Journal of Financial Economics 128 (2018) 66–85

Contents lists available at ScienceDirect

Journal of Financial Economics

journal homepage: www.elsevier.com/locate/jfec

Do universal banks finance riskier but more productive firms?☆

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ARTICLE INFO

Article history:
Received 31 October 2014
Revised 28 October 2016
Accepted 29 November 2016
Available online 8 February 2018

JEL classification:
E20
G20
G21

Keywords:
Universal banking
Financial deregulation
Bank scope
Firewalls
Cross-selling

ABSTRACT

Using variation in bank scope generated by the stepwise repeal of the Glass–Steagall Act in the US, 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.

1. Introduction

In this paper, we use the stepwise repeal of the Glass–Steagall Act in the US to empirically evaluate the effect of bank-scope deregulation on the performance of bank-dependent firms. In doing so, we seek to measure 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

☆ We thank Bo Becker (discussant), Harold L. Cole, Alexandra Effenberger, Xavier Gabaix, Itay Goldstein (discussant), Alessandro Lizzeri, Ulrike Malmendier, David Martinez-Miera (discussant), Hamid Mehran, Anthony Saunders, Philipp Schnabl, Sophie Shive (discussant), Per Strömberg, Vikrant Vig, Alexander Wagner, Jeffrey Wurgler and Alminas Žaldokas, as well as seminar participants at New York University Stern School of Business, New York University (Department of Economics), Federal Reserve Bank of Boston, HEC Paris, London Business School, Cambridge Judge Business School, University of Cambridge (Faculty of Economics), Stockholm School of Economics, Brown University, Federal Reserve Bank of New York, University of Illinois at Urbana-Champaign, Federal Reserve Board of Governors, Federal Reserve Bank of Philadelphia, Einaudi Institute for Economics and Finance Rome, Collegio Carlo Alberto, University of Amsterdam, University of Wisconsin at Madison, Brandeis University, the 2014 Federal Deposit Insurance Corporation and Journal of Financial Services Research Annual Bank Research Conference, the 2014 Annual Cambridge-Princeton Conference, the 2014 Bocconi-Center for Applied Research in Finance International Banking Conference, the 2015 European Winter Finance Conference, and the 2015 Swiss Winter Conference on Financial Intermediation for their comments and suggestions.

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https://doi.org/10.1016/j.jfineco.2018.01.011
0304-405X/

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 et al., 2010). As a result, volatile ventures are the marginal projects that stand to benefit the most from reduced informational asymmetries under universal banking. We conclude that 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 (TFP), 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 with 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.

Fig. 1 illustrates our findings. We plot the loan-weighted average six-year sales-growth volatility of public firms in the US that received loans from commercial and universal banks. In Panel A, 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 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. After 1996, the firm-level risk associated with cross-sold universal bank loans exceeds that of non-cross-sold loans. In Panel B, 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 gains 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, not 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. 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: (1) those firms whose lender and underwriter merged with each other and (2) 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 and 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. 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.

Fig. 1. Loan-weighted average six-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, with cross-sold loans 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 Securities Data Company (SDC) underwriting data.

To this end, we analyze the age of firms in IPOs run by universal banks compared with 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 US stock market.

In summary, our paper shows 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.

1.1. 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 US. 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 instead of branching deregulation, we provide evidence of increasing volatility and productivity. We also employ a different identification strategy than is usually found 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 external finance instead of 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 little evidence exists 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, and 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 preexisting 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, 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 show 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 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.

2.1. 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 left 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 on April 30, 1987, commercial banks were allowed to open 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, firewalls were still 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. 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

[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.

[2] 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.

[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.

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. The removal of informational firewalls interacted with cross-selling in a meaningful way, as it allowed for the possibility of sharing nonpublic 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.

2.2. Hypothesis development

Our basic premise is that the 1996 deregulation boosted cross-selling by universal banks, and cross-selling in turn represents a positive shock to the quality of banks' information about borrower firms (Kanatas and Qi, 1998; 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. Thus, 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, Hypothesis 1 follows:

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. Hypothesis 2 results:

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

We 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 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. 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:

$$y_{ijt} = \beta_1 \mathit{Universal\ bank\ loan}_{jt} \times \mathit{After}(1996)_t + \beta_2 \mathit{Universal\ bank\ loan}_{jt} + \beta_3 X_{it} + \delta_t + \eta_j + \epsilon_{ijt}, \quad (1)$$

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 one 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 with pure commercial or investment banks before and after 1996. 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

[4] This specific regulatory event period culminated in the Federal Reserve Board's announcement on August 22, 1997 of replacing further firewalls. 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.

[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).

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.

Because the difference-in-differences estimate is at the bank-year level *jt*, we cannot include bank-year fixed effects. 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 can even be intertwined; that is, 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 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 their 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 or 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.

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 due to informational economies of scope, not 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).

To determine whether a bank was a universal bank at the time of a given loan transaction, we compare the transaction date with 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 history 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 that 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 Eq. (1) on the sample of firm-loan years to estimate the riskiness of borrowers contracting

[6] Our sample also includes US 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 US before 1978 and were established as universal banks outside the US were allowed to continue their business model in the US (as long as they would not expand their activities further). None of the banks in our sample was 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 US investment bank First Boston Corporation in December 1988.

Table 1
Timeline of universal banks.

Bank	Section 20	M&A
Established before August 1, 1996		
BankBoston (later acquired by Fleet)	×	
Bankers Trust (later acquired by Bank of America)	×	
Bank of America	×	
Bank of New England (defunct since 1991)	×	
Bank One (later acquired by J.P. Morgan)	×	
BankSouth	×	
Barnett Bank (later acquired by NationsBank)	×	
Chase Manhattan (later acquired by J.P. Morgan)	×	
Chemical Bank (later acquired by Chase Manhattan)	×	
Citicorp[a]	×	
Crédit Suisse (First Boston)		×
Dauphin Deposit Corp.	×	
Deutsche Bank USA		×
Equitable (later acquired by SunTrust)		×
First Chicago NBD	×	
First Union	×	
Fleet (later acquired by Bank of America)	×	
HSBC Bank USA		×
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)	×	
Sovran Bank (later acquired by NationsBank)		×
SunTrust	×	
Travelers Group[a]		×
Established on or after August 1, 1996		
BB&T	×	
BNY	×	
Citigroup[a]		×
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)	×	
Wells Fargo		×

[a] 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.

with universal banks before versus after the 1996 deregulation. We use as the dependent variable the difference between a logged six-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 of data for their calculation, such as firms' total factor productivity, we add all firm years (from Compustat) without any loan transactions. As changes in productivity perhaps do 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:

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

where y_{it} is the natural log of firm *i*'s outcome in year *t*, *Universal bank loan*$_{jt}$ is an indicator variable that is equal to one 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 one 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, 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:

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

where y_{it} is the natural log of firm *i*'s outcome in year *t* and *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 one 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., whether the investment bank previously had an underwriting relationship with firm *i* is undetermined). The indicator variable *Underwriting from IB that merged with CB*$_{it}$ is equal to one 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., whether the commercial or universal bank previously had a lending relationship with firm *i* is undetermined). The indicator variables *Any loan*$_{it}$ and *Any underwriting*$_{it}$ are equal to one 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 covers 11 years, so we can realistically accommodate the triplet of events (loan transaction, underwriting, and any mergers). Our ten-year window for the two transaction types (loan and underwriting) ends in year $t-1$, not in 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 they 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, and 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 one for both treatment and control, and *Loan from CB, underwriting from IB, both merged with each other*$_{it}$ is equal to one 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*$_{it}$ × *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*$_{it}$ × *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

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

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; Ljungqvist et al., 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. In the control group, 52.1% (59.8%) returned to either 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 is 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, Center for Research in Security Prices (CRSP) stock prices, DealScan loan data, and Securities Data Company (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 US in the DealScan database since 1987 and US IPOs listed in the SDC database since 1976. We focus on the lead arrangers of syndicated loans. 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 data sets. To identify mergers between any two banks in DealScan loan data and SDC underwriting data, we use the SDC mergers and acquisitions (M&A) database in conjunction with mergers obtained through a LexisNexis news search.

2.5.1. Outcome variables

Firm-level risk measures are among the most important outcome variables considered in this paper. We focus primarily on the six-year volatility of sales-growth rates γ_{it} of firm i in year t. We use a six-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:

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

where x_{it} denotes sales from Compustat.

Using these growth rates, we obtain the six-year standard deviation of firm i's sales growth over six years, $\sigma(\widehat{sales_i})^{6y}$. We also consider, as alternative measures of firm-level risk associated with loans, six-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 three-month implied volatilities calculated using the volatility surface from option prices, which are obtained from OptionMetrics 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).

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. We also use, as alternative outcome variables, capital expenditure (from Compustat) and market capitalization (i.e., the market value of equity) from CRSP.

2.5.2. 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. On this basis, we generate the firm-loan-years sample which contains 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 has 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 five-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, and only 19% of loans associated with concurrent equity underwriting were cross-sold.

We also give an overview of the number of banks in DealScan. 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 Panel B, we move to the firm-loan-years sample, which summarizes all loans that a given firm received in

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

Table 2
Summary statistics.

Panel A: Loans sample (1987 – 2010)	Mean	Standard deviation	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
Number of lead arrangers	1.122	0.343	1	6	19,053
All-in-drawn spread in basis points	186.879	137.681	0.700	1490.020	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	4008
(all conditional on loan & underwriting)					
Number of UBs M&A					8
Number of UBs M&A before Aug. 1, 1996					6
Number of UBs Section 20					37
Number of UBs Section 20 before Aug. 1, 1996					28
Number of IBs					95
Number of CBs					449
Panel B: Firm-loan-years sample (1987 – 2006)					
$\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$	−0.020	0.850	−3.586	2.656	3362
$\Delta_t \ln(\sigma(return_i)^{6y})$	0.006	0.390	−2.234	1.759	3556
$\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$	0.006	0.404	−2.374	1.754	3556
Bankruptcy in the next ten years	0.234	0.423	0	1	6393
Number of firms					1695
Number of firms with multiple relationships					1442
Number of firms observed with CB/IB, UB before 1996, and UB after 1996					116
Firm-loan years associated with firms observed with CB/IB, UB before 1996, and UB after 1996					477
Panel C: Compustat sample (1987 – 2010)					
$TFP_{i,t+1}$	0.664	0.344	0.006	9.957	52,435
$CapEx_{it}$ (in billions of 2010 dollars)	0.173	1.026	0.000	59.283	91,686
$MarketCap_{it}$ (in billions of 2010 dollars)	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 billions of 2010 dollars	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, both merged with each other	0.035	0.184	0	1	93,181
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
Panel D: IPO sample (1976 – 2006)					
IPO age in years	14.371	20.230	0.000	165.000	3835
UB	0.166	0.372	0	1	3835
Sales in billions of 2010 dollars	0.309	1.395	0.000	41.698	3835
Number of employees in thousands	1.461	6.204	0.001	203.001	3835
Book-value leverage	0.192	0.209	0.000	0.890	3835
Gross spread in percent	7.484	1.336	0.700	20.250	3835
IPO count	69.154	100.402	1	582	3835
Number of UBs M&A					5
Number of UBs M&A before Aug. 1, 1996					5
Number of UBs Section 20					15
Number of UBs Section 20 before Aug. 1, 1996					12
Number of IBs					460

a year. For firm-loan year *it*, $\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$ is the difference between the logged six-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 six-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 six-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 3–5.

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 six-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. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	$\Delta_t \ln(\sigma(\widehat{sales_i})^{6y})$				
Variable	(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	No	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	No	No
State-year fixed effects	No	No	Yes	Yes	Yes
Industry fixed effects	No	Yes	Yes	No	No
Firm fixed effects	No	No	No	Yes	Yes
N	3362	3362	3362	3362	3362

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 six-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. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	$\Delta_t \ln(\sigma(return_i)^{6y})$				
Variable	(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	No	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	No	No
State-year fixed effects	No	No	Yes	Yes	Yes
Industry fixed effects	No	Yes	Yes	No	No
Firm fixed effects	No	No	No	Yes	Yes
N	3556	3556	3556	3556	3556

For our Compustat sample in Panel C, we merge our DealScan data with Compustat data starting in 1987, including firms that never received loans recorded in DealScan. The first four variables listed in Panel C correspond to the dependent variables in Table 7 and Online Appendix Tables O8, O9, and O15. 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 $\sigma_{it}^{implied}$ 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 Panel D is limited to IPOs with no more than one bookrunner, leaving a regression sample of 3835 IPOs. This sample is conditional on the availability

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 six-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. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	$\Delta_t \ln(\sigma^{6y}_{idiosyncratic,i})$				
Variable	(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	No	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	No	No
State-year fixed effects	No	No	Yes	Yes	Yes
Industry fixed effects	No	Yes	Yes	No	No
Firm fixed effects	No	No	No	Yes	Yes
N	3556	3556	3556	3556	3556

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, and the remaining firms were taken public by universal banks. In the SDC IPO data, all 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 Eq. (1), and we use as the dependent variable the borrower firms' percent change in six-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 Column 2, 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 universal banks before the deregulation. As in Panel A of Table 2, this applies to three-quarters of all universal banks.

This already substantial effect increases to 17.9% in Column 3 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, captures 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 Column 4, 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 in Panel B of Table 2. Our sample consists of 3362 firm-loan years and 1695 firms, out of which 1442 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 Panel A 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 Column 4 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 Column 5 suggests that investment banks did not finance differentially risky firms compared with commercial banks. The estimate is, however, significantly lower than the difference-in-differences estimate for universal banks.

As a robustness check, we test whether any notable pre-trends exist in universal banks' lending behavior. In particular, we replace $After(1996)_t$ by a placebo year, 1993. The difference-in-differences estimates in Online Appendix Table O1 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 they 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, determining which type of bank the increases in firm-level volatility can be attributed to is difficult. In Online Appendix Tables O2–O4, 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 3362 (3556) 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 columns of Tables 3–5), our estimates are robust to dropping all firms with investment bank loans across all three volatility measures in Online Appendix Table O5.

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 Online Appendix Table O6, 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 five-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 the dependent variable an indicator for whether the loan and the underwriting product were issued by the same bank.

In the first three columns of Online Appendix Table O6, we employ the same fixed effects structure as in the second to fourth columns of Tables 3–5. We find that, following the 1996 deregulation, universal banks were 8 to 9 percentage points more likely to cross-sell than investment banks.[10] For only universal banks and their mode of establishment, in Columns 3–5, universal banks established through Section 20 subsidiaries, not through M&A, were 8–10 percentage points more likely to cross-sell after 1996, which could be due to their early specialization in corporate securities underwriting, instead of any other investment-banking operations.

To characterize the source of higher firm-level volatility, we 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 the dependent variable an indicator for whether the borrowing company went bankrupt in the ten years following the loan-issue year.[11] Our results are robust to variations in the horizon. 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 Column 4, universal bank loans were associated with significantly less bankruptcy risk after 1996. Furthermore, in Column 5, following the 1996 deregulation, investment banks, unlike commercial and universal banks, financed firms that were at least 10 percentage points more

[9] This can 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.

[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 all-in-drawn spread, which is the sum of the spread over the London Interbank Offered Rate (LIBOR) and any annual fees paid to the lender syndicate (see Online Appendix Table O7).

[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 noncompliance with rules of) float or assets; company request, liquidation; bankruptcy, declared insolvent; delinquent in filing; nonpayment 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).

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. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	Bankruptcy in the next ten years $\in \{0, 1\}$				
Variable	(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	No	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	No	No
State-year fixed effects	No	No	Yes	Yes	Yes
Industry fixed effects	No	Yes	Yes	No	No
Firm fixed effects	No	No	No	Yes	Yes
N	6393	6393	6393	6393	6393

likely to be delisted for bankruptcy-related reasons within ten years after the loan issue. This effect is significant at the 1% level.

These results also address another concern. 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 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 six-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 measures related to firm-level risk only 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 Section 3.1. The only difference is that we estimate long-run within-firm effects on annual observations instead of on six-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 contains all publicly listed firms for which all our non-banking-related variables are available. This corresponds to our Compustat sample in Panel C of Table 2. We then run regression specification Eq. (2) on this sample, including all firm-year observations from 1987, and we cluster the standard errors at the firm-year level. 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 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 Column 2, 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 Column 3. Conversely, in Column 4, 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 five-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*.[12] The results

[12] Because we use TFP in year $t+1$ as the outcome variable for the above-mentioned reasons, but use capital expenditure and market capitalization in year *t*, we verify in untabulated tests that our results for the

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, and 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. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	$\ln(TFP_{i,t+1})$			
Variable	(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	No	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	No
State-year fixed effects	No	No	Yes	Yes
N	52,435	52,435	52,435	52,435

are in Online Appendix Tables O8 and O9, 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%.[13]

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. 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 a real component exists to the increase in risk shown 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 subsection, 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.

latter two dependent variables are robust to using their realizations in year $t+1$.

[13] 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 Panel A of Table 2).

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 Eq. (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 Fig. 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 Eq. (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 with 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 (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 one. For the control group, all indicator variables except the one associated with β_1 are equal to one.

Our estimates of β_1 are in the first row of Table 8, and they indicate that TFP increased by 2–3%, which is similar to the effects shown in Table 7. This result is robust to including state-year fixed effects in Column 3 and, in addition, industry-year fixed effects in Column 4, 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 Online Appendix Tables O10 and O11. The magnitude is somewhat higher for

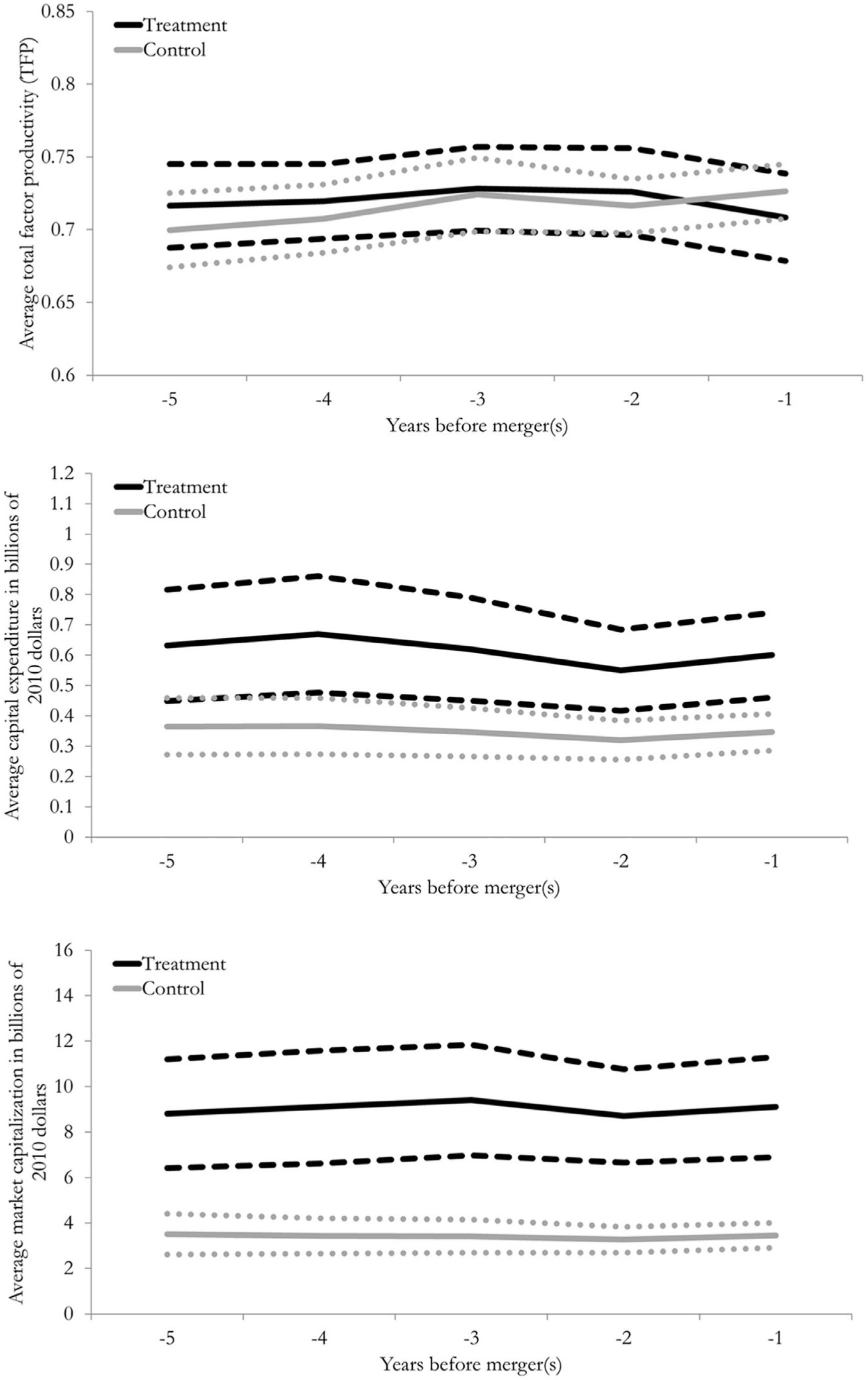

Fig. 2. Pre-trends among treatment and control firms contracting with universal banks. The graphs in Panels A, B, and C plot, respectively, the average TFP, capital expenditure, and market capitalization by firms in the treatment and the control group over five years before the respective universal bank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

capital expenditure than in Online Appendix Table O8, and it is significant at the 1% level throughout. However, the magnitude is weaker for market capitalization in comparison with Online Appendix Table O9, and it loses statistical significance after including state-year fixed effects in the third column of Online Appendix Table O11.

These estimates are robust to changing the time window for the triplet of events (loan transaction, underwriting, and potential merger) from 11 years to nine years (see Online Appendix Tables O12–O14). In summary, the results based on our alternative universal bank mergers identification strategy point to informational economies of scope as

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, and 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 these 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. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	$\ln(TFP_{i,t+1})$			
Variable	(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	No	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	No
State-year fixed effects	No	No	Yes	Yes
Industry-year fixed effects	No	No	No	Yes
N	52,435	52,435	52,435	52,435

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 starting only in 1996, this measure has the advantage of allowing the construction of an annual measure, which fits our empirical setup in Eq. (3).

The results in Online Appendix Table O15 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 3–5, 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 with 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; Schenone, 2010). Examining the effect of universal banking on IPO age can also be a fruitful exercise because Brown and Kapadia (2007) and Fink et al. (2010) find that higher idiosyncratic risk in the US stock market is associated with younger firms that go public.

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 Fig. 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

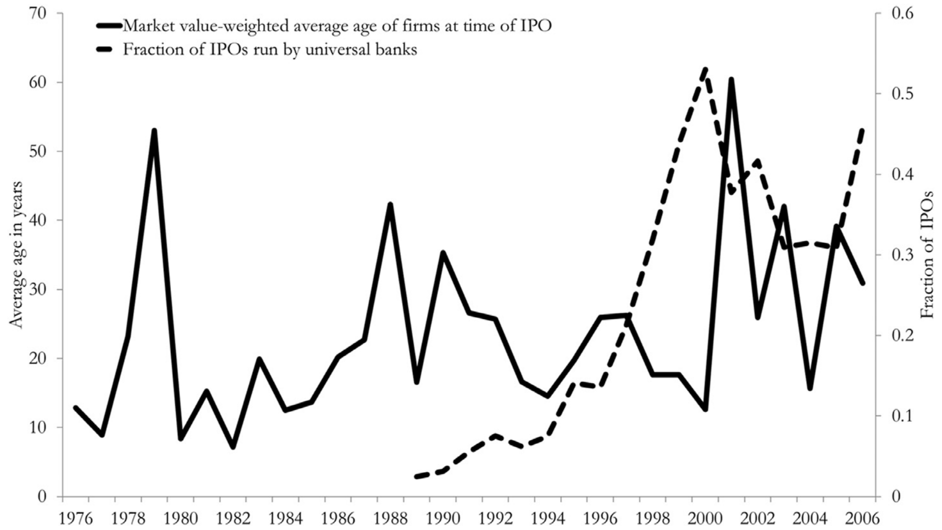

Fig. 3. Market value-weighted average age of firms at their IPOs versus fraction of IPOs run by universal banks (1976 – 2006). Sources: SDC IPOs and firm-age data from Loughran and Ritter (2004).

after 1996. 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 to be treated under the 1996 deregulation. We run the following regression specification:

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

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 $\mathit{industry}_i$, 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 Column 1 of Table 9, we estimate Eq. (5) without any firm or IPO-specific controls. The difference-in-differences estimate for universal banks compared with 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 Column 2 as well as state-year fixed effects (leading to a drop in the sample size due to data availability) in Column 3. State-year fixed effects capture any confounding effects of, for instance, bank-branching deregulation.

In Column 4, 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 could 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, indicating 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 Column 5, 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 significantly so only for universal banks established through M&A.

To evaluate whether these results could be driven by any other characteristics that differ between universal banks established through M&A and Section 20 subsidiaries, we collect 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

Table 9
Universal-bank underwriting and age of firms at their initial public offerings.
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. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	ln(IPO age)				
Variable	(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 × *After*(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 dollars		0.132***	0.125***	0.127***	0.122***
		(0.016)	(0.028)	(0.029)	(0.028)
Log of number of 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 percent		−0.018	−0.096***	−0.096***	−0.097***
		(0.027)	(0.034)	(0.035)	(0.034)
Industry fixed effects	No	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	No	No
State-year fixed effects	No	No	Yes	Yes	Yes
N	3835	3835	2471	2471	2471

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 Online Appendix Table O16 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 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 US. 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 US. Based on Campbell et al. (2001) and Comin and Philippon (2006) show that idiosyncratic firm risk has been rising over the past 30 years. Our results suggest that bank-scope deregulation could have contributed to this phenomenon. The explanation we propose 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 reestablish the Glass–Steagall Act, our evidence suggests that concurrent lending and underwriting of corporate securities could 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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