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

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Abstract

Firms face a trade-off between patenting, thereby disclosing innovation, and secrecy. We show that this trade-off interacts with firms' financing choices. As 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. We find that such increased innovation disclosure helps firms switch lenders, resulting in lower cost of debt, and facilitates their access to syndicated-loan and public capital markets. Our evidence lends support to the idea that public-information provision through patents and private information in financial relationships are substitutes, and that innovation disclosure makes credit markets more contestable.

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1 Introduction

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

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

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

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

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

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

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

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

Prior to the passage of the AIPA (see Johnson and Popp (2003), Graham and Hegde (2015), and Hegde and Luo (2018) for a detailed description of the event), information about patents became public only after they were granted, over two years on average after filing. Firms could therefore delay revealing the content of their patents. In contrast, the AIPA made patent applications public 18 months after the filing date, even for patents that were not granted eventually. In the pre-AIPA era, industries differed in the time lag between patent applications and grant dates. Thus, we define the cross-sectional intensity of the AIPA's passage based on this delay. We argue that industries with longer lags between application filing and patent disclosure before the AIPA were more heavily affected by its passage. For the validity of our identification strategy, any such pre-AIPA delay measure

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

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 the 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.[3] 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.

In summary, before the AIPA was passed, firms faced a trade-off between publicly disclosing information about their pending patent applications or keeping it secret.[4] Even if some firms would have preferred the AIPA not to pass, the forced disclosure of pending patents provided the side benefit of releasing private information partly held by incumbent lenders. What we find is that when patent applications are made public, firms take the opportunity to reevaluate their lending relationships. Taken together, our results suggest that private information about patents is economically important in the lending context, and it may generate real hold-up effects.

Our paper contributes to the literature on how the development of the financial sector interacts with firms' patenting decisions (Benfratello, Schiantarelli, and Sembenelli (2008);

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

[4] Understanding the full trade-off is beyond the scope of this paper. What we claim is that due to strategic considerations, some firms are likely to value trade secrecy over disseminating pending patent information and lowering their cost of capital.

Amore, Schneider, and Žaldokas (2013); Chava, Oettl, Subramanian, and Subramanian (2013); Cornaggia, Mao, Tian, and Wolfe (2015)).[5] Patents might have an additional role on top of recording firm-level innovation: Mann (2018) analyzes patents as collateral for loans, while Chava, Nanda, and Xiao (2017) show that increased patent protection and creditor rights over collateral result in cheaper loans. This opens up the possibility that in financial contracting patents serve multiple purposes, besides intellectual property protection. In this vein, we demonstrate the importance of innovation disclosure and public-information production through patents, building on the idea that patents are a credible signal for the quality of otherwise hard-to-observe innovation (Bhattacharya and Ritter (1983); Cao and Hsu (2011); Francis, Hasan, Huang, and Sharma (2012); Hsu and Ziedonis (2013)).

By analyzing the interplay between banks' potential for information acquisition and corporate-innovation disclosure, our paper also relates to the literature on how banks acquire information about firms and, thereby, mitigate informational asymmetries. Banks learn about borrower firms through screening and monitoring activities (Diamond (1984), Ramakrishnan and Thakor (1984), Allen (1990), Winton (1995), Dass and Massa (2011)), and they are likely to learn even more if they provide multiple services to the firm (Boot (2000), Degryse and Van Cayseele (2000), Neuhann and Saidi (2018)).

As we posit that the value of private information between lenders and borrowers governs firms' ability to switch lenders, our paper connects with Rajan (1992), who argues that banks may use their private information to hold up, and extract economic rents from, firms. By testing this claim, we provide evidence on the stability and duration of banking relationships, as discussed in Ongena and Smith (2001), Ioannidou and Ongena (2010), Gopalan, Udell, and Yerramilli (2011), and Bonfim, Nogueira, and Ongena (2017).[6]

To the extent that the feasibility of switching lenders depends on the latter's joint reaction

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

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

to new information about borrowers, our paper is also related to Hertzberg, Liberti, and Paravisini (2011). They argue, and provide evidence, that a change in credit reporting and the associated higher level of public-information disclosure enable creditors to coordinate their lending decisions when firms are close to financial distress.

Our paper also relates to studies on voluntary disclosure and proprietary costs in disclosing information. In testing the hypotheses generated by a voluminous theoretical literature (e.g., Darrough (1993), Gigler (1994), Evans and Sridhar (2002), Ganglmair and Oh (2014)), empirical work faces the challenge that most of firms' public disclosure might have limited proprietary costs. We consider a case where such proprietary costs are significant, namely firms' trade-off between patenting their innovation and keeping it secret (Moser (2005), Moser (2012), and Glaeser (2018)). In a related paper, Brown and Martinsson (2017) study the impact of information environments on firms' innovative activities. Furthermore, Dass, Nanda, and Xiao (2015) analyze firms' stock liquidity as an additional concern that might encourage firms to patent a larger stock of their knowledge.

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

2 Effect of Innovation Disclosure on the Stability of Lending Relationships

We start our analysis by investigating whether more public information about firms' innovation is related to a decrease in the value of private information between banks and firms.

For this purpose, we scrutinize how a shock to firms' innovation disclosure through patent applications alters their relationships with existing lenders. We posit that an increase in publicly available information about a firm's innovation leads to potential break-ups of existing bank-firm relationships, as other banks become comparatively more competitive in financing the firm. We should thus observe more firms switching banks when more public information is available and private information becomes less valuable.

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

2.1 American Inventor's Protection Act of 1999

We use the passage of the 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." The AIPA became effective on November 29, 2000, and harmonized U.S. patent laws with other developed economies by requiring public disclosure of patent applications 18 months after the filing date, even if the patent is not granted eventually.[7] Given this requirement that the publication of the patent has to occur within 18 months after the filing, we find that the average time between patent filing and publication has decreased to 17.5 months.

The passage of the AIPA can be described as a contentious and uncertain process. In Appendix A, we provide a summary of its legislative history as given by Ergenzinger (2006). In particular, Ergenzinger (2006) includes multiple quotes that indicate that legislators and

[7] Firms could still opt for secrecy after the AIPA, but only at the cost of foregoing foreign patenting. Graham and Hegde (2015) report that very few – one-digit percentage of – firms did so.

other experts deemed the disclosures as harmful for U.S. innovators.[8] This reinforces our assumption that the AIPA imposed a level of (involuntary) disclosure of innovation-related information that firms considered suboptimal.

The effect that we identify through the 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 (2018), publication in foreign countries is not equivalent to publication in the U.S. because of the lack of public records available prior to the 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.

In addition, one could be concerned whether the AIPA constitutes a significant enough event for the public disclosure of innovation. Hegde, Lev, and Zhu (2018) discuss a few examples of how investors use the information in pre-grant disclosures of Amazon's patents to develop a better understanding of its R&D potential. Shortly after patent disclosure, the media start covering the impact of pending patents on the firm's outlook, and equity analysts promptly incorporate it in their reports.

We complement this evidence by investigating the stock-price reactions on publication dates (but before the respective grant dates) of eventually granted patents. To be consistent with the rest of the analysis, we consider only the years $1996 - 2005$ and only firms that enter our main analysis. We measure one-day returns on the days of patent publication and grant.

We report the results in Table C.1 of the Online Appendix. We find that before the AIPA, when patents were published on their grant dates, the stock price reacted positively.

[8] During the AIPA adoption process, several senior congressmen made statements that the reason why the AIPA was expected to hurt American inventors was the disclosure of information contained in patent documents. For instance, Rep. Rohrabacher was quoted saying that "patent lawyers from foreign companies would cull the USPTO files and fax published applications directly to competitors in Thailand, China, Korea, and Japan." Conservative pundit Schlafly called the bill a game plan for foreigners and multinationals to steal American technology.

However, after the AIPA we see a positive reaction both on the grant date and on the publication date. That is, once the patent information is made publicly available, the stock price increases even if the patent is not yet granted. This effect is in fact larger than the effect on the grant date, suggesting that the stock market appreciates the information about firms' innovation made public, and possibly reevaluates the probability that the patent will be granted eventually.

2.2 Identification Strategy

We describe our identification strategy in a number of steps. First, we characterize the variation across firms in their exposure to the passage of the AIPA which we exploit for identification. Second, we motivate that this variation is not correlated with other observable characteristics that could contaminate our identification. Third, we document the net effect of the AIPA on firms' innovation disclosure. Fourth, we provide some basic evidence using cross-sectional regressions. Fifth, we describe the specification that we adopt to identify the treatment effect of the AIPA and higher innovation disclosure on firms' lending relationships.

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

As our continuous treatment measure, we use the average time lag between patent applications and grants (when their content was made public) for each firm's SIC2 industry[9] over

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

five years during the pre-AIPA period from 1996 to 2000.[10] We argue that firms operating in industries with longer historical delays from filing to grant were affected more heavily by the passage of the AIPA, which imposed a delay time of 18 months.

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

In Table 2, we list all SIC2 industries, their average delays from filing to grant, and the associated number of bank-firm pairs and firms in our sample.

Correlation with other industry-level drivers of banking relationships. Importantly, this delay measure is not meaningfully correlated with cross-industry variation in access to finance or other characteristics that might influence banking relationships directly or through other channels. To show this, we report the estimates for cross-sectional regressions at the SIC2-industry level in Table 3. Our dependent variable is the mean difference in years between filing and grant dates, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. Similarly, independent variables are measured as their respective total (or, where appropriate, average) values from 1996 to 2000.

The first column reports the correlation between our SIC2-industry delay measure and international-trade characteristics of the industry, namely its import as well as export penetration. Arguably, a firm's integration into global trade and openness to foreign competition could affect both its strategic decision to patent innovation as well as its banking relation-

[10] 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 $F = 5.46$ ($p = 0.023$).

ships (see Manova (2013) and Foley and Manova (2015)). We measure import penetration as total imports over the total value of shipments plus total imports minus total exports in a given SIC2 industry, and export penetration as total exports over the total value of shipments in a given SIC2 industry. We find no relationship between our delay measure and import or export penetration.

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

Additionally, in the third column, we consider the average total factor productivity in a given SIC2 industry, using the semiparametric estimation procedure by Olley and Pakes (1996). Industries with long delays in their patent grants are neither more nor less productive, reassuring us that our measure does not capture such confounding industry characteristic.

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

Finally, we consider stock-market run-ups before 2000. As the AIPA was passed around the time of the dot-com bubble, one may worry that we capture any effects of the latter if longer delays are prevalent among technology companies. To explore this, we compute the equal-weighted average of stock returns between 1996 and 2000 for each SIC2 industry, and correlate it with our delay measure. We find no statistically or economically significant relation, suggesting that the dot-com bubble and the subsequent crash are not driving our results.[11] In addition, we will also show that our results are invariant to the exclusion of high-tech companies that played an important role in the dot-com boom and bust.

Net effect on innovation disclosure through firms' patenting activity. We next analyze the effect of the AIPA on firms' patenting activity. One could argue that while the AIPA increased the disclosure of innovation-related information in the course of patent publication, such disclosure constituted an additional cost on patenting, and might have negatively affected the incentives to patent (Aoki and Spiegel (2009)). Thus, the overall level of innovation disclosure might have decreased. Alternatively, note that the AIPA could have simultaneously led to an increase in the benefits of patenting: for example, the importance of a patent publication increased due to a reduction in search costs of identifying licensees, which facilitated better comparison and evaluation of licensors' technology.

In Panel A of Table 4, we use our sample from 1987 to 2006 to build a firm-year panel in conjunction with our treatment variable, based on industry-level delays from 1996 to 2000, to test whether the AIPA has a negative effect on firm-level patenting. In the first two columns, we find that the interaction effect $Treatment_i \times Post_t$ is negative but insignificant, suggesting that the two effects counterbalance each other.

In the last two columns, we also consider the value of patents, rather than patent counts, for which we proxy by means of the stock-market value added on the dates of the announcements of patent grants (see Kogan, Papanikolaou, Seru, and Stoffman (2017) for the construction of the measure). This is to test for the possibility that while firms do not patent less in total, they might still reduce their patenting activity for particularly valuable patents. When using as dependent variable the logged total value of all patents of firm i in year t (the dependent variable is equal to zero if a firm did not patent in a given year), we yield a positive coefficient on $Treatment_i \times Post_t$ which is, again, insignificant.

[11] 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 the AIPA. In addition, the AIPA itself also made other changes to the patent system, such as cracking down on invention promotion firms or fee reductions. However, our identification relies on the AIPA having a differential impact based on pre-AIPA industry-level delays between patent applications and grants. For any confounding events or other AIPA terms to bias our estimates, such events should have a similar ranking of industry-level exposure.

Cross-sectional evidence. We proceed to cross-sectional evidence on the relationship between the AIPA and the stability of lending relationships. In particular, we start with a panel of all firms with at least one loan within the previous five years leading up to the AIPA (pre-period from 1996 to 2000) or within the first five years after the AIPA (post-period from 2001 to 2005). That is, for each firm we record two observations that we use to define post-AIPA vs. pre-AIPA lending relationships.

We characterize lending relationships by considering any loans from a bank in a five-year window instead of constructing an annual panel, as most firms do not seek syndicated-loan financing on an annual basis but typically refinance themselves at a similar frequency as the average maturity of such loans (roughly four years). This limits our ability to consider longer-run trends. However, in order to make sure that our results are not driven by any pre-existing differential trend that could have shown up shortly before the implementation of the AIPA, we will later conduct a placebo test.

We then use our industry-level delay measure that captures variation in the treatment intensity under the AIPA, and link it to the proportion of pre-AIPA banking relationships that were preserved post AIPA. This basic cross-sectional specification provides preliminary evidence for the effect that we will identify in a refined framework where we can control for firms' loan demand, banks' credit supply, and the match quality between banks and firms.

The results are in Panels B and C of Table 4. In the first three columns of Panel B, we use as dependent variable the proportion of the total loan volume of a firm in the post-AIPA period granted by banks that it already received a loan from in the pre-AIPA period. We regress this on our treatment-intensity (delay) measure.

In the first column, the constant is positive, and indicates that at the baseline 23.9% of the firms engage in recurring loan transactions with their incumbent lenders. Conversely, the coefficient on our delay measure is negative and significant, indicating that firms in treated industries are less likely to return to their incumbent lenders. An increase in the pre-AIPA delay by one standard deviation is associated with $0.053 \times 0.223 = 1.2\%$ more break-ups (or fewer recurring relationships).

The AIPA should have affected primarily firms that patent. Given that patenting is persistent over time, we proxy for firms' propensity to patent in $2001 - 2005$ by whether they patented over $1996-2000$. In the second column of Panel B, we find that the results are stronger for patenting firms. In addition, firms for which patenting is more important should be more affected. As in Panel A, we proxy for the importance of patents for firms by the value of their recent patent portfolios, based on the stock-market value added on the dates of the announcements of patent grants (Kogan, Papanikolaou, Seru, and Stoffman (2017)). When we interact our treatment measure with the value of firms' recent patent portfolios, estimated over $1996 - 2000$, we find that treated firms with more valuable patents are more likely to break up existing lending relationships.

We visualize this baseline result by plotting the proportion of the total loan volume stemming from recurrent relationships. In Figure 1 for the subsample of patenting firms (and in Figure B.1 of the Online Appendix for all firms), we record for each year from 1996 to 2005 the proportion of the total loan volume granted by banks that firms already received loans from in the previous five-year window (from 1991 to 1995 for the pre-AIPA period, and from 1996 to 2000 for the post-AIPA period).

For both five-year periods – the pre-AIPA period starting in 1996 and the post-AIPA period starting in 2001 – we observe a negative slope, reflecting a general tendency to break up relationships over the course of time. This holds equally for both treated and control firms (firms in the top and the bottom quintile of the distribution of the pre-AIPA delay measure $Treatment_i$) during the pre-AIPA period. However, the proportion of the total loan volume from recurrent relationships drops more for treated firms following the passage of the AIPA. Treated firms source credit from existing lending relationships at an at least 11-percentage-point lower likelihood than control firms do.

In our cross-sectional regressions, we continue to yield similar results when instead of the loan volume, we use the number of maintained relationships. In the last three columns of Panel B, we replace the dependent variable by the proportion of pre-AIPA lending relationships that firms kept in the post-AIPA period.

In Panel C, we explore whether these effects are governed by the denominators of our dependent variables. That is, we examine whether firms diversify their portfolio of lenders, e.g., because of an increase in total demand that incumbent banks could not accommodate.

In the first three columns of Panel C, we consider the percent change in the total loan amount received by a firm, and find no correlation with our treatment-intensity measure (first column). However, we do find positive effects for patenting firms and firms with valuable patents in the second and third column. In the remaining columns of Panel C, we furthermore consider the change in the number of lending relationships (with different banks), and yield no correlation with the treatment measure across all three specifications.

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

While these tests provide suggestive evidence, they do not fully absorb unobservables that could be correlated with the effect we try to identify. For instance, firms in treated industries might switch lenders because banks they previously borrowed from have reduced their supply of credit. This could be the case during our sample period because certain banks might have been affected by the 2001 downturn and, thus, might have reduced their lending. If there is overall higher information asymmetry about the firms in treated industries, these banks might have first stopped lending to these firms that subsequently have to switch lenders. However, such effect would not be due to the AIPA. It is therefore important to control for bank-time fixed effects that could capture such changes in the supply of credit.

As we have seen in the second and third column of Panel C, the total loan volume did increase for patenting firms and firms with particularly valuable patents. While the latter finding suggests improved access to debt financing, which would be consistent with our conjecture and for which we will provide further evidence in our analysis, it also suggests that it is important to control for firm-level demand.

This renders it difficult to disentangle a general demand effect from actual switching in such cross-sectional regressions. Other firm characteristics that influence switching behavior might also have changed over time. If our treatment variable was correlated with changes in these characteristics, we might be incorrectly attributing switching to the AIPA while it was driven by some other change in firm or industry characteristics. It is thus important to control for firm-time fixed effects, which we are unable to include in Panels B and C of Table 4 as the identifying variation is at the firm-time level.

Finally, some bank-firm relationships may be inherently less stable than others, which warrants controlling for bank-firm fixed effects. For instance, it could well be the case that firms in certain industries, which may be more heavily treated under the AIPA, would have switched from certain banks after 2000 even absent the AIPA. One could imagine that before the dot-com crash, technology firms might have borrowed from a certain type of banks, but started borrowing from another type of banks thereafter.

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

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

This setup allows us to include not just bank-firm fixed effects that capture a particular bank-firm match, but also firm-period fixed effects to capture shifts in firm-level demand for loans *across all banking relationships*, and bank-period fixed effects to capture shifts in bank-level supply *across all firms contracting with the same bank*. Naturally, our industry-level treatment measure interacted with a post-AIPA dummy is captured by firm-period fixed effects. However, as we are interested in the development of pre-existing banking relationships, we interact our treatment measure, a post-AIPA dummy, and an indicator for whether a bank-firm pair ij already contracted in the pre-AIPA period (similar to Figure 1). This gives us variation at the bank-firm-period level, and we estimate the following specification:

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

where y_{ijt} is the natural logarithm of one plus the 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.

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

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

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

That is, if a break-up is not accompanied by the establishment of a new relationship, then β_1 and β_2 should be zero. Now assume that the same firm borrowed \$300m from another *bank B* after the 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.

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

It may still be that borrowers diversify their portfolio of lenders by increasing the number of sources of loans. If this is the case, then partial rather than complete switching may lead to negative estimates of β_1 and β_2 even when using an indicator for any non-zero loans as dependent variable.

However, as seen in the last three columns of Panel C in Table 4, firms in treated industries did not increase the number of their banking relationships after the AIPA. Furthermore, the difference in the number of relationships in the post-AIPA vs. pre-AIPA period per firm exhibits a correlation of -0.02 with our treatment-intensity (delay) measure.

2.3 Data Description

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

In Panel A of Table 1, we present summary statistics for our main analysis in Tables 5 to 9. We record two observations per bank-firm pair. We have 9,333 such pairs.[12] 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).

The summary statistics in Panel B correspond to the top panel of Table 4, which is based on the main sample of loan-financed firms in DealScan. The summary statistics in Panel C correspond to the last two panels of Table 4, based on all firms in Compustat that could be matched with the NBER patent data, irrespective of whether these firms received syndicated loans during the sample period in our main analysis. Interestingly, these firms are very similar in terms of assets, sales, and employment to those with syndicated loans in DealScan. While our sample consists of relatively large firms, this is consistent with earlier work on bank finance and corporate innovation (Mann (2018), Mao (2017)).

Finally, in Panel D of Table 1, we include summary statistics for our loan-level analysis in Tables 11 and C.6. The respective loans sample consists of syndicated loans of public

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

firms from 1987 to 2010 in DealScan.

3 Empirical Results

We now turn to our empirical results for the effect of the AIPA and higher innovation disclosure on firms' lending relationships. Then, we discuss the heterogeneity of the treatment, further robustness checks, and whether higher innovation disclosure also helps firms access syndicated-loan and public capital markets.

3.1 Main Results

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

In Table 5, we estimate specification (1), where we use as dependent variable the natural logarithm of one plus 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 firms in treated industries.

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

As can be seen in the second column of Table 5, the treatment effect is much weaker than in the first column, and not statistically significant. Conversely, the coefficient on $Initial\ relationship_{ij} \times Post_t$, which estimates the baseline break-up rate, remains negative and significant, which is a natural consequence of the fact that most borrowers have the tendency to switch lenders over the course of ten years. Yet, this effect is not related to the placebo-treatment intensity, suggesting that the treatment effect in our baseline specifications does not arise mechanically.

Furthermore, our baseline sample is limited to bank-firm pairs with non-zero loans in the pre-AIPA period, the post-AIPA period, or both. By controlling for firm-period fixed effects and, thus, any shocks to firm-level demand for loans, this allows us to identify firms that switched lenders. However, observed bank-firm pairs may be subject to a selection effect that might bias our estimates. To test for such selection in the most conservative way possible, we enrich our sample by all theoretically possible bank-firm pairs, i.e., including those with zero transactions throughout, in the third column of Table 5, where our result is robust.

In general, the coefficients for the treatment effect are large in absolute size. A potential reason for this is that the effect operates also at the extensive margin, and the logarithm is not a good approximation for the (negative) growth rate when total loan volume drops to zero in the post-AIPA period. To gauge the extent of complete rather than partial switching, we alternatively use as dependent variable an indicator for the occurrence of *any* loan transaction between firm i and bank j in period t. We re-estimate the specifications from Table 5 with the latter dependent variable, and report the results in Table 6. 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 6 indicates that an increase in the pre-AIPA delay by one standard deviation is associated with $0.086 \times 0.223 = 1.9\%$ more break-ups. The economic significance of this estimate is given by its comparison to the baseline proportion of recurring relationships of 17.6% in Table 1. Henceforth, we will focus on the intensive margin of lending relationships, and report the corresponding estimates for the extensive margin in the Online Appendix.

3.2 Heterogeneity of the Treatment

We provide further empirical support for our proposed mechanism by studying whether the impact of the AIPA differs across affected firms in predictable ways. In particular, we expect our results to be stronger for patenting firms and for firms that value secrecy more, such as those that patent highly valuable innovations. In addition, we expect our results to be stronger for firms that are more bank-dependent. Finally, our results should be weaker for firms that are located in states where they find it easier to protect trade secrets, and stronger for firms that are exposed to countries with stronger patent protection.

Patenting and bank-dependent firms. Theoretically, patenting firms should be affected more heavily by the AIPA. We use firms' patenting status in the pre-AIPA period as a proxy for whether we expect them to patent in the post-AIPA period. In the first column of Panel A in Table 7, we indeed find that the treatment effect is significantly stronger (i.e., the coefficient is more negative) for firms that patented in the pre-AIPA period.

The AIPA imposed innovation disclosure at levels that firms likely deemed to be suboptimal for product-market-related reasons. Still, firms can only be treated by the AIPA-induced higher innovation disclosure if they do not stop patenting after the AIPA. That is, the firms driving our results are those for which the general benefits of patenting exceed potential costs, including innovation disclosure, although secrecy may still be generally valuable for them. As a consequence, they opt to patent rather than to keep innovation completely secret. This is consistent with our evidence in Panel A of Table 4, namely that firms in treated industries did not reduce their patenting activity.

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

For this purpose, we interact $Treatment_i \times Initial\ relationship_{ij} \times Post_t$ with *Value of patents*$_i$, which – as before – is the stock-market value added on the dates of the announcements of patent grants (Kogan, Papanikolaou, Seru, and Stoffman (2017). This variable is measured across all patents issued by a given firm in the pre-AIPA period (or zero if a firm did not patent in the pre-AIPA period). In the second column of Panel A, the treatment effect is indeed driven by firms with particularly valuable patents in the pre-AIPA period.

Finally, we expect the treatment effect to be stronger for firms that actually depend on debt financing from banks. We proxy for firms' bank dependence by *Bank dependence*$_i$, which is the ratio between firm i's total volume of syndicated loans over total assets in the pre-AIPA period. In the third column, the respective interaction effect with $Treatment_i \times Initial\ relationship_{ij} \times Post_t$ is negative and significant. Comparing this estimated coefficient with the coefficient on the interaction effect with firms' patenting status in the first column, the treatment effect for patenting firms is equal to the treatment effect for firms with syndicated loans amounting to approximately one-third ($= -5.657/-17.452$) of their assets.

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

To this end, we consider whether a firm's headquarters were located in a state where the courts recognized the Inevitable Disclosure Doctrine (IDD). The 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 the 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 (2018)). Due to the protection of trade secrets under the IDD, firms had higher incentives to keep innovation secret and patent less even before the AIPA came into effect.

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

Indeed, the coefficient on $Treatment_i \times Initial\ relationship_{ij} \times IDD_i \times Post_t$ is positive and significant in the first column of Panel B. This suggests that firms in industries with longer delays, which should have been affected more heavily by the AIPA, were less likely to switch lenders if they operated out of states that had adopted the IDD.[13]

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

Finally, we also consider an opposite test where instead of variation in the protection of trade secrets, we exploit variation in patent protection. Since domestic patent protection in the U.S. is governed by federal law and is, thus, largely uniform in our sample period, we rely on industry-level variation in the U.S. in terms of exposure to intellectual-property-protection laws in other countries.

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

To this end, we use a cross-country index provided in Park (2008), and construct an industry-level index, IPR_i (of firm i's SIC2 industry), based on each U.S. SIC2 industry's exposure to different countries through its exports. That is, for each industry we construct its export share to a particular country over the total exports from that industry plus the value of domestic shipments.[14] We then use these weights to generate the weighted exposure of each U.S. industry to intellectual property protection around the world.

Indeed, if an industry is exporting to a country with weak protection of intellectual property, it exposes itself to copycat goods. In constructing such an exposure variable, we also include the exposure to domestic U.S. markets by weighting the U.S. index value (of 4.88) by the share of domestic shipments out of total exports from that industry plus the value of domestic shipments. That is, if an industry were not to export at all, it would be recorded as being exposed only to U.S. intellectual property protection.

In the third column of Panel B, we find a negative and statistically significant coefficient on the interaction term between IPR_i with $Treatment_i \times Initial\ relationship_{ij} \times Post_t$, suggesting that high exposure to patent protection, and thus lower reliance of firms on secrecy to guard their innovations, magnifies the treatment effect. This remains to hold true when we consider the extensive margin of lending relationships as well (see Table C.2).

3.3 Robustness

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

Further heterogeneity tests. In Table 8, we present additional robustness checks. First, one might be concerned that the effect on banking relationships could have operated in an

[14] The data are available from Peter Schott's website: http://faculty.som.yale.edu/peterschott/sub_international.htm

indirect way. For instance, higher innovation disclosure could have affected firms' licensing of patents and might therefore have had an effect on firms' cash flows, forcing them to rearrange their banking relationships.

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

However, we take this concern seriously, and – as a very conservative robustness check – exclude all firms involved in licensing alliances anytime during our sample period. We gather data on whether a firm is involved in a licensing agreement from the Thomson Reuters SDC Platinum database. As can be seen in the first column of Table 8, our results hold up to excluding these firms.[15] This suggests that our results for firms' switching lenders are unlikely to be driven by licensing alliances.

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

Third, we limit the pre- and post-AIPA sample to banks with experience in lending to innovative firms in the pre-AIPA period, i.e., we only consider banks that lent to firms that filed for patents in the pre-AIPA period. As in Chava, Nanda, and Xiao (2017), we thereby focus on banks that are more likely to use innovation-related information from patent applications. In line with this argument, in the third column, we show a considerably

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

stronger treatment effect for this set of banks.

Finally, in the fourth column of Table 8, 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 estimate is very similar to our baseline estimate in the first column of Table 5. In untabulated tests, we find that the robustness of the estimates extends to dropping all firms that were delisted for *any* reason. As bankruptcy-related reasons for observed break-ups of banking relationships are equivalent to a negative shock to firm-level demand, this further attests to the validity of our identification strategy, in that firm-period fixed effects fully capture such shocks.[16]

Treatment-intensity measure. In Table 9, we examine the robustness of the treatment effect to the definition of our delay measure $Treatment_i$. As can be seen in the first column, our results are robust to using the *median*, rather than the mean, SIC2-level delay from filing to grant as our continuous treatment variable.

In the second column, we vary the length of the time window around the AIPA from five (as in our baseline regressions in Table 5) to three years. Reducing the length of the time window around the AIPA to three years also provides suggestive evidence of the dynamic effects. After doing so, we yield a very similar but slightly larger effect. This suggests that firms react immediately after the passage of the AIPA by switching lenders.

In the third column, we limit the sample to firms that patented at least once between 1996 and 2000, and use their firm-level, rather than their respective industry-level, delays from filing to grant in the pre-AIPA period as treatment variable. The treatment effect remains robust.

In the fourth column, we use as our continuous treatment variable a delay measure that is based on the portions of delays that were more likely to be due to examiners. To construct this alternative delay measure, we download transaction histories from the Patent

[16] Another concern could be that some firms have lobbied for and against the Act. If we exclude politically connected firms, defined as in Akey (2015) based on the political contributions of firms in the 1996 and 1998 election cycles, our results continue to hold. These results are available upon request. We thank Pat Akey for sharing the data with us.

Application Information Retrieval (PAIR) database for every patent issued to a publicly listed firm between 1996 and 2000. We then exclude the time lapsed between "Mail Non-Final Rejection" and "Response after Non-Final Action" as well as the time lapsed between "Mail Notice of Allowance" and "Issue Fee Payment Received." Our estimates are virtually unaltered after using this alternative delay measure.

Finally, we tackle the fact that the AIPA was enacted in November 1999, but affected new patent applications starting only in November 2000. In untabulated results, which are available upon request, we find that the treatment effect is also robust to using a continuous treatment variable that is based on delays from 1995 to 1999, instead of 1996 to 2000, thereby excluding the year during which firms were aware of the forthcoming implementation of the AIPA while still filing patents under the old regime. All of these findings are, again, invariant to considering the extensive margin of lending relationships (see Tables C.3 and C.4).

Stable control group. A general concern with our AIPA identification may be that it is a fuzzy design, as suggested by de Chaisemartin and D'Haultfœuille (2018). That is, in our baseline specifications, all firms are assumed to be treated to some extent, and they differ only by their experiencing a higher increase in the treatment rate. This is because at the industry level, the minimum average delay across firms from filing to grant in the pre-AIPA period, which we use as treatment intensity, is greater than 18 months (see Table 1). However, when we measure the delay at the firm level, for some firms in our sample the delay is as low as 7.5 months.

We have already exploited this feature when using firm-level delays as our treatment variable in the third column of Table 9. To address the concerns raised by de Chaisemartin and D'Haultfœuille (2018), we re-run the same regressions, and limit the sample to firms with pre-AIPA delays of at most two years. In this manner, we also safeguard the similarity between treated and non-treated firms while exploiting the sharpness of the treatment only for firms with delays greater than 18 months. The results in the first and third column of Table C.5 suggest that, if anything, the treatment effect becomes stronger.

In the second and fourth column of Table C.5, we use the fact that firms with delays below 18 months should not be treated by the AIPA, and re-define $Treatment_i$ as a binary rather than a continuous variable. Namely, it is equal to 0 for all firms with average delays below 18 months, and 1 for all firms with average delays of at least 18 months (but again at most two years). Our estimates remain robust. Most strikingly, the treatment effect in the last column reflects a severe increase in the switching rate, implying that treated firms broke up 14.5% more relationships than control firms.

3.4 Access to Financial Markets

By exploring whether greater innovation disclosure has enabled firms to switch lenders, our previous analysis conditions on firms' borrowing activity before the AIPA. We now shift our focus to the composition of other firms profiting from this development. In particular, it should be the case that firms that were not previously attaining syndicated loans reap the greatest benefits from reduced information asymmetry due to innovation disclosure.

In order to investigate this, we consider the sample of all Compustat firms, irrespective of whether they appear in the syndicated-loan data (DealScan) in the pre-AIPA period only, in the post-AIPA period only, in both periods, or none. In particular, we are interested in investigating whether firms that do not appear in the syndicated-loan data in the pre-AIPA period (from 1996 to 2000) are more likely to receive a syndicated loan thereafter.

This analysis is conducted at the firm level, so we again make use of the cross-sectional specification from Panels B and C in Table 4. One caveat attached to the interpretation of these results is that the respective estimates do not control for other sources of time-varying unobserved heterogeneity at the firm level, in particular loan demand.

In the first two columns of Table 10, we test whether among firms in industries that we deem to have their information asymmetry reduced by a greater extent thanks to the AIPA, those with no prior bank credit (from the syndicated-loan market) gain facilitated access to syndicated loans. The estimates suggest that previously "unbanked" firms in treated industries are indeed more likely to attain syndicated loans after the AIPA, both in terms of total loan volume (first column) and in terms of attaining any syndicated loans in the post-AIPA period (second column).

In the third and fourth column, we further examine 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)).

To test this conjecture, similarly to the first two columns, we consider the sample of all Compustat firms, irrespective of whether they are recorded to have issued public bonds or raised public equity in the pre-AIPA period only, in the post-AIPA period only, in both periods, or none. For this purpose, we use data on public issuances of equity and debt from the Thomson Reuters SDC Platinum database, where for each firm we record the total amount of equity and debt raised before and after the AIPA.

Indeed, the estimates in the third and fourth column suggest that among firms in treated industries, those that did not raise public debt or equity anytime during the pre-AIPA period gain facilitated access to public capital markets thereafter.

Finally, in the last two columns of Table 10, we combine the two tests, and consider the union of syndicated loans, public debt, and public equity. Again, we find that the treated firms that neither appear in the syndicated-loan data nor in the SDC data in the pre-AIPA period find it easier to access external financing sources following the passage of the AIPA.

While we provide evidence that at least some of the firms in treated industries also increase issuances of public securities, in this paper we do not consider firms switching between different sources of capital. As the choice between bank and arm's length financing might involve other considerations such as bankruptcy costs, asymmetric information, and agency costs, we study one particular source of financing, namely borrowing from banks in the market for syndicated loans. Holding the type of financing constant allows us to abstract from these considerations.

4 Firms' Financing Conditions

In this section, we discuss the implications of our finding that higher innovation disclosure following the AIPA led to firms switching lenders. For this purpose, we examine whether firms whose ability to switch lenders has grown thanks to the AIPA subsequently face more favorable financing conditions.

4.1 Cost of Debt

We first scrutinize whether firms in treated industries profited from lower cost of debt. To quantify this, in Table 11, we use loan-level data, and implement the same difference-in-differences strategy as before at the firm-year level, as in Panel A of Table 4. To be consistent with our construction of the AIPA sample, we always include firm fixed effects so as to identify the treatment effect using firms that received loans in both pre- and post-AIPA periods.

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

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

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

These results are insightful in that they alleviate some of the concerns and provide a more precise interpretation of our AIPA treatment effect on switching. First, our findings link to the discussion in Rajan (1992) on interim public signals. According to Rajan (1992), loan rates charged by outside banks are lower, which is consistent with our findings. Moreover, he shows that rates for inside banks do not decrease if outside banks interpret bad signals incorrectly. One reason for inside banks' loan rates to decrease nonetheless – but less so than for outside banks – in our setting is that the AIPA is more likely to produce good, rather than bad, signals about firms' innovation process. This is because the AIPA forced the disclosure of innovation-related information that firms did not desire to release previously for reasons related to product-market competition rather than for the purpose of hiding negative news.

Second, one may argue that the AIPA has increased the cost of patenting because rival firms are able to obtain technical knowledge earlier and, thus, treated firms were forced to raise more capital to invest in shielding their innovation from replication by rival firms. Therefore, partial switching of lenders might be driven by the incumbent bank's inability to provide a larger amount of required funding. While we control for such shocks to firm-level loan demand by firm-period fixed effects, the fact that the cost of debt of switching firms has dropped suggests that our results are unlikely to be driven by additional costs of patenting implied by the AIPA.

Finally, given that incumbent banks can always reduce their share of rents from lending relationships and lower the cost of debt, one might wonder why firms switch to new lenders in the first place. One reason may be that the match quality of the new relationships produces rents that could not be offered by the incumbent bank, but the firm was previously unable to switch out of its relationship due to hold-up by its incumbent lender. This would be in line with Ioannidou and Ongena (2010), who find that firms that *voluntarily* switch from one bank to another profit from lower loan rates. In contrast, using *forced instances* of firms switching banks in large groups due to branch closings, Bonfim, Nogueira, and Ongena (2017) show that such transfers are not associated with lower loan rates. Their evidence is consistent with the idea that incumbent lenders acquire valuable private information about their borrowers, and that this informational link is lost following branch closings.

Our empirical setting combines aspects of both Ioannidou and Ongena (2010) and Bonfim, Nogueira, and Ongena (2017) as in our case increased innovation disclosure under the AIPA constitutes a forced change in borrowers' innovation-disclosure levels that reduces the informational advantage of incumbent lenders. In this regard, our results can be interpreted as indicating that higher innovation disclosure leads to a reduction in lenders' rents from informational monopolies. Firms do not only use this opportunity to switch lenders, but they also potentially escape hold-up by their incumbent lenders and are subsequently charged lower loan rates. This reduction in loan rates for switchers reflects the value of previously private information as well as the improved match quality of borrowers and lenders.

4.2 Other Features of Bank Loans

We next complete the characterization of the treatment effect on contractual outcomes, besides firms' cost of debt, at the loan level. For this purpose, in Table C.6, we re-estimate the specification from the third column of Table 11 for four more loan-contractual outcomes. We find no effect on the loan amount (second column), the use of financial covenants (third column), or the degree to which loans are secured (fourth column). In line with lower spreads charged by lenders, this suggests that lenders did not gain any bargaining power as a result of increased innovation disclosure.[17]

Conversely, as can be seen in the first column, switching firms received loans with signif-

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

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

5 External Validity

We finish our analysis by providing an assessment of how significant of a shock the AIPA is to the level of public information about innovation. Our conjecture is that the AIPA has led to an increase in the level of publicly available information about innovation because patent applications are made available more quickly, and even eventually rejected patent applications are now public.

Our empirical evidence suggests that this change in innovation disclosure enabled treated firms to switch lenders and lower their cost of debt. In order to better assess the external validity of our findings, we compare our estimates with the effect of two alternative innovation-disclosure events on the lending relationships of firms in our sample.

First, in 1995 patent bibliographical data and abstracts were made publicly available on the internet by the USPTO at no charge. Second, in 1998 the USPTO started publishing full patent texts online. As in our main analysis for the AIPA, we build pre- and post-periods for each bank-firm pair based on these two events. As these two events lack variation in the treatment intensity across industries, we use as our identifying variation at the bank-firm-year level interactions of $Initial\ relationship_{ij} \times Post_t$ with $Patenting_i$ and $Value\ of\ patents_i$ (as defined in Panel A of Table 7).

The underlying rationale is that both patenting firms and firms with particularly valuable patents should be more likely to be treated by these instances of higher innovation disclosure. We report the results in Table D.1 of the Online Appendix. In both cases, we find that patenting firms and firms with particularly valuable patents are significantly more likely to switch lenders.

Comparing the estimates in Table D.1 to those in the first two columns of Panel A in Table 7 of our AIPA-based identification, we focus on the effects for patenting firms in pre-existing relationships with banks, as captured by the coefficients on $Initial\ relationship_{ij} \times Patenting_i \times Post_t$ and $Initial\ relationship_{ij} \times Value\ of\ patents_i \times Post_t$. They are of similar magnitude across the two disclosure events in Table D.1: -0.937 and -0.679 for patenting firms, and -0.186 and -0.216 when multiplied by firms' value of patents.

One advantage of using the AIPA as a shock to innovation disclosure is the between-industry variation in the treatment intensity, aiding our identification. Evaluated at the mean value of our treatment-intensity variable $Treatment_i$ (which is 2.201), we yield very similar estimates using the AIPA: -0.489 ($= 11.962 - 5.657 \times 2.201$) for patenting firms and -0.189 ($= 1.191 - 0.627 \times 2.201$) when multiplied by firms' value of patents. The average economic magnitude is robust across all three disclosure events that took place at different dates. That is, we find that other events that increase public knowledge about innovation lead to similar outcomes in the credit market.

6 Conclusion

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

In particular, we show that when more information about corporate innovation becomes publicly available, the incumbent bank partly loses the advantage that it had in financing the firm due to its previously undertaken information acquisition. This leads to break-ups of existing bank-firm relationships, as other banks become comparatively more competitive in financing the firm, and results in lower cost of borrowing.

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

Another implication of our findings is that if patents are used to signal the quality of otherwise hard-to-observe innovation to capital providers such as banks, then their signaling value is diminishing in banks' private-information acquisition. As a consequence, informed lending might induce innovating firms to rely more on secrecy, rather than patenting, at the margin so as to avoid the cost of innovation disclosure.

A 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. Therefore, economies with strong reliance on banking relationships may follow different growth paths than those with more developed public capital markets.

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7 Figures

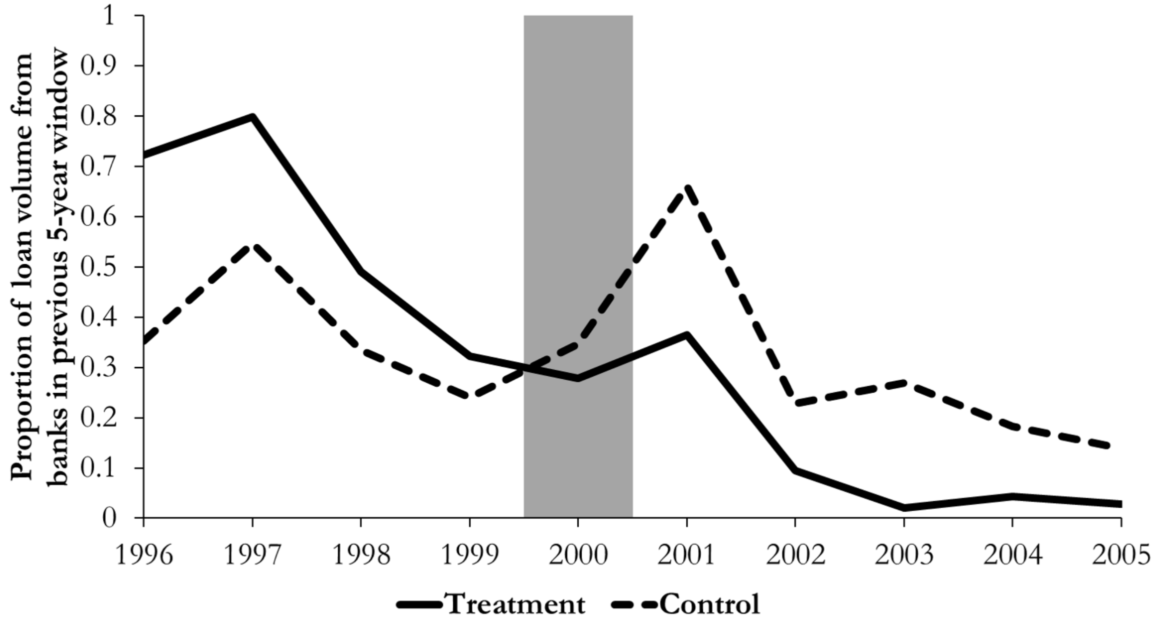

Figure 1: **Effect of AIPA on Lending Relationships of Patenting Firms.** This figure plots the proportion of the total loan volume in a given year from 1996 to 2000 (2001 to 2005) granted by banks that firms, which patented in the pre-AIPA period (from 1996 to 2000), already received loans from in the previous five-year window from 1991 to 1995 (1996 to 2000), separately for firms in the top ("treatment") and the bottom ("control") quintile of the distribution of the pre-AIPA delay measure.

8 Tables

Table 1: **Summary Statistics**

Panel A: Main sample (bank-firm-period level, 1996 – 2005 summarized as two periods)	Mean	Std. dev.	Min	Max	N
Number of bank-firm pairs					9,333
Number of firms					5,005
Number of banks					476
Loan indicator (sum over both periods)	1.101	0.301	1	2	9,333
Initial relationship in pre-AIPA period	0.573	0.495	0	1	9,333
Proportion of recurrent relationships	0.176	0.381	0	1	5,352
Patenting firm in pre-AIPA period	0.365	0.481	0	1	8,348
Total no. of patents in pre-AIPA period	53.158	444.413	0	18,632	8,348
Total value of patents in 2010 $bn in pre-AIPA period	1.845	15.075	0	424.744	8,348
Bank dependence	0.071	0.486	0	22.083	9,333
IPR index	4.083	0.367	0.000	4.817	2,666
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
Panel B: Firm sample (firm level, 1996 – 2005)	Mean	Std. dev.	Min	Max	N
Patenting firm in pre-AIPA period	0.262	0.440	0	1	4,363
Total no. of patents in pre-AIPA period	41.413	419.012	0	18,632	4,363
Total value of patents in 2010 $bn in pre-AIPA period	1.474	14.409	0	418.095	4,363
Average assets in 2010 $bn	2.309	11.816	0.003	417.173	4,191
Average sales in 2010 $bn	2.094	9.247	0.000	230.287	4,190
Average no. of employees in thousands	8.165	31.503	0	1,150.5	4,163
Panel C: Compustat sample (firm-year level, 1987 – 2006)	Mean	Std. dev.	Min	Max	N
No. of patents	12.152	92.339	0	4,344	61,160
Value of patents in 2010 $bn	0.206	2.097	0	129.180	61,160
Assets in 2010 $bn	2.513	14.595	0.000	866.122	96,356
Sales in 2010 $bn	2.166	10.572	0.000	366.362	96,360
No. of employees in thousands	8.021	33.422	0	1,800	96,360
Panel D: Loans sample (1987 – 2010)	Mean	Std. dev.	Min	Max	N
All-in-drawn spread in bps	186.888	137.695	0.700	1,490.020	16,858
Total cost of borrowing in bps	110.578	96.248	4.443	864.974	10,855
Maturity in years	3.476	2.071	0.083	30.167	17,566
Deal amount in 2010 $bn	0.446	1.159	0.000	34.282	18,922
Covenant $\in \{0,1\}$	0.470	0.499	0	1	18,922
Secured $\in [0,1]$	0.732	0.442	0.000	1	12,373

Notes: The variables in Panel A correspond to the respective descriptions in Tables 5 to 9, those in Panel B correspond to the top panel of Table 4, those in Panel C correspond to the last two panels of Table 4, and those in Panel D correspond to Tables 11 and C.6.

Table 2: **Industry-level Treatment**

SIC2 industry (code)	Mean delay from filing to grant in years (in pre-AIPA period)	No. of bank-firm pairs	No. of firms
Agricultural Production – Crops (01)	2.137	33	15
Agricultural Services (07)	2.627	6	4
Metal & Mining (10)	2.016	20	18
Coal Mining (12)	2.463	23	10
Oil & Gas Extraction (13)	2.083	422	211
Nonmetallic Minerals, Except Fuels (14)	2.242	22	11
General Building Contractors (15)	1.897	109	47
Heavy Construction, Except Building (16)	2.018	34	18
Special Trade Contractors (17)	2.768	43	22
Food & Kindred Products (20)	2.224	240	125
Tobacco Products (21)	2.360	6	4
Textile Mill Products (22)	2.119	116	52
Apparel & Other Textile Products (23)	2.237	135	62
Lumber & Wood Products (24)	2.255	42	23
Furniture & Fixtures (25)	2.004	74	39
Paper & Allied Products (26)	2.114	140	67
Printing & Publishing (27)	2.258	160	75
Chemical & Allied Products (28)	2.339	499	281
Petroleum & Coal Products (29)	2.121	73	33
Rubber & Miscellaneous Plastics Products (30)	1.957	148	78
Leather & Leather Products (31)	2.182	42	19
Stone, Clay, & Glass Products (32)	2.025	59	34
Primary Metal Industries (33)	1.997	188	86
Fabricated Metal Products (34)	2.096	193	90
Industrial Machinery & Equipment (35)	2.162	564	322
Electronic & Other Electric Equipment (36)	2.224	612	364
Transportation Equipment (37)	2.042	286	133
Instruments & Related Products (38)	2.256	445	269
Miscellaneous Manufacturing Industries (39)	2.192	132	66
Railroad Transportation (40)	2.159	28	14
Trucking & Warehousing (42)	2.086	92	55
Water Transportation (44)	2.368	42	19
Transportation by Air (45)	2.555	83	31
Transportation Services (47)	2.395	33	20
Communications (48)	2.414	436	217
Electric, Gas, & Sanitary Services (49)	2.294	113	65
Wholesale Trade – Durable Goods (50)	2.330	347	176
Wholesale Trade – Nondurable Goods (51)	2.113	193	90
Building Materials Gardening Supplies (52)	2.496	39	18
General Merchandise Stores (53)	2.778	110	50
Food Stores (54)	1.895	81	41
Automotive Dealers & Service Stations (55)	1.656	58	26
Apparel & Accessory Stores (56)	2.148	112	58
Furniture & Homefurnishings Stores (57)	2.352	90	44
Eating & Drinking Places (58)	1.953	197	108
Miscellaneous Retail (59)	1.830	286	145
Depository Institutions (60)	2.079	28	18
Nondepository Institutions (61)	2.417	43	27
Security & Commodity Brokers (62)	2.024	22	12
Insurance Carriers (63)	2.413	45	34
Insurance Agents, Brokers, & Service (64)	2.252	13	11
Real Estate (65)	2.088	25	15
Holding & Other Investment Offices (67)	2.281	82	55
Hotels & Other Lodging Places (70)	2.098	64	36
Personal Services (72)	1.902	52	25
Business Services (73)	2.356	996	598
Auto Repair, Services, & Parking (75)	1.912	41	18
Miscellaneous Repair Services (76)	2.575	8	6
Motion Pictures (78)	2.285	68	39
Amusement & Recreation Services (79)	1.734	123	64
Health Services (80)	2.290	290	145
Educational Services (82)	2.275	28	19
Engineering & Management Services (87)	2.385	190	119
Non-Classifiable Establishments (99)	2.273	9	9
Total		9,333	5,005

Notes: This table reports for each SIC2 industry 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, the total number of bank-firm relationships, and the total number of firms in our sample.

Table 3: **Correlations between Treatment and Other Industry Characteristics**

	Mean delay from filing to grant in days (1996 − 2000)				
	(1)	(2)	(3)	(4)	(5)
Export penetration	135.047				
	(124.258)				
Import penetration	-51.806				
	(68.311)				
Number of patents filed		0.000			
		(0.000)			
Industry productivity			300.103		
			(249.052)		
Financial dependency				7.930	
				(16.118)	
Industry return					21.737
					(35.930)
N	20	57	53	56	57

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

Table 4: **Effect of AIPA on Firms' Patenting Activity and Lending Relationships**

Panel A: Effect of AIPA on firms' patenting activity

	ln(1+Patents)		ln(1+Value of patents)	
Sample	Compustat firms, 1987 – 2006			
	(1)	(2)	(3)	(4)
Treatment × Post	-0.071	-0.110	0.090	0.044
	(0.199)	(0.196)	(0.163)	(0.159)
Controls	N	Y	N	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	61,160	61,160	61,160	61,160

Panel B: Cross-sectional evidence of effect on existing lending relationships

	Prop. of loan volume from previous banks			Prop. of relationships with previous banks		
Sample	All firms with loan(s) in pre- or post-period					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.053**	-0.033	-0.031	-0.056**	-0.039	-0.037
	(0.025)	(0.036)	(0.033)	(0.027)	(0.039)	(0.037)
Treatment × Patenting		-0.123*			-0.122*	
		(0.065)			(0.069)	
Patenting		0.304**			0.299*	
		(0.146)			(0.155)	
Treatment × Value of patents			-0.045***			-0.044**
			(0.016)			(0.019)
Value of patents			0.108***			0.107**
			(0.036)			(0.042)
Constant	0.239***	0.192**	0.187**	0.244***	0.205**	0.199**
	(0.056)	(0.079)	(0.073)	(0.059)	(0.086)	(0.081)
N	5,005	4,363	4,363	5,005	4,363	4,363

Panel C: Cross-sectional evidence of effect on total lending

	Δln(1+Total loan volume)			ΔNumber of banks		
Sample	All firms with loan(s) in pre- or post-period					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.587	-0.484	0.058	0.036	0.021	0.007
	(1.769)	(1.633)	(1.689)	(0.121)	(0.133)	(0.132)
Treatment × Patenting		9.323***			0.132	
		(2.927)			(0.349)	
Patenting		-19.146***			-0.143	
		(6.561)			(0.782)	
Treatment × Value of patents			1.747***			0.044
			(0.482)			(0.087)
Value of patents			-3.442***			-0.051
			(1.086)			(0.199)
Constant	-3.600	-2.450	-3.661	-0.167	-0.232	-0.206
	(3.851)	(3.641)	(3.728)	(0.264)	(0.299)	(0.294)
N	5,005	4,363	4,363	5,005	4,363	4,363

Notes: In Panel A, the sample consists of all available observations from Compustat, and the unit of observation is the firm-year level it. The dependent variable in the first two columns is the natural logarithm of one plus firm i's number of patents in year t. The dependent variable in the last two columns is the natural logarithm of one plus the total value of all patents of firm i in year t, based on market reactions to patent publications from Kogan, Papanikolaou, Seru, and Stoffman (2017). Control variables are measured in year t, and include the natural logarithm of firm i's sales and the natural logarithm of its number of employees. In Panels B and C, all regressions are estimated at the firm level, and the sample is limited to firms with at least one loan within the previous five years leading up to the AIPA (pre-period from 1996 to 2000) or within the first five years after the AIPA (post-period from 2001 to 2005). In the first three columns of Panel B, the dependent variable is the proportion of the total loan volume of firm i in the post-AIPA period granted by banks that firm i already received a loan from in the pre-AIPA period. In the last three columns of Panel B, the dependent variable is the proportion of lending relationships (with different banks) of firm i in the post-AIPA period with banks that firm i already contracted with in the pre-AIPA period. In the first three columns of Panel C, the dependent variable is the difference in the natural logarithm of one plus the total loan volume of firm i granted by all banks in the post-AIPA period compared to the pre-AIPA period. In the last three columns of Panel C, the dependent variable is the difference in the number of lending relationships (with different banks) in the post-AIPA period compared to the pre-AIPA period. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Post_t$ 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 in the pre-AIPA period. $Value\ of\ patents_i$ is the natural logarithm of one plus the total value of all patents of firm i in the pre-AIPA period, based on market reactions to patent publications from Kogan, Papanikolaou, Seru, and Stoffman (2017). Public-service firms are dropped. Robust standard errors (clustered at the two-digit SIC level) are in parentheses.

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

	ln(1+Loan volume)		
Sample	Loan(s) in pre- or post-period		Full matrix
		Placebo	
	(1)	(2)	(3)
Treatment × Initial relationship × Post	-1.887***	-0.861	-1.154**
	(0.687)	(0.651)	(0.550)
Initial relationship × Post	-27.767***	-30.045***	-13.435***
	(1.418)	(1.524)	(1.417)
Bank-firm FE	Y	Y	Y
Bank-period FE	Y	Y	Y
Firm-period FE	Y	Y	Y
No. of bank-firm pairs	9,333	8,939	2,382,380
N	18,666	17,878	4,764,760

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

	Loan from bank $\in \{0, 1\}$		
Sample	Loan(s) in pre- or post-period		Full matrix
		Placebo	
	(1)	(2)	(3)
Treatment × Initial relationship × Post	-0.086***	-0.038	-0.064**
	(0.030)	(0.034)	(0.028)
Initial relationship × Post	-1.458***	-1.591***	-0.707***
	(0.066)	(0.082)	(0.068)
Bank-firm FE	Y	Y	Y
Bank-period FE	Y	Y	Y
Firm-period FE	Y	Y	Y
No. of bank-firm pairs	9,333	8,939	2,382,380
N	18,666	17,878	4,764,760

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

Table 7: **Impact of AIPA on Intensive Margin of Lending Relationships – Variation in Treatment Intensity**

Panel A: Role of patenting and bank dependence			
	ln(1+Loan volume)		
Sample	Loan(s) in pre- or post-period		
	(1)	(2)	(3)
Treatment × Initial relationship × Post	-0.058	-0.656	-0.675
	(0.843)	(0.760)	(0.949)
Initial relationship × Post	-31.601***	-30.144***	-30.115***
	(1.741)	(1.594)	(1.952)
Treatment × Initial relationship × Patenting × Post	-5.657***		
	(1.855)		
Initial relationship × Patenting × Post	11.962***		
	(3.993)		
Treatment × Initial relationship × Value of patents × Post		-0.627**	
		(0.262)	
Initial relationship × Value of patents × Post		1.191**	
		(0.545)	
Treatment × Initial relationship × Bank dependence × Post			-17.452**
			(8.110)
Initial relationship × Bank dependence × Post			34.190**
			(17.107)
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	8,348	9,333
N	16,696	16,696	18,666

Panel B: Intellectual property protection			
	ln(1+Loan volume)		
Sample	Loan(s) in pre- or post-period		
Industries	All	All	Manufacturing
IDD definition	No reversals	All	
	(1)	(2)	(3)
Treatment × Initial relationship × Post	-1.695*	-2.094**	76.382**
	(0.862)	(0.867)	(36.699)
Initial relationship × Post	-28.442***	-27.519***	-198.277**
	(1.764)	(1.756)	(80.041)
Treatment × Initial relationship × IDD × Post	3.963***	2.985**	
	(1.390)	(1.414)	
Initial relationship × IDD × Post	-7.709**	-5.986*	
	(3.089)	(3.087)	
Treatment × Initial relationship × IPR index × Post			-19.205**
			(8.520)
Initial relationship × IPR index × Post			41.624**
			(18.565)
Bank-firm FE	Y	Y	Y
Bank-period FE	Y	Y	Y
Firm-period FE	Y	Y	Y
No. of bank-firm pairs	6,071	6,071	2,666
N	12,142	12,142	5,332

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). The sample is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to the AIPA (pre-period from 1996 to 2000) or within the first five years after the AIPA (post-period from 2001 to 2005). The sample in the first two columns of Panel B is furthermore limited to firms that did not change their headquarters. The sample in the last column of Panel B is limited to firms in the manufacturing sector (SIC codes $2000-3999$). The dependent variable is the natural logarithm of one plus the total volume of all loan transactions between firm i and bank j, separately for the pre- and post-period. $Treatment_i$ is defined at the industry level (based on two-digit SIC codes), and measures the mean difference in years between the filing date and the grant date, across all patents granted to publicly listed firms in the respective industry between 1996 and 2000. $Initial\ relationship_{ij}$ is an indicator variable for whether firm i received a loan from bank j anytime in the pre-period. $Post_t$ is a dummy variable for the post-period from 2001 to 2005. $Patenting_i$ is an indicator variable for whether firm i issued any patents in the pre-AIPA period. $Value\ of\ patents_i$ is the natural logarithm of one plus the total value of all patents of firm i in the pre-AIPA period, based on market reactions to patent publications from Kogan, Papanikolaou, Seru, and Stoffman (2017). $Bank\ dependence_i$ is the ratio between firm i's total volume of syndicated loans over total assets in the pre-AIPA period. IDD_i reflects whether firm i was exposed to the adoption of the Inevitable Disclosure Doctrine (IDD), and is defined differently across the first two columns. In the first column of Panel B, it is an indicator variable for whether firm i operated out of a state that had adopted the IDD by the first available year of the pre-AIPA period from 1996 to 2000, and did not reverse it thereafter, whereas in the second column, we also include states the courts of which eventually rejected the IDD after its adoption (namely, Florida in 2001, Michigan in 2002, and Texas in 2003). $IPR\ index_i$ is an index capturing the export-weighted exposure of firm i's industry to intellectual property protection around the world, based on 2000 data. Public-service firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

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

	ln(1+Loan volume)			
Sample	At least one loan in pre- or post-period			
Robustness	No licensers	No tech	Experts	Survivors
	(1)	(2)	(3)	(4)
Treatment × Initial relationship × Post	-1.879**	-2.124***	-3.248***	-1.648**
	(0.813)	(0.694)	(0.954)	(0.703)
Initial relationship × Post	-27.669***	-27.195***	-24.002***	-28.170***
	(1.742)	(1.427)	(1.977)	(1.494)
Bank-firm FE	Y	Y	Y	Y
Bank-period FE	Y	Y	Y	Y
Firm-period FE	Y	Y	Y	Y
No. of bank-firm pairs	7,474	8,102	4,393	7,678
N	14,948	16,204	8,786	15,356

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

Table 9: **Impact of AIPA on Intensive Margin of Lending Relationships – Robustness II**

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

Notes: All regressions are estimated at the bank-firm-period level (two observations per bank-firm pair). In the first, third, and fourth column, the sample is limited to bank-firm (ij) pairs with at least one loan within the previous five years leading up to the AIPA (pre-period from 1996 to 2000) or within the first five years after the 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 natural logarithm of one plus the total volume of all loan transactions between firm i and bank j, separately for the pre- and post-period. In the first two columns, $Treatment_i$ 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 column, and from 2001 to 2003 in the second column. Public-service firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 10: **Impact of AIPA on Firms' Access to External Financing**

	ln(1+Total loan vol.)	Any loan	ln(1+Debt and equity issues)	Any issues	ln(1+Total financing)	Any financing
Definition:						
No financing before	Syndicated loans		Public debt or equity		Syndicated loans and public debt or equity	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × No financing before	3.820*	0.188*	1.838*	0.282*	5.030**	0.271*
	(2.266)	(0.114)	(1.046)	(0.142)	(2.064)	(0.160)
Treatment	-5.069**	-0.246**	-0.537	-0.050	-4.991**	-0.077
	(2.452)	(0.120)	(0.967)	(0.109)	(2.305)	(0.084)
No financing before	-14.949***	-0.736***	-5.422**	-0.829***	-15.890***	-0.835**
	(4.995)	(0.252)	(2.219)	(0.307)	(4.535)	(0.360)
Constant	20.211***	0.997***	3.636*	0.554**	18.845***	0.711***
	(5.246)	(0.256)	(1.990)	(0.224)	(5.171)	(0.177)
N	13,927	13,927	13,927	13,927	13,927	13,927

Notes: All regressions are estimated at the firm level, and the sample includes all Compustat firms in the period from 1996 to 2005. In the first column, the dependent variable is the natural logarithm of one plus the total syndicated-loan volume attained by firm i during the post-AIPA period from 2001 to 2005. In the second column, the dependent variable is an indicator for whether firm i received any syndicated loans during the post-AIPA period from 2001 to 2005. In the third column, the dependent variable is the natural logarithm of one plus the total debt and equity financing of firm i through public capital markets (as recorded in SDC) during the post-AIPA period from 2001 to 2005. In the fourth column, the dependent variable is an indicator for whether firm i raised any debt or equity through public capital markets (as recorded in SDC) during the post-AIPA period from 2001 to 2005. In the fifth column, the dependent variable is the natural logarithm of one plus the sum of the total syndicated-loan volume attained by firm i and its total debt and equity financing through public capital markets (as recorded in SDC) during the post-AIPA period from 2001 to 2005. In the sixth column, the dependent variable is an indicator for whether firm i received any syndicated loans, or raised any debt or equity in public capital markets (as recorded in SDC) during the post-AIPA period from 2001 to 2005. $Treatment_i$ 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. In the first two columns, *No financing before*$_i$ is an indicator variable for whether firm i received no syndicated loans in the pre-AIPA period from 1996 to 2000. In the third and fourth column, *No financing before*$_i$ is an indicator variable for whether firm i raised no debt or equity in public capital markets in the pre-AIPA period from 1996 to 2000. In the last two columns, *No financing before*$_i$ is an indicator variable for whether firm i received no syndicated loans, nor raised any debt or equity in public capital markets in the pre-AIPA period from 1996 to 2000. Public-service firms are dropped. Robust standard errors (clustered at the two-digit SIC level) are in parentheses.

Table 11: **Impact of AIPA on Firms' Cost of Debt**

	ln(All-in-drawn spread)			ln(Total cost of borrowing)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	-0.127**	-0.056	-0.559***	-0.199***	-0.124*	-1.539***
	(0.053)	(0.063)	(0.177)	(0.067)	(0.074)	(0.539)
Treatment × Patenting × Post		-0.296**			-0.257*	
		(0.122)			(0.148)	
Patenting × Post		0.658**			0.540*	
		(0.282)			(0.326)	
Treatment × Initial relationship × Post			0.446**			1.355**
			(0.197)			(0.562)
Initial relationship × Post			-0.656			-2.627**
			(0.443)			(1.227)
Controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Industry-year (SIC1) FE	Y	Y	Y	Y	Y	Y
N	16,858	14,958	16,858	10,855	9,988	10,855

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

A Summary of the Legislative Process behind the Passage of the AIPA

In this section, we briefly summarize the legislative process behind the passage of the 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 the AIPA, a $390 billion omnibus spending bill implementing the biggest changes to patent law since 1952. The process of passing the AIPA turned out extremely convoluted and lasted three Congresses, inciting four years of heated debates among politicians, activists, and Nobel Prize winners, and encountering multiple roadblocks in political institutions.

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

To facilitate the passage of new reforms in the 104^{th} 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 104^{th} Congress.

The 105^{th} 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 the AIPA during the 105^{th} Congress. While H.R. 400 finally made it through the House, its companion bill S. 507 was facing strong opposition in the Senate. The bill’s progress in the Senate was further interrupted in 1997 when the opposition to S. 507 was joined by a noted conservative pundit Phyllis Schlafly. Schlafly “maintained that the bill was an ominous attack on independent inventors, calling the bill the result of a game plan by the lobbyists for ‘foreigners and multinationals’ to steal American technology.” She insisted that S. 507 had no redeeming value.

Besides Schlafly, 26 Nobel laureates in Economics, Physics, Chemistry, and Medicine expressed their opposition to the bill in the fall of 1997, claiming that S. 507 would be damaging to American small inventors and go against the spirit of the U.S. patent system. They stated that “provisions for 18-month publication and prior-user rights would curtail the protection obtained through patents for small businesses and individual inventors relative to large multi-national corporations, and thus would discourage the flow of new inventions.”

In an individual statement, Franco Modigliani wrote that “the effort to rush through the Senate this questionable and potentially highly detrimental legislation is inexcusable,” and that S. 507 “is against the spirit of the U.S. patent system, which is a great economic and cultural invention.”

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

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

In the Senate, the proposed AIPA reform bill was included into a much larger $385 billion spending package along with two other intellectual property bills, the “Anti-Cybersquatting Act” and the “Satellite Home Viewer Act.” The omnibus spending bill was approved by the Senate, and on November 29, 1999, ten days after the vote in the Senate, President Bill Clinton signed the AIPA into law. The 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.