Can Credit Rating Agencies See Through Transitory Shocks to Credit Risk?☆

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Abstract

Credit Rating Agencies (CRAs) assert that ratings are useful because, by responding only to permanent changes in credit risk, they are more stable than market-based measures. Stability is valuable since rating changes have real consequences for private contracts and access to capital. We test whether CRAs can actually distinguish between permanent and transitory shocks to credit risk in real-time. We use mutual fund equity fire-sales as a transitory shock to credit risk, because declines in stock prices increase leverage and signal lower cash flows. We find that CRAs are significantly less likely to downgrade fire-sale firms relative to matched controls that experience permanent shocks. In contrast, Credit Default Swap spreads increase similarly for both treated and control firms. These results suggest that replacing ratings with market-based estimates of credit risk may result in stronger real effects from transitory price shocks.

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

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Abstract

Credit Rating Agencies (CRAs) assert that ratings are useful because, by responding only to permanent changes in credit risk, they are more stable than market-based measures. Stability is valuable since rating changes have real consequences for private contracts and access to capital. We test whether CRAs can actually distinguish between permanent and transitory shocks to credit risk in real-time. We use mutual fund equity fire-sales as a transitory shock to credit risk, because declines in stock prices increase leverage and signal lower cash flows. We find that CRAs are significantly less likely to downgrade fire-sale firms relative to matched controls that experience permanent shocks. In contrast, Credit Default Swap spreads increase similarly for both treated and control firms. These results suggest that replacing ratings with market-based estimates of credit risk may result in stronger real effects from transitory price shocks.

Credit ratings agencies (CRAs) have traditionally played an important role as information intermediaries in financial markets. However, CRAs are now under siege. A vast academic literature finds that the issuer-pays model and competitive pressures create distortions in the incentives of CRAs to issue accurate ratings.[1] Regulators and other observers have pointed to inflated ratings as one of the key causes 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 of 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 (CDS) dominate CRAs in the accuracy or timeliness of their forecasts (Chava, Ganduri and Ornthanalai, 2016). In fact, Flannery, Houston and Partnoy (2010) argue that market-based estimates of credit risk should replace credit ratings for use by both regulators and private investors.

Nonetheless, CRAs continue to thrive. The Economist reports that revenues at the two largest CRAs, Moody's and Standard & Poor's, surpassed pre-crisis levels in 2014 with gross margins of 40%-50%.[3] Thus, despite their flaws and diminished regulatory relevance, CRAs appear to pass the market test. But, how do CRAs add value in a world where accurate market-based estimates of credit risk are easily available?

CRAs themselves argue that they remain relevant because the volatility of market-based estimates of credit risk has real effects. For example, Cantor and Mann (Moodys; 2006) say *"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 rating agencies distinguish between permanent and transitory shocks to credit risk? A simple way to differentiate between such shocks

[1] See for example, Griffin, Nickerson and Tang (2013) and Becker and Milbourn (2011), and other references in footnote 9.

[2] See 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.

[3] 'Credit where credits due', Economist April 19, 2014.

is just to wait long enough and allow the passage of time to reveal whether the shock is temporary or permanent. However this strategy has no economic benefits relative to a moving average of market-based estimates. For CRAs to add value beyond a moving average, they must be able to distinguish between such shocks soon after the shock occurs. We therefore test whether CRAs can indeed discern transitory changes in credit quality from permanent ones in real-time.

A shock to credit risk can be transitory because it is a false signal, or because it is a fundamental shock that reverses quickly. Of course, it is very difficult to distinguish between these two possibilities in practice: for example, a rumor of a takeover that later proves to be unfounded has elements of both explanations. In our context, distinguishing between these possibilities is not crucial because even temporary changes to ratings are costly. Hence, we follow academic research and CRAs in using the term transitory for both possibilities.[4]

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

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 are a signal of bad news about the firm's fundamentals (Fama, 1981; Kothari and Sloan, 1992). In particular, CRAs state that this is one of the mechanisms through which stock prices may affect credit ratings.[5]

We use mutual fund fire sales as in Edmans, Goldstein and Jiang (2012) to identify firms with transitory

[4]See Cornaggia and Cornaggia (2013) and Moodys (2002) for the false signal reason and Löffler (2013) and Cantor and Mann (Moodys; 2006) for the temporary change reason.

[5]For example, Adelson (Standard & Poor's; 2008) states that *"...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."*. Other mechanisms that Adelson (Standard & Poor's; 2008) mentions are that equity price drops may affect the ability of firms to raise new capital and also impact the ability of 'confidence-sensitive' firms such as banks and brokers to operate.

shocks to equity value. Edmans, Goldstein and Jiang (2012) show that mutual fund fire sales 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 treated firm returns reverse ex-post, while control firm returns do not. Treated and control firms also exhibit covariate balance 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) model. Downgrade probabilities and returns for treated and control firms also exhibit parallel trends before the fire sale quarter.

Our key finding is that CRAs are able to distinguish between transitory and permanent shocks to equity prices. In the event quarter, the difference between downgrade probabilities of treated and control firms (0.9%) is statistically significant and about half of the downgrade probability of treated and control firms in the pre-event quarter (2%). Over the six-month period starting with the event quarter, the difference in downgrade probabilities increases to 1.5%. Results are similar if we take the severity of downgrades into account. The difference (0.05) in the average number of downgrade notches during the event and subsequent quarter is about half that (0.08) of treated and control firms in the 6 months before the event.

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, time or industry variation in fundamental shocks and downgrade propensities also apply to both treated and control firms.

We find even stronger results when we focus on the sample of firms where the fire sales are likely to be most salient. In a sub-sample with negative event quarter returns (-12% on average), the average differnce in the number of downgrade notches between treated and control firms nearly doubles (from 0.049 in the full sample) to 0.084. Even among stocks with negative returns, we expect that results should be strongest in firms where we have the highest confidence that the shocks are temporary. In particular, stocks that exhibit the most pronounced 'V'-shape pattern in returns, with a deep drop in the event quarter and a strong subsequent recovery, should have a larger difference in expected downgrade notches between treated and control firms, relative to the shallow drop and weak recovery group. We find this is indeed the case with the difference in downgrade notches in the event quarter tripling as we go from shallowest to steepest 'V-shape'.

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 suggests no special role for CRAs. We therefore 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 CDS markets lead bond markets. We find that both treated and control firm spreads increase by the same amount as do CDS-implied rating downgrades. Both treated and control firm spreads also increase over the next quarter, consistent with Hilscher, Pollet and Wilson (2015) who find that information flows from equity to CDS markets. Treated firm spreads eventually revert while 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 time-series, so it is possible that these tests do not have power. However, we find that even in the sample where CDS data exists, treated firms have significantly lower rating downgrade probability than controls (by nearly 4% during the 6 months starting with event quarter), but not smaller increases in credit spreads (20 basis points wider spreads for treated firms versus 16 basis points for controls over the same period).

Why do CRAs appear to see through transitory shocks to equity prices, while CDS spreads do not? Two non-mutually exclusive explanations are differences in information, and differences in objectives. CRAs historically have had privileged access to information from firm managers. For example, CRAs were specifically exempt from Regulation Fair Disclosure (Reg FD) when it was originally enacted. Thus, it is possible that after seeing a price shock, CRAs can determine whether there is a substantial change in the firm's fundamentals by requesting information from management if necessary, and only downgrade firms where there is a change in fundamentals (Jorion, Liu and Shi, 2005). The second possible explanation is that CDS markets and CRAs have different objectives. As discussed above, CRAs aim to respond to only permanent changes in credit risk, while CDS spreads are 'point-in-time' in that they reflect conditional expectations of risk neutral default probabilities. Because market leverage increases with a downward shock equity prices, it can be argued that the firm is closer to default and hence CDS spread should rise.[6]

One potential concern with using fire sales to identify exogenous transitory shocks is that investors may withdraw capital from mutual funds that hold stocks that they believe will perform poorly in the future. If this is true, 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 use hypothetical trades of the distressed funds by assuming that mutual funds sell their holdings in proportion to their beginning-of-quarter portfolio weights before 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, 2015). To minimize observable differences between treated and control firms, we also match on the firm's propensity to experience fire sales. We also find no evidence that fire-sale firms have relatively worse past performance: past returns and downgrades do not predict fire-sales.

Our results survive a battery of robustness tests including changes to the matching procedure such as using a finer or coarser caliper for the propensity score match, using several instead one nearest neighbor, and using a finer or coarser industry classification; as well as excluding specific industries (e.g., financials and utilities) and periods. 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 credit ratings. Finally, we examine a set of placebo tests, where the treatment variable is based not on fire sales, but on all mutual fund sales, excluding fire sales. 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 that mutual funds choose to hold or sell.

Finally, we examine whether the ability of CRAs to distinguish temporary shocks from permanent ones has changed over time. This test is informative for two reasons. First, we would like to test whether our results are pervasive or specific to a sub-sample. Second, the Dodd-Frank act removes the exemption that

[6]However, it is not entirely clear that CDS spreads on 5-year contracts should increase if market participants realize that the equity price shocks are temporary and will reverse on average over the next few months. An increase in CDS spreads may occur if capital constrained traders buy underpriced equity and hedge their exposure by buying CDS protection.

CRAs have from Reg FD, thereby perhaps limiting their access to private information (Dimitrov, Palia and Tang, 2015; Ali, Kyung and Li, 2016). However, CRAs argue that removing the specific exemption from Reg FD was irrelevant, because they meet other criteria for exemption.[7] We find that CRAs' ability to see through transitory shocks is strong throughout the sample except for two sub-periods, the 2007-2009 financial crisis, and 2013-2015. The later period provides suggestive, but statistically insignificant, evidence that the ability of CRAs to distinguish between transitory and permanent shocks has declined after the enactment of the Dodd-Frank Act.

Our paper contributes to two literatures: the role and impact of CRAs, and the real effects of financial prices. 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, and Ellul, Jotikasthira and Lundblad (2011) show that downgrades result in fire sales in corporate bonds.[8] 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.[9] It is important to note that our results do not imply that CRAs are a free of conflicts of interest, or that ratings are more accurate than market-based estimates. Instead, we argue that because accuracy is only a part of the CRA's objective function, lower accuracy need not imply that CRAs are redundant. The other objective of CRAs—ratings stability to mitigate the adverse real effects of downgrades—is also important. Thus, our paper is related to research that examines the trade-off between ratings stability and accuracy (Altman and Rijken (2004), Altman and Rijken (2006), Cornaggia and Cornaggia (2013) and 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 moving averages of market-based estimates. We thus provide an answer to why CRAs continue to thrive despite flaws documented by prior research and the availability of substitutes. Our paper also complements Cornaggia, Cornaggia and Israelsen (2017), who find that

[7]In particular, they do not seek to make investment decisions based on the private information and their engagement letters with firms contain confidentiality agreements (Carbone, 2010).

[8]Also see Kisgen (2009), Tang (2009), Sufi (2007), and Manso (2013).

[9]One 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).

municipal bond ratings matter for prices even without a change in fundamentals. Our results provide an explanation for why investors consider ratings informative.

Our paper is also related to the literature on the real effects of financial markets (see Bond, Edmans and Goldstein (2012) for a survey). This research shows that managers and other decision-makers learn from stock prices and use this information to guide their decisions. As in our setup, a growing body of empirical research employs mutual fund fire sales as transitory equity price shocks to examine the effect of stock prices on corporate policies and shows that economic agents take decisions based on these non-fundamental shocks.[10] Our result suggest that the real effects of financial prices may have influenced the evolution of the financial eco-system. In particular, CRA rating policies may have evolved to mitigate the adverse real effects of financial markets.[11] It is possible that CRAs and markets together provide agents with superior information relative to either of them by themselves. For example, in the equilibrium framework of Bond, Goldstein and Prescott (2009), information independent of market prices allows agents to better interpret prices.

Finally, our results suggest that recent regulatory efforts in the Dodd-Frank Act to limit the privileged position of CRAs, albeit with the laudable goal of encouraging investors to do independent analysis, may also 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 the datasets we use, the methodology, and construction of variables.

1.1. Ratings and other data

Our primary data is based on the intersection of four datasets: mutual fund holdings from 13F filings, mutual fund returns and total net assets from the Center for Research in Security Prices (CRSP)

[10] 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).

[11] Other 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.

Survivorship-Bias Free mutual fund database, credit ratings and firm accounting data from Compustat, and equity returns and prices from CRSP. 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 Appendix.[12] 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 A, 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.[13] 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 that we use are standard and are defined in Appendix B.

1.2. Methodology

Our goal is to test whether credit rating agencies can distinguish between transitory, information-free shocks to equity prices and permanent shocks that are driven by information. 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. Note that the returns of matches are not due to fire sales (and hence are presumably permanent). We test whether realized downgrade probabilities are different for treated firms relative to controls over the fire sale and subsequent quarter. 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.

The fire sales 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 gen-

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

[13]Results 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.

erating 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 stocks. We confirm that in our sample, the shocks to treated firms are temporary, while the shocks to controls are not. Treated firms experience negative returns that revert back over the next few quarters, while controls exhibit similar returns that do not reverse. We therefore follow the literature in referring to the fire sale shocks as 'transitory', or 'non-fundamental'.

The next step is to identify a set of firm that serve as controls. Controls have similar characteristics to treated firms at the start of EQ and similar returns during EQ. In particular, controls have the same Fama-French five industry classification, a similar propensity score to be a fire sale, the same credit rating at the beginning of EQ and closest stock return during EQ. We pick one control firm that has the minimal absolute distance in stock return from the treated firm in EQ, while being within a 2.5% propensity score caliper, which is approximately one-third of its standard deviation. We also require that the distance in EQ return between a treated and a matched control firm is within 2.5% which corresponds to approximately one-fifth of EQ return standard deviation amongst the treated firms. 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 choices we make on the matching are subject to the usual trade-off. Tighter criteria mean that we lose more firms that cannot find matches. Looser criteria result in poorer matches. Section 4.5 shows that results are similar if we relax or tighten the criteria, or use a different matching procedure.

The matching procedure allow us to control for common shocks across treated and control firms. The control sample provides an estimate for the downgrade rate that we expect for firms with similar characteristics and a similar EQ return as treated firms. The key difference between the two samples is that the treated firm return is transitory on average. While we cannot rule out that possibility that some treated firms experience permanent shocks or that some controls experience transitory shocks, our setup implies 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 (conditional on covariates) 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: mutual fund investor decisions to buy or sell shares in funds are unlikely to be due to information about changes in credit ratings of specific stocks within the fund. Investors which 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. Second, as in Edmans, Goldstein and Jiang (2012), we exclude sector funds to eliminate events where there may be 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 be a fire sale stock in a given quarter and match on the estimated propensity scores in the beginning of the event quarter. A fire sale firm could be different from a typical firm for several reasons (Berger, 2015). First, because they are owned by certain mutual funds, they may have more 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 possibility 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 twelve months as well as rating changes over the past three and twelve months as additional predictor variables in the propensity score model. While we cannot rule out the possibility that treated firms are different from controls along some unobserved or mismeasured dimension related to past performance, this seems unlikely because, as we see below, past returns do not predict selection into the fire sales sample. Moreover, if fire sale stocks are of worse quality that controls, this will bias us towards finding they are more likely to be downgraded than the controls.

1.3. Measuring fire sales

We follow the approach in Edmans, Goldstein and Jiang (2012) closely to construct $MFFlow$, the implied price pressure calculated by assuming that funds subject to large outflows (>5% of their assets) adjust their existing holdings proportionally across the board based upon the 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:

$$Outflow_{j,q} = -(TNA_{j,q} - TNA_{j,q-1}(1 + r_{j,q})), \tag{1}$$

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:

$$MFFlow_{i,q} = \sum_{j=1}^{m} \frac{Outflow_{j,q} * s_{i,j,q-1}}{\mathrm{Volume_{i,q}}}, \tag{2}$$

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

$$s_{i,j,q} = \frac{Shares_{i,j,q} * Price_{i,q}}{\mathrm{TNA_{j,q}}}, \tag{3}$$

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 the Appendix A.

Coval and Stafford (2007) and Edmans, Goldstein and Jiang (2012) define a fire sale as a firm-quarter where *MFFlow* falls below 10th percentile value of the full sample. However, imposing a single threshold in the entire sample period affects the balance of the treated firm sample across time. In unreported tests, we find a large concentration of fire sale firm-quarters during the Internet boom in 1999 when using the full-sample 10% cutoff. To address the concern that applying a single cutoff yields a temporally concentrated sample, which may reflect specifics of a particular 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 et al. (1997) (DGTW, Panel B). Both panels show that CAARs for the full sample of stocks declines significantly by 4-5% during the event quarter. We do not observe any significant decline in stock return prior to the event. 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 sub-sample 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 firms that have credit ratings are generally larger and more liquid than those without ratings 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. Distinguishing between these alternatives is difficult and not crucial for our research question.

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 stocks and matches.

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 some of the other 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. *The 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 adds in ratings changes over the past three months (i.e. quarter q-1) and the past twelve months (q-4 through q-1). The effects of past rating changes are in the opposite direction from that predicted by the hypothesis that fire sales firms are of worse quality than a typical stock. An upgrade over the past twelve 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 twelve-month coefficient is close to zero). Specification 3 shows that past three-month and past twelve-month returns do not predict the likelihood of downgrades. These results are not consistent with the hypothesis that some mutual funds experience fire sales because the stocks they hold have worse performance prior to the event quarter.

Column four 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.[14] This figure suggests that there is reasonable overlap between treated firms and controls. Only treated firms with extremely high fire sale propensities seem to have few potential matches.

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 devi-

[14] This 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.

ations 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 before the event quarter but a somewhat stronger tendency for treated firms to be downgraded over the twelve months before the event quarter than controls. Although this difference is not statistically significant at conventional levels, we confirm that there are no pre-EQ trends in rating actions in our subsequent analysis.

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, book-to-market, and CHS default probability are similar across treated and control samples. Most important, risk-adjusted event quarter returns are also similar for treated and control firms at -2%. We also see that treated firm returns are transitory: they completely revert over the next 9 months. However, control firm returns do not revert at all over the next 9 months. Panel B also reveals that these equity price shocks translate to changes in default probability. We measure default probability using the model estimated in Campbell, Hilscher and Szilagyi (2008). To interpret magnitudes of default probabilities, note that this model outputs a monthly probability of default for month t+12. Because treated and control firms are well-matched at the start of EQ and have similar returns in EQ, changes in CHS default probability are similar for treated and control firms during EQ. Average default probabilities for treated (control) firms increase from 0.053% (0.055%) to 0.060% (0.062%) over the event quarter. While these magnitudes may appear modest, note that these are monthly probabilities and these increases correspond to a move from the 60th to 75th percentile for a typical year. Moreover, the increases are statistically significant at 5% confidence level. Treated firm default probabilities begin to reverse 9 months after EQ, but those of control firms remain elevated, resulting in significant differences between treated and control firm default probabilities 9 months after EQ. Thus, perhaps not surprisingly, the CHS model does not distinguish between permanent and transitory shocks to credit risk in real-time. The differences between these shocks is only visible in the default probabilities 9 months after the shock, when transitory equity returns have fully reversed.

3. Key Results

This section presents our key results on whether CRAs and CDS markets can see through 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. Realized downgrade probability is the fraction of firms in the relevant sample (treated or control) that experience a downgrade over a given period. Over the six month period EQ-2 and EQ-1, treatment and control firms exhibit parallel trends with similar six-month downgrade probabilities of 3.9% for treated firms as compared with 3.8% for controls. The difference is statistically and economically insignificant. During the event quarter, treated firms have much lower downgrade probability of 2.2%, when compared to the downgrade probability of 3.1% for control firms. The difference of 92 basis points is highly significant statistically (heteroskedasticity robust t-statistic of -2.87).[15] The effect is present one quarter after EQ as well, with the difference in downgrade probabilities between treated (2.4%) and control firms (3.3%) of 89 basis points. Overall, for the six month period starting with EQ, the difference between treated and control firms is -1.53% (t-statistic of -3.43), 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 (2.2%), this does not necessarily show that CRAs are fooled by 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. The downgrade probability for treated (or control) firms in the quarter prior to the event quarter meets this criteria.

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

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

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 (0.114 versus 0.163). This difference of 0.0486 downgrade notches is large relative to the average downgrade notches of 0.083 (0.085) over the six months before *EQ* for treated (control) firms.

One possibility is that control firms are just more volatile than treated 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 small economically (0.016 versus 0.049 for expected downgrade notches during *EQ* and *EQ*+1 as per Panel B) and suggest that CRAs are actually relatively more positive about control firm creditworthiness pre-*EQ*. Also, because we match on the end of *EQ*-1 credit rating, this pre-*EQ* difference implies that a few control firms were 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 (e.g., see Cantor and Mann, 2006).

As reported in Table 3, both treated and control firms have negative average excess returns in the event quarter. However, on average these returns are relatively small in absolute magnitude (about -2%). 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 (untabulated).

Panels A2 and B2 show significant differences between treated and control firms in downgrade probability (7.3% versus 9.2%, Panel A2) and expected number of notches downgraded (0.195 versus 0.28, Panel B2) over the six month period (*EQ* and *EQ*+1) in this subsample. For *EQ* alone, the difference in downgrade probabilities is 133 bps. Thus, the treatment effect increases by a factor of 1.45 for the subsample with negative *EQ* returns relative to the full sample.[16] Panel C2 shows that there are virtually no differences in the

[16]In this negative return subsample, the downgrade rate is higher for the treated firms too. This could be because of CRAs are

expected upgrade notches during or before the EQ in this subsample. However, over EQ and EQ+1, treated firms have slightly 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 firm receiving a large, permanent negative shock on average in this sub-sample.

3.2. Can CDS markets see through transitory shocks?

A potential alternative explanation for our results is that CRAs may learn which shocks are transitory from other markets, rather than through any independent analysis on their part. Table 5 examines this hypothesis, by focusing on the subsample of firms with traded Credit Default Swaps (CDS). This limits the sample to the period from 2002 onwards, and to significantly larger firms (on average, \$12 billion in market value as opposed to \$3.7 billion for the main sample as per Table 1).[17]

Panel A repeats the analysis from Panel A of our main Table 4 on CRA downgrades for treated and control firms for the sub-sample of firms with CDS contracts. Panel B reports CDS spread changes for the same sub-sample. Finally, Panel C examines changes in credit ratings implied by the level of CDS spreads (rather than actual ratings by CRAs) as estimated by Markit, the CDS data provider. Since the implied rating changes will reflect only substantial changes in the perceived creditworthiness, they could be viewed as more comparable to the CRA's actions.

Panel A shows that the difference in CRA downgrade probability is 2% during the EQ and 3.9% during the 6 months starting with EQ and that both are significant at the 1% confidence level. These effects are larger than those in the main sample (featuring 4,260 treated firm-quarters instead of 592 here). Meanwhile, Panel B shows that CDS spreads increase by about 20 basis points for both treated and control firms over EQ and EQ+1. There are no significant differences between treated and control firms 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. Panel C shows that results are similar if implied ratings from CDS markets are used instead of

less 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.

[17] We use the same matching criteria as in the main analysis (section 2.3) but re-estimate the fire sale probability model from 2 on the CDS subsample and augment it with past changes in the CDS implied rating. 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 C, 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 sub-sample to get a better match with parallel trends pre-event.

CDS spreads.

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 in EQ. This lag is also visible in Figure 3, which presents stock returns (Panel A) along with cumulative CDS spread changes (Panel B) for the CDS sample over the three quarters around EQ. 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.[18] 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 end of $EQ+3$, thereby confirming our research design. Most importantly, these results show that unlike CRAs, CDS markets do not distinguish between transitory and permanent shocks in real-time.

The question of whether CDS spreads *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 CDS market participants are aware that the shock was transitory, would spreads on five year CDS contracts increase? In any case, either due to differing objectives between CDS markets and CRAs, or because CRAs have information that CDS markets do not, there is a difference between the reaction of CRAs and CDS spreads to transitory price shocks in equity markets. If CRAs did not exist, and CDS spreads were used in contracts as measures of credit risk, these results suggest that transitory price shocks in equity markets would be reflected in CDS spreads and potentially propagate to the real economy.

4. Additional tests

In this section, we dig deeper to understand the cross-section and time series of the response of CRAs to fire sale stocks. In the cross-section we test whether the treatment effects are strongest where we a priori expect them to be if fire sale shocks are transient. Then we test whether the treatment effect is different across rating categories and over time. Finally, we examine a placebo test and subject our matching procedure to robustness tests.

[18]Chava, 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").

4.1. The 'V'-shaped pattern in returns and rating agency 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 $MFFlow$-variable has 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.

To examine if this is indeed the case, 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.[19] We choose the six month horizon because on average, treated 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 difference), 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 6 reports differences in downgrade probability. Firms with the most pronounced 'V'-shape—in the low event quarter return and high post-EQ return group—have a lower likelihood of downgrades relative to controls by -1.44% while the difference in downgrade probability for the least 'V'-shaped group is -0.57%. 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 a similar patterns: the difference in the most 'V'-shaped group is 2.5-4 times larger than in the least 'V'-shaped group; the difference in downgrade probability becomes more negative as the event quarter return falls if there is strong recovery.

[19]We 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.

4.2. The effect across rating categories

Figure 4 shows downgrade probabilities for treated and control firms by broad rating category. Note that the 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 then increasing thereafter.[20] BBB firms are just above the investment grade threshold.

Panel B shows that the treatment effect—the difference between downgrade probabilities of treated and matched control firms—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 also do not see any significant difference in the treatment effect around the investment grade threshold (BBB to BB). As one moves from A to BBB ratings, both treated and control firms have lower downgrade probabilities. Moving from BBB to BB results in an increase in downgrade probabilities for both treated and control firms. The treatment effect is present for both BBB and BB firms, but is not statistically different across the investment grade threshold.

A priori, it is not clear what to expect for difference in downgrade probabilities between treated and control firms at the investment grade threshold. One possible argument is that because the real effects of a downgrade are highest at this threshold, CRAs should be the most careful and therefore better distinguish between transitory and permanent shocks in situations when investment grade status is at stake. However, this argument (i) implies that CRAs apply insufficient effort to distill information from stock returns at other rating levels, and (ii) ignores greater incentives for all BBB-rated firms to take corrective actions (such as asset sales and raising additional equity) to retain their investment grade status.

[20] We do not plot AAA and categories below B because they have few observations.

4.3. The treatment effect over time

This section examines time-variation in the treatment effect. There are two motivations for examining this time-series. First, we want to be sure that the results are not driven by a few years, which might imply that they are due to specific events (e.g., the financial crisis). A second motivation is that sub-samples based on time may allow us to examine whether the ability of CRAs to see through transitory shocks is due to their access to non-public information from firm managers.

In particular Regulation Fair Disclosure ('Reg FD') changed the access of CRAs to non-public information relative to the market when it was enacted in October 2000. CRAs were specifically exempt from the provisions of Reg FD, which applied to other market participants (e.g. equity analysts). Jorion, Liu and Shi (2005) find that the enactment of Reg FD affected the information content of downgrades for financial markets. Stock price reactions to downgrades are significantly greater in the period after Reg FD relative to just before.

The financial crisis and its aftermath have also affected the legal environment that CRAs operate in. In October 2010 the SEC removed the explicit exemption that CRAs have from Reg FD. It is not clear whether this change has had any material effect on the access of CRAs to non-public information. CRAs argue that they are not covered by Reg FD in any case because they meet other criteria for exemption: they do not seek to make investment decisions based on the private information and because their engagement letters with firms contain confidentiality agreements (Carbone, 2010). Nevertheless, the repeal of the exemption may have created ambiguity among firms, who may decide to play it safe and restrict access of CRAs to non-public information.

Besides removing the exemption from Reg FD, the Dodd-Frank Act also made other changes to the legal environment for CRAs. These include increasing the liability for issuing inaccurate ratings, making it easier for the SEC to impose sanctions and to bring claims against CRAs. Dimitrov, Palia and Tang (2015) find that these diminished the information content of CRAs for equity and bond markets. Finally, the public criticisms of the role of CRAs in financial crisis may have diminished the trust that markets have in credit ratings. This lack of trust may also diminish the incentives for firms to provide non-public information to CRAs after the crisis, if they believe that market participants will not pay attention to ratings.

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. We examine four sub-periods: the period before Regulation FD (from 1991 until the last quarter of 2000), the original Reg FD period before the crisis (first quarter of 2001 to the second quarter of 2007), the crisis (third quarter of 2007 to first quarter 2009), and the post Reg FD exemption removal period. In general, it appears that the treatment effect is highest in the original Reg FD period and lowest during the crisis. It is positive, but small in the post-crisis, post-Reg FD period. However, we cannot draw strong conclusions from this analysis as sub-sample means are typically not significantly different from each other, and it is difficult to rule out alternative explanations specific to particular points in time.

4.4. 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 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 that have total selling pressure in the top local and global quartile of mutual fund sales. We use quartiles as the cut-off to ensure that sample sizes are comparable over the placebo treated and actual fire sale treated samples. Control firms are identified exactly in the same manner as in our main results in Table 4.

Table 7 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 the variation related to the mutual fund ownership and trading in general (e.g. mutual fund holdings, stock liquidity etc.) does not 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.

4.5. *Robustness tests*

As discussed above, our matching procedure balances the need to maximize sample size with the need to have close matches. Table 8 examines the robustness of our results to changing our matching criteria. Columns (1) through (5) are identical to those in our main specification in Table 4. For brevity, we focus on differences in downgrade probabilities between treated and control firms over the 6 month interval starting with EQ.

The first line in Table 8 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 includes the previous specification and 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 about a 1000 firms.

The next specification examines the robustness of the main result to excluding the period of financial crises of 2007-2009, firms in finance-related industries and utilities, 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 with established CDS market, respectively).

Next, we change criteria within 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, but then also look for 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 about 1000 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 (Fama-French 12 industries) instead of the five industry classification used in the baseline. We also consider a match without regards to industry. Neither of these changes materially affect the significance and magnitude of the treatment effect.

Finally, 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.

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.[21]

5. Conclusion

This paper shows that CRAs distinguish between temporary, non-fundamental shocks in equity prices and permanent ones while CDS markets 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 the concerns regarding the accuracy of CRAs due to conflicts of interest and catering discussed in the introduction, it is not clear what value CRAs add as information intermediaries. We show one particular channel through which CRAs add value: they are able to 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

[21] Additional unreported tests show that inferences stay the same for similar robustness tests on the negative EQ-return and CDS subsamples.

regulations. Our results suggest that if measures of credit risk based on market prices are embedded into contracts or used for regulations, it might allow transitory shocks in financial markets to propagate to the real economy. For example, if a transitory shock to credit risk triggers a contractual provision across all a firm's suppliers that affects their ability to purchase raw materials, this might affect production and 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.

Of course 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. 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 the reliance of regulators on ratings and removes the exemption of CRAs from Reg FD, among other changes. A final implication of our results is that although 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), they 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 equity prices. This could amplify the real effects of transitory shocks to equity 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 obervations 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.

Panel A: The rated firm-quarter sample

	Treated			Not Treated		
	Firm-Qtrs	Downgrades	Ratio	Firm-Quarters	Downgrades	Ratio
1990	70	6	0.086	1,873	116	0.062
1991	110	2	0.018	2,362	109	0.046
1992	84	0	0.000	2,397	67	0.028
1993	87	1	0.011	2,919	92	0.032
1994	219	1	0.005	3,377	83	0.025
1995	185	4	0.022	3,558	89	0.025
1996	236	6	0.025	3,853	81	0.021
1997	276	4	0.014	4,063	95	0.023
1998	318	7	0.022	4,829	171	0.035
1999	390	6	0.015	5,296	220	0.042
2000	475	18	0.038	4,970	236	0.047
2001	174	8	0.046	4,647	266	0.057
2002	359	18	0.050	4,909	325	0.066
2003	302	4	0.013	4,757	216	0.045
2004	369	4	0.011	4,983	143	0.029
2005	313	12	0.038	5,018	187	0.037
2006	329	7	0.021	5,177	179	0.035
2007	274	4	0.015	5,178	204	0.039
2008	238	24	0.101	5,103	304	0.060
2009	221	21	0.095	4,694	320	0.068
2010	264	3	0.011	4,699	99	0.021
2011	186	4	0.022	4,899	132	0.027
2012	325	6	0.018	4,786	137	0.029
2013	241	1	0.004	4,840	90	0.019
2014	218	3	0.014	5,073	92	0.018
2015	101	2	0.020	2,607	90	0.035
Total	6,364	176	0.028	110,867	4,143	0.037

Panel B: Other variables

	Firm-Qtrs	mean	sd	skew	p1	p25	p50	p75	p99
Return (Raw)	143,376	0.029	0.211	0.275	-0.569	-0.079	0.028	0.133	0.758
CAPM β	137,108	1.094	0.710	1.178	-0.113	0.610	0.996	1.440	3.375
Return (DGTW)	105,518	-0.001	0.176	0.334	-0.507	-0.094	-0.006	0.084	0.609
log(Realized Variance)	143,657	-7.714	1.122	0.498	-9.876	-8.502	-7.809	-7.042	-4.628
log(Mkt CAP)	143,646	7.396	1.859	-0.260	2.573	6.263	7.458	8.599	11.576
Book-to-Market	128,119	0.786	1.221	7.054	0.013	0.283	0.549	0.910	5.779
Debt-to-EV	128,157	0.282	0.178	0.343	-0.000	0.146	0.282	0.404	0.662
Mutual Fund Ownership	139,960	0.141	0.110	1.137	0.001	0.051	0.119	0.212	0.436
Amihud Ratio	143,646	0.023	0.052	2.906	0.000	0.000	0.002	0.012	0.244
Rating Change past 12 months	142,298	-0.215	1.606	-6.969	-5.000	0.000	0.000	0.000	2.000
MFFlow	143,657	-0.007	0.097	-300.1	-0.073	-0.006	-0.002	-0.000	0.000
CHS Default Prob (%)	109,816	0.085	0.201	10.37	0.015	0.029	0.042	0.067	0.944
CDS spread change (bps)	24,048	0.107	3.494	29.65	-3.145	-0.130	-0.007	0.085	4.889

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 both local (top decile that quarter) and global (top quintile across all quarter) ctieria for mutual fund fire-sales 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 at firm level, */**/*** denote significance at 10/5/1% confidence level.

	OLS			Logit
	(1)	(2)	(3)	(4)
log(Market Cap)	−0.0111***	−0.0111***	−0.0111***	−0.0219***
	(−8.97)	(−8.94)	(−8.92)	(−22.93)
log(1+ Debt-to-EV)	−0.0421***	−0.0410***	−0.0415***	−0.0669***
	(−4.31)	(−4.15)	(−4.14)	(−10.18)
MF Ownership	0.2746***	0.2742***	0.2744***	0.3712***
	(12.67)	(12.59)	(12.59)	(31.37)
Amihud Ratio	1.0441***	1.0443***	1.0422***	0.8550***
	(14.60)	(14.59)	(14.50)	(30.38)
log(Realized Variance)	−0.0260***	−0.0257***	−0.0257***	−0.0389***
	(−15.53)	(−15.24)	(−15.23)	(−29.98)
Rating Change (3 month)		−0.0280**	−0.0293**	−0.0710***
		(−2.15)	(−2.24)	(−2.81)
Rating Change (12 month)		0.0383***	0.0401***	0.0988***
		(2.97)	(3.12)	(4.42)
Return (3 month)			0.0019	0.0095
			(0.53)	(1.38)
Return (12 month)			−0.0023	−0.0026
			(−1.17)	(−0.82)
Year-Quarter Effects	Yes	Yes	Yes	Yes
Observations	111,116	110,278	110,277	110,277
R^2 / Pseudo R^2	0.0371	0.0372	0.0372	0.0771

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. Each treated firm-quarter is matched to a control firm that has the closest Event Quarter (EQ) return, in the same Fama-French 5 industry, and narrow credit rating, and within a one-third of a standard deviation of the propensity score to experience fire sales as estimated in Table 2, specification 4. We drop any matches where the absolute difference in EQ returns between treated and control firms is greater than 2.5% and consider matches within a broader rating category (i.e. ignoring '+','-') if a satisfactory match cannot be found within a narrow rating category. 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. See Sections 2.2 and 2.3 for more detail.

Panel A: Propensity-score contributors

	N(Treated)=4260, N(Control)=4023				
	Means			Std Deviations	
	Treated	Control	P-value	Treated	Control
	(1)	(2)	(3)	(4)	(5)
MCap(USD bln)	3.745	3.903	0.451	8.244	7.437
Debt-to-EV	0.318	0.322	0.682	0.210	0.209
Mutual Fund Ownership	0.191	0.184	0.223	0.119	0.114
Rating Change past 3 months	-0.008	0.000	0.489	0.415	0.471
Rating Change past 12 months	-0.033	-0.003	0.129	0.777	0.915
Return past 3 months	0.033	0.035	0.541	0.155	0.165
Return past 12 months	0.150	0.146	0.759	0.338	0.358
Volatility past 3 month	0.060	0.059	0.336	0.034	0.036
Amihud Ratio	0.019	0.013	0.000	0.040	0.034

Panel B: Other variables of interest

		Means			Std Deviations	
		Treated	Control	P-value	Treated	Control
		(1)	(2)	(3)	(4)	(5)
pre EQ	CAPM β	0.937	0.968	0.393	0.604	0.663
	Book-to-Market	0.728	0.722	0.820	0.855	0.896
	Book leverage	0.386	0.403	0.274	0.279	0.273
	CHS Default Probability	0.053	0.055	0.308	0.060	0.073
During EQ	Raw Return	0.013	0.012	0.987	0.147	0.147
	Excess Return (Mkt)	-0.018	-0.018	0.988	0.129	0.128
	Excess Return (DGTW)	-0.015	-0.019	0.241	0.121	0.123
	CHS Default Probability	0.060	0.062	0.253	0.101	0.104
9m after EQ	Cumulative Return	0.143	0.096	0.004	0.343	0.327
	Cumulative Return (vs Mkt)	0.070	0.025	0.007	0.316	0.300
	Cumulative Return (vs DGTW)	0.041	0.001	0.003	0.267	0.266
	CHS Default Probability	0.058	0.063	0.069	0.093	0.101

Table 4: Fire-sales and credit ratings

This table reports realized downgrade probabilities of fire-sale ('treated') stocks and controls. Each treated firm-quarter is matched to a control by industry, credit rating, return in the event quarter (EQ), and the propensity score to experience fire sales as estimated in Table 2. In Panel A, the outcome variable equals 1 if the credit rating was downgraded during different time periods and zero otherwise. These periods are EQ-2 and EQ-1 (the 6 months before the start of EQ), EQ-1 (the 3 months before the start of EQ), EQ, EQ+1 (the 3 months after the end of EQ), and EQ and EQ+1 (the 6 months starting at the beginning of EQ). In Panel B, we take into account the severity of downgrades by reporting the average number of notches decrease in credit rating. In particular, columns (1) and (2) report the average of a variable that equals the number of notches downgraded if there is a downgrade and zero otherwise. Panel C reports the average number of rating upgrade notches. Standard errors and t-statistics reported in columns (4) and (5) respectively are robust for heteroskedasticity.

Panel A: $Pr\{Downgrade\}$

	N(Treated) = 4260				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.039	0.038	0.0014	0.0039	0.36
EQ-1	0.020	0.020	-0.0002	0.0029	-0.08
Event Quarter	0.022	0.031	-0.0092	0.0032	-2.87
EQ+1	0.024	0.033	-0.0089	0.0034	-2.64
EQ and EQ+1	0.045	0.060	-0.0153	0.0044	-3.43

Panel B: $E[\#NotchesDown]$

	N(Treated) = 4260				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.085	0.083	0.0021	0.0096	0.22
EQ-1	0.042	0.043	-0.0007	0.0065	-0.11
Event Quarter	0.050	0.070	-0.0204	0.0076	-2.70
EQ+1	0.066	0.097	-0.0310	0.0146	-2.12
EQ and EQ+1	0.114	0.163	-0.0486	0.0171	-2.84

Panel C: $E[\#NotchesUp]$

	N(Treated) = 4258				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.068	0.084	-0.0160	0.0080	-2.00
EQ-1	0.031	0.038	-0.0075	0.0040	-1.89
Event Quarter	0.029	0.030	-0.0014	0.0036	-0.39
EQ+1	0.031	0.029	0.0026	0.0040	0.65
EQ and EQ+1	0.060	0.059	0.0012	0.0052	0.23

Table 4: Fire-sales and credit ratings (*Continued*)

Panels A2, B2, and C2 report the same tests as panels A, B, and C but on a sub-sample of firms with negative Event Quarter returns.

Panel A2: $Pr\{Downgrade|EQ\,ret < 0\}$

	N(Treated) = 2103				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.050	0.045	0.0052	0.0064	0.81
EQ-1	0.024	0.028	-0.0033	0.0047	-0.71
Event Quarter	0.036	0.049	-0.0133	0.0059	-2.26
EQ+1	0.041	0.053	-0.0119	0.0063	-1.87
EQ and EQ+1	0.073	0.092	-0.0195	0.0081	-2.40

Panel B2: $E[\#NotchesDown|EQ\,ret < 0]$

	N(Treated) = 2103				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.114	0.102	0.0114	0.0153	0.74
EQ-1	0.056	0.060	-0.0038	0.0107	-0.36
Event Quarter	0.082	0.112	-0.0300	0.0145	-2.07
EQ+1	0.117	0.177	-0.0594	0.0292	-2.03
EQ and EQ+1	0.195	0.280	-0.0842	0.0342	-2.46

Panel C2: $E[\#NotchesUp|EQ\,ret < 0]$

	N(Treated) = 2103				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.074	0.079	-0.0057	0.0097	-0.59
EQ-1	0.037	0.038	-0.0010	0.0065	-0.15
Event Quarter	0.029	0.025	0.0048	0.0049	0.96
EQ+1	0.028	0.018	0.0095	0.0050	1.88
EQ and EQ+1	0.057	0.043	0.0138	0.0070	1.97

Table 5: Transitory fire-sale effects: Credit Ratings Agencies versus CDS markets

This table reports credit rating downgrades and CDS spread changes for fire-sale stocks and controls. Each treated firm-quarter is matched to a control by industry, credit rating, return in the event quarter (EQ), and the propensity score to experience fire sales as estimated in Table 2 (see Sections 2.3 and 3.2 for detailed matching criteria). Panel A reports the probability of downgrades during different periods (6 months before the EQ, 3 months before the EQ, the EQ, three months after the end of EQ, and 6 months starting with the beginning of EQ) for treated and control firms. Column (3) presents 'Average Treatment effect on Treated' (ATT), or the difference in realized downgrade probabilities between treated and control firms; a negative number indicates lower downgrade probabilities for treated firms relative to controls. Heteroskedasticity robust standard errors and t-statistics for the ATT statistic are reported in columns (4) and (5). Panel B conducts similar analysis but with changes in the Credit Default Swap spreads over the respective periods instead of downgrades as the outcome variable. Panel C repeats the analysis for ratings implied by the CDS spread levels (as reported by Markit) instead ratings issued by CRAs. Due the CDS data availability, the sample is constrained to 2002-2015 only.

	N(Treated) = 592				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
Panel A: Downgrades by CRA					
EQ-2 and EQ-1	0.042	0.029	0.0135	0.0100	1.35
EQ-1	0.019	0.014	0.0051	0.0068	0.74
Event Quarter	0.017	0.037	-0.0203	0.0075	-2.69
EQ and EQ+1	0.029	0.068	-0.0389	0.0099	-3.94
Panel B: CDS Spread changes (basis points)					
EQ-2 and EQ-1	0.92	-6.67	7.553	7.486	1.01
EQ-1	-0.36	-3.58	3.221	5.503	0.59
Event Quarter	5.40	2.99	2.418	9.228	0.26
EQ and EQ+1	20.28	15.83	4.456	12.588	0.35
Panel C: Implied Downgrades by CDS					
EQ-2 and EQ-1	0.032	0.032	0.0000	0.0090	0.00
EQ-1	0.012	0.015	-0.0034	0.0048	-0.70
Event Quarter	0.017	0.017	0.0000	0.0064	0.00
EQ and EQ+1	0.037	0.035	0.0017	0.0105	0.16

Table 6: Fire-sale effects by return group

This table reports differences in credit rating downgrades for fire-sale stocks and controls across different groups of stocks formed based on pre and post event quarter returns. Each treated firm-quarter is matched to a control by industry, credit rating, return in the event quarter (EQ), and the propensity score to experience fire sales as estimated in Table 2. Firms are sorted into two groups based on returns in the treated firm's event quarter (EQ), and also based on returns in excess of the market over the 6 months after the EQ. In Panel A (B), we report differences in the probability of downgrades (expected downgrade notches) during EQ. Panels A2 and B2 restrict the sample to firms with negative EQ returns.

Panel A: ATT for $Pr\{Downgrade\}$

		Excess Return 6 months after EQ		
		Low	High	All
Return in the Event Quarter	Low	-0.0056	-0.0144	-0.0093
	High	-0.0057	-0.0116	-0.0090
	All	-0.0057	-0.0130	-0.0092

Panel B: ATT for $E[\#NotchesDown]$

		Excess Return 6 months after EQ		
		Low	High	All
Return in the Event Quarter	Low	-0.0094	-0.0339	-0.0191
	High	-0.0115	-0.0302	-0.0219
	All	-0.0103	-0.0321	-0.0204

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

		Excess Return 6 months after EQ		
		Low	High	All
Return in the Event Quarter	Low	-0.0090	-0.0320	-0.0190
	High	-0.0074	-0.0098	-0.0087
	All	-0.0081	-0.0198	-0.0133

Panel B2: ATT for $E[\#NotchesDown|EQret < 0]$

		Excess Return 6 months after EQ		
		Low	High	All
Return in the Event Quarter	Low	-0.0158	-0.0800	-0.0422
	High	-0.0148	-0.0246	-0.0199
	All	-0.0153	-0.0495	-0.0300

Table 7: Placebo selling pressure and credit ratings

This table presents placebo tests for the results reported in Table 4. We reconstruct the treatment variable, $MFFlow$, using all fund outflows instead of restricting the sample to funds that experience outflows $> 5\%$ as we do in our main tests. See Section 4.4 for details. We then replicate the analysis in Table 4 for this placebo treatment.

Panel A: $Pr\{Downgrade\}$

	N(Treated) = 4092				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.053	0.052	0.0007	0.0046	0.16
EQ-1	0.029	0.025	0.0034	0.0034	1.00
Event Quarter	0.032	0.035	-0.0032	0.0036	-0.88
EQ+1	0.037	0.040	-0.0027	0.0039	-0.69
EQ and EQ+1	0.064	0.069	-0.0054	0.0049	-1.09

Panel B: $E[\#NotchesDown]$

	N(Treated) = 4092				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.128	0.124	0.0039	0.0120	0.33
EQ-1	0.070	0.061	0.0090	0.0084	1.07
Event Quarter	0.075	0.084	-0.0093	0.0094	-0.99
EQ+1	0.115	0.102	0.0130	0.0188	0.69
EQ and EQ+1	0.184	0.181	0.0034	0.0218	0.16

Panel C: $E[\#NotchesUp]$

	N(Treated) = 4083				
	Treated	Control	ATT	SE	t-stat
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.060	0.057	0.0027	0.0057	0.47
EQ-1	0.030	0.029	0.0012	0.0041	0.30
Event Quarter	0.030	0.026	0.0044	0.0037	1.19
EQ+1	0.026	0.029	-0.0029	0.0041	-0.72
EQ and EQ+1	0.056	0.055	0.0012	0.0054	0.22

Table 8: Robustness tests

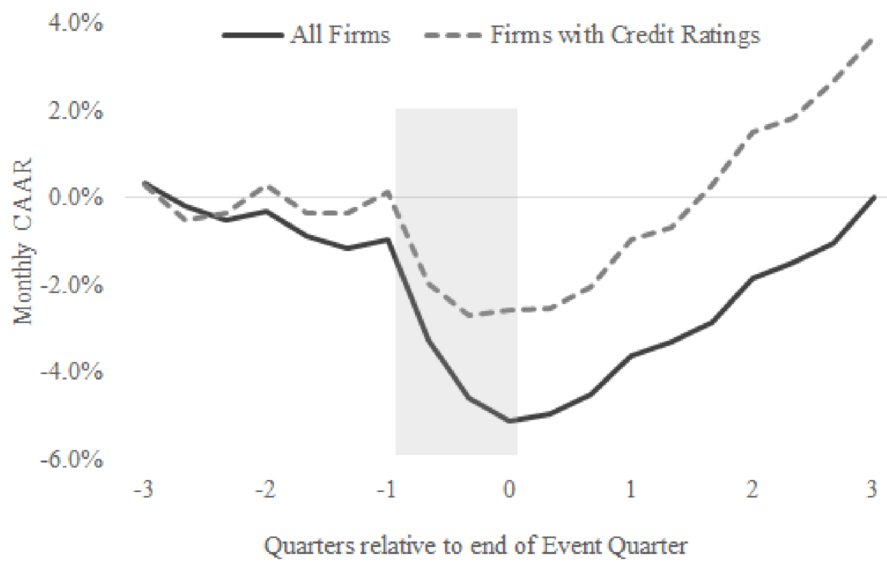
This table examines the robustness of the main results reported in Table 4 to different matching criteria and sample composition. Each row reports the difference in the realized probability of downgrades between treated and control firms during the 6 months starting at the beginning of the event quarter.

	Industries cut	EQ Return caliper	Pscore caliper	Controls per 1 Treated	N(Treated)	Treated	Control	ATT	SE	t-stat
						(1)	(2)	(3)	(4)	(5)
Baseline (Table 4 panel A)	5	0.025	0.025	1	4,260	0.045	0.060	-0.0153	0.0044	-3.43
+ wider return caliper	5	0.050	0.025	1	4,949	0.047	0.065	-0.0184	0.0041	-4.46
+ wider pscore caliper	5	0.050	0.050	1	5,383	0.048	0.064	-0.0152	0.0037	-4.15
+ multiple controls	5	0.050	0.050	2	5,383	0.048	0.064	-0.0156	0.0037	-4.25
Subsamples										
Exclude fin. crisis (3Q'07-1Q'09)	5	0.025	0.025	1	3,975	0.036	0.055	-0.0181	0.0038	-4.13
Exclude "Money"&"Util" (FF12)	5	0.025	0.025	1	2,701	0.050	0.068	-0.0178	0.0048	-2.99
Exclude "Manufacturing" (FF5)	5	0.025	0.025	1	2,836	0.045	0.056	-0.0109	0.0045	-2.06
Different criteria within the baseline matching scheme										
Exact rating only match	5	0.025	0.025	1	3,017	0.040	0.055	-0.0149	0.0041	-2.71
+ wider return caliper	5	0.050	0.025	1	3,954	0.041	0.058	-0.0175	0.0041	-3.62
+ wider pscore caliper	5	0.050	0.050	1	4,620	0.044	0.057	-0.0128	0.0042	-3.04
+ multiple controls	5	0.050	0.050	2	4,620	0.044	0.058	-0.0142	0.0038	-3.38
Finer industry match	12	0.025	0.025	1	3,362	0.044	0.054	-0.0107	0.0039	-2.12
+ wider return caliper	12	0.050	0.025	1	4,221	0.045	0.059	-0.0135	0.0045	-3.00
+ wider pscore caliper	12	0.050	0.050	1	4,840	0.045	0.061	-0.0159	0.0041	-3.86
+ multiple Controls	12	0.050	0.050	2	4,840	0.045	0.060	-0.0154	0.0041	-3.74
Match without regards to industry	0	0.025	0.025	1	5,314	0.049	0.065	-0.0154	0.0036	-4.30
+ tighter return caliper	0	0.010	0.025	1	4,561	0.045	0.057	-0.0118	0.0039	-3.06
+ tighter pscore caliper	0	0.010	0.010	1	3,747	0.042	0.052	-0.0091	0.0047	-1.94
+ multiple controls	0	0.010	0.010	2	3,747	0.042	0.055	-0.0121	0.0047	-2.60
Different matching scheme – based on EQ return quantile and closest pscore										
Exact rating	5	5	0.025	1	3,719	0.039	0.052	-0.0134	0.0052	-2.57
+ fine return grid	5	10	0.025	1	3,272	0.037	0.060	-0.0229	0.0058	-3.96
+ fine return & industry grids	12	10	0.025	5	2,300	0.039	0.054	-0.0157	0.0063	-2.49
Coarse Rating	5	5	0.025	1	4,488	0.045	0.057	-0.0127	0.0051	-2.51
+ fine return grid	5	10	0.025	1	4,388	0.045	0.059	-0.0144	0.0054	-2.67
+ fine return & industry grids	12	10	0.025	5	3,605	0.044	0.065	-0.0214	0.0051	-4.16

Figure 1: Transitory fire-sales and abnormal stock returns

This figure plots cumulative average abnormal returns (CAAR) in the three quarters before and after 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.

Panel A: Market-adjusted



Panel B: DGTW-adjusted

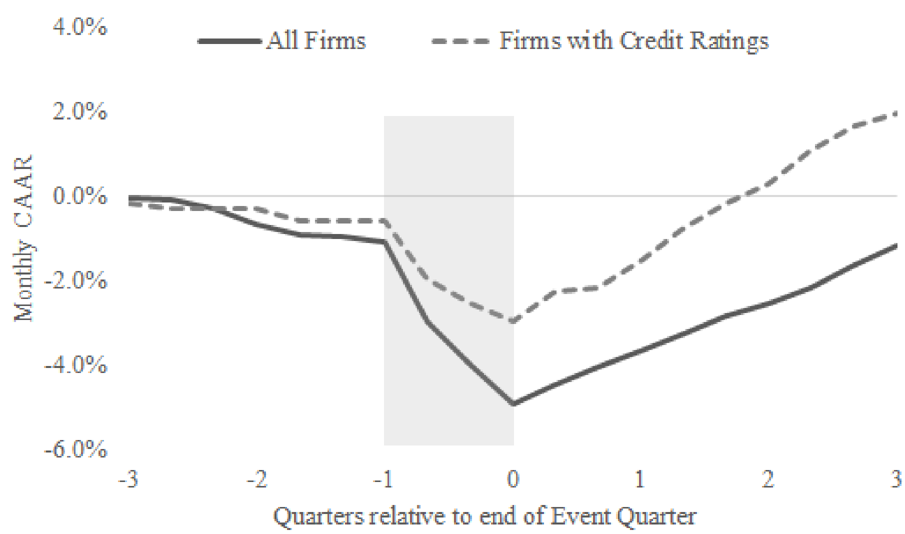
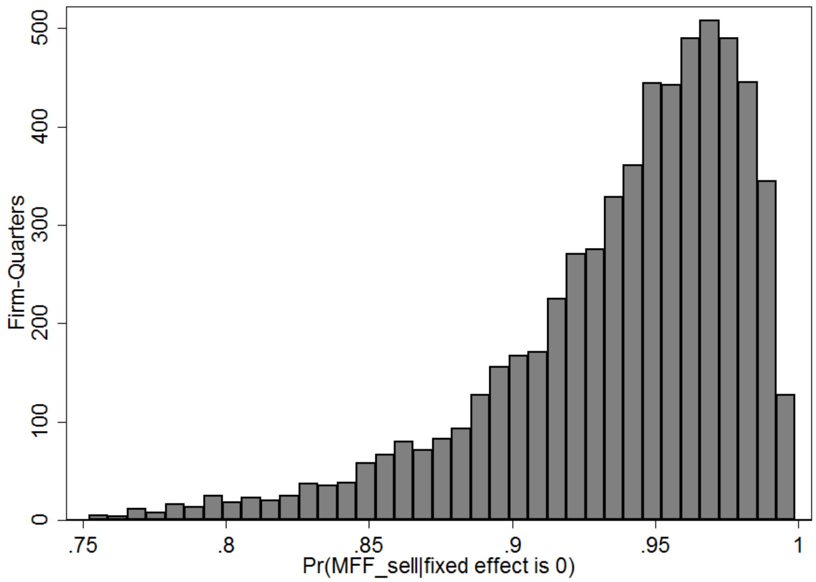


Figure 2: Propensity scores

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 firm-quarters classified as treated (Panel A) and all others (Panel B). We set year-quarter fixed effects to zero to enable comparability of score across time. See Sections 1.3 and 2.2 for details.

Panel A: Treated



Panel B: Not Treated

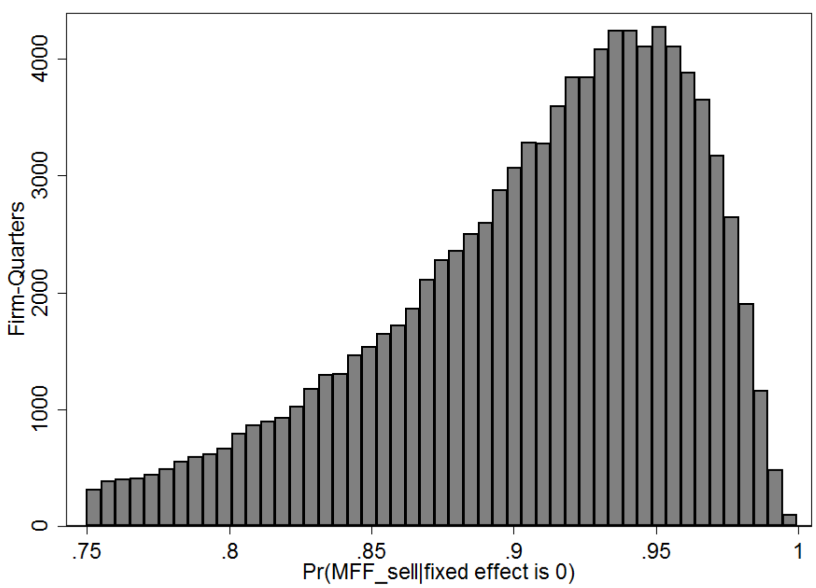
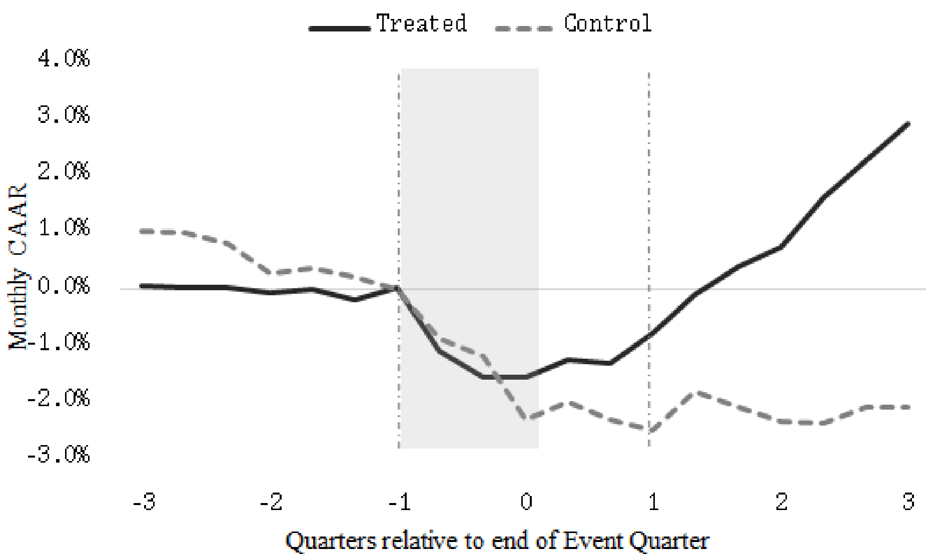


Figure 3: Matched sample analysis of transitory fire-sales: Stock returns versus CDS markets

This figure plots cumulative average abnormal returns (CAAR) and CDS spread changes for the fire-sale stocks (as defined in section 1.3) and matched controls (see Section 2.3 for matching criteria) during the 2002-2015 period. Panel A reports CAARs relative to characteristic-matched portfolios. Panel B reports CDS spread changes for the same set of treated and control firms. The shaded area is the Event Quarter (EQ), and the area between vertical dashed lines are the 6 months starting at the beginning of EQ.

Panel A: Cumulative DGTW-adjusted stock returns



Panel B: Cumulative changes in CDS spreads

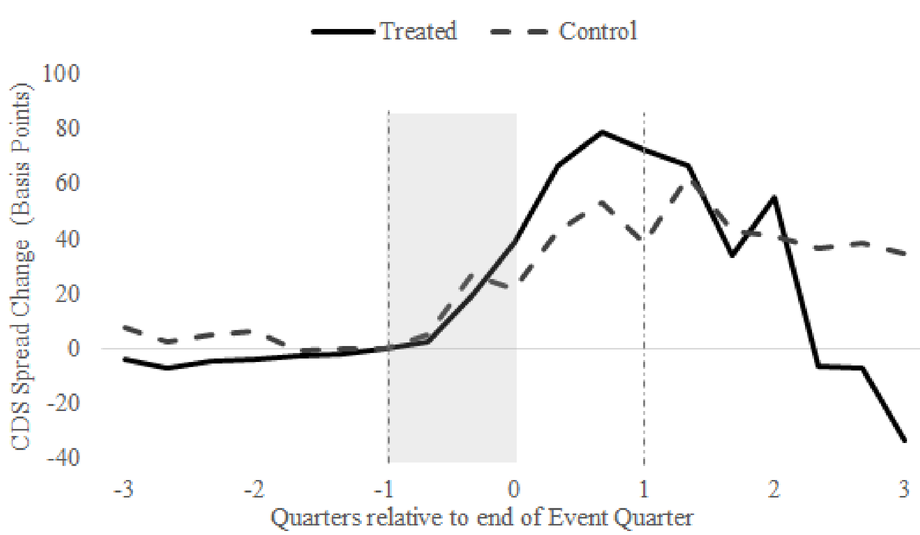
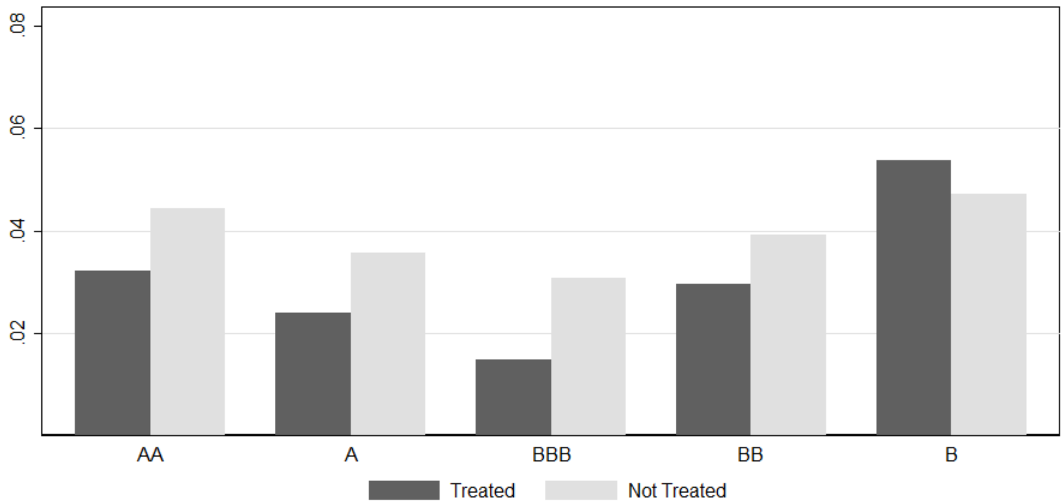


Figure 4: Fire-sale effects by rating level

This figure compares credit rating downgrade probability for fire-sales stocks (as defined in Section 1.3) to all other stocks (Panel A), and to matched controls (Panel B) by rating category.

Panel A: Full sample



Panel B: Matched sample

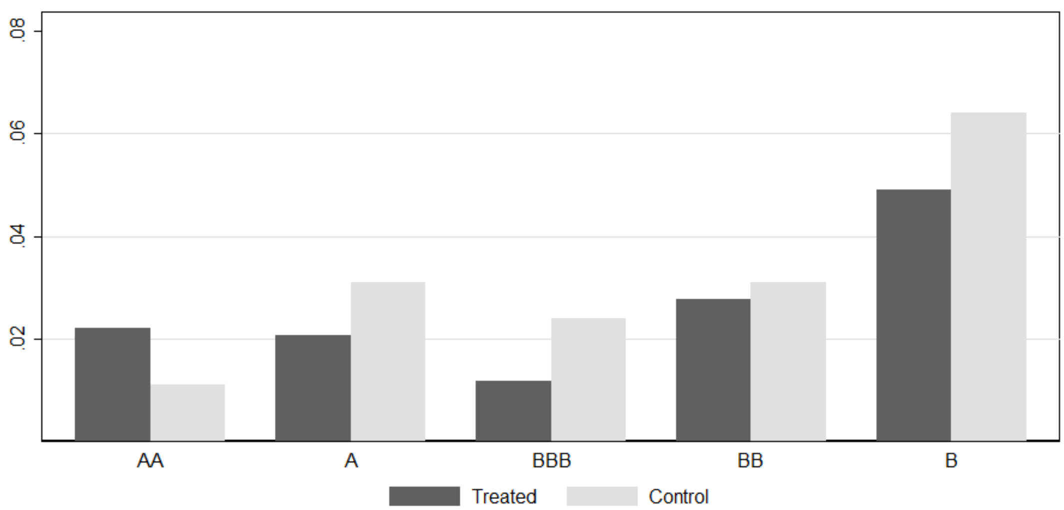
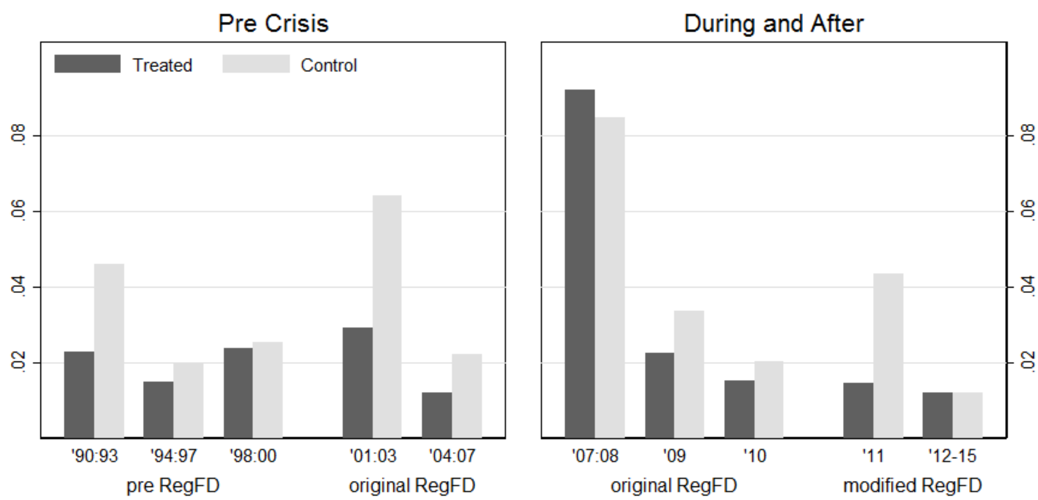


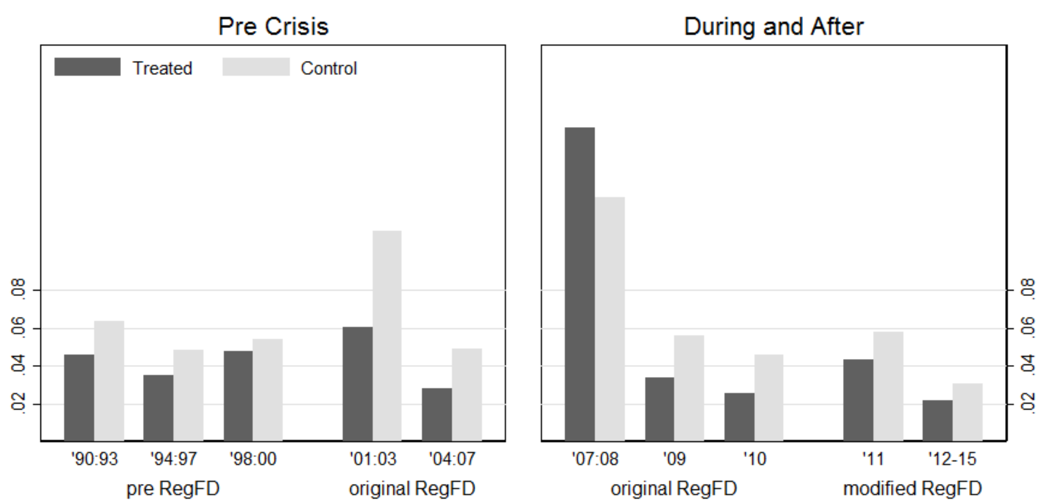
Figure 5: Fire-sale effects over time

This figure compares realized rating downgrade probabilities for fire-sales stocks (as defined in section 1.3) to matched controls (see Section 2.3 for matching criteria) over different time periods and regulatory regimes. Panel A reports results for the Event Quarter, whereas Panel B reports results for 6 months staring at the beginning of the Event Quarter.

Panel A: Event Quarter (EQ) ATT



Panel B: EQ to EQ+1 ATT



Appendix A. Mutual funds fire-selling

This section describes the calculation of the mutual fund price pressure variable. This study uses a dataset that combines mutual fund holdings data and firm-level data for the period from 1990 to 2014. For the mutual fund, we use the Center for Research in Security Prices (CRSP) Survivorship Bias Free Mutual Fund database. Although all the information is provided at the share class level, the underlying portfolio for the different share classes within a fund is the same. Therefore, to aggregate data at the fund level, we use the MFLINKS data provided by Wharton Research Data Services (WRDS).[22] A fund's return series, expense ratio, and turnover ratio are the weighted averages of the same variables of its different share classes. The weights are based on the total net assets (TNA) of each share class at the beginning of the period. We then merge CRSP mutual fund database with the Thompson Financial CDA/Spectrum holdings database.

Our mutual fund sample includes equity mutual funds. Following Coval and Stafford (2007), we exclude funds with the following Investment Objective Codes: international, municipal bonds, bond and preferred, or metals. We also excludes sector mutual 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.

To construct liquidity-driven trades variable, we use implied trades variable proposed in Edmans, Goldstein, and Jiang (2012) that capture plausibly exogenous variations in stock prices induced by mutual fund outflows. While mild fund outflows can be absorbed by a fund's cash position, extreme outflows are more likely to force managers to liquidate assets, thereby creating a significant price impact on the underlying assets.[23]

More precisely, we first calculate the percentage outflow of the fund j from the end of quarter t to the end of quarter $t+1$ and is given as follows

$$Outflow_{j,q} = -(TNA_{j,q} - TNA_{j,q-1}(1+r_{j,q})), \tag{A.1}$$

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:

$$MFFlow_{i,q} = \sum_{j=1}^{m} \frac{Outflow_{j,q} * s_{i,j,q-1}}{\mathrm{Volume_{i,q}}}, \tag{A.2}$$

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

[22] Despite the use of the MFLINKS file, some share classes are still not mapped to any identifier. Therefore, for these remaining observations, we use the CRSP portfolio number to aggregate the different share classes.

[23] See Coval and Stafford (2007), Edmans, Goldstein, and Jiang (2012), and Khan, Kogan, and Serafeim (2012)

$$s_{i,j,q} = \frac{Shares_{i,j,q} * Price_{i,q}}{\mathrm{TNA_{j,q}}}, \tag{A.3}$$

is fund *j*'s holdings of stock *i* as a percentage of fund *j*'s TNA at the end of the quarter.

Finally, *Treated* firm-quarters are identified as those with $MFFlow_{i,q}$ below the 20th percentile value of the full sample (the global cutoff) and the 10th percentile value of each quarterly sample (the local cutoff).[24]

Our trading pressure variable is first motivated by Coval and Stafford (2007) who document that extreme fund outflows force mutual funds to engage in liquidity-motivated trades of their holdings, thereby creating a downward price pressure on stocks commonly held by such funds. However, instead of using the pressure variable based upon the actual trades of the funds, as in Coval and Stafford (2007), our measure is calculated by assuming that the distressed funds adjust their existing holdings proportionally across the board based upon the previous portfolio weights. Computing implied trades motivated by Edmans, Goldstein, and Jiang (2012) mitigates the possibility of adding the discretionary selling by the managers.

Furthermore, the exogeneity of our variable can be violated if the fundamentals of the funds' holdings negatively affect mutual fund performance and , as a consequence, generates fund outflows. This conjecture is likely for sector funds during industry downturns. For this reason, we exclude sector funds.

Appendix B. Variables Definitions

All variables are firm-quarter unless stated otherwise.

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.

[24] Coval and Stafford (2007) and Edmans, Goldstein, and Jiang (2012) use the 10th percentile selling pressure value of the full sample as the threshold. However, imposing the uniform threshold prevents this study from constructing the balanced treatment samples across the sample period. That is, applying this single cutoff will yield temporally concentrated samples and weaken the statistical power of this study to detect the differential rating decisions by credit rating agency. To have the balanced data across the sample period, we relaxed the restriction.

All variables are firm-quarter unless stated otherwise.

Variable	Description
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 *MFFlow* 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. TIn particular, 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.
Return (Mkt)	Stock return, including dividends, minus the total return on CRSP value-weighted index for the same period. Source: CRSP.

All variables are firm-quarter unless stated otherwise.

Variable	Description
Return (DGTW)	Stock return, including dividends, minus the return on the characteristics-matched portfolio 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.
MCap	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. Source: Markit
CDS Implied Downgrades	Based on ratings implied by 5 year CDS contracts on a firm as computed by Markit.

Table A2: Covariate balance for treated firms and controls: Moody's 1990-2008 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 firm that has the closest Event Quarter (EQ) return, same narrow credit rating, and within a one-third of a standard deviation of the propensity score to experience fire sales as estimated in Table 2, specification 4. We drop any matches where the absolute difference in EQ returns between treated and control firms is greater than 2.5% and consider matches within a broader rating category (i.e. ignoring 1,2,3) if a satisfactory match cannot be found within a narrow rating category. 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.

Panel A: Propensity-score contributors

	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 B: Other variables of interest

		Means			St.Deviations	
		Treated	Control	P-value	Treated	Control
		(1)	(2)	(3)	(4)	(5)
pre EQ	CAPM β	0.908	0.964	0.334	0.581	0.630
	Book-to-Market	1.115	0.921	0.036	1.680	1.388
	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
During EQ	Raw Return	-0.009	-0.009	0.978	0.164	0.164
	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
9m after EQ	Cumulative Return	0.151	0.099	0.018	0.396	0.373
	Cumulative Return (vs Mkt)	0.065	0.016	0.016	0.371	0.347
	Cumulative Return (vs DGTW)	0.027	-0.009	0.001	0.334	0.315
	CHS Default Prob.	0.082	0.089	0.264	0.130	0.145

Table A3: Fire-sales and credit ratings: Moody's 1990-2008 sample

This table reports realized downgrade probabilities of fire-sale ('treated') stocks and controls. Each treated firm-quarter is matched to a control by industry, credit rating, return in the event quarter (EQ), and the propensity score to experience fire sales. In Panel A, the outcome variable equals 1 if the credit rating was downgraded during different time periods and zero otherwise. These periods are EQ-2 and EQ-1 (the 6 months before the start of EQ), EQ-1 (the 3 months before the start of EQ), EQ, EQ+1 (the 3 months after the end of EQ), and EQ and EQ+1 (the 6 months starting at the beginning of EQ). In Panel B, we take into account the severity of downgrades by reporting the average number of notches decrease in credit rating. In particular, columns (1) and (2) report the average of a variable that equals the number of notches downgraded if there is a downgrade and zero otherwise. Panel C reports the average number of rating upgrade notches. Standard errors and t-statistics reported in columns (4) and (5) respectively are robust for heteroskedasticity.

Panel A: $Pr\{Downgrade\}$

	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 B: $E[\#NotchesDown]$

	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 C: $E[\#NotchesUp]$

	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