Do rating agencies deserve some credit? Evidence from transitory shocks to credit risk $\stackrel{\leftrightarrow}{\sim}$

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Abstract

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Keywords: Credit Ratings, Mutual Funds, Institutional Investors, Financial Intermediation

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Abstract

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Credit ratings agencies (CRAs) have historically played an important role as information intermediaries in financial markets. However, CRAs are now under siege. A vast academic literature finds that the issuerpays model and competitive pressures distort the incentives of CRAs to issue accurate ratings.¹ Regulators and other observers have pointed to inflated ratings as a key cause of the mortgage securitization boom of the early 2000s and the subsequent recession.² The Dodd-Frank Act now requires regulatory agencies to remove all references to CRAs from regulations, thereby limiting regulatory uses of ratings. Finally, research finds that estimates of default probability that include information from equity (Hilscher and Wilson, 2016) or Credit Default Swap markets (Chava, Ganduri, and Ornthanalai, 2016) are more accurate or timely than ratings. In fact, Flannery, Houston, and Partnoy (2010) argue that both regulators and private investors should use market-based estimates of credit risk instead of credit ratings.

Nevertheless, CRAs continue to thrive. By 2014, revenues at the three largest CRAs surpassed precrisis levels with profits at or near record highs (Economist, April 19, 2014). Thus, despite their flaws and diminished regulatory relevance, CRAs appear to pass the market test. But, how do CRAs add value in situations where accurate market-based estimates of credit risk are available?

CRAs claim that they add value because market-based estimates of credit risk are noisy and this noise can have real effects. For example, Cantor and Mann (Moody's; 2006) state:

Our conversations with investors, issuers and regulators have led us to conclude that many market participants have a strong preference for credit ratings that are not only accurate but also stable. They want ratings to reflect enduring changes in credit risk because rating changes have real consequences—due primarily to ratings based portfolio governance rules and rating triggers—that are costly to reverse. Market participants, moreover, do not want ratings that simply track market-based measures of credit risk. Rather, ratings should reflect independent analytical judgments that provide counterpoint to often volatile market-based assessments.

In this paper, we investigate whether CRAs actually do what they say: can CRAs distinguish between permanent ("enduring") and transitory shocks to credit risk? A shock to market-based assessments of credit risk may be transitory either because it is a true shock that eventually reverses, or because it is a false signal

¹See for example, Griffin, Nickerson, and Tang (2013) and Becker and Milbourn (2011), and other references in footnote 5.

²See for example, the Financial Crisis Inquiry Commission Report, and SEC Commissioner Luis A. Aguilar's public statement "Restoring Integrity to the Credit Rating Process" on August 27, 2014.

resulting from a temporary deviation of market prices from fundamentals (see for e.g Duffie, 2010). Either way, for CRAs to add value relative to markets, they must be able to discern whether a shock is transitory soon after the shock occurs, rather than by waiting long enough to see if the shock eventually reverses. If CRAs can indeed discern which shocks are transitory in real-time, they may serve a valuable role in the economy by dampening some of the adverse real effects of transitory shocks to financial market prices. For example, if market-based measures of credit risk are embedded in contracts instead of ratings, suppliers could deny credit based on a transitory increase in credit risk, thereby impacting a firm's profitability and production and turning a transitory financial shock into a permanent real one.

An ideal setup to test whether CRAs can discern which shocks are transitory is to consider two ex ante identical firms. Suppose market participants perceive a similar increase in credit risk for both firms. However, the 'treated' firm's increase in risk is due to a transitory shock and the 'control' firm's is due to a permanent shock. If CRAs are able to distinguish between these two types of shocks in real-time, we expect that the treated firm will be less likely to be downgraded than the control soon after the shock.

Our empirical tests operationalize this ideal setup. We employ shocks to equity prices as our measure of shocks to credit risk. Adverse changes in equity value can translate into changes in credit risk in two ways. First, they increase market leverage, thereby directly increasing credit risk (Merton, 1974). Second, declines in stock prices signal bad news about the firm's fundamentals (Fama, 1981; Kothari and Sloan, 1992). CRAs also state that they consider stock prices as signals in reviewing ratings (Adelson, 2008).

We use mutual fund fire sales as in Edmans, Goldstein, and Jiang (2012) to identify firms with transitory shocks to equity value. Edmans, Goldstein, and Jiang (2012) show that implied fire sales by distressed mutual funds result in economically meaningful shocks to equity prices that reverse over several quarters. We designate firms that experience fire sales in a given quarter as treated firms. Control firms have similar returns in the event quarter and are also matched by credit rating, industry, and propensity to experience fire sales at the start of the event quarter. We confirm that on average, treated-firm returns reverse ex post, while control-firm returns do not.

Our key finding is that CRAs can in fact distinguish between transitory and permanent shocks to credit risk. Treated firms are 0.9% less likely to be downgraded than controls. This reduction is approximately half the unconditional downgrade probability of treated or control firms of about 2%. The treatment effect

increases to 1.5% if we also include one quarter after the fire sale in the event period. Results are similar if we take the severity of downgrades into account. The difference in the average number of downgrade notches between treated and control firms during the fire sale and subsequent quarter is 0.034, which is approximately 40% of the average unconditional number of downgrade notches of treated firms at 0.085. We find even stronger results when we focus on the sample of firms in which fire sales are likely to be most salient. In a subsample with negative event quarter returns (-12% on average), the average difference in the number of downgrade notches between treated and control firms nearly doubles to 0.064, from 0.034 in the full sample.

One concern with using fire sales to identify exogenous transitory shocks is that investors may be more likely to withdraw capital from mutual funds that they believe will perform poorly in the future. If investors behave in such a manner, stocks subject to fire sales are of worse quality, and hence likely to have greater downgrade probabilities than a typical firm, which would bias us against finding our results. Nevertheless, our empirical strategy contains two elements to mitigate such selection biases. First, as in Edmans, Goldstein, and Jiang (2012), we identify fire sales using hypothetical distressed fund trades, which are computed assuming that funds sell their holdings in proportion to their portfolio weights before the extreme outflows. Although this strategy addresses selection biases arising from fund manager discretion during the event quarter, fire sale firms may be different from typical firms prior to the event quarter in terms of variables related to mutual fund ownership and past performance (Berger, 2017). To minimize observable differences between treated and control firms, we also match on the firm's propensity to experience fire sales. We find no evidence that fire-sale firms have relatively worse past performance in our sample of rated firms: past returns and downgrades do not predict fire sales.

Our treatment-control setup precludes alternative explanations for these results that are common to all rated firms. For example, explanations that rely on coarseness (Goel and Thakor, 2015) or lack of timeliness of ratings (Chava, Ganduri, and Ornthanalai, 2016) apply to both treated and control firms. Similarly, macroeconomic factors such as business cycles, which cause variation in fundamental shocks and downgrade propensities over time, also affect both sets of firms. Because the validity of our design depends on the quality of the matches, we confirm that our matching procedure works well. Treated and control firms are balanced for a wide range of variables at the start of the event quarter including mutual fund ownership,

size, leverage, past returns, and default probability estimated using the Campbell, Hilscher, and Szilagyi (2008, CHS) model. Downgrade probabilities and returns for treated and control firms also exhibit parallel trends before the fire-sale quarter.

A possible alternative explanation for these results is that credit markets actually distinguish between permanent and temporary equity price shocks, and CRAs passively follow credit markets. If true, this ability of credit markets to see through temporary shocks in equity markets may be interesting in itself; however, such behavior does not imply a special role for CRAs. Consequently, we examine whether credit markets respond to equity fire sales. In particular, we examine Credit Default Swap (CDS) markets because Blanco, Brennan, and Marsh (2005) find that price discovery of credit risk happens in CDS markets rather than bond markets. We find that both treated and control firm CDS spreads and CDS-implied rating downgrades increase by similar magnitudes. Most of the increase in spreads for both treated and control firms occurs in the quarter after the fire sale, consistent with Hilscher, Pollet, and Wilson (2015), who find that information flows from equity to CDS markets. Treated firm spreads eventually reverse while those of control firms do not, re-confirming that the fire sales shocks are indeed temporary. The sample with available CDS data is smaller than the ratings sample in the cross-section as well as the time-series and hence, it is possible that these tests do not have power. However, we find that even in the sample where CDS data exist, CRAs remain significantly less likely to downgrade treated firms than controls. We also find that the predicted default probability from the CHS model that uses information from accounting statements as well as equity markets displays a similar pattern as CDS spreads. CHS default probabilities rise by similar magnitudes for both treated and control firms during the event and subsequent quarter. Treated firm CHS default probabilities reverse thereafter, but control firm probabilities remain elevated.

Thus, CRAs realize in real-time that shocks to treated and control firms are different, while CDS and equity markets take several quarters to do so. Moreover, the differential response of CRAs to treated and control firm shocks is consistent with the pattern in future realized defaults. We find that over the five years after the shock, treated firms are half as likely as control firms to experience bankruptcy, validating the CRA decision to not downgrade them as frequently.

Why do CRAs appear to see through transitory shocks to equity prices, while market-based measures do not? One hypothesis is that after seeing a shock to prices, CRAs may seek both public and private infor-

mation to determine whether there is actually a substantial decline in the firm's fundamentals that warrants a downgrade. This hypothesis is consistent with what CRAs say they do. For example, Adelson (Standard & Poor's; 2008) states: "...sudden changes in the price of a company's stock sometimes signal abrupt changes in the company's fundamental condition or prospects. Accordingly, we respond to a sudden change in stock price by exploring the underlying causes." Besides public sources, CRAs also have access to nonpublic information that may not be available to market participants. Such information includes "...budgets and forecasts, financial statements on a stand-alone basis, internal capital allocation schedules, contingent risks analyses and information relating to new financings, acquisitions, dispositions and restructurings."³ The information that CRAs collect from direct sources can help them better interpret information in market prices, consistent with the implications of the model in Bond, Goldstein, and Prescott (2009). Their model shows that market prices need not be completely informative about current fundamentals because prices also impound information on expected actions by regulators and other economic agents; the model also implies that a well-informed agent infers fundamentals from market prices more accurately than a less-informed one.

We therefore examine whether the ability to discern which shocks are transitory is related to information advantage of CRAs relative to markets. First, we test whether the treatment effect is stronger when there is greater uncertainty in the firm's public information environment. Following Barron, Kim, Lim, and Stevens (1998), we use the cross-sectional dispersion in sell-side equity analyst forecasts, as well as the time-series standard deviation of their median forecast errors as measures of information uncertainty. We find that the interaction between the treatment and these measures of uncertainty is significant: CRAs are less likely to downgrade treated firms with high levels of information uncertainty relative to matched controls. These results suggest that CRAs complement equity analysts: they add more value in situations where analyst forecasts are less precise.

Second, we follow Jorion, Liu, and Shi (2005) and use the enactment of Regulation Fair Disclosure (Reg FD) as a shock to the relative information advantage of CRAs. Reg FD prohibited publicly traded firms from selectively disclosing material information to investors or securities professionals, but provided an exemption for disclosure to CRAs. If access to nonpublic information is the channel through which CRAs identify transitory shocks, we expect that the differences in downgrades between treated and control firms

³From Standard & Poor's November 2002 submission to the Securities and Exchange Commission.

will increase post Reg FD. We find that this is indeed the case, with a significant increase in the treatment effect after Reg FD, even after controlling for time, rating, and industry fixed effects as well as a host of firm characteristics.

Our results survive a battery of robustness tests including changes to the matching procedure such as using finer or coarser calipers, multiple neighbors instead of one, and a finer or coarser industry classification. Results are also similar if we exclude specific industries (e.g., financials and utilities) and the period of the 2007-2009 financial crisis. We also find that the effect is pervasive and not statistically different across rating categories and verify that our results are not driven by differences in volatility of the ratings. Finally, we examine a placebo test where the treatment variable is based on sales by all and not just distressed funds (and exclude fire-sale firms). Such sales are likely to be information driven, and ex post we find that they result in permanent shocks to prices. We find no differences in rating downgrades for this 'placebo treatment', suggesting that our results are not driven by the characteristics of stocks held by mutual funds that experience outflows.

Our paper contributes to the literature on the role and impact of CRAs. Overall, the literature on CRAs finds that their actions affect market participants, but also highlights concerns about their incentives. For example, Kisgen (2007) argues that downgrades can result in significant real costs to firms including a loss of eligible investors and customers and higher costs of borrowing, Almeida, Cunha, Ferreira, and Restrepo (2017) show that downgrades have real effects on firm investments, and Ellul, Jotikasthira, and Lundblad (2011) find that downgrades result in fire sales in corporate bonds.⁴ Research on CRAs also finds that the issuer-pays compensation structure as well as regulatory and contractual reliance on ratings results in distortions in incentives for CRAs to issue accurate ratings.⁵ Our results do not imply that CRAs are free from conflicts of interest, or that ratings are more accurate than market-based estimates. Instead, we argue that because accuracy is only one part of the CRA's objective function, lower accuracy need not imply that CRAs are real effects of downgrades—is also important.

⁴Also see Kisgen (2009), Tang (2009), Sufi (2007), and Manso (2013).

⁵One source of distortions is the compensation structure of CRAs (Skreta and Veldkamp, 2009; Sangiorgi, Sokobin, and Spatt, 2009; Bolton, Freixas, and Shapiro, 2012; Griffin, Nickerson, and Tang, 2013; Cornaggia and Cornaggia, 2013; Fulghieri, Strobl, and Xia, 2013; Xia, 2014; Sangiorgi and Spatt, 2016). The other source of distortion is the regulatory and contractual reliance on ratings (Kisgen and Strahan, 2010; Opp, Opp, and Harris, 2013; Bruno, Cornaggia, and Cornaggia, 2015).

Thus, our paper contributes to research that examines the trade-off between ratings stability and accuracy (Altman and Rijken, 2004, 2006; Cornaggia and Cornaggia, 2013; Löffler, 2013). Our paper complements this research by showing that CRAs are able to distinguish between transitory and permanent shocks in real time, thereby adding value relative to smoothed market-based estimates. We thus provide an answer to why CRAs continue to thrive despite the flaws documented by prior research and the availability of substitutes. Our paper also complements Cornaggia, Cornaggia, and Israelsen (2017), who find that municipal bond ratings matter for prices even without a change in fundamentals. Our results provide an explanation for why investors may consider ratings informative.

Additionally, our paper is related to the literature on the real effects of financial markets (see Bond, Edmans, and Goldstein (2012) for a survey). This literature shows that managers and other decision-makers learn from stock prices and use this information to guide their decisions. Similar to our setup, a growing body of research employs mutual fund fire sales as transitory equity price shocks and shows that economic agents take decisions based on these non-fundamental shocks.⁶ Our results suggest that CRA rating policies may have evolved to mitigate some of the adverse real effects of financial prices.⁷

Our results also suggest that regulatory efforts to limit the privileged position of CRAs (for e.g., see discussion in section 4.2 on the Dodd-Frank Act), albeit with the laudable goal of encouraging investors to do independent analysis, can have unintended consequences. Transitory shocks in financial markets are more likely to propagate to the real economy if regulations restrict the access of CRAs to private information (thereby inhibiting their ability to discern which shocks are temporary) or create disincentives for CRAs to issue independent opinions.

1. Data and Methodology

This section describes our datasets, methodology, and construction of variables.

⁶ See Acharya, Almeida, Ippolito, and Perez (2014), Ali, Wei, and Zhou (2011), Derrien, Kecskés, and Thesmar (2013), Phillips and Zhdanov (2013), Khan, Kogan, and Serafeim (2012).

⁷Other institutions may also play a similar role. For example, Sulaeman and Wei (2012) find that a subset of skilled equity analysts are able to issue price-correcting recommendations for stocks subject to flow-driven mispricing.

1.1. Ratings and other data

Our main dataset is based on the intersection of four databases: (i) mutual fund holdings from Thompson 13F filings, (ii) mutual fund returns and total net assets from the Center for Research in Security Prices (CRSP) Survivorship-Bias Free mutual fund database, (iii) credit ratings and firm accounting data from Compustat, and (iv) equity returns and prices from CRSP. We also use data from Capital IQ and I/B/E/S for supplementary tests. The filters we impose on the mutual fund data follow prior research and are described in Appendix A.

We use data on Standard and Poor's (S&P) issuer ratings in our main tests, but also provide robustness results for Moody's ratings in the Internet Appendix.⁸ We translate each letter rating into a numerical rating, so that a one unit increase reflects a one notch improvement of rating (e.g. from BBB+ to A). We also obtain Credit Default Swaps (CDS) data from Markit. As described in Appendix B, we use the 5-year contract with the document clause that is likely to be the most liquid CDS contract on that stock. Our measure of CDS spreads each month is the mean CDS spread over the last five trading days that month.⁹ We also use CDS implied downgrades from Markit, which are based on ratings computed only using CDS spreads by Markit. Finally, for each stock-quarter, we compute the 12-month ahead default probability following Campbell, Hilscher, and Szilagyi (2008) (henceforth, CHS). Other variables are standard and defined in Appendix B.

1.2. Methodology

Our goal is to test whether credit rating agencies can distinguish between transitory and permanent shocks to credit risk. To do so, we use a matched sample, difference-in-difference methodology. Treated firms are those that experience fire sales in a given quarter. Matches have similar characteristics as treated firms at the start of, and similar returns during, the fire-sale quarter. We test whether realized downgrade probabilities are different for treated firms relative to controls over the fire sale and subsequent quarters. This is a 'difference-in-difference' test in that it is the difference in the change in credit ratings between treated and matched firms over the event and subsequent quarter.

⁸We focus on S&P because our sample of Moody's data is shorter and has a lower match rate with CRSP and Compustat.

⁹Results are similar if we use the last day or the mean spread over the entire month. We report results based on the mean over the last five days, because the last day's price is more volatile, and the mean over the entire month is stale relative to stock returns based on end-of-month prices.

The fire-sale approach is motivated by the observation that while mild fund outflows can be absorbed by a fund's cash position, extreme outflows are more likely to force managers to liquidate stocks, thereby generating price pressure on these stocks. Coval and Stafford (2007) show that stocks subject to fire sales suffer a substantial decline in prices that is transitory. Edmans, Goldstein, and Jiang (2012) refine the approach in Coval and Stafford (2007) to address a potential source of endogeneity: mutual fund managers choose which stocks to sell and their selection criteria may be linked to the outcome variable. Hence (as discussed in further detail below), they use trades implied by a fund's portfolio weights and outflows rather than actual trades. We follow the approach in Edmans, Goldstein, and Jiang (2012) to identify fire sales. We confirm that in our sample, the shocks to treated firms are temporary, while the shocks to controls are not. On average, treated firm returns reverse over the next few quarters, while control firm returns do not reverse. We therefore follow the literature in referring to the fire-sale shocks as 'transitory'.

The next step is to identify a set of firms that serve as controls. Our goal is to identify firms with similar characteristics and credit risk at the start of the event quarter, and similar observed market performance during the event quarter. However, control firm returns are not due to fire sales and hence are likely to be permanent. Thus, we expect that control firms have a real fundamental shock and treated firms have a transitory, nonfundamental shock in the event quarter. We therefore search for control firms with similar characteristics to those of treated firms at the start of the Event Quarter (EQ) and similar returns during EQ. In particular, our matching procedure consists of the following steps.

- 1. As of the beginning of EQ, we search for controls that have:
 - (a) the same narrow credit rating,
 - (b) the same Fama-French five industry classification, and
 - (c) a propensity to be a fire-sale stock within 2.5% of that of the treated firm.
- 2. From these potential matches, we pick the firm with the minimal absolute distance in stock return from the treated firm in EQ, excluding any matches with an absolute return difference of more than 2.5%.
- 3. If a satisfactory match cannot be established within a narrow rating category, we then look for a control candidate within a broader rating category (i.e., ignoring '+','-').

The propensity score caliper of 2.5% corresponds to one-third of the standard deviation of treated-firm propensity scores adjusted for time fixed effects. The return caliper of 2.5% corresponds to one-fifth of the standard deviation of treated firm *EQ* returns. Our matching criteria are chosen to balance the need for a tight match and a large sample. We show in Section 2.3 that this procedure results in treated and control firm samples that are similar across a variety of dimensions, and in Section 5.4 that our main results are similar if we relax or tighten the criteria, or use a different matching procedure.

The matched sample analysis allows us to account for common shocks across treated and control firms. The control sample provides an estimate of the downgrade rate that we expect for firms with similar characteristics and a similar EQ return to treated firms. The key difference between the two samples is that the treated firm EQ-return is transitory. While we cannot rule out the possibility that some treated firms experience permanent shocks or that some controls experience transitory shocks, our setup ensures that the treated sample is more likely to experience transitory shocks. Moreover, we confirm in the data that on average, returns for the treatment firms reverse while those for the controls do not.

A causal interpretation of our results requires that the selection of stocks into the fire-sale sample is independent from the actions of CRAs. The argument for such independence is similar to the argument that Edmans, Goldstein, and Jiang (2012) make for fire sales and takeover likelihood: decisions by investors to buy or sell a particular mutual fund are unlikely to be due to information about changes in credit ratings of specific stocks within the fund. Investors with such information are more likely to trade on the individual stock or bond rather than the fund. Nevertheless, our research methodology consists of several elements that are designed to address potential sources of endogeneity. First, as discussed above, we follow Edmans, Goldstein, and Jiang (2012) in using implied rather than actual sales of mutual funds as the source of exogenous variation. Thus, our tests do not reflect discretionary trades that may be based on changes in fund manager views about the firm in the event quarter. This methodology can be interpreted as a 'Bartik-like' instrument (see Goldsmith-Pinkham et al., 2018), in which identification comes from the pre-event quarter weights and instrument relevance from potentially endogenous flows during the event quarter. Second, as in Edmans, Goldstein, and Jiang (2012), we exclude sector funds to eliminate flows which may be due to specific information about the industry as a whole. Finally, we use propensity-score matching to ensure that there are no meaningful observable differences between treated and control firms prior to the fire-sale

quarter.

In particular, we estimate a probability model for a firm to experience fire sales in a given quarter and match on the estimated propensity scores in the beginning of the event quarter. A fire-sale firm can differ from a typical firm for several reasons (Berger, 2017). First, because they are owned by certain mutual funds, they may have greater mutual fund ownership in general and also possess other characteristics associated with mutual fund ownership. We therefore include mutual fund ownership, size, leverage, liquidity, and volatility in our propensity score model. A second possible difference is that fire-sale stocks are in some way worse than the typical stock. This might be because fire-sale stocks are owned by fund managers that are losing assets under management—presumably because they have under-performed. We therefore include returns over the past three and past 12 months as well as rating changes over the past three and 12 months as additional predictor variables in the propensity score model. This empirical design implements the recommendations in Berger (2017) to address potential differences in characteristics between treated and control firms. While we cannot rule out the possibility that treated firms are different from controls along some unobserved or mismeasured dimensions related to past performance, this possibility seems unlikely because, as we see below, past returns do not predict selection into the fire sales sample. Moreover, if firesale stocks are of worse quality than controls, this will bias us towards finding they are more likely to be downgraded than the controls.

1.3. Measuring fire sales

We closely follow the approach in Edmans, Goldstein, and Jiang (2012) to construct *MFFlow*, the implied price pressure calculated by assuming that funds subject to large outflows (>5% of their assets) adjust their existing holdings in proportion to their previous portfolio weights. More precisely, we first calculate the dollar outflows of fund *j* from the end of quarter q - 1 to the end of quarter *q* as follows:

$$Out flow_{j,q} = -(TNA_{j,q} - TNA_{j,q-1}(1+r_{j,q})),$$
(1)

where $TNA_{j,q}$ is the assets under management of fund j = 1, ..., m, in quarter q and r is the net return of fund j in quarter q. In every quarter q, summing only over the m funds for which the percentage outflow $(\frac{Outflow_{j,q}}{TNA_{j,q-1}})$ is greater than 5%, we then construct:

$$MFFlow_{i,q} = \sum_{j=1}^{m} \frac{Out flow_{j,q} * s_{i,j,q-1}}{Volume_{i,q}},$$
(2)

where i = 1, ..., n indexes stocks, $Volume_{i,q}$ is the total dollar trading volume of stock during quarter q.

$$s_{i,j,q} = \frac{Shares_{i,j,q} * Price_{i,q}}{\text{TNA}_{j,q}},$$
(3)

is fund *j*'s holdings of stock *i* as a percentage of fund *j*'s TNA at the end of the quarter. Additional details regarding the construction of *MFFlow* are in Appendix A.

Coval and Stafford (2007) and Edmans, Goldstein, and Jiang (2012) define a fire sale as a firm-quarter where *MFFlow* falls below the 10th percentile value of the full sample. However, imposing a single threshold for the entire sample period affects the balance of the treated firm sample across time. In unreported tests, we find that using a single threshold for the full sample results in a large concentration of fire-sale firm-quarters during the Internet boom in 1999. To ensure that our results are not driven by a specific time period, we modify the full-sample 10% threshold. We define an event as a firm-quarter in which a firm's *MFFlow* is in the top decile of all firms that quarter (the 'local cutoff'), *and* to ensure that these are indeed fire sales, we also require that it is in the top quintile of the full sample (the 'global cutoff').

Figure 1 plots cumulative average abnormal returns (CAARs) in the three quarters before and after fire sales for all fire-sale firms as well as the subsample of these firms that have credit ratings. In particular, the abnormal returns are measured relative to the CRSP equal-weighted index (Panel A), and also to characteristic-matched portfolios from Daniel, Grinblatt, Titman, and Wermers (1997) (DGTW, Panel B). Both panels show that abnormal returns for the full sample of stocks are significantly negative (-4% to -5%) during the event quarter. We do not observe significant negative abnormal returns prior to the event quarter. The figure is similar to that in Edmans, Goldstein, and Jiang (2012), except that we find a slightly quicker recovery due to differences in sample periods and in the threshold imposed. The figure also shows that CAARs for the subsample of treated firms that have credit ratings appear muted relative to the full sample. Firms with credit ratings that experience fire sales have a smaller dip in prices in the event quarter and a faster recovery. These patterns are consistent with the fact that rated firms are generally larger and more liquid than unrated firms and hence more resilient to price pressure from mutual fund fire sales. These patterns

are also consistent with rating agencies successfully dampening down the effects of fire sales. Determining whether ratings cause a smaller return response to fire sales is difficult because firms select whether to be rated and answering this question is beyond the scope of this paper.¹⁰

2. Setting up the tests

This section presents summary statistics, the propensity score model for a stock to be a fire sale, and the properties of treated and control firms.

2.1. Summary statistics

Table 1 presents summary statistics for the sample used in this paper. Panel A shows the number of firm-quarters that are treated and not treated every year for the sample of firms that have credit ratings. The panel also reports the fraction of treated and non-treated firms that are downgraded every year. Overall, there are about 6,400 treated firm-quarters that are reasonably evenly distributed over time. Panel B displays summary statistics for other important variables used in our analysis including raw returns, risk-adjusted returns, CDS spread changes and firm characteristics such as (log) market capitalization, book-to-market equity, leverage, liquidity, and mutual fund ownership.

2.2. Propensity score model

Table 2 presents results for a propensity score model for a firm to be a fire-sale stock in quarter q. The predictor variables are as of the end of quarter q-1. We estimate both OLS and logit models with a dependent variable that equals one if a stock is a fire-sale stock that quarter. The first three specifications use OLS and also include time fixed effects (year-quarter). The first specification shows that small, illiquid stocks with low leverage and high mutual fund ownership are more likely to experience fire sales. The second specification also includes ratings changes over the past three months (i.e., quarter q-1) and the past 12 months (q-4 through q-1). The effects of past rating changes are, if anything, in the opposite direction from

¹⁰Nevertheless, in the Internet Appendix, we test whether average returns in the fire-sale quarter of rated firms are different from those of unrated firms. We find that rated firms have smaller declines in prices than rated firms, however, these differences are not present after controlling for firm characteristics. Because firms select into the rated sample, we also use Reg FD as shock to CRA information advantage and test if the difference in event quarter returns changes after Reg FD. We find that the dip in prices is indeed smaller post Reg FD, but statistical significance is present only in the full sample.

that predicted by the hypothesis that fire-sale firms are of worse quality than a typical stock. An upgrade (rather than a downgrade) over the past 12 months increases the stock's likelihood to be a fire-sale stock. However, this effect disappears over the past three months (the sum of the past three- and past 12-month coefficient is close to zero). Specification 3 shows that past three-month and past 12-month returns do not predict the likelihood of downgrades. These results are not consistent with the hypothesis that selection into the fire-sale sample is related to a firm's performance before the event quarter.

Column (4) in Table 2 is the specification that we use for propensity score matches in our tests. This is a conditional logit specification that allows for fixed effects in a panel setting. As in the earlier specifications, we have year-quarter fixed effects. The column reports marginal effects evaluated at mean values. Coefficients are similar in magnitude between the OLS and conditional logit specifications.

Figure 2 shows propensity scores for treated and control firms. To ensure comparability across time, we set the fixed effects to zero.¹¹ This figure suggests that there is reasonable overlap between treated firms and controls.

2.3. The matches

Table 3 shows that our matching procedure, described in detail in Section 1.2, achieves reasonable covariate balance. Despite imposing stringent matching criteria, we are able to find matches for over two-thirds of the treated sample. Panel A shows that treated and control firms have similar means and standard deviations for all variables in the propensity score model. In particular, means for size, leverage, mutual fund ownership, and past returns are not different between treated and control firms in economic or statistical terms. The Amihud ratio is statistically higher for treated firms than for controls. However, the difference is economically small (about 0.005 or one-seventh of a standard deviation in the treatment sample) and unreported tests confirm that the Amihud ratio does not predict downgrades. There appears to be no difference in the average change in credit rating between treated and control firms in the quarter before the event. However, there is a somewhat more negative average change in credit rating for treated firms over the 12 months before the event than controls. This difference is not statistically significant at the 5% level, and as we see

¹¹This implies that the levels of the propensity scores are not easily interpretable as a probability. Specifically, the mean probability of being a fire-sale stock in the figure is much higher than the true mean, because the intercepts that are set to zero, are negative. However, it ensures that the distance in probability between stocks is comparable across different periods.

later, stems from higher pre-EQ upgrade rates for controls rather than any differences in downgrades.

Panel B shows a reasonable balance between treatment and control samples even for a set of variables not included in the propensity score model. CAPM β and book-to-market are similar across treated and control samples. Most notably, treated and control firms have virtually identical CHS default probabilities before the start of the event quarter.

Consistent with our matching design, Panel B also shows that DGTW-adjusted event quarter returns for treated and control firms are similar in magnitude and statistically indistinguishable from each other. The table also shows that treated firm returns are transitory: they completely reverse over the next six months.¹² However, control firm returns do not reverse over the next six months after the end of EQ, and are significantly different from treated firm returns. The EQ return shock and subsequent recovery are relatively modest in absolute magnitude (1.5%–2%). This is the price we pay for using hypothetical instead of actual fire sales to mitigate selection biases that could arise from manager discretion, examining rated firms that are typically larger and less susceptible to fire sale pressure, and also for choosing stringent matching criteria that result in a well-balanced sample, but also increase the likelihood that firms with more extreme EQ returns remain unmatched. For example, when we relax the requirement that controls are in the same industry as treated firms, we are able to increase the sample size by a quarter resulting in a deeper decline in EQ prices (untabulated) and similar results on CRA actions (see Table 10). We also show that our results are stronger in subsamples where the EQ return shocks are more pronounced on average.

3. Key Results

This section presents our key results on whether CRAs and markets can discern transitory shocks to credit risk.

3.1. Can CRAs see through transitory shocks?

Table 4 presents the main results of this paper. Panel A presents realized downgrade probabilities of treated and control firms over the four quarters EQ-2 through EQ+1, where EQ is the fire sale event quarter.

 $^{^{12}}$ If anything, the recovery is "too strong", in the sense that treated stocks recover about 1% more than they lose in EQ. The Internet Appendix examines calendar-time portfolios of treated and control firms around EQ, and finds that the excess reversal is due to a relatively short period of time when markets were recovering after the 2007–2009 financial crisis. Our results are robust to excluding this period.

The realized downgrade probability is the fraction of firms in the relevant sample (treated or control) that experience a downgrade over a given period. The third column presents the Average Treatment effect on Treated (henceforth, 'Treatment Effect'), or the mean difference in the outcome variable between treated and control firms. Over the three-month period EQ-1 (six-month period EQ-2 and EQ-1), treatment and control firms exhibit parallel trends with similar downgrade probabilities of 2.0% (3.9%) for treated firms and 2.1% (3.8%) for controls. During the event quarter, treated firms have a much lower downgrade probability (2.1%) than controls (3.0%). The difference of -0.92% is highly significant statistically (heteroskedasticity robust t-statistic of -3.3).¹³ The treatment effect is present one quarter after EQ as well, with the difference in downgrade probabilities between treated (2.4%) and control firms (3.2%) of -0.82%. Overall, for the six month period starting with EQ, the difference between treated and control firms is -1.5% (t-statistic of -3.8), two-fifth of the pre-EQ downgrade rate for treated (or control) firms.

Although the average realized downgrade probability for treated firms during EQ is not zero (2.1%), this does not necessarily show that CRAs have failed to identify some transitory shocks. Because treated firms are also subject to fundamental shocks, zero is not the relevant benchmark. The correct benchmark reflects the rate of arrival of fundamental shocks to a sample of firms that is similar to the treated sample and does not condition on contemporaneous returns. A simple estimate that meets this criteria is the downgrade probability for treated (or control) firms in the quarter prior to the event. We see that downgrade probabilities for treated firms are similar in EQ and EQ-1 (2.1% and 2.0%), suggesting that CRAs ignore the price pressure due to fire sales.

Next, we incorporate the severity of rating downgrades in our analysis. Panel B reports results of tests that use the number of notches downgraded as the dependent variable. This variable is zero for upgrades or if there is no change in the credit rating, and equals the number of notches downgraded if there is a downgrade over the test period. These results are similar to those for realized downgrade probabilities considered in Panel A. Treatment and control samples again display parallel trends before the event quarter, with similar expected downgrade notches over the six moths prior to the fire-sale quarter. Over the following two quarters, treated firms have significantly lower expected downgrade notches as compared with controls (0.110 versus 0.144). This difference of 0.0338 downgrade notches is large relative to the average downgrade

¹³We follow Abadie and Imbens (2006) to compute standard errors using the conditional variance with up to 15 nearest neighbors.

notches of 0.085 (0.083) over the six months before EQ for treated (control) firms.

One possibility is that controls are just more volatile than treated firms during the event quarter with greater probabilities of upgrades and downgrades (although we explicitly match on pre-EQ volatility and EQ returns). To investigate this question, Panel C of table 4 shows that there are no differences between treated and controls in the upgrade notches in EQ and the subsequent quarter. We do find a statistically significant difference in the upgrades for the control firms before EQ. However, the difference in expected upgrade notches during EQ-2 and EQ-1 is smaller economically (0.023 versus 0.034 for expected downgrade notches during EQ and EQ+1 as per Panel B) and suggests that CRAs are relatively more positive about control firm creditworthiness pre-EQ. Because we match on the end of EQ-1 credit rating, this pre-EQ difference implies that a few control firms are upgraded to their current rating more recently than treated firms. This makes the subsequent difference in downgrade rates even more surprising because, if anything, CRAs prefer not to reverse recent changes in ratings (Cantor and Mann, 2006).

The matched sample setup implies that any hypotheses that rely on features of ratings common across treated and control firms are unlikely to explain our results. For example, both treated and control firms are equally impacted by discreteness in rating categories, or if CRAs are slow in general to update ratings.

As reported in Table 3, both treated and control firms have negative average excess returns in the event quarter. However, these returns are relatively small in magnitude (about -2% on average). The next panels test whether CRAs are able to discern which shocks are transitory when the negative shocks are large in magnitude and potentially have greater economic impact. To do so, we restrict the sample to firms with negative raw returns in the event quarter. For this subsample, average returns in the event quarter are -12% for both treated and control firms (Internet Appendix). The Internet Appendix also shows that this subsample retains reasonable covariate balance.

Panels A2 and B2 show significant differences between treated and control firms in downgrade probability (6.9% versus 9.2%, Panel A2) and expected number of notches downgraded (0.186 versus 0.250, Panel B2) over the six month period (EQ and EQ+1) in this subsample. For EQ alone, the difference in downgrade probabilities is 1.6%. Thus, the treatment effect increases by a factor of 1.7 for the subsample with negative EQ returns relative to the full sample.¹⁴ Panel C2 shows that there are virtually no differences in

¹⁴In this negative return subsample, the downgrade rate is higher for the treated firms too. This could be because CRAs are less

the expected upgrade notches before the EQ in this subsample. However, over EQ and EQ+1, treated firms have greater expected upgrade notches than control firms. Note that this difference arises from a greater post-EQ decline in upgrade probabilities for control firms relative to treated firms, consistent with control firms receiving a large, permanent negative shock on average in this subsample.

3.2. Can markets see through fire-sale shocks?

An alternative explanation for our results is that CRAs learn which shocks are transitory from markets, rather than through any independent analysis on their part. We consider two prominent market-based assessments of default risk: CDS spreads and predicted default probability from the hazard model in Campbell, Hilscher, and Szilagyi (2008, CHS). CDS spreads are likely to be a better measure of default risk than estimates implied from bond prices, because prior research shows that price discovery primarily happens in CDS markets rather than bond markets (Blanco, Brennan, and Marsh, 2005). Moreover, Collin-Dufresne, Goldstein, and Spencer (2001) show that a large fraction of the variation in bond spreads is driven by liquidity premia, potentially confounding inferences on credit risk. We also consider CHS default probability because this measure optimally combines information from equity markets and accounting statements to predict defaults, and is also available for a wider sample across both firms and time.

3.2.1. CDS markets

Panel A of Table 5 tests whether CDS spreads respond differently to transitory and permanent shocks in equity prices. The sample of firms with available CDS data has only 587 treated firm-quarters (as opposed to 4255 in the main tests) over the period 2002-2015 and significantly larger firms (on average \$12 billion in market capitalization as opposed to \$3.7 billion).¹⁵

Panel A shows that CDS spreads increase by similar magnitudes for both treated and control firms (20 and 14 basis points respectively) over EQ and EQ+1. There are no significant differences between treated

confident in these cases and/or because a higher fraction of treated firms actually experience adverse fundamental shocks along with the liquidity shock when we condition on negative returns.

¹⁵We use the same matching criteria as in the main analysis (section 1.2) but re-estimate the fire sale probability model from Table 2 on the CDS subsample and augment it with past changes in the CDS implied rating (see Internet Appendix for the model and covariate balance). The results are very similar (available upon request) if the overall sample propensity is used instead (or other covariates added) except for the implied rating change analysis in Panel A, where the implied rating downgrade probability is lower pre-EQ (while not being different during or post EQ). Hence, we re-estimate the model over the CDS subsample to get a better match with parallel trends pre-event.

and control firm CDS spreads either before or after EQ; if anything, treated firm CDS spreads tend to widen slightly more than controls over EQ and EQ+1. The next set of tests in Panel A show that results are similar if implied ratings from CDS markets are used instead of CDS spreads. The implied ratings are computed by Markit, and take into account the discreteness of rating categories. Finally, we confirm that our results on CRAs hold in the subsample of firms with traded CDS contracts. In particular, we repeat the analysis of CRA downgrades for treated and control firms from Panel A of Table 4 for this subsample. The difference in CRA downgrade probability between treated and control firms is large and statistically significant. The treatment effect of 1.7% for EQ and 3.9% for the 6 months starting with EQ is, if anything, larger than the effect in the main sample. Taken together, these results show that unlike CRAs, CDS spreads do not distinguish between transitory and permanent shocks in real time.

We also note that the CDS spreads appear to lag stock markets—a large part of the increase in spreads takes place during EQ+1 rather than EQ. This lag is also visible in Panel A of Figure 3, which presents cumulative CDS spread changes for the CDS sample over the three quarters around EQ. The figure computes the cross-sectional mean of the outcome variable in each quarter, followed by time-series mean as in Coval and Stafford (2007). Hence, magnitudes are not exactly the same as those in the table which presents full-sample means, although inferences are similar. The lagged response of CDS markets to stock returns is consistent with Hilscher, Pollet, and Wilson (2015) who find that information appears to flow from equity to CDS markets.¹⁶ Figure 3 also shows that increases in CDS spreads for treated firms are indeed transitory; spreads reverse back to their pre-EQ levels during EQ+2. However, control firm CDS spreads remain elevated through the EQ+3, thereby confirming our research design.

3.2.2. Predicted default probability

Panel B examines CHS default probability for treated and control firms. Similar to CDS spreads, monthly default probabilities increase for both treated (by 0.011%) and control (by 0.007%) firms over the event and subsequent quarter. The increase is economically significant relative to the mean CHS default probability for treated and control firms before the fire-sale quarter of approximately 0.05% per month.

¹⁶Chava, Ganduri, and Ornthanalai (2016) find that equity market responses to credit rating downgrades are muted if the firm has CDS contracts traded on it. They argue that this is due to information flowing from CDS to equity markets prior to the downgrade (they do not find that information flows from CDS to equity markets "at times other than just prior to downgrades").

Panel B in Figure 3 shows that treated firms default probabilities begin to revert after EQ + 1, but control firm probabilities remain elevated over the next three quarters. This is perhaps not surprising, given that one of the inputs into the CHS measure is the exponentially weighted excess stock return over the past 12 months.

The question of whether CDS spreads and predicted default probabilities *should* react to transitory equity price shocks is a thorny one. At one level, the market value of equity has just fallen, thereby increasing leverage and hence default probability, so perhaps an increase in CDS spreads is warranted. But this increase is transitory and reverses over the next few quarters. If credit market participants are aware that the shock was transitory, would spreads on five year CDS contracts increase?

3.2.3. Realized defaults after fire-sales

Panel C of Table 5 provides evidence on whether the increase in credit spreads and predicted default probabilities is justified by realized future defaults. Although both treated and control firms have similar expected default probabilities and levels of CDS spreads before the fire-sale quarter, over the next five years actual realized defaults are twice as high for control firms relative to treated firms (t-statistic of -3.2). The difference in downgrade rates is consistent with our empirical design: treated firms have transitory (nonfundamental) shocks, while controls have permanent (fundamental) ones. The difference in defaults also validates the CRA decision to downgrade control firms more frequently than treated firms, and contrasts with the similar increase in market-implied measures of default risk for both sets of firms. It is interesting to note that the difference in realized defaults occurs more than a year after the event quarter. This suggests that the difference in realized defaults between treated and control firms is unlikely to be caused by any potential direct effects of CRA actions in the event quarter.

4. The CRA information advantage channel

Why do CRAs succeed in discerning that fire-sale shocks to market prices are transitory when markets fail? One possible explanation that does not violate semi-strong form market efficiency is that CRAs possess nonpublic information about a firm's prospects. As discussed in the introduction, CRAs claim that they routinely receive nonpublic information including budgets, internal capital allocation schedules, potential acquisitions, and restructurings in the process of rating a firm. Thus, after seeing a shock to prices, CRAs

can seek information to verify whether fundamentals have indeed deteriorated before issuing a downgrade. Consistent with the existence of information advantage for CRAs, prior research finds that markets react to rating downgrades, and Jorion, Liu, and Shi (2005) show that this reaction increased after information advantage of CRAs increased with the enactment of Reg FD.

We therefore test whether the treatment effect is larger in situations where the CRA information advantage is likely to be larger: in the cross-section, we examine firms with more uncertain fundamentals, and in the time-series, we use Reg FD as a shock to the information advantage of CRAs.

4.1. Uncertainty in public information and fire sale downgrades

For firms with a more uncertain information environment, it is likely that market participants will find it more difficult to tell if a given shock to prices is due to fire sales or changes in fundamentals. For such firms, we expect deeper fire-sale shocks and a greater information advantage of CRAs relative to markets. We test if the treatment effect is greater for such firms. We use measures of uncertainty derived from analyst estimates: the cross-sectional dispersion in analyst forecasts and the time-series standard deviation in forecast errors over a rolling 3-year window before the event quarter.

Table 6 shows results for tests that regress downgrades on a dummy variable for treated firms and interactions with uncertainty variables for our matched sample of treated and control firms. The first specification confirms that our main result holds for the sample of firms with I/B/E/S analyst coverage in a regression setting with year-quarter and industry fixed effects. The coefficient on treated at -0.9% is virtually identical to the EQ ATT in Table 4. Specification 2 shows a similar coefficient, after including rating fixed effects and controls for firm characteristics used in the propensity score model for fire sales in Table 2. Specification 3 introduces a dummy variable that equals 1 if the cross-sectional standard deviation of analyst Earnings Per Share (EPS) estimates is above the matched-sample median. Firms with more analyst disagreement are more likely to be downgraded, consistent with uncertain firms being more risky for debt holders. The interaction between treated and disagreement is large and significant. Treated firms that are uncertain are much less likely to be downgraded relative to controls and treated firms that are less uncertain. The next specification shows that results are similar if the uncertainty dummy variable is based on the time-series standard deviation of median forecast errors instead of analyst disagreement.

The table also shows that treated firms that have below median values of the uncertainty measures are

not less likely to be downgraded than controls. This is at least partially because the decline in prices in the event quarter for treated firms with low uncertainty is less than half that of firms with high uncertainty (unreported results). These treated firms are matched to controls that have similarly small dips in prices in the event quarter. Thus, both treated and control firms with low uncertainty are unlikely to have a substantial fundamental shock in the event quarter, and hence, are both unlikely to be downgraded. These results also suggest another test: the extent of the price dip due to fire sales should matter for the treatment effect, which we examine in section 5.1 below.

In our final specification, we also test if the extent of analyst coverage as measured by the 1/Number of analysts covering a stock matters for the treatment effect. This variable is insignificant by itself and also when interacted with the treated dummy, suggesting that the extent of coverage by itself does not matter, but the uncertainty of analysts does.

These results show that CRAs are complementary to equity analysts, in the sense that CRAs add the most value in situations in which analyst forecasts are less precise.

4.2. Reg FD as a shock to the information advantage of CRAs

To further identify the channel through which CRAs discern transitory shocks, we use Reg FD as an exogenous shock to the information environment of CRAs. In October 2000, the enactment of Reg FD prohibited firms from selectively disclosing material information to investors or market professionals such as equity analysts. However, disclosure to CRAs was exempt from its provisions. Thus, we expect that the information advantage of CRAs relative to the market increased after the enactment of Reg FD. Jorion, Liu, and Shi (2005) find evidence consistent with this hypothesis: stock price reactions to downgrades increased significantly after Reg FD. Hence, we use Reg FD as a shock to the information advantage of CRAs and test whether the ability of CRAs to discern transitory shocks to stock prices improved after it was enacted. These results can be interpreted causally, if we assume that the enactment of Reg FD was exogenous and hence, the assignment of fire sales to firms did not change after Reg FD.

To minimize the effect of other potential changes, we restrict our sample in this test to a relatively short period around Reg FD. In particular, we use exactly the same sample period as Jorion et al. (2005)—9 quarters before and 9 quarters after October 2000. We estimate the following regression:

$D_{i,t} = b_1 \times Treated_{i,t} + b_2 \times Treated_{i,t} * RegFD_t + b'_3 \times Controls_{i,t} + FEs + \varepsilon_{i,t},$

where $D_{i,t}$ is 1 (0) if firm *i* experiences a downgrade (upgrade or no change) in period *t*, *Treated*_{*i*,*t*} is 1 (0) if the firm *i* is a treated (control) firm, *RegFD*_{*t*} is 1(0) for nine quarters after (before) October 2000, *FEs* are fixed effects, and *Controls* are firm specific characteristics used in the propensity score model in Table 2. All specifications includes time fixed effects. These fixed effects, along with our treatment-control setup, mitigate the influence of any shocks that affect all firms at a given point in time (e.g., business cycles). We also do not include *RegFD*_{*t*} by itself as it is subsumed by the time fixed effects.

Table 7 reports results from this regression. The first specification tests whether the treatment effect is significant over the entire subsample without conditioning on Reg FD. We see in Panel A that the treatment effect over the fire sale quarter is -1.3%, which is slightly larger in magnitude than the -0.9% treatment effect reported in Table 4. However, perhaps because the sample size is now much smaller (with only about one-fifth the number of treated firms relative to the main tests), the coefficient is marginally insignificant in the event quarter in Panel A, while it is significant over the event and subsequent quarter in Panel B. The next specification finds similar results after including the firm characteristics from the propensity score model in Table 2 as controls.

Specification (3) is the first test that includes the *RegFD* dummy interacted with *Treated*. The interaction is statistically and economically significant in both panels. The treatment effect is -4.7% over the nine quarters after Reg FD and virtually zero in the nine quarters immediately preceding the regulation.¹⁷ The next two specifications introduce Industry and Credit Rating fixed effects, with very little change in the coefficients of the *RegFD* × *Treated* interaction. Thus, these results show that Reg FD significantly improved the ability of CRAs to distinguish between transitory and permanent shocks to stock prices.

In October 2010, the Dodd Frank Act removed the explicit exemption for CRAs from Reg FD. We do not use this second shock as an additional test for two reasons. First, it is not clear whether this change has had any material effect on the access of CRAs to nonpublic information. CRAs argue the removal of the specific exemption does not affect their access to nonpublic information because they meet other criteria

¹⁷However, the lack of an effect just prior to Reg FD does not imply that there was no effect in the entire sample period before Reg FD. We see below in section 5.3 that a treatment effect exists over 1990-1997.

for exemption in Reg FD: they do not seek to make investment decisions based on the private information, and their engagement letters with firms contain confidentiality agreements (Carbone, 2010). Second, the Dodd Frank Act is not a clean shock to the information environment because it also made other changes to the legal environment of CRAs. These changes include increasing the legal liability for issuing inaccurate ratings, and making it easier for the SEC to impose sanctions against CRAs. Dimitrov, Palia, and Tang (2015) find that these changes affected the information content of ratings for equity and bond markets.

5. Falsification tests and robustness

In this section, we perform a series of tests that examine whether the treatment effects are robust and vary across different settings in directions we a priori expect. In particular, we first test whether the treatment effects are strongest for firm quarters in which fire sales have the greatest impact. Second, we examine the robustness of the effect over time and in the cross-section of rated firms. Third, we test whether the treatment effects are absent in a falsification test in which the mutual fund selling pressure is not based on fire sales, but on all mutual fund sales. Finally, we examine robustness of the treatment effect to changes in the matching procedure.

5.1. The V-shaped pattern in returns and CRA actions

The defining feature of a deep transitory shock is the V shape in returns: stock prices fall in the event quarter and recover over the next few quarters. A shallower dip or no subsequent recovery may be situations in which the economic impact of fire sales is small or where we have misclassified permanent shocks as temporary ones. If CRAs are indeed able to perceive transitory shocks, their actions should be most salient for shocks that most closely exhibit the V-shaped pattern in returns. Moreover, treated stocks with non-negative abnormal returns in the fire sale quarter are matched to firms that have similar returns. In this situation, neither treated nor control firms are likely to have experienced a negative fundamental shock and hence we should expect no difference in downgrade rates.

To test this hypothesis, we classify treated firms into two groups based on their returns in the event quarter. We also independently sort treated firms into two groups based on excess returns (over the market) in the six months after the event quarter.¹⁸ We choose the six month horizon because on average, fire-sale firm returns in our sample recover by the end of quarter EQ+2 (see Figure 1). We measure excess returns over the market for the recovery because any information that CRAs have is likely to be firm-specific and not systematic. Firms in the top right bin (low EQ return, high return over the next two quarters) are firms with the most pronounced V-shape in returns, where we expect the greatest difference between downgrade probabilities of treatment and control firms. In contrast, in the bottom left bin (high EQ return, low next two quarter return), we are less confident that the shock is indeed transitory or even present. We expect smaller differences between treatment and control firms in this bin.

Panel A of Table 8 reports differences in downgrade probability. Firms with the most pronounced V-shape—those in the low event quarter return and high post-EQ return group—have a lower likelihood of downgrades relative to controls by -1.38% while the difference in downgrade probability for the least V-shaped group is -0.58%. Panel B conducts similar analysis but with regards to downgrade notches while Panels A2 and B2 focus on the subsample with only negative EQ returns (as do Panels A2-B2 of Table 4). Across all panels, we see similar patterns: the difference in the most V-shaped group is 2.5-4 times larger than in the least V-shaped group; if there is strong recovery, the difference in downgrade probability becomes more negative as the event quarter return falls.

5.2. Placebo test

This section reports a placebo test that examines whether our results are specific to fire sales by mutual funds, or are due to properties of mutual fund ownership and outflows. In the placebo test, our treatment sample is derived from all mutual fund sales that are not fire sales. In particular, the fire sale variable in our main tests is constructed by assuming that mutual funds that experience outflows greater than 5% of their assets sell their holdings in proportion to their beginning-of-quarter weights. For the placebo test, we remove the 5% threshold and generate a placebo treatment variable based on sales of *all* mutual funds in proportion to their weights in response to outflows. To ensure that this is truly a placebo, we exclude any fire-sale stock-quarters from the placebo sample. Placebo-treated firms are those with total implied selling pressure in the top local and global quartile of mutual fund sales. Because we lose some firms when we exclude

¹⁸We note that higher downgrade rates for control firms may by itself cause lower returns in the control group. We therefore explicitly avoid conditioning on the control group returns in this analysis.

true fire sales, we use quartiles as the cutoff to ensure that sample sizes are comparable over placebo and actual treated samples. Control firms are identified in the same manner as in our main results in Table 4 (see Internet Appendix for the probability model and covariate balance for the placebo sample).

Table 9 shows the results of this placebo test. We find no difference in the downgrade probabilities between the placebo-treated and control firms before, during, or after the placebo treatment quarter. These results show that any alternative explanations that rely on differences in characteristics of stocks owned by mutual funds, or the existence of information about a stock's prospects before the event quarter are unlikely to drive our results. In unreported results, we find that returns for the placebo treatment sample do not reverse after the placebo treatment quarter, confirming that these are not transitory shocks.

5.3. The effect across rating categories and over time

Figure 4 shows downgrade probabilities for treated and control firms by broad rating category. Realized downgrade probabilities continue to measure downgrades across narrow categories—we merely present results by broad rating categories (i.e., ignoring '+' and '-') in the figure to ensure sufficiently large samples within each rating category. Panels A and B show that in general across all firms, whether treated or control, downgrade probabilities follow a 'U'-shaped pattern. Downgrade probabilities decrease as credit risk increases from AA, reaching a minimum at BBB, and increase thereafter.¹⁹ BBB firms are just above the investment grade threshold.

Panel B shows that the treatment effect is robust. In particular, treated firms are less likely to be downgraded than controls for all categories except AA. The latter difference may be insignificant because we are able to match only 90 AA treated firm-quarters (untabulated), 3.5 times less than the next smallest bin (B).

We next examine time-variation in the treatment effect to test whether the results are driven by a few years, which might imply that they are due to specific events such as the financial crisis. Figure 5 shows downgrade probabilities for treated and control firms in the event quarter (Panel A) and in the event quarter and subsequent quarter (Panel B). We split the sample into pre and post the onset of the 2007–2009 financial crisis. Overall, although the treatment effect varies over time, the figure shows that our results are not driven by a few points in time. The treatment effect is highest in the original Reg FD period when CRAs

¹⁹We do not plot AAA and categories below B because they have few observations.

had privileged access to information and lowest during the crisis. The effect is present, but small in the post-crisis, modified-Reg FD period.

5.4. The matching procedure

As discussed above, our matching procedure balances the need to maximize sample size with the need to have close matches. Table 10 examines the robustness of our results to changing our matching criteria. Columns (1) through (5) are identical to the corresponding columns in Table 4. For brevity, we focus on differences in downgrade probabilities between treated and control firms over the six-month interval starting with EQ.

The first line in Table 10 reproduces the baseline results from Table 4 for ease of comparison. We consider several changes to the matching procedure and to the data sample. Within each major robustness category, we report results where we increase the maximal event quarter return distance between a treated and control firm ('+ wider return caliper'), maximal pre-EQ propensity score distance between treated and control firms ('+ wider pscore caliper'), and number of controls matched for each treated firm ('+ multiple controls'). The + sign denotes that the current specification is the previous specification along with the change specified after the + sign.

In the first set of results, we see that increasing the return caliper, increasing the propensity score caliper, and adding additional controls per treated firm do not significantly affect the baseline results. These changes serve to increase the sample size by approximately 1,000 firms.

The next specification examines the robustness of the main result to excluding the period of financial crises of 2007–2009, firms in financial services and utility industries, and dropping the biggest part of the sample—manufacturing firms. We see that the difference between treated and control groups is statistically significant across these subsamples, and is typically within one standard deviation of our baseline estimations for all firms and for those with established CDS markets.

Next, we change some of the criteria used in the baseline matching scheme. First, we examine the effects of matching only within narrow rating categories (i.e., BBB is now considered different from BBB–). In our main specifications, we first search for a match within the narrow category, then seek a match across the broad category if no matches are available within the narrow category. Restricting matches to within narrow categories reduces the sample size by approximately 1,000 firms. However, the treatment effect

remains robust and about the same magnitude as the baseline. Increasing sample size by relaxing return and propensity score calipers within the narrow matching scheme does not affect results.

Next, we repeat the analysis using a finer industry classifications scheme, the Fama-French 12 industry classification, instead of the five industry classification used in the baseline. We then consider a match without regards to industry. Neither change materially affects the significance and magnitude of the treatment effect.

We also consider a different matching scheme. Rather than matching treated firms to controls that have the closest EQ return within a propensity score caliper, we match them to controls with the closest propensity score (subject to a maximum difference of 0.025) and EQ returns within the same quintile (or decile). We do this for both the narrow rating matches as well as coarse rating matches. Results are robust to all these changes.

Additionally, in the Internet Appendix (Tables IA.1 and IA.2), we examine whether results are similar if we use Moody's ratings instead of S&P. Our Moody's sample is shorter (1990–2008) and has a lower match rate to CRSP data, leading to substantially fewer observations. Nevertheless, the difference between treated and control firms in downgrade probability (as well as the number of notches downgraded over EQand EQ+1) is significant and of similar magnitude to that of the larger S&P sample.

Finally, a possible hypothesis is that CRAs wait to see if a shock begins to reverse within a quarter and only downgrade firms if there is no reversal. It is important to note, however, that treated firms are matched to controls with similar returns for the full quarter. So firms that partially or fully recover are matched to controls with small or no fundamental shocks. Second, our results suggest that CRAs do not wait for the entire quarter to see a recovery, because they downgrade controls more frequently for shocks within the event quarter. Nevertheless, we restrict the sample of treated firms to those with no meaningful recovery within EQ and find similar results. Additional details of these tests are available in the Internet Appendix.

Overall, these results suggest that our baseline estimations are representative of the effect of fire sales on rating downgrades and that our statistical inference is robust to the matching scheme choice and peer-firm definition.²⁰

 $^{^{20}}$ Additional unreported tests show that inferences stay the same for similar robustness tests on the negative *EQ*-return and CDS subsamples.

6. Conclusion

This paper shows that CRAs distinguish between transitory and permanent shocks to credit risk, while market based estimates of default risk do not. Our paper has three related implications. First, our results imply that CRAs actually play a role as information intermediaries. One of the traditional arguments for the existence of CRAs is that they act as intermediaries between borrowers and the market. Rather than revealing potentially private information to the entire market (including competitors), firms can reveal information to CRAs who analyze the information and provide a public summary of the information that markets are interested in: is the borrower still creditworthy? However, given the availability of market-implied measures of credit risk for publicly-traded firms and concerns regarding the accuracy of CRAs due to conflicts of interest and catering, it is not clear what value CRAs add as information intermediaries. We demonstrate one channel through which CRAs add value as intermediaries: they distinguish between transitory and permanent shocks to credit risk.

A related implication is that markets are not perfect substitutes for CRAs. For example, Flannery, Houston, and Partnoy (2010) argue that CDS spreads should be used instead of credit ratings in contracts and regulations. Our results suggest that if measures of credit risk based on market prices are embedded into contracts or used for regulatory purposes, it might allow transitory shocks in financial markets to propagate to the real economy. For example, a transitory shock to credit risk could trigger a contractual provision across all of a firm's suppliers and thereby affect the firm's ability to purchase raw materials. This will in turn affect the firm's production and could cause additional real effects downstream. Thus, our results suggest that CRAs may act as circuit-breakers by dampening the real effects of friction-driven shocks in equity markets.

This role of CRAs depends crucially on their access to private information and their ability to process this information. Since the financial crisis, CRAs have lost some credibility with regulators and markets and the thrust of regulatory policy over the past few years has been to reduce the special role of CRAs. For example, the Dodd-Frank act mandates the removal of ratings from regulations and also removes the exemption of CRAs from Reg FD. Although the actual regulatory action may not have materially impacted the access of CRA, its intent appears to be to reduce such access. A final implication of our results is that although any future regulations that reduce the access of CRAs to private information may have benefits (e.g. encouraging information production from other market participants instead of relying on CRAs), such regulations also have costs. Specifically, if CRAs do not have access to information, they may not be able to distinguish between real and transitory shocks to market prices. This could amplify the real effects of transitory shocks to market prices.

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Table 1: Summary statistics

This table reports summary statistics for the data used in this study. Panel A reports the number of firm-quarters of treated and not treated observations each year in our sample. Treated firms are those that experience fire sales by mutual funds as defined in Section 1.3. Panel B provides summary statistics for other variables used in our study at the firm-quarter frequency.

		Treated		Not Treated			
	Firm-Qtrs	Downgrades	Ratio	Firm-Quarters	Downgrades	Ratio	
1990	72	6	0.083	1,906	122	0.064	
1991	111	2	0.018	2,394	118	0.049	
1992	86	0	0.000	2,415	67	0.028	
1993	89	1	0.011	2,966	92	0.031	
1994	222	1	0.005	3,410	84	0.025	
1995	186	4	0.022	3,595	90	0.025	
1996	239	6	0.025	3,900	82	0.021	
1997	278	4	0.014	4,099	96	0.023	
1998	325	7	0.022	4,946	178	0.036	
1999	392	7	0.018	5,366	229	0.043	
2000	476	18	0.038	5,015	245	0.049	
2001	176	8	0.045	4,696	277	0.059	
2002	362	18	0.050	4,931	330	0.067	
2003	304	4	0.013	4,791	224	0.047	
2004	369	4	0.011	5,021	150	0.030	
2005	314	12	0.038	5,061	193	0.038	
2006	333	7	0.021	5,233	182	0.035	
2007	278	6	0.022	5,241	217	0.041	
2008	242	24	0.099	5,148	312	0.061	
2009	224	22	0.098	4,725	328	0.069	
2010	264	3	0.011	4,721	104	0.022	
2011	185	4	0.022	4,931	139	0.028	
2012	329	6	0.018	4,801	139	0.029	
2013	242	1	0.004	4,869	95	0.020	
2014	218	3	0.014	5,091	94	0.018	
2015	151	2	0.013	4,051	144	0.036	
Total	6,467	180	0.028	113,323	4,331	0.038	

Panel A: The rated firm-quarter sample

Panel B: Other variables

	Firm Otro	maan	ed	alzany	n 1	n25	n5 0	p75	p 00
		mean	su	SKEW	pı	p23	p50	p75	p99
Return (Raw)	146,272	0.029	0.224	0.690	-0.578	-0.081	0.026	0.132	0.761
САРМ β	139,881	1.097	0.711	1.179	-0.116	0.611	0.998	1.444	3.375
Return (DGTW)	135,494	0.000	0.184	0.611	-0.514	-0.091	-0.005	0.084	0.591
log(Realized Variance)	146,564	-7.708	1.139	0.473	-9.905	-8.499	-7.807	-7.036	-4.545
log(Mkt CAP)	146,030	7.393	1.869	-0.273	2.517	6.258	7.458	8.602	11.582
Book-to-Market	130,288	0.799	1.287	7.010	0.013	0.280	0.549	0.912	6.202
Debt-to-EV	130,299	0.281	0.178	0.339	-0.001	0.144	0.282	0.403	0.663
Mutual Fund Ownership	142,152	0.142	0.110	1.123	0.001	0.052	0.120	0.213	0.436
Amihud Ratio	146,553	0.023	0.053	2.878	0.000	0.000	0.002	0.012	0.245
Rating Change past 12 months	145,168	-0.249	1.889	-8.800	-5.000	0.000	0.000	0.000	2.000
MFF selling	146,565	-0.007	0.096	-300.2	-0.072	-0.006	-0.002	-0.000	0.000
MFF Pscore FE=0	127,436	0.898	0.077	-1.974	0.616	0.867	0.917	0.951	0.991
Placebo selling	146,565	0.005	0.109	-179.5	-0.072	-0.003	0.000	0.007	0.144
Placebo Pscore FE=0	127,436	0.777	0.080	-1.648	0.485	0.739	0.790	0.833	0.894
CHS Default Prob (%)	135,335	0.083	0.190	9.805	0.015	0.029	0.041	0.066	0.916
CDS spread change	24,076	0.001	0.035	29.66	-0.032	-0.001	-0.000	0.001	0.049

Table 2: A probability model for fire sales

We estimate models for the probability of a stock to experience a fire sale as a function of one-quarter lagged firm characteristics, past rating changes, stock returns, and year-quarter fixed effects. The outcome is one if the firm-quarter meets the criteria to be a fire sale as defined in Section 1.3, and zero otherwise. Specification (1) through (3) report linear probability model estimates. Specification (4) reports marginal effects estimated at means from a conditional logit model. Standard errors for t-statistics (reported in parentheses) are clustered by firm, and */**/*** denote significance at 10/5/1% confidence level.

		OLS		Logit
	(1)	(2)	(3)	(4)
log(Market Cap)	-0.0112***	-0.0113***	-0.0113***	-0.0233***
	(-9.24)	(-9.26)	(-9.21)	(-23.64)
log(1+ Debt-to-EV)	-0.0409^{***}	-0.0413***	-0.0406^{***}	-0.0684^{***}
	(-4.27)	(-4.23)	(-4.12)	(-10.16)
MF Ownership	0.2725***	0.2733***	0.2724***	0.3860***
	(12.71)	(12.71)	(12.63)	(31.88)
Amihud Ratio	1.0257***	1.0223***	1.0250***	0.8803***
	(14.69)	(14.59)	(14.58)	(30.59)
log(Realized Variance)	-0.0259***	-0.0260***	-0.0256^{***}	-0.0403^{***}
	(-15.90)	(-15.87)	(-15.58)	(-30.67)
Return (3 month)		0.0040	0.0043	0.0138**
		(1.28)	(1.33)	(2.08)
Return (12 month)		-0.0021	-0.0028	-0.0040
		(-1.21)	(-1.63)	(-1.32)
Rating Change (3 month)			-0.0319 **	-0.0746^{***}
			(-2.51)	(-2.97)
Rating Change (12 month)			0.0423***	0.1020***
			(3.35)	(4.57)
Observations	113,597	113,469	112,695	112,610
R^2 / Pseudo R^2	0.0374	0.0374	0.0376	0.0788

Table 3: Covariate balance for treated firms and controls

This table presents means and standard deviations of selected variables for fire-sale ('treated') stocks and controls. A 'treated' firm experiences a fire sale in a given event quarter (EQ). Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 1.2 for details. The fewer number of control relative to treatment firm-quarters indicates that some controls are matched to multiple treated firms. Panel A reports variables used in the propensity score model while Panel B reports other variables of interest.

	N(Treated)=4255, N(Control)=4020						
		Means		St.Deviations			
	Treated	Control	P-value	Treated	Control		
	(1)	(2)	(3)	(4)	(5)		
MCap(USD bln)	3.702	3.911	0.348	8.179	7.496		
Debt-to-EV	0.317	0.323	0.540	0.208	0.210		
Mutual Fund Ownership	0.191	0.185	0.193	0.119	0.115		
Rating Change past 3 months	-0.007	-0.001	0.573	0.410	0.473		
Rating Change past 12 months	-0.032	0.001	0.073	0.773	0.920		
Return past 3 months	0.034	0.036	0.498	0.158	0.171		
Return past 12 months	0.152	0.147	0.674	0.347	0.369		
Realized Volatility past 3 months	0.060	0.059	0.389	0.034	0.035		
Amihud Ratio	0.019	0.014	0.000	0.040	0.035		

Panel A: Propensity-score contributors

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		T 1	Means	D 1	St.Deviations	
		Treated	Control	P-value	Treated	Control
		(1)	(2)	(3)	(4)	(5)
	САРМ β	0.937	0.969	0.362	0.602	0.666
mma EO	Book-to-Market	0.729	0.726	0.898	0.846	0.901
pre EQ	Book leverage	0.386	0.403	0.309	0.281	0.273
	CHS Default Prob.	0.051	0.052	0.573	0.057	0.066
	Raw Return	0.012	0.012	0.990	0.145	0.145
during EQ	Excess Return (Mkt)	-0.017	-0.017	0.990	0.127	0.127
	Excess Return (DGTW)	-0.014	-0.016	0.554	0.120	0.119
6 month	Cumulative Return	0.088	0.063	0.005	0.269	0.267
	Cumulative Return (vs Mkt)	0.041	0.016	0.009	0.249	0.246
allel EQ	Cumulative Return (vs DGTW)	0.026	0.004	0.004	0.221	0.219

Table 4: Fire sales and credit ratings

This table examines the effect of mutual fund fire sales on credit ratings. A 'treated' firm experiences a fire sale in a given event quarter (EQ). Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 1.2 for details. Column (3) presents 'Average Treatment effect on Treated' (ATT), or the difference in the outcome variable between treated and control firms; a negative number indicates lower mean outcomes for treated firms relative to controls. In Panel A, the outcome variable equals 1 if the credit rating was downgraded during the period and zero otherwise. In Panel B, we take into account the severity of downgrades by reporting the average number of notches downgraded (E[#NotchesDown]). Panel C reports the average number of notches upgraded. Panels A2, B2, and C2 report the same tests as panels A, B, and C but on a subsample of firms with negative EQ returns. The time periods we consider are: EQ-2 and EQ-1 (the 6 months before the start of EQ), EQ-1, EQ, EQ+1, and EQ and EQ+1 (the 6 months starting at the beginning of EQ). Standard errors and t-statistics reported in columns (4) and (5) respectively are robust for heteroskedasticity.

		N(Treated) = 4255						
	Treated	Control	ATT	SE	t-stat			
	(1)	(2)	(3)	(4)	(5)			
EQ-2 and EQ-1	0.039	0.038	0.0009	0.0034	0.28			
EQ-1	0.020	0.021	-0.0007	0.0025	-0.29			
Event Quarter	0.021	0.030	-0.0092	0.0028	-3.32			
EQ+1	0.024	0.032	-0.0082	0.0029	-2.88			
EQ and EQ+1	0.043	0.057	-0.0146	0.0038	-3.82			

Panel A: *Pr*{*Downgrade*}

Panel B:	E[#NotchesDown]	
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	N(Treated) = 4255						
	Treated	Control	ATT	SE	t-stat		
	(1)	(2)	(3)	(4)	(5)		
EQ-2 and EQ-1	0.085	0.083	0.0019	0.0082	0.23		
EQ-1	0.042	0.044	-0.0016	0.0056	-0.29		
Event Quarter	0.047	0.066	-0.0195	0.0066	-2.96		
EQ+1	0.065	0.082	-0.0172	0.0126	-1.36		
EQ and EQ+1	0.110	0.144	-0.0338	0.0148	-2.29		

Panel C: *E*[#*NotchesUp*]

	N(Treated) = 4266					
	Treated	Control	ATT	SE	t-stat	
	(1)	(2)	(3)	(4)	(5)	
EQ-2 and EQ-1	0.068	0.091	-0.0230	0.0080	-2.88	
EQ-1	0.030	0.044	-0.0134	0.0040	-3.34	
Event Quarter	0.030	0.030	-0.0005	0.0038	-0.12	
EQ+1	0.032	0.028	0.0035	0.0039	0.89	
EQ and EQ+1	0.061	0.058	0.0030	0.0053	0.58	

Table 4: Fire sales and credit ratings (Continued)

	N(Treated) = 2128						
	Treated	Control	ATT	SE	t-stat		
	(1)	(2)	(3)	(4)	(5)		
EQ-2 and EQ-1	0.050	0.045	0.0047	0.0059	0.80		
EQ-1	0.024	0.028	-0.0038	0.0043	-0.88		
Event Quarter	0.033	0.048	-0.0155	0.0054	-2.89		
EQ+1	0.039	0.053	-0.0136	0.0057	-2.40		
EQ and EQ+1	0.069	0.092	-0.0230	0.0073	-3.14		

Panel A2: $Pr\{Downgrade | EQret < 0\}$

Panel B2: E[#NotchesDown|EQret < 0]

	N(Treated) = 2128						
	Treated	Control	ATT	SE	t-stat		
	(1)	(2)	(3)	(4)	(5)		
EQ-2 and EQ-1	0.113	0.102	0.0108	0.0139	0.78		
EQ-1	0.056	0.061	-0.0047	0.0099	-0.48		
Event Quarter	0.076	0.108	-0.0320	0.0134	-2.39		
EQ+1	0.113	0.151	-0.0376	0.0260	-1.44		
EQ and EQ+1	0.186	0.250	-0.0639	0.0306	-2.09		

	N(Treated) = 2133						
	Treated	Control	ATT	SE	t-stat		
	(1)	(2)	(3)	(4)	(5)		
EQ-2 and EQ-1	0.073	0.080	-0.0066	0.0104	-0.63		
EQ-1	0.037	0.038	-0.0019	0.0076	-0.25		
Event Quarter	0.030	0.023	0.0075	0.0051	1.48		
EQ+1	0.029	0.017	0.0122	0.0050	2.45		
EQ and EQ+1	0.059	0.040	0.0192	0.0070	2.73		

Panel C2: E[#NotchesUp|EQret < 0]

Table 5: Markets versus Ratings

This table examines the effect of mutual fund fire sales on debt markets and CRA decisions. A 'Treated' firm experiences a fire sale in a given event quarter (EQ). Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 3.2 for details. Panel A compares the CDS spread change and CDS-implied rating downgrades changes with those by CRAs, for the sample with traded CDS contracts, Panel B compares downgrades by CRAs with changes in the default probability estimates from the model in Campbell et al. (2008) that uses market and accounting data for the full sample (1991-2015), and Panel C compares the realized frequency of bankruptcy filings by treated and control firms during one- and five-year periods after EQ for the CDS and the full samples.

		Treated	Control	ATT	SE	t-stat
		(1)	(2)	(3)	(4)	(5)
	EQ-2 and EQ-1	0.839	-6.604	7.4426	10.0154	0.74
CDS spread	EQ-1	-0.438	-3.694	3.2557	8.8726	0.37
change (bps)	Event Quarter	5.328	2.363	2.9649	15.0708	0.20
	EQ and EQ+1	20.339	14.207	6.1325	19.4703	0.31
	EQ-2 and EQ-1	0.034	0.034	0.0000	0.0091	0.00
CDS implied	EQ-1	0.012	0.015	-0.0034	0.0048	-0.71
downgrade	Event Quarter	0.017	0.017	0.0000	0.0065	0.00
	EQ and EQ+1	0.039	0.037	0.0017	0.0107	0.16
	EQ-2 and EQ-1	0.043	0.032	0.0102	0.0099	1.03
CDA down one do	EQ-1	0.019	0.015	0.0034	0.0071	0.48
CKA downgrade	Event Quarter	0.019	0.036	-0.0170	0.0074	-2.29
	EQ and EQ+1	0.031	0.070	-0.0392	0.0097	-4.04

	Panel	B : Full Sa	ample			
		Treated Control		ATT	SE	t-stat
		(1)	(2)	(3)	(4)	(5)
CHS default prob. change	EQ-2 and EQ-1	0.001	0.001	-0.0001	0.0017	-0.08
	EQ-1	0.001	0.001	0.0004	0.0015	0.30
	Event Quarter	0.006	0.006	-0.0007	0.0020	-0.36
	EQ and EQ+1	0.011	0.007	0.0040	0.0039	1.03
CPA downgrada	EQ-2 and EQ-1	0.040	0.040	0.0002	0.0037	0.06
	EQ-1	0.021	0.022	-0.0018	0.0027	-0.69
CICH downgrade	Event Quarter	0.023	0.033	-0.0103	0.0030	-3.48
	EQ and EQ+1	0.047	0.065	-0.0184	0.0041	-4.46

Panel C: Realized default probabilities post-EQ

		Treated	Control	Diff	SE	t-stat
		(1)	(2)	(3)	(4)	(5)
CDS sample	within 1 year within 5 years	$0.002 \\ 0.002$	0.004 0.017	0.0021 -0.0157	0.0033 0.0061	-0.62 -2.58
Full sample	within 1 year within 5 years	0.002 0.015	0.002 0.029	0.0007 -0.0139	0.0012 0.0043	0.62 -3.18

Table 6: Rating downgrades and informational asymmetry

This table examines the effect of mutual fund fire sales on credit ratings across different groups of firms. Using the matched sample of treated and control firm-quarters with analyst coverage, we regress a dummy variable that equals one if there is a downgrade in the fire sale quarter on a dummy variable that equals one for fire-sale firms ('Treated'). In specification (1), we corroborate the analysis reported in Table 4 (Panel A, EQ ATT) in a regression setting with time-by-industry fixed effects. In specification (2), we add credit rating fixed effects as well as firm characteristics from the fire-sale propensity model used for matching (see Table 2). In other specifications, we include interactions of the 'Treated' dummy with measures of information uncertainty from one quarter before the event quarter. 'AF Disagreement' ('AF StdError') is a dummy variable taking a value of one if the cross-sectional standard deviation of analyst EPS forecasts (analysts' median EPS forecast error over 3-year rolling window) exceeds the matched sample median. '1/Analysts' is the inverse of the number of analysts covering the stock. We report t-statistics in parentheses that are based on standard errors clustered by event quarter, and */**/*** denote significance at the 10/5/1% confidence level.

	(1)	(2)	(3)	(4)	(5)
Treated	-0.009**	-0.010**	-0.003	-0.001	-0.011*
	(-2.32)	(-2.54)	(-0.55)	(-0.25)	(-1.95)
Treated × AF Disagreement			-0.019**		
			(-2.25)		
Treated × AF StdError				-0.022^{***}	
				(-2.69)	
Treated \times 1/Analysts					-0.005
					(-0.19)
AF Disagreement			0.025***		
			(3.79)		
AF StdError				0.022***	
				(3.10)	
1/Analysts					0.009
					(0.43)
Credit Rating FE	No	Yes	Yes	Yes	Yes
Firm Characteristics	No	Yes	Yes	Yes	Yes
FE across all specifications		Year	-Quarter-by-In	dustry	
Observations	7,468	7,468	6,221	6,086	6,221
R^2	0.001	0.033	0.035	0.035	0.032

Table 7: Downgrades, fire sales, and Regulation Fair Disclosure

This table examines the effect of mutual fund fire sales on downgrades before and after the adoption of Regulation Fair Disclosure (Reg FD). In Panel A (B) we regress a dummy variable that equals one if there is a downgrade in the fire sale quarter (fire sale and subsequent quarter) on a dummy variable that equals 1 for treated firms ('Treated') and an interaction between Treated and a Reg FD dummy. The Reg FD dummy equals one in the 9 quarters following the enactment of Reg FD in October 2000. Specifications (3)–(5) include all the characteristics used in the propensity score model in Table 2. The sample is restricted to treated and control firms for 9 quarters before and 9 quarters after October 2000, following Jorion et al. (2005). We report t-statistics in parentheses that are based on standard errors clustered by event quarter, and */**/*** denote significance at the 10/5/1% confidence level.

	(1)	(2)	(3)	(4)	(5)
Treated	-0.013	-0.015	0.001	0.001	0.002
	(-1.41)	(-1.69)	(0.23)	(0.20)	(0.35)
Treated \times RegFD=1			-0.047 * *	-0.047 **	-0.048 * *
			(-2.50)	(-2.50)	(-2.58)
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Firm Characteristics	No	Yes	Yes	Yes	Yes
Industry FE	No	No	No	Yes	Yes
Credit Rating FE	No	No	No	No	Yes
Observations	1,717	1,717	1,717	1,717	1,717
R^2	0.021	0.058	0.061	0.063	0.071

Panel A: Event quarter

Panel B: Event and subsequent quarter

	(1)	(2)	(3)	(4)	(5)
Treated	-0.024*	-0.028**	-0.008	-0.009	-0.007
	(-1.96)	(-2.44)	(-0.80)	(-0.82)	(-0.67)
Treated \times RegFD=1			-0.055^{***}	-0.056^{***}	-0.058***
			(-3.08)	(-3.08)	(-3.29)
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Firm Characteristics	No	Yes	Yes	Yes	Yes
Industry FE	No	No	No	Yes	Yes
Credit Rating FE	No	No	No	No	Yes
Observations	1,717	1,717	1,717	1,717	1,717
R^2	0.025	0.092	0.094	0.097	0.115

Table 8: Fire-sale effects by return group

This table examines the effect of mutual fund fire sales on credit ratings across different groups of firms. Treated firms are sorted into four groups based on returns in the fire sale quarter (EQ) and on returns in excess of the market over the 6 months after EQ. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and return in EQ. We report 'Average Treatment effect on Treated' (ATT) for each group during EQ. In Panel A (B), ATT is the difference in downgrade probability (expected downgrade notches) between treated and control firms. Panels A2 and B2 restrict the sample to firms with negative EQ returns.

		Excess Return 6 months after EQ				
		Low	High	All		
Return in the	Low	-0.0085	-0.0138	-0.0107		
Event Quarter	High	-0.0058	-0.0097	-0.0074		
	All	-0.0073	-0.0118	-0.0092		

Panel A: ATT for *Pr*{*Downgrade*}

		Excess Return 6 months after EQ				
		Low	High	All		
Return in the Event Quarter	Low	-0.0141	-0.0328	-0.0205		
	High	-0.0116	-0.0264	-0.0184		
	All	-0.0130	-0.0296	-0.0195		

Panel B: ATT for *E*[#*NotchesDown*]

		Excess Return 6 months after EQ				
		Low	High	All		
Return in the	Low	-0.0157	-0.0321	-0.0221		
Event Quarter	High	-0.0090	-0.0114	-0.0102		
	All	-0.0120	-0.0207	-0.0155		

Panel A2: ATT for $Pr\{Downgrade | EQret < 0\}$

Panel B2:	ATT for	E[#Not]	chesDown	EQret	< 0	ĺ
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		Excess Return 6 months after EQ				
		Low	High	All		
Return in the Event Quarter	Low	-0.0246	-0.0743	-0.0432		
	High	-0.0181	-0.0277	-0.0229		
	All	-0.0210	-0.0486	-0.0320		

Table 9: Placebo selling pressure and credit ratings

This table presents a placebo test for the main result of this paper. To identify placebo selling pressure, we reconstruct the treatment variable, MFFlow, using all funds that experience outflows instead of just those whose outflows are greater than 5% as we do in our main tests. We exclude any fire sale stock-quarters from the placebo sample (see Section 5.2 for details). We then replicate the analysis in Table 4 for this placebo treatment.

	N(Treated) = 4775						
	Treated	Control	ATT	SE	t-stat		
	(1)	(2)	(3)	(4)	(5)		
EQ-2 and EQ-1	0.053	0.050	0.0025	0.0037	0.69		
EQ-1	0.028	0.024	0.0044	0.0028	1.59		
Event Quarter	0.031	0.030	0.0010	0.0030	0.35		
EQ+1	0.033	0.034	-0.0004	0.0031	-0.14		
EQ and EQ+1	0.059	0.060	-0.0002	0.0040	-0.05		

Panel A: *Pr*{*Downgrade*}

	N(Treated) = 4775							
	Treated	Control	ATT	SE	t-stat			
	(1)	(2)	(3)	(4)	(5)			
EQ-2 and EQ-1	0.126	0.120	0.0059	0.0099	0.59			
EQ-1	0.067	0.058	0.0088	0.0069	1.28			
Event Quarter	0.074	0.074	0.0006	0.0080	0.08			
EQ+1	0.087	0.085	0.0021	0.0143	0.15			
EQ and EQ+1	0.156	0.155	0.0010	0.0168	0.06			

Panel B: E[#NotchesDown]

	N(Treated) = 4777						
	Treated	Control	ATT	SE	t-stat		
	(1)	(2)	(3)	(4)	(5)		
EQ-2 and EQ-1	0.058	0.059	-0.0010	0.0058	-0.18		
EQ-1	0.028	0.028	-0.0004	0.0040	-0.10		
Event Quarter	0.031	0.030	0.0008	0.0036	0.23		
EQ+1	0.030	0.032	-0.0027	0.0041	-0.66		
EQ and EQ+1	0.059	0.062	-0.0027	0.0054	-0.50		

Panel C: *E*[#*NotchesUp*]

Table 10: Robustness tests

This table examines the robustness of the main results reported in Table 4 to different matching criteria and sample composition. Each row reports the difference in the realized probability of downgrades between treated and control firms during the 6 months starting at the beginning of the event quarter.

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	x	Str. Per	Della	Š	JOL (TES)	Ţ				
	110	\$U	8101	Cor	40,	Treated	Control	ATT	SE	t-stat
						(1)	(2)	(3)	(4)	(5)
Baseline (Table 4 panel A)	5	0.025	0.025	1	4255	0.043	0.057	-0.0146	0.0038	-3.82
+ wider return caliper	5	0.050	0.025	1	4957	0.046	0.063	-0.0173	0.0038	-4.55
+ wider propensity caliper	5	0.050	0.050	1	5406	0.048	0.066	-0.0179	0.0035	-5.12
+ multiple controls	5	0.050	0.050	2	5406	0.048	0.064	-0.0164	0.0035	-4.67
Subsamples										
Exclude fin. crisis (30'07-10'09)	5	0.025	0.025	1	3982	0.036	0.053	-0.0173	0.0037	-4.63
Exclude "Money"&"Util" (FF12)	5	0.025	0.025	1	2685	0.047	0.066	-0.0186	0.0048	-3.87
Exclude "Manufacturing" (FF5)	5	0.025	0.025	1	2842	0.043	0.054	-0.0109	0.0045	-2.45
Different criteria within the baselin	e ma -	tching so	cheme							
Exact rating only match	5	0.025	0.025	1	2999	0.038	0.054	-0.0157	0.0041	-3.81
+ wider return caliper	5	0.050	0.025	1	3925	0.039	0.057	-0.0173	0.0041	-4.22
+ wider propensity caliper	5	0.050	0.050	1	4616	0.044	0.056	-0.0119	0.0038	-3.17
+ multiple controls	3	0.050	0.050	2	4616	0.044	0.057	-0.0130	0.0038	-3.46
Finer industry match	12	0.025	0.025	1	3344	0.042	0.056	-0.0141	0.0039	-3.60
+ wider return caliper	12	0.050	0.025	1	2905	0.039	0.048	-0.0096	0.0044	-2.20
+ wider propensity caliper	12	0.050	0.050	1	4842	0.044	0.060	-0.0161	0.0037	-4.31
+ multiple Controls	12	0.050	0.050	2	4842	0.044	0.059	-0.0150	0.0037	-4.01
Match without regards to industry	0	0.025	0.025	1	5325	0.048	0.066	-0.0175	0.0034	-5.09
+ tighter return caliper	0	0.010	0.025	1	4550	0.044	0.058	-0.0141	0.0034	-4.10
+ tighter propensity caliper	0	0.010	0.010	1	3729	0.042	0.051	-0.0088	0.0037	-2.38
+ multiple controls	0	0.010	0.010	2	3729	0.042	0.054	-0.0118	0.0037	-3.17
		0		1	.1					
Dijjereni maiching scheme – basea	on E	Q return 5	0 025	2 ana 1	233A	0 038	0.053	-0.0150	0.0056	2.65
\pm fine return grid	5	10	0.025	1	3038	0.038	0.055	-0.0130	0.0050	-2.05
+ fine return & industry grids	12	10	0.025	5	1949	0.040	0.039	-0.0130	0.0055	-2.55
	-	-	0.025		1)1)	0.051	0.010	0.0137	0.0005	2.11
Coarse Rating	5	5	0.025	1	4163	0.047	0.058	-0.0115	0.0052	-2.20
+ fine return grid	5	10	0.025	1	2779	0.038	0.052	-0.0140	0.0059	-2.38
+ line return & industry grids	12	10	0.025	3	3293	0.045	0.059	-0.0156	0.0055	-2.87
No recovery during EQ										
Baseline matching scheme	5	0.025	0.025	1	922	0.072	0.101	-0.0293	0.0070	-4.19
+ wider return caliper	5	0.050	0.025	1	1598	0.068	0.097	-0.0294	0.0070	-4.21
+ wider propensity caliper	5	0.050	0.050	1	2042	0.069	0.097	-0.0289	0.0064	-4.48
+ multiple controls	5	0.050	0.050	2	2042	0.069	0.088	-0.0196	0.0064	-3.04

Figure 1: Mutual fund fire sales and abnormal stock returns

This figure plots cumulative average abnormal returns (CAARs) in the three quarters before and after mutual fund fire sales (as defined in Section 1.3) for the full sample of firms between 1990 and 2015 and the subsample with credit ratings. Panel A reports CAARs relative to the CRSP equal-weighted index. Panel B reports CAARs relative to characteristic-matched portfolios.



Figure 2: Propensity scores

This figure plots the propensity score estimates for being a fire-sale stock (as defined in Section 1.3) from the conditional logit model in Table 2 for the fire-sale firm-quarters ('treated') in Panel A and all others in Panel B. We set year-quarter fixed effects to zero for comparability of scores across time. See Sections 1.3 and 2.2 for details.



Panel A: Treated

Panel B: Not Treated





This figure plots cumulative changes in the default probability estimate using the model of Campbell et al. (2008) (Panel A) and in Credit Default Swap spread (Panel B) for the fire-sale stocks (as defined in section 1.3) and matched controls during the 2002-2015 period. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 1.2 for details. The shaded area is the Event Quarter (EQ), and the area between vertical dotted lines indicates the six month period starting at the beginning of EQ.



Panel A: Cumulative changes in CDS spreads

Figure 4: Fire-sale effects by rating level

This figure compares credit rating downgrade probability for fire-sale stocks (as defined in Section 1.3) to all other stocks (Panel A), and to matched controls (Panel B) by rating category. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 1.2 for details.







Figure 5: Fire-sale effects over time

This figure compares realized rating downgrade probabilities for fire-sale stocks (as defined in section 1.3) to matched controls over different time periods and regulatory regimes. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and return in EQ. Panel A reports results for the Event Quarter (EQ), whereas Panel B reports results for 6 months starting at the beginning of EQ.



Panel A: Event Quarter (EQ) ATT





Appendix A. Data used to compute fire sales

This section describes the data used to calculate mutual fund fire sales. The CRSP Survivorship Bias Free Mutual Fund database provides data at the mutual fund share class level. We use the MFLINKS file provided by Wharton Research Data Services (WRDS) to aggregate data to the fund level. For any observations not matched to MFLINKS, we use the CRSP portfolio number to aggregate the different share classes. We then merge the CRSP mutual fund database with the Thompson Financial CDA/Spectrum holdings database. We use the holdings data from CDA/Spectrum to compute the number of shares and value of equity holdings of mutual funds as of the quarter end.

Our mutual fund sample includes only equity mutual funds. Following Coval and Stafford (2007), we exclude funds with fewer than 20 holdings in the past as well as those that report the following Investment Objective Codes: international, municipal bonds, bond and preferred, or metals. We also exclude sector funds that specialize in specific industries by removing funds with Lipper classification codes AU, H, FS, NR, RE, TK, UT, CG, CMD, CS, ID, BM, or TL, or Strategic Insight codes GLD, HLT, FIN, NTR, RLE, TEC, UTI, or SEC, or Wiesenberger objective codes GPM, HLT, FIN, ENR, TCH, or UTL.

Lastly, we apply the screening criteria employed by Coval and Stafford (2007). First, to control for data discrepancies between the CDA/Spectrum equity holdings and the CRSP database, we restrict the difference between the TNA reported in the CRSP database and in the CDA/Spectrum database— $1/1.3 < (TNA_{CDA}/TNA_{CRSP}) < 1.3$). Second, we restrict changes in TNA— $-0.5 < \Delta TNA_{j,t}/\Delta TNA_{j,t-1} < 2.0$.

Variable	Description
Rating	Standard & Poor's long term issuer credit rating or Moody's senior unsecured issuer rating (in Appendix). 21 notches from AAA/Aaa to C, and 1 default category. 'Coarse rating' ignores subcategories (i.e., +/- and 1,2,3), while 'narrow rating' includes subcategories. Changes over the 3- and 6-month horizons are measured relative to the level at the beginning of the period, independently for upgrades (exclude AAA/Aaa) and downgrades (exclude already defaulted). Sources: Compustat, Moody's Corporate Default Risk Service Database.
$Pr\{Downgrade\}$	Realized probability of downgrade computed as a ratio of downgrade events divided by the number of firms in a given period. Multiple downgrades for a firm within the period are counted as one.
E[#NotchesDown]	The number of notches downgraded divided by number of firms, where notch is a change in a narrow rating category.
E[#NotchesUp]	The number of notches upgraded divided by number of firms, where notch is a change in a narrow rating category.
Industry	Fama-French five (Consumer, HighTech, Healthcare, Manufacturing, Other) or twelve (BusEq, Chems, Durbl, Enrgy, Hlth, Manuf, Money, NoDur, Shops, Telcm, Utils, Other) industry classifications based on the company's historical SIC4 code. Sources: Ken French's website, CRSP.
MFFlow	Mutual fund fire sales defined as the imputed dollar amount sold in a stock by all mutual funds experiencing an outflow $\geq 5\%$ of their assets, normalized by the stock's quarterly trading volume. See Appendix A for details. Sources: CRSP, Thompson Reuters.
Treated (1/0)	All firm-quarters were <i>MFFlow</i> is below the 20th percentile value of the full sample (the global cutoff) and the 10th percentile for that quarter (the local cutoff).
Event Quarter	The quarter for which the treated firm's MFFlow is below the global and local cutoffs.
Control Firm	Defined for each treated firm. Must have similar characteristics as the treated firm as of the start of the event quarter and the closest return to the treated firm during the event quarter. In particular, (i) the control must be in the same industry as the treated firm, (ii) have a similar propensity to be treated, (iii) the same credit rating at the beginning of the quarter, and (iv) closest stock return during the event quarter. In the main tests, we pick one control within a 2.5% propensity score caliper and also require that the distance in returns is within 2.5%. If a satisfactory match cannot be established within a narrow rating category, we then look for a control candidate within coarse rating category.
Mutual Fund Ownership	The fraction of a firm's shares outstanding owned by mutual funds. Source: Thomson Reuters.
CHS Default Prob	Probability of default for month $t+12$ obtained using the model parameter estimates from the 12-month ahead model in Table 4 of Campbell, Hilscher, and Szilagyi (2008).
Return (Raw)	Stock return for the respective period, including dividends. Source: CRSP.

Appendix B. Variables Definitions

Variable	Description
Return (Mkt)	Stock return, including dividends, minus the total return on CRSP value-weighted index for the same period. Source: CRSP.
Return (DGTW)	Stock return, including dividends, minus the return on the characteristics-matched portfo- lio following the methodology of Daniel, Grinblatt, Titman, and Wermers (1997). Sources: CRSP.
CAARs	Cumulative Average Abnormal Return, either relative to CRSP value-weighted index (Mkt) or the characteristics-matched portfolio (DGTW). Cumulative over time, average across firms. Sources: CRSP, Russ Wermers' website.
Realized Variance	Sum of squared stock returns over the quarter. Source: CRSP.
МСар	Market value of common equity. End of quarter value. Source: CRSP.
Debt-to-EV	Book value of long- and short-term debt outstanding divided by the sum thereof and the market value of common equity. End of quarter value. Source: CRSP, Compustat.
Book leverage	Book value of long- and short-term debt outstanding divided by the sum thereof and book value of common equity. End of quarter value. Source: CRSP, Compustat.
Book-to-Market	Book value of common equity divided by the market value of common equity. End of quarter value. Source: CRSP, Compustat.
CAPM β	Rolling estimate from monthly stock returns regressed on the value-weighted CRSP returns. At least (most) 12 (60) months required. End of quarter value. Source: CRSP.
Amihud Ratio	Quarterly average of daily absolute returns to dollar volume traded, winsorized at 0.0001 and 0.3 as in Acharya and Pedersen (2005). Source: CRSP.
CDS spread changes	The CDS sample is restricted to contracts with 5 years to maturity on names traded in the United States in US Dollars. Monthly CDS spreads are the average of CDS spreads over the last five days of the month. For each firm we choose the contract that is likely to be the most liquid. In particular, we give first preference to contracts whose spreads are based on at least three quotes within the currency group (Composite Fallback level of 'CccyGrp'). If none are available, we prefer contracts with document clause XR or XR14 after November 2010 (the CDS 'Big Bang') and MR before that date. If neither are available, we use contracts with document clause CR or CR14. We compute changes in average monthly spreads within a particular contract type. Quarterly changes are the sum of monthly changes over the quarter. Source: Markit
CDS Implied Downgrades	Based on ratings implied by five-year CDS contracts on a firm as computed by Markit.
Realized default probabilities	Binary variable based on bankruptcy filing data as reported by Capital IQ.

Internet Appendix

Do rating agencies deserve some credit? Evidence from transitory shocks to credit risk

May, 2018

This appendix provides supplementary results for the analysis conducted in the main text of the paper.

- Moody's ratings: We redo our key analyses using ratings from Moody's instead of S&P. Tables IA.1– IA.2 report covariate balance and treatment effects for treated firms and controls for Moody's ratings sample.
- Alternative probability models for placebo fire sales and the CDS subsample: Table IA.3 reports estimates of a probability model that predicts placebo fire sales for the full and CDS subsamples. For comparison, the table also reports estimates for similar models for true fire sales.
- Covariate balance for treated and control firms for different subsamples used in the main paper:
 - Firms with negative returns in EQ: Table IA.4
 - Firms with traded CDSs: Table IA.5
 - Placebo-treated firms and controls: Table IA.6
- No recovery subsample: We restrict the sample to firms that do not exhibit a meaningful recovery in returns during the fire-sale quarter. In particular, we only include firms whose returns for the event quarter are within one-fifth of their minimum return for the quarter. Tables IA.7 reports covariate balance and IA.8 reports treatment effects for this subsample.
- Ratings and the extent of the price shocks due to fire sales: We analyze whether having a credit rating moderates the effect of fire sales on stock returns during the event quarter. Table IA.9 reports a probability model for having a credit rating. We then match rated firms to unrated firms with a similar probability of having a rating. Table IA.10 then examines the effects of Reg FD on the depth of the price shock in the event quarter for fire-sale firms that are rated relative to those that are not, for the full sample as well as restricting the sample to the 9 quarters before and after the implementation of Reg FD as in Jorion, Liu, and Shi (2005). The Table shows that rated firms after Reg FD have a smaller dip in prices due to fire sales over the full sample. In the subsample for the 19 quarters around Reg FD, magnitudes are similar but the interaction coefficient between Treated and Reg FD is insignificant.

• Table IA.11 reports results of calendar time portfolios of treated and control firms that represent returns for periods between EQ – 1 year to EQ + 1 year. Overall, calendar time results are broadly similar to event time results reported in Table 3 in the main text. Both results show abnormal negative returns during the event quarter and approximately zero returns before the event quarter for both treated and control firms. Control firm returns show no meaningful recovery after the event quarter, while treated firms recover. However, treated firms seem to recover by a greater amount after the event quarter relative to the drop in prices in the event quarter. This 'excess' recovery is possibly due to sampling variation in just a few years. In particular, in Panel B, we report alphas that control for the period 2009-2013, when market recovered from the 2007–2009 financial crisis. It is likely that fire sales by mutual funds were small during this period of high market returns. Panel B shows that the excess recovery is much smaller after controlling for this period. As Figure 5 of the main paper shows, our results for CRA actions are similar if we exclude this period.

Table IA.1: Covariate balance for treated firms and controls: Moody's sample

This table presents means and standard deviations of selected variables for fire-sale ('treated') stocks and controls. Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 1.2 in main text for details. The fewer number of control relative to treatment firm-quarters indicates that some controls are matched to multiple treated firms. Panel A reports variables used in the propensity score model while Panel B reports other variables of interest. The sample period is 1990-2008.

	N(Treated)=1203, N(Control)=1153						
		Means			St.Deviations		
	Treated	Control	P-value	Treated	Control		
	(1)	(2)	(3)	(4)	(5)		
MCap(USD bln)	2.041	2.088	0.881	5.331	4.592		
Debt-to-EV	0.408	0.389	0.333	0.213	0.231		
Mutual Fund Ownership	0.125	0.128	0.725	0.090	0.098		
Rating Change past 3 months	-0.035	-0.032	0.905	0.487	0.577		
Rating Change past 12 months	-0.115	-0.077	0.324	0.850	0.895		
Return past 3 months	0.022	0.022	0.982	0.174	0.189		
Return past 12 months	0.115	0.113	0.930	0.360	0.416		
Volatility past 3 month	0.070	0.069	0.564	0.044	0.040		
Amihud Ratio	0.049	0.036	0.030	0.067	0.058		

Panel A: Propensity-score contributors

Panel B: O	ther variable	s of interest
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		Means		St.Deviations		
		Treated	Control	P-value	Treated	Control
		(1)	(2)	(3)	(4)	(5)
	САРМ β	0.908	0.964	0.334	0.581	0.630
	Book-to-Market	1.115	0.921	0.036	1.680	1.388
pre EQ	Book leverage	0.461	0.462	0.913	0.290	0.293
	CHS Default Prob.	0.073	0.069	0.408	0.093	0.083
	Raw Return	-0.009	-0.009	0.978	0.164	0.164
durin a EO	Excess Return (Mkt)	-0.038	-0.038	0.992	0.150	0.149
	Excess Return (DGTW)	-0.032	-0.033	0.848	0.145	0.144
	CHS Default Prob.	0.084	0.083	0.940	0.126	0.130
	Cumulative Return	0.126	0.053	0.002	0.377	0.343
6 month after EQ	Cumulative Return (vs Mkt)	0.065	-0.003	0.011	0.349	0.320
	Cumulative Return (vs DGTW)	0.042	-0.018	0.016	0.333	0.292
	CHS Default Prob.	0.066	0.077	0.246	0.092	0.128

Table IA.2: Fire sales and credit ratings: Moody's sample

This table examines the effect of mutual fund fire sales on credit ratings. Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 1.2 in main text for details. Column (3) presents 'Average Treatment effect on Treated' (ATT), or the difference in the outcome variable between treated and control firms; a negative number indicates lower mean outcomes for treated firms relative to controls. In Panel A, the outcome variable equals 1 if the credit rating was downgraded during the period and zero otherwise. In Panel B, we take into account the severity of downgrades by reporting the average number of notches downgraded (E[#NotchesDown]). Panel C reports the average number of notches upgraded. The time periods we consider are: EQ-2 and EQ-1 (the 6 months before the start of EQ), EQ-1, EQ, EQ+1, and EQ and EQ+1 (the 6 months starting at the beginning of EQ). Standard errors and t-statistics reported in columns (4) and (5) respectively are robust for heteroskedasticity. The sample period is 1990-2008.

	N(Treated) = 1203						
	Treated	Control	ATT	SE	t-stat		
	(1)	(2)	(3)	(4)	(5)		
EQ-2 and EQ-1	0.041	0.042	-0.0017	0.0074	-0.23		
EQ-1	0.019	0.022	-0.0033	0.0054	-0.61		
Event Quarter	0.027	0.037	-0.0091	0.0059	-1.54		
EQ+1	0.030	0.043	-0.0133	0.0068	-1.97		
EQ and EQ+1	0.054	0.074	-0.0200	0.0086	-2.31		

Panel A: *Pr*{*Downgrade*}

	N(Treated) = 1203						
	Treated	Control	ATT	SE	t-stat		
	(1)	(2)	(3)	(4)	(5)		
EQ-2 and EQ-1	0.098	0.103	-0.0050	0.0192	-0.26		
EQ-1	0.045	0.056	-0.0108	0.0135	-0.80		
Event Quarter	0.067	0.094	-0.0274	0.0154	-1.78		
EQ+1	0.076	0.126	-0.0499	0.0214	-2.33		
EQ and EQ+1	0.138	0.214	-0.0765	0.0263	-2.91		

Panel B:	E[#NotchesDot	own
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Panel C:	E[#NotchesUp]
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	N(Treated) = 1202						
	Treated	Control	ATT	SE	t-stat		
	(1)	(2)	(3)	(4)	(5)		
EQ-2 and EQ-1	0.044	0.065	-0.0208	0.0084	-2.48		
EQ-1	0.021	0.037	-0.0166	0.0056	-2.96		
Event Quarter	0.029	0.022	0.0075	0.0066	1.13		
EQ+1	0.032	0.034	-0.0017	0.0077	-0.22		
EQ and EQ+1	0.060	0.056	0.0042	0.0100	0.41		

Table IA.3: Probability models for fire sales: Placebo treatment and CDS subsamples

We estimate models for the probability of a stock to experience a true or placebo fire sale as a function of one-quarter lagged firm characteristics, past rating changes, stock returns, and year-quarter fixed effects. The outcome is one if the firm-quarter meets the criteria to be a true [placebo] fire sale as defined in main text Section 1.3 [Section 5.2], and zero otherwise. Specification (1) [(3)] report conditional logit model estimates for true [placebo] fire sale, as in the main text; specifications (2) [(4)] augment model (1) [(3)] with recent changes in the CDS spreads, which limits the samples to firm-quarters with CDS activity in the quarter before fire sale. We report t-statistics in parentheses that are based on standard errors clustered by firm, and */**/*** denote significance at the 10/5/1% confidence level.

	True fi	re sale	Placebo	fire sale
	(1)	(2)	(3)	(4)
Rating Change (3 month)	-0.0746***	-0.0457**	-0.0388	0.0098
	(-2.97)	(-2.08)	(-0.92)	(0.31)
Rating Change (12 month)	0.1020***	0.0479**	0.0719*	0.0028
	(4.57)	(2.55)	(1.91)	(0.11)
Return (3 month)	0.0138**	0.0026	-0.0281**	-0.0152
	(2.08)	(0.39)	(-2.24)	(-1.36)
Return (12 month)	-0.0040	0.0011	-0.0962^{***}	-0.0315^{***}
	(-1.32)	(0.40)	(-14.86)	(-5.50)
log(Market Cap)	-0.0233***	-0.0040***	0.0247***	0.0188***
	(-23.64)	(-4.99)	(15.00)	(13.57)
log(1+ Debt-to-EV)	-0.0684^{***}	-0.0281^{***}	-0.0057	-0.0237 **
	(-10.16)	(-4.90)	(-0.44)	(-2.31)
Amihud Ratio	0.8803***	0.3855***	-0.0462	0.6709***
	(30.59)	(7.73)	(-0.50)	(4.51)
MF Ownership	0.3860***	0.1371***	0.2960***	0.1916***
	(31.88)	(12.70)	(11.85)	(9.21)
log(Realized Variance)	-0.0403***	-0.0117***	-0.0001	0.0041*
	(-30.67)	(-9.86)	(-0.02)	(1.88)
Imp. Rating Upgrade from CDS (3 month)		0.0009		-0.0020
		(0.40)		(-0.58)
Imp. Rating Upgrade from CDS (12 month)		-0.0001		-0.0012
		(-0.10)		(-0.60)
Imp. Rating Downgrade from CDS (3 month)		-0.0044		-0.0008
		(-1.48)		(-0.25)
Imp. Rating Downgrade from CDS (12 month)		-0.0010		0.0038*
		(-0.72)		(1.92)
log(CDS Variance)		-0.0000		0.0006
		(-0.17)		(1.04)
Observations	112,610	23,563	104,595	22,785
R^2 / Pseudo R^2	0.0788	0.0517	0.0152	0.0269

Table IA.4: Covariate balance for treated firms and controls: negative return subsample

This table presents means and standard deviations of selected variables for fire-sale ('treated') stocks and controls. Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 1.2 in main text for details. We limit the sample to treated firms with negative EQ returns. Panel A reports variables used in the propensity score model while Panel B reports other variables of interest.

	N(Treated)=2128, N(Control)=1994						
		Means	St.Deviations				
	Treated	Control	P-value	Treated	Control		
	(1)	(2)	(3)	(4)	(5)		
MCap(USD bln)	3.141	3.451	0.206	7.137	7.263		
Debt-to-EV	0.347	0.342	0.571	0.221	0.222		
Mutual Fund Ownership	0.185	0.182	0.640	0.119	0.118		
Rating Change past 3 months	-0.015	-0.020	0.633	0.496	0.503		
Rating Change past 12 months	-0.077	-0.023	0.048	0.885	0.893		
Return past 3 months	0.027	0.029	0.719	0.176	0.199		
Return past 12 months	0.117	0.110	0.612	0.386	0.419		
Realized Volty past 3 month	0.067	0.067	0.996	0.042	0.043		
Amihud Ratio	0.027	0.020	0.001	0.050	0.045		

Panel A: Propensity-score contributors

Panel B: Other variables of	interest
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		Means			St.Deviations	
		Treated	Control	P-value	Treated	Control
		(1)	(2)	(3)	(4)	(5)
	САРМ β	0.987	1.041	0.171	0.635	0.696
mma EO	Book-to-Market	0.764	0.742	0.371	0.780	0.889
pre EQ	Book leverage	0.415	0.425	0.426	0.283	0.274
	CHS Default Prob.	0.061	0.063	0.408	0.073	0.087
	Raw Return	-0.121	-0.121	0.817	0.120	0.120
EO	Excess Return (Mkt)	-0.114	-0.113	0.873	0.122	0.120
EQ	Excess Return (DGTW)	-0.094	-0.094	0.993	0.125	0.120
	CHS Default Prob.	0.087	0.088	0.837	0.138	0.139
	Bankruptcy in 1 year	0.004	0.004	0.748	0.066	0.061
	Bankruptcy in 5 years	0.027	0.040	0.080	0.161	0.196
	Cumulative Return	0.088	0.064	0.011	0.338	0.332
6 month	Cumulative Return (vs Mkt)	0.038	0.013	0.010	0.304	0.296
after EQ	Cumulative Return (vs DGTW)	0.020	-0.007	0.003	0.266	0.262
	CHS Default Prob.	0.077	0.080	0.450	0.125	0.128

Table IA.5: Covariate balance for treated firms and controls: CDS subsample

This table presents means and standard deviations of selected variables for fire-sale ('treated') stocks and controls. Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of EQ), and return in EQ. Both treated and control have to have traded CDS contracts in EQ - 1 and EQ. See Section 1.2 for details. Panel A reports variables used in the propensity score model while Panel B reports other variables of interest.

	N(Treated)=587, N(Control)=559					
	Means			St.Deviations		
	Treated	Control	P-value	Treated	Control	
	(1)	(2)	(3)	(4)	(5)	
MCap(USD bln)	12.302	15.070	0.129	16.179	23.351	
Debt-to-EV	0.238	0.259	0.260	0.180	0.190	
Mutual Fund Ownership	0.234	0.211	0.002	0.083	0.087	
Rating Change past 3 months	-0.010	0.007	0.608	0.409	0.330	
Rating Change past 12 months	-0.090	-0.060	0.553	0.803	0.724	
Return past 3 months	0.042	0.030	0.256	0.130	0.153	
Return past 12 months	0.179	0.150	0.163	0.295	0.348	
Realized Volty past 3 month	0.051	0.052	0.648	0.028	0.031	
Amihud Ratio	0.003	0.001	0.353	0.015	0.011	
CDS spread level	0.011	0.011	0.541	0.013	0.013	
CDS spread volatility	0.012	0.009	0.330	0.018	0.015	

Panel A: Propensity-score contributors

Panel B: Other variables of interest

		Means			St.Deviations	
		Treated	Control	P-value	Treated	Control
		(1)	(2)	(3)	(4)	(5)
	САРМ β	0.859	0.929	0.244	0.490	0.633
mma EO	Book-to-Market	0.544	0.505	0.308	0.456	0.404
pre EQ	Book leverage	0.363	0.411	0.339	0.282	0.344
	CHS Default Prob.	0.036	0.040	0.067	0.032	0.054
	Raw Return	0.022	0.022	0.948	0.124	0.124
FO	Excess Return (Mkt)	-0.008	-0.009	0.945	0.101	0.101
EQ	Excess Return (DGTW)	-0.007	-0.010	0.601	0.106	0.102
	CHS Default Prob.	0.039	0.043	0.121	0.059	0.062
	Cumulative Return	0.065	0.051	0.349	0.226	0.234
6 month	Cumulative Return (vs Mkt)	0.030	0.015	0.251	0.188	0.184
after EQ	Cumulative Return (vs DGTW)	0.020	0.008	0.372	0.173	0.177
	CHS Default Prob.	0.040	0.043	0.420	0.061	0.048

Table IA.6: Covariate balance for treated firms and controls: Placebo

This table presents means and standard deviations of selected variables for placebo fire-sale ('treated') stocks and controls. Each placbo treated firm-quarter with is matched to a control by credit rating, propensity score (all as of the start of EQ), and return in EQ. See Sections 1.2 and 5.2 in the main text for details. Panel A reports variables used in the propensity score model while Panel B reports other variables of interest.

	N(Treated)=4775, N(Control)=4464							
	Means			St.Deviations				
	Treated	Control	P-value	Treated	Control			
	(1)	(2)	(3)	(4)	(5)			
MCap(USD bln)	10.190	9.780	0.213	20.746	20.591			
Debt-to-EV	0.315	0.312	0.578	0.215	0.219			
Mutual Fund Ownership	0.170	0.173	0.303	0.103	0.105			
Rating Change past 3 months	-0.031	-0.019	0.246	0.494	0.488			
Rating Change past 12 months	-0.112	-0.090	0.196	0.913	0.916			
Return past 3 months	0.014	0.019	0.101	0.177	0.184			
Return past 12 months	0.053	0.065	0.039	0.317	0.330			
Realized Volty past 3 month	0.066	0.066	0.836	0.039	0.042			
Amihud Ratio	0.006	0.006	0.582	0.019	0.019			

Panel A: Propensity-score contributors

Panel B: Other variables of interest

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			Maans		St Dov	intions
		Treated	Control	P-value	Treated	Control
		(1)	(2)	(3)	(4)	(5)
	САРМ β	1.051	1.037	0.200	0.649	0.659
	Book-to-Market	0.649	0.660	0.468	0.659	0.699
pre EQ	Book leverage	0.400	0.384	0.018	0.282	0.273
	CHS Default Prob.	0.054	0.052	0.228	0.062	0.059
	Raw Return	0.005	0.004	0.908	0.159	0.159
	Excess Return (Mkt)	-0.025	-0.025	0.939	0.138	0.138
	Excess Return (DGTW)	-0.020	-0.020	0.812	0.131	0.129
	CHS Default Prob.	0.058	0.059	0.722	0.083	0.088
	Bankruptcy in 1 year	0.002	0.001	0.537	0.046	0.038
	Bankruptcy in 5 years	0.019	0.024	0.166	0.138	0.153
	Cumulative Return	0.075	0.079	0.487	0.273	0.283
6 month	Cumulative Return (vs Mkt)	0.021	0.025	0.629	0.251	0.255
after EQ	Cumulative Return (vs DGTW)	0.004	0.010	0.376	0.221	0.228
	CHS Default Prob.	0.059	0.059	0.946	0.094	0.095

Table IA.7: Covariate balance for treated firms and controls: no recovery subsample

This table presents means and standard deviations of selected variables for fire-sale ('treated') stocks and controls. Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 1.2 in main text for details. We restrict the sample to treated firms whose returns do not meaningfully recover during EQ. Specifically, we require that cumulative EQ returns are within 1/5 of the minimum return in EQ. Panel A reports variables used in the propensity score model while Panel B reports other variables of interest.

	N(Treated)=1539, N(Control)=1475					
	Means			St.Deviations		
	Treated	Control	P-value	Treated	Control	
	(1)	(2)	(3)	(4)	(5)	
MCap(USD bln)	3.808	3.887	0.814	10.762	10.629	
Debt-to-EV	0.353	0.348	0.597	0.229	0.229	
Mutual Fund Ownership	0.177	0.174	0.646	0.120	0.117	
Rating Change past 3 months	-0.023	-0.010	0.416	0.568	0.568	
Rating Change past 12 months	-0.081	-0.016	0.010	0.956	0.914	
Return past 3 months	0.024	0.020	0.366	0.176	0.192	
Return past 12 months	0.119	0.099	0.074	0.379	0.409	
Realized Volty past 3 month	0.068	0.070	0.173	0.040	0.043	
Amihud Ratio	0.029	0.022	0.017	0.052	0.047	

Panel A: Propensity-score contributors

Panel B: Other va	riables of	interest
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		Means			St.Deviations		
		Treated Control P-value		P-value	Treated	Control	
		(1)	(2)	(3)	(4)	(5)	
	САРМ β	0.976	1.056	0.099	0.621	0.700	
mma EO	Book-to-Market	0.765	0.763	0.933	0.880	0.937	
pre EQ	Book leverage	0.424	0.429	0.691	0.282	0.277	
	CHS Default Prob.	0.062	0.066	0.167	0.077	0.095	
	Raw Return	-0.067	-0.066	0.856	0.179	0.176	
БО	Excess Return (Mkt)	-0.091	-0.091	0.871	0.156	0.154	
EQ	Excess Return (DGTW)	-0.077	-0.080	0.570	0.143	0.144	
	CHS Default Prob.	0.083	0.087	0.375	0.126	0.133	
	Cumulative Return	0.102	0.072	0.005	0.343	0.341	
6 month after EQ	Cumulative Return (vs Mkt)	0.051	0.021	0.008	0.310	0.306	
	Cumulative Return (vs DGTW)	0.032	-0.001	0.008	0.268	0.271	
	CHS Default Prob.	0.076	0.081	0.191	0.124	0.130	

Table IA.8: Treatment effects in the no recovery subsample

This table examines the effect of mutual fund fire sales on credit ratings. Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 1.2 in main text for details. We restrict the sample to treated firms whose returns do not meaningfully recover during EQ. Specifically, we require that cumulative EQ returns are within 1/5 of the minimum return in EQ. Column (3) presents 'Average Treatment effect on Treated' (ATT), or the difference in the outcome variable between treated and control firms; a negative number indicates lower mean outcomes for treated firms relative to controls. In Panel A, the outcome variable equals 1 if the credit rating was downgraded during the period and zero otherwise. In Panel B, we take into account the severity of downgrades by reporting the average number of notches downgraded (E[#NotchesDown]). Panel C reports the average number of notches upgraded. The time periods we consider are: EQ-2 and EQ-1 (the 6 months before the start of EQ), EQ-1, EQ, EQ+1, and EQ and EQ+1 (the 6 months starting at the beginning of EQ). Standard errors and t-statistics reported in columns (4) and (5) respectively are robust for heteroskedasticity.

	N(Treated) = 1539							
	Treated	Control	ATT	SE	t-stat			
	(1)	(2)	(3)	(4)	(5)			
EQ-2 and EQ-1	0.052	0.043	0.0091	0.0076	1.19			
EQ-1	0.026	0.026	0.0000	0.0054	0.00			
Event Quarter	0.032	0.049	-0.0169	0.0062	-2.73			
EQ+1	0.038	0.055	-0.0169	0.0069	-2.44			
EQ and EQ+1	0.067	0.097	-0.0299	0.0088	-3.39			

Panel A: *Pr*{*Downgrade*}

Panel B:	E[#Notc]	hesDown
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	N(Treated) = 1539							
	Treated	Treated Control ATT SE						
	(1)	(2)	(3)	(4)	(5)			
EQ-2 and EQ-1	0.125	0.104	0.0208	0.0193	1.08			
EQ-1	0.064	0.060	0.0045	0.0132	0.35			
Event Quarter	0.072	0.112	-0.0396	0.0150	-2.65			
EQ+1	0.105	0.147	-0.0422	0.0326	-1.29			
EQ and EQ+1	0.173	0.251	-0.0786	0.0372	-2.11			

Panel C: *E*[#*NotchesUp*]

	N(Treated) = 1538							
	Treated	Control	ATT	SE	t-stat			
	(1)	(2)	(3)	(4)	(5)			
EQ-2 and EQ-1	0.070	0.086	-0.0156	0.0107	-1.46			
EQ-1	0.042	0.044	-0.0013	0.0079	-0.17			
Event Quarter	0.034	0.029	0.0046	0.0066	0.69			
EQ+1	0.031	0.014	0.0169	0.0072	2.34			
EQ and EQ+1	0.064	0.044	0.0208	0.0093	2.24			

Table IA.9: A probability model for being rated

For the sample of fire-sale stocks, we estimate proability models for a stock to have a credit rating (from either S&P or Moody's) as a function of one-quarter lagged firm characteristics, stock returns, and year-quarter fixed effects. The outcome is one if the firm is rated that quarter. We report t-statistics in parentheses that are based on standard errors clustered by firm, and */**/*** denote significance at 10/5/1% confidence level.

	(1)	(2)	(3)	(4)	(5)
log(Market Cap)	0.1206***	0.1168***	0.1034***	0.1093***	0.1309***
	(90.01)	(81.04)	(38.98)	(44.41)	(79.06)
log_age		0.1197***	0.1224***	0.1241***	0.1017***
		(61.14)	(59.21)	(59.34)	(53.12)
Amihud Ratio			-0.3905***	-0.4030***	-0.2601***
			(-7.79)	(-7.87)	(-6.22)
log(Realized Variance)				0.0177***	0.0162***
				(7.56)	(10.80)
Return (3 month)					-0.0211***
					(-3.86)
Return (12 month)					-0.0391^{***}
lag(1) Daht to EV)					(-12.45)
$\log(1 + Debt-to-Ev)$					(80.21)
ME Ownership					(09.21)
Mr Ownersnip					(18.20)
VO by Industry FF	Vas	Vac	Vac	Vac	(10.29) Vec
Observations	510 184	366 917	366 753	366 241	302.963
R^2	0 3527	0.4182	0 4207	0.4221	0 4951
<u>N</u>	0.3321	0.4102	0.4207	0.7221	0.7931

Table IA.10: Fire sale returns and Regulation Fair Disclosure

This table examines returns to fire sale before and after the adoption of Regulation Fair Disclosure. The sample includes only stocks that experience fire-sales. We match each stock with a credit rating to one without a credit rating using propensity scores from specification 5 in Table IA.9. We regress fire-sale quarter returns on a dummy variable 'Rated' that equals to one if the firm has a credit rating from either S&P or Moody's in the quarter and (in even specifications only) an interaction between 'Rated' and a 'Reg FD', which is a dummy that equals one after the enactment of Regulation Fair Disclosure in October 2000. All specifications include time-by-industry fixed effects; specifications (3), (4), (7), and (8) include fire-sale pressure as well as firm characteristics that enter the propensity model used for matching; specifications (5) through (8) restrict matched samples to treated and control firms for 9 quarters before and 9 quarters after October 2000, following Jorion et al. (2005). We report t-statistics in parentheses that are based on standard errors clustered by event quarter, and */**/*** denote significance at the 10/5/1% confidence level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rated	0.015***	0.006	-0.000	-0.008	0.009	0.005	-0.015*	-0.019**
	(3.98)	(1.15)	(-0.11)	(-1.47)	(1.24)	(0.52)	(-1.97)	(-2.20)
Rated \times RegFD		0.016**		0.014*		0.014		0.016
		(2.21)		(1.93)		(1.17)		(1.30)
Fire sale pressure, t & t-1	No	No	Yes	Yes	No	No	Yes	Yes
Firm Characteristics	No	No	Yes	Yes	No	No	Yes	Yes
FE across all specifications				Year-Quarte	er-by-Industry			
Observations	12,008	12,008	12,008	12,008	3,209	3,209	3,209	3,209
R^2	0.002	0.002	0.035	0.035	0.000	0.001	0.042	0.042

Table IA.11: Calendar time analysis

We form equal-weighted portfolios of treated and control stocks for the following periods: -2 is a portfolio that represents quarters EQ - 4 & EQ - 3, -1 is quarters EQ - 2 & EQ - 1, 0 is EQ, 1 is EQ + 1& EQ + 2, and 2 is EQ + 3& EQ + 4. For example, all stocks in the -1 portfolio in month *m* will have a fire-sale some time in the period month m + 1 - m + 7. These monthly portfolio returns are regressed on the Fama-French-Carhart 6 factor model and the reported alphas are scaled to reflect returns over the entire period (for all 6-month periods, alphas are multiplied by 6, and by 3 for the event quarter). Cum. α reports cumulative α s starting from the beginning of period -2. Panel B reports alphas from similar regressions that also include a dummy variable that is one in the period 2009-2013.

Panel A: Baseline model							
	Treated					trols	
Period	α(%)	t-stat	Cumulative α	α(%)	t-stat	Cumulative α	
-2	-0.8	-0.95	-0.8	0.1	0.13	0.1	
-1	0.8	1.07	0.0	0.0	-0.03	0.1	
0	-2.4	-3.44	-2.4	-2.1	-3.75	-2.0	
1	3.3	3.11	0.8	-0.2	-0.19	-2.2	
2	3.2	2.93	4.1	0.7	0.75	-1.5	

Panel B: Baseline model with 2009–2013 Dummy									
	Treated					Controls			
Period	α(%)	t-stat	Cumulative α	α(%)	t-stat	Cumulative α			
-2	-1.3	-1.41	-1.3	-0.3	-0.51	-0.3			
-1	0.4	0.45	-0.9	-0.9	-1.2	-1.2			
0	-2.5	-3.28	-3.4	-2.2	-3.57	-3.3			
1	2.8	2.49	-0.8	-0.7	-0.9	-4.0			
2	2.0	1.92	1.2	-0.1	-0.16	-4.1			