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Did CDS trading improve the market for corporate bonds? ☆

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ABSTRACT

Financial innovation through the creation of new markets and securities impacts related markets as well, changing their efficiency, quality (pricing error), and liquidity. The credit default swap (CDS) market was undoubtedly one of the salient new markets of the past decade. In this paper we examine whether the advent of CDS trading was beneficial to the underlying secondary market for corporate bonds. We employ econometric specifications that account for information across CDS, bond, equity, and volatility markets. We also develop a novel methodology to utilize all observations in our data set even when continuous daily trading is not evidenced, because bonds trade much less frequently than equities. Using an extensive sample of CDS and bond trades over 2002–2008, we find that the advent of CDS was largely detrimental. Bond markets became less efficient, evidenced no reduction in pricing errors, and experienced no improvement in liquidity. These findings are robust to various slices of the data set and specifications of our tests.

1. Introduction

A major innovation in the fixed-income and credit markets since the turn of the century is the introduction of the credit default swap (CDS), a credit insurance contract with a payoff linked to that of the default or change in credit characteristics of an underlying reference bond or issuer. Innovation, however, is a double-edged sword with likely mixed positive and negative outcomes. The creation of new securities could complete markets and favorably impact information generation and dissemination, as well as liquidity, yet, such innovations could also have negative externalities if the gains accrue to only a few market participants and cause an adverse impact on the rest of the market. In this paper we examine whether the advent of the CDS market improved the secondary corporate bond market in terms of its underlying efficiency, market quality, and liquidity.[1] Taking a time series perspective, we examine the following question: did an issuer's bonds become more efficient and liquid after CDS trading was instituted on the reference instruments of the issuer? From a cross sectional perspective, we query: Are bonds

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[1] The CDS market is over-the-counter over the period of this study and, hence, decentralized. CDS introduction is initiated by dealer banks depending on factors such as size of outstanding debt of an issuer, underlying credit risk of the issuer, and demand for credit protection. More recently, CDS contracts are exchange-traded on a centralized clearing system. In contrast, most equity options are exchange-traded. Hence, the introduction of an equity option is decided by the corresponding options exchange depending upon factors such as trading volume, market capitalization, and turnover of the underlying stock.

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of firms with traded CDS contracts more efficient and liquid than bonds of firms without any CDS contracts?

Did corporate bond trading decline after the introduction of CDSs because traders were able to implement a credit view better and more cheaply in the CDS markets? Fig. 1 shows the mean size of bond trades relative to the date of inception of CDS trading for our sample of firms with traded CDS contracts benchmarked to a control sample of firms with no CDS introduction. The mean trade size falls in the two years following CDS introduction, indicating an evident decline in secondary bond market activity. Similarly, Fig. 2 depicts a likely drop in mean turnover of bonds of issuers with CDS contracts once CDS trading begins, with no appreciable change for control sample bonds.

Figs. 1 and 2 indicate that bond trading could have declined, but it is likely that bond market efficiency improved if the CDS market generated useful information that was quickly reflected in bond prices. As our empirics show, relative to other asset classes the informational efficiency of corporate bonds is poor both before and after the advent of CDS trading, and interestingly, bonds become more inefficient after CDS trading commences. This suggests that the CDS markets had a detrimental effect on bond market efficiency. Bond market quality showed no signs of improvement after CDS introduction. Also, using various measures of liquidity we find that post-CDS, on a relative basis, more liquidity attributes deteriorated than improved.

The prior literature on bond market efficiency examines lead–lag relations between corporate bonds and equity markets as a way of assessing the relative efficiency of bonds to equity (e.g., Kwan, 1996; Hotchkiss and Ronen, 2002; Downing, Underwood, and Xing, 2009; Ronen and Zhou, 2013). The findings are mixed. Our goal in this paper is different from that of the prior literature. Whereas we

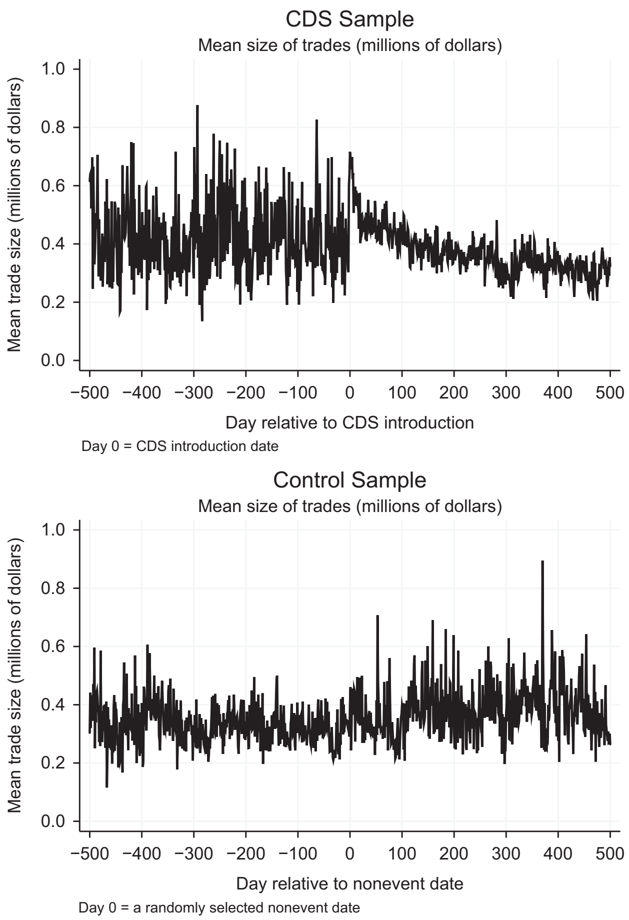

Fig. 1. Mean size of bond trades before and after introduction of credit default swaps (CDS).
The upper plot shows the average size of each bond transaction (in millions of dollars) on a daily basis over periods of 500 trading days (two years) before and after the introduction of CDSs for the sample of CDS issuers, and the lower plot depicts the same for a pooled control sample of CDS nonissuers. The control sample includes all bond issues by firms that meet the selection criteria outlined in Appendix A but did not issue any CDSs until the end of 2009. The plots are based on data organized as continuous time series in which zero trade days are included. Panel A of Table 7 reports trade volume based on discrete panel data that exclude zero trade days.

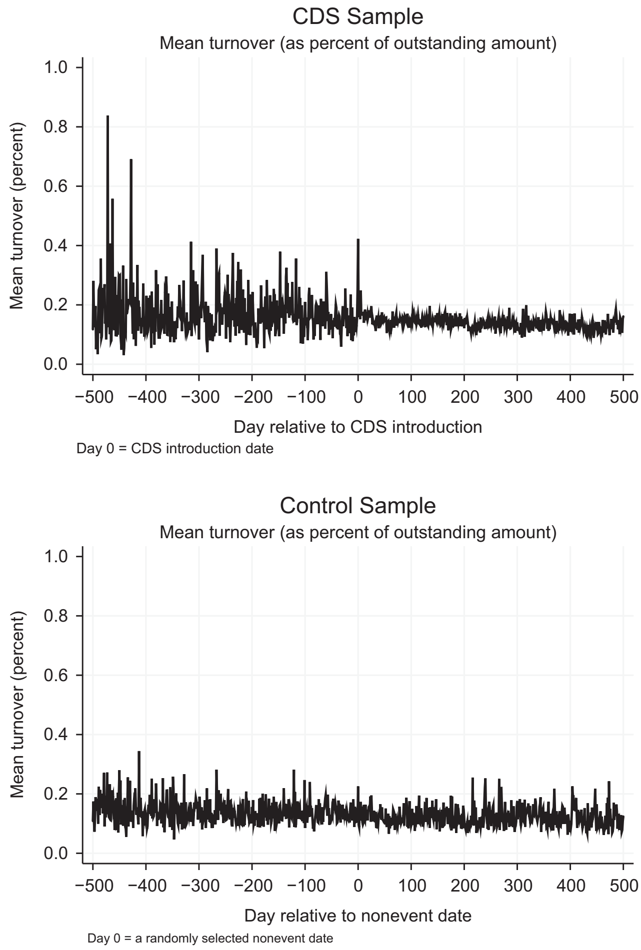

Fig. 2. Mean bond turnover before and after introduction of credit default swaps (CDS).
The upper plot shows the average turnover for each bond transaction (volume as a percent of total amount outstanding) on a daily basis over periods of 500 trading days (two years) before and after the introduction of CDSs for the sample of CDS issuers, and the lower plot depicts the same for a pooled control sample of CDS nonissuers. The control sample includes all bond issues by firms that meet the selection criteria outlined in Appendix A but did not issue any CDSs until the end of 2009. The plots are based on data organized as continuous time series in which zero trade days are included. Panel A of Table 7 reports turnover based on discrete panel data that exclude zero trade days.

do revisit the issue of bond market efficiency, our goal is to assess what role the CDS markets played vis-á-vis the bond markets and to determine whether CDS trading was beneficial or detrimental to the underlying bond markets on criteria such as efficiency, quality, and liquidity. We examine these criteria before and after the inception of CDS trading when benchmarked against a control sample of firms with no CDS introduction.

Our econometric specifications extend earlier work, necessitated by the increasing complexity of the fixed-income markets. Corporate bonds contain call and amortization features, various default triggers, and conversion and put options. Therefore, in this paper we consider multivariate lead–lag relations of corporate bond returns to returns on various other securities that would also be incorporating issuer-specific and systematic market-wide information. The issuer-specific information includes equity return of the issuer [as equity value is an input to deriving a firm's credit spread in structural models pioneered by Merton, 1974] and CDS spreads of the issuer (to measure the underlying credit risk of the firm). We consider aggregate systematic variables such as implied volatility embedded in equity index options (to capture information about market-wide business and credit risk) and return on interest rate swaps (which are increasingly used as benchmark interest rate instruments). Hence, a wide range of factors are used to assess the efficiency of bonds relative to other securities. In addition, lagged corporate bond returns could explain current returns if bonds are weak-form inefficient.

To judge whether or not the introduction of CDS s was beneficial in enhancing bond market efficiency, we run relative efficiency tests for periods prior to the commencement of CDS trading for a firm, and we compare these results to the period after CDS trading commences. By regressing bond returns on contemporaneous and lagged values of these issuer-specific and aggregate variables, and testing for the joint significance of the lagged variables, we determine whether bonds are relatively inefficient compared with other securities that are also impounding information about the firm. Using this approach, we find that corporate bonds became increasingly inefficient as CDS markets matured. Our empirical analysis of bond market efficiency is robust and valid for a wide range of econometric tests and data configurations. We analyze bonds individually and also jointly in panel data analyses. We control for endogeneity in the decision to introduce CDS and confirm that there is nothing unique about the firms for which CDS trading commences (such as firms that are expected to become more illiquid or decline in credit quality) that drives our results.[2] To complement the lagged regression analyses, we undertake difference-in-differences (DID) tests in which we augment our sample of pre- and post-CDS bond transactions for CDS issuers with control samples of bond transactions by CDS non-issuers (firms with no CDS introduction). This analysis also provides definite evidence of declining bond market efficiency after CDS introduction. We also construct different subsamples (eliminating the financial crisis period, removing periods of nascency in the CDS market, examining subperiods of maturity in the CDS trading of a reference issuer, examining if the results differ for liquid versus illiquid bonds, vary by firm size, and by ratings and maturity) and find that the results hold for these subsamples as well. In short, the post-CDS decline in efficiency of bonds relative to other asset classes persists across various subsamples and for alternate robust specifications.

Did CDS trading improve the accuracy of bond prices? Following Hotchkiss and Ronen (2002) we implement the market quality measure (q) of Hasbrouck (1993). Hasbrouck's measure examines the discrepancy between efficient prices and transaction prices to assess the extent of pricing error. The inverse of the variance of pricing error is a metric of market quality. Whereas this metric is related to market efficiency, its focus is on whether prices accurately impound information. We compute q measure for bonds before and after the advent of CDS trading. The measure does not improve after CDS trading begins, suggesting that CDS markets did not enhance bond market quality.

Did bond market liquidity respond favorably to the inception of CDSs? We compute several proxies for liquidity before and after the introduction of CDS trading. Our metrics include number and dollar volume of bond trades, turnover, the LOT illiquidity measure of Lesmond, Ogden, and Trzcinka (1999), the illiquidity metric of Amihud (2002) and the related Amivest liquidity measure, the spread illiquidity measure of Roll (1984), the covariance illiquidity gamma of Bao, Pan, and Wang (2011), and the zeros impact and Roll impact illiquidity metrics based on Goyenko, Holden, and Trzcinka (2009). Many trading and price impact measures remain unaffected by inception of CDS markets. Amongst metrics that changed, more liquidity attributes deteriorated after the introduction of CDSs than those that improved. Overall, no evidence shows that CDS introduction improved the liquidity of underlying bonds.

Unlike equity markets that have much higher trading frequencies, examining these properties of bond markets is complicated by the fact that bonds are thinly traded, and consecutive days of trading might not always exist to compute returns for our tests. To ensure that available data are used to the best extent possible, we develop alternative approaches to augmenting the data, thereby resulting in larger data sets. The procedures are described in Section 3 and Appendix C. Re-running our analysis on an augmented data set confirms the robustness of our empirical analyses. Taken together, the results suggest that CDS introduction did not improve secondary bond market efficiency, quality, or liquidity.

What explains our results? One possible explanation is that price discovery mainly occurs in the CDS market because of microstructure factors that make it the most convenient location for the trading of credit risk. Another is that different participants in the cash and derivative markets trade for different reasons (e.g., Blanco, Brennan, and Marsh, 2005). The CDS market involves very active trading and is mostly dominated by institutional players and, hence, constitutes a highly likely venue for all

[2] In fact, we find that firms that are of better quality and have more liquid equity are more likely to be selected for CDS introduction.

informed trading.[3] At the same time, the corporate bond market is significantly less liquid. Bonds are traditionally held by buy-and-hold investors. Further, with the proliferation of the collateralized debt obligation securitization market, corporate bonds were increasingly parked inside pools and were not actively traded. For these reasons, as institutional investors migrated to the CDS market over time, corporate bond markets became less liquid and active [though Trade Reporting and Compliance Engine (TRACE) mandates did improve bond market liquidity; see, Edwards, Harris, and Piwowar, 2007; Bessembinder, Maxwell, and Venkataraman, 2006]. When we track the extent of trading by institutions before and after CDS introduction, we find evidence of a likely demographic shift by large institutional traders from trading bonds to trading CDSs to implement their credit views, resulting in declining efficiency and quality in bond markets. In addition, an analysis of the signed bond transactions by insurance companies reveals higher trade execution costs in the post-CDS period.

Our findings echo earlier results found in option markets, where the price discovery role of options is more pronounced when the liquidity of the option market is higher compared with that of the stock market, when options provide higher leverage, and when the probability of informed trading is high (Easley, O'Hara, and Srinivas, 1998).

Informed trading and price discovery in credit markets now also occurs in CDS markets, in addition to bond markets. Credit auctions also enhance the information in bond markets as clarity about recovery values increases (Gupta and Sundaram, 2012). Recent global over-the-counter (OTC) derivative market reform, in particular the regulatory efforts in the US spearheaded by the Commodity Futures Trading Commission and the Securities and Exchange Commission, recognize the important role of CDS markets vis-á-vis the bond markets, and it remains to be seen how new regulations will impact the bond markets.[4] CDS trading is moving to centralized clearing counterparties (CCCs), in which the techniques in this paper could be applied in future work to assess whether the opening of a CCC has a beneficial impact on bond markets.

The paper proceeds as follows. In Section 2 we review related work and distinguish our goals and methodology from earlier research. Section 3 describes the data set we employ. This section is complemented by Appendix C, which explains our new approach to creating nonoverlapping returns with a view to utilizing the entire data set for analysis, particularly for robustness tests. Section 4 presents tests of bond market efficiency and the finding that CDS markets could have been detrimental to bond market efficiency. We explore alternative cuts of the data set and variations of specifications (such as difference-in-differences tests, controls for endogeneity and fixed effects, and tests across subsamples) as robustness tests and show that the main findings about decline in efficiency are preserved. Section 5 explores the impact of CDS trading on bonds through the lens of the Hasbrouck (1993) *q*-measure and finds no improvement in market quality. Section 6 examines how CDS trading impacted the liquidity of underlying bonds using several metrics. There is no evidence of liquidity enhancement in bond markets. In Section 7 we consider one likely mechanism by which CDS introduction could hurt the efficiency of bond markets: the demographic shift by large institutional traders from trading bonds to trading CDSs. Conclusions and discussion are offered in Section 8.

2. Background and related literature

Early work on bond market efficiency focused on whether bond prices rapidly and accurately incorporated relevant information about issuer firms. A simple way to examine this proposition is to look at whether information that is incorporated into equity prices is also incorporated fully into bond prices in a timely manner. Such an analysis does not presuppose that the equity markets are efficient, yet tests whether the bond market is less efficient than the equity market.

For example, Kwan (1996) finds that, although a negative contemporaneous relation exists between changes in bond yields and stock returns, stocks lead bonds in incorporating firm-specific information, suggesting that bonds are less efficient than stocks of issuing firms. Downing, Underwood, and Xing (2009) report that, at the hourly level, stock returns lead nonconvertible bond returns for low credit quality bonds and convertible bond returns for all credit qualities. They conclude that bonds that are of lesser quality and have complex features are less efficient than stocks. In contrast, Hotchkiss and Ronen (2002) find that individual bonds are as informationally efficient as equity in rapidly responding to event-driven news and that market quality is no different for bonds than for the corresponding underlying stocks.[5] Similarly, Ronen and Zhou (2013) find that the corporate bond market is a venue for information-based trading. The concurrent introduction of TRACE and trading in CDSs suggest two likely mechanisms that impact the bond markets. TRACE induced greater price transparency but could also have generated information that spurred the CDS markets to the detriment of the bond markets. Alternately, TRACE enhanced bond market transparency, driving down dealer margins, making it unprofitable for

[3] Bank for International Settlements indicates that more than 95% of CDS transactions occur between financial institutions.

[4] CDS markets were blamed for naked shorting and excessive speculation, lowering capital requirements for financial institutions, lowering underwriting standards in asset-backed securities markets, and lowering monitoring incentives for banks, among others (source: International Swaps and Derivatives Association). The Dodd-Frank Wall Street Reform and Consumer Protection Act in the US introduces regulatory measures such as dealer collateral requirements, promoting transparency, setting up centralized clearinghouses, regulating naked CDS positions, and imposing position limits.

[5] The authors suggest that the introduction of the fixed-income pricing system (FIPS) by the National Association of Securities Dealers (NASD) in 1994 could have enhanced bond market transparency, thereby leading to improved informational efficiency. Beginning in July 2002, coinciding roughly with the start of our data, transparency has been enhanced with FIPS being rolled into a larger NASD system, the Trade Reporting and Compliance Engine.

market makers to trade in bond markets. This incentivized large players to move to the CDS markets instead, resulting in a drop-off in liquidity and efficiency in bond markets. Our analysis finds the latter effect, complementing the results in Goldstein, Hotchkiss, and Sirri (2007).

Previous work also examines the source of linkages between bond and equity markets. For example, Gebhardt, Hvidkjaer, and Swaminathan (2005) show momentum spillovers from equities to investment grade corporate bonds of the same firm. In addition, corporate news events such as mergers, takeovers, new debt issues, and stock repurchases involving wealth transfer to equity holders can further induce linkages between bonds and underlying stocks (Alexander, Edwards, and Ferri, 2000; Maxwell and Stephens, 2003).

Growing evidence exists of the linkage between bond and CDS markets, too. For instance, Hull, Predescu, and White (2004) study the information impact of CDS spreads on bond market ratings and find that credit spreads provide helpful information in estimating the probability of negative credit rating changes (downgrades, reviews for downgrade, and negative outlooks). Blanco, Brennan, and Marsh (2005) find that the CDS market leads the bond market in determining the price of credit risk. For 27 firms they examined, the CDS market contributes on average of around 80% of price discovery. In four of the remaining six cases, CDS prices Granger-cause credit spreads, suggesting price leadership. Baba and Inada (2009) find that subordinated bond and CDS spreads for Japanese banks are largely cointegrated and that the CDS spread plays a bigger role in price discovery than the bond spread as evidenced by stronger reactions of the CDS spread to financial market variables and bank-specific accounting variables than the bond spread. Norden and Wagner (2008) find that CDS spreads explain syndicated loan rates much better than spreads of similar-rated bonds.

Forte and Pena (2009) study the long-run equilibrium relations between bond, CDS, and stock market implied spreads and find that stocks lead CDSs and bonds more frequently than the reverse and that the CDS market leads the bond market. Ashcraft and Santos (2009) find that CDS introduction has not lowered the cost of debt financing or loan funding for the average borrower. They further report that risky and informationally opaque firms appear to have been more adversely affected by the CDS market. However, they look at bonds at the time of issue whereas our analysis spans the life cycle of bonds pre- and post-CDS. Norden and Weber (2009) study the intertemporal relations between CDS, stock, and bond markets. They find that stock returns lead CDS and bond spread changes and that the CDS market contributes more to price discovery than the bond market. The latter effect is stronger for US than for European firms.

Recently, Boehmer, Chava, and Tookes (2010) examine the implications of derivatives and corporate debt markets on equity market quality. They find that listed options have more liquid equity and more efficient stock prices. In contrast, firms with traded CDS contracts have less liquid equity and less efficient stock prices. Overall, they find that the impact of CDS markets is generally negative. Subrahmanyam, Tang, and Wang (2011) examine whether the existence of traded CDS contracts increases the credit risk of the reference entities. They find that the probabilities of credit downgrade as well as bankruptcy increase after the inception of CDS trading on account of the empty-creditor problem. In contrast, for sovereign bond markets, Ismailescu and Phillips (2011) provide evidence that the introduction of sovereign credit default swaps improved efficiency in the underlying government bonds.

These recent findings raise the question as to whether the introduction of CDS markets could have impaired or improved the informational efficiency of underlying corporate bond markets, and we empirically assess this question in this paper.[6] Unlike earlier work, our focus lies in assessing whether the inception of CDS trading was beneficial to underlying corporate bond markets in terms of efficiency, pricing error, and liquidity. We also use an extensive data set. Our conclusions are mostly consistent with much of the literature in that we find corporate bonds to be relatively inefficient. In addition, we show that corporate bonds did not become more efficient after the introduction of CDS trading. Efficiency, in fact, appears to have deteriorated. We find no evidence of increases in market quality, as defined by Hasbrouck's measure. We also do not observe any evidence of improvement in bond liquidity, using several different metrics, after the emergence of CDS markets.

3. Data

We construct a comprehensive data set of bonds and CDS trades for the period spanning the third quarter of 2001 to the third quarter of 2009. The sample period spans the years in which the CDS markets experienced rapid growth. We undertake an extensive sample construction and data-filtering process to arrive at our final data set. We first obtain corporate bond trading data from TRACE. Our initial sample consists of trades in 34,900 bonds issued by 4,869 firms, resulting in 5,768,201 daily time series observations. Next we collect daily trade data on five-year maturity CDSs from Bloomberg. Our preliminary sample consists of CDS trades of 620 issuing firms, amounting to 598,221 daily CDS spread observations.

We merge the data for trades on bonds and CDSs with bond issue-specific data from the Fixed Income Securities Database (FISD) and with equity data from the Center for Research in Security Prices (CRSP). We filter out bonds with incomplete data and retain bonds issued between 1994 and 2007. We keep only those bonds that are US-domestic, dollar-denominated, nonconvertible issues. Our final sample includes straight bonds, as well as bonds with call and put features. After eliminating bonds that do not belong to publicly traded firms, and merging and matching

[6] There is a long history of articles examining the impact of new derivatives markets on the market for the underlying security. Studies such as Conrad (1989) and Skinner (1989) show that equity option listing results in volatility reduction for the underlying security. Long, Schinski, and Officer (1994) find a marked increase in trading in the underlying security, but no change in price volatility. Sorescu (2000) finds positive (negative) abnormal returns for options listed during 1973–1980 (post-1980).

FISD, CRSP, TRACE, and CDS data sets, we end up with 1,545 bonds issued by 350 firms in our data set, with 1,365,381 transactions. The final data span the period from 2002 to 2008 and include, on average, 884 trading days per bond issue. Appendix A provides the details of sample construction and data-filtering process. The data summary is provided in Appendix B.

Our empirical tests require the joint use of data from equity, bond, CDS, and volatility markets. Accordingly, we filter the data sample for days on which concurrent observations are available on returns in the different markets under consideration. Details on the breakdowns based on concurrent returns as well as the breakdowns based on pre-CDS and post-CDS partitions are in Appendix B. While CDS and equity markets are highly liquid and have daily returns for most of the relevant time spans (69% and 95% of the sample for CDS and equity markets, respectively), bond markets are far less liquid. Valid daily bond returns (namely, two consecutive trading days with valid bond prices) exist for only 24% of the bond transaction data of 1.36 million bond trading days, implying that more than three-fourths of the bond transaction data is sparse (that is, bond prices exist for nonconsecutive trading days only), making it harder to construct daily bond returns. Overall, only 18% of the total time series data sample has valid (daily) returns jointly for bonds, CDSs, and stocks. Of the total of 1.36 million bond price observations, the vast majority (i.e., 1.25 million transactions or 92% of bond trading prices) occur after the introduction of the corresponding CDSs, and only about 21% of such post-CDS trading days have valid returns jointly for bonds, CDSs, and stocks. The fact that the coverage period of the TRACE database almost exactly coincides with the emergence of the CDS market explains why the vast majority of bond transaction observations correspond to the post-CDS period (and not before).

Appendix B also contains definitions of the five empirical daily return variables used in our tests. These are the return on corporate bonds (computed as changes in daily yields), return on stocks, changes in CDS spreads, changes in matching maturity swap rates (used as benchmark yields), and changes in the volatility index (VIX).

Table 1 presents the classification of time series observations on bond trading by year and by relation to CDS trading, as well as the descriptive statistics of different return variables. Table 1, Panel A, reports the number of bond trades with valid returns each year (classified based on whether these trades correspond to the period before

Table 1
Classification of bond transactions and descriptive statistics of returns.

The table presents the number of bond transaction observations by year and by credit default swap (CDS) status of the firm on transaction date and the descriptive statistics of various returns. Bond transactions of firms with CDS issues are classified into two types: trades that occurred before the introduction of CDS (pre-CDS sample) and trades that occurred after CDS introduction (post-CDS sample). Panel A shows the breakdown of observations by year; Panel B lists the breakdown for data sample selection criteria 1 and 2 (discussed in Section 3 and Appendix C). Panel C reports the descriptive statistics of various returns (defined in Appendix B) winsorized at the 1% level.

Panel A: Bond transactions and new CDS issues by year

Year	Pre-CDS sample	Post-CDS sample	All issues	Number of new CDS introductions
2002	2,022	5,637	7,659	104
2003	40,860	105,209	146,069	83
2004	33,472	155,798	189,270	107
2005	15,477	271,305	286,782	14
2006	9,558	257,474	267,032	4
2007	7,096	241,738	248,834	4
2008	2,449	217,286	219,735	16
Total	110,934	1,254,447	1,365,381	332

Panel B: Bond transactions by sample selection criteria

Sample selection criteria	Pre-CDS sample	Post-CDS sample	All issues
Sample selection criteria 1	11,128	187,003	198,131
Percent	10.03	14.91	14.51
Sample selection criteria 2	27,771	383,377	411,148
Percent	25.03	30.56	30.11
Total	110,934	1,254,447	1,365,381

Panel C: Descriptive statistics of daily return variables

Variable	Number of observations	Mean	Standard deviation	Minimum	Maximum
bndret	328,130	0.0122	0.5170	−2.2404	2.3245
cdsret	938,944	0.2001	4.4810	−19.0559	24.3352
stkret	1,294,161	0.0284	1.8567	−6.4143	6.4693
tryret	1,365,381	0.0008	0.0557	−0.1643	0.1683
vixchng	1,365,381	0.0041	1.3436	−5.5600	5.4500

or after the introduction of CDS on the underlying bond). From 2002 to 2008, the number of bond transactions rises at first, peaks in 2005, and declines thereafter. Panel A also reports the number of new CDSs introduced by year. The vast majority of CDSs are introduced in the first three years of our sample, 2002–2004 (the table excludes the 18 CDSs introduced in 2001).

Given the disparate types of securities involved in our analysis, i.e., stocks, bonds, and CDSs, obtaining consecutive days on which all these securities are traded is a challenge but is nevertheless required to compute returns for the econometric tests we conduct. Of the 1,365,381 daily transaction observations, only 249,605 have valid daily returns jointly for bonds, stocks, and CDSs. However, our requirement is even more stringent in the empirical analysis of efficiency: We need concurrent as well as lagged daily returns simultaneously for all securities. This results in additional attrition of the sample, biasing it toward bonds that are more actively traded, have more information available, and are likely more efficient (thus, this biases the tests against a finding of inefficiency in the bond markets).

We apply two alternative sample selection criteria to parse the data for our empirical tests. The default approach, which we call "sample selection criteria 1," invokes three successive trading days requirement: Transaction observations for any day t are included only if all return variables exist for day t as well as lagged trading day $t-1$. We also use an alternate data sampling procedure, which we denote as "sample selection criteria 2." This approach is based on a novel parsing of the data and results in more observations than criteria 1. Criteria 2 allows us to include more data for our analysis and constitutes both an innovation in data construction for efficiency tests and a robustness test for our main results. Appendix C provides details of the data construction procedure under criteria 2.

Table 1, Panel B, reveals that available data double when criteria 2 is implemented. For example, from the initial total sample of 1,365,381 observations, only 198,131 observations (15%) meet the requirement that valid contemporaneous as well as one-day lagged values exist for all five return variables under sample selection criteria 1. The size of this screened subsample increases to 411,148 observations (30%) under sample selection criteria 2.

Panel C in Table 1 presents the descriptive statistics of the daily returns on bonds, CDSs, stocks, benchmark swaps, and the VIX. The number of observations for bonds is lower than that for the other securities, confirming that bonds trade less frequently than stocks, CDSs, swaps, and volatility. We also compute the correlations between various contemporaneous and one-day lagged return variables for samples underlying selection criteria 1 and 2 (results not tabulated for brevity). We find almost all pairwise correlations to be significant at the 1% level. As expected, bond returns are positively correlated to stock and benchmark swap returns, and they are negatively correlated to volatility and CDS spreads, both contemporaneously and lagged. Bonds, therefore, reflect news shocks to other markets accordingly. Moreover, bond returns are negatively autocorrelated to lagged bond returns, suggesting that there could be frequent return reversals in the bond markets. The correlations have the expected signs for all securities, which sets the stage for more formal empirical analyses undertaken in Section 4.

We invoke three adjustments to the data samples obtained under sample selection criteria 1 and 2 prior to their adoption in formal empirical tests of bond efficiency, quality, and liquidity.[7]

First, we balance the pre- and post-CDS samples underlying the efficiency tests. Panel B of Table 1 reveals substantially greater number of observations for the period after the introduction of CDSs than the preceding period. The pre- and post-CDS sample sizes are unbalanced because the inherent structure of the two main data sets (Bloomberg CDS trades and TRACE bond transactions) prevent us from obtaining longer pre-CDS transaction history. Most CDS introductions take place early in the chosen 2002–2008 sample period (294 of 332 or 89% of CDS introductions occur in the first three years). The TRACE database commences in mid-2002. Hence, relatively limited pre-CDS bond trade history exists for 89% of CDS introductions. To alleviate this disparity in sample sizes, we create balanced pre- and post-CDS samples for regressions underlying tests of market efficiency by truncating the post-CDS period to just two years. We eliminate all transactions that occur more than two years after CDS introduction.

Next, we impose event windows in the analysis of bond liquidity and quality. To minimize possible confounding effects of unrelated events that could arise over long time spans (say, if entire pre- and post-CDS time periods are used), we consider a four-year window ([−2,+2] years) surrounding the CDS introduction event to assess the impact of the event on bond quality and liquidity. Given the sparse nature of bond trades, it appears appropriate to use two years of pre- and post-CDS trades for the tests implemented. This chosen window also reconciles with the preceding balanced sample approach we adopt in tests of bond efficiency.

Finally, we use control samples to benchmark the results obtained for the bonds of firms with CDS introduction. We consider two types of control samples: pooled unmatched control sample and pair-wise matched control sample. As Goldstein, Hotchkiss, and Sirri (2007) and Davies and Kim (2009) point out, both approaches of forming control samples have their own merits and applicability, yet each has likely limitations. An unmatched pooled control sample could include bonds that are different from the event sample bonds, which could affect the results. If a pair-wise matched control sample is used, results could be sensitive to the attributes used for matching and the particular choice of bonds selected. Therefore, we follow Goldstein, Hotchkiss, and Sirri (2007) and use both approaches. We form two control samples of bonds by firms with no CDS introduction as follows.

[7] We thank the anonymous referee for beneficial suggestions on these three methodological issues.

To construct the pooled unmatched control sample (henceforth, pooled control sample), we aggregate, as a pooled panel, all the bond issues by CDS nonissuers: firms that did not issue any CDSs until the end of 2009 and have bonds that meet the selection criteria outlined in Appendix A. An arbitrarily selected date, derived from uniform random distribution, within the range of the first and last trading dates of the bond is used as the event date for the control sample bonds. We retain only the transactions that occur within two years (before and after) the chosen event date.

To obtain the pair-wise matched control sample (hereafter, matched control sample), for each bond issue of a CDS introducing firm, we locate the closest matching bond of a CDS nonissuer based on bond size (outstanding amount), Standard and Poor's (S&P) rating (or Moody's rating if S&P rating is unavailable), time to maturity, and firm size (total asset value). Matching is done using the values of these attributes in the fiscal quarter of CDS introduction. On an ex-post basis, the matching control firm needs to have no CDS introduction for at least two years after the matching quarter (results are unchanged if we relax this constraint). The CDS introduction date is used as the event date for both bonds in a matched pair. We eliminate transactions that occur beyond a $[-2,+2]$ years window around the event date.

We conduct empirical analyses of bond efficiency using balanced pre- and post-CDS samples and impose a four-year ($[-2,+2]$ years) window for tests of bond quality and liquidity. We benchmark the results against the pooled control sample if a pooled panel of all observations is used and against the matched control sample if tests rely on individual bonds.

4. Empirical analysis of bond efficiency

To ascertain whether delays exist in relevant information being incorporated into bond prices, we regress contemporaneous bond returns on contemporaneous and lagged values of the following: stock returns, benchmark swap returns, changes in equity volatility, changes in CDS spreads (for the post-CDS period), and lagged bond returns. If the lagged variables in these regressions are jointly significant, it implies that information has been incorporated in other traded securities (issuer-specific as well as systematic), but not yet in the issuer's bonds. Thereby, it is evident that the bonds are relatively inefficient in comparison with other traded securities.

We start with period-partitioned regressions, that is, we run separate pre-CDS period versus post-CDS period regressions. The regression model used is as follows (Appendix B provides the variable definitions):

$$\begin{aligned} bndret_{it} = {} & a_{i0} + a_{i1} stkret_{it} + a_{i2} tryret_{it} + a_{i3} vixchng_{it} \\ & + a_{i4} cdsret_{it} + b_{i0} bndret_{i,t-1} + b_{i1} stkret_{i,t-1} \\ & + b_{i2} tryret_{i,t-1} + b_{i3} vixchng_{i,t-1} + b_{i4} cdsret_{i,t-1} + \varepsilon_{it} \end{aligned} \quad (1)$$

In the pre-CDS period, the variable relating to changes in CDS spreads, *cdsret*, is nonexistent. In the post-CDS period, we could choose to exclude this variable to be consistent with the regression in the pre-CDS period or include it to better reflect the entire information set available to the bond market. To be agnostic on this choice, we do both, with consistent results. Moreover, we recognize that the lagged bond return is likely to be the most informative lagged variable. So we run regressions without and with lagged bond returns and obtain similar inferences. We run all bond efficiency regressions using balanced data. We truncate the post-CDS period to just two years. Each regression implements the Newey and West (1987) adjustment for heteroskedasticity and autocorrelation.

In all regressions, we compute the *F*-statistic for the joint significance of the lagged return variables. If the lagged variables are jointly significant, it implies that the bonds are relatively inefficient. For corroboration, we create a second measure based on Hou and Moskowitz (2005). The $D1$ measure compares the fit of a model that has only contemporaneous data on the right-hand side of the regression (giving the constrained R^2) and the fit of a model with both contemporaneous and lagged data (yielding the unconstrained R^2) and is denoted

$$D1 = 1 - \left(\frac{\text{Constrained } R^2}{\text{Unconstrained } R^2} \right) \in (0,1) \quad (2)$$

The higher the $D1$ measure is, the greater the extent to which current bond returns are explained by lagged information: $D1$ is a measure of bond inefficiency. We compute this measure separately for pre- and post-CDS periods.

4.1. Analysis of individual bonds

First, we consider individual bonds with at least 30 valid observations (i.e., observations with simultaneous returns for bonds, stocks, and CDSs, if post-CDS, on two consecutive days) in both pre- and post-CDS periods. We obtain 45 individual bond issues for sample selection criteria 1 and 130 for criteria 2. For each bond, we use returns based on mean daily yields if there are more than one observed transaction during the day (Appendix B formally defines bond returns).[8] For individual bonds, we implement the regression model described above separately for pre- and post-CDS periods, without and with lagged bond returns, and compute the corresponding $D1$ measures. Table 2 reports the mean and median values of $D1$ measure in the pre- and post-CDS periods. The two panels correspond to the two sample selection criteria.

We analyze the pre-CDS and post-CDS inefficiency of individual bonds based on the reported $D1$ measures in Table 2. For analysis done without as well as with lagged bond returns, and for both sample selection criteria, the $D1$ metric becomes larger in magnitude in the post-CDS period. For example, the mean $D1$ increases from 0.33 to 0.37 (from 0.31 to 0.34) without lagged bond returns

[8] All results reported in this paper use bond returns based on mean daily yields. As robustness checks (not reported), we redo all efficiency regressions using bond returns computed based on median and end-of-the-day (last) daily yields instead. Qualitatively similar results obtain for all three measures of bond returns.

Table 2
Individual bond regressions.

For individual bonds of firms with credit default swap (CDS) issues, we run separate pre-CDS and post-CDS regressions of contemporaneous bond returns on contemporaneous and lagged values of the following variables: stock returns, swap returns, changes in volatility index (VIX), as well as with and without lagged bond returns (variables defined in Appendix B). The sample includes bonds with at least 30 observations in pre- and post-CDS periods. Panel A summarizes the results for sample selection criteria 1; Panel B, for sample selection criteria 2 (both criteria discussed in Section 3 and Appendix C).

In Panel A, number of individual bonds with at least 30 observations in pre- and post-CDS periods is 45. Without lagged bond returns, 21 bonds experience decrease in value of $D1$ measure after the introduction of CDSs, and 24 bonds experience increase in value. When lagged bond returns are included, 15 bonds experience decrease in value of $D1$ measure after the introduction of CDSs, and 30 bonds experience increase in value.

In Panel B, number of individual bonds with at least 30 observations in pre- and post-CDS periods is 130. Without lagged bond returns, 61 bonds experience decrease in value of $D1$ measure after the introduction of CDSs, and 69 bonds experience increase in value. When lagged bond returns are included, 59 bonds experience decrease in value of $D1$ measure after the introduction of CDSs, and 61 bonds experience increase in value.

	$D1$ measure		Number of bonds for which lagged variables are jointly significant at		
	Mean	Median	1% Level	5% Level	10% Level
Panel A: Sample selection criteria 1					
Without lagged bond returns					
Pre-CDS period	0.33	0.27	3	4	6
Post-CDS period	0.37	0.35	4	9	10
With lagged bond returns					
Pre-CDS period	0.71	0.79	33	38	38
Post-CDS period	0.82	0.84	40	44	44
Number of bond issues	45				
Number of pre-CDS observations	6,163				
Number of post-CDS observations	10,588				
Panel B: Sample selection criteria 2					
Without lagged bond returns					
Pre-CDS period	0.31	0.25	4	12	19
Post-CDS period	0.34	0.30	12	22	29
With lagged bond returns					
Pre-CDS period	0.67	0.72	83	95	98
Post-CDS period	0.69	0.77	88	96	102
Number of bond issues	130				
Number of pre-CDS observations	18,290				
Number of post-CDS observations	34,164				

under criteria 1 (criteria 2) and from 0.71 to 0.82 (from 0.67 to 0.69) with lagged bond returns. Similar increases occur for median $D1$ values. Furthermore, in all four cases, more individual bonds experience an increase in value of $D1$ measure after the introduction of CDSs than those that experience a decrease in value. Also, at different significance levels, lagged variables are jointly significant for a greater number of bonds in the post-CDS period. Taken together, these findings indicate that individual bonds appear to have become relatively more inefficient after the inception of CDS trading.

We recognize the limitations of small sample sizes when we consider individual bonds. Given the sparseness of bond trades, few individual bonds qualify the stringent requirement of at least 30 observations with simultaneous returns for bonds, stocks, and CDSs (if post-CDS) on two consecutive days in both pre- and post-CDS periods. We address the data sample size issue in two ways. First, in empirical analysis in the following subsections, instead of focusing on individual bonds, we use a pooled panel of all observations aggregated irrespective of the identity of the bonds. The default pooled panel consists of 198,131 observations and constitutes sample selection criteria 1. Second, we redo all tests using the extended pooled panel that consists of 411,148 observations and constitutes sample selection criteria 2. Therefore, empirical tests in the following subsections use reasonably sized samples, even though certain additional filters (such as truncation of post-CDS data) do reduce the samples (e.g., market efficiency tests in Section 4.2 rely on 58,462 and 130,526 observations, respectively).[9]

4.2. Partitioned panel data analysis

Next, instead of an analysis of individual bonds, we conduct period-partitioned regressions for pooled panel data (sample selection criteria 1 and 2). We implement the regression model listed above separately for pre- and post-CDS periods using the pooled panel of all observations lined up irrespective of the identity of the bonds. The post-CDS period is restricted to two years after CDS introduction. The results are reported in Panels A and B of Table 3. The two panels correspond to sample selection criteria 1 and 2, respectively. We run two versions of post-CDS regressions. We do not include contemporaneous or lagged changes in CDS spreads (*cdsret*) to keep the information sets common across the pre- and post-CDS periods.

[9] Our sample size, in terms of the number of individual bonds as well as the number of time series observations, compares favorably with those used in similar earlier work (e.g., Kwan, 1996; Hotchkiss and Ronen, 2002; Ashcraft and Santos, 2009).

Table 3
Panel regressions.

We run panel regressions of contemporaneous bond returns on contemporaneous and lagged stock returns, swap returns, changes in volatility index (VIX), changes in credit default swap (CDS) spreads (for the post-CDS period), as well as with and without lagged bond returns (variables defined in Appendix B). Panels A and B correspond to partitioned panel regressions separately for pre- and post-CDS periods. Panels C and D correspond to joint panel regressions using both pre- and post-CDS samples simultaneously with CDS dummy interaction. Interaction variable *CDS* is a dummy variable that has a value of one for the post-CDS period and zero for the pre-CDS period. Post-CDS sample is restricted to two years after CDS introduction. Each regression implements Newey and West (1987) adjustment for heteroskedasticity and autocorrelation. Panels A and C present the results for sample selection criteria 1, and Panels B and D present the results for sample selection criteria 2 (both criteria discussed in Section 3 and Appendix C). The four panels report the number of observations, adjusted R^2 values, and different *F*-statistics (and corresponding *p*-values). Panels C and D also report all the regressions coefficients (and associated *p*-values). Panels E and F correspond to joint panel tests with control for endogeneity. Panel E conducts a probit for CDS introduction (dependent variable equals one if CDS is introduced in a calendar quarter and zero otherwise), and Panel F repeats the joint panel regressions of Panels C and D with probability of CDS introduction as an additional variable. * indicates differences in means significant at 1% level.

Panel A: Partitioned panel regressions, sample selection criteria 1

	Without lagged bond returns			With lagged bond returns		
	Pre-CDS	Post-CDS panel		Pre-CDS	Post-CDS panel	
	panel	No *cdsret*	With *cdsret*	panel	No *cdsret*	With *cdsret*
Number of observations	11,128	47,334	47,334	11,128	47,334	47,334
Adjusted R^2	0.008	0.005	0.003	0.178	0.215	0.220
F-statistic (*p*-value)						
Overall model	14.42 (0.00)	60.79 (0.00)	56.88 (0.00)	75.13 (0.00)	381.67 (0.00)	318.45 (0.00)
Lagged variables	4.21 (0.00)	14.49 (0.00)	11.40 (0.00)	115.10 (0.00)	563.13 (0.00)	465.47 (0.00)
*D*1 measure	0.20	0.32	0.23	0.96	0.98	0.97

Panel B: Partitioned panel regressions, sample selection criteria 2

	Without lagged bond returns			With lagged bond returns		
	Pre-CDS	Post-CDS panel		Pre-CDS	Post-CDS panel	
	panel	No *cdsret*	With *cdsret*	panel	No *cdsret*	With *cdsret*
Number of observations	27,771	102,755	102,755	27,771	102,755	102,755
Adjusted R^2	0.004	0.003	0.004	0.141	0.177	0.179
F-statistic (*p*-value)						
Overall model	18.87 (0.00)	73.26 (0.00)	60.46 (0.00)	63.12 (0.00)	291.35 (0.00)	233.99 (0.00)
Lagged variables	4.68 (0.00)	15.53 (0.00)	14.98 (0.00)	85.70 (0.00)	393.49 (0.00)	319.86 (0.00)
*D*1 measure	0.14	0.25	0.24	0.97	0.98	0.98

Panel C: Joint panel regressions with CDS dummy interaction, sample selection criteria 1

	Without lagged bond returns		With lagged bond returns	
	Coefficient	*p*-Value	Coefficient	*p*-Value
$stkret_t$	0.015	0.00	0.018	0.00
$tryret_t$	0.489	0.00	0.540	0.00
$vixchng_t$	−0.011	0.07	−0.008	0.16
bndret_{t-1}			−0.412	0.00
stkret_{t-1}	0.010	0.00	0.018	0.00
tryret_{t-1}	0.085	0.25	0.258	0.00
$\mathrm{vixchng}_{t-1}$	−0.002	0.77	−0.005	0.36
$stkret_t \times CDS$	−0.011	0.00	−0.014	0.00
$tryret_t \times CDS$	0.025	0.76	0.062	0.41
$vixchng_t \times CDS$	0.012	0.08	0.009	0.14
$cdsret_t \times CDS$	−0.004	0.00	−0.005	0.00
$bndret_{t-1} \times CDS$			−0.048	0.02
$stkret_{t-1} \times CDS$	−0.004	0.21	−0.010	0.00
$tryret_{t-1} \times CDS$	0.011	0.89	0.076	0.30
$vixchng_{t-1} \times CDS$	0.003	0.58	0.007	0.27
$cdsret_{t-1} \times CDS$	−0.002	0.00	−0.004	0.00
Intercept	0.006	0.00	0.009	0.00
Number of observations	58,462		58,462	
Adjusted R^2	0.007		0.210	
F-statistic (*p*-value)				

Table 3 (*continued*)

Panel C: Joint panel regressions with CDS dummy interaction, sample selection criteria 1

	Without lagged bond returns		With lagged bond returns	
	Coefficient	*p*-Value	Coefficient	*p*-Value
Overall model	38.86 (0.00)		212.13 (0.00)	
All interaction variables	13.03 (0.00)		23.57 (0.00)	
Lagged interaction variables	3.78 (0.00)		16.95 (0.00)	

Panel D: Joint panel regressions with CDS dummy interaction, sample selection criteria 2

	Without lagged bond returns		With lagged bond returns	
	Coefficient	*p*-Value	Coefficient	*p*-Value
$stkret_t$	0.013	0.00	0.016	0.00
$tryret_t$	0.538	0.00	0.559	0.00
$vixchng_t$	−0.012	0.03	−0.005	0.10
bndret_{t-1}			−0.378	0.00
stkret_{t-1}	0.010	0.00	0.015	0.00
tryret_{t-1}	0.059	0.33	0.256	0.00
$\mathrm{vixchng}_{t-1}$	0.005	0.30	0.001	0.80
$stkret_t \times CDS$	−0.009	0.01	−0.012	0.00
$tryret_t \times CDS$	−0.004	0.96	0.028	0.68
$vixchng_t \times CDS$	0.014	0.02	0.011	0.05
$cdsret_t \times CDS$	−0.001	0.00	−0.002	0.00
$bndret_{t-1} \times CDS$			−0.055	0.02
$stkret_{t-1} \times CDS$	−0.004	0.21	−0.007	0.02
$tryret_{t-1} \times CDS$	0.048	0.46	0.077	0.22
$vixchng_{t-1} \times CDS$	−0.003	0.53	0.001	0.78
$cdsret_{t-1} \times CDS$	−0.001	0.00	−0.001	0.00
Intercept	0.007	0.00	0.010	0.00
Number of observations	130,526		130,526	
Adjusted R^2	0.004		0.169	
F-statistic (*p*-value)	42.90 (0.00)		159.45 (0.00)	
Overall model	7.95 (0.00)		13.78 (0.00)	
All interaction variables	4.64 (0.00)		13.34 (0.00)	
Lagged interaction variables				

Panel E: Controlling for endogeneity, stage 1 probit

	Results of probit		Mean values	
Variable	Coefficient	*p*-Value	CDS issuers	Nonissuers
Age (years since initial public offering)	0.003	0.02	36.92*	29.02*
Size = ln(total asset value)	0.121	0.00	22.77*	21.55*
Annualized six-month equity return	−0.001	0.17	7.87	11.36
Six-month equity return volatility	−0.238	0.07	0.13*	0.22*
Amihud equity illiquidity measure	−0.011	0.00	4.58*	14.89*
Standard & Poor's rating of long-term debt (numerical value)	−0.015	0.04	8.15*	10.94*
Return on assets	0.048	0.00	2.94*	2.61*
Tobin's q (market-to-book value of assets)	−0.104	0.01	1.46	1.52
Total debt to total assets ratio	0.670	0.00	0.32	0.31
Intercept	−2.827	0.00		

Panel F: Controlling for endogeneity, stage 2 joint panel regression with CDS dummy interaction

	Sample selection criteria 1		Sample selection criteria 2	
	Without lagged bond returns	With lagged bond returns	Without lagged bond returns	With lagged bond returns
Number of observations	58,426	58,426	130,362	130,362
Adjusted R^2	0.007	0.210	0.004	0.169
Probability of CDS introduction				
Coefficient	−0.008	−0.012	−0.009	−0.012
p-Value	0.08	0.02	0.01	0.00
F-statistic (*p*-value)				
Overall model	36.51 (0.00)	199.74 (0.00)	40.51 (0.00)	150.35 (0.00)
All interaction variables	12.98 (0.00)	23.52 (0.00)	7.88 (0.00)	13.68 (0.00)
Lagged interaction variables	3.76 (0.00)	16.90 (0.00)	4.84 (0.00)	13.58 (0.00)

Then we redo the regressions with the CDS variables included for the post-CDS period so that the new information set is used. All regressions are repeated with and without lagged bond returns. The results in Panels A and B of Table 3 complement the results of Table 2 by taking all bonds together in a panel.

The empirical implications of the panel regressions are twofold. First, the information model is validated by the observed regression coefficients (not tabulated for brevity). The coefficients on contemporaneous equity returns and benchmark swap returns are highly significant, implying that bond returns are responding to common information in other securities. Bonds are also informationally inefficient relative to other securities in both pre-CDS and post-CDS periods. The lagged equity and swap returns are highly significant. When we include the data on contemporaneous and lagged changes in CDS spreads, both coefficients are negative and significant, indicating that bond returns respond to information in CDSs as well.

Second, as tabulated results in Panels A and B of Table 3 reveal, the F-statistic for joint significance of the lagged variables is always significant across all regressions confirming the informational inefficiency of bonds. In addition, these F-statistic for lagged variables are considerably larger in the post-CDS period in comparison with the pre-CDS period values. Although values of F-statistic obtained from separate regressions are not comparable, there is indication that lagged variables are probably more material in the post-CDS period. This is affirmed when we compute the $D1$ measures for the period-partitioned panel regressions.

As in Table 2 for individual bonds, we find that the $D1$ values are higher in the post-CDS panels relative to the values in the pre-CDS panels. Because direct statistical comparison of pre- and post-CDS $D1$ values is not feasible, we carry out bootstrapping tests for comparison. From the pooled panel of all observations, we randomly select, without replacement, subsamples of 500 (or 1000) observations from pre- and post-CDS periods, and we compute $D1$ values for the pre- and post-CDS subsamples. We repeat the process for 5000 different iterations each for sample selection criteria 1 and 2 (without and with lagged bond returns) and compare the five thousand pairs of pre- and post-CDS $D1$. We find that post-CDS $D1$ values are significantly greater than pre-CDS $D1$ values and that the statistical significance is more pronounced if regressions include lagged bond returns but exclude contemporaneous and lagged CDS returns (results not tabulated). Thus, both F-statistics and $D1$ measures suggest an increase in relative inefficiency of bonds after the inception of CDS markets.

Are these results also economically significant? To explore this, we impose 1μ (1 mean value) or 1σ (1 standard deviation) perturbations in each explanatory return variable. The direction of perturbation depends on the sign of the corresponding coefficient in the panel regression. Because F-values are sign-independent and test whether the regression coefficients jointly deviate from zero irrespective of which direction they deviate, we impose the perturbations in the same direction as the regression coefficients so that their product remains sign-free. Then, using the product of regression coefficients and the imposed perturbations, we compute the resultant change (increase) in current bond returns. We find that the impact of lagged variables on bond returns increases post-CDS for both 1μ shift and 1σ shock analysis (detailed results not tabulated). For example, in regressions with lagged bond returns, 1μ shifts in lagged variables increase bond returns by 20–25 basis points in the pre-CDS period and by 27–31 basis points in the post-CDS period. In percentage terms, the 1μ shifts in lagged variables account for 70% of the innovations in current bond returns before CDS introduction and 80% after. Similar results obtain for 1σ shocks. Hence, the economic consequences of inefficiency are material, more so in the post-CDS period.

4.3. Joint panel data analysis

To combine the period-partitioned data and use them completely, we now undertake joint panel regressions using a CDS dummy, CDS_{it}, which equals one for post-CDS observations and zero otherwise. These tests entail application of a single regression to the combined panel of pre- and post-CDS observations. We implement two variations of joint panel tests. The first allows us to evaluate the significance of lagged variables separately for pre- and post-CDS periods, and the second computes the incremental impact of lagged information after CDS introduction. Taken together, the results of the two tests engender inferences on the impact of CDS introduction on bond market efficiency.

The first specification effectively keeps the pre-CDS and the post-CDS variables distinct, though both are combined into one regression as follows:

$$\begin{aligned} bndret_{it} = {} & a_{i0} + (1-CDS_{it}) * [a_{i1} stkret_{it} + a_{i2} tryret_{it} \\ & + a_{i3} vixchng_{it}] + (1-CDS_{i,t-1}) * [b_{i0} bndret_{i,t-1} \\ & + b_{i1} stkret_{i,t-1} + b_{i2} tryret_{i,t-1} + b_{i3} vixchng_{i,t-1}] \\ & + CDS_{it} * [c_{i1} stkret_{it} + c_{i2} tryret_{it} \\ & + c_{i3} vixchng_{it} + c_{i4} cdsret_{it}] + CDS_{i,t-1} * [d_{i0} bndret_{i,t-1} \\ & + d_{i1} stkret_{i,t-1} + d_{i2} tryret_{i,t-1} \\ & + d_{i3} vixchng_{i,t-1} + d_{i4} cdsret_{i,t-1}] + \varepsilon_{it} \end{aligned} \tag{3}$$

For brevity, we do not tabulate the results; we summarize the key findings as follows. The individual significance of lagged variables is more pronounced in the post-CDS period than in the pre-CDS period. The F-statistics for the joint significance of lagged variables are significant in pre- and post-CDS periods, with considerably larger values observed for the post-CDS period. In regressions without lagged bond returns, pre-CDS F-statistic is 4.39 (4.90) and post-CDS F-statistic is 11.32 (14.90) under criteria 1 (criteria 2), and in regressions with lagged bond returns, pre-CDS F-statistic is 115.07 (85.65) and post-CDS F-statistic is 465.71 (319.96) under criteria 1 (criteria 2). All values are significant at the 1% level. The relatively larger values of post-CDS F-statistics confirm the preceding partitioned panel results and indicate greater dependence on lagged variables in the post-CDS period.

Next, we implement a robust variation of the preceding specification that allows us to tease out the incremental post-CDS effects instead of merely separating the results for pre- and post-CDS periods. We run joint panel regressions with CDS dummy interactions as follows:

$$\begin{aligned} bndret_{it} = a_{i0} &+ [a_{i1} stkret_{it} + a_{i2} tryret_{it} + a_{i3} vixchng_{it}] \\ &+ [b_{i0} bndret_{i,t-1} + b_{i1} stkret_{i,t-1} + b_{i2} tryret_{i,t-1} \\ &+ b_{i3} vixchng_{i,t-1}] + CDS_{it} * [c_{i1} stkret_{it} + c_{i2} tryret_{it} \\ &+ c_{i3} vixchng_{it} + c_{i4} cdsret_{it}] + CDS_{i,t-1} * [d_{i0} bndret_{i,t-1} \\ &+ d_{i1} stkret_{i,t-1} + d_{i2} tryret_{i,t-1} + d_{i3} vixchng_{i,t-1} \\ &+ d_{i4} cdsret_{i,t-1}] + \varepsilon_{it} \end{aligned} \quad (4)$$

In this specification, all explanatory return variables (lagged and contemporaneous) are included twice: stand-alone and multiplied by the CDS dummy, CDS_{it} or $CDS_{i,t-1}$. By design, only the post-CDS variables get included in the interaction form. Thus, the interaction variables reflect the incremental explanatory role of the return variables in the post-CDS period relative to (i.e., over and above) that in the pre-CDS period. Consequently, this specification allows us to decompose the explanatory power of all included variables into two parts: a period-independent base effect and an incremental post-CDS effect. We implement a single regression using the combined panel of pre- and post-CDS observations, and we emphasize particular attention to the joint significance of the lagged dummy interaction variables because this subset of variables highlights any incremental dependence on past information (and, hence, reveals the decline in efficiency, if any), in the post-CDS period.

Panels C and D of Table 3 report the results of joint panel regressions with CDS dummy interaction. The two panels correspond to sample selection criteria 1 and 2. As before, the post-CDS sample is restricted to two years after CDS introduction. We report the results of this regression without and with lagged bond returns. We do not assign any value to the variable relating to changes in CDS spreads, *cdsret*, in the pre-CDS period. This variable, by design, manifests only in interaction form in the post-CDS period.

The results reveal that, in stand-alone form, the coefficients corresponding to lagged bond, stock, and benchmark swap returns are significant and depict expected signs. The interaction variables corresponding to contemporaneous stock and CDS returns and lagged bond, stock and CDS returns are always significant, indicating that these returns demonstrate relatively greater incremental explanatory power after the introduction of CDSs. This is confirmed by the large and significant *F*-statistics for all interaction variables considered jointly. Crucially, the *F*-statistics corresponding to lagged interaction variables always show very strong significance (*F*-values of 3.78, 16.95, 4.64, and 13.34, all with *p*-values less than 0.01). These reveal that lagged return variables jointly bear significantly greater information content for current bond returns after CDS introduction compared with the pre-CDS period.

To summarize the preceding results, we find that *F*-statistics corresponding to lagged variables have higher values in the post-CDS period than the pre-CDS period, post-CDS $D1$ values are larger than pre-CDS $D1$ values, and *F*-statistics corresponding to incremental contribution of lagged variables in the post-CDS period are always significant. In other words, the dependence of bond returns on lagged information is higher after CDS introduction and the incremental impact of CDS introduction is statistically significant. Thus, we infer that bond market efficiency relative to other securities deteriorated after the inception of CDS trading.

In additional tests, we repeat the joint panel regressions with CDS dummy interaction after including fixed effects for bond issuer firm, bond issue and calendar year (results not tabulated). Fixed effects do exist for some specifications that include issuer- or issue-specific dummy variables. Nevertheless, even after inclusion of different fixed effects, the values of *F*-statistics corresponding to lagged interaction variables remain significant and almost identical to those reported in Panels C and D of Table 3.

To assess the economic significance of preceding results, we compute the magnitude of change in bond returns when each explanatory variable is perturbed by 1μ (1 mean value) or by 1σ (1 standard deviation). We do not tabulate the detailed results. We find that 1μ shifts in (1σ shocks to) lagged return variables result in 6–13 (277–626) basis points additional bond returns after CDS introduction compared with the pre-CDS period. Alternately, the contribution of changes in lagged return variables to innovations in current bond returns increases incrementally by 14–21% in the post-CDS period.[10] These results confirm the statistical as well as economic significance of the decline in bond market efficiency subsequent to the introduction of CDS.

4.4. Controlling for endogeneity

Dealers most likely issue CDSs for a corporate entity based on certain bond issuer characteristics. To understand how dealers select the issuers to introduce CDSs and how the endogeneity inherent in this choice decision could impact the empirical analysis of bond returns, we carry out a two-stage Heckman (1979) sample selection regression analysis along the lines of Mayhew and Mihov (2004), who conduct a similar analysis to examine how exchanges select a stock for option listing.

We create a panel data set on a quarterly basis. Each calendar quarter, we line up all CDS issuer and nonissuer firms constituting our overall sample. For each firm-quarter observation, we collect different issuer-specific variables from CRSP and Compustat [chosen variables are along the lines of Das, Hanouna, and Sarin, 2009]. We drop all firm-quarter observations post CDS-introduction; that is, if a CDS has been introduced for a firm prior to a given quarter, we drop that firm-quarter from the analysis (because CDS introduction is no longer a choice decision for these firms). For the remaining observations, we define a CDS introduction event binary variable that takes a value of one if a CDS is introduced for the firm during that quarter and zero if no CDS is introduced until the end of the quarter. We obtain 332 event firm-quarters and 10,945 nonevent control firm-quarters. Panel E of Table 3 reports the results of the first stage probit of the

[10] In terms of magnitude, 1σ shock effects are many times larger than 1μ shift effects because the standard-deviation-to-mean ratios of all explanatory variables are extremely large (Panel C of Table 1). However, in terms of percentage effects, 1μ shift and 1σ shock analyses yield largely similar inferences.

CDS introduction event on various issuer attributes along with the comparison of mean values of these attributes for CDS issuers versus CDS nonissuers.

Interestingly, unconditional comparison of means as well as results of the probit reveal that a CDS is more likely to be introduced for older, larger, better rated, more profitable firms with higher equity liquidity and lower equity volatility (or, in short, for less distressed firms). From a distress perspective, only the positive coefficient of leverage and the negative coefficient of Tobin's q are along expected lines. But even these two attributes manifest their effects only in the multivariate setup. Unconditionally, the values of both variables for CDS issuers versus nonissuers are not significantly different. The results of CDS introduction probit (and subsequent endogeneity-controlled regressions discussed below) remain unchanged if we add bond vintage (age in years) as a variable, use value of equity capitalization instead of total value of all assets, and use book-to-market value of equity instead of Tobin's q.

For the second step of the two-stage procedure, we compute the probability of CDS introduction as the predicted probability of the dependent variable in the probit specification. If CDS has already been introduced for a firm on a given observation date, we impute a value of one to the probability of CDS introduction. We repeat the joint panel regressions of Panels C and D with the probability of CDS introduction as an additional explanatory variable. The results are reported in Panel F of Table 3 (we ignore the coefficients of other explanatory variables).

Notwithstanding the inclusion of implied probability of CDS introduction as an additional explanatory variable, in all four specifications the joint significance of the lagged interaction variables, as captured by F-statistics, is almost the same as those reported in Panels C and D. Hence, the inference of deterioration of bond market efficiency following CDS introduction remains robust to controls for endogeneity. We also find that the implied probability of CDS introduction has a negative impact on bond returns; that is, bond returns are lower if a CDS has already been introduced or the prospect of impending CDS introduction is high. This effect is corroborated in work by Subrahmanyam, Tang, and Wang (2011) who find that probabilities of credit downgrades as well as bankruptcies increase after the inception of CDS trading.

4.5. Difference-in-differences tests

The analysis so far reveals that bond returns demonstrate greater dependence on the lagged information set following the introduction of CDSs. However, such inferences of deteriorating bond market efficiency are likely to be unconvincing if there are permanent differences between firms with and without CDS introduction (as the preceding probit analysis indicates), or if there exist trends (such as systematic evolution in the type of firms issuing bonds or fundamental shifts in market or economic conditions) over the time period of study. Empirical inferences of market inefficiency can be disentangled from such contaminating effects through the use of difference-in-differences tests that entail the comparison of CDS-introducing firms with a control group of firms with no underlying CDS in a single combined panel.

To implement the DID tests, we augment our sample of pre- and post-CDS bond transactions for CDS issuers with control samples of bond transactions by CDS nonissuers (firms with no CDS introduction). The sample of observations for CDS issuers is obtained from sample selection criteria 1 and 2. Each is merged with the matched as well as pooled control sample. For the four combined samples so obtained, we run the following regression.[11]

$$\begin{aligned} bndret_{it} = {} & \alpha_i + \beta_1 CV_i + \beta_2 LV_i + \beta_3 CDS_{it} * CV_i + \beta_4 CDS_{i,t-1} * LV_i \\ & + \beta_5 E_i * CV_i + \beta_6 E_i * LV_i + \beta_7 E_i * CDS_{it} * CV_i \\ & + \beta_8 E_i * CDS_{i,t-1} * LV_i + \varepsilon_{it}, \end{aligned} \tag{5}$$

where $CV_i \equiv \{stkret_{it}, tryret_{it}, vixchng_{it}, cdsret_{it}\}$, $LV_i \equiv \{bndret_{i,t-1}, stkret_{i,t-1}, tryret_{i,t-1}, vixchng_{i,t-1}, cdsret_{i,t-1}\}$, CDS_{it} equals one if post-CDS period and zero if pre-CDS period, and E_i equals one for the event sample of CDS issuers and zero for the control sample of nonissuers. CV_i denotes the set of contemporaneous return variables and LV_i encompasses one-day lagged return variables. For the sample of CDS issuers as well as the matched control sample of CDS nonissuers, transactions are classified into pre- and post-CDS periods (CDS_{it}=0 versus 1) based on the actual CDS introduction date. For each bond in the pooled control sample of CDS nonissuers, an arbitrarily selected date (derived from uniform random distribution) within the range of the first and last trading dates of the bond is used to categorize pre- and post-treatment observations.[12] As before, post-CDS sample is restricted to two years.

We implement eight different DID regressions (two sets of CDS observations × two control samples × regressions without and with lagged bond returns). Table 4 reports the F-statistics (and associated p-values) corresponding to the joint significance of various subsets of explanatory variables. The β coefficients capture differences in relative explanatory power of contemporaneous and lagged variables for event sample versus control sample bonds and between pre- and post-CDS periods. The significance of coefficients β_2, β_4, $\beta_5+\beta_6$, β_6, and β_8 are of particular interest to us. Coefficient β_2 reveals the unconditional relevance (independent of sample type and time period) of lagged variables for current bond returns. Coefficient β_4 reflects sample-independent time trends in the significance of lagged variables, that is, whether lagged variables become more material in the post-CDS period for the joint sample of bonds of CDS issuers and nonissuers. Coefficients β_6 and $\beta_5+\beta_6$ intimate time-independent fundamental differences between event and

[11] The DID regression is an extended version of the joint panel regression underlying Table 3. If we retain only the E_i=1 observations, the regression collapses into the second regression reported in Section 4.3.

[12] The choice of an arbitrary treatment date for the pooled control sample is motivated by a similar approach adopted in event study literature. Results of DID regressions remain unaltered when we implement different simulation runs to generate distinct values of these arbitrarily selected dates. As a check for robustness, we employ two alternate choices for the treatment (nonevent) date for the pooled control sample bonds: the midpoint between the first and last trading dates of each bond, and the fixed midpoint of our data sample, June 30, 2005. Results of DID regressions remain materially unaffected under all three choices.

Table 4
Full panel difference-in-differences regressions.

We augment our sample of pre- and post-introduction bond transactions for credit default swap (CDS) issuers with control samples of bond transactions by CDS nonissuers (firms with no CDS introduction). Control samples are constructed in two ways (detailed in Section 3): matched control sample consists of transactions of the closest matching bond (in terms of bond size, rating, maturity and firm size) of a CDS nonissuer for each bond of CDS issuers; and pooled control sample aggregates all bond transactions of issuing firms that meet the selection criteria outlined in Appendix A but did not issue any CDSs until the end of 2009. Post-CDS sample is restricted to two years after CDS introduction. Both the control samples are truncated to a $[-2,+2]$ years window around the chosen event date. For the combined (event plus control) sample of all observations, we run the following regression with Newey and West (1987) adjustment for heteroskedasticity and autocorrelation:

$$bndret_{it} = \alpha_i + \beta_1 CV_i + \beta_2 LV_i + \beta_3 CDS_{it} * CV_i + \beta_4 CDS_{i,t-1} * LV_i + \beta_5 E_i * CV_i + \beta_6 E_i * LV_i + \beta_7 E_i * CDS_{it} * CV_i + \beta_8 E_i * CDS_{i,t-1} * LV_i,$$

where $CV_i \equiv \{stkret_{it}, tryret_{it}, vixchng_{it}, cdsret_{it}\}$, $LV_i \equiv \{bndret_{i,t-1}, stkret_{i,t-1}, tryret_{i,t-1}, vixchng_{i,t-1}, cdsret_{i,t-1}\}$, CDS_{it} equals one if post-CDS period and zero if pre-CDS period, and E_i equals one for the event sample of CDS issuers and zero for the control sample of nonissuers. CV_i and LV_i denote contemporaneous and lagged (explanatory) variables, respectively (variables defined in Appendix B). Classification of transactions into pre- and post-CDS periods ($CDS_{it}=0$ versus 1) is based on the actual CDS introduction date for CDS issuers and matched control sample of CDS nonissuers and on an arbitrarily selected date (derived from uniform random distribution) for pooled control sample of CDS nonissuers. Panel A presents the results for sample selection criteria 1; Panel B, for sample selection criteria 2 (both criteria discussed in Section 3 and Appendix C). We report the F-statistics (and associated p-values) for the overall model and those corresponding to the joint significance of various sets of explanatory variables.

	F-Statistics (*p*-value)			
	Matched control sample		Pooled control sample	
Regression coefficients	Without lagged bond returns	With lagged bond returns	Without lagged bond returns	With lagged bond returns
Panel A: Sample selection criteria 1				
Full model	94.72 (0.00)	292.52 (0.00)	35.08 (0.00)	149.79 (0.00)
$\beta_1+\beta_2$	59.63 (0.00)	237.67 (0.00)	42.16 (0.00)	97.95 (0.00)
β_2	36.24 (0.00)	336.53 (0.00)	38.90 (0.00)	126.94 (0.00)
$\beta_3+\beta_4$	11.77 (0.00)	14.01 (0.00)	1.59 (0.14)	1.40 (0.20)
β_4	2.18 (0.09)	9.76 (0.00)	2.48 (0.06)	1.22 (0.30)
$\beta_5+\beta_6$	6.50 (0.00)	7.49 (0.00)	12.48 (0.00)	16.16 (0.00)
β_6	7.08 (0.00)	11.33 (0.00)	12.71 (0.00)	21.42 (0.00)
$\beta_7+\beta_8$	16.00 (0.00)	25.77 (0.00)	11.72 (0.00)	20.01 (0.00)
β_8	4.23 (0.00)	17.14 (0.00)	4.17 (0.00)	13.36 (0.00)
Panel B: Sample selection criteria 2				
Full model	121.08 (0.00)	353.38 (0.00)	36.80 (0.00)	149.09 (0.00)
$\beta_1+\beta_2$	99.40 (0.00)	364.57 (0.00)	51.42 (0.00)	91.38 (0.00)
β_2	36.13 (0.00)	509.60 (0.00)	48.99 (0.00)	111.52 (0.00)
$\beta_3+\beta_4$	3.01 (0.01)	3.93 (0.00)	1.63 (0.14)	2.28 (0.03)
β_4	2.24 (0.08)	2.72 (0.03)	3.01 (0.03)	3.65 (0.01)
$\beta_5+\beta_6$	3.88 (0.00)	4.21 (0.00)	15.28 (0.00)	19.76 (0.00)
β_6	6.35 (0.00)	7.08 (0.00)	15.52(0.00)	26.62 (0.00)
$\beta_7+\beta_8$	6.60 (0.00)	11.03 (0.00)	11.47 (0.00)	19.77 (0.00)
β_8	5.00 (0.00)	11.32 (0.00)	4.65 (0.00)	13.89 (0.00)

control samples, whether bond returns of CDS issuers incorporate lagged and contemporaneous information differently from those of CDS nonissuers in pre- as well as post-CDS periods. The key coefficient of interest, β_8, informs the incremental effect of lagged variables in the post-CDS period relative to the pre-CDS period over and above similar time trend effects within the control group. A significant β_8 provides conclusive evidence of post-CDS deterioration of bond efficiency.

We find that β_2 is significant in all eight specifications; which implies that, unconditionally (i.e., without distinguishing event and control sample bonds, or pre- and post-CDS status), lagged variables always impact current bond returns. Bond markets, in general, are inefficient. Coefficient β_4 is marginally significant (i.e., significant at the 5% level for four of the eight cases), indicating that, for the joint event and control sample, lagged variables probably influence current bond returns more in the post-CDS period. There is a weak overall deterioration of bond efficiency over time. Because $\beta_5+\beta_6$ is always significant, we infer that bonds of CDS issuers always react to contemporaneous and lagged information from other securities to a greater extent than control sample bonds. In particular, the significant β_6 divulges that, irrespective of CDS introduction status, lagged variables always bear greater impact on bond returns of CDS issuers than those of nonissuers. Bonds of CDS issuers appear fundamentally different in terms of the price discovery process (i.e., in incorporating information).

Most important, β_8 is convincingly significant in all eight implementations of DID regressions. The F-statistics corresponding to β_8 depict values of 4.17–5.00 without lagged bond returns and 11.32–17.14 with lagged bond returns. The corresponding p-values are always less than 0.01. Lagged return variables influence bond returns of CDS issuing firms to a greater extent in the post-CDS period compared with the pre-CDS period even after controlling for time trends and differences between CDS issuers and nonissuers. To elaborate, because β_2, β_4, β_6, and β_8 are all significant, we can assert that the event of CDS introduction significantly deteriorates the efficiency of underlying bonds, even though bonds are inherently inefficient, there is a likely decline in efficiency of all bonds over time, and bonds of CDS issuers are fundamentally different in terms of price discovery. The observed decline in efficiency is robust even after controls for these three accompanying effects.

Unlike the matched control sample, the pooled control sample is designed completely independent of the CDS sample. Do likely differences in the distribution of observations (e.g., number of trades per year) in the CDS and pooled control samples affect our results? We address this issue by undertaking two additional sets of DID tests.[13] First, we run the four regressions involving the pooled control sample (two sample selection criteria × regressions without and with lagged bond returns) after including fixed effects for calendar year (detailed results not tabulated). We find coefficient β_8 significant in all four tests: F-statistics (p-values) are 3.50 (0.01), 14.66 (0.00), 3.92 (0.00), and 11.54 (0.00). The calendar-year dummy variables are not significant in three of the four tests, indicating that nonequality of annual distributions is not materially relevant.

Next, instead of the independent pooled control sample used so far, we construct a new pooled control sample based on stratified sampling. Under this approach, annual distribution of observations in the CDS sample serve as input probabilities in the sampling process, and pooled control sample observations and associated event dates are drawn up based on these input probabilities. Appendix D details the implementation steps of the stratified sampling process. By design, the distribution of control sample observations is conditionally dependent on the distribution of CDS sample observations. We run the DID regressions using the stratified pooled control sample. Again we find that coefficient β_8 is significant in all four tests: F-statistics (p-values) are 3.41 (0.01), 14.12 (0.00), 5.21 (0.00), and 11.19 (0.00). The observed decline in post-CDS efficiency remains robust.

4.6. *Joint panel regressions over subsamples*

As additional tests for the robustness of our findings, we now implement the joint panel regressions with CDS dummy interactions for different subsamples formed based on time period and firm- and issue-specific attributes. Table 5 reports the values of different F-statistics for various subsamples and different test specifications. We focus particular interest on the F-statistic corresponding to the lagged interaction variables, which captures the incremental significance of lagged variables in the post-CDS period relative to the pre-CDS period.

4.6.1. *Effect of the financial crisis*

The last two years of our data sample, 2007–2008, overlap the financial crisis period. If the financial crisis resulted in unexpected credit and liquidity shocks to financial markets, we expect a drying up of liquidity in equity and bond markets. A large decline in liquidity is likely to cause delays in relevant information being incorporated in security prices. Is the observed post-CDS decline in bond efficiency mainly an outcome of the financial crisis?

To test this proposition, we drop the crisis years 2007 and 2008 from our sample, and we repeat the joint panel regressions. We find that the values of F-statistics corresponding to lagged interaction variables are almost identical to those reported in Panels C and D of Table 3, and remain strongly significant. Our results are not materially influenced by the financial crisis. Increased post-CDS dependence on lagged information persists even for noncrisis periods.

4.6.2. *Effect of nascency of the CDS market*

The initial years of any financial innovation are likely to be characterized by uncertainty, gradual evolution, and limited informational impact. In the first few years after their introduction, CDS contracts could be expected to be thinly traded and slow in incorporating changes in credit views about the underlying firm. The low liquidity and slow price discovery in the CDS market during the nascent phase, in turn, is likely to have minimal effects on the price discovery process in the underlying bond market. Do the observed post-CDS increase in bond inefficiency apply only to the post-incubation (evolved) phase of this financial innovation, and not when this market segment was nascent?

We do not have trading data or related variables in our CDS data set to enable us to track the evolution of CDS markets. Instead, we examine the posed question by applying the joint panel regressions to two separate subsamples: the nascent three-year subperiod 2002–2004 and the post-incubation subperiod after eliminating the initial years 2002 and 2003. First, in results not tabulated, we find that the coefficients and p-values corresponding to CDS returns ($cdsret_t$ and $cdsret_{t-1}$) are similar for regressions over the two subsamples. So the price discovery in bonds arising from CDS markets appears largely unchanged over time. Next, Table 5 reveals that the F-statistics corresponding to lagged interaction variables are strongly significant for both subsamples (the magnitudes are slightly different, but statistical significance is not different across the two subsamples). Thus, our results are not driven by the infancy of the CDS market in the initial years of our sample.

4.6.3. *Effect over time after introduction of CDSs*

The nascency effects could apply not only to aggregate financial innovations such as emergence of a new market segment, but also to first-time issuance of a security for individual firms. To elaborate, when a CDS contract is introduced for a specific firm, the initial years could depict low liquidity and slow price discovery, and efficiency probably improves in the subsequent years. All efficiency results reported so far are based on two-year post-CDS data (for balancing purposes). Is the decline in bond efficiency after CDS introduction a short-term negative effect at firm level that fades away over longer periods?

To test this proposition, we form subsamples by including only those bond transactions that occur within one, two, and three years after the introduction of CDSs. For these three subsamples and an unrestricted sample that retains all post-CDS observations, we implement joint panel regressions. In Table 5, we find that lagged

[13] We thank the anonymous referee for suggesting these additional tests.

Table 5
Joint panel regressions with credit default swap (CDS) dummy interaction for different subsamples.

For different subsamples of data, we repeat tests of Table 3, Panels C and D. We run joint panel regressions of contemporaneous bond returns on contemporaneous and lagged stock returns, swap returns, changes in volatility index (VIX), changes in CDS spreads (for the post-CDS period), as well as with and without lagged bond returns, using both pre- and post-CDS samples simultaneously. We form subsamples that (a) exclude 2007–2008 (the liquidity crisis years) observations, (b) include only 2002–2004 (the initial years of CDS market) observations, (c) exclude 2002–2003 (the first two years of emergence of CDS market) observations, (d) include only one-, two-, and three-years of post-CDS observations, and (e) are classified low versus high amount outstanding, short versus long vintage, and small versus large firm size based on median values in the year of CDS introduction. Post-CDS subsamples [except for those under (d)] are restricted to two years after CDS introduction. The explanatory return variables are included without and with interaction with *CDS*, a dummy variable that has a value of one for the post-CDS period and zero for the pre-CDS period. Each regression implements Newey and West (1987) adjustment for heteroskedasticity and autocorrelation. Panel A presents the results for sample selection criteria 1; Panel B, for sample selection criteria 2 (both criteria discussed in Section 3 and Appendix C). We report the *F*-statistics (and associated *p*-values) for the overall model and those corresponding to the *CDS* dummy interaction variables.

		F-Statistics (*p*-value)					
		Without lagged bond returns			With lagged bond returns		
Subsample	Number of observations	Overall model	All interaction variables	Lagged interaction variables	Overall model	All interaction variables	Lagged interaction variables
Panel A: Sample selection criteria 1							
Excluding 2007–2008	58,068	39.50 (0.00)	12.96 (0.00)	3.74 (0.00)	213.13 (0.00)	23.48 (0.00)	16.93 (0.00)
Only 2002–2004	44,718	36.27 (0.00)	9.82 (0.00)	3.11 (0.01)	170.18 (0.00)	17.37 (0.00)	13.52 (0.00)
Excluding 2002–2003	25,598	19.75 (0.00)	3.89 (0.00)	2.79 (0.02)	80.85 (0.00)	5.56 (0.00)	4.75 (0.00)
Only one-year post-CDS	23,703	14.75 (0.00)	3.70 (0.00)	0.38 (0.83)	81.98 (0.00)	6.96 (0.00)	3.34 (0.01)
Only two-years post-CDS	58,462	38.86 (0.00)	13.03 (0.00)	3.78 (0.00)	212.13 (0.00)	23.57 (0.00)	16.95 (0.00)
Only three-years post-CDS	98,981	60.60 (0.00)	19.39 (0.00)	2.53 (0.04)	357.90 (0.00)	37.02 (0.00)	18.33 (0.00)
Unrestricted post-CDS	198,131	96.05 (0.00)	62.51 (0.00)	10.61 (0.00)	556.68 (0.00)	106.32 (0.00)	51.67 (0.00)
Low amount outstanding	14,991	12.19 (0.00)	5.06 (0.00)	3.75 (0.00)	71.23 (0.00)	6.71 (0.00)	7.09 (0.00)
High amount outstanding	43,471	36.96 (0.00)	9.82 (0.00)	3.10 (0.01)	150.44 (0.00)	18.62 (0.00)	11.53 (0.00)
Short vintage	31,385	35.21 (0.00)	6.13 (0.00)	1.92 (0.10)	104.51 (0.00)	7.82 (0.00)	5.40 (0.00)
Long vintage	25,874	13.66 (0.00)	9.71 (0.00)	2.84 (0.02)	122.92 (0.00)	14.10 (0.00)	10.64 (0.00)
Small-size firms	23,589	14.16 (0.00)	7.10 (0.00)	2.87 (0.02)	92.17 (0.00)	11.69 (0.00)	6.76 (0.00)
Large-size firms	34,626	28.32 (0.00)	9.11 (0.00)	3.88 (0.00)	125.31 (0.00)	14.12 (0.00)	11.85 (0.00)
Panel B: Sample selection criteria 2							
Excluding 2007–2008	128,445	43.37 (0.00)	7.63 (0.00)	4.47 (0.00)	159.57 (0.00)	13.39 (0.00)	12.91 (0.00)
Only 2002–2004	91,129	38.44 (0.00)	5.90 (0.00)	3.45 (0.01)	128.70 (0.00)	10.48 (0.00)	10.13 (0.00)
Excluding 2002–2003	69,884	22.86 (0.00)	3.63 (0.00)	2.81 (0.02)	65.65 (0.00)	4.88 (0.00)	4.09 (0.00)
Only one-year post-CDS	60,290	17.71 (0.00)	2.53 (0.01)	0.69 (0.60)	64.71 (0.00)	3.91 (0.00)	1.98 (0.08)
Only two-years post-CDS	130,526	42.90 (0.00)	7.95 (0.00)	4.64 (0.00)	159.45 (0.00)	13.78 (0.00)	13.34 (0.00)
Only three-years post-CDS	213,016	62.55 (0.00)	10.91 (0.00)	4.41 (0.00)	256.66 (0.00)	19.69 (0.00)	17.10 (0.00)
Unrestricted post-CDS	411,148	69.81 (0.00)	32.40 (0.00)	12.50 (0.00)	390.60 (0.00)	42.69 (0.00)	35.94 (0.00)
Low amount outstanding	51,829	10.53 (0.00)	3.93 (0.00)	3.66 (0.01)	65.67 (0.00)	3.98 (0.00)	4.99 (0.00)
High amount outstanding	78,697	38.85 (0.00)	6.61 (0.00)	3.31 (0.01)	102.63 (0.00)	11.13 (0.00)	9.36 (0.00)
Short vintage	68,213	40.87 (0.00)	6.78 (0.00)	2.86 (0.02)	78.09 (0.00)	5.49 (0.00)	3.93 (0.00)
Long vintage	58,355	14.99 (0.00)	7.76 (0.00)	4.06 (0.00)	94.16 (0.00)	7.75 (0.00)	9.42 (0.00)
Small-size firms	51,677	16.47 (0.00)	5.41 (0.00)	3.05 (0.01)	76.62 (0.00)	8.06 (0.00)	5.01 (0.00)
Large-size firms	78,344	29.18 (0.00)	6.38 (0.00)	3.41 (0.01)	91.79 (0.00)	8.76 (0.00)	9.11 (0.00)

interaction variables are not significant when we use only one year of post-CDS data and regressions do not include lagged bond returns. However, the *F*-statistics corresponding to lagged interaction variables are strongly significant in the remaining 13 cases. Even when we use an unrestricted post-CDS horizon, the post-CDS increase in dependence on lagged information set is significant. Therefore, we can reject the hypothesis that the decline in bond efficiency after CDS introduction at firm level is a short-term effect and efficiency improves in the long run.

4.6.4. *Effect of underlying bond liquidity*

Efficiency in price discovery and liquidity are inherently correlated. A thinly traded security is likely to be slow in incorporating new information. For example, Chordia, Sarkar, and Subrahmanyam, 2011 find a strong link between lead–lag relations (autocorrelations) in returns and illiquidity of small-cap equities. Similarly, we could expect a positive relation between the observed findings of post-CDS declines in efficiency and underlying bond illiquidity. Does the introduction of CDS adversely affect the informational efficiency of less liquid bonds while maintaining (or improving) the efficiency of widely traded issues?

We address this question by using two simple proxies of bond liquidity proposed by Houweling et al. (2005): total bond amount outstanding and vintage (bond age). Using median values of amount outstanding and vintage on CDS introduction dates, we classify all observations into low or high amount outstanding and short or long vintage portfolios, and we repeat the joint panel regressions. We find that the *F*-statistics corresponding to lagged interaction variables are large and significant for all four liquidity portfolios. Thus, CDS inception had an adverse impact on the efficiency of bonds irrespective of underlying liquidity.

We formally explore the impact of CDS introduction on bond liquidity in detail in Section 6.

4.6.5. Effect of firm size

Smaller firms are inherently more risky and are less likely to be covered by informed institutional market participants, and their bonds are more likely to be lower rated and less liquid. Thus, along the lines of Chordia, Sarkar, and Subrahmanyam (2011), it could be expected that bonds of smaller firms are less efficient. Is the increased dependence on lagged information following the introduction of CDSs greater for bonds of smaller firms than those of larger firms?

To this end, based on the median values of equity market capitalization in the year of CDS introduction, we classify our sample into small and large firm size portfolios, and we replicate the joint panel tests for the two size-based subsamples. We find that the *F*-statistics corresponding to lagged interaction variables are large and significant for both firm size portfolios. The dependence on lagged information and, hence, inefficiency increases after the introduction of CDSs for bonds of smaller as well as larger firms.

4.6.6. Effect of bond ratings and maturity

Default risk and duration are key determinants of bond returns that also influence bond liquidity; e.g., higher rated and shorter maturity bonds are more liquid. Because efficiency and liquidity are likely correlated, we could expect changes in bond efficiency following the start of CDS trading to depend on bond rating and issue maturity. Are the observed findings of deteriorating post-CDS efficiency more pronounced for lower rated or longer maturity bonds?

Focusing on the subset of bonds that are outstanding on the CDS introduction date (i.e., dropping issues that mature before or are offered after CDS introduction date), we form two S&P and Moody's ratings-based portfolios (investment grade: BBB−/Baa3 and above; junk grade: BB+/Ba1 and below) and three maturity-based portfolios (short term: <7 years; medium term: 7–15 years; long term: >15 years). A vast majority of bonds in our sample are investment grade (90%) and short term (49%). We implement the joint panel regressions for each of the five portfolios. In results not reported for brevity, we again find that the *F*-statistics corresponding to lagged interaction variables are always large and significant. Thus, all our findings are robust to controls for credit and duration risk.

We also implement the difference-in-differences tests (Table 4 regressions) for many of the preceding subsamples: short versus long bond vintage portfolios, small versus large firm size portfolios, investment grade versus junk grade bond portfolios, short versus medium versus long term bond portfolios, and portfolios based on time period (intermediate years 2002–2005 versus extreme years 2002 and 2006–2008). We find the difference-in-differences results robust across all subsamples (results not tabulated): *F*-statistics corresponding to β_8, the key coefficient of interest, is significant in almost all subsamples for different tests specifications (eight regressions for each subsample).

4.7. Speed of price discovery

The results obtained so far could also be interpreted as information spillovers from other markets to the bond market. This could follow from microstructure reasons. Informed traders could prefer the CDS market to bonds. Information first gets into CDS prices and then through common intermediaries (such as bond dealers) and arbitrageurs gets incorporated into bond prices with a delay. The preceding price efficiency analysis, based on the lead–lag structure, concludes that the bond market is less efficient after the inception of CDSs. However, the information could get into bond prices quicker (or earlier) in the post-CDS world than in a scenario in which the CDS market does not exist. Therefore, it is useful to consider the speed of price discovery in our analysis.[14]

Price inefficiency is inferred from *F*-statistics and the *D*1 metric in regressions based on daily data and involving one-day lagged returns. However, empirical tests for speed of price discovery mandate high frequency data and multiple lags in explanatory variables. Because our data are organized as daily observations and bond trades are sporadic in nature, it is difficult to implement rigorous tests for speed of price discovery. Nevertheless, we conduct the following two sets of simple analysis.

First, to establish whether our observed results merely indicate information spillover from the new information channel (CDS) to bond markets or, alternately, reveal deterioration in bond efficiency as inferred, we conduct two tests. (1) We compare the incremental post-CDS significance of lagged CDS returns versus other lagged returns based on the *F*-statistics corresponding to lagged interaction variables in the joint panel regressions of Panels C and D in Table 3. (2) We contrast the pre- and post-CDS *D*1 metric with and without CDS returns in the partitioned panel regressions of Panels A and B in Table 3. The results (not tabulated) reveal that, even though lagged CDS return demonstrates the greatest explanatory power over current bond returns, other lagged variables are relevant. Lagged bond returns and, to some extent, lagged stock, swap, and VIX returns also bear significant impact on current bond returns. Furthermore, the *D*1 metric always goes up following CDS introduction, and post-CDS *D*1 is greater than pre-CDS *D*1 even without the inclusion of lagged CDS returns.[15] We can thus conclude that though there could be information spillover from the emergent CDS market to the bond market, there is also increased self-price discovery (from lagged bond returns) and non-CDS cross-price discovery (from lagged stock, swap, and VIX returns) in the bond market. These indicate an increase in bond inefficiency.

Second, we examine the speed of price discovery by undertaking joint panel regressions based on two-day lags instead of a one-day lag. We use smaller subsamples of data obtained from sample selection criteria 1 and 2 that consist of observations with simultaneous returns for

[14] We thank the anonymous referee for suggesting this alternate perspective.

[15] Bootstrapping tests similar to those in Section 4.2 reveal that differences in pre-CDS and post-CDS *D*1 values are statistically significant.

Table 6
Market quality before and after introduction of credit default swaps (CDSs).

We report market quality of the bond market before and after the introduction of CDSs, separately for bonds of CDS issuers and those of control sample of CDS nonissuers (firms with no CDS introduction). For each bond and each subperiod, we compute Hasbrouck (1993) q measure of market quality as

$$q=\frac{\sigma_e^2-2a\cdot \mathrm{Cov}(e_t,e_{t-1})}{\sigma_e^2+a^2\sigma_e^2-2a\cdot \mathrm{Cov}(e_t,e_{t-1})},$$

where a is the coefficient on a MA(1) process without intercept for bond returns, σ_e^2 is the variance of MA(1) residuals, and $\mathrm{Cov}(e_t,e_{t-1})$ is the covariance of lagged MA(1) residuals.

Panel A employs the full pooled sample of all observations aggregated as a panel. Bonds of the CDS sample are compared with bonds of the pooled control sample (aggregate pool of firms that meet the selection criteria outlined in Appendix A but did not issue any CDSs until the end of 2009). Panel B is based on 82 pairs of individual bonds with at least 30 valid observations in both pre- and post-CDS periods. Each pair includes a bond of a CDS issuer and the closest matching bond (in terms of bond size, rating, maturity and firm size) of a CDS nonissuer. Within each panel, we report the average values of the q measure over a restricted four-year event window that uses only two years each of pre- and post-CDS observations.

For the sample of 82 pairs of individual bonds, subsequent to CDS introduction the value of the q measure decreases for 48 (35) bonds, remains unchanged for one (zero) bond, and increases for 33 (47) bonds of CDS sample (matched control sample).

	CDS sample			Control sample			Difference-
	Pre-CDS mean	Post-CDS mean	Difference (p-value)	Pre-CDS mean	Post-CDS mean	Difference (p-value)	in-differences (p-value)
Panel A: Based on all observations as a panel							
q measure over $[-2,+2]$ years	0.91	0.87	0.04 (0.11)	0.90	0.91	−0.01 (0.72)	
Panel B: Based on 82 pairs of individual bonds							
q measure over $[-2,+2]$ years	0.90	0.88	0.02 (0.42)	0.85	0.92	−0.07 (0.02)	0.09 (0.05)

bonds, stocks, and CDSs, if post-CDS, on three consecutive days. We replicate both the regressions listed in Section 4.3 with two-day lags, and we compute the F-statistics for joint significance of one-day and two-day lagged interaction variables. The results (not tabulated) reveal no difference in pre- and post-CDS speed of price discovery. In both pre- and post-CDS periods, price discovery likely takes place within one day (i.e., only the one-day lagged variables are significant) if lagged bond returns are ignored, and it takes two days or more (i.e., both one-day and two-day lagged variables are significant) if lagged bond returns are included in regressions. Although this finding would be even better supported with the use of high frequency data with multiple lags, our daily analysis reveals no indication that CDS introduction increased the speed of price discovery.

5. Empirical analysis of bond market quality

Section 4 reveals the detrimental impact of CDS introduction on the efficiency of the bond market. Did the inception of CDSs benefit the bond markets via other channels? For example, did CDS trading improve the accuracy of bond prices? In this section we assess the accuracy of bond prices (henceforth, bond market quality) before and after the introduction of CDS trading using the measure proposed in Hasbrouck (1993). Hasbrouck defines pricing error s_t of a security as the difference in its log transaction price (p_t) and its efficient log price (m_t). The return of a security is equal to $r_t=m_t-m_{t-1}+s_t-s_{t-1}$. The variance of pricing error divided by the variance of return (i.e., σ_s^2/σ_r^2) is a metric of normalized pricing error. Market quality q is defined as one minus this ratio, i.e.,

$$q=1-\frac{\sigma_s^2}{\sigma_r^2}. \tag{6}$$

Higher q denotes better market quality, i.e., lower risk of deviation of prices from their efficient levels. The formula is implemented by estimating an MA(1) process (without intercept) for security returns, i.e.,

$$r_t=e_t-a\cdot e_{t-1}, \tag{7}$$

where the values $\{a,\sigma_e^2\}$ are obtained from the MA(1) estimation and then used in the equation for q above. The resulting expression for q is

$$q=\frac{\sigma_e^2-2a\cdot \mathrm{Cov}(e_t,e_{t-1})}{\sigma_e^2+a\sigma_e^2-2a\cdot \mathrm{Cov}(e_t,e_{t-1})}\in(0,1). \tag{8}$$

Details of this derivation are provided in Appendix E, which provides more information about Hasbrouck's model.

The q measure is applied to an aggregated pooled panel of all observations and individual bonds with at least 30 trading days with return observations in both pre- and post-CDS periods. This measure accesses more data than the efficiency regressions because it does not require concurrent data across markets (i.e., stock returns, CDS spreads, Treasury returns, or volatility data). It also does not mandate the existence of lagged bond returns for each selected observation. For example, when individual bonds are considered, the total number of bonds meeting our screening criteria is 82, more than the 45 bonds that are available using sample selection criteria 1.

The values of q measure are presented in Table 6. Panel A of Table 6 employs the full pooled sample of all observations lined up as a panel irrespective of identity of the bond, and the pooled control sample is used as the benchmark sample. Panel B is based on 82 pairs of individual bonds with at least 30 pre- and post-CDS observations. Each pair includes a bond of a CDS issuer and the closest matching bond from the matched control sample. In each panel, we compute the bond market quality measure over a restricted four-year event window that uses only two years each of pre- and post-CDS observations. For bonds of CDS issuers and control sample bonds, we report the pre- and post-CDS mean values of the q measure and the pre-minus-post differences in mean q. Panel B also reports the value of difference-in-differences. The significance of this number indicates whether post-CDS changes in the values of q within the event sample are significantly different from post-treatment changes in value within the control sample.[16]

For the pooled panel data of all observations, CDS introduction has little or weak detrimental impact on the market quality of underlying bonds. Quality of bonds of CDS issuers decreases from 0.91 to 0.87 and that of control sample bonds slightly increases from 0.90 to 0.91. However, these differences are not significant. When the sample of 82 pairs of individual bonds are considered, the quality of bonds of CDS issuers declines from 0.90 to 0.88. This decline in quality of bonds of CDS issuers is not significant in isolation, but the quality of bonds of CDS nonissuers increases substantially from 0.85 to 0.92. Consequently, as the difference-in-differences value reveals, on a comparative basis, CDS introduction appears to have a detrimental impact on the market quality of the underlying bonds.

When we track the changes in the value of q for each pair of individual bonds, we find that a greater fraction of bonds of CDS issuers experience a post-CDS decline in the value of q, whereas a larger fraction of matched control sample bonds demonstrate an increase in the value of q. For example, in the sample of 82 pairs of matched individual bonds, subsequent to the CDS introduction the value of q decreases for 48 bonds of CDS issuers and increases for 33 bonds. In contrast, the value of q decreases for 35 matched control sample bonds and increases for 47 bonds.

Thus, we find no evidence that bond market quality (or the accuracy of bond prices) has improved after the inception of CDS market. In fact, market quality likely declined.[17]

6. Empirical analysis of bond liquidity

A likely consequence of CDS trading is that fixed-income traders no longer need to use bond markets to speculate on or hedge credit risk. The evidence in Figs. 1 and 2 shows changes in bond trading patterns after the introduction of CDS trading. Indication exists of post-CDS drop-off in both trading volume and turnover. The question is: Did liquidity in the bond market also suffer following CDS introduction?

To assess this, we compute multiple measures that are either proxies for liquidity or could be highly correlated to liquidity. We undertake the analysis of these measures as a panel. All observations, irrespective of identity of the bond, are lined up as a panel with respect to the CDS introduction date. These results are shown in Table 7, Panel A. The pooled control sample is used as the benchmark sample in Panel A. The analysis of liquidity for individual bonds is shown in Table 7, Panel B. This sample consists of 82 individual bonds with at least 30 observations of returns in pre- and post-CDS periods (details in Section 5). Each bond is paired with the closest matching bond from the matched control sample, and the reported DID t-statistics indicate whether the post-CDS changes in the values of liquidity for bonds of CDS issuers are significantly different from the post-treatment changes in value for the matched control sample bonds.

We compute the following measures of bond liquidity and price impact.[18]

1. A simple count of the number of trades. We report the total number of trades over the entire pre- or post-CDS period, as well as the average number of trades per day (excluding and including zero trade days).
2. The dollar volume of trading, in millions of dollars. We compute the total volume over the entire period and the mean trade size per day and per transaction. Fig. 1 plots the trend of the mean size of each trade for the pooled sample of CDS issuers and the pooled control sample of CDS nonissuers.
3. Turnover, defined as the trading volume as a percentage of the outstanding amount of the bond issue. Again, we report the total (full period), mean daily, and mean per trade values. Fig. 2 plots the trend of mean turnover per transaction for the pooled samples of CDS issuers and nonissuers.[19]
4. The LOT measure of Lesmond, Ogden, and Trzcinka (1999). We use the Das and Hanouna (2010) adaptation of the LOT measure and compute three versions of this measure separately for pre- and post-CDS periods: (1) fraction of zero return trading days, (2) fraction of zero volume (i.e., no trade) trading days, and (3) fraction of zero return plus zero volume trading days. The total number of trading days in the entire pre- or post-CDS period constitutes the denominator of these fractions. Because nontrading days are included, the selection criteria for individual bonds is relaxed and the LOT measures reported in Panel B of Table 7 involve 257 pairs of individual bonds.
5. The Amihud (2002) illiquidity measure. It is computed as

$$\text{Amihud Illiquidity}_i = \frac{1}{DAY\ S_i} \sum_{t=1}^{DAY\ S_i} \frac{|bndret_{it}|}{\$VOL_{it}} \times 10^6, \qquad (9)$$

[16] Because Panel A involves pooled unmatched data, difference-in-differences analysis cannot be meaningfully conducted for Panel A.

[17] For comparison with the bond markets, we also compute the market quality measure (both before and after CDS introduction) for equities corresponding to the bonds in our data set. We find that, on average, the q measure is 0.98 in the pre-CDS period and 0.99 in the post-CDS period (the difference is not statistically significant). Similarly, when we examine the post-introduction quality of the CDS market itself, we obtain an average of 0.92 for the q measure. Therefore, the quality of equities is much higher than that of bonds as well as CDSs. The quality of the CDS market compares favorably with that of the underlying bond market.

[18] We thank the anonymous referee for suggesting some of these measures.

[19] The figures are based on data organized as continuous time series when zero trade days are included. Panel A of Table 7 reports trade size and turnover using discrete panel data that exclude zero trade days.

Table 7
Bond liquidity attributes before and after introduction of credit default swaps (CDSs).

We report the values of various bond liquidity and price impact metrics before and after the introduction of CDSs, separately for bonds of CDS issuers and control sample bonds of CDS nonissuers. Panel A employs the full pooled sample of all observations aggregated as a panel. All observations for CDS issuers are augmented with the pooled control sample, which consists of all bond transactions for firms that meet the selection criteria outlined in Appendix A but did not issue any CDSs until the end of 2009. Panel B is based on 82 pairs of individual bonds [257 pairs for the relaxed LOT (Lesmond, Ogden, and Trzcinka, 1999) measure] with at least 30 valid observations in both pre- and post-CDS periods. Each pair includes a bond of a CDS issuer and the closest matching bond (in terms of bond size, rating, maturity and firm size) of a CDS nonissuer. Tests for difference between means and medians are based on t-test and Wilcoxon (Mann-Whitney) rank-sum test, respectively. We compute the following liquidity attributes for all bonds in CDS sample and both control samples. Every measure is computed over a four-year window using two years each of pre- and post-CDS observations.

1. Number of trades.
2. Trading volume in millions of dollars.
3. Turnover = trading volume as a percentage of outstanding amount.
4. LOT zeros measure: based on Das and Hanouna (2010) adaptation of Lesmond, Ogden, and Trzcinka (1999) measure; computed as frequency of zero return and zero volume trading days as a fraction of total number of trading days.
5. Amihud illiquidity measure: based on Amihud (2002).

$$\text{Amihud Illiquidity}_i = \frac{1}{DAYS_i}\sum_{t=1}^{DAYS_i}\frac{|bndret_{it}|}{\$VOL_{it}} \times 10^6.$$

6. Roll impact illiquidity measure: based on Roll (1984) and Goyenko, Holden, and Trzcinka (2009).

$$\text{Roll Estimator}_i = \begin{cases} \sqrt{-(\text{Cov}(\Delta P_{it}, \Delta P_{i,t-1}))} & \text{if Cov}(\Delta P_{it}, \Delta P_{i,t-1}) < 0 \\ 0 & \text{otherwise} \end{cases}$$

and

$$\text{Roll Impact}_i = \frac{10^6 \times \text{Roll Estimator}_i}{(\sum_{t=1}^{DAYS_i}\$VOL_{it})/DAYS_i}.$$

$bndret_{it}$ is the ith bond's return on day t, P_{it} is the daily mean bond price, $\$VOL_{it}$ is the total daily trading volume in dollars, and $DAYS_i$ is the total number of trading days in the entire pre- or post-CDS period.
* indicates measures based on 257 pairs of individual bonds.

Panel A: Based on all observations as a panel

Liquidity measure	CDS sample Pre-CDS mean	CDS sample Post-CDS mean	CDS sample Difference (p-value)	Pooled control sample Pre-CDS mean	Pooled control sample Post-CDS mean	Pooled control sample Difference (p-value)
Number of trades						
Total	232.78	478.22	−245.43 (0.00)	389.17	642.24	−253.08 (0.00)
Daily (excluding zero trade days)	2.45	3.04	−0.60 (0.00)	3.35	3.41	−0.06 (0.81)
Daily (including zero trade days)	1.11	1.81	−0.70 (0.00)	1.95	1.84	0.11 (0.68)
Trading volume (in millions of dollars)						
Total	97.36	202.76	−105.40 (0.00)	126.16	218.39	−92.22 (0.00)
Daily	1.24	1.45	−0.21 (0.06)	1.40	1.29	0.11 (0.34)
Per trade	0.55	0.55	−0.00 (0.95)	0.48	0.41	0.07 (0.02)
Turnover (as percent of outstanding)						
Total	35.28	54.28	−19.00 (0.00)	29.52	46.78	−17.26 (0.00)
Daily	0.68	0.55	0.12 (0.03)	0.55	0.57	−0.02 (0.74)
Per trade	0.30	0.24	0.06 (0.06)	0.28	0.28	0.00 (0.87)
LOT zeros (as fraction)						
Zero return days	0.11	0.11	0.00 (0.35)	0.11	0.11	−0.00 (0.80)

Table 7 (*continued*)

Panel A: Based on all observations as a panel

Liquidity measure	CDS sample Pre-CDS mean	CDS sample Post-CDS mean	CDS sample Difference (p-value)	Pooled control sample Pre-CDS mean	Pooled control sample Post-CDS mean	Pooled control sample Difference (p-value)
Zero volume days	0.75	0.71	0.05 (0.00)	0.73	0.72	0.01 (0.18)
Zero return+zero volume days	0.86	0.82	0.05 (0.00)	0.84	0.83	0.01 (0.17)
Amihud illiquidity						
Mean	39.55	39.93	−0.38 (0.98)	46.47	51.22	−4.75 (0.74)
Median	9.08	9.35	(0.42)	9.20	9.64	(0.60)
Roll impact (illiquidity)						
Mean	3.34	3.09	0.25 (0.75)	3.46	3.76	−0.30 (0.63)
Median	0.92	1.17	(0.44)	1.72	1.66	(0.62)

Panel B: Based on 82 pairs of individual bonds

Liquidity measure	CDS sample Pre-CDS mean	CDS sample Post-CDS mean	CDS sample Difference (p-value)	Matched control sample Pre-CDS mean	Matched control sample Post-CDS mean	Matched control sample Difference (p-value)	Difference-in-differences (p-value)
Number of trades							
Total	737.65	1113.29	−375.64 (0.02)	1119.00	1202.51	−83.51 (0.78)	−292.13 (0.11)
Daily (excluding zero trade days)	3.92	3.66	0.26 (0.59)	6.99	4.46	2.53 (0.19)	−2.27 (0.12)
Daily (including zero trade days)	2.62	2.50	0.12 (0.80)	5.68	2.83	2.85 (0.13)	−2.73 (0.07)
Trading volume (in millions of dollars)							
Total	228.41	320.48	−92.07 (0.12)	376.49	318.18	58.31 (0.55)	−150.38 (0.23)
Daily	1.69	1.34	0.35 (0.12)	2.76	1.57	1.19 (0.02)	−0.84 (0.01)
Per trade	0.47	0.37	0.11 (0.08)	0.44	0.43	0.01 (0.93)	0.10 (0.21)
Turnover (as percent of outstanding)							
Total	69.38	67.97	1.41 (0.90)	49.81	67.52	−17.71 (0.08)	19.12 (0.09)
Daily	0.53	0.42	0.11 (0.00)	0.42	0.34	0.08 (0.08)	0.03 (0.04)
Per trade	0.13	0.10	0.03 (0.07)	0.13	0.15	−0.02 (0.36)	0.04 (0.23)
LOT zeros (as fraction)*							
Zero return days	0.11	0.11	−0.00 (0.85)	0.11	0.11	−0.00 (0.57)	0.00 (0.64)
Zero volume days	0.58	0.59	−0.01 (0.63)	0.60	0.62	−0.02 (0.42)	0.01 (0.74)
Zero return+zero volume days	0.70	0.71	−0.01 (0.65)	0.71	0.74	−0.02 (0.39)	0.01 (0.63)
Amihud illiquidity							
Mean	9.68	20.19	−10.51 (0.04)	7.70	38.91	−31.21 (0.07)	20.70 (0.89)
Median	6.01	8.32	(0.02)	3.13	7.66	(0.00)	
Roll impact (illiquidity)							
Mean	2.67	2.57	0.10 (0.93)	1.80	1.39	0.41 (0.46)	−0.31 (0.66)
Median	0.97	0.84	(0.82)	0.63	0.68	(0.75)	

where $bndret_{it}$ is the ith bond's return on day t, $\$VOL_{it}$ is the total daily trading volume in dollars, and $DAYS_i$ is the total number of trading days in the entire pre- or post-CDS period.

6. The Roll impact illiquidity measure. This is an extended Amihud proxy measure recommended by Goyenko, Holden, and Trzcinka (2009) for low frequency data. It is based on Roll (1984) spread illiquidity estimator and is computed as

$$\text{Roll Estimator}_i = \begin{cases} \sqrt{-(\mathrm{Cov}(\Delta P_{it}, \Delta P_{i,t-1}))} & \text{if } \mathrm{Cov}(\Delta P_{it}, \Delta P_{i,t-1}) < 0 \\ 0 & \text{otherwise} \end{cases} \tag{10}$$

and

$$\text{Roll Impact}_i = \frac{10^6 \times \text{Roll Estimator}_i}{(\sum_{t=1}^{DAY\ S_i} \$VOL_{it})/DAY\ S_i}, \tag{11}$$

where P_{it} is the daily mean bond price on day t, $\$VOL_{it}$ is the total daily trading volume in dollars, and $DAY\ S_i$ is the total number of trading days in the entire pre- or post-CDS period.

For bonds of CDS issuers as well as for pooled and matched control sample bonds, all these measures are computed over a four-year ($[-2,+2]$ years) event window. The reported pre- and post-CDS values are based on two years of observations each. The covariances for the Roll spread estimator are computed on a trading day basis and not on a calendar time basis (i.e., day $t-1$ is the previous trading day prior to the day t observation, ignoring interim zero trade days, holidays, and weekends). The Amihud and Roll impact illiquidity measures are skewed (as inferred from the mean-to-median ratios), so we report the mean as well as median values for these two measures. The tests for difference between means are based on t-tests and between medians on Wilcoxon (Mann-Whitney) rank-sum tests.

Table 7 reveals that results are mixed as to whether the inception of CDS markets affected bond liquidity.

The following measures indicate that liquidity of bonds of CDS issuers deteriorated subsequent to the introduction of CDSs relative to the control sample bonds: daily and per trade turnover for the pooled sample of all observations in Panel A; and daily and per trade turnover for the 82 individual bonds in Panel B. In contrast, the following measures support the likelihood that liquidity of bonds of CDS issuers probably improved in the post-CDS period when compared with the control sample bonds: number of daily trades, daily and per trade trading volume, and LOT zeros for the pooled sample of all observations in Panel A; number of daily trades and daily trading volume for the 82 individual bonds in Panel B.

For the remaining liquidity (trading and price impact) attributes, there is no conclusive inference. Either the pre-CDS and post-CDS values are not significantly different for the bonds of CDS issuers or post-treatment trends observed for the bonds of CDS issuers are not appreciably different from similar trends for control sample bonds of nonissuers.[20]

Overall, no definite or conclusive evidence shows that CDS introduction improved the liquidity of the bonds underlying the CDS entity.

7. How CDS introduction impacts bonds

What explains our results? One possible explanation for the decline in efficiency and quality of bond markets subsequent to CDS introduction is the likely migration of large institutional traders from trading bonds to trading CDSs to implement their credit views. As seen in Fig. 1, the mean trade size drops in the two years after CDS introduction, signifying that the large institutional traders could have moved from trading bonds to trading CDSs. Fig. 2 supports this likely demographic shift by indicating a reduction in bond turnover after CDS introduction.

Since its inception, the CDS market is primarily dominated by institutional participants (Avellaneda and Cont, 2010). The gross notional CDS amount was reported to be \$25.5 trillion on December 31, 2010 (source: International Swaps and Derivatives Association). According to Chen, Fleming, Jackson, Li, and Sarkar (2011), 14 key financial institutions constitute 78% of the population of CDS protection buyers and 85% of protection sellers. These institutions undertake exposures to CDSs for hedging, trading, portfolio balancing, balance sheet management, and speculation. Because underlying corporate debt or loans might not be traded actively, institutions use CDS markets to incur synthetic exposures to the debt market.

To explore this issue, we determine the extent of trading by institutions before and after CDS introduction. We track likely shifts in institutional trading in two ways. First, we examine the institutional trades in the TRACE database. In TRACE, institutional trades are identified by the size of the disseminated quantity of a bond issue in a completed trade transaction. Trades with reported par values of \$5 million and above for investment grade (Baa3/BBB− and above) bonds or reported par values of \$1 million and above for below investment grade (Ba1/BB+ and below) or unrated bonds are classified as institutional trades.[21] Institutional trades are relatively

[20] We also compute four additional measures of bond liquidity and price impact.

1. The zeros impact illiquidity measure used by Goyenko, Holden, and Trzcinka (2009).
2. The Amivest liquidity measure (reciprocal of the Amihud measure) used by Goyenko, Holden, and Trzcinka (2009).
3. The Roll (1984) spread illiquidity estimator.
4. The Bao, Pan, and Wang (2011) covariance illiquidity gamma.

Based on all four of these measures as well, no evidence exists that liquidity of bonds improved after CDS introduction (results not tabulated). In fact, when all 10 liquidity measures (six tabulated and four unreported) are considered, more liquidity attributes deteriorated than improved after the inception of CDS markets.

[21] Our motivation for using separate cutoffs based on bond ratings arises from evidence that mean trade size of institutional trades for investment grade bonds are significantly larger than those for below investment grade bonds (e.g., Ellul, Jotikasthira, and Lundblad, 2011). In an alternate classification, we redefine all transactions with trade size

rare. The (unfiltered) TRACE sample of completed trades for 2002–2009 consists of 5,768,201 time series (bond issues $\times$ trading days) observations. Of these, 704,612 (12.22%) are institutional trade observations. Alternatively, in our final filtered sample of 1,365,381 time series observations, whereas 507,605 days (37.18%) report at least one valid bond trade, only 46,234 days (3.39%) have one or more institutional trades. We analyze the TRACE institutional transactions for bonds of CDS sample and both the control samples over a four-year ($[-2,+2]$ years) window.

Next, we inspect the trades in the National Association of Insurance Commissioners (NAIC) database, which lists bond transactions by all insurance companies (life insurance companies, property and casualty insurance companies, and health maintenance organizations).[22] Using NAIC data that span the period 1994–2007, we obtain 76,703 trades (39,132 pre-CDS and 37,571 post-CDS) by insurances companies in the four-year ($[-2,+2]$ years) window surrounding the introduction of CDSs for 1,379 bonds of CDS-issuing firms. We also collect the corresponding trades by insurances companies over $[-2,+2]$ years window for pooled and matched control sample bonds of CDS nonissuers.

Table 8 reports the number, volume, and turnover of TRACE institutional bond trades as a percentage of all bond trades; the LOT measures corresponding to TRACE institutional bond trades; and the number, volume, and turnover of bond trades by insurance companies in the NAIC database. Panel A presents the statistics for all observations, irrespective of identity of the bond, grouped as panel data, and Panel B focuses on 82 individual bonds (257 for the relaxed LOT measures) with at least 30 observations of returns in pre- and post-CDS periods. The pooled control sample is used as the benchmark sample in Panel A; the matched control sample, in Panel B.

An examination of Panels A and B of Table 8 reveals a relative decline in institutional bond trades post-CDS. Panel A, for example, shows that proportional institutional trading volume and turnover stay unchanged for the CDS sample, whereas they go up in the control sample. Panel B affirms the more pronounced drop in institutional trades for the CDS sample compared with the control sample as suggested by the DID values. The LOT measures for institutional trades relatively increase post-CDS for the CDS issuers more than that for the control sample, suggesting that liquidity in this segment of the bond markets dwindled. Further, both Panels A and B highlight that the transactions by insurance companies in the CDS sample witnessed a steeper decline in trading volume and turnover compared with the control sample.

In short, it appears likely that a demographic shift in bond trading is a driver of the empirical results we obtain.

In addition, we implement the liquidity tests adopted by Bessembinder, Maxwell, and Venkataraman (2006), who estimate an effective spread measure for signed insurance company trades and decompose price changes into informational and non-informational components. For the sample of bonds by CDS issuers and the pooled control sample of bonds of CDS nonissuers, we obtain trades by insurance companies from the NAIC database in the four-year ($[-2,+2]$ years) window surrounding the event date. For each transaction at time t, we collect the transaction price (P_t) and an order flow indicator variable (Q_t) that has a value of $+1$ for buyer-initiated trades and -1 for seller-initiated trades. We implement the two-stage estimation model of Bessembinder, Maxwell, and Venkataraman (2006) as follows:

(I) The first stage employs the following first-order autocorrelation model for order flows:

$$Q_t = a + bQ_{t-1} + \varepsilon_t, \tag{12}$$

where $t-1$ is the day of last trade prior to the current transaction for the same bond. The residual from the estimate (ε_t) is denoted as Q_t^*, the surprise in order flow.

(II) The second stage involves the regression of prices changes on changes in order flow, surprise in order flow, and three control variables:

$$\begin{aligned}\Delta P_t = {} & \theta + \beta_1 tryret_t + \beta_2 stkret_t + \beta_3 \Delta DEF_t \\ & + \gamma_0 Q_t^* + \gamma_1 Q_t^* \times CDS_t \\ & + \alpha_0 \Delta Q_t + \alpha_1 \Delta Q_t \times CDS_t + \omega,\end{aligned} \tag{13}$$

where control variables $tryret_t$, $stkret_t$, and ΔDEF_t denote daily matching-maturity swap return, stock return, and return on the default spread (BAA yield minus 10-year swap yield), respectively. CDS_t equals one if post-CDS period and zero if pre-CDS period.

Both regressions are estimated using a weighted least squares (WLS) approach in which the weights are the inverse of the elapsed time (in days, plus one) between the trades at $t-1$ and t. The coefficient on Q_t^* captures the information component of the bid–ask spread and indicates the effect of private information from order flows on bond prices. The coefficient on ΔQ_t reflects the non-information portion of the half spread and estimates one-way trade execution costs (which include inventory and order processing costs, as well as possible economic rents) for institutional bond trades. We interact both these variables with the CDS dummy variable (equals zero if pre-CDS and equals one if post-CDS) to capture the incremental roles of these two components of bid–ask spread following CDS introduction. Panel C of Table 8 reports the two-stage results separately for CDS sample and pooled control sample.

For the informational component of bid–ask spreads, we find no change in the role of private information on the price evolution of bonds for CDS issuers ($\gamma_1 = 0$), but the effect of private information decreases for the control sample bonds ($\gamma_1 < 0$). For the non-informational portion of bid–ask spreads, the post-CDS trade execution costs increase for bonds of CDS issuers as well as control sample bonds ($\alpha_1 > 0$). The observed increase in trade execution costs reconciles with the decrease

(*footnote continued*)
greater than \$1 million, irrespective of ratings, as institutional trades and redo Table 8. We find that all conclusions remain unchanged.

[22] Campbell and Taksler (2003) report that insurance companies account for about one-third of all institutional bond holdings.

Table 8
Institutional trades before and after introduction of credit default swaps (CDSs).

The table presents the analysis of pre- and post-CDS trades by institutions and by insurance companies for bonds of CDS issuers as well as control sample of nonissuers. Trades by institutions are obtained from Trade Reporting and Compliance Engine (TRACE). Completed trades with reported par values of \$5 million and above for investment grade (Baa3/BBB− and above) bonds or reported par values of \$1 million and above for below investment grade (Ba1/BB+ and below) or unrated bonds are classified as institutional trades. The National Association of Insurance Commissioners (NAIC) database lists bond transactions by all insurance companies. Panels A and B report the values of different institutional liquidity measures. LOT measure is based on Das and Hanouna (2010) adaptation of Lesmond, Ogden, and Trzcinka (1999) measure and is computed as frequency of zero return and zero volume trading days as a fraction of total number of trading days. Panel A employs the full pooled sample of all observations aggregated as a panel. Bonds of the CDS sample are compared with those constituting the pooled control sample aggregating all bond issues by firms that meet the selection criteria outlined in Appendix A but did not issue any CDSs until the end of 2009. Panel B is based on 82 pairs of individual bonds (257 pairs for the relaxed LOT measure) with at least 30 valid observations in both pre- and post-CDS periods. Each pair includes a bond of a CDS issuer and the closest matching bond (in terms of bond size, rating, maturity and firm size) of a CDS nonissuer. All reported measures (for CDS sample and both control samples) are computed over a four-year window using two years each of pre- and post-CDS observations. Panel C implements the following two-stage regression model for effective NAIC spreads along the lines of Bessembinder, Maxwell, and Venkataraman (2006):

(I) First-order autocorrelation model for order flows: $Q_t = a + bQ_{t-1} + \varepsilon_t$.
(II) Regression of prices changes: $\Delta P_t = \theta + \beta_1 tryret_t + \beta_2 stkret_t + \beta_3 \Delta DEF_t + \gamma_0 Q_t^* + \gamma_1 Q_t^* \times CDS_t + \alpha_0 \Delta Q_t + \alpha_1 \Delta Q_t \times CDS_t + \omega$.

For each bond, P_t is the NAIC transaction price at time t and Q_t is the time t NAIC order flow indicator variable that has a value of $+1$ for buyer-initiated trades and -1 for seller-initiated trades. Q_t^* is the residual (ε_t) from Stage I estimate and $\Delta Q_t = Q_t - Q_{t-1}$. Controls $tryret_t$, $stkret_t$, and ΔDEF_t denote daily matching-maturity swap return, stock return, and return on the default spread (BAA yield minus 10-year swap yield), respectively. CDS_t equals one if post-CDS period and zero if pre-CDS period. Both regressions are implemented over $[-2,+2]$ years window and are estimated using weighted least squares approach in which the weights are the inverse of the elapsed time (in days, plus one) between the trades at $t-1$ and t.

* indicates measures based on 257 pairs of individual bonds.

Panel A: Values of institutional liquidity measures, based on all observations as a panel

	CDS sample			Pooled control sample		
Liquidity measure	Pre-CDS mean	Post-CDS mean	Difference (*p*-value)	Pre-CDS mean	Post-CDS mean	Difference (*p*-value)
Institutional trades as a percent of all trades						
Number of trades	6.14	5.13	1.01 (0.20)	6.75	7.72	−0.97 (0.18)
Trading volume	22.55	22.81	−0.26 (0.84)	20.63	23.92	−3.29 (0.02)
Turnover	22.55	22.81	−0.26 (0.84)	20.63	23.92	−3.29 (0.02)
LOT measure of institutional trades						
Zero return days	0.007	0.010	−0.003 (0.22)	0.010	0.008	0.002 (0.21)
Zero volume days	0.960	0.971	−0.011 (0.01)	0.959	0.967	−0.007 (0.05)
Zero return+zero volume days	0.966	0.982	−0.015 (0.01)	0.969	0.975	−0.006 (0.10)
Trades by insurance companies						
Total number of trades	28.48	26.76	1.72 (0.27)	30.22	29.94	0.28 (0.80)
Number of trades per month	1.68	1.32	0.36 (0.00)	1.63	1.37	0.26 (0.00)
Total trading volume (millions of dollars)	56.14	45.76	10.38 (0.00)	80.32	74.82	5.49 (0.21)
Trading volume per month (millions of dollars)	3.70	2.21	1.49 (0.00)	4.46	3.43	1.03 (0.00)
Total turnover (as percent)	20.29	15.80	4.49 (0.00)	26.10	21.52	4.58 (0.00)
Turnover per month (as percent)	1.32	0.82	0.50 (0.00)	1.47	1.06	0.41 (0.00)

Panel B: Values of institutional liquidity measures, based on 82 pairs of individual bonds

	CDS sample			Matched control sample			Difference-
Liquidity measure	Pre-CDS mean	Post-CDS mean	Difference (*p*-value)	Pre-CDS mean	Post-CDS mean	Difference (*p*-value)	in-differences (*p*-value)
Institutional trades as a percent of all trades							
Number of trades	6.15	4.08	2.07 (0.11)	6.23	5.97	0.26 (0.85)	1.81 (0.09)
Trading volume	27.45	22.54	4.91 (0.09)	26.63	30.20	−3.57 (0.22)	8.48 (0.01)
Turnover	27.45	22.54	4.91 (0.09)	26.63	30.20	−3.57 (0.22)	8.48 (0.01)
LOT measure of institutional trades*							
Zero return days	0.007	0.010	−0.003 (0.22)	0.009	0.008	0.001 (0.49)	−0.004 (0.12)
Zero volume days	0.924	0.952	−0.028 (0.11)	0.953	0.961	−0.008 (0.42)	−0.021 (0.08)
Zero return+zero volume days	0.931	0.961	−0.031 (0.07)	0.962	0.969	−0.007 (0.46)	−0.025 (0.04)
Trades by insurance companies							

Table 8 (*continued*)

Panel B: Values of institutional liquidity measures, based on 82 pairs of individual bonds

Liquidity measure	CDS sample			Matched control sample			Difference-in-differences
	Pre-CDS mean	Post-CDS mean	Difference (p-value)	Pre-CDS mean	Post-CDS mean	Difference (p-value)	(p-value)
Total number of trades	50.02	31.56	18.46 (0.00)	47.78	35.66	12.12 (0.05)	6.35 (0.06)
Number of trades per month	2.75	1.51	1.24 (0.00)	3.61	2.69	0.92 (0.00)	0.32 (0.11)
Total trading volume (millions of dollars)	105.32	65.22	40.10 (0.01)	72.31	49.68	22.63 (0.05)	17.47 (0.07)
Trading volume per month (millions of dollars)	6.34	2.99	3.35 (0.00)	6.29	3.37	2.92 (0.08)	0.43 (0.16)
Total turnover (as percent)	27.17	15.60	11.57 (0.00)	20.76	15.95	4.81 (0.09)	6.76 (0.05)
Turnover per month (as percent)	1.60	0.72	0.88 (0.00)	1.75	0.95	0.80 (0.01)	0.08 (0.54)

Panel C: Two-stage regression of effective NAIC spreads, based on all observations as a panel

	CDS sample			Pooled control sample		
	(1)	(2)	(3)	(1)	(2)	(3)
Stage I coefficients (p-values)						
Q_{t-1}	0.023 (0.00)	0.023 (0.00)	0.023 (0.00)	0.113 (0.00)	0.113 (0.00)	0.113 (0.00)
Intercept	−0.116 (0.00)	−0.116 (0.00)	−0.116 (0.00)	−0.136 (0.00)	−0.136 (0.00)	−0.136 (0.00)
Adjusted R^2	0.001	0.001	0.001	0.012	0.012	0.012
Stage II coefficients (p-values)						
$tryret_t$	0.058 (0.00)	0.057 (0.00)	0.057 (0.00)	0.271 (0.00)	0.267 (0.00)	0.268 (0.00)
$stkret_t$	0.151 (0.00)	0.152 (0.00)	0.152 (0.00)	0.081 (0.00)	0.076 (0.00)	0.078 (0.00)
ΔDEF_t	−0.002 (0.70)	−0.002 (0.76)	−0.002 (0.76)	−0.129 (0.00)	−0.130 (0.00)	−0.131 (0.00)
Q_t^*	0.004 (0.00)	0.004 (0.00)	0.004 (0.00)	−0.001 (0.00)	−0.001 (0.00)	0.002 (0.00)
$Q_t^* \times CDS$			0.000 (0.90)			−0.005 (0.00)
ΔQ_t	0.006 (0.00)	0.003 (0.00)	0.004 (0.00)	0.003 (0.00)	−0.001 (0.04)	−0.002 (0.00)
$\Delta Q_t \times CDS$		0.004 (0.00)	0.004 (0.00)		0.005 (0.00)	0.006 (0.00)
Intercept	0.010 (0.00)	0.010 (0.00)	0.010 (0.00)	−0.001 (0.00)	−0.001 (0.00)	−0.001 (0.00)
Adjusted R^2	0.046	0.048	0.048	0.071	0.077	0.078

in trading activity by insurance companies shown in Panels A and B of Table 8. Hence, we confirm that introduction of CDSs increased bond illiquidity for institutional transactions as indicated by the effective spread measure of Bessembinder, Maxwell, and Venkataraman (2006).

8. Conclusions and discussion

The credit default swap market was one of the salient new markets of the past decade. Trading in CDS has been blamed for the speculative frenzy leading to the beginning of the financial crisis in 2008, though Stulz (2010) concludes that credit default swaps were not responsible for causing or worsening the crisis. Nobelist Joseph Stiglitz went so far as to suggest that CDS trading by large banks should be banned.[23] Still, the creation of new markets could have beneficial information and liquidity effects on underlying markets. Conrad (1989) and Skinner (1989) show that options trading reduced volatility in underlying equity markets. In sovereign bond markets, Ismailescu and Phillips (2011) provide evidence that the introduction of credit default swaps improved efficiency in the underlying sovereign bonds.

We examine whether CDS trading was beneficial to bonds in reference names by looking at whether informational efficiency, market quality, and liquidity improved once CDS trading commenced. Our econometric specification accounts for information across CDS, bond, equity, and volatility markets. We also develop a novel methodology to utilize all observations in our data set even when continuous daily trading is not evidenced, because bonds trade much less frequently than equities. The empirical evidence suggests that the advent of CDS was largely detrimental to secondary bond markets. Bond markets became less efficient relative to other securities and evidenced greater pricing errors and lower liquidity. These findings are robust to various slices of the data set and specifications of our tests. Our findings have bearings on the recent CDS market regulatory reform proposals and the debate surrounding the impact and usefulness of CDS markets.

Whereas we examine bond market efficiency, quality, and liquidity, this research did not examine the effect on credit, i.e., the impact on the quality of firms that experienced CDS introduction. Our endogeneity corrections did note that bond returns are negatively related to the implicit probability of CDS introduction, complementing the comprehensive analysis of this issue in Subrahmanyam, Tang, and Wang (2011). Other open questions remain that are not considered in this paper. Does CDS trading make forecasting default easier for reference names than for firms on which no CDS trades? How do capital structures change for firms that have CDSs traded

[23] Reported by Bloomberg, October 12, 2009: "Stiglitz Says Banks Should Be Banned From CDS Trading," by Ben Moshinsky.

versus firms with no CDSs? How does ratings volatility change when CDSs are introduced? Are firms that have CDSs traded more likely or less likely to have securitized debt? And, eventually, how does the trading of CDSs on centralized exchanges change the information environment for CDSs and bonds? These issues and questions are left for future research.

Appendix A. Bond sample construction

The project data come from four sources: corporate bonds (TRACE and FISD), stocks (CRSP), CDS (Bloomberg), and swap rates and VIX (Datastream).

Step 1: *TRACE data*. We start with the Trade Reporting and Compliance Engine bond transaction database, which lists all over-the-counter secondary market bond transactions since July 2002 by all brokers or dealers who are member firms of Financial Industry Regulatory Authority (FINRA). We collect transaction information such as trade date, trade price, trade size, and underlying yield corresponding to all bond transactions between July 1, 2002 and September 30, 2009. Because TRACE reports multiple intraday bond transactions, for each bond we aggregate all intraday transactions into a single summary transaction observation each day. For each bond transaction date, the aggregated observation consists of number of trades; mean and total trading size; and mean, median, and closing (last) daily yields and prices.

We impose certain screening criteria on the sample of bond transactions. We exclude transactions identified as trade cancelations or corrections, when-issued trades, trades with commissions, as-of-trades, special price trades, and trades with sale conditions. The screened sample consists of 5,768,201 transaction date observations for 34,900 bond issues by 4,869 firms.

Step 2: *FISD data*. Separately, from Mergent's Fixed Investment Securities Database, which includes in depth issue- and issuer-related information on all US debt securities maturing in 1990 or later, we collect issuance-related information such as issuance date, maturity date, offer amount, and other related variables for all bonds issued between 1994 and 2007. From dynamic FISD tables, we extract bond ratings and amount outstanding on the transaction date of each bond trade. For bond ratings, we use the Standard & Poor's rating if it exists; otherwise, we use Moody's rating.

Based on FISD variables, we further exclude the following bond issues: bonds with redeemable, exchangeable, convertible, sinking fund, enhancement, or asset-backed features; perpetual and variable rate bonds; medium-term notes; Yankee, Canadian, and foreign currency issues; Rule 144A issues; Treasury Inflation Protected Securities (TIPS), Treasuries, munis (municipal bonds), Treasury coupon- and principal-strips; and agency-type bonds. We retain bonds with call and put features. The FISD sample yields 11,950 US domestic corporate bond issues.

Step 3: *Intersection of FISD and CRSP data*. Using the six-digit CUSIP identifiers, the screened subsample of FISD bond issues is then merged with the Center for Research in Security Prices database in Wharton Research Data Services (WRDS). We eliminate bond issues that do not belong to firms with public equity; that is, they do not have any matching stocks in the CRSP database. The merged FISD-CRSP sample consists of 8,291 US domestic corporate bond issues.

Step 4: *Intersection of TRACE and FISD-CRSP data*. Based on the six-digit CUSIP identifiers, we merge the TRACE bond transaction sample with the FISD-CRSP bond issue- and issuer-attributes sample. The merged sample consists of 843,442 trading date observations for 2,806 bond issues by 967 issuers.

Step 5: *Bloomberg data*. We obtain trades data on five-year CDSs from Bloomberg. Bloomberg consists of two sources of CDS data: Bloomberg Generic Average Price (mnemonic CBGN) and Credit Market Analysis, New York (mnemonic CMAN). CBGN is Bloomberg's own composite data and reports the generic price data for each CDS as an average of the contributed spreads from multiple data vendors. CMAN is an external data provider that offers its pricing data on the Bloomberg terminal. We assume that the starting date of CDS spreads in Bloomberg is also the date of introduction of the CDSs. The assumption is reasonable given that Bloomberg has an extensive coverage of CDS data and is recognized as a benchmark pricing source.[24]

We use the Bloomberg default CBGN source as the primary data. For 314 CDS issues, CBGN data are complete and are used as is. For 293 CDSs, CBGN data are incomplete (largely before 2008) and are augmented with data from CMAN. For another 13 CDSs, CBGN has no data and CMAN becomes the primary source of CDS spreads. Altogether, we obtain daily CDS spreads on 620 CDS issues by 620 US firms for a total of 598,221 daily observations between August 3, 2001 and September 30, 2009.

Step 6: *Intersection of TRACE, FISD, CRSP, and Bloomberg data*. We merge the data obtained from TRACE, FISD, CRSP, and Bloomberg to yield a composite sample of 2,806 bond issues by 967 issuers and 1,987,410 time series observations.[25] Of the composite sample, 355 issuing firms (37%) or, equivalently, 1,559 bond issues (56%) have corresponding CDS issues.

We impose a few additional filters. We eliminate 2009 data because they are incomplete. We exclude 612 bond issuing firms that do not introduce any CDSs until the end of 2008. We remove 8,208 bond trades reported in TRACE for 28 bonds that occur after the maturity date reported in FISD. And we discard 645 bond issues that have valid stock returns before July 2002 but the stock is delisted prior to the bond transaction data being available on TRACE.

Our final screened sample consists of 1,365,381 time series observations on 1,545 bond issues by 350 issuing firms (which also had CDSs introduced between 2001 and 2008).

Step 7: *Augmentation with Datastream data*. From Datastream, we collect daily values for the volatility index (VIX)

[24] Because the CDS market is new, there is no single agreed-upon source of CDS data. There are multiple vendors of data, and it is likely that the start dates differ across these data providers. To verify the robustness of our assumption, we collect the CDS inception dates from an alternate database, Markit. We run our empirical tests using these alternate starting dates. Our results and implied conclusions remain unchanged.

[25] 1,987,410 = (# of bonds) $*$ (days with valid return on at least one of the three securities: bonds, CDSs, or stocks). Hence, this number is larger than the separate daily observations reported in Steps 1, 4, and 5.

Table A1
Sample construction steps and sample sizes.
Sample selection criteria 1 and 2, discussed in Appendix C, extract subsamples with valid one-day lagged returns from the screened full sample obtained in Step 6.B. TRACE, Trade Reporting and Compliance Engine; FISD, Fixed Investment Securities Database; CRSP, Center for Research in Security Prices; CDS, credit default swap.

Step	Description	Number of		
		Issuers	Issues	Observations
1.A	Raw TRACE data (July 2002–September 2009)	4,869	34,900	40,044,493
1.B	Eliminate special or canceled trades	4,869	34,900	34,140,337
1.C	Combine all intraday trades into a single daily transaction observation	4,869	34,900	5,768,201
4	Intersect TRACE and screened FISD+CRSP	967	2,806	843,442
6.A	Merge TRACE+FISD+CRSP+Bloomberg data sets	967	2,806	1,987,410
6.B	Retain only CDS issuers, apply other filters	350	1,545	1,365,381
Sample selection criteria 1		316	1,277	198,131
Sample selection criteria 2		340	1,469	411,148

and daily swap rates for 15 different maturities (ranging between one year and 30 years) from August 2001 to December 2008. Each bond trading date is matched to a corresponding swap rate based on linear interpolation of the two closest neighboring maturity swap yields. This yields a time series of swap rates matching in maturity to the corresponding bond issue. The swap rates and VIX values are augmented to our screened data sample.

Table A1 succinctly describes the key steps of sample construction and screening, and it lists the sample size after each step.

Appendix B. Data summary statistics and definition of variables

B. 1. Final merged data summary statistics.

- Sample period: 2002–2008
- 1,545 bond issues by 350 issuing firms with CDS issues
- 1,365,381 time series observations (bond issues × trading days)
- 110,934 observations before CDS introduction and 1,254,447 after
- 883.74 trading days per bond issue
- 1,545 bond issues
  - 1,352 senior issues, remaining some form of junior issues
  - 1,520 fixed coupon issues, 25 zero coupon issues
  - All issues nonconvertible
  - 662 callable, 63 putable, 820 straight bonds
  - 983 industrials, 355 financials, 207 utilities
- 1,365,381 time series observations
  - Number with valid bond returns = 328,130 (24.03%)
  - Number with valid CDS spread changes = 938,944 (68.77%)
  - Number with valid stock returns = 1,294,161 (94.78%)
  - Number with valid bond returns + CDS spread changes = 258,945 (18.97%)
  - Number with valid bond returns + CDS spread changes + stock returns = 249,605 (18.28%)
  - Number of observations prior to the introduction of CDSs = 110,934 (8.13%)
  - Number of observations subsequent to the introduction of CDSs = 1,254,447 (91.88%)
- 110,934 pre-CDS time series observations
  - Number with valid bond returns = 17,159 (15.47%)
  - Number with valid stock returns = 105,517 (95.12%)
  - Number with valid bond returns + stock returns = 16,236 (14.64%)
- 1,254,447 post-CDS time series observations
  - Number with valid bond returns = 310,971 (24.79%)
  - Number with valid CDS spread changes = 938,944 (74.85%)
  - Number with valid stock returns = 1,188,644 (94.75%)
  - Number with valid bond returns + CDS spread changes = 258,945 (20.64%)
  - Number with valid bond returns + CDS spread changes + stock returns = 249,605 (19.90%)

B. 2. Definitions of variables

- *bndret*: refers to bond returns (in percent) obtained as the difference of consecutive mean daily yields, i.e., as $-(y_t - y_{t-1})$, where y_t and y_{t-1} are mean bond yields (in percent) on days t and $t-1$, respectively.
- *cdsret*: refers to CDS returns (in basis points) based on CDS spread changes and computed as the difference of consecutive daily yields, i.e., as $(y_t - y_{t-1})$, where y_t and y_{t-1} are CDS spreads (in basis points) on days t and $t-1$, respectively.
- *stkret*: refers to daily stock return (in percent).
- *tryret*: refers to swap return (in percent) defined as change in matching maturity consecutive swap yields, i.e., as $-(y_t - y_{t-1})$, where y_t and y_{t-1} are swap yields (in percent) on days t and $t-1$, respectively; yields of each bond are paired with appropriate swap yields computed based on interpolation of swap maturities to equal bond maturity.
- *vixchng*: refers to change in VIX measure (index value) over consecutive days.

All five variables are winsorized at the 1% level.

Table D1
Annual percentage observations in treatment sample.

	2002	2003	2004	2005	2006	2007	2008
Sample selection criteria 1	8.06%	47.88%	20.49%	14.91%	7.97%	0.55%	0.14%
Sample selection criteria 2	4.48%	41.94%	23.54%	18.46%	9.94%	1.09%	0.55%

Appendix C. Alternate approaches to data construction

In this Appendix we describe an alternative to the primary data sampling approach used in the paper. The primary filter of the data is explained in Section 3 and Appendix A, and it is denoted "sample selection criteria 1." The extended "sample selection criteria 2" is based on the alternate approach described below.

The main objective of extending the data construction methodology of default sample selection criteria 1 is to obtain more observations for analysis and to offer a robustness test of the key results of the paper. Under criteria 1, we retain those days on which there are three consecutive observations of all traded securities in our sample. Hence, under the alternate approach, we focus on periods of active trading, which are more likely when information is being released. These are exactly the periods when we want to test for market efficiency.

Under this extended data construction approach, we not only include those days on which we have three consecutive observations but also expand the calculation of returns to windows of time that are greater than one day between observations of transactions (i.e., observations are nonconsecutive). In periods when information about the bond issuer is high, the inter-arrival time between transactions is small, and in periods of low information, inter-arrival times are large. So the extended data approach allows for efficiency tests on nonstandard inter-transaction times. The extended sample selection criteria 2 is implemented as follows.

CDS and bond trades are sporadic. The gaps in the data occur because of the absence of consecutive days when both CDSs and bonds trade, precluding return calculations. Therefore, in this approach we use all dates on which both the bond and the CDS of the firm were traded (and had observations). These dates need not be consecutive. As an illustration, suppose we have trades of the bond and the CDS only on days $\{1,2,5,7,8,9,12,15,16\}$. For both, we compute a return series (i.e., yield changes) as $R(t,t+k) = [y(t+k)-y(t)]/k$, where k is the number of days between observations and $y(t)$ is either the yield on the bond or the CDS spread. The sign of the numerator depends on whether we are looking at CDSs or bonds. For bonds, a "–" precedes the numerator. By dividing by k we still obtain an average daily return, thereby constructing a nonoverlapping time series of average daily returns. Consequently, all the tests applied to daily returns remain the same and could be applied just as in the main set of tests. It is important for the tests of bond efficiency that the information sets for contemporaneous returns and lagged returns do not overlap, and this is still maintained when we construct our return series using this approach. This approach has the advantage of focusing more on days when there was trading, i.e., days when information was more likely to be released. It also substantially increases the sample size.

Appendix D. Pooled control sample based on stratified sampling

The pooled control sample used in the paper is based on uniform sampling. A random date is uniformly sampled within the range of the first and last trading dates of each bond of a non-CDS firm and the chosen date is denoted as the event date for control sample bonds. Under this approach, the distribution of observations in the treatment and the pooled control sample are independent.

This Appendix proposes an alternative for additional robustness tests. We construct the alternate pooled control sample based on stratified sampling.[26] We form strata by year, and annual distributions of observations in the CDS sample serve as input probabilities to draw up the pooled control sample observations. This design makes the distribution of control sample observations conditionally dependent on the distribution of CDS sample observations. We implement the stratified sampling process as follows.

Step 1: We define strata as calendar years, and each year we calculate the number of observations of balanced treatment sample (bond trades of CDS issuers) as a fraction of the overall sample size. Table D1 reports the annual percentage values that serve as input probabilities constituting a stratified distribution in the sampling process.

Step 2: We aggregate, as a pooled panel, all the bond issues by CDS nonissuers: firms that did not issue any CDSs until the end of 2009 and have bonds that meet the selection criteria outlined in Appendix A.

Step 3: For each bond i belonging to CDS nonissuers, we compute normalized selection probability in year t as

$$NSelProb_{it} = \frac{1}{AdjFac_i} \times In\ Prob_t \times \frac{NDAYS_{it}}{365}, \tag{14}$$

where $In\ Prob_t$ is the input probability for year t, $NDAYS_{it}$ is the number of days (including zero trade days) in year t when bond i has a valid existence, and $AdjFac_i$ is the bond-specific adjustment factor such that $\sum_{t \in \{2002-2008\}} NSelProb_{it} = 1$.

Step 4: Using the bond-specific annual normalized selection probabilities, we select a random date between the first and last trading dates of each bond and designate it as the event date for that control sample bond.

[26] Stratified sampling is a weighted sampling process involving two key steps: (1) the population is divided into smaller groups called strata formed based on members' shared attributes or characteristics, and (2) a random sample is drawn in which the probability of selection of an observation from each stratum is proportional to the stratum's size relative to the population size.

Step 5: Based on the chosen event dates, control sample observations are suitably reclassified into pre- and post-event subsamples, and we balance the two subsamples by retaining only the observations in the $[-2,+2]$ years window.

We implement additional difference-in-differences tests on the stratified pooled control sample thus obtained.

Appendix E. Market quality measure (q)

This Appendix presents a brief summary of the Hasbrouck (1993) model of market quality for a security. We retain the same notation, though our final measure is different (albeit in the same spirit).

Hasbrouck defines market quality as the inverse of the variance of the pricing error after accounting for the efficient component of returns.

The log transaction price of a security is given as

$$p_t = m_t + s_t, \tag{15}$$

where m_t is the efficient component (i.e., a random walk) and s_t is the pricing error. The smaller that the variance $Var(s_t)$ is, the higher is market quality q. The security's continuous return can be written as the difference of log transaction prices:

$$r_t = m_t - m_{t-1} + s_t - s_{t-1}. \tag{16}$$

It remains to specify the processes for m_t and s_t. The process for the former is a simple random walk, i.e.,

$$m_t = m_{t-1} + w_t. \tag{17}$$

and the process for the pricing error could be information-related, i.e., related to innovation w_t, or it could be non-information-related, i.e., independent of w_t with separate innovation term η_t. To cover both cases, Hasbrouck posits that

$$s_t = \alpha w_t + \eta_t, \tag{18}$$

where the information-related pricing error is the case where $\alpha \neq 0$ and $\eta = 0$. In the case of a non-information-related pricing error, we have $\alpha = 0$ and $\eta \neq 0$ instead.

We consider the first case, i.e., information-related pricing errors. Substituting Eqs. (17) and (18) in Eq. (16) and setting $w_t = (1-a)e_t$ and $\alpha = 1/(1-a)$, we get after simplification

$$r_t = e_t - a e_{t-1}, \tag{19}$$

which is an MA(1) process. Estimating this process on return data gives the parameters $\{a, \sigma_e^2\}$. We can see that $\sigma_s = a\sigma_e$.

Now we consider the second case, i.e., non-information-related pricing errors. Setting $w_t = (1-a)e_t$ and $s_t = \eta_t = a e_t$, and substituting these values into Eq. (16) results in the same MA(1) process as before, i.e., $r_t = e_t - a e_{t-1}$. Again, we note that $\sigma_s = a\sigma_e$.

We do not need to ascertain whether the first or the second case applies, because the pricing error equation is the same in both cases. The value of parameter a varies empirically depending on the structure of the pricing error, i.e., whether it is related to information or not. Once we compute the total return error, $\sigma_r^2 = Var(r_t)$, we can compute the measure of market quality, i.e.,

$$q = 1 - \frac{\sigma_s^2}{\sigma_r^2} = \frac{\sigma_e^2 - 2a\,\mathrm{Cov}(e_t, e_{t-1})}{\sigma_e^2 + a^2\sigma_e^2 - 2a\,\mathrm{Cov}(e_t, e_{t-1})}. \tag{20}$$

It is clear that, when $a=0$, the market quality is $q=1$.

The above measure of market quality is very similar in spirit to Hasbrouck (1993) except that we standardize the measure and obtain a closed form expression for it.

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