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Identifying empty creditors with a shock and micro-data

Hans Degryse
(KU Leuven and CEPR)

Yalin Gündüz
(Deutsche Bundesbank)

Kuchulain O'Flynn
(University of Zurich
and Swiss Finance Institute)

Steven Ongena
(University of Zurich,
Swiss Finance Institute,
KU Leuven and CEPR)

Deutsche Bundesbank, Wilhelm-Epstein-Straße 14, 60431 Frankfurt am Main,
Postfach 10 06 02, 60006 Frankfurt am Main

Tel +49 69 9566-0

Please address all orders in writing to: Deutsche Bundesbank,
Press and Public Relations Division, at the above address or via fax +49 69 9566-3077

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Non-technical summary

Research Question

Credit default swaps (CDSs) have been subject to intense criticism related to both the opacity regarding the holder of the ultimate exposures as well as their role in the financial crisis. In this paper, we use CDS trading data combined with a quasi-natural experiment to reveal a particular negative side effect of CDS trading on referenced firms – the "empty creditor" effect. We show that firms with CDSs traded on their debt have a higher probability of default when exposed to creditors that trade CDSs on them, and thus have an incentive to push them into bankruptcy in order to collect their CDS insurance payment.

Contribution

This paper contributes to the literature on the effects of CDS trading on the underlying firms in two important ways. First, by employing a quasi-natural experiment that removes the empty creditor effect for some firms, we are able to disentangle the effect of empty creditors from other potentially confounding effects. This distinction is important, as firms that are exposed to the empty creditor effect experience an involuntary increase in their probability of default, while they may have control over their exposure to other effects of CDS trading. Second, we shed light on the firm and bank-firm relationship characteristics that increase the intensity of the impact of empty creditors on CDS firms. In particular, we are able to directly analyse how the intensity of a firm's creditors' CDS trading affects the potency of the empty creditor effect.

Results

We find that the probability of default for firms drops on average by about 2 percentage points when the effect of empty creditors is removed. Importantly, we also show that this effect becomes larger the more CDSs a firm's creditors purchase. The extant literature was unable to analyse this effect due to the prior lack of data on banks' CDS and credit positions. Finally, we show that intensity of this empty creditor effect varies with firm, bank-firm, and creditor hedging characteristics.

Nichttechnische Zusammenfassung

Fragestellung

Kreditausfallswaps (Credit Default Swaps – CDS) stehen in der Kritik, was zum einen an der mangelnden Transparenz hinsichtlich der Frage, wer letztlich das Risiko trägt, und zum anderen an ihrer Rolle in der Finanzkrise liegt. Im vorliegenden Beitrag wird der Effekt untersucht, der von Gläubigern ausgeht, deren Risiko gegenüber einem Schuldner durch Gegengeschäfte mit CDS beseitigt wurde (Empty Creditors). Es könnte sein, dass die Ausfallwahrscheinlichkeit von Unternehmen steigt, wenn solche Gläubiger mit deren CDS handeln, weil diese einen Anreiz haben, die Unternehmen in die Insolvenz zu drängen, um ihre Versicherungsleistung aus dem CDS-Kontrakt zu erhalten.

Beitrag

Das vorliegende Papier leistet in zweifacher Hinsicht einen Beitrag zur Fachliteratur über die Auswirkungen des CDS-Handels. Erstens wird ein quasi-natürliches Experiment herangezogen. Dadurch lässt sich der Effekt durch Empty Creditors von anderen möglicherweise verzerrenden Einflüssen trennen. Zweitens wird beleuchtet, welche Unternehmensmerkmale und welche Arten von Firmen-Bank-Beziehungen den Empty-Creditor-Effekt verstärken.

Ergebnisse

Es wird aufgezeigt, dass sich die Ausfallwahrscheinlichkeit eines Unternehmens um 2 Prozentpunkte verringert, wenn der Effekt durch Empty Creditors herausgerechnet wird. Als wichtiges Ergebnis hat sich zudem herausgestellt, dass der Empty-Creditor-Effekt umso stärker ausfällt, je mehr CDS die Gläubiger eines Unternehmens erwerben. Schließlich wird auch aufgezeigt, dass das Ausmaß des Empty-Creditor-Effekts variiert, je nachdem, welche Merkmale die Unternehmen, die Banken-Firmen-Beziehung und die Absicherung des Gläubigers aufweisen.

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Hans Degryse
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University of Zurich,
and Swiss Finance Institute

Steven Ongena
University of Zurich,
Swiss Finance Institute,
KU Leuven and CEPR

Abstract

Firms with credit-default swaps (CDS) traded on their debt may face "empty creditors" as hedged creditors have less incentive to participate in firm restructuring. We test for the existence of empty creditors by employing an exogenous change to the bankruptcy code in Germany that effectively removes their potential impact on CDS firms. Using a unique dataset on bank-firm CDS net notional and credit exposures we find that the probability of default for CDS firms drops when the effect of empty creditors is removed. This effect increases in the average CDS hedge position of a firm's creditors and in the concentration of the firm's debt. Firms with longer credit relationships, with higher average collateral ratios of their debt, and financially safer firms are less affected by empty creditors. Banks that are not capital constrained and that are liquidity constrained embed the empty creditor effect into their probability of default estimates of affected firms to a larger extent. So do banks that monitor their creditors less and that earn a smaller portion of their income from interest activities.

Keywords: Empty creditors, default, bankruptcy, credit default swaps, micro-data.

JEL classification: G21, G33, G38.

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†Corresponding author: Universität Zürich, Institut für Banking und Finance, Plattenstrasse 14, CH-8032 Zürich, Switzerland. Email: kuchulain.oflynn@bf.uzh.ch.

1 Introduction

Credit Default Swaps (CDS) allow investors to trade on a firm's credit risk. Creditors of a referenced entity (i.e., a firm that issued debt on which a CDS trades), could become "empty creditors" through the hedging of their credit risk in the CDS market. CDS have been subject to intense criticism related to the opacity of who holds the ultimate exposures and for their role in the financial crisis.[1] While these criticisms may be warranted, to properly determine if a financial product is beneficial, the costs and benefits associated with that product need to be understood.[2] For a bank, for example, the ability to lay off credit risk should be weighed against the incentives to monitor its borrowers (Parlour and Winton, 2013). In this paper, we focus how the referenced firm is affected by the legal scope of credit events in CDS contracts (over which the firm has no control).

Creditors hold control rights under the debt contract. CDS may change the relationship between creditors and borrowers as the formal ownership of debt claims can be decoupled from the economic exposure to credit deterioration. Creditors who purchase "no-restructuring" CDS contracts have an increased incentive to push the CDS referenced entity into bankruptcy to collect the payout from the CDS contract. Restructuring is not a credit event in case of a no-restructuring CDS contract. This gives rise to "empty creditors" who have less incentive to accommodate in firm restructuring (Bolton and Oehmke, 2011). In contrast to creditors, other parties with positions in CDS are not involved in a firm's restructuring or bankruptcy decisions and thus do not have control rights. This interplay between empty creditors and other parties may thereby determine the likelihood and path of restructuring and bankruptcy decisions. In this paper, we use a change in German insolvency law as a quasi-natural experiment to identify if and how the presence of empty creditors causes changes in firms' probability of default.

The potency of the empty creditor effect crucially relies on the combination between the creditors' ability to restructure a firm and the recognition of restructuring as a credit event in the CDS contract. Standard North-American CDS contracts only consider bankruptcy as a credit event. Besides bankruptcy, Standard European CDS contracts also feature restructuring as a credit event (ISDA, 2009a,b).[3] Prior to 2012, restructuring (in- or out-of-court) was close to unachievable under German insolvency law, implying that CDS on German firms were de facto Standard North-American contracts as they

[1] Referring to CDS, Pope Francis has stated that "The spread of such a kind of contract without proper limits has encouraged the growth of a finance of chance, and of gambling on the failure of others, which is unacceptable from the ethical point of view" (Rennison, 2018). See Stulz (2010) for a detailed discussion on the role of CDS in the crisis.

[2] See Augustin, Subrahmanyam, Tang, and Wang (2016) for an overview of the literature on the costs and benefits of CDS. The authors point out the need for more research to be conducted on the topic to attain a better understanding of the welfare implications.

[3] This is confirmed empirically by the DTCC Trade Information Warehouse Market Activity Reports. These reports show that, in almost all time periods, "> 95%" of all transactions on European reference entities were executed with restructuring clauses applying (DTCC, 2020).

only recognized bankruptcy as a credit event.[4]

Prior to 2012, German firms with CDS traded on them were thus, potentially, exposed to empty creditors. The modification of the German insolvency law in 2012 (the ESUG reform act) made restructuring a credit event, bringing them in line with Standard European CDS contracts.[5] In particular, the legal change introduces debt-equity swaps in restructuring, activating the restructuring pay-out clause on German reference entities. After the modification of the German insolvency law, creditors which purchase restructuring CDS contracts, under normal circumstances, have a decreased incentive to push the CDS referenced entity into bankruptcy. The reason is that these CDSs also pay out when the referenced entity merely restructures its debt (i.e., "before" bankruptcy). Thus, the legal change weakened the effect of empty creditors for these entities. This quasi-natural experiment therefore allows us to identify the impact of empty creditors on firms' probability of default. In particular, the removal of the impact of empty creditors for German reference entities should have led to a decrease in their probability of default around this event relative to other similar German firms that do not have CDS traded on them. German firms that do not have CDS traded on them are also exposed to this reform but they were never affected by empty creditors in the first place.

The literature on the cost and benefits of CDS for the referenced firms focuses on the role of CDS in debt renegotiations. Bolton and Oehmke (2011), for example, model how creditors who are able to purchase CDS improve their bargaining power in restructuring negotiations. In their model, CDS then act as a commitment device as creditors are more easily able to force bankruptcy when firms strategically default (e.g., through cash-flow manipulation). This reduces a firm's incentive to strategically default, which, ex-ante, increases its financing capacity. The improved financing capacity leads to an increase in firms' investment, leverage, value, and a decrease in their cost of debt. However, Bolton and Oehmke (2011) show that, in equilibrium, creditors will "over-insure" (i.e., purchase more CDS on a firm than is socially optimal). This increases the probability of default for these firms. The latter reflects the impact of empty creditors on CDS firms. The increase in the probability of default, due to over-insurance, may lead to a decrease in the financing capacity of firms. This leads to an ambiguous effect of CDS trading on firms financing capacity. Similarly, the increase in leverage may lead to an increase in

[4] Out-of-court restructuring was close to unachievable in German prior to 2012 as German insolvency law automatically triggers a formal bankruptcy if a firm faces insolvency, if an insolvency is imminent, or if a firm is over-indebted. Further, there were several institutional biases against workouts (Jostarndt and Sautner, 2010). For example, if the management of a firm does not file for bankruptcy in a timely manner, they may be held personally liable, and may be sentenced for up to three years in prison. This limits the time in which a workout can be arranged and hence makes out-of-court restructuring less likely. The pertinence of this law can be seen in the response of the German Federal Ministry of Justice and Consumer Protection to the economic consequences of the COVID-19 pandemic. Part of the ministry's response was to pass legislation temporarily suspending the obligation to file for insolvency with the "COVID-19 Insolvency Suspension Act" (COVInsAG - BGBl. I S. 569). The result of this suspension of obligation was a 13.1 percent reduction in the number of business filings for insolvency during first three quarters of 2020 compared to the same period in 2019 (Destatis, 2020).

[5] "Gesetz zur weiteren Erleichterung der Sanierung von Unternehmen" - BGBl. I S. 2582.

the probability of default for these firms. Thus, when considering both the commitment effect of CDS and the empty creditor effect, the costs and benefits of CDS are ambiguous, even theoretically.[6]

We focus on the Bolton and Oehmke (2011) model as it allows us to disentangle the increase in a referenced firm's probability of default that is a result of the empty creditor effect from the increase that is a result of the firm increasing their leverage through the commitment effect. This distinction is important, as firms have control over the effect of CDS on their probability of default through the commitment effect. This is because they make the decision to use the increased financing capacity by increasing their leverage and hence their probability of default. Contrarily, firms which are exposed to the empty creditor effect experience an increase in their probability of default without their consent.

To empirically identify the impact of empty creditors, some researchers rely on the initiation of CDS trading on a firm (Subrahmanyam, Tang, and Wang, 2014; Colonnello, Efing, and Zucchi, 2019). This initiation, however, can be endogenous as CDS may be introduced on a referenced entity when a firm's probability of default is increasing. Other researchers employ the implementation of the CDS Big Bang Protocol, which essentially removes restructuring as a credit event (Danis, 2016). This implementation may fail to identify the effect of empty creditors as any increase in the probability of default could also be a result of the commitment effect causing an increase in leverage, and thus the probability of default.[7] We resolve these issues by employing an exogenous shock to the bankruptcy law of Germany which specifically changes the restructuring law. We are thus able to identify the effect of CDS trading on a firm's probability of default which is the result of exposure to empty creditors.

Our empirical analysis draws upon multiple datasets providing us with all relevant information. We obtain CDS position data at the creditor-firm level from the Depository Trust & Clearing Corporation (DTCC), and combine this with creditor positions from the German Credit Registry (MiMik). We then merge this with a database containing detailed firm information (USTAN), and another dataset containing CDS spreads (Markit). We supplement this dataset with quarterly bank balance sheet (BISTA), and income statement information (GuV), respectively. Lastly, we combine the resulting dataset with macroeconomic data (DataStream).

The impact of empty creditors can be expected to primarily manifest itself at the firm level; hence, we collapse the dataset to this level. When collapsing from bank-firm

[6] In this paper we use the terms "commitment effect" and "empty creditor effect" to reference what may be called the ex-ante and ex-post empty creditor effect, respectively. We use this terminology to highlight the fact that the commitment effect only requires creditors to be hedged and not necessarily to be "empty".

[7] More generally, the use of North American CDS reference entities in a study of the empty creditor effect may result in this issue. While Danis (2016) does not study the probability of default to measure the effect of empty creditors, the likelihood of a successful restructuring may still be related to firm leverage.

to firm level, we weigh each bank-firm observation by the proportion of credit the bank grants to the firm. This provides us with more accurate firm-level estimates of bank-firm variables. In particular, the estimates by the creditors of the firm's probability of default are weighted by the proportion of credit each creditor grants to the firm. By collapsing the bank-firm level data to firm level data in this way, we embed the view that larger creditors have more information on the firm and are more important to the firm. We then average firm observations in the period before the announcement and in the period after the implementation to avoid serial correlation in the standard errors (Bertrand, Duflo, and Mullainathan, 2004).[8]

We find that empty creditors affect CDS firms as the probability of default for these firms drops when the effect of empty creditors is removed.[9] In particular, compared to German non-CDS firms, German firms with CDS traded on them witness a decrease in their probability of default by up to 2 percentage points (pp) after the change in the law, i.e., after the empty creditor effect is modulated. Additionally, we find that the impact of empty creditors as measured through the drop in the probability of default increases in the average ex-ante CDS hedge position of its creditors. Specifically, the empty creditor effect for a firm with a one standard deviation increase in the firm's hedge ratio increases by an extra 1.1 pp. Further, we find that firms with less concentrated debt, longer credit relationships and firms with higher average collateral ratios of their debt are less affected by empty creditors. While financially risky firms are strongly affected by empty creditors, safe firms are not. Banks that are not capital constrained, but that are liquidity constrained embed the empty creditor effect into their probability of default estimates of affected firms to a larger extent.[10] Furthermore, banks' business models affect the degree to which they embed the empty creditor effect. Banks that monitor their creditors less and that earn a smaller portion of their income from interest activities, embed the empty creditor effect to a larger extent.

We further test the assumption in Bolton and Oehmke (2011) that the incentive for empty creditors to push CDS firms into default is "priced in" to the CDS spreads. We find that this is indeed the case. We do so by comparing CDS spreads of treated German entities with those of other European companies unaffected by the change in German insolvency law. We find that after the treatment, CDS spreads on average drop by 49

[8] The main results are robust when we average the data, both across time (Table 14) and across bank-firm pairs (Table 7).

[9] In line with our predictions, we do not find a significant treatment effect when comparing the leverage of CDS firms to non-CDS firms, suggesting the commitment effect was not at play. Hence, the change in the probability of default is a result of the modulation of the empty creditor effect and not due to a reduction in leverage.

[10] The difference in the effect of capital versus liquidity constraints may be due to the fact that banks' trading of CDS on their borrowers alleviates credit constraints as it reduces the credit exposure to the borrowers, while liquidity constraints are, if anything, increased when a creditor trades CDS on their borrowers due to the CDS premium payments. Another explanation for our finding that capital constrained banks embed the empty creditor effect to a lesser extent is that they may be less willing to do so as it would further exacerbate their capital constraints.

to 120 basis points (bps).[11]

We conduct multiple robustness tests and find that the treatment and control groups follow parallel trends prior to the event. While generally important in a difference-in-differences set-up, it is key for our findings as the treatment and control group are both affected by the law change. Indeed, because the change in the bankruptcy law affects both the treatment and control group, the groups need to be identical apart from the treatment group being CDS referenced entities. If this were not the case, any differential response to the law change may be a result of some characteristic other than the treatment having CDS traded on their debt. To account for this concern, we match the treatment and control groups on firm size, alternative z-score, book leverage, and change in the probability of default in the pre-announcement period, using the coarsened exact matching technique (Iacus, King, and Porro, 2012). As a further robustness test, we change the control group to European CDS firms which, while more similar to German CDS firms than German non-CDS firms, did not have the law change applied to them. Hence, the average treatment effect where the control firms are European CDS firms includes both the empty creditor effect and the effect of the ability to restructure a firm. Thus, while the use of European CDS firms provides a good robustness check, it is not the ideal control group, and hence we use matched German non-CDS firms in major parts of our analysis.

Additionally, we enforce constant membership over the sample so as to account for any asymmetric selection bias. That is, we define treatment firms as firms which have CDS traded on their debt over the whole sample period, while control firms are firms that never have CDS traded on their debt. This is done to account for the possibility that the event alters the likelihood of being treated (i.e., being a CDS referenced entity), differentially across the treatment and control groups, which would bias the results. Finally, the results are robust to placebo testing, matching, using alternative matching variables, a shorter event window, or using different weights in averaging bank-firm data and to not averaging the data at all.

This paper contributes to the literature on the effects of CDS trading on the underlying firms in six ways. First, by virtue of having detailed CDS and credit position data, we are able to avoid a common assumption in the literature that the existence of CDS implies that the creditors of the firm are trading CDS on the firm. Second, by making use of these data, we are able to determine that the effect of empty creditors is increasing in magnitude with the CDS hedge position of a firm‘s creditors. Third, by employing an exogenous shock to the bankruptcy law, we avoid a potential endogeneity issue associated with a common event used in the literature, the initiation of CDS trading. Fourth, by employing this particular exogenous shock, we can disentangle the effect of CDS as a commitment device from the effect of empty creditors. Fifth, we shed

[11] This does not imply that CDS are fairly priced, only that the empty creditor effect is “priced in” to the CDS spreads to some degree. Further, banks may still have an incentive to trade CDS for regulatory reasons even if they are already “fairly priced”.

light on the firm and bank-firm relationship characteristics that increase the intensity of the impact of empty creditors on CDS firms. And finally, we confirm an important assumption of many theoretical papers in the literature, namely that the incentive for empty creditors to push CDS firms into default is priced into CDS spreads.

The remainder of this paper is organised as follows: Section 2 reviews the theoretical and empirical literature on the effects of CDS trading. In section 3 we develop the identification of empty creditors and associated hypotheses. In section 4 the data, its sources as well as its construction are discussed. Sections 5 to 9 present the results while section 10 presents several robustness checks. Section 11 concludes.

2 Credit Default Swaps & Corporate Default

What are empty creditors?

While the primary purpose of CDS is to reduce the credit risk of the protection buyer by transferring it to the protection seller, it also has significant effects on the bank-firm relationship.[12] The main channel through which CDS affects the bank-firm relationship is through its effect of separating a creditor's control rights from its credit exposure to the referenced firm.

When a creditor gives a loan to a firm, it obtains both credit exposure, the risk the firm may not be able to pay back the loan, as well as control rights. Control rights can be formal, as in the right to vote in bankruptcy proceedings, or informal, as in a creditor's ability to refuse to roll over a firm's debt unless certain conditions are met. A CDS contract, on the other hand, contains only credit exposure as it is a contract with a third party (the CDS counter-party, usually another bank). Hence, when a creditor purchases or sells CDS on a firm to which it lends, it can adjust its credit exposure to the firm, while leaving its control rights unaffected.

Further, while CDS is often compared to an insurance contract, a key difference between CDS and an insurance contract is the limit to the insured amount. Traditional insurance limits the insured amount to the underlying exposure, while there is no limit to the exposure through CDS. For example, if a home-owner wants to purchase fire insurance on her home, the maximum insurance value is the market value of the property. Under the same rules as for CDS, she would be able to insure the house for more than its worth or sell fire insurance on the property, in which case, her counter-party in the insurance contract would be purchasing insurance on an asset to which they do not otherwise have an economic exposure.

In the case a creditor purchases CDS on its borrower, they reduce their credit exposure, and may in fact reverse their exposure, such that they benefit from a credit deterioration of the referenced firm. However, these creditors retain their control rights

[12] See Appendix A.1 for detail on Credit Default Swap contracts.

over the firm both formally, a vote in bankruptcy proceedings, and informally. Legal scholars (Scott-Quinn and Walmsley, 1998; Hu and Black, 2008a,b) discuss the potential for this separation to cause creditors that no longer have an interest in the efficient continuation of the CDS referenced entity to push the firm into an inefficient liquidation or bankruptcy in order to collect the CDS insurance. A creditor which purchases CDS on its borrower is known as an "empty creditor" to highlight its lack of credit exposure to the firm. As there is no limit on CDS exposures, empty creditors may purchase CDS such that they would benefit from a referenced firm's bankruptcy. In this case, the empty creditor has both the incentive, CDS insurance payment, and the means, formal and informal control, to push a firm into default.

The theory of empty creditors

The theoretical literature on the effect of CDS trading on referenced entities, using a range of theoretical setups, predicts that the introduction of CDS trading on a firm increases its probability of default (Bolton and Oehmke, 2011; Arping, 2014; Danis and Gamba, 2018), decreases strategic default (Bolton and Oehmke, 2011), increases firm investment (Bolton and Oehmke, 2011; Arping, 2014; Danis and Gamba, 2018), increases leverage and firm value (Bolton and Oehmke, 2011; Arping, 2014), increases the maturity of debt (Arping, 2014), and reduces debt covenants (Arping, 2014). The theory disagrees on the impact of the relative bargaining power of creditors and shareholders. While Bolton and Oehmke (2011) predicts larger benefits to the firm, Colonnello et al. (2019) predict that firms with creditors which have relatively weak ex-ante bargaining power are more likely to face the empty creditor problem.

Further, the theory predicts that these effects may vary by firm, debt market and CDS trading characteristics. Firms enjoying the largest benefit from CDS trading on their debt are predicted to be small, opaque, non-profitable, and/or volatile (in assets or cash-flow). Their debt is predicted to be of low credit quality, difficult to renegotiate, and more likely to undergo restructuring (Bolton and Oehmke, 2011; Arping, 2014; Danis and Gamba, 2018).

We focus on the Bolton and Oehmke (2011) model as it allows us to disentangle the increase in a referenced firm's probability of default that is a result of the empty creditor effect from other, potentially confounding, effects (e.g., the commitment effect).

Bolton and Oehmke (2011) take a neutral view on the effect of CDS trading on referenced entities. The authors model the effect of CDS in a limited commitment model of credit to determine ex-ante and ex-post consequences of default insurance on credit outcomes. They argue that while empty creditors may indeed have the incentive not to accept a restructuring proposal of a distressed borrower, this does not necessarily imply an inefficient outcome (i.e., there may be potential benefits). The source of these potential benefits stems from a reduction in moral hazard (cash flow manipulation) and hence an increase in the financing capacity of referenced firms.

Figure 1: A stylized diagram of the Bolton and Oehmke (2011) model

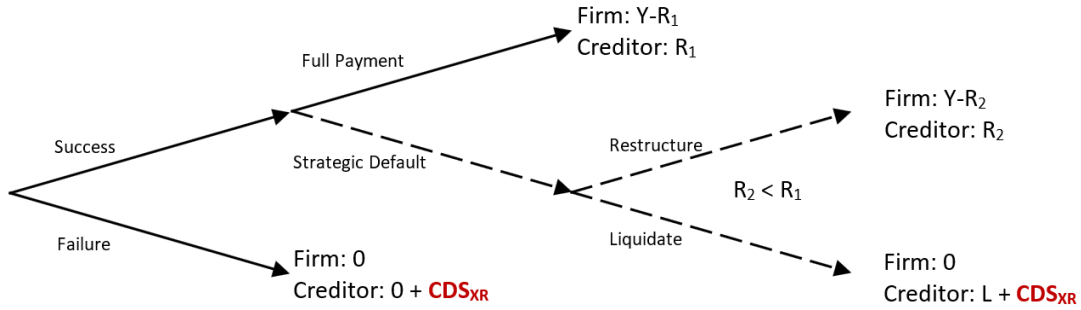


Figure 2: The Bolton and Oehmke (2011) model with restructuring as a credit event

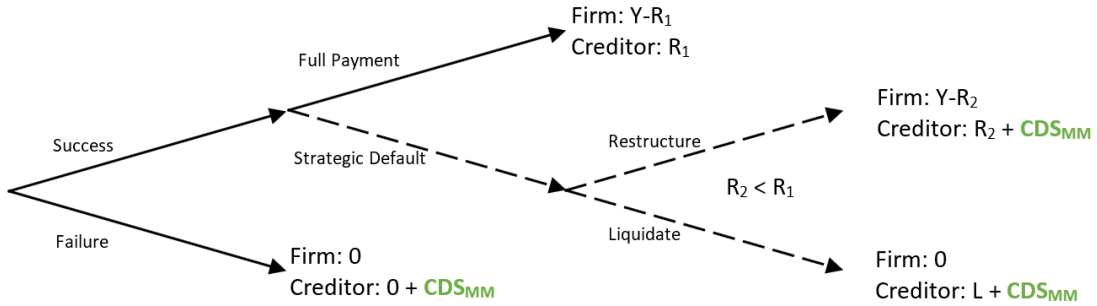
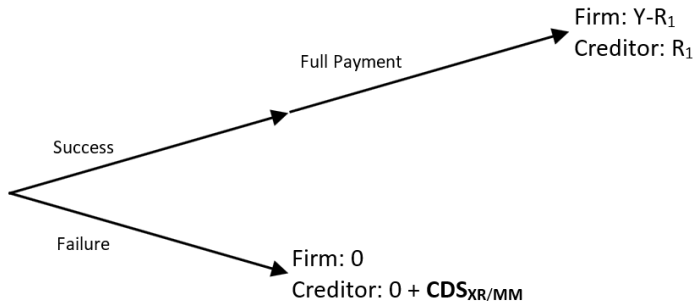


Figure 3: The Bolton and Oehmke (2011) model without restructuring



Note: Figure 1 illustrates a stylized version of the Bolton and Oehmke (2011) model. The figure includes two key assumptions in the model. First, as represented with the red CDS_{XR}, the CDS contract must only pay out in the case of bankruptcy (i.e., restructuring cannot be recognized as a credit event). Second, the applicable bankruptcy law should permit restructuring, as represented by the dashed lines. Under these assumptions, a firm experiences both the commitment effect (increase in leverage, and potential increase / decrease in probability of default), and the empty creditor effect (increase in probability of default). Figure 2 includes CDS insurance, CDS_{MM}, which pays out both in the event of bankruptcy and on restructuring. With this adjusted assumption, the firm experiences neither the empty creditor effect nor the commitment effect. Figure 3 illustrates the Bolton and Oehmke (2011) model where restructuring is not permitted. Here the type of CDS contract is not relevant as only bankruptcy is possible. Under these assumptions, the firm experiences only the empty creditor effect.

A stylized version of the Bolton and Oehmke (2011) model is depicted in Figure 1, which is a representation of a cash-flow manipulation problem incorporating CDS contracts. Here, firms finance a project with a positive net present value by borrowing the required investment from a creditor. The firm's realised cash-flow, Y, is privately known to the firm. Thus, in the case the project is successful, the firm has an option to under-report its cash-flows and default on its debt (i.e., strategically default), thereby entering into debt renegotiation with its creditors. Creditors are only able to verify the firm's realised cash-flow by incurring a verification cost.[13]

If a firm strategically defaults, it enters into a renegotiation with its creditors and if successful it keeps the difference between the realised cash-flow and the renegotiated amount, $Y-R_2 > Y-R_1$. This causes a moral hazard problem as firms have the incentive to strategically default, while banks can only confirm strategic default by conducting a costly audit. The potential for strategic default to occur causes creditors to reduce the credit they are willing to supply to the firm. In order to reduce a firm's incentive to strategically default, a creditor would need to commit to forcing the firm into bankruptcy or liquidation and thus remove the benefit the firm gains from strategically defaulting. Creditors have a limited ability to commit to forcing bankruptcy in the case of strategic default as it is assumed that the value of the firm in continuation (i.e., restructured) is greater than its liquidation value, $R_2 > L$.

Figure 1 includes CDS insurance, CDS_{XR}, which only pays out in the event of bankruptcy. When a creditor can trade CDS, the creditor will not accept the restructuring proposal if they are sufficiently insured and the CDS contract only pays out in the event of bankruptcy ($L + CDS_{XR} > R_2$).

The implication is that CDS act as a commitment device where creditors are more easily able to commit to forcing bankruptcy in the case of strategic default. The presence of CDS then reduces a firm's incentive to strategically default, which, ex-ante, increases the firms debt capacity.[14] The improved debt capacity of firms leads to an increase in their investment, leverage, firm value, and a decrease in their cost of debt. Bolton and Oehmke (2011) show that these benefits are larger for firms with a low proportion of fixed assets or with mostly unsecured creditors, where creditor bargaining power is weak (low credit concentration). Further, firms which are more likely to restructure (highly volatile or low credit quality firms) should benefit more from this commitment effect.

However, Bolton and Oehmke (2011) show that creditors tend to "over-insure" in equilibrium (i.e., purchase more CDS on a firm than is socially optimal). In this case the empty creditor has the incentive to push an otherwise healthy firm into bankruptcy, $CDS_{XR} > R_1$, and has the means to do so, as it has formal and informal control. This results in an increase in the probability of default for these firms, the empty creditor effect. The increase in the probability of default, due to the empty creditor effect,

[13] This implies the bargaining power of the creditor is negatively related to its verification cost.

[14] An equivalent interpretation is that CDS improves a firm's ability to credibly commit to pay out cash-flows.

may lead to a decrease in the financing capacity of firms, which leads to an overall ambiguous effect of CDS trading on firms' financing capacity. Thus, in the case of over-insurance, the benefits of CDS are unclear. Further, as referenced firms may increase their leverage through CDS acting as a commitment device, their probability of default may increase. Hence, when CDS acts as a commitment device, the source of the increase in the referenced firms probability of default is unclear.

Empirical evidence

The empirical investigations into the effects of CDS and empty creditors on financially distressed firms have produced mixed findings. Subrahmanyam et al. (2014) show in a large sample of distressed and healthy firms that the introduction of CDS increases the probability of bankruptcy. Using a small sample of distressed companies, Bedendo, Cathcart, and El-Jahel (2016) find that CDS do not have a significant effect on the likelihood of bankruptcy. Danis (2016) contributes to this debate by providing further evidence that empty creditors have a negative effect on out-of-court debt restructuring. Caglio, Darst, and Parolin (2019) employ granular data on US banks' CDS trading and lending behaviour to investigate the joint effect of the commitment effect and the empty creditor effect. The authors investigate the effect of banks' CDS trading on their borrowers' probabilities of default at both the extensive margin (existence of CDS trading on a firm) and the intensive margin (the hedging behaviour of a firm's lenders). The authors do not find any statistically significant effect of banks' CDS trading on their borrowers' probabilities of default.[15] Colonnello et al. (2019) find that firms with strong shareholders have more CDS insurance written on their debt and that CDSs increases the bankruptcy risk of these firms.

In this paper, we aim to resolve this disagreement by employing a novel identification strategy that disentangles the direct effect of empty creditors on referenced entities from the confounding effects through the effect of CDS as a commitment device. Additionally, this identification strategy avoids potential endogeneity issues associated with the use of the initiation of CDS trading as an event, a common approach employed in the literature.

Further, by virtue of having granular data on creditors' CDS positions, we are able to extend the CDS literature by investigating which firm, bank, and bank-firm characteristics affect the intensity of the empty creditor effect. Moreover, this granular CDS position data enables the avoidance of a common assumption in the literature, that the existence of CDS implies the creditors of the referenced entities are trading CDS on the referenced entity. We are able to avoid having to make this assumption as we combine

[15] A key contribution of this paper is that the aggregate measures of CDS hedging in the US (that do not condition on a lending relationship) miss important heterogeneity in banks' credit risk hedging behaviour. In particular, for high yield firms, these aggregate measures may overstate credit risk hedging activity by as much as 45 percent. This is important as a large part of the the existing CDS literature that uses US data relies on the assumption that the bank holding companies which are active in the derivative market are potential empty creditors (e.g., Subrahmanyam et al. (2014)).

bank-firm credit exposures with banks' exposures to firms via CDS, thus providing a bank-firm level CDS hedge ratio.

The empirical literature is not limited to the negative effects of CDS through empty creditors and has investigated a wide range of effects of CDS. Narayanan and Uzmanoglu (2018) show that a firm's value decreases on the initiation of CDS trading, and that this effect increases in CDS trading activity at firm level. They show the reduction in firm value is caused by an increase in the cost of capital for these firms, through a reduction in their stock liquidity and credit quality. Subrahmanyam, Tang, and Wang (2017) find that CDS firms hold more cash after CDS trading commences, which they argue is to avoid negotiations with tougher empty creditors. Saretto and Tookes (2012) find that firms with CDS traded on them can sustain higher leverage and borrow at longer debt maturities. Ashcraft and Santos (2009) show that the improvement of credit terms hinges on the riskiness of the firm, where safe and transparent firms see an improvement in their borrowing terms when CDS begin to trade on them. Bartram, Conrad, Lee, and Subrahmanyam (2019) conduct a cross-country analysis and show that the initiation of CDS trading on firms debt affects real decisions within these firms, such as leverage, investment and the riskiness of their investments. Further, they find these effects to be larger in countries where CDS help to mitigate weak property rights and where there is less uncertainty about the enforcement of obligations due under the CDS contract. Gündüz, Ongena, Tümer-Alkan, and Yu (2020) find that a decrease in the cost of buying CDS leads banks to extend more debt to CDS traded firms as well as hedge their exposures to these CDS firms for effectively (i.e., closer to fully hedged positions with a hedge ratio of 1). Beyhaghi, Massoud, and Saunders (2016) investigate the propensity of banks to use credit risk transfer instruments, such as CDS. They find that banks are more likely to use credit risk transfer instruments (e.g., CDS) the more capital or liquidity constrained they are.

3 Institutional Setting: Change in Bankruptcy Law

Before introducing the institutional setting and the quasi-natural experiment, we discuss two key assumptions of the Bolton and Oehmke (2011) model and how the failure of each of these assumptions alters the way in which CDS affects referenced firms. Afterwards, we will indicate how the change in bankruptcy law, given our institutional setting, allows us to identify the effect of empty creditors.

There are two key assumptions in the Bolton and Oehmke (2011) model. First, the applicable bankruptcy law should permit restructuring, specifically debt-to-equity swaps. Second, the CDS contract must only pay out in the case of bankruptcy (i.e., restructuring cannot be recognized as a credit event). Clearly, the second condition only plays a role if the first condition is met, i.e., if it is not possible to restructure; it does not matter if restructuring is defined as a credit event.

The second assumption, that CDS only pays out in the event of bankruptcy, CDS_{XR} in Figure 1, is required for the benefits (the commitment effect) to be felt. If the CDS contract defines restructuring as a credit event, it would pay out on restructuring and bankruptcy, CDS_{MM}, as represented in Figure 2. Hence, the CDS pay out would not affect the creditors decision to accept the referenced firm's restructuring proposal, $R_2 + CDS_{MM} > L + CDS_{MM} \leftrightarrow R_2 > L$. Therefore, the commitment effect requires the assumption that the CDS contract only pays out in the event of bankruptcy to hold (Bolton and Oehmke, 2011). Further, insured creditors have reduced incentives to push referenced firms into bankruptcy as they would collect the CDS insurance payment when the firm restructures. Hence, the referenced firm will not experience the empty creditor effect. Therefore, in the scenario represented by Figure 2 the referenced firm experiences neither the empty creditor effect nor the commitment effect.

The first assumption that firms are able to restructure their debt is key, too.[16] This is because the commitment effect only arises as a result of a reduction in strategic default, which would only be entered into if restructuring is possible. This assumption is represented by the dashed lines in Figure 2. However, if firms are not able to restructure, the impact of empty creditors will still be felt as creditors still have the incentive to push firms into default and receive the CDS pay-out, as in Figure 3.

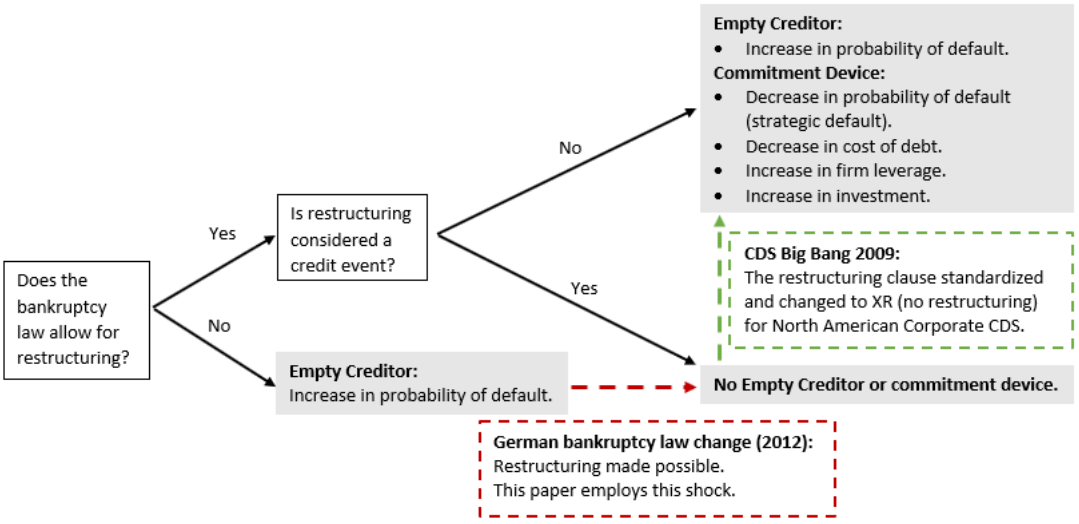
In summary, when both conditions are met, CDS trading on a referenced firm creates both the commitment effect and the empty creditor effect, as in Figure 1. The adverse effect of CDS trading, the effect of empty creditors, requires only the first assumption to fail, as in Figure 3. If restructuring is permitted and defined as a credit event, in this context, CDS has no effect on the underlying firm, as in Figure 2. Figure 4 outlines these conditions as well as the effect of two key events, the change to the German Bankruptcy law and the "CDS Big Bang".

In this analysis, our sample includes European CDS referenced firms, which trade with the standard European CDS contract. This standard contract defines restructuring as a credit event, and hence CDS on European firms will pay out when they restructure. Therefore, the second assumption does not hold and there is no commitment effect of CDS trading for European referenced entities, even if the applicable bankruptcy law allows for restructuring. This implies that European CDS referenced firms are either in the scenario represented by Figure 2 (no empty creditor effect or commitment effect), or by Figure 3 (only the empty creditor effect) if the domicile country's law does not allow restructuring.

German insolvency law automatically triggers a formal bankruptcy if a firm faces insolvency, if an insolvency is imminent, or if a firm is over-indebted. In Germany prior to 2012, the insolvency law all but prohibited in-court restructuring, which is evident as they occurred in only 2 percent of insolvency cases (Höher, 2012). Out-of-court

[16] Bolton and Oehmke (2011) refer specifically to an out-of-court restructuring (e.g., through a debt exchange or a debt-for-equity swap).

Figure 4: CDS credit events and bankruptcy law



Note: This figure illustrates the two conditions for the positive and negative effects of CDS on an underlying firm to be felt. The first is the ability to restructure a company under the domicile country's law. The second condition only plays a role if the first condition is met. The second condition is that the CDS contract type should not pay out when the underlying debt is restructured. If restructuring is not permitted under law, then the CDS contract type is irrelevant and the firms have no incentive to strategically default (as they will be liquidated). However, the incentive for empty creditors to push a firm into bankruptcy remains. If restructuring is permitted and defined as a credit event, in this context, CDSs have no effect on the underlying firm. Finally, if restructuring is permitted and not defined as a credit event, then CDSs act as a commitment device as well as increases the probability of default through the effect of empty creditors.

restructuring was similarly unlikely in German prior to 2012 due to several institutional biases against workouts (Jostarndt and Sautner, 2010). For example, if the management of a firm does not file for bankruptcy in a timely manner, they may be held personally liable, and may be sentenced for up to three years in prison.[17] Further institutional biases against out-of-court restructuring include the difficulty in obtaining senior financing, the restrictions on informal debt-equity swaps, uncertainty around voluntary winding-ups, and the fact that the German government provides social security funding to pay the salaries of firms which file for bankruptcy.

The lack of restructuring negotiations, i.e., the first assumption failed, meant that German reference entities experienced the effect of empty creditors without the potential confounding effects on their probabilities of default through CDS as a commitment device, as in Figure 3.

In 2012, German insolvency law was substantially reformed by the ESUG reform act. This law change allowed for debt-equity swaps in restructuring negotiations.[18] This reform effectively fulfilled the first assumption of the (Bolton and Oehmke, 2011) model as restructuring became possible. However, as European CDS define restructuring as a credit event, the reform essentially activated the restructuring pay-out clause on German reference entities and thus removed the impact of empty creditors for these firms, as in Figure 2.

Therefore, the prediction is that German CDS referenced firms should have experienced a decrease in their probability of default after the change in bankruptcy law relative to similar German firms without CDS traded on them.[19]

Further, the theory, as outlined in the previous section, predicts that the intensity of the impact of empty creditors varies with certain firm, bank and bank-firm characteristics.

Lastly, another more technical assumption of the Bolton and Oehmke (2011) model is that the incentive for empty creditors to push referenced firms into default is priced into the CDS spread. As the impact of empty creditors was removed for German ref-

[17] The relevance of this law can be seen in the response of the German Federal Ministry of Justice and Consumer Protection to the economic consequences of the COVID-19 pandemic. Part of the ministry's response was to pass legislation temporarily suspending the obligation to file for insolvency with the "COVID-19 Insolvency Suspension Act" (COVInsAG - BGBl. I S. 569). The result of this suspension of obligation was a 13.1 percent reduction in the number of businesses filing for insolvency during first three quarters of 2020 compared to the same period in 2019 (Destatis, 2020).

[18] See Appendix A.2 for detail on the German Insolvency Law and the changes in 2012, ESUG. Closset and Urban (2018) also study this reform. More specifically, the authors rely upon a size threshold which affects a particular part of the reform that would improve the bargaining power of large firms' creditors but not for small firms. They find that large firms decrease their leverage and investment, while small firms see an increase in their leverage and investment as well as a decrease in their cost of debt. In our methodology, we therefore match on firm size and other variables to make CDS firms comparable to non-CDS firms, allowing us to extract the impact of the law on the effect of empty creditors. Further, in all regressions except for the ones in which we test for the intensity of the impact of empty creditors, we control for the definition of "large firm" as defined by German Law using three thresholds: total assets, total sales, and number of employees.

[19] This reduction cannot be explained by a reduction in risk-shifting as modelled by Campello and Matta (2012) as restructuring was not possible prior to the bankruptcy law change.

erence entities by the reform, the CDS spread of German reference entities should have decreased relative to the CDS spreads of firms from other European countries which did not see any change to their exposure to empty creditors over this time period. However, as the restructuring clause became active, there should have been a relative increase in the CDS spread of German firms, as restructuring CDS pay out with more ease (Packer and Zhu, 2005). Thus, we cannot determine the magnitude of the empty creditors' effect on CDS spreads, but a reduction in spreads would be evidence that it is indeed priced, and that it outweighs the restructuring effect.

4 Data

To test the aforementioned theories, we combine different datasets containing detailed CDS position data, CDS pricing data, credit exposure data, firm characteristics, bank characteristics and macro economic data. Table 1 contains information on each variable used in this paper, including their unit, definition, and source.

We obtain detailed CDS position data from the Trade Information Warehouse (TIW) of the Depository Trust and Clearing Corporation (DTCC). The DTCC-TIW datasets are the most comprehensive datasets on granular CDS positions available, containing between 90 percent and 95 percent of global CDS activity (Mayordomo, Peña, and Schwartz, 2014). We employ a relevant subset of this data. In particular, we obtain data on all CDS positions where either a German bank is a party or counter-party, or where the reference entity is German. The DTCC position level dataset contains individual bank's CDS positions on a reference entity with a particular counter-party, at a weekly frequency. We collapse these data to obtain CDS positions of each bank on each reference entity at a quarterly frequency. This level of granularity is not available from the public database of the DTCC, which only shows CDS positions aggregated at firm level, and only for the top 1,000 reference entities by gross notional.

We further match the DTCC data with CDS pricing data obtained from Markit. This dataset contains the CDS spread and liquidity data for all traded CDS at a daily frequency. Markit obtains this data by polling the CDS dealers for the price and liquidity measures (e.g., bid-ask spreads).

Next, we match the resultant dataset with Moody's CreditEdge which contains information on each firm‘s loss given default, expected default frequency, market value of assets, and other market based risk measurements.

We then match the CDS data with the German credit register (MiMik) which contains bank-firm credit exposures and banks' estimates of their borrowers' probability of default, amongst other data, at a quarterly frequency. This makes it possible to determine individual bank-firm credit exposures and CDS hedge positions. The German Credit registry contains detailed information on the credit exposures of each bank in

Table 1: Variable Descriptions

Variable	Unit	Definition	Source
Probability of Default	percent	Probability that the firm defaults on its debt. This is submitted by each creditor of the firm.	MiMik
Net Notional / Total Credit	-	The hedge ratio defined as CDS net notional / total credit.	MiMik & DTCC
Treated	0/1	Equal to 1 if the firm is a German firm with CDS traded on it over the entire sample period. In major parts of our analysis it is equal to 0 if the firm is German and it never had CDS traded on it in the sample period. In other parts of our analyis it is equal to 0 if the firm is a non-German European firm with CDS tradesd on it over the entire sample period. This difference is always made clear in notes of the respective analyses.	DTCC
Spread	bps	Five year fixed maturity CDS spreads where credit events are defined so as to include restructuring, with a modified modified clause.	Markit
Length of Credit Relationship	qrts	The length, in quarters, the firm has had a credit relationship with a particular bank.	MiMik
Number of Banking Relationships	-	The number of credit relationships a firm has.	MiMik
Firm Credit Concentration	percent	The Herfindahl-Hirschman Index of a firms credit market. Calculated as the sum of the squared share of each creditor of a firm.	MiMik
Collateral / Total Credit	percent	The current value of the collateral attached to a credit exposure / the current principal of the credit exposure outstanding.	MiMik
Firm Size	log	Log of total assets of the firm.	USTAN
Book Leverage	percent	Book value of debt / total assets.	USTAN
Alternative Z-Score	percent	The Altman's Z-score as modified by MacKie-Mason (1990), which excludes leverage. A low Z-score indicates high default risk.	USTAN
Tangible Asset Ratio	percent	Tangible assets / total assets.	USTAN
Net Working Capital / TA	percent	Net working capital / total assets.	USTAN
Retained Earnings / TA	percent	Retained earnings to total assets.	USTAN
EBIT / TA	percent	Earnings before interest and tax / total assets.	USTAN
Legal Size	0/1	Equal to 1 if the firm met at least two of the size criteria under the German Commercial Code at any point in the period prior to the reform. The criteria are: average number of employees greater than 50, total sales greater than EUR38.5m, and total assets greater than EUR19.25m.	USTAN
Liquidity	percent	Bank: liquid assets / total assets.	BISTA
Capital	percent	Bank: total capital / total assets.	BISTA
NII	percent	Bank: net interest income / gross earnings.	GuV
Monitoring	percent	Bank: staff and administration expense / gross earnings.	GuV

the economy to each of its borrowers.[20] Further, each bank submits an estimate of a firm's probability of default, which we use as the measure of bankruptcy risk. Since each creditor submits its own estimate of the firm's probability of default, there is variation in a firm's probability of default at the firm-time and bank-firm-time level which we exploit in a later regression.

It should be noted that banks' probability of default estimates of their borrowers do not take into account the loss given default, and hence, they are not affected directly by hedging practices. In other words, a bank's estimate of a borrower's probability of default only measures the likelihood the borrower defaults, not the loss to the bank given default. This is important, as otherwise the banks' hedging practices (CDS positions) would affect their probability of default estimates directly, not only indirectly through the empty creditor effect.

We match this dataset with the Deutsche Bundesbank's BISTA and GuV databases, containing quarterly bank balance sheet and income statement information, respectively. We employ these datasets to calculate banks' capital ratio, liquidity ratio, net interest income, and monitoring costs.

Further, we combine the dataset with detailed firm balance sheet and income statement data obtained from the Deutsche Bundesbank's USTAN database for the German firms, and Compustat for the European firms employed in our pricing regressions. Further, we restrict the data to only include non-financial firms as there is a different insolvency law for financial and insurance companies.

Additionally, we add macroeconomic data from DataStream and the Federal Reserve Bank of St. Louis. We obtain overnight interest rate swap data from DataStream, and GDP and inflation data from the Federal Reserve Bank of St. Louis' economic database.

While we investigate the bank-firm level variation in a later regression, for the main results we collapse the dataset to firm-time level taking the credit weighted average of bank-firm level variables. For example, the credit-weighted average probability of default is calculated as:

$$PD_{f,t} = \sum_{b=0}^{N_c} \frac{Total\,Credit_{b,f,t}}{\sum_{b=0}^{N_c} Total\,Credit_{b,f,t}} \times PD_{b,f,t}$$

where banks, firms and time are indexed by b, f and t, respectively. N_c represents the total number of creditors for firm f.

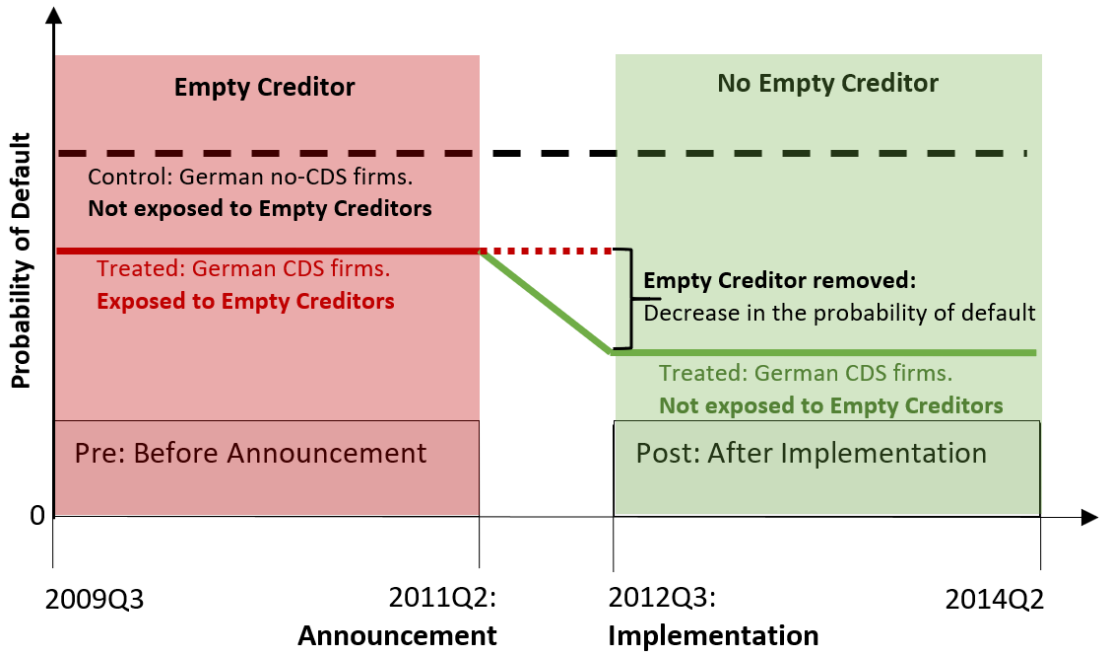
Similarly we calculate the credit-weighted average hedge position of a firm's creditors as:

$$Net\,Notional\,/\,Total\,Credit_{f,t} = \sum_{b=0}^{N_c} \frac{Total\,Credit_{b,f,t}}{\sum_{b=0}^{N_c} Total\,Credit_{b,f,t}} \times \frac{CDSNetNotional_{b,f,t}}{Total\,Credit_{b,f,t}}$$

Further, we collapse data on the length of each bank-firm relationship, and the

[20] In the German credit register, credit exposure is defined as total credit exposure, which includes public debt as well as loan exposure.

Figure 5: Event Timeline



Note: The figure illustrates the event time line. The red area depicts the period (2009Q3 to 2011Q2) prior to the announcement of the change in the bankruptcy law where German CDS firms were exposed to empty creditors. The green area depicts the period after (2012Q3 to 2014Q2) the implementation where the treatment group was no longer exposed to empty creditors. The horizontal lines for the treatment, solid, and control group, dashed, illustrate that the data has been collapse to firm-period level (i.e., one observation per firm representing the average probability of default for that firm in that particular period.). Finally, the level drop in the line for the treated group after the implementation of the law change (green line - red line) represents our hypothesis that the removal of the impact of empty creditors is associated with a decrease in the probability of default of affected firms.

proportion of collateral attached to each bank-firm credit exposure on firm-time level, taking the credit weighted averages of all variables. This provides us with more accurate firm level estimates of bank-firm variables. By collapsing the bank-firm level data to firm level data in this way, we embed the view that larger creditors have more information on the firm and are more important to the firm.

The data is further collapsed to avoid serial correlation in the standard errors (Bertrand et al., 2004). When collapsing at firm-time level, the time level is determined by the model which we test. For the main results we employ the change to the German bankruptcy law as an exogenous shock and thus we average firm level data before the announcement of the law change and after the implementation. Figure 5 depicts the time periods of interest for the main regressions. As a robustness check, we test for placebo effects by using the CDS Small Bang as the event. In this case, we average before and after the implementation of the CDS Small Bang. In both cases we employ data eight quarters before the event and eight quarters after the event. The results are robust to using a shorter pre/post window of four quarters.

Defining treatment and control groups

For all regressions we define the treatment group as German CDS reference entities which have CDS traded on them (CDS reference entities). In all regressions except the pricing regressions, the control group is defined as German firms which are not CDS reference entities. This control group is not feasible for the pricing regressions as there is no CDS price for a firm if there are no CDS traded on its debt. Therefore, we define the control group in the pricing regressions as other European CDS reference entities (i.e., European CDS reference entities excluding German reference entities). The use of other European CDS reference entities as a control group is further motivated by the fact that they did not see any change to their exposure to empty creditors over this time period.

Two potential issues need to be accounted for given our definition of treatment and control, i.e., treatment firms have CDS traded on their debt and control firms do not. The previous literature on the effects of CDS trading on referenced entities has shown that firms see an increase in their probability of default when CDS begin to trade on their debt (Subrahmanyam et al., 2014; Danis and Gamba, 2018). Hence, if the event alters the probability of being treated heterogeneously across treatment and control firms, this may cause a difference in the average probability of default for treatment and control firms across this event. To account for the possibility that the event changes the probability of being treated heterogeneously across our treatment and control group, we restrict the definition of treatment and control to not allow switching between the groups. That is, a firm is treated if it is a CDS reference entity during the whole sample period, and is defined as a control firm if it never has CDS traded on it during that same period.[21]

[21] This restriction has a limited effect with only seven firms being excluded by it, and does not affect the

Table 2: Descriptive Statistics: German Reference Entity Vs. German Non-Reference Entity

		Control - German Firms with No CDS Traded								*Treatment - German Firms with CDS Traded*							
		Count	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**	**Count**	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**
Before Announcement of Change in Bankruptcy Law	Probability of Default	1,088	1.67	7.07	0.07	0.14	0.32	0.81	2.55	40	1.45	3.49	0.10	0.17	0.32	0.55	3.26
	Firm Size	1,088	12.35	1.02	11.30	11.59	12.11	12.83	13.86	40	16.27	1.08	14.87	15.36	16.30	17.24	17.84
	Book Leverage	1,088	64.87	18.01	42.82	53.41	65.70	76.91	85.53	40	64.36	16.50	43.82	53.76	65.97	77.13	83.31
	Alternative Z-Score	1,088	0.67	0.65	0.03	0.30	0.61	0.88	1.59	40	0.58	0.52	0.05	0.26	0.51	0.78	1.12
	Δ Probability of Default	1,088	-0.59	7.32	-0.94	-0.19	-0.03	0.02	0.13	40	-0.32	1.22	-1.76	-0.18	-0.04	0.02	0.11
	Tangible Asset Ratio	1,088	54.87	36.55	1.00	15.81	67.72	90.01	93.15	40	6.04	11.35	0.01	0.18	1.61	7.15	18.38
	Firm Credit Concentration	1,088	13.25	172.32	0.06	0.87	4.36	10.63	19.56	40	1.21	5.18	0.02	0.05	0.12	0.44	1.65
	Net Working Capital / TA	1,088	-2.77	17.56	-22.62	-8.81	-1.55	3.76	16.23	40	-1.33	19.45	-23.35	-16.07	-3.74	8.70	18.50
	Retained Earnings / TA	1,088	13.98	21.64	-0.12	0.36	10.79	27.32	41.11	40	14.70	11.06	4.36	9.46	11.24	20.24	30.78
	EBIT / TA	1,088	2.26	6.57	-2.12	0.34	1.60	5.01	9.02	40	3.89	3.59	0.34	1.55	3.69	5.40	9.14
	Sales / TA	1,088	43.45	50.36	4.58	12.08	17.80	65.76	114.09	40	26.02	37.90	0.00	0.00	5.71	40.50	77.05
	Length of Credit Relationship	1,088	28.41	16.40	7.13	15.11	26.94	41.46	51.91	40	21.65	7.27	12.98	16.32	20.63	27.42	32.31
	# Credit Relationships	1,088	7.03	6.98	2.00	3.40	5.38	8.13	12.25	40	66.39	56.53	19.38	28.63	43.31	89.19	140.75
	Collateral / Total Credit	1,022	67.43	55.01	2.81	24.67	67.77	98.64	118.70	40	11.33	12.89	0.55	1.60	6.22	16.01	24.71
	Net Notional / Total Credit	1,088	0	0	0	0.	0	0	0	40	1.40	2.47	-0.27	0.04	0.36	2.65	4.26
After Implementation of Change in Bankruptcy Law	Probability of Default	1,088	2.09	9.72	0.06	0.10	0.24	0.61	2.34	40	0.56	1.16	0.08	0.11	0.16	0.41	1.10
	Firm Size	1,022	12.42	1.03	11.34	11.65	12.16	12.90	13.93	38	16.42	1.07	15.02	15.51	16.40	17.32	17.97
	Book Leverage	1,022	63.10	18.16	40.17	51.87	63.76	75.43	85.32	38	60.98	17.71	39.03	49.67	61.74	72.15	81.65
	Alternative Z-Score	1,022	0.72	0.71	0.01	0.34	0.66	0.97	1.55	38	0.59	0.57	-0.05	0.29	0.51	0.78	1.41
	Tangible Asset Ratio	1,022	56.13	36.37	1.07	17.22	72.23	89.84	92.97	38	5.86	12.41	0.01	0.09	1.19	6.55	14.33
	Firm Credit Concentration	1,088	10.37	64.77	0.01	0.58	4.19	11.08	20.59	40	0.78	2.68	0.01	0.01	0.04	0.18	1.24
	Net Working Capital / TA	1,022	-2.07	18.03	-20.70	-7.32	-1.62	3.87	17.14	38	-5.77	18.81	-26.38	-16.58	-5.84	2.14	12.97
	Retained Earnings / TA	1,022	15.71	22.06	0.00	0.64	12.97	29.86	44.20	38	17.34	13.46	4.46	9.21	13.67	22.05	30.68
	EBIT / TA	1,022	2.80	6.45	-1.16	0.52	1.82	4.90	9.81	38	4.91	5.15	0.67	2.61	3.46	6.22	11.86
	Sales / TA	1,022	42.82	50.90	5.00	12.38	16.79	63.66	113.35	38	25.34	38.94	0.00	0.00	1.13	42.28	73.46
	Length of Credit Relationship	1,088	32.56	19.37	8.62	16.91	31.33	46.80	61.04	40	24.09	8.86	12.54	17.49	23.61	31.77	35.68
	# Credit Relationships	1,088	7.92	10.80	2.00	3.63	5.50	8.20	13.13	40	82.09	76.67	16.13	26.69	74.19	110.50	146.00
	Collateral / Total Credit	1,006	70.47	44.93	5.67	32.33	78.48	100.30	113.68	40	21.67	25.50	1.07	2.69	5.65	43.72	63.38
	Net Notional / Total Credit	1,088	0	0	0	0	0	0	0	40	0.78	2.07	-0.42	-0.04	0.40	1.22	2.24

Note: The table contains the sample statistics for the credit weighted sample. The table is split by treatment and control group, and by time (before announcement, after implementation). There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The pre-announcement period runs from 2009Q2 to 2011Q2, and the post-implementation period runs from 2012Q3 to 2014Q1. The treatment group is comprised of German firms which had CDS traded on them both before the implementation and after the announcement of the law change. The control group is comprised of German firms which never had CDS traded on them in the sample period. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. Each observation represents the credit weighted average value for the firm in the respective period (e.g., 40 observations of treated companies prior to the announcement represents 40 firms' credit weighted average value for probability of default prior to the announcement of the law change) Table 1 contains information on each variable used in the table, including their unit, definition, and source.

A second potential issue is that our treatment group, German CDS referenced firms, are different from our control group, German firms not referenced in CDS contracts, in such a way that biases the treatment effect. To account for this potential issue, we match the treatment and control groups on firm size, book leverage, alternative z-score, and average pre-announcement change in the probability of default using the coarsened exact matching technique (Iacus et al., 2012). In other words, we match the treatment and control group in order to make the groups more similar to each other, such that we can conclude that any differential response to the event is a result of CDS firms having had the empty creditor effect removed, rather than some other systematic way in which CDS firms differ from non-CDS firms. The use of the coarsened exact matching technique is motivated by the fact that propensity score matching often increases imbalance, inefficiency, model dependence and bias (King and Nielsen, 2019). However, the main results are robust to matching technique and to the choice of matching variables.

An alternative option would be to define European CDS firms as the control group, as they are more similar to German CDS firms than German non-CDS firms. Further, European CDS firms did not experience any change in their exposure to empty creditors over the sample period. Hence, there should be a differential response to the change in bankruptcy law in Germany. However, using these firms as a control group would result in the average treatment effect containing the effects of the ability to restructure a firm as European CDS firms did not have this law change apply to them. Hence, the more appropriate choice for the control group is German firms which do not have CDS traded on their debt.

The final dataset consists of 1,128 firms, observed over the period 2009-2014, of which 40 have CDS traded on them. Table 2 contains descriptive statistics for the sample used in the main regressions, split by treatment and control group, and by time period.[22] As we match on firm size, an alternative z-score, and book leverage before the event, the mean of these variables for the treatment and control group are more similar than they are in the unmatched sample.[23]

While we do not match on the outcome variable, probability of default, it can be noted from Table 2 and Table 18, in Appendix A.3, that the probability of default of the control group becomes more similar to that of the treatment group prior to the event. This is a first indication that the matching process was successful in creating a control group that is more similar to the treatment group than the unmatched control group.

main results.

[22] In Appendix A.3, Table 18 provides descriptive statistics for an unmatched sample and Table 19 provides descriptive statistics for an unweighted, matched sample. The sample prior to matching consists of 71,163 firms observations for probability of default and 9,969 firms with full data.

[23] It can be noted that even after matching, the treatment and control group are still different in terms of firm size. However, this difference in size does not drive the main results as the treatment effect is still statistically siginficant when controlling for firm size as shown in Columns 1 and 2 of Table 5 and in Appendix A.3, Table 16.

Further, the fact that the average probability of default of the treatment group decreases after the event while the control group increases is preliminary evidence that the impact of empty creditors was removed for the treatment group after the insolvency law change.

Finally, it can be noted that prior to the law change, on average, creditors of CDS firms had a CDS hedge ratio of 1.4. While the average CDS hedge ratio reduced to 0.78 after the law change, the reduction was not statistically significant and only accounted for approximately 25 percent of one standard deviation (2.47). This implies that although creditors did not adjust their CDS hedge ratios in a statistically significant manner, the removal of their incentives to push CDS firms into default was sufficient to see a reduction in the empty creditor effect on the probability of default of affected firms.

5 Average Empty Creditor Effect

Before analysing the factors that alter the intensity of the empty creditor effect, we test the average empty creditor effect. To do so, we employ the following regression specification:

$$\Delta PD_f = \beta_1 Treated_f + \beta_4 Z_{f,BA} + \alpha_k + \epsilon_f \tag{1}$$

where firms and sectors are indexed by f and k, respectively. ΔPD_f is the change in a firm f's average probability of default after the implementation, denoted by "AI", of the law change compared to the average before the announcement, denoted by "BA" (i.e., $PD_{f,AI} - PD_{f,BA}$) . The vector $Z_{f,BA}$ is an indicator variable to control for the legal definition of size, as legally large firms had an additional clause that applied to them in the reform. Finally, α_k are sector fixed effects and ϵ_f is an error term.[24] In all regressions, the standard errors are clustered at sector level to account for the correlation structure of the errors (Petersen, 2009).

$Treated_f$ is equal to 1 if a firm f had CDS traded on it over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. Given that the effect of empty creditors is removed for treated firms after the change in bankruptcy law, these firms should see a relative decrease in their probability of default, and therefore, β_1 is expected to be negative.

Table 3 contains The results of the base line regressions, Equation 1, where the total average treatment effect is tested.[25] We find positive, and significant, evidence for the impact of empty creditors through a reduction in the probability of default for CDS firms when the impact of empty creditors is removed. CDS firms see a 1.31 to 2.01 pp decrease in their probability of default when the effect of empty creditors is removed,

[24] Sector codes are defined as in WZ 2008 which is the German equivalent of NACE codes and comply with the requirements of NACE Rev. 2.

[25] It can be noted that the Adjusted R-squared values are low or sometimes negative in the regression results tables. If we take column 1 of Table 3 as an example, *Treated* is equal to 1 for only 40 of the 1,128. Hence, *Treated* cannot explain the change in the probability of default for the 1,088 non-CDS firms.

Table 3: Empty Creditor Effect - Base Results

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD
Treated	-1.308**	-1.354*	-2.010**	-1.993**
	(-2.20)	(-1.75)	(-2.34)	(-2.05)
Legal Size Control	No	Yes	No	Yes
Sector FEs	No	No	Yes	Yes
Observations	1,128	1,128	1,117	1,117
Adj. R2	0.000	-0.001	0.032	0.031

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other German firms. The regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_4 Z_{f,BA} + \alpha_k + \epsilon_f$$

where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $Z_{f,BA}$ includes a control for the legal size of firm f to control for the effect of a particular clause in the reform that affected only firms which were classified as large under the German Commercial Code. Finally, α_k are sector fixed effects. Here the event is defined as zero for eight quarters before the announcement of the law in 2011Q2 and 1 for eight quarters after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. All regressions cluster the standard errors at sector level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

depending on whether sector fixed effects are included or not. This is economically significant given that the average probability of default in the entire sample is about 1.67 percent, and represents 38 percent to 57 percent of one standard deviation (3.49 pp) of the probability of default in the treatment group prior to the law change. In unreported equivalent results we find a reduction in the expected default frequency (the default probability derived by Moody's Analytics, Inc.) for CDS firms when the impact of empty creditors is removed.

6 Empty Creditors in the Tails

Having tested the average effect of empty creditors, we now investigate the heterogeneous effect of empty creditors over the whole probability of default distribution by separating the sample into quartiles of probability of default prior to the reform and then running the main regression, Equation 1, on that particular quartile. When determining the quartiles we do so based on the distribution of the probability of default of referenced entities only. This is done to ensure that there are an equal number of treatment firms in each quartile, i.e., ten treatment firms in each quartile.[26]

We expect the effect of empty creditors to be particularly important in the right tail of the probability of default distribution, and potentially non-existent in the left tail. By separating the sample in quartiles of probability of default prior to the event, we aim to determine if the riskiness of a firm determines the extent to which it experiences the impact of empty creditors. Put simply, if firms are riskier, creditors need not "push" them into default, but rather "nudge" them into default. Thus, we expect to see that firms in the right tail of the probability of default distribution experience the impact of empty creditors while it is unclear if safer firms would be affected.

The results of these regressions are presented in Table 4. The first column shows the results for the whole matched sample, which is equivalent to column 1 of Table 3. Columns 2 to 5 contain the results where the sample is restricted to firms which belong to the indicated quartile of probability of default prior to the event. The results from columns 2 to 5 show that the treatment effect is statistically significant for riskier firms (quartiles three and four). Further, the size of the treatment effect is larger for firms in higher quartiles. The average empty creditor effect for the riskiest firms (firms in the fourth quartile) is 5.015 pp, which is approximately ten times larger than less risky firms (firms in the third quartile). This indicates that riskier or more vulnerable firms experience empty creditors more severely.

[26] The results are robust to using the entire sample to determine the quartile cut-offs, these are presented in Appendix A.3, Table 15.

Table 4: Empty Creditor Effect - Treatment Group Quartile Separation

	(1) Full Sample ΔPD	(2) Quartile 1 ΔPD	(3) Quartile 2 ΔPD	(4) Quartile 3 ΔPD	(5) Quartile 4 ΔPD
Treated	-1.308**	-0.292	-0.506	-0.517**	-5.015***
	(-2.20)	(-1.08)	(-1.07)	(-2.21)	(-2.94)
Legal Size Controls	Yes	Yes	Yes	Yes	Yes
Observations	1,128	195	205	185	442
Control Firms	1,088	185	195	175	432
Treated Firms	40	10	10	10	10
Adj. R2	0.000	-0.005	-0.005	-0.002	0.003

Note: Difference-in-differences regression results for the change in probability of default, where the regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_4 Z_{f,BA} + \epsilon_f$$

where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $Z_{f,BA}$ includes a control for the legal size of firm f to control for the effect of a particular clause in the reform that affected only firms which were classified as large under the German Commercial Code. Here the event is defined as zero before the announcement of the law in 2011Q2 and 1 after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. All regressions cluster the standard errors at sector level. In column 1 the full sample is used. In column 2-5 only those firms in the indicated quartile of $PD_{f,AI}$ are included. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

7 Intensity of the Empty Creditor Effect

We employ the following regression specification to test for the firm and bank-firm relationship characteristics that affect the intensity of the empty creditor effect:

$$\Delta PD_f = \beta_1 Treated_f + \beta_2 X_{f,BA} + \beta_3 Treated_f X_{f,BA} + \alpha_k + \epsilon_f \tag{2}$$

where firms and sector are indexed by f and k, respectively. ΔPD_f is the change in a firm f's average probability of default after the implementation (denoted by "AI") of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$) compared to the average before the announcement (denoted by "BA"). The vector $Z_{f,BA}$, from Equation 1 was not included to avoid multicollinearity issues. Finally, α_k are sector fixed effects and ϵ_f is an error term. $Treated_f$ is equal to 1 if a firm f had CDS traded on it over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. In all regressions, the standard errors are clustered at sector level to account for the correlation structure of the errors (Petersen, 2009).

The vector $X_{f,BA}$ contains average, pre-announcement, credit weighted average bank-firm and firm variables. The use of pre-announcement averages is equivalent to lagging the $X_{f,BA}$ vector, and is done to account for potential endogeneity. These variables have further been demeaned, or centred, in order to improve the interpretation of the β_1 estimates. The interpretation of β_1 is improved as once the $X_{f,BA}$ variables have been demeaned the magnitude of β_1 represents the average empty creditor effect when the $X_{f,BA}$ variables are at their mean level. $X_{f,BA}$ include bank-firm based variables: the number of credit relationships a firm has, the average length of these relationships, the ratio of loan collateral to loan value, the average creditors' hedge ratio, and the concentration of the firm's debt. Further, it includes other firm characteristics: firm size, book leverage, asset tangibility and EBIT to total assets.

Given that the effect of empty creditors is removed for treated firms after the change in bankruptcy law, these firms should see a relative decrease in their probability of default, and therefore, β_1 is expected to be negative. The expected sign for the β_3 estimates depends on the characteristic, $X_{f,BA}$, being tested. The firm characteristics, firm size, book leverage, asset tangibility and EBIT to total assets are common determinants of the probability of a firm defaulting in general. That is, firms which are smaller, highly leveraged, have low asset tangibility, and which have low profitability, are expected to be more likely to default. Hence, in line with the results where we split the sample in the riskier and safe firms, Table 4, we expect the empty creditor effect to be larger for firms with characteristics that are associated with higher default in general.

In Table 5 we present the results for Equation 2 with firm characteristics as interactions. For each variable, we report the results with and without sector fixed effects. In line with the results from Table 4, we find that firms with riskier characteristics are more affected by empty creditors. Here we learn that firms which are smaller (column

Table 5: Intensity of the Impact of Empty Creditors - Firm Variables

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD	(5) ΔPD	(6) ΔPD	(7) ΔPD	(8) ΔPD	(9) ΔPD	(10) ΔPD
Treated	-3.950*	-3.461*	-1.341**	-2.125**	-0.944	-1.075	-1.894**	-2.603***	-4.950***	-3.188**
	(-1.94)	(-1.97)	(-2.38)	(-2.55)	(-1.29)	(-0.95)	(-2.47)	(-2.87)	(-3.25)	(-2.02)
Firm Size	-0.270	-0.194							-0.417	-0.177
	(-0.87)	(-0.62)							(-1.34)	(-0.58)
Treated × **Firm Size**	0.979*	0.620							1.411***	0.763
	(1.79)	(1.21)							(3.12)	(1.53)
Book Leverage			0.012	0.009					0.013	0.007
			(0.81)	(0.53)					(0.84)	(0.47)
Treated × **Book Leverage**			-0.079*	-0.050					-0.084***	-0.053
			(-1.75)	(-1.33)					(-2.69)	(-1.49)
Tangible Asset Ratio					-0.022***	0.006			-0.024***	0.004
					(-2.82)	(0.48)			(-2.74)	(0.27)
Treated × **Tangible Asset Ratio**					0.030*	0.017			0.037*	0.028
					(1.73)	(0.74)			(1.68)	(0.97)
EBIT / TA							0.011	-0.046	-0.014	-0.044
							(0.37)	(-1.05)	(-0.39)	(-1.00)
Treated × **EBIT / TA**							0.361*	0.317***	0.311**	0.255**
							(1.84)	(2.75)	(2.64)	(2.24)
Sector FEs	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,128	1,117	1,128	1,117	1,128	1,117	1,128	1,117	1,128	1,117
Adj. R2	-0.001	0.031	-0.001	0.031	0.005	0.031	-0.001	0.032	0.004	0.027

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other German firms. The regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_2 X_{f,BA} + \beta_3 Treated_f X_{f,BA} + \alpha_k + \epsilon_f$$

where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $X_{f,BA}$ contains firm characteristics: firm size, book leverage, asset tangibility and concentration of its credit. Finally, α_k are sector fixed effects. Table 1 contains information on each variable used in the table, including their unit, definition, and source. The event is defined as zero before the announcement of the law in 2011Q2 and 1 after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in then probability of default in the prior period. All regressions cluster the standard errors at sector level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

1), have higher book leverage (column 3), have lower asset tangibility (column 5), and which are less profitable (columns 7 and 8) are more affected by empty creditors.

In particular, the empty creditor effect for the average firm size is between 3.46 pp and 3.95 pp, where a one standard deviation increase in size (1.08) sees the empty creditor effect reduce by 98 bps. Similarly, the empty creditor effect for a firm with an average book leverage (64.85 percent) is between 1.34 and 2.23 pp, where a one standard deviation increase in the firm‘s book leverage (16.5 percent) sees the empty creditor effect increase by an extra 1.3 pp. This result implies that the empty creditor effect has consequences for the capital structure decisions of firms, as CDS referenced firms have to embed the increased empty creditor effect into the trade-off between the debt tax shield and expected bankruptcy costs. This may explain the result of Subrahmanyam et al. (2017), where firms hold more cash, i.e., reduce leverage, once they become CDS referenced entities, as they are adjusting their optimal capital structure in response to the empty creditor effect.

In Table 6 we present the results for Equation 2 with bank-firm based variables as interactions. These are: the number of credit relationships a firm has, the average length of these relationships, the ratio of loan collateral to loan value, the average creditors' hedge ratio, and the concentration of the firm‘s debt.

A creditors incentive to push a firm into default is predicted to be increasing in their CDS hedge position, as their payout becomes larger when the firm defaults (Bolton and Oehmke, 2011). Indeed, column 10 in Table 6 indicates that the larger the CDS hedge positions of a firm‘s creditors, the larger the empty creditor effect is for the firm. In particular, the empty creditor effect for a firm with a one standard deviation increase in the firm‘s hedge ratio increased by an extra 1.1 pp.

Further, firms with long credit relationships (columns 1 and 2) are less severely affected by empty creditors, where a one standard deviation (7.27) increase in the length of a firm‘s credit relationships decreases the empty creditor effect by 1.13 pp. Similarly, firms with more collateral attached to their debt (columns 5 and 6) are less severely affected by empty creditors, where a one standard deviation (13 percent) increase in the amount of collateral coverage on a firm‘s debt increases the impact of empty creditors by 68 bps.

The effect of empty creditors is predicted to increase in the number of creditors a firm has, as creditors compete to “empty” themselves first (Bolton and Oehmke, 2011). This occurs because creditors would want to ensure they are protected against losses in case other creditors are empty creditors and push the firm into default. However, if a firm has multiple creditors, it is more able to substitute away from an empty creditor. Hence, these two effects make the question regarding the sign of the effect of the number of creditors a firm has on the intensity of the empty creditor effect an empirical one. From columns 3 and 4 in Table 6 we learn that the empty creditor effect for a firm with an average number of creditors (9.14) is between 1.94 and 2.55 pp. However, β_3 is

Table 6: Intensity of the Impact of Empty Creditors - Bank-Firm Based Variables

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD	(5) ΔPD	(6) ΔPD	(7) ΔPD	(8) ΔPD	(9) ΔPD	(10) ΔPD	(11) ΔPD	(12) ΔPD
Treated	-0.576	-1.256*	-1.940**	-2.545***	0.380	-0.669	-3.623***	-2.531***	-1.054	-1.489*	-1.546	0.804
	(-1.39)	(-1.82)	(-2.46)	(-2.70)	(0.32)	(-0.49)	(-7.09)	(-4.65)	(-1.56)	(-1.90)	(-0.84)	(0.53)
Len. of Relationships	-0.042**	-0.025									-0.031	-0.027
	(-2.27)	(-1.48)									(-1.59)	(-1.29)
Treated × **Len. of Relationships**	0.156**	0.109**									0.119*	0.165**
	(2.39)	(2.19)									(1.74)	(2.37)
# Creditor Relationships			-0.010	0.001							-0.032	-0.019
			(-0.48)	(0.03)							(-1.45)	(-0.71)
Treated × **# Creditor Relationships**			0.022	0.010							0.039*	0.020
			(0.93)	(0.44)							(1.69)	(0.73)
Collateral / Total Credit					-0.020***	-0.016***					-0.018***	-0.015***
					(-3.65)	(-3.97)					(-3.91)	(-4.03)
Treated × **Collateral / Total Credit**					0.052**	0.040*					0.039	0.033
					(2.02)	(1.68)					(1.40)	(1.36)
Firm Credit Concentration							0.000	-0.001***			0.000	-0.001***
							(1.15)	(-7.92)			(0.20)	(-13.38)
Treated × **Firm Credit Concentration**							-0.200***	-0.048			-0.157**	0.012
							(-10.59)	(-1.53)			(-2.44)	(0.19)
Net Notional / Total Credit									-0.181	-0.448***	-0.187	-0.577***
									(-1.40)	(-4.34)	(-1.27)	(-3.48)
Sector FEs	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,128	1,117	1,128	1,117	1,062	1,053	1,128	1,117	1,128	1,117	1,062	1,053
Adj. R2	0.004	0.032	-0.002	0.031	0.011	0.045	-0.002	0.031	-0.001	0.032	0.008	0.041

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other German firms. The regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_2 X_{f,BA} + \beta_3 Treated_f X_{f,BA} + \alpha_k + \epsilon_f$$

where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $X_{f,BA}$ contains bank-firm based variables: the number of credit relationships a firm has, the average length of these relationships, and the ratio of loan collateral to loan value. Finally, α_k are sector fixed effects. Table 1 contains information on each variable used in the table, including their unit, definition, and source. The event is defined as zero before the announcement of the law in 2011Q2 and 1 after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. All regressions cluster the standard errors at sector level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

statistically insignificant, hence we cannot conclude that the number of creditors affects the intensity of the empty creditor effect. This may be due to the competition to "empty first" offsetting the ability to substitute away from empty creditors. However, we find that firms which have a more concentrated market for their debt experience empty creditors more severely, as a one standard deviation (5.18) increase in the concentration of the firms credit market sees the impact of empty creditors increase by 100 bps.

8 Evidence at the Bank-Firm Level

Average empty creditor effect

As a further test, we estimate the average empty creditor effect at bank-firm level using the following specification:

$$\Delta PD_{b,f} = \beta_1 Treated_f + \beta_2 Z_{f,BA} + \alpha_k + \alpha_b + \epsilon_{b,f} \tag{3}$$

where firms, banks and sector are indexed by f, b and k, respectively. $\Delta PD_{b,f}$ is the change in bank b's estimate of firm f's probability of default after the implementation of the law change (i.e., $PD_{b,f,AI} - PD_{b,f,BA}$). The vectors $Treated_f$ and $Z_{f,BA}$ remain the same as in the main regressions. Finally, α_k and α_b are sector and bank fixed effects, respectively. As in the main regressions, β_1 is expected to be negative.

The results using Equation 3 and employing the data on probability of default at the bank-firm level are displayed in Table 7. To enhance comparison with the results reported in Table 3, we weigh the observations according to the the number of banks each firm has a credit relationship with when estimating Equation 3. This ensures firms with many banking relationships are not over-weighted in the bank-firm regressions. We find that CDS firms see a 1.39 to 2.83 pp decrease in their probability of default when the effect of empty creditors is removed, depending on whether sector fixed effects, bank fixed effects, or both are included or not. This is economically significant given that the average probability of default in the entire bank-firm sample is about 1.73 percent, and represents 18 percent to 36 percent of one standard deviation (7.95 pp) of the probability of default in the treatment group prior to the law change.[27] These results imply that our base results in Table 3 are robust to our decision to collapse the data to firm level.

Intensity of the empty creditor effect

Additionally, we test the bank characteristics that are associated with an increased empty creditor effect, by employing the following specification:

$$\Delta PD_{b,f} = \beta_1 CDSNetNotional_{b,f,BA} + \beta_2 X_{b,BA} + \beta_3 CDSNetNotional_{b,f,BA} X_{b,BA} + \epsilon_{b,f} \tag{4}$$

[27] In Appendix A.3, Table 20 provides descriptive statistics for the bank-firm sample.

where firms and banks are indexed by f and b, respectively. $\Delta PD_{b,f}$ is the change in bank b's estimate of firm f's probability of default after the implementation of the law change (i.e., $PD_{b,f,AI} - PD_{b,f,BA}$). $CDSNetNotional_{b,f,BA}$ is equal to average CDS Net Notional bank b purchased on firm f prior to the law change, weighted by the average amount of credit bank b provided firm f prior to the law change. The vector $X_{b,BA}$ contains pre-announcement average bank-based variables which have been demeaned: collateral to total credit, the bank's liquidity ratio, capital ratio, net-interest income to gross revenue (NII), and the banks monitoring expenses (staff and administrative expenses).

The results for Equation 4 are displayed in Table 8. Here we learn that banks that jointly trade CDS on their borrowers and which have more collateral attached to the credit they provide to these firms (column 1), are less liquid (column 2), are better capitalised (column 4), which earn a larger part of their revenue from interest generating activities (column 3) and which spend fewer resources monitoring (column 5) embed the empty creditor effect into their probability of default estimates of affected firms to a larger extent.

In particular, a one standard deviation increase in a bank's capital ratio (2.54 percent) which jointly holds the average CDS hedge position sees the empty creditor effect increase by an extra 10 bps. This may appear to be counter-intuitive as it may be expected that banks that are capital constrained are more severe empty creditors in order to convert the risky asset (the loan) into a safe asset (cash from the CDS insurance payment). However, as this is the joint effect of a bank with the average capital ratio which then trades CDS on their borrowers, the motive for trading CDS is less likely to be to improve their regulatory capital. Hence, an interpretation of this result is that if a bank trades CDS on borrowers when it is well capitalised, the bank will embed the empty creditor effect into their probability of default estimates of affected firms to a larger extent. This may be due to the fact that banks' trading of CDS on their borrowers alleviates credit constraint as it reduces the credit exposure to the borrowers, hence the trading of CDS may be motivated by the reduced capital contraints rather than the payout from the CDS contract. Another explanation for this finding is that they may be less willing to embed the empty creditor effect into their probability of default estimates as doing so would further exacerbate their capital constraints.

Similarly, the result for the collateral to total credit variable may seem to contradict the results from Table 6. Where the result in Table 6 indicates that a higher collateral ratio implies a reduced empty creditor effect, the result in Table 8 indicates that if a bank trades CDS on the borrower and jointly has more collateral attached to the credit that they provide to a CDS referenced entity, the bank will embed the empty creditor effect into their probability of default estimates of affected firms to a larger extent. In particular, a one standard deviation increase in the collateral ratio (52.37 percent), where the bank holds the average CDS hedge position, sees the empty creditor effect increase

by an extra 100 bps. While collateral may reduce a creditors incentive or need to trade CDS on a borrower, as it is to some degree a substitute in laying off credit risk, collateral provides empty creditors with a larger pay-off when the firm defaults. Hence, this result is not in contradiction with the result from Table 6 as it does not measure the effect of the creditors incentive to trade CDS.

Finally, the variables NII and monitoring expenses measure different aspects of a bank's business model. Banks with higher NII are less likely to purchase CDS for trading purposes and thus their CDS trading behaviour is a stronger signal of the empty creditor effect. Hence, a one standard deviation increase in the NII ratio (16.3 percent), where the bank holds the average CDS hedge position, sees the empty creditor effect increase by an extra 2.64 pp.

Banks that allocate a large portion of their resources to expenses associated with monitoring are more likely to be relationship lenders which gather valuable soft information on their borrowers over time. Hence, a one standard deviation increase in the monitoring expense (26.82 percent), where the bank holds the average CDS hedge position, sees the empty creditor effect decrease by 5.36 pp. This implies CDS referenced firms which borrow from relationship lenders are mostly unaffected by the empty creditor effect.

9 The Empty Creditor Spread

To test if the incentive for empty creditors to push CDS firms into default is priced into CDS spreads as assumed by Bolton and Oehmke (2011), we employ the following regression specification:

$$\Delta CDSSpread_f = \beta_1 Treated_f + \beta_2 X_{f,BA} + \beta_3 Z_{f,BA} + \alpha_k + \epsilon_f \tag{5}$$

Here, $\Delta CDSSpread_f$ is the change in firm f's average five year MMR CDS spread after the implementation of the law change (i.e., $CDSSpread_{f,AI} - CDSSpread_{f,BA}$). The vector $Z_{f,BA}$ includes macroeconomic variables to control for the effect the general economic environment has on changes to the probability of default. Finally, α_k are sector fixed effects.

$Treated_f$ is equal to 1 if firm f had CDS traded on it over the entire sample period and was a German reference entity, and equal to 0 if it has CDS traded on it but was domiciled in another European country. The use of German non-CDS referenced entities as a control group is not feasible for the pricing regressions as there is no CDS price for a firm if there are CDS traded on its debt. Therefore, we define the control group in the pricing regressions as other European CDS reference entities (i.e., European CDS reference entities excluding German reference entities). The use of other European CDS reference entities as a control group is motivated by the fact that they should not have

experienced any change in their exposure to empty creditors over this time period.

The vector $X_{f,BA}$ contains average, pre-announcement, firm control variables, which includes Tobin's Q, return on equity, volatility of assets, Altman z-score, market value of assets and sectoral loss given default. While the treatment and control groups are matched as in the previous regressions, we include these firm controls to further account for any differences between German and other European CDS referenced entities, as well as other factors that affect CDS spreads as identified in the literature.

The sign of β_1 could either be positive or negative. While the removal of the impact of empty creditors for the treated firms would cause the sign of β_1 to be negative. The fact that the restructuring clause, for treated firms, has no value before the event and has value after the event, would cause the sign of β_1 to be positive (Packer and Zhu, 2005). However, a negative β_1 is evidence that the incentive for empty creditors to push CDS firms into default is priced into CDS spreads and dominates the impact of the restructuring clause.

Table 9 presents the results of estimating Equation 5, where β_1 provides a joint estimate of the effect of empty creditors and the implementation of the restructuring clause. While statistically insignificant in one of the four specifications, all treatment effects take a negative sign, suggesting that the effect of empty creditors dominates the restructuring effect. We take this as evidence for the impact of empty creditors being priced into the CDS spreads of affected reference entities.[28]

We find that German CDS firms saw a 49 to 122 bps decrease in their five year CDS spreads when the effect of empty creditors was removed, depending on whether sector fixed effects, controls, or both are included. This is economically significant given that the average five year CDS spread in the entire sample is about 170 bps, and represents 30 percent to 76 percent of one standard deviation (161 bps) of the five year CDS spread in the treatment group prior to the law change.[29]

10 Robustness

In this section, we first show that the treatment and control groups follow parallel trends prior to the event. We further present results regarding several robustness tests (matching, choice of matching variables, and length of the event window). Additionally, we show that the results are robust to placebo testing.

[28] These results are robust to shortening the event window as presented in Appendix A.3, Table 17. In this table, the pre/post event windows are narrowed to four quarters as opposed to eight quarters. Using a shorter event window, the results become more statistically significant and the beta estimates remain negative in sign.

[29] In Appendix A.3, Table 21 provides descriptive statistics for the sample used in these regressions.

Parallel trends

An assumption of the difference-in-differences methodology is that the treatment and control group follow parallel trends prior to the event. To investigate the validity of this assumption, we conduct the same exercise as the main regressions using each quarter, starting from eight quarters before the announcement of the reform, as the event. Figure 6 shows the parallel trends test where the bars in the graph depict the 95% confidence interval for the treatment effect of each quarter. The assumption holds as the treatment effect is insignificant for the quarters prior to the announcement in 2011Q2, as the bars in the graph prior to the event cross the x-axis

Varying the matching variables

Our main results are robust to matching the treatment and control groups. The results when not matching (i.e., taking the universe of control firms and treated firms) are shown in columns 1 and 2 of Table 10. We notice that all treatment effects are statistically significant, and their magnitude is economically similar to when matching.

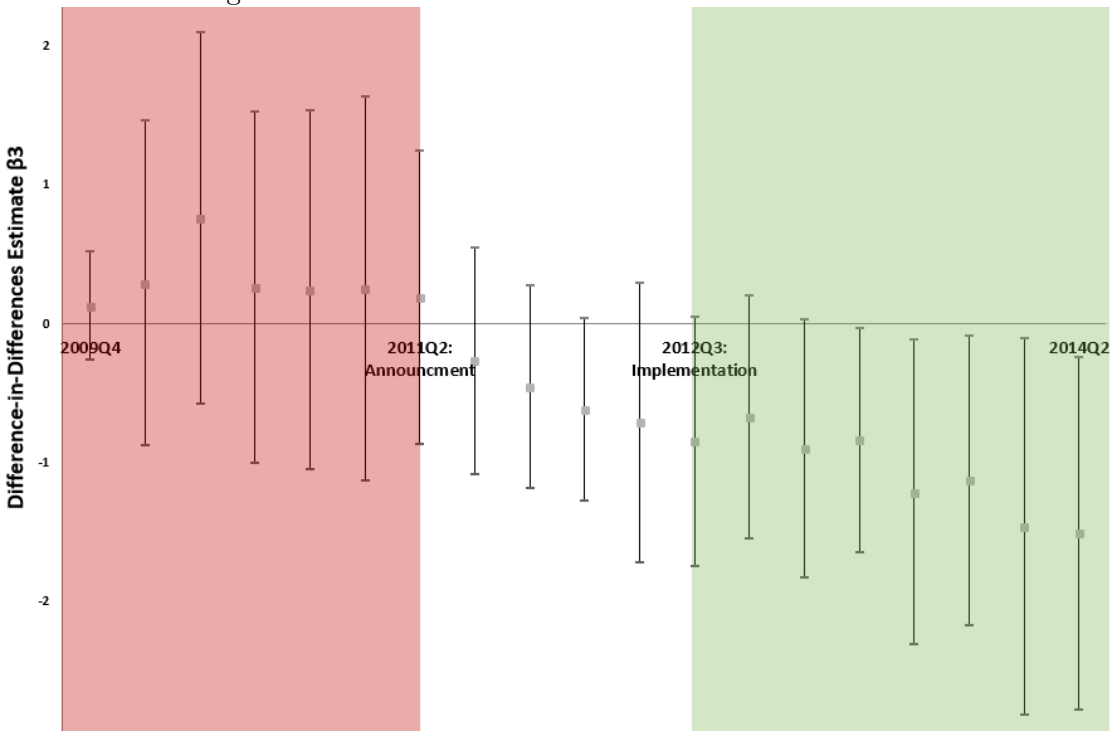
Additionally, the results are robust to the choice of matching variables. Columns 2 to 10 of Table 10 presents results where the employed control sample varies using different matching variables. The sign, statistical significance, and economic magnitude remain mostly unchanged across the different combinations of matching variables. Thus, the main results are robust to the choice of matching variables.

Alternative control group

The purpose of matching is to make the treatment and control group more similar so as to reduce selection bias. An alternative approach to testing the robustness of the main results to selection bias is to use a control group that is more similar to the treatment group. In this context, we can use European CDS firms as a control group, as opposed to German non-CDS firms. While European CDS firms may be more similar to German CDS firms than German non-CDS firms, as previously discussed, the use of European CDS firms results in the treatment effect including the ability to restructure a firm. The main results are robust to the use of an alternative control group and are presented in columns 1 and 2 of Table 11. Relative to the main results, we find the results using European CDS firms as a control group to be lower in magnitude. This may be as a result of the German CDS firms gaining the ability to restructure due to the law change.

Columns 3 and 4 of Table 11 relate to columns 9 and 10 of Table 8. Here we test the robustness of our finding that the CDS trading behaviour of referenced firms' creditors affects the intensity of the empty creditor effect. As with the main results, the use of the different control group reduces the magnitude of the impact, where a one standard deviation increase in the firm‘s CDS hedge ratio (2.47) sees the empty creditor effect increase by an extra 63 bps. While the magnitude of the effects are smaller when using

Figure 6: Parallel Trends - Treatment Effect Over Time



Note: The graph represents the point estimate (grey squares) and the 95% confidence interval (black lines) for the difference-in-differences between CDS German firms and non-CDS German firms for each period, estimated using the following regression equation:

$$PD_{f,t} = \beta_1 Treated_{f,t} + \beta_2 Quarter + \beta_3 Treated_{f,t} Quarter$$

where the treatment variable $Treated_{f,t}$ is defined as 1 if the borrower is a German CDS reference entity over the entire sample, and 0 where the borrower is a German firm with no CDS traded on it. β_3 and its 95% confidence interval are plotted in the graph. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period.

Table 7: Empty Creditor Effect - Base Results - Bank-Firm Level

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD	(5) ΔPD	(6) ΔPD	(7) ΔPD	(8) ΔPD
CDS Reference Entity	-1.401***	-1.497**	-2.758**	-2.826**	-1.393***	-1.403**	-2.216***	-2.160**
	(-3.06)	(-2.48)	(-2.46)	(-2.46)	(-2.94)	(-2.46)	(-2.68)	(-2.45)
Legal Size Control	No	Yes	No	Yes	No	Yes	No	Yes
Bank FEs	No	No	No	No	Yes	Yes	Yes	Yes
Sector FEs	No	No	Yes	Yes	No	No	Yes	Yes
Observations	4357	4357	4356	4356	4344	4344	4343	4343
Adj. R2	0.000	0.000	0.044	0.044	0.099	0.098	0.141	0.141

Note: Difference-in-differences regression results for the change in probability of default, where the regression equation is:

$$\Delta PD_{b,f} = \beta_1 Treated_f + \beta_2 Z_{f,AI} + \alpha_k + \alpha_b + \epsilon_{b,f}$$

where $\Delta PD_{b,f}$ is the change in bank b's estimate of firm f's probability of default after the implementation of the law change (i.e., $PD_{b,f,AI} - PD_{b,f,BA}$). $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $Z_{f,AI}$ includes a control for the legal size of firm f to control for the effect of a particular clause in the reform that affected only firms which were classified as large under the German Commercial Code. Finally, α_b and α_k are bank and sector fixed effects, respectively. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event. The pre-announcement period runs from 2009Q3 to 2011Q2, and the post-implementation period runs from 2012Q3 to 2014Q2. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. All regressions cluster the standard errors at sector level. t statistics are reported between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Intensity of the Impact of Empty Creditors - Bank-Firm Level

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD	(5) ΔPD
CDS Net Notional	-0.005**	-0.007**	-0.010**	0.007*	-0.011**
	(-2.55)	(-2.25)	(-2.09)	(1.82)	(-2.17)
Collateral / Total Credit	-0.003				
	(-1.11)				
Net Notional × Collateral / Total Credit	-0.004**				
	(-2.37)				
Liquidity		0.149			
		(1.02)			
Net Notional × Liquidity		0.002**			
		(1.96)			
NII			-0.752		
			(-1.25)		
Net Notional × NII			-0.034*		
			(-1.88)		
Capital				-0.021	
				(-0.32)	
Net Notional × Capital				-0.008*	
				(-1.96)	
Monitoring					-0.123
					(-0.16)
Net Notional × Monitoring					0.042**
					(2.08)
Observations	2640	4036	4031	4036	4031
Adj. R2	-0.001	0.000	-0.001	-0.001	-0.001

Note: Difference-in-differences regression results where the regression equation is:

$$\Delta PD_{b,f} = \beta_1 CDSNetNotional_{b,f,BA} + \beta_2 X_{b,BA} + \beta_3 CDSNetNotional_{b,f,BA} X_{b,BA} + \epsilon_{b,f}$$

where $\Delta PD_{b,f}$ is the change in bank b's estimate of firm f's probability of default after the implementation of the law change (i.e., $PD_{b,f,AI} - PD_{b,f,BA}$). $CDSNetNotional_{b,f,BA}$ is equal to average CDS Net Notional bank b purchased on firm f prior to the law change, weighted by the average amount of credit bank b provided firm f. The vector $X_{b,BA}$ contains pre-announcement average bank-based variables: collateral to total credit, the bank's liquidity ratio, capital ratio, net interest income to gross revenue (NII), and the banks monitoring expenses (staff and administrative expenses). Table 1 contains information on each variable used in the table, including their unit, definition, and source. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). All regressions cluster the standard errors at the sector level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 9: Empty Creditor Effect In Prices

	(1) $\Delta Spread$	(2) $\Delta Spread$	(3) $\Delta Spread$	(4) $\Delta Spread$
$Treated$	-49.365**	-55.087**	-55.804	-122.225**
	(-2.04)	(-2.32)	(-0.83)	(-2.39)
Macro Controls	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes
Sector FEs	No	No	Yes	Yes
Observations	92	85	54	45
Adj. R2	0.012	0.070	0.167	0.119

Note: Difference-in-differences regression results for the change in the five year CDS spread (MM), where the regression equation is:

$$\Delta CDSSpread_f = \beta_1 Treated_f + \beta_2 X_{f,BA} + \beta_3 Z_{f,BA} + \alpha_k + \epsilon_f$$

where $\Delta CDSSpread_f$ is the change in a firm's average five year MMR CDS spread after the implementation of the law change (i.e., $CDSSpread_{f,AI} - CDSSpread_{f,BA}$). The vector $X_{f,BA}$ contains average, pre-announcement, firm control variables, which includes Tobin's Q, return on equity, volatility of assets, Altman z-score, market value of assets and sectoral loss given default. $Treated_f$ is equal to 1 if a firm had CDS traded on them over the entire sample period and is a German reference entity, and equal to 0 if it has CDS traded on it but is domiciled in another European country. The vector $Z_{f,AI}$ includes macroeconomic variables, to control for the effect the general economic environment across countries. Finally, α_k are sector fixed effects. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The pre-announcement period runs from 2009Q2 to 2011Q2, and the post-implementation period runs from 2012Q3 to 2014Q1. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, and Altman z-score. All regressions cluster the standard errors at sector level. t statistics are reported between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 10: Empty Creditor Effect - Choice of Matching Variables

Matching Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Size			Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Book Leverage					Yes	Yes	Yes	Yes	Yes	Yes
Alternative Z-Score							Yes	Yes	Yes	Yes
Prior change in then Probability of Default									Yes	Yes
Treated	-2.110***	-2.043**	-1.516**	-2.247***	-1.623**	-1.880**	-1.711**	-2.876**	-1.354*	-1.993**
	(-3.38)	(-2.40)	(-2.57)	(-2.71)	(-2.53)	(-2.35)	(-2.16)	(-2.55)	(-1.75)	(-2.05)
Legal Size Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FEs	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	64468	64467	2345	2339	2345	2339	1726	1720	1,128	1,117
Adj. R2	0.003	0.073	0.001	0.022	0.002	0.027	0.005	0.021	-0.001	0.031

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other German firms. The regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_4 Z_{f,BA} + \alpha_k + \epsilon_f$$

where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $Z_{f,BA}$ includes a control for the legal size of firm f to control for the effect of a particular clause in the reform that affected only firms which were classified as large in the reform. Finally, α_k are sector fixed effects. Here the event is defined as zero before the announcement of the law in 2011Q2 and 1 after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The treatment and control group are matched using the coarsened exact matching method on different variables that vary across the columns as indicated. For each variable, the sample is split into quartiles and then matched on the quartile membership (e.g., treatment and control groups in the 4th quartile of firm size). All regressions cluster the standard errors at sector level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

European CDS firms as the control group, the sign is the same as the main results and the effects remain statistically significant. Hence, we conclude that selection bias is not driving the results.

Shorter event window period

A potential concern is that the main specification employs data from only those firms that survive the whole period, i.e., survivorship bias. This is due to the fact that the variable of interest is the change in the probability of default, requiring probability of default observations both before and after the event. Hence, the firms need to survive the entire event window in order to be included in the sample. To test if this is driving the results we shorten the event window to 1 year (4 quarters) before the announcement and 1 year after the implementation. The results are robust to shortening the event window and are presented in columns 3 & 4 of Table 12.

Placebo Test - Small Bang Protocol

To conduct a placebo test, we run the same analysis as the main regressions, including the time period of averaging, and use the CDS Small Bang as the event. The "Small Bang" brought a greater degree of standardization in the CDS market in 2009, and spurred more trading in CDS. This is a strong placebo test as the CDS Small Bang would have affected the treatment group, German CDS reference entities, but not the control group, German non-CDS firms. However, the impact of empty creditors should only be affected by changes to the bankruptcy law or the CDS restructuring clause, thus there should be no treatment effect with the Small Bang as an event. The results in the last two columns of Table 12 confirm this as the treatment effect is statistically insignificant across both specifications.

Averaging bank-firm level variables

While the bank-firm level results in Table 7 imply that our main results in Table 3 are robust to our decision to average the bank-firm level data, to further test the robustness of the main results, we conduct two additional robustness tests. First, while for the main results we took the credit weighted average of the bank-firm level variables, as a robustness test we take the unweighted, or simple average. The results of the main regressions using a sample constructed using unweighted averages are presented in Table 13. We find that the positive, and significant, evidence for the impact of empty creditors remains. As all treatment effects are statistically significant, and their magnitude is economically similar to when using credit weighted averages, we conclude that the main results are robust to the use of weights when averaging.

Second, we use the change in the probability of default in the main results, while as a robustness test we use the level, or non-time-averaged data. To do so, we employ an

Table 11: Empty Creditor Effect - Base Results - Euro Controls

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD
Treated	-1.041**	-0.612*	-0.791	-0.196
	(-2.34)	(-1.85)	(-1.41)	(-0.47)
Net Notional / Total Credit			0.000	-0.000***
			(0.68)	(-6.39)
Treated **X Net Notional / Total Credit**			-0.179	-0.257**
			(-1.36)	(-2.54)
Sector FEs	No	Yes	No	Yes
Observations	166	145	166	145
Adj. R2	0.020	-0.046	0.013	-0.033

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other European CDS firms. The regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_2 X_{f,BA} + \beta_3 Treated_f X_{f,BA} + \alpha_k + \epsilon_f$$

where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on it over the entire sample period, and equal to 0 if firm f is a European firm with CDS traded on it over the entire sample period. The vector $X_{f,BA}$ contains the firm's creditors' hedge ratio. Finally, α_k are sector fixed effects. Table 1 contains information on each variable used in the table, including their unit, definition, and source. The event is defined as zero before the announcement of the law in 2011Q2 and 1 after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). All regressions cluster the standard errors at sector level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 12: Empty Creditor Effect - Different Events

	(1) Change in Bankruptcy Law 2 Year Window	(2) Change in Bankruptcy Law 2 Year Window	(3) Change in Bankruptcy Law 1 Year Window	(4) Change in Bankruptcy Law 1 Year Window	(5) CDS Small Bang	(6) CDS Small Bang
	ΔPD	ΔPD	ΔPD	ΔPD	ΔPD	ΔPD
Treated	-1.354*	-1.993**	-0.869*	-1.441*	-0.596	-0.746
	(-1.75)	(-2.05)	(-1.72)	(-1.75)	(-1.10)	(-0.99)
Legal Size Control	Yes	Yes	Yes	Yes	Yes	Yes
Sector FEs	No	Yes	No	Yes	No	Yes
Adj. R2	-0.001	0.031	0.004	0.033	-0.000	0.068
Observations	1,128	1,117	1091	1080	774	765

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other German firms. The regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_4 Z_{f,BA} + \alpha_k + \epsilon_f$$

where ΔPD_f is the change in a firm's average probability of default after the event (i.e., $PD_{f,A} - PD_{f,B}$). $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $Z_{f,BA}$ includes a control for the legal size of firm f to control for the effect of a particular clause in the reform that affected only firms which were classified as large under the German Commercial Code. Finally, α_k are sector fixed effects. Here the event definition varies across the columns. For columns 1-4, the event is the change to the German Bankruptcy Law in 2012 (ESUG). While in columns 1-2 the event window is two years before and after the event, in columns 3-4 the window is shortened to one year. For columns 5-6 the CDS Small Bang is the event where seven quarters before and seven quarters after the event are employed. These are averaged by firm and period (pre/post). The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. All regressions cluster the standard errors at sector level. t statistics are reported between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 13: Empty Creditor Effect - Base Results - Simple Averages

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD
Treated	-1.454***	-1.873**	-2.165**	-2.589**
	(-2.67)	(-2.43)	(-2.47)	(-2.54)
Legal Size Control	No	Yes	No	Yes
Sector FEs	No	No	Yes	Yes
Observations	1,167	1,167	1,156	1,156
Adj. R2	0.000	0.005	0.030	0.034

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other German firms. The regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_4 Z_{f,BA} + \alpha_k + \epsilon_f$$

where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the simple average is employed. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $Z_{f,BA}$ includes a control for the legal size of firm f to control for the effect of a particular clause in the reform that affected only firms which were classified as large under the German Commercial Code. Finally, α_k are sector fixed effects. Here the event is defined as zero before the announcement of the law in 2011Q2 and 1 after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in then probability of default in the prior period. All regressions cluster the standard errors at sector level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 14: Empty Creditor Effect - Base Results - No Time Averaging

	(1) $PD_{f,t}$	(2) $PD_{f,t}$	(3) $PD_{f,t}$	(4) $PD_{f,t}$
Treated	-0.968	-4.319**	-3.656	-5.107*
	(-1.32)	(-2.94)	(-1.56)	(-1.95)
Post ESUG	0.550	-0.195	0.579	-0.042
	(1.62)	(-0.68)	(1.09)	(-0.10)
Treated **X Post ESUG**	-1.402***	-0.671***	-1.682***	-1.075***
	(-3.50)	(-10.55)	(-3.83)	(-2.98)
Legal Size Control	No	Yes	No	Yes
Sector FEs	No	No	Yes	Yes
Observations	10,886	10,886	10,886	10,886
Adj. R2	0.001	0.070	0.122	0.160

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other German firms. The regression equation is:

$$PD_{f,t} = \beta_1 Treated_f + \beta_2 Post\ ESUG_t + \beta_3 Treated_f Post\ ESUG_t + \beta_4 Z_f + \alpha_k + \epsilon_{f,t}$$

Where, $PD_{f,t}$ is firm f's average probability of default in quarter, t. Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Post\ ESUG_t$ is equal to 0 for eight quarters before the announcement of the law in 2011Q2 and 1 for eight quarters after the implementation in 2012Q3. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $Z_{f,BA}$ includes a control for the legal size of firm f to control for the effect of a particular clause in the reform that affected only firms which were classified as large under the German Commercial Code. Finally, α_k are sector fixed effects. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. All regressions cluster the standard errors at sector and quarter level. We report t statistics between parentheses *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

alternative difference-in-differences specification:

$$PD_{f,t} = \beta_1 Treated_f + \beta_2 Post\,ESUG_t + \beta_3 Treated_f Post\,ESUG_t + \beta_4 Z_f + \alpha_k + \epsilon_{f,t} \quad (6)$$

where firms and quarters are indexed by f and t, respectively. $PD_{f,t}$ is firm f's average probability of default in quarter t. $Treated_f$ is equal to 1 if a firm f had CDS traded on it over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. $Post\ ESUG_t$ is equal to 1 for eight quarters after the implementation in 2012Q3 and 0 for eight quarters before the announcement of the law in 2011Q2. Finally, α_k are sector fixed effects and $\epsilon_{f,t}$ is an error term. In all regressions, the standard errors are clustered at sector level to account for the correlation structure of the errors (Petersen, 2009).

The results in Table 14 indicate that the main results are robust to using pre/post averages, as all treatment effects are statistically significant and their magnitude is economically similar to the main results. Further, the large t-statistics for the difference-in-differences estimates, β_3, relative to those from the main result may indicate the need to account for the serial correlation of the standard errors as is done in the main regressions.

11 Conclusion

Firms having credit-default swaps (CDS) traded on them may face "empty creditors". Indeed, CDS allow creditors to undo their economic exposure to credit deterioration. This paper investigates the importance of empty creditors by employing a quasi-natural experiment, i.e., a change in German bankruptcy law, that removes the impact of empty creditors.

We find that the removal of the effect of empty creditors following the change in bankruptcy law leads to a reduction in the probability of default for CDS firms of about 2 pp. Additionally, we find that the impact of empty creditors as measured through the drop in the probability of default increases in the average CDS hedge position of its creditors. Specifically, the empty creditor effect for a firm with a one standard deviation increase in the firm's hedge ratio increased by an extra 1.1 percentage point. Further, we find that firms with less concentrated debt, longer credit relationships and firms with higher average collateral ratios of their debt are less affected by empty creditors. While financially risky firms are severely affected by empty creditors, safe firms are not affected. Banks that are not capital constrained and that are liquidity constrained embed the empty creditor effect into their probability of default estimates of affected firms to a larger extent. Furthermore, banks' business models affect the degree to which they embed the empty creditor effect. Banks that monitor their creditors less and that earn a smaller portion of their income from interest activities embed the empty creditor effect to a larger extent.

We account for endogeneity issues which affect the extant literature by employing an exogenous shock to the bankruptcy law of Germany, specifically changes to the restructuring law. We are, therefore, able to identify the direct effect of CDS trading on a firm‘s probability of default.

While CDS have been subject to intense criticism, the current lack of understanding of the actual social benefit and costs of CDS, as well as what causes CDS to have these costs and benefits, makes it difficult for policy makers to determine appropriate regulation. While this paper finds evidence for a negative side effect of CDS trading, this does not necessarily mean regulators should aim to reduce the level of CDS trading. On the contrary, CDS may still have large benefits from a risk diversification point of view for banks and thus improve the credit supply to these CDS firms. However, this paper shows the conditions which cause a negative side-effect, empty creditors, and that it can be removed.

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A Appendix

A.1 Credit default swap contracts

Similar to insurance contracts, credit default swaps (CDSs) transfer credit risk of an underlying reference entity from the buyer of the contract to the seller. More specifically, single name corporate CDSs provide the buyer of the CDS contract protection against the default of a single corporate reference entity. CDSs differ from traditional insurance, since in traditional insurance the protection buyer is usually required to be exposed to the risk which they are being protected against. For example, in order to purchase fire insurance on a house, it is usually the case that the insured party needs to own the property. Further, in a traditional insurance contract, the level of protection can not usually exceed the value of the underlying exposure, e.g., the value of the house being insured for fire damage. This is not the case for CDS contracts as credit insurance may be purchased without any exposure to the reference entity and without any cap on insured value.

A single name corporate CDS contract has several key inputs. First, it outlines the parties involved (i.e., the protection buyer and seller) as well as the source of the credit risk (i.e., the reference firm). Second, it specifies the maturity of the contract, ranging typically from one to ten years, with the most common maturity being five years. Third, it defines what type of credit event (i.e., restructuring or bankruptcy) would trigger the payment of the protection value from the seller to the buyer of the CDS contract. Fourth, the CDS spread is specified, which is the premium that the buyer pays to the seller for the credit risk protection on a periodic basis. Finally, the settlement type is specified, which can either be a cash settlement or a physical settlement. A cash settlement entails paying the difference between the protection value and the value of the underlying debt at the time of the credit event, while a physical settlement entails the CDS buyer transferring a defined bond to the CDS seller in return for the full protection amount (ISDA, 2003).

As CDSs are bilateral contracts, which include many clauses that could be changed, there is a large potential for negotiation. However, this negotiation may harm CDS market liquidity as it takes time to negotiate these contracts. It may further increase the complexity of managing a portfolio of CDSs, and thus negatively impact the liquidity of the market, especially in the case that each contract has individually negotiated clauses. In order to improve the liquidity of the CDS market the International Swap and Derivative Association (ISDA) released the 2003 ISDA Credit Definitions which define standardized terms for CDS contracts. The 2003 ISDA Credit Definitions standardized the contracts by reference entity type (e.g., single name corporate) and the domicile country of the reference entity (i.e., North American vs European) (ISDA, 2009a,b). The type of standardized contract to be used is determined by the type of the reference entity and the domicile of that entity (e.g., CDS on a German firm would be based on the Standard European corporate CDS contract).

The standard North American CDS contract and the European CDS contract mainly differ in their restructuring clause (i.e., what the contract defines to be a credit event). The 2003 ISDA Credit Definitions defines four types of restructuring clauses: Old Restructuring, Modified Restructuring, Modified-Modified Restructuring, and No Restructuring. Prior to 2009 both the North American single-name CDS contract and the European single-name CDS contract traded with restructuring clauses which recognise restructuring as a credit event. At that time, the North American contracts that traded with a Modified Restructuring limited the deliverable obligation to debt with a maturity no more than 30 months. On the other hand, the European contacts that traded with a Modified-Modified Restructuring limited the deliverable obligation to debt with a maturity no more than 60 months for restructured obligations and 30 months for all other obligations.

In order to further improve the liquidity of the respective CDS markets, ISDA further standardized the North America corporate CDS (via the CDS Big Bang) and the European corporate CDS (via the CDS Small Bang). Both reforms were introduced at similar times, with the CDS Small Bang being implemented in June 2009 and the CDS Big Bang implemented in April 2009. Further, both reforms included further standardizations of the settlement auctions along with other changes (Markit, 2009a,b). (Markit, 2009a). The key difference between these reforms is that the North America corporate CDS contract was standardized to no longer recognise restructuring as a credit event (i.e., trade with a no restructuring clause), while the European corporate CDS contract continued to recognise restructuring as a credit event (i.e., trade with a Modified-Modified Restructuring clause).

A.2 German insolvency law & reform

The Insolvency Act (Insolvenzordung - hereafter InsO) governs all insolvency proceedings in Germany. InsO came into force on 1 January 1999, and has since been amended on occasion, with the Act for the Further Facilitation of the Restructuring of Companies (ESUG) being the last major reform. Other amendments include an amendment to the personal insolvency code regarding the discharge of residual debt, in July 2014, as well as a reform to the insolvency code that governs group insolvencies, in March 2015 (Erb and Tashiro, 2014, 2016).

When InsO was introduced in 1999 to modernize Germany's Bankruptcy code, it replaced the existing liquidation-orientated code with a code that allowed for the potential for distressed firms to be restructured, amongst other changes. While the enactment of InsO achieved a modernization of the Germany bankruptcy code, the cumbersome and costly process and the complex requirements for debt-equity swaps (a key tool in restructuring) meant that the ability for distressed firms to restructure and continue operating was still severely hindered (Halladay and Jark, 2012).

InsO automatically triggers a formal bankruptcy if a firm faces insolvency, if an

insolvency is imminent, or if a firm is over-indebted. In Germany prior to 2012, the insolvency law all but prohibited in-court restructuring, which is evident as they occurred in only 2 percent of insolvency cases (Höher, 2012). Out-of-court restructuring was similarly unlikely in German prior to 2012 due to several institutional biases against workouts (Jostarndt and Sautner, 2010). For example, if the management of a firm does not file for bankruptcy in a timely manner, they may be held personally liable, and may be sentenced for up to three years in prison.[30] Further institutional biases against out-of-court restructuring include the difficulty in obtaining senior financing, the restrictions on informal debt-equity swaps, uncertainty around voluntary winding-ups, and the fact that the German government provides social security funding to pay salaries of firms which file for bankruptcy.

This deficiency in the ability to restructure a firm was made clear with the enactment of the European Insolvency Regulation (EIR) adopted by the EU Council in May 2002. This resulted in the German insolvency law becoming less attractive relative to other European insolvency regimes. The EIR entitles firms to file for insolvency in any member state in which they have an establishment for assets, while the main insolvency proceedings have to be commenced where a firm's Centre of Main Interest (COMI) is located. A firm‘s COMI is determined by the location of its registered office in the absence of proof to the contrary (i.e., its COMI is located in another jurisdiction). Thus, a firm could potentially move its COMI to another member state where the bankruptcy law is more favourable to the firm. However, this is unlikely to happen as it is costly for firms to move their COMI and may be rebutted (Kaczor, 2010), and indeed it remained rare for firms to change their COMI. Therefore, although the enactment of the regulation, did not, in general, cause firms to move their COMI, it made the deficiency clear, which the German Bundestag (German parliament) aimed to address by reforming InsO.

The German Bundestag aimed to reform InsO to improve restructuring and recapitalization opportunities for firms, and thus make German insolvency more attractive compared to foreign insolvency regimes (Dimmling, 2015). In 2012, German insolvency law was substantially reformed by the so-called ESUG reform act (i.e., Gesetz zur weiteren Erleichterung der Sanierung von Unternehmen). ESUG was initially developed in September 2010, announced in March 2011, passed by the German Bundestag in December 2011, and came into effect in March 2012. Although a full year passed between its first draft and it being passed in parliament, there was uncertainty as to when it would be passed and the exact details of what was to be included in the reform (Closset and Urban, 2018).

ESUG made five major modifications to German insolvency regulation. The reform

[30] The relevance of this law can be seen in the response of the German Federal Ministry of Justice and Consumer Protection to the economic consequences of the COVID-19 pandemic. Part of the ministry's response was to pass legislation temporarly suspending the obligation to file for insolvency with the "COVID-19 Insolvency Suspension Act" (COVInsAG - BGBl. I S. 569). The result of this suspension of obligation was a 13.1 percent reduction in the number of a business filing for insolvency during first three quarters of 2020 compared to the same period in 2019 (Destatis, 2020).

made preliminary creditor committees (PCC) mandatory for large companies and possible for smaller companies at the request of the debtor. Additionally, it extended the already existing right of the creditors' meeting to choose an insolvency administrator for the PCC. Further, the reform strengthened existing insolvency plan proceedings as well as created new instruments to support self administration. Lastly, the reform allowed for the possibility of a debt-equity-swap as a part of an insolvency plan. Overall, the reform made insolvency proceedings in Germany more effective and predictable, and changed the "liquidation culture" into more of a "rescue culture" (Erb and Tashiro, 2014).

A.3 Supplementary tables

Table 15: Empty Creditor Effect - Full Distribution Quartile Separation

	(1) Full Sample ΔPD	(2) Quartile 1 ΔPD	(3) Quartile 2 ΔPD	(4) Quartile 3 ΔPD	(5) Quartile 4 ΔPD
CDS Reference Entity	-1.308**	-0.168	-0.873*	-2.702***	-6.008**
	(-2.20)	(-1.16)	(-1.88)	(-3.29)	(-2.13)
Legal Size Controls	Yes	Yes	Yes	Yes	Yes
Observations	1,128	363	404	205	156
Control Firms	1,088	349	386	201	152
Treated Firms	40	14	18	4	4
Adj. R2	0.000	-0.003	-0.002	-0.003	-0.004

Note: Difference-in-differences regression results for the change in probability of default, where the regression equation is: The regression equation is $\Delta PD_f = \beta_1 Treated_f + \beta_4 Z_{f,BA} + \epsilon_f$ where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $Z_{f,BA}$ includes a control for the legal size of firm f to control for the effect of a particular clause in the reform that affected only firms which were classified as large under the German Commercial Code. Here the event is defined as zero before the announcement of the law in 2011Q2 and 1 after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. All regressions cluster the standard errors at sector level. In column 1 the full sample is used. In column 2-5 only those firms in the indicated quartile of $PD_{f,AI}$ are included. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 16: Empty Creditor Effect - Continuous Size Controls

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD
Treated	-1.769*	-2.117**	-2.736**	-3.248**
	(-1.89)	(-2.11)	(-2.43)	(-2.62)
Firm Size	0.079	0.062	0.160	0.184
	(0.34)	(0.26)	(0.91)	(1.05)
Legal Size Control	No	Yes	No	Yes
Sector FEs	No	No	Yes	Yes
Observations	1,167	1,167	1,156	1,156
Adj. R2	-0.001	0.004	0.030	0.034

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other German firms. The regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_4 X_{f,BA} + \beta_4 Z_{f,BA} + \alpha_k + \epsilon_f$$

where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $X_{f,BA}$ contains firm size to further control for the difference is size between the treatment and control group. The vector $Z_{f,BA}$ includes a control for the legal size of firm f to control for the effect of a particular clause in the reform that affected only firms which were classified as large under the German Commercial Code. Finally, α_k are sector fixed effects. Here the event is defined as zero for eight quarters before the announcement of the law in 2011Q2 and 1 for eight quarters after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. All regressions cluster the standard errors at sector level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 17: Empty Creditor Effect In Prices - Four Quarter Event Window

	(1) $\Delta Spread$	(2) $\Delta Spread$	(3) $\Delta Spread$	(4) $\Delta Spread$
$Treated$	-71.40***	-81.53***	-106.00*	-154.48**
	(-4.97)	(-4.44)	(-1.98)	(-2.78)
Macro Controls	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes
Sector FEs	No	No	Yes	Yes
Observations	105	98	65	55
Adj. R2	0.064	0.071	-0.039	-0.126

Note: Difference-in-differences regression results for the change in the five year CDS spread (MM), where the regression equation is:

$$\Delta CDSSpread_f = \beta_1 Treated_f + \beta_2 X_{f,BA} + \beta_3 Z_{f,BA} + \alpha_k + \epsilon_f$$

where $\Delta CDSSpread_f$ is the change in a firm's average five year MMR CDS spread after the implementation of the law change (i.e., $CDSSpread_{f,AI} - CDSSpread_{f,BA}$). The vector $X_{f,BA}$ contains average, pre-announcement, firm control variables, which includes Tobin's Q, return on equity, volatility of assets, Altman z-score, market value of assets and sectoral loss given default. $Treated_f$ is equal to 1 if a firm had CDS traded on them over the entire sample period and is a German reference entity, and equal to 0 if it has CDS traded on it but is domiciled in another European country. The vector $Z_{f,AI}$ includes macroeconomic variables, to control for the effect the general economic environment across countries. Finally, α_k are sector fixed effects. There is a 1 year window pre and post the event (i.e., four quarters = 0 prior, and four quarters = 1 after the event). The pre-announcement period runs from 2010Q3 to 2011Q2, and the post-implementation period runs from 2012Q3 to 2013Q2. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, and Altman z-score. All regressions cluster the standard errors at sector level. t statistics are reported between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 18: Descriptive Statistics: German Reference Entity Vs. German Non-Reference Entity - Unmatched

		Control - German Firms with No CDS Traded								*Treatment - German Firms with CDS Traded*							
		Count	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**	**Count**	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**
Before Announcement of Change in Bankruptcy Law	Probability of Default	64428	6.06	18.56	0.03	0.23	0.79	2.28	8.17	40	1.45	3.49	0.10	0.17	0.32	0.55	3.26
	Firm Size	9340	10.13	1.60	8.28	9.09	9.99	11.08	12.21	40	16.27	1.08	14.87	15.36	16.30	17.24	17.84
	Book Leverage	9340	75.35	137.50	44.99	59.68	74.17	87.16	97.13	40	64.36	16.50	43.82	53.76	65.97	77.13	83.31
	Alternative Z-Score	9340	2.31	2.42	0.19	0.93	2.06	3.25	4.62	40	0.58	0.52	0.05	0.26	0.51	0.78	1.12
	Δ Probability of Default	64428	1.26	15.57	-1.40	-0.19	0.00	0.19	1.59	40	-0.32	1.22	-1.76	-0.18	-0.04	0.02	0.11
	Tangible Asset Ratio	9340	34.15	29.94	1.80	8.42	25.44	53.78	86.38	40	6.04	11.35	0.01	0.18	1.61	7.15	18.38
	Firm Credit Concentration	64428	23520.25	3782930.92	0.00	0.00	0.00	0.00	1.81	40	1.21	5.18	0.02	0.05	0.12	0.44	1.65
	Net Working Capital / TA	9340	5.49	65.76	-24.04	-6.88	5.85	23.02	39.81	40	-1.33	19.45	-23.35	-16.07	-3.74	8.70	18.50
	Retained Earnings / TA	9340	10.19	37.21	-1.79	0.00	3.69	21.73	39.31	40	14.70	11.06	4.36	9.46	11.24	20.24	30.78
	EBIT / TA	9340	6.16	17.12	-2.36	1.03	4.78	10.55	18.37	40	3.89	3.59	0.34	1.55	3.69	5.40	9.14
	Sales / TA	9340	190.09	209.40	13.27	69.68	152.56	254.70	390.25	40	26.02	37.90	0.00	0.00	5.71	40.50	77.05
	Length of Credit Relationship	64428	17.99	16.39	2.00	4.50	12.50	27.50	43.50	40	21.65	7.27	12.98	16.32	20.63	27.42	32.31
	# Credit Relationships	64428	1.88	2.31	1.00	1.00	1.00	2.00	3.38	40	66.39	56.53	19.38	28.63	43.31	89.19	140.75
	Collateral / Total Credit	53443	89.71	1149.17	3.29	33.14	70.81	99.79	116.51	40	11.33	12.89	0.55	1.60	6.22	16.01	24.71
	Net Notional / Total Credit	64428	0.00	0.00	0.00	0.00	0.00	0.00	0.00	40	1.40	2.47	-0.27	0.04	0.36	2.65	4.26
After Implementation of Change in Bankruptcy Law	Probability of Default	64428	8.74	24.28	0.04	0.19	0.64	2.32	15.75	40	0.56	1.16	0.08	0.11	0.16	0.41	1.10
	Firm Size	8657	10.27	1.60	8.38	9.20	10.15	11.26	12.36	38	16.42	1.07	15.02	15.51	16.40	17.32	17.97
	Book Leverage	8657	70.76	40.21	40.72	56.12	71.09	84.76	95.44	38	60.98	17.71	39.03	49.67	61.74	72.15	81.65
	Alternative Z-Score	8657	2.33	2.26	0.27	0.94	2.10	3.25	4.55	38	0.59	0.57	-0.05	0.29	0.51	0.78	1.41
	Tangible Asset Ratio	8657	34.34	29.97	1.66	8.73	25.58	53.97	86.67	38	5.86	12.41	0.01	0.09	1.19	6.55	14.33
	Firm Credit Concentration	64428	9814.45	1746969.98	0.00	0.00	0.00	0.00	1.04	40	0.78	2.68	0.01	0.01	0.04	0.18	1.24
	Net Working Capital / TA	8657	6.42	41.59	-23.91	-6.74	5.97	23.89	40.97	38	-5.77	18.81	-26.38	-16.58	-5.84	2.14	12.97
	Retained Earnings / TA	8657	12.07	30.12	-0.47	0.00	4.77	25.39	44.08	38	17.34	13.46	4.46	9.21	13.67	22.05	30.68
	EBIT / TA	8657	6.37	13.22	-1.86	1.20	4.87	10.39	17.69	38	4.91	5.15	0.67	2.61	3.46	6.22	11.86
	Sales / TA	8657	187.57	197.06	13.41	64.42	151.95	253.86	383.86	38	25.34	38.94	0.00	0.00	1.13	42.28	73.46
	Length of Credit Relationship	64428	23.53	18.17	4.06	10.06	18.78	33.00	50.51	40	24.09	8.86	12.54	17.49	23.61	31.77	35.68
	# Credit Relationships	64428	1.95	2.76	1.00	1.00	1.00	2.00	3.50	40	82.09	76.67	16.13	26.69	74.19	110.50	146.00
	Collateral / Total Credit	53335	102.44	1572.10	10.33	41.08	75.81	99.82	117.88	40	21.67	25.50	1.07	2.69	5.65	43.72	63.38
	Net Notional / Total Credit	64428	0.00	0.00	0.00	0.00	0.00	0.00	0.00	40	0.78	2.07	-0.42	-0.04	0.40	1.22	2.24

Note: The table contains the sample statistics split by treatment and control group, and by time (before announcement, after implementation). There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The pre-announcement period runs from 2009Q2 to 2011Q2, and the post-implementation period runs from 2012Q3 to 2014Q1. The treatment group is comprised of German firms which had CDS traded on them both before the implementation and after the announcement of the law change. The control group is comprised of German firms which never had CDS traded on them in the sample period. Each observation represents the average value for the firm in the respective period (e.g., 40 observations of treated companies prior to the announcement represents 40 firms' average values for probability of default prior to the announcement of the law change) Table 1 contains information on each variable used in the table, including their unit, definition, and source.

Table 19: Descriptive Statistics: German Reference Entity Vs. German Non-Reference Entity - Simple Weights

		Control - German Firms with No CDS Traded								*Treatment - German Firms with CDS Traded*							
		Count	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**	**Count**	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**
Before Announcement of Change in Bankruptcy Law	Probability of Default	1127	1.99	7.49	0.09	0.17	0.39	1.06	2.97	40	1.79	3.62	0.13	0.24	0.59	1.30	4.75
	Firm Size	1127	12.30	1.02	11.22	11.53	12.06	12.77	13.79	40	16.27	1.08	14.87	15.36	16.30	17.24	17.84
	Book Leverage	1127	63.94	18.33	40.79	52.80	65.72	76.83	84.90	40	64.36	16.50	43.82	53.76	65.97	77.13	83.31
	Alternative Z-Score	1127	0.68	0.64	0.03	0.30	0.60	0.89	1.57	40	0.58	0.52	0.05	0.26	0.51	0.78	1.12
	Δ Probability of Default	1127	0.04	8.09	-0.71	-0.16	-0.02	0.04	0.48	40	0.23	2.14	-1.18	-0.13	-0.04	0.02	2.09
	Tangible Asset Ratio	1127	54.29	36.46	1.02	14.54	66.44	89.92	93.10	40	6.04	11.35	0.01	0.18	1.61	7.15	18.38
	Firm Credit Concentration	1127	9.64	111.96	0.02	0.57	3.50	9.22	16.27	40	0.82	3.17	0.01	0.04	0.10	0.36	1.22
	Net Working Capital / TA	1127	-2.23	17.62	-21.17	-8.70	-1.32	4.29	16.38	40	-1.33	19.45	-23.35	-16.07	-3.74	8.70	18.50
	Retained Earnings / TA	1127	13.70	19.32	-0.11	0.21	9.91	25.87	40.84	40	14.70	11.06	4.36	9.46	11.24	20.24	30.78
	EBIT / TA	1127	2.16	6.70	-2.35	0.31	1.51	5.03	8.77	40	3.89	3.59	0.34	1.55	3.69	5.40	9.14
	Sales / TA	1127	43.87	51.42	4.55	12.03	17.85	66.55	120.57	40	26.02	37.90	0.00	0.00	5.71	40.50	77.05
	Length of Credit Relationship	1127	24.67	14.37	5.85	12.72	24.05	34.53	44.55	40	13.89	4.23	9.44	10.93	13.23	15.39	18.93
	# Credit Relationships	1127	6.79	7.44	1.75	3.00	5.13	7.88	11.75	40	66.39	56.53	19.38	28.63	43.31	89.19	140.75
	Collateral / Total Credit	1044	91.39	318.76	6.44	35.60	79.18	105.06	137.87	40	31.66	28.70	3.94	8.45	23.29	44.28	59.58
	Net Notional / Total Credit	1127	0.00	0.00	0.00	0.00	0.00	0.00	0.00	40	78.33	1061.42	-5.24	-0.05	0.87	10.74	226.19
After Implementation of Change in Bankruptcy Law	Probability of Default	1127	2.50	11.03	0.07	0.13	0.32	0.89	3.01	40	0.84	1.33	0.12	0.16	0.39	0.86	1.56
	Size	1048	12.36	1.02	11.30	11.60	12.14	12.85	13.84	38	16.42	1.07	15.02	15.51	16.40	17.32	17.97
	Book Leverage	1048	63.16	23.18	39.97	51.82	64.18	75.72	84.06	38	60.98	17.71	39.03	49.67	61.74	72.15	81.65
	Alternative Z-Score	1048	0.71	0.69	0.01	0.33	0.64	0.93	1.63	38	0.59	0.57	-0.05	0.29	0.51	0.78	1.41
	Tangible Asset Ratio	1048	55.60	36.29	1.29	16.85	69.85	89.75	92.95	38	5.86	12.41	0.01	0.09	1.19	6.55	14.33
	Firm Credit Concentration	1127	8.55	54.28	0.00	0.33	3.22	9.35	15.94	40	0.56	1.86	0.01	0.01	0.04	0.15	0.95
	Net Working Capital / TA	1048	-2.42	22.42	-20.70	-7.61	-1.74	3.87	17.10	38	-5.77	18.81	-26.38	-16.58	-5.84	2.14	12.97
	Retained Earnings / TA	1048	15.26	28.58	0.00	0.47	11.77	27.06	43.67	38	17.34	13.46	4.46	9.21	13.67	22.05	30.68
	EBIT / TA	1048	2.76	7.05	-1.16	0.52	1.76	4.85	9.68	38	4.91	5.15	0.67	2.61	3.46	6.22	11.86
	Sales / TA	1048	43.50	51.80	4.35	12.38	16.99	64.42	122.43	38	25.34	38.94	0.00	0.00	1.13	42.28	73.46
	Length of Credit Relationship	1127	28.10	16.87	7.64	13.98	26.41	39.33	51.14	40	14.40	4.71	8.42	10.25	15.20	17.06	20.92
	# Credit Relationships	1127	7.65	11.71	1.86	3.13	5.38	7.88	12.50	40	82.09	76.67	16.13	26.69	74.19	110.50	146.00
	Collateral / Total Credit	1039	89.88	139.37	10.88	45.94	87.21	108.22	136.20	40	67.49	76.78	4.20	11.25	36.10	94.47	181.40
	Net Notional / Total Credit	1127	0.00	0.00	0.00	0.00	0.00	0.00	0.00	40	-20.45	180.31	-18.54	-1.09	0.24	4.07	23.47

Note: The table contains the sample statistics split by treatment and control group, and by time (before announcement, after implementation). There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The pre-announcement period runs from 2009Q2 to 2011Q2, and the post-implementation period runs from 2012Q3 to 2014Q1. The treatment group is comprised of German firms which had CDS traded on them both before the implementation and after the announcement of the law change. The control group is comprised of German firms which never had CDS traded on them in the sample period. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. Each observation represents the average value for the firm in the respective period (e.g., 40 observations of treated companies prior to the announcement represents 40 firms' average values for probability of default prior to the announcement of the law change) Table 1 contains information on each variable used in the table, including their unit, definition, and source.

Table 20: Descriptive Statistics: German Reference Entity Vs. German Non-Reference Entity - Bank-Firm

		Control - German Firms with No CDS Traded								*Treatment - German Firms with CDS Traded*							
		Count	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**	**Count**	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**
Before Announcement of Change in Bankruptcy Law	Probability of Default	3721	1.82	8.64	0.03	0.09	0.27	0.79	2.12	636	1.73	7.95	0.03	0.10	0.24	0.54	2.69
	Firm Size	3721	12.30	1.02	11.22	11.53	12.08	12.78	13.82	636	16.27	1.07	14.87	15.36	16.30	17.24	17.84
	Book Leverage	3721	64.25	17.84	41.75	52.94	65.79	76.91	84.90	636	64.36	16.30	43.82	53.76	65.97	77.13	83.31
	Alternative Z-Score	3721	0.67	0.64	0.02	0.30	0.60	0.89	1.57	636	0.58	0.51	0.05	0.26	0.51	0.78	1.12
	Δ Probability of Default	3721	-0.11	7.57	-0.22	0.00	0.00	0.00	0.13	636	-0.45	8.95	-0.26	-0.05	0.00	0.00	0.12
	Tangible Asset Ratio	3721	54.70	36.23	1.35	15.61	67.40	89.92	93.07	636	6.04	11.22	0.01	0.18	1.61	7.15	18.38
	Firm Credit Concentration	3721	9.77	113.30	0.03	0.61	3.51	9.22	16.27	636	0.82	3.13	0.01	0.04	0.10	0.36	1.22
	Net Working Capital / TA	3721	-2.41	17.33	-21.17	-8.81	-1.36	4.15	15.58	636	-1.33	19.22	-23.35	-16.07	-3.74	8.70	18.50
	Retained Earnings / TA	3721	13.77	19.24	-0.02	0.27	9.99	25.75	40.80	636	14.70	10.93	4.36	9.46	11.24	20.24	30.78
	EBIT / TA	3721	2.17	6.70	-2.35	0.31	1.47	5.01	8.82	636	3.89	3.55	0.34	1.55	3.69	5.40	9.14
	Sales / TA	3721	43.94	51.41	4.77	12.08	17.85	66.55	119.80	636	26.02	37.45	0.00	0.00	5.71	40.50	77.05
	Length of Credit Relationship	3721	23.24	21.66	1.50	4.00	16.50	36.50	61.50	636	19.07	19.98	1.50	3.50	11.50	27.50	52.50
	# Credit Relationships	3721	6.89	7.50	2.00	3.13	5.25	8.00	11.88	636	66.39	55.87	19.38	28.63	43.31	89.19	140.75
	Collateral / Total Credit	2378	107.52	1110.08	0.06	48.88	96.94	103.06	130.36	262	29.34	52.37	0.00	0.00	1.40	36.14	100.00
	Net Notional / Total Credit	3721	0.00	0.00	0.00	0.00	0.00	0.00	0.00	636	475.87	6928.40	-3.93	0.00	0.00	0.00	17.40
After Implementation of Change in Bankruptcy Law	Probability of Default	3721	2.43	11.52	0.04	0.07	0.22	0.69	2.32	636	0.94	3.74	0.03	0.08	0.17	0.45	1.52
	Firm Size	3524	12.37	1.02	11.30	11.61	12.14	12.86	13.86	624	16.42	1.06	15.02	15.51	16.40	17.32	17.97
	Book Leverage	3524	63.53	22.87	40.72	52.08	64.41	75.75	83.96	624	60.98	17.49	39.03	49.67	61.74	72.15	81.65
	Alternative Z-Score	3524	0.71	0.69	0.01	0.33	0.64	0.93	1.65	624	0.59	0.56	-0.05	0.29	0.51	0.78	1.41
	Tangible Asset Ratio	3524	56.05	36.02	1.46	18.09	70.21	89.71	92.95	624	5.86	12.26	0.01	0.09	1.19	6.55	14.33
	Firm Credit Concentration	3721	8.64	54.91	0.00	0.36	3.30	9.58	15.99	636	0.56	1.84	0.01	0.01	0.04	0.15	0.95
	Net Working Capital / TA	3524	-2.58	22.31	-20.64	-7.61	-1.75	3.69	16.81	624	-5.77	18.57	-26.38	-16.58	-5.84	2.14	12.97
	Retained Earnings / TA	3524	15.39	28.56	0.00	0.52	11.77	27.29	43.61	624	17.34	13.29	4.46	9.21	13.67	22.05	30.68
	EBIT / TA	3524	2.80	7.08	-1.10	0.52	1.76	4.91	9.76	624	4.91	5.09	0.67	2.61	3.46	6.22	11.86
	Sales / TA	3524	43.62	51.84	4.85	12.43	17.07	64.33	121.91	624	25.34	38.45	0.00	0.00	1.13	42.28	73.46
	Length of Credit Relationship	3721	30.85	24.15	2.40	10.50	24.50	45.43	72.50	636	23.23	22.57	1.50	4.50	17.00	34.50	57.57
	# Credit Relationships	3721	7.77	11.83	2.00	3.13	5.38	8.00	12.63	636	82.09	75.77	16.13	26.69	74.19	110.50	146.00
	Collateral / Total Credit	2341	110.71	788.66	6.90	58.41	97.15	104.41	137.17	226	42.87	63.91	0.00	0.00	7.24	65.29	141.16
	Net Notional / Total Credit	3721	0.00	0.00	0.00	0.00	0.00	0.00	0.00	636	12.68	297.00	-9.25	0.00	0.00	0.00	5.22

Note: The table contains the sample statistics split by treatment and control group, and by time (before announcement, after implementation). There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The pre-announcement period runs from 2009Q2 to 2011Q2, and the post-implementation period runs from 2012Q3 to 2014Q1. The treatment group is comprised of German firms which had CDS traded on them both before the implementation and after the announcement of the law change. The control group is comprised of German firms which never had CDS traded on them in the sample period. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. Each observation represents the average value for the firm or the bank firm variable in the respective period (e.g., 636 observations of treated companies prior to the announcement represents 636 banks' estimates of the firms' average values for probability of default prior to the announcement of the law change) Table 1 contains information on each variable used in the table, including their unit, definition, and source.

Table 21: Descriptive Statistics: German Reference Entity Vs. European Reference Entity

		Control - German Firms with No CDS Traded								*Treatment - German Firms with CDS Traded*							
		Count	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**	**Count**	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**
Before Announcement of Change in Bankruptcy Law	Spread	67	165.20	148.16	62.62	76.39	109.77	232.51	302.17	26	181.33	161.01	55.15	65.45	143.16	221.66	382.00
	Firm Size	67	9.89	1.32	8.32	8.69	9.84	10.95	11.70	26	9.93	1.23	8.14	8.90	9.94	10.50	11.81
	Book Leverage	67	27.61	11.84	14.88	21.10	24.24	33.85	44.90	26	26.23	10.68	14.56	19.63	23.16	31.55	41.07
	Alternative Z-Score	67	0.49	0.51	-0.04	0.30	0.49	0.70	0.89	26	0.47	0.42	0.16	0.39	0.51	0.67	0.93
	Loss Given Default	65	0.61	0.09	0.52	0.54	0.63	0.72	0.72	26	0.61	0.10	0.50	0.54	0.63	0.72	0.72
	Return on Equity	65	28.06	97.82	1.96	7.02	11.06	16.85	29.40	26	11.70	7.65	4.12	6.54	10.74	14.57	19.52
	Volatility of Assets	64	4.91	13.02	0.54	0.73	1.50	2.54	10.03	25	2.01	1.54	0.63	0.76	1.44	2.66	3.72
	Market Value of Assets	63	9.90	1.18	8.48	9.05	9.80	10.83	11.76	25	10.04	1.20	8.42	9.06	10.00	10.64	11.85
After Implementation of Change in Bankruptcy Law	Spread	67	158.16	132.14	65.74	73.92	111.86	181.49	303.16	26	126.26	75.22	51.60	75.89	92.41	172.65	226.10
	Firm Size	67	9.95	1.32	8.35	8.77	9.95	11.02	11.81	26	10.07	1.24	8.47	9.06	10.13	10.54	11.82
	Book Leverage	66	26.77	12.07	14.19	20.10	23.98	34.66	42.59	26	23.63	11.16	13.70	15.34	21.72	30.69	37.81
	Alternative Z-Score	66	0.52	0.48	-0.09	0.24	0.58	0.80	0.92	26	0.59	0.34	0.28	0.43	0.54	0.80	1.08
	Loss Given Default	65	0.65	0.14	0.56	0.56	0.57	0.76	0.92	26	0.61	0.11	0.41	0.57	0.58	0.76	0.76
	Return on Equity	63	14.46	33.09	-1.67	6.11	11.02	17.19	22.82	26	11.08	8.42	-2.52	4.52	12.34	17.43	19.13
	Volatility of Assets	66	5.66	19.21	0.42	0.63	1.69	3.12	7.87	26	1.79	1.50	0.62	0.84	1.40	2.36	2.77
	Market Value of Assets	63	10.02	1.18	8.49	9.05	10.09	10.91	11.83	26	10.20	1.20	8.43	9.20	10.20	10.82	11.87

Note: The table contains the sample statistics split by treatment and control group, and by time (before announcement, after implementation). There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The pre-announcement period runs from 2009Q2 to 2011Q2, and the post-implementation period runs from 2012Q3 to 2014Q1. The treatment group is comprised of German firms which had CDS traded on them both before the implementation and after the announcement of the law change. The control group is comprised of other European firms which had CDS traded on them in the sample period. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. Each observation represents the average value for the firm in the respective period (e.g., 26 observations of treated companies prior to the announcement represents 26 German firms' average CDS spreads prior to the announcement of the law change) Table 1 contains information on each variable used in the table, including their unit, definition, and source.

Table 22: Descriptive Statistics: German Reference Entity Vs. European Reference Entity

		Control - German Firms with No CDS Traded								*Treatment - German Firms with CDS Traded*							
		Count	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**	**Count**	**Mean**	**Std. Dev**	**P10**	**P25**	**P50**	**P75**	**P90**
Before Announcement of Change in Bankruptcy Law	Probability of Default	128	0.84	1.28	0.09	0.13	0.29	0.99	2.65	40	1.50	3.72	0.10	0.17	0.32	0.57	3.26
	Firm Size	101	10.01	1.35	8.12	9.21	10.08	10.85	11.85	32	10.07	1.15	8.62	9.28	10.09	10.84	11.57
	Book Leverage	101	0.35	0.26	0.15	0.22	0.32	0.41	0.51	32	0.29	0.10	0.19	0.22	0.25	0.36	0.40
	Alternative Z-Score	85	0.50	0.43	0.06	0.22	0.48	0.69	0.92	30	0.61	0.24	0.32	0.45	0.64	0.71	0.96
	Δ Probability of Default	128	-0.16	1.87	-0.90	-0.33	-0.03	0.01	0.28	40	-0.64	2.25	-2.76	-0.25	-0.06	0.02	0.16
	Tangible Asset Ratio	101	0.28	0.19	0.06	0.12	0.25	0.41	0.53	32	0.28	0.15	0.13	0.17	0.25	0.35	0.48
	Firm Credit Concentration	128	0.00	0.00	0.00	0.00	0.00	0.00	0.00	40	1.21	5.18	0.01	0.03	0.12	0.44	1.65
	Length of Credit Relationship	128	13.56	8.80	5.89	8.10	11.72	16.02	24.93	40	21.66	7.18	13.48	16.32	20.63	27.42	31.77
	# Credit Relationships	128	16.19	21.82	3.00	4.59	8.56	16.13	41.00	40	66.08	56.67	18.88	28.63	43.31	89.19	140.75
	Collateral / Total Credit	104	10.50	20.50	0.00	1.21	3.60	11.53	22.55	40	11.87	13.24	0.63	1.50	6.22	16.75	25.06
	Net Notional / Total Credit	128	-59.39	628.81	-2.46	-0.20	0.89	3.08	7.02	40	1.40	2.47	-0.27	0.04	0.36	2.65	4.26
After Implementation of Change in Bankruptcy Law	Probability of Default	128	0.99	2.87	0.08	0.13	0.24	0.58	1.54	40	0.61	1.30	0.08	0.11	0.17	0.41	0.99
	Firm Size	99	10.07	1.39	8.02	9.22	10.10	10.93	12.04	32	10.15	1.17	8.80	9.33	10.24	10.88	11.80
	Book Leverage	99	0.33	0.22	0.16	0.22	0.29	0.40	0.50	31	0.27	0.10	0.15	0.19	0.23	0.33	0.38
	Alternative Z-Score	81	0.51	0.40	0.07	0.29	0.49	0.67	0.88	30	0.64	0.26	0.31	0.48	0.63	0.79	1.10
	Tangible Asset Ratio	99	0.27	0.20	0.05	0.11	0.25	0.40	0.54	31	0.28	0.16	0.12	0.16	0.26	0.36	0.52
	Firm Credit Concentration	128	0.00	0.00	0.00	0.00	0.00	0.00	0.00	40	0.86	3.13	0.01	0.01	0.04	0.17	1.07
	Length of Credit Relationship	128	17.11	8.47	7.43	11.76	15.14	21.87	28.99	40	24.01	8.80	12.38	16.30	23.72	31.77	35.39
	# Credit Relationships	128	20.51	30.88	2.67	4.35	8.56	22.25	65.00	40	81.60	76.35	16.50	26.69	74.19	110.50	140.21
	Collateral / Total Credit	93	16.37	33.01	0.00	0.23	2.81	11.09	48.42	40	20.12	23.44	0.83	2.70	5.61	38.28	60.81
	Net Notional / Total Credit	128	-166.23	1892.94	-2.52	-0.07	1.02	3.12	5.67	40	0.78	2.07	-0.42	-0.04	0.40	1.22	2.24

Note: The table contains the sample statistics for the credit weighted sample. The table is split by treatment and control group, and by time (before announcement, after implementation). There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The pre-announcement period runs from 2009Q2 to 2011Q2, and the post-implementation period runs from 2012Q3 to 2014Q1. The treatment group is comprised of German firms which had CDS traded on them both before the implementation and after the announcement of the law change. The control group is comprised of European firms which had CDS traded on them both before the implementation and after the announcement of the law change. The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in the probability of default in the prior period. Each observation represents the credit weighted average value for the firm in the respective period (e.g., 40 observations of treated companies prior to the announcement represents 40 firms' credit weighted average values for probability of default prior to the announcement of the law change) Table 1 contains information on each variable used in the table, including their unit, definition, and source.

Table 23: Intensity of the Impact of Empty Creditors - All Variables

	(1) ΔPD	(2) ΔPD
Treated	-3.113**	5.759**
	(-2.04)	(2.10)
Net Notional / Total Credit	-0.199***	-0.696***
	(-2.89)	(-4.49)
Len. of Relationships	-0.023	-0.031
	(-1.24)	(-1.53)
Treated × **Len. of Relationships**	0.061*	0.146***
	(1.86)	(3.52)
# Creditor Relationships	0.004	0.009
	(0.11)	(0.21)
Treated × **# Creditor Relationships**	0.001	-0.002
	(0.03)	(-0.05)
Collateral / Total Credit	-0.016***	-0.017***
	(-3.88)	(-4.20)
Treated × **Collateral / Total Credit**	0.041**	0.080***
	(2.16)	(3.01)
Firm Credit Concentration	-0.000	-0.001***
	(-0.76)	(-5.91)
Treated × **Firm Credit Concentration**	-0.072	0.085**
	(-1.03)	(2.30)

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Table 23 – continued from previous page

	(1) ΔPD	(2) ΔPD
Firm Size	-0.540	-0.317
	(-1.19)	(-0.65)
Treated × **Firm Size**	1.092*	-0.088
	(1.92)	(-0.13)
Book Leverage	0.001	-0.005
	(0.09)	(-0.33)
Treated × **Book Leverage**	-0.065**	-0.059**
	(-2.02)	(-2.37)
Tangible Asset Ratio	-0.014*	0.012
	(-1.74)	(0.81)
Treated × **Tangible Asset Ratio**	0.015	0.025
	(0.82)	(0.92)
EBIT / TA	-0.021	-0.045
	(-0.47)	(-0.89)
Treated × **EBIT / TA**	0.311**	0.332***
	(2.26)	(3.09)
Legal Size Controls	No	No
Sector FEs	No	Yes
Observations	1,062	1,053
Adj. R2	0.005	0.036

Note: Difference-in-differences regression results for the change in probability of default of German CDS reference entities relative to other German firms. The regression equation is:

$$\Delta PD_f = \beta_1 Treated_f + \beta_2 X_{f,BA} + \beta_3 Treated_f X_{f,BA} + \alpha_k + \epsilon_f$$

where ΔPD_f is the change in firm f's average probability of default after the implementation of the law change (i.e., $PD_{f,AI} - PD_{f,BA}$). Here the averaging is done by weighting each observation by the percentage of the firm's total credit provided by the reporting bank. $Treated_f$ is equal to 1 if firm f is a German firm with CDS traded on them over the entire sample period, and equal to 0 if it never had CDS traded on it in the sample period. The vector $X_{f,BA}$ contains firm characteristics: firm size, book leverage, asset tangibility and concentration of its credit, and bank-firm based variables: the number of credit relationships a firm has, the average length of these relationships, and the ratio of loan collateral to loan value. Finally, α_k are sector fixed effects. Table 1 contains information on each variable used in the table, including their unit, definition, and source. The event is defined as zero before the announcement of the law in 2011Q2 and 1 after the implementation in 2012Q3. There is a two year window pre and post the event (i.e., eight quarters = 0 prior, and eight quarters = 1 after the event). The treatment and control group are matched using the coarsened exact matching method on firm size, book leverage, alternative z-score, and change in then probability of default in the prior period. All regressions cluster the standard errors at sector level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.