

# **Do Information Processing Costs Matter to Regulators? Evidence from the U.S. Shadow Bank Supervision**

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## **Abstract**

This study examines the effect of reducing information processing costs on U.S. state regulators who supervise mortgage companies (i.e., “shadow banks”). State regulators traditionally disclosed enforcement actions against mortgage companies only on their individual websites. A centralized repository introduced in 2012 assembled enforcement records across states in one place, substantially reducing a regulator’s cost to learn about enforcements in other states. To isolate the incremental effect of the centralized repository, we manually collect enforcement records from all state regulators’ websites both before and after the introduction of the centralized repository. We find that enforcement records posted on the centralized repository significantly increase the probability of subsequent enforcement actions against the same firm in other states, suggesting that reducing information processing costs helps state regulators identify companies that engage in misconduct. Additional analyses show that the effect is stronger for records from state websites where information is harder to process and for state regulators with more limited resources. Last, we find that lenders approve significantly fewer loan applications (particularly from risky borrowers) in the other states after their enforcement records in one state are posted on the centralized repository, indicating that these lenders reduce credit supply in response to heightened regulatory scrutiny.

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## 1. Introduction

Regulators play a central role in protecting consumer welfare and enhancing market stability. However, anecdotal evidence suggests that many of them struggle to accomplish their regulatory missions due to limited resources (e.g., Coursen 2021; Yoder 2018). In particular, limited resources may constrain regulators from collecting the necessary information for decision making. For example, Luis A. Aguilar, the former SEC commissioner, contended that the 2008 financial crisis revealed that the SEC either lacked information about the markets or did not process the information effectively (Aguilar 2015). One way to improve regulatory effectiveness is to reduce information processing costs so that regulators can process more information, holding constant their staffing and budget constraints. Whereas prior studies have examined the impact of information processing costs on investors and firms (e.g., Blankespoor 2019; Blankespoor et al. 2019; Christensen et al. 2017; Cuny et al. 2021), there is little, if any, evidence of how such costs affect regulators.

Our paper examines the effect of reducing information processing costs on regulatory outcomes in the context of mortgage companies (also known as “shadow banks” or “nonbanks”).<sup>1</sup> In the past decade, mortgage companies such as Quicken Loans have experienced tremendous market growth. As of 2020, they originate over 68% of all U.S. mortgages (McCaffrey 2021). Because these companies serve less creditworthy borrowers than banks and rely heavily on short-term credit facilities for funding, academics and media outlets have expressed concern about their risks and called for more regulatory oversight of these companies (e.g., Kim et al. 2018; Light 2020). However, unlike traditional banks, which are heavily regulated by federal agencies (e.g., the Federal Reserve, the Office of the Comptroller of the Currency), mortgage companies are

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<sup>1</sup> Demyanyk and Loutskina (2016) and Buchak et al. (2018) use the term “shadow banks” to refer to mortgage companies because they are non-depository institutions that fall outside the scope of traditional banking regulation.

primarily regulated by state regulators, which have fewer resources and have been criticized for lax supervision (e.g., Agarwal et al. 2014). One critic is Michelle Bowman, the Federal Reserve governor, who in 2020 concluded that “the oversight and regulatory infrastructure for mortgage companies is much less well developed than for banks” (Bowman 2020).

Our setting focuses on the introduction of a centralized enforcement action repository in 2012 that, for the first time, gave state regulators a central location to share enforcement action records about mortgage companies. Prior to the repository’s inception, state regulators only disclosed these records on their own individual websites. The centralized repository substantially reduces regulators’ information processing costs in two ways. First, the large number of U.S. states makes it costly for an individual regulator to monitor enforcement records across states. By gathering and centralizing enforcement records in one place, the repository enhances the regulator’s awareness of regulatory activity in other states. Second, state regulators disclose enforcement records in vastly different formats. For example, some state websites pool enforcement actions against mortgage companies with actions against other entities (e.g., payday lenders), while others present them separately. By standardizing the disclosure of enforcement actions, the centralized repository makes it easy for state regulators to acquire these records and integrate them into their decision making.

Although a state regulator’s enforcement action only pertains to a company’s misconduct *in that state*, it could inform regulators elsewhere by revealing the company’s systematic aggressiveness or problems that may emerge in other states where the company conducts business. Because state regulators often lack adequate resources for mortgage supervision, the centralized repository may help them identify mortgage companies that engage in misconduct. For example, in Oregon, only eight examiners oversaw nearly 1,500 mortgage companies in 2008 (Manning

2008). We hypothesize that if the centralized repository reduces regulators' cost of acquiring enforcement records in other states, the regulators will be more likely to incorporate those records into their supervisory purview, resulting in a higher probability of subsequent enforcement action against the same companies in the regulators' own states.

To examine the effect of information processing costs on regulators' supervision, we employ a difference-in-differences design centered on the introduction of the centralized repository. Our empirical strategy compares enforcement records posted on the centralized repository (i.e., the treatment records) with those solely disclosed on state websites (i.e., the control records) and examines whether they lead to a differential probability of subsequent enforcement actions in other states. To implement this empirical strategy, we hand-collect all enforcement actions from each state regulator's website from 2007 to 2014.

Our main finding is that when an enforcement record about a mortgage company in one state is posted to the centralized repository, the probability of enforcement against the same company in other states during the subsequent two years increases, on average, by a statistically significant 2.3%, corresponding to a 46% increase relative to the unconditional mean of enforcement actions. This suggests that the repository facilitates regulators' access to other states' enforcement records, which influences their subsequent supervisory decisions.

An alternative explanation of our finding is that severe enforcement records may be more likely to be posted in the centralized repository, and these records would draw other state regulators' attention even if they are not posted there. To alleviate this concern, we directly examine whether the enforcement actions posted in the repository are more severe than those not posted. We measure enforcement severity using four proxies—the number of reasons for the enforcement, the enforcement document length, the monetary penalty, and the non-monetary penalty—and find no

evidence suggesting that the enforcement records in the repository are more severe than those not posted. These findings are consistent with multiple regulators' responses to our direct inquiry about why they posted some, but not all, enforcement actions in the repository at its launch: They mainly attributed this decision to lack of staff and said the choice of which actions to post was not strategically planned.

Next, we provide more evidence to corroborate that the mechanism driving our main findings is the reduction in information processing costs. To do so, we focus on two factors that determine the information processing costs: 1) *what* is processed, because some information is fundamentally more costly to process due to the way it is disclosed; and 2) *who* processes it, because regulators have different information processing capacities, making the same information more costly for some regulators to process than for others.

To examine the first factor, we exploit heterogeneity in enforcement disclosure across state websites. For example, some state regulators disclose a separate list of enforcement actions against mortgage companies on their websites, making these records relatively easy to collect. In contrast, other state regulators pool enforcement actions against all types of companies without specifying each company's industry (see Appendix B for examples). Because the centralized repository uses a standardized disclosure format for all states, we expect the reduction in information processing costs to be larger for enforcement records from states with less user-friendly websites.<sup>2</sup> To test this, we partition the repository's enforcement records based on the difficulty of acquiring them from state websites. Consistent with our expectation, we find that the effect of the centralized repository is stronger for the records from state websites that impose higher information processing costs.

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<sup>2</sup> We do not claim that the only way for state regulators to obtain enforcement action records in other states was through state websites. For example, they could have requested records directly from other state regulators. However, to stay up to date with enforcement records, a regulator would have had to constantly request records from all other states, which would likely have been even more costly than acquiring records from state websites.

To examine the second factor, we start with the premise that regulators’ ability to process information depends on their resources. Regulators with scarce resources should have less capacity to acquire data and thus be more likely to lack information about the companies they supervise, so we expect them to benefit more from the centralized repository. Because resource constraints typically manifest in understaffing, we use the ratio of a state’s mortgage examiners to its regulated mortgage companies as a proxy for resource constraints and partition our sample based on this ratio. We find that our results are significantly stronger for regulators with fewer examiners per company, suggesting that resource-constrained regulators benefit more from the centralized repository.

A possible implication of our results is that after regulators begin obtaining enforcement records more easily via the repository, they “free-ride” on other states’ findings instead of investigating companies on their own. If state regulators solely rely on records in the repository and not their own internal investigations, their subsequent enforcement actions are likely to share the *same* reasons (i.e., the same types of identified misconduct) as those posted in the repository. To evaluate this possibility, we identify all cases where a company has an enforcement record posted in the repository and subsequently receives another enforcement action in *another* state. We find that 87% of the subsequent enforcement actions reveal *new* misconduct—issues not covered by the repository’s previous enforcement records—suggesting that, in most cases, the regulators conduct their own comprehensive investigations after observing the records in the repository.<sup>3</sup>

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<sup>3</sup> In our setting it is nearly impossible for regulators to completely free-ride on other states’ records because a state enforcement action requires hard evidence of the company’s misconduct *in that state*. This means that, even for an enforcement action that provides the same reason as an enforcement action in the repository, the state regulator still has to exert effort to prove that the misconduct exists in its own state.

Next, we examine the centralized repository's effects on mortgage lenders.<sup>4</sup> The rapid market expansion of mortgage companies in the post-2008 crisis period is primarily attributed to a lax regulatory environment (Ackerman 2019; Buchak et al. 2018). This implies that these companies may reduce their credit supply as regulatory scrutiny heightens. Because our main findings suggest that a state's record sharing in the centralized repository increases regulatory scrutiny in other states, our setting allows us to examine the effect of heightened scrutiny on mortgage companies' credit supply.

Following prior literature (e.g., Xie 2016; Dou et al. 2018), we isolate a mortgage company's credit supply from borrower demand using its approval/denial decisions on mortgage applications. We find that, after mortgage lenders' enforcement records in one state are posted in the centralized repository, those lenders approve significantly fewer loan applications in *other* states, relative to sanctioned lenders whose enforcement records are not posted in the repository in these states. This result suggests that mortgage lenders reduce the credit supply when they face potentially higher scrutiny. Additional cross-sectional tests show that the reduction in credit supply is larger for risky borrowers (i.e., borrowers with higher loan-to-income ratios), suggesting that it is partly attributable to lenders' reducing their risk-taking.

Last, we conduct a falsification test in the pre-period to test the parallel trends assumption. Specifically, we pretend that the centralized repository was launched in 2010 and focus on the two-year window around this pseudo-event. Consider an enforcement record that occurred in 2010 and was posted in the centralized repository in 2012. Our main findings above suggest that this record should lead to a higher enforcement probability in 2013–2014. If the findings are truly driven by the repository instead of the record itself, the record should *not* lead to a higher enforcement

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<sup>4</sup> Mortgage companies can be lenders, brokers, and servicers. For this test, we focus on lenders because we do not have data on brokers' and servicers' activities.

probability in 2011, the year before the repository was launched. Consistent with this expectation, we find that the records that are *eventually* posted in the centralized repository neither increase the enforcement probability nor reduce the credit supply in the period before the repository was available. This test corroborates the inference that our main findings are driven by the repository and alleviates the concern that the records posted in the repository would have drawn state regulators' attention even if they had not been posted there.

Our paper offers important policy implications. Many regulators lack the resources they need to pursue their regulatory missions. For example, Coursen (2021) reports that “inadequate resources have forced EPA [U.S. Environmental Protection Agency] to cut back on enforcement activities, despite evidence of widespread non-compliance with environmental requirements.” To improve their supervisory efficiency, some regulators are taking steps that are similar to what we witness in our setting. For example, the SEC is currently developing the Consolidated Audit Trail, a data repository that is designed to centralize securities trades and orders across exchanges and markets (Clayton 2020; Peirce 2022). Our inference that a centralized repository affects regulatory outcomes in the setting of mortgage companies may be of interest to resource-constrained regulators in other fields who might benefit by adopting data repositories.

Our paper contributes to three strands of literature. First, we add to the growing research on information processing costs. A recent review paper by Blankespoor et al. (2020) states that “we are only just beginning to understand [information processing costs'] effects” (p. 1). To our knowledge, our paper is the first to show the real effects of information processing costs on regulators. As such, it answers the Blankespoor et al. (2020) call for research on information processing costs in broader contexts.



Second, our study contributes to the literature on shadow banks. Although banks' off-balance-sheet subsidiaries and mortgage companies are both part of the shadow banking system (Demyanyk and Loutskina 2016), the latter have "received far less scholarly attention" despite their proliferation in the post-crisis period (Metrick and Tarullo 2021, p. 151). To our knowledge, this study is the first to examine the regulatory oversight of mortgage companies. By highlighting the importance of centralizing information from the fragmented supervisory system, we answer the call by Kim et al. (2022a), who states that "[h]ow to carry out more effective regulatory oversight of mortgage nonbanks remains an open question" (p. 163).

Third, our paper contributes to the literature on enforcement actions. While most prior studies focus on enforcement actions by federal regulators such as the SEC, OCC, and FDIC (e.g., Dechow et al. 1996; Delis et al. 2017; An et al. 2021; Kleymenova and Tomy 2022; Davidson and Pirinsky 2022; Silvers 2016), our study is among the first to examine enforcement actions by state regulators.

## **2. Institutional Background**

### *2.1. Regulatory Environment of Mortgage Companies*

Although there has been substantially more regulation on mortgage lending since the 2007–2008 financial crisis (e.g., Dodd-Frank Act), most of these regulatory changes (e.g., stress tests, higher capital requirements, new liquidity coverage ratio requirements) are imposed on banks but not mortgage companies (Gete and Reher 2021; Kim et al. 2022a). As a result, U.S. banks' share in the mortgage markets has contracted significantly during the past decade, while mortgage companies have experienced enormous growth, raising their market share from less than 30% in 2009 to over 68% in 2020 (Seru 2019; McCaffrey 2021).

Unlike for banks, the primary regulators of mortgage companies are state financial regulators (CSBS 2019b).<sup>5</sup> The state regulators' regulatory missions are the same as those of federal bank regulators, encompassing 1) safety and soundness and 2) consumer protection compliance, but carrying out these missions in the case of mortgage companies is more challenging. Regarding the safety and soundness mission, mortgage companies are riskier than banks because they rely heavily on short-term credit lines instead of deposits for funding and are not eligible to borrow from the Federal Reserve System (Kim et al. 2022a). Banking regulators, academics, and the media have repeatedly raised concerns about the risks that mortgage companies pose to the U.S. financial system (Ackerman 2019; Bowman 2020; Kim et al. 2018; Light 2020). Regarding the consumer protection mission, mortgage companies on average serve less creditworthy, low-income borrowers, who are more susceptible to predatory lending practices. Therefore, these companies may have more opportunities than banks to exploit borrowers' interests.

Mortgage companies are overseen by the regulators from the states in which they conduct business. Most mortgages are originated by mortgage companies whose business crosses state borders.<sup>6</sup> State regulators chiefly rely on examinations to supervise mortgage companies. However, due to resource constraints, they may not routinely examine all regulated mortgage companies and may instead “rely on company report data, complaints, information collected from other regulators, and public records” to determine which firms to investigate (p. 31, CSBS 2019a). This approach allows regulators to “prioritize their time and resources on the companies believed to pose the highest risk” (p. 31, CSBS 2019a). In addition, state regulators may not fully review a company's every aspect (e.g., loan portfolio, individual originator licensing, and financial condition), so

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<sup>5</sup> CSBS (2019b, p1) states that “state financial regulators are the primary regulators of nonbanks operating within the United States.”

<sup>6</sup> Multistate-licensed mortgage companies accounted for over 80% of total originations by mortgage companies in 2019 (CSBS 2019b).

examinations can vary substantially in length and scope across companies.<sup>7</sup> If the state regulator identifies a potential violation during an examination, the regulator will initiate an investigation, which could result in an enforcement action.

We note that state regulators are not evaluated by any federal agency or by peers based on the number of enforcement actions. A state regulator we talked to specifically said that “it is both simplistic and dangerous to make any assumption that a regulatory body would ‘adjust’ its practices based on any numerical standard of “enforcement actions issued” instead of applying a case by case, fact by fact analysis of a company’s actions during an examination.”

Last, we note that state regulators are not the only regulator of mortgage companies: the Consumer Financial Protection Bureau (CFPB), a federal agency established by the Dodd-Frank Act, also oversees mortgage companies. However, the CFPB is not the mortgage companies’ primary regulator, because 1) it oversees mortgage companies only for consumer protection but not safety and soundness, whereas state regulators oversee them on both dimensions; and 2) it issues significantly fewer enforcement actions against mortgage companies than state regulators do.<sup>8</sup> The CFPB is not related to our setting, because the CFPB started issuing enforcement actions against mortgage companies *after* July 2013, while in our setting, the treatment and control records consist of enforcements (posted on state websites or in the centralized repository) from 2007 to 2012. Moreover, our identification strategy is to compare the state regulators’ enforcement records posted in the repository (i.e., the treatment group) with their enforcement records disclosed on

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<sup>7</sup> Due to confidentiality concern, state regulators cannot provide more information about how they select mortgage companies for examinations. Therefore, similar to other regulators such as the SEC or PCAOB, the state regulation is by nature a black box.

<sup>8</sup> From 2013 to 2014, for example, the CFPB issued eight enforcement actions against mortgage companies while state regulators issued 1,003.

state websites only (i.e., the control group), which should control for any potential confounding events that simultaneously affect both groups (see more details in Section 3).

## *2.2. The Introduction of the Enforcement Action Centralized Repository*

In 2011 the State Regulatory Registry, a subsidiary of the Conference of State Bank Supervisors (i.e., the national association of state regulators), developed a centralized repository that allows state regulators to post their enforcement actions against mortgage companies and affiliated individuals in one place. One of the stated goals of this repository is to “facilitate the sharing of regulatory enforcement information among state regulators” (NMLS 2011, p.1).<sup>9</sup> Each regulator is required to complete the same standardized information form when posting an enforcement record.

The centralized repository not only allows a state regulator to observe enforcement actions across states in a single view, but also automatically sends a notification to other regulators whenever a regulator uploads an enforcement action in the repository.<sup>10</sup> The repository was made available to state regulators in October 2011. By the end of 2012, 35 of the 51 state regulators had posted at least some of their enforcement records there (CSBS 2012).<sup>11</sup> The state regulators who did not immediately post records in the repository could still access the repository to observe enforcement records in other states.

A unique feature of our setting is that all the states’ enforcement records were available to state regulators before the repository was introduced. This is because the majority of state regulators made their records publicly available via their websites; the few states that did not do

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<sup>9</sup> According to several state regulators we talked to, mortgage companies are not required to report to a state regulator about the company’s enforcement actions in other states.

<sup>10</sup> Given that we are not allowed to access the regulator’s interface of the repository, our knowledge about its functionality comes from meetings with state regulators.

<sup>11</sup> State regulators were encouraged but not required to post their enforcement actions in the repository. However, over time almost all state regulators have posted their enforcement actions in the repository.

so still made their records available to other state regulators upon request (due to nationwide cooperative protocols). This feature differentiates our setting from those examined in prior literature. For example, Silvers (2020, 2021) examines regulatory cooperation in a global setting in which regulators gained new access to previously unavailable information through certain information-sharing protocols. Moreover, global regulatory cooperation often bundles reductions in regulators' information processing costs with enhancements to other regulatory tactics and the coordination of regulatory requirements. By contrast, our setting specifically isolates information processing costs.

Last, similar to the enforcement actions that were publicly disclosed on state websites prior to the centralized repository, the enforcement actions posted in the repository are also publicly disclosed—in this case via a regulatory website called “Consumer Access.” This website is the repository's public interface but *not* its regulator interface, the latter of which contains substantially more functions. Because the introduction of the centralized repository also reduces the public's information processing costs (via the Consumer Access website), a potential concern with our setting is that subsequent regulatory outcomes are driven by better public (i.e., financial consumers') discipline over state regulators.

We believe this concern is unlikely for two reasons. First, Flannery et al. (2022) find that consumers do not react to the posting of enforcement actions on the Consumer Access site. Their additional analyses suggest that the cause of the borrowers' inaction is that most borrowers are unaware of the website. Flannery et al. state that their findings are consistent with the regulators' observation that most traffic to the site is from mortgage companies rather than borrowers. Second, there is no formal mechanism through which borrowers can discipline state regulators. The only way a borrower can push a state regulator to scrutinize a mortgage company is to file a complaint

against that company, and the complaint must show evidence that the complaining borrower's interest has been harmed. Because an enforcement action in *another state* does not necessarily mean that the out-of-state consumer's interests have been harmed, it is not grounds for a valid complaint.

### *2.3. Is It Obvious that the Centralized Repository Affects Regulatory Outcomes?*

Although the centralized repository is intended to make it easier for state regulators to learn about enforcement records in other states, it is not obvious *ex ante* whether the repository actually affects regulators for several reasons. First, state regulators may already use other states' websites or have pre-existing professional relationships that extend to the broader regulatory community, making a formal repository irrelevant. Second, regulators may believe they already possess sufficient knowledge of regulated mortgage companies, so they might not utilize the centralized repository. Third, regulators may hesitate to increase their scrutiny of mortgage companies because they derive significant revenue from licensing them (Brooks and Calomiris 2020). This conflict of interest may prompt regulators to intentionally disregard the repository even if they are aware of its informational value—a form of regulatory capture. Fourth, regulators may believe that it is optimal to let sanctions in other states deter malfeasant company-wide behavior, thus obviating the need for additional sanctions.

Even if the centralized repository affects regulators, it is not obvious whether it would increase the probability of subsequent enforcement actions. This is because an enforcement action posted in the repository from one state may lead a mortgage company to check and correct its misbehavior in other states. In other words, if mortgage companies anticipate that the launch of the repository leads to heightened regulatory attention from other states, then they might be incentivized to fix their behavior in those states. Therefore, we would expect the enforcement

probability in other states to be lower instead of higher after the company's enforcement record is posted on the centralized repository. Ultimately, it is an open empirical question whether and how the centralized repository affects supervisory outcomes of mortgage companies.

### **3. Research Design**

The ideal setting in which to isolate the effect of reduced information processing costs on regulators is one where the processing cost is unexpectedly lowered for one information set but unchanged for another information set of the same type. The centralized repository offers such a scenario because some of the regulators not only posted their new enforcement records in the repository going forward but also posted their *pre-existing* records in the repository when it launched. As a result, a large set of enforcement records were posted in the repository in 2012, including ones that were new and ones from prior years. Because some states did not immediately contribute to the repository and some only posted a portion of their past records, many enforcement actions that occurred in the same periods as posted records were still disclosed only on state websites after 2012. As a result, regulators before 2012 incur high costs to acquire any enforcement records from other states; after 2012 they continue to incur high costs for records not posted in the repository but incur low costs for the records posted in the repository.

Our identification strategy can be illustrated with a simple example. In 2011 firms A and B each received an enforcement action, but firm A was sanctioned in Texas and firm B was sanctioned in Florida. Both state regulators disclosed the actions on their websites in 2011. Firms A and B also originate mortgages in California. At the centralized repository launch in 2012, the Texas regulator posted its enforcement against firm A in the repository, but the Florida regulator did not post its enforcement against firm B. Thus, both records, which are potentially informative to the California regulator, have been available on state websites, but the centralized repository

significantly reduces the information processing cost of firm A's record. If a reduced information processing cost increases the probability that a regulator will access another state's enforcement records, then the California regulator should be more likely to scrutinize firm A than firm B, which in turn should lead to a higher probability of a subsequent enforcement action against firm A in California.

While a greater scrutiny of firm A could be due to its record being posted on the centralized repository, there are two potential concerns with this interpretation. The first concern is that the records posted in the repository may not be random. For example, state regulators might post only records that they believe will be useful to other regulators. If this is the case, then even if we find that records posted in the centralized repository are more likely to lead to higher enforcement probability in other states, it may be because these records are fundamentally more likely to attract attention—and would attract attention even if they were only disclosed on the state websites. To alleviate this concern, we collect enforcement records that are disclosed on state websites in the pre-repository period. In the pre-period, state regulators have no option but to disclose *all* enforcement actions on their websites, regardless of which records are more likely to attract other regulators' attention. As a result, some of the records in this period are a good counterfactual for the records posted in the centralized repository; thus, they are included in our regression sample.

The second concern, which is related to the first, is that the California regulator's higher scrutiny of firm A may stem from fundamental differences between the two firms. In other words, it is possible that, because of differences between the firms, the unconditional probability of being sanctioned in California is inherently higher for firm A than firm B even in the absence of the centralized repository or prior enforcement records for either company. Therefore, directly comparing the enforcement probability of firms A and B in the post-period may not reflect the true



effect of the centralized repository. To address this concern, we include the pre-period and use firm  $\times$  state fixed effects to control for pre-existing differences in the California regulator's attention to firms A and B. The firm  $\times$  state fixed effects enable us to compare the *change* in the likelihood of California's enforcement against firm A from the pre-period to the post-period versus the *change* in the likelihood of California's enforcement against firm B from the pre-period to the post-period. This is essentially a difference-in-differences design.

Based on the argument above, we focus on the four-year window around the repository's introduction. Figure 1 illustrates our research design. As shown, we exclude 2012, the year that most state regulators started to post enforcement records to the repository. The vertical dotted line in 2012 represents the date of the treatment's administration. Thus, our pre-period spans 2010 to 2011 and our post-period spans 2013 to 2014. We start the sample period from 2010 to avoid the 2007–2009 financial crisis. For each period, we assume a three-year preceding window during which regulators may learn about enforcement records from other states.<sup>12</sup> Specifically, for the pre-period, we assume that a state regulator may acquire enforcement records that occurred in other states from 2007 to 2009 (referred to as the “observation window for the pre-period”) and integrate them into its supervisory decisions, possibly leading to enforcement actions being issued in 2010 and 2011. For the post-period, a state regulator may acquire enforcement records that occurred in other states from 2010 to 2012 (referred to as the “observation window for the post-period”) and incorporate them into its supervisory decisions, possibly leading to enforcement actions being issued in 2013 and 2014.

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<sup>12</sup> We use three years because regulators likely find the records in this timeframe most relevant when identifying mortgage companies involved in misconduct. Our results are robust to using two years or four years as alternative observation periods (see Table A2 of the online Appendix).

In the pre-period (2010 and 2011), the repository was not available, so state regulators incurred high information processing costs to learn about all recent enforcement records. Line A reflects the probability of an enforcement action during the pre-period. In the post-period (2013 and 2014), state regulators incur reduced information processing costs to acquire the enforcement records posted in the centralized repository (remember that some states posted their records for prior years at the launch of the repository) but continue to incur high information processing costs for the records not posted in the repository. Line B reflects the probability of an enforcement action in the post-period when firms' enforcement records are only disclosed on state websites, and line C reflects the probability of an enforcement action in the post-period when firms' enforcement records are also posted in the repository. Our interest is in the gap between lines B and C, which reflects the *incremental* enforcement probability in the post-period when firms' enforcement records are available in the centralized repository.

We estimate the following OLS regression using a company-state-period panel dataset:

$$\begin{aligned}
\text{Enforcement}_{i,s,t} = & \beta_1 \text{Repository}_{i,\neq s,(t-3,t-1)} + \beta_2 \text{Records}_{i,\neq s,(t-3,t-1)} + \beta_3 \text{Records}_{i,s,(t-3,t-1)} \\
& + \beta_4 \text{Log Population}_{s,t} + \beta_5 \text{Log Income}_{s,t} + \beta_6 \text{Education}_{s,t} + \beta_7 \text{Minority}_{s,t} \\
& + \beta_8 \text{Log Gov Expenditure}_{s,t} + \beta_9 \text{Log Gov Employees}_{s,t} \\
& + \text{Company} \times \text{State FEs} + \text{Period FEs} + \varepsilon_{i,s,t}
\end{aligned} \tag{1}$$

where  $i$  indexes the mortgage company,  $s$  indexes the state, and  $t$  indexes the period. The unit of observation is a firm-state-period.  $\text{Enforcement}_{i,s,t}$  is an indicator variable that equals one if firm  $i$  is sanctioned in state  $s$  in period  $t$ .  $\text{Repository}_{i,\neq s,(t-3,t-1)}$  is the number of firm  $i$ 's enforcement records in states other than state  $s$  from 2010 to 2012 (i.e., the observation window for the post-period) that are posted in the centralized repository at its launch in 2012. By construction, this variable is zero for observations in the pre-period and can be positive or zero for observations in the post-period, depending on whether a record is posted in the repository or only

on a state website.  $Records_{i, \neq s, (t-3, t-1)}$  is the number of firm  $i$ 's enforcement records in states other than state  $s$  from 2007 to 2009 (2010 to 2012) for observations in the pre-period (post-period), regardless of where the records are disclosed.  $Records_{i, s, (t-3, t-1)}$  is the number of firm  $i$ 's enforcement records in state  $s$  from 2007 to 2009 (2010 to 2012) for observations in the pre-period (post-period), regardless of where the records are disclosed.

Even without the centralized repository, state regulators may acquire the enforcement records from other states for use in their own supervision.  $Records_{i, \neq s, (t-3, t-1)}$  should capture this information spillover effect. Because the enforcement records posted in the centralized repository are a subset of all enforcement actions, *Repository* captures the *incremental* effect of posting enforcement actions in the centralized repository relative to disclosing them only on state websites. If the centralized repository alerts the regulator about a company's misconduct in other states, the probability of the same firm being sanctioned should increase, so we expect  $\beta_I$  to be positive. Unlike in a traditional difference-in-differences design, our variable of interest is a continuous variable instead of an indicator variable because a firm may have been sanctioned in multiple states in the previous three years, and more than one enforcement record could be posted in the centralized repository. This design is similar to the difference-in-differences design with a continuous treatment used in DeFond and Lennox (2017).

Our design is also similar to Raghunandan and Ruchti (2022) in how it sets up explanatory variables and fixed effects.<sup>13</sup> For example, both their models and ours include prior enforcement actions in other states, prior enforcement actions in the same state, and company  $\times$  state fixed

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<sup>13</sup> Raghunandan and Ruchti (2022) examine the pattern of firms' workplace safety violations under the supervision of the Occupational Safety and Health Administration, a federal agency that already had a centralized repository of misconducts. Their main finding is that firms caught violating in one state subsequently violate more in other states. The primary difference between their study and ours is that we uniquely exploit the introduction of a centralized repository and examine its impact on regulators.

effects to predict subsequent enforcement actions. However, unlike their setting, ours includes a shock to regulators’ information processing costs: the introduction of the centralized repository. Therefore, we have an additional explanatory variable, *Repository*, which is our variable of interest.

Following Naughton et al. (2015) and Cuny et al. (2020), we control for the state-level demographic characteristics, including state population (*Log Population*), average resident income (*Log Income*), the percentage of residents with a bachelor’s degree or higher (*Education*), and the minority percentage (*Minority*). We also control for two state government characteristics—the total governmental expenditure (*Log Gov Expenditure*) and the number of state government employees (*Log Gov Employees*). Because mortgage companies are private firms whose financial information is not publicly available, we do not control for their financial characteristics. Last, we control for company  $\times$  state and period fixed effects because the panel dataset we use for this test is at the company-state-period level. We cluster standard error by company.

To test the effect of the centralized repository on credit supply, we estimate the following equation using a loan application–level dataset:

$$\begin{aligned}
 Approval_{i,j,y} = & \beta_1 Repository_{i,\neq s,(t-3,t-1)} + \beta_2 Record_{i,\neq s,(t-3,t-1)} + \beta_3 Record_{i,s,(t-3,t-1)} \\
 & + \beta_4 Log Borrower Income + \beta_5 Log Loan Amount + \beta_6 Borrower Gender \\
 & + Company \times State FEs + Application Year \times Loan Characteristics FEs + \varepsilon_{i,s,y}
 \end{aligned}
 \tag{2}$$

This equation is similar to Equation (1) except that the unit of observation is a loan application. *Approval* equals 1 if bank *i* approves borrower *j*’s application in year *t* and 0 if bank *i* denies the application. *Repository*<sub>*i, ≠s, (t-3, t-1)*</sub>, *Record*<sub>*i, ≠s, (t-3, t-1)*</sub>, and *Record*<sub>*i, s, (t-3, t-1)*</sub> are defined the same way as in Equation (1) and are merged to lender *i*’s loan applications from state *s* in year *y*. Following Dou et al. (2018), our loan controls include borrower income (*Log Borrower Income*), loan amount (*Log Loan Amount*), and an indicator variable for borrower gender (*Borrower*

*Gender*). Also, following their study, we interact the application year fixed effects with loan-characteristics fixed effects such as loan type, loan purpose, and property type. We cluster standard error by company.

#### **4. Data**

We begin by constructing a comprehensive sample of all state regulator enforcement actions against mortgage companies from 2007 to 2014 posted on state websites. Of the 50 states and the District of Columbia, five states (Iowa, Kansas, Delaware, North Dakota, and Wyoming) do not disclose enforcement action records on their websites. For two of them (Iowa and Kansas), we obtained the enforcement records through Freedom of Information Act requests. We did not obtain enforcement records from Delaware or North Dakota, because these states have laws prohibiting regulators from disclosing the enforcement records to the public.<sup>14</sup> We did not obtain enforcement records from Wyoming, because it has never issued an enforcement action against a mortgage company. As a result, our sample does not include Delaware, North Dakota, or Wyoming and thus consists of 47 states and the District of Columbia.

We first locate each state regulator’s webpage that discloses enforcement actions (see Appendix B for examples), then scrape the enforcement records from each site. A few states, including Texas and California, have two state regulators that supervise mortgage companies; in these states, we treat the two as a single regulator and combine their records (see Appendix C). We manually read through each record to exclude enforcement actions imposed on non-mortgage companies (e.g., payday lenders, money transmitters).

In our setting, enforcement records reflect both supervisory inputs and outputs. First, enforcement records are used as an *input* of state regulators’ supervisory information set.

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<sup>14</sup> These two states confirmed to us that their enforcement actions are available to other state regulators upon request.

Specifically, in the pre-period (2010–2011), we assume that a state regulator may learn about enforcement actions that occurred from 2007 to 2009 (i.e., the observation window for the pre-period) from other states. Similarly, in the post-period (2013–2014), we assume that a state regulator may learn about enforcement actions that occurred from 2010 to 2012 (i.e., the observation window for the post-period) from other states. This means that, to merit inclusion in our sample, a mortgage company must be sanctioned by a state regulator at least once from 2007 to 2012. Second, enforcement actions are also an *output* of state regulators’ supervision: a state regulator may issue its own enforcement action against a company after learning about the firm’s enforcement records in other states and conducting its own investigation. Thus, we require that a company that is sanctioned in state  $s$  from 2007 to 2009 still exist during 2010 and 2011 so that we can examine its likelihood of being sanctioned in other states during this period. Similarly, we require that a company that is sanctioned in state  $s$  from 2010 to 2012 still exist during 2013 and 2014 so that we can examine its likelihood of being sanctioned in other states during this period. These criteria yield 4,626 enforcement actions against 3,123 mortgage companies.

Next, for each mortgage company that is sanctioned from 2007 to 2012, we obtain the state-level license history from the Nationwide Mortgage Licensing System (NMLS) database and construct a company-state-year panel dataset. The purpose of this dataset is to examine the likelihood of the company’s receiving another enforcement action from other states subsequent to the posting of its enforcement records. Because a company is rarely sanctioned by the same state in two consecutive years, we collapse two years into a single period.<sup>15</sup> As a result, there are two observations for each company-state pair in our sample: one for the pre-period (2010–2011) and one for the post-period (2013–2014). Because we include company-state fixed effects, we exclude

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<sup>15</sup> Only 0.08% of firm-states in our sample receive more than one enforcement action in a two-year period.

company-state pairs that have only one of the two period observations (i.e., singletons). Our final sample includes 17,806 company-state-period observations, mapped to 1,632 enforcement records associated with 1,102 unique mortgage companies from 2007 to 2012.

Last, we merge these 1,632 records with the centralized repository to determine which of them are also posted in the repository. These treatment records include 366 enforcement actions that occurred from 2010 to 2012 and were posted in the centralized repository in 2012. These records experience a reduction in information processing costs in 2012 and therefore are likely to affect state regulators' supervisory actions in the post-period (2013–2014). The remaining 1,266 records are used as control records.

To construct state-level control variables, we collect, from the U.S. Census Bureau's American Community Survey, each state's total population, average income, average education attainment, and minority percentage. We obtain the number of employees in each state government from the Census's Annual Survey of Public Employment and Payroll. And we obtain each state government's total expenditures from the state government financial statements collected by Kim et al. (2022b).

## **5. Empirical Results**

### *5.1. The Effect of the Centralized Repository on Subsequent Enforcement Actions*

We report descriptive statistics of the company-state-period sample in Table 1. We find that 5% of company-states in our sample receive an enforcement action in either the pre-period or the post-period. The mean of  $Records_{i, \neq s, (t-3, t-1)}$  is 1.07, indicating that a company-state, on average, has 1.07 enforcement records in other states during the preceding three years. The 0.22 mean of  $Repository_{i, \neq s, (t-3, t-1)}$  indicates that a company-state, on average, has 0.22 enforcement records from other states that are posted in the centralized repository. Regarding the state-level controls,

we find that the average household annual income is \$52,440 and that 29% of residents hold a bachelor's degree or higher.

To examine the centralized repository's impact on subsequent enforcement actions, we estimate Equation (1) using the company-state-period sample discussed in Section 4 and report the results in Table 2. Columns (1) and (2) report the results without and with control variables, respectively. We find that the coefficient on *Repository*, the main variable of interest, is positive and statistically significant, ranging between 0.023 and 0.039 across the two columns. The 0.023 coefficient magnitude in Column 2 (with all controls) implies that for every enforcement record posted in the centralized repository, the probability of that same company being sanctioned by another state regulator in the subsequent two years increases, on average, by 2.3%, corresponding to a 46% increase of the unconditional mean of enforcement actions (i.e., 5.0%).

Regarding the control variables, we find that the coefficient on *Records<sub>*i, ≠s, (t-3, t-1)*</sub>* is positive but statistically insignificant, suggesting that state regulators on average do not often acquire the enforcement records in other states if they are not posted in the repository. The coefficient on *Records<sub>*i, s, (t-3, t-1)*</sub>* is significantly negative, suggesting that a firm is significantly less likely to be sanctioned again in a state where it has been previously sanctioned. This pattern could be explained by mortgage companies' correcting their misbehavior in accordance with the requirements enumerated by the local regulator in the enforcement action. Also, we find that regulators in states with larger populations, more government expenditures, and more government employees are more likely to issue enforcement actions against mortgage companies.

We conduct several robustness tests. For brevity, we report the results of these analyses in Table A2 of the Online Appendix. First, we rerun the main tests at the firm-state-year level and report the results in Column (1). In the main test, we collapsed two years into a one-period



observation in both the pre-period and the post-period because firms rarely receive two enforcements from the same state in two consecutive years. We find that our results are robust to not collapsing two years into a period. Second, we construct alternative specifications for the record-based variables ( $Repository_{i, \neq s, (t-3, t-1)}$ ,  $Records_{i, \neq s, (t-3, t-1)}$ , and  $Records_{i, s, (t-3, t-1)}$ ). In Column (2), we convert all the continuous record-based variables into binary variables. Using the binary variables could make the coefficients easier to interpret but does not account for an additive effect in which posting more of a firm's enforcement records in the repository attracts more of the regulator's attention to that firm. In Column (3), we convert the continuous record-based variables into their log function, which better captures the marginal diminishing effect of posting one more record in the centralized repository. Our results remain the same using these two alternative explanations. Third, instead of assuming a three-year observation window for the record-based variables, we use two years and four years as alternative observation windows and reconstruct these three variables in Columns (4) and (5), respectively. We find that our inferences remain the same using these specifications.

### *5.2. Are Severe Enforcement Records More Likely to Be Posted in the Repository?*

One potential concern with the results above is that the records posted on the centralized repository might not be random. Specifically, if more severe enforcement actions are more likely to be posted in the repository, and if these actions were more likely to have drawn other state regulators' attention even if they had not been posted, this would explain our results above. To alleviate this concern, we examine whether the enforcement records posted in the repository are more severe than those not posted.

We use four variables to measure the severity of an enforcement action. Because an enforcement action in response to multiple types of misconduct is presumably more severe, we

first measure severity using the number of reasons for the action. For example, when a firm is sanctioned because it both overcharges borrowers and originates loans without a license, the number of reasons is two. To count the reasons why a mortgage company is sanctioned, we manually read through each enforcement action record. (Common reasons, we find, include hiring unlicensed loan officers, failure to notify regulators of significant events, failure to submit required documents to regulators, deceptive advertisements, and overcharging borrowers.) The second severity measure is the word count of an enforcement record. A higher word count may indicate that more issues were identified or that more corrective actions were required by the regulator. The third severity measure is the size of the monetary penalty imposed by the regulator. An enforcement action with a higher penalty is presumably more severe. And the fourth severity measure is an indicator variable for whether the mortgage company's state license is suspended or revoked. License suspension or revocation is arguably the most serious non-monetary penalty because it prohibits the company from conducting business in that state.

We conduct the test at the record level. Because *Repository*, our variable of interest in the main test, is defined based on whether a company's enforcement records from 2010 to 2012 are posted on the centralized repository, we restrict the record sample to those occurring during that period. We exclude enforcement records whose documents are not available. The dependent variable, *Post on Repository*, is an indicator variable that is equal to one if the record is posted in the repository, and zero otherwise. In Panel A of Table 3 we report the descriptive statistics of these variables. In Panel B of Table 3, we report the regression results of this analysis, where we do not find significant associations between *Post on Repository* and any of the four severity measures, suggesting that records posted in the centralized repository are not more severe than those not posted.

Furthermore, we directly asked state regulators about why they posted some, but not all, past enforcement actions in the repository at its launch in 2012. According to the state regulators who responded to our inquiries, they mostly attributed this decision to lack of staff and said that the choice of which actions to post was not strategically planned. For example, one state regulator explained to us that it had to find the enforcement record's physical file, scan it, and match it to the sanctioned company's ID in the NMLS system. Therefore, it may not be practical to post all past enforcement actions given the limited staff they have. This anecdotal evidence, together with our empirical findings above, helps alleviate the concern that our main results are driven by the severity of the posted records instead of the centralized repository itself.

### *5.3. Mechanism*

We argue that the mechanism linking the centralized repository to subsequent enforcement actions is a reduction in information processing costs. To provide evidence supporting this mechanism, we explore two factors that could affect information processing costs in our setting. First, some information is more costly to acquire than other information. In our setting, for example, some state regulators disclose a separate list of enforcement actions against mortgage companies on their websites, making it relatively easy for other regulators to acquire these records (even if the records are not posted on the centralized repository). In contrast, other state regulators pool their enforcement actions against mortgage companies with enforcement actions against other types of companies (e.g., payday lenders). To separate actions against mortgage companies from actions against other entities in these states, one would have to read each enforcement record, which increases information processing costs. Because the centralized repository uses a standardized disclosure format, we expect the reduction in information acquisition costs to be greater for records from states whose websites make information harder to process.

To test our prediction, we partition the enforcement records posted on the centralized repository based on the difficulty of acquiring those same records from state websites. Specifically, we classify a state website as “low information processing cost” if it either separately lists enforcement records about mortgage companies or pools all enforcement actions but labels each entity by type. We classify a state website as “high information processing cost” if 1) it pools all enforcement actions and does not label entities by their type, or 2) does not directly disclose enforcement actions. Among the 48 states in our sample, 26 are “low information processing cost” and 22 are “high information processing cost.”<sup>16</sup> Accordingly, we partition our *Repository* variable into *Repository High-cost Records* and *Repository Low-cost Records*. To match the partition on *Repository*, we partition our *Records* variable into *High-cost Records* and *Low-cost Records*.

We report the results of this analysis in Table 4. We find that the coefficients on *Repository High-cost Records* and *Repository Low-cost Records* are both significantly positive. The difference between these two variables is significant ( $F$ -statistic= 9.12,  $p$ -value < 0.01), suggesting that the centralized repository’s effect on subsequent enforcement actions is stronger for records from states whose websites make information harder to process. In addition, the association between *Low-cost Records* and subsequent enforcement actions is positive and statistically significant, while the association between *High-cost Records* and subsequent enforcement actions is statistically insignificant. This suggests that state regulators acquire records from state websites with low information processing costs but not from state websites with high information processing costs.

The second factor that may affect information processing costs is differences in the regulators’ ability to process the information. In other words, the same information could be more

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<sup>16</sup> The 22 states with high information processing costs include Iowa and Kansas—the two states that do not disclose enforcement actions on the website but provide the records upon request.

costly for some regulators to process than for others. The capacity to process information is largely determined by resource levels. Because regulators with limited resources are more likely to lack information about the companies they supervise, we expect them to benefit more from the centralized repository.

We measure a regulator's resource constraints based on the state's ratio of mortgage examiners to regulated mortgage companies.<sup>17</sup> A lower examiner-to-company ratio indicates a higher likelihood of understaffing. We surveyed all 48 regulators in our sample in 2021 about the number of mortgage examiners in their departments and received responses from 35. We obtained the needed information for five of the 13 remaining states through state websites, so our final sample, for this analysis, consists of 40 states (83% of the 48 state regulators). We list the state regulators we surveyed and the number of mortgage examiners in each state in Appendix C. We find that the median state regulator has 12 examiners. Using the mortgage company license information from NMLS, we find that the median state regulator oversees 850 mortgage companies. That means, for a typical state in our sample, each mortgage examiner oversees approximately 71 mortgage companies. This ratio is consistent with anecdotal evidence that some state regulators are severely understaffed in terms of mortgage company supervision (e.g., Ip and Paletta 2007; Manning 2008).

We next partition our sample into two subsamples: one above and one below the median state's examiner-to-company ratio. We estimate Equation (1) for each subsample and report results of this analysis in Table 5. We find that the coefficient on *Repository* is significantly positive both for states whose regulators have a lower examiner-to-company ratio and for states whose

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<sup>17</sup> We cannot scale the number of examiners by the total number of these mortgage companies' loans because many companies are mortgage brokers or servicers and their total assets and loans originated (or served) are not publicly available.

regulators have a higher examiner-to-company ratio. Consistent with our expectation, the difference between the two variables' coefficients is significant ( $\chi^2 = 7.44$ ,  $p$ -value  $< 0.01$ ), suggesting that the centralized repository indeed holds more value for states with fewer resources.

#### *5.4. Do Regulators Free-Ride on the Records in the Repository?*

Our results thus far suggest that regulators are more likely to scrutinize and sanction mortgage companies whose enforcement records are posted in the repository. A potential concern is that the repository lets regulators free-ride by investigating companies less thoroughly and instead relying on other states' findings. To evaluate the validity of this concern, we identify enforcement actions that could be linked and examine how often they cite the same reason for the enforcement.

Specifically, for each enforcement action against firm  $i$  in state  $s$  in the post-period of 2013–2014, we try to identify, in the repository, enforcement actions against firm  $i$  occurring from 2010 to 2012 in states other than  $s$ .<sup>18</sup> In doing so, we link 105 enforcement actions in the post-period with at least one enforcement action in the repository. If state regulators do free-ride on records in the repository, the reasons for subsequent enforcement actions should be either the same as, or a subset of, the reasons for the enforcement actions posted in the repository. For example, an enforcement action in the repository might reveal that a firm overcharges borrowers in a certain state. After observing this record in the repository, an effort-minimizing regulator in another state might check whether the firm is overcharging borrowers in the regulator's own state but forgo a comprehensive investigation into other types of misconduct. As a result, the misconduct identified in the later enforcement action would be the same as in the repository record.

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<sup>18</sup> If there is more than one record in the repository, we take the union of the reasons disclosed in the records.

We find that 87% (91/105) of subsequent enforcement actions contain types of misconduct that were not covered by the earlier linked enforcement records in the repository. This suggests that, in most cases, regulators conduct additional investigations that reveal other types of misconduct by the company.

Last, we highlight that the issuance of a state enforcement action requires hard evidence of a company's misconduct in that state. Therefore, even if an enforcement action provides the same reason as an action in the repository, the state regulator still has to exert effort to prove that the misconduct exists in its own state.

### *5.5. The Effect of the Centralized Repository on Mortgage Lenders*

In this section, we examine whether the centralized repository has real effects on mortgage lenders, a subset of mortgage companies. Our results thus far suggest that the centralized repository reduces information processing costs for regulators, leading to heightened scrutiny of companies whose enforcement actions are posted in the repository. Anecdotal evidence suggests that the rapid expansion of mortgage companies in the post-crisis period is primarily due to lax supervision (Ackerman 2019; Marte 2018). We therefore predict that a mortgage company, in response to a potential increase in regulatory scrutiny driven by a post in the repository, may reduce its credit supply in the states where it is not sanctioned.

To test this hypothesis, we obtain loan-level application data from HMDA and merge it with the mortgage companies in our sample. Because we test the effect on loan origination, our sample for this analysis only includes mortgage lenders. (Other mortgage companies, such as brokers or servicers, are excluded.) Following Dou et al. (2018), we restrict applications to those whose decision is either approval or denial, and we require the loan amount to be greater than

\$1,000 and the borrower's annual income to be greater than \$10,000.<sup>19</sup> Because there are only two loans unsecured by a lien and 280 loans with a Home Ownership and Equity Protection Act (HOEPA) status, we exclude them from the sample instead of indicating them with binary variables in the regression model. We also require a lender-state-year to receive a minimum of 100 applications. Last, to maintain a balanced sample, we further remove loan applications whose lender-states exist in the pre-period or post-period but not both. The final sample consists of 6,743,140 loan applications from 242 unique mortgage companies.

We report descriptive statistics of the loan application sample in Panel A of Table 6. We find that the lenders in our sample, on average, approve 78% of loan applications. The borrowers, on average, have an annual income of \$95,000 and apply for a mortgage of \$211,000. We report the regression results of Equation (2) in Panel B of Table 6. We find that the coefficients on *Repository* are significantly negative, suggesting that lenders whose enforcement records are posted in the centralized repository reduce their credit supply in other states relative to lenders whose enforcement records are only disclosed on state websites. The -2.5% coefficient magnitude (in Column 2 with all controls) implies that for every enforcement record posted in the centralized repository, the loan approval rate of the sanctioned lender in other states declines by 2.5%, corresponding to 3% of the unconditional mean of loan approval. Regarding the control variables, we find that borrowers with higher incomes and smaller loan amounts are more likely to receive loan approvals.

In addition, we examine whether the centralized repository's effect on credit supply varies with borrower riskiness. Risky borrowers have a higher likelihood of future default and thus are likely to draw greater attention from regulators, so we expect the reduction in credit supply to be

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<sup>19</sup> We do not include applications that are withdrawn by borrowers or closed for incompleteness.



larger for them. Following Fuster et al. (2021), we measure borrower riskiness using the loan-to-income ratio, with a higher ratio indicating greater risk. We partition the application sample into two subsamples based on the median loan-to-income ratio and run Equation (2) separately for each. Table 7 presents the results of this analysis. We find that the coefficient on *Repository* is significantly negative for both high- and low-risk borrowers. The difference in coefficients on *Repository* between the two subsamples is statistically significant ( $\chi^2 = 3.82$ ,  $p$ -value  $< 0.10$ ), suggesting that the centralized repository's effect on credit supply is stronger for riskier borrowers.

### 5.6. A Falsification Test

An important assumption underlying the difference-in-differences design is that the treatment and control groups should exhibit parallel trends in the absence of the treatment. A common empirical approach to testing this assumption is to examine whether treatment firms and control firms exhibit parallel trends in the outcome variable in the pre-period. Whereas most studies define the treatment and control groups at the firm or user level, we define them at the *record* level. Therefore, in order to test the parallel trends assumption, we examine whether, during the pre-period, the treatment records (i.e., those posted on the centralized repository) exhibit a similar probability of subsequent enforcement action as the control records.

Specifically, we pretend that the centralized repository was launched in 2010 rather than 2012 and set 2011 (the year before the repository's actual launch year) as the pseudo post-period and 2009 as the pseudo pre-period. For each period, we assume a two-year preceding window for regulators to learn about enforcement records from other states. This means that the observation window for the pseudo pre-period (pseudo post-period) is 2007–2008 (2009–2010).<sup>20</sup> The

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<sup>20</sup> We shorten both the enforcement action periods and their observation windows by one year (compared with the main analyses) because we need to conduct the falsification test during 2007–2011, the period before the centralized repository was launched. This period in total is shorter than the period used in the main analyses (2007–2014).

rationale for this test can be illustrated with an example. Our main findings show that if a 2010 enforcement record is posted on the centralized repository upon its 2012 launch, the sanctioned company faces a higher probability of subsequent enforcement actions in other states in 2013–2014. If this effect is truly driven by the repository rather than the record itself, we should not observe that the same record leads to a higher probability of subsequent enforcement actions in other states in 2011, the year *before* the repository’s launch (i.e., the pseudo post-period).

To conduct this analysis, we re-estimate Equations (1) and (2) using the pseudo sample periods and report the results of these analyses in Table 8. We find that the coefficients on *Pseudo Repository Post*  $i, \neq s, (t-2, t-1)$  are statistically insignificant, suggesting that the records posted in the centralized repository do not lead to a higher probability of subsequent enforcement actions or reduce credit supply in the period *before* they are posted in the repository. These results further support the notion that our main findings are driven by the centralized repository itself rather than by the regulator’s selection of which pre-period records to post there. This test also helps further mitigate the concern that the records posted in the repository are not random and would have elicited other state regulators’ attention even if they had not been posted in the repository.

## **6. Conclusion**

To our knowledge, this is the first study to examine the effect of information processing costs on regulators. Exploiting the centralized repository’s introduction as a shock that reduces information processing costs, we find that records that are posted in the repository are more likely to be used by other state regulators and to subsequently influence their supervisory actions. Additional cross-sectional analyses show that the effect is stronger for records that are subject to a greater reduction in processing costs (i.e., those from states with less user-friendly websites) and for regulators with more limited resources. Finally, we show that the heightened scrutiny resulting

from decreasing regulators' information processing costs causes lenders to reduce their credit supply.

Although our study shows that information processing costs affect regulators' supervisory outcomes, we caveat that it does not directly speak to whether reducing such costs makes regulators more effective in supervising mortgage companies. A regulator's effectiveness is inherently unobservable because more effective regulation means there are fewer firms that engage in misconduct and are *not* caught by regulators. However, we can only observe firms' misconduct that regulators uncover. Therefore, our study only provides evidence that information processing costs *matter* to regulators. This is similar to prior accounting literature that speaks to whether recognition versus disclosure matters but cannot suggest which is better (e.g., Bratten et al. 2013, Michels 2017, Neilson et al. 2022).

Another possible concern is whether our main findings mean that regulators "herd" to sanction the same company. We believe that such an interpretation is not likely because enforcement actions cannot be solely based on speculation. To issue an enforcement action, a regulator must possess hard evidence of the mortgage company's misconduct *in the regulator's own state*. For example, the fact that a company's loan offers charged excessive and unlawful fees in Georgia warrants an enforcement action only in Georgia. To issue a similar enforcement action in Florida, the Florida regulator would have to investigate and make sure that the same issue exists there.

Last, based on our discussions with state regulators, we learned that state regulators often try to resolve a company's misbehavior prior to issuing an enforcement action. Specifically, state regulators may issue a warning and give the company a grace period to fix the problem, then follow up to see whether the problem has been resolved. As a result, our study likely understates the effect

of reducing regulators' information-processing costs, as our data does not allow us to quantify the repository's effect on regulators' unobservable actions and their outcomes.

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## Appendix A. Variable Definitions

Variable	Definition
<b>Enforcement related variables</b>	
<i>Enforcement</i> $i, s, t$	= 1 if firm $i$ receives an enforcement action in state $s$ in period $t$ , 0 otherwise.
<i>Repository</i> $i, \neq s, (t-3, t-1)$	The number of firm $i$ 's enforcement records in states other than state $s$ from 2010 to 2012 that are posted in the centralized repository at its launch in 2012. The 2010–2012 period is the three-year observation window before the post period (2013–2014).
<i>Repository High-cost Records</i> $i, \neq s, (t-3, t-1)$	The number of firm $i$ 's enforcement records in states (other than state $s$ ) with high website processing costs from 2010 to 2012 that are posted in the centralized repository at its launch in 2012. We classify a state website as “high information processing cost” if 1) it pools all enforcement records and does not identify those related to mortgage companies or 2) does not directly disclose enforcement records on the website.
<i>Repository Low-cost Records</i> $i, \neq s, (t-3, t-1)$	The number of firm $i$ 's enforcement records in states (other than state $s$ ) with low website processing costs from 2010 to 2012 that are posted in the centralized repository at its launch in 2012. We classify a state website as “low information processing cost” if it 1) separately discloses enforcement actions against mortgage companies or 2) pools all enforcement records and identifies those related to mortgage companies.
<i>Pseudo Repository</i> $i, \neq s, (t-2, t-1)$	The number of firm $i$ 's enforcement records in states other than state $s$ in 2009–2010 that are posted in the centralized repository at its launch in 2012. The years 2009 and 2010 are the two-year observation window before the pseudo post period (2011).
<i>Records</i> $i, \neq s, (t-3, t-1)$	The number of firm $i$ 's enforcement records in states other than state $s$ in the three-year observation window before period $t$ .
<i>High-cost Records</i> $i, \neq s, (t-3, t-1)$	The number of firm $i$ 's enforcement records in states (other than state $s$ ) with high website processing costs in the three-year observation window before period $t$ . We classify a state website as “high information processing cost” in the same way as <i>Repository High-cost Records</i> $i, \neq s, (t-3, t-1)$
<i>Low-cost Records</i> $i, \neq s, (t-3, t-1)$	The number of firm $i$ 's enforcement records in states (other than state $s$ ) with low website processing costs in the three-year observation window before period $t$ . We classify a state website as “low information processing cost” in the same way as <i>Repository Low-cost Records</i> $i, \neq s, (t-3, t-1)$
<i>Pseudo Records</i> $i, \neq s, (t-2, t-1)$	The number of firm $i$ 's enforcement records in states other than state $s$ in the two-year observation window before the pseudo period $t$ .
<i>Records</i> $i, s, (t-3, t-1)$	The number of firm $i$ 's enforcement records in state $s$ in the three-year observation window before period $t$ .



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<i>Pseudo Records</i> $i, s, (t-2, t-1)$	The number of firm $i$ 's enforcement records in state $s$ in the two-year observation window before the pseudo period $t$ .
<u>State-level controls</u>	
<i>Log Population</i> $s, t$	The natural logarithm of the average population (in millions) in state $s$ in period $t$ .
<i>Log Income</i> $s, t$	The natural logarithm of the median household income (in \$thousands) in state $s$ in period $t$ .
<i>Education</i> $s, t$	The average percentage of individuals 25 years old or above who hold a bachelor's degree or higher in state $s$ in period $t$ .
<i>Minority</i> $s, t$	The average percentage of Black or Hispanic residents in the state $s$ population in period $t$ .
<i>Log Gov Expenditure</i> $s, t$	The natural logarithm of average annual expenditure of state government $s$ in period $t$ (in \$billions).
<i>Log Gov Employees</i> $s, t$	The natural logarithm of average number of employees of state government $s$ in period $t$ (in thousands).
<u>Record-level variables</u>	
<i>Post on Repository</i>	=1 if an enforcement record is posted on the centralized repository, 0 otherwise.
<i>Number of Reasons</i>	The number of reasons for which the firm is sanctioned in the enforcement record.
<i>Doc Length</i>	The word count of the enforcement record (in thousands).
<i>Fines</i>	The dollar amount of penalty imposed by the enforcement action (in \$thousands).
<i>Revoke License</i>	=1 if the company's mortgage license is suspended or revoked as a result of the enforcement action, 0 otherwise.
<u>Loan-level variables</u>	
<i>Approval</i>	= 1 if the borrower's loan application is approved and 0 if it is denied.
<i>Log Borrower Income</i>	The natural logarithm of the borrower's annual income (in \$ thousands).
<i>Log Loan Amount</i>	The natural logarithm of the principal loan amount (in \$ thousands).
<i>Borrower Gender</i>	= 1 for male borrowers and 0 for female borrowers.
<i>Race</i>	A categorical variable indicating the borrower race, including Asian, African American, native Hawaiian, other Pacific Islander, and white.
<i>Ethnicity</i>	= 1 if the borrower is Hispanic/Latino and 0 otherwise.
<i>Loan Purpose</i>	A categorical variable indicating the loan purpose, including for home purchase, home improvement, and refinancing.
<i>Loan Type</i>	A categorical variable indicating the loan type, including a loan that is conventional, insured by the Federal Housing Administration, guaranteed by the Veterans Administration, and guaranteed by the Farm Service Agency or Rural Housing Service.
<i>Property Type</i>	A categorical variable indicating the property type, including one- to four-family housing, manufactured housing, and multifamily housing.
<i>Owner Occupancy</i>	= 1 if the property is occupied by owner and 0 if the property is not occupied by owner.

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## Appendix B: Examples of State Websites that Disclose Enforcement Actions

1) State websites with *low* information acquisition costs:

**Kentucky** separately discloses enforcement actions against mortgage companies.

The screenshot shows the website for the Kentucky Department of Financial Institutions (DFI). The header includes the DFI logo and the text "DEPARTMENT OF FINANCIAL INSTITUTIONS". Below the header are navigation links: Depository, Non-Depository, Securities, Legal, Licensing & Registration, and Resources. A search bar is present with "Search" and "Clear" buttons. A disclaimer states: "DISCLAIMER: Please note that licenses or registrations which are denoted as 'revoked,' 'denied' or 'suspended' within the past 30 days are subject to appeal and may be in the process of being appealed. Click on the link below to view enforcement actions by year." Below this, there is a section for "Mortgage Enforcement Actions" for the year 2022. A table lists two actions:

Entity Name	Action	Date
Ruff Mortgage Company	Commissioner's Findings of Fact, Conclusions of Law, Final Order	05/19/2022
Nyra Lochner	Agreed Order	05/12/2022

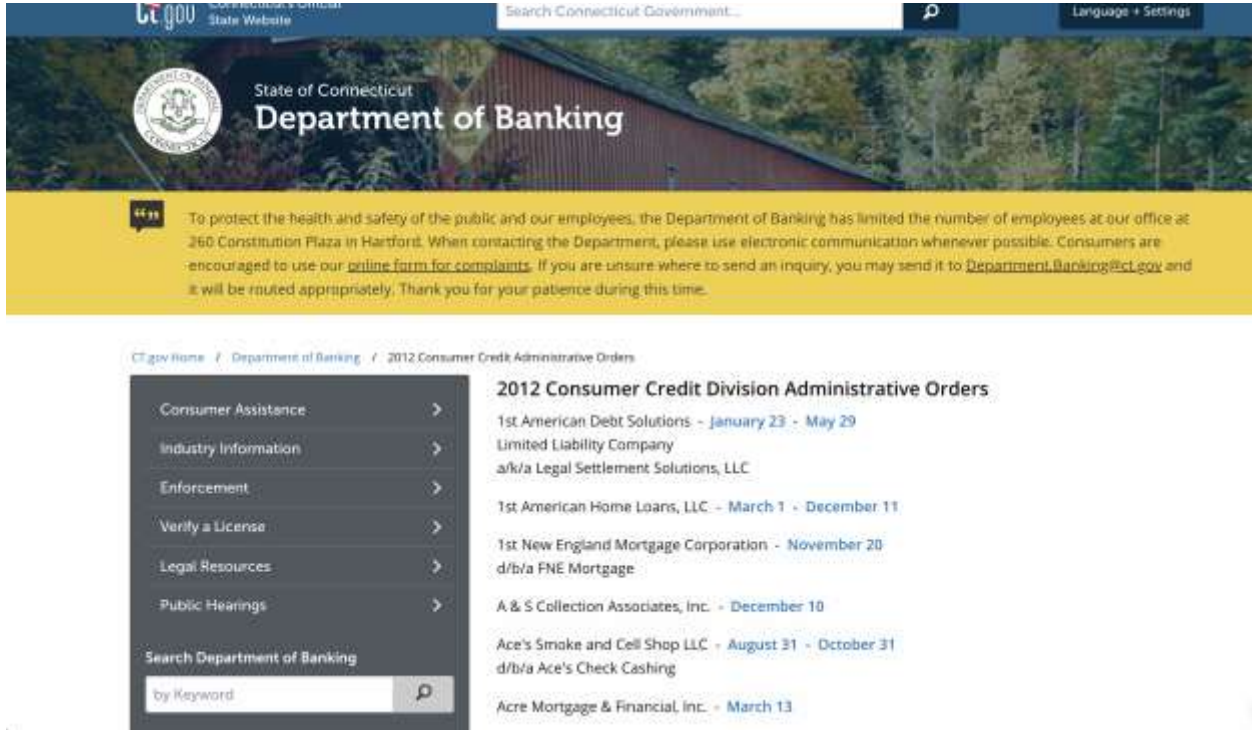
**Massachusetts** labels the industries of sanctioned companies, although it pools all enforcement actions.

The screenshot shows the website for the Massachusetts Department of Banking. It features a table of enforcement actions for the year 2022. The table has the following columns: Date, Regulatory Action and Docket Number, Entity Name and License Number, Entity Type, and Related. To the right of the table is a sidebar with contact information for the department, including phone numbers, hours of operation, and an address in Boston, MA.

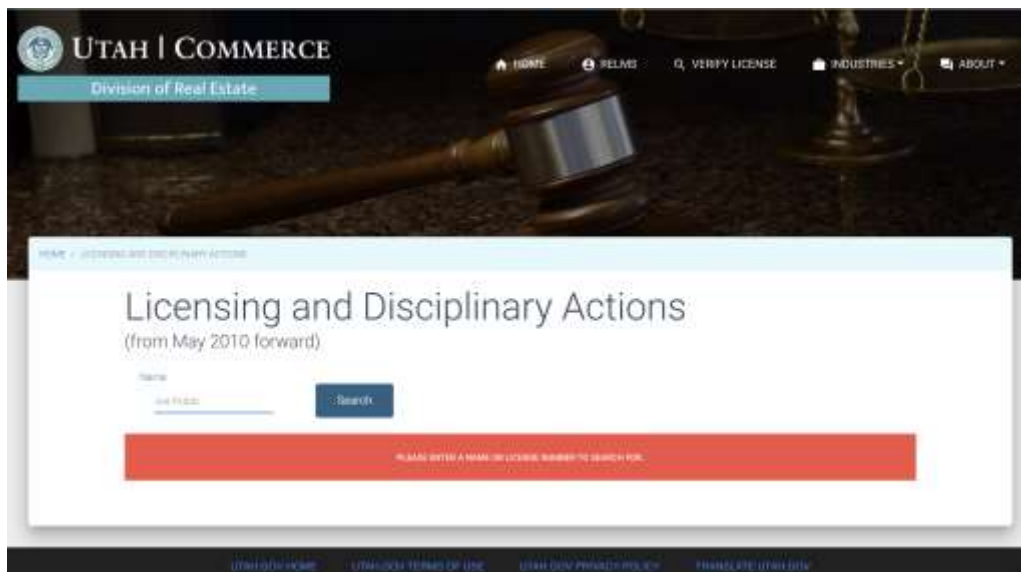
Date	Regulatory Action and Docket Number	Entity Name and License Number	Entity Type	Related
05/11/2022	<a href="#">Consent Order 2022-001</a>	Park Crest Corporation	Debt Collector	
04/13/2022	<a href="#">Consent Order 2021-004</a>	Monterey Financial Services, LLC	Debt Collector	
03/08/2022	<a href="#">Settlement Agreement 2021-003</a>	Andrew Marquis	Mortgage Lender and Mortgage Broker	Supersedes and replaces November 30, 2021 Temporary Order to Cease and Desist.
03/09/2022	<a href="#">Consent Order 2021-003</a>	Cross Country Mortgage, LLC	Mortgage Lender and Mortgage Broker	Supersedes and replaces November 30, 2021 Temporary Order to Cease and Desist.
02/07/2022	<a href="#">Consent Order 2021-006</a>	Mutual of Omaha Mortgage, Inc.	Mortgage Broker and Mortgage Lender	Supersedes and replaces 1/4/2022 Temporary Cease and Desist Order

2) State websites with *high* information acquisition costs:

**Connecticut** pools all enforcement actions and does not label the industries of sanctioned companies.



**Utah** only provides a search function for its enforcement actions.



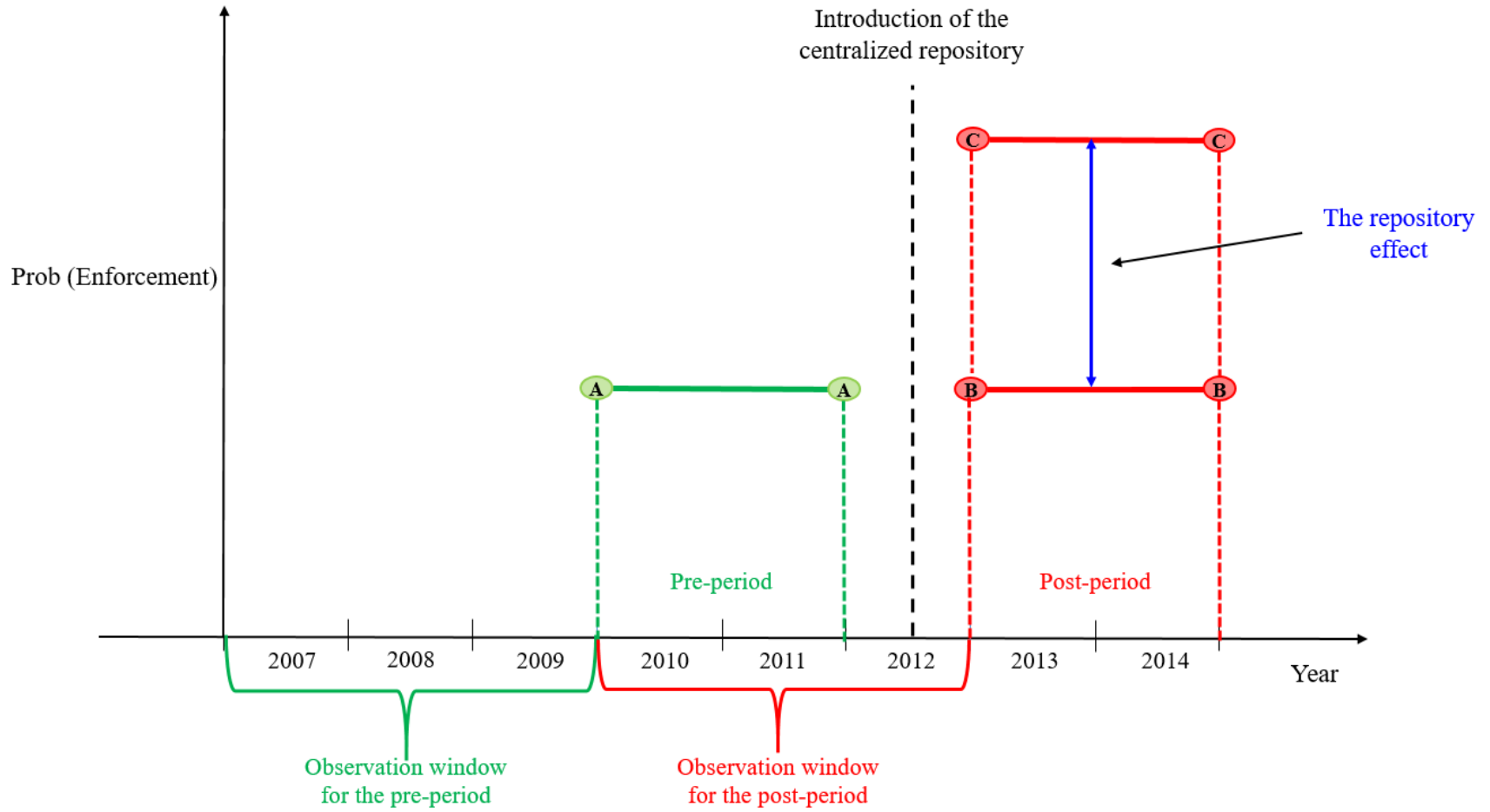
### Appendix C: The List of State Regulators

State	Regulatory Agency	Mortgage Examiners #
AK	Department of Commerce, Community, and Economic Development	26
AL	State Banking Department	N/A
AR	Securities Department	N/A
AZ	Department of Financial Institutions	8
CA	Department of Financial Protection and Innovation; Department of Real Estate	127
CO	Department of Regulatory Agencies- Department of Real Estate	N/A
CT	Department of Banking	12
DC	Department of Insurance, Securities and Banking Bureau	N/A
FL	Office of Financial Regulation	19
GA	Department of Banking and Finance	24
HI	Division of Financial Institutions	13
IA	Division of Banking	7
ID	Department of Finance	7
IL	Department of Financial and Professional Regulation	16
IN	Secretary of State Securities Division; Department of Financial Institutions	6
KS	Office of the State Bank Commissioner	9
KY	Department of Financial Institutions	7
LA	Office of Financial Institutions – Non-Depository Division	12
MA	Division of Banks	36
MD	Office of the Commissioner of Financial Regulation	26
ME	Bureau of Consumer Credit Protection	17
MI	Department of Insurance and Financial Services	13
MN	Department of Commerce	9
MO	Division of Finance	6
MS	Department of Banking and Consumer Finance	11
MT	Division of Banking and Financial Institutions	7
NC	Commissioner of Banks Office	20
NE	Department of Banking and Finance	N/A
NH	Banking Department	12
NJ	Department of Banking and Insurance	N/A
NM	Financial Institutions Division	5
NV	Division of Mortgage Lending	19
NY	Department of Financial Services - Mortgage Banking Division	N/A
OH	Division of Financial Institutions, Consumer Finance	9
OK	Department of Consumer Credit Licensing	38
OR	Division of Financial Regulation	8
PA	Department of Banking and Securities	N/A
RI	Department of Business Regulation	3
SC	Board of Financial Institutions; Department of Consumer Affairs	5
SD	Division of Banking	2
TN	Department of Financial Institutions	29

TX	Department of Savings and Mortgage Lending; Office of Consumer Credit Commissioner	17
UT	Division of Real Estate; Department of Financial Institutions	40
VA	Bureau of Financial Institutions	13
VT	Department of Financial Regulation	12
WA	Department of Financial Institutions; Division of Consumer Services	34
WI	Department of Financial Institutions	5
WV	Division of Financial Institutions	4

This list reports the regulator that oversees mortgage companies in each state and its number of individual mortgage examiners (as of 2021). For states with more than one regulator, we report the sum of their mortgage examiners.

**Figure 1. Mapping Enforcement Records to Subsequent Enforcements**



**Table 1**  
**Descriptive Statistics**

Enforcement action tests (17,806 firm-state-period observations)	Mean	STD	25 <sup>th</sup>	Median	75 <sup>th</sup>
<b>Dependent variables</b>					
<i>Enforcement</i> $i, s, t$	0.050	0.218	0.000	0.000	0.000
<b>Independent variables</b>					
<i>Repository</i> $i, \neq s, (t-3, t-1)$	0.221	0.502	0.000	0.000	0.000
<i>Repository High-cost Records</i> $i, \neq s, (t-3, t-1)$	0.062	0.241	0.000	0.000	0.000
<i>Repository Low-cost Records</i> $i, \neq s, (t-3, t-1)$	0.160	0.427	0.000	0.000	0.000
<i>Records</i> $i, \neq s, (t-3, t-1)$	1.066	1.263	0.000	1.000	1.000
<i>High-cost Records</i> $i, \neq s, (t-3, t-1)$	0.303	0.600	0.000	0.000	0.000
<i>Low-cost Records</i> $i, \neq s, (t-3, t-1)$	0.762	1.000	0.000	0.000	1.000
<i>Records</i> $i, s, (t-3, t-1)$	0.078	0.269	0.000	0.000	0.000
<i>Population</i> $s, t$	8.252	8.125	2.979	5.814	9.888
<i>Income</i> $s, t$	52.44	8.859	45.70	49.76	59.89
<i>Education</i> $s, t$	0.294	0.060	0.258	0.283	0.329
<i>Minority</i> $s, t$	0.320	0.152	0.197	0.307	0.425
<i>Gov Expenditure</i> $s, t$	38.40	39.59	15.81	25.99	47.38
<i>Gov Employees</i> $s, t$	133.0	99.41	73.79	103.0	165.7

This table reports descriptive statistics for the firm-state-period sample that we use in the enforcement action tests. See Appendix A for variable definitions.

**Table 2**  
**The Centralized Repository's Effect on Subsequent Enforcements**

Dependent variable = <i>Enforcement</i>	(1)	(2)
<i>Repository</i> $i, \neq s, (t-3, t-1)$	<b>0.039***</b> (6.92)	<b>0.023***</b> (4.68)
<i>Records</i> $i, \neq s, (t-3, t-1)$		0.003 (1.42)
<i>Records</i> $i, s, (t-3, t-1)$		-0.412*** (-29.39)
<i>Log Population</i> $s, t$		0.415** (2.06)
<i>Log Income</i> $s, t$		-0.133 (-0.69)
<i>Education</i> $s, t$		-5.145*** (-8.22)
<i>Minority</i> $s, t$		-5.831*** (-9.30)
<i>Log Gov Expenditure</i> $s, t$		0.330*** (4.43)
<i>Log Gov Employees</i> $s, t$		0.092* (1.77)
Company × State FEs	Y	Y
Period FEs	Y	Y
Observations	17,806	17,806
Adj. $R^2$	0.13	0.40

This table presents the regression results of estimating the centralized repository's effect on subsequent enforcement actions. The unit of observation is a company-state-period. The pre-period is 2010–2011 and the post-period is 2013–2014. See Appendix A for variable definitions. Standard errors are calculated by clustering observations by company.  $t$ -statistics are reported in parentheses below the coefficient estimates. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels.



**Table 3**  
**Are Severe Records More Likely to Be Posted in the repository?**

Panel A. Descriptive statistics

N = 926 enforcement action records	Mean	STD	25 <sup>th</sup>	Median	75 <sup>th</sup>
<b>Dependent variables</b>					
<i>Post on Repository</i>	0.378	0.485	0.000	0.000	1.000
<b>Independent variables</b>					
<i>Number of Reasons</i>	1.379	0.864	1.000	1.000	1.000
<i>Doc Length</i>	1.180	0.796	0.647	1.005	1.369
<i>Fines (\$000s)</i>	8.923	22.74	0.500	1.250	5.750
<i>Revoke Licenses</i>	0.205	0.404	0.000	0.000	0.000

Panel B. Regression results

Dependent variable = <i>Posted on Repository</i>	(1)	(2)	(3)	(4)
<i>Number of Reasons</i>	0.142 (1.02)			0.145 (0.96)
<i>Doc Length</i>		-0.007 (-0.03)		-0.040 (-0.17)
<i>Fines</i>			-0.000 (-0.01)	-0.001 (-0.11)
<i>Revoke Licenses</i>			-0.632 (-1.56)	-0.612 (-1.52)
State FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
Observations	926	926	926	926
<i>Pseudo R</i> <sup>2</sup>	0.53	0.53	0.53	0.53

This table presents the results of estimating whether severe records are more likely to be posted in the centralized repository. Panel A present the descriptive statistics of variables and Panel B present the regression results based on the logit model. We measure the severity of enforcement actions using the number of reasons behind the sanction (*Number of Reasons*), the word count of enforcement action dockets (*Doc Length*), monetary penalties (*Fines*), and non-monetary penalties (*Revoke License*). The unit of observation is an enforcement action record. Because our variable of interest in the main model (*Repository*) is defined based on whether a company's enforcement records from 2010 to 2012 are posted on the centralized repository, we restrict the record sample to the enforcement records occurring during that period. See Appendix A for variable definitions. Standard errors are calculated by clustering observations by company. *t*-statistics are reported in parentheses below the coefficient estimates. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels.

**Table 4**  
**The Centralized Repository's Effect on Subsequent Enforcements**  
**by Records' Pre-Repository Processing Costs**

Dependent variable = <i>Enforcement</i>	(1)
<i>Repository High-cost Records</i> $i, \neq s, (t-3, t-1)$	<b>0.050***</b> (4.41)
<i>Repository Low-cost Records</i> $i, \neq s, (t-3, t-1)$	<b>0.014***</b> (2.92)
<i>High-cost Records</i> $i, \neq s, (t-3, t-1)$	-0.002 (-0.58)
<i>Low-cost Records</i> $i, \neq s, (t-3, t-1)$	0.004* (1.68)
<i>Records</i> $i, s, (t-3, t-1)$	-0.412*** (-29.42)
<i>Log Population</i> $s, t$	0.409** (2.03)
<i>Log Income</i> $s, t$	-0.128 (-0.66)
<i>Education</i> $s, t$	-5.173*** (-8.27)
<i>Minority</i> $s, t$	-5.876*** (-9.38)
<i>Log Gov Expenditure</i> $s, t$	0.331*** (4.45)
<i>Log Gov Employees</i> $s, t$	0.093* (1.79)
Test of difference: <i>Repository High-cost Records vs. Repository Low-cost Records</i>	<b>F = 9.12***</b>
Company × State FEs	Y
Period FEs	Y
Observations	17,806
Adj. $R^2$	0.40

In this table, we partition the centralized repository's enforcement records into two groups based on the information-processing costs of the state websites that post the records. We replace *Repository* with *Repository High-cost Records* and *Repository Low-cost Records* (the sum of the two equals *Repository*) and replace *Records* with *High-cost Records* and *Low-cost Records* (the sum of the two equals *Records*). We classify a state website as "high information processing cost" if 1) it pools all enforcement records and does not identify those related to mortgage companies or 2) does not directly disclose enforcement records on its website (e.g., only providing a searching function). We classify a state website as "low information processing cost" if it 1) separately discloses enforcement actions against mortgage companies or 2) pools all enforcement actions but labels those against mortgage companies. See Appendix A for variable definitions. Standard errors are calculated by clustering observations by company. *t*-statistics are reported in parentheses below the coefficient estimates. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels.

**Table 5**  
**The Centralized Repository's Effect on Subsequent Enforcements**  
**by Regulators' Resource Constraints**

Examiner-to-company ratio: Dependent variable = <i>Enforcement</i>	Low (1)	High (2)
<i>Repository</i> $i, \neq s, (t-3, t-1)$	<b>0.035***</b> <b>(5.04)</b>	<b>0.013**</b> <b>(1.99)</b>
<i>Records</i> $i, \neq s, (t-3, t-1)$	0.003 (1.13)	0.001 (0.54)
<i>Records</i> $i, s, (t-3, t-1)$	-0.378*** (-20.35)	-0.371*** (-12.98)
<i>Log Population</i> $s, t$	-0.435 (-1.49)	0.483 (1.25)
<i>Log Income</i> $s, t$	-0.501 (-1.41)	0.029 (0.12)
<i>Education</i> $s, t$	-4.565*** (-4.14)	-4.932*** (-4.73)
<i>Minority</i> $s, t$	-5.744*** (-5.60)	-2.857*** (-2.88)
<i>Log Gov Expenditure</i> $s, t$	0.385*** (3.39)	0.103 (0.89)
<i>Log Gov Employees</i> $s, t$	0.317*** (4.90)	-0.133 (-1.62)
Test of coefficient difference	$\chi^2 = 7.44***$	
Company $\times$ State FEs	Y	Y
Period FEs	Y	Y
Observations	7,972	6,796
Adj. $R^2$	0.41	0.31

In this table, we partition the sample into two subsamples based on the resource constraints of the state regulators that observe the records in the centralized repository. We classify a state as more (less) resource-constrained if its ratio of mortgage examiners to regulated mortgage companies is below (above) the median. See Appendix A for variable definitions. Standard errors are calculated by clustering observations by company.  $t$ -statistics are reported in parentheses below the coefficient estimates. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels.

**Table 6**  
**The Centralized Repository's Effect on Credit Supply**

Panel A. Descriptive Statistics

N = 6,743,140 loan applications	Mean	STD	25 <sup>th</sup>	Median	75 <sup>th</sup>
<i>Approval</i>	0.782	0.413	1.000	1.000	1.000
<i>Repository</i> $i, \neq s, (t-3, t-1)$	0.250	0.512	0.000	0.000	0.000
<i>Records</i> $i, \neq s, (t-3, t-1)$	1.443	1.453	0.000	1.000	2.000
<i>Records</i> $i, s, (t-3, t-1)$	0.082	0.275	0.000	0.000	0.000
<i>Borrower Income</i>	0.095	0.103	0.048	0.074	0.115
<i>Loan Amount</i>	0.211	0.147	0.116	0.178	0.273
<i>Borrower Gender</i>	0.707	0.455	0.000	1.000	1.000

Panel B. Regression results

Dependent variable = <i>Approval</i>	(1)	(2)
<i>Repository</i> $i, \neq s, (t-3, t-1)$	<b>-0.016*</b> <b>(-1.68)</b>	<b>-0.025**</b> <b>(-2.33)</b>
<i>Records</i> $i, \neq s, (t-3, t-1)$		0.007 (1.52)
<i>Records</i> $i, s, (t-3, t-1)$		-0.011 (-1.29)
<i>Log Borrower Income</i>		0.066*** (7.64)
<i>Log Loan Amount</i>		-0.032*** (-3.91)
<i>Borrower Gender</i>		0.000 (0.07)
Company × State FEs	Y	Y
Application Year FEs	Y	N
Application Year × Loan-characteristics FEs	N	Y
Observations	6,743,140	6,743,140
Adj. $R^2$	0.21	0.22

Panels A and B of this table report descriptive statistics and the regression results of estimating the centralized repository's effect on the lender's loan approval probability. The observation is a loan application. Following Dou et al. (2018), we include but do not tabulate fixed effects for application year × each of *Race*, *Ethnicity*, *Loan Purpose*, *Loan Type*, *Property Type*, and *Owner Occupancy*. See Appendix A for variable definitions. Standard errors are calculated by clustering observations by company. *t*-statistics are reported in parentheses below the coefficient estimates. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels.

**Table 7**  
**The Centralized Repository's Effect on Credit Supply**  
**by Borrower Riskiness.**

Dependent variable = <i>Approval</i>	Low-risk borrowers	How-risk borrowers
	(1)	(2)
<b><i>Repository</i></b> $i, \neq s, (t-3, t-1)$	<b>-0.017*</b> <b>(-1.69)</b>	<b>-0.030**</b> <b>(-2.46)</b>
<i>Records</i> $i, \neq s, (t-3, t-1)$	0.005 (1.19)	0.009* (1.68)
<i>Records</i> $i, s, (t-3, t-1)$	-0.008 (-1.03)	-0.015 (-1.44)
<i>Log Borrower Income</i>	0.002 (0.44)	0.202*** (8.81)
<i>Log Loan Amount</i>	0.024*** (5.45)	-0.180*** (-8.48)
<i>Borrower Gender</i>	0.003 (1.56)	-0.004* (-1.92)
Test of coefficient difference	$\chi^2 = 3.82^*$	
Company $\times$ State FEs	Y	Y
Application Year $\times$ Loan-characteristics FEs	Y	Y
Observations	3,369,293	3,373,847
Adj. $R^2$	0.22	0.25

This table presents the regression results of estimating the effect of the centralized repository on lenders' loan approval probability by borrower riskiness. We partition the loan application sample into two subsamples based on borrower riskiness. We define a borrower as high (low) risk if their loan-to-income ratio is above (below) the sample median. See Appendix A for variable definitions. Standard errors are calculated by clustering observations by company.  $t$ -statistics are reported in parentheses below the coefficient estimates. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels.

**Table 8**  
**A Falsification Test**

Dependent variable =	<i>Enforcement</i> (1)	<i>Approval</i> (2)
<i>Pseudo Repository</i> $i, \neq s, (t-2, t-1)$	<b>0.001</b> <b>(0.07)</b>	<b>0.014</b> <b>(0.66)</b>
<i>Pseudo Records</i> $i, \neq s, (t-2, t-1)$	0.011*** (3.83)	0.004 (0.43)
<i>Pseudo Records</i> $i, s, (t-2, t-1)$	-0.297*** (-17.33)	-0.022** (-2.43)
<i>Log Population</i> $s, t$	-0.777*** (-2.78)	
<i>Log Income</i> $s, t$	-0.328** (-2.47)	
<i>Education</i> $s, t$	0.612 (1.28)	
<i>Minority</i> $s, t$	-1.196 (-1.39)	
<i>Log Gov Expenditure</i> $s, t$	0.112 (1.25)	
<i>Log Gov Employees</i> $s, t$	-0.024 (-0.25)	
<i>Log Borrower Income</i>		0.083*** (7.64)
<i>Log Loan Amount</i>		-0.057*** (-4.68)
<i>Borrower Gender</i>		-0.001 (-0.68)
Observations	11,178	2,183,885
Adj. $R^2$	0.27	0.25

This table presents the regression results of re-estimating Equations (1) and (2) using the pseudo-event. We pretend that the centralized repository was launched in 2010 and focus on the two-year window around this pseudo-event. For both the pseudo pre-period and the pseudo post-period, we assume a two-year preceding observation window during which regulators may learn about enforcement records from other states. For the subsequent enforcement test (column 1), we include company  $\times$  state and period fixed effects. For the credit supply test (column 2), we include company  $\times$  state and application year  $\times$  loan-characteristic fixed effects. See Appendix A for variable definitions. Standard errors are calculated by clustering observations by company.  $t$ -statistics are reported in parentheses below the coefficient estimates. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels.

## Online Appendix

**Table A1. Additional Variable Definitions**

Variable	Definition
<u>Firm-state-period variables</u>	
<i>Repository Indicator</i> $i, \neq s, (t-3, t-1)$	= 1 if at least one firm $i$ 's enforcement record in states other than state $s$ from 2010 to 2012 is posted in the centralized repository at its launch in 2012, 0 otherwise.
<i>Repository Log</i> $i, \neq s, (t-3, t-1)$	The natural logarithm of $(1 + \text{Repository } i, \neq s, (t-3, t-1))$ .
<i>Repository</i> $i, \neq s, (t-2, t-1)$	The number of firm $i$ 's enforcement records in states other than state $s$ from 2011 to 2012 that are posted in the centralized repository at its launch in 2012. We assume a two-year observation window (2011–2012) for the post period (2013–2014).
<i>Repository</i> $i, \neq s, (t-4, t-1)$	The number of firm $i$ 's enforcement records in states other than state $s$ from 2009 to 2012 that are posted in the centralized repository at its launch in 2012. In doing so, we assume a four-year observation window (2009–2012) for the post period (2013–2014).
<i>Records Indicator</i> $i, \neq s, (t-3, t-1)$	= 1 if firm $i$ has at least one enforcement record in a state other than state $s$ in the three-year observation window before period $t$ , 0 otherwise.
<i>Records Log</i> $i, \neq s, (t-3, t-1)$	The natural logarithm of $(1 + \text{Records } i, \neq s, (t-3, t-1))$ .
<i>Records</i> $i, \neq s, (t-2, t-1)$	The number of firm $i$ 's enforcement records in states other than state $s$ in the two-year observation window before period $t$ .
<i>Records</i> $i, \neq s, (t-4, t-1)$	The number of firm $i$ 's enforcement records in states other than state $s$ in the four-year observation window before period $t$ .
<i>Records Indicator</i> $i, s, (t-3, t-1)$	= 1 if firm $i$ has at least one enforcement record in state $s$ in the three-year observation window before period $t$ , 0 otherwise.
<i>Records Log</i> $i, s, (t-3, t-1)$	The natural logarithm of $(1 + \text{Records } i, s, (t-3, t-1))$ .
<i>Records</i> $i, s, (t-2, t-1)$	The number of firm $i$ 's enforcement records in state $s$ in the two-year observation window before period $t$ .
<i>Records</i> $i, s, (t-4, t-1)$	The number of firm $i$ 's enforcement records in state $s$ in the four-year observation window before period $t$ .

**Table A2. Robustness Tests**

	Using the company-state -year panel	Alternative Specifications		Alternative Observation Windows	
	(1)	Indicators (2)	Log Form (3)	2-year (4)	4-year (5)
<i>DV = Enforcement</i>					
<b><i>Repository</i> <math>i, \neq s, (t-3, t-1)</math></b>	<b>0.013*** (4.87)</b>				
<b><i>Repository Indicator</i> <math>i, \neq s, (t-3, t-1)</math></b>		<b>0.036*** (5.99)</b>			
<b><i>Repository Log</i> <math>i, \neq s, (t-3, t-1)</math></b>			<b>0.038*** (4.98)</b>		
<b><i>Repository</i> <math>i, \neq s, (t-2, t-1)</math></b>				<b>0.030*** (4.78)</b>	
<b><i>Repository</i> <math>i, \neq s, (t-4, t-1)</math></b>					<b>0.012*** (3.02)</b>
<i>Records</i> $i, \neq s, (t-3, t-1)$	0.002 (1.52)				
<i>Records Indicator</i> $i, \neq s, (t-3, t-1)$		0.005 (1.29)			
<i>Records Log</i> $i, \neq s, (t-3, t-1)$			0.007 (1.54)		
<i>Records</i> $i, \neq s, (t-2, t-1)$				0.000 (0.12)	
<i>Records</i> $i, \neq s, (t-4, t-1)$					0.004** (2.36)
<i>Records</i> $i, s, (t-3, t-1)$	-0.214*** (-28.94)				
<i>Records Indicator</i> $i, s, (t-3, t-1)$		-0.411*** (-29.36)			
<i>Records Log</i> $i, s, (t-3, t-1)$			-0.587*** (-29.85)		
<i>Records</i> $i, s, (t-2, t-1)$				-0.306*** (-19.08)	
<i>Records</i> $i, s, (t-4, t-1)$					-0.491*** (-29.87)
<i>Log Population</i> $s, t$	0.134 (1.47)	0.407** (2.02)	0.400** (1.99)	0.858*** (3.66)	0.071 (0.36)
<i>Log Income</i> $s, t$	0.108 (1.33)	-0.124 (-0.64)	-0.125 (-0.65)	-0.183 (-0.89)	-0.083 (-0.43)
<i>Education</i> $s, t$	-0.785*** (-3.67)	-5.124*** (-8.23)	-5.137*** (-8.23)	-6.240*** (-9.12)	-3.706*** (-5.99)
<i>Minority</i> $s, t$	-1.095*** (-3.80)	-5.837*** (-9.29)	-5.798*** (-9.32)	-7.210*** (-10.25)	-3.878*** (-6.52)
<i>Log Gov Expenditure</i> $s, t$	0.090*** (2.63)	0.330*** (4.43)	0.333*** (4.46)	0.258*** (3.27)	0.387*** (5.31)
<i>Log Gov Employees</i> $s, t$	0.089*** (3.32)	0.091* (1.75)	0.089* (1.71)	0.130** (2.23)	0.069 (1.40)
Company $\times$ State FEs	Y	Y	Y	Y	Y



Period FEs	Y	Y	Y	Y	Y
Observations	34,432	17,806	17,806	17,806	17,806
Adj. $R^2$	0.15	0.40	0.40	0.26	0.43

This table presents the robustness tests of estimating the centralized repository's effect on subsequent enforcement actions. In column (1), we report the results without collapsing two years into one period (i.e., the unit of observation is a company-state-year). In columns (2) and (3), we report the results using two alternative specifications of *Repository*. In columns (4) and (5), we report the results using two alternative observation windows. See Appendix A for variable definitions. Standard errors are calculated by clustering observations by company.  $t$ -statistics are reported in parentheses below the coefficient estimates. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels.