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

Zhenhua Chen
Louisiana State University E. J. Ourso College of Business
zc11@lsu.edu

Mehmet Cihan
mcihan@tulane.edu

Candace E. Jens
Martin J. Whitman School of Management at Syracuse University
cejens@syr.edu (corresponding author)

T. Beau Page
Office of the Comptroller of the Currency (OCC)
trenton.page@occ.treas.gov

Abstract

We study how firms alter investment projects to mitigate exposure to political uncertainty. We examine deal-level merger data and find that, in addition to delaying and forgoing merger announcements, acquirers shift merger announcements earlier in time to avoid the period between announcement and effective dates overlapping an election, shift targets geographically away from election states, decrease the size of election-year deals, and shift from equity to cash financing for election-year deals. These results are stronger for acquirers with tighter financial constraints and deals more likely to be financed with equity and show financing matters to firms' responses to election uncertainty.

I. Introduction

> …Great injury results from an unstable government. The want of confidence in the public councils dampens every useful undertaking, the success and profit of which may depend on a continuance of existing arrangements. …In a word, no great improvement or laudable enterprise can go forward, which requires the auspices of a steady system of national policy. James Madison, Federalist No. 62, Feb. 27, 1788.

Since the founding of the USA, uncertainty about the likelihood of change in government policies has been recognized as a threat to investment. Yet, a burgeoning literature has only recently begun to examine the real effects of political uncertainty. This literature focuses primarily on how political uncertainty within

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a state or country affects the level of investment of firms domiciled in that location and quantifies the costs of political uncertainty in forgone investment (Julio and Yook (2012), Gulen and Ion (2016)). In comparison, less studied is whether and how firms alter investment projects when uncertainty is high beyond cancelation or postponement. Firms may be able to avoid lost investment if, for example, projects can be shifted earlier in time to avoid high-uncertainty periods or away from states or countries with relatively higher uncertainty.

Determining empirically whether and how firms alter investment projects when exposed to uncertainty is difficult. Studies in this literature primarily focus on investment data aggregated at the firm level, like capital expenditures (Julio and Yook (2012), Gulen and Ion (2016)) and R&D expenses (Atanassov, Julio, and Leng (2016)). In general, disaggregating such data into individual projects is not feasible. Additionally, endogeneity in the form of selection bias is also a challenge. Most common measures of policy and political uncertainty, including the economic policy uncertainty (EPU) index (Baker, Bloom, and Davis (2016)), vary only in the time series. Without cross-sectional variation in uncertainty, correlations between the types of projects firms undertake and firm sensitivity to policy uncertainty are impossible to disentangle. For example, large and small firms invest differently and are also disparately sensitive to policy uncertainty. How, then, can a researcher determine which differences between projects by small and large firms to attribute to firm size and which to attribute to sensitivity to policy uncertainty or variation in uncertainty across time?

We use an empirical setting to address each of these challenges. We examine deal-level mergers and acquisitions (M&A) data, which constitute data on individual projects. Our sample includes 36,685 deals from 1986 to 2016 and details on these projects including announcement and effective dates, size, location of target, and mix of cash and equity financing.

We combine these data with an exogenous source of political uncertainty with cross-sectional variation, the timing of U.S. gubernatorial elections. The timing of U.S. gubernatorial elections is predetermined and widely recognized as exogenous (Jens (2017), Çolak, Durnev, and Qian (2017), and Atanassov et al. (2016)).[1] Election cycles are staggered across states with at least some states electing a governor each year. Additionally, although firms are not randomly distributed among states, election cycles are fairly random across states. This variation in election timing allows us to use a difference-in-differences (DD) framework. Our DD setup compares merger activity in election states against similar activity in nonelection states.

Our dependent variables are aggregations of the total number of targets or acquirers announced in a state in a quarter. In our main empirical specifications, we link acquirers and targets to election cycles using their states of headquarters. In robustness work, we link firms with states of operations calculated using information from 10-Ks and find stronger effects of political uncertainty on merger activity.

[1]See Jens (2017) for a more in-depth discussion of the assumptions required for the identification of causal effects using a DD framework and gubernatorial elections as a source of uncertainty and Jens and Page (2021) for a discussion on the difficulty in interpreting the effects of election closeness due to correlated confounding variables.

In our first set of empirical tests, we show that acquirers shift announcements backward in time to avoid election uncertainty and prefer targets headquartered in nonelection states to targets headquartered in election states. We find that, approximately 1 year before the election, there are 2% more announcements by acquirers headquartered in election states than in nonelection states. Then, for approximately 1 year around the election, there are 2%–3% fewer announcements by election-state acquirers relative to nonelection-state acquirers.

We observe larger election-year shifts in target announcements. In Q3 and Q4 of years before elections, we find 2.7% and 3% more announcements for targets headquartered in election states, respectively. Around elections, we see up to 4% fewer target announcements in election states than in nonelection states. Because we observe declines in both acquirer and target announcements around elections, even after controlling for same-state deals and deals in which both acquirers and targets are headquartered in election states, we can infer that some acquirers avoid political uncertainty by shifting from election to nonelection-state targets.

This shift in target selection from election to nonelection states has important implications for the effects of political uncertainty on firms. Arguably, firms can shift R&D and capital expenditures over borders and back again, reversing the effects of political uncertainty on these investments.[2] However, mergers are rarely undone. Our results suggest that, through geographically shifting target selection, acquirers adjust their real activities to shield themselves from political uncertainty in a way that can permanently alter the geographic area in which they operate. Thus, spikes in political uncertainty, even if temporary, can have long-lasting consequences for firms and states beyond a few quarters of lower investment levels.

Additionally, these results show that higher costs of capital driven by political uncertainty around elections affect merger activity. Theoretically, there are two ways in which political uncertainty can affect investment. First, political uncertainty can cause firms to delay projects by increasing the option value of delaying investment. Real options theory predicts that high uncertainty should cause prolonged declines in activity until uncertainty resolves, like the lower announcement activity Bonaime, Gulen, and Ion (2017) find when EPU is high. Second, higher costs of capital around elections should also cause lower merger activity around elections;[3] these effects cannot be distinguished from real options effects. However, the effects of higher financing costs around elections are not limited to this decline. Because elections are predictable sources of uncertainty, firms can also proactively announce deals before elections. The higher pre-election merger activity we observe is consistent with firms timing announcements to avoid higher costs of capital around elections, not real options theory.

To further demonstrate the importance of financing costs and constraints to the effects of political uncertainty on mergers, we examine deals that are particularly susceptible to such effects. First, we examine deals by acquirers with relatively

[2]In later subsample results, we show that mergers most affected by political uncertainty are by acquirers that do not acquirer serially (acquirer multiple targets consecutively). We find no reversal in our effects.

[3]Political uncertainty increases the cost of debt (Waisman, Ye, and Zhu (2015), Liu and Zhong (2017), and Kaviani, Kryzanowski, Maleki, and Savor (2020)) and equity issuances (Gungoraydinoglu, Çolak, and Öztekin (2017)).

higher costs of capital. We show that small and unrated acquirers have larger pre-election boosts in announcements than large and rated acquirers. Additionally, we find that deals paid for entirely in stock are more likely to be announced in the year before an election and less likely to be announced around elections. In contrast, there are no differences between the number or timing of election-year and nonelection-year all-cash deal announcements.

We show that our results are likely not driven by only acquirer characteristics or payment type, but by an interplay between the two. For example, we find strong evidence that announcements of small deals (less than $250 million) are shifted backward in time away from elections likely because these deals, on average, involve small, financially constrained acquirers. However, when we examine deal size scaled by acquirer size, we find that relatively larger deals (relative to acquirer size) are more affected by political uncertainty than relatively smaller deals. We show that, as deals increase in size relative to acquirer size, they are more likely to be paid for in stock. Taken together, these results suggest that both acquirer and deal characteristics matter to a deal's sensitivity to political uncertainty.

In our last empirical test, we examine deals involving serial acquirers. Serial acquirers acquire five or more targets in a 3-year period (Fuller, Netter, and Stegemoller (2002)). Additionally, serial acquisitions are more likely to be funded with equity than any other subsample of deals we examine. Approximately one-third of serial acquisitions are funded entirely with stock, whereas less than 18% of serial acquisitions are funded entirely in cash. Despite this reliance on equity financing, serial acquirers are more likely than non-serial acquirers to have bond ratings. This subsample of deals is interesting to examine because, typically, firms with greater access to lower-cost sources of financing like debt rely less on equity financing.

We find that serial acquirers do not significantly decrease election-year merger activity but shift from equity to cash financing. These acquirers are able to shift from higher-cost equity to lower-cost financing because lower-cost financing is available to them. We see much more modest shifts in payment type, but larger decreases in announcements, for firms with tighter financial constraints. Taken together, these results show that understanding the role of financing costs in how political uncertainty affects merger activity is important.

Our focus on political uncertainty's effect on investment via financing costs is unique in the literature. Real options theory motivates several studies of the effects of uncertainty on merger activity, including Bhagwat, Dam, and Harford (2016) and Bonaime et al. (2017), who measure uncertainty with VIX and the EPU index, respectively. Our use of a predictable source of uncertainty is key to our ability to observe firms planning around expected periods of higher costs of capital. This pre-election adjustment in announcement timing is similar to the pre-election saving Jens and Page (2021) document that smooths firms' costs of capital through election years.

Our results constitute fairly conservative estimates of the effects of political uncertainty on merger activity. We focus on state-level variation in political uncertainty in the USA, which is a relatively stable country politically, because doing so increases our ability to control for confounding factors and plausibly measure a causal relation, as well as convincingly show a shift in investment between states.

However, our results if extended to an international sample suggest that political uncertainty within a country can have large adverse effects on, for example, foreign direct investment.[4]

Although mergers constitute a large fraction of corporate investment,[5] there are relatively few studies on the effects of uncertainty on merger activity. Our focus is distinct from studies examining how time-series variation in the EPU index is related to aggregate U.S. merger activity (Nguyen and Phan (2017), Bonaime et al. (2017)). Our use of a cross-sectional research design is key to studying geographic shifts in investment and disentangling how political uncertainty affects the attributes of projects.

Additionally, our results provide an interesting contrast to those in Bhagwat et al. (2016) and Cao, Li, and Liu (2019). Whereas Bhagwat et al. (2016) find the decline in merger activity related to VIX only in large deals by public acquirers, we find a decline in the number of smaller deals and in acquisitions with public and private acquirers for public and private targets. Our results also contrast with those in Cao et al. (2019), who find an increase in international merger activity when political uncertainty is high. Fully reconciling results between studies examining different types of uncertainties is beyond the scope of our research question. However, discussing differences between the results here and those in Bhagwat et al. (2016) and Cao et al. (2019) highlight that there is still work to be done in terms of understanding how M&As and investment, generally, are affected disparately by political and broader types of uncertainties.

II. Related Literature and Hypothesis Development

In this article, we ask how political uncertainty causes firms to alter investment projects. To motivate our empirical tests, we begin with a discussion of empirical evidence of uncertainty's effects on merger activity and why we expect merger activity is sensitive to uncertainty from gubernatorial elections. We then discuss theoretical literature on uncertainty and investment more broadly. We finish by linking our empirical setup with this theoretical literature to provide testable hypotheses for our setting.

Mergers are an ideal setting in which to examine how political uncertainty affects characteristics of investment, including the timing, geographic location, size, and sources of financing of projects. Compustat data on, for example, capital expenditures or R&D cannot be easily disaggregated into projects and so is less useful to answer our research question. In contrast, deal-level merger data constitute individual investment projects. In merger data, we observe announcement and effective dates, which provide a clear time horizon for a project, as well as the size and location of the target. Additionally, in some deals, acquirers and targets are headquartered and operate in different locations. Our use of a cross-sectional measure of uncertainty allows us to examine deals in which acquirers and targets

[4]Julio and Yook (2016) show that aggregate foreign direct investment from the USA is sensitive to election cycles.

[5]In 2015, the total deal value of U.S. M&As was just over $2 trillion, larger than total capital expenditures of $1.64 trillion (S&P Global).

are individually affected by political uncertainty and to observe geographic shifting of target selection from election to nonelection states.

Existing literature shows that merger activity is sensitive to a variety of sources of uncertainty. In one of the first empirical studies of the effects of uncertainty on merger activity, Bhagwat et al. (2016) show that aggregate announcements in the USA are inversely related to the VIX index. In the same vein, Bonaime et al. (2017) show that higher policy uncertainty, measured with the EPU index, depresses aggregate merger announcements for several quarters and find no evidence of a subsequent reversal or recouping of lost deals.

Consistent with empirical results in these academic studies, policy and political uncertainty is widely recognized by industry practitioners as detrimental to M&A activity. In a 2017 outlook for M&A activity, Deloitte writes, "Uncertainty tends to drag on corporate confidence…Unsurprisingly, during the financial crisis, Eurozone debt crisis and Brexit, there was a slowdown in M&A." Deloitte cites upcoming "…elections across Europe, policy announcements from the newly-elected U.S. government, and progress in the UK's exit negotiations with the EU" as sources of expected future uncertainty ("Uncertainty is the 'New Normal'." Deloitte M&A Index: Outlook for 2017. https://www2.deloitte.com). In early 2019, 35% of surveyed Health Care M&A professionals, "…cited political uncertainty as the factor most likely to sink deals in the coming year…." Consistent with the Deloitte publication, relevant sources of uncertainty this survey identifies include the U.S. election season and Brexit ("Political Uncertainty Top Factor Threatening Healthcare M&A." Jacqueline LaPointe. May 29, 2019. https://revcycleintelligence.com).

Similarly, merger activity should be sensitive to political uncertainty created by state-level elections. State-level policies including taxation, regulation, and business incentives affect firms' profitability and ability to do business. Because these policies affect firms, uncertainty about these policies affects firms. Indeed, Baker et al. (2016) show that one source of policy uncertainty that EPU measures is elections.[6] Additionally, empirical evidence shows that political uncertainty caused by gubernatorial elections affects capital expenditures (Jens (2017)), R&D (Atanassov et al. (2016)), equity and debt issuance activity (Jens (2017)), and saving (Jens and Page (2021)). Correspondingly, we expect this source of uncertainty to affect M&A investment.

Although most anecdotal evidence focuses on uncertainty created by national "headliner" elections, discussions about the uncertainty created by national elections extend naturally to state-level elections. A recent publication by Grant Thorton focuses on tax policy uncertainty driven by the 2020 presidential election with "…the potential to dramatically affect the economic consequences of future M&A activity" ("Election Pivotal for Tax Impact of Future Deals." https://grantthornton.com). The article compares a number of expected tax rates under the Trump administration versus a hypothetical Biden administration that can affect M&A profitability, including corporate rates, individual rates, and long-

[6]See Jens (2017) for a discussion on why gubernatorial elections subsume other state-level elections in importance. Jens and Page (2021) argue that congressional and Senate elections cannot confound gubernatorial elections.

term capital gains rates. Windes' M&A advisors highlight the importance of state and local tax concerns in mergers and write that these taxes can "…impact the overall value of the deal and present significant delays in closing or at worst, kill the deal altogether" (Robert Corbin. State Tax Issues in M&A Transactions. Dec. 3, 2014. https://Windes.com). Windes notes that important state-level tax policies include income, sales and use, franchise, and property taxes, as well as worker classification and payroll taxes. These taxes, in addition to other state-level policies, affect merger activity. Thus, uncertainty about these state-level policies should also affect merger activity.

Theoretically, uncertainty can affect investment through three mechanisms.[7] First, Bernanke (1983), among others, discusses an investment as a real option. According to this theoretical framework, uncertainty increases the option value of delaying. Consequently, when uncertainty is high, firms delay irreversible or costly-to-reverse projects on the margin until uncertainty resolves.

Uncertainty can also affect investment through the Abel–Hartman–Oi convexity effect. According to Abel (1983), Hartman (1976), and Oi (1961), under certain conditions, marginal Q is increasing in the volatility of output prices or productivity, so volatility increases investment. Thus, an increase in uncertainty increases contemporaneous investment. In general, however, empirical literature, beginning with Leahy and Whited (1996), documents an inverse effect between uncertainty and investment, which contradicts this theory. In the political uncertainty literature, a number of studies including Julio and Yook (2012), Gulen and Ion (2016), and Jens (2017) show that political uncertainty decreases investment.

Finally, uncertainty can decrease investment through increasing financing costs. Pástor and Veronesi (2013) and Kelly, Pástor, and Veronesi (2016) show that investors demand a risk premium for political risk. Additionally, studies show that political uncertainty increases financing costs.[8] All else equal, rising financing costs should reduce investment.

We focus primarily on exploring the nuances in how increasing costs of capital around elections affect merger activity for two reasons. First, less literature is devoted to this mechanism. Real options theory motivates existing studies of uncertainty and merger activity, including Bhagwat et al. (2016) and Bonaime et al. (2017). This focus persists despite the fact that the broader political uncertainty literature shows that financing costs are important to understanding how political uncertainty affects firms. For example, Jens and Page (2021) show that firms anticipate higher costs of capital coinciding with elections and increase pre-election savings to smooth financing costs over time and that these effects are strongest for investment-intensive and rapidly growing firms.

Second, our empirical setting provides a unique opportunity to test hypotheses regarding political uncertainty, costs of capital, and merger activity. Gubernatorial elections are a source of uncertainty that is predictable (Jens and Page (2021)). Thus, we can observe pre-election announcement activity to determine whether firms shift announcements backward in time away from elections so that the time

[7]See Bloom (2009) for a more in-depth discussion.

[8]See, for example, Waisman et al. (2015), Liu and Zhong (2017), Kaviani et al. (2020), and Gungoraydinoglu et al. (2017).

period between the announcement and effective dates do not overlap the election. We expect that higher financing costs around elections result in lower merger activity. While we cannot differentiate lower activity caused by increased costs of capital from declines caused by real options effects, any pre-election boosts in activity are inconsistent with real options theory. Our focus on a predictable source of uncertainty is key to our ability to provide empirical evidence that higher costs of capital significantly affect merger activity.

We use details of our empirical setting and theory about uncertainty and investment to make the following predictions. First, we expect that firms will mitigate their exposure to higher costs of capital coinciding with elections. We expect that acquirers will shift announcements backward in time to avoid the period between the announcement and effective dates (which, on average, last approximately 4–6 weeks) overlapping an election. We also expect acquirers to forgo announcements closer to elections and to select more targets from states in which there are no upcoming elections. We expect these reductions in announcements to result in lower aggregate deal value in election years.

Additionally, we expect these effects to be stronger for deals relatively more sensitive to political uncertainty. Firms that are more geographically concentrated in their states of headquarters, which are more likely to be smaller acquirers, are relatively more exposed to political uncertainty from gubernatorial elections. Similarly, we expect stronger effects before closer, more uncertain elections.

Finally, these effects should be greater for acquirers more sensitive to rising financing costs. Deals by acquirers with tighter financing constraints or deals more likely to be paid for with equity financing should be more affected by the higher costs of capital driven by political uncertainty. In some cases, we may see that higher financing costs drive acquirers to switch payments in whole or part from equity toward cash.

III. Data and Variables

A. Measuring Uncertainty

Our sample includes 419 gubernatorial elections (all regular gubernatorial elections from 1986 to 2016 and the 2010 Utah special election). There were four special elections during the sample period: Utah (2010), West Virginia (2011), Wisconsin (2012), and California (2013). The latter three are excluded from the sample because the elections were planned too quickly for firms to observe and react to pre-election political uncertainty. 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 the 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 Supplementary Material.

We create variables using gubernatorial and presidential election data from Congressional Quarterly Press. ELECTION is a binary variable equal to 1 if a state

elects a governor in a year.[9] We control for uncertainty from the presidential election cycle with PRES_ELECTION, an indicator variable equal to 1 if a presidential election occurs in a year.[10]

We include two additional proxies for uncertainty in our empirical tests: VIX and the EPU index. VIX measures expected future volatility in equities using S&P 500 index options and is from the Chicago Board Options Exchange. We begin our sample in 1986 because this is the earliest VIX available. There are several different components to the EPU index, which measures uncertainty regarding future policy.[11] These components comprise i) analysis of text from major newspapers, ii) analysis of reports from the Congressional Budget Office on tax code provisions due to expire, and iii) measures of dispersion between predictions in the Federal Reserve Bank of Philadelphia's Survey of Professional Forecasters. Baker et al. (2016) further detail the EPU index's construction.

B. 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 of 1986 to 2016 in which a publicly traded or private firm buys another publicly traded or private firm. To be included in the sample, a deal must be classified as i) an acquisition, ii) an acquisition of assets, iii) an acquisition of majority interest, or iv) 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, e.g., 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 that our results are not driven by the smallest mergers, we examine subsamples constructed using several measures of the firm and deal size. For inclusion in our sample, we require the 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 final merger sample includes 36,685 deals.

Our sample begins in 1986. Because of concerns about data quality before 1990, Bhagwat et al. (2016) limit their sample to deals announced after 1989. To preserve the sample size, we include deals from 1986 to 1989 in our sample. Our results are stronger if we start our sample in 1990, rather than in 1986 (see Table IA.15 in the Supplementary Material).

Our dependent variables are aggregated quarterly state-level M&A activity, measured as either the total number or value of deals. Our data include 31,434 public acquirers, 5,251 private acquirers, 9,853 public targets, and 26,832 private targets. To create our count variables, we calculate the total number of acquirers, public acquirers, private acquirers, targets, private targets, and public targets for each state-quarter. The aggregate deal value is calculated as the natural logarithm of

[9] All variable names are in all caps throughout the article.

[10] See Jens and Page (2021) for a discussion of why Senate and other congressional elections are less important to control for in this setting.

[11] EPU data are available at https://policyuncertainty.com.

1 plus the sum of deal values (2016 U.S. $) per state-quarter. We link acquirers and targets to elections based on their state of headquarters reported in SDC. We identify the timing of a merger within a quarter using the announcement date because firms are most susceptible to policy changes during the time period between the announcement and effective dates (Bhagwat et al. (2016)). Our final sample includes 6,150 state-quarter observations for each merger activity measure.

We also create M&A activity variables for subsamples of interest. To create these variables, we begin by splitting our sample of deals along a subsample definition, for example, small and large acquirers. For each year, we determine which acquirers are above and below the median asset value. Then, we re-calculate state-quarter activity variables separately for both large and small acquirers. We repeat this procedure for all subsamples we examine.

One concern with our dependent variable construction is the quality of the SDC headquarters data. Heider and Ljungqvist (2015) discuss that headquarters locations in Compustat data are backfilled, so that, for example, a firm that moved its headquarters in 2001 from Seattle to Chicago is listed with an Illinois headquarters for all Compustat observations, including those before 2001. We compare SDC headquarters with Compustat headquarters data and confirm that SDC headquarters data have the same flaw.

To mitigate this concern, we correct the headquarters states in our data using several methods. First, we implement the headquarter changes Heider and Ljungqvist (2015) identify from 1989 to 2011. Next, for observations after 2011, we update firms' headquarters using the business address in the filer information section of the most recent 10-K filing before the deal announcement. We are unable to repeat this procedure for our pre-1989 sample (comprising only 3 years), as electronic 10-K filings are only available back to 1994. Reassuringly, however, Heider and Ljungqvist (2015) note that backfilling is less of a problem in pre-1990 data.

Using these procedures, we update the headquarters states of 962 acquirer firms and 69 target firms, constituting approximately 2.6% and 0.19% of these samples, respectively. Unfortunately, these fixes are only available for public acquirers and targets and private targets and acquirers that file 10-Ks prior to the deal announcement. Over 85% of acquirers but only 35.5% of targets in our sample are public. Consequently, these adjustments improve the quality of headquarters data for acquirers more than targets in our sample. However, it is important to note that inaccurate headquarters' locations link firms to states for which they should not be sensitive to election uncertainty and, thus, biases against our results.

A related but distinct concern is that firms are more sensitive to political uncertainty from the states in which they operate rather than their states of headquarters. For example, The Boeing Company is headquartered in Chicago, Illinois, but its major manufacturing plant is in Everett, Washington. Arguably, then, Boeing is more sensitive to gubernatorial election uncertainty from Washington, rather than Illinois. To address this concern, we examine merger activity for firms linked to states we identify using the 10-K state-name count procedure from García and Norli (2012) that attempts to identify states relevant to firms' operations. We detail this 10-K count procedure in the Supplementary Material. As expected, our results are

stronger for states more relevant to firms' exposure to political uncertainty (see Figure IA.1 in the Supplementary Material).

Our results are also robust to examining monthly, rather than quarterly, state merger activity. Although merger data can be aggregated monthly by announcement date, monthly merger activity is 0 in many states. To use our DD framework, we need a sufficiently large cross section of states with measurable merger activity. Therefore, for our main empirical tests, we aggregate our data quarterly. However, our conclusions are unchanged when using monthly data, and monthly results are presented in Figure IA.2 in the Supplementary Material.

While we have sufficient variation in state-level merger activity for our aggregate sample tests, the sparseness of data in some states becomes a problem for some subsample tests. In subsamples, merger activity in some states can be perfectly predicted with fixed effects, so we exclude 17 states with relatively low merger activity from subsample tests. When we drop these low-activity states, our sample includes 4,059 state-quarters, 273 gubernatorial elections, and 34,872 deals. Table IA.1 in the Supplementary Material reports the total number of acquirers and targets for each state and identifies low-activity states. 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 the identification of the effects of political uncertainty. We also estimate subsample results without state fixed effects, including observations from these states, and find much larger declines in acquisition activity around elections, so the results presented in this article are conservative estimates.

Table 1 presents summary statistics. The summary statistics in Panel A show merger activity at the state-quarter level. There is an average of 5.97 acquirer and target announcements per state-quarter, with a median of 2 for acquirer and 3 for target announcements, respectively. Approximately 31.7% of mergers involve targets and acquirers headquartered in the same state. On average, there are more deals for smaller targets, by firms with bond ratings, and by non-serial acquirers. We find relatively similar merger activity by large and small acquirers and for all-stock and all-cash deals.

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, a fairly large percentage of our observations are zeros; for example, the 25th percentile for all acquirer activity variables is zero. Thus, we elect to use a two-stage model, a hurdle Poisson model, that accommodates both the large number of zeros and discrete quality of our data. We discuss this specification further in Section IV.B.

IV. Empirical Specification and Identification

A. 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

TABLE 1

Summary Statistics

Table 1 reports summary statistics for state-level acquisition activity measures and control variables for state-level tests for a sample of 6,150 quarterly observations from 1986 to 2016. Quarterly M&A activity variables are the sum of the number of deals announced per state-quarter for acquirers and targets. Firms are linked to states by the state of headquarters reported in SDC Platinum augmented by corrections from Heider and Ljungqvist (2015) and 10-K filings post-2011. ELECTION is a binary variable equal to 1 if a gubernatorial election was held in that state in that year. EPU_BASELINE is the beginning-of-quarter value of baseline economic policy uncertainty, as described in Baker et al. (2016). VIX is the beginning-of-quarter value of the VIX index. UNEMP is quarterly average state unemployment from the Bureau of Labor Statistics, for which we have observations for 5,800 state-quarters. RECESSION is a dummy variable equal to 1 if at least 1 month during the quarter is designated by the NBER as being in a recession. ΔGDP is annual change in state GDP from the Bureau of Economic Analysis, for which we have observations for 5,550 state-quarters.

Panel A. Variables for Main and Subsample Tests

	Acquirers					Targets				
Variables	Pctl(25)	Mean	Med.	Pctl(75)	Std. Dev.	Pctl(25)	Mean	Med.	Pctl(75)	Std. Dev.
Whole sample	0	5.97	2	7	10.93	1	5.97	3	7	10.66
Same-state acquisitions	0	1.89	0	2	4.89	0	1.89	0	2	4.89
Cross-state acquisitions	0	4.07	2	5	6.60	1	4.07	2	5	6.25
Deals > $250 mil.	0	0.96	0	1	2.15	0	0.96	0	1	2.08
Deals < $250 mil.	0	5.01	2	6	9.20	1	5.01	2	6	8.96
Relatively large deals	0	1.63	1	2	3.16	0	1.63	1	2	3.03
Relatively small deals	0	1.66	0	2	3.57	0	1.66	1	2	3.43
Acq. by large acquirers	0	1.66	0	2	3.23	0	1.66	1	2	3.17
Acq. by small acquirers	0	1.66	0	2	3.78	0	1.66	1	2	3.52
With bond ratings	0	0.94	0	1	1.82	0	0.94	0	1	1.85
Without bond ratings	0	5.02	2	6	9.54	1	5.02	2	6	9.14
Serial acquirers	0	0.84	0	1	2.36	0	0.84	0	1	2.19
Non-serial acquirers	0	5.13	2	6	9.19	1	5.13	2	6	8.97
All stock deals	0	1.41	0	2	3.56	0	1.41	0	2	3.54
All cash deals	0	1.42	0	2	2.79	0	1.42	1	2	2.70

Variables	Pctl(25)	Mean	Med.	Pctl(75)	Std. Dev.
Panel B. Variables for Robustness Tests					
Private acquirers	0	0.85	0	1	1.85
Public acquirers	0	5.11	2	6	9.65
Private targets	0	4.36	2	5	8.16
Public targets	0	1.60	1	2	2.94
Panel C. Election and Macroeconomic Control Variables					
ELECTION	0	0.27	0	1	0.44
EPU_BASELINE	81.18	108.79	99.67	124.94	33.77
VIX	14.57	20.47	18.53	24.30	7.82
UNEMP	4.3	5.7	5.4	6.8	1.9
RECESSION	0	0.11	0	0	0.32
ΔGDP	0.03	0.05	0.05	0.07	0.04

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 can 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 exogenous.[12] Provided

[12]See, for example, Julio and Yook (2012), Julio and Yook (2016), Kelly et al. (2016), Gao, Murphy, and Qi (2019), Çolak et al. (2017), and Jens (2017).

that firms do not self-select into states because of election timing, which is unlikely, our election variables capture only the effects of political uncertainty around elections.

A natural concern in articles studying M&As is endogeneity caused by self-selection. Only some firms engage in mergers, which can result in bias caused by unobservable differences between acquirer and non-acquirer or target and non-target samples. We minimize concerns about such endogeneity by comparing mergers in election and nonelection states; effectively, all of the firms in our sample have self-selected as acquirers or targets. Our treated samples are acquirers and targets headquartered in states with gubernatorial elections in a given year; our control samples are acquirers and targets headquartered in nonelection states. We control for pre-existing differences between states 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 sidestep any concerns about self-selection and omitted variables.

In a DD framework, it is important to show pre-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 (Roberts and Whited (2013)). 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 or 2 years before a gubernatorial election. Thus, states cycle in and out of the treated and control samples over the course of our estimation.

Figure 1 shows pre-trends in an average number of acquirers and targets in election and nonelection states. Figure 1 plots the average number of acquirers (Graph A) and targets (Graph B) headquartered in states in the treated (blue boxes) and control (red dots) samples around the timing of elections. The quarter of the election is indicated by the vertical dashed line. For acquirers and targets in nonelection years, merger activity shows an upward trend over the course of the year. Election-year activity follows this upward trend for the first and second quarters of the year (2 and 3 quarters before the election), but drops down sharply in the quarter before the election. Election-year activity remains low relative to nonelection-year activity through 4 quarters after the election. Because the standard deviations of the merger activity variables are so high (Table 1), none of these differences statistically differ from each other. However, the figure strongly suggests election uncertainty affects merger activity.

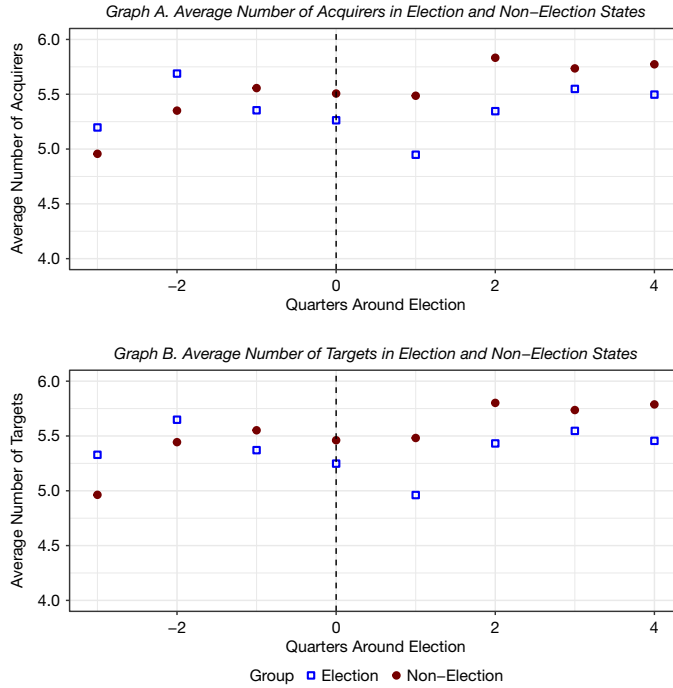
B. Empirical Specification

We face three issues in choosing an empirical specification to test our hypotheses: i) the count nature of the data, ii) an excess of zero counts, and iii) the need to control for state-level unobserved heterogeneity. Traditional linear regression is inappropriate in our setting, as our main dependent variable is the count of merger announcements in a state-quarter. A linear regression can predict non-integer and negative outcomes, making its estimates suspect. A better approach is to use a count model, such as Poisson regression. However, our data have an excessive number of zero outcomes for a Poisson regression (Table 1). While the zero-inflated Poisson model of Lambert (1992) can handle excess zero counts, it is inappropriate in a

FIGURE 1

Pre-Trends for Election and Nonelection State-Quarter Deal Totals

Figure 1 is a pre-trends plot showing average state-level mergers and acquisitions (M&A) activity in election and nonelection states for a sample of 6,150 state-quarter observations from 1986 to 2016. State-level M&A variables are calculated as the total number of acquirers or targets announced in a state 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 (nonelection) sample the year before and 2 years before election years in their state of headquarters.



panel setting with time-invariant fixed effects. We expect state-level unobserved heterogeneity is important for explaining merger activity, as states differ in the number and types of firms that may be acquirers or targets, so we must use an empirical model that allows us to incorporate state fixed effects.

We address the combined issues of an excess of zero counts and unobservable heterogeneity by including fixed effects in a hurdle model.[13] A hurdle model treats the underlying data as the outcome of a two-stage process. The first stage determines whether the outcome realization is 0 or positive. A positive realization implies that some "hurdle" has been crossed. In the second stage, a truncated count model produces the actual count if it is greater than 0. The two stages are

[13]Majo and van Soest (2011) and Gillingham and Tsvetanov (2019) develop an estimator that removes unobserved heterogeneity without the use of fixed effects in the estimation. We do not use their estimator because there is no consistent method for computing discrete effects using it, although parameter estimates from their estimator are essentially the same as those we find. Allison and Waterman (2002) use simulation evidence to show that including fixed effects in a negative binomial model does not appear to bias the estimates, and standard errors can be corrected using the deviance statistic (a correction we also use). Our results are robust to using a negative binomial model instead of a hurdle model, but we find the hurdle model to be a more natural model given the excess zeros.

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

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

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 t and quarter q, we estimate the first stage of the hurdle model as

$$f_1(0)=\frac{e^{\mu_{itq}}}{1+e^{\mu_{itq}}}, \tag{2}$$

and the second stage as

$$f_2\left(y|\lambda_{itq}\right)=\frac{\lambda_{itq}^{y}}{y!\left(e^{\lambda_{itq}}-1\right)}. \tag{3}$$

The variables included in μ_{itq} and λ_{itq} depend on the quarter for which we estimate the effect of political uncertainty.

In each estimation, we include indicator variables for years before (YEAR_BEFORE), years of (ELECTION), and years after (YEAR_AFTER) gubernatorial elections. To capture the effects of political uncertainty as firms move toward the quarter of the election (the fourth quarter of the election year), we estimate a series of 11 models, each including a quarter indicator variable (Q1, Q2, Q3, or Q4) and YEAR_BEFORE, ELECTION, and YEAR_AFTER, starting 6 quarters before the election (YEAR_BEFORE × Q2) and ending 4 quarters after the election (YEAR_AFTER × Q4). In this framework, we can observe effects leading up to the quarter of the election and whether and how long any post-election effects exist.

For example, when we estimate the effect of political uncertainty on the count of target firms in the third quarter of election years, we estimate

$$\mu_{itq}=\alpha_i^1+\alpha_t^1+\beta_1^1\text{ELECTION}+\beta_2^1\text{Q3}+\beta_3^1\text{ELECTION}\times\text{Q3}+\delta^1X_{itq}+u_{itq}^1, \tag{4}$$

and

$$\ln\lambda_{itq}=\alpha_i^2+\alpha_t^2+\beta_1^2\text{ELECTION}+\beta_2^2\text{Q3}+\beta_3^2\text{ELECTION}\times\text{Q3}+\delta^2X_{itq}+u_{itq}^2, \tag{5}$$

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 an election year in a target firm headquarters state; Q3 is a dummy variable for the third quarter; and X_{itq} includes i) dummy variables for the other quarters, ii) a dummy variable for presidential election years, and iii) an interaction between the presidential election variable and quarter. Our control variables proxy for the competing source of political uncertainty that comes from presidential elections and for competing sources of uncertainty (VIX and EPU measured at the end of the previous quarter). We discuss the inclusion of additional

control variables, which have no effect on our results, in the robustness section. The parameters of interest from equations (4) and (5) 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.

V. Results

A. Political Uncertainty Decreases State-Level Merger Activity

In Table 2, we present the estimates of the effect of political uncertainty on merger activity. We examine acquirer and target announcements in election states separately; the results are presented in Panels A and B, respectively. Table 2 presents coefficient estimates for both stages of the hurdle model and the discrete effects of elections.

Because we use a nonlinear model, our coefficient estimates cannot be interpreted as easily as if we were using OLS. Therefore, we provide discrete effects that are directly comparable to the sample means presented in our summary statistics (Table 1). To calculate these discrete effects, we hold control variables at their sample averages, calculate the predicted acquirer activity level with the election-year indicator variable (e.g., YEAR_BEFORE) and the election-year and quarter interaction (e.g., YEAR_BEFORE × Q2) set equal to 1, and then repeat the calculation with these variables set equal to 0. The discrete effect is the difference between these predictions, the estimated effect of the quarter for the treated sample relative to the control sample. Thus, the discrete effect is a composite effect of the

TABLE 2

Effect of Political Uncertainty on State-Quarter Deal Counts

Table 2 shows estimates from a hurdle model for the full sample of 6,150 state-quarter M&A observations from 1986 to 2016. The dependent variables are the number of acquirers (columns 1–6) and targets (columns 7–12) in each state-quarter. Coefficients are combined estimates on YEAR_BEFORE, ELECTION, or YEAR_AFTER and an interaction between YEAR_BEFORE, ELECTION, or YEAR_AFTER and the appropriate quarter, with the quarter interaction labeled by row. Coefficients are presented for both stages of the hurdle model. The first stage estimates the propensity of observations to be greater than 0 (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, e.g., is associated with a variable). Estimates for 6 quarters before, the quarter of, and 4 quarters after the election are presented. Discrete effects reported are calculated as the average predicted acquirer or target activity in election year-quarters, relative to nonelection year-quarters, with control variables held at their sample averages, and are directly comparable to the sample averages reported in Table 1. We include as controls a presidential election year and quarter interaction, quarter fixed effects, state fixed variables, year fixed effects, and beginning of the period values of the VIX and baseline economic policy uncertainty indexes. *, **, and *** indicate significance at the 5%, 1%, and 0.1% levels, respectively.

	Panel A. Acquirers						*Panel B. Targets*					
Dependent Variable	Zero Model		Count Model		Discrete Effect		Zero Model		Count Model		Discrete Effect	
	Estimate	SE	Estimate	SE	Estimate	SE	Estimate	SE	Estimate	SE	Estimate	SE
	1	2	3	4	5	6	7	8	9	10	11	12
YEAR_BEFORE × Q2	0.13	0.19	0.02	0.03	0.04	0.03	0.19	0.20	0.00	0.03	0.02	0.04
YEAR_BEFORE × Q3	0.04	0.20	0.07*	0.03	0.11***	0.03	0.25	0.20	0.06*	0.03	0.16***	0.04
YEAR_BEFORE × Q4	0.14	0.20	0.08**	0.03	0.14***	0.03	0.22	0.21	0.07*	0.03	0.18***	0.04
ELECTION × Q1	0.01	0.20	−0.03	0.03	−0.04	0.03	0.49*	0.21	0.00	0.03	0.02	0.04
ELECTION × Q2	−0.02	0.19	−0.04	0.03	−0.06*	0.03	−0.18	0.20	−0.04	0.03	−0.10**	0.04
ELECTION × Q3	−0.03	0.20	−0.12***	0.03	−0.17***	0.03	−0.21	0.20	−0.10**	0.03	−0.23***	0.04
ELECTION × Q4	0.13	0.20	−0.12***	0.03	−0.16***	0.03	−0.09	0.20	−0.08**	0.03	−0.20***	0.04
YEAR_AFTER × Q1	0.05	0.21	−0.11***	0.03	−0.16***	0.03	−0.22	0.21	−0.07*	0.03	−0.17***	0.04
YEAR_AFTER × Q2	0.22	0.21	−0.09**	0.03	−0.12***	0.03	0.03	0.22	−0.03	0.03	−0.08	0.04
YEAR_AFTER × Q3	0.21	0.21	−0.04	0.03	−0.06	0.03	0.04	0.22	−0.01	0.03	−0.02	0.04
YEAR_AFTER × Q4	−0.10	0.21	−0.05	0.03	−0.07*	0.03	0.12	0.22	−0.05	0.03	−0.12**	0.04

first-stage zero model and the second-stage count model, and is statistically significant if the composite effect statistically differs from 0.

The results presented in Panel A of Table 2 show an increase in acquirer activity approximately 1 year before elections followed by relatively fewer acquirer announcements from 2 quarters before to 2 quarters after elections. The dependent variable for these estimations is the number of quarterly mergers announced with an acquirer headquartered in a state. Each row presents a coefficient estimate on only the quarter interaction term from the estimation framework we discuss in Section IV.B for the specification indicated by the row label. Four and five quarters before elections, we estimate the discrete effects of 0.11 and 0.14, respectively. These estimates mean that the number of announcements by acquirers headquartered in election states is higher than the number of announcements by nonelection-state acquirers. Relative to the sample average of 5.97 acquirer announcements in a state-quarter, these estimates suggest a 1.8% and 2.3% increase in acquirer activity in these quarters, respectively. This increase in acquirer activity is consistent with acquirers shifting announcements backward in time away from elections to avoid election uncertainty.

Additionally, we find an economically significant decline in acquirer announcements around elections. We estimate negative discrete effects for 2 quarters before (ELECTION $\times$ Q2 and ELECTION $\times$ Q3), the quarter of (ELECTION $\times$ Q4), and 2 quarters after (YEAR_AFTER $\times$ Q1 and YEAR_AFTER $\times$ Q2) the election. Again, relative to the sample average of 5.97 announcements per state-quarter, we find that announcements in the treated (election) sample are 1%, 2.8%, 2.7%, 2.7%, and 2% lower than announcements in the control (nonelection) sample in these quarters, respectively. Any effects of political uncertainty on acquirer activity largely dissipate by 3 quarters after the election. We estimate lower acquirer activity 3 and 4 quarters following elections, but these estimates are only marginally significant. While the increase in acquirer activity in the years before elections lasts only 2 quarters, the decline surrounding elections is of greater or similar magnitude and exists for 4 quarters, so the net effect of political uncertainty on acquirer activity is negative.

These results in Table 2 provide new evidence on how political uncertainty affects merger activity. Given the results in Bonaime et al. (2017), we expect high uncertainty to depress merger activity for several quarters. However, we also observe a temporary boost in pre-election merger announcements, which is distinct from results in other studies of uncertainty and merger activity. This boost is only observable because firms can predict election timing and adjust pre-election activity accordingly. Additionally, this boost is not consistent with the real options framework that motivates tests in Bonaime et al. (2017) and suggests that firms are shifting merger activity away from the period around elections in which costs of capital are higher. Our results suggest that political uncertainty's effect on cost of capital influences how firms adjust election-year merger activity.

The results presented in Panel B of Table 2 show how political uncertainty in the state of headquarters of the target affects state-level target announcements. The dependent variable for these estimations is the number of quarterly mergers announced with a target headquartered in the state. Again, in the year before elections, there is an increase in announcements of approximately 2.7% and 3.0% in Q3 and Q4

for targets in treated states, relative to the sample average of 5.97. In the quarter before, quarter of, and 2 quarters after elections in the state of headquarters of the target, there are 1.7%, 3.9%, 3.4%, and 2.8%, respectively, lower levels of target announcement activity than in nonelection states.

The target results follow a similar pattern to the acquirer results, but the interpretation is subtly different. These results show that acquirers are relatively more likely to select targets in election states approximately 1 year before elections and then less likely to select targets in election states around elections. Again, the decline in merger activity surrounding the election dwarfs the pre-election boost, so that the total effect of political uncertainty caused by elections on target activity within election states is negative.

Our cross-sectional research design is unique in that it allows us to observe the effects of political uncertainty on both acquirer and target announcements. Research on how political uncertainty affects investment is primarily limited to examining aggregate firm investment measured as capital expenditures or R&D expenses.[14] In such studies, there is no practical way to isolate "projects" from Compustat data. There has been less research on how political uncertainty affects merger activity, and these studies are primarily limited to exploring time-series effects with measures of political or policy uncertainty that do not vary cross-sectionally.[15] In these studies, there is no difference between time-series counts of acquirer or target announcements. Our target activity tests provide evidence of how firms adjust investment projects in election years and also begin to hint at the costs of relatively higher political uncertainty in a state (or geographic region) in terms of forgone investment projects from that state. We test whether political uncertainty causes geographic shifting of investment projects more directly in the following section.

A potential concern with our interpretation of the results in Table 2 is that our results may be driven by deals in which a firm acquirers another firm headquartered in the same state. This possibility is unlikely because most deals in our sample involve firms with different states of headquarters (Table 1). However, if this were true, our acquirer and target results in Table 2 are demonstrating the same effect. We present evidence in the following section, which alleviates this concern.

B. Political Uncertainty Shifts State-Level Merger Activity

We show that acquirers shift target selection away from election states by examining samples of same-state and cross-state deals and in samples of deals where there is an election only in the state of headquarters of the target or acquirer. For these and subsequent results, we present only discrete effect estimates and 95% confidence intervals on the quarter interaction variables with figures. For comparison with these results, Graph A of Figure 2 presents the discrete effect estimates from Table 2. We include tables with coefficient and discrete effect estimates for all figures in the Supplementary Material.

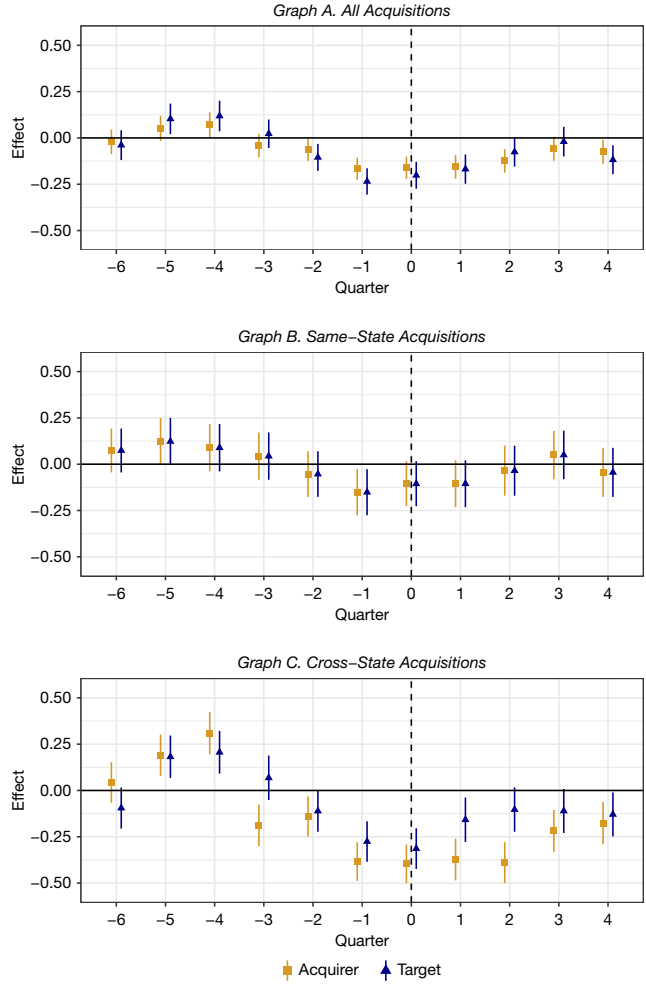
[14]See, for example, Julio and Yook (2016), Jens (2017), and Atanassov et al. (2016).

[15]See, for example, Bhagwat et al. (2016) and Bonaime et al. (2017).

FIGURE 2

Effect of Political Uncertainty on State-Quarter Deal Totals

Figure 2 shows a plot of discrete effects (i.e., the difference in acquisitions from the baseline level predicted by the hurdle model) calculated using estimates from state-quarter acquirer and target observations for all acquisitions (Graph A), same-state acquisitions (Graph B), and cross-state acquisitions (Graph C). Coefficient estimates for Graph A are presented in Table 2, and estimates for Graphs B and C are presented in Table IA.2 in the Supplementary Material.



The results in Graphs B and C of Figure 2 provide discrete effect estimates for political uncertainty on same-state and cross-state mergers, respectively. The results in Graph B of Figure 2 show a decline in same-state deals in the quarter before, quarter of, and quarter after elections (results are the same for acquirers and targets because the samples are identical). Relative to the sample average of 1.89 (Table 1), the discrete effects in Panel B show that election-year same-state mergers are approximately 6.6% more likely than nonelection-year same-state mergers before elections. We find a decline in merger activity of roughly the same magnitude for 3 quarters around elections.

The results in Graph C of Figure 2 show that political uncertainty has larger effects on announcements for cross-state acquisitions than same-state acquisitions. We find that cross-state deals are approximately 4.9%–6.1% more likely in the year before the election, relative to the sample average of 4.07 (Table 1). Additionally, there is a decline in announcements for both acquirers and targets for cross-state deals that persists for as long as 5 quarters. We estimate that, depending on the quarter, this decline in cross-state merger activity ranges approximately from 6.1% to 9.2%. These results show clearly that earlier results are not driven by acquirers and targets with the same state of headquarters; political uncertainty greatly affects cross-state *and* same-state merger activity.

Our target results in Graph C of Figure 2 can be interpreted in two ways. We find fewer targets for cross-state deals headquartered in election states, relative to nonelection states. It is possible that out-of-state acquirers decrease their overall level of acquisitions and cancel deals with targets headquartered in election states. Alternatively, out-of-state acquirers can shift from selecting election to nonelection-state targets. A combination of both stories is also possible. Because there are at least a few gubernatorial elections in each year, we cannot explicitly test against the first story. To show whether the occurrence of any gubernatorial election decreases the level of out-of-state acquirers, we need at least 1 year with no gubernatorial elections, which does not occur in the USA. Regardless, the results in Figure 2 provide evidence that out-of-state acquirers shift from selecting election-state to nonelection-state targets.

There are two key features of our empirical design that allow us to interpret our target results in Figure 2 as a geographic shift in investment away from states with higher uncertainty, rather than a decline in investment in states with higher uncertainty. First, we limit our sample to deals with acquirers and targets headquartered in the USA. This means that our total number of acquirers equals the total number of targets. Second, we examine the decline in both the number of acquirers and the number of targets from election states.

A numerical example provides intuition for our interpretation. In our example, say there are only two states, Texas and Louisiana. These states do not hold gubernatorial elections in the same cycle. If there is no election in either state, there are 10 acquirers headquartered in each state and 10 targets headquartered in Texas. This means there must be 10 targets in Louisiana because there must be 20 targets in total. In the following year, there is an election in Louisiana. Consistent with our empirical results, there are now fewer acquirers in Louisiana relative to Texas, 9 versus 10. We know the total number of targets must be 19, but how will the targets be distributed between the states? The exact distribution depends on whether the lost deal was a same-state or cross-state deal. There are two possible scenarios that result in fewer election-state targets, which is consistent with our empirical results. First, the canceled deal could have been a same-state deal. This would result in 9 acquirers and targets in Louisiana and 10 acquirers and targets in Texas. Alternatively, the canceled deal could have been a cross-state deal and a Texas acquirer can switch from a Louisiana to a Texas target. If the canceled deal was a cross-state deal and no Texas acquirer switch occurs, there would be 9 targets in Texas and 10 in Louisiana, contradicting our empirical results. This reasoning applies to our empirical tests with all election states represented as Louisiana and

all nonelection states represented as Texas. Because we find a decrease in acquirer and target activity in election states, even when our sample is limited to same-state and cross-state deals, we can interpret our results in Figure 2 as evidence of a geographic shift in target selection.

We further support this conclusion with one additional set of tests we leave in Figure IA.3 in the Supplementary Material. In these tests, we go one step further and limit our sample to targets with an acquirer headquartered in a nonelection state and acquirers with a target headquartered in a nonelection state. Specifying the test in this way decreases the number of deals in our sample, but provides the cleanest look at whether uncertainty from both the target state and the acquirer state is important to the merger decision and the geographic shift in target selection as a result of political uncertainty.

Using this setup, we find that the decline in acquirer announcements is strong and significant, starting 3 quarters before the election and lasting up to a year after the election. These results suggest that political uncertainty related to elections causes a large, prolonged decline in acquirer announcements, separate from any effects related to elections in the state of the target. Additionally, we find that the effect of political uncertainty caused by elections in the state of headquarters of the target is strong when there is no election in the acquirer state. Again, we see a decline in activity starting as early as 3 quarters before the election and lasting to the quarter of the election. Taken together, results from Figures 2 and IA.3 in the Supplementary Material show that elections in the states of potential targets have economically significant effects on acquirers' target selection.

C. Does Political Uncertainty Affect Aggregate Deal Value?

In Figure 3, we test whether political uncertainty causes firms to decrease the total value of deals. The dependent variable for these tests is the natural logarithm of 1 plus the sum of deal value in each state-quarter. In tests with this dependent variable, we use a heterogeneous Tobit model, reflecting the continuous nature of the variable, variation in values across states, and that a sizable portion of the data are zero observations.[16]

Graph A of Figure 3 shows that political uncertainty does not significantly affect the total dollar value of deals by acquirers. Discrete effects for differences between value of deals announced by election-state acquirers and nonelection-state acquirers are very close to zero 4–6 quarters before and 2–4 quarters after elections. These estimates are generally negative in the quarters surrounding elections, but do not differ significantly from 0 at the 95% level. Taken together, these results show limited evidence that political uncertainty causes lower aggregate deal value for election-state acquirers.

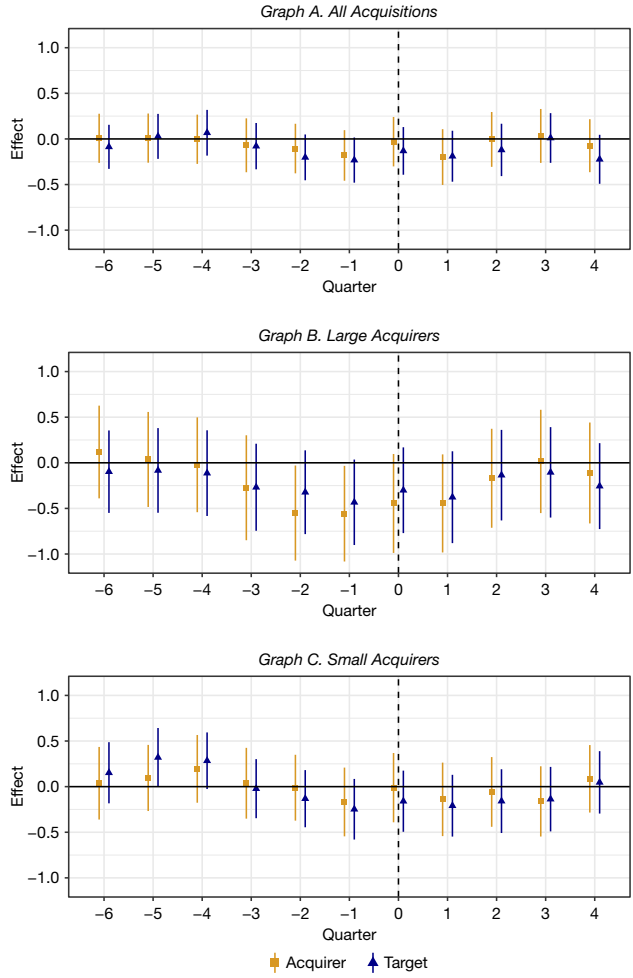
We find weak evidence that uncertainty causes a reduction in aggregate deal value for targets. Like the acquirer results, discrete effects for targets in Graph A of Figure 3 are close to zero 4–6 quarters before and 2–4 quarters following elections. In the quarters surrounding elections, we find negative discrete effects, only one of

[16]See Messner, Mayr, and Zeileis (2016). We find similar coefficient estimates using OLS, a regular Tobit, and a heterogeneous Tobit. We prefer the heterogeneous Tobit because the standard errors are the most appropriate for our data.

FIGURE 3

Effect of Political Uncertainty on State-Quarter Total Deal Value

Figure 3 plots discrete effects (i.e., the difference in total deal value from the baseline level predicted by a heterogeneous Tobit model) calculated using estimates from state-quarter acquirer and target observations for all acquisitions (Graph A), acquisitions made by large acquirers (Graph B), and acquisitions made by small acquirers (Graph C). The dependent variable is the natural logarithm of 1 plus the sum of deal value in the state-quarter. Coefficient estimates for Graph A are presented in Table IA.5 and for Graphs B and C in Table IA.6 in the Supplementary Material.



which is marginally significant at the 10% level. These estimates suggest that acquirers only slightly decrease the total deal value for targets headquartered in election states, relative to targets headquartered in nonelection states.

D. Deal Value Results in Subsamples

In this section, we examine deal value results in an important set of subsamples, small and large acquirers, and show evidence that political uncertainty affects aggregate state-level deal value in subsamples. In the previous section, we

find weak evidence that deal value in our aggregate sample changes around elections. However, we expect significant heterogeneity in our results driven by deals' sensitivity to political uncertainty. Consistent with this expectation, we find a pre-election boost in aggregate deal value by small acquirers and no such increase by large acquirers. Additionally, large acquirers decrease deal value around elections, whereas small acquirers do not. The timing of results in these subsamples offset, so there is limited evidence in the aggregate sample that political uncertainty affects deal value.

In Graph B of Figure 3, we examine how political uncertainty affects aggregate deal value by large acquirers. Large acquirers significantly reduce the total value of their election-year deals for several quarters before elections. Additionally, these acquirers reduce the total value of their election-year targets for 4 quarters around the elections. The target estimates in this graph are not statistically significant from 0 at the 95% level. However, the magnitude of the target estimates is economically significant and the standard errors are larger than for estimates in Graph A of Figure 3, reflecting the more limited sample size.

In contrast, in Graph C of Figure 3, we find weaker evidence that small acquirers reduce election-year deal size. However, we do see an increase in aggregate deal value for election-year targets, relative to nonelection-year targets, for small acquirers. We do not see a similar boost in the value of deals by large acquirers in Graph B of Figure 3.

Large and small acquirers each comprise one-half of our sample, so it is easy to see how significant results in Graphs B and C of Figure 3 combine to create null results in Graph A. Small acquirers increase aggregate deal value, whereas large acquirers do not, and then large acquirers decrease aggregate deal value, whereas small acquirers do not. Because political uncertainty causes these subsamples to adjust their merger activity disparately, these subsample results combine with the relatively weak aggregate sample evidence we see in Graph A. However, the results across all the 3 graphs suggests that there is some evidence that political uncertainty causes pre-election increase and subsequent decrease in aggregate deal value, but that this evidence is weaker than that we see for aggregate deal counts or geographic shifting of target selection.

The tests in Graphs B and C of Figure 3 suggest that there is significant heterogeneity in how political uncertainty affects merger activity. These results may seem puzzling because related literature shows that small firms are relatively more sensitive to political uncertainty created by gubernatorial elections. Jens (2017), for example, shows that small firms are more likely to be geographically concentrated within the state of headquarters and thus are more sensitive to political uncertainty from state elections. Accordingly, we would expect stronger effects for small acquirers than large acquirers. However, Figure 3 shows that large firms decrease total deal size more than small firms around elections, and that movements in aggregate small acquirer deal value are, in comparison, fairly modest.

There are two explanations for these possibly counterintuitive results, which are not mutually exclusive. First, there can be differences between how small and large firms alter election-year activity; large acquirers may be more likely to reduce deal size, whereas small acquirers may be more likely to forgo deals. The results in

TABLE 3

Summary Statistics for Subsamples

Table 3 presents summary statistics for subsamples of interest. Panel A reports that the probability of an acquirer is large (above median assets), has an S&P credit rating, or is a serial acquirer (five or more acquisitions in the previous 3 years) based on its attributes. Panel B presents deal characteristics for each of the subsamples in the 3 quarters surrounding the election (ELECTION) and in the calendar year 2 years before an election (NONELECTION). DEAL_SIZE is the mean dollar amount of the acquisition, DEAL_COUNT is the mean number of acquisitions for a state/quarter for acquirers in the subsample, SAME_STATE is the percentage of deals in which the acquirer and the target are headquartered in the same state, ALL_STOCK is the percentage of deals funded entirely using common stock, ALL_CASH is the percentage of deals funded entirely using cash, and STOCK_USED is the mean dollar amount of stock used in deals scaled by deal size presented as a percentage. Statistics for the large and small acquirer subsamples are computed for the 20,448 deals for which the acquirer's data are available in Compustat, whereas all other statistics are computed for the full sample of 36,685 deals.

Panel A. Probability of Attribute Given an Attribute

Attribute	*P*(Large|Attribute)	*P*(Rated|Attribute)	*P*(Serial|Attribute)
Large	100.00	50.03	21.75
Small	0.00	6.35	12.06
Rated	88.75	100.00	21.34
Unrated	34.83	0.00	12.72
Serial	64.36	23.89	100.00
Non-serial	47.12	14.43	0.00

Panel B. Election Versus Nonelection Deal Characteristics by Attribute

	Large		Small	
Variable	Election	Nonelection	Election	Nonelection
DEAL_SIZE ($)	804.15	838.84	48.83	65.01
SAME_STATE (%)	51.81	53.99	48.19	46.01
ALL_STOCK (%)	19.97	23.82	18.91	25.19
ALL_CASH (%)	30.03	28.51	20.27	17.37
STOCK_USED (%)	28.20	32.19	28.91	35.24
	Rated		Unrated	
DEAL_SIZE ($)	1152.12	1095.91	309.86	339.91
SAME_STATE (%)	11.40	11.33	88.60	88.67
ALL_STOCK (%)	17.93	22.84	21.74	24.97
ALL_CASH (%)	32.12	28.88	24.09	22.29
STOCK_USED (%)	25.43	30.19	29.72	33.19
	Serial		Non-Serial	
DEAL_SIZE ($)	291.58	525.41	469.71	448.37
SAME_STATE (%)	11.03	9.65	88.97	90.35
ALL_STOCK (%)	23.08	31.10	20.81	23.69
ALL_CASH (%)	21.33	17.85	26.04	24.12
STOCK_USED (%)	29.38	37.42	28.98	32.03

Figure 3 only capture one way in which acquirers adjust election-year deals, and so do not provide a full look at large versus small acquirer effects.

Second, these results may reflect our choice of a DD model. Our model compares levels of merger activity across states. Table 3 provides summary statistics for merger activity by small and large acquirers. Table 3 reports that the average nonelection-year deal (in millions) for large acquirers is almost 13 times the size of the average nonelection-year deal for small acquirers. Both samples decrease the average size of their election-year deals relative to their nonelection year deals; the average size of deals by large acquirers decreases from $838.84 to $804.15, a reduction of approximately 4.1%, and the average size of deals by small acquirers decreases from $65.01 to $48.83, a reduction of approximately 24.9%. However, despite having a larger percentage decrease, the *level* of average small-acquirer deal value only decreases by $16.18, whereas the level of average large-acquirer deal value decreases by $34.69. Our DD model calculates differences in levels and so measures larger level changes as more significant.

Differences between subsamples in Figure 3 may not be driven by differences in sensitivity to political uncertainty, but are consistent with acquirers' relative costs of capital being an important source of heterogeneity. Table 3 reports that over 50% of large acquirers but only 6.35% of small acquirers have bond ratings. Small acquirers' sources of funding for deals are thus more limited than those of large acquirers. This difference between these acquirers shows up in summary statistics for deal payment type in Panel B. Approximately 25% and 24% of deals by small and large acquirers are financed using all stock. However, 17.37% of small-acquirer deals and 28.51% of large-acquirer deals are in all cash, so large-acquirer deals are 64% more likely than small-acquirer deals to be financed using all cash. Because small acquirers rely more on more expensive equity financing than large acquirers, they should be more sensitive to financing costs. Thus, we expect that small acquirers are more likely to shift their acquisition activity backward from elections to avoid the period of increased cost of capital around elections.

The evidence in Graphs B and C of Figure 3 is consistent with this expectation. Small acquirers increase pre-election aggregate deal value, whereas large acquirers do not. In contrast, changes to election-year aggregate deal value in large acquirers are more consistent with evidence that Bonaime et al. (2017) show and argue is driven by real options considerations. In the next section, we provide additional evidence demonstrating the importance of acquirer costs of capital and deal payment type as sources of heterogeneity in our results.

E. Acquirer Financing Costs and Deal Payment-Type Matter

In this section, we provide empirical evidence that changes to election-year merger activity are stronger for deals relatively more sensitive to higher costs of capital. These deals include those involving acquirers with higher costs of capital and deals with relatively higher proportions of equity financing. These sources of heterogeneity are correlated and complex. Examining the interplay between these sources of heterogeneity is important to understanding how political uncertainty affects merger activity in the aggregate and across subsamples.

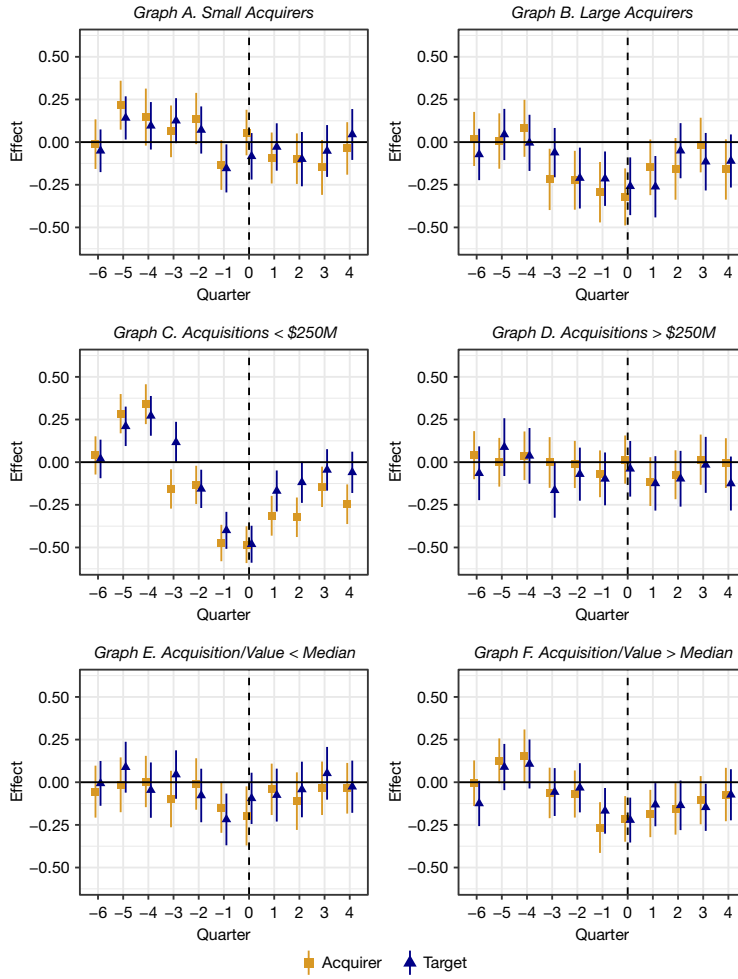
In Figure 4, the dependent variable is state-quarter deal count. In Graphs A and B, we show evidence that both large and small acquirers decrease the number of election-year deals. Small acquirers show a greater pre-election boost in acquirer and target announcements, whereas large acquirers show a larger drop in activity in the quarters surrounding the election. These results echo our earlier results in Figure 3 in which small firms are more likely than large firms to increase deal value before elections, whereas large firms are more likely to reduce deal value around elections. Thus, both small and large acquirers adjust election-year deal counts, but do so at different times throughout the year.

In Graphs C and D of Figure 4, we find that larger acquisitions by value relative to acquirer size are more affected by political uncertainty. The relationship between raw deal size, acquirer size, and relative deal size is complex, so, in the Supplementary Material, we use regressions to examine correlations between these variables and the likelihood of all-stock financing for a deal (Table IA.16 in the Supplementary Material). While controlling for acquirer size, we find that both deal size and relative deal size increase the probability of all-stock financing. Thus, these

FIGURE 4

Effect of Political Uncertainty on State-Quarter Deal Totals for Size Subsamples

Figure 4 plots discrete effects (i.e., the difference in acquisitions from the baseline level predicted by the hurdle model) calculated using estimates from state-quarter acquirer and target observations. Graphs A and B present results for deals with value less than and greater than $250 million (in 2016 dollars), Graphs C and D present results for deals with deal size relative to acquirer market value of equity less than or greater than the sample median, and Graphs E and F present results for deals made by small (below median assets) and large (above median assets) acquirers. Coefficient estimates for Graphs A and B are presented in Table IA.7, for Graphs C and D in Table IA.8, and for Graphs E and F in Table IA.9 in the Supplementary Material.



results support our conclusions that deals more sensitive to cost of capital changes around elections (including deals more likely to be paid for with stock and by acquirers with relatively more expensive sources of capital) are more likely to be shifted backward in time away from elections.

One possible conclusion from the results in Figure 4 is that our results are driven solely by deal payment type, rather than partly by deal payment type *and* acquirer size or characteristics correlated with acquirer size, like financing constraints. To test whether payment type drives our results, in Figure 5, we examine

how political uncertainty affects deal counts for all-stock and all-cash deals in Graphs A and B, respectively. We find that there are relatively more all-stock acquisitions for acquirers and targets headquartered in election states relative to nonelection-state activity approximately 1 year before elections. Additionally, there are relatively fewer announcements for all-stock deals in the quarters around elections. In contrast, there are no economic or statistically significant differences between the number of announcements for all-cash deals in election states relative to nonelection states.

In Figure 6, we examine whether political uncertainty affects the aggregate deal value of all-stock (Graph A) and all-cash (Graph B) deals. In these tests, we find that there is little fluctuation in the aggregate deal value of all-stock and all-cash deals in election years. These results are important because they suggest that differences between large and small acquirer results are *not* just driven by

FIGURE 5

Effect of Political Uncertainty on State-Quarter All-Stock and All-Cash Deal Totals

Figure 5 plots discrete effects (i.e., the difference in acquisitions from the baseline level predicted by the hurdle model) calculated using estimates from state-quarter acquirer and target observations for all stock deals (Graph A) and all cash deals (Graph B). Coefficient estimates for the graphs are presented in Table IA.12 in the Supplementary Material.

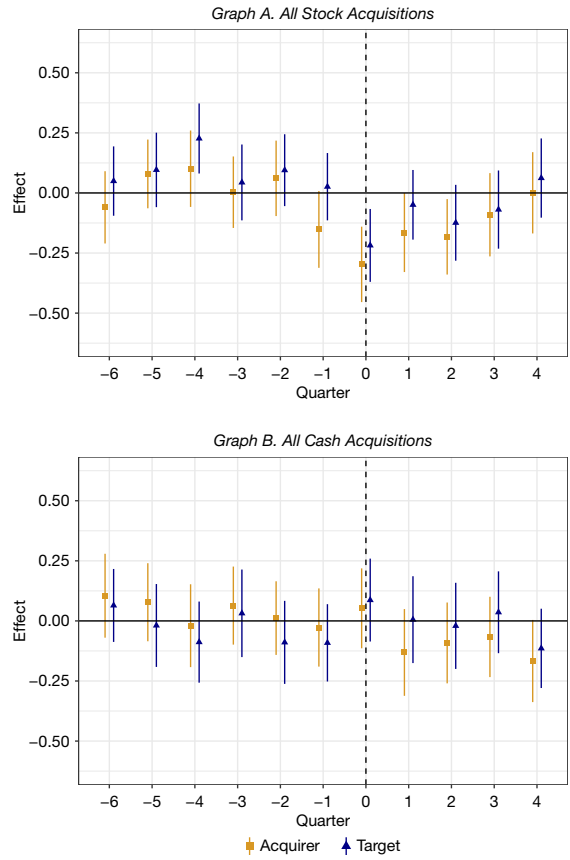
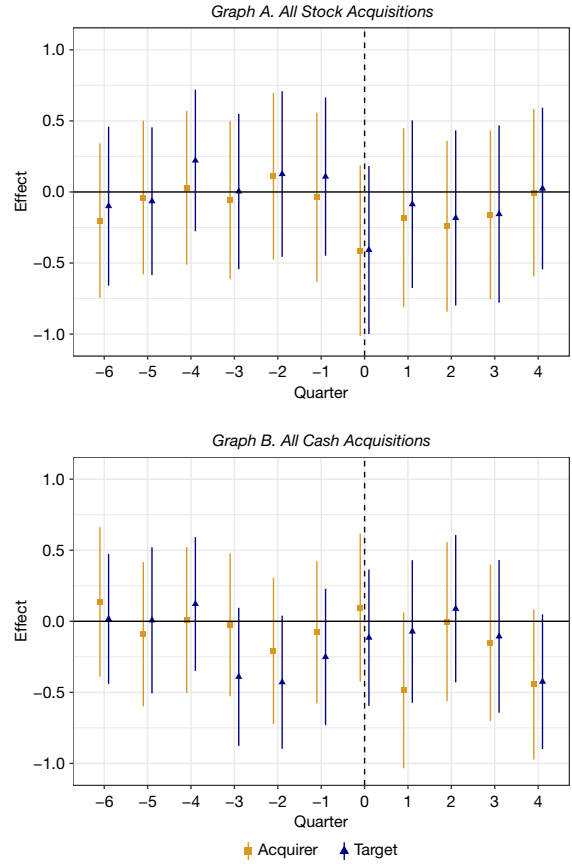


FIGURE 6

Effect of Political Uncertainty on State-Quarter All-Stock and All-Cash Total Deal Value

Figure 6 plots discrete effects (i.e., the difference in total deal value from the baseline level predicted by a heterogeneous Tobit model) calculated using estimates from state-quarter acquirer and target observations for all stock deals (Graph A) and all cash deals (Graph B). The dependent variable is the natural logarithm of 1 plus the sum of deal value in the state-quarter.



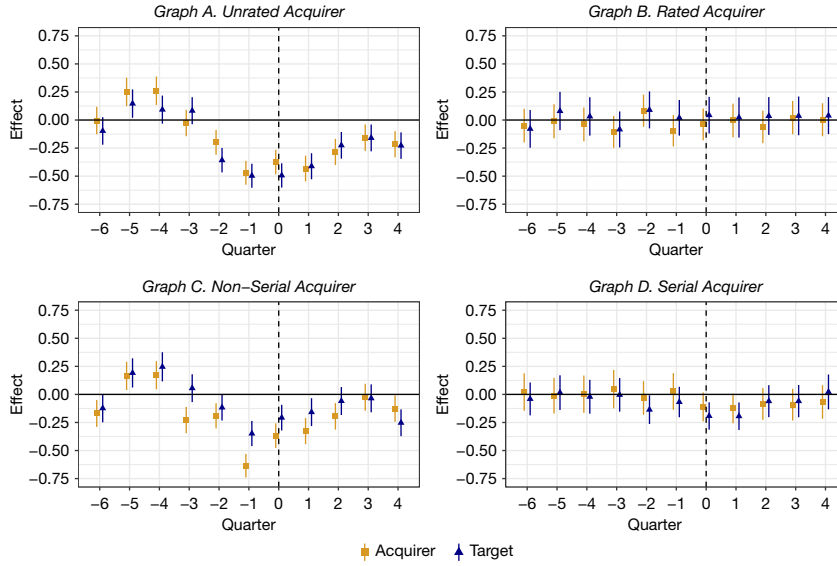
differences in deal payment type. If the results were driven solely by payment type, we would see significant fluctuation in the value of all-stock deals around elections like the fluctuation we see in the count of all-stock deals around elections. However, the results in Figure 6 are similar to those in Graph A of Figure 3. Thus, results in Figure 3 likely reflect the same heterogeneity across subsamples as we document in Figure 4. This heterogeneity suggests that how acquirers disparately adjust election-year merger activity is likely driven by multiple correlated characteristics (which can include acquirer size), not just deal payment type.

Having explored acquirer size as a source of heterogeneity in how political uncertainty affects merger activity, we now use a more direct measure of firm financial constraints and split our sample according to whether an acquirer has an S&P bond rating. If differences between large and small acquirer results are

FIGURE 7

Effect of Political Uncertainty on State-Quarter Deal Totals for Subsamples

Figure 7 plots discrete effects (i.e., the difference in acquisitions from the baseline level predicted by the hurdle model) calculated using estimates from state-quarter acquirer and target observations. Graphs A and B present results for deals in which the acquirer either has or does not have an S&P credit rating, and Graphs C and D present results for deals by serial and non-serial acquirers. Coefficient estimates for Graphs A and B are presented in Table IA.10 and for Graphs C and D in Table IA.11 in the Supplementary Material.



driven, in part, by differences in financial constraints, we should see stronger differences between effects when comparing rated versus unrated acquirers than those in earlier tests examining large versus small acquirers.

In Graphs A and B of Figure 7, we examine merger activity by rated and unrated acquirers. As expected, differences in results between these samples are stark. We see a greater upswing in pre-election announcements for unrated acquirers than rated acquirers. Additionally, we find a large decline in announcements by unrated acquirers for 7 quarters around elections. Unrated acquirers announce, on average, 5.02 deals per state-quarter, so discrete effect estimates of approximately −0.50 suggest a 10% loss in announcements per quarter around elections. These results constitute the largest forgone election-year acquisition activity we find in any subsample and support our earlier conclusions regarding the importance of acquirer cost of capital to the effects of political uncertainty on merger activity.

We provide one final piece of evidence that heterogeneity in our results is not solely driven by acquirer cost of capital or deal payment type, but rather by an interplay between the two. In Graphs C and D of Figure 7, we examine differences between how political uncertainty affects serial and non-serial acquirers. Serial acquirers engage in at least five deals in a 3-year period in our sample (Fuller et al. (2002)). These subsamples are particularly interesting to look at because nonelection-year acquisitions are most likely of any subsample of deals

to be paid for wholly in stock (Table 3); almost one-third of deals by serial acquirers are all-stock deals. Based on this statistic, we would expect deals by serial acquirers to be sensitive to higher costs of capital around elections. However, serial acquirers are also more likely to have S&P ratings than non-serial acquirers (23.89% vs. 14.43%, respectively). Thus, deals by serial acquirers provide an opportunity to look at a sample in which acquirer cost of capital and deal payment type are less strongly correlated.

In Graphs C and D of Figure 7, we find that deals by non-serial acquirers are more affected by political uncertainty than deals by serial acquirers. These results confirm that payment type is only one source of sensitivity of deals to political uncertainty. If payment type were the only source of heterogeneity in our results, announcements by serial acquirers would be relatively more affected than announcements by non-serial acquirers. However, these results are more consistent with heterogeneity from acquirer cost of capital, proxied by whether an acquirer has a bond rating.

The results in Graphs C and D of Figure 7 and summary statistics in Table 3 suggest that serial acquirers do not significantly decrease election-year merger activity, but do shift their payment type away from equity to cash financing. Table 3 reports that 31.1% of nonelection year deals and 23.08% of election year deals by serial acquirers are all-stock, which constitutes almost a 26% decline. Additionally, the proportion of stock used increases from 29.38% in election to 37.42% in nonelection year deals by serial acquirers, respectively, which constitutes over a 27% increase. To bolster these univariate statistics, we estimate a series of regressions in Table IA.7 in the Supplementary Material, in which we report that serial acquirers shift toward using equity financing in years before elections and toward cash financing (away from equity financing) around elections. These results comprise our strongest evidence that the interplay between deal payment type and acquirer financing constraints is an important source of heterogeneity in our results; serial acquirers do not reduce the number of election-year announcements for deals that, on average, tend to be financed more heavily with equity but shift payment toward cash away from equity. Such a shift would be unlikely or impossible for firms with tighter financing constraints.

F. Robustness

1. Alternate Measures of Uncertainty

We confirm that our results are stronger using a binary measure of election closeness as our treatment variable. To test whether closer, more uncertain elections affect merger activity, we limit our sample to election-year observations. We then re-estimate our model replacing ELECTION with CLOSE, a binary variable equal to 1 if an election had a vote differential (the percentage of votes received by the first place candidate minus the percentage of votes received by the second place candidate), in the bottom tercile of the sample. Intuitively, the estimation is then capturing the effect of close elections on merger activity, above any effects already presented in this article. In Figure IA.4 in the Supplementary Material, we show that the pre-election boost in merger activity begins several quarters earlier before close elections, relative to non-close elections. Additionally, lower merger activity persists

for one additional quarter following close elections. These results support the interpretation that the decline in merger activity around elections is driven by increased political uncertainty rather than election cycles.

2. Public and Private Acquirers and Targets

In Figure IA.5 in the Supplementary Material, we re-estimate our main results from Table 2 on public and private acquirers and targets separately. Our strategy to address endogeneity inherent in studying M&As requires comparing firms that have self-selected into the same samples. However, summary statistics show large differences between public and private acquirers and public and private targets. We examine these four groups separately to ensure that our results are not driven by differences between these samples (self-selection bias). In the figure, we find that political uncertainty affects announcements for public and private targets and acquirers.

The results in Figure IA.5 in the Supplementary Material also provide an interesting contrast to results Bhagwat et al. (2016) present. Bhagwat et al. (2016) show that a decline in aggregate U.S. merger activity related to increases in the VIX index is limited to deals for public targets only. This is the second major difference between results in this study and the results in Bhagwat et al. (2016) that we document. We also find results strongest in deals for relatively smaller targets, whereas results in Bhagwat et al. (2016) are strongest in deals for large targets. Differences between our results and those in Bhagwat et al. (2016), along with our results exploring heterogeneity in how political uncertainty affects merger activity, suggest that different types of uncertainty disparately affect merger activity and more research directly comparing the effects of uncertainty proxies is warranted.

3. Alternate Variable and Sample Constructions

Our results are also robust to alternate variable and sample constructions and empirical specifications. We consider alternative definitions for our subsamples. We choose $250 and $75 million to define small and large deals for comparability with Bhagwat et al. (2016), who also use those cutoffs. We ensure our results are not sensitive to the selection of other cutoffs, including median and quintiles. Any cut of small and large deals results in the same conclusions. We present results defining financial constraints using bond ratings in this article because bond ratings are arguably the least endogenous measure of financial constraints (Riddick and Whited (2009)). However, we find similar results using more endogenous measures, including above or below median average and standard deviation of operating cash flows to define financial constraints, which is in the spirit of the AR(1) results from Riddick and Whited (2009). Our serial and non-serial acquirer results are robust to other definitions of serial and non-serial acquirers from Fuller et al. (2002). In sum, our subsample results are robust to alternate definitions.

Additionally, we ensure that our results are robust to the inclusion of measures of state-level economic conditions. In Figure IA.6 in the Supplementary Material, we add to our estimation state unemployment and change in state GDP.[17] These data

[17]Monthly state unemployment data are from the Bureau of Labor Statistics. Annual state GDP levels are available from the Bureau of Economic Analysis.

are available in a consistent time series through 2013, so their inclusion limits our sample. Regardless, results from these tests are consistent with our main results.

VI. Conclusion

We study how firms alter investment projects to mitigate exposure to political uncertainty. We examine merger deal-level data, which represent individual projects, and find that firms i) shift merger announcements earlier in time to avoid the period between announcement and effective dates overlapping an election, ii) shift targets geographically away from states in which elections are being held, iii) decrease the size of election-year deals, and iv) in some limited samples, shift from equity to cash financing for election-year deals. Our results comprise new evidence of how political uncertainty affects firm investment beyond causing firms to delay or forego investment.

Our empirical setting is key to our study. In order to ask whether political uncertainty affecting a project can cause a firm to select another project, we must be able to disentangle political uncertainty affecting firms and projects. We focus on M&As and exploit the geographic variation in the states of headquarters of acquirers and targets. Our measure of political uncertainty, gubernatorial elections, is state-level and exogenous, allowing us to examine the effects of political uncertainty on election-state targets headquartered when there is no election in the acquirer state and vice versa. Our question is unanswerable without such cross-sectional variation in uncertainty.

While measuring political uncertainty with gubernatorial elections provides us with crucial cross-sectional variation, our estimates of the effects of political uncertainty on firm investment are likely conservative. Gubernatorial elections provide clean identification, but measure temporary uncertainty in a relatively politically stable country. Yet, we find that firms, particularly those that do not habitually engage in acquisitions, shift their target selection geographically to avoid uncertainty. These results suggest even higher costs of political uncertainty to cities, states, and countries with relatively higher levels of uncertainty than have previously been documented.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109022000904.

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