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Abstract

Formal enforcement actions issued against banks for violations of laws and regulations related to safety and soundness can theoretically have both positive and negative effects on the terms of lending. Using hand-collected data on such enforcement actions issued against U.S. banks, we show that they have a strong negative effect on price terms (loan spreads and fees) for corporate loans and a positive one on non-price terms (loan maturity, size, covenants, and collateral). The results also indicate that in the absence of enforcement actions, the cost of borrowing during the subprime crisis would have been much higher, while punished banks intensify use of collateral.

Keywords: Bank supervision; Enforcement actions; Syndicated loans; Price and non-price terms of lending
JEL codes: E44; E51; G21; G28

The role of market regulation in preventing failures has been a central theme in economic research since at least the time of Pigou, but regulatory enforcement has received much less attention. In the banking industry, which perhaps is the most heavily regulated one, macro-prudential regulation is the *sine qua non* of the effort to contain and smooth out the harmful real effects of banking crises. However, regulations are void without enforcement, which is why enforcement actions are the single most important tool for implementing regulatory policy in banking (e.g., Flannery, 1998). But do these actions have real welfare effects by shaping lending terms? In this paper, we investigate *for the first time* how formal enforcement actions enacted against banks for safety and soundness reasons affect the main terms (price and non-price) of corporate lending.

Regulators levy enforcement actions against banks for violations of laws, rules, or regulations, as well as for unsafe or unsound practices, breaches of fiduciary duty, and violations of final orders (Fed, 2015). Enforcement actions take a number of forms, including financial penalties, prompt corrective actions, safety and soundness orders, cease and desist orders, etc., and they can be formal or informal. On their websites, regulators publicly announce formal enforcement actions, which are the ones we study. The main purpose of these actions is to give regulations the "teeth to bite," and in this respect, they are regulators' main enforcement mechanisms. Their effectiveness in securing the banking system directly relates to the efficient supervision of the banking system.

The potential effects of enforcement actions on lending terms have strong implications for borrowers and, by extension, for the real economy. Theoretically, enforcement actions can either raise or lower the cost of borrowing. On one hand, enforcement actions are costly for banks on many levels, including directly (e.g., monetary penalties) and indirectly (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, there are two reasons enforcement actions could lower the cost of borrowing. First, borrowers may perceive that a punished bank behaves anti-competitively and that they can find better lending terms elsewhere, making that bank more vulnerable to competition. Second, and perhaps more important, is that the borrowers may perceive the punished bank as highly 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 borrowers to increase its competitiveness in the loan market and avoid losing these borrowers.

To highlight these conjectures, we first provide an analytical framework in which monopolistically competitive banks lend to entrepreneurs using a perfectly elastic supply of funds from depositors. Taking into account general economic or other conditions (i.e., the state of the world), these banks might be subject to enforcement actions for safety and soundness reasons if a high proportion of their borrowers default. We show that the solution to this problem has both a positive and negative influence on how enforcement actions affect the cost of lending (the lending rate, in our framework). The positive effects (higher cost of lending) originate mainly in the actual cost of the enforcement action. The negative effects (lower cost of lending) originate in what we call a "reputation effect" of the enforcement action and an ambiguous (bearing both positive and negative forces) "competition effect."

Which of the effects prevail is an empirical question. To answer this question, we hand-collect information on all formal enforcement actions against U.S. banks and bank-holding companies supervised by the Federal Reserve, the Federal Deposit Insurance Corporation (FDIC), and the Office of the Comptroller of the Currency (OCC) during 2000–2010. For the

enforcement actions, we have information (apart from the issuing regulator) on their rationale and the enactment (or enforcement) date. In line with our theoretical priors, we use only the formal enforcement actions that substantially relate to safety and soundness practices. Subsequently, we match these actions with bank-level accounting data from Call Reports, firm-level accounting data from Compustat, and loan-level data from DealScan (syndicated loans). Thus, we match large corporate loans from specific lead banks (lead arrangers) that received formal enforcement actions to specific firms.

Using this novel data set, we examine how enforcement actions affect the terms of syndicated loans. Our empirical analysis recognizes that there are two important, interrelated identification challenges. The first is that enforcement actions are not random events but constitute a "treatment" for specific problems banks face or for specific violations of laws and regulations. The second empirical identification problem is that enforcement actions could simply lead banks to lend to less risky firms.

The structure of our data set (loan-level data with information on both the lender and the borrower) allows us to mitigate these concerns in a fashion similar to Jimenez, Ongena, Peydro, and Saurina (2012; 2014). Specifically, our data set includes multiple loans originated by the same punished lead bank to the same firms within the so-called event window, which in most of our empirical analysis consists of the year before the enforcement action, the year of the action, and the year after the action.

This structure has three important advantages. First, the enforcement actions occur at different times for different banks. As we also clean our event windows from other major bank corporate events (e.g., M&As, failures), this prevents the enforcement actions from systematically capturing the effects of other events. Second, our sample allows the inclusion of firm fixed effects, which fully control for lending to less risky firms within the enforcement window and almost fully for other unobserved firm characteristics that potentially affect

lending terms. Third, and even more important, the multiple loan deals by the same bank within a single year allows including bank fixed effects for the asymmetrically-timed enforcement windows that are not collinear with the enforcement action. Using a pre-analysis, we econometrically demonstrate that including bank fixed effects in the three-year time window is extremely important because these fixed effects capture, *inter alia*, the financial reasons for the enforcement action, thus leaving the effect of the policy action unaffected by such unobserved bank characteristics.

Our results provide a strong pro-intervention representation of bank supervision. We find that virtually all the pricing terms of loans originated after a syndicate's lead bank receives a formal enforcement action are better for borrowers compared to loans originated by the same banks before the enforcement action. This price drop is very significant economically (approximately 23 basis points for the all-in-drawn-spread, or $4.75 million less in interest income from each loan) and denotes more competition for large corporate loans following an enforcement action. The improved competitive conditions in turn benefit borrowing firms, which see a substantial decrease in their cost of borrowing for reasons external to their operations.

Among the non-price terms of lending, there is an increase in loan sizes and durations following enforcement actions (the average loan is extended by approximately three months and is 6.7% larger following an enforcement action). In contrast, punished banks increase their covenant standards and the probability of asking for collateral for the loans originated after an enforcement action. Thus, punished banks are willing to price their loans more competitively, extend their length, and increase their size, but they are unwilling to bend on issues related to loan safety and information asymmetry. Given the increased competitiveness and the fact that the syndicated loans originated after enforcement actions should still be profitable for syndicate

members, our findings provide clear evidence of the welfare-improving effects of enforcement actions at least for large corporate loan deals.

We also find that the subprime crisis offset all the statistically and economically significant effects, except from the use of covenants and collateral. A glass-half-empty interpretation of this finding is that the subprime crisis rendered insignificant any positive effects on competition, welfare, and lending terms. This would imply that enforcement actions improve competitiveness in corporate lending only in good economic periods. In contrast, a glass-half-full interpretation suggests that enforcement actions prevented lending terms from worsening (due to increased informational asymmetry costs) during the subprime crisis, *ceteris paribus*. Thus, enforcement actions improved competition and welfare during the crisis, as the cost of borrowing in these periods would have been much higher in their absence. We argue that the latter reading is a better representation of the role of enforcement actions during periods of turmoil.

Our paper is related to a recent literature examining the effects of enforcement actions on bank risk, capital, and loan growth (Delis, Staikouras, and Tsoumas; 2016; Berger et al., 2016; Danisewicz et al., 2014). The approach of these studies is to use bank accounting data and their findings are consistent with a decrease in bank risk, mainly through a restructuring of risky assets and shrinking of loan portfolio. Our analysis is quite different in both the research question (looking into the terms of lending) and in the level of analysis (loan-level data with a bearing on empirical identification). Thus, our findings provide quite distinct and novel implications in reflecting positive effects of enforcement actions on economic welfare (through the improvement in the terms of lending, including lower lending rates and larger loan deals).

The paper proceeds as follows. Section I provides theoretical considerations regarding the effect of enforcement actions on lending terms through an analytical framework of a monopolistically competitive market for corporate loans. Section II presents the data set used

in the empirical analysis, with brief descriptions of the economics behind enforcement actions and the syndicated loan market. Section III discusses in some detail the empirical identification strategy of our paper. Section IV presents and discusses the empirical results, and Section V concludes.

I. Theoretical Considerations

How does supervisory intervention affect the terms of bank lending? The premise of this section is to answer that question by providing an analytical framework in which banking supervisors intervene to enforce laws and, through that intervention, affect the terms of lending.

An inherent assumption in our analysis is that banks set their own lending rates to earn profits and thus the market deviates from a perfectly competitive framework. The consensus of theoretical and empirical literature examining market power in banking is that monopolistic competition in the loan market most closely reflects banking systems worldwide (e.g., Beck, De Jonghe, and Schepens, 2013) and the U.S. market for corporate loans in particular (Delis, Kokas, and Ongena, 2015). In what follows, we model lending rates (the price terms of lending), but the same competitive forces can shape the non-price characteristics of a loan, such as the maturity and guarantees.

Our model assumes a continuum of entrepreneurs, banks, depositors, and a bank supervisor. To keep our framework simple, we assume loans are fully financed by a perfectly elastic supply of funds from depositors at time $t = 0$. The full amount of deposits is insured and the deposit rate is zero.

There is a continuum of penniless entrepreneurs of measure one, indexed by $i \in [0,1]$. At time $t = 0$, the entrepreneurs require credit to finance risky projects that either succeed or fail. To run the investment projects, one unit of corporate loans is needed. If L projects are financed, the gross return of the marginal project, if it is successful, is $R(L)$, with $R(L)$ being a

strictly decreasing function. If the project is not successful (still at time $t = 0$), the entrepreneur does not repay the bank. Building on Colliard (2014), we assume that a random proportion (π) of projects fail, following a distribution *f(π,s)*. A family of cumulative distribution functions (cdfs) $\{F(.,\hat{s}), \hat{s} \in [\underline{s}, \bar{s}]\}$, with support over [0,1], represents the set of plausible risk models that describe the distribution of π. This family of cdfs can be interpreted as one model with different parameters, or models from different families. We also denote $\{f(.,\hat{s})\}$ as the corresponding probability distribution functions (pdfs). The correct risk model *s* is randomly selected by nature in $[\underline{s}, \bar{s}]$ according to some cdf G(.), with associated density g(.). The actual proportion of π defaulting borrowers follows the distribution *F(π,s)*, which is continuously differentiable in both arguments with $\frac{\partial F(\pi,s)}{\partial \pi} > 0$.

Following Gerali et al. (2010) and Damjanovic (2013), there is a continuum of monopolistically competitive banks of measure one, indexed by $j \in [0,1]$, whose market power in the loan market is modelled in a Dixit-Stiglitz framework. Given the default rate π, the individual bank maximizes its expected profits by choosing the rate for loans r_j at time $t = 1$. One unit of capital purchased by the entrepreneur is a basket of differentiated financial products with a constant elasticity of substitution $\varepsilon > 1$ (the case of perfect competition is when $\varepsilon \to 1$), resulting in a homogeneous loan input that is sold to entrepreneurs according to the technology:

$$1 = \left(\int l_j^{\frac{\varepsilon-1}{\varepsilon}} dj \right)^{\frac{\varepsilon}{\varepsilon-1}}, \tag{1}$$

where l_j is a quantity purchased of product *j*. Then, the cost of borrowing for the entrepreneur is given by:

$$\int (1 + r_j) l_j dj, \tag{2}$$

where r_j is the lending rate.

The entrepreneur's task is to maximize utility subject to the supply of loans given from equation (1):

$$max_{l_j} U = R(l_j) - \int (1 + r_j) l_j dj, \quad (3)$$

with $R(l_j)$ being the return from each loan. The resulting downward-sloping demand is given by:

$$l_j = \left(\frac{1+r_j}{1+r}\right)^{-\varepsilon}, \quad (4)$$

with $1 + r = \left[\int (1 + r_j)^{1-\varepsilon} dj\right]^{\frac{1}{1-\varepsilon}}$ being the aggregate gross rate. All the entrepreneurs demand the same amount of corporate loans l_j from bank *j*; thus, the total demand the bank faces is:

$$L_j = \left(\frac{1+r_j}{1+r}\right)^{-\varepsilon} L(r), \quad (5)$$

where total demand *L(r)* is exogenously given and a decreasing function of *r*.

At time *t* = 0, the banking regulator establishes the rules of conduct. These rules are perfectly known by all players in the model and, if there is misconduct, an enforcement action (penalty) might be imposed. Following Colliard (2014), the penalty function has the form *P(π,s)*; that is, it is a function of the ratio of the defaulting entrepreneurs, π, and the state of the world, *s*. In our framework, *P(π,s)* is continuously differentiable in both its arguments, with $\frac{\partial P(\pi,s)}{\partial \pi} > 0$. This means that when the ratio of failing projects increases, the size or severity of the penalty increases.

Our goal here is to highlight the mechanisms through which enforcement actions affect lending terms. We do not consider forces that generate the enforcement actions. Thus, we assume that the penalty function *P(π,s)* is not derived from a benevolent supervisor's welfare-maximization problem. In other words, we assume that similar (important) enforcement actions bear uniform competitive and reputational effects on punished banks and that the reasons for the enforcement actions do not directly affect the terms of individual loans. They only affect the bank's optimization problem. We feel that this is a reasonable representation of the real

world, where the bank supervisor's actions do not directly reflect banks' pricing decisions for individual loans but affect their whole business models.

To this end, an enforcement action occurs following a combination of the default of the loan and the supervisor's discretion. Therefore, we allow for a probabilistic variable to represent the supervisor's decision to issue the penalty, which happens at time $t = 2$. In particular, $\mu \in [0,1]$ is the probability that the supervisor will not issue the enforcement action, and *1-*μ is the probability that the supervisor will issue the enforcement action. A higher μ suggests lower probability of the supervisor to issue the enforcement action (more tolerant supervisor).

Given the default rate π, the individual bank maximizes its expected profits Π_j by choosing the rate of loans r_j at time $t = 1$, subject to expectations about the supervisor's actions (i.e., the probability *1-*μ of enforcing the penalty). The proportion of losses $\pi(r_j)$ above which the intermediary defaults is determined by $(1 + r_j)L_j(1 - \pi) - L_j = 0$. In a symmetric equilibrium, all banks set the same interest rate r_j. Denoting as E_s the expectation according to the distribution *F(π,s)*, we can write the problem of the bank for a given *s* as:

$$max_{r_j} E_s(\Pi_j) = \int_0^{\pi(r_j)} [(1 + r_j)L_j(1 - \pi) - L_j - (1 - \mu)\,P(\pi, s)]f(\pi, s)d\pi, \qquad (6)$$

subject to (i) the total demand L_j the bank faces as given by equation (5) and (ii) to $\pi(r_j) = \frac{r_j}{1+r_j}$. From the first-order condition $\frac{dE_s(\Pi_j)}{dr_j} = 0$, the equilibrium interest rate r_j is given as a solution to the following equation:

$$\frac{(1-\mu)P(\pi(r_j),s)}{(1+r_j)^2} + \left(\frac{1+r_j}{1+r}\right)^{-\varepsilon} L(r)F(\pi(r_j), s)\left[(1 - \varepsilon)\left(1 - \pi + \pi(r_j)\right) + \varepsilon\frac{1}{1+r_j}\right] = 0. \quad (7)$$

To examine the effects of an increase in the failure ratio of entrepreneurial projects and the probability of an enforcement action, we use equation (7) to compute the relevant comparative statics. Specifically, total differentiating (7) with respect to r_j and μ, and assuming

that π remains constant, we find that a change in r_j due to changes in the probability of the regulator not to enforce the penalty is given by:

$$\frac{dr_j}{d\mu} = \frac{P(\pi(r_j),s)\frac{1}{(1+r_j)^2}}{A(r_j,s,\mu)}. \tag{8}$$

where

$$A(r_j,s,\mu) = \frac{1}{1+r_j}\left\{\frac{(1-\mu)}{(1+r_j)^2}\left[\frac{\partial P}{\partial \pi(r_j)} - 2P(\pi(r_j),s)\right] + \left(\frac{1+r_j}{1+r}\right)^{-\varepsilon} L(r)\left[\left(\frac{\partial F}{\partial \pi(r_j)}\frac{1}{(1+r_j)^2} - \right.\right.\right.$$

$$\left.\left.\left.\varepsilon F(\pi(r_j),s)\right)\left((1-\pi)+\varepsilon\pi+(1-\varepsilon)(2-\pi)r_j\right) + F(\pi(r_j),s)(1-2\varepsilon)\right]\right\}. \tag{9}$$

Given the assumptions for the $P(\pi,s)$, the numerator of equation (8) is positive. Also, given the assumptions for the penalty function $P(\pi,s)$, the part $\left[\frac{\partial P}{\partial \pi(r_j)} - 2P(\pi(r_j),s)\right]$ of equation (9) yields an ambiguous "marginal penalty effect." The ambiguity in the sign arises from the positive effect stemming from an increase in μ (i.e., lower probability of enforcement) on the interest rate r_j ($\frac{\partial P}{\partial \pi(r_j)}$), which we call the "reputation effect of the penalty," and the negative effect stemming from the enforcement of the penalty ($-2P(\pi(r_j),s)$), which we call the "cost effect of the penalty." Similarly, the part $\left(\frac{\partial F}{\partial \pi(r_j)}\frac{1}{(1+r_j)^2} - \varepsilon F(\pi(r_j),s)\right)\left((1-\pi)+\varepsilon\pi+(1-\varepsilon)(2-\pi)r_j\right)$ of equation (9) yields an ambiguous "marginal competition effect." Finally, $F(\pi(r_j),s)(1-2\varepsilon)$ yields a negative "cost competition effect."

In essence, this theoretical framework highlights both positive and negative forces influencing how enforcement actions affect lending terms. The forces related to worsened terms of lending (negative forces) indicate that enforcement actions bear a cost because they mainly reflect the higher probability of default of projects chosen by banks with higher probability of being subject to enforcement actions. The forces related to improved terms of

lending (positive forces) reflect the high market power and deteriorating reputation of punished lenders. These lenders are subsequently forced to lower their market power and improve their reputation by improving their lending terms. Which forces prevail is naturally an empirical question.

II. Data

We obtain data from four sources. Information about 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 loan-level data from DealScan, using loans originated by the punished lead banks (lead arrangers) within a time window before and after the year of the enforcement action. In most of our analysis we use a 3-year window (the year before the action, the year of the action, and the year after the action) but also experiment with a 5-year window. Then, we match this data set with bank-level accounting data from Call Reports and firm-level accounting data from Compustat. After cleansing our data from missing observations on the variables to be included in our analysis, we have 3,492 loan deals (for the 3-year window) originated by 57 lead banks that received 62 enforcement actions during 2000–2010. For the 5-year window that number of loans is 5,195. The time window around these events increases the time span of sample from 1999 to 2011 (1997-2013) for the 3-year (5-year) window.

We examine the terms of lending for individual loans given by punished lead arrangers before and after the enforcement action. Our decision to focus on lead arrangers is guided by the fact that it is the lead banks that essentially decide the terms of lending in syndicated loan deals. In what follows, we discuss the variables in our empirical analysis with an emphasis on an economic analysis of enforcement actions. In Table A1 of the Appendix we provide formal

definitions for all the variables in our empirical analysis, and in Table 1 we provide summary statistics.

[Insert Table 1 about here]

A. Enforcement Actions

We hand-collect data on all formal enforcement actions against U.S. commercial banks and bank-holding companies from the Federal Reserve, the FDIC, and the OCC.[1] These 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)).[2] 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).

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

[1] 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.

[2] 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.

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 that have reputational or competition effects (Delis, Staikouras, and Tsoumas, 2016).

Formal enforcement actions come, at times, regardless of CAMELS ratings. They occur whenever a federal supervisory agency 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).[3]

We read the rationales for all the formal enforcement actions enacted during 2000–2010 and use only those that relate to the financial safety and soundness of lead arrangers. We provide a detailed discussion of our selection of enforcement actions according to their rationale in Table A2 of the Appendix. Our guidance for this selection is the internal 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

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

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 (M&As, either as acquirer or target, liquidations, and failures). This practice cleans the effects of the enforcement actions from these of other events. We further drop enforcement actions and banks that received an enforcement action more than once during a 3-year window. The reason is that in these cases it is hard to pinpoint the direct effect of the event on the terms of lending. After the matching process with loan-level from DealScan, we have 62 enforcement actions issued on 57 lead arrangers of syndicated loan deals.

Evidently, most banks in our sample received an enforcement action only once during our sample period. Note that the number of enforcement actions is not quite relevant to the sample size of the empirical analysis because we assume (and we impose) that these are uniform events (see Table A2 and discussion in the Appendix).[4] What matters, and what constitutes the unit of our analysis, are the numbers of loans pre and post enforcement. These loans are relatively evenly distributed for the periods before and after the action.

B. Terms of Lending

[4] For example, the vast majority of event studies look at the effect of one or a few homogeneous events.

We conduct our empirical analysis at the loan level using syndicated loan data from DealScan (for details, see Ivashina and Scharfstein, 2010a, b). The outcome variables of our analysis characterize various price and non-price lending terms. We define all these variables in Table A1 of the Appendix.

Lenders generally use a complex pricing structure, incorporating a menu of spreads and different fee types rather than a single price measure to ensure an appropriate expected return (Berg, Saunders, and Steffen, 2015). Thus, to analyze the effect of bank enforcement actions on pricing structure comprehensively, we use a number of different variables, which include all-in-spread drawn (*AISD*), defined as spread plus facility fee, spread over LIBOR paid on amounts drawn on credit lines (*Spread*), 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*).[5] All of these spreads and fees characterize the total price of lending, which we construct following Berg, Saunders, and Steffen (2015) and also use in the empirical analysis (*Total cost of borrowing*). For more details, please see Table A1.

We also examine the effect of enforcement actions on the non-price terms of lending and in particular on loan durations (*Loan maturity*), loan size (*Loan size*), the number of covenants in the loan contract (*Total covenants*), and whether the loan facility requires collateral (*Collateral*). These non-price terms still entail a cost for the loan, but this cost is not explicitly priced. Moreover, *Total covenants* and *Collateral* relate explicitly to the security of

[5] 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, Saunders, and Steffen, 2015).

the loan facility after its origination and thus to the minimization of informational asymmetries (mainly moral hazard) in loan contracts.

In Table A3 we provide information on the means of our response variables across loans before and after the enforcement actions. Virtually all the price lending terms improve for borrowers post enforcement, except from the *Commitment fee* which remains stable. All responses are statistically significant at conventional levels except from that of the *Letter-of-credit fee*. For the non-price terms there is a statistically significant increase in *Loan maturity* and in *Loan size*, while the responses of *Total covenants* and *Collateral* are statistically insignificant. These are first-hand results that lending terms improve for borrowers after the enforcement actions and it remains to be examined whether there is a causal effect running from the actions to the lending terms.

C. Control Variables

We use a number of control variables at the loan, bank, and firm levels in various stages of our empirical analysis (see Table A1 for formal definitions and Table 1 for summary statistics). When we estimate the specifications for the price terms, we control for the non-price terms of the loans discussed in the previous subsection. At the loan level, 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 (*Term loan*). This distinction is important, especially given the emergence of the subprime crisis in 2007. For example, firms after the eruption of the crisis were refining their revolvers

at higher rates. The use of year effects along with *Term loan* potentially mitigates part of this bias.[6]

At the firm level, we use a number of control variables that potentially affect the lending terms (data from Compustat). Specifically, we control for firm size, profitability, leverage, cash-flow volatility, asset tangibility, the market-to-book ratio, and firms' debt ratings from Standard & Poor's. Notably, by additionally using firm fixed effects for the time window around an enforcement action, most of these firm characteristics are insignificant determinants of the price and non-price terms. Thus, we abstain from using additional firm-level controls.[7]

In a similar fashion, we experiment with many bank-control variables, especially those that characterize the CAMELS ratings (e.g., capital ratios, ratios related to loan losses, liquidity ratios, performance ratios, etc.). However, we find that using bank fixed effects renders all of these variables statistically insignificant (see also the relevant analysis below on the determinants of enforcement actions). This makes sense intuitively, as bank fixed effects collectively capture the reasons for enforcement actions and fully control for any related observed and unobserved characteristics. With these issues in mind, we turn to the discussion of our strategy in identifying the causal effect of enforcement actions on lending terms.

III. Identification Strategy

The general form of the empirical model is:

$$TL_{lbft} = a_0 + a_1 EA_{bt} + a_2 L_{l,t} + a_3 B_{b,t-1} + a_4 F_{f,t-1} + u_{lbft} \quad (10)$$

In equation (10), *TL* represents the terms of lending of loan *l*, granted by bank *b* to firm *f* in year *t*. *EA* is a dummy variable equal to 1 in the year (or two years) after the year of the

[6] We also experiment with specifications that include only data on term loans. The statistical significance of these estimates is almost the same with the specifications on the full sample (that also includes revolvers). The economic significance from the specifications with term loans only is about 25% smaller, which can be attributed to the smaller sample.

[7] In fact, for this very reason, we conduct most of our empirical analysis without firm-level control variables, which to a large extent simply overidentify the models while decreasing our sample size.

enforcement action and zero in the year before the enforcement action. For the year of the enforcement action the dummy equals to 1 if the loan was originated after the enactment of the enforcement action and 0 if the loan was originated before the enactment. Further, *L*, *B*, and *F* are the vectors of loan, bank, and firm characteristics used as control variables where appropriate.

We decompose the stochastic disturbance, *u*, into bank, firm, loan purpose, year fixed effects, and the remainder disturbance. This decomposition is crucial for our identification strategy. Note that our empirical model does not in principle suffer from selection bias: the enforcement action (treatment) is not endogenous to the terms of lending (outcomes) of a single loan. Phrased differently, enforcement actions do not occur because of the terms of a particular loan. In contrast, our approach does potentially suffer from omitted-variable bias, given the potential importance of unobserved bank, firm, and time-specific characteristics in affecting the terms of lending.

Three elements in our identification approach remedy the omitted-variables problem and yield an almost natural experiment. The first is that enforcement actions occur at different points it time. This allows for a panel data set with non-repeated observations on loan facilities, but with repeated observations on firms and banks. Thus the enforcement actions on each bank are not systematically correlated to specific other events that take place at the same time.[8] In addition, the panel structure of our data set allows including year fixed effects to control for shocks common to all banks and firms that can similarly affect the lending terms for all loans within a given year (e.g., the homogeneous effect of the subprime crisis across all loans).

Second, the fact that firms repeatedly obtain different loan facilities during the sample period allows the inclusion of firm fixed effects. The firm fixed effects control for a large

[8] Also note that, as we discuss Section II.A and in Table A2 in the Appendix, we drop the enforcement actions on banks for which there are other important corporate events during the 3-year window of the enactment.

amount of firm characteristics that affect the terms of lending, rendering the impact of firm-level controls mostly insignificant. In principle, the use of these fixed effects imply that we observe loans made to the same firm before and after the action. Thus, we minimize the risk that our results come from lending to less risky borrowers as a response to the enforcement action. In even more restrictive specifications we use bank*firm fixed effects, which implies identification only from loans made by the same lead arranged to the same firm before and after the enforcement action.

Of particular importance are the unobserved bank characteristics that might be correlated with both *EA* and *TL* in equation (10). With our empirical model we aim to capture the general supervisory-policy effect of an enforcement action on the terms of lending that is related to the reputational and disciplinary natures of these actions. We are not merely interested in the narrow effects stemming from a single bank characteristic, such as the bank's non-performing loans, capitalization, etc. that could have triggered the action. Given that a single bank in our sample is involved in multiple loan facilities (even within the same year), this allows including bank fixed effects. These fixed effects are not collinear with the *EA* dummy variable, which equals one only in the one-out-of-three bank-year observations within the 3-year window. In other words, the event window is small enough to avoid bank fixed effects not capturing the effects of observed and unobserved bank characteristics that led to the enforcement action.

To provide further evidence that the bank fixed effects fully predict enforcement actions, we conduct a pre-analysis by estimating a model in which we regress *EA* on a number of bank-year variables directly related to CAMELS ratings (Flannery, 1998). Specifically, we use the ratio of total capital to risk-weighted assets (risk-weighted capital) as a proxy for bank capital; the ratios of commercial and industrial loans to total loans (i.e., high-risk loans) and allowance for loan losses to total loans as measures of specialization in corporate lending, asset

quality, and credit risk management; the Z-score as a measure of the quality and volatility of earnings; and the ratio of liquid assets to total assets as a measure of liquidity. These variables reflect the components of CAMELS ratings (e.g., Flannery, 1998; Berger, Davies, and Flannery, 2000) and should strongly determine the probability of receiving an enforcement action, especially those most closely related to the Basel Committee Core Principles for Effective Banking Supervision (Basel, 2012).

We posit that if these variables are statistically significant determinants of *EA* in models without bank fixed effects but become insignificant when we add bank fixed effects, then bank fixed effects are sufficient to fully control for the reasons underlying the enforcement action. Thus, the full policy-driven (e.g., reputation- and disciplinary-related) effect of enforcement actions on lending terms would be purified from the very specific omitted bank characteristics that correlate with the enforcement action and directly affect the terms of lending.

We report the OLS results from this initial analysis in Table 2.[9] All explanatory variables are in first-year lags to capture the fact that enforcement actions are enacted given the previous condition of banks. In the first column, we provide the results without bank fixed effects. In line with expectations, we find that higher ratios of commercial and industrial loans (high-risk loans) to total loans, higher allowance-for-loan losses ratios, and lower liquidity ratios and Z-scores strongly predict *post-enforcement loan*. The relevant coefficient estimates are statistically significant at the 1% or the 5% level and highly economically relevant. Notably, the R-squared of the model shows that these five variables explain approximately 68% of the probability of receiving and action. The results are quite similar when we include loan purpose fixed effects (column 3) or when we use a two-year time window before and after the enforcement date (column 5).

[9] Given that the dependent variable is a dummy, we also tried a logit fixed effects model, which however faced computation difficulties (given the large number of fixed effects) and does not give results.

[Insert Table 2 about here]

In column 2 of Table 2 we introduce bank fixed effects. The model's R-square increases to more than 90%, but all of the previously significant explanatory variables become insignificant at conventional levels. This is a strong indication that bank fixed effects better control for the reasons behind the enforcement actions, as they include almost the full set of idiosyncratic reasons behind the actions and not only the ones related to CAMELS. We obtain similar results when controlling for loan-purpose fixed effects (column 4) and when using a two-year time period around the enforcement date (column 6). We experiment with about 30 other independent variables in these regressions, including other measures of credit and liquidity risk, other capital ratios, profitability ratios, proxies for non-interest income, etc. We do not find significant changes in our results and we report these regressions in Table A4 of the Appendix. Importantly, in column 6 of Table A4 we include regulator*year fixed effects. The R-squared in this regression increases to more than 98%, indicating that beyond the bank-related determinants of enforcement actions regulatory discretion explains another 8% of the probability to issue an action. This finding is in line with our discussion in Section II.A, but such effects are exogenous to the business model of banks. We thus conclude that including bank fixed effects is sufficient to identify the bank-related reasons for enforcement actions.

IV. Empirical Results

A. The Price Effects of Enforcement Actions

We first analyze the impact of enforcement actions on loan pricing and report our baseline results in Table 3. We report coefficient estimates and t-statistics, obtained from standard errors clustered by bank and firm. The general finding is that enforcement actions negatively and significantly, both statistically and economically, affect the price terms of lending. Specifically, the coefficient on *enforcement action* is statistically significant at the 1% level across almost

all the dependent variables. An enforcement action lowers *AISD* by approximately 23 basis points, which is equal to a 15.7% reduction for the average *AISD* in our sample.

The results are economically meaningful. Given that the mean sample loan size is $508 million and the average loan's time to maturity is around four years, if a bank is punished, the interest income it receives from each loan will fall by $4.75 million (=507.9× 0.00228×4.1). We observe equivalent reductions in *spread*, *total cost of borrowing*, and *AISU* (columns 2-4). For the total cost of borrowing in particular, we observe a 19.6 basis point reduction (approximately 17%). Further, we find that the facility, commitment and letter-of-credit fees are also significantly lower for the loans after the enforcement action.

[Insert Table 3 about here]

We run several robustness tests for these baseline results. First, in Table A5 we replicate the regressions of Table 3 by including firm characteristics. Given that we include firm fixed effects, we expect most of these to be statistically insignificant, which is indeed the case. The only significant variables are *firm size*, *firm profitability*, and *firm tangibility* in some of the response variables. The coefficient estimates on *enforcement action* are similar to those of Table 3, albeit somewhat smaller which stems from the reduction in the sample's size. Indeed, if we use the reduced sample without the firm characteristics, the estimates are almost identical to those shown in Table A5. Similarly, in Table A6 we add the bank-level control variables used in Table 2. Our results show that only *bank liquidity* plays a role and only in determining the facility fee (and through that the total cost of borrowing), but without changing the statistical significance of *post-enforcement loan*.

Second, our results are equivalent to our baseline specifications if we consider only the enforcement actions that are more tightly related to safety and soundness (i.e., only those relating to the Basel Principles) and exclude those imposed on banks' board members, managers, and other affiliated parties (results reported in Table A7). Further, the results are

quantitatively and qualitatively similar to those in Table 3 if we use a two-year window after the year of the enactment (Table A8).

An even more restrictive model includes bank*firm fixed effects among the explanatory variables. In these models, the effect of the enforcement action is identified only from loans originated by specific lead arrangers to the same firm before and after the action. With such fixed effects it is particularly hard to think of omitted variables that might affect the results on the effect of enforcement actions. These models include the largest number of fixed effects (1,797), thus still allowing for considerable degrees of freedom. The results, reported in Table 4, show negligible differences compared to the equivalent in Table 3. Therefore, we conclude that the bank year fixed effects simply overidentify our baseline model without affecting our findings.

[Insert Table 4 about here]

As a final exercise in this section, we conduct a placebo test by using only the non-lead punished banks. We expect that as the non-lead banks play little or no role in the setting of the terms of lending, an enforcement action issued on them should have little or no effect on the terms of lending. As the results of Table 5 show, *enforcement action* has statistically negligible effects on all the price terms of lending.

[Insert Table 5 about here]

Overall, these results have important economic implications. The supervisory interventions lead to significant improvements in the pricing of large loan contracts and thus to improved competitiveness. Also, given that it is unlikely that syndicated loan contracts are unprofitable and banks make losses, our results point to the existence of supernormal profits on syndicated loans prior to enforcement actions. That is, the average bank (or the average bank syndicate) extracts anti-competitive loan pricing from borrowers before an enforcement action, potentially due to market power or reputation, as highlighted in our theoretical

framework. This in turn leads to a loss in allocative efficiency and, thus, economic welfare, *ceteris paribus*.

The results also highlight that enforcement actions taken 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 banking products. Not only there is no evidence whatsoever that punished banks pass the cost of enforcement actions to their corporate borrowers, but in fact we find strong evidence that large borrowers substantially benefit from supervisory intervention. In turn, this implies that supervisory intervention brings enhanced investment and growth opportunities for borrowers.

B. Heterogeneous Price Effects of Enforcement Actions

In this section we hypothesize that the price effects of enforcement actions are heterogeneous based on certain bank characteristics and on the state of the economy. We first posit that lead banks that have a stronger relationship with borrowing firms are more likely to reduce loan prices significantly if they get punished. The reason is that in strong bank-firm relationships, reputation carries more weight due to the lower informational asymmetries between the bank and the borrower. Thus, the issues that led to the enforcement action are more obvious, and the punished lender might have to do more to keep the borrower, especially in a competitive market such as the syndicated loan market.

We test this conjecture in the regression reported in column 1 of Table 6 using an interaction term between *enforcement action* and a variable measuring the number of loans by the lead bank of the syndicate to the specific borrower in the last five years (we name the variable *intensity of relationship lending*). For expositional brevity, we only report the results on *total cost of borrowing,* but the findings are almost the same with *AISD* and *Spread*. We indeed find that the stronger the bank-firm relationship, the more negative the effect of

enforcement actions. The coefficient on *enforcement action* remains negative and statistically significant, and the coefficient on the interaction term further adds approximately 1.4 basis points to this reduction.

[Insert Table 6 about here]

A second hypothesis concerning a potential heterogeneous effect of enforcement actions on the price terms of lending concerns the specialization and the capitalization of banks. We expect that banks specializing in specific types of lending, particularly corporate and industrial lending, can offset the negative effect enforcement actions have on price terms. The reason is that a high degree of specialization in corporate lending usually implies some sort of market power (in pricing or accessing information) in the market for corporate loans. This market power translates to lending price stickiness irrespective of enforcement actions, rendering those enforcement actions less potent (milder competition effect).

We test this conjecture by estimating an equation that includes an interaction term between *enforcement action* and the ratio of commercial and industrial loans to total loans (*Bank's C&I loans*). The results, reported in column 2 of Table 6, are in line with our hypothesis. The coefficient estimate on *enforcement action* remains negative and statistically significant, while the interaction term is positive and statistically significant. By taking the derivative of this equation with respect to *enforcement action*, we find the value on *bank's C&I loans* above which the negative effect of *enforcement action* turns positive. This value is 0.32, which corresponds to only 34 observations. Thus, it becomes apparent that the effect of enforcement actions changes sign only for very few loans (about 1% of our sample).

A final hypothesis relates to the effect of the subprime crisis on our baseline results. The premise here is that during crisis periods, informational asymmetry costs increase due to increased adverse selection and moral hazard, leading to increased cost of borrowing (e.g., Bernanke and Gertler, 1989; Ivashina and Scharfstein, 2010b). In turn, the lead arrangers might

be unable to reduce lending prices during these periods, as we show that it happens during our full sample period. In other words, the subprime crisis constitutes a semi-natural experiment (especially to the extent that the crisis was not caused by individual corporate loans): if we observe a lower decrease in lending price terms during 2007–2009, this would imply that supervisory actions will offset increases in lending price terms (due to the rising cost of informational asymmetry).

This essentially calls for a model involving the interaction term between the enforcement action and a crisis dummy (taking the value one for the period 2007–2009 and zero otherwise). The results reported in column 3 of Table 6 confirm this crisis-related hypothesis. Specifically, the coefficient on *enforcement action* remains negative and statistically significant and is equal to 20.3, while the coefficient on the interaction term between *enforcement action* and *crisis* is positive, statistically significant, and equal to 19. Thus, the negative effect of enforcement actions on the cost of borrowing in normal economic periods was almost completely offset during the subprime crisis. We further comment on this finding below.

C. The non-Price Effects of Enforcement Actions

In this section we analyze the impact of enforcement actions on loan duration, loan size, intensity of covenants, and use of collateral (the non-price terms of loans). We use the same empirical methodology as the one described in Section 3 and implemented in Sections 4.1 and 4.2. Table 7 reports the baseline results. We find that the average loan is extended by 6% (i.e., three months) and is 6.7% larger (i.e., $34 million) following an enforcement action. Also, enforcement actions increase the number of covenants and the probability of using collateral.[10]

[10] It would be econometrically more efficient to estimate the regression for collateral using a probit or a logit model. However, we prefer to use the high-dimensional fixed effects at the expense of econometric efficiency to better capture the full set of bank and firm fixed effects (the probit model cannot include firm fixed effects and the logit model cannot run in our sample due to the large number of fixed effects).

The latter results show that, if anything, banks become more risk averse following enforcement actions.

[Insert Table 7 about here]

We show that these baseline results are robust to (i) the inclusion of firm characteristics (Table A9); (ii) using only enforcement actions related to safety and soundness (i.e., those relating to the Basel Principles) (Table A10); and (iii) using a five-year window (Table A11).

Symmetrically to our analysis on the price terms of lending, in Table 8 we report the results from regressions including bank*firm fixed effects. Evidently, there are no notable changes in the results. Also, in Table 9 we once again demonstrate that our results lose their statistical significance when using the sample of non-lead punished banks.

[Insert Tables 8 & 9 about here]

Overall, and especially given the findings in Table 7, we can conclude that the effect of enforcement actions on the non-price terms of lending still point to increased competitiveness but are less potent than the respective effects on the price terms. This is quite intuitive from a bank managerial viewpoint, because banks receive enforcement actions first and foremost for safety and soundness reasons. If banks significantly loosen covenants and collateral requirements, this implies they decrease the quality of monitoring and screening applicants, yielding higher credit risk. Thus, extending loan length and size without lowering covenants and collateral requirements is quite rational.

D. Heterogeneous non-Price Effects of Enforcement Actions

Symmetrically to the heterogeneous price effects of enforcement actions due to a number of bank characteristics and the subprime crisis, we consider the non-price effects. In contrast to the results reported in Table 6, the coefficient estimates on the interaction terms with *Intensity of relationship lending* and *Bank's C&I loans* are statistically insignificant in the equations

evaluating non-price terms (results are available on request). Thus, we concentrate our analysis on the effect of the subprime crisis.

Our expectations are very similar to those in Section 4.2: the subprime crisis reverses improvements in non-price lending terms because of the increased costs of asymmetric information. Thus, absent an enforcement action, the non-price terms of lending would have been worse (much like the price terms). Potentially, these effects should be more pronounced for the time length and the size of the loan and less so for covenant intensity and use of collateral, because lowering the barriers for the latter two non-price loan characteristics would signal a more risky behavior during the subprime crisis. We report the results in Table 10. In line with expectations, improvements in the length and the size of the loan are almost completely offset during the crisis period. In contrast, the crisis period plays no role in the effect of actions on covenants, while the effect of actions on the probability of using collateral actually increases during the crisis period.

[Insert Table 10 about here]

V. 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 price and non-price terms of lending. This question is particularly important, as the effect can theoretically go both ways. Proponents of free markets suggest that banks pass the cost of regulatory enforcement to their customers, while proponents of regulatory intervention suggest that enforcement actions pressure punished banks to offer better lending terms to avoid losing customers.

We first study an analytical framework in which monopolistically competitive banks in the corporate-loans market supply credit to entrepreneurs. When large proportions of credit default occur, a supervisor might impose an enforcement action to discipline the risky bank.

We show that this process generates costs to the bank that worsen its terms of lending *and* have competitive and reputational effects that improve the terms of lending.

Using a novel data set with merged information on enforcement actions, syndicated loans, and bank- and firm-accounting data, we find that the pro-intervention view dominates. In particular, the price terms of lending significantly improve after an enforcement action against a lead bank of a syndicate compared to the terms on loans originated before the enforcement action. We also find that an action increases the duration and size of the average loan by approximately three months and $34 million, without lowering the barriers on the risk-related terms of lending (covenant intensity and use of collateral).

The results lose some of their potency when the punished banks specialize more in corporate lending. More important, the positive effects of enforcement actions on the price terms of lending, the duration, and the size of loans almost vanish during the 2007–2009 period. Our reading of this finding is that enforcement actions prevented any worsening (due to increased informational asymmetry costs) in the price and non-price terms of lending during the subprime crisis.

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 price and non-price terms of lending, and thus improves economic welfare. 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. These issues open new pathways for future research.

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Table 1. Summary statistics

The table reports basic summary statistics (mean, median, standard deviation, quarter centiles, minimum and maximum) for the variables used in the empirical analysis. The sample corresponds to the 3-year window used in the bulk of the empirical analysis (3,492 loans).

Variable	Mean	Median	Std. deviation	25th percentile	75th percentile	Min.	Max.
A. Dependent variables: Price terms							
AISD	145.82	125	118.7	55	200	0	1,100
Spread	142.14	125	120.46	50	200	0	1,100
Total cost borrowing	113.59	87.9	105.28	40.65	156.88	0	1,100
AISU	17.02	11.75	19.19	0	25	0	200
Facility fee	3.68	0	7.65	0	5.5	0	75
Commitment fee	13.71	0	20.14	0	25	0	200
Letter-of-credit fee	63.11	0	93.91	0	112.5	0	625
B. Dependent variables: Non-price terms							
Loan 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	14
Collateral	0.47	0	0.5	0	1	0	1
C. Explanatory variables							
Enforcement action	0.58	1	0.49	0	1	0	1
Bank capital	0.09	0.08	0.01	0.08	0.09	0.07	0.19
Allowance for loan losses	0.02	0.02	0	0.01	0.02	0	0.04
Bank liquidity	0.04	0.03	0.02	0.03	0.04	0	0.15
Bank Z-score	2.48	2.37	0.26	2.34	2.77	0.45	3.95
Firm size	7.54	7.42	1.68	6.38	8.65	0.36	13.08
Firm profitability	0.14	0.13	0.08	0.09	0.18	-0.66	0.58
Firm leverage	0.29	0.27	0.21	0.16	0.38	0	1.74
Firm Z-score	3.67	3.12	2.81	2.11	4.66	-6.08	35.64
Firm cash-flow volatility	0.04	0.04	0.03	0.03	0.05	0	0.47
Firm tangibility	0.29	0.23	0.22	0.12	0.42	0	0.95
Firm market-to-book	1.73	1.47	0.94	1.18	1.98	0.52	18
Firm rating	14.83	13	6.54	10	23	1	23
Relationship lending	0.52	1	0.5	0	1	0	1
Term loan	0.22	0	0.42	0	0	0	1
D. Variables for interaction terms							
Intensity of relationship lending	0.88	0	1.95	0	1	0	12
HHI_loan	0.46	0.44	0.05	0.44	0.45	0.28	1
Bank's C&I loans	0.12	0.09	0.06	0.09	0.1	0	0.33
Crisis	0.04	0	0.19	0	0	0	1

Table 2. Pre-analysis on the determinants of enforcement actions

The table reports coefficients and t-statistics (in parentheses) from OLS regressions with *enforcement action* as the dependent variable, year dummies, and robust standard errors clustered by bank. The unit of the analysis are syndicated loans originated in the three-year enforcement window. All explanatory variables are in one-year lags. The variables are defined in Table A1. Each model includes fixed effects as specified in the lower part of the table. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Baseline results		Include loan-purpose fixed effects		Five-year window	
	(1)	(2)	(3)	(4)	(5)	(6)
Bank capital	-2.894	3.332	-2.925	3.385	-2.771	-0.666
	[-1.182]	[0.953]	[-1.196]	[0.995]	[-1.076]	[-0.253]
Bank's C&I loans	1.682***	0.112	1.670***	0.095	4.598***	0.581
	[3.999]	[0.170]	[3.943]	[0.147]	[4.055]	[0.875]
Allowance for loan losses	14.881**	9.423	15.337**	9.062	21.536	13.573
	[2.091]	[1.106]	[2.181]	[1.104]	[1.174]	[1.852]
Bank liquidity	-1.637**	3.635	-1.651**	3.366	-1.040	-0.489
	[-2.383]	[0.682]	[-2.379]	[0.628]	[-0.549]	[-0.341]
Bank Z-score	-0.202**	-0.090	-0.204**	-0.092	-0.421***	-0.094*
	[-2.206]	[-0.770]	[-2.215]	[-0.806]	[-3.914]	[-1.761]
Observations	3,492	3,492	3,492	3,492	5,195	5,195
Adjusted R-squared	0.681	0.903	0.682	0.904	0.659	0.919
Loan purpose effects	N	N	Y	Y	N	N
Year effects	Y	Y	Y	Y	Y	Y
Bank effects	N	Y	N	Y	N	Y

Table 3. Price terms of lending and enforcement actions: Baseline regressions

The table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	AISD	Spread	Total cost of borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-22.829***	-22.225***	-19.553***	-3.251***	-0.604***	-2.612***	-6.985***
	[-11.960]	[-11.936]	[-12.238]	[-13.667]	[-6.552]	[-8.114]	[-9.091]
Log(Loan maturity)	-7.051*	-7.016*	1.659	1.553***	-0.035	1.496**	8.077**
	[-2.062]	[-2.017]	[0.217]	[4.588]	[-0.329]	[2.869]	[2.277]
Log(Loan size)	-3.861**	-3.685**	-1.109	-1.969***	-0.176**	-1.958***	-2.317*
	[-2.356]	[-2.334]	[-0.275]	[-10.543]	[-2.646]	[-7.245]	[-2.062]
Collateral	34.724**	32.990**	28.783**	5.379***	1.734***	3.483***	38.847***
	[2.351]	[2.202]	[2.981]	[7.961]	[5.940]	[4.716]	[6.993]
Relationship lending	-3.310	-3.317	-1.744**	0.476**	0.007	0.415*	0.171
	[-0.767]	[-0.766]	[-2.355]	[2.506]	[0.166]	[1.948]	[0.158]
Term loan	32.339***	32.748***	106.332***	-25.483***	-0.409***	-24.578***	-60.597***
	[4.553]	[4.571]	[11.151]	[-12.926]	[-4.240]	[-13.176]	[-9.919]
Observations	3,492	3,492	3,324	3,492	3,492	3,492	3,492
Adjusted R-squared	0.688	0.690	0.651	0.341	-0.038	0.386	0.251
Loan purpose effects	Y	Y	Y	Y	Y	Y	Y
Year effects	Y	Y	Y	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table 4. Price terms of lending and enforcement actions: Including bank*firm fixed effects

The table reports coefficients and t-statistics (in parentheses) from the estimations of the same models with Table 2, while including bank*firm fixed effects. The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	AISD	Spread	Total cost of borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-23.015***	-22.512***	-19.859***	-3.258***	-0.503***	-2.710***	-6.784***
	[-11.111]	[-10.697]	[-13.428]	[-14.168]	[-11.472]	[-9.866]	[-7.721]
Log(Loan maturity)	-6.858*	-6.811*	1.917	1.564***	-0.047	1.518**	8.269**
	[-2.021]	[-1.981]	[0.254]	[4.906]	[-0.831]	[2.938]	[2.258]
Log(Loan size)	-4.000**	-3.809**	-1.149	-2.019***	-0.191***	-1.993***	-2.412**
	[-2.181]	[-2.217]	[-0.285]	[-9.885]	[-3.500]	[-6.909]	[-2.312]
Collateral	32.367**	30.642**	27.304***	4.924***	1.726***	3.045***	37.973***
	[2.391]	[2.224]	[3.049]	[7.617]	[6.886]	[4.719]	[6.194]
Relationship lending	-4.017	-3.923	-1.831**	0.365*	-0.094	0.391**	-0.033
	[-1.020]	[-0.970]	[-2.259]	[2.015]	[-0.903]	[2.249]	[-0.026]
Term loan	31.873***	32.259***	106.182***	-25.509***	-0.386***	-24.625***	-60.510***
	[4.332]	[4.342]	[10.983]	[-12.888]	[-3.843]	[-13.210]	[-9.908]
Observations	3,492	3,492	3,324	3,492	3,492	3,492	3,492
Adjusted R-squared	0.691	0.693	0.653	0.345	-0.033	0.390	0.255
Loan purpose effects	Y	Y	Y	Y	Y	Y	Y
Year effects	Y	Y	Y	Y	Y	Y	Y
Bank*Firm effects	Y	Y	Y	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table 5. Price terms of lending and enforcement actions: Non-lead banks only

The table reports coefficients and t-statistics (in parentheses) from the sample with non-lead banks only (placebo tests). The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	AISD	Spread	Total cost of borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-0.850	-0.896	-1.604	0.155	0.046	0.246	-1.783
	[-0.178]	[-0.189]	[-0.365]	[0.104]	[0.267]	[0.343]	[-0.502]
Log(Loan maturity)	-6.070	-6.482	-0.437	1.616	0.413	0.939	15.051
	[-0.066]	[-0.048]	[-0.128]	[0.060]	[0.070]	[0.353]	[0.068]
Log(Loan size)	-0.759	-1.171	-0.467	-0.425	0.412	-0.741	2.067
	[-0.006]	[-0.009]	[-0.109]	[-0.022]	[0.079]	[-0.040]	[0.027]
Collateral	30.393	33.080	28.605***	5.625	-2.687	8.148	45.180
	[0.420]	[0.832]	[4.236]	[0.862]	[-0.747]	[0.282]	[0.711]
Relationship lending	-2.979	-3.557	-3.108	-1.167	0.578	-1.696	-6.110
	[-0.046]	[-0.089]	[-0.632]	[-0.095]	[0.123]	[-0.114]	[-0.072]
Term loan	29.372	30.828	109.577***	-24.052**	-1.456	-22.053	-83.301***
	[0.527]	[0.819]	[16.394]	[-2.203]	[-0.410]	[-1.420]	[-3.528]
Observations	3,253	3,253	3,147	3,253	3,253	3,253	3,253
Adjusted R-squared	0.753	0.760	0.755	0.437	0.404	0.454	0.460
Loan purpose effects	Y	Y	Y	Y	Y	Y	Y
Year effects	Y	Y	Y	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table 6. Total cost of borrowing and enforcement actions: Heterogeneity due to bank characteristics and the subprime crisis

This table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the three-year enforcement window. In all specifications dependent variable is the total cost of borrowing. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. All regressions include the control variables of Table 3 and the lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
Enforcement action	-20.735***	-24.511***	-20.336***
	[-9.296]	[-4.515]	[-24.675]
Intensity of relationship lending	-3.408***		
	[-4.386]		
Enforcement action * Intensity of relationship lending	-1.371***		
	[-3.084]		
HHI_loan			
Enforcement action * HHI_loan			
Bank capital			
Enforcement action * Bank capital			
Bank's C&I loans		439.155	
		[0.644]	
Enforcement action * Bank's C&I loans		76.986***	
		[3.734]	
Crisis			0.000
			[0.000]
Enforcement action * Crisis			18.990***
			[18.591]
Observations	3,324	3,492	3,492
Adjusted R-squared	0.651	0.651	0.651
Controls as in Table 3	Y	Y	Y
Loan purpose effects	Y	Y	Y
Year effects	Y	Y	Y
Firm effects	Y	Y	Y
Bank effects	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm

Table 7. Non-price terms of lending and enforcement actions: Baseline regressions

The table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
Dependent variable:	Log(Loan maturity)	Log(Loan size)	Log(1+Total covenants)	Collateral
Enforcement action	0.060***	0.067***	0.062***	0.012**
	[8.601]	[4.160]	[8.525]	[2.579]
Log(Loan maturity)		0.152***	0.026**	0.014
		[5.672]	[2.733]	[0.789]
Log(Loan size)	0.046***		0.022***	0.011***
	[6.179]		[6.463]	[3.703]
Collateral	0.050	0.126***	0.806***	
	[0.824]	[4.129]	[14.708]	
Relationship lending	-0.016**	-0.069***	-0.077***	0.000
	[-2.547]	[-7.462]	[-9.322]	[0.002]
Term loan	0.157***	0.038	0.020***	-0.003
	[9.927]	[0.493]	[3.900]	[-0.471]
Observations	3,492	3,492	3,492	3,492
Adjusted R-squared	0.524	0.713	0.806	0.741
Loan purpose effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table 8. Non-price terms of lending and enforcement actions: Including bank*firm fixed effects

The table reports coefficients and t-statistics (in parentheses) from the estimations of the same models with Table 7, but including bank*firm fixed effects. The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
Dependent variable:	Log(Loan maturity)	Log(Loan size)	Log(1+Total covenants)	Collateral
Enforcement action	0.063***	0.065***	0.068***	0.010*
	[10.965]	[3.662]	[8.657]	[1.887]
Log(Loan maturity)		0.152***	0.026**	0.016
		[5.662]	[2.927]	[0.940]
Log(Loan size)	0.046***		0.020***	0.010**
	[6.266]		[7.946]	[2.628]
Collateral	0.057	0.115**	0.804***	
	[0.992]	[2.907]	[14.001]	
Relationship lending	-0.020**	-0.069***	-0.084***	0.002
	[-2.588]	[-8.309]	[-6.367]	[0.180]
Term loan	0.156***	0.040	0.023***	-0.003
	[9.873]	[0.515]	[5.728]	[-0.516]
Observations	3,492	3,492	3,492	3,492
Adjusted R-squared	0.525	0.714	0.808	0.745
Loan purpose effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Bank*Firm effects	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table 9. Non-price terms of lending and enforcement actions: Non-lead banks only

The table reports coefficients and t-statistics (in parentheses) from the sample with non-lead banks only (placebo tests). The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	(1) Log(Loan maturity)	(2) Log(Loan size)	(3) Log(1+Total covenants)	(4) Collateral
Enforcement action	-0.013	-0.023	-0.022	0.004
	[-0.633]	[-1.473]	[-0.622]	[1.020]
Log(Loan maturity)		0.367***	0.027	0.021
		[15.021]	[0.075]	[0.037]
Log(Loan size)	0.065		0.022	0.001
	[0.098]		[0.022]	[0.004]
Collateral	0.094	0.361***	0.816	
	[0.294]	[16.373]	[1.438]	
Relationship lending	-0.061	-0.005	-0.033	-0.065
	[-0.105]	[-0.161]	[-0.040]	[-0.208]
Term loan	0.169	0.212***	0.032	0.001
	[0.353]	[12.092]	[0.071]	[0.005]
Observations	3,253	3,253	3,253	3,253
Adjusted R-squared	0.482	0.594	0.839	0.811
Loan purpose effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table 10. Non-price terms of lending and enforcement actions: Heterogeneity due to the subprime crisis

The table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. All regressions include the control variables of Table 6 and the lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
Dependent variable:	Log(Loan maturity)	Log(Loan size)	Log(1+Total covenants)	Collateral
Enforcement action	0.062***	0.071***	0.065***	0.011*
	[11.610]	[4.873]	[9.825]	[2.094]
Crisis	-0.219	0.299	0.009	0.177
	[-0.629]	[0.933]	[0.017]	[0.414]
Enforcement action * Crisis	-0.055***	-0.065***	-0.011	0.022***
	[-8.456]	[-5.001]	[-1.028]	[4.045]
Observations	3,492	3,492	3,492	3,492
Adjusted R-squared	0.524	0.713	0.806	0.741
Control variables as in Table 7	Y	Y	Y	Y
Loan purpose effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Supplementary Appendix

This appendix is intended as an online supplement to the data set and results presented in the main body of the text. Table A1 explicitly defines the variables used in the empirical analysis and Table A2 provides the details of the sample of enforcement actions included in the empirical analysis and how we reached the decision to include a specific enforcement action or not after reading its rationale.

Table A3 reports the means of the loan-level response variables before and after the enforcement action enacted on a lead bank. These responses provide initial anecdotal evidence that the punished banks improve the terms of lending following the enforcement actions.

Table A4 provides sensitivity results on the baseline pre-analysis results reported in Table 2. The rest of the Tables in this appendix provide sensitivity analyses for Tables 3-10 as discussed in the main text.

Table A1. 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
Spread	Spread over LIBOR paid on drawn amounts on lines of credit.	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 (2015) to calculate the measure.	DealScan
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. Dependent variables: Non-price terms		
Maturity	Facility duration in months.	DealScan DealScan
Loan size	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
C. Explanatory variables		
Enforcement action	Dummy equal to 1 in the year (or two years) after the year of the enforcement action and zero for the year before the enforcement action. For the year of the enforcement action the dummy equals to 1 if the loan was originated after the enactment of the enforcement action and 0 if the loan was originated before the enactment. See Table A.2 for more on enforcement actions.	Regulators' websites
Bank capital	Tier 1 plus Tier 2 capital divided by risk-weighted assets.	Call Reports
Bank's C&I loans	Bank's C&I loans to total assets ratio (year before the loan origination).	Call Reports
Allowance for loan losses	Ratio of the allowance for loan losses to total loans.	Call Reports
Bank liquidity	Ratio of liquid assets (cash and short-term securities) to total assets.	Call Reports
Bank Z-score	(ROA+EA)/σROA, where ROA is the return on assets, EA is the ratio of equity capital to total assets and σROA is calculated over a rolling five-year window. We take the natural logarithm of this measure because the Z-score is highly skewed.	Call Reports
Firm size	Log of total firm assets.	Compustat
Firm profitability	The ratio of earnings before interest, taxes, depreciation, and amortization (EBITDA) to total assets.	Compustat

Firm leverage	The ratio of total debt to total assets.	Compustat
Firm Z-score	The modified Altman's (1968) Z-score (= (1.2 Working Capital+1.4 Retained Earnings+3.3 EBIT+0.999 Sales)/Total Assets).	Compustat
Firm cash-flow volatility	The standard deviation of the borrower's quarterly cash flow from operations over the five fiscal years prior to the loan origination year, scaled by the total assets.	Compustat
Firm tangibility	The ratio of tangible assets to total assets.	Compustat
Firm market-to-book ratio	The ratio of the market value of assets to the book value of assets.	Compustat
Firm rating	The numeric measure of S&P debt rating.	Compustat
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
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 type is one of the following: "Term Loan", "Term Loan A"-"Term Loan H", or "Delay Draw Term Loan".	DealScan

D. Variables for interaction terms

Intensity of relationship lending	The number of loans by lead bank to the same borrower in the last five years.	DealScan
HHI_loan	Herfindahl-Hirschman index based on different types of loans, such as real estate loan, commercial and industry loans, consumer loans, and others.	Call Reports
Bank's C&I loans	Bank's C&I loans to total assets ratio.	Call Reports
Crisis	Dummy that takes the value 1 for crisis years (2007-2009) and 0 otherwise.	

Table A.2
Information on formal enforcement actions included in the empirical analysis

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 first and foremost 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 must 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 very closely related to safety and soundness issues and, according to the recent study of Delis, Staikouras, and Tsoumas (2016), are the only ones with a direct impact on the risk-taking behavior of banks. Thus, these are the enforcement 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.2. The first type 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.2. These actions are very 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 are excluded 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 would be very remotely related 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.

Table A3. Means of the terms of lending before and after the enforcement action

The table reports means of the lending terms (dependent variables of our study) before and after the enforcement action.

Variable	Before enforcement	After enforcement
A. Dependent variables: Price terms		
AISD	150.98	138.56
Spread	145.79	134.33
Total cost borrowing	117.31	107.80
AISU	17.68	16.80
Facility fee	5.19	4.23
Commitment fee	12.98	12.99
Letter-of-credit fee	61.65	59.62
B. Dependent variables: Non-price terms		
Loan maturity (log)	3.57	3.72
Loan size (log)	5.40	5.51
Total covenants (log)	1.42	1.44
Collateral	0.41	0.43

Table A4. Additional pre-analysis on the determinants of enforcement actions

The table reports coefficients and t-statistics (in parentheses) from OLS regressions with *enforcement action* as the dependent variable, bank and year fixed effects, and robust standard errors clustered by bank. The unit of the analysis are syndicated loans originated in the three-year enforcement window. All explanatory variables are in one-year lags. The first five variables are defined in Table A1. Equity multiplier is the ratio of total assets to total equity. Non-performing loans is the ratio of non-performing loans to total loans. Operating expenses is the ratio of overheads to total assets. Non-interest income is the ratio of non-interest income to total assets. Bank size is the natural logarithm of total assets.

	(1) Total equity capital	(2) Non-performing loans	(3) Operating expenses	(4) Non-interest income	(5) Bank size	(6) Regulator-year fixed effects
Bank capital		4.003	-0.192	-0.534	-1.006	0.494
		[1.074]	[-0.043]	[-0.119]	[-0.224]	[0.154]
Bank's C&I loans	0.301	0.138	0.147	0.140	0.768	-0.065
	[0.427]	[0.222]	[0.234]	[0.221]	[1.071]	[-0.143]
Allowance for loan losses	12.046	-2.057	-14.433	-10.108	-20.199	-7.333
	[1.143]	[-0.208]	[-1.474]	[-0.912]	[-1.503]	[-0.531]
Bank liquidity	4.201	4.644	2.735	2.837	0.543	-3.396
	[0.824]	[0.776]	[0.438]	[0.454]	[0.082]	[-1.228]
Bank Z-score	-0.023	-0.140	-0.143	-0.148	-0.157	-0.134
	[-0.183]	[-1.493]	[-1.501]	[-1.507]	[-1.573]	[-1.450]
Equity multiplier	-0.030					
	[-1.196]					
Non-performing loans		5.485	4.218	3.679	4.403	0.797
		[1.037]	[0.767]	[0.678]	[0.887]	[0.220]
Operating expenses			-0.105	-0.100	-0.122	-0.035
			[-1.348]	[-0.988]	[-1.150]	[-0.296]
Non-interest income				-0.322	-0.105	-0.025
				[-0.926]	[-0.264]	[-0.053]
Bank size					-1.159	-0.715
					[-1.550]	[-1.447]
Observations	3,492	3,492	3,492	3,492	3,492	3,492
Adjusted R-squared	0.903	0.903	0.904	0.905	0.906	0.984
Year effects	Y	Y	Y	Y	Y	N
Bank effects	Y	Y	Y	Y	Y	Y
Regulator*year fixed effects	N	N	N	N	N	Y

Table A5. Price terms of lending and enforcement actions: Including firm characteristics

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 unit of the analysis are syndicated loans originated in the three-year enforcement window. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	AISD	Spread	Total cost borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-15.823***	-15.078***	-9.982***	-1.675*	-0.744	-0.856	-9.589**
	[-4.110]	[-4.422]	[-3.179]	[-1.866]	[-1.323]	[-1.293]	[-2.105]
Firm size	-17.700**	-17.097**	-16.582*	-5.735**	-0.604	-5.013**	-4.213
	[-2.214]	[-2.265]	[-2.021]	[-2.158]	[-0.558]	[-2.224]	[-0.387]
Firm profitability	-173.032	-166.591	-174.189**	-51.560**	-6.441	-40.294**	57.308
	[-1.697]	[-1.583]	[-2.221]	[-2.608]	[-0.671]	[-2.621]	[0.625]
Firm leverage	7.590	5.230	29.005	3.950	2.360	0.064	-48.676
	[0.215]	[0.141]	[1.254]	[0.567]	[0.604]	[0.014]	[-1.315]
Firm Z-score	-2.302	-2.289	1.116	0.755	-0.013	0.618	-2.544
	[-1.137]	[-1.176]	[0.507]	[0.920]	[-0.027]	[1.377]	[-0.985]
Firm cash-flow volatility	127.164	139.230	-112.193	62.904	-12.066	82.967	-106.991
	[0.578]	[0.638]	[-0.552]	[0.852]	[-0.521]	[1.087]	[-0.428]
Firm tangibility	-98.979**	-98.228**	-81.949*	-50.723***	-0.751	-49.585***	-63.058
	[-2.409]	[-2.456]	[-1.828]	[-3.186]	[-0.110]	[-3.324]	[-0.848]
Firm market-to-book	-9.533	-9.383	-7.549*	-2.606	-0.151	-2.478**	-11.565
	[-1.651]	[-1.500]	[-1.913]	[-1.621]	[-0.192]	[-2.060]	[-1.692]
Firm rating	2.010	2.367	1.383	-0.595	-0.357***	-0.239	1.632
	[1.313]	[1.563]	[1.680]	[-1.223]	[-4.440]	[-0.500]	[0.891]
Log(Loan maturity)	-3.401	-3.938	1.804	1.797***	0.537	0.923*	18.954***
	[-0.671]	[-0.832]	[0.897]	[3.128]	[1.354]	[1.988]	[8.016]
Log(Loan size)	0.350	-0.195	-3.695*	-0.392	0.545**	-1.119*	0.189
	[0.169]	[-0.101]	[-1.960]	[-0.496]	[2.441]	[-1.702]	[0.057]
Collateral	39.236***	42.506***	29.264***	8.182**	-3.270***	11.470***	66.164***
	[4.336]	[4.659]	[3.193]	[2.673]	[-4.466]	[4.696]	[5.892]
Relationship lending	-5.003*	-4.998*	-3.148	-1.347	-0.005	-1.294	-10.179**
	[-1.730]	[-1.772]	[-1.191]	[-1.505]	[-0.010]	[-1.476]	[-2.737]
Term loan	28.408***	30.455***	91.199***	-26.813***	-2.047***	-24.391***	-148.470***
	[11.727]	[11.775]	[10.139]	[-13.065]	[-3.823]	[-10.789]	[-10.683]
Observations	1,730	1,730	1,730	1,730	1,730	1,730	1,730
Adjusted R-squared	0.771	0.782	0.790	0.431	0.482	0.490	0.554
Loan purpose effects	Y	Y	Y	Y	Y	Y	Y
Year effects	Y	Y	Y	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table A6. Price terms of lending and enforcement actions: Including firm and bank characteristics

The table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	AISD	Spread	Total cost borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-19.425***	-18.546***	-16.127***	-2.417***	-0.879**	-1.524*	-11.448***
	[-4.863]	[-4.634]	[-5.196]	[-2.871]	[-2.166]	[-1.811]	[-3.003]
Firm size	-4.628	-5.588	-3.304	-3.161	0.960	-4.026**	-7.434
	[-0.430]	[-0.519]	[-0.375]	[-1.560]	[1.074]	[-1.986]	[-0.722]
Firm profitability	-201.951***	-196.690***	-178.422***	-15.991	-5.261	-7.891	-6.089
	[-3.078]	[-3.016]	[-3.350]	[-1.016]	[-0.785]	[-0.533]	[-0.087]
Firm leverage	64.788	65.741	38.920	6.350	-0.953	7.104	-26.411
	[1.631]	[1.639]	[1.263]	[0.853]	[-0.308]	[1.112]	[-0.956]
Firm Z-score	-0.528	-0.368	-0.258	0.563	-0.160	0.686*	-1.435
	[-0.236]	[-0.163]	[-0.129]	[1.280]	[-0.697]	[1.740]	[-0.536]
Firm cash-flow volatility	-83.305	-99.549	-117.468	-45.672	16.245	-58.791	-61.536
	[-0.482]	[-0.581]	[-0.886]	[-1.202]	[0.742]	[-1.472]	[-0.315]
Firm tangibility	-40.645	-40.859	-26.653	-21.549	0.214	-22.346*	-27.663
	[-0.655]	[-0.656]	[-0.479]	[-1.586]	[0.042]	[-1.748]	[-0.495]
Firm market-to-book	-6.017	-6.396	-0.832	-1.500	0.380	-1.955*	-4.166
	[-1.121]	[-1.159]	[-0.201]	[-1.452]	[0.500]	[-1.683]	[-0.764]
Firm rating	1.165	1.396	0.928	-0.212	-0.231	0.011	-0.712
	[0.702]	[0.852]	[0.917]	[-0.560]	[-1.510]	[0.032]	[-0.505]
Bank capital	-259.412	-421.640	-264.393	46.463	162.228	-105.452	1,093.892
	[-0.193]	[-0.315]	[-0.191]	[0.165]	[1.471]	[-0.447]	[0.617]
Bank's C&I loans	105.933	175.012	46.844	-6.310	-69.078	45.009	187.328
	[0.254]	[0.445]	[0.106]	[-0.091]	[-1.348]	[0.780]	[0.390]
Allowance for loan losses	-1,035.674	-1,063.111	-295.179	45.963	27.436	91.807	-4,048.022
	[-0.320]	[-0.329]	[-0.101]	[0.068]	[0.111]	[0.151]	[-0.966]
Bank liquidity	847.495	687.819	1,086.869*	217.078	159.675**	25.649	1,098.069
	[1.123]	[0.901]	[1.764]	[1.155]	[1.979]	[0.155]	[1.119]
Bank Z-score	18.958	18.707	4.878	-2.795	0.251	-1.779	-29.263
	[1.221]	[1.183]	[0.332]	[-0.706]	[0.168]	[-0.519]	[-1.297]
Log(Loan maturity)	-0.747	-1.387	4.710*	1.995***	0.639**	1.183**	20.040***
	[-0.207]	[-0.388]	[1.717]	[3.247]	[2.419]	[1.997]	[6.765]
Log(Loan size)	-1.560	-2.015	-2.400	-0.800	0.455***	-1.531***	1.546
	[-0.568]	[-0.737]	[-0.987]	[-1.438]	[2.814]	[-2.738]	[0.548]
Collateral	36.600***	37.935***	30.152***	6.195***	-1.335	7.630***	51.613***
	[3.463]	[3.613]	[3.532]	[3.517]	[-1.622]	[4.368]	[7.272]
Relationship lending	-2.113	-1.243	-1.916	-1.159	-0.870*	-0.205	-6.492*
	[-0.546]	[-0.327]	[-0.602]	[-1.576]	[-1.815]	[-0.276]	[-1.729]
Term loan	28.192***	30.370***	90.936***	-26.937***	-2.178***	-24.414***	-148.584***
	[10.640]	[10.863]	[10.236]	[-12.390]	[-4.777]	[-10.526]	[-10.395]
Observations	1,729	1,729	1,729	1,729	1,729	1,729	1,729
Adjusted R-squared	0.772	0.782	0.790	0.432	0.493	0.489	0.553
Loan purpose effects	Y	Y	Y	Y	Y	Y	Y
Year effects	Y	Y	Y	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table A7. Price terms of lending and enforcement actions: Only actions related to Basel Principles

The table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. In these specifications we use only the loans originated by banks that received enforcement actions related to the Basel Principles and exclude the loans by banks that received actions related to board members, managers, and banks' affiliated parties (see also Table A2). All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	AISD	Spread	Total cost borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-21.168***	-20.524***	-19.663***	-3.180***	-0.644	-2.560***	-11.790***
	[-7.403]	[-8.105]	[-5.447]	[-7.778]	[-1.611]	[-7.153]	[-3.555]
Log(Loan maturity)	-4.399	-5.032	4.470***	1.726***	0.632***	0.846**	16.582***
	[-0.741]	[-0.900]	[3.520]	[6.250]	[6.589]	[2.303]	[6.170]
Log(Loan size)	-3.945***	-4.238***	-5.571***	-0.800*	0.293***	-1.120**	3.611
	[-3.363]	[-3.497]	[-3.170]	[-2.037]	[5.029]	[-2.677]	[1.040]
Collateral	37.597***	39.746***	35.804***	6.778***	-2.148***	8.822***	45.172***
	[3.782]	[3.822]	[4.394]	[4.167]	[-2.863]	[6.709]	[11.381]
Relationship lending	-2.972	-3.863	-1.767	-0.806	0.892***	-1.631***	-7.923***
	[-0.634]	[-0.806]	[-0.381]	[-1.313]	[4.152]	[-3.053]	[-3.320]
Term loan	35.025***	37.099***	110.922***	-24.002***	-2.075***	-21.515***	-88.109***
	[13.749]	[13.394]	[13.761]	[-15.637]	[-8.314]	[-13.577]	[-19.772]
Observations	2,594	2,594	2,594	2,594	2,594	2,594	2,594
Adjusted R-squared	0.729	0.736	0.724	0.385	0.406	0.392	0.445
Loan purpose effects	Y	Y	Y	Y	Y	Y	Y
Year effects	Y	Y	Y	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table A8. Price terms of lending and enforcement action: Five-year window

The table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the five-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	AISD	Spread	Total cost borrowing	AISU	Facility fee	Commitment fee	Letter-of-credit fee
Enforcement action	-15.257***	-14.344***	-13.039***	-2.066***	-0.913***	-1.211**	-7.836***
	[-3.180]	[-3.068]	[-2.721]	[-3.845]	[-3.503]	[-2.525]	[-3.926]
Log(Loan maturity)	-6.394*	-7.008*	-1.457	2.322***	0.613***	1.445***	13.477***
	[-1.687]	[-1.944]	[-0.261]	[6.414]	[2.871]	[4.175]	[4.301]
Log(Loan size)	-3.230**	-3.649**	-0.515	-0.875	0.419***	-1.103*	3.609
	[-2.069]	[-2.463]	[-0.160]	[-1.517]	[2.673]	[-1.895]	[1.297]
Collateral	24.626***	27.216***	23.154***	5.906***	-2.590***	8.081***	43.692***
	[3.846]	[4.159]	[5.459]	[4.231]	[-3.773]	[8.570]	[9.205]
Relationship lending	-3.385	-3.704	-1.888	-0.868	0.319	-1.153**	-6.681*
	[-1.235]	[-1.330]	[-0.746]	[-1.196]	[0.831]	[-2.016]	[-1.762]
Term loan	31.615***	33.281***	112.720***	-25.021***	-1.666***	-22.411***	-82.069***
	[6.230]	[6.162]	[22.271]	[-14.332]	[-4.164]	[-11.513]	[-12.709]
Observations	5,195	5,195	5,022	5,195	5,195	5,195	5,195
Adjusted R-squared	0.752	0.756	0.754	0.473	0.416	0.484	0.476
Loan purpose effects	Y	Y	Y	Y	Y	Y	Y
Year effects	Y	Y	Y	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y	Y	Y	Y
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table A9. Non-price terms of lending and enforcement actions: Including firm characteristics

The table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
Dependent variable:	Log(Loan maturity)	Log(Loan size)	Log(1+Total covenants)	Collateral
Enforcement action	0.086**	0.103***	-0.119*	0.046***
	[2.763]	[3.669]	[-2.012]	[2.829]
Firm size	-0.012	0.463***	0.188**	0.036
	[-0.237]	[5.956]	[2.375]	[0.745]
Firm profitability	0.201	2.803***	1.404	-0.198
	[0.554]	[3.187]	[1.413]	[-0.502]
Firm leverage	-0.321***	0.028	-0.395	0.506***
	[-3.309]	[0.152]	[-0.624]	[4.305]
Firm Z-score	-0.009	0.037	0.023	-0.016
	[-0.873]	[1.526]	[1.037]	[-0.871]
Firm cash-flow volatility	0.156	2.754*	-0.915	2.374***
	[0.117]	[1.881]	[-0.765]	[3.665]
Firm tangibility	-0.726**	0.453	-0.615*	0.264
	[-2.262]	[1.314]	[-1.762]	[0.771]
Firm market-to-book	0.010	-0.130***	-0.117***	0.025
	[0.219]	[-3.649]	[-3.099]	[0.655]
Firm rating	-0.002	-0.011	-0.003	0.008
	[-0.128]	[-0.765]	[-0.177]	[0.730]
Log(Loan maturity)		0.197***	0.039	0.012
		[6.391]	[1.401]	[0.839]
Log(Loan size)	0.113***		0.076***	-0.007
	[6.366]		[3.415]	[-1.056]
Collateral	0.063	-0.069	1.220***	
	[0.786]	[-1.292]	[20.275]	
Relationship lending	-0.060	0.029	-0.063	-0.048***
	[-1.296]	[1.102]	[-1.086]	[-3.018]
Term loan	0.160***	-0.036	0.017	0.015
	[7.008]	[-0.363]	[1.409]	[1.201]
Observations	1,730	1,730	1,730	1,730
Adjusted R-squared	0.387	0.733	0.754	0.818
Loan purpose effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y
Clustered standard errors	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table A10. Non-price terms of lending and enforcement actions: Only actions related to Basel Principles

The table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the three-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. In these specifications we use only the loans originated by banks that received enforcement actions related to the Basel Principles and exclude the loans by banks that received actions related to board members, managers, and banks' affiliated parties (see also Table A2). All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	(1) Log(Loan maturity)	(2) Log(Loan size)	(3) Log(1+Total covenants)	(4) Collateral
Enforcement action	0.108***	0.222***	-0.059***	0.057***
	[3.967]	[8.170]	[-3.785]	[7.880]
Log(Loan maturity)		0.180***	0.052***	0.018
		[5.333]	[3.491]	[1.469]
Log(Loan size)	0.072***		0.031**	0.007
	[4.362]		[2.452]	[1.290]
Collateral	0.085	0.078	0.798***	
	[1.331]	[1.559]	[14.288]	
Relationship lending	-0.037	0.027	-0.055	-0.070***
	[-1.161]	[1.038]	[-1.301]	[-4.909]
Term loan	0.161***	0.081	0.029**	0.006
	[11.272]	[0.553]	[2.584]	[0.936]
Observations	2,594	2,594	2,594	2,594
Adjusted R-squared	0.435	0.720	0.826	0.805
Loan purpose effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y
Clustered standard errors	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table A11. Non-price terms of lending and enforcement action: Five-year window

The table reports coefficients and t-statistics (in parentheses). The unit of the analysis are syndicated loans originated in the five-year enforcement window. The dependent variable of each specification is shown on the first line of the table. The variables are defined in Table A1. All regressions are estimated with OLS on the fixed-effects model, with robust standard errors clustered by bank and firm. The lower part of the table indicates the type of fixed effects included in the specifications. The *, **, *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
Dependent variable:	Log(Loan maturity)	Log(Loan size)	Log(1+Total covenants)	Collateral
Enforcement action	0.095***	0.163***	-0.052**	0.030**
	[2.788]	[3.004]	[-2.633]	[2.601]
Log(Loan maturity)		0.175***	0.055***	0.021**
		[5.337]	[3.080]	[2.436]
Log(Loan size)	0.063***		0.024**	-0.002
	[5.628]		[2.036]	[-0.498]
Collateral	0.081**	-0.022	0.725***	
	[2.403]	[-0.535]	[20.260]	
Relationship lending	-0.070***	0.022	-0.043	-0.032***
	[-3.602]	[0.961]	[-1.330]	[-4.331]
Term loan	0.143***	0.078	0.030***	0.000
	[10.743]	[0.840]	[3.724]	[0.023]
Observations	5,195	5,195	5,195	5,195
Adjusted R-squared	0.513	0.746	0.829	0.787
Loan purpose effects	Y	Y	Y	Y
Year effects	Y	Y	Y	Y
Firm effects	Y	Y	Y	Y
Bank effects	Y	Y	Y	Y
Clustered standard errors	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

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