Do universal banks finance riskier but more productive firms? ☆

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Abstract

Using variation in bank scope generated by the stepwise repeal of the Glass-Steagall Act in the U.S., we show that the deregulation of universal banks allowed them to finance firms with 14% higher volatility. This increase in risk is compensated by lasting improvements in firms' total factor productivity of 3%. Using bank-scope-expanding mergers to identify shocks to universal banks' private information about borrower firms, we provide evidence that informational economies of scope across loans and non-loan products account for the firm-level real effects of universal banking.

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1. Introduction

In this paper, we use the stepwise repeal of the Glass-Steagall Act in the U.S. to empirically evaluate the effect of bank-scope deregulation on the performance of bank-dependent firms. In doing so, we take a step toward measuring the value added of large universal banks as suppliers of financing to the real economy.

The Glass-Steagall Act of 1933 imposed a strict separation between commercial banking (e.g., borrowing and lending) and investment banking (e.g., securities underwriting). Its repeal allowed for the formation of *universal banks*, which offer both loans and non-loan products. We argue that this deepening of bank-firm relationships reduced informational asymmetries and broadened financial-contracting opportunities. This, in turn, generated economies of scope in financial intermediation, and relaxed constraints in the provision of external finance.

We map this channel to the data by asking whether the deregulation of universal banks allowed them to provide financing for firms making risky investments. Our argument is that constraints on external finance that stem from asymmetric information are typically particularly tight for volatile projects (e.g., Stiglitz and Weiss, 1981; Greenwood, Sanchez, and Wang, 2010). As a result, volatile ventures are the marginal projects that stand to benefit the most from reduced informational asymmetries under universal banking. We answer this question in the affirmative: universal banks financed firms with at least 14% higher sales-growth volatility. We find effects of similar magnitude for firms' stock-return and idiosyncratic volatilities. In addition, we show that these risk increases were accompanied by higher total factor productivity, higher capital expenditure, and higher market capitalization for universal-bank-financed firms.

To identify the effect of bank scope on firm-level outcomes, we focus on a deregulation that occurred in 1996 and removed some of the firewalls in extant universal banks. Before 1996, these firewalls limited universal banks' ability to offer loans and concurrent non-loan products in a coordinated manner. Their removal allowed universal banks to share more resources and information across their commercial-bank and securities divisions, and it enabled universal banks to use this information to enter richer intermediation relationships. Thus, our empirical strategy is to use the 1996 deregulation as a shock to universal banks' propensity to engage in deeper relationships with their borrowers, thereby allowing them to derive informational economies of scope. We then compare changes in the volatility of universal-bank-financed firms before and after 1996 to the volatility of firms that received loans from banks whose scope of banking was unaffected by the deregulation. In this manner, we provide evidence that the increased scope of banking activities enabled universal banks to finance riskier firms.

Figure 1 illustrates our findings. We plot the loan-weighted average 6-year sales-growth volatility of public firms in the U.S. that received loans from commercial and universal banks. In the top panel, we focus on loans granted by universal banks. Among universal-bank loans, we differentiate between *cross-sold* and *non-cross-sold* loans: We label loans as cross-sold when the respective debtor firms also received an underwriting

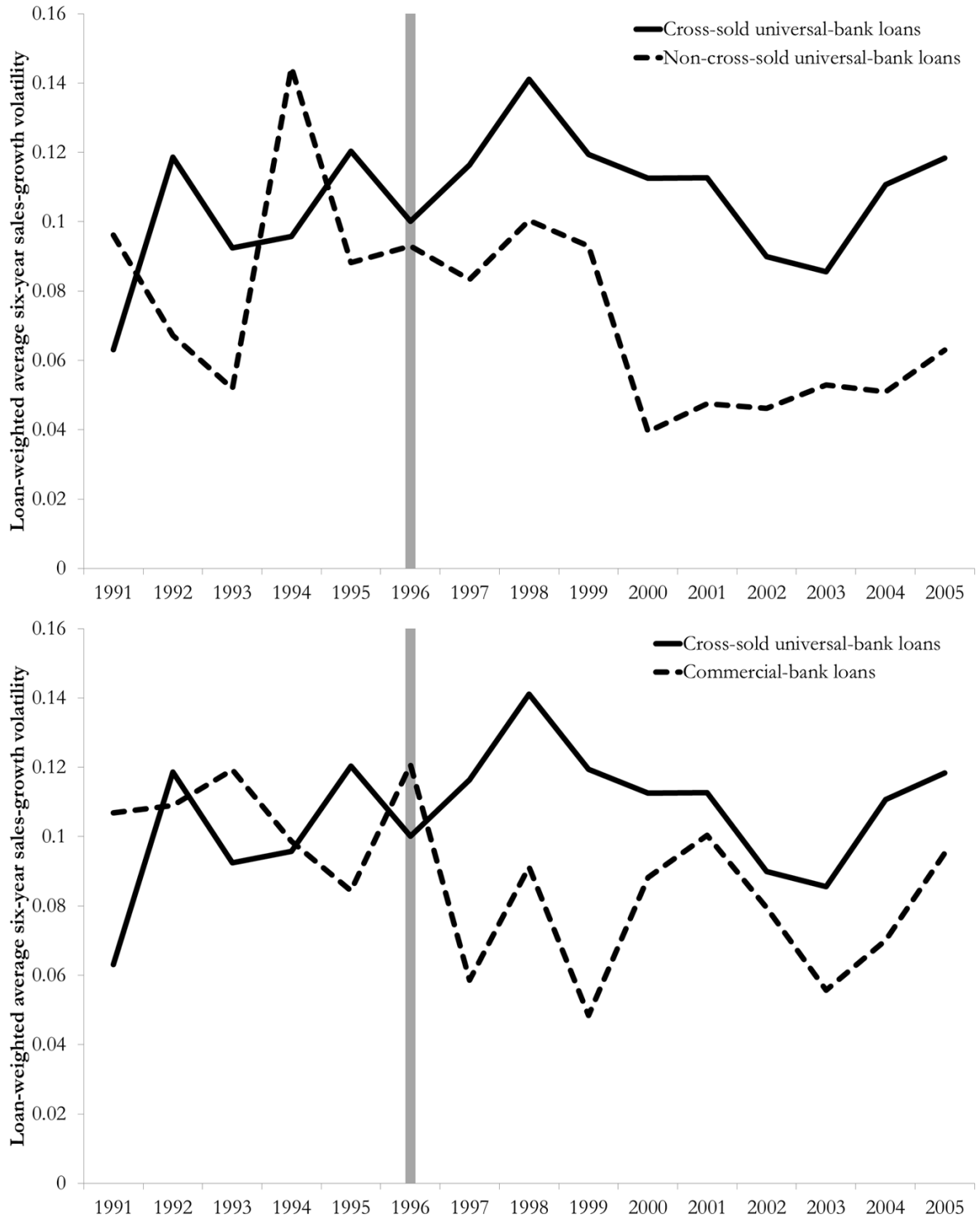

Fig. 1. Loan-weighted average 6-year [t,t+5] sales-growth volatility associated with loans granted to public firms by universal and commercial banks (1991-2005). Loans by universal banks are split into cross-sold and non-cross-sold loans, where cross-sold loans are defined as loans whose debtor firms also received an underwriting product (debt or equity) from the same universal bank anytime within the last three years. Sources: Compustat, DealScan loan data, and SDC underwriting data.

product (debt or equity) from the same universal bank. Until 1996, cross-sold and non-cross-sold universal-bank loans are associated with similar levels of firm risk; but after 1996, the firm-level risk associated with cross-sold universal-bank loans exceeds that of non-cross-sold loans. In the bottom panel, we contrast cross-sold universal-bank loans and commercial-bank loans. The two series exhibit similar levels before 1996, but cross-sold universal-bank loans are associated with substantially higher firm-level volatility after 1996. This suggests that informational economies of scope from cross-selling are a key driver of universal banks' ability to finance riskier firms.

Our empirical results withstand the inclusion of firm fixed effects, so that we identify the treatment effect using firms with multiple bank relationships. Thus, the deregulation of universal banks helped firms to realize projects that were riskier than the projects for which they could secure financing from universal banks before the deregulation. In the aggregate, our results indicate the possibility that the deregulation of universal banks boosted the supply of credit for firms making risky investments.

Because our identification strategy is based on time variation at the bank level, we need to ensure that our treatment effect is not contaminated by other shocks to credit supply around the 1996 deregulation. A key concern in this period is the state-level deregulation of bank branching. We control for this shock by including state-year fixed effects, and our results remain robust.

We then turn to the question of whether the 1996 deregulation of universal banks led to the financing of excessively risky firms that were more likely to go bankrupt, or whether these risk-increasing developments were accompanied by higher productivity of universal-bank-financed firms. First, we provide evidence that the increases in firm-level risk were not associated with higher bankruptcy risk. Second, we show that the deregulation of universal banks helped to finance long-lasting within-firm increases in total factor productivity of approximately 3%. Further results indicate that these productivity increases stem from increases in capital expenditure that are associated with positive market valuations. Our findings attest to a potentially efficiency-increasing effect of deregulating bank scope: when universal banks gain the enhanced ability to cross-sell loans and non-loan products, they provide credit for firms that make risky, productivity-increasing investments.

Next, we present evidence that the firm-level real effects of universal banking derive from informational economies of scope across divisions, rather than from higher bank revenues from cross-selling. To demonstrate this, we exploit mergers among commercial and investment banks as a source of variation in the resulting universal banks' information about borrower firms. Specifically, we consider firms that received both a loan from a commercial bank and an underwriting product from an investment bank, and we then contrast two groups: (i) those firms whose lender and underwriter merged with each other, and (ii) those firms whose lender and underwriter did not merge with each other, but instead merged with other banks to form universal banks.

While both groups' banks are now universal banks, only the former group's banks are able to access both extant loan and non-loan private information about the same firms.

As a result, our approach varies universal banks' information about borrower firms, but holds constant the potential revenues from the intermediation relationship. We find that firms that deal with better-informed lenders, once again, exhibit increases in total factor productivity of up to 3%, which lends support to the idea that our treatment effects are due to informational economies of scope.

Last, we complement our analysis based on loans issued by mature, public firms with evidence on firms earlier in their life cycle. Namely, we examine whether universal banks extended their risk-taking behavior to their role as underwriters by serving as bookrunners for initial public offerings (IPOs) of younger and, thus, potentially riskier firms. To this end, we analyze the age of firms in IPOs run by universal banks compared to investment banks, whose scope of banking activities was unaffected by the deregulation, both before and after 1996. We find that as a response to the deregulation, universal banks took firms public that were at least 18.5% younger than those serviced by investment banks. Our evidence on IPO age supports the idea that the deregulation of universal banks facilitated the entry of younger and riskier firms into the U.S. stock market.

In summary, our paper documents the real effects of bank-scope deregulation. We establish that universal banks reap informational economies of scope that enable them to finance riskier projects with higher productivity.

Related literature

Our paper is related to two main strands of literature. The first strand examines the impact of banking deregulation on firm-level real outcomes, most notably in the context of bank-branching deregulation. The second strand examines the effects of expanding bank scope and relationship banking.

Regarding bank-branching deregulation, Morgan et al. (2004) and Correa and Suarez (2009) find stabilizing effects on state-level growth and firm-level volatility, respectively, among large, publicly listed firms in the U.S. Most closely related to our paper is Krishnan et al. (2015), who show that interstate branching increased the supply of credit for financially constrained firms, allowing them to use these funds to invest in productive projects. By focusing on bank-scope deregulation rather than branching deregulation, we provide evidence of increasing volatility and productivity. We also employ a different identification strategy than is typical in the branching literature. Benfratello et al. (2008) and Amore et al. (2013) use a typical approach: they exploit the staggered timing of branching deregulation across states, and then distinguish between bank-dependent and non-bank-dependent firms in treated states. However, we use data on firms' lending relationships with universal banks to directly identify the impact of financial deregulation on firm-level outcomes.[1] Butler and Cornaggia (2011) share our focus on the effects of finance on productivity, but they exploit variations in demand interacted with access to

[1] While this idea is similar in spirit to that pursued by Herrera and Minetti (2007) using data from Italy, they do not make use of any regulatory quasi-experiment to identify the impact of informed lending on firm outcomes.

external finance rather than variations in bank structure.

Drucker and Puri (2007) survey the literature on universal and relationship banking. One part of this literature considers the bank-level effects of the repeal of the Glass-Steagall Act, as in Saunders et al. (1990) and Cornett et al. (2002). Ang and Richardson (1994), Kroszner and Rajan (1994), and Puri (1994) argue that there is little evidence of a conflict of interest in universal banking in the pre-Glass-Steagall era by examining the long-run performance of bank-underwritten securities. Consistent with a certification role for universal banks, Puri (1996) finds that investors were willing to pay higher prices for securities underwritten by universal banks than for securities underwritten by investment banks, while Gande et al. (1997) show that price differentials between universal-bank and investment-bank underwritings are larger when information costs are high. More recently, Duarte-Silva (2010) shows that an issue's certification is enhanced by private information acquired through pre-existing lending relationships.

In line with these papers, we highlight economies of scope from concurrent lending and underwriting, but we track their impact on firm-level real outcomes.[2] We also present direct evidence suggestive of *informational* economies of scope. These have been studied theoretically in the context of underwriting by Kanatas and Qi (1998) and Kanatas and Qi (2003).

Cross-selling has been discussed in Yasuda (2005), Ljungqvist et al. (2006), and Bharath et al. (2007), among others. Puri and Rocholl (2008) and Santikian (2014) stress the role of cross-selling in retail deposit markets and small business lending. Most of these studies document the pricing effects of cross-selling. Drucker and Puri (2005) and Calomiris and Pornrojnangkool (2009) present evidence that universal banks are more likely to offer discounted yield spreads on concurrent loans. We advance this research by providing evidence of universal banks' ability to bring economies of scope to bear on firm-level real outcomes. Another example is Schenone (2004), who finds significantly less IPO underpricing for firms that had pre-IPO lending relationships with prospective underwriters (i.e., universal banks). Unlike Schenone (2004), we use the risk and productivity of universal-bank-financed firms to infer informational economies of scope.

2. Empirical strategy and data

We start our analysis by first describing the institutional background of the stepwise repeal of the Glass-Steagall Act. We then develop our key hypotheses, and present our identification strategy. Finally, we describe the empirical implementation and the data.

[2] Note that our paper does not focus on universal banks' holding equity stakes in companies and their representation on the boards of these companies (see Ferreira and Matos, 2012), as is the case under the classical model of universal banking in Germany.

2.1. Institutional background

The Glass-Steagall Act of 1933 separated commercial and investment banking – until its stepwise repeal, which began in 1987. Under Section 20 of the Glass-Steagall Act, commercial banks were prohibited from engaging in any kind of underwriting or securities business. These activities were subsequently entirely in the hands of investment banks and other investment houses. The repeal of the Act allowed for the formation of universal banks that combined both commercial- and investment-banking services.

Starting April 30, 1987, commercial banks were allowed to open so-called Section 20 subsidiaries and generate up to 5% of their gross revenues from underwriting and dealing in certain securities, namely municipal revenue bonds, mortgage-related securities, consumer-receivable-related securities, and commercial paper. Two years later, on January 18, 1989, banks were allowed to engage in veritable investment-banking activities, most notably corporate debt and equity underwriting. Then, on September 13, 1989, the revenue limit was raised to 10%. In addition to the formation of Section 20 subsidiaries, this revenue-limit change gave rise to another opportunity for commercial banks to become universal banks by acquiring or merging with investment banks. These measures constituted the first stage of the repeal of the Glass-Steagall Act, followed by seven years of regulatory inactivity.

Although universal banks were able to engage in both lending and corporate-securities underwriting at this point, there were still firewalls in place that separated the two activities. An important consequence of these firewalls was that universal banks could not actively cross-sell loans and non-loan products. Indeed, such cross-selling was prohibited, or at least severely restricted, under the Federal Reserve Act (Sections 23A and B). This affected banks' lending decisions insofar as loans are granted upon approval by a credit committee, often on the basis of high expected depth of cross-selling.[3]

In a major expansion of cross-selling opportunities, the Federal Reserve Board proposed the elimination of some of the informational and financial firewalls on August 1, 1996, and simultaneously raised the revenue limit on underwriting securities from 10% to 25%.[4] This change also enabled more commercial banks to expand into universal banking by directly merging with an investment bank. In particular, the removal of informational firewalls interacted with cross-selling in a meaningful way, as it allowed for the possibility of sharing non-public customer information between commercial-banking and securities divisions. Thus, the 1996 deregulation deepened bank-firm relationships, and enhanced banks' monitoring capabilities, generating economies of scope across financial products.

[3] Bharath et al. (2007) provide ample evidence of cross-selling of loans and non-loan products (fee-generating services), such as debt and equity underwriting. Drucker and Puri (2005) and Yasuda (2005) examine the relationship between past lending relationships and seasoned equity offerings and debt underwriting, respectively.

[4] This specific regulatory event period culminated in the Federal Reserve Board's announcement of replacing further firewalls on August 22, 1997. Thus, August 1, 1996 was only the beginning of a one-year period during which multiple aspects of what we dub the "1996 deregulation" were implemented.

2.2. Hypothesis development

Our basic premise is that the 1996 deregulation boosted cross-selling by universal banks, while cross-selling in turn represents a positive shock to the quality of banks' information about borrower firms (Kanatas and Qi, 1998 and Kanatas and Qi, 2003). A robust conclusion from theoretical corporate finance is that lender informedness is particularly effective at reducing barriers to external finance for *risky firms*. For example, Greenwood et al. (2010) show that in a canonical costly-state-verification framework, cash-flow volatility reduces the firm's pledgeable income and borrowing capacity. However, they also show that these frictions can be overcome more easily by an informed lender. This means that information frictions disproportionately reduce risky firms' access to external finance, and informational economies of scope in universal banking improve the funding opportunities of risky enterprises. Therefore, our first hypothesis is the following:

Hypothesis 1. *Firms financed through universal banks exhibit higher firm-level volatility than other bank-dependent firms after the 1996 deregulation.*

When firms' investment projects are ordered along a risk-return frontier, the costly-state-verification framework employed by Greenwood et al. (2010) also predicts that increased lender informedness leads to higher productivity. The reason is that firms can finance projects further along the risk-return frontier when firm risk is no longer an impediment to external financing in and of itself. This leads to our second hypothesis:

Hypothesis 2. *Firms financed through universal banks exhibit higher productivity than other bank-dependent firms after the 1996 deregulation.*

We will show that Hypothesis 1 holds across various firm-level risk measures, and that universal banks take public younger and, thus, riskier firms. In line with Hypothesis 2, we will also show that universal-bank-financed firms exhibit higher total factor productivity, capital expenditure, and market capitalization.

2.3. Identification strategy

Our identification strategy exploits the 1996 deregulation as a shock to universal banks' propensity to cross-sell loans and non-loan products, allowing them to derive informational economies of scope. In order to empirically evaluate the impact of such increases in bank scope on real outcomes of borrower firms, we employ a difference-in-differences framework. Our treatment group consists of borrower firms that received universal-bank loans. The control group consists of firms that received loans from other types of banks, typically commercial banks but also investment banks, whose scope of banking activities was unaffected by the 1996 deregulation.

To test the impact of the 1996 deregulation on the characteristics of universal-bank-financed firms, we estimate the following difference-in-differences specification at the level

of years in which a firm i received at least one loan from one or multiple banks j:

$$\begin{aligned} y_{ijt} &= \beta_1 \textit{Universal-bank loan}_{jt} \times After(1996)_t + \beta_2 \textit{Universal-bank loan}_{jt} \\ &\quad + \beta_3 X_{it} + \delta_t + \eta_j + \epsilon_{ijt}, \end{aligned} \tag{1}$$

where y_{ijt} is a firm-level outcome in loan year t, e.g., change in firm-level volatility, *Universal-bank loan*$_{jt}$ is an indicator variable that is equal to 1 if any of the lead arrangers j was a universal bank at the time of any loan transaction in year t, $After(1996)_t$ is an indicator for whether the firm's loan year in question was 1997 or later, X_{it} denotes other control variables measured in year t, and δ_t and η_j denote year and bank fixed effects, respectively, where bank fixed effects are included for all lead arrangers of all loans of firm i in year t.

This specification effectively estimates the average risk associated with loans granted by universal banks compared to pure commercial or investment banks before and after 1996. In particular, we do not rely on the establishment dates of universal banks (i.e., the conversion of commercial into universal banks) as our main variation in bank scope, as commercial banks endogenously chose to become universal banks.[5] Conversely, it is unlikely that banks and firms were anticipating the deregulatory policy before 1996. This is affirmed by the fact that the banking industry had already proposed the elimination of firewalls in 1991, which was rejected by the United States House Committee on Financial Services.

In the presence of bank fixed effects η_j, the difference-in-differences estimate β_1 is identified using the lending behavior of commercial banks that became universal banks before the deregulation and, thus, experienced an expansion in the scope of their activities in 1996. To estimate β_1 and β_2, a given bank j must therefore be observed as a lender 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_2), and as a universal bank after the 1996 deregulation (β_1).

A potential concern is that post-1996 risk taking by universal banks may be due to the sorting of new firms with different risk profiles seeking financing from universal banks after the deregulation was implemented. This would render it problematic to compare universal-bank loans before and after 1996. To address this issue, we also include firm fixed effects in our regressions. We thus identify the treatment effect using firms that received multiple loans over time. In the presence of firm fixed effects, the difference-in-differences estimate β_1 is identified using multiple loans granted to firm i by at least two different banks. Furthermore, firm i must be observed to contract at least once with a commercial or investment bank, a universal bank before the deregulation, and a universal bank after the deregulation.

[5] As noted by Bhargava and Fraser (1998) among others, the initiation of universal-banking deregulation from 1987 to 1989 was in large part based on the Federal Reserve's responses to specific requests from large banks (Bankers Trust, Citicorp, and J.P. Morgan).

Note that because the difference-in-differences estimate is at the bank-year level jt, we cannot include bank-year fixed effects. In order to interpret β_1 as a shift in bank-level credit supply for risky firms, we must ensure that β_1 is not contaminated by other shocks to credit supply around the 1996 deregulation. A key concern in this period is the relaxation of bank-branching restrictions (e.g., Jayaratne and Strahan, 1996), which constituted a positive credit-supply shock at the state level while allowing commercial banks to expand the range of their products through mergers with existing universal banks within and across states. To control for this possibility, we also include state-year fixed effects, as defined by the state in which the borrower firm's headquarters are located.

We then take one more step to provide supporting evidence of our conjecture that the firm-level real effects of universal banking arise due to informational economies of scope. To do so, we must take into account that cross-selling not only varies lenders' information about borrower firms but also enables universal banks to derive more revenues from their relationships with firms. These two channels may even be intertwined; informational economies of scope support the cross-marketing efforts of universal banks, leading to increased revenues, and the very process of cross-selling generates further information about the client through closer intermediation relationships.

To identify the effect of information on firm-level outcomes, we exploit the fact that in addition to removing firewalls, the 1996 deregulation also raised the revenue limit on underwriting securities from 10% to 25%. This spurred a wave of scope-expanding mergers between commercial banks (or existing universal banks) and investment banks. We use such bank mergers as a shock to bank-level information acquisition about borrower firms, because merged universal banks can make use of the information embodied in both its extant commercial-bank and investment-bank division.

We operationalize this strategy by comparing firms that had previously received both a loan from a commercial (or existing universal) bank as well as a debt- or equity-underwriting product from an investment bank. In the treatment group, the two institutions merged with each other, thereby pooling their information about borrower firms. In the control group, both banks merged with financial institutions of complementary scope (i.e., the commercial/universal bank merged with an investment bank and vice versa) but not with each other. In this manner, we hold constant the potential for future revenues through cross-selling to treatment and control firms, as both treated and control firms remain in relationships with universal banks after the mergers. Yet, firms in the treatment group interact with a better-informed universal bank, while those in the control group do not.

We show that the results based on our difference-in-differences estimations also hold for this universal-bank-mergers identification strategy. This serves as evidence that universal banks' ability to finance riskier and more productive firms is indeed due to informational economies of scope rather than due to differences in revenues from bank-firm relationships.

2.4. Empirical implementation

To test our claim that universal banks financed riskier firms, we use transaction-level data on syndicated loans of public firms in the DealScan database. We focus on lead arrangers when characterizing the types of banks granting loans. For our analysis, we collapse the loans sample to the firm-loan-year level (i.e., we summarize all the loans of a firm in a given year).

In order to determine whether a bank was a universal bank at the time of a given loan transaction, we compare the transaction date to the completion date of a bank-scope-expanding merger (i.e., a merger of a commercial bank with an investment bank) or the opening date of the respective bank's first Section 20 subsidiary.

As an example, consider the historical anatomy of J.P. Morgan. Before acquiring Bank One on July 1, 2004, J.P. Morgan had already become a universal bank by opening a Section 20 subsidiary on April 30, 1987, followed by a merger with Chase Manhattan, which had a Section 20 subsidiary since December 30, 1988 (and later merged with Chemical Bank). Similarly, Bank One, J.P. Morgan's acquisition target in 2004, maintained a Section 20 subsidiary which it had opened on February 2, 1989. Thus, despite a series of mergers, J.P. Morgan became a universal bank through opening a Section 20 subsidiary in 1987, and any loan granted by J.P. Morgan after April 30, 1987 is labeled as a loan granted by a universal bank.[6] In Table 1, we provide an overview of all universal banks and their mode of establishment in our loan data.

In our baseline regression, we run the difference-in-differences specification (1) on the sample of firm-loan years to estimate the riskiness of borrowers contracting with universal banks before versus after the 1996 deregulation. As dependent variable, we use the difference between a logged 6-year volatility measure from t to $t+5$ and the same measure from $t-6$ to $t-1$, where t is the firm-loan year in question. That is, the outcome variable measures the percent change in risk around year t in which a firm received at least one loan. Standard errors are clustered at the bank level, using a vector of all banks j that acted as lead arrangers to firm i in a given year t.

Because the sample is limited to years in which firm i received at least one loan, the omitted category consists of firm-loan years with only commercial or investment banks as lead arrangers, none of which experienced a change in their scope of banking activities following the 1996 deregulation.

When we move to analyzing firm-level outcomes that do not require multiple years

[6] Our sample also includes U.S. banks of international origin. These banks are special cases in that before the International Banking Act of 1978, they were not subject to the Glass-Steagall Act. As a consequence, international banks that were active in the U.S. before 1978 and were established as universal banks outside the U.S. were allowed to continue their business model in the U.S. (as long as they would not expand their activities further). None of the banks in our sample were subject to the International Banking Act. For instance, Deutsche Bank became a universal bank only after acquiring Morgan Grenfall, a London-based investment bank, in 1990. Similarly, Crédit Suisse acquired a controlling stake in the U.S. investment bank First Boston Corporation in December 1988.

of data for their calculation, such as firms' total factor productivity (TFP), we also include all firm years (from Compustat) without any loan transactions. As changes in productivity might not materialize immediately after a loan issue, we define firm-loan years based on whether a firm received a loan anytime in the past five years. For all loans after 1996, this definition is censored at the year 1997.[7] This implies that our estimated effects of universal-bank versus non-universal-bank loans on, among other outcomes, TFP last, or show only in, up to five years. Given that we also include firm-year observations for which all loans-related variables are zero, firms with no loan in a given year are the omitted category. Furthermore, this enables us to include firm fixed effects and estimate the within-firm effects of universal-bank loans by means of the following regression specification:

$$\begin{aligned} y_{it} &= \beta_1 \textit{Universal-bank loan}_{jt} \times After(1996)_t + \beta_2 \textit{Universal-bank loan}_{jt} \\ &\quad + \beta_3 X_{it} + \delta_t + \mu_i + \eta_j + \epsilon_{it}, \end{aligned} \tag{2}$$

where y_{it} is the natural log of firm i's outcome in year t, $\textit{Universal-bank loan}_{jt}$ is an indicator variable that is equal to 1 if any of the lead arrangers j was a universal bank at the time of any loan transaction from year $t-4$ to year t, $After(1996)_t$ is an indicator variable that is equal to 1 if the year in question was 1997 or later, X_{it} denotes other control variables measured in year t, and δ_t, μ_i, and η_j denote year, firm, and bank fixed effects, respectively, where bank fixed effects are included for all lead arrangers of all loans of firm i from year $t-4$ to year t. Standard errors are clustered at the firm-year level.

Finally, in order to disentangle the revenue channel from informational economies of scope using bank mergers, we compare firm outcomes across two groups of firms that each received both a loan from a commercial bank (or from an existing universal bank) and an underwriting service from an investment bank. The treatment group consists of firms that contracted with a commercial bank (or with an existing universal bank) and an investment bank that later *merged with each other*. The control group consists of firms whose commercial and investment banks merged with other institutions to form a universal bank, but *did not merge with each other*. Formally, we estimate the following specification:

$$\begin{aligned} y_{it} &= \beta_1 \textit{Loan from CB, underwriting from IB, both merged with each other}_{it} \\ &\quad + \beta_2 \textit{Loan from CB that merged with IB}_{it} \\ &\quad \times \textit{Underwriting from IB that merged with CB}_{it} \\ &\quad + \beta_3 \textit{Loan from CB that merged with IB}_{it} \\ &\quad + \beta_4 \textit{Underwriting from IB that merged with CB}_{it} \\ &\quad + \beta_5 \textit{Any loan}_{it} \times \textit{Any underwriting}_{it} \\ &\quad + \beta_6 \textit{Any loan}_{it} + \beta_7 \textit{Any underwriting}_{it} + \beta_8 X_{it} + \delta_t + \mu_i + \epsilon_{it}, \end{aligned} \tag{3}$$

[7] Our results are robust to variations of the 5-year horizon. Additional results are available upon request.

where y_{it} is the natural log of firm i's outcome in year t, *Loan from CB, underwriting from IB, both merged with each other*$_{it}$ indicates whether firm i received a loan from a commercial or universal bank and an underwriting product from an investment bank anytime from $t-10$ to $t-1$, with these banks then merging *with each other* to form a universal bank at any point until year t. The indicator variable *Loan from CB that merged with IB*$_{it}$ is equal to 1 if firm i received a loan anytime from $t-10$ to $t-1$ from a commercial or universal bank that later merged with any investment bank (i.e., the investment bank may or may not have previously had an underwriting relationship with firm i). The indicator variable *Underwriting from IB that merged with CB*$_{it}$ is equal to 1 if firm i received an underwriting product anytime from $t-10$ to $t-1$ from an investment bank that later merged with any commercial or universal bank (i.e., the commercial or universal bank may or may not have previously had a lending relationship with firm i). The indicator variables *Any loan*$_{it}$ and *Any underwriting*$_{it}$ are equal to 1 if firm i received any loan or any underwriting product, respectively, from any commercial, universal, or investment bank anytime from $t-10$ to $t-1$. X_{it} denotes other control variables measured in year t, and δ_t and μ_i denote year and firm fixed effects, respectively. Standard errors are clustered at the firm-year level.

The relevant time window comprises eleven years, so we can realistically accommodate the triplet of events (loan transaction, underwriting, and any mergers). Note that our 10-year window for the two transaction types (loan and underwriting) ends in year $t-1$, rather than year t (i.e., the last possible year that we consider for a potential merger). In this manner, we safeguard that both loan and underwriting transactions took place before any potential merger of the two banks, and did not take place as a result of the merger. We show that our results are robust to a shorter time window in the Online Appendix.

The difference between the treatment and control groups (treatment effect) is given by β_1. In constructing these groups, we restrict attention to firms that received both a loan from a commercial bank and an underwriting service from an investment bank, with both commercial and investment banks later becoming part of a universal bank. The treatment group consists of firms whose lender and underwriter became a universal bank by merging with each other, while the control group consists of firms whose lender and underwriter each became part of a universal bank by merging with some other bank of complementary scope. The only difference between the groups thus is whether the formed universal bank had two sources of private information about the firm because both its commercial-banking and underwriting division had previously dealt with the same firm. Formally, this means that the independent variables associated with β_2 through β_7 are equal to 1 for both treatment and control, while *Loan from CB, underwriting from IB, both merged with each other*$_{it}$ is equal to 1 for the treatment group only.

To see why, note that firms in both the treatment and the control group received loans and underwriting products from banks that later became part of a universal bank, so that *Loan from CB that merged with IB*$_{it}$ $\times$ *Underwriting from IB that merged with CB*$_{it}$ $= 1$ for both groups. Moreover, the fact that they received these products in the first place implies that *Any loan*$_{it}$ $\times$ *Any underwriting*$_{it} = 1$. Only firms in the treatment

group contracted with banks that became part of the same universal bank by merging with each other, however, and β_1 thus captures this difference between the two groups.

We interpret β_1 as an intention-to-treat effect under the premise that the respective firm is likely to continue contracting with the newly formed universal bank, which now has more information about its borrowers. The literature on lock-in in underwriting relationships (e.g., James, 1992 and Ljungqvist, Marston, and Wilhelm, 2006) provides evidence for this interpretation. We find similar evidence in our regression sample. Among firms in the treatment group, 50.9% (68.3%) returned to the merged universal bank for another loan (underwriting product) within five years after the merger; and in the control group, 52.1% (59.8%) returned to any one of the two universal banks involved in mergers for another loan (underwriting product). These ex-post probabilities are high, and remarkably similar despite the comparison between returning to one versus two universal banks involved in mergers.

2.5. Data description

The focus of our analysis will be on estimating the impact of universal banking on different firm-level outcomes, most notably risk and productivity. To this end, we use as our main data sources Compustat accounting data, CRSP stock prices, DealScan loan data, and SDC debt- and equity-underwriting data. We match DealScan with Compustat data using the link provided by Chava and Roberts (2008). As is customary, we drop public-service, energy, and financial-services firms from our analysis. On the transaction level, we use syndicated loans of public firms in the U.S. in the DealScan database since 1987, as well as on U.S. IPOs listed in the SDC database since 1976. We focus on the lead arrangers of syndicated loans, and for IPOs we consider the bookrunners.

In addition, we use string matching to generate unique bank identifiers for commercial, universal, and investment banks across these datasets. To identify mergers between any two banks in DealScan loan data and SDC underwriting data, we use the SDC M&A database in conjunction with hand-collected mergers obtained through a LexisNexis news search.

Outcome variables

Firm-level risk measures are among the most important outcome variables considered in this paper. We focus primarily on the 6-year volatility of sales-growth rates γ_{it} of firm i in year t. We use a 6-year window to limit the number of firms dropping out of our sample due to firm death, and we follow Davis et al. (2007) in constructing annual growth rates that accommodate entry and exit:

$$\gamma_{it} = \frac{x_{it} - x_{i,t-1}}{\frac{1}{2}\left(x_{it} + x_{i,t-1}\right)}, \tag{4}$$

where x_{it} denotes sales from Compustat.

Using these growth rates, we obtain the 6-year standard deviation of firm i's sales

growth over six years, $\sigma(\widehat{sales_i})^{6y}$. As alternative measures of firm-level risk associated with loans, we also consider 6-year stock-return volatilities $\sigma(return_i)^{6y}$, which are calculated using monthly CRSP stock-return data, and idiosyncratic volatilities $\sigma^{6y}_{idiosyncratic,i}$, estimated from the Fama and French (1993) three-factor model. In the Online Appendix, we also use 3-month implied volatilities calculated using the volatility surface from option prices, which are obtained from Option Metrics and are available starting in 1996.

Given that public firms in DealScan are typically mature, we use another outcome measure to capture firm risk earlier in the firm's life cycle: the firm's age at the time of its IPO. To calculate the latter, we use the founding dates of firms with IPOs recorded in SDC until 2006, collected by Loughran and Ritter (2004).

Besides the above-mentioned risk measures, we also analyze effects on firm-level TFP, for which we use data from Imrohoroglu and Tuzel (2014), who employ the semiparametric estimation procedure by Olley and Pakes (1996) for the panel of Compustat firms. As alternative outcome variables, we will also use capital expenditure (from Compustat) as well as market capitalization (i.e., the market value of equity) from CRSP.

Summary statistics

In Table 2, we present summary statistics of firm-specific and transaction-level variables for all major regression samples used in the paper. We start with our *loans sample* from DealScan, and on this basis, we generate the *firm-loan-years sample* which comprises only years in which a given firm received at least one loan. Next, we construct the *Compustat sample* by using Compustat to add observations on years in which firms did not receive any loans. Finally, we use SDC IPO data to generate our *IPO sample*.

Our loans sample is based on DealScan data from 1987 to 2010. The respective regression sample comprises 19,053 loans of public firms in general, 64% of which were granted by universal banks. Another 11% were granted by investment banks, and the remainder were granted by commercial banks (i.e., by universal banks when they were still commercial banks, or by banks that remained pure-play commercial banks throughout the sample period). Only universal and investment banks can offer both loans and non-loan products. Among such loans granted by universal and investment banks, 12,061 were associated with concurrent underwriting of corporate securities of the same borrower firm within a 5-year circle around the loan issue, of which 79% were cross-sold by universal or investment banks. Within this sample, 11,863 loans were associated with concurrent debt underwriting, and 4,008 were associated with concurrent equity underwriting.[8] Loans were much more likely to be cross-sold with debt-underwriting mandates: 85% of loans associated with concurrent debt underwriting were cross-sold by the same universal or investment bank, while only 19% of loans associated with concurrent equity underwriting were cross-sold.

[8] Note that these numbers add up to more than 12,061 because some loans were associated with both concurrent debt and equity underwriting.

We also give an overview of the number of banks in DealScan. In particular, six out of the eight universal banks that came into existence through mergers and acquisitions were established before August 1, 1996, and 28 out of the 37 commercial banks turned into universal banks through opening Section 20 subsidiaries before the deregulation.

In the second panel, we move to the firm-loan-years sample, which summarizes all loans that a given firm received in a year. For firm-loan year it, $\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$ is the difference between the logged 6-year standard deviation of firm i's sales growth from t to $t+5$ and the same measure from $t-6$ to $t-1$. $\Delta_t \ln(\sigma(return_i)^{6y})$ is the difference between the logged 6-year standard deviation of firm i's stock returns from t to $t+5$ and the same measure from $t-6$ to $t-1$. $\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$ is the difference between the logged 6-year idiosyncratic volatility of firm i's stock returns from t to $t+5$ and the same measure from $t-6$ to $t-1$, estimated from the Fama and French (1993) three-factor model and expressed in annualized terms. In case of multiple loans per firm in consecutive years, $t-1$ is replaced by the last year without any loans for the respective firm before the sequence of years with loans, and t is replaced by the last year in the sequence. Remarkably, the average effect of a loan on a borrower firm's riskiness is close to zero across all three variables. These variables correspond to the dependent variables in Tables 3 to 5.

For our Compustat sample in the third panel, we merge our DealScan data with Compustat data starting in 1987, including firms that never received loans recorded in DealScan. The variables in the first four rows correspond to the dependent variables in Table 7 and Appendix Tables A.8, A.9, and A.15. The smaller sample size for the TFP measure is due to data availability in our TFP-data source (Imrohoroglu and Tuzel, 2014), which covers the period from 1987 to 2009. Similarly, option-implied volatility $\sigma^{implied}_{it}$ is available from 1996 only. We also provide summary statistics for the definition of treatment and control observations for our alternative identification strategy, which is based on 150 scope-expanding mergers between commercial banks (or existing universal banks) and investment banks from 1990 to 2010.

Our SDC IPO sample in the last panel is limited to IPOs with no more than one bookrunner, leaving us with a regression sample of 3,835 IPOs. This sample is conditional on the availability of IPO age (based on Loughran and Ritter, 2004). Unlike in the loan data, investment banks dominate the IPO market: 460 investment banks were responsible for 83% of the IPOs, while the remainder of the firms were taken public by universal banks. In the SDC IPO data, five out of five universal banks established through M&A existed before August 1, 1996. Among Section 20 subsidiaries, 12 out of 15 were opened before the deregulation.

3. Results

We now turn to the estimation results using the loan data. We begin by investigating Hypothesis 1, and test whether universal banks financed more volatile firms. Then, we test whether these volatility-increasing developments were accompanied by within-

firm increases in total factor productivity and investment, as predicted by Hypothesis 2. To isolate informational economies of scope as the driving force, we use universal-bank mergers as a source of variation in bank-level information about borrower firms. Finally, we consider risk taking by universal banks in the market for IPOs, and we analyze whether the firms taken public by universal banks were younger than the firms taken public by investment banks following the 1996 deregulation.

3.1. Volatility of universal-bank-financed firms

In Table 3, we estimate (1), and we use as dependent variable the borrower firms' percent change in 6-year sales-growth volatility, $\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$. After including industry fixed effects alongside transaction-specific and firm-level control variables in the second column, we find that universal banks financed firms with 13.8% higher sales-growth volatility following the 1996 deregulation. As we always include bank fixed effects, the difference-in-differences estimate is identified using the lending behavior of commercial banks that converted to become universal banks before the deregulation. As we saw in the first panel of Table 2, this applies to three-quarters of all universal banks.

This already substantial effect increases to 17.9% in the third column after including state-year fixed effects. We introduce these fixed effects to control for the possibility that our difference-in-differences estimate, which varies at the bank-year level, may capture any effects of bank-branching deregulation. For instance, bank branching could interact with universal banking by expanding the geographical access to universal banking for firms.

In the fourth column, we add firm fixed effects, which further increases the difference-in-differences estimate to 23.6%. This suggests that the increase in firm risk partly operates at the intensive margin. That is, firms that previously obtained financing from universal banks also engaged in riskier ventures after the 1996 deregulation.

The inclusion of firm fixed effects forces our identification to come from firms with multiple loans granted by at least two different banks. For our regressions using sales-growth volatility as dependent variable, we have summarized key figures for the observations that allow us to use this identification strategy at the bottom of the second panel of Table 2. Specifically, our sample consists of 3,362 firm-loan years and 1,695 firms, out of which 1,442 had at least two bank relationships. We achieve full identification within firms that contracted with at least two banks and that we observe to contract at least once with a commercial or investment bank, a universal bank before the deregulation, and a universal bank after the deregulation. This is the case for 477 firm-loan years (116 firms).

Thus far, the omitted category consists of all banks whose scope of banking activities was unaffected by the 1996 deregulation (i.e., commercial and investment banks). Investment banks are the least active group of banks in the syndicated-loans market, as seen in the first panel of Table 2. Because these two types of banks differ along other dimensions as well, it is worthwhile to separately estimate the effect of the 1996 deregulation on their

lending behavior. To this end, we re-run the same specification as in the fourth column of Table 3, but we explicitly include a difference-in-differences term for investment banks, leaving commercial banks as the omitted category.[9] The estimated coefficient of 0.004 in the last column suggests that investment banks did not finance differentially risky firms compared to commercial banks. The estimate is, however, significantly lower than the difference-in-differences estimate for universal banks.

As a robustness check, we test whether there were any notable pre-trends in universal banks' lending behavior. In particular, we replace $After(1996)_t$ by a placebo year, 1993. The difference-in-differences estimates in Appendix Table A.1 are insignificant throughout.

All results from Table 3 carry over to firms' stock-return volatility, $\Delta_t \ln(\sigma(return_i)^{6y})$, and idiosyncratic volatility, $\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$. The results are in Tables 4 and 5, respectively. The increase in stock-return volatility is similar to that estimated for sales-growth volatility across the first three columns, but it is lower after including firm fixed effects. Conversely, the estimates for idiosyncratic volatility are similar to those for stock-return volatility after including firm fixed effects, but are somewhat lower without them.

Note that we allow for the possibility that firms received loans from both universal and investment banks in a given year. Hence, it may be ambiguous which type of bank we can attribute the increases in firm-level volatility to. In Appendix Tables A.2, A.3, and A.4, we show that our findings for all three volatility measures are robust to dropping (the few) observations that are associated with both universal-bank and investment-bank loans in a given year.

Going one step further, the most conservative robustness check would be to drop all firms that ever received investment-bank loans. Doing so would affect 630 (671) of 3,362 (3,556) observations in Table 3 (Tables 4 and 5). Because our specifications with firm fixed effects estimate the difference-in-differences from multiple loans to the same firm granted by different types of banks, dropping all investment banks would remove a substantial portion of the variation required for this strategy. We are therefore unable to run these specifications for this highly restricted sample. For the remaining specifications (the second and third column of Tables 3, 4, and 5), our estimates are robust to dropping all firms with investment-bank loans across all three volatility measures in Appendix Table A.5.

The economic mechanism that we propose is that the differential risk-taking effect is due to universal banks' economies of scope from cross-selling after the 1996 deregulation. To provide further evidence for this channel, we compare the incidence of cross-selling before and after the deregulation for universal and investment banks, the two types of banks that theoretically have the capacity to offer both loans and non-loan products. In Appendix Table A.6, we limit the sample of loans to those that were associated with concurrent underwriting of debt or equity by the same borrower firm within a 5-year

[9] Note that this may lead to a change in the estimated coefficient on *Universal-bank loan*$_{jt}\times$ $After(1996)_t$, as some firms received loans from both universal and investment banks in a given year.

circle (from year $t-2$ to $t+2$, where t corresponds to the year of the loan issue in question), and we use as dependent variable an indicator for whether the loan and the underwriting product were issued by the same bank.

In the first three columns of Appendix Table A.6, we employ the same fixed-effects structure as in the second to fourth columns of Tables 3 to 5, and we find that following the 1996 deregulation, universal banks were eight to nine percentage points more likely to cross-sell than investment banks.[10] Focusing only on universal banks and their mode of establishment in the last three columns, universal banks established through Section 20 subsidiaries, rather than through M&A, were eight to ten percentage points more likely to cross-sell after 1996, which could be due to their early specialization in corporate-securities underwriting, rather than any other investment-banking operations.

To characterize the source of higher firm-level volatility, we next assess whether universal-bank loans were associated with higher credit risk. That is, we examine whether universal banks relaxed financial constraints for risky projects, or whether they financed excessively risky firms that were on the verge of bankruptcy. Our hypothesis is that they did the former.

In Table 6, we return to our firm-loan-years sample, and use as dependent variable an indicator for whether the borrowing company went bankrupt[11] in the 10 years following the loan-issue year.[12] As Table 6 shows, universal-bank loans were not associated with greater bankruptcy risk among borrower firms after the 1996 deregulation (i.e., the difference-in-differences estimate is not significantly different from zero). However, after the inclusion of firm fixed effects in the fourth column, universal-bank loans were associated with significantly *less* bankruptcy risk after 1996. Furthermore, in the last column, we find that following the 1996 deregulation, investment banks, unlike commercial and universal banks, financed firms that were at least 10 percentage points more likely to be delisted for bankruptcy-related reasons within 10 years after the loan issue. This effect is significant at the 1% level.

These results also address another concern. Recall that our benchmark measures of firm risk were based on the within-firm change in risk after versus before loan issues. A potential downside to this forward-looking definition of the outcome variables is that we may be systematically omitting (or prematurely dropping) firms that did not survive $6+6=12$ years, because they were excessively risky. Our results in Table 6 indicate

[10] However, we do not find that universal banks, on average, extended loans at more favorable terms after the 1996 deregulation, as measured by the so-called all-in-drawn spread, which is the sum of the spread over LIBOR and any annual fees paid to the lender syndicate (see Appendix Table A.7).

[11] We use the following CRSP delisting codes to identify bankruptcy: any type of liquidation (400-490); price fell below acceptable level; insufficient capital, surplus, and/or equity; insufficient (or non-compliance with rules of) float or assets; company request, liquidation; bankruptcy, declared insolvent; delinquent in filing; non-payment of fees; does not meet exchange's financial guidelines for continued listing; protection of investors and the public interest; corporate governance violation; and delist required by Securities Exchange Commission (SEC).

[12] Our results are robust to variations in the horizon.

that this was not the case. Note also that the sample we use for these bankruptcy-risk tests is not conditional on the availability of 6-year-volatility data before and after the firm-loan year.

3.2. Productivity and investment of universal-bank-financed firms

Thus far, we have considered only measures related to firm-level risk as outcomes. We now turn to the question as to whether the additional risk of universal-bank-financed firms was rewarded by higher productivity, as suggested by a risk-return trade-off. Our analysis proceeds much like that in the previous section. The only difference is that we estimate long-run within-firm effects on annual observations rather than on 6-year volatilities. For this purpose, we modify our loans-related variables, so they are based on any loan transactions within the past five years. Bank fixed effects are defined based on loans of firm i within the past five years. We also include all firm years in which no loan transactions occurred.

The resulting sample comprises all publicly listed firms for which all our non-banking-related variables are available. This corresponds to what we label as our *Compustat sample* in the third panel of Table 2. We then run regression specification (2) on this sample, including all firm-year observations from 1987, and we cluster the standard errors at the firm-year level. Note that we now also include firm-year observations for which all loans-related variables are zero, so that firms with no loan in a given year become the omitted category.

In Table 7, we use the natural logarithm of firm-level total factor productivity (TFP) in year $t+1$ as the dependent variable. We use TFP in $t+1$ because our TFP measure is the result of an estimation, conducted by Imrohoroglu and Tuzel (2014), that uses as input variables capital and labor in period t, which are potentially correlated with our right-hand-side variables. After including transaction-specific and firm-level controls in the second column, we find a significantly positive difference-in-differences estimate of 2.9% for universal-bank loans after 1996. This estimate withstands including state-year fixed effects in the third column. Conversely, in the last column, we find a negative and insignificant difference-in-differences estimate for investment-bank loans after 1996. These results paint a picture that is analogous to the risk estimates. What is more, our estimated treatment effects are relatively long-lived – up to six years – due to the definition of the 5-year window and an additional lag due to the measurement of TFP in year $t+1$.

To show that these increases in productivity also translate into increases in actual investment and market capitalization, we re-run the regressions from Table 7, and use as the dependent variable the natural logarithm of firms' capital expenditure in year t as well as the natural logarithm of firms' market value of equity in year t.[13] The results are

[13] Because we used TFP in year $t+1$ as the outcome variable for the above-mentioned reasons, but used capital expenditure and market capitalization in year t, we verified in untabulated tests that our results for the latter two dependent variables are robust to using their realizations in year $t+1$.

in Appendix Tables A.8 and A.9, respectively, and they demonstrate that our previous findings for TFP are also valid for these measures. Capital expenditure increased by at least 2%, although not all results are robustly significant, and firms' market capitalization increased by at least 9%.[14]

To conclude, we find that universal-bank loans were associated with higher TFP, higher capital expenditure, and higher market capitalization after the 1996 deregulation. These results complement our findings for firm-level risk and guide the economic interpretation. Specifically, our evidence is consistent with firms making risky, productivity-increasing investments along a risk-return frontier subject to a risk-based constraint on external finance. The constraint is, in turn, relaxed by universal-banking deregulation. This implies that there is a real component to the increase in risk documented in this paper. Still, this leaves open the question as to whether the productivity increases are large enough to compensate for higher risk. At the very least, our evidence does not contradict the possibility of firm-level efficiency gains from universal banking.

3.3. Bank-level information acquisition through universal-bank mergers

In this section, we provide evidence that the effects presented above are due to better information on the part of universal banks rather than due to higher revenues from cross-selling.

To do so, we use universal-bank mergers as a source of variation in bank-level information. We follow the identification strategy associated with regression specification (3). We compare firms that contracted with a loan-granting commercial bank and also received an underwriting product from an investment bank, both of which have merged – either with each other (treatment group) or with other banks of complementary scope (control group). In Figure 2, we provide evidence of parallel pre-trends in terms of TFP, capital expenditure, and market capitalization among treatment and control firms before the bank mergers.

In Table 8, we estimate (3), and use firm-level TFP as the dependent variable. As discussed in Section 2.4, the treatment effect is given by β_1, which estimates the differential effect on a firm that received a loan from a commercial bank and received an underwriting service from an investment bank that later merged with that same commercial bank. We compare this to a control group of firms that received a loan from a commercial bank and received an underwriting service from an investment bank that did not merge with that same commercial bank; rather, both banks merged with another bank of complementary scope. Thus, for the treatment group, all explanatory (indicator) variables shown in Table 8 are equal to 1, whereas for the control group, all indicator variables *except* the one associated with β_1 are equal to 1.

Our estimates of β_1 are in the first row of Table 8, and they indicate that TFP

[14] The effects we find for market capitalization are unlikely to be due to equity-raising activities, as universal banks cross-sold loans and debt-underwriting services much more frequently than loans and equity-underwriting services (see first panel of Table 2).

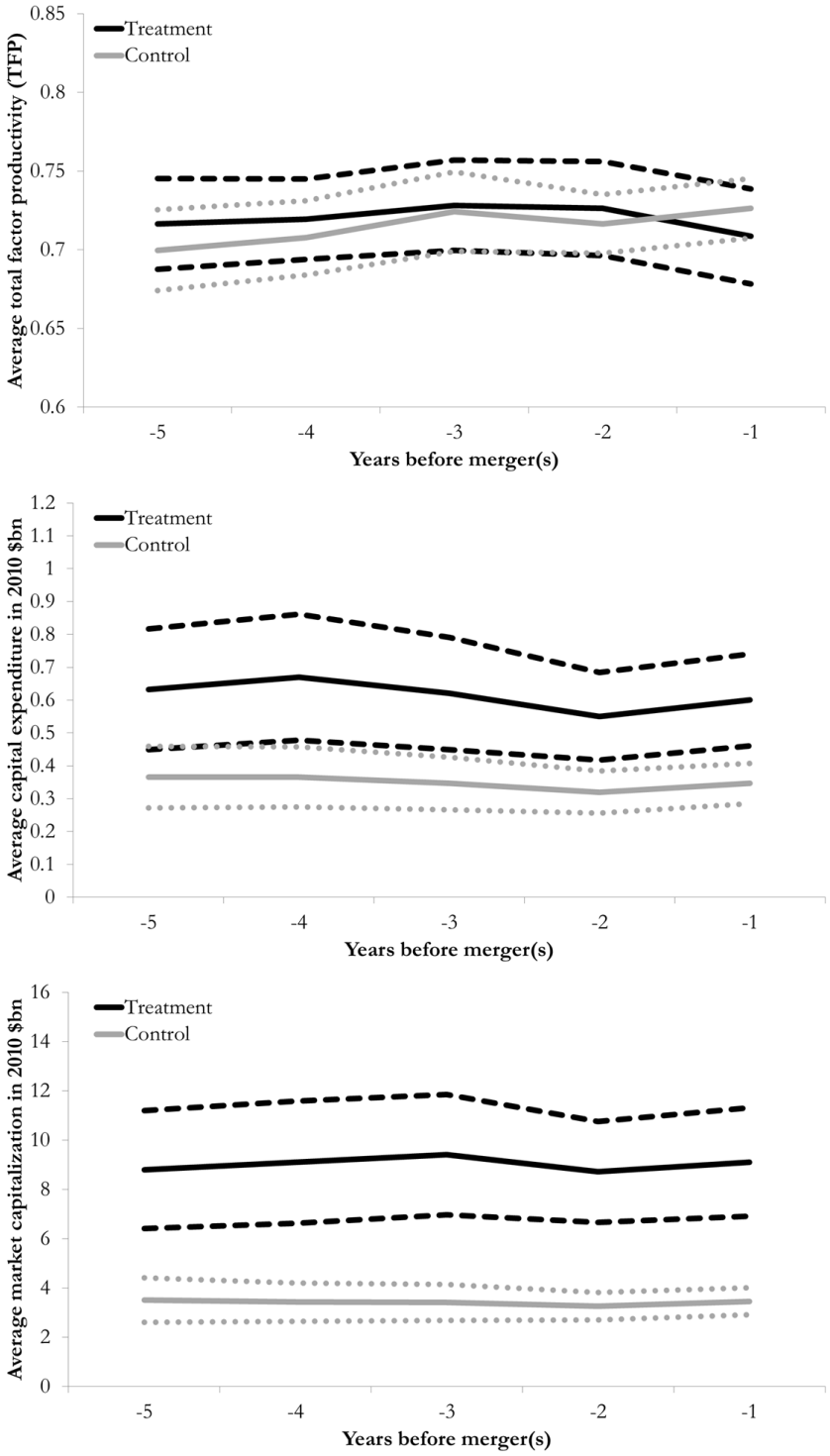

Fig. 2. Pre-trends among treatment and control firms contracting with universal banks. The graphs in the top, middle, and bottom panel plot, respectively, the average TFP, capital expenditure, and market capitalization by firms in the treatment and the control group over five years before the respective universal-bank mergers in year 0. Sources: Compustat, DealScan loan data, as well as SDC underwriting and M&A data.

increased by 2 to 3%, which is similar to the effects shown in Table 7. This result is robust to including state-year fixed-effects in the third column and, in addition, industry-year fixed effects in the last column, which capture other time-varying factors underlying banks' considerations to merge with each other, such as the nature of client portfolios.

We also report positive treatment effects on capital expenditure and market capitalization in Appendix Tables A.10 and A.11. The magnitude is somewhat higher for capital expenditure than in Appendix Table A.8, and it is significant at the 1% level throughout. However, the magnitude is weaker for market capitalization in comparison to Appendix Table A.9, and it loses statistical significance after including state-year fixed effects in the third column of Appendix Table A.11.

These estimates are robust to changing the time window for the triplet of events (loan transaction, underwriting, and a potential merger) from eleven years to nine years (see Appendix Tables A.12 to A.14). In summary, the results based on our alternative universal-bank-mergers identification strategy point to informational economies of scope as the driving force underlying the firm-level real effects of universal banking.

To show that this insight holds also for the risk-increasing effect of bank-scope deregulation, we consider an alternative risk measure, namely option-implied volatility $\sigma_{it}^{implied}$. While the data are available only starting in 1996, this measure has the advantage of allowing the construction of an annual measure, which fits our empirical setup in (3).

The results in Appendix Table A.15 suggest a significantly positive treatment effect on this risk measure, ranging from 5.5% in the first column to 2.3% after including state-year and industry-year fixed effects in the last column. While somewhat weaker in magnitude than our difference-in-differences estimates in Tables 3 to 5, these results should be comparable in their interpretation given the forward-looking nature of option-implied volatility. As argued by Christensen and Prabhala (1998), it does not just subsume information from past-realized volatility; it is also forward looking in the sense that it helps forecast future volatility.

3.4. IPO age of firms with universal banks as bookrunners

The evidence from the loan data suggests that universal-bank-financed firms were more volatile, but the analysis is confined to publicly listed and thus mature firms. We now complement our loans-based analysis with evidence on firms earlier in their life cycle, and scrutinize the impact of universal banking on the age of firms when they go public.

For this IPO-level analysis, we compare the average age of IPOs with universal banks as bookrunners to the average age of IPOs with investment-bank bookrunners before and after 1996. We use the age of firms at the time of their IPOs as a risk measure, following the logic that younger firms are typically riskier (Pastor and Veronesi, 2003 and Schenone, 2010). Examining the effect of universal banking on IPO age may also be a fruitful exercise because previous research by Brown and Kapadia (2007) and Fink et al. (2010) has found that higher idiosyncratic risk in the U.S. stock market was associated with younger firms that went public.

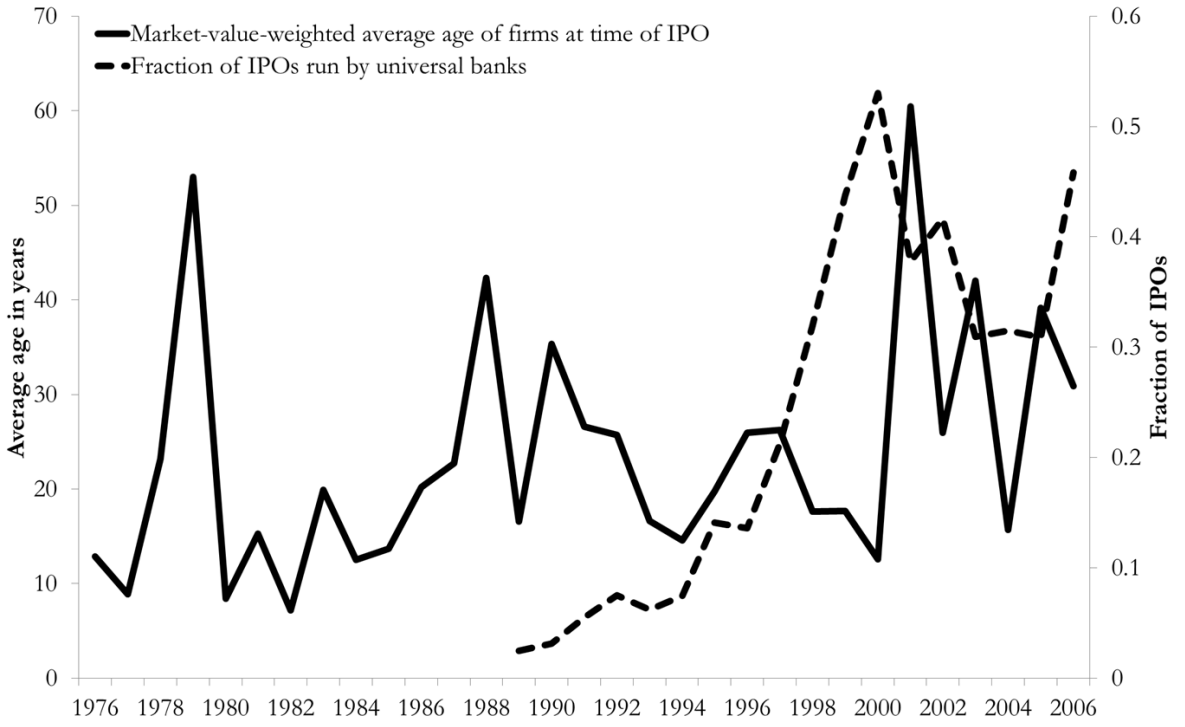

Fig. 3. Market-value-weighted average age of firms at their IPOs vs. fraction of IPOs run by universal banks (1976-2006). Sources: SDC IPOs and firm-age data from Loughran and Ritter (2004).

The 1996 deregulation carries particular significance for the underwriting activities of universal banks. Besides the increased scope for cross-selling, commercial bank divisions could now lend up to 10% of bank capital to securities divisions to cross-finance riskier investment-banking operations. In Figure 3, we plot the market-value-weighted average age of firms at the time of their IPOs and the proportion of IPOs accompanied by universal banks. We observe a negative correlation that is stronger after 1996. Note that the IPO market share of universal banks soared around 1996 as well.

In a difference-in-differences setup akin to that employed before, we test whether following the 1996 deregulation, the firms taken public by universal banks were younger than those taken public by investment banks whose scope of banking activities was unaffected by the deregulation. Given that commercial banks that are not yet universal banks cannot be bookrunners, the control group consists of investment banks. As before, in the presence of bank fixed effects, a universal bank must be established before the deregulation in order to be treated under the 1996 deregulation. We run the following regression specification:

$$\begin{aligned} \ln(IPO\ age_{ijt}) &= \beta_1 UB_j \times After(\text{Aug. 1, 1996})_t + \beta_2 After(\text{Aug. 1, 1996})_t \\ &\quad + \beta_3 X_{ijt} + \beta_4 industry_i + \delta_t + \eta_j + \epsilon_{ijt}, \end{aligned} \tag{5}$$

where $IPO\ age_{ijt}$ is firm i's age in years at the time t of its IPO with bank j as bookrunner, UB_j (*M&A* or *Section 20*) is an indicator variable for whether the bookrunner was a

universal bank (formed through a merger or through opening a Section 20 subsidiary), $After(\text{Aug. 1, 1996})_t$ indicates whether the IPO date was on or after August 1, 1996, X_{ijt} denotes firm and IPO characteristics, and $industry_i$, δ_t, and η_j are industry, year, and bank fixed effects, respectively. Standard errors are clustered at the bookrunner level.

Note that UB_j is not time-varying because commercial banks can act as bookrunners only once they have become universal banks. Therefore, UB_j is subsumed by bank fixed effects. In the first column of Table 9, we estimate (5) without any firm or IPO-specific controls. The difference-in-differences estimate for universal banks compared to the control group of pure investment banks, which is captured by the coefficient on $UB_j \times After(1996)_t$, is significantly negative (at the 1% level). It suggests that universal banks served as bookrunners for IPOs of firms that were 18.5% younger after the deregulation.

The difference-in-differences estimate β_1 increases further in size after the inclusion of firm and IPO-specific controls in the second column, as well as state-year fixed effects (leading to a drop in the sample size due to data availability) in the third column. State-year fixed effects capture any confounding effects of, for instance, bank-branching deregulation.

In the fourth column, we consider a market-structure-based explanation for the younger age of firms that were taken public by universal banks. Commercial banks entering the underwriting business as newly formed universal banks naturally lack a track record for IPOs. This may force them to take younger firms public in an effort to build a track record.

To test this, we include the interaction of UB_j with $IPO\ count_{jt}$, which is the number of IPOs accompanied by the respective universal and investment banks, up to and including the IPO in question (i.e., the IPO of firm i with bookrunner j at time t). If lack of a track record was responsible for our findings, then one would expect the interaction effect to be positive, and this would indicate that universal banks with an established track record of IPOs took older firms public. However, we fail to find a significant differential effect of $IPO\ count_{jt}$ for universal banks. This suggests that the explanatory power of this alternative mechanism is limited in regard to the effects of increased bank scope on IPO age.

In the last column, we delineate the treatment effect by the universal banks' mode of establishment, namely whether the universal bank in question was established through M&A or through opening a Section 20 subsidiary. The difference-in-differences estimates are both negative, but only significantly so for universal banks established through M&A.

In order to evaluate whether these results may be driven by any other characteristics that differ between universal banks established through M&A and Section 20 subsidiaries, we collected key summary statistics for the bank-holding companies in our sample from a year before they became universal banks to a year after they became universal banks. We could not include universal banks for which the data do not cover all three time periods,

such as those that were established just when the data became available (1987) or those that were not the surviving entity following mergers with other banks. As Appendix Table A.16 shows, universal banks established through M&A are typically larger than Section 20 subsidiaries. Such mergers constitute one-time increases in total assets, net income, and the number of employees. Section 20 subsidiaries grow more gradually. Nevertheless, both types of universal banks are strikingly similar in their equity-to-assets and cash-to-assets ratios. As a result, higher risk taking by universal banks established through M&A cannot be readily explained by a different leverage position or excess cash. Loans-to-assets ratios are somewhat higher for universal banks formed through Section 20 subsidiaries, as non-underwriting investment-banking operations are a smaller portion of their business model.

4. Conclusion

In this paper, we focus on a deregulation that expanded the scope of banking in the U.S. We provide evidence that the advent of universal banking improved the access to finance for risky but productive enterprises through informational economies of scope across loans and non-loan products.

Our findings are in accordance with previous research on the evolution of firm-level volatility in the U.S. Based on Campbell et al. (2001), Comin and Philippon (2006) document that idiosyncratic firm risk has been rising over the past 30 years. Our results suggest that bank-scope deregulation may have contributed to this phenomenon. Indeed, the explanation we propose in this paper can accommodate the dichotomy found in Davis et al. (2007) that volatility has been increasing for public firms but not for private firms, because the cross-selling of underwriting products affects primarily public firms. An interesting direction for future research could be to quantify the explanatory power of increased bank scope for the observed run-up in firm-level fluctuations.

In light of recent proposals to limit the scope of banking and to re-establish the Glass-Steagall Act, our evidence suggests that concurrent lending and underwriting of corporate securities may produce firm-level efficiency gains, and these gains should be balanced against the risks associated with banks becoming "too big to fail" and other concerns about macroeconomic fragility.

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Table 1
Timeline of universal banks.

Section 20	M&A
Established before August 1, 1996	
BankBoston (later acquired by Fleet)	Crédit Suisse (First Boston)
Bankers Trust (later acquired by Bank of America)	Deutsche Bank USA
Bank of America	Equitable (later acquired by SunTrust)
Bank of New England (defunct since 1991)	HSBC Bank USA
Bank One (later acquired by J.P. Morgan)	Sovran Bank
BankSouth	(later acquired by NationsBank)
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	
Liberty National Bank	
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	
Security Pacific Bank (later acquired by Bank of America)	
SouthTrust (later acquired by Wachovia/First Union)	
SunTrust	
Established on or after August 1, 1996	
BB&T	Citigroup*
BNY	Wells Fargo
Commerce Bancshares	
CoreStates/Philadelphia National Bank (later acquired by First Union)	
Crestar Bank	
First Tennessee	
KeyBank	
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 by our definition through a series of mergers, most notably with investment banks Smith Barney and Salomon Brothers, and Citicorp had registered a Section 20 subsidiary. Given the size of 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.

Loans sample ($1987-2010$*)*	Mean	Std. dev.	Min	Max	N
Universal-bank (UB) loan	0.641	0.480	0	1	19,053
Investment-bank (IB) loan	0.108	0.311	0	1	19,053
Deal size/assets	0.275	0.475	0.000	39.604	19,053
Refinancing	0.501	0.500	0	1	19,053
No. of lead arrangers	1.122	0.343	1	6	19,053
All-in-drawn spread in bps	186.879	137.681	0.700	1,490.020	16,967
Loan cross-sold by UB or IB	0.791	0.407	0	1	12,061
Cross-sold with debt underwriting	0.851	0.357	0	1	11,863
Cross-sold with equity underwriting	0.190	0.392	0	1	4,008
(all conditional on loan & underwriting)					
No. of UBs M&A					8
No. of UBs M&A before Aug. 1, 1996					6
No. of UBs Section 20					37
No. of UBs Section 20 before Aug. 1, 1996					28
No. of IBs					95
No. of CBs					449
Firm-loan-years sample ($1987-2006$*)*	Mean	Std. dev.	Min	Max	N
$\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$	-0.020	0.850	-3.586	2.656	3,362
$\Delta_t \ln(\sigma(return_i)^{6y})$	0.006	0.390	-2.234	1.759	3,556
$\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$	0.006	0.404	-2.374	1.754	3,556
Bankruptcy in the next ten years	0.234	0.423	0	1	6,393
No. of firms					1,695
No. of firms with multiple relationships					1,442
No. of firms observed with CB/IB,					116
UB before 1996, and UB after 1996					
Firm-loan years associated with firms observed					477
with CB/IB, UB before 1996, and UB after 1996					
Compustat sample ($1987-2010$*)*	Mean	Std. dev.	Min	Max	N
$TFP_{i,t+1}$	0.664	0.344	0.006	9.957	52,435
$CapEx_{it}$ (in 2010 \$bn)	0.173	1.026	0.000	59.283	91,686
$MarketCap_{it}$ (in 2010 \$bn)	2.398	13.842	0.000	780.502	92,665
$\sigma^{implied}_{it}$	0.572	0.384	0.023	5.447	24,779
Sales in 2010 \$bn	1.941	9.655	0.000	430.402	93,181
No. employees in thousands	7.510	34.640	0.001	2100.001	93,181
Loan from CB, underwriting from IB,	0.035	0.184	0	1	93,181
both merged with each other					
Loan from CB that merged with IB	0.318	0.466	0	1	93,181
Underwriting from IB that merged with CB	0.205	0.404	0	1	93,181
IPO sample ($1976-2006$*)*	Mean	Std. dev.	Min	Max	N
IPO age in years	14.371	20.230	0.000	165.000	3,835
UB	0.166	0.372	0	1	3,835
Sales in 2010 \$bn	0.309	1.395	0.000	41.698	3,835
No. of employees in thousands	1.461	6.204	0.001	203.001	3,835
Book-value leverage	0.192	0.209	0.000	0.890	3,835
Gross spread in %	7.484	1.336	0.700	20.250	3,835
IPO count	69.154	100.402	1	582	3,835
No. of UBs M&A					5
No. of UBs M&A before Aug. 1, 1996					5
No. of UBs Section 20					15
No. of UBs Section 20 before Aug. 1, 1996					12
No. of IBs					460

Table 3
Sales-growth volatility of universal-bank-financed firms – firm-loan-years sample.

All regressions are run at the firm-year level it, limited to years in which firm i received at least one loan from one or multiple banks j, where the loans sample consists of all completed syndicated loans of publicly listed firms. For firm-loan year it, $\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$ is the difference between the logged 6-year standard deviation of firm i's sales growth from t to $t+5$ and the same measure from $t-6$ to $t-1$. *Universal-bank loan*$_{jt}$ is an indicator variable for whether at the time of any loan transaction in year t any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the firm's loan year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the ratio of the average deal size across all loans in a given year over firm i's assets, and the average value of the refinancing indicator. Bank fixed effects are included for *all* lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i in a given year. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	$\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$				
	(1)	(2)	(3)	(4)	(5)
Universal-bank loan × After(1996)	0.153***	0.138***	0.179**	0.236***	0.237**
	(0.045)	(0.048)	(0.076)	(0.087)	(0.099)
Universal-bank loan	-0.049	-0.054	-0.043	-0.069	-0.069
	(0.050)	(0.057)	(0.072)	(0.099)	(0.099)
Investment-bank loan × After(1996)					0.004
					(0.157)
Controls	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	N	N	N
State-year FE	N	N	Y	Y	Y
Industry FE	N	Y	Y	N	N
Firm FE	N	N	N	Y	Y
N	3,362	3,362	3,362	3,362	3,362

Table 4
Stock-return volatility of universal-bank-financed firms – firm-loan-years sample.

All regressions are run at the firm-year level it, limited to years in which firm i received at least one loan from one or multiple banks j, where the loans sample consists of all completed syndicated loans of publicly listed firms. For firm-loan year it, $\Delta_t \ln(\sigma(return_i)^{6y})$ is the difference between the logged 6-year standard deviation of firm i's stock returns from t to $t+5$ and the same measure from $t-6$ to $t-1$. *Universal-bank loan*$_{jt}$ is an indicator variable for whether at the time of any loan transaction in year t any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the firm's loan year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the ratio of the average deal size across all loans in a given year over firm i's assets, and the average value of the refinancing indicator. Bank fixed effects are included for *all* lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i in a given year. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	$\Delta_t \ln(\sigma(return_i)^{6y})$				
	(1)	(2)	(3)	(4)	(5)
Universal-bank loan × After(1996)	0.124***	0.110***	0.142***	0.115***	0.104**
	(0.028)	(0.027)	(0.036)	(0.036)	(0.041)
Universal-bank loan	-0.055**	-0.053**	-0.054	-0.020	-0.016
	(0.025)	(0.026)	(0.034)	(0.043)	(0.044)
Investment-bank loan × After(1996)					-0.055
					(0.065)
Controls	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	N	N	N
State-year FE	N	N	Y	Y	Y
Industry FE	N	Y	Y	N	N
Firm FE	N	N	N	Y	Y
N	3,556	3,556	3,556	3,556	3,556

Table 5
Idiosyncratic volatility of universal-bank-financed firms – firm-loan-years sample.

All regressions are run at the firm-year level it, limited to years in which firm i received at least one loan from one or multiple banks j, where the loans sample consists of all completed syndicated loans of publicly listed firms. For firm-loan year it, $\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$ is the difference between the logged 6-year idiosyncratic volatility of firm i's stock returns from t to $t+5$ and the same measure from $t-6$ to $t-1$, estimated from the Fama and French (1993) three-factor model and expressed in annualized terms. *Universal-bank loan*$_{jt}$ is an indicator variable for whether at the time of any loan transaction in year t any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the firm's loan year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the ratio of the average deal size across all loans in a given year over firm i's assets, and the average value of the refinancing indicator. Bank fixed effects are included for *all* lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i in a given year. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	$\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$				
	(1)	(2)	(3)	(4)	(5)
Universal-bank loan × After(1996)	0.077***	0.062**	0.078**	0.095***	0.090**
	(0.029)	(0.028)	(0.034)	(0.036)	(0.042)
Universal-bank loan	-0.039	-0.039	-0.031	-0.044	-0.042
	(0.035)	(0.034)	(0.038)	(0.045)	(0.047)
Investment-bank loan × After(1996)					-0.029
					(0.063)
Controls	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	N	N	N
State-year FE	N	N	Y	Y	Y
Industry FE	N	Y	Y	N	N
Firm FE	N	N	N	Y	Y
N	3,556	3,556	3,556	3,556	3,556

Table 6
Incidence of bankruptcy among universal-bank-financed firms – firm-loan-years sample.

All regressions are run at the firm-year level it, limited to years in which firm i received at least one loan from one or multiple banks j, where the loans sample consists of all completed syndicated loans of publicly listed firms. For firm-loan year it, the dependent variable is an indicator variable for whether the borrowing company went bankrupt (according to CRSP delisting codes) in the ten years following the loan issue (i.e., $t+1$ to $t+10$). *Universal-bank loan*$_{jt}$ is an indicator variable for whether at the time of any loan transaction in year t any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the firm's loan year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the ratio of the average deal size across all loans in a given year over firm i's assets, and the average value of the refinancing indicator. Bank fixed effects are included for *all* lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i in a given year. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	Bankruptcy in the next ten years $\in \{0,1\}$				
	(1)	(2)	(3)	(4)	(5)
Universal-bank loan × After(1996)	0.014	0.031	0.031	-0.038**	-0.016
	(0.022)	(0.024)	(0.022)	(0.017)	(0.017)
Universal-bank loan	-0.045*	-0.029	-0.045*	0.004	-0.003
	(0.025)	(0.023)	(0.025)	(0.015)	(0.015)
Investment-bank loan × After(1996)					0.102***
					(0.023)
Controls	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	N	N	N
State-year FE	N	N	Y	Y	Y
Industry FE	N	Y	Y	N	N
Firm FE	N	N	N	Y	Y
N	6,393	6,393	6,393	6,393	6,393

Table 7
Total factor productivity of universal-bank-financed firms – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level it. $TFP_{i,t+1}$ is firm i's total factor productivity in year $t+1$ from Imrohoroglu and Tuzel (2014). *Universal-bank loan*$_{jt}$ is an indicator variable for whether, given any loans received by firm i from year $t-4$ to t, at the time of any loan transaction any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether, given any loans received by firm i from year $t-4$ to t, any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-4$ to t, and the proportion of refinancing loans from $t-4$ to t. Bank fixed effects are included for all lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i from year $t-4$ to t. State-year fixed effects are based on the location of firm i's headquarters in year t. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(TFP_{i,t+1})$			
	(1)	(2)	(3)	(4)
Universal-bank loan × After(1996)	0.032***	0.029***	0.029***	0.028***
	(0.008)	(0.008)	(0.008)	(0.008)
Universal-bank loan	-0.013*	-0.012*	-0.013*	-0.013*
	(0.007)	(0.007)	(0.007)	(0.007)
Investment-bank loan × After(1996)				-0.018
				(0.012)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
N	52,435	52,435	52,435	52,435

Table 8

Impact of bank information acquisition on total factor productivity – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level *it*. $TFP_{i,t+1}$ is firm i's total factor productivity in year $t+1$ from Imrohoroglu and Tuzel (2014). *Loan from CB that merged with IB*$_{it}$ is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank that merged with an investment bank thereafter. *Underwriting from IB that merged with CB*$_{it}$ is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received an underwriting product from an investment bank that merged with a commercial or universal bank thereafter. The interaction of the latter two indicator variables is to be distinguished from the explanatory variable of interest in the first row, which indicates whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank, an underwriting product from an investment bank, and both banks merged with each other until year t. *Any loan*$_{it}$ and *Any underwriting*$_{it}$ are indicator variables for whether firm i received any loan or any underwriting product, respectively, from any commercial, universal, or investment bank anytime from $t-10$ to $t-1$. Unless mentioned otherwise, control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-10$ to $t-1$, and the proportion of refinancing loans from $t-10$ to $t-1$. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry-year fixed effects are based on one-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(TFP_{i,t+1})$			
	(1)	(2)	(3)	(4)
Loan from CB, underwriting from IB, both merged with each other	0.034***	0.029***	0.021***	0.021**
	(0.008)	(0.008)	(0.008)	(0.008)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	0.007	0.007	0.008	0.007
	(0.010)	(0.010)	(0.010)	(0.010)
Loan from CB that merged with IB	-0.020***	-0.020***	-0.026***	-0.026***
	(0.008)	(0.007)	(0.008)	(0.008)
Underwriting from IB that merged with CB	0.001	-0.008	-0.013	-0.014
	(0.009)	(0.009)	(0.009)	(0.009)
Any loan × Any underwriting	0.029***	0.029***	0.028***	0.028***
	(0.009)	(0.009)	(0.009)	(0.009)
Any loan	-0.021**	-0.034***	-0.032***	-0.030***
	(0.009)	(0.010)	(0.010)	(0.010)
Any underwriting	-0.042***	-0.042***	-0.044***	-0.042***
	(0.007)	(0.007)	(0.007)	(0.007)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
Industry-year FE	N	N	N	Y
N	52,435	52,435	52,435	52,435

Table 9
Universal-bank underwriting and age of firms at their IPOs.

The unit of observation is a firm's IPO. The dependent variable is the log of firm i's age at the time t of its IPO with bank j as bookrunner. UB_j (*M&A* or *Section 20*) is an indicator variable for whether the bookrunner was a universal bank (formed through a merger or through opening a Section 20 subsidiary). $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the IPO date was on or after August 1, 1996. $IPO\ count_{jt}$ denotes the number of IPOs accompanied by universal or investment bank j, up to and including the current IPO. Book-value leverage is winsorized at the 1st and 99th percentiles. All firm-level explanatory variables are measured at the end of the IPO year. Industry fixed effects are based on two-digit SIC codes. State-year fixed effects are based on the location of firm i's headquarters in year t. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bookrunner level) are in parentheses.

	ln(IPO age)				
	(1)	(2)	(3)	(4)	(5)
UB × After(Aug. 1, 1996)	-0.185**	-0.220**	-0.280**	-0.403**	
	(0.091)	(0.088)	(0.129)	(0.164)	
UB × IPO count				0.001	
				(0.001)	
IPO count				-0.000	
				(0.001)	
UB M&A × After(Aug. 1, 1996)					-0.456***
					(0.134)
UB Section 20 × A.(Aug. 1, 1996)					-0.071
					(0.134)
After(Aug. 1, 1996)	-0.092	-0.084	-0.160	-0.143	-0.153
	(0.089)	(0.090)	(0.152)	(0.160)	(0.153)
Log of sales in 2010 $		0.132***	0.125***	0.127***	0.122***
		(0.016)	(0.028)	(0.029)	(0.028)
Log of no. employees		0.118***	0.098***	0.097***	0.101***
		(0.026)	(0.026)	(0.026)	(0.027)
Book-value leverage		0.030	0.053	0.053	0.048
		(0.089)	(0.137)	(0.137)	(0.138)
Gross spread in %		-0.018	-0.096***	-0.096***	-0.097***
		(0.027)	(0.034)	(0.035)	(0.034)
Industry FE	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	N	N	N
State-year FE	N	N	Y	Y	Y
N	3,835	3,835	2,471	2,471	2,471

Online Appendix

to

Do universal banks finance riskier but more productive firms?

by Daniel Neuhann and Farzad Saidi

Table A.1
Sales-growth volatility of universal-bank-financed firms: placebo – firm-loan-years sample.

All regressions are run at the firm-year level it, limited to years in which firm i received at least one loan from one or multiple banks j, where the loans sample consists of all completed syndicated loans of publicly listed firms. For firm-loan year it, $\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$ is the difference between the 6-year standard deviation of firm i's sales growth from t to $t+5$ and the same measure from $t-6$ to $t-1$. *Universal-bank loan*$_{jt}$ is an indicator variable for whether at the time of any loan transaction in year t any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether any of the lead arrangers j was an investment bank. $After(1993)_t$ is an indicator for whether the firm's loan year in question was 1994 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the ratio of the average deal size across all loans in a given year over firm i's assets, and the average value of the refinancing indicator. Bank fixed effects are included for *all* lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i in a given year. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	$\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$				
	(1)	(2)	(3)	(4)	(5)
Universal-bank loan × After(1993)	0.047	0.034	0.116	-0.028	-0.042
	(0.060)	(0.059)	(0.080)	(0.104)	(0.112)
Universal-bank loan	-0.006	-0.010	-0.034	0.056	0.064
	(0.054)	(0.058)	(0.079)	(0.127)	(0.128)
Investment-bank loan × After(1993)					-0.084
					(0.139)
Controls	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	N	N	N
State-year FE	N	N	Y	Y	Y
Industry FE	N	Y	Y	N	N
Firm FE	N	N	N	Y	Y
N	3,362	3,362	3,362	3,362	3,362

Table A.2
Sales-growth volatility of universal-bank-financed firms: robustness – firm-loan-years sample.

All regressions are run at the firm-year level it, limited to years in which firm i received at least one loan from one or multiple banks j, where the loans sample generally consists of all completed syndicated loans of publicly listed firms. Furthermore, firm-loan years in which a given firm received loans from both universal and investment banks are dropped from the sample. For firm-loan year it, $\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$ is the difference between the logged 6-year standard deviation of firm i's sales growth from t to $t+5$ and the same measure from $t-6$ to $t-1$. *Universal-bank loan*$_{jt}$ is an indicator variable for whether at the time of any loan transaction in year t any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the firm's loan year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the ratio of the average deal size across all loans in a given year over firm i's assets, and the average value of the refinancing indicator. Bank fixed effects are included for *all* lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i in a given year. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	$\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$				
	(1)	(2)	(3)	(4)	(5)
Universal-bank loan × After(1996)	0.126***	0.109**	0.152*	0.239***	0.233**
	(0.048)	(0.051)	(0.081)	(0.086)	(0.106)
Universal-bank loan	-0.050	-0.058	-0.038	-0.058	-0.056
	(0.052)	(0.060)	(0.076)	(0.105)	(0.104)
Investment-bank loan × After(1996)					-0.027
					(0.177)
Controls	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	N	N	N
State-year FE	N	N	Y	Y	Y
Industry FE	N	Y	Y	N	N
Firm FE	N	N	N	Y	Y
N	3,306	3,306	3,306	3,306	3,306

Table A.3
Stock-return volatility of universal-bank-financed firms: robustness – firm-loan-years sample.

All regressions are run at the firm-year level it, limited to years in which firm i received at least one loan from one or multiple banks j, where the loans sample generally consists of all completed syndicated loans of publicly listed firms. Furthermore, firm-loan years in which a given firm received loans from both universal and investment banks are dropped from the sample. For firm-loan year it, $\Delta_t \ln(\sigma(return_i)^{6y})$ is the difference between the logged 6-year standard deviation of firm i's stock returns from t to $t+5$ and the same measure from $t-6$ to $t-1$. *Universal-bank loan*$_{jt}$ is an indicator variable for whether at the time of any loan transaction in year t any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the firm's loan year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the ratio of the average deal size across all loans in a given year over firm i's assets, and the average value of the refinancing indicator. Bank fixed effects are included for *all* lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i in a given year. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	$\Delta_t \ln(\sigma(return_i)^{6y})$				
	(1)	(2)	(3)	(4)	(5)
Universal-bank loan × After(1996)	0.134***	0.117***	0.147***	0.125***	0.104**
	(0.028)	(0.027)	(0.037)	(0.038)	(0.043)
Universal-bank loan	-0.057**	-0.056**	-0.056*	-0.033	-0.026
	(0.026)	(0.027)	(0.034)	(0.042)	(0.042)
Investment-bank loan × After(1996)					-0.096
					(0.066)
Controls	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	N	N	N
State-year FE	N	N	Y	Y	Y
Industry FE	N	Y	Y	N	N
Firm FE	N	N	N	Y	Y
N	3,493	3,493	3,493	3,493	3,493

Table A.4
Idiosyncratic volatility of universal-bank-financed firms: robustness – firm-loan-years sample.

All regressions are run at the firm-year level it, limited to years in which firm i received at least one loan from one or multiple banks j, where the loans sample generally consists of all completed syndicated loans of publicly listed firms. Furthermore, firm-loan years in which a given firm received loans from both universal and investment banks are dropped from the sample. For firm-loan year it, $\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$ is the difference between the logged 6-year idiosyncratic volatility of firm i's stock returns from t to $t+5$ and the same measure from $t-6$ to $t-1$, estimated from the Fama and French (1993) three-factor model and expressed in annualized terms. *Universal-bank loan*$_{jt}$ is an indicator variable for whether at the time of any loan transaction in year t any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the firm's loan year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the ratio of the average deal size across all loans in a given year over firm i's assets, and the average value of the refinancing indicator. Bank fixed effects are included for *all* lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i in a given year. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	$\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$				
	(1)	(2)	(3)	(4)	(5)
Universal-bank loan × After(1996)	0.084***	0.067**	0.080**	0.107***	0.087**
	(0.029)	(0.028)	(0.035)	(0.036)	(0.041)
Universal-bank loan	-0.042	-0.043	-0.036	-0.059	-0.053
	(0.036)	(0.035)	(0.038)	(0.043)	(0.043)
Investment-bank loan × After(1996)					-0.090
					(0.058)
Controls	N	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	N	N	N
State-year FE	N	N	Y	Y	Y
Industry FE	N	Y	Y	N	N
Firm FE	N	N	N	Y	Y
N	3,493	3,493	3,493	3,493	3,493

Table A.5
Volatility of universal-bank-financed firms: no investment banks – firm-loan-years sample.

All regressions are run at the firm-year level it, limited to years in which firm i received at least one loan from one or multiple banks j, where the loans sample generally consists of all completed syndicated loans of publicly listed firms. Furthermore, all loans associated with firms that ever received a loan from any investment bank are dropped from the sample. All dependent variables are defined at the firm-loan year it. The dependent variable in the first two columns is $\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$, the difference between the logged 6-year standard deviation of firm i's sales growth from t to $t+5$ and the same measure from $t-6$ to $t-1$. The dependent variable in the third and fourth column is $\Delta_t \ln(\sigma(return_i)^{6y})$, the difference between the logged 6-year standard deviation of firm i's stock returns from t to $t+5$ and the same measure from $t-6$ to $t-1$. The dependent variable in the last two columns is $\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$, the difference between the logged 6-year idiosyncratic volatility of firm i's stock returns from t to $t+5$ and the same measure from $t-6$ to $t-1$, estimated from the Fama and French (1993) three-factor model and expressed in annualized terms. *Universal-bank loan*$_{jt}$ is an indicator variable for whether at the time of any loan transaction in year t any of the lead arrangers j was a universal bank. $After(1996)_t$ is an indicator for whether the firm's loan year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the ratio of the average deal size across all loans in a given year over firm i's assets, and the average value of the refinancing indicator. Bank fixed effects are included for *all* lead arrangers – i.e., all commercial and universal banks – of all loans of firm i in a given year. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	$\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$		$\Delta_t \ln(\sigma(return_i)^{6y})$		$\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$	
	(1)	(2)	(3)	(4)	(5)	(6)
Universal-bank loan × After(1996)	0.106*	0.162**	0.105***	0.163***	0.058*	0.100**
	(0.057)	(0.077)	(0.032)	(0.052)	(0.032)	(0.047)
Universal-bank loan	-0.047	-0.037	-0.066***	-0.082***	-0.055**	-0.066**
	(0.060)	(0.081)	(0.021)	(0.027)	(0.028)	(0.030)
Controls	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Year FE	Y	N	Y	N	Y	N
State-year FE	N	Y	N	Y	N	Y
Industry FE	Y	Y	Y	Y	Y	Y
N	2,732	2,732	2,885	2,885	2,885	2,885

Table A.6
Universal banking and likelihood of cross-selling – loans sample.

The sample consists of all completed syndicated loans (package level) of publicly listed firms, conditional on the borrower firm i of the respective loan granted in year t also receiving an underwriting product from any universal or investment bank (in the first three columns) or from any universal bank only (in the last three columns) anytime from the beginning of year $t-2$ to the end of year $t+2$. The dependent variable is an indicator for whether a given loan in year t was associated with a cross-sold underwriting product by the same bank from $t-2$ to $t+2$. UB_{jt} is an indicator variable for whether at date t of the respective loan any of the lead arrangers j was a universal bank. *UB Section 20*$_{jt}$ is an indicator variable for whether at date t of the respective loan any of the lead arrangers j was a universal bank established through a Section 20 subsidiary, rather than through mergers and acquisitions. $After(\text{Aug. 1, 1996})_t$ is an indicator for whether the loan in question was issued on or after August 1, 1996. Bank fixed effects are included for *all* lead arrangers, i.e., all commercial, universal, and investment banks. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	Cross-sold loan conditional on loan & underwriting $\in \{0,1\}$					
	(1)	(2)	(3)	(4)	(5)	(6)
UB × After(Aug. 1, 1996)	0.076$^{(*)}$	0.079*	0.088***			
	(0.051)	(0.045)	(0.030)			
UB Section 20 × A.(Aug. 1, 1996)				0.095*	0.092*	0.081**
				(0.049)	(0.050)	(0.033)
After(Aug. 1, 1996)	-0.040	-0.042	-0.048	-0.057	-0.052	-0.037
	(0.049)	(0.047)	(0.037)	(0.054)	(0.058)	(0.046)
Log of sales at close	0.031***	0.030***	0.003	0.032***	0.031***	0.004
	(0.005)	(0.005)	(0.007)	(0.005)	(0.006)	(0.008)
Log of no. employees	0.020***	0.021***	0.045***	0.016**	0.017**	0.032***
	(0.006)	(0.006)	(0.013)	(0.006)	(0.007)	(0.010)
Log of deal size/assets	0.052***	0.051***	0.038***	0.049***	0.048***	0.039***
	(0.007)	(0.006)	(0.007)	(0.008)	(0.008)	(0.007)
Refinancing $\in \{0,1\}$	0.016	0.014	0.012	0.010	0.009	0.006
	(0.011)	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)
Bank FE	Y	Y	Y	Y	Y	Y
Year FE	Y	N	N	Y	N	N
State-year FE	N	Y	Y	N	Y	Y
Industry FE	Y	Y	N	Y	Y	N
Firm FE	N	N	Y	N	N	Y
Sample	Universal and investment banks			Universal banks only		
N	12,061	12,061	12,061	10,773	10,773	10,773

Table A.7
Impact of universal-bank financing on loan rates – loans sample.

The sample consists of all completed syndicated loans (package level) of publicly listed firms, subject to availability of the dependent variable. The dependent variable is the natural logarithm of the all-in-drawn spread (in bps), which is the sum of the spread over LIBOR and any annual fees paid to the lender syndicate. *Universal-bank loan*$_{jt}$ is an indicator variable for whether at date t of the respective loan any of the lead arrangers j 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. Bank fixed effects are included for *all* lead arrangers, i.e., all commercial, universal, and investment banks. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

	ln(All-in-drawn spread)		
	(1)	(2)	(3)
Universal-bank loan × After(Aug. 1, 1996)	0.052	0.041	-0.006
	(0.042)	(0.041)	(0.035)
Universal-bank loan	-0.039	-0.024	-0.002
	(0.066)	(0.064)	(0.039)
After(Aug. 1, 1996)	-0.151***	-0.139***	-0.123***
	(0.035)	(0.034)	(0.041)
Log of sales at close	-0.196***	-0.189***	-0.101***
	(0.012)	(0.011)	(0.010)
Log of no. employees	-0.079***	-0.083***	-0.096***
	(0.009)	(0.009)	(0.012)
Log of deal size/assets	0.038***	0.036***	-0.015
	(0.013)	(0.013)	(0.010)
Refinancing $\in \{0, 1\}$	0.053***	0.051***	-0.015
	(0.012)	(0.012)	(0.010)
Bank FE	Y	Y	Y
Year FE	Y	N	N
State-year FE	N	Y	Y
Industry FE	Y	Y	N
Firm FE	N	N	Y
N	16,967	16,967	16,967

Table A.8
Capital expenditure of universal-bank-financed firms – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level *it*. $CapEx_{it}$ is firm i's capital expenditure in year t. *Universal-bank loan*$_{jt}$ is an indicator variable for whether, given any loans received by firm i from year $t-4$ to t, at the time of any loan transaction any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether, given any loans received by firm i from year $t-4$ to t, any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-4$ to t, and the proportion of refinancing loans from $t-4$ to t. Bank fixed effects are included for all lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i from year $t-4$ to t. State-year fixed effects are based on the location of firm i's headquarters in year t. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(CapEx_{it})$			
	(1)	(2)	(3)	(4)
Universal-bank loan × After(1996)	0.037**	0.017	0.023$^{(*)}$	0.023$^{(*)}$
	(0.018)	(0.015)	(0.015)	(0.015)
Universal-bank loan	0.106***	0.039***	0.037***	0.037***
	(0.017)	(0.013)	(0.014)	(0.014)
Investment-bank loan × After(1996)				0.013
				(0.019)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
N	91,686	91,686	91,686	91,686

Table A.9

Market capitalization of universal-bank-financed firms – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level it. $MarketCap_{it}$ is firm i's market value of equity in year t. *Universal-bank loan*$_{jt}$ is an indicator variable for whether, given any loans received by firm i from year $t-4$ to t, at the time of any loan transaction any of the lead arrangers j was a universal bank. *Investment-bank loan*$_j$ is an indicator variable for whether, given any loans received by firm i from year $t-4$ to t, any of the lead arrangers j was an investment bank. $After(1996)_t$ is an indicator for whether the year in question was 1997 or later. Control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-4$ to t, and the proportion of refinancing loans from $t-4$ to t. Bank fixed effects are included for all lead arrangers – i.e., all commercial, universal, and investment banks – of all loans of firm i from year $t-4$ to t. State-year fixed effects are based on the location of firm i's headquarters in year t. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(MarketCap_{it})$			
	(1)	(2)	(3)	(4)
Universal-bank loan × After(1996)	0.098***	0.092***	0.113***	0.112***
	(0.017)	(0.016)	(0.016)	(0.016)
Universal-bank loan	0.060***	0.016	0.007	0.007
	(0.016)	(0.014)	(0.014)	(0.014)
Investment-bank loan × After(1996)				-0.017
				(0.020)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
N	92,665	92,665	92,665	92,665

Table A.10

Impact of bank information acquisition on capital expenditure – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level *it*. $CapEx_{it}$ is firm i's capital expenditure in year t. *Loan from CB that merged with* IB_{it} is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank that merged with an investment bank thereafter. *Underwriting from IB that merged with* CB_{it} is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received an underwriting product from an investment bank that merged with a commercial or universal bank thereafter. The interaction of the latter two indicator variables is to be distinguished from the explanatory variable of interest in the first row, which indicates whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank, an underwriting product from an investment bank, and both banks merged with each other until year t. *Any* $loan_{it}$ and *Any* $underwriting_{it}$ are indicator variables for whether firm i received any loan or any underwriting product, respectively, from any commercial, universal, or investment bank anytime from $t-10$ to $t-1$. Unless mentioned otherwise, control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-10$ to $t-1$, and the proportion of refinancing loans from $t-10$ to $t-1$. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry-year fixed effects are based on one-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(CapEx_{it})$			
	(1)	(2)	(3)	(4)
Loan from CB, underwriting from IB, both merged with each other	0.114***	0.082***	0.057***	0.043***
	(0.019)	(0.014)	(0.014)	(0.014)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.091***	-0.040**	-0.047***	-0.047***
	(0.022)	(0.017)	(0.017)	(0.017)
Loan from CB that merged with IB	0.144***	0.063***	0.054***	0.055***
	(0.016)	(0.013)	(0.013)	(0.013)
Underwriting from IB that merged with CB	0.186***	0.032**	0.036**	0.031**
	(0.019)	(0.015)	(0.015)	(0.015)
Any loan × Any underwriting	-0.003	0.030*	0.029*	0.027*
	(0.019)	(0.016)	(0.016)	(0.016)
Any loan	-0.041**	-0.119***	-0.111***	-0.098***
	(0.019)	(0.016)	(0.016)	(0.016)
Any underwriting	0.207***	0.005	0.003	0.005
	(0.013)	(0.011)	(0.011)	(0.011)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
Industry-year FE	N	N	N	Y
N	91,686	91,686	91,686	91,686

Table A.11
Impact of bank information acquisition on market capitalization – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level *it*. $MarketCap_{it}$ is firm i's market value of equity in year t. *Loan from CB that merged with IB*$_{it}$ is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank that merged with an investment bank thereafter. *Underwriting from IB that merged with CB*$_{it}$ is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received an underwriting product from an investment bank that merged with a commercial or universal bank thereafter. The interaction of the latter two indicator variables is to be distinguished from the explanatory variable of interest in the first row, which indicates whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank, an underwriting product from an investment bank, and both banks merged with each other until year t. *Any loan*$_{it}$ and *Any underwriting*$_{it}$ are indicator variables for whether firm i received any loan or any underwriting product, respectively, from any commercial, universal, or investment bank anytime from $t-10$ to $t-1$. Unless mentioned otherwise, control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-10$ to $t-1$, and the proportion of refinancing loans from $t-10$ to $t-1$. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry-year fixed effects are based on one-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(MarketCap_{it})$			
	(1)	(2)	(3)	(4)
Loan from CB, underwriting from IB, both merged with each other	0.067***	0.042***	0.016	0.011
	(0.019)	(0.016)	(0.016)	(0.016)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.037*	-0.007	-0.002	-0.004
	(0.021)	(0.018)	(0.018)	(0.018)
Loan from CB that merged with IB	0.078***	0.021	0.023*	0.029**
	(0.015)	(0.013)	(0.013)	(0.013)
Underwriting from IB that merged with CB	0.116***	0.012	0.014	0.015
	(0.017)	(0.015)	(0.015)	(0.015)
Any loan × Any underwriting	0.118***	0.142***	0.135***	0.136***
	(0.017)	(0.015)	(0.015)	(0.015)
Any loan	-0.184***	-0.238***	-0.233***	-0.224***
	(0.017)	(0.016)	(0.016)	(0.016)
Any underwriting	-0.040***	-0.181***	-0.176***	-0.170***
	(0.012)	(0.011)	(0.011)	(0.010)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
Industry-year FE	N	N	N	Y
N	92,665	92,665	92,665	92,665

Table A.12
Impact of bank information acquisition on total factor productivity: robustness – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level it. $TFP_{i,t+1}$ is firm i's total factor productivity in year $t+1$ from Imrohoroglu and Tuzel (2014). *Loan from CB that merged with IB*$_{it}$ is an indicator variable for whether anytime from $t-8$ to $t-1$, firm i received a loan from a commercial or universal bank that merged with an investment bank thereafter. *Underwriting from IB that merged with CB*$_{it}$ is an indicator variable for whether anytime from $t-8$ to $t-1$, firm i received an underwriting product from an investment bank that merged with a commercial or universal bank thereafter. The interaction of the latter two indicator variables is to be distinguished from the explanatory variable of interest in the first row, which indicates whether anytime from $t-8$ to $t-1$, firm i received a loan from a commercial or universal bank, an underwriting product from an investment bank, and both banks merged with each other until year t. *Any loan*$_{it}$ and *Any underwriting*$_{it}$ are indicator variables for whether firm i received any loan or any underwriting product, respectively, from any commercial, universal, or investment bank anytime from $t-8$ to $t-1$. Unless mentioned otherwise, control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-8$ to $t-1$, and the proportion of refinancing loans from $t-8$ to $t-1$. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry-year fixed effects are based on one-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(TFP_{i,t+1})$			
	(1)	(2)	(3)	(4)
Loan from CB, underwriting from IB, both merged with each other	0.026***	0.022***	0.016*	0.017*
	(0.008)	(0.008)	(0.009)	(0.009)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.005	-0.002	-0.001	-0.001
	(0.010)	(0.010)	(0.010)	(0.010)
Loan from CB that merged with IB	-0.012	-0.013*	-0.019***	-0.020***
	(0.007)	(0.007)	(0.007)	(0.007)
Underwriting from IB that merged with CB	0.007	-0.002	-0.007	-0.009
	(0.010)	(0.009)	(0.009)	(0.010)
Any loan × Any underwriting	0.025***	0.024***	0.022**	0.022**
	(0.009)	(0.009)	(0.009)	(0.009)
Any loan	-0.024***	-0.037***	-0.035***	-0.032***
	(0.008)	(0.009)	(0.009)	(0.009)
Any underwriting	-0.036***	-0.036***	-0.037***	-0.035***
	(0.006)	(0.006)	(0.006)	(0.006)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
Industry-year FE	N	N	N	Y
N	52,435	52,435	52,435	52,435

Table A.13

Impact of bank information acquisition on capital expenditure: robustness – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level it. $CapEx_{it}$ is firm i's capital expenditure in year t. *Loan from CB that merged with IB*$_{it}$ is an indicator variable for whether anytime from $t-8$ to $t-1$, firm i received a loan from a commercial or universal bank that merged with an investment bank thereafter. *Underwriting from IB that merged with CB*$_{it}$ is an indicator variable for whether anytime from $t-8$ to $t-1$, firm i received an underwriting product from an investment bank that merged with a commercial or universal bank thereafter. The interaction of the latter two indicator variables is to be distinguished from the explanatory variable of interest in the first row, which indicates whether anytime from $t-8$ to $t-1$, firm i received a loan from a commercial or universal bank, an underwriting product from an investment bank, and both banks merged with each other until year t. *Any loan*$_{it}$ and *Any underwriting*$_{it}$ are indicator variables for whether firm i received any loan or any underwriting product, respectively, from any commercial, universal, or investment bank anytime from $t-8$ to $t-1$. Unless mentioned otherwise, control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-8$ to $t-1$, and the proportion of refinancing loans from $t-8$ to $t-1$. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry-year fixed effects are based on one-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(CapEx_{it})$			
	(1)	(2)	(3)	(4)
Loan from CB, underwriting from IB, both merged with each other	0.108***	0.073***	0.052***	0.039***
	(0.020)	(0.015)	(0.015)	(0.015)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.100***	-0.055***	-0.056***	-0.053***
	(0.022)	(0.017)	(0.017)	(0.017)
Loan from CB that merged with IB	0.146***	0.064***	0.056***	0.056***
	(0.015)	(0.012)	(0.013)	(0.013)
Underwriting from IB that merged with CB	0.195***	0.043***	0.045***	0.038***
	(0.019)	(0.015)	(0.015)	(0.015)
Any loan × Any underwriting	-0.020	0.017	0.016	0.014
	(0.018)	(0.015)	(0.015)	(0.015)
Any loan	-0.030*	-0.106***	-0.099***	-0.086***
	(0.018)	(0.015)	(0.015)	(0.015)
Any underwriting	0.196***	0.010	0.008	0.007
	(0.013)	(0.010)	(0.010)	(0.010)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
Industry-year FE	N	N	N	Y
N	91,686	91,686	91,686	91,686

Table A.14
Impact of bank information acquisition on market capitalization: robustness – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level it. $MarketCap_{it}$ is firm i's market value of equity in year t. *Loan from CB that merged with* IB_{it} is an indicator variable for whether anytime from $t-8$ to $t-1$, firm i received a loan from a commercial or universal bank that merged with an investment bank thereafter. *Underwriting from IB that merged with* CB_{it} is an indicator variable for whether anytime from $t-8$ to $t-1$, firm i received an underwriting product from an investment bank that merged with a commercial or universal bank thereafter. The interaction of the latter two indicator variables is to be distinguished from the explanatory variable of interest in the first row, which indicates whether anytime from $t-8$ to $t-1$, firm i received a loan from a commercial or universal bank, an underwriting product from an investment bank, and both banks merged with each other until year t. *Any* $loan_{it}$ and *Any* $underwriting_{it}$ are indicator variables for whether firm i received any loan or any underwriting product, respectively, from any commercial, universal, or investment bank anytime from $t-8$ to $t-1$. Unless mentioned otherwise, control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-8$ to $t-1$, and the proportion of refinancing loans from $t-8$ to $t-1$. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry-year fixed effects are based on one-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(MarketCap_{it})$			
	(1)	(2)	(3)	(4)
Loan from CB, underwriting from IB, both merged with each other	0.061***	0.034**	0.008	0.001
	(0.020)	(0.017)	(0.017)	(0.017)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.069***	-0.042**	-0.028	-0.026
	(0.021)	(0.019)	(0.019)	(0.018)
Loan from CB that merged with IB	0.091***	0.034***	0.035***	0.039***
	(0.014)	(0.013)	(0.013)	(0.013)
Underwriting from IB that merged with CB	0.156***	0.053***	0.050***	0.049***
	(0.017)	(0.015)	(0.015)	(0.015)
Any loan × Any underwriting	0.099***	0.124***	0.116***	0.118***
	(0.017)	(0.015)	(0.015)	(0.015)
Any loan	-0.177***	-0.232***	-0.225***	-0.215***
	(0.016)	(0.015)	(0.015)	(0.015)
Any underwriting	-0.032***	-0.162***	-0.158***	-0.154***
	(0.011)	(0.010)	(0.010)	(0.010)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
Industry-year FE	N	N	N	Y
N	92,665	92,665	92,665	92,665

Table A.15
Impact of bank information acquisition on option-implied volatility – Compustat sample, long-run within-firm effects.

The sample consists of all available observations from Compustat, the unit of observation is the firm-year level *it*. $\sigma_{it}^{implied}$ is firm i's 3-month implied volatility in year t, calculated using the volatility surface from option prices (source: Option Metrics), which is available starting in 1996. *Loan from CB that merged with IB*$_{it}$ is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank that merged with an investment bank thereafter. *Underwriting from IB that merged with CB*$_{it}$ is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received an underwriting product from an investment bank that merged with a commercial or universal bank thereafter. The interaction of the latter two indicator variables is to be distinguished from the explanatory variable of interest in the first row, which indicates whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank, an underwriting product from an investment bank, and both banks merged with each other until year t. *Any loan*$_{it}$ and *Any underwriting*$_{it}$ are indicator variables for whether firm i received any loan or any underwriting product, respectively, from any commercial, universal, or investment bank anytime from $t-10$ to $t-1$. Unless mentioned otherwise, control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-10$ to $t-1$, and the proportion of refinancing loans from $t-10$ to $t-1$. State-year fixed effects are based on the location of firm i's headquarters in year t. Industry-year fixed effects are based on one-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm-year level) are in parentheses.

	$\ln(\sigma_{it}^{implied})$			
	(1)	(2)	(3)	(4)
Loan from CB, underwriting from IB, both merged with each other	0.055***	0.051***	0.027**	0.023**
	(0.011)	(0.011)	(0.011)	(0.011)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.045***	-0.042***	-0.034**	-0.031**
	(0.014)	(0.014)	(0.014)	(0.014)
Loan from CB that merged with IB	-0.005	-0.011	-0.023*	-0.024*
	(0.013)	(0.013)	(0.014)	(0.013)
Underwriting from IB that merged with CB	0.028**	0.032***	0.018	0.015
	(0.012)	(0.012)	(0.012)	(0.012)
Any loan × Any underwriting	0.072***	0.060***	0.052***	0.057***
	(0.020)	(0.019)	(0.020)	(0.019)
Any loan	-0.046**	-0.057***	-0.036*	-0.037*
	(0.021)	(0.021)	(0.022)	(0.021)
Any underwriting	-0.033***	-0.023**	-0.018*	-0.016
	(0.011)	(0.011)	(0.011)	(0.010)
Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	N	N
State-year FE	N	N	Y	Y
Industry-year FE	N	N	N	Y
N	24,779	24,779	24,779	24,779

Table A.16
Summary statistics for universal banks established through M&A and section 20 subsidiaries.

This table reports means with standard deviations in parentheses, for universal banks established through M&A in the first three columns and for Section 20 subsidiaries in the last three columns. The data are based on banks' call reports. t indicates the year of the respective call report: $t = 0$ denotes the first call report after the bank becomes a universal bank, and $t = -1$ and $t = 1$ correspond to the call reports one year before and after the call report used for $t = 0$, respectively. Cash balance is the sum of non-interest-bearing balances and currency and coin, and interest-bearing balances in U.S. offices.

	M&A			Section 20		
	$t = -1$	$t = 0$	$t = 1$	$t = -1$	$t = 0$	$t = 1$
Total assets in 2010 $bn	285.111	580.702	584.812	66.081	74.359	81.684
	(275.350)	(636.615)	(611.766)	(75.150)	(79.660)	(86.138)
Net income in 2010 $bn	3.389	2.562	6.549	0.228	0.105	0.377
	(4.075)	(3.714)	(7.101)	(0.463)	(0.978)	(0.514)
No. employees in thds	76.319	123.431	122.145	16.790	18.973	20.534
	(68.777)	(125.802)	(126.678)	(17.451)	(19.794)	(21.082)
Total equity/assets in %	6.712	6.602	7.485	7.181	7.213	7.442
	(1.137)	(0.846)	(1.131)	(1.839)	(1.860)	(2.236)
Cash balance/assets in %	4.944	5.177	4.776	5.484	5.768	5.338
	(2.571)	(2.764)	(2.040)	(1.663)	(2.320)	(2.027)
Total loans/assets in %	68.451	58.021	55.726	64.663	64.274	63.759
	(6.435)	(15.121)	(12.795)	(6.889)	(8.121)	(8.973)
N		4			30	