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Credit Default Swaps and Lender Incentives in Bank Debt Renegotiations

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Abstract

Using a regression-discontinuity design and within lender–borrower variation, we analyze how credit default swaps (CDSs) affect bank incentives and borrower outcomes in renegotiations after covenant violations. While existing studies document an investment decline after covenant violations, we find that covenant-violating firms maintain their investment subsequent to the introduction of CDS trading. Moreover, after CDS introduction, covenant-violating firms are less likely to default. Our results suggest that in the private debt markets, CDSs discipline borrowers, while the empty creditor problem due to CDS is mitigated because of lenders' reputation concerns and lower coordination frictions.

I. Introduction

Recent literature on commercial debt renegotiations has established two important but seemingly contradictory empirical facts. First, when firms violate bank loan covenants, lenders intervene and discipline the firms, which ultimately leads to an improvement in the violating firms' outcomes (Nini, Smith, and Sufi (2012)). However, in contrast, firms have a higher likelihood of bankruptcy when lenders renegotiate the debt of firms with traded CDS (Danis (2016)). This suggests that lenders have incentives to force firms with traded CDS into default (Hu and Black (2008), Subrahmanyam, Tang, and Wang (2014)).

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What explains these divergent firm outcomes? A common rationalization is that as CDS allows for the separation of cash flow rights and control rights, these contracts can misalign incentives between a lender and a borrower (Duffee and Zhou (2001), Campello and Matta (2012)). In the extreme, this incentive misalignment can lead to an "empty creditor problem," wherein lenders have incentives to overinsure using CDS and force firms into bankruptcy (Bolton and Oehmke (2011)).

However, the extant literature does not distinguish between the private and public debt markets despite significant differences in frictions and incentives faced by lenders and borrowers across these markets (Lin, Ma, Malatesta, and Xuan (2013)). For instance, as we discuss in detail shortly, public debt markets are typically characterized by arms-length lending and greater coordination frictions among lenders. In contrast, the private bank loan market relies on relationship-based lending and has lower coordination frictions within lending syndicates. Thus, incentives to preserve relationship and reputational capital are likely higher in the private debt market, which can mitigate the incentive misalignment problem due to CDS.

We find that the effect of CDS on borrowing firms crucially depends on the source of financing. Our article shows that CDS firms do not experience the empty creditor problem in the bank loan market when renegotiating debt. Moreover, in contrast to the findings in the previous literature, banks allow covenant-violating firms with CDS to maintain and even increase investment. These renegotiation dynamics in the private debt market in the presence of CDS are also economically important. In the last two decades, about 52% of the bank debt market by size is covered by CDS (see Figure 1). In comparison, 73% of the public debt market by size is covered by CDS during this period.

Our empirical evidence is consistent with the often overlooked predictions in Bolton and Oehmke (2011) and Campello and Matta (2020): There are benefits of CDS protection as a commitment device in renegotiations. Furthermore, Campello and Matta (2020) point out that CDS overinsurance increases borrower discipline and debt repayment incentives in nondefault states. Thus, the stronger bargaining power of creditors with CDS lowers a borrower's incentive to inefficiently renegotiate down payments for strategic reasons. With a lower likelihood of strategic default, when borrowers are unable to pay (e.g., due to liquidity reasons), lenders' efficient response is to help firms overcome liquidity constraints and allow the borrowers to continue operations. Our results are consistent with such an efficient lender response in the case of banks and CDS firms. Thus, the presence of CDS can likely discipline bank borrowers ex ante and help avoid underinvestment and excessive defaults.

The existing literature has mostly focused on the empty creditor problem of CDS (Hu and Black (2008), Subrahmanyam et al. (2014), Bedendo, Cathcart, and El-Jahel (2016), Danis (2016)). To the best of our knowledge, we are the first to empirically test and provide evidence for the disciplining effect of CDS. In contrast to the evidence for the empty creditor problem in the public debt market, we find that the disciplining effect of CDS dominates the empty creditor problem effect in the private debt market.

Why do banks respond differently in debt renegotiations in the presence of CDS compared with public debt markets?

One potential reason is reputation concerns (Gopalan, Nanda, and Yerramilli (2011)). Unlike public debt markets where investors are arm's length and often purchase debt in the secondary market, bank lending is a repeated game (both between the banks and their borrowers, as well as between one bank and other banks in the lending syndicate). Thus, the incentive to preserve reputational and relationship capital can mitigate the empty creditor problem.

A second potential reason is the ability of banks to screen and monitor firms. Campello and Matta (2020) show theoretically that the empty creditor problem manifests mostly in firms with more severe management–creditor agency problems, and in firms with assets that are costlier to verify. However, banks are repositories of information on borrowing firms as they have the ability to screen and monitor firms when providing credit (Rajan (1992), Petersen and Rajan (1994)). Thus, banks can mitigate severe agency problems and verify the assets of borrowing firms, thereby making bank lending less prone to the empty creditor problem.

A third potential reason for the different effect of CDS in the public versus private debt markets is the lower coordination frictions among lenders in the private debt markets. Bolton and Oehmke (2011) note that lenders may overinsure in the CDS market to also gain bargaining power over other lenders. However, in the case of banks, lending syndicate members often coordinate their responses through lead lenders (Gopalan et al. (2011)). Hence, banks might again have lower incentives to seek bargaining power over other lenders, unlike the dispersed public debt market lenders.

Overall, our empirical results are consistent with the theoretical arguments presented above, suggesting that the empty creditor problem is mitigated in the bank loan market. Moreover, the efficiency gains from using CDS as a commitment device, as outlined in Bolton and Oehmke (2011) and Campello and Matta (2020), allow borrowing firms to maintain investment and avoid potentially inefficient liquidation.

An empirical challenge in inferring lender incentives and their associated actions is that debt providers are typically not in charge of firm policy. Therefore, our empirical strategy focuses on covenant violations associated with bank loan contracts, which provide a natural setting to test the disciplining effect versus the empty creditor problem effect of CDS in the private debt market. First, covenant violations (or technical defaults) shift control rights from the shareholders and managers of the firm to the firm's lenders, who can force immediate repayment of the principal and terminate any lending commitments (Chava and Roberts (2008), Nini, Smith, and Sufi (2009)).[1] Thus, covenant violations provide an opportunity for empty creditor banks to force firms into default and collect their CDS insurance payments. Second, the discrete nature of covenant violations allows for identification using a regression discontinuity design (RDD).

[1]See Chava, Fang, Kumar, and Prabhat (2019) for a review of how creditor governance after covenant violations affects borrowers. See also Chava, Fang, and Prabhat (2021) for an example of how borrowers use covenants to provide a costly signal of their quality.

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Using the RDD methodology, we focus on isolating any discontinuous changes in the outcomes of firms in the presence of CDS trading at the covenant violation threshold, which is precisely where the transfer of control rights from firms to their lenders takes place. The RDD analysis is predicated on the assumption that in the absence of the control rights transfer to the lender after a covenant violation, firm outcomes will trend smoothly across the covenant violation threshold.

We use two primary firm outcome variables in our analysis: firm investment and distance-to-default. Chava and Roberts (2008) show that covenant violations lead to investment cuts for the violating firm, with these cuts being larger for the violating firms that have potentially greater agency conflicts (e.g., higher cash holdings). Consistent with Chava and Roberts (2008), we observe a decline of 0.7–1.1 percentage points (pp) in investment per quarter as a fraction of capital for firms in the absence of CDS trading. However, in contrast, we find that in the presence of CDS trading, firms are able to maintain the same or increase investment even after violating covenants. Furthermore, when control rights shift to lenders after covenant violations, firms in the presence of CDS trading do not experience a higher default likelihood relative to firms in the absence of CDS trading.

A potential concern in our analysis is that firms with a traded CDS are likely to be different than firms without a traded CDS. Therefore, differences in firm outcomes in the presence and absence of CDS trading could be attributed to the differences in firm characteristics rather than to the differences in lender behavior due to CDS trading. For instance, as firms with a traded CDS tend to be larger, a lower investment sensitivity to covenant violations of such firms relative to firms without a traded CDS might reflect the greater bargaining power of larger firms vis-á-vis their lenders.

While we cannot completely address the potential selection concerns associated with CDS trading, we attempt to mitigate these concerns in several ways. First, our results hold when we estimate the effect of CDS trading by fixing the firm–lender relationship (i.e., using firm–lead-lender fixed effects) and compare the investment response to covenant violations before and after the introduction of CDS trading on the firm. Second, we also control for any differential investment response to covenant violation between CDS and non-CDS firms due to potential differences in observable characteristics (e.g., size, leverage, cash, and investment opportunities). In alternative specifications, we mitigate potential lender-level confounding factors by including LEAD_LENDER × YEAR fixed effects, which control for time-varying changes in lender characteristics. These specifications identify the effect of CDS trading by comparing how the same lead lender responds to a CDS firm and a non-CDS firm that violate covenants in the same year. Furthermore, we do not find any evidence that firms in the presence of CDS trading experience other coincident discontinuities or engage in manipulation around the covenant violation threshold, which supports the validity of our RDD analysis.

Next, we provide additional evidence that supports the disciplining effect of CDS. We exploit cross-sectional variation in CDS liquidity across firms. Greater CDS liquidity should make it easier for lenders to obtain CDS protection, and thus should increase the disciplining effect. We find that when CDS liquidity is higher, covenant-violating CDS firms experience smaller investment cuts and are also subsequently less likely to default in the post-violation period. Finally, we also show that the disciplining effect matters more when the potential for agency conflicts are higher. That is, we find that covenant-violating CDS firms with large cash holdings are also able to maintain investment and do not approach default in the post-violation period. Overall, our evidence in the private debt market is consistent with the disciplining effect of CDS compared to the empty creditor problem.

Despite our attempts, we do not claim that our empirical analysis has eliminated the potential endogeneity concerns associated with CDS trading. As CDS trading is endogenous to firm and lender circumstances, we cannot completely eliminate the possibility of an omitted variable that drives both CDS introduction and firm outcomes of covenant-violating firms.

Another limitation of our empirical analysis is that discontinuous changes in firm outcomes at the covenant violation threshold are predicated upon lender intervention in violating firms. However, we do not directly observe lender intervention for all firms in our sample. Nevertheless, our findings remain the same for the small subset of firms where we can identify lender intervention. Finally, due to data limitations, we utilize CDS contracts on bonds (as opposed to loans) in our main analyses. We also use available loan CDS data, which cover a small subset of firms, and find qualitatively similar results.

Our article contributes to the literature on the costs and benefits of the CDS market. Theoretical studies such as Duffee and Zhou (2001) and Parlour and Winton (2013) show that while CDSs allow banks to manage credit risk, they could generate potential costs by reducing the banks' monitoring and screening incentives. Bolton and Oehmke (2011) study the trade-off between the disciplining effect and the empty creditor problem effect of CDS. Recently, theoretical work by Campello and Matta (2020) suggests that CDS overinsurance is more pronounced during good economic times and for safer firms, which mitigates the empty creditor problem effect of CDS. In empirical work, Danis (2016) analyzes out-of-court restructurings of public debt and shows that CDS firms face difficulties renegotiating debt, thus increasing their bankruptcy likelihood. Subrahmanyam et al. (2014) also show that CDS introduction leads to a higher incidence of bankruptcy. However, they do not distinguish between public and private debt, and do not condition their analysis on events that give creditors control over firms. In contrast to the public debt market, we find that the disciplining effect of CDS dominates the empty creditor problem effect in the private debt market. Our results highlight how financial innovation (such as CDS) can have contrasting effects on the public and private debt markets.

Our article is also related to the literature on how CDS introduction affects ex ante contracting between borrowers and lenders in the private debt market. This literature provides mixed findings on the disciplining effect of CDS. On the one hand, after the CDS introduction, Shan, Tang, and Winton (2019) show that firms face looser covenants and pledge less collateral to their lenders, Martin and Roychowdhury (2015) show that firms are less conservative in their accounting reporting practices, and Kang, Williams, and Wittenberg-Moerman (2021) show an increase in the likelihood of nonrelationship lenders acting as lead arrangers.

Together, the evidence in these articles is consistent with the disciplining effect. On the other hand, Amiram, Beaver, Landsman, and Zhao (2017) find that lead arrangers hold larger shares of the syndicated loan to firms and charge higher loan spreads after the CDS introduction, and Kim, Shroff, Vyas, and Wittenberg-Moerman (2018) find that shareholder monitoring increases and CDS firms provide more voluntary disclosures. In contrast to the disciplining effect of CDS, these studies suggest that information frictions between lenders and borrowers intensify after the introduction of CDS on a firm's debt.

This mixed evidence highlights the challenge in testing the disciplining effect of CDS based on ex ante loan contract terms as a lender's ability to monitor a borrowing firm, the quality of the borrowing firm, and the associated loan contract terms are jointly determined in equilibrium. In contrast, our study identifies the disciplining effect of CDS by directly studying the ex post actions of lenders conditional on their borrowing firms' covenant violations.

Overall, our results suggest that CDS can be beneficial in the bank lending market. Consequently, we contribute to the policy debate on the regulation of CDS and other similar financial derivatives by documenting a beneficial role of CDS.[2]

II. Data

This section presents the data sources, sample selection, and descriptive statistics.

A. Data Sources and Sample Selection

We obtain data on loans from the Loan Pricing Corporation DealScan database and the data on CDS from the Credit Market Analysis (CMA) Datavision, Bloomberg, and Markit databases. Financial data at the firm–quarter level are obtained from the Compustat, and equity return data are obtained from the CRSP databases.

The DealScan database consists of private loans made by bank and nonbank lenders to U.S. corporations. As Carey and Hrycray (1999) and Chava and Roberts (2008) note, DealScan loans comprise 50%–75% of all commercial loans by value in the United States during the early 1990s, and the fraction of this coverage has increased since 1995. *Loan facility* is the basic unit of loan-level information reported in the DealScan database. The various types of loan facilities (e.g., term loans and revolvers) are further grouped into packages. Loan information (e.g., loan amount, maturity, and type of loan) is reported at the facility level, whereas information on covenants is generally reported at the package level and applied to all the loans in a given package.

The data on the timing of CDS introduction are obtained from three separate sources: Markit, CMA Datavision, and Bloomberg. We use these databases to identify all the firms that have a traded CDS contract. We take the earliest CDS quote date from these three databases as the first sign of active CDS trading on a firm's debt. We use CDS contracts where the underlying security is a bond for our

[2]Regulatory restrictions on banks can have unforeseen negative effects on lending and economic output (see, e.g., Chakraborty, Hai, Holter, and Stepanchuk (2017)).

main analyses for the following reasons: First, the "failure to pay" credit event in the standardized bond CDS contracts provided by the International Swaps and Derivatives Association (ISDA) accounts for nonpayment on a broad set of firm obligations, which include bank loans. Second, Basel II and Basel III allow banks to use bond CDS contracts to mitigate the credit risk exposure of their loan portfolios due to the greater coverage and liquidity of the bond CDS market. Nevertheless, we show the validity of our results using loan CDS data in Section IV.A.

We begin with the universe of Compustat nonfinancial firms (i.e., excluding firms with 6000–6999 SIC codes) over the period between 1996 and 2020. We construct our covenant violation sample following Chava and Roberts (2008) for the nonfinancial firms with the current ratio, net worth, or tangible net worth covenants because these covenants are frequent, and the accounting measures associated with these covenants are relatively unambiguous, standardized, and less susceptible to manipulation. Our initial Compustat–DealScan merged sample, which contains firms with loans that have a current ratio or a net worth covenant, consists of 2,345 firms which correspond to 3,863 packages and 662 unique lead lenders.[3] Of these 2,345 firms, 278 are firms that have a traded CDS at some point of time during our sample period. These firms correspond to 616 packages and 214 unique lead lenders. We consider a package's covenants to apply to a firm from the package's start date to the package's maturity date.[4] We also update the covenants that govern the relationship between a firm and its lead lender in any given quarter by accounting for dynamic covenant thresholds (i.e., thresholds that change over time), the refinancing of loans in a package, and the origination of new packages and loans.

First, we collapse our Compustat–DealScan merged sample at the firm–lead-lender–quarter level by retaining the tightest covenant across multiple existing packages in a given quarter. In this manner, we identify the tightest covenant that governs the relationship between a firm and its lead lender in any given quarter. Covenant tightness is measured by the relative distance of the covenant threshold from its corresponding accounting variable.[5] Next, following Chava and Roberts (2008), we retain only those firms in our sample that have violated the current ratio or net worth covenants at least once during our sample period. We do this because our empirical strategy identifies the effect of covenant violations "within firm" by including firm–lead-lender fixed effects. As a result, the firms that do not violate their covenants do not contribute toward the identification of the covenant violation effect; however, retaining them might underestimate our standard errors and overstate our statistical significance. After this filter, our sample includes 841 covenant-violating firms that have borrowed from 660 unique lead lenders.

Next, we drop CDS firms that do not have no-restructuring-type CDS contracts. This ensures that CDS payments are not automatically triggered if debt is restructured after a covenant violation, thereby preserving meaningful

[3] Chava and Roberts (2008) had 1,599 firms and 2,075 packages over their 1994–2005 time period.

[4] A package's start date is the earliest start date among all its loan facilities. Similarly, a package's maturity date is the latest maturity date among all its loan facilities.

[5] For example, the covenant tightness for the current ratio covenant is given by the ratio $\frac{(\text{CURRENT\_RATIO} - \text{CURRENT\_RATIO\_COVENANT\_THRESHOLD})}{\text{CURRENT\_RATIO\_COVENANT\_THRESHOLD}}$. The smaller this ratio, the tighter the covenant.

renegotiation incentives between the lender and the violating firm.[6] Finally, we omit firms that do not have information on all our control variables. This leaves us with 710 covenant-violating firms which have borrowed from 389 unique lead lenders. Among these 710 firms, there are 53 firms with a traded CDS which have borrowed from 99 unique lead lenders. In our final sample, 20.24% of the firm–quarter observations for firms with a traded CDS are in covenant violation, whereas 28.07% of the firm–quarter observations are in covenant violation for non-CDS firms.

B. Descriptive Statistics

First, Figure 1 displays a motivation for the importance of the renegotiation dynamics analyzed in this article. The figure shows the relative sizes of the private and public debt issued by firms in the presence of CDS trading. The figure shows that a significant fraction of the private debt issued by size in the last two decades occurred in the presence of CDS trading. Over this period, on average, about 52% of the private debt market by size is covered by CDS. In comparison, Figure 1 shows that this coverage is 73% on average for the public debt markets. Thus, a large portion of the private debt market faces the renegotiation dynamics analyzed in this article.

FIGURE 1

Private and Public Debt Market Coverage by CDS

Figure 1 shows the relative sizes of the private and public markets covered by credit default swap (CDS) in the Capital IQ–Compustat merged universe of firms. The solid line represents the total amount of outstanding loans issued by CDS firms relative to all the firms. The dashed line represents the total amount of outstanding bonds issued by CDS firms relative to all the firms. Both fractions are expressed in percentage terms.

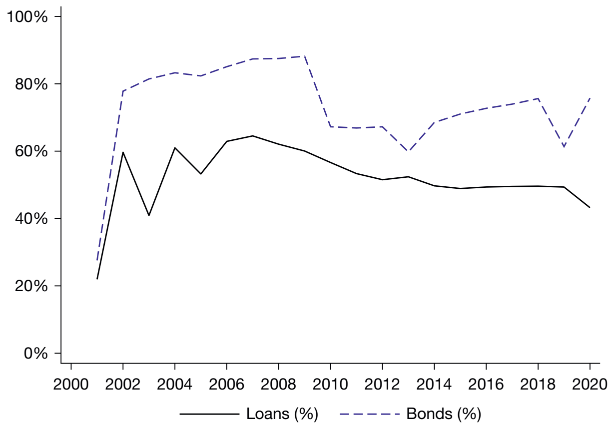


Table 1 reports the summary statistics for the firms in our sample based on whether a firm–quarter observation is determined to be in covenant violation. Firm–quarter observations are considered to be in covenant violation if the

[6]We thank the referee for this suggestion. Table IA.2 in the Supplementary Material reports that the baseline results are not sensitive to applying this data filter.

TABLE 1

Summary Statistics of the Covenant Violation Sample: CDS Versus Non-CDS Firms

Table 1 reports the summary statistics (mean (median)) for the covenant violation sample. The sample period is from 1996 to 2020 and consists of nonfinancial firms that have violated the current ratio or the net worth covenant at least once during the sample period. The sample is further divided based on whether a firm–quarter observation is determined to be in covenant violation status (denoted by "VIOL") or not in covenant violation status (denoted by "SLACK"). Firm–quarter observations are classified as "credit default swap (CDS)" observations if there are CDS contracts trading on the firm's debt in that quarter. The median is provided in parentheses. Variable definitions of all the firm characteristics in the table are provided in the Supplementary Material.

	CDS Firms		Non-CDS Firms	
Covenant Violation	VIOL	SLACK	VIOL	SLACK
INVESTMENT	0.051 (0.034)	0.048 (0.038)	0.055 (0.032)	0.075 (0.048)
log(DISTANCE_TO_DEFAULT)	1.307 (1.467)	1.645 (1.773)	0.782 (0.978)	1.250 (1.377)
PROFITABILITY	0.122 (0.119)	0.132 (0.119)	0.055 (0.081)	0.104 (0.116)
log(ASSETS)	8.469 (8.423)	8.746 (8.543)	5.073 (4.990)	5.308 (5.258)
BOOK_LEVERAGE	0.656 (0.640)	0.608 (0.619)	0.611 (0.629)	0.511 (0.519)
MARKET_TO_BOOK	1.287 (1.201)	1.373 (1.223)	1.341 (1.092)	1.540 (1.234)
MACRO_Q	3.678 (1.733)	6.724 (2.456)	5.330 (1.936)	8.057 (2.902)
TANGIBLE_CAPITAL	0.571 (0.530)	0.410 (0.405)	0.411 (0.337)	0.369 (0.298)
CASH_FLOW	0.042 (0.040)	0.111 (0.062)	−0.065 (0.027)	0.053 (0.059)
INTEREST_EXPENSE	0.024 (0.023)	0.023 (0.021)	0.028 (0.025)	0.021 (0.017)
CASH	0.042 (0.022)	0.074 (0.050)	0.063 (0.020)	0.091 (0.035)

firm's net worth or current ratio in the quarter is below its corresponding covenant threshold. The covenant-violating firm–quarter observations are denoted by VIOL, whereas the nonviolating firm–quarter observations are denoted by SLACK. Table 1 and the accompanying Figure 2 show a decline in investment for firms in the absence of CDS trading when they violate their covenants, which is consistent with previous studies (Chava and Roberts (2008), Nini et al. (2012)). However, the investment for firms in the presence of CDS trading remains relatively unchanged when they violate their covenants.

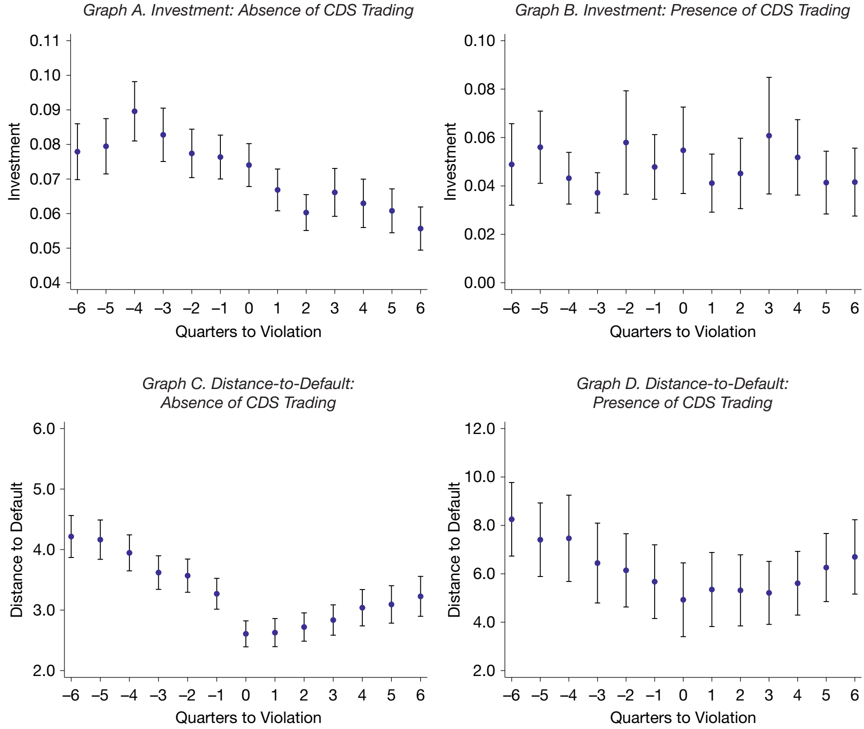
Table 1 also reports that firms are closer to default when they are in violation of their covenants. Again, however, firms in the presence of CDS trading have a smaller increase in their default likelihood compared to firms in the absence of a traded CDS. These summary statistics suggest that in the presence of CDS trading, firms do not face a higher default risk after covenant violations, which is inconsistent with the empty creditor problem hypothesis.

Furthermore, firm characteristics that likely affect firm investment, such as investment opportunities (market-to-book ratio and macro q), cash flow, and profitability, are also lower when the firm is in violation of its covenants. Again, investment and profitability fall less for firms in the presence of CDS trading, which indicates that firm operations are less affected in the presence of CDS trading despite lenders gaining control over the firm after covenant violations.

FIGURE 2

Investment and Distance-to-Default in Event Time Around Covenant Violation

Figure 2 shows the average firm investment and distance-to-default in event time around covenant violations. Graphs A and C show the mean values in the absence of credit default swap (CDS) trading. Graphs B and D show the mean values in the presence of CDS trading. The black vertical lines show the 95% confidence intervals. The blue solid circles represent the mean values.



We note that Table 1 also reports significant heterogeneity across firm characteristics in the presence and absence of CDS trading. For instance, firms with a traded CDS tend to be larger, more profitable, generate higher cash flows, and are farther away from default compared with the firms without a traded CDS. We control for these differences in firm characteristics in our regression framework. In addition to firm-level differences, our empirical framework also controls for lender-level heterogeneity and differences in loan terms. Table IA.1 in the Supplementary Material reports loan and lender characteristics for firms with and without a traded CDS. Firms with a traded CDS are larger, have better financial health, and have more pledgeable income.[7]

[7]Table IA.1 in the Supplementary Material reports that firms with a traded CDS have larger loans, larger loan syndicates, lower loan spreads, and are less likely to have restrictive nonprice terms (e.g., tighter covenants at loan initiation). Lead lenders of firms with a traded CDS have a greater market share in the syndicated loan market and lend to better-quality firms more generally.

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III. Results

We present our main results in this section. Section III.A estimates the impact of covenant violation on firm investment in the presence of CDS trading. Section III.B investigates whether firms move closer to default after violating a covenant in the presence of CDS trading. These analyses allow us to investigate whether the empirical evidence on bank debt renegotiations are consistent with the empty creditor problem or the disciplining effect of CDS (Bolton and Oehmke (2011), Campello and Matta (2020)). The analysis in Section III.C attempts to mitigate selection bias concerns.

A. Firm Investment at Covenant Violation

The existing literature shows that firms violating bank loan covenants experience significant investment cuts (Chava and Roberts (2008), Nini et al. (2012)). Moreover, these investment cuts are larger for firms associated with greater agency problems, which suggests that lenders intervene in covenant-violating firms to mitigate agency problems and discipline firms. In contrast, the existing theoretical literature on CDS suggests that lenders may not have incentives to intervene and discipline firms if lenders are able to lay off the firm's credit risk using CDS (Duffee and Zhou (2001)).

To reconcile the insights above, this section investigates how CDS trading affects lender intervention in the bank loan market for covenant-violating firms. The central idea in the theoretical model of Bolton and Oehmke (2011) is that CDS increases the bargaining power of lenders vis-á-vis their borrowers during debt renegotiations. This increased bargaining power of lenders has two potential effects. One is the empty creditor problem, where lenders have incentives to overinsure using CDS and inefficiently force firms into default. The other is the disciplining effect, where the increased bargaining power of lenders in debt renegotiations disincentivizes borrowers to violate covenants for strategic reasons or due to agency conflicts (Campello and Matta (2020)). As a result, efficient lender intervention should predict the continuation of existing investments since these investments are less likely to be subject to agency problems.

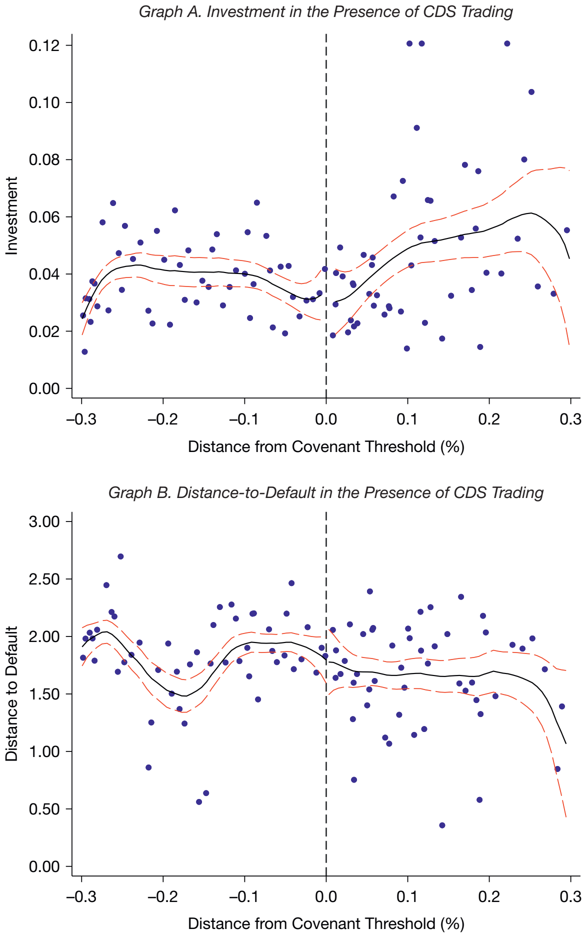
Thus, the disciplining effect of CDS suggests that lender intervention after covenant violations should result in smaller investment cuts for CDS firms relative to non-CDS firms. On the other hand, under the empty creditor problem, lenders can force violating firms into default, thereby financially constraining firms and resulting in greater investment cuts.

Moreover, in the private bank loan market, which is the focus of our study, the disciplining effect is likely to dominate the empty creditor problem effect. As mentioned previously, lenders in the private bank loan market have incentives to preserve relationship and reputational capital with their borrowers and lending syndicates. Banks are also expected to have better information on their borrowers as they screen and monitor their borrowers. Together, these factors can reduce the incentives of lenders in the private bank loan market to force firms into bankruptcy and profit from it, thereby mitigating the empty creditor problem.

FIGURE 3

Investment and Distance-to-Default Versus Distance-to-Violation in the Presence of CDS Trading

Figure 3 shows the investment and distance-to-default versus distance to covenant violation for firms in the presence of credit default swap trading. Distance to covenant violation threshold is defined as the negative of the relative covenant distance ($-\frac{\text{RATIO}-\text{COVENANT\_THRESHOLD\_RATIO}}{\text{COVENANT\_THRESHOLD\_RATIO}}$). For cases in which both net worth and current ratio covenants are present, the tighter of the two covenants is chosen to compute the distance to covenant violation. The solid black line fits a nonparametric local linear polynomial using a triangular kernel within a bandwidth of 0.3 around the covenant threshold. The long-dashed red lines show the 95% confidence intervals. The solid blue circles display the mean distance-to-default for 50 bins defined along the distance to covenant violation on each side of the covenant threshold.



As a first step, Graph A of Figure 3 shows the relation between firm investment and distance-to-violation in the presence of CDS trading. The figure does not indicate a reduction in investment for firms with a traded CDS, which is consistent with the disciplining effect of CDS. Moreover, this result contrasts with existing studies that document an investment cut after covenant violations (Chava and Roberts (2008), Nini et al. (2012)).

Next, we formally estimate and compare the investment response to covenant violations in the presence and absence of CDS trading. We use a sharp RDD design around the covenant violation threshold where control rights over violating firms are transferred to their lenders. Ideally, our empirical design should resemble a fuzzy RDD design which accounts for the probability of lender invention after covenant violations. However, similar to other articles that study firm responses to covenant violations, we do not directly observe whether lenders intervene in covenant-violating firms. Thus, we employ a sharp RDD design similar to Chava and Roberts (2008) and estimate the following model for firm i borrowing from a lead bank b with firm investment Y as the outcome variable:

$$(1)\quad Y_{i,b,t} = \alpha + \beta_1 \times \text{VIOL}_{i,b,t-1} + \beta_2 \times \text{VIOL}_{i,b,t-1} \times \text{CDS}_{i,t} + \beta_3 \times \text{CDS}_{i,t} + f(X_{i,b,t-1}) + \Gamma_{i,b} + \tau_t + \delta_t + \varepsilon_{i,b,t}.$$

The main independent variables of interest are $\text{VIOL}_{i,b,t-1}$ and the interaction term $\text{VIOL}_{i,b,t-1} \times \text{CDS}_{i,t}$. The indicator variable $\text{VIOL}_{i,b,t-1}$ equals 1 if firm i in quarter $t-1$ is in violation of lender b's loan covenant, and equals 0 otherwise. Similarly, $\text{CDS}_{i,t}$ is an indicator variable that equals 1 if there is a traded CDS contract for firm i in quarter t.

The coefficient β_1 captures the effect of covenant violations on investment in the absence of CDS trading, whereas the coefficient $\beta_1 + \beta_2$ captures the effect of covenant violations on investment in the presence of CDS trading. Thus, the coefficient β_2 captures the differential response of investment to covenant violations in the presence of CDS trading and tests for its statistical significance. The vector $f(X_{i,b,t-1})$ controls for potential time-varying characteristics at the firm level (e.g., assets and leverage) and firm–lender level (e.g., relationship strength and lending syndicate size) that might affect firm investment. Firm–lender fixed effects are denoted by $\Gamma_{i,b}$. The variables τ_t and δ_t denote calendar year–quarter fixed effects and fiscal quarter fixed effects, respectively, to control for common time-varying factors (e.g., economic boom and bust periods) and seasonal reporting patterns by firms. The Supplementary Material provides detailed definitions of all the firm controls and outcome variables.

While our data set is at the firm–lead-lender level, we observe the investment response only at the firm level. As a result, the outcome variable will be repeated across a firm's multiple lead banks and could bias the coefficients of interest and their standard errors due to the mechanical correlation of the error terms within firms. Thus, we conduct a weighted regression to ensure that the effect of each lender on the firm's outcome variable is given equal treatment.[8] We also double-cluster the standard errors at both the firm level and the lead-lender level to account for plausible correlations among error terms within firms and within lenders.[9]

While Chava and Roberts (2008) focus on identifying β_1, the specification above also seeks to identify β_2 (i.e., the differential impact of CDS trading on the

[8]The regression weights are the inverse of the number of lead lenders associated with all outstanding loans in a firm-quarter in our data. Our data consist of loans with either the current ratio covenant, net-worth covenant, or both.

[9]An alternative way to conduct our analysis is to average the data at the firm level. However, this alternative method reduces our flexibility to exploit the effect of CDS trading within a firm–lender link. Nevertheless, as robustness, we also conduct our analysis at the firm level with firm fixed effects and find that our baseline results are very similar (see Table IA.3 in the Supplementary Material).

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outcomes of covenant-violating firms). However, the identification of β_2 is confounded by a potential selection bias. Firms with a traded CDS are likely different compared with firms without a traded CDS. Therefore, any difference in the investment sensitivity to covenant violation between CDS and non-CDS firms could be attributed to the differences in their firm characteristics rather than to the differences in lender behavior due to CDS trading. For example, as CDS firms tend to be larger, a lower investment sensitivity to covenant violations of CDS firms relative to non-CDS firms might simply reflect the greater bargaining power of larger firms vis-á-vis their lenders in their ex post renegotiations after covenant violations.

While we cannot completely eliminate the aforementioned potential selection concerns, we attempt to mitigate these concerns using several steps. First, we estimate the effect of CDS trading *within* firm–lead lenders by including firm–lead-lender fixed effects ($\Gamma_{i,b}$) in all our specifications. Thus, the coefficient VIOL × CDS is estimated by holding the firm–lead-lender relationship fixed and comparing the investment response to covenant violations in the presence and absence of CDS trading. Second, we also include the interaction of the covenant violation indicator VIOL with observable characteristics (e.g., size and leverage) that tend to differ between CDS firms and non-CDS firms. These interactions help control for differential responses between CDS firms and non-CDS firms that might be due to differences in observable characteristics. Furthermore, to ensure the validity of the RDD analysis for CDS firms, we also conduct RDD diagnostics. We do not find any evidence for other coincident discontinuities or manipulation around the covenant violation threshold for CDS firms (see Figures IA.1 and IA.2 in the Supplementary Material).

Table 2 reports the results after estimating variations of equation (1). Columns 1–3 isolate the discontinuities in firm investment by fitting a separate fourth-degree global distance polynomial on each side of the covenant violation threshold. Columns 4–6 restrict the analysis within an optimal bandwidth (OB) sample to focus on observations close to the covenant violation threshold. Both these methods involve a bias-variance trade-off. The global polynomial approach utilizes the entire sample and thus improves the statistical power of the RDD estimates. On the other hand, the OB reduces bias in the RDD estimates by focusing on a small region around the covenant threshold.

The main coefficient of interest β_2, which is associated with VIOL × CDS, is positive and significant in all the specifications. The results suggest that at the covenant violation threshold when control rights shift to the firm's lenders, CDS firms can maintain a 1.5–1.9 pp higher investment as a fraction of capital (i.e., property, plant, and equipment or PP&E) per quarter relative to non-CDS firms. In contrast, the negative coefficient on VIOL suggests a reduction in the investment-to-capital ratio for non-CDS firms by 0.7–1.1 pp when control rights shift to the firm's lenders. These point estimates associated with VIOL are consistent with Chava and Roberts (2008) both in terms of sign and magnitude.

We progressively add firm-level and firm–lender-level controls across the specifications in Table 2 for the full sample and the OB sample. In general, the coefficient on VIOL × CDS is not very sensitive to including or excluding these controls.

TABLE 2

Investment Response to Covenant Violation

Table 2 reports the results for the sensitivity of investment to covenant violations using a regression discontinuity design. The *Full Sample* consists of firm–lead-lender–quarter observations for firms that have experienced either a net worth or a current ratio covenant violation at least once during our sample period of 1996 to 2020. The *OB Sample* is a subset of the full sample within an optimal bandwidth of 0.367 computed using Imbens and Kalyanaraman (2012) for the investment sample. VIOL is an indicator variable equal to 1 if a firm–lead-lender–quarter observation is determined to be in covenant violation, and 0 otherwise. CDS is an indicator variable equal to 1 if there is a traded credit default swap contract for that firm–quarter observation. All control variables are lagged by 1 quarter. Variable definitions are provided in the Supplementary Material. Standard errors are *double*-clustered at the firm and lead-lender level, and *t*-statistics are displayed in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: INVESTMENT					
	Full Sample			OB Sample		
	1	2	3	4	5	6
VIOL × CDS	0.018***	0.015***	0.016***	0.017***	0.019***	0.019***
	(4.33)	(3.14)	(3.12)	(4.92)	(4.53)	(4.42)
VIOL	−0.008**	−0.008**	−0.008**	−0.011***	−0.008***	−0.007***
	(−1.99)	(−2.06)	(−2.04)	(−5.46)	(−3.70)	(−3.64)
CDS	0.003	0.015**	0.014*	0.002	0.005	0.004
	(0.52)	(1.98)	(1.89)	(0.20)	(0.58)	(0.50)
Firm-level controls						
MACRO_Q		0.001***	0.001***		0.001***	0.001***
		(7.86)	(7.86)		(3.99)	(4.01)
CASH_FLOW		0.009**	0.009**		0.010***	0.010***
		(2.50)	(2.49)		(2.67)	(2.69)
log(ASSETS)		0.002	0.001		−0.035**	−0.036**
		(0.15)	(0.08)		(−2.19)	(−2.23)
log(ASSETS)^2		−0.001	−0.001		0.002	0.002
		(−1.30)	(−1.22)		(1.49)	(1.56)
CASH		0.070***	0.070***		0.053**	0.054**
		(3.48)	(3.47)		(2.50)	(2.53)
BOOK_LEVERAGE		−0.029**	−0.029**		−0.029*	−0.031*
		(−2.28)	(−2.27)		(−1.70)	(−1.82)
INTEREST_EXPENSE		−0.338***	−0.337***		−0.371***	−0.357***
		(−3.69)	(−3.69)		(−3.39)	(−3.25)
RATED		0.000	0.000		−0.002	−0.003
		(0.06)	(0.04)		(−0.37)	(−0.54)
Firm–lender-level controls						
log(SYNDICATE_SIZE)			−0.008			0.001
			(−1.03)			(0.12)
RELATIONSHIP_STRENGTH			0.003			0.010*
			(0.64)			(1.65)
LEAD_SHARE			−0.017			−0.005
			(−0.68)			(−0.18)
INITIAL_COVENANT_TIGHTNESS			0.001			−0.009
			(0.34)			(−1.27)
Firm–lead-lender FE	✓	Yes	✓	✓	✓	✓
Calendar year–qtr FE	✓	✓	✓	✓	✓	✓
Fiscal qtr FE	✓	✓	✓	✓	✓	✓
Viol × Dist. poly.	✓	✓	✓			
Σ Coeff.	0.010*	0.008	0.008	0.006*	0.012***	0.012***
	(1.87)	(1.38)	(1.41)	(1.77)	(2.99)	(2.97)
N	16,133	16,133	16,133	8,954	8,954	8,954
Adj. R^2	0.366	0.406	0.406	0.401	0.428	0.428

The total effect of the sensitivity of investment to covenant violations for CDS firms (i.e., $\beta_1 + \beta_2$) is reported at the bottom of Table 2. This total effect is positive across all specifications and is statistically significant with a point estimate of 1.2 pp in our most restrictive and saturated specification in column 6. This increase in investment for CDS firms is also economically significant as it is about 24% of the mean investment rate of 4.9 pp per quarter for CDS firms.

We also conduct additional checks to support the general validity of our RDD analysis. For instance, we show the density of the distance-to-violation for CDS firms and non-CDS firms in Figure IA.2 in the Supplementary Material and find no evidence of manipulation around the covenant violation threshold for both types of firms. This result is consistent with Chava and Roberts (2008), who argue that the net worth and current ratio covenants are more standardized and unambiguous, and thus are less susceptible to manipulation.

We obtain similar results to those reported in Table 2 when we analyze the subsamples of covenant-violating CDS and non-CDS separately in Table IA.4 in the Supplementary Material. Furthermore, estimates in Table IA.4 are bias-corrected RDD estimates with a robust variance estimator implemented using data-driven statistical inference developed by Calonico, Cattaneo, and Titiunik (2014). However, we prefer the analysis using the baseline specification in equation (1) (as opposed to Table IA.4) because it allows us to estimate and compare the effect of covenant violations for CDS and non-CDS firms within the same model, and further easily extend the analysis with additional interaction terms to pin down the economic channel.

Overall, the baseline results suggest that firms do not reduce investment after covenant violations in the presence of CDS trading. These results are consistent with the disciplining effect of CDS trading and contrast with the findings in the existing literature that documents an investment cut for covenant-violating firms in the absence of CDS trading.

B. Distance-to-Default at Covenant Violation

This section tests how the default probability of covenant-violating firms changes in the presence of CDS trading. Default probabilities offer a direct test for the empty creditor problem as it suggests a higher default likelihood of covenant-violating firms due to lenders' lower incentives for beneficial debt renegotiations. On the other hand, the disciplining effect of CDS suggests that there should be no increase in the default likelihood due to lender actions. This is because the disciplining effect suggests greater lender incentives for beneficial debt renegotiations.

Table 3 estimates equation (1) to compare the sensitivity of the logarithm of distance-to-default to covenant violations between CDS firms and non-CDS firms. We compute the distance-to-default following Bharath and Shumway (2008). The distance-to-default of a firm reflects the firm's 1-year default probability.

Columns 1–3 of Table 3 report results for the full sample after fitting a separate fourth-degree global distance polynomial on each side of the covenant violation threshold, whereas columns 4–6 report results for the OB sample. Again, all specifications include firm–lender fixed effects to control for the endogenous pairing of lenders and firms. Thus, the effect of CDS trading is identified *within* an existing firm–lender relationship.

First, the negative coefficients associated with VIOL across all specifications in Table 3 indicate that violating a covenant moves a firm closer to default. The most

TABLE 3

Distance-to-Default Response to Covenant Violation

Table 3 reports the results for the sensitivity of distance-to-default (DD) to covenant violations using a regression discontinuity design. The *Full Sample* consists of firm–lead-lender–quarter observations for firms that have experienced either a net worth or a current ratio covenant violation at least once during our sample period of 1996 to 2020. The *OB Sample* is a subset of the full sample within an optimal bandwidth of 0.383 computed using Imbens and Kalyanaraman (2012) for the distance-to-default sample. VIOL is an indicator variable equal to 1 if a firm–lead-lender–quarter observation is determined to be in covenant violation, and 0 otherwise. CDS is an indicator variable equal to 1 if there is a traded credit default swap contract for that firm–quarter observation. All control variables are lagged by 1 quarter. Variable definitions are provided in the Supplementary Material. Standard errors are *double*-clustered at the firm and lead-lender level, and *t*-statistics are displayed in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: log(DD)					
	Full Sample			OB Sample		
	1	2	3	4	5	6
VIOL × CDS	0.143	0.022	0.027	0.064	0.014	0.019
	(1.44)	(0.23)	(0.28)	(0.70)	(0.17)	(0.23)
VIOL	−0.060	−0.042	−0.042	−0.197***	−0.119***	−0.122***
	(−1.14)	(−0.87)	(−0.87)	(−6.30)	(−4.62)	(−4.65)
CDS	0.035	0.194*	0.181*	0.069	0.155	0.150
	(0.34)	(1.95)	(1.86)	(0.47)	(1.22)	(1.19)
Firm-level controls						
MARKET_TO_BOOK		0.263***	0.267***		0.311***	0.315***
		(10.86)	(11.09)		(8.07)	(7.92)
PROFITABILITY		1.209***	1.206***		1.299***	1.295***
		(12.04)	(11.99)		(10.37)	(10.37)
log(ASSETS)		0.063	0.047		−0.150	−0.149
		(0.42)	(0.31)		(−0.82)	(−0.82)
log(ASSETS)^2		−0.012	−0.011		0.003	0.003
		(−0.90)	(−0.86)		(0.18)	(0.18)
CASH		0.573***	0.551***		0.614*	0.626*
		(3.02)	(2.92)		(1.76)	(1.79)
BOOK_LEVERAGE		−0.815***	−0.805***		−1.065***	−1.050***
		(−5.45)	(−5.39)		(−5.27)	(−5.21)
INTEREST_EXPENSE		−3.526**	−3.428**		−3.229**	−3.290**
		(−2.53)	(−2.45)		(−2.03)	(−2.06)
RATED		−0.130	−0.125		−0.016	−0.000
		(−1.62)	(−1.56)		(−0.13)	(−0.00)
Firm–lender-level controls						
log(SYNDICATE_SIZE)			−0.163			−0.161
			(−1.38)			(−0.86)
RELATIONSHIP_STRENGTH			−0.045			−0.022
			(−0.55)			(−0.24)
LEAD_SHARE			−0.892***			−0.733
			(−2.90)			(−1.16)
INITIAL_COVENANT_TIGHTNESS			0.027			0.167
			(0.84)			(1.20)
Firm–lead-lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar year–qtr FE	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal qtr FE	Yes	Yes	Yes	Yes	Yes	Yes
VIOL × Dist. poly.	Yes	Yes	Yes			
Σ Coeff.	0.083	−0.020	−0.015	−0.133	−0.105	−0.103
	(0.77)	(−0.19)	(−0.15)	(−1.50)	(−1.36)	(−1.33)
N	13,807	13,807	13,807	7,152	7,152	7,152
Adj. R^2	0.621	0.679	0.680	0.639	0.697	0.698

restrictive and saturated specification in column 6 indicates that the distance-to-default of firms on average decreases by 12.2% at the covenant violation threshold when control rights shift to lending banks. Thus, a firm experiences a discontinuous increase in its default probability after a covenant violation, which is consistent with Nini et al. (2012), who show that the likelihood of a firm's exit increases by 28%

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within 1 year after the covenant violation, and that these exits are driven mainly by distressed exits.[10]

As before, the estimated coefficient on the interaction term VIOL × CDS is our main coefficient of interest. We find positive coefficients, although statistically insignificant, in all the specifications. These estimates indicate that covenant-violating firms in the presence of CDS trading do not move closer to default *relative* to covenant-violating firms in the absence of CDS trading. Furthermore, the total effect of covenant violations on the distance-to-default for firms in the presence of CDS trading is presented at the bottom of Table 3. Although this total effect is negative in most specifications, it is statistically insignificant and smaller in magnitude relative to the coefficient on VIOL. Overall, the results in Table 3 do not support the empty creditor problem, which predicts an increase in the default likelihood of covenant-violating firms in the presence of CDS trading.

Together, our baseline results in Tables 2 and 3 suggest that in the presence of CDS trading, intervention by banks after covenant violations does not reduce firm investment and does not force firms toward default. These two results, which individually contrast with the previous findings on the effect of CDS trading and the effect of lender intervention after covenant violations, can be jointly reconciled based on the disciplining hypothesis as discussed before. That is, CDS disciplines firm behavior and mitigates agency conflicts, and as a result helps avoid underinvestment and excessive defaults when lenders gain control rights over their borrowing firms.

C. Mitigating Selection Bias Concerns

This section attempts to mitigate the potential selection issues associated with CDS trading that can confound our analysis. Selection issues associated with CDS trading can occur along two dimensions: i) the firm level and ii) the lender level. For example, the introduction of CDS trading on a firm's debt could be correlated with other changes to firm characteristics (e.g., firm leverage). Similarly, the types of banks that lend to firms with traded CDS might also be different. In both cases, differences in firm characteristics and lender types, as opposed to CDS trading, could drive the differential response to covenant violations in the presence and absence of CDS trading. We investigate such potential concerns below.

1. Firm-Level Differences

This subsection investigates a few important observable firm characteristics that tend to differ between CDS and non-CDS firms and tests whether these differences are driving our results. The first important firm characteristic we consider that can drive a heterogeneous response to covenant violations is firm size. CDS firms tend to be larger than non-CDS firms. It is possible that the investments of larger firms are less sensitive to covenant violations than smaller firms because larger firms have greater bargaining power vis-á-vis their lenders when their lenders assume control of the firm after a covenant violation.

[10]The firm exit hazard rate in Nini et al. (2012) increases from 3.2% to 4.1%, which is an increase of about 28%.

To mitigate such a concern, we classify the firms in our sample into size quintiles, and we include the interactions of the indicator variables for each quintile with the covenant violation indicator in our regression specification (e.g., VIOL × $\text{SIZE}_i\ \forall i = \{1,5\}$). This approach controls for any differential sensitivity of smaller and larger firms to covenant violations in a flexible and nonparametric manner. Moreover, the effect of CDS trading on covenant violations is also identified *within* these size quintiles. In other words, if the differential sensitivity to covenant violations between CDS and non-CDS firms is in fact driven by differences in their respective sizes, then VIOL × SIZE_i should subsume all the variations in VIOL × CDS.

The results after including the interaction of the size quintile variable with VIOL are reported in column 1 of Table 4.[11] Panel A reports the results for firm investment, and Panel B reports the results for distance-to-default. The magnitude

TABLE 4

Controlling for Differential Response Due to Firm Heterogeneity

Table 4 reports the results for the sensitivity of investment (Panel A) and distance-to-default (DD; Panel B) to covenant violations using a regression discontinuity design after flexibly controlling for firm characteristics. VIOL is an indicator variable equal to 1 if a firm–lead-lender–quarter observation is determined to be in covenant violation, and 0 otherwise. In columns 1–4, we categorize each firm characteristic (size, leverage, investment opportunities, and cash) into quintiles, which are represented by their corresponding categorical variables SIZE, LEVERAGE, INVOPP, and CASH, respectively. Next, we include the interactions of indicator variables for each quintile with the covenant violation indicator (e.g., VIOL × Size_i $\forall i = \{1,5\}$). The specifications in Panels A and B include all the control variables from Tables 2 and 3, respectively. However, we omit the continuous variable from the specification when we include its corresponding quintile categorical variable. Variable definitions of all the firm characteristics are provided in the Supplementary Material. Standard errors are *double*-clustered at the firm and lead-lender level, and *t*-statistics are displayed in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Quintile	SIZE	LEVERAGE	INVOPP	CASH
	1	2	3	4
Panel A. Investment				
	Dependent Variable: INVESTMENT			
VIOL × CDS	0.012**	0.013**	0.014***	0.014***
	(2.04)	(2.50)	(3.05)	(2.78)
CDS	0.007	0.014*	0.012	0.014*
	(1.21)	(1.94)	(1.60)	(1.94)
N	16,133	16,133	16,133	16,133
Adj. R^2	0.405	0.407	0.408	0.406
Panel B. Distance-to-Default				
	Dependent Variable:log(DD)			
VIOL × CDS	0.025	−0.038	−0.032	−0.053
	(0.21)	(−0.36)	(−0.31)	(−0.52)
CDS	0.131	0.232**	0.201**	0.227**
	(1.24)	(2.25)	(2.16)	(2.16)
N	11,690	11,690	12,741	11,690
Adj. R^2	0.689	0.693	0.704	0.689
Controls for Panels A and B				
VIOL × Quintile	Yes	Yes	Yes	Yes
Firm–lead-lender FE	Yes	Yes	Yes	Yes
Calendar year–qtr FE	Yes	Yes	Yes	Yes
Fiscal qtr FE	Yes	Yes	Yes	Yes
VIOL × Dist. poly.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

[11]The base quintile variable and its interaction terms with VIOL are not reported in Table 4 in order to conserve space.

of the coefficient associated with VIOL × CDS is relatively unchanged compared to the baseline specifications. Moreover, the coefficient on VIOL × CDS is still statistically significant for the investment regression, and it is insignificant for the distance-to-default regression. These results suggest that our baseline results are unlikely to be driven by differences in firm size between CDS and non-CDS firms.

Differences along some other firm characteristics may also provide alternative explanations for our findings. First, one could argue that CDS firms have a higher borrowing capacity and leverage as CDS trading relieves debt financing frictions (Saretto and Tookes (2013), Chava, Ganduri, and Ornthanalai (2018)). Second, CDS trading has also been associated with higher cash reserves at firms (Subrahmanyam, Tang, and Wang (2017)). Finally, CDS trading may also be correlated with an increase in the firm's investment opportunity set. All these potential differences may allow CDS firms to suffer less after covenant violations relative to non-CDS firms.

To mitigate the above potential concerns, we follow the same empirical strategy that was used for firm size in column 1 of Table 4. We classify firms into quintiles-based leverage, macro q, and cash holdings, and then we interact the quintile variable with VIOL to include it as a control in our regression analysis. These results are reported in columns 2–4 and remain similar to our baseline results.[12]

2. Lender-Level Differences

This subsection investigates whether selection concerns associated with the type of banks that lend to CDS firms might confound our analysis. For instance, if the type of banks that lend to CDS firms are inherently more likely to respond differently when they gain control rights over a covenant-violating firm, then our results could be driven by bank type as opposed to CDS trading.

To mitigate this potential lender selection concern, we include LEAD_LENDER × YEAR fixed effects in our regressions, which control for time-varying changes in lender characteristics. In these specifications, VIOL × CDS is estimated by comparing how the same lead lender responds to a CDS firm and a non-CDS firm that violate a covenant in the same year.[13]

Table 5 reports that the magnitude of the estimated coefficient on VIOL × CDS remains relatively unchanged when compared to our baseline results reported in Tables 2 and 3. This result suggests that differences in lender-level characteristics are less likely to explain the differential response of CDS and non-CDS firms to covenant violations.

Overall, our results are robust to controlling for important observable differences in firm and lender characteristics that could potentially drive the differential response to covenant violations between CDS and non-CDS firms. However, as it is challenging to account for the complete set of observable and unobservable factors

[12]The results are also similar if we use the interaction of continuous variables for size, leverage, macro q, and cash with VIOL. Table IA.5 in the Supplementary Material reports that our results are robust to conducting a similar interaction analysis with analyst following, institutional ownership, and stock market liquidity.

[13]As we include Lead-lender × Year fixed effects in Table 5, we include Firm fixed effects as opposed to the Firm–Lead-lender fixed effects in our baseline specification of equation (1).

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TABLE 5

Controlling for Time-Varying Lender Characteristics

Table 5 reports the results for the sensitivity of investment and distance-to-default (DD) to covenant violations using a regression discontinuity design. The *Full Sample* consists of firm–lead-lender–quarter observations for firms that have experienced either a net worth or a current ratio covenant violation at least once during our sample period of 1996 to 2020. The *OB Sample* is a subset of the full sample within an optimal bandwidth of 0.399 for the DD sample and 0.359 for the investment sample computed using Imbens and Kalyanaraman (2012). VIOL is an indicator variable equal to 1 if a firm–lead-lender–quarter observation is determined to be in covenant violation, and 0 otherwise. The specifications for the investment and distance-to-default regressions include all the control variables from Tables 2 and 3, respectively. Standard errors are *double*-clustered at the firm and lead-lender level, and *t*-statistics are displayed in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable			
	INVESTMENT		log(DD)	
	Full	OB	Full	OB
	1	2	3	4
VIOL × CDS	0.013***	0.011**	0.009	0.038
	(2.91)	(2.26)	(0.10)	(0.55)
VIOL	−0.007*	−0.005**	−0.061	−0.096***
	(−1.89)	(−2.07)	(−1.36)	(−3.58)
CDS	0.008	0.007	−0.020	−0.067
	(1.08)	(0.67)	(−0.19)	(−0.54)
Firm FE	Yes	Yes	Yes	Yes
Calendar year–qtr FE	Yes	Yes	Yes	Yes
Fiscal qtr FE	Yes	Yes	Yes	Yes
Lead lender × Year FE	Yes	Yes	Yes	Yes
VIOL × Dist. poly.	Yes		Yes	
Controls	Yes	Yes	Yes	Yes
N	15,813	8,662	13,515	6,902
Adj. R^2	0.451	0.470	0.744	0.761

associated with CDS trading, we cannot completely eliminate the possibility of an omitted variable driving both CDS introduction and firm outcomes of covenant-violating firms.

IV. Additional Results

This section conducts additional analyses to support the main results. These analyses attempt to mitigate concerns associated with the data limitations on loan CDS and the inability to implement a fuzzy RDD design as mentioned previously. Section IV.A conducts analysis with the loan CDS that we were able to gather. Section IV.B implements a fuzzy RDD design for a subset of firms where we were able to identify lender intervention upon covenant violation.

A. Analysis with Loan CDS

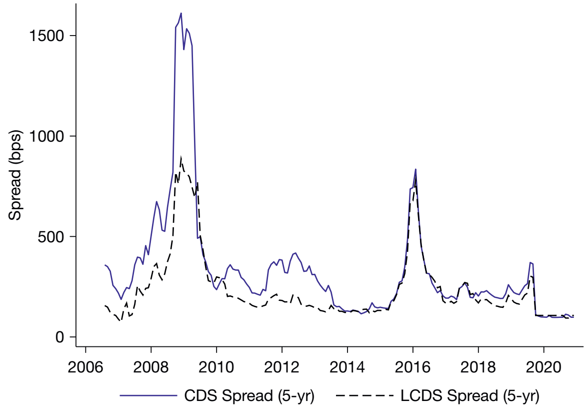
Our main analysis thus far relies on CDS contracts on bonds. However, there might be concerns about using CDS data on bonds when our analysis focuses on bank loans. Thus, to mitigate these concerns, we obtain data on loan credit default swap (LCDS) from Markit. To the best of our knowledge, these are the most comprehensive LCDS data that are available. However, these data cover only a subset of the bond CDS firms in our sample. Compared with the 83 firms with a bond CDS in our sample, we identify only 23 firms with an LCDS. More importantly, only 8 of these firms have an LCDS at the time of covenant violation. Even

FIGURE 4

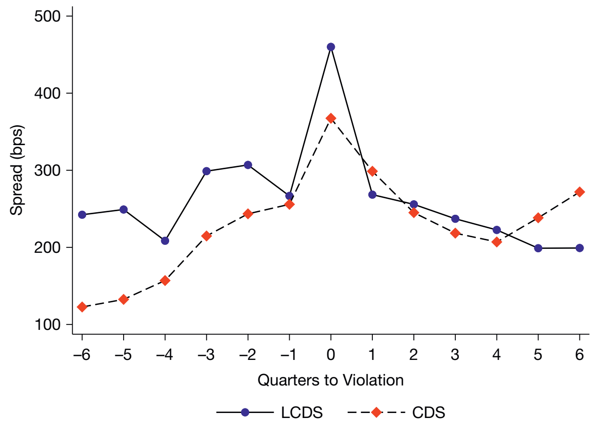
CDS and Loan CDS Spreads over Time

Figure 4 shows the loan credit default swap (LCDS) and credit default swap (CDS) spreads for firms. Both LCDS and CDS spreads have a tenor of 5 years, are of the no-restructuring contract type, and are associated with debt issued by U.S. firms with a senior lien. Graph A shows the spreads for the set of firms that have both a traded CDS and LCDS. Graph B shows the spreads for the subset of covenant-violating firms around the event.

Graph A. Spreads over Time



Graph B. Spreads Around Violation Event



though these LCDS data are limited and relatively illiquid, this section attempts to show the validity of our baseline results with these data.

Figure 4 shows LCDS and CDS spreads over time. Even though single-name LCDS contracts are relatively illiquid, the figure shows that at the aggregate level the two spreads are correlated. Figure 4 further shows that for the subset of covenant-violating firms where LCDS and CDS data are available, the two spreads increase together at the time of covenant violation. There is also existing research that connects these two markets. Kryzanowski, Perrakis, and Zhong (2021) investigate the price parity of LCDS and CDS markets. While they do find evidence of parity violations, there is a strong correlation between the two markets nonetheless.

Next, as a simple test, Table IA.6 in the Supplementary Material regresses the 5-year LCDS spread on the 5-year CDS spread to understand the comovement between LCDS and CDS spreads. LCDS and CDS for the same firm have different CDS spread levels due to differences in liquidity and recovery rates. Column 1 of Table IA.6 reports this regression without any controls, and column 2 includes firm and year-month fixed effects to estimate a within-firm comovement. Both columns suggest that LCDS and CDS spreads are correlated. Column 2 suggests that a 1-pp increase in CDS spreads is associated with a 0.41-pp increase in the LCDS spreads of the firm. Overall, there is evidence of a significant positive correlation between the two types of credit default swap (CDS) spreads.

There are several reasons for the positive correlation between CDS and LCDS spreads, thus helping provide an economic justification for the use of CDS in our analysis. First, practitioners suggest that many firms have cross-default provisions in their bond and loan contracts, which allows for both the LCDS and the CDS to be priced using the same default curve (see https://www.markit.com/news/Official_LCDX_Marketing_Presentation.pdf). Second, existing articles show how CDSs are associated with bank loans. Ivanov, Santos, and Vo (2016) discuss the tying of bank loan interest rates to borrower's CDS spreads. Minton, Stulz, and Williamson (2009) provide excerpts from banks' annual reports on their use of CDS to hedge bank loan portfolios. This evidence is broadly consistent with regulatory and contractual features of the CDS market. For instance, Basel II and Basel III allow banks to use bond CDS contracts to mitigate the credit risk exposure of their loan portfolios due to the greater coverage and liquidity of the bond CDS market, and because bonds are junior obligations to loans (see paragraph 191(g) in Basel Committee on Banking Supervision at https://www.bis.org/publ/bcbs128.pdf). Furthermore, the "failure to pay" credit event in the standardized bond CDS contracts provided by ISDA accounts for nonpayment on a broad set of firm obligations, which includes bank loans (see Section 4.5 "Failure to Pay" in the 2014 ISDA Master Agreement).

Next, despite limited data, Table 6 conducts our main tests with the subsample of firms that have an LCDS. The control group includes firms without a traded LCDS or CDS. Identification of the interaction term VIOL × LCDS is obtained from only 8 firms that violate covenants and have an LCDS. Hence, to conserve statistical power, we reduce the set of controls in these analyses. Based on insights from previous literature on the typical differences between CDS and non-CDS, we include all the fixed effects from our baseline specification and control for firm size, leverage, cash, and the presence of a credit rating (Ashcraft and Santos (2009), Saretto and Tookes (2013), and Subrahmanyam et al. (2017)). Excluding all controls and including just the fixed effects also leads to similar results.

Table 6 reports the results. Column 1 shows that, as in Table 2 with bond CDS, in the presence of covenant violations, covenant-violating firms with loan CDS experience smaller investment cuts. Column 2 confirms these findings using a sample within an OB. The next 2 columns focus on distance-to-default. Similar to the findings in Table 3, we find positive although statistically insignificant coefficients, in both the specifications. Thus, covenant-violating firms in the presence of loan CDS trading do not move closer to default relative to covenant-violating firms in the absence of loan CDS trading.

TABLE 6

Robustness Using Loan CDS

Table 6 reports the results for the sensitivity of investment and distance-to-default (DD) to covenant violations using a regression discontinuity design. The sample consists of firm–lead-lender–quarter observations for firms that have experienced either a net worth or a current ratio covenant violation at least once during our sample period of 1996 to 2020. VIOL is an indicator variable equal to 1 if a firm–lead-lender–quarter observations is determined to be in covenant violation, and 0 otherwise. LCDS is an indicator variable equal to 1 if there is a traded loan credit default swap contract for that firm–quarter observation. The specifications for the investment and distance-to-default regressions include assets, leverage, cash, and an indicator variable for whether the firm is rated. Standard errors are *double*-clustered at the firm and lead-lender level, and *t*-statistics are displayed in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable			
	INVESTMENT		log(DD)	
	Full	OB	Full	OB
	1	2	3	4
VIOL × LCDS	0.009**	0.020*	0.171	0.090
	(2.08)	(1.80)	(1.41)	(0.66)
VIOL	−0.007	−0.010***	−0.026	−0.179***
	(−1.60)	(−4.93)	(−0.48)	(−6.08)
LCDS	0.008**	−0.011	−0.232**	−0.250
	(2.15)	(−1.03)	(−2.07)	(−1.40)
Firm–lead-lender FE	Yes	Yes	Yes	Yes
Calendar year–Qtr FE	Yes	Yes	Yes	Yes
Fiscal qtr FE	Yes	Yes	Yes	Yes
VIOL × Dist. poly.	Yes		Yes	
Controls	Yes	Yes	Yes	Yes
N	14,702	8,748	12,446	7,552
Adj. R^2	0.378	0.388	0.633	0.646

Overall, the point estimates of the VIOL × LCDS term again suggest that following the inception of LCDS on a firm's debt, corporate investment decline post-violation is mitigated. Furthermore, we again find that covenant-violating firms in the presence of LCDS trading do not move closer to default relative to covenant-violating firms in the absence of LCDS trading.

B. A Fuzzy RDD Approach

Ideally, our empirical RDD framework should resemble a fuzzy RDD setup. This is because covenant violations do not necessarily imply lender intervention in firms. For instance, lenders can grant unconditional waivers to violating firms without renegotiating or requiring any changes to loan terms. Thus, violating a covenant only increases the probability of lender intervention (i.e., treatment) discontinuously around the covenant violation threshold. Unfortunately, we are unable to implement the fuzzy RDD design in our baseline model because we do not observe whether a lender actually intervenes in firm operations following a covenant violation. This data limitation is also a characteristic of previous studies that explore firm responses' to covenant violations (e.g., Chava and Roberts (2008), Nini et al. (2012)).

In order to get closer to the ideal empirical framework, we implement the fuzzy RDD design using a subset of firms for which we may be able to identify lender intervention upon covenant violation. While imperfect, we use two methods to identify covenant violations that led to lender intervention and describe these methods in detail in Section B of the Supplementary Material. First, we use the

DealScan Facility Amendment data set to identify a firm's loan facilities that were amended after the firm's covenant violation. Second, many amended loans are also entered into DealScan as new loans (Roberts (2015)). To capture these loans, we identify new loans in DealScan that were made to covenant-violating firms within 1 year after covenant violation by the same lead lender who had a covenant violated. Additionally, we require the new loans to be of the same type (e.g., term loans and revolvers) as the loans affected by the covenant violations in our sample. If we are able to identify such amended or renegotiated loans using the above two steps, we classify the covenant violation to be associated with lender intervention. Based on the above 2-step procedure, we are able to classify 17% of covenant violations to be associated with lender intervention.[14] However, it is important to note that our classification is imperfect as the failure to identify any amended or renegotiated loans need not necessarily imply nonintervention by the lenders.

Next, in similar spirit to our baseline model in equation (1), we estimate the following 2-stage least squares (2SLS) model for the fuzzy RDD design:

$$(2) \qquad Y_{i,b,t} = \begin{array}{l} \alpha + \beta_1 \times \text{INTERVENTION}_{i,b,t-1} + \beta_2 \times \text{INTERVENTION}_{i,b,t-1} \\ \times \text{CDS}_{i,t} + \beta_3 \times \text{CDS}_{i,t} + f(X_{i,b,t-1}) + \Gamma_{i,b} + \tau_t + \delta_t + \varepsilon_{i,b,t}, \end{array}$$

where $\text{INTERVENTION}_{i,b,t-1}$ equals 1 if lender b intervenes in firm i in year-quarter $t-1$. The variables $\text{INTERVENTION}_{i,b,t-1}$ and $\text{INTERVENTION}_{i,b,t-1} \times \text{CDS}_{i,t}$ are instrumented using $\text{VIOL}_{i,b,t-1}$ and $\text{VIOL}_{i,b,t-1} \times \text{CDS}_{i,t}$. The rest of the variables are defined as in equation (1). The 2SLS model is identified under the assumption that covenant violations (VIOL) affect firm outcomes Y only through lender intervention (INTERVENTION) (i.e., the exclusion restriction).

Table 7 reports the results. The results are qualitatively consistent with baseline results. As expected, the magnitudes are similar to our baseline estimates scaled by the propensity of lender intervention obtained from first-stage fuzzy RDD estimates. The coefficient on INTERVENTION in column 1 suggests that lender intervention after covenant violations leads to a reduction in firm investment on average. However, as seen from the coefficient on INTERVENTION × CDS, in the presence of CDS trading, firms are able to maintain higher investment levels. Column 2 shows that our results are robust to employing the fuzzy RDD approach with an OB sample.

Column 3 of Table 7 reports that, on average, the default likelihood increases for firms when lenders intervene after covenant violations. By contrast, the positive and insignificant coefficient on INTERVENTION × CDS in column 3 suggests that covenant-violating CDS firms do not move closer to default. Column 4 employs an OB sample and presents similar results.

The bottom of Table 7 reports the estimated coefficients from the first stage of the 2SLS regression (i.e., the relevance condition). In general, the first-stage estimates show that the likelihood of lender intervention increases after covenant violations. Moreover, in the presence of CDS trading, this likelihood of lender intervention is higher than in the absence of CDS trading. Thus, the incentives of

[14]In comparison, Barakova, Hasan, and Parthasarathy (2018) find that 15% of public firms are denied a waiver or amendment.

TABLE 7

A Fuzzy Regression Discontinuity Design Approach

Table 7 reports the results for the sensitivity of investment and distance-to-default (DD) to covenant violations using a fuzzy regression discontinuity design. The sample consists of firm–lead-lender–quarter observations for firms that have experienced either a net worth or a current ratio covenant violation at least once during our sample period of 1996 to 2020. INTERVENTION is an indicator variable equal to 1 if a covenant violation leads to lender intervention. Lender intervention is identified based on amended and renegotiated loans using the procedure described in Section IV.B. VIOL is an indicator variable equal to 1 if a firm–lead-lender–quarter observation is determined to be in covenant violation, and 0 otherwise. CDS is an indicator variable equal to 1 if there is a traded credit default swap contract for that firm–quarter observation. The bottom of the table reports the first-stage regression coefficients for INTERVENTION and the Kleibergen–Paap Wald rk *F*-statistic for the weak identification test with critical values ranging from 3.6 to 7. The specifications for the investment and distance-to-default regressions include all the control variables from Tables 2 and 3, respectively. Standard errors are *double*-clustered at the firm and lead-lender level, and *t*-statistics are displayed in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable			
	INVESTMENT		log(DD)	
	Full	OB	Full	OB
	1	2	3	4
INTERVENTION × CDS	0.073***	0.097***	0.335	0.486
	(3.11)	(3.39)	(1.15)	(1.51)
INTERVENTION	−0.028*	−0.049***	−0.478**	−0.781***
	(−1.72)	(−3.08)	(−2.34)	(−3.63)
CDS	0.014**	0.003	0.164*	0.111
	(2.12)	(0.40)	(1.77)	(0.89)
Firm–lead-lender FE	Yes	Yes	Yes	Yes
Calendar year–qtr FE	Yes	Yes	Yes	Yes
Fiscal qtr FE	Yes	Yes	Yes	Yes
Dist. poly.	Yes		Yes	
Controls	Yes	Yes	Yes	Yes
N	16,133	8,954	13,807	7,152
Adj. R^2	0.065	0.014	0.167	0.116
First-stage coefficients				
VIOL	0.155***	0.149***	0.157***	0.157***
	(7.11)	(7.24)	(6.90)	(7.10)
VIOL × CDS	0.106*	0.118*	0.198***	0.171***
	(1.82)	(1.82)	(3.19)	(3.32)
K–P Wald rk *F*-stat	25.63	26.52	24.85	25.40

lenders to intervene are present for CDS firms too. The Kleibergen–Paap rk Wald *F*-statistics reported at the bottom of the table reject the hypothesis that our instruments are weak.

Overall, the fuzzy RDD results in Table 7 provide similar inferences as our baseline results. However, as these results are based on a small subset of firms where we can identify lender intervention, one needs to exercise caution while generalizing these results to the broader sample.

V. Testing the Disciplining Effect Associated with CDS Trading

Section IV shows that our results are consistent with the disciplining effect, which suggests that CDS trading mitigates agency frictions between banks and firms. As a result, lenders are less likely to reduce the investment of a covenant-

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violating firm in the presence of CDS trading. In this section, we provide additional evidence that supports the disciplining effect of CDS as the potential channel for our results.

A. Exploiting Variation in CDS Liquidity

Greater CDS liquidity should make it easier for lenders to buy CDS protection. Thus, if the disciplining effect is the main channel for our results, then this disciplining effect should be greater for more liquid CDS contracts. That is, the post-violation investment cut should be smaller for firms that have greater CDS liquidity. In contrast, in the presence of the empty credit problem, the default probability should be greater for firms that have greater CDS liquidity.

We test these predictions in Table 8. We compute a measure for CDS liquidity motivated by the Amihud (2002) illiquidity measure using the daily CDS spread changes and the number of CDS dealer quotes (see Section A of the Supplementary Material for details). Next, we divide the CDS firms into two groups (namely high and low) based on the median value of the illiquidity measure. We then estimate our baseline specification in equation (1) by substituting the CDS indicator with

TABLE 8

Exploiting Variation in CDS Trading Liquidity

Table 8 reports the results for the sensitivity of investment and distance-to-default (DD) to covenant violations using a regression discontinuity design. The *Full Sample* consists of firm–lead-lender–quarter observations for firms that have experienced either a net worth or a current ratio covenant violation at least once during our sample period of 1996 to 2020. The *OB Sample* is a subset of the full sample within an optimal bandwidth of 0.399 for the DD sample and 0.359 for the investment sample, computed using Imbens and Kalyanaraman (2012). VIOL is an indicator variable equal to 1 if a firm–lead-lender–quarter observation is determined to be in covenant violation, and 0 otherwise. CDS_LIQUIDITY is a categorical variable equal to 0 for non-credit default swap (CDS) firms (omitted category), and equal to 1 = LOW (below median) and 2 = HIGH (above median), respectively, for the low and high liquidity of traded CDS contracts. The specifications for the investment and distance-to-default regressions include all the control variables from Tables 2 and 3, respectively. Standard errors are *double*-clustered at the firm and lead-lender level, and *t*-statistics are displayed in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable			
	INVESTMENT		log(DD)	
	Full	OB	Full	OB
	1	2	3	4
VIOL × CDS_LIQUIDITY(HIGH)	0.025***	0.020***	0.180**	0.054
	(3.06)	(3.98)	(1.98)	(0.46)
VIOL × CDS_LIQUIDITY(LOW)	0.010**	0.018***	−0.092	0.026
	(2.27)	(3.51)	(−0.79)	(0.23)
VIOL	−0.008**	−0.007***	−0.042	−0.123***
	(−2.07)	(−3.62)	(−0.88)	(−4.71)
CDS_LIQUIDITY(HIGH)	0.013*	0.009	0.204**	0.236
	(1.65)	(0.88)	(2.01)	(1.46)
CDS_LIQUIDITY(LOW)	0.016**	0.006	0.112	0.045
	(2.43)	(0.74)	(0.96)	(0.33)
Firm–lead-lender FE	Yes	Yes	Yes	Yes
Calendar year–qtr FE	Yes	Yes	Yes	Yes
Fiscal qtr FE	Yes	Yes	Yes	Yes
VIOL × Dist. poly.	Yes		Yes	
Controls	Yes	Yes	Yes	Yes
N	16,133	8,954	13,807	7,152
Adj. R^2	0.406	0.428	0.680	0.698

indicators for the two types of firms based on CDS illiquidity and the baseline non-CDS traded group. Table 8 reports the results. As before, we include firm–lead-lender fixed effects and firm characteristics as controls, and also estimate the specifications using the global polynomial and the OB methods.

Columns 1 and 2 of Table 8 suggest that firms with higher CDS liquidity *increase* their investment after covenant violation. Columns 3 and 4 report the results for distance-to-default as the outcome variable. The results indicate that firms with more liquid CDSs, if anything, move further away from default when banks gain control rights. Overall, the results reported in Table 8 provide evidence that supports the disciplining effect of CDS, and they provide further evidence against the existence of the empty creditor problem effect of CDS in the bank lending market.

B. Exploiting Variation in Agency Frictions and Lender Reputation

Chava and Roberts (2008) show that lenders cut firm investment more when they face greater information frictions with respect to the covenant-violating firm. For instance, consistent with greater agency conflicts associated with higher cash holding (i.e., the free cash flow hypothesis of Jensen (1986)), Chava and Roberts (2008) show that covenant violations result in greater investment cuts when the violating firm has higher cash holdings. However, if CDS disciplines firms, then this disciplining effect should lead to a smaller investment cut after covenant violations even for firms facing higher information frictions with respect to their lending banks.

We test these predictions in Table 9. We divide the CDS firms based on their cash holdings into two groups, namely high-cash (above median) and low-cash (below median) groups. We then estimate our baseline specification in equation (1) by interacting the indicator variable for the high-cash group with VIOL × CDS.[15]

Column 1 of Table 9 reports how the sensitivity of investment to covenant violations varies with the cash holdings of the violating firm. First, consistent with Chava and Roberts (2008), we find that after covenant violations, firms with higher cash holdings experience a 1.6% larger reduction in investment as seen from the coefficient on VIOL × HIGH.

In contrast, the positive and statistically significant coefficient on VIOL × CDS × HIGH suggests that in the presence of CDS trading, the high-cash firms do not experience a reduction in investment after covenant violations. This result is consistent with lenders perceiving high-cash CDS firms as less subject to the information frictions that are typically associated with high-cash holdings. For completeness, column 3 of Table 9 estimates the triple interaction specification for distance-to-default. The results show that the presence of CDS trading for high-cash covenant-violating firms does not lead to an increase in their default likelihood. In sum, the evidence in columns 1 and 3 is consistent with the disciplining effect of CDS.

[15] In Table 9, we include firm fixed effects and lead-lender fixed effects instead of firm–lead-lender fixed effects to exploit greater variation in cash holdings across firms and reputation across lenders.

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TABLE 9

Exploiting Variation in Agency Frictions and Lender Reputation

Table 9 shows the results for the sensitivity of investment and distance-to-default (DD) to covenant violations using a regression discontinuity design. The sample consists of firm–lead-lender–quarter observations for firms that have experienced either a net worth or a current ratio covenant violation at least once during our sample period of 1996 to 2020. VIOL is an indicator variable equal to 1 if a firm–lead-lender–quarter observation is determined to be in covenant violation, and 0 otherwise. CDS is an indicator variable equal to 1 if there is a traded credit default swap contract for that firm–quarter observation. The indicator variable HIGH in columns 1 and 3 equals 1 for covenant-violating firms with above-median cash holdings, and equals 0 otherwise. Following Gopalan et al. (2011), the indicator variable HIGH in columns 2 and 4 equals 1 for high-reputation lenders (i.e., 0 scaled bankruptcies) and equal to 0 for low-reputation lenders (i.e., scaled bankruptcies > 0.1). Scaled bankruptcies for a lead lender in a given year is defined as the scaled total loan amount (both lent and outstanding) the lead lender has granted borrowers who have filed for bankruptcy in that year. The scaling factor is the lead lender's average annual amount lent to all its borrowers over the past 2 years. The specifications for the investment and distance-to-default regressions include all the control variables from Tables 2 and 3, respectively. Standard errors are *double*-clustered at the firm and lead-lender level, and *t*-statistics are displayed in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable			
	INVESTMENT		log(DD)	
	Cash	Reputation	Cash	Reputation
Group Variables	1	2	3	4
VIOL × CDS × HIGH	0.027***	0.016	0.174	0.248
	(2.91)	(1.12)	(0.93)	(1.31)
VIOL × CDS	0.005	0.004	−0.014	−0.175
	(0.71)	(0.54)	(−0.14)	(−1.15)
VIOL × HIGH	−0.016***	−0.000	−0.021	0.107
	(−4.31)	(−0.12)	(−0.37)	(1.47)
VIOL	−0.001	−0.008	−0.007	−0.186**
	(−0.17)	(−1.35)	(−0.11)	(−2.39)
CDS × HIGH	−0.005	−0.012	0.029	0.066
	(−0.73)	(−1.64)	(0.26)	(0.74)
CDS	0.013*	0.016	0.081	−0.055
	(1.77)	(1.57)	(0.87)	(−0.50)
HIGH	0.007*	−0.002	−0.041	0.027
	(1.75)	(−0.58)	(−0.65)	(0.39)
Firm FE	✓	✓	✓	✓
Lead-lender FE	✓	✓	✓	✓
Calendar year–qtr FE	✓	✓	✓	✓
Fiscal qtr FE	✓	✓	✓	✓
VIOL × Dist. poly.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	✓
N	16,133	8,349	13,807	7,026
Adj. R^2	0.397	0.438	0.665	0.696

Another important element in the private debt market is a lender's reputation. A lender's reputational capital in the loan syndication market can be negatively impacted if the lender forces borrowing firms into default. Consistent with this idea, Gopalan et al. (2011) show that lead banks suffer a reputation loss in the loan syndication market when their borrowing firms file for bankruptcy.

Thus, in columns 2 and 4 of Table 9, we test whether the sensitivity of investment and default likelihood to covenant violations varies with lender reputation. We follow Gopalan et al. (2011) and construct the reputation measure for a lender based on the lender's scaled bankruptcies.[16] Following Gopalan et al. (2011),

[16]Scaled bankruptcies for a lead lender in a given year is defined as the scaled total loan amount (both lent and outstanding) the lead lender has granted borrowers who have filed for bankruptcy in that year. The scaling factor is the lead lender's average annual amount lent to all its borrowers over the past 2 years.

high-reputation lenders are lenders that have 0 scaled bankruptcies, whereas *low-reputation* lenders are lenders that have scaled bankruptcies greater than 0.1.

Although statistically insignificant, the positive coefficients on VIOL × CDS × HIGH and VIOL × CDS in columns 2 and 4 of Table 9 indicate that when covenant-violating firms in the presence of CDS trading borrow from high-reputation lenders, they are less likely to experience an increase in their default likelihood and more likely to maintain higher investment levels. Overall, these results suggest that greater reputation concerns among bank lending syndicates mitigate the empty creditor problem of CDS in the private debt market.

VI. Conclusion

CDSs, which allow investors to trade and transfer credit risk, can misalign the incentives between a lender and a borrower. In the extreme, CDS can lead to an empty creditor problem, where lenders can force firms into bankruptcy. Previous literature finds supporting evidence for the empty creditor problem in the public debt market.

Our article shows that the effect of CDS on distressed firms crucially depends on the financing source. We document that in the case of bank financing, the presence of CDS trading does not lead to adverse effects on distressed borrowing firms. Furthermore, during debt renegotiations, banks also allow distressed firms in the presence of CDS trading to maintain or even increase investment. Together, our results suggest that the presence of CDS trading can help discipline borrowing firms and mitigate agency frictions, thereby improving the incentive alignment between lenders and borrowers. Our evidence is consistent with the often overlooked predictions in the theoretical models of Bolton and Oehmke (2011) and Campello and Matta (2020), which suggest that the higher bargaining power obtained by lenders due to CDS can discipline borrowers and thereby avoid underinvestment and excessive defaults.

Overall, our results suggest that CDS could be beneficial in the bank lending market, which is an important source of firm financing that is also characterized by frequent debt renegotiations through covenant violations. We add to the policy debate on the regulation of CDS markets by documenting a beneficial role of CDS. Most importantly, our article highlights how financial innovation can have contrasting economic effects in the public and private lending markets.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109022000709.

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