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Three Essays in Financial Economics

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by

Salil Uday Gadgil

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ABSTRACT OF THE DISSERTATION

Three Essays in Financial Economics

by

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Doctor of Philosophy in Management
University of California, Los Angeles, 2022
Professor Avanidhar Subrahmanyam, Chair

In Chapter 1, we investigate how price discovery in the credit default swap (CDS) market has been impacted by regulation implemented since the Global Financial Crisis. We find that single-name CDS spreads impound less private information prior to rating downgrades and adjust more slowly after downgrades are announced. We also show that CDS spreads lead corporate bond spreads less strongly following the adoption of stringent margin requirements that apply only to derivatives. This decline is sharpest for reference entities that are most exposed to the new rules. Price discovery appears to be unharmed for CDS indices, which are largely centrally cleared and, thus, less affected by many of the reforms. We rationalize the findings with a model in which increased transaction costs for single names drive informed agents to trade indices. Together, the results highlight a lesser-studied channel through which post-crisis regulation has influenced financial markets.

In Chapter 2 (with Wenxin Du, Michael Gordy, and Clara Vega), we investigate how market participants price and manage counterparty credit risk in the post-2008 global financial crisis period using confidential trade repository data on single-name 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.

In Chapter 3 (with Jason Sockin), we use a sample of prominent scandals to study how employees are impacted by corporate misconduct. We find that worker sentiment decreases sharply and persistently following a scandal, driven by diminished perceptions of a firm's culture and senior management. Further, fewer employees receive variable pay and those that do see it fall 10 percent on average. Base pay and fringe benefits remain unchanged, however, highlighting that variable-pay earners are differentially exposed to firm-level shocks. As rank-and-file employees receive no compensation to offset the declines in job satisfaction and variable pay, we conclude that corporate misconduct leaves them strictly worse off.

The dissertation of Salil Uday Gadgil is approved.

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University of California, Los Angeles

2022

To Mom, Dad, Gauri, Suraj, and Hannah,

for your steadfast support.

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CHAPTER 1

Do Credit Default Swaps Still Lead? The Effects of Regulation on Price Discovery

1.1 Introduction

Over-the-counter (OTC) derivative markets played a central role in the propagation of the Global Financial Crisis. Large credit default swap (CDS) positions, in particular, were a major source of systemic instability. The bailout of AIG, for example, was motivated by fears that the failure of the insurance giant would lead to cascading defaults among its CDS counterparties (Financial Crisis Inquiry Commission 2011). In the wake of the crisis, policymakers introduced regulation intended to limit the risk generated by derivatives trading. In the United States, Congress passed the Dodd-Frank Wall Street Reform and Consumer Protection Act, which includes a number of measures pertaining to OTC swaps. Basel III was also developed in part to address perceived shortcomings in the treatment of off-balance sheet exposures. A literature studying the effects of these reforms has since emerged. A majority of the work considers outcomes such as pricing and liquidity (e.g., Loon and Zhong 2016; Cenedese, Ranaldo, and Vasios 2020). In this paper, we instead investigate how post-crisis regulation has impacted price discovery in the CDS market.

We focus on the effects of three reforms: the Supplementary Leverage Ratio (SLR) from Basel III, the so-called Volcker Rule from the Dodd-Frank Act, and the Uncleared Margin Rules (UMR). The SLR is a minimum capital requirement that adjusts leverage for swaps and other off-balance-sheet assets. It has become a key constraint for the largest U.S. banks (Duffie 2018) and, because it recognizes both net and gross exposures, is particularly onerous for uncleared positions. The Volcker Rule prohibits dealer banks from engaging in proprietary trading. While market-making activity is still permitted, compliance costs associated with the rule are substantial (OCC 2014). Much of the empirical analysis centers on the UMR, which are intended to strengthen collateralization practices for swaps. Implemented in phases beginning in September 2016, they impose stringent margin terms for uncleared positions and restrict the rehypothecation of collateral.

Consistent with the notion that reforms have raised trading costs, we find that single-name CDS have become less informative as regulation has been introduced. The decline is more pronounced for reference entities that are most exposed to the new measures. Price discovery for CDS indices, which are primarily centrally cleared and thus less affected by certain rules, appears unharmed. These results accord with a model in which informed agents shift their orders towards indices as it becomes comparatively more expensive to trade single names.

Several factors render the CDS market a good setting for this study. It is among the largest OTC markets in the world, with $8.4 trillion notional value outstanding as of December 2020 (BIS 2021). Further, there are two well-established methods used to assess the contributions of CDS spreads to price discovery. One series of papers, beginning with Hull, Predescu, and White 2004, demonstrates that CDS spreads anticipate credit rating downgrades. A second literature dating back to

Blanco, Brennan, and Marsh 2005 shows that changes in CDS spreads lead changes in corporate bond spreads. Testing if the anticipatory ability and lead-lag relationship have been weakened allows us to determine if reforms have indeed negatively impacted CDS informativeness. The breadth of reference entities in the CDS market also facilitates cross-sectional analysis to establish that regulation, not some other force, drives the decline in price discovery. Such tests would not be possible with more homogeneous derivative classes.

We begin by developing a simple, one-period model in the style of Ernst 2020 to generate hypotheses. There are two single-name securities, A and B, with unknown payoffs and an equal-weighted index composed of both. Risk neutral market makers face either a strategic insider who knows the value of one single name or an uninformed liquidity trader. Informed agents trade either the single name whose payoff they know or the index, but they are allowed to randomize their selection. Liquidity traders are restricted to trading their associated security. Both types of agents incur a transaction cost for single name, but not index trades. The adoption of post-crisis regulation is represented by increases in the cost parameter, as it makes single names more expensive relative to the index. Market makers cannot distinguish if the agent submitting an order has insider knowledge, so they charge a bid-ask spread to avoid losses due to asymmetric information.

For a given share of informed agents, insiders exclusively trade single names if the transaction cost is sufficiently small. When this condition is not met, however, insiders trade the index with positive probability. If information acquisition is endogenized, increasing transaction costs results in fewer agents becoming informed. It also leads insiders to trade the index with more frequency. These shifts cause both single names and the index to become less informative. Because market makers do not know if potential insider index trades come from A- or B-informed agents, the reduction in informational efficiency is more pronounced for single names.

We next empirically document changes in CDS trading patterns over time. Using quarterly liquidity data from the Depository Trust & Clearing Corporation (DTCC) from 2010 through 2019, we show that notional outstanding and daily volume have fallen dramatically for North American single-name CDS. For the major corporate indices, however, notional outstanding has grown and daily volume is relatively unchanged. The contrast suggests that regulation has driven market participants away from single names and into indices.

We then begin testing our central hypothesis that reforms have harmed price discovery in the single-name CDS market. Following Boyarchenko et al. 2018, we split the 10-year sample window into the "Pre-Rule" period, which spans from January 2010 through December 2013 and the "Rule Implementation" period, which extends from January 2014 through December 2019. The start and end dates of the full sample window ensure that results are not influenced by the GFC or COVID-19 pandemic. Consistent with prior studies, we find that spread movements anticipate rating changes in both the Pre-Rule and Rule Implementation periods. While there is no drift in spreads after downgrades in the former period, a 1.3-1.6% post-event drift emerges following the introduction of post-crisis regulation. This result indicates that CDS spreads incorporate information more slowly in the Rule Implementation period.

To determine if less private information is impounded in CDS spreads, we test for a decline in the shares of cumulative abnormal spread changes that occur before downgrades are announced. We compute the pre-event ratio (PER) by dividing the mean cumulative abnormal spread change from 90 days prior to two days prior to a rating downgrade by the mean cumulative abnormal spread change from 90 days prior to one day after a downgrade. If the CDS market fully anticipates rating changes, this ratio will be equal to one. If, however, spreads continue to increase after the event date, the ratio will be smaller than one. We find that the pre-event ratio is 6.5-7.2 percentage points lower in the Rule Implementation period. Though this analysis is not causal, it establishes that CDS spreads have become absolutely less informative. Decomposing the sample into finer time bins suggests that UMR is the primary catalyst for the decrease.

We next study if UMR also alters the lead-lag structure between CDS and corporate bond spreads. While Basel III and the Volcker Rule affect trading costs in both markets, margin reforms apply only to derivatives. We therefore reclassify September 2016 onward as the Post-UMR period and test for changes around this implementation date. Following Hilscher, Pollet, and Wilson 2015 and Lee, Naranjo, and Velioglu 2018, we use panel vector autoregressions (VARs) of percentage spread changes to measure the strength of the lead-lag relationship. We find that less information flows from CDS to bonds after margin rules are introduced. In the Pre-UMR period, a 1% increase in CDS spread is associated with a 0.2% increase in bond spread the following day. In the Post-UMR period, the same increase in CDS spread corresponds to only a 0.12% increase in bond spread.

If margin requirements cause the deterioration of informativeness in the CDS market, contracts most exposed to the regulation should drive the decline. Sequential trading models suggest that each incremental trade contributes to price discovery. It follows that reference entities with the most bilateral trading should be particularly affected by UMR. We classify entities as High- and Low-Volume based on their average daily notional traded in the quarter immediately preceding UMR adoption. We then estimate separate panel VARs for each group in both periods and conduct a difference-in-differences test. The drop in informativeness indeed stems from High-Volume entities. Decomposing the time periods into finer bins reveals no evidence of differential trends in the Pre-UMR period. Point estimates for Low-Volume entities are similar throughout the sample window, but the lag CDS change coefficients in regressions with bond spreads as the dependent variable drop markedly for High-Volume entities in the Post-UMR period.

Since the margin reforms apply only to bilateral positions, the results should be most pronounced for underlyings that are less likely to be cleared. We therefore compute the clearing propensity of each reference entity prior to UMR adoption and partition the High Volume group into three finer categories: Non-Clearable, Low Clearing Propensity, and High Clearing Propensity. We then estimate separate panel VARs for each group in both the Pre- and Post-UMR periods. The first lag CDS change coefficient in bond spread regressions declines in all three categories across periods, but the magnitude of the change is monotonically decreasing in clearing propensity. Further, while the difference for the Non-Clearable group is significant at the 1% level, its counterpart for the High Propensity group fails to achieve significance at even the 10% level. Again, these findings are consistent with UMR causing informativeness to deteriorate.

The final cross-sectional test is motivated by the model. Underlyings whose spread changes are highly correlated with the those of the CDS indices are more substitutable with the baskets. It follows that these reference entities should experience the largest reductions in informativeness when the relative cost of single-name trading rises. Since both central counterparty and UMR margins are determined using Value at Risk measures, underlyings with high index correlations may also incur larger margin charges, as they offer less portfolio diversification benefit. We therefore compute correlations between single-name and index spread changes prior to the introduction of UMR, then estimate a panel regression on bond spread changes that includes triple interactions of lagged spread changes, a post-UMR indicator variable, and the correlation coefficient. The estimate for the triple interaction term with the first lag of CDS spread changes is negative and statistically significant. This result indicates informativeness does, in fact, decline more sharply for highly substitutable reference entities.

We lastly study whether price discovery for CDS indices has been affected by post-crisis regulation. Because index contracts are primarily centrally cleared, margin reforms have had less impact on this segment of the market. Directly measuring the informational efficiency of indices is difficult, so we again test for differential changes in CDS and bond markets. Estimating VARs using index spread changes shows that CDS indices have not become less informative relative to bond indices. If anything, more information appears to flow from CDS spreads to bond spreads. This finding is consistent with the model's prediction that an increase in the relative transaction costs of single names prompts informed agents to trade indices instead.

This paper adds to the growing literature on the effects of post-crisis regulation on the CDS market. Loon and Zhong 2014 find that CDS spreads decrease after contracts become eligible for clearing, consistent with central counterparties reducing counterparty risk. Duffie, Scheicher, and Vuillemey 2015 use supervisory data to demonstrate that initial margin requirements for dealers greatly increase collateral demand across the entire market. Loon and Zhong 2016, Collin-Dufresne, Junge, and Trolle 2020, and Riggs et al. 2020 study how swap execution facilities have affected various aspects of the CDS index market. Bellia et al. 2019 demonstrate that counterparty risk and capital costs impact traders' decision to clear single-name positions. Boyarchenko, Costello, and Shachar 2019 examine how new rules have affected dealer behavior in various segments of the CDS market. Paddrik and Tompaidis 2019 find that prices in the single-name market have become more sensitive to dealer inventories.

We also contribute to the literature on price discovery in the CDS market. Hull, Predescu, and White 2004, Norden and Weber 2004 and Lee, Naranjo, and Velioglu 2018 find that CDS spreads anticipate rating changes. Longstaff, Mithal, and Neis 2003, Blanco, Brennan, and Marsh 2005, and Lee, Naranjo, and Velioglu 2018 demonstrate that CDS spreads lead corporate bond spreads. Evidence regarding the lead-lag relationship between credit derivatives and equities is more mixed. Acharya and Johnson 2007 show that information flows from the CDS to the stock market. Norden and Weber 2009 and Hilscher, Pollet, and Wilson 2015 conclude, on the other hand, that information flows from equities to CDS, but not the other way. In closely related work, Marra, Yu, and Zhu 2019 find that the ability of CDS-unique information to predict future stock returns has decreased as the CDS market has become more transparent.

The model is connected to the theoretical literature studying the effects of increased index trading. Subrahmanyam 1991 finds that the introduction of a basket prompts some noise traders to leave individual security markets. Their exit makes it more difficult to exploit private signals, so individual security prices reflect less security-specific information. Gorton and Pennacchi 1993 show that baskets enable uninformed agents to minimize their expected losses to insiders. Our framework is based largely on Ernst 2020, who allows informed agents to trade both single stocks and ETFs, and shows that ETFs can reveal stock-specific information. Bond and Garcia 2018 find that lowering the cost of indexing actually increases the informational efficiency of individual securities by pushing less-informed traders to the basket.

We proceed as follows. In Section 1.2 we provide background information on CDS and the relevant regulatory changes. In Section 1.3, we introduce the model and develop hypotheses. Section 1.4 describes the data and Section 1.5 discusses trends in CDS trading activity. Section 1.6 presents results on price discovery around rating changes, while Section 1.7 covers the lead-lag relationship between CDS and corporate bond spreads. Section 1.8 concludes.

1.2 Background

In this section, we provide an overview of the CDS market and the relevant regulatory measures that have been implemented since the GFC. Further institutional details about CDS can be found in Boyarchenko, Costello, and Shachar 2019, while additional information regarding post-crisis regulation, particularly as it relates to derivatives trading, is available in Boyarchenko et al. 2018.

1.2.1 Credit Default Swaps

A CDS is a derivative contract that provides synthetic insurance against a credit event affecting the underlying. Single-name CDS reference individual firms and require the buyer of protection to make quarterly premium payments to the seller of protection equal to one-fourth of the fixed coupon rate on the contract multiplied by the notional size of the contract. In the event of a credit event before the expiry of the swap, premium payments end and the seller pays the buyer the notional amount of the contract times one minus the recovery rate of the underlying bond. Liquidity in the North American corporate CDS market is concentrated in contracts that have a five-year tenor, are denominated in USD, reference senior unsecured debt, and do not permit financial restructuring.

As in equity markets, there exist index contracts that pool together CDS on a specified set of constituents. The two major North American corporate indices, the CDXNAIG and the CDXNAHY, are composed of the most heavily traded investment grade and high yield single names, respectively. Both indices are equal-weighted and rolled with an updated set of reference entities biannually. More complex instruments such as index options and tranches are also traded, but we focus exclusively on plain-vanilla single-name and index swaps.

1.2.2 Central Clearing

Because CDS are bilateral contracts, market participants bear *counterparty risk*, i.e. the risk that their trading partner fails to meet its obligations. The turmoil caused by the declines of Lehman Brothers, Bear Stearns, and AIG during the financial crisis made apparent the threat that large counterparty exposures pose to financial stability. Title VII of the Dodd-Frank Act, which was passed in the wake of the crisis, includes measures intended to minimize the systemic risk generated by derivatives trading. A key component of this regulation was the introduction of central clearing. Prior to the crisis, CDS were traded in a traditional dealer-intermediated OTC market. Under the clearing model, parties enter contracts facing a tightly-governed central counterparty (CCP), instead of one another. Thus, the only source of counterparty risk is the CCP itself. The clearinghouse is capitalized by its members, which are typically large dealer banks, and requires that market participants adhere to stringent margining rules.[1]

Clearing was implemented on a voluntary basis for the most liquid reference entities in 2009. Uptake for indices was robust, but adoption by non-dealers in the single-name market was low, as initial margin requirements for cleared portfolios typically exceeded the amounts demanded by dealers on uncleared positions (Rennison 2015). Between March and September of 2013, the Commodity Futures Trading Commission (CFTC) phased in a series of rules that mandate nearly all standard contracts referencing the major indices be centrally cleared. Most single name trades are now eligible to be cleared, but clearing remains voluntary in this segment of the market. As of December 2020, non-dealers had cleared roughly 60% of their gross index exposure, but only 25% of their gross single-name exposure (Du et al. 2020).

1.2.3 Basel III

The Basel III regulatory framework was also developed in response to the financial crisis. It imposes stricter capital requirements on banks than prior Basel Accords in order to improve the resilience of the financial sector in periods of stress. A key component of Basel III is the Supplementary Leverage Ratio (SLR). The US version of the ratio was proposed in July 2013, finalized in 2014, and officially took effect in

1. Capponi et al. 2020 find that clearinghouse collateralization rates are an order of magnitude larger than those implied by standard Value-at-Risk rules.

2018.[2] The SLR numerator is equal to Tier 1 capital, while the denominator is the sum of on-balance sheet and certain off-balance sheet assets. The ratio is computed without applying risk adjustments, as it is intended to protect against model and measurement error that may arise from risk-based weights. Large US banks must maintain an SLR of at least 3%, while globally systemically important banks are required to keep a minimum ratio of 5%.

Both the replacement values and potential future exposures (PFEs) of a bank's CDS positions are included in its SLR denominator. The former are based on the mark-to-market values of outstanding contracts, while the latter are calculated using both net and gross notional positions. Per SLR guidelines, netting across counterparties is not allowed when computing PFEs. Because dealers have large gross exposures relative to their net positions, this restriction makes it costly to intermediate derivative markets that, like the single-name CDS market, have a sizable bilateral segment. The SLR is less onerous for index intermediation, as all cleared trades face the CCP and are therefore able to be netted.

1.2.4 Volcker Rule

Section 619 of the Dodd Frank Act, known colloquially as the Volcker Rule, was initially proposed in 2011, but was not implemented until April 2014. It prohibits large dealer banks from engaging in proprietary trading and limits their ability to sponsor or invest in hedge funds and private equity funds. The Volcker Rule is

2. Choi, Holcomb, and Morgan 2020 show that while compliance was not required until 2018, institutions affected by the SLR began adjusting their securities holdings and deleveraging immediately after the finalization date in 2014. They find no evidence of additional changes around the actual compliance date.

intended to prevent incidents such as the London Whale scandal of 2012, in which large CDS positions taken by a derivatives trader at JPMorgan resulted in losses exceeding $6 billion. The rule does permit depository institutions to continue to trade for market making purposes. Compliance costs associated with the Volcker Rule have proven to be burdensome, however, and recent literature shows that it has negatively impacted corporate bond liquidity (e.g. Bao, O'Hara, and Zhou 2018; Bessembinder et al. 2018).

1.2.5 Uncleared Margin Requirements

In order to mitigate the counterparty risk associated with bilateral swap trading, market participants may exchange collateral. Such collateralization takes two forms: *initial margin*, which is posted at trade inception and retained until a contract is terminated or matures, and *variation margin*, which is exchanged during the life of the contract to cover changes in its market value over time. Traditionally, margin terms were negotiated bilaterally and, thus, quite varied. Dealers, for example, did not exchange initial margin with one another, while clients would often exchange variation margin with dealers but post initial margin only unilaterally.

To standardize and strengthen collateralization practices, the Basel Committee and International Organization of Securities Commissions (IOSCO) introduced a set of Uncleared Margin Rules (UMR) in 2016. The new requirements were made intentionally stringent, as regulators sought to incentivize central clearing in addition to promoting financial stability (BIS 2015). The UMR mandate that all market participants exchange both initial and variation margin. To prevent rehypothecation, they also stipulate that collateral be held by third-party custodians. Bilateral margin is determined at the netting set level, where these sets broadly correspond to derivative asset classes (e.g. credit, commodities). Required initial margin may be computed using either a standard schedule or a regulator-approved model that assesses the risk of the entire portfolio. Market participants are not allowed to switch between model- and schedule-based calculations for transactions within the same netting set.

Under the schedule-based approach, *gross initial margin* is computed by first multiplying the notional amount of each open contract by a standard margin rate, then summing across contracts. For credit derivatives, the standard rate ranges from two to ten percent depending on contract tenor. The gross initial margin is then adjusted based on the net-to-gross ratio across all positions. While the simplicity of the schedule may appeal to small or infrequent market participants, dealers and other sophisticated traders tend to use models, as they account for diversification within portfolios. Regulatory guidelines dictate that the latter approach set initial margin according to the 99% loss quantile of the netting set over a horizon of 10 days.

The first phase of UMR, which introduced initial margin standards for the largest market participants, was implemented in September 2016. Variation and initial margin rules impacting an increasing number of traders have since been instituted. These provisions have made it substantially more costly for dealers, in particular, to hold bilateral positions, and prompted the widespread adoption of clearing in more homogeneous derivative classes such as interest rate swaps (Barnes 2017). Given the disparity in clearing rates across segments of the CDS market (Du et al. 2020), the new margin requirements have also impacted trading costs more for single names than for indices.

1.3 The Model

We begin by developing a model to generate predictions about the effects of regulatory changes on price discovery in the CDS market. The framework builds upon Ernst 2020, most notably by adding transaction costs and endogenizing information acquisition. All proofs are presented in Appendix 1.A.

1.3.1 Setting

The market consists of single-name securities, A and B, and an index composed of the two. At future date 1, the single names pay independent liquidating dividends, $\hat{V}_A$ and $\hat{V}_B$, that are equally likely to be 0 or 1. The index is equal-weighted, so $\hat{V}_I = (1/2)\hat{V}_A + (1/2)\hat{V}_B$.

At time 0, risk neutral, competitive market makers face either a strategic insider who knows the value of one of the single names or an uninformed liquidity trader. Market makers begin with prior beliefs $P(\hat{V}_A = 1) = P(\hat{V}_B = 1) = 1/2$ and, similar to other models in the style of Glosten and Milgrom 1985, update their expectations based on the observed order. Market makers do not know if an agent is informed, so their only signal is the order itself.

Both informed and uninformed agents incur a linear transaction cost, c, when trading security A or B. As in Oehmke and Zawadowski 2017, c reflects dealers' cost of efficient liquidity provision and inventory management in addition to expenses, such as initial margin, that are directly borne by customers. There is no analogous c for trading indices, consistent with the empirical evidence that such costs are larger for single names (e.g. Capponi et al. 2020). As noted in the previous section, Basel

III and UMR affect intermediation and collateralization more for bilateral trades than for cleared trades. Since clearing rates are substantially higher for indices than for single names, our maintained assumption is that the reforms raise transaction costs more for the latter. We therefore use increases in c to represent the changes in relative trading costs of single-name and index CDS brought about by post-crisis regulation.

Initially, a fraction α of traders are informed. Half of these agents observe the final value of security A, while the other half observe the final value of security B. Informed agents are risk neutral and strategic. They submit orders that maximize their expected profits. These agents trade either the single-name security whose value they know or the index, but they are allowed to randomize the choice.[3]

The remaining $1 - \alpha$ fraction of agents are uninformed and trade in response to unmodeled inventory shocks. To keep the share of informed traders constant across asset classes, a quarter of the uninformed agents are security A liquidity traders, a quarter are security B liquidity traders, and the remaining half are index liquidity traders. Uninformed agents are restricted to trading a single unit of their associated security. They buy and sell with equal probabilities. Informed agents must mimic the order sizes of their uninformed counterparts, because market makers would price discriminate if they could determine the identities of traders based on the quantities demanded or supplied.

3. Not allowing simultaneous trades is a standard assumption in sequential trade models with multiple assets (e.g. Easley, O'hara, and Srinivas 1998).

1.3.2 Equilibrium

In equilibrium, all agents act optimally. Informed traders submit orders that maximize their profits and market makers quote prices that earn them zero expected profit. Rational expectations also hold. Informed traders correctly determine how their orders will affect prices and market makers correctly infer the strategies of these insiders.

After establishing how transaction costs affect the equilibrium for a given level of α, we endogenize information acquisition and study the effects of increases in c. In these exercises, the expected profits of informed agents must equal the cost of acquiring information, which we denote g. To streamline exposition, we focus solely on symmetric equilibria. That is to say, we limit discussion to cases where security A and security B informed traders employ the same strategies.

1.3.3 Preliminary Lemmas

Before presenting the central propositions about informed trader strategies, we prove two lemmas about market maker beliefs and informational efficiency. Absent asymmetric information, market makers would set the price of each security equal to its unconditional expected value. The presence of informed traders forces market makers to charge a bid-ask spread in to avoid expected losses. As shown in the following lemma, because all agents trade the same quantities, market makers' inferences are based solely on the signs of orders.

Lemma 1. *Let ϕ_j be the probability that a j-informed agent trades the single-name security $j \in \{A, B\}$ instead of the index. The bid and ask prices for security j are*

$$ask_j = \frac{4\phi_j\alpha + 1 - \alpha}{4\phi_j\alpha + 2 - 2\alpha}$$
$$bid_j = \frac{1 - \alpha}{4\phi_j\alpha + 2 - 2\alpha}.$$

For symmetric equilibria, $\phi_A = \phi_B = \phi$, so for the index

$$ask_I = \frac{2\alpha - 3\alpha\phi + 1}{2(\alpha - 2\alpha\phi + 1)}$$
$$bid_I = \frac{1 - \alpha\phi}{2(\alpha - 2\alpha\phi + 1)}.$$

In both cases, bids decrease and asks increase as information asymmetry becomes more pronounced. Informed traders directing more of their orders towards the index therefore narrows the bid-ask spread for single names, but widens it for the index.

Our ultimate goal is to understand how increases in transaction costs affect the informativeness of markets. Let $E[\hat{V}_j|Q]$ be the expected value of security j conditional on the observed order Q. As in Biais and Hillion 1994, the difference between the conditional expectation of the security value and its true value, $E[\hat{V}_j|Q] - V_j$, can be interpreted as an estimation error. As the variance of this error increases, the informational efficiency of the market decreases. Expressions for the estimation errors of the single-name securities and the index are derived in the following lemma.

Lemma 2. *The estimation error for single-name security j conditional on order Q is*

$$Var(E[V_j|Q] - V_j) = \frac{1}{4} - \frac{\phi^2\alpha^2}{4(2\phi\alpha - \alpha + 1)} - \frac{(1-\phi)^2\alpha^2}{8(\alpha - 2\phi\alpha + 1)}$$

with $j \in \{A, B\}$. For the index it is

$$Var(E[V_I|Q] - V_I) = \frac{1}{8} - \frac{\phi^2\alpha^2}{8(2\phi\alpha - \alpha + 1)} - \frac{(1-\phi)^2\alpha^2}{8(\alpha - 2\phi\alpha + 1)}.$$

1.3.4 Informed Trader Strategies

We now explore how the transaction cost, c, affects informed traders' strategies and, by extension, the informativeness of the single-name securities and index. Informed agents choose whether to mimic single name or index liquidity traders based on which order will yield a higher payoff. With a fixed share of informed traders, α, the lower the transaction cost, the more profitable it is to trade single names. As shown in the following proposition, it is therefore the case that when c is small, informed agents opt to only trade single-name securities.

Proposition 1. *For a given level of informed trading, α, the pure-strategy equilibrium in which informed agents always trade single names prevails when the transaction cost, c, is sufficiently small. Increasing c leads the fraction of informed traders to decrease and, as a result, reduces the informational efficiencies of both the single-name securities and the index.*

Market makers learn about the true index value from informed single-name trades, so the informational efficiency of the index drops as α declines, even in the pure-strategy equilibrium where all informed agents trade the single names. As shown in the following proposition, increasing c does not affect informativeness in the pure-strategy equilibrium where insiders only trade the index, because doing so does not decrease expected profits.

Proposition 2. *For a given level of informed trading, α, the pure-strategy equilibrium in which informed agents always trade the index obtains when the transaction cost, c, is sufficiently large. The informational efficiencies of both the single-name securities and the index are unaffected by increases in c.*

The estimation error for single names in the equilibrium in which informed agents only trade the individual securities is

$$\frac{1}{4}\left[1-\frac{\alpha^2}{1+\alpha}\right].$$

In the equilibrium in which informed agents only trade the index it is

$$\frac{1}{4}\left[1-\frac{\alpha^2}{2(1+\alpha)}\right].$$

The first quantity is clearly smaller for $\alpha \in (0,1)$, which implies that for a fixed share of informed traders, informational efficiency is higher in the former equilibrium. The estimation error for the index, however, is

$$\frac{1}{8}\left[1-\frac{\alpha^2}{1+\alpha}\right].$$

in both equilibria.

The decrease in informational efficiency for the single-name securities makes intuitive sense, since all trading in the markets for A and B becomes uninformed. Furthermore, index trades provide only a noisier signal of single name values because, from the perspective of market makers, informed orders could stem from either A- or B-informed agents. This uncertainty does not affect the accuracy of market makers' expectations for the final index value.

Thus far, we have considered only pure-strategy equilibria that obtain when transaction costs are sufficiently small or large. As discussed in the following proposition, for intermediate values of c, informed traders randomize between the single-name securities and the index.

Proposition 3. *For intermediate transaction costs, informed agents mix between single names and the index. When the transaction cost, c, is increased, the share of informed traders, α, decreases and the probability of trading the index, ϕ, rises. These shifts lead to reduced informational efficiency of both the index and the single-name securities, but the decline is less pronounced for the index.*

Mixed strategies prevail under certain conditions, as they allow informed agents to exploit their insider knowledge while simultaneously tempering information revelation. Changes in the transaction cost alter the share of informed agents as well as their mixing probabilities, so they ultimately affect informational efficiency.

1.3.5 Empirical Implications

The model predicts that the increased transaction costs associated with post-crisis regulation will lead to less informed trading in all segments of the CDS market. Since single names become relatively more costly to trade, insiders will direct more of their orders towards indices. This change will ultimately lead the decline in informational efficiency to be more pronounced for single names than for indices. While the hypotheses may seem straightforward, the existing theoretical literature, which primarily centers on equities and the adoption of ETFs, yields mixed predictions about the effects such shifts in trading behavior will have on informativeness. In the baseline model of Bond and Garcia 2018, for example, a relative decrease in indexing costs leads less-informed agents to exit single-name markets and enter index markets. The change in participation causes price efficiency to increase for single names and decline for indices.

Given that the model includes only two single-name securities, it may be unclear if the hypotheses hold when the number of index members is large. While liquidating dividends in the model are purely idiosyncratic, single name price movements are correlated in practice. Indices with many constituents therefore remain a viable alternative for exploiting private information that pertains to multiple reference entities.

1.4 Data Description

1.4.1 Credit Default Swaps

End-of-day par spreads for single-name CDS come from Markit. The vendor provides composite spreads calculated using indicative dealer quotes. We utilize spreads for contracts written on North American corporate entities that have a five-year tenor, allow no restructuring, are denominated in U.S. dollars, and reference senior unsecured debt. Index constituent information also comes from Markit, while end-of-day par spreads for on-the-run CDX contracts with a five-year tenor are pulled from Bloomberg.

Liquidity data come from the Depository Trust & Clearing Corporation (DTCC). Every quarter, DTCC publishes trading activity metrics including the number of active dealers, trades per day, and average daily notional for each of the "Top 1000" single-names. While this limit could potentially give rise to truncation issues, entities at the bottom of the list average less than 1 trade and $2.5MM notional volume per day. DTCC rounds average daily notionals to the nearest $2.5MM for amounts less than $25MM and up to the nearest $25MM for amounts over $25MM.

Weekly data on gross notional open interest for the Top 1000 reference entities also come from DTCC. These data are only available from January 2010 through February 2017, however. Daily data on cleared notional open interest for eligible entities come from ICE Clear Credit, the primary clearinghouse for North American corporate CDS.

To avoid the influence of the GFC and the COVID-19 Pandemic, the sample period begins in January 2010 and ends in December 2019. Following Boyarchenko et al. 2018, we classify January 2010 through December 2013 as the "Pre-Rule" period and January 2014 onward as the "Rule Implementation" period.

1.4.2 Corporate Bonds

Bond pricing information comes from the TRACE database, which covers nearly the entire universe of public transactions during the sample period. The data are cleaned using the procedure outlined in Dick-Nielsen 2014. As bonds are infrequently traded, we follow Lee, Naranjo, and Velioglu 2018 and compute bond yield spreads by subtracting maturity-matched treasury yields from the reported transaction yields. We then calculate daily volume-weighted average bond yield spreads at the nine-digit CUSIP level. Bonds are matched to CDS using Markit's Reference Entity Database in conjunction with header information.

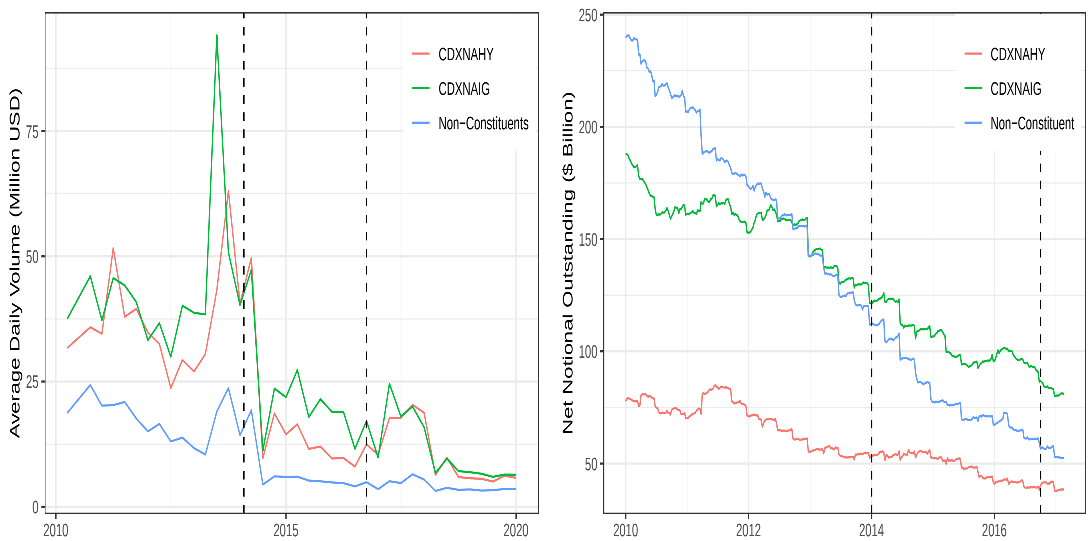
Bond characteristic information comes from Mergent Fixed Income Securities Database (FISD). Following Jostova et al. 2013, we drop preferred shares, non-US dollar denominated bonds, bonds with atypical coupons, bonds with warrants, bonds that are mortgage or asset backed, and bonds that are convertible or part of unit deals. To ensure congruence with CDS contracts, we also require bonds be senior unsecured.

The sample of credit events also comes from Mergent FISD. Due to data availability, we use security-, not entity- level S&P rating changes. Reassuringly, for firms with multiple active nine-digit CUSIPs, security-level changes cluster on the same date. Moreover, based on a manual assessment, security-level changes appear to align with their entity-level counterparts. To ensure results are not driven by extraneous events, we delete any rating changes that clearly impact only a single security and not the entity as a whole.

1.5 CDS Trading Activity

In this section, we explore how trading activity in the CDS market has evolved over time. Figure 1.1 depicts the average daily notional volume and net notional outstanding for North American corporate single-names that appear on DTCC's Top 1000 list. Entities are grouped based on their index membership. In both panels, the first dashed vertical line represents the boundary between the Pre-Rule and Rule Implementation periods, while the second represents the first UMR phase-in date. The left panel shows that trading volume is appreciably higher for index constituents than non-constituents in the Pre-Rule period. There is a sharp drop in daily notional across all three categories from 2014 onward. By the end of 2019, the average notional for constituents of both indices is roughly \$5MM, which is only slightly larger than that of non-constituents.

Figure 1.1: Single-Name Trading Activity



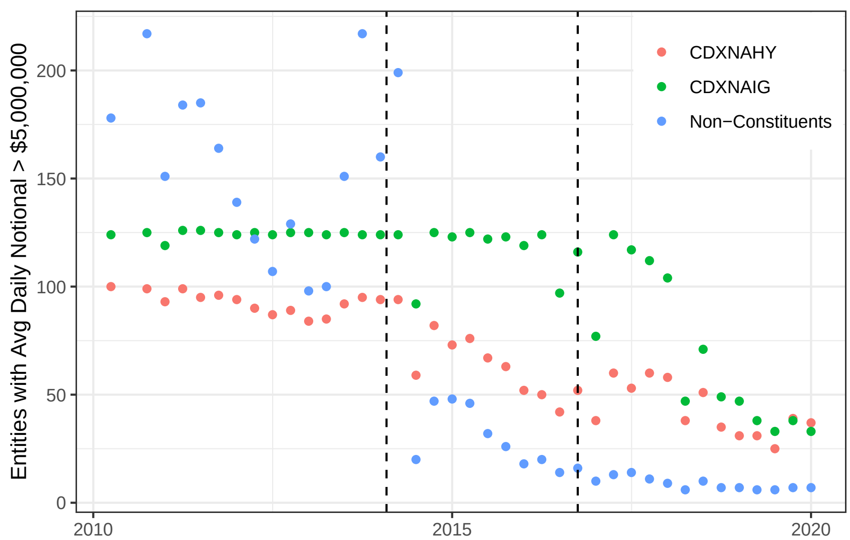
Notes: The left panel depicts the average daily notional volume for North American corporate entities that appear on DTCC's Top 1000 Single-Names table. The right panel depicts the total net notional outstanding for the same set of reference entities. The CDXNAIG and CDXNAHY groups are composed of members of the respective indices, while Non-Constituents are underlyings not listed on either index. The first dashed vertical line represents the boundary between the Pre-Rule and Rule Implementation periods, while the second represents the beginning of UMR phase-ins.

The right panel shows that notional outstanding is also markedly higher for all three groups in the Pre-Rule period. While the figure highlights that market activity has decreased, the introduction of multilateral compression and central clearing during the sample period make it difficult to directly compare magnitudes over time.

Both practices result in market participants netting gross exposures across counterparties, which partially explains the reduction in notional outstanding.

To ensure that heterogeneity is not being masked by computing means across single names, we next plot the number of reference entities with an average daily notional greater than $5MM in a given quarter. The number of non-constituents that pass this threshold is volatile in the Pre-Rule period, but quickly falls to just above zero in the Rule Implementation period. Members of the CDXNAHY also begin to fall below the $5MM level in 2014. While CDXNAIG constituents are actively traded at the beginning of the Rule Implementation period, their volume drops once UMR requirements are introduced.

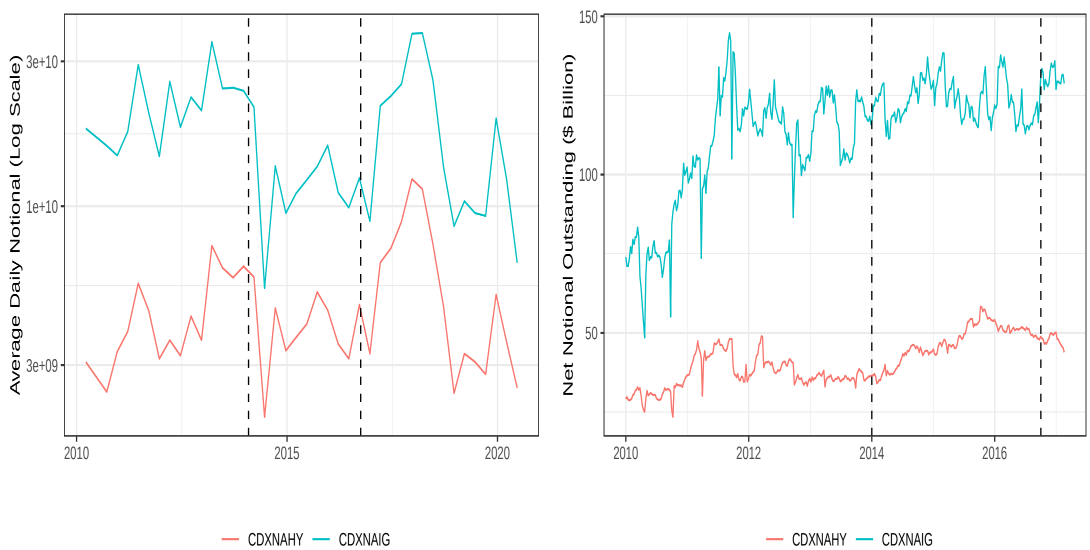
Figure 1.2: Number of Frequently Traded Single-Names



Notes: This figure presents the number of reference entities on DTCC's Top 1000 list with an average daily notional volume greater than $5MM. The CDXNAIG and CDXNAHY groups are composed of members of the respective indices, while Non-Constituents are underlyings not listed on either index. The first dashed vertical line represents the boundary between the Pre-Rule and Rule Implementation periods, while the second represents the beginning of UMR phase-ins.

We also study trading activity for the major North American corporate indices. The left panel of Figure 1.3 depicts the average daily notional for the on-the-run CDXNAIG and CDXNAHY. The number of trades is fairly volatile, but unlike in the single-name market, there is no notable drop during the sample period. The right panel shows that notional outstanding increases over time, particularly between 2010 and 2012. Again, it is difficult to compare magnitudes at various points in the sample due to the introduction of central clearing.

Figure 1.3: CDX Trading Activity



Notes: The left panel depicts the average daily notional volume for the on-the-run CDXNAIG and CDXNAHY. The right panel depicts the net notional outstanding for the same underlyings. The first dashed vertical line represents the boundary between the Pre-Rule and Rule Implementation periods, while the second represents the beginning of UMR phase-ins.

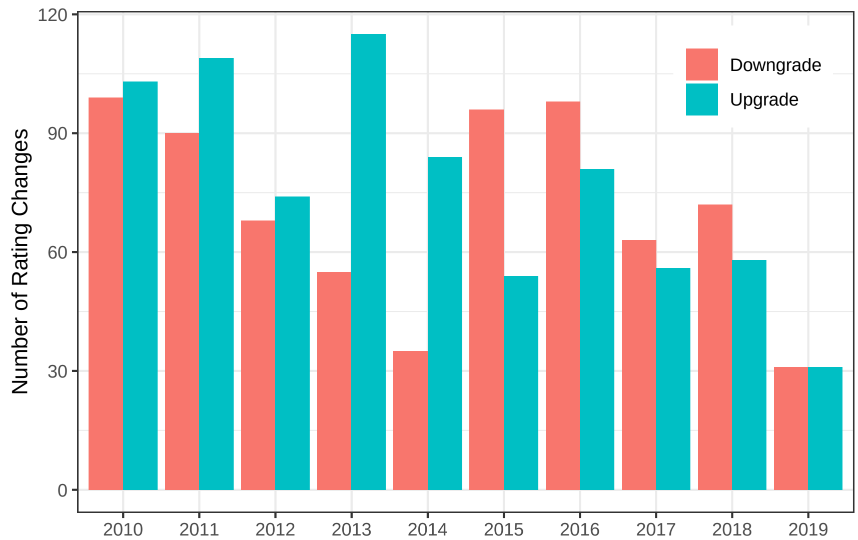
1.6 CDS Reaction to Rating Changes

The exploratory analysis in the previous section reveals that trading volume for single-name CDS has declined in the Rule Implementation period. In this section, we use windows around credit rating changes to test if the decrease has coincided with single-name spreads becoming less informative.

Beginning with Hull, Predescu, and White 2004, a series of papers have shown that the CDS market anticipates rating downgrades. The evidence that the market anticipates upgrades has been mixed. While both types of changes elicit reactions immediately around announcement dates, neither has been shown to generate drift after a rating event. A drop in CDS informativeness could manifest in two ways: a weaker anticipatory effect or the emergence of post-event drift. The former would suggest market participants are less inclined to exploit private information in the single-name CDS market, while the latter would indicate that prices are slower to incorporate public information. We formally test both channels.

Figure 1.4 plots the number of upgrades and downgrades that occur in each year of the sample period. While the distribution is not uniform across time, neither the 707 downgrades nor the 765 upgrades are concentrated in a particular period. For each rating change, we use data from event days -200 through -91 to estimate the model $SC_{it} = \beta_i MKTSC_t + \epsilon_{it}$, where SC_{it} is the percentage change in entity i's spread on date t and, for investment grade (high yield) entities, $MKTSC_t$ is the percentage change in the CDXNAIG (CDXNAHY) spread. Similar to Loon and Zhong 2014, we compute the abnormal percent spread change $ASC_{it} = SC_{it} - \hat{\beta}_i MKTSC_t$, where $\hat{\beta}_i$ is the estimated market beta. The cumulative abnormal spread change in the event window $[\tau_1, \tau_2]$ is then $CASC_i^{(\tau_1,\tau_2)} = [\prod_{\tau=\tau_1}^{\tau_2}(1 + ASC_{i\tau})] - 1 - \hat{\beta}_i[\prod_{\tau=\tau_1}^{\tau_2}(1 + MKTSC_\tau) - 1]$.

Figure 1.4: Distribution of Rating Changes Over Time



Notes: This figure presents the number of rating events for firms with traded CDS between January 2010 and December 2019. In total, there are 707 downgrades and 765 upgrades.

Following Hull, Predescu, and White 2004, we separately consider abnormal spread changes that occur in event day windows $[-90, -2]$, $[-1, 1]$, and $[2, 10]$. These bins allow us to determine if spreads react prior to, during, and after rating events, respectively. Table 1.1 presents the mean cumulative abnormal spread changes in each bin. The pre-event column in the top panel reveals that the CDS market anticipates downgrades in both the Pre-Rule and Rule Implementation periods. The column corresponding to the [-1,1] bin shows that there is also significant movement in spreads during the small window around rating change dates in both periods. There are no significant spread changes in the [2,10] window for downgrades during the Pre-Rule period, but a 1.6% post-event drift emerges in the Rule Implementation period. This finding is the first indication that the CDS market may incorporate public information less quickly after the adoption of post-crisis regulation.

The bottom panel of Table 1.1 presents analogous results for upgrades. The [-90, -2] column again indicates that the market anticipates rating changes in both the Pre-Rule and Rule Implementation periods. Like prior studies on stock price and CDS spread changes, we find that the magnitudes of estimates are appreciably smaller for upgrades (e.g. Hull, Predescu, and White 2004). There are significant decreases in spreads in the narrow window around rating change dates, but no post-event drifts in either period. Given that upgrades yield only modest spread changes even in the Pre-Rule period, we consider only downgrades when evaluating the effects of post-crisis regulation in the remainder of this section.

We first formally test if there is more post-event drift in the Rule Implementation period than the Pre-Rule period. To do so parsimoniously, we estimate the following regression

$$CASC_{it}^{(2,10)} = \alpha + \beta RuleImplementation_t + X_{it} + \epsilon_{it} \tag{1.1}$$

where $RuleImplementation_t$ is an indicator equal to one if the rating change takes place from January 2014 onward and X_{it} is a vector of controls that includes an indicator equal to one if the entity is investment grade prior to the rating change as well as the return of the S&P500, the change in the VIX, and the change in the Jurado, Ludvigson, and Ng 2015 measure of macroeconomic uncertainty over the three months preceding the full event window. If drifts are higher in the Rule Implementation period, β will be positive.

Results are presented in Table 1.2. The estimates are similar and statistically significant in both specifications. The cumulative abnormal spread change is 1.3–

Table 1.1: CDS Reactions to Rating Changes

	Period	N	[-90, -2]	[-1, 1]	[2, 10]
Downgrades					
All	Pre-Rule	312	0.1408***	0.0060**	−0.0003
			(0.0177)	(0.0029)	(0.0042)
	Rule Implementation	395	0.1613***	0.0161***	0.0158***
			(0.0206)	(0.0029)	(0.0041)
High Yield	Pre-Rule	147	0.1875***	0.0094**	0.0006
			(0.0276)	(0.0043)	(0.0059)
	Rule Implementation	165	0.2035***	0.0185***	0.0225***
			(0.0379)	(0.0051)	(0.0076)
Investment	Pre-Rule	165	0.0991***	0.0030	−0.0011
Grade			(0.0222)	(0.0038)	(0.0059)
	Rule Implementation	230	0.1310***	0.0144***	0.0110**
			(0.0226)	(0.0034)	(0.0045)
Upgrades					
All	Pre-Rule	401	−0.0360***	−0.0118***	−0.0008
			(0.0104)	(0.0019)	(0.0028)
	Rule Implementation	364	−0.0248**	−0.0094***	−0.0014
			(0.0111)	(0.0019)	(0.0031)
High Yield	Pre-Rule	217	−0.0629***	−0.0162***	−0.0003
			(0.0138)	(0.0029)	(0.0039)
	Rule Implementation	191	−0.0564***	−0.0111***	0.0021
			(0.0164)	(0.0030)	(0.0046)
Investment	Pre-Rule	184	−0.0043	−0.0065***	−0.0014
Grade			(0.0154)	(0.0023)	(0.0040)
	Rule Implementation	173	0.0101	−0.0075***	−0.0053
			(0.0143)	(0.0024)	(0.0042)

Notes: This table presents mean cumulative abnormal percentage CDS spread changes in various time bins around rating events between January 2010 and December 2019. Standard errors are reported in parentheses. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

1.6% larger in the Rule Implementation period. This finding affirms that the CDS market incorporates information more slowly than it did before the introduction of post-crisis regulation.

Table 1.2: Change in Post-Event Drift

	$CASC^{(2,10)}$	$CASC^{(2,10)}$
Rule Implementation	0.0161***	0.0131**
	(0.0060)	(0.0067)
Pre-Rule Mean	-0.0003	0.0006
N	707	707
Controls	N	Y

Notes: This table presents results when Equation 1.1 is estimated for rating downgrades between January 2010 and December 2019. The dependent variable is the cumulative abnormal spread change in the window 2 through 10 days after a downgrade. The controls are an indicator equal to one if the entity was investment grade prior to the change as well as the return of the S&P 500, the change in the VIX, and the change in the Jurado, Ludvigson, and Ng 2015 measure of macroeconomic uncertainty over the 3 months preceding the full event window. In the specification with controls, the Pre-Rule Mean is computed using the average values of the control variables. Standard errors are reported in parentheses. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

We next test if less information is impounded in CDS spreads prior to rating events in the Rule-Implementation period. To do so, we compute the share of the cumulative abnormal spread response that occurs before a downgrade. We then test whether this pre-event ratio (PER) is larger in the Pre-Rule period. The measure is analogous to the Delayed Response Ratio (DRR) used by DellaVigna and Pollet 2009 in the earnings announcement literature. The measure is computed $PER^{90,-2}_{-90,1} = \overline{CASC}^{(-90,-2)} / \overline{CASC}^{(-90,1)}$, where $\overline{CASC}^{(\tau_1,\tau_2)}$ is the mean cumulative abnormal percentage spread change in the window $[\tau_1, \tau_2]$. Means are calculated using regression coefficients estimated from Equation 1.1. For specifications with controls, ratios are computed at the mean control values. If the CDS market fully anticipates rating events, the pre-event ratio will be equal to one. If, however, an appreciable share of the reaction occurs during or after a rating change, the ratio will be less than one.

The first two rows of Table 1.3 present pre-event ratios in the Pre-Rule and Rule Implementation periods. Across specifications, the ratio is statistically different than one in both periods. The magnitudes of the coefficients are, however, 6.5–7.2% smaller in the latter period. The third row demonstrates that the difference of the ratios across periods is statistically significant, albeit less so when controls are introduced. The results indicate that CDS spreads incorporate less private information prior to rating changes in the Rule Implementation period.

The findings thus far demonstrate that single-name CDS spreads have become less informative since January 2014. We now begin to build the case that regulatory changes are behind the decline. In Figure 1.5, mean post-event drifts and pre-event ratios are plotted when rating events are grouped into narrower time bins. The vertical lines extending from each point represent 95% confidence intervals for the estimate. The two leftmost points in the left panel reveal that post-event drifts are very close to zero in both of the bins comprising the Pre-Rule period. In each of the three bins that constitute the Rule Implementation period, however, the mean drifts are positive and significant at the 10% level. The large estimate in the final bin reveals that informativeness deteriorated markedly after the first UMR phase-in date. The right panel depicts a similar pattern for pre-event ratios.

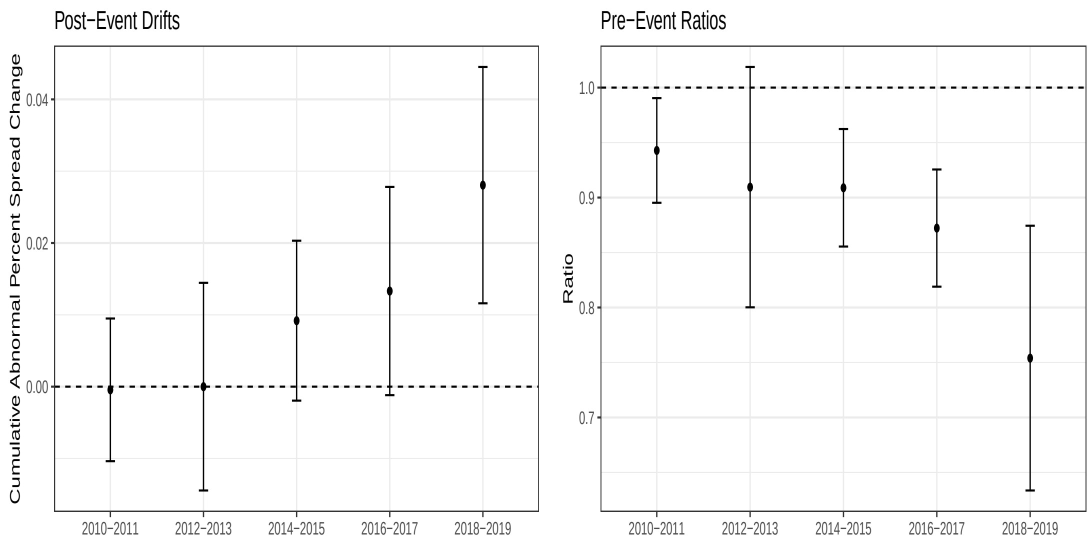
The decomposition into narrower time bins suggests that SLR and UMR both negatively affect price discovery in the CDS market, but that the latter has a more pronounced impact. Cross-sectional analysis is ultimately required to conclude that

Table 1.3: Change in Pre-Event Ratio

Period	PER	PER
Pre-Rule	0.9347^{***}	0.9408^{*}
	(0.0244)	(0.0333)
Rule Implementation	0.8697^{***}	0.8690^{***}
	(0.0179)	(0.0253)
Rule Implementation - Pre-Rule	-0.0650^{**}	-0.0718^{*}
	(0.0303)	(0.0418)
N	707	707
Controls	N	Y

Notes: The first two rows of this table present the pre-event ratio (PER) of cumulative abnormal spread changes around rating downgrades in the Pre-Rule and Rule Implementation periods. Ratios are computed by dividing the mean cumulative abnormal spread change (CASC) from event day -90 to -2 by the mean CASC from event day -90 to 1. Means are calculated using regression coefficients estimated from Equation 1.1. The controls are an indicator equal to one if the entity was investment grade prior to the change as well as the return of the S&P 500, the change in the VIX, and the change in the Jurado, Ludvigson, and Ng 2015 measure of macroeconomic uncertainty over the 3 months preceding the full event window. For specifications with controls, ratios are computed at the mean control values. Standard errors computed using the delta method are presented in parentheses. In the first two rows, t-tests are used to determine if the ratios are statistically different than one. In the third row, we test if the differences across periods are statistically different from 0. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Figure 1.5: Pre-Announcement Ratios and Post-Event Drifts in Narrower Time Bins



Notes: The left panel plots the mean post-event drift, defined as the cumulative abnormal spread change (CASC) from event day 2 to 10, when rating changes are grouped into two-year time bins spanning the full sample period. The right panel plots pre-event ratios for the same set of time bins. Ratios are computed by dividing the mean CASC from event day -90 to -2 by the mean CASC from event day -90 to 1. The vertical lines represent 95% confidence intervals for the estimates.

regulation causes the decline, but the limited number of rating downgrades makes it difficult to conduct tests with sufficient statistical power. We address this shortcoming in the next section by studying changes in the relative informativeness of single-name CDS and corporate bond spreads.

1.7 Lead-Lag Relationship Between CDS and Bonds

A sizable literature dating back to Longstaff, Mithal, and Neis 2003 and Blanco, Brennan, and Marsh 2005 establishes that the single-name CDS market leads the corporate bond market in the price discovery process. Motivated by the decline in informativeness documented in the previous section, we investigate if this lead-lag structure has also changed. If the results thus far are driven by structural reforms to derivative markets, then CDS spreads should lead bond spreads more weakly toward the end of the sample period. Alternatively, if the drop in pre-event ratio and the emergence of post-event drift are the result of other factors, such as systematic modifications to S&P's rating change criteria, the lead-lag relationship between CDS and bond spreads should be stable over time.

Like its CDS counterpart, the corporate bond market has faced broad regulatory changes since the GFC. It bears clarifying, then, why we might expect to find a shift in relative informativeness. Recent literature shows that bond dealers have responded to post-crisis reforms by committing less capital to intermediation and, instead, pre-arranging trades between buyers and sellers (e.g., Bessembinder et al. 2018; Goldstein and Hotchkiss 2020). While CDS trades may require less capital, offsetting derivative contracts in a similar manner does not provide full regulatory relief, because SLR guidelines limit the netting of gross exposures across counterparties.

Since the Volcker Rule also affects the largest intermediaries in both markets, the initial reforms should have comparable effects on price discovery for CDS and bonds. Margin requirements, on the other hand, apply only to derivatives. We therefore reclassify September 2016 onward as the Post-UMR period, and test for differences in the lead-lag relationship across time.[4]

Prior studies have documented large shifts in bond market liquidity and transaction costs in the years immediately following the crisis (e.g., Anderson and Stulz 2017; Bessembinder et al. 2018), so we truncate the sample prior to May 2013 to avoid the undue influence of differential trends that may occur well before the introduction of margin rules. This start date is chosen to make the Pre- and Post-UMR periods the same length, but all results in this section are robust to shifting it in either direction.

1.7.1 Single Names

To measure the strength of the lead-lag relationship between single-name CDS and bonds, we estimate panel vector autoregressions (PVARs) similar to those of Hilscher, Pollet, and Wilson 2015 and Lee, Naranjo, and Velioglu 2018. The model is given by the equation

$$\begin{bmatrix} \Delta S_{it}^{CDS} \\ \Delta S_{it}^{Bond} \end{bmatrix} = \begin{bmatrix} \beta_{0,i,CDS} \\ \beta_{0,i,Bond} \end{bmatrix} + \sum_{k=1}^{n} \begin{bmatrix} \beta_{k,CDS,CDS} & \beta_{k,CDS,Bond} \\ \beta_{k,Bond,CDS} & \beta_{k,Bond,Bond} \end{bmatrix} \begin{bmatrix} \Delta S_{it-k}^{CDS} \\ \Delta S_{it-k}^{Bond} \end{bmatrix} + \begin{bmatrix} \epsilon_{it}^{CDS} \\ \epsilon_{it}^{Bond} \end{bmatrix} \tag{1.2}$$

4. Macchiavelli and Zhou 2021 show that money market reform implemented by the SEC in October 2016 raised funding costs for certain dealers and, by extension, hindered their ability to serve as intermediaries in the corporate bond market. If the reform impairs price discovery for bonds, our results will understate the true effect of margin regulation on CDS informativeness.

where ΔS_{it}^{CDS} and ΔS_{it}^{Bond} are percentage changes in CDS and bond spreads for firm i on date t, respectively. Entity-level bond spread changes are the volume-weighted average changes taken across individual CUSIPs. Standard errors are clustered by date. For parsimony, the lag-order is fixed at two throughout this section. Results are stable across lag-orders and robust to the use of the Schwarz Criterion for lag selection. To ensure findings are not driven by outliers, both bond and CDS spread changes are winsorized at the 0.5% and 99.5% levels.

Panel A of Table 1.4 presents results when Equation 1.2 is estimated separately on the Pre-UMR and Post-UMR samples using daily spread changes. The positive, significant coefficients in the first and third rows of Columns 2 and 4 confirm that CDS lead bonds in both periods. The smaller point estimates in Column 4 suggest that information flow may be weaker in the latter period. Before testing the significance of the differences, we re-estimate Equation 1.2 using weekly spread changes. Since corporate bonds are infrequently traded and the intensity of trading has only diminished in recent years, the decline may be due to selection in the set of bonds that continue to trade frequently. Reassuringly, the results in Panel B are consistent with those in Panel A. CDS continue to lead bonds in both periods and the magnitudes of the point estimates are larger prior to September 2016. Moreover, observation counts at both frequencies are similar across periods, indicating that the number of entities is stable across time.

Impulse response functions from the PVARs with daily spread changes are plotted in the left panel of Figure 1.6. As noted previously, there appears to be less information flow from the CDS market to the bond market once uncleared margin requirements are introduced. In the Pre-UMR period, a 1% increase in CDS spread is associated with a 0.2% increase in bond spread the following day. In the Post-

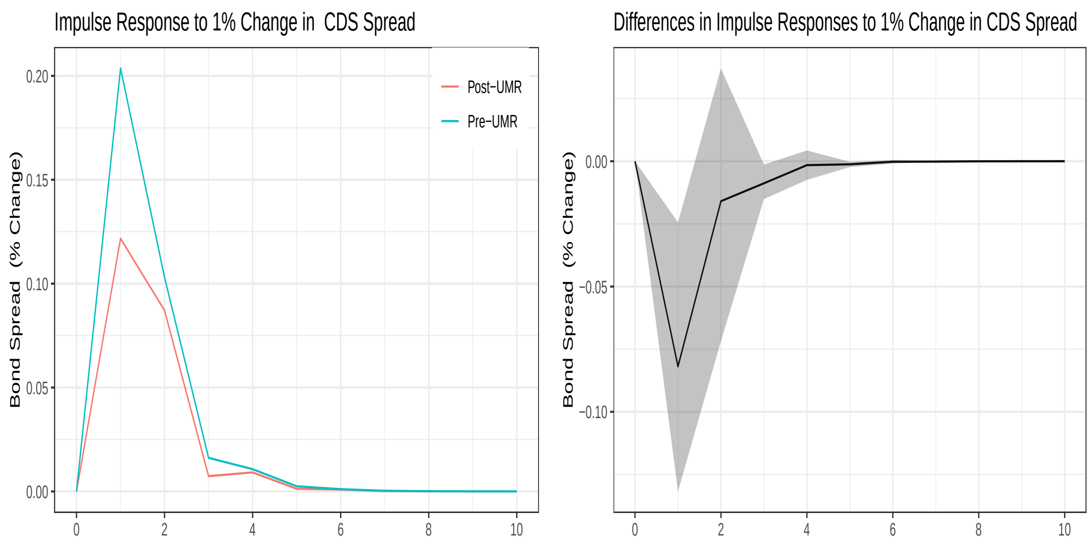
Table 1.4: Single-Name Lead-Lag Relationship

	Pre-UMR		Post-UMR	
Panel A: Daily	Δ CDS	Δ Bond	Δ CDS	Δ Bond
Δ CDS L1	0.1469***	0.2046***	0.0794***	0.1216***
	(0.0143)	(0.0229)	(0.0171)	(0.0311)
Δ Bond L1	0.0008*	−0.1554***	0.0013***	−0.1252***
	(0.0004)	(0.0049)	(0.0004)	(0.0044)
Δ CDS L2	0.0651***	0.1051***	0.0852***	0.0929***
	(0.0189)	(0.0270)	(0.0159)	(0.0279)
Δ Bond L2	0.0013***	−0.0053	0.0004	−0.0026
	(0.0004)	(0.0044)	(0.0004)	(0.0039)
Observations	316,162	316,162	311,075	311,075
Panel B: Weekly	Δ CDS	Δ Bond	Δ CDS	Δ Bond
Δ CDS L1	0.0773**	0.1634***	0.0395	0.1249***
	(0.0306)	(0.0294)	(0.0386)	(0.0335)
Δ Bond L1	0.0031**	−0.1297***	0.0019	−0.1122***
	(0.0015)	(0.0086)	(0.0015)	(0.0078)
Δ CDS L2	0.0181	0.0199	0.0180	0.0182
	(0.0276)	(0.0289)	(0.0305)	(0.0365)
Δ Bond L2	0.0017	0.0074	0.0016	−0.0077
	(0.0015)	(0.0081)	(0.0013)	(0.0078)
Observations	88,859	88,859	82,985	82,985

Notes: This table documents the lead-lag relationship between single-name CDS and bonds during the Pre-UMR and Post-UMR periods. In Panel A, the PVAR given by Equation 1.2 is estimated using daily percentage spread changes. In Panel B, the same equation is estimated using weekly percentage spread changes. All variables are winsorized at the 0.1% and 99.9% levels. Standard errors clustered by date are reported in parentheses. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

UMR period, the same change in CDS spread corresponds to only a 0.12% increase in bond spread. The right panel depicts the differences across periods. The solid line is the point estimate of the difference at various horizons, while the shaded area represents a 95% confidence interval recovered by bootstrapping. The difference at the one-day horizon is negative and statistically significant, affirming that CDS spreads lead bond spreads less strongly from September 2016 onward.

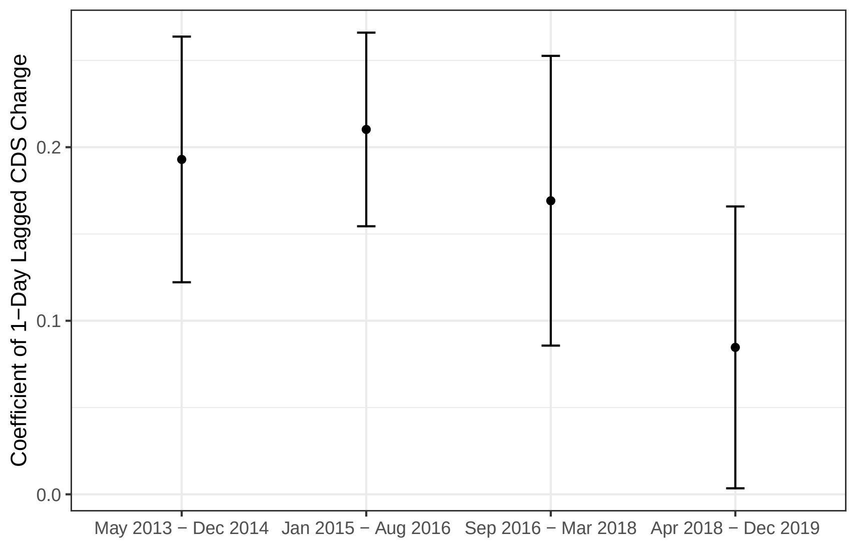
Figure 1.6: Single-Name Impulse Responses



Notes: The left panel depicts the impulse responses when the PVAR given by Equation 1.2 is estimated using daily data. The right panel depicts the differences in the individual impulse responses at various horizons. The shaded area represents the 95% confidence interval recovered by bootstrapping.

To ensure the decrease in relative informativeness is not driven by a secular time trend, we partition the sample into four time bins of equal length and estimate Equation 1.2 separately for each. Figure 1.7 plots the first lag CDS change coefficients from the regressions with bond spreads as the dependent variable. The vertical lines again depict 95% confidence intervals. The first two point estimates are similar, indicating there is no trend in the Pre-UMR period and that Basel III and the Volcker Rule did not have a differential impact on CDS and bond market informativeness. The coefficients from the fourth and fifth bins, which represent the Post-UMR period, are smaller than those of their predecessors. The stark decline again suggests that margin requirements significantly harm price discovery for single-name CDS.

Figure 1.7: Evolution of Lag CDS Coefficient over Time



Notes: This figure presents coefficients of first-order lagged CDS spread changes from regressions with bond spreads as the dependent variable when the PVAR given by Equation 1.2 is estimated separately for various time bins. The vertical lines extending from each point represent 95% confidence intervals.

The time series evidence confirms that the lead-lag relationship between CDS and bonds does not deteriorate gradually over the sample period. It also suggests the drop in pre-event ratio and emergence of post-event drift around downgrades do not stem from revisions to S&P's rating change process. It fails, however, to demonstrate that UMR, not some other factors affecting CDS trading, cause the decrease in informativeness. If only certain underlyings were subject to the regulation, we could easily identify the impact of UMR. Since the margin requirements apply to all reference entities, however, we conduct a series of cross-sectional tests to determine if underlyings most exposed to the regulation experience the sharpest declines.

We begin by testing if the lead-lag relationship between CDS and bonds deteriorates more for underlyings with large bilateral trading volumes. Sequential trading models suggest that every incremental informed trade contributes to price discovery. It follows that reference entities with the most uncleared trading will be particularly adversely affected by the new margin requirements. We classify underlyings as High Volume if they appear on DTCC's Top 1000 list in the quarter immediately preceding the introduction of UMR and Low Volume if they do not.[5] The classification scheme may elicit concerns that mean-reversion in trading activity will drive results, but the relative liquidity of underlyings in the CDS market is stable over time. We will also demonstrate that lag CDS change coefficients for all reference entities are steady in the pre-period.

Table 1.5 presents results when Equation 1.2 is estimated separately for High and Low Volume entities in both the Pre-UMR and Post-UMR periods. For brevity, we include estimates only when bond spreads are the dependent variable. While the first lag CDS spread change coefficient is steady across periods for Low Volume entities, we see a marked decline in the corresponding coefficient for High Volume underlyings. In the last column, we test if the relative change in the estimates is significant. Indeed, we find that the difference-in-differences estimate is economically and statistically significant for the first lag of CDS spread change. The same quantity

5. Reference entities that appear on the list have an average daily notional trading volume of at least $2.5MM. While the DTCC data do not distinguish between cleared and bilateral volume, these quantities are almost certainly highly correlated.

is small and insignificant for the other three coefficients. Also, for both the High and Low Volume groups, observation counts are similar across periods. This consistency again eases concerns about selection, as it suggests the set of reference entities is stable over time.

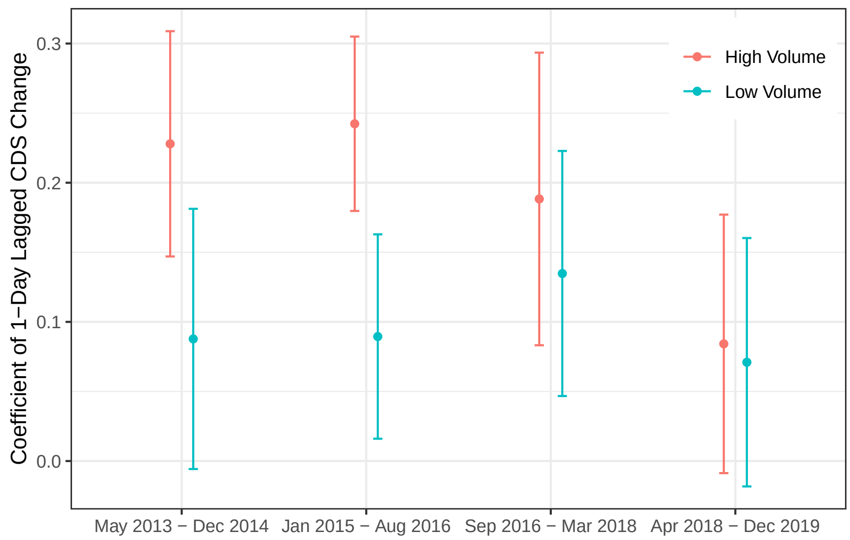
Table 1.5: Difference-in-Differences Test Around UMR Adoption

	Pre-UMR		Post-UMR		DiD
	High	Low	High	Low	
Δ CDS L1	0.2365***	0.0885***	0.1248***	0.1069***	0.1301**
	(0.0263)	(0.0314)	(0.0382)	(0.0332)	(0.0650)
Δ Bond L1	−0.1456***	−0.1693***	−0.1155***	−0.1398***	−0.0006
	(0.0058)	(0.0077)	(0.0053)	(0.0068)	(0.0129)
Δ CDS L2	0.1150***	0.0594*	0.0962***	0.0907***	0.0501
	(0.0299)	(0.0360)	(0.0347)	(0.0332)	(0.0671)
Δ Bond L2	−0.0097*	0.0004	−0.0017	−0.0052	−0.0137
	(0.0052)	(0.0077)	(0.0047)	(0.0066)	(0.0124)
Observations	209,019	107,122	204,419	101,603	

Notes: The first four columns of this table present coefficient estimates when the panel VAR given by Equation 1.2 is estimated separately across periods for both High and Low Volume entities. Only coefficients when changes in bond spreads are the dependent variable are reported. The Pre-UMR period spans from May 2013 through August 2016, while the Post-UMR period extends from September 2016 through December 2019. Entities in the Low (High) category are those with average daily notional volume less than (at least) 2.5MM in the CDS market in the quarter immediately preceding UMR adoption. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

In Figure 1.8, we split the Pre- and Post-UMR periods more finely and plot the first lag CDS change coefficient when Equation 1.2 is estimated separately for the Low and High Volume groups. The blue bars demonstrate that the point estimates for Low Volume entities are stable over time. The red bars, on the other hand, reveal a large decline in the point estimates for the High Volume group after UMR adoption. As expected, the latter group drives the weakening of the lead-lag relationship between CDS and corporate bond spreads in the Post-UMR period.

Figure 1.8: Evolution of Lag CDS Coefficient by Volume



Notes: This figure presents the estimates for the first-order lag CDS spread change coefficient when Equation 1.2 is estimated separately for High and Low volume entities in various time bins. Low (High) volume entities are those with an average daily CDS notional less than (at least) 2.5MM in the quarter immediately preceding UMR adoption. The vertical lines extending from each point represent the 95% confidence interval for the estimate.

We next investigate if the results are most pronounced for High Volume entities that are less likely to be cleared. For each underlying, we compute clearing propensity immediately prior to UMR adoption by dividing cleared open interest by total gross open interest. We then partition the High Volume group into three finer categories: Non-Clearable, Low Propensity, and High Propensity. The first group consists of underlyings that were not eligible to be cleared in September 2016. The Low (High) Propensity group consists of eligible entities with clearing propensities below (above) the median. If the drop in informativeness is indeed due to uncleared margin regulation, it should be starkest for entities with the largest bilateral trading shares.

Table 1.6 presents results when Equation 1.2 is estimated separately for each of the three groups in both the Pre- and Post-UMR periods. Again, we only report estimates when bond spread changes are the dependent variable. The first two columns provide coefficients for the two time periods, while the third tests if the changes across periods are statistically significant. The differences for the first lag of CDS spread changes are negative for all three groups, but the magnitudes decrease as clearing propensity increases. Moreover, while the difference for the Non-Clearable group is significant at the 1% level, its counterpart for the High Propensity group is not significant at all. The results support the notion that uncleared margin requirements are the cause of the decline in relative informativeness.

The model presented in Section 1.3 suggests that the increased costs associated with UMR may push informed agents from single names to indices. Liquidating dividends are purely idiosyncratic in the model, but in practice reference entities' spreads tend to comove with one another. Underlyings whose spread changes are highly correlated with those of the CDXNAIG and CDXNAHY may experience larger reductions in informativeness, because the indices serve as closer substitutes. Moreover, since both central counterparty and model-based UMR margins are determined using Value at Risk measures, reference entities with high index correlations may incur larger margin charges, as they offer less portfolio diversification benefit.

To test this hypothesis, we first compute correlations between single-name and index spread changes using data from the twelve months immediately preceding the adoption of UMR. We pair entities that are investment grade at the end of August

Table 1.6: Lead-Lag Relationship by Clearing Propensity

Panel A: Non-Clearable	Pre-UMR	Post-UMR	Difference
Δ CDS L1	0.2306***	0.0783**	0.1523***
	(0.0387)	(0.0396)	(0.0553)
Δ Bond L1	−0.1766***	−0.1175***	−0.0591***
	(0.0094)	(0.0082)	(0.0125)
Δ CDS L2	0.1227***	0.0591	0.0636
	(0.0436)	(0.0380)	(0.0579)
Δ Bond L2	−0.0140	−0.0053	−0.0087
	(0.0089)	(0.0082)	(0.0122)
Observations	63,820	64,884	

Panel B: Low Propensity	Pre-UMR	Post-UMR	Difference
Δ CDS L1	0.2767***	0.1618***	0.1150**
	(0.0335)	(0.0450)	(0.0561)
Δ Bond L1	−0.1297***	−0.1134***	−0.0163
	(0.0100)	(0.0087)	(0.0132)
Δ CDS L2	0.1063***	0.1091**	−0.0029
	(0.0354)	(0.0428)	(0.0556)
Δ Bond L2	−0.0101	−0.0034	−0.0068
	(0.0092)	(0.0082)	(0.0123)
Observations	68,036	65,985	

(table continued on next page)

Panel C: High Propensity	Pre-UMR	Post-UMR	Difference
Δ CDS L1	0.1980***	0.1225**	0.0755
	(0.0323)	(0.0488)	(0.0585)
Δ Bond L1	−0.1365***	−0.1264***	−0.0101
	(0.0090)	(0.0087)	(0.0125)
Δ CDS L2	0.1201***	0.1004**	0.0197
	(0.0343)	(0.0432)	(0.0551)
Δ Bond L2	−0.0116*	−0.0001	−0.0115
	(0.0070)	(0.0079)	(0.0106)
Observations	78,231	74,325	

Notes: This table present coefficient estimates when the panel VAR given by Equation 1.2 is estimated separately across periods for Non-Clearable, Low Propensity, and High Propensity entities. Only coefficients when changes in bond spreads are the dependent variable are reported. The Pre-UMR period spans from May 2013 to August 2016, while the Post-UMR period goes from September 2016 to December 2019. The Non-Clearable group consists of entities that appear on DTCC's Top 1000 list in the quarter preceding UMR adoption but are not eligible for clearing. The Low (High) Propensity group consists of clear-eligible entities that appear on the Top 1000 list with a clearing propensity below (above) the median. Clearing propensity is defined as the cleared notional outstanding divided by the total gross notional outstanding. The final column tests if the differences across periods are statistically significant. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

2016 with the CDXNAIG and those that are high yield with the CDXNAHY. We then estimate the regression

$$\begin{aligned}\Delta S_{it}^{Bond} =& \beta_{0,i}^{Bond} + \beta_1 \Delta S_{it-1}^{CDS} \times PostReg_t \times \rho_i^{CDX} + \\ & \beta_2 \Delta S_{it-2}^{CDS} \times PostUMR_t \times \rho_i^{CDX} + \\ & \beta_3 \Delta S_{it-1}^{Bond} \times PostUMR_t \times \rho_i^{CDX} + \beta_4 \Delta S_{it-2}^{Bond} \times PostUMR_t \times \rho_i^{CDX} + \\ & \beta_5 \Delta S_{it-1}^{CDS} \times PostUMR_t + \beta_6 \Delta S_{it-2}^{CDS} \times PostUMR_t + \\ & \beta_7 \Delta S_{it-1}^{Bond} \times PostUMR_t + \\ & \beta_8 \Delta S_{it-2}^{Bond} \times PostUMR_t + \beta_9 \Delta S_{it-1}^{CDS} \times \rho_i^{CDX} + \beta_{10} \Delta S_{it-2}^{CDS} \times \rho_i^{CDX} + \\ & \beta_{11} \Delta S_{it-1}^{Bond} \times \rho_i^{CDX} + \beta_{12} \Delta S_{it-2}^{Bond} \times \rho_i^{CDX} + \beta_{13} \Delta S_{it-1}^{CDS} + \beta_{14} \Delta S_{it-2}^{CDS} + \\ & \beta_{15} \Delta S_{it-1}^{Bond} + \beta_{16} \Delta S_{it-2}^{Bond} + \\ & \beta_{17} PostUMR_t \times \rho_i^{CDX} + \beta_{18} PostUMR_t + \epsilon_{it}^{Bond}\end{aligned} \tag{1.3}$$

where ΔS_{it}^{CDS} and ΔS_{it}^{Bond} are percentage changes in CDS and bond spreads for firm i on date t, $PostUMR_t$ is a dummy equal to 1 from September 2016 onward, ρ_i^{CDX} is the correlation between the spreads of firm i and the corresponding CDX index, and $\beta_{0,i}^{Bond}$ is a firm fixed effect. Standard errors are clustered by date. We opt for a single regression with interactions, because the correlation variable is purely continuous and does not lend itself to the discrete sample splits used previously.[6] Nickell bias is not major concern given the length of the panel, so we use ordinary least squares instead of a dynamic panel estimator.

6. All of the prior results are extremely similar if we instead estimate regressions of this sort.

Results are presented in Table 1.7. The term of interest is the triple interaction between lagged CDS spread changes, the regulation indicator, and the index correlation. The corresponding coefficient estimate is negative and statistically significant, which indicates informativeness declines more sharply for highly substitutable reference entities following UMR adoption. Taken together, the time series and cross-sectional findings provide strong evidence that UMR causes the deterioration of the lead-lag relationship between CDS and bonds.

1.7.2 Indices

We next investigate if regulation has impacted price discovery for CDS indices. The UMR are less burdensome for index intermediation, as CDX trades are significantly more likely to be cleared than single name trades. The model introduced in Section 1.3 predicts that an increase in the relative transaction costs of single names will drive informed agents toward index markets. It follows that informativeness may not decrease for either the CDXNAIG or the CDXNAHY in the Post-UMR period.

As noted by Bond and Garcia 2018, it is challenging to measure the informational efficiency of an index. Citing this difficulty, the authors provide only indirect evidence in support of their prediction that lower ETF entry costs correspond to reduced price efficiency. We are also unable to directly assess the informativeness of the CDX indices, so we again test for differential changes in the CDS and bond markets. More specifically, we investigate if the lead-lag relationships between the CDX indices and the Intercontinental Exchange Bank of America (ICE BoA) composite bond market indices have deteriorated since the introduction of margin requirements.

Table 1.7: Index Correlation

	Δ Bond Spread
Δ CDS Spread L1 $\times$ Post-UMR $\times$ ρ^{CDX}	-0.2473^{**}
	(0.1119)
Δ Bond Spread L1 $\times$ Post-UMR $\times$ ρ^{CDX}	-0.0843^{*}
	(0.0450)
Δ CDS Spread L2 $\times$ Post-UMR $\times$ ρ^{CDX}	-0.0179
	(0.1284)
Δ Bond Spread L2 $\times$ Post-UMR $\times$ ρ^{CDX}	-0.0026
	(0.0319)
Δ CDS Spread L1 $\times$ ρ^{CDX}	0.4470^{***}
	(0.0856)
Δ Bond Spread L1 $\times$ ρ^{CDX}	0.1184^{***}
	(0.0456)
Δ CDS Spread L2 $\times$ ρ^{CDX}	0.0187
	(0.0798)
Δ Bond Spread L2 $\times$ ρ^{CDX}	0.0271
	(0.0315)
Δ CDS Spread L1 $\times$ Post-UMR	0.0070
	(0.0447)
Δ Bond Spread L1 $\times$ Post-UMR	0.0471^{***}
	(0.0128)
Δ CDS Spread L2 $\times$ Post-UMR	0.0086
	(0.0496)
Δ Bond Spread L2 $\times$ Post-UMR	0.0045
	(0.0083)

(table continued on next page)

	Δ Bond Spread
Post-UMR $\times$ ρ^{CDX}	-0.2695
	(0.3484)
Post-UMR	0.5402^{***}
	(0.1156)
Δ CDS Spread L1	0.0615^{*}
	(0.0318)
Δ Bond Spread L1	-0.1761^{***}
	(0.0115)
Δ CDS Spread L2	0.0858^{***}
	(0.0308)
Δ Bond Spread L2	-0.0085
	(0.0072)
N	613,435

Notes: This table reports results when Equation 1.3 is estimated using daily percentage spread changes from May 2013 through December 2019. Post-UMR is an indicator equal to one after the implementation of UMR in September 2016. ρ^{CDX} is the correlation coefficient between a firm's spread changes and those of the CDX over the year immediately preceding UMR adoption. The regression includes firm fixed effects and standard errors are clustered by date. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Formally, we estimate vector autoregressions (VARs) given by the following equation separately for the CDXNAIG and CDXNAHY:

$$\begin{bmatrix} \Delta S_t^{CDX} \\ \Delta S_t^{Bond} \end{bmatrix} = \begin{bmatrix} \beta_{0,CDX} \\ \beta_{0,Bond} \end{bmatrix} + \sum_{k=1}^{n} \begin{bmatrix} \beta_{k,CDX,CDX} & \beta_{k,CDX,Bond} \\ \beta_{k,Bond,CDX} & \beta_{k,Bond,Bond} \end{bmatrix} \begin{bmatrix} \Delta S_{t-k}^{CDX} \\ \Delta S_{t-k}^{Bond} \end{bmatrix} + \begin{bmatrix} \epsilon_t^{CDX} \\ \epsilon_t^{Bond} \end{bmatrix} \tag{1.4}$$

where ΔS_t^{CDX} and ΔS_t^{Bond} are percentage changes in the CDX and corresponding bond index spreads on date t. The CDXNAIG is paired with the ICE BoA BBB Corporate Index and the CDXNAHY is matched with the ICE BoA BB Corporate Index. The bond index choices are motivated by Oehmke and Zawadowski 2017, who show that liquidity in the single-name CDS market is concentrated in entities with ratings close to the investment grade threshold. Since CDX membership is governed by trading volume, the BBB and BB bond indices are the most direct comparisons. Again, both sets of index spread changes are winsorized at the 0.5% and 99.5% levels.

Panel A of Table 1.8 presents results when Equation 1.4 is estimated separately on the Pre-UMR and Post-UMR samples using daily spread changes from the high yield indices. The positive, significant coefficients of CDXNAHY in Columns 2 and 4 demonstrate that it leads the corresponding bond index in both periods. The larger point estimates in Column 4 suggest that the CDXNAHY may actually be relatively more informative after the introduction of margin requirements. We repeat this exercise with the investment grade indices. Panel B of Table 1.8 once again presents results when Equation 1.4 is estimated using daily spread changes. The CDXNAIG leads the ICE BoA BBB Corporate index in both periods, but the magnitude of the first lag CDX change coefficient is only marginally larger in the Post-UMR period.

Table 1.8: Index Lead-Lag Relationships

	Pre-UMR		Post-UMR	
	Δ CDXNAHY	Δ Bond	Δ CDXNAHY	Δ Bond
Δ CDXNAHY L1	0.009	0.362***	0.055	0.524***
	(0.039)	(0.034)	(0.043)	(0.050)
Δ HY Bond L1	0.076*	−0.084**	−0.039	−0.217***
	(0.046)	(0.039)	(0.037)	(0.043)
Δ CDXNAHY L2	−0.031	0.121***	0.015	0.237***
	(0.042)	(0.036)	(0.045)	(0.052)
Δ HY Bond L2	−0.026	−0.049	0.026	0.020
	(0.042)	(0.036)	(0.035)	(0.040)
Observations	840	840	838	838

	Δ CDXNAIG	Δ Bond	Δ CDXNAIG	Δ Bond
Δ CDXNAIG L1	0.043	0.112***	0.021	0.124***
	(0.038)	(0.011)	(0.038)	(0.012)
Δ IG Bond L1	0.149	0.075**	−0.174	0.049
	(0.126)	(0.038)	(0.111)	(0.036)
Δ CDXNAIG L2	−0.048	0.028**	0.013	0.043***
	(0.040)	(0.012)	(0.039)	(0.013)
Δ IG Bond L2	0.001	0.169***	0.092	0.229***
	(0.116)	(0.035)	(0.104)	(0.034)
Observations	839	839	838	838

Notes: This table presents coefficients when the VAR given by Equation 1.4 is estimated separately for the Pre-UMR and Post-UMR periods using daily percentage spread changes. The top panel contains estimates for the CDXNAHY and ICE BoA BB Corporate Bond Index, while the bottom panel contains estimates for the CDXNAIG and ICE BoA BBB Corporate Bond Index. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Impulse response functions from the VARs on daily spread changes are plotted in Figure 1.9. The top left plot depicts a stark increase in relative informativeness across periods for the CDXNAHY. The top right panel shows the difference is statistically significant. The increase is consistent with informed traders shifting away from single-name CDS and 2016 money market fund reform making it more difficult for some dealers to intermediate bond trades.[7] The bottom two panels present the same set of results for the investment grade indices. There does not appear to be much difference in relative informativeness between periods for the CDXNAIG.

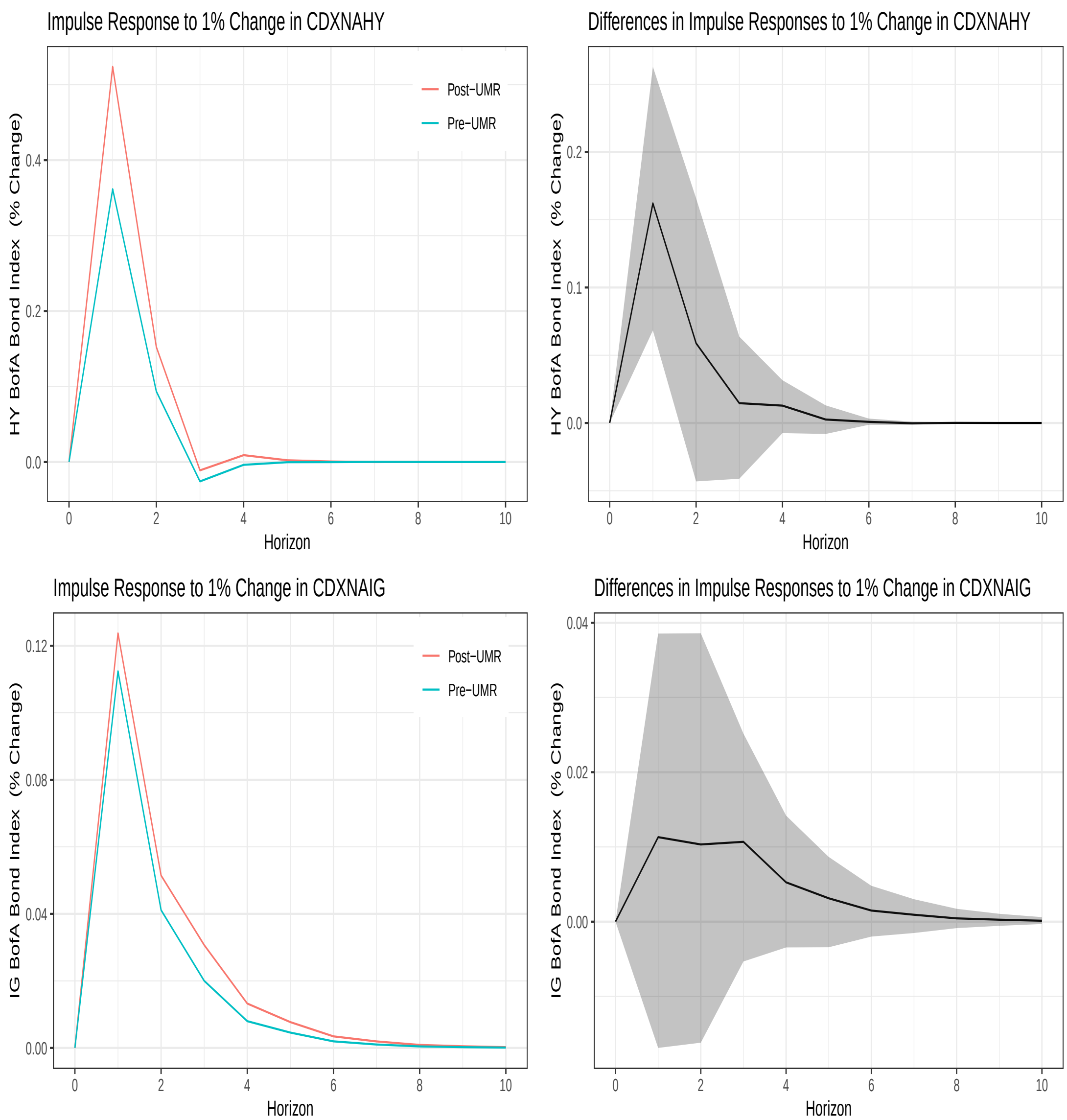
The evidence that price discovery for CDS indices improves in the Post-UMR period is mixed, but it is clear that the CDXNAHY and CDXNAIG have not become less informative relative to their corporate bond counterparts. These findings support the notion that regulation and not some other force impacting all segments of the CDS market drives the deterioration in price discovery for single names. They also accord with the prediction of the model that informed traders migrate from single-name to index markets when the cost of trading in the former increases.

1.8 Conclusion

In this paper, we investigate how post-crisis regulation has impacted price discovery in the CDS market. Using windows around rating downgrades, we find evidence that CDS spreads impound less private information and are slower to incorporate public information in the Rule Implementation period. Estimates from panel VARs

7. The rise may also stem from two new inclusion criteria for the CDXNAHY that were introduced in September 2015. The first limited the over-representation of individual sectors, while the second required underlyings to be sufficiently liquid. Index turnover was only modestly larger following the adoption of the criteria, so it is unlikely they fully explain the growth in the impulse response.

Figure 1.9: Index Impulse Responses



Notes: The top left panel plots the impulse responses when the VAR given by Equation 1.4 is estimated using daily percentage spread changes for the CDXNAHY and ICE BoA BB Corporate Bond index. The bottom left panel plots the analogous impulse response functions for the CDX-NAIG and ICE BoA BBB index. The right panels show the differences in the individual impulse responses at various horizons. The shaded areas represent 95% confidence intervals recovered by bootstrapping.

reveal that CDS spreads lead bond spreads less strongly following the introduction of uncleared margin requirements in September 2016. The deterioration of informativeness is driven by reference entities that are most exposed to the new rules. Price discovery for CDS indices appears to be unharmed, consistent with the fact these contracts are often centrally cleared and, thus, less affected by margin reforms. The results accord with a model in which increases in single-name transaction costs lead informed agents to trade indices instead.

The paper highlights a lesser-studied channel through which post-crisis regulation has affected financial markets. We have focused on the informativeness of CDS spreads, but the findings have implications for other derivative classes, such as foreign exchange swaps, that also have large uncleared segments.

APPENDICES

1.A Proofs

Proof of Lemma 1. As in Glosten and Milgrom 1985, market makers use Bayes' rule to update their beliefs about the final values of securities A and B given the sign of the observed order. Because market makers are risk neutral and competitive, they set bids and asks equal to their revised conditional expectations. Thus,

$$\begin{aligned} ask_A &= P(\hat{V}_A = 1|\text{buy } A) \\ &= \frac{P(\text{buy } A|\hat{V}_A = 1)P(\hat{V}_A = 1)}{P(\text{buy } A|\hat{V}_A = 1)P(\hat{V}_A = 1) + P(\text{buy } A|\hat{V}_A = 0)P(\hat{V}_A = 0)} \\ &= \frac{4\phi_A\alpha + 1 - \alpha}{4\phi_A\alpha + 2 - 2\alpha} \end{aligned}$$

and

$$\begin{aligned} bid_A &= P(\hat{V}_A = 1|\text{sell } A) \\ &= \frac{P(\text{sell } A|\hat{V}_A = 1)P(\hat{V}_A = 1)}{P(\text{sell } A|\hat{V}_A = 1)P(\hat{V}_A = 1) + P(\text{sell } A|\hat{V}_A = 0)P(\hat{V}_A = 0)} \\ &= \frac{1 - \alpha}{4\phi_A\alpha + 2 - 2\alpha}. \end{aligned}$$

Since we are focusing on symmetric equilibria, $\phi = \phi_A = \phi_B$. For the index

$$\begin{aligned} ask_I =& 1 \cdot P(\hat{V}_A = 1 \;\&\; \hat{V}_B = 1|\text{buy } I) \\ &+ 0.5 \cdot P(\hat{V}_A = 1 \;\&\; \hat{V}_B = 0|\text{buy } I) + 0.5 \cdot P(\hat{V}_A = 0 \;\&\; \hat{V}_B = 1|\text{buy } I). \end{aligned}$$

Note that

$$P(\hat{V}_A = 1 \ \&\ \hat{V}_B = 1 | \text{buy } I) = \frac{P(\text{buy } I \mid \hat{V}_A = 1 \ \&\ \hat{V}_B = 1) P(\hat{V}_A = 1 \ \&\ \hat{V}_B = 1)}{P(\text{buy } I)}$$

$$= \frac{(1-\phi)\frac{\alpha}{2} + (1-\phi)\frac{\alpha}{2} + \frac{1-\alpha}{4}}{4\left[(1-\phi)\frac{\alpha}{2} + \frac{1-\alpha}{4}\right]}.$$

Applying the same logic again gives us

$$ask_I = \frac{(1-\phi)\frac{\alpha}{2} + (1-\phi)\frac{\alpha}{2} + \frac{1-\alpha}{4}}{4\left[(1-\phi)\frac{\alpha}{2} + \frac{1-\alpha}{4}\right]} + \frac{(1-\phi)\frac{\alpha}{2} + \frac{1-\alpha}{4}}{4\left[(1-\phi)\frac{\alpha}{2} + \frac{1-\alpha}{4}\right]}$$

$$= \frac{2\alpha - 3\alpha\phi + 1}{2(\alpha - 2\alpha\phi + 1)}$$

and

$$bid_I = \frac{\frac{1-\alpha}{4}}{4\left[(1-\phi)\frac{\alpha}{2} + \frac{1-\alpha}{4}\right]} + \frac{(1-\phi)\frac{\alpha}{2} + \frac{1-\alpha}{4}}{4\left[(1-\phi)\frac{\alpha}{2} + \frac{1-\alpha}{4}\right]}$$

$$= \frac{1 - \alpha\phi}{2(\alpha - 2\alpha\phi + 1)}.$$

□

Proof of Lemma 2. We again, without loss of generality, start by considering the case of security A. Since security B trades convey no information about the true value of A, we have

$$\begin{aligned}
Var(E[V_A|Q]) =& P(\text{B Trade}) \cdot 0 + \\
& P(\text{A Trade})[0.5(ask_A - 0.5)^2 + 0.5(0.5 - bid_A)^2] + \\
& P(\text{Index Trade})[0.5(ask_I - 0.5)^2 + 0.5(0.5 - bid_I)^2] + \\
=& \left(\frac{\alpha\phi}{2\alpha\phi - \alpha + 1}\right)^2 \left(\frac{\phi\alpha}{2} + \frac{1-\alpha}{4}\right) + \\
& \left(\frac{\alpha(1-\phi)}{2(\alpha - 2\phi\alpha + 1)}\right)^2 \left((1-\phi)\alpha + \frac{1-\alpha}{2}\right) \\
=& \frac{\phi^2\alpha^2}{4(2\phi\alpha - \alpha + 1)} + \frac{(1-\phi)^2\alpha^2}{8(\alpha - 2\phi\alpha + 1)}.
\end{aligned}$$

Applying similar logic gives

$$Cov(E[V_A|Q], V_A) = \frac{\phi^2\alpha^2}{4(2\phi\alpha - \alpha + 1)} + \frac{(1-\phi)^2\alpha^2}{8(\alpha - 2\phi\alpha + 1)},$$

so

$$Var(E[V_A|Q] - V_A) = \frac{1}{4} - \frac{\phi^2\alpha^2}{4(2\phi\alpha - \alpha + 1)} - \frac{(1-\phi)^2\alpha^2}{8(\alpha - 2\phi\alpha + 1)}.$$

For the index we have

$$
\begin{aligned}
Var(E[V_I|Q]) =& P(\text{Index Trade}) \left[0.5\left(ask_I - 0.5\right)^2 + 0.5\left(0.5 - bid_I\right)^2\right] + \\
& P(\text{A Trade})\times \\
& \left[P(V_A = 1)\left(\frac{ask_A}{2} + \frac{1}{4} - \frac{1}{2}\right)^2 + P(V_A = 0)\left(\frac{1}{2} - \frac{bid_A}{2} - \frac{1}{4}\right)^2\right] + \\
& P(\text{B Trade})\times \\
& \left[P(V_B = 1)\left(\frac{ask_B}{2} + \frac{1}{4} - \frac{1}{2}\right)^2 + P(V_B = 0)\left(\frac{1}{2} - \frac{bid_B}{2} - \frac{1}{4}\right)^2\right] + \\
=& \left(\frac{\alpha\phi}{4\alpha\phi - 2\alpha + 2}\right)^2 \left(\phi\alpha + \frac{1-\alpha}{2}\right) + \\
& \left(\frac{\alpha(1-\phi)}{2(\alpha - 2\phi\alpha + 1)}\right)^2 \left((1-\phi)\alpha + \frac{1-\alpha}{2}\right) \\
=& \frac{\phi^2\alpha^2}{8(2\phi\alpha - \alpha + 1)} + \frac{(1-\phi)^2\alpha^2}{8(\alpha - 2\phi\alpha + 1)}
\end{aligned}
$$

and

$$
Cov(E[V_I|Q], V_I) = \frac{\phi^2\alpha^2}{8(2\phi\alpha - \alpha + 1)} + \frac{(1-\phi)^2\alpha^2}{8(\alpha - 2\phi\alpha + 1)}
$$

so

$$
Var(E[V_I|Q] - V_I) = \frac{1}{8} - \frac{\phi^2\alpha^2}{8(2\phi\alpha - \alpha + 1)} - \frac{(1-\phi)^2\alpha^2}{8(\alpha - 2\phi\alpha + 1)}.
$$

□

Proof of Proposition 1. In order for informed agents to always trade single names, the expected payoff of doing so must be higher when they mimic security A or

B liquidity traders than when they mimic index liquidity traders. Bids and asks are determined by setting $\phi = 1$ in the expressions derived in Lemma 1. For the representative case of security A informed trader when $\hat{V}_A = 1$, we get the inequality

$$\begin{aligned} E[\text{Payoff from trading A}] &> E[\text{Payoff from trading Index}] \\ E[\hat{V}_A] - ask_A - c &> (0.5E[\hat{V}_A] + 0.5E[\hat{V}_B]) - ask_I \\ 1 - \frac{3\alpha + 1}{2\alpha + 2} - c &> \frac{1}{4}. \end{aligned}$$

The left hand side is decreasing in c, which implies that c must be sufficiently small in order for the inequality to hold.

Now, let g denote the cost of acquiring information about security A or B. In this equilibrium, the proportion of informed traders α is then such that

$$\begin{aligned} g &= E[\text{Payoff from trading A}] \\ &= 1 - \frac{3\alpha + 1}{2\alpha + 2} - c \end{aligned}$$

As previously established, the right hand side is decreasing in c. Further,

$$\frac{\partial}{\partial \alpha} E[\text{Payoff from trading A}] = \frac{-1}{2(1 + \alpha)^2}$$

which is negative for $\alpha \in [0, 1]$, so expected profits are decreasing in α. It follows that in order to maintain an equilibrium, an increase in c must be offset by a decrease in α.

Setting $\phi = 0$ in the expression from Lemma 2 and differentiating gives

$$\frac{\partial}{\partial \alpha} Var(E[V_A|Q] - V_A) = \frac{-\alpha}{4(1+\alpha)^2} (2+\alpha) .$$

Since the derivative is negative for $\alpha \in (0, 1]$, the estimation error is decreasing in α. By the same argument, the estimation error for the index is also decreasing in α. As increases in transaction costs lead to declines in the shares of informed traders, they also result in less informational efficiency. $\square$

Proof of Proposition 2. In order for this equilibrium to prevail, informed agents' expected payoffs must be higher when they mimic index liquidity traders than when they mimic single name liquidity traders. Bids and asks are determined by setting $\phi = 0$ in the expressions derived in Lemma 1. We get the following inequality for A-informed traders when $V_A = 1$

$$\begin{aligned} E[\text{Payoff from trading A}] &< E[\text{Payoff from trading Index}] \\ E[\hat{V}_A] - ask_A - c &< (0.5E[\hat{V}_A] + 0.5E[\hat{V}_B]) - ask_I \\ \frac{1}{2} - c &< \frac{1-\alpha}{4(\alpha+1)}. \end{aligned}$$

The left hand side is decreasing with c. It follows that the transaction cost must be sufficiently large for this equilibrium to obtain.

Again, let g denote the cost of acquiring information about security A or B. In this equilibrium, the proportion of informed traders α is then such that

$$\begin{aligned} g &= E[\text{Payoff from trading A}] \\ &= \frac{1-\alpha}{4(\alpha+1)} \end{aligned}$$

The expected profits of informed traders do not depend on the transaction cost. As a result, the share of informed traders α does not change as c increases. It follows that raising transaction costs will not cause a reduction in informativeness for either the single names or the index. □

Proof of Proposition 3. For this equilibrium to prevail, informed agents must be indifferent between trading single-name securities and the index. In the representative case of an A-informed trader when $V_A = 1$ we have the condition

$$E[\text{Payoff from trading A}] = E[\text{Payoff from trading Index}].$$

Substituting expressions derived in Lemma 1 yields the equation

$$\left[1 - \frac{4\phi\alpha + 1 - \alpha}{4\phi\alpha + 2 - 2\alpha} - c\right] = \left(\frac{3}{4} - \frac{2\alpha - 3\alpha\phi + 1}{2(\alpha - 2\alpha\phi + 1)}\right).$$

Now, let g once more denote the cost of acquiring information about security A or B. In this equilibrium, the proportion of informed traders α is then such that

$$g = \left[1 - \frac{4\phi\alpha + 1 - \alpha}{4\phi\alpha + 2 - 2\alpha} - c\right] = \left(\frac{3}{4} - \frac{2\alpha - 3\alpha\phi + 1}{2(\alpha - 2\alpha\phi + 1)}\right).$$

Implicitly differentiating the index profit condition and solving for the derivative of ϕ with respect to α gives

$$\frac{\partial \phi}{\partial \alpha} = \frac{1-\phi}{\alpha(1-\alpha)},$$

which is positive for $\alpha, \phi \in (0,1)$. The sign makes intuitive sense, since increasing ϕ leads to less informed index trading, which narrows the bid-ask spread and, by extension, increases profits. This effect must be offset by an increase in the overall share of informed trading in order to maintain equilibrium.

Returning to the single name condition, it is clear that profits are decreasing c. Increases in transaction costs must therefore be offset by reducing α. To demonstrate that this shift leads to a decline in informational efficiency, we let

$$K_1 = \frac{\phi^2\alpha^2}{2\phi\alpha - \alpha + 1}$$
$$K_2 = \frac{(1-\phi)^2\alpha^2}{\alpha - 2\phi\alpha + 1}.$$

Differentiating the first equation with respect to α gives

$$\frac{\partial K_1}{\partial \alpha} = \frac{(2\alpha\phi - \alpha + 1)(2\phi^2\alpha + 2\phi\alpha^2\frac{\partial \phi}{\partial \alpha}) - (\phi^2\alpha^2)(2\phi + 2\alpha\frac{\partial \phi}{\partial \alpha} - 1)}{(2\alpha\phi - \alpha + 1)^2}.$$

The derivative will be positive whenever the numerator is greater than zero. Expanding terms and simplifying gives the inequality

$$2\alpha^2\phi^3 + \alpha\phi^2(2-\alpha) + 2\alpha^2\phi\frac{\partial \phi}{\partial \alpha}(1-\alpha) + 2\alpha^3\phi^2\frac{\partial \phi}{\partial \alpha} > 0$$

which is always true for $\alpha, \phi \in (0,1)$. By a similar argument, we also have that $\frac{\partial K_2}{\partial \alpha} > 0$ for $\alpha, \phi \in (0,1)$.

We can rewrite the estimation errors from Lemma 2

$$Var(E[V_A|Q] - V_A) = \frac{1}{4} - \frac{1}{4}K_1 - \frac{1}{8}K_2$$
$$Var(E[V_I|Q] - V_I) = \frac{1}{8} - \frac{1}{8}K_1 - \frac{1}{8}K_2.$$

Since the derivatives of K_1 and K_2 with respect to α are both strictly positive, increases in c will lead to larger estimation errors for both single-name securities and the index. Furthermore, the corresponding loss in informational efficiency will always be more pronounced for the single names.

□

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CHAPTER 2

Counterparty Risk and Counterparty Choice in the Credit Default Swap Market

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2.1 Introduction

Counterparty risk in over-the-counter (OTC) derivative markets played an important role in the propagation of the global financial crisis (GFC) in 2008. The reluctance of market participants to trade with Bear Stearns and Lehman Brothers, as their troubles became apparent, hastened the descent of those dealers 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] Since the onset of the COVID-19 pandemic, there has been renewed interest in central clearing and counterparty risk management (see, e.g., Huang and Takáts 2020).

In this paper, we investigate how market participants manage and price counterparty risk in the credit default swap (CDS) market. We use 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 dealers' 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.

We first investigate the impact of counterparty credit risk on the pricing of CDS contracts in the OTC market. When the client is buying (selling) default protection, we expect the CDS spread to decrease (increase) in the default risk of the dealer counterparty. In an uncollateralized CDS transaction, the protection buyer risks losing the full notional of default protection if the seller defaults when the reference entity defaults. Therefore, the concern for counterparty risk default is more severe for CDS contracts than for other OTC derivatives that do not involve an exchange

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.

of principal, such as plain-vanilla interest rate swaps (IRS) (Gregory 2010).[2] The pricing impact of counterparty risk on CDS contracts depends on several factors, including the degree of collateralization and the correlation between default risk of the dealer and that of the reference entity (see, for example, Huge and Lando 1999). Ultimately, the magnitude of this effect must be determined empirically.

Using four years (2010–2013) of transaction data from the DTCC, we provide the first direct account of the pricing impact of counterparty risk using actual OTC derivatives transaction prices. Our sample contains the peak of the European debt crisis, during which dealers' credit spreads increased significantly in level and dispersion. We find that when the client buys CDS protection from a dealer, the CDS spread decreases significantly with respect to seller's credit risk, while holding the buyer and the contract characteristics fixed, but the economic magnitude of the effect is very small: an increase of 100 basis points in the seller's credit spread translates into about a 0.6 basis point reduction in the CDS transaction spread. Our benchmark estimate based on actual transaction spreads is similar in economic magnitude to the finding in Arora, Gandhi, and Longstaff (2012) based on CDS quotes received by a single buyside client. Furthermore, when we perform the same regressions on a sample of interdealer transactions, we find even smaller pricing impacts, if any at all,

2. 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 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).

arising from counterparty credit risk. This difference in sensitivity to counterparty risk is consistent with differences in provisions for collateral.[3]

If prices do not adjust materially to dealer credit risk in the OTC derivative markets, *quantities* might adjust, i.e., non-dealers might avoid transacting with high credit risk dealers. In this paper, we also provide the first direct test of this quantity conjecture.[4] 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 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.

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

3. 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 client 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.

4. This quantity adjustment conjecture was raised in Litzenberger (1992) while discussing counterparty risk for interest rate swaps, but has not been directly tested in OTC derivatives to our knowledge.

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 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 counterparty choice results shed light on a form of credit rationing. Unlike the textbook case of a risky 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. Thus, our results indicate that credit rationing may arise under wider circumstances than previously recognized.

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 GFC. 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 2019). 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 client's 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.[5] 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.

Lastly, we examine the impact of central clearing on the pricing of counterparty risk. Within the span of our sample period, contracts on numerous single-name reference entities became eligible for central clearing. 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. 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 interdealer trades are significantly *lower* relative to spreads on contemporaneous uncleared interdealer transactions, which is consistent with the view that counterparty risk does not have a first-order impact on pricing and bolsters the evidence of Hau et al. (2019) and Loon and Zhong (2016) for enhanced pricing competition on on anonymous platforms such as swap execution facilities (SEFs). A lower spread on centrally cleared interdealer trades in our sample is also consistent with the findings of lower spreads on centrally cleared dealer-client transactions in Cenedese, Ranaldo, and Vasios (2019) based on IRS transactions.

5. 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.

The risk-mitigating benefits of clearing notwithstanding, in the single-name CDS market clients were initially slow to embrace it as initial margin requirements for cleared portfolios typically exceeded the amounts demanded by dealers on uncleared portfolios (Rennison 2015). At end-2013, central counterparties accounted for 0.1% of aggregate client exposure in the single-name market, whereas dealer counterparties accounted for 99.0%. Thus, an advantage to our chosen sample period is that it predates the availability of central clearing as a practical alternative to bilateral counterparty exposure, and thereby allows us to sidestep the question of endogenous choice of whether or not to clear.[6] While central clearing now accounts for a significant share of total outstanding positions in credit and interest rate swaps, it accounts for negligible share (less than 5%) of outstanding positions in foreign exchange derivatives, equity-linked derivatives and all types of options (Financial Stability Board 2018). Bilateral counterparty risk management thus remains an important consideration for OTC market participants.

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. Gündüz (2018) shows that financial institutions buy more protection on a dealer as reference entity when exposed to that dealer through counterparty

6. This endogenous choice raises important and interesting questions of its own (see Bellia et al. 2017), but would potentially confound our identification of the effect of dealer counterparty risk on pricing and counterparty selection.

relationships. Focusing on the period of the 2008 GFC period, Shachar (2012) shows that liquidity deteriorates as counterparty exposures between dealers accumulate. Aragon, Li, and Qian (2019) highlight a potential cost to transacting with weak dealers, showing that mutual funds which faced Lehman Brothers as a CDS counterparty suffered abnormal outflows and poor performance following the investment bank's collapse. Eisfeldt et al. (2018) studies the core-periphery structure of the CDS dealers and examine the implications of the failure of a dealer.

We proceed as follows. In Section 2.2, we provide background on counterparty risk in the CDS market and describe the DTCC data. In Section 2.3, we examine the effects of counterparty credit risk on CDS pricing. In Section 2.4, we estimate the multinomial choice model for buyers and sellers of protection. The pricing impact of central clearing is assessed in Section 2.5, and Section 2.6 concludes.

2.2 Background and Data Description

In this paper, we focus exclusively on the single-name CDS market. A CDS 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 elaborated below, the single-name market during our sample period was a traditional dealer-intermediated OTC market for non-dealer participants. For a broad survey of the literature on the CDS market, see Augustin et al. (2014).[7]

2.2.1 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 be left holding an unsecured claim on the bankruptcy estate of the defaulting counterparty for the market value of an in-the-money swap.[8] 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

7. As in equity markets, there exist index contracts that pool together CDS on a specified set of index constituents. The index contracts are now mostly traded on exchanges and are excluded from our sample.

8. If the defaulting party were in-the-money, the surviving party would still be obliged to pay in full.

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.

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.[9] 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.[10] See ISDA (2010b) on prevailing practices at the start of our sample period.

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 GFC, ratings-based thresholds may prove ineffective,

9. 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.

10. 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 2010a).

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 (2010a), 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.

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. 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. Gündüz (2018) 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. This indicates that non-dealers find dynamic hedging of counterparty exposure to be difficult or expensive to execute. Since the focus of our paper is on non-dealers' management of counterparty credit risk, we view the results of Gündüz (2018) as complementary to our own.

Fourth, market participants can mitigate counterparty risk simply by trading preferentially with lower risk counterparties. If dealer ABC were perceived to be less creditworthy than its peers, participants might prefer to execute new trades with other dealers unless the new contract happened to offset existing bilateral exposure with ABC. Similarly, market participants may avoid buying protection from a counterparty whose credit risk is highly correlated with the credit risk of the reference entity, e.g., 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. Eisfeldt et al. (2018) provide an alternative theory of counterparty choice that emphasizes diversification (rather than avoidance) of counterparty risk. To the extent that non-dealers are able to exit and replace existing positions when counterparty risk of the dealer increases or to diversify counterparty credit risk by trading with multiple dealers, we would be less likely to find that non-dealers avoid trading with riskier dealers.

Finally, counterparty risk may be reflected in transaction prices of derivative contracts. The credit valuation adjustment (CVA) measures the difference in value 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.[11] It is important to recognize that adjustments to pricing do not mitigate counterparty risk, but rather serve as compensation for bearing that 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

11. 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.

for cleared CDS trades.[12] 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.

2.2.2 DTCC CDS Transaction Data

DTCC maintains a trade repository of nearly all bilateral CDS transactions worldwide. Each transaction record specifies transaction type, transaction date, 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 accordance 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.

12. 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.

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, Societe Generale, UBS, and Nomura.[13] 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

13. Relative to the list of 14 dealers appearing in the sample of Arora, Gandhi, and Longstaff (2012), Lehman is absent (as it no longer exists), Bank of America and Merrill Lynch are merged, and Nomura Holdings and Societe Generale are added.

entity, but is truncated with respect to the choice of counterparty available to the client. Our sample period is January 2010 through December 2013.[14]

2.2.3 Summary Statistics

After applying a series of data filters described in Appendix 2.A, we have 86,757 transactions on 12 reference entities in the baseline sample, and 1,469,775 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 2.1, our baseline sample contains 197 non-dealer buyers of protection in 12,377 transactions, of which 8166 reference US5 dealers and the remainder reference other FRB-regulated institutions. Our US5 counterparty sample contains 829 non-dealer buyers of protection in 195,256 transactions and 1196 reference entities, of which 258 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.

14. 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).

Table 2.1: Summary Statistics for Client-Facing Transactions

	(1) Baseline sample	(2) US5 CP sample
Number of reference entities	12	1,196
Of which ...		
Financial	12	258
Sovereign	0	76
US domecile	12	510
Eligible for clearing	4	318
Number of non-dealers	197	829
Of which ...		
Short horizon	52	90
Captive	24	305
Frequent trader	53	66
Hedge fund	72	247
Asset manager	71	206
Non-dealer bank	26	76
Other	28	300
Number of trades between non-dealers and large dealers	12,377	195,256
Of which ...		
A US5 dealer appears as the reference entity	8,166	4,056
Reference entity is not eligible for clearing	10,712	136,355

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.

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, over 70% of baseline sample transactions are done by clients who trade with eight or more of the 14 international dealers, and close to 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.

The dependent variable in our pricing analysis 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 2.2. 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 19.6 basis points. In the US5 counterparty sample, the median and 95th percentile of the absolute difference are 4.2 basis points and 32.5 basis points, respectively.

Table 2.2: Summary Statistics for Pricing Analysis

Panel A: Alll transaction spreads and Markit quotes in baseline sample ($N = 86,757$)									
Percentile	p1	p5	p10	p25	p50	p75	p90	p95	p99
$cds^{DTCC} - \overline{cds}$	-28.8	-11.9	-7.0	-2.2	0.8	4.2	9.2	14.1	30.3
$\|cds^{DTCC} - \overline{cds}\|$	0.0	0.2	0.5	1.3	3.3	6.9	13.1	19.6	39.3
$\log(cds^{DTCC}) - \log(\overline{cds})$	-0.2	-0.1	-0.1	0.0	0.0	0.0	0.1	0.1	0.2
$\|\log(cds^{DTCC}) - \log(\overline{cds})\|$	0.0	0.0	0.0	0.0	0.0	0.1	0.1	0.1	0.2
Panel B: Contracts with at least ten trades per day in baseline sample($N = 25,596$)									
Percentile	p1	p5	p10	p25	p50	p75	p90	p95	p99
$\sigma_t[\log(cds^{DTCC}) - \log(\overline{cds})]$	0.000	0.000	0.002	0.008	0.014	0.022	0.035	0.047	0.083
No. of transactions per day	1.0	1.0	1.0	1.4	1.8	2.3	2.9	3.3	4.4
Average No. of sellers per buyer	1.0	1.0	1.0	1.4	1.8	2.3	3.0	3.4	4.6
Panel C: All transaction spreads and Markit quotes in US5 counterparty sample ($N = 1,469,775$)									
Percentile	p1	p5	p10	p25	p50	p75	p90	p95	p99
$cds^{DTCC} - \overline{cds}$	-39.4	-15.6	-9.0	-2.6	0.8	5.8	15.2	25.8	63.5
$\|cds^{DTCC} - \overline{cds}\|$	0.1	0.3	0.6	1.6	4.2	9.9	20.8	32.5	71.7
$\log(cds^{DTCC}) - \log(\overline{cds})$	-0.2	-0.1	-0.1	0.0	0.0	0.0	0.1	0.1	0.2
$\|\log(cds^{DTCC}) - \log(\overline{cds})\|$	0.0	0.0	0.0	0.0	0.0	0.1	0.1	0.2	0.3
Panel D: Contracts with at least ten trades per day in US5 counterparty sample ($N = 192,230$)									
Percentile	p1	p5	p10	p25	p50	p75	p90	p95	p99
$\sigma_t[\log(cds^{DTCC}) - \log(\overline{cds})]$	0.016	0.023	0.000	0.000	0.000	0.006	0.012	0.020	0.033
No. of transactions per day (same contract)	1.8	0.8	1.0	1.0	1.0	1.2	1.7	2.2	2.8
Average No. of sellers per buyer (same contract)	1.9	0.9	1.0	1.0	1.0	1.2	1.8	2.4	3.1

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.

Panel B of Table 2.2 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 in log spreads. 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.

2.2.4 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.[15] For observation date t, the lagged spread is denoted $\overline{cds}^{s}_{t-1}$ when dealer s is the seller of protection. There is substantial cross-sectional and time variation in dealers' credit risk in our sample. Across our sample period, the median difference in CDS spread

15. 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).

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.[16] 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.[17]

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 gross notional value that market participant b traded with dealer s in the recent past. We measure this share using the past 28 calendar 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

16. 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.

17. 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.

a minimum of 10 transactions. To express $Relations^{s,b}_{t-1}$ in share terms, we divide the total notional value transacted between b and s by the total notional value that market participant b traded.

2.3 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 and contract, we expect sellers' CDS spreads to be negatively associated with transaction spreads.

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

$$\begin{aligned} \log(cds^{s,b}_{i,t}) - \log(\overline{cds}_{i,t}) =& \alpha^{b}_{i,t} + \beta \log(\overline{cds}^{s}_{t-1}) + \eta WWR^{s}_{t} + \\ & \lambda Relations^{s,b}_{t-1} + \delta \log(size) + \epsilon^{s,b}_{i,t}, \end{aligned} \tag{2.1}$$

where $\log(cds_{i,t}^{s,b})$ is the log par spread on CDS transaction on reference entity i at time t.[18] 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. Note that the regression specification has contract and date fixed effects, thus our reported coefficients would be unchanged if we used the transaction spread as our dependent variable, instead of using the difference between a transaction spread and Markit quote. Only the intercept would change.

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

18. As discussed in Appendix 2.A, the actual market price of the CDS contract is an *upfront* payment after the CDS Big Bang. For investment grade reference entities, par spread remains 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. 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. Our empirical results are entirely robust to measuring prices in upfront points instead of par spreads. In addition, we apply the log transformation to spreads in order to mitigate heteroskedasticity. In default intensity models such as Duffie and Singleton (1999), the instantaneous variance of the credit spread is roughly proportional to its level. This relationship is confirmed empirically by Ben Dor et al. (2007). Our results are robust to running the regression without the log transformation.

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 (2.1) in Table 2.3. 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.

Table 2.3: Effects of Seller 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
Seller's CDS	-0.00667**	-0.00522	-0.00758**	-0.00610*	-0.00308**	-0.00434***
	(0.00333)	(0.00322)	(0.00341)	(0.00321)	(0.00128)	(0.00160)
Wrong-Way Risk (Indicator)	0.00527***		0.00548***			
	(0.00203)		(0.00205)			
Wrong-Way Risk (Correlation)		0.0176*		0.0242**	-0.00287	-0.0178
		(0.0104)		(0.0102)	(0.0125)	(0.0112)
Past Relationship	-0.000475	-0.00124	0.00149	0.000651	0.00457***	0.00507***
	(0.00398)	(0.00398)	(0.00427)	(0.00423)	(0.00153)	(0.00171)
No. of effective obs.	3,552	3,552	3,044	3,044	31,387	22,494
No. of transactions	12,377	12,377	10,712	10,712	195,256	136,355

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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

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 less than 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 again enters with positive sign, and the coefficient on seller credit spread is no longer significant. In Columns 3–4, we restrict the sample to the set of reference entities that are ineligible for central clearing, and obtain estimates 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.[19]

In Table 2.4, we re-estimate equation (2.1) 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 a reduction in the transaction spread of less than 0.2 basis points. 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.

19. Throughout all regression specifications in the paper, the coefficient on $\log(size)$ is very close to zero and insignificant and is not reported in the tables.

Table 2.4: Effects of Seller CDS Spreads on Log Par Spread Differentials (Interdealer 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	-0.00150*	-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	0.000853
	(0.00379)	(0.00381)	(0.00387)	(0.00389)	(0.00203)	(0.00220)
No. of effective obs.	18,255	18,255	17,480	17,480	201,722	160,498
No. of transactions	52,555	52,555	48,258	48,258	885,611	648,361

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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

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 (2.1) as follows:

$$\begin{aligned} \log(cds^{s,b}_{i,t}) - \log(\overline{cds}_{i,t}) =& \alpha^b_{i,t} + \alpha^s + \beta \log(\overline{cds}^s_{t-1}) \\ &+ \eta WWR^s_t + \lambda Relations^{s,b}_{t-1} + \delta \log(size) + \epsilon^{s,b}_{i,t}, \end{aligned} \tag{2.2}$$

We present regression results with additional seller fixed effects in Table 2.5. The coefficient on the seller's credit spread increases in magnitude, but remains modest.

Table 2.5: Effects of Seller CDS Spreads on Log Par Spread Differentials with Additional Seller Fixed Effects (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	-0.0180***	-0.0189***	-0.0192***	-0.0197***	-0.00307	-0.00691*
	(0.00554)	(0.00566)	(0.00645)	(0.00651)	(0.00390)	(0.00367)
Wrong-Way Risk (Indicator)	0.0116***		0.00382			
	(0.00400)		(0.00349)			
Wrong-Way Risk (Correlation)		0.0379*		0.0233	-0.00718	-0.0212*
		(0.0224)		(0.0250)	(0.0120)	(0.0123)
Past Relationship	2.14e-05	-0.000160	0.00226	0.00233	0.00239	0.00265
	(0.00414)	(0.00416)	(0.00442)	(0.00436)	(0.00156)	(0.00173)
No. of effective obs.	3,552	3,552	3,044	3,044	31,387	22,494
No. of transactions	12,377	12,377	10,712	10,712	195,256	136,355
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.

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 fact that CSA provisions are symmetric between large dealers, but are more likely to be asymmetric in favor of dealers for client-facing transactions, as discussed in Section 2.2.1. Neither WWR nor past relationship affects transaction spreads in a robust manner.

2.4 Effects of Counterparty Risk on Counterparty Choice

2.4.1 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 et al. 2018; Li and Schürhoff 2019). 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 2.4.4 we relax this assumption and our results are qualitatively similar.

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 implies that estimates of regression coefficients (though not fixed effects) remain consistent when the sample is truncated to a restricted choice set.[20]

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

$$\Pr(y^b_{i,t} = s | x^s_{i,t}) = \frac{\exp(x^s_{i,t}\beta)}{\sum_{\hat{s}=1}^{D_i} \exp(x^{\hat{s}}_{i,t}\beta)}, \qquad s = 1, \ldots, D_i. \tag{2.3}$$

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 165,112 ($= 8166 \times 13 + (12{,}377 - 8166) \times 14$) observations. In the US5 counterparty sample, our multinomial model has 972,224 ($= 4056 \times 4 + (195{,}256 - 4056) \times 5$) observations. The independent regressors are: credit risk of the seller, proxied as before by the CDS

20. 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.

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; 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; and interactions between seller dummy variables and the notional value of the traded contract, to allow for the possibility that buyers may gravitate towards particular sellers for larger trades. The coefficients on seller dummy variables and interactions are omitted from the discussion below to respect the confidentiality of the data.

We present regression estimates for equation (2.3) in Table 2.6. 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 is large, positive and statistically significant, which is indicative of persistence in trading relationships.

Table 2.6: Counterparty Choice of Non-dealers Buying Protection from Dealers

	(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	-0.268***	-0.264***	-0.260***	-0.258***	-0.159***	-0.176***
	(0.0383)	(0.0383)	(0.0409)	(0.0409)	(0.0104)	(0.0125)
Wrong-Way Risk (Indicator)	-0.184***		0.000676			
	(0.0474)		(0.0682)			
Wrong-Way Risk (Correlation)		-0.883***		-0.442	-0.713***	-0.695***
		(0.244)		(0.273)	(0.0726)	(0.0842)
Past Relationship	3.441***	3.443***	3.615***	3.614***	2.463***	2.511***
	(0.0491)	(0.0491)	(0.0534)	(0.0533)	(0.0107)	(0.0125)
Number of observations	165,112	165,112	141,802	141,802	972,224	677,719
Number of transactions	12,377	12,377	10,712	10,712	195,256	136,355
Number of buyers	197	197	196	196	829	801
Pseudo R-squared	0.386	0.386	0.392	0.392	0.423	0.432

Notes: We include seller fixed effects, the spread of the CDX.NA.IG index interacted with an indicator variable for each seller, and the notional amount of the trade 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.

To assess the economic importance of these coefficients we report marginal effects for these multinomial logit estimations in Table 2.7. 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%).[21] 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 2.0 percentage points. A one standard deviation increase in past-month transaction count increases the probability of selection by 4.1 percentage points. Relative to unconditional transaction shares of 7–13 percentage points, these effects are all of large economic magnitude.

21. We do not report marginal effects at the dealer level due to confidentiality restrictions.

Table 2.7: 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	0.041
0.06-0.01	-0.010	-0.007	0.015
Baseline sample excluding reference entities eligible for clearing			
0.13-0.07	-0.025	0.00006	0.040
0.06-0.01	-0.010	0.000002	0.017
US5 CP sample			
0.30-0.15	-0.025	-0.015	0.077
US5 CP sample excluding reference entities eligible for clearing			
0.30-0.15	-0.027	-0.022	0.050

Notes: Marginal effects are based on coefficients reported in Table 2.6.

In Columns 3 and 4 of Table 2.6 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.

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.

2.4.2 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.[22] Our

22. 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.

reported results are robust to an alternative rank-order measure of liquidity provided by DTCC.[23]

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.[24] 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 2.8. 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

23. 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.

24. 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.

entirely through the interaction term. The coefficient on the interaction with the log spread of the reference entity is small in magnitude.

Table 2.8: Counterparty Choice of Non-dealers Buying: Reference Entity Characteristics

	(1) US5 CP sample	(2) US5 CP sample	(3) US5 CP sample
Seller's CDS	-0.155***	-0.172***	-0.171***
	(0.0377)	(0.0389)	(0.0389)
Seller's CDS x log(Reference Entity's CDS)	-0.0108*	0.00268	0.00246
	(0.00612)	(0.00623)	(0.00624)
Seller's CDS x WWR (Correlation)	-0.290***	0.00859	0.00868
	(0.0346)	(0.0408)	(0.0409)
Seller's CDS x Liquidity of Reference Entity	0.0140***	0.00507***	0.00515***
	(0.00146)	(0.00154)	(0.00154)
Seller's CDS x Reference Entity is Financial		-0.207***	-0.209***
		(0.0202)	(0.0202)
Seller's CDS x Reference Entity is Sovereign		-0.0542***	-0.0560***
		(0.0191)	(0.0191)
WWR (Correlation)	-0.0235	-0.484***	-0.222*
	(0.0983)	(0.107)	(0.118)
WWR (Correlation) x Reference Entity is Financial			-0.667***
			(0.173)
WWR (Correlation) x Reference Entity is Sovereign			-1.307***
			(0.332)
Past Relationship	2.455***	2.438***	2.437***
	(0.0107)	(0.0107)	(0.0107)
Number of observations	972,224	972,224	972,224

Notes: We include seller fixed effects, U.S. reference entity fixed effect, the spread of the CDX.NA.IG interacted with an indicator variable for each seller, and the notional amount of the trade interacted with an indicator variable for each seller as controls. Robust standard errors are in parentheses. *** p¡0.01, ** p¡0.05, * p¡0.1.

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 remains statistically significant, but the effect of the interaction with WWR and the log spread of the reference entity 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.

2.4.3 Interactions with Characteristics of the Client

The determinants of the buyer's choice of seller might also depend on the client's own characteristics. The characteristics that we conjecture to be important are: 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.) and control for the extent to which a buyer is captive. 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.[25] 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.[26] 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 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

25. 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.

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

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 2.9. 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 2.10. 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.

Table 2.9: Counterparty Choice of Non-dealers Buying (Baseline Sample): Non-dealer Characteristics

	(1)	(2)	(3)
Seller's CDS	-0.340***	-0.142*	-0.213***
	(0.0467)	(0.0797)	(0.0818)
Seller's CDS x Buyer is Captive	0.235***		0.242***
	(0.0688)		(0.0697)
Seller's CDS x Buyer is a Frequent Trader	-0.0552		-0.0227
	(0.0407)		(0.0436)
Seller's CDS x Buyer has Short-Term Credit Exposure	0.222***		0.238***
	(0.0385)		(0.0405)
Seller's CDS x Buyer is a Hedge Fund		-0.114	-0.172**
		(0.0772)	(0.0833)
Seller's CDS x Buyer is an Asset Manager		-0.141*	-0.128
		(0.0789)	(0.0820)
Seller's CDS x Buyer is a Bank Non-dealer		-0.208**	-0.247**
		(0.0933)	(0.0977)
WWR (Indicator)	-0.198***	-0.184***	-0.196***
	(0.0474)	(0.0474)	(0.0474)
Past Relationship	3.413***	3.447***	3.414***
	(0.0492)	(0.0492)	(0.0494)
Number of observations	165,112	165,112	165,112

Notes: We include seller fixed effects, the spread of the CDX.NA.IG interacted with an indicator variable for each seller, and the notional of the trade interacted with an indicator variable for each seller as controls. Wrong-way risk is measured as 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. Robust standard errors are in parentheses. *** p¡0.01, ** p¡0.05, * p¡0.1.

Table 2.10: Counterparty Choice of Non-dealers Buying (US5 CP Sample): Non-dealer Characteristics

	(1)	(2)	(3)
Seller's CDS	-0.156***	-0.133***	-0.158***
	(0.0126)	(0.0210)	(0.0223)
Seller's CDS x Buyer is Captive	0.0659***	0.424	0.0625***
	(0.0172)		(0.0177)
Seller's CDS x Buyer is a Frequent Trader	-0.0461***		-0.0463***
	(0.0101)		(0.0103)
Seller's CDS x Buyer has Short-Term Credit Exposure	0.103***		0.0952***
	(0.0109)		(0.0112)
Seller's CDS x Buyer is a Hedge Fund		-0.00164	0.0192
		(0.0200)	(0.0211)
Seller's CDS x Buyer is an Asset Manager		-0.0555***	-0.0170
		(0.0201)	(0.0212)
Seller's CDS x Buyer is a Bank Non-dealer		0.0125	0.0407
		(0.0254)	(0.0262)
WWR (Correlation)	-0.611***	-0.610***	-0.617***
	(0.0732)	(0.0732)	(0.0732)
Past Relationship	2.450***	2.454***	2.451***
	(0.0107)	(0.0107)	(0.0107)
Number of observations	972,224	972,224	972,224

Notes: We include seller fixed effects, U.S. reference entity fixed effect, the spread of the CDX.NA.IG interacted with an indicator variable for each seller, and the notional amount of the trade 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.

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 2.11, 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.

Table 2.11: Number of transcations 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-financials	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	64,899	44%	71,341	40%	28,565	37%
Non-dealer Banks	10,959	7%	13,557	8%	6,665	9%
Financial Services	1,414	1%	1,297	1%	1,453	2%
Hedge Funds	61,617	41%	84,568	48%	31,548	40%
Insurance	1,939	1%	1,923	1%	770	1%
Non-financials	30	0%	21	0%	30	0%
Pension Plans	1,048	1%	1,110	1%	1,123	1%
Unclassified	6,878	5%	3,954	2%	7,972	10%
Total	148,784		177,771		78,126	

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.

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 that are "unclassified." For the baseline sample, results in Column 2 of Table 2.9 suggest that asset managers and bank non-dealers are more sensitive to credit risk than non-dealers of the omitted category. For the US5 counterparty sample, results in Column 2 of Table 2.10 indicate that asset managers are more sensitive to credit risk than non-dealers of all other types.

2.4.4 Additional Robustness Exercises

We conduct a variety of robustness exercises, and report results in Tables 2.12 and 2.13 for the baseline and US5 counterparty samples, respectively. For ease of comparison, Column 1 in each table repeats the benchmark specification from Table 2.6. 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.[27] Nevertheless, we have

27. 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.

taken precautions and added robustness checks aimed at addressing this issue. First, 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 2.12 and 2.13, 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. Second, 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 aggressively the dealer pursues CDS trade flow from clients. For a given dealer at date t, we identify the set of baseline sample client-facing transactions involving the dealer and any client for the 28 calendar days prior to date t.[28] 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 2.12 and 2.13, 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.

28. 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.

Table 2.12: Counterparty Choice of Non-dealers Buying from Dealers (Baseline Sample): Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Seller's CDS	-0.268***	-0.274***	-0.294***	-0.228***	-0.154***		-0.226***
	(0.0383)	(0.0388)	(0.0396)	(0.0423)	(0.0489)		(0.0395)
Seller's Net Positions		0.0558*					
		(0.0339)					
Seller's Price Aggressiveness			-1.272				
			(0.940)				
Seller's Log(Equity Market Cap)				0.199**			
				(0.0964)			
Seller's CDS x I(Eur. Debt Crisis)					0.000318		
					(0.000449)		
Seller's CDS x I(Volcker Rule)					-0.00189***		
					(0.000446)		
Seller's 5-Year Prob. of Default						-0.236***	-0.171***
						(0.0404)	(0.0412)
WWR (Indicator)	-0.184***	-0.185***	-0.198***	-0.186***	-0.183***	-0.185***	-0.186***
	(0.0474)	(0.0505)	(0.0488)	(0.0474)	(0.0475)	(0.0473)	(0.0473)
Past Relationship	3.441***	3.488***	3.475***	3.438***	3.445***	3.452***	3.445***
	(0.0491)	(0.0502)	(0.0517)	(0.0490)	(0.0491)	(0.0490)	(0.0489)
Number of observations	165,112	158,840	154,300	165,112	165,112	165,112	165,112

Notes: We include seller fixed effects, the spread of the CDX.NA.IG index interacted with an indicator variable for each seller, and the notional amount of the trade interacted with an indicator variable for each seller as controls. Wrong-way risk is measured as 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 2.13: Counterparty Choice of Non-dealers Buying from Dealers (US5 CP Sample): Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Seller's CDS	-0.156***	-0.161***	-0.149***	-0.188***	-0.159***		-0.0868***
	(0.0104)	(0.0105)	(0.0108)	(0.0122)	(0.0147)		(0.0126)
Seller's Net Positions		0.0351***					
		(0.00809)					
Seller's Price Aggressiveness			-0.233				
			(0.208)				
Seller's Log(Equity Market Cap)				-0.166***			
				(0.0317)			
Seller's CDS x I(Eur. Debt Crisis)					0.00088***		
					(0.00013)		
Seller's CDS x I(Volcker Rule)					-0.00087***		
					(0.00013)		
Seller's 5-Year Prob. of Default						-0.287***	-0.209***
						(0.0176)	(0.0209)
WWR (Correlation)	-0.602***	-0.549***	-0.545***	-0.615***	-0.590***	-0.624***	-0.618***
	(0.0731)	(0.0755)	(0.0734)	(0.0732)	(0.0731)	(0.0735)	(0.0734)
Past Relationship	2.454***	2.459***	2.456***	2.455***	2.451***	2.450***	2.449***
	(0.0107)	(0.0108)	(0.0108)	(0.0107)	(0.0107)	(0.0107)	(0.0107)
Number of observations	972,224	963,304	952,305	972,224	972,224	972,224	972,224

Notes: We include seller fixed effects, U.S. reference entity fixed effect, the spread of the CDX.NA.IG interacted with an indicator variable for each seller, and the notional of the trade 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.

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 for the Volcker Rule.[29] 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 7), both are negative and statistically significant.

29. 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." 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 et al. (2018) 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 2018). 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.

2.5 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.[30]

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 2.3 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 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.

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

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

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 platform, such as a SEF, that 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 2.2.1, 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 cleared and uncleared transactions on clearable reference entities using the union of the baseline and US5 counterparty samples. Of the 489,473 transactions on clearable reference entities in which either the buyer or 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 394,140 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 2.14 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 ("Seller CCP") 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 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 ("Buyer CCP") 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 2.14: Effects of Clearing and Counterparty Characteristics on Log Spread Differentials

	(1) Seller	(2) Buyer	(3) Pair
Seller CCP	-0.00330***		-0.00364***
	(0.000966)		(0.000460)
Buyer CCP		-0.00172*	-0.00312***
		(0.000905)	(0.000462)
Backload clear	-0.00178**	-0.000201	-0.00191***
	(0.000834)	(0.000756)	(0.000404)
Seller non-dealer	-0.00553***		-0.00964***
	(0.00101)		(0.000556)
Buyer non-dealer		0.00468***	0.00897***
		(0.000839)	(0.000447)
Number of observations	353,148	394,140	489,473
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 2.14 also documents a significant dealer pricing advantage, consistent with the evidence of dealer market power presented in Siriwardane (2019) and Cenedese, Ranaldo, and Vasios 2019 for CDS and IRS markets. In Column 1, we find that non-dealer sellers obtain 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. The reduction in spreads is potentially due to the effects on competitive structure associated with migration from opaque bilateral OTC trading to more transparent trading on electronic platforms. 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.

2.6 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 using actual transaction spreads. 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.

Our results highlight that credit rationing may arise under wider circumstances than previously recognized. The likelihood of a counterparty "run" in OTC derivative markets offers another compelling reason for the transition towards central clearing. Despite the significant growth in central clearing for IRS and CDS products in recent years, managing OTC counterparty risk exposure remains important as several large derivative markets (e.g., foreign exchange, equity-linked derivatives, and options) remain largely uncleared. Our results also have implications for the newly agreed international rules that require bilateral provision of initial margin to be held in segregated third-party custodial accounts, as these provisions would reduce counterparty losses in the event of dealer default and make non-dealers less sensitive to dealer risk.

APPENDICES

2.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.

Throughout the sample period of 2010–2013, CDS were 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 an observation 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 leave us transactions on 12 reference entities.[31] 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 86,757 and 1,551,414 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 1,469,775 transactions on 1635 single-name reference entities. Regression results are robust to relaxing this restriction to a cap of 5000 basis points.

31. 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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CHAPTER 3

Caught in the Act: How Corporate Scandals Hurt Employees

With Jason Sockin *(University of Pennsylvania)*

3.1 Introduction

Corporate scandals are costly for firms. Revelations of misconduct can lead to fines, damage firm reputation, and hurt future performance (Karpoff, Lee, and Martin 2008; Armour, Mayer, and Polo 2017). Wells Fargo, for example, paid over \$3 billion in legal settlements and saw its earnings fall sharply after its long-running fraudulent sales practices came to light in 2016.[1] Academic studies and the popular press have extensively covered the effects of misconduct on financial measures such as stock returns, but employee outcomes have received considerably less attention. This disparity is notable, as corporate governance and social responsibility have become increasingly important considerations for members of the workforce (Winograd and

1. For further details, see "Wells Fargo Posts Weaker Earnings After Sales-Practices Scandal," *Wall Street Journal*.

Hais 2014). In this paper, we help fill the gap by determining the causal impact that scandals have on employees' job satisfaction and compensation.

Our data come from the online platform Glassdoor, which crowdsources reviews of employers and pay reports from workers. Through the platform, we have access to measures of workers' satisfaction with their firms along various dimensions, their base and variable earnings, and their assessments of their employers' fringe benefits packages. The data allow us to study employee welfare, as we can measure if losses in any of these dimensions are offset by increases in another. Firms may, for example, respond to decreases in worker sentiment by improving remuneration. If any such declines are not offset, however, we can conclude that corporate scandals leave employees strictly worse off. The granularity of the data enable us to conduct analysis across a variety of individual and employer characteristics, and determine if certain workers are more exposed to misconduct. We also test if changes to employee sentiment and pay are persistent, to ascertain if the effects of scandals are long-lasting or tied to short-term phenomena such as increased news coverage.

We begin by investigating how corporate scandals affect workers' perceptions of their employers. Utilizing a difference-in-differences framework, we find that employees report less satisfaction with their firms in the wake of such events. More specifically, the overall rating among employee reviews drops on average 0.07 stars (on a 1–5 stars scale) in the two years following a scandal. Based on the estimate from Sockin 2021 that an additional star of job satisfaction is worth about $9,400, this decrease translates to an average annual loss of about $700 for each employee. Subcategory ratings reveal that the decline in overall satisfaction is driven by worse evaluations of a firm's senior management and its culture and values. In the aftermath of a scandal, employees are also less approving of their chief executive officers (CEOs) and less likely to recommend their employer to friends. The declines in worker sentiment may have adverse consequences for firms, including lower labor productivity and increased difficulty hiring and retaining employees (Burks et al. 2015; Brown, Setren, and Topa 2016).

To validate our identification strategy and determine if the decreases in ratings are long-lived, we repeat the analysis using finer time intervals around event dates. We find that employee sentiment drops immediately after corporate misdeeds become public and remain lower than their pre-scandal averages throughout the two years that follow. Further, we find that the ratings of scandal-hit and control firms evolve similarly leading up to event dates. Thus, though underlying characteristics may render certain firms more likely to engage in misconduct (Liu 2016; Ji, Rozenbaum, and Welch 2017), we find no evidence that the parallel trends assumption underlying our analysis is violated.

While the baseline regressions include an array of worker and employer controls, our findings could stem from changes in reviewer composition. To address this concern, we re-estimate the baseline regressions with additional person-specific fixed effects. This limits the sample to individuals who leave multiple reviews, but the results are similar to those from our benchmark specifications. We further test whether the erosion in sentiment is limited to certain categories of workers. We document declines for every subset of workers we consider, particularly in the culture and values subcategory, indicating that the drop in ratings occurs across reviewer characteristics. The one exception to this universal decrease comes when we test if employees hired after scandals report lower levels of job satisfaction than their peers did prior to such events. We find no effect on sentiment for newer hires, suggesting that job seekers who are less perturbed by corporate misconduct may sort into scandal-hit firms.

We next study how scandals impact employee compensation. We find, after controlling for a rich set of worker and firm characteristics, that base pay is unaffected by such events. Variable pay for those who earn it, however, drops on average 10.2 percent at scandal-hit firms relative to control firms. We also find a weakly significant 5.4 percentage point decline in the probability of earning variable pay, suggesting that the set of employees who receive such earnings may shrink following a scandal. Our results are consistent with downward nominal rigidity in base wages (Fallick, Lettau, and Wascher 2016) and demonstrate that variable compensation acts as a transmission mechanism for passing firm-level shocks on to workers. The finding that variable wage earners are differentially exposed to such shocks is quite novel, as traditional labor market datasets rarely separate earnings into base and variable components. Partitioning the sample by experience shows that while junior employees bear larger percentage point reductions in variable pay, their senior colleagues face larger declines in dollars. Decomposing the baseline results into annual effects reveals no pre-trends prior to scandals and demonstrates that the decrease in variable earnings persists for at least three years.

Finally, using employees' ratings of their employers' fringe benefits, we find no evidence that firms augment benefits packages following a scandal. Taken together, our results demonstrate that workers at scandal-hit firms are left strictly worse off. They experience a reduction in job satisfaction and variable pay, but are not compensated for these declines with improved base wages or fringe benefits.

Our work is most closely related to three studies from the accounting literature. Lee et al. 2021 and Zhou and Makridis 2021 document declines in Glassdoor ratings following news of tax avoidance and Auditing Enforcement Releases (AAERs) by the Securities and Exchange Commission (SEC), respectively. The latter also find that reviewer comments discuss firm culture more negatively after misconduct comes to light. Choi and Gipper 2021 use confidential Census data to show that wages fall during and after periods of misconduct, and that employee turnover increases following AAERs. Relative to these papers, we utilize a broader set of events and outcome variables. By jointly considering employee sentiment and compensation, we are able to draw conclusions about employee welfare. We find no decline in pay prior to scandals, highlighting that wage dynamics are different around AAERs than our events. We also show the importance of separately considering base and variable pay. This decomposition is not possible through the Census data, but it reveals that workers who earn variable compensation are more exposed to corporate misconduct. Other novel outcomes we study include fringe benefits ratings and job application rates.[2] To demonstrate our findings are not driven by accounting fraud, we show that the magnitude and statistical significance of our results are similar when we re-estimate each of our baseline regressions using only the subset of "non-fraud" scandals.

Other studies on negative reputation events such as financial misconduct (Karpoff, Lee, and Martin 2008; Murphy, Shrieves, and Tibbs 2009; Armour, Mayer, and Polo 2017), environmental violations (Karpoff, Lott, and Wehrly 2005), product re-

2. In Appendix 3.D, we show that Glassdoor users are less likely to apply to scandal-hit firms in the immediate aftermath of such events. Our job search data have several limitations, but the results provide preliminary evidence that job seekers respond to corporate misconduct.

calls (Jarrell and Peltzman 1985; Liu and Shankar 2015), and data breaches (Kamiya et al. 2020) tend to focus on financial outcomes. Much of this work has found negative effects, including declines in earnings and heightened stock price volatility. One exception is Akey et al. 2021, who show that firms increase spending on corporate social responsibility following data breaches in order to rebuild reputational capital. We contribute to this literature by studying how negative reputation events affect employees.

A related series of papers centers on collective reputation (Freedman, Kearney, and Lederman 2012; Bai, Gazze, and Wang 2021; Bachmann et al. 2022). These studies find that incidents such as the Volkswagen emissions and Chinese dairy quality scandals have negative spillovers on firms in similar product markets and industries. Because we focus on employees from scandal-hit firms, our difference-in-differences estimates will understate the true impact of misconduct if there are negative externalities on sentiment and wages at competing firms.

Lastly, our work is related to the empirical literature studying how idiosyncratic shocks affect employee compensation. Garin and Silverio 2018 find that employee pay falls for incumbent workers and not new hires in response to negative shocks to export demand. Baghai et al. 2021 document that export shocks caused by currency movements cause firms to lose employees with high human capital. Guiso, Pistaferri, and Schivardi 2005 study risk-sharing behavior between firms and workers using residual firm productivity to identify shocks. Sockin and Sockin 2021 show that workers' earnings fall when their firm's credit rating is downgraded from investment to speculative grade. Our results demonstrate that workers are not uniformly exposed to firm-level shocks, as those who receive variable pay are more likely to see their income affected.

3.2 Data

3.2.1 Corporate Scandals

Our sample of corporate scandals comes from the popular press. Several publications, including Fortune Magazine, produce annual articles detailing the year's most notable "business misdeeds." We aggregate these lists and restrict attention to firms with appreciable coverage on Glassdoor. To ensure the presence of sufficient data both before and after a scandal, we consider only events that take place between 2013 and 2018. Table 3.1 presents information on the 23 scandals in our primary sample.[3] As shown in Column 3, the events are heterogeneous, but each involves misconduct precipitated by firm culture or carried out directly by managers. Event dates are those on which a transgression became publicly known, not necessarily when it occurred. Our maintained assumption is that while certain insiders may have been aware of impropriety prior to these dates, the majority of rank-and-file employees were not. To demonstrate that our results are not driven by the types of financial misconduct studied in Choi and Gipper (2021) and Zhou and Makridis (2021), we conduct robustness tests using only the "non-fraud" scandals identified with asterisks in Table 3.1.

3. Additional details about sample construction as well as links to background news articles and the underlying scandal lists are provided in Appendix 3.A.

Table 3.1: Corporate Scandal Sample

Event date	Employer	Description	CEO exits	Employer reviews	Base pay	Variable pay	Benefits ratings
February 10, 2013	Carnival*	Stranded ship	06/25/2013	138	143	63	–
March 18, 2013	lululemon*	Product recall	06/10/2013	153	225	51	–
July 11, 2013	GlaxoSmithKline	Bribery	N	480	1,011	769	–
October 24, 2013	Macy's*	Racial profiling	N	6,462	5,189	796	–
December 05, 2014	Sony*	Data breach	02/06/2015	1,317	1,248	684	–
July 20, 2015	Toshiba	Accounting fraud	07/21/2015	458	440	232	–
September 20, 2015	Volkswagen*	Emissions violations	09/23/2015	301	328	177	–
October 21, 2015	Valeant	Accounting fraud	03/21/2016	144	129	85	–
July 06, 2016	Fox*	Sexual harassment	07/21/2016	836	1,113	252	135
August 18, 2016	Mylan Inc*	Price gouging	N	285	374	237	108
September 02, 2016	Samsung*	Product recall	N	1,359	1,435	920	354
September 08, 2016	Wells Fargo	Sales fraud	10/12/2016	13,184	18,051	8,263	3,204
February 19, 2017	Uber*	Sexual harassment	06/21/2017	1,909	3,737	1,304	766
April 10, 2017	United Airlines*	Customer abuse	N	1,796	2,486	779	581
September 07, 2017	Equifax*	Data breach	09/26/2017	464	820	425	166
December 20, 2017	Apple*	Planned obsolescence	N	7,201	12,710	3,400	2,575
January 25, 2018	Wynn Resorts*	Sexual harassment	02/06/2018	229	447	78	59
February 01, 2018	Guess?*	Sexual harassment	06/12/2018	462	625	75	142
March 06, 2018	Google*	Secret gov't contract	N	4,258	17,247	8,551	2,236
March 15, 2018	Facebook*	Data misuse	N	1,971	7,765	4,180	1,305
July 27, 2018	CBS*	Sexual harassment	09/09/2018	562	181	7	212
August 07, 2018	Tesla	Potential securities fraud	N	2,293	5,325	1,441	732
November 19, 2018	Nissan	Appropriation of funds	11/19/2018	541	1,340	577	175

Notes: This table briefly describes each of the scandals in our sample. Columns 5–8 display observation counts around event dates. For employer reviews, the event window ranges from 24 months before through 24 months after events; for pay reports, four years before through three years after; and for benefits ratings, three years before through three years after. Events with asterisks are included in the "non-fraud" subsample.

3.2.2 Glassdoor Data

Our data come from Glassdoor, an online platform with information about firms, compensation, and the labor market more broadly. Glassdoor data are particularly well-suited for the study of corporate scandals, as employers are identified by name and we observe both wages and job satisfaction for employees at each firm. Public surveys and other standard data sources typically redact employer identities and are published at low frequencies with a non-trivial lag between data acquisition and publication. We make use of three Glassdoor datasets: i) reviews of employers, ii) pay reports, and iii) ratings of fringe benefits.

3.2.2.1 Reviews of Employers

Employer reviews are provided voluntarily and anonymously by visitors to Glassdoor. Individuals are incentivized to submit reviews through a "give to get" policy, whereby visitors gain access to information on the website by contributing to its content.[4] When completing a review, workers are asked to submit ratings of their employer on a Likert scale from 1 star to 5 stars, with more stars corresponding to higher degrees of satisfaction. Respondents can also evaluate the firm on the same 5-star scale along the following five sub-dimensions: culture and values, career opportunities, senior management, compensation and benefits, and work-life balance. Reviewers are further asked if they approve of CEO performance, whether they would recommend the employer to a friend, and if they have a positive business outlook for the firm over the next six months. For each of these three questions, we generate

4. This "give to get" policy has been shown to reduce the selection bias inherent to online reviews, by motivating individuals with more moderate opinions to disclose (Marinescu et al. 2021).

indicator variables equal to one for positive responses and zero for neutral or negative responses. Respondents may also disclose the following information: whether they are a current or former employee, employment status (i.e., full-time, part-time, contract, or intern), job title, length of employment, and location. In total, we observe 4.9 million employer reviews. Observation counts around event dates for each firm in the scandal sample are reported in Column 5 of Table 3.1.

3.2.2.2 Pay Reports

Employee pay reports are also submitted to Glassdoor voluntarily under the "give to get" policy. Reports are anonymous, so individuals have little, if any motive to distort wages. The self-reported nature of the data may elicit concerns about measurement error, but multiple studies have demonstrated that Glassdoor wages closely align with those from traditional labor market datasets. Karabarbounis and Pinto 2018 find that certain industries are overrepresented in the Glassdoor data, but that the wage distributions within industries match those from the Quarterly Census of Income and Wages (QCEW) and the Panel Study of Income Dynamics (PSID). As our pay regressions exploit variation within firms and industry-job title pairs, the lack of representativeness across industries does not compromise the validity of our results. Gibson 2021 shows that average salaries by occupation in Glassdoor closely align with averages in the Occupational Employment Statistics (OES) produced by the Bureau of Labor Statistics. Sockin 2021 also documents a correlation of 0.92 in mean earnings across industry-occupation pairs between Glassdoor and the Annual Social and Economic Supplement (ASEC) of the Current Population Survey.

When submitting a pay report, respondents are asked for the following information: employer name, job title, year of salary, base income, additional income earned through cash and stock bonuses, profit sharing, sales commissions and tips/gratuities, pay frequency (annual, hourly, or monthly), gender, years of work experience, location, whether current or former employee, and employment status.[5] Pay is reported by calendar year, and we inflation-adjust to 2018 dollars using U.S headline CPI. To limit measurement error arising from retrospective reporting, we restrict the sample to the 87.6% of pay reports submitted for the current or previous year. Since we do not observe hours worked, we also limit the sample to full-time employees. We define variable pay as the sum of an employee's cash and stock bonuses, profit sharing, and sales commissions.[6] To avoid undue influence from outliers and misreporting, we exclude reports that document less than $200 in any compensation category and those in the top and bottom 0.05 percent of base or variable pay. We observe a total of 5.2 million pay reports. Table 3.1 displays the number of base pay (Column 6) and variable pay (Column 7) observations for each firm in the scandal sample.

3.2.2.3 Ratings of Fringe Benefits

Employer benefits ratings are also submitted voluntarily and anonymously under the aforementioned "give to get" policy. Respondents are asked to rate their overall

5. For the 28.0 percent of full-time employees providing hourly wages, we annualize pay by assuming 50 weeks worked at 40 hours per week. We drop the 1.4 percent of reports providing monthly wages. The 34.0 percent of pay reports without information on gender are assigned to an "unavailable" group.

6. Since we do not include tips in our measure of variable pay, we drop the food services industry. We also do not consider public sector employees or those in mining or agriculture, as these sectors are not well represented on Glassdoor. None of the scandal-hit firms are in these industries and few of their employees earn tips, so these exclusions have little impact on our results.

benefits package, which includes items such as health insurance and retirement plans, on a Likert scale from 1 star to 5 stars. They are further asked whether they are a current or former employee and for their employment status, job title, and location. Benefits ratings were introduced in 2014, which limits our sample to 14 scandals. We observe 0.8 million benefits reviews in total. Counts around event dates are presented in Column 8 of Table 3.1.

3.3 Effects on Employee Sentiment

In this section, we study the effects of corporate scandals on employee sentiment. Because these events convey negative information about firms, we expect them to adversely affect worker satisfaction. In particular, we expect employees to have worse assessments of senior management and firm culture. If shocks to reputation are long-lived, as in Liu and Shankar 2015, then the decline in sentiment will be persistent. If, however, diminished employee perception is driven by negative news coverage or other short-term phenomena, ratings will quickly revert to their pre-event levels.

In order for a scandal to shift employee ratings down, it must constitute a "surprise." If a worker believes *ex ante* that their employer is prone to misconduct, then their sentiment may not change when a scandal does occur. Further, if rank-and-file employees learn about misconduct prior to the public, then we will not observe a decrease in ratings around event dates. Actions taken by an employer in the aftermath of a scandal may also lead to improved sentiment. If firm leadership quickly addresses misconduct or attempts to placate employees by improving working conditions, then ratings may increase after such events.

3.3.1 Summary Statistics

Summary statistics for the employer reviews dataset are presented in Table 3.2. The control group consists of firms that experienced neither a corporate scandal nor a data breach between 2013 and 2018.[7] Since scandal-hit firms tend to be large employers, we also restrict the control sample to include only large employers for congruence. Following Haltiwanger, Jarmin, and Miranda 2010, we define large firms as those with at least 500 employees. As shown in Columns 3 and 6, the average overall and subcategory ratings are slightly larger for scandal-hit firms than control firms. The average overall rating for the former group is is 3.58 stars, for example, compared to 3.40 for the latter. Averages of the three binary response variables and the demographic variables presented in the bottom panel are similar across groups. It does not appear that employees at firms committing corporate misdeeds have lower levels of job satisfaction ex-ante.

3.3.2 Baseline Difference-in-Differences Regressions

To ensure our estimates are not biased by secular trends in ratings over time, we employ a generalized difference-in-differences framework to assess how scandals affect employee sentiment. The key identifying assumption is that absent an event, ratings for scandal-hit firms would have evolved in a similar manner to those for control firms. Our benchmark specification is given by the equation

$$R_{ikt} = \beta \cdot PostScandal_{kt} + \lambda X_i + \gamma_k + \gamma_{\iota(k)t} + \epsilon_{ikt} \tag{3.1}$$

7. In Appendix 3.B, we study the evolution of ratings around data breaches to demonstrate that our findings are not driven by increased news coverage. For parsimony, we therefore exclude the 27 firms that experienced large data breaches during this period from the control sample.

Table 3.2: Summary Statistics for Employee Reviews

	Corporate scandals			Control firms		
Measure	N	mean	sd	N	mean	sd
Overall rating	47	3.58	1.25	4,837	3.40	1.33
Career opportunities	40	3.40	1.33	4,114	3.17	1.38
Compensation and benefits	40	3.58	1.26	4,111	3.28	1.30
Culture and values	40	3.50	1.41	4,083	3.33	1.46
Senior management	40	3.12	1.40	4,048	2.96	1.46
Work-life balance	40	3.32	1.34	4,118	3.26	1.39
Would refer a friend	47	0.52	0.50	4,837	0.49	0.50
Positive business outlook	28	0.59	0.49	3,089	0.54	0.50
Approve of CEO performance	28	0.41	0.49	3,089	0.46	0.50
Is current employee	47	0.53	0.50	4,837	0.53	0.50
Concealing employee	47	0.52	0.50	4,837	0.49	0.50
Low tenure employee	28	0.59	0.49	3,089	0.54	0.50
Female employee	45	0.28	0.45	4,440	0.28	0.45
Full time employee	35	0.69	0.46	3,970	0.74	0.44
HQ employee	26	0.19	0.39	2,832	0.26	0.44
Managerial employee	47	0.08	0.28	4,828	0.11	0.31
Age	15	31.6	9.4	1,296	33.3	10.5
Firm employment (1000s)	47	134.0	96.2	4,836	64.0	155.2

Notes: This table reports summary statistics from the Glassdoor ratings dataset for scandal-hit and control firms. The control sample consists of large firms that did not experience a scandal or data breach between 2013 and 2018. Observation counts (N) are reported in thousands.

where R_{ikt} is the star rating or indicator response for worker i employed at firm k in year-month t, γ_k is a firm fixed effect, $\gamma_{\iota(k)t}$ is an industry-year-month fixed effect, X_i is a vector comprised of current employee and employment status indicators, and $PostScandal_{kt}$ is an indicator equal to one if firm k faced a scandal prior to or during year-month t. Standard errors are clustered at the firm level, as employee ratings for a given firm are likely to be correlated across time. In our primary specification, the pre-period consists of the twenty-four calendar months before an event and the post-period is composed of the event month and the subsequent twenty-three months.

The results are reported in Panel A of Table 3.3. The overall rating among newly-submitted reviews drops by an average of 0.069 stars in the two years following a scandal. Relative to the pre-scandal average of 3.64 out of 5 stars, this represents a 1.9 percent decrease. Sockin 2021 estimates that one star in overall Glassdoor rating is worth about \$9,400 in additional annual pay. The 0.069-star decline therefore translates to an average loss of about \$700 per year for each employee at the firm.[8] Consistent with our hypotheses, coefficients from the subcategory regressions indicate that the decline in overall rating is driven primarily by diminished perceptions of firm culture and values (–0.115 stars) and senior management (–0.094 stars). The significant, negative estimate for career opportunities suggests that scandals may also reduce employees' opinions of their firms' future prospects. The insignificance of the compensation and benefits and work-life balance coefficients affirm that employees do not indiscriminately report that all aspects of the firm are worse.

8. For workers in the upper quintile of the earnings distribution, Sockin 2021 estimates that one additional star in overall rating is worth about \$20,600. The loss in job satisfaction value caused by scandals is thus more than twice as large for high earners.

Table 3.3: Difference-in-Differences Results for Employer Reviews

	Overall rating	Culture and values	Senior mgmt.	Career opp.	Comp. and benefits	Work-life balance	Would refer a friend	Positive business outlook	Approve of CEO
Panel A: Full sample									
After scandal	-0.069***	-0.115***	-0.094***	-0.057***	-0.030	-0.000	-0.023***	-0.064***	-0.076***
	(0.026)	(0.029)	(0.029)	(0.021)	(0.023)	(0.040)	(0.009)	(0.017)	(0.022)
N	4872600	4111917	4076533	4143486	4140066	4147415	3839869	3617913	3241784
Scandal N	46803	39902	39544	40277	40247	40233	37283	35187	33112
Adjusted R^2	0.17	0.17	0.15	0.15	0.17	0.15	0.14	0.13	0.14
Panel B: Non-fraud sample									
After scandal	-0.101***	-0.119***	-0.110***	-0.053*	-0.050*	-0.065*	-0.030***	-0.047**	-0.038**
	(0.032)	(0.045)	(0.041)	(0.030)	(0.030)	(0.034)	(0.011)	(0.021)	(0.017)
N	4855500	4097110	4061845	4128555	4125142	4132481	3826281	3605103	3230021
Scandal N	29703	25095	24856	25346	25323	25299	23695	22377	21349
Adjusted R^2	0.17	0.17	0.15	0.15	0.17	0.15	0.14	0.13	0.14

Notes: This table reports coefficients when Equation 3.1 is estimated for corporate scandals on the dependent variable listed in each column heading. The pre- and post-periods are each 24 months. Regressions include firm, industry x year-month, current employee, and employment status fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

The final three columns in Panel A present results for regressions on the three binary response variables. We estimate that a corporate scandal causes a 2.3-percentage-point decline in the fraction of employees who would recommend their employer to a friend. As referral networks are a valuable recruiting channel (Burks et al. 2015; Brown, Setren, and Topa 2016), this finding suggests misconduct may hinder a firm's ability to hire new workers. We also find a 6.4-percentage-point decline in the share of employees who hold a positive business outlook for their employer. This result accords with the reduction in perceptions of career opportunities at the firm and may predict poor future stock performance (Sheng 2021). Lastly, we document a 7.6-percentage-point drop in the rate of CEO approval in the wake of a scandal.

In Panel B of Table 3.3, we report results when Equation 3.1 is re-estimated using only the set of "non-fraud" scandals. The coefficients remain significant and similar in magnitude to those for the full sample, indicating that all types of scandals, not just those involving financial misconduct, lead to declines in employee sentiment. We also conduct a series of untabulated robustness checks. They include regressions with a broader set of control firms, stacked regressions to avoid possible bias from the staggered timing of events (Goodman-Bacon 2021; Cengiz et al. 2019), weighted regressions to account for changes in sample composition across firms over time, and a set of "leave-one-out" regressions to ensure that no single event drives our findings. In all cases, the results are similar to those from our benchmark specification.

We next study how the effects of corporate scandals on worker sentiment evolve over time. Decomposing the baseline estimates allows us to determine if satisfaction levels erode in the immediate aftermath of an event and recover quickly, or if the drop persists. To obtain time-varying effects, we re-estimate Equation 3.1 but replace

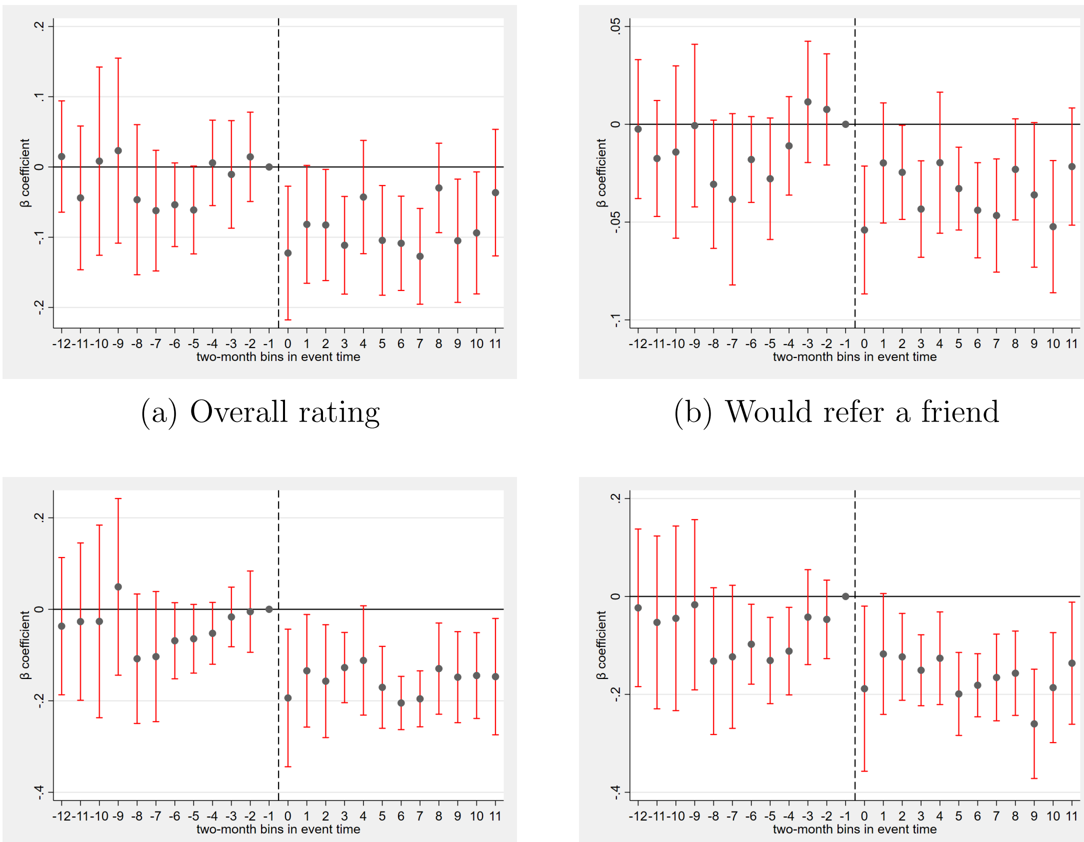
$\beta \cdot PostScandal_{kt}$ with separate terms for each consecutive two-month bin ranging from twenty-four months before through twenty-four months after a scandal. The omitted group consists of the two calendar months immediately prior to an event.

Results for the focal outcomes are presented in Figure 3.1. Estimates from the periods prior to events suggest that our results are not driven by pre-trends. The negative effects of scandals on overall ratings, culture and values, and senior management develop quickly and persist. Ratings in each category drop more than one-tenth of a star in the immediate aftermath of a scandal and remain below their pre-scandal levels over the subsequent two years. Referral probabilities also fall sharply and remain depressed throughout the post-scandal period.

3.3.3 Further Results

Given that our dataset is comprised of cross-sections of individuals, the previous findings may partially reflect shifts in the types of employees who submit reviews after a scandal occurs. We therefore restrict the sample to individuals who leave multiple reviews and re-estimate Equation 3.1 with additional worker fixed effects to account for time-invariant idiosyncratic characteristics. Results for the three focal ratings — overall, culture and values, and senior management— estimated with and without worker fixed effects are presented in Table 3.4. Despite an appreciably smaller number of observations, we find significant declines in overall and culture and values ratings when the additional fixed effects are included. The similarity of these estimates to those from our benchmark specifications indicate that shifts in reviewer composition do not explain our results.

Figure 3.1: Employee Ratings around Scandal Dates, Dynamic Results



(a) Overall rating

(b) Would refer a friend

(c) Culture and values

(d) Senior management

Notes: These figures display coefficients from the dynamic version of Equation 3.1. We use 48-month windows around corporate scandal dates. Horizontal dashes indicate a 95 percent confidence interval around each point estimate. Regressions include firm, industry x year-month, current employee, and employment status fixed effects. Each coefficient is relative to the 2-month period prior to the scandal. Standard errors are clustered by firm.

Table 3.4: Outcomes After Scandals Incorporating Worker Fixed Effects

	Overall rating		Culture and values		Senior management	
	Panel A: Full sample					
After scandal	-0.031	-0.071*	-0.122**	-0.119**	-0.047	-0.062
	(0.055)	(0.041)	(0.053)	(0.057)	(0.051)	(0.053)
Worker FE	N	Y	N	Y	N	Y
Pre-scandal mean	3.28	3.28	3.28	3.28	2.88	2.88
N	492896	492896	426978	426978	422869	422869
Scandal firm N	4243	4243	3675	3675	3631	3631
	Panel B: Non-fraud sample					
After scandal	-0.081	-0.095*	-0.113	-0.103*	-0.058	-0.055
	(0.080)	(0.052)	(0.078)	(0.061)	(0.076)	(0.065)
Worker FE	N	Y	N	Y	N	Y
Pre-scandal mean	3.46	3.46	3.44	3.44	3.02	3.02
N	490362	490362	424792	424792	420696	420696
Scandal firm N	2749	2749	2379	2379	2345	2345

Notes: This table reports results when Equation 3.1 is re-estimated with additional worker fixed effects on the dependent variable listed in each column heading. The pre- and post-periods are each 24 months. Regressions include firm, industry x year-month, current employee, and employment status fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

In the subsequent section, we show that base pay is unchanged following a scandal, but variable pay declines. To determine if wage loss drives the downturn in sentiment after corporate misconduct, we re-estimate Equation 3.1 with an interaction term between the post-scandal indicator and an additional indicator equal to one if the reviewer has a high likelihood of receiving variable pay. We classify a reviewer as having a high likelihood of receiving variable pay if the percentage of workers with their employer and job title pair who earn variable pay is above the median. Panel A of Table 3.5 reveals a significant decrease in ratings for both groups, which suggests the decline in sentiment is not due to reductions in pay. In Panel B, we show that negative effects arise for both managers and rank-and-file employees, which suggests that workers across the corporate hierarchy are unaware of misconduct before it becomes public. We conduct similar untabulated tests for other characteristics such as job seniority, gender, and firm tenure. In all cases, we find statistically significant declines in, at minimum, the culture and values ratings across groups.

Given that scandals have long-lasting effects on employee sentiment and firm reputation, they may affect worker sorting. Individuals who value firm culture, for example, may choose to avoid firms associated with impropriety. To test for sorting, we re-estimate Equation 3.1 with an interaction term between the post-scandal indicator and an additional indicator equal to one if the reviewer was hired after the event date. We also extend the post-event window to five years for this specification, to augment the number of new employees. The results, presented in Panel C of Table 3.5, reveal a significant decline in ratings only for workers hired before scandals take place. This disparity suggests that individuals who are less disapproving of corporate misconduct may indeed sort into firms that have recently faced scandals.

Table 3.5: Difference-in-Difference Results for Scandals, Worker Heterogeneity

	Overall rating	Culture and values	Senior management
Panel A: Probability of earning variable pay			
Total effect: low probability	-0.075*	-0.088**	-0.072*
	(0.040)	(0.041)	(0.040)
	[6630]	[6041]	[5986]
Total effect: high probability	-0.065***	-0.124***	-0.101***
	(0.025)	(0.028)	(0.026)
	[18671]	[15179]	[15051]
Panel B: Managerial role			
Total effect: manager	-0.139***	-0.207***	-0.175***
	(0.049)	(0.057)	(0.055)
	[2199]	[2048]	[2046]
Total effect: non-manager	-0.062**	-0.106***	-0.085***
	(0.026)	(0.029)	(0.028)
	[23073]	[19145]	[18964]
Panel C: When hired			
Total effect: hired before scandal	-0.078***	-0.105***	-0.099***
	(0.027)	(0.027)	(0.028)
	[8766]	[8205]	[8169]
Total effect: hired after scandal	-0.014	-0.038	0.022
	(0.048)	(0.059)	(0.059)
	[2230]	[1984]	[1942]

Notes: This table reports coefficient estimates for the effects following a scandal on different partitions of reviewers. The pre- and post-periods are each 24 months, except the post-period for Panel C is 60 months to sufficiently incorporate new hires. Regressions include firm, industry x year-month, current employee, and employment status fixed effects. To account for the partitioned characteristic, the following observables are included in each panel respectively: an indicator for low probability, an indicator for managerial job title, and firm tenure fixed effects. Standard errors are clustered by firm. Sample counts for scandal-hit employers in the post-period are given in brackets. Significance levels: * 10%, ** 5%, *** 1%.

Together, the findings in this section demonstrate that scandals erode employee sentiment. Workers' ratings of employers drop appreciably and persistently in response to such events, signifying a deterioration in job satisfaction. The results do not stem from changes in reviewer composition and are robust to alternative specifications.

3.4 Effects on Compensation

Our findings in the preceding section show that job satisfaction worsens following a corporate scandal. If employees derive utility from workplace attributes unrelated to compensation (Maestas et al. 2018), the reduction in sentiment constitutes a loss in the amenity value they receive from their employers. The theory of equalizing differences (Rosen 1986) suggests that firms may look to compensate workers for this loss by raising wages.

Alternatively, if firms seek to offset the pecuniary costs associated with scandals (e.g. fines, loss of revenue) or productivity declines due to diminished job satisfaction (McGregor 1960), wages may fall after such events. In a model where the surplus from an employee-employer match is split at a fixed rate between the worker and the firm, as in Mortensen and Pissarides 1994, negative shocks to surplus are passed on to workers through reduced wages. In the presence of downward rigidity in base wages (Fallick, Lettau, and Wascher 2016) and centropy toward equity in base pay for new hires (Bewley 1995), the shock might be most apparent in variable pay, the more flexible component of employee compensation (Sockin and Sockin 2019; Grigsby, Hurst, and Yildirmaz 2021). In this section, we use wage data from Glassdoor to test the competing hypotheses.

3.4.1 Base and Variable Pay

Summary statistics for the sample of 5.2 million Glassdoor pay reports are displayed in Table 3.6. We present statistics separately for scandal-hit and control firms (i.e., the 41,321 large employers that faced neither a corporate scandal nor a data breach). While demographic information is similar across samples, employees at scandal-hit firms earn an average of $88,900 in base wages, which is $20,000 more than those at control firms. A key advantage of the Glassdoor data is that earnings are broken down into base and variable components. Scandal-hit firms are 12 percentage points more likely to compensate their employees with variable pay than control firms. Among workers that earn variable pay, those at scandal hit firms receive $15,600 more on average than their peers at control employers. In our regression specifications, we control for a rich set of observables, including firm and job title, to account for these differences.

To explore heterogeneity within firms, we classify workers as low (high) experience if they have at most (more than) three years of experience. Across samples, low-experience workers are about eight years younger, twenty percentage points less likely to receive variable pay, and earn 38 percent less in base wages than their more experienced peers. Conditional on earning variable pay, high-experience workers receive on average 2.5x–3x more than their low-experience colleagues. Demographics are similar within groups across samples, but employees at scandal-hit firms are still paid appreciably more. We also partition employees based on their standing in the corporate hierarchy, as this assignment may better capture the allocation of variable pay (Sockin and Sockin 2019). We define junior (senior) positions as industry and job title pairs for which the median years of work experience is at most (more than) three years.[9] As evidenced by the final row of Table 3.6, there is commonality between low-experience and junior workers, but the classifications do not perfectly overlap.

Table 3.6: Summary Statistics for Wages Sample

	Corporate scandals			Control firms		
	All	Low Experience	High Experience	All	Low Experience	High Experience
Sample size (1000s)	82	40	42	5,151	2,365	2,786
Base pay ($1000s)	88.9	68.9	107.9	69.0	52.9	82.7
	(56.4)	(43.8)	(60.3)	(42.5)	(29.5)	(46.9)
Variable pay ($1000s)	21.5	9.9	32.5	5.9	3.0	8.5
	(54.5)	(29.9)	(68.6)	(22.7)	(13.2)	(28.1)
Earns variable pay (%)	40.5	30.5	50.1	28.3	21.6	33.9
Age (years)	31.8	28.2	35.5	33.3	29.0	37.5
Years of experience	5.6	1.6	9.5	6.4	1.5	10.5
Salaried (%)	74.9	64.4	84.8	73.1	63.9	80.9
Junior position (%)	55.5	74.9	36.2	48.0	68.7	30.2

Notes: This table presents summary statistics from the pay reports data for scandal-hit and control firms. Each entry reflects the within-sample mean. Base and variable pay are inflation-adjusted using U.S. headline CPI to 2018 dollars, and their standard errors are reported in parentheses. Variable pay mean and standard error are conditional among those who earn it. Low and high experience employees have at most or no more than three years of experience, respectively. Junior positions are industry-job titles for which the median years of work experience is at most three years.

To formally test how firms alter worker compensation in the wake of a scandal, we again implement a generalized difference-in-differences framework. The benchmark

9. There are 21 industries included in our sample: Accounting & Legal, Aerospace & Defense, Arts, Entertainment & Recreation, Biotech & Pharmaceuticals, Business Services, Construction, Consumer Services, Education, Finance, Government, Health Care, Information Technology, Insurance, Manufacturing, Media, Non-Profit, Energy, Real Estate, Retail, Telecommunications, Transportation, and Travel. To limit measurement error, industry-job title pairs with fewer than 20 pay reports are omitted.

regression specification is

$$w_{ijkmt} = \beta \cdot PostScandal_{kt} + \lambda X_{it} + \gamma_k + \gamma_{\iota(k)jt} + \gamma_{mt} + \epsilon_{ijkmt} \tag{3.2}$$

where w_{ijkmt} is a pay measure for worker i with job title j employed at firm k in metro m for year t, γ_k is a firm fixed effect, $\gamma_{\iota(k)jt}$ is an industry-job title-year fixed effect, γ_{mt} is a metro-year fixed effect, X_{it} is a vector of individual controls that includes years of experience squared and gender, and $PostScandal_{kt}$ is an indicator equal to one if firm k faced a scandal prior to year t.[10] Standard errors are clustered by firm. By incorporating firm, industry-job title-year, and metro-year fixed effects, our coefficient of interest, β, captures the change in pay for employees at scandal-hit firms in the years following an event relative to employees in similar roles at peer firms, accounting for trends over time in local labor markets. The rich set of control variables ensures that we recover tightly-identified estimates.

Results for the full sample of corporate scandals are presented in the first three columns of Table 3.7. Column 1 shows that base pay is unchanged following a scandal, which we view as evidence in support of downward nominal wage rigidity (Fallick, Lettau, and Wascher 2016). We note that a scandal-hit firm's total labor bill for base wages may not remain unchanged, however, as they could respond by laying off workers. Column 2 reveals that variable pay, on the other hand, *is* affected by corporate misconduct. Employees at scandal-hit firms see their variable wages fall

10. Because employee pay is reported by calendar year, we are unable to determine exactly when a worker's base and variable compensation is set. We therefore exclude the year of the scandal from the analysis. Metro areas in Glassdoor data correspond roughly to core-based statistical areas (CBSAs). There are 858 unique metros in the Glassdoor pay data and 929 CBSAs.

10.2 percent (10.8 log points), on average, relative to their peers at control firms.[11] This finding affirms that variable compensation acts as a mechanism for employers to pass firm-level shocks on to workers. In Column 3, we estimate Equation 3.2 using an indicator equal to one if the worker earns variable pay as the dependent variable. The marginally significant negative coefficient suggests that firms may also shrink the set of workers who receive variable pay after a scandal occurs.

Table 3.7: Difference-in-Differences Results for Employee Pay

	Full sample			Non-fraud sample		
	Base pay	Variable pay	Earns VP	Base pay	Variable pay	Earns VP
After scandal	-0.014	-0.108***	-0.054*	-0.023	-0.134***	-0.068*
	(0.013)	(0.035)	(0.031)	(0.016)	(0.039)	(0.038)
Pre-scandal mean	11.09	9.85	0.49	11.25	10.47	0.46
N	4435101	1160649	4683995	4414951	1152476	4662111
Scandal firm N	63615	24228	67866	44231	16468	46779
Adjusted R^2	0.83	0.64	0.32	0.83	0.64	0.32

Notes: This table reports coefficients for corporate scandals and data breaches estimated from Equation 3.2 on the dependent variable listed in each column. The pre- and post-periods are four and three years, respectively, and the year of the scandal is excluded. Regressions include years of experience squared along with firm, industry-job title x year, metro x year, gender, and pay frequency fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

To demonstrate that instances of fraud do not drive our findings, we re-estimate Equation 3.2 for each of the three compensation metrics using only "non-fraud" scandals. The results, presented in the last three columns of Table 3.7, are similar to those from the full sample. Base pay remains unchanged in the post-scandal

11. The decrease is not inconsistent with the null result on employees' ratings of compensation and benefits from Table 3.3, as there is no decline in base wages and not all employees earn variable income.

period, the estimate for variable pay is negative and significant at –12.5 percent (–13.4 log points), and there is a marginally significant 6.8 percentage point decline in the probability of earning variable pay. We again conduct a series of untabulated robustness tests. As with employer ratings, we consider an alternate control sample using all firms, re-weight observations to account for changes in sample composition across firms over time, stack regressions to avoid possible bias from the staggered timing of events, and estimate "leave-one-out" regressions by iteratively excluding one firm from the scandal sample to ensure our findings are not driven by a single event. In all cases, the results are similar to those obtained under the baseline specifications.

We next return to the full sample of corporate scandals and re-estimate Equation 3.2 separately for low- and high-experience employees. Columns 1 and 2 of Table 3.8 indicate that base pay for both groups is unaffected by a scandal. Column 3 reveals that for employees with at most three years of experience, variable pay drops by 16.8 percent (18.3 log points) following an event, which amounts to roughly $1,700 per year. For high-experience employees (Column 4), the decline in variable pay is smaller in percentage-point terms, but amounts to about $2,500 dollars. Estimates are similar when we separate employees by job hierarchy rather than years of experience, which confirms that declines in variable pay are felt throughout the firm.

Table 3.8: Difference-in-Differences Results for Low- and High-Level Employee Compensation

	Work experience				Job hierarchy			
	Base pay		Variable pay		Base pay		Variable pay	
	Low	High	Low	High	Junior	Senior	Junior	Senior
After scandal	-0.010	-0.017	-0.183***	-0.081**	-0.017*	-0.011	-0.140***	-0.090**
	(0.009)	(0.012)	(0.041)	(0.036)	(0.010)	(0.019)	(0.043)	(0.044)
Pre-scandal mean	10.83	11.36	9.29	10.24	10.83	11.42	9.36	10.25
N	1968759	2276654	377385	715761	1952238	2096411	391712	698006
Scandal firm N	30417	31098	8515	14625	33183	26264	10355	12538
Adjusted R^2	0.81	0.82	0.62	0.63	0.81	0.80	0.63	0.63

Notes: This table reports coefficients when Equation 3.2 is estimated separately for low and high experience employees, or junior and senior positions. The pre- and post-periods are four and three years, respectively, where the year of the scandal is excluded. Low and high experience employees have at most or no more than three years of experience, respectively. Junior and senior positions are industry-job title pairs for which the median years of experience is at most or more than three years, respectively. Regressions include a quadratic in years of experience along with firm, industry-job title x year, metro x year, gender, and pay frequency fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

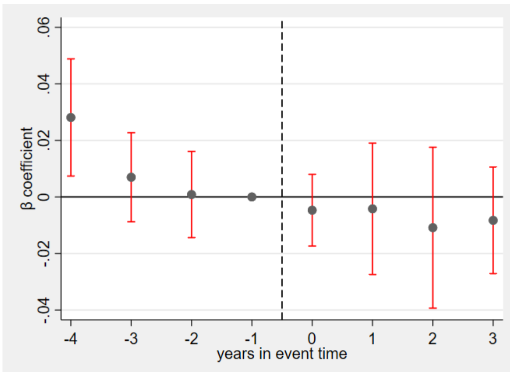
To explore how the effects of scandals on compensation vary over time, we re-estimate Equation 3.2, but allow the coefficient on the scandal indicator to vary by event-year. The top panels of Figure 3.2 reaffirm that, for all employees, base pay remains unchanged while variable pay declines in the aftermath of a scandal. The middle-left panel reveals that the reduction in variable pay for less experienced employees is large and statistically significant throughout the post-event window. The coefficient estimates for high-experience workers are negative and stable, but lack statistical significance. Estimates from the pre-event periods suggest that our findings are not driven by pre-trends.

The reduction in variable wages could emerge for several reasons. One possibility is that firms look to offset costs associated with a scandal, such as fines or increased spending on corporate social responsibility (Akey et al. 2021). An alternative explanation is that firm productivity falls after a scandal. The loss of reputation stemming from misconduct may, for example, hurt sales by eroding relationships with clients (Hoffmann and Müller 2009; Schramm-Klein et al. 2016). Labor productivity may also decrease if more productive workers exit the firm or employees exert less effort due to lower job satisfaction (McGregor 1960). In Appendix 3.C, we present evidence consistent with a reduction in productivity. Using sales and employment data from Compusat, we show that a firm's average labor productivity, defined as sales per employee (Cronqvist et al. 2009), falls 9 percent on average following a scandal.

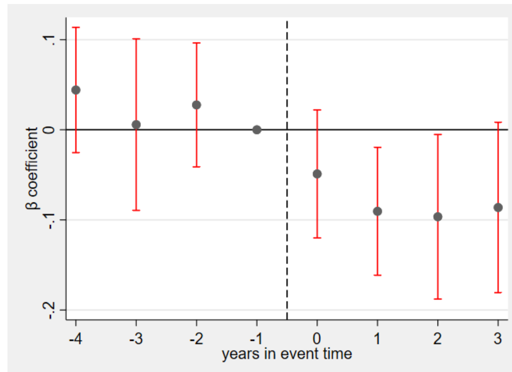
3.4.2 Fringe Benefits

Our findings thus far have shown that the losses in amenity value borne by employees at scandal-hit firms are not offset by increased wages. In fact, for workers who receive

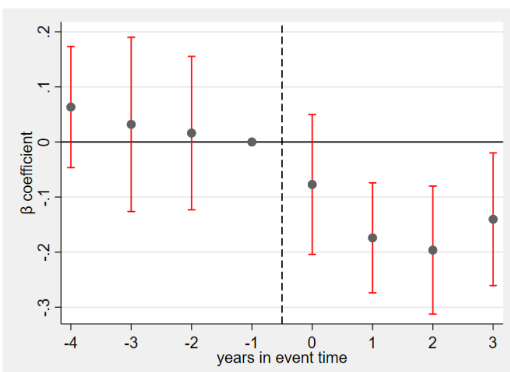
Figure 3.2: Compensation after Corporate Scandals, Dynamic Results



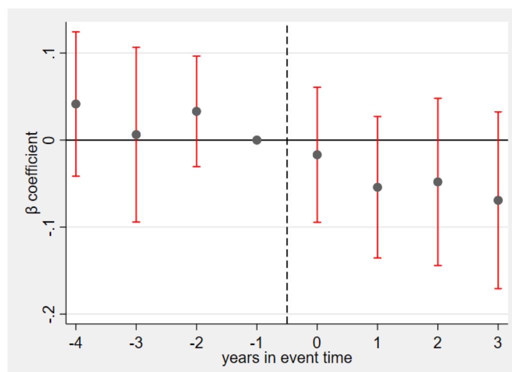
(a) Base pay



(b) Variable pay



(c) Variable pay, low experience



(d) Variable pay, high experience

Notes: Each panel displays coefficients estimated for log variable or base pay within an eight-year window around event dates. Low and high experience employees have at most or no more than three years of experience, respectively. Junior and senior positions are industry-job title pairs for which the median years of experience is at most or more than three years, respectively. Regressions include a quadratic in years of experience along with firm, industry-job title x year, metro x year, gender, and pay frequency fixed effects. Horizontal dashes indicate a 95 percent confidence interval around each point estimate. Each coefficient is relative to one year prior to each event. Standard errors are clustered by firm.

variable pay, we observe a sizable reduction in earnings. An alternative channel through which employers may provide additional compensation is an improvement in fringe benefits such as health insurance, retirement contributions, and paid time off. Enhancing benefits could be more appealing to employers than raising wages if the marginal cost of benefit provision is lower (Rosen 1986).

Though we cannot directly observe benefits, we are able to test for changes in employees' Glassdoor ratings of their firms' benefits packages. The mean overall benefits rating for scandal-hit employers is 4.14 stars with a standard deviation of 1.07. For the control group, the average rating is lower at 3.69 stars and has a standard deviation of 1.22. If employers improve benefits in response to a scandal, then ratings should increase in the post-event period. To formally test this possibility, we again employ a difference-in-differences framework. The regression equation is

$$r_{ikt} = \beta \cdot PostScandal_{kt} + \lambda X_{it} + \gamma_k + \gamma_{\iota(k)t} + \epsilon_{ikt} \tag{3.3}$$

where r_{ikt} is the benefits rating from worker i employed at firm k in year t, γ_k is a firm fixed effect, $\gamma_{\iota(k)t}$ is an industry-year fixed effect, X_{it} is a vector of individual controls including employment status and current employee indicators, and $PostScandal_{kt}$ is an indicator equal to one if firm k faced a scandal prior to or during year t. The control group is comprised of 26,182 large firms that experienced neither a scandal nor a breach from 2013 through 2018.

The full sample estimate, presented in Table 3.9, is not statistically different from zero. We also find no effect for the set of non-fraud scandals. The null result suggests that firms do not respond to scandals by improving fringe benefits.[12]

As the declines in amenity value are not offset by improvements to wages or benefits, we conclude that workers at scandal-hit employers are left strictly worse off. It follows that firms may face difficulties retaining and hiring employees in the wake of such events. Though Glassdoor data do not allow us to test the first hypothesis, Choi and Gipper 2021 show that employee separation rates rise after periods of fraudulent financial reporting. In Appendix 3.D, we present evidence that scandals also impair a firm's ability to attract new hires. We find job seekers on Glassdoor are 17–28 percent less likely to apply to a firm's job listings in the six months following a scandal. Thereafter, application rates return to their pre-event levels. As Glassdoor is not primarily a job board and the application data only span from March 2017 through August 2019, we consider this a preliminary but suggestive finding that job seekers respond to corporate misconduct.

3.5 Conclusion

In this paper, we explore how corporate scandals affect the relationship between firms and their employees. Using data from the website Glassdoor, we find that scandals have both immediate and longer-term negative effects. Employee sentiment experiences a sharp, lasting decline, driven primarily by diminished perceptions of

12. In addition to rating an employer's overall benefits package, workers may separately rate fifty-four distinct fringe benefits. Our conclusions are unchanged if, as in Liu et al. 2021, we estimate regressions including the full set of benefit-specific ratings instead of only overall ratings.

Table 3.9: Difference-in-Difference Results for Ratings of Overall Benefits

	Full sample	Non-fraud sample
After scandal	-0.039	-0.076
	(0.055)	(0.071)
Pre-event mean	4.12	4.24
N	801591	797480
Scandal firm N	12750	8639
Adjusted R^2	0.25	0.25

Notes: This table reports coefficients for corporate scandals and data breaches estimated from Equation 3.3 on employee ratings of employers' overall benefits. The pre- and post-periods are each three years, respectively. Regressions include firm, industry x year, current employee, and employment status fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

senior management and firm culture. Further, workers at scandal-hit firms are less optimistic about their employers' business outlooks, less willing to recommend their employers to members of their social networks, and less approving of their employers' chief executives. We find that the deterioration in worker sentiment is not offset by improvements to wages or fringe benefits. On the contrary, both the prevalence and magnitude of variable pay fall after a corporate scandal. These results highlight that workers are differentially exposed to negative firm-level shocks, as those who receive variable wages are more likely to see their income decline.

Since employees receive no compensation, pecuniary or otherwise, to offset the losses in job satisfaction and remuneration, we conclude that they are left strictly worse off. While misconduct is often precipitated by firm culture or carried out directly by executives, our paper demonstrates that rank-and-file workers still bear significant adverse consequences from corporate misdeeds.

APPENDICES

3.A Scandal Information

In this appendix, we further describe the construction of our corporate scandals sample. We first aggregate annual lists of "corporate misdeeds" published by Fortune Magazine and Yahoo! Finance. We are unable to locate such a list for the year 2014, so we utilize a list published by Inc. Magazine instead. We supplement these events with allegations of sexual harassment levied against corporate CEOs and chairmen documented by the New York Times in 2018. We then apply a set of filters to ensure that our scandals are legitimate shocks to firm reputation. We exclude events that do not involve any wrongdoing (e.g., the relationship between Sergei Brin and a Google employee), impact several firms, or are continuations of prior events (e.g., J.P. Morgan paying fines for past transgressions). When a firm appears on multiple lists, we use the first event that satisfies the aforementioned criteria. While the persistence of our results may be partially driven by this restriction, the majority of firms in our sample only suffer a single scandal. Furthermore, subsequent events are often directly related to the first event or come to light precisely because of the increased scrutiny brought about by the initial scandal. Finally, we exclude events involving firms that do not have an appreciable presence in the Glassdoor data. Table 3.10 provides links to news articles summarizing each event as well as the underlying list from which each event was pulled.

Table 3.10: Corporate Scandal Background Information

Company	Date Public	Background Article	Source
Carnival Cruise	02-10-2013	Passengers Face... Squalor on Carnival Ship Stranded in the Gulf	Fortune
lululemon	03-18-2013	Recall Is Expensive Setback for Maker of Yoga Pants	Fortune
GlaxoSmithKline	07-11-2013	GlaxoSmithKline Accused of Corruption by China	Fortune
Macy's	10-24-2013	Profiling Complaints... Followed Changes to Stores' Security Policies	Fortune
Sony	12-02-2014	Sony Films Are Pirated, and Hackers Leak Studio Salaries	Inc.
Toshiba	07-20-2015	Scandal Upends Toshiba's Lauded Reputation	Fortune
Volkswagen	09-18-2015	VW Is Said to Cheat on Diesel Emissions; U.S. to Order Big Recall	Fortune
Valeant	10-21-2015	Valeant's Shares Fall on Report's Fraud Claim	Fortune
Fox	07-06-2016	Gretchen Carlson... Files Harassment Suit Against Roger Ailes	Fortune
Mylan Inc	08-18-2016	Mylan Raised EpiPen's Price....	Fortune
Samsung	09-02-2016	Samsung to Recall 2.5 Million Galaxy Note 7s Over Battery Fires	Fortune
Wells Fargo	09-08-2016	Wells Fargo Fined $185 Million for Fraudulently Opening Accounts	Fortune
Uber	02-19-2017	Uber Investigating Sexual Harassment Claims by Ex-Employee	Fortune
United Airlines	04-10-2017	United Airlines Passenger Is Dragged From an Overbooked Flight	Fortune
Equifax	09-07-2017	Equifax Says Cyberattack May Have Affected 143 Million in the U.S.	Fortune
Apple	12-20-2017	Is Apple Slowing Down Old iPhones? Questions and Answers	Fortune
Wynn Resorts	01-25-2018	Stephen Wynn... Accused of Decades of Sexual Misconduct	New York Times
Guess?	02-01-2018	Guess Inc. responds to sexual harassment allegations....	New York Times
Google	03-06-2018	Google Is Helping the Pentagon Build AI for Drones	Yahoo! Finance
Facebook	03-15-2018	Facebook's Role in Data Misuse Sets Off Storms on Two Continents	Yahoo! Finance
CBS	07-27-2018	Les Moonves and CBS Face Allegations of Sexual Misconduct	New York Times
Tesla	08-07-2018	Elon Musk Says Tesla May Go Private, and Its Stock Soars	Yahoo! Finance
Nissan	11-19-2018	Nissan Chairman... Arrested Over Financial Misconduct Allegations	Yahoo! Finance

Notes: This table provides additional information on the corporate scandals in our sample. It includes links to articles from the popular press detailing each event and the list from which each event was pulled.

3.B Elevated News Coverage

In this appendix, we demonstrate that elevated news coverage does not explain our results. To do so, we contrast the effects of scandals with those of data breaches, another type of negative, firm-specific shock that generates increased media attention. While breaches may reflect poorly on firm management, they do not necessarily involve misconduct or reveal information about firm culture. Our sample of such events comes from Privacy Rights Clearinghouse, which defines a data breach as a "security violation in which sensitive, protected or confidential data is copied, transmitted, viewed, stolen or used by an unauthorized individual." We restrict attention to firms that experienced a loss of at least one million records between 2013 and 2018 and have appreciable coverage on Glassdoor. The 27 breaches that meet the criteria are listed in Table 3.11.[13]

We use data from Google Trends to validate that both scandals and breaches generate heightened news coverage. The web application provides daily scores reflecting the relative search intensity for particular keywords within a specified date window. Scores range from 0 to 100, with 100 representing the peak intensity over the period of interest. For each scandal-hit and breach-hit firm in our sample, we pull the news category trend score for a 60-day window centered around the event date. The average scores across firms for each event type are plotted in Figure 3.3. The large jumps when scandals and breaches become public confirm that both are salient, unanticipated shocks that garner media attention.

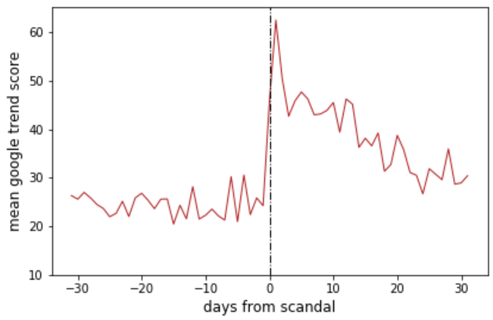
13. Two of our scandals are, in fact, data breaches. The Equifax hack was particularly damaging to firm reputation due to its magnitude and because it was covered up by executives for several months. The Sony breach was harmful despite the loss of comparatively few records, because hackers leaked compromising emails written by executives.

Table 3.11: Summary of Breach Employer Samples

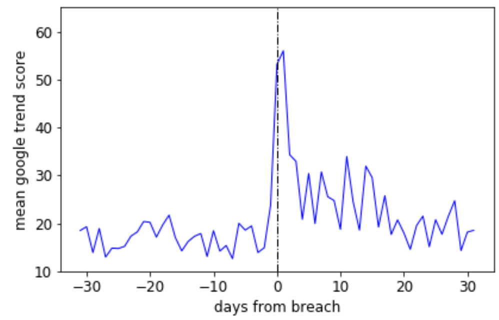
Event date	Employer	Records Lost (MM)	CEO exits
October 04, 2013	Adobe	2.9	N
December 13, 2013	Target	40.0	05/05/2014
January 10, 2014	Neiman Marcus	1.1	N
January 25, 2014	Michaels	2.6	N
May 21, 2014	eBay	145.0	N
August 18, 2014	Community Health Systems	4.5	N
August 28, 2014	J.P. Morgan	76.0	N
September 02, 2014	The Home Depot	56.0	N
October 20, 2014	Staples	1.2	N
February 05, 2015	Anthem	80.0	N
March 17, 2015	Premera Blue Cross	11.0	N
May 20, 2015	Carefirst	1.1	N
July 17, 2015	UCLA Health	4.5	N
October 01, 2015	Experian	15.0	N
May 17, 2016	LinkedIn	117.0	N
June 13, 2016	Twitter	32.0	N
August 03, 2016	Banner Health	3.6	N
September 22, 2016	Yahoo!	500.0	N
September 07, 2017	Equifax	145.5	09/26/2017
October 12, 2017	T-Mobile	69.6	N
March 30, 2018	Under Armour	150.0	N
April 01, 2018	Lord & Taylor's	5.0	N
April 01, 2018	Saks	5.0	N
April 20, 2018	SunTrust	1.5	N
June 28, 2018	adidas	2.0	N
October 01, 2018	Chegg	40.0	N
November 30, 2018	Marriott International	327.0	N

Notes: This table describes each of the data breaches in the sample. The list consists of hacks from Privacy Rights Clearinghouse that involved at least one million individual records.

Figure 3.3: Google Trends Scores around Event Dates



Corporate Scandals



Data Breaches

Notes: These figures display equally-weighted average Google Trends news scores for firm names in the 60-days around scandal and breach dates. Google Trends scores are normalized to range from 0 to 100, with 100 representing the peak intensity over the queried time period.

To determine the effects of data breaches on employees, we re-estimate the benchmark job satisfaction and wage regressions with the set of breach-hit firms as the treated group. Table 3.12 reports the results for our focal outcome measures. While negative, the coefficient estimates on the employee review variables are small and insignificant, suggesting that negative publicity does not necessarily cause declines in employee sentiment. Further, we find no evidence that variable compensation falls following a data breach. We interpret these results as evidence that impropriety, not news coverage, drives our results.

Table 3.12: Difference-in-Difference Results for Data Breaches

	Employee reviews				Employee wages			
	Overall rating	Culture & values	Senior mgmt.	Would refer a friend	Base pay	Variable pay	Earns VP	Benefits rating
After breach	-0.043	-0.035	-0.055	-0.017	-0.001	-0.023	0.006	-0.021
	(0.029)	(0.037)	(0.035)	(0.013)	(0.009)	(0.036)	(0.007)	(0.027)
Pre-breach mean	3.45	3.50	3.01	0.66	10.89	9.28	0.49	3.94
N	4879723	4119107	4083763	3847073	4424839	1155908	4674366	794574
Breach firm N	53926	47092	46774	44487	53348	19584	58240	5733
Adjusted R^2	0.17	0.17	0.15	0.14	0.83	0.64	0.32	0.25

Notes: This table reports coefficients when Equations 3.1 and 3.2 are estimated on the sample of data breaches. The control group and control variables are the same as in the benchmark regressions for corporate scandals. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

3.C Average Labor Productivity

In this appendix, we study if firm-level labor productivity drops as a result of corporate scandals. Such a decline may help explain why workers earn less in variable compensation after misconduct comes to light. Following Cronqvist et al. 2009, we measure average labor productivity as the natural logarithm of sales per employee using data from Compustat. We formally test for changes by estimating the difference-in-differences regression

$$y_{kt} = \beta \cdot PostScandal_{kt} + \lambda J_{kt} + \gamma_k + \gamma_{\iota(k)t} + \epsilon_{kt} \tag{3.4}$$

where y_{kt} is average labor productivity for firm k in calendar quarter t, γ_k is a firm fixed effect, $\gamma_{\iota(k)t}$ is an industry-quarter fixed effect, J_{kt} is the firm's log assets, and $PostScandal_{kt}$ is an indicator equal to one if firm k faced a scandal prior to quarter t. The control group is comprised of large employers in Compustat that experienced neither a scandal nor a data breach. For consistency with our pay regressions, the event window spans from 16 quarters prior to 16 quarters after a scandal. We also test for changes in the number of total workers employed at the firm, but this measure is only available at an annual frequency.

The results are presented in Table 3.13. The estimate in Column 1 indicates that average labor productivity falls 9 percent following a scandal, though the coefficient is statistically significant only at the 10 percent level. The magnitude of the decline is similar when we consider only "non-fraud" scandals, but the coefficient is significant at the 5 percent level. We find no evidence that firms shrink their workforce in the wake of a scandal. The results are consistent with our hypothesis that the decline in labor productivity may help explain the reduction in variable wages.

Table 3.13: Firm-Level Outcomes Following Scandals

	Full sample		Non-fraud sample	
	Log sales per worker	Log employment	Log sales per worker	Log employment
After scandal	-0.086*	0.104	-0.092**	0.124
	(0.047)	(0.076)	(0.047)	(0.093)
Pre-scandal mean	11.55	10.69	11.59	10.42
Dependent var. mean	11.30	6.45	11.30	6.45
Dependent var. std. dev.	1.34	2.72	1.34	2.72
Control firms	10426	10522	10426	10522
N	275765	77862	275583	77816
Scandal firm N	553	138	371	92
Adjusted R^2	0.83	0.98	0.83	0.98

Notes: This table reports coefficients when Equation 3.4 is estimated on the dependent variable in each column heading. The event window spans from 16 quarters prior to 16 quarters after a scandal. The log sales-per-worker regressions are at the firm-quarter level, while the log employment regressions are at the firm-year level. The independent variables are log assets and fixed effects for firm and industry–year, where industry is based on six-digit GICS codes. The scandal-hit firms included in this analysis are: Apple, CBS, Carnival, Equifax, Fox, Facebook, GlaxoSmithKline, Google, Guess?, Macy's, Mylan Inc, Nissan, Sony, Tesla, Toshiba, Valeant, Wells Fargo, Wynn Resorts, and lululemon. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

3.D Job Seeker Behavior

In this appendix, we study how a corporate scandal affects a firm's ability to attract job seekers. In addition to submitting reviews of employers and pay reports, users on Glassdoor can search for and apply to jobs. Glassdoor aggregates job postings from online job boards, applicant tracking systems, and company websites, capturing about 81 percent of total U.S. job openings as measured by the Job Openings and Labor Turnover Survey (Chamberlain and Zhao 2019). The website's platform presents job seekers with a list of job openings based on their search criteria. A job posting being displayed to a user constitutes an *impression* for that listing, regardless of whether the user chooses to explore it further. If the user ultimately begins an application for the vacancy, this constitutes an *apply*. We do not observe whether users complete the applications they start. Our principal measure of interest is the application rate, which we define as applies per 100 impressions.

For each job listing, we calculate the total impressions and applies over a 72-hour window from when the posting is first listed on Glassdoor. Each user contributes at most once to a job listing's totals.[14] We restrict our sample to active job postings by considering only listings with impressions from at least five unique jobseekers. We also exclude postings for which the metropolitan location of the vacancy is unavailable and those made by firms with fewer than ten employer reviews prior to the listing date. After imposing these filters, our sample consists of 9.60 million listings from large employers. Search data are available from March 2017 through August 2019, which limits our treatment sample to eight scandals.[15]

14. A user is a jobseeker with a registered profile, and can thus be identified across sessions. We impose the 72-hour window in order to mitigate trends in search intensity over the life cycle of a vacancy. For certain days, we are unable to calculate totals over a 72-hour window and must resort to either a 48-hour or 24-hour window. We incorporate day-of-posting fixed effects in our regressions in part to account for this issue.

15. The eight firms are: Apple (5289 ; 259,000), CBS (6324 ; 265,000), Equifax (2,342 ; 71,000), Facebook (1517 ; 99,000), Google (3,617 ; 246,000), Guess? (2,843 ; 60,000), Tesla (6957 ; 203,000), and Wynn Resorts (641 ; 14,000). The figures in parentheses are the total job listings and unique impressions for each firm in the period four months prior to twelve months after a scandal, respectively.

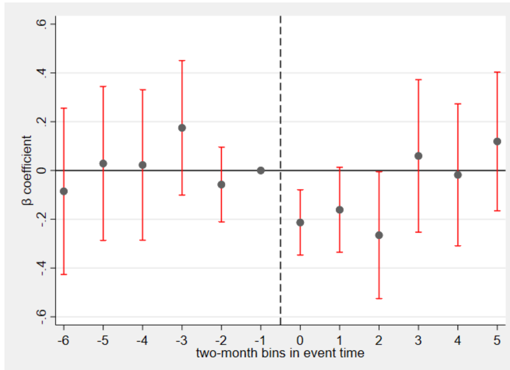
To test whether jobseeker behavior responds to a scandal, we estimate the following difference-in-differences regression

$$r_{ijkmt} = \sum_{\tau \neq -1} \beta_\tau \cdot ScandalFirm_{k\{t \in \tau\}} + \gamma_k + \gamma_{\iota(k)jq(t)} + \gamma_{mq(t)} + \gamma_t + \epsilon_{ijkmt} \qquad (3.5)$$

where r_{ijkmt} is the apply rate for job listing i advertising job title j at firm k in metro m that is posted on calendar date t, γ_k is a firm fixed effect, $\gamma_{\iota(k)jq(t)}$ is an industry-job title-quarter fixed effect, $\gamma_{mq(t)}$ is a metro-quarter fixed effect, γ_t is a date-of-posting fixed effect, $ScandalFirm_{k\{t \in \tau\}}$ is an indicator equal to one if firm k experienced a scandal and calendar date t is in firm k's event-time bin τ. The τ subscripts correspond to two-month bins beginning twelve months prior and ending twelve months after a scandal. The β_τ coefficients therefore measure differences in apply rates relative to the omitted period, which consists of the two calendar months immediately preceding an event. In order to avoid undue influence from less visible postings, we weight job listings by their impression counts. The set of control firms consists of the 17,969 large employers with job postings on Glassdoor that faced neither a corporate scandal nor a data breach. If scandals reduce interest in firms, then the β_τ coefficients will be negative in the periods following an event.

Figure 3.4 shows that application rates are stable in the months leading up to a scandal, then fall in the immediate aftermath of an event. The point estimate of –0.213 for the two-month period including the event month represents a 23 percent decrease relative to the mean rate of 0.93 applies per 100 impressions in the reference period. The drop is substantial, but the coefficients recover to their pre-scandal levels after six months. While suggestive, our findings should be taken with caution. Due to data limitations, we are only able to study a limited set of scandals. Further, job search is not typically the primary function associated with Glassdoor. We consider the relationship between misconduct and job seeker behavior to be an interesting topic for further research.

Figure 3.4: Application Rates after Corporate Scandals, Dynamic Results



Notes: The panels above display coefficients from Equation 3.5 for click rate and apply rate within a 24-month window around scandals. Horizontal dashes indicate a 95 percent confidence interval around each point estimate. Regressions are weighted by each job posting's impression total and include firm, industry x job title x quarter, metro x quarter, and date-of-posting fixed effects. Each coefficient is relative to the two months prior to the scandal. Standard errors are clustered by firm.

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