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Enforcement of banking regulation and the cost of borrowing

Abstract

We show that borrowing firms benefit substantially from important enforcement actions issued on U.S. banks for safety and soundness reasons. Using hand-collected data on such actions from the main three U.S. regulators and syndicated loan deals over the years 1997-2014, we find that enforcement actions decrease the total cost of borrowing by approximately 22 basis points (or $4.6 million interest for the average loan). We attribute our finding to a competition-reputation effect that forces banks to lower their cost of credit, irrespective of other changes in their business models after the enforcement action.

Keywords: Bank supervision; Enforcement actions; Syndicated loans; Loan pricing
JEL codes: E44; E51; G21; G28

1. Introduction

Is the enforcement of banking regulation beneficial or costly for borrowing firms? The role of market regulation in preventing failures has been a central theme in economic and finance research since at least the time of Pigou, but regulatory enforcement has received much less attention. In the banking industry, regulation is the *sine qua non* of the effort to prevent, contain, and smooth out the harmful real effects of banking crises. However, regulations are void without effective supervision and enforcement, which is why enforcement actions are the single most important tool for implementing prudential regulatory policy in banking.

Banking regulators levy enforcement actions for violations of laws, rules, and regulations, as well as for unsafe or unsound practices, breaches of fiduciary duty, and violations of final orders (Fed, 2015). These actions take a number of forms, including financial penalties, prompt corrective actions, safety and soundness orders, cease and desist orders, etc. Enforcement actions can be formal or informal; on their websites, the U.S. regulators publicly announce formal enforcement actions while the informal ones are strictly confidential. The most important ones from the formal actions concern the financial safety and soundness of the banking system and are the ones we study.

In this research, we use information on formal enforcement actions and syndicated loans to examine, for the first time, the effect of regulatory enforcement on the cost of borrowing (cost of loans). We find a substantial negative effect, which we attribute to two reasons. The first comes from a *risk-taking effect*, implying that punished banks behave more prudently post-enforcement by lending to less risky firms at a lower cost. This result is consistent with Berger et al. (2016) and Delis et al. (2017), who show that banks have higher risk-based capital ratios post-enforcement, mainly by curtailing their risky assets and lending activities.

However, and most importantly for the objectives of our research, we also find lower cost of borrowing post-enforcement even in models involving lending to the same firm pre- and post-enforcement within the year of the enforcement action. We posit that this lower cost of lending over and above the risk-taking effect relates to the lower reputation of punished banks post-enforcement and the need to maintain lending relationships with firms within a competitive market. This effect, named the *competition-reputation effect*, has important and positive welfare implications because regulatory initiatives outside the borrowing firms' operations or the risk-taking incentives of banks lower the cost of borrowing.

We use hand-collected data with information on the formal enforcement actions against U.S. banks and bank-holding companies supervised by the Federal Reserve (Fed), the Federal Deposit Insurance Corporation (FDIC), and the Office of the Comptroller of the Currency (OCC) during 2001–2010 (our end sample of loan-level data extends to the years 1997-2014 to allow for a window around these events). In line with our theoretical priors and the relevant taxonomy of enforcement actions, we use only the enforcement actions that substantially relate to safety and soundness practices. These actions have a significant bearing on the reputation of lead arrangers.

Subsequently, we match these actions with loan-level data from DealScan (syndicated loans). We measure the cost of syndicated loans by (i) the all-in-drawn spread, (ii) the measure of total cost of borrowing of Berg et al. (2016), and (iii) specific fees. In turn, our main explanatory variable is a dummy that takes the value one for syndicated loans originated by at least one punished lead bank after the date of the enforcement action, zero for the loans originated by that punished lead bank before the date of the enforcement action, and zero for loans originated by non-punished banks. Thus, we differentiate between "punished loans" and "non-punished loans."

We aim to establish a causal effect running from formal enforcement actions to the cost of borrowing and the structure of our data set helps toward this aim. An important identification challenge is to exclude alternative demand (firm)-side explanations of our findings. In multiple occasions, firms borrow more than once within a year and this allows including firm*year fixed effects. These fixed effects saturate the model from time-varying firm characteristics and the associated changes in the risk-taking incentives of banks that might affect the cost of borrowing (e.g., Jimemez et al., 2012; 2014). Using this approach, we find that the cost of loans originated after a syndicate's lead bank receives a formal enforcement action is significantly lower for borrowers compared to loans originated before the enforcement action. Results suggest a reduction in the all-in-drawn spread by 23 basis points and the total cost of borrowing by 22 basis points (or approximately $4.6 million less in interest income for the loan with the average size and maturity).

To further shut down more general changes in the bank balance sheets following an enforcement action, we include bank*year fixed effects, which saturate the model from any time-varying changes in lead bank (supply-side) behavior between the pre- and post-enforcement periods. These changes include changes in bank corporate governance, capital structure, liability structure, etc. To further increase variation in our data, we interact the enforcement action with bank capitalization, assuming that banks with higher capital will lower the cost of loans by less following an action. Our results indeed show that the negative effect of enforcement actions on the cost of borrowing holds, but is less potent as bank capital increases. An even more stringent identification method is to additionally include bank*firm fixed effects. This implies considering only the loan facilities where the lead arrangers and borrowing firms are the same pre- and post-enforcement. The results show an approximately 26 basis points reduction in either the all-in-drawn-spread or the total cost of borrowing.

In addition, we pinpoint the existence of a competition-reputation effect of enforcement actions (and not the existence of another unknown effect) by showing that (i) the Lerner index of bank market power is lower post-enforcement and (ii) the inclusion of the Lerner index in our baseline model absorbs the effect of enforcement actions on the cost of borrowing. Thus, we highlight that regulatory enforcement benefits firms via lower cost of borrowing for reasons totally unrelated with their operations. In turn, this implies improved welfare, as firms use most of the syndicated loans to finance their operations and investments. On that line, we also find that firms receiving a punished loan experience larger increases in their stock prices vis-à-vis firms receiving non-punished loans.

Our baseline results survive in several respecifications and additional tests. Most importantly, and even though we are interested in the effect of the enforcement actions once these have been enacted (and not the effect stemming from loans given by other banks that perhaps should have been punished but were not), we conduct a test to identify if our results change when allowing for such an effect. Following Delis et al. (2017), we model the probability of being punished using in a first-stage regression of an instrumental variables (IV) model the share of female bank examiners in local examiner offices to the total examiners as the instrument. The negative effect of enforcement actions on the cost of borrowing becomes even more potent compared to our baseline specification when using this IV model.

The paper proceeds as follows. Section 2 briefly analyzes the theoretical mechanisms through which regulatory enforcement might affect the cost of borrowing. Section 3 presents the data set used in the empirical analysis, with brief descriptions of the economics behind enforcement actions and the syndicated loan market. Section 4 discusses in detail the empirical identification strategy of our paper. Section 5 presents and discusses the empirical results, and Section 6 concludes.

2. Theoretical considerations and contribution

Theoretically, formal enforcement actions enacted on banks for safety and soundness reasons can either raise or lower the cost of borrowing. Enforcement actions bear either direct costs for punished banks (e.g., monetary penalties) or indirect costs (loss of reputation, partial loss of management control, etc.). If the punished banks succeed in passing those costs to their borrowers via lending terms, then enforcement actions bear a real cost to economic activity. Such an outcome would provide a leviathan view of regulatory intervention and would raise concerns about the severity of the enforcement actions and their public announcement.

On the other hand, we can think of two mechanisms through which enforcement actions can lower the cost of borrowing. The first mechanism relates to the mere reason behind the type of enforcement actions studied in this paper, namely banks' risk. In the post-enforcement period, punished banks must improve their financial soundness. If this risk-taking effect prevails, then we should observe a reduction in the cost of borrowing because of the reallocation of credit to less risky borrowers to which banks naturally lend at lower cost.

A reduction in the punished banks' risk in the post-enforcement period is consistent with Berger et al. (2016) and Delis et al. (2017). These studies examine the effect of formal enforcement actions on several indicators of bank soundness (obtained from accounting data) and uncover a decrease in punished banks' liquidity creation and increase in the risk-based capital (mainly via less lending and non-performing loans) of punished banks post-enforcement. The analyses are conducted using ex post risk indicators and leave open the question of whether regulatory enforcement affects new risk (i.e., banks' risk-taking).

The second mechanism through which regulatory enforcement might lower the cost of borrowing relates to a competition-reputation effect. Specifically, borrowers might perceive that a punished bank is overly risky and less reputable after it becomes the subject of an important enforcement action related to its safety and soundness. A punished bank may then

offer better lending terms to (the same) borrowers post-enforcement to avoid losing firms to competition. Thus, the competition-reputation effect comes over and above the risk-taking effect and has important and clear-cut welfare implications. This is because lower cost of borrowing, irrespective of changes in the risk-taking incentives of banks, implies more competitive loan pricing post-enforcement to the same or very similar firms. In contrast, the risk-taking effect does not necessarily have welfare implications because the more prudent behavior of banks can be offset by credit rationing.

Essentially, we cannot theoretically identify any additional mechanisms through which regulatory enforcement might affect the cost of borrowing. Our contribution in this paper is thus sequential. We first identify a negative effect of regulatory enforcement on the cost of borrowing, thus ruling out the cost-shifting effect. Given that we study loans, this effect concerns new risk and can be attributed to either the risk-taking or the competition-reputation effects. We then aim to examine whether regulatory enforcement has clear-cut welfare implications. To do this, we need to shut down the risk-taking effect and examine the prevalence of the competition-reputation effect. The latter is the main goal of our analysis.

Our paper is related to three strands of literature. The first and most closely related to our work, considers the effects of enforcement actions on regulatory behavior, bank risk, and capital. Ioannidou (2005) and Agarwal et al. (2014) note that supervisory behavior is different between the three main U.S. regulators due to the Fed's monetary policy objective and other differences in their institutional designs. Berger et al. (2016) show that liquidity creation and bank lending decreases following regulatory interventions and Danisewicz et al. (2018) extend these effects to a study of the real economy post-enforcement. Delis et al. (2017) study the effects of enforcement actions on key bank balance sheet variables, such as capital, non-performing loans, and liquidity. In one of the earliest contributions to this literature, Jordan et

al. (2000) find that enforcement actions cause market reactions to banks' equity prices, with variations in these reactions explained by the quality and timeliness of the actions.

In many respects, this literature is quite useful in grouping the enforcement actions and analyzing several aspects of their effects on the cost of borrowing. However, our analysis is different, as it considers competition-reputation effects of enforcement actions on the cost of borrowing that come over and above changes in risk. Essentially, our results provide a success story for regulatory enforcement, in the sense that the cost of borrowing is lower even for the same or very similar firms in the post-enforcement period. This allows establishing a clear-cut positive effect of regulatory enforcement on the terms of borrowing that firms use to finance their operations and investments.

The second strand of related literature analyzes a number of factors shaping the cost of borrowing (e.g., Berg et al., 2016; Ivashina and Scharfstein, 2010a; b; Graham et al., 2008). These studies are very useful in guiding the construction of the data set and our identification methods but are obviously quite distinct from our study as they do not consider the effect of important regulatory interventions on loan pricing.

From a more general perspective, we also add to the literature of regulatory and financial economics (e.g., Beck, 2010; references therein). A key element in this literature is that the objective of financial stability should be balanced with the goal of improved efficiency in the financial markets in general and banking in particular. From this perspective, we show that formal enforcement actions achieve this fine balance by fostering improved welfare through lower cost of borrowing even for the same or similar firms.

3. Data

We obtain data from three sources. Hand-collected information on the formal enforcement actions (their reason and enactment or enforcement date) is from the Federal Reserve, FDIC,

and OCC websites. We subsequently match this information with syndicated loans from DealScan (for details, see Ivashina and Scharfstein, 2010a; b; Berg, et al., 2016) using information (codes) from the Call Reports. In certain cases, we have to hand-match banks with enforcement actions using banks' name to avoid losing relevant observations.

Our baseline sample includes 44,025 loan facilities originated by 755 lead banks.[1] These loan facilities correspond to 7,327 firms receiving at least two loans with a different *AISD* and *Total cost of borrowing* in one year (this is the number of firms not dropping out of our baseline specifications when using firm*year fixed effects). The median lead bank originates an average of 8.8 loans per year (the average is as high as 182 loans driven by few banks originating many loans). From these, 54 banks received 62 enforcement actions (events) during 2001–2010.[2] The number of enforcement actions in each specification never falls below 55 in our alternative specifications. Evidently, most banks in our sample received an enforcement action only once during our sample period.[3] The enforced banks originate at least one loan before the enforcement action in the year of the action and another loan after the enforcement action in the same year. The number of these "punished loans" in our sample is 5,890.

We focus on enforcement actions given to lead arrangers because these banks decide the terms of lending in syndicated loan deals. If there are multiple lead arrangers, then the loan

[1] The unit of our analysis is the loan facility and not the loan package. The difference between the two is that the loan facility refers to each individual portion of a deal, whereas the deal itself possibly (but not usually) comprises more than one loan facilities and covers the full amount of credit granted to the firm on that occasion. A loan-facility analysis is appropriate for the following reason. Loan facilities may have different starting dates, maturity, amount, and loan type. Hence, multiple loan facilities, even when in the same loan deal, are not fully dependent observations (e.g., simply adding facilities and ignoring their differences, may therefore introduce a bias in the estimates). However, all results presented in this paper are robust to a loan-package analysis.

[2] The number of enforcement actions by year is: 2001 (5 enforcement actions), 2002 (5), 2003 (5), 2004 (6), 2005 (5), 2006 (6), 2007 (5), 2008 (7), 2009 (9), 2010 (9). Thus, the number of enforcement actions is relatively evenly distributed across years. This is in contrast to Delis et al. (2017), who use almost the entirety of supervised U.S. banks and denote a clear concentration of enforcement actions during and shortly after the crisis period.

[3] Note that the number of enforcement actions is not relevant to the sample size of the empirical analysis because, due to our taxonomy, these are uniform events. For example, the vast majority of event studies in the finance literature look at the effect of one or a few homogeneous events.

is a "punished loan" if at least one of the lead arrangers is punished. In what follows, we discuss the variables in our empirical analysis with an emphasis on an economic analysis of enforcement actions. Table 1 provides formal definitions for all the variables in our empirical analysis and Table 2 reports summary statistics.

[Insert Tables 1 and 2 about here]

3.1. Enforcement actions

We use formal enforcement actions against U.S. commercial banks and bank-holding companies supervised by the Federal Reserve, the FDIC, and the OCC.[4] The three federal bank supervisors monitor safety and soundness through a combination of on- and off-site surveillance programs. The most general rule is that the appropriate federal banking agency conducts a full-scope on-site examination of each insured depository institution at least once every 12 months (12 US Code 1820(d)(1)).[5] The full scope of examination encompasses an audit procedure that evaluates all components of the Uniform Financial Institutions Ratings Systems (UFIRS) or the CAMELS rating system assigned to each bank. The components of CAMELS are capital adequacy (C), asset quality (A), management (M), earnings (E), liquidity (L), and sensitivity to market risk (S).

[4] Under its capacity as the chartering authority, the OCC supervises national banks (federally chartered banks) and is responsible for the federal branches or agencies of foreign banks and the Federal Savings Association (12 U.S. Code 481 and 1813(q)(1)). The FDIC, along with the state or federal chartering authority, oversees insured state banks that are not members of the Federal Reserve system, as well as foreign banks that have an insured branch and state savings associations (12 U.S. Code 1813(q)(2)). The board of governors of the Federal Reserve System, together with the state chartering authority, monitors state banks that are members of the Federal Reserve System. They monitor bank-holding companies and their subsidiaries; foreign banks with U.S. operations but without insured branches; foreign banks with U.S. state-chartered branches and agencies; and agencies or commercial lending companies other than federal agencies such as savings-and-loan holding companies and their subsidiaries (12 U.S. Code 1813(q)(2) and 3101 et seq). Evidently, under the U.S. dual banking system, more than one authority (federal/state) can claim supervisory jurisdiction with respect to any depository institution.

[5] Different on-site audit frequencies can apply to banks that have been examined by the state authorities, to well-capitalized and well-managed small banks, to banks in operation for less than five years, and to bank-holding companies, depending on their size and complexity.

The findings from the on-site examinations and the CAMELS ratings play an influential (but non-binding) role in the decision to issue a formal or an informal enforcement action. Informal actions are voluntary commitments made by the bank's board members and serve as evidence of the board's commitment to correct identified problems before they affect the bank's condition. Informal enforcement actions include commitment letters (board resolutions), memoranda of understanding, and approved safety and soundness plans. Formal enforcement actions, on the other hand, are statutorily authorized or mandated, are generally more severe, and are made public. In this paper, we only consider how *formal enforcement actions, as vehicles encompassing public information,* affect the terms of lending. The reason is that these are publicly disclosed actions (unlike the confidential informal actions) that can have competition-reputation effects due to their public announcement.

Formal enforcement actions come, at times, regardless of CAMELS ratings. They occur whenever the supervisor becomes aware of a problem that warrants immediate attention and correction (e.g., through off-site monitoring). They are also imposed when a bank appears unable or unwilling to efficiently address either detected deficiencies or previously identified but unaddressed weaknesses. Conversely, banks with unfavorable CAMELS ratings might still not receive formal enforcement actions if specific circumstances argue strongly against it (e.g., implementation of a thorough corrective plan that is expected to result in significant improvement).[6]

Based on our reading of the rationales for all the formal enforcement actions during 2001-2010, we use only those that relate to the financial safety and soundness of lead arrangers; in general, these are the most important formal enforcement actions bearing the highest reputational cost. We provide a detailed discussion of our selection of enforcement actions according to their rationale in Appendix A1. Our guidance for this selection is the internal

[6] See Fed (2012), s. 5040.1; OCC (2007), pp. 46-47; FDIC (2012), s.15.1.

taxonomy of the so-called "prudential requirements" as set out in the Basel Committee Core Principles for Effective Banking Supervision (Basel, 2012). The first set of principles covers capital adequacy, asset quality, loan-loss provisions and reserves, large exposures, and exposures to related parties (principles 16, 18-20), thus corresponding to enforcement actions tightly related to safety and soundness. A second group of principles pertains to internal control and audit systems, as well as to management information and risk-management arrangements (principles 14-15, 26). Thus, we also include in our empirical analysis enforcement actions related to these issues. At an ancillary level, enforcement actions against board members, senior management, and persons closely connected to the bank (institution-affiliated parties) might also reflect overly risky strategies and operational risk. We include them in the empirical analysis, even though we also conduct sensitivity tests without them.

Subsequently, we drop all the enforcement actions, where there is another important corporate event during the same year for the punished lead arrangers (M&As, either as acquirer or target, liquidations, and failures). This practice cleans the effects of the enforcement actions from these other events. We further drop enforcement actions and banks that received an enforcement action more than once in the same year (3 cases in total). One reason is to avoid issues regarding changes in bank fundamentals and the strategic behavior of banks after the first enforcement action. This would lead to a less clean identification of the reputation effect of the second enforcement action. We do, however, conduct sensitivity tests in this respect because the reputation effect might be stronger for repeated offenders.

3.2. Cost of borrowing

The outcome variables of our analysis characterize various price lending terms. Lenders generally use a menu of spreads and different fee types rather than a single price measure to ensure an appropriate expected return (Berg et al., 2016). Thus, we use a number of different

variables, which include all-in-spread drawn (*AISD*), defined as spread plus facility fee, the all-in-spread undrawn *(AISU)*, the annual fee paid on the entire committed amount (*Facility fee*), the fee paid on the unused amount of loan commitments (*Commitment fee*), and the fee paid on amounts drawn on the letter-of-credit sublimit (*Letter-of-credit fee*).[7] All of these spreads and fees characterize the total price (or cost) of lending, which we construct following Berg et al. (2016) and use in the empirical analysis as our main dependent variable (*Total cost of borrowing*).[8] For more details, see Table 1.

3.3. Control variables

We use a number of control variables at the loan level. We control for loan maturity (*Maturity*), loan size (*Loan size*), the number of financial and general covenants in the loan contract (*Total covenants*), and whether the loan facility requires collateral (*Collateral*) and performance pricing provisions (*Performance provisions*). These non-price terms still entail a cost for the loan, but this cost is not explicitly priced. Moreover, *Total covenants*, *Collateral*, and *Performance provisions* relate explicitly to the security of the loan facility after its origination and thus to the minimization of informational asymmetries (mainly moral hazard) in loan contracts. These loan characteristics are also valuable to saturate the model from alternative explanations of the findings, such as banks leaning toward at least some loans with more loan guarantees and lower direct costs after the enforcement action.

[7] The traditional loan price measure, *AISD*, only considers the simple spread and the facility fee, while largely ignoring other fee components, such as the upfront fee, commitment fee, and letter-of-credit fee. Consider, for example, the case of a term loan; in addition to the annual spread, a borrower has to pay a one-time upfront fee on the total borrowed amount in most cases, as well as a facility fee in few cases (<10% of all syndicated loans). Therefore, for a term loan, fully relying on *AISD* will always understate the true price of the loan. Another stylized fact in the syndicated loan market is that only a fraction (usually 57%) of borrowers' credit lines is actually drawn down, and the rest is often used for letters of credit. Therefore, for a typical revolver loan, the total price can be higher or lower than the *AISD*, depending on the loan spread paid on the fraction of credit drawn and various fees on the drawn or undrawn components (Berg et al., 2016).

[8] One potentially important fee in our analysis is the upfront fee. The reason is that this fee, or at least a disproportionally large amount of it, usually only goes to the lead arranger. However data on upfront fees is scarcer in Dealscan and we do not have enough observations to run a meaningful regression.

We also control for fixed effects based on the purpose of the loan (e.g., corporate purposes, working capital, takeovers or acquisitions, debt repay, etc.), and for whether the lead arranger has lent to the same borrower in the last five years (*Relationship lending*) to account for the strength of the relationship between a lender and a borrower. Importantly, we use a dummy variable to distinguish between a term loan and a revolver (the dummy is named *Term loan*). This distinction is important, especially given that 76% of the term loans do not have any fee (see also Berg et al., 2016).

We do not need controls at the firm-year level, as we will saturate our baseline model with firm*year fixed effects. At the bank level, we control for proxies of CAMELS ratings that usually change pre- and post-enforcement. *Bank capital* is the ratio of Tier 1 plus Tier 2 bank capital to total risk-weighted assets. *Bank non-performing loans* is the ratio of non-performing loans to total loans. *Bank return on assets* is the ratio of banks' profits before taxes to total assets. *Bank liquidity* is the ratio of cash plus reserves plus short-term (up to three-month) government securities to total assets. We also control for potential changes in bank funding costs pre-post enforcement as well as for changes in (see Table 1 for variable definitions).

4. Identification strategy

Table 3 provides a first indication for a significant decrease in either *AISD* or the *Total cost of borrowing* post enforcement. The facility fee is also smaller and maturity longer, whereas the changes in the rest of the fees are statistically insignificant. These are first-hand results that loan spreads improve for borrowers after the enforcement actions; it remains to be examined whether there is a causal effect running from the actions to the lending terms and pinpointing the channels through which this happens.

[Insert Table 3 about here]

To this aim, the general form of the empirical model is:

$$CB_{lbft} = a_0 + a_1 Enforcement\ action_{bt} + a_2 L_{lt} + a_3 F_{ft} + u_{lbft} \quad (1)$$

In equation (1), *CB* represents the cost of borrowing *l*, granted by lead bank *b* to firm *f* in year *t*. Further, *L* is the vector of loan characteristics used as control variables, and *F* denotes a set of fixed effects or bank control variables to be included where appropriate. Also, *u* is the remainder stochastic disturbance.

Enforcement action is a dummy variable equal to 1 for all loans originated by a punished lead bank in the period after the date[9] of the enforcement action and 0 otherwise (i.e., for the loans originated from all other banks or the punished bank before the enforcement action). Let us consider an example for increased clarity. Assume that on September 1, 2010, there is an enforcement action to the lead bank b1 of the loan syndicate. Then, *Enforcement action* takes the value 1 for the loans originated by bank b1 from September 1, 2010 onward and 0 before that date. If another bank b2 is also a lead arranger, then as long as b1 is a lead bank and this loan is originated post-enforcement, *Enforcement action* equals 1. However, for loans originated by bank b2 but not bank b1 post-enforcement, *Enforcement action* equals 0.

Consistent with Berg et al. (2016), 74% of all the term loans in our sample do not have any fee, implying a substantial difference between term loans and credit lines. To account for a potentially different effect of enforcement actions on the cost of credit lines *versus* term loans, we include in the loan spread specifications (when dependent variable is either *AISD* or *Total cost of borrowing*) the interaction term *Enforcement action***Term loan*. For the specifications on loan fees, we drop term loans from our sample.

[9] This is the release date on the site of the supervisors. Supervisors must preserve the confidentiality of the actions until they are publicly announced. Among many other documents and agreements, please refer to the Interagency Advisory on the Confidentiality of the Supervisory Rating and Other Nonpublic Supervisory Information (2005), available at https://www.federalreserve.gov/boarddocs/press/bcreg/2005/20050228/attachment.pdf).

Our empirical model does not in principle suffer from selection bias: the enforcement actions do not occur because of the terms of a particular loan.[10] In contrast, our approach does potentially suffer from two other identification challenges. The first is that enforcement actions are not random events but constitute a "treatment" for specific violations of laws and regulations. Here, we seek to identify the effect of these events and not the effect from other (possibly unobserved) bank or firm characteristics that change between the pre- and post-enforcement periods. Second, we seek to distinguish between the risk-taking effect and the competition-reputation effect.

To remedy these identification problems we exploit the structure of our data set. First, we use firm*year fixed effects, which saturate the model from any time-varying observed and unobserved firm (demand-side) characteristics that might affect the terms of lending. Further, and equally important, the firm*year fixed effects reflect the time-varying riskiness of firms and associated risk-taking incentives of banks, which we aim to fully control for to identify the competition-reputation effect. In turn, in robustness tests, we use bank*year fixed effects, which control for the reasons of the enforcement action before the enactment, as well as for the possible general response of the punished banks to the penalty (in terms of capital, general credit risk, corporate governance, liability structure, etc.).[11] An even more stringent identification method is to additionally include bank*firm fixed effects. This implies

[10] One argument against this premise is that the average loan pricing of punished banks before the enforcement action could cause (at least in part) the enforcement action. The bank*year fixed effects and the IV strategy discussed below should mitigate any such problems of reverse causality, if present.

[11] The bank*year fixed effects are not perfectly collinear with Enforcement action. There are two sources of variation. The first source is that enforcement actions take place at specific dates within year, allowing within-year (before-after the action) variation for those punished lead banks originating loans both before and after the action. This holds for almost all lead banks in our sample. The second source of variation comes from lead banks being part of multiple loan deals. Thus, a non-punished lead bank can be a member of a syndicate for a "punished loan" (that is a loan for which one of the lead banks is punished) but also part of a syndicate of a non-punished loan (where none of the syndicate's lead banks are punished in the same year). This again creates within-year variation because the same bank originates punished loans and non-punished loans. Note that the second source of variation implies that even loan pricing is different because we are exploiting variation from different loans. In Appendix A.2, we further explain this issue with the help of a data-based example.

considering only the loan facilities where the lead arrangers and borrowing firms are the same pre- and post-enforcement.

In this study we are interested in the effect of the enforcement actions *once these have been enacted* and not the effect stemming from loans given by other banks that perhaps should have been punished but were not. However, we also conduct a test to identify if our results change when allowing for such an effect (also potentially including anticipation effects by bank). In particular, we use an IV method and the instrument proposed by Delis et al. (2017) in their study of the effect of enforcement actions on bank risk. We use data from FedScope Employment Cube, which includes information on the gender of the bank examiners on a yearly basis.[12] We calculate the ratio of female bank examiners to the total number of the bank examiners for each supervisor per state and year, and match states with bank headquarters. The resulting variable is further refined to exclude the positive trend in the growth of female bank examiners over time and local socioeconomic effects. This is done by regressing the average gender variable on an annual trend and state fixed effects. The residuals are our instrumental variable, which we term *Examiners' gender*.

The theoretical basis for our instrument comes from separate corporate governance and psychology literatures (e.g., Adams and Ferreira, 2009; Friesdorf et al., 2015). Female employees are found to be more diligent in screening activities and have an inclination toward moral reasoning. In the bank examination field, this would imply more diligent auditing of banks and higher probability of enforcement. In contrast, we expect that our instrument would have an effect on the cost of borrowing only through the supervisory process, especially after extracting the state fixed effects and the time trend from the original variable.[13]

[12] Unfortunately, this information is only available for the FDIC and the OCC, and not for the FRB; this results to a loss of observations.

[13] As Delis et al (2017). note, a potential limitation of this instrumental variable is that it is observed at the state-year level, while the formal enforcement actions are observed at the bank-year level. As a result, the instrumental variable could capture artificial correlations for clusters of banks within states. They suggest that clustering standard errors by bank should limit this bias to a considerable extent. Here, this is even less worrisome given the

The particular IV model takes the form:

$$EA_{bt} = b_0 + b_1 z_{st} + b_2 B'_{b,t-1} + e_{it}, \quad (2)$$

$$CB_{lbft} = a_0 + a_1 \widehat{EA}_{bt} + a_2 L_{lt} + a_3 B_{b,t-1} + a_4 F_{f,t-1} + u_{lbft}. \quad (3)$$

In equation (2) z is *Examiners' gender* at state s and year t and B' is a vector of bank-level variables affecting the probability of receiving an enforcement action. The system of equations (2) and (3) is not the usual two-stage least squares (2SLS) model in the sense that not all variables of the 2nd stage are included in the 1st stage (Baltagi, 2008, refers to this as feasible 2SLS). We use this model because the loan-level controls do not significantly explain the probability of receiving an enforcement action and simply overidentify the model and increase the estimation bias.[14] As most of the enforcement actions received for safety and soundness reasons reflect inferior CAMELS ratings, we include related variables in the vector B' in equation (2). Specifically, we use *Bank capital*, *Non-performing loans*, *Bank Z-score*, and *Bank liquidity*. All these variables are lagged once (information from previous quarter) to reflect the punished bank's financial health in the quarter prior to the enforcement action.

5. Empirical results

5.1. Baseline results

In Table 4 we report results from the estimation of equation (1) using OLS and firm*year fixed effects (along with loan type and loan purpose fixed effects). The lower part of the table reports the number of different clusters (loan facilities, banks, and firms), the control variables, and fixed effects. We cluster standard errors by bank. The general finding is that enforcement actions negatively and significantly, both statistically and economically, affect the price terms of lending.

fixed effects used in the second stage of our model that would capture any such common effects. Also, clustering the standard errors by state does not affect our results.

[14] To apply the correct mean squared error, we correct the variance-covariance matrix as in http://www.stata.com/support/faqs/statistics/instrumental-variables-regression/.

Specifically, an enforcement action lowers the *AISD* of the average loan facility by approximately 23 basis points. We observe equivalent reductions in *Total cost of borrowing* (a 22-basis point reduction). The interaction term *Enforcement action*Term loan* suggests that enforcement actions do not have a significantly different effect on the pricing of term loans compared to the rest of the loans.

These effects are economically large. Given that the mean sample loan size is $508 million and the average loan's time to maturity is around 4.1 years, if a bank is punished, the interest income it receives from each loan will fall by approximately $4.6 million (=508× 0.0022×4.1). Thus, enforcement actions have a large impact on the cost of borrowing, which comes over and above changes in punished banks' risk-taking post-enforcement (captured by the firm*year fixed effects). Our results imply a considerable improvement in the competitiveness of loan pricing.

Notably, the effect on the total cost of borrowing comes primarily from the reduction in the loan spread and not so much from reduction in the fees. We observe that the response of *AISU* is negative, but mainly driven by the commitment fee. The effects on the rest of the fees are statistically insignificant.

[Insert Table 4 about here]

In Table 5 we rerun the same regressions also including bank*year fixed effects. To further increase variation in our empirical setting, we include the interaction term *Enforcement action*Bank capital* (along with triple interactions with *Term loan* where applicable). We expect that the competition-reputation effect will be less significant for banks with substantial capital. The results are in line with Table 4, showing that regulatory enforcement exerts a significant effect, mainly on loan spreads. In line with our expectations, we also find that for banks with a higher capital ratio the effect is lower. We obtain similar results when the interaction term is with *Non-performing loans* (instead of *Bank capital*).

[Insert Table 5 about here]

In Table 6 we include bank*firm fixed effects, essentially holding the lender and the borrower constant. Using this approach inevitably reduces the number of clusters (loan facilities, banks, and firms). The results are still statistically and economically significant, and even more potent compared to the ones in Table 4. Specifically, the effects of an enforcement action on *AISD* and *Total cost of borrowing* are 26.2 and 25.6 basis points, respectively. Again, most of the effect comes from loan spreads and not the fees.

[Insert Table 6 about here]

In Table 7, we turn to the results from the IV estimation of equations (2) and (3). We conduct this analysis only for banks supervised by the FDIC and OCC banks, given the unavailability of the instrument for the Fed-supervised banks. Column (1) reports the results from the first stage, which is the same for all the second-stage regressions in columns (2) to (7). Similar to Delis et al. (2017), the first-stage results show that *Examiners' gender* is a statistically significant predictor of *Enforcement action* and with the expected sign: higher shares of female directors in local supervisor offices increase the probability of a bank in their jurisdiction to be punished. The effect of the bank-level controls is also strong and in line with expectations. Banks with a higher capital ratio, Z-score, and liquidity are less likely to receive an enforcement action, while banks with higher shares of non-performing loans are more likely to receive an enforcement action.

[Insert Table 7 about here]

The second-stage results are equivalent to the results of Table 4, albeit with an increase in the responses. Specifically, the effect of *Enforcement action* on *AISD* is 24 basis points, and the respective effect on the *Total cost of borrowing* is approximately 25 basis points. Further,

besides the commitment fee, the facility fee is also significantly lower following the enforcement action.[15]

Our baseline results are robust to several robustness tests. First, the enforcement actions strictly related to the bank as a whole entity (first two types according to Table A.1 in the Appendix) and not to individuals (board members, senior management, and other persons associated with the punished bank) should carry more weight in terms of reputation. The Basel committee specifically mentions the first two types of enforcement actions as integral parts of the supervisory mechanism to assist financial safety and soundness of banks. Having controlled for the risk-taking mechanism, especially via the firm*year fixed effects, the Basel-related enforcement actions should yield stronger competition-reputation effects.

Therefore, in the analysis of Table 8, we reconstruct *Enforcement action* to include information only from the first two types of actions. If these actions are positively correlated with reputational burden, we expect to find stronger effects on the cost of borrowing, following the competition-reputation mechanism. This is indeed the case. The *AISD* falls by approximately 30 basis points and the total cost of borrowing by approximately 27 basis points. Even the fees are significantly lower following Basel-related enforcement actions, suggesting a significant reduction in the cost of borrowing stemming also from lower fees.

[Insert Table 8 about here]

[15] There are at least three possible explanations behind the increased potency of the effects observed in Table 7 *vis-à-vis* those in Table 4. An economic explanation is that the models in Table 7 also include information from banks that, according to the first stage of the model, should have been punished but were not. In this paper, we are mostly interested in the effect of the enforcement action *per se* and in this regard the results of Table 4 are more appealing, especially as they control for the reasons of the treatment *via* the bank*year fixed effects. A second economic explanation is that the effect of enforcement actions after dropping banks supervised by the Fed (our IV is not available for these banks) is more potent. A statistical explanation is that by dropping singleton observations when including the high-dimensional fixed effects, we also lose some valuable information regarding the effect of enforcement actions. However, and more importantly, we can still conclude that there is a strong negative causal effect of regulatory enforcement actions on the cost of bank loans. For the size of this effect, we use the more conservative results from Table 4 as benchmark and, following the discussion in this paragraph, we note that if anything our benchmark results are downward biased.

Second, in Table 9 we artificially slide the enforcement action dummy two (Panel A) or four (Panel B) months before the actual enforcement date. This constitutes a placebo test to show that what matters is the announcement date of the enforcement action, and anticipation effects are not driving our results. We indeed find that the effect of the enforcement actions dummy becomes statistically insignificant. Moreover, in the Appendix A.3 (Tables A.2-A.4), we show that our results are robust to (i) the use of a clean from other corporate events window for all banks in our sample (punished and non-punished), (ii) including the few cases with repeated enforcement actions within the enforcement year, (iii) setting *Enforcement action* equal to 0 for banks receiving TARP funds during the enforcement year, (iv) weighting enforcement actions with punished bank's share of the loan, and (v) introducing interaction terms with supervisor dummies (no differences found).

[Insert Table 9 about here]

Overall, our findings have important economic implications. Given the inclusion of firm*year effects that saturate the model from banks' propensity to reduce their overall risk-taking post-enforcement, as well as the robustness tests, we must attribute the negative effect to the reputation-competition effect. The supervisory interventions lead to significant reduction in the pricing of large loan contracts and thus to improved competitiveness in this market. This is especially true as it is unlikely that syndicated loan contracts are unprofitable and banks make losses post-enforcement. In turn, the results highlight that enforcement actions issued for safety and soundness reasons work as a positive externality for borrowers' costs of financing and provide evidence against the leviathan view of regulatory intervention, at least for pricing corporate loans. Thus, given the nature of most syndicated loans, our results suggest that supervisory intervention brings enhanced investment and growth opportunities for borrowers, improving economic efficiency and welfare, *ceteris paribus*.

5.2. The role of market power

In the bulk of our analysis so far, we shut down the risk-taking effect and attribute the remainder negative effect of enforcement actions to the competition-reputation effect. A potential criticism to this approach is that other unknown channels, via which enforcement actions might affect the cost of borrowing, drive the results. In this section, we provide a procedure to further back up the competition-reputation storyline and exclude effects from other unknown channels.

First, we show that enforcement actions lower the price-cost margin (Lerner index) of punished banks post-enforcement. The Lerner index is the most commonly used measure of market power and shows deviations of bank lending rates from the marginal cost (competitive pricing); thus, higher values indicate higher market power or less competitive pricing. For the estimation of the Lerner index, we use the approach described in Appendix A.4.

In column (1) of Table 10 we report the results from the effect of *Enforcement action* on the *Lerner index*. The results show that an enforcement action significantly reduces the Lerner index of the punished banks by approximately 0.030 points. Given that the mean Lerner index in our sample equals 0.10, this is an economically large decrease.

[Insert Table 10 about here]

Having established that the Lerner index is lower post-enforcement, we include this index in equation (1). If our theoretical considerations are precise, we should expect that the Lerner index enters equation (1) with a positive and significant coefficient, while absorbing most of the significance of *Enforcement action*. The results (column 2) show that the coefficient on *Enforcement action* is statistically insignificant. In turn, the coefficient on the Lerner index is positive and statistically significant, reflecting that lower market power is associated with lower cost of borrowing.

Given the role of market power from the banks' viewpoint, an interesting extension is to study the role of firm market power. In column (3) of Table 10, we introduce an interaction

term between *Enforcement action* and *Firm Tobin's q*. The coefficient on *Enforcement action* remains negative and significant, showing that punished loans have a lower *AISD* by approximately 15 basis points compared to non-punished loans. The interaction term *Enforcement action*Firm Tobin's q* shows that this effect is significantly higher for firms with high q. This implies that firms with high market valuation and potentially high market power see a larger reduction in their cost of borrowing post-enforcement, a finding consistent with the competition-reputation effect.

5.3. Effects of enforcement actions on firm stock prices

The positive real effects on firms implied by the lower cost of borrowing, open up the possibility to examine how markets perceive these effects. We use a differences-in-differences model, comparing the differential effect on the stock price of firms that received a loan from a punished bank after the enforcement action to the stock price of firms that received loans from non-punished banks. The estimated model takes the form:

$$r_{f,t+1} = c_0 + c_1 Loan + c_2 EA_{bt} + c_3 Loan * EA_{bt} + c_4 C + w_{ft}, \quad (4)$$

where *r* is the stock return of firm *f* in the three months after loan origination (see also Table 1), *Loan* equals 1 if the firm obtained a loan from any bank after the enforcement action (0 otherwise), and EA takes the value 1 if the firm obtained a loan from a punished lead bank (and 0 otherwise). *C* and *w* are firm controls and the disturbance, respectively. A positive and significant coefficient c_3 shows that the effect of obtaining a loan on stock returns is more potent for the firms obtaining funding from a punished bank.

We report the results in column (1) of Table 11. The estimate on c_1 shows that firms receiving a loan see an average increase in their stock price by 0.7% (Li and Ongena, 2015, report similar results). Importantly, this effect is significantly higher (by another 0.8%) for the

firms receiving loans from punished banks, potentially reflecting the lower average cost of borrowing.

[Insert Table 11 about here]

With the results of column (1), we are *a priori* agnostic about the effect of the actual cost of borrowing on stock returns, and let the estimates show if stock returns are higher or lower for credit-receiving firms in general and for firms obtaining credit from punished banks in particular. Subsequently, in column (2) we use the *Total cost of borrowing* instead of *Loan*. The coefficient on the interaction term *Total cost of borrowing*EA* shows that the larger the decrease in *Total cost of borrowing* from punished banks, the higher the stock price increase. This analysis provides further evidence on positive effects of enforcement actions for the borrowing firms via decreases in the cost of borrowing.

6. Conclusions

In this paper, we ask whether formal enforcement actions against banks for violations of laws and regulations related to safety and soundness affect the cost of borrowing. The answer to this question has clear economic implications regarding the effect of regulatory enforcement on the competitive conditions in the market for corporate loans. Using a novel data set with merged information on hand-collected data on enforcement actions, DealScan data on syndicated loans, and bank-level data from the Call Reports, we find that the pro-intervention view clearly dominates. In particular, the cost of borrowing is significantly lower even for the same firms after an enforcement action against a lead bank of a syndicate compared to the terms on loans originated before the enforcement action. These results are corroborated by a series of identification tests and model re-specifications, and highlight a competition-reputation effect that is at work over and above the risk-taking and other changing incentives of punished banks post-enforcement.

Our policy implications suggest that formal bank regulatory intervention for safety and soundness reasons, as well as close inspection of the implementation of rules and regulations, does not lead banks to pass enforcement costs to corporate customers. In contrast, we find that intervention leads to increased competitiveness in the syndicated loan market, and thus improves economic welfare, especially as syndicated loans are mostly originated for investment purposes. Thus, we provide a strong story of success of banking regulation and its enforcement.

The results also have implications for new legislation. Our findings reveal quite clearly that what matters for the efficient allocation of credit is the actual implementation of law on the books. This calls for new thinking about regulatory design, especially for those policy initiatives and regulations that might impose new and perhaps unnecessary costs on financial intermediation. What matters most is clearly effective supervision and not more stringent laws.

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Table 1. Variable definitions and sources

Variable	Description	Source
A. Dependent variables: Price terms		
AISD	All-in-spread-drawn, defined as the sum of the spread over LIBOR plus the facility fee.	DealScan
Total cost of borrowing	For term loans: Total cost of borrowing = Upfront Fee / Loan Maturity in Years + (Facility Fee + Spread) + Prob(Utilization>UtilizationThreshhold | Usage > 0) x Utilization Fee + Prob(Cancellation) x Cancellation Fee For revolvers without letter of credit: Total cost of borrowing = Upfront Fee / Loan Maturity in Years + (1-PDD) x (Facility Fee + Commitment Fee) + PDD x (Facility Fee + Spread) + PDD x Prob(Utilization>UtilizationThreshhold | Usage > 0) x Utilization Fee + Prob(Cancellation) x Cancellation Fee where PDD is the likelihood that the credit line is drawn down; Prob(Utilization>UtilizationThreshhold | Usage > 0) is the probability that the utilization of the credit line is higher than the thresholds specified in the loan contract conditional on observing utilization. Prob(Cancellation) is the probability that the loan is going to be cancelled. We follow the program/code in the website of Berg, Saunders and Steffen (2016) to calculate the measure.	DealScan and own calculations
AISU	All-in-spread-undrawn, defined as the sum of the facility fee and the commitment fee.	DealScan
Facility fee	Annual fee paid on the entire committed amount, regardless of usage.	DealScan
Commitment fee	Commitment fee paid on the unused amount of loan commitments.	DealScan
Letter-of-credit fee	Fee paid on drawn amounts on the letter-of-credit sublimit.	DealScan
B. Instrumental variable		
Examiners' gender	This variable is constructed by first calculating the ratio of female to the total of bank examiners by state and year (quarter from 2007Q3 onward). Data are from the FedScope Employment Cubes, and are available online from the Office of Personnel Management. Subsequently, we use this as dependent variable with a linear trend and state fixed effects as explanatory variables. We use the residuals from these regressions as an instrumental variable in equation.	Regulators' websites
C. Explanatory variables		
Enforcement action	This is a dummy variable taking the value 1 for all loans originated by a punished bank after the date of the enforcement action and 0 otherwise (i.e., for the loans originated from all other banks or the punished bank before the enforcement action). See Table A.2 for more on enforcement actions. Alternatively, in one of the sensitivity tests (Table 11) we multiply the dummy variable with the share of the loan held by the punished lead arranger.	Regulators' websites
Maturity	Facility duration in log months.	DealScan
Loan size	The log of the loan facility amount in millions of dollars.	DealScan
Total covenants	The total number of general and financial covenants in the loan contract.	DealScan
Collateral	Dummy equal to 1 if the loan is secured, 0 otherwise.	DealScan
Relationship lending	Dummy equal to 1 if the lead arranger lent to the same borrower in the five years before the current loan, 0 otherwise.	DealScan
Performance provisions	Dummy variable equal to 1 if the loan has performance pricing provisions, 0 otherwise.	Dealscan
Term loan	A term loan is one where a firm borrows a certain amount for a certain length of time. The firm pays off the loan by the time the term ends. A loan is a term loan if its loan	DealScan

	type is one of the following: "Term Loan", "Term Loan A"-"Term Loan H", or "Delay Draw Term Loan".	
Bank capital	The ratio of total risk-based capital ratio to total risk-weighted assets.	Call Reports
Bank loans	The ratio of total loans to total assets.	Call Reports
Non-performing loans	The ratio of non-performing loans (90 days+) to total loans.	Call Reports
Bank Z-score	(Return on assets + Bank capital)/(σReturn on assets), where σReturn on assets is calculated over a 5-year horizon on quarterly data.	Own calculations based on Call Reports data
Bank liquidity	The ratio of cash and short-term securities to total assets.	Call Reports
Bank funding costs	The ratio of total interest expenses (for deposits and other liabilities) to total liabilities.	Call Reports
Bank deposits	The ratio of total deposits to total assets.	Call Reports
Lerner index	$LI_{bt} = \frac{P_{bt} - MC_{bt}}{P_{bt}} W_{bt}$, where P and MC are the price of bank output at quarter t and the marginal cost of the production of this output weighted by the shares of each bank W in the syndicated loan (equal shares are imposed where this information is not available). Marginal cost is estimated using a log-linear production function and total output is measured by total earning assets.	Own estimations (see Appendix A.4) based on Call Reports data
Firm stock returns	Cumulative returns (or compound return) for the 3 months forward the enforcement action	CRSP
Firm Tobin's q	The firm's market to book value.	Compustat
Firm ROA	The ratio of pre-tax profits to total assets.	Compustat
Firm leverage	The ratio of total debt to total assets.	Compustat
Firm size	The natural logarithm of total assets.	Compustat

Table 2. Summary statistics

The table reports basic summary statistics (mean, median, standard deviation, quarter centiles, minimum and maximum) for the main variables used in the empirical analysis. The sample corresponds to the full sample of 70,441 loan facilities.

Variable	Mean	Median	Std. deviation	25th percentile	75th percentile	Min.	Max.
A. Dependent variables: Price terms							
AISD (bps)	145.82	125	118.7	55	200	0	1,100
Total cost of borrowing (bps)	113.59	87.9	105.28	40.65	156.88	0	1,100
AISU (bps)	17.02	11.75	19.19	0	25	0	200
Facility fee (bps)	3.68	0	7.65	0	5.5	0	75
Commitment fee (bps)	13.71	0	20.14	0	25	0	200
Letter-of-credit fee (bps)	63.11	0	93.91	0	112.5	0	625
B. Instrumental variable							
Examiners' gender	0.34	0.34	0.042	0.33	0.37	0.125	0.50
C. Explanatory variables							
Enforcement action	0.58	1	0.49	0	1	0	1
Maturity (month)	49.59	60	20.45	36	60	0	121
Loan size ($mil)	507.9	250	784.75	100	600	0.22	10,000
Total covenants	5.22	5	4.08	0	8	0	17
Collateral	0.47	0	0.5	0	1	0	1
Relationship lending	0.52	1	0.5	0	1	0	1
Performance provisions	0.33	0	0.47	0	1	0	1
Term loan	0.22	0	0.42	0	0	0	1
Lerner index	0.10	0	0.06	0.02	0.26	-0.18	0.79
Firm stock returns	0.024	0.074	0.14	-0.050	0.127	-0.67	0.66

Table 3. Means of the price terms of lending before and after the enforcement action

The table reports means of the price lending terms (dependent variables of our study) before and after the enforcement action for the banks that received an enforcement action. It also reports the difference between the two regimes and the respective t-stat, with the ***, **, and * marks denoting statistical significance at the 1%, 5%, and 10% levels.

	Before enforcement	After enforcement	Difference	t-stat
AISD (bps)	150.98	138.56	12.42	5.27***
Total cost borrowing (bps)	117.31	107.80	9.51	4.11***
AISU (bps)	17.68	16.80	0.88	1.72*
Facility fee (bps)	5.19	4.23	0.96	2.28**
Commitment fee (bps)	12.98	12.99	-0.01	0.18
Letter-of-credit fee (bps)	61.65	59.62	2.03	1.19
Loan size ($mil logs)	4.25	4.28	-0.03	0.08
Maturity (month)	43.98	48.34	-4.36	2.19**

Table 4. Regressions with firm*year fixed effects

The table reports coefficients and t-statistics (in parentheses). The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table 1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank. The lower part of the table indicates the control variables and the fixed effects included in the specifications. It also indicates the number of loan facilities, the number of banks, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	AISD	Total cost of borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-23.370***	-22.220***	-3.017***	-0.460	-2.790***	-0.877
	[-2.801]	[-2.745]	[-3.011]	[-0.932]	[-3.520]	[-0.408]
Term loan	23.563***	37.879***				
	[3.711]	[5.635]				
Enforcement action*Term loan	-3.120	-2.728				
	[-0.761]	[-0.630]				
No. of loan facilities	44,025	44,025	30,377	30,377	30,377	30,377
No. of banks	755	755	678	678	678	678
No. of firms	7,327	7,357	5,143	5,143	5,143	5,143
Adjusted R-squared	0.385	0.407	0.360	0.117	0.271	0.325
Loan controls	Y	Y	Y	Y	Y	Y
Bank controls	Y	Y	Y	Y	Y	Y
Loan purpose effects	Y	Y	Y	Y	Y	Y
Firm*year effects	Y	Y	Y	Y	Y	Y
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Table 5. Including bank*year fixed effects and interactions with bank capital

The table reports coefficients and t-statistics (in parentheses). The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table 1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank. The lower part of the table indicates the control variables and the fixed effects included in the specifications. It also indicates the number of loan facilities, the number of banks providing at least two loans within a year, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	AISD	Total cost of borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-20.341***	-19.725**	-3.472***	-0.420	-2.847***	-0.749
	[-2.687]	[-2.637]	[-3.126]	[-0.728]	[-3.716]	[-0.560]
Term loan	20.359***	34.691***				
	[3.307]	[4.816]				
Enforcement action*Bank capital	9.511**	9.366**	1.146*	0.088	1.029*	0.311
	[2.049]	[2.014]	[1.858]	[0.145]	[1.663]	[0.678]
Enforcement action*Term loan	-2.677	-2.345				
	[-0.829]	[-0.726]				
Bank capital*Term loan	2.228	2.895				
	[0.482]	[0.503]				
Enforcement action*Bank capital *Term loan	0.633	0.745				
	[0.367]	[0.526]				
No. of loan facilities	40,701	40,701	28,083	28,083	28,083	28,083
No. of banks	698	698	611	611	611	611
No. of firms	6,740	6,740	4,503	4,503	4,503	4,503
Adjusted R-squared	0.689	0.614	0.366	0.311	0.375	0.220
Loan controls	Y	Y	Y	Y	Y	Y
Bank controls	N	N	N	N	N	N
Loan purpose effects	Y	Y	Y	Y	Y	Y
Firm*year effects	Y	Y	Y	Y	Y	Y
Bank*year effects	Y	Y	Y	Y	Y	Y
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Table 6. Regressions with bank*firm fixed effects

The table reports coefficients and t-statistics (in parentheses). The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table 1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank. The lower part of the table indicates the control variables and the fixed effects included in the specifications. It also indicates the number of loan facilities, the number of banks providing at least two loans within a year, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	AISD	Total cost of borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-26.248***	-25.625***	-4.485***	-0.340	-2.649***	-0.630
	[-3.018]	[-2.938]	[-4.228]	[-0.618]	[-3.274]	[-0.315]
Term loan	27.605***	38.690***				
	[3.940]	[5.712]				
Enforcement action*Term loan	-4.186	-3.390				
	[-0.843]	[-0.716]				
No. of loan facilities	38,883	38,883	26,824	26,824	26,824	26,824
No. of banks	666	666	582	582	582	582
No. of firms	6,542	6,542	4,374	4,374	4,374	4,374
Adjusted R-squared	0.528	0.510	0.298	0.285	0.306	0.157
Loan controls	Y	Y	Y	Y	Y	Y
Bank controls	Y	Y	Y	Y	Y	Y
Loan purpose effects	Y	Y	Y	Y	Y	Y
Firm*year effects	Y	Y	Y	Y	Y	Y
Bank*firm effects	Y	Y	Y	Y	Y	Y
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Table 7. IV regressions

The table reports coefficients and t-statistics (in parentheses). The dependent variable of each specification is shown on the first line of the table. The first column reports the results from the first stage regression (probit regression). The variables are defined in Table 1. All regressions are estimated with an IV fixed-effects model, with robust standard errors clustered by bank. The results on the first stage are reported in column (1). It also indicates the number of loan facilities, the number of banks providing at least two loans within a year, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

First stage		Second stage						
	(1)		(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Enforcement action		AISD	Total cost borrowing	AISU	Facility fee	Commitment fee	Letter of credit fee
Examiners' gender	9.929***	Enforcement action	-24.240***	-24.551***	-2.477***	-0.905*	-3.140***	-1.169
	[12.921]		[-2.730]	[-2.825]	[-3.695]	[-1.668]	[-4.915]	[-1.287]
Bank capital	-157.758***	Term loan	33.620***	54.338***				
	[-19.558]		[3.316]	[5.140]				
Non-performing loans	90.339***	Enforcement	-4.318	-3.849				
	[7.841]	action*Term loan	[-1.172]	[-0.963]				
Bank Z-score	-1.693***							
	[-12.927]							
Bank liquidity	-58.582***							
	[-14.003]							
No. of loan facilities	19,630		19,630	19,630	13,544	13,544	13,544	13,544
No. of banks	212		212	212	196	196	196	196
No. of firms			2,041	2,041	1,403	1,403	1,403	1,403
Loan controls	N		Y	Y	Y	Y	Y	Y
Bank controls	As noted above		Y	Y	Y	Y	Y	Y
Loan purpose effects	N		Y	Y	Y	Y	Y	Y
Year effects	Y		N	N	N	N	N	N
Firm*year effects	N		Y	Y	Y	Y	Y	Y
Clustering	Bank		Bank	Bank	Bank	Bank	Bank	Bank

Table 8. Only actions related to Basel principles

The table reports coefficients and t-statistics (in parentheses). The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table 1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank. The lower part of the table indicates the control variables and the fixed effects included in the specifications. It also indicates the number of loan facilities, the number of banks, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	AISD	Total cost of borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-29.625***	-27.448***	-4.028***	-0.904*	-2.780***	-1.776**
	[-3.387]	[-3.259]	[-3.733]	[-1.678]	[-3.415]	[-2.381]
Term loan	23.516***	36.845***				
	[3.685]	[5.577]				
Enforcement action*Term loan	-3.103	-2.693				
	[-0.755]	[-0.619]				
No. of loan facilities	44,025	44,025	30,377	30,377	30,377	30,377
No. of banks	755	755	678	678	678	678
No. of firms	7,327	7,357	5,143	5,143	5,143	5,143
Adjusted R-squared	0.387	0.411	0.363	0.118	0.275	0.327
Loan controls	Y	Y	Y	Y	Y	Y
Bank controls	Y	Y	Y	Y	Y	Y
Loan purpose effects	Y	Y	Y	Y	Y	Y
Firm*year effects	Y	Y	Y	Y	Y	Y
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Table 9. Anticipation effects

The table replicates the results of Table 4, where the date of the enforcement action is slid two (Panel A) or four (Panel B) months before the enactment date. The table reports coefficients and t-statistics (in parentheses). The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table 1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank. The lower part of the table indicates the control variables and the fixed effects included in the specifications. It also indicates the number of loan facilities, the number of banks, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	(1) AISD	(2) Total cost of borrowing	(3) AISU	(4) Facility fee	(5) Commitment fee	(6) Letter-of-credit fee
	Panel A: Sliding enforcement actions 2 months before enactment					
Enforcement action	-5.142	-5.329	-0.518	-0.328	-1.040	-0.288
	[-1.325]	[-1.405]	[-0.370]	[-0.719]	[-1.416]	[-0.129]
Term loan	24.619***	38.108***				
	[3.807]	[5.918]				
Enforcement action*Term loan	-0.718	-0.384				
	[-0.224]	[-0.257]				
	Panel B: Sliding enforcement actions 4 months before enactment					
Enforcement action	-2.519	-2.255	-0.568	-0.389	-0.815	-0.379
	[-0.946]	[-0.924]	[-0.441]	[-0.811]	[-1.117]	[-0.326]
Term loan	23.582***	376.385***				
	[3.725]	[5.319]				
Enforcement action*Term loan	-0.820	-0.645				
	[-0.306]	[-0.719]				
No. of loan facilities	44,025	44,025	30,377	30,377	30,377	30,377
No. of banks	755	755	678	678	678	678
No. of firms	7,327	7,357	5,143	5,143	5,143	5,143
Loan controls	Y	Y	Y	Y	Y	Y
Bank controls	Y	Y	Y	Y	Y	Y
Loan purpose effects	Y	Y	Y	Y	Y	Y
Firm*year effects	Y	Y	Y	Y	Y	Y
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Table 10. The role of market power

The table reports coefficients and t-statistics (in parentheses). The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table 1. Both regressions are estimated with OLS with robust standard errors clustered by bank. Both specifications (2) and (3) are estimated on the loan-level data set of previous regressions. The lower part of the table indicates the type of fixed effects included in the specifications. It also indicates the number of bank-quarter observations (specification 1), the number of loan facilities, the number of banks, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
Dependent variable:	Lerner index	Total cost of borrowing	Total cost of borrowing
Enforcement action	-0.030***	-5.206	-14.827**
	[-4.180]	[-1.349]	[-2.216]
Lerner index		284.16***	
		[2.696]	
Enforcement action*Tobin's q			-8.235**
			[-2.030]
Bank-quarter observations	29,428		
No. of loan facilities		44,025	44,025
No. of banks	755	755	755
No. of firms		7,327	7,327
Adjusted R-squared	0.204	0.391	0.411
Bank controls	Y	Y	Y
Loan controls		Y	Y
Term loan interactions		Y	Y
Loan purpose effects		Y	Y
Firm*year effects		Y	Y
Bank effects	Y	N	N
Clustering	Bank	Bank	Bank

Table 11. Stock price effects on borrowing firms

The table reports coefficients and t-statistics (in parentheses) from the estimation of equation (4). The dependent variable is Firm stock returns and the firm control variables are listed in Table 1, along with definitions. In specification (1), *Loan* equals to 1 if the firm obtained a loan from any bank after the enforcement action (0 otherwise). In specification (2), *Loan* equals the *Total cost of borrowing*. Both regressions are estimated with OLS with robust standard errors clustered by firm. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)
Enforcement action	-0.0002	0.0004
	[-0.248]	[0.327]
Loan	0.0068**	-0.0007***
	[2.405]	[3.518]
Enforcement action*Loan	0.0079***	-0.0006***
	[3.107]	[-2.718]
Number of firms	4,718	4,718
Adjusted R-squared	0.052	0.055
Firm controls	Y	Y
Clustering	Firm	Firm

Online Appendix
Enforcement of banking regulation and the cost of borrowing

Abstract

This appendix is intended as an online supplement to the analysis presented in the main body of the text. Section A.1 provides the details of the sample of enforcement actions included in the empirical analysis and discusses how we reached the decision to include a specific enforcement action or not after reading its rationale. Section A.2 analyzes why the bank*year fixed effects allow the model to be identified and are not collinear with *Enforcement action*. Section A.3 provides additional robustness tests. Finally, Section A.4 discusses the estimation of the Lerner index.

A.1. More information on formal enforcement actions

Table A.1

Type	Reasons
1	Capital adequacy and liquidity, asset quality, provisions and reserves, large exposures and exposures to related parties
2	Internal control and audit systems, money laundering, bank secrecy, consumer protection and foreign assets control
3	Breaches of the requirements concerning the fitness and propriety of banks' board members and senior management, as well as other persons closely associated with banks (institution affiliated parties)

Each of the three main regulators in the U.S. has its own system to categorize enforcement actions. For example, the Federal Reserve lists seven types of enforcement actions (http://www.federalreserve.gov/apps/enforcementactions/search.aspx), the OCC also lists seven types but these are not precisely the same (http://www.occ.gov/topics/laws-regulations/enforcement-actions/enforcement-actions-types.html), and the FDIC lists 28 types (https://www.fdic.gov/bank/individual/enforcement/edoaction.html).

In this paper, we aim to distinguish between the enforcement actions that are significant enough to have a bearing on the business model of the bank as regards loan pricing and those that affect other parts of the business model of the bank. To this end, we create our own categorization of formal enforcement actions. We suggest that the best possible categorization reflects the internal taxonomy of the so-called "prudential requirements" as set out in the Basel Committee Core Principles for Effective Banking Supervision (Basel, 2012). These enforcement actions are closely related to safety and soundness issues and, according to Delis et al. (2017), are the only ones with a direct impact on the risk-taking behavior of banks. Thus, these are the actions that are important enough to essentially matter as a device affecting reputation, competition, and eventually the terms of lending.

We identify three such types of enforcement actions as reflected in Table A.1. The first covers capital adequacy, asset quality, loan-loss provisions and reserves, large exposures and exposures to related parties (Basel Principles 16, 18-20), thus corresponding to the scope of Type 1 actions in Table A.1. These actions are closely linked to safety and soundness issues and, thus, potentially have a large impact on the reputation of the punished banks and their associated business model.

A second group of enforcement actions (Type 2) concerns the robustness of internal organization procedures, such as internal control and audit systems, as well as management information and risk management arrangements. All of these procedures are clearly defined in Basel's Principles 14-15, 26, as very important procedures mirroring safety and soundness issues, even though not as directly as the procedures yielding Type 1 enforcement actions. Still, the robustness and functionality of these internal procedures are quite important for the reputation of banks and this is why we include them in our analysis.

Formal enforcement actions against board members, senior management and persons closely connected with the bank (institution-affiliated parties) comprise the Type 3 enforcement actions used in our analysis. These actions mainly cover instances of professional incompetence, fraud and insider abuse. The reason we include these actions in our analysis is that they tend to hit the news and, thus, potentially have a reputational impact. However, the association of such enforcement actions with financial safety and soundness could be relatively weak, for several reasons: (a) supervisors are heavily oriented towards addressing concerns regarding the safety and soundness of *ailing banks per se* ("institutional enforcement") and, as a consequence, they give the greatest priority to Type 1 and Type 2 actions rather than to actions against individuals or other institution-affiliated parties; (b) investigation and successful prosecution of fraud and insider abuse cases is extremely complex and time consuming (e.g., involves massive and complicated transactions, records may be poor or even nonexistent, the

effect of white-collar crimes may appear with substantial delays), which also undermines the effectiveness of the relevant actions regarding financial safety and soundness; (c) internal organization inefficiencies lie behind the development of fraud, insider abuse, or even incompetence, hence enforcement actions against institution-affiliated parties are likely to be already captured by the Type 2 formal enforcement actions arguments (Brunmeier and Willardson, 2006; GAO, 1989/4). For these reasons we also conduct sensitivity analysis without Type 3 enforcement actions (i.e., including only the Basel-related Type 1 and Type 2 actions) and show that our results do not change.

There are of course many other types of enforcement actions, which we exclude from our analysis. These can be actions for typical infringements of laws, including, Home Mortgage Disclosure Act and Flood Insurance Act, penalties assessed against a banking organization for the late filing of call reports, denials of acquisition of control for individual managers, denials of section 19 applications (which are only available after 2008), prohibitions to open up new branches, and orders requiring banks to reimburse customers for violations of consumer protection laws. For details, see FDIC's website provided above. Evidently, these penalties would encompass actions with considerably heterogeneous underlying cause and do not relate to financial safety and soundness of banks. On this line, we do not expect that these enforcement actions would have any serious reputational and competition effect on the terms of lending and thus we exclude them from our analysis.

A.2. Inclusion of bank*year fixed effects

Despite bank*year fixed effects there is within bank variation from two sources. The first source is that enforcement actions take place at specific dates within year, allowing within-year (before-after the action) variation for those punished lead banks originating loans both before

and after the action. The second source of variation comes from lead banks being part of multiple loan deals.

As an example, consider the data set attached in the Table below, which replicates the structure of our actual data set. There are 6 banks, each issuing a number of loans over 5 years. There are 25 loans, each issued by at least two lead banks: observations 1 to 25 reflect the *first* lead banks of each loan, observations 25 to 50 reflect the *second* lead banks, and observations 50-75 reflect the *third* lead banks if there are any. The column CB denotes the cost of each loan.

In this simulation, we show three interesting scenarios. In the first scenario, highlighted with green, Bank 1 receives an enforcement action within year 1, just before issuing Loan 3. Loan 3 and every loan from Bank 1 from the enforcement date onward, takes the value 1. However, Loan 3 is also issued by Banks 3 and 4 (observations 28 and 53), for which only Loan 3 takes the value 1, and not other loans from Banks 3 and 4 post enforcement. Thus, there is additional within-year variation coming from other loans issued by Banks 3 and 4.

In the second scenario, highlighted with blue, Bank 3 receives a penalty just before issuing Loan 13. For Loan 13 the rest of the lead arranges are Bank 1, which has already been punished, and Bank 5. Thus, in this scenario we have a case of an enforcement action to a bank in a loan deal where one of the rest of the lead arrangers has already been penalized. Still, besides the usual within-year variation coming from loans issued by Bank 3 within the enforcement year, there is additional variation coming only from other loans issued by Bank 5 (but not from Bank 1, which has already been punished).

In our last scenario, highlighted with pink, Bank 2 receives an action just before issuing Loan 23. The rest of the lead banks are Bank 4, which is a lead bank in a punished loan later, and Bank 6. Again, besides the within-year variation stemming from the loans of Bank 2, we

get variation from the loans (even post-enforcement) issued by Bank 4 (except from the loan in observation 48) and all loans from Bank 6.

Obs.	Bank	Year	Loan	EA	CB
1	1	1	1	0	5
2	1	1	2	0	6
3	1	1	3	1	7
4	1	1	4	1	8
5	1	1	5	1	9
6	1	2	6	1	3
7	1	2	7	1	2
8	1	3	8	1	4
9	1	4	9	1	6
10	1	4	10	1	9
11	1	4	11	1	8
12	1	5	12	1	7
13	1	5	13	1	6
14	1	5	14	1	5
15	1	5	15	1	4
16	1	5	16	1	3
17	2	1	17	0	6
18	2	1	18	0	7
19	2	1	19	0	5
20	2	2	20	0	4
21	2	3	21	0	8
22	2	4	22	0	5
23	2	4	23	1	4
24	2	4	24	1	3
25	2	5	25	1	5
26	3	1	1	0	5
27	3	1	2	0	6
28	3	1	3	1	7
29	3	1	4	0	8
30	3	1	5	0	9
31	3	2	6	0	3
32	3	2	7	0	2
33	3	3	8	0	4
34	3	4	9	0	6
35	3	4	10	0	9
36	3	4	11	0	8
37	3	5	12	0	7
38	3	5	13	1	6
39	3	5	14	1	5

40	3	5	15	1	4
41	3	5	16	1	3
42	3	5	17	1	6
43	3	5	18	1	7
44	4	1	19	0	5
45	4	2	20	0	4
46	4	3	21	0	8
47	4	4	22	0	5
48	4	4	23	1	4
49	4	4	24	0	3
50	4	5	25	0	5
51	4	1	1	0	5
52	4	1	2	0	6
53	4	1	3	1	7
54	4	1	4	0	8
55	5	1	5	0	9
56	5	2	6	0	3
57	5	2	7	0	2
58	5	3	8	0	4
59	5	4	9	0	6
60	5	4	10	0	9
61	5	4	11	0	8
62	5	5	12	0	7
63	5	5	13	1	6
64	5	5	14	0	5
65	5	5	15	0	4
66	5	5	16	0	3
67	5	1	17	0	6
68	5	1	18	0	7
69	5	1	19	0	5
70	5	2	20	0	4
71	6	3	21	0	8
72	6	4	22	0	5
73	6	4	23	1	4
74	6	4	24	0	3
75	6	5	25	0	5

A.3. Additional robustness tests

So far, we have looked at enforcement actions with a clean window from other corporate events for the punished banks (but not for the rest of the banks) and a clean window from repeated enforcement actions to the same bank. In Table A.2, we sequentially relax these two assumptions. First, in Panel A, we extend the rule of the clean window from other corporate

events to the control group of (non-punished) banks. The reason for this sensitivity test is that our previous analysis might imply a selection bias: if the treatment group (banks with enforcement actions) excludes banks involved in corporate events like M&As, but the control group does not, then estimates might come from differences between banks with and without other corporate events. Despite the drop in available observations, we find that this is not the case: the effects of *Enforcement action* on the price terms of lending is quite similar to our benchmark specifications.

Second, we retain the few (3 in total) cases, where banks receive a second enforcement action in the year of the first (for the enforcement actions received in subsequent years, the effect is been shut down by the bank*year fixed effects). We find that there is indeed a small increase in the reputation-competition effect: the effect of *Enforcement action* on both *AISD* and *Total cost of borrowing* is approximately 12 basis points.

Third, from the 62 enforcement actions, there are 9 cases in which the punished banks received TARP funds in the same year with the enforcement action. In Table A.2, Panel C of the Appendix, we remove these cases (set *Enforcement action* equal to 0) and show that our results are similar to our baseline.

As an additional sensitivity test, we consider the case that punished loans differ according to the share of the punished lead bank in that loan. In this test, *Enforcement action* is not a dummy variable as in our previous analyses but equals the share of the punished lead bank in those loans that *Enforcement action* used to equal 1 (equals 0 for the non-punished loans). The interesting element of this test is that the reputation-competition effect of an enforcement action on the cost of borrowing should increase with the share of the punished lead bank in that loan.[16] This is what we document in the regressions of Table 11. A one

[16] The new variable is still 0 for non-punished loans and thus we cannot include both the new variable (essentially the interaction term between *Enforcement action* and the share of the punished lead bank) and the level terms because of collinearity.

standard deviation increase in the punished lead lender's share (equal to approximately 21%) yields a 10.29 (=21×0.490) decrease in the total cost of borrowing, which is equivalent to the finding in Table 5. We obtain similar results for the rest of the outcome variables.

Table A2. Dropping corporate events for all banks and using repeated enforcement actions

The table reports coefficients and t-statistics (in parentheses). We replicate Table 4, with the following changes: in Panel A, we use a clean 5-year window for all banks in our sample (both punished and non-punished); in Panel B, we include banks that received more than one (repeated) enforcement actions within the 5-year window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table 1. All regressions are estimated with OLS, with robust standard errors clustered by bank. The lower part of the table indicates the type of fixed effects included in the specifications. The lower part of each panel indicates the number of loan facilities, the number of banks, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	(1) AISD	(2) Total cost of borrowing	(3) AISU	(4) Facility fee	(5) Commitment fee	(6) Letter-of-credit fee
Panel A: Clean 5-year window from other corporate events in all banks (punished and non-punished)						
Enforcement action	-21.428**	-20.649**	-3.915*	-0.347	-3.982**	-0.816
	[-2.580]	[-2.430]	[-1.788]	[-1.311]	[-1.983]	[-0.827]
Term loan	24.316***	35.198***				
	[3.805]	[5.146]				
Enforcement action*Term loan	-2.957	-2.122				
	[-0.603]	[-0.510]				
No. of loan facilities	37,025	37,025	25,547	25,547	25,547	25,547
No. of banks	633	633	555	555	555	555
No. of firms	6,056	6,056	4,172	4,172	4,172	4,172
Adjusted R-squared	0.380	0.400	0.348	0.097	0.264	0.296
Panel B: Using repeated enforcement actions						
Enforcement action	-27.321***	-26.449***	-3.837*	-0.228	-3.740***	-0.398
	[-3.027]	[-3.924]	[-1.711]	[-0.619]	[-4.682]	[-0.527]
Term loan	29.755***	45.632***				
	[4.028]	[6.853]				
Enforcement action*Term loan	-1.519	-1.847				
	[-0.137]	[-0.390]				
No. of loan facilities	44,025	44,025	30,377	30,377	30,377	30,377
No. of banks	755	755	678	678	678	678
No. of firms	7,327	7,357	5,143	5,143	5,143	5,143
Adjusted R-squared	0.385	0.407	0.358	0.116	0.266	0.340
Panel C: Excluding TARP banks						
Enforcement action	-19.772**	-18.669***	-3.345	-0.395	-2.429***	-0.930
	[-2.284]	[-2.255]	[-1.520]	[-0.762]	[-2.918]	[-0.627]
Term loan	22.788***	35.397***				
	[3.401]	[5.332]				
Enforcement action*Term loan	-2.380	-2.058				
	[-0.645]	[-0.374]				
No. of loan facilities	44,025	44,025	30,377	30,377	30,377	30,377
No. of banks	755	755	678	678	678	678
No. of firms	7,327	7,357	5,143	5,143	5,143	5,143
Adjusted R-squared	0.385	0.407	0.358	0.116	0.266	0.340
Loan controls	Y	Y	Y	Y	Y	Y
Bank controls	Y	Y	Y	Y	Y	Y
Loan purpose effects	Y	Y	Y	Y	Y	Y
Firm*year effects	Y	Y	Y	Y	Y	Y
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Table A3. Weighting enforcement actions with punished lead lender's share of the loan

The table reports coefficients and t-statistics (in parentheses). We replicate Table 4 but defining *Enforcement action* with the product of the dummy used in previous tables with the share held by the punished lead lender. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table 1. All regressions are estimated with OLS, with robust standard errors clustered by bank. The lower part of the table indicates the type of fixed effects included in the specifications. It also indicates the number of loan facilities, the number of banks, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	AISD	Total cost of borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-0.615***	-0.597***	-0.047	-0.060	-0.865	-0.384
	[-2.866]	[-2.843]	[-0.675]	[-0.048]	[-1.472]	[-0.226]
Term loan	23.612***	37.598***				
	[3.728]	[5.583]				
Enforcement action*Term loan	-0.040	-0.063				
	[-0.558]	[-0.613]				
No. of loan facilities	36,718	36,718	25,335	25,335	25,335	25,335
No. of banks	625	625	550	550	550	550
No. of firms	6,040	6,040	4,160	4,160	4,160	4,160
Adjusted R-squared	0.365	0.378	0.340	0.129	0.224	0.277
Loan controls	Y	Y	Y	Y	Y	Y
Bank controls	Y	Y	Y	Y	Y	Y
Loan purpose effects	Y	Y	Y	Y	Y	Y
Firm*year effects	Y	Y	Y	Y	Y	Y
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Table A4. Supervisor differences

The table reports coefficients and t-statistics (in parentheses). The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table 1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank. The lower part of the table indicates the control variables and the fixed effects included in the specifications. It also indicates the number of loan facilities, the number of banks, and the number of firms with at least two loans with different loan pricing within one year. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)
Dependent variable:	AISD	Total cost of borrowing
Enforcement action	-22.017***	-21.190**
	[-2.605]	[-2.518]
FDIC dummy	0.449	0.407
	[0.295]	[0.306]
OCC dummy	1.629	1.317
	[0.923]	[0.711]
Enforcement action*FDIC dummy	-0.620	-0.719
	[-0.418]	[-0.503]
Enforcement action*OCC dummy	-0.745	-0.705
	[-0.291]	[-0.352]
No. of loan facilities	44,025	44,025
No. of banks	755	755
No. of firms	7,327	7,357
Adjusted R-squared	0.385	0.407
Loan controls	Y	Y
Bank controls	Y	Y
Loan purpose effects	Y	Y
Firm*year effects	Y	Y
Clustering	Bank	Bank

A.4. Estimation of marginal cost

We obtain estimates for the Lerner index from Clerides et al. (2015). In this section, we replicate their approach for convenience. We use the following log-linear cost function:

$$lnC_{it} = a_1 + a_2(z_{it})lnQ_{it} + a_3 lnw_{it}^{l} + a_4 lnw_{it}^{k} + a_5 lnw_{it}^{d} + a_6 lnw_{it}^{e} + e_{it} \qquad \text{(A.1)}$$

In (A.1) C is the total cost of the bank i at quarter t, measured by the deflated total interest expenses and total noninterest expenses; Q is the total output of each bank, measured by the deflated total earning assets (or simply total assets in robustness tests); w^l is the price of labor, measured by the ratio of personnel expenses to total assets; w^k is the price of physical capital, measured by the ratio of overheads minus personnel expenses to fixed assets; w^d is the price of intermediation funds, measured by the ratio of total interest expenses to total customer deposits;

and w^e is the price of financial capital, as measured by the ratio of equity capital to total assets. We collect data for these variables from the Call Reports (quarterly data).

Equation (A.1) has parametric parts (those related to the input prices) and a non-parametric part (that related to bank output). The variable *z*, which is the so-called smoothing parameter, is crucial for the identification of the model and must be a variable that is highly correlated with a_2 and considerably varies by bank-year. Clerides et al. (2015) propose using $z = lnw_{it}^{l} + lnw_{it}^{k} + lnw_{it}^{e}$, which is intuitive given the high potential correlation of input prices with the output elasticity of costs. We use the same approach and we also verify that using each input price separately yields similar results. Further, we impose the linear homogeneity restriction in input prices by normalizing total cost and the input prices by the price of deposits before taking logs. From (A.1) we can obtain the marginal cost at the bank-year level as $\frac{\partial C_{it}}{\partial Q} = a_2(\frac{C_{it}}{Q_{it}})$ to calculate the Lerner index.

By dropping the *t* subscript for simplicity, we can write (A.1) in econometric form as follows:

$$Y_i = \mathrm{E}\left(Y_i \mid W_i\right) + e_i = X_i\beta_1 + V_i\beta_2\left(Z_i\right) + e_i \,. \tag{A.2}$$

In this equation, β_2 is a function of one or more variables with dimension *k* added to the vector *Z*. The linear part in (A.2) is in line with the idea of the semiparametric model as opposed to a nonparametric model. The coefficients of the linear part are estimated in the first step as averages of the polynomial fitting by using an initial bandwidth chosen by cross-validation. We then average these estimates β_{1i} and β_{2i} to receive β_1 and β_2 in (A.2).

In the second step, we use the average estimates and (A.2) to redefine the dependent variable as follows:

$$Y_i^* \equiv Y_i - X_i\hat{\beta}_i = V_i\beta_2\left(z\right) + e_i^* \,, \tag{A.3}$$

where the asterisks denote the redefined dependent variable and error term. $\beta_2(z)$ is a vector of smooth but unknown functions of z_i, estimated using a local least squares of the form

$$\hat{\beta}_2(z) = \left[(n\lambda^k)^{-1} \sum_{j=1}^{n} V_j^2 K\left(\frac{z_j - z}{\lambda} \right) \right]^{-1} \left[(n\lambda^k)^{-1} \sum_{j=1}^{n} V_j Y_j^* K\left(\frac{z_j - z}{\lambda} \right) \right] = [B_n(z)]^{-1} C_n(z), \quad \text{(A.4)}$$

where $B_n(z) = (n\lambda^k)^{-1} \sum_{j=1}^{n} V_j^2 K\left(\frac{z_j - z}{\lambda} \right)$, $C_n(z) = n\lambda^k)^{-1} \sum_{j=1}^{n} V_j Y_j^* K\left(\frac{z_j - z}{\lambda} \right)$.

Equation (A.4) represents a local constant estimator, where $K(z, \lambda)$ is a kernel function, λ is the smoothing parameter (chosen by generalized cross validation) for sample size n, and k is the dimension of z_i.

If we assume that z is a scalar and K is a uniform kernel, then (A.4) can be written as follows:

$$\hat{\beta}_2(z) = \left[\sum_{|z_j - z| \le \lambda} V_j^2 \right]^{-1} \left[\sum_{|z_j - z| \le \lambda} V_j Y_j^* \right]. \quad \text{(A.5)}$$

In (A.5), $\hat{\beta}_2(z)$ is a least squares estimator obtained by regressing Y_j^* on V_j, using the observations of (V_j, Y_j^*) for which the corresponding z_j is close to z, that is, $|z_j - z| \le \lambda$. Therefore, to estimate $\hat{\beta}_2(z)$, we only use observations within this "sliding window." Note that no assumptions are made about this estimator globally, but locally—within the sliding window—we assume that $\hat{\beta}_2(z)$ can be well-approximated. Also, because $\beta_2(z)$ is a smooth function of z, $|\beta_2(z_j) - \beta_2(z)|$ is small when $|z_j - z|$ is small. The condition that $n\lambda$ is large ensures that we have sufficient observations within the interval $|z_j - z| \le \lambda$ when $\beta_2(z_j)$ is close to $\beta_2(z)$. Therefore, under the conditions $\lambda \to 0$ and $n\lambda^k \to \infty$ (for $k \ge 1$), the local least

squares regression of Y_j^* on V_j provides a consistent estimate of $\beta_2(z)$. Therefore, the estimation method is usually referred to as a local regression.

After obtaining estimates for *MC*, we calculate the Lerner index as shown in Table 1. The main merit of this approach is that it is quite more flexible than the usual parametric functional forms (e.g., the translog) and this can lead to substantial improvement in the precision of the estimates. Clerides et al. (2015) note that when using a fully parametric model (i.e., a_3 to a_6 are also functions of z), the results are similar.

We apply this procedure to the 755 lead banks of our study over our sample period (1997-2014). The bank-quarterly data required for the estimation of A.1 and the Lerner index are from the Call Reports.

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