How Does Firms' Innovation Disclosure Affect Their Banking Relationships?*

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Abstract

Firms face a trade-off between patenting, thereby disclosing innovation, and secrecy. We show that this trade-off interacts with firms' financing choices, as public-information provision through patents and private information in financial relationships are substitutes. As a shock to innovation disclosure, we study the American Inventor's Protection Act that made firms' patent applications public 18 months after filing, rather than when granted. Such increased innovation disclosure helped firms switch lenders, resulting in lower cost of debt. Our evidence lends support to the idea that lenders derive rents from informational monopolies when firms seek to finance innovation.

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1 Introduction

The value added by relationships in economic partnerships and other transactions is inevitably linked to the degree of information asymmetry between the contracting parties. The information environment is an important determinant of the relationship quality and stability. Superior private information, compared to publicly available information, is thought to be a competitive advantage in many markets.

In this context, much attention has been given to financing relationships, especially in the literature on financial intermediation (e.g., Diamond (1984)), going so far as to hypothesize that solving problems of asymmetric information may be the raison d'être of banks (Boot (2000)). We test this conjecture by relating fluctuations in the value of private information to the depth and stability of banking relationships.

In particular, we focus on firms' innovation disclosure through patents and the associated signaling value in loan contracting. We do so for the following reasons. In lending and other relationships, private information about borrowers is valuable because it is costly to acquire. This holds all the more true for extremely uncertain investments for which borrowers seek financing, such as corporate innovation. One channel through which information about innovation is disclosed is patenting that aims to protect innovators' intellectual property. However, such disclosure comes at the potential cost of competitors obtaining certain technical knowledge. Thus, firms often need to take a decision whether to patent their innovation or to keep it secret.[1]

In this paper, we argue that innovating firms face an interplay between the patenting-

[1] Bankers often acknowledge that information about corporate innovation is relevant in lending decisions as it provides a better understanding of the potential of a firm's business. For instance, a report published in 2003 by the Intellectual Property Office (the patent office of the United Kingdom), titled "Banking on IP? The role of intellectual property and intangible assets in facilitating business finance," quotes Richard Holden, Head of Manufacturing at Lloyds Banking Group, saying that "at least when it comes to understanding a company's overall position, [intellectual property] may provide comfort between doing something or not. It doesn't necessarily follow [...] that lending will increase or be directly assigned to the IP, but it might make the difference between lending and not lending. The benefits would include a better understanding of the customer, to inform lending decisions. If the credit team has confidence that relationship managers have 'dug beneath the surface' of a business, they will have a lot more comfort in offering terms."

secrecy trade-off and their banking relationships. As in Bhattacharya and Ritter (1983), the disclosure about firms' technological progress is relevant for their financing choices, assuming it provides a credible signal about their innovation process. This shapes firms' trade-off between patenting and secrecy insofar as "it is not possible to disclose technological information to potential investors without competing firms becoming aware of this knowledge."[2]

Our study hinges on an empirical setting that uses variation in innovation disclosure as a shock to the value of private information in banking relationships. The analysis is based on the premise that tighter bank-firm relationships reduce informational asymmetry between lenders and borrowers through private-information acquisition, whereas patents produce public information through innovation disclosure.

To estimate the effect of innovation disclosure on the stability of lending relationships, we exploit the American Inventor's Protection Act of 1999 (AIPA) as a source of variation in the disclosure of patent applications. The value of such relationships should be linked to the level of private information (about innovation and other activities) between borrowers and lenders. Therefore, break-ups and switching of lending relationships indicate a drop in the relative value of private information, as compared to public information that is exogenously disclosed to markets due to AIPA.

Prior to the passage of AIPA (see Johnson and Popp (2003), Graham and Hegde (2015), and Hegde and Luo (2017) for a detailed description of the event), information about patents became public only after they were granted, over two years on average after filing. Firms could therefore delay revealing the content of their patents. In contrast, AIPA made patent applications public 18 months after the filing date, even for patents that were not granted eventually. In the pre-AIPA era, industries differed in the time lag between patent applications and grant dates. Thus, we define the cross-sectional intensity of AIPA's passage based on this delay. We argue that industries with longer lags between application filing and patent disclosure before AIPA were more heavily affected by its passage. For the validity of our identification strategy, any such pre-AIPA delay measure must not be – and we show it

[2] A similar point is made by Bhattacharya and Chiesa (1995) as well as Yosha (1995).

is not – correlated with cross-industry variation in access to finance or other characteristics that might influence banking relationships in other ways than through innovation disclosure.

After controlling for shocks to firm-level demand and bank-level supply of loans, we find that firms in industries that were affected more heavily in their time to innovation disclosure following AIPA were significantly more likely to break up their existing banking relationships and switch to other lenders. This suggests that after the publicity of firms' innovation increases, the value of formerly private information in banking relationships drops, thereby allowing firms to switch to lenders whose informational disadvantage compared to the incumbent lender is subsequently reduced. Switching appears to have been voluntary and beneficial for firms in treated industries, as we find that the cost of debt drops for switching firms. Importantly, our results are not driven at all by high-tech companies that went through the dot-com boom and bust around the same time.

Due to the 18-month rule, AIPA likely imposed innovation disclosure at a level that firms would not have supported otherwise, but they in return gained the ability to switch lenders. Our evidence therefore suggests that innovation disclosure and private information acquired in banking relationships are substitutes. To the extent that the feasibility of switching lenders depends on the latter's joint reaction to new information about borrowers, our paper is closely related to Hertzberg, Liberti, and Paravisini (2011). They argue, and provide evidence, that a change in credit reporting and the associated higher level of public-information disclosure enable creditors to coordinate their lending decisions when firms are close to financial distress. An important alternative would be that the change in credit reporting reduces lenders' rents from an informational monopoly. Hertzberg, Liberti, and Paravisini (2011) rule out this explanation by showing that borrowers with a single lender are not affected.

The coordination channel is unlikely to thrive in our setting, because we use variation in a type of corporate disclosure the cost of which is not necessarily the disclosure of negative news but, instead, linked to product-market considerations. We show that when such innovation-related information is disclosed, the value of private information acquired by incumbent lenders drops and firms switch lenders, irrespective of the number of lenders from which firms borrow. Our evidence suggests that higher innovation disclosure leads to a reduction in lenders' rents from informational monopolies because switching firms are subsequently charged lower loan spreads and lower total cost of borrowing.

By analyzing the interplay between banks' potential for information acquisition and corporate-innovation disclosure, our paper also relates to the literature on how banks acquire information about firms and, thereby, mitigate informational asymmetries. Banks learn about borrower firms through screening and monitoring activities (Diamond (1984), Ramakrishnan and Thakor (1984), Allen (1990), Winton (1995), Dass and Massa (2011)), and they are likely to learn even more if they provide multiple services to the firm (Boot (2000), Degryse and Van Cayseele (2000), Neuhann and Saidi (2016)).

As we posit that the value of private information between lenders and borrowers governs firms' ability to switch lenders, our paper connects with Rajan (1992), who argues that banks may use their private information to hold up, and extract economic rents from, firms. By testing this claim, we provide evidence on the stability and duration of banking relationships, as discussed in Ongena and Smith (2001), Ioannidou and Ongena (2010), Gopalan, Udell, and Yerramilli (2011), and Bonfim, Nogueira, and Ongena (2017).[3]

Furthermore, we contribute to the literature on how the development of the financial sector interacts with firms' patenting decisions (Benfratello, Schiantarelli, and Sembenelli (2008); Amore, Schneider, and Žaldokas (2013); Chava, Oettl, Subramanian, and Subramanian (2013); Cornaggia, Mao, Tian, and Wolfe (2015)).[4] Some papers highlight that patents might have an additional role on top of recording firm-level innovation. Mann (2016) analyzes patents as collateral for loans, while Chava, Nanda, and Xiao (2015) show that increased patent protection and creditor rights over collateral result in cheaper loans. This opens up the possibility that in financial contracting patents serve multiple purposes, besides intellectual

[3] Typically, the duration of banking relationships is used as a measure of their strength, which has been shown to positively affect credit availability (Petersen and Rajan (1994), Berger and Udell (1995)). Instead, we consider the stability of banking relationships as an outcome resulting from changes in the relative value of private information. See also Boot (2000) for a more extensive summary on relationship banking or Houston and James (1996) for evidence on public firms.

[4] See also Kerr and Nanda (2015) for an extensive survey of the literature where they acknowledge the increasingly important role of bank finance (and debt) for innovation, even among mature firms.

property protection. In this vein, we demonstrate the importance of innovation disclosure and public-information production through patents, building on the idea that patents are a credible signal for the quality of otherwise hard-to-observe innovation (Bhattacharya and Ritter (1983); Francis, Hasan, Huang, and Sharma (2012)).

Our paper also relates to studies on voluntary disclosure and proprietary costs in disclosing information. In testing the hypotheses generated by a voluminous theoretical literature (e.g., Darrough (1993), Gigler (1994), Evans and Sridhar (2002), Ganglmair and Oh (2014)), empirical work faces the challenge that most of firms' public disclosure might have limited proprietary costs. We consider a case where such proprietary costs are significant, namely firms' trade-off between patenting their innovation and keeping it secret (Moser (2005) and Moser (2012)). In a related paper, Brown and Martinsson (2015) study the impact of information environments on firms' innovative activities. Furthermore, Dass, Nanda, and Xiao (2015) analyze firms' stock liquidity as an additional concern that might encourage firms to patent a larger stock of their knowledge.

Unlike other types of corporate disclosure, innovation disclosure is special because of its relationship with firms' trade-off between patenting and secrecy. In particular, if firms attempt to avoid such disclosure, this is unlikely to hide negative information from their capital providers but, instead, to keep competitors from obtaining certain technical knowledge. Therefore, innovation-related information is especially costly to acquire for lenders.

Altogether, our results speak to the literature on information sharing and bank competition. In line with Pagano and Jappelli (1993), we find that greater availability of information makes credit markets more competitive as it becomes easier to contest existing bank-firm relationships. We argue that firms might have other constraints, besides having to hide poor creditworthiness, which could explain why banks are able to establish information monopolies. Firms try to protect their innovative ideas in order to preserve their position in the product market. The acquisition of innovation-related information, in turn, enables incumbent banks to hold up firms in lending relationships. Thus, innovation disclosure makes credit markets more contestable.

2 Effect of Innovation Disclosure on the Stability of Lending Relationships

We start our analysis by investigating whether more public information about firms' innovation is related to a decrease in the value of formerly private information between banks and firms. To shed light on the interplay between firms' public-information production and banks' private-information acquisition, we scrutinize how a shock to firms' innovation disclosure through patent applications alters their relationships with existing lenders.

Once formerly private information about a firm is revealed publicly, the incumbent bank partly loses the advantage that it had in financing the firm due to its previously undertaken information acquisition. We posit that an increase in publicly available information about a firm's innovation leads to potential break-ups of existing bank-firm relationships, as other banks become comparatively more competitive in financing the firm. We should thus observe more firms switching banks when more public information is available and private information becomes less valuable.[5]

To test this hypothesis, we exploit the American Inventor's Protection Act of 1999 (AIPA). In the following, we describe our identification strategy building on AIPA as a source of variation in the disclosure of patent applications.

2.1 American Inventor's Protection Act of 1999

We use the passage of AIPA as a shock to the proportion of information on firm-level innovation that is public, rather than private. Historically, U.S. patent applications were kept secret until the final patent was granted (Graham and Hegde (2015)). Firms could thus avoid revealing the content of their patents publicly without losing intellectual property

[5] Note that our argument does not rely on the amount of private information that incumbent banks had about firms' innovative activities before the increase in public information. However, now that more information is revealed publicly, *any* private information, on firms' innovative and other activities, that banks had becomes relatively less valuable. In this context, we do not make any assumption on whether previously private information has been substituted for by the released public information, or whether the total amount of public and private information has increased.

protection (while foregoing licensing income), a practice known as "submarine patenting." AIPA became effective on November 29, 2000, and harmonized U.S. patent laws with other developed economies by requiring public disclosure of patent applications 18 months after the filing date, even if the patent is not granted eventually.[6]

The passage of AIPA can be described as a contentious and uncertain process. In Section 11 of the Online Appendix, we provide a summary of its legislative history as given by Ergenzinger (2006). In particular, Ergenzinger (2006) includes multiple quotes that indicate that legislators and other experts deemed the disclosures as harmful for U.S. innovators. This reinforces our assumption that AIPA imposed a level of (involuntary) disclosure of innovation-related information that firms considered suboptimal.

The effect that we identify through AIPA is likely to be mitigated by the fact that some firms were filing international patents that were already subject to an 18-month disclosure rule. However, as argued by Hegde and Luo (2017), publication in foreign countries is not equivalent to publication in the U.S. because of the lack of public records available prior to AIPA that linked U.S. patent applications to their foreign-country counterparts. Equivalent foreign patent applications may also have been published in foreign languages, while many U.S.-based entities would only search the U.S. Patent and Trademark Office's databases due to resource and time constraints. AIPA thus had a subdued effect on the informational environment of firms with such international patents.

2.2 Identification Strategy

We describe our identification strategy in a number of steps. First, we characterize the variation across firms in their exposure to the passage of AIPA which we exploit for identification. Second, we motivate that this variation is not correlated with other observable characteristics that could contaminate our identification. Third, we provide preliminary evidence using cross-sectional regressions. Fourth, we describe the specification that we adopt to identify

[6] Even after AIPA, firms could opt for secrecy by foregoing foreign patenting. As shown by Graham and Hegde (2015), only a small proportion – one-digit percentage – of inventors decided to do so.

the treatment effect of AIPA and higher innovation disclosure on firms' lending relationships.

Variation in innovation disclosure. Arguably, prior to AIPA, firms differed in the secrecy of their patent applications. One particular consideration in whether firms keep innovation secret or make it public is the proprietary cost of rivals obtaining certain technical knowledge (Hall, Helmers, Rogers, and Sena (2014)). This is especially true if the patent is not granted eventually, in which case the firm neither receives the intellectual property protection, nor keeps the knowledge in-house. Industry conditions are then likely determinants of firms' decision whether to patent or to keep their innovation secret.

As our continuous treatment measure, we use the average time lag between patent applications and grants (when their content was made public) for each firm's SIC2 industry[7] over five years during the pre-AIPA period from 1996 to 2000.[8] We argue that firms operating in industries with longer historical delays from filing to grant were affected more heavily by the passage of AIPA, which imposed a delay time of 18 months. The longer the historical delay in the industry, the more likely patent applications were kept secret for a longer period of time.

Such delays may even have been due to purely non-strategic reasons, such as technical complexities in the patent-review process in a given industry. Graham and Hegde (2015) also report some heterogeneity in terms of inventors' disclosure choices across technology fields. For instance, they show that computers and communication technologies were more likely than drugs and chemicals to use pre-AIPA secrecy for reasons such as cross-licensing, fencing, litigation, and submarine patenting. As can be seen in the top panel of Table 1, the average delay across different industries is 26 months, and none of the industries under consideration has a mean delay below 18 months.

[7] We use the industry-level average lag to capture both the actual delay for firms that filed for patents in that period and the potential delay for firms that did not file in that particular period but might have filed before or would do so later on. All results hold up to using firm-level delays as our treatment measure for the subsample of firms that filed for patents in the pre-AIPA period, or a delay measure based on the technological fields of firms' patents, as in Graham and Hegde (2015).

[8] While our baseline delay measure is based on the years 1996 to 2000, we provide a robustness check where we vary the time window. In fact, when estimated annually, we find that such measure exhibits significant serial correlation. A Wooldridge test for serial correlation performed over annual SIC2-level delay data from 1976 to 2000 would reject the null hypothesis of no autocorrelation with $F = 5.46$ ($p = 0.023$).

Correlation with other industry-level drivers of banking relationships. Importantly, this delay measure is not meaningfully correlated with cross-industry variation in access to finance or other characteristics that might influence banking relationships directly or through other channels. To show this, we report the estimates for cross-sectional regressions at the SIC2-industry level in Table 2. Our dependent variable is the mean difference in years between filing and grant dates, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. Similarly, independent variables are measured as their respective total (or, where appropriate, average) values from 1996 to 2000.

The first column reports the correlation between our SIC2-industry delay measure and international-trade characteristics of the industry, namely its import as well as export penetration. Arguably, a firm's integration into global trade and openness to foreign competition could affect both its strategic decision to patent innovation as well as its banking relationships (see Manova (2013) and Foley and Manova (2015)). We measure import penetration as total imports over the total value of shipments plus total imports minus total exports in a given SIC2 industry, and export penetration as total exports over the total value of shipments in a given SIC2 industry. We find no relationship between our delay measure and import or export penetration.

Furthermore, we also consider the possibility that our delay measure may be correlated with the number of patents filed. For instance, one could argue that industries that patent heavily and are, thus, presumably more innovative could have shorter delays, as patent officers learn more about the respective technologies. These industries could also differ in their banking relationships (Amore, Schneider, and Žaldokas (2013); Chava, Oettl, Subramanian, and Subramanian (2013); Cornaggia, Mao, Tian, and Wolfe (2015)). In the second column, we find no statistically significant correlation between our delay measure and the number of patents in the industry, suggesting that differences in patenting activity are unlikely to explain industry-level variation in the delay in disclosing patent information.

Additionally, in the third column, we consider the average total factor productivity in a given SIC2 industry, using the semiparametric estimation procedure by Olley and Pakes

(1996). Industries with long delays in their patent grants are neither more nor less productive, reassuring us that our measure does not capture such confounding industry characteristic.

In column 4, we consider financial dependence, measured as the median value of financing needs across firms in a given SIC2 industry (Rajan and Zingales (1998)). For each firm, financing needs are measured as total capital expenditures minus total operating cash flows, over total capital expenditures. Again, we find no correlation with our delay measure.

Finally, we consider stock-market run-ups before 2000. As AIPA was passed around the time of the dot-com bubble, one may worry that we capture any effects of the latter if longer delays are prevalent among technology companies. To explore this, we compute the equal-weighted average of stock returns between 1996 and 2000 for each SIC2 industry, and correlate it with our delay measure. We find no statistically or economically significant relation, suggesting that the dot-com bubble and the subsequent crash are not driving our results.[9] In addition, we will also show that our results are invariant to the exclusion of high-tech companies that played an important role in the dot-com boom and bust.

Cross-sectional evidence. To estimate the effect of AIPA on the stability of lending relationships, we start with a panel of all firms with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). That is, for each firm we record two observations that we use to characterize post-AIPA vs. pre-AIPA lending relationships. We then use our industry-level delay measure that captures variation in the treatment intensity under AIPA, and link it to the proportion of pre-AIPA banking relationships that were preserved post AIPA. This basic cross-sectional specification provides preliminary evidence for the effect that we will identify in a refined framework where we can control for firms' loan demand, banks' credit supply, and the match quality between banks and firms.

[9] Other events (e.g., the passage of SFAS 141 and 142 or the Sarbanes-Oxley Act) might have also partially coincided with the passage of AIPA. In addition, AIPA itself also made other changes to the patent system, such as cracking down on invention promotion firms or fee reductions. However, our identification relies on AIPA having a differential impact based on pre-AIPA industry-level delays between patent applications and grants. For any confounding events or other AIPA terms to bias our estimates, such events should have a similar ranking of industry-level exposure.

These regressions use dependent variables based on data from both pre-AIPA and post-AIPA periods and are, thus, cross-sectional in nature. In the first column of Table 3, we use as dependent variable the proportion of the total loan volume of a firm in the post-AIPA period granted by banks that it already received a loan from in the pre-AIPA period. We regress this on our treatment-intensity (delay) measure. The constant is positive, and indicates that at the baseline 23.9% of the firms engage in recurring loan transactions with their incumbent lenders. Conversely, the coefficient on our delay measure is negative and significant, indicating that firms in treated industries are less likely to return to their incumbent lenders. An increase in the pre-AIPA delay by one standard deviation is associated with $0.053 \times 0.223 = 1.2\%$ more break-ups (or fewer recurring relationships). We yield a similar result when instead of the loan volume, we look at the number of maintained relationships. To this end, in the second column, we replace the dependent variable by the proportion of pre-AIPA lending relationships that the firm kept in the post-AIPA period.

We also explore whether the effect is governed by the denominators of our dependent variables. That is, we examine whether firms diversify their portfolio of lenders, e.g., because of an increase in total demand that incumbent banks could not accommodate. To stay consistent with the cross-sectional nature of our specification, we use as dependent variables two measures that constitute differences between the post-AIPA and the pre-AIPA period. In the third column of Table 3, we consider the percent change in the total loan amount received by a firm, and find no correlation with our treatment-intensity measure. In the last column, we furthermore consider the change in the number of lending relationships (with different banks), and again yield no correlation with the treatment measure.

These findings indicate that firms in treated industries are less likely to return to their incumbent lenders, but borrow the same amount as before from the same number of lenders. In combination, this suggests that firms in treated industries do not only break up existing relationships, but actually switch lenders.

While these tests provide suggestive evidence, they do not fully absorb unobservables that could be correlated with the effect we try to identify. For instance, firms in treated industries might switch lenders because banks they previously borrowed from have reduced their supply of credit. Moreover, while our estimates in column 3 of Table 3 indicate that total loan volume was, on average, not affected by AIPA, it might well be that it did increase for firms that switched lenders, which renders it difficult to disentangle a general demand effect from actual switching in such cross-sectional regressions. Other firm characteristics that influence switching behavior might also have changed over time. Finally, some bank-firm relationships may be inherently less stable than others.

To address these issues, we next propose a methodology to identify firms' switching lenders, holding constant firms' total loan demand and other characteristics, banks' overall credit supply, as well as the (time-invariant) nature of bank-firm matches.

Baseline specification. We augment the above-mentioned panel of firms to the level of all bank-firm pairs (ij) with at least one loan in the pre-AIPA or post-AIPA period. In this manner, we yield two observations per bank-firm pair. For each observation, we measure either the total loan volume received by firm i from bank j, which serves as our measure of the intensive margin of lending relationships (while also capturing some of the extensive margin), or an indicator for non-zero loan volume, reflecting the extensive margin.

This setup allows us to include not just bank-firm fixed effects that capture a particular bank-firm match, but also firm-period fixed effects to capture shifts in firm-level demand for loans *across all banking relationships*, and bank-period fixed effects to capture shifts in bank-level supply *across all firms contracting with the same bank*. Naturally, our industry-level treatment measure interacted with a post-AIPA dummy is captured by firm-period fixed effects. However, as we are interested in the development of pre-existing banking relationships, we interact our treatment measure, a post-AIPA dummy, and an indicator for whether a bank-firm pair ij already contracted in the pre-AIPA period. This gives us variation at the bank-firm-period level, and we estimate the following specification:

$$\begin{aligned} y_{ijt} &= \beta_1 Treatment_i \times Initial\ relationship_{ij} \times Post_t \\ &\quad + \beta_2 Initial\ relationship_{ij} \times Post_t + \mu_{it} + \eta_{jt} + \theta_{ij} + \epsilon_{ijt}, \qquad (1) \end{aligned}$$

where y_{ijt} is the logged total loan volume or an indicator for non-zero loans at the bank-firm level for each period, $Treatment_i$ is defined at the industry level (based on SIC2 codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000, $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period, and $Post_t$ is a dummy variable for the post-period from 2001 to 2005. μ_{it}, η_{jt}, and θ_{ij} denote firm-period, bank-period, and bank-firm fixed effects, respectively, and standard errors are clustered at the bank level.[10]

With firm-period, bank-period, and bank-firm fixed effects, β_1 and β_2 can be estimated to be non-zero only if a firm reduces its exposure to an existing lender, while at the same time switching to or adding another lender. β_2 estimates the baseline propensity to break up an existing relationship. It is natural that over the course of ten years (in our sample period from 1996 to 2005), firms would regularly switch lenders, so we expect β_2 to be negative. Our coefficient of interest, however, is β_1, which reflects deviations from the baseline break-up rate for borrowers in industries that were especially affected by AIPA.

In (1), the estimation of a firm breaking up or reducing its exposure to an existing relationship is equivalent to establishing a new banking relationship. To see this, assume that a firm ceased an existing relationship with *bank A* from which it borrowed \$500m in the pre-AIPA period and \$0 in the post-AIPA period. If the firm did not borrow from any bank in the post-AIPA period – in the extreme case, due to bankruptcy – then the effect should be explained entirely by firm-level demand and, thus, by the firm-period fixed effects μ_{it}.

That is, if a break-up is not accompanied by the establishment of a new relationship, then β_1 and β_2 should be zero. Now assume that the same firm borrowed \$300m from another *bank B* after AIPA. Then, we have a pre-AIPA and a post-AIPA observation for the firm with each bank: \$500m and \$0 from bank A as well as \$0 and \$300m from bank B. Only in this case, β_1 and β_2 can be negative. The extent to which they are negative depends on (i) the reduction in the amount borrowed from old lenders in the post-AIPA period and (ii) the

[10] We cluster standard errors at the bank level to conservatively account for the fact that the relevant level of variation is a bank-firm relationship. Our results are also robust to clustering at the industry level.

amount borrowed from new lenders in the post-AIPA period, compared to the total amount borrowed in the pre-AIPA period. In other words, β_1 and β_2 are going to be more negative the more the firm replaces pre-AIPA lenders with new lenders. Partial switching will yield an estimate with a lower absolute magnitude than complete switching.

We are also interested in disentangling complete from partial switching. For this purpose, as dependent variables we not only use the dollar amount firm i borrowed from bank j in period t but also an indicator for any non-zero loan volume. If firms switch lenders only partially and the number of lenders does not change, then our estimates will be biased towards zero when using an indicator for any non-zero loans, but less so when using loan amounts as dependent variable.

However, it may still be that borrowers diversify their portfolio of lenders by increasing the number of sources of loans. If this is the case, then partial rather than complete switching may lead to negative estimates of β_1 and β_2 even when using an indicator for any non-zero loans as dependent variable. However, as seen in the last column of Table 3, firms in treated industries did not increase the number of their banking relationships after AIPA. Furthermore, the difference in the number of relationships in the post-AIPA vs. pre-AIPA period per firm exhibits a correlation of -0.02 with our treatment-intensity (delay) measure.

2.3 Data Description

Our main sample comprises public firms from 1996 to 2005. Our syndicated-loan data come from DealScan, and we focus on the lead arranger(s) to identify the relevant lender(s). To calculate $Treatment_i$, we use the patent dataset of the National Bureau of Economic Research (NBER), which contains information on all patents awarded by the U.S. Patent and Trademark Office (USPTO) as well as citations made to these patents (Hall, Jaffe, and Trajtenberg (2001)). We match the NBER patent dataset with DealScan via Compustat data, following the procedures in Hall, Jaffe, and Trajtenberg (2001) and Bessen (2009).

In the top panel of Table 1, we present summary statistics for our main analysis in Tables

4 to 7. We record two observations per bank-firm pair. We have 9,333 such pairs.[11] Of these 9,333 bank-firm relationships, 57.3% – i.e., 5,352 – already existed in the pre-AIPA period. That is, 42.7% of all bank-firm pairs came into existence only in the post-AIPA period. Of the 5,352 pre-existing relationships, 17.6% still existed in the post-AIPA period. This also explains the average sum of the loan indicator over both periods, as $0.176 \times 0.573 + 1 = 1.101$ (we condition on at least one loan transaction for any bank-firm pair, so the minimum value over both periods is 1 and the maximum is 2).

In the bottom panel of Table 1, we include summary statistics for our loan-level analysis in Table 12. The respective loans sample consists of syndicated loans of public firms from 1987 to 2010 in DealScan.

3 Empirical Results

We now turn to our empirical results for the effect of AIPA and higher innovation disclosure on firms' lending relationships. Then, we discuss the heterogeneity of the treatment and further robustness checks.

3.1 Main Results

We start by presenting our main result, namely the break-up of lending relationships for firms in industries that were affected more heavily by AIPA. We proceed as follows. As described in Section 2.2, we yield two observations for each bank-firm pair (ij). We record all bank-firm pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). Our continuous treatment variable is the mean delay from filing to patent grant in years, which varies at the SIC2-industry level.

In Table 4, we estimate specification (1), where we use as dependent variable the log of the

[11] The sample drops to 8,348 pairs when we add patent measures from the NBER patent dataset.

total volume of all loan transactions per period between firm i and bank j, which reflects the intensive margin of lending relationships. The treatment effect in the first column is given by the coefficient on $Treatment_i \times Initial\ relationship_{ij} \times Post_t$. The effect is negative and significant at the 1% level, thereby indicating significantly more break-ups of lending relationships and switching by firms in treated industries.

We perform a few important robustness checks. First, we verify the nonexistence of any pre-trends, and conduct a placebo test by shifting the first year of the post-AIPA period forward by three years, namely from 2001 to 1998. The treatment intensity in this case is measured over the $1993 - 1997$ period. This reduces the sample size somewhat as there are fewer bank-firm pairs with non-zero loans in the pre- and/or post-placebo period.

As can be seen in the second column of Table 4, the treatment effect is much weaker than in the first column, and not statistically significant. Conversely, the coefficient on $Initial\ relationship_{ij} \times Post_t$, which estimates the baseline break-up rate, remains negative and significant, which is a natural consequence of the fact that most borrowers have the tendency to switch lenders over the course of ten years. Yet, this effect is not related to the placebo-treatment intensity, suggesting that the treatment effect in our baseline specifications does not arise mechanically.

Furthermore, our baseline sample is limited to bank-firm pairs with non-zero loans in the pre-AIPA period, the post-AIPA period, or both. By controlling for firm-period fixed effects and, thus, any shocks to firm-level demand for loans, this allows us to identify firms that switched lenders. However, observed bank-firm pairs may be subject to a selection effect that might bias our estimates. To test for such selection in the most conservative way possible, we enrich our sample by all theoretically possible bank-firm pairs, i.e., including those with zero transactions throughout, in the third column of Table 4, where our result is robust.

In general, the coefficients for the treatment effect are large in absolute size. A potential reason for this is that the effect operates also at the extensive margin, and the logarithm is not a good approximation for the (negative) growth rate when total loan volume drops to zero in the post-AIPA period. To gauge the extent of complete rather than partial switching, we alternatively use as dependent variable an indicator for the occurrence of *any* loan transaction between firm i and bank j in period t. We re-estimate the specifications from Table 4 with the latter dependent variable, and report the results in Table 5. All findings are robust.

Focusing on the main treatment effect, based on a standard deviation of 0.223 for $Treatment_i$ (see Table 1), the first column of Table 5 indicates that an increase in the pre-AIPA delay by one standard deviation is associated with $0.086 \times 0.223 = 1.9\%$ more break-ups. The economic significance of this estimate is given by its comparison to the baseline proportion of recurring relationships of 17.6% in Table 1.

3.2 Heterogeneity of the Treatment

We provide further empirical support for our proposed mechanism by studying whether the impact of AIPA differs across affected firms in predictable ways. In particular, we expect our results to be stronger for patenting firms and for firms that value secrecy more, such as those that patent highly-cited innovations. In addition, we expect our results to be stronger for firms located in states where they find it more difficult to protect trade secrets (as an alternative to protecting intellectual property through patenting).

Patenting firms and firms with highly-cited patents. Theoretically, firms that patented in the pre-AIPA period should be affected more heavily by AIPA. We find this to also hold empirically in the first column of Tables 6 and 7, where the treatment effect is even stronger (i.e., the coefficient is more negative) for firms that patented in the pre-AIPA period.

AIPA imposed innovation disclosure at levels that firms likely deemed to be suboptimal for product-market-related reasons. Still, firms can only be treated by the AIPA-induced higher innovation disclosure if they do not stop patenting after AIPA. That is, the firms driving our results are those for which the general benefits of patenting exceed potential costs, including innovation disclosure, although secrecy may still be generally valuable for them. As a consequence, they opt to patent rather than to keep innovation completely secret.

However, firms could still choose the degree of secrecy even if they patent, by delaying the patenting process and taking advantage of laxer disclosure rules before AIPA. This suggests that firms that would value secrecy more, but are still patenting, should be affected more heavily by the implementation of AIPA. We conjecture that firms with valuable patents would value secrecy more, and therefore investigate whether firms with particularly valuable patents were more likely to switch lenders.

For this purpose, we interact $Treatment_i \times Initial\ relationship_{ij} \times Post_t$ with a variable that is equal to the average number of forward citations per patent across all patents issued by a given firm in the pre-AIPA period (or zero if a firm did not patent in the pre-AIPA period). In the second column of Tables 6 and 7, we show that the treatment effect on both the intensive and extensive margin of lending relationships is indeed driven by firms with particularly valuable patents in the pre-AIPA period.

Insofar as highly cited patents reflect generally valuable innovation activities on the firm side, and assuming that firms that produced highly valuable innovation in the pre-AIPA period continued to do so in the post-AIPA period, these estimates can be interpreted as evidence that firms in treated industries were more likely to switch lenders when AIPA led to the disclosure of highly valuable innovation-related information.

Stronger protection of trade secrets. In the last two columns of Tables 6 and 7, we exploit variation in firms' patenting activity as derived from the ease with which firms could protect their trade secrets. Trade secrets constitute an alternative to protecting intellectual property through patenting, so that stronger protection of trade secrets should induce firms to patent less.

To this end, we consider whether a firm's headquarters were located in a state where the courts recognized the Inevitable Disclosure Doctrine (IDD). IDD was targeted at employees who possess knowledge of a firm's trade secrets, and restricted their ability to take up similar assignments at rival firms. Thus, the adoption of IDD by state courts enhanced the protection of trade secrets for firms located in the respective states, as it reduced the risk that a firm's departing employees could reveal its trade secrets to industry rivals (see Klasa, Ortiz-Molina, Serfling, and Srinivasan (2017)). Due to the protection of trade secrets under IDD, firms had higher incentives to keep innovation secret and patent less even before AIPA came into effect.

Given that following the adoption of IDD firms could more readily exploit trade secrets instead of relying on patenting, we hypothesize that the treatment effect of AIPA on lending relationships should be weaker for firms facing stronger protection of trade secrets. To test this, we measure the presence of IDD in a given firm's state in the first available year of the pre-AIPA period from 1996 to 2000, IDD_i. By 1996 (2000), the courts of 14 (20) states recognized IDD.

Indeed, the coefficient on $Treatment_i \times Initial\ relationship_{ij} \times IDD_i \times Post_t$ is positive and significant in the third column of Tables 6 and 7. This suggests that firms in industries with longer delays, which should have been affected more heavily by AIPA, were less likely to switch lenders if they operated out of states that had adopted IDD.[12]

For these estimates, we have $IDD_i = 0$ for three states – Florida, Michigan, and Texas – that eventually rejected IDD after its adoption. These states dropped IDD relatively soon after AIPA, from 2001 to 2003. As a robustness check, we re-define $IDD_i = 1$ for Florida, Michigan, and Texas. As a falsification test, we should yield a weaker interaction effect compared to the third column. This is indeed the case, as can be seen in the last column of Tables 6 and 7, where the respective (positive) interaction effect is not just smaller in size, but also somewhat less significant.

3.3 Robustness

We continue with further heterogeneity tests, e.g., by excluding instances where the effect of AIPA on lending relationships could be explained by alternative channels. Then, we demonstrate the robustness of our results to the construction of the delay measure $Treatment_i$. Finally, we exploit the discontinuity at 18 months to form a stable control group.

[12] Note that the sample size drops, as we perform this analysis only for firms that did not change their headquarters during our sample period.

Further heterogeneity tests. In Tables 8 and 9, we present additional robustness checks. First, one might be concerned that the effect on banking relationships could have operated in an indirect way. For instance, higher innovation disclosure could have affected firms' licensing of patents and might therefore have had an effect on firms' cash flows, forcing them to rearrange their banking relationships.

Gans, Hsu, and Stern (2008) show that a discontinuous jump in licensing occurs only right after patent allowance (i.e., grant), and there is no corresponding increase before, e.g., when patent applications are published 18 months after their filing date. The authors claim that such a discontinuity in licensing provides evidence for the frictions in the market for ideas and the value of formal intellectual property rights in facilitating technology transfer. Indeed, due to "the ability of licensees to expropriate knowledge that is disclosed by the licensor but unprotected by intellectual property," very little licensing takes place before patent allowance (Gans, Hsu, and Stern (2008)).

However, we take this concern seriously, and – as a very conservative robustness check – exclude all firms involved in licensing alliances anytime during our sample period. We gather data on whether a firm is involved in a licensing agreement from the Thomson Reuters SDC Platinum database. As can be seen in the first column of Tables 8 and 9, our results hold up to excluding these firms.[13] This suggests that our results for firms' switching lenders are unlikely to be driven by licensing alliances.

Second, related to the concern that our treatment may have coincided with firms being differentially affected by the dot-com bubble, we show in the second column of Tables 8 and 9 that our treatment effect is virtually unchanged after dropping high-tech companies from our sample.

Third, we limit the pre- and post-AIPA sample to banks with experience in lending to innovative firms in the pre-AIPA period, i.e., we only consider banks that lent to firms that filed for patents in the pre-AIPA period. As in Chava, Nanda, and Xiao (2015), we

[13] Furthermore, we also find that the number of licensing alliances has not increased in industries more heavily affected by AIPA. We additionally confirm these results using data on material licensing contracts from firms' 10-K, 10-Q, and 8-K filings. These results are available upon request.

thereby focus on banks that are more likely to use innovation-related information from patent applications. In line with this argument, in the third column, we show a considerably stronger treatment effect for this set of banks.

Finally, in the fourth column of Tables 8 and 9, we drop firms that were delisted for bankruptcy-related reasons anytime before the end of the estimation period in order to filter out break-ups of lending relationships due to bankruptcy. The estimates are very similar to our baseline estimates in the first column of Tables 4 and 5. In untabulated tests, we find that the robustness of the estimates extends to dropping all firms that were delisted for *any* reason. As bankruptcy-related reasons for observed break-ups of banking relationships are equivalent to a negative shock to firm-level demand, this further attests to the validity of our identification strategy, in that firm-period fixed effects fully capture such shocks.

Treatment-intensity measure. In Tables 10 and 11, we examine the robustness of the treatment effect to the definition of our delay measure $Treatment_i$. As can be seen in the first column, our results are robust to using the *median*, rather than the mean, SIC2-level delay from filing to grant as our continuous treatment variable. We also vary the length of the time window around AIPA from five (as in our baseline regressions in Tables 4 and 5) to three years in the second column. In the third column, we limit the sample to firms that patented at least once between 1996 and 2000, and use their firm-level, rather than their respective industry-level, delays from filing to grant in the pre-AIPA period as treatment variable. The treatment effect remains robust.

In the fourth column, we use as our continuous treatment variable a delay measure that is based on the portions of delays that were more likely to be due to examiners. To construct this alternative delay measure, we download transaction histories from the Patent Application Information Retrieval (PAIR) database for every patent issued to a publicly listed firm between 1996 and 2000. We then exclude the time lapsed between "Mail Non-Final Rejection" and "Response after Non-Final Action" as well as the time lapsed between "Mail Notice of Allowance" and "Issue Fee Payment Received." Our estimates are virtually unaltered after using this alternative delay measure.

Finally, we tackle the fact that AIPA was enacted in November 1999, but affected new patent applications starting only in November 2000. In untabulated results, which are available upon request, we find that the treatment effect is also robust to using a continuous treatment variable that is based on delays from 1995 to 1999, instead of 1996 to 2000, thereby excluding the year during which firms were aware of the forthcoming implementation of AIPA while still filing patents under the old regime.

Stable control group. A general concern with our AIPA identification may be that it is a fuzzy design, as suggested by de Chaisemartin and D'Haultfœuille (2017). That is, in our baseline specifications, all firms are assumed to be treated to some extent, and they differ only by their experiencing a higher increase in the treatment rate. This is because at the industry level, the minimum average delay across firms from filing to grant in the pre-AIPA period, which we use as treatment intensity, is greater than 18 months (see Table 1). However, when we measure the delay at the firm level, for some firms in our sample the delay is as low as 7.5 months.

We have already exploited this feature when using firm-level delays as our treatment variable in the third column of Tables 10 and 11. To address the concerns raised by de Chaisemartin and D'Haultfœuille (2017), we re-run the same regressions, and limit the sample to firms with pre-AIPA delays of at most two years. In this manner, we also safeguard the similarity between treated and non-treated firms while exploiting the sharpness of the treatment only for firms with delays greater than 18 months. The results in the first and third column of Table B.1 suggest that, if anything, the treatment effect becomes stronger.

In the second and fourth column, we use the fact that firms with delays below 18 months should not be treated by AIPA, and re-define $Treatment_i$ as a binary rather than a continuous variable. Namely, it is equal to 0 for all firms with average delays below 18 months, and 1 for all firms with average delays of at least 18 months (but again at most two years). Our estimates remain robust. Most strikingly, the treatment effect in the last column reflects a severe increase in the switching rate, implying that treated firms broke up 14.5% more relationships than control firms.

4 Hold-Up by Incumbent Lenders and Informational Monopolies

In this section, we discuss whether higher innovation disclosure following AIPA led to a reduction in lenders' rents from informational monopolies. For this purpose, we examine whether firms whose ability to switch lenders has grown thanks to AIPA subsequently face more favorable loan and general financing conditions.

4.1 Cost of Debt

We first scrutinize whether firms in treated industries profited from lower cost of debt. In this manner, we show that higher innovation disclosure leads to a reduction in lenders' rents from informational monopolies.

This is in line with Ioannidou and Ongena (2010), who find that firms that *voluntarily* switch from one bank to another profit from lower loan rates. In contrast, using *forced instances* of firms switching banks in large groups due to branch closings, Bonfim, Nogueira, and Ongena (2017) show that such transfers are not associated with lower loan rates. Their evidence is consistent with the idea that incumbent lenders acquire valuable private information about their borrowers, and that this informational link is lost following branch closings.

Our empirical setting combines aspects of both Ioannidou and Ongena (2010) and Bonfim, Nogueira, and Ongena (2017) in a way that allows us to draw inference about the degree of hold-up due to private-information acquisition and incumbent lenders' rents from informational monopolies. Namely, increased innovation disclosure under AIPA constitutes a forced change in borrowers' innovation-disclosure levels that reduces the informational advantage of incumbent lenders. This enables us to show that firms do not only use this opportunity to switch lenders, but they also escape hold-up by their incumbent lenders and are subsequently charged lower loan rates. This reduction in loan rates for switchers reflects the value of previously private information as well as the improved match quality of borrowers and lenders.[14]

To quantify this, in Table 12, we use loan-level data, and implement the same difference-in-differences strategy as before at the firm-year level. To be consistent with our construction of the AIPA sample, we always include firm fixed effects so as to identify the treatment effect using firms that received loans in both pre- and post-AIPA periods.

In the first column, we find a significantly negative treatment effect on firms' cost of debt, as approximated by the all-in-drawn spread of a syndicated loan. This effect is driven primarily by firms that patented in the pre-AIPA period, as one can see in the second column.

In the third column, we split up the difference-in-differences estimate by whether the post-AIPA loan in question was granted by a bank with which the firm already had a relationship in the pre-AIPA period from 1996 to 2000. The coefficient on the respective triple interaction is positive and significant. However, the sum of the coefficients on $Treatment_i \times Post_t$ and $Treatment_i \times Initial\ relationship_{ij} \times Post_t$ is negative and significant at the 4% level. That is, while firms that keep their previous relationship receive significantly higher treatment-induced interest rates than firms that switch, they are still offered lower interest rates by their incumbent banks thanks to the treatment.

In the remaining columns, we estimate the same specifications as in the first three columns, but replace the dependent variable by a measure for the total cost of borrowing, including fees, as in Berg, Saunders, and Steffen (2016). The sample size drops somewhat due to the more limited availability of the outcome variable, but we yield qualitatively similar effects as for the all-in-drawn spread, which are both statistically and economically significant.

These results are insightful, in that they alleviate some of the concerns and provide a more precise interpretation of our AIPA treatment effect on switching. First, our findings link to the discussion in Rajan (1992) on interim public signals. According to Rajan (1992), loan

[14] Improved match quality of borrowers and lenders could be one explanation why incumbent banks are not able to retain switching firms, even if they reduce the borrowing costs to reflect the lower cost of information acquisition following AIPA.

rates charged by outside banks are lower, which is consistent with our findings. Moreover, he shows that rates for inside banks do not decrease if outside banks interpret bad signals incorrectly. One reason for inside banks' loan rates to decrease nonetheless – but less so than for outside banks – in our setting is that AIPA is more likely to produce good, rather than bad, signals about firms' innovation process. This is because AIPA forced the disclosure of innovation-related information that firms did not desire to release previously for reasons related to product-market competition rather than for the purpose of hiding negative news.

Second, while the disclosure of every single filed patent has increased, one could argue (see, e.g., Aoki and Spiegel (2009)) that firms in treated industries patented less after AIPA. Given these conflicting forces, one may wonder whether the aggregate amount of innovation disclosure has increased or decreased. An alternative interpretation could be that firms' switching lenders has been forced by a reduction in the aggregate amount of innovation disclosure. In contrast, lower cost of debt suggests that the aggregate disclosure by firms has in fact increased.

Third, one may argue that AIPA has increased the cost of patenting because rival firms are able to obtain technical knowledge earlier and, thus, treated firms were forced to raise more capital to invest in shielding their innovation from replication by rival firms. Therefore, partial switching of lenders might be driven by the incumbent bank's inability to provide a larger amount of required funding. While we control for such shocks to firm-level loan demand by firm-period fixed effects, the fact that the cost of debt of switching firms has dropped suggests that our results are not driven entirely by additional costs of patenting implied by AIPA.

Finally, given that incumbent banks can always reduce their share of rents from lending relationships and lower the cost of debt, one might wonder why firms switch to new lenders in the first place. One reason may be that when switching becomes more feasible, the match quality of the new relationships produces rents that could not be offered by the incumbent bank. For instance, larger firms might find it more valuable to contract with larger, rather than smaller, banks. If the firm matched with the incumbent bank when it was smaller, but could not switch out of this relationship due to hold-up, AIPA could have facilitated recontracting and matching to a new, larger bank. Such assortative matching can explain why firms prefer to switch even if incumbent banks can also lower the cost of debt.

4.2 Other Supportive Evidence

We next provide additional supportive evidence by considering other contractual outcomes as well as treated firms' increased ability to access public capital markets.

In Table B.2, we re-estimate the specification from the third column of Table 12 for four more loan-contractual outcomes. We find no effect on the loan amount (second column), the use of financial covenants (third column), or the degree to which loans are secured (fourth column). In line with lower spreads charged by lenders, this suggests that lenders did not gain any bargaining power as a result of increased innovation disclosure.[15]

Conversely, as can be seen in the first column, switching firms received loans with significantly longer maturities than did firms that keep their previous relationship. That is, while the coefficient on $Treatment_i \times Post_t$, which is the treatment effect for both switchers and non-switchers, is positive, the additional effect for non-switchers, captured by the coefficient on $Treatment_i \times Initial\ relationship_{ij} \times Post_t$, is negative (albeit significant only at the 14% level) and the sum of the two effects is almost precisely zero (and insignificant). This is in line with the logic in Diamond (1991), Rajan (1992), and Diamond (1993). Longer maturities reflect borrower firms' ability to escape potential hold-up situations with their lenders, whereas shorter maturities give lenders more control, as they can threaten not to renew the loan.

We finish the presentation of our AIPA-based evidence by examining whether firms in treated industries raised more capital from public markets after the increase in disclosure

[15] The fact that these loan terms have not changed reflects the idea that the type of funded projects did not change significantly either. For instance, increased innovation disclosure could have made innovative projects more expensive, inducing firms to substitute away from corporate innovation. Less corporate innovation and, thus, fewer growth options might have reduced informational asymmetries between borrowers and lenders, and subsequently facilitated switching between lenders. Our evidence suggests that this is not the case.

of innovation-related information. Presumably, since the initial informational advantage of incumbent lenders decreased, it has become easier for firms not only to switch to other private lenders but also to reach out to public capital markets (see, e.g., Atanassov (2016)).

To shed light on this, we construct measures of public issuances of equity and debt from the Thomson Reuters SDC Platinum database, where for each firm we record the total principal amount of equity and debt raised over a year, compared to loan financing. In line with Tables 12 and B.2, the sample starts in 1987 and ends in 2010. Our results are in Table B.3, and indicate that firms in treated industries indeed raised more capital in public markets (first column). The effect amounts to more than one-quarter of a standard deviation, and is driven primarily by debt (second column, although the effect is significant only at the 13% level) rather than equity issues (third column).

Taken together with our findings in Tables 12 and B.2, the fact that firms in treated industries also gained the ability to raise more capital in public debt markets lends support to the idea that higher innovation disclosure under AIPA has helped firms to reduce the degree of hold-up in existing financing relationships.

5 Banks' Information Acquisition and Firms' Innovation Disclosure through Patents

Our evidence is consistent with innovating firms facing a trade-off between patenting and secrecy. In this trade-off, one of the costs of patenting is innovation disclosure. Firms might be reluctant to patent if competitors benefit from such innovation disclosure in their technological advancement. Lenders to innovating firms thus have to resort to costly private acquisition of such information[16] that firms are less willing to disclose publicly.

In this section, we discuss evidence lending support to our assumption that lenders acquire

[16] Our findings presented in Section 4.1 attest to the idea that acquiring innovation-related information is costly for lenders. This is because we show that the cost of debt drops after the publicity of firms' innovation increases, i.e., information acquisition is no longer impounded in the borrowing costs.

private information which is relevant for innovation. Since actual information acquisition is unobservable, we instead vary banks' costs of information acquisition to test whether lower costs, which would correspond to higher private-information acquisition, lead to lower public-information production through patents issued by firms.

Indeed, if private-information acquisition in relationships and public-information production through patents are substitutes, then information acquisition by banks should reduce the signaling value of patents in loan contracting, thereby leading firms to patent less (to avoid the cost of innovation disclosure) without altering their investment in innovation. In other words, if we find that facilitating information acquisition by banks leads to lower information production through patents, our assumption that lenders acquire information which is relevant for innovation could be considered valid.

We describe the empirical methodology and results in detail in Section B.3 of the Online Appendix. In summary, our source of variation in the costs of information acquisition are mergers between loan-granting commercial, or already existing universal, banks and investment banks that offer underwriting services. The underlying rationale is that banks' incentives and ability to acquire information are related to the type of relationship they have with firms. In particular, banks of wide scope, such as universal banks that result from mergers between commercial and investment banks, have more means of attaining information about their borrowers, operationalized through cross-marketing of loans and underwriting services.

To capture this empirically, we compare changes in the disclosure behavior across two groups of firms that each received both a loan from a commercial, or already existing universal, bank and an underwriting service from an investment bank. The treatment group comprises firms that transacted with a commercial, or already 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 banks to form a universal bank, but *not* with each other.

We find that when banks accumulate information about their borrowers through both previous loan and non-loan transactions, firms patent less, while their actual level of innovation – as measured by R&D and related expenditures as well as new-product announcements – is not negatively affected. This suggests that complementary information acquisition from lending and underwriting activities reduces the signaling value of patents in loan contracting. As a consequence, firms patent less to avoid the cost of innovation disclosure, while they do not reduce their investment in innovation.

These findings relate to Bhattacharya and Ritter (1983), who show that the trade-off between patenting and secrecy gives rise to the possibility that firms might prefer financing arrangements that do not require them to use patents as a signal for their innovation, thereby allowing them to finance innovation without disclosing it publicly. One way to facilitate this would be through private-information acquisition in banking relationships, in contrast to public innovation disclosure through patents. In this regard, our findings further buttress our crucial assumption that banks' private information about innovation is valuable because firms would at the margin prefer not to disclose such information.

6 Conclusion

Firms that innovate face a trade-off between patenting and secrecy. In this paper, we argue that this trade-off extends to financing relationships. While patents are a valuable signal about otherwise hard-to-observe innovation, they carry a significant cost as innovation disclosure potentially enables competitors to obtain technical knowledge. We use this trade-off to relate fluctuations in the value of private information to the depth and stability of banking relationships that firms may use to finance innovation.

In particular, we show that when more information about corporate innovation becomes publicly available, the incumbent bank partly loses the advantage that it had in financing the firm due to its previously undertaken information acquisition. This leads to break-ups of existing bank-firm relationships, as other banks become comparatively more competitive in financing the firm. Such reduction in hold-up by incumbent banks is associated with lower cost of borrowing for firms, and makes credit markets more contestable.

The disclosure that we study is different from other types of information that firms might be reluctant to share publicly, i.e., the disclosure of negative news. In our case, firms prior to AIPA are likely not to share innovation-related information in order to keep certain technical knowledge from their competitors. Thus, our results suggest that switching costs in banking relationships might be endogenous to product-market considerations of firms' innovation disclosure. Given that such switching costs are a potential constituent of a bank lending channel in the transmission of monetary policy (Hubbard, Kuttner, and Palia (2002)) and the diffusion of financial shocks, future research could study the welfare effects of the externalities created from interactions in information production in financial and product markets.

7 Tables

Table 1: **Summary Statistics**

Main sample (*bank-firm-period level,* 1996 – 2005 *summarized as two periods*)	Mean	Std. dev.	Min	Max	N
Number of bank-firm pairs					9,333
Number of firms					5,005
Number of banks					476
Loan indicator (sum over both periods)	1.101	0.301	1	2	9,333
Initial relationship in pre-AIPA period	0.573	0.495	0	1	9,333
Proportion of recurrent relationships	0.176	0.381	0	1	5,352
Patenting firm in pre-AIPA period	0.365	0.481	0	1	8,348
Total number of patents in pre-AIPA period	53.158	444.413	0	18,632	8,348
Average cites per patent in pre-AIPA period	2.208	4.873	0	89.300	8,348
Total loan volume per period in 2010 \$bn	0.655	2.823	0.000	89.645	18,666
Mean delay from filing to grant in years (per SIC2 industry in pre-AIPA period)	2.201	0.223	1.656	2.778	64
Median delay from filing to grant in years (per SIC2 industry in pre-AIPA period)	2.048	0.225	1.656	2.726	64
Loans sample (1987 – 2010)	Mean	Std. dev.	Min	Max	N
All-in-drawn spread in bps	186.888	137.695	0.700	1,490.020	16,858
Total cost of borrowing in bps	110.578	96.248	4.443	864.974	10,855
Maturity in years	3.476	2.071	0.083	30.167	17,566
Deal amount in 2010 \$bn	0.446	1.159	0.000	34.282	18,922
Covenant $\in \{0,1\}$	0.470	0.499	0	1	18,922
Secured $\in [0,1]$	0.732	0.442	0.000	1	12,373

Notes: The variables in the top panel correspond to the respective descriptions in Tables 4 to 7, and those in the bottom panel correspond to Tables 12 and B.2.

Table 2: **Correlations between Treatment and Other Industry Characteristics**

	Mean delay from filing to grant in days (1996 − 2000)				
Export penetration	135.047				
	(124.258)				
Import penetration	-51.806				
	(68.311)				
Number of patents filed		0.000			
		(0.000)			
Industry productivity			300.103		
			(249.052)		
Financial dependency				7.930	
				(16.118)	
Industry return					21.737
					(35.930)
N	20	57	53	56	57

Notes: All regressions are estimated at the industry level (based on two-digit SIC codes). The table displays cross-sectional regressions. The dependent variable is the mean difference in days between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. Independent variables are measured as either sums or averages from 1996 to 2000. Export penetration refers to total exports over the total value of shipments in a given SIC2 industry. Import penetration refers to total imports over the total value of shipments plus total imports minus total exports in a given SIC2 industry. Number of patents filed is the number of patents filed in a given SIC2 industry. Industry productivity is the average total factor productivity in a given SIC2 industry from Imrohoroglu and Tuzel (2014). Financial dependency is measured as the median value of financing needs across SIC2 firms, as in Rajan and Zingales (1998). Financing needs are measured as total capital expenditures minus total operating cash flows, over total capital expenditures. Industry return is the industry-level return, equally weighted across firms, from 1996 to 2000. Public-service, energy, and financial-services firms are dropped. Robust standard errors are in parentheses.

Table 3: **Firms' Lending Relationships Before vs. After AIPA – Cross-sectional Evidence**

	Prop. of loan volume from previous banks	Prop. of relationships with previous banks	Δln(1+Total loan vol.)	ΔNo. of banks
Sample	All firms with loan(s) in pre- or post-period			
Treatment	-0.053**	-0.056**	0.587	0.036
	(0.025)	(0.027)	(1.769)	(0.121)
Constant	0.239***	0.244***	-3.600	-0.167
	(0.056)	(0.059)	(3.851)	(0.264)
N	5,005	5,005	5,005	5,005

Notes: All regressions are estimated at the firm level. The sample is limited to firms with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). The dependent variable in the first column is the proportion of the total loan volume of firm i in the post-AIPA period granted by banks that firm i already received a loan from in the pre-AIPA period. The dependent variable in the second column is the proportion of lending relationships (with different banks) of firm i in the post-AIPA period with banks that firm i already contracted with in the pre-AIPA period. The dependent variable in the third column is the difference in the log of the total loan volume of firm i granted by all banks in the post-AIPA period compared to the pre-AIPA period. The dependent variable in the fourth column is the difference in the number of lending relationships (with different banks) in the post-AIPA period compared to the pre-AIPA period. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the two-digit SIC level) are in parentheses.

Table 4: **Impact of AIPA on Intensive Margin of Lending Relationships**

	ln(1+Loan volume)		
Sample	Loan(s) in pre- or post-period		Full matrix
		Placebo	
Treatment × Initial relationship × Post	-1.887***	-0.861	-1.154**
	(0.687)	(0.651)	(0.550)
Initial relationship × Post	-27.767***	-30.045***	-13.435***
	(1.418)	(1.524)	(1.417)
Bank-firm FE	Y	Y	Y
Bank-period FE	Y	Y	Y
Firm-period FE	Y	Y	Y
No. of bank-firm pairs	9,333	8,939	2,382,380
N	18,666	17,878	4,764,760

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). The sample in the first two columns is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). Furthermore, as a placebo test, the sample in the second column is limited to bank-firm (ij) pairs with at least one loan in the pre-period from 1993 to 1997 or in the post-period from 1998 to 2002, whereas AIPA was implemented in late 2000. The sample in the third column comprises all theoretically possible bank-firm (ij) pairs, i.e., including those with zero transactions throughout. The dependent variable is the log of the total volume of all loan transactions between firm i and bank j, separately for the pre- and post-period. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the placebo post-period from 1998 to 2002 in the second column, and for the post-period from 2001 to 2005 in all remaining columns. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 5: **Impact of AIPA on Extensive Margin of Lending Relationships**

	Loan from bank $\in \{0, 1\}$		
Sample	Loan(s) in pre- or post-period		Full matrix
		Placebo	
Treatment × Initial relationship × Post	-0.086***	-0.038	-0.064**
	(0.030)	(0.034)	(0.028)
Initial relationship × Post	-1.458***	-1.591***	-0.707***
	(0.066)	(0.082)	(0.068)
Bank-firm FE	Y	Y	Y
Bank-period FE	Y	Y	Y
Firm-period FE	Y	Y	Y
No. of bank-firm pairs	9,333	8,939	2,382,380
N	18,666	17,878	4,764,760

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). The sample in the first two columns is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). Furthermore, as a placebo test, the sample in the second column is limited to bank-firm (ij) pairs with at least one loan in the pre-period from 1993 to 1997 or in the post-period from 1998 to 2002, whereas AIPA was implemented in late 2000. The sample in the third column comprises all theoretically possible bank-firm (ij) pairs, i.e., including those with zero transactions throughout. The dependent variable is an indicator for the occurrence of any loan transaction between firm i and bank j. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the placebo post-period from 1998 to 2002 in the second column, and for the post-period from 2001 to 2005 in all remaining columns. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 6: **Impact of AIPA on Intensive Margin of Lending Relationships – Quality of Pre-AIPA Patents and Inevitable Disclosure Doctrine (IDD)**

	ln(1+Loan volume)			
Sample	Loan(s) in pre- or post-period			
IDD definition			No reversals	All
Treatment × Initial relationship × Post	-0.058	-0.707	-1.695**	-2.094**
	(0.843)	(0.817)	(0.862)	(0.867)
Initial relationship × Post	-31.601***	-30.236***	-28.442***	-27.519***
	(1.741)	(1.692)	(1.764)	(1.756)
Treatment × Initial rel. × Patenting × Post	-5.657***			
	(1.855)			
Initial relationship × Patenting × Post	11.962***			
	(3.993)			
Treatment × Initial rel. × Avg. cites × Post		-0.518**		
		(0.213)		
Initial relationship × Avg. cites × Post		1.081**		
		(0.460)		
Treatment × Initial relationship × IDD × Post			3.963***	2.985**
			(1.390)	(1.414)
Initial relationship × IDD × Post			-7.709**	-5.986*
			(3.089)	(3.087)
Bank-firm FE	Y	Y	Y	Y
Bank-period FE	Y	Y	Y	Y
Firm-period FE	Y	Y	Y	Y
No. of bank-firm pairs	8,348	8,348	6,071	6,071
N	16,696	16,696	12,142	12,142

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). The sample is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). The sample in the last two columns is furthermore limited to firms that did not change their headquarters. The dependent variable is the log of the total volume of all loan transactions between firm i and bank j, separately for the pre- and post-period. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the post-period from 2001 to 2005. $Patenting_i$ is an indicator variable for whether firm i issued any patents in the pre-period. $Avg.\ cites_i$ is the average number of forward citations per patent across all patents issued by firm i in the pre-period, and defined to be zero in the case of no patents issued. IDD_i reflects whether firm i was exposed to the adoption of the Inevitable Disclosure Doctrine (IDD), and is defined differently across the last two columns. In the third column, it is an indicator variable for whether firm i operated out of a state that had adopted IDD by the first available year of the pre-AIPA period from 1996 to 2000, and did not reverse it thereafter, whereas in the last column, we also include states the courts of which eventually rejected IDD after its adoption (namely, Florida in 2001, Michigan in 2002, and Texas in 2003). Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 7: **Impact of AIPA on Extensive Margin of Lending Relationships – Quality of Pre-AIPA Patents and Inevitable Disclosure Doctrine (IDD)**

	Loan from bank $\in \{0,1\}$			
Sample	Loan(s) in pre- or post-period			
IDD definition			No reversals	All
Treatment × Initial relationship × Post	0.009	-0.019	-0.090**	-0.117***
	(0.042)	(0.039)	(0.041)	(0.042)
Initial relationship × Post	-1.669***	-1.604***	-1.458***	-1.395***
	(0.085)	(0.080)	(0.089)	(0.097)
Treatment × Initial rel. × Patenting × Post	-0.344***			
	(0.097)			
Initial relationship × Patenting × Post	0.760***			
	(0.214)			
Treatment × Initial rel. × Avg. cites × Post		-0.041***		
		(0.011)		
Initial relationship × Avg. cites × Post		0.090***		
		(0.025)		
Treatment × Initial relationship × IDD × Post			0.178**	0.151**
			(0.071)	(0.073)
Initial relationship × IDD × Post			-0.346**	-0.310*
			(0.157)	(0.159)
Bank-firm FE	Y	Y	Y	Y
Bank-period FE	Y	Y	Y	Y
Firm-period FE	Y	Y	Y	Y
No. of bank-firm pairs	8,348	8,348	6,071	6,071
N	16,696	16,696	12,142	12,142

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). The sample is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). The sample in the last two columns is furthermore limited to firms that did not change their headquarters. The dependent variable is an indicator for the occurrence of any loan transaction between firm i and bank j. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the post-period from 2001 to 2005. $Patenting_i$ is an indicator variable for whether firm i issued any patents in the pre-period. $Avg.\ cites_i$ is the average number of forward citations per patent across all patents issued by firm i in the pre-period, and defined to be zero in the case of no patents issued. IDD_i reflects whether firm i was exposed to the adoption of the Inevitable Disclosure Doctrine (IDD), and is defined differently across the last two columns. In the third column, it is an indicator variable for whether firm i operated out of a state that had adopted IDD by the first available year of the pre-AIPA period from 1996 to 2000, and did not reverse it thereafter, whereas in the last column, we also include states the courts of which eventually rejected IDD after its adoption (namely, Florida in 2001, Michigan in 2002, and Texas in 2003). Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 8: **Impact of AIPA on Intensive Margin of Lending Relationships – Robustness I**

	ln(1+Loan volume)			
Sample	At least one loan in pre- or post-period			
Robustness	No licensers	No tech	Experts	Survivors
Treatment × Initial relationship × Post	-1.879**	-2.124***	-3.248***	-1.648**
	(0.813)	(0.694)	(0.954)	(0.703)
Initial relationship × Post	-27.669***	-27.195***	-24.002***	-28.170***
	(1.742)	(1.427)	(1.977)	(1.494)
Bank-firm FE	Y	Y	Y	Y
Bank-period FE	Y	Y	Y	Y
Firm-period FE	Y	Y	Y	Y
No. of bank-firm pairs	7,474	8,102	4,393	7,678
N	14,948	16,204	8,786	15,356

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). Across all columns, the sample is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). In the first column, we exclude all firms involved in licensing alliances anytime during our sample period from 1996 to 2005. In the second column, we drop all high-tech companies, following Ljungqvist and Wilhelm (2003), which are active in the following SIC codes: 3571, 3572, 3575, 3577, 3578 (computer hardware), 3661, 3663, 3669 (communications equipment), 3674 (electronics), 3812 (navigation equipment), 3823, 3825, 3826, 3827, 3829 (measuring and controlling devices), 4899 (communication services), and 7370, 7371, 7372, 7373, 7374, 7375, 7378, and 7379 (software). In the third column, we limit the sample to observations associated with banks in the top third of the distribution of the proportion of loans granted to patenting firms in the pre-period. In the fourth column, firms that were delisted for bankruptcy-related reasons anytime until (and including) 2005 are dropped from the sample. Bankruptcy is identified using the following CRSP delisting codes: any type of liquidation (400-490); price fell below acceptable level; insufficient capital, surplus, and/or equity; insufficient (or non-compliance with rules of) float or assets; company request, liquidation; bankruptcy, declared insolvent; delinquent in filing; non-payment of fees; does not meet exchange's financial guidelines for continued listing; protection of investors and the public interest; corporate governance violation; and delist required by Securities Exchange Commission (SEC). The dependent variable is the log of the total volume of all loan transactions between firm i and bank j, separately for the pre- and post-period. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the post-period from 2001 to 2005. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 9: **Impact of AIPA on Extensive Margin of Lending Relationships – Robustness I**

	Loan from bank $\in \{0, 1\}$			
Sample	At least one loan in pre- or post-period			
Robustness	No licensers	No tech	Experts	Survivors
Treatment × Initial relationship × Post	-0.096***	-0.082**	-0.114***	-0.083***
	(0.036)	(0.033)	(0.041)	(0.030)
Initial relationship × Post	-1.437***	-1.454***	-1.311***	-1.452***
	(0.081)	(0.071)	(0.104)	(0.070)
Bank-firm FE	Y	Y	Y	Y
Bank-period FE	Y	Y	Y	Y
Firm-period FE	Y	Y	Y	Y
No. of bank-firm pairs	7,474	8,102	4,393	7,678
N	14,948	16,204	8,786	15,356

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). Across all columns, the sample is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). In the first column, we exclude all firms involved in licensing alliances anytime during our sample period from 1996 to 2005. In the second column, we drop all high-tech companies, following Ljungqvist and Wilhelm (2003), which are active in the following SIC codes: 3571, 3572, 3575, 3577, 3578 (computer hardware), 3661, 3663, 3669 (communications equipment), 3674 (electronics), 3812 (navigation equipment), 3823, 3825, 3826, 3827, 3829 (measuring and controlling devices), 4899 (communication services), and 7370, 7371, 7372, 7373, 7374, 7375, 7378, and 7379 (software). In the third column, we limit the sample to observations associated with banks in the top third of the distribution of the proportion of loans granted to patenting firms in the pre-period. In the fourth column, firms that were delisted for bankruptcy-related reasons anytime until (and including) 2005 are dropped from the sample. Bankruptcy is identified using the following CRSP delisting codes: any type of liquidation (400-490); price fell below acceptable level; insufficient capital, surplus, and/or equity; insufficient (or non-compliance with rules of) float or assets; company request, liquidation; bankruptcy, declared insolvent; delinquent in filing; non-payment of fees; does not meet exchange's financial guidelines for continued listing; protection of investors and the public interest; corporate governance violation; and delist required by Securities Exchange Commission (SEC). The dependent variable is an indicator for the occurrence of any loan transaction between firm i and bank j. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the post-period from 2001 to 2005. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 10: **Impact of AIPA on Intensive Margin of Lending Relationships – Robustness II**

	ln(1+Loan volume)			
Sample	At least one loan in pre- or post-period			
Robustness	Median delay	3y window	Firm delay	Examiners
Treatment × Initial relationship × Post	-1.807***	-1.933***	-1.553**	-2.034***
	(0.576)	(0.745)	(0.736)	(0.696)
Initial relationship × Post	-28.218***	-28.910***	-28.882***	-28.404***
	(1.117)	(1.548)	(1.793)	(1.174)
Bank-firm FE	Y	Y	Y	Y
Bank-period FE	Y	Y	Y	Y
Firm-period FE	Y	Y	Y	Y
No. of bank-firm pairs	9,333	5,917	2,321	9,333
N	18,666	11,834	4,642	18,666

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). In the first, third, and fourth column, the sample is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). In the second column, we vary the time window around AIPA to three years (pre-period from 1998 to 2000, post-period from 2001 to 2003). The dependent variable is the log of the total volume of all loan transactions between firm i and bank j, separately for the pre- and post-period. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the median difference in the first column, and the mean difference in the second column, in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. In the third column, $Treatment_i$ is defined at the firm level (conditional on firms having patented at least once between 1996 and 2000), and measures the mean difference between the filing date and the grant date for all patents of firm i between 1996 and 2000. In the fourth column, $Treatment_i$ is at the industry level and measured using only the portions of delays that were more likely to be due to examiners. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the post-period from 2001 to 2005 in the first, third and fourth columns, and from 2001 to 2003 in the second column. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 11: **Impact of AIPA on Extensive Margin of Lending Relationships – Robustness II**

	Loan from bank $\in \{0,1\}$			
Sample	At least one loan in pre- or post-period			
Robustness	Median delay	3y window	Firm delay	Examiners
Treatment × Initial relationship × Post	-0.098***	-0.085***	-0.057*	-0.096***
	(0.028)	(0.030)	(0.035)	(0.033)
Initial relationship × Post	-1.446***	-1.515***	-1.507***	-1.482***
	(0.059)	(0.062)	(0.089)	(0.060)
Bank-firm FE	Y	Y	Y	Y
Bank-period FE	Y	Y	Y	Y
Firm-period FE	Y	Y	Y	Y
No. of bank-firm pairs	9,333	5,917	2,321	9,333
N	18,666	11,834	4,642	18,666

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). In the first, third, and fourth column, the sample is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). In the second column, we vary the time window around AIPA to three years (pre-period from 1998 to 2000, post-period from 2001 to 2003). The dependent variable is an indicator for the occurrence of any loan transaction between firm i and bank j. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the median difference in the first column, and the mean difference in the second column, in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. In the third column, $Treatment_i$ is defined at the firm level (conditional on firms having patented at least once between 1996 and 2000), and measures the mean difference between the filing date and the grant date for all patents of firm i between 1996 and 2000. In the fourth column, $Treatment_i$ is at the industry level and measured using only the portions of delays that were more likely to be due to examiners. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the post-period from 2001 to 2005 in the first, third and fourth columns, and from 2001 to 2003 in the second column. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 12: **Impact of AIPA on Firms' Cost of Debt**

	ln(All-in-drawn spread)			ln(Total cost of borrowing)		
Treatment × Post	-0.127**	-0.056	-0.559***	-0.199***	-0.124*	-1.539***
	(0.053)	(0.063)	(0.177)	(0.067)	(0.074)	(0.539)
Treatment × Patenting × Post		-0.296**			-0.257*	
		(0.122)			(0.148)	
Patenting × Post		0.658**			0.540*	
		(0.282)			(0.326)	
Treatment × Initial relationship × Post			0.446**			1.355**
			(0.197)			(0.562)
Initial relationship × Post			-0.656			-2.627**
			(0.443)			(1.227)
Controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Industry-year (SIC1) FE	Y	Y	Y	Y	Y	Y
N	16,858	14,958	16,858	10,855	9,988	10,855

Notes: The sample consists of all completed syndicated loans (package level) of publicly listed firms i at date t granted by lead arranger(s) j. The dependent variable in the first three columns is the log of the all-in-drawn spread (in bps), which is the sum of the spread over LIBOR and any annual fees paid to the lender syndicate. The dependent variable in the last three columns is the log of the total cost in borrowing (in bps), as defined in Berg, Saunders, and Steffen (2016). $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Post_t$ is a dummy variable for the post-AIPA period from 2001 onwards. $Patenting_i$ is an indicator variable for whether firm i issued any patents during the pre-AIPA period from 1996 to 2000. $Initial\ relationship_{ij}$ is a dummy variable for whether firm i already received at least one loan from lead arranger j anytime during the pre-AIPA period from 1996 to 2000; the variable is non-zero only for the post-AIPA period ($Post_t = 1$). Control variables are measured in year t, and include the log of firm i's sales and the log of its number of employees. Bank fixed effects are included for all lead arrangers. 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 bank level) are in parentheses.

ONLINE APPENDIX

A Summary of the Legislative Process behind the Passage of AIPA

In this section, we briefly summarize the legislative process behind the passage of AIPA, based on Ergenzinger (2006). All quotations that are marked as such are taken from Ergenzinger (2006).

The American Inventor's Protection Act (AIPA) was signed into law by Bill Clinton on November 29, 1999. Its origin dates back to 1995 when Sen. Joseph Lieberman (D-Conn.) first introduced a bill intended to protect independent inventors from exploitation by invention-development companies. At the time, the bill was well received by independent inventors and their allies, yet what started as a straightforward patent bill to protect inventors ended up evolving into AIPA, a $390 billion omnibus spending bill implementing the biggest changes to patent law since 1952. The process of passing AIPA turned out extremely convoluted and lasted three Congresses, inciting four years of heated debates among politicians, activists, and Nobel Prize winners, and encountering multiple roadblocks in political institutions.

The problematic nature of AIPA's legislative process was first revealed in the 104$^{\text{th}}$ Congress. On June 9, 1995, Sen. Lieberman introduced S.909, also known as the Inventor Protection Act of 1995. It had a companion bill H.R. 2419 that was later introduced by Rep. Moorhead, both bills being aimed at protecting individual inventors from fraudulent practices by invention-development firms. These patent reforms that were introduced with H.R. 2419 came from different sources and complicated the legislative process. A few of them were related to the Uruguay Round Agreements Act (URAA) provisions which were negotiated between Japan and the U.S. under the Global Agreement on Tariffs and Trade (GATT), among which was H.R. 1733, which would require to publish patent applications 18 months after the initial filing date.

H.R. 1733 in particular received strong opposition from Rep. Rohrabacher, who claimed that this bill was a "concession to Japan that would weaken the U.S. patent system." He also predicted that "patent lawyers from foreign companies would cull the USPTO files and fax published applications directly to competitors in Thailand, China, Korea, and Japan." Rep. Rohrabacher insisted that mitigating this problem would necessitate the applicant to obtain a world-wide patent, which would be cost-prohibitive for most independent inventors. His opposition to H.R. 1733 showcased the inherent conflicting interests in the proposed reforms.

To facilitate the passage of new reforms in the 104th Congress, H.R. 1733 and four other proposed reforms were combined into a single omnibus patent-reform bill H.R. 3460. Despite facing criticism from Rep. Rohrabacher as well as from independent inventor groups for favoring large corporate patent holders, H.R. 3460 was expected to facilitate the passage of the multiple reforms into law. However, H.R. 3460 did not reach the voting stage due to a lack of consensus and budget problems. Thus, H.R. 3460 and its constituent patent reforms did not come into law during the 104th Congress.

The 105th Congress saw the introduction of H.R. 400 that was nearly identical to H.R. 3460 from the previous Congress. Despite its supporters seemingly having the upper hand over the opposition, H.R. 400 still faced significant problems and failed to materialize into AIPA during the 105th Congress. While H.R. 400 finally made it through the House, its companion bill S. 507 was facing strong opposition in the Senate. The bill's progress in the Senate was further interrupted in 1997 when the opposition to S. 507 was joined by a noted conservative pundit Phyllis Schlafly. Schlafly "maintained that the bill was an ominous attack on independent inventors, calling the bill the result of a game plan by the lobbyists for 'foreigners and multinationals' to steal American technology." She insisted that S. 507 had no redeeming value.

Besides Schlafly, 26 Nobel laureates in Economics, Physics, Chemistry, and Medicine expressed their opposition to the bill in the fall of 1997, claiming that S. 507 would be damaging to American small inventors and go against the spirit of the U.S. patent system. They stated that "provisions for 18-month publication and prior-user rights would curtail the protection obtained through patents for small businesses and individual inventors relative to large multi-national corporations, and thus would discourage the flow of new inventions." In an individual statement, Franco Modigliani wrote that "the effort to rush through the Senate this questionable and potentially highly detrimental legislation is inexcusable," and that S. 507 "is against the spirit of the U.S. patent system, which is a great economic and cultural invention."

Ultimately, in 1998 the supporters of the bill tried to attach S. 507 to a separate bill as an amendment due to the reluctance of Republicans to allow the bill to reach the floor by itself. However, objections from the Republican side prevented the amendment from being offered for a vote, and the omnibus patent reform was not passed in the 105th Congress.

In the 106th Congress, the omnibus patent reform was called AIPA for the first time and was eventually passed, though not without difficulty. Despite opposition from Schlafly and the Alliance for American Innovation, which claimed to represent small inventors, the House passed H.R. 1907 on August 4, 1999. Having been passed in the House, the bill faced another difficulty: "any Senate Bill was anticipated to lag the House due to the Senate's preoccupation with the impeachment trial of President Clinton."

In the Senate, the proposed AIPA reform bill was included into a much larger $385 billion spending package along with two other intellectual property bills, the "Anti-Cybersquatting Act" and the "Satellite Home Viewer Act." The omnibus spending bill was approved by the Senate, and on November 29, 1999, ten days after the vote in the Senate, President Bill Clinton signed AIPA into law. AIPA came into effect one year later, namely on November 29, 2000, which was the first date at which patent applications would be subject to it.

B Supplementary Tables

Table B.1: **Impact of AIPA on Lending Relationships of Patenting Firms – Stable Control Group**

	ln(1+Loan volume)		Loan from bank $\in \{0,1\}$	
Sample	At least one loan in pre- or post-period, pre-AIPA delay $\leq$ 2y			
Delay (treatment) measure	Continuous	Binary	Continuous	Binary
Treatment $\times$ Initial relationship $\times$ Post	-5.696***	-3.902***	-0.177**	-0.145***
	(2.056)	(1.056)	(0.085)	(0.041)
Initial relationship $\times$ Post	-22.209***	-28.851***	-1.326***	-1.512***
	(3.444)	(1.256)	(0.138)	(0.055)
Bank-firm FE	Y	Y	Y	Y
Bank-period FE	Y	Y	Y	Y
Firm-period FE	Y	Y	Y	Y
No. of bank-firm pairs	901	901	901	901
N	1,802	1,802	1,802	1,802

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). The sample is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to AIPA (pre-period from 1996 to 2000) or within the first five years after AIPA (post-period from 2001 to 2005). Furthermore, the sample is limited to firms that patented at least once in the pre-AIPA period from 1996 to 2000, and for which the mean difference between the filing date and the grant date for these patents was at most two years. The dependent variable in the first two columns is the log of the total volume of all loan transactions between firm i and bank j, separately for the pre- and post-period. The dependent variable in the last two columns is an indicator for the occurrence of any loan transaction between firm i and bank j. In the first and third column, $Treatment_i$ is defined at the firm level, and measures the mean difference between the filing date and the grant date for all patents of firm i between 1996 and 2000. In the second and fourth column, $Treatment_i$ is re-defined to be equal to 0 for all firms with average delays below 18 months, and 1 for all firms with average delays of at least 18 months. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the post-period from 2001 to 2005. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table B.2: **Impact of AIPA on Firms' Loan Conditions**

	ln(Maturity)	ln(Loan amount)	Covenant $\in \{0,1\}$	Secured $\in [0,1]$
Treatment $\times$ Post	0.392*	-0.167	-0.018	-0.206
	(0.229)	(0.229)	(0.149)	(0.190)
Treatment $\times$ Initial rel. $\times$ Post	-0.405	-0.015	-0.002	0.120
	(0.268)	(0.260)	(0.159)	(0.197)
Initial relationship $\times$ Post	0.716	0.008	-0.003	-0.166
	(0.584)	(0.599)	(0.352)	(0.433)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y
Industry-year (SIC1) FE	Y	Y	Y	Y
N	17,566	18,922	18,922	12,373

Notes: The sample consists of all completed syndicated loans (package level) of publicly listed firms i at date t granted by lead arranger(s) j. The dependent variable in the first column is the logged maturity, in the second column the log of the loan amount, in the third column an indicator for whether the loan has at least one financial covenant, and in the fourth column the proportion of facilities within the package that are secured. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Post_t$ is a dummy variable for the post-AIPA period from 2001 onwards. $Initial\ relationship_{ij}$ is a dummy variable for whether firm i already received at least one loan from lead arranger j anytime during the pre-AIPA period from 1996 to 2000; the variable is non-zero only for the post-AIPA period ($Post_t = 1$). Control variables are measured in year t, and include the log of firm i's sales and the log of its number of employees. Bank fixed effects are included for all lead arrangers. 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 bank level) are in parentheses.

Table B.3: **Impact of AIPA on Firms' Ability to Raise Capital in Public Markets**

	$\frac{\text{Public issues}}{\text{Public issues + Loans}}$	$\frac{\text{Debt issues}}{\text{Public issues + Loans}}$	$\frac{\text{Equity issues}}{\text{Public issues + Loans}}$
Treatment × Post	0.114*	0.086	0.027
	(0.061)	(0.064)	(0.053)
Controls	Y	Y	Y
Firm FE	Y	Y	Y
Industry-year (SIC1) FE	Y	Y	Y
Mean of dep. variable	0.747	0.222	0.525
Std. dev. of dep. variable	0.434	0.401	0.487
N	23,219	23,219	23,219

Notes: The sample consists of all available observations from Compustat, conditional on the respective firm i raising any capital in public markets as recorded in SDC; the unit of observation is the firm-year level it. For the dependent variables, $\textit{Public issues}_{it}$ denotes the sum of total debt and equity financing of firm i through public capital markets (as recorded in SDC) in year t, $\textit{Loans}_{it}$ is the total debt financing of firm i through syndicated loans (as recorded in DealScan) in year t, and $\textit{Debt issues}_{it}$ and $\textit{Equity issues}_{it}$ are equal to, respectively, total debt and total equity financing of firm i through public capital markets (as recorded in SDC) in year t. $\textit{Treatment}_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $\textit{Post}_t$ is a dummy variable for the post-AIPA period from 2001 onwards. Control variables are measured in year t, and include the log of firm i's sales and the log of its number of employees. 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 level) are in parentheses.

C Effect of Information Acquisition by Universal Banks on Firm-level Patenting and Innovation

In this section, we devise an empirical strategy to test whether banks acquire private information that is related to their borrowers' innovation. In line with the reasoning of a trade-off between patenting and secrecy, we show that when lender informedness increases, firms patent less, arguably to avoid the cost of innovation disclosure, without altering their investment in innovation.

C.1 Bank-level Information Acquisition and Universal Banking

One determinant of the level of information asymmetry between lenders and borrowers is the scope of bank-firm interactions. In particular, the stepwise repeal of the Glass-Steagall Act in the U.S. allowed commercial banks to become universal banks that could offer a wide array of financial instruments. The Glass-Steagall Act of 1933 originally imposed a separation of commercial banking (deposit taking and lending) and investment banking (especially underwriting of corporate securities). The first major step of the repeal, which ultimately culminated in the Gramm-Leach-Bliley Act in 1999, took place in January and September 1989. Starting in 1989, commercial banks were allowed to generate a certain proportion (10%) of their revenues through underwriting activities, including underwriting of corporate debt and equity, typically through so-called Section 20 subsidiaries.

However, there were still firewalls in place that separated the two activities, and did not allow universal banks to share non-public customer information across commercial-bank and securities divisions. The respective firewalls were abolished by the Federal Reserve Board in a second step on August 1, 1996. Simultaneously, the revenue limit on underwriting securities was raised from 10 to 25%, allowing more commercial banks to expand into universal banking by directly merging with an investment bank.

Drucker and Puri (2005) and Neuhann and Saidi (2016) argue that this improved universal banks' ability to derive informational economies of scope across loans and non-loan products. For example, a firm's downside is crucial for a credit analyst who is assessing a firm's quality as a borrower, whereas an equity-underwriting analyst focuses on a firm's upside when trying to justify its stock price for an initial public or seasoned equity offering.

Variation in bank-level information acquisition. As in Neuhann and Saidi (2016), we identify the impact of lender informedness using the variation in bank-scope-expanding mergers, i.e., between loan-granting commercial, or already existing universal, banks and underwriting investment banks. Based on a firm-year-level Compustat sample, including observations on years in which firms did not receive any loans or underwriting products, we estimate the following regression specification:

$$
\begin{aligned}
y_{it} \quad = \quad & \beta_1 \textit{Loan from CB, underwriting from IB, both merged with each other}_{it} \\
& +\beta_2 \textit{Loan from CB that merged with IB}_{it} \\
& \times \textit{Underwriting from IB that merged with CB}_{it} \\
& +\beta_3 \textit{Loan from CB that merged with IB}_{it} \\
& +\beta_4 \textit{Underwriting from IB that merged with CB}_{it} \\
& +\beta_5 \textit{Any loan}_{it} \times \textit{Any underwriting}_{it} + \beta_6 \textit{Any loan}_{it} + \beta_7 \textit{Any underwriting}_{it} \\
& +\beta_8 X_{it} + \mu_i + \epsilon_{it}, \qquad (2)
\end{aligned}
$$

where y_{it} is an outcome variable at the firm-year level, *Loan from CB, underwriting from IB, both merged with each other*$_{it}$ 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, *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, and *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. *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 $t-1$, X_{it} denotes other control variables measured in year t, including state-year and industry-year fixed effects, and μ_i denotes firm fixed effects. Standard errors are clustered at the firm level.

We use a time window of eleven years so as to accommodate the triplet of events (loan transaction, underwriting, and any mergers).[17] Note that our ten-year window for the two transactions (loans and underwriting) starts in $t-1$, rather than t (the last possible year that we consider for a potential merger), so as to safeguard that both transactions took place before any potential merger of the two banks, rather than them being a result of the merger.

Both treated and control firms received 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. Hence, the independent variables associated with β_2 through β_7 are equal to 1 for treatment and control. The only difference between the two groups is that the treatment group's underwriter and commercial bank became a universal bank by merging *with each other*, while the control group's underwriter and commercial bank became part of a universal bank by merging with *some other* bank of complementary scope.

This difference is captured by β_1, which estimates whether a firm that received a loan from a commercial, or already existing universal, bank *and* an underwriting product (debt or equity) from a separate investment bank changed its behavior after the two respective banks merged with each other. Treatment and control differ in that only universal banks of treated firms are able to pool information from past loans and underwriting transactions. β_1 can be interpreted as an intention-to-treat effect insofar as both sets of firms are likely to continue contracting with the surviving universal banks. This is indeed reflected in our sample. Among firms in the treatment group, 50.9% (68.3%) return to the merged universal bank for another loan (underwriting product) within five years after the merger, and in the control group, 52.1% (59.8%) return to *any one of the two universal banks* involved in mergers for another loan (underwriting product). These ex-post probabilities are high, and remarkably similar.

[17] In untabulated tests, we show that our results are robust to changing the time window for the triplet of events from eleven years to nine years.

The underlying identifying assumption is that individual firms are small enough in order to not be decisive for the respective banks' decision to merge with each other. If this did not hold, it would not be appropriate to assume parallel trends for our treated and control firms, which is necessary for β_1 to be identified. In Figure C.1, we verify that the patenting behavior and R&D expenditures among our treatment and control groups are parallel in the period leading up to the merger(s) of commercial/universal and investment banks.

A potential remaining concern could be that banks of complementary scope merge with each other not in order to realize informational economies of scope from contracting with specific firms, but to explore other complementarities based on their portfolio characteristics some of which may correlate with characteristics of the specific firm in question. To control for this possibility and other sources of time-varying unobserved heterogeneity at the industry level (of the client firm), we include industry-year fixed effects. Finally, we also add state-year fixed effects to control for state-level regulatory shocks to banks in our sample period that may govern the supply of loans and non-loan products as well as bank mergers.

Data description. We match the NBER patent dataset with Compustat data. As we analyze the possibility that patents are a way to disclose information, we wish to control for the actual level of corporate innovation. We measure inputs of the innovation process by firms' research and development (R&D) expenditures.[18] We also appreciate that not all inputs of the innovation process (e.g., CEO compensation) are recorded as R&D. As an alternative measure, we consider the sum of R&D as well as selling, general, and administrative (SG&A) expenditures. Similarly, as some of the inputs in the innovation process (e.g., innovators' hardware) are recorded as investment into fixed assets, we also consider firms' capital expenditure and other assets.

In terms of outputs of the innovation process, we focus on new-product announcements, following Mukherjee, Singh, and Žaldokas (2017). In constructing measures of new-product announcements, we combine textual analysis with event studies. We first search the Lexis-Nexis news database for company press releases which are tagged under the subject "new

[18] We follow Koh and Reeb (2015) in that we replace missing values for R&D expenditures in Compustat by zeros if the respective firm is recorded to have issued zero patents in the NBER dataset.

products" and the headlines of which contain any keywords, or roots of words, such as "launch," "product," "introduce," "begin," or "unveil." From these press releases, we parse out firm ticker symbols and the date of the announcement. We only consider firms listed on NYSE, NASDAQ, or AMEX. Using this criterion, we obtain 98,221 unique press releases.

We next identify material information about new products among these press releases. The underlying idea is that if a press release containing our new-product keywords indeed refers to a major innovation, the stock market should respond to the news. Similar in spirit to Kogan, Papanikolaou, Seru, and Stoffman (2017), who estimate the value of patents by relying on stock-price reactions to patent grants, we calculate firms' stock-price reactions to measure the expected value of the product announcement.

We use event-study methodology by fitting a market model over the (-246,-30) period to yield the expected returns on the firm's stock, and then estimating cumulative abnormal returns over the (-1,1) day period around the announcement. After estimating abnormal returns, we are left with 56,797 announcements. To obtain the total number of material new-product announcements over the year, we either (i) count the number of positive cumulative abnormal returns around product announcements made by firms over the year, or (ii) count the number of announcements with cumulative abnormal returns above the 75^{th} percentile in the sample (2.61%).[19]

To detect lending relationships, we use our loans sample from DealScan. In doing so, we focus on the lead arranger(s) of said loans. We match the respective borrowers with data on debt and equity underwriting in SDC. Building on Neuhann and Saidi (2016), we generate unique bank identifiers across these datasets. This enables us to use the SDC M&A database, alongside any mergers that we record through a LexisNexis news search, to detect mergers between any two banks in our DealScan loan data and SDC underwriting data. In this manner, we identify 150 scope-expanding mergers between commercial, or already existing universal, banks and investment banks from 1990 to 2010.

[19] We focus on positive abnormal returns to remove any announcements that were not associated with new-product introductions, such as "delays in new-product introductions" or "new-product recalls."

In Table C.1, we present the respective summary statistics. Our sample starts in 1987 and ends in 2010, with the exception of the NBER patent data, which are available only until 2006. For the purpose of including state-year fixed effects, in Tables C.2 to C.4, the sample is limited to firms that did not change their headquarters.[20]

C.2 Empirical Results

We estimate regression specification (2), and present the results for firms' patenting behavior in Table C.2. In this setting, treatment and control differ only in whether their previous contracting partners – commercial/universal and investment banks – merged with each other, rather than with other banks of complementary scope. The treatment effect is captured by the coefficient on *Loan from CB, underwriting from IB, both merged with each other*$_{it}$, and reflects the impact of increased lender informedness (thanks to pooling information from past loans and underwriting transactions) on firms' patenting behavior. In particular, treated and control firms should not differ in the extent to which they would profit from intellectual property rights or any other benefits of patents, except for the signaling value that patents have for the respective bank-firm relationship.

All regressions include industry-year fixed effects, which capture some of the time-varying factors underlying banks' considerations to merge with each other, such as the nature of client portfolios, as well as other industry-level shocks, e.g., AIPA. We also include state-year effects, which capture any confounding effects of, e.g., state-level changes in banking regulation or the Inevitable Disclosure Doctrine (IDD).

In the first column of Table C.2, we find that treated firms issue 27.9% fewer patents. In the next two columns, we investigate whether treated firms stop patenting altogether, or whether they only cut back on patenting. In the second column, we find no effect on an indicator for whether a given firm issues *any* patents. Instead, in the third column, we find a significantly negative treatment effect on the number of patents after conditioning on years

[20] All results are robust to omitting state-year fixed effects and using the subsequently larger sample.

in which firms issue non-zero patents, implying that treated firms do not stop patenting.[21]

Finally, we consider the possibility that firms may be cutting back on low-quality patents. This is, however, not the case, as can be seen in the fourth column, where we use as dependent variable the total number of forward citations across all patents. Treated firms' patents are associated with significantly fewer citations, so our negative treatment effect pertains also to high-quality patents.

To show that treated firms do not patent less because they become less innovative, we consider various measures that reflect inputs and outputs of the innovation process. In the first two columns of Table C.3, we find no effect on R&D or the sum of R&D and SG&A expenditures. We also fail to find any effect on our measures of the number of new-product announcements in the last two columns.

Furthermore, in the first column of Table C.4, we find a positive, albeit insignificant, effect on capital expenditure. In the remaining columns, we test whether the negative treatment effect on patents holds generally for any class of collateralizable assets, because patents may be used as collateral (Mann (2016)). However, we find only positive, and no negative, treatment effects on assets as well as property, plant, and equipment.

Our findings suggest that firms' incentives for signaling the quality of their otherwise hard-to-observe innovation decrease after a positive shock to universal banks' information about their borrowers. Finally, we investigate a competing explanation, other than increased lender informedness, for the treatment effect on patents. Namely, firms' ability to afford costly patents may be adversely affected by deeper banking relationships, as universal banks could extract more rents from their borrowers by charging higher loan rates. However, in further untabulated tests, we find no changes in charged loan spreads associated with loans of treated firms after the respective universal-bank mergers.

[21] All patent-related results are robust to excluding all observations after the year 2003, as motivated by Dass, Nanda, and Xiao (2017), so as to avoid any truncation bias present in the NBER patent dataset.

C.3 Figures

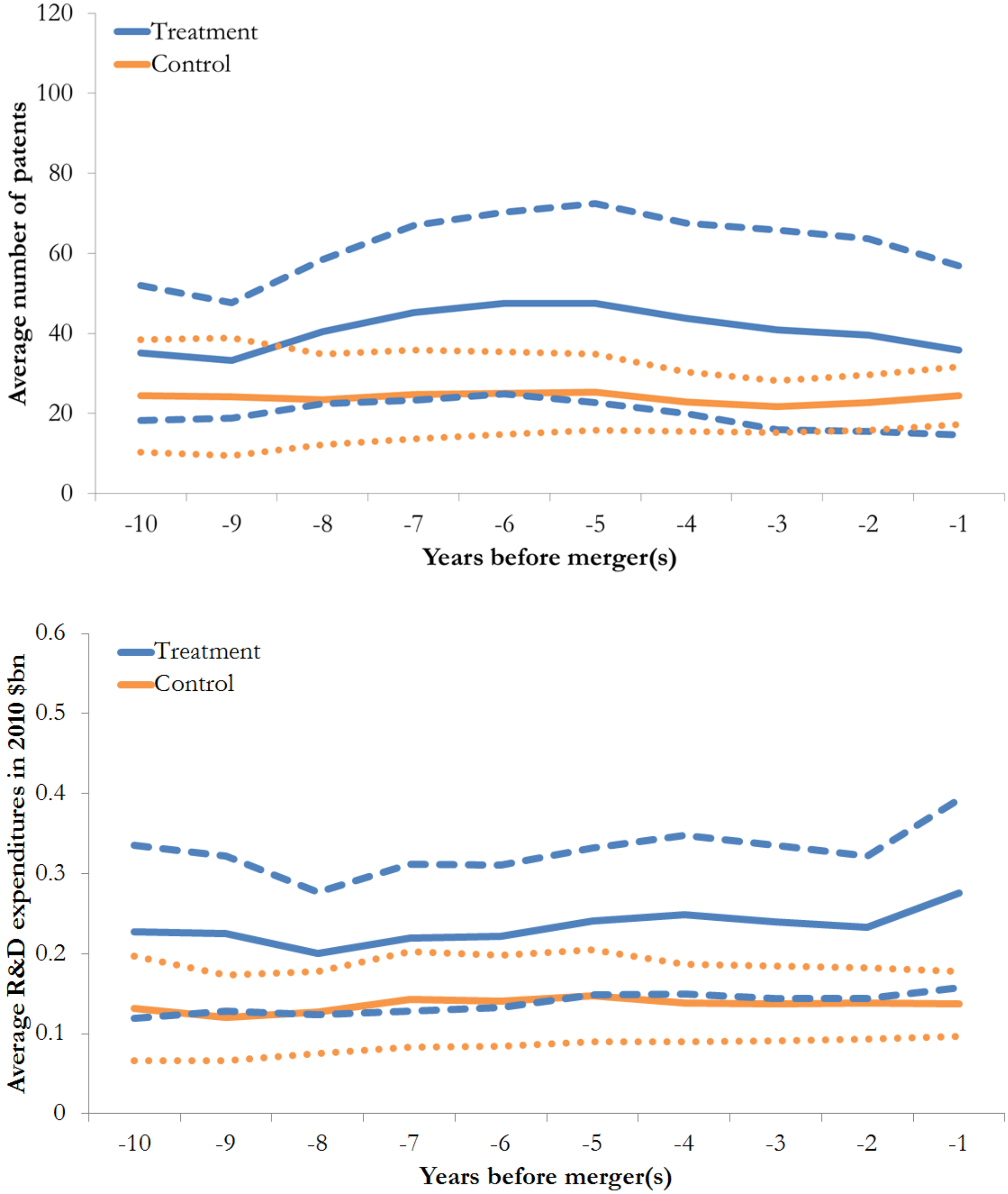

Figure C.1: **Pre-trends among Treatment and Control Firms Contracting with Universal Banks.** The graphs in the top panel and the bottom panel plot, respectively, the average number of patents and the average research and development (R&D) expenditures by firms in the treatment and the control group over ten years. Firms in both groups received a loan from a commercial or universal bank as well as an underwriting product (debt or equity) from an investment bank anytime from year -10 to -1, and both banks merged with each other (treatment group) or with other banks of complementary scope (control group) in year 0.

C.4 Tables

Table C.1: **Summary Statistics**

Compustat sample (firm-year level, 1987 – 2010)	Mean	Std. dev.	Min	Max	N
Number of patents	9.823	75.040	0	4,344	52,015
Patenting $\in \{0, 1\}$	0.383	0.486	0	1	52,015
Total cites of patents (if Patenting = 1)	210.525	1,188.558	0	45,559	19,914
R&D expenditures in 2010 $bn	0.067	0.419	0.000	14.434	63,921
R&D + SG&A expenditures in 2010 $bn	0.463	2.195	0.000	79.347	58,554
New-product announcements	0.248	1.928	0	110	93,181
New-product announcements*	0.125	0.967	0	56	93,181
Capital expenditure in 2010 $bn	0.133	0.806	0.000	44.326	91,686
Book value of assets in 2010 $bn	2.190	14.286	0.000	866.122	93,181
Gross PP&E in 2010 $bn	1.287	7.563	0.000	373.938	92,672
Net PP&E in 2010 $bn	0.719	4.017	0.000	199.548	92,895
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

Notes: The variables correspond to the respective descriptions in Tables C.2 to C.4.

Table C.2: **Impact of Bank Information Acquisition on Firm-level Patenting**

Sample	ln(1+Patents) All	Patenting $\in \{0,1\}$ All	ln(Patents) Patents $\neq 0$	ln(Cites) Patents $\neq 0$
Loan from CB, underwriting from IB,	-0.279***	-0.021	-0.128*	-0.415***
both merged with each other	(0.059)	(0.018)	(0.073)	(0.104)
Loan from CB that merged with IB	-0.028	-0.005	0.016	0.058
$\times$ Underwriting from IB	(0.036)	(0.014)	(0.049)	(0.073)
that merged with CB				
Loan from CB that merged with IB	0.035	0.010	0.084**	0.057
	(0.022)	(0.011)	(0.038)	(0.060)
Underwriting from IB	0.097***	0.020*	0.065*	0.044
that merged with CB	(0.028)	(0.012)	(0.038)	(0.056)
Any loan $\times$ Any underwriting	-0.000	-0.006	0.031	0.022
	(0.022)	(0.012)	(0.039)	(0.064)
Any loan	-0.026	-0.012	-0.063	-0.078
	(0.022)	(0.012)	(0.048)	(0.072)
Any underwriting	-0.012	-0.002	-0.031	-0.013
	(0.016)	(0.008)	(0.028)	(0.041)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
N	52,015	52,015	19,914	19,914

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

Table C.3: **Impact of Bank Information Acquisition on Firm-level Innovation**

	ln(R&D)	ln(R&D+SG&A)	ln(1+Products)	ln(1+Prod.*)
Loan from CB, underwriting from IB,	0.024	0.009	0.017	-0.005
both merged with each other	(0.027)	(0.016)	(0.015)	(0.011)
Loan from CB that merged with IB	-0.052**	-0.017	0.016	0.012
× Underwriting from IB	(0.024)	(0.016)	(0.013)	(0.010)
that merged with CB				
Loan from CB that merged with IB	0.065***	0.049***	0.016**	0.009*
	(0.018)	(0.013)	(0.007)	(0.005)
Underwriting from IB	0.090***	0.046***	0.024**	0.012
that merged with CB	(0.020)	(0.013)	(0.011)	(0.008)
Any loan × Any underwriting	0.026	-0.004	-0.005	-0.004
	(0.018)	(0.012)	(0.007)	(0.005)
Any loan	-0.053***	-0.029**	-0.012*	-0.007
	(0.019)	(0.013)	(0.007)	(0.005)
Any underwriting	0.015	0.060***	0.006	0.007
	(0.013)	(0.009)	(0.006)	(0.004)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
N	63,921	58,554	93,181	93,181

Notes: The sample consists of all available observations for firms in Compustat that did not change their headquarters, the unit of observation is the firm-year level it. The dependent variable in the first column is the log of firm i's research and development (R&D) expenditures in year t, in the second column the logged sum of firm i's R&D and selling, general, and administrative (SG&A) expenditures in year t, and in the third column the logged number of firm i's new-product announcements in year t, for which we use event-study methodology by fitting a market model over the (-246,-30) period to yield the expected returns on the firm's stock, estimating cumulative abnormal returns (CARs) over the (-1,1) period around the announcement, and finally counting all announcements associated with positive CARs over the year. In the last column, we use as dependent variable an alternative definition for the logged number of firm i's new-product announcements in year t, counting all announcements associated with CARs above the 75^{th} percentile in the sample over the year. *Loan from CB that merged with* IB_{it} is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank that merged with an investment bank thereafter. *Underwriting from IB that merged with* CB_{it} is an indicator variable for whether anytime from $t-10$ to $t-1$, firm i received an underwriting product from an investment bank that merged with a commercial or universal bank thereafter. The interaction of the latter two indicator variables is to be distinguished from the explanatory variable of interest in the first row, which indicates whether anytime from $t-10$ to $t-1$, firm i received a loan from a commercial or universal bank, an underwriting product from an investment bank, and both banks merged with each other until year t. *Any* $loan_{it}$ and *Any* $underwriting_{it}$ are indicator variables for whether firm i received any loan or any underwriting product, respectively, from any commercial, universal, or investment bank anytime from $t-10$ to $t-1$. Unless mentioned otherwise, control variables are measured in year t, and include the log of firm i's sales, the log of its number of employees, the log of the average ratio of deal size across all loans over firm i's assets from $t-10$ to $t-1$, and the proportion of refinancing loans from $t-10$ to $t-1$. State-year fixed effects are based on firm i's headquarters in year t. Industry-year fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm level) are in parentheses.

Table C.4: **Impact of Bank Information Acquisition on Firm-level Capital Expenditure and Assets**

	ln(CapEx)	ln(Assets)	ln(Gross PP&E)	ln(Net PP&E)
Loan from CB, underwriting from IB,	0.020	0.035**	-0.001	0.038**
both merged with each other	(0.023)	(0.016)	(0.016)	(0.018)
Loan from CB that merged with IB	-0.038	0.004	-0.040**	-0.045**
× Underwriting from IB	(0.026)	(0.016)	(0.017)	(0.020)
that merged with CB				
Loan from CB that merged with IB	0.053**	0.052***	0.028**	0.063***
	(0.021)	(0.012)	(0.014)	(0.017)
Underwriting from IB	0.032	0.048***	0.072***	0.057***
that merged with CB	(0.022)	(0.015)	(0.015)	(0.018)
Any loan × Any underwriting	0.025	0.037***	-0.033**	-0.029
	(0.022)	(0.013)	(0.014)	(0.019)
Any loan	-0.091***	-0.067***	0.050***	0.001
	(0.023)	(0.013)	(0.016)	(0.019)
Any underwriting	0.008	0.028***	0.109***	0.082***
	(0.016)	(0.010)	(0.010)	(0.013)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
N	91,686	93,181	92,672	92,895

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

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