Do Information Processing Costs Matter to Regulators?
Evidence from U.S. Mortgage Companies' Supervision

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Abstract

We examine the impact of reducing information processing costs on U.S. state regulators who supervise mortgage companies. State regulators traditionally disclosed enforcement actions only on their individual websites. A centralized repository, introduced in 2012, assembled enforcement records across states in one place, reducing a regulator's cost to learn about enforcements in other states. Using a difference-in-differences design, 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. This suggests that reducing information processing costs helps state regulators identify companies engaging in misconduct. This effect is stronger for records from state websites where information is harder to process and for state regulators with more limited resources. Last, we observe that sanctioned lenders reduce the credit supply in other states after their enforcement records in one state are posted on the centralized repository.

JEL Classifications: G21; G18; K2; M4

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1. Introduction

Regulators play a central role in maintaining market stability and protecting consumer welfare. However, anecdotal evidence suggests that many of them struggle to accomplish their regulatory missions due to resource constraints (e.g., Coursen 2021; Yoder 2018). Specifically, these constraints may hinder regulators from gathering 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). Although it is conceivable that reducing information processing costs could impact regulators' effectiveness, there has been little, if any, empirical evidence on the subject to date. This study aims to fill this gap by examining the consequences of a regulatory innovation in the context of mortgage companies.

Over the past decade, mortgage companies such as Rocket Mortgage have experienced tremendous growth. As of 2020, they accounted for over 68% of residential mortgage originations in the U.S. (McCaffrey 2021). These companies are generally considered riskier than banks because they rely heavily on short-term credit lines (instead of deposits) for funding and serve less creditworthy borrowers. Academics and media sources have repeatedly called for more regulatory oversight of these companies (e.g., Kim et al. 2018; Light 2020). However, unlike banks, which are heavily regulated by federal agencies (e.g., the Federal Reserve, the Office of the Comptroller of the Currency), mortgage companies are primarily regulated by state regulators, which often have fewer resources and have been criticized for their lenient supervision (e.g., Agarwal et al. 2014). Michelle Bowman, the Federal Reserve governor, emphasized this concern in 2020, stating that "the oversight and regulatory infrastructure for mortgage companies is much less well developed than for banks" (Bowman 2020).

The setting we exploit is the introduction of a centralized enforcement action repository in 2012, which for the first time gave state regulators a central location in which to share records about enforcement actions against mortgage companies. Before the repository's establishment, state regulators only disclosed these records on their individual websites. The centralized repository substantially reduces regulators' information processing costs.[1] First, the large number of U.S. states makes it costly for an individual regulator to monitor enforcement records across states. By gathering enforcement records in one place and automatically notifying regulators of updates, the repository enhances the regulator's awareness of regulatory activity in other states (i.e., reducing awareness cost). 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 (i.e., reducing acquisition cost).

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 operates. As state regulators often face resource constraints in 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' costs of processing enforcement records from other states, the regulators will be more likely to integrate those records into their

[1] Blankespoor et al. (2020, pp. 2-6) define information processing costs as consisting of three components: 1) awareness cost, 2) acquisition cost, and 3) integration cost.

supervisory purview, resulting in a higher probability of subsequent enforcement actions against the same companies in the regulators' own states.

The 2012 introduction of the centralized repository enables us to conduct a difference-in-differences (DID) design for two reasons. First, state regulators are allowed, but not required, to post all their enforcement actions on the repository. Consequently, many enforcement records were still *not* posted on the repository even after its launch in 2012, thus serving a natural control group for the records posted. Second, although state regulators were only expected to post new enforcement records in the repository going forward (NMLS 2011), many of them also uploaded their old (pre-2012) enforcement records when the repository launched. Therefore, these pre-existing records together experience a one-time reduction in information processing costs in 2012. Our empirical strategy compares the enforcement records posted on the centralized repository with those solely disclosed on state websites around the repository introduction and examines whether they lead to a differential probability of subsequent enforcement actions in other states.

Using a hand-collected sample of enforcement records against mortgage companies from all state websites between 2007 and 2014, we find that when an enforcement record from one state is posted to the centralized repository, the likelihood of enforcement against the same company in other states during the subsequent two years increases significantly, relative to that of the enforcement records only disclosed on state websites. The 1.1% increase in enforcement likelihood corresponds to a 42% increase relative to the unconditional mean of enforcements. This finding suggests that the repository facilitates regulators' access to other states' enforcement records, which subsequently influences the regulators' supervisory decisions.

Although using the enforcement records not posted on the centralized repository as the control group helps mitigate the concern about confounding events, a regulator's decision to post

a record on the repository might not be random. Specifically, one could argue that the records posted on the repository would have drawn other state regulators' attention even if they had not been posted, in which case our main results could be explained by the records posted themselves instead of the repository. We conduct three tests to alleviate this concern. First, using multiple proxies for enforcement severity, we find that severe enforcement records—the records that regulators presumably pay more attention to—are no more likely than other records to be posted in the centralized repository. Second, in a falsification test, we show that both the records posted on the repository and those not posted exhibit similar trends in the likelihood of subsequent enforcement actions in other states prior to the repository's launch. Third, we directly asked state regulators why they posted some, but not all, enforcement actions in the repository at its launch. They primarily attributed this decision to lack of staff and stated that the choice of which actions to post was not strategically planned, which corroborates our empirical evidence.

Another potential concern is that our findings may be attributable to the Consumer Financial Protection Bureau (CFPB), who starts to cooperate with state regulators around the same period. To address this concern, we control for CFPB complaints in all regression models and perform robustness tests where we exclude companies with a CFPB complaint or enforcement action. Moreover, we show that CFPB complaints are not associated with state regulators' decision to post an enforcement record on the repository and that our main findings are similar between companies receiving more CFPB attention and those receiving less CFPB attention, further alleviating concerns related to the CFPB.

To further support that our results are driven by the centralized repository—a shock that reduces regulators' information processing costs—as opposed to enforcement records posted or confounding events, we exploit two determinants of 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 measure the variation in information processing costs arising from the information side (i.e., the first determinant), 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. 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.[2] 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.

To measure the variation in information processing costs arising from the regulator side (i.e., the second determinant), we start with the premise that regulators' ability to process information depends on their resources. Regulators with fewer 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

[2] 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.

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 interpretation of our results is that after the repository makes it easier for regulators to acquire enforcement records, many regulators "free-ride" on other states' findings instead of investigating companies on their own. To evaluate whether this is the case, we identify all companies that have an enforcement record posted in the repository and subsequently receive another enforcement action in a different state. The idea is that if state regulators solely rely on records in the repository and not their own 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. We find that 86% 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.[3]

In addition to examining mortgage companies with a prior enforcement record in other states (i.e., intensive-margin companies), we examine whether the repository enables state regulators to expand enforcements against those with *no* prior enforcement records in other states (i.e., extensive-margin companies). We find that it does not; in fact, it leads states to sanction fewer, albeit insignificant, extensive-margin firms after the repository's launch. This is likely because regulators use the extra resources made available by the repository to acquire more information rather than to perform other tasks such as supervising extensive-margin companies. The additional

[3] 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 and go through a lengthy process to prove that the misconduct exists in its own state.

enforcement records acquired by regulators from the repository allow them to allocate attention among mortgage companies more effectively: they shift more attention to intensive-margin companies and less to extensive-margin companies, as the former presumably has a higher likelihood of committing misconduct in the current state.

Last, we examine the real effects of reducing regulators' information processing costs on the regulated entities—mortgage lenders.[4] The rapid market expansion of mortgage companies in the post-2008 crisis period is primarily attributed to the lax regulatory environment surrounding them (Ackerman 2019; Buchak et al. 2018). This implies that these companies may reduce their credit supply as regulatory scrutiny heightens. Because our findings above 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 use a mortgage company's approval/denial decisions on mortgage applications to isolate its credit supply from borrower demand. 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, suggesting that mortgage lenders reduce credit supply when they face potentially higher scrutiny. Because the control group is the sanctioned lenders whose enforcement records are not posted in the repository, the reduction in credit supply is not driven by the occurrence of enforcement actions but instead by the posting in the repository.

The change in credit supply has an ambiguous effect on social welfare: while an inadequate credit supply impedes economic growth, an excessive one can pose a risk to financial stability. To evaluate its effect in our setting, we examine whether the reduction in credit supply is more

[4] Mortgage companies can be lenders, brokers, or servicers. For this test, we focus on lenders because we do not have data on brokers' and servicers' activities.

pronounced for less risky or riskier loans. We find that lenders reduce credit supply more for loans exhibiting higher risk-taking (i.e., a higher loan-to-income ratio). These findings lean toward the notion that a reduction in credit supply may improve welfare by constraining excessive risk-taking.

Our paper contributes to three strands of literature. First, we extend the growing body of research on information processing costs. Extant studies have focused on the impact of these costs on investors and firms (e.g., Blankespoor 2019; Blankespoor et al. 2019; Christensen et al. 2017; Cuny et al. 2021), however, there is little, if any, evidence of how such costs affect regulators. As regulators have goals, powers, and information access that are distinct from those of regulated firms or investors, whether information processing costs would impact them similarly to those groups is unclear. To our knowledge, this paper is the first to fill this gap. By doing so, we answer Blankespoor et al. (2020)'s call for research on information processing costs in broader contexts.

Second, our study contributes to the literature on regulators' resource constraints. Prior studies primarily focus on the Securities and Exchange Commission (SEC) and the issues arising from its resource constraints (e.g., Kedia and Rajgopal 2011; Gillette et al. 2020; Maffett et al. 2021; Bonsall et al. 2024; Gunny and Hermis 2020; Ege et al. 2020). Our study stands apart in that it focuses on a possible solution to regulators' resource constraints (i.e., the repository) and on a different set of regulators (i.e., state regulators).

Third, our study contributes to the literature on shadow banks—a system that includes not only banks' off-balance-sheet subsidiaries but also independent mortgage companies (Demyanyk and Loutskina 2016). Although mortgage companies have proliferated in the post-crisis period, they have "received far less scholarly attention" than banks' off-balance-sheet subsidiaries (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 state that "[h]ow to carry out more effective regulatory oversight of mortgage nonbanks remains an open question" (p. 163).

Our study also has several policy implications. First, a stated goal of the centralized repository upon its initiation was to facilitate information sharing between state regulators. Our empirical evidence suggests that this goal has been largely achieved, which should be of interest to state regulators. Second, almost all regulators face resource constraints. A 2018 workforce report by the U.S. Office of Personnel Management reveals that 83% of federal agencies' mission accomplishment is adversely affected by the agencies' capacity shortcomings (U.S. OPM 2018). To alleviate their resource constraints, some regulators are considering the implementation of data repositories akin to the one in our setting. For instance, the SEC is in the process of developing the Consolidated Audit Trail (CAT), a data repository aimed at centralizing securities trades and orders across exchanges and markets (Clayton 2020; Peirce 2022). Therefore, our findings in the mortgage companies market could have broader implications for regulators beyond those examined in our study.

2. Institutional Background

2.1. Regulatory Environment of Mortgage Companies

The residential mortgage market is the largest consumer loan market in the United States. As of 2020, the total mortgages outstanding amount to $13.4 trillion. In comparison, the total corporate bonds outstanding is $6.5 trillion (Financial Accounts of the United States). Although there has been substantially more regulation since the 2007–2008 financial crisis (e.g., Dodd-Frank Act), most of the post-crisis regulatory changes (e.g., stress tests, higher capital requirements, new liquidity coverage ratio requirements) have been imposed on banks but not mortgage companies

(Gete and Reher 2021; Kim et al. 2022a).[5] As a result, U.S. banks' share in the mortgage markets has contracted significantly over the past decade, while mortgage companies have experienced tremendous growth, raising their market share from less than 30% in 2009 to over 68% in 2020 (Seru 2019; McCaffrey 2021).

In contrast with banks, mortgage companies' primary regulators are at the state level (CSBS 2019b, p1). State regulators share the same regulatory missions as federal bank regulators, namely 1) safety and soundness and 2) consumer protection. However, executing these missions for mortgage companies is more challenging. Regarding safety and soundness, 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). Bank regulators, academics, and the media have repeatedly raised concerns about the risks mortgage companies pose to the U.S. financial system (Ackerman 2019; Bowman 2020; Kim et al. 2018; Light 2020). Regarding consumer protection, 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.[6] State regulators primarily rely on examinations to supervise mortgage companies. However, due to resource constraints, they may not routinely examine all regulated mortgage companies. Instead, they may "rely on company report data, complaints, information collected

[5] 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.
[6] Multistate-licensed mortgage companies accounted for over 80% of total originations by mortgage companies in 2019 (CSBS 2019b).

from other regulators, and public records" to determine which firms to investigate (CSBS 2019a, p. 31). This approach allows regulators to "prioritize their time and resources on the companies believed to pose the highest risk" (CSBS 2019a, p. 31). In addition, state regulators may not fully review a company's every aspect (e.g., loan portfolio, individual originator licensing, and financial condition), so examinations can vary substantially in length and scope across companies. If the state regulator identifies indications for a potential violation during an examination, the regulator will initiate an investigation, which is a "fact-finding endeavor that may or may not result in evidence that leads to a finding of violation" (CSBS 2019a, p.34). Once regulators gather sufficient evidence to demonstrate the violation, they will issue an enforcement action.

A state regulator only has jurisdiction within its own state and scrutinizes a mortgage company's activities solely in that state. For example, the Florida regulator might issue an enforcement action against a mortgage company because the company failed to provide required disclosures to certain borrowers in Florida. This enforcement record would only reflect misconduct in Florida. Therefore, even if the company also failed to provide required disclosures to borrowers in Georgia or Texas, this would not be covered in Florida's enforcement record.

Last, state regulators are not evaluated by any federal agency or by peers based on the number of enforcement actions. A regulator we spoke with stressed 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."

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).[7] The State Regulatory Registry recommended, but did not require, that state regulators post their enforcement actions in the repository. The regulators that do so all fill out the same form, so the enforcement records are posted in a standardized format. The repository was made available to state regulators in October 2011, and, by the end of 2012, 35 of the 51 state regulators had posted at least some of their enforcement records there (CSBS 2012). The state regulators who did not immediately post records in the repository could still access the repository to view enforcement records from other states.

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 an enforcement action is uploaded to the repository.[8] In our discussions, many state regulators confirmed the repository's benefits in their responses to our inquiries. For example, the Florida regulator states that "The ability to rely on other states uploading their regulatory actions [to the repository] provides other states such as Florida with immediate notification of the action thereby affording the state the opportunity to be proactive instead of reactive." Similarly, the Virginia regulator states that "Regulatory actions posted by other state agencies in NMLS [repository] may bring an issue or emerging risk to our attention that otherwise would not have been detected until we performed an examination of the institution."

[7] According to the state regulators we spoke with, mortgage companies are not obligated to self-report their enforcement actions to other states.

[8] As we are not permitted to access the regulator's interface of the repository, our understanding of its functionality comes from multiple meetings with state regulators.

A unique feature of our setting is that all the states' enforcement records were already available to state regulators before the introduction of the repository. The vast majority of state regulators made their records publicly available via their websites; the few states that did not do 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 (e.g., Silvers 2020; Balakrishnan and Ertan 2021). 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, when the CSBS launched the centralized repository in 2012, it did not bundle it with other regulatory changes. As a result, we can use our setting to specifically isolate information processing costs.

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

The centralized repository is intended to make it easier for state regulators to learn about enforcement records in other states, but whether the repository actually affects regulators' behaviors is unclear ex ante. There are several reasons why this is the case. First, state regulators may already use other states' websites or have pre-existing professional relationships that extend to the broader regulatory community, rendering a formal repository unnecessary. Second, regulators may believe they already possess sufficient knowledge of regulated mortgage companies, so they might not utilize the centralized repository. Third, regulators might be reluctant 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 (Gallemore 2022).

Even if the centralized repository does affect regulators, it might not increase the likelihood of subsequent enforcement actions. For example, an enforcement action posted in the repository by one state could prompt a mortgage company to rectify its misbehavior in other states, leading us to expect a lower, rather than higher, probability of enforcement in those states. Ultimately, whether and how the centralized repository affects the supervisory outcomes of mortgage companies remains a subject for empirical investigation.

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 regulators not only posted their new enforcement records in the repository going forward but also posted their *pre-existing* records upon its launch. 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 or only posted a portion of their past records, many enforcement actions that occurred in the same periods as repository-posted records continued to be disclosed only on state websites after 2012. In sum, regulators incurred high costs to acquire *any* enforcement records from other states before 2012; after 2012, they continued to incur high costs for records not posted in the repository but low costs for the records posted in the repository.

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 when most state regulators started

posting 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. For each period, we assume a three-year preceding window during which regulators may learn about enforcement records from other states.[9] 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.

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 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

[9] 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 A1 of the Online Appendix).

reflects the *incremental* enforcement probability in the post-period when firms' enforcement records are available in the centralized repository.

Our identification strategy can be illustrated with a simple example. In 2011, Rocket Mortgage and Envoy Mortgage each received an enforcement action, but Rocket was sanctioned in Texas and Envoy was sanctioned in Florida. Both state regulators disclosed the actions on their websites in 2011. Both firms also originate mortgages in California. At the centralized repository's launch in 2012, the Texas regulator posted its enforcement against Rocket in the repository, but the Florida regulator did not post its enforcement against Envoy. 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 Rocket'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 Rocket than Envoy, which in turn should lead to a higher probability of a subsequent enforcement action against Rocket in California.[10]

There may be two concerns with this strategy. First, the California regulator may have routinely visited Texas' enforcement website, but not Florida's, prior to the repository's launch. Therefore, a higher probability of enforcement actions against Rocket than Envoy in California might be driven by pre-existing cross-state learning instead of the repository. To address this concern, we assume an observation window for the pre-period, similar to how we do for the post-period, and include the firms that are sanctioned during this window in our sample. This is because

[10] The firm-level state examination data are not publicly available, so we cannot use them as the outcome variable. According to the CSBS (2019a, p.47), even the state-level number of examinations against mortgage companies is unavailable. Based on interactions with multiple state regulators, we understand that state regulators consider their examination selection process and related data to be highly confidential. As a result, we acknowledge that the variable may not perfectly capture regulators' activities.

the likelihood of these companies' subsequent enforcement actions in other states during the pre-period captures the extent to which each state regulator learns about other states' records before the repository's launch. Second, the California regulator's higher scrutiny of Rocket may stem from fundamental differences between the two firms. Therefore, directly comparing the enforcement probability of the two firms in the post-period may not accurately reflect the true effect of the centralized repository. To address this concern, we include these firms in the pre-period and use firm × state fixed effects to control for pre-existing differences in the California regulator's attention to Rocket and Envoy.

We estimate the following OLS regressions using a company-state-year panel dataset:

$$\begin{aligned} Enforcement_{i,s,t} = {} & \beta_1 Repository_{i,\neq s,w} + \beta_2 Records_{i,\neq s,w} + \beta_3 Records_{i,s,w} + \beta_4 Log\ Complaints_{i,w} \\ & + \beta_5 Population_{s,t} + \beta_6 Log\ Income_{s,t} + \beta_7 Education_{s,t} + \beta_8 Minority_{s,t} \\ & + \beta_9 Log\ Gov\ Expenditure_{s,t} + \beta_{10} Log\ Gov\ Employees_{s,t} \\ & + \text{Company} \times \text{State FEs} + \text{Year FEs} + \varepsilon_{i,s,t} \end{aligned} \quad (1)$$

$$\begin{aligned} Enforcement_{i,s,t} = {} & \beta_1 Repository_{i,\neq s,w} + \beta_2 Records_{i,\neq s,w} + \beta_3 Records_{i,s,w} + \beta_4 Log\ Complaints_{i,w} \\ & + \text{Company} \times \text{State FEs} + \text{State} \times \text{Year FEs} + \varepsilon_{i,s,t} \end{aligned} \quad (2)$$

where i indexes the mortgage company, s indexes the state, t indexes the year, and w indexes the observation window before a (either pre- or post) period. Because we control for a large number of fixed effects, we run the OLS model instead of the logit model, following deHaan (2021) and Dou et al. (2018). The unit of observation is a firm-state-year. $Enforcement_{i,s,t}$ is an indicator variable that equals one if firm i is sanctioned in state s in year t. We use enforcement actions to infer regulators' private supervisory activities, consistent with prior literature on financial regulators (Kedia and Rajgopal 2011; Silvers 2020; Wheeler 2019). $Repository_{i,\neq s,w}$ is the number of firm i's enforcement records in states other than state s from the observation window of the post-period (i.e., 2010 to 2012) 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. $Records_{i, \neq s, w}$ is the number of firm *i*'s enforcement records in states other than state *s* from the observation window *w*, regardless of whether they are posted only on state websites or also on the repository. The observation window *w* for the pre-period (post-period) is 2007 to 2009 (2010 to 2012). $Records_{i, s, w}$ denotes the number of firm *i*'s enforcement records in state *s* from the observation window *w*, irrespective of where the records are disclosed.

Even without the centralized repository, state regulators may acquire enforcement records from other states for use in their own supervision. $Records_{i, \neq s, w}$ 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 β_1 to be positive. Unlike in a traditional DID 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 DID design with a continuous treatment used in DeFond and Lennox (2017) and Stuber and Hogan (2021).

Our design also resembles Raghunandan and Ruchti (2022) in how it sets up explanatory variables and fixed effects.[11] Specifically, both their model and ours include prior enforcement

[11] Raghunandan and Ruchti (2022) examine the pattern of firms' workplace safety violations under the supervision of a federal agency—the Occupational Safety and Health Administration (OSHA). They show that firms sanctioned in

actions in other states, prior enforcement actions in the same state, and company × state fixed 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 include an additional explanatory variable, *Repository*, our variable of interest.

We control for the number of consumer complaints filed against a company to the CFPB, as state regulators may learn about these complaints from the CFPB. Similar to *Repository*, *Log Complaints* is calculated based on the observation window before each (pre- or post-) period. When we do not control for state-year fixed effects in Model (1), we control for state demographic and governmental characteristics, including state population (*Log Population*), average resident income (*Log Income*), the percentage of residents with a bachelor's degree or higher (*Education*), the minority percentage (*Minority*), total governmental expenditure (*Log Gov Expenditure*), and the number of state government employees (*Log Gov Employees*). In Model (2), where we control for state-year fixed effects, these variables are absorbed by these fixed effects and thus omitted. Since mortgage companies are private firms whose financial information is not publicly available, we do not control for their financial characteristics. We cluster standard errors by company.

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

$$\begin{aligned} Approval_{j,s,t} = {} & \beta_1 Repository_{i,\neq s,w} + \beta_2 Record_{i,\neq s,w} + \beta_3 Record_{i,s,w} + \beta_4 Log\ Complaints_{i,w} \\ & + \beta_5 Log\ Borrower\ Income_{j,s,t} + \beta_6 Log\ Loan\ Amount_{j,s,t} + \beta_7 Borrower\ Gender_{j,s,t} \\ & + \text{Company} \times \text{State FEs} + \text{State} \times \text{Application Year FEs} \\ & + \text{Application Year} \times \text{Loan Characteristics FEs} + \varepsilon_{j,s,t} \end{aligned} \quad (3)$$

one state subsequently violate less in that state but more in other states. Their setting differs from ours in that OSHA already has a centralized repository *before* their sample period starts. Its repository contains information about the sanctioned firm, the state, the enforcement date, and the reason for the sanction (p. 12). Therefore, regulators in their setting can easily learn about enforcement actions in other states via this repository throughout the sample period. In contrast, in our setting, state regulators cannot easily learn about enforcements in other states in the pre-period. By exploiting the introduction of the centralized repository as a shock, we can examine the real effect of reducing information processing costs on regulators.

This equation is similar to Equation (2) except that the unit of observation is a loan application. *Approval* equals 1 if the bank approves the application and 0 if the bank denies the application. $Repository_{i,\neq s,w}$, $Record_{i,\neq s,w}$, and $Record_{i,s,w}$ are defined the same way as in Equation (1) and (2) and are merged to lender *i*'s loan applications from state *s* in year *t*. 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 errors 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. We obtained the enforcement records for Iowa and Kansas through Freedom of Information Act requests. We did not obtain enforcement records from Delaware or North Dakota, because these states have laws prohibiting the records' public disclosure.[12] 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).

[12] These two states confirmed to us that their enforcement actions are available to other state regulators upon request.

We manually read through each record to exclude enforcement actions imposed on non-mortgage companies such as payday lenders and money transmitters.

In our setting, enforcement records serve as both supervisory inputs and outputs. First, enforcement records are used as an *input* of state regulators' supervisory information set. 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 qualify for inclusion in our sample, a mortgage company must be sanctioned by a state regulator at least once from 2007 to 2012. Second, enforcement actions also serve as 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. To address the concern that multi-state enforcement actions might mechanically lead to a correlation of enforcements across states, we exclude three such cases between 2010-2014 from the sample. These criteria yield 4,611 enforcement actions against 3,123 mortgage companies between 2007 and 2014.

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.

Based on enforcement records, we construct a company-state-year panel dataset to examine the likelihood of the company receiving another enforcement action from other states subsequent to the posting of its enforcement records. Because we include company-state fixed effects, we exclude company-state pairs that have observations in only one period, either pre- or post, but not both. Our final sample includes 34,432 company-state-year observations, mapped to 1,625 enforcement records associated with 1,102 unique mortgage companies from 2007 to 2012.

Last, we merge these 1,625 records with the centralized repository to determine which of them are also posted in the repository.[13] These treatment records include 362 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 1,263 records are used as control records.

We collect the consumer complaint data from the CFPB, which began to collect complaints in December 2011. To construct state-level control variables, we collect data from the U.S. Census Bureau's American Community Survey on each state's total population, average income, education attainment, and minority percentage. We obtain the number of state government employees from the Census's Annual Survey of Public Employment and Payroll. We also source each state government's total expenditures from their financial statements collected by Kim et al. (2022b).

5. Empirical Results

[13] We collect the records posted on the repository from its public interface – "Consumer Access", similarly to Flannery et al. (2023). However, because the repository allows regulators to share records only among themselves, a concern is that we may incorrectly classify the records that are shared only among regulators as not being posted on the repository. To evaluate this potential measurement error, we contacted the NMLS and they confirmed that all enforcement records that have been disclosed on state websites and uploaded to the repository are made available on the repository's public interface. Furthermore, all 19 state regulators who responded to our inquiry confirmed that they have not posted any enforcement actions in the repository that are restricted to regulators only.

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

We report the descriptive statistics of the company-state-year sample in Table 1. We find that 2.6% of the company-state-years in our sample receive an enforcement action. The mean of $Records_{i, \neq s, w}$ is 1.07, indicating that, on average, a company-state has 1.07 enforcement records in other states in the observation window before each period. The mean of $Repository_{i, \neq s, w}$ is 0.22, indicating that, for an average company-state, 0.22 enforcement records from other states are posted in the centralized repository at its launch.

To examine the centralized repository's impact on subsequent enforcement actions, we estimate Equations (1) and (2) and report the results in Table 2. We include state × company fixed effects in both columns. Column (1) reports the results with year fixed effects, while Column (2) reports the results with (more granular) state × year fixed effects, which absorb state controls such as *Log Population*. We find that the coefficient on $Repository_{i, \neq s, w}$, the main variable of interest, is positive and statistically significant in both columns. The coefficient magnitude of 0.011 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 1.1%, corresponding to a 42% increase of the unconditional mean of enforcement actions (i.e., 2.6%).

Regarding the control variables, we find that the coefficient on $Records_{i, \neq s, w}$ is significantly positive in Column (1) but statistically insignificant in Column (2), suggesting some evidence that state regulators may sometimes acquire enforcement records from other state websites. The coefficient on $Records_{i, s, w}$ is significantly negative in both columns, suggesting that a firm is significantly less likely to be sanctioned again in a state where it has been previously

sanctioned.[14] This pattern could be explained by mortgage companies correcting their misbehavior in accordance with the requirements set forth by the local regulator in the enforcement action. Additionally, we find that CFPB complaints are positively associated with subsequent state enforcement actions, suggesting that state regulators learn from CFPB complaints. Because our results hold when we control for CFPB complaints, it means that the repository provides information that goes beyond the information in the CFPB complaints.

We conduct a number of robustness tests. First, we assume a three-year observation window for three record-based variables ($Repository_{i, \neq s, w}$, $Records_{i, \neq s, w}$, and $Records_{i, s, w}$) in the main test. In this robustness test, we adopt two years and four years as alternative observation windows and reconstruct these variables, respectively. Table A1 of the Online Appendix reports the results. We find that our inferences remain unchanged.

Second, we construct two alternative specifications for the record-based variables. Specifically, we convert all the continuous record-based variables into binary variables. Using 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. We also convert the continuous record-based variables into their log forms, which better captures the marginal diminishing effect of posting one more record in the centralized repository. Table A2 of the Online Appendix reports these results. We find that our inferences remain the same using these specifications.

Third, a possible concern is that a few enforcement records posted on the repository draw media attention, so state regulators may acquire the information from the media instead of from

[14] This variable is not equivalent to the lagged *Enforcement* because the former is a count variable of firm *i*'s enforcement actions in state *s* during the three-year observation window before each period, while the latter is a binary variable indicating whether firm *i* was sanctioned in state *s* in year *t*-1. Our results are robust to excluding this variable (see Table A4 of the online appendix).

the repository itself. To address this concern, we identify all enforcement records with any media coverage. Specifically, for each enforcement action from 2007 to 2012, we follow prior literature (Miller 2006; Gao et al. 2020; Nagar et al. 2019) and search for news articles related to the sanctioned firms in the Factiva and Access News databases over a (-3 days, +180 days) window around the enforcement action. We then manually read through each article to determine whether it covered the state regulator's enforcement action. We find that only 4.6% of enforcement actions were covered by newspapers. Table A3 of the Online Appendix reports the results after removing all firms whose enforcement actions were covered by a newspaper. We find that our inferences remain unchanged.

5.2. Are the Findings Driven by the Records Posted on the Repository or the Repository Itself?

Although we alleviate the concern about confounding events by using the enforcement records not posted on the centralized repository as the control group, individual state regulators' choice of which records to post might not be random. Therefore, an alternative explanation for our main finding is that the records posted on the repository would have drawn other state regulators' attention even if they had not been posted there. In other words, our main finding may be driven by the records posted on the repository rather than the repository itself. To address this concern, we perform three tests.

5.2.1. What Record Is More Likely to Be Posted on the Repository?

Conceptually, the enforcement actions that naturally draw other state regulators' attention are likely to be the severe ones, so we first examine whether severe enforcement records are more likely to be posted on the centralized repository. We employ four variables to measure the severity of an enforcement action. The first measure is the number of reasons for the action (i.e., *Number of Reasons*). An enforcement action addressing multiple types of misconduct is presumably more

severe. For example, if 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. (We find that common reasons 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 (i.e., *Doc Length*). 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 (i.e., *Fines*). An enforcement action with a higher penalty is presumably more severe. Our fourth severity measure is an indicator variable for whether the mortgage company's state license is suspended or revoked (i.e., *Revoke Licenses*). 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.[15] The dependent variable, *Post on Repository*, is an indicator variable that equals one if the record is posted in the repository, and zero otherwise. We run a logit model and report the regression results in Panel A

[15] There are 312 unique companies whose enforcements actions are posted in the repository (e.g., GuardHill Financial Corp, Wolfe Financial), 480 companies whose enforcements actions are not posted in the repository (e.g., Village Capital & Investment LLC, Reliance First Capital, LLC), and 72 companies involved in both (e.g., Draper and Kramer Mortgage Corp, Tower Mortgage Corporation). 64.4% (80.5%) of the records posted on the repository and 70.2% (59.9%) of the records not posted on the repository involve law violations related to safety and soundness (consumer protection). The statistics remain similar when using the entire enforcement sample from 2010 to 2014: 70.2% of the enforcement actions in our sample involve law violations related to safety and soundness, and 67.8% involve consumer protection (the sum of two percentages exceeds 100% because an enforcement action may pertain to both types of violations).

of Table 3. We do not find significant associations between *Post on Repository* and three severity measures (i.e., *Number of Reasons*, *Doc Length*, *Fines*). Although *Post on Repository* is weakly associated with *Revoke Licenses*, it means that less severe records (i.e., enforcement records without revoking licenses) are more likely to be posted on the repository. Overall, we do not find evidence suggesting that records posted in the centralized repository are not more severe than those not posted.[16]

In this test, we also examine whether CFPB complaints affect state regulators' decisions to post an enforcement record on the repository. We find that CFPB complaints are not associated with whether the complained-about company's enforcement record is posted on the repository, corroborating the notion that the CFPB information does not correlate with the records in the repository.

5.2.2. A Falsification Test Using the Pre-period

An important assumption underlying the DID 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 the treatment and control groups exhibit parallel trends in the outcome variable in the pre-period (Baik et al., 2024; Jiang et al. 2019; Kielty et al. 2023). Because we define the treatment and the control groups at the record level, we examine whether, during the pre-period, the treatment records (i.e., those posted on the centralized repository) exhibit a similar trend in the likelihood of subsequent enforcement actions in other states as the control records.

Specifically, we pretend that the centralized repository was launched in 2010 rather than 2012, so we set 2011 (the year before the repository's actual launch year) as the pseudo post-period

[16] Relatedly, we do not find that enforcement actions covered by media are more likely to be posted on the repository.

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).[17] We re-estimate Equation (2) using the pseudo sample periods and report the results in Panel B of Table 3. We find that the coefficient on *Repository Pseudo* $_{i, \neq s, w}$ is statistically insignificant, suggesting that the records posted in the centralized repository do not exhibit an increasing trend in the likelihood of subsequent enforcement actions in the pre-period relative to the records not posted. These results support the parallel trends assumption underlying the DID design and reinforce the inference 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.

5.2.3. Direct Inquiry of State Regulators.

Last, we directly asked state regulators about why they posted some, but not all, of their past enforcement actions in the repository after it launched in 2012. Their responses suggest that the primary reason was a lack of staff. They also recall that the choice of which actions to post was not strategically planned. For example, one state regulator explained that they had to find the enforcement record's physical file, scan it, and match it to the sanctioned company's ID in the NMLS system. The labor-intensive nature of the work, together with limited staffing, made it infeasible to post all of the past enforcement actions. This anecdotal evidence, in conjunction with our empirical findings above, helps alleviate the concern that our main results are driven by the severity of the posted records rather than the centralized repository itself.

[17] Because our sample of enforcement actions collected from state websites starts from 2007 and the falsification test should be conducted strictly in the pre-period (i.e., before the repository's launch in 2012), the available period for this test spans from 2007 to 2011. Therefore, we have to shorten both the pre- and post- periods and their observation windows by one year (compared with the main analyses). Despite being shorter, they are still sufficiently long for us to test whether the treatment and the control records exhibit parallel trends in the likelihood of attracting other states' attention before the repository's launch.

5.3. Are The Findings Driven by The CFPB's Information Sharing?

The CFPB starts to cooperate with state regulators around the same period as the centralized repository's launch. Specifically, in 2011, the CFPB and the CSBS signed a memorandum of understanding (MOU) to establish a foundation for state and federal information sharing. In 2013, the CFPB and CSBS announced a framework to implement the MOU. Therefore, a potential concern is that our findings are attributable to state regulators learning from the CFPB. We believe this is not likely for two reasons. First, the MOU is established between the CFPB and the CSBS—the national association of all state regulators, so if the CFPB shares any record with the CSBS, the record should be equally accessible to all state regulators. In other words, the CFPB's information sharing presumably affects all states. In that case, our DID design helps remove the effect of this information sharing (and other nationwide events). Second, the specification of state × year fixed effects allows us to compare the likelihood of a state regulator sanctioning a firm whose enforcement record is posted on the repository with the likelihood of the same regulator sanctioning a firm whose enforcement record is not posted. By doing so, we isolate the repository effect from other contemporaneous changes experienced by this regulator, including the CFPB's establishment and the CFPB's MOU with state regulators. Nevertheless, we have conducted a large number of tests to further alleviate concerns related to the CFPB's supervision and potential information sharing with state regulators.

A growing body of literature shows that the CFPB consumer complaint database reveals potential perpetrators (Begley and Purnanandam 2021; Dou and Roh 2023; Hayes et al. 2021; Mazur 2023). To alleviate the concern that our results are attributable to CFPB consumer complaints, we first include CFPB consumer complaints as an additional control in all regression analyses. Second, we conduct a robustness test in which we remove all lenders who received a

CFPB complaint during our sample period. Despite losing a significant portion of our sample (36%), we find that our inferences remain unchanged (see Panel A, Column (1) of Table 4). The CFPB may also share its own enforcement records with state regulators. Although it started issuing enforcements against mortgage companies in 2013—a year after the repository's launch—the CFPB might have shared its findings with state regulators before the final orders were issued. Therefore, we conduct a robustness test where we exclude all lenders that receive a CFPB enforcement action at any point during our sample period. We find that our results remain robust to excluding these lenders (see Panel A, Column (2) of Table 4).

In addition, if our findings are due to the CFPB effect, they should be more pronounced for companies receiving greater CFPB attention. To test this, we first model which companies attract more attention from the CFPB. Following Kedia and Rajgopal (2011) and Dou and Roh (2023), we hypothesize that companies with more consumer complaints and those geographically closer to the CFPB's headquarters receive more attention. Specifically, for each mortgage company in our sample, we calculate the number of CFPB complaints and the distance to the CFPB's headquarters (i.e., Washington, D.C.) and run the regression at the firm level. Confirming our predictions, we find that these companies are significantly more likely to be sanctioned by the CFPB (see Panel B of Table 4). Then, following Wheeler (2019), we use the predicted values from this model to measure the CFPB's regulatory attention and partition our sample based on the median firm-level CFPB attention. We find that our results hold in both subgroups, and the difference in coefficients on *Repository* between the two groups is not statistically significant (see Panel C of Table 4). These findings further mitigate the concern that our results may be related to the CFPB's supervision.

Last, following recent literature (e.g., Abdel-Meguid et al. 2021, Call et al. 2022, Dambra et al. 2023), we employ the impact threshold of a confounding variable to evaluate the likelihood that our results are driven by an omitted correlated variable. We find that an unobserved omitted variable would need to have an impact larger than that of any of our control variables and fixed effects to overturn our results. To invalidate our inferences, 61% of our sample would need to be replaced with observations for which there is no effect (Frank et al. 2013). While we cannot completely rule out the possibility of an omitted variable affecting our results, these findings suggest that such a variable would need to be quite large in magnitude to overturn our findings.

5.4. Mechanism

In this section, we provide more evidence that our main findings are driven by the introduction of the centralized repository. Specifically, we exploit two factors that determine regulators' 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.

5.3.1. Variation in Information Processing Costs Arising from the Information Side

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 those against other types of companies (e.g., payday lenders).[18] To separate actions against mortgage companies from actions

[18] Identifying mortgage companies by name may not be straightforward, as approximately 47% of sanctioned firms in our sample do not include "mortgage" in their company name. For example, Village Capital & Investment LLC,

against other entities in these states, a regulator would have to read each enforcement record, increasing its 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 it 1) pools all enforcement actions and does not label entities by 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."[19] Accordingly, we partition our *Repository* variable into *Repository High-cost* and *Repository Low-cost.* 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 5. 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= 7.49, p-value < 0.01), suggesting that the centralized repository's effect on subsequent enforcement actions is indeed 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

GuardHill Financial Corp, and American Advisors Group are all mortgage companies.

[19] 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.

enforcement actions is statistically insignificant. This suggests that state regulators tend to acquire records from state websites with low information processing costs but not from state websites with high information processing costs.

5.3.2. Variation in Information Processing Costs Arising from the Regulator Side

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 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. 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. This means that, 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 (2) for each subsample and report the results of this analysis in Table 6. 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 regulators have a higher examiner-to-company ratio. Consistent with our expectation, the difference between the two variables' coefficients is significant (χ^2 = 8.14, p-value < 0.01), suggesting that the centralized repository indeed holds more value for states with fewer resources.

Because in three states (Alaska, Utah, Vermont), mortgage examiners are also responsible for banks (in addition to mortgage companies), we show that our results are robust both to excluding these three states and to adjusting the number of examiners in these states using the percentage of nonbanks or the percentage of mortgages originated by nonbanks (See Table A5 of the Online Appendix).

In addition, we adopt an alternative proxy for state regulators' resource constraints: instead of scaling the number of mortgage examiners by the number of mortgage companies, we scale it by the number of mortgages originated by mortgage companies in that state. We partition our sample into two subsamples based on the median of the examiner-to-mortgage ratio and report the results in Table A6 of the Online Appendix. The results corroborate the inference that state regulators with fewer resources benefit more from the centralized repository.

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 on other states' findings instead of conducting their own thorough investigations. To evaluate the validity of this concern, we identify enforcement

actions that could be linked and examine how often they cite the same reasons for 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*.[20] In doing so, we link 97 enforcement actions in the post-period with at least one enforcement action in the repository.[21] If state regulators free-ride on the repository records, then 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. Take, for example, a repository record indicating that a firm has overcharged borrowers in one 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 subsequent enforcement action would be the same as in the repository record.

We find that 86% (83/97) 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

[20] If there is more than one record in the repository, we take the union of the reasons disclosed in the records.

[21] The most frequent reasons for the posted enforcement actions include (a) hiring unlicensed loan officers (33.6%), (b) falsifying or omitting documents required by regulators (12.6%), (c) insufficient equity or surety bond (8.4%), and (d) failing to provide required disclosures to borrowers (7.7%). The most frequent new reasons for subsequent enforcement actions include (a) hiring unlicensed loan officers (23.08%), (b) falsifying or omitting documents required by regulators (14.1%), (c) failing to provide required disclosures to borrowers (10.26%), and (d) failing to maintain necessary loan records (6.41%). The new reasons in the subsequent actions appear to overlap with the reasons in the posted records due to the aggregated nature of these statistics. For example, a record addressing a company hiring unlicensed officers (reason A) in one state could prompt a subsequent action in another state, which uncovers a new issue about inadequate disclosures to borrowers (reason B). Conversely, a different company's record highlighting inadequate disclosures to borrowers (reason B) in one state might lead to a subsequent action in another state, uncovering a new issue involving unlicensed officers (reason A). At an aggregated level, the total reasons for the two records posted are reasons A and B, which perfectly overlap with the total new reasons for the two subsequent actions.

misconduct by the company.[22]

5.5. Do Regulators Expand Enforcements against Extensive-Margin Companies?

So far, our analyses have focused on mortgage companies with prior enforcement records in other states (i.e., intensive-margin companies). As the repository reduces regulators' information processing costs and relaxes their resource constraints, a natural question arises: will they increase supervisory efforts over companies with *no* prior enforcement records in other states (i.e., extensive-margin companies)? Because the regulator's effort (e.g., examination length or scope) is inherently unobservable and we can only see detected misconducts, we examine whether enforcement actions against extensive-margin companies increase after the repository's launch, relative to actions against intensive-margin companies.[23]

Specifically, for each state and year, we calculate the ratio of enforcement actions against extensive-margin companies to the total enforcement actions. If extensive-margin companies receive more regulatory attention after the repository's launch, the proportion of enforcement actions against them should increase after 2012. We present the mean percentage of extensive-margin enforcement actions in the pre- and post-periods and plot the year-by-year trend in Figure 2. We find that the percentage of enforcement actions against extensive-margin companies decreases after the repository's launch, though the change is not statistically significant. This finding suggests that regulators do not seem to increase their supervisory efforts on extensive-margin companies after the repository's launch.

A possible explanation for the results is that the extra resources made available by the

[22] Issuing a state enforcement action requires concrete evidence of a company's misconduct in that state. Therefore, even if an enforcement action cites the same reason as a record in the repository, the state regulator must still prove that the misconduct has occurred in its own state.

[23] Given that regulators' efforts are inherently unobservable, we acknowledge that our results are descriptive and exploratory. Therefore, the inference should be interpreted with caution.

repository are used not for supervising extensive-margin companies but for acquiring more information. Conceptually, there are two alternative consequences of reducing information processing costs. First, regulators acquire the same information as before but at a lower cost. As a result, they have extra resources to expend on other tasks, such as supervising extensive-margin companies. Second, regulators acquire more information than before but at the same cost. In this case, they might not free up extra resources for other tasks.

Our findings lend more support for the second scenario, as we find that regulators utilize more enforcement information from other states after the repository's launch. We also show that regulators conduct comprehensive investigations on intensive-margin companies rather than free-riding on the repository's records. This corroborates the notion that regulators do not save resources on intensive-margin companies after the repository's launch.

The additional information from the repository helps regulators more effectively allocate their attention among mortgage companies. This likely explains why regulators shift more of their focus to intensive-margin companies: companies with prior enforcement records in other states presumably have a higher likelihood of committing misconduct in the current state and therefore warrant more regulatory attention.

5.6. 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 hypothesize that when a mortgage company

anticipates increased regulatory scrutiny due to a post in the repository, it may reduce its credit supply in the states where it has not been sanctioned.

To test this hypothesis, we obtain the loan-level application data from the Home Mortgage Disclosure Act (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 with either approval or denial decisions and require loan amounts to exceed $1,000 and the borrower's annual income to exceed $10,000.[24] 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 either the pre-period or post-period but not both. Our final sample consists of 6,743,140 loan applications from 242 unique mortgage companies.

We report the descriptive statistics of the loan application sample in Panel A of Table 7. On average, lenders in our sample approve 78% of loan applications. Borrowers have an average annual income of $95,000 and apply for mortgages of $211,000. Panel B of Table 7 reports the regression results of Equation (3). We find that the coefficient on *Repository* is 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 -2.4% coefficient magnitude implies that for each enforcement record posted in the centralized repository, the loan approval rate of the sanctioned

[24] We do not include applications that are withdrawn by borrowers or closed for incompleteness.

lender in other states declines by 2.4%, corresponding to 3% of the unconditional mean of loan approval. A *t*-value of 2.37 is comparable to values in prior studies based on millions of loan observations (e.g., Ertan et al. 2017; Kang et al. 2021). Regarding control variables, we find that borrowers with higher incomes and smaller loan amounts are more likely to receive loan approvals.

The reduction in credit supply does not necessarily imply a loss in social welfare. While too little credit impedes economic growth, too much credit leads to excessive risk-taking, as we witnessed during the 2008 financial crisis. To evaluate its effect in our setting, we examine whether the reduction in credit supply is more pronounced for riskier loans. Following Fuster et al. (2021), we measure risk-taking at the loan level using the loan-to-income ratio. A higher ratio indicates greater risk-taking. For example, a borrower earning $10,000 annually would be taking on excessive risk by borrowing $1 million. We partition the application sample into two subsamples based on the median loan-to-income ratio and run Equation (3) separately for each. Table 8 presents the results of this analysis. We find that the coefficient on *Repository* is significantly negative for both high- and low-risk borrowers. The difference in coefficients on *Repository* between the two subsamples is statistically significant ($\chi^2 = 4.38$, p-value < 0.05), suggesting that lenders reduce credit supply more for loans exhibiting higher risk-taking when their enforcement records are posted on the centralized repository.

In addition, we conduct another cross-sectional test by partitioning the sample into low- versus high-enforcement states. This test is based on the idea that a reduction in credit supply would enhance welfare if it curtails excessive risk-taking, which is more probable in states with lax enforcement. We classify a state as high-enforcement (low-enforcement) if its percentage of sanctioned mortgage companies is above (below) the median. The results, reported in Table A7 of the Online Appendix, indicate a larger reduction in credit supply in low-enforcement states, where

excessive risk-taking is more likely. The difference in coefficients on *Repository* between the two groups is almost statistically significant (*p*-value= 0.12). Overall, these results lean toward the notion that the credit supply reduction resulting from the repository may improve social welfare by constraining risk-taking.

6. Conclusion

To our knowledge, this study is the first to examine the effect of information processing costs on regulators. By leveraging the introduction of the centralized repository 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 more pronounced for records that experience 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 lowering regulators' information processing costs causes lenders to reduce their credit supply.

A possible concern is whether our main findings imply that regulators "herd" to sanction the same company. We believe that such an interpretation is unlikely, as enforcement actions cannot be based solely on speculation. To issue an enforcement action, a regulator must possess concrete evidence of the mortgage company's misconduct *in the regulator's own state.*

Last, based on our discussions with state regulators, we learned that state regulators often attempt to resolve a company's misbehavior prior to issuing an enforcement action. Specifically, state regulators may issue a warning and grant the company a grace period to rectify the problem, then follow up to see whether the problem has been resolved. Our data does not allow us to quantify

the repository's effect on regulators' unobservable actions and their outcomes, so our study likely underestimates the effect of reducing regulators' information-processing costs.

Last, while we show that state regulators do not appear to free-ride on each other's enforcements, we caveat that our findings do not directly suggest that the centralized repository makes regulators more effective in supervising mortgage companies. Because misconduct *not* caught by state regulators is inherently unobservable, it is impossible for us to measure how effective regulators are in detecting misconduct. Thus, our study is akin to previous accounting literature that examines whether recognition versus disclosure matters but cannot determine which is better (e.g., Bratten et al. 2013, Michels 2017, Neilson et al. 2022).

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Appendix A. Variable Definitions

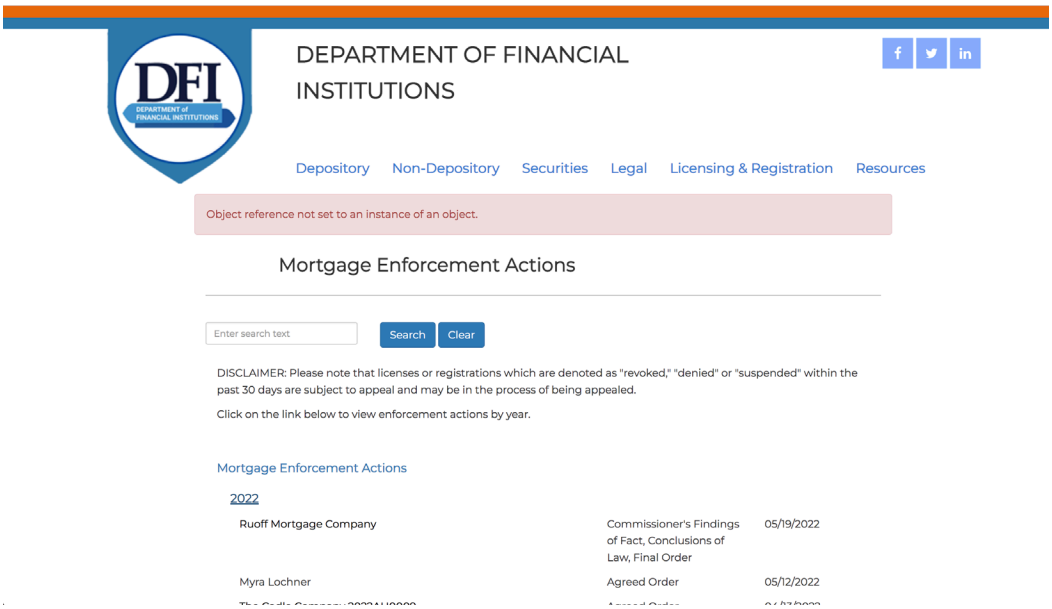
Variable	Definition
Approval $_{j, s, t}$	= 1 if the loan application *j* from state *s* in year *t* is approved and 0 if it is denied.
Borrower Gender $_{j, s, t}$	= 1 if loan application is submitted by a male borrower, and 0 if it is by a female borrower.
CFPB Enforcement	= 1 if a mortgage company is sanctioned by the CFPB during the sample period of 2010-2014, 0 otherwise.
Distance to CFPB Headquarter	The distance from the mortgage company's headquarter to the CFPB headquarter (i.e., Washington, D.C.)
Doc Length $_{r}$	The word count of enforcement record *r* (in thousands).
Education $_{s, t}$	The average percentage of individuals 25 years old or above who hold a bachelor's degree or higher in state *s* in year *t*.
Enforcement $_{i, s, t}$	= 1 if firm *i* receives an enforcement action in state *s* in year *t*, 0 otherwise.
Ethnicity $_{j, s, t}$	= 1 if loan application *j* from state *s* in year *t* is submitted by a Hispanic/Latino borrower, 0 otherwise.
Fines $_{r}$	The dollar amount of the penalty imposed by enforcement record *r* (in $thousands).
Loan Purpose $_{j, s, t}$	A categorical variable for loan purposes (i.e., home purchase, home improvement, and refinancing).
Loan Type $_{j, s, t}$	A categorical variable for loan types (i.e., conventional, Federal Housing Administration, Veterans Administration, Farm Service Agency or Rural Housing Service).
Log Borrower Income $_{j, s, t}$	The natural logarithm of annual borrower income (in $ thousands).
Log Complaints $_{i, w}$	The natural logarithm of one plus the number of CFPB consumer complaints against firm *i* in the three-year observation window *w* before each period. We assign a value of zero if there are no complaints against a company.
Log Gov Employees $_{s, t}$	The natural logarithm of average number of government employees in state *s* in year *t* (in thousands).
Log Gov Expenditure $_{s, t}$	The natural logarithm of average annual government expenditure in state *s* in year *t* (in $billions).
Log Household Income $_{s, t}$	The natural logarithm of the median household income (in $ thousands) in state *s* in year *t*.
Log Loan Amount $_{j, s, t}$	The natural logarithm of the loan application's principal amount (in $ thousands).
Log Population $_{s, t}$	The natural logarithm of the average population (in millions) in state *s* in year *t*.
Minority $_{s, t}$	The average percentage of Black or Hispanic residents in the state *s* population in year *t*.
Number of Reasons $_{r}$	The number of reasons for which the firm is sanctioned in enforcement record *r*.
Owner Occupancy $_{j, s, t}$	=1 if the loan application's property is occupied by the owner, 0 otherwise.
Post on Repository $_{r}$	=1 if enforcement record *r* is posted on the centralized repository, 0 otherwise.
Property Type $_{j, s, t}$	A categorical variable for property types (i.e., one- to four-family housing, manufactured housing, and multifamily housing).
Race $_{j, s, t}$	A categorical variable for borrower races (i.e., Asian, African American, native Hawaiian, other Pacific Islander, and white).
Records $_{i, \neq s, w}$	The number of firm *i*'s enforcement records in states other than state *s* in the three-year observation window *w* before each (pre- or post-) period. The three-year observation window before the pre- (post-) period is 2007-2009 (2010-2012).

Records High-cost $_{i, \neq s, w}$	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 *w* before each (pre- or post-) period. We classify a state website as "high information processing cost" in the same way as *Repository High-cost Records* $_{i, \neq s, (t-3, t-1)}$.
Records Low-cost $_{i, \neq s, w}$	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 *w* before each (pre- or post-) period. We classify a state website as "low information processing cost" in the same way as *Repository Low-cost Records* $_{i, \neq s, w}$.
Records Pseudo $_{i, \neq s, w}$	The number of firm *i*'s enforcement records in states other than state *s* in the two-year observation window *w* before each pseudo (pre- or post-) period. The two-year observation window before the pseudo pre- (post-) period is 2007-2008 (2009-2010).
Records $_{i, s, w}$	The number of firm *i*'s enforcement records in state *s* in the three-year observation window *w* before each (pre- or post-) period. The three-year observation window before the pre- (post-) period is 2007-2009 (2010-2012).
Records Pseudo $_{i, s, w}$	The number of firm *i*'s enforcement records in state *s* in the two-year observation window *w* before each pseudo (pre- or post-) period *t*. The two-year observation window before the pseudo pre- (post-) period is 2007-2008 (2009-2010).
Repository $_{i, \neq s, w}$	The number of firm *i*'s enforcement records in states other than state *s* from the observation window for the post-period (i.e., 2010 to 2012) that are posted in the centralized repository at its launch in 2012. See details in Figure 1.
Repository High-cost $_{i, \neq s, w}$	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 it 1) pools all enforcement records and does not identify those related to mortgage companies or 2) does not directly disclose enforcement records on the website.
Repository Low-cost $_{i, \neq s, w}$	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.
Repository Pseudo $_{i, \neq s, w}$	The number of firm *i*'s enforcement records in states other than state *s* from 2009 to 2010 that are later posted in the centralized repository at its launch in 2012. The 2009–2010 period is the two-year observation window before the pseudo post-period.
Revoke License $_{r}$	=1 if the company's mortgage license is suspended or revoked as a result of enforcement action *r*, 0 otherwise.

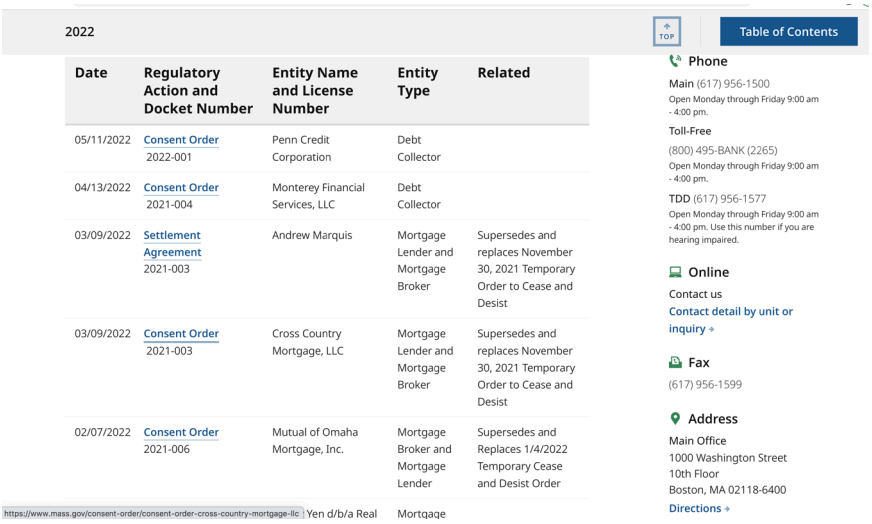
Appendix B: Examples of State Websites that Disclose Enforcement Actions

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

Kentucky separately discloses enforcement actions against mortgage companies.

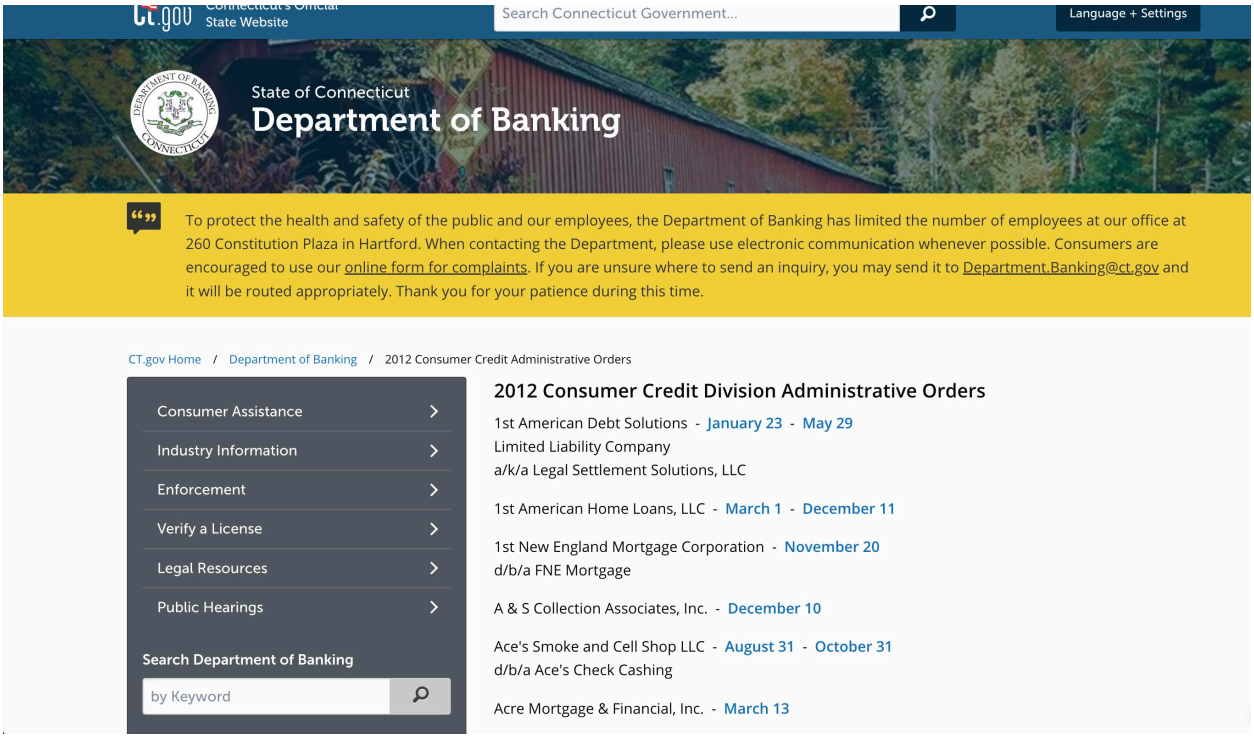


Massachusetts labels the industries of sanctioned companies, although it pools all enforcement actions.

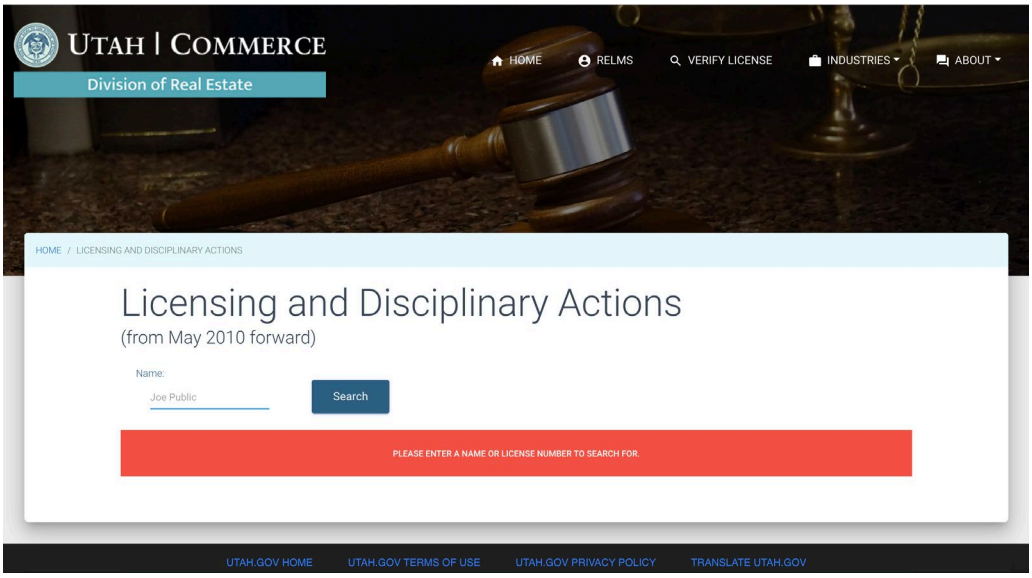


2) State websites with *high* information acquisition costs:

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 the regulators' mortgage examiners.

Figure 1. Mapping Enforcement Records to Subsequent Enforcements

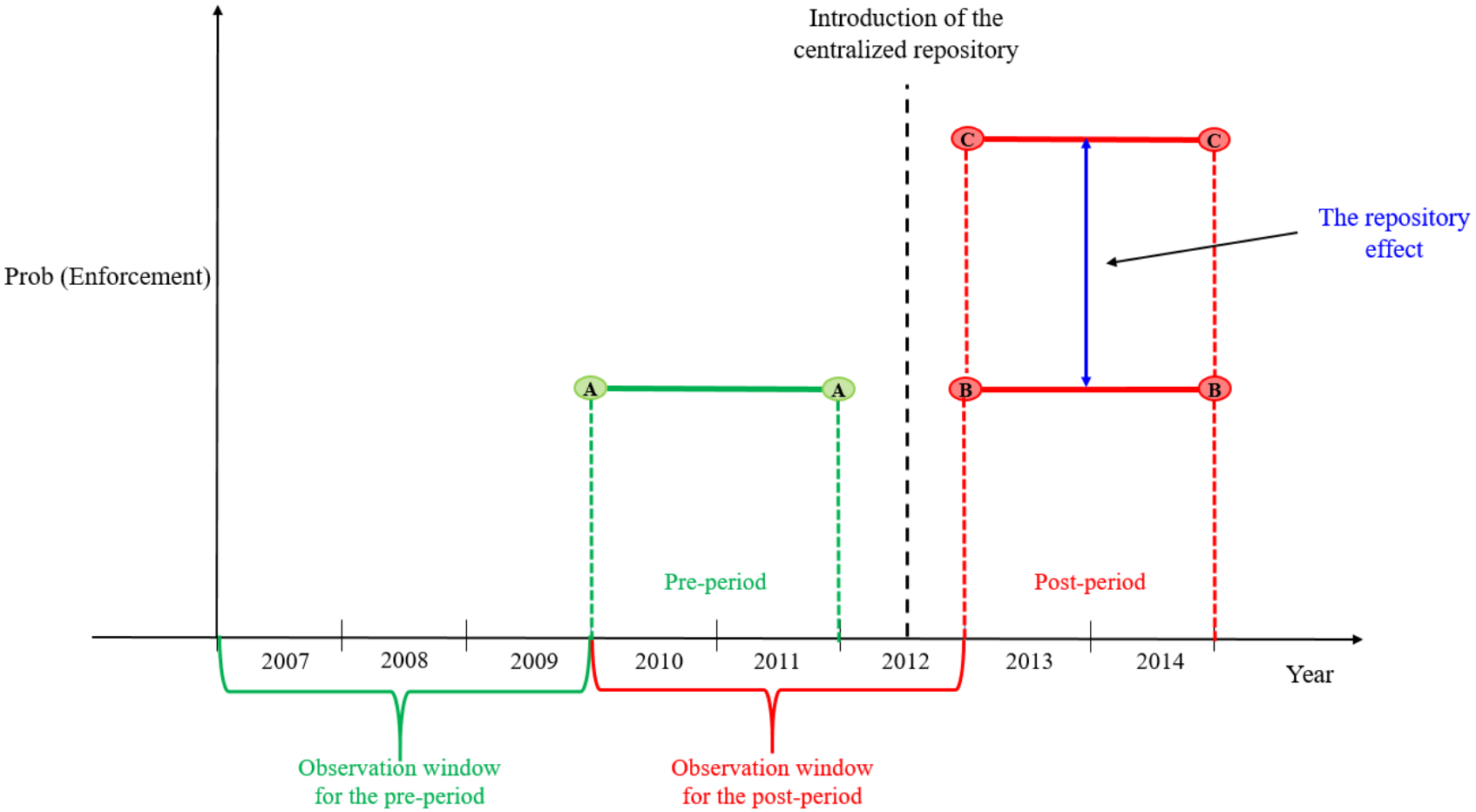


Figure 2. Enforcement Actions against Extensive-margin Companies

We examine whether enforcement actions against companies that have *not* been sanctioned by other states (i.e., "extensive-margin companies") increase after the repository's launch, relative to actions against companies that have been sanctioned by other states (i.e., "intensive-margin companies"). Specifically, for each state and year, we calculate the ratio of enforcement actions against extensive-margin companies to the total enforcement actions. We tabulate the mean percentage of extensive-margin enforcement actions and plot the year-by-year trend during our sample period below:

	Pre-period	Post-period	*t*-test Difference	*t*-test *p*-value
Enforcement actions against extensive-margin companies (%)	72.0%	68.0%	4.0%	0.24

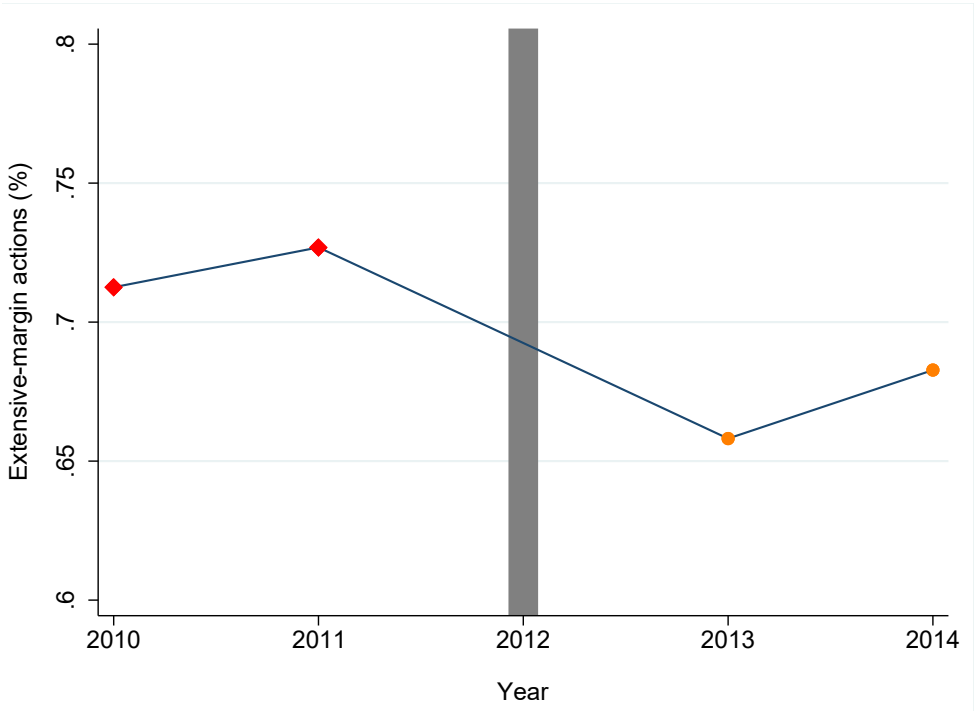


Table 1
Descriptive Statistics

This table reports descriptive statistics for variables used in the enforcement action tests. See Appendix A for variable definitions.

	N	Mean	STD	25th	Median	75th
Dependent variables						
Enforcement $_{i, s, t}$	34,432	0.026	0.159	0.000	0.000	0.000
Independent variables						
Repository $_{i, \neq s, w}$	34,432	0.217	0.494	0.000	0.000	0.000
Repository Pseudo $_{i, \neq s, w}$	11,178	0.055	0.228	0.000	0.000	0.000
Repository High-cost $_{i, \neq s, w}$	34,432	0.154	0.415	0.000	0.000	0.000
Repository Low-cost $_{i, \neq s, w}$	34,432	0.063	0.243	0.000	0.000	0.000
Records $_{i, \neq s, w}$	34,432	1.068	1.248	0.000	1.000	1.000
Records Pseudo $_{i, \neq s, w}$	11,178	0.865	1.059	0.000	1.000	1.000
Records High-cost $_{i, \neq s, w}$	34,432	0.766	0.995	0.000	0.000	1.000
Records Low-cost $_{i, \neq s, w}$	34,432	0.301	0.599	0.000	0.000	0.000
Records $_{i, s, w}$	34,432	0.079	0.269	0.000	0.000	0.000
Records Pseudo $_{i, s, w}$	11,178	0.078	0.269	0.000	0.000	0.000
Complaints $_{i, w}$	34,432	5.901	53.65	0.000	0.000	0.000
Population $_{s, t}$	34,432	8.230	8.116	2.979	5.839	9.882
Household Income $_{s, t}$	34,432	52.45	8.891	45.75	50.23	58.88
Education $_{s, t}$	34,432	0.294	0.060	0.258	0.283	0.328
Minority $_{s, t}$	34,432	0.320	0.152	0.197	0.304	0.427
Gov Expenditure $_{s, t}$	34,432	38.32	39.61	15.75	26.19	47.56
Gov Employees $_{s, t}$	34,432	132.8	99.38	73.24	103.9	167.1

Table 2
The Centralized Repository's Effect on Subsequent Enforcements

Dependent variable = *Enforcement* $_{i, s, t}$	(1)	(2)
Repository $_{i, \neq s, w}$	**0.011*** **	**0.011*** **
	(4.91)	**(4.89)**
Records $_{i, \neq s, w}$	0.002*	0.002
	(1.94)	(1.63)
Records $_{i, s, w}$	-0.215***	-0.212***
	(-29.02)	(-29.40)
Log Complaints $_{i, w}$	0.002**	0.002*
	(2.52)	(1.83)
Log Population $_{s, t}$	0.139	
	(1.53)	
Log Household Income $_{s, t}$	0.111	
	(1.37)	
Education $_{s, t}$	-0.790***	
	(-3.71)	
Minority $_{s, t}$	-1.091***	
	(-3.77)	
Log Gov Expenditure $_{s, t}$	0.083**	
	(2.43)	
Log Gov Employees $_{s, t}$	0.085***	
	(3.25)	
State × Company FEs	Y	Y
State × Year FEs		Y
Year FEs	Y	
Observations	34,432	34,432
Adj. R^2	0.15	0.16

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-year. 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 the Findings Driven by the Records Posted on the Repository or the Repository Itself?

Panel A: What records are more likely to be posted on the repository?

DV = *Posted on Repository* $_r$	(1)	(2)	(3)	(4)
Number of Reasons $_r$	0.193			0.167
	(1.41)			(1.11)
Doc Length $_r$		0.089		0.065
		(0.34)		(0.23)
Fines $_r$			0.001	-0.001
			(0.09)	(-0.18)
Revoke Licenses $_r$			-0.692*	-0.710*
			(-1.73)	(-1.80)
Log Complaints $_{i, w}$	-0.151	-0.140	-0.178	-0.179
	(-1.02)	(-0.96)	(-1.19)	(-1.18)
State, Year FEs	Y	Y	Y	Y
Observations	922	922	922	922
Pseudo R^2	0.54	0.54	0.55	0.55

Panel B: A falsification test using the pre-period

DV = *Enforcement* $_{i, s, t}$	(1)
Repository Pseudo $_{i, \neq s, w}$	**0.001**
	(0.06)
Records Pseudo $_{i, \neq s, w}$	0.011***
	(3.79)
Records Pseudo $_{i, s, w}$	-0.303***
	(-17.87)
State × Company FEs	Y
State × Year FEs	Y
Observations	11,178
Adj. R^2	0.28

This table presents the results of two tests that address the concern that our findings are driven by the records posted on the repository rather than the repository itself. Panel A reports the results of estimating whether severe records are more likely to be posted on the repository. The unit of observation is an enforcement record. Panel B reports the results of a falsification test using the pre-period. We pretend that the centralized repository was launched in 2010 and examine whether the records posted on the repository exhibit a similar trend in the likelihood of subsequent enforcements in other states as the records not posted. The pseudo pre- and post-periods are 2009 and 2011, respectively. For each pseudo period, we assume a two-year preceding window for regulators to learn about enforcement records from other states. The unit of observation is a company-state-year. We do not include *Log Complaints* as a control because the CFPB complaint data only starts from December 2011. 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. Are The Findings Driven by The CFPB's Information Sharing?

Panel A: Excluding companies who receive a CFPB complaint or a CFPB enforcement action

DV = *Enforcement* $_{i, s, t}$	Excluding firms with a CFPB complaint	Excluding firms with a CFPB enforcement action
	(1)	(2)
Repository $_{i, \neq s, w}$	**0.016*** **	**0.012*** **
	(5.40)	**(5.21)**
Records $_{i, \neq s, w}$	0.001	0.001
	(0.96)	(1.47)
Records $_{i, s, w}$	-0.210***	-0.210***
	(-26.27)	(-28.96)
Log Complaints $_{i, w}$		0.002**
		(2.02)
State × Company FEs	Y	Y
State × Year FEs	Y	Y
Observations	22,010	33,185
Adj. R^2	0.17	0.16

Panel B. What firm receives more CFPB attention?

DV = *CFPB Enforcement* $_i$	(1)	(2)	(3)
Log Complaints $_i$	0.722***		0.781***
	(3.66)		(4.09)
Distance to CFPB Headquarter $_i$		-0.370*	-0.492**
		(-1.84)	(-2.42)
Observations	1,102	1,102	1,102
Pseudo R^2	0.10	0.02	0.12

Panel C. Are the main findings stronger for firms with more CFPB attention?

DV = *Enforcement* $_{i, s, t}$	Low CFPB attention	High CFPB attention
	(1)	(2)
Repository $_{i, \neq s, w}$	**0.013*** **	**0.007** **
	(4.18)	**(2.14)**
Records $_{i, \neq s, w}$	0.002	0.001
	(0.98)	(0.99)
Records $_{i, s, w}$	-0.202***	-0.223***
	(-23.37)	(-18.09)
Log Complaints $_{i, w}$	0.013	0.001
	(1.40)	(1.08)
Test of coefficient difference	**χ^2 = 2.26 (*p*-value = 0.13)**	
State × Company FEs	Y	Y
State × Year FEs	Y	Y
Observations	17,395	17,037
Adj. R^2	0.17	0.15

This table presents the results of tests that address the concern that our findings are driven by the CFPB's potential information sharing with state regulators. Panel A reports the results of robustness tests where we exclude companies who receive either a CFPB complaint or a CFPB enforcement action during our sample period. Panel B reports the results of what firms are likely to be sanctioned by the CFPB. In Panel C, we partition the sample into two subsamples by the median firm-level CFPB attention. We use the predicted value from the results in Panel B to measure the CFPB's attention. 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 Records' Pre-Repository Processing Costs

DV = *Enforcement* $_{i, s, t}$	(1)
Repository High-cost $_{i, \neq s, w}$	**0.022***** **(4.63)**
Repository Low-cost $_{i, \neq s, w}$	**0.007***** **(2.80)**
Records High-cost $_{i, \neq s, w}$	-0.001 (-0.39)
Records Low-cost $_{i, \neq s, w}$	0.002* (1.79)
Records $_{i, s, w}$	-0.212*** (-29.44)
Log Complaints $_{i, w}$	0.002* (1.92)
Test of difference: *Repository High-cost* vs. *Repository Low-cost*	***F* = 7.49***
State × Company FEs	Y
State × Year FEs	Y
Observations	34,432
Adj. R^2	0.16

In this table, we partition the centralized repository's enforcement records into two groups based on the information-processing costs associated with the state websites that post the records. We replace *Repository* with *Repository High-cost* and *Repository Low-cost* (the sum of the two equals *Repository*). We classify a state website as "high information processing cost" if it 1) pools all enforcement records without specifically identifying those related to mortgage companies or 2) does not directly disclose enforcement records on its website (e.g., only provides 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 6
The Centralized Repository's Effect on Subsequent Enforcements by Regulators' Resource Constraints

Examiner-to-company ratio:	Low	High
DV = *Enforcement* $_{i, s, t}$	(1)	(2)
Repository $_{i, \neq s, w}$	**0.017*** **	**0.005***
	(5.03)	**(1.84)**
Records $_{i, \neq s, w}$	0.002	0.002
	(1.06)	(1.32)
Records $_{i, s, w}$	-0.199***	-0.198***
	(-20.25)	(-13.45)
Log Complaints $_{i, w}$	0.003**	0.000
	(2.27)	(0.12)
Test of coefficient difference	χ^2 **= 8.14*** **	
State × Company FEs	Y	Y
State × Year FEs	Y	Y
Observations	15,399	13,171
Adj. R^2	0.18	0.10

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 7
The Centralized Repository's Effect on Credit Supply

Panel A. Descriptive statistics

N = 6,743,140 loan applications	Mean	STD	25th	Median	75th
Approval $_{j, s, t}$	0.782	0.413	1.000	1.000	1.000
Repository $_{i, \neq s, w}$	0.250	0.511	0.000	0.000	0.000
Records $_{i, \neq s, w}$	1.443	1.452	0.000	1.000	2.000
Records $_{i, s, w}$	0.082	0.275	0.000	0.000	0.000
Complaints $_{i, w}$	53.08	186.5	0.000	0.000	4.000
Borrower Income $_{j, s, t}$	0.095	0.103	0.048	0.074	0.115
Loan Amount $_{j, s, t}$	0.211	0.147	0.116	0.178	0.273
Borrower Gender $_{j, s, t}$	0.707	0.455	0.000	1.000	1.000

Panel B. Regression results

DV = *Approval* $_{j, s, t}$	(1)
Repository $_{i, \neq s, w}$	**-0.024****
	(-2.37)
Records $_{i, \neq s, w}$	0.007
	(1.53)
Records $_{i, s, w}$	-0.012
	(-1.23)
Log Complaints $_{i, w}$	-0.002
	(-0.69)
Log Borrower Income $_{j, s, t}$	0.066***
	(7.65)
Log Loan Amount $_{j, s, t}$	-0.032***
	(-3.92)
Borrower Gender $_{j, s, t}$	0.000
	(0.09)
State × Company FEs	Y
State × Application Year FEs	Y
Application Year × Loan-characteristics FEs	Y
Observations	6,743,140
Adj. R^2	0.22

Panels A and B of this table report the descriptive statistics and the regression results of estimating the centralized repository's effect on the lender's loan approval probability, respectively. The observation unit 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 8
The Centralized Repository's Effect on Credit Supply
by Loan Risk

DV = *Approval* $_{j, s, t}$	Low-risk borrowers (1)	High-risk borrowers (2)
Repository $_{i, \neq s, w}$	**-0.016***	**-0.030****
	(-1.77)	**(-2.51)**
Records $_{i, \neq s, w}$	0.005	0.009*
	(1.27)	(1.67)
Records $_{i, s, w}$	-0.010	-0.014
	(-1.17)	(-1.18)
Log Complaints $_{i, w}$	-0.002	-0.001
	(-0.99)	(-0.52)
Log Borrower Income $_{j, s, t}$	0.002	0.202***
	(0.49)	(8.86)
Log Loan Amount $_{j, s, t}$	0.023***	-0.180***
	(5.35)	(-8.51)
Borrower Gender $_{j, s, t}$	0.003	-0.004*
	(1.58)	(-1.90)
Test of coefficient difference	χ^2 = **4.38****	
State × Company FEs	Y	Y
State × Application Year FEs	Y	Y
Application Year × 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 after we partition the loan application sample into two subsamples based on loan risk. We define a loan as high (low) risk if the loan-to-income ratio is above (below) the sample median. 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.

Online Appendix

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Table A1
Alternative Observation Windows

Observation Windows:	Two years	Four years
DV = *Enforcement* $_{i, s, t}$	(1)	(2)
Repository $_{i, \neq s, w2}$	**0.013*****	
	(4.68)	
Repository $_{i, \neq s, w4}$		**0.006*****
		(3.11)
Records $_{i, \neq s, w2}$	0.001	
	(0.75)	
Records $_{i, \neq s, w4}$		0.002*
		(1.88)
Records $_{i, s, w2}$	-0.156***	
	(-18.81)	
Records $_{i, s, w4}$		-0.255***
		(-29.72)
Log Complaints $_{i, w}$	0.003**	0.000
	(2.51)	(0.51)
Company × State FEs	Y	Y
State × Year FEs	Y	Y
Observations	34,432	34,432
Adj. R^2	0.11	0.17

This table presents the results of estimating the centralized repository's effect on subsequent enforcement actions using two alternative observation windows. Specifically, instead of assuming a three-year observation window, we use two years and four years as alternative observation windows and reconstruct all variables. Note that alternative observation windows do not affect the construction of *Log Complaints* because the CFPB complaint data starts in December 2011. The pre-period is 2010–2011 and the post-period is 2013–2014. 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 A2
Alternative Specification of Record-related Variables

DV = *Enforcement* $_{i, s, t}$	Indicators (1)	Log Form (2)
Repository Indicator $_{i, \neq s, w}$	**0.016*** **	
	(6.06)	
Repository Log $_{i, \neq s, w}$		**0.018*** **
		(5.25)
Records Indicator $_{i, \neq s, w}$	0.003	
	(1.64)	
Records Log $_{i, \neq s, w}$		0.004*
		(1.80)
Records Indicator $_{i, s, w}$	-0.211***	
	(-29.40)	
Records Log $_{i, s, w}$		-0.303***
		(-29.35)
Log Complaints $_{i, w}$	0.002*	0.002*
	(1.94)	(1.86)
Company × State FEs	Y	Y
State × Year FEs	Y	Y
Observations	34,432	34,432
Adj. R^2	0.16	0.16

This table presents the results of estimating the centralized repository's effect on subsequent enforcement actions using two alternative specifications of record-related variables. In Column (1), we convert all three (continuous) record-based variables into binary variables. In Column (2), we convert them into their log forms, which better captures the marginal diminishing effect of posting one more record in the centralized repository. The pre-period is 2010–2011 and the post-period is 2013–2014. 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 A3
Excluding Enforcement Records with Media Coverage

DV = *Enforcement* $_{i, s, t}$	(1)	(2)
Repository $_{i, \neq s, w}$	**0.011*****	**0.010*****
	(4.56)	**(4.54)**
Records $_{i, \neq s, w}$	0.003**	0.002**
	(2.36)	(2.13)
Records $_{i, s, w}$	-0.210***	-0.207***
	(-27.78)	(-28.24)
Log Complaints $_{i, w}$	0.002**	0.001
	(2.20)	(1.59)
Log Population $_{s, t}$	0.118	
	(1.22)	
Log Household Income $_{s, t}$	0.067	
	(0.78)	
Education $_{s, t}$	-0.822***	
	(-3.62)	
Minority $_{s, t}$	-1.254***	
	(-4.05)	
Log Gov Expenditure $_{s, t}$	0.108***	
	(2.95)	
Log Gov Employees $_{s, t}$	0.095***	
	(3.43)	
Company × State FEs	Y	Y
State × Year FEs		Y
Year FEs	Y	
Observations	30,538	30,538
Adj. R^2	0.14	0.15

This table presents the results of estimating the centralized repository's effect on subsequent enforcement actions after we exclude firms whose enforcement records are covered by at least one newspaper article. We identify enforcement actions with media coverage using the Factiva and Access News databases. The unit of observation is a company-state-year. The pre-period is 2010–2011 and the post-period is 2013–2014. See Appendix A of the paper 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 A4
Excluding *Records* $_{i, s, w}$ from The Regression Model

DV = *Enforcement* $_{i, s, t}$	(1)	(2)
Repository $_{i, \neq s, w}$	**0.018*** **	**0.016*** **
	(5.84)	**(5.80)**
Records $_{i, \neq s, w}$	0.001	0.000
	(0.60)	(0.06)
Log Complaints $_{i, w}$	0.004***	0.003**
	(3.17)	(2.54)
Log Population $_{s, t}$	0.408***	
	(3.67)	
Log Household Income $_{s, t}$	0.097	
	(1.15)	
Education $_{s, t}$	-1.101***	
	(-4.98)	
Minority $_{s, t}$	-1.894***	
	(-5.93)	
Log Gov Expenditure $_{s, t}$	0.008	
	(0.23)	
Log Gov Employees $_{s, t}$	0.129***	
	(4.21)	
State × Company FEs	Y	Y
State × Year FEs		Y
Year FEs	Y	
Observations	34,432	34,432
Adj. R^2	0.06	0.08

This table presents the results of re-estimating the centralized repository's effect on subsequent enforcement actions after excluding the *Records* $_{i, s, w}$ variable. The unit of observation is a company-state-year. The pre-period is 2010–2011 and the post-period is 2013–2014. See Appendix A of the paper 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 A5: Robustness Checks for the Resource Constraint Test

	Excluding the three states		Adjust examiners using the percentage of nonbanks		Adjust examiners using the percentage of mortgages originated by nonbanks	
Examiner-to-company ratio:	Low	High	Low	High	Low	High
DV = *Enforcement* $_{i, s, t}$	(1)	(2)	(3)	(4)	(5)	(6)
Repository $_{i, \neq s, w}$	**0.018*** **	**0.005***	**0.017*** **	**0.005***	**0.017*** **	**0.006****
	(5.03)	**(1.72)**	**(5.03)**	**(1.84)**	**(4.95)**	**(2.03)**
Records $_{i, \neq s, w}$	0.002	0.001	0.002	0.002	0.002	0.001
	(1.06)	(1.00)	(1.06)	(1.32)	(1.23)	(1.11)
Records $_{i, s, w}$	-0.199***	-0.190***	-0.199***	-0.198***	-0.199***	-0.199***
	(-20.25)	(-12.83)	(-20.25)	(-13.45)	(-20.30)	(-13.34)
Log Complaints $_{i, w}$	0.003**	0.000	0.003**	0.000	0.003**	-0.000
	(2.28)	(0.38)	(2.27)	(0.12)	(2.41)	(-0.07)
Test of coefficient difference	χ^2 **= 8.59*** **		χ^2 **= 8.14*** **		χ^2 **= 7.71*** **	
State × Company FEs	Y	Y	Y	Y	Y	Y
State × Year FEs	Y	Y	Y	Y	Y	Y
Observations	14,858	12,477	15,399	13,171	14,931	13,639
Adj. R^2	0.18	0.10	0.18	0.10	0.18	0.10

This table reports the results of robustness tests for the cross-sectional test based on regulators' resource constraints (Table 6 of the paper). In Columns (1) and (2), we exclude the three states where mortgage examiners are also responsible for banks (i.e., Alaska, Utah, Vermont). In Columns (3) and (4), we include these three states but adjust their number of mortgage examiners using the percentage of mortgage companies (i.e., the number of mortgage companies divided by the sum of mortgage companies and banks). In Columns (5) and (6), we adjust the three states' number of mortgage examiners using the percentage of mortgages originated by mortgage companies (i.e., the number of mortgages originated by mortgage companies divided by all mortgages originated). We classify a state as more or less resource-constrained in the same way as Table 6 of the paper. See Appendix A of the paper 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 A6
Alternative Proxy for Regulators' Resource Constraints

Examiner-to-mortgage ratio:	Low	High
DV = *Enforcement* $_{i, s, t}$	(1)	(2)
Repository $_{i, \neq s, w}$	**0.018*****	**0.003**
	(5.03)	**(1.26)**
Records $_{i, \neq s, w}$	0.002	0.001
	(1.13)	(1.22)
Records $_{i, s, w}$	-0.193***	-0.221***
	(-21.04)	(-11.97)
Log Complaints $_{i, w}$	0.003**	-0.000
	(2.29)	(-0.21)
Test of coefficient difference	χ^2 **= 10.94*****	
Company × State FEs	Y	Y
State × Year FEs	Y	Y
Observations	17,264	11,306
Adj. R^2	0.17	0.11

This table presents the results of estimating the centralized repository's effect on subsequent enforcement actions after we partition the sample based on an alternative proxy for resource constraints of state regulators who observe the records in the centralized repository. We classify a state as more (less) resource-constrained if the ratio of the number of mortgage examiners to the number of loans originated by mortgage companies in that state is below (above) the median. See Appendix A of the paper 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 A7
The Centralized Repository's Effect on Credit Supply
by State Enforcement Intensity

Enforcement Intensity	Low	High
DV = *Approval* $_{j, s, t}$	(1)	(2)
Repository $_{i, \neq s, w}$	**-0.030*** **	**-0.020***
	(-2.93)	**(-1.83)**
Records $_{i, \neq s, w}$	0.008	0.007
	(1.48)	(1.43)
Records $_{i, s, w}$	-0.026	-0.004
	(-1.44)	(-0.45)
Log Complaints $_{i, w}$	-0.002	-0. (p001
	(-0.97)	(-0.46)
Log Borrower Income $_{j, s, t}$	0.066***	0.067***
	(6.90)	(7.89)
Log Loan Amount $_{j, s, t}$	-0.034***	-0.030***
	(-3.75)	(-3.75)
Borrower Gender $_{j, s, t}$	0.002	-0.001
	(0.60)	(-0.42)
Test of coefficient difference	**χ^2 = 2.42 (*p*-value = 0.12)**	
State × Company FEs	Y	Y
State × Application Year FEs	Y	Y
Application Year × Loan-characteristics FEs	Y	Y
Observations	2,948,586	3,794,554
Adj. R^2	0.22	0.23

This table presents the results of estimating the effect of the repository on lenders' loan approval probability after we partition the loan application sample into two subsamples based on state regulators' enforcement intensity. We classify a state as high-enforcement (low-enforcement) intensity if its percentage of sanctioned mortgage companies is above (below) the median. See Appendix A of the paper 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.