Elections decrease Financial Regulatory Activity: Evidence from Investment Adviser Disclosures

By Paul Berenberg-Gossler
Gonçalo Pina*

This paper shows that elections decrease regulatory actions against misconduct in the financial industry. In the United States, regulation of financial misconduct is carried out by three types of regulators: federal, state, and self-regulatory. Most state-level financial regulators are directly appointed by state governors. We analyze a new database on regulatory actions covering 49 US states from 1990 until 2019, to assess whether state-level electoral incentives affect regulatory activity. Exploiting exogenous electoral cycles of US gubernatorial elections, we find causal evidence that state-level financial regulators reduce regulatory activity starting four months before gubernatorial election events. This slump occurs even earlier if elections are contested. Federal and self-regulatory regulators do not punish less in response to gubernatorial elections, suggesting a direct link between gubernatorial elections and state regulators. Using data on the average duration of preparation for each case, we show that there is a significant rush to finish regulatory actions up to five months prior to gubernatorial elections. We find no evidence for a catch-up effect after gubernatorial election events. These results underscore the persistence of electoral cycles in financial regulation.
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Regulatory activity plays a crucial role in the functioning of financial markets. However, government officials and regulators often face pressure from constituents and special interests that may lead to deviations from optimal regulatory policy. These pressures may be particularly large around election events, when the jobs of policy makers are on the line. This paper uses a novel database on regulatory actions against misconduct in the investment advisor industry, an industry that has been at the forefront of recent financial crises, and shows that state gubernatorial elections decrease regulatory financial activity in the United States of America.

The US states gubernatorial elections provide an almost ideal laboratory to test the presence of electoral cycles in financial regulatory activity for three reasons. First, the regulatory framework of investment advisers is well-developed

* Berenberg-Gossler: German Institute for Economic Research (DIW), Mohrenstraße 58, 10117 Berlin, Germany., paul.berenberg-gossler@outlook.com. Pina: ESCP Business School, Heubnerweg 8-10, 14059 Berlin., gpina@escp.eu. We thank Henrik Enderlein and Mark Kayser for helpful discussions, suggestions, and input. We thank Meryem Masmoudi for excellent research assistance.

and relatively standardized across states. Because many provisions, legal code, and bureaucratic procedures are common across states, we can focus on the application of regulations, the intensive margin of regulatory policy, when testing for electoral cycles. Second, the administrative structure of financial regulation provides a natural falsification test for gubernatorial election cycles. US financial regulation is composed of federal, state, and self-regulating regulators. Thus, we can use the regulatory activity of federal and self-regulating regulators as a natural falsification test, as these regulators should not be affected by gubernatorial elections. Third, there is substantial variation in the timing of predetermined gubernatorial elections across states within the US. Under the assumption that election dates are exogenous with respect to regulatory activity, we can estimate the causal effect of electoral cycles on regulatory activity by comparing states with gubernatorial elections to states without.

Two institutional features suggest the potential presence of electoral cycles in regulatory activity at the US state-level. First, most state-level financial regulators are appointed by state governors, or elected at the same time as governors. Second, the investment advisor industry is highly regulated and the role of state regulators has been largely extended during the Dodd-Frank regulatory overhaul in 2010. Unsurprisingly, the financial sector is often at the center stage of election campaigns and public discussion. It is also a large campaign contributor.[1]

Our main hypothesis is that upcoming gubernatorial elections alter state-level political pressure, which in turn affects financial regulatory activity against the investment advisor industry. The investment advisor industry is relatively concentrated when compared to its costumers, suggesting that there is no natural counterbalance in terms of political pressure, in particular outside of financial crisis episodes. Therefore, we hypothesize that before elections an increase in political pressure from the investment advisor industry decreases regulatory activity by policy makers. This hypothesis is related to a large literature on how elections affect macroeconomic and regulatory policy (Stigler, 1971; Nordhaus, 1975; Peltzman, 1976; Rogoff and Sibert, 1988; Akhmedov and Zhuravskaya, 2004; Cahan, 2019).[2] It is also related to a more recent and growing literature that focuses on financial policy (Mian, Sufi and Trebbi, 2010, 2013; Müller, 2019), where politicians influence financial policy to increase their chances of winning reelection. Although political pressure is not always observable, we can observe regulatory actions directly using publicly available data.

To test our hypothesis, and in contrast to the previous literature, we use newly assembled monthly data on regulatory actions from US regulators over the 1990-2019 period against the investment advisor industry.[3] Our data is obtained from the US Securities and Exchange Commission (SEC) Investment Adviser Public

[1] The database followthemoney.org shows that the industries Commercial Banking and Securities & Investment are both featured in the top ten largest contributors to gubernatorial campaigns.

[2] See Dubois (2016) for a review.

[3] The previous literature focuses on de jure measures of financial regulation, for example hand coded indices of legal restrictions, and explores the data at a lower frequency and using cross-country variation.

Disclosure (IAPD) database. It covers 9690 regulatory actions across 49 states over the 1990-2019 period. We go through an extensive data cleaning process as described in Berenberg-Gossler and Pina (2020) to avoid double-counting and to attribute the regulatory action to the relevant US state. Our identification strategy exploits variation in exogenous gubernatorial election dates across US States to analyze how regulatory intensity changes before and after state-level gubernatorial elections.

Econometrically, we use a difference-in-difference setup, comparing state-level regulatory actions in states with upcoming gubernatorial elections to states without upcoming elections. To do so, we lag and lead dummies indicating the occurrence of state-level gubernatorial elections. It is important to use monthly data in order to capture short-run electoral cycles and because we do not observe the exact date of the intervention. Although we capture the exact date of the gubernatorial election, most of the impact may be felt before, either around a primary election or during campaign fundraising season. Furthermore, after an election, policy makers do not immediately take office. Using monthly data allows us to include multiple lags and leads around election events and capture such effects. Our dependent variable is the conviction rate, defined for each state as the absolute number per month of regulatory actions, by each regulatory level, normalized by the number of employees in the financial sector in that state. The panel data structure of our data allows accounting for time and US state fixed effects with clustered standard errors.

Our main contribution is to show that exogenous gubernatorial elections causally decrease the conviction rate by state regulators in the run-up to the election. We first show results for a simple event-study regression with only state- and time-fixed effects. In our baseline regressions, we include also state-level controls that increase the size and precision of the estimates for the electoral cycle. The effect of elections on state-level regulatory activity is consistently negative starting 5 months before the election. After the election, signs alternate between positive and negative. The effect size is largest two months before the election, when the average conviction rate is reduced by -2.3 per thousand workers, about 16% of the monthly standard deviation in conviction rates by state regulators. This effect is not always precisely estimated. However, 4 months before the election, when it equals -1.7 per thousand workers, it is significantly different from zero at the 95% confidence level. Furthermore, we can reject a joint significance test that all coefficients six months before the election are equal to zero at the 90% confidence level. However, we can not reject that all coefficients six months after the election are equal to zero.

Turning to federal regulators and self-regulatory agencies, we do not observe any effect of elections on conviction rates. The coefficients associated with the time period prior to gubernatorial elections are small, indistinguishable from zero at traditional significance levels, and show alternating signs without a clear pattern. This natural placebo test suggests a mechanism by which gubernatorial elections affect regulatory activity via state regulatory agencies only, which is consistent with the view that political pressure is driving the reduction in regulatory activity by these agencies.

Our findings are obtained after controlling for a variety of state-specific factors including leads and lags for senatorial elections, electoral competitiveness of gubernatorial elections, party affiliation of the incumbent governor, whether the election yields a party change, the type of securities law adopted by the state, shutdowns in state government, a state-level recession dummy, and the unemployment rate at the state level. Beyond these controls, we also correct for potential auto-correlation and include lags of the dependent variable in the baseline specification. Our results are robust to including different numbers of lags or no lag at all. Notably, the effect of elections on regulatory activity is not present for senatorial elections, at any level of regulatory activity. This provides a second natural placebo test that highlights a specific channel from gubernatorial elections and state regulators.

We then show that for contested elections, the negative effect on conviction rates is felt already five months before the election. However, most of this effect is transferred from four months before the election, such that the overall effect of a gubernatorial election on conviction rates at the state level is similar for both contested and non-contested elections.

Finally, we look at the sample of completed regulatory actions and study the effect of elections on the average duration of preparation for each case before its resolution. It is important to note that we do not observe when the case work started, instead we only observe when the investment advisor was first notified of the investigation and when the regulatory action was concluded. Taking the measure of case duration as the difference between these two dates, we show that average case duration is significantly smaller in the run-up to the election. Coefficients prior to the election are negative, while signs alternate between positive and negative after the election. We can reject a joint significance test that all coefficients six months before the election are equal to zero at the 99% confidence level.

This paper relates to three strands of the literature on financial regulatory activity. First, it relates to literature studying regulatory cycles in financial regulation. Current explanations focus on the fact that financial innovation is often ahead of regulators (Claessens, Ratnovski and Singh, 2012; Claessens and Kodres, 2014) and that regulations and regulatory activity are procyclical (Dagher, 2018; Almasi, Dagher and Prato, 2018; Berenberg-Gossler and Pina, 2020).

Second, it relates to a strand of literature proposing private interest theories of financial regulation. Benmelech and Moskowitz (2010), for instance, show that private interests drive greater financial regulation, as wealthy political incumbents seek to protect their own interests. Kroszner and Strahan (1999) also argue in this direction. Several other papers further investigate the decision making by financial regulators. These papers focus either on lobbying by banks or firms

(Lambert, 2019; Correia, 2014; de Figueiredo Jr and Edwards, 2007) or on incentives and institutional designs influencing individual decisions by regulators (Agarwal et al., 2014; Kisin and Manela, 2018). Tenekedjieva (2020) highlights a mechanism through which future job opportunities for insurance commissioners affect regulatory activity of insurance firms. However, these papers do not explicitly link to electoral effects. See Dal Bó (2006) for a review on regulatory capture that covers also non-financial industries.

Third, our paper is most closely related to literature suggesting electoral influence on financial regulatory agencies and their decisions-making as a potential explanation for cycles in regulatory activity. Akey, Heimer and Lewellen (2020) show that powerful senate members influence consumer lending to communities protected by fair-lending regulations. Mehta (2017) shows that SEC enforcement actions against a constituent firm is negatively associated with the likelihood that a congressional politician is subsequently reelected. Our paper is also connected to recent evidence that elections reduce the probability of interventions on failing firms by insurance commissioners (Leverty and Grace, 2018) or in banks (Liu and Ngo, 2014; Brown and Dinc, 2005). Leverty and Grace (2018) and Liu and Ngo (2014) use a similar empirical strategy using US state or US senate elections, but focus their analysis on the resolution of failing insurance firms or banks using yearly data. Our paper uses monthly data and focuses on regulatory activity of investment advisers by different regulators and how they respond to gubernatorial elections.

The rest of the paper is structured as follows. Section I explains the data used in this paper. Next, we outline our empirical strategy and identifying assumptions in section II. Section III shows the main result that gubernatorial elections reduce regulatory activity by state regulators. Section IV shows that senatorial elections have no effects on regulatory activity. Section V investigates the role of contested elections, while section VI concludes.

I. Data

Data on regulatory actions come from the US Securities and Exchange Commission (SEC) Investment Adviser Public Disclosure (IAPD) database. The SEC raw data cover all registered and unregistered US investment advisers. The SEC defines investment advisers as individuals or firms giving advice about securities. Our data captures annual filings of roughly 24,822 registered and unregistered investment advisors.[4] These firms need to regularly file an Investment Adviser Disclosure Form with the SEC. Notably, investment advisers need to indicate any prior regulatory action they have been subjected to in response to any kind of misconduct. In the raw data, firms need to indicate the exact start date, e.g. the date they have been first informed about an ongoing regulatory action, and the

[4]Investment Advisers is a relatively broad generic term. We capture firms that consist of solely one employee, as well as banks, asset managers, and other types of investment funds in our data.

date at which the action has been resolved. Further, the data also cover many other aspects, such as the monetary fine attached to each action, the reason for the regulatory action, or the regulator in charge.

We go through an extensive data cleaning process to correct for two major drawbacks of the raw data for our purposes. First, firms are organized according to the location of their headquarter and not where the regulatory action took place. Thus, a Californian subsidiary of a firm headquartered in New York that has been fined by a Californian regulator will still be geographically attributed to New York. We employ a "set of keywords" strategy and re-attribute regulatory actions to their original geographical location, based on all available information in the data. Notably, we exploit jurisdictions of regulators and the manually entered text fields summarizing allegations or the regulatory action itself. Second, the raw data are well known to suffer from double-counting because regulators tend to inflate their numbers to maintain their budget (Velikonja, 2015, 2017). For each state, we only keep final regulatory actions with unique starting dates and monetary amounts. Given that we have exact daily start and end dates for each regulatory action, it is unlikely that duplicate entries remain in our clean dataset. Finally, we aggregate our dataset at the monthly state level. For a detailed overview of the data, our approach and the text-mining algorithm employed see Berenberg-Gossler and Pina (2020).

Next, we build our measure of regulatory actions that corrects for the fact that states with a larger financial sector tend to see a greater number of regulatory infringements. We retrieve monthly data from the Bureau of Labor Statistics (BLS) on employees per sector and state. We define a *Conviction Rate* based on the absolute number of convictions per month per employee in the financial sector:

$$ConvRate_{r,s,t} = \frac{Conv_{r,s,t}}{E_{s,t}} \tag{1}$$

where $Conv$ is sum of the number of actions, the convictions, by regulator r in state s at time t. We have three types of regulators in our data: federal, state, and self-regulatory. We normalize the sum of convictions per US state by $E_{s,t}$, the number of employees in the financial sector of the respective state s at time t.

We also use information on the average duration it takes to build a case. We compute the average length (in weeks) in between the start and end date of each regulatory action. Start date refers to the exact date at which a firm has been first informed about an ongoing regulatory action. Note that start dates do not account for any investigations prior to notifying the firm. This variable is, thus, potentially downward biased because it might take some time to build a case prior to the firm being notified. End dates correspond to the exact date at which the regulatory was resolved. Our *Duration* variable is defined as follows:

$$(2) \qquad Duration_{r,s,t} = \frac{\sum_{c=1}^{c=n} [EndDate_{c,s,i,t} - StartDate_{c,r,i,t}]}{Conv_{r,s,t}}$$

where $EndDate_{c,r,s,t}$ and $StartDate_{c,r,s,t}$ are the resolution and start dates of each regulatory action c, enumerated between 1 and n, carried out by regulator type r in state s at time t. We divide the sum of weeks of all cases by the sum of the number of actions, the convictions, of regulator r in state s at time t. Thus, we simply calculate the average duration it takes to build each case that was resolved in each month, by state and regulator type.

Next, we collect data on all gubernatorial and senatorial elections over the 1990-2019 period. For all election events we also include special elections. Overall, we have 391 gubernatorial and 505 senatorial election events. Our primary source for gubernatorial election dates and outcomes is the Council of State Governments' *Book of the States*, which publishes information on past and upcoming gubernatorial elections. We augment these data with information on election outcomes from news sources and Dave Leip's atlas on US elections. Our main source for senatorial elections is the MIT Election Data and Science Lab (Data and Lab, 2017), which we augment with data on special senatorial elections from news sources.

There is substantial heterogeneity in the timing of election events across states, due to pre-determined scheduling differences or special elections. The main source of heterogeneity is that some states are aligned with presidential elections, others occur during midterm elections, while other have elections one year before the presidential election. Some states have shorter gubernatorial terms.[5] Special elections occur, for example, because incumbents decease or resign while holding office. We have two special gubernatorial elections in our data. Most gubernatorial and senatorial elections are held in November. However, two gubernatorial elections were held in June and October.[6] There is a large literature using these staggered cycles to identify empirically the effect of elections on economic outcomes (Cahan, 2019; Leverty and Grace, 2018).

The data also comprise eight special senatorial elections events that take place at various months.[7] Some states (Louisiana, California, Washington) have a top two primary system for their gubernatorial elections, where all candidates appear on the ballot for the first round. For these states we count the second round

[5]New Hampshire and Vermont have two-year cycles, Kentucky, Louisiana, Mississippi, New Jersey, and Virginia elect their respective governors during off-year elections.

[6]The California recall election of 2003 was held in October and the 2012 Wisconsin recall election was held in June.

[7]Special senate elections that were not held at the same time as regular elections are as follows: Pennsylvania November 1991; North Dakota December 1992; Texas June 1993; Oregon January 1996; Massachusetts January 2010; Massachusetts June 2013; New Jersey October 2013; Alabama December 2017.

election date, because in most of the contests it is clear which party is going to end up in the second round. We also abstract from primary elections in our analysis. Out of the 391 gubernatorial elections in our data, there are 148 election events that do not take place on the same date as any senatorial election. Figure 2 shows the distribution of gubernatorial and senatorial election cycles. It plots the number of states that have a gubernatorial election (top panel) or senatorial election (lower panel) per month.

We also add information on electoral competitiveness, which we define as the margin of victory between the first two largest parties of a gubernatorial election. Electoral competitiveness also exhibits large heterogeneity across states. On average, first and second contenders are separated by roughly 16 percentage points. However, some elections were extremely close. The 2004 Washington gubernatorial election, for instance, had a margin of victory of 0.005 percentage points. On the other end of the spectrum is the 1996 Montana gubernatorial election with a landslide victory of the Republican party by margin of victory of 58.4 percentage points. Figure 1 shows the occurrence and outcome of each gubernatorial election in each state.

Additional control variables come from various sources. First, we gather data on federal and state government shutdowns over the 1990-2018 period using mostly newspaper articles. Second, we gather data on state-level securities legislation, which might potentially influence our results. State-level legislation is mainly based on the Uniform Securities Acts. These acts are model statutes drafted by the National Conference of Commissioners on Uniform State Laws (NCCUSL) that may serve as template to help states write their own state securities laws. Since the 1950s many states have adopted different versions of these statutes. Currently, there are four possible state-level regulatory frameworks in place. (1) The 1956 Uniform Securities act; (2) the 1985 Revised Uniform Securities act; (3) the Uniform Securities Act of 2002; (4) or distinct state-specific laws, for instance in California and New York. Based on multiple sources, mostly different editions of Rapp, Sowards and Hirsch (2020), we collect data on the adoption of different Uniform State Laws over our total sample period and create a full set of dummy variables.

Finally, we use data on the business cycle. First, we collect state-level monthly data on unemployment rates originating from the BLS. Second, because real GDP is not available at monthly frequency, we use the coincident economic activity index as an alternative measure of the business cycle. The coincident index is available for each state at the monthly frequency (Crone and Clayton-Matthews, 2005). It is based on four economic indicators: non-farm payroll employment, the unemployment rate, average hours worked in manufacturing, and real wages and salaries. The trend for each state's index is set to match the trend for real gross state product. Based on the coincident index we construct an indicator of state-level recessions as in Crone and others (2006).[8]

[8]To be in a recession, two conditions have to be met. First, the cumulative decline in the state's

Table 1—: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Conviction Rate All	17,346	4.56	15.17	0	0	2.7	392
Conviction Rate Federal	17,346	0.30	2.38	0	0	0	177
Conviction Rate SRO	17,346	0.88	3.52	0	0	0	93
Conviction Rate State	17,346	3.39	14.52	0	0	0	392
Gub. Election Dummy	17,346	0.02	0.15	0	0	0	1
Gub. Election Competitiveness	16,224	0.16	0.13	0.00	0.05	0.22	0.58
Sen. Election Dummy	17,346	0.03	0.17	0	0	0	1
State Gov. Shutdown (days)	17,346	0.01	0.55	0	0	0	31
State Unemployment Rate	17,346	5.46	1.85	2.10	4.10	6.50	14.60

This table shows summary statistics of our sample. Conviction Rates are multiplied by 1000 for better readability.

Summary statistics of all variables are presented in table 1. Mean conviction rates differ largely depending on the type of regulator. State regulators tend to have a larger number of regulatory actions, followed by self-regulatory organizations and federal regulators. There is also substantial variation in conviction rates for different state-levels and over time. Out of the 391 gubernatorial election events in the data, 101 were associated with a change in the ruling party. We create a variable measuring electoral competitiveness for each gubernatorial election based on the margin of victory of the first relative to the second largest party. To proxy electoral competitiveness in a particular state, we carry the electoral competitiveness variable forward. That is, we assume that states and regulators base their priors on how close the upcoming election will be on the result of the previous election.[9] State government shutdowns are a rare event. Over the total sample period of 1990-2019, states experience less than a day of state government shutdowns per month, with a maximum of 31 days.

II. Hypotheses and empirical strategy

This section first presents the empirical framework we use to test for electoral cycles under the assumption that elections are exogenous treatments. Next we test whether the control and treatment groups follow parallel trends in the absence of treatment.

coincident index must be at least 0.5 percent, which is the smallest decline in the national index for any recession in the last quarter century. Second, the period from the state index's peak to its trough must be at least three months.

[9]We also estimated our main results with electoral competitiveness carried backwards. Our results are robust to this change.

A. Test for electoral cycles

Our identifying assumption is that the timing of gubernatorial elections is exogenous with respect to conviction rates. We use monthly data to estimate the following equation:

$$y_{s,t} = \sum_{j \in \{-6;6\}} \alpha_j g_{j,s,t} + \beta y_{s,t-1} + \tau_t + f_{s,m} + controls_{s,t}\gamma + \epsilon_{s,t} \tag{3}$$

where y stands for the regulatory outcome, s identifies the state, and t is time measured at the monthly frequency. We consider the following regulatory outcomes for each regulatory level (state, federal or self-regulatory): (i) conviction rates, and (ii) mean duration of completed regulatory actions. To control for US-level shocks, we include a full set of time controls τ_t. To account for state-specific fixed effects and state-specific seasonality, that may be related to regulatory requirements at the state level, we include state-month fixed effects $f_{s,m}$, where m identifies each month in a calendar year from January to December.

One potential concern is that the data on regulatory activity is autocorrelated. We include one lag of the dependent variable for simplicity, although our results are robust to using a different number of lags. State-specific observable controls measured at t are included in the matrix $controls_{s,t}$ and captured by the vector of coefficients γ. As controls variables, we include dummies for the five different types of state-level regulatory frameworks that are relevant for financial advisers. Note that changes in federal laws may also be important, however these are subsumed by the time fixed effects. Additionally, we control for the economic cycle using the unemployment rate measured at the state level, and using a dummy variable that captures whether the state is in a recession as explained in the data section I. We also control for state-level government shutdowns, measured as number of shutdown days per month. Government shutdowns may cause sudden reductions in regulatory activity. We also control for the party affiliation of the current governor in office to capture partisan effects. Finally, we control for one specific situation that arises after an incumbent governor lost her seat. Because new governors are only sworn in two to three months after the actual election date, the incumbent or the incumbent administration might wait for the new governor to take significant action. All our variables are measured at the monthly frequency.

Our main coefficients of interest are given by α_j, with $j \in \{-6; 6\}$, which capture the effect of electoral cycles on regulatory activity. The variable $g_{j,s,t}$ is a dummy variable that takes on the value of one if t is j months away from a gubernatorial election. We can identify these coefficients because there is substantial cross-state variation in gubernatorial election dates. Therefore, we employ a difference-in-difference estimator that compares regulatory activity in the treated group (states with a gubernatorial election) to the control group (states without a gubernatorial election). Under the assumption that elections are exogenous, the assignment between treatment and control is exogenous in our data and our estimates indicate causal effects of election on regulatory outcomes. The panel data structure of our data allows accounting for time and state-month fixed effects that control for unobserved differences over time, and unobserved, state and seasonal specific differences across states. Our estimates for the effects of elections are then obtained from within-state and within-time changes in regulatory activity. Standard errors are clustered at state level.

Our choice of $j \in \{-6; 6\}$ is driven by election scheduling. Although most gubernatorial elections happen every four years, New Hampshire and Vermont, for instance, have gubernatorial elections every two years in even numbered years. Rhode Island also had gubernatorial elections every two years until 1994, but then changed its election cycle to four years. Using a larger window would create an overlap between lags and leads for these elections. This issue is also present when distinguishing the effect of gubernatorial elections from presidential and midterm senatorial elections. For example, Kentucky, Louisiana and Missouri have gubernatorial elections one year before the presidential election while New Jersey and Virginia have gubernatorial elections one year after. The United States midterm elections, which are held around the midpoint of the president's four-year term of office, include all 435 seats in the House of Representatives and 33 or 34 of the 100 seats in the Senate. Again, having a larger window would create an overlap between lags and leads for different elections, making it difficult to identify the effect of gubernatorial elections on state-level regulatory activity. Although most elections are held on election day, defined as the first Tuesday after November 1st, some special elections are held on other dates. We opt then for using a six-month window before and after the election month to balance the identification of the electoral cycle with the need to minimize overlap between elections, and, therefore, focus our attention on the short-run effect of electoral cycles on regulatory activity.

For our baseline dependent variable, the conviction rate, positive estimates of α represent increases in regulatory activity, while negative estimates imply decreases. If α is negative before an election and positive after an election, this would indicate an electoral cycle that delays regulatory actions. If α is positive before an election and negative after an election, this would indicate an electoral cycle that anticipates regulatory actions. If α is zero throughout, this would be indication of no effect of gubernatorial elections on regulatory activity.

To identify the effect of gubernatorial elections on regulatory activity at the state level, we run regressions for all levels of regulators: state, federal and self-regulatory. If the effect of gubernatorial elections is only present for regulatory activity by state regulators, but not for the two other levels of regulators, this suggests that the causal effect of gubernatorial elections on regulatory activity only affects state regulators. This would be consistent with the institutional features of regulators at the state level, most of which are appointed by governors or elected at the same time as governors.

One challenge for identification of gubernatorial election effects is that timing potential coincides with presidential and congressional elections. Presidential elections are captured by the time-fixed effects as they affect all states at the same time. However, to further distinguish between the effect of gubernatorial and congressional elections we also estimate the following regression:

$$y_{s,t} = \sum_{j \in \{-6;6\}} \alpha_j g_{j,s,t} + \sum_{j \in \{-6;6\}} \gamma_j c_{j,s,t} + \beta y_{s,t-1} + \tau_t + f_{i,m} + controls_{s,t}\phi + \epsilon_{s,t} \tag{4}$$

where all variables are defined like in equation (3), except the coefficients ϕ_j which capture the effect of congressional electoral cycles on regulatory activity. We focus on US Senate elections in our analysis and do not control for elections for the House of Representatives in order to include only state-wide elections. The variable $c_{j,s,t}$ is a dummy variable that takes on the value of one if t is j months from a senatorial election. Coefficient estimates γ capture the senatorial election regulatory cycle. More importantly, estimates for α from equation (3) are now solely based on gubernatorial elections that happen at different dates than senatorial or presidential elections.

B. Parallel pre-trends assumption

One key-assumption underlying our identification strategy in equation (3) is that our control and treatment groups follow parallel trends in absence of treatment. Thus, in absence of gubernatorial elections, conviction rates should change in the same way in states with upcoming elections as in states that do not have upcoming election events. This section performs a number of tests to verify whether this assumption holds in our setting. We present four facts that strongly suggest that the pre-trends assumption holds in our empirical setup.

First, we visually compare trends of conviction rates in treated with untreated states. Figure 3 plots mean conviction rates by month for treated and untreated states for all three regulatory levels in the data. Our main interest is the pre-election period. We plot a 11-month window before and after the election event, because it represents the longest time period in between gubernatorial election cycles across states in our sample. We start by inspecting the levels of conviction rates for treated and untreated groups because similarity in levels makes the parallel trend assumption more likely to hold (Kahn-Lang and Lang, 2020). State, federal, and self-regulatory regulators start with very similar levels at $\tau - 11$. For state-level regulators, mean conviction rate for the treatment group is, on average, slightly higher compared to the control group. For federal and self-regulatory regulators, it is the control group that is slightly higher. However, the difference in levels is marginal and largely below one per one thousand. In equation (3) time controls τ_t and state-month fixed effects f_{sm} account for this difference by controlling for all common shocks and constant time-invariant heterogeneity across states and months.

Second, we focus on the pre-trends itself. Conviction rates of control and treatment group show an upward trend for state and federal regulators. In both cases, trends of control and treatment group prior to gubernatorial elections clearly pass the visual inspection. However, control and treatment groups for self-regulatory regulators show less of a common trend. Because large differences in trends among control and treatment groups could render inference imprecise or erroneous, we follow a cautious approach when interpreting results on self-regulatory regulators. The main focus of this paper are then state and federal level regulators.

Third, the parallel trends assumption is also supported by a number of other tests. Our treatment and control groups pass many sensitivity tests suggested in Roberts and Whited (2013).[10] First, our sample has multiple treatment and control groups which reduces noise and biases related to just one comparison. Differences across our treatment and control groups arise naturally due to special elections and different election cycles that occur on distinct dates in different states (see also figure 2). Next, the timing of behavior change in state level conviction rates is clearly occurring right before treatment.

Fourth, we additionally employ three types of falsification tests. The first two are placebo tests based on the hypothesis that variables that should not be affected by the event are unaffected by it. Section III tests the effect of gubernatorial elections on federal and self-regulatory regulators. Section IV tests the effect of senatorial election on conviction rates of different regulators. Both tests suggest that gubernatorial elections only alter the behavior of state level regulators. The last falsification test shifts the leads and lags of the treatment variable, the indicator for a gubernatorial election, by 24 and 36 months, respectively. Statistically insignificant results for this exercise would represent another strong indication that our results are due to changes in the treatment variable. Results of this exercise can be found in the appendix.

We describe the results for the placebo tests more carefully in the next sections. For now, we note that all estimates of the placebo treatment effect on regulatory deliver coefficients with alternating signs, and the tests of joint significance of coefficients are statistically indistinguishable from zero in the run-up to gubernatorial elections.

III. Main result: gubernatorial elections decrease regulatory activity

In this section, we present evidence that upcoming gubernatorial elections decrease regulatory actions by state-level regulators. Table 2 presents the results of estimating equation (3). Column (1) shows the effect of a gubernatorial election cycle on conviction rates at the state-level. The coefficients before the election

[10]Leverty and Grace (2018) have a very similar empirical setup. Thus, our study design naturally fulfills many of the elements they test in their study to verify the parallel trends assumption.

are all negative or close to zero. The first statistically significant decrease in state-level regulatory actions takes place four months prior to the election. The effect is even larger two months before the election, but this effect is not finely estimated. Two months after the election, there is another significant decrease in regulatory actions at the state-level. However, coefficients after the election alternate between positive and negative coefficients. Column (2) includes additional control variables, potentially affecting the dependent variable. Controls slightly change the magnitude and precision of the esitmates. The effect four months prior to elections increases in magnitude, while the effect two months after the gubernatorial election loses statistical significance.

Additionally, for each pre- and post-treatment period, we run joint-hypothesis tests to verify whether the coefficients of pre- and post-treatment periods are zero. Columns (1) and (2) present F-statistics for the pre- ($\tau_{-6} + \tau_{-5} + \tau_{-4} + \tau_{-3} + \tau_{-2} + \tau_{-1} = 0$) and post-treatment period ($\tau_{+1} + \tau_{+2} + \tau_{+3} + \tau_{+4} + \tau_{+5} + \tau_{+6} = 0$). Without controls the corresponding p-value for the pre-period is 0.12. With controls the p-value decreases to 0.08. Thus, we can reject that estimated coefficients on the pre-period are equal to zero at the 90% confidence level. P-values for the post-treatment period do not permit to reject the hypothesis that all coefficients are equal zero.

Conviction rates at the state-level are also shaped by the origin of the regulatory framework in place, the type of Uniform Securities Act, and state-level unemployment rates. In particular we highlight that the adoption of regulatory frameworks tends to increase regulatory activity by state-regulators, and decrease by federal and self-regulatory agencies.[11] Furthermore, the state unemployment rate is a crucial determinant of regulatory activity again solely affecting state-regulators. These results indicate that regulatory activity by state-regulators is malleable when compared to other regulators, with respect to institutional frameworks, economic conditions, and, as shown in this paper, to elections.

Columns (3) to (6) run placebo estimates and estimate the response of federal- and self-regulatory conviction rates to the gubernatorial election cycle. The estimates are either not significant at conventional levels of statistical significance or very small in magnitude. For instance, column (6) depicts that conviction rates of self-regulatory regulators also decrease three months prior to gubernatorial elections. However, there is no clear pattern of coefficient signs before elections. Joint-hypothesis tests on pre- and post-treatment periods in columns (3)-(6) do not permit to reject the hypothesis that all coefficients are equal to zero at conventional levels. This is a another strong indication that gubernatorial election

[11] This effect occurs relative to state-specific regulatory frameworks which serve as a base category in our estimations. States that have state-specific regulatory frameworks in place are as follows: Arizona (1990-2019), California (1990-2019), Florida (1990-2019), Georgia (1990-2009), Illinois (1990-2019), Louisiana (1990-2019), New York (1990-2019), North Dakota (1990-2019), Ohio (1990-2019), and Texas (1990-2019). Note that the regulatory framework with different vintages may be adapted by states at a later time. It is not the case that, for example, the dummy Regulatory Framework UA 1985 captures only adoptions in 1985.

events affect only state regulators, and not federal or self-regulatory agencies.

Next, we take the sample of resolved cases and explore if the average duration of cases depends on gubernatorial elections. We are interested whether there is a rush towards finishing regulatory actions in presence of elections. Columns (1) and (2) of table 3 present estimates of the average duration of cases conditional on the occurrence of at least one regulatory action. Estimates suggest that there is a significant decrease in the average duration of cases four months prior to gubernatorial elections. Coefficients before the election are all negative. After the election, they alternate between positive and negative. Joint-hypothesis tests again reject that pre-treatment period coefficients are equal to zero, this time at the 99% confidence level. However, they do not permit to do so for the post-treatment period.

IV. Placebo: Senatorial elections

Given that most governors directly or indirectly appoint state-level regulators, results from section III suggest a plausible direct link between state-level election cycles and regulatory actions by state regulators. To test the robustness of this result, this section uses elections for the US Senate as a placebo test. These elections also vary at the state level and do not coincide completely with gubernatorial elections. If we find no effect of senatorial elections on regulatory activity by state regulators, this would be further evidence for the causal link between the gubernatorial election cycle and regulatory activity by state regulators. We run the regression given by equation (3), but replace the gubernatorial election variable with dummies for senatorial elections.

Table 4 shows results for upcoming senatorial elections. Note that all estimates include time fixed effects that account for common shocks to all states, such as presidential elections. Additionally, all estimates also include state-month fixed effects accounting for time invariant state heterogeneity and potential seasonality trends linked to specific months. Columns (1) and (2) indicate that senatorial elections have no effect on conviction rates. The coefficients are small and statistically indistinguishable from zero. Interestingly, columns (3)-(4) show a decrease of regulatory activity by federal regulators in senatorial election months, and an increase after senatorial elections. Columns (5)-(6) show that senatorial elections do not have significant effects on regulatory activity of self-regulating regulators. Additionally, for all estimates, we can not reject that all coefficients six months before the election are equal to zero. After the election, we can reject only for federal regulators that all coefficients are equal to zero at the 95% significance level.

Finally, we estimate equation (4), including both gubernatorial and senatorial elections. Identification is now solely obtained from gubernatorial elections that do not coincide with senatorial elections. Table 5 confirms all main results of section III. In particular we confirm the negative effect of gubernatorial elections on regulatory activity by state regulators, with no clear effect on other regulators,

Table 2—: Gubernatorial elections and regulatory actions: Panel "within" Estimates Conviction Rates

	Conviction Rate State	Conviction Rate State	Conviction Rate Federal	Conviction Rate Federal	Conviction Rate Self-Regulatory	Conviction Rate Self-Regulatory
	(1)	(2)	(3)	(4)	(5)	(6)
Month $\tau-$ 6	0.480	0.048	-0.167^{*}	-0.152	0.809	0.991^{*}
	(1.099)	(1.240)	(0.086)	(0.093)	(0.504)	(0.586)
Month $\tau-$ 5	-0.969	-1.052	0.143	0.194	0.224	0.375
	(1.154)	(1.176)	(0.154)	(0.173)	(0.303)	(0.350)
Month $\tau-$ 4	-1.554^{**}	-1.667^{**}	0.202	0.259	0.531	0.572
	(0.641)	(0.748)	(0.271)	(0.323)	(0.479)	(0.579)
Month $\tau-$ 3	-0.154	0.008	-0.215	-0.179^{*}	-0.441^{*}	-0.520^{*}
	(0.897)	(1.054)	(0.135)	(0.105)	(0.251)	(0.287)
Month $\tau-$ 2	-1.884	-2.307	0.020	0.002	-0.024	-0.000
	(1.341)	(1.600)	(0.180)	(0.220)	(0.207)	(0.245)
Month $\tau-$ 1	0.545	0.265	-0.103	-0.143	-0.276	-0.420
	(1.271)	(1.545)	(0.121)	(0.146)	(0.241)	(0.279)
Month 0: Gubernatorial Elect.	-0.489	-0.297	0.048	0.099	0.214	0.313
	(0.975)	(1.172)	(0.080)	(0.125)	(0.227)	(0.256)
Month $\tau+$ 1	0.609	0.744	-0.082	-0.108	-0.241	-0.308
	(0.674)	(1.012)	(0.115)	(0.137)	(0.262)	(0.365)
Month $\tau+$ 2	-1.262^{**}	-1.614^{*}	-0.248^{*}	-0.169	0.424	0.542
	(0.617)	(0.877)	(0.139)	(0.192)	(0.347)	(0.470)
Month $\tau+$ 3	-0.698	-0.426	0.261	0.248	-0.005	0.008
	(0.848)	(0.818)	(0.340)	(0.378)	(0.241)	(0.280)
Month $\tau+$ 4	2.103	2.334	0.109	0.131	-0.248	-0.277
	(1.464)	(1.604)	(0.090)	(0.098)	(0.266)	(0.306)
Month $\tau+$ 5	-1.568	-1.714	-0.139	-0.162	-0.060	-0.096
	(1.206)	(1.356)	(0.126)	(0.145)	(0.341)	(0.366)
Month $\tau+$ 6	0.247	-0.031	-0.246^{**}	-0.264^{**}	-0.379	-0.565
	(1.089)	(1.278)	(0.110)	(0.127)	(0.466)	(0.539)
Gub. Election Competitiveness		3.077		-0.327		-0.201
		(2.271)		(0.222)		(0.475)
Gov. Party Dummy = Independent		-3.765^{***}		0.501^{***}		-0.935^{***}
		(0.630)		(0.102)		(0.281)
Gov. Party Dummy = Republican		0.636		-0.086		-0.192
		(0.389)		(0.060)		(0.173)
Party Change Dummy τ		-0.269		-0.224		0.127
		(1.896)		(0.220)		(0.466)
Party Change Dummy $\tau+1$		-1.048		-0.003		0.122
		(1.789)		(0.140)		(0.352)
Party Change Dummy $\tau+2$		0.208		-0.074		-0.612
		(1.054)		(0.117)		(0.411)
Regulatory Framework UA 1956		1.144^{*}		-0.108		-0.807^{**}
		(0.627)		(0.086)		(0.398)
Regulatory Framework UA 1985		4.104^{***}		-0.227		-0.095
		(1.001)		(0.146)		(0.561)
Regulatory Framework UA 2002		1.387^{**}		-0.438^{***}		-0.750^{*}
		(0.579)		(0.138)		(0.451)
Regulatory Framework UA 1956+1985		0.540		-0.557^{***}		-0.234
		(0.940)		(0.114)		(0.468)
State Gov. Shutdown (days)		0.007		-0.003		-0.007
		(0.038)		(0.033)		(0.022)
State Recession dummy		-0.016		0.104		-0.084
		(0.403)		(0.261)		(0.138)
Unemployment R. (State)		-0.579^{***}		-0.036		-0.004
		(0.208)		(0.022)		(0.061)
lag Conviction Rate State	0.133^{***}	0.121^{***}				
	(0.030)	(0.031)				
lag Conviction Rate FED			0.020^{**}	0.016^{*}		
			(0.009)	(0.009)		
lag Conviction Rate SRO					0.092	0.084
					(0.059)	(0.060)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	State	State	State	State	State	State
Observations	16,758	14,705	16,758	14,705	16,758	14,705
Adjusted R^2	0.128	0.135	0.025	0.024	0.077	0.090
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.129	0.083	0.666	0.954	0.310	0.331
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.786	0.742	0.472	0.571	0.244	0.223

Notes: $^{***}< 0.01$
$^{**}< 0.05$
$^{*}< 0.1$
This table shows regressions from simple OLS. The dependent variables are Conviction Rates. All estimations include time and state*month fixed effects. Standard errors are clustered at the state level. Conviction Rates are multiplied by 1000 for better readability. F-statistics test heteroskedasticity-robust joint-significance of lead and lagged election coefficients. With H0: Coefficients are 0.

Table 3—: Gubernatorial election and duration of regulatory actions conditioned on regulatory action: Panel "within" Estimates

	Duration (weeks) State	Duration (weeks) State
	(1)	(2)
Month $\tau-$ 6	−4.497	0.002
	(15.355)	(17.332)
Month $\tau-$ 5	−1.613	−11.937
	(15.013)	(16.679)
Month $\tau-$ 4	−38.952*	−25.294
	(21.399)	(21.747)
Month $\tau-$ 3	−16.845	−20.030
	(11.360)	(17.779)
Month $\tau-$ 2	−13.096	−16.709
	(10.345)	(12.427)
Month $\tau-$ 1	−10.153	−14.551
	(12.423)	(12.747)
Month 0: Gubernatorial Elect.	−22.644**	−20.218**
	(10.469)	(9.817)
Month $\tau+$ 1	−79.971	−83.129
	(68.015)	(75.864)
Month $\tau+$ 2	−41.436	−42.541
	(47.955)	(45.851)
Month $\tau+$ 3	5.495	6.515
	(19.090)	(21.367)
Month $\tau+$ 4	−26.504	−29.538*
	(16.711)	(17.097)
Month $\tau+$ 5	9.384	12.334
	(21.319)	(21.234)
Month $\tau+$ 6	18.730	21.418
	(17.672)	(18.208)
Gub. Election Competitiveness		30.828
		(19.215)
Gov. Party Dummy = Independent		28.053*
		(16.490)
Gov. Party Dummy = Republican		−4.772
		(4.995)
Regulatory Framework UA 1956		−5.238
		(6.017)
Regulatory Framework UA 1985		−5.641
		(11.865)
Regulatory Framework UA 2002		6.748
		(10.450)
Regulatory Framework UA 1956+1985		24.241**
		(10.425)
State Recession dummy		6.649
		(8.151)
Unemployment R. (State)		−0.925
		(2.099)
Controls	No	Yes
Time FE	Yes	Yes
State x Month FE	Yes	
Clustered SE	State	State
Observations	1,541	1,443
Adjusted R^2	0.005	0.000
Hypothesis	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.004	0.007
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.168	0.181

Notes:
***< 0.01
**< 0.05
*< 0.1
This table shows regressions from simple OLS. Dependent variables are either the average duration it takes to build a case in weeks conditioned on the occurence of a regulatory action. All estimations include time and state*month fixed effects. Standard errors are clustered at the state level. F-statistics test heteroskedasticity-robust joint-significance of lead and lagged election coefficients. With H0: Coefficients are 0.

Table 4—: Senatorial elections and regulatory actions: Panel "within" Estimates Conviction Rates

	Conviction Rate State	Conviction Rate State	Conviction Rate Federal	Conviction Rate Federal	Conviction Rate Self-Regulatory	Conviction Rate Self-Regulatory
	(1)	(2)	(3)	(4)	(5)	(6)
Month $\tau-$ 6	−1.973	−1.511	−0.216	−0.151	−0.195	−0.281
	(1.224)	(1.003)	(0.162)	(0.159)	(0.355)	(0.376)
Month $\tau-$ 5	−0.067	0.248	0.162	0.163	−0.323	−0.451
	(1.311)	(1.461)	(0.166)	(0.187)	(0.285)	(0.345)
Month $\tau-$ 4	−0.775	−1.312	−0.025	−0.023	0.020	0.033
	(0.831)	(1.169)	(0.171)	(0.209)	(0.258)	(0.299)
Month $\tau-$ 3	−0.058	−0.122	−0.377	−0.476	0.042	−0.021
	(0.659)	(0.775)	(0.349)	(0.427)	(0.204)	(0.242)
Month $\tau-$ 2	0.322	−0.020	−0.233	−0.316	−0.074	−0.159
	(0.867)	(0.904)	(0.257)	(0.331)	(0.155)	(0.191)
Month $\tau-$ 1	1.021	1.623	−0.011	−0.027	0.170	0.125
	(1.108)	(1.335)	(0.121)	(0.145)	(0.156)	(0.157)
Month 0: Senatorial Elect.	−0.200	−0.411	−0.086	-0.159^{*}	−0.055	−0.161
	(0.976)	(1.167)	(0.087)	(0.096)	(0.177)	(0.173)
Month $\tau+$ 1	0.238	0.058	0.184^{*}	0.170	0.007	−0.009
	(0.593)	(0.723)	(0.099)	(0.118)	(0.165)	(0.186)
Month $\tau+$ 2	−0.118	0.058	0.049	0.068	−0.222	−0.331
	(0.773)	(0.853)	(0.123)	(0.145)	(0.200)	(0.229)
Month $\tau+$ 3	0.903^{*}	0.612	0.598	0.652	0.075	0.026
	(0.549)	(0.590)	(0.391)	(0.473)	(0.139)	(0.171)
Month $\tau+$ 4	−0.581	−0.566	−0.010	−0.003	−0.088	−0.033
	(1.125)	(1.222)	(0.089)	(0.103)	(0.243)	(0.279)
Month $\tau+$ 5	0.336	0.176	0.105	0.112	−0.207	−0.105
	(0.836)	(0.886)	(0.082)	(0.097)	(0.308)	(0.333)
Month $\tau+$ 6	1.069	0.944	0.002	−0.005	0.110	0.142
	(0.780)	(0.879)	(0.059)	(0.067)	(0.252)	(0.301)
Gov. Party Dummy = Independent		-3.565^{***}		0.508^{***}		-0.941^{***}
		(0.713)		(0.101)		(0.273)
Gov. Party Dummy = Republican		0.696^{*}		−0.092		−0.196
		(0.397)		(0.061)		(0.170)
Party Change Dummy τ		−0.554		−0.148		0.310
		(1.914)		(0.165)		(0.467)
Party Change Dummy $\tau+1$		−0.803		−0.051		−0.029
		(1.544)		(0.139)		(0.249)
Party Change Dummy $\tau+2$		−0.828		−0.154		−0.291
		(0.843)		(0.107)		(0.289)
Regulatory Framework UA 1956		1.138^{*}		−0.108		-0.808^{**}
		(0.644)		(0.086)		(0.398)
Regulatory Framework UA 1985		4.062^{***}		−0.222		−0.096
		(1.027)		(0.150)		(0.560)
Regulatory Framework UA 2002		1.167^{**}		-0.415^{***}		−0.737
		(0.551)		(0.143)		(0.458)
Regulatory Framework UA 1956+1985		0.398		-0.541^{***}		−0.222
		(1.008)		(0.112)		(0.465)
State Gov. Shutdown (days)		0.020		−0.002		−0.007
		(0.045)		(0.033)		(0.021)
State Recession dummy		−0.072		0.113		−0.076
		(0.408)		(0.260)		(0.136)
Unemployment R. (State)		-0.645^{***}		−0.028		0.001
		(0.206)		(0.022)		(0.060)
lag Conviction Rate State	0.133^{***}	0.122^{***}				
	(0.030)	(0.031)				
lag Conviction Rate FED			0.020^{**}	0.016^{*}		
			(0.009)	(0.009)		
lag Conviction Rate SRO					0.092	0.084
					(0.059)	(0.060)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	State	State	State	State	State	State
Observations	16,758	14,705	16,758	14,705	16,758	14,705
Adjusted R^2	0.128	0.134	0.025	0.025	0.076	0.088
Notes:	$^{***}< 0.01$					
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.452	0.606	0.174	0.181	0.477	0.215
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.352	0.556	0.035	0.049	0.641	0.706

$^{*}< 0.1$

This table shows regressions from simple OLS. The dependent variables are Conviction Rates. All estimations include time and state*month fixed effects. Standard errors are clustered at the state level. Conviction Rates are multiplied by 1000 for better readability.

and no effect of senatorial elections on regulatory activity by state regulators.

V. Contested elections

This section tests whether the effect of gubernatorial elections on regulatory activity differs depending on the electoral competitiveness linked to gubernatorial election events. Evidence that regulatory actions decrease by a larger amount in states with closer elections would suggest that regulators or their superiors avoid attracting bad press or actively give out favors to donors.[12] At the same time, the negative effect of elections on regulatory actions might also be stronger in noncompetitive election environments, because regulators do not need to fear change of leadership and continue their work as usual.

We define a contested election as any gubernatorial election with a margin of victory below the 1st quantile of 5.4 percentage points. Out of the 391 gubernatorial elections in our dataset, 98 (25%) fulfill this criterion. We create a dummy variable *Contested* that takes on the value of one for contested elections. Then we interact this variable with our leads and lags indicating upcoming elections.

Table 6 presents evidence on the effect of contested elections. Focusing on column (2), the baseline regression that includes controls, we can see an anticipation of the reduction in regulatory activity. The coefficient five months before a contested election is equal to -3.302, compared to -0.281 for an uncontested election. However, most of the decrease five months before the election appears to come from the coefficient four months before. There, the coefficient for a contested election is -0.226, compared to -2.110 for an uncontested election. The other coefficients do not show additional effects of contested elections on regulatory activity.

VI. Conclusion

We tested for political cycles in regulatory activity against the US financial investment advisor industry. Exploiting a new monthly panel dataset on regulatory actions at the US state-level, we use the staggered occurrence of nearly 400 distinct gubernatorial elections over the 1990-2019 period to test for the effect of elections on conviction rates. The different regulatory layers of the US financial regulatory framework, state, federal and self-regulatory organizations, and the staggered occurrence of senatorial elections provide natural falsification tests for our results.

Our findings can be summarized as follows. (i) We find that elections consistently decrease regulatory actions starting 5 months prior to the election. The magnitude of the effect is largest two months before the election, when the average conviction rate is reduced by -2.3 per thousand workers, about 16% of the monthly standard deviation in conviction rates by state regulators. (ii) Using

[12]See also Mehta (2017) who presents evidence that SEC enforcement actions against a constituent firm is negatively associated with the likelihood that a congressional politician is subsequently reelected.

Table 5—: Robustness Gubernatorial and senatorial elections: Panel "within" Estimates Conviction Rates

	Conviction Rate State	Conviction Rate State	Conviction Rate Federal	Conviction Rate Federal	Conviction Rate Self-Regulatory	Conviction Rate Self-Regulatory
	(1)	(2)	(3)	(4)	(5)	(6)
Gub. Month $\tau-6$	0.463	0.034	-0.171^{**}	-0.154^{*}	0.810	0.992^{*}
	(1.092)	(1.239)	(0.085)	(0.093)	(0.503)	(0.584)
Gub. Month $\tau-5$	-0.952	-1.039	0.146	0.195	0.222	0.372
	(1.142)	(1.171)	(0.154)	(0.173)	(0.299)	(0.348)
Gub. Month $\tau-4$	-1.563^{**}	-1.678^{**}	0.204	0.263	0.533	0.573
	(0.645)	(0.756)	(0.272)	(0.324)	(0.482)	(0.580)
Gub. Month $\tau-3$	-0.153	0.007	-0.217	-0.181^{*}	-0.440^{*}	-0.519^{*}
	(0.898)	(1.055)	(0.137)	(0.105)	(0.252)	(0.287)
Gub. Month $\tau-2$	-1.888	-2.319	0.020	-0.000	-0.026	-0.002
	(1.336)	(1.594)	(0.181)	(0.221)	(0.207)	(0.245)
Gub. Month $\tau-1$	0.559	0.285	-0.102	-0.142	-0.273	-0.418
	(1.276)	(1.552)	(0.122)	(0.147)	(0.241)	(0.279)
Month 0: Gubernatorial Elect.	-0.497	-0.307	0.046	0.093	0.213	0.311
	(0.971)	(1.174)	(0.080)	(0.125)	(0.226)	(0.255)
Gub. Month $\tau+1$	0.610	0.747	-0.080	-0.104	-0.240	-0.306
	(0.673)	(1.005)	(0.115)	(0.136)	(0.263)	(0.366)
Gub. Month $\tau+2$	-1.269^{**}	-1.620^{*}	-0.251^{*}	-0.173	0.422	0.539
	(0.619)	(0.877)	(0.141)	(0.194)	(0.345)	(0.471)
Gub. Month $\tau+3$	-0.696	-0.429	0.262	0.243	-0.003	0.011
	(0.850)	(0.824)	(0.339)	(0.373)	(0.241)	(0.280)
Gub. Month $\tau+4$	2.094	2.331	0.104	0.126	-0.248	-0.276
	(1.463)	(1.598)	(0.091)	(0.099)	(0.265)	(0.307)
Gub. Month $\tau+5$	-1.574	-1.721	-0.140	-0.163	-0.060	-0.096
	(1.210)	(1.359)	(0.125)	(0.145)	(0.340)	(0.365)
Gub. Month $\tau+6$	0.246	-0.045	-0.247^{**}	-0.265^{**}	-0.377	-0.567
	(1.083)	(1.276)	(0.110)	(0.127)	(0.466)	(0.541)
Sen. Month $\tau-6$	-1.966	-1.520	-0.218	-0.151	-0.185	-0.273
	(1.222)	(1.006)	(0.162)	(0.158)	(0.354)	(0.373)
Sen. Month $\tau-5$	-0.074	0.227	0.162	0.166	-0.328	-0.456
	(1.304)	(1.453)	(0.165)	(0.185)	(0.284)	(0.345)
Sen. Month $\tau-4$	-0.788	-1.328	-0.022	-0.020	0.026	0.036
	(0.828)	(1.168)	(0.173)	(0.211)	(0.265)	(0.303)
Sen. Month $\tau-3$	-0.034	-0.110	-0.379	-0.479	0.033	-0.031
	(0.654)	(0.771)	(0.349)	(0.426)	(0.204)	(0.243)
Sen. Month $\tau-2$	0.299	-0.048	-0.232	-0.315	-0.070	-0.156
	(0.861)	(0.896)	(0.256)	(0.330)	(0.154)	(0.191)
Sen. Month $\tau-1$	1.037	1.625	-0.013	-0.028	0.166	0.123
	(1.112)	(1.352)	(0.121)	(0.145)	(0.156)	(0.157)
Month 0: Senatorial Elect.	-0.209	-0.408	-0.085	-0.157	-0.049	-0.152
	(0.974)	(1.169)	(0.088)	(0.096)	(0.177)	(0.173)
Sen. Month $\tau+1$	0.253	0.078	0.185^{*}	0.167	0.003	-0.016
	(0.593)	(0.720)	(0.099)	(0.118)	(0.164)	(0.185)
Sen. Month $\tau+2$	-0.116	0.068	0.048	0.064	-0.221	-0.328
	(0.773)	(0.859)	(0.122)	(0.144)	(0.202)	(0.231)
Sen. Month $\tau+3$	0.899	0.649	0.600	0.647	0.074	0.021
	(0.547)	(0.606)	(0.391)	(0.471)	(0.139)	(0.169)
Sen. Month $\tau+4$	-0.569	-0.545	-0.011	-0.007	-0.088	-0.033
	(1.128)	(1.229)	(0.089)	(0.103)	(0.243)	(0.279)
Sen. Month $\tau+5$	0.324	0.211	0.106	0.110	-0.201	-0.099
	(0.828)	(0.876)	(0.082)	(0.096)	(0.309)	(0.332)
Sen. Month $\tau+6$	1.078	0.984	0.003	-0.005	0.117	0.149
	(0.779)	(0.883)	(0.059)	(0.067)	(0.251)	(0.303)
Gub. Election Competitiveness		3.086		-0.320		-0.202
		(2.277)		(0.222)		(0.473)
Gov. Party Dummy = Independent		-3.737^{***}		0.521^{***}		-0.926^{***}
		(0.641)		(0.103)		(0.280)
Gov. Party Dummy = Republican		0.635		-0.086		-0.193
		(0.389)		(0.060)		(0.173)
Party Change Dummy τ		-0.246		-0.214		0.134
		(1.945)		(0.219)		(0.468)
Party Change Dummy $\tau+1$		-1.052		-0.011		0.122
		(1.773)		(0.136)		(0.351)
Party Change Dummy $\tau+2$		0.209		-0.073		-0.596
		(1.053)		(0.114)		(0.415)
Regulatory Framework UA 1956		1.141^{*}		-0.109		-0.808^{**}
		(0.627)		(0.086)		(0.398)
Regulatory Framework UA 1985		4.100^{***}		-0.228		-0.098
		(1.002)		(0.146)		(0.561)
Regulatory Framework UA 2002		1.386^{**}		-0.438^{***}		-0.753^{*}
		(0.581)		(0.138)		(0.452)
Regulatory Framework UA 1956+1985		0.547		-0.555^{***}		-0.231
		(0.946)		(0.114)		(0.466)
State Gov. Shutdown (days)		0.018		-0.002		-0.007
		(0.044)		(0.033)		(0.021)
State Recession dummy		-0.021		0.105		-0.086
		(0.402)		(0.262)		(0.138)
Unemployment R. (State)		-0.579^{***}		-0.036		-0.004
		(0.208)		(0.022)		(0.061)
lag Conviction Rate State	0.134^{***}	0.122^{***}				
	(0.030)	(0.031)				
lag Conviction Rate FED			0.020^{**}	0.016^{*}		
			(0.009)	(0.009)		
lag Conviction Rate SRO					0.092	0.083
					(0.059)	(0.060)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	State	State	State	State	State	State
Observations	16,758	14,705	16,758	14,705	16,758	14,705
Adjusted R^2	0.128	0.134	0.025	0.024	0.077	0.090

Notes: $^{***}< 0.01$
$^{**}< 0.05$
$^{*}< 0.1$
This table shows regressions from simple OLS. The dependent variables are Conviction Rates. All estimations include time and state*month fixed effects. Standard errors are clustered at the state level. Conviction Rates are multiplied by 1000 for better readability.

Table 6—: Contested gubernatorial elections and regulatory actions: Panel "within" Estimates Conviction Rates

	Conviction Rate State	Conviction Rate State	Conviction Rate Federal	Conviction Rate Federal	Conviction Rate Self-Regulatory	Conviction Rate Self-Regulatory
	(1)	(2)	(3)	(4)	(5)	(6)
Month $\tau-6$	0.320	0.249	-0.160^{*}	-0.153	0.927	1.113
	(1.474)	(1.649)	(0.097)	(0.106)	(0.612)	(0.697)
Contested*Election $\tau-6$	0.573	-0.789	-0.035	-0.011	-0.441	-0.453
	(2.900)	(2.965)	(0.142)	(0.149)	(0.694)	(0.720)
Month $\tau-5$	-0.248	-0.281	0.147	0.203	0.174	0.295
	(1.218)	(1.261)	(0.148)	(0.162)	(0.265)	(0.289)
Contested*Election $\tau-5$	-2.867^{**}	-3.021^{**}	-0.016	-0.039	0.188	0.295
	(1.343)	(1.443)	(0.350)	(0.374)	(0.502)	(0.549)
Month $\tau-4$	-1.976^{***}	-2.110^{**}	0.282	0.360	0.165	0.160
	(0.735)	(0.846)	(0.314)	(0.373)	(0.297)	(0.348)
Contested*Election $\tau-4$	1.666	1.804	-0.312	-0.404	1.439^{**}	1.643^{*}
	(1.370)	(1.509)	(0.223)	(0.268)	(0.725)	(0.935)
Month $\tau-3$	-0.574	-0.484	-0.214	-0.185	-0.279	-0.344
	(0.832)	(0.965)	(0.145)	(0.125)	(0.197)	(0.241)
Contested*Election $\tau-3$	1.655	2.017	-0.003	0.018	-0.638^{**}	-0.709^{**}
	(1.205)	(1.475)	(0.178)	(0.205)	(0.295)	(0.307)
Month $\tau-2$	-1.945	-2.551	0.021	0.014	0.093	0.120
	(1.561)	(1.788)	(0.195)	(0.241)	(0.259)	(0.300)
Contested*Election $\tau-2$	0.249	1.018	-0.001	-0.049	-0.456	-0.476
	(1.238)	(1.112)	(0.281)	(0.352)	(0.378)	(0.405)
Month $\tau-1$	0.723	0.623	-0.098	-0.139	-0.390	-0.525^{*}
	(1.562)	(1.888)	(0.123)	(0.148)	(0.246)	(0.274)
Contested*Election $\tau-1$	-0.709	-1.405	-0.020	-0.017	0.450	0.416
	(1.685)	(1.944)	(0.150)	(0.180)	(0.357)	(0.346)
Month 0: Gubernatorial Elect.	-0.418	-0.100	0.114	0.142	0.412	0.509
	(1.064)	(1.198)	(0.106)	(0.149)	(0.299)	(0.332)
Month 0: Contested*Election	-0.285	-0.629	-0.258	-0.233	-0.779^{*}	-0.934^{*}
	(1.066)	(1.322)	(0.167)	(0.176)	(0.453)	(0.563)
Month $\tau+1$	0.529	0.692	-0.107	-0.094	-0.249	-0.285
	(0.826)	(1.148)	(0.105)	(0.136)	(0.250)	(0.346)
Contested*Election $\tau+1$	0.311	0.529	0.100	-0.096	0.030	-0.122
	(1.391)	(1.563)	(0.174)	(0.120)	(0.401)	(0.454)
Month $\tau+2$	-1.037^{*}	-1.315	-0.232	-0.180	0.367	0.495
	(0.598)	(0.843)	(0.156)	(0.207)	(0.303)	(0.409)
Contested*Election $\tau+2$	-0.885	-0.998	-0.064	0.008	0.225	0.186
	(0.901)	(1.032)	(0.111)	(0.120)	(0.581)	(0.674)
Month $\tau+3$	-0.422	-0.084	0.553	0.546	0.100	0.110
	(0.768)	(0.728)	(0.562)	(0.626)	(0.258)	(0.300)
Contested*Election $\tau+3$	-1.091	-1.239	-1.149	-1.154	-0.416^{*}	-0.396
	(0.916)	(1.025)	(0.905)	(0.987)	(0.220)	(0.241)
Month $\tau+4$	1.924	2.224	0.146	0.163	-0.472^{*}	-0.542^{*}
	(1.802)	(1.979)	(0.108)	(0.118)	(0.260)	(0.292)
Contested*Election $\tau+4$	0.706	0.509	-0.146	-0.128	0.882	1.019^{*}
	(2.344)	(2.398)	(0.100)	(0.105)	(0.554)	(0.588)
Month $\tau+5$	-2.146^{*}	-2.342^{*}	-0.194	-0.233^{*}	0.099	0.065
	(1.279)	(1.377)	(0.119)	(0.137)	(0.412)	(0.448)
Contested*Election $\tau+5$	2.280	2.502	0.218	0.266	-0.627	-0.629
	(2.557)	(2.506)	(0.269)	(0.291)	(0.413)	(0.460)
Month $\tau+6$	0.477	0.244	-0.228^{**}	-0.254^{**}	-0.543	-0.802
	(1.052)	(1.195)	(0.104)	(0.120)	(0.460)	(0.511)
Contested*Election $\tau+6$	-0.929	-0.982	-0.073	-0.048	0.660	0.923^{**}
	(1.343)	(1.387)	(0.131)	(0.139)	(0.501)	(0.454)
Gov. Party Dummy = Independent		-3.623^{***}		0.475^{***}		-0.953^{***}
		(0.676)		(0.102)		(0.276)
Gov. Party Dummy = Republican		0.688^{*}		-0.095		-0.196
		(0.392)		(0.060)		(0.170)
Party Change Dummy τ		-0.370		-0.176		0.255
		(1.984)		(0.200)		(0.466)
Party Change Dummy $\tau+1$		-1.295		0.029		0.150
		(1.797)		(0.132)		(0.358)
Party Change Dummy $\tau+2$		0.132		-0.053		-0.617
		(1.039)		(0.110)		(0.427)
Regulatory Framework UA 1956		1.154^{*}		-0.108		-0.808^{**}
		(0.642)		(0.086)		(0.397)
Regulatory Framework UA 1985		4.055^{***}		-0.223		-0.092
		(1.024)		(0.149)		(0.562)
Regulatory Framework UA 2002		1.185^{**}		-0.413^{***}		-0.738
		(0.545)		(0.142)		(0.455)
Regulatory Framework UA 1956+1985		0.413		-0.549^{***}		-0.223
		(1.003)		(0.115)		(0.467)
State Gov. Shutdown (days)		0.007		-0.003		-0.008
		(0.038)		(0.033)		(0.021)
State Recession dummy		-0.063		0.108		-0.087
		(0.412)		(0.257)		(0.136)
Unemployment R. (State)		-0.645^{***}		-0.027		0.001
		(0.204)		(0.022)		(0.059)
lag Conviction Rate State	0.134^{***}	0.122^{***}				
	(0.030)	(0.031)				
lag Conviction Rate FED			0.020^{**}	0.016^{*}		
			(0.009)	(0.009)		
lag Conviction Rate SRO					0.092	0.085
					(0.059)	(0.060)
Controls	No	Yes	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	State	State	State	State	State	State
Observations	16,758	14,705	16,758	14,705	16,758	14,705
Adjusted R^2	0.128	0.134	0.025	0.024	0.078	0.092

Notes: $^{***}< 0.01$
$^{**}< 0.05$
$^{*}< 0.1$
This table shows regressions from simple OLS. The dependent variables are Conviction Rates. All estimations include time and state*month fixed effects. Standard errors are clustered at the state level. Conviction Rates are multiplied by 1000 for better readability.

decisions of federal and self-regulatory regulators as a falsification test, we show that gubernatorial elections have no effect on regulatory activity for these agencies. (iii) Similarly, estimates suggest that the staggered occurrence of senatorial elections also has no effect on state-level regulatory behavior. (iv) Using the sample of completed regulatory actions we show that average case duration is significantly smaller in the run-up to the election, suggesting a rush to finish cases. (v) Finally, we present evidence that for contested elections, the negative effect on conviction rates is felt already five months before the election.

The results in this paper highlight the malleability of regulatory activity performed by state regulators. As part of the Dodd-Frank regulatory overhaul in 2010, states obtained more control over the regulation of the financial investment advisor industry. Our results show causal effects of elections on regulatory activity that suggest deviations from optimal regulatory policy. These results suggest a significant trade-off in regulatory policy. Any benefits of re-attributing regulatory oversight to the local level should then be compared to these potential costs. Two important caveats are that our causal research design only allows us to perform a short-term analysis of these effects and focuses on the intensive margin of regulatory activity, that is, the application of current regulations. Although we control for changing regulatory frameworks, we do not identify causally the effect of these changes on regulatory activity in this paper. Investigating these issues in the financial adviser industry from these two broader perspectives remains for future work.

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Table 1—: Falsification test: 24-month lag of gubernatorial elections: Panel "within" estimates conviction rates

	Conviction Rate State	Conviction Rate Federal	Conviction Rate Self-Regulatory
	(1)	(2)	(3)
Month $\tau-$ 6	0.541	0.237**	0.309
	(1.226)	(0.107)	(0.551)
Month $\tau-$ 5	0.599	0.025	−0.383
	(1.641)	(0.148)	(0.339)
Month $\tau-$ 4	0.076	−0.070	0.135
	(0.807)	(0.065)	(0.312)
Month $\tau-$ 3	0.636	−0.215	0.108
	(0.767)	(0.283)	(0.240)
Month $\tau-$ 2	−0.062	0.209	−0.018
	(0.642)	(0.152)	(0.355)
Month $\tau-$ 1	0.723	−0.044	0.143
	(0.809)	(0.094)	(0.298)
Month 0: Gubernatorial Elect. lag 24	−0.416	−0.179**	−0.007
	(0.794)	(0.090)	(0.153)
Month $\tau+$ 1	−1.482	0.097	0.322
	(1.157)	(0.177)	(0.299)
Month $\tau+$ 2	−1.164	0.032	−0.231
	(1.115)	(0.179)	(0.220)
Month $\tau+$ 3	−0.631	0.234	0.363
	(0.823)	(0.178)	(0.262)
Month $\tau+$ 4	2.053*	−0.132	−0.658
	(1.080)	(0.265)	(0.485)
Month $\tau+$ 5	1.125	−0.138	−0.206
	(1.090)	(0.142)	(0.279)
Month $\tau+$ 6	0.783	0.334**	−0.606
	(0.885)	(0.160)	(0.404)
lag Conviction Rate State	0.133***		
	(0.030)		
lag Conviction Rate FED		0.020**	
		(0.009)	
lag Conviction Rate SRO			0.092
			(0.059)
Controls	No	No	No
Time FE	Yes	Yes	Yes
State x Month FE	Yes	Yes	Yes
Clustered SE	State	State	State
Observations	16,758	16,758	16,758
Adjusted R^2	0.128	0.025	0.077
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.311	0.741	0.547
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.760	0.210	0.205

Notes: * (p<0.1), ** (p<0.05), *** (p<0.01); This table shows falsification tests on our main results from table 3.2. The occurrence of gubernatorial elections is shifted by 24 or 36 months forward (lagged) or backwards (lead). The dependent variables are Conviction rates. All estimations include time and state*month fixed effects. Standard errors are clustered at the state level. Conviction rates are multiplied by 1000 for better readability. F-statistics test heteroskedasticity-robust joint-significance of lead and lagged election coefficients. With H0: Coefficients are 0.

Table 2—: Falsification test: 36-month lag of gubernatorial elections: Panel "within" estimates conviction rates

	Conviction Rate State	Conviction Rate Federal	Conviction Rate Self-Regulatory
	(1)	(2)	(3)
Month $\tau-$ 6	-0.290	-0.097	0.903
	(1.029)	(0.077)	(0.565)
Month $\tau-$ 5	2.134	-0.230	-0.176
	(1.348)	(0.305)	(0.365)
Month $\tau-$ 4	-0.409	-0.207^{**}	0.118
	(1.088)	(0.088)	(0.209)
Month $\tau-$ 3	0.923	0.225	0.535^{*}
	(0.848)	(0.275)	(0.294)
Month $\tau-$ 2	0.530	-0.051	-0.332
	(0.980)	(0.043)	(0.207)
Month $\tau-$ 1	-0.123	-0.041	0.113
	(0.926)	(0.119)	(0.148)
Month 0: Gubernatorial Elect. lag 36	0.155	-0.025	-0.360
	(1.044)	(0.088)	(0.356)
Month $\tau+$ 1	1.049	-0.237^{*}	-0.047
	(2.408)	(0.123)	(0.143)
Month $\tau+$ 2	1.824	0.362	0.320
	(1.678)	(0.295)	(0.356)
Month $\tau+$ 3	-2.255^{*}	-0.030	-0.409
	(1.247)	(0.107)	(0.341)
Month $\tau+$ 4	-0.905	0.085	0.097
	(0.794)	(0.189)	(0.384)
Month $\tau+$ 5	0.477	-0.101	-0.252
	(1.005)	(0.100)	(0.337)
Month $\tau+$ 6	0.320	0.149	0.615
	(1.327)	(0.103)	(0.619)
lag Conviction Rate State	0.133^{***}		
	(0.030)		
lag Conviction Rate FED		0.020^{**}	
		(0.009)	
lag Conviction Rate SRO			0.092
			(0.059)
Controls	No	No	No
Time FE	Yes	Yes	Yes
State x Month FE	Yes	Yes	Yes
Clustered SE	State	State	State
Observations	16,758	16,758	16,758
Adjusted R^2	0.128	0.025	0.077
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.336	0.350	0.054
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.854	0.560	0.637

Notes: * (p<0.1), ** (p<0.05), *** (p<0.01); This table shows falsification tests on our main results from table 3.2. The occurrence of gubernatorial elections is shifted by 24 or 36 months forward (lagged) or backwards (lead). The dependent variables are Conviction rates. All estimations include time and state*month fixed effects. Standard errors are clustered at the state level. Conviction rates are multiplied by 1000 for better readability. F-statistics test heteroskedasticity-robust joint-significance of lead and lagged election coefficients. With H0: Coefficients are 0.

Table 3—: Falsification test: 24-month lead of gubernatorial elections: Panel "within" estimates conviction rates

	Conviction Rate State	Conviction Rate Federal	Conviction Rate Self-Regulatory
	(1)	(2)	(3)
Month $\tau-$ 6	0.594	0.145*	0.265
	(1.215)	(0.079)	(0.508)
Month $\tau-$ 5	−0.938	0.027	−0.333
	(1.148)	(0.141)	(0.338)
Month $\tau-$ 4	−0.704	−0.056	0.110
	(0.795)	(0.063)	(0.283)
Month $\tau-$ 3	0.810	−0.193	0.099
	(0.813)	(0.287)	(0.230)
Month $\tau-$ 2	−0.109	0.245	−0.015
	(0.597)	(0.173)	(0.344)
Month $\tau-$ 1	0.107	0.009	0.154
	(0.704)	(0.093)	(0.313)
Month 0: Gubernatorial Elect. lead 24	−1.103	−0.161*	0.120
	(1.172)	(0.087)	(0.136)
Month $\tau+$ 1	−1.431	0.105	0.334
	(1.074)	(0.172)	(0.299)
Month $\tau+$ 2	−1.239	0.125	−0.227
	(1.078)	(0.163)	(0.241)
Month $\tau+$ 3	−0.921	0.297*	0.404*
	(0.846)	(0.172)	(0.245)
Month $\tau+$ 4	1.999*	−0.059	−0.664
	(1.028)	(0.259)	(0.480)
Month $\tau+$ 5	1.848	−0.128	−0.215
	(1.179)	(0.139)	(0.282)
Month $\tau+$ 6	0.280	0.359**	−0.546
	(0.816)	(0.164)	(0.424)
lag Conviction Rate State	0.133***		
	(0.031)		
lag Conviction Rate FED		0.020**	
		(0.009)	
lag Conviction Rate SRO			0.092
			(0.059)
Controls	No	No	No
Time FE	Yes	Yes	Yes
State x Month FE	Yes	Yes	Yes
Clustered SE	State	State	State
Observations	16,758	16,758	16,758
Adjusted R^2	0.128	0.025	0.077
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.924	0.686	0.519
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.779	0.041	0.283

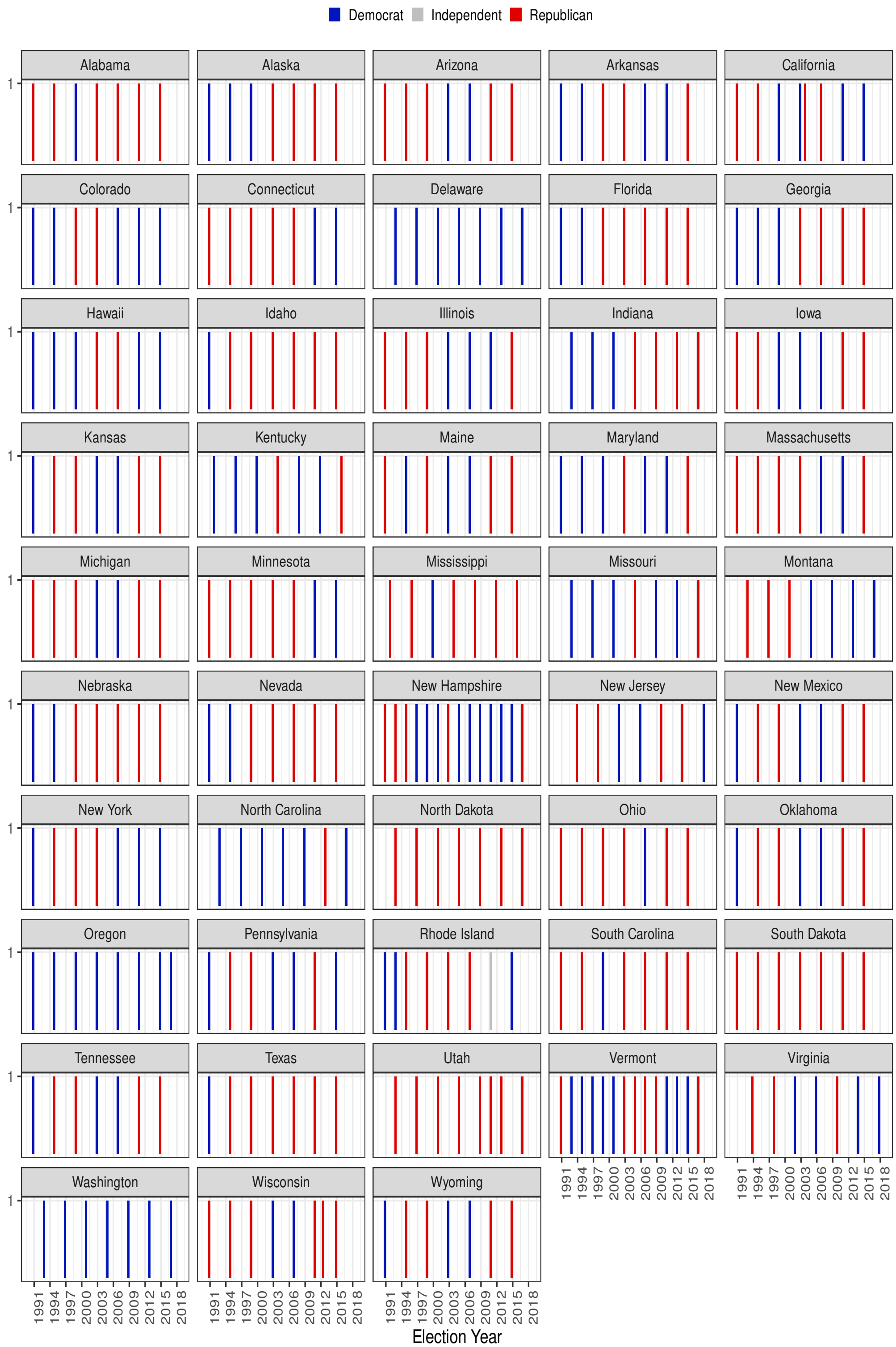
Notes: * (p<0.1), ** (p<0.05), *** (p<0.01); This table shows falsification tests on our main results from table 3.2. The occurrence of gubernatorial elections is shifted by 24 or 36 months forward (lagged) or backwards (lead). The dependent variables are Conviction rates. All estimations include time and state*month fixed effects. Standard errors are clustered at the state level. Conviction rates are multiplied by 1000 for better readability. F-statistics test heteroskedasticity-robust joint-significance of lead and lagged election coefficients. With H0: Coefficients are 0.

Table 4—: Falsification test: 36-month lead of gubernatorial elections: Panel "within" estimates conviction rates

	Conviction Rate State	Conviction Rate Federal	Conviction Rate Self-Regulatory
	(1)	(2)	(3)
Month $\tau-$ 6	0.068	0.302*	−0.738
	(1.109)	(0.155)	(0.509)
Month $\tau-$ 5	0.727	0.361	0.516
	(2.117)	(0.326)	(0.381)
Month $\tau-$ 4	−0.232	0.171	−0.031
	(0.624)	(0.119)	(0.245)
Month $\tau-$ 3	−1.026	−0.339	−0.108
	(0.767)	(0.234)	(0.308)
Month $\tau-$ 2	1.323	0.112	−0.279
	(1.005)	(0.163)	(0.262)
Month $\tau-$ 1	0.985	0.005	0.098
	(1.247)	(0.100)	(0.196)
Month 0: Gubernatorial Elect. lead 36	1.698	0.112	−0.155
	(1.392)	(0.118)	(0.287)
Month $\tau+$ 1	1.439	0.101	0.012
	(2.479)	(0.187)	(0.175)
Month $\tau+$ 2	1.116	−0.366	−0.006
	(1.627)	(0.258)	(0.276)
Month $\tau+$ 3	3.336	−0.125	−0.120
	(2.057)	(0.086)	(0.181)
Month $\tau+$ 4	0.909	0.095	0.086
	(0.758)	(0.124)	(0.389)
Month $\tau+$ 5	0.471	0.074	0.145
	(0.986)	(0.107)	(0.395)
Month $\tau+$ 6	−0.444	−0.135	−0.681
	(1.530)	(0.095)	(0.572)
lag Conviction Rate State	0.133***		
	(0.031)		
lag Conviction Rate FED		0.020**	
		(0.009)	
lag Conviction Rate SRO			0.092
			(0.059)
Controls	No	No	No
Time FE	Yes	Yes	Yes
State x Month FE	Yes	Yes	Yes
Clustered SE	State	State	State
Observations	16,758	16,758	16,758
Adjusted R^2	0.128	0.025	0.077
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.585	0.200	0.316
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.092	0.378	0.403

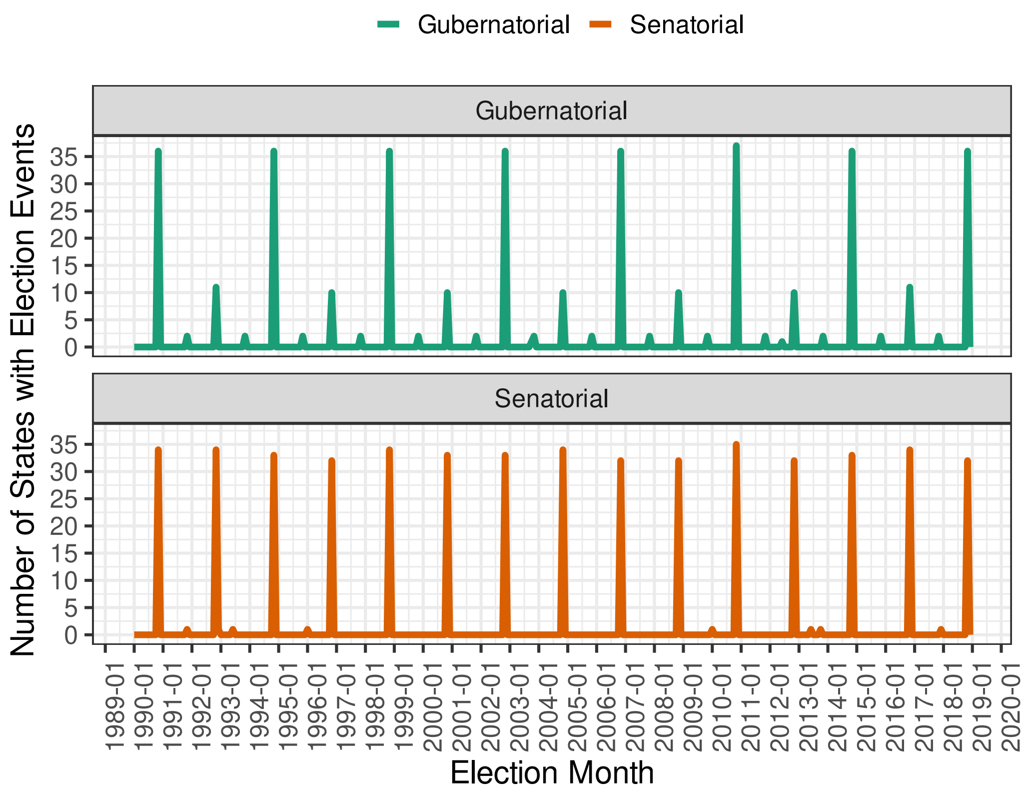
Notes: * (p<0.1), ** (p<0.05), *** (p<0.01); This table shows falsification tests on our main results from table 3.2. The occurrence of gubernatorial elections is shifted by 24 or 36 months forward (lagged) or backwards (lead). The dependent variables are Conviction rates. All estimations include time and state*month fixed effects. Standard errors are clustered at the state level. Conviction rates are multiplied by 1000 for better readability. F-statistics test heteroskedasticity-robust joint-significance of lead and lagged election coefficients. With H0: Coefficients are 0.

Figure 1. : Gubernatorial elections in the United States.



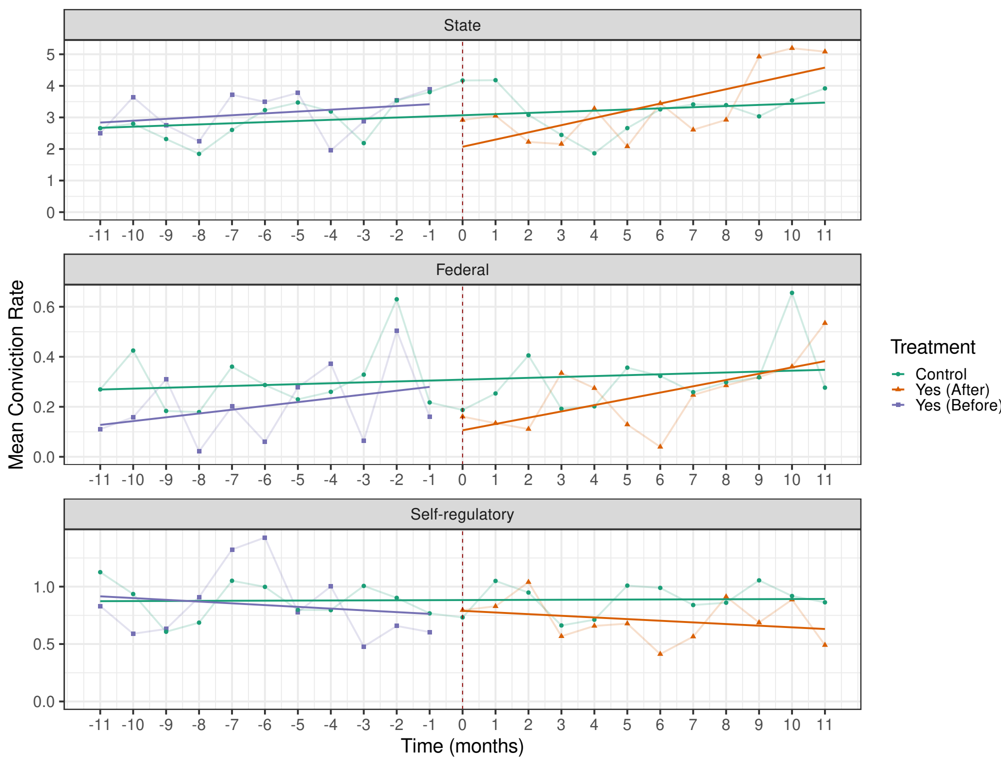
Note: This figure plots the yearly occurrence of gubernatorial level elections per state. Each bar represents one gubernatorial election.

Figure 2. : Gubernatorial and senatorial election cycles in the United States.



Note: This figure plots the number of states having gubernatorial or senatorial election events per month.

Figure 3. : Pre-trends in Regulatory Actions: Treated vs. Non-Treated States



Note: This figure plots mean conviction rate in treated and untreated states for 11-month windows around gubernatorial elections. Treatment: Gubernatorial election. We attribute election months to the period after because most elections are in the beginning of each month.