Bank Deregulation and the Rise of Institutional Lending*

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Abstract

We study the determinants of increased participation of non-bank financial intermediaries in the market for syndicated loans prior to the 2008 financial crisis. Institutional investors who do not have monitoring expertise disproportionately purchase loan tranches originated by banks able to offer both loans and underwriting services to firms. Our argument is that non-loan exposures to firm performance ensure monitoring incentives even when banks retain small loan shares. Since such universal banking was permitted only after the repeal of the Glass-Steagall Act, our findings suggest a direct link from bank deregulation to the rise of non-bank intermediaries.

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1 Introduction

The credit boom that culminated in the 2008 financial crisis was characterized by the growing capital-market participation of non-bank institutional investors and the associated rise of securitization (Ivashina and Sun (2011a) and Gorton and Metrick (2013)). In the market for corporate credit, these developments were most dramatic in the context of syndicated lending, where banks increasingly sold loan tranches to institutional buyers, who then securitized and re-sold many of the securities they purchased.[1]

The traditional view of financial intermediation holds that bank loans cannot be sold because banks have no incentives to monitor loans whose performance they are no longer exposed to. This raises the question of why institutional investors were increasingly willing to buy bank loans despite these concerns. This paper argues that bank deregulation prior to the $2001 - 2007$ credit boom allowed banks to maintain sufficient exposure to borrower performance even when selling increasingly large loan shares to third-party investors. Bank deregulation thus facilitated the entry of non-bank intermediaries into the market for corporate credit by increasing the marketability of bank-originated loans.

Our focus is on increases in bank scope after the repeal of the Glass-Steagall Act, which allowed for the formation of universal banks – large financial institutions able to simultaneously offer both loans and underwriting services to firms. We start from the premise that the advent of such cross-selling of financial products (see, among others, Drucker and Puri (2005)) served to broaden intermediation relationships. We then use a variant of the Holmström and Tirole (1997) model of informed lending to argue that broader intermediation relationships in turn increased universal banks' exposure to firm performance beyond that afforded by its loan share alone, making it easier to provide monitoring incentives. Institutional investors may therefore trust universal banks to monitor borrowers at loan shares at which they would not trust a stand-alone commercial or investment banks to do so.

[1] Ivashina and Sun (2011a) estimate that nearly 70% of the increase in syndicated-loan issuance during the $2001 - 2007$ credit boom is accounted for by institutional funding. Nadauld and Weisbach (2012) show that term-loan B facilities, which represent a majority of the syndicated-loan tranches obtained by institutional investors, were much more frequently securitized than other facilities. Therefore, institutional investment in syndicated loans is an important step in the securitization process.

Using a narrowly defined treatment that increases the propensity of universal banks to engage in cross-selling loans and non-loan products, our most conservative estimates suggest that the advent of universal banking rendered marketable roughly $100 billion in corporate loans that could otherwise not have been sold over the time period from late 1996 to 2008. Bank deregulation thus altered the structure of loan syndicates, and boosted the marketability of assets demanded by institutional investors by altering lending relationships between universal banks and borrower firms.

Figure 1 depicts our key results. In the top panel, we plot the average loan share retained by lead arrangers from 1987 to 2008 for three samples: all syndicated loans, syndicated loans in which at least one lead arranger was a universal bank, and syndicated loans in which no lead arranger was a universal bank. The average lead share fell across all samples. We find large cross-sectional differences: syndicates in which at least one lead arranger was a universal bank exhibit substantially lower lead shares than syndicates without any universal-bank lead arrangers throughout the time series. However, these differences are particularly pronounced in the years leading up to the financial crisis.

We juxtapose these developments with the participation of institutional investors in the market for syndicated loans in the bottom panel of Figure 1. Universal-bank-led and non-universal-bank-led syndicates initially exhibit similar institutional-investor participation, as reflected by their total participant share. The gap subsequently widens during the second half of the 1990s. Quantitatively speaking, the larger participant share held by institutional investors in syndicates led by universal banks matches well with the smaller lead share documented in the top panel of the figure. These facts are suggestive of our hypothesis that the advent of universal banking and the resulting richness of bank-firm interactions are key determinants of the evolution of lead shares and institutional-investor participation from the mid 1990s onwards.

To establish these facts empirically, we exploit the variation in bank scope generated by the stepwise repeal of the Glass-Steagall Act. As in Neuhann and Saidi (2016), we focus on a shock to bank scope through a deregulatory event in 1996 that removed some of

the informational and financial firewalls between securities and lending divisions in existing universal banks. Specifically, Neuhann and Saidi (2016) show that the 1996 deregulation boosted the cross-selling of loans and non-loan products by universal banks, and enabled these banks to realize (informational) economies of scope in monitoring.

To distill the impact of bank scope on syndicate structures, we separately analyze (i) the choice of lead arrangers within syndicates, and (ii) the loan share retained by the lead bank ("lead share"). We employ a difference-in-differences specification around the 1996 deregulation that compares universal banks to banks whose scope was not affected by said deregulation, such as stand-alone commercial and investment banks or institutional investors.

We find that within loan syndicates, universal banks were significantly more likely to be chosen as lead arrangers following the 1996 deregulation. This result holds when using commercial banks and investment banks as our control group, and is particularly strong for institutional investors. The gap in the likelihood of becoming lead arranger between universal banks and institutional investors widened by up to 25 percentage points after the 1996 deregulation. Similarly, we show that syndicates with universal banks assigned up to five percentage points smaller loan shares to their lead arrangers after the 1996 deregulation than did syndicates without any universal-bank participation.

Using the same difference-in-differences strategy, we then document that institutional investors disproportionately participated in syndicates with universal banks after the deregulation. We find the most striking results in a subsample of loans granted to relatively less volatile firms, where the total share of institutional participants in loan syndicates almost doubled after the deregulation. The same cross-sectional comparative static holds for lead shares more generally: universal banks' lead shares fall disproportionately among loans to less volatile firms. Since previous monitoring experience is likely to be less informative for the state of the firm today if firm-level outcomes are highly volatile, we believe that these findings support our proposed mechanism that universal banks benefited from economies of scope in monitoring. This is true particularly since alternative mechanisms that explain reduced lead shares by higher risk-sharing needs would imply that we should find the strongest

effects among more, and not less, volatile firms.

What is more, the relative strength of the effect of the 1996 deregulation on institutional-investor participation in syndicates with universal banks is strongest for the loan shares of institutional-investor participants who have the least direct monitoring expertise, such as mutual funds, pension funds, and insurance companies.

In sum, our results suggest that the repeal of the Glass-Steagall Act boosted loan liquidity, and facilitated the participation of institutional investors by broadening intermediation relationships among universal banks and their borrower firms. They also highlight how structural changes in the banking sector altered capital markets more generally, and point to a role for bank deregulation in triggering the rise of institutional lending and, therefore, securitization prior to the 2008 financial crisis.

Our findings are thus relevant for recent political debates suggesting a renewed appetite for the reinstatement of the Glass-Steagall Act. While we do not intend to imply that the increased syndication of corporate loans was a direct cause of the crisis, we do argue that the advent of universal banking fostered the growth of institutional investors and securitization that is central to many narratives of the crises (Gorton and Metrick (2013)). In support of this hypothesis, Manconi, Massa, and Yasuda (2012) provide evidence that the contagion of the crisis from securitized assets to corporate bonds operated through liquidity-constrained institutional investors, while Goldstein, Jiang, and Ng (2016) argue that the growth of corporate bond funds may represent a new source of financial fragility.

Our proposed mechanism suggests that universal banking boosted loan liquidity without excessively harming monitoring incentives. Importantly, this does not imply that increased loan syndication and securitization was efficient in the aggregate. Neuhann (2016) studies a model in which loan sales lead to the origination of inefficiently risky loans even as both buyers and sellers find it optimal to continue trading securitized products.

Related literature. Our paper is related to the literature that investigates the determinants of lead shares in syndicated lending. Sufi (2007) argues that unrated firms are harder to

monitor than rated firms, and finds that lead arrangers retain a larger share of loans to unrated firms as a result. Our paper differs in that we investigate the bank-level, rather than the firm-level, determinants of syndicate structures. We also refine the results in Sufi (2007) by interacting bank-level and firm-level characteristics.

Ivashina (2009) estimates the costs of asymmetric information between lead arrangers and syndicate members by explicitly accounting for bank-level risk exposure. We complement her findings by characterizing the role of bank scope in determining the structure of loan syndicates through non-loan interactions. Ivashina and Scharfstein (2010a) as well as Ivashina and Scharfstein (2010b) focus on the cyclical properties of loan shares retained by lead arrangers. Our paper complements their work by focusing on secular trends in the lead share in response to bank deregulation.

We also relate to a recent literature on the role of institutional investors in syndicated lending. Ivashina and Sun (2011a) show increased demand pressure by institutional investors reduced corporate loan spreads, while Lim, Minton, and Weisbach (2014) argue that loan facilities held by institutional investors carry higher spreads on average. In addition, Berg, Saunders, Steffen, and Streitz (2016) document that institutional-investor participation explains loan-spread differences between U.S. and European non-investment-grade term loans. Ivashina and Sun (2011b) and Massoud, Nandy, Saunders, and Song (2011) argue that institutional investors may trade on private information gleaned in the syndicated-loans market. This may be all the more important as institutional investors can simultaneously hold both equity and debt claims of the same company (Jiang, Li, and Shao (2010)).

We contribute to this literature by studying why institutional investors began to heavily participate in the syndicated-loans market in the first place. In analyzing the factors underlying the growth of securitization and originate-to-distribute banking, our paper is also related to Gorton and Pennacchi (1995) and Gorton and Metrick (2013).

Our theoretical framework is based on Holmström and Tirole (1997). We enrich this model by incorporating the notion that repeated interactions between borrowers and lenders relax intermediation frictions. We therefore build on a large theoretical literature on rela-

tionship banking (Sharpe (1990), Rajan (1992), and von Thadden (2004)). By providing an application to bank scope, our proposed mechanism also relates to the literature on universal banking that focuses on the ability of universal banks to simultaneously offer loans and underwriting services. For example, Kanatas and Qi (1998) and Kanatas and Qi (2003) argue that universal banks can save on information costs by monitoring a firm once and for all, while stand-alone banks have to exert monitoring effort for each service separately.

2 Theoretical Framework

In this section, we introduce the theoretical framework we use to generate empirical predictions. We first lay out a model of informed lending that follows Holmström and Tirole (1997). We then map the key model elements to the data, and discuss their empirical content.

2.1 Model

The basic setting is as in Holmström and Tirole (1997). There is a firm with capital A_f which requires $I - A_f$ in external funding to finance a project of size I. The project yields a return of R if it is successful and 0 if it fails. The firm is run by an entrepreneur who can deliberately reduce the probability of success to enjoy a private benefit B. If he does so, the probability of success is p_L rather than p_H, with $\Delta p = p_H - p_L > 0$. The firm can obtain financing from two sources: *outside investors*, who are uninformed in the sense that they do not possess any monitoring expertise, and *intermediaries*, who can reduce the private benefit of shirking from B to b by exerting privately costly effort. Outside investors and intermediaries both have access to an outside option with return γ.

We modify the setting of Holmström and Tirole (1997) in two ways. First, we repeat the model twice. This allows us to study multiple interactions between the firm and its lenders. Second, we allow intermediaries to become more efficient at monitoring over time. Formally, we assume that the cost of monitoring is c_H if the intermediary has not monitored the firm

in the past, and $c_L < c_H$ if it has. This feature allows us to characterize the importance of intermediation relationships. If an intermediary has monitored the firm in the past, we say that it is *experienced*.

To focus on the role of the cost differential $\Delta c = c_H - c_L > 0$ in shaping the intermediation contract, we assume that firms cannot use retained earnings to finance investment, that there are no long-term contracts, and that the firm receives funding from at most one intermediary in each period. Accordingly, we assume that firms, intermediaries, and investors receive an endowment of A_f, A_m, and A_u, respectively, at the beginning of each period, consume the proceeds from the project at the end of each period, and that the firm applies for funding anew at the beginning of each period. We assume throughout that A_u is large enough that outside investors can supply all required funds that are not supplied by intermediaries, but that outside investors are not willing to invest unless the firm is monitored by an intermediary.

Since there are no long-term contracts, the only link across periods is the potential reduction in monitoring costs stemming from monitoring experience. As will become clear, fully dynamic contracts would only serve to strengthen the results that drive our empirical predictions.

We start our analysis in the second and final period. Wherever possible, we follow the notation and results in Holmström and Tirole (1997). Let the project's payoffs be divided up so that $R_f + R_m + R_u = R$, where R_f, R_m, and R_u denote the shares of the firm, the intermediary, and outside investors, respectively. Assuming that the firm is monitored by an intermediary, the firm's incentive constraint is $R_f \geq \frac{b}{\Delta p}$, while an intermediary with cost $c \in \{c_L, c_H\}$ prefers to monitor if $R_m \geq \frac{c}{\Delta p}$. Lower costs thus relax the intermediary's incentive constraint.

The rate of return on intermediary capital within the period is $\beta = p_H \frac{R_m}{I_m}$, where I_m denotes the amount of capital that the intermediary lends to the firm. Since monitoring is costly, β must exceed the intermediary's outside option γ, and firms prefer uninformed to informed intermediary capital whenever possible. As a result, firms borrow just enough capital from intermediaries in order to ensure monitoring incentives, $I_m(\beta, c) = p_H \frac{c}{\Delta p \beta}$. The

remaining capital $I - A_f - I_m$ is provided by outside investors at rate γ. In the context of syndicated lending, I_m corresponds to the lead share, because lead arrangers are the designated monitors who represent informed capital. $I - A_f - I_m$ in turn represents the participant share allocated to uninformed outside investors. As we will show below, the scope for monitoring experience offered by repeated interactions will reduce the required lead share and increase the participant share.

The return on intermediary capital that delivers an expected return equal to the outside option γI_m is $\underline{\beta} = \gamma \frac{p_H}{p_L} > \gamma$. When the intermediary sector is sufficiently well capitalized (A_m is large) and there is no experienced intermediary, competition dissipates all rents until the equilibrium rate of return on intermediary capital is $\underline{\beta}$. We assume throughout that A_m is indeed large enough that intermediaries earn no rents when there is no experienced intermediary. As a result, intermediaries offer $I_m^0(\underline{\beta}, c_H) = p_H \frac{c_H}{\Delta p \underline{\beta}}$ units of capital in exchange for a payment of $R_m = \frac{c_H}{\Delta p}$, and receive a rate of return of exactly $\underline{\beta}$ whenever there is no experienced intermediary.

In the presence of an experienced intermediary, however, a large A_m is no longer sufficient to dissipate all rents: the experienced intermediary can use its cost advantage to undercut all competitors and still earn excess profits. The experienced intermediary can, of course, also pass on the cheaper monitoring costs to the firm through better lending terms. The worst case for the firm is that the experienced intermediary offers exactly the same terms as an inexperienced intermediary, and retains all rents for itself. In this case, the experienced intermediary acts as a monopolist limited only by a competitive fringe of inexperienced intermediaries. The best case for the firm is that the experienced intermediary offers $I_m(\underline{\beta}, c_L) = p_H \frac{c_L}{\Delta p \underline{\beta}}$ units of capital in exchange for a payment of $R_m = \frac{c_L}{\Delta p}$. This is the outcome that would arise under full competition among a number of experienced intermediaries.

The exact division of the surplus is immaterial to our analysis. We therefore simply assume that the experienced intermediary offers a convex combination of the "monopolist" and "perfect-competition" contracts, with the intermediary's bargaining power $\mu \in [0, 1]$

determining the weight on the monopolist contract:

$$I_m^*(\underline{\beta},\mu) = p_H \frac{\mu c_H + (1-\mu)c_L}{\Delta p \underline{\beta}} \qquad \text{and} \qquad R_m(\mu) = \frac{\mu c_H + (1-\mu)c_L}{\Delta p}.$$

This contract delivers the expected return $p_H \frac{\mu c_H + (1-\mu)c_L}{\Delta p} - c_L$ to the experienced intermediary. Two observations follow. First, the experienced intermediary earns an expected return that is strictly larger than the outside option $\gamma I_m(\underline{\beta},\mu)$ as long as it has some bargaining power ($\mu > 0$). Since inexperienced intermediaries earn exactly the outside option, we denote the additional expected profits earned by the experienced intermediary by $v(\mu)$. Second, the firm obtains cheaper financing, and the lead share I_m falls as long as the firm has some bargaining power ($\mu < 1$). Lending relationships thus make bank loans cheaper and more liquid, in that a larger share can be sold to uninformed investors at lower rates. Moreover, experienced intermediaries are more likely to be chosen as lead arrangers by the firm because they lend at lower rates.

We now turn to the first period. Here, all intermediaries face the monitoring cost c_H. The firm's problem is the same as above. Intermediaries instead take into account that being experienced tomorrow has the promise of additional rents $v(\mu)$. Rather than fully analyzing the firm's choice of intermediary in the second period, we simply parameterize the intermediary's probability of being the firm's monitor tomorrow *conditional* on monitoring the firm today by $\alpha \in [0,1]$. This parameter can reflect concerns outside the scope of the model, such as that the firm may choose a different lender due to exogenous shocks, will have no need for financing tomorrow, that the intermediary will be unable to lend tomorrow, or that the lender's expertise is not a good match for the firm's needs tomorrow. Hence, the intermediary prefers to monitor if

$$R_m \geq \frac{c_H}{\Delta p} - \alpha v(\mu).$$

To ensure monitoring incentives, the intermediary must therefore contribute at least $I_m^{**}(\beta,\mu) = \frac{p_H\left(\frac{c_H}{\Delta p} - \alpha v(\mu)\right)}{\beta}$. We have already shown that $v(\mu) > 0$ as long as the intermediary has some

bargaining power ($\mu > 0$). As a result, the required share of informed capital I_m is lower as long as there is a chance that the lender is chosen as an intermediary in the future ($\alpha > 0$). The promise of future rents thus relaxes financial constraints for the firm *today*, and simultaneously makes bank loans more liquid by reducing the required lead share.

2.2 Empirical Predictions

To derive empirical predictions from our model, we make two parametric assumptions. First, we assume that the bargaining-power parameter is strictly greater than zero and smaller than one ($\mu \in (0, 1)$), so that both firm and informed lender reap some of the surplus generated by repeated interactions. Second, we assume the probability of repeat interaction α is increasing in bank scope since banks of wide scope can provide a wider range of financial services. Therefore, banks of wide scope expect to monitor the firm as an experienced intermediary more frequently than a bank of narrow scope. Finally, we identify the required share of informed capital I_m as the theoretical counterpart of the empirical lead share, since it represents precisely the stake of the monitor bank. The model then generates the following predictions:

Empirical Prediction 1 (Lead-arranger probability) *Banks of wide scope are more likely to be chosen as lead arrangers.*

Empirical Prediction 2 (Lead shares) *Banks of wide scope receive smaller loan shares conditional on becoming lead arrangers.*

We also use the model to generate predictions for the cross section of firms. To do so, we assume that the cost advantage for experienced monitors is small when the borrower firm is more volatile, because previous monitoring experience is less likely to be useful for monitoring today when the firm's condition changes frequently. Formally, we thus say that $c_H - c_L$ is small for more volatile firms. Consequently, the reduction in lead shares resulting

from monitoring experience $I_m^* - I_m^0$ is also small. This generates the following empirical prediction:

Empirical Prediction 3 (Comparative statics) *The reduction in lead shares among banks of wide scope is less pronounced for more volatile firms.*

3 Empirical Strategy and Data

We next discuss our identification strategy based on the bank-scope deregulation following the stepwise repeal of the Glass-Steagall Act, as already used in Neuhann and Saidi (2016). Then, we will describe the data on syndicated loans and our sample selection.

3.1 Identification Strategy

An important prerequisite for estimating the impact of bank scope on syndicate structures is a setting that provides variation in bank scope. In Neuhann and Saidi (2016), we argue that the stepwise repeal of the Glass-Steagall Act constitutes such a setting. The Glass-Steagall Act of 1933 imposed a separation of commercial banking (deposit taking and lending) and investment banking (especially underwriting of corporate securities).

Starting April 30, 1987, commercial banks were allowed to become universal banks, and generate up to 5% of their gross revenues from underwriting and dealing in securities *other than* corporate debt and equity. The first major step of the repeal took place in January and September 1989, which is when commercial banks could generate a higher proportion (10% in 1989, which increased to 25% in 1996) of their revenues through underwriting activities, *including* underwriting of corporate debt and equity. Commercial banks became universal banks typically by opening so-called Section 20 subsidiaries for these purposes. Another possibility was to acquire an investment bank.

While this first step towards universal banking led to an increase in bank size by allowing banks to engage in both commercial and investment banking, they did so with firewalls

in place separating the two activities. Some of the informational and financial firewalls within bank-holding companies were, however, abolished by the Federal Reserve Board in a second step on August 1, 1996. The elimination of these firewalls between securities and commercial-bank divisions enabled universal banks to cross-sell loans and non-loan products, which used to be severely restricted, not to say forbidden, under the Federal Reserve Act (Sections 23A and B). Furthermore, the removal of informational firewalls allowed for the possibility of sharing non-public customer information between commercial banking and securities divisions.

We wish to test whether banks of wide scope are more likely to be chosen as lead arrangers and whether, conditional on becoming lead arrangers, they retain smaller shares of loans. In our model, the underlying mechanism is based on the idea that universal banks are more experienced intermediaries because they tend to have deeper bank-firm relationships. Empirically, this is operationalized through cross-selling loans and non-loan products. Thus, we hypothesize that universal, rather than commercial or investment, banks are more likely to become lead arrangers, and retain smaller shares of loans when their ability to enter deeper bank-firm relationships is strengthened.

In an attempt to match this interpretation of our model, we make use of the 1996 deregulation to capture varying propensities to cross-sell. As shown in Neuhann and Saidi (2016), the proportion of cross-sold loans increased significantly for universal banks, rather than investment banks, after the 1996 deregulation. We therefore employ a similar difference-in-differences strategy around August 1, 1996 for treated universal banks vs. other banks that were unaffected in their scope of banking activities.

The validity of our identification argument rests on two key assumptions. First, the timing of the 1996 deregulation must have been unexpected. This assumption is affirmed by the fact that the banking industry had already proposed the elimination of firewalls in 1991, but was rejected by the United States House Committee on Financial Services. Hence, it is unlikely that banks and firms were anticipating the deregulatory policy before 1996. Second, we assume that universal-bank and non-universal-bank loan shares followed parallel trends

prior to the deregulation, for which we have presented some evidence in Figure 1.

We next turn to the empirical implementation. Each syndicated loan is a package that consists of one or multiple facilities which, in turn, consist of loan shares provided by one or multiple syndicate lenders. As we are interested in each individual bank's likelihood to become lead arranger, our level of observation is the package-bank level. That is, for each syndicated loan, we include multiple observations per package, and combine all facilities within each package into one observation per (participating or lead) bank. This allows us to include package-level fixed effects as well. We estimate the following regression specification:

$$\textit{Lead arranger}_{ijkt} = \beta_1 UB_{jt} + \beta_2 UB_{jt} \times After(\text{Aug. 1, 1996})_t + \lambda_j + \psi_k + \epsilon_{ijkt}, \quad (1)$$

where $\textit{Lead arranger}_{ijkt}$ is an indicator variable for whether bank j was a lead arranger for loan (package) k (subsuming the borrower firm i) at date t, UB_{jt} is an indicator variable for whether at the time of the loan transaction (the participating or lead) bank j was a universal bank, and $After(\text{Aug. 1, 1996})_t$ is an indicator variable for whether the loan was issued on or after August 1, 1996. λ_j denotes bank fixed effects, and ψ_k are package-level fixed effects. Standard errors are clustered at the package level.[2]

Note that we can estimate a coefficient on UB_{jt} even in the presence of bank fixed effects, because we track commercial banks that may have opted to become universal banks after their first loan transaction in the data, so that UB_{jt} varies within banks. In the presence of bank fixed effects λ_j, the difference-in-differences estimate β_2 is identified off commercial banks that became universal banks prior to the deregulation and, therefore, experienced an expansion in the scope of their activities in 1996. That is, to estimate β_1 and β_2, a given bank j needs to be observed in at least three instances: when it was still a commercial bank (captured by the bank fixed effects), after it opted to become a universal bank but before the 1996 deregulation (β_1) and, finally, as a universal bank after the 1996 deregulation (β_2).

Note that we record each bank's loan share separately within each package (loan), so we

[2] Standard errors for all our estimates are virtually invariant to clustering at the bank level.

can include package fixed effects. These fixed effects capture many relevant loan characteristics, most importantly the loan date, borrower characteristics at the time of loan issue, and general characteristics of the syndicate, e.g., the number and the actual network of syndicate lenders. Including package fixed effects also alleviates concerns that would ultimately lead to a violation of the parallel-trends assumption between universal and other banks, such as bank-firm matching based on unobserved time-varying firm-level characteristics, as the latter are invariant within a package.

This empirical setup enables us to gauge the impact of bank scope on syndicate structures. In doing so, we connect with Sufi (2007) and Ivashina (2009) who also scrutinize the distribution of shares retained by syndicate lenders. The most important advance that we attempt to make is to account for heterogeneity in bank scope among syndicate lenders, differentiating at the very least between universal and pure commercial/investment banks. This, in turn, allows us to exploit the variation in bank scope generated by the stepwise repeal of the Glass-Steagall Act. In this manner, we strengthen our causal interpretation that syndicate lenders with a wide bank scope are more likely to be chosen as lead arrangers.

When we scrutinize lead shares, we need to move from the package-bank ($ijkt$) level to the package (ikt) level, which no longer allows us to include package-level fixed effects. We translate our difference-in-differences strategy to the analysis of loan shares retained by *lead arrangers*, as reflected by the following regression specification:

$$outcome_{ikt} = \beta_1 UB_{kt} + \beta_2 UB_{kt} \times After(\text{Aug. 1, 1996})_t + \beta_3 X_{it} + \mu_k + \delta_t + \epsilon_{ikt}, \quad (2)$$

where $outcome_{ikt}$ is an outcome variable associated with loan (package) k (subsuming the borrower firm i) at date t, UB_{kt} is an indicator variable for whether at the time of the loan transaction k any one of the (participating or lead) banks was a universal bank, and $After(\text{Aug. 1, 1996})_t$ is an indicator variable for whether the loan was issued on or after August 1, 1996. X_{it} summarizes time-varying borrower characteristics, including industry-year and state-year fixed effects, μ_k denotes syndicate fixed effects that are included for *all* participating and lead banks at the package level k, and δ_t denotes month-year fixed effects.

When we move from analyzing lead-arranger probabilities for all syndicate members to explaining lead shares at the aggregate package/loan level, we face the challenge that according to our model, universal banks are picked as lead arrangers precisely because of their smaller required lead shares. Thus, we cannot condition on loan shares being granted by lead-arranger universal banks. Instead, we define UB_{kt} at the aggregate loan level, so that its indication of whether *any* bank in the syndicate was a universal bank instruments for the heightened probability that said universal bank was chosen as lead arranger following the 1996 deregulation.

As in specification (1), the omitted category consists of other types of banks, typically commercial and investment banks, that were unaffected by the 1996 deregulation.

3.2 Data Description

Before presenting the results, we briefly describe our data. As our main data source, we use syndicated loans issued by publicly listed U.S. firms from 1987 to 2008 from the DealScan database. We complement our loan data with CRSP stock prices, SDC debt- and equity-underwriting, as well as Compustat data.

We consider all completed loan transactions in the DealScan database involving publicly listed U.S. firms, with valid data on syndicate banks' lead-arranger status and/or loan shares. As described above, each loan (package) consists of multiple facilities, but multiple banks may participate in a single facility. Our observations are at the package-bank level, i.e., we build multiple observations per package, and within each package one observation per (participating or lead) bank. To calculate the share retained by each bank, we first determine the unique share retained by each bank in every role it has in the syndicate by dropping duplicate observations. We then sum up all loan shares for each bank within a package, and keep only one observation per bank, which includes the information whether the respective bank (also) acted as a lead arranger in the syndicate.

For the purpose of identifying all universal banks, we make use of our hand-collected

data on all universal banks and their establishment dates in the DealScan database. For the above-described sample, we yield 43 universal banks, 40 of which overlap with the universal banks identified in Neuhann and Saidi (2016).[3] We provide a list of these universal banks alongside a differentiation by their date and mode of establishment (opening of a Section 20 subsidiary or bank-scope-expanding M&A) in Table 1.

When we consider cross-selling of loans and corporate-securities-underwriting services (mostly debt and equity underwriting), we merge DealScan with SDC underwriting data. This enables us to determine whether a loan share was accompanied by debt or equity issued through the same universal or investment bank as the one in the loan syndicate.

To characterize the ex-ante riskiness of a firm, we use six-year, leading up to the year of the loan issue, stock-return volatilities, which are calculated using monthly CRSP data, and idiosyncratic volatilities estimated from the Fama and French (1993) three-factor model.

Summary statistics. In the first panel of Table 2, we present summary statistics for our regression sample with data available for all variables used in the regressions, except for the loan share. The level of observation is the package-bank level. We learn that 37% of the package-bank observations are associated with universal banks, compared to a small group of investment banks that makes for 1.7% of all observations. However, both types of banks are similarly likely to be observed to cross-sell their loan shares with any debt- or equity-underwriting products: 34% for universal banks and 38% for investment banks.

In line with this paper's motivation, the participation of institutional investors in the syndicated-loans market, 47% of the observations are associated with institutional investors. We define institutional, or non-bank, investors as syndicated-loan participants or lead arrangers that fall in any one of the following groups in DealScan: "Corporation," "Distressed (Vulture) Fund," "Finance Company," "Inst. Invest. CDO," "Inst. Invest. Hedge Fd," "Inst. Invest. Insur. Co.," "Inst. Invest. Other," "Inst. Invest. Prime Fd," "Insurance Company," "Law Firm," "Mutual Fund," "Pension Fund," and "Trust Company."

[3] This is because we do not focus exclusively on lead arrangers in this paper. Furthermore, three additional universal banks appear in this data set, namely Republic New York (which was eventually acquired by HSBC), Swiss Bank Corp, and Union Bank of Switzerland (both of which merged in 1998).

In the second panel of Table 2, we move to the package level, requiring data on loan shares retained by lead arrangers and the distribution of loan shares within each syndicated loan. This restriction yields 11,718 loans (packages), and implies that we have approximately 6.6 (participating or lead) banks per loan.[4] The vast majority of loans has only one lead arranger, namely 9,892 out of 11,718. We also consider the concentration of loan shares, as captured by a Herfindahl Index, which is defined irrespective of the number of lead arrangers.

Our sample drops to 7,123 when we require six years of stock-return data up to the year of loan issue for the calculation of volatilities. Overall, roughly two-thirds of all loans involved at least one universal bank as participant or lead arranger.

4 Results

We now turn to our estimation results. We proceed in three steps. First, we show that following the 1996 deregulation, universal banks were more likely to become lead arrangers of loan syndicates. Second, we move from the within-syndicate analysis to the between-loan level, and investigate the impact of syndicates with and without universal banks on reduced lead shares. Finally, we show that universal-banking deregulation allowed non-bank institutional investors to participate more prominently in the syndicated-loans market.

4.1 Impact of Bank-scope Deregulation on Universal Banks' Lead-arranger Probability

We start with the regression results for lead-arranger choices. In Table 3, we estimate regression specification (1) with an indicator for lead-arranger status as dependent variable. The difference-in-differences estimate is given by the coefficient on $UB_{jt} \times After(\text{Aug. 1, 1996})_t$.

Across all columns of Table 3, this estimate is positive and significant at the 1% level.

[4] This reflects the fact that in DealScan, banks' lead-arranger status is more broadly available than loan shares: our sample drops from 167,370 to $77,339 = 11,718 \times 6.6$ observations when we condition on the availability of loan shares.

Overall, we find that universal banks established before 1996 were at least eleven percentage points more likely to become lead arrangers following the 1996 deregulation than were commercial and other banks whose scope of banking activities was unaffected. In particular, we include the term $Institutional\ investor_j \times After(\text{Aug. 1, 1996})_t$, which is significantly negative, implying that the gap between universal banks and non-bank institutional investors is even larger than the gap between universal banks and the omitted category of commercial and investment banks.

While always including bank fixed effects, we run this specification without package fixed effects in the first three columns, and include them in the last two columns. In the absence of package fixed effects, we include, instead, month-year fixed effects as well as industry-year fixed effects (in the first and second column). In the third column, we also control for state-year fixed effects. In this manner, we ensure that our treatment effect, which is based on time variation at the bank level, is not contaminated by other shocks to credit supply around the 1996 deregulation. In particular, a key concern in this period is the state-level deregulation of bank branching. Our estimates remain robust.

Finally, in the last two columns, the inclusion of package fixed effects enables us to control for unobserved heterogeneity at the package level, ranging from loan characteristics (including its date of origination) to borrower characteristics at the time of the loan issue. This further increases our difference-in-differences estimate. In the last column, we also include the term $IB_j \times After(\text{Aug. 1, 1996})_t$ to separately test whether universal banks were more likely to become lead arrangers than investment banks after August 1, 1996. This is indeed the case, as the difference of 9.5 percentage points between the two respective interaction effects is significant at the 1% level.

Note that our empirical strategy effectively excludes the possibility that universal banks' heightened likelihood to become lead arrangers is driven by the mere fact that there have been more of them over time. First, we control for a host of time-varying aggregate factors, through fixed effects at the month-year, industry-year, and state-year levels. Second, in the presence of bank fixed effects, the difference-in-differences estimate is identified using

the lending behavior of universal banks that already existed before the 1996 deregulation. Third, the inclusion of package fixed effects controls for all sources of variation at the loan level, including the date of the loan issue and any networks of banks that repeatedly join forces in the syndication process.

Our findings remain unaltered when we limit the sample to loans for which we have additional data on loan shares, rather than just the lead-arranger status of individual syndicate lenders, in Table 4. This indicates that even after conditioning on the availability of loan shares, investment banks were only somewhat more likely to become lead arrangers after 1996 than were commercial banks. However, universal banks were significantly more likely to become lead arrangers after the 1996 deregulation than both commercial and investment banks, which enabled them to enter into deeper bank-firm relationships. We view all of this as evidence in favor of our Empirical Prediction 1.

The inclusion of package fixed effects in the last two columns of Tables 3 and 4 implies that for the identification of our treatment effect, we compare universal banks' to all other loan shares within a package, but the differential impact of the 1996 deregulation only across packages. This is because our treatment is defined at the bank-month-year level. We argue that the 1996 deregulation spurred cross-selling by universal banks, which, in turn, enabled them to become lead arrangers at lower loan shares. This opens up an alternative identification approach, where we compare – within packages – cross-sold and non-cross-sold loan shares.

As only universal and investment banks could cross-sell loans and underwriting services, we limit the sample to loan shares held by universal and investment banks in Table 5. This implies that we drop commercial banks from the sample, including those that eventually became universal banks as long as they were not yet universal banks. The coefficient on $\textit{Cross-selling}_{ijt}$ therefore estimates the effect of cross-selling on being chosen as lead arranger, while the interaction with UB_j (which is now time-invariant due to the sample selection) captures the differential effect of cross-selling by universal as compared to investment banks.

As the incidence of cross-selling is not exogenous, we have previously used the 1996

deregulation as a source of variation. In Table 5, we lack suck variation, but can still include package fixed effects, which – as argued before – control for unobserved time-varying firm-level characteristics. In addition, package fixed effects control for any shocks to firm-level demand, e.g., firms' concurrent demand for loans and non-loan products.

Before we include package fixed effects, we start by including only bank and month-year fixed effects in the first column. Cross-selling universal banks were 41.5 percentage points more likely to be chosen as lead arrangers than cross-selling investment banks. In the second column, this (rather large) estimate is robust to including industry-year, state-year, and bank-year fixed effects. Bank-year fixed effects control for bank-level supply shocks, and can now be included (unlike in Tables 3 and 4) because cross-selling is defined at the bank-firm-year, and not at the bank-year, level (unlike the identifying variation for our difference-in-differences strategy). The estimated effect is larger in size than our previous difference-in-differences estimate of 16.7 percentage points in the last column of Table 3 because the 1996 deregulation increased only the *capacity for cross-selling*.

In this setting, the coefficient on $UB_j \times Cross\text{-}selling_{ijt}$ may capture some unobserved dimension of the specific bank-firm relationship in question, in particular the possibility of reverse causality. To partially control for this, we add bank-firm fixed effects in the third column, so that we identify the effect off firms that contracted with multiple banks that did not always cross-sell their loan shares to the respective firm. After doing this, cross-sold universal-bank loan shares are associated with a 20.6 percentage points higher lead-arranger likelihood than cross-sold investment-bank loan shares.

In the last column, we add package fixed effects, which further control for unobserved time-varying heterogeneity of firms that may govern banks' propensity to cross-sell to them. The coefficient on $UB_j \times Cross\text{-}selling_{ijt}$ remains significant at the 5% level, and at 14.3 percentage points is fairly close to the difference in the difference-in-differences estimates for universal vs. investment banks in the last column of Table 3. These results lend additional support to our hypothesis that cross-selling universal banks are more likely to become lead arrangers because they are willing to monitor even when retaining lower loan shares.

4.2 Impact of Universal Banking on Reduced Lead Shares

To test our Empirical Prediction 2, we move to the package level, and analyze the average loan share retained by lead arrangers. As the outcome variable of interest is defined at the package level, this does not allow us to include package-level fixed effects, which in our previous tests absorbed variation at the borrower-firm and loan-specific levels.[5]

Our model predicts the treatment effect of universal-banking deregulation on lead shares to be negative. To investigate this, in Table 6, we run specification (2) with syndicate and month-year fixed effects in the first column. At the package level, syndicate fixed effects constitute fixed effects for each participating or lead bank in the syndicate. We find that while syndicates comprising at least one universal bank generally had – 22.4 percentage points – smaller average lead shares than pure commercial-bank syndicates, the gap widened significantly by another 5.1 percentage points after August 1, 1996. These estimates mirror the developments in Figure 1.

Our findings are robust to including borrower-firm-level explanatory variables and industry-year fixed effects in the second column, and state-year fixed effects in the third column. Finally, in the fourth column, we limit the sample to syndicates with only one lead arranger. We do this to safeguard that our results in the first three columns are not driven by a potential increase in the number of lead arrangers – and, thus, in the denominator of our dependent variable – in universal-bank syndicates after the 1996 deregulation. 84.4% of the loans in our sample had only one lead arranger, and when limiting our sample to the latter group, the difference-in-differences estimate remains very similar to that in the third column.

In Table 7, we repeat the same estimations as in Table 6, but add an interaction effect for syndicates with any universal banks following the September-1989 deregulation, which was when universal banks were first allowed to generate up to 10% of their revenues from

[5] This weakness is in part exacerbated by both the imperfect availability of loan-share data in DealScan. This criticism applies to studies using DealScan data in general. An alternative data source is the Shared National Credit program (SNC), which provides more comprehensive data on loan shares. As pointed out by Bord and Santos (2012), lead shares are relatively constant for credit lines, rather than term loans. In an attempt to cope with the criticism of not having loan-share data over the run-time of syndicated loans, we re-ran all regressions on the subset of credit lines, with robust results throughout.

underwriting corporate debt and equity (see Section 3.1). The respective interaction effect is statistically insignificant and close to zero throughout all columns.

This serves as evidence for the absence of any differential pre-trends between syndicated with and without universal banks. What is more, both deregulations in 1989 and 1996 were associated with higher revenues from marketing, but not necessarily cross-selling, loans and non-loan products. Conversely, only the 1996 deregulation allowed universal banks to proactively cross-sell these products and use non-public information across commercial-bank and securities divisions. Therefore, the evidence in this table lends further support to our conjecture that universal banks require smaller lead shares because they benefit from economies of scope in monitoring.

Table 8 shows that all findings from Table 6 remain to hold when we use as dependent variable our loan-share concentration measure, which is a Herfindahl Index between 0 and 1. Since reduced loan shares allow for better diversification across the syndicate, the loan-share concentration should drop as well. In line with Empirical Prediction 2, we find the loan-share concentration to decrease in universal-bank syndicates following the 1996 deregulation. Furthermore, the difference-in-differences estimates are quantitatively very similar to those in Table 6.

In Table 9, we also shed light on the effect on average *participant* shares. This is to fathom whether the number of participants has increased in proportion to the now-higher residual share due to the reduction in lead shares. To this end, we re-run the same set of regressions, and use as dependent variable the average participant share. Our model does not predict any changes in average participant shares due to bank-scope deregulation, which should translate into a zero difference-in-differences estimate. This is confirmed in Table 9, at least after including industry-year fixed effects in the second column. We infer from this that the universal-bank-induced reduction in lead shares after the 1996 deregulation has led to the inclusion of additional participants – most importantly, as we will show later, institutional investors – with constant average participant shares.

Next, we consider the comparative statics of lead shares with respect to firm characteris-

tics. Empirical Prediction 3 states that the decrease in lead shares should be less pronounced for risky firms. The rationale is that past monitoring effort is less useful in assessing the firm today if firm-level volatility is high. This mechanism dampens the treatment effect of universal banking.

To test this, we split our sample into loans that were associated with a measure of pre-loan firm-level riskiness in the bottom vs. top 50% of the distribution in our loans sample, and re-run the regression from the second column of Table 6 on these subsamples. In the first two columns of Table 10, we use as a measure of pre-loan firm-level riskiness the borrower firm's six-year stock-return volatility, $\sigma^{t-5,t}_{return,i}$. In line with our prediction, the negative treatment is significant only in the sample of low-risk firms, and economically so at negative 8.2 percentage points (first column). This is in stark contrast to the non-existing (and even positive) treatment effect for high-risk firms (in the second column). In the last two columns of Table 10, we show that these estimates hold up to using splits based on idiosyncratic volatility, calculated over the six years leading up to the year of the loan issue, $\sigma^{t-5,t}_{idiosyncratic,i}$.

Altogether, our findings yield support for all of the model's predictions. In particular, we have shown that bank scope is a key determinant of lead-arranger status and lead shares, and that the comparative statics in the data line up with those in the model.

4.3 Linking Universal Banking to the Rise of Institutional-investor Participation

Having shown that following the 1996 deregulation, universal banks were more likely to become lead arrangers of syndicated loans while retaining smaller lead shares, we now link the resulting occurrence of a larger residual share in loan syndicates with universal banks to increasing participant shares of institutional investors.

To show this, in Table 11 we re-run all regressions from Table 6, and use as dependent variable the total participant share of a given loan held by institutional investors. Mirroring our findings in the bottom panel of Figure 1, loan syndicates with universal-bank members

were associated with greater participation of institutional investors, and particularly so after the 1996 deregulation. Interestingly, the difference-in-differences estimates are fairly similar to the absolute reduction in lead shares estimated in Table 6: syndicates with universal banks had at least three percentage points lower lead shares, while in these syndicates, institutional investors held at least three percentage points larger participant shares.

As mentioned above, a potential weakness of our analysis at the package level is that we cannot control for shocks to borrower-level demand or any confounding shocks to lender-level supply, other than the 1996 deregulation. This is a particularly valid identification concern when it comes to explaining the increase in institutional-investor participation. Our difference-in-differences estimates leave open whether institutional investors had constant demand for participating in syndicated loans that could be served only after the deregulation of universal banks in 1996, or whether institutional investors wanted to increase their participation in this market, and chose to do so by entering syndication agreements with universal banks after the 1996 deregulation.

To at least partly control for any trends in institutional investors' appetite for entering the syndicated-loans market, we additionally control for *Total institutional holdings*$_{kt}$, which is the aggregate volume of institutional holdings (as participants or lead arrangers) in the market for syndicated loans excluding k in month-year t, in the first column of Table 12. In comparison to the third column of Table 11, this – if anything – increases our difference-in-differences estimate from 3 to 3.8 percentage points.

In the second and third column of Table 12, we re-run the specification from the third column of Table 11, and split up the dependent variable into the total participant share held by institutional investors that were finance companies (second column), namely those classified as "Distressed (Vulture) Fund," "Finance Company," or "Inst. Invest. Hedge Fd" in DealScan, as opposed to the remaining institutional investors (third column).

While the difference-in-differences estimates are largely invariant across these two groups, the coefficients on UB_{kt} are not. The latter reflect to what extent syndicates with universal banks had greater capacity for accommodating institutional-investor participation prior to

the 1996 deregulation. This capacity is halved for institutional investors that were not part of the above-listed set of finance-companies (see third column). Therefore, the additional effect of the 1996 deregulation had a greater *relative* effect on said institutional investors' participant shares. This chimes with the rationale of our hypothesis insofar as non-finance companies are most likely among the least informed entities in the market for syndicated loans, so they should profit the most from universal banks' information production, which was spurred by the 1996 deregulation.

Our primary interest in institutional-investor shares in syndicated loans stems from the idea that these loan shares are more likely to be securitized or sold off in the secondary loan market. As shown by, among others, Ivashina and Sun (2011a) and Nadauld and Weisbach (2012), term loans B, C, and higher are often securitized or sold to institutional investors. Against this background, in the last column of Table 12, we replace our dependent variable by the sum of total institutional-investor participant shares *and* loan shares associated with term loans B to H, which may or may not (yet) be held by institutional investors. Doing so results in a slightly larger difference-in-differences estimate compared to our estimate in the third column of Table 11.

Thus far, we have focused on institutional-investor *participant* shares, and we argue that increased institutional-investor participation was made possible by universal banks requiring smaller lead shares for their monitoring efforts, freeing up a greater residual share for participation by institutional investors.

We also show that institutional investors primarily entered the syndicated-loans market as participants, rather than as lead arrangers. This can be inferred from our findings in Tables 3 and 4: institutional investors were significantly less likely to become lead arrangers than universal banks (and other banks) following the 1996 deregulation, which affected solely the scope of banking activities engaged in by universal banks. Therefore, we can conclude that syndicates with universal banks were associated only with higher participant, but not lead, shares retained by institutional investors. That is, increased institutional-investor involvement in loan syndicates was confined to loan shares without monitoring responsibility,

which was assumed by universal banks.

Finally, to further buttress that institutional-investor participation increased due to reduced lead shares of universal banks following the 1996 deregulation, we confirm Empirical Prediction 3 for participant shares of institutional investors. To do this, we re-run the regressions from Table 10, and use the loan share held by institutional investors as dependent variable.

The results are in Table 13, and confirm our prediction for institutional-investor participant shares, which have increased significantly in syndicates with universal banks after 1996 only for low-risk firms (see first and third column). This comparative-static result is of particular interest because it suggests that the increased participation of institutional investors in syndicates with universal banks was not due to the entry of new investors with higher risk appetite (as suggested by Nandy and Shao (2010)), but rather operates through the enhanced monitoring experience of universal banks.

5 Conclusion

In this paper, we investigate how the deregulation of bank scope affected the participation of institutional investors who do not have monitoring expertise in the market for syndicated loans. We first show that universal banks retain substantially smaller lead shares than standalone commercial or investment banks, and are more likely to be chosen as lead arrangers. We then document that in syndicates with universal banks, a larger fraction of the loan is sold to such institutional investors. The mechanism we propose is that universal banks have broader lending relationships with borrower firms. These relationships sustain bank-monitoring incentives even when the lead arranger retains only a small fraction of a given loan. We thus argue that understanding monitoring incentives may require taking a broader view of bank-firm interactions. Our results indicate that bank deregulation contributed to the increased liquidity of previously illiquid bank loans, and fostered the increased participation of institutional investors in capital markets prior to 2008.

Previous work by Ivashina and Sun (2011a), Benmelech, Dlugosz, and Ivashina (2012), and Nadauld and Weisbach (2012) has shown that loan syndication is an important step towards the eventual securitization of bank loans. Our findings therefore suggest that bank deregulation partly precipitated the rise of securitization that many observers view as an important cause of the 2008 financial crisis (Gorton and Metrick (2013)). Against this background, our paper highlights how interactions between different types of financial institutions are shaped by financial regulation.

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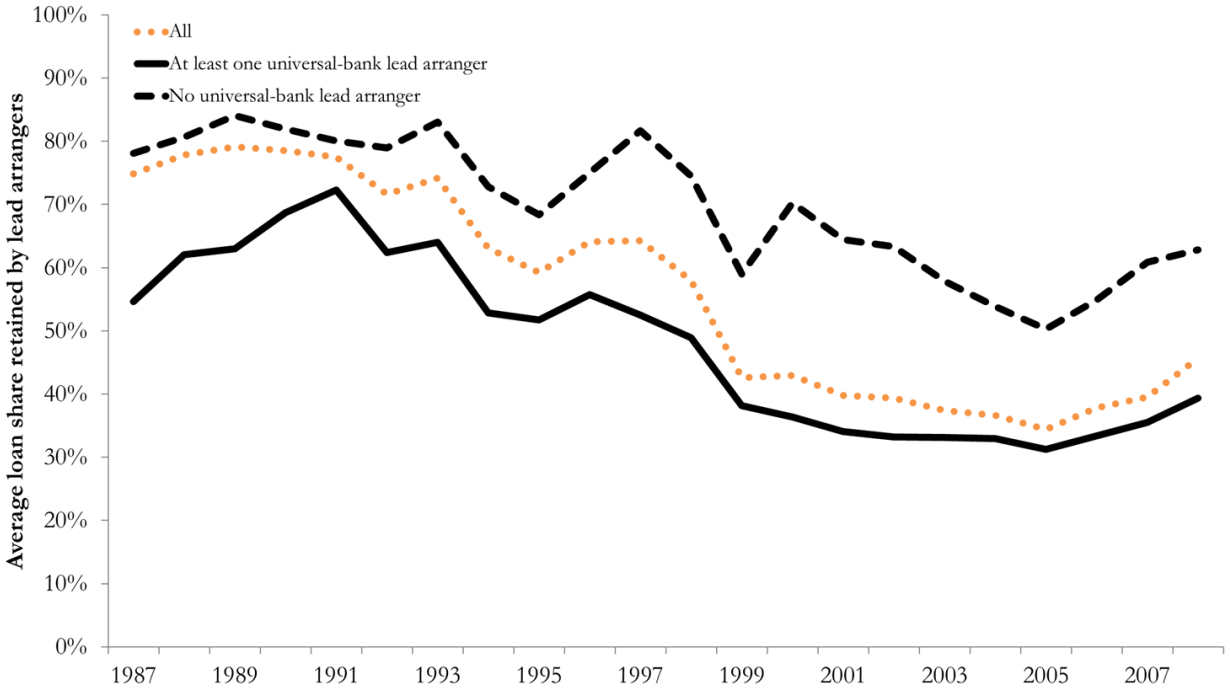
6 Figures



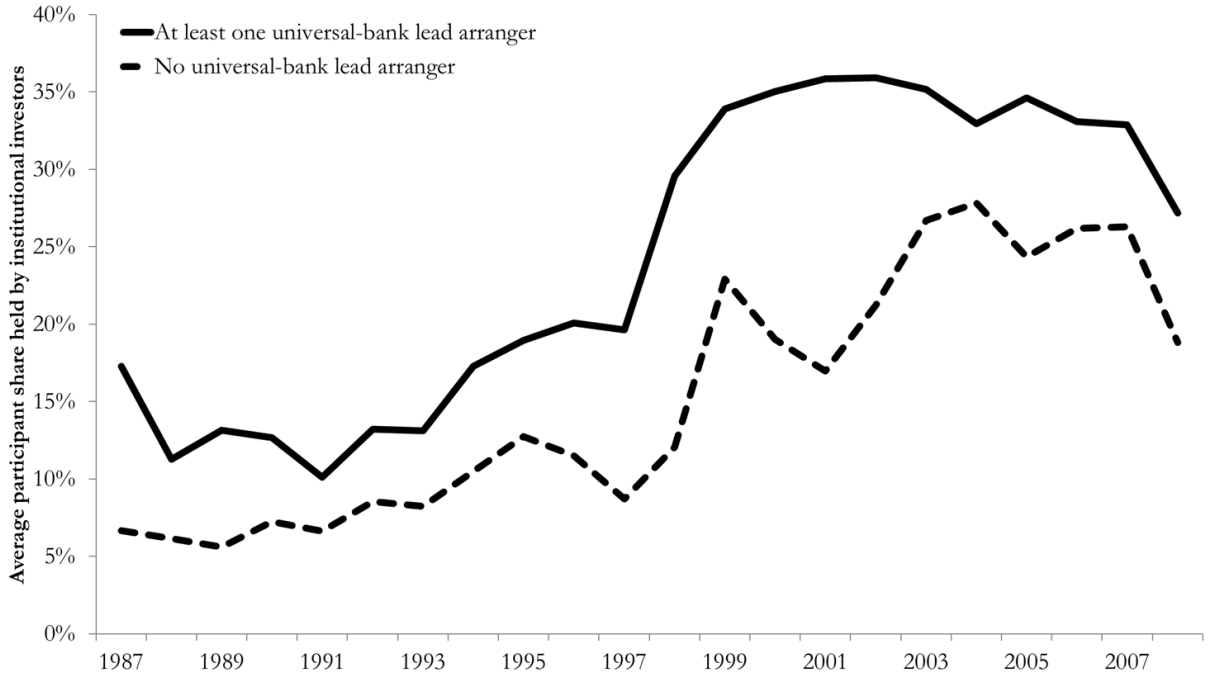


Figure 1: **Lead Shares and Institutional-investor Participant Shares in Syndicates: Universal-bank vs. Other Lead Arrangers (1987-2008).** Source: own analysis based on DealScan loan data.

7 Tables

Table 1: **Overview of Universal Banks**

Section 20	M&A
Established before August 1, 1996	
BankBoston (later acquired by Fleet)	Credit Suisse (First Boston)
Bankers Trust (later acquired by Bank of America)	Deutsche Bank USA
Bank of America	Equitable (later acquired by SunTrust)
Bank One (later acquired by J.P. Morgan)	HSBC Bank USA
Barnett Bank (later acquired by NationsBank)	Travelers Group*
Chase Manhattan (later acquired by J.P. Morgan)	
Chemical Bank (later acquired by Chase Manhattan)	
Citicorp*	
Dauphin Deposit Corp.	
First Chicago NBD	
First Union	
Fleet (later acquired by Bank of America)	
Huntington Bancshares	
J.P. Morgan	
KeyBank	
Marine Midland Bank (later acquired by HSBC Bank USA)	
Mellon (later acquired by BNY)	
National City (later acquired by PNC)	
National Westminster Bank USA (later acquired by Fleet)	
NationsBank (later acquired by Bank of America)	
Norstar (later acquired by Fleet)	
Norwest (later acquired by Wells Fargo)	
PNC	
Republic New York (later acquired by HSBC Bank USA)	
SouthTrust (later acquired by Wachovia/First Union)	
SunTrust	
Union Bank of Switzerland	
Established on or after August 1, 1996	
BB&T	Citigroup*
BNY	Swiss Bank Corp (later acquired by Union Bank of Switzerland)
Commerce Bancshares	Wells Fargo
CoreStates/Philadelphia National Bank (later acquired by First Union)	
Crestar Bank	
First Tennessee	
U.S. Bancorp	
Wachovia (first acquired by First Union and later by Wells Fargo	

* Citigroup emerged as a result of the merger of Travelers Group and Citicorp on October 8, 1998. Before, Travelers Group became a universal bank through M&A (e.g., with Smith Barney), and Citicorp had registered a Section 20 subsidiary. Given this merger of equals, we do not treat either one as the surviving entity and, instead, label Citigroup as a separate universal bank established through M&A in 1998.

Table 2: **Summary Statistics**

Package-bank level	Mean	Std. dev.	Min	Max	N
Lead arranger $\in \{0,1\}$	0.195	0.396	0	1	167,370
UB $\in \{0,1\}$	0.366	0.482	0	1	167,370
Institutional investor $\in \{0,1\}$	0.472	0.499	0	1	167,370
IB $\in \{0,1\}$	0.017	0.129	0	1	167,370
Cross-selling $\in \{0,1\}$ if UB $= 1$	0.336	0.472	0	1	61,244
Cross-selling $\in \{0,1\}$ if IB $= 1$	0.383	0.486	0	1	2,849
Package/loan level	Mean	Std. dev.	Min	Max	N
No. of unique package-bank observations	6.617	8.550	1	176	11,718
Average lead share $\in [0,1]$	0.554	0.379	0.002	1	11,718
Loan share of single lead arranger $\in [0,1]$	0.617	0.371	0.012	1	9,892
No. of lead arrangers	1.229	0.903	1	36	11,718
Concentration of loan shares $\in [0,1]$	0.517	0.397	0.013	1	11,718
Average participant share $\in [0,1]$	0.093	0.122	0.000	0.920	11,718
No. of participants	5.388	8.252	0	175	11,718
Share of institutional participants	0.190	0.222	0.000	1	11,718
Share of inst. participants + term loan B-H	0.193	0.227	0.000	1	11,718
UB $\in \{0,1\}$	0.669	0.471	0	1	11,718
Sales at close in 2010 $	2.934	10.222	0.000	377.110	11,718
No. of employees in thousands	10.016	29.439	0.001	428.001	11,718
$\sigma^{t-5,t}_{return,i}$	0.428	0.207	0.114	3.886	7,123
$\sigma^{t-5,t}_{idiosyncratic,i}$	0.385	0.202	0.104	3.928	7,123

Notes: The sample consists of all loans successfully issued by publicly listed U.S. firms. In the first panel, observations are at the package-bank level, i.e., each package comprises multiple observations, but only one observation per (participating or lead) bank. In the second panel, observations are at the (aggregate) package/loan level. In general, summary statistics are shown for our regression sample with data available on participating and lead-arranger banks. Only in the second panel, the regression sample is furthermore conditioned on the availability of data for $\textit{Loan share}_{ijkt}$, which is the share (between 0 and 1) of the loan k granted to firm i retained by bank j at date t. $\textit{Lead arranger}_{ijkt}$ is an indicator variable for whether bank j served as a lead arranger for loan k granted to firm i at date t. UB_{jt} is an indicator variable for whether at the time t of the loan transaction (the participating or lead) bank j was a universal bank. $\textit{Institutional investor}_j$ and IB_j are indicator variables for whether (the participating or lead) bank j was an institutional investor or investment bank, respectively. $\textit{Cross-selling}_{ijt}$ is an indicator variable for whether the universal or investment bank's loan share granted to firm i in year t was associated with a cross-sold underwriting product by the same bank j anytime from the beginning of year $t-2$ to the end of year $t+2$. At the package level, $\textit{Concentration of loan shares}_{ikt}$ is a Herfindahl index which is equal to the sum of the squared loan shares of all (participating or lead) banks in the syndicate, and varies from 0 to 1 (maximal concentration). UB_{kt} is an indicator variable for whether at the time t of the loan transaction k any one of the (participating or lead) banks was a universal bank. $\sigma^{t-5,t}_{return,i}$ is the six-year standard deviation of borrower firm i's stock return from year $t-5$ to year t, and is expressed in annualized terms. $\sigma^{t-5,t}_{idiosyncratic,i}$ is firm i's corresponding idiosyncratic volatility, estimated from the Fama and French (1993) three-factor model using monthly CRSP data, and is also expressed in annualized terms.

Table 3: **Impact of Universal-bank Financing on Lead-arranger Status – Package-bank Level**

	Lead arranger				
UB	0.033***	0.044***	0.045***	0.059***	0.058***
	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
UB × After(Aug. 1, 1996)	0.117***	0.114***	0.115***	0.165***	0.167***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
Inst. Inv. × After(Aug. 1, 1996)	-0.086***	-0.078***	-0.076***	-0.088***	-0.088***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
IB × After(Aug. 1, 1996)					0.072***
					(0.023)
Log of sales at close		-0.035***	-0.036***		
		(0.001)	(0.001)		
Log of no. employees		0.001	0.001		
		(0.001)	(0.001)		
Bank FE	Y	Y	Y	Y	Y
Month-year FE	Y	Y	Y	N	N
Industry-year FE	N	Y	Y	N	N
State-year FE	N	N	Y	N	N
Package FE	N	N	N	Y	Y
N	165,799	165,763	165,721	156,935	156,935

Notes: The sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the package-bank level, i.e., each package comprises multiple observations, but only one observation per (participating or lead) bank. All singletons are dropped from the total number of observations N. The dependent variable is an indicator variable for whether bank j served as a lead arranger for loan k granted to firm i at date t. UB_{jt} is an indicator variable for whether at the time t of the loan transaction (the participating or lead) bank j was a universal bank. *Institutional investor*$_j$ and IB_j are indicator variables for whether (the participating or lead) bank j was an institutional investor or investment bank, respectively. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. State-year fixed effects are based on the location of firm i's headquarters in year t. Robust standard errors (clustered at the package level) are in parentheses.

Table 4: **Impact of Universal-bank Financing on Lead-arranger Status: Robustness – Package-bank Level**

	Lead arranger				
UB	-0.023**	-0.008	-0.006	0.024**	0.024**
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
UB × After(Aug. 1, 1996)	0.095***	0.092***	0.091***	0.134***	0.134***
	(0.012)	(0.011)	(0.011)	(0.013)	(0.013)
Inst. Inv. × After(Aug. 1, 1996)	-0.098***	-0.083***	-0.081***	-0.084***	-0.085***
	(0.011)	(0.010)	(0.010)	(0.012)	(0.012)
IB × After(Aug. 1, 1996)					0.054**
					(0.027)
Log of sales at close		-0.046***	-0.049***		
		(0.002)	(0.002)		
Log of no. employees		0.002*	0.001		
		(0.001)	(0.001)		
Bank FE	Y	Y	Y	Y	Y
Month-year FE	Y	Y	Y	N	N
Industry-year FE	N	Y	Y	N	N
State-year FE	N	N	Y	N	N
Package FE	N	N	N	Y	Y
N	75,005	74,947	74,899	70,618	70,618

Notes: The sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the package-bank level, i.e., each package comprises multiple observations, but only one observation per (participating or lead) bank. Only observations with valid loan-share data are included. All singletons are dropped from the total number of observations N. The dependent variable is an indicator variable for whether bank j served as a lead arranger for loan k granted to firm i at date t. UB_{jt} is an indicator variable for whether at the time t of the loan transaction (the participating or lead) bank j was a universal bank. $Institutional\ investor_j$ and IB_j are indicator variables for whether (the participating or lead) bank j was an institutional investor or investment bank, respectively. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. State-year fixed effects are based on the location of firm i's headquarters in year t. Robust standard errors (clustered at the package level) are in parentheses.

Table 5: **Impact of Cross-selling by Universal and Investment Banks on Lead-arranger Status – Package-bank Level**

	Lead arranger			
Sample	Only universal banks and investment banks			
UB × Cross-selling	0.415***	0.438***	0.206***	0.143**
	(0.006)	(0.008)	(0.019)	(0.062)
Cross-selling	0.037***	0.035***	0.011	0.046
	(0.004)	(0.007)	(0.017)	(0.062)
Log of sales at close	-0.058***	-0.054***	-0.011	
	(0.002)	(0.002)	(0.007)	
Log of no. employees	0.009***	0.000	-0.015***	
	(0.001)	(0.002)	(0.006)	
Bank FE	Y	N	N	N
Month-year FE	Y	Y	Y	N
Industry-year FE	N	Y	Y	N
State-year FE	N	Y	Y	N
Bank-year FE	N	Y	Y	Y
Bank-firm FE	N	N	Y	Y
Package FE	N	N	N	Y
N	64,045	63,705	50,749	44,022

Notes: The sample consists of all loans with universal- and/or investment-bank involvement issued by publicly listed U.S. firms. Observations are at the package-bank level, i.e., each package may comprise multiple observations, but only one observation per (participating or lead) universal or investment bank. All singletons are dropped from the total number of observations N. The dependent variable is an indicator variable for whether bank j served as a lead arranger for loan k granted to firm i at date t. UB_j is an indicator variable for whether (the participating or lead) bank j was a universal bank, rather than an investment bank. $\textit{Cross-selling}_{ijt}$ is an indicator variable for whether the universal or investment bank's loan share granted to firm i in year t was associated with a cross-sold underwriting product by the same bank j anytime from the beginning of year $t-2$ to the end of year $t+2$. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. State-year fixed effects are based on the location of firm i's headquarters in year t. Robust standard errors (clustered at the package level) are in parentheses.

Table 6: **Impact of Universal-bank Financing on Lead Shares – Package Level**

	Average lead share			
Sample	All	All	All	One LA
UB	-0.224***	-0.204***	-0.201***	-0.182***
	(0.009)	(0.009)	(0.009)	(0.010)
UB × After(Aug. 1, 1996)	-0.051***	-0.026**	-0.027**	-0.032**
	(0.012)	(0.012)	(0.013)	(0.013)
Log of sales at close		-0.059***	-0.059***	-0.058***
		(0.003)	(0.003)	(0.003)
Log of no. employees		0.004*	0.003	0.003
		(0.002)	(0.002)	(0.003)
Syndicate FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
Industry-year FE	N	Y	Y	Y
State-year FE	N	N	Y	Y
N	11,718	11,513	11,341	9,477

Notes: In general, the sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the (aggregate) package/loan level. The sample in the fourth column is limited to loans with one lead arranger. All singletons are dropped from the total number of observations N. The dependent variable corresponds to the (average) share of the loan retained by the lead arranger(s), and is defined between 0 and 1. UB_{kt} is an indicator variable for whether at the time t of the loan transaction k any one of the (participating or lead) banks was a universal bank. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. Syndicate fixed effects indicate the inclusion of bank fixed effects for *all* participating and lead banks at the package level. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. State-year fixed effects are based on the location of firm i's headquarters in year t. Robust standard errors (clustered at the package level) are in parentheses.

Table 7: **Impact of Universal-bank Financing on Lead Shares: Placebo – Package Level**

	Average lead share			
Sample	All	All	All	One LA
UB	-0.226***	-0.205***	-0.207***	-0.177***
	(0.018)	(0.018)	(0.019)	(0.020)
UB × After(Aug. 1, 1996)	-0.052***	-0.026**	-0.029**	-0.031**
	(0.012)	(0.012)	(0.013)	(0.014)
UB × After(Sept. 1, 1989)	0.002	0.001	0.007	-0.007
	(0.019)	(0.019)	(0.020)	(0.021)
Log of sales at close		-0.059***	-0.059***	-0.058***
		(0.003)	(0.003)	(0.003)
Log of no. employees		0.004*	0.003	0.003
		(0.002)	(0.002)	(0.003)
Syndicate FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
Industry-year FE	N	Y	Y	Y
State-year FE	N	N	Y	Y
N	11,718	11,513	11,341	9,477

Notes: In general, the sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the (aggregate) package/loan level. The sample in the fourth column is limited to loans with one lead arranger. All singletons are dropped from the total number of observations N. The dependent variable corresponds to the (average) share of the loan retained by the lead arranger(s), and is defined between 0 and 1. UB_{kt} is an indicator variable for whether at the time t of the loan transaction k any one of the (participating or lead) banks was a universal bank. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. $After(\text{Sept. 1, 1989})_t$ is an indicator for whether the loan in question was issued on or after September 1, 1989. Syndicate fixed effects indicate the inclusion of bank fixed effects for *all* participating and lead banks at the package level. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. State-year fixed effects are based on the location of firm i's headquarters in year t. Robust standard errors (clustered at the package level) are in parentheses.

Table 8: **Impact of Universal-bank Financing on Concentration of Loan Shares – Package Level**

	Concentration of loan shares			
Sample	All	All	All	One LA
UB	-0.251***	-0.231***	-0.227***	-0.206***
	(0.009)	(0.010)	(0.010)	(0.011)
UB × After(Aug. 1, 1996)	-0.065***	-0.036***	-0.038***	-0.044***
	(0.012)	(0.013)	(0.014)	(0.014)
Log of sales at close		-0.056***	-0.056***	-0.057***
		(0.003)	(0.003)	(0.004)
Log of no. employees		0.002	0.002	0.002
		(0.002)	(0.002)	(0.003)
Syndicate FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
Industry-year FE	N	Y	Y	Y
State-year FE	N	N	Y	Y
N	11,718	11,513	11,341	9,477

Notes: In general, the sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the (aggregate) package/loan level. The sample in the fourth column is limited to loans with one lead arranger. All singletons are dropped from the total number of observations N. The dependent variable is a Herfindahl index which is equal to the sum of the squared loan shares of all (participating or lead) banks in the syndicate, and varies from 0 to 1 (maximal concentration). UB_{kt} is an indicator variable for whether at the time t of the loan transaction k any one of the (participating or lead) banks was a universal bank. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. Syndicate fixed effects indicate the inclusion of bank fixed effects for *all* participating and lead banks at the package level. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. State-year fixed effects are based on the location of firm i's headquarters in year t. Robust standard errors (clustered at the package level) are in parentheses.

Table 9: **Impact of Universal-bank Financing on Participant Shares – Package Level**

	Average participant share			
Sample	All	All	All	One LA
UB	0.071***	0.069***	0.070***	0.076***
	(0.005)	(0.005)	(0.005)	(0.006)
UB × After(Aug. 1, 1996)	0.012*	0.009	0.004	0.010
	(0.007)	(0.007)	(0.008)	(0.008)
Log of sales at close		0.009***	0.011***	0.015***
		(0.001)	(0.002)	(0.002)
Log of no. employees		0.000	0.000	-0.001
		(0.001)	(0.001)	(0.002)
Syndicate FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
Industry-year FE	N	Y	Y	Y
State-year FE	N	N	Y	Y
N	11,718	11,513	11,341	9,477

Notes: In general, the sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the (aggregate) package/loan level. The sample in the fourth column is limited to loans with one lead arranger. All singletons are dropped from the total number of observations N. The dependent variable corresponds to the average share of the loan retained by the participants (but not by any lead arranger), and is defined between 0 and 1. UB_{kt} is an indicator variable for whether at the time t of the loan transaction k any one of the (participating or lead) banks was a universal bank. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. Syndicate fixed effects indicate the inclusion of bank fixed effects for *all* participating and lead banks at the package level. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. State-year fixed effects are based on the location of firm i's headquarters in year t. Robust standard errors (clustered at the package level) are in parentheses.

Table 10: **Impact of Universal-bank Financing on Lead Shares: Comparative Statics – Package Level**

Sample	Average lead share			
	Less stock-return volatility	More stock-return volatility	Less idiosyncratic volatility	More idiosyncratic volatility
UB	-0.274***	-0.173***	-0.283***	-0.161***
	(0.024)	(0.018)	(0.024)	(0.018)
UB × After(Aug. 1, 1996)	-0.082**	0.029	-0.083**	0.016
	(0.040)	(0.025)	(0.041)	(0.025)
Log of sales at close	-0.042***	-0.071***	-0.041***	-0.072***
	(0.006)	(0.008)	(0.006)	(0.008)
Log of no. employees	0.007**	0.000	0.009***	0.001
	(0.003)	(0.007)	(0.003)	(0.007)
Syndicate FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
N	3,294	3,288	3,282	3,290

Notes: In general, the sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the (aggregate) package/loan level. In the first and second column, we limit the sample to loans that are associated with six-year firm-level stock-return volatility (from $t-5$ to t) in the bottom and top 50%, respectively. In the third and fourth column, we limit the sample to loans that are associated with six-year idiosyncratic volatility (from $t-5$ to t) in the bottom and top 50%, respectively. All singletons are dropped from the total number of observations N. The dependent variable corresponds to the (average) share of the loan retained by the lead arranger(s), and is defined between 0 and 1. UB_{kt} is an indicator variable for whether at the time t of the loan transaction k any one of the (participating or lead) banks was a universal bank. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. Syndicate fixed effects indicate the inclusion of bank fixed effects for *all* participating and lead banks at the package level. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. Robust standard errors (clustered at the package level) are in parentheses.

Table 11: **Impact of Universal-bank Financing on Institutional-investor Participation – Package Level**

	Share of institutional participants			
Sample	All	All	All	One LA
UB	0.096***	0.094***	0.096***	0.091***
	(0.005)	(0.005)	(0.006)	(0.006)
UB × After(Aug. 1, 1996)	0.043***	0.034***	0.030***	0.028***
	(0.007)	(0.008)	(0.008)	(0.009)
Log of sales at close		0.002	0.004*	0.009***
		(0.002)	(0.002)	(0.002)
Log of no. employees		0.002	0.001	-0.001
		(0.002)	(0.002)	(0.002)
Syndicate FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
Industry-year FE	N	Y	Y	Y
State-year FE	N	N	Y	Y
N	11,718	11,513	11,341	9,477

Notes: In general, the sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the (aggregate) package/loan level. The sample in the fourth column is limited to loans with one lead arranger. All singletons are dropped from the total number of observations N. The dependent variable corresponds to the total share of the loan retained by institutional investors as participants (but not as lead arrangers), and is defined between 0 and 1. UB_{kt} is an indicator variable for whether at the time t of the loan transaction k any one of the (participating or lead) banks was a universal bank. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. Syndicate fixed effects indicate the inclusion of bank fixed effects for *all* participating and lead banks at the package level. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. State-year fixed effects are based on the location of firm i's headquarters in year t. Robust standard errors (clustered at the package level) are in parentheses.

Table 12: **Impact of Universal-bank Financing on Institutional-investor Participation: Robustness – Package Level**

	Share of institutional participants			
Definition	All	Finance comps	Other	+Term loan B-H
UB	0.084***	0.095***	0.051***	0.096***
	(0.006)	(0.006)	(0.005)	(0.006)
UB × After(Aug. 1, 1996)	0.038***	0.027***	0.023***	0.032***
	(0.008)	(0.007)	(0.009)	(0.008)
ln(Total institutional holdings)	-2.285***			
	(0.213)			
Log of sales at close	0.006***	0.003	-0.001	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Log of no. employees	0.001	0.002	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.002)
Syndicate FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y
N	11,341	11,341	11,341	11,341

Notes: In general, the sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the (aggregate) package/loan level. The sample in the fourth column is limited to loans with one lead arranger. All singletons are dropped from the total number of observations N. The dependent variable in the first column corresponds to the total share of the loan retained by institutional investors as participants (but not as lead arrangers), and is defined between 0 and 1. The dependent variables in the second and third column split up the dependent variable from the first column into the total shares held by, respectively, non-bank finance companies, such as hedge funds, and all other institutional investors. The dependent variable in the last column corresponds to the total share of the loan retained by institutional investors as participants (but not as lead arrangers) in addition to term-loan tranches B to H, and is defined between 0 and 1. UB_{kt} is an indicator variable for whether at the time t of the loan transaction k any one of the (participating or lead) banks was a universal bank. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. *Total institutional holdings*$_{kt}$ is the aggregate volume of institutional holdings in the market for syndicated loans excluding k in month-year t. Syndicate fixed effects indicate the inclusion of bank fixed effects for *all* participating and lead banks at the package level. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. State-year fixed effects are based on the location of firm i's headquarters in year t. Robust standard errors (clustered at the package level) are in parentheses.

Table 13: **Impact of Universal-bank Financing on Institutional-investor Participation – Package Level, Comparative Statics**

Sample	Share of institutional participants			
	Less stock-return volatility	More stock-return volatility	Less idiosyncratic volatility	More idiosyncratic volatility
UB	0.129***	0.084***	0.129***	0.082***
	(0.013)	(0.013)	(0.012)	(0.013)
UB × After(Aug. 1, 1996)	0.087***	0.019	0.074***	0.020
	(0.022)	(0.018)	(0.022)	(0.017)
Log of sales at close	-0.011**	0.006	-0.013***	0.008
	(0.005)	(0.006)	(0.004)	(0.006)
Log of no. employees	-0.001	0.001	0.001	-0.000
	(0.003)	(0.005)	(0.003)	(0.005)
Syndicate FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
N	3,294	3,288	3,282	3,290

Notes: In general, the sample consists of all loans successfully issued by publicly listed U.S. firms. Observations are at the (aggregate) package/loan level. In the first and second column, we limit the sample to loans that are associated with six-year firm-level stock-return volatility (from $t-5$ to t) in the bottom and top 50%, respectively. In the third and fourth column, we limit the sample to loans that are associated with six-year idiosyncratic volatility (from $t-5$ to t) in the bottom and top 50%, respectively. All singletons are dropped from the total number of observations N. The dependent variable corresponds to the total share of the loan retained by institutional investors as participants (but not as lead arrangers), and is defined between 0 and 1. UB_{kt} is an indicator variable for whether at the time t of the loan transaction k any one of the (participating or lead) banks was a universal bank. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. Syndicate fixed effects indicate the inclusion of bank fixed effects for *all* participating and lead banks at the package level. Industry-year fixed effects are based on two-digit SIC codes of borrower firms. Robust standard errors (clustered at the package level) are in parentheses.