

Patents as Substitutes for 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 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. We also consider the reverse link of the substitution relationship, and show that banks' private-information acquisition allows firms to disclose less innovation without affecting its investment.

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1 Introduction

The value of relationships in economic partnerships and other transactions is inevitably linked to the degree of information asymmetry between the contracting parties. Relationships are formed on the premise of facilitating (repeated) transactions, based on the level of information asymmetry and other factors such as agency concerns. In turn, the very nature of these arrangements changes the contracting parties' incentives for disclosure and for acquiring information about one another. The altered level of information asymmetry feeds back to the value of relationships and, thus, affects their duration.

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, our study focuses 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 patents that aim 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

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."

In this paper, we argue that innovating firms face an interplay between the patenting-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 two empirical settings which relate the value of private information in banking relationships to the cost of innovation disclosure through patents. 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. In order to analyze how public-information provision through patents and private information acquired in relationships are substitutes, our two settings separately vary each one of these disclosure channels to determine the effect on the other.

First, we exploit the American Inventor's Protection Act of 1999 (AIPA) as a source of variation in the disclosure of patent applications to estimate the effect of innovation disclosure on the stability of lending relationships. As the value of such relationships should be linked to the level of private information (about innovation or any other activities) between borrowers and lenders, 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. Second, we establish that private-information acquisition in relationships and public-information production through patents are indeed substitutes. To do this, we explore the reverse effect of varying the depth of relationships with informed lenders on firms' patenting behavior. We use scope-expanding bank mergers as a source of variation in lender informedness, and estimate its impact on firms' decision to disclose their innovation through patents.

Turning to our first setting, AIPA forced firms to disclose the content of their patent

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

applications within 18 months after the filing date (see Johnson and Popp (2003), Graham and Hegde (2015), and Hegde and Luo (2016) for a detailed description of the event). Prior to this legal change, 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 without losing intellectual property protection. In contrast, AIPA imposed a maximum delay of 18 months, 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 the longest lag between application filing and patent disclosure before AIPA were most 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 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 *involuntary* innovation disclosure and private information acquired in banking relationships are substitutes.

To show that this insight is generalizable to *voluntary* innovation disclosure, we can make

use of the patenting-secrecy trade-off, which suggests that innovating firms choose to disclose innovation-related information by patenting. This enables us to test the reverse relationship between corporate disclosure and banking relationships. 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.

We rely on our second empirical setting to estimate the impact of increased lender informedness on firms' patenting behavior. Banks have incentives to produce information about firms, which, in turn, affects firms' own disclosure decisions. Such incentives and ability of banks to produce information are also related to the type of relationship they have with firms. For instance, banks of wide scope, such as universal banks, have more means of generating 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.

To the best of our knowledge, this paper constitutes the first comprehensive empirical analysis of the interplay between banks' potential for information acquisition and corporate-innovation disclosure. In doing so, our paper relates to the literature on how banks produce 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)).

The empirical literature has focused on relationship lending as one way how banks' information about a firm and its reusability interact with firms' financing decisions (see Houston and James (1996) for evidence on public firms, or Boot (2000) for a more extensive summary). Our paper suggests that banks' information acquisition affects the public information that firms release to markets, and in reverse, when such public information is made more available, firms can more easily break up existing relationships.

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), and Gopalan, Udell, and Yerramilli (2011).³ To the extent that the feasibility of switching lenders depends on the latter's joint reaction to new information about borrowers, our paper is also related to Hertzberg, Liberti, and Paravisini (2011).

Furthermore, we contribute to the literature on how the development of the financial sector affects 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 These papers have largely focused on how banking development affected firm-level innovation strategies as captured by new patent grants. Some of them come to seemingly conflicting conclusions. For instance, while Amore, Schneider, and Žaldokas (2013) find a positive effect of interstate banking on patent counts,

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.

⁴ See also Kerr and Nanda (2014) 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.

Cornaggia, Mao, Tian, and Wolfe (2015) find that the subsequent interstate-branching deregulation led to fewer patents. Under both deregulations, credit-supply effects are typically assumed to relax financial constraints for firms seeking to finance innovation. This may, however, not be captured by patent counts if patents serve additional purposes that could interact with the nature of the banking deregulation under consideration.

Some recent papers have highlighted that patents might have an additional role on top of recording firm-level innovation. For instance, Mann (2016) argues that patents can act as collateral for loans. Chava, Nanda, and Xiao (2015) show that certain facets of patents – increased patent protection and creditor rights over collateral – result in cheaper loans. We uncover a supplemental, reverse channel through which credit affects patenting, 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)).

This opens up the possibility that in debt contracting patents serve multiple purposes, besides intellectual property protection. While the level of asymmetric information is usually assumed to be low between banks and publicly listed firms, such as those under scrutiny in this paper, innovation is among the most uncertain types of investments undertaken by these firms. We show that there is value attached to information acquisition about public firms in lending relationships, which speaks to the monitoring channel of bank finance. We thus complement the literature on the monitoring role of (typically equity) financing for early-stage firms (see, for instance, Bernstein, Giroud, and Townsend (2016)). As far as the main idea of our paper is concerned, our findings should be even stronger for firms with greater information asymmetry – such as early-stage firms – seeking private equity capital.

Lastly, 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., Darrrough (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 (2015a) analyze firms' stock liquidity as an additional concern that might encourage firms to patent a larger stock of their knowledge. In contrast, we show how firms' relationships with their creditors govern their decision to patent existing innovation.

2 Empirical Strategy and Data

Our empirical strategy involves two settings in which we rely, first, on AIPA as a source of variation in the disclosure of patent applications and, second, on universal-bank mergers to vary lender informedness. In this manner, we show that public-information production through patents and private-information acquisition in relationships act as substitutes. In the following, we provide background information on AIPA, information acquisition in universal banking, and our associated identification strategies. Then, we will describe our data on innovation and banking relationships.

2.1 American Inventor's Protection Act of 1999

To pin down the interplay between firms' public-information production and banks' private-information acquisition, we look at how a shock to firms' innovation disclosure in the form of patent applications alters their relationships with existing lenders. Such lending relationships are characterized by costly private-information acquisition and monitoring and, as argued by Rajan (1992), can also lead to hold-up by banks. 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 a potential break-up of existing bank-firm relationships, as other banks become comparatively more competitive in financing the firm.

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 protection (while foregoing licensing income), a practice known as “submarine patenting.” On the other hand, they could still signal this information privately to financiers. AIPA became effective on November 29, 2000,⁵ and harmonized U.S. patent laws with other developed economies by requiring public disclosure of patent applications 18 months after the filing day, even if the patent is not granted eventually.⁶

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 (2016), 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.

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 ap-

⁵ See Section A in the Appendix for a summary of Ergenzinger (2006), who provides a legislative history of the contentious and uncertain process of the AIPA passage.

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.

plications and grants (when their content was made public) for each firm’s SIC2 industry⁷ over five years during the pre-AIPA period from 1996 to 2000.⁸ We argue that industries with longer historical delays from filing to grant were affected more heavily by the passage of AIPA, which imposed a maximum delay time of 18 months. The longer the delay in the industry, the more likely patent applications were kept secret for a longer period of time. Such delays may have even 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 displayed 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.

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 run regressions at the SIC2-industry level, reporting the estimates for cross-sectional regressions 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

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. 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 $F = 5.46$ ($p = 0.023$).

could affect both its strategic decision to patent innovation as well as its banking relationships (see Manova (2013) and the survey by 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 as well as 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 the fourth column, we use the financial dependence of industries, measured as the median value of financing needs across firms in a given SIC2 industry, as in 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 association with our delay measure.

Finally, we consider stock-market run-ups before 2000. As the passage of AIPA took place around the same time as 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

Variation in firm-level innovation disclosure. We use this industry-level delay measure to capture variation in the treatment intensity under AIPA. In particular, firms operating in industries associated with longer delays from filing to grant were affected more heavily by AIPA insofar as their innovations became public information relatively more quickly compared to the pre-AIPA regime.

To estimate the effect of AIPA on the stability of lending relationships, we build a panel of all bank-firm pairs (ij) 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).¹⁰ 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 thereof), or an indicator for non-zero loan volume, reflecting the extensive margin.

This setup allows us to include not just bank-firm fixed effects, 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 run the

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. However, note that 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 to bias our estimates, such events should have a similar ranking of industry-level exposure.

¹⁰ In the Appendix, we also provide robustness checks for a three-year window around AIPA.

following specification:

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

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.

In the presence of 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 either entirely breaks up an existing relationship or just reduces its exposure to the respective lender, while completely switching to or adding another lender. β_2 can be interpreted as a baseline propensity to break up an existing relationship. It is natural that over the course of ten years (our sample period runs 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 especially affected by AIPA.

In the above specification, 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 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.

One might be interested whether we are able to disentangle 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, e.g., by increasing the number of sources of loans. If this is the case, then partial rather than complete switching may still lead to negative estimates of β_1 and β_2 when using an indicator for any non-zero loans as dependent variable. To rule out that this governs our estimates of β_1 , we check whether firms in treated industries increased the number of their banking relationships after AIPA. This is not the case: 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.¹¹

From an identification point of view, AIPA offers plausibly exogenous variation in innovation disclosure that enables us to identify the effect on banking relationships. Our research

¹¹ This zero correlation is robust to testing it in a regression framework, even after including SIC1-level industry fixed effects (note that our treatment-intensity measure is defined at the SIC2 level) and additional firm-level observables. These results are available upon request.

design tests the impact of innovation disclosure, namely the publicity of patent applications, on firms switching lenders.

Note that our estimated effect does not rely on the amount of private information that incumbent banks had about firms' innovative activities before AIPA. However, now that more information is revealed publicly, *any* private information, on firms' innovative or any 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.

With regard to the interpretation of such estimates, one faces the open question as to *why* AIPA constitutes an important forced change in the disclosure of innovation-related information by patentees. First, AIPA could have led to greater disclosure of innovation because firms did not want to disclose more in the pre-AIPA period, so AIPA induced firms to reshuffle their banking relationships accordingly. Alternatively, firms might have faced significant constraints in disclosing their patents and innovation activity and, thus, AIPA relaxed such constraints.

Both channels would be consistent with the Rajan (1992) hypothesis that banks' private information enables them to hold up their borrowers, for which we provide evidence by showing that increased innovation disclosure under AIPA leads to break-ups of banking relationships and switching, accompanied by a drop in treated firms' cost of debt. The former channel is distinct from the latter, in that it implies that a trade-off between patenting and secrecy is important in banking relationships. Under said trade-off, firms would not deem it optimal to disclose as much as they need to after AIPA due to the product-market implications of the disclosure of patent applications.

Following Bhattacharya and Ritter (1983), this gives rise to the possibility that some innovating firms might prefer financing arrangements that do not require them to use patents as a costly signal for their innovation, allowing them to finance innovation without disclosing it publicly. A natural way to facilitate this would be through private-information acquisition

in banking relationships, in contrast to public-information production through patents and AIPA. In the next section, we design and discuss an empirical test to shed light on the potential impact of deeper banking relationships through lender informedness on firms' patenting behavior.

2.2 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 Neuhaun and Saidi (2016), we identify the impact of lender informedness off 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:

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

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 industry-year fixed effects, and μ_i denotes firm fixed effects. Standard errors are clustered at the firm-year level.

We use a time window of eleven years so as to accommodate the triplet of events (loan transaction, underwriting, and any mergers).¹² 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 treatment 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 universal bank *and* an underwriting product (debt or equity) from a separate investment bank changed its behavior after the two respective banks have merged. 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%) returned to the merged universal bank for another loan (underwriting product) within five years after the merger, and in the control group, 52.1% (59.8%) returned to *any one of the two universal banks* involved in mergers for another loan (underwriting product). These ex-post probabilities are high, and remarkably similar.

The main identifying assumption is that treatment and control firms differ only in the degree of informedness of the respective universal banks when dealing with the same firm in the future. The underlying rationale for this assumption is that individual firms are small enough in order to not be decisive for the respective banks' decision to merge with

¹² 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.

each other. Furthermore, in Figure 1, we provide evidence of parallel pre-trends in terms of patenting behavior and R&D expenditures among our treatment and control groups in the period leading up to the merger(s) of commercial/universal and investment banks.

2.3 Data Description

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 Compustat data for the period from 1987 to 2006, following the procedures laid out in Hall, Jaffe, and Trajtenberg (2001) and Bessen (2009).

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.¹³ 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 (2016). 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 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.

¹³ 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.

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 (2016), 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 75th percentile in the sample (2.61%).¹⁴

Our loans sample comprises syndicated loans of public firms from DealScan, and we focus on the lead arranger(s) to identify the relevant lender(s). 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.

Summary statistics. In the top panel of Table 1, we present summary statistics for our AIPA-based analysis in Tables 3 to 6. In the middle panel, we present them for our universal-banking-related identification in Tables 8 to 10, and in the bottom panel for our loans sample used in Table 7. Our sample starts in 1987 and ends in 2010, with the exception of the NBER patent data, which are available only until 2006.

¹⁴ 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."

Note that in the top panel, we record two observations per bank-firm pair. We have 9,333 such pairs.¹⁵ 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).

3 Results

In this section, we turn to the estimation results of our two empirical settings. First, we use the American Inventor’s Protection Act of 1999 (AIPA) as an exogenous variation in the disclosure of patent applications. By estimating the effect of AIPA on the stability of lending relationships, we investigate whether the subsequently (more) public information about firms’ innovation led to a decrease in the value of formerly private information between banks and firms. Second, we explore the reverse relationship, and use bank mergers as a source of variation in lender informedness. We show that banks’ private-information acquisition weakens firms’ incentives to use patents as a signal for the quality of their innovation. Conversely, we find no such negative effects on other measures of innovation activities and related expenditures.

3.1 Effect of Innovation Disclosure on the Stability of Lending Relationships

We first analyze the effect of AIPA and higher innovation disclosure on firms’ lending relationships. Then, we discuss the heterogeneity of the treatment, and present further supporting evidence, most notably the impact on treated firms’ cost of debt.

¹⁵ The sample drops to 8,348 pairs when we add firm-level patent measures from the NBER patent dataset.

3.1.1 Main Results and Robustness

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 2.1, 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. After controlling for bank-firm, bank-period, and firm-period fixed effects, our estimated treatment effect is the triple interaction between our delay measure, an indicator for whether firm i already transacted with bank j in the pre-AIPA period, and an indicator for the post-AIPA period.

In Table 3, we run specification (1), where we use as dependent variable the log of the 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 treated firms.

Note that we include bank-firm pairs with non-zero loans in the pre-AIPA period, the post-AIPA period, or both, while controlling for firm-period fixed effects. In this way, the triple interaction $Treatment_i \times Initial\ relationship_{ij} \times Post_t$ allows us to estimate a negative treatment effect for firms with pre-existing relationships, which in this setting is equivalent to firms switching lenders, rather than firms reducing their demand for loans. This is because any shocks to firm-level demand for loans *across all banking relationships* would be captured by firm-period fixed effects.

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 second column of Table 3, where the treatment effect is even stronger (i.e., the coefficient is more negative) for firms that patented

in the pre-AIPA period.

We perform a number of 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-AIPA period when the period is altered. As can be seen in the third column of Table 3, 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 reflects 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. However, 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 at least one loan in either the pre- or the post-AIPA period. 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 fourth column of Table 3, 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. Therefore, 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-run the specifications from Table 3 with the latter dependent variable, and report the results in Table 4. 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 4 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. In the second column, this effect is magnified for patenting firms to $(0.344 - 0.009) \times 0.223 = 7.5\%$ more break-ups per one-standard-deviation increase. The economic significance of these estimates is given by their comparison to the baseline proportion of recurring relationships of 17.6% and new relationships of 83.4% in Table 1.¹⁶

We continue with robustness checks in Tables B.1 and B.3. Our results are generally robust, but statistical significance is at times reduced. First, 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. Second, we vary the length of the time window around AIPA from five (as in our baseline regressions in Table 3) 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. We do this to preclude that our treatment effects are contaminated with any strategic delays on the firm side. 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.

We provide further robustness tests in Tables B.2 and B.4. In particular, we tackle

¹⁶ As argued in Section 2.1, given that we do not find that firms in treated industries increased the number of their lenders, the negative treatment effect for the extensive margin of lending relationships indicates that a non-negligible portion of the treated firms switched completely from one set of banks to another.

the fact that AIPA was enacted in November 1999, but affected new patent applications starting only in November 2000. In the first column, we show that our finding is 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.

Related to the concern that our treatment may have coincided with firms associated with the dot-com bubble (see last column of Table 2), we show in the second column of Tables B.2 and B.4 that our treatment effect is, if anything, stronger after dropping high-tech companies from our sample.

In the third column, we limit the pre- and post-AIPA sample to banks with experience lending to innovative firms in the pre-AIPA period. As in Chava, Nanda, and Xiao (2015), we thereby focus on banks that are most likely to use innovation-related information from patent applications. In line with this argument, we yield a considerably stronger treatment effect for this set of banks.

Finally, in the fourth column of Tables B.2 and B.4, 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 3 and 4. 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.

A general concern with our AIPA identification may be that it is a fuzzy design, as suggested by de Chaisemartin and D'Haultfœuille (2016). That is, all firms are treated to some extent, and they differ only by their experiencing a higher increase in the treatment rate. At the industry level, the minimum average delay from filing to grant in the pre-AIPA period, which we use as treatment intensity, is greater than 18 months (see Table 1). However, at the firm level, the minimum in our sample is as low as 229 days. We have

already exploited this feature when using firm-level delays as our treatment variable in the third column of Tables B.1 and B.3.

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 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.5 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 uniformly 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 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.

Regarding the economic mechanism underlying the AIPA-induced treatment effect, 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 within 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. However, we take this concern seriously, and check whether the number of licensing alliances has increased in industries more heavily affected by AIPA. We do not find that this is the case.¹⁷

¹⁷ These results are available upon request. Also, our results in Tables 3 and 4 hold up to excluding firms involved in licensing alliances anytime during our sample period.

3.1.2 Heterogeneity of the Treatment

We further study the heterogeneity of our treatment effect. Firms that patented in the pre-AIPA period should be – and, as we have just shown, have in fact been – affected more heavily by AIPA. We first investigate whether firms with particularly valuable patents, as measured by the average number of forward citations per patent across all patents issued by a given firm in the pre-AIPA period, were more likely to switch lenders. In the first column of Tables 5 and 6, we show that the treatment effect on both the intensive and extensive margin of lending relationships is indeed driven by particularly valuable patents.

We add interactions with the total number of patents issued in the pre-AIPA period, and find that the negative treatment effect for highly cited patents is not explained by firms that patented more. Insofar as highly cited patents may 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 could be interpreted as evidence that firms in treated industries were particularly likely to switch lenders when AIPA led to the disclosure of highly valuable innovation-related information.

In the last two columns of Tables 5 and 6, we exploit variation in firms' patenting activity as derived from the ease with which firms could protect their trade secrets. To this end, we consider whether the 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 (2015) for more information). Due to the protection of trade secrets under IDD, firms should have opted for keeping innovation secret and patenting less even before AIPA came into effect.

Given that following the adoption of IDD, firms would have found trade secrets more

appealing than patenting, we hypothesize that the treatment effect of AIPA on firms facing strong protection of trade secrets should be weaker. 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 have recognized IDD. We then compare whether the treatment effect of AIPA on lending relationships was weaker for firms that could more readily exploit trade secrets, i.e., for firms in states that have restricted key employee mobility through IDD.

Indeed, the coefficient on $Treatment_i \times Initial\ relationship_{ij} \times IDD_i \times Post_t$ is positive and significant in the second column of Tables 5 and 6. 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.¹⁸

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 consequence, we should yield a weaker interaction effect compared to the second column. This is indeed the case, as can be seen in the last column of Tables 5 and 6, where the respective (positive) interaction effect is not just smaller in size, but also somewhat less significant.

3.1.3 Cost of Debt and Other Supporting Evidence

We further scrutinize whether firms in treated industries also profited from lower cost of debt, in line with Ioannidou and Ongena (2010). In Table 7, we use loan-level data and perform 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 off firms that received loans in both pre- and post-AIPA periods.

¹⁸ Note that the sample size drops, as we perform this analysis only for firms that did not change their headquarters during our sample period.

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

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, the fact that switching firms were able to reduce their cost of debt suggests that they voluntarily switched lenders for economic reasons. 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. Instead, lower cost of debt suggests that the aggregate disclosure produced by firms has in fact increased.

Another alternative interpretation could be 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 increase in 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.

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 matched by the incumbent bank. For instance, larger firms might find it more valuable to contract with larger, rather than smaller, banks. If the firm has matched with the incumbent bank when it was smaller, but could not switch out of this relationship due to hold-up, AIPA could have made recontracting and matching to a new, larger bank easier. Such assortative matching can explain why firms prefer to switch even if incumbent banks can also lower the cost of debt.

In Table B.6, we re-run the specification from the third column of Table 7 for three more loan-contractual outcomes. We find no effect on the use of financial covenants (second column) or the degree to which loans are secured (third 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.¹⁹ Conversely, switching firms receive loans with significantly longer maturities than do firms that keep their previous relationship (in fact, the sum of the coefficients on $Treatment_i \times Post_t$ and $Treatment_i \times Initial\ relationship_{ij} \times Post_t$ is almost precisely zero). 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 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)).

¹⁹ 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 innovation 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.

To shed light on this, we construct measures of public issuances of equity and debt from the SDC database, where for each firm we record the total principal amount of equity and debt raised over a year, compared to loan financing. Our results are reported in Table B.7, 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) rather than equity issues (third column).

Taken together with our findings in Tables 7 and B.6, 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 increased innovation disclosure under AIPA has helped firms to reduce the degree of hold-up in existing financing relationships.

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

Our evidence is consistent with innovating firms facing a trade-off between patenting and secrecy. One of the costs of patenting, present in this trade-off, is innovation disclosure. We show that such public disclosure has an impact on banking relationships, as it increases firms' ability to switch to banks whose informational disadvantage compared to the incumbent lender is subsequently reduced.

Hence, public-information provision by AIPA substitutes for private information acquired in lending relationships. To provide further support for this substitution relationship, we next show that the reverse holds true as well, namely that private-information acquisition in lending relationships acts as a substitute for public innovation disclosure through patents.

Against this background, we scrutinize whether deeper banking relationships induce firms to patent less. In particular, our measure for deeper banking relationships is increased lender informedness. We use mergers between commercial/universal and investment banks as a source of variation in lender informedness to show 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 their investment in innovation is not reduced.

We estimate (2), and present the results for firms' patenting behavior in Table 8. 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, treatment 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.

In the first column, we find that firms issued 23.6% fewer patents. In the next two columns, we investigate whether treated firms stopped 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 issued *any* patents. Instead, in the third column, we find a significantly negative treatment effect on the number of patents after conditioning on years in which firms issued non-zero patents, implying that treated firms did not stop patenting.²⁰

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 did not patent less because they became less innovative, we consider various measures that reflect inputs and outputs of the innovation process. In Table 9, we find no effect on R&D or the sum of R&D and SG&A expenditures. We find a positive

²⁰ In untabulated tests, all patent-related results are robust to excluding all observations after the year 2003, as motivated by Dass, Nanda, and Xiao (2015b), so as to avoid any truncation bias present in the NBER patent dataset.

effect on one of our measures of the number of new-product announcements.²¹

Furthermore, in the first column of Table 10, we even find a *positive* effect on capital expenditure. In the remaining three columns, we test whether the negative treatment effect on patents may hold generally for any class of collateralizable assets, because patents may be used as collateral in loan agreements (Mann (2016)). However, we find only positive, and no negative, treatment effects on assets as well as property, plant, and equipment.

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. What is more, all insights hold up to including state-year effects (leading to a drop in the sample size due to data availability), which capture any confounding effects of, e.g., IDD. The respective results with state-year fixed effects are in Tables B.8, B.9, and B.10.

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 untabulated tests, we find no changes in charged loan spreads associated with loans of treated firms after the respective universal-bank mergers.

4 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

²¹ A positive effect on new-product announcements is consistent with our results on patents. If there is less information about a firm's innovation activities in general, any announcement about new-product development should be greeted with a larger stock-price reaction. In untabulated tests, our findings are robust to using the sum of all positive cumulative abnormal returns around product announcements, which captures the total incremental value of all new-product introductions by a firm during the year.

about otherwise hard-to-observe innovation, they carry a significant cost as increased 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 this manner, we show that public-information production through patents and private-information acquisition in relationships are substitutes.

Our results suggest that switching costs in banking relationships might be endogenous to product-market-induced 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.

Building on our finding that innovating firms rely more on secrecy, rather than patenting, due to informed lending, another fruitful avenue for future research would be to shed light on the wider economic effects of the interaction between banking deregulation and innovation secrecy. For instance, innovation secrecy potentially constitutes an impediment to technological spillovers and ultimately economic growth. One implication of our results may, thus, be that economies with strong reliance on banking relationships may follow different growth paths than those with more developed public capital markets.

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5 Figures

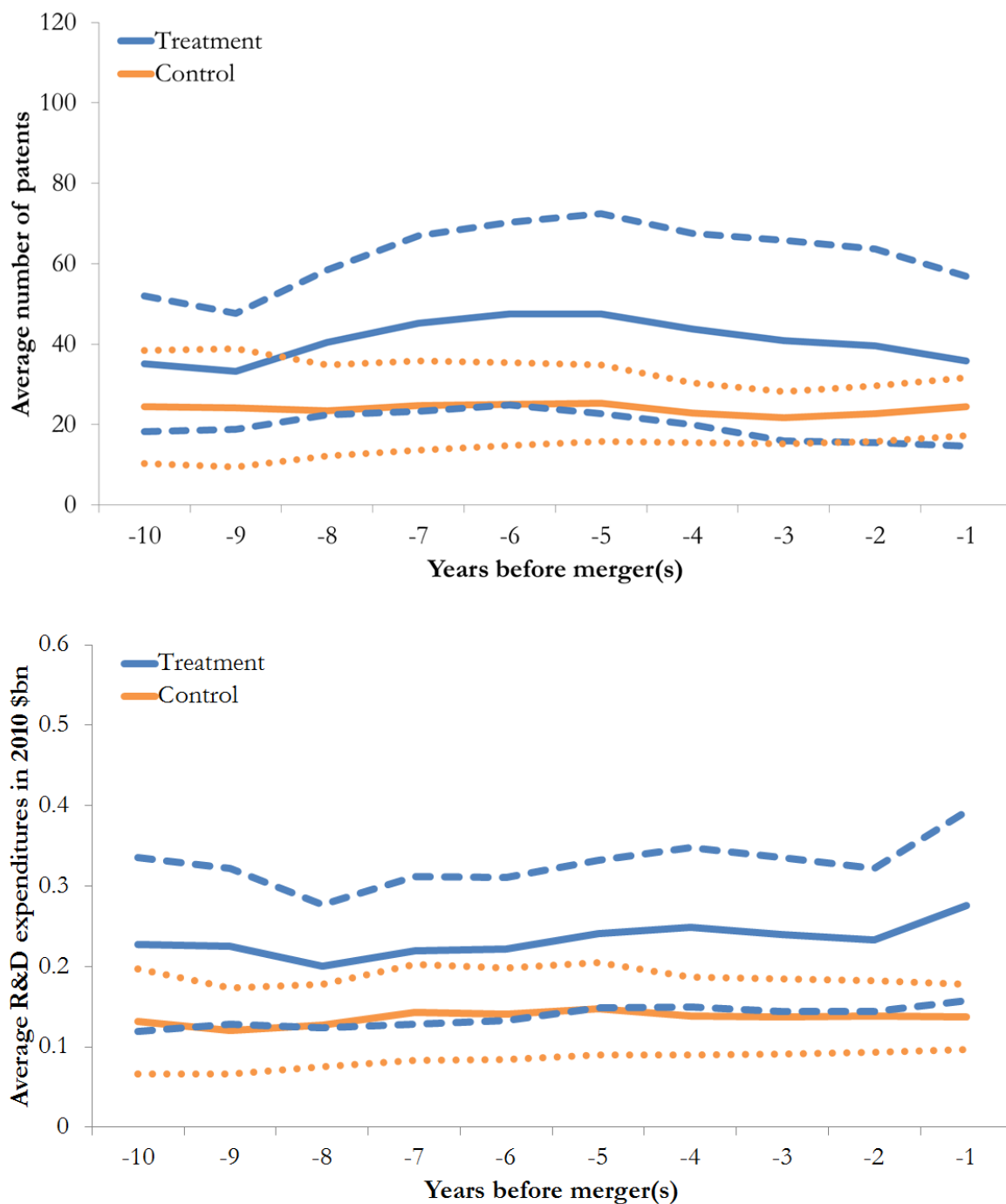


Figure 1: **Pre-trends among Treatment and Control Firms Contracting with Universal Banks.** The graphs in the top panel and the bottom panel plot, respectively, the average number of patents and the average research and development (R&D) expenditures by firms in the treatment and the control group over 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.

6 Tables

Table 1: **Summary Statistics**

<i>AIPA sample (bank-firm-period level, 1996 – 2005 summarized as two periods)</i>					
	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
<i>Compustat sample (firm-year level, 1987 – 2010)</i>					
	Mean	Std. dev.	Min	Max	N
Number of patents	12.136	92.279	0	4,344	61,242
Total cites of patents	91.860	806.595	0	45,559	61,242
Patenting $\in \{0, 1\}$	0.369	0.483	0	1	61,242
R&D expenditures in 2010 \$bn	0.086	0.495	0.000	14.434	76,819
R&D + SG&A expenditures in 2010 \$bn	0.574	2.564	0.000	79.347	69,814
New-product announcements	0.226	1.804	0	110	111,673
New-product announcements*	0.113	0.905	0	56	111,673
Capital expenditure in 2010 \$bn	0.188	1.116	0.000	45.078	109,604
Book value of assets in 2010 \$bn	2.861	16.162	0.000	866.122	111,669
Gross PP&E in 2010 \$bn	1.870	10.975	0.000	458.276	110,996
Net PP&E in 2010 \$bn	0.997	5.398	0.000	218.567	111,296
Loan from CB, underwriting from IB, both merged with each other	0.030	0.170	0	1	111,673
Loan from CB that merged with IB	0.271	0.444	0	1	111,673
Underwriting from IB that merged with CB	0.189	0.392	0	1	111,673
<i>Loans sample (1987 – 2010)</i>					
	Mean	Std. dev.	Min	Max	N
All-in-drawn spread in bps	186.888	137.695	0.700	1,490.020	16,858
Maturity in years	3.476	2.071	0.083	30.167	17,566
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
Deal size/assets	0.275	0.476	0.000	39.604	18,922
Refinancing $\in \{0, 1\}$	0.502	0.500	0	1	18,922

Notes: The variables in the top panel correspond to the respective descriptions in Tables 3 to 6, those in the middle panel correspond to their descriptions in Tables 8 to 10, and those in the bottom panel correspond to Table 7.

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: Regressions are run 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: Impact of AIPA on Intensive Margin of Lending Relationships

Sample	ln(1+Loan volume)			Full matrix
	Loan(s) in pre- or post-period	Placebo		
Treatment \times Initial relationship \times Post	-1.887*** (0.687)	-0.058 (0.843)	-0.861 (0.651)	-1.154** (0.550)
Initial relationship \times Post	-27.767*** (1.418)	-31.601*** (1.741)	-30.045*** (1.524)	-13.435*** (1.417)
Treatment \times Initial relationship \times Patenting \times Post		-5.657*** (1.855)		
Initial relationship \times Patenting \times Post		11.962*** (3.993)		
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	8,348	8,939	2,382,380
N	18,666	16,696	17,878	4,764,760

Notes: All regressions are run at the bank-firm-period level (two observations per bank-firm pair). The sample in the first three 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 third 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 fourth 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 third column, and for the post-period from 2001 to 2005 in all remaining columns. $Patenting_i$ is an indicator variable for whether firm i issued any patents in the pre-period. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

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

Sample	Loan from bank $\in \{0, 1\}$			
	Loan(s) in pre- or post-period			Full matrix
			Placebo	
Treatment \times Initial relationship \times Post	-0.086*** (0.030)	0.009 (0.042)	-0.038 (0.034)	-0.064** (0.028)
Initial relationship \times Post	-1.458*** (0.066)	-1.669*** (0.085)	-1.591*** (0.082)	-0.707*** (0.068)
Treatment \times Initial relationship \times Patenting \times Post		-0.344*** (0.097)		
Initial relationship \times Patenting \times Post		0.760*** (0.214)		
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	8,348	8,939	2,382,380
N	18,666	16,696	17,878	4,764,760

Notes: All regressions are run at the bank-firm-period level (two observations per bank-firm pair). The sample in the first three 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 third 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 fourth 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 third column, and for the post-period from 2001 to 2005 in all remaining columns. $Patenting_i$ is an indicator variable for whether firm i issued any patents in the pre-period. 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 Intensive Margin of Lending Relationships – Quality of Pre-AIPA Patents and Inevitable Disclosure Doctrine (IDD)

Sample IDD definition	ln(1+Loan volume)		
	Loan(s)	in pre- or post-period No reversals	All
Treatment × Initial relationship × Post	-0.733 (0.820)	-1.695** (0.862)	-2.094** (0.867)
Initial relationship × Post	-30.162*** (1.704)	-28.442*** (1.764)	-27.519*** (1.756)
Treatment × Initial relationship × Avg. cites × Post	-0.607*** (0.218)		
Initial relationship × Avg. cites × Post	1.307*** (0.472)		
Treatment × Initial relationship × Patents × Post	0.007*** (0.002)		
Initial relationship × Patents × Post	-0.016*** (0.004)		
Treatment × Initial relationship × IDD × Post		3.963*** (1.390)	2.985** (1.414)
Initial relationship × IDD × Post		-7.709** (3.089)	-5.986* (3.087)
Bank-firm FE	Y	Y	Y
Bank-period FE	Y	Y	Y
Firm-period FE	Y	Y	Y
No. of bank-firm pairs	8,348	6,071	6,071
N	16,696	12,142	12,142

Notes: All regressions are run 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 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. $Avg.\ cites_i$ is the average number of forward citations per patent across all patents issued by firm i in the pre-period. $Patents_i$ is the total number of patents issued by firm i in the pre-period. 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 second 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 6: Impact of AIPA on Extensive Margin of Lending Relationships – Quality of Pre-AIPA Patents and Inevitable Disclosure Doctrine (IDD)

Sample IDD definition	Loan from bank $\in \{0, 1\}$		
	Loan(s) in pre- or post-period		
	No reversals		All
Treatment \times Initial relationship \times Post	-0.020 (0.039)	-0.090** (0.041)	-0.117*** (0.042)
Initial relationship \times Post	-1.602*** (0.081)	-1.458*** (0.089)	-1.395*** (0.097)
Treatment \times Initial relationship \times Avg. cites \times Post	-0.044*** (0.012)		
Initial relationship \times Avg. cites \times Post	0.095*** (0.025)		
Treatment \times Initial relationship \times Patents \times Post	0.000 (0.000)		
Initial relationship \times Patents \times Post	-0.000 (0.000)		
Treatment \times Initial relationship \times IDD \times Post		0.178** (0.071)	0.151** (0.073)
Initial relationship \times IDD \times Post		-0.346** (0.157)	-0.310* (0.159)
Bank-firm FE	Y	Y	Y
Bank-period FE	Y	Y	Y
Firm-period FE	Y	Y	Y
No. of bank-firm pairs	8,348	6,071	6,071
N	16,696	12,142	12,142

Notes: All regressions are run 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 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. $Avg.\ cites_i$ is the average number of forward citations per patent across all patents issued by firm i in the pre-period. $Patents_i$ is the total number of patents issued by firm i in the pre-period. 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 second 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 Firms' Cost of Debt

	ln(Interest)		
Treatment \times Post	-0.129** (0.053)	-0.057 (0.064)	-0.566*** (0.179)
Treatment \times Patenting \times Post		-0.303** (0.122)	
Patenting \times Post		0.673** (0.282)	
Treatment \times Initial relationship \times Post			0.452** (0.200)
Initial relationship \times Post			-0.668 (0.448)
Log of deal size/assets	-0.012 (0.009)	-0.014 (0.010)	-0.011 (0.009)
Refinancing $\in \{0, 1\}$	-0.023** (0.011)	-0.023** (0.011)	-0.024** (0.011)
Controls	Y	Y	Y
Firm FE	Y	Y	Y
Bank FE	Y	Y	Y
Industry-year (SIC1) FE	Y	Y	Y
N	16,858	14,958	16,858

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 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. $Treatment_i$ 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.

Table 8: **Impact of Bank Information Acquisition on Firm-level Patenting**

Sample	$\ln(1+\text{Patents})$ All	Patenting $\in \{0, 1\}$ All	$\ln(\text{Patents})$ Patents $\neq 0$	$\ln(\text{Cites})$
Loan from CB, underwriting from IB, both merged with each other	-0.236*** (0.031)	-0.014 (0.012)	-0.108** (0.043)	-0.388*** (0.063)
Loan from CB that merged with IB \times Underwriting from IB that merged with CB	-0.035* (0.020)	-0.016 (0.010)	-0.008 (0.031)	-0.000 (0.047)
Loan from CB that merged with IB	0.036*** (0.013)	0.015* (0.008)	0.097*** (0.025)	0.102** (0.041)
Underwriting from IB that merged with CB	0.079*** (0.015)	0.019*** (0.008)	0.064*** (0.023)	0.090*** (0.035)
Any loan \times Any underwriting	0.002 (0.014)	-0.006 (0.009)	0.001 -0.031 (0.026)	(0.046)
Any loan	-0.024* (0.014)	-0.010 (0.009)	-0.061** (0.030)	-0.070 (0.052)
Any underwriting	0.002 (0.009)	0.000 (0.005)	-0.002 (0.016)	0.026 (0.027)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
N	61,242	61,242	22,593	22,593

Notes: Generally, the sample consists of all available observations from Compustat, 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 $t - 1$. 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-year level) are in parentheses.

Table 9: **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, both merged with each other	0.021 (0.013)	0.007 (0.008)	0.027*** (0.010)	0.004 (0.007)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.053*** (0.013)	-0.025*** (0.008)	0.017** (0.007)	0.012** (0.006)
Loan from CB that merged with IB	0.060*** (0.009)	0.049*** (0.006)	0.013*** (0.004)	0.007** (0.003)
Underwriting from IB that merged with CB	0.103*** (0.010)	0.059*** (0.007)	0.024*** (0.005)	0.013*** (0.004)
Any loan × Any underwriting	0.035*** (0.009)	-0.008 (0.007)	-0.003 (0.004)	-0.003 (0.004)
Any loan	-0.061*** (0.010)	-0.027*** (0.007)	-0.015*** (0.004)	-0.009*** (0.003)
Any underwriting	0.025*** (0.006)	0.066*** (0.005)	0.004 (0.003)	0.005** (0.002)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
N	76,819	69,814	111,673	111,673

Notes: The sample consists of all available observations from Compustat, 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 75th 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 $t - 1$. 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-year level) are in parentheses.

Table 10: 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, both merged with each other	0.032** (0.013)	0.040*** (0.007)	0.000 (0.008)	0.052*** (0.009)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.023 (0.016)	0.002 (0.008)	-0.030*** (0.009)	-0.038*** (0.010)
Loan from CB that merged with IB	0.073*** (0.013)	0.061*** (0.006)	0.025*** (0.007)	0.071*** (0.008)
Underwriting from IB that merged with CB	0.024* (0.013)	0.058*** (0.007)	0.062*** (0.007)	0.053*** (0.009)
Any loan × Any underwriting	0.032** (0.015)	0.047*** (0.007)	-0.035*** (0.008)	-0.029*** (0.010)
Any loan	-0.086*** (0.015)	-0.060*** (0.007)	0.062*** (0.009)	0.014 (0.010)
Any underwriting	0.009 (0.009)	0.025*** (0.005)	0.117*** (0.005)	0.089*** (0.006)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
N	109,604	111,669	110,996	111,296

Notes: The sample consists of all available observations from Compustat, 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 $t - 1$. 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-year level) are in parentheses.

Supplementary Appendix (Not for Publication)

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 legislative history of AIPA stands as an example of how a relatively straightforward patent law can snowball into a convoluted legal and political process, lead to a bill encompassing many more reforms, and incite disagreements stemming from bipartisan differences.

The problematic nature of AIPA's legislative process was first revealed in the 104th 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 Representative 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.” Mitigating this problem would necessitate the applicant to obtain a world-wide patent which would be cost-prohibitive for most independent inventors, insisted Rohrabacher. His opposition to H.R. 1733 showcased the inherent conflicting interests in the proposed reforms, resulting in a problematic legislative process for the remainder of the 104th Congress.

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. Facing opposition from Rep. Rohrabacher, H.R. 400 underwent an amendment and was passed on a unanimous vote, thus finally making it through the House. However, 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 anti-feminist 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: due to the presidential scandal, “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 Intensive Margin of Lending Relationships – Robustness I

Sample Robustness	ln(1+Loan volume)			
	At least one loan in pre- or post-period			
	Median delay	3y window	Firm delay	Examiners
Treatment \times Initial relationship \times Post	-1.807*** (0.576)	-1.933*** (0.745)	-1.553** (0.736)	-2.034*** (0.696)
Initial relationship \times Post	-28.218*** (1.117)	-28.910*** (1.548)	-28.882*** (1.793)	-28.404*** (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 run 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 B.2: **Impact of AIPA on Intensive Margin of Lending Relationships – Robustness II**

Sample	ln(1+Loan volume)			
	At least one loan in pre- or post-period			
Robustness	Delay till 1999	No tech	Experts	Survivors
Treatment \times Initial relationship \times Post	-1.416** (0.688)	-2.124*** (0.694)	-3.248*** (0.954)	-1.648** (0.703)
Initial relationship \times Post	-28.937*** (1.374)	-27.195*** (1.427)	-24.002*** (1.977)	-28.170*** (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	9,333	8,102	4,393	7,678
N	18,666	16,204	8,786	15,356

Notes: All regressions are run 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 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 1995 and 1999 in the first column, and between 1996 and 2000 in the last three columns. $Initial\ relationship_{ij}$ 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.3: **Impact of AIPA on Extensive Margin of Lending Relationships – Robustness I**

Sample	Loan from bank $\in \{0, 1\}$			
	At least one loan in pre- or post-period			
Robustness	Median delay	3y window	Firm delay	Examiners
Treatment \times Initial relationship \times Post	-0.098*** (0.028)	-0.085*** (0.030)	-0.057* (0.035)	-0.096*** (0.033)
Initial relationship \times Post	-1.446*** (0.059)	-1.515*** (0.062)	-1.507*** (0.089)	-1.482*** (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 run 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 B.4: **Impact of AIPA on Extensive Margin of Lending Relationships – Robustness II**

Sample	Loan from bank $\in \{0, 1\}$			
	At least one loan in pre- or post-period			
Robustness	Delay till 1999	No tech	Experts	Survivors
Treatment \times Initial relationship \times Post	-0.080** (0.033)	-0.082** (0.033)	-0.114*** (0.041)	-0.083*** (0.030)
Initial relationship \times Post	-1.479*** (0.070)	-1.454*** (0.071)	-1.311*** (0.104)	-1.452*** (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	9,333	8,102	4,393	7,678
N	18,666	16,204	8,786	15,356

Notes: All regressions are run 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 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 1995 and 1999 in the first column, and between 1996 and 2000 in the last three columns. $Initial\ relationship_{ij}$ 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.5: Impact of AIPA on Lending Relationships of Patenting Firms – Stable Control Group

Sample	ln(1+Loan volume)		Loan from bank $\in \{0, 1\}$	
	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*** (2.056)	-3.902*** (1.056)	-0.177** (0.085)	-0.145*** (0.041)
Initial relationship \times Post	-22.209*** (3.444)	-28.851*** (1.256)	-1.326*** (0.138)	-1.512*** (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 run 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.6: **Impact of AIPA on Firms' Loan Conditions**

	ln(Maturity)	Covenant $\in \{0, 1\}$	Secured $\in [0, 1]$
Treatment \times Post	0.534** (0.226)	0.010 (0.135)	-0.182 (0.188)
Treatment \times Initial rel. \times Post	-0.531* (0.275)	-0.012 (0.149)	0.099 (0.194)
Initial relationship \times Post	1.004* (0.594)	0.007 (0.330)	-0.120 (0.427)
Log of deal size/assets	0.183*** (0.016)	0.072*** (0.008)	0.019*** (0.005)
Refinancing $\in \{0, 1\}$	0.120*** (0.017)	0.206*** (0.026)	0.014* (0.007)
Controls	Y	Y	Y
Firm FE	Y	Y	Y
Bank FE	Y	Y	Y
Industry-year (SIC1) FE	Y	Y	Y
N	17,566	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 an indicator for whether the loan has at least one financial covenant, and in the third 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.7: **Impact of AIPA on Firms' Ability to Raise Capital in Public Markets**

	Public issues	Debt issues	Equity issues
	Public issues + Loans	Public issues + Loans	Public issues + Loans
Treatment \times Post	0.114**	0.086*	0.027
	(0.049)	(0.045)	(0.040)
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, $Public\ issues_{it}$ denotes the sum of total debt and equity financing of firm i through public capital markets (as recorded in SDC) in year t , $Loans_{it}$ is the total debt financing of firm i through syndicated loans (as recorded in DealScan) in year t , and $Debt\ issues_{it}$ and $Equity\ issues_{it}$ are equal to, respectively, total debt and total equity financing of firm i through public capital markets (as recorded in SDC) in year t . $Treatment_i$ 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. 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-year level) are in parentheses.

Table B.8: Impact of Bank Information Acquisition on Firm-level Patenting – Robustness to State-year Fixed Effects

Sample	$\ln(1+\text{Patents})$ All	Patenting $\in \{0, 1\}$ All	$\ln(\text{Patents})$ Patents $\neq 0$	$\ln(\text{Cites})$
Loan from CB, underwriting from IB, both merged with each other	-0.279*** (0.032)	-0.021* (0.012)	-0.128*** (0.044)	-0.415*** (0.066)
Loan from CB that merged with IB \times Underwriting from IB that merged with CB	-0.028 (0.022)	-0.005 (0.011)	0.016 (0.032)	0.058 (0.050)
Loan from CB that merged with IB	0.035*** (0.013)	0.010 (0.008)	0.084*** (0.025)	0.057 (0.043)
Underwriting from IB that merged with CB	0.097*** (0.017)	0.020** (0.009)	0.065*** (0.024)	0.044 (0.039)
Any loan \times Any underwriting	-0.000 (0.015)	-0.006 (0.009)	0.031 (0.028)	0.022 (0.049)
Any loan	-0.026* (0.015)	-0.012 (0.010)	-0.063** (0.031)	-0.078 (0.054)
Any underwriting	-0.012 (0.010)	-0.002 (0.006)	-0.031* (0.018)	-0.013 (0.030)
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-year level) are in parentheses.

Table B.9: Impact of Bank Information Acquisition on Firm-level Innovation – Robustness to State-year Fixed Effects

	ln(R&D)	ln(R&D+SG&A)	ln(1+Products)	ln(1+Prod.*)
Loan from CB, underwriting from IB, both merged with each other	0.024* (0.013)	0.009 (0.008)	0.017* (0.010)	-0.005 (0.007)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.052*** (0.013)	-0.017** (0.009)	0.016** (0.008)	0.012* (0.006)
Loan from CB that merged with IB	0.065*** (0.009)	0.049*** (0.006)	0.016*** (0.004)	0.009** (0.003)
Underwriting from IB that merged with CB	0.090*** (0.010)	0.046*** (0.007)	0.024*** (0.006)	0.012** (0.005)
Any loan × Any underwriting	0.026*** (0.010)	-0.004 (0.008)	-0.005 (0.005)	-0.004 (0.004)
Any loan	-0.053*** (0.010)	-0.029*** (0.008)	-0.012** (0.005)	-0.007* (0.004)
Any underwriting	0.015** (0.007)	0.060*** (0.005)	0.006* (0.004)	0.007** (0.003)
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 75th 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-year level) are in parentheses.

Table B.10: Impact of Bank Information Acquisition on Firm-level Capital Expenditure and Assets – Robustness to State-year Fixed Effects

	ln(CapEx)	ln(Assets)	ln(Gross PP&E)	ln(Net PP&E)
Loan from CB, underwriting from IB, both merged with each other	0.020 (0.014)	0.035*** (0.007)	-0.001 (0.008)	0.038*** (0.009)
Loan from CB that merged with IB × Underwriting from IB that merged with CB	-0.038** (0.017)	0.004 (0.009)	-0.040*** (0.009)	-0.045*** (0.011)
Loan from CB that merged with IB	0.053*** (0.013)	0.052*** (0.006)	0.028*** (0.007)	0.063*** (0.008)
Underwriting from IB that merged with CB	0.032** (0.015)	0.048*** (0.008)	0.072*** (0.008)	0.057*** (0.010)
Any loan × Any underwriting	0.025 (0.016)	0.037*** (0.007)	-0.033*** (0.008)	-0.029*** (0.010)
Any loan	-0.091*** (0.016)	-0.067*** (0.008)	0.050*** (0.010)	0.001 (0.012)
Any underwriting	0.008 (0.011)	0.028*** (0.006)	0.109*** (0.006)	0.082*** (0.007)
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	93,180	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-year level) are in parentheses.