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Abstract

This article examines whether elections for state offices that regulate mortgage lenders affect mortgage markets. Some scholars assert that election-related political uncertainty depresses economic activity; others contend that incumbents pursue policies to boost short-term growth prior to elections; and a third group claims that market activity fluctuates around partisan transitions. We test these theories using national data on mortgage characteristics and election data for two important state regulators. We first conduct event studies comparing mortgage market outcomes before and after elections. We then utilize difference-in-difference models to compare states in which partisan control of key offices switched following an election. Our results do not show consistent support for any of these theories. We find that elections have few significant effects on mortgage markets, suggesting that delegating regulatory power to elected state officials may be efficient.

Keywords: Political economy; Law and economics; Mortgage; Household finance; Political uncertainty;

Political business cycle; State law; Federalism

JEL Classification: D14, D18, D72, G21, K22, P16

This study probes the relationship between elections and mortgage-market activity. State elected officials and their appointees play a prominent role in regulating residential mortgage lenders—giving state office-holders seeking to make their mark on mortgage finance no shortage of levers to pull.[1] They are the primary regulators of state-chartered lenders; enforce a host of state laws against nationally chartered lenders;[2] are authorized to enforce federal lending laws against all lenders; and are the *only* regulator of non-bank lenders that originate over 50 percent of all residential mortgages.

These state officials may face pressure to adopt an electorally advantageous regulatory posture. Lenders, in turn, may react to these official positions, as well as uncertainty over which candidate, and thus which regulatory stance, will prevail in an upcoming election. In this way, election-related uncertainty and partisan swings in policy could raise compliance costs, discourage lenders from originating loans that would be economically viable in a stable regulatory environment, and thus slow the economy (Agarwal, Lucca, Seru, & Trebbi 2013).

A substantial literature in political economy and related fields states that political officeholders' electoral incentives can distort financial markets and lessen the efficacy of regulation. These officials, directly or through their appointees, may vary their oversight of financial institutions based on their party's ideological posture (Mian, Sufi, & Trebbi 2010). Growing partisan polarization at the state level (Caughey, Warshaw, & Xu 2017) may amplify this variance. State officials also may pursue policies to bolster economic growth leading up to their reelection campaigns, taking their foot off the gas afterwards (Nordhaus 1975). Further, greater political uncertainty may generate contractions in economic activity prior to elections, including in high risk mortgage lending (Kara & Yook 2019).

Drawing on this literature, we present a taxonomy of theories predicting that electoral pressures generate distortions in financial markets. We then test each theory using detailed data on mortgage origination and performance in the U.S. between 2004 and 2013. The first theory, the *Political Uncertainty Account*, holds that political uncertainty—for instance, in the lead-up to an election—dampens firms' investment behavior (see, e.g., Pastor & Veronesi 2012). The *Political Business Cycle Account*, by contrast, points to officials' incentives to use the policy levers at their disposal to facilitate short-term economic growth in the lead-up to their next election (Nordhaus 1975). Finally, the *Partisan Differences Account* posits that the different policies that Democratic versus Republican officeholders pursue generates differences in firm-level or macroeconomic

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[1]A complex patchwork of federal and state agencies regulate and, in many cases, supervise lenders based on their legal form. A second set of federal and state agencies police lenders' consumer-facing activities. Should a borrower default, a third set of government actors, this time at the state level, oversees the foreclosure process (Feinstein, 2018).

[2]These laws concern predatory lending, unfair and deceptive practices, information disclosure, and mortgage servicing.

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variables around electoral transitions (Alesina 1987).

Mortgage markets provide an ideal test case for these theories. That they are regulated to a significant extent by state officials allows for state-level analysis, which provides greater statistical power than analyses that focus on quadrennial presidential elections. Further, virtually all residential mortgages are secured by a property located in a single state, meaning that this study better captures only in-state political effects relative to studies that examine the effects of in-jurisdiction elections on economic or securities-market activities that span multiple states or countries. Finally, the rich data on multiple mortgage features employed here allows for the running of multiple models as a validity check.

Accordingly, to assess these theories we examine changes in mortgage lenders' behavior around state elections for governor and attorney general ("AG"). These two positions play important—and, for AGs, overlooked—roles in regulating mortgage markets. Our analysis does not provide consistent support for any of the predominant theories of electoral politics and financial markets. Virtually all of our tests yield null results. The few significant political impacts we observe in our data do not accord with existing theories of electoral politics. Our results suggest that state-level political pressures do not generate significant distortions and inefficiencies in the regulation and function of mortgage markets. Accordingly, it may be possible to provide state officials with greater authority to regulate the growing "shadow bank sector" without generating substantial politically-induced market distortions.

This article proceeds as follows. Section I provides an overview of the major theories concerning how elections affect economic activity. Section II describes the outsized role that state governors and AGs play in regulating their states' mortgage markets, thus showing the particular suitability of their elections in assessing these theories. Section III develops hypotheses based on these theories and details our research design. Sections IV and V present the results and highlight several implications.

1   Theories of Political Influence

This section presents three theories concerning the impact of elections on mortgage markets: the Political Uncertainty, Political Business Cycle, and Partisan Differences accounts.[3] The section describes each

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[3]A fourth perspective holds that elections affect economic outcomes through cronyism. In this telling, firms or industries that are well-connected to newly elected officials prosper and those connected to the old regime suffer. This fourth theory is beyond the scope of this paper. Nonetheless, to the extent that plaintiff-side consumer-protection lawyers are major donors to Democratic AGs (Zambrano 2018) and business interests are key contributors to Republican AGs (Bennett 2018), the notion that Democratic or Republican officials could adopt, respectively, a strong or weak regulatory enforcement posture to satisfy their donors seems plausible.

account's theoretical underpinnings and previous empirical tests.

1.1 Political Uncertainty

The notion that uncertainty generates an investment cycle is well-established in economic theory (Bloom, Bond, & Van Reenen 2007; Dixit & Pindyck 1994; Bernanke 1983). In brief, when deciding when to time irreversible investment decisions, firms weigh the prospect of generating additional revenue by investing early against the possibility of learning valuable new information by waiting. As uncertainty regarding the long-run implications of a future event increases, so does a firm's option value of deferring investment decisions. As a consequence, investment levels fluctuate cyclically, as firms hold back on investing prior to significant events (Bernanke 1983).

The periods preceding elections generate one prominent form of uncertainty. Because officeholders adopt different priorities bearing on firms' bottom lines, uncertainty regarding the winner of a future election may dampen current economic activity (Julio & Yook 2012). Indeed, a large empirical literature generally confirms this theory concerning asset prices and firm investment decisions (for a review of this literature, see Goodell & Vahamma 2013). This literature tends to focus on presidential elections. Nonetheless, several studies examine state-level political uncertainty; gubernatorial elections are inversely related to corporate investment by firms headquartered in that state (Jens 2013), IPOs by in-state firms (Colak et al. 2017), and in-state municipal bond issuance (Gao & Qi 2013). On the other side, Waisman et al. (2015) find no connection between gubernatorial elections and the cost of corporate debt for in-state firms.

A second constellation of studies demonstrates how economic policy uncertainty—from sources other than upcoming state-level elections—adversely affects the housing market. Policy uncertainty is inversely related to loan supply (Bordo, Duca, and Koch 2016) and housing prices (Bahmani-Oskooee 2017; Antonakakis et al. 2016). Industry groups advance similar claims. For instance, a think tank affiliated with the U.S. Chamber of Commerce asserts that "uncertainty over continued litigation" by political officeholders has resulted in "Americans no longer hav[ing] access to the necessary capital to buy a home" (Pincus 2014). Following the financial crisis, bankers criticized the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 ("Dodd-Frank") for granting rule-writers and regulatory enforcers wide discretion, thus contributing to policy uncertainty and depressing lending activity (Bordo, Duca, & Koch 2016).

Within these literatures, Kara & Yook (2020) and Canes-Wrone & Park (2014) are the studies closest to

ours. The former finds that banks reduce the number and value of mortgages, in various states, that they originate or hold on their balance sheets prior to gubernatorial elections in the state in which the banks are headquartered. The latter reports that home sales and the sale prices for those transacted homes decline in the lead-up to in-state gubernatorial elections.

Our test of the Political Uncertainty Account builds on these studies in several respects. First, we analyze the effects of political uncertainty on a larger set of mortgage-market features. Because state officials' powers over mortgage lenders focus largely on permissible loan terms and foreclosure procedures, we examine the number of mortgages with high-risk features and the proportion of loans that later result in default or foreclosure in addition to the number of loans originated and the corresponding home values.

Second, we examine AG elections as well as gubernatorial elections. As detailed below, AGs play a crucial role in enforcing mortgage-finance laws at the state level. With Dodd-Frank's empowerment of state AGs to enforce a broad set of federal laws, the Chamber of Commerce argues that AGs "can add significant uncertainty and costs to a business community that needs predictability and reliability" (Pincus 2014).

Third, and relatedly, including AGs also may enable us to disentangle effects that are grounded in borrowers' versus lenders' political uncertainty. An unstated assumption of much of the literature concerning presidential elections is that firms' uncertainty around these elections reduces economic activity. But some consumers presumably also have beliefs regarding the effect of presidential transitions on the macroeconomy or their own economic prospects. Most studies that find economic effects of policy uncertainty around presidential elections generally do not weigh in on whether these effects are driven by supply- versus demand-side uncertainty.

Our research design, by contrast, may provide some purchase on this question. When deciding whether to take out a mortgage to purchase a home, individuals face the same largely irreversible investment decision and uncertain future economic conditions as do lenders. These future conditions hinge in part on the outcomes of upcoming state gubernatorial and, as detailed below, especially AG elections. Yet borrowers' information deficit concerning the likely impact of various possible election outcomes on mortgage markets likely is far greater than lenders'. Put bluntly, AG elections are simply too obscure for most voters to follow. Consider that only one-third of survey respondents can recall their AG's name—to say nothing of his or her views and priorities (Delli Carpini & Keeter 1992). Assuming that borrowers are mostly unaware of potential changes in their state AG's enforcement posture following an election, one can infer that changes

in mortgage markets around an AG election are largely attributable to shifts in lenders' behavior rather than that of borrowers.

1.2 The Political Business Cycle

The Political Business Cycle Account offers an alternative perspective regarding the influence of politics on markets. Nordhaus (1975) posits that incumbent politicians manipulate fiscal and, to the extent possible, monetary policy prior to an election to reduce unemployment, thus boosting their reelection prospects. After they are reelected, they enact austerity measures to address the inflation that their pre-election expansionary policies produced—thus causing unemployment to increase. In this way, unemployment falls then rises, and inflation rises then falls, on either side of an election.

Moving beyond the Phillips curve, incumbents also may opportunistically pursue policies that boost market returns prior to an election—with a course correction after Election Day (see, e.g., Lewis-Beck, 1988). Myriad studies have tested this theory, mostly using policy instruments like tax and transfer programs, and mostly yielding confirmatory results (Dubois, 2016).

Although the literature on the political business cycle tends to focus on national leaders, the same logic applies to governors. According to Peltzman (1987), governors' powers are comparable to those of "an executive in a small open economy without a central bank." Although only a limited literature exists regarding political business cycles at the state level, the existing evidence is equivocal. Chang, Kim, & Ying (2009) find statistically significant changes in spending around gubernatorial elections, but Nelson (2000) finds no difference in tax rates for before and after state elections. Our results contribute to this literature by narrowly and comprehensively studying the effect of state political business cycles on mortgage markets.

Moreover, our research design allows us to separate the effect of political business cycles from the opposing pressures of policy uncertainty. If policy uncertainty depresses economic activity prior to an election, but political business cycles push incumbent candidates to boost economic activity prior to elections, the combined effect may appear as if neither effect is at work. To disentangle these channels, we use a binding term limit design to separately identify the policy uncertainty effect, allowing us to infer the effect of political business cycles alone by contrasting the two results.

1.3 Partisan Differences

The final theory of political influence on mortgage markets emphasizes partisanship. On this account, because Democrats generally pursue more aggressive regulation and enforcement than Republicans, lenders adopt a more cautious approach to lending when Democrats are in office and a more spendthrift posture when Republicans control the reins.

This Partisan Differences Account gestures to theoretical (Alesina 1987) and empirical work (Alesina & Roubini 1992) showing that in a two-party system in which the parties have different goals concerning unemployment and inflation, changes in party control will systematically affect these variables in a "partisan cycle." In the United States, the parties adopt markedly different postures concerning enforcement of environmental (Atlas 2007), antitrust (Moe 1982; Wood & Anderson 1993), consumer-protection, and securities regulations (Moe 1982). In the depths of the 2008 financial crisis, liberal lawmakers were more likely than conservatives to vote for emergency legislation to assist homeowners near or in default, controlling for their own constituents' default rates (Mian, Sufi, & Trebbi 2010).[4] At the state level, Republican control of statehouses and governorships is associated with the repeal of restrictions on bank branching (Kroszner & Strahan 1999).

Party divisions have increased substantially in recent decades, with liberals sorting into the Democratic Party and conservatives to the GOP at both the federal and state levels (Shor & McCarty 2017; McCarty, Poole, & Rosenthal 2006). Concomitantly, the parties have adopted distinct and increasingly entrenched views on the proper scope of financial regulation (Pickerill & Bowling 2014). Indeed, among the 23 elected Democratic AGs serving in 2020, seven included consumer financial protection among the small number of topics highlighted on the "issues" or "priorities" page on their most recent campaign website. For the 19 elected Republican AGs serving in that year, the figure is zero.

These differences raise the prospect that state-level mortgage-finance and consumer-protection policies are subject to partisan swings. In a polarized political climate, a rational firm could be presumed to alter its lending behavior based on the party that controls its regulator. Depending on the context, however, the literature has found mixed effects of state partisanship on policy and market outcomes. Beland (2015) and Fredriksson, Wang, and Warren (2013) both find significant impacts of state gubernatorial partisanship on

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[4]Ideological sorting between the parties indicates that the ideological positions of the members of Congress in Mian et al.'s study closely predict their partisan affiliations (McCarty, Poole, & Rosenthal 2006).

racial earnings gaps and tax policy, respectively. However, in a comprehensive study of ideological policy-making and their real outcomes, Leigh (2008) finds no consistent effects of state gubernatorial partisanship on individual's physical or financial well being.

2 The State Law & Politics of Mortgage Finance

We test these theories by examining fluctuations in state-level mortgage markets during the period around state elections. Testing these theories by probing changes in mortgage markets around state elections assumes, first, that state officials exercise influence over these markets and, second, that they may be differentially motivated to exercise this influence. To support the first assumption, this section describes the broad powers that state officials hold in this area. We describe the expansive regulatory authority that state banking officials, who are appointed by the governor in most states, possess. We then introduce AGs—often overlooked actors in political-economic research who possess substantial discretionary enforcement authority concerning mortgage finance. To support the second assumption, we detail the growing ideological differences between the two parties at the state level and, relatedly, Democratic versus Republican officeholders' electoral imperative to appeal to supporters and donors with divergent views on mortgage-finance regulation.

2.1 Regulation by Gubernatorial Appointees

State banking regulators—who are appointed either directly by the governor or are appointed by the governor's agent and removable at-will by the governor in 43 states (Council of State Governments 2004-2014)—play an important role in mortgage markets. For one, they determine whether to grant charters to operate new institutions, assessing whether applicants have a reasonable likelihood of both financial success and sound operations. Many state banking regulators impose interest rate ceilings or other anti-predatory lending mandates as prerequisites to maintaining a charter (Zaring 2018). They supervise state-chartered institutions for safety-and-soundness: monitoring these institutions' risk profiles, assessing how they manage these risks, and undertaking corrective action where necessary (Eisenbach, et al. 2016).[5]

In addition, state banking regulators license and oversee independent non-bank mortgage lenders—a growing share of the market (Kim et al. 2018)—and others involved in housing-finance, including mortgage

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[5]They perform these functions in most cases alongside the Federal Reserve.

brokers, loan processors, underwriters, and debt-collectors (Eggert 2011). Further, they are authorized to enforce a broad array of state mortgage-lending and consumer protection laws, through administrative proceedings and via the courts. Finally, Dodd-Frank's dual-enforcement provisions empowered state regulators (and AGs) to bring civil actions to enforce federal consumer-protection and mortgage-finance laws against state-chartered institutions.

State law grants state regulators wide latitude in exercising their powers (Pahl 2007). For instance, New York State's mortgage-servicing requirements—which are perhaps the most stringent in the nation—were not issued by the state's legislature, but as regulations by the state's superintendent of banks (Eggert 2011). In addition, many state laws permit regulators to grant exemptions from mortgage-brokerage and other occupational-licensing requirements (Eggert 2011).[6] In light of the discretion afforded to state banking regulators, it is unsurprising that they adopt markedly different regulatory postures (Agarwal, Lucca, Seru, & Trebbi 2011).

2.2 Enforcement by State AGs

AGs serve as their states' chief law enforcement officers, and possess powers that, according to Provost (2010), "nearly rival[] the governor's office." They are the primary enforcers of state mortgage-lending laws, along with state prudential banking regulators in some states. In 42 states, the AG has standing to enforce the state's principal consumer- protection law. In most states, they also can file suit under their state's predatory lending, discriminatory lending, and mortgage-servicing statutes (Morris 2017).

Alongside local prosecutors, AGs are authorized to initiate prosecutions based on violations of any state law in 46 states, and can intervene in, assist with, or take control of local prosecutors' cases in most states (Lemos 2012). AGs have several tools at their disposal that are not available to the typical litigant. For instance, they receive and investigate consumer complaints, which can be used to discern patterns of behavior, and can issue civil investigative demands that allow for discovery prior to filing a complaint (Totten 2013). The power to issue a civil investigative demand to reveal information prior to filing suit can serve as a force-multiplier, alerting other actors to potential improprieties, which can spur changes apart from in-state litigation. Further, under some laws, multiple AGs can band together across states (Engel 2016). Finally, they have access to state funding, often supplemented by partnering with the plaintiffs' bar (Wilkins 2010).

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[6]The extent to which states examine non-bank lenders varies by state (McCoy 2019).

AGs played a prominent role in the enforcement of mortgage-finance laws throughout our study period. Prior to the enactment of Dodd-Frank in 2010, when federal regulators adopted a largely hands-off approach, AGs aggressively pursued state-chartered financial institutions and non-bank lenders for predatory lending, discriminatory lending, and mortgage servicing abuses (Willis & Jackman 2010). Dodd-Frank empowered AGs to go even further, enabling them, with limited exception, to enforce state law against nationally chartered lenders and to enforce federal law against all lenders (Totten 2015; Lemos 2012; Wilmarth 2011).

2.3 State Officials' Place in the Political Landscape

Ideological differences between the two parties and, relatedly, the divergent interests of their supporters may influence state officials' exercise of these powers. Governors and AGs—who are directly elected in 43 states and the District of Columbia[7]—must appeal to voters and donors to retain their positions or move up (Lemos 2011).

Today, pursuing elected office largely entails emphasizing ideology and motivating one's co-partisans (Nolette 2017). Over the past several decades, a deep ideological cleavage has developed at the state level between Democrats and Republicans. Governors diverge on a host of economic issues (Brownstein & Czekalinski 2013; see also Bulman-Pozen 2014) and band together in party-based associations of governors (Schleicher 2017).[8] AGs also are divided. They exhibit a similar degree of polarization as members of Congress (Bonica 2013)—a remarkably divided institution (Mann & Ornstein 2012). In fact, AGs in 35 states in 2009-2010 were more ideologically extreme than the mean same-party state legislator (Bonica 2013).[9]

The two parties' ideological divergence extends to consumer finance. In general, Democrats tend to favor greater regulation and Republicans less (Poole & Rosenthal 2006). Concerning consumer finance specifically, nine of the twenty-four elected Democratic AGs who are currently serving include consumer financial protection among the small number of issues that highlighted on the "issues" or "priorities" page on their most recent campaign website. For the eighteen current elected Republican AGs, that figure is zero.

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[7]That 43 states also have a gubernatorial-controlled state banking regulator is coincidental.

[8]Once again, these developments affect policy. A state's election of a Democratic versus a Republican governor has a demonstrable (albeit somewhat modest) effect on the liberalism of that state's policies (Caughey, Warshaw, & Xu 2017). The size of this effect, however, is relatively modest compared to policy differences across states. By way of explanation, Caughey et al. note that governors' need to engage with the legislature to pass laws promotes incremental changes. Not so, in several respects, concerning mortgage-finance regulations that are hashed out by gubernatorial appointees and ordinarily do not require the assent of the other branches.

[9]This polarization has policy consequences. The political composition of the state's electorate is the most important factor in predicting whether an elected AG will join a multi-state enforcement action (Provost 2003).Further, coalitions of AGs signing Supreme Court *amici* briefs have become more clear party-based in recent decades (Lemos & Quinn 2015).

State politicians' pursuit of campaign contributions may exacerbate partisan divergence on consumer-financial regulation. National donors play a growing role in state elections and are more polarized than in-state voters (Bulman-Pozen 2014; Phillips & Lax 2012). Further, banking interests are particularly prominent supporters of the Republican Party. The American Bankers Association Political Action Committee (PAC) was the largest business or labor donor to Republican candidates during the 2019-2020 election cycle, with credit unions' PAC ranked eighth.[10] By contrast, the first financial industry group to appear on the Democrats' list of top business or labor donors is the credit unions' PAC in the fifteenth position (Center for Responsive Politics 2020).

In light of the growing polarization between the parties, their differing stances on consumer-protection measures, and the economic interests of their distinct donor classes, the assumption that state officials are differentially motivated to exercise their influence over mortgage markets is reasonable.

3 Research Design

3.1 Hypotheses

The Political Uncertainty Account predicts that, as uncertainty concerning future policy increases in the period preceding an election, lenders will exhibit greater caution, reducing the number of loans that they originate. Further, those loans that are originated will be smaller in size and less likely to include terms that lenders consider to be high risk, high reward. In other words:

- Hypothesis 1a: Lenders will exhibit a more restrained lending posture in the period immediately preceding a state gubernatorial or AG election, relative to the period immediately following the election.

We define "restrained lending posture" as fewer loans originated and, conditional on origination, lower loan sizes, fewer high-risk adjustable rate features, and fewer high-risk balloon-payment features. We also examine the fraction of mortgages originated during the relevant period that enter into default or foreclosure within four years of origination. These variables proxy the riskiness of the loan, both through metrics observable at origination and those, like loan performance, that are unobservable at origination but

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[10]Auto dealers, who facilitate auto loan financing from banks for the vast majority of auto purchases (Levitin 2020), and thus are players in consumer-finance markets, rank third.

may be correlated with soft information used by lenders in the origination process. Because every election in our sample is contested by candidates from both major parties, we assume that there is some degree of uncertainty—albeit small in some cases—in each election.

The Political Uncertainty Account further predicts that elections with greater uncertainty will be associated with even greater lending restraint. Accordingly, we next examine the subset of gubernatorial and AG elections in which term limits apply. Because incumbency status is a key predictor of state election outcomes (Ansolabehere & Snyder, 2002), elections in which term limits bar the incumbent from pursuing reelection may give rise to higher policy uncertainty. Hypothesis 1b captures this logic.

- Hypothesis 1b: Lenders will exhibit a relatively more restrained pre-election lending posture for state gubernatorial or AG elections in which the incumbent is facing a binding term limit.

The Political Business Cycle Account offers an opposing prediction: that incumbents will relax regulatory enforcement in the period leading up to elections. With more borrowers approved—for larger loans and more high-risk terms that are relatively easier for borrowers to meet in the short-term but more likely to lead to foreclosure in the longer term—a permissive regulatory posture satisfies both borrowers and lenders prior to the election. After the election, officials adopt a more aggressive stance in regulating high-risk lending, resulting in a more restrained lending posture *after* an election. Hypothesis 2 presents this rationale.

- Hypothesis 2: Lenders will exhibit a more assertive lending posture in the period immediately preceding a state gubernatorial or AG election, relative to the period immediately following the election.

Finally, the Partisan Differences Account posits that differences in the party affiliations of state officials impact lender behavior: that Democratic state officials are associated with more stringent regulation and enforcement of mortgage lenders, and Republicans with greater leniency. Accordingly,

- Hypothesis 3a: A state switching from a Republican to a Democratic governor or AG will be associated with lenders adopting a more restrained lending posture in that state.

- Hypothesis 3b: A state switching from a Democratic to a Republican governor or AG will be associated with lenders adopting a more assertive lending posture in that state.

Specifically, if Hypotheses 3a and 3b are correct, then we should observe that states that switch to Democratic officials have fewer loan originations but higher quality loans, with fewer risky contract features and better performance. Further, post-switch Democrat-led states may have smaller loan amounts and lower transacted house prices to reflect the lower availability of credit. Republican AGs and governors would be associated with the opposite results. To summarize, Table 1 shows the expected post-election change in lending posture that each of these hypotheses predict. After a state election, high-risk and overall lending levels are hypothesized to fit the table below.

Table 1: Predictions of Post-Election Changes in Lending Risk

Theory	All Elections	Elections w/ Term-Limited Incumbent	GOP-to-Dem. Transitions	Dem.-to-GOP Transitions
After a state election, high-risk and overall lending levels will...				
Political Uncertainty	Increase (H. 1a)	Increase (H. 1b)		
Political Business Cycle	Decrease (H. 2)			
Partisan Differences			Decrease (H. 3a)	Increase (H. 3b)

3.2 Data

Our data combines a commercial loan-level mortgage dataset with data on state politics. Our political data is a mix of existing data and hand coded information on AG and gubernatorial elections. First, we collect data on the partisan affiliation of every elected governor or AG whose tenure in office included any portion of the 2004-2013 period. To identify governors'affiliations, we start with a database of state elections assembled by Carl Klarner (2013). This dataset covers state governor elections, including party and election date, between 2004 and 2011. We extend this data to include all state governor elections until 2013 via biennial reports published by an association of state governments (Council of State Governments 2004-2014). We use these same reports to identify AGs'partisan affiliations. Combining these sources, we assemble a comprehensive dataset of party control of these state offices at the state-year, along with election dates.

Our mortgage market data comes from Black Knight's McDash residential mortgage dataset. This large dataset includes the majority of all residential mortgage loans originated in the United States during the 2004-2013 period. The dataset contains information on loans at origination, including loan-to-value ratio,

interest rate, loan amount, the borrower's debt-to-income ratio, whether the loan terms include a prepayment penalty, and the amount of mortgage insurance required as a percentage of the loan among. The McDash dataset also includes panel data on loan performance, e.g., information on the amount of principal outstanding over time, and whether or not the loan became delinquent, was modified, or was foreclosed on in a particular period, among other variables. We use several of these variables as proxies to measure loan performance and riskiness: probability that a loan carries prepayment penalty or adjustable rate, probability of balloon mortgage, whether a loan was in default for 90 days, and whether the loan was foreclosed on.

The primary analysis involves aggregating mortgage data at the ZIP code-month level and then connecting it to the data on elections and officials' partisan affiliations. In so doing, we capture the number of loans originated in a ZIP code area in a particular year, as well as average loan characteristics and the fraction of loans that end up in delinquency, default, and foreclosure. After aggregating data to the ZIP code-level and month, the sample contains 2,371,611 zip code-month-level observations. We refer to each of these observations as a "market." zip code-month We then limit the included zip code-month markets to only those within 12 months on either side of an included state election. This limitation reduces our dataset to 681,960 markets. Table 2 reports summary statistics for these markets.

The mean zip code-month market reports 59.6 new loans originated, with a heavy right-skewed distribution. The mean property value is \$303,946. Approximately 23.1% of the loans in the mean market include a prepayment penalty clause, 15% are adjustable rate mortgages, and 3.5% are balloon mortgages. Table 2 also reports an average default rate of 12.5% and an average foreclosure rate of 4.8%.

Table 2: Summary Statistics

	Mean	10th Perc.	Median	90th Perc.
Number of Loans	59.646	4.000	15.000	58.000
House Price	303,946	110,941	208,000	559,600
Prepayment Penalty	0.231	0.000	0.208	0.455
Adjustable Rate	0.150	0.000	0.067	0.438
Balloon Mortgage	0.035	0.000	0.000	0.135
Default	0.125	0.000	0.091	0.308
Foreclosure	0.048	0.000	0.000	0.143
Observations		681,960		

Notes: Summary statistics derived from 2004-2013 sample used in full analysis. Observations measured at the zip code - month level.

3.3 Empirical Strategy

Our empirical approach is to disentangle political fluctuations surrounding state elections from other determinants of shifts in mortgage markets. Specifically, we control for state and mortgage fixed effects and look for a residual effect of elections on mortgage lending, characteristics, and performance. To assess the Political Uncertainty and Political Business Cycle Accounts, we test whether mortgage markets are smaller or lending standards are tighter before versus after elections for governors and AGs. We then compare these changes across elections with and without binding term limits on state officials. This analysis provides an additional test of the Political Uncertainty Account, on the theory that open-seat elections generation additional political uncertainty, and market participants have early, complete information that an incumbent will not be on the ballot where that incumbent is term-limited. To test the Partisan Differences Account, we examine whether the elected official's partisan affiliation impacts mortgage markets by comparing states with a switch in party control of these positions to other states.[11]

This research design builds on past work in several respects. For one, state-level elections include greater temporal and geographic variation than studies that examine national elections. Further, using mortgage loans as the unit of analysis has an additional advantage firm-level investment decisions or stock prices as the unit of analysis in other studies. Unlike these other units, individual mortgage loans are situated entirely within a single state, and thus offer a cleaner test of the effect of state elections on firms' in-state behavior.

3.3.1 All Election Event Studies

First, we document patterns in mortgage market outcomes before and after governor and AG elections. To do so, we use an event study approach, looking at the 12 months before and 12 months after the election date.

$$Y_{st} = \sum \beta_{TimetoElect} \delta_{TimetoElect} + \gamma_s + \gamma_t + \epsilon_{st} \tag{3.1}$$

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[11]Standard errors are clustered at the state level in all models. Since the primary explanatory variable in each model varies at the state level, clustering accounts for variation within states that should not drive variation on either side of an election or between Democratic and Republican regimes (Cameron & Miller 2015).

The left hand side variables of interest Y_{st} include the total volume of loans, the size of the loan reflected by the transacted house price, indicators for the loan type, including adjustable rate and balloon mortgages, and indicators of performance, including default and foreclosure. Dummies δ_i are one for the month i and zero otherwise. Fixed effects for state and month, denoted γ_s and γ_t, control for macroeconomic trends and differences in mortgage market conditions across states. This model is run separately for governor elections and AG elections. The result of the event study tests our Hypotheses 1a and 2. If β_i for post-election months $i = [1, 12]$ are consistently higher than the pre-election months $i = [-12, -1]$, the pattern would be consistent with the Political Uncertainty Account proposed in Hypothesis 1a. On the other hand, if β_1 is consistently lower after the election, relative to before, the pattern would be consistent with the Political Business Cycle approach laid out in Hypothesis 2. Our null hypothesis is that all values of β_i are statistically equivalent, and being unable to reject the null would serve as evidence that neither the Political Uncertainty nor the Political Business Cycle story applied to state governor and AG elections.

3.3.2 Binding Term Limit Difference-in-Difference

Next, we ask whether elections subject to binding term limits, which give rise to higher policy uncertainty, differ in their impact on mortgage market outcomes. To do so, we estimate the following model for both governor and AG elections:

$$Y_{st} = \beta_{BindingTL}\delta_{BindingTL} + \beta_{preElection}\delta_{preElection} + \lambda\delta_{BindingTL}\delta_{preElection} + \gamma_s + \gamma_t + \epsilon_{st} \qquad (3.2)$$

This model essentially uses a difference-in-difference approach, differencing outcome means before and after the election, for elections where the incumbent faces a binding term limit minus those where the incumbent may be re-elected. Hypothesis 1b predicts that if policy uncertainty is driving changes in market outcomes around state elections, λ should be negative for outcomes indicating a robust mortgage market. States in which term limits barred the incumbent AG or governor from running in an election in our sample period are shown in Figures 1 and 2, respectively.

3.3.3 Partisanship Difference-in-Difference

Finally, we utilize difference-in-difference models to compare states with "switch elections"—i.e., states in which control of the governorship or AG's office switched from a Democrat to a Republican (or vice-

Figure 1: State AG Term Limit Map
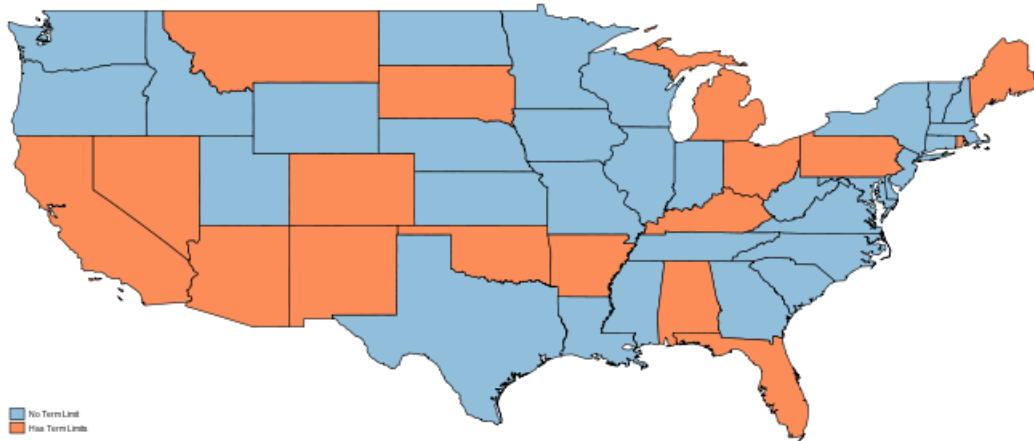
States with AG elections where incumbents faced a binding term limit at some point in our 2004-2013 sample period are highlighted in orange; states without such an election are in blue.

Figure 2: State Governor Term Limit Map
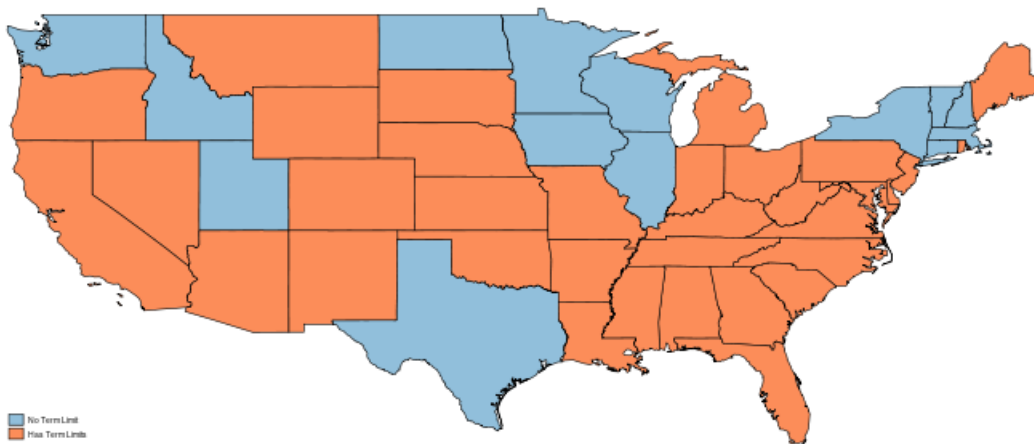
States with governor elections where incumbents faced a binding term limit at some point in our 2004-2013 sample period are highlighted in orange; states without such an election are in blue.

versa)—to a control group where the relevant office stayed in the same party's hands. Comparing mortgage outcome differences in switch- versus non-switch states, both before and after elections, provides a more granular look at the effect of partisanship on mortgage markets than the previous, more general difference-in-means models could.

To do so, we generate four switch databases: one each for the election of Republican AGs, Democratic AGs, Republican governors, and Democratic governors. The composition of each dataset is reported in Figures 4 and 3. The correlation between the election of a Republican AG and mortgage outcomes relies on comparing

the dark blue states to the light blue states in Figure 4, while the analogous correlation for Democratic AGs is measured by comparing the dark red states to the orange states. Yellow states—which experienced both a Republican-to-Democrat and Democrat-to-Republican switch during the study period—also are included. White states—i.e., states in which the AG is an appointed position—are excluded. Similar comparisons are done in Figure 3.

Figure 3: State Governor Election Map
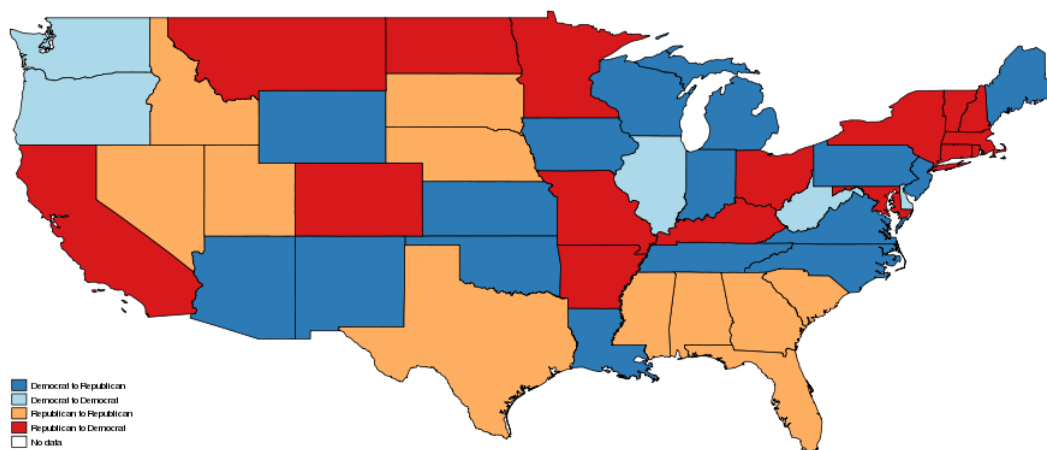
Notes: Figure depicts states included in the treatment and control groups for the Governor switch analysis. Democrat-to-Democrat states act as controls for states that receive the Democrat-to-Republican treatment, while Republican-to-Republican states are controls for Republican-to-Democrat states. "Both Switch" states are treatment groups for both experiments, because they see party turnover twice in our sample period. "No Data" states have appointed, rather than elected, AGs. Alaska and Hawaii are excluded from the map but are included in the analysis.

We use a difference-in-difference specification to study the effect of partisan switches. The estimating equation, below, includes fixed effects for state and time to account for economic fluctuations in the mortgage market.

$$Y_{st} = \beta_{SwitchState}\delta_{SwitchState} + \beta_{postElection}\delta_{postElection} + \alpha\delta_{SwitchState}\delta_{postElection} + \gamma_s + \gamma_t + \epsilon_{st} \qquad (3.3)$$

This strategy relies on two key identifying assumptions. First, loan characteristics, counts, and performance within "switch states" were not already on a different trend than loan characteristics, counts, and performance in control states before the election.[12] Second, no other changes that would systematically affect mortgage markets occurred contemporaneously with party switch elections that would directly influence loan performance and characteristics. If these assumptions are true, α can be interpreted as the causal effect

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[12]Evidence of parallel pre-trends can be obtained using an event-study approach and are available from the authors upon request.
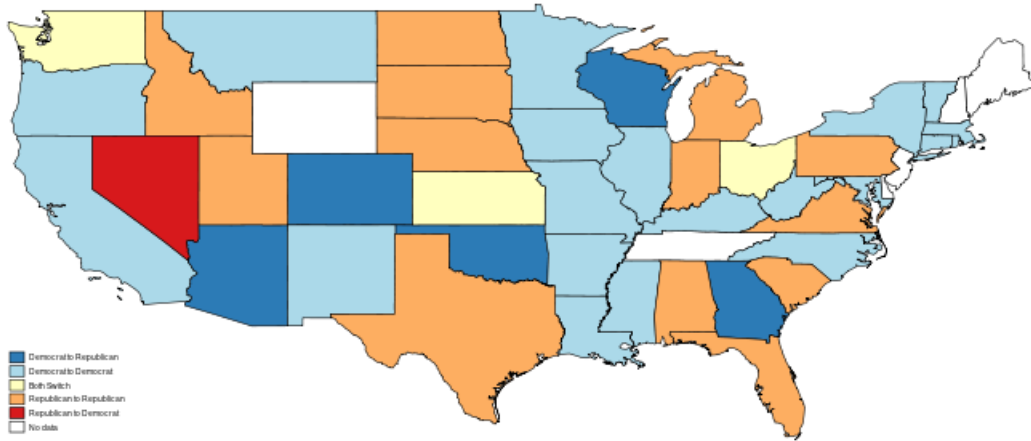
Figure 4: State AG Election Map

Figure depicts states included in the treatment and control groups for the AG switch analysis. Democrat-to-Democrat states act as controls for states that receive the Democrat-to-Republican treatment, while Republican-to-Republican states are controls for Republican-to-Democrat states. "Both Switch" states are treatment groups for both experiments, because they see party turnover twice in our sample period. "No Data" states have appointed, rather than elected, AGs. Alaska and Hawaii are excluded from the map but are included in the analysis.

of electing a state official from a political party on mortgage lending posture. Note that this model can also be interpreted as a correlation if the identifying assumptions are violated.

A significant negative coefficient α in elections of Democratic officials would provide evidence in favor of Hypothesis 3a, meaning that Democrats are associated with a more restrained lending posture. On the other hand, a significant positive coefficient in elections of Republican officials would support Hypothesis 3b, in which Republicans are associated with less restrained lending. Our null hypothesis is that α is zero, which would suggest that neither partisanship story is supported in our data.

4 Results

This section presents results for key indicators of mortgage market health, including indicators of overall credit supply, risky lending, and loan performance. The first two of these variables—the number of new loans and the house prices associated with these transactions—measure the overall health of the housing market as well as the willingness of lenders to extend credit to mortgage borrowers. We then turn to two indicators of risky lending: the fraction of loans with adjustable rates and balloon payments. Each of these loan types has been associated with increased individual and aggregate financial risk, including during

the 2008 financial crisis. Finally, we include two welfare-relevant measures of performance and financial distress: default and foreclosure.[13] Taken together, these variables represent aspects of the mortgage market that reflect lending standards, with lower values of each variable being associated with a more restrained lending posture.

4.1 Political Uncertainty and Political Business Cycles

4.1.1 Governor Elections

Figure 5 shows the relationship between state governor elections and key outcomes. State governor elections are not associated with systematically fewer loans originated before elections. Although a small increase in the number of loans appears five months after the election, this change is not persistent. Moreover, the size of the effect is economically insignificant, with an increase of less than 2 loans per zip code in a month. The average property value, fraction of adjustable rate and balloon mortgages, and the rate of default on these loans all show similar patterns —there is a slight upward trend after the election, but there is no statistically significant difference in these outcomes pre- and post-election. In the occasional months after the election where statistically significant effects arise, such as the higher fraction of balloon mortgages originated five months after the election, the size of the effect is economically insignificant, with the increase being less than .5 percentage points. Foreclosure rates are entirely unaffected by elections.

These results have two important implications. First, the lack of significant pre- or post-election trends in the outcome variables suggests that state and time fixed effects adequately control for fluctuations in the housing market. Second, the lack of either a significant expansion or contraction of lending posture suggests that there is no support here for either the Political Uncertainty Account or the Political Business Cycle Account of financial fluctuations. This may be due to both these effects being statistically insignificant or due to the two effects opposing each other and resulting in a zero net effect.

To separate these explanations, we move to the term limit analysis that isolates and tests the policy uncertainty story. We estimate equation 3.2 to compare the difference in pre-election outcomes in elections where incumbents face a binding term limit to those elections where no term limit binds on the incumbent.

The results are described in Table 3.The coefficient of interest is reported in row 3 show that there is no difference in lending behavior before term limit elections, relative to elections in which the incumbent

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[13]Note: Our data also includes a variety of other metrics. These results are available on request.

Figure 5: Impact of Governor Elections
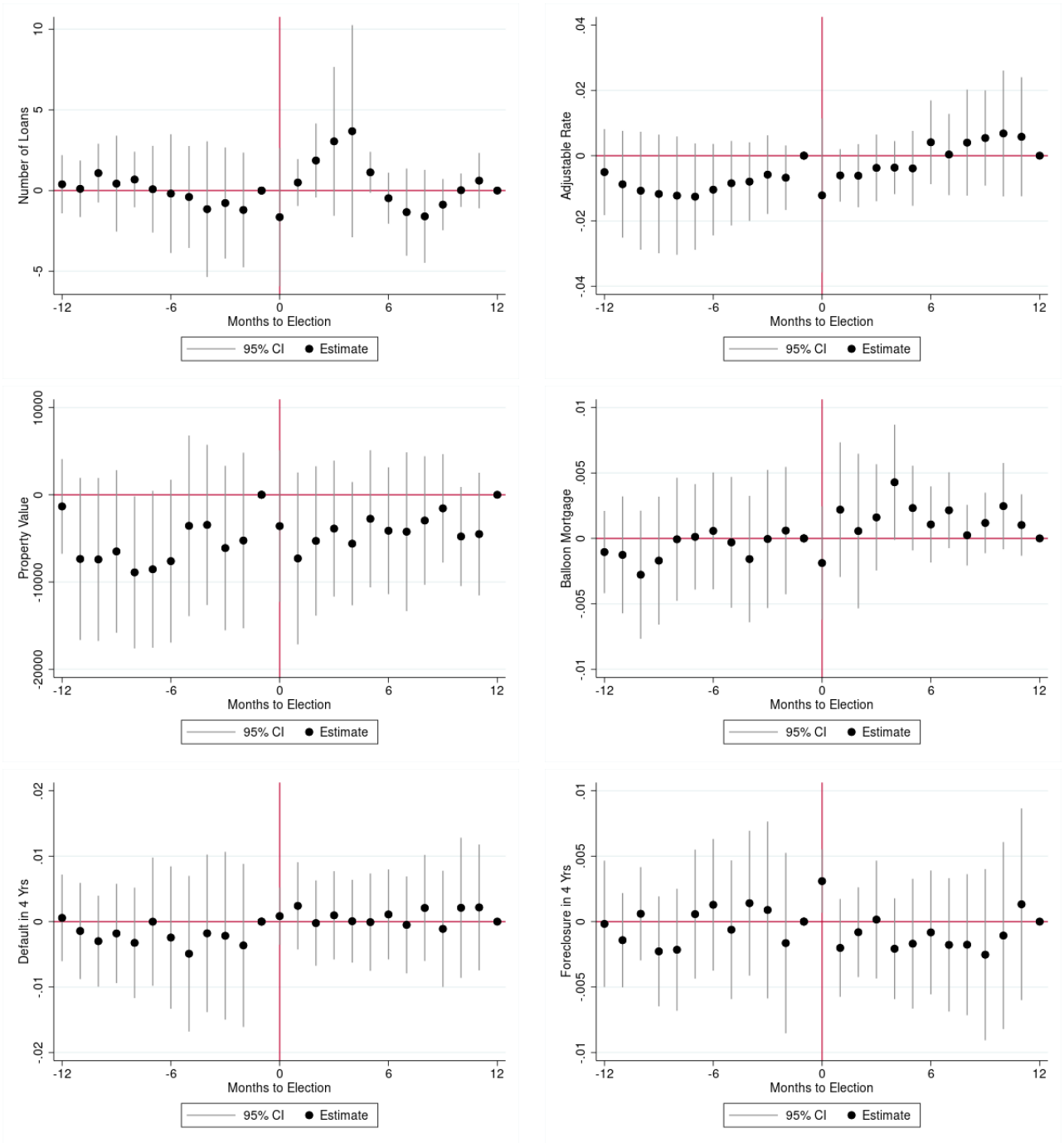
Figures depict mortgage-market outcomes during the 24-month period around gubernatorial elections. Measured at the zip-month level for the 2004-2013 period. The coefficients δ_i from 3.1 are plotted in blaxk, with 95% confidence intervals in gray. The excluded category is the month prior to the election. Regressions include time and state fixed effects. Standard errors are clustered at the state level.

Table 3: Governor Term Limits

	(1) # of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
Binding TL	1.698	-5838.5	-0.00967	0.000897	-0.00332	0.000543
	(0.59)	(-0.58)	(-0.47)	(0.22)	(-0.65)	(0.05)
Pre-Election	-0.620	-1491.8	-0.00536	-0.000459	0.000246	0.0000806
	(-0.66)	(-0.86)	(-0.84)	(-0.42)	(0.19)	(0.04)
Binding TL * Pre-Election	0.911	-2040.6	0.000376	0.0000426	0.000456	-0.00234
	(0.34)	(-0.93)	(0.07)	(0.02)	(0.17)	(-0.59)
Constant	34.25***	265943.5***	0.162***	0.0337***	0.0475***	0.115***
	(18.84)	(54.13)	(13.73)	(17.19)	(21.74)	(22.18)
N	2255475	2243624	2255475	2255475	2255475	2255475

t statistics in parentheses

$^{*}\ p < 0.05,\ ^{**}\ p < 0.01,\ ^{***}\ p < 0.001$

Table reports results from equation 3.2, utilizing the dataset of outcomes at the zip-month level between 2004 and 2013 covering the 24-month period surrounding gubernatorial elections. Each column includes time and state fixed effects. Standard errors are clustered at the state level.

may remain for another term. Moreover, the results in row 2 reiterate that for all of our proxies for lending posture are statistically equivalent in the pre-election period to the post-election period. These results do not support Hypothesis 2. Alongside the event study results above, we can conclude that there is no evidence in our data for either the Political Uncertainty Account or the Political Business Cycle theory.

4.1.2 AG Elections

Figure 5 shows the impact of AG elections on mortgage lending. The number of loans does not change significantly around the election, though a slight downward trend can be seen after the election. The size of mortgages do not vary significantly, with house price remaining constant before and after the election, with an economically small exception in the form of a drop in house prices 8 months prior to the election. Significant cyclical behavior can be seen in the origination of adjustable rate mortgages, with a higher rate of ARM lending prior to the election and a 1% drop after the new AG is elected. No significant effects are present concerning balloon mortgages, defaults, or foreclosures, despite occasional months with statistically significant coefficients. Taken together, only ARM originations show consistent support for any of our hypotheses. The direction of the effect is consistent with a political business cycle, and is not consistent with political uncertainty.

Figure 6: Impact of AG Elections
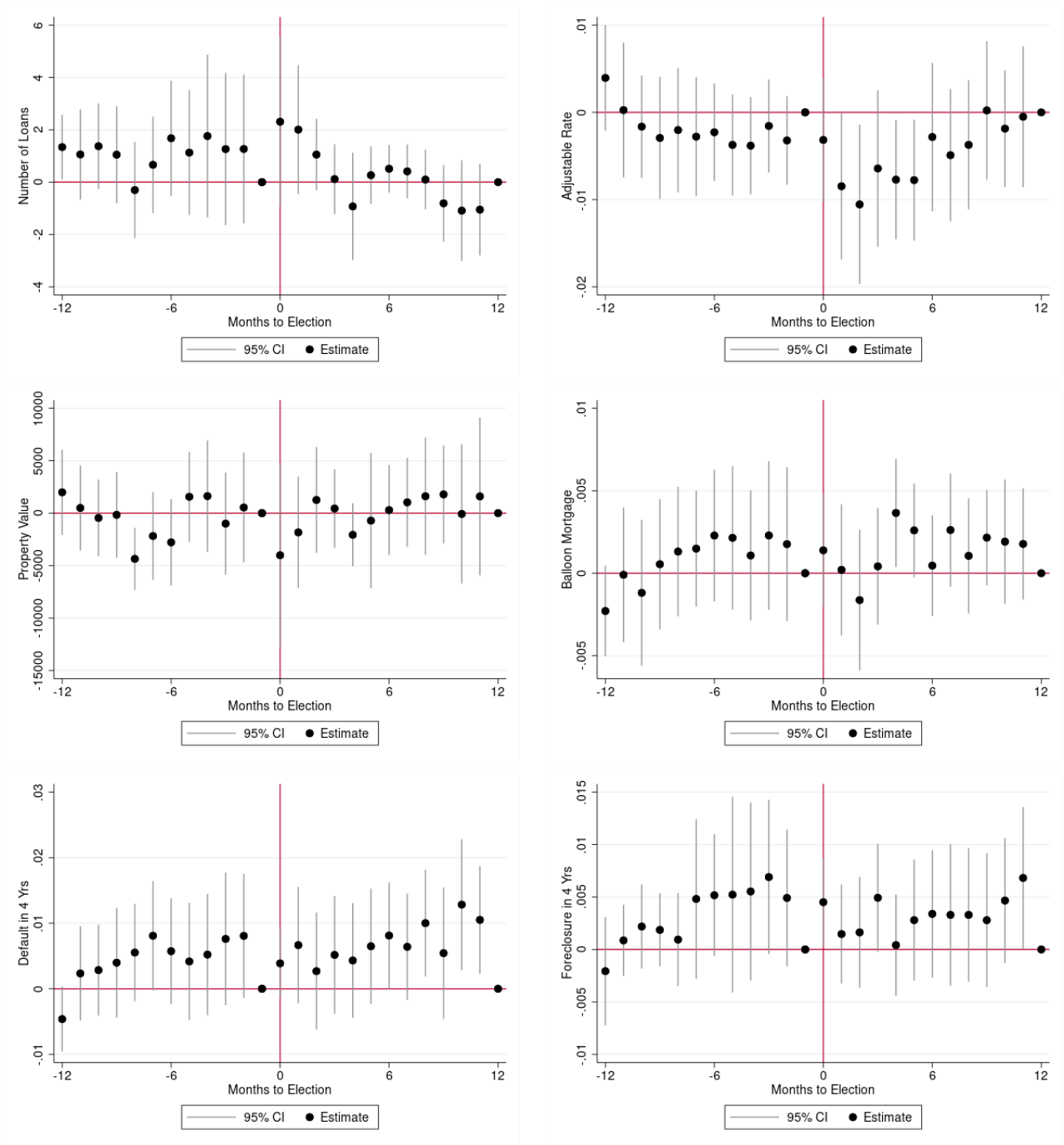
Figures depict mortgage-market outcomes during the 24-month period around AG elections. Measured at the zip-month level for the 2004-2013 period. The coefficients δ_i from 3.1 are plotted in blaxk, with 95% confidence intervals in gray. The excluded category is the month prior to the election. Regressions include time and state fixed effects. Standard errors are clustered at the state level.

Table 4: AG Term Limits

	(1) \# of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
Binding TL	-1.098	-13359.9	-0.00165	0.00190	-0.0258*	-0.0173
	(-0.23)	(-0.90)	(-0.06)	(0.38)	(-2.13)	(-1.35)
Pre-Election	1.018	-814.0	0.000307	-0.000997	-0.000871	-0.00187
	(1.69)	(-0.70)	(0.14)	(-1.19)	(-0.59)	(-1.03)
Binding TL * Pre-Election	-3.052	8304.1	-0.00426	0.00247	0.0113	0.00467
	(-1.65)	(1.98)	(-0.76)	(1.19)	(1.73)	(0.72)
Constant	38.45***	268484.9***	0.158***	0.0347***	0.0485***	0.117***
	(104.77)	(327.37)	(113.80)	(88.26)	(54.29)	(115.76)
N	1825486	1818115	1825486	1825486	1825486	1825486

t statistics in parentheses

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

Notes: Table reports results from equation 3.2, utilizing the dataset of outcomes at the zip-month level between 2004 and 2013 covering the 24-month period surrounding AG elections. States with appointed AGs are excluded. Each column includes time and state fixed effects. Standard errors are clustered at the state level.

To confirm that our data does not support a Political Uncertainty Account, we assess whether elections in which term limits bar the incumbent from running are associated with a more restrained pre-election lending posture, compared to elections in which the incumbent is not term-limited. The results shown in Row 3 of Table 4 demonstrate that no outcomes are significantly different in binding term limit elections. However, the coefficients reported in Row 3 are larger and much closer to statistical significance than those reported for gubernatorial elections in 3. For instance, house prices seem slightly *higher* prior to AG elections with binding term limits, which directly contradicts the prediction of Hypothesis 2. Accordingly, we find no evidence to support the Policy Uncertain Account. Some results concerning AG elections—but not gubernatorial elections—are consistent with the Political Business Cycles Account, however.

4.2 Partisan Differences

Next, we turn to examining "party switches"—or transitions from a Democrat to a Republican official, or vice-versa—within a given state. We employ a difference-in-difference framework to assess whether states in which party control of these offices switches exhibit changes in mortgage outcomes, relative to a set of "control" states in which no change in party control occurred. The goal is to test Hypothesis 3a and 3b regarding the role of state electoral partisanship in affecting mortgage markets.

4.2.1 Republican-to-Democrat Switches

First, we test whether our data shows any support for Hypothesis 3a, which posits that electing Democratic governors and AGs should be associated with a more restrained lending posture, specifically due to threats of regulatory oversight as suggested by political rhetoric. Table 5 reports the effect of electing a Democratic governor, comparing Republican-to-Democrat governor switches to states that retained a Republican governor. Row 2 shows that there are no statistically significant effects of electing a Democratic governor. These null results suggest that the election of a Democratic governor does not impact mortgage lenders or the housing market.

Table 5: Republican-to-Democratic Governor Switches

	(1) # of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
After Election	-20.97*	3622.5	-0.00468	0.00686	-0.00548	-0.0161
	(-2.07)	(0.27)	(-0.13)	(0.90)	(-0.60)	(-1.09)
After Party Switch	22.63	-2024.7	0.00724	-0.00361	-0.00891	0.00991
	(1.68)	(-0.14)	(0.17)	(-0.61)	(-1.63)	(0.90)
Constant	64.74***	318619.1***	0.167***	0.0385***	0.0580***	0.137***
	(47.53)	(95.36)	(25.05)	(14.98)	(15.39)	(25.24)
Observations	466733	466437	466733	466733	466733	466733

t statistics in parentheses

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

Table reports results from estimating equation 3.3. The coefficient of interest is labeled "After Party Switch." Treated states elect a Democratic governor after a Republican, while control states have Republican governors before and after the election. All regressions include state and time fixed effects. The "switch state" variable is collinear with fixed effects and excluded. Standard errors are clustered at the state level.

Then, we consider the impact of electing Democratic AGs, comparing states that switch from a Republican to a Democratic AG to those where Republican AGs remain before and after the election. Once again, we cannot reject the null hypothesis that these features do not differ in switch states relative to the relevant set of control states, as shown in Table 6. Compared to the effect of electing a Democratic governor, however, the lending market appears to react more strongly in the expected direction—the coefficients on the effect on house prices and foreclosures are economically significant, though not statistically significant. Though it is more likely that Democratic AGs may have an impact on lending than Democratic governors, there is no evidence in this dataset to suggest that the rhetoric of the Democratic Party, including their assurances of strong regulatory oversight of mortgage markets, is merely cheap talk.

Table 6: Republican-to-Democrat AG Switches

	(1) ## of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
After Election	-31.45**	-17676.4	-0.00804	-0.00539	0.00797	0.0127
	(-3.64)	(-1.55)	(-0.35)	(-1.02)	(0.89)	(0.94)
After Party Switch	4.090	-13424.3	-0.0296	-0.000318	-0.0138	-0.00571
	(0.21)	(-1.16)	(-0.76)	(-0.06)	(-1.40)	(-1.06)
Constant	73.68***	224370.1***	0.170***	0.0503***	0.0662***	0.152***
	(17.86)	(43.55)	(19.37)	(20.35)	(13.27)	(21.77)
Observations	276499	276284	276499	276499	276499	276499

t statistics in parentheses

$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Table reports results from estimating equation 3.3. The coefficient of interest is labeled "After Party Switch." Treated states elect a Democratic AG after a Republican, while control states have Republican AG before and after the election. All regressions include state and time fixed effects. The "switch state" variable is collinear with fixed effects and excluded. Standard errors are clustered at the state level.

4.2.2 Democrat-to Republican Switches

Next, we test Hypothesis 3b by studying the impact of electing Republican governors and AGs, compared to states which consistently have Democratic governors and AGs. Table 7 reports the impact on mortgage markets of switching from a Democrat to a Republican governor, relative to those states in which the Democratic Party retained control of the governorship. As row 2 of the table shows, these models yield null results except in mortgage defaults. Democrat-to-Republican governor switches are associated with a 4.4 percentage-point reduction in the default rate, or nearly 25% relative to the baseline. This striking effect runs counter to partisan rhetoric regarding Republican officials, who do not typically promise crackdowns on risky lending in their campaigns.

We conduct a parallel analysis for states in which the party controlling the AGs' office switches from Democrat to Republican. The results are shown in Table 8. As with the governor switches, we see that AG party switches do not have statistically significant impacts on most mortgage market outcomes. The only exception is in the number of loans, which increase by 16 loans per zip-month after a Republican AG is elected, which is equivalent to approximately 23% relative to the baseline. This increase is more in accord with the typical partisanship story. There are two broad takeaways from these results. First, Republican officials generally do not universally expand lending markets. Second, the officials that do deliver on their partisan promises, if any, are more likely to be AGs than governors.

Table 7: Democrat-to-Republican Governor Switches

	(1) # of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
After Election	-13.29	23710.4*	0.00569	-0.00589	0.0151	0.0295*
	(-0.85)	(2.13)	(0.33)	(-0.99)	(1.52)	(2.18)
After Party Switch	-0.0816	-33804.5	-0.00622	0.00384	-0.0226	-0.0437*
	(-0.01)	(-1.89)	(-0.24)	(0.44)	(-1.48)	(-2.14)
Constant	95.94***	173656.3***	0.149***	0.0418***	0.0844***	0.179***
	(7.99)	(20.54)	(13.24)	(12.09)	(11.25)	(16.21)
Observations	298537	298395	298537	298537	298537	298537

t statistics in parentheses

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

Table reports results from estimating equation 3.3. The coefficient of interest is labeled "After Party Switch." Treated states elect a Republican governor after a Democrat, while control states have Democratic governors before and after the election. All regressions include state and time fixed effects. The "switch state" variable is collinear with fixed effects and excluded. Standard errors are clustered at the state level.

Table 8: Democrat-to-Republican AG Switches

	(1) # of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
After Election	-30.08**	-6327.0	-0.00403	-0.00771	-0.00276	-0.0151
	(-2.89)	(-0.49)	(-0.22)	(-1.62)	(-0.29)	(-0.88)
After Party Switch	16.38*	-1664.1	-0.0182	0.00380	-0.00855	-0.00369
	(2.10)	(-0.15)	(-0.81)	(1.06)	(-1.00)	(-0.30)
Constant	67.62***	301969.2***	0.202***	0.0526***	0.102***	0.184***
	(9.27)	(39.93)	(28.19)	(16.02)	(15.96)	(18.75)
Observations	427751	427570	427751	427751	427751	427751

t statistics in parentheses

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

Table reports results from estimating equation 3.3. The coefficient of interest is labeled "After Party Switch." Treated states elect a Republican AG after a Democrat, while control states have Democratic AGs before and after the election. All regressions include state and time fixed effects. The "switch state" variable is collinear with fixed effects and excluded. Standard errors are clustered at the state level.

5 Discussion

In the aggregate, we cannot reject the null hypothesis associated with any of the accounts. In other words, *none* of the three theoretical pathways of electoral influence on market activity finds support here.

These results contrast with significant effects of policy uncertainty on corporate investment (Jens 2016), initial public offering activity (Colak, Durnev, & Qian 2017), and mortgage lending (Kara & Yook 2019), sig-

nificant political business cycles in macroeconomics indicators around gubernatorial elections (Chang, Kim, & Ying 2009), and significant impacts of gubernatorial partisanship on racial earnings gaps (Beland 2015) and tax policy (Fredriksson, Wang, & Warren 2013). We demonstrate that state elections and partisanship do not result in significant differences in real mortgage market outcomes, in line with work on redistributional policies (Dilger 1998), fiscal policy (Poterba 1994), state taxes (Nelson 2000), and state policies that are ideologically salient (Leigh 2008).[14] That our results contrast with studies of some policies areas and are consistent with others suggests that the effects of politics on economic outcomes differ by sector.

The null results here are noteworthy. Abadie (2020) shows that in a large-sample setting without a substantial prior probably mass at the point null, "rejection of a point null often carries very little information, while failure to reject is highly informative."[15] In other words, where (1) a large dataset is used (which increases the likelihood of observing statistically significant results), and (2) the analyst has sound theoretical reasons to expect to see a statistically significant result, the failure ot reject the null hypothesis can be highly informative. Both conditions are present here. This article (1) employs mortgage performance data with the largest available coverage, which is *large enough to interpret our results as a population effect*, to (2) test three well-established theories that offer unequivocal nonzero predictions. Given these conditions, our null results are particularly informative.

Table 9 reproduces the earlier table showing the predicted post-election changes in lending, high-risk lending, and loan failures associated with each hypothesis. The table then reports the overall findings regarding actual post-election changes.[16]

As Table 9 shows, there are no consistent and statistically significant results that point to any of the three predominant theories by which electoral politics may distort the functioning of financial markets. These results are consistent with elections and partisan transitions having little to no impact on mortgage markets. Rather than reacting to state elections and political transitions, lenders may perceive a nationally cohesive approach to mortgage-finance regulation. That AGs have a well-documented history of working across

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[14]Our results also are consistent with event studies showing the effects of adverse regulatory changes (rather than elections that precipitating presumed regulatory changes, as in our study) on corporate valuations may be overstated (Coglianese & Walters 2020).

[15]In consequence, increasing number of researches including Abadie (2020) "advocate[s] visible reporting and discussion of non-significant results in empirical practice" (see also Angrist et al. 2019; Abdulkadiroglu, Angrist, & Pathak 2014; Abadie 2006; and Krueger & Maleckov 2003).

[16]Although most of our findings paint a consistent picture of null effects, we do see statistically significant effects for three variables: AG elections are associated with a decrease in ARMs (but not concerning other high-risk features); Democrat-to-Republican gubernatorial transitions are associated with fewer defaults (but not fewer foreclosures or the origination of mortgages with fewer high-risk features that could lead to defaults); and Democrat-to-Republican AG transitions are associated with more new originations (but, again, no other changes). There is no reasonable explanation for *only* these particular variables to be associated with elections and political transitions—and only in a subset of elections, at that. That the coefficient estimates achieve conventional levels of statistical significance in a few of the dozens of model specifications reported above may be spurious.

Table 9: Predictions of Post-Election Changes in Lending Posture

Theory	All Elections	Elections w/ Term-Limited Incumbent	GOP-to-Dem. Transitions	Dem.-to-GOP Transitions
After a state election, high-risk and overall lending levels will . . .				
Policy Uncertainty	Increase (H. 1a)	Increase (H. 1b)		
Political Business Cycle	Decrease (H. 2)			
Partisan Differences			Decrease (H. 3)	Increase (H. 3)
After a state election, high-risk and overall lending levels . . .				
Gov. Elections	Unaffected	Unaffected	Unaffected	Unaffected except drop in default
AG Elections	Unaffected except drop in ARMs	Unaffected	Unaffected	Unaffected except increase in # loans

state lines in the wake of the 2008 financial crisis provides support for this perspective. For instance, a bipartisan coalition of 49 AGs negotiated a national settlement with mortgage servicers for abuses against mortgage borrowers in 2012. In this account, stable mortgage markets around elections reflect predictable and consistent regulation and enforcement.

Alternatively, our results could be consistent with multiple, cross-cutting dynamics—consistent with the Policy Uncertainty, Political Business Cycle, and Partisan Differences Accounts—at play simultaneously. These competing phenomena pull lenders in different directions. Perhaps the competencies and priorities of the specific individuals holding these offices may cause one of these three accounts to predominate in a particular state and year. In the aggregate, however, one observes null results. Thus, this placid surface may hide strong, cross-cutting currents just beneath.

In either case, our results suggest that the growing power of state actors to regulate and enforce mortgage markets, and financial markets as a whole, may be efficient. These results disrupt conventional wisdoms based on the three major theories—each of which generates firm predictions that were not borne out here. Instead, political pressures do not appear to distort markets, either in anticipation of state elections, or after a change in leadership. Accordingly, these results mollify concerns that political actors' increased role in mortgage-finance regulation and enforcement, particularly as the shadow banking sector grows, will lead to politics-induced inefficiencies.

This article's research design and findings make four additional contributions to the extant literature. First, we expand on the types of political actors typically studied in election models. Although past literature has considered the impact of presidential elections and state governor elections, the literature has largely overlooked state actors with greater enforcement power, but lower public profiles, such as AGs. Our results show that a greater number of significant results are present concerning AG elections than gubernatorial contests. This result suggests that greater scholarly attention to AGs—who are relatively lower-profile than governors but play a leading role in regulatory enforcement in many areas—is warranted.

Second, our uniquely detailed outcome data, including the number of loans originated, their size, and other characteristics, allow us to pick up even small changes in mortgage markets.

Third, our analysis has implications for the use of event studies in financial markets outside the usual context of corporate law and stock prices. A large literature uses the price at which financial assets are *traded*, namely contemporaneous stock prices, to assess the effects of corporate litigation and regulation on shareholder wealth (see, e.g., Coglianese & Walters 2020; Bhagat & Romano 2002). A slightly different set of considerations arises when moving away from that setting, which focuses on investors' *beliefs* about an asset's value, towards a context where the outcome of interest can be measured directly - the characteristics of loans originated at different times in the political cycle. One issue that is often raised when using event studies to assess the effect of regulatory changes is whether those changes have been anticipated by market participants (Schwert 1981). Our setting directly tests this by developing and testing the Political Uncertainty Account, which is derived from anticipation effects. We see no evidence of anticipatory effects. To validate our move away from traded asset prices to loan characteristics, we study multiple characteristics of the loan market that, taken together, can provide a complete picture of risk taking and credit screening in the mortgage loan market. Our approach adapts traditional event studies used to study the impact of law to the broader context of elections and financial markets.

Finally, the null results reported herein are valuable on their own in this policy area. To the extent that publication bias in favor of statistically significant results provides scholars and policymakers with a distorted picture (DellaVigna & Linos 2020), these findings provide a needed course correction.

6   Conclusion

For many Americans, homeownership is synonymous with the "American Dream" (Shlay 2006). That dream is made real via $15.8 trillion in outstanding mortgage loans (Howley 2020). The importance of mortgage-finance to a broad swath of Americans, along with politicians' attention to housing policy during political campaigns and government's close regulation of the sector, raises the question of how, if at all, elections influence mortgage-market activity. In their lead-up, elections generate uncertainty regarding the identities of future officeholders and, thus, their policies. After the votes are counted, the results may serve as harbingers for post-inauguration changes in policy. Given these connections between elections and the future regulatory environment facing mortgage lenders, the extant literature hypothesizes that electoral pressures may impact financial markets via three channels: increased political uncertainty, cyclical changes due to political business cycles, and inconsistent regulations due to partisanship.

Our analysis does not yield consistent support for any of these three channels. That finding raises the possibility that state-level regulation and enforcement are steady in the face of political changes. When political transitions do generate shifts in regulatory posture, those changes may be slow enough to enable the market to gradually adjust. These results also leave open the possibility that several of the theoretical dynamics are present at once, pushing lenders in different directions, perhaps to a degree that varies by election.

Our results suggest that concerns that political uncertainty, electorally-motivated regulation, or divergent partisan regulatory postures distort mortgage-lending markets may be overblown. Industry groups criticize the post-Dodd-Frank devolution of regulatory power to state actors, arguing that empowering a diverse set of politically motivated actors, whose identities change with elections, generates policy uncertainty and depresses lending (Pincus 2014; Bordo, Duca, & Koch 2016). That notion does not find support here. Instead, these results suggest that increased regulatory power in the hands of state politicians may have fewer costs and more benefits than previously predicted.

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A   Appendix

This appendix reports the results of two robustness checks. First, we re-run the models testing the Political Uncertainty and Political Business Cycle Accounts excluding presidential election years. Then, we re-run these models with additional demographic controls.

A.1   Excluding Presidential Election Years

As discussed above, studies testing the Political Uncertainty and Political Business Cycle Accounts have uncovered abnormal market behavior around presidential elections. To avoid simultaneous treatment effects or any other contamination from presidential elections, we re-run the Political Uncertainty and Political Business Cycle analyses excluding presidential election years. [17]

As Tables 10 and 11 show, the results of this robustness check are consistent with our main analysis. For the governor term limits results in Table 10, the coefficients are slightly larger, but the differences between elections in which the governor is term limited and other gubernatorial election are not statistically significant.

Figures 7 and 8 present results for the event-study analyses, excluding presidential election years, for gubernatorial and AG elections, respectively. Once again, these figure are substantially similar to those in the main analysis.

A.2   Including Census Data

In addition to controlling for time-varying confounders that are specific to the zip-code level, we ran several models with demographic covariates. Median household income, unemployment rate, and population are common covariates in mortgage-market research. We obtain median household income and the population estimates from Census data, and unemployment figures from the Bureau of Labor Statistics. [18]

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[17]We do not re-run the Partisan Differences models with this specification because the Partisan Differences Account does not imply the prospect of simultaneous treatment effects of presidential election cycles and partisan switches in state officeholders.

[18]Because these data are reported at the county level, we utilize a crosswalk developed by the Department of Housing and Urban Development to match these data to the zip-code level. For cross-county zipcode areas, we average over the counties to be used as controls.

Figure 7: Impact of Governor Elections
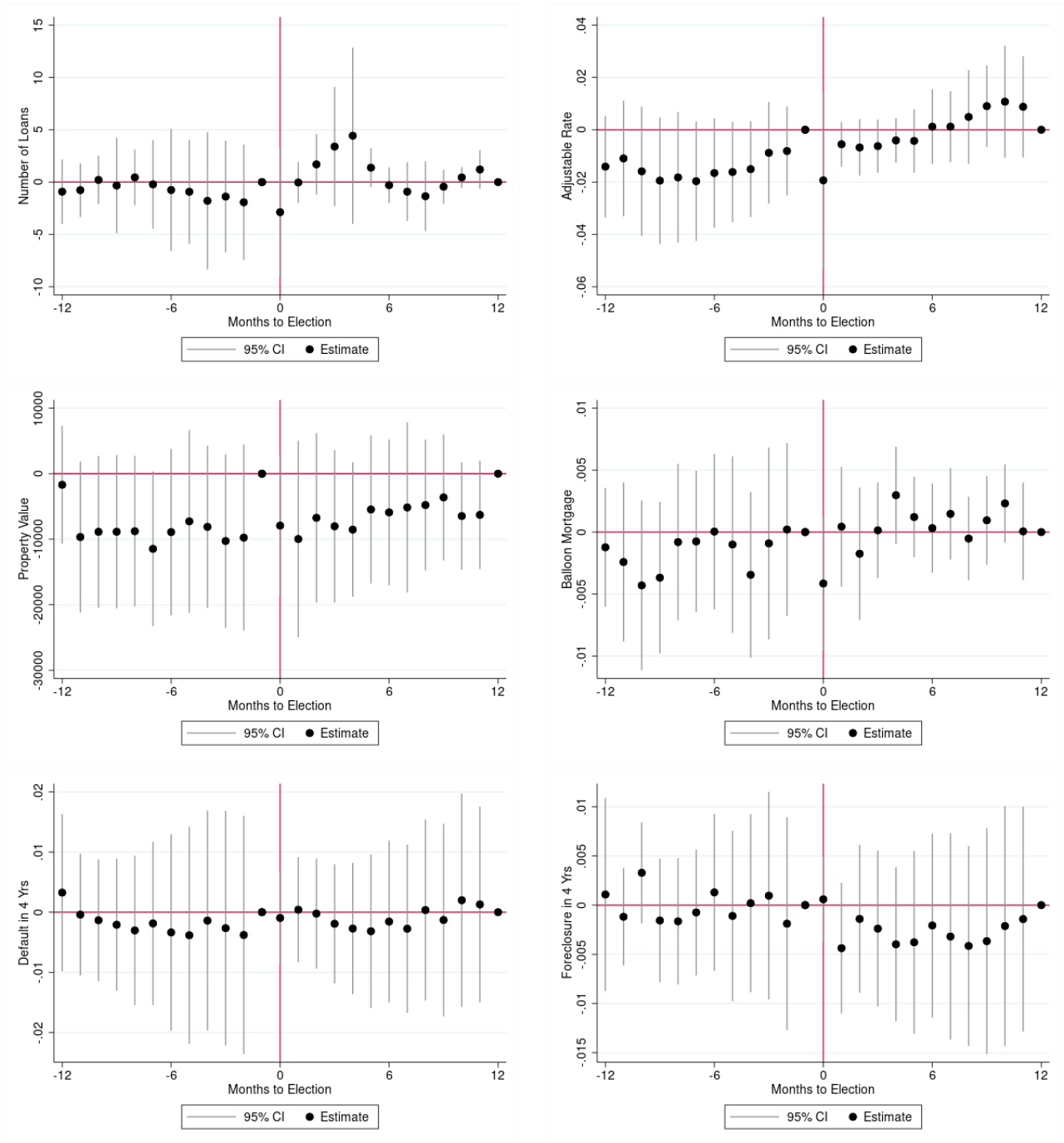
Notes: Figures depict mortgage market outcomes before and after governor elections. Outcomes data are measured at the zip-month level and cover the 24-month window around gubernatorial elections between 2004 and 2013. Elections that were coincident with presidential elections are excluded. The coefficients δ_i from 3.1 are plotted in black, with 95% confidence intervals in gray. The excluded category is the month prior to the election. Standard errors are clustered at the state level.

Table 10: Governor Term Limits

	(1) #of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
Binding TL	1.893	-5009.8	-0.0115	0.000164	-0.00265	0.00247
	(0.57)	(-0.44)	(-0.52)	(0.04)	(-0.50)	(0.20)
Pre-Election	-1.718	-2881.6	-0.0110	-0.000689	0.00110	0.00151
	(-1.12)	(-1.23)	(-1.51)	(-0.38)	(0.43)	(0.41)
Binding TL * Pre-Election	1.318	-1167.0	0.00252	0.000477	0.00162	-0.00220
	(0.45)	(-0.52)	(0.44)	(0.28)	(0.64)	(-0.54)
Constant	35.06***	271278.1***	0.173***	0.0359***	0.0467***	0.114***
	(15.02)	(49.01)	(12.73)	(17.17)	(20.02)	(19.02)
N	2063677	2053183	2063677	2063677	2063677	2063677

t statistics in parentheses

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

Notes: Table reports results from equation 3.2. Outcomes data are measured at the zip-month level and cover the 24-month period around gubernatorial elections between 2004 and 2013. Elections that were coincident with presidential election years are excluded. Each column includes time and state fixed effects. Standard errors are clustered at the state level.

Table 11: AG Term Limits

	(1) #of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
Binding TL	-1.422	-13252.4	-0.000315	0.00258	-0.0284*	-0.0179
	(-0.28)	(-0.80)	(-0.01)	(0.48)	(-2.35)	(-1.31)
Pre-Election	0.907	-735.6	-0.000904	-0.00142	-0.000279	-0.00115
	(1.14)	(-0.36)	(-0.43)	(-1.16)	(-0.11)	(-0.35)
Binding TL * Pre-Elect	-1.781	9218.0	-0.00517	0.00303	0.0129	0.00700
	(-0.89)	(2.02)	(-0.81)	(1.48)	(1.91)	(1.10)
Constant	39.43***	274927.7***	0.167***	0.0372***	0.0485***	0.117***
	(84.42)	(211.87)	(108.05)	(60.67)	(36.36)	(65.64)
N	1641423	1635209	1641423	1641423	1641423	1641423

t statistics in parentheses

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

Notes: This table reports results from equation 3.2, utilizing the dataset of outcomes at the zip-month level between 2004 and 2013 covering the 24-month period surrounding AG elections, excluding elections that were coincident with presidential election years. Each column includes time and state fixed effects, and standard errors are clustered at the state level.

Once again, the results with these additional control variables are consistent with those reported above. Results from the event-study analyses are reported in Figures 9 and 10; those for the term-limits analyses appear in Tables 12 and 13. Notably, the AG term limits analysis in Table 13 now report pre-election

differences between term-limited and non-term-limited AG elections that are not statistically significant. Otherwise, the results are substantially similar to those reported in the main text.

Finally, we re-ran the party switch analyses, which test the Partisan Differences hypothesis, with these additional demographic controls. Tables 14-17 report these results. None of the coefficient estimates for our variable of interest, *After Party Switch*, achieve conventionally accepted levels of statistical significance, with one exception: Democrat-to-Republican governor switches are associated with a statistically significant $33,731 decline in house price. That results cuts against the Partisan Differences Account, which predicts that the turn to Republican administration will lead to an *expansion* of the credit supply.

Table 12: Governor Term Limits

	(1) # of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
Binding TL	-0.453	-2402.4	-0.00745	0.00107	-0.00399	-0.000211
	(-0.14)	(-0.34)	(-0.40)	(0.28)	(-0.72)	(-0.02)
Pre-Election	-0.952	-1833.4	-0.00526	-0.000328	0.000154	-0.0000679
	(-0.85)	(-0.98)	(-0.86)	(-0.30)	(0.12)	(-0.03)
Binding TL * Pre-Elect	0.622	-3313.6	-0.00125	-0.0000302	0.000506	-0.00196
	(0.31)	(-1.00)	(-0.24)	(-0.02)	(0.19)	(-0.48)
Mean Income	3.60e-4***	7.311***	2.28e-6***	2.78e-7***	-7.61e-8	-3.58e-7*
	(4.20)	(5.46)	(6.02)	(3.84)	(-1.18)	(-2.33)
Unemployment	-1.367*	-6393.4	-0.00697	-0.000262	0.00184***	0.00210
	(-2.44)	(-0.78)	(-1.71)	(-0.58)	(3.52)	(1.79)
Population	3.07e6*	0.0279**	1.03e-8	1.45e-9	-7.66e-10	4.56e-10
	(2.19)	(3.28)	(1.89)	(1.56)	(-0.85)	(0.39)
Constant	7.072	-70462.8	0.0895	0.0204*	0.0393***	0.118***
	(0.79)	(-0.72)	(1.96)	(2.55)	(5.89)	(7.60)
N	2077703	2069102	2077703	2077703	2077703	2077703

t statistics in parentheses

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

Notes: This table reports results from equation 3.2, utilizing the dataset of outcomes at the zip-month level between 2004 and 2013 covering the 24-month period surrounding governor elections, including controls for census variables. Each column includes time and state fixed effects, and standard errors are clustered at the state level.

Table 13: AG Term Limits

	(1) \# of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
Binding TL	1.518	-7485.5	-0.00173	0.00219	-0.0250*	-0.0164
	(0.44)	(-0.83)	(-0.07)	(0.47)	(-2.07)	(-1.27)
Pre-Election	0.781	-2457.1	-0.000783	-0.00110	-0.000695	-0.00154
	(1.26)	(-1.61)	(-0.30)	(-1.30)	(-0.47)	(-0.83)
Binding TL * Pre-Elect	1.633	19293.8	0.00665	0.00307	0.00867	0.00190
	(1.61)	(1.55)	(0.73)	(1.32)	(1.35)	(0.30)
Mean Income	3.89e-4***	7.592***	2.38e-6***	2.97e-7***	-8.38e-8	-3.06e-7
	(4.43)	(5.07)	(5.64)	(3.79)	(-1.12)	(-1.81)
Unemployment	-1.437*	-7836.6	-0.00808	-0.000391	0.00174**	0.00202
	(-2.41)	(-0.88)	(-1.77)	(-0.86)	(3.04)	(1.71)
Population	2.96e-6*	0.0277**	1.02e-08	1.41e-09	-7.77e-10	4.65e-10
	(2.22)	(3.31)	(1.89)	(1.55)	(-0.87)	(0.40)
Constant	5.789	-75113.1	0.0880	0.0216**	0.0411***	0.117***
	(0.72)	(-0.70)	(1.99)	(2.96)	(5.63)	(7.50)
N	1817375	1810060	1817375	1817375	1817375	1817375

t statistics in parentheses

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

Notes: This table reports results from equation 3.2, utilizing the dataset of outcomes at the zip-month level between 2004 and 2013 covering the 24-month period surrounding AG elections, including controls for census variables. Each column includes time and state fixed effects, and standard errors are clustered at the state level.

Figure 8: Impact of AG Elections
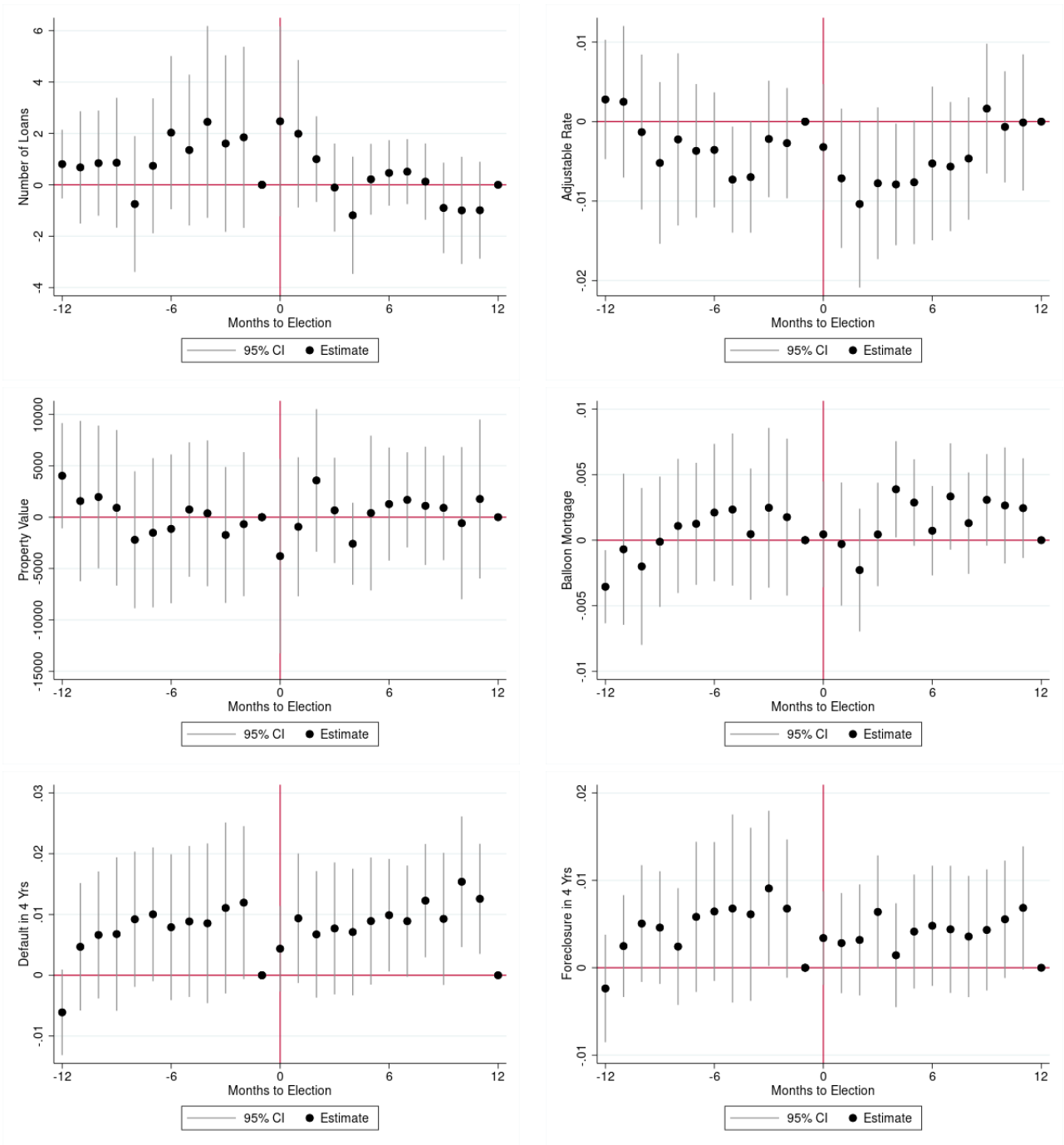
Notes: Monthly event study graphs showing mortgage market outcomes before and after AG elections, using data at the zip-month level for the 24 month window around every AG election between 2004 and 2013, excluding election years that were coincident with presidential elections. Note that this sample excludes states where AGs are appointed by the governor. Plotted in black are the coefficients δ_i from 3.1, with 95% confidence intervals in grey. The excluded category is the month prior to the election. Standard errors are clustered at the state level.

Figure 9: Impact of Governor Elections
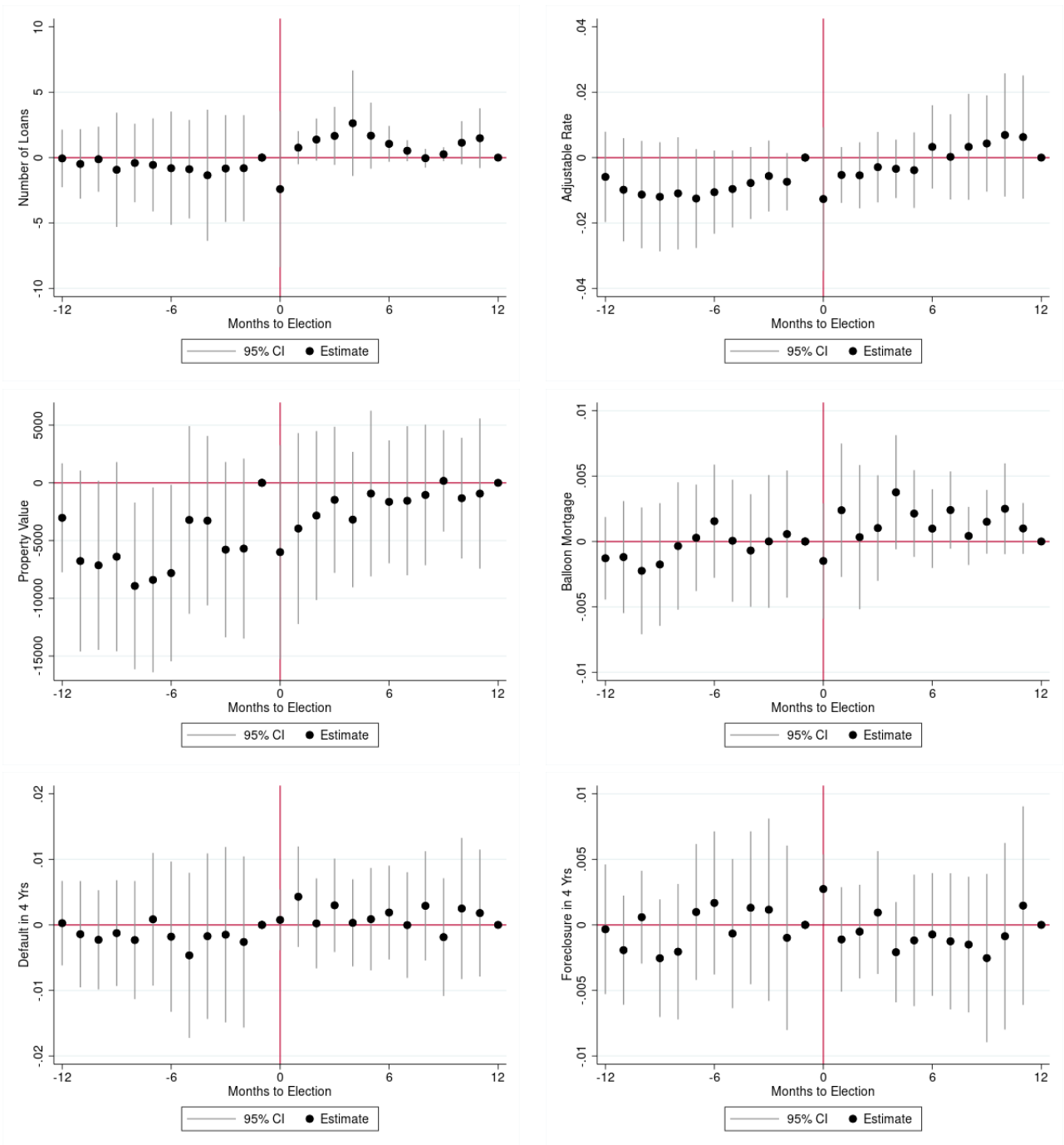
Notes: Monthly event study graphs showing mortgage market outcomes before and after governor elections, using data at the zip-month level for the 24 month window around every governor election between 2004 and 2013, including controls for census variables at the zipcode level. Plotted in black are the coefficients δ_i from 3.1, with 95% confidence intervals in grey. The excluded category is the month prior to the election. Standard errors are clustered at the state level.

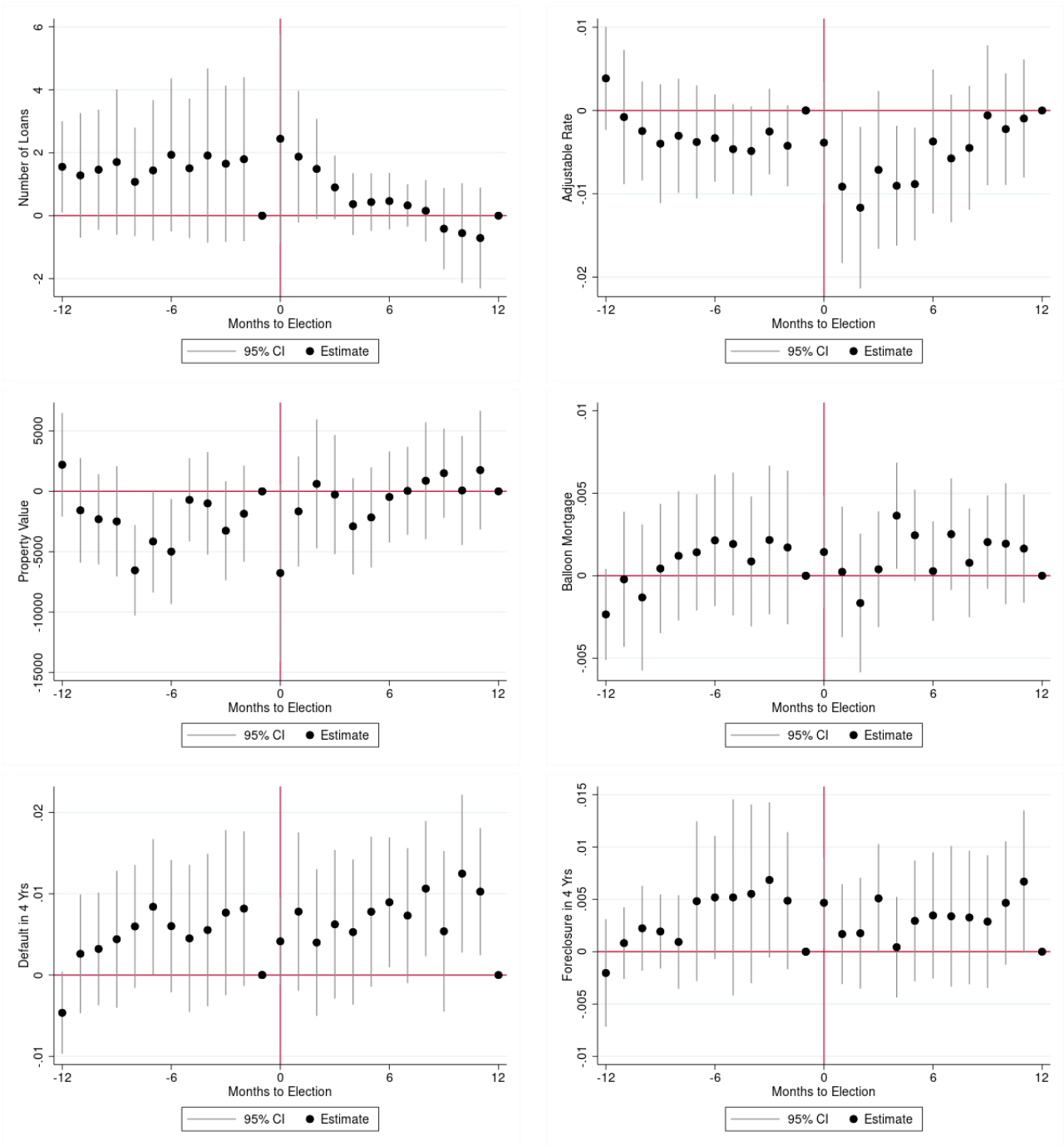Figure 10: Impact of AG Elections

Notes: Monthly event study graphs showing mortgage market outcomes before and after AG elections, using data at the zip-month level for the 24 month window around every AG election between 2004 and 2013, including controls for census variables at the zipcode level. Plotted in black are the coefficients δ_i from 3.1, with 95% confidence intervals in grey. The excluded category is the month prior to the election. Standard errors are clustered at the state level.

Table 14: Republican-to-Democratic Governor Switches

	(1)	(2)	(3)	(4)	(5)	(6)
	# of Loans	House Price	ARM Flag	Balloon Flag	Foreclosure	Default
After Election	-10.50	26418.8	0.00606	0.00675	-0.00645	-0.0184
	(-1.78)	(1.63)	(0.21)	(0.91)	(-0.69)	(-1.23)
After Party Switch	13.65	-23295.7	-0.00260	-0.00347	-0.00806	0.0121
	(1.70)	(-1.24)	(-0.07)	(-0.59)	(-1.38)	(1.07)
Mean Income	3.80e-4**	8.198***	2.51e-6**	4.43e-7**	-1.55e-7	-3.59e-7
	(2.90)	(4.70)	(3.62)	(3.36)	(-1.77)	(-1.33)
Unemployment	-0.304	-19167.6	-0.0106*	0.000687	0.000930	0.00227
	(-0.24)	(-1.55)	(-2.71)	(0.89)	(1.70)	(1.11)
Population	1.23e-6	0.0298***	8.35e-9*	1.51e-9	-9.57e-10	7.74e-10
	(0.94)	(4.06)	(2.46)	(1.36)	(-1.94)	(0.56)
Constant	8.776	-19328.9	0.0952	0.00909	0.0613***	0.140***
	(0.76)	(-0.15)	(1.61)	(0.66)	(7.33)	(5.36)
Observations	464558	464263	464558	464558	464558	464558

t statistics in parentheses

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

Table reports results from estimating equation 3.3. The coefficient of interest is labeled "After Party Switch." Treated states elect a Democratic governor after a Republican, while control states have Republican governors before and after the election. All regressions include state and time fixed effects. The "switch state" variable is collinear with fixed effects and excluded. Standard errors are clustered at the state level.

Table 15: Republican-to-Democrat AG Switches

| | (1) | (2) | (3) | (4) | (5) | (6) |
	# of Loans	House Price	ARM Flag	Balloon Flag	Foreclosure	Default
After Election	-12.73**	4293.0	0.00192	-0.00368	0.00726	0.0103
	(-3.85)	(0.36)	(0.11)	(-0.68)	(0.77)	(0.76)
After Party Switch	4.902	-13013.2	-0.0300	-0.000339	-0.0136	-0.00524
	(0.37)	(-1.45)	(-0.97)	(-0.07)	(-1.38)	(-0.93)
Mean Income	2.97e-4*	4.881***	1.49e-6**	3.35e-7	-6.20e-8	-5.97e-7*
	(2.22)	(6.56)	(3.16)	(1.77)	(-0.54)	(-2.56)
Unemployment	-2.933	-5213.7*	-0.0112*	-0.000734	0.00173	0.000868
	(-2.00)	(-2.18)	(-2.15)	(-0.77)	(1.79)	(0.36)
Population	8.73e-6*	0.0211*	1.88e-8*	5.83e-9**	5.78e-9	1.21e-8
	(2.53)	(2.74)	(2.68)	(3.47)	(1.23)	(1.52)
Constant	33.79*	-5289.3	0.152*	0.0343*	0.0552***	0.170***
	(2.66)	(-0.10)	(2.81)	(2.37)	(6.20)	(7.40)
Observations	275194	274980	275194	275194	275194	275194

t statistics in parentheses

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

Table reports results from estimating equation 3.3. The coefficient of interest is labeled "After Party Switch." Treated states elect a Democratic AG after a Republican, while control states have Republican AG before and after the election. All regressions include state and time fixed effects. The "switch state" variable is collinear with fixed effects and excluded. Standard errors are clustered at the state level.

	(1) \# of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
After Election	-7.129	30772.4***	0.00843	-0.00540	0.0147	0.0263
	(-0.92)	(3.95)	(0.54)	(-0.94)	(1.39)	(1.76)
After Party Switch	3.056	-33731.8*	-0.00766	0.00308	-0.0224	-0.0386
	(0.46)	(-2.20)	(-0.31)	(0.37)	(-1.36)	(-1.63)
Mean Income	2.07e-4	4.660***	1.38e-6***	1.11e-7	-1.23e-7	-4.00e-6
	(1.76)	(17.99)	(3.82)	(1.21)	(-1.15)	(-1.64)
Unemployment	-0.544	-2704.1	-0.000986	-0.0000524	0.00215	0.00507**
	(-1.51)	(-1.07)	(-0.47)	(-0.11)	(1.97)	(3.20)
Population	5.31e-6***	0.0356***	2.34e-8***	3.18e-9**	3.48e-9*	6.82e-9***
	(7.76)	(5.12)	(6.68)	(2.90)	(2.57)	(5.33)
Constant	77.74***	-148485.9***	0.0310	0.0296**	0.0751***	0.163***
	(8.07)	(-6.24)	(0.96)	(3.06)	(9.58)	(8.86)
Observations	295646	295526	295646	295646	295646	295646

t statistics in parentheses

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

Table reports results from estimating equation 3.3. The coefficient of interest is labeled "After Party Switch." Treated states elect a Republican governor after a Democrat, while control states have Democratic governors before and after the election. All regressions include state and time fixed effects. The "switch state" variable is collinear with fixed effects and excluded. Standard errors are clustered at the state level.

Table 17: Democrat-to-Republican AG Switches

	(1) # of Loans	(2) House Price	(3) ARM Flag	(4) Balloon Flag	(5) Foreclosure	(6) Default
After Election	-18.05*	6044.0	0.000488	-0.00732	-0.00351	-0.0164
	(-2.62)	(0.48)	(0.03)	(-1.55)	(-0.36)	(-0.92)
After Party Switch	12.07	-315.9	-0.0193	0.00416	-0.00831	-0.00316
	(1.37)	(-0.03)	(-0.90)	(1.16)	(-0.96)	(-0.25)
Mean Income	2.33e-4	7.658***	2.34e-6***	2.98e-7	-3.80e-7	-5.63e-7
	(1.44)	(6.43)	(7.01)	(1.85)	(-1.44)	(-0.97)
Unemployment	-2.018	-13294.6	-0.0110	0.000317	0.00138	0.00295
	(-1.56)	(-1.20)	(-1.76)	(0.35)	(1.63)	(1.22)
Population	1.17e-6	0.0273**	8.00e-09	1.52e-09	-2.39e-09*	-4.28e-10
	(1.03)	(3.65)	(1.64)	(1.63)	(-2.12)	(-0.21)
Constant	38.03	-28394.2	0.132*	0.0315	0.0723**	0.158**
	(1.90)	(-0.31)	(2.44)	(1.91)	(3.17)	(2.85)
Observations	425214	425033	425214	425214	425214	425214

t statistics in parentheses

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

Table reports results from estimating equation 3.3. The coefficient of interest is labeled "After Party Switch." Treated states elect a Republican AG after a Democrat, while control states have Democratic AGs before and after the election. All regressions include state and time fixed effects. The "switch state" variable is collinear with fixed effects and excluded. Standard errors are clustered at the state level.