Identifying Empty Creditors with a Shock and Micro-Data *

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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 firms with CDS traded on them 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. Further, we find that 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 recognise the empty creditor effect to a larger extent. Furthermore, banks' business models affect the degree to which they recognise the empty creditor effect. Where banks that monitor their creditors less and that earn a smaller portion of their income from interest activities, recognise the empty creditor effect to a larger extent.

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

JEL classification: G21, G33, G38.

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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), for example, could hedge their credit risk through the CDS market, and in this way become "empty creditors". 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. 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. Next to bankruptcy, Standard European CDS contracts also have restructuring as a credit event (ISDA, 009a,b). Prior to 2012, the German insolvency law prohibited restructuring, implying that CDS on German firms were de facto Standard North-American contracts as they only recognized bankruptcy as a credit

[1] In reference 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." (Pope Francis, 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.

event.

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.[3] 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 do not have an increased incentive to push the CDS referenced entity into bankruptcy. The reason is that these CDS also pays 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 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 who do not have CDS traded on them. German firms who do not have CDS traded on them are also exposed to this reform but they were never affected by empty creditors.

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) model how creditors who are able to purchase CDS to 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., 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 creditors will "over-insure" (i.e., purchase more CDS on a firm than is socially optimal) in equilibrium. 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 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.

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

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

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 relied on the initiation of CDS trading on a firm (Subrahmanyam, Tang, and Wang, 2014; Colonnello, Efing, and Zucchi, 2016). This initiation may 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.[4] 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 a 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 the Deutsche Bundesbank's BISTA and GuV databases, containing quarterly bank balance sheet, and income statement information, respectively. Lastly, we combine the resulting dataset with macroeconomic data obtained from DataStream.

The impact of empty creditors can be expected to manifest itself at the firm level; hence, we collapse the dataset to this level. When collapsing from bank-firm to firm level, we weigh each bank-firm observation by the proportion of credit the bank provides to the firm. This provides us with more accurate firm-level estimates of bank-firm variables. In particular, a firm's creditors' estimates of the firm's probability of default is weighted by the proportion of credit each creditor provides to the firm. By collapsing the bank-firm

[4]More generally, the use of North American CDS reference entities will 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.

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).

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.[5] 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 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 CDS hedge position of its creditors. Specifically, the empty creditor effect for a firm with an average CDS hedge ratio (0.05) is between 1.05 and 1.49 percentage points, where a one standard deviation increase in the firm's CDS hedge ratio (2.47) sees the empty creditor effect increase by an extra 1.1 percentage points. 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 recognise the empty creditor effect to a larger extent. Furthermore, banks' business models affect the degree to which they recognise the empty creditor effect. Where banks that monitor their creditors less and that earn a smaller portion of their income from interest activities, recognise the empty creditor effect to a larger extent.

We further test the assumption of 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 to 120 basis points.

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. This is to say that because the change in the bankruptcy law affects both the treatment and control group, the groups need to be identical apart

[5] 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.

from the treatment group being CDS referenced entities. If this weren't 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 are, 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 for the majority of our analysis.

Additionally, we enforce constant membership over the sample so as to account for 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 the particular exogenous shock, we can disentangle the effect of CDS as a commitment device from the effect of empty creditors. Fifth, we shed 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, 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.[6] 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 to.

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 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

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

for this separation to cause creditors, which 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 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. (2016) predict that firms with relatively strong shareholders experience more severe negative side effects of CDS trading on their debt.

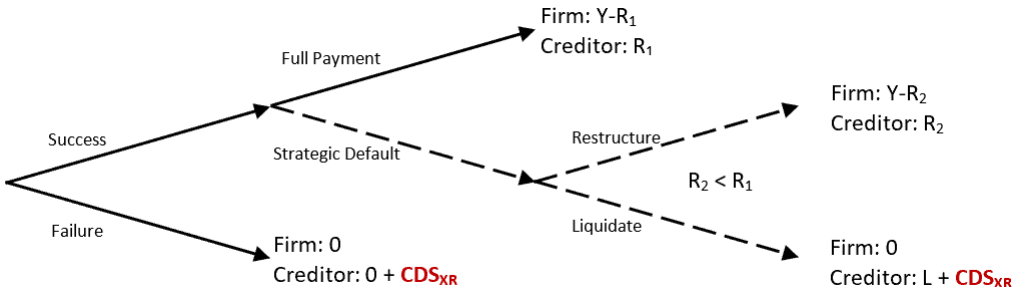
Further, the theory predicts that these effects vary by firm characteristics, debt market characteristics and CDS trading characteristics. Firms which are small, opaque, have low profitability, have high asset or cash-flow volatility, have low credit quality, that are financially constrained, have low asset tangibility, whose debt is difficult to renegotiate, or which are more likely to undergo restructuring enjoy the largest benefit from CDS trading on their debt (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



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.[7]

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 firms 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 on 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 on bankruptcy ($L + CDS_{XR} > R_2$).

The implication is that CDS act as a commitment device where creditors are more

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

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. 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, 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. However, Caglio, Darst, and Parolin (2018) find that CDS hedging by a firm's creditors may in fact decrease the probability of default of these firms. 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. Colonnello et al. (2016) find that the relative bargaining power of shareholders and creditors affects the propensity for creditors to trade CDS which affects the intensity of the real effects of empty creditors.

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 event employed in the literature.

Further, by virtue of having granular data on creditors' CDS positions, we are able to extend the extant 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 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 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 begins 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 (2017) find that banks purchase more protection on riskier firms and firms to which they have higher existing credit exposures. Further, the authors find an increase in a bank's CDS position leads to a relatively higher credit exposure to safer firms after the CDS Small Bang. 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.

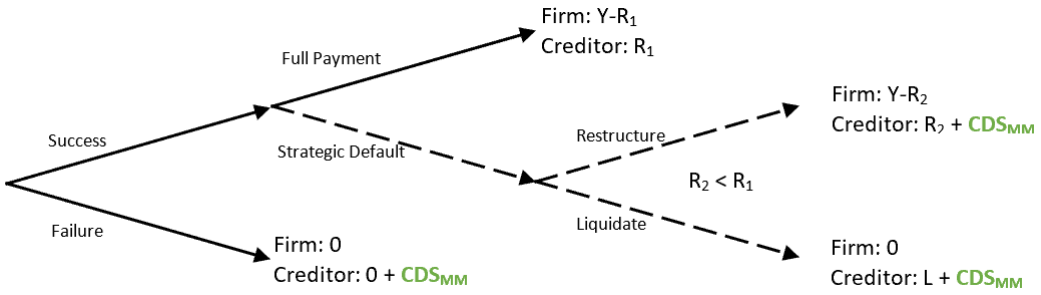
3 Institutional Setting: Change in Bankruptcy Law

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 recognised 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 payout on restructuring and bankruptcy, CDS_{MM}, as represented in Figure 2. Hence, the CDS payout 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 on bankruptcy, to hold (Bolton and Oehmke, 2011). Further, insured creditors have reduced invectives 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.

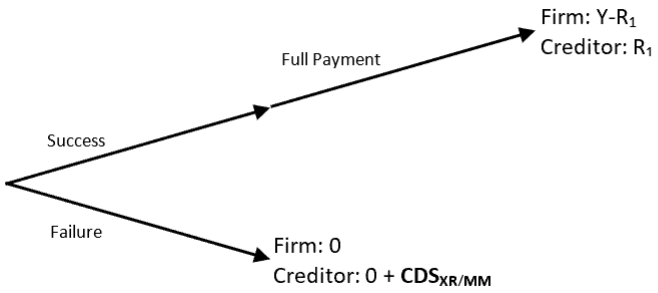
Figure 2: The Bolton and Oehmke (2011) model with restructuring as a credit event



The first assumption that firms are able to restructure their debt is key too.[8] 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.

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



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.

In Germany prior to 2012, the bankruptcy law all but prohibited restructuring, which is evident as they occurred in only 2% of insolvency cases (Höher, 2012). 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 affects on their probabilities of default through CDS as a commitment device, as in

[8] 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 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.[9] 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.[10]

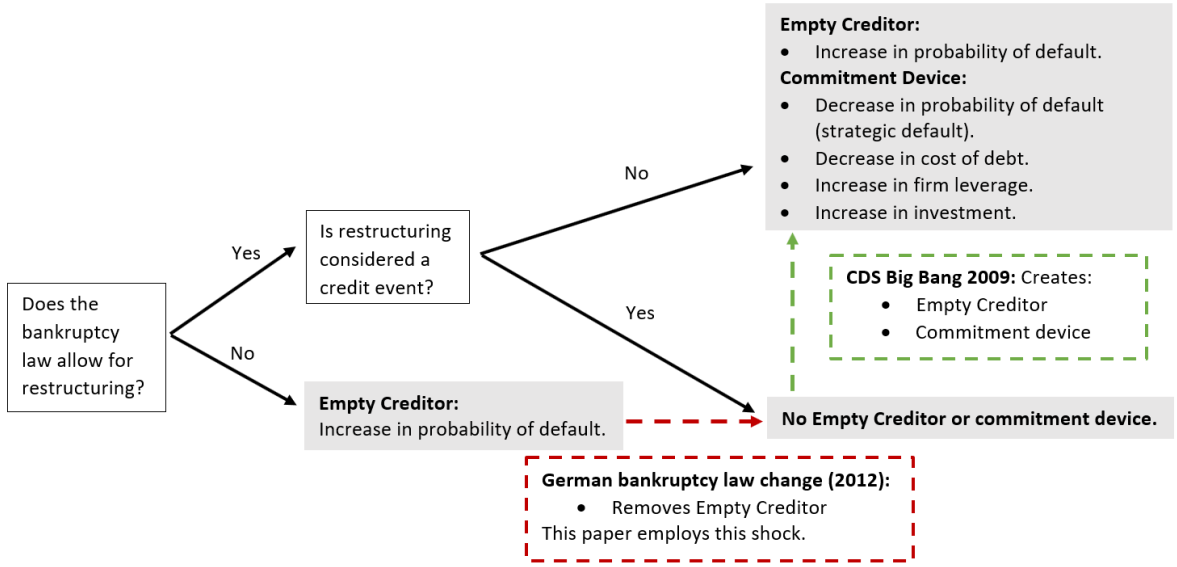
Further, the theory, as outlined in the previous section, predicts that the intensity of the impact of empty creditors varies with certain firm, bank and bank-firm characteristics.

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 reference 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 empty creditors' affect on CDS spreads, but a reduction in spreads would be evidence that it is indeed priced, and outweighs the restructuring effect.

[9] 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 leverage 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 legal definition of large firm.

[10] 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.

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, CDS has no effect on the underlying firm. Finally, if restructuring is permitted and not defined as a credit event, then CDS acts as a commitment device as well as increases the probability of default through the effect of empty creditors.

4 Data

To test the aforementioned theories, we combine ten 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% and 95% of global CDS activity (Mayordomo, Peña, and Schwartz, 2014). We employ a 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 1000 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, 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 exposures of each bank in the economy to each of its borrowers. 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 firm-time level which we exploit in a later regression.

Table 1: Variable Descriptions

Variable	Unit	Definition	Source
Probability of Default	%	Probability that the firm defaults on its debt. This is submitted by each creditor of the firm.	MiMik
Net Notional / Total Credit	unit	The hedge ratio defined as CDS net notional / total credit	MiMik & DTCC
CDS Reference Entity	0/1	Equal to 1 if the firm 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.	DTCC
German CDS Reference Entity	0/1	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.	DTCC
Spread	bps	5 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	no.	The number of credit relationships a firm has.	MiMik
Firm Credit Concentration	%	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	%	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	%	Book value of debt / total assets	USTAN
Alternative Z-Score	%	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	%	Tangible assets / total assets.	USTAN
Net Working Capital / TA	%	Net working capital / total assets.	USTAN
Retained Earnings / TA	%	Retained earnings to total assets.	USTAN
EBIT / TA	%	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	%	Bank: liquid assets / total assets.	BISTA
Capital	%	Bank: total capital / total assets.	BISTA
NII	%	Bank: net interest income / gross earnings.	GuV
Monitoring	%	Bank: staff and administration expense / gross earnings.	GuV

It should be noted that banks' probability of default estimates of their borrowers do not take into account the loss given default, and hence, are not affected directly by hedging practices. In other words, a bank's estimate of a borrowers 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 he 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.

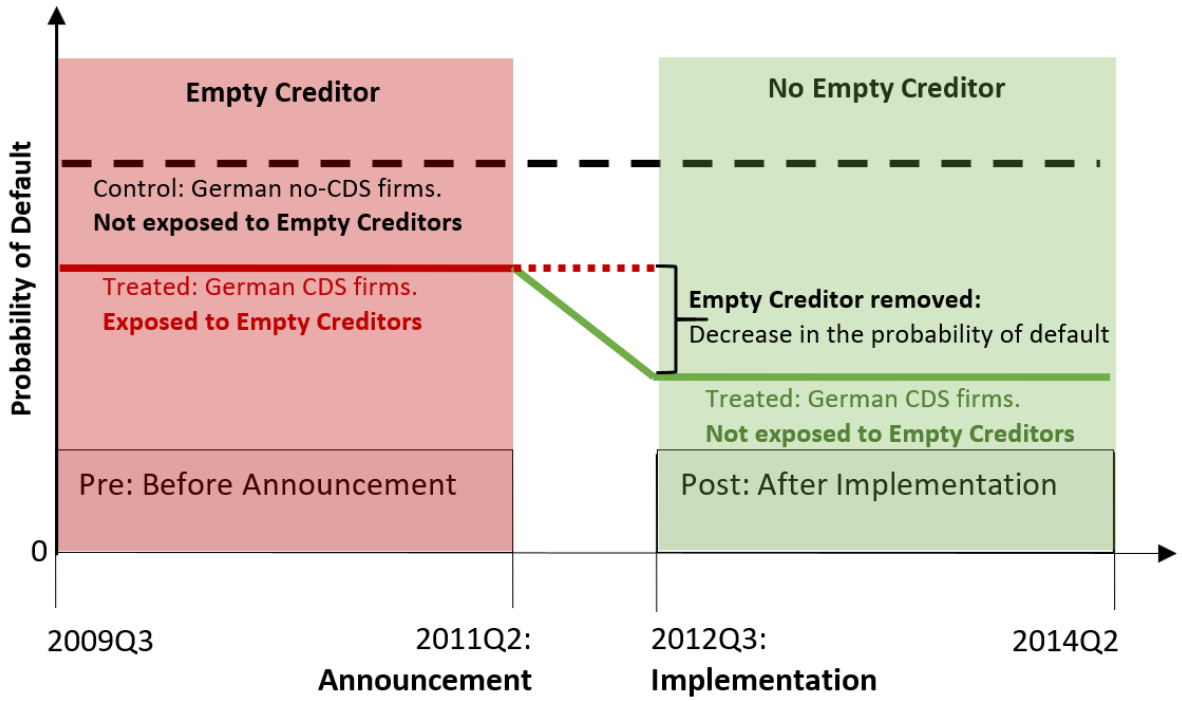
Similarly we calculate the credit-weighted average hedge position of a firm's creditors as:

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

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

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

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.

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 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 the implementation of the CDS Small Bang. In both cases we employ data 8 quarters before the event and 8 quarters after the event. The results are robust to using a shorter pre/post window of 4 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 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 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 begins to trade on their debt (Subrahmanyam et al., 2014; Danis and Gamba, 2018; Colonnello et al., 2016). 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 potential 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 over the whole sample, and is defined as a control firm if it never has CDS traded on it, in 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	1088	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	1088	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	1088	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	1088	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	1088	-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	1088	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	1088	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	1088	-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	1088	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	1088	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	1088	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	1088	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	1088	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	1022	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	1088	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	1088	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	1022	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	1022	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	1022	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	1022	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	1088	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	1022	-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	1022	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	1022	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	1022	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	1088	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	1088	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	1006	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	1088	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 for the credit weighted sample. The table is split by treatment and control group, and by time (before announcement, after implementation). There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 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 CEM 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.

sample.

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, 2018). However, the main results are robust to matching and 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 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.[11] As we match on firm size, an alternative z-score, 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.

While we do not match on the outcome variable, probability of default, it can be noted from Table 2 and Table 17, 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

[11] In Appendix A.3, Table 17 provides descriptive statistics for an unmatched sample and Table 18 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.

group that is more similar to the treatment group than the unmatched control group. Further, the fact that the average probability of default of the treatment group decreases while the control group increases is initial 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% 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_i 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 average before the announcement (denoted by "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.[12] 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 for the base line regressions, Equation 1, where the total average treatment effect is tested.[13] We find positive, and significant, evidence

[12] 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

[13] 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, CDS Ref. Entity is equal to 1 for only

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 percentage point decrease in their probability of default when the effect of empty creditors is removed, 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% to 57% of one standard deviation (3.49 percentage points) of the probability of default in the treatment group prior to the law change.

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., 10 treatment firms in each quartile.[14]

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 for 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 3 and 4). 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 forth

40 of the 1128. Hence, CDS Ref. Entity cannot explain the change in the probability of default for the 1088 non-CDS firms.

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

Table 3: Empty Creditor Effect - Base Results

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD
CDS Ref. Entity	-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	1128	1128	1117	1117
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 8 quarters before the announcement of the law in 2011 Q2 and 1 for 8 quarters after the implementation in 2012Q3. There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 quarters = 1 after the event). The treatment and control group are matched using the CEM 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. It can be noted that the Adjusted R-squared values are low or sometimes negative. This can be explained by the fact that CDS Ref. Entity cannot explain the change in the probability of default for non-CDS firms. If we take column 1 as an example, CDS Ref. Entity is equal to 1 for only 40 of the 1128.

quartile) is 5.015 percentage points, which is approximately 10 times larger than less risky firms (firms in the 3rd quartile). This indicates that riskier or more vulnerable firms experience empty creditors more severely.

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_i 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 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 $X_{f,BA}$ variable 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 depend 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

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
CDS Reference Entity	-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	1128	195	205	185	442
Control Firms	1088	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 2011 Q2 and 1 after the implementation in 2012Q3. There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 quarters = 1 after the event). The treatment and control group are matched using the CEM 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 $PD_{i,AI}$ are included. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

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
CDS Ref. Entity	-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)
CDS Ref. Entity × 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)
CDS Ref. Entity × 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)
CDS Ref. Entity × 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)
CDS Ref. Entity × 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	1128	1117	1128	1117	1128	1117	1128	1117	1128	1117
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 2011 Q2 and 1 after the implementation in 2012Q3. There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 quarters = 1 after the event). The treatment and control group are matched using the CEM 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.

levered, 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 without and with 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 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 size firm is between 3.46 percentage points and 3.95 percentage points, where a one standard deviation increase in size (1.08) sees the empty creditor effect reduce by 98 basis points. Similarly, the empty creditor effect for a firm with an average book leverage (64.85%) is between 1.34 and 2.23 percentage points, where a one standard deviation increase in the firm's book leverage (16.5%) sees the empty creditor effect increase by an extra 1.3 percentage points. This result implies that the empty creditor effect has consequences for the capital structure decisions of firms, as CDS referenced firms have to include 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 an average CDS hedge ratio (0.05) is between 1.05 and 1.49 percentage points, where a one standard deviation increase in the firm's CDS hedge ratio (2.47) sees the empty creditor effect increase by an extra 1.1 percentage points.

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

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
CDS Ref. Entity	-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)
CDS Ref. Entity × 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)
CDS Ref. Entity × # 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)
CDS Ref. Entity × 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)
CDS Ref. Entity × 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	1128	1117	1128	1117	1062	1053	1128	1117	1128	1117	1062	1053
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 2011 Q2 and 1 after the implementation in 2012Q3. There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 quarters = 1 after the event). The treatment and control group are matched using the CEM 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.

of a firms credit relationships decreases the empty creditor effect by 1.13 percentage points. 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%) increase in the amount of collateral coverage on a firms debt increases the impact of empty creditors by 68 basis points. Finally, 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 basis points.

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, as creditors would want to ensure they are protected against losses in the 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 act to make the question of the sign of the effect of the number of creditors a firm has on the intensity of the empty creditor effect, and 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 percentage points. However, β_3 is statistically insignificant, hence we can not 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.

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 i's probability of default after the implementation of the law change (i.e., $PD_{b,f,AI} - PD_{b,f,BA}$).[30] 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 percentage point 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% to 36% of one standard deviation (7.95 percentage points) of the probability of default in the treatment group prior to the law change.[15] 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:

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

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 i'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) recognise a larger empty creditor effect on the firm.

In particular, a one standard deviation increase in a bank's capital ratio (2.54%)

[15] In Appendix A.3, Table 19 provides descriptive statistics for the bank-firm sample.

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 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 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 CEM 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.

which jointly holds the average CDS hedge position, sees the empty creditor effect increase by an extra 10 basis points. 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 trade 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 a borrowers when it is well capitalised, the bank will recognise a larger empty creditor effect on the firm.

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 recognise a larger empty creditor effect on the firm. In particular, a one standard deviation increase in the collateral ratio (52.37%) where the bank holds the average CDS hedge position, sees the empty creditor effect increase by an extra 100 basis points. 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 a contradiction to the result from Table 6 as it does not measure the former effect, incentive to trade CDS, because the interaction implies that the effect is conditional on the bank trading CDS on the borrower.

Finally, the variables net-interest income to gross revenue 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%) where the bank holds the average CDS hedge position, sees the empty creditor effect increase by an extra 2.64 percentage points.

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%) where the bank holds the average CDS hedge position, sees the empty creditor effect decrease by 5.36 percentage points. This implies CDS referenced firms which borrower from relationship lenders are mostly not affected by the empty creditor effect.

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 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 quarters = 1 after the event). All regressions cluster the standard errors at the borrower level. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

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 5-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 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 had 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.

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: Empty Creditor Effect In Prices

	(1) $\Delta Spread$	(2) $\Delta Spread$	(3) $\Delta Spread$	(4) $\Delta Spread$
German CDS Reference Entity	-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 5 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 5-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 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 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 CEM 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 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.[16]

We find that German CDS firms saw a 49 to 122 basis point decrease in their 5 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 5 year CDS spread in the entire sample is about 170 basis points, and represents 30% to 76% of one standard deviation (161 basis points) of the 5 year CDS spread in the treatment group prior to the law change.[17]

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.

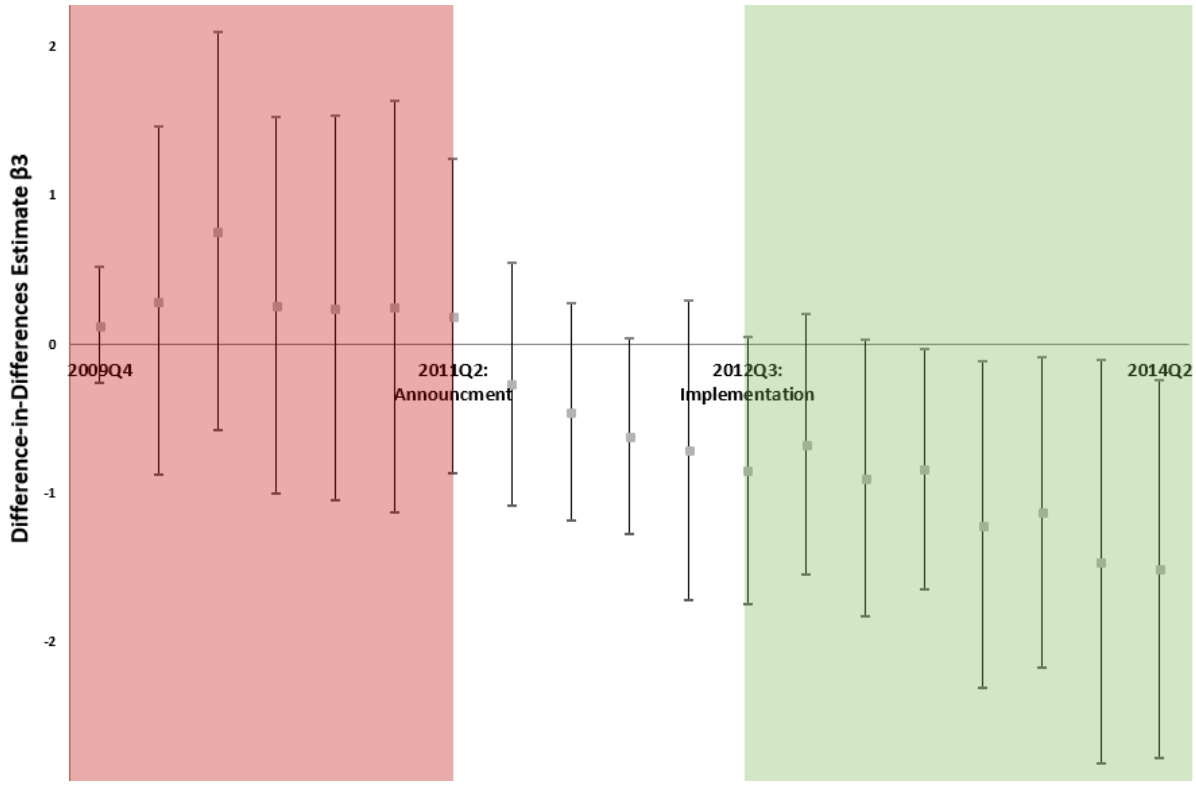
Parallel trends

An assumption of the difference-in-differences methodology is that the treatment and control group follow parallel trends prior to the the event. To investigate the validity of this assumption, we conduct the same exercise as the main regressions using each quarter, starting from 8 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 2011 Q2, as the bars in the graph prior to the event cross the x-axis

[16] These results are robust to shortening the event window as presented in in Appendix A.3, Table 16. In this table the pre/post event windows are narrowed to 4 quarters as opposed to 8 quarters. Using a shorter event window, the results become more statistically significant and the beta estimates remain negative in sign.

[17] In Appendix A.3, Table 20 provides descriptive statistics for the sample used in these regressions.

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:

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

Where, the treatment variable $Treated_{f,t}$ is define 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 CEM method on: firm size, book leverage, alternative z-score, and change in the probability of default in the prior period.

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 11. 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 11 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 are, 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 10. 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 10 relate to columns 9 and 10 of Table 10. 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 basis points. While the magnitude of the effects are smaller when using 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.

Table 10: Empty Creditor Effect - Base Results - Euro Controls

	(1) ΔPD	(2) ΔPD	(3) ΔPD	(4) ΔPD
German Ref. Entity	-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)
German Ref. Entity 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 2011 Q2 and 1 after the implementation in 2012Q3. There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 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 11: 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
CDS Ref. Entity	-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	1128	1117
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 2011 Q2 and 1 after the implementation in 2012Q3. There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 quarters = 1 after the event). The treatment and control group are matched using the CEM 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.

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 interested is the change in the probability of default, requiring a probability of default observations both before and after the event. Hence, the firms' need to survive the entire event window 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 6.

Placebo Test - Small Bang Protocol

To conduct the 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, 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 by conducting 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 for the main regressions using a sample constructed using unweighted averages are presented in Table 13. We find 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, as a

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
CDS Ref. Entity	-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	1128	1117	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). Where columns 1-2 the event window is 2 years before and after the event, columns 3-4 the window is shortened to 1 year. For columns 5-6 the CDS Small Bang is the event where 7 quarters before, and 7 quarters after the event are employed These are averaged by firm and period (pre/post). The treatment and control group are matched using the CEM 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
CDS Ref. Entity	-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	1167	1167	1156	1156
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 2011 Q2 and 1 after the implementation in 2012Q3. There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 quarters = 1 after the event). The treatment and control group are matched using the CEM 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) $PD_{f,t}$
CDS Ref. Entity	-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)
CDS Ref. Entity 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	10886	10886	10886	10886
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:

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

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 zero for 8 quarters before the announcement of the law in 2011 Q2 and one for 8 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 CEM 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.

robustness test we use the level, or non-time-averaged data. To do so, we employ an 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 8 quarters after the implementation in 2012Q3 and 0 for 8 quarters before the announcement of the law in 2011 Q2. 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 percentage points. 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 an average CDS hedge ratio (0.05) is between 1.05 and 1.49 percentage points, where a one standard deviation increase in the firm's CDS hedge ratio (2.47) sees the empty creditor effect increase by an extra 1.1 percentage points. 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. Finally, we find that bank's which are not capital constrained, which monitor their borrowers less, earn a smaller portion of their income from interest activities, and which are liquidity constrained recognise the empty creditor effect to a larger extent.

We account for endogeneity issues which affects 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.

A Appendix

A.1 Credit default swap contracts

A credit default swap (CDS) contract is similar to an insurance contract where one party pays another party for protection from a particular event.[18] In the case of CDS, a protection buyer (buyer of the CDS) makes periodic payments to the protection seller (seller of the CDS) and the protection seller pays the buyer a protection value if a "credit event" occurs. The CDS contract specifies the parties (protection buyer and seller) as well as the reference entity on which credit risk protection is being purchased. Further, it specifies the maturity, which ranges from 2 to 10 years, with the most liquid being 5-year maturity. Further, CDS contracts define what the parties agree to be a credit event (restructuring or bankruptcy), which triggers a payment from the protection seller to the protection buyer. For this protection, the CDS buyer pays a periodic premium to the protection seller, called the CDS spread. Once the event occurs a settlement takes place where the value of the compensation that the protection buyer receives from the seller is determined. Finally, the contract specifies the way in which this payment from the protection seller to buyer is to be settled. This can be done through a cash settlement where the protection buyer receives the difference between the protection value and the current value of the underlying debt,[19] or by physical settlement, where the protection buyer delivers a certain bond of the the reference entity to the seller and receives the full insured amount in cash, in return (ISDA, 2003).

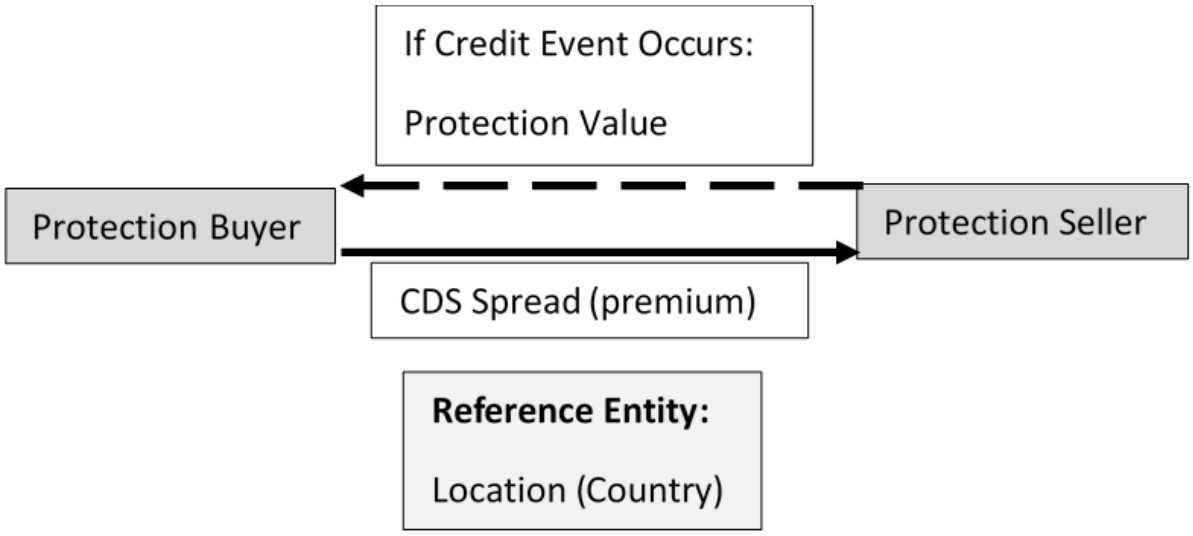
Clearly, there are many points that could potentially be negotiated in these bilateral contracts. This could harm the liquidity of the CDS market, as parties would have to negotiate these before entering into agreements. To address this, the International Swap and Derivative Association release the 2003 ISDA Credit Definitions which aimed to standardize many of the contract terms and thus improve liquidity in the CDS market (ISDA, 2003). These contracts were standardized by reference entity type and location of the reference entity. For the single name corporate CDS (CDS where the reference entity is a single firm) there are two main standardized contracts, the Standard European Corporate CDS Contract and the Standard North American CDS Contract (ISDA, 009a,b). The choice of contract is determined by the location of the reference entity (i.e.,A CDS contract on a European firm trades with a Standard European Corporate CDS contract).

The main difference between European CDS contracts and North American CDS

[18] CDS differ from standard insurance contracts as a protection buyer does not need to hold the underlying asset and it may purchase protection that exceeds the value of its position in the underlying. This can be thought of as buying insurance on your neighbour's house, or insuring your own home for more than its worth.

[19] The residual value of the underlying debt is typically determined by polling the dealers in the market.

Figure 7: CDS Contract Payment Diagram



Note: This figure illustrates the payment structure of a CDS contract. A protection buyer (buyer of the CDS) makes periodic payments (CDS Spread) to the protection seller (seller of the CDS). The protection seller pays the buyer a protection value if a "credit event" occurs. The definition of a "credit event" depends of the standardized contract used, which is determined based on the domicile country of the underlying reference entity. A single-name CDS contract on a firm domiciled in Germany trades with a European standard CDS contact which specifies restructuring as a credit event.

contracts is their definition of a credit event, called the restructuring clause. Under the 2003 ISDA Credit Definitions, there are four types of restructuring clauses: Old Restructuring (CR), Modified Restructuring (MR), Modified-Modified Restructuring (MMR), and No Restructuring (XR). European corporate CDS mostly trade with a MMR restructuring clause where restructuring is recognised as a credit event and the deliverable obligation is limited to debt with a maturity no more than 60 months for restructured obligations and 30 months for all other obligations. Prior to 2009 most North American corporate CDS contracts traded with a MR restructuring clause, which limits the deliverable obligation to debt with a maturity no more than 30 months. However, in 2009, with the CDS Big Bang protocol, the restructuring clause was standardized and changed to XR for North American CDS corporates (Markit, 009a).

In 2009 the ISDA further standardized both European CDS corporate and North American CDS corporate contracts. The CDS Big Bang was implemented in April 2009, while the CDS Small Bang was implemented in June 2009. The CDS Big Bang Protocol further standardized the contracts of North American CDS corporate contracts, while the CDS Small Bang Protocol did the same to European CDS corporate contracts. Both standard contracts had terms changed to improve the liquidity of the respective CDS markets, these changes included standardization of the settlement auctions, amongst

other changes (Markit, 009a,b). The main difference between the changes is that the North American contract was standardized to trade only with an XR restructuring clause, while the European contract's restructuring clause remained MMR.

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 modernisation 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).

This deficiency was made clear with the enactment of the European Insolvency Regulation (EIR) adopted by the EU Council in May 2002. This resulted in the Germany 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 has to be commenced where a firms 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 in 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 to be included in the reform (Closset and Urban, 2018).

ESUG made five major modifications to the German Insolvency Regulation. The reform 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" to a more "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	1128	363	404	205	156
Control Firms	1088	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 2011 Q2 and 1 after the implementation in 2012Q3. There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 quarters = 1 after the event). The treatment and control group are matched using the CEM 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 $PD_{i,AI}$ are included. We report t statistics between parentheses. *, ** and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 16: Empty Creditor Effect In Prices - 4 Quarter Event Window

	(1) $\Delta Spread$	(2) $\Delta Spread$	(3) $\Delta Spread$	(4) $\Delta Spread$
German CDS Reference Entity	-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 5 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 5-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. 4 quarters = 0 prior, and 4 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 CEM 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 17: 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 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 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 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.

Table 18: 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 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 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 CEM 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 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.

Table 19: 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 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 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 CEM 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' estimate of the firms' 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.

Table 20: 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 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 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 CEM 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 spread 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	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 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 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 CEM 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.

Table 22: Intensity of the Impact of Empty Creditors - All Variables

	(1) ΔPD	(2) ΔPD
CDS Ref. Entity	-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)
CDS Ref. Entity × Len. of Relationships	0.061*	0.146***
	(1.86)	(3.52)
# Creditor Relationships	0.004	0.009
	(0.11)	(0.21)
CDS Ref. Entity × # Creditor Relationships	0.001	-0.002
	(0.03)	(-0.05)
Collateral / Total Credit	-0.016***	-0.017***
	(-3.88)	(-4.20)
CDS Ref. Entity × Collateral / Total Credit	0.041**	0.080***
	(2.16)	(3.01)
Firm Credit Concentration	-0.000	-0.001***
	(-0.76)	(-5.91)
CDS Ref. Entity × Firm Credit Concentration	-0.072	0.085**
	(-1.03)	(2.30)

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Table 22 – continued from previous page

	(1) ΔPD	(2) ΔPD
Firm Size	-0.540	-0.317
	(-1.19)	(-0.65)
CDS Ref. Entity × Firm Size	1.092*	-0.088
	(1.92)	(-0.13)
Book Leverage	0.001	-0.005
	(0.09)	(-0.33)
CDS Ref. Entity × Book Leverage	-0.065**	-0.059**
	(-2.02)	(-2.37)
Tangible Asset Ratio	-0.014*	0.012
	(-1.74)	(0.81)
CDS Ref. Entity × Tangible Asset Ratio	0.015	0.025
	(0.82)	(0.92)
EBIT / TA	-0.021	-0.045
	(-0.47)	(-0.89)
CDS Ref. Entity × EBIT / TA	0.311**	0.332***
	(2.26)	(3.09)
Legal Size Controls	No	No
Sector FEs	No	Yes
Observations	1062	1053
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 2011 Q2 and 1 after the implementation in 2012Q3. There is a 2 year window pre and post the event (i.e. 8 quarters = 0 prior, and 8 quarters = 1 after the event). The treatment and control group are matched using the CEM 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.

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