Gubernatorial Elections Decrease State-level Regulatory Actions of Investment Advisers

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This paper shows that gubernatorial elections decrease state-level financial regulatory actions. We text mine investment adviser public disclosure forms to construct a new database on regulatory actions at the monthly frequency for US states from 1990 until 2019. Regulation of financial advisers is carried out by three levels of regulators: federal, state, and self-regulatory agencies. Exploiting pre-determined electoral cycles, we find causal evidence that state-level regulators decrease the number of regulatory actions starting four months before gubernatorial elections. This slump occurs even earlier if elections are contested. Federal and self-regulatory agencies do not reduce the number of regulatory actions around gubernatorial elections. Furthermore, senatorial elections do not affect state-level regulatory activity. These findings highlight a direct link between gubernatorial elections and state regulators.
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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 deviations can be particularly large around election events, when the jobs of policy makers are on the line. This paper constructs a novel database on regulatory actions against misconduct in the investment advisory industry and shows that gubernatorial elections decrease state-level financial regulatory activity in the United States of America.

Our setting is an almost ideal laboratory to test the presence of electoral cycles in financial regulatory activity for four reasons. First, the regulatory framework of investment advisers is well-developed and relatively standardized across states. Because many provisions, legal code, and bureaucratic procedures are common across states, we can focus on the application of existing regulations when testing for electoral cycles. Second, the administrative structure of regulation of financial advisers provide natural falsifications tests for the effects of gubernatorial elections on state-level regulatory activity. US financial regulation is composed of federal, state, and self-regulating regulators. Given that governors can not di-

rectly control Federal and self-regulating regulators, these should not be affected by gubernatorial elections. Furthermore, Senatorial elections should not affect state-level regulatory activity. 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. Finally, because our data are available at the monthly level, we can study cycles at a relatively high frequency and consider catch-up effects.

Our main hypothesis is that upcoming gubernatorial elections alter state-level political pressure, which in turn affects financial regulatory activity against the investment advisory industry. Several institutional features are consistent with this view. First, state-level financial regulators are commonly appointed by state governors or elected at the same time. Second, the investment advisory industry is highly regulated and the role of state regulators has been 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] Finally, this 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.

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 where politicians influence financial policy to increase their chances of winning reelection (Mian, Sufi and Trebbi, 2010, 2013; Müller, 2019). Although political pressure is not always observable, we can observe regulatory actions directly using publicly available data. 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.

To study how regulatory actions vary over the election cycle, the first contribution fo this paper is to present a new database on financial regulatory actions using text mining techniques. The data originate from the US Securities and Exchange Commission (SEC) *Investment Adviser Public Disclosure* (IAPD) database and cover 9,750 actions in 49 states and the federal district over the 1990-2019 period.[3]

Relatively little attention has been paid to the regional variation in regulatory actions on financial advisors within the US. The main reason why these data have been very rarely used is that the SEC statistics double count actions at different levels (Velikonja, 2015, 2017) and attribute regulatory actions to the geographical headquarter location, which is not necessarily where the infringement

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[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]Data for West Virginia are not covered by the SEC bulk data.

occurred.[4] We overcome these limitations by developing a text-mining algorithm to re-attribute actions across states based on the geographical location of the infringement. Doing so, we are able to reconstruct a full sample of regulatory actions by financial regulators at the US state-level.[5]

In the empirical part of the paper, 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 dummy variables indicating the occurrence of state-level gubernatorial elections. It is important to use monthly data in order to capture short-run electoral cycles and remain flexible regarding the timing of the effects. Although we capture the exact date of the gubernatorial election, 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 or may engage in catch-up regulatory activity. Using monthly data allows us to include multiple lags and leads around election events and capture such effects.

Regulatory activity is multidimensional and it difficult to measure the financial impact of each regulatory action. Therefore, we use as our dependent variable the conviction rate, defined for each state as the number of regulatory actions per month conducted by each regulatory level, normalized by the number of employees in the state's financial sector. The panel data structure of our data allows accounting for time and US state fixed effects with clustered standard errors at the state level.

Our main contribution is to show that exogenous gubernatorial elections decrease the conviction rate by state regulators in the run-up to the election. Our baseline model is an event-study regression with state- and time-fixed effects. We then include 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 four months before the election. After the election, signs alternate between positive and negative. The effect size is largest four months before the election, when the average conviction rate is reduced by close to -1.9 per thousand workers, about 13% of the monthly standard deviation in conviction rates by state regulators. This effect is not precisely estimated for each month. However, 4 months before the election, it is significantly different

from zero at the 95% confidence level. More importantly, we can reject a joint significance test that all coefficients six months before the election are equal to zero at the 95% 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 at the state level. 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. These natural placebo tests suggest 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.[6]

These 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 negative effect of upcoming elections on regulatory activity is not present for senatorial elections, at any level of regulatory activity. This provides a second natural placebo test that further highlights a channel from gubernatorial elections to state-level regulatory activity.[7]

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.

This paper relates to different 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;

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[6]Our findings that state regulators respond to elections while federal regulator do not, is consistent with the results by Charoenwong, Kwan and Umar (2019) using client complaints, that show that state-level regulatory activity is of lower quality than federal level.

[7]Gubernatorial elections have been used to study different outcomes, including merger and acquisitions (Bonaime, Gulen and Ion, 2018), capital investment (Jens, 2017), borrowing costs (Kaviani et al., 2020) and IPOs (Çolak, Durnev and Qian, 2017). Looking at adjusted critical values for reused natural experiments proposed by Heath et al. (2021), our main result that convictions go down four months before the election is significant at the 95% significance level for 9 different outcomes using gubernatorial elections. More importantly, our setup allows us to consider natural placebo tests, which show no effects of gubernatorial elections on federal and self-regulatory regulatory actions, and no effects of senatorial elections on state-level regulatory actions, which provide additional evidence for the relevance of the mechanism we emphasize.

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.[8] 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 most closely connected to a recent literature showing 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. We contribute by using new data, that we can explore at a monthly frequency, under an institutional environment with different regulators that allows to run natural placebo tests.

The rest of the paper is structured as follows. Section I provides background on the US investment advisor industry and the governance structure of the regulatory system. Section II describes the data used in this paper, including details on the construction of the dataset on regulatory activity. Next, we outline our empirical strategy and identifying assumptions in section III. Section IV shows the main result that gubernatorial elections reduce regulatory activity by state regulators. Section V shows that senatorial elections have no effects on regulatory activity. Section VI investigates the role of contested elections, while section VII concludes.

I. Financial regulation and the investment advisory industry

A. Financial regulation in the US: A brief overview

There are currently three distinct layers of financial (securities) regulation in the US: (1) State-level institutions; (2) Federal institutions; and (3) Self-Regulatory Organizations (SROs).

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[8]See Dal Bó (2006) for a review on regulatory capture that covers also non-financial industries.

Since the early 20th century, state-level securities laws, the so-called "Blue Sky Laws", have been adopted all over the United States. However, this process has been far from uniform. The law literature usually distinguishes three major periods. The first early law period started with the Kansas securities law of 1911. Kansas is often credited as being the first state to enact a modern securities law (Treasury, 2008). While other states were quick to follow, the Great Depression of 1929 lead to the first major federal law of 1933, which is still a major pillar of current securities regulation. The second period starts with the Uniform Securities Act of 1956. Uniform Securities laws are drafted by different actors, most notably the National Conference of Commissioners on Uniform State Laws (NCCUSL), and aim at streamlining regulatory frameworks at the state-level. The 1956 Uniform Securities act has been widely successful and has been adopted by up to 37 states until today at different points in time(Rapp, Sowards and Hirsch, 2020). The successive 1985 Revised Uniform Securities Act has been only adopted by four states. In 1996, the federal National Securities Markets Improvement Act (NSMIA) redefined state and federal roles. Several prominent voices had criticized the administrative inefficiencies created by the large disparities of the dual federal/state regulatory system for registration of securities distributions (Campbell Jr, 1984). NSMIA addressed parts of these issues and preempted state authorities to exercise registration and "merit review" for one specific class of "covered securities" (Rapp, Sowards and Hirsch, 2020). The third period starts in 2002 with the latest push for greater state-level uniformity in the form of the Uniform Securities Act of 2002. It outlines state authority for the registration of securities, the registration and supervision of broker-dealers, investment advisers, and other securities professionals, and enforcement, investigatory, and subpoena powers consistent with federal law (Treasury, 2008). So far, it has been adopted by 21 states.[9]

Today, 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, most notably in California and New York. The "Blue Sky Laws" adopted by most U.S. states usually involve three components (Rapp, Sowards and Hirsch, 2020). First, they regulate security registration. Each security offering in a state is subject to prior registration. Opposed to federal-level regulation, which is based on the disclosure of important financial information, state-level regulation is based on "merit review". While on the federal level (see Securities Act of 1933) it is up to investors to make informed judgments about whether to purchase a company's securities (SEC, 2020), "merit review" interposes state-regulators which judge the suitability and fairness of specific regulatory products. Second, investment advisers or broker dealers need to be licensed at the state-level at which they operate.[10] Third, state-securities regulation aims at preventing fraudulent practices. Thus,

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[9]See the website of the NCCUSL for an overview.

[10]The only exception is Wyoming (Rapp, Sowards and Hirsch, 2020).

states enforce their respective laws and pronounce penalties.

Federal-level regulation has emerged last. Federal securities regulation today comprises numerous, sweeping statutes and countless regulations. The SEC is the main administrator and carries out enforcement jointly with states. The Securities Act and the Securities Exchange Act of 1934 ("Exchange Act"), together with the Investment Company Act of 1940 ("Investment Company Act") and the Investment Advisers Act of 1940 ("Advisers Act"), form the backbone of current regulation. Recently, major changes include the National Securities Markets Improvement Act (NSMIA) and the Securities Litigation Uniform Standards Act of 1998, both partly preempting state-level regulators. The Gramm–Leach–Bliley Act of 1999 allowed commercial banks, investment banks, securities firms, and insurance companies to consolidate. After the major scandals of Enron, the Sarbanes–Oxley Act was adopted in 2002 and hardened financial disclosure requirements. The Dodd-Frank Act of 2010, represented a major overhaul of the US financial regulatory system after the 2008 financial crisis. The 2012 Jumpstart Our Business Startups Act (JOBS) act allowed firms to use crowdfunding in order to issue securities. Finally, the 2012 FAST act partly aimed at supporting small private firms with their capital raising efforts.

SRO organizations perform self-regulatory functions for many types of exchanges, such as stock exchanges, options exchanges, and exchanges that trade security futures products. Today, SROs have broad authority and set governance standards and rules. They also carry out enforcement and disciplinary proceedings with respect to their members (Treasury, 2008). However, activities of the SROs are subject to SEC oversight.

In our analysis we take particular care to control for all these legislative and regulatory changes, both at the federal and the state-level.

B.   *The US investment advisory industry*

Investment advisers are firms or persons advising worthy individuals on their investments and portfolio choice. The US investment advisor industry is large in size. According to the Investment Company Institute (Institute, 2019), the US industry body, 17,079 registered investment advisers reported total assets under management of nearly $21.4 billion US dollars in 2018. The US investment advisor industry is by far the largest worldwide, as measured by the amount of assets under management.

Based on the different federal and state laws, in particular the 'Investment Advisers Act of 1940', investment advisers are required to provide information to regulatory institutions. Specifically, each Investment Advisor is required to file a reporting form, "Form ADV", either with the SEC if they manage more than $100 million in client assets, or with their respective state securities regulator if they manage less than this amount. Form ADV consists of two sections. Part 1 provides information about past disciplinary actions, if any, against the advisor. Part 2 summarizes the advisor's background, investment strategies, services, and

fees.

We exploit information in part 1 on past disciplinary actions. In particular, Item 11 of the first section of part 1 requires investment advisers to indicate all prior disciplinary actions they've been subjected to, including their advisory affiliates. This disclosure may be limited to ten year following the date of the regulatory event, for advisers registered or registering with the SEC, or that are exempt reporting. This information constitutes the raw data that we use to identify regulatory actions, as we describe in the next section.

II.   Financial regulatory actions and elections: Methodology and data

This section presents the newly constructed database on financial regulatory actions. We exploit existing raw data originating from the SEC and use an algorithm based on a "set of keywords" strategy to construct indicators of regulatory actions at the US state-level. The dataset described here represents a major improvement compared to previous work allowing for novel estimates of *de facto* measures of financial regulation for individual US states.[11] Sub-section II.A describes data construction, sub-section II.B defines the two main measures of regulatory activity, II.C presents election data, and II.D outline additional controls.

A.   Data construction and text mining

The *Investment Adviser Public Disclosure* (IAPD) raw data cover registered and exempted investment advisor firms that have to file ADV information on past regulatory actions they have been subjected to in all their operating markets. The data, thus, comprise many different types of regulators, domestic and foreign, and a large time span. We collect unique information on specific investment advisers, such as firm names and geographical location, that can be matched with past regulatory actions. For each regulatory action two dates are available. The 'start date' corresponds to the first time the firm has had knowledge about an investigation and 'end date' is the closing date of the investigation. Unfortunately, we do not observe the data at which an investigation has started as it is possible that it occurs before firms are notified. Further, our data include the name of the regulator, the allegations, summary information of the action, and regulatory sanctions. For our empirical analysis, we keep only regulatory actions that are carried out by US regulators.

Since we have regulatory data on both registered and unregistered investment advisers, we cover a slightly bigger share of advisers in the US investment advisor industry compared to what the US Investment Company Institute reports (Institute, 2019). By construction, the data comprise regulatory actions targeting

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[11]Previous papers have used the same raw data but focused on other sub-parts. Gupta and Sachdeva (2019), for instance, uses one specific question to assess the role of inside investment on hedge fund performance. Loughran and McDonald (2011) and Gong and Yannelis (2018) text mine K-10 statements to develop measures of economic sentiment and financial regulation.

advisers that have filed at least once over the 2001-2019 period. This means that we do not observe regulatory actions for financial advisers that stopped filing in 2000. We use the data over the 1990-2019 period in our baseline regressions in section IV since most of the major players of the US investment advisory industry have filed at least once since 2001 and include information for over ten years in their disclosure forms. Figure 1 shows the yearly sum of distinct registered and exempted investment advisers over the 2001-19 period that have filed at least once and are therefore captured by our data. On average, 24,822 investment advisers file regulatory information each year.

Figure 1. : Number of firms in the data
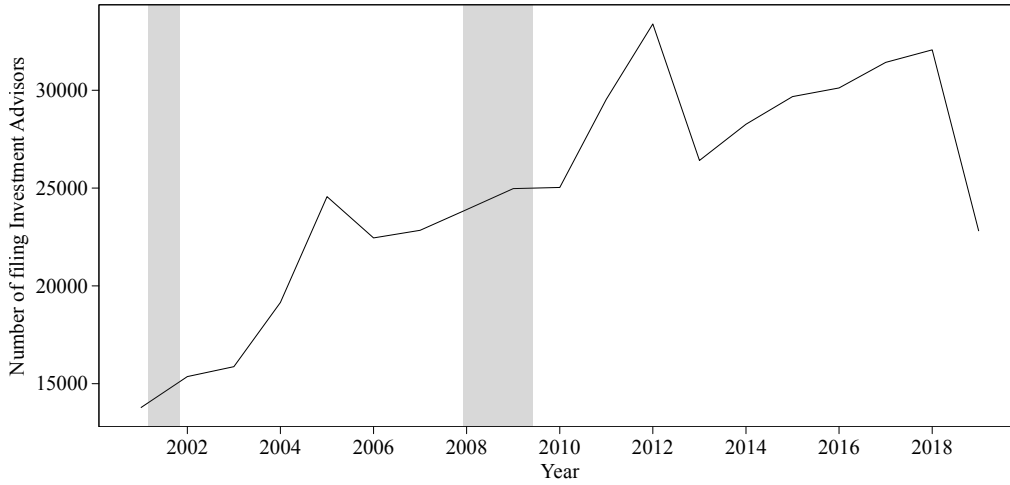
This figure plots the sum of all distinct firms that we capture in the SEC bulk data. The shaded areas show NBER recession dates.

We solve two of the major shortcomings of the SEC raw data. First, although matching convictions to firms is possible using an identification variable, doing so only attributes convictions based on the location of the firm's headquarters, which is not necessarily the location of the regulatory action. Second, regulators tend to inflate their numbers in order to avoid budget cuts (Velikonja, 2017). To overcome these issues we proceed in two parts.

In the first part, we develop an algorithm based on a "set of keywords" to resolve the issue of misattributed geographical location of regulatory actions. Consider the following hypothetical example: an affiliate or branch of investment advisor A is fined in Massachusetts by a local state or federal regulatory agency. In the raw data this conviction is attributed to A 's headquarter location in New York, since this is where the firm is officially located. To address this issue and impute the regulatory infringement of A back to Massachusetts, we exploit all information available in the data. We develop an algorithm that proceeds in three steps.

First, we build a dictionary of the names and abbreviations of all US States, US cities with a population greater than 40,000, and state capitals. Making use of our dictionary, step one consists of identifying regulatory actions that have been pronounced by regulators with clear jurisdictions and references to geographic locations, such as the 'Alabama Securities Commission'. In a second step, we locate actions that have been handled by FINRA district offices. Each FINRA district has a specific number and supervises at least one state.The number of districts and their numbers have not changed since 1990 (See table 1 for an overview). Either the FINRA district covers only one specific state or it covers multiple states. For the first case, imputation is straightforward. For the second possibility, we search whether the firm or its affiliate linked to the regulatory action is located in the district. If we have an intersection of firm location and district jurisdiction, we attribute the regulatory action to this specific state. Third, for remaining regulatory actions we exploit the manually entered text fields. Each conviction includes a summary, a summary of allegations and a summary of the sanctions undertaken. We match every string of these fields against our geographical location dictionary. Then, we count the number of matches and pick the state location with the highest number of mentions among these fields and the company location.

To clarify this last step, consider the following example in table 2. Firm A might be headquartered in New York City. Thus, the entered state-location in the raw data is New York. However, the regulatory action was executed by the NASD District Committee 7. According to table 1, NASD/FINRA District 7 covers multiple states: Georgia, North Carolina, South Carolina, Florida, Puerto Rico, Panama, and the Virgin Islands. Thus, we can not directly infer the location of the regulatory action. Matching our location dictionary against the 'Summary' field in the table we find the city of Charlotte. However, there are currently eleven cities that are called Charlotte in the United States. Cross-checking the information in the summaries, allows us to conclude that the regulatory action most likely took place in Charlotte, North Carolina.

In the second part, we resolve instances of double counting. We take a conservative approach and keep regulatory actions that are finalized and unique on three dimensions: state, starting date, and monetary amount. Since we have the exact daily start and end dates for each regulatory action, it is unlikely that duplicate entries remain in our clean dataset.[12]

Table 3 compares the number of regulatory actions for each individual US state for the total sample before and after we apply our algorithm. The largest differences occur in the major states hosting financial investment advisers. For instance, using our approach, we can relocate more than 1500 convictions from New York state to other states. Similarly, we can re-attribute 235 convictions to

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[12]We also reran results using only regulatory actions that additionally include unique case numbers, regulatory levels, and differing manually entered summaries. Using this less conservative approach, our sample size is slightly larger but all our results hold.

Table 1—: FINRA Districts

	District	State
1	San Francisco	California, Nevada, Hawaii
2	Los Angeles	California, Nevada
3	Denver	Alaska, Arizona, Colorado, Idaho, Montana, New Mexico, Oregon, Utah, Washington, Wyoming
4	Kansas City	Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota
5	New Orleans	Alabama, Arkansas, Louisiana, Mississippi, Oklahoma, Tennessee
6	Dallas	Texas
7	Atlanta and Boca Raton	Georgia, North Carolina, South Carolina, Florida, Puerto Rico, Panama, Virgin Islands
8	Chicago	Illinois, Indiana, Kentucky, Michigan, Ohio, Wisconsin
9	New Jersey and Philadelphia	New Jersey, New York, Delaware, District of Columbia, Maryland, Pennsylvania, Virginia, West Virginia
10	New York and Long Island	New York
11	Boston	Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, Vermont

Table 2—: Example

Regulator	Firm	Summary	Allegations	Sanction
NASD-District Business Conduct Committee-District 7	A	Ordered to disgorge to NASDR the sum of $62,640, an amount equal to the fees A received from the municipal securities business it conducted with the City of Charlotte from 2001 through 2003.	Unlawful Municipal Securities Business	Disgorgement

Texas that initially showed up elsewhere.[13]

Figure 2 shows the effect of our algorithm when summing up regulatory actions by quarter. Again, we see consistently large differences in between the location of the firm and the location of the regulatory infringement. Some states, like Wyoming, have very few convictions, while others have many. Unsurprisingly, states that host important financial centers, such as New York or Chicago, have consistently more convictions compared to other states. Based on these considerations, a measure of regulatory actions needs to account for the size of the financial sector in a specific state economy. This is the subject of the next section.

B.   Measuring regulatory activity

Regulatory actions typically involve sanctioning a firm or one of its affiliates for a specific regulatory infringement. A measure of financial regulatory actions, thus, has to correct for the fact that states with a larger financial sector tend to see a greater number of regulatory infringements. To do so, 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:

$$(1) \qquad\qquad ConvRate_{r,s,t} = \frac{Conv_{r,s,t}}{E_{s,t}}$$

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 $E_{s,t}$,

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[13]Some cases can not be tied to specific locations of the firms or affiliates. However, our algorithm re-attributes these cases to a state based on the location of the regulator. This step justifies why there are more cases in the corrected column than in the uncorrected column.

Figure 2. : Regulatory actions in the United States by US state
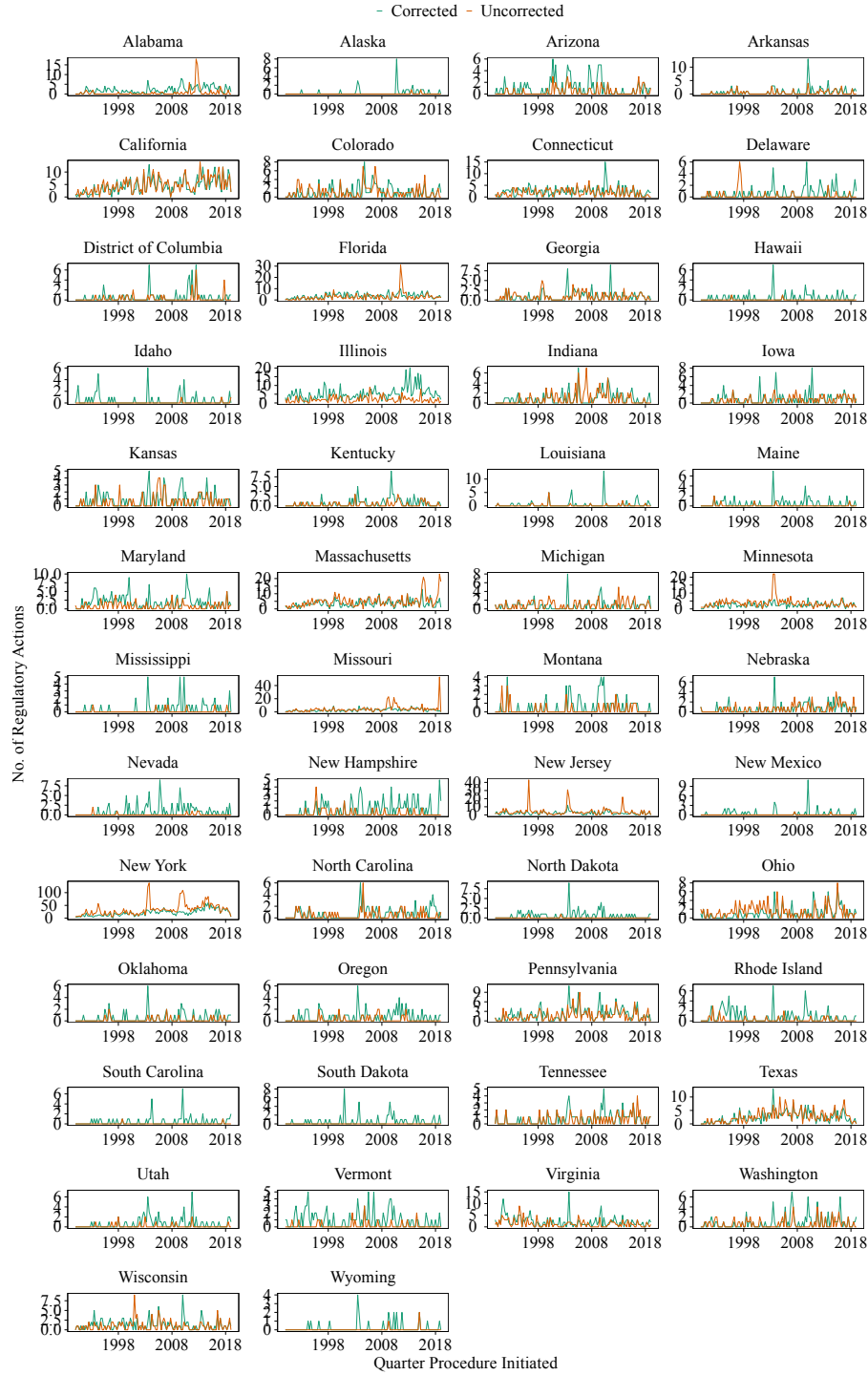
Note: This figure plots the quarterly sum of the absolute number of regulatory actions per US state with monetary fines larger than 0. 'Corrected' figures are obtained using our algorithm. 'Uncorrected' figures are obtained using the original state location information associated with a regulatory action.

Table 3—: Number of convictions per state

	State	Nº	Nº (not corr.)	Difference
1	Alabama	240	83	157
2	Alaska	33	2	31
3	Arizona	118	43	75
4	Arkansas	93	44	49
5	California	504	555	-51
6	Colorado	135	97	38
7	Connecticut	287	200	87
8	Delaware	70	16	54
9	District of Columbia	67	19	48
10	Florida	403	311	92
11	Georgia	106	82	24
12	Hawaii	50	2	48
13	Idaho	49	1	48
14	Illinois	627	223	404
15	Indiana	107	68	39
16	Iowa	102	55	47
17	Kansas	103	54	49
18	Kentucky	85	37	48
19	Louisiana	58	13	45
20	Maine	56	6	50
21	Maryland	233	88	145
22	Massachusetts	412	582	-170
23	Michigan	80	66	14
24	Minnesota	292	421	-129
25	Mississippi	51	5	46
26	Missouri	322	493	-171
27	Montana	71	22	49
28	Nebraska	85	59	26
29	Nevada	111	5	106
30	New Hampshire	102	21	81
31	New Jersey	261	429	-168
32	New Mexico	53	2	51
33	New York	2481	3959	-1478
34	North Carolina	85	42	43
35	North Dakota	72	2	70
36	Ohio	129	138	-9
37	Oklahoma	57	15	42
38	Oregon	87	19	68
39	Pennsylvania	247	177	70
40	Rhode Island	100	17	83
41	South Carolina	51	2	49
42	South Dakota	61	0	61
43	Tennessee	82	57	25
44	Texas	290	310	-20
45	Utah	78	11	67
46	Vermont	115	15	100
47	Virginia	266	152	114
48	Washington	107	55	52
49	Wisconsin	154	107	47
50	Wyoming	26	3	23
51	Total	9854	9185	669

the number of employees in the financial sector of the respective state s at time t.

C. *Gubernatorial and senatorial elections*

Next, we collect data on all gubernatorial and senatorial elections over the 1990-2019 period, including 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.[14] 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.[15] 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.[16] 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 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 3 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

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

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

[16] 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.

Figure 3. : Gubernatorial and senatorial election cycles in the United States.
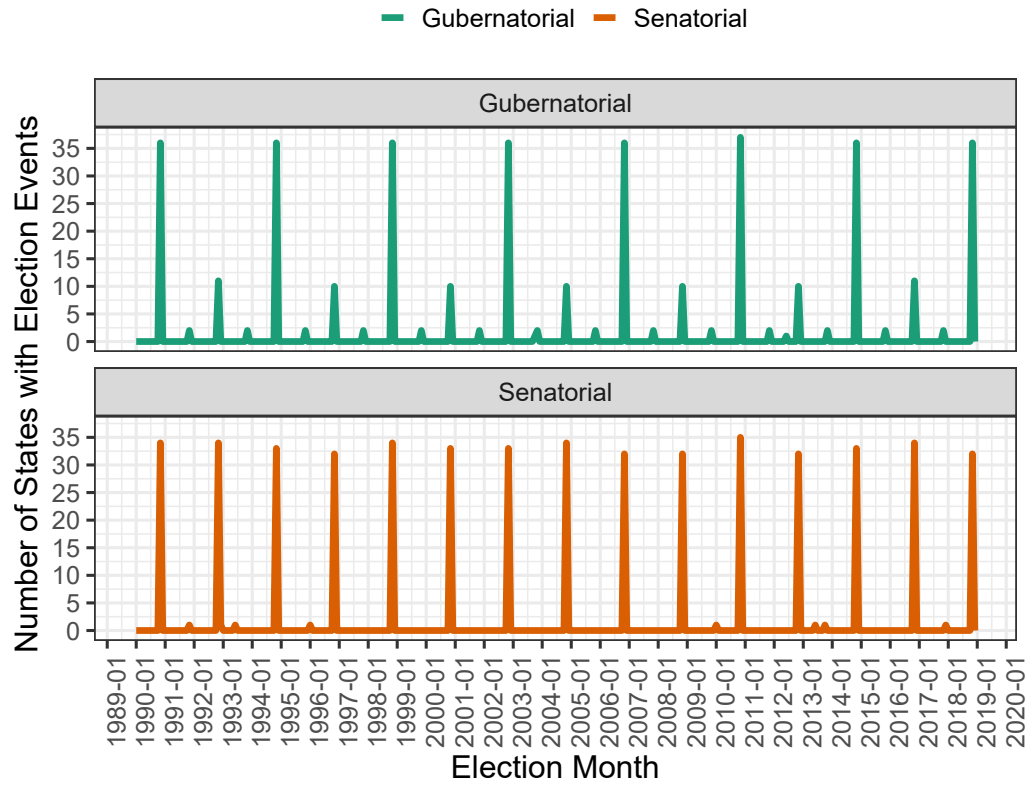
Note: This figure plots the number of states having gubernatorial or senatorial election events per month.

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.

D.   *Additional controls*

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 for each.[17]

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).[18]

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[17]The omitted category are state-specific regulatory frameworks, which include: 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.

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

Table 4—: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Conviction Rate All	17,346	4.56	15.17	0.00	392.16
Conviction Rate Federal	17,346	0.30	2.38	0.00	176.99
Conviction Rate SRO	17,346	0.88	3.52	0.00	92.59
Conviction Rate State	17,346	3.39	14.52	0.00	392.16
Gub. Election Dummy	17,346	0.02	0.15	0	1
Gub. Election Competitiveness	16,224	0.16	0.13	0.00	0.58
Sen. Election Dummy	17,346	0.03	0.17	0	1
State Gov. Shutdown (days)	17,346	0.01	0.55	0	31
State Unemployment Rate	17,346	5.46	1.85	2.10	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 4. 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. 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.[19] 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.

III.   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.

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

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

following equation:

$$(2) \qquad y_{s,t} = \sum_{j \in \{-6;6\}} \alpha_j g_{j,s,t} + \beta y_{s,t-1} + \tau_t + f_s + controls_{s,t}\gamma + \epsilon_{s,t}$$

where y stands for the conviction rates for each regulatory level (state, federal or self-regulatory), s identifies the state, and t is time measured at the monthly frequency. To control for US-level shocks, we include a full set of time controls τ_t. We also include a set of state-fixed effects to control for time-invariant state-level unobservables. In a robustness check, to control for state-specific fixed effects and state-specific seasonality that may be related to regulatory requirements at the state level, we replace these state fixed effects with 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 II. We 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 situations where an incumbent governor is unseated. Because new governors are only sworn in two to three months after the actual election date, the incumbent or the current administration may 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 each level of regulation: 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 gubernatorial elections affect regulatory activity only through 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 the timing potentially 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:

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

where all variables are defined like in equation (2), 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 (2) 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 (2) 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 4 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 (2) 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).[20] 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 3). 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 IV tests the effect of gubernatorial elections on federal and self-regulatory regulators. Section V 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 indication that our results are due to changes in the treatment variable. Results for 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.

IV.   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 5 presents the results of estimating equation (2). Column (1) shows the effect of a gubernatorial election cycle on conviction rates at the state-level. The coefficients before the election 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. 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.

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

Figure 4. : Pre-trends in Regulatory Actions: Treated vs. Non-Treated States
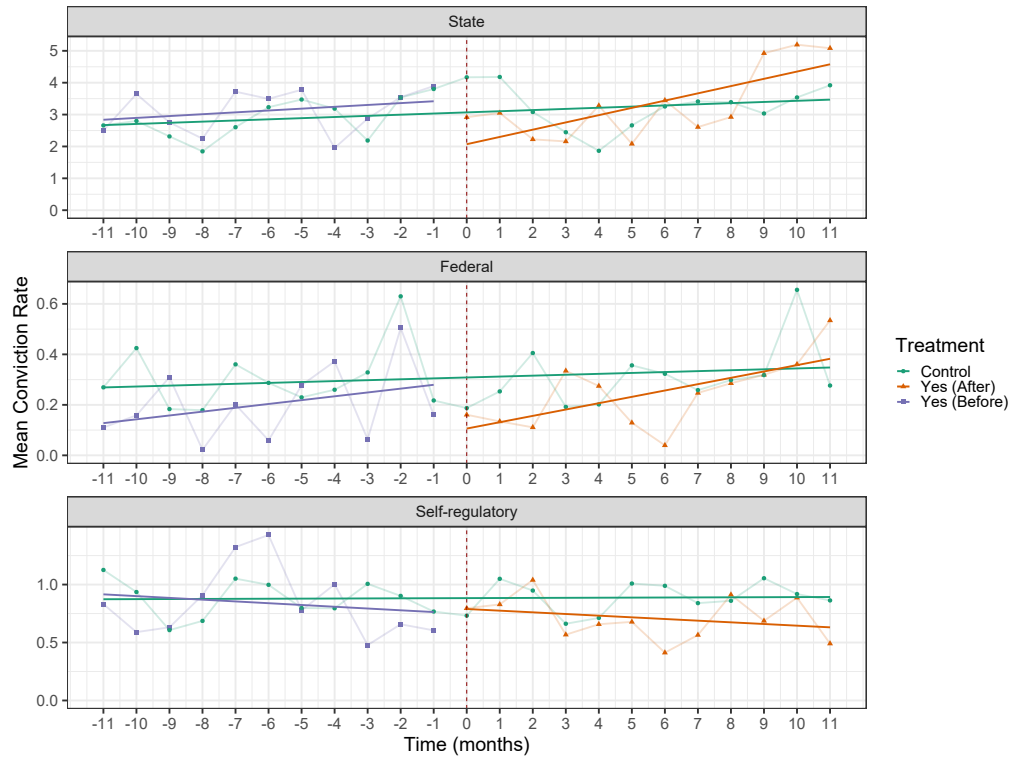
Note: This figure plots mean conviction rate at different regulatory levels in treated and untreated states for 11-month windows around gubernatorial elections. The treatment is a gubernatorial election. When computing slopes we attribute election months to the period after an election because most elections in the US are at the beginning of the month.

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 0.052. With controls the p-value decreases to 0.025. Thus, we can reject that estimated coefficients on the pre-period are equal to zero at the 95% confidence level. P-values for the post-treatment period do not permit to reject the hypothesis that all coefficients are equal zero.

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 or very small in magnitude. For instance, column (3) shows that conviction rates for federal 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 indicates that gubernatorial election events affect only state regulators, and not federal or self-regulatory agencies.

V. Placebo: Senatorial elections

Given that most governors directly or indirectly appoint state-level regulators, results from section IV 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 (2), but replace the gubernatorial election variable with dummies for senatorial elections.

Table 6 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

Table 5—: 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 $\tau - 6$	0.24	0.13	-0.15^*	-0.14	0.82	0.98^*
	(1.19)	(1.27)	(0.08)	(0.10)	(0.49)	(0.57)
Month $\tau - 5$	-0.96	-0.91	0.12	0.16	0.27	0.41
	(1.11)	(1.10)	(0.15)	(0.17)	(0.29)	(0.33)
Month $\tau - 4$	-1.79^{***}	-1.88^{**}	0.21	0.27	0.50	0.53
	(0.64)	(0.74)	(0.27)	(0.32)	(0.46)	(0.56)
Month $\tau - 3$	-0.07	0.07	-0.22	-0.18^*	-0.35	-0.43^*
	(0.73)	(0.83)	(0.13)	(0.11)	(0.22)	(0.24)
Month $\tau - 2$	-1.24	-1.49	0.03	-0.01	0.04	0.07
	(0.85)	(0.98)	(0.17)	(0.21)	(0.15)	(0.18)
Month $\tau - 1$	-0.09	-0.49	-0.13	-0.15	-0.30	-0.43
	(1.44)	(1.74)	(0.11)	(0.14)	(0.23)	(0.27)
Month 0: Gubernatorial Elect.	-0.52	-0.39	0.03	0.06	0.31	0.41
	(1.09)	(1.32)	(0.08)	(0.12)	(0.31)	(0.33)
Month $\tau + 1$	0.30	0.41	-0.09	-0.11	-0.24	-0.31
	(0.68)	(0.99)	(0.11)	(0.13)	(0.26)	(0.36)
Month $\tau + 2$	-1.48^{**}	-1.94^{**}	-0.25^*	-0.19	0.30	0.50
	(0.60)	(0.84)	(0.13)	(0.19)	(0.31)	(0.44)
Month $\tau + 3$	-1.05	-0.85	0.25	0.23	0.02	0.01
	(0.92)	(0.92)	(0.32)	(0.35)	(0.24)	(0.27)
Month $\tau + 4$	1.74	1.93	0.10	0.14	-0.23	-0.28
	(1.22)	(1.36)	(0.09)	(0.10)	(0.26)	(0.31)
Month $\tau + 5$	-1.98	-2.19	-0.13	-0.13	-0.07	-0.14
	(1.38)	(1.56)	(0.12)	(0.14)	(0.32)	(0.34)
Month $\tau + 6$	-0.05	0.04	-0.23^{**}	-0.24^*	-0.38	-0.59
	(1.03)	(1.23)	(0.10)	(0.12)	(0.43)	(0.50)
Gub. Election Competitiveness		2.07		-0.34		0.20
		(2.76)		(0.23)		(0.33)
Gov. Party Dummy = Independent		-4.40^{***}		0.44^{***}		-0.93^{***}
		(0.59)		(0.10)		(0.17)
Gov. Party Dummy = Republican		0.54		-0.10		-0.10
		(0.39)		(0.07)		(0.11)
Party Change Dummy τ		-0.15		-0.17		0.20
		(1.84)		(0.21)		(0.43)
Party Change Dummy $\tau + 1$		-0.75		-0.04		0.05
		(1.69)		(0.13)		(0.33)
Party Change Dummy $\tau + 2$		0.65		-0.07		-0.59
		(0.94)		(0.12)		(0.38)
Regulatory Framework UA 1956		-0.49		-0.33^{***}		0.26
		(0.39)		(0.11)		(0.20)
Regulatory Framework UA 1985		-0.26		-0.18		0.26
		(0.84)		(0.16)		(0.27)
Regulatory Framework UA 2002		-0.27		-0.54^{***}		-0.05
		(0.50)		(0.10)		(0.15)
Regulatory Framework UA 1956+1985		0.58		-0.58^{***}		0.20
		(0.53)		(0.16)		(0.14)
State Gov. Shutdown (days)		0.04		-0.01		-0.02
		(0.04)		(0.03)		(0.03)
State Recession dummy		-0.10		0.14		-0.15
		(0.40)		(0.26)		(0.13)
Unemployment R. (State)		-0.30		-0.00		-0.01
		(0.22)		(0.02)		(0.05)
lag Conviction Rate State	0.12^{***}	0.11^{***}				
	(0.03)	(0.03)				
lag Conviction Rate FED			0.01	0.01		
			(0.01)	(0.01)		
lag Conviction Rate SRO					0.03	0.02
					(0.04)	(0.04)
Controls	No	Yes	No	Yes	No	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	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	0.14	0.14	0.03	0.03	0.14	0.15
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6} + \tau_{-5} + \tau_{-4} + \tau_{-3} + \tau_{-2} + \tau_{-1} = 0$	0.052	0.025	0.597	0.894	0.232	0.256
$\tau_{+1} + \tau_{+2} + \tau_{+3} + \tau_{+4} + \tau_{+5} + \tau_{+6} = 0$	0.254	0.268	0.462	0.594	0.186	0.178

Notes:

$^{***} < 0.01$
$^{**} < 0.05$
$^* < 0.1$

This table shows regressions from simple OLS as defined by equation (2). The dependent variables are Conviction Rates. The treatment is a gubernatorial election. The independent variables are dummies that capture the pre- and post-treatment period. All estimations include time and state 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. Under H0: Coefficients are equal to zero.

federal regulators that all coefficients are equal to zero at the 95% significance level.

Finally, we estimate equation (3), including both gubernatorial and senatorial elections. Identification is now solely obtained from gubernatorial elections that do not coincide with senatorial elections. Table 7 confirms all main results of section IV. In particular we confirm the negative effect of gubernatorial elections on regulatory activity by state regulators, with no clear effect on other regulators, and no effect of senatorial elections on regulatory activity by state regulators.

VI. Additional results and robustness checks

This section begins by testing 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.[21] 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 8 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 -3.25, compared to -0.11 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 -0.73, compared to -2.26 for an uncontested election. The other coefficients do not show additional effects of contested elections on regulatory activity.

In the appendix section A, we show that results are robust to different specifications. First, we consider state-month fixed effects to capture state-specific seasonality that may be related to regulatory requirements at the state level. Table A1 shows that results are largely unchanged. Next, we consider different windows around the treatment. Table A2 shows that results are robust to using nine month windows. Finally, tables A3 to A6 show falsification tests on our main results. The occurrence of gubernatorial elections is shifted by 24 or 36 months forward (lagged) or backwards (lead). Unsurprisingly, no effects of gubernatorial elections are identified.

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[21]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 6—: 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 $\tau-6$	-2.12^*	-1.57	-0.23	-0.16	-0.25	-0.33
	(1.19)	(0.98)	(0.16)	(0.16)	(0.35)	(0.37)
Month $\tau-5$	-0.10	0.22	0.17	0.17	-0.34	-0.45
	(1.30)	(1.44)	(0.16)	(0.18)	(0.27)	(0.32)
Month $\tau-4$	-0.82	-1.22	-0.01	-0.02	0.03	0.04
	(0.82)	(1.14)	(0.17)	(0.21)	(0.24)	(0.29)
Month $\tau-3$	-0.09	-0.12	-0.38	-0.48	0.06	-0.01
	(0.65)	(0.76)	(0.34)	(0.42)	(0.21)	(0.24)
Month $\tau-2$	0.41	0.04	-0.25	-0.34	-0.05	-0.14
	(0.84)	(0.91)	(0.25)	(0.32)	(0.16)	(0.19)
Month $\tau-1$	0.97	1.50	-0.02	-0.03	0.17	0.13
	(1.07)	(1.29)	(0.12)	(0.14)	(0.15)	(0.15)
Month 0: Senatorial Elect.	-0.23	-0.41	-0.09	-0.17^*	-0.06	-0.18
	(0.95)	(1.14)	(0.08)	(0.09)	(0.18)	(0.17)
Month $\tau+1$	0.25	0.09	0.18^*	0.17	0.02	-0.03
	(0.58)	(0.72)	(0.10)	(0.11)	(0.16)	(0.18)
Month $\tau+2$	-0.14	-0.01	0.06	0.08	-0.21	-0.33
	(0.75)	(0.82)	(0.13)	(0.14)	(0.21)	(0.23)
Month $\tau+3$	0.86	0.59	0.60	0.64	0.09	0.03
	(0.54)	(0.57)	(0.37)	(0.44)	(0.14)	(0.17)
Month $\tau+4$	-0.61	-0.63	-0.00	0.01	-0.05	-0.03
	(1.08)	(1.20)	(0.09)	(0.10)	(0.24)	(0.27)
Month $\tau+5$	0.29	0.07	0.12	0.15^*	-0.16	-0.07
	(0.81)	(0.84)	(0.08)	(0.09)	(0.30)	(0.33)
Month $\tau+6$	1.02	0.88	0.01	0.00	0.12	0.12
	(0.75)	(0.85)	(0.06)	(0.06)	(0.25)	(0.29)
Gov. Party Dummy = Independent		-4.25^{***}		0.45^{***}		-0.92^{***}
		(0.66)		(0.10)		(0.17)
Gov. Party Dummy = Republican		0.57		-0.10		-0.10
		(0.41)		(0.07)		(0.11)
Party Change Dummy τ		-0.42		-0.12		0.41
		(1.89)		(0.15)		(0.48)
Party Change Dummy $\tau+1$		-0.62		-0.09		-0.12
		(1.52)		(0.13)		(0.24)
Party Change Dummy $\tau+2$		-0.50		-0.16^*		-0.31
		(0.78)		(0.10)		(0.28)
Regulatory Framework UA 1956		-0.39		-0.35^{***}		0.27
		(0.37)		(0.11)		(0.20)
Regulatory Framework UA 1985		-0.12		-0.22		0.27
		(0.98)		(0.17)		(0.27)
Regulatory Framework UA 2002		-0.25		-0.56^{***}		-0.05
		(0.48)		(0.10)		(0.15)
Regulatory Framework UA 1956+1985		0.69		-0.61^{***}		0.21
		(0.56)		(0.15)		(0.13)
State Gov. Shutdown (days)		0.05		-0.01		-0.02
		(0.04)		(0.03)		(0.03)
State Recession dummy		-0.14		0.15		-0.15
		(0.41)		(0.26)		(0.13)
Unemployment R. (State)		-0.33		0.01		-0.01
		(0.22)		(0.02)		(0.05)
lag Conviction Rate State	0.12^{***}	0.11^{***}				
	(0.03)	(0.03)				
lag Conviction Rate FED			0.01	0.01		
			(0.01)	(0.01)		
lag Conviction Rate SRO					0.03	0.02
					(0.04)	(0.04)
Controls	No	Yes	No	Yes	No	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	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	0.14	0.14	0.04	0.03	0.13	0.15
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.385	0.590	0.166	0.159	0.506	0.230
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.410	0.650	0.030	0.040	0.807	0.720

Notes:

$^{***}< 0.01$
$^{**}< 0.05$
$^{*}< 0.1$

This table shows regressions from simple OLS as defined by equation (2). The dependent variables are Conviction Rates. The treatment is a senatorial election. The independent variables are dummies that capture the pre- and post-treatment period. All estimations include time and state 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. Under H0: Coefficients are equal to zero.

Table 7—: 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 $\tau- 6$	0.22	0.11	−0.16*	−0.14	0.82	0.98*
	(1.18)	(1.27)	(0.08)	(0.10)	(0.49)	(0.56)
Gub. Month $\tau- 5$	−0.94	−0.89	0.12	0.16	0.27	0.41
	(1.10)	(1.10)	(0.15)	(0.17)	(0.29)	(0.33)
Gub. Month $\tau- 4$	−1.80***	−1.89**	0.21	0.27	0.50	0.53
	(0.65)	(0.75)	(0.27)	(0.32)	(0.47)	(0.56)
Gub. Month $\tau- 3$	−0.07	0.07	−0.22	−0.18*	−0.35	−0.43*
	(0.73)	(0.84)	(0.13)	(0.11)	(0.22)	(0.24)
Gub. Month $\tau- 2$	−1.24	−1.50	0.03	−0.01	0.04	0.06
	(0.85)	(0.98)	(0.17)	(0.21)	(0.15)	(0.18)
Gub. Month $\tau- 1$	−0.08	−0.48	−0.13	−0.14	−0.29	−0.42
	(1.45)	(1.75)	(0.11)	(0.14)	(0.23)	(0.26)
Month 0: Gubernatorial Elect.	−0.53	−0.40	0.02	0.05	0.31	0.41
	(1.09)	(1.32)	(0.08)	(0.12)	(0.31)	(0.33)
Gub. Month $\tau+ 1$	0.31	0.41	−0.09	−0.10	−0.24	−0.31
	(0.68)	(0.99)	(0.11)	(0.13)	(0.26)	(0.36)
Gub. Month $\tau+ 2$	−1.48**	−1.94**	−0.25*	−0.20	0.30	0.50
	(0.60)	(0.84)	(0.14)	(0.19)	(0.31)	(0.44)
Gub. Month $\tau+ 3$	−1.05	−0.85	0.25	0.23	0.02	0.02
	(0.92)	(0.93)	(0.32)	(0.34)	(0.24)	(0.27)
Gub. Month $\tau+ 4$	1.73	1.93	0.10	0.13	−0.23	−0.28
	(1.22)	(1.36)	(0.09)	(0.10)	(0.26)	(0.31)
Gub. Month $\tau+ 5$	−1.98	−2.20	−0.13	−0.14	−0.07	−0.14
	(1.38)	(1.56)	(0.12)	(0.14)	(0.32)	(0.34)
Gub. Month $\tau+ 6$	−0.05	0.03	−0.23**	−0.24*	−0.38	−0.60
	(1.02)	(1.23)	(0.10)	(0.12)	(0.43)	(0.51)
Sen. Month $\tau- 6$	−2.11*	−1.58	−0.23	−0.16	−0.24	−0.33
	(1.19)	(0.98)	(0.16)	(0.16)	(0.35)	(0.37)
Sen. Month $\tau- 5$	−0.11	0.20	0.17	0.17	−0.35	−0.46
	(1.29)	(1.43)	(0.16)	(0.18)	(0.26)	(0.32)
Sen. Month $\tau- 4$	−0.84	−1.24	−0.00	−0.02	0.04	0.04
	(0.81)	(1.14)	(0.17)	(0.21)	(0.25)	(0.29)
Sen. Month $\tau- 3$	−0.07	−0.10	−0.38	−0.48	0.06	−0.02
	(0.64)	(0.76)	(0.34)	(0.42)	(0.21)	(0.24)
Sen. Month $\tau- 2$	0.39	0.02	−0.25	−0.34	−0.05	−0.14
	(0.84)	(0.91)	(0.25)	(0.32)	(0.16)	(0.19)
Sen. Month $\tau- 1$	0.97	1.49	−0.02	−0.03	0.17	0.12
	(1.07)	(1.31)	(0.12)	(0.14)	(0.15)	(0.15)
Month 0: Senatorial Elect.	−0.23	−0.41	−0.09	−0.16*	−0.05	−0.17
	(0.95)	(1.14)	(0.09)	(0.09)	(0.18)	(0.17)
Sen. Month $\tau+ 1$	0.26	0.10	0.18*	0.17	0.02	−0.04
	(0.58)	(0.72)	(0.10)	(0.11)	(0.15)	(0.18)
Sen. Month $\tau+ 2$	−0.14	−0.01	0.06	0.08	−0.21	−0.33
	(0.75)	(0.83)	(0.12)	(0.14)	(0.21)	(0.23)
Sen. Month $\tau+ 3$	0.86	0.62	0.60	0.64	0.09	0.03
	(0.54)	(0.60)	(0.37)	(0.44)	(0.14)	(0.17)
Sen. Month $\tau+ 4$	−0.59	−0.62	−0.00	0.00	−0.05	−0.03
	(1.09)	(1.20)	(0.09)	(0.10)	(0.24)	(0.27)
Sen. Month $\tau+ 5$	0.27	0.09	0.12	0.15*	−0.15	−0.06
	(0.80)	(0.83)	(0.08)	(0.09)	(0.30)	(0.33)
Sen. Month $\tau+ 6$	1.03	0.92	0.01	0.00	0.13	0.13
	(0.75)	(0.85)	(0.06)	(0.06)	(0.25)	(0.29)
Gub. Election Competitiveness		2.08		−0.34		0.20
		(2.76)		(0.23)		(0.33)
Gov. Party Dummy = Independent		−4.37***		0.46***		−0.92***
		(0.60)		(0.10)		(0.17)
Gov. Party Dummy = Republican		0.54		−0.10		−0.10
		(0.39)		(0.07)		(0.11)
Party Change Dummy τ		−0.13		−0.16		0.20
		(1.88)		(0.21)		(0.43)
Party Change Dummy $\tau + 1$		−0.76		−0.05		0.05
		(1.68)		(0.13)		(0.33)
Party Change Dummy $\tau + 2$		0.65		−0.07		−0.58
		(0.94)		(0.12)		(0.38)
Regulatory Framework UA 1956		−0.50		−0.33***		0.26
		(0.39)		(0.11)		(0.20)
Regulatory Framework UA 1985		−0.28		−0.19		0.25
		(0.83)		(0.16)		(0.27)
Regulatory Framework UA 2002		−0.28		−0.55***		−0.06
		(0.50)		(0.10)		(0.15)
Regulatory Framework UA 1956+1985		0.58		−0.58***		0.20
		(0.53)		(0.16)		(0.14)
State Gov. Shutdown (days)		0.05		−0.01		−0.02
		(0.04)		(0.03)		(0.03)
State Recession dummy		−0.10		0.15		−0.15
		(0.40)		(0.26)		(0.13)
Unemployment R. (State)		−0.30		−0.00		−0.01
		(0.22)		(0.02)		(0.05)
lag Conviction Rate State	0.12***	0.11***				
	(0.03)	(0.03)				
lag Conviction Rate FED			0.01	0.01		
			(0.01)	(0.01)		
lag Conviction Rate SRO					0.03	0.02
					(0.04)	(0.04)
Controls	No	Yes	No	Yes	No	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	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	0.14	0.14	0.04	0.03	0.14	0.15

Notes:
***< 0.01
**< 0.05
*< 0.1
This table shows regressions from simple OLS as defined by equation (3). The dependent variables are Conviction Rates. The treatments are gubernatorial and senatorial elections. The independent variables are dummies that capture the pre- and post-treatment period. All estimations include time and state 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. Under H0: Coefficients are equal to zero..

Table 8—: 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 $\tau-6$	0.06	0.27	−0.14	−0.13	0.95	1.13
	(1.56)	(1.69)	(0.10)	(0.11)	(0.60)	(0.68)
Contested*Election $\tau-6$	0.64	−0.64	−0.04	−0.04	−0.52	−0.54
	(2.86)	(2.88)	(0.13)	(0.13)	(0.65)	(0.67)
Month $\tau-5$	−0.19	−0.11	0.14	0.19	0.26	0.38
	(1.22)	(1.25)	(0.14)	(0.16)	(0.26)	(0.27)
Contested*Election $\tau-5$	−3.06**	−3.14**	−0.10	−0.13	0.02	0.12
	(1.38)	(1.46)	(0.34)	(0.37)	(0.44)	(0.50)
Month $\tau-4$	−2.16***	−2.26***	0.30	0.37	0.14	0.12
	(0.75)	(0.83)	(0.31)	(0.37)	(0.27)	(0.32)
Contested*Election $\tau-4$	1.47	1.53	−0.34	−0.41	1.40*	1.63
	(1.29)	(1.42)	(0.21)	(0.26)	(0.77)	(0.98)
Month $\tau-3$	−0.52	−0.47	−0.21	−0.18	−0.19	−0.27
	(0.67)	(0.78)	(0.14)	(0.12)	(0.17)	(0.21)
Contested*Election $\tau-3$	1.79	2.18	−0.03	−0.01	−0.61***	−0.63**
	(1.12)	(1.41)	(0.15)	(0.17)	(0.23)	(0.26)
Month $\tau-2$	−1.22	−1.64	0.02	0.01	0.16	0.17
	(1.02)	(1.10)	(0.19)	(0.23)	(0.18)	(0.21)
Contested*Election $\tau-2$	−0.08	0.61	0.01	−0.06	−0.48	−0.39
	(1.17)	(1.02)	(0.30)	(0.37)	(0.29)	(0.31)
Month $\tau-1$	0.15	−0.08	−0.12	−0.14	−0.37	−0.48*
	(1.76)	(2.10)	(0.12)	(0.14)	(0.24)	(0.27)
Contested*Election $\tau-1$	−0.94	−1.65	−0.02	−0.00	0.28	0.22
	(1.68)	(1.98)	(0.13)	(0.16)	(0.37)	(0.37)
Month 0: Gubernatorial Elect.	−0.46	−0.25	0.08	0.09	0.48	0.57
	(1.18)	(1.35)	(0.10)	(0.14)	(0.38)	(0.40)
Month 0: Contested*Election	−0.24	−0.48	−0.23*	−0.19	−0.67	−0.76
	(1.04)	(1.28)	(0.14)	(0.14)	(0.41)	(0.50)
Month $\tau+1$	0.29	0.41	−0.11	−0.10	−0.24	−0.28
	(0.86)	(1.13)	(0.10)	(0.13)	(0.25)	(0.34)
Contested*Election $\tau+1$	0.05	0.20	0.09	−0.07	0.02	−0.13
	(1.40)	(1.49)	(0.16)	(0.11)	(0.33)	(0.39)
Month $\tau+2$	−1.26**	−1.64*	−0.23	−0.19	0.29	0.51
	(0.61)	(0.84)	(0.15)	(0.21)	(0.28)	(0.40)
Contested*Election $\tau+2$	−0.86	−1.15	−0.09	−0.04	0.02	−0.02
	(0.92)	(1.06)	(0.10)	(0.11)	(0.51)	(0.59)
Month $\tau+3$	−0.80	−0.52	0.54	0.52	0.16	0.16
	(0.82)	(0.81)	(0.55)	(0.59)	(0.27)	(0.29)
Contested*Election $\tau+3$	−0.98	−1.23	−1.15	−1.13	−0.54**	−0.56**
	(0.91)	(1.01)	(0.91)	(0.99)	(0.22)	(0.23)
Month $\tau+4$	1.64	1.88	0.16	0.19	−0.38	−0.45
	(1.54)	(1.70)	(0.11)	(0.12)	(0.25)	(0.29)
Contested*Election $\tau+4$	0.40	0.22	−0.22**	−0.22**	0.60	0.64
	(2.35)	(2.41)	(0.10)	(0.11)	(0.57)	(0.60)
Month $\tau+5$	−2.65*	−2.94*	−0.17	−0.19	0.10	0.02
	(1.44)	(1.59)	(0.12)	(0.14)	(0.39)	(0.42)
Contested*Election $\tau+5$	2.65	2.89	0.16	0.19	−0.67*	−0.65
	(2.59)	(2.55)	(0.26)	(0.28)	(0.36)	(0.39)
Month $\tau+6$	0.16	0.25	−0.21**	−0.22*	−0.51	−0.79
	(0.96)	(1.14)	(0.10)	(0.12)	(0.43)	(0.48)
Contested*Election $\tau+6$	−0.86	−0.78	−0.09	−0.07	0.50	0.77*
	(1.13)	(1.21)	(0.11)	(0.12)	(0.49)	(0.46)
Gov. Party Dummy = Independent		−4.31***		0.41***		−0.94***
		(0.62)		(0.10)		(0.18)
Gov. Party Dummy = Republican		0.56		−0.11		−0.10
		(0.40)		(0.07)		(0.11)
Party Change Dummy τ		−0.20		−0.13		0.28
		(1.92)		(0.19)		(0.43)
Party Change Dummy $\tau+1$		−0.88		−0.02		0.06
		(1.70)		(0.13)		(0.32)
Party Change Dummy $\tau+2$		0.66		−0.04		−0.60
		(0.92)		(0.12)		(0.39)
Regulatory Framework UA 1956		−0.38		−0.36***		0.28
		(0.37)		(0.11)		(0.20)
Regulatory Framework UA 1985		−0.10		−0.24		0.28
		(0.95)		(0.17)		(0.27)
Regulatory Framework UA 2002		−0.22		−0.57***		−0.04
		(0.50)		(0.11)		(0.15)
Regulatory Framework UA 1956+1985		0.70		−0.63***		0.21
		(0.57)		(0.15)		(0.13)
State Gov. Shutdown (days)		0.04		−0.01		−0.02
		(0.04)		(0.03)		(0.03)
State Recession dummy		−0.13		0.15		−0.16
		(0.42)		(0.25)		(0.13)
Unemployment R. (State)		−0.33		0.01		−0.01
		(0.22)		(0.02)		(0.05)
lag Conviction Rate State	0.12***	0.11***				
	(0.03)	(0.03)				
lag Conviction Rate FED			0.01	0.01		
			(0.01)	(0.01)		
lag Conviction Rate SRO					0.03	0.02
					(0.04)	(0.04)
Controls	No	Yes	No	Yes	No	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	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	0.14	0.14	0.04	0.03	0.14	0.15

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.

VII. Conclusion

We show the existence of political cycles in state-level regulatory activity against the US financial investment advisory industry. Exploiting a newly collected 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 by state regulators. 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.

Our findings can be summarized as follows. (i) We find that gubernatorial elections decrease state-level regulatory actions starting four months prior to the election. The magnitude of the effect is -1.9 per thousand workers, about 13% of the monthly standard deviation in conviction rates by state regulators. (ii) Using decisions of federal and self-regulatory regulators as falsification tests, 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) Finally, we present evidence that for contested elections the negative effect on conviction rates is felt already five months before the election.

Although it is not possible to identify how much of the drop in regulatory activity at the state-level is caused by less strict regulatory requirements relative to changes in fraudulent activity by investment advisers, our results point to a significant role of state-level regulatory for two reasons. First, changes in fraudulent behavior would likely affect all regulators, but we find effects only on state-regulators. Second, regulatory actions are often complex. The average duration of a regulatory action in our sample is around 18 weeks. Unfortunately, we do not capture the actual start of the investigation, only when the firm is informed about an investigation and the closing date of the investigation. However, this means that changes in fraudulent behavior in response to future elections would have to be on average taken at least 12 to 18 weeks.

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 advisory 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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Additional Robustness Checks

Table A1—: 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 $\tau-6$	0.48	0.05	−0.17*	−0.15	0.81	0.99*
	(1.10)	(1.24)	(0.09)	(0.09)	(0.50)	(0.59)
Month $\tau-5$	−0.97	−1.05	0.14	0.19	0.22	0.38
	(1.15)	(1.18)	(0.15)	(0.17)	(0.30)	(0.35)
Month $\tau-4$	−1.55**	−1.67**	0.20	0.26	0.53	0.57
	(0.64)	(0.75)	(0.27)	(0.32)	(0.48)	(0.58)
Month $\tau-3$	−0.15	0.01	−0.21	−0.18*	−0.44*	−0.52*
	(0.90)	(1.05)	(0.13)	(0.10)	(0.25)	(0.29)
Month $\tau-2$	−1.88	−2.31	0.02	0.00	−0.02	−0.00
	(1.34)	(1.60)	(0.18)	(0.22)	(0.21)	(0.25)
Month $\tau-1$	0.55	0.26	−0.10	−0.14	−0.28	−0.42
	(1.27)	(1.54)	(0.12)	(0.15)	(0.24)	(0.28)
Month 0: Gubernatorial Elect.	−0.49	−0.30	0.05	0.10	0.21	0.31
	(0.97)	(1.17)	(0.08)	(0.13)	(0.23)	(0.26)
Month $\tau+1$	0.61	0.74	−0.08	−0.11	−0.24	−0.31
	(0.67)	(1.01)	(0.12)	(0.14)	(0.26)	(0.36)
Month $\tau+2$	−1.26**	−1.61*	−0.25*	−0.17	0.42	0.54
	(0.62)	(0.88)	(0.14)	(0.19)	(0.35)	(0.47)
Month $\tau+3$	−0.70	−0.43	0.26	0.25	−0.00	0.01
	(0.85)	(0.82)	(0.34)	(0.38)	(0.24)	(0.28)
Month $\tau+4$	2.10	2.33	0.11	0.13	−0.25	−0.28
	(1.46)	(1.60)	(0.09)	(0.10)	(0.27)	(0.31)
Month $\tau+5$	−1.57	−1.71	−0.14	−0.16	−0.06	−0.10
	(1.21)	(1.36)	(0.13)	(0.15)	(0.34)	(0.37)
Month $\tau+6$	0.25	−0.03	−0.25**	−0.26**	−0.38	−0.57
	(1.09)	(1.28)	(0.11)	(0.13)	(0.47)	(0.54)
Gub. Election Competitiveness		3.08		−0.33		−0.20
		(2.27)		(0.22)		(0.47)
Gov. Party Dummy = Independent		−3.77***		0.50***		−0.93***
		(0.63)		(0.10)		(0.28)
Gov. Party Dummy = Republican		0.64		−0.09		−0.19
		(0.39)		(0.06)		(0.17)
Party Change Dummy τ		−0.27		−0.22		0.13
		(1.90)		(0.22)		(0.47)
Party Change Dummy $\tau+1$		−1.05		−0.00		0.12
		(1.79)		(0.14)		(0.35)
Party Change Dummy $\tau+2$		0.21		−0.07		−0.61
		(1.05)		(0.12)		(0.41)
Regulatory Framework UA 1956		1.14*		−0.11		−0.81**
		(0.63)		(0.09)		(0.40)
Regulatory Framework UA 1985		4.10***		−0.23		−0.10
		(1.00)		(0.15)		(0.56)
Regulatory Framework UA 2002		1.39**		−0.44***		−0.75*
		(0.58)		(0.14)		(0.45)
Regulatory Framework UA 1956+1985		0.54		−0.56***		−0.23
		(0.94)		(0.11)		(0.47)
State Gov. Shutdown (days)		0.01		−0.00		−0.01
		(0.04)		(0.03)		(0.02)
State Recession dummy		−0.02		0.10		−0.08
		(0.40)		(0.26)		(0.14)
Unemployment R. (State)		−0.58***		−0.04		−0.00
		(0.21)		(0.02)		(0.06)
lag Conviction Rate State	0.13***	0.12***				
	(0.03)	(0.03)				
lag Conviction Rate FED			0.02**	0.02*		
			(0.01)	(0.01)		
lag Conviction Rate SRO					0.09	0.08
					(0.06)	(0.06)
Controls	No	Yes	No	Yes	No	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State-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	0.13	0.13	0.02	0.02	0.08	0.09
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 as defined by equation (2) but with state-month fixed effects. The dependent variables are Conviction Rates. The treatment is a gubernatorial election. The independent variables are dummies that capture the pre- and post-treatment period. All estimations include time and state 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. Under H0: Coefficients are equal to zero.

Table A2—: 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 $\tau-9$	0.77	1.08	0.25	0.29	0.42	0.49
	(0.82)	(1.00)	(0.23)	(0.26)	(0.27)	(0.33)
Month $\tau-8$	−0.63	−0.80	−0.18**	−0.19*	0.18	0.15
	(0.66)	(0.68)	(0.08)	(0.09)	(0.17)	(0.18)
Month $\tau-7$	2.05	1.10	−0.24	−0.28	0.10	0.07
	(1.33)	(1.05)	(0.28)	(0.32)	(0.42)	(0.50)
Month $\tau-6$	0.28	0.18	−0.15*	−0.14	0.82	0.99*
	(1.20)	(1.29)	(0.08)	(0.10)	(0.49)	(0.57)
Month $\tau-5$	−0.95	−0.86	0.13	0.18	0.30	0.44
	(1.06)	(1.05)	(0.15)	(0.17)	(0.33)	(0.38)
Month $\tau-4$	−1.84**	−1.77**	0.25	0.30	0.56	0.56
	(0.70)	(0.77)	(0.29)	(0.33)	(0.43)	(0.51)
Month $\tau-3$	0.61	0.84	−0.23	−0.19	−0.40	−0.37*
	(0.70)	(0.77)	(0.15)	(0.12)	(0.25)	(0.21)
Month $\tau-2$	−1.48	−1.45	0.04	−0.01	0.10	0.07
	(0.89)	(0.97)	(0.19)	(0.21)	(0.16)	(0.18)
Month $\tau-1$	0.04	−0.33	−0.13	−0.15	−0.30	−0.45
	(1.45)	(1.75)	(0.12)	(0.15)	(0.25)	(0.30)
Month 0: Gubernatorial Elect.	−0.76	−0.67	0.03	0.06	0.30	0.40
	(1.21)	(1.49)	(0.08)	(0.14)	(0.33)	(0.35)
Month $\tau+1$	0.15	0.09	−0.09	−0.13	−0.26	−0.36
	(0.62)	(0.80)	(0.11)	(0.14)	(0.30)	(0.41)
Month $\tau+2$	−1.44**	−1.89**	−0.25*	−0.19	0.30	0.50
	(0.59)	(0.82)	(0.13)	(0.19)	(0.31)	(0.44)
Month $\tau+3$	−0.88	−0.60	0.29	0.29	0.10	0.11
	(0.94)	(0.97)	(0.33)	(0.35)	(0.26)	(0.28)
Month $\tau+4$	1.67	1.83	0.07	0.10	−0.20	−0.25
	(1.18)	(1.34)	(0.09)	(0.10)	(0.25)	(0.30)
Month $\tau+5$	−1.59	−1.95	−0.17	−0.19	−0.05	−0.13
	(1.39)	(1.58)	(0.13)	(0.16)	(0.30)	(0.31)
Month $\tau+6$	−0.01	0.09	−0.23**	−0.24*	−0.38	−0.59
	(1.02)	(1.23)	(0.10)	(0.12)	(0.44)	(0.50)
Month $\tau+7$	−0.15	−0.00	0.07	0.10	0.16	0.13
	(0.82)	(0.88)	(0.10)	(0.12)	(0.35)	(0.37)
Month $\tau+8$	0.10	0.36	0.11	0.16	0.08	0.13
	(0.74)	(0.74)	(0.13)	(0.16)	(0.29)	(0.34)
Month $\tau+9$	3.66*	3.93*	−0.03	−0.02	−0.14	0.29
	(1.87)	(2.04)	(0.12)	(0.13)	(0.20)	(0.31)
Gub. Election Competitiveness		1.98		−0.35		0.19
		(2.78)		(0.23)		(0.33)
Gov. Party Dummy = Independent		−4.49***		0.44***		−0.94***
		(0.59)		(0.10)		(0.17)
Gov. Party Dummy = Republican		0.49		−0.10		−0.11
		(0.40)		(0.07)		(0.11)
Party Change Dummy τ		−0.13		−0.17		0.19
		(1.94)		(0.23)		(0.47)
Party Change Dummy $\tau+1$		−0.14		0.01		0.10
		(1.63)		(0.14)		(0.37)
Party Change Dummy $\tau+2$		0.64		−0.06		−0.59
		(0.95)		(0.12)		(0.38)
Regulatory Framework UA 1956		−0.09		−0.33***		0.27
		(0.44)		(0.11)		(0.21)
Regulatory Framework UA 1985		0.18		−0.18		0.27
		(0.91)		(0.17)		(0.28)
Regulatory Framework UA 2002		−0.15		−0.55***		−0.05
		(0.50)		(0.10)		(0.15)
Regulatory Framework UA 1956+1985		0.76		−0.59***		0.19
		(0.58)		(0.16)		(0.14)
State Gov. Shutdown (days)		0.04		−0.01		−0.02
		(0.04)		(0.03)		(0.03)
State Recession dummy		−0.13		0.14		−0.15
		(0.40)		(0.26)		(0.13)
Unemployment R. (State)		−0.32		0.00		−0.01
		(0.23)		(0.02)		(0.05)
lag Conviction Rate State	0.12***	0.11***				
	(0.03)	(0.03)				
lag Conviction Rate FED			0.01	0.01		
			(0.01)	(0.01)		
lag Conviction Rate SRO					0.03	0.02
					(0.04)	(0.04)
Controls	No	Yes	No	Yes	No	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	State	State	State	State	State	State
Observations	16,464	14,561	16,464	14,561	16,464	14,561
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.078	0.052	0.756	0.995	0.179	0.230
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.329	0.279	0.436	0.544	0.293	0.257

Notes:

*** < 0.01
** < 0.05
* < 0.1
This table shows regressions from simple OLS as defined by equation (2). The dependent variables are Conviction Rates. The treatment is a gubernatorial election. The independent variables are dummies that capture the pre- and post-treatment period. All estimations include time and state 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. Under H0: Coefficients are equal to zero.

Table A3—: 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 $\tau-6$	0.30	0.22**	0.33
	(1.13)	(0.10)	(0.52)
Month $\tau-5$	0.23	0.02	−0.36
	(1.78)	(0.15)	(0.32)
Month $\tau-4$	−0.09	−0.08	0.16
	(0.78)	(0.06)	(0.30)
Month $\tau-3$	0.38	−0.21	0.10
	(0.69)	(0.28)	(0.22)
Month $\tau-2$	−0.24	0.19	−0.12
	(0.63)	(0.15)	(0.32)
Month $\tau-1$	0.42	−0.05	0.12
	(0.77)	(0.09)	(0.28)
Month 0: Gubernatorial Elect. lag 24	−0.49	−0.17**	0.15
	(0.88)	(0.09)	(0.15)
Month $\tau+1$	−2.07	0.07	0.26
	(1.31)	(0.17)	(0.29)
Month $\tau+2$	−0.54	0.04	−0.12
	(0.97)	(0.17)	(0.17)
Month $\tau+3$	−0.49	0.22	0.34
	(0.67)	(0.17)	(0.24)
Month $\tau+4$	1.79*	−0.11	−0.69
	(0.98)	(0.25)	(0.47)
Month $\tau+5$	1.12	−0.15	−0.25
	(1.10)	(0.14)	(0.27)
Month $\tau+6$	0.47	0.32**	−0.56
	(0.89)	(0.16)	(0.39)
lag Conviction Rate State	0.12***		
	(0.03)		
lag Conviction Rate FED		0.01	
		(0.01)	
lag Conviction Rate SRO			0.03
			(0.04)
Controls	No	No	No
Time fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Clustered SE	State	State	State
Observations	16,758	16,758	16,758
Adjusted R^2	0.14	0.03	0.14
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6} + \tau_{-5} + \tau_{-4} + \tau_{-3} + \tau_{-2} + \tau_{-1} = 0$	0.706	0.828	0.642
$\tau_{+1} + \tau_{+2} + \tau_{+3} + \tau_{+4} + \tau_{+5} + \tau_{+6} = 0$	0.891	0.241	0.220

Notes: * (p<0.1), ** (p<0.05), *** (p<0.01); This table shows falsification tests on our main results. 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 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 A4—: 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	−0.47	−0.10	0.87*
	(1.09)	(0.08)	(0.51)
Month $\tau-$ 5	1.73	−0.24	−0.15
	(1.10)	(0.30)	(0.36)
Month $\tau-$ 4	−0.56	−0.21**	0.17
	(1.15)	(0.08)	(0.20)
Month $\tau-$ 3	0.67	0.22	0.50*
	(0.86)	(0.26)	(0.30)
Month $\tau-$ 2	0.32	−0.06	−0.43*
	(0.87)	(0.04)	(0.22)
Month $\tau-$ 1	−0.35	−0.04	0.09
	(0.97)	(0.11)	(0.15)
Month 0: Gubernatorial Elect. lag 36	0.14	−0.04	−0.23
	(0.94)	(0.09)	(0.31)
Month $\tau+$ 1	0.51	−0.25*	−0.05
	(2.25)	(0.14)	(0.14)
Month $\tau+$ 2	2.35	0.36	0.38
	(2.24)	(0.28)	(0.44)
Month $\tau+$ 3	−2.08	−0.03	−0.34
	(1.26)	(0.10)	(0.31)
Month $\tau+$ 4	−1.12	0.10	0.05
	(0.79)	(0.21)	(0.35)
Month $\tau+$ 5	0.53	−0.12	−0.21
	(0.92)	(0.10)	(0.30)
Month $\tau+$ 6	0.14	0.15	0.58
	(1.22)	(0.10)	(0.57)
lag Conviction Rate State	0.12***		
	(0.03)		
lag Conviction Rate FED		0.01	
		(0.01)	
lag Conviction Rate SRO			0.03
			(0.04)
Controls	No	No	No
Time fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Clustered SE	State	State	State
Observations	16,758	16,758	16,758
Adjusted R^2	0.14	0.04	0.14
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6} + \tau_{-5} + \tau_{-4} + \tau_{-3} + \tau_{-2} + \tau_{-1} = 0$	0.615	0.306	0.094
$\tau_{+1} + \tau_{+2} + \tau_{+3} + \tau_{+4} + \tau_{+5} + \tau_{+6} = 0$	0.913	0.620	0.570

Notes: * (p<0.1), ** (p<0.05), *** (p<0.01); This table shows falsification tests on our main results. 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 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 A5—: 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 $\tau-6$	0.29	0.14*	0.27
	(1.10)	(0.07)	(0.47)
Month $\tau-5$	−1.39	0.03	−0.32
	(1.32)	(0.14)	(0.32)
Month $\tau-4$	−0.96	−0.06	0.11
	(0.77)	(0.06)	(0.27)
Month $\tau-3$	0.46	−0.19	0.09
	(0.78)	(0.28)	(0.22)
Month $\tau-2$	−0.36	0.23	−0.11
	(0.59)	(0.17)	(0.32)
Month $\tau-1$	−0.21	−0.00	0.13
	(0.76)	(0.09)	(0.29)
Month 0: Gubernatorial Elect. lead 24	−1.16	−0.16*	0.26*
	(1.20)	(0.09)	(0.15)
Month $\tau+1$	−2.05*	0.08	0.28
	(1.22)	(0.17)	(0.29)
Month $\tau+2$	−0.62	0.13	−0.12
	(0.89)	(0.16)	(0.19)
Month $\tau+3$	−0.78	0.28*	0.38*
	(0.69)	(0.16)	(0.22)
Month $\tau+4$	1.74*	−0.04	−0.70
	(0.94)	(0.25)	(0.47)
Month $\tau+5$	1.82	−0.14	−0.24
	(1.22)	(0.13)	(0.28)
Month $\tau+6$	0.06	0.36**	−0.50
	(0.86)	(0.16)	(0.42)
lag Conviction Rate State	0.12***		
	(0.03)		
lag Conviction Rate FED		0.01	
		(0.01)	
lag Conviction Rate SRO			0.03
			(0.04)
Controls	No	No	No
Time fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Clustered SE	State	State	State
Observations	16,758	16,758	16,758
Adjusted R^2	0.14	0.04	0.14
Hypothesis	Pr(>F)	Pr(>F)	Pr(>F)
$\tau_{-6}+\tau_{-5}+\tau_{-4}+\tau_{-3}+\tau_{-2}+\tau_{-1}=0$	0.457	0.732	0.681
$\tau_{+1}+\tau_{+2}+\tau_{+3}+\tau_{+4}+\tau_{+5}+\tau_{+6}=0$	0.926	0.051	0.320

Notes: * (p<0.1), ** (p<0.05), *** (p<0.01); This table shows falsification tests on our main results. 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 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 A6—: 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 $\tau- 6$	-0.07	0.31^*	-0.66
	(1.10)	(0.16)	(0.48)
Month $\tau- 5$	0.32	0.35	0.50
	(1.86)	(0.31)	(0.36)
Month $\tau- 4$	-0.42	0.15	-0.01
	(0.69)	(0.11)	(0.23)
Month $\tau- 3$	-1.20	-0.34	-0.12
	(0.74)	(0.23)	(0.29)
Month $\tau- 2$	1.18	0.11	-0.37
	(0.96)	(0.16)	(0.32)
Month $\tau- 1$	0.78	0.00	0.06
	(1.10)	(0.10)	(0.19)
Month 0: Gubernatorial Elect. lead 36	1.70	0.09	-0.03
	(1.34)	(0.11)	(0.23)
Month $\tau+ 1$	0.89	0.07	-0.01
	(2.34)	(0.18)	(0.17)
Month $\tau+ 2$	1.82	-0.34	0.05
	(2.10)	(0.25)	(0.28)
Month $\tau+ 3$	3.39	-0.13	-0.07
	(2.16)	(0.08)	(0.18)
Month $\tau+ 4$	0.69	0.11	0.06
	(0.73)	(0.12)	(0.36)
Month $\tau+ 5$	0.50	0.05	0.12
	(1.06)	(0.10)	(0.36)
Month $\tau+ 6$	-0.64	-0.14	-0.62
	(1.38)	(0.09)	(0.53)
lag Conviction Rate State	0.12^{***}		
	(0.03)		
lag Conviction Rate FED		0.01	
		(0.01)	
lag Conviction Rate SRO			0.03
			(0.04)
Controls	No	No	No
Time fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Clustered SE	State	State	State
Observations	16,758	16,758	16,758
Adjusted R^2	0.14	0.04	0.14
Hypothesis	Pr($>$F)	Pr($>$F)	Pr($>$F)
$\tau_{-6} + \tau_{-5} + \tau_{-4} + \tau_{-3} + \tau_{-2} + \tau_{-1} = 0$	0.839	0.226	0.274
$\tau_{+1} + \tau_{+2} + \tau_{+3} + \tau_{+4} + \tau_{+5} + \tau_{+6} = 0$	0.159	0.334	0.507

Notes: * (p<0.1), ** (p<0.05), *** (p<0.01); This table shows falsification tests on our main results. 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 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.