Counterparty Risk and Counterparty Choice in the Credit Default Swap Market*

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Abstract

We investigate how market participants price and manage counterparty credit risk in the post-crisis period using confidential trade repository data on single-name credit default swap (CDS) transactions. We find that counterparty risk has a modest impact on the pricing of CDS contracts, but a large impact on the choice of counterparties. We show that market participants are significantly less likely to trade with counterparties whose credit risk is highly correlated with the credit risk of the reference entities and with counterparties whose credit quality is low. Our results suggest that credit rationing may arise under wider circumstances than previously recognized.

Keywords: Counterparty credit risk, credit default swaps, central clearing, credit rationing, counterparty choice.
JEL Classifications: G12, G13, G24

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1 Introduction

Counterparty risk in over-the-counter (OTC) derivative markets played an important role in the propagation of the global financial crisis in 2008. The inability of Bear Stearns and Lehman Brothers to find counterparties willing to trade, as their troubles became apparent, hastened their descent into insolvency (Duffie, 2010). Senior policymakers justified government assistance in the sale of Bear Stearns to JP Morgan Chase, in large part, by the need to avoid the further dislocations in OTC derivative markets that would have ensued in a rush to replicate positions with new counterparties. Structural reforms introduced by Title VII of the Dodd-Frank Act in the United States and similar measures in the European Union were intended to reduce dramatically the scope for counterparty risk in derivative markets to generate systemic crises.[1]

In this paper, we investigate how market participants manage and price counterparty risk in the credit default swap (CDS) market. We use four years (2010–13) of confidential transaction level data from the trade repository maintained by the Depository Trust & Clearing Corporation (DTCC) to estimate the effects of counterparty risk on pricing and on counterparty selection. We find negligible effects of counterparty risk on the pricing of CDS contracts, but, consistent with the experience of Bear Stearns and Lehman Brothers, find large effects of counterparty risk on the client's choice of dealer counterparty.

In an early discussion of counterparty risk in the OTC derivative markets, Litzenberger (1992) in his Presidential Address to the American Finance Association observed that pricing of interest rate swaps (IRS) appears to be insensitive to counterparty credit ratings. Subsequent empirical studies largely confirm Litzenberger's claim (e.g., Duffie and Singleton, 1997). Furthermore, theoretical studies of IRS pricing predict counterparty spreads an order of magnitude smaller than bond spreads of equivalent rating (e.g., Duffie and Huang, 1996; Huge and Lando, 1999).

As noted by Huge and Lando (1999), IRS results do not necessarily carry over to the CDS market due to the presence of undiversifiable default risk contagion between dealer and

[1]The Financial Crisis Inquiry Commission (2011) report provides a detailed narrative based on primary documents and testimony of senior policymakers and industry leaders. See especially pp. 287, 291, 329, and 347.

reference entity.[2] Nonetheless, Arora, Gandhi, and Longstaff (2012) find an economically small impact of dealer credit risk in a sample of dealer CDS quotes to a single large buyside client. We confirm this finding in a sample of client-facing (i.e., between dealer and non-dealer) transactions. Running the same regressions on a sample of interdealer transactions, we find even smaller pricing impacts, if any at all, arising from counterparty credit risk. This divergence in sensitivity to counterparty risk is consistent with differences in provisions for collateral. Interdealer transactions have uniform collateral terms involving daily exchange of variation margin and (prior to 2016) no initial margin, while most client-facing transactions entail significant counterparty exposure to the dealer, either due to thresholds for posting variation margin or to unilateral requirements that the client post (but not receive) initial margin.

Central clearing affords another test of pricing impact. If dealer counterparty risk were a material determinant of equilibrium market prices, then one would expect to see an increase in CDS spreads upon the introduction of central clearing. Clearinghouses have strict collateral and margin requirements for clearing members and maintain additional default funds to cover capital shortfall in the event of counterparty default, and thereby greatly reduce counterparty risk. Loon and Zhong (2014) hypothesize that centrally cleared trades should have higher spreads than uncleared trades due to counterparty risk mitigation, and report evidence in support. Contrary to their findings, we find that transaction spreads on centrally cleared trades are significantly *lower* relative to spreads on contemporaneous uncleared transactions, which is consistent with the view that counterparty risk does not have a first-order impact on pricing.

If prices do not adjust materially to dealer credit risk in the OTC derivative markets, then Litzenberger (1992) conjectured that *quantities* might adjust, i.e., that non-dealers might avoid transacting with weaker dealers. In this paper, we provide the first direct test of this conjecture. We estimate a multinomial logit model for the client's choice of dealer counterparty, and find very strong evidence that clients are less likely to buy protection from dealers whose credit quality is relatively low. We also find that clients are less likely to buy

[2] A large recent empirical literature provides strong evidence of contagion in the incidence of corporate defaults (Azizpour, Giesecke, and Schwenkler, forthcoming) and for its effect on credit pricing (Berndt, Ritchken, and Sun, 2010; Bai, Collin-Dufresne, Goldstein, and Helwege, 2015).

CDS protection from a dealer whose credit risk is highly correlated with the credit risk of the reference entity, i.e., buyers of protection avoid wrong-way risk.

Our counterparty choice results shed light on a form of credit rationing, and indicate that such rationing may arise under wider circumstances than previously recognized. Unlike the textbook case of a borrower in search of a loan, counterparty credit in the OTC derivatives market is contingent (i.e., on the future value of the position) and incidental (i.e., because it arises as an undesired side-effect of the transaction and not as its impetus). Nonetheless, if a client declines to trade with a dealer due to the risk of future dealer default, then the dealer's access to counterparty credit (and the associated flow of business) has been rationed. In the classical model of Stiglitz and Weiss (1981), credit rationing arises due to imperfect information. While dealer balance sheets may be opaque, abundant information on creditworthiness is available in the form of agency ratings and market prices on bonds issued by dealers and CDS referencing dealers, so the scope for asymmetric information in our setting seems limited. Bester (1985) shows that credit rationing can be mitigated by introducing collateral requirements. An absence of collateral arrangements may explain credit rationing in the federal funds market as documented by Afonso, Kovner, and Schoar (2011). However, CDS contracts are for the most part collateralized in that dealer and client regularly exchange variation margin equal to the change in the mark-to-market value of the bilateral portfolio.

In extremis, as occured in the cases of Bear Stearns and Lehman Brothers, a flight of derivative counterparties can drain a dealer of liquidity and thereby behave like a bank run; see Duffie (2010, pp. 65–67) on the mechanics of this form of run. In this respect, credit rationing of dealers in the CDS market is related to runs in other collateralized markets during the global financial crisis. Copeland, Martin, and Walker (2014) show that Lehman Brothers did not experience higher margins when seeking funding in the triparty repo market before bankruptcy; instead, cash investors simply pulled their funding away from the distressed dealer. Covitz, Liang, and Suarez (2013) document a run in the asset-backed commercial paper programs in 2007 and show that the runs were more severe for riskier programs.

Our findings may be attributable to two market imperfections. First, clients may not be able to extract full pricing compensation for bearing counterparty risk because dealers have some monopoly power (Siriwardane, 2015). Second, collateral arrangements are imperfect. It is well understood by practitioners that variation margin offers little protection against the jump risk in market price movements likely to accompany the failure of a large dealer. Less obvious, perhaps, is that prevailing collateral arrangements of the post-crisis period for unilateral provision by the client to the dealer of *initial margin* exacerbates counterparty risk from the clients' perspective because it exposes the posted collateral to the risk of dealer failure. Newly agreed international rules on swap margining will require bilateral provision of initial margin to be held in segregated third-party custodial accounts.[3] Our results suggest that the new framework will reduce the likelihood of a counterparty "run" in OTC derivative markets. As these provisions will dramatically reduce counterparty losses in the event of dealer default, non-dealers should become less sensitive to dealer credit risk in choosing a counterparty.

After establishing the benchmark counterparty choice result, we explore how counterparty choice depends on characteristics of the reference entity and characteristics of the client. With respect to reference entity characteristics, we find that the choice is more sensitive to the credit risk of the dealer when the reference entity is financial. By shifting attention from prices to quantities, our paper helps resolve the puzzling finding of Arora, Gandhi, and Longstaff (2012) that counterparty risk is not priced for financial reference entities, where the counterparty risk concern should be heightened due to wrong-way risk. We also explore how market liquidity affects the choice of counterparty. Since the client may anticipate that it will be more difficult to terminate a trade on an illiquid reference entity than on a liquid one, the client should be more reluctant to trade with a high credit risk dealer when the reference entity is illiquid. Consistent with our hypothesis, we find a large and statistically significant coefficient on an interaction term between dealer CDS spread and reference entity liquidity.

Client characteristics matter as well. Clients that trade in and out of positions quickly should be less sensitive to dealer credit risk, as they should anticipate a shorter exposure to counterparty risk. We find strong evidence for this conjecture. We also hypothesize that captive clients (that is, clients who trade predominantly with a single dealer) should be less sensitive to counterparty risk. This too is supported by the evidence, which suggests that such clients may be especially vulnerable to counterparty credit losses in a financial crisis.

[3]The principles of the new framework are set forth in the Basel Committee on Banking Supervision (2015), and the US implementation is promulgated in Federal Register (2015). Trades between large dealers are already subject to the new framework, but application to client-facing trades will be phased in through 2020.

Finally, we consider differences across institutional types. We find some evidence that hedge funds, asset managers, and non-dealer banks are more sensitive to dealer credit risk than institutions, such as insurance companies, that are perceived to be less sophisticated in the practice of risk-management.

Our paper is related to several other empirical papers on counterparty risk. Besides Arora, Gandhi, and Longstaff (2012), Giglio (2014) infers a price impact of counterparty risk from the corporate bond-CDS basis. Our paper departs from the earlier literature and emphasizes trade quantities (i.e., via choice of counterparty) over trade prices. While ours is the first paper to study the determinants of the client's choice, several recent papers have reported findings consistent with our theme. Aragon, Li, and Qian (2016) find that bond mutual funds are more likely to close existing CDS positions as buyers of protection when the counterparty risk of the dealer is high. Gündüz (2015) shows that financial institutions buy more protection on a dealer as reference entity when exposed to that dealer through counterparty relationships. Focusing on the period of the global financial crisis period, Shachar (2012) shows that liquidity deteriorates as counterparty exposures between dealers accumulate.

We proceed as follows. In Section 2, we provide background on counterparty risk in the CDS market and describe the DTCC data. In Section 3, we examine the effects of counterparty credit risk on CDS pricing. In Section 4, we estimate the multinomial choice model for buyers and sellers of protection. Section 5 concludes.

2 Background and Data Description

A single-name credit default swap is a derivative contract designed to provide synthetic insurance on the default of a specified firm, known as the *reference entity*. The parties to the contract are the *seller of protection* and *buyer of protection*, henceforth usually denoted the seller and the buyer, respectively. The buyer makes quarterly payments to the seller of a premium given by the coupon rate on the contract, divided by four and multiplied by the *notional size* of the contract. In the event of default of the reference entity before the expiry of the swap, premium payments cease and the seller pays the buyer the notional amount of the contract times a loss fraction, where the loss fraction is one minus the recovery rate of the bond. Liquidity in the CDS market tends to be concentrated at the five year tenor, which accounts for about 80% of transactions in our sample.

As in equity markets, there exist index contracts that pool together CDS on a specified set of index constituents. Trading volume in the main index contracts is generally much larger than the trading volume in the underlying single-name CDS contracts, and the index contracts are now mostly traded on exchanges. In this paper, we focus exclusively on the single-name CDS market. As elaborated below, the single-name market during our sample period was still (and, for the most part, remains today) a traditional dealer-intermediated OTC market for non-dealer participants. For a broad survey of the literature on the CDS market, see Augustin, Subrahmanyam, Tang, and Wang (2014).

2.1 Pricing and Managing Counterparty Risk

Swaps traded in OTC markets are subject to counterparty credit risk, i.e., the risk that one's counterparty to a trade will default prior to the maturity of the swap. Absent collateral, the surviving party would lose the market value of the swap if the surviving party were in-the-money, but would still be obliged to compensate the estate of the defaulting counterparty if the defaulting party were in-the-money.[4] Market participants respond to counterparty risk either by managing the risk or by demanding compensation for bearing the risk. Below we describe the available mechanisms: netting and collateral, central clearing, dynamic hedging, counterparty choice, and price adjustments. The latter two mechanisms, which are the focus of our study, indirectly evidence the limitations of the first three. That is, if counterparty risk could effectively be eliminated at low cost with netting arrangement and collateral exchange, central clearing, or hedging, then there would be no need to ration weak counterparties or to depart from the law of one price.

First, counterparties arrange for netting of offsetting bilateral positions and collateralize trades under the terms of a credit support annex (CSA) to an ISDA Master Agreement. Collateralization takes two forms: *Initial margin* (also known as independent amount) is exchanged at trade inception and retained until the trade is terminated or matures. *Variation margin* is exchanged during the life of the contract to cover changes over time in its market value.

[4] More precisely, the surviving party would have an unsecured claim on the bankruptcy estate of the defaulting counterparty for the market value of an in-the-money swap.

In the aftermath of the financial crisis, interdealer CSAs have required daily exchange of variation margin equal to the change in the mark-to-market value of the bilateral portfolio. Prior to 2016, dealers did not exchange initial margin with one another.[5] Client CSAs are subject to negotiation and are therefore more varied in terms. Typically, hedge funds CSAs require bilateral daily exchange of variation margin, and further require that the client unilaterally post initial margin to the dealer. For other institutional classes, collateral requirements may depend on agency or internal credit ratings. For highly-rated clients, exchange of variation margin takes place when the unsecured exposure exceeds an agreed threshold, which essentially serves as a limit on a line of credit. Smaller or riskier clients would have zero threshold and could be required to post initial margin.[6]

From the client perspective, margin provisions do not eliminate counterparty risk. Variation margin mitigates the risk, but a dealer in distress can exploit valuation disputes and grace periods to delay delivery of collateral, and the failure of a dealer is likely to coincide with unusual market volatility and reduced liquidity. As witnessed in the case of AIG during the financial crisis, ratings-based thresholds may prove ineffective, as the event of downgrade of a large financial institution may trigger the immediate default of that institution (see Financial Crisis Inquiry Commission, 2011, Chapter 19). Moreover, counterparty risk is exacerbated if the CSA imposes unilateral posting of initial margin to the dealer. There is no provision for third-party custodial control of client collateral, and segregation of client collateral (i.e., from other client collateral) is very rare. As noted by ISDA (2010), clients suffered significant losses of initial margin in the defaults of Lehman Brothers and MF Global.

As with other studies in this literature, we have no access to counterparty-level data on CSA agreements, exchange of collateral, and exposures in other derivative classes that are likely to be in the same netting set (e.g., interest rate derivatives). Thus, we cannot address the effects of collateralization and netting in mitigating counterparty risk at the client level. We also cannot identify the extent to which individual clients may be exposed to dealers due to initial margin provisions. We simply maintain the assumption that bilateral CDS positions entail exposure of clients to dealer counterparty risk.

[5]Prior to 2016, received collateral would be held on counterparty's own accounts. Symmetric exchange of initial margin between two dealers would cancel out, and thus serve no purpose.

[6]As a form of overcollateralization, initial margin works in opposition to a variation margin threshold. Therefore, a CSA may feature a threshold or initial margin, but not both (ISDA, 2010).

Second, regulatory reform has mandated central clearing of trades on most standardized and liquid OTC contracts. Central counterparties impose standardized margining rules and effectively mutualize counterparty risk. In the CDS market, recent series of the most heavily-traded indices are eligible for clearing, as are the constituent single-name swaps. While central clearing of many North American indices is now mandatory, central clearing of single-name swaps remains voluntary. During our sample period, central clearing of interdealer single-name swaps was already commonplace, but clearing of client-facing single-name swaps was virtually non-existant.[7] We proceed under the assumption that central clearing was not yet a viable risk-mitigating option for non-dealers engaged in trading single-name CDS in 2010–13.

Third, market participants can hedge counterparty risk by purchasing CDS protection on their dealer counterparties as reference entities. Such hedging would be difficult to execute rigorously due to the stochastic size of the exposure, but market participants might pursue approximate strategies. Gündüz (2015) shows that financial institutions buy more protection on a dealer as reference entity when exposed to that dealer through counterparty risk. However, he finds that non-dealers hedge in this manner at lower frequency than do the dealer banks.

Fourth, market participants can mitigate counterparty risk simply by trading preferentially with counterparties that are less risky or less correlated with the underlying reference entity. For example, if dealer ABC were to become too risky, participants might preferentially trade with ABC when a contract offsets existing bilateral exposure, but otherwise preferentially trade with other dealers. In addition, market participants may avoid buying protection from counterparties whose credit risk is highly correlated with credit risk of the reference entities. For example, a buyer of CDS protection on French banks might avoid transacting with a French dealer. A related idea is that market participants may be more likely to exit existing positions when the counterparty risk of the dealer is high. Aragon, Li, and Qian (2016) find support for this hypothesis in the portfolio turnover of U.S. bond mutual funds.

[7] Cleared client single-name notional reported on the ICE website in "Credit Default Swaps: Growth in Clearing & Futures" is close to zero in 2013 and starts to pick up only in the third quarter of 2015. In our data, we observe only two instances of client clearing in a sample of over one thousand transactions on eligible single-name reference entities involving a non-dealer counterparty.

Finally, counterparty risk may be reflected in transaction prices of derivative contracts. The credit valuation adjustment (CVA) measures the difference in values between a derivative portfolio and a hypothetical equivalent portfolio that is free of counterparty risk. Intuitively, it represents the cost of hedging counterparty risk in the bilateral portfolio. To the extent that this cost can be imposed on the counterparty through the terms of trade, we will observe the price of a contract varying with the credit risk of the counterparties.[8] It is important to recognize that adjustments to pricing do not mitigate counterparty risk, but rather serve as compensation for bearing the risk. The CVA is the net present value of future losses, so in normal circumstances it will be orders of magnitude smaller than the potential losses that could result from counterparty default.

Whether managed or priced, counterparty risk in the CDS market has a natural asymmetry between buyer and seller of protection. If the seller of protection defaults prior to the reference entity, loss to the buyer can be as large as the notional value of the contract. If the buyer defaults, the seller's loss is bounded above by the discounted present value of the remaining stream of premium payments, which is typically one or two orders of magnitude smaller than the notional amount. This asymmetry is recognized as well in current FINRA rules on posting of initial margin for cleared CDS trades.[9] Furthermore, because financial firms (especially dealer banks) are more likely to default when prevailing credit losses are high, wrong-way risk is invariably borne by the buyer of protection. Thus, we expect the buyer of credit protection to be more sensitive to the credit risk of the seller than the seller is to the credit risk of the buyer.

2.2 DTCC CDS Transaction Data

DTCC maintains a trade repository of nearly all bilateral CDS transactions worldwide. Each transaction record specifies transaction type, transaction time, contract terms, counterparty names and transaction price. We access the data via the regulatory portal of the Federal Reserve Board (FRB) into DTCC servers. The portal truncates the DTCC data in accor-

[8] In practice, compensation for CVA may be limited by the bilateral nature of counterparty risk. If two equally risky counterparties with symmetric collateral terms enter a trade in which return distributions are roughly symmetric, then each demands similar compensation from the other. If the trade is to be executed, it will be executed near the hypothetical CVA-free price, so neither party will be compensated.

[9] As of 18 July 2016, Rule 4240 of the FINRA Manual specifies that initial margin requirement for the buyer of protection shall be set to 50% of the corresponding requirement for the seller of protection.

dance with so-called entitlement rules (Committee on Payment and Settlement Systems, 2013, S3.2.4). As a prudential supervisor, the FRB is entitled to view transactions for which

(i) at least one *counterparty* is an institution regulated by the FRB, *or*

(ii) the *reference entity* is an institution regulated by the FRB.

Within each of these entitlement windows, our samples are complete. Thus, in a sample limited to trades on FRB-regulated institutions as reference entities, we observe all trades worldwide regardless of the identities of the counterparties. In a sample limited to trades involving FRB-regulated institutions as a party, we observe all trades regardless of the identity of the counterparty and the reference entity.

The set of FRB-regulated institutions includes the largest dealer banks in the US: Bank of America, Citibank, Goldman Sachs, JP Morgan Chase and Morgan Stanley. We refer to these major US dealer-banks collectively as the "US5." Between them, the US5 dealers are party to a majority of CDS transactions worldwide. Comparing the transaction volumes in our sample to tallies published by DTCC for the same period, we find that our sample of transactions involving a US5 dealer as counterparty captures about two-thirds of all new transaction volume in the single-name CDS market.

We now describe construction of our two main samples. First, the *baseline sample* consists of transactions for which the underlying reference entity is regulated by the FRB. This sample is complete with respect to the choice of counterparty available to the client. Similar to Arora, Gandhi, and Longstaff (2012), in this sample we restrict our analyses to trades involving at least one of the 14 largest CDS dealers: Bank of America Merrill Lynch, Barclays, BNP Paribas, Citibank, Credit Suisse, Deutsche Bank, Goldman Sachs, HSBC, JP Morgan Chase, Morgan Stanley, RBS Group, Société Générale, UBS, and Nomura.[10] These 14 dealers account for 99.8% of trades in our sample of liquid, FRB-regulated reference entities. Second, the *US5 counterparty sample* consists of all transactions on any reference entity (financial and non-financial) for which at least one counterparty is a US5 dealer. This sample is much larger and much more diverse with respect to characteristics of the reference entity, but is truncated with respect to the choice of counterparty available to the client.

[10]Relative to the list of 14 dealers appearing in the sample of Arora, Gandhi, and Longstaff (2012), Lehman is dropped (as it no longer exists), Bank of America and Merrill Lynch are merged, and Nomura Holdings and Société Générale are added.

Our sample period is January 2010 through December 2013.[11] After applying a series of data filters described in Appendix A, we have 83,335 transactions on 12 reference entities in the baseline sample, and 1,435,205 transactions on 1635 reference entities in the US5 counterparty sample. Within each of these samples, the subsample of primary interest consists of client-facing transactions in which a non-dealer buys protection from a dealer counterparty. As reported in Table 1, our baseline sample contains 196 non-dealer buyers of protection in 11,932 transactions, of which 7918 reference US5 dealers and the remainder reference other FRB-regulated institutions. Our US5 counterparty sample contains 828 non-dealer buyers of protection in 190,838 transactions and 1248 reference entities, of which 259 are financial firms and 76 are sovereigns. The heterogeneity across reference entities in the US5 counterparty sample will allow us to investigate whether investors manage counterparty credit risk differently for different reference entities.

A non-dealer can trade with a dealer only when a signed ISDA Master Agreement is in place. The *de facto* choice set for some counterparties, therefore, may only be a subset of the alternatives included in the counterparty choice regressions. While we cannot directly observe whether an agreement is in place, we show in Internet Appendix A that over 75% of baseline sample transactions are done by clients who trade with eight or more of the 14 international dealers, and over 80% of transactions in the US5 counterparty sample are done by clients who trade with all US5 dealers. We therefore conclude that a large majority of active non-dealer participants were maintaining a significant number of ISDA Master Agreements during the sample period.

2.3 Main Explanatory Variables

We next define key explanatory variables used in our analyses: risk of dealer default, wrong-way risk, and trading relationship. We measure the risk of dealer default by the dealer's five year CDS spread quoted at the end of the previous trading day.[12] For observation date t, the lagged spread is denoted $\overline{cds}^{s}_{t-1}$ when dealer s is the seller of protection. As documented in Internet Appendix B, there is substantial cross-sectional and time variation in dealers'

[11] Our window has no overlap with the period of March 2008 to January 2009 studied by Arora, Gandhi, and Longstaff (2012), and overlaps only partially with the period of 2009–11 studied by Loon and Zhong (2014).

[12] In robustness exercises, we consider an alternative measure for the risk of dealer default based on the bankruptcy hazard rate model of Chava and Jarrow (2004).

credit risk in our sample. Across our sample period, the median difference in CDS spread between the riskiest and the safest of the 14 international dealers is about 140 basis points.

In the baseline sample of entitled reference entities, our preferred measure of wrong-way risk, WWR_i^s, is a dummy variable equal to one if both the seller of protection is a US5 dealer and the reference entity is either a US5 dealer or Wells Fargo. Wells Fargo is grouped with the US5 dealers for this purpose due to similarity in size and national scale of banking operations and its shared status as a G14 derivatives dealer.[13] Within the US5 counterparty sample, there is no variation across the dealers in the US5+Wells Fargo dummy variable, so a coefficient on this variable would be unidentified. Primarily for use with this sample, we define an alternative measure of WWR_t^s based on the correlation between the log CDS spread changes on the reference entity and on the selling dealer. The correlations are estimated using weekly observations on a five-year rolling window.[14]

Perhaps to achieve operational efficiencies, trading relationships in OTC markets often persist through time. In the case of buyer of protection b and seller of protection s, $Relations_{t-1}^{s,b}$ is defined as the share of notional value that market participant b traded with dealer s in the recent past. We measure this share using the past 28 business days prior to the transaction if there were more than 28 transactions in the last month, otherwise we estimate the share using the past 28 transactions, requiring a minimum of 10 transactions. To express $Relations_{t-1}^{s,b}$ in share terms, we divide the total notional value transacted between b and s by the total notional value that market participant b traded.

3 Effects of Counterparty Risk on CDS Pricing

In this section, we study the effects of dealer credit risk on CDS pricing. If single-name CDS trading entails counterparty risk, then protection sold by high-risk counterparties should be less valued than protection sold by low-risk counterparties. Whether this difference affects market prices, however, is an empirical question. If it does, then, holding fixed the buyer

[13]Though Wells Fargo is not a significant player in the CDS market, it has a larger presence in other OTC derivative markets. The US5 and Wells Fargo are the only G14 dealer banks domiciled in the U.S., and the only participants in ICE Clear Credit that are FRB-regulated at the holding company level.

[14]A caveat is that the variation across US5 dealers in this correlation measure is usually modest. For most reference entities in the broader universe of single-name CDS, it is not obvious that differences in correlations across dealers within the US5 counterparty choice set would be salient to investors.

and contract, we expect sellers' CDS spreads to be negatively associated with transaction spreads. We perform fixed effect panel regressions in Section 3.1 to test the hypothesis. Furthermore, if the effect on market prices is material, then we expect to see higher CDS spreads on centrally cleared than on bilateral uncleared transactions. This hypothesis is tested in Section 3.2.

The dependent variable throughout this section is a measure of distance between the par spread on a transaction in the DTCC data and Markit's end-of-day par spread quote on the same reference entity. Summary statistics for the spread difference are given in Table 2. Panels A and C shows that the median difference is within one basis point in each sample, which confirms that Markit quotes track prevailing traded spreads quite closely on average. In the baseline sample, the median absolute difference is 3.3 basis points and the 95th percentile of the absolute difference is 18.9 basis points. In the US5 counterparty sample, the median and 95th percentile of the absolute difference are 4.2 basis points and 32.3 basis points, respectively.

Panel B of Table 2 summarizes characteristics of baseline sample transactions on the same reference entity with the same tenor, tier, currency, restructuring or non-restructuring clause and fixed coupon rate, traded on the same date. We restrict these summary statistics to the subsample in which there are at least ten trades on the identical contracts during the same day, which is about 28 percent of our baseline sample. We find significant pricing dispersion within the day on the same contract, with a median within-day standard deviation of 1.4 percent. Pricing dispersion in the US5 counterparty sample is qualitatively similar, as shown in Panel D.

In terms of counterparty choice, we see that in both samples a buyer trades with more than one seller on the same contract and the same day on average. Observing multiple counterparties for the same party and the same contract serves to identify whether cross-sectional pricing dispersion in transaction spreads varies with cross-sectional dispersion in counterparty credit spreads.

3.1 Effect of Seller Credit Risk on CDS Pricing

We investigate whether counterparty risk is priced in the CDS market from the protection buyer's perspective. Our benchmark specification is similar in spirit to that of Arora, Gandhi, and Longstaff (2012). We compare the transaction spreads on the same contract, traded on the same date, bought by the same buyer, but sold by different sellers that vary in their credit risk. Identification comes from pricing dispersion within the same day. Our benchmark specification is

$$\log(cds_{i,t}^{s,b}) - \log(\overline{cds}_{i,t}) = \alpha_{i,t}^{b} + \beta \log(\overline{cds}_{t-1}^{s}) + \eta WWR_t^s + \lambda Relations_{t-1}^{s,b} + \delta \log(size) + \epsilon_{i,t}^{s,b}, \tag{1}$$

where $\log(cds_{i,t}^{s,b})$ is the log par spread on CDS transaction on reference entity i at time t. Superscripts s and b denote the seller and buyer of credit protection, respectively. We denote by $\overline{cds}_{i,t}$ the par spread quoted by Markit on reference entity i on date t. The dependent variable measures the difference between a specific transaction spread and the Markit quote on the same reference entity at time t.[15]

Independent variables of primary interest are the log of the seller's quoted CDS spread ($\overline{cds}_{t-1}^{s}$), the wrong-way risk variable measured either as an indicator (WWR^s (Indicator)) or based on the dealer-reference entity correlation (WWR^s (Correlation)), and the measure of past buyer-seller relationship ($Relations_{t-1}^{s,b}$). The fixed effect $\alpha_{i,t}^{b}$ interacts indicators for buyer, contract and time. The log of the notional value of the traded contract, $\log(size_{i,t})$, is included in the regression to allow for the contract size to have some potential impact on transaction spreads. As seller default risk and wrong-way risk reduce the value of the protection leg of the swap, we expect $\beta < 0$ and $\eta < 0$.

We present regression estimates for equation (1) in Table 3. In all specifications, we restrict the seller of protection to be one of the 14 largest dealers. We report the number of *effective* observations for which one buyer transacts with at least two different sellers in each fixed effect group.

[15] As discussed in Appendix A, the actual market price of the CDS contract is an *upfront* payment. For investment grade reference entities, par spreads remain the quoting convention in the marketplace. We follow this convention in working with par spreads instead of upfront prices because par spreads (approximately) eliminate the effect of contract maturity and coupon rates in measuring the sensitivity of contract value to explanatory variables. This is analogous to the widespread use of yield to maturity instead of discount price in the bond pricing literature. Yield to maturity allows for easier comparison across bonds differing in maturity and coupon rates. Furthermore, the existing literature on the pricing impact of counterparty risk (specifically, Arora, Gandhi, and Longstaff, 2012) relies on par spreads, and we want to facilitate comparison. In Internet Appendix C, we show that our empirical results are entirely robust to measuring prices in upfront points instead of par spreads.

Our benchmark specification, presented in Column 1, examines the effect of seller's credit spreads on transaction spreads for non-dealers as buyers of protection. The coefficient on the seller's credit spread is negative and statistically significant, but the economic magnitude of the coefficient value is very small, as a 100 percent increase in the seller's log spread leads to only a 0.7 percent decrease in the transaction spread. To translate the change from percentages to levels, we note that the mean level of transaction spread is about 195 basis points in the estimated sample and the mean dealer spread is about 173 basis points, and hence a 100 basis point increase in the seller's credit spread translates into about 0.6 basis point reduction in the transaction spread $\left(= 195 \times \left[\left(\frac{173+100}{173}\right)^{-0.007} - 1\right]\right)$. The median (and mode) notional value of client-facing trades in the baseline sample is \$5 million, so the 0.6 basis point pricing impact on transaction spread translates into about \$300 difference in the total per-annum cost of a median-sized trade. Our finding that the impact of seller credit spread is significant, but modest in economic magnitude, is qualitatively consistent with the finding in Arora, Gandhi, and Longstaff (2012) that a 100 basis point increase in dealer spreads translates to 0.15 basis point reduction in the quoted CDS spread. Furthermore, we find that the WWR variable enters slightly positive, the sign opposite to that predicted by the counterparty risk hypothesis. This counterintuitive finding is consistent with Arora, Gandhi, and Longstaff (2012) who find that counterparty risk is not priced for financial reference entities.

In Column 2, we use the correlation-based measure of wrong-way risk. WWR no longer enters significantly and the coefficient on the seller's spread remains small. In Columns 3–4, we restrict the sample to the set of reference entities that are ineligible for central clearing, and obtain coefficient estimates very similar to those in Columns 1–2. This suggests that clearing eligibility does not significantly affect client-dealer pricing. In Columns 5–6, we repeat the regressions for transactions in the US5 counterparty sample. The coefficients on seller credit spread become even smaller.

In Table 4, we re-estimate equation (1) on interdealer transactions. We obtain smaller negative coefficients on the seller's CDS spreads in the baseline sample in Columns 1–4, but slightly positive and insignificant coefficients in the US5 counterparty sample in Columns 5–6. For the baseline sample, an increase of 100 basis point in the seller's spread translates into only about 0.2 to 0.3 basis point reduction in the transaction spread. The coefficient on past relationship is marginally negative in the baseline sample, i.e., buyers obtain slightly more favorable prices from dealers with whom they traded more in the past. The WWR variable is insignificant in all specifications.

One potential concern with the benchmark specification is that the seller's credit spread could be correlated with other unobserved characteristics of the sellers which also affect pricing of the contract. To mitigate this concern, we add seller fixed effects α^s to control for the impact of seller's time-invariant characteristics to equation (1) as follows:

$$\log(cds_{i,t}^{s,b}) - \log(\overline{cds}_{i,t}) = \alpha_{i,t}^{b} + \alpha^{s} + \beta \log(\overline{cds}_{t-1}^{s}) + \eta WWR_{t}^{s} + \lambda Relations_{t-1}^{s,b} + \delta \log(size) + \epsilon_{i,t}^{s,b}, \tag{2}$$

We present regression results with additional seller fixed effects in Table 5. The coefficient on the seller's credit spread increases in magnitude, but remains modest.

In summary, we find significant but economically small effects for non-dealers as protection buyers, and either even smaller or insignificant effects of seller's credit spreads on transaction spreads for dealers as protection buyers from other dealers. These results are consistent with the anecdotal evidence that CSA provisions are symmetric between large dealers, but are more likely to be asymmetric in favor of dealers for client-facing transactions. Neither WWR nor past relationship affects transaction spreads in a robust manner.

3.2 Pricing Effects of Central Clearing

In this section, we examine the effects of central clearing on the pricing of CDS contracts. Selected single-name reference entities became eligible for clearing by Intercontinental Exchange (ICE) in waves beginning in December 2009. By the end of our sample period, most index constituents had been made eligible for clearing.[16]

Loon and Zhong (2014) find that central clearing significantly increases CDS spreads, and attribute this to mitigation of counterparty risk. Their finding could be seen as inconsistent with our result in Section 3.1 and those of Arora, Gandhi, and Longstaff (2012) that counterparty risk has a minimal effect on pricing. We exploit the DTCC transaction data to compare CDS spreads on centrally cleared transactions against spreads on uncleared trades on the same day and on the same reference entity. We find that transaction spreads from

[16] Campbell and Heitfield (2014) describe post-crisis reforms aimed at encouraging central clearing. The single-name index constituents that remain ineligible are primarily the European dealer banks listed in iTraxx Europe. US dealer banks are excluded from the CDX.NA.IG index, and also remain ineligible for clearing.

centrally cleared trades are actually associated with *lower* spreads than uncleared trades. We do not dispute the importance of central clearing in mitigating counterparty risk. However, we conclude that its impact on pricing is limited simply because the pricing impact of uncleared counterparty risk is itself limited.[17]

In our sample period, there were two methods by which market participants could engage in cleared trades. Under the first method, known as backload clearing, the parties initially transact bilaterally in the OTC market, and subsequently (typically on the following Friday) submit the trade to a central counterparty (CCP) for clearing. Our assumption is that the backloaded trades were designated for clearing by the counterparties at the time of the bilateral transaction. Under the second method, the trade is cleared on the same day as the initial trading date. These same-day clearing trades are often cleared at inception and executed on a swap execution facility (SEF), which matches buyer and seller anonymously. A same-day clearing trade appears in the repository data as two simultaneous transactions with a CCP as buyer on one leg and as seller on the other. As discussed in Section 2.1, non-dealers almost never clear single-name trades during our sample period, so all cleared transactions in our sample are interdealer trades.

We construct a sample of transactions on clearable reference entities using the union of the baseline and US5 counterparty samples. Of the 487,826 transactions on clearable reference entities in which either the buyer of the seller is one of the 14 largest dealers, we have 353,148 transactions in which the buyer is one of the 14 largest dealers and 392,493 transactions in which the seller is one of the 14 largest dealers. We categorize transactions into four types: (i) same-day clearing trade; (ii) backload clearing trade; (iii) uncleared OTC client-facing trade; and (iv) uncleared OTC interdealer trade. The fourth type is the omitted category in the regressions.

Table 6 presents results on how transaction characteristics affect CDS pricing. In Column 1, we estimate the effect of seller characteristics when the buyer is one of the 14 largest dealers. Holding contract, date and the buyer fixed, we find that same-day clearing trades are associated with significantly lower spreads than OTC interdealer trades, with a magnitude around 0.33 percent. Backloaded clearing trades have marginally significantly lower spreads than the OTC uncleared interdealer spreads at about 0.2 percent. In Column 2, we estimate

[17] Internet Appendix D provides a detailed analysis of why our results differ from Loon and Zhong (2014) in both the cross-section and time-series.

the effect of buyer characteristics when the seller is one of the 14 largest dealers. Holding contract, date and seller fixed, we again find that same-day clearing trades are associated with lower spreads, with a magnitude around 0.2 percent. Backloaded clearing trades do not differ significantly in spreads from OTC interdealer trades. In Column 3, we fix contract and date only and allow both buyer and seller's characteristics to enter simultaneously. Here too we find that same-day clearing trades are associated with significantly lower transaction spreads, with a magnitude around 0.3–0.4 percent. As in Column 1, spreads on backloaded clearing trades are slightly lower than on comparable interdealer OTC trades by about 0.2 percent.

Table 6 also documents a dealer pricing advantage consistent with Siriwardane (2015), who shows that the market is dominated by a handful of buyers and sellers of protection, the majority of which are dealers. Siriwardane (2015) finds that a reduction in the capital of these dealers has an impact on CDS prices. In Column 1, we find that non-dealer sellers sell to dealers at spreads about 0.6 percent lower than on comparable OTC interdealer transactions. In Column 2, we find that non-dealers buyers of protection in OTC transactions pay dealers about 0.4 percent more than dealers pay in comparable OTC interdealer transactions. Estimated dealer rents in the final specification are even larger, with magnitudes around 0.9–1 percent.

Our key finding in this analysis is that centrally cleared trades are associated with lower spreads compared with OTC uncleared interdealer trades. Possibly this reduction in spreads is due to the effects on competitive structure associated with migration from opaque bilateral OTC trading to transparent SEF trading. Clearly, however, it is opposite in sign to what would be expected if compensation for counterparty risk were a significant component in the pricing of single-name CDS.

4 Effects of Counterparty Risk on Counterparty Choice

4.1 Benchmark Specifications

In this section, we show that market participants actively manage counterparty risk by choosing counterparties of better credit quality and less subject to wrong-way risk. We also explore how characteristics of the non-dealer and of the reference entity alter the sensitivity of counterparty choice to dealer credit quality. As in Shachar (2012), we assume that OTC trades in the CDS market are initiated by the non-dealer, and that the dealer supplies liquidity upon demand. This identifying assumption is commonly imposed (explicitly or implicitly) in the empirical literature on dealer-intermediated markets (see, for example, in the context of corporate bond markets, Edwards, Harris, and Piwowar, 2007; Bessembinder, Jacobsen, Maxwell, and Venkataraman, forthcoming; Li and Schürhoff, 2014). An immediate implication is that the matching of counterparties in a transaction is determined exclusively by the choice of the non-dealer as client. In Section 4.4 we relax this assumption and our results are qualitatively similar.

Table 7 provides preliminary evidence of aversion to wrong-way risk in our baseline sample. We divide the 14 dealers by domicile (U.S. vs. foreign), and also sort the FRB-regulated reference entities into two groups by severity of WWR when the seller is a U.S. dealer. A group of reference entities composed of the US5 dealers and Wells Fargo is deemed "high WWR," and the remaining FRB-regulated reference entities are deemed "low WWR." We then calculate for each group of dealers the aggregate share of protection sold (by notional value) on the high and low WWR groups of reference entities. Panel A shows that the trading share of the US5 dealers is 44 percent when selling protection on one of the US5 entities or Wells Fargo, compared to 54 percent for other reference entities. That is, the market share of the US5 dealers is lower for the reference entities for which the US5 dealers pose the most severe wrong-way risk from the perspective of buyers of protection. Panel B of the table demonstrates that this difference is robust to excluding the period of the European debt crisis as defined in Section 2.3.[18]

We estimate McFadden's (1974) multinomial conditional logit model for the choice made by the buyer of protection among the 14 dealers in the baseline sample and five dealers in the US5 counterparty sample. In the latter case, the model-estimated choice probabilities are *conditioned* on choosing a member of the US5 set. We emphasize that this restriction does not give rise to a selection bias. A necessary condition for the consistency of the multinomial logit estimator is the independence of irrelevant alternatives (IIA). This same assumption

[18]We define the European debt crisis period from October 4, 2011, when the Belgian government announced Dexia's bailout, to July 26, 2012, when Mario Draghi announced that "the ECB is ready to do whatever it takes to preserve the euro. And believe me, it will be enough."

implies that estimates of regression coefficients (though not fixed effects) remain consistent when the sample is truncated to a restricted choice set.[19]

The probability of choosing dealer s conditional on characteristics $x_{i,t}^{s}$ is specified as

$$\Pr(y_{i,t}^{b} = s|x_{i,t}^{s}) = \frac{\exp(x_{i,t}^{s}\beta)}{\sum_{\hat{s}=1}^{D_i} \exp(x_{i,t}^{\hat{s}}\beta)}, \qquad s = 1, \ldots, D_i. \tag{3}$$

In the baseline sample, the choice set has cardinality $D_i = 13$ when the reference entity i is a US5 dealer and $D_i = 14$ otherwise, i.e. we do not give the choice of trading with dealer i when the reference entity is i. In the US5 counterparty sample, the choice set has cardinality $D_i = 4$ when the reference entity i is a US5 dealer and $D_i = 5$ otherwise. In the baseline sample, our multinomial model has 159,130 ($= 7918 \times 13 + (11{,}932 - 7918) \times 14$) observations. In the US5 counterparty sample, our multinomial model has 950,271 ($= 3919 \times 4 + (190{,}838 - 3919) \times 5$) observations. The independent regressors are: credit risk of the seller, proxied as before by the CDS spread on the seller of protection quoted on Markit on date $t-1$; wrong-way risk, measured as an indicator variable (WWR_i^s (Indicator)) or as a continous correlation (WWR_i^s (Correlation)); past relationship ($Relations_{t-1}^{s,b}$), to allow for "stickiness" in buyer-dealer relationships; a set of seller fixed effects for the D_i dealers, to allow for baseline differences in market share; and interactions between seller dummy variables and the spread on the five-year CDX.NA.IG index, to allow for the possibility that buyers may gravitate towards particular sellers when market-wide spreads are high. Results are reported in Table 8. The coefficients on seller dummy variables are omitted to respect the confidentiality of the data.

In Columns 1 and 2 we report coefficients estimated on the baseline sample for our two alternative measures of wrong-way risk. As predicted, the coefficient on seller's CDS is negative and statistically significant, i.e., customers are less likely to buy protection from a dealer whose own CDS spread is high relative to other dealers. The coefficient on either measure of WWR is large, negative and statistically significant, which shows that buyers avoid wrong-way risk in their choice of dealer. Finally, the coefficient on past relationship

[19] The IIA assumption in our setting means that the odds that a non-dealer chooses to transact with dealer A over B does not depend on whether an alternative dealer C is available. Essentially, when we use the US5 counterparty sample to estimate our model we are estimating the probability that a non-dealer chooses dealer A conditional on the non-dealer choosing from within the set of US5 dealers. The fact that the non-dealer's actual choice set includes nine other non-US dealers is irrelevant.

is large, positive and statistically significant, which is indicative of persistence in trading relationships.

To assess the economic importance of these coefficients we report marginal effects for these multinomial logit estimations in Table 9. For the baseline sample, we separately report marginal effects at sample means for the large dealers (those with unconditional transaction shares of 7–13%) and small dealers (those with unconditional transaction shares of 1–6%).[20] We find that a 100 basis point increase in a large dealer's CDS spread is associated with an average decline in the likelihood of buying protection from that dealer of 2.6 percentage points. Wrong-way risk reduces the probability by 2 percentage points. A one standard deviation increase in past-month transaction count increases the probability of selection by 4 percentage points. Relative to unconditional transaction shares of 7–13 percentage points, these effects are all of large economic magnitude.

In Columns 3 and 4 of Table 8 we report coefficients estimated on the subsample of FRB-regulated reference entities that are not eligible for clearing. The coefficients on the seller's CDS and past relationships are similar to those in Columns 1 and 2, which suggest that clearing eligibility has not had an impact on how non-dealers respond to our measure of dealer credit risk. Neither measure of WWR has a statistically significant coefficient, but this is unsurprising because the reference entities that are eligible for clearing are also those that suffer least from wrong-way risk, so dropping these observations makes it difficult to identify the impact of WWR.

In Column 5 we report coefficients estimated on the US5 counterparty sample. Consistent with our predictions and the coefficients estimated using the baseline sample, the coefficients on seller's CDS and WWR are negative and statistically significant. The coefficient on past relationships is positive, large, and statistically significant. The absolute magnitude of the coefficients on seller's CDS and WWR are smaller in the US5 counterparty sample than in the baseline sample. In the next subsection, we investigate the possibility that investors are more sensitive to credit risk when the reference entities are financial than non-financial, which may explain differences in the coefficients' estimates across samples since our US5 counterparty sample includes both financial and non-financial reference entities, while the baseline sample includes only financial reference entities.

[20] We do not report marginal effects at the dealer level due to confidentiality restrictions.

In Column 6 we report coefficients estimated on the subsample of reference entities that are not eligible for clearing. The coefficients on the seller's CDS, WWR and past relationships are similar to those in Column 5. In contrast to the baseline sample, the set of uncleared entities in the broader US5 counterparty sample could be sufficiently heterogeneous to allow identification of the effect of wrong-way risk.

4.2 Interactions with Reference Entity Characteristics

We explore how characteristics of the reference entity may affect the non-dealer's sensitivity to counterparty credit risk. First, we conjecture that non-dealer sensitivity to dealer credit risk should increase in the presence of wrong-way risk. A direct test of this hypothesis is given by introducing an interaction term between dealer CDS spread and wrong-way risk. We also consider whether sensitivity to counterparty risk is heightened for reference entities in certain sectors. Arora, Gandhi, and Longstaff (2012) conjecture that non-dealers should be more sensitive to dealer credit risk when trading financial reference entities, but did not find supporting evidence in prices. In view of the large literature on the interdependence of sovereign and bank credit risk, particularly in the wake of the European debt crisis, we consider the sovereign sector as well.

Second, we conjecture that non-dealer sensitivity to dealer credit risk should decrease with the liquidity of the reference entity. If a reference entity is liquid, a non-dealer may anticipate that it will be easier to terminate the trade with the current dealer in the future, which should make the non-dealer less reluctant to trade with a high credit risk dealer today. Conversely, the non-dealer may perceive that a trade on an illiquid reference entity will be costly to terminate in the future, and therefore that the credit exposure to the dealer would be difficult to unwind. Our metric for liquidity, taken from the DTCC public tables, is the number of dealers that executed transactions on the reference entity at least once per month.[21] Our reported results are robust to an alternative rank-order measure of liquidity provided by DTCC.[22]

[21] More precisely, DTCC counts the number of dealers that executed at least one transaction in a given month. This monthly count is reported as a quarterly average. Our source is the DTCC table on Top 1000 Single Names: Aggregated Transaction Data by Reference Entity.

[22] DTCC rank orders the top 1000 reference entities in each quarter by trade count. We construct a liquidity measure by assigning a value of 1000 to the most frequently-trade name, a value of 1 to the least-traded name on the list, and a value of 0 to reference entities not on the list.

Third, we consider how the credit risk of the reference entity may affect the buyer's sensitivity to the credit risk of the dealer counterparty. The probability of joint default by reference entity and the dealer will, *ceteris paribus*, increase with the default risk of the reference entity.[23] However, higher-risk (so called *high-yield*) reference entities may differ in other respects from lower-risk (investment grade) reference entities. In particular, to the extent that high-yield reference entities tend to default for idiosyncratic reasons, the credit risk of the reference entity may also stand in as a proxy for (lower) wrong-way risk. To avoid overweighting reference entities in severe distress, we use the log CDS spread of the reference entity as our measure of its credit risk of the reference entity, but our results are qualitatively robust to using the level of the reference entity CDS spread.

Due to the limited variation in reference entity characteristics in the baseline sample, we focus exclusively on the US5 counterparty sample. Results are reported in Table 10. In Column 1, we see that sensitivity of counterparty choice to the seller's CDS spread is increasing in wrong-way risk and decreasing in the liquidity of the reference entity. Both effects are statistically significant and large in magnitude. The coefficient on the seller CDS spread remains negative and statistically significant, but the coefficient on WWR essentially vanishes, i.e., the impact of WWR comes entirely through the interaction term. The coefficient on the interaction with the log spread of the reference entity is small in magnitude and statistically insignificant.

In Column 2 we include interactions of seller CDS spreads with indicator variables for financial and sovereign reference entities. As predicted, the buyer is more sensitive to the dealer CDS spread when trading these reference entities. Both of these interaction terms are statistically significant, but only the interaction with the financial sector is economically large. Interaction terms with liquidity and with the high-yield indicator remain statistically significant, but the effect of the interaction with WWR essentially vanishes. In Column 3, we introduce interactions of WWR with the sector indicator variables. We find that the sensitivity of buyer choice of counterparty to WWR is materially stronger when trading in financial and sovereign reference entities. Thus, the reference entity sector can be seen as complementary to the correlation-based measure in capturing wrong-way risk.

[23] Consistent with this intuition, initial margin requirements increase in the credit risk of the reference entity under current FINRA rules for initial margin on cleared CDS trades.

4.3 Interactions with Characteristics of the Client

The determinants of the buyer's choice of seller might also depend on client's own characteristics. The characteristics that we conjecture to be important are: the extent to which a buyer is captive; how often the buyer trades; and the credit exposure horizon of the buyer. We also examine differences in behavior across client institutional types (hedge funds, asset managers, insurance companies, etc.). We define a *captive client* as a non-dealer who trades more than 60 percent of the time with one dealer. Such a client may have a strong relationship with the favored dealer in other markets. Since we cannot observe these relationships directly, we infer from revealed preference in the transaction records. Another possibility is that a captive client may be limited in the number of dealers with which it maintains an ISDA Master Agreement. Since trading can take place only when such an agreement is in place, it may be that captive clients simply maintain few active agreements and therefore have a smaller choice set.[24] We expect captive clients to be less sensitive to dealer credit risk.

The frequency with which a non-dealer trades may influence the non-dealer's sensitivity to counterparty credit risk. The predicted sign is ambiguous. On the one hand, a frequent trader may be more likely to have favorable CSA terms for collateral exchange, in which the trader should be less sensitive to dealer credit risk. On the other hand, trade frequency may stand as a proxy for the level of sophistication in risk management practices, in which case a frequent trader may be more attuned to counterparty risk. We define a frequent trader as a non-dealer who is in the upper 5^{th} percentile of the distribution in terms of the number of transactions.[25] Frequent traders account for 66.2% of transactions involving a non-dealer buyer of protection in the baseline sample, and 61.4% of such transactions in the US5 counterparty sample.

Finally, we expect that a buyer intending to hold a CDS position for a short period of time should be less sensitive to counterparty credit risk than a buyer intending to hold a position for a long period of time. We cannot measure intention directly, so we construct a proxy based on observed behavior. We define an indicator variable equal to one if the

[24] Captive clients account for under 7.4% of transactions involving a non-dealer buyer of protection in the baseline sample, and under 9.2% of such transactions in the US5 counterparty sample. Thus, these clients collectively command a fairly small weight in the overall sample.

[25] Our results are robust to defining a frequent trader in terms of the notional value traded rather than the number of transactions.

non-dealer terminates or assigns at least fifty percent of its new trades within 28 days of the original trade date. These clients, whom we label *short-term credit exposure clients*, account for 34.8% of transactions involving a non-dealer buyer of protection in the baseline sample, and 24.4% of such transactions in the US5 counterparty sample.

We re-estimate the benchmark choice model including interactions of the dealer's CDS spread with indicator variables for captive trader, frequent trader, and short-term credit exposure trader. Results for the baseline sample are reported in Column 1 of Table 11a. Consistent with our predictions, captive buyers of protection and buyers with short-term credit exposure are less sensitive to dealer CDS spread. Coefficients are statistically significant and large in magnitude. Qualitatively similar results are found for the US5 counterparty sample, and reported in Column 1 of Table 11b. In addition, the results in the US5 counterparty sample show that frequent traders appear to be more sensitive to counterparty credit risk, which is consistent with the story that frequent traders are more likely to employ sophisticated risk management practices. However this last result is not robust across samples. As argued above, it is not obvious *a priori* whether frequent traders would be more or less sensitive to counterparty credit risk, so we are not surprised by the lack of robustness.

We next consider whether sensitivity to counterparty risk varies across institutional class. Certain types of institutional investors may be bound by regulation or investor prospectus to buy-and-hold trading strategies. Insurance companies, pension plans, non-financial corporations, and financial services firms are likely to be of this type, and firms of these types are often believed to be relatively unsophisticated in risk management practices. In Table 12, we see that firms in these institutional classes tend overwhelmingly to be long-term in credit exposure. However, we also see that these firms account for a small share of total transactions in the sample. The most active market participants are hedge funds, asset managers, and non-dealer banks. These three classes (especially hedge funds) are heterogeneous in trading strategy and in sophistication, so we do not expect institutional class to capture much variation in trading behavior.

We re-estimate the benchmark choice model including interactions of the dealer's CDS spread with indicator variables for hedge funds, asset managers, and non-dealer banks. The omitted category includes the investor types we take to be buy-and-hold in strategy and/or less sophisticated in risk management: insurance companies, pension plans, non-financial corporations, financial services firms, and small firms which are "unclassified." For the baseline sample, results in Column 2 of Table 11a suggest that asset managers and bank non-dealers are more sensitive to credit risk than non-dealers of the omitted category. Differences between asset managers, bank non-dealers, and hedge funds are not statistically significant. For the US5 counterparty sample, results in Column 2 of Table 11b indicate that asset managers are more sensitive to credit risk than non-dealers of all other types.

4.4 Additional Robustness Exercises

We conduct a variety of robustness exercises, and report results in Tables 13a and 13b for the baseline and US5 counterparty samples, respectively. For ease of comparison, Column 1 in each table repeats the benchmark specification from Table 8.

We begin by relaxing the assumption that the dealer is a passive provider of liquidity. We believe the assumption is true in a first-order sense that the client chooses the dealer and not the other way around, and do not perceive this view as controversial. Indeed, providing liquidity to end-investors is the central function of a dealer, and this assumption is widely imposed (explicitly or implicitly) in the theoretical and empirical literature on OTC dealer-intermediated markets. For example, in the corporate bond literature it is commonly assumed that dealers provide liquidity.[26] Nevertheless, we have taken precautions and added robustness checks aimed at addressing this issue. First, as discussed in Appendix A, the baseline and US5 samples exclude a single non-dealer that accounted for a notable share of protection sold in the CDS market. One concern was that a client of this size could serve the dealers as a "seller of last resort," which would violate the spirit of the identifying assumption. Second, we include in our choice model the dealer's inventory holdings of the reference entity, measured as of Friday close-of-business in the week prior to the transaction. As shown in Column 2 of Tables 13a and 13b, the inclusion of dealer inventory holdings affects neither the statistical nor the economical significance of the key variables of the model, i.e., the dealer's credit risk, wrong-way risk and past relationships. Third, we include as a control variable a proxy for the dealer's prevailing pricing aggressiveness at the time of the transaction. This is intended to address a concern that the risk of dealer default may somehow affect how

[26] In addition, we note that during our sample period, proprietary trading by the dealers was very much on the wane, especially relative to the pre-crisis period. The beginning of our sample period in January 2010 coincides with the request by President Obama to include the Volcker Rule in the Dodd-Frank Act. Legally, the Volcker Rule was implemented about mid-way through our sample, but the dealer banks were moving into compliance well before that date.

aggressively the dealer pursues CDS trade flow from clients. For a given dealer at date t, we identity the set of baseline sample client-facing transactions involving the dealer and any client for the 28 business days prior to date t.[27] We measure pricing aggressiveness as the average within this set of transactions of the difference between the log par spread and the corresponding log Markit par spread. As shown in Column 3 of Tables 13a and 13b, coefficients on this aggressiveness measure are insignificant, and our coefficients on variables of primary interest remain virtually unchanged in both our baseline sample and the US5 counterparty sample.

In Column 4 we control for the size of the dealers as banks, as measured by the log of the equity market capitalization of the holding company. In Column 5, we allow for time-variation in the response. Specifically, we introduce interactions of dealer CDS spread with dummy variables for the European debt crisis and the Volcker Rule.[28] We find that the sign and significance of these additional controls may vary across sample, but in all cases the coefficients on the variables of primary interest (i.e., dealer CDS, WWR, and past relationship) remain robust.

In Columns 6–7, we consider an alternative measure for dealer default risk. The dealer's five year probability of default (PD) is estimated by Kamakura Corporation using the hazard rate model of Chava and Jarrow (2004). Whereas the CDS spread represents an assessment of credit risk under the pricing measure $\mathbb{Q}$, the PD is an assessment under the empirical measure $\mathbb{P}$. When we replace dealer CDS spread with the PD measure (Column 6), the coefficient on PD is qualitatively similar in magnitude and significance to the coefficient on dealer CDS spread in the benchmark specification. When we include both measures (Column

[27] If there were fewer than 28 transactions in the last month, then we estimate the average using the past 28 transactions, requiring a minimum of 10 transactions.

[28] The European debt crisis is defined as an indicator variable for the period October 4, 2011 to July 26, 2012. Evaluating the impact the Volcker Rule may have had is complicated by the fact that there is no unambiguous choice of Volcker Rule event date. The Dodd-Frank Act was signed into law on July 21, 2010, but was implemented in phases, with some effective dates as long as five years after signing (e.g., for banks regulated by the Federal Reserve, Volcker Rule was fully in effect on Jul 22, 2015). Some studies on the impact of Volcker Rule on corporate bond liquidity use a date close to the end of our sample, e.g., Bessembinder, Jacobsen, Maxwell, and Venkataraman (forthcoming) use July 2012 as the event date, and others use a date outside our sample period, such as April 1, 2014 (Bao, O'Hara, and Zhou, forthcoming). The Volcker Rule indicator variable we use is equal to one from November 7, 2011 (the date on which the rule was published in the Federal Register) to the end of our sample.

7), both are negative and statistically significant. The dealer CDS spread dominates in the baseline sample, whereas the PD dominates in the US5 counterparty sample.

Throughout the paper, we have been focused on the effects of counterparty risk from the perspective of non-dealers buying protection. Due to the asymmetric loss exposure, we expect counterparty risk to matter more for protection buyers than for protection sellers. In Internet Appendix E, we repeat the benchmark analysis for pricing and counterparty choice from the perspective of non-dealers as protection sellers. Consistent with our conjecture, we find that the effects of dealers' credit risk on pricing and on counterparty choice are more muted and less robust when the non-dealer is the seller of protection.

5 Conclusion

In this paper, we study how market participants price and manage counterparty credit risk in the CDS market. Using confidential transaction data from the DTCC, we find negligible effects of counterparty risk on the pricing of CDS contracts. However, the lack of pricing response to counterparty risk does not mean that counterparties of different credit worthiness are treated equally. We provide the first direct empirical evidence that dealer credit risk has a large effect on swap clients' choice of counterparty. Our results demonstrate that participants in the CDS market manage counterparty risk by buying protection preferentially from counterparties of lower credit risk and lesser "wrong-way" correlation with the reference entity.

These results are qualitatively consistent with the experience of Lehman and Bear Stearns in 2008 in which they could not find willing counterparties and suggest that credit rationing may arise under wider circumstances than previously recognized. These results also have implications for the newly agreed international rules which require bilateral provision of initial margin to be held in segregated third-party custodial accounts. Since these provisions will dramatically reduce counterparty losses in the event of dealer default, non-dealers should become less sensitive to dealer credit risk in choosing a counterparty, and the likelihood of a counterparty "run" in OTC derivatives should be reduced.

APPENDIX

A Sample Construction

Throughout our analyses, we consider only new, price-forming trades. Specifically, we drop novations, terminations, intra-family housekeeping transactions, and records resulting from trade compression. For a very small number of observations, the seller of the transaction is also the reference entity. Such contracts pose an extreme form of wrong-way risk (termed *specific* wrong-way risk in Basel capital rules), so it is somewhat puzzling that this is ever observed. We drop these few observations.

To avoid undue influence of a single market participant on the results, we exclude trades involving the largest non-dealer participant in the CDS market during our sample period. This non-dealer appears as seller of protection in over 25% of client-facing transactions in our baseline sample (described below), bur rarely as buyer.[29] Results for our pricing models and for models of counterparty choice by non-dealer buyers of protection are robust to including this non-dealer in the sample. To maintain confidentiality of the data, we cannot report results both with and without this non-dealer in sample, as this would reveal trading behavior specific to this firm.

Throughout the sample period of 2010–2013, CDS have been traded on the basis of fixed coupon rate with an upfront payment to compensate for the non-zero initial value of the contract. We use initial payment, total notional amount and the ISDA interest accrual convention to compute the upfront points associated with each transaction, and then apply the program provided by ISDA for conversion of upfront points into par spreads. We drop observations if a valid par spread cannot be constructed or the underlying cannot be matched to a Markit spread for the same terms on the same date. Further, to ensure the comparison between spreads is valid, we drop trades that do not adhere to standard ISDA market conventions on reporting protocols, coupon rates, credit event settlement procedures, and other administrative details.

To mitigate any bias associated with illiquidity, we drop from the baseline sample five reference entities that are traded less than once per month on average. These restrictions

[29] Excluding this market participant reduces the Herfindahl index of concentration among sellers in notional value transacted from 500 to 389. The effect on concentration among buyers is much smaller.

leave us transactions on 12 reference entities.[30] The five reference entities that are dropped account for only 81 transactions in total. Thus it is not surprising that our results are qualitatively similar when we include these illiquid reference entities. We do not impose minimum trading frequency requirements on the US5 counterparty sample.

To ensure our results are not driven by large pricing outliers, we drop observations for which the absolute difference between the logs of the Markit and DTCC spreads is greater than 0.3. This cutoff corresponds to the 98.5 percentile of absolute differences in the baseline sample. The baseline and US5 counterparty samples reduce to 83,335 and 1,516,625 transactions, respectively. Our regression results are robust to relaxing this restriction to a cap of 0.5.

The US5 counterparty sample contains some highly distressed reference entities which may be subject to significant intraday volatility. Therefore, we drop transactions for which the Markit par spread on the contract on that date is above 1000 basis points. This restriction leaves the baseline sample unchanged, and reduces the US5 counterparty sample to 1,435,205 transactions on 1635 single-name reference entities. Regression results are robust to relaxing this restriction to a cap of 5000 basis points.

[30]The 12 entities are Ally Financial, American Express, Bank of America, Capital One Bank, Capital One Financial Corporation, CIT Group, CitiGroup, JPMorgan Chase, Metlife, Morgan Stanley, Goldman Sachs Group, and Wells Fargo.

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Table 1: Summary Statistics for Client-Facing Transactions

	(1) Baseline sample	(2) US5 CP sample
Number of reference entities	12	1,248
Of which ...		
Financial	12	259
Sovereign	0	76
US domecile	12	512
Eligible for clearing	4	326
Number of non-dealers	196	828
Of which ...		
Short horizon	52	90
Captive	24	305
Frequent trader	53	66
Hedge fund	72	247
Asset manager	70	205
Non-dealer bank	26	76
Other (Insurance, pension plans, non-financial firms, financial services)	28	300
Number of transactions between non-dealers and one of the largest dealers	11,932	190,838
Of which ...		
A US5 dealer appear as the reference entity	7,918	3,919
Reference entity is not eligible for clearing	10,295	133,567

Notes: Our samples consist of client-facing transactions from 2010 to 2013 in which the non-dealer is buying protection from a dealer counterparty. In column (1) we tabulate transactions on FRB-regulated reference entities. In column (2) we tabulate transactions in which the seller of protection is a US5 dealer.

Table 2: Summary Statistics for Pricing Analysis

Panel A: Summary statistics for all transaction spreads and Markit quotes in baseline sample ($N = 83,401$)									
Percentile	p1	p5	p10	p25	p50	p75	p90	p95	p99
$cds^{DTCC} - \overline{cds}$	-28.57	-11.56	-6.74	-2.09	0.79	4.23	9.14	13.67	29.00
$\lvert cds^{DTCC} - \overline{cds}\rvert$	0.05	0.24	0.48	1.30	3.25	6.83	12.77	18.91	37.94
$\log(cds^{DTCC}) - \log(\overline{cds})$	-0.18	-0.09	-0.05	-0.01	0.01	0.03	0.07	0.07	0.07
$\lvert\log(cds^{DTCC}) - \log(\overline{cds})\rvert$	0.00	0.00	0.00	0.01	0.02	0.05	0.10	0.14	0.24
Panel B: Summary statistics for contracts with at least ten trades per day in baseline sample($N = 23,209$)									
Percentile	p1	p5	p10	p25	p50	p75	p90	p95	p99
$\sigma_t[\log(cds^{DTCC}) - \log(\overline{cds})]$	0.0000	0.0000	0.0043	0.0088	0.0143	0.0223	0.0361	0.0478	0.0880
No. of transactions per day	10.0	10.0	10.0	11.0	13.0	18.0	25.0	31.0	56.0
Average No. of sellers per buyer	1.0	1.0	1.2	1.5	1.9	2.4	2.9	3.3	4.4
Panel C: Summary statistics for all transaction spreads and Markit quotes in US5 counterparty sample($N = 1,519,410$)									
Percentile	p1	p5	p10	p25	p50	p75	p90	p95	p99
$cds^{DTCC} - \overline{cds}$	-39.34	-15.52	-8.98	-2.65	0.85	5.77	15.13	25.60	63.00
$\lvert cds^{DTCC} - \overline{cds}\rvert$	0.06	0.27	0.56	1.57	4.18	9.84	20.69	32.30	71.29
$\log(cds^{DTCC}) - \log(\overline{cds})$	-0.21	-0.11	-0.07	-0.02	0.01	0.04	0.09	0.14	0.24
$\lvert\log(cds^{DTCC}) - \log(\overline{cds})\rvert$	0.00	0.00	0.00	0.01	0.03	0.06	0.12	0.17	0.26
Panel D: Summary statistics for contracts with at least ten trades per day in US5 counterparty sample ($N = 182,502$)									
Percentile	p1	p5	p10	p25	p50	p75	p90	p95	p99
$\sigma_t[\log(cds^{DTCC}) - \log(\overline{cds})]$	0.0000	0.0000	0.0000	0.0067	0.0121	0.0204	0.0336	0.0458	0.0913
No. of transactions per day	10.0	10.0	10.0	11.0	13.0	18.0	26.0	34.0	60.0
Average No. of sellers per buyer	1.0	1.0	1.0	1.2	1.7	2.2	2.8	3.3	4.4

Notes: Our sample period is from 2010 to 2013. In Panel A and C, we report differences between DTCC transaction spreads, cds_t^{DTCC}, and Markit quotes, $\overline{cds}_t$. The spreads are expressed in basis points. The log differences in the two spreads are also reported. In Panel B and D, we report distribution of standard deviation for transaction spreads on the same contract within the same day, given that there are at least ten trades per day on the same contract. In addition, we report the distribution of the number of sellers for the same buyer on the same day.

Table 3: Effects of Seller CDS Spreads on Log Par Spread Differentials (Client-Dealer Transactions)

	(1) Baseline	(2) Baseline	(3) Baseline not eligible for clearing	(4) Baseline not eligible for clearing	(5) US5 CP	(6) US5 CP not eligible for clearing
Seller's CDS	-0.00710**	-0.00561*	-0.00817**	-0.00663**	-0.00287**	-0.00398***
	(0.00351)	(0.00338)	(0.00359)	(0.00337)	(0.00124)	(0.00153)
Wrong-Way Risk (Indicator)	0.00538**		0.00561***			
	(0.00213)		(0.00216)			
Wrong-Way Risk (Correlation)		0.0170		0.0238**	-0.00278	-0.0173
		(0.0107)		(0.0105)	(0.0125)	(0.0109)
Past Relationship	0.000271	-0.000471	0.00236	0.00159	0.00503***	0.00555***
	(0.00417)	(0.00417)	(0.00440)	(0.00437)	(0.00152)	(0.00169)
No. of effective obs.	3,385	3,385	2,891	2,891	30,680	22,018
No. of transactions	11,932	11,932	10,295	10,295	190,838	133,567

Notes: In all columns, we consider only transactions where the seller is one of the 14 largest dealers and the buyer is a client. The dependent variable is difference between the the DTCC par spread and the Markit par spread. In Columns 1-2, we use transactions in the baseline sample. In Columns 3-4, we use transactions on reference entities that are eligible for clearing in the baseline sample. In Columns 5, we use transactions in the US5 counterparty sample. In Column 6, we use transactions on reference entities that are eligible for clearing in the US5 counterparty sample. In each regression, we include contract-buyer-date fixed effects. The number of effective observations is reported for buyers facing at least two different sellers for the same contract on a single trading date. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Effects of Seller CDS Spreads on Log Par Spread Differentials (Interdealer Transactions)

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Baseline	Baseline	Baseline	US5 CP	US5 CP
			not eligible for clearing	not eligible for clearing		not eligible for clearing
Seller's CDS	-0.00150*	-0.00175**	-0.00146	-0.00171**	0.000290	0.000324
	(0.000862)	(0.000829)	(0.000893)	(0.000858)	(0.000347)	(0.000381)
Wrong-Way Risk (Indicator)	-0.000899		-0.000915			
	(0.000587)		(0.000590)			
Wrong-Way Risk (Correlation)		-0.00333		-0.00348	0.00101	0.00241
		(0.00230)		(0.00233)	(0.00153)	(0.00170)
Past Relationship	-0.00708*	-0.00699*	-0.00702*	-0.00691*	0.00254	0.000852
	(0.00379)	(0.00381)	(0.00387)	(0.00389)	(0.00203)	(0.00220)
No. of effective obs.	18,237	18,237	17,465	17,465	201,257	160,186
No. of transactions	52,682	52,682	48,385	48,385	960,348	703,871

Notes: In all columns, we consider only transactions where the both the buyer and seller are one of the largest 14 dealers. The dependent variable is difference between the DTCC upfront point and the Markit upfront point. In Columns 1-2, we use transactions in the baseline sample. In Columns 3-4, we use transactions on reference entities that are eligible for clearing in the baseline sample. In Columns 5, we use transactions in the US5 counterparty sample. In Column 6, we use transactions on reference entities that are eligible for clearing in the US5 counterparty sample. In each regression, we include contract-buyer-date fixed effects in all regressions.The number of effective observations is reported for buyers facing at least two different sellers for the same contract on a single trading date. Log notional of the trade is included as a control in all regressions. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Effects of Seller CDS Spreads on Log Par Spread Differentials with Additional Seller Fixed Effects (Client-Dealer Transactions)

	(1) Baseline	(2) Baseline	(3) Baseline not eligible for clearing	(4) Baseline not eligible for clearing	(5) US5 CP	(6) US5 CP not eligible for clearing
Seller's CDS	-0.0183***	-0.0193***	-0.0194***	-0.0199***	-0.00352	-0.00741**
	(0.00559)	(0.00570)	(0.00654)	(0.00659)	(0.00395)	(0.00367)
Wrong-Way Risk (Indicator)	0.0120***		0.00415			
	(0.00410)		(0.00354)			
Wrong-Way Risk (Correlation)		0.0384*		0.0240	-0.00618	-0.0198*
		(0.0230)		(0.0254)	(0.0119)	(0.0119)
Past Relationship	0.00148	0.00135	0.00372	0.00386	0.00272*	0.00304*
	(0.00436)	(0.00438)	(0.00462)	(0.00455)	(0.00156)	(0.00172)
No. of effective obs.	3,385	3,385	2,891	2,891	30,680	22,018
No. of transactions	11,932	11,932	10,295	10,295	190,838	133,567
Fixed effects	Contract × Buyer × Date, Seller					

Notes: In all columns, we consider only transactions where the seller is one of the 14 largest dealers and the buyer is a client. The dependent variable is difference between the the DTCC par spread and the Markit par spread. In Columns 1-2, we use transactions in the baseline sample. In Columns 3-4, we use transactions on reference entities that are eligible for clearing in the baseline sample. In Columns 5, we use transactions in the US5 counterparty sample. In Column 6, we use transactions on reference entities that are eligible for clearing in the US5 counterparty sample. The two panels differ by the fixed effect specifications. In each regression, we include contract-buyer-date fixed effects and additional seller fixed effects. The number of effective observations is reported for buyers facing at least two different sellers for the same contract on a single trading date. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Effects of Clearing and Counterparty Characteristics on Log Spread Differentials

	(1) Seller	(2) Buyer	(3) Pair
Seller CCP	-0.00330***		-0.00361***
	(0.000966)		(0.000461)
Buyer CCP		-0.00175*	-0.00310***
		(0.000907)	(0.000463)
Backload clear	-0.00178**	-0.000206	-0.00189***
	(0.000834)	(0.000757)	(0.000405)
Seller non-dealer	-0.00553***		-0.00964***
	(0.00101)		(0.000557)
Buyer non-dealer		0.00460***	0.00897***
		(0.000844)	(0.000449)
Number of observations	353,148	392,493	487,826
Fixed effects	Contract × Date × Buyer	Contract × Date × Seller	Contract × Date

Notes: This table shows effects of counterparty characteristics and clearing on transaction spreads. In Column 1 we hold time and the buyer fixed and estimate effects of seller's characteristics. The buyer is one of the 14 largest dealers. In Column 2 we hold time and the seller fixed and estimate effects of buyer's characteristics. The seller is one of the 14 largest dealers. In Columns 3 we hold time fixed and jointly estimate effects of buyer's and seller's characteristics. Either the buyer of the seller is one of the 14 largest dealers. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7: Wrong-Way Risk

Panel A: Baseline sample		
	US sellers' trading shares	Foreign sellers' trading shares
Reference entity is a US5 dealer or Wells Fargo	0.44	0.56
Reference entity is not a US5 dealer nor Wells Fargo	0.54	0.46
Panel B: Baseline sample excluding European debt crisis		
	US sellers' trading shares	Foreign sellers' trading shares
Reference entity is a US5 dealer or Wells Fargo	0.45	0.55
Reference entity is not a US5 dealer nor Wells Fargo	0.58	0.42

Notes: This table tabulates trading shares in the baseline sample for reference entities that are highly correlated with U.S. seller's credit risk (the reference entity is a US5 dealer or Wells Fargo) and for reference entities that are less correlated with U.S. seller's credit risk (the reference entity is neither a US5 dealer nor Wells Fargo). We restrict the reference entities to FRB regulated reference entities. In Panel A we use the full baseline sample. In Panel B we use the baseline sample and exclude observations from October 4, 2011, to July 26, 2012.

Table 8: Counterparty Choice of Non-dealers Buying Protection from Dealers

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Baseline	Baseline	Baseline	US5 CP	US5 CP
			not eligible for clearing	not eligible for clearing		not eligible for clearing
Seller's CDS	-0.280***	-0.277***	-0.269***	-0.267***	-0.158***	-0.170***
	(0.0397)	(0.0397)	(0.0425)	(0.0425)	(0.0106)	(0.0127)
Wrong-Way Risk (Indicator)	-0.217***		-0.0673			
	(0.0474)		(0.0692)			
Wrong-Way Risk (Correlation)		-0.753***		-0.302	-0.545***	-0.613***
		(0.248)		(0.280)	(0.0737)	(0.0848)
Past Relationship	3.476***	3.478***	3.661***	3.662***	2.456***	2.501***
	(0.0497)	(0.0497)	(0.0542)	(0.0542)	(0.0107)	(0.0126)
Number of observations	159,130	159,130	136,212	136,212	950,271	663,916
Number of transactions	11,932	11,932	10,295	10,295	190,838	133,567
Number of buyers	196	196	195	195	828	800
Pseudo R-squared	0.383	0.383	0.389	0.389	0.425	0.434

Notes: We include seller fixed effects and the spread of the CDX.NA.IG index interacted with an indicator variable for each seller as controls. In columns (5) and (6) we also include a fixed effect for U.S. domicile reference entities. We use two different measures of wrong-way risk. One measure is an indicator variable equal to one if the seller of protection is a U.S. dealer and the reference entity is one of the US5 dealers or Wells Fargo. The other measure is the correlation between the weekly change in log CDS spread on the reference entity and on the selling dealer estimated using a rolling five-year window. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9: Marginal Effects of the Buyer Choice Model

Range of probability of choice	(1) Average change in probability given a 100 bp change in seller CDS	(2) Average change in probability from no WWR to WWR	(3) Average change in probability given a 1 std. dev. change in past relationship
Baseline sample			
0.13-0.07	-0.026	-0.020	0.039
0.06-0.01	-0.010	-0.008	0.015
Baseline sample excluding reference entities eligible for clearing			
0.13-0.07	-0.025	-0.006	0.040
0.06-0.01	-0.010	-0.003	0.016
US5 CP sample			
0.30-0.15	-0.010	-0.016	0.070
US5 CP sample excluding reference entities eligible for clearing			
0.30-0.15	-0.009	-0.017	0.066

Notes: Marginal effects are based on coefficients reported in Table 8.

Table 10: Counterparty Choice of Non-dealers Buying Protection from Dealers: Characteristics of the Reference Entity

	(1) US5 CP sample	(2) US5 CP sample	(3) US5 CP sample
Seller's CDS	-0.168***	-0.192***	-0.192***
	(0.0384)	(0.0394)	(0.0395)
Seller's CDS x log(Reference Entity's CDS)	-0.00781	0.00892	0.00886
	(0.00618)	(0.00629)	(0.00629)
Seller's CDS x WWR (Correlation)	-0.321***	-0.0151	-0.0139
	(0.0352)	(0.0412)	(0.0412)
Seller's CDS x Liquidity of Reference Entity	0.0147***	0.00470***	0.00475***
	(0.00148)	(0.00156)	(0.00156)
Seller's CDS x Reference Entity is Financial		-0.197***	-0.199***
		(0.0207)	(0.0207)
Seller's CDS x Reference Entity is Sovereign		-0.0499***	-0.0512***
		(0.0192)	(0.0192)
WWR (Correlation)	0.0882	-0.406***	-0.217*
	(0.0992)	(0.108)	(0.119)
WWR (Correlation) x Reference Entity is Financial			-0.534***
			(0.174)
WWR (Correlation) x Reference Entity is Sovereign			-0.782**
			(0.334)
Past Relationship	2.456***	2.437***	2.437***
	(0.0107)	(0.0107)	(0.0107)
Number of observations	950,271	950,271	950,271
Number of transactions	190,838	190,838	190,838
Number of buyers	828	828	828
Pseudo R-squared	0.425	0.427	0.427

Notes: We include seller fixed effects, U.S. domicile reference entity fixed effect and the spread of the CDX.NA.IG index interacted with an indicator variable for each seller as controls. Wrong-way risk is measured with the correlation between the weekly change in log CDS spread on the reference entity and on the selling dealer estimated using a rolling five-year window. Liquidity of the reference entity, drawn from DTCC public tables, is the number of dealers that executed transactions on the reference entity at least once per month. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 11a: Counterparty Choice of Non-dealers Buying Protection from Dealers (Baseline Sample): Characteristics of Non-dealers

	(1)	(2)	(3)
	Baseline sample	Baseline sample	Baseline sample
Seller's CDS	-0.408***	-0.137*	-0.224***
	(0.0506)	(0.0804)	(0.0827)
Seller's CDS x Buyer is Captive	0.301***		0.322***
	(0.0710)		(0.0714)
Seller's CDS x Buyer is a Frequent Trader	0.00387		0.0438
	(0.0434)		(0.0465)
Seller's CDS x Buyer has Short-Term Credit Exposure	0.238***		0.242***
	(0.0391)		(0.0407)
Seller's CDS x Buyer is a Hedge Fund		-0.118	-0.221***
		(0.0777)	(0.0845)
Seller's CDS x Buyer is an Asset Manager		-0.184**	-0.215**
		(0.0800)	(0.0839)
Seller's CDS x Buyer is a Bank Non-dealer		-0.211**	-0.288***
		(0.0941)	(0.0989)
Wrong-Way-Risk (indicator variable)	-0.230***	-0.218***	-0.229***
	(0.0474)	(0.0474)	(0.0474)
Past Relationship	3.445***	3.488***	3.451***
	(0.0499)	(0.0499)	(0.0500)
Number of observations	159,130	159,130	159,130
Number of transactions	11,932	11,932	11,932
Number of buyers	196	196	196
Pseudo R-squared	0.384	0.384	0.384

Notes: We include seller fixed effects, U.S. domicile reference entity fixed effect and the spread of the CDX.NA.IG index interacted with an indicator variable for each seller as controls. Wrong-way risk is measured with the correlation between the weekly change in log CDS spread on the reference entity and on the selling dealer estimated using a rolling five-year window. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 11b: Counterparty Choice of Non-dealers Buying Protection from Dealers (US5 CP Sample): Characteristics of Non-dealers

	(1) US5 CP sample	(2) US5 CP sample	(3) US5 CP sample
Seller's CDS	-0.170***	-0.120***	-0.152***
	(0.0130)	(0.0210)	(0.0223)
Seller's CDS x Buyer is Captive	0.0814***		0.0760***
	(0.0174)		(0.0178)
Seller's CDS x Buyer is a Frequent Trader	-0.0320***		-0.0257**
	(0.0103)		(0.0106)
Seller's CDS x Buyer has Short-Term Credit Exposure	0.113***		0.101***
	(0.0110)		(0.0112)
Seller's CDS x Buyer is a Hedge Fund		-0.00794	0.00303
		(0.0199)	(0.0210)
Seller's CDS x Buyer is an Asset Manager		-0.0869***	-0.0558***
		(0.0201)	(0.0213)
Seller's CDS x Buyer is a Bank Non-dealer		0.0184	0.0418
		(0.0253)	(0.0261)
Wrong-Way-Risk (Correlation)	-0.554***	-0.561***	-0.565***
	(0.0737)	(0.0737)	(0.0738)
Past Relationship	2.452***	2.455***	2.452***
	(0.0107)	(0.0107)	(0.0107)
Number of observations	950,271	950,271	950,271
Number of transactions	190,838	190,838	190,838
Number of buyers	828	828	828
Pseudo R-squared	0.425	0.425	0.425

Notes: We include seller fixed effects, U.S. domicile reference entity fixed effect and the spread of the CDX.NA.IG index interacted with an indicator variable for each seller as controls. Wrong-way risk is measured with the correlation between the weekly change in log CDS spread on the reference entity and on the selling dealer estimated using a rolling five-year window. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 12: Number of transactions done by different types of clients

	No. Transactions	Percent	No. Transactions	Percent	No. Transactions	Percent
	Short-term exposure		Captive clients		Frequent traders	
Asset Managers	10,904	23%	4,462	26%	47,238	40%
Non-dealer Banks	3,075	7%	477	3%	7,369	6%
Financial Services	39	0%	156	1%	0	0%
Hedge Funds	31,285	67%	8,334	48%	61,354	52%
Insurance	0	0%	16	0%	1,169	1%
Non-financial Corporations	0	0%	9	0%	0	0%
Pension Plans	75	0%	13	0%	0	0%
Unclassified	1,094	2%	4,018	23%	0	0%
Total	46,472		17,485		117,130	
	Long-term exposure		Non-captive clients		Non-frequent traders	
Asset Managers	60,481	42%	66,923	39%	24,147	33%
Non-dealer Banks	10,959	8%	13,557	8%	6,665	9%
Financial Services	1,414	1%	1,297	1%	1,453	2%
Hedge Funds	61,617	43%	84,568	49%	31,548	43%
Insurance	1,939	1%	1,923	1%	770	1%
Non-financial Corporations	30	0%	21	0%	30	0%
Pension Plans	1,048	1%	1,110	1%	1,123	2%
Unclassified	6,878	5%	3,954	2%	7,972	11%
Total	144,366		173,353		73,708	

Notes: Our sample period is from 2010 to 2013. The number of transactions is based on the US5 counterparty sample when client is a buyer.

Table 13a: Counterparty Choice of Non-dealers Buying Protection from Dealers (Baseline Sample): Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Seller's CDS	-0.280***	-0.279***	-0.303***	-0.228***	-0.155***		-0.245***
	(0.0397)	(0.0397)	(0.0410)	(0.0439)	(0.0510)		(0.0408)
Seller's Net Positions		0.0477					
		(0.0327)					
Seller's Price Aggressiveness			-1.036				
			(0.745)				
Seller's Log(Equity Market Cap)				0.253***			
				(0.0980)			
Seller's CDS x I(European Debt Crisis)					0.000221		
					(0.000474)		
Seller's CDS x I(Volcker Rule)					-0.00185***		
					(0.000472)		
Seller's 5-Year Probability of Default						-0.211***	-0.143***
						(0.0411)	(0.0418)
WWR (Indicator)	-0.217***	-0.222***	-0.226***	-0.220***	-0.215***	-0.218***	-0.219***
	(0.0474)	(0.0475)	(0.0487)	(0.0473)	(0.0474)	(0.0473)	(0.0473)
Past Relationship	3.476***	3.479***	3.505***	3.470***	3.478***	3.485***	3.478***
	(0.0497)	(0.0499)	(0.0524)	(0.0497)	(0.0497)	(0.0497)	(0.0496)
Number of observations	159,130	158,794	148,813	159,130	159,130	159,130	159,130
Number of transactions	11,932	11,932	11,932	11,932	11,932	11,932	11,932
Number of buyers	196	196	196	196	196	196	196
Pseudo R-squared	0.383	0.385	0.423	0.384	0.384	0.383	0.384

Notes: We include seller fixed effects and the spread of the CDX.NA.IG index interacted with an indicator variable for each seller as controls. Wrong-way risk is measured with an indicator variable equal to one if the seller of protection is a U.S. dealer and the reference entity is one of the US5 dealers or Wells Fargo. European debt crisis starts on October 4, 2011 and ends on July 26, 2012. Volcker rule starts on November 7, 2011. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 13b: Counterparty Choice of Non-dealers Buying Protection from Dealers (US5 CP Sample): Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Seller's CDS	-0.158***	-0.163***	-0.150***	-0.181***	-0.157***		-0.0905***
	(0.0106)	(0.0107)	(0.0109)	(0.0123)	(0.0150)		(0.0128)
Seller's Net Positions		0.0353***					
		(0.00809)					
Seller's Price Aggressiveness			-0.135				
			(0.211)				
Seller's Log(Equity Market Cap)				-0.120***			
				(0.0321)			
Seller's CDS x I(European Debt Crisis)					0.000875***		
					(0.000134)		
Seller's CDS x I(Volcker Rule)					-0.000903***		
					(0.000131)		
Seller's 5-Year Probability of Default						-0.285***	-0.204***
						(0.0179)	(0.0212)
WWR (Correlation)	-0.545***	-0.487***	-0.506***	-0.554***	-0.534***	-0.566***	-0.561***
	(0.0737)	(0.0763)	(0.0740)	(0.0738)	(0.0737)	(0.0741)	(0.0740)
Past Relationship	2.456***	2.460***	2.456***	2.456***	2.452***	2.452***	2.450***
	(0.0107)	(0.0108)	(0.0109)	(0.0107)	(0.0107)	(0.0107)	(0.0107)
Number of observations	950,271	941,511	931,064	950,271	950,271	950,271	950,271
Number of transactions	190,838	190,838	190,838	190,838	190,838	190,838	190,838
Number of buyers	828	828	828	828	828	828	828
Pseudo R-squared	0.425	0.43	0.436	0.425	0.425	0.425	0.425

Notes: We include seller fixed effects, U.S. domicile reference entity fixed effect and the spread of the CDX.NA.IG index interacted with an indicator variable for each seller as controls. Wrong-way risk is measured with the correlation between the weekly change in log CDS spread on the reference entity and on the selling dealer estimated using a rolling five-year window. European debt crisis starts on October 4, 2011 and ends on July 26, 2012. Volcker rule starts on November 7, 2011. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

INTERNET APPENDIX

Counterparty Risk and Counterparty Choice in the Credit Default Swap Market

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A Distribution of the Number of Client Trading Relationships

A non-dealer typically only trade with a dealer when a signed ISDA Master Agreement is in place. The *de facto* choice set for some counterparties, therefore, may only be a subset of the alternatives included in the counterparty choice regressions (i.e., the set of 14 international dealers in the baseline sample or the set of five U.S. dealers in the US5 counterparty sample). While we cannot directly observe whether an agreement is in place, we do observe that the great majority of reasonably active participants in the CDS market trade with a large number of dealers, which suggests that these participants were maintaining a significant number of ISDA Master Agreements during the sample period.

For each client j in the baseline sample, we form the *match set* of dealers with whom client j is observed to trade at least once within that sample, and define $K_{baseline}(j)$ as the number of dealers in this match set. We similarly construct match sets for clients in the US5 counterparty sample, and define $K_{us5}(j)$ as the number of dealers in this match set. Next, within the baseline sample, we count the number of transactions involving clients with $K_{baseline}(j) = 14$, the number involving clients with $K_{baseline}(j) = 13$, and so on. Similarly, within the US5 counterparty sample, we count the number of transactions involving clients with $K_{us5}(j) = 5$, the number involving clients with $K_{us5}(j) = 4$, and so on. Results are reported in Table IA.1. For the baseline sample, we see that roughly half of all trades are done by clients who trade with 12 or more dealers, and over 75% of transactions are done by clients who trade with eight or more dealers. Similarly, for the US5 counterparty sample, over 80% of transactions are done by clients who trade with all US5 dealers. Thus, the *de facto* choice set is large in the majority of transactions.

B Variations in the Dealers' Credit Risk

In Table IA.2 we show that there is economically important cross-sectional variation across dealers' credit risk. Define $cdsRange_{baseline}(t)$ as the cross-sectional difference on date t between the maximum and minimum five year CDS spread among the 14 international dealers appearing in the baseline sample, and similarly define $cdsRange_{us5}(t)$ as the range of CDS spreads on date t within the set of US5 dealers. The median value across sample dates of $cdsRange_{baseline}(t)$ is 138 basis points, and on only 5% of sample dates is the CDS range under 60 basis points. Relative to typical investment grade credit spreads, the median range is quite large. For the US5 counterparty sample, the CDS range among the US5 dealers must be smaller than for the broader set of 14 international dealers, i.e., we must have

$$0 \leq cdsRange_{us5}(t) \leq cdsRange_{baseline}(t)$$

by construction. Nonetheless, the range is still economically significant. The median value of $cdsRange_{us5}(t)$ is 98 basis points, and the upper percentile values are close to the upper percentile values for $cdsRange_{baseline}(t)$. Excluding the European debt crisis has a relatively modest effect on the percentile values.

In Table IA.3 we show that there is also substantial time-series variation. The standard deviation across time of dealers' CDS spreads range from 101 basis points (for Morgan Stanley) to 26 basis points (for JP Morgan Chase). The time-series variation is qualitatively similar to that shown in Arora, Gandhi, and Longstaff (2012, Table 2); for some dealers our variance is bigger and for others smaller.

C Price Effect of Seller Credit Spreads Expressed in Upfront Points

To promote contract fungibility, the ISDA "Big Bang" Protocol introduced in April 2009 standardized coupon rates and auction settlement procedures following a credit event. Throughout our sample period of 2010–13, CDS have been traded on the basis of a fixed coupon rate, generally 100 or 500 basis points per annum, and therefore have initial market value in favor of one party. This initial value is paid by that party to its counterparty as an *upfront* payment. Even though the upfront is the actual market price of the CDS contract, we argue in Section 3.1 that transformation to par spreads is preferred because spreads (approximately) eliminate the effect of contract maturity and coupon rates in measuring the sensitivity of contract value to explanatory variables. In this appendix, we verify the robustness of our pricing regressions to using upfront prices instead.

In Table IA.4, we repeat the pricing analysis using the difference between DTCC transaction prices and Markit quotes measured in upfront points as the dependent variable. The specification is now:

$$upf_{i,t}^{s,b} - \overline{upf}_{i,t} = \alpha_{i,t}^{b} + \beta \overline{cds}_{t-1}^{s} + \eta WWR_{t}^{s} + \lambda Relations_{t-1}^{s,b} + \delta \log(size) + \epsilon_{i,t}^{s,b}, \tag{IA.1}$$

where $upf_{i,t}^{s,b}$ is the price in annualized upfront points on the DTCC transaction and $\overline{upf}_{i,t}$ is the corresponding annualized Markit upfront. We (approximately) annualize upfront points by dividing the upfront by maturity. This facilitates comparison to the effect on par spreads and also avoids heteroskedasticity in the regression. Since the upfront points can take negative values, we run the regressions of upfront point differentials in levels on seller's CDS spread levels, instead of running the regressions in logs as in Table 3.

The benchmark coefficient on the seller's credit spread in Column 1 of Table IA.4 is equal to -0.0073, which implies that a 100 basis point increase in the seller's credit spread translates to a 0.73 basis point reduction in the annualized upfront payment. This translates roughly to a difference of \$540 in the annualized total cost of an average-sized trade of \$7.4 million, which is very similar to the price impact obtained using the par spread as dependent variable. In other specifications (Columns 2–4) on the baseline sample, coefficients on the seller's credit spread remain negative, but not always statistically significant. The coefficients on the two WWR measures are large, statistically significant, and (contrary to prediction) positive. This puzzling result on WWR confirms the anomaly noted in Section 3.1 of the paper, which appears also in Arora, Gandhi, and Longstaff (2012). For the US5 counterparty sample (Columns 5–6), the effects of seller credit spread and WWR are statistically insignificant.

D The Effects of Clearing Eligibility

D.1 Central Clearing Background

Central clearing was first introduced for CDX.NA.IG, an index composed of investment grade North American corporates, and iTraxx Europe, its European counterpart. Selected single-name reference entities became eligible for clearing by Intercontinental Exchange (ICE) in waves beginning in December 2009. By the end of our sample period, most index constituents had been made eligible for clearing. The single-name index constituents that remain ineligible are primarily the European dealer banks listed in iTraxx Europe. US dealer banks are excluded from the CDX.NA.IG index, and also remain ineligible for clearing.

Within an investment grade index, reference entities have been made eligible for central clearing in cohorts, which typically consist of several firms in the same sector. Table IA.5 summarizes the introduction of clearing eligibility during our sample period by sector for CDX.NA.IG constituents. Cohorts are typically small, and the sorting of reference entities into cohorts is driven primarily by how long the reference entity has been a constituent of the index. For example, five firms in the Technology sector were made eligible for clearing on 19 April 2010, two more on 28 March 2011, and a final two on 13 June 2011.[1] The first cohort was composed of reference entities that had been continuously listed in the CDX.NA.IG since at least September 2008. The second cohort consisted of reference entities added to the index by March 2009, while the last cohort was composed of reference entities added by March 2011. Part of our analysis exploits the staged introduction of clearing for CDX.NA.IG constituents to study time series effects of central clearing on transaction spreads.

D.2 On the Sample of Clearing Eligibility Events

Our sample of event dates for the introduction of central clearing differs slightly from the sample constructed by Loon and Zhong (2014). In this appendix, we review the corrections we have applied to the event sample.

A reference entity is identified in CDS transaction data by its so-called RED code, which is an identifier assigned by Markit. Upon certain technical events associated with changes to corporate legal structure, Markit will retire the old RED code and assign a new one; these are known as the *predecessor* and *successor* codes, respectively. These events are not associated with firm default or distress, and pre-existing contracts on the predecessor convert automatically to contracts on the successor. If the predecessor was already eligible for clearing, ICE makes the the successor eligible as well. ICE includes these technical events in its tables of eligibility dates, but there is no economic sense in which these events should count as the *inception* of clearing eligibility. In two cases (CenturyTel Inc. and Burlington Northern Santa Fe Corp.), the event date used by Loon and Zhong (2014) is actually the event date for a RED successor. We use the earlier clearing eligibility dates for the predecessors.

[1] Sector information comes from Markit. On February 15, 2012, Markit modified its classification scheme. To avoid inconsistencies, we categorize reference entities by their sector under the old scheme. For the small number of reference entities that did not exist before February 15, 2012, we use the sector of their most recent predecessor classified under the old scheme.

Reference entities typically appear in ICE clearing volume reports immediately after they become eligible for clearing. However, in three cases (Aetna Inc., American Express Co., and Comcast Corp.), we observe lengthy delays between the published eligibility date and the first reported cleared trades. Whereas Loon and Zhong (2014) use the published date, we set our clear-eligibility dates for these entities based on actual reported trades.

Finally, we also include in our sample ten additional clearing eligibility events that took place subsequent to the end of the sample in Loon and Zhong (2014).

D.3 Time Series Comparison for Pricing Around Central Clearing Event Dates

In this section, we examine the key finding in Loon and Zhong (2014) that central clearing increases CDS spreads. Loon and Zhong (2014) use an event study approach to measure the excess return in CDS spreads around the introduction of clearing eligibility. Specifically, they run individual regressions for each clearable reference entity i of the following form:

$$sc_{i,t} = \alpha_i + \beta_i mktsc_t + \gamma_{1,i} D1_{i,t} + \gamma_{2,i} D2_{i,t} + \gamma_{3,i} D3_{i,t} + \gamma_{4,i} D4_{i,t} + \epsilon_{i,t}, \tag{IA.2}$$

where $sc_{i,t}$ is the daily percentage change in the CDS spread; $mktsc_t$ is the daily percentage change in the CDX spread; $D1_{i,t}$ is a dummy variable that is equal to 1/10 on -20 to -11 days since clearing eligibility and zero otherwise; $D2_{i,t}$ is a dummy variable that is equal to 1/10 on -10 to -1 days since clearing eligibility and zero otherwise; $D3_{i,t}$ is a dummy variable that is equal to 1/11 on 0 to 10 days since clearing eligibility and zero otherwise; and $D4_{i,t}$ is a dummy variable that is equal to 1/10 on 11 to 20 days since clearing eligibility and zero otherwise. The regression is estimated using data from -250 to 20 days around the event. After estimating individual regression coefficients, Loon and Zhong (2014) then take an average of the regression coefficients across reference entities and conduct statistical inference.

In Column 1 of Table IA.6, we copy the estimates shown in Loon and Zhong (2014, Table 2, Column 1) for comparison. The main finding is that the coefficients on D2 and D3 are significantly positive, suggesting positive cumulative abnormal spread changes between days -10 and 10 since clearing eligibility. In Column 2, we repeat the event study methodology in Loon and Zhong (2014) using our sample. We can see that even though the statistical significance on D2 and D3 is lower in our sample, the magnitude of the coefficient remains positive and sizable.

We conduct three experiments to show that the results of Loon and Zhong (2014) do not withstand closer inspection. First, the positive excess spread can be replicated in a placebo test in which we randomly assign sample reference entities to dates in the pool of sample events. For example, if reference entities A, B and C were made clearing eligible on dates t_a, t_b and t_c, respectively, then a random reassignment might pair A with date t_b, B with date t_c and C with date t_a. We maintain the original cohort sizes for each event date in the sample, and require for each reference entity that the randomly assigned clearing date be at least 20 days apart from the true clearing date. We replicate this experiment 500 times, and report average coefficients in Column 3 of Table IA.6.[2] Even though these clearing dates are random and false, we continue to find positive coefficients on D_2 and D_3. In particular, the coefficient on D_2 doubles and becomes more statistically significant. These "false positives" undermine the causal relationship between clearing eligibility and the spread increase established by the event study in Loon and Zhong (2014).

Second, the positive finding is not robust to using a refined market proxy as $mktsc_t$. Loon and Zhong (2014) define $mktsc_t$ as the *composite spread* for CDX.NA.IG spread based on direct dealer quotes on the index, and so far we have done the same in Columns 1–3. However, following the financial crisis the liquidity of the single-name CDS market diminished relative to the index market, and the CDX.NA.IG composite spread fails to capture the time-varying liquidity premium. Besides the composite spread, Markit reports the so-called *model spread*, which is constructed directly from the single-name spreads on the underlying 125 index constituents. In Column 4, we replace the composite spread with the model spread, and find that the coefficient on D2 becomes significantly *negative* and the coefficient on D3 decreases notably. We observe as well that the average regression coefficient on model spread returns is very close to unity, whereas the coefficient on $mktsc$ in Column 1 is 0.56. As the CDX index swap references an equally-weighted portfolio of individual swaps, the average "beta" with respect to the market proxy should indeed be near one. This suggests that the model spread

[2]Let $\gamma_{j,i}^{(r)}$ be the coefficient on dummy Dj for replication r and reference entity i. For each replication, we calculate $\gamma_j^{(r)}$ by taking the mean across i. The reported γ_j is the mean across r. To obtain t-statistics on the coefficients, we divide by the standard deviation across r of the $\gamma_j^{(r)}$. This method is the closest analog to how Loon and Zhong (2014) assess statistical significance across their set of individual regressions.

should be preferred over the composite spread as the common factor in the single-name CDS market.

The low value of the estimated coefficient on the composite spread returns is indicative of errors-in-variables attenuation. The difference between the composite and model spreads, known as the *index skew*, is often used by practitioners as a metric of relative liquidity between index and single-name markets. As depicted in Figure IA.1, the skew on the on-the-run five year CDX.NA.IG spiked during the global financial crisis and remained large in magnitude and volatile during 2009. For most of the clearing-eligibility events in our sample, a portion of this volatile period is included in the regression window of -250 to 20 days around the event. When the composite spread is used as market proxy, the missing liquidity component in $mktsc$ leads to a bias towards zero in $\hat{\beta}_i$ in equation (IA.2). Even though the index skew is relatively small in magnitude around most of the event dates, the bias in $\hat{\beta}_i$ implies an undercorrection for the common market factor, so that a portion of the innovations in the CDX spread during the event windows will be incorrectly attributed to the event window dummy regressors. This also serves to explain how we can obtain a positive and significant coefficient on D2 in the placebo test.

Third, the positive finding disappears when we use a difference-in-differences (DID) methodology. To facilitate comparison between an event study methodology along the lines of Loon and Zhong (2014) and our DID approach, we first estimate a panel version of the Loon and Zhong (2014) specification:

$$sc_{i,t} = \alpha_{sector} + \beta mktsc_t + \gamma_1 D1_{i,t} + \gamma_2 D2_i + \gamma_3 D3_i + \gamma_4 D4_{i,t} + \epsilon_{i,t}. \tag{IA.3}$$

Equation (IA.3) is very similar to (IA.2) except that the regression coefficients are directly estimated in a panel regression with sector fixed effects, instead being averaged across regressions for individual reference entities. As in Loon and Zhong (2014), this model is estimated without a control group.

We recast equation (IA.3) in a DID setting as follows:

$$\begin{aligned} sc_{it} = {} & \alpha_{sector,t} + \beta Treat_i + \gamma Treat_i \times mktsc_t + \delta_1 Treat_i \times D1_{i,t} \\ & + \delta_2 Treat_i \times D2_{i,t} + \delta_3 Treat_i \times D3_{i,t} + \delta_4 Treat_i \times D4_{i,t} + \epsilon_{it} \end{aligned} \tag{IA.4}$$

where $\alpha_{sector,t}$ denotes sector and date interactive fixed effects. The variable $Treat_i$ is a dummy indicating whether the reference entity i is in the treatment group. The four δ coefficients measure the time-series effects of clearing eligibility. We use treatment and control groups in the same sector to mitigate the impact of common macroeconomic and sectoral shocks on our estimates. An important advantage to the DID methodology is that we can be agnostic on the choice of market proxy, as the impact of common market factors is picked up in the fixed effects.

We estimate DID on three different treatment/control designs. In the first design, reference entities cleared in the first cohort for a sector form the treatment group, and pre-eligibility reference entities of the same sector cleared in later cohorts form the control group. In the second design, reference entities in later cohorts form the treatment group, and post-eligibility reference entities in the first cohort within the same sector form the control group.[3] In the third design, reference entities that become eligible during the sample period form the treatment group, and non-clearable North American investment grade reference entities form the control group.

Results are reported in Table IA.7 for both the panel regression and the DID exercise. Columns 1, 3 and 5 report panel regression results on the treatment group based on equation IA.3; we refer to this as the event study (ES) analysis. Columns 2, 4 and 6 report the DID estimation of equation IA.4. We can see that the coefficient on $Treat \times D_2$ is positive and significant under the ES specification in Columns 1 and 5, but becomes negative and insignificantly under DID. Similarly, the coefficient on $Treat \times D_3$ is smaller in the DID columns than in the ES columns across all three specifications. Results are qualitatively robust to including *sector* $\times$ *mktsc* interactions in the event study panel regressions (not tabulated).

Finally, in Figure IA.2 we plot the log of the mean spreads for treatment and control groups around clearing-eligibility event dates, as well as the difference between the two groups. For each of the three DID designs, we plot log-spreads drawn from DTCC transaction

[3]We ensure that entities in both treatment and control groups of the first two DID designs were already CDX index members before clearing was introduced, so that the effects of clearing eligibility can be separately identified from the effects of index inclusion.

data in the left panel and log-spreads drawn from Markit quotes in the right panel.[4] In all cases, we see that treatment and control group log-spreads co-move closely around the event dates. There is no discernable change over the event window in the difference across the two groups in log-spread.

E Effects of Counterparty Risk on Non-dealers Selling Protection

To explore the impact of counterparty risk on transactions involving a non-dealer *seller* of protection to a dealer, we modify the notation introduced in Section 2.3 for our main explanatory variables. We construct our measures of right-way risk RWR_i^b for sale of protection to dealer b in exactly the same way as we construct WWR_i^s for the purchase of protection from dealer s. Our indicator-based measure is a dummy variable equal to one if both the buyer of protection b is a US5 dealer and the reference entity i is either a US5 dealer or Wells Fargo. The correlation-based measure is the linear correlation between the log CDS spread changes on the reference entity i and on the buying dealer b. We modify the variable name (from WWR to RWR) only to highlight that the economic interpretation is reversed.

When the dealer is acting as buyer, we denote the lagged CDS spread on the dealer as $\overline{cds}_{t-1}^{b}$. We define $Relations_{t-1}^{b,s}$ as the mirror image of $Relations_{t-1}^{s,b}$: $Relations_{t-1}^{b,s}$ is the notional value that market participant s traded with dealer b in the recent past, expressed as a fraction of the total notional value that market participant s traded.

E.1 Effects on CDS Pricing

Parallel to the analysis in Section 3, we consider the effect of the buyer's CDS spread on transaction spreads. We hold the seller, contract and trade date fixed, and examine the

[4] Our DTCC sample is the union of the baseline and US5 counterparty samples. To distinguish clearly from possible time-variation in the magnitude of pricing advantage over non-dealers, we include interdealer and cleared transactions only. We take a volume-weighted average spread for each reference entity on each date and then take logs.

impact of buyer's credit spreads on transaction spreads using the following regression:

$$\log(cds^{s,b}_{i,t}) - \log(\overline{cds}_{i,t}) = \alpha^s_{i,t} + \beta \log(\overline{cds}^b_{t-1}) + \eta RWR^b + \lambda Relations^{b,s}_{t-1} + \delta \log(size) + \epsilon^{s,b}_{i,t}, \tag{IA.5}$$

where $\overline{cds}^b_{t-1}$ denotes the buyer's CDS spread and $\alpha^s_{i,t}$ is the seller-contract-time fixed effect. The value of the premium leg of the swap decreases with buyer default risk but increases with right-way risk, so we expect $\beta > 0$ and $\eta < 0$.

Summary statistics for each side of the client-facing market are collected in Table IA.8. Columns (1) and (3), which appear in Table 1 in the paper, tabulate client-facing transactions in which the non-dealer is the buyer of protection. Columns (2) and (4) provide corresponding figures for transactions in which the non-dealer is the seller of protection. We have fewer observations on the "client seller" side of the client-facing market, which implies that this side of the market is more concentrated. In the baseline sample, there are 169 non-dealer sellers of protection in 7760 transactions, of which 5689 reference US5 dealers. In the US5 counterparty sample, there are 680 non-dealers of protection in 88,531 transactions and 1156 reference entities, of which 245 are financial firms and 73 are sovereigns.

Table IA.9 reports our estimation results for non-dealers as protection sellers. The coefficient on the buyer's credit spread is very close to zero and insignificant. Neither the RWR nor the past relationship enters significantly. These insignificant results hold in both baseline and US5 counterparty samples.

As in Section 3.1 of the paper, we also estimate a regression with additional buyer fixed effects to control for time-invariant buyer characteristics:

$$\log(cds^{s,b}_{i,t}) - \log(\overline{cds}_{i,t}) = \alpha^s_{i,t} + \alpha^b + \beta \log(\overline{cds}^b_{t-1}) + \eta RWR^b + \lambda Relations^{b,s}_{t-1} + \delta \log(size) + \epsilon^{s,b}_{i,t}. \tag{IA.6}$$

Results for the regression with additional fixed effects are presented in Table IA.10. Consistent with our null hypothesis, the coefficient on the buyer's credit spread becomes significantly positive in the benchmark specification, but remains small in magnitude.

E.2 Effects on Counterparty Choice

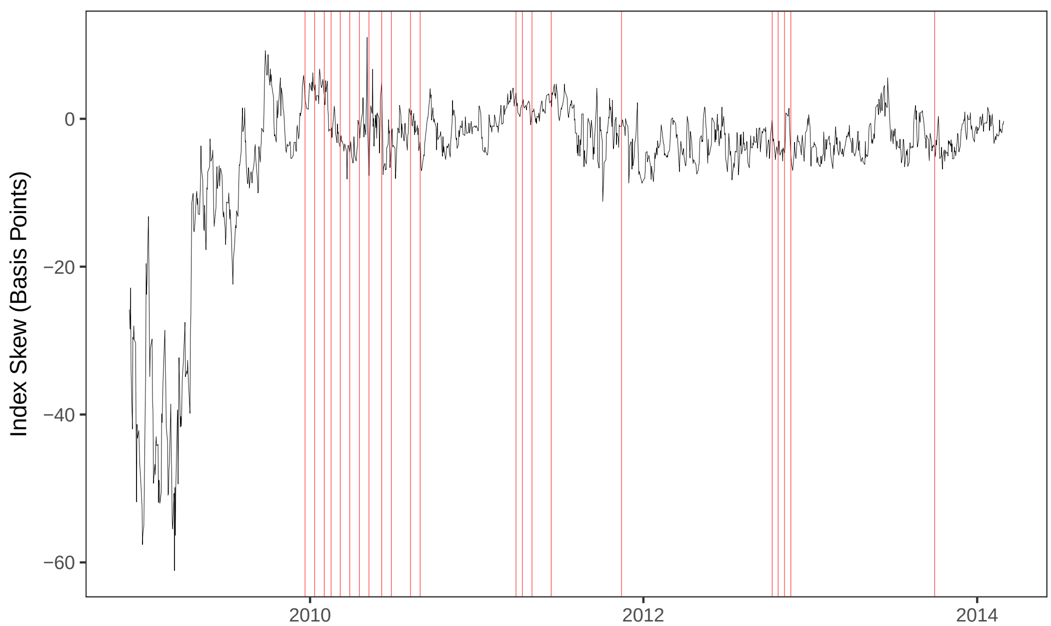
We repeat our analyses of Section 4 from the perspective of non-dealer sellers of protection and report the results in Table IA.11. Consistent with the asymmetry between buyer and seller counterparty exposure, we find that the seller's choice of dealer counterparty is slightly less sensitive to the dealer's credit risk. The marginal effects for this regression (not tabulated) indicate that a 100 basis point increase in a large dealer's CDS spread is associated with an average decline in the likelihood of a non-dealer selling protection to that dealer of 2.2 percentage points compared to 2.6 percentage points in the buyer-choice model. The effect of right-way risk depends on the choice of measure. It is small in magnitude and statistically insignificant when we use the US5+Wells Fargo indicator variable, but negative and significant when we use the rolling-window correlation measure.

References

Arora, N., P. Gandhi, and F. A. Longstaff (2012): "Counterparty Credit Risk and the Credit Default Swap Market," *Journal of Financial Economics*, 103(5), 280–293.

Loon, Y. C., and Z. K. Zhong (2014): "The Impact of Central Clearing on Counterparty Risk, Liquidity and Trading: Evidence from Credit Default Swap Market," *Journal of Financial Economics*, 112(1), 91–115.

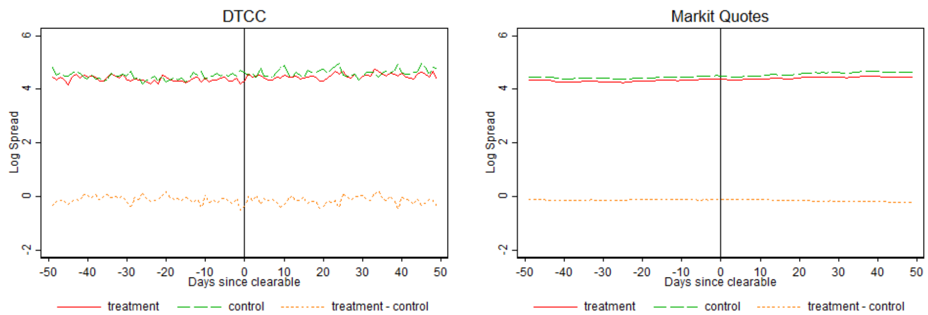
Figure IA.1: Index Skew for On-the-Run 5 Year CDX.NA.IG



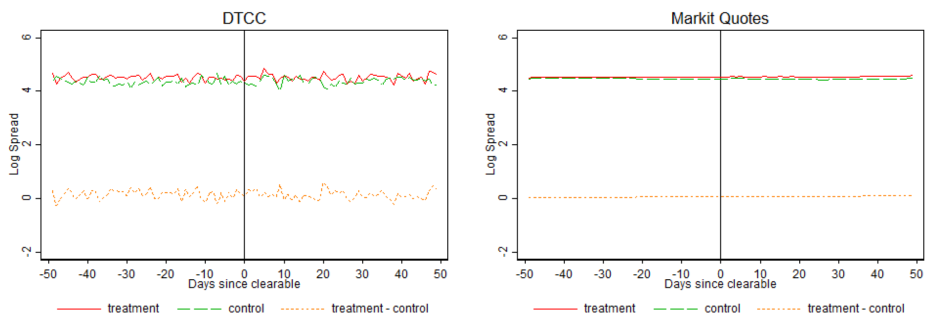
Notes: This figure plots the index skew on the 5 year CDX.NA.IG index, where the index skew is defined as the difference between the composite spread and model spread reported by Markit. The vertical lines indicate clearing-eligibility event dates in our sample.

Figure IA.2: Time Series Effects of Clearing Eligibility on Transaction Spreads

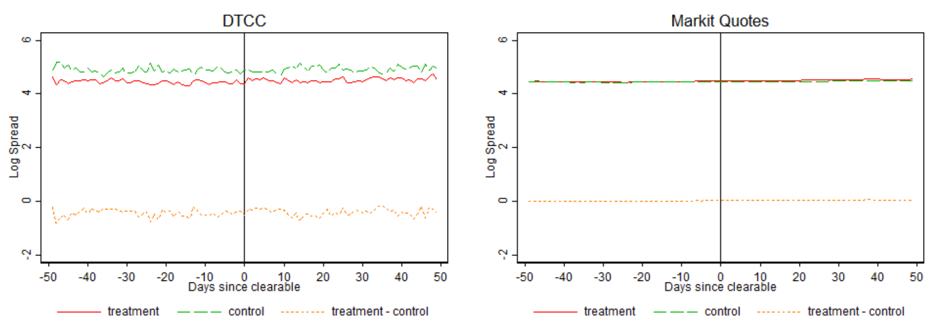
(a) DID Design I



(b) DID Design II



(c) DID Design III



Notes: The left panels plot results based on DTCC transaction spreads and the right panels plot results based on Markit quotes. In row (a), the treatment group consists of transactions or quotes on reference entities cleared in the first cohort for each sector of the CDX.NA.IG index before and after clearing. The control group consists of pre-clearing transactions or quotes of reference entities in CDX.NA.IG that are cleared in later cohorts. In row (b), the control group consists of post-clearing reference entities cleared in the first cohort for each sector of the CDX.NA.IG index. The treatment group consists of transactions or quotes of reference entities in the same sector of CDX.NA.IG that are cleared in later cohorts. In row (c), the treatment group consists of transactions or quotes on all clearable reference entities in CDX.NA.IG before and after clearing. The control group consists of transactions or quotes on all other non-clearable investment grade reference entities in North America. Only interdealer and same-day cleared transactions are used in computing the means.

Table IA.1: Transaction count by number of dealers in client's match set

Panel A: Baseline sample

	Non-dealer is buyer			Non-dealer is seller		
Number of dealers	Transactions	Percent	Cumulative Percent	Transactions	Percent	Cumulative Percent
14	1,417	11.88	11.88	419	5.40	5.40
13	3,174	26.60	38.48	2,773	35.73	41.13
12	1,094	9.17	47.65	908	11.7	52.83
11	837	7.01	54.66	496	6.39	59.22
10	906	7.59	62.25	391	5.04	64.26
9	871	7.30	69.55	699	9.01	73.27
8	660	5.53	75.08	562	7.24	80.51
7	545	4.57	79.65	456	5.88	86.39
6	679	5.69	85.34	316	4.07	90.46
5	479	4.01	89.35	308	3.97	94.43
4	307	2.57	91.92	165	2.13	96.56
3	434	3.64	95.56	70	0.90	97.46
2	497	4.17	99.73	159	2.05	99.51
1	32	0.27	100	38	0.49	100

Panel B: US5 counterparty sample

	Non-dealer is buyer			Non-dealer is seller		
Number of dealers	Transactions	Percent	Cumulative Percent	Transactions	Percent	Cumulative Percent
5	152,979	80.16	80.16	72,945	82.39	82.39
4	20,182	10.58	90.74	10,741	12.13	94.52
3	9,219	4.83	95.57	2,845	3.21	97.73
2	6,056	3.17	98.74	948	1.07	98.8
1	2,402	1.26	100	1,052	1.2	100

Notes: Our sample period is from 2010 to 2013. In Panel A, baseline sample, we consider transactions where a non-dealer is either buying or selling protection on an FRB regulated reference entity. In Panel B, we consider transactions where a non-dealer is either buying protection from or selling protection to a US5 dealer on any reference entity.

Table IA.2: Distribution of the difference between CDS spreads of the riskiest and safest dealer (basis points)

	(1)	(2)	(3)	(4)
	Baseline sample		US5 counterparty sample	
Percentile	Full sample	Excluding European debt crisis	Full sample	Excluding European debt crisis
Maximum	427	333	413	330
99 percentile	340	286	330	248
95 percentile	283	233	275	210
90 percentile	257	216	249	170
75 percentile	211	150	183	113
50 percentile	138	129	98	89
25 percentile	110	104	67	62
10 percentile	83	76	48	44
5 percentile	60	56	37	32
1 percentile	51	49	28	28
Minimum	46	46	24	24

Notes: Our sample period is from 2010 to 2013. We use Markit daily data to compute the difference between the five-year CDS spread of the dealer with the highest CDS spread on that day (the riskiest dealer) minus the five-year CDS spread of the dealer with the smallest CDS spread (the safest dealer). For the baseline sample we have 14 CDS spreads each day, for the US5 CP sample we have 5 CDS spreads.

Table IA.3: Summary statistics for CDS contracts referencing dealers

Dealer	Mean	Standard deviation	Minimum	Maximum
Bank of America	186.74	88.18	79.90	504.97
Barclays	151.14	44.78	74.33	295.04
BNP Paribas	152.95	66.58	53.17	373.60
Citigroup	169.56	58.27	73.10	361.02
Credit Suisse	116.99	35.54	54.45	213.08
Deutsche Bank	131.56	42.73	70.91	332.89
Goldman Sachs	179.42	72.32	90.94	430.45
HSBC	97.47	26.91	51.78	185.91
JP Morgan Chase	100.20	26.23	47.33	187.85
Morgan Stanley	219.49	101.13	88.35	596.20
Nomura	222.71	84.22	86.05	422.06
Societe Generale	195.57	87.72	64.02	451.76
RBS	215.35	67.67	111.15	415.48
UBS	122.73	41.94	63.67	247.87

Notes: Our sample period is from 2010 to 2013. We use Markit daily data on five-year CDS spreads.

Table IA.4: Effects of Seller CDS Spreads on Upfront Point Differentials (Client-Dealer Transactions)

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Baseline	Baseline not eligible for clearing	Baseline not eligible for clearing	US5 CP	US5 CP not eligible for clearing
Seller's CDS	-0.00730**	-0.00611*	-0.00769*	-0.00638	-0.000583	-0.000573
	(0.00371)	(0.00347)	(0.00429)	(0.00396)	(0.00113)	(0.00144)
Wrong-Way Risk (Indicator)	1.075**		1.093**			
	(0.495)		(0.508)			
Wrong-Way Risk (Correlation)		4.366**		4.925**	-1.599	-3.534
		(2.112)		(2.218)	(1.860)	(2.582)
Past Relationship	0.151	0.0480	0.350	0.219	1.015***	1.257***
	(1.234)	(1.209)	(1.339)	(1.304)	(0.362)	(0.433)
No. of effective obs.	3,352	3,352	2,858	2,858	29,926	21,462
No. of transactions	11,932	11,932	10,295	10,295	190,838	133,567

Notes: In all columns, we consider only transactions where the seller is one of the 14 largest dealers and the buyer is a client. The dependent variable is difference between the DTCC upfront point and the Markit upfront point. In Columns 1-2, we use transactions in the baseline sample. In Columns 3-4, we use transactions on reference entities that are eligible for clearing in the baseline sample. In Columns 5, we use transactions in the US5 counterparty sample. In Column 6, we use transactions on reference entities that are eligible for clearing in the US5 counterparty sample. In each regression,, we include contract-buyer-date fixed effects. The number of effective observations is reported for buyers facing at least two different sellers for the same contract on a single trading date. Log notional of the trade is included as a control in all regressions. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table IA.5: Clearing Dates for CDX.NA.IG Constituents

	Materials	Con. Goods	Con. Srvs	Financials	Healthcare	Industrials	Oil/Gas	Technology	Telecom	Utilities
12/21/2009	0	0	0	0	0	0	0	0	0	3
1/11/2010	0	0	0	0	0	0	0	0	0	3
2/1/2010	0	0	0	0	0	0	0	0	3	0
2/15/2010	0	0	0	0	0	15	0	0	0	0
3/8/2010	0	4	0	0	0	0	5	0	0	0
3/29/2010	0	0	14	0	0	0	0	0	0	0
4/19/2010	5	0	0	0	0	0	0	5	0	0
5/10/2010	0	0	1	6	4	0	0	0	0	0
6/7/2010	0	0	0	1	1	0	0	0	0	0
6/28/2010	0	0	1	0	0	0	0	0	0	0
8/9/2010	0	8	0	0	0	0	2	0	0	0
8/30/2010	0	0	8	0	0	0	0	0	0	0
3/28/2011	2	2	4	0	0	0	0	2	0	0
4/11/2011	0	0	0	10	0	0	0	0	0	0
5/2/2011	0	0	0	0	3	3	2	0	0	0
6/13/2011	0	1	3	5	0	0	0	2	0	0
11/14/2011	0	1	1	0	1	0	1	0	0	0
10/9/2012	0	0	0	5	0	0	0	0	0	0
10/22/2012	0	0	0	0	0	0	6	0	0	0
11/5/2012	1	2	1	1	0	1	0	0	0	2
11/19/2012	0	0	0	1	0	0	0	0	0	0
9/30/2013	0	2	2	2	0	2	0	0	0	0
Total	8	20	35	31	9	21	16	9	3	8

Notes: This table reports clearing dates by sector for reference entities in CDX.NA.IG that become eligible for central clearing at Intercontinental Exchange (ICE). We exclude clearable reference entities that had cleared predecessors. Source: ICE.

Table IA.6: Time Series Impact of Central Clearing on CDS Spreads

	(1) Loon-Zhong	(2) Baseline	(3) Placebo	(4) Model Spread
$mktsc$	0.562***	0.588***	0.566***	1.003***
$D_1 : [-20, -11]$	0.354	0.843	0.164	0.32
$D_2 : [-10, -1]$	1.846**	1.299*	2.348***	-1.020**
$D_3 : [0, 10]$	1.774**	0.936	0.865	0.001
$D_4 : [11, 20]$	0.618	0.597	0.389	-0.321
Constant	0.014	0.004	-0.023	0.094***
# of observations	132	142	142	142

Notes: In Column 1, we reproduce results from Loon and Zhong (2014) based on Markit CDS spread changes. In Column 2, we report average regression coefficients on running daily changes in the CDS spread on daily changes in the Markit composite spread, *mktsc*, and the 4 time dummies for 142 clearable reference entities. In Column 3, we conduct a placebo test by shuffling clearing dates among reference entities. Each reference entity is randomly assigned a new clearing date that is at least 20 days apart from their true clearing date. We repeat the same regression analysis as in Column 2, but based on the false clearing dates. In Column 4, we again perform the same regression analysis as in Column2, but use the Markit model spread instead of the composite spread as *mktsc*. All regression are estimated on days [-250, 20] since clearing eligibility. Following Loon and Zhong (2014), we test whether the regression coefficients are different from zero using a standard t-test based on coefficients from individual regressions. ***p<0.01, ** p<0.05, * p<0.1.

Table IA.7: Time Series Effects of Clearing Eligibility

	Design I		Design II		Design III	
	(1)	(2)	(3)	(4)	(5)	(6)
	ES1	DID1	ES2	DID2	ES3	DID3
$Treat \times D_1$	2.154*	-1.196	0.0273	2.024*	1.076	-0.182
	(1.308)	(1.866)	(1.138)	(1.053)	(0.794)	(1.628)
$Treat \times D_2$	4.128***	-0.0848	-1.328	-0.851	1.348*	-0.273
	(1.328)	(1.956)	(1.152)	(1.075)	(0.804)	(1.645)
$Treat \times D_3$	0.766	0.147	0.667	0.403	0.956	0.598
	(1.381)	(2.054)	(1.201)	(1.107)	(0.838)	(1.715)
$Treat \times D_4$	2.290*	-2.292	-0.0599	0.328	0.884	0.445
	(1.299)	(1.927)	(1.145)	(1.049)	(0.794)	(1.623)
$mktsc$	0.663***		0.540***		0.592***	
	(0.00846)		(0.00702)		(0.00513)	
$Treat$		0.0538		0.00861		-0.0193
		(0.0384)		(0.0286)		(0.0345)
$Treat \times mktsc$		0.0568***		-0.0240***		0.326***
		(0.0121)		(0.00926)		(0.0110)
Number of observations	16,187	62,882	16,974	71,603	37,427	765,759
FE	Sector	Sector-Date	Sector	Sector-Date	Sector	Sector-Date

Notes: In Design I, the treatment group consists of reference entities cleared in the first cohort for each sector of the CDX.NA.IG index before and after clearing. The control group consists of reference entities in CDX.NA.IG that are cleared in later cohorts. In Deisgn II, the control group consists of post-clearing reference entities cleared in the first cohort for each sector of the CDX.NA.IG index. The treatment group consists of reference entities in the same sector of CDX.NA.IG that are cleared in later cohorts. In Design III, the treatment group consists of all clearable reference entities in CDX.NA.IG before and after clearing. The control group consists of all other non-clearable investment grade reference entities in North America. Columns 1, 3 and 5 estimate the event study Equation IA.3 for the treatment group only without using controls. Columns 2, 4 and 6 perform the DID analysis based on IA.4. Fixed effects for the interaction of sector and trade dates are used in columns 2, 4 and 6. In all columns, we use daily percentage changes of Markit CDS spreads as the dependent variable for event window from 250 days before the introduction of clearing eligibility to 20 days afterwards. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table IA.8: Summary Statistics for Client-Facing Transactions

	(1) Baseline sample non-dealer buyer	(2) Baseline sample non-dealer seller	(3) US5 CP sample non-dealer buyer	(4) US5 CP sample non-dealer seller
Number of reference entities	12	12	1,248	1,156
Of which...				
Eligible for clearing	4	4	326	312
Financial	12	12	259	245
Sovereign	0	0	76	73
Number of non-dealers	196	169	828	680
Of which...				
Short horizon	52	39	90	85
Captive	24	19	305	204
Frequent trader	53	45	66	66
Hedge fund	72	57	247	216
Asset manager	70	62	205	186
Non-dealer bank	26	26	76	76
Number of transactions between non-dealers and one of the largest dealers	11,932	7,760	190,838	88,531
Of which...				
A US5 dealer appear as the reference entity	7,918	5,689	3,919	2,398
The reference entity is not eligible for clearing	10,295	6,948	133,567	65,217

Notes: Our sample period is from 2010 to 2013. In column (1) we only consider transactions where a non-dealer is buying protection on an FRB regulated reference entity. In column (2) we only consider transactions where a non-dealer is selling protection on an FRB regulated reference entity. In column (3) we only consider transactions where a non-dealer is buying protection from a US5 dealer on any reference entity. In column (4) we only consider transactions where a non-dealer is selling protection to a US5 dealer on any reference entity.

Table IA.9: Effects of Buyer's CDS Spreads on Log Par Spread Differentials (Client-Dealer Transactions)

	(1) Baseline	(2) Baseline	(3) Baseline not eligible for clearing	(4) Baseline not eligible for clearing	(5) US5 CP	(6) US5 CP not eligible for clearing
Log buyer's CDS	0.00165	0.00193	0.00372	0.00355	-0.00175	-0.00194
	(0.00278)	(0.00263)	(0.00298)	(0.00281)	(0.00155)	(0.00160)
Right-Way Risk (Indicator)	-4.63e-05		-0.000563			
	(0.00215)		(0.00220)			
Right-Way Risk (Correlation)		-0.00365		-0.00174	0.00597	0.00337
		(0.00981)		(0.0100)	(0.0137)	(0.0149)
Past Relationship	0.00158	0.00134	0.000785	0.000903	-0.00192	-0.000868
	(0.00604)	(0.00587)	(0.00623)	(0.00604)	(0.00195)	(0.00197)
No. of effective obs.	2,158	2,158	1,946	1,946	11,264	8,781
No. of transactions	7,761	7,761	6,949	6,949	88,532	65,218

Notes: In all columns, we consider only transactions where the buyer is one of the 14 largest dealers and the seller is a client. The dependent variable is difference between the DTCC par spread and the Markit par spread. In Columns 1-2, we use transactions in the baseline sample. In Columns 3-4, we use transactions on reference entities that are eligible for clearing in the baseline sample. In Columns 5, we use transactions in the US5 counterparty sample. In Column 6, we use transactions on reference entities that are eligible for clearing in the US5 counterparty sample. In each regression, we include contract-seller-date fixed effects. The number of effective observations is reported for buyers facing at least two different sellers for the same contract on a single trading date. Log notional of the trade is included as a control in all regressions. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table IA.10: Effects of Buyer's CDS Spreads on Log Par Spread Differentials (Client-Dealer Transactions)

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Baseline	Baseline not eligible for clearing	Baseline not eligible for clearing	US5 CP	US5 CP not eligible for clearing
Log buyer's CDS	0.0168***	0.0156**	0.0166**	0.0163**	-0.00255	-0.00440
	(0.00644)	(0.00662)	(0.00667)	(0.00698)	(0.00287)	(0.00279)
Right-Way Risk (Indicator)	0.00631*		0.00130			
	(0.00353)		(0.00424)			
Wrong-Way Risk (Correlation)		0.0157		-0.00226	0.00698	0.00694
		(0.0209)		(0.0218)	(0.0146)	(0.0159)
Past Relationship	-0.00218	-0.00223	-0.00206	-0.00221	-0.00160	-0.000475
	(0.00720)	(0.00729)	(0.00742)	(0.00745)	(0.00192)	(0.00196)
No. of effective obs.	2,158	2,158	1,946	1,946	11,264	8,781
No. of transactions	7,761	7,761	6,949	6,949	88,532	65,218
Fixed effects	Contract × Seller × Date, Buyer					

Notes: In all columns, we consider only transactions where the buyer is one of the 14 largest dealers and the seller is a client. The dependent variable is difference between the DTCC par spread and the Markit par spread. In Columns 1-2, we use transactions in the baseline sample. In Columns 3-4, we use transactions on reference entities that are eligible for clearing in the baseline sample. In Columns 5, we use transactions in the US5 counterparty sample. In Column 6, we use transactions on reference entities that are eligible for clearing in the US5 counterparty sample. In each regression, we include contract-seller-date fixed effects and additional buyer fixed effects. The number of effective observations is reported for buyers facing at least two different sellers for the same contract on a single trading date. Log notional of the trade is included as a control in all regressions. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table IA.11: Counterparty Choice of Non-dealers Selling Protection to Dealers

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Baseline	Baseline	Baseline	US5 CP	US5 CP
			not eligible for clearing	not eligible for clearing		not eligible for clearing
Buyer's CDS	-0.250***	-0.244***	-0.186***	-0.191***	-0.0659***	-0.0553***
	(0.0528)	(0.0527)	(0.0558)	(0.0559)	(0.0158)	(0.0183)
Right-Way Risk (Indicator)	-0.0816		0.278***			
	(0.0583)		(0.0759)			
Right-Way Risk (Correlation)		-2.510***		-1.714***	-0.799***	-0.838***
		(0.311)		(0.338)	(0.0982)	(0.107)
Past Relationship	3.392***	3.379***	3.375***	3.337***	2.248***	2.211***
	(0.0658)	(0.0659)	(0.0717)	(0.0717)	(0.0165)	(0.0192)
Number of observations	102,951	102,951	91,583	91,583	440,257	323,687
Number of transactions	7,760	7,760	6,948	6,948	88,531	65,217
Number of sellers	169	169	165	165	680	660
Pseudo R-squared	0.383	0.384	0.382	0.382	0.407	0.408

Notes: We include buyer fixed effects and the spread on the CDX.NA.IG index interacted with an indicator variable for each buyer as controls. In columns (5) and (6) we also include a fixed effect for U.S. domicile reference entities. We use two different measures of right-way risk. One measure is an indicator variable equal to one if the buyer of protection is a U.S. dealer and the reference entity is one of the US5 dealers or Wells Fargo. The other measure is the correlation between the weekly change in log CDS spread on the reference entity and on the buying dealer estimated using a rolling five-year window. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.