Creditor Interventions and Firm Innovation: Evidence from Debt Covenant Violations*

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Abstract

We examine the effect of creditor interventions on corporate innovation and firm value via the lens of debt covenant violations, where control rights are shifted from equity holders to creditors. Using a difference-in-differences approach and a regression discontinuity design, we find that creditor interventions have a negative, causal effect on innovation output. We further show that the reduction in innovation output is concentrated in innovation activities that are unrelated to the violating firm's main business, which leads to a more focused scope of innovation investment and ultimately an increase in firm value. Human capital redeployment appears a plausible underlying mechanism through which creditor interventions refocus firm innovation scope and enhance firm value. Our findings are consistent with the argument that creditors help mitigate investment distortions in innovation arising from conflicts of interest between managers and shareholders. Our paper sheds new light on the real effect of creditor interventions.

JEL Classification: G21, G32, G34, O31

Keywords: Creditor interventions, innovation, covenant violations, conflicts of interest, firm value

1. Introduction

Motivating technological innovation is vital for a country's economic growth (Solow, 1957; Romer, 1986) and competitive advantage (Porter, 1992). According to Rosenberg (2004), 85% of economic growth could be attributable to technological innovation. However, effectively motivating innovation is challenging for most organizations and firms. Given its importance, investigation into factors that enhance or impede innovation is warranted. While the existing literature has a good understanding about the effects of various ingredients of equity markets on innovation (e.g., Aghion et al., 2013; Chemmanur et al., 2013; He and Tian, 2013; Nanda and Rhodes-Kropf, 2013), studies examining how credit markets affect innovation are relatively sparse. In this paper, we focus on one key ingredient of credit markets, i.e., banks.

Banks are generally passive investors. Unlike active equity investors such as venture capitalists or hedge fund activists, banks do not get involved in a firm's daily operations when the firm is in good financial conditions. Therefore, it is difficult to gauge (if any) the direct effect of bank financing on firm innovation. However, upon a firm's debt covenant violation, control rights are shifted from equity holders to creditors who are able to affect a firm's innovation policy through their influence on firm managers during debt contract renegotiations. While more non-bank financial institutions (e.g., mutual funds and hedge funds) participate in the syndicated loan market in recent years, lead lenders that are responsible for negotiating and renegotiating loan contract terms are exclusively banks. Therefore, we use the words "creditors", "lenders", and "banks" interchangeably in this paper. Specifically, upon covenant violations, banks have the ability to accelerate debt principal, increase the loan rate, and terminate unused credit line facilities.[1] In this paper, we use an observable event, debt covenant violations, to evaluate the causal effect of bank interventions on firm innovation, using a rich set of identification strategies. We further explore the valuation effect of bank interventions on violating firms through the innovation channel.

We develop two hypotheses based on existing literature and the prevailing views of bank interventions. Our first hypothesis argues that bank interventions reduce firm innovation output. This reduction in innovation output could because of both bad reasons and good reasons. First,

[1]Although creditors often waive the violation, the potential threat associated with these activities allows banks to exert significant influence over the firm. See, e.g., Chava and Roberts (2008), Roberts and Sufi (2009), and Nini, Smith, and Sufi (2009, 2012), for recent studies that show how the transfer of control rights triggered by covenant violations influences firms' financial and investment policies and performance.

due to the payoff structure of creditors (i.e., creditors do not share upside returns when innovation succeeds but suffer from downside losses when innovation fails), Stiglitz (1985) points out that a debt contract is not well suited for innovative firms with uncertain and volatile returns. Second, there is a hold-up problem associated with bank interventions. Because banks collect soft information about the firm (such as the underlying quality and prospects of its innovative projects) that the firm cannot easily communicate to other investors, banks have bargaining power over the returns from the firm's investing in innovative projects, once the innovation process has started. Hence, as argued by Hellwig (1991) and Rajan (1992), powerful banks frequently stifle innovation by extracting informational rents. If these arguments are true, we expect firm innovation output drops after banks control. Given that innovation output is positively associated with firm value (Hall, Jaffe, and Trajtenberg, 2005), the reduction in innovation output leads to a drop in firm value.

Alternatively, bank interventions could reduce firm innovation for good reasons. For example, due to conflicts of interest between managers and shareholders, managers may overinvest in innovation projects to enjoy their private benefits from such activities. Scharfstein and Stein (2002) argues that specialized investment such as investment in innovation whose process are long, risky, and idiosyncratic effectively entrenches the management. In addition, managers with career concerns who want to "grandstand" could overinvest in innovative projects that may not necessarily best serve shareholders' interest (e.g., Gompers, 1996). Finally, overconfident CEOs could overinvest in innovative projects in non-innovative industries that do not improve firm performance (Hirshleifer, Low, and Teoh, 2012). After creditors step in upon covenant violations, they could help curtail excessive investments in innovative projects that are tangent to the firm's main business and hence are value-destroying. If this argument is true, we expect that firm innovation output drops after bank interventions but this reduction in innovation output leads to an increase in firm value

Our second hypothesis argues that bank interventions may not affect firm innovation and therefore have no effect on firm value through the innovation channel. As Holmstrom (1989) points out, innovative activities involve the exploration of untested and unknown approaches that is risky and idiosyncratic. Hence, firms investing more heavily in innovative projects may have to make partial disclosure and have a greater degree of information asymmetry (Bhattacharya and Ritter, 1983) and hence have a harder time raising external capital to fund innovative projects (Myers and Majluf, 1984). Banks may potentially help mitigate this distortion of investment in innovation because their financing is more relationship-based than arm's length.[2] Through intensive and repeated personal interactions between bank officers and firm managers, banks are able to closely monitor the firm, collect significant soft information about the firm, and better understand the true value of the firm's investments in innovative projects. These advantages of banks allow the firm, upon covenant violation, to continue their current investment in innovative projects (rather than being forced to shut them down), so that their innovation output is not affected by covenant violations.

We disentangle above hypotheses by first examining the effect of bank interventions (triggered by covenant violations) on firm innovation. Obtaining information from the National Bureau of Economic Research (NBER) Patent Citation database, we use the number of patents granted to a firm and the number of future citations received by each patent to measure innovation output. Specifically, the former captures the quantity of innovation and the latter proxies the quality of innovation. Our use of patenting to capture firms' innovation output has become standard in the innovation literature (e.g., Acharya et al., 2014; Aghion et al., 2013; Nanda and Rhodes-Kropf, 2013).

Our baseline ordinary least squares (OLS) results suggest that bank interventions are negatively related to firm innovation. Firms that violate covenants produce fewer patents and patents with lower impact one, two, and three years after the covenant violation compared to firms that do not violate covenants. Specifically, bank interventions due to a violation of debt covenants are associated with a 2.9% drop in patent counts and a 3.3% decline in the number of citations per patent three years after the violation.

While the baseline results are consistent with the hypothesis that bank interventions reduce firm innovation, an important concern is that bank interventions due to covenant violation are likely to be endogenous. Unobservable firm heterogeneity correlated with both covenant violations and firm innovation could bias our results (i.e., the omitted variable concern). Meanwhile, firms with low innovation potential (and therefore lower future innovation output) may be fundamentally lower quality firms and therefore are more likely to violate covenants, which is the reverse causality concern. To establish causality, we use two identification strategies.

[2] Existing literature has intensively documented the benefits of relationship lending, e.g., Hoshi, Kashyap, and Scharfstein (1990), Petersen and Rajan (1994), and Berger and Udell (1995), etc.

Our first identification strategy is to use a difference-in-differences (DiD) approach that compares the innovation output of firms that violate covenants (i.e., the treatment firms) to that of firms that do not violate covenants (i.e., the control firms). We first carefully match treatment and control firms using the propensity score matching algorithm that includes a wide variety of firm characteristics. After undertaking various diagnostic tests to ensure the satisfaction of the parallel trend assumption and the removal of observable differences between these two groups of firms, we find that treatment firms experience an average of 17.4% larger drop in patent counts and an 11.5% larger decline in patent citations in the first three years after the covenant violation.

Our second identification strategy is to use a regression discontinuity design (RDD), following Chava and Roberts (2008). The RDD relies on "locally" exogenous variation in covenant violations generated by the distance to the covenant threshold. This empirical approach essentially compares the innovation output of firms that just violate covenants to those that barely avoid violating covenants. The RDD is a powerful and appealing identification strategy because for these firms falling in a narrow band of the distance to the covenant threshold, the covenant violation is very close to an independent, random event and therefore is unlikely correlated with firm unobservable characteristics. Our results from the RDD suggest that bank interventions due to covenant violations lead to a 15.9% drop in patent counts and a 9.4% drop in citations per patent three years after the violation.

Having established a negative, casual effect of bank interventions on firm innovation, we next attempt to answer the "bottom-line" question regarding the economic value implications of reductions in innovation output caused by bank interventions. We find that the reduction in innovation output is concentrated in innovation activities that are unrelated to a firm's main business. However, patent production related to a firm's main business remains unchanged after control rights are shifted to banks. As a result, firms have a more focused scope of innovation output after bank interventions. To the extent that innovative activities unrelated to a firm's main business could arise from managers' private benefits and could be out of managers' expertise, which are value-destroying, a more focused scope of a firm's innovation activities should enhance firm value.[3] We confirm this conjecture by showing that the refocus of a firm's innovation scope leads to an increase in firm value.

[3] See, e.g., John and Ofek (1995) and Daley, Mehrotra, and Sivakumar (1997) for a similar argument in the context of spinoffs and asset sales that increase corporate focus.

Finally, we show that human capital redeployment appears an underlying mechanism through which creditors curtail overinvestment in innovative projects that are unrelated to the firm's main business and hence enhance firm value. We find that leavers (inventors who leave the firm after the violation) of covenant violating firms are more likely to have expertise that are unrelated to a firm's main business than leavers of non-violating firms. In contrast, new hires (inventors who join the firm after the violation) of covenant violating firms are more likely to have expertise that is related to the firm's main business. Stayers (inventors who stay in the firm after the violation) of violating firms generate a smaller fraction of patents that are unrelated to the firm's main business post-violation than those of non-violating firms.

Our findings regarding the economic value added by creditor interventions suggest that creditors help mitigate investment distortions in innovation arising from conflicts of interest between managers and shareholders, which enhances firm performance. The evidence is consistent with the existing literature, e.g., Chava and Roberts (2008) and Nini, Smith, and Sufi (2009, 2012), which shows that creditor interventions increase firm value.

The rest of the paper is organized as follows. Section 2 discusses the related literature. Section 3 describes sample selection and variable constructions, and reports summary statistics. Section 4 presents the baseline results and addresses identification issues. Section 5 discusses economic efficiency implications of creditor interventions and explores possible underlying mechanisms. Section 6 concludes.

2. Relation to the existing literature

Our paper contributes to two strands of literature. First, our paper is related to the fast growing literature on finance and innovation. Theoretical work from Holmstrom (1989) argues that innovation activities mix poorly with routine activities in an organization. Aghion and Tirole (1994) suggest that the organizational structure of firms matters for innovation. Manso (2011)'s model shows that the optimal contract that motivates innovation involves a combination of tolerate for failure in the short run and reward for success in the long run.

Empirical evidence suggests that various equity market environment and characteristics affect managerial incentives to innovate. Specifically, a larger institutional ownership (Aghion, Van Reenen, and Zingales, 2013), private instead of public equity ownership (Lerner, Sorensen, and Stromberg, 2011; Bernstein, 2012), corporate venture capital (Chemmanur, Loutskina, and

Tian, 2013), “hot” rather than “cold” markets (Nanda and Rhodes-Krpof, 2013), lower stock liquidity (Fang, Tian, and Tice, 2013), and lower analyst coverage (He and Tian, 2013) alter managerial incentives and hence motivate managers to focus more on long-term innovation activities.[4] However, existing studies have largely ignored the role played by credit market investors. Although an emerging literature examines how banking deregulation and competition affect innovation (e.g., Amore et al., 2013; Chava et al., 2013; Cornaggia et al., 2013), there is no study that provides direct evidence on the effect of bank interventions on innovation. We contribute to this line of research by filling in this gap.

Our paper is related to Atanassov, Nanda, and Seru (2007) who make an important attempt to link a firm’s financing arrangements and innovation output. They show that arm’s length financing (equity and public debt) is positively related to innovation while relationship-based bank financing is negatively related to innovation. Our paper advances this line of inquiry in two important dimensions. First, using covenant violations that shift control rights from equity holders to creditors, we are able to directly examine the effect of bank interventions on innovation rather than relying on a firm’s loan stock to infer the effect of relationship-based bank financing. Second, using both a DiD approach and the RDD, our identification strategies allow us to evaluate the causal effect of bank interventions on firm innovation (as opposed to a partial correlation documented by their study).

Our paper is also related to a contemporaneous paper that studies the relation between bank financing and innovation (Chava, Nanda, and Xiao, 2013). Our paper differs from theirs in several ways. First, while our paper focuses on ex-post innovation consequences of bank interventions triggered by covenant violations, Chava, Nanda, and Xiao (2013) mainly examine the ex-ante effect of a firm’s innovativeness on its ability to obtain bank loans. Second, our paper captures firm innovation using their patenting activity, while they use R&D as an innovation proxy when examining the consequences of covenant violations. We believe patenting activity captures a firm’s innovation better than R&D because patenting is an innovation output variable, which encompasses the successful usage of all (both observable and unobservable) innovation inputs. In contrast, R&D expenditures only capture one particular observable quantitative input

[4] Other studies examine the effects of venture capital investment, product market competition, bankruptcy and labor laws, financial market development, firm boundaries, and investors’ attitudes toward failure on firm innovation (e.g., Kortum and Lerner, 2000; Aghion et al., 2005; Acharya and Subramanian, 2009; Acharya, Baghai, and Subramanian, 2013; Hsu, Tian, and Xu, 2014; Seru, 2014; Tian and Wang, 2014).

(Aghion, Van Reenen, and Zingales, 2013) and are sensitive to accounting norms such as whether it should be capitalized or expensed (Acharya and Subramanian, 2009). In addition, information on R&D expenditures reported in Compustat is quite unreliable, which may introduce a significant measurement error problem.[5]

Second, our paper adds to a growing literature on credit controls and the effects of covenant violations. Chava and Roberts (2008) find a decline in firm investment after the violation and this reduction is more pronounced in firms in which agency and information problems are more severe. Roberts and Sufi (2009) focus on the effect of covenant violations on capital structure and find that net debt issuing drop significantly and the decline is persistent following covenant violations. Nini, Smith, and Sufi (2009) show that 32% of private credit agreements contain an explicit restriction on the firm's capital expenditures and these restrictions cause a reduction in firm investments in tangible assets. Nini, Smith, and Sufi (2012) find that covenant violations are followed by a decline in acquisitions and capital expenditures, a reduction in leverage and payouts, and an increase in CEO turnover. They also show that firm operating and stock price performance improve after creditor interventions. Billett, Esmer, and Yu (2013) find that covenant violations affect rival firms' product market behavior.

3. Sample selection, variable construction, and summary statistics

3.1. Data and sample construction

We start with a sample of 10,537 non-financial U.S. firms and 262,673 firm-quarter observations from 1996 through 2008 for which Nini, Smith and Sufi (2012) have collected information on whether a firm is in violation of a financial covenant based on 10-Q or 10-K SEC filings.[6] Our dataset of patents and citation is obtained from three sources. First, we retrieve our patent data from the latest version of the NBER Patent Citation database. The NBER database provides information for all utility patents granted by the US Patent and Trademark Office (USPTO) over the period of 1976-2006. Second, we supplement the information for patents

[5] In the Compustat database, more than 50% of firms do not report R&D expenditures in their financial statements. However, the fact that a firm does not report its R&D expenditures does not necessarily mean that the firm is not undertaking innovation activities. Replacing missing values of R&D expenditures with zeros, a common practice in the existing literature, introduces additional noise that could bias the estimated effect on innovation measured by R&D expenditures.

[3] This dataset is available at Professor Amir Sufi's website http://faculty.chicagobooth.edu/amir.sufi/. The sample begins in 1996 and covenant violations are disclosed in the 10-Q or 10-K SEC filings. Detailed sample selection is provided in Nini, Smith, and Sufi (2012).

granted over the period of 2007-2009 provided by Kogan et al. (2012) that is available at https://iu.box.com/patents. Third, we construct a dataset for patent citations over the period of 2007-2009 using the Harvard Business School (HBS) patent and inventor database available at http://dvn.iq.harvard.edu/dvn/dv/patent.

To calculate control variables, we obtain firms' accounting information from the Compustat database, stock returns from the CRSP database, and institutional holdings data from Thomson's CDA/Spectrum database (form 13F). We end up with a final sample of 60,954 firm-year observation from 9,687 firms. Among the sample, 3,442 firms are in violation of financial covenants at least once during our sample period, resulting in 3,698 firm-year violation observations. In other words, 36% firms in our sample breach a covenant during the sample period. This observation is similar to those documented in previous studies, which suggests that covenant violation is a fairly common phenomenon (Robert and Sufi, 2009; Nini, Smith, and Sufi, 2009, 2012).

3.2. Variable construction

3.2.1. Measuring innovation

We use two measures to gauge a firm's innovation output. The first measure is the total number of patents applied in a given year (and eventually granted). Hall, Jaffe, and Trajtenberg (2001) find that there is a two to three years lag between patent application year and grant year with a significant variance. We use the application year instead of grant year because the actual timing of the patented innovation is closer to the application year. The number of patents captures the quantity of innovation. To measure the quality of patent, we construct the second measure, the total number of citations each patent receives in subsequent years.

Both measures are subject to truncation problems. Since we only observe granted patents, patents applied in the last several years of our sample may not be granted. Similarly, patents tend to receive citations over a long period, but we observe at best the citations receive up to 2010. To deal with these truncation problems, we adjust the patent and citation data by using the "weight factors" computed from the empirical distributions of application-grant lag and by estimating the shape of the citation-lag distribution, respectively. In particular, we correct the truncation problem of patent counts during the last 6 sample years following Fang, Tian, and Tice (2013). To correct the truncation problem with the number of citations for our extended sample period, we move the adjustment factors created by NBER patent data project forward by four years since our sample is extended by four years from 2006.[7] Moreover, Hall, Jaffe, and Trajtenberg (2001) suggest that most patents are granted within two years, therefore we exclude the last two year of patent data (2009-2010) to mitigate the truncation problem.

3.2.2. Control variables

Following the prior literature in innovation, we control for a set of firm and industry characteristics that might affect a firm's future innovation output. All variables are computed for firm *i* over its fiscal year *t*. In the baseline regressions, the control variables include firm size, *Ln(MV)*, measured by the natural logarithm of market value of equity; profitability, *ROA,* measured by return-on-assets ratio; investments in innovation, *R&D_Assets*, measured by R&D expenditure divided by total assets; asset tangibility, *PPE_Assets*, measured by net property, plants, and equipment divided by total assets; leverage, *Leverage*, measured by total debt scaled by total assets; capital expenditure to total assets ratio, *CAPEX_Assets*; product market competition, *HI*, measured by the Herfindahl index based on annual sales; growth opportunities, measured by *Tobin's Q*; financial constraints, *KZ_Index*, measured by the five variable KZ index described in Kaplan and Zingales (1997); firm age, *Ln(Age),* measured by the natural logarithm of one plus the number of years the firm is listed on Compustat; institutional holdings, *INST,* calculated as the arithmetic mean of the four quarterly institutional holdings reported through form 13F; debt to EBITDA ratio, *Debt_EBITDA*, measured by the total debt divided by earnings before interest, taxes, depreciation, and amortization; net worth to assets ratio, *Networth_Assets*, measured by the total assets minus total liabilities scaled by total assets; and current ratio, *Current_Ratio*, measured by the total current assets divided by total current liabilities. To circumvent potential non-linear effects of product market competition (Aghion et al., 2005), we also include the squared Herfindahl index in our baseline regressions. We describe detailed variable definitions in Appendix.

3.3. Summary statistics

[7] For example, in the original NBER data, for a patent granted in year 1998 and have a "chemical" classification, the adjustment factor for its citations received is 1.9238. Now, for a patent granted in year 2002 (1998+4) and have a "chemical" classification, the adjustment factor for its citations received is 1.9238.

To mitigate the effect of outliers, we winsorize all variables at the upper and bottom 1% of their distribution. Table 1 presents summary statistics of the main variables used in the analysis of a sample of U.S. non-financial firms from 1996 to 2008. On average, a firm in our sample generates 3.4 patents per year and each patent receives 3.8 subsequent citations. This result is comparable to those documented in previous literature (e.g., Atanassov et al, 2007; He and Tian, 2013). In our sample, about 13% of firm-year observations are in violation of financial covenants, suggesting that covenant violation is not a rare event. A median firm in our sample has a market capitalization of $114 million, ROA of 9%, leverage of 18%, Tobin's *Q* of 1.61, and is 11 years old since its first appearance in Compustat.

4. Empirical results

4.1. OLS results

To assess the effect of creditor interventions on firm innovation, we estimate the following model using the OLS:

$$LnPat_{i,t+n}(LnCite_{i,t+n}) = \alpha + \beta Violation_{i,t} + \gamma' Controls_{i,t} + Year_t + Industry_j + \varepsilon_{i,t}, \tag{1}$$

where i indexes firm, j indexes industry, t indexes time, and n equals one, two, or three. The dependent variable, $LnPat_{i,t+n}$, is the natural logarithm of one plus total number of patents filed (and eventually granted) in one, two, and years later, and results are reported in columns (1) – (3), respectively. The dependent variable, $LnCite_{i,t+n}$, is the natural logarithm of one plus the number of citations per patent for patents generated in one, two, and three years later, and results are reported in columns (4) – (6), respectively. The variable of interest, $Violation_{i,t}$, is a dummy variable that equals one if a covenant violation occurs in year t for firm i and not preceded by a violation in the previous year (t-1), and zero otherwise. Following existing literature, we include a vector of control variables that may affect a firm's innovation output as we discussed in Section 3.2.3. We also include both year fixed effects $Year_t$ and industry fixed effects $Industry_j$ in all regressions to absorb any variations that vary only by year or by industry but cannot explain our main findings. We cluster standard errors at the firm level to avoid inflated t-statistics.

We report the results estimating equation (1) in Table 2. The coefficient estimates on *Violation* are negative and significant at the 5% level in all three columns (1) – (3) when the number of patents is the dependent variable, suggesting that a covenant violation is associated with a reduction in patent counts. To be more concrete, based on the coefficient estimate reported in column (3), a covenant violation is associated with a 2.9% reduction in the number of patents generated three years after the violation. In columns (4) – (6), we replace the dependent variable with patent quality and find that the coefficient estimates on *Violation* are again negative and significant in all three columns. Based on the coefficient estimate reported in column (6), patents generated by violating firms three years after the violation receive 3.3% fewer subsequent citations.

We conduct various robustness tests for our baseline specifications. To save space, we tabulate these robustness test results in the Internet Appendix. First, we employ an alternative econometric model, the quantile regression model, to address the skewness of our innovation variables (only about 25% of our firm-year observations have a non-zero number of patents). Consistent with our baseline results, we find that the coefficient estimates of *Violation* are negative and significant in all specifications when we run the quantile regressions at the 75^{th}, 80^{th}, 85^{th}, or 90^{th} percentiles.

Second, we check the robustness of our results using alternative proxies for innovation, patent originality and generality, following Hall, Jaffe, and Trajtenberg (2001). Patents that cite a wider array of technology classes of patents are considered as having greater originality. We define a patent's originality score as one minus the Herfindahl index of the three-digit technology class distribution of all the patents it cites. In a similar spirit, patents that are being cited by a wider array of technology classes of patents are viewed as having greater generality. We define a patent's generality score as one minus the Herfindahl index of the three-digit technology class distribution of all the patents that cite it. We find that *Violation* is negatively and significantly related to *Generality* in all specifications and negatively and significantly related to *Originality* in the first year after violation (*t+1*).

Third, a reasonable concern is that large firms often enhance innovation by acquiring small firms (Sevilir and Tian, 2012). In the meantime, covenant violating firms make substantially less acquisitions (Nini, Smith, and Sufi, 2012). Therefore, our baseline findings may be affected by firms' acquisitions. To address this concern, we construct a variable, *AcqAssets*, which equals a firm's acquisition expenditures normalized by its total assets, and include it in equation (1). We obtain both quantitatively and qualitatively similar results.

Overall, our evidence from the OLS regressions suggest that bank interventions are negatively related to a firm's innovation output, consistent with the first hypothesis.

4.2. Identification

In this section, we attempt to address the identification issue and establish causality. It is possible that both firm innovation and debt covenant violations are determined by firm unobservable characteristics. For example, firms with depleted future investment opportunities are more likely to perform poorly, and hence violate debt covenants. Such firms are also likely to have lower level of future innovation output due to lack of investment opportunities. Therefore, unobservable attributes could bias our results. Section 4.2.1 discusses our first identification strategy that uses a DiD approach. Section 4.2.2 discusses the second identification strategy using the RDD to infer the direction of causality.

4.2.1 The difference-in-differences approach

Our first identification strategy is to use the DiD approach. Specifically, we compare the innovation output of a sample of treatment firms that violate debt covenants to the innovation output of control firms that do not violate any covenant but are otherwise comparable, before and after the shift in control rights to creditors. The DiD approach has some key advantages. First, the DiD methodology rules out omitted trends that are correlated with covenant violation and innovation in both the treatment and control groups. As an example of an omitted trend, changes in firms' investment opportunities may simultaneously affect the likelihood of covenant breach and future innovation. The DiD approach rules out the possibility that a change in firms' investment opportunities is driving the change in innovation rather than a shift in bank control rights. Second, the DiD approach controls for constant unobserved differences between the treatment and the control group. For example, certain management traits such as their overconfidence could be correlated with both a firm's propensity to violate covenants and innovation, and hence may drive the negative relation between them.[8]

We construct a treatment group and a control group of firms using the propensity score matching. Specifically, we begin with a sample of all non-financial U.S. Compustat firms that violate or do not violate debt covenants during 1996-2008 and have non-missing matching variables and non-missing innovation outcome variables in the pre-violation year (*t-1*) and the post-violation year (*t+1*), with *t* being the fiscal year during which new covenant violation

[8] Note that although the use of the DiD is very powerful at ruling out alternative explanations, it does not entirely eliminate the possibility of an unobservable that affects the treatment and control groups differentially and is correlated with innovation. We address this concern using the RDD in Section 4.2.2.

occurred for firm *i*. According to Nini, Smith, and Sufi (2012), new violations are defined to be violations in which the firm has not violated any financial covenant in the previous four quarters. The control group is defined as those non-violating firm-year observations and not preceded by a violation in the previous year (*t-1*). To eliminate the selection bias between firms with and without loan contracts, we also require the control group to have bank loans outstanding.

We employ a propensity score matching algorithm to identify matches between firms violating covenants and firms not violating. When applying the propensity score matching, we first estimate a probit model based on the 3,606 sample firm-year observations with new violations of covenant and 18,867 non-violation firm-year observations. The dependent variable is one if the firm-year belongs to the violating firm group and zero otherwise. In the probit model we include all control variables from the baseline specification in equation (1) that are measured in the year immediately preceding the violation, Fama-French industry dummies, and year fixed effects. We also include the pre-violation innovation growth variables (i.e., the growth in the number of patents (*Pat_growth)* and the growth in the number of citations per patent (*Cite_growth)*, both computed over the three-year period before the violation). We include these two variables to help satisfy the parallel trends assumption because the DiD estimator should not be driven by the differences in any industry or firm characteristics.[9]

We report the probit model results in column (1) of Table 3 Panel A. The results suggest that the specification captures a significant amount of variation in the choice variable, as indicated by a pseudo-R^2 of 14.7% and a p-value from the χ^2 test of the overall model fitness well below 0.001. We then use the predicted probabilities, or propensity scores, from column (1) and perform a nearest-neighbor propensity score matching procedure. Specifically, we match each violating firm-year observation (labeled as treatment group) to a firm-year observation from the non-violating sample (labeled as control group) with the closest propensity score. If a firm from the control group is matched with more than one treatment firm, we retain the pair for which the distance between the two firms' propensity scores is the smallest. We also require the differences in propensity score to be less than 0.1. We end up with 3,604 unique pairs of matched firms.

[9] As Lemmon and Roberts (2010) point out, the parallel trends assumption does not require the level of outcome variables (innovation variables in our setting) to be identical across the treatment and control firms or across the two regimes, because these distinctions are differenced out in the estimation. Instead, this assumption requires similar trends in the innovation variables during the pre-violation regime for both the treatment and control groups.

Because the validity of the DiD estimate critically depends on the satisfaction of the parallel trends assumption, we undertake a number of diagnostic tests to verify that we do not violate the assumption. First, we re-run the probit model restricted to the matched sample and report the probit estimates in column (2) of Table 3 Panel A. None of the independent variables is statistically significant. In particular, the coefficient estimates of pre-violation innovation growth variables are not statistically significant, suggesting that there are no observable different trends in innovation outcomes between the two groups of firms before the violation. Also, the coefficient estimates in column (2) are generally much smaller in magnitude than the ones in column (1), suggesting that the results in column (2) are not simply an artifact of a decline in degrees of freedom due to the drop in sample size. Finally, the pesudo-R^2 drops dramatically from 14.7% prior to the matching to 0.2% post the matching, and a χ^2 test for the overall model fitness shows that we cannot reject the null hypothesis that all of the coefficient estimates of independent variables in column (2) are zero (with a p-value of 0.99).

In our second diagnostic test, we evaluate the accuracy of the matching process and report the difference between the propensity scores of treatment group and those of their matched control group. Table 3 Panel B shows that the difference in the propensity scores is very small. For example, the average distance is 0.001 and it is 0.003 up to the 95th percentile of the difference distribution.

Finally, we report the univariate comparisons between the characteristics of treatment and control firms pre-violation and their corresponding *t*-statistics in Panel C. None of the observed differences between the treatment and control firms' characteristics is statistically significant in the pre-violation regime. In particular, the two groups of firms have similar levels of ROA, Tobin's Q, and leverage, although the treatment group is in violation of debt covenants whereas the control group is not. Moreover, the univariate comparisons for innovation growth variables suggest that the parallel trends assumption is likely satisfied. Overall, the diagnostic tests reported above suggest that the propensity score matching process has removed meaningful observable differences (other than the difference in the shift of control rights upon covenant violations), which raises the likelihood that the changes in innovation output are caused only by the shift in bank control rights due to breach of debt covenants.

Table 3 Panel D presents the DiD test results. Column (1) reports the average change in the number of patents (labeled as *Pat*) and the average change in the citations each patent receives (labeled as *Cite*) for the treatment group. The changes are computed by first subtracting the total number of patents (citations per patent) counted over the three-year period immediately preceding covenant violation from the number of patents (citations per patent) counted over the three-year period immediately post violation for each treatment firm. The differences are then averaged over the treatment group. By the same token, we compute the average changes in the number of patents and citations per patent for the control group and report them in column (2). In columns (3) and (4), we report the DiD estimators and the corresponding two-tailed *t*-statistics testing the null hypothesis that the DiD estimators are zero. We find that the DiD estimators of *Pat* and *Cite* are both negative and statistically significant.

The magnitude of the DiD estimators of *Pat* and *Cite* suggests that the negative effect of bank interventions on innovation is economically significant. On average, bank interventions due to covenant violations result in a drop of about 1.8 more patents and 1.3 more citations per patent in the three-year period immediately post the violation relative to the three-year immediately preceding the violation for the treatment firms than for the control firms. The DiD estimators of *Pat* corresponds to approximately a drop of 1.8/3 = 0.6 more patents per year, 17.4% of 3.4 patents, the sample average of the number patents granted per year. Similarly, the DiD estimators of *Cite* corresponds to a drop of 1.3/3 = 0.43 more citations per patent a year, 11.5% of 3.8, the sample average of the number of citations each patent receives.

We next show the innovation dynamics of the DiD results in a regression framework to address a potential reverse causality concern, namely, reductions in innovation output due to poor investment opportunities lead firms to violate debt covenants. Following Bertrand and Mullainathan (2003), we retain firm-year observations for both treatment and control firms for a seven-year window centered in the violation year and estimate the following model:

$$\begin{aligned} LnPat\ (LnCite\ ) &= \alpha + \beta_1 Violator * Before^{-1} + \beta_2 Violator * Current + \beta_3 Violator * After^{1} \\ &+ \beta_4 Violator * After^{2\&3} + \beta_5 Before^{-1} + \beta_6 Current + \beta_7 After^{1} + \beta_8 After^{2\&3} + \beta_9 Violator + \varepsilon. \end{aligned} \tag{2}$$

The dependent variable is either *LnPat,* the natural logarithm of one plus firm *i*'s number of patents in a given year, or *LnCite*, the natural logarithm of one plus firm *i*'s number of citations per patent in a given year. *Violator* is a dummy that equals one for treatment firms (violating firms) and zero for control firms (non-violating firms). $Before^{-1}$ is a dummy that equals one if a firm-year observation is from the year immediately before the covenant violation (year -1) and zero otherwise. *Current* is a dummy that equals one if a firm-year observation is in the violation year (year 0) and zero otherwise. $After^1$ is a dummy that equals one if a firm-year observation is from the year immediately after the violation (year 1) and zero otherwise. $After^{2\&3}$ is a dummy that equals one if a firm-year observation is from two or three years after the violation (year 2 and 3) and zero otherwise. Therefore, the omitted group (benchmark) is the observations two or three years before the violation (year -2 and -3). We present the regression results estimating equation (2) in Panel E of Table 3. We report the robust standard errors.

The coefficient estimates of interest are β_1, β_2, β_3, and β_4. If the negative relationship between covenant violation and firm innovation is driven by reverse causality – reductions in innovation activities associated with poor investment opportunities lead firms to violate debt covenants, then we should observe significant and negative coefficients of β_1, and β_2. In both columns, we find statistically insignificant coefficient estimates of β_1, and β_2, which suggests that there is not a pre-existing trend in firm innovation output. The coefficient estimate of β_4 is negative and statistically significant, although the coefficient estimate of β_3 is insignificant, suggesting that compared to the control firms, the treatment firms experience a significant reduction in patent counts and citations per patent starting the 2nd year following bank interventions. One possible explanation is that it takes time to undertake innovation activities and develop patents and therefore we expect a time lag between bank interventions and the effect on observable innovation output. Overall, the results suggest that there is no pre-existing trend in innovation before bank interventions and our findings are not driven by reverse causality.

4.2.2. The regression discontinuity design

Our second identification strategy is to use the RDD, following Chava and Roberts (2008). This approach depends on "locally" exogenous variation in covenant violations generated by the distance to the covenant threshold. This empirical approach essentially compares the innovation output of firms that just violate covenants to those that barely avoid violating covenants. The RDD is a powerful identification strategy because for these firms falling in a narrow band of the distance to the covenant threshold, the violation is very close to an independent, random event and therefore is unlikely correlated with firm unobservable characteristics.

For this purpose, we limit our attention to a sample of bank loans for which we know the covenant thresholds, as well as any changes (or "buildup") in those thresholds over time during

1996-2008. This analysis alleviates two potential concerns using covenant violations reported in 10-K filings: (1) we do not know the exact covenant threshold, and (2) we only observe reported covenant violations. We follow Chava and Roberts (2008) and restrict the sample to observations that satisfy the following requirements: (1) they must be non-financial firms that exist in both merged CRSP-Compustat database and the Dealscan database; and (2) they must be firms that have had a loan contract containing either a current ratio or net worth covenant to ensure an accurate measurement of the relevant accounting variable.[10] Current ratio and net worth information is available on quarterly basis, thus we are able to identify whether a firm is in breach of current ratio or net worth covenants every quarter. However, innovation output is measured on annual basis, so our analysis is conducted on annual frequency. Our final sample consists of all firm-year observations in which a covenant restricting the current ratio or net worth of the firm is imposed by a private loan contract found in Dealscan during 1996 to 2008.[11]

Following the existing literature (i.e., Lee and Lemieux, 2010; Cuñat, Gine, and Guadalupe, 2012; Bradley, Kim, and Tian, 2013), we start our RDD analysis with an estimation of a polynomial model that makes use of all the observations in the sample. This method allows us to incorporate the precise distance to the covenant threshold into our regression specification. Specifically, we estimate the following model:

$$Ln(Innovation_{i,t+n}) = \alpha + \beta Violoation_{i,t} + P_l(CR_i) + P_r(CR_i) + P_l(NW_i) + P_r(NW_i) + Yr_t + Ind_j + \varepsilon_{i,t}, \qquad (3)$$

where i denotes firm, j denotes industry, and t denotes time. To determine whether or not a firm is in violation, we compare the firm's actual accounting measure (i.e., current ratio or net worth) to the covenant threshold implied by the terms of the debt contract. $Violation_{i,t}$ is a dummy variable that equals one if a firm's current ratio or net worth falls below the corresponding covenant threshold in any of the four quarters in a fiscal year. $P_l(CR_i)$ and $P_r(CR_i)$ are flexible polynomial functions of the distant to default on the left-hand and right-hand side (respectively) with respect to the current ratio covenant threshold for firm i with different orders. $P_l(NW_i)$ and $P_r(NW_i)$ are flexible polynomial functions of the distant to default on the left-hand and right-hand side (respectively) with respect to the net worth covenant threshold for firm i with different

[10] Covenants restricting the debt to EBITDA ratio, for example, create a problem when trying to measure this ratio with Compustat accounting data since "debt" can refer to any component of a firm's debt structure including: long-term, short-term, senior, junior, secured, total, funded, etc.

[11] Our sample is larger than that in Chava and Roberts (2008) because they restrict their attention to the subsample of firms that experience at least one covenant violation. In contrast, we include the entire sample of firms, including those that have not had any covenant violation in our sample period.

orders. Distant to default is the absolute difference between current ratio or net worth and the corresponding covenant thresholds. If a firm does not violate covenants, we include in the regressions the polynomials of the average of the distant to default in all four quarters. If a firm violates covenant in a particular quarter, we use the polynomials of the distant to default in the violating quarter. However, if a firm violates covenant in more than one quarter in a fiscal year, we use the polynomials of the maximum distant to default.

The key variable of interest is β, which captures the causal effect of bank interventions on firm innovation output n years after the covenant violation. Note, however, that due to the local exogeneity nature of the RDD, this coefficient should be interpreted locally in the immediate vicinity of the covenant violation threshold.

We present the results estimating equation (3) in Table 4 Panel A. We report the result with polynomials of order two, but our results are qualitatively similar using other polynomial orders. The coefficient estimates on *Violation* are negative and significant in all columns, consistent with our baseline findings. The results are also economically significant. Based on the regressions with three years post-violation innovation output as the dependent variable reported in columns (3) and (6), we find that a covenant violation leads to a 15.9% decline in patent quantity and 9.4% decline in patent quality, respectively.

We also conduct the RDD in an alternative form by considering narrow "bands" around the covenant threshold. Thus, these firms' current ratio or net worth falls either right above or right below covenant threshold. Following Chava and Roberts (2008), we restrict the sample to those observations in which the absolute value of the relative distance to the covenant threshold is less than 0.20. We report the results in Panel B. The coefficient estimates of *Violation* are negative in all specifications, and are statistically significant at the 1% level despite of the smaller sample size. Furthermore, the RDD with a small 'band' surrounding the threshold suggests an even larger economic effect of covenant violation on firm innovation compared to what we obtain from the RDD using the full sample. We observe that a covenant violation leads to a 22.1% decline in patent quantity and 19.9% decline in patent quality within a narrow "band" around the threshold.

Given that the above results are obtained from a very narrow margin around the covenant threshold and to the extent that firms falling in this narrow margin are considered almost "identical" in other firm characteristics, the violation of covenant is "locally" exogenous and therefore any subsequent differences in innovation output should be attributable to change in control rights to creditors.

Comparing the results obtained from the OLS, the DiD, and the RDD analyses, it appears that the OLS biases the effect of covenant violations on firm innovation upward due to endogeneity. This observation suggests that some omitted variable simultaneously make firm more innovative and more likely to violate covenants. Certain CEO traits such as CEO overconfidence could be an example of such an omitted variable. For example, overconfident CEOs invest more in innovation and obtain more patents and patent citations (Hirshleifer et al., 2012). Meanwhile, overconfident CEOs tend to overestimate their ability and judgment when managing firms and hence are more likely to violate covenants. This positive correlation between covenant violations and innovation arising from omitted variables thus biases the estimated effect upward. Our DiD and RDD mitigate the bias and thus document a larger effect (i.e., more negative estimated effect of covenant violations on innovation). Overall, our identification tests suggest that bank interventions triggered by covenant violations have a negative, causal effect on firm innovation.

5. Economic value implications of creditor interventions

Our results so far suggest that creditor interventions triggered by covenant violations cause a significant reduction in a firm's innovation output. However, it is unclear if the reduction in a firm's innovation output is value-enhancing or value-destroying. Existing literature tends to find that innovation output is positively associated with firm value (Hall, Jaffe, and Trajtenberg, 2005), and hence our findings could imply that creditors reduce firm value by impeding innovation. On the other hand, many theories in the literature (e.g., Jensen, 1986; Aghion and Bolton, 1992; Dewatripont and Tirole, 1994) argue that creditor interventions might enhance firm value by mitigating value-destroying managerial actions, such as excessive investments in innovation projects that reduces firm value, which arises from conflicts of interest between managers and shareholders. For example, Scharfstein and Stein (2002) argue that specialized investment such as investment in innovation projects effectively entrenches the management. Therefore, our findings could also imply that creditor interventions enhance firm value. This latter interpretation is consistent with the existing literature, e.g., Chava and Roberts (2008) and Nini, Smith, and Sufi (2009, 2012).

In this section, we focus on the "bottom-line" question regarding the economic consequences of innovation reductions after creditor interventions. Specifically, we examine how a reduction in innovation output affects firm value and explore underlying mechanisms through which this occurs.

5.1. Firm value and refocus of innovation scope

To investigate the value implications of our findings that creditor interventions reduce innovation output, we use Tobin's Q to measure firm value. If the stock market is efficient, we examine any effects of reduction in innovation output should be impounded in future Q. We use the change in Tobin's Q surrounding the covenant violation as the dependent variable, and relate it to the changes in innovation output caused by creditor interventions. Specifically, we use the propensity-score-matched sample of violating and non-violating firms from Section 4.2.1 and retain firm-year observations for both treatment and control firms for a seven-year window centered in the violation year. We then estimate the following model:

$$\begin{aligned}\Delta Q_{i,t-1\to t+3} &= \alpha + \beta_1 Violator_{i,t} + \beta_2 Violator_{i,t} * \Delta Pat_{i,t-3\to t+3} + \beta_3 \Delta Pat_{i,t-3\to t+3} + \gamma' CovenantControls_{i,t-1} \\ &+ Industry_j + Year_t + \varepsilon_{i,t},\end{aligned} \tag{4}$$

where i indexes firm, j indexes industry, and t indexes time. The dependent variable ($\Delta Q_{i,t-1\to t+3}$) is firm i's Tobin's Q three year after the violation year minus its Tobin's Q one year before the violation year (i.e., $Q_{i,t+3}$ - $Q_{i,t-1}$). Tobin's Q is computed as the sum of market value of equity and book value of debt, divided by book value of total assets. There are three main independent variables in this analysis. $Violator_i$ is a dummy that equals one for violating firms and zero for matched non-violating firms. We measure the impact of creditor interventions on firm innovation output, $\Delta Pat_{i,t-3\to t+3}$, as the change in the firm's total number of patents filed over three years after and before the covenant violation year, i.e., $\sum_{n=1}^{3} Pat_{i,t+n} - \sum_{n=1}^{3} Pat_{i,t-n}$. The key variable of interest is the interaction term, $Violator_{i,t}*\Delta Pat_{i,t-3\to t+3}$, which captures the differential effect of the change in innovation output on the change in Tobin's Q between violating and non-violating firms.

Following Nini, Smith, and Sufi (2012), we include a set of covenant control variables ($CovenantControls$) in the regressions to account for their effect on the change in Tobin's Q. These variables include five most common ratios used in financial covenants: ROA (operating cash flow divided by lagged assets), the leverage ratio (book value of total debt divided by lagged assets), the interest-to-asset ratio (interest expenses divided by lagged assets), the ratio of net worth to asset, and the current ratio (current assets divided by current liabilities). All these variables are measured at the year right before the covenant violation year. In one specification, we also control for Tobin's *Q* measured at the year right before the covenant violation year to check the robustness of our results. Finally, $Industry_j$ and $Year_t$ represent industry and year fixed effects.

The variable of interest is the interaction term $Violator_{i,t}{*}\Delta Pat_{i,t\text{-}3\rightarrow t+3}$. If creditor intervention corrects the misaligned interest between shareholders and managers through curbing excessive investments in innovation, then we expect that reduction in patents upon covenant violations is associated with an increase in firm value, i.e., a negative and significant coefficient on the interaction term. In contrast, if creditor interference intensifies conflicts of interest between shareholders and debt holders through curbing investments in value-enhancing innovative projects, then we expect that reduction in patents upon covenant violations is associated with a decrease in firm value, i.e., a positive and significant coefficient on the interaction term. We report the results estimating equation (4) in Table 5 Panel A. We exclude lagged Tobin's *Q* in column (1) and control for lagged Tobin's *Q* in column (2).

The coefficient estimates of $Violator_{i,t}{*}\Delta Pat_{i,t\text{-}3\rightarrow t+3}$ are negative and significant at the 5% level in both columns. This finding suggests that, for violating firms, the change in patent counts around debt covenant violations are significantly negatively related to the subsequent change in firm value. Given that we have documented creditor interventions lead to a significant reduction in the number of patents firms generate post-violation, our results suggest that creditors' curbing innovation output upon covenant violations enhances firm value. This finding is consistent with the argument that creditor interventions mitigate the conflicts of interest between managers and shareholders, thereby correct managers' over-investments in innovation that are value-destroying.

To understand the reason why a reduction in the number of patents post-intervention leads to an increase in firm value, we postulate that creditor interventions help firms adjust their innovation focus and scope and push them to cut tangent patents that are unrelated to their main business. Prior studies have documented that improvements in corporate focus resulted from spinoffs and asset sales are associated with better operating performance and greater shareholder value (e.g., John and Ofek, 1995; Daley, Mehrotra, and Sivakumar, 1997). Innovation activities that are not related to a firm's main business are likely to be out of managers' expertise and hence are likely value-destroying. Managers who pursue such innovation activities are probably for their own private benefits rather than enhancing firm value. Creditor interventions upon covenant violations mitigate misaligned incentives between managers and shareholders and thus curtails investments in such innovation activities. In contrast, creditors preserve innovation activities that are related to the firm's main business, which should enhance firm value.

We test this conjecture by first classifying a firm's patents into two groups: patents that are related to a firm's main business (labelled as related patents) and patents that are unrelated to a firm's main business (labelled as unrelated patents), Specifically, we define the patents that are in a firm's main two-digit SIC industry as related patents, and the patents that are not in a firm's main two-digit SIC industry as unrelated patents. A practical difficulty, however, is that the USPTO database does not assign a patent's industry membership in the SIC framework. Instead, the USPTO adopts a patent classification system that assigns patents to three-digit technology classes that are based on technology categorization instead of final product categorization. We use a concordance table that connects most USPTO technology classes to two-digit SIC codes constructed in Hsu, Tian, and Xu (2014) to map patents in each technology class to one or multiple two-digit SIC codes. We then compute the number of related patents in a firm's main two-digit SIC industry by multiplying patent counts with the corresponding mapping weight. We calculate the number of unrelated patents by subtracting the number of related patents from the total number of patents a firm has in a year.[12]

Next, we investigate the changes in patent counts in each of these two groups of patents surrounding covenant violations in a DiD framework. We use the propensity-score-matched sample of treatment and control firms obtained from Section 4.2.1, and report the results in Table 6. Panel A reports the univariate DiD test results. Column (1) shows the average change in the number of unrelated and related patents surrounding creditor interventions for treatment firms. The changes are computed by first subtracting the total number of unrelated (related) patents counted over the three-year period immediately preceding the covenant violation from the

[12] For example, 63% of USPTO technology class 1 is mapped to two-digit SIC industry 35, 32% of technology class 1 is mapped to two-digit SIC industry 36, and 5% of technology class 1 is mapped to two-digit SIC industry 37. USPTO technology class 7 is mapped to ten two-digit SIC industries, with 13% of patents mapped to two-digit SIC industry 35. Suppose that a firm's main two-digit SIC code is 35, and it has 3 and 5 patents in USPTO technology class 1 and 7, respectively. Then the number of patents that is related to this firm's main business is calculated as 3*63%+5*13% = 2.54, and the number of patents that are not in its main business is 5.46 (= 3 + 5 – 2.54).

number of unrelated (related) patents counted over the three-year period immediately post the violation for each of the treatment firms. The differences are then averaged over the treatment group. By the same token, we compute the average changes in the number of unrelated (related) patents for the control group and report them in column (2). In columns (3) and (4), we present the DiD estimators and the corresponding p-values of the two-tailed *t*-statistics testing the null hypothesis that the DiD estimators are zero.

The DiD estimator for the number of unrelated patents is negative and significant at the 1% level. On average, bank interventions after covenant violations result in a reduction in about 1.3 more unrelated patents in the three-year period immediately post the violation relative to the three-year immediately preceding the volition for the treatment firms than for the control firms. The DiD estimators corresponds to approximately a drop of 1.3/3 = 0.4 more patents per year, which is 25% of 1.7 patents, the sample average number of patents unrelated to main business granted per year. In contrast, the DiD estimator for the number of related patents is negative but statistically insignificant, suggesting that there is not a significant difference between the treatment and the control firms in terms of the change in related patent counts surrounding creditor interventions.

We next investigate the dynamics of unrelated and related patents surrounding creditor interventions in a regression framework. We employ the propensity-score-matched sample of treatment and control firms and retain firm-year observations for both treatment and control firms for a seven-year window centered in the violation year. We estimate the following regression model separately for the number of unrelated patents and related patents a firm has:

$$\begin{aligned} UnrelatedPat\ (RelatedPat) = {} & \alpha + \beta_1 Violator * Before^{-1} + \beta_2 Violator * Current + \beta_3 Violator * After^{1} \\ & + \beta_4 Violator * After^{2\&3} + \beta_5 Before^{-1} + \beta_6 Current + \beta_7 After^{1} + \beta_8 After^{2\&3} + \beta_9 Violator + \varepsilon. \end{aligned} \quad (5)$$

where we define all independent variables the same way as we do for equation (2).

We report the results estimating equation (5) in Panel B of Table 6. The dependent variables in column (1) and (2) are a firm's number of unrelated patents and related patents in a year, respectively. In column (1), we find statistically insignificant coefficient estimates of β_1, and β_2, suggesting that there is not a pre-existing trend in firm innovation output that is unrelated to its main business. The coefficient estimates of β_3 and β_4, however, are negative and significant at the 5% and 1% level, respectively. This finding suggests that compared to the control firms, the treatment firms have a significant reduction in the number of unrelated patent starting from the 1st year following the creditor intervention. In column (2), we find that none of the coefficients estimates of the interaction terms is statistically significant, which suggests that creditor interventions do not cause any significant changes in innovation output that is related to a firms' main business. These findings are consistent with our evidence reported in Panel A and lend further support to our conjecture that creditor interventions curb firms' excessive investments in innovation projects that are unrelated to their main business, which allows firms to focus more on innovation activities within their expertise.

Next, we link this finding that firms refocus their innovation scope after creditor interventions back to our firm value analysis reported in Table 5. Specifically, we replace $\Delta Pat_{i,t-3\rightarrow t+3}$ that captures the change in patent counts with $\Delta UnrelatedPat_{i,t-3\rightarrow t+3}$ that measures the change in a firm's number of unrelated patents surrounding creditor interventions. We then re-estimate equation (5). The variable of interest is the interaction term $Violator_{i,t}*\Delta Unrelated\ Pat_{i,t-3\rightarrow t+3}$. We report the regression results in Table 5 Panel B. The coefficient estimates of the interaction term are negative and significant at the 5% level in both specifications, suggesting that a reduction in a firm's number of unrelated patents around creditor intervention leads to an increase in firm value. In an untabulated analysis, we replace $\Delta UnrelatedPat_{i,t-3\rightarrow t+3}$ with $\Delta RelatedPat_{i,t-3\rightarrow t+3}$ that measures the change in a firm's number of related patents surrounding creditor interventions in the regressions. We find that the coefficient estimate of $Violator_{i,t}*\Delta Related\ Pat_{i,t-3\rightarrow t+3}$ is statistically insignificant, suggesting that the change in related patent counts surrounding creditor interventions does not contribute to firm value.

5.2. Human capital redeployment

Our findings from Section 5.1 appear to suggest that covenant violating firms refocus their innovation scopes after creditor interventions, which enhances their firm value. In this subsection, we explore a human capital redeployment mechanism through which bank interventions curb excessive innovative projects that are unrelated to a firm's main business. Because human capital is a key input of innovation, we postulate that the intervention of creditors pushes firms to refocus on innovative projects within their expertise through layoffs of inventors whose skill sets are unrelated to their main business, and hiring inventors whose skill sets are related to their main business. We also conjecture that creditors encourage inventors who stay within the firm to develop skills and produce more related patents after their interventions.

To investigate this possible channel, we use the propensity-score-matched sample of treatment and control firms. For each matched pair, we restrict our sample to a window of three years before and three years after the covenant violation for both the treatment and matched control firms. We follow Bernstein (2012) and identify three groups of inventors.[13] The first group of inventors is "leavers": the inventors who produce at least one patent in a treatment (or control) firm before the violation and at least one patent in a different firm after the violation. The second group of inventors is "stayers": the inventors who produce at least one patent in a treatment (or control) firm both before and after the violation. The third group of inventors is "new hires": the inventors who produce at least one patent after but none before the violation in a treatment (or control) firm, and produce at least one patent in a different firm before the violation.

If human capital redeployment is a mechanism through which creditor interventions curtail excessive innovation that are unrelated to a firms' main business, we expect to observe that "leavers" of treatment firms are more likely to specialize on areas that are unrelated to the firm's main business and hence generate more unrelated patents than those of the control firms before they leave the firm. Meanwhile, when firms recruit new talents, the treatment firms are more likely to hire inventors who have a track record of producing patents that are related to their main business compared. Regarding stayers of the treatment firms who generate patents both before and after the covenant violation, we expect them to focus more on areas that are related to the firms' main business and hence generate more related patents after the creditor interventions compared to the stayers of the control firms. We report the results testing these conjectures in Table 7.

In Panel A, we present the univariate comparison for the differences in the percentage of unrelated patents produced by leavers and new hires three years prior to the covenant violation between the treatment and control groups. In particular, we first divide the total number of an inventor's unrelated patents generated over the three-year period preceding covenant violation by his total number of patents during the same period. We then take an average of this percentage in

[13] The HBS inventor database provides information for both inventors (the persons who are producing innovation) and assignees (the entity that owns the patent, which could be a government, a firm, or an individual). It provides a unique identifier for each inventor. Hence, we are able to track the mobility of individual inventors. See Lai et al. (2013) for details.

the treatment group and report the results in column (1).[14] By the same token, we compute the average percentage of unrelated patents of inventors in the control group and report the results in column (2). The difference in the mean of the treatment and control group is reported in column (3). We report the p-values of the two-tailed *t*-statistics testing the null hypothesis that the mean differences are zero in column (4).

We first compare leavers of these two groups of firms. Leavers of the treatment firms have a significantly higher fraction of unrelated patents compared with those of the control firms before the covenant violation. This finding suggests that firms actively force out inventors whose expertise is different from their main business after bank interventions. For new hires, we find that the percentage of unrelated patents produced by new hires of the treatment firms is significantly lower than that produced by new hires of the control firms. This finding suggests that covenant violating firms are actively seeking new talents who have track records of producing patents that are related to their own main business.[15]

In Panel B, we report the results for stayers. Because stayers remain in the firms, we are able to observe their patent generation both before and after the covenant violation, which allows us to estimate the treatment effect of creditor interventions in the DiD framework. We compute the DiD estimator by first subtracting the percentage of unrelated patents generated over the three-year period preceding the violation from the percentage of unrelated patents generated over three three-year period after the violation for each stayer of the treatment firms. The difference is then averaged over all stayers in the treatment group and reported in column (1). We repeat the same procedure for stayers of the control firms and report the average changes in the percentage of unrelated patents surrounding the covenant violation year in column (2). The DiD estimator is simply the difference in the differences for the treatment and the control firms, and we report it in column (3). We report the p-values of the DiD estimators in column (4). The DiD estimator is negative and significant at the 5% level, suggesting that, compared with the stayers of the control

[14] Note that for new hires, even though they are working for firms other than the violating firms over this period, we are still able to calculate this measure for them because we are able to observe their patents' as well as the violating firms' industry classifications.

[15] One may notice that the percentages of unrelated patents produced by individual inventors are quite high (> 50% on average). This is due to the mapping between NBER technology classes and two-digit SIC industry classifications. According to the matching algorithm of Hsu, Tian, and Xu (2014), each NBER technology class is on average mapped to about 20 two-digit SIC industries with a probability distribution, which suggests that patent classes are very diversely corresponding to the SIC codes.

firm, stayers of the treatment firms generate patents that are more related to the firms' main business after the creditor intervention.

Next, we explore this question from a different angle by examining the percentage of inventors whose expertise is different from the firms' main business. We first define an inventor as an unrelated inventor if most of the inventor's patents produced over a 10-year period prior to the covenant violation are in a two-digit SIC industry other than the firm's main two-digit SIC industry. We then compare the differences in the percentage of unrelated inventors for leavers and new hires between the treatment and control groups.[16] We report the results in Panel C.

The percentage of unrelated inventors is computed as the following: for each type of inventors, we first divide the total number of unrelated inventors by the total number of inventors in each firm in the treatment group**.** We then compute the average percentage of unrelated inventors across all firms in the treatment group and report it in column (1). By the same token, we compute the average percentage of unrelated inventors for each of the three types of inventors in the control group and report it in column (2). In columns (3) and (4), we present the difference in the percentage of unrelated inventors between the treatment and control group and the corresponding *p-value* testing the null hypothesis that the differences are zero, respectively.

We find that the percentage of unrelated leavers is significantly higher in violating firms than in matched control firms, which suggests that, compared with non-violating firms, violating firms are more likely to layoff inventors whose expertise is different from their own main business after bank interventions. In contrast, we find a significantly lower fraction of unrelated new hires in violating firms than in non-violating firms, suggesting that firms are less likely to hire inventors whose expertise is different from their own main business.

In summary, our evidence reported in this subsection is consistent with the conjecture that upon a covenant violation, creditors help the firm refocus its innovation scope by getting rid of inventors who generate patents that are unrelated to the firm's main business and by hiring inventors whose expertise is in the firm's main business. Creditors also seem to force inventors who stay in the firm to produce patents that are more related to the firms' main business after the violation. Overall, human capital redeployment within a firm appears a plausible underlying

[16] Note that this test does not apply to stayers because the way we define unrelated inventors. Hence, we do not report the results for stayers.

mechanism through which creditors help violating firms refocus on its innovation scope, which ultimately enhances firm value.

6. Conclusion

In this paper, we examine the effect of creditor interventions triggered by debt covenant violations. Our baseline results show that creditor interventions are negatively related to both innovation quantity and quality. We use a DiD approach and a regression discontinuity design to establish causality and find a negative, causal effect of bank interventions on firm innovation. We further show that the reduction in innovation output is concentrated in innovation projects that are unrelated to a firm's main business, which leads to a more focused scope of innovation output and ultimately an increase in firm value. Human capital redeployment appears a plausible underlying mechanism through which creditor interventions refocus firm innovation scope and enhance firm value. Our findings are consistent with the argument that creditors help mitigate investment distortions in innovation arising from conflicts of interest between managers and shareholders. Our paper sheds new light on the real effect of creditor interventions.

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Table 1: Summary statistics

This table presents summary statistics for variables constructed based on the sample of U.S. non-financial firms from 1996 to 2008. We include all Compustat firms that violate or do not violate debt covenants in Amir Sufi's debt covenant violations dataset that is available at http://faculty.chicagobooth.edu/amir.sufi/. All variables are defined in Appendix.

Variable	N	Mean	S.D.	P25	Median	P75
Pat	60,954	3.44	13.76	0	0	0
Cite	60,954	3.75	10.72	0	0	0
Violation	60,954	0.13	0.34	0	0	0
Ln(MV)	60,954	4.73	2.41	3.04	4.74	6.43
R&D_Assets	60,954	0.08	0.18	0	0	0.08
ROA	60,954	-0.14	0.92	-0.08	0.09	0.15
PPE_Assets	60,954	0.26	0.23	0.08	0.18	0.38
Leverage	60,954	0.30	0.55	0.02	0.18	0.38
CAPEX_Assets	60,954	0.06	0.07	0.02	0.04	0.07
HI	60,954	0.22	0.17	0.10	0.17	0.28
HI^2	60,954	0.08	0.14	0.01	0.03	0.08
Tobin's Q	60,954	3.39	7.59	1.12	1.61	2.78
KZ_Index	60,954	-9.42	48.09	-5.40	-0.40	1.44
Ln(Age)	60,954	2.46	0.81	1.95	2.40	3.04
INST	60,954	0.26	0.31	0	0.12	0.49
Debt_EBITDA	60,954	1.20	5.95	0	0.35	2.48
Networth_Assets	60,954	0.29	1.32	0.30	0.50	0.72
Current_Ratio	60,954	2.91	3.47	1.13	1.89	3.25

Table 2: Baseline regression of innovation outcomes on covenant violations

This table reports the OLS results estimating the effect of debt covenant violations on innovation output variables. We estimate the pooled OLS regression of the following model:

$$LnPat_{i,t+n}(LnCite_{i,t+n}) = \alpha + \beta Violation_{i,t} + \gamma' Controls_{i,t} + Year_t + Industry_j + \varepsilon_{i,t},$$

using a sample of all U.S. and non-financial Compustat firms during 1996 to 2008 that violate or do not violate debt covenants from Amir Sufi's debt covenant violations dataset. The dependent variable $LnPat_{i,t+n}$ is the natural logarithm of one plus total number of patents filed (and eventually granted) in one (t+1), two (t+2), and three (t+3) years, and results are reported in columns (1) – (3), respectively. The dependent variable $LnCitet_{i,t+n}$ is the natural logarithm of one plus the number of citations received per patent in one (t+1), two (t+2), and three (t+3) years, and results are reported in columns (4) – (6), respectively. $Violation_{i,t}$ is a dummy variable that equals one if covenant violation occurs in year *t* for firm *i* and not preceded by a violation in the previous year (*t-1*), and zero otherwise. Year fixed effects $Year_t$ and industry fixed effects $Industry_j$ are included in all regressions. All other variables are as defined in Appendix. P-values based on standard errors clustered by firm are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	$LnPat_{i,t+1}$	$LnPat_{i,t+2}$	$LnPat_{i,t+3}$	$LnCite_{i,t+1}$	$LnCite_{i,t+2}$	$LnCite_{i,t+3}$
Violation	-0.023**	-0.030**	-0.029**	-0.039***	-0.026*	-0.033**
	(0.039)	(0.012)	(0.027)	(0.003)	(0.066)	(0.019)
Ln(MV)	0.174***	0.178***	0.180***	0.128***	0.123***	0.115***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
R&D_Assets	0.317***	0.317***	0.306***	0.417***	0.409***	0.358***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
ROA	-0.008	-0.009	-0.007	0.024***	0.023***	0.022**
	(0.306)	(0.276)	(0.461)	(0.003)	(0.009)	(0.012)
PPE_Assets	-0.155***	-0.151***	-0.150***	-0.164***	-0.145***	-0.115***
	(<0.001)	(<0.001)	(0.001)	(<0.001)	(<0.001)	(0.001)
Leverage	-0.015	-0.018	-0.018	-0.030**	-0.040***	-0.039**
	(0.299)	(0.256)	(0.313)	(0.024)	(0.005)	(0.011)
CAPEX_Assets	0.021	0.060	0.086	0.231***	0.282***	0.236***
	(0.776)	(0.461)	(0.340)	(0.003)	(<0.001)	(0.003)
HI	-0.390**	-0.338*	-0.318*	-0.127	-0.035	-0.058
	(0.019)	(0.057)	(0.095)	(0.327)	(0.788)	(0.666)
HI^2	0.628***	0.569***	0.555**	0.292**	0.172	0.182
	(0.002)	(0.009)	(0.019)	(0.048)	(0.259)	(0.247)
Tobin's Q	-0.006***	-0.005***	-0.004***	0.001	0.000	0.001
	(<0.001)	(<0.001)	(<0.001)	(0.518)	(0.661)	(0.176)
KZ_Index	0.001***	0.001***	0.001***	0.000***	0.000***	0.000***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(0.002)	(0.007)
Ln(Age)	0.119***	0.112***	0.108***	0.043***	0.036***	0.035***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(0.001)

INST	0.096**	0.113**	0.133***	0.136***	0.135***	0.147***
	(0.022)	(0.013)	(0.007)	(<0.001)	(<0.001)	(<0.001)
Debt_EBITDA	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Networth_Assets	-0.054***	-0.055***	-0.055***	-0.028***	-0.036***	-0.033***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Current_Ratio	0.012***	0.011***	0.010***	0.021***	0.018***	0.015***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Constant	-0.569***	-0.584***	-0.583***	-0.048	-0.041	-0.007
	(<0.001)	(<0.001)	(<0.001)	(0.184)	(0.264)	(0.852)
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	60,954	51,865	43,641	60,954	51,865	43,641
Adjusted R^2	0.292	0.295	0.298	0.219	0.217	0.213

Table 3: Difference-in-differences (DiD) test results

This table reports diagnostic tests and the DiD results on how violations of debt covenants affect firm innovation. Sample selection begins with Amir Sufi's debt covenant violations dataset that is available at http://faculty.chicagobooth.edu/amir.sufi/. Our sample contains all Compustat firms that violate or do not violate debt covenants and have non-missing matching variables and non-missing innovation outcome variables in the pre-violation year (*t-1*) and the post-violation year (*t+1*), with *t* being the fiscal year during which new covenant violation occurred for firm *i*. New violations are defined to be violations where the firm has not violated a financial covenant in the previous four quarters. We match firms using a one-to-one nearest neighbor propensity score matching, with replacement, on a host of observable characteristics including all independent variables used to estimate equation (1) (see Table 2), growth in innovation variables (i.e., the growth in the number of patents *Pat_growth* and the growth in the number of citations per patent *Cite_growth*, both computed over the three-year period before covenant violations), Fama-French 12-industry dummies, and year fixed effects. Definitions of all other variables are listed in Appendix. Our treatment group contains firms that breach debt covenants and not preceded by a violation in the previous year (*t-1*). Our control group includes non-violating firm-year observations and not preceded by a violation in the previous year (t-1). To eliminate the selection bias between firms with and without loan contracts, we also require the control group to have bank loans outstanding. Panel A reports parameter estimates from the probit model used in estimating the propensity scores for the treatment and control groups. The dependent variable equals one for the firm-year belonging to the treatment group and zero for those belonging to the control group. The "Pre-Match" column contains the parameter estimates of the probit model estimated using the sample prior to matching. These estimates are then used to generate the propensity scores for matching. The "Post-Match" column contains the parameter estimates of the probit model estimated using the subsample of matched treatment-control pairs after matching. Fama-French 12-industry fixed effects and year fixed effects are included in both columns of Panel A. P-values based on standard errors clustered by firm are displayed in parentheses below each coefficient estimate. Panel B reports the distribution of estimated propensity scores for the treatment firms, control firms, and the difference in estimated propensity scores post matching. Panel C presents the univariate comparisons between the treatment and control firms' characteristics and their corresponding *t*-statistics. Panel D gives the DiD test results. *PAT* is the sum of firm *i*'s number of patents in the three-year window before or after covenant violations. *CITE* is the sum of firm *i*'s number of citations per patent in the three-year window before or after covenant breaches. Panel E reports the regression results that estimate the innovation dynamics of treatment and control firms surrounding covenant violations. We estimate the following model:

$$LnPat\ (LnCite\ ) = \alpha + \beta_1 Violator * Before^{-1} + \beta_2 Violator * Current + \beta_3 Violator * After^{1}$$
$$+ \beta_4 Violator * After^{2\&3} + \beta_5 Before^{-1} + \beta_6 Current + \beta_7 After^{1} + \beta_8 After^{2\&3} + \beta_9 Violator + \varepsilon.$$

The dependent variable is either *LnPat*, the natural logarithm of one plus firm *i*'s number of patents in a given year, or *LnCite*, the natural logarithm of one plus firm *i*'s number of citations per patent in a given year. *Violator* is a dummy that equals one for treatment firms and zero for control firms. $Before^{-1}$ is a dummy that equals one if a firm-year observation is from the year immediately before the covenant violation (year -1) and zero otherwise. *Current* is a dummy that equals one if a firm-year observation is in the violation year (year 0) and zero otherwise. $After^{1}$ is a dummy that equals one if a firm-year observation is from the year immediately after the violation (year 1) and zero otherwise. $After^{2\&3}$ is a dummy that equals one if a firm-year observation is from two or three years after the violation (year 2 and 3) and zero otherwise. P-values based on robust standard errors are displayed in parentheses below each coefficient estimate. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Pre-match propensity score regression and post-match diagnostic regression

	(1) Pre-match	(2) Post-Match
$Ln(MV)_{t-1}$	-0.215***	0.001
	(<0.001)	(0.903)
$R\&D\_Assets_{t-1}$	-0.197	-0.249
	(0.169)	(0.111)
ROA_{t-1}	-0.236***	-0.052
	(0.001)	(0.391)
PPE_Assets_{t-1}	-0.502***	-0.107
	(<0.001)	(0.267)
$Leverage_{t-1}$	0.212**	0.091
	(0.020)	(0.264)
$CAPEX\_Assets_{t-1}$	1.263***	0.145
	(<0.001)	(0.578)
HI_{t-1}	-0.051	0.071
	(0.798)	(0.796)
HI^2_{t-1}	-0.018	-0.200
	(0.942)	(0.558)
$Tobin's\ Q_{t-1}$	0.001	0.003
	(0.831)	(0.646)
KZ_Index_{t-1}	-0.000	0.000
	(0.618)	(0.885)
$Ln(Age)_{t-1}$	-0.088***	-0.018
	(<0.001)	(0.413)
$INST_{t-1}$	-0.329***	0.033
	(<0.001)	(0.624)
$Debt\_EBITDA_{t-1}$	0.004*	-0.000
	(0.072)	(0.888)
$Networth\_Assets_{t-1}$	0.254***	0.081
	(<0.001)	(0.174)
$Current\_Ratio_{t-1}$	-0.033***	-0.002
	(<0.001)	(0.840)
Pat_growth_{t-1}	0.003	0.003
	(0.476)	(0.667)
$Cite\_growth_{t-1}$	-0.002*	-0.002
	(0.053)	(0.250)
Constant	0.557***	-0.027
	(<0.001)	(0.818)

Year fixed effect	Yes	Yes
Industry fixed effect	Yes	Yes
Observations	22,473	7,208
P-value of Chi-square	<0.001	0.999
Pseudo R^2	0.147	0.002

Panel B: Estimated propensity score distributions

Propensity Scores	No. of Obs	Min	P5	Median	Mean	S.D.	P95	Max
Treatment	3,604	0.001	0.059	0.255	0.274	0.154	0.561	0.846
Control	3,604	0.007	0.059	0.256	0.274	0.153	0.561	0.921
Difference	-	0.000	0.000	0.000	0.001	0.004	0.003	0.092

Panel C: Post-Match differences

Variable	Treatment	Control	Differences	T-statistics
$Ln(MV)_t$	4.406	4.382	0.024	1.123
$R\&D\_Assets_t$	0.048	0.051	-0.004	-1.254
ROA_t	0.024	0.024	0.001	0.088
PPE_Assets_t	0.274	0.277	-0.004	-0.662
$Leverage_t$	0.299	0.299	-0.001	-0.087
$CAPEX\_Assets_t$	0.068	0.067	0.001	0.417
HI_t	0.221	0.224	-0.002	-0.556
HI^2_t	0.078	0.080	-0.003	-0.809
$Tobin's\ Q_t$	2.004	1.992	0.012	0.141
KZ_Index_t	-6.132	-6.170	0.038	0.045
$Ln(Age)_t$	2.392	2.410	-0.018	-1.091
$INST_t$	0.213	0.209	0.004	0.864
$Debt\_EBITDA_t$	2.038	2.056	-0.018	-0.116
$Networth\_Assets_t$	0.413	0.400	0.013	0.953
$Current\_Ratio_t$	2.315	2.294	0.021	0.450
Pat_growth_t	0.076	0.056	0.020	0.340
$Cite\_growth_t$	-0.795	-0.558	-0.237	-1.114

Panel D: DiD estimators

	(1) Mean Treatment Difference (after-before)	(2) Mean Control Difference (after-before)	(3) Mean DiD estimator (treat-control)	(4) T-statistics for DiD estimator
Pat	-3.326	-1.520	-1.806***	0.001
(s.e.)	(0.430)	(0.331)	(0.540)	
Cite	-7.522	-6.227	-1.295*	0.069
(s.e.)	(0.535)	(0.489)	(1.007)	

Panel E: DiD analysis for innovation dynamics

Dependent Variable	(1) *LnPat*	(2) *LnCite*
*Violator*Before*$^{-1}$	0.001	-0.018
	(0.929)	(0.392)
*Violator*Current*	-0.005	-0.030
	(0.584)	(0.147)
*Violator*After*^{1}	-0.012	-0.019
	(0.266)	(0.365)
*Violator*After*$^{2\&3}$	-0.041***	-0.034*
	(<0.001)	(0.080)
Before^{-1}	-0.005	-0.007
	(0.492)	(0.654)
Current	-0.018**	-0.009
	(0.016)	(0.558)
After^{1}	-0.026***	-0.045***
	(0.001)	(0.005)
After$^{2\&3}$	-0.001	-0.023
	(0.928)	(0.140)
Violator	0.021**	0.015
	(0.013)	(0.352)
Constant	0.306***	0.574***
	(<0.001)	(<0.001)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	40,922	40,922
Adjusted R^2	0.832	0.591

Table 4: Regression discontinuity results

This table reports the results estimating the effect of covenant violations on innovation output using the RDD. The sample consists of all firm-year observations in which a covenant restricting the current ratio or net worth of the firm is imposed by a private loan found in Dealscan during 1996 to 2008. We follow Chava and Roberts (2008) and restrict the sample to observations that satisfy the following requirements: (1) they must be non-financial firms that exist in both merged CRSP-Compustat database and the Dealscan database; and (2) they are firms that have had a loan contract containing a debt covenant restricting its current ratio or net worth to stay above a certain threshold. The dependent variable in columns (1) – (3), $LnPat_{i,t+n}$, is the natural logarithm of one plus patent counts, in one, two, and three years after breach of covenants. In columns (4) – (6), the dependent variable $LnCite_{i,t+n}$, is the natural logarithm of one plus citation counts scaled by patents, in one, two, and three years after breach of covenants. See Appendix for a detailed explanation of the variables. *Violation* is a dummy variable that equal to one if a firm's current ratio or net worth falls below the corresponding covenant threshold in any of the four quarters in a fiscal year. Polynomial (2) represents polynomials of order two of distant to default with respect to current ratio and net worth covenants. Distant to default is the absolute difference between current ratio or net worth and the corresponding covenant threshold. If a firm does not violate covenants, we include in the regressions the average of the distant to default in all four quarters as polynomials. If a firm violates covenant in a particular quarter, we use the distant to default in the violating quarter as polynomials. However, if a firm violates covenant in more than one quarter in a fiscal year, we use the maximum distant to default as polynomials. Panel A reports the results from estimating a polynomial model specified in equation (3) using the entire *Dealscan* sample. Panel B presents the results for the discontinuity in the narrow band around the threshold *Dealscan* sample, defined as those firm-year observations in which the absolute value of the relative distance to the covenant threshold is less than 0.20. All specifications include year-fixed and Fama-French 12-industry fixed effects. P-values based on standard errors clustered by firm are displayed in parentheses below each coefficient estimate. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Entire Dealscan sample with polynomials

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	$LnPat_{i,t+1}$	$LnPat_{i,t+2}$	$LnPat_{i,t+3}$	$LnCite_{i,t+1}$	$LnCite_{i,t+2}$	$LnCite_{i,t+3}$
Violation (Net worth	-0.131***	-0.156***	-0.159***	-0.084*	-0.089*	-0.094*
or current ratio)	(0.001)	(<0.001)	(<0.001)	(0.058)	(0.065)	(0.052)
Constant	0.417***	0.411***	0.424***	0.628***	0.618***	0.606***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Polynomial (2)	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,028	7,095	6,217	8,028	7,095	6,217
Adjusted R^2	0.129	0.126	0.125	0.152	0.151	0.154

Panel B: Narrow bands around threshold Dealscan sample

Dependent Variable	(1) $LnPat_{i,t+1}$	(2) $LnPat_{i,t+2}$	(3) $LnPat_{i,t+3}$	(4) $LnCite_{i,t+1}$	(5) $LnCite_{i,t+2}$	(6) $LnCite_{i,t+3}$
Violation (Net worth or current ratio)	-0.183***	-0.217***	-0.221***	-0.177***	-0.174***	-0.199***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Constant	0.425***	0.433***	0.429***	0.623***	0.616***	0.585***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,069	2,746	2,414	3,069	2,746	2,414
Adjusted R^2	0.152	0.146	0.150	0.166	0.159	0.168

Table 5: The effect of changes in innovation upon covenant violation on firm value

This table reports the results from the regressions estimating the effect of changes in patent counts upon covenant violation on firm value. We use the propensity-score-matched sample of violating and non-violating firms and retain firm-year observations for both treatment and control firms for a seven-year window centered in the violation year. The dependent variable is $\Delta Q_{i,t+3}$, which is a firm's Tobin's Q three years after violation minus its Tobin's Q one year before the violation. $Violator_i$ is a dummy that equals one for violating firms and zero for non-violating firms. In Panel A, we measure the impact of creditor interference on firm innovation using the change in the number of patents during three years after and three years before covenant violation, $\Delta Pat_{i,t\text{-}3ttur}$. In Panel B, we measure the impact of creditor interference on firm innovation using the change in the number of unrelated patents during three years after and three years before covenant violation, $\Delta UnrelatedPat_{i,t\text{-}3tela}$. ROA is operating cash flow divided by lagged assets; the leverage ratio is book value of total debt divided by lagged assets; the interest-to-asset ratio is interest expenses divided by lagged assets; net worth/asset is assets minus liabilities, then divided by assets, the current ratio is current assets divided by current liabilities, and Tobin's Q is computed as the sum of market value of equity and book value of debt, then divided by book value of total assets. All these variables are lagged by one year. We exclude lagged Tobin's Q in column (1) and control for lagged Tobin's Q in column (2). Both year fixed effects *Year* and industry fixed effects *Industry* are included in all regressions. P-values based on robust standard errors are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Panel A All patent cuts		**Panel B Unrelated patent cuts**	
	(1)	(2)	(1)	(2)
Dependent Variable	$\Delta Q_{i,t+3}$	$\Delta Q_{i,t+3}$	$\Delta Q_{i,t+3}$	$\Delta Q_{i,t+3}$
$\Delta Pat_{i,t-3\rightarrow t+3}$	0.005	0.011**		
	(0.418)	(0.014)		
$Violator_{i,t}$*$\Delta Pat_{i,t-3\rightarrow t+3}$	-0.019**	-0.013**		
	(0.014)	(0.030)		
$\Delta Unrelated\ Pat_{i,t-3\rightarrow t+3}$			0.004	0.013**
			(0.581)	(0.018)
$Violator_{i,t}$*$\Delta Unrelated\ Pat_{i,t-3\rightarrow t+3}$			-0.023**	-0.017**
			(0.029)	(0.030)
$Violator_{i,t}$	0.053	-0.062	0.055	-0.062
	(0.627)	(0.515)	(0.616)	(0.516)
ROA_{t-1}	0.669	-1.135**	0.668	-1.135**
	(0.255)	(0.034)	(0.256)	(0.034)
$Leverage_{t-1}$	1.088	-0.054	1.089	-0.055
	(0.265)	(0.959)	(0.265)	(0.958)
$Interest\text{-}to\text{-}asset\ ratio_{t-1}$	11.793	12.625*	11.782	12.631*
	(0.167)	(0.095)	(0.168)	(0.095)
$Net\ worth_{t-1}$	1.520***	0.115	1.519***	0.115
	(<0.001)	(0.765)	(<0.001)	(0.765)

Current ratio$_{t-1}$	-0.132***	-0.060**	-0.132***	-0.060**
	(0.004)	(0.049)	(0.004)	(0.049)
Q_{t-1}		-0.565***		-0.565***
		(<0.001)		(<0.001)
Year fixed effect	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes
Observations	2,916	2,916	2,916	2,916
Adjusted R^2	0.091	0.269	0.091	0.269

Table 6: Changes in patent counts unrelated and related to main business: DiD tests

This table reports the DiD results on how violations of debt covenants affect firm innovation in industries unrelated and related to firm's main business. We employ the propensity-score-matched sample of treatment and control firms and retain firm-year observations for both treatment and control firms for a seven-year window centered in the violation year. Panel A gives the DiD test results. The number of unrelated (related) patents is the sum of firm *i*'s number of patents that are unrelated (related) to its main business in the three-year window before or after covenant violations. Panel B reports the regression results that estimate the innovation dynamics of treatment and control firms surrounding covenant violations. We estimate the following model:

$$UnrelatedPat\ (RelatedPat) = \alpha + \beta_1 Violator * Before^{-1} + \beta_2 Violator * Current + \beta_3 Violator * After^{1}$$
$$+ \beta_4 Violator * After^{2\&3} + \beta_5 Before^{-1} + \beta_6 Current + \beta_7 After^{1} + \beta_8 After^{2\&3} + \beta_9 Violator + \varepsilon.$$

The dependent variable is either firm *i*'s number of patents unrelated to its main business in a given year, or firm *i*'s number of patents related to its main business in a given year. *Violator* is a dummy that equals one for treatment firms and zero for control firms. $Before^{-1}$ is a dummy that equals one if a firm-year observation is from the year immediately before the covenant violation (year -1) and zero otherwise. *Current* is a dummy that equals one if a firm-year observation is in the violation year (year 0) and zero otherwise. $After^{1}$ is a dummy that equals one if a firm-year observation is from the year immediately after the violation (year 1) and zero otherwise. $After^{2\&3}$ is a dummy that equals one if a firm-year observation is from two or three years after the violation (year 2 and 3) and zero otherwise. P-values based on robust standard errors are displayed in parentheses below each coefficient estimate. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: DiD estimators of patents unrelated and related to main business

	(1) Mean Treatment Difference (after-before)	(2) Mean Control Difference (after-before)	(3) Mean DiD estimator (treat-control)	(4) P-value for DiD Estimator
No. of unrelated patents	-0.990	0.310	-1.300***	0.006
(s.e.)	(0.334)	(0.346)	(0.472)	
No. of related patents	-0.315	-0.039	-0.276	0.192
(s.e.)	(0.154)	(0.147)	(0.211)	

Panel B: DiD analysis for innovation dynamics of unrelated and related patents

Dependent Variable	(1) No. of unrelated patents	(2) No. of related patents
*Violator*Before*$^{-1}$	-0.014	0.003
	(0.793)	(0.927)
*Violator*Current*	-0.099	-0.003
	(0.138)	(0.959)
*Violator*After*^{1}	-0.178**	-0.023
	(0.022)	(0.691)
*Violator*After*$^{2\&3}$	-0.285***	-0.099
	(0.005)	(0.160)
Before^{-1}	0.005	0.009
	(0.914)	(0.763)
Current	-0.003	-0.048
	(0.966)	(0.317)
After^{1}	0.010	-0.044
	(0.884)	(0.389)
After$^{2\&3}$	0.011	-0.035
	(0.881)	(0.502)
Violator	0.199**	-0.007
	(0.031)	(0.901)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Constant	0.749***	0.492***
	(<0.001)	(<0.001)
Observations	39,982	39,982
Adjusted R^2	0.855	0.884

Table 7: Innovation skills in unrelated industries and inventor turnover

This table reports the percentage of unrelated patents and the percentage of unrelated inventors of different types of inventors. We restrict the tests within the propensity-score-matched sample of treatment and control firms. For each matched pair, we restrict our sample to a window of three years before and after bank intervention for both treatment and matched control firms. We then identify three groups of inventors. 1) A "leaver" is an inventor who produces at least one patent in a treatment (or control) firm before the violation year and at least one patent in a different firm after the violation. 2) A "stayer" is an inventor who produces at least one patent in a treatment (or control) firm both before and after the violation year. 3) A "new hire" is an inventor who produces at least one patent after but not before the violation year in a treatment (or control) firm, and also produces at least one patent in a different firm before the violation. Panel A reports the percentage of patents unrelated to firms' main business for "leavers" and "new hires" of the treatment and the control group. For each inventor, we first divide the total number of his/her patents unrelated to a firm's main business over the three-year period preceding covenant violation by his/her total number of patents during the same period. Then we take an average of the percentages of all inventors of a particular type within the treatment group and report the results in column (1). By the same token, we compute the average percentage of unrelated patents for "leavers" and "new hires" in the control group and report them in column (2). In columns (3) and (4), we present the mean difference between treatment and control group and the corresponding *P-value* testing the null hypothesis that the differences are zero. Panel B reports the results of the DiD test for the change of the percentage of unrelated patents by "stayers." We compute the DiD estimator by first subtracting the percentage of unrelated patents generated over the three-year period preceding the violation from the percentage of unrelated patents generated over three three-year period after the violation for each of the stayers of the treatment firms. The difference is then averaged over all stayers in the treatment group and reported in column (1). We repeat the same procedure for control firms and report the average changes in the percentage of unrelated patents surrounding the covenant violation year in column (2). The DiD estimator is simply the difference in the differences for the treatment and the control firms, and we report it in column (3). We report the p-values of the DiD estimators in column (4). Panel C reports the percentage of inventors who are specialized in unrelated innovation in "leavers" and "new hires" in the treatment and control group. We define an inventor as an unrelated inventor if most of the inventor's patents produced over a 10-year period prior to the covenant violation are in a two-digit SIC industry other than the firm's main two-digit SIC industry. The percentage of unrelated inventors is computed as the following: for each type of inventors, we first divide the total number of unrelated inventors by the total number of inventors in each firm in the treatment (control) group**.** We then compute the average percentage of unrelated inventors across all firms in the treatment (control) group and report it in column 1 (2). In columns (3) and (4), we present the difference in the percentage of unrelated inventors between the treatment and control group and the corresponding *P-value* testing the null hypothesis that the differences are zero, respectively. In columns (3) and (4), we present the mean difference between treatment and control group and the corresponding *P-value* testing the null hypothesis that the differences are zero.

Panel A: Univariate test for the percentage of unrelated patents by leavers and new hires

	Treatment Mean (1)	Control Mean (2)	Mean Difference (treatment-control) (3)	P-Value (4)
Leavers				
% of unrelated patents	0.633	0.609	0.024***	<0.001
New Hires				
% of unrelated patents	0.679	0.695	-0.016**	0.020

Panel B: DiD test for the change of the percentage of unrelated patents by stayers

	Treatment Mean Change (after-before) (1)	Control Mean Change (after-before) (2)	Mean DiD estimator (treatment-control) (3)	P-Value (4)
Stayers				
% of unrelated patents	0.004	0.017	-0.014**	0.035
(s.e.)	(0.005)	(0.004)	(0.007)	

Panel C: Univariate test for the percentage of unrelated inventors

	Treatment Mean (1)	Control Mean (2)	Mean Difference (treatment-control) (3)	P-Value (4)
Leavers				
% of unrelated inventors	0.448	0.369	0.079***	<0.001
New Hires				
% of unrelated inventors	0.195	0.280	-0.086***	<0.001

Appendix: Variable definitions

Variables	Definition
Innovation Measures	
Pat	Total number of patents filed (and eventually granted) in a given year after adjustment for truncation
Cite	Number of citations received per patent in a given year after adjustment for truncation
Firm Characteristics	
Ln(MV)	Natural logarithm of market value of equity
R&D_Assets	Research and development expenditure divided by book value of total assets, set to zero if missing
ROA	Earnings before interest, taxes, depreciation, and amortization divided by book value of total assets
PPE_Assets	Net physical plant, property, and equipment scaled by book value of total assets
Leverage	Ratio of total debt to book value of total asset
CAPEX_Assets	Capital expenditure scaled by book value of total assets
HI	Herfindahl index of 4-digit SIC industry where firm i belongs, based on sales
Tobin's Q	Ratio of market value of assets (book value of assets minus book value of equity plus market value of equity) to book value of total assets
KZ_Index	KZ index is calculated as -1.002 * Cash flow + 0.28 * Tobin's Q + 3.18 * Leverage - 39.368 * Dividends - 1.315 * Cash holdings
Ln(Age)	Natural logarithm of the number of years since the firm appeared in Compustat
INST	The institutional holdings (%) for firm, calculated as the arithmetic mean of the four quarterly institutional holdings reported through form 13F
Debt_EBITDA	Total debt divided by before interest, taxes, depreciation, and amortization.
Networth_Assets	Net worth (total assets minus total liabilities) scaled by total assets
Current_Ratio	Total current assets divided by total current liabilities
Pat_growth	Growth in the number of patent computed over the three-year period before covenant violations
Cite_growth	Growth in the number of citations per patent computed over the three-year period before covenant violations
$\Delta Pat_{i,t-3\rightarrow t+3}$	The change in the number of patents from three years before the covenant violation to three years after the covenant violation, $\sum_{n=1}^{3} Pat_{i,t+n} - \sum_{n=1}^{3} Pat_{i,t-n}$.
$\Delta UnrelatedPat_{i,t-3\rightarrow t+3}$	The change in the number of unrelated patents from three years before the covenant violation to three years after the covenant violation, $\sum_{n=1}^{3} UnrelatedPat_{i,t+n} - \sum_{n=1}^{3} UnrelatedPat_{i,t-n}$.
No. of unrelated patents	Number of patents that are unrelated to a firm's main business, i.e., patents that are not mapped to a firm's main two-digit SIC industry.
No. of related patents	Number of patents that are related to a firm's main business, i.e., patents that are mapped to a firm's main two-digit SIC industry.