Financial Regulatory Actions over the Cycle

By Paul Berenberg-Gossler
Gonçalo Pina*

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This paper shows that regulatory actions against misconduct in the financial industry are driven by business cycles. Using text mining techniques, we construct a new database of regulatory actions based on US asset managers filings distinguishing between state, federal, and self-organized regulators. Our data cover 9,750 regulatory actions and fines across 49 states over the 1990-2019 period. We then show that the number of regulatory actions responds to the business cycle with a lag. It is consistently lower following economic boom periods and higher following busts. To establish causal evidence, we combine our data with information on 24 million federally administered military contracts that affect state-level business cycles. Exploiting these contracts, we find that after a positive state-level output shock, regulatory actions decrease up to 60 months at the state regulatory level, but increase at the federal-level. Measures based on regulatory fines mirror this pattern. Our findings suggest that state-level regulatory actions are pro-cyclical, and federal-level regulatory actions are counter-cyclical, suggesting that different levels of regulatory agencies respond differently to business cycles.
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Policy makers and the general public regularly highlight misconduct and morally reprehensible behavior of the US asset manager industry. While descriptive evidence has shown that the regulatory stance towards finance tends to weaken during booms and strengthen following busts (Dagher, 2018), the factors driving enforcement of regulations have not yet been systematically documented. Given that the moral hazard issues of financial sector misconduct and subsequent government intervention have been at the center stage of recent crisis episodes, it is important to know how enforcement of regulatory laws differs over the cycle. This paper attempts to document the behavior of US regulatory institutions at different stages of the economic cycle. We examine regulatory actions at different

* Berenberg-Gossler: Hertie School, Friedrichstrasse 180, 10117 Berlin., paul.berenberg-gossler@outlook.com. Pina: ESCP Business School, Heubnerweg 8-10, 14059 Berlin., gpina@escp.eu.
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administrative levels, and shed light on how the business cycle shapes financial regulatory actions.

To study how regulatory actions vary over the cycle, we first construct 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.[1] Focusing on US data at the state-level allows us to run our analysis over a common legal and institutional framework, and to isolate the effect of the business cycle on regulatory actions.

Relatively little attention has been paid to the regional variation in regulatory actions. 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 firm location, which is not necessarily where the infringement occurred.[2] 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.[3]

The corrected data show that the average fine imposed on US financial asset managers is $1,7 million 2019 US dollars. However, there is large variation in the number of regulatory actions across US states, type of regulators, and time periods. The number of actions typically skyrockets after episodes of recessions and falls following economic booms.

We aggregate our newly constructed dataset at the monthly level to investigate whether financial regulatory actions are related to the business cycle. We use two measures of regulatory activity. First, we define a 'conviction rate', referring to the number of regulatory actions, e.g. convictions of asset managers, as a share of state-level employment in the financial sector. Second, we compute a 'fine rate', taking into account the monetary fines levied on asset managers following a conviction normalized by state-level employment in the financial sector.

To study regulatory actions at the state-level, we exploit within-US variation in state-level business cycles while controlling for the aggregate US cycle. This approach allows us to abstract from laws and institutions that are common to

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

[2]Exceptions are the data-sets from Karpoff, Lee and Martin (2008) and Correia (2014). In general, there are two other potential data sources. First, the NYU Stern Securities Enforcement Empirical Database (SEED), which tracks and records information for SEC enforcement actions. However, the NYU data only focus on actions filed against public companies traded on major US exchanges and their subsidiaries and mostly focus on the US as a whole. Second, the FINRA Brokercheck database has data on the background of broker dealers and firms, which have a partial overlap with the IAPD database (Egan, Matvos and Seru, 2019). In January 2019, for instance, 46% of all investment advisor offices in our database also carried out broker related activities. These data, thus, overlap with our raw data as brokers are also supervised by FINRA. However, compared to Egan, Matvos and Seru (2019), who study the labor market consequences of individual financial advisor misconduct over the 2005-15 period, we draw our data from firm-level regulatory actions for financial advisory activities, over a larger time-span, and focus on its cyclical properties.

[3]The data present a large improvement and cover a larger time span relative to previous work. See for instance data from 2002-2014 from Velikonja (2015).

all states, as well as common economic shocks. Thus, we focus on the intensive margin of regulatory actions, that is the enforcement of laws by regulators, at the state-level.[4]

One challenge for identifying the effect of the business cycle on regulatory actions is that economic conditions are potentially endogenous as regulatory actions may affect economic activity, even in the short-run. To solve this potential reverse causality problem, we use an instrumental variable approach inspired by the fiscal multiplier literature (Nakamura and Steinsson, 2014; Demyanyk, Loutskina and Murphy, 2019; Auerbach, Gorodnichenko and Murphy, 2019). We collect new data on military spending over the 1999-2019 period covering 24 million federally administered contracts. We exploit two main features of the military spending data. First, national military spending fluctuates with wars abroad and military draw-downs. Second, states respond distinctively to these fluctuations. Using these differences in the sensitivity of military spending across states to fluctuations in national military spending, we identify military expenditure shocks that affect state-level economic activity, as the change in national military expenditure interacted with state-specific sensitivity to aggregate military expenditure shocks. Our identifying assumptions are that only the business cycle directly relates to state-specific, exogenous, military expenditure shocks to regulatory actions, and that this relationship is strong. Under these assumptions, we obtain causal estimates on the relationship between state-business cycles and regulatory actions.

Our results show that the business cycle causes regulatory actions. We find that following a positive state-level output shock, the conviction rate is reduced, but only at the state regulatory level. This effect is significantly different from zero at conventional levels of statistical significance. On the contrary, at the federal level, the conviction rate increases following a positive shock on output. The findings for the fine rate mirror this pattern. It decreases following a positive shock at the state-level, but increases at the federal level, although the latter is not significantly different from zero.

We test the robustness of our main results and the added value of our new database of financial regulatory actions. Our main results are robust to using alternative specifications, to different measures of economic activity, to controlling for state and federal-level government shutdowns, and taking into account electoral cycles. Rerunning our main specifications using the untreated SEC data shows different results and highlights the importance of our first contribution, the newly created dataset on regulatory actions.

Our findings extend and contribute to three strands of the literature. First, the data and stylized facts are relevant because they provide a *de facto* measure of financial regulation, thereby completing previous research which usually relies

[4]Crucially, in this paper we focus on the overall effect of the business cycle on regulatory actions, without decomposing the effect through different mechanisms. For example, our data do not allow us to distinguish if changes in regulatory actions are driven by more misconduct or a stricter regulatory stance.

on *de jure* measures of financial regulation. *De jure* measures are usually obtained by hand coding laws on a specific scale and have been extensively used in the literature (Porta et al., 1998; La Porta, Lopez-de Silanes and Shleifer, 2006). However, *de jure* measures come with two major drawbacks. First, they do not necessarily translate into binding legislative action.[5] Second, current *de jure* regulatory convergence across US States is high, making this extensive margin of financial regulation less relevant for the US. Thus, most papers either focus on historical episodes or cross-country studies when studying the determinants and the effects of financial regulation on economic outcomes.[6] While both approaches have merit, specific historical episodes are not necessarily informative for current policy and cross-country studies are often too coarse to establish causation because there are numerous confounding factors (Jackson and Zhang, 2015). In this paper, we focus instead on the intensive margin of financial regulation and on how it is determined over the cycle.

Second, we contribute to the literature on regulatory enforcement within the US. Our results showing that state and federal regulatory agencies respond differently to business cycles are particularly related to Agarwal et al. (2014), who show that federal regulators are tougher in regulating banks compared to state-level regulators. Other strands of this growing literature have focused on political incentives (Heese, 2019), on whether regulatory agencies are captured by interest groups (Correia, 2014; Heese, Khan and Ramanna, 2017), on the economic effects of *de jure* regulation (Danisewicz et al., 2018; Dagher and Fu, 2017; Granja and Leuz, 2017), on the labor market effects of individual financial advisor misconduct (Egan, Matvos and Seru, 2019), and on the direct effect of regulatory changes on individual behavior (Kowaleski, Vetter and Sutherland, 2020). We complement this literature by investigating the business cycle determinants of *de facto* financial regulation in the United States.[7]

Third, the findings of our paper are also related to the literature on the interplay between financial regulation policies and economic outcomes outside of the US (Claessens, Ghosh and Mihet, 2013; Richter, Schularick and Shim, 2019; De Schryder, Opitz and others, 2019).

The rest of the paper is structured as follows. Section I explains the background of the US asset manager industry and the governance structure of the regulatory system. Next, we describe the dataset, its construction, and stylized facts of regulatory actions in section II. In section III we exploit our detailed data on regulatory actions and provide causal evidence on how regulatory intensity varies over the business cycle. Section IV concludes.

[5]Simply because a securities regulator has ample resources does not necessarily guarantee that it utilizes them to bring enforcement actions (Jackson and Roe, 2009; Jackson, 2007).

[6]Benmelech and Moskowitz (2010), for instance, study usury laws in 19th century America and Porta et al. (1998); La Porta, Lopez-de Silanes and Shleifer (2006) use cross-country variation of legal rules.

[7]See Leuz and Wysocki (2016) for a survey of the empirical literature on the economic consequences of financial reporting and disclosure regulation while examining some key studies that investigate economic outcomes of voluntary disclosures.

I. Background: Financial regulation and the US investment advisor industry

This section gives a short overview of the US financial regulatory framework and the US financial asset advisor industry.

A. *Financial regulation in the US: A brief overview*

The current US financial regulatory framework has developed since the 19th century. Currently, there are three distinct layers of financial (securities) regulation: (1) State-level institutions; (2) Federal institutions; and (3) Self-Regulatory Organizations (SRO).

State-level regulation was the first to emerge. 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 time periods(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.[8]

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

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

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 advisors or broker dealers need to be licensed at the state-level at which they operate.[9] Third, state-securities regulation aims at preventing fraudulent practices. Thus, 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. Historically, SRO organizations were the first to emerge. 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.

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

B. The US investment advisor industry

Investment Advisors 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 asset managers reported total assets under management of nearly $21.4 billion US dollars in 2018. The US asset management 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 advisors are required to provide information to regulatory institutions. Specifically, each Investment Advisor is required to file Form Investment Advisors Disclosure (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 advisors 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: 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.[10] Sub-section II.A describes data construction, sub-section II.B defines the two main measures of regulatory activity, and sub-section II.C discusses stylized facts on financial regulatory actions.

[10]Previous papers have used the same raw data but focused on other sub-parts. Gupta (2017), 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.

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 asset advisors, such as firm names and geographical location, that can be matched with past regulatory actions. For each regulatory action we observe the exact start and end dates of the procedure. ‘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. 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.

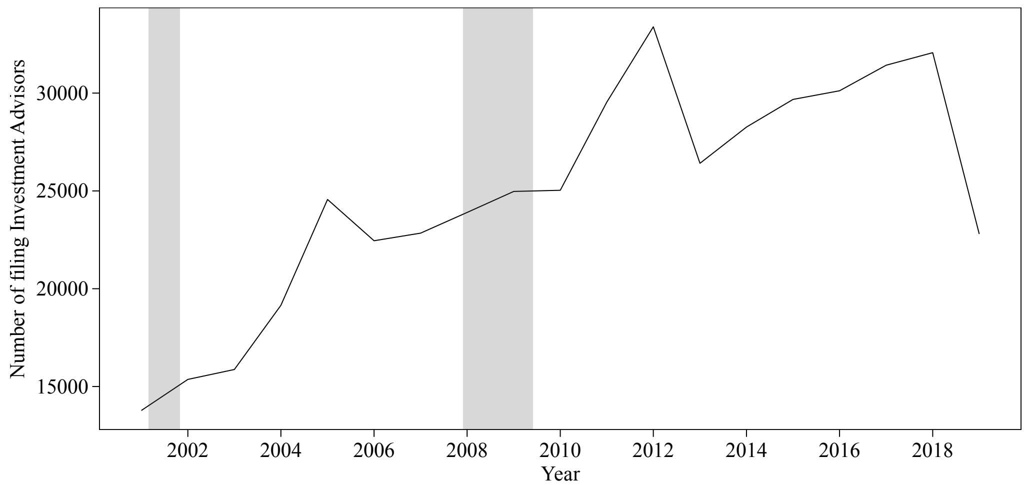
Since we have regulatory data on both registered and unregistered investment advisors, we cover a slightly bigger share of advisors in the US asset industry compared to what the US Investment Company Institute reports (Institute, 2019). By construction, the data comprise regulatory actions targeting advisors that have filed at least once over the 2001-2019 period. A regulatory action targeting an advisor that stopped filing in 2000, but was subject to regulatory actions prior to this date, does not show in the data. Thus, our main analysis in section III covers the 1999-2019 period. Yet, we still present the data over the 1990-2019 period since most of the major players of the US asset manager industry have filed at least once since 2001.[11] Figure 1 shows the yearly sum of distinct registered and exempted investment advisors over the 2001-19 period that have filed at least once and are therefore captured by our data. On average, 24,822 investment advisors file regulatory information each year.

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, 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 asset manager 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.

[11]Table 4 shows that we do not see a significant drop in disclosure on regulatory actions prior to 2001. Descriptive statistics and figures in this section thus cover the 1990-2019 period. Our empirical estimates for the effect of the business cycle on regulatory actions are, however, only based on the 1999-2019 period.

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.

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.[12] Thus, we face two possibilities. 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 regulatory actions that have not yet been located we exploit manually entered descriptions. Each conviction is provided with a manually entered 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 1. 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 Committe 7. According to table A1, NASD/FINRA District 7 covers

[12]The number of districts and their numbers have not changed since 1990. See table A1 for an overview.

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 given, allows us to conclude that the regulatory action most likely took place in Charlotte, North Carolina.

Table 1—: 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

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.[13]

Table 2 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 advisors. 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 Texas that initially showed up elsewhere.[14]

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

[13]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.

[14]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.

Table 2—: Number of convictions per state

	State	N	N (not corr.)	Difference
1	Alabama	127	60	67
2	Alaska	32	2	30
3	Arizona	118	46	72
4	Arkansas	93	44	49
5	California	500	530	-30
6	Colorado	133	96	37
7	Connecticut	297	193	104
8	Delaware	71	17	54
9	District of Columbia	60	11	49
10	Florida	405	308	97
11	Georgia	103	83	20
12	Hawaii	50	2	48
13	Idaho	49	1	48
14	Illinois	603	233	370
15	Indiana	105	66	39
16	Iowa	99	55	44
17	Kansas	101	55	46
18	Kentucky	85	38	47
19	Louisiana	56	13	43
20	Maine	56	6	50
21	Maryland	233	90	143
22	Massachusetts	425	599	-174
23	Michigan	80	61	19
24	Minnesota	299	424	-125
25	Mississippi	51	6	45
26	Missouri	339	494	-155
27	Montana	79	30	49
28	Nebraska	84	63	21
29	Nevada	112	11	101
30	New Hampshire	99	21	78
31	New Jersey	279	463	-184
32	New Mexico	52	1	51
33	New York	2427	4006	-1579
34	North Carolina	83	41	42
35	North Dakota	71	2	69
36	Ohio	127	137	-10
37	Oklahoma	57	15	42
38	Oregon	85	17	68
39	Pennsylvania	230	168	62
40	Rhode Island	99	19	80
41	South Carolina	51	2	49
42	South Dakota	59	0	59
43	Tennessee	78	52	26
44	Texas	543	308	235
45	Utah	72	9	63
46	Vermont	103	8	95
47	Virginia	209	140	69
48	Washington	72	28	44
49	Wisconsin	84	51	33
50	Wyoming	25	2	23
51	Total	9750	9127	623

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 intensity

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. We compute two measures of regulatory actions. First, equation (1) defines a *conviction Rate* measure based on the absolute number of convictions per month.

$$ConvRate_{it} = \frac{Conv_{it}}{E_{it}}, \tag{1}$$

where $Conv$ is sum of the number of actions, e.g. convictions, in state i at time t. We normalize the sum of convictions per US state by E, the number of employees in the financial sector of the respective state at time t. Thus, similar to a measure of crime rate, we adjust for the fact that states with a higher number of employees in the finance sector may have a higher number of regulatory infringements.

Our second measure *FineRate* defined in (2) is based on the magnitude of regulatory fines.

$$FineRate_{it} = \frac{Fine_{it}}{E_{it}}, \tag{2}$$

where $Fine_{it}$ is the deflated regulatory fine attached to each conviction. We sum deflated monetary fines per state and normalize by E, the number of employees in the financial sector of the respective state. We deflate fines using monthly data on Consumer Price Indices (CPI) from the Bureau of Labor Statistics (BLS). Since CPI data for US states are only available at the Davis-Bacon Related Acts (DBRA) region level (e.g. Northeast), we deflate fines using the corresponding region-level figures (CPI = 100 for January 2019). For E, the number of employees in the respective state's financial sector, we retrieve monthly data from the BLS for each state over the 1990-2019 period.

C. Financial regulatory actions: Stylized facts

The data on regulatory convictions and fines reveal a series of insights on financial regulation.

The first insight is the large cross-state heterogeneity, both concerning the mean of convictions and the attached regulatory fines. Table 3 computes descriptive statistics for the total sample and individual states. The average number of

monthly convictions across all 49 states and the Federal District of Columbia in the sample is 0.2 and the monthly mean for regulatory fines is 1.7 million 2019 US Dollars. New York is the state with the highest value for the monthly mean of convictions, equal to 2.3, and the highest monthly mean fine, equal to 43.7 million 2019 US Dollars. Measures of standard deviation for these two variables also show very large heterogeneity across states.

The second insight is that there is substantial heterogeneity across types of regulators. Table 4 shows descriptive statistics for state, federal, or Self Regulating Organization (SRO) regulators. While the mean of regulatory actions and its standard deviation are relatively similar for state regulators and SRO, fines tend to be larger when pronounced by the federal level. The mean fine of federal legislators is 3.4 million 2019 US dollars, while mean fines of state- and SRO-regulators equal 1.4 and 0.2 million 2019 US Dollars.

The third insight is that there is variation over time. While the mean number of cases is mostly around 0.1-0.3 over the 1990-2019 period, the amount of fines peak in certain years, with a maximum in 2014 with a mean fine of 12.8 million 2019 US Dollars. In the rest of the paper we investigate whether the variation over time in regulatory intensity is related to economic conditions at the state-level.

III. Regulatory actions over the cycle

In this section we use our detailed data on regulatory actions and study how regulatory intensity varies over the business cycle. To motivate our empirical analysis, we first show aggregate data for the United States and discuss potential reasons for the cyclical behavior of regulatory actions. Finally, we study systematically how regulatory actions depend on economic cycles at the state-level. Exploring variation within US states represents a unique research design to study regulatory intensity because many institutional variables that matter for regulation are common to all states. Thus, it allows us to focus on the effect of the intensive margin of regulation, that is, the enforcement of existing laws and institutions, rather than the development of new laws or institutions as it is usually done in cross-country research.

A. Regulatory actions in the United States

In figure 3 we plot the measures of regulatory actions in the United States, together with shaded areas representing NBER recession dates. For the two measures of regulatory actions defined in section II.B, the conviction rate and the fine rate, we show pooled data over the three regulatory levels, federal, state and self-regulatory organizations.

The top panel plots the evolution of the conviction rate while the bottom panel shows the fine rate. The sample covers the full extension of our dataset, 1990 to 2019. Although the data at the monthly level are relatively noisy, it is possible to see for both time-series evidence of cyclical behavior related to the economic

Table 3—: Regulatory actions by state

	Convictions					Fine (Mill. US Dollar.)			
	N	Mean	St. Dev.	Min	Max	Mean	St. Dev.	Min	Max
Total Sample	9,750	0.2	0.8	0	27	1.7	83.4	0	13,613.6
By State									
Alabama	127	0.1	0.4	0	3	0.3	4.7	0	114.2
Alaska	32	0.0	0.2	0	4	0.0	0.5	0	17.5
Arizona	118	0.1	0.4	0	4	0.2	4.0	0	118.8
Arkansas	93	0.1	0.4	0	9	0.1	3.5	0	113.0
California	500	0.5	0.9	0	6	3.9	87.1	0	2,820.9
Colorado	133	0.1	0.4	0	3	1.4	18.7	0	416.2
Connecticut	297	0.3	0.6	0	8	1.0	12.2	0	306.5
Delaware	71	0.1	0.3	0	5	0.2	3.7	0	112.1
District of Columbia	60	0.1	0.3	0	6	0.1	3.5	0	113.0
Florida	405	0.4	0.8	0	7	0.4	4.3	0	113.3
Georgia	103	0.1	0.4	0	7	0.2	4.0	0	114.2
Hawaii	50	0.0	0.3	0	7	0.2	3.8	0	114.8
Idaho	49	0.0	0.3	0	4	0.1	3.7	0	117.0
Illinois	603	0.6	1.0	0	10	0.4	5.7	0	147.9
Indiana	105	0.1	0.4	0	3	0.1	3.4	0	107.9
Iowa	99	0.1	0.4	0	6	0.2	3.8	0	109.2
Kansas	101	0.1	0.4	0	4	0.5	10.6	0	326.0
Kentucky	85	0.1	0.3	0	3	0.1	3.5	0	114.2
Louisiana	56	0.0	0.4	0	8	0.0	0.7	0	21.7
Maine	56	0.0	0.3	0	4	0.2	3.8	0	113.0
Maryland	233	0.2	0.6	0	7	1.2	20.7	0	562.9
Massachusetts	425	0.4	0.8	0	6	2.3	21.0	0	351.2
Michigan	80	0.1	0.4	0	8	0.2	3.6	0	107.5
Minnesota	299	0.3	0.6	0	5	4.0	71.4	0	2,081.7
Mississippi	51	0.0	0.3	0	4	0.1	3.5	0	112.6
Missouri	339	0.3	0.7	0	4	0.7	9.5	0	190.8
Montana	79	0.1	0.3	0	3	0.1	3.6	0	115.4
Nebraska	84	0.1	0.3	0	3	1.1	30.6	0	990.1
Nevada	112	0.1	0.5	0	7	0.2	3.6	0	116.4
New Hampshire	99	0.1	0.4	0	3	0.1	1.5	0	44.0
New Jersey	279	0.3	0.6	0	4	14.3	418.0	0	13,613.6
New Mexico	52	0.0	0.3	0	8	0.0	0.6	0	17.5
New York	2,427	2.3	3.4	0	27	43.5	393.7	0	9,435.9
North Carolina	83	0.1	0.3	0	3	1.1	26.3	0	827.2
North Dakota	71	0.1	0.3	0	6	0.2	3.5	0	107.9
Ohio	127	0.1	0.4	0	4	0.3	4.3	0	109.8
Oklahoma	57	0.0	0.3	0	5	0.1	1.8	0	52.8
Oregon	85	0.1	0.4	0	5	0.2	3.8	0	117.9
Pennsylvania	230	0.2	0.6	0	6	0.4	5.2	0	114.4
Rhode Island	99	0.1	0.4	0	6	0.5	11.2	0	347.8
South Carolina	51	0.0	0.3	0	5	0.0	0.7	0	20.6
South Dakota	59	0.1	0.3	0	6	0.1	3.3	0	107.2
Tennessee	78	0.1	0.3	0	3	0.1	1.0	0	26.5
Texas	543	0.5	0.9	0	6	2.4	17.3	0	216.6
Utah	72	0.1	0.4	0	7	0.2	3.8	0	116.4
Vermont	103	0.1	0.4	0	4	0.2	3.7	0	113.0
Virginia	209	0.2	0.7	0	10	0.4	6.8	0	140.7
Washington	72	0.1	0.4	0	5	0.2	3.7	0	117.9
Wisconsin	84	0.1	0.4	0	5	0.2	3.8	0	109.8
Wyoming	25	0.0	0.2	0	4	0.1	3.6	0	117.0

This table shows summary statistics of convictions and fines for the monthly balanced panel. Data for West Virginia are not covered by the SEC bulk data. We include the District of Columbia although it is not a state and therefore these data are not used in the empirical regressions. Fines are in million for better readability and deflated with DBRA region level CPI (January 2019 = 100).

Table 4—: Regulatory actions by type of regulator

	Convictions					Fine (Mill. US Dollar.)			
	N	Mean	St. Dev.	Min	Max	Mean	St. Dev.	Min	Max
By Regulator Type									
Federal and Other	1,283	0.1	0.4	0	14	3.4	133.3	0	13,613.6
SRO	4,294	0.2	1.0	0	27	0.2	8.9	0	829.4
State	4,173	0.2	0.6	0	10	1.4	55.1	0	6,913.0
By Year									
1990	84	0.0	0.2	0	3	0.0	0.4	0	19.0
1991	160	0.1	0.4	0	5	0.0	0.0	0	0.6
1992	217	0.1	0.5	0	11	0.2	8.1	0	345.6
1993	205	0.1	0.4	0	4	0.4	13.7	0	562.9
1994	276	0.2	0.5	0	5	0.1	1.5	0	61.3
1995	219	0.1	0.5	0	7	0.0	0.2	0	8.4
1996	295	0.2	0.6	0	13	0.0	0.5	0	12.0
1997	256	0.1	0.6	0	12	0.1	3.2	0	135.5
1998	230	0.1	0.5	0	6	0.0	0.9	0	37.9
1999	314	0.2	0.6	0	8	0.1	1.3	0	36.0
2000	252	0.1	0.6	0	9	0.1	1.4	0	56.1
2001	224	0.1	0.5	0	6	0.0	0.5	0	14.4
2002	233	0.1	0.6	0	9	0.1	3.5	0	141.8
2003	562	0.3	1.0	0	17	2.4	25.1	0	829.4
2004	406	0.2	0.8	0	11	3.6	31.8	0	801.9
2005	401	0.2	0.7	0	10	3.3	19.5	0	256.1
2006	344	0.2	0.8	0	17	8.7	321.5	0	13,613.6
2007	354	0.2	0.8	0	12	0.3	4.6	0	137.9
2008	263	0.1	0.6	0	8	0.1	0.8	0	18.8
2009	514	0.3	0.8	0	8	1.0	26.5	0	1,115.3
2010	540	0.3	0.9	0	10	0.5	8.0	0	257.4
2011	402	0.2	0.7	0	8	0.6	7.4	0	187.7
2012	410	0.2	0.9	0	16	2.6	84.9	0	3,585.8
2013	438	0.2	1.0	0	16	1.6	21.9	0	534.9
2014	497	0.3	1.3	0	27	12.8	281.2	0	9,435.9
2015	471	0.3	1.1	0	15	3.8	61.9	0	2,110.9
2016	438	0.2	1	0	16	2.9	68.6	0	2,820.9
2017	311	0.2	0.8	0	16	1.0	15.8	0	438.2
2018	366	0.2	0.8	0	15	3.3	57.3	0	2,081.7
2019	68	0.1	0.3	0	5	0.0	0.4	0	10.3

This table shows summary statistics of convictions and fines for the monthly balanced panel. The sample stops in June 2019, thus we have fewer observations for 2019. SRO means Self Regulatory Organizations. Fines are in million for better readability and deflated with DBRA region level CPI (January 2019 = 100).

cycle. However, this relationship is not contemporaneous. For example, following the recession of 1990-91, both the conviction rate and fine rate experience two local peaks, approximately one and three years later. Following the expansion of the 1990s, the conviction rate observes a local trough just before the recession in 2001. These variables peak again two to five years later, and decrease during the second half of the 2000s, hitting a trough during the recession of 2007-09. A new peak follows a few years after the great recession.

These data suggest that regulatory actions respond to economic cycles with a lag. One reason why regulatory actions may react with a lag relative to the economic cycle is that it takes time to produce them. In our data we record only the date when a regulatory authority notifies a firm that it is being investigated and the resolution date of the case. However, we do not observe the date that triggered a particular investigation, nor the period while the process is under "informal investigation" or "matter under inquire". Given that these dates are not observable, we will allow in our empirical specifications for regulatory actions to respond to the cycle with a lag, without specifying a specific lag structure. Instead, our empirical analysis uses local projections, a method that allows us to highlight the dynamics of the response of regulatory actions to economic shocks.

B. *Estimating the effect of the business cycle on regulatory actions*

Measuring the business cycle

As described in section II, one of the unique features of our data on regulatory actions is that we have data at the daily frequency. However, data on economic conditions at the state-level are usually at a lower frequency. For example, real GDP, the most common way to measure the business cycle, is only available at the quarterly level for a subset of our sample. Instead, we turn to the coincident economic activity index, which is available for each state at the monthly frequency (Crone and Clayton-Matthews, 2005). This index includes 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 (GSP).

We use this index to extract one measure of the state of the economy at the monthly level. We compute the two year log-difference of the coincident economic activity index. Our choice of two years in the baseline regressions is related to the instrumental variable strategy that we implement in section III.D. Our second approach to measure the state of the economy mirrors Auerbach and Gorodnichenko (2013) and Jordà, Schularick and Taylor (2019)). We apply the Hodrick–Prescott filter, with a lambda equal to 129600 to match monthly data. After obtaining residuals from the trend index, we compute the coincident economic activity index gap as the deviation from the trend in percent of the trend. The correlation between our two measures of the business cycle is 0.62. Figures 6 and 7 plot the monthly time series of state-level two year log-difference of the coincident eco-

nomic activity index for Florida, California and the US economy. It is possible to see that these two states track the overall US economy over its booms and busts but substantial variation remains. In our empirical analysis we always control for the US business cycle, and explore state-specific differences relative to this cycle.[15] Figure 5 shows time-series for the coincident economic activity index by state, together with state-level recessions dated using the approach in Crone and others (2006).

Table 5—: Descriptive statistics: Local projections

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
Fine rate (State regulators)	10,500	11,791	182,809	0	9.505e+06
Conviction rate (State regulators)	10,500	0.00390	0.0160	0	0.392
Fine rate (All regulators)	10,500	27,694	496,765	0	3.904e+07
Conviction rate (All regulators)	10,500	0.00526	0.0167	0	0.392
Fine rate (Federal regulators)	10,500	15,282	458,960	0	3.904e+07
Conviction rate (Federal regulators)	10,500	0.000375	0.00271	0	0.177
gap	10,290	-0.00119	0.0192	-0.117	0.0768
gapUS	10,500	-0.00116	0.0123	-0.0261	0.0281
Total coincident (log diff 24)	10,290	0.0442	0.0476	-0.220	0.153
Total coincident US (log diff 24)	10,500	0.0402	0.0297	-0.0405	0.0691
predicted military expenditures L24 10 year predetermined from NS2014	10,500	0.000877	0.00415	-0.0233	0.0370

[15]See Crone and others (2006) for an analysis of state-specific business cycles using the economic coincident index.

Empirical strategy

We first examine the effects of business cycles on regulatory actions using Jordà (2005) local projections method which allows for continuous instruments. We estimate impulse response functions by computing responses to a shock measured over different horizons h, where the initial impact is defined as h=*0*, the one-month impact as h=*1*, and so forth. We use local projections since this method imposes fewer constraints on impulse response functions compared to VARs. Additionally, this approach allows us to estimate the average treatment effect of shocks given state heterogeneity. Furthermore, using local projections facilitates the inclusion of state-specific controls and instrumental variables. For each lead h = 1, 2, ..., n (in months), we estimate by ordinary least squares:

$$y_{i,t+h} = \alpha^{s}_{i,h} + shock_{i,t}\beta_h + shock_{US,t}\gamma_h + \boldsymbol{x'}_{i,t}\boldsymbol{\delta}_h + \varepsilon_{i,t+h} \tag{3}$$

where $y_{i,t+h}$ is a measure of regulatory actions in state i at horizon $t+h$ and $\alpha^{s}_{i,h}$ is a state fixed effect. The variable $shock_{i,t}$ captures the business cycle for state i at time t, using either the two year log-difference of the coincident economic activity index or the coincident economic activity index gap. The variable $shock_{US,t}$ is the corresponding variable capturing the business cycle for the United States, and $x_{i,t}$ is a vector of controls at the state-level. The variable $y_{i,t+h}$ refers to the cumulative sum over horizon h of either the fine rate or the conviction rate, as defined in section II.B. Note, however, that we fix the denominator in these rates at $h = 0$, to avoid effects coming from the change in employment in finance related to the business cycle. The impulse response at horizon h is β_h.

One important issue for identifying the effect of the business cycle on regulatory actions using equation (3) is that the measure of the business cycle is potentially endogenous. Regulatory actions, or the expectation of regulatory actions in the future, may have an impact on economic activity. For example, actions that include fines represent negative wealth shocks and may create frictions in financial intermediation that can impact investment, employment, and output.

To solve this reverse causality problem, we estimate equation (3) using an instrumental variable approach. Our approach is inspired by one strand of the macroeconomics literature that uses military expenditures to estimate open economy multiplier effects of government expenditures on different macroeconomic outcomes (see Nakamura and Steinsson (2014), Demyanyk, Loutskina and Murphy (2019), and Auerbach, Gorodnichenko and Murphy (2019)). These papers instrument for state-specific military expenditures by using aggregate military expenditures interacted with either a state-specific fixed effect, that captures the constant sensitivity of a particular state to aggregate movements in total military expenditures, or a Bartik-weight that captures the percentage of military expenditure received by a given state in the first years of the sample. One concern is that state-specific military contracts may depend on economic conditions, or

may not represent shocks if they are anticipated. Interacting aggregate swings in military expenditure, which are often determined by wars or military draw-downs abroad, with state-specific sensitivity parameters, provides a source of exogenous variation. This exogenous variation in state-specific military expenditure can be used to compute the effect of government expenditures on the variable of interest. When exploiting this instrumental variable strategy, we need to be aware that the effects of military contracts on economic activity may take time to materialize. To account for this, Nakamura and Steinsson (2014) estimates of fiscal multipliers use two-year percent changes in military expenditure, deflated by the CPI index, as a percent of state-level real GDP, as the fiscal impulse. They also use an analogous measure at the national level. In this section, we reproduce their approach using monthly data, and use two year changes in military expenditure, interacted with Bartik-weights, as our instrument for the business cycle.

We first obtain data from the US Department of Defense on prime-level award contracts for the fiscal years 2001-2019. Fiscal years run between October 1 of year t and September 30 of year $t+1$. After cleaning the data, we distribute amounts uniformly over the duration of the contract following the method outlined in Demyanyk, Loutskina and Murphy (2019). Because Demyanyk, Loutskina and Murphy (2019) cover the period 1999-2013, we use their data from 1999-2001 to extend our data backwards up to 1999. However, contrary to their approach, we take a more conservative approach and clean cancellations of contracts using the unique contract identifier. This leaves us with more than 24 million unique contracts. After collapsing at the monthly state-level, our data are strongly correlated with the dataset from Demyanyk, Loutskina and Murphy (2019), with correlation coefficients above 0.95 for the overlapping fiscal years in both samples.

Note, however, that our focus is not on the effect of government expenditures on state-level output. Instead we exploit these data as a source of exogenous variation in output to compute the causal effect of economic conditions on regulatory actions. The exclusion restriction assumption is that aggregate military expenditure affects regulatory actions at the state-level only through its effect on state-level output as measured by the coincident economic index. We assume further that regulatory intensity at the state-level does not have a causal impact on aggregate military expenditures.

The first-stage of our estimation is given by:

$$shock_{i,t} = \zeta_i + \eta_h shock_{US,t} + \zeta_i X expenditures_{US,t} + \boldsymbol{x'}_{i,t}\theta\gamma_{i,t} + \epsilon_{i,t} \tag{4}$$

where ζ_i represents a Bartik-weight that is state specific, and $expenditures_{US,t}$ are aggregate US military expenditures. The term X captures an interaction. From this equation we obtain predicted values for $shock_{i,t}$, defined as $shock^p_{i,t}$, which can then be replaced in the second-stage of our estimation:

$$y_{i,t+h} = \alpha^s_{i,h} + shock^p_{i,t}\beta_h + shock_{US,t}\gamma_h + \boldsymbol{x'}_{i,t}\boldsymbol{\delta}_h + \varepsilon_{i,t+h} \tag{5}$$

Alternatively, we could run a three-stage procedure, instrumenting state-level military expenditures with aggregate military expenditures, interacted with a Bartik-weight, in the first-stage to obtain predicted state-level military expenditures. These could then be used to instrument the economic shock at the state-level in the second-stage, obtain predicted values for our measure of the business cycle, and use these predicted values in the third-stage. However, the latter predicted values would be equal to the first predicted values times a constant as no additional variation is being used. Therefore we use aggregate military expenditures directly to instrument for economic conditions, the reduced-form formulation, and estimate the causal effect of the economic cycle on regulatory actions.[16]

Turning to instrument relevance, we obtain F-statistics in the first stage described in equation (4) equal to 38 at horizon zero. The F-statistic slowly declines for estimations at longer horizons and equals 22 for our maximum horizon of 80 months. Armed with this strong instrument, we estimate equations (3) and (5).

We use impulse response functions to illustrate the response of regulatory actions to economic shocks. In the figures that follow, the thick blue lines display the effects following a one-standard-deviation increase in the variable measuring the economic cycle, with confidence intervals at the 90%- and at the 95%-levels shown by thin black and dashed lines, respectively. To be consistent with the instrumental variable estimation, the sample includes only observations for which we have data on military expenditures. Table 5 shows descriptive statistics for our monthly panel between 1999:1 - 2019:5. We show results using both the two-year log difference in the coincident economic index and the total coincident gap as measures of the business cycle.

C. The correlation between business cycles and regulatory intensity

In this sub-section we present results from estimating equation (3) using ordinary least squares. We show results for all levels of regulatory agencies, but also for state regulator, federal regulators and self-regulatory organizations.

Examining figure 8 shows that the correlation between economic conditions and convictions is zero at small horizons, but turns negative and bottoms out after 60 months. The results for conviction rates by state regulators, depicted in the upper right panel of figure 8, look remarkably similar. However, the results for conviction rates by federal regulators represented by the lower left panel in figure 8 are different. The economic cycle is positively correlated with conviction rates by federal regulatory agencies, with the effect peaking at about 50 months.

[16]We also ran a specification assuming that state-level military expenditures are exogenous and, therefore, a valid instrument for state-output. We obtain similar results.

We now turn to the fine rate. Figure 9 shows that the fine rate is positively correlated with economic conditions, peaking at 50 months. This result is consistent with results for both the state and federal regulators as shown in the top right and bottom left panels of figure 9.

D. *The causal effect of business cycles on regulatory intensity*

We now turn to the causal effect of business cycles on regulatory actions, using the strategy outlined above. Figure 10 indicates that the causal effect of economic conditions on the conviction rate is now negative earlier and bottoms out after about 50 months. The results for the conviction rate by state regulators, depicted in the upper right panel of figure 10, look similar. However, they are stronger compared to the OLS estimate and bottom out earlier in between 30 and 50 months. The results for the conviction rate by federal regulators represented in the lower left panel in figure 10 once again look the opposite. The economic cycle has a positive causal effect on the conviction rate by federal regulatory agencies, with the effect peaking at about 45 months. This effect is also stronger, in terms of magnitude, when compared to OLS estimates.

Finally, we analyze the fine rate, which is represented in figure 11. The pattern is once again similar to OLS, but the monetary amounts are closer to zero for all types of regulators and the federal level. However, at the state-level, we obtain different results compared to OLS estimates. The economic cycle has a causal negative impact on fines levied by state regulators at all horizons, bottoming out at around 50 months.

E. *Robustness*

In this section, we investigate the robustness of our findings to a different measure of the economic cycle, introducing additional controls at the state-level and placebo tests.

Alternative measures of the business cycle

In this sub-section we reproduce our baseline results using total economic coincident index gaps as the measure of the business cycle. To obtain measures of total economic coincident index gaps similar to Auerbach and Gorodnichenko (2013); Jordà, Schularick and Taylor (2019), we use a two step process. First, we apply the Hodrick–Prescott filter, with a lambda equal to 129600 to match monthly data. Second, after obtaining residuals from the trend index, we compute the output gap as the deviation from the trend in percent of the trend. Additionally, our specifications also control for state-fixed effects and the global US total economic coincident index gap, and cluster standard errors at the state-level. Figures 12-15 show a similar pattern to our baseline regressions both in the OLS and the IV specifications. However, note that the F-statistic for the

first-stage in our IV regressions using this measure of the economic cycle is 6.1, which is smaller than the corresponding statistic for the two-year log differences.

Additional controls

We now include additional controls to our instrumental variables estimation. First, we include a dummy variable that takes on a value of 1 at t if either the federal or the state government are experiencing a shutdown. The shutdown dummy captures whether there are resources to produce regulatory actions. Anecdotal evidence suggests that some regulatory agencies see a drop in resources during these events. Second, we test the robustness of our results to political motives and electoral cycles. Important contributions to the literature have shown that incumbent governments benefit from positive economic results and, thus, have an incentive to manipulate policies prior to elections.[17] Thus, we first include a complete set of dummies for the party of the governor in each state at t. The party affiliation dummy captures any potential political alignment stance related to regulatory intensity. Figures 16 and 17 show that results are robust to these state-specific controls.

Placebo tests

Finally, we include results for two sets of placebo tests. First we regress regulatory actions on the 24-month forward difference in the total economic activity index, instrumented by the 24-month forward change in total federal military expenditure interacted with state-fixed effects. Figures 18 and 19 show the results. It is possible to see that the estimated effect sizes are much smaller compared to the lag differences.

Second, we run the baseline regressions using untreated data. That is, we do not use the algorithm described in section II.A to correctly attribute cases to each state, although we correct for duplicates. Figures 20 and 21 show the results. Importantly, we see that results for all levels of regulators are again closer to zero. These results highlight the importance of the first contribution of this paper in correctly treating the data from the SEC.

F. Discussion

Although the goal of this paper is to establish stylized facts regarding the behavior of regulatory intensity over the cycle, it is useful to consider reasons why regulatory intensity may depend on the business cycle. There are at least three different perspectives on this relationship.

The first one relates to the motives of the regulatory agencies. For example, regulators may be sensitive to economic conditions and/or political pressure from different constituencies. In an economic boom, regulators may be less likely to

[17]For a recent overview of the literature see Drazen (2000); Dubois (2016) and Müller (2019).

start regulatory actions due to two reasons. First, they may face less pressure from the public. Second, they may wish to avoid disturbing financial institutions. However, in an economic downturn, they may face substantial pressure to investigate substandard practices that occurred either in the previous boom or the subsequent bust, and increase regulatory action. This perspective suggests that state regulators may be more sensitive to state-specific considerations compared to federal regulators. Dagher (2018) refers to a variation of this mechanism as the sentiment hypothesis.

An alternative explanation relates to the resources available to regulatory agencies relative to private financial firms. In an economic boom, the labor market for regulatory agents may be relatively tight, and public institutions may have a hard time competing with private financial firms in the labor market. This relative lack of resources in regulators vs. private financial firms may translate in less regulatory actions when economic conditions are good. However, in a downturn, downsizing of private financial firms may change this relationship, providing the necessary resources for regulators to develop regulatory actions. Both perspectives deliver relatively less regulatory actions when the economic is doing well, and more regulatory actions following a downturn. According to this perspective, we should see similar behavior of regulatory actions over the cycle for both state and federal regulators. Ultimately, the incentives of financial firms also depend on the likelihood that they face regulatory actions, and this may amplify the mechanisms described above.

A third explanation relates to the incentives of market participants. Assuming constant motives and resources of regulators, we can explain pro-cyclical regulatory intensity with slowly adapting beliefs of market participants about the current state of the economy. Povel, Singh and Winton (2007) and Wang, Winton and Yu (2010) explain that an individual firm's propensity to commit fraud varies over the business cycle. This is because firm and investor beliefs about the current state of economy only adjust with a time lag. In boom periods, investors are less likely to monitor firms receiving favorable public coverage, but investors are sensitive to negative coverage. Thus, incentives for firm-level fraud to cook the books are high. At the end of a prolonged boom, firms and investors react with a lag to the changing state of the economy, which explains why fraud peaks at these moments.

Our findings suggest that state regulators are more affected by the economic cycle compared to federal regulators, which appears to be consistent with a mechanism related to the capture of state regulators, but not of federal regulators. Although our empirical approach does not allow us to investigate the mechanism through which business cycles affect regulatory intensity, our data and results provide a set of stylized facts that papers on the dynamics of regulation should match.

IV. Conclusion

Descriptive evidence on the procyclicality of the regulatory cycle points towards a potential linkage between output and regulatory stance. This paper investigates how *de facto* regulation, measured by regulatory actions and fines, responds to the business cycle. We apply a text-mining algorithm to build a new detailed dataset on regulatory actions levied against asset managers in the US and the regulatory stance of three layers of the US financial regulatory system: state, federal, and private regulatory institutions. Our data cover 49 states over the 1990-2019 period and provide information on the intensive margin of financial regulation. We define two monthly measures of financial regulation: Conviction rate and fine rate. The first corresponds to the sum of regulatory actions per state normalized by the number of employees in the state's financial sector. The second is the sum of the attached monetary fines per state normalized by the number of employees in a respective state's financial sector.

We combine our new dataset with extended data on military contracts over the 1999-2019 period and build an instrumental variable approach to test for causal effects of the cycle on regulatory intensity. Using exogenous variation in defense spending, we establish causal evidence on the procyclicality of the intensive margin of state-level regulation. A positive state-level output shock reduces conviction rates for a period of up to 50 months after the shock, but only at the state regulatory level. At the federal level, the intensive margin of financial regulation is counter-cyclical. A positive state-level output shock increases conviction rates for period of up to 60 months after the shock. We conclude that output cycles affect regulatory intensity over time. However, different levels of regulatory agencies respond differently to business cycles, consistent with a mechanism related to the capture of state regulators, but not of federal regulators.

Our data provide a promising avenue for further research on the *de facto* intensive margin of financial regulation exploiting within US variation. Our findings for the cyclical behavior of financial regulatory intensity are important for both sides of the market in asset management, managers and investors. On the policy side, our results show that state-level financial regulation may intensify boom and bust cycles, suggesting a role for federal regulators that may be dependent on the economic cycle.

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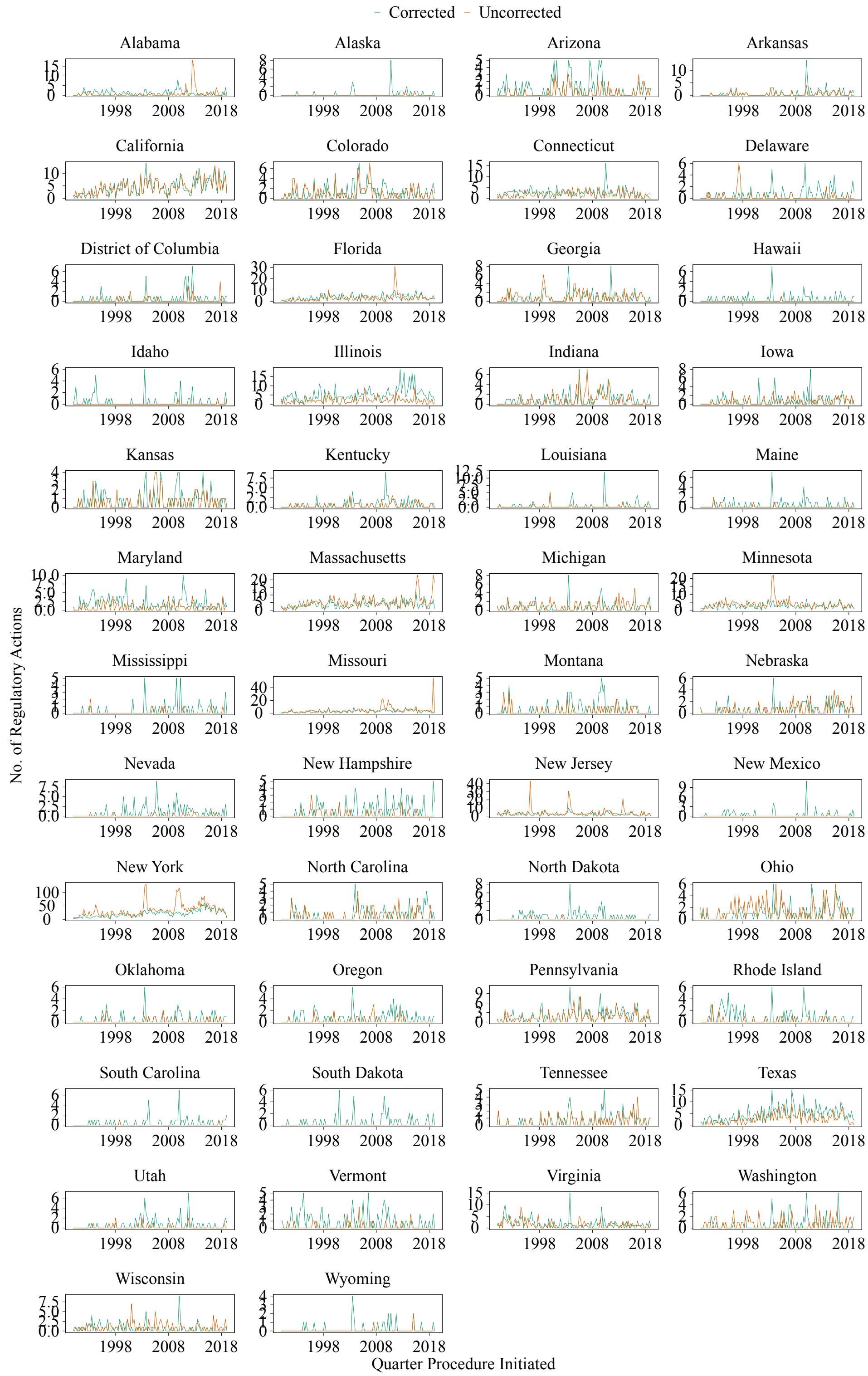
Appendix

Table A1—: FINRA Districts

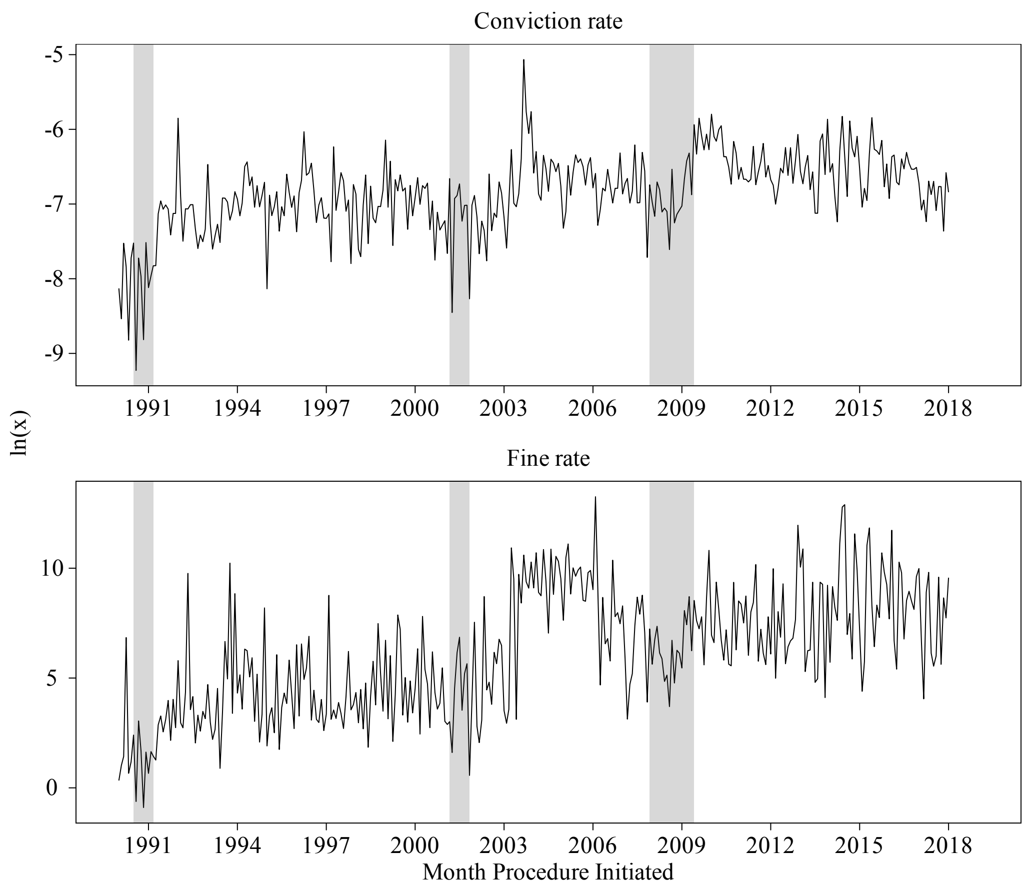
	District	State
1	San Francisco	California, Nevada, Hawaii
2	Los Angeles	California, Nevada
3	Denver	Alaska, Arizona, Colorado, Idaho, Montana, New Mexico, Oregon, Utah, Washington, Wyoming
4	Kansas City	Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota
5	New Orleans	Alabama, Arkansas, 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

Figure 2. : Regulatory Actions in the United States per US State



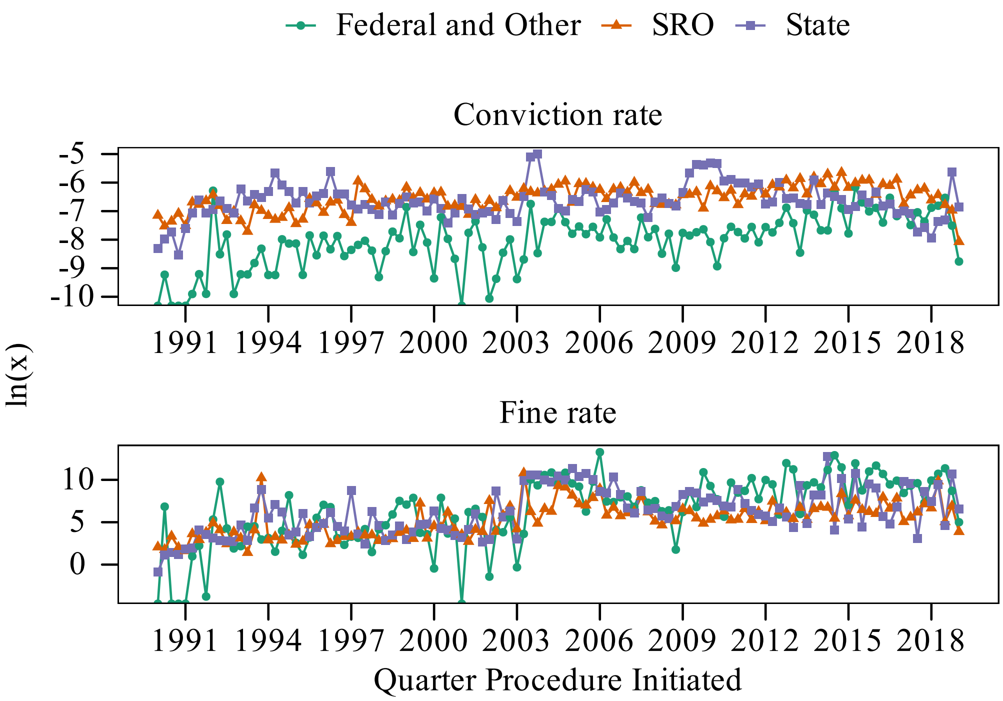
Note: This figure plots the quarterly sum of the absolute number of regulatory actions per US state with monetary fines larger than 0. The shaded areas show NBER recession dates. 'Corrected' figures are obtained using our algorithm. 'Uncorrected' figures are obtained using the original state location information associated with a regulatory action.

Figure 3. : Regulatory actions in the United States



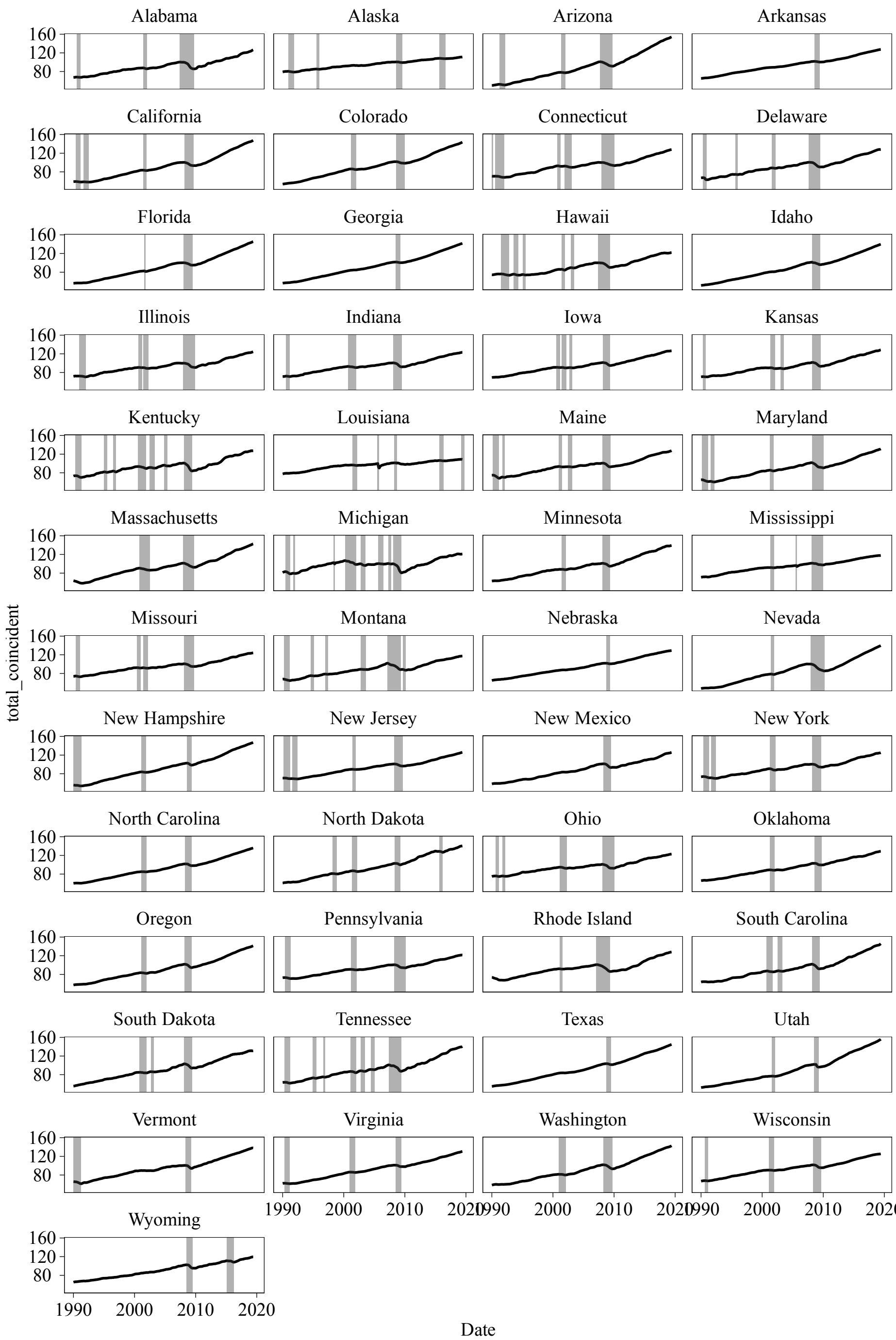
Note: This figure plots the natural logarithm of conviction and fine rates. Conviction rate is defined as the number of regulatory actions per state normalized by the number of employees in the finance sector per state. Fine rate is defined as the deflated monetary amount of regulatory fines per state normalized by the number of employees in the finance sector per state. We deflate with regional CPI, where Jan 2019 =100. The shaded areas show NBER recession dates.

Figure 4. : Regulatory actions in the United States by regulatory level



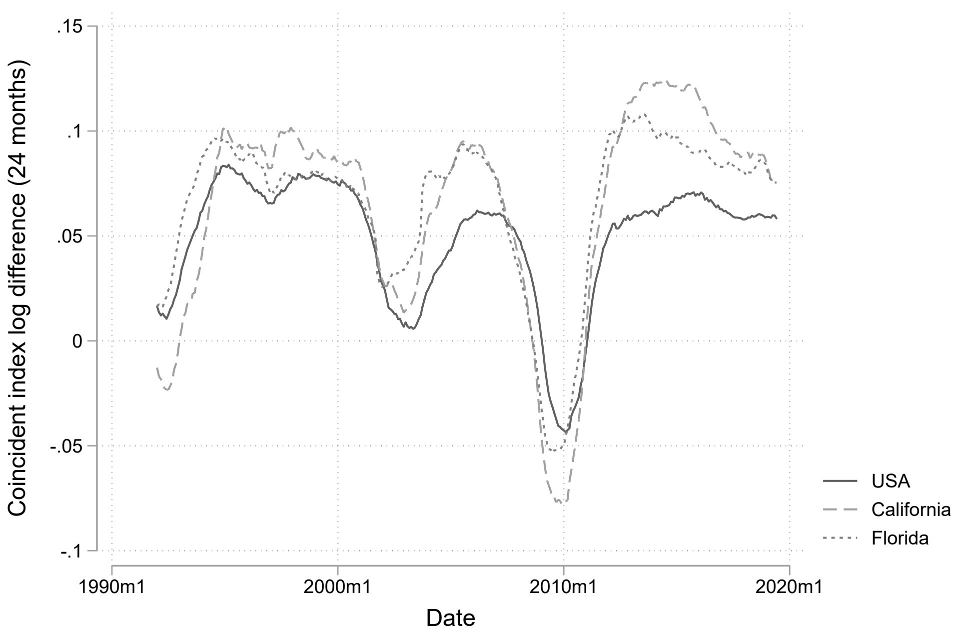
Note: This figure plots the natural logarithm of conviction and fine rates. Conviction rate is defined as the number of regulatory actions per state normalized by the number of employees in the finance sector per state. Fine rate is defined as the deflated monetary amount of regulatory fines per state normalized by the number of employees in the finance sector per state. We deflate with Regional CPI, where Jan 2019 = 100.

Figure 5. : State-level economic growth and recessions



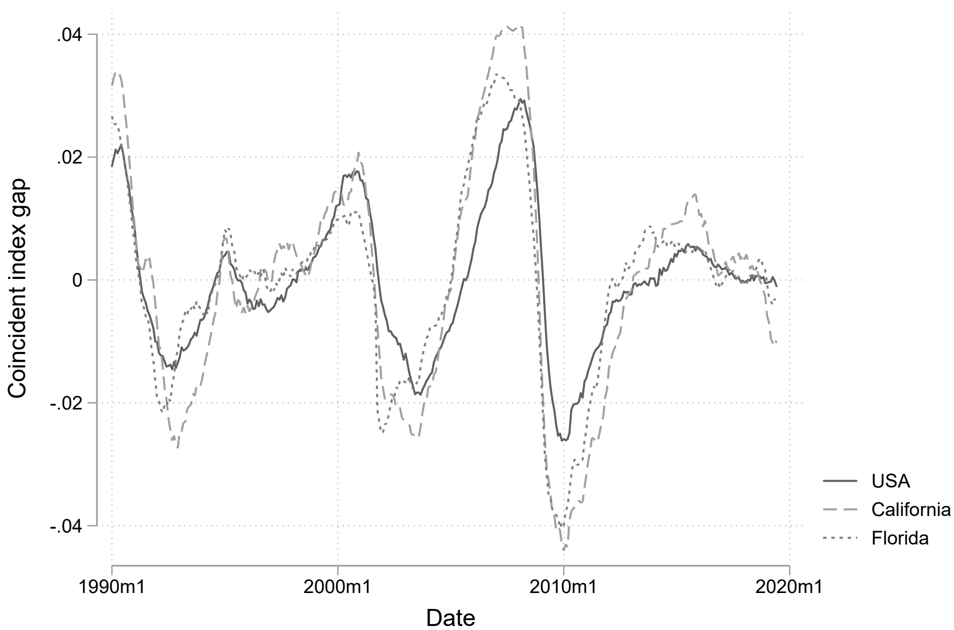
Note: This figure plots the monthly mean time series of state-level economic growth based on the FED's coincident index (Index 2007=100) and state-level recession dates. The Coincident Economic Activity Index includes four indicators: non-farm payroll employment, the unemployment rate, average hours worked in manufacturing and wages and salaries. The trend for each state's index is set to match the trend for gross state product. The recession shading is based on the following criteria. (i) The cumulative decline in the states 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. (ii) The period from the state indexs peak to its trough must be at least three months. We are missing data on the District of Columbia and West Virginia.

Figure 6. : State-level coincident economic activity index



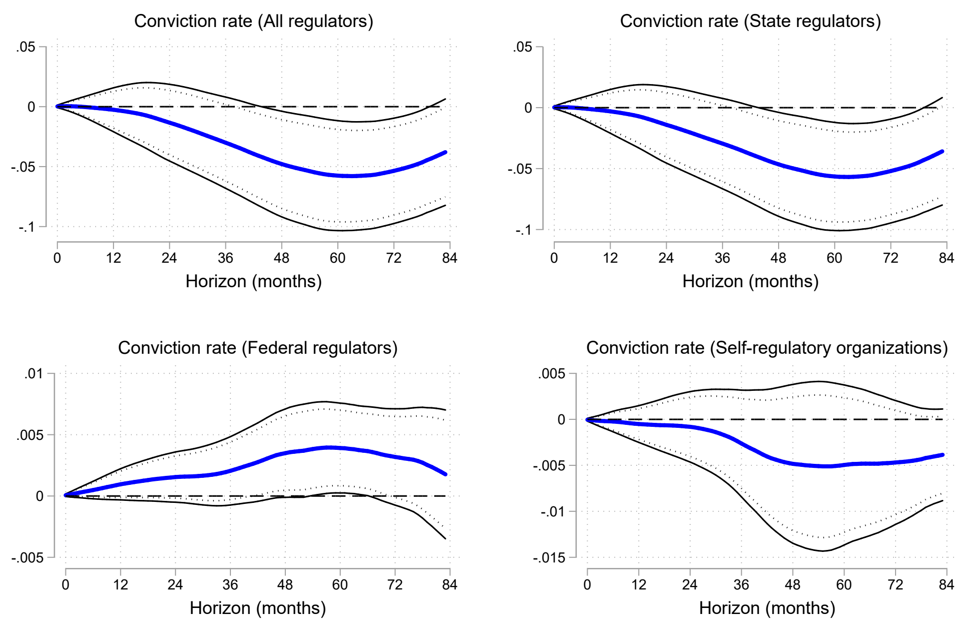
Note: This figure plots the monthly time series for state-level, two year log-differences, of the coincident economic activity index for selected states and the US economy.

Figure 7. : State-level coincident economic activity index gap



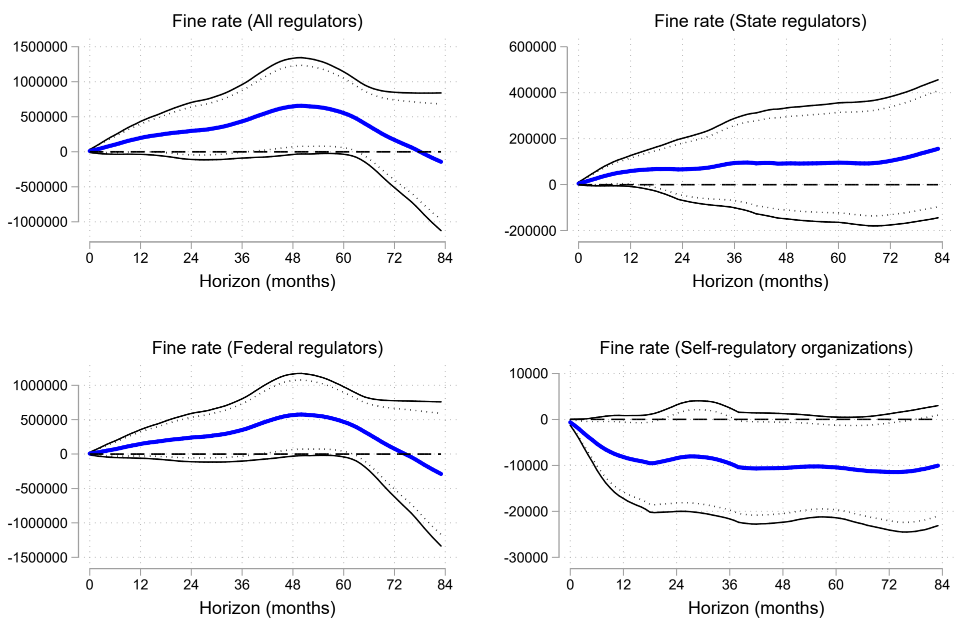
Note: This figure plots the monthly time series for state-level, coincident economic activity index, gaps for selected states and the US economy.

Figure 8. : Baseline results for the conviction rate (LP-OLS)



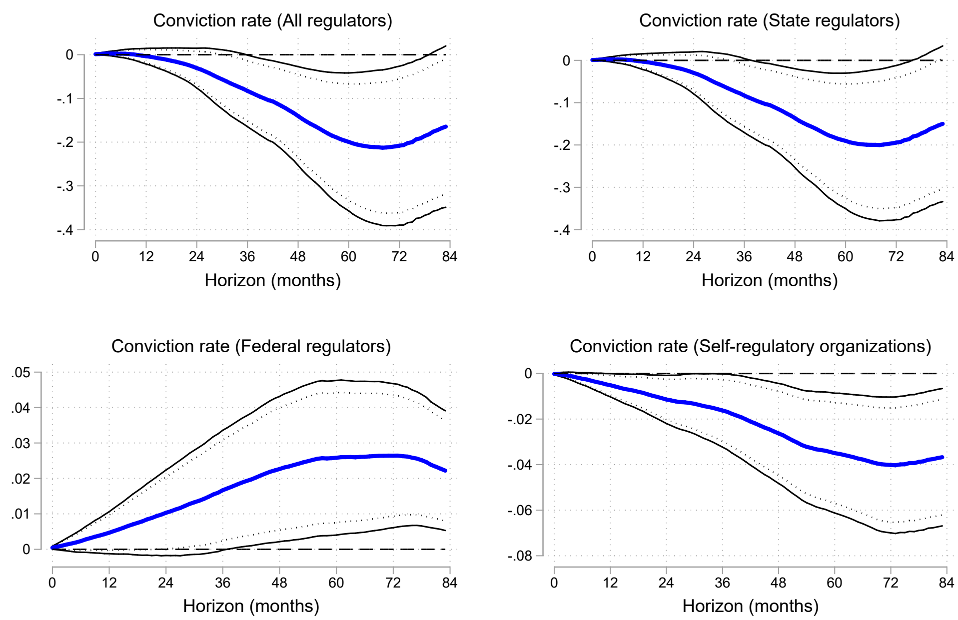
Note: This figure plots the conviction rate responses to a one-standard-deviation increase in the log difference over 24 months of the coincident economic index. LP-OLS estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 9. : Baseline results for the fine rate (LP-OLS)



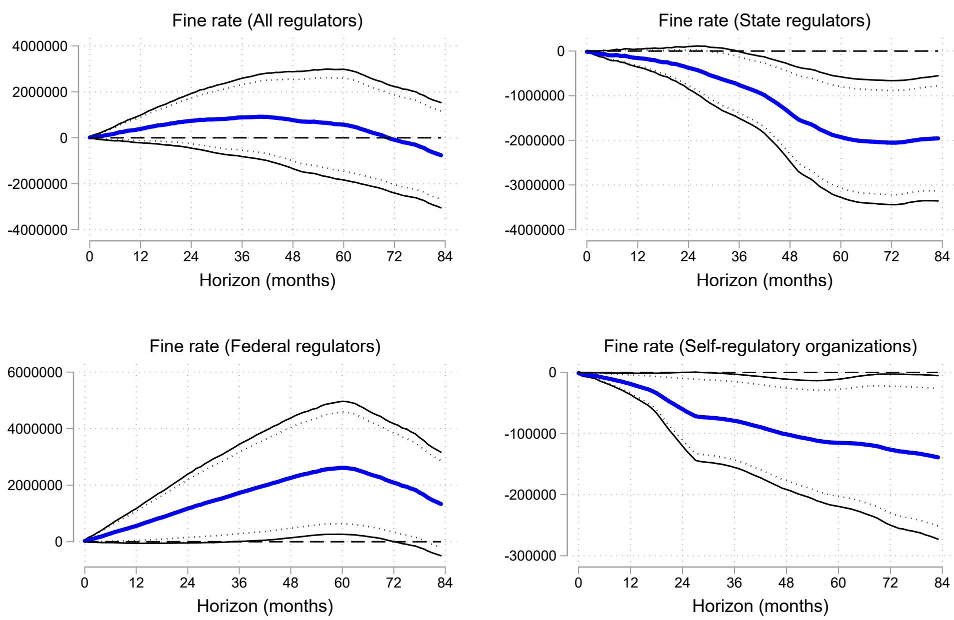
Note: This figure plots the conviction rate responses to a one-standard-deviation increase in the log difference over 24 months of the coincident economic index. LP-OLS estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 10. : Baseline results for the conviction rate (LP-IV estimates)



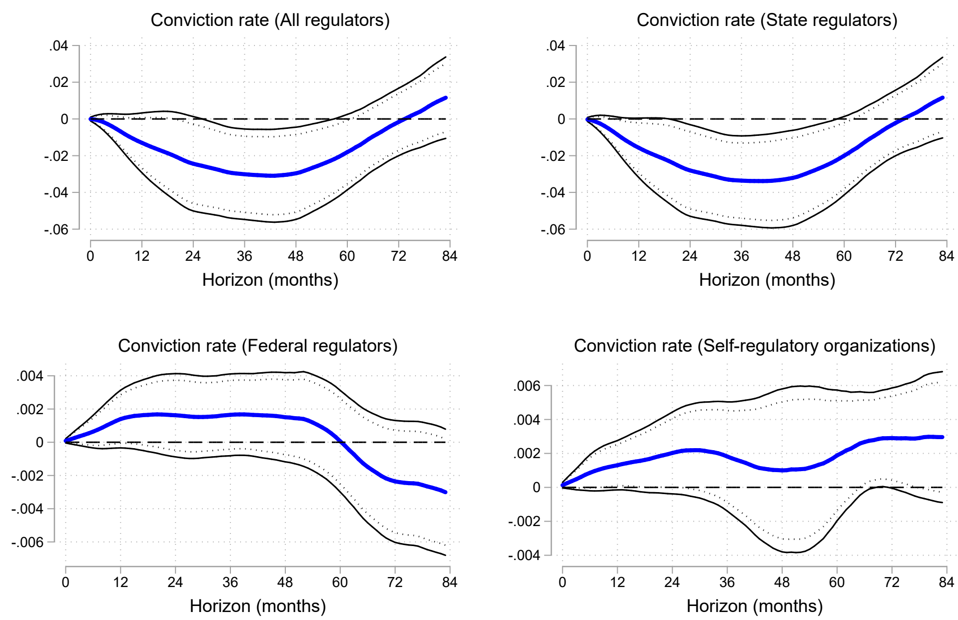
Note: This figure plots the conviction rate responses to a one-standard-deviation increase in the log difference over 24 months of the coincident economic index. LP-OLS estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 11. : Baseline results for the fine rate (LP-IV estimates)



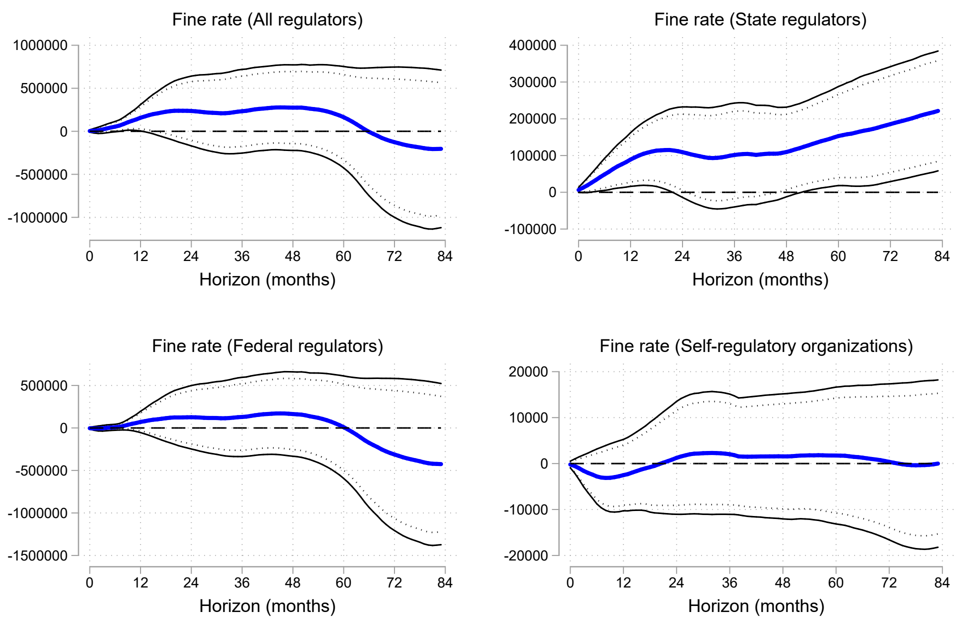
Note: This figure plots the fine rate responses to a one-standard-deviation increase in the log difference over 24 months of the coincident economic index. LP-OLS estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 12. : Alternative measure of the cycle results for the conviction rate (LP-OLS)



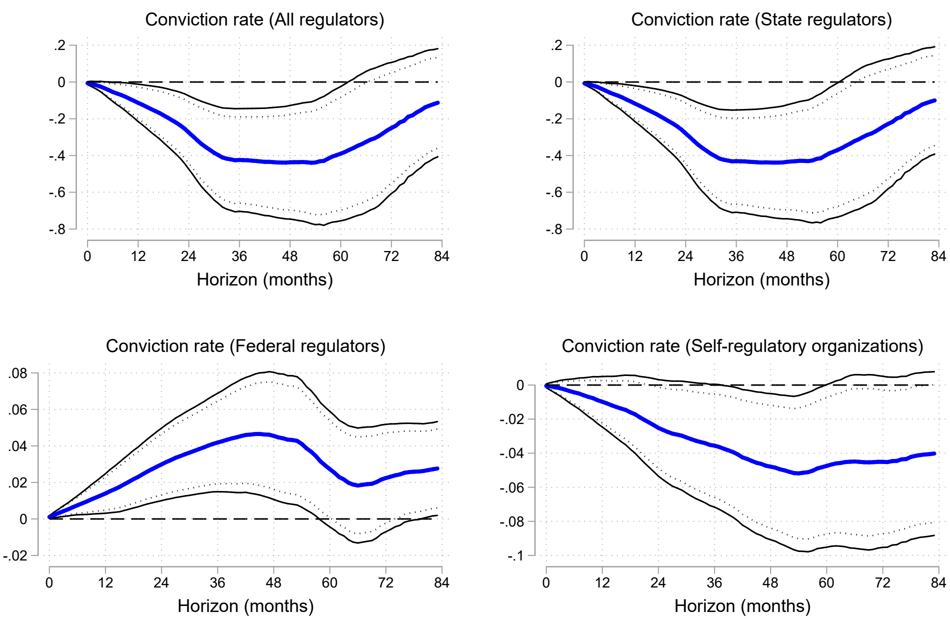
Note: This figure plots the conviction rate responses to a one-standard-deviation increase in the coincident economic index gap. LP-OLS estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 13. : Alternative measure of the cycle results for the fine rate (LP-OLS)



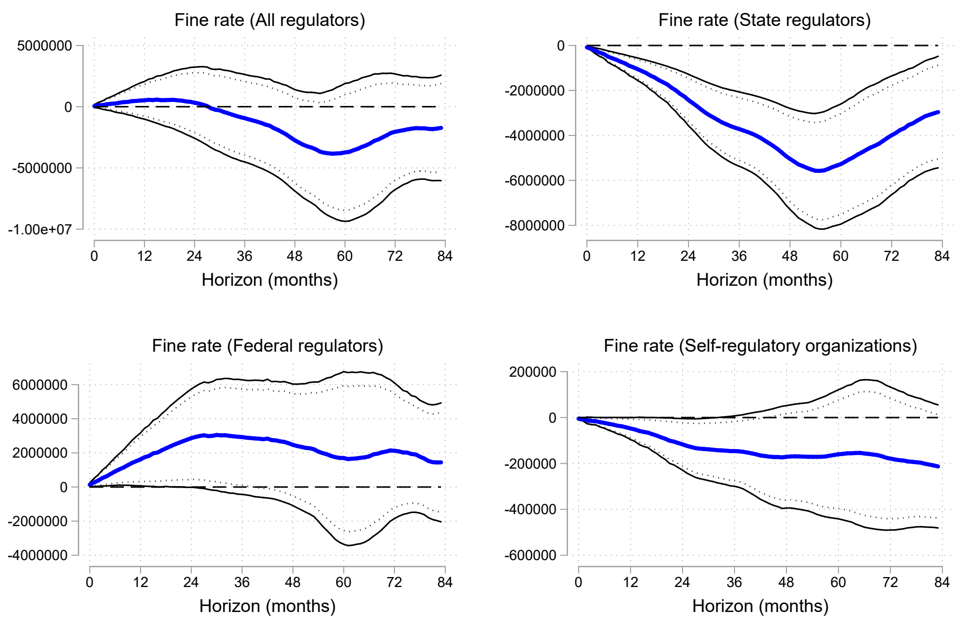
Note: This figure plots the fine rate responses to a one-standard-deviation increase in the coincident economic index gap. LP-OLS estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 14. : Alternative measure of the cycle results for the conviction rate (LP-IV estimates)



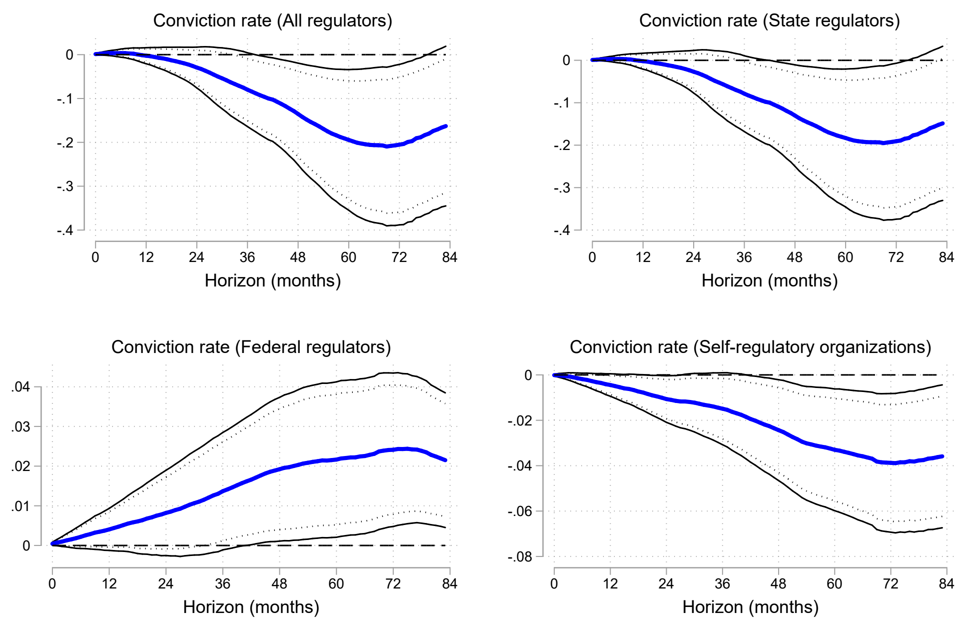
Note: This figure plots the conviction rate response to a one-standard-deviation increase in the coincident economic index gap. LP-IV estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 15. : Alternative measure of the cycle results for the fine rate (LP-IV estimates)



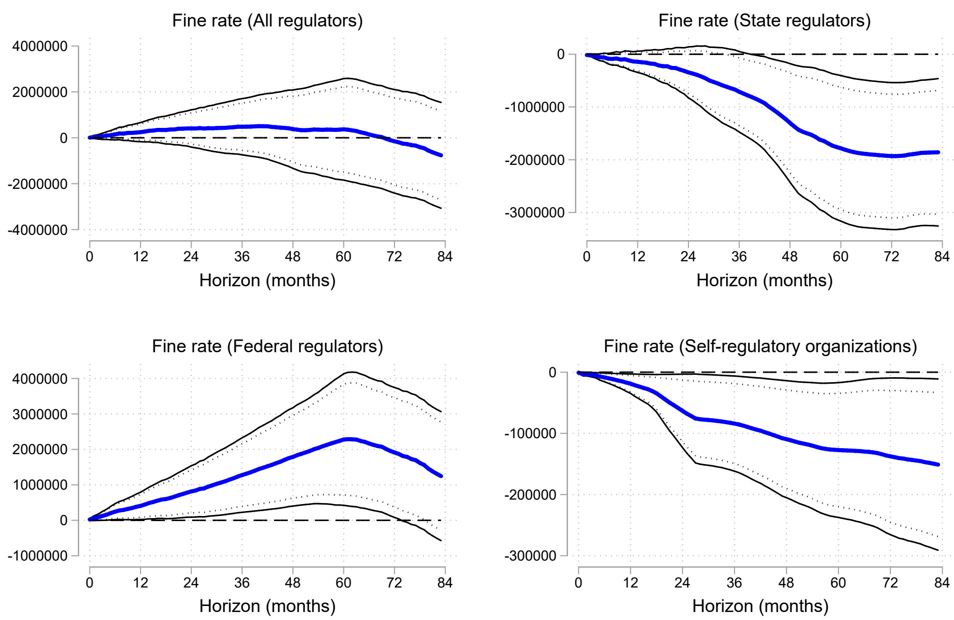
Note: This figure plots the fine rate response to a one-standard-deviation increase in the coincident economic index gap. Notes: LP-IV estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 16. : Results with additional controls for the conviction rate (LP-IV estimates)



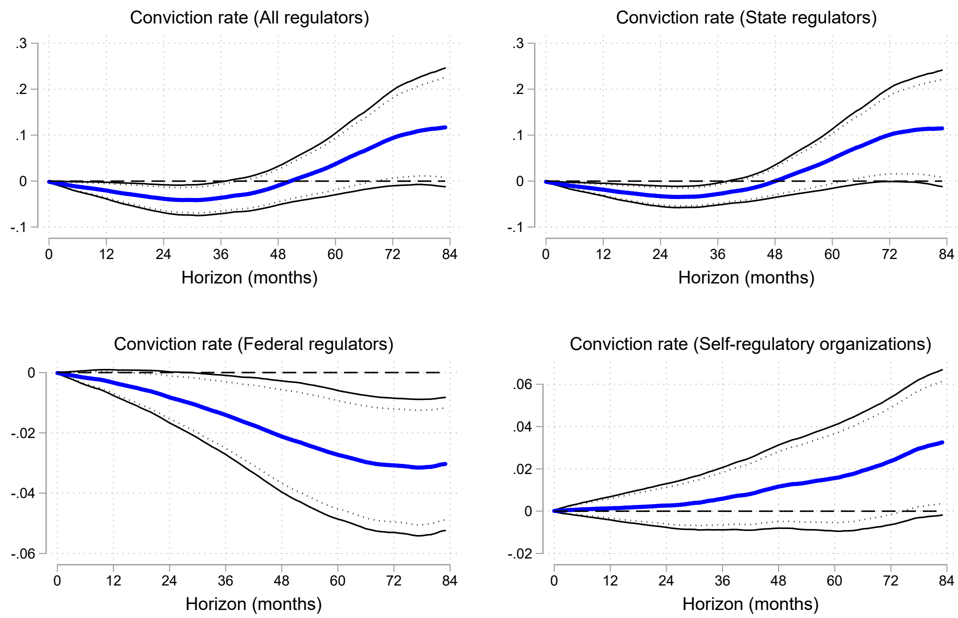
Note: This figure plots the conviction rate response to a one-standard-deviation increase in the log difference over 24 months of the coincident economic index. LP-IV estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 17. : Results with additional controls for the fine rate (LP-IV estimates)



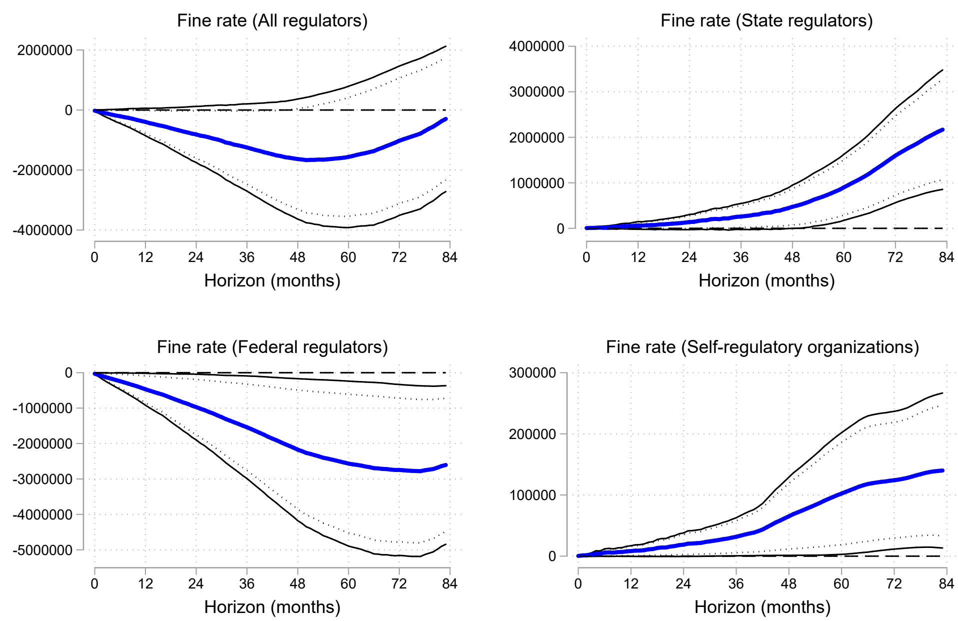
Note: This figure plots the fine rate response to a one-standard-deviation increase in the log difference over 24 months of the coincident economic index. Notes: LP-IV estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 18. : Placebo results using forward differences for the conviction rate (LP-IV estimates)



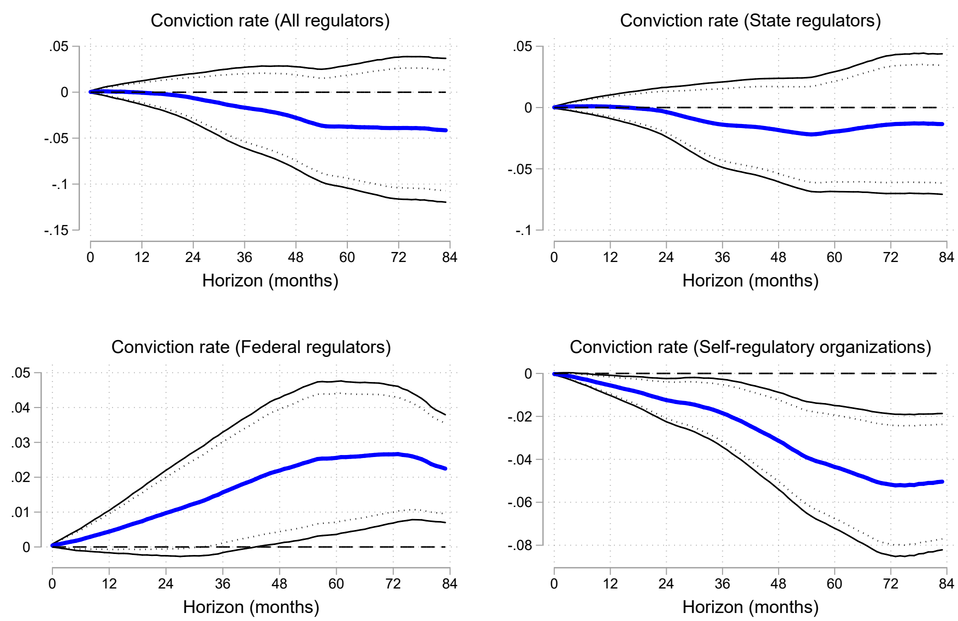
Note: This figure plots the conviction rate response to a one-standard-deviation increase in the log forward difference over 24 months of the coincident economic index. LP-IV estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 19. : Placebo results for the fine rate using forward differences (LP-IV estimates)



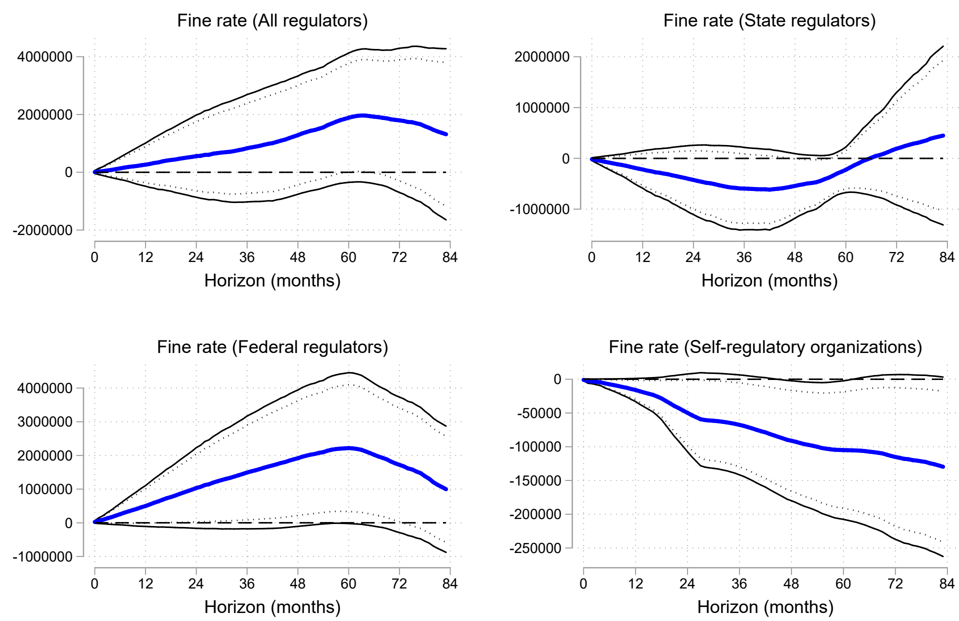
Note: This figure plots the fine rate response to a one-standard-deviation increase in the log forward difference over 24 months of the coincident economic index. Notes: LP-IV estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 20. : Placebo results using untreated data for the conviction rate (LP-IV estimates)



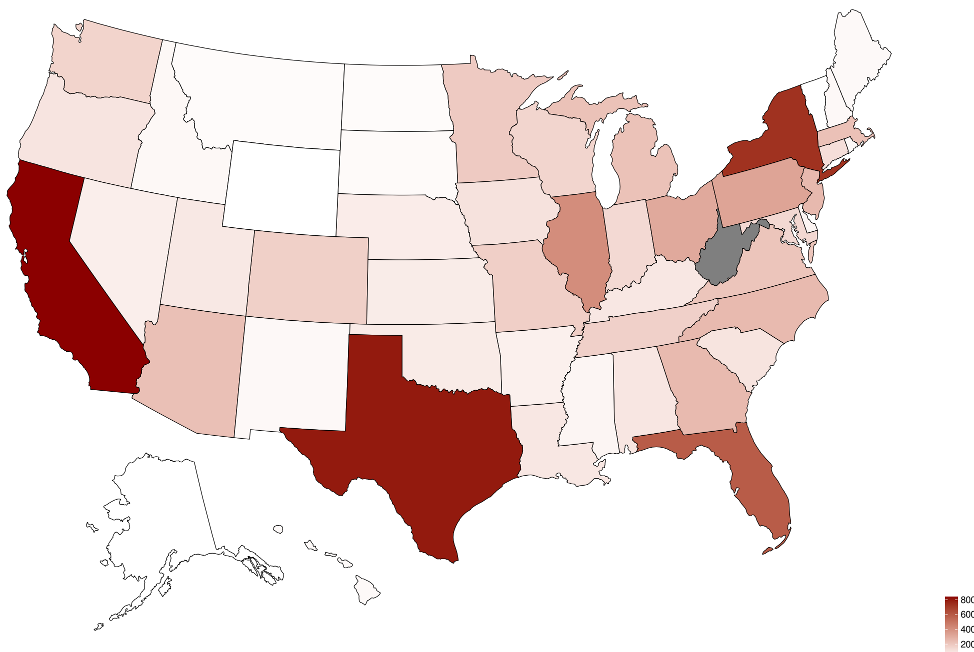
Note: This figure plots the conviction rate response to a one-standard-deviation increase in the log forward difference over 24 months of the coincident economic index. LP-IV estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure 21. : Placebo results using untreated data for the fine rate (LP-IV estimates)



Note: This figure plots the fine rate response to a one-standard-deviation increase in the log forward difference over 24 months of the coincident economic index. Notes: LP-IV estimates displayed with a solid blue line and 95% and 90% confidence bands.

Figure A1. : Geographical Distribution of Financial Employess (April 2019)



Note: Grey states have zero observations.