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Enforcement Actions on Banks and the Structure of Loan Syndicates

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Enforcement Actions on Banks and the Structure of Loan Syndicates

Abstract

We investigate the effect of regulatory enforcement actions on banks' reputation by estimating the effect of non-compliance with laws and regulations among lead arrangers on the structure of syndicated loans. Consistent with a regulatory reputational stigma, a punished lead arranger increases her loan share to entice participants to continue to co-finance the loan. Consequently, when punished lead arranger initiates a new syndicated loan, then this loan tends to be more concentrated and co-funded by participants with previous collaboration with the lead arranger. However, the observed share increases by punished lead arrangers are seemingly mitigated by extending the loan guarantees, performance pricing provisions, and covenants.

JEL classification: D82; G21; G28

Keywords: Syndicated loan market; Syndicate structure; Reputation of lead arranger; Enforcement actions; Incomplete and asymmetric information; Loan-level data

I. Introduction

What effects do regulatory enforcement actions enacted on banks for breaches of laws and regulations have on the punished lead banks' reputation? Our study is the first to address this question coupling comprehensive sanctions with syndicated loan market data, to test hypotheses derived from a representative theoretical framework. In the syndicated loan market, the lead (principal) arranger and the participants form a syndicate to provide large corporate loans that a single bank cannot (or is unwilling to) finance alone. Regulatory enforcement actions enacted on lead arrangers potentially impose a reputational burden between the punished banks and their relationship with the participant banks. Reputational considerations play a central role in the syndicated loan market because participant lenders repeatedly interact and have long organizational memories that influence their beliefs and asymmetric information. Evidence from this burden would come from a significant change in the loan syndicate structure, whereby the punished lead arranger is forced to retain a considerably larger share of the syndicated loan. This is the first paper that investigates the reputation burden stemming from the punishment of banks with enforcement actions.

We first discuss the link between the syndicate structure and the punished lead bank's reputational loss. Using a theoretical framework, we posit that a punished lead arranger faces a reputational loss reflected in her diminished ability to attract participants and in turn enhanced needs to incentivize participant banks to co-finance the project. To do so, the lead arranger must hold a larger share of the loan that will commit the arranger to a great deal of monitoring effort (and thus in the model to increase the project's success potential). Apart from the screening and monitoring effort, the prospective risk to earnings or capital arising from a joint venture with a punished lead arranger might also lead to concentrated syndicates and loans with higher covenants restrictions.

We empirically examine the validity of these theoretical arguments using data from three different sources. Specifically, we use the enforcement actions enacted on lead lenders from a hand-collected dataset by Delis et al. (2017b), data on all U.S. syndicated loans (the unit of our analysis) available in DealScan, and matched identification codes on banks from the Call Reports. We stress that these enforcement actions are quite important penalties, enacted purely for safety and soundness reasons and thus are fairly homogeneous events. Our data set spans the period 1997 through 2014 to allow a window around enforcement actions enacted during the years 2001-2010.

Our empirical model aims to establish causality running from the enforcement actions to the structure of the syndicated loan. Our main explanatory variable is a dummy that takes the value one for loans originated by punished banks after the enforcement action, zero for the loans originated by punished banks before the enforcement action, and also zero for loans originated by non-punished banks. More closely related to our theoretical model, we use as dependent variable the lead arranger's share of the loan. In alternative specifications, we also use as dependent variables the Herfindahl–Hirschman index (HHI) to analyze the concentration of holdings within the syndicate, the number of lenders participating in the syndicate, the total number of financial and general covenants, and the percentage of shares held by participant banks that have the same supervisor as the lead bank.

Our main identification method accounts for potential unobserved variables, especially bank-year and firm-year ones that might bias our inference on the effect of the enforcement actions. Specifically, our data set comprises a cross-section of loans. Each lead arranger (including the punished ones) originates many loans, *sometimes along with other lead arranger(s)* within one year. This allows including bank*year fixed effects because the different lead arrangers in the same loan do not receive enforcement actions in the same year. Thus, we use a difference-in-differences-in-differences (DDD) exercise, where we compare

the share of the punished lead bank with: (i) its own share in other similar loans (given loan controls) before the enforcement action, (ii) the share of the non-punished lead bank(s) of the same loan, and (iii) the share of all other lead banks in other loans. In this exercise, saturation fully accounts for alternative supply-side explanations of the findings, including changes in the business model of banks precisely related to the reasons of the enforcement action. Further, and equally important, in multiple occasions, firms borrow more than once within a year. This allows us to include firm*year fixed effects, which comprehensively account for alternative demand-side explanations of the findings. In addition, the bank*year and firm*year fixed effects fully control for common effects on all banks and firms, such as the impact of the subprime crisis.

Our baseline specification shows that an enforcement action enacted on a lead arranger increases that lender's share by approximately 2.9 percentage points, which is a 15% increase for the average punished lead lender's share in our sample. The HHI of the syndicate also increases by approximately 2.5 percentage points, the number of lenders decreases by approximately one lender, financial covenants increase by approximately 6.4%, and general covenants increase by approximately 12.5%. These results are aligned with the theoretical model's predictions on the reputational impact of the enforcement action and the associated increased monitoring effort required from the lead arranger by the participants. The lead arranger increases the shares to provide a stronger signal for borrower's quality and to commit to higher screening and monitoring in order to incentivize the participants.

Our results are robust to alternative model re-specifications, different subsamples, and an instrumental variable estimation. One important test is to further saturate the model with lead bank*firm*year fixed effects. Importantly, these fixed effects simultaneously control for the time-varying supply and demand effects on top of bank-firm relationships. A second important sensitivity test is to restrict our sample only to those loans where the bank syndicate

members and the borrower are the same before and after the enforcement action. This implies that our results cannot be affected by changes in the members of the syndicate but only from the reputational shock. Finally, we consider an instrumental variables treatment effects model. As an IV, we identify the share of female examiners in local supervisory offices, cleaned from annual trends and local conditions (Delis et al., 2017b). We extensively discuss how these variables meet the exclusion restriction and show their significant explanatory power as determinants of enforcement actions. The results from the IV method are aligned with those from the fixed effects method.

We also show empirically that there are important loan and bank characteristics that moderate the positive effect of enforcement actions on the lead lender's participation share. Evidently, the inclusion of loan characteristics like guarantor, performance pricing provisions, and covenants moderates the positive impact of enforcement actions through reducing informational asymmetries. Similarly, relationship lending between either the lead bank and participant banks or the lead bank and the borrower plays a similar role. Finally, the effect of enforcement actions is potent in the medium term (three to four years post-enforcement) but is completely offset for banks that improve their performance by that period.

Our paper makes contributions to three strands of literature. First, several studies provide incentives in which bank reputation serves as a bounding device that deters highly reputable banks from shirking their information production (Booth and Smith, 1986; Chemmanur and Fulghieri, 1994, among others). Demiroglu and James (2010) indicates that borrowing cost are lower for loans sponsored from private equity groups. The existing literature on loan syndicate structure uses a lead arranger's market share as a proxy for its reputation and shows that when borrowing firms require more-intense monitoring, the lead arranger retains a larger share of the loan and forms a more concentrated syndicate (Dennis and Mullineaux, 2000; and Sufi, 2007). Gopalan et al. (2011) examine the effect of lead lenders' reputation,

measured by large-scale bankruptcies, on its subsequent syndication activity and Lin et al. (2012) show that when control-ownership divergence is large, then the lead arranger retains higher shares.

Although this evidence is consistent with a reputation story, it is also consistent with alternative explanations based on matching between better quality borrowers and large lead arrangers, and the exercise of bank market power (Delis et al., 2017a). Unlike these papers, we do not assume the effectiveness of the reputation mechanism, but instead explore variation in it in the cross-section and over time. Our research studies the enforcement actions enacted on banks for safety and soundness reasons, as the "reputational devices" that affects the structure of the syndicate. This is crucial, because supervisory actions related to safety and soundness of banks, might have a strong reputational effect on which the literature is generally silent. Moreover, such important enforcement actions provide almost by definition clear-cut reputational "innovations" that allow us to test a rich set of predictions regarding the effectiveness, and limitations, of reputation-based disciplining mechanisms.

A second strand of literature analyzes the effect of enforcement actions on banks' risk and performance. Delis et al. (2017b) document that enforcement actions only moderately reduce the aggregate risk-weighted assets and non-performing loans ratios of punished banks, with no accompanying increase in the level of regulatory capital. Ioannidou (2005) suggests that a central bank with a dual mandate (monetary policy and bank supervision) alters bank supervisory behavior in terms of imposing penalties vis-à-vis supervisors without a dual mandate. Nguyen et al. (2016) show that board monitoring is effective in reducing the probability that banks receive enforcement actions from regulators.[1] A more dated literature (e.g., Brous and Leggett, 1996; Slovin et al., 1999) provides similar findings on the effect of

[1] Delis and Staikouras (2011) use aggregate data on the number of enforcement actions across countries and document similar results. Danisewicz et al. (2014) suggest that enforcement actions have adverse short-term effects on the macro-economy.

enforcement actions on bank risk. Despite the important implications of enforcement actions on banks' business management and corporate culture, existing research on the consequences of these actions on the real economy is limited. Our paper provides a direct evidence on how regulatory market-discipline actions affect the real economy by looking at the contract design of the syndicated loan market.

Finally, we establish a tradeoff between reputation and lead arranger shares that has analogies in the broader corporate governance literature. For example, Calomiris and Carlson (2016) show that bank manager ownership is a substitute for formal corporate governance tools to ensure proper effort by the manager. In general, bank managers who have large stakes in their banks' performance could exert greater effort in managing risk to preserve their own financial wealth (Demsetz et al., 1997; Laeven and Levine, 2009). Thus, the analysis conducted here for lead lender shares provides empirical ground for a broad theoretical basis that goes back to at least Holmstrom and Tirole (1997).

The paper proceeds as follows. The next section discusses our theoretical mechanism and, based on its implications, specifies our testable hypotheses. Section III describes the empirical model and our identification method. Section IV discusses the empirical results. Section V concludes.

II. Theoretical Mechanism

Information asymmetries between contracting parties fundamentally impact the design of optimal contract (e.g., Leland and Pyle, 1977; Holmstrom and Tirole, 1997). For syndicated loan deals, which involve two or more parties lending to a single borrower, information asymmetries can exist between the borrower and lenders as well as among the lenders themselves (Pichler and Wilhelm, 2001). To create incentives for monitoring, a lead arranger retains a financial stake in the loan due to the inability of syndicate participants to directly

observe the level of monitoring activities. The share retained is larger as the moral hazard problem increases (Dennis and Mullineaux, 2000; Sufi, 2007; among others).

In this section, we sketch our theoretical argument by adopting a narrative approach to the formation of the syndicate when the lead lenders' reputation suffers a loss due to an enforcement action. A formal and extended theoretical model for this argument is provided in Appendix I. The syndicated loan market is an ideal setup to identify the necessary conditions for a reputation-based disciplining mechanism to work because the reputation of the lead arranger determines participants' beliefs. The main reason for this is the principal arranger responsibility for all price-setting decisions and the monitoring process of the loan (Sufi, 2007; Ivashina, 2009).

Assuming that a principal arranger wants to partially finance a project with positive net present value using the syndicated loan market, she writes a contract determining the share of the loan financed by her and the share of the loan financed by the potential participants. The project's success positively depends on the project's inherent success potential and on the lead arranger's monitoring effort. The monitoring effort is extremely important in our case because a potential participant would like the lead arranger to exert as much monitoring effort as possible to maximize the possibility of the loan's success.

In the syndicated loan market, there is no third party that can enforce the level of monitoring effort and hence the unobservability of lead's monitoring effort creates potentially severe asymmetric information problems that must be considered in optimal syndicate design (Ivashina, 2009). This represents a possible source of moral hazard and the potential participant must form rational expectations about the monitoring effort of the lead arranger based on the available information.

Given that a lead arranger's monitoring technology does not depend on the share of the project that she finances, it is evident that *her incentives to monitor the project are increasing*

in the share of the project that she finances. Indeed, a lender is more willing to undertake costs that increase the chances that she will "get her money back" when she has contributed a large amount of money rather than when she only participated to a small extent.

In this study, we closely link the lead arranger's reputation with the regulator's signal on the lead arranger's compliance with regulatory law on the books. Specifically, if a principal arranger is found to have engaged in legal or regulatory misconduct, especially for important reasons related to financial safety and soundness, then she receives an enforcement action that is publicly announced. It is then natural to assume that such actions reveal that the lead arranger has certain undesired characteristics, most notably risky behavior, leading to increased moral hazard for participants. This increased moral hazard stems from the worsened reputation of the punished lead arranger. It follows that penalized lead arrangers are less appealing to potential participants compared to non-penalized lead banks.

Hence, punished lead arrangers need to compensate potential participants for these reputational costs. Theory predicts that an important way through which participant banks can be compensated is participation share. Leland and Pyle (1977) and many others henceforth, highlight that an increase in the ownership of the informed party would signal a higher quality of the underlying project thereby reducing the cost of asymmetric information. In our case, this will be more so because a higher participation share by the punished lead bank will signal higher monitoring effort. This leads to our testable hypotheses:

Hypothesis 1: *Signed contracts designed by lead arrangers with high reputational risks, should be such that the lead arranger's share is higher compared with the lead arranger's shares in other signed contracts.*

Hypothesis 2: *Signed contracts designed by lead arrangers with high reputational risk, will have a more concentrated structure compared to contracts design by lead arrangers with low reputational risk.*

Hypothesis 3: *Participant banks should be less willing to engage in a contract that is designed by lead arrangers with high reputational risks.*

III. Empirical Model, Data, and Identification Problems

A. Empirical Specification and Variables

To empirically test our hypothesis, we use the following equation:

$$S = a_f + a_1 PEL_{bt} + a_2 L_{l,t} + u_{fblt}. \quad (1)$$

In Equation (1), S represents the syndicate loan structure. In most part, we use the loan share of a lead bank *b*. L is a vector of loan characteristics used as control variables. In turn, a_f denotes a vector of fixed effects and *u* is the remainder disturbance.

The variable of main interest is *PEL (post enforcement loan)*, which is a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action. *Post enforcement loan* also takes the value zero for all loans originated by lead banks that were never punished during our sample period (see also Table I, which includes definitions for all variables used in our analysis). A positive value on a_1 implies that once a lead arranger is punished, the lead arranger's mean loan share is larger relative to the lead arranger's mean loan share before the enforcement action or the mean leads' share of never punished banks.

[Insert Table I about here]

To estimate Equation (1), we combine information from three different sources. First, enforcement actions are reported on the websites of the three main banking supervisors in the

U.S.: The Federal Reserve System (Fed), the Federal Deposit Insurance Corporation (FDIC), and the Office of the Comptroller of the Currency (OCC). All insured commercial and savings banks in the U.S. have one of the above agencies as their primary federal supervisor (Ioannidou, 2005). In general, the supervisory organization conducts a full-scope on-site examination of each insured depository institution at least once every 12 months.[2] This examination involves an audit procedure necessary to evaluate all components of the Uniform Financial Institutions Ratings Systems (UFIRS) or the CAMELS rating system assigned to each bank.[3] The findings from the on-site examinations and CAMELS determine whether a formal enforcement action will be enacted.[4] These are legally enforced and disclosed to the public.

Importantly, only the important enforcement actions related to bank safety and soundness should bear reputational risk (e.g., Nguyen et al., 2015). Delis et al. (2017b) group the formal enforcement actions according to their rationale into a number of groups, mostly reflecting the action's severity and relation with safety and soundness issues.[5] We follow this study and only include actions related to the Basel Committee Core Principles for Effective Banking Supervision (Basel, 2012). These include capital adequacy and liquidity, asset quality, provisions and reserves, large exposures and exposures related to parties, internal control and audit systems, money laundering, bank secrecy, and foreign assets control. We also include breaches of the requirements concerning the fitness and propriety of banks' board members

[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. In our sample, most of the banks are large and are under relatively uniform inspection by regulators, most of the time involving the regulators maintaining offices inside the banks' headquarters.

[3] The components of CAMELS are capital adequacy (C), asset quality (A), management (M), earnings (E), liquidity (L), and sensitivity to market risk (S).

[4] There are also informal actions that are not disclosed to the public, so information on them is private and does not contain reputational risk. Such actions mostly are voluntary commitments made by a bank's board members to correct problems and consist of commitment letters, memoranda of understanding, and approved safety and soundness plans.

[5] Delis et al. (2017b) define the enforcement action dummy equals to one at the event quarter during which a sanction is imposed on a bank and zero otherwise, while no other formal enforcement action of the same or different class has been imposed on the same bank within the (-4, +4) quarter window around the event

and senior management, as well as other persons closely associated with banks (institution-affiliated parties). However, we demonstrate that our results are robust when excluding the latter. We extensively discuss the types of enforcement actions and their rationale (both those we use and those we do not) in Appendix II.

Second, we obtain data for U.S. syndicated loans from *DealScan* over the period 1997-2014. Thus, even for enforcement actions enacted in 2010, we have four years of data after their enactment. We also match information from DealScan with banks that received enforcement actions using bank-level coding from the *Call Reports*. This matching process allows identifying the accounting characteristics of banks involved in the loan and using these characteristics as control variables. We do the same for firms, by matching our end sample with Compustat.

Following the literature (e.g., Sufi, 2007), we measure the syndicate loan structure with several alternative measures. First, we use the share of the loan held by the lead lender, which is the dependent variable most directly relevant to our theoretical considerations. A closely related variable is the Herfindahl-Hirschman index (HHI) of the syndicate, which shows the concentration of holdings within a loan syndicate. Finally, we also examine different subcategories of the total number of lenders participating in the syndicate to explore whether the average syndicate size decreases following an enforcement action on a lead arranger.

We control for various loan characteristics such as the *maturity* and amount of the loan facility (e.g., Ivashina, 2009). *Downgrading* is a dummy variable equal to one if the loan is downgraded and zero otherwise. In a similar fashion, we use *performance pricing, collateral*, and *relationship lending* (Ioannidou and Ongena, 2010), which are also dummy variables, taking a value equal to one if the loan has performance pricing provisions, is secured with collateral, and the lead arranger has made a loan to the same borrower in the past five years before the current loan, respectively, and zero otherwise. We also use loan type and loan

purpose fixed effects to saturate our model from differences in syndicate structure due to loan type or purpose (for more extensive definitions, see Table I).

After removing from our data missing observations on the variables to be included in our analysis, we have 75,125 loan facilities originated by 763 lead banks.[6] From these, 74 banks received 79 enforcement actions (events) during 2001-2010.[7] The number of post-enforcement loans in our baseline specifications is 15,885. The vast majority of the banks received an enforcement action only once, while in very few cases banks received two actions more than three years apart. Thus, we anticipate that the reputation effect on the syndicate loan structure should be strong, as banks in our sample are receiving enforcement actions once (in most cases) or twice at most. 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.[8] What matters, and what constitutes the unit of our analysis, are the numbers of loans pre and post enforcement.

Table II provides basic descriptive statistics for the sample of banks that received a penalty at some point during our sample period (Panel A), for the full sample of lead arrangers (Panel B) and for our dependent variables for the punished and non-punished lead arrangers (Panel C). The summary statistics of Panel C are particularly interesting. They reveal a statistically significant 9.7 percentage point difference between the non-punished and punished lead lender shares, alongside a 9.2 percentage points higher HHI and a lower number of lenders

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

[7] The number of enforcement actions by year is: 2001 (8 enforcement actions), 2002 (6), 2003 (7), 2004 (7), 2005 (6), 2006 (8), 2007 (5), 2008 (7), 2009 (14), 2010 (11). Thus, the number of enforcement actions is relatively evenly distributed across years, even though with some small increase in the crisis period. This is in contrast to Delis et al. (2017b), who use almost the entirety of supervised U.S. banks and denote a clear concentration of enforcement actions during and shortly after the crisis period.

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

(by approximately 2.7 lenders). In our empirical analysis, we aim to examine whether these effects are causal.

[Insert Table II about here]

B. Identification Problems

Our empirical model aims to test the hypothesis that an enforcement action enacted on a lead arranger hampers the lead arranger's reputation and requires him to hold a larger share of the loan. This is also equivalent to the lead arranger deciding to keep a larger share of the loan to persuade participants to co-finance the loan. In our context, we cannot imagine reasons for reverse causality, because an enforcement action is not enacted in response to the structure of a specific loan's syndicate. Identifying the causal effect of an enforcement action on syndicate structure can be impeded, however, by omitted variables that the syndicate structure of post-enforcement loans could capture erroneously.

Such omitted-variable bias could lead to two alternative explanations of the findings. First, from a supply-side viewpoint, enforcement actions are not exogenous to the business model of banks. The types of enforcement actions especially considered in our paper are a treatment for important safety and soundness problems incurred by banks in the pre-treatment period. Post-treatment, the punished banks ought to move to a more prudent risk equilibrium, thus restructuring their lending and their loan shares. The changes in the business model of banks pre- and post-enforcement, including associated changes in corporate governance, need to be accounted for to be able to attribute the effect of enforcement actions on loan shares to reputation. Similarly, our results might be due to heightened supervisory scrutiny, which limits the ability of punished banks to continue their lending operations as in the pre-enforcement period.

Second, from a demand-side viewpoint, an enforcement action could lead to lower demand for credit by firms from the punished bank and this might be especially true during the crisis. If demand drops, the lead arranger might decide to hold a larger share of the loan to either show confidence in the firm or simply because the drop in demand from other firms freed resources. Obviously, this has less to do with the lead bank's reputation within the syndicate. Similarly, if the less risky firms decide to leave the punished banks after the enforcement action, the punished bank will be left with the riskier firms, for which it holds a higher loan share irrespective of the action.

In the next section, we discuss solutions to these identification problems. We begin with a event-type study estimated with time-invariant fixed effects regressions and, subsequently, implement models with time-varying fixed effects and instrumental variables treatment effects.

IV. Identification Methods and Empirical Results

A. Baseline Results

Table III reports our baseline results. In our event study, the timing of the enforcement actions varies across banks and so, we employ a D-in-D effect with the staggered enactment of sanctions. We utilize the bank fixed effects to capture differences between the treatment and benchmark groups and time fixed effects to aggregate fluctuations in the dependent variable. In addition, we employ firm fixed effects to saturate unobserved firm-specific characteristics affecting the structure of the loan syndicate, and loan-type and loan-purpose fixed effects to control for the respective loan types (Sufi, 2007).

In all specifications, the effect of the enforcement action on various measures of syndicate structure in the first year after the enactment is statistically significant at 1% level (except column III which is at 5% level). The results in column I show that an enforcement action increases the lead lender's share in the syndicate by approximately 2.1 percentage points.

A very similar picture appears when using as our dependent variables the HHI of the loan syndicate and the number of lenders. We find that an enforcement action increases the concentration of holdings within the syndicate by 1.8 percentage points and a decrease in the number of lenders by one.[9]

[Insert Table III about here]

Our first buffer against the alternative explanations of our findings is to use the multi-level structure of our data set. The banks in our sample originate many loans per year both pre-enforcement and post-enforcement (within the enforcement year). Including bank*year fixed effects controls for much of the reasons leading to the enforcement action (in the year before the action) and for changes in bank behavior in the year after the year of the enforcement action.

The bank*year fixed effects are not perfectly collinear with PEL for two reasons. First, there is within-year variation in the year of the enforcement because PEL changes within that year if a punished bank gives loans both pre- and post-enforcement. Second, for the same loan for which a lead arranger receives an enforcement action (so that post enforcement loan equals one), the other lead arranger(s) have not received an action. However, these non-punished lead arrangers originate other loans with other non-punished banks, and for these loans post enforcement loan equals zero in the year of the action.[10]

This creates a DDD exercise. The first differencing involves the share of the loan a lead bank holds before and after the enforcement action within the enforcement year; the second involves the share of the *same* loan of the punished lead bank *vs.* that of the non-punished lead bank(s); and the third involves the share of the punished lead bank *vs.* the share of all other lead banks in other loans. This procedure creates an almost ideal natural experiment to

[9] In Appendix III we report results using a sub-sample only for punished banks that received a sanction during our sample period. In all specifications of Table A.II, the effect of the enforcement action on various measures of syndicate structure in the first year after the enactment is quantitatively and qualitatively similar to the baseline results.

[10] This is better explained with the help of an example, which we provide in Appendix IV.

completely saturate the reputation effect from other supply (punished lead bank)-side explanations of the findings, including the reasons that led to the penalty and changes in the business model of banks post-enforcement.

We should also note that this method reduces concerns on our results being due to increased regulatory scrutiny (and not due to reputational effects). Supervisory scrutiny is increased not at the time of the enforcement action but well before the enforcement date when the supervisor notes the underlying safety and soundness problems (related to the important enforcement actions we use in this paper). This implies that supervisory scrutiny is not strictly changing with enforcement, but most probably is considerably higher in both pre- and post-enforcement periods from which we obtain identification when using the bank*year fixed effects. Further, syndicated loans are mostly given by large banks. Regulators usually hold offices within these banks, so that supervisory scrutiny is already quite high for these banks.

Equally important, many firms in our sample borrow more than once within each year. This allows the inclusion of firm*year fixed effects. These fixed effects saturate the model from unobserved firm (demand) characteristics that could also render the effect of *post enforcement loan* endogenous. Further, the differences in the timing of the enactment across banks implies that the existence of a systematic omitted variable affecting both *post enforcement loan* and the structure of the syndicate is unlikely. Also, there are several enforcement actions before the crisis, so that the results are not solely driven by developments in this period (even though the bank*year and firm*year fixed effects must saturate the findings from crisis effects).

Table IV reports our results from the fixed effects models. In the first three specifications, the effect of the enforcement action on various measures of syndicate structure is statistically significant at the 1% level. The results in column I show that an enforcement action increases the lead lender's share (the dependent variable most closely related to our

theoretical predictions) in the syndicate by approximately 2.9 percentage points. For the punished lead lender with an average share (equal to 19.3% in our sample), this finding implies a large increase of approximately 15%. Comparing these results to the univariate analysis in Panel C of Table II, we note that 2.9 percentage points out of the 9.7 percentage points difference in *Lead lender shares* between the pre- and post-enforcement periods are attributed to the reputational effects of the enforcement action.

[Insert Table IV about here]

A very similar picture appears when using as our dependent variables the HHI of the loan syndicate and the number of lenders (columns II and III of Table IV, respectively). We find that an enforcement action increases the concentration of holdings within the syndicate by 2.6 percentage points or 15% for a punished lead bank with an average HHI in our sample. Concerning the total number of lenders, we find a reduction of approximately 1.1 lenders following an enforcement action. This reduction is still statistically significant but economically smaller compared with the previous variables. Thus, although there is a decrease in the number of lenders that participate in a loan syndicate when the lead arranger receives an enforcement action, the most significant effect comes from the lead arranger taking up a larger share of the loan. Once more, these effects are economically larger compared to the respective effects identified in Table III, showing the importance of including the relevant fixed effects to limit the omitted variable bias.

The participants' decision to collaborate in the syndicated loan market is taken through a mix of borrower's quality and the reputation of the lead arranger. To the extent that participants are uncertain about a lead arranger's reputation, enforcement actions are likely to lower participants' assessment of the lead arranger's ability and thus damaging her reputation. On the other hand, the presence of financial and general covenants is motivated by these instruments' ability to mitigate adverse selection problems because they can restrict the

borrower's financial activity and therefore decrease the uncertainty to the lender (Murfin 2012).[11] In our context, enforcement actions are associated with a reputational burden that disincentivizes participants to do business. Thus, a tightening contract is positively related with the participation decision through a higher number of covenants. Columns IV and V of Table IV show that post-enforcement loans generated by punished lead arrangers have more financial and general covenants by 0.064 and 0.125 points, respectively.

The identifying assumption in our empirical analysis thus far is that after controlling for time-varying borrower (demand-side) effects, time-varying lender (supply-side) effects, and loan characteristics, enforcement actions are exogenous. This allows us to interpret our findings as arising from a loss of the lead arranger's reputation following enforcement actions enacted from the regulator. However, an alternative driving mechanism emerges from the potential unwillingness of participant lenders to do business with a bank that is "on the target list" of the regulator.[12] To assess this alternate explanation of our findings, we perform the following test. In column VI of Table IV, we use the percentage of shares held by participant banks that have the same supervisor with the lead bank as the dependent variable. If our results are due to the alternative explanation, we should expect a negative and statistically significant coefficient on *post enforcement loan*. However, the statistically insignificant coefficient on *post enforcement loan* we find indicates otherwise and does not support this alternative explanation.

[11] General covenants restrict a borrower's actions, such as the amount of acquisitions or debt issuance while financial covenants require borrowers to maintain a minimum level of a financial ratio or value, such as a maximum debt ratio. If a borrowing firm fails to satisfy a covenant, a technical default results. Lenders will often waive these covenant violations in return for renegotiating the loan's terms.

[12] Enforcement actions are costly not only because the affected entities have to spend money and resources correcting the problems identified by the regulator, but also because they become the regulator's target until problems are fixed. In the words of Thomas J. Curry (Comptroller of the Currency, November 2014), "The enforcement actions we are issuing today make clear that the OCC will take forceful action, not only when the institutions we supervise engage in wrongdoing, but when management fails to exercise the oversight necessary to ensure that employees follow laws and regulations intended to protect customers and maintain the integrity of markets."

Our results are aligned with the hypotheses one and two. Specifically, once a lead arranger is punished, the structure of the syndicated loan changes so that the lead arranger holds significantly larger shares and forms more concentrated syndicates, ceteris paribus. Given our identification method, the economic mechanism is that sanctions hurt the lead arranger's reputation and the participant banks exert a market-discipline mechanism demanding from the lead arranger to have more "skin" in the syndicate to commit in higher monitoring. In Sections F below, we empirically dig deeper into this conjecture regarding the lead arranger's monitoring effort.

B. Additional Identification Tests Using the Fixed Effects Model

In even more restrictive specifications, we resort to the inclusion of firm*year*lead bank fixed effects. These fixed effects simultaneously control for the time-varying demand and time-varying supply unobserved factors as discussed above, but also for unobserved factors specific to the lead bank-firm relationship that might affect our main results.[13] The results, reported in Table V, are very similar with those of Table IV. The effect of *post enforcement loan* on the *lead lender shares* (column I) and *HHI* (column II) are, if anything, economically a bit stronger; the results on the *number of lenders* (column III) a bit weaker; *financial covenants* (column IV) weaker; *general covenants* (column V) slightly stronger; and *participant share (%)* (column VI) statistically insignificant results.

[Insert Table V about here]

In Table VI we conduct several additional robustness tests. We report the results only for the lead lender's share. First, in column I we restrict our sample only to the observations where all the syndicate members, both banks and firms, are repeated before and after the

[13] An alternative would be to include firm*year*syndicate fixed effects to keep the syndicate constant, but in that case the degrees of freedom drop to very low levels.

enforcement action. This is a powerful test for the effect of the enforcement action on lead lender shares because the results on *post enforcement loan* cannot be attributed, *inter alia*, to a change in the synthesis of the syndicate. The results are still statistically and economically significant. Specifically, the effect of an enforcement action on the lead lender's share is 1.2 percentage points or approximately 6.4% for the average loan share of a punished lead arranger. We attribute the somewhat smaller economic effect of the results mainly to the much smaller sample.

[Insert Table VI about here]

In column II we examine the effect of an enforcement action only to participant banks, in order to disentangle changes in the structure of the loan syndicates transmitted from participants to lead arrangers. This essentially is a placebo test: identifying an effect arising from the side of the participants would imply that our baseline results capture something else besides the reputation effect on the lead arranger. Evidently, the effect of *post enforcement loan* is economically small and statistically insignificant, implying that enforcement actions on participant lenders do not play a role in the structure of the loan syndicate.

In column III we examine the sensitivity of our findings when we exclude loans for leveraged buyouts (LBOs) and for mergers and acquisitions (M&As). These loans present, in principle, more complete information because the syndicate has acquired private information about the borrowing firm from prior transactions (Ivashina and Kovner, 2011). Thus, we expect that the participant banks would be even more reluctant to fully engage in loans that exclude LBOs and M&As (i.e., the participants would require higher participation shares from the lead lender compared with our baseline findings). Indeed, the coefficient estimates on *post enforcement loan* are economically more significant when we exclude loans for LBOs and M&As, reflecting the importance of incomplete information in forming the effect of enforcement actions on loan syndicate structure.

Further, in column IV we use only the enforcement actions directly related to the guidelines of the Basel Committee Core Principles for Effective Banking Supervision (Basel, 2012), which bear a higher reputational risk on the punished bank (Delis et al., 2017b; Vallascas and Hagendorff, 2013). In this respect, we do not mark as post-enforcement loans those when enforcement actions are issued on lead-bank affiliated members (e.g., bank directors and managers). Given that the latter actions are less closely related to safety and soundness, the remainder ones should have a more potent impact on the reputation of the lead arranger and, thus, a higher effect on *lead lender's share*. As the results show, this is indeed the case.

The 2007-2009 crisis significantly affected the syndicated loan market in many respects, including interest rates and loan syndicate structures. An argument against our reputation-based story of our findings is that large banks are more likely to work with institutional investors as lead arrangers and their share would have mechanically gone up in the crisis when the institutional investors pulled back from the syndicated loan market. In this respect, we conduct two additional tests. First, we consider models including bank-year characteristics and only bank (not bank*year) fixed effects. We report the results from two such models (columns I and II of Table VII), the first including bank size and the second without bank size. The results of the two specifications are almost identical and equivalent to our baseline in Table IV. We interpret these results as evidence against the bank-size explanation of our findings. Second, in the columns III and IV of Table VII, we split the sample between enforcement actions enacted in the periods 2000-2006 and 2007-2010.[14] We find that the coefficient on post enforcement loan is statistically and economically significant in both sub-periods.

[Insert Table VII about here]

[14] We prefer splitting the sample to introducing an interaction term with a relevant dummy. The reason is that the crisis has potentially affected all the explanatory variables and, by splitting the sample, we allow changes in the slopes of all variables (as opposed to only changing the slope of *post enforcement loan*).

C. Results from Instrumental Variables Estimation

A potential problem with the inclusion of several fixed effects is that we drop significant information from our results, given that not all banks (firms) in our sample lend (borrow) pre-enforcement and post-enforcement. In this section, we consider an instrumental variables model that includes only loan type and loan purpose fixed effects. We select our instrument using the implications of a recent literature on the determinants of enforcement actions.

Delis et al. (2017b) instrument enforcement actions with the share of female examiners in local supervisory offices (see Table I). The theory backing this instrument comes from corporate governance and psychology literatures, suggesting that females conduct more intense monitoring (e.g., Adams and Ferreira, 2009), are more risk-averse accounting auditors (e.g., Hardies et al. 2010, Ittonen et al., 2013) and possess greater moral reasoning skills (e.g., Eynon et al. 1997, Ittonen and Vähämaa 2012). We refine this variable by using the residuals from the regression of the share of female examiners in local offices on an annual trend (to capture the annual increase in the share of female examiners) and state fixed effects (to capture other local conditions). The exclusion restriction is satisfied in our context because the lead lenders' shares are not directly formulated by bank examiners; they are indirectly formulated via the examination process and the enactment or not of enforcement actions.

We conduct the IV analysis using a treatment effects model, which is suitable when the endogenous variable is binary. In Table VIII, columns I-VI, we replicate the results of Table IV, using the treatment effects model. In all specifications, we control in the first-stage regressions for the bank-year variables that describe the financial reasons behind the enforcement actions, namely *risk-based capital ratio, non-performing loans, non-interest income, bank ROA, liquidity*, and *bank size*. The first-stage results are in line with Delis et al. (2017b). The coefficients on the bank-year controls (available on request) show that banks with

higher capital, ROA, and liquidity are considerably less likely to receive actions. In contrast, banks with higher non-performing loans are more likely to receive actions, whereas non-interest income and size do not seem to play a significant role. The instrumental variable strongly and positively explains the probability of receiving enforcement actions.

Apart from the small increase in standard errors, the second-stage results are in line with those of Table IV. The somewhat larger coefficient estimate on *post enforcement loan* in column I, suggests that an enforcement action increases lead lender shares by approximately 3.4 percentage points. So, we conclude that inference from the treatment effects model is similar to the fixed effects model. To infer the validity of our instrumental variable, we also regress the second-stage residuals on all exogenous variables and the instrument (e.g., Cao et al., 2017). The results show that the adjusted R-Squared values are very close to zero and the chi-squared test easily rejects the null that the variables used in the probit first-stage are jointly significant in explaining the second-stage residuals. We report these results in the lower part of Table VIII.[15]

[Insert Table VIII about here]

A somewhat related identification problem is that lead lender shares might be simultaneously determined with loan amount, maturity, and loan guarantees, so that the latter are "bad controls." The syndicated loans literature is mostly silent about this problem, but some papers do discuss possible solutions (e.g., Dennis et al., 2000; Delis et al., 2017b). The general solution is then to examine specifications without these loan controls (Angrist and Pischke, 2008). We report the relevant results from the fixed effects and treatment effects models in Appendix V and Table A.III. The results show only small differences in coefficient on post enforcement loan relative to our baseline results.[16] In addition, in the Appendix VI we test for

[15] We obtain similar result when we regress the second stage residuals on the instrumental variable.

[16] There is also an argument against the type of simultaneity implying a bad controls problem. On average, the procedure for the origination of syndicated loans is as follows. First, the firm goes to a lead bank, asking for a specific loan amount and maturity. Subsequently, the lead bank decides that this is a fruitful project, negotiates

pre-enforcement trends in the syndicated structure. Our results provide evidence that there is no pre-trend in the syndicate structure.

D. Additional Specifications against a Regulatory Scrutiny Explanation of our Findings

We have argued so far that our results cannot be overly attributed to regulatory scrutiny because increased regulatory scrutiny takes place well before the enforcement action, so that its effect is absorbed by the bank*year fixed effects (which essentially allow identification from loans originated within a maximum window of six months around the action). Indeed, Delis et al. (2017b) show that bank fundamentals react at least one quarter before the action, revealing the work of regulators prior to the enactment date.

In this section, we conduct placebo tests to show that the effect of regulatory scrutiny is limited in the syndicated loans market, especially when using the fixed effects and treatment effects models. The core idea of these tests is that if regulatory scrutiny affects the syndicated lending activity of banks, we should observe significant differences in the number and volume of syndicated loans around enforcement actions, especially after controlling for bank*year fixed effects or bank variables identifying the reasons behind the action (i.e., bank capital, asset quality, earnings, liquidity, etc.). The same should hold for the probability of extending syndicated loans to new borrowers.

In Appendix VII, we report results from specifications using the number of loans, the loan amount, and the probability of originating loans to new borrowers. We report two sets of results for fixed effects and treatment effects models. All specifications yield statistically insignificant coefficients on *post enforcement loan*. Interestingly, this is not the case when we

the loan amount and maturity (if needed), and sets an indicative interest rate. Once these have been set with some level of accuracy, the bank seeks for other interested banks to co-finance the loan. Of course, the participant banks can make requests, especially on the interest rate and loan guarantees, but these will be marginal compared to the proposition of the lead bank(s). In contrast, participant banks can (and do) ask for different shares of the loan, based on the rest of the loan, bank and firm characteristics (Sufi, 2007).

use a model without bank*year fixed effects. We interpret these findings as evidence that our setup buffers the effect of regulatory scrutiny on the syndicated lending decisions of banks.[17]

E. Syndicate Structure and Participant Choice

Overall, the results in the previous subsections indicate that, ceteris paribus, enforced lead arrangers retain a larger fraction of the loan post-enforcement. This evidence is consistent with our reputation hypothesis. In this subsection, we explore how *post enforcement loan* affects which lenders end up as syndicate members to provide evidence on hypothesis three. In Table IX we further inquire into the way the loan syndication is made up, using alternative elements of syndicated loan structure as dependent variables. Column I shows that the number of participant lenders decreases approximately by one lender. Column II shows that there is a statistically significant reduction in the number of lead lenders but that the economic significance is small. Similarly, participants are less likely to be foreign lenders when the lead arranger receives an enforcement action (column III). Finally, the results in column IV show that, post-enforcement, the amount held by the punished lead lender is approximately 3% higher at the mean (0.074/2.97). Combining these findings with the results of column III in Tables III, IV and V, we conclude that the reduction in the number of lenders comes mainly from the reduction in the number of participants.

[Insert Table IX about here]

Furthermore, we analyze how an enforcement action affects the participation decision. We employ a logit model with fixed effects. We define the "potential" participant set as (i) all financial institutions that were acting at least once as participants in the syndicated loan market over the whole sample period or (ii) financial institutions that were acting at least once as

[17] Of course, this is not the situation for the general bank balance sheet of punished banks, as we observe an 8% decrease in total risk-weighted assets of the punished banks in our sample.

participants in the syndicated loan market in the last five years. The lead arranger on the loan is not included in the participant choice set. The logit model explains how *post enforcement loan* (abbreviated as PEL in the equation) influences the probability of a financial institution being chosen as a participant:

$$L(participant_{jlt} = 1) = \frac{e^{(a_f + a_1 PEL_{it} + a_2 L_{lt} + u_{jlt})}}{1 + e^{(a_f + a_1 PEL_{it} + a_2 L_{lt} + u_{jlt})}} \quad (2)$$

We are interested in the effect of PEL on the probability that a lender *j* is chosen as a participant in a loan *l* at time *t*. We control for unobserved heterogeneity using loan purpose, loan type, year and bank fixed effects. Table X reports marginal effects. In columns I and III, we use all banks that were acting at least once as participants in the syndicated loan market over the sample period. In columns II and IV, we limit the sample to banks that were acting at least once as participants in the syndicated loan market in the last 5 years. The estimation includes the loan control variables included in the previous tables. The results show that when the lead arranger receives an enforcement action, the probability of a lender being chosen as participant decreases. These findings provide evidence backing our testable hypothesis 3.

[Insert Table X about here]

F. Reducing Informational Asymmetries as a Buffer for the Identified Effects

Banks clearly want to avoid enforcement actions, but after they occur, a lead bank in a loan syndicate must deal with its reputation and the syndicate structure. The emerging question is whether there exists a strategy that a punished lead arranger can follow to moderate the effect of the enforcement action on loan syndicate structure. An important issue in this respect is the alleviation of informational asymmetry problems among the participants, the lead arranger, and the borrower, so that the participant banks will perceive the loan as less risky. Given the model's assumptions, the lead arranger's monitoring effort and participation share should be positively related. In a nutshell, we expect that loan characteristics related to lower

informational asymmetry and increased monitoring effort (or rather, increased monitoring efficiency in the empirical sense of these characteristics) might have a moderating effect in the positive nexus between enforcement actions and the lead lender's share.

In column I of Table XI, we introduce an interaction term between *post enforcement loan* and *guarantee.* Loan guarantees, thoroughly defined in Table I, are a more enhanced form of collateral aiming at lowering a loan's riskiness in case of adverse developments for the borrower. The interaction term is negative and significant at the 1% level, and the marginal effect of *post enforcement loan* is negative and statistically significant. Clearly, the lead arranger can offset the effect of enforcement actions on his loan share by requesting a guarantee facility, indirectly passing the cost of the enforcement action to the borrower.

[Insert Table XI about here]

Similarly, we use information on (i) whether the loan has performance pricing provisions and (ii) the number of general loan covenants. These are the main characteristics in loan contracts that directly relate to loan monitoring. We thus expect that use of such loan characteristics will also lower the potency of the effect of enforcement actions. We report the respective results in columns II and III of Table XI. In both specifications, the interaction terms between *post enforcement loan* and the variables related to loan monitoring are negative and statistically significant. Thus, our results confirm the discipline mechanism our model, showing that participant banks do require enhanced monitoring activity in the form of performance pricing provisions and general covenants.

Another interesting angle that can moderate the effect of the enforcement action on loan syndicate structure is relationship intensity. Banks and borrowers repeatedly interact in the syndicated loan market and have long organizational memories that can mitigate reputational considerations. For every loan in our sample, we measure the relationship intensity by looking back and searching the past lending records of the banks. We construct three alternative

measures of relationship lending that capture the private information in the last five years before a current loan was issued between (i) lead-participant, (ii) lead-borrower and (iii) participant-borrower. The first measure is a dummy variable equal to one if the lead arranger has collaborated in previous syndicated loans with a given participant bank. The second dummy is equal to one if the lead arranger lent to the same borrower; and the last is equal to one if the participant bank lent to the same borrower.[18] The key idea is to exploit a comparison with loans that they have relationships in the above categories, as prior experience reflects how members of the syndicate use "soft" information, which is hard to verify. We expect that the interaction term of these variables with *post enforcement loan* will be negatively associated with *lead lender shares*.

Indeed, as reported in Table XII, we find strong evidence that relationship lending mitigates the reputational burden. In column I, we interact *post enforcement loan* with relationship lending between the lead arranger and the participant. The underlying assumption is that participants with prior experience with the lead arranger have a longer and deeper relationship, and thus the effect of enforcement actions on syndicate structure should be lower. The interaction term is negative and statistically significant at 1%, suggesting that a previous relationship between the lead arranger and the participant mitigates the reputational burden by increasing the lead lender shares by 1.38% (marginal effect), as opposed to 2.24% for loans without such relationship lending. We identify similar reductions in the effect of enforcement actions when we examine relationship lending between lead lender and borrower (column II) and participant lender and borrower (column III). These findings are consistent with theoretical considerations on the relevant reduction in informational asymmetries via repeated interaction, acquisition of soft information, etc. (e.g., Bharath et al., 2011). In Appendix VIII and Table

[18] For the five-year window, we follow the approach of Bharath et al. (2011).

A.VI, we dig deeper into the bank's sectoral exposure in mitigating the effect of enforcement actions.

[Insert Table XII about here]

E. Persistence of the Enforcement Actions

In Table XIII we examine the persistence of the effect of enforcement actions in the medium-term. In columns I and II, we use the full sample and modify *post enforcement loan* by restricting its impact to the first three years (column I) and the first four years (column II) after the penalty. The results show that the effect is positive, statistically significant, and close to the one identified in the baseline specification. This shows that the effect of the enforcement action on the lead lender's share is still potent in the medium term.

[Insert Table XIII about here]

Can banks do something to mitigate this lasting effect? Given that many of the punished banks have low or negative profits, returning to profitability will signal improved financial health, which would in turn mitigate the reputational effect of enforcement actions in the medium term. Then, in columns III-IV we introduce an interaction term between *post enforcement loan* and a dummy variable equal to one for loans originated by punished banks that have a return on assets ratio higher than the mean of the punished banks three years post-enforcement. The dummy also takes the value one for loans originated by non-punished lead banks; it takes the value zero for loans originated by punished banks that have a return on assets lower than the mean value of punished banks three years post-enforcement. The interaction term in columns III and IV is statistically significant at 1%, showing that the effect of an enforcement action on lead lender shares is almost completely alleviated for loans originated

by banks with higher profitability. Thus, any lasting reputational effect (three to four years post-enforcement) is related to continuing poor performance of punished banks.

V. Conclusions and Extensions

With an aim to identify the reputational effect of regulatory enforcement of law on the books, we empirically study the role of important regulatory enforcement actions, enacted on banks for breaches of laws and regulations, on loan syndicate formation. The theoretical framework we develop suggests that an increase in reputational risk induces an increase in the lead arranger's equilibrium participation share in the syndicate.

We therefore match hand-collected data on enforcement actions with data for syndicated loans, as well as data for characteristics of the lead arrangers and the borrowing firms, and we conduct an empirical analysis to test this theoretical hypothesis. We show that loans originated by a principal arranger after an enforcement action have a significantly higher participation share by the lead arranger. According to our baseline specification, an enforcement action increases the lead lender's share by approximately 2.9 percentage points, a 15% increase for a punished lead lender with an average share. The empirical results are very similar when we consider a battery of robustness tests, including an IV approach.

We further empirically show that this strong effect of an enforcement action can be mitigated, by including guarantees, performance pricing provisions, and covenants in the loan contract, or when there is prior relationship lending between either the lead bank of the syndicate and participant banks or between the lead bank and the borrower. These features apparently ease participant lenders' concerns resulting from the lower informational asymmetry and higher monitoring efficiency of these loan contracts, elements that significantly reduce enforcement actions' reputational effects. We also show that the effect of enforcement

actions on syndicate structure can be lasting if the performance of punished banks does not recover.

Our study opens new avenues for research in the field of regulatory enforcement actions and/or syndicated lending. Two such avenues are particularly interesting. First, we do not explore in this paper the effect of enforcement actions on syndicated loan pricing. On the one hand, enforcement actions might trigger more-competitive pricing to prevent losing business in light of reputational effects. On the other hand, the banks might pass along the cost of enforcement actions to borrowers, especially if banks have some market power in niche markets and specific industries or strong relationships with specific firms.

Second, the reasons behind enactment of enforcement actions are potentially interesting. Examining the price and non-price terms of syndicated loans for punished lead banks vis-à-vis the price and non-price terms of syndicated loans enacted on lead arrangers with similar CAMELS ratings that did not receive an enforcement action, might highlight important effects stemming from differences between regulators, networks of banks, political connections, and so on. Such a study would be constrained by the fact that regulatory decisions for enforcement actions are to some extent discretionary, which is endogenous and difficult to measure. Because we have covered a lot of ground already in this paper, we leave these ideas for future research.

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Table I
Variable Definitions and Sources

Variable	Description	Source
Dependent variables:		
Lead lender shares (%)	The share of the loan held by the lead lender.	DealScan
HHI (%)	A Herfindahl–Hirschman index used as a measure of concentration of holdings within the loan syndicate. Higher values reflect higher concentration.	DealScan
Number of lenders	The total number of lenders participating in the loan syndicate.	DealScan
Financial covenants	The number of financial covenants in the loan contract.	Dealscan
General covenants	The number of non-financial covenants in the loan contract.	DealScan
Participant shares (%)	Percentage of non-lead share held by participant banks that have the same supervisor.	Dealscan
Number of participants	The total number of lenders participating in each loan.	Dealscan
Number of lead lenders	The total number of lead lenders participating in each loan.	Dealscan
Number of foreign participants	The total number of foreign lenders participating in each loan.	Dealscan
Deal amount held by lead ($M)	The natural logarithm of the loan amount in $M held by the lead lender.	Dealscan
Main explanatory variable:		
Post enforcement loan	A dummy variable equal to one for all loans originated by a punished bank in the years after the year of the enforcement action and zero otherwise (i.e., for the loans originated from all other banks or the punished bank before the enforcement action and the year of the enforcement action). As the sample of enforcement actions spans the years 2001-2010, we extend the sample to 1997-2014 to allow time before and after all enforcement actions. The enforcement actions include all actions (penalties) enacted on lead arrangers for breaches of laws and regulations in a number of cases. These cases include laws and regulations related to the Basel Committee Core Principles for Effective Banking Supervision (i.e., capital adequacy and liquidity, asset quality, provisions and reserves, large exposures and exposures related to parties, internal control and audit systems, money laundering, bank secrecy, consumer protection, and foreign assets control). They also include 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).	Websites of FED, FDIC, and OCC
Post enforcement loan3	A dummy variable equal to one for the loans originated by a punished bank in the first three years after the year of the enforcement action and zero otherwise. Similar as in *Post enforcement loan.*	Websites of FED, FDIC, and OCC
Post enforcement loan4	A dummy variable equal to one for the loans originated by a punished bank in the first four years after the year of the enforcement action and zero otherwise. Similar as in *Post enforcement loan.*	Websites of FED, FDIC, and OCC
Loan controls:		
Maturity	The natural logarithm of loan maturity in months.	DealScan
Facility amount	The natural logarithm of the loan (facility) amount.	DealScan
Downgrading	Dummy variable equal to one if the loan is downgraded and zero otherwise.	DealScan
Performance pricing	Dummy variable equal to one if the loan has performance pricing provisions and zero otherwise.	DealScan
Collateral	Dummy variable equal to one if the loan is secured with collateral and zero otherwise.	DealScan
Guarantee	A facility backing the assumption of accountability for payment of a debt or performance of a person or entity obligation if the liable party fails to comply with expectations.	DealScan
Loan purpose	Set of dummy variables describing the loan's primary purpose.	Dealscan

Loan type	Set of dummy variables describing loan type. The most common types are lines of credit (such as Revolver/Line, 364-Day Facility, Limited Line) or term loans (term loan A, B, C, D, E) or a letter of credit or bridge loans.	Dealscan
Bank controls:		
Lead-participant relationship	Dummy variable equal to one if the lead arranger has collaborated in the past five years with a given participant and zero otherwise.	Dealscan and own calculation
Lead-borrower relationship	Dummy variable equal to one if the lead arranger lent to the same borrower in the past five years and zero otherwise.	Dealscan and own calculation
Participant-borrower relationship	Dummy variable equal to one if the participant bank lent to the same borrower in the past five years and zero otherwise.	Dealscan and own calculation
Bank similarity (SIC2)	Bank similarity between banks *i* and *j* in two digit borrower SIC code is a positive number, with zero corresponding to lack of similarity and larger values reflecting stronger similarity.	Dealscan and own calculation
Risk-based capital ratio	The ratio of Tier1 and Tier 2 capital to risk-weighted assets.	Call Reports
Non-performing loans	The ratio of non-performing loans (90 days) to total loans.	Call Reports
Non-interest income	The ratio of non-interest income to total income.	Call Reports
Bank ROA	The ratio of pre-tax profits to total assets.	Call Reports
Liquidity	The ratio of liquid assets (cash plus short-term securities) to total assets.	Call Reports
Bank size	The natural logarithm of total assets.	Call Reports
Firm controls:		
Firm Z-score	Altman's Z-score.	Compustat and Orbis
Firm ROA	The ratio of pre-tax profits to total assets.	Compustat and Orbis
Firm leverage	The ratio of total debt to total assets.	Compustat and Orbis
Firm size	The natural logarithm of total assets.	Compustat and Orbis
Instrumental variable:		
Female examiners	The basis of this variable is the annual share of female examiners to total bank examiners in supervisors' local offices. The variable used in the treatment effects model comprises the residuals of the regression of the share of female examiners on an annual trend (to capture the annual increase in the share of female examiners) and state fixed effects (to capture other local conditions).	Delis et al. (2017b)

Table II
Summary Statistics

The table reports summary statistics for the main variables used in the empirical analysis. The variables are defined in Table I. Panel A reports the characteristics only for the punished lead arrangers. Panel B provides descriptive statistics for the total sample of active lead arrangers while panel C describes the difference between the punished and non-punished lead arrangers and their t-test.

Panel A: Summary Statistic only for Banks with EA

					Percentile distribution		
Variables	Level	Obs.	Mean	Std. Dev.	25th	Median	75th
Lead lender shares (%)	Loan	5,406	19.319	19.856	8.333	12.500	21.277
HHI (%)	Loan	5,406	17.620	19.042	7.143	11.111	20.000
Number of lenders	Loan	5,406	11.671	10.629	5.000	9.000	15.000
Maturity	Loan	5,404	3.766	0.636	3.584	4.094	4.094
Facility amount	Loan	5,406	5.410	1.375	4.605	5.521	6.310
Downgrading	Loan	5,406	0.282	0.450	0.000	0.000	1.000
Performance pricing	Loan	5,406	0.573	0.495	0.000	1.000	1.000
Collateral	Loan	5,406	0.491	0.500	0.000	0.000	1.000
Lead-borrower relationship	Loan	5,406	0.509	0.500	0.000	1.000	1.000
Guarantee	Loan	5,406	0.114	0.318	0.000	0.000	0.000

Panel B: Summary Statistic for Total Lead Arrangers

Lead lender shares (%)	Loan	75,125	36.450	31.505	12.500	25.000	50.000
HHI (%)	Loan	75,125	32.951	29.808	11.111	20.000	50.000
Number of lenders	Loan	75,125	7.514	8.515	2.000	5.000	9.000
Financial covenants	Loan	75,125	0.983	1.478	0.000	0.000	2.000
General covenants	Loan	75,125	0.878	1.270	0.000	0.000	2.000
Participant shares (%)	Loan	75,125	29.030	23.595	14.286	20.513	33.333
Number of participants	Loan	75,125	4.524	3.672	2.000	3.000	6.000
Number of lead lenders	Loan	75,125	2.173	1.994	1.000	2.000	2.000
Number of foreign participants	Loan	75,125	2.327	4.544	0.000	1.000	3.000
Deal amount held by lead ($M)	Loan	75,125	3.010	1.696	2.120	3.056	3.945
Post enforcement loan	Bank	75,125	0.070	0.256	0.000	0.000	0.000
Lead-participant relationship	Bank	75,125	0.253	0.387	0.000	0.000	1.000
Lead-borrower relationship	Bank	75,125	0.229	0.589	0.000	0.000	0.000
Participant-borrower relationship	Bank	75,125	0.078	0.268	0.000	0.000	0.000
Bank similarity (SIC2)	Bank	64,514	0.388	0.162	0.283	0.374	0.468
Maturity	Loan	75,125	3.823	0.671	3.584	4.094	4.190
Facility amount	Loan	75,125	4.513	1.787	3.350	4.605	5.784
Downgrading	Loan	75,125	0.126	0.331	0.000	0.000	0.000
Performance pricing	Loan	75,125	0.260	0.439	0.000	0.000	1.000
Collateral	Loan	75,125	0.459	0.498	0.000	0.000	1.000
Guarantee	Loan	75,125	0.057	0.231	0.000	0.000	0.000

Panel C: Summary Statistics for Punished and Non-Punished Lead Arrangers

	Punished banks (A)			Non-punished banks (B)			Difference (B) – (A)
Variables	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median	Mean
Lead lender shares (%)	17.377	16.362	12.500	27.036	27.670	16.667	9.658***
HHI (%)	15.805	15.709	11.111	25.003	26.661	14.286	9.197***
Number of lenders	12.205	11.317	10.000	9.526	10.410	7.000	2.679***

Table III
Baseline Results

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1 PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III
Dependent variable:	Lead lender shares (%)	HHI (%)	Number of lenders
Post enforcement loan	2.101***	1.723***	-1.143**
	[3.621]	[2.957]	[-2.375]
Maturity	-2.492***	-2.095***	0.399***
	[-7.069]	[-7.027]	[4.438]
Facility amount	-0.957***	-1.290***	0.383***
	[-4.617]	[-5.947]	[5.072]
Downgrading	0.256	0.563	0.677
	[0.209]	[0.591]	[1.535]
Performance pricing	-8.050***	-7.617***	2.553***
	[-12.843]	[-12.463]	[5.932]
Collateral	-5.512***	-3.867***	1.436***
	[-6.062]	[-5.371]	[6.386]
Lead-borrower relationship	-8.663***	-7.104***	1.699***
	[-6.887]	[-8.049]	[4.370]
Observations	75,556	75,556	75,556
Adjusted R-squared	0.852	0.860	0.745
Loan-purpose FE	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Clustered standard errors	Firm, Bank	Firm, Bank	Firm, Bank

Table IV
Results from Fixed Effects Models

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1 PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III	IV	V	VI
Dependent variable:	Lead lender shares (%)	HHI (%)	Number of lenders	Financial Covenants	General Covenants	Participant shares (%)
Post enforcement loan	2.910***	2.559***	-1.082***	0.064**	0.125**	-0.207
	[3.186]	[2.888]	[-3.103]	[2.526]	[2.350]	[-0.153]
Maturity	-2.492***	-2.150***	0.662***	0.013	0.003	-0.750*
	[-7.141]	[-7.442]	[6.771]	[1.243]	[0.381]	[-1.791]
Facility amount	-0.807***	-1.134***	0.773***	0.004	0.003	-0.949***
	[-4.356]	[-5.877]	[6.866]	[1.001]	[1.066]	[-4.436]
Downgrading	0.137	0.531	0.619**	0.034	0.030	-1.148
	[0.111]	[0.545]	[2.420]	[0.897]	[0.800]	[-1.638]
Performance pricing	-7.586***	-7.111***	3.214***	0.527***	0.494***	-6.165***
	[-11.953]	[-11.513]	[9.700]	[12.477]	[14.834]	[-8.175]
Collateral	-5.361***	-3.647***	1.078***	0.555***	0.551***	-1.874**
	[-6.017]	[-5.457]	[4.117]	[8.426]	[10.788]	[-2.084]
Lead-borrower relationship	-8.267***	-6.430***	0.620***	0.066	0.114***	-0.856
	[-6.373]	[-7.997]	[4.672]	[1.424]	[2.639]	[-1.337]
Observations	75,125	75,125	75,125	75,125	75,125	75,125
Adjusted R-squared	0.855	0.864	0.567	0.932	0.956	0.847
Loan-purpose FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank

Table V
Including firm*lead bank*year fixed effects

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1 PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III	IV	V	VI
Dependent variable:	Lead lender shares (%)	HHI (%)	Number of lenders	Financial Covenants	General Covenants	Participant shares (%)
Post enforcement loan	3.086***	2.704***	-1.024*	0.040**	0.171**	0.902
	[3.305]	[3.024]	[-1.834]	[2.249]	[2.185]	[1.431]
Maturity	-2.194***	-1.939***	0.389***	0.021**	0.008	-0.278
	[-6.844]	[-6.917]	[4.711]	[2.002]	[1.198]	[-1.030]
Facility amount	-0.860***	-1.200***	0.274***	0.003	0.002	-0.243*
	[-4.428]	[-5.947]	[4.027]	[0.742]	[0.865]	[-1.839]
Downgrading	0.391	0.772	0.631	0.038	0.033	0.237
	[0.322]	[0.827]	[1.574]	[1.029]	[0.932]	[0.231]
Performance pricing	-7.708***	-7.295***	2.031***	0.414***	0.410***	-5.057***
	[-12.340]	[-11.990]	[4.643]	[8.054]	[9.295]	[-4.935]
Collateral	-5.428***	-3.822***	1.424***	0.497***	0.485***	-1.800*
	[-6.255]	[-5.654]	[4.821]	[6.214]	[8.156]	[-1.674]
Lead-borrower relationship	-8.368***	-6.252***	1.085**	-0.019	0.085	0.290
	[-6.291]	[-7.875]	[2.076]	[-0.323]	[1.569]	[0.203]
Observations	75,125	75,125	75,125	75,125	75,125	75,125
Adjusted R-squared	0.854	0.863	0.673	0.965	0.964	0.946
Loan-purpose FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Lead Bank*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank

Table VI
Sensitivity Tests on the Fixed Effects Model

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1\, PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where b refers to bank; l for loans, f for firms and t for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S is defined as the *lead lender shares (%)* and represents the syndicate loan structure. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III	IV
	Repeated syndicate members	EA for participants	Exclude loans for LBOs and M&As	Basel-related actions only
Post enforcement loan	1.212***	0.452	3.767***	3.831**
	[3.006]	[1.153]	[6.205]	[2.257]
Maturity	0.006	-0.436***	-3.237***	-2.362***
	[0.012]	[-2.878]	[-9.224]	[-3.004]
Facility amount	-0.401***	0.003	-2.213***	-0.725***
	[-7.058]	[0.028]	[-7.769]	[-5.118]
Downgrading	0.287	0.612*	-1.707**	-1.347
	[0.647]	[1.868]	[-2.324]	[-1.494]
Performance pricing	-3.153***	-2.064***	-7.283***	-7.194***
	[-7.647]	[-5.120]	[-10.989]	[-15.459]
Collateral	-1.604**	-0.584	-2.920***	-2.801
	[-2.749]	[-0.739]	[-4.304]	[-1.635]
Lead-borrower relationship	-1.753**	-1.767***	-2.902***	-2.401
	[-2.425]	[-2.868]	[-6.968]	[-1.015]
Observations	5,605	75,125	74,883	8,954
Adjusted R-squared	0.848	0.782	0.752	0.850
Loan purpose FE	Yes	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes	Yes
Firm*Year FE	Yes	Yes	Yes	Yes
Bank*Year FE	Yes	Yes	Yes	Yes
Clustering	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank

Table VII
A Role for Bank Size or the Financial Crisis?

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1\, PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III	IV
	Without bank size	Including bank size	Actions in 2000-2006	Actions in 2007-2010
Post enforcement loan	3.114***	3.022***	2.547***	3.117***
	[3.040]	[2.928]	[2.711]	[3.455]
Bank size		-1.510		
		[-1.004]		
Loan controls	Yes	Yes	Yes	Yes
Bank controls	Yes	Yes	No	No
Observations	81,040	81,040	30,647	44,478
Adjusted R-squared	0.634	0.627	0.868	0.832
Loan purpose FE	Yes	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes	Yes
Firm*Year FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	No	No
Bank*Year FE	No	No	Yes	Yes
Clustering	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank

Table VIII
Results from the Treatment Effects Model

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1 PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on using a two-stage treatment effects model with loan-level frequency originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. In the first stage (Panel A), we use the share of *female examiners* in local supervisory offices as an instrument to enforcement actions, while we control for risk-based capital ratio, non-performing loans, non-interest income, bank ROA, liquidity, and bank size. In Panel B, we report the second-stage results. All variables are defined in Table I. In the lower part of the table, we report the results from instrument validity tests (Adj. R-squared and Chi-squared), as described in the text. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

Panel A: First-stage results

Female examiners	1.213***
	[3.432]
Bank controls	Yes
Pseudo R-squared	0.227

Panel B: Second-stage results

	I	II	III	IV	V	VI
Dependent variable:	Lead lender shares (%)	HHI (%)	Number of lenders	Financial Covenants	General Covenants	Participant shares (%)
Post enforcement loan	3.517***	1.340***	-1.195***	0.077***	0.163**	-0.197
	[3.195]	[4.991]	[-2.910]	[2.610]	[2.407]	[-0.120]
Maturity	-3.450***	-4.306***	0.820***	0.097***	0.005	-0.920**
	[-11.516]	[-8.980]	[6.328]	[4.592]	[0.397]	[-2.071]
Facility amount	-1.316***	-1.602***	0.794***	-0.014	0.003	-1.125***
	[-7.014]	[-5.303]	[7.311]	[-1.373]	[0.826]	[-4.738]
Downgrading	0.713	0.913	0.548*	0.351***	0.042	-1.246
	[1.207]	[1.309]	[1.855]	[6.687]	[0.907]	[-1.429]
Performance pricing	-9.105***	-3.673***	3.385***	0.608***	0.524***	-7.943***
	[-14.716]	[-2.962]	[10.020]	[13.352]	[13.820]	[-11.685]
Collateral	-2.125***	-3.866***	0.946***	0.833***	0.497***	-0.173
	[-3.047]	[-4.660]	[3.764]	[3.731]	[8.299]	[-0.072]
Lead-borrower relationship	-3.416***	-5.232***	0.652***	0.076	0.128***	-0.745
	[-7.305]	[-4.714]	[4.741]	[1.596]	[2.790]	[-1.420]
Observations	75,125	75,125	75,125	75,125	75,125	75,125
Adj. R-squared of instrument test	0.00007	0.00011	0.00008	0.00007	0.00015	0.00023
Chi-squared of instrument test (P-value)	0.623	0.517	0.640	0.711	0.416	0.329
Loan-purpose FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank

Table IX
Additional Elements of Syndicated Loan Structure

The table reports coefficients and t-statistics (in brackets). We estimate the regression: $S = a_f + a_1\, PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III	IV
Dependent variable	# of participants	# of lead lenders	# of foreign participants	Deal amount held by lead ($M)
Post enforcement loan	-0.969**	-0.036**	-0.204**	0.074*
	[-2.549]	[-2.037]	[-2.132]	[1.814]
Maturity	0.222***	0.024	0.194***	-0.078***
	[4.707]	[1.214]	[4.137]	[-4.915]
Facility amount	0.141***	0.026***	0.152***	0.972***
	[5.645]	[3.625]	[3.498]	[116.489]
Downgrading	-0.013	-0.079	-0.017	0.001
	[-0.060]	[-1.443]	[-0.064]	[0.024]
Performance pricing	1.756***	0.112***	1.479***	-0.202***
	[10.792]	[2.896]	[7.055]	[-5.179]
Collateral	0.408***	0.027	0.399***	-0.198***
	[4.469]	[0.773]	[3.808]	[-6.470]
Lead-borrower relationship	0.535***	0.040	0.420**	-0.285***
	[3.550]	[0.399]	[2.499]	[-5.287]
Observations	75,125	75,125	75,125	75,125
Adjusted R-squared	0.812	0.936	0.870	0.754
Loan-purpose FE	Yes	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes	Yes
Firm*Year FE	Yes	Yes	Yes	Yes
Bank*Year FE	Yes	Yes	Yes	Yes
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm

Table X
Logit Results for Participant

The table reports marginal coefficients and t-statistics (in brackets) for a logit model with fixed effects. We estimate the following model: $L(participant_{jlt} = 1) = \frac{e^{(a_f + a_1 PEL_{it} + a_2 L_{lt} + u_{jlt})}}{1 + e^{(a_f + a_1 PEL_{it} + a_2 L_{lt} + u_{jlt})}}$. We define the "potential" participant set as all banks that were acting at least once as participants in the syndicated loan market over (i) the whole sample period or (ii) in the last five years. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III	IV
	Full sample	Last 5 years	Full sample	Last 5 years
Post enforcement loan	-0.105**	-0.099**	-0.033***	-0.060***
	[-2.382]	[-1.984]	[-5.931]	[-8.710]
Maturity	0.038***	0.036***	0.040***	0.039***
	[6.394]	[5.836]	[30.696]	[27.234]
Facility amount	0.063***	0.064***	0.081***	0.084***
	[9.221]	[9.337]	[147.640]	[138.368]
Downgrading	0.013	0.011	0.017***	0.014***
	[1.412]	[1.161]	[8.893]	[6.936]
Performance pricing	0.081***	0.081***	0.054***	0.054***
	[8.706]	[8.225]	[31.625]	[27.988]
Collateral	-0.026***	-0.022***	-0.015***	-0.013***
	[-2.982]	[-2.749]	[-8.983]	[-7.001]
Participant-borrower relationship	0.040***	0.045***	0.040***	0.045***
	[3.595]	[4.084]	[26.740]	[27.200]
Observations	295,207	233,084	295,017	232,903
Loan-purpose FE	Yes	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
Clustering	Bank	Bank	Bank	Bank

Table XI
The Role of Variables Mitigating Informational Asymmetry

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1\, PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where b refers to bank; l for loans, f for firms and t for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III
Post enforcement loan	3.893**	4.490***	1.834***
	[2.254]	[5.095]	[3.192]
Post enforcement loan * Guarantee	-4.381***		
	[-21.230]		
Post enforcement loan * Performance pricing		-3.062***	
		[-4.386]	
Post enforcement loan * General covenants			-1.367***
			[-3.109]
Maturity	-2.355***	-2.960***	-2.526***
	[-2.997]	[-10.126]	[-7.251]
Facility amount	-0.722***	-2.405***	-0.966***
	[-4.895]	[-8.333]	[-4.632]
Downgrading	-1.388	-2.066***	0.268
	[-1.534]	[-2.969]	[0.220]
Performance pricing	-7.208***	-8.825***	-7.505***
	[-15.737]	[-16.055]	[-13.053]
Collateral	-2.860	-3.653***	-4.470***
	[-1.628]	[-4.793]	[-4.630]
Lead-borrower relationship	-2.343	-2.876***	-8.636***
	[-0.984]	[-6.814]	[-6.899]
Marginal effect of post enforcement loan	-0.488	1.428	0.467
(P-value)	(0.000)	(0.000)	(0.000)
Observations	75,125	75,125	75,125
Adjusted R-squared	0.850	0.716	0.852
Loan purpose FE	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes
Firm*Year FE	Yes	Yes	Yes
Bank*Year FE	Yes	Yes	Yes
Clustered standard errors	Firm, Bank	Firm, Bank	Firm, Bank

Table XII
The Role of Relationship Lending in Mitigating Informational Asymmetry

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1\, PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III
	Lead-Participant	Lead-Borrower	Participant-Borrower
Post enforcement loan	2.239***	2.605***	2.643***
	[2.863]	[3.102]	[3.283]
Relationship lending	-4.217***	-8.273***	-8.263***
	[-4.419]	[-4.698]	[-4.715]
Post enforcement loan * Relationship lending	-0.857**	-0.936*	-0.432**
	[-1.977]	[-1.788]	[-2.226]
Maturity	-2.528***	-2.480***	-2.482***
	[-7.680]	[-6.326]	[-6.443]
Facility amount	-1.204***	-0.806***	-0.806***
	[-6.285]	[-2.852]	[-2.841]
Downgrading	1.082	0.139	0.139
	[1.169]	[0.093]	[0.092]
Performance pricing	-7.891***	-7.563***	-7.567***
	[-13.435]	[-10.146]	[-10.118]
Collateral	-4.839***	-5.338***	-5.343***
	[-5.731]	[-5.826]	[-5.691]
Marginal effect of post enforcement loan	1.382	1.669	2.211
(P-value)	[0.000]	[0.000]	[0.000]
Observations	75,125	75,125	75,125
Adjusted R-squared	0.904	0.905	0.905
Loan-purpose FE	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes
Firm*Year FE	Yes	Yes	Yes
Bank*Year FE	Yes	Yes	Yes
Clustering	Bank-Firm	Bank-Firm	Bank-Firm

Table XIII
Evolution of Enforcement Actions

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1 PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S is defined as the *lead lender shares (%)* and represents the syndicate loan structure. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. Columns I-II are estimated for the full sample. Columns III-IV are estimated on banks subsample with profitability above the mean. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III	IV
	Full sample		More Profitable firms	
	3-years window	4-years window	3-years window	4-years window
Post enforcement loan	2.258**	2.120*	2.984***	2.841***
	[2.037]	[1.908]	[3.820]	[3.720]
Post enforcement loan * Bank ROA			-2.552***	-2.614***
			[-2.688]	[-2.881]
Marginal effect of post enforcement loan			0.432	0.227
(P-value)			0.000	0.000
Loan controls	Yes	Yes	Yes	Yes
Observations	75,125	75,125	26,796	48,329
Adjusted R-squared	0.855	0.855	0.896	0.896
Loan-purpose FE	Yes	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes	Yes
Firm*Year FE	Yes	Yes	Yes	Yes
Bank*Year FE	Yes	Yes	Yes	Yes
Clustering	Firm, Bank	Firm, Bank	Bank-Firm	Bank-Firm

Appendix I. Reputation and Loan Syndicate Structure: A Formal Argument

In this Appendix, we formulate a theoretical argument that stresses the role of reputation of the lead arranger in the structure of a loan syndicate, with reputation emerging from a regulator's decision in whether or not to enact an enforcement action. The equilibrium outcome of our model yields the prediction of our testable hypothesis discussed in Section II.

The set of players is given by $\{B, A, P\}$, where B is the borrower (firm), A is the lead (principal) arranger (the bank that designs the contract), and P is the potential participant (the bank that is offered the contract).[19]

The borrower wants to finance a project that costs one dollar but lacks funds. Hence, he requests financing from the principal arranger. The principal arranger might want to (i) lend the borrower the entire amount, (ii) partially finance the project herself, or (iii) not finance the project at all. In the first case, she provides a loan of one dollar to the borrower. In the second, she asks another potential participant to participate in providing the borrower a syndicate loan of one dollar. In the third case, she turns down the borrower's loan application. If the loan (individual or syndicate) is approved, the lead arranger monitors the use of the borrower's funds.

The timing of the game is as follows:

Stage 1. The borrower applies for a loan of one dollar at a fixed interest rate, r, to finance a project.

Stage 2. If the principal arranger does not want to finance this project at all, the game ends here. If the principal arranger wants to finance the project (even partially), the principal arranger writes a contract (a_A, a_P) such that $a_A + a_P = 1$ and $a_i \geq 0$ for every $i \in \{A, P\}$. We use a_i to denote the participation share of player i in the loan (the share of the loan that this

[19] We assume that an arbitrary number of potential participants provides no additional intuition to our analysis and only complicates formal arguments.

player finances). Given that $a_P = 1 - a_A$, we usually refer to a loan contract only by the share held by the lead arranger.

Stage 3. The potential participant observes the contract and decides whether or not to sign it. We consider a contract approved if the potential participant signs the contract.

Stage 4. If the contract is not approved, the game ends here (no loan is given). If the contract is approved, the project is financed and the principal arranger decides how much monitoring effort to exert.

Stage 5. The returns of the project are made public information.

Stage 6. Players receive their payoffs.

The project's success is subject to uncertainty. Formally, we assume that the project's quality will be the outcome of a random draw from a uniform distribution on [0, $s(e)$], where $s(e) = s + e$.[20] Parameter $s \geq 1 + r$ can be viewed as the project's inherent success potential, and it is assumed to be common information, while $e \geq 0$ measures the lead arranger's monitoring effort. The larger the lead arranger's monitoring effort, the larger the project's success prospects. Therefore, a potential participant would like the principal arranger to exert as much monitoring effort as possible. We stress, however, that there is no third party that can enforce any level of monitoring effort, and hence the monitoring effort that the principal arranger will exert after the loan is approved cannot be part of the contract. This scenario represents a possible source of moral hazard, and the principal arranger must form rational expectations about it based on the information available to her.

If the project is financed and its quality turns out to be $\gamma \geq 1 + r$, then the payoff of the borrower is 1, the payoff of the lead arranger is $v(a_A(1 + r), a_A) - c(e)$, and the payoff of

[20] The uniform distribution is just an auxiliary device that greatly simplifies analysis and has no substantial implication on our findings. Indeed, what is vital for our results, is that the project's success probability is increasing in the monitoring effort of the principal arranger. The precise way that one chooses to model this outcome through a distribution is essentially inconsequential as far as the main structure of the underlying incentives is concerned.

the potential participant is $v(a_P(1+r), a_P) + a_P q_A$. In contrast, if the project quality turns out to be $\gamma < 1 + r$, then the payoff of the borrower is 0, the payoff of the principal arranger is $v(0, a_A) - c(e)$, and the payoff of the potential participant is $v(0, a_P) + a_P q_A$, where $q_A \in \{-q, q\}$ for some $q > 0$. To make the analysis easier to follow, we consider that $v(x, y) = x - y^{\xi}$, where $\xi > 1$ and that $c(x) = x^2$. We stress though that all our qualitative findings are robust to more general formulations.[21]

The parameter q_A approximates the characteristics of player *A*—and it is hence known to *A*—that affect the potential participant's willingness to do business with *A*, but q_A need not be known to the potential participant. When the potential participant is unaware of the particular value of q_A, we consider that she believes that its value is $-q$ with probability ½ and q with probability ½. When there is no uncertainty, q_A takes one of the two admissible values. This parameter can be interpreted as the reputational risk of doing business with *A*.

In this study, we closely link the lead arranger's reputation with the regulator's signal on the lead arranger's compliance with regulatory law on the books. Specifically, if a principal arranger has recently been audited by the regulator and found to have engaged in legal or regulatory misconduct, then she receives an enforcement action that is publicly announced. It is then natural to assume that q_A is known and takes the value $-q$. This implies that potential participants incur costs by forming loan syndicates with principal arrangers with bad reputations (i.e., those punished by the regulator).

On the other hand, when *A* has been audited and found to comply with laws and regulations, then q_A is also known but takes the value q. This essentially implies that potential participants gain reputation by associating with principal arrangers with good reputations.

[21] For example, we can replicate the analysis considering general forms of *u* and *c*—for our results to hold, it is essential that the lead arranger's expected utility is strictly concave in the size of her share and that *c* is strictly convex in effort—without adding anything to the intuition that we obtain from analyzing the current specification. However, this exercise bears considerable cost in the complexity of formal arguments.

Finally, when little is known regarding q_A, we can assume that the potential participant assigns equal probability to any of the two eventualities, which is identical to conducting business with a principal arranger of intermediate reputation.

Overall, we consider that reputational risk is proportional to the degree of association. If the potential participant contributes a small (large) amount to a loan designed by *A*, it undertakes little (great) reputational risks associated with this loan. This relationship is the reason why we multiply q_A with a_P in the payoff of *P*.

Because this is a game of incomplete (the monitoring effort exerted by the principal arranger is unobservable) and asymmetric (the principal arranger is better informed about q_A than the potential participant) information, the natural solution concept is a perfect Bayesian equilibrium (PBE). For a proper characterization of such an equilibrium, one should identify a profile of players' strategies along with a consistent system of beliefs such that Bayes' rule is applied whenever possible. To investigate how a PBE should look like in this framework, we start by focusing on the fourth stage of the game.

After a contract (a_A, a_P) is approved in stage 3, the last decision of the game occurs in stage 4: The principal arranger decides how much monitoring effort to exert. Given our assumptions, therefore, at this stage the principal arranger solves the following problem:

$$max_{e\geq 0}\left\{\int_{1+r}^{s+e}\left[\frac{1}{s+e}(1+r)a_A\right]d\gamma - a_A^{\xi} - e^2\right\}. \quad \text{(A.1)}$$

Equation (A.1) simply amounts to the lead arranger deciding $e \geq 0$ in order to maximize her expected payoff, given that the contract (a_A, a_P) was approved. Simple algebra establishes that, for any positive participation share on behalf of the principal arranger, $a_A > 0$, there exists a unique interior solution $e^* > 0$, which is characterized by

$$2e^*(e^* + s)^2 = a_A(1+r)^2 \quad \text{(A.2)}$$

and it is such that:

$$\frac{\partial e^*}{\partial a_A} = \frac{(1+r)^2}{(s+e^*)^2\left(2+\frac{2a_A(1+r)^2}{(s+e^*)^3}\right)} > 0. \quad \text{(A.3)}$$

Observation 1: All else constant, the principal arranger's monitoring effort, and subsequently the project's cumulative success potential, strictly increases along with the principal arranger's participation share, a_A.

Observation 1 is quite intuitive, because the principal arranger has much greater incentive to improve the project's success potential when she has financed a large part of it compared with when she holds only a small part of the loan. To study what happens in the contract design stage, we put forward a formal assumption regarding when the potential participant signs a proposed contract and when she declines.

Assumption 1: We assume that the potential participant signs the contract if and only if her expected payoff from doing so is larger than investing the same amount of money in an outside option with success probability $w \in (0,1)$.

Taking into account that the only reasonable expectations regarding the monitoring effort that *A* will exert in the fourth stage of the game are uniquely defined for every admissible triplet (s, a_A, r), the participation constraint of the potential participant is

$$\int_{1+r}^{s+e^*(s,a_A,r)} \left[\frac{1}{s+e^*(s,a_A,r)}(1+r)a_P\right] d\gamma - a_P^{\xi} + a_P E(q_A) \geq w(1+r)a_P - a_P^{\xi}. \quad \text{(A.4)}$$

All these suggest that a PBE of this game is characterized by a solution of the following maximization problem:

$$max_{a_A \in [0,1]} \left\{ \int_{1+r}^{s+e^*(s,a_A,r)} \left[\frac{1}{s+e^*(s,a_A,r)}(1+r)a_A\right] d\gamma - a_A^{\xi} - e^*(s,a_A,r)^2 \right\} \quad \text{(A.5)}$$

s.t.

$$\int_{1+r}^{s+e^*(s,a_A,r)} \left[\frac{1}{s+e^*(s,a_A,r)}(1+r)\right] d\gamma + E(q_A) \geq w(1+r) \tag{A.6}$$

or

$$a_A \in \{0,1\}. \tag{A.7}$$

This maximization problem is well defined and hence always admits a unique solution—that is, we always have a unique equilibrium. When $a_A^* = 0$, no contract is offered, and when $a_A^* = 1$, the principal arranger finances the whole project (so approval of the contract by any other potential participant is unnecessary). Thus only the case in which $a_A^* \in (0,1)$ is interesting. Notice that the syndicate loan case $a_A^* \in (0,1)$ is generic: When *s* is larger than $1 + r$, but not excessively large, then the principal arranger wants to finance part of the project; and when *w* is sufficiently small, then the potential participant is willing to participate too. When $a_A^* \in (0,1)$, the constraint could be binding or not.

The question of interest relates to the comparative statics of this solution with respect to a discrete variable, namely $E(q_A)$. Notice that $E(q_A) \in \{-q, 0, q\}$ because either *P* knows the value of q_A—and hence we have either $E(q_A) = -q$ or $E(q_A) = q$—or she does not, in which case we have $E(q_A) = 0$. In other words, *P* either knows or does not know whether *A* has been subject to an enforcement action.

Consider first that $E(q_A) = 0$ and that the solution, a_A^*, is such that the constraint is not binding. Then,

$$\int_{1+r}^{s+e^*(s,a_A^*,r)} \left[\frac{1}{s+e^*(s,a_A^*,r)}(1+r)\right] d\gamma \geq w(1+r) \tag{A.8}$$

and the equilibrium contract, a_A^*, is characterized by

$$s + e^*(s, a_A^*, r) = \frac{a_A^*(1+r)^2}{a_A^* + a_A^* r - {a_A^*}^{-\xi}\xi}. \tag{A.9}$$

Intuitively, this case is not as interesting from a real-world viewpoint because enforcement actions are public information.[22]

So what happens if we keep everything constant but change the value of $E(q_A)$ from zero to $-q$? In that case, if the constraint is still satisfied when computed for the initial contract, a_A^*, then the equilibrium contract should remain identical to the initial one. This is because in such a case, the solution should coincide with the principal arranger's ideal contract, $\hat{a}_A$ (understood as the solution of the principal arranger's unconstrained maximization problem). As we saw earlier, this ideal contract never depends on the exact value of $E(q_A)$.

Because $E(q_A)$ changes from zero to a negative value, however, it might be the case that the contract, a_A^*, is such that

$$\int_{1+r}^{s+e^*(s,a_A^*,r)} \left[\frac{1}{s+e^*(s,a_A^*,r)}(1+r)\right] d\gamma - q \geq w(1+r), \tag{A.10}$$

which suggests that the new solution, a_A^{**}, involves a binding constraint. In such a case,

$$\int_{1+r}^{s+e^*(s,a_A^{**},r)} \left[\frac{1}{s+e^*(s,a_A^{**},r)}(1+r)\right] d\gamma - w(1+r) = q. \tag{A.11}$$

We notice that

$$\partial\left(\int_{1+r}^{s+e^*(s,a_A,r)} \left[\frac{1}{s+e^*(s,a_A,r)}(1+r)\right] d\gamma\right)/\partial a_A = \left(\frac{1+r}{s+e^*(s,a_A,r)}\right)^2 \frac{\partial e^*}{\partial a_A} > 0. \tag{A.12}$$

In other words, the constraint can switch from being not binding to being binding if and only if $a_A^{**} > a_A^*$. The intuition is clear: When the reputational risks increase because of the enactment of an enforcement action ($E(q_A)$ jumps from zero to $-q$), a potential participant either still finds the principal arranger's initial contract, a_A^*, appealing enough to sign it or she refuses to sign unless the principal arranger increases the project's success probability and hence compensates for the extra reputational risk that *P* now undertakes. The only way that *A*

[22] There are certain informal enforcement actions imposed on banks that are not made public, which we discuss below. One can also think of the special case where $E(q_A) = 0$ as when participants only suspect that a principal arranger has been subject to informal action.

can credibly commit to increasing the project's success probability is by taking a larger share of the loan herself, thus increasing her incentive to exert more monitoring effort after the contract is signed. Of course, if q is very large, then we could have that $a_A^{**} = 0$ (i.e., no contract is offered), because it might be impossible for *A* to propose a deal that is both profitable for her and good enough for *P* to participate. But for non-extreme values of q, one should expect *A* to propose a contract with a strictly larger a_A.

Now consider that $E(q_A) = 0$ and that the solution, a_A^*, is such that the constraint is binding. It is obvious that if we change the value of $E(q_A)$ from zero to $-q$, then it cannot be the case this constraint still holds for the same contract. The arguments presented above should make clear that in this case, the new solution, a_A^{**}, is such that the left-hand side of Equation (A.11) is equal to q and, hence, $a_A^{**} > a_A^*$. Again, all these are conditional on q not being extremely large, because in such case we could have $a_A^{**} = 0$. Hence, again, the principal arranger reacts to a decrease in $E(q_A)$ by taking a larger share of the loan in order to commit herself to do more to improve the loan's success potential.

All the above hold for any decrease in $E(q_A)$, not just for changes from zero to $-q$. Symmetric arguments guarantee that an increase in $E(q_A)$ (for example, a change from zero to q) will cause *A* either to decrease the share of the loan that she finances or to leave the contract unchanged.

Observation 2: All else constant, a decrease (increase) in $E(q_A)$ induces an increase (decrease) in the lead arranger's equilibrium participation share in a syndicate loan.

Appendix II. More Information on Formal Enforcement Actions

Table A.I

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 reputation of the bank and, by extension, to its partnerships. We suggest that the best possible categorization for our objective 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 et al. (2017b), 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.

We identify three such types of enforcement actions as reflected in Table A.I. 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.I. 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.

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 effect and thus we exclude them from our analysis.

Appendix III. Sub-sample only for punished banks

In this Appendix we report results using a sub-sample only for punished banks that received a sanction during our sample period. In all specifications, the effect of the enforcement action on various measures of syndicate structure in the first year after the enactment is statistically significant at the 1% level (except column III which is at 5% level) for the most important dependent variables) The results in column I show that an enforcement action increases the lead lender's share in the syndicate by approximately 3.5 percentage points. For the lead lender with an average share (equal to 19.3% in our sample), this finding implies a very large increase of approximately 23.3%. A very similar picture appears when using as our dependent variables the HHI (%) of the loan syndicate and the number of lenders. Thus, although there is a decrease in the number of lenders that participate in a loan syndicate when the lead arranger receives an enforcement action, the most significant effect comes from the lead arranger taking up a larger share of the loan.

Table A.II
Results only for punished banks

The table reports coefficients and t-statistics (in brackets) only for punished lead lenders. We estimate the regression: $S = a_f + a_1\, PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III
Dependent variable:	Lead lender shares (%)	HHI (%)	Number of lenders
Post enforcement loan	3.554***	2.731***	-0.647**
	[7.603]	[7.173]	[-2.388]
Maturity	-3.645***	-3.408***	0.763***
	[-16.065]	[-14.272]	[27.564]
Facility amount	-0.851***	-1.124***	0.297***
	[-33.524]	[-23.142]	[9.040]
Downgrading	1.303**	2.362***	-1.900***
	[2.505]	[3.543]	[-9.875]
Performance pricing	-5.293***	-5.766***	1.533***
	[-39.915]	[-47.034]	[19.890]

Collateral	-1.415	-0.558	2.627***
	[-1.438]	[-0.857]	[9.904]
Lead-borrower Relationship	-1.142***	-1.986***	0.006
	[-3.404]	[-4.032]	[0.043]
Observations	2,562	2,562	2,060
Adjusted R-squared	0.750	0.772	0.508
Loan-purpose FE	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes
Year effects	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes
Bank effects	Yes	Yes	Yes
Clustered standard errors	Firm, Bank	Firm, Bank	Firm, Bank

Appendix IV. Use of Fixed Effects

In this appendix, intended for online use only, we provide an example to show why the estimation of equation (13) with bank*year fixed effects does not yield perfect collinearity between these fixed effects and PEL. Consider the data set attached in the Table below, which replicates the structure of our actual data set. There are 6 banks, each issuing a number of loans over 6 years. There are 33 loans, each issued by at least two lead banks: observations 1 to 33 reflect the *first* lead banks of each loan and observations 34 to 66 reflect the *second* lead banks. The first lead banks 2 and 3 have received enforcement actions in years 2 and 4, respectively. The second lead banks 5 and 6 have received enforcement actions in years 2 and 3, respectively. The column S denotes the share (in percentage) of each lead bank in the loan.

If we use observations 1 to 33 with bank*year fixed effects, PEL drops out due to perfect collinearity. In Stata, this comes from the commands:

egen by=group(Bank Year)

reghdfe S PEL, a(by)

This is irrespective of the sample size: indeed one can increase the sample size of our example and PEL will still drop out simply because PEL is a bank*year variable. However, adding a second lead bank in the observations 34-66, that received an enforcement action in a different year compared to the first lead bank, means that identification can be obtained from the differences in PEL between the first and the second lead arrangers within the same loan-year. We hope that this example facilitates a better reading of our empirical approach.

Obs.	Bank	Year	Loan	PEL	S
1	1	1	1	0	5
2	1	1	2	0	6
3	1	2	3	0	4
4	1	3	4	0	7
5	1	4	5	0	5
6	1	5	6	0	3

7	1	5	7	0	8
8	1	5	8	0	9
9	1	6	9	0	2
10	1	6	10	0	5
11	1	6	11	0	6
12	1	6	12	0	7
13	2	1	13	0	4
14	2	1	14	0	7
15	2	1	15	0	8
16	2	1	16	0	9
17	2	2	17	1	5
18	2	2	18	1	6
19	2	2	19	1	7
20	2	2	20	1	8
21	2	3	21	1	5
22	2	3	22	1	4
23	2	3	23	1	5
24	3	1	24	0	6
25	3	1	25	0	3
26	3	1	26	0	8
27	3	3	27	0	7
28	3	3	28	0	6
29	3	4	29	1	5
30	3	4	30	1	9
31	3	4	31	1	8
32	3	5	32	1	7
33	3	6	33	1	4
34	4	1	1	0	5
35	4	1	2	0	6
36	4	2	3	0	4
37	4	3	4	0	7
38	4	4	5	0	5
39	4	5	6	0	3
40	4	5	7	0	8
41	4	5	8	0	9
42	4	6	9	0	2
43	4	6	10	0	5
44	4	6	11	0	6
45	4	6	12	0	7
46	5	1	13	0	4
47	5	1	14	0	7
48	5	1	15	0	8
49	5	1	16	0	9
50	5	2	17	0	5
51	5	2	18	0	6
52	5	2	19	0	7

53	5	2	20	1	8
54	5	3	21	1	5
55	5	3	22	1	4
56	5	3	23	1	5
57	6	1	24	0	6
58	6	1	25	0	3
59	6	1	26	0	8
60	6	3	27	1	7
61	6	3	28	1	6
62	6	4	29	1	5
63	6	4	30	1	9
64	6	4	31	1	8
65	6	5	32	1	7
66	6	6	33	1	4

Appendix V. Results without Loan Controls

In this Appendix, we report results from specifications without loan controls. In line with Angrist and Pischke (2008, pp. 47-51), this safeguards our analysis from a bad controls problem (some of the controls actually being outcome variables). In column I of the table below we report results from a specification using the fixed effects model (equivalent to specification I in Table IV). In column II we use the treatment effects model (equivalent to specification I in Table VIII). The results are very similar to their equivalents in the main text.

Table A.III
Models without Loan Controls

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1\ PEL_{b,t} + u_{f,b,l,t}$, where *b* refers to bank; *l* for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II
	Fixed effects model	Treatment effects model
Post enforcement loan	3.417***	4.012***
	[3.516]	[3.602]
Observations	75,125	62,344
Adjusted R-squared	0.854	
Loan-purpose FE	Yes	Yes
Loan-type FE	Yes	Yes
Firm*Year FE	Yes	No
Bank*Year FE	Yes	No
Clustering	Firm, Bank	Firm, Banks

Appendix VI. Results from a Dynamic Treatment Model

In this Appendix, we examine the dynamics enforcement actions, to see whether a sanction's effect appears before the enactment. Our results below in Table A.IV provide evidence that there is no pre-trend in the syndicate structure.

Table A.IV
Dynamic treatment

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1\, PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where b refers to bank; l for loans, *f* for firms and *t* for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S is defined as the *lead lender shares (%)* and represents the syndicate loan structure. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I
Dependent variable:	Lead lender shares (%)
Pre enforcement loan	0.065
	[0.598]
Post enforcement loan	4.962***
	[2.876]
Post enforcement loan (t+1)	3.450***
	[2.737]
Maturity	-5.384***
	[-18.936]
Facility amount	-6.972***
	[-14.778]
Downgrading	-2.463***
	[-4.830]
Performance pricing	-8.156***
	[-8.920]
Collateral	2.502***
	[3.133]
Lead-borrower relationship	-5.155***
	[-14.246]
Observations	95,981
Adjusted R-squared	0.450
Loan-purpose FE	Yes
Loan-type FE	Yes
Year FE	Yes

Firm FE	Yes
Bank FE	Yes
Clustered standard errors	Firm and Bank

Appendix VII. Results for Lending

In this Appendix, we report results from specifications with the number of loans (columns I and II in the table below), the facility amount (columns III and IV), and the probability to work with new borrowers (column V and VI) as dependent variables.

Table A.V
Results for Lending

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1\, PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where b refers to bank; l for loans, f for firms and t for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. Specifications I, III, and Y are estimates with OLS on the fixed effects model and specifications II, IV, and VI with maximum likelihood on the treatment effects model (equivalent to specification I in Table VIII). All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III	IV	V	VI
Dependent variable:	Number of loans	Number of loans	Facility amount	Facility amount	New borrowers	New borrowers
Post enforcement loan	-0.107	-0.235	-0.017	-0.012	0.009	-0.015
	[-0.762]	[-0.955]	[-0.625]	[-0.311]	[0.107]	[-0.322]
Lon controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	75,125	75,125	75,125	75,125	75,125	75,125
Adjusted R-squared	0.734		0.802		0.911	
Loan-purpose FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Year FE	Yes	No	Yes	No	Yes	No
Bank*Year FE	Yes	No	Yes	No	Yes	No
Clustering	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank	Firm, Bank

Appendix VIII. Results from Bank Specialization

In Table A.IV we dig deeper into the role of a bank's sectoral exposure, as given by the loan amounts by industry (1-digit, 2-digit, 3-digit, and 4-digit SIC code). We follow Cai et al. (2018) and Gupta et al. (2017) to calculate banks' sectoral exposure in each area of specialization as the share of lending of each bank to different sectors. Subsequently, for each lead arranger we compute the inverted and standardized Euclidean distance with the participant banks as follows:

$$Bank\ similarity = \left(\sqrt{\sum_{s=1}^{S} \left(w_{b_1,t}^{s} - w_{b_2,t}^{s} \right)^2} \right)^{-1}$$

with

$$w_{b,t}^{s} = \frac{Loan_t^{b \to s}}{Total\ loan_t^{b \to S}}\text{, for any bank b.}$$

$Loan_t^{b \to s}$ is the amount (in millions USD) lent by bank *b* to sector *s* at time *t* and $Total\ loan_t^{b \to S}$ is the total amount (in millions USD) that bank *b* lent during the same year to the total number of sectors (*S*). Note that our analysis will assign a greater similarity measure to lead-participant banks that are "closer" to each other; hence, the use of inverse distances. *Bank Similarity* is a positive number, with zero corresponding to lack of similarity and larger values reflecting stronger similarity.

The results are reported in Table A.IV. In columns I-IV we analyze one interaction term at a time between *post enforcement loan* and *bank similarity* based on the 1-digit, 2-digit, 3-digit, and 4-digit borrower SIC code, respectively. In all regressions, the coefficient of the interaction term is negative, indicating that higher similarity in the business model between the lead arranger and participant mitigates the positive effect of the enforcement action.

Table A.VI
The Role of Sectoral Exposure in Mitigating Informational Asymmetry

The table reports coefficients and t-statistics (in brackets) only for lead lenders. We estimate the regression: $S = a_f + a_1\, PEL_{b,t} + a_2 L_{l,t} + u_{f,b,l,t}$, where b refers to bank; l for loans, f for firms and t for years. We estimate this regression on a loan-level sample originated from 1997 to 2014. S represents the syndicate loan structure and is reported defined in the second line. *PEL* (post enforcement loan) is the main explanatory and is defined as a dummy taking the value one for loans originated by a punished lead bank after the date t of the enforcement and zero for loans originated before the date of the enforcement action or for all loans originated by lead banks that were never punished during our sample period. All variables are defined in Table I. All regressions include fixed effects as shown in the lower part of the table and the standard errors are clustered by firm and bank. The *, **, *** marks denote statistical significance at the 10, 5, and 1% level, respectively.

	I	II	III	IV
	SIC1	SIC2	SIC3	SIC4
Post enforcement loan	3.380**	4.412***	3.519***	2.178***
	[2.056]	[3.388]	[3.849]	[3.302]
Bank sectoral exposure	-12.371***	-8.183*	-1.234	1.512
	[-4.393]	[-2.008]	[-0.364]	[0.614]
Post enforcement loan * Bank sectoral exposure	-1.154	-4.485*	-2.094**	-1.933*
	[-0.354]	[1.777]	[-2.177]	[-1.855]
Maturity	-1.673***	-1.707***	-1.769***	-1.748***
	[-4.825]	[-4.247]	[-4.057]	[-3.493]
Facility amount	-0.092	-0.093	-0.100	-0.106
	[-0.840]	[-0.625]	[-0.644]	[-0.657]
Downgrading	-0.179	-0.214	-0.030	-0.134
	[-0.161]	[-0.173]	[-0.025]	[-0.109]
Performance pricing	-5.033***	-5.236***	-5.384***	-5.366***
	[-9.127]	[-9.512]	[-9.352]	[-9.031]
Collateral	-3.917***	-3.945***	-3.745***	-3.835***
	[-4.719]	[-4.964]	[-4.845]	[-4.829]
Lead-borrower relationship	-5.177***	-4.916**	-3.545**	-3.393**
	[-3.958]	[-2.432]	[-2.521]	[-2.406]
Marginal effect of post enforcement loan	2.226	-0.073	1.427	0.242
(P-value)	0.000	0.000	0.000	0.000
Observations	63,190	63,872	63,244	62,868
Adjusted R-squared	0.882	0.882	0.881	0.881
Loan-purpose FE	Yes	Yes	Yes	Yes
Loan-type FE	Yes	Yes	Yes	Yes
Firm*Year FE	Yes	Yes	Yes	Yes
Bank*Year FE	Yes	Yes	Yes	Yes
Clustering	Bank-Firm	Bank-Firm	Bank-Firm	Bank-Firm