically important, and contribute to the growing understanding of the importance of political uncertainty to corporate investment behavior.

Appendix: Discussion of special and Louisiana gubernatorial elections

There are four special elections during our sample period: California (2003), Utah (2010), West Virginia (2011), and Wisconsin (2012). These elections include two recall elections: California (2003) and Wisconsin (2012). We consider the circumstances and timing of each election and choose to only include the Utah election in our sample.

We choose to include the Utah special election in our sample because there was ample warning of an impending special election – effectively, the election followed the same general time line as a regularly-scheduled gubernatorial election. Additionally, the election was sufficiently early that the pre- and post-election periods are clearly defined. A special election was held in Utah in 2010 after Governor Jon Huntsman resigned in 2009 to become the U.S. Ambassador to China¹⁷. The special election took place on November 2, 2010. Utah then held its next regularly-scheduled gubernatorial election two years later, in November, 2012. The next gubernatorial election in that state will be in 2016.

We omit the 2011 West Virginia special gubernatorial election from our sample because it resulted in two back-to-back elections, in 2011 and 2012. Since we define pre- and post-election periods on an annual basis, this results in the definition of 2011 as both a pre-election year and an election year; 2012 is both an election year and a post-election year. Governor Joseph Manchin III resigned to become a Senator in 2010. A special election was held in November, 2011, and was followed by the regularly-scheduled gubernatorial election in 2012. In the robustness section, we also re-estimate our results omitting all observations from West Virginia in 2011 and 2012. Our results are robust to omitting both the 2011 and 2012 West Virginia gubernatorial elections.

The special elections in California in 2003 and Wisconsin in 2012 were recall elections, meaning that the constituents forced an election in which they could replace the sitting

¹⁷Information from the Utah historic archives, maintained by the Utah Department of Administrative Services, Division of Archives & Records Service.

governor earlier than the regularly-scheduled gubernatorial election. One important reason we use gubernatorial elections as our source of political uncertainty is because they are pre-scheduled, so firms can observe the timing of the election and plan mergers accordingly. Because of the uncertainty surrounding the timing of the recall elections, we omit them from our sample.

In California, a formal recall petition was started on March 25, 2003¹⁸. The timing of the recall election depended on when enough signatures (897,158 were needed to force a recall election) were collected. If the necessary signature were collected and certified by September 3, 2003, the election would have been held on October 7, 2003. If the signatures were collected and certified after September 4, 2003, the election would have been held on March 2, 2004. Even after enough signatures were collected and verified, and the election scheduled for October 7th, legal wrangling threatened to postpone the election to March. On September 15th, a federal appeals court, a three-judge panel of the United States Court of Appeals for the Ninth Circuit, postponed the election to March over concerns about punch-card voting machines¹⁹. An eleven judge panel then over-turned this ruling on September 23rd, so the recall election could proceed only two weeks later. Opponents chose not to appeal the ruling to the U.S. Supreme Court²⁰. However, had opponents appealed, or threatened to appeal, the timing of the election would have become even more uncertain. Results of the 2003 California recall election were finalized by mid-November, and the election resulted in Governor Gray Davis being replaced by Governor Arnold Schwarzenegger.

The uncertainty of the timing of the 2012 Wisconsin recall election was similar to that seen in California in 2003. Controversy over the policies of Governor Scott Walker, elected in Wisconsin in 2010, erupted shortly after he took office and culminated in an effort to recall him beginning approximately one year after he was elected, in November of 2011. On

¹⁸Information from the California Secretary of State web page, Statewide Special Election – FAQs About Recalls, October 7, 2003.

¹⁹“Court Delays Recall Vote in California; Faulty Ballots Cited,” by Dean E. Murphy, September 15, 2003, *The New York Times*

²⁰“U.S. Appeals Court Puts Recall In California Back on Schedule,” by Dean E. Murphy, September 24, 2003, *The New York Times*

March 15, 2012, a judge in Wisconsin postponed an election from May 29, 2012 to June 5, 2012 by giving the Government Accountability Board (GAB) additional time to review and verify recall petition signatures²¹. Governor Walker won the recall election, which was held on June 5, 2012, and was subsequently re-elected in 2014.

²¹“Judge OKs petition review extension, June 5 recall election,” by Dee J. Hall, March 15, 2012, *Wisconsin State Journal*

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Fig. 1. Model comparative statics plot showing the effect of model parameters on the value of pursuing the acquisition. In each plot, the solid line is the firm's value without the acquisition, while the dashed and dot-dash lines are the firm's value with the acquisition. If acquisition value is in the gray area below the solid line, the firm does not pursue the acquisition. The top-left plot shows the effect of certainty (ρ) alone. The other five plots show each parameter's effect given either high (dot-dash line) or low (dashed line) uncertainty. From left to right, these plots show the comparative static for firm size (X), the cash shortfall penalty (χ), acquisition riskless value (π), acquisition cost (α), and acquisition risky value (λ).

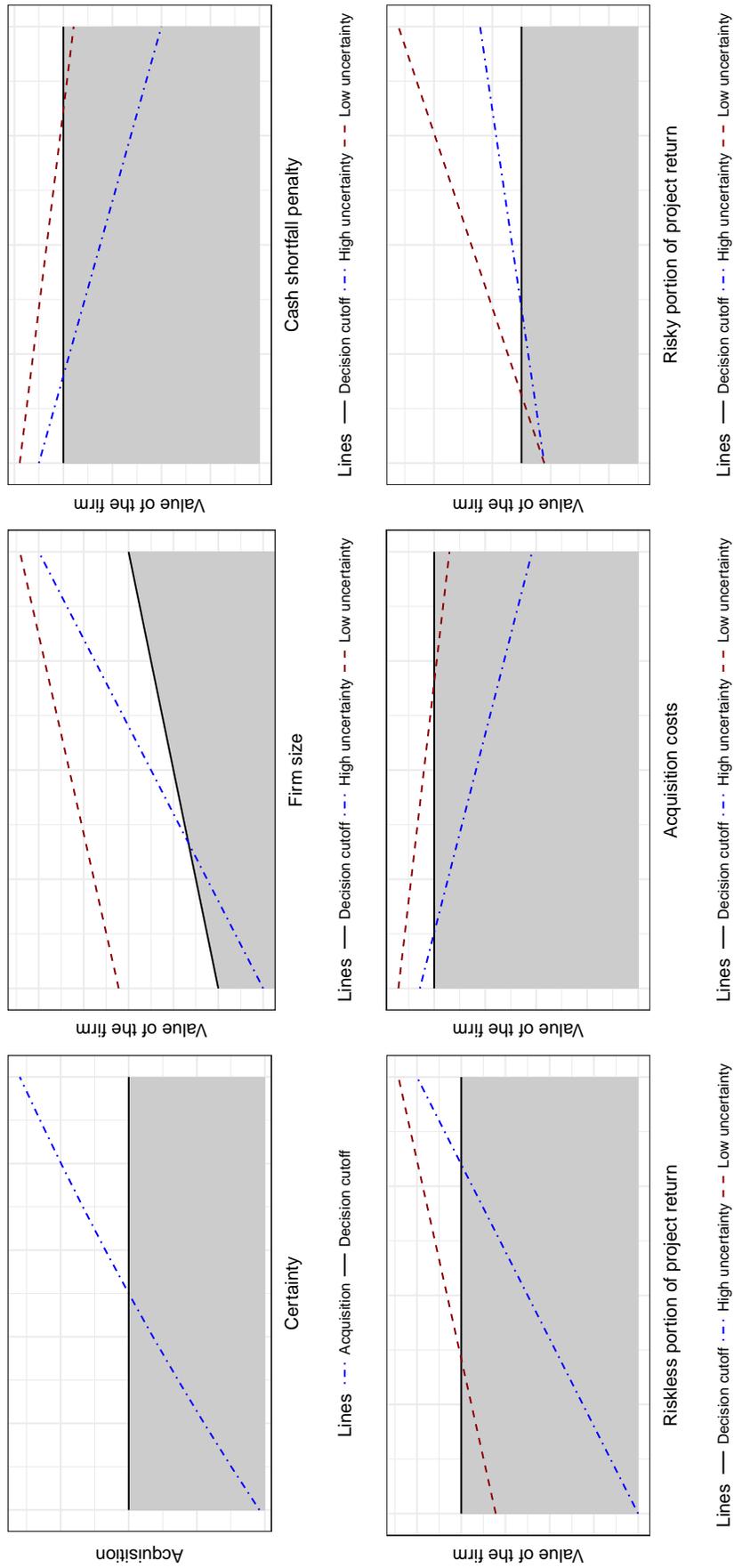


Table 1: The number of acquirers and targets per state for a sample of 30,978 deals from 1982 to 2014. Merger data, including state of headquarters, are from Thomson Reuters SDC Platinum.

State	Total		Total		Private		Public		State		Total		Private		Public	
	acquirers	targets	targets	targets	targets	targets	targets	targets			acquirers	targets	targets	targets	targets	targets
Alabama*	358	259	206	53	Montana*	33	58	42	16							
Alaska*	12	41	28	13	Nebraska*	161	112	82	30							
Arizona	438	518	428	90	Nevada	282	341	276	65							
Arkansas*	123	121	105	16	New Hampshire	133	185	129	56							
California	5,243	5,676	4,290	1,386	New Jersey	1,264	1,107	734	373							
Colorado	888	860	634	226	New Mexico*	83	138	115	23							
Connecticut	746	554	335	219	New York	2,656	2,062	1,448	614							
Delaware*	56	289	265	24	North Carolina	672	639	468	171							
Florida	1,816	2,013	1,607	406	North Dakota*	24	42	39	3							
Georgia	1,077	991	752	239	Ohio	951	836	576	260							
Hawaii*	24	51	39	12	Oklahoma	265	346	261	85							
Idaho*	66	76	59	17	Oregon	220	295	220	75							
Illinois	1,208	1,198	876	322	Pennsylvania	1,340	1,152	746	406							
Indiana	397	487	348	139	Rhode Island*	102	80	54	26							
Iowa*	112	174	125	49	South Carolina	164	230	162	68							
Kansas	147	207	164	43	South Dakota*	13	33	30	3							
Kentucky	202	267	199	68	Tennessee	525	464	348	116							
Louisiana	239	382	317	65	Texas	2,978	2,735	2,103	632							
Maine*	52	89	69	20	Utah	307	286	230	56							
Maryland	602	591	438	153	Vermont*	39	49	29	20							
Massachusetts	1,378	1,312	924	388	Virginia	775	836	611	225							
Michigan	539	590	441	149	Washington	527	602	471	131							
Minnesota	619	549	377	172	West Virginia*	117	122	92	30							
Mississippi	136	126	86	40	Wisconsin	326	338	238	100							
Missouri	525	410	298	112	Wyoming*	18	59	55	4							
					Total	30,978	30,978	22,969	8,009							

*States with low variance in M&A activity that are dropped in subsamples tests including state-level fixed effects.

Table 2: Summary statistics for state-level acquisition activity measures and control variables for state-level tests for a sample of 6,400 quarterly observations from 1982 to 2014. Quarterly M&A activity variables are the sum of the number of deals announced per state-quarter for acquirers, targets, private targets, and public targets. Firms are linked to states by the state of headquarters reported in SDC Platinum. *Election* is a binary variable equal to one if a gubernatorial election was held in that state in that year. *Unemp* is quarterly average state unemployment from the Bureau of Labor Statistics. *ChangeGDP* is annual change in state GDP from the Bureau of Economic Analysis. Further details on variable construction and data sources are given in the Appendix.

Statistic	N	Pctl(25)	Mean	Median	Pctl(75)	St. Dev.
<i>Summary statistics for all deals</i>						
Acquirers	6,400	0	4.84	2	6	9.28
Targets	6,400	0	4.84	2	5	9.21
Private targets	6,400	0	3.59	1	4	7.17
Public targets	6,400	0	1.25	0	2	2.43
<i>Summary statistics for deals by serial acquirers</i>						
Acquirers	6,400	0	1.28	0	1	3.03
Targets	6,400	0	1.28	0	1	2.98
Private targets	6,400	0	0.96	0	1	2.36
Public targets	6,400	0	0.32	0	0	0.85
<i>Summary statistics for deals by non-serial acquirers</i>						
Acquirers	6,400	0	3.56	1	4	6.76
Targets	6,400	0	3.56	2	4	6.65
Private targets	6,400	0	2.63	1	3	5.18
Public targets	6,400	0	0.93	0	1	1.84
<i>Summary statistics for explanatory variables</i>						
Election	6,400	0	0.26	0	1	0.44
ChangeGDP	6,400	0.03	0.05	0.05	0.08	0.04
Unemp	6,400	0.04	0.06	0.06	0.07	0.02

Fig. 2. Parallel trends plot showing average state-level M&A activity in election and non-election states for a sample of 6,400 state-quarter observations from 1982 to 2014. State-level M&A variables are calculated as the total number of acquirers, targets, private targets, and public targets announced in a quarter. State headquarters are from SDC Platinum. Firms are in the treated (election) sample in the year of and year after an election in their state of headquarters. Firms are in the control (non-election) sample the year before and two years before election years in their state of headquarters.

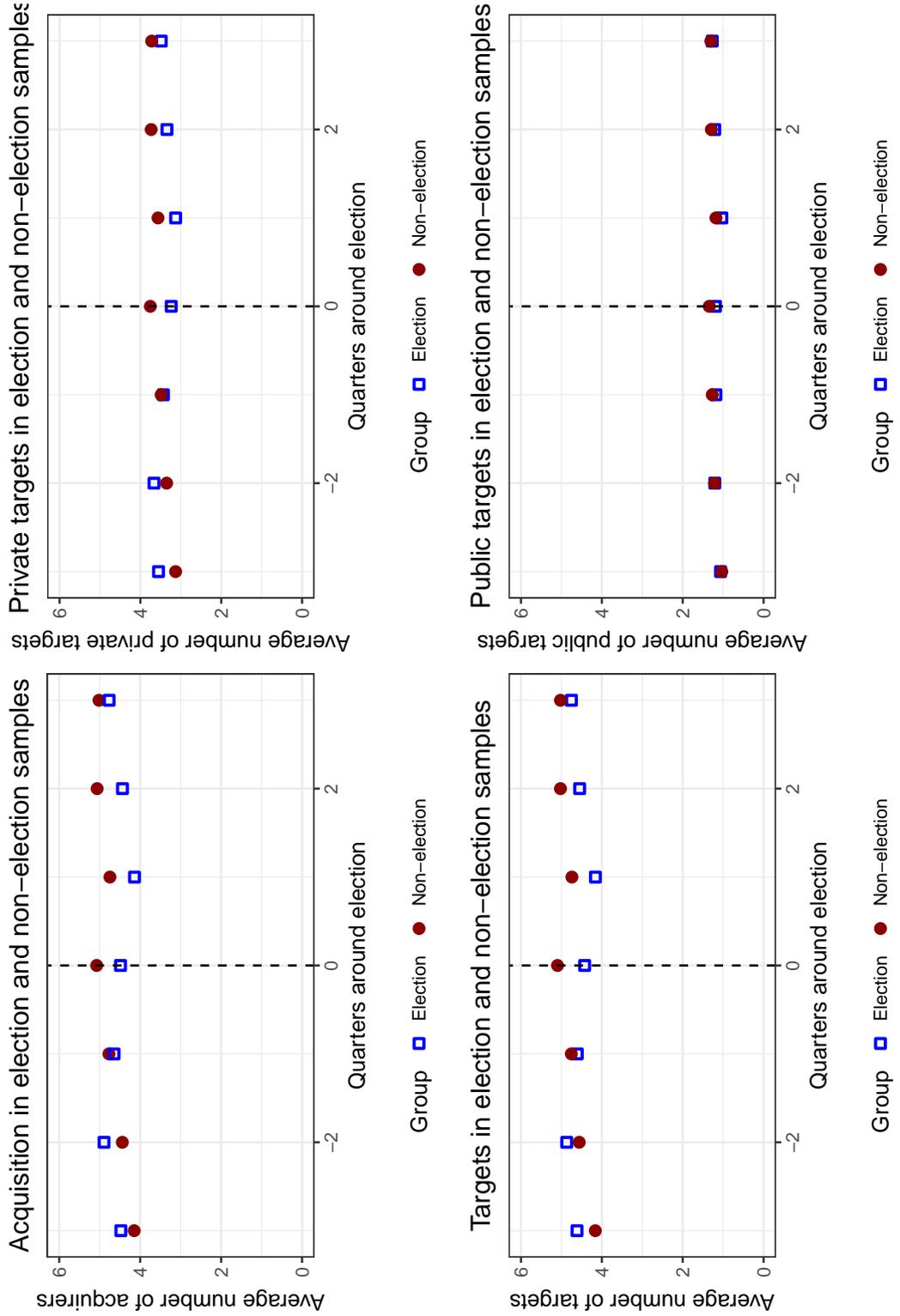


Table 3: Estimates from a hurdle negative binomial model for a sample of 6,400 state-quarter M&A activity observations from 1982 to 2014. The dependent variable is the number of acquirers (columns 1 to 3), targets (columns 4 to 6), private targets (columns 7 to 9), and public targets (columns 10 to 12) in each state-quarter. Coefficients are combined estimates on *election* and an interaction between *election* and the appropriate quarter (-3 is three quarters before the election, or $Q1$). Coefficients are presented for both stages of the negative binomial model. The first stage estimates the propensity of observations to be greater than zero (a negative coefficient means a variable is associated with a greater probability of no M&A activity), and the second stage estimates a count model (a negative coefficient means a lower number of acquirers per state-quarter, for example, is associated with a variable). Marginal effects reported are calculated as the average predicted acquirer, target, private target, or public target activity in election year-quarters, relative to non-election year-quarters, with control variables held at their sample averages, and are directly comparable to the sample averages reported in Table 2. The model is estimated for three quarters before, the quarter of, and three quarters after the election. We include as controls a presidential election year and quarter interaction, quarter fixed effects, state fixed variables, year fixed effects, state unemployment, and change in state GDP in each specification.

	All acquirers			All targets			Private targets			Public targets		
	hurdle	count	marg. eff.	hurdle	count	marg. eff.	hurdle	count	marg. eff.	hurdle	count	marg. eff.
-3	-0.026 (0.177)	0.015 (0.033)	0.017 (0.177)	0.325 (0.177)	0.035 (0.032)	0.078 (0.165)	0.173 (0.165)	0.088** (0.037)	0.126 (0.037)	0.119 (0.178)	-0.034 (0.079)	0.028
-2	-0.252 (0.176)	0.014 (0.031)	-0.002 (0.177)	-0.227 (0.177)	0.003 (0.031)	-0.009 (0.166)	-0.206 (0.166)	0.024 (0.037)	-0.033 (0.037)	-0.140 (0.179)	0.002 (0.075)	-0.043
-1	-0.054 (0.178)	-0.083*** (0.032)	-0.128 (0.177)	-0.248 (0.177)	-0.095*** (0.032)	-0.182 (0.165)	-0.147 (0.165)	-0.101*** (0.037)	-0.137 (0.037)	-0.132 (0.178)	-0.083 (0.075)	-0.076
election	0.122 (0.180)	-0.138*** (0.032)	-0.193 (0.178)	-0.266 (0.178)	-0.138*** (0.032)	-0.248 (0.167)	-0.091 (0.167)	-0.165*** (0.038)	-0.172 (0.038)	-0.179 (0.178)	-0.060 (0.073)	-0.081
+1	-0.038 (0.179)	-0.015 (0.034)	-0.023 (0.177)	-0.231 (0.177)	0.005 (0.033)	-0.011 (0.168)	-0.147 (0.168)	0.026 (0.039)	-0.025 (0.039)	0.072 (0.179)	-0.036 (0.078)	0.012
+2	0.127 (0.179)	-0.079** (0.033)	-0.106 (0.181)	-0.203 (0.181)	-0.015 (0.032)	-0.039 (0.170)	-0.405 (0.170)	0.010 (0.038)	-0.103 (0.038)	-0.247 (0.180)	0.073 (0.074)	-0.048
+3	0.171 (0.182)	0.009 (0.032)	0.027 (0.181)	-0.151 (0.181)	0.031 (0.032)	0.047 (0.170)	0.027 (0.170)	0.041 (0.038)	0.047 (0.038)	-0.020 (0.179)	0.051 (0.073)	0.016

asterisks

Fig. 3. Plot of the marginal effects estimated in Table 3 for acquirers (top left), targets (bottom left), private targets (top right), and public targets (bottom right). Details on estimation and marginal effect calculations are given in the caption of Table 3.

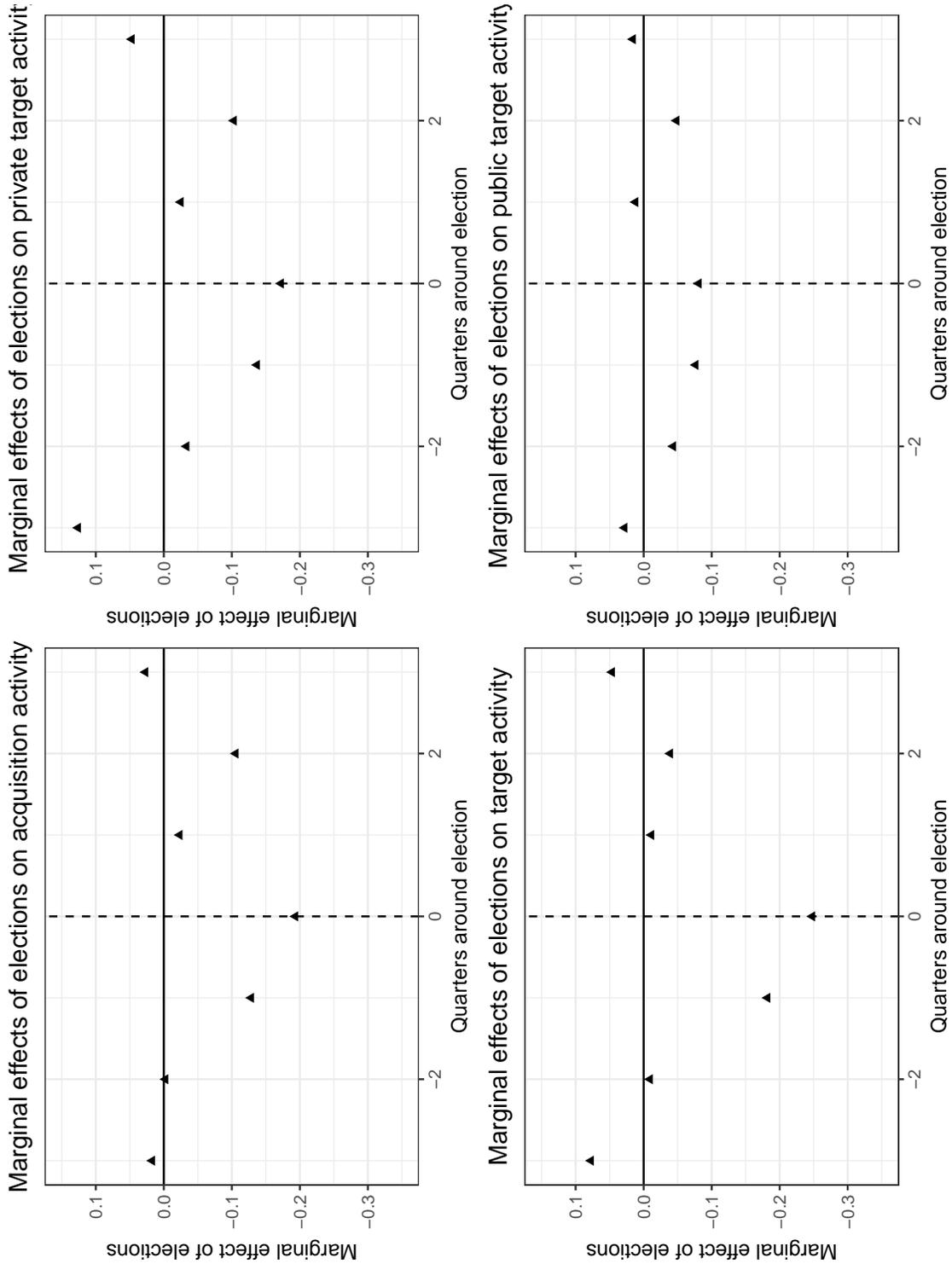


Fig. 4. Plot of marginal effects of elections on acquirer (top) and target (bottom) activity for deals above and below \$250 million dollars (left) and \$75 million dollars (right). Details on the sample, model, and marginal effect calculations are given in the caption of Table 3.

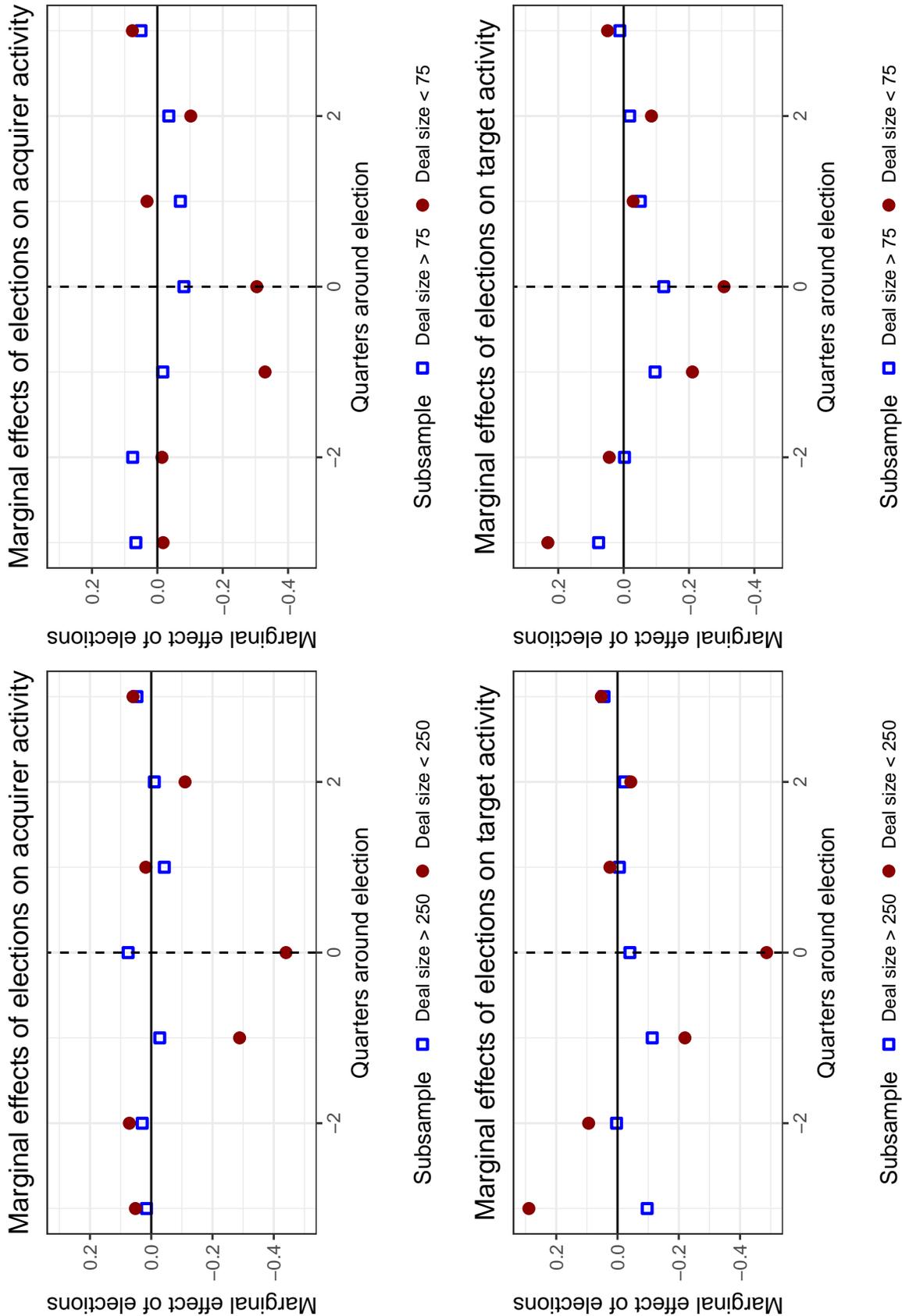


Fig. 5. Plot of marginal effects of elections on acquirer (top) and target (bottom) activity for deals with acquirers with and without bond ratings at the time of deal announcement. Bond rating data are from Compustat. Details on the sample, model, and marginal effect calculations are given in the caption of Table 3.

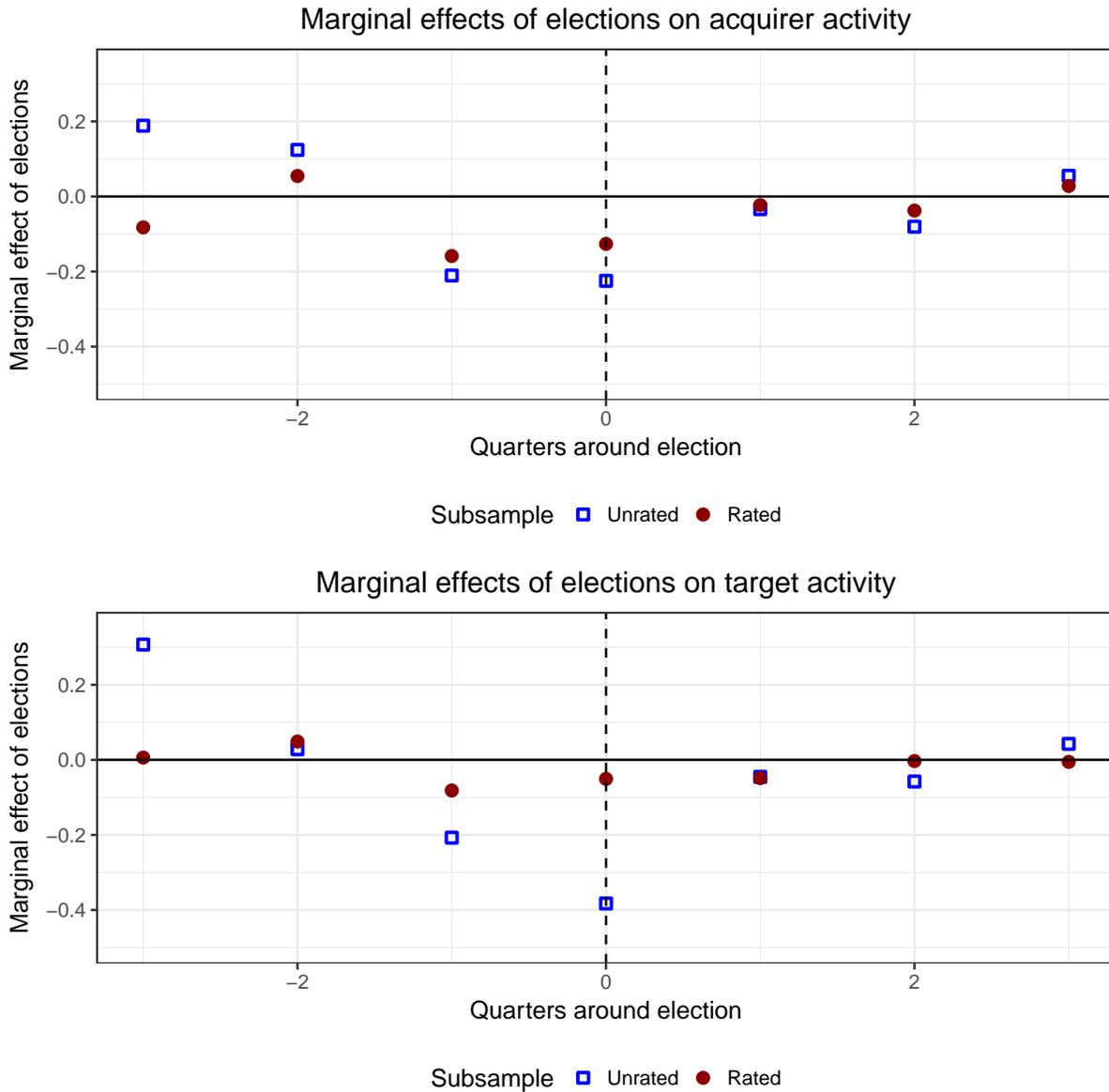


Fig. 6. Plot of marginal effects of elections on acquirer (top) and target (bottom) activity for deals above and below the median acquirer sample cash flow (left) and cash holdings \$75 million dollars (right). Details on the sample, model, and marginal effect calculations are given in the caption of Table 3.

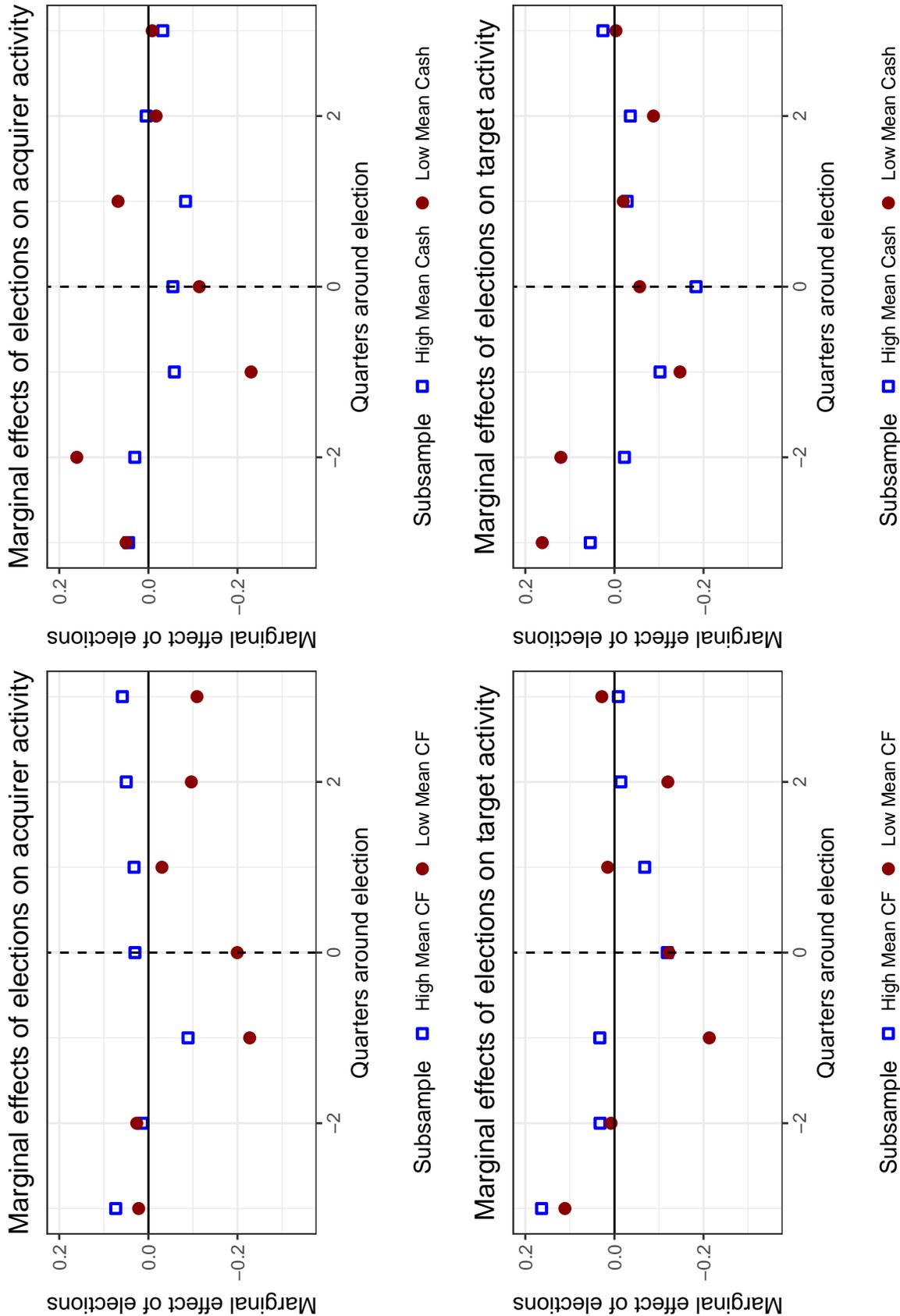


Table 4: Coefficients for a re-estimation of the model detailed in Table 3 on samples of serial and non-serial acquirers. Panel A presents estimates for serial acquirers and Panel B presents estimates for non-serial acquirers. The dependent variable is the number of acquirers (columns 1 to 3), targets (columns 4 to 6), private targets (columns 7 to 9), and public targets (columns 10 to 12) in each state-quarter. Serial acquirers are defined as firms that made more than five acquisitions in the preceding three year period. Details on the sample, model, and marginal effect calculations are given in the caption of Table 3.

	All acquirers			All targets			Private targets			Public targets		
	hurdle	count	marg. eff.	hurdle	count	marg. eff.	hurdle	count	marg. eff.	hurdle	count	marg. eff.
<i>Panel A: Serial Acquirers</i>												
-3	-0.069 (0.202)	0.193*** (0.070)	0.049	0.261 (0.192)	0.082 (0.071)	0.134	0.356* (0.194)	0.082 (0.085)	0.145	0.089 (0.209)	-0.173 (0.201)	0.012
-2	0.126 (0.202)	0.210*** (0.066)	0.184	-0.040 (0.193)	0.109 (0.067)	0.043	-0.098 (0.194)	0.168* (0.082)	0.013	0.135 (0.199)	-0.234 (0.194)	0.022
-1	-0.134 (0.202)	0.128* (0.068)	0.010	-0.076 (0.193)	-0.090 (0.070)	-0.072	0.144 (0.194)	-0.070 (0.085)	0.023	-0.057 (0.203)	-0.406** (0.194)	-0.023
election	-0.285 (0.202)	-0.172** (0.072)	-0.201	-0.369* (0.193)	-0.271*** (0.075)	-0.246	-0.378* (0.196)	-0.257*** (0.090)	-0.187	-0.206 (0.205)	-0.582*** (0.214)	-0.054
+1	0.017 (0.203)	-0.121 (0.077)	-0.039	-0.033 (0.192)	-0.050 (0.078)	-0.032	-0.022 (0.195)	-0.071 (0.095)	-0.023	-0.266 (0.218)	0.432** (0.203)	-0.035
+2	-0.155 (0.204)	-0.187*** (0.072)	-0.167	-0.142 (0.193)	-0.066 (0.073)	-0.084	-0.158 (0.194)	-0.039 (0.090)	-0.069	-0.137 (0.205)	0.215 (0.198)	-0.022
+3	0.327 (0.203)	-0.111 (0.071)	0.084	-0.065 (0.193)	0.002 (0.073)	-0.023	-0.024 (0.194)	0.037 (0.090)	0.001	-0.192 (0.207)	0.197 (0.185)	-0.034
<i>Panel B: Non-serial acquirers</i>												
-3	0.004 (0.248)	0.008 (0.041)	0.020	-0.030 (0.258)	0.065 (0.040)	0.175	-0.183 (0.219)	0.131 (0.048)	0.165	0.015 (0.185)	-0.080 (0.099)	-0.020
-2	-0.037 (0.246)	-0.024 (0.040)	-0.068	-0.397 (0.256)	0.007 (0.040)	0.011	-0.268 (0.220)	0.022 (0.048)	-0.017	-0.235 (0.186)	0.039 (0.092)	-0.057
-1	-0.443* (0.247)	-0.119*** (0.040)	-0.338	-0.510** (0.252)	-0.0168* (0.040)	-0.208	-0.024 (0.222)	-0.112** (0.049)	-0.187	-0.002 (0.185)	-0.083 (0.092)	-0.031
election	0.117 (0.259)	-0.063 (0.040)	-0.176	-0.356 (0.261)	-0.042 (0.040)	-0.131	0.040 (0.226)	-0.097** (0.048)	-0.151	0.007 (0.186)	-0.012 (0.089)	-0.003
+1	-0.118 (0.241)	0.007 (0.042)	0.015	0.301 (0.258)	0.016 (0.041)	0.049	0.252 (0.223)	0.032 (0.050)	0.099	0.185 (0.186)	-0.081 (0.099)	0.032
+2	-0.027 (0.245)	-0.011 (0.040)	-0.030	-0.342 (0.254)	0.026 (0.040)	0.066	-0.420* (0.218)	0.042 (0.049)	-0.015	-0.092 (0.189)	0.053 (0.092)	-0.007
+3	0.415 (0.262)	0.009 (0.040)	0.034	-0.282 (0.253)	0.036 (0.040)	0.099	-0.177 (0.219)	0.042 (0.049)	0.035	-0.132 (0.188)	0.096 (0.093)	-0.004

Fig. 7. Plot of marginal effects of elections on acquirer (top) and target (bottom) activity for serial and non-serial acquirers as estimated in Table 4. Details on the sample, model, and marginal effect calculations are given in the caption of Table 3.

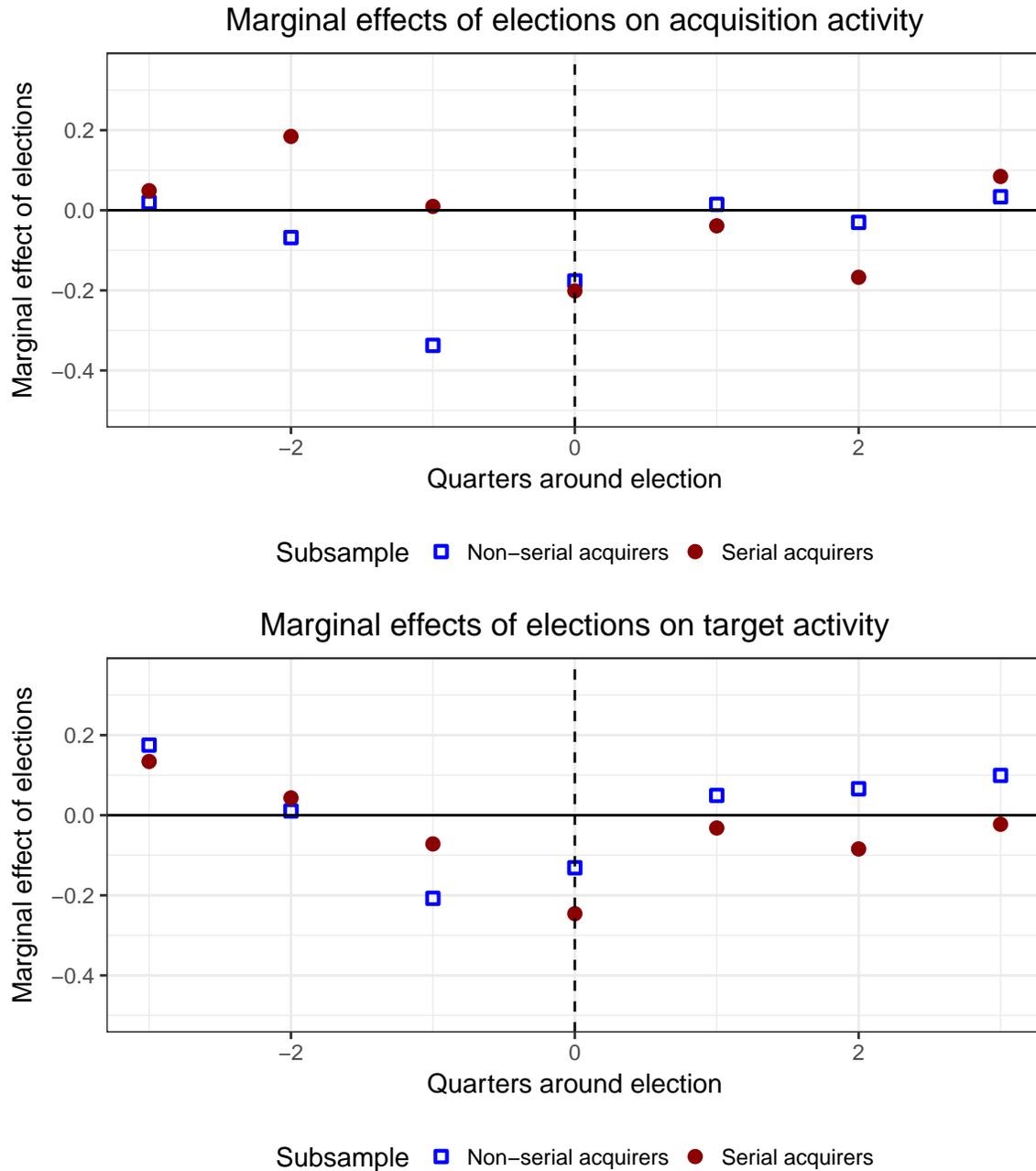


Fig. 8. Plot of marginal effects of elections (left) and a one standard deviation change in the EPU index (right) on acquirer and target activity. The marginal effects of elections are calculated with the EPU index held at its mean (average EPU) or one standard deviation above its mean (high EPU). The marginal effects of the EPU index is calculated as the difference in predicted M&A activity with the EPU one standard deviation above its mean (high EPU) relative to its mean, with elections held at either one (election) or zero (no election). All other variables are held at their means for marginal effects calculations. Details on the sample, model, and marginal effect calculations are given in the caption of Table 3. EPU data are from Nick Bloom's website and are calculated starting in 1985, limiting our estimation to 5,800 state-quarter observations. Baker et al. (2016) fully describes the calculation of the EPU index.

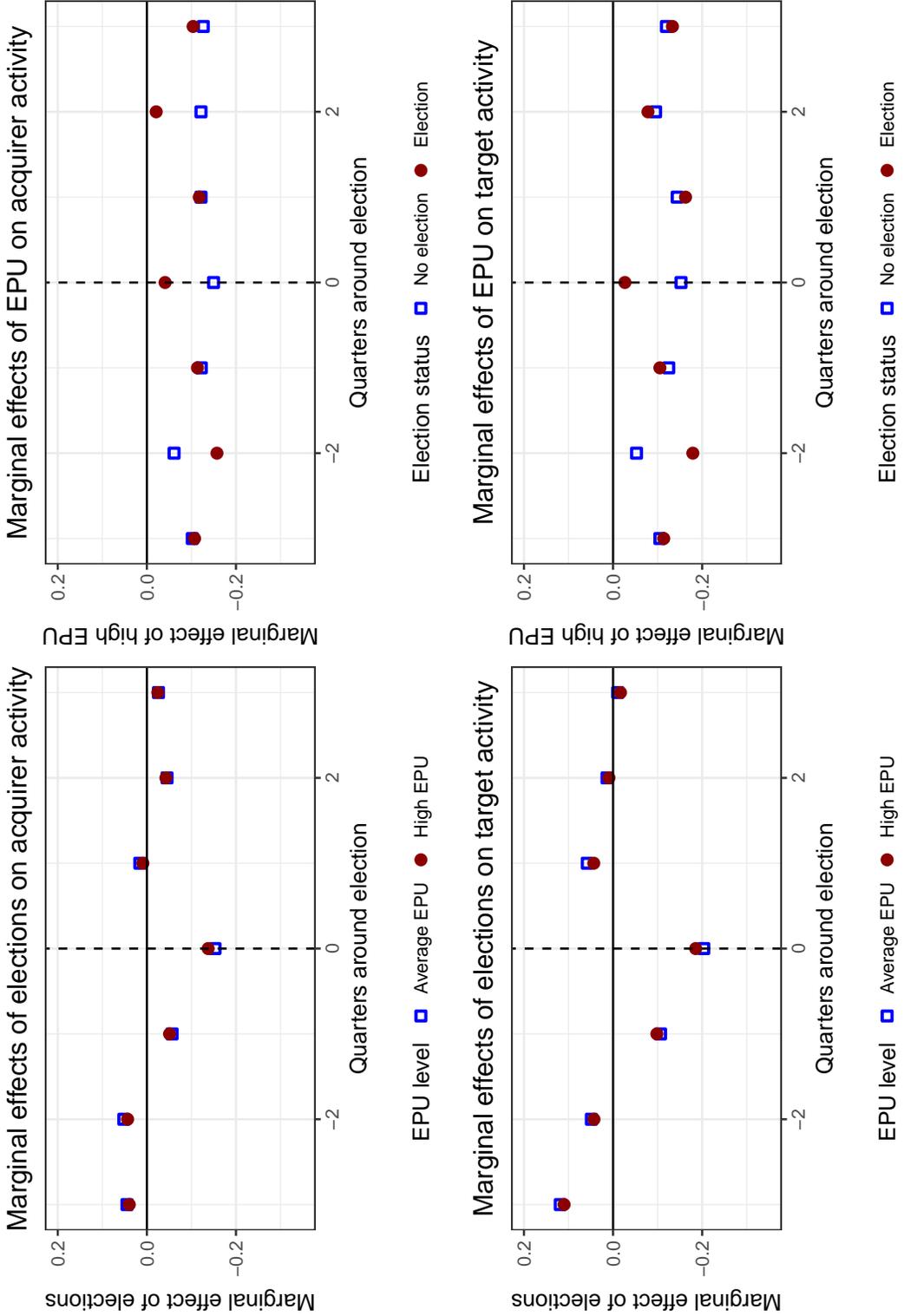


Fig. 9. Plot of marginal effects of elections (left) and a one standard deviation change in the VIX uncertainty index (right) on acquirer and target activity. The marginal effects of elections are calculated with the VIX held at its mean (average VIX) or one standard deviation above its mean (high VIX). The marginal effects of the VIX is calculated as the difference in predicted M&A activity with the VIX one standard deviation above its mean (high VIX) relative to its mean, with elections held at either one (election) or zero (no election). All other variables are held at their means for marginal effects calculations. Details on the sample, model, and marginal effect calculations are given in the caption of Table 3. VIX data are from the Chicago Board Options Exchange (CBOE) website. VIX data are available starting in 1990, limiting our estimation to 4,800 state-quarter observations.

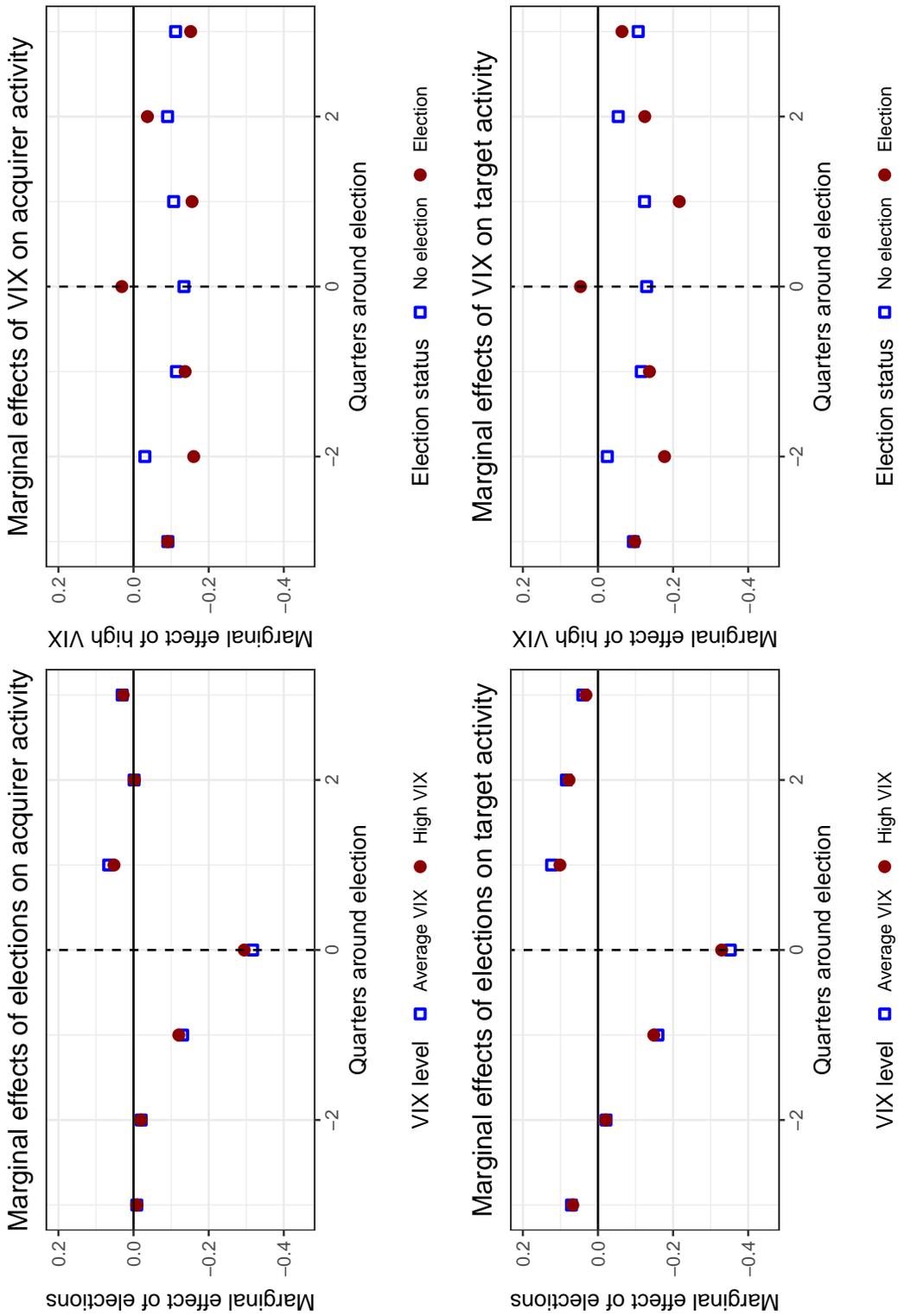


Fig. 10. Plot of marginal effects calculated using estimates from monthly state-level acquirer (top) and target (bottom) levels. The estimation is identical to that described in the caption of Table 3, except that the dependent variables are calculated monthly (state-month) instead of quarterly (state-quarter).

