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Abstract

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(96 words)

Keywords: Credit default swaps, credit exposure, hedging, bank lending, Depository Trust and Clearing Corporation (DTCC).

JEL Codes: G21.

I. Introduction

Banks tackle and mitigate the asymmetric information frictions present in imperfect capital markets, and may therefore enhance the supply and allocation of credit. Consequently, financial intermediation may determine economic performance and growth. The importance of the financial intermediation–growth nexus has led to a vast literature empirically documenting the association and causality between finance and economic growth. The findings generally suggest that finance, and financial intermediation, in particular, plays a key role in spurring and propagating economic growth through the facilitation of the problems inherent in imperfect credit markets (e.g., Levine [2005], Aghion [2006] and Papaioannou [2008]). What has been studied less is how the existence and effectiveness of other financial markets may affect the ability of banks to mitigate the aforementioned frictions.

Take credit default swaps (CDSs) which "are insurance-type contracts that offer their buyers protection against default by a debtor."[1] This important innovation in financial markets has fundamentally altered how financial institutions have managed their credit risks during the last two decades. However, their role in amplifying credit risk has been debated heavily, especially after the global financial crisis.[2]

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[1] Subrahmanyam, Tang, and Wang [2014], *op. cit.* p. 2926. For reviews see Stulz [2010], Augustin, Subrahmanyam, Tang, and Wang [2014] and Augustin, Subrahmanyam, Tang, and Wang [2016]. A large empirical literature explains CDS spreads and trading volume (e.g., Ericsson, Jacobs, and Oviedo [2009], Gârleanu, Pedersen, and Poteshman [2009], Zhang, Zhou, and Zhu [2009], Tang, and Yan [2010], Bongaerts, De Jong, and Driessen [2011], Galil, Shapir, Amiram, and Ben-Zion [2014], Gehde-Trapp, Gündüz, and Nasev [2015], and Kiesel, Kolaric, and Schiereck [2016]), and the usage of CDS to potentially mitigate *sovereign* risk (e.g., Acharya, Gündüz, and Johnson [2018], *renegotiation* risk when facing strong shareholders (Colonnello, Effing, and Zucchi [2019]) or *counterparty* risk (e.g., Duffie, Scheicher, and Vuillemey [2015], Gündüz [2018])), when not already contractually mitigated (e.g., Udry [1994]).

[2] CDSs also affect lenders' monitoring incentives as well as incentives to initiate new lending relationships (Kang, Williams, and Wittenberg-Moerman [2021]). The reduced lender monitoring is found to lead corporate shareholders to intensify their monitoring and demand increased voluntary disclosure from managers (Kim, Shroff, Vyas, and Wittenberg Moerman [2018]).

Creditors such as banks may be interested in buying CDSs for several reasons. First, CDSs enable banks to lay off their credit risk at any point in time: Even if banks choose not to hedge immediately upon granting credit, the existence of CDS contracts offers banks the possibility to hedge credit risk later, thereby providing additional opportunities to diversify their credit exposure and making holding credit risk more attractive (Ashcraft, and Santos [2009], Saretto, and Tookes [2013]).

In addition, CDS contracts can be used for regulatory capital relief purposes, i.e., for reducing the required capital on loans that banks had purchased protection on. Given that banks view equity issuance as costly, this could possibly free up banks' lending capacity (Yorulmazer [2014]; Klingler, and Lando [2018]; Shan, Tang, Yan, and Zhou [2021]). CDSs also enable banks to offload credit risks while maintaining client relationships, which could be valuable especially when the firm is in distress (e.g., Puri, Rocholl, and Steffen [2011]). Lastly, given lower trading costs, the introduction of CDSs may allow buy-and-hold investors, who are efficient holders of the illiquid bonds, to temporarily transfer unwanted credit risks. (e.g., Oehmke, and Zawadowski [2015]).[3]

With all these benefits, one would expect CDS contracts to play a key role in affecting banks' ability to assist firms to relax their credit constraints. However, the empirical evidence of the effect of CDSs on reducing borrowing costs is surprisingly ambiguous,[4] and almost invariably based on

[3] There are demonstrably fewer frictions in the CDS market than in the bond market (Oehmke, and Zawadowski [2017]), though their resultant effect on prices may also depend on trade size. Biswas, Nikolova, and Stahel [2015] for example find that for trade sizes larger than $500K, CDS are not cheaper to trade than bonds. In a parallel strand of research, the introduction of CDSs has been shown to have a detrimental effect on loan sales markets (e.g., Duffee, and Zhou [2001] and Parlour, and Winton [2013]), illustrating financial markets – banking interactions (e.g., Andolfatto, Berentsen, and Martin [2019]).

[4] For example, Ashcraft, and Santos [2009] fail to find evidence that the onset of CDS trading lowers the cost of debt financing for the average borrower. On the other hand, the evidence of the effect of CDSs on credit supply is somewhat more positive, see Hirtle [2009] and Saretto, and Tookes [2013]. We provide a detailed discussion below.

rather aggregated information. Even with granular data, identifying any causal effect of CDS trading on bank credit granting remains an empirical challenge that is not easy to deal with, because hedging and credit extension are decisions, which most likely will be made simultaneously.

In this paper, and in order to achieve identification of the impact, we couple bank-firm exposure data from the German credit register with granular CDS trading data from the Depository Trust and Clearing Corporation (DTCC); hence, both datasets provide bank-firm level information that can be matched. We then uniquely link exogenous reforms in CDS markets to changes in hedging and subsequent changes in credit granting, by focusing especially on the heterogeneous effect across markets participants.

We find that relative to CDS dealer banks, non-dealer banks that (can be shown to) benefit more from the reform, extend more credit to reform-affected firms and hedge more effectively on these firms, by better matching their credit exposures with CDSs. By using a CDS market reform as a shock, we contribute to this literature by identifying the causal effects of hedging on credit supply. Studying granular DTCC data at the bank-firm level enables us to overcome the shortcomings of previous papers, which had to rely on aggregate data.

The reform we are employing is a sequence of two events called "Big Bang" and "Small Bang". They brought contract and convention changes that facilitated a higher degree of standardization in the CDS market on April 8 and June 20, 2009, respectively. The Big Bang, and the subsequent Small Bang, entailed global contract changes, as well as CDS trading convention changes in North America and Europe, separately. The Bangs brought fundamental improvements in the infrastructure of the CDS markets, making the contracts more suitable for central clearing and the markets more transparent. Furthermore, the Bangs likely also cut CDS transaction costs.[5]

[5] The adoption of fixed coupon and upfront payment facilitates netting and assignment of CDS contracts, possibly also reducing dealers' inventory costs. Investors' search costs also decrease, as the standardization reduces the number of

In spite of the overall lowering of transaction costs that all market participants can take advantage of, dealers and non-dealers may not have benefitted in similar ways. We show that non-dealers benefitted more from this standardization than dealers, since non-dealers, who buy protection most of the time from dealers, were able to obtain more advantageous prices than before. This is not only explained by a lower level of market friction, but is also a result of the market standardization that tilted new CDS contracts towards standard types, with identical terms and comparable values for those terms (Chen, Fleming, Jackson, Li, and Sarkar [2011]). Standardization has implications for transparency: As soon as the CDS prices are comparable, all parties will have a higher chance to realize the trading intentions behind incoming quotes. This increase in transparency results in more competition between protection sellers, making purchasing CDS contracts cheaper. Dealers, from whom the non-dealers buy CDS more than 90% of the time, found that selling CDS contracts was no longer such a lucrative business than when the market was more opaque and with more frictions.[6]

Hence, when investigating the effects on CDS trading brought by the Bangs, we actually focus on the differential impact the Bangs have on dealers and non-dealers. Our identification strategy presumes that the Bangs have a larger effect on the non-dealers, since the cost of credit protection is lower after the Bangs, and the dealers have to be content with a lower profit margin when selling protection.

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dimensions in which a single-name CDS contract varies, making dealer quotes more comparable. By introducing a so-called event determination committee and binding settlements, CDS "unspooling" following a credit event will not be as long and uncertain a process as before. This reduces the complexity of the CDS pricing ex ante, as well as the transaction costs involved.

[6] As observed in the *Financial Times* (2009), "CDS market's Big Bang arrives": "Dealers had every incentive to keep the market opaque and bespoke, which boosted margins – and profits, while downplaying infrastructure issues. Thus, when groups such as […] a large hedge fund have campaigned for change in the past, they have encountered resistance." Another report in the *Financial Times* (2010) was entitled "Derivatives dealers voice support for clearing" and raised similar ideas: "The large OTC dealers do not have a sufficient incentive to speed up the process of standardization. Large dealers profit too handsomely from the current system in which they have far more information and far more leverage than other market participants."

We explore this asymmetric effect in two steps. First, by studying the evolution of the spread between, on the one hand, the transaction price of a CDS purchase *by a non-dealer* from a particular dealer, and, on the other hand, the (almost contemporaneous) purchase of the *same* CDS *by a dealer* from the *same* dealer, we can reliably show that the cost of buying a CDS contract after the Bangs became (relatively) cheaper for non-dealers. We find that the aforementioned spread decreases by on average around 21 basis points (bps) or 16 percent. The decrease in such spread is much larger for more bespoke and less liquid contracts, confirming our prediction that standardization reduces the costs of protection purchase.

In the second step, we couple this unique and comprehensive bank-firm CDS trading data with German credit register containing all relevant bank-firm credit exposures. We investigate bank lending to Bangs affected firms, i.e., firms located in Europe and North America that have CDSs traded on them. We are particularly interested in whether non-dealer banks extend more credit to Bangs affected firms relative to dealer banks, because of the relative decrease in the default insurance cost in comparison to dealer banks. Our estimates show that despite a concurrent lending contraction that started in Germany in 2009, non-dealer banks grant relatively more credit to Bangs affected firms after the Bangs than dealer banks, and that this finding is robust to varying the post-Bangs impact period.[7]

To further identify the channel through which credit granting is enhanced for non-dealer banks, we construct an indicator to assess whether this effect can be attributed to the lower insurance cost and the more effective hedging of their credit exposure. The hedging ratio, defined as the ratio of 'the net notional amount of a CDS on a firm held by a bank' to the credit exposure the bank has to

[7] We thereby extend Minton, Stulz, and Williamson [2009] who document that banks hold derivatives positions solely for dealer activities rather than for credit hedging purposes.

the same firm, measures the proportion of the credit the bank has hedged in each quarter. By investigating the evolution of the hedging ratio, we find that the non-dealer banks more effectively hedge their credit exposures, i.e., with net CDS purchases matching their credit exposure, and this is especially the case after the Bangs. Such results hold not only in the whole sample where we find non-dealers did better than dealers after the Bangs, but also in the subsample comprising only non-dealers. This precludes an alternative story which could be that non-dealers less effectively hedge after the Bangs, but still better than dealers. Moreover, a firm for which a bank already had CDS purchases matching its exposures in the firm (such that the bank extended credit to the firm) before the Bangs receives more credit from the same bank after the Bangs, especially when the bank is a non-dealer. This also confirms the hedging channel for the observed increase in credit granting.

The Bangs occurred after Lehman during the recovery phase from the financial crisis, raising concerns that other events such as the Dodd-Frank Act had confounding effects (the Dodd-Frank Act was enacted on July 21, 2010, but its derivatives reforms took effect not earlier than 2011). While the bulk of the banks in our sample are German and hence not directly affected, with a battery of robustness tests we dispel any lingering concerns also with respect to many other contemporaneous policy reactions of potentially lesser importance for CDS (e.g., Bao, O'Hara, and Zhou [2018], Bessembinder, Jacobsen, Maxwell, and Venkataraman [2018]).

We start by estimating the benchmark models for three different sample periods, the shortest one symmetric around the Bangs ending in 2010:Q4. The coefficients of interest are stable across the three sample periods in terms of sign and significance. Furthermore, in our first set of exercises identifying the reduced default insurance costs, we restrict the sample to one year around the Bangs ending on June 20, 2010, right before the Dodd-Frank enactment. This date also long predates the first centrally cleared trade on single-name CDSs by German market participants, excluding the possibility that the decrease in insurance costs is driven by improvement of market transparency or reduced counterparty risks caused by central clearing.[8]

We also investigate the impact of the Bangs on CDS trading itself, especially within a very short period around the Bangs, since the standardization of contracts should facilitate CDS trading. Not surprisingly, we find that CDS trading, as measured by the log of gross notional amount of CDS contracts, significantly increases after the Bangs. Specially, we separately test the change in CDS trading within two symmetric eleven weeks windows around either Bang such that the testing period ends on September 4, 2009. This is much earlier than the Dodd-Frank enactment, and also before the G20 Pittsburgh Summit took place on September 24-25, 2009, where coordinated changes were agreed upon.

For the former test around the Big Bang, where the "treated" firms are the Big Bang affected firms and the "control" firms are (at that time) mainly Small Bang affected firms, we show (in an Appendix: Exogeneity of the Small Bang, and the accompanying Table A.1) that there was no anticipation of the effect of the Small Bang, i.e., the Small Bang affected firms were not traded more. However, the increase in CDS trading on Big Bang affected firms continues even after the Small Bang takes place. This lends support to the lack of anticipation of the Small Bang, i.e., the Small Bang is not predicted by market participants as a natural extension of the Big Bang and the impact of Bangs takes some time to materialize.

Finally, in all our analysis we limit our sample to banks that participate in the CDS markets. Although smaller and less sophisticated than the dealer banks, the non-dealer banks in our sample are still among the top banks and some of them suffered large losses during the financial crisis (as

[8] The literature that studies the effects of CDS central clearing on transaction costs so far has yielded mixed results. Loon, and Zhong [2014] for example document that for a sample of voluntarily cleared single-name contracts CDS spreads increase, while Du, Gadgil, Gordy, and Vega [2019] find that CDS spreads decrease in absolute value. Our estimated spread identifying the CDS insurance costs differs from the transaction costs used in the literature. We will motivate and discuss the differences later.

some of the dealer banks did). Therefore, compared to small banks, which never entered the CDS markets, dealer and non-dealer banks are not that differently affected by the financial crisis and resulting policy actions. More importantly, we rely on estimations that are within-bank-type/time as well as within-firm-type/time. Such estimations account for all observable and unobservable time-varying heterogeneity between bank groups, i.e., dealer versus non-dealers, and firm groups, i.e., CDS firms versus non-CDS firms. This estimation strategy involving comprehensive saturation with fixed effects for each quarter/week and bank/firm group type combination mitigates this final lingering concern.

In sum, banks exploit the financial innovation that the Bangs introduced to better hedge their credit exposure: The banks that benefit more from the CDS market changes and take advantage of a lowered default insurance cost, choose to hedge and grant relatively more credit after the Bangs. Thus, in this way we do establish a link between hedging with CDS and credit granting, and confirm that when banks are in a better position in hedging the credit risks of firms, i.e., facing cheaper costs to hedge, they also extend more credit to those firms that they hedge.

We are not the first to investigate the CDS – credit nexus, but − as far as we are aware − we are the first to couple bank-firm CDS trading information to comprehensive bank-firm level credit exposures to uniquely identify the effect of lowered insurance cost on hedging *per se* and on the supply of new credit in the period around a quasi-natural experiment.[9]

Previous studies of how CDS spur credit supply mostly use bank- or firm-level data,[10] nor do they identify whether the increased credit supply by CDS trading banks are associated with hedging

[9] A somewhat related literature investigates the impact of loan securitization on bank lending (e.g., Loutskina, and Strahan [2009], Loutskina [2011], and Kara, Marqués-Ibáñez, and Ongena [2016]).

[10] Using bank-firm level data, Hasan, and Wu [2017] examine the relationship between bank CDS use and loan sales involving large syndicated credit facilities. They find that banks' usage of CDS hedging complements loan sales (see also Hasan, and Wu [2015]).

more effectively on those firms they extend credit to. Hirtle [2009], utilizing bank-level CDS usage data, find only limited evidence of the association between greater CDS usage and greater bank credit, but a stronger effect in the subsample of newly negotiated loans extended to large corporate borrowers. At the firm level, Saretto, and Tookes [2013] document a larger effect of CDS initiation on the levels of corporate debt. They find that firms with traded CDSs are able to maintain higher leverage and longer debt maturity.

Both studies use aggregate measures such as whether or not a bank trades CDSs or a CDS exists for a certain firm. Such aggregate measures can provide only little information on whether existing creditors of a certain firm buy the firm's CDS to hedge their credit risk, or whether the increased credit supply can be attributed to creditors making use of CDSs. One might argue for the existence of alternative channels; for example, banks may use CDSs to offload some of their credit risk, release tied-up capital, and supply more credit in general and not specifically to firms they hedged. Similarly, it is also possible that firms with traded CDS contracts obtain more credit from banks, which never trade CDSs on these firms, rather than from banks that have high volumes of gross CDS position on the firms. Without more granular data at the bank-firm level, one cannot reliably identify the mechanism of how CDSs may spur the supply of credit. Our empirical approach together with the use of granular data addresses these limitations.

We also contribute to the studies on the standardization of the over-the-counter (OTC) derivatives market. For example, Chen, et al. [2011] provide a descriptive analysis on CDS transactions and the level of standardization; Oehmke, and Zawadowski [2015] and Oehmke, and Zawadowski [2017] discusses how CDS markets emerge as "alternative trading venues" serving a standardization and liquidity role. Our study sheds light on an understudied mechanism relating standardization to the costs of derivative contracts, and provides the first evidence on how the initiation of standardization affects different market participants differently, namely benefitting the non-dealers by lowering their costs of buying the contracts, while squeezing the profit margin of dealers. Our paper also relates to the studies on the CDS Big and Small Bangs. Previous studies use this market change to study the empty creditor hypothesis (Danis [2016]), the illiquidity spillovers between CDS and equity markets (Haas, and Reynolds [2019]), the funding liquidity and market liquidity (Wang, Wu, Yan, and Zhong [2021]), and the effects on CDS liquidity (Fulop, and Lescourret [2019]). However, none of these papers study the impact on credit supply by the Bangs. This is the focus of our paper.

In addition to the studies linking CDS to credit supply, there is also a growing literature on the effects of CDSs in reducing borrowing costs. While Ashcraft, and Santos [2009] fails to find evidence supporting that CDSs could lower debt financing costs for the average firm[11], yet, they uncover economically significant adverse effects on risky and informationally opaque firms (see also Shan, Tang, and Winton [2019]). Kim [2016], on the other hand, find that CDS initiation lowers corporate bond spreads for firm with a priori high strategic default incentives.[12] Amiram, Beaver, Landsman, and Zhao [2017] find the initiation of CDS trading on a firm's debt increases the share of loans retained by the lead arranger of syndicate loans, as well as the loan spread, since CDSs make lead arranger's stake in the loan a less effective mechanism to mitigate adverse selection and moral hazard problems (see also Streitz [2016] who study similar questions but focus on bank risk management incentives). Shan, Tang, and Yan [2014] find that loans issued to CDS-referenced

[11] Saretto, and Tookes [2013] argue why the introduction of CDS has little impact on reducing borrowing cost as found by Ashcraft, and Santos [2009]. One possible explanation is that the demand curve for credit is relatively flat, such that an outward shift in supply has a larger effect on quantity than on price. A more plausible explanation in their opinion is that the CDS introduction also influences non-price terms, such as maturity. Oehmke, and Zawadowski [2015] also aims to reconcile the evidence of the effects of CDSs on debt financing by looking at how CDS introduction crowds out bond trading but improves the long-term bond allocation. They theoretically model the effect of CDS introduction on bond prices and confirm the ambiguous or lack of effects found in Ashcraft, and Santos [2009].

[12] CDSs are shown to have important ex ante commitment benefits. In Bolton, and Oehmke [2011] for example CDSs raise the debtor's pledgeable income and help reduce the incidence of strategic default by strengthening creditors' bargaining power. In Arping [2014] CDS issuance improves the credibility of foreclosure threats, which can have positive implications for borrower incentives and credit availability ex ante. See also Shan, et al. [2019].

borrowers are larger and have higher yield spreads if the lead banks in the syndicate are active in CDS trading. They also show that banks become more aggressive in risk taking after they begin using credit derivatives (see also Subrahmanyam, et al. [2014], as well as the model in Che, and Sethi [2014]). Norden, Silva Buston, and Wagner [2014] show that banks with larger gross positions in credit derivatives charge significantly lower corporate loan spreads, while banks' net positions are not consistently related to loan pricing.

In sum, these papers provide mixed and conditional results of the effects of CDSs in lowering borrowing costs. Though being able to empirically identify a few channels, the fact that the literature fails to find evidence of CDSs reducing debt-financing costs for the average firm is still puzzling. Our study sheds light on a possible explanation: The CDS market friction and opacity could limit the extent to which non-dealer banks can benefit from trading CDS, since non-dealers may face a "lemon's premium" when buying protection if informed investors are active in the market (Ashcraft, and Santos [2009]). However, the improved market transparency and competition among dealers, as well as the lowered trading costs brought by the Bangs may alleviate such premium, and relax the constraints non-dealers face in hedging and supplying credit.

Lastly, the literature finds that CDS usage intensifies risk-taking by banks (e.g., Saretto, and Tookes [2013], Subrahmanyam, et al. [2014] or Shan, et al. [2019]). As our estimates imply that following the Big and the Small Bang hedging through CDSs for non-dealers became cheaper and subsequent more lending took place, we provide well-identified and comprehensive evidence on the benefits of financial innovation for risk mitigation.

The remainder of the paper is organized as follows. In Section 2, we briefly review the contours of the CDS market and the Big and Small Bang. In Section 3, we describe the data and the methodology. We present the main estimation results explaining the changes in hedging costs and subsequent changes in credit exposures and hedging in Section 4, followed by a series of robustness tests. Section 5 concludes.

II. The "Big Bang" and "Small Bang" in the CDS Market

Ever since the 2007-2008 global financial crisis, the CDS market has drawn the attention of regulators, as CDS was blamed for amplifying systematic risk.[13] The single-name CDS market is still a relatively opaque OTC market, with a small number of dealers (mainly large and sophisticated banks) dominating the CDS pricing, and with a very limited pre-trade and post-trade transparency. In order to have a transparent CDS market and to prevent the potential domino effects of counterparty failures, regulators have been pushing the CDS deals to be centrally cleared. In parallel to the G-20 Pittsburgh Summit commitment in 2009 to improve transparency and mitigate risks in OTC markets, the "Big Bang" and "Small Bang" Protocols had a central role in standardizing the market by facilitating higher efficiency.

In an Appendix we discuss all the relevant elements of the Bangs that were key towards building an efficient central clearing infrastructure already in 2009. These changes contribute to the lowering of transaction costs, possibly through a number of channels. First, the adoption of fixed coupon and upfront payment facilitates netting and the assignment of CDS contracts. The dealers can better manage their inventory risk after they sell CDS protection, and the transaction cost may become lower ex-ante. Second, through standardization efforts such as North American CDS dropping restructuring as a credit event, the number of dimensions in which a single-name CDS contract could vary was reduced. This may not only decrease inventory holding costs for dealers as a result of

[13] Even Pope Francis dismissed CDS as "ticking time bombs" that "encourage betting on the ruin of others." See, *Financial Times*, "Pope says credit default swaps are unethical", May 17, 2018.

lowered contract uniqueness, but also non-dealers may find that the dealer quotes become more comparable, hence dropping search costs. Third, according to the bond pricing literature, features that can complicate bond valuation will result in a higher transaction costs (e.g., Harris, and Piwowar [2006], Edwards, Harris, and Piwowar [2007]). After the Bangs an event determination committee and binding settlements were introduced; so that the CDS settlement following a credit event became more transparent and efficient, because it converged to a process not as long and uncertain as before. This may reduce the complexity of CDS pricing, lowering the transaction costs. Although Wang, et al. [2021] show that the adoption of upfront payments may increase funding costs and have a negative effect on liquidity for some contracts, we hypothesize that on average the CDS transaction costs will be lower after the Bangs.

In addition to transaction costs, the Bangs may also change the competitiveness of the CDS markets. Since the price of a given transaction reflects the economics of its terms, a sequence of time-stamped transaction prices is most meaningful when the economics is comparable from transaction to transaction, i.e., when instruments come with identical terms and comparable values for those terms (Chen, et al. [2011]). That is to say, standardization initiatives such as introducing a fixed coupon may imply a higher level of transparency, as the investors see transaction prices in a given time period as more comparable. This could intensify the competition between protection sellers. Participants in the CDS markets are affected differently: The non-dealer participants, who almost always buy CDS from dealers, find themselves facing better prices as the competition intensifies. While the dealers can also take the advantage of the improved market efficiency, their profit margin may be affected by the fact that OTC derivative trading business might not be as lucrative as when the market was more opaque and was based on non-standard products. The analysis in this paper will make use of transaction level CDS data to verify that the impact of the Bangs on non-dealer banks is more positive than the impact on large dealers.

III. Data Sources

We employ three data sources. A first unique dataset we access is from the Trade Information Warehouse (TIW) of the *Depository Trust and Clearing Corporation* (DTCC). This data source covers more than 95 percent of the global activity for standard single-name CDSs, making it by far the most comprehensive dataset for CDS positions and trading.[14]

The DTCC position-level data provides a weekly bought and sold position for each financial institution on each firm with each counterparty after accounting for all new trades, assignments, terminations and amendments that have happened since the earlier week. The coverage of the dataset was stable during our observation period and less likely to be affected by the increasing tendency to use central counterparty clearing.[15] Different from the weekly firm-level data,[16] which are aggregated across all trades and publicly available on the DTCC website, we have unique access to the position-level data, which are mainly maintained for regulatory supervision purposes. It includes two subsets: The first subset contains detailed information on *all* German financial institutions and their weekly CDS trading with each individual counterparty of *all* extant individual firm CDS contracts. We also employ a second subset, which contains the same detailed information, on *all* financial institutions trading on *all* German reference entities. The original data from both sources are at the financial institution/counterparty/CDS reference entity/week level. We append these two subsets and eliminate duplicate reporting. For each financial institution, we aggregate its

[14] Gehde-Trapp, et al. [2015] note that "*The DTCC estimates that its coverage of credit derivatives amounts to 95 percent of single-name CDS in terms of the number of contracts, and 99 percent of single-name CDS with respect to notional amounts.*" In our analysis, we use both the position-level data and the transaction-level data from DTCC. We will briefly discuss the transaction-level data in the results section (as we use this dataset only in our first set of exercises), and concentrate here on the position-level data.

[15] In order to rule out any trend in coverage, we will be looking at alternative time intervals to check the robustness of the proposed results.

[16] Using firm-level aggregated positions of this dataset, Oehmke, and Zawadowski [2017] for example document trading and arbitrage activity on the CDS market.

CDS contracts on each individual firm (reference entity) across positions with different counterparties at the weekly level. These positions of protection bought and sold on each firm uniquely capture the risk taking of the financial institution related to the respective firm. None of this information is available on the public website of the DTCC.

In a first pass, we construct a sample with those financial institutions in the DTCC dataset that are also active in credit markets. We match the financial institutions trading CDS and the underlying reference entities with the German credit register (*MiMik*), which records the credit exposure of each financial institution with each borrower by the end of each quarter. The credit register includes not only borrower-level loan exposures to the firms that we observe in the DTCC dataset as reference entities, but also other broadly-defined credit exposure on these firms, such as the financial institution's corporate bond exposures.[17] We select those financial institutions in the DTCC dataset which also have to report their credit exposures to the German regulators and are thus in the credit register sample, i.e., German banks which trade CDS, as well as large foreign banks which trade CDS on German firms, having German branches and are required to report to German regulators. By doing so, we filter out those financial institutions which are not in the credit register sample, i.e., asset management companies which do not extend loans. Eventually, this exercise lets us keep all the financial institutions that are in both CDS position-level data and credit register data, and all the CDS reference entities (firms) they trade CDS on.[18] We call this sample the "CDS Sample".

[17] For a more detailed definition of the bank exposures, see Section 19 of the Banking Act (Deutsche_Bundesbank [2001]). The following items are deemed not to be bank exposures: Shares in other enterprises and securities in the trading portfolio. Details on this credit register can also be found in Schmieder [2006], and in published work by Schertler, Buch, and von Westernhagen [2006], Hayden, Porath, and von Westernhagen [2007], Ongena, Tümer-Alkan, and von Westernhagen [2012], Behn, Haselmann, and Wachtel [2016], Haselmann, Schoenherr, and Vig [2018] and Ongena, Tümer-Alkan, and von Westernhagen [2018], for example. The Bundesbank also maintains a website with most papers based on its credit register.

[18] We also enforce the restriction that these firms should have an identifier in the credit register data. Actually only very few CDS firms are dropped due to this restriction.

As the fourth largest economy in the world and a bank-based system, Germany is a particularly interesting country to study the link between CDS trading and the supply of credit. The German universal banking system is structured along three pillars, i.e., commercial banks, public sector banks and credit cooperatives (Krahnen, and Schmidt [2004]), and all three types of banks lend to corporates and participate in the CDS market.[19] Despite obtaining, withdrawing and repaying credit – possibly frequently – firms keep their individual credit amount from banks surprisingly constant over time. This observed persistency in individual exposures makes our ensuing estimates of the impact of CDS trading on credit even more economically relevant. The credit register contains information on large credit exposures of 1.5 million Euros and above on each quarter end.[20] Therefore, exposures to small and medium-sized firms are under-represented in this database. However, for our study this threshold is of little or no concern as most if not all CDS contracts that are traded pertain to large firms with commensurately large exposures.

In a similar manner as we did for the "CDS Sample", we include from the full credit register only those financial institutions that trade CDS contracts, while excluding small banks which never participate in the CDS markets. This dataset consists of all the CDS trading financial institutions that are also in the credit register, and all the firms those financial institutions extend credit to (also including firms never having CDS being traded on). We call this sample the "Credit Exposure

[19] According to the Bundesbank Banking Statistics, by the end of 2008, there were 1,864 banks in the country of which 64 percent were credit cooperatives. However, as credit cooperatives are very small institutions, and commercial banks include the four largest institutions in the country, the picture in terms of market shares is substantially different. Commercial banks account for 36 percent of all bank assets, mortgage and special purpose banks 20 percent, whereas public sector banks also take 33 percent, and credit cooperatives together with their central institutions only 11 percent. These figures clearly indicate the importance of the public sector banks, which include the savings banks ("Sparkassen") and their central institutions ("Landesbanken").

[20] If the sum of the exposures to firms in a borrower unit exceeds the threshold of 1.5 million Euros, the individual exposure to a firm in that borrower unit is also reported, even if it is a small exposure below this threshold. For a more detailed definition, see Section 14 of the Banking Act (Deutsche_Bundesbank [2001]). If exposures of 1.5 million Euros or above existed during the reporting period but are partly or fully repaid, the remaining exposure is reported even if the amount is zero. We take the actual amounts of the single-borrower exposures into consideration rather than those at the holding company level.

Sample". Since our following analyses (except the first one with CDS transaction data) are always based on one of these two samples, we always refer to these samples by their labels "CDS Sample" or "Credit Exposure Sample" (for maximum clarity we describe their construction once more in Figure 4)[21].

As the regulatory CDS data are only available since 2008, we decided to adopt the sample period starting from 2008Q1 until 2016Q2 (while excluding the two Bang implementation quarters of 2009Q2 and 2009Q3) for both the CDS sample and Credit Exposure Sample. Despite the difference in the coverage of firms, the two samples cover the same set of financial institutions that appear in both data sources. Since the majority of these financial institutions are banks (except for a few financial services firms which also need to report credit exposures), we will call them "banks" in the following discussions. A detailed description of the number of banks/firms/bank-firm pairs in each sample is provided in Table 1. The 95 banks left in our Credit Exposure sample after matching the CDS and credit exposure data account for around 50 percent of total credit received by all sample firms, although by the end of 2008, there were almost 2,000 banks in Germany.

Figure 5 displays the weekly-reported total gross and net CDS notional amount held (in Million Euros) by all the banks before and after the Bangs. A number of elements are worth noting. While the gross amount hovers between 2.5 and 3.5 Trillion Euros throughout the period (which ranges from the beginning of 2008 to the end of 2011), the net amount increases from around (or below) zero to 50 Billion Euros starting six months after the Small Bang (on July 24, 2009). This (somewhat) belated increase in net amount is consistent with the increase in the number of CDS trading non-dealer banks from 53 to 73 and the number of firms engaged (both sets of statistics are reported

[21] Notice that in the end we have 98 banks in the CDS Sample and 95 banks in the Credit Exposure Sample (indicated as in Figure 4 and Table 1). This is because we go for the most comprehensive samples that are possible in all time periods, and consider those banks that ever appear in the credit register data and ever trade CDS as well. Therefore, the CDS and Credit Exposure Samples, which are confined to a certain time period, could have varying numbers of banks from each other.

in Table 1). Indeed, banks entering the CDS market may have to set up a team of trained personnel, and this takes time in particular when engaging new firms. In Figure 6, which is discussed in the following section, we will see that non-dealer credit exposures follow a similar time path, visually providing a timing correspondence between CDS and credit growth, which our empirical analysis then more firmly establishes.

IV. Hypotheses and Results

A. Dealer vs. Non-dealer Banks in the Post-Bangs Period

One of the major targets of the Big and the Small Bang is to improve the standardization of CDS contracts, and the contracts and convention changes by the Bangs may lower the CDS insurance costs for non-dealers, as discussed in Section 2. We initially hypothesize that non-dealers pay less to buy credit protection after the Bangs, as a result of the lowered transaction costs and intensified competition between dealers. We will call this our "cheaper-insurance" hypothesis. Consequently, the non-dealers may have stronger incentives to hedge their credit exposures and subsequently extend more credit after the Bangs. On the other hand, the literature on opaque markets (e.g., Pagano, and Röell [1996]) suggests that larger dealers may be reluctant to adopt these standardization-improving changes, because they could profit more from an opaque OTC market. While dealers could also be positively affected from improved market efficiency, they may find it insufficient to offset their squeezed profit margin due to intensified competition. Hence, they might be relatively negatively affected overall. More precisely put, non-dealers could be more positively affected by the contract standardisation and other changes brought by the Bangs in relative terms.

The first exercise we do is to assess if non-dealer banks pay relatively less to purchase CDS protection from a dealer after the Bangs, by matching both on the reference entity and risk class involved, and on the seller and buyer of the protection. Non-dealers also sell CDS contracts to dealers, i.e., some hedge fund or asset management companies sell CDSs to speculate or obtain bond-like cash flow.[22] As we are mainly interested in credit extension by banks, rather than by non-bank institutions not covered in credit register data, we will restrict our scope to non-dealer banks *buying* CDS trades in this first exercise. We will be looking at the spread between transaction prices of *matched* "dealer-buys-from-dealer" trades (or dealer-to-dealer, D2D trade) and "non-dealer-buys-from-dealer" trades (or dealer-to-customer, D2C trade) and check whether this spread has decreased after the Bangs.

One possible concern could be that the above spread between D2D trade and D2C trade may actually be a liquidity measure, i.e., D2C prices may well represent the ask prices, which could be also done with publicly available bid-ask spread data. Given the limitation on the availability of CDS trading data, the most widely used liquidity metric, i.e., the bid-ask spread, is usually calculated in the CDS literature with CDS quotes rather than actual trading prices. For some data sources (e.g., Markit daily composite spreads) it equals the difference between two composite quotes, i.e., both bid and ask quotes are averages across all market quotes (including D2D quotes) by the end of a day. From a composite quote it is obviously impossible to disentangle the relative difference between D2D and D2C quotes. Moreover, even if some bid-ask spreads are an average across D2C quotes only (e.g., CMA offer/bid quotes), it is still unclear whether any decrease in the bid-ask spreads reflect the decreases in transaction costs (or specifically the default insurance costs), or other market level changes (e.g., funding costs decrease, in which case the D2D bid-ask spreads also decrease) and reference entity related factors that can contribute to changes in liquidity. Lastly, the available bid and ask quotes are usually only indicative and not binding commitments, and the

[22] Dealer are shown to be liquidity providers that earn the bid-ask spread in the US corporate bond market even during the global financial crisis (Choi, Shachar, and Shin [2019]). Although some large asset managers have taken dominant positions in the CDS market in recent years, dealers maintain their role as protection sellers to a large proportion of buyside participants.

actual transaction prices after negotiation between dealers and customers can be quite different from the indicative quotes (Jankowitsch, Nashikkar, and Subrahmanyam [2011],Biswas, et al. [2015]).

B. Transaction Price Data

In order to investigate whether the matched spreads between D2C trades and D2D trades changed after the Bangs, we make use of an additional data source, the transaction-level data of German banks trading CDSs on all global reference entities. The transaction dataset, which is also part of the TIW of the DTCC, includes information on the trading party, counterparty, transaction date, transaction volumes, transaction prices, reference entity and other details about the CDS contract being traded. Different than the position-level data that aggregate the existing CDS contracts each German bank has with each counterparty on each reference entity at a weekly level, the transaction-level data record information on every single transaction with at least one trading party being a German financial institution. Moreover, focusing on only German banks' full set of transactions will facilitate a comparison between the transaction prices of a group of non-dealers and dealers operating under the same economic and regulation environment. In order to study the difference in transaction prices between D2C trades and D2D trades, we filter out only new trades that took place between one year before and one year after the Big (Small) Bang for Big (Small) Bang affected firms. That said, the sample period for this exercise is between April 8, 2008 and June 20, 2010.

One important difference between position-level data and transaction-level data is that the latter enables us to observe the transaction price for each single transaction. As one of the major convention changes of the Big and the Small Bang, transaction prices of new trades after the Bangs are adapted to the form of fixed coupon rate (usually 100 bps or 500 bps, although 25 bps, 1,000 bps, 300 bps or 750 bps also occur) plus upfront payment. We convert any prices quoted in fixed coupon rate plus upfront payment into the conventional prices (non-standard coupon rate without upfront payment) using the spread converter adopted by ISDA, such that they are all comparable.

C. Matching Trades before and after the Bangs

Our identification strategy relies on the evolution of the spread between transaction prices of a CDS purchase by a non-dealer from a dealer, and the purchase of the same CDS by other dealers from the same dealer, and on the investigation of whether this spread has changed after the Bangs. We do this in two steps. In the first step, we calculate the following absolute spread before the Bangs:

$$\begin{aligned} &Absolute\ Spread\ Before\ Bangs_i \\ &\qquad = Price\ of\ Trade\ 1\ (Dealer\ X\ sells\ CDS_i\ to\ Non-dealer\ Y) \\ &\qquad - Price\ of\ Trade\ 2\ (Dealer\ X\ sells\ CDS_i\ to\ Dealer\ Zs) \end{aligned} \tag{1}$$

Here, Trade 1 and Trade 2 are both trades within one year before the Big (Small) Bang on North American (European) firms. For each 'non-dealer buying from dealer' trade (Trade 1), we search for the matched 'dealer buying from dealer' trade (Trade 2), meeting the following conditions:

1. Trades 1 and 2 have the same seller (Dealer X).
2. The CDS contract of Trade 1 and 2 must have the same reference entity, same seniority (Secured/Senior Unsecured/Subordinated), same restructuring indicator (with or without restructuring) and same maturity (in number of quarters).[23]

[23] Specifically, when calculating the maturity of a standard CDS contract after the Bangs, we take into account how markets consider on-the-run contracts. On the 20th of March, June, September and December, the market moves to new on-the-run contracts. The termination date of a standard CDS contract is always one of the above four dates, so one can calculate the maturity in quarters. As an example, a 5-year CDS contract starting on January 1, 2010 and terminating on March 20, 2015 and another 5-year CDS contract starting on March 19, 2010 are both on-the-run 5-year contracts before March 20, 2010 and are fully fungible. However, after March 20, 2010 both of these two contracts become off-the-run and are less liquid.

3. Trade 1 and 2 have very close trading dates. The difference of the two trading dates should be maximum three days.

4. Trade 1 and 2 have similar trading volumes. That is, the absolute value of the differences of the log of the trading volume should be smaller than 1.

$$|Log(notional\ amount) of\ Trade\ 1 - Log(notional\ amount)\ of\ Trade\ 2| \leq 1 \qquad (2)$$

Since D2C trades are less frequent than D2D trades, there are usually multiple Trade 2s, possibly with different dealers (Zs) that match with Trade 1. In these cases, we take the average prices of all the matched trades as the price of Trade 2, such that it is actually a composite price.

Although we account for the individual characteristics and the size effect on the absolute spreads by matching the two prices, there could still be a concern that a CDS with a high price would usually have a high absolute spread as well. Hence, we also calculate a relative spread:

$$Relative\ Spread\ before\ Bangs_{\,i} = \frac{(Price\ of\ Trade\ 1 - Price\ of\ Trade\ 2)}{(Price\ of\ Trade\ 1\ +\ Price\ of\ Trade\ 2)/2} \qquad (3)$$

where we use the mid-price of Trade 1 and Trade 2 as the benchmark.

Next, we calculate the same spread but for trades within one year after the Big Bang (Small Bang) for North American (European) firms. That is, for each 'non-dealer buying from dealer' trade (Trade 3), we search for 'dealer buying from dealer' trade (Trade 4) and calculate the following spread:

$$\begin{aligned} &Absolute\ Spread\ After\ Bangs_i \\ &\qquad = Price\ of\ Trade\ 3\ (Dealer\ X\ sells\ CDS_i\ to\ Non - dealer\ Y) \qquad (4) \\ &\qquad - Price\ of\ Trade\ 4\ (Dealer\ X\ sells\ CDS_i\ to\ Dealer\ Zs) \end{aligned}$$

The exact same conditions stated above should be met for matching Trade 3 and Trade 4. We calculate the relative spread after the Bangs as well, that is:

$$Relative\ Spread\ after\ Bangs_i = \frac{(Price\ of\ Trade\ 3 - Price\ of\ Trade\ 4)}{(Pirce\ of\ Trade\ 3\ +\ Price\ of\ Trade\ 4)/2} \tag{5}$$

The above spreads are related to CDS transaction costs in Biswas, et al. [2015] and Collin-Dufresne, Junge, and Trolle [2020], where they make assumptions on the model form and use prices on individual transactions to directly estimate the transaction costs, i.e., the effective half-spread with respect to the mid-quote. Our estimated spreads between D2D trades and D2C trades is related to their transaction costs, however, our approach also has several advantages. First, the transaction cost is the overall cost of trading a CDS contract, which is affected by observed and unobserved characteristics of the reference entity, trading size, buyer, seller or the market. Simply looking at the changes in transaction costs around the Bangs, we cannot distinguish between different factors that may drive the changes in transaction costs. However, by matching on the reference entity, the seniority, the maturity, the size, the trading date and the buyer-seller, we not only control for almost all the contract level characteristics that can affect the CDS transaction costs, but also seller-specific effects or any change in such effects (e.g., a change in individual dealer market power or funding constraint) that are rarely captured in other studies. Second, estimating transaction costs based on certain functional form requires a minimum number of daily trades for each reference entity, and such a filter will exclude a large number of illiquid CDSs. While Collin-Dufresne, et al. [2020] focus on standard products of index CDSs, and Biswas, et al. [2015] keep only those liquid 5-year single-name CDS contracts, which represent around 20 percent of our sample, we retain all the transactions on single-name CDSs, such that our sample is a better representation of those relatively illiquid or bespoke CDSs. Lastly, the exercise of testing the evolution of spreads over time will help us understand whether the default insurance costs decrease for non-dealers relative to dealers after the Bangs, which may further lead to an increase in the credit supply from non-dealers relative to dealers. While none of these papers examine changes in transaction costs around the Big and Small Bang, this is what we intend to do in this section.

In the next step, we match the spreads before and after the Bangs in two ways, and calculate the change in the spreads. A rigorous way is to match the spreads on the same non-dealer buyer and seller, as well as the same reference entities. More specifically, we match the spreads before and after the Bangs by requiring Trade 1 and 3 to have the same reference entity, same buyer Y and same seller X. A less rigorous way is to match the spreads only on the same reference entity. For both ways, we form pairwise combinations of all matched pairs of spreads, and then subtract the spread before the Bangs from the spread after the Bangs, and regress the differences of spreads on a constant:

$$Spread\ After\ Bangs_i - Spread\ before\ Bangs_i = \beta Constant \quad (6)$$

In Table 3 panel A, we display the number of matched observations and the mean of the spreads. Notice that in the first step, we could match 6,940 Trade 1s to Trade 2s, which have the same reference entity and are sold by the same seller, and 2,270 Trade 3s to Trade 4s, respectively. The number of matched pairwise combinations of before- and after-Bangs spreads are much smaller compared to the original sample size, and it is not surprising that we could obtain more combinations from the less rigorous matching method. Although a more accurate comparison between the before- and after-Bangs spreads might be obtained through matching, simply looking at the mean of spreads we find that the mean of after-Bangs spreads are generally lower than the mean of the before-Bangs spreads.[24] This is consistent with our later findings through regressions.

[24] Some mean of spreads in Table 3.A are negative, and may seem counter-intuitive at the first sight. However, this may be related to findings in Biswas, et al. [2015] that the transaction costs for D2D trades are not necessarily cheaper than D2C trades. For trade sizes above $50 million, they find that the transaction costs of D2D trades and D2C trades are not significantly distinguishable, while for trade sizes smaller than $2.5 million, the transaction costs of D2C trades are significantly cheaper.

In Panel B we display the regression results using the two methods of matching, separately. Columns 1 and 2 present the loadings on the constant from the regressions (Equation 6), using the rigorous matching (Column 1, same buyer-seller and same reference entity) and the less rigorous matching (Column 2, same reference entity only). It is quite clear that for both absolute spread and relative spread, the constants are negative and significant in both Columns 1 and 2, implying a significant decrease of the spreads after the Bangs: The absolute spread decreases by 21 bps, translating into a decrease in the relative spread by 16 percent. This confirms our hypothesis that non-dealers pay less to buy protection after the Bangs, and the Bangs benefit non-dealers more than the dealers, who have to give up some of their profits in selling protection to non-dealers.

In Columns 3 and 4 of Panel B, we re-run the regressions by constraining both the after- and before-Bangs spreads to (i) spreads on the most liquid standard contracts (Column 3, 5-year senior unsecured contracts) or (ii) spreads on other contracts (Column 4), using the matching method on the reference entity only, which yields more observations. Even without formally testing, a comparison of the coefficients in Columns 3 and 4 suggests that these coefficients are much larger in absolute value for the sub-sample of the other contracts.[25] This is further evidence that the decrease in insurance costs are driven mainly by the lowering of the costs of the less liquid contracts, whose prices might have been more opaque, and, exactly those that dealers could have initially made more profits from.

[25] Notice that the sample sizes in Column 3 and 4 do not add up to the sample size in Column 2. In Column 2, the before- and after-Bangs spreads are matched only on the same reference entity, implying that a before-Bangs spread on 5-year senior unsecured contract can be matched to an after-Bang spread on the other contracts. Though still requiring the same entity, in Column 3, both the before-and after-Bangs spreads are on 5-year senior unsecured contracts, while in Column 4, both before- and after- Bangs spreads relate to contracts other than the 5-year senior unsecured ones.

D. Dealer vs. Non-dealer Credit Extension in the Post-Bang Period

We have shown in the previous session that non-dealer banks pay significantly less to purchase credit protection after the Bangs. As our next step, we would like to test a key outcome of this change: Whether this will lead non-dealer banks to lend relatively more to those CDS firms after the Bangs because of the decreased insurance cost of default. As a first pass, we plot in Figure 6 the mean of outstanding credit exposure for dealer and non-dealer banks granting to Bangs affected and unaffected firms. The vertical bars represent the quarterly differences between affected and unaffected firms, while the gray horizontal lines represent the average difference between affected and unaffected firms for the periods 2008Q1-2009Q1 and the three impact periods 2009Q4-2010Q4, 2009Q4-2012Q4 and 2009Q4-2016Q2, respectively (with the shades of gray again corresponding to those introduced in Figure 1). This average difference is consistently higher in the after-Bang period only for the lower panel where we focus on non-dealer banks, in contrast to the upper panel where we focus on dealer banks where in the longest impact period the average difference is negative.

This provides first-hand evidence that non-dealer banks might lend relative more to Bangs affected firms in the post-Bang period, which is not the case for dealers. To formally test this finding, we match the bank-firm level credit exposure data with the bank-firm level CDS position data, and estimate the following model based on the Credit Exposure Sample:

$$\begin{aligned} &Log(Bank - Firm\ Exposure)_{ijt} \\ &\quad = \beta\ Bangs\ Affected\ Firm_j \times 1\{t > 2009Q2\}_t \\ &\quad \times Non - Dealer\ Bank_i + \alpha_{i(bank\ type)t} + \alpha_{j(firm\ type)t}\ (or\ \alpha_{jt}) \\ &\quad + \alpha_{i(bank\ type)j(firm\ type)} + \varepsilon_{ijt} \end{aligned} \tag{7}$$

The dependent variable is the log of bank-firm credit exposure, i.e., the outstanding on-balance-sheet credit that each bank extended to each firm at each quarter end. The model is estimated with the Credit Exposure Sample as defined in the previous section. In the following analysis we will focus on the intensive margin, i.e., we restrict the sample to those observations with positive credit exposures.

In order to explain the time-varying bank-firm exposure after the Bangs, we use a firm-level treatment variable, $Bangs\ Affected\ Firm_j$, which equals to one if the firm j has CDS traded on it before the Bangs,[26] and the firm is located in North America or Europe.

We are aware that all the new CDS contracts will be affected by the Big or Small Bang contract changes, however, the convention changes are separately done for the firms located in North America (Big Bang affected) or Europe (Small Bang affected). To be conservative,[27] the control group for this variable comprises either firms without traded CDSs before the Bangs (the vast majority) or CDS firms located in other areas of the world.

We are also aware of the fact that firms with CDS traded on them are larger and may differ in many other characteristics from non-CDS firms (e.g., Degryse, Gündüz, O'Flynn, and Ongena [2020]). In our line-up of exercises, we therefore saturate specifications with various sets of fixed effects, including Firm Type * Quarter and Firm * Quarter fixed effects. Firm Type is an indicator equal to 1/2/0 for 'Small Bang affected CDS firm' / 'Big Bang affected CDS firm' / Other; and Quarter represents dummies for each year-quarter. Specially, Firm * Quarter fixed effects directly

[26] CDSs traded before the Bangs are unaffected as legacy contracts (Weiss [2009]), but as these firms were traded on before the Bangs, we expect them to be traded on also after the Bangs under the new rules. Moreover, the contracting parties can also choose to amend legacy contracts bilaterally to adapt them to the new protocols, making these contracts more liquid.

[27] Not incorporating CDS firms in other areas into Bangs Affected Firm is also supported by Table A.1, where we find the Big Bang did not affect European CDS firms earlier before the Small Bang.

(linearly) control for all time-varying observable and unobservable firm characteristics, and (importantly) also "adroitly" selects only those firms that have multiple bank relationships – and are therefore comparable in size (e.g., Detragiache, Garella, and Guiso [1997]; Ongena, and Smith [2000]) – to contribute to the estimates. This set of fixed effects also control for any change in the credit demand by firms, as the Bangs may affect firms' credit decisions (see, e.g., the study by Bartram, Conrad, Lee, and Subrahmanyam [2021] on how the introduction of CDS affects the firms' investments). In further robustness checks, we will also select similar firms by matching on bank portfolio size (e.g., Berger, Miller, Petersen, Rajan, and Stein [2005]) for example.

We tabulate the number of observations and the summary statistics of the samples in Tables 1 and 2. Table 1 demonstrates that out of the 202,404 firms that the sample banks have credit exposures to, only 1,389 firms have traded CDS contracts before the Bangs, and of which 1,162 firms are affected by the Big or Small Bang. These 1,162 firms constitute our treatment group. The remaining 201,242 that are not traded or traded elsewhere, mainly in Asia, constitute our control group.

The other key variable is the *Non-Dealer Bank* dummy, which is a time-invariant variable since there is no switching of group taking place for dealers and non-dealers during our sample period. We interact the Bangs Affected Firm, Non-Dealer Bank and $1\{t > 2009Q2\}_t$, a dummy indicating the quarters after the Bangs as our main explanatory variable in explaining the log of credit exposure. Our main conjecture is that *given that after the Bangs non-dealer banks pay significantly less to purchase credit protection, we expect firms with CDSs that are affected by the Bangs to obtain more credit from non-dealer banks.* We will call this our "cheaper-insurance-more-credit" hypothesis.

Finally, we add fixed effects, including Bank Type * Quarter fixed effects, Firm Type * Quarter fixed effects (or Firm * Quarter) and Bank Type * Firm Type fixed effects, to control for unobserved time-varying bank type and time-varying firm type effects.[28] Bank Type is a dummy equal to one for non-dealers and zero for dealers. These fixed effects also help to alleviate the concern that inherent differences exist between dealers and non-dealers, as well as between CDS and non-CDS firms, both before and after the Bangs. We adopt the same sets of fixed effects in almost all of the remaining tables,[29] and cluster at bank-firm level throughout the paper to correct for potential dependency over time.

The results are shown in the first three columns in Table 4. The columns differ in the time period the sample covered, that is we include 2008Q1 to 2010Q4 (a symmetric but short time period around the Bangs), 2008Q1 to 2012Q4 (which implies a longer impact period which ends before the implementation of Basel III in Germany for example) and 2008Q1 to 2016Q2 (the maximum impact period) separately in the three columns. Since all the contracts and convention changes of the Bangs are not implemented immediately within one day (and it could have actually taken a few months before they are all set), we remove the second and third quarter of 2009 from all sample periods (in all exercises except those in Table A.1 for reasons mentioned then), such that the contrast before and after the Bangs on the credit exposure becomes more cleanly observable. However, no matter which time period used, the triple interaction between the three variables Bangs Affected Firm, the dummy for quarters after 2009Q2 and the Non-Dealer Bank is always positive and significant, confirming our cheaper-insurance-more-credit hypothesis. This finding implies that non-

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[28] As Firm Type can take three values, there will be three fixed effects covering this variable. In order to estimate Equation 7, which has three sets of fixed effects, we employ the approach by Correia [2019]. In the regression results we also report the coefficients of the constant, however we are aware that this coefficient does not tell much when fixed effects are included.

[29] In tables which employ weekly CDS data (Tables 7 and A.1), we adopt Bank Type * Week FE, Firm Type * Week FE and Bank Type * Firm Type FE, in which Week represent dummies for each year-week.

dealer banks extended relatively more credit to ‘Bangs affected’ firms after the Bangs, compared to non-CDS firms or unaffected firms, that the effect was large involving an increase of exposure at the bank-firm level by around 50 percent (but remember their reduction in insurance cost was around 20 percent), and that the effect lasted for a long time period.

In Column 4, we implement a placebo test, i.e., we use the same treatment dummy to explain the dependent variable, but for a different period. Specifically, we adopt the period of 2014Q1 to 2016Q2 as the placebo period, and use a dummy equal to one for quarters after 2015Q1 as the fake Post- period, and zero otherwise. We interact this dummy with the Non-Dealer Bank and Bangs Affected Firm variables. The insignificant and negative coefficient indicates that our treatment is not random, and that the effects likely come from the Big and the Small Bang. Similar placebo tests are done for all models in later tables; and it is noteworthy that the key terms in placebo tests always have different signs and are usually insignificant, showing that the differential effects between dealers and non-dealers are most likely determined by the Bangs.

Another concern is that our results could be driven by some firms borrowing from one bank in one quarter and from another bank in another quarter (in which case the increase in credit could be due to firm characteristics that spur bank switching and not to bank type). To address this concern, and although our main treatment variable is a time-invariant firm level treatment, we employ an even more saturated fixed effects set. In Columns 5 to 8, instead of Firm Type * Quarter FE, we use a demanding Firm * Quarter FE set. Notice that the number of observations decreases by more than half compared to the observations number in the first three columns, evidence of the fact that many firms obtain credit from only one sample bank in one quarter. However, the coefficients on the triple interactions are still positive and significant in all different sample periods, indicating that the effects of the Big or the Small Bang on lending involve an increase of around 20 percent on the intensive margin and hence are robust to the absence of bank switching.

In Table 5, instead of the firm-level treatment (on top of being a CDS firm), we would like to further distinguish between the extent to which a bank has been trading CDS on a firm, as a measure of how intense the treatment effect could be. A bank frequently trading CDS on a firm may continue with a high level of trading after the Bangs, while it may take some time to increase its level of trading to a certain level if the bank never traded the firm's CDS before the Bangs. We try an alternative bank-firm level treatment dummy, which is Bangs Affected Bank-Firm Pair. It equals one if the bank has bought or sold (or both) CDS contracts on the firm in the week before the Big (Small) Bang and the firm is a Big (Small) Bang affected firm. As a bank-firm level treatment variable, we are able to incorporate the term Bangs Affected Bank-Firm Pair * 1(>= 2009Q2) alone with the same set of fixed effects as in the first four columns in Table 4. The results are shown in the first three columns of Table 5. The negative and significant coefficient of this interaction term may indicate that in general, being a 'Bangs affected' bank-firm pair will lead to a lower credit exposure after the Bangs, possibly due to the lending contraction after the financial crisis. In Columns 5 to 7, we include the full set of interactions between Bangs Affected Bank-Firm Pair, 1(>= 2009Q2) and Non-Dealer Bank. Specifically, the triple interaction between the three variables are again positive and significant, confirming our cheaper-insurance-more-credit hypothesis using the firm-level treatment. That is, due to the lowered insurance cost, non-dealer banks extend relatively more credit to the 'Bangs affected' firms, especially when the bank has traded CDS on the firm before the Bangs, such that the treatment effect might be larger to the bank-firm pair.

E. Dealer vs. Non-dealer Hedging in the Post-Bang Period

In order to better identify the channel through which credit extension is enhanced for non-dealer banks, we would like to further look at whether the non-dealer banks indeed hedge their extended credit with CDS more effectively after the Bangs, as a result of the lowered insurance cost. Looking simply at the net CDS position at bank-firm level does not take into account how much volume the bank actually needs to hedge. Therefore, we aim to distinguish between hedging and speculation. Our strategy is to construct an indicator for banks that seem to hedge their credit exposure, and to look at the evolution of this bank-firm level indicator across time. With their cost of insuring decreasing, we expect non-dealer banks after the Bangs to hedge more effectively, i.e., match their credit exposures in the firms they buy protections on. This is our "cheaper-insurance-better-hedging" hypothesis.

We first construct the Hedging Ratio, defined as the ratio of the net notional amount of CDS on a firm held by a bank to the credit exposure the bank has to the same firm. We take a snapshot of the weekly CDS data at the end of each quarter and divide the quarter-end net notional amount of a CDS contract by the credit exposure in that quarter. Thus, the Hedging Ratio is a bank-firm level variable showing what proportion of credit the bank has hedged in each quarter. Specially, in this exercise we employ the CDS Sample, and restrict the sample to those bank-firm pairs with either buy or sell CDS contracts (or both); and the Hedging Ratio could actually belong to one of these following cases:

1) Both net CDS and credit exposure are non-zero and the Hedging Ratio is in the interval [0.5, 2].

2) Both net CDS and credit exposure are non-zero and the Hedging Ratio is smaller than 0.5 (including negative net CDS contracts).

3) Both net CDS and credit exposure are non-zero and the Hedging Ratio is larger than 2.

4) Both net CDS and credit exposure are zero, but net CDS is maintained at zero (with positive buy and sell contract volumes equally offsetting one another).

5) Net CDS is maintained at zero but with positive credit exposure.

6) Positive net CDS contracts with zero credit exposure.

7) Negative net CDS contracts with zero credit exposure.

We now define the Hedging Ratio Dummy equal to one if the Hedging Ratio belongs to Cases 1 and 4, and equal to zero for all other cases. As Case 4 rarely happens, in essence the dummy equals one if the ratio is between 0.5 and 2, i.e., in Case 1 the net CDS exposure is broadly equal to the credit exposure suggesting that effective hedging is taking place. We use the Hedging Ratio Dummy rather than a continuous variable, because banks may have different intentions when the Hedging Ratio falls between different thresholds, i.e., a negative number could indicate that the bank takes on a lot of risk of the firm and a positive number larger than 2 could be that the bank is speculating on the firm' default.

We first illustrate in Figure 7 the change in mean of the Hedging Ratio Dummy for dealer and non-dealer banks pairings with Bangs affected and unaffected bank-firm pairs. Contrary to dealer banks, the mean of the Hedging Ratio Dummy for non-dealer banks experiences a considerable increase after the Bangs, and the increase seems more immediate for 'Bangs affected' bank-firm pairs after the Bangs. This echoes the findings in Caglio, Darst, and Parolin [2019], who show that the percentage of firms' lenders switching from net sell to net buy positions of CDS contracts rose from 2011 to 2016.

In Table 6, we explain the quarterly-level Hedging Ratio Dummy with Bangs Affected Bank-Firm Pair, Non-Dealer Bank and the time dummies for the Bangs. In Columns 1 to 3, where Non-Dealer Bank is not included, Bangs affected bank-firm pairs are hedged more effectively after the Bangs, compared to those unaffected bank-firm pairs. However, the coefficient is only significant at the 10 percent level. In Columns 5 to 7, where we add the Non-Dealer Bank and all its interactions, it becomes obvious that the non-dealer banks more effectively hedge their credit exposures and this is especially the case after the Bangs. In sum, this confirms our cheaper-insurance-better-hedging hypothesis.

As robustness, we further distinguish the two possible scenarios in being 'not effectively hedged'. Namely, compared to 'effectively hedged', a bank can either be 'over-exposed' to a firm or be an 'empty creditor'. In the former case, the bank did not hedge or hedge enough of their credit exposure. In the latter case, the 'empty creditor', the bank has hedged more than needed as it might be speculating on the default of the firm.[30] In Appendix Table A.2, we construct a Hedging Ratio Indicator, which has three values. It equals 0 if the bank is effectively hedged on the firm (Hedging Ratio Dummy being one), -1 if the bank is over-exposed to the firm and +1 if the bank is an empty creditor. In Panels A and B of the table, we separately drop the observations in the 'empty creditor zone' and the 'over-exposed zone', and replicate the earlier regressions of Table 6. In Panel A where the Hedging Ratio Indicator could be either minus one or zero, the triple interaction term is positive and significant in all sample periods, indicating that non-dealer banks tend to hedge more effectively on a 'Bangs affected' CDS firm after the Bangs if they are over-exposed to this firm before. In Panel B, we drop the 'over-exposed zone' such that the Hedging Ratio Indicator could be either zero or one. Now the triple interaction term becomes negative and significant, also implying that non-dealer banks are converging to hedging their exposures to affected firms. To sum up, the two panels together show that non-dealers indeed converge to hedging more effectively, especially to those CDS firms more affected by the Bangs.

Finally, we are also interested in assessing the absolute impact on the hedging pursued by non-dealers. We therefore retain the subsample of non-dealers only and study the impact of the Bangs on both the Hedging Ratio Dummy and Indicator directly. The estimates are in Appendix Tables A.3 and A.4, respectively, and demonstrate that non-dealers effectively hedge after the Bangs (and not less effectively but merely more effectively than dealers).

[30] Cases 2, 5 and 7 could all be categorized into the 'over-exposed zone'. Cases 3 and 6 could be categorized into the 'empty creditor zone'.

F. CDS Trading in the Post-Bang Period

So far, we have tested the direct effects of the Bangs on banks' credit extension and their hedging behaviour. However, we still have not examined the impact of the Big or the Small Bang on CDS trading itself (even though not the main thrust of our paper, it is an auxiliary assessment informative to pursue). The Bangs conjured up contract and convention changes enhancing contract standardisation, and facilitated clearing and netting. So, we are expecting that the Bangs should first and foremost spur more CDS trading. We call this our "cheaper-trading-more-trading" hypothesis.

To investigate this impact, we make full use of the weekly CDS data, and explain the Log of Gross CDS with a bank-firm level treatment dummy and the Non-Dealer Bank variable using the CDS Sample. Gross CDS is the gross notional amount of CDS contracts a bank has on a firm in a week, and gross CDS contracts is the sum of all buy and sell contracts. Thus, it is potentially a good proxy for the CDS trading activity.

In Table 7 we present the results. In Columns 1 to 3, where we only have the variable Bangs Affected Bank-Firm Pair and a dummy for the quarters after the Bangs, it is quite obvious that 'Bangs affected' bank-firm pairs are traded more immediately after the Bangs (Columns 1 and 2), confirming our cheaper-trading-more-trading hypothesis. However, we expect that this effect is not necessarily permanent. Actually, after 2013, the shrinking of the Single-Name CDS market becomes more pronounced (e.g., Aldasoro, and Ehlers [2019]), as a result of tightened regulation (especially through Basel III). In addition, when we add the Non-Dealer Bank and interact it with Bangs Affected Bank-Firm Pair, the triple interaction only becomes significant from Column 6 onwards, which supports the argument that the non-dealer banks may not immediately trade more on 'Bangs affected' firms after the Bangs compared to dealer banks. This is reasonable as dealers engage in market making by nature and trade more CDS contracts than the non-dealers, and the Bangs should have positively affected the easiness of CDS trading for both of them. Once more, we check if this is also an absolute effect by re-estimating the impact for the subsample of non-dealers only. The estimates are in Appendix Table A.5 and confirm that after the Bangs non-dealers effectively trade more (and not merely more than dealers).

G. Does Hedging Intensity Predict Credit Exposure?

We have shown that credit extension by non-dealer banks is boosted after the Bangs for 'Bangs affected' firms or bank-firm pairs. This is achieved by a lowered insurance cost and could be verified by the findings that non-dealer banks hedged more effectively after the Bangs. As our last step, we would like to show whether credit extension could be predicted by banks' hedging their exposure even before the Bangs. That is, whether the banks who are inclined to hedge before the Bangs (with credit exposure at the same time) will lend more after the Bangs to those firms on which they have hedged, due to a lowered insurance cost, and whether this effect is more significant for the non-dealer banks. In essence, this is a variation of our "cheaper-insurance-more-credit" hypothesis.

In Table 8, we introduce the Hedging Ratio Dummy in 2009Q1, which is a time-invariant dummy equal to the Hedging Ratio Dummy for the bank-firm pair at the end of 2009Q1, as 2009Q1 is the last quarter before the Bangs on which we can calculate the quarterly Hedging Ratio. Notice that the Hedging Ratio Dummy in 2009Q1 equals one for only a very small proportion of the sample, as can be seen from the sample mean (0.001) shown in Table 2. However, the triple interaction terms between the Hedging Ratio Dummy, Non-Dealer Bank and the dummy after the Bangs are significant across all time periods, implying that banks, especially non-dealers, which have hedged effectively before the Bangs, indeed extend more credit after the Bangs to those firms they have hedged. We thus establish the link between hedging and credit extension, even without introducing firm or bank-firm pairs, which are affected by the Bangs.

H. Credit Extension of CDS Trading and Non-CDS Trading Banks after the Bangs

An interesting angle that we can examine is to compare the lending behaviour of CDS trading banks and banks that do not trade CDSs. From the previous discussion, it is clear that non-dealer CDS trading banks tend to lend relatively more than dealers after the Bangs, due to a lowered insurance cost. However, it is not clear whether these non-dealer CDS trading banks will lend more to the firms they can buy insurance on compared to other banks, which do not participate in the CDS market at all. The banks which choose not to trade in the CDS market may be small ones or be engaged heavily on traditional lending business, or may be lending mainly to local firms which are not traded as CDS reference entities.

To make the comparison between non-dealer CDS trading banks and non-CDS trading banks more meaningful, we match non-CDS trading banks to the non-dealer banks on the number of credit relationships. We count the number of firms each bank has on-balance-sheet credit exposure to by the end of 2007, and rank by the number of credit relationships. Since we can find 51 non-dealer banks in the credit register data, we match them to the top 204 (=51*4) non-CDS trading banks by their number of credit relationships. We consider all the bank-firm credit exposures these banks have (including non-dealer banks and non-CDS banks) and explain the log of credit exposure with Bangs Affected Firms, the dummy for periods after the Bangs, and a dummy indicating whether the bank is a non-dealer bank.

The results are shown in Appendix Table A.6. Due to lending contraction after the Bangs, 'Bangs affected' firms get less credit after the Bangs, as already indicated in Table 5. However, from Columns 6 to 8, we find that they receive relatively more credit from non-dealer banks rather than from non-CDS trading banks; this is evidence for the non-dealer banks benefiting from lowered insurance cost and therefore lending more to those CDS firms.

We also do similar placebo tests as we did for the previous tables, i.e., we run similar regressions for the sample period from 2014Q1 to 2016Q2. These placebo tests are shown in Columns 4 and 9. We find that the non-dealer banks recuperated to some extent their "initial losses" and gained a "lasting advantage", and that the effect of Bangs even lasts during the placebo period. To further test whether non-dealer banks and non-CDS-trading banks are different at the beginning, or the difference only occurs after the Bangs, we additionally do a second placebo test. We study the sample in 2008 and use Q1 and Q2 of 2008 as the fake "Pre-Bangs" period, and Q3 and Q4 of 2008 as the fake "Post-Bangs" period. The results in Column 10 show that, non-dealer banks and non-CDS-trading banks are not different in lending to CDS reference entities before the Bangs, and that the difference between them only appears after the Bangs.

V. Conclusions

We couple comprehensive bank-firm level CDS trading data from the Depository Trust and Clearing Corporation to the German credit register containing bilateral bank-firm credit exposures. We mainly focus on the differential impact of the Big Bang and the Small Bang across non-dealer versus dealer banks. We find that after the Bangs, the cost of buying CDS contracts becomes cheaper for non-dealer banks, and that – because of the decreased insurance cost of default – these banks extend more credit to Bangs affected firms.

Because our estimates imply that following the Bangs cheaper and more effective hedging through CDSs and subsequent lending took place, and that only banks that have hedged before lend more, we provide first-hand evidence on the benefits of financial innovation for risk mitigation. Hence, policies that foster financial innovation and spur the usage of credit default swaps are not necessarily associated with more moral hazardous bank risk-taking, but rather with more risk mitigation.

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Figure 1

Timeline

The figure displays the timeline of our study. For the Small Bang the convention changes (e.g., adopt fixed coupon and upfront fee) began on June 20, 2009 and contract changes (hardwiring auction, credit determination committee, restructuring clause changes etc.) happened later: In particular "the adherence Period for the Small Bang Protocol opens on July 14, 2009 and closes on July 24, 2009 at 5pm NY time" (Source: ISDA). We take the later date to demarcate the start of the impact periods for the Small Bang.

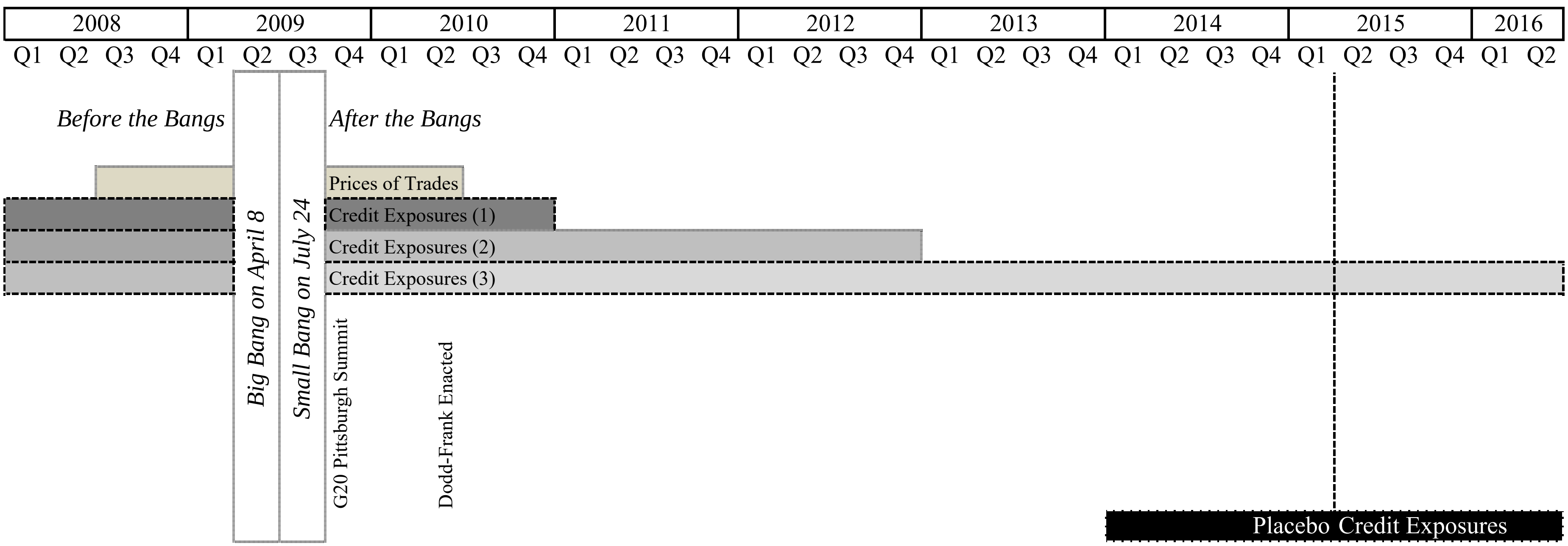


Figure 2
Samples, hypotheses and main findings of the paper

Samples	Hypotheses	Explaining	After Bangs		Tables
CDS Transaction Data	Cheaper Insurance	CDS Spreads	for non-dealers	↓	3B
Credit Exposure Sample	Cheaper Insurance More Credit	Log (Bank-Firm Exposure)	by non-dealers to Bangs affected firms or when hedged	↑	4, 5, 8
CDS Sample	Cheaper Insurance Better Hedging	Hedging Ratio Dummy	by non-dealers of Bangs affected firms	↑	6, A.2-A.4
CDS Sample	Cheaper Trading More Trading	Log (Gross CDS)	of Bangs affected firms	↑	7, A.5

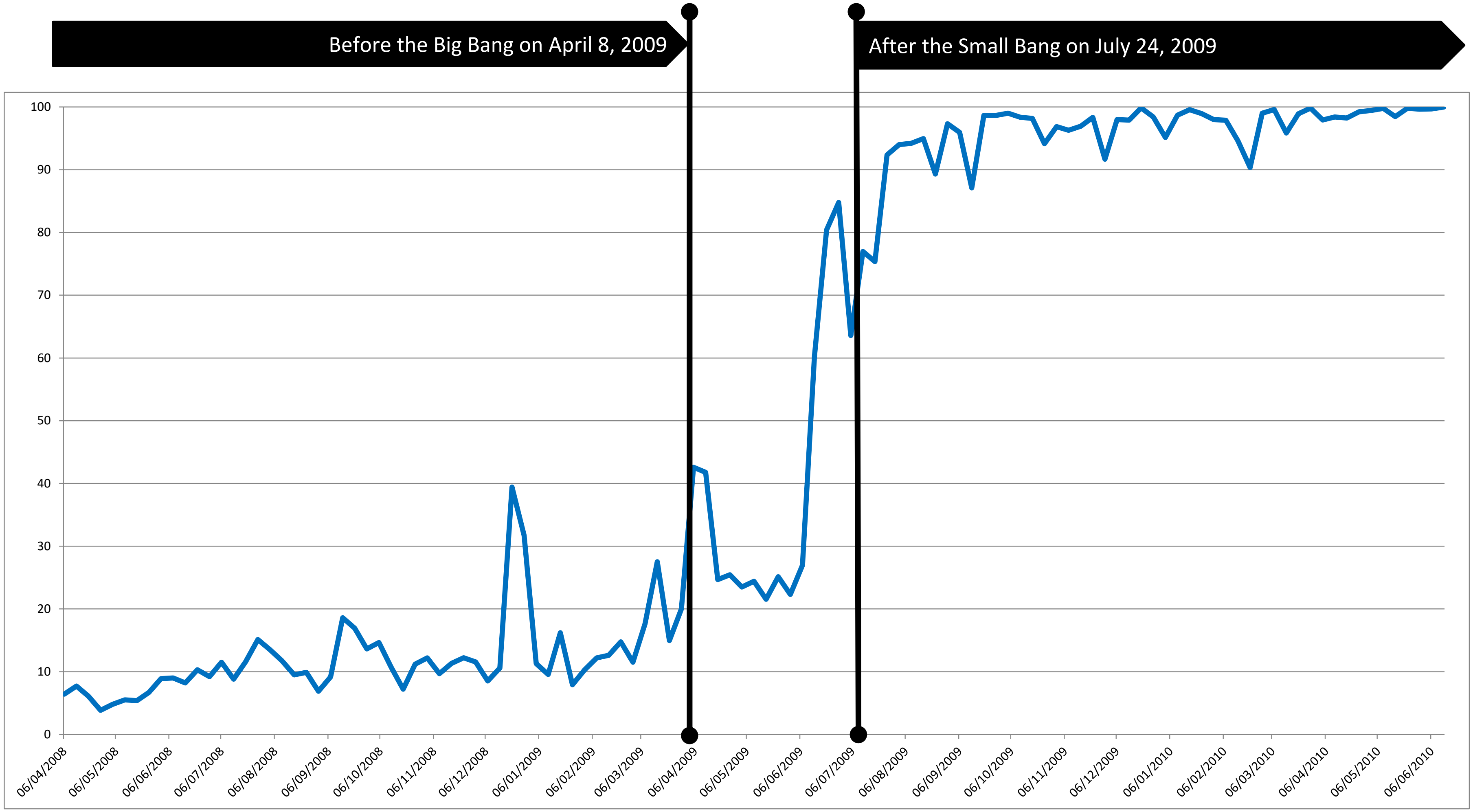


Figure 3

Percentage of CDS transactions which adopt standard coupon rate

The figure displays the percentage of CDS new transactions which adopt standard coupon rate (25/100/500/1,000 bps).

Figure 4
Filtering of samples

CDS Sample

German financial institutions trade CDS on all the firms **+** German reference entities traded by all the financial institutions

⇩

Select only financial institutions which need to report their credit exposure to regulator

⇩

98 financial institutions (mainly banks) which are in both the CDS Position and Credit Exposure data sets, and all reference entities they trade CDS on, which amounts to 2,384 firms

Credit Exposure Sample

All the German financial institutions which need to report credit exposure to regulator, as well as German branches of foreign financial institutions

⇩

Select CDS trading banks

⇩

95 financial institutions (mainly banks) which are both in the CDS Position and Credit Exposure data sets, and all the firms they extend credit to (including firms without traded CDS)

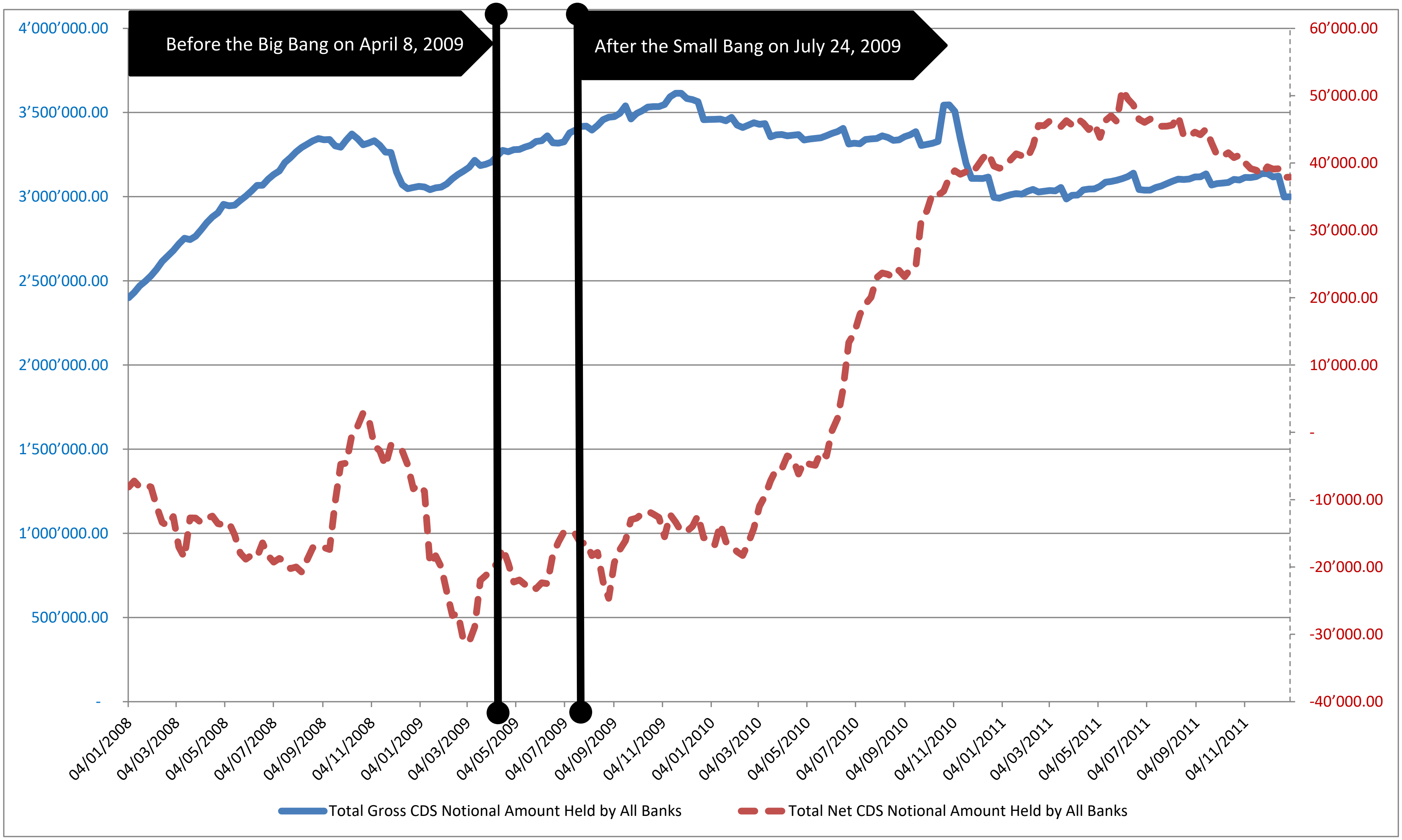
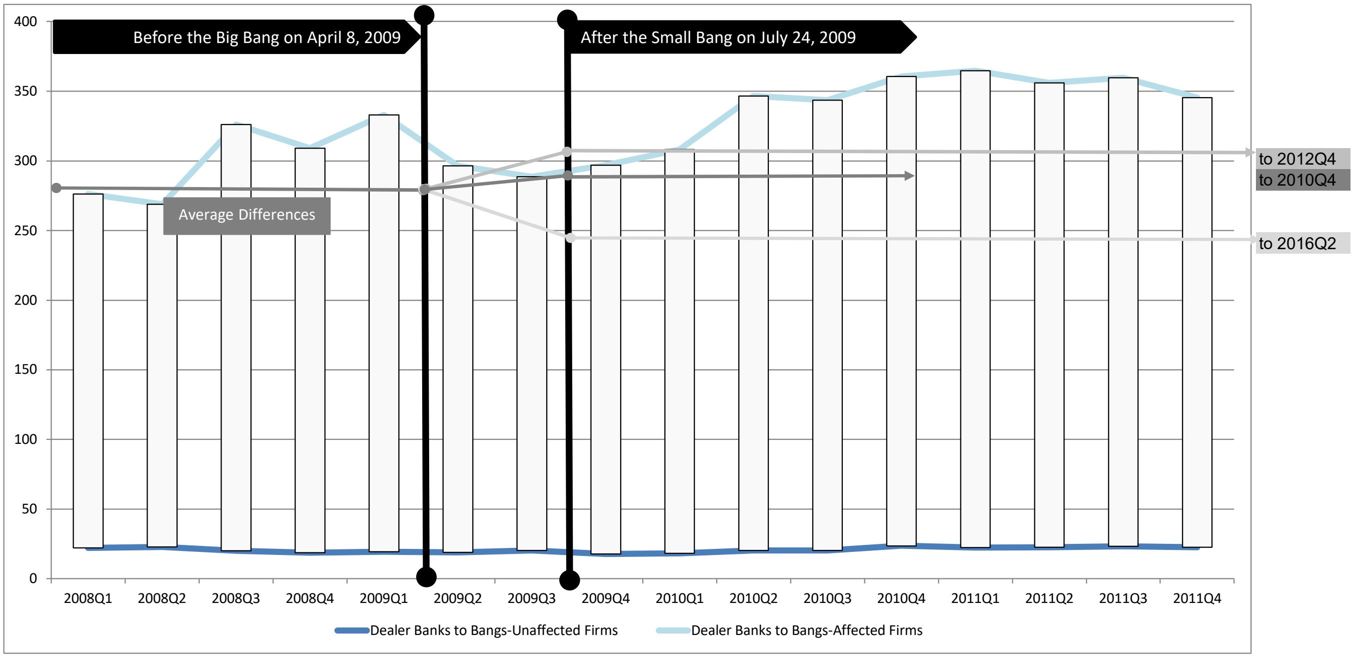


Figure 5
Total gross and net CDS notional amounts
The figure displays the total gross and net CDS notional amount held by all the banks (in Million Euros).



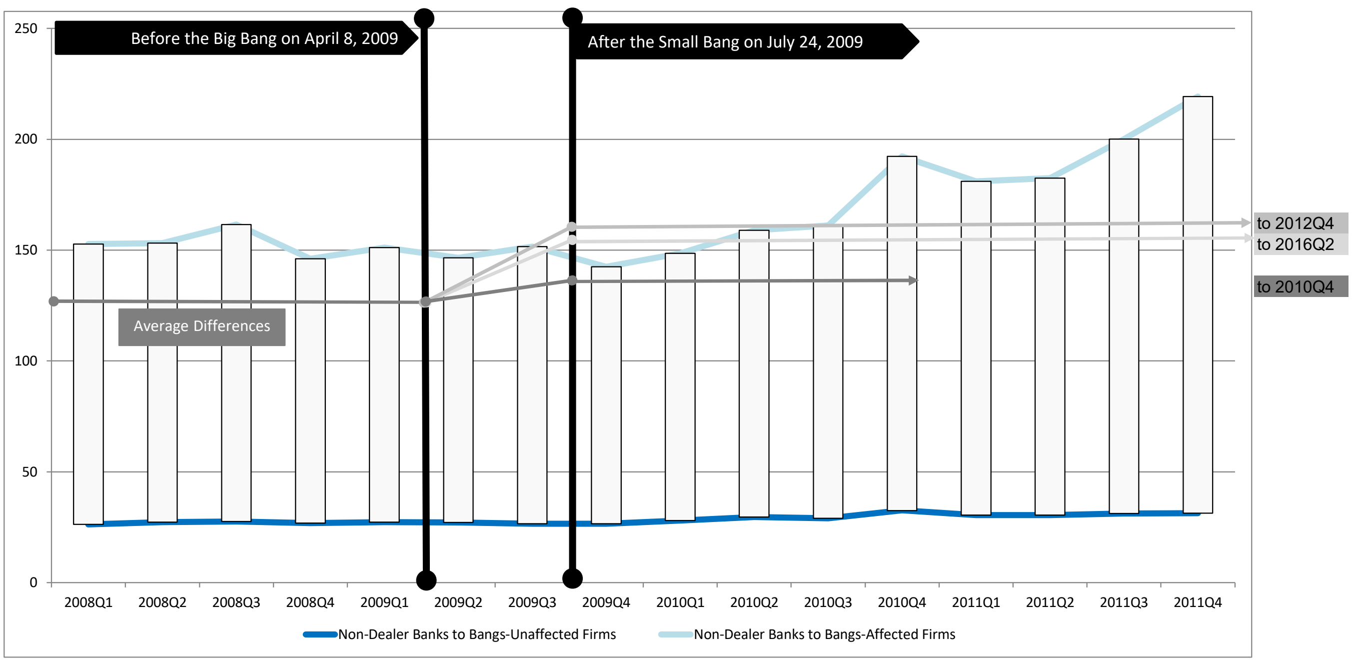


Figure 6
Credit exposures
The figure displays the mean outstanding credit exposures (in Million Euros) for dealer and non-dealer banks granting credit to Bangs-affected and -unaffected firms. The vertical bars represent the quarterly differences between affected and unaffected firms, while the gray horizontal lines represent the average difference between affected and unaffected firms for the periods 2008Q1-2009Q1 and the three impact periods 2009Q4-2010Q4, 2009Q4-2012Q4 and 2009Q4-2016Q2, respectively, with the shades of gray corresponding to those introduced in Figure 1.

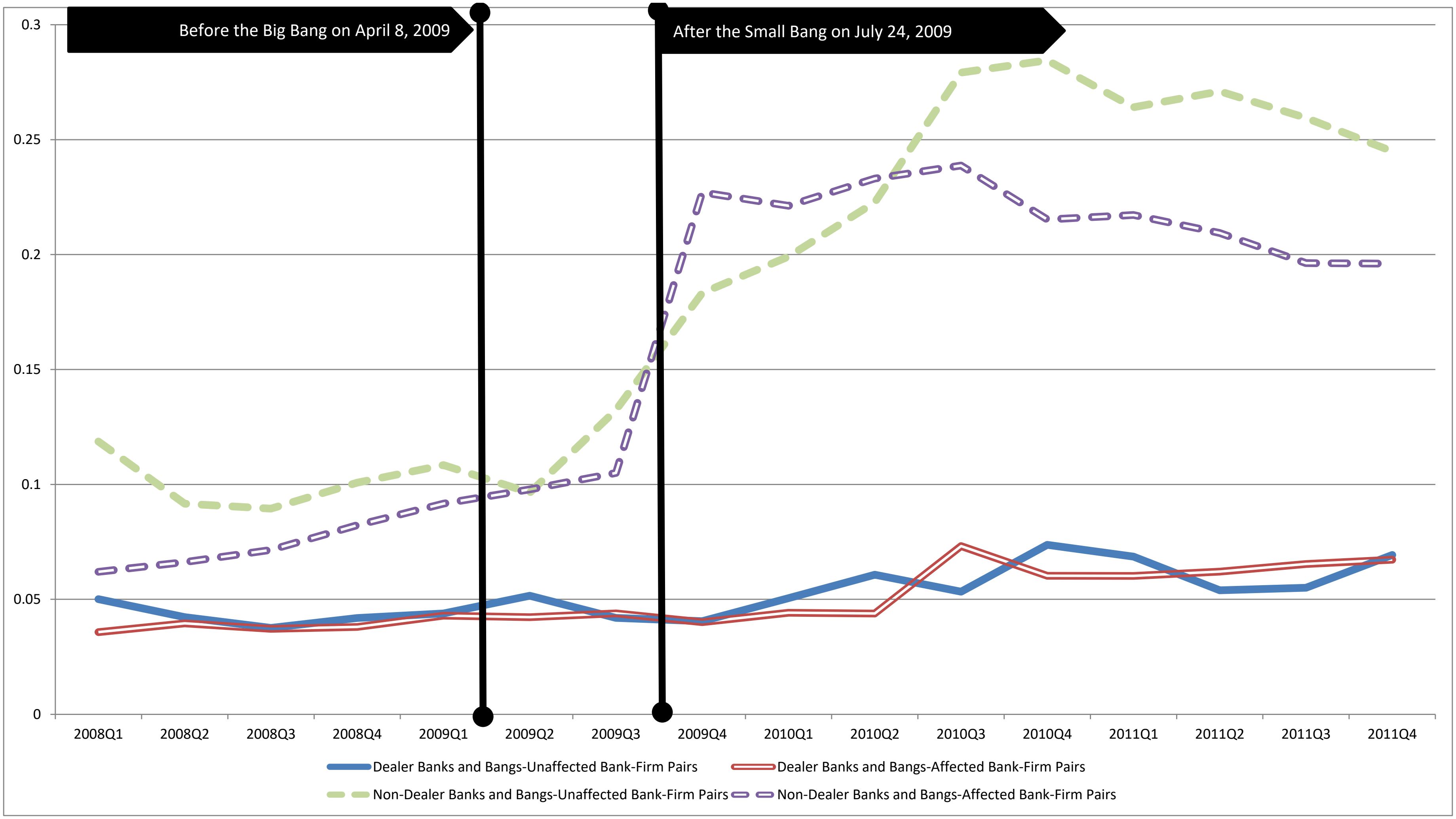


Figure 7
Hedging ratio dummies
The figure displays the mean of Hedging Ratio Dummy for dealer and non-dealer banks pairings with Bangs-affected and -unaffected bank-firm pairs.

Table 1
Sample statistics

Period	2008Q1-2016Q2	2008Q1-2009Q1	2009Q4-2016Q2
Credit Exposures by CDS Trading Banks			
Number of Banks	95	73	93
of which, the number of Dealers	22	20	21
of which, the number of Non-Dealers	73	53	72
Number of Firms	202,404	101,236	179,068
of which, **CDS** firms (with CDS traded on Before the Bangs)	1,389	1,148	1,289
-of which, Small Bang affected firms	485	428	458
-of which, Big Bang affected firms	677	529	626
-In total: Bangs affected firms	1,162	957	1,084
Number of Bank-Firm Pairs	293,651	143,469	256,407
of which, Dealer-Firm pairs	146,962	62,413	128,432
of which, Non-Dealer-Firm pairs	146,689	81,056	127,975
of which, Pairs that include Bangs affected firms	8,046	5,140	6,975
- of which, Pairs with buy or sell (or both) CDS contracts before the Bangs	2,694	1,823	2,415
Average credit exposure (in Million €)	31.528	30.720	31.789
CDS Positions by CDS Trading Banks			
Number of Banks	98	68	96
of which, the number of Dealers	22	21	21
of which, the number of Non-Dealers	76	47	75
Number of Firms	2,384	1,944	2,260
-of which, Small Bang affected firms	700	519	685
-of which, Big Bang affected firms	950	851	882
-In total: Bangs affected firms	1,650	1,370	1,567
Number of Bank-Firm Pairs	11,043	8,144	10,130
of which, Dealer-Firm pairs	5,787	4,732	5,425
of which, Non-Dealer-Firm pairs	5,256	3,412	4,705
of which, Pairs that include Bangs affected firms	8,748	6,401	8,065
- of which, Pairs with buy or sell (or both) CDS contracts before the Bangs	5,876	5,744	5,689
Average CDS gross notional amount of contracts (in Million €)	423.825	433.315	418.954

Notes. The sample Credit Exposures by CDS Trading Banks includes CDS trading banks that are present in the CDS Position sample with all their firm exposures. The sample CDS Positions by CDS Trading Banks includes the CDS positions of CDS trading banks and all the CDS firms they trade on.

Table 2
Variable definitions and summary statistics

Variable	Definition	Unit	Sample	Number of Observations	Mean	Standard Deviation	1%	99%
Log (Bank-Firm Exposure)	The log of the euro amount of credit exposure the bank has to the firm by the end of each quarter.	-	Credit Exposure	3,439,404	14.767	2.359	6.909	19.618
Non-Dealer Bank	A dummy equal to one if the bank is a non-dealer bank, and zero if the bank is a dealer bank.	0/1	Credit Exposure	3,439,404	0.564	0.496	0	1
			CDS	2,617,957	0.356	0.479	0	1
Bangs Affected Firm	A firm-level dummy that is equal to one if the firm has CDS traded on before the Big Bang, and the contract on this firm will be affected by the Big Bang or Small Bang contract changes, and equal to zero in all other cases.	0/1	Credit Exposure	3,439,404	0.030	0.171	0	1
			CDS	2,617,957	0.880	0.326	0	1
Bangs Affected Bank-Firm Pair	A bank-firm level dummy equal to one if the bank has buy or sell (or both) CDS contracts on the firm in the week before the Big Bang (Small Bang) and the firm is a Big Bang (Small Bang) affected firm.	0/1	Credit Exposure	3,439,404	0.011	0.104	0	1
			CDS	2,617,957	0.738	0.440	0	1
Hedging Ratio Dummy	A time-varying dummy equal to one if the Hedging Ratio by the end of each quarter, which is defined as the net notional amount of CDS on a firm held by a bank to the credit exposure the bank has to this firm, is between 0.5 and 2 (including the case when the bank has zero credit exposure to the firm and CDS net notional amount on the firm is intentionally maintained at zero (but with positive buy and sell contracts)), and equals to zero in all other cases.	0/1	CDS	197,314	0.090	0.286	0	1
Hedging Ratio Dummy on 2009Q1	A time-invariant dummy equal to one if the Hedging Ratio Dummy, as defined above, equals to one by the end of 2009Q1, and equals to zero otherwise.	0/1	Credit Exposure	3,439,404	0.001	0.038	0	0
Log (Gross CDS)	The log of the notional amount of gross CDS contracts on each individual firm held by each individual bank. Gross CDS contracts is the sum of buy contracts and sell contracts.	-	CDS	2,617,957	17.852	2.156	13.592	22.297
Small Bang Affected Bank-Firm Pair	A firm-level dummy equal to one if the bank has buy or sell (or both) CDS contract on the firm in the week before the Small Bang and the firm is a Small Bang affected firm, equals to zero in all other cases.	0/1	CDS	148,294	0.512	0.500	0	1
Big Bang Affected Bank-Firm Pair	A firm-level dummy equal to one if the bank has buy or sell (or both) CDS contract on the firm in the week before the Big Bang and the firm is a Big Bang affected firm, equals to zero in all other cases.	0/1	CDS	147,331	0.353	0.478	0	1
Hedging Ratio Indicator	A time-varying indicator with three values. It equals to zero if the bank is effectively hedged, that is the Hedging Ratio by the end of each quarter, which is defined as the net notional amount of CDS on a firm held by a bank to the credit exposure the bank has to this firm, is between 0.5 and 2 (including the case when bank has zero credit exposure to the firm and CDS net notional amount on the firm is intentionally maintained at zero (but with positive buy and sell contracts)); and the indicator equals to -1 if the bank is over-exposed to the firm, which includes (1) Hedging Ratio <0.5 and unequal to zero, (2) the net notional amount of CDS contracts is zero, but the bank has positive credit exposure to the firm, (3) the net notional amount of CDS contracts is negative, and the bank has no credit exposure to the firm ; and the indicator equals to 1 if the bank is an empty creditor, which includes (1) Hedging Ratio >2, (2) the bank has no credit exposure to the firm, but with positive net notional amount of CDS.	-1/0/1	CDS	197,314	-0.101	0.949	-1	1

Notes. The table reports the variable names, definitions and summary statistics of all dependent and independent variables. The number of observations and other summary statistics are calculated based on the sample where the variable is used.

Table 3

Transaction price analysis

Panel A: Summary Statistics

	Number of Observations	Mean of Absolute Spread	Mean of Relative Spread
Total number of transactions within one year before and after the Bangs	702,916		
of which, dealers buy CDS from dealers	607,690		
of which, non-dealers buy CDS from dealers	57,984		
Matching-Step1			
Trade 1: "non-dealer buys CDS from dealer" before the Bangs	6,969	5.108	0.009
Trade 3 : "non-dealer buys CDS from dealer" after the Bangs	2,270	7.893	-0.221
Matching-Step2: Match spreads before and after the Bangs			
Method 1: same buyer-seller and entity			
Matched pairwise combination of after- and before-Bangs spreads	1,404		
number of unique matched "non-dealer buys CDS from dealer" trades **before** the Bangs	546	-0.578	0.010
number of unique matched "non-dealer buys CDS from dealers" trades **after** the Bangs	401	-22.785	-0.175
Method 2: Same entity			
Matched pairwise combination of after- and before-Bang spreads	56,547		
number of unique matched "non-dealers buy CDS from dealers" trades **before** the Bangs	5,241	52.923	0.012
number of unique matched "non-dealers buy CDS from dealers" trades **after** the Bangs	2,132	8.894	-0.213

Table 3
Transaction price analysis

Panel B: Explaining the differences between the after- and before-Bangs spreads

Loadings on the constant of the regression explaining the difference between the after- and before-Bangs spreads

		(1)	(2)	(3)	(4)
Matching Method	*H: Cheaper Insurance*	*Nr. 1*	*Nr. 2*	*Nr. 2*	*Nr. 2*
Matching by requiring the before- and after-Bangs spreads to have		*Same buyer-seller and entity*	*Same entity*	*Same entity*	*Same entity*
Sample		*Full*	*Full*	*Standard 5-Year Contracts*	*Other Contracts*
Absolute Spread	**< 0**	-20.762**	-16.905***	-14.539***	-69.244***
		[8.166]	[4.390]	[2.321]	[10.134]
Relative Spread	**< 0**	-0.158***	-0.130***	-0.031***	-0.861***
		[0.034]	[0.007]	[0.003]	[0.072]
Observations		1,404	56,547	41,708	4,362

Notes. In Panel A the first step of matching involves matching 'non-dealer buys CDS from dealer' trades to 'dealer buys CDS from dealer' trades on seller and reference entity (including maturity, seniority, restructuring code, trading volume and trading date). Trade 1 refers to the matched 'non-dealer buys CDS from dealer' trades within one year before the Big (Small) Bang for Big (Small) Bang affected firms. Trade 3 refers to the matched 'non-dealer buys CDS from dealer' trades within one year after the Big (Small) Bang for Big (Small) Bang affected firms. In the second step of matching, Method 1 is a rigorous matching of after- and before-Bangs spreads on the same non-dealer buyer Y and seller X, as well as the same reference entity. Method 2 is a less rigorous matching of after- and before-Bangs spreads on the same reference entity only. In Panel B, we report the regression results using the aforementioned two methods of matching, separately, basing on the observations as obtained in Panel A. The reported loadings are the coefficients on regressions that regress the differences in after- and before-Bangs spreads on a constant. The sample and matching method used in each regression is indicated in each column heading. In Column 3 and Column 4, we constrain both the after- and before-Bangs spreads to spreads on the most liquid standard contracts (Column 3, 5-year senior unsecured contracts) and other contracts (Column 4), separately.

Table 4
Explaining credit exposure with firm-level treatment variable

	H: Cheaper Insurance More Credit	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1	2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2	2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*	*Period 1*	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Log (Bank-Firm Exposure)							
Bangs Affected Firm * 1(>= 2009Q2) * Non-Dealer Bank	**> 0**	0.467***	0.516***	0.535***		0.169*	0.208**	0.283***	
		[0.078]	[0.081]	[0.080]		[0.093]	[0.097]	[0.095]	
Bangs Affected Firm * 1(>= 2015Q2) * Non-Dealer Bank	**= 0**				-0.071				0.040
					[0.077]				[0.094]
Constant		14.931***	14.871***	14.746***	14.564***	15.678***	15.629***	15.543***	15.424***
		[0.005]	[0.005]	[0.005]	[0.006]	[0.006]	[0.007]	[0.007]	[0.007]
Bank Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Type * Quarter FE		Yes	Yes	Yes	Yes	No	No	No	No
Bank Type * Firm Type FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm * Quarter FE		No	No	No	No	Yes	Yes	Yes	Yes
Observations		1,033,807	1,791,947	3,229,539	1,077,197	409,468	688,337	1,203,665	388,815
R-squared		0.081	0.080	0.080	0.070	0.611	0.606	0.599	0.584

Notes. The table reports estimates from ordinary least squares regressions on the Credit Exposure Sample, and the dependent variable is the log of Credit Exposure at bank-firm level. Bangs Affected Firm is a firm-level dummy that is equal to one if the firm has CDS traded on before the Big Bang, and the contract on this firm will be affected by the Big Bang or Small Bang contract changes. 1(>= 2009Q2) is a dummy equal to one for all quarters after 2009Q1, and 1(>= 2015Q2) is a dummy equal to one for all quarters after 2015Q1. Table 2 contains all definitions and the summary statistics for each included variable. All specifications refer to the intensive margin (restricted to the sample with positive bank-firm exposure) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for dealer banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.

Table 5

Explaining credit exposure with bank-firm level treatment variable

	H: Cheaper Insurance More Credit	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1	2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2	2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*	*Period 1*	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Log (Bank-Firm Exposure)							
Bangs Affected Bank-Firm Pair		0.040	0.088	0.118	-0.219**	-0.090	0.073	0.153	-0.385*
		[0.071]	[0.072]	[0.072]	[0.105]	[0.139]	[0.145]	[0.142]	[0.198]
Bangs Affected Bank-Firm Pair * 1(>= 2009Q2)		-0.315***	-0.369***	-0.371***		-0.559***	-0.557***	-0.555***	
		[0.064]	[0.068]	[0.071]		[0.095]	[0.097]	[0.098]	
Bangs Affected Bank-Firm Pair * Non-Dealer Bank						0.115	-0.068	-0.159	0.256
						[0.160]	[0.167]	[0.165]	[0.227]
Bangs Affected Bank-Firm Pair * 1(>= 2009Q2) * Non-Dealer Bank	**> 0**					0.527***	0.413***	0.414***	
						[0.111]	[0.117]	[0.120]	
Bangs Affected Bank-Firm Pair * 1(>= 2015Q2)					0.066				0.063
					[0.068]				[0.090]
Bangs Affected Bank-Firm Pair * 1(>= 2015Q2) * Non-Dealer Bank	**= 0**								0.017
									[0.114]
Constant		14.938***	14.881***	14.757***	14.565***	14.939***	14.882***	14.757***	14.566***
		[0.005]	[0.005]	[0.005]	[0.006]	[0.005]	[0.005]	[0.005]	[0.006]
Bank Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Type * Firm Type FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		1,033,807	1,791,947	3,229,539	1,077,197	1,033,807	1,791,947	3,229,539	1,077,197
R-squared		0.081	0.080	0.080	0.070	0.081	0.080	0.080	0.070

Notes. The table reports estimates from ordinary least squares regressions on the Credit Exposure Sample, and the dependent variable is the log of Credit Exposure at bank-firm level. Bangs Affected Bank-Firm Pair is a bank-firm level dummy equal to one if the bank has buy or sell (or both) CDS contract on the firm in the week before the Big Bang (Small Bang) and the firm is a Big Bang (Small Bang) affected firm. 1(>= 2009Q2) is a dummy equal to one for all quarters after 2009Q1, and 1(>= 2015Q2) is a dummy equal to one for all quarters after 2015Q1. Table 2 contains all definitions and the summary statistics for each included variable. All specifications refer to the intensive margin (restricted to the sample with positive bank-firm exposure) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for dealer banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.

Table 6
Explaining time-varying Hedging Ratio Dummy

	H: Cheaper Insurance Better Hedging	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1	2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2	2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*	*Period 1*	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Hedging Ratio Dummy							
Bangs Affected Bank-Firm Pair		0.002	0.002	0.003	0.023**	-0.025*	-0.026*	-0.009	-0.006
		[0.011]	[0.011]	[0.011]	[0.011]	[0.014]	[0.015]	[0.014]	[0.013]
Bangs Affected Bank-Firm Pair * 1(>= 2009Q2)		0.029*	0.025*	0.025*		-0.027	-0.009	-0.006	
		[0.016]	[0.015]	[0.013]		[0.018]	[0.017]	[0.015]	
Bangs Affected Bank-Firm Pair * Non-Dealer Bank						0.046**	0.048**	0.019	0.060***
						[0.019]	[0.020]	[0.019]	[0.021]
Bangs Affected Bank-Firm Pair * 1(>= 2009Q2) * Non-Dealer Bank	> 0					0.089***	0.062***	0.062***	
						[0.021]	[0.021]	[0.019]	
Bangs Affected Bank-Firm Pair * 1(>= 2015Q2)					0.011				0.029***
					[0.009]				[0.011]
Bangs Affected Bank-Firm Pair * 1(>= 2015Q2) * Non-Dealer Bank	= 0								-0.038**
									[0.017]
Constant		0.072***	0.081***	0.074***	0.059***	0.091***	0.097***	0.086***	0.065***
		[0.007]	[0.006]	[0.005]	[0.006]	[0.007]	[0.006]	[0.005]	[0.006]
Bank Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Type * Firm Type FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		63,352	105,281	165,027	42,374	63,352	105,281	165,027	42,374
R-squared		0.067	0.057	0.045	0.013	0.070	0.060	0.047	0.014

Notes. The table reports estimates from ordinary least squares regressions on the CDS Sample (quarterly level data), and the dependent variable is the time-varying Hedging Ratio Dummy. We took the snapshot of the original CDS sample at each quarter end and calculate the Hedging Ratio Dummy as equal to one if the Hedging Ratio, which is defined as the net notional amount of CDS on a firm held by a bank to the credit exposure the bank has to this firm, is between 0.5 and 2 (including the case when bank has zero credit exposure to the firm and CDS net notional amount on the firm is maintained at zero (but with positive buy and sell contracts)), and equals to zero in all other cases. 1(>= 2009Q2) is a dummy equal to one for all quarters after 2009Q1, and 1(>= 2015Q2) is a dummy equal to one for all quarters after 2015Q1. Table 2 contains all definitions and the summary statistics for each included variable. All specifications refer to the intensive margin (restricted to the sample with positive buy or sell (or both) CDS contracts) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for dealer banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.

Table 7
Explaining log of gross notional amount of CDS contracts

	H: Cheaper Trading More Trading	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1	2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2	2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*	*Period 1*	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Log (Gross CDS)							
Bangs affected Bank-Firm Pair		1.270***	1.273***	1.278***	0.829***	2.010***	1.914***	1.872***	0.917***
		[0.081]	[0.081]	[0.081]	[0.071]	[0.125]	[0.119]	[0.117]	[0.122]
Bangs affected Bank-Firm Pair * 1(>= 2009Q2)	> 0	0.313***	0.186**	-0.153*		0.449***	0.051	-0.486***	
		[0.094]	[0.092]	[0.092]		[0.121]	[0.121]	[0.120]	
Bangs affected Bank-Firm Pair * Non-Dealer Bank						-1.248***	-1.089***	-1.019***	-0.177
						[0.125]	[0.119]	[0.116]	[0.138]
Bangs affected Bank-Firm Pair * 1(>= 2009Q2) * Non-Dealer Bank	> 0					-0.116	0.230**	0.553***	
						[0.094]	[0.097]	[0.099]	
Bangs affected Bank-Firm Pair * 1(>= 2015Q2)	= 0				-0.136***				-0.166**
					[0.051]				[0.082]
Bangs affected Bank-Firm Pair * 1(>= 2015Q2) * Non-Dealer Bank	= 0								0.065
									[0.091]
Constant		16.802***	16.875***	17.011***	17.132***	16.537***	16.706***	16.910***	17.108***
		[0.045]	[0.040]	[0.037]	[0.048]	[0.053]	[0.049]	[0.048]	[0.060]
Bank Type * Week FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Type * Week FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Type * Firm Type FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		835,328	1,390,417	2,192,635	571,172	835,328	1,390,417	2,192,635	571,172
R-squared		0.187	0.201	0.195	0.175	0.193	0.204	0.196	0.175

Notes. The table reports estimates from ordinary least squares regressions on the CDS Sample (weekly level data), and the dependent variable is the log of gross notional amount of CDS contracts. 1(>= 2009Q2) is a dummy equal to one for all quarters after 2009Q1, and 1(>= 2015Q2) is a dummy equal to one for all quarters after 2015Q1. Table 2 contains all definitions and the summary statistics for each included variable. All specifications refer to the intensive margin (restricted to the sample with positive buy or sell (or both) CDS contracts) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for dealer banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.

Table 8
Explaining credit exposure with Hedging Ratio Dummy at 2009Q1

	H: Cheaper Insurance More Credit	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1	2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2	2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*	*Period 1*	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Log (Bank-Firm Exposure)							
Hedging Ratio Dummy at 2009Q1		0.611***	0.617***	0.626***	0.424*	1.334***	1.401***	1.445***	1.064***
		[0.100]	[0.101]	[0.102]	[0.241]	[0.149]	[0.150]	[0.148]	[0.364]
Hedging Ratio Dummy at 2009Q1 * 1(>= 2009Q2)		-0.386***	-0.503***	-0.428***		-0.794***	-0.832***	-0.700***	
		[0.129]	[0.140]	[0.154]		[0.228]	[0.228]	[0.235]	
Hedging Ratio Dummy at 2009Q1 * Non-Dealer Bank						-1.408***	-1.527***	-1.597***	-1.448***
						[0.180]	[0.182]	[0.180]	[0.440]
Hedging Ratio Dummy at 2009Q1 * 1(>= 2009Q2) * Non-Dealer Bank	> 0					0.833***	0.665**	0.480*	
						[0.259]	[0.273]	[0.290]	
Hedging Ratio Dummy at 2009Q1 * 1(>= 2015Q2)					-0.073				-0.131
					[0.160]				[0.228]
Hedging Ratio Dummy at 2009Q1 * 1(>= 2015Q2) * Non-Dealer Bank	= 0								0.076
									[0.308]
Constant		14.936***	14.879***	14.754***	14.563***	14.936***	14.879***	14.754***	14.563***
		[0.005]	[0.005]	[0.005]	[0.006]	[0.005]	[0.005]	[0.005]	[0.006]
Bank Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Type * Firm Type FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		1,033,807	1,791,947	3,229,539	1,077,197	1,033,807	1,791,947	3,229,539	1,077,197
R-squared		0.081	0.080	0.080	0.070	0.081	0.080	0.080	0.071

Notes. The table reports estimates from ordinary least squares regressions on the Credit Exposure Sample, and the dependent variable is the log of Credit Exposure at bank-firm level. Hedging Ratio Dummy at 2009Q1 is a time-invariant dummy equal to one if the Hedging Ratio Dummy equals to one by the end of 2009Q1, and equals to zero otherwise. Table 2 contains all definitions and the summary statistics for each included variable. All specifications refer to the intensive margin (restricted to the sample with positive bank-firm exposure) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for dealer banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.

Internet Appendix

CDS and Credit:
After the Bangs
Cheaper Credit Insurance, More Lending and Hedging

Appendix: Exogeneity of the Small Bang

One concern one may have about the Bangs is that, the Bangs, in particular the Small Bang which follows the Big Bang, are not exogenous. As one of the major changes implemented after the financial crisis aiming at reducing systematic risks, the Big and the Small Bang could have contributed to improving CDS trading together with other changes that happened later. It is also reasonable to think that as a natural extension of the Big Bang, people were expecting the Small Bang before it was announced; thus, the CDSs of Small Bang firms are traded more even earlier.

To examine whether these possibilities were present or not, we focus on the time period around the Bangs. As discussed in Section 2, the changes brought by the Big and the Small Bang took effect on April 8 and June 20, 2009, respectively. Since our weekly CDS position data have the reporting day always on Fridays, there are eleven weeks (reporting Fridays) between April 8 and June 20.[i] To be able to answer whether indeed there is increased CDS trading immediately after the Big Bang for 'Big Bang affected' firms, but not for 'Small Bang affected' firms, we separately take eleven weeks before the Big Bang and eleven weeks after the Big Bang, such that the last week ends right before the Small Bang. We explain the Log of Gross CDS on this 22-week subsample, using the dummy of the Big Bang Affected Bank-Firm Pair, which equals to one if the bank buys or sells (or both) CDS contracts on the firm in the week before the Big Bang and the firm is affected by the Big Bang, and equals to zero in all other cases.

The results are shown in Table A.1 Panel A. Big Bang affected pairs are traded more after April 8, in contrast to the control group, which mainly consists of the 'Small Bang affected' pairs. This indicates that the influence of the Small Bang did not take place earlier, and the Small Bang can still be interpretable as unanticipated and somewhat exogenous.

In Panels B and C, we restrict the sample to the period around the Small Bang, and take eleven weeks before the Small Bang and eleven weeks after the Small Bang, such that the sample starts after the Big Bang. We again explain the log of Gross CDS with the dummy of Small Bang Affected Bank-Firm Pair, which is one if the firm is a 'Small Bang affected' firm and the bank has CDS contracts on the firm before the Small Bang. It might not be surprising that the interaction between Small Bang Affected Bank-Firm Pair and the dummy for the weeks after June 20, 2009 is not significant. This could be evidence that the effect of Big Bang still continues after the Big Bang, such that we cannot distinguish the effect on the Big Bang firms and the Small Bang firms around the Small Bang.

This intuition is confirmed in Panel C, where we restrict the sample to the same period as in Panel B and look at the change in CDS trading on Small Bang Firms and Big Bang Firms separately. The results in Panels B and C both lend support to our model design, i.e., our pooling of the treatment effect of the Big Bang and Small Bang together, by removing the second and third quarter of 2009 from the baseline sample, in order to have a better contrast between the pre- and post-periods on the pooled treatment.

[i] June 19, 2009 is a Friday, and the first trading day after the Small Bang is actually June 22.

Appendix: Relevant Elements of the Big and Small Bang

a. General Setting

The Big and Small Bang Protocols in this context refer to North American and European dealers making a commitment to regulators to begin clearing index and single name CDS trades through central counterparties. These protocols entailed a series of standardization of CDS trades, accompanied by convention and contract changes that occurred in North American and European CDS markets (Figure 1 contains a timeline of the changes and the sample periods used in our analysis; Figure 2 summarizes the samples, hypotheses, and main findings of our paper).

The Big Bang, which took place on April 8, 2009, primarily entailed global contract and trading convention changes for North American CDSs. Implemented on June 20, 2009, as a follow-up, the Small Bang additionally entailed contract changes related to restructuring, alongside separate convention changes to the European corporate CDS market and Western European Sovereign CDS trades (Markit [2009b]).

The changes to promote greater standardization of contracts were expected to improve the ability of central clearing parties to conduct daily hedging operations and reduce systematic counterparty risk, as well as benefit trade compression and processing. Under these Big and Small Bang Protocols, the contract and convention changes were not explicitly required for central clearing of CDS trades, but they quickly became a market standard for both cleared and non-cleared trades. While the mandatory use of a swap execution facility (SEF) was introduced for central clearing of index CDSs in the U.S. only in 2013, and, the European Commission has first mandated central counterparty clearing of certain index CDSs only in 2016, the Big and the Small Bang fundamentally changed the rules of the game through their new trading protocols already in 2009.[ii]

b. Trading Convention Changes

Before the Bangs, the CDS spread reflected the riskiness of the reference entity as a premium to be paid by the buyer (in basis points of the notional amount of the contract) and no upfront payment was needed at the time of trade. The Bangs introduced a terminology of a "standard" contract, in which CDSs started to trade with fixed coupons plus an upfront fee in the market. Any CDS that has a spread, which is typically unequal to predefined fixed coupon rates will have upfront payments exchanged between the buyer or seller at the transaction initiation, with the payer depending on whether the CDS spread being lower or higher than the contractually specified fixed coupon rate. After this initial exchange of upfront payments, the buyer of the contract is obliged to deliver the fixed coupon rate to the CDS seller at a quarterly frequency until maturity. This, in effect, has facilitated a higher flexibility to CDS traders for their bilateral assignments and termination negotiations throughout the maturity.

[ii] As a supplement to the European Market Infrastructure Regulation ('EMIR', Regulation (EU) No. 648/2012), the Delegated Regulation (EU) 2016/592 mandates central clearing of index CDSs for the first time. Any mandate on central clearing of single-name CDS has not been regulated to date in the U.S. or Europe, although certain central counterparties are authorized to clear corporate and sovereign single-name entities on a voluntary basis.

Starting from April 8, 2009 with the Big Bang, single name CDS in North America traded with a fixed coupon (spread), which equals either 100 or 500 basis points. The Small Bang has introduced a similar convention change in Europe on June 20, 2009 after which two additional fixed coupons (i.e., 25 and 1,000) have been also available for new trades.[iii]

The adoption of the fixed coupon rate is implemented quite thoroughly after the Bangs. According to our sample comprising all new CDS trades by all German financial institutions, 90.5 percent of the new trades in 2008 were priced in the then conventional form (i.e., a non-standard coupon rate without any upfront payment), while this proportion had dropped to 6.3 percent in 2010. This is also illustrated in Figure 3, where we show the time trend of the proportion of new trades that adopts the standard coupon rates (25/100/500/1,000). Obviously, there is a large jump of this proportion after June 20, 2009 getting the proportion close to 100 percent.[iv]

c. Contract Changes

In contrast to the convention changes, the contract changes brought by the Bangs usually require changes in the CDS contractual language (Markit [2009a], Markit [2009b]). There are two main aspects of the contract changes brought by the Bangs.

The first aspect is the introduction of an *event determination committee*, with which a central decision maker is created to indicate whether or not a credit (default) event took place; a decision that defines if the obligation payment by the seller of the CDS should be triggered.[v] This decision included its type and date, in order to prevent differing conclusions regarding the same event from arising, and again facilitating a higher standardization. If a credit event is deemed to occur, the committee will make decisions on the acceptable deliverable debt obligations as well as the specific terms of the auction that will follow. The determination committee consists of dealers (taking up the majority voting position), a small number of non-dealer banks, and a few non-voting positions (e.g., ISDA). Members of the determination committee must satisfy a few requirements, i.e., a dealer must be a participating bidder in auctions.

The second aspect is the hardwired auction mechanism that was expected to support a binding settlement price when such a credit event occurred. Before the Bangs, the CDS contracts addressed physical or cash settlement in case of a credit event, and an auction was optional, such that each market participant that have signed auction protocols could take part in the auctions to determine the final recovery rate. Although auctions may serve as a feasible approach to settlement, it is not quite efficient to track all investors and ask them to separately sign up to the protocols each time if it is not a binding process. The Bangs have made settlement and the auction become hardwired for

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[iii] Actually, two additional coupons, i.e., 300 and 750, are also sometimes observed in transactions. These two fixed rates are mainly used for the re-couponing of legacy trades.

[iv] We mark the Small Bang as of after July 24, 2009 in Figure 3 and all other figures. This is because new trades that are accompanied by the convention changes (e.g., that adopt fixed coupon and upfront fee) began on Jun 20, 2009, and contract changes (i.e., hardwiring auction, credit determination committee, restructuring clause changes, etc.) happened later. During the adherence phase, the ISDA noted in particular, "The adherence Period for the Small Bang Protocol opens on July 14, 2009 and closes on July 24, 2009 at 5pm NY time". We take the later date to demarcate the start of the impacting period for the Small Bang, especially for ensuing our analysis with quarterly credit exposure data, in which we exclude the two Bang implementation quarters of 2009Q2 and 2009Q3.

[v] While the Big Bang entails contract changes related to the settlement of a credit event (including not paying on time and designation of bankruptcy) and of a succession event, it does not include a restructuring event. The Small Bang addresses the restructuring as a credit event, especially because the CDS contracts covering restructuring are more effective for capital relief purposes in European countries.

all credit events, so that the potential risk of a market squeeze in a physical settlement could be mitigated.[vi]

All in all, the event determination committee and the hardwiring of auctions limit unpredictable outcomes after a credit event, and support a binding and standard cash settlement price.

[vi] Prior to the Bangs, physical settlements had the feature that large amount of outstanding CDS positions could drive up the bond price in case of limited number of deliverable cash bonds after the occurrence of a credit event. This indeed happened in the bankruptcy of Delphi Corporation in 2005.

Bibliography to the Appendices

MARKIT. *The CDS Big Bang: Understanding the Changes to the Global CDS Contract and North American Conventions*. London Report 2009a.

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Appendix Table A.1

Explaining log of gross notional amount of CDS contracts around the Bangs

Panel A: Around the Big Bang

	(1)	(2)
Time Period	23Jan2009-19Jun2009	23Jan2009-19Jun2009
Dependent Variable	Log (Gross CDS)	
Big Bang Affected Bank-Firm Pair	0.953***	1.345***
	[0.327]	[0.165]
Big Bang Affected Bank-Firm Pair * 1(>8 Apr 2009)	1.499***	1.694***
	[0.418]	[0.294]
Constant	17.434***	17.115***
	[0.079]	[0.056]
Firm * Week FE	Yes	
Bank Type * Firm Type FE	Yes	Yes
Bank Type * Week FE	Yes	Yes
Firm Type * Week FE		Yes
Observations	135,125	147,331
R-squared	0.510	0.157

Panel B: Around the Small Bang

	(1)	(2)
Time Period	10Apr2009-4Sep2009	10Apr2009-4Sep2009
Dependent Variable	Log (Gross CDS)	
Small Bang Affected Bank-Firm Pair	1.252***	1.924***
	[0.200]	[0.177]
Small Bang Affected Bank-Firm Pair * 1(>20 Jun 2009)	0.057	-0.132
	[0.257]	[0.233]
Constant	17.359***	16.966***
	[0.085]	[0.065]
Firm * Week FE	Yes	
Bank Type * Firm Type FE	Yes	Yes
Bank Type * Week FE	Yes	Yes
Firm Type * Week FE		Yes
Observations	135,768	148,294
R-squared	0.519	0.163

Panel C: Subsample regression around the Small Bang

	Big Bang Affected Firms		Small Bang Affected Firms	
	(1)	(2)	(3)	(4)
Time Period	10Apr2009-4Sep2009	10Apr2009-4Sep2009	10Apr2009-4Sep2009	10Apr2009-4Sep2009
Dependent Variable	Log (Gross CDS)			
1(>20 Jun 2009)	0.021**	0.023**	0.047***	0.045***
	[0.009]	[0.009]	[0.008]	[0.007]
Constant	17.802***	17.802***	18.104***	18.105***
	[0.032]	[0.031]	[0.022]	[0.021]
Firm FE	Yes		Yes	
Bank Type FE	Yes		Yes	
Bank Type * Firm FE		Yes		Yes
Observations	52,115	52,115	77,474	77,473
R-squared	0.461	0.505	0.607	0.662

Notes. The table reports estimates from ordinary least squares regressions on the CDS Sample (weekly level data), and the dependent variable is the log of gross notional amount of CDS contracts. The time period in Panel A consists of 11 weeks before the Big Bang and 11 weeks after the Big Bang, such that whole period ends right before the Small Bang. The time periods in Panel B and C both consist of 11 weeks before the Small Bang and 11 weeks after the Small Bang, such that the whole period starts from the first week after Big Bang. Big Bang Affected Bank-Firm Pair and Small Bang Affected Bank-Firm Pair are bank-firm level dummies equal to one if the bank has buy or sell (or both) CDS contracts on the firm in the week before the Big Bang or Small Bang, and the firm is a Big or Small Bang affected firm, respectively, and equals to zero in all other cases. Table 2 contains all definitions and the summary statistics for each included variable. All specifications refer to the intensive margin (restricted to the sample with positive buy or sell (or both) CDS contracts) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for dealer banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.

Appendix Table A.2
Explaning time-varying Hedging Ratio Indicator

Panel A: Subsample of Hedging Ratio Indicator being -1 or 0 (without the empty creditor zone)

	H: Cheaper Insurance Better Hedging	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1	2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2	2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*	*Period 1*	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Hedging Ratio Indicator							
Bangs Affected Bank-Firm Pair		0.001	0.001	0.002	0.036**	-0.027	-0.029	-0.006	-0.009
		[0.017]	[0.017]	[0.017]	[0.016]	[0.022]	[0.023]	[0.022]	[0.025]
Bangs Affected Bank-Firm Pair * 1(>= 2009Q2)		0.028	0.029	0.035*		-0.060**	-0.036	-0.023	
		[0.025]	[0.023]	[0.020]		[0.028]	[0.027]	[0.025]	
Bangs Affected Bank-Firm Pair * Non-Dealer Bank						0.044	0.048	0.011	0.071**
						[0.029]	[0.030]	[0.028]	[0.031]
Bangs Affected Bank-Firm Pair * 1(>= 2009Q2) * Non-Dealer Bank	**> 0**					0.132***	0.097***	0.090***	
						[0.030]	[0.029]	[0.027]	
Bangs Affected Bank-Firm Pair * 1(>= 2015Q2)					0.009				0.049**
					[0.013]				[0.021]
Bangs Affected Bank-Firm Pair * 1(>= 2015Q2) * Non-Dealer Bank	**= 0**								-0.066***
									[0.024]
Constant		-0.869***	-0.853***	-0.869***	-0.902***	-0.850***	-0.834***	-0.855***	-0.896***
		[0.010]	[0.009]	[0.008]	[0.009]	[0.010]	[0.010]	[0.008]	[0.010]
Bank Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Type * Firm Type FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		37,980	61,770	98,288	26,670	37,980	61,770	98,288	26,670
R-squared		0.088	0.065	0.049	0.010	0.092	0.069	0.051	0.011

Appendix Table A.3
Explaining time-varying Hedging Ratio Dummy for non-dealer banks

	H: Cheaper Insurance Better Hedging	(1)	(2)	(3)	(4)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Hedging Ratio Dummy			
Bangs Affected Bank-Firm Pair		0.009	0.009	0.009	0.053***
		[0.016]	[0.016]	[0.016]	[0.018]
Bangs Affected Bank-Firm Pair * 1(>= 2009Q2)	**> 0**	0.079***	0.065***	0.056***	
		[0.023]	[0.021]	[0.020]	
Bangs Affected Bank-Firm Pair * 1(>= 2015Q2)	**= 0**				-0.009
					[0.014]
Constant		0.116***	0.130***	0.118***	0.082***
		[0.010]	[0.009]	[0.007]	[0.009]
Firm Type * Quarter FE		Yes	Yes	Yes	Yes
Observations		23,819	38,682	58,154	13,889
R-squared		0.067	0.046	0.041	0.013

Notes. The table reports estimates from ordinary least squares regressions on the CDS Sample (quarterly level data), including non-dealer banks only, and the dependent variable is the time-varying Hedging Ratio Dummy. We took the snapshot of the original CDS sample at each quarter end and calculate the Hedging Ratio Dummy as equal to one if the Hedging Ratio, which is defined as the net notional amount of CDS on a firm held by a bank to the credit exposure the bank has to this firm, is between 0.5 and 2 (including the case when bank has zero credit exposure to the firm and CDS net notional amount on the firm is maintained at zero (but with positive buy and sell contracts)), and equals to zero in all other cases. 1(>= 2009Q2) is a dummy equal to one for all quarters after 2009Q1, and 1(>= 2015Q2) is a dummy equal to one for all quarters after 2015Q1. Table 2 contains all definitions and the summary statistics for each included variable. All specifications refer to the intensive margin (restricted to the sample with positive buy or sell (or both) CDS contracts) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for dealer banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.

Appendix Table A.4
Explaining time-varying Hedging Ratio Indicator for non-dealer banks

Panel A: Subsample of Hedging Ratio Indicator being -1 or 0 (without the empty creditor zone)

	H: Cheaper Insurance Better Hedging	(1)	(2)	(3)	(4)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Hedging Ratio Indicator			
Bangs Affected Bank-Firm Pair		0.013	0.013	0.013	0.063***
		[0.024]	[0.024]	[0.024]	[0.021]
Bangs Affected Bank-Firm Pair * 1(>= 2009Q2)	> 0	0.078**	0.067**	0.058**	
		[0.034]	[0.030]	[0.028]	
Bangs Affected Bank-Firm Pair * 1(>= 2015Q2)	= 0				-0.018
					[0.016]
Constant		-0.822***	-0.811***	-0.840***	-0.904***
		[0.014]	[0.012]	[0.010]	[0.011]
Firm Type * Quarter FE		Yes	Yes	Yes	Yes
Observations		16,524	27,928	44,501	12,061
R-squared		0.101	0.067	0.065	0.015

Panel B Subsample of Hedging Ratio Indicator being 0 or 1 (without the over-exposed zone)

	H: Cheaper Insurance Better Hedging	(1)	(2)	(3)	(4)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Hedging Ratio Indicator			
Bangs Affected Bank-Firm Pair		-0.020	-0.020	-0.020	-0.111**
		[0.038]	[0.038]	[0.038]	[0.051]
Bangs Affected Bank-Firm Pair * 1(>= 2009Q2)	< 0	-0.133***	-0.120***	-0.124***	
		[0.048]	[0.046]	[0.045]	
Bangs Affected Bank-Firm Pair * 1(>= 2015Q2)	= 0				-0.095**
					[0.046]
Constant		0.738***	0.697***	0.684***	0.636***
		[0.020]	[0.018]	[0.017]	[0.031]
Firm Type * Quarter FE		Yes	Yes	Yes	Yes
Observations		10,879	17,186	22,439	3,316
R-squared		0.086	0.077	0.073	0.050

Notes. The table reports estimates from ordinary least squares regressions on the CDS Sample (quarterly level data), including non-dealer banks only, and the dependent variable is the time-varying Hedging Ratio Indicator. We took the snapshot of the original CDS sample at each quarter end and calculate the Hedging Ratio Indicator which has three values; it equals to -1 /0/1 if the bank is over-exposed to the firm/effectively hedged/ empty creditor of the firm, respectively. A more comprehensive definition and summary statistics could be found in Table 2. In Panel A we exclude the observations with Hedging Ratio Indicator being 1, and in Panel B we exclude the observations with Hedging Ratio Indicator being -1. All specifications refer to the intensive margin (restricted to the sample with positive buy or sell (or both) CDS contracts) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for dealer banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.

Appendix Table A.5

Explaining log of gross notional amount of CDS contracts for non-dealer banks

	H: Cheaper Trading More Trading	(1)	(2)	(3)	(4)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2
		Period 1	*Period 2*	*Period 3*	*Placebo*
Dependent Variable		Log (Gross CDS)			
Bangs affected Bank-Firm Pair		0.795***	0.795***	0.795***	0.746***
		[0.075]	[0.075]	[0.075]	[0.068]
Bangs affected Bank-Firm Pair * 1(>= 2009Q2)	**> 0**	0.281***	0.319***	0.139*	
		[0.086]	[0.083]	[0.082]	
Bangs affected Bank-Firm Pair * 1(>= 2015Q2)	**= 0**				-0.117**
					[0.051]
Constant		16.330***	16.264***	16.251***	16.159***
		[0.041]	[0.034]	[0.029]	[0.041]
Firm Type * Week FE		Yes	Yes	Yes	Yes
Observations		315,654	515,211	776,897	186,379
R-squared		0.062	0.095	0.108	0.073

Notes. The table reports estimates from ordinary least squares regressions on the CDS Sample (weekly level data), including non-dealer banks only, and the dependent variable is the log of gross notional amount of CDS contracts. 1(>= 2009Q2) is a dummy equal to one for all quarters after 2009Q1, and 1(>= 2015Q2) is a dummy equal to one for all quarters after 2015Q1. Table 2 contains all definitions and the summary statistics for each included variable. All specifications refer to the intensive margin (restricted to the sample with positive buy or sell (or both) CDS contracts) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for dealer banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.

Appendix Table A.6

Explaining credit exposure with firm level treatment variable on the sample of non-dealer banks and non-CDS trading banks

	H: Cheaper Insurance More Credit	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre Bangs Period		2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1	2008Q1-2008Q2	2008Q1-2009Q1	2008Q1-2009Q1	2008Q1-2009Q1	2014Q1-2015Q1	2008Q1-2008Q2
Post Bangs Period		2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2	2008Q3-2008Q4	2009Q4-2010Q4	2009Q4-2012Q4	2009Q4-2016Q2	2015Q2-2016Q2	2008Q3-2008Q4
		Period 1	*Period 2*	*Period 3*	*Placebo*	*Placebo*	*Period 1*	*Period 2*	*Period 3*	*Placebo*	*Placebo*
Dependent Variable		Log (Bank-Firm Exposure)									
Bangs Affected Firm * 1(>= 2009Q2)		-0.107***	-0.162***	-0.328***							
		[0.015]	[0.016]	[0.017]							
Bangs Affected Firm * 1(>= 2015Q2)					-0.170***						
					[0.016]						
Bangs Affected Firm * 1(>= 2008Q3)						-0.033***					
						[0.013]					
Bangs Affected Firm * 1(>= 2009Q2) * Non-Dealer Bank	> 0						0.076**	0.064*	0.134***		
							[0.032]	[0.036]	[0.039]		
Bangs Affected Firm * 1(>= 2015Q2) * Non-Dealer Bank	= 0									0.164***	
										[0.040]	
Bangs Affected Firm * 1(>= 2008Q3) * Non-Dealer Bank	= 0										0.030
											[0.027]
Constant		14.478***	14.420***	14.254***	13.996***	14.512***	14.476***	14.415***	14.243***	13.993***	14.511***
		[0.004]	[0.004]	[0.003]	[0.004]	[0.004]	[0.004]	[0.004]	[0.003]	[0.004]	[0.004]
Bank Type * Quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Type FE		Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Firm Type * Quarter FE		No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Bank Type * Firm Type FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		2,115,243	3,725,172	7,021,748	2,512,244	867,457	2,115,243	3,725,172	7,021,748	2,512,244	867,457
R-squared		0.102	0.095	0.095	0.082	0.103	0.102	0.095	0.095	0.082	0.103

Notes. This set of results are based on the sample of 51 non-dealer banks (matched to the credit register data) and 204 non-CDS trading banks (with their credit exposures to all the firms), which are selected by ranking their number of credit relationships with firms by the end of 2007, and taking the top 204 (=51*4) banks which do not trade CDS. The main treatment variable 'Bangs affected firm' is a firm-level dummy that is equal to one if the firm has CDS traded on before the Big Bang, and the contract on this firm will be affected by the Big Bang or Small Bang contract changes, and equal to zero in all other cases. Non-Dealer Bank is a dummy equal to one if the bank is a non-dealer CDS trading bank, and it equals to zero if the bank is a non-CDS trading bank. All specifications refer to the intensive margin (restricted to the sample with positive bank-firm exposure) and differ in the fixed effects being included and time period of the regression sample. Bank Type is a dummy equal to one for non-dealer banks, and equal to zero for non-CDS banks. Firm Type is an indicator equal to 1 for Small Bang affected CDS firms, 2 for Big Bang affected CDS firms and 0 for the others. Coefficients are listed in the first row, robust standard errors which are clustered at bank-firm level are reported in the row below, and the corresponding significance levels are placed adjacently. "Yes" indicates that the set of fixed effects is included. "No" indicates that the set of fixed effects is not included. *** Significant at 1%, ** significant at 5%, * significant at 10%.