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

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Abstract

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

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Abstract

We find that Credit Rating Agencies (CRAs) see through transitory shocks to credit risk that stem from transitory shocks to equity prices, while market-based measures of credit risk do not. For a given stock return, CRAs are significantly less likely to downgrade firms with transitory shocks than those with permanent shocks. However, credit default swap spreads and model-implied default probabilities do not distinguish between such shocks. These results explain why ratings are useful despite the availability of market-based estimates of credit risk: the ability to ignore transitory shocks is valuable because rating changes have real consequences for private contracts and access to capital.

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

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

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

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

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

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

[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.

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

An ideal setup to test whether CRAs can discern which shocks are transitory is to consider two ex ante identical firms. Suppose market participants perceive a similar increase in credit risk for both firms. However, the 'treated' firm's increase in risk is due to a transitory shock and the 'control' firm's is due to a permanent shock. If CRAs are able to distinguish between these two types of shocks in real-time, we expect that the treated firm will be less likely to be downgraded than the control soon after the shock. Note that because we compare downgrade rates of firms that experience transitory shocks to firms that experience permanent shocks of the same magnitude, these tests are not influenced by factors common to all firms. For example, the coarseness of rating categories (Goel and Thakor, 2015) or sluggishness of the response of ratings to news (Chava, Ganduri, and Ornthanalai, 2016) will not influence our results.

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

We first consider two simple tests of whether CRAs can discern which shocks are transitory. These tests consider firms that experience a significant negative shock—abnormal returns relative to their characteristic-matched portfolio benchmarks (as in Daniel et al., 1997, DGTW) in the bottom quintile within their rating category that quarter. We split these firms into two groups based on the likelihood that their return shock is transitory. The first test uses 'perfect foresight' to determine the groups. Firms in the top (bottom) quintile of abnormal returns over the next year are in the 'More' ('Less') transitory group. The second test uses extreme quintile sorts on ex ante Amihud illiquidity to form the 'More' and 'Less' transitory groups. We expect (and confirm in the data) that more illiquid firms will exhibit greater reversals for a given return shock relative to the more liquid firms. Both these tests support the hypothesis that CRAs distinguish between transitory and permanent shocks. The difference in downgrade rates between the less and more transitory shock groups is -1.2% for the perfect foresight sorts and -1.4% for the liquidity sorts. Both differences are statistically significant at the 1% level and economically meaningful relative to the average downgrade rate of 6.3% for firms that in the bottom quintile of abnormal returns in a given quarter.

Our main tests use mutual fund fire sales to identify firms with transitory shocks to equity value. These tests have the benefit that they are due to a specific economic mechanism for transitory shocks. Edmans, Goldstein, and Jiang (2012) (EGJ) show that implied fire sales by distressed mutual funds result in meaningful shocks to equity prices that reverse over several quarters. We confirm that this pattern exists in our sample of rated firms. We find that firms in the highest decile of fire sales have abnormal (DGTW adjusted) returns of -3.0% to -4.0% that quarter, which almost entirely reverse over the next two years.

We find that CRAs are significantly less likely to downgrade firms that experience fire sales conditional on a given return that quarter. This lower likelihood of downgrades in response to fire sales is economically meaningful: conditional on a negative return, moving from the 25th to the 75 percentile of fire sales lowers downgrade rates by approximately 2%, relative to the mean downgrade rate of 4.7%.

As with most empirical results, a key concern is that fire-selling pressure is not exogenous: firms that experience greater fire selling pressure may be different from those that do not, and this difference rather than the ability of CRAs to identify transitory shocks is responsible for the difference in downgrade rates. One possible story is that investors may be more likely to withdraw capital from mutual funds that they believe will perform poorly in the future. If investors behave in such a manner, stocks subject to fire sales are of worse quality, and hence likely to have greater downgrade probabilities than a typical firm, which would bias us against finding our results.

Nevertheless, our empirical strategy contains several elements to mitigate such selection biases. First, as in Edmans, Goldstein, and Jiang (2012), we identify fire sales using hypothetical distressed fund trades, which are computed assuming that funds sell their holdings in proportion to their portfolio weights before the extreme outflows. Although this strategy addresses selection biases arising from fund manager discretion during the event quarter, fire-sale firms may be different from typical firms prior to the event quarter in terms of variables related to mutual fund ownership and past performance (Berger, 2017). We therefore control for several variables that predict fire sales including mutual fund ownership, past returns, past rating changes, size, volatility, model-implied default probability, and liquidity. We also perform a matched sample analysis in which we match not only on the propensity of firms to experience fire sales, but also select as controls firms that have experienced fire sales in the past three years (but not in the current quarter) as suggested by Berger (2017) to control for unmodelled heterogeneity. Finally, Wardlaw (2018) argues that the EGJ measure inadvertently also sorts firms on permanent shocks because it contains contemporaneous dollar volume in its denominator. This is not a concern for our research design because we always control for contemporaneous returns. All we need is that for a given return, more fire-selling pressure is more likely to be associated with a transitory shock (which we confirm is true in our sample). Finally, we also find similar results for an alternate measure of fire sales from Lou (2012) that does not include contemporaneous dollar volume and hence is not subject to the Wardlaw (2018) critique.

A possible alternative explanation for these results is that credit markets actually distinguish between permanent and temporary equity price shocks, and CRAs passively follow credit markets. If true, this ability of credit markets to see through temporary shocks in equity markets may be interesting in itself; however, such behavior does not imply a special role for CRAs. Consequently, we examine whether credit markets respond to equity fire sales. In particular, we examine Credit Default Swap (CDS) markets because Blanco, Brennan, and Marsh (2005) find that price discovery of credit risk happens in CDS markets rather than bond markets. We find that CDS spreads and CDS-implied rating downgrades increase for firms in the top decile of fire sales. Most of the increase in spreads occurs in the quarter after the fire sale, consistent with Hilscher, Pollet, and Wilson (2015), who find that information flows from equity to CDS markets. These spreads eventually reverse, confirming that the shock is indeed transitory in CDS markets. We find that once for we control for returns, CDS spread changes are not correlated with fire-selling pressure, showing that investors cannot use CDS spreads to discern whether these shocks are transitory. The sample with available CDS data is smaller than the ratings sample in the cross-section as well as the time-series and hence, it is possible that these tests do not have power. However, we find that even in the sample where CDS data exist, CRA downgrades remain significantly negatively correlated with fire sales. We also find that the predicted default probability from the CHS model that uses information from accounting statements as well as equity markets displays a similar pattern as CDS spreads.

Thus, CRAs differentiate in real-time between transitory and permanent shocks, while CDS and equity markets take several quarters to do so. Moreover, the differential response of CRAs is consistent with the pattern in future realized defaults. We find that controlling for returns, and other characteristics including CHS default probability, greater fire-selling pressure is associated with lower bankruptcy rates over the five years after the shock, thereby validating the CRA decision to not downgrade them as frequently.

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

We therefore examine whether the ability to discern which shocks are transitory is related to information advantage of CRAs relative to markets. First, we test whether the treatment effect is stronger when there is greater uncertainty in the firm's public information environment. Following Barron, Kim, Lim, and Stevens (1998), we use the cross-sectional dispersion in sell-side equity analyst forecasts, as well as the time-series standard deviation of their median forecast errors as measures of information uncertainty. We find that the

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

interaction between the treatment and these measures of uncertainty is significant: CRAs are less likely to downgrade treated firms with high levels of information uncertainty relative to matched controls. These results suggest that CRAs complement equity analysts: they add more value in situations where analyst forecasts are less precise.

Second, we follow Jorion, Liu, and Shi (2005) and use the enactment of Regulation Fair Disclosure (Reg FD) as a shock to the relative information advantage of CRAs. Reg FD prohibited publicly traded firms from selectively disclosing material information to investors or securities professionals, but provided an exemption for disclosure to CRAs. If access to nonpublic information is the channel through which CRAs identify transitory shocks, we expect that the differences in downgrades between treated and control firms will increase post Reg FD. We find that this is indeed the case, with a significant increase in the treatment effect after Reg FD, even after controlling for time, rating, and industry fixed effects as well as a host of firm characteristics.

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

Thus, our paper contributes to research that examines the trade-off between ratings stability and accuracy (Altman and Rijken, 2004, 2006; Cornaggia and Cornaggia, 2013; Löffler, 2013). Our paper complements

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

[5]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).

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

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

Our results also suggest that regulatory efforts to limit the privileged position of CRAs (for e.g., see discussion in section 3.3.2 on the Dodd-Frank Act), albeit with the laudable goal of encouraging investors to do independent analysis, can have unintended consequences. Transitory shocks in financial markets are more likely to propagate to the real economy if regulations restrict the access of CRAs to private information (thereby inhibiting their ability to discern which shocks are temporary) or create disincentives for CRAs to issue independent opinions.

1. Data and simple tests

This section describes the data we use in the paper and also provides results of two simple tests that motivate our main analysis.

1.1. Data

Our main dataset is based on the intersection of four databases: (i) mutual fund holdings from Thompson 13F filings, (ii) mutual fund returns and total net assets from the Center for Research in Security Prices

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

[7] 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.

(CRSP) Survivorship-Bias Free mutual fund database, (iii) credit ratings and firm accounting data from Compustat, and (iv) equity returns and prices from CRSP. We also use data from Capital IQ and I/B/E/S for supplementary tests. The filters we impose on the mutual fund data follow prior research and are described in Appendix A.

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

Table 1 presents summary statistics for the key 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.

1.2. Simple Tests

Our goal is to test whether credit rating agencies can distinguish between transitory and permanent shocks to credit risk. All our tests are based on one simple idea that ensures that our results are not due to sluggishness of CRAs in responding to news. We compare two sets of firms that receive an identical negative shock to stock prices in a given 'event' quarter (EQ). We choose the two sets of firms such that the negative shock is more likely to be transitory for one set relative to the other. We test whether the transitory-shock group is more or less likely to experience downgrades than the other group during EQ—before the transitory nature of the shock is revealed to market participants. Because this is a test of differences, it is immune from the effects of sluggishness of CRAs that should apply to both groups.

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

[9]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.

We begin with two simple tests. We first restrict the sample to firms that have experienced a significant negative shock in EQ—i.e. DGTW adjusted stock returns in the bottom quintile. We then sort firms into subsamples based on whether the shock is likely to be transitory using two criteria:

- Perfect foresight: This measure uses ex post returns to identify which firms are more likely to have experienced transitory shocks. 'Less Transitory' firm-quarters are those in the bottom quintile of DGTW adjusted returns over the next year, while 'More Transitory' firm-quarters are in the top quintile. The sorts on ex post returns are within each credit rating and quarter to ensure balance across rating categories and time.

- Amihud ratio: We expect that for a given return shock, a more illiquid firm is more likely to experience reversals arising price impact due to selling pressure. Firms in the top Amihud illiquidity quintile (most illiquid), measured over the quarter before the return shock, are classified as 'More Transitory', while those in the bottom quintile are 'Less Transitory'. As with perfect foresight, sorts on illiquidity are within quarter and credit rating.

To ensure that both groups experience identical return shocks in EQ, we match firms by their EQ returns. For each firm in the more transitory group, we find the firm in the less transitory group with the closest EQ return within the same rating category, and exclude any pairs with absolute difference in return greater than 10%.

We examine whether CRAs distinguish between these more and less transitory groups in real-time (during EQ). Table 2 shows downgrade rates and mean abnormal returns over the quarter in which the firm receives a negative abnormal return shock, along with cumulative abnormal returns over the next year for each group. The table also reports mean differences between the two groups for these variables. p-values for differences in means are based on simple t-tests—inferences are unchanged if we use the non-parametric Wilcoxon signed rank test.

Panel A examines groups based on perfect foresight sorts. We see that restricting the sample to firms in the bottom quintile of abnormal returns in a given quarter results in economically significant mean abnormal returns of approximately -19% for both more and less transitory shock groups. By construction, the less transitory group has substantial negative and the more transitory group has substantial positive abnormal returns over the next year. The interesting result is that despite having essientialy identical return shocks, the downgrade rate is significantly lower in the more transitory group than the less transitory group in the event quarter, before the ex post recovery (or lack of) is observed. The difference in downgrade rates is -1.2% per quarter, which is approximately 20% of the downgrade rate for the less transitory shock firms.

Panel B examines sorts on the Amihud liquidity measure. Both the low and high Amihud groups have event quarter returns of approximately -18%. Abnormal returns over the next year validate the use of Amihud groups in our research design: average returns of the more transitory group (the most illiquid firms) are +2.7%, while those of the Less Transitory group are -1.3% over the next year. Thus, the more transitory group subsequently outperforms the less transitory group by approximately 4% over the next year, which is about a quarter of the original event quarter shock. CRAs downgrade the more transitory group less frequently, with a difference in downgrade rates of -1.4% between the two groups. This difference is also almost a third of the mean downgrade rate for the less transitory shock group.

2. Mutual fund fire sales: Test setup

The tests in the previous section have the virtue of simplicity. However, they have some drawbacks. The perfect foresight shock is somewhat mechanical in construction and does not have an economic channel that links the reversal to the original shock. Thus, it is open to the critique that the reversal or continuation is the result of the actions of CRAs rather than vice-versa. The Amihud ratio based split is better in this regard. It has an underlying economic channel and does not condition on future information. But, the extent of reversals with this measure is relatively small. Also, less liquid firms could be different from the more liquid firms in other dimensions correlated with downgrade rates.

To address these concerns, we turn to transitory shocks to equity prices resulting from fire sales by mutual funds as a measure of transitory shocks to perceived credit risk. The fire-sale approach is motivated by the observation that while mild fund outflows can be absorbed by a fund's cash position, extreme outflows are more likely to force managers to liquidate stocks, thereby generating price pressure on these stocks. Coval and Stafford (2007) show that stocks subject to fire sales suffer a substantial transitory decline in prices, which recover after several quarters.

A potential concern with using actual fire sales to measure transitory shocks is that mutual fund managers choose which stocks to sell in response to redemption pressure. Hence, we use the approach in Edmans, Goldstein, and Jiang (2012, EGJ) that uses trades implied by a fund's portfolio weights and outflows rather than actual trades. Using implied weights can be interpreted as a 'Bartik-like' instrument (see Goldsmith-Pinkham et al., 2018), in which identification comes from the pre-event quarter weights and instrument relevance from potentially endogenous flows during the event quarter.

A causal interpretation of our results requires that fire-sales are independent from the actions of CRAs. The argument for such independence is similar to the argument that Edmans, Goldstein, and Jiang (2012) make for fire sales and takeover likelihood: decisions by investors to buy or sell a particular mutual fund are unlikely to be due to information about changes in credit ratings of specific stocks within the fund. Investors with such information are more likely to trade on the individual stock or bond rather than the fund. We also exclude sector funds, as in Edmans, Goldstein, and Jiang (2012), to eliminate flows which may be due to specific information about the industry as a whole.

2.1. Measuring fire sales

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

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

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:

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

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.

We also construct the Lou (2012) measure:

$$FIT_{j,t} = \frac{\sum_i shares_{i,j,t-1} * flow_{i,t} * PSF_{i,t-1}}{\sum_i shares_{i,j,t-1}}. \tag{4}$$

where *PSF* is the partial scaling factor estimated using regressions of percentage changes in shares held of stock *i* by fund *j*, on fund *j*'s flows and interactions of flows with portfolio-level liquidity and ownership as in specifications in columns 3 and 7 of Table 2 in Lou (2012). $Flows$ is the capital flow to fund *i* during quarter *t* expressed as a percentage of the funds lagged TNA, and *shares* is the number of shares held by fund *i* as of the end of the previous quarter. Additional details regarding the construction of both measures are in Appendix A.

There are a few differences between the two measures. First, the Lou measure includes both inflows and outflows, while the EGJ measure focuses only on outflows from funds that experience large outflows. Second, the EGJ measure scales the flow-induced trades by contemporaneous dollar volume. Scaling by dollar volume is consistent with the idea that a larger volume is more able to absorb a given amount of selling pressure without a significant price impact. Hence, it is possible that the EGJ measure may deliver deeper transitory shocks. We compute event quarter returns and the extent of reversals for both measures in the next subsection.

We convert both fire-sales measures into percentile ranks standardized to be between 0 and 1 in our main regression tests. There are situations in which we need a discrete measure of firms that experience substantial fire sales (e.g., the matched sample analysis). In such situations we define 'Treated' firm-quarters as those in which a firm's *MFFlow* is in the top decile of firms that quarter in our sample (firms with non-zero fire sales and existing credit ratings).

2.2. Addressing recent critiques of mutual fund fire sale measures

Recent work by Berger (2017) and Wardlaw (2018) critiques specific aspects of the fire sale methodology. Berger (2017) finds that firms that experience fire sales have different characteristics than those that do not. She finds that these differences in characteristics drive some of the earlier results in the literature. Our empirical tests address this critique using two approaches. First, we include controls for firm characteristics that predict the propensity for fire sales including firm size, mutual fund ownership, past returns, past rating changes and default probability from the model in Campbell, Hilscher, and Szilagyi (2008). Because it is possible that some of these variables may be related non-linearly to fire-sales and rating downgrades, we also conduct a matched-sample analysis. This analysis also allows us to mitigate concerns about unmodelled differences between treated and control firms, by only selecting firms as controls if they have be categorized as treated firms some time in the last three years (but not in the current or previous quarter) as recommended by Berger (2017). The matched sample approach is described in greater detail in Section 4.1.

Wardlaw (2018) argues that including contemporaneous dollar volume in the denominator of $MFFLow$ may cause it to be mechanically related to returns in the event quarter. Hence, he argues that some firms designated as fire-sale firms according to this measure may actually have experienced permanent shocks. This critique is not relevant our tests because we always control for returns (or match on them) in the event quarter. Moreover, any permanent shocks miscategorized as fire sales bias us against finding that CRAs can ignore transitory shocks. Finally, Lou (2012) also finds evidence that mutual fund sales result in transitory price pressure that eventually reverses. As described above, his measure of fire sales is not subject to this critique because it does not contain contemporaneous dollar volume. We therefore also examine the Lou (2012) measure and find similar results in robustness tests.

2.3. Properties of the fire-sale measures

Table 3 reports two important properties of the EGJ (Panel A) and Lou (Panel B) measures of fire sales. The table reports transition probabilities across fire sale quintiles over adjacent quarters as well as abnormal returns during and after the quarter in which fire sales are measured. Because we designate the largest decile as 'Treated' firms, the penultimate row of each panel also shows the fraction of firms in each EQ-1 quintile that transition to the top EQ decile. The transition probabilities show that both EGJ and Lou measures are fairly persistent. For example, 46% of the firms in the top EGJ quintile in a given quarter are also in the top quintile in the previous quarter. This pattern is even more pronounced for firms in the highest EGJ decile.

Only about 12% of these firms were in the bottom 40% of fire sales in the previous quarter.[10]

The table shows that the EGJ measure delivers a deeper shock for firms in the highest fire sale decile than the Lou measure. Event quarter abnormal returns (relative to the DGTW benchmarks) are -3.3% for the top EGJ decile and -1.38% for the top Lou decile. Also, we find that firms almost entirely recover these loses over the next 21 months for the EGJ measure, with 21 month abnormal returns of 2.72%. For the Lou measure returns more than recover, with 21 month returns of 2.43%.[11]

One potentially troubling pattern is that the recovery is not proportional to the EQ return shock—for example, both EGJ and Lou variables the 3rd quintile recovers by about 2% even though its EQ shock is close to 0. It is possible that the persistence in shocks documented above affects the recovery. Figure 2 investigates the interactions between persistence in fire sale shocks and the subsequent recovery. We regress recovery returns on dummy variables for whether firm i is in fire sale decile d in quarter $EQ+q$, where q goes from 1 to 7. The figure plots the residual from the regression along with the negative of the EQ return shock for each fire sale quintile. The figure shows that once we control for future fire sale shocks, the recovery matches up closely with the EQ return shock for both measures.

Overall, these results show that both the Lou and EGJ measures deliver transitory shocks to equity prices. However, perhaps because the EGJ measure focuses on funds with large outflows and also normalizes by volume it delivers a deeper shock than the Lou measure. Hence we use the EGJ measure for our primary tests and perform robustness checks using the Lou measure.

Figure 1 plots cumulative average abnormal returns (CAARs) in the year before before and two years after the event quarter for firms in the top decile of fire sales. Abnormal returns are measured relative to the CRSP equal-weighted index and also to DGTW characteristic-matched portfolios. Both measures of abnormal returns are significantly negative (-3% to -5%) during the event quarter. We observe a small pre-trend in returns, treated firm returns decline by about -1.5% over the prior year. Both measures of abnormal returns also show a substantial recovery after the fire sale quarter, with abnormal returns of about 3% to 5% over the two years after fire sales.[12] Panel B shows that the pre-trend is related to the persistence in shocks.

[10]Note that the fractions in a given row do not add up to 1 because there are some firms that do not have any fire sales in the previous quarter and hence are not in the sample.

[11]Lou (2012) also reports a greater recovery than the original shock for value-weighted returns. Our sample is tilted to large stocks because we require the existence of ratings.

[12]In unreported results, we find a larger recovery for the sample of firms with credit ratings relative to all firms, consistent with

The panel shows that the pre-trend disappears if we exclude firm-quarters that were also treated in the prior quarter. Panel C shows evidence that the transitory shock to equity prices is transmitted to CDS markets. CDS spreads increase in EQ and EQ+1 and then reverse, slowing decreasing to almost their original level over 2 years. Panel D shows a similar pattern for default probability based on the Campbell et al. (2008). We discuss these results in greater detail below.

3. Main results

This section reports full sample regression analysis that convey our main results. We contrast the CRA responses to fire sale pressure with that of the market price-based measures of creditworthiness and then examine how fire sale pressure associates with future bankruptcy filings.

3.1. Fire selling and CRA downgrades

Table 4 reports results of regressions of downgrades (specification 1 to 5) or notches downgraded (specification 6) on $Mfflow$ with controls for firm returns in the event quarter, rating and year-quarter fixed effects, and standard errors clustered by industry (using the SIC 3 digit classification). The sample is restricted to firms with non-zero $MFFlow$ to avoid an explanatory variable with a large mass of firms with zero values. This also reduces the possibility that our results are due to unmodelled heterogeneity related to firms that experience fire sales being fundamentally different from those that do not (because they are not owned by mutual fund with large outflows for example). The first four specifications restrict the sample to firm-quarters with negative returns only to focus on firm-quarters with negative shocks. Specifications 5 and 6 include all firm quarters.

Coefficients on $MFFlow$ are negative across all specifications: higher outflows are associated with lower downgrade probabilities or notches downgraded. The first specification only controls for EQ returns. The next two specifications add in additional firm-level characteristics as controls that are related to the propensity of a stock to experience mutual fund fire-selling pressure (these variables are part of our propensity score model in the matched-sample analysis). These include past rating upgrades or downgrades, past returns, log

the ability of CRAs to mitigate transitory shocks to prices. Because rated firms are different from unrated firms along several other dimensions, this is by no means conclusive evidence.

market capitalization, market leverage, mutual fund ownership, return volatility and the Amihud ratio. All of these variables are as of the start of the event quarter. The $MFFlow$ variable remains significant with similar magnitude as additional controls are added.

Specification 4 is the most saturated. It includes industry dummies as well as a dummy for the highest EGJ decile that a firm attains over the past three years. This controls for unmodelled heterogeneity between firms that is associated with fire-sale propensity. Thus, for example if a firm is of a type that never attains a high EGJ decile (for e.g., if it has low mutual fund ownership) and this type is for some reason less likely to be downgraded, this effect would be absorbed by the dummy. The specification also includes default probabilities from the Campbell, Hilscher, and Szilagyi (2008, CHS) model to control for any differences in credit quality in firms within a rating category. The specification also includes changes in CHS default probability during the event quarter to control for public news that could affect default probability arriving during the event quarter. Both the lagged level and contemporaneous changes in CHS default probability predict downgrades, however the coefficient on $MFFlow$ remains significant. The significance of the lagged variables in predicting downgrades is consistent with CRAs being slow to respond to news; however, this sluggishness can not explain our results. The coefficient on $MFFlow$ shows that firms with low fire-selling do get downgraded at greater rates in response to the same return shock.

The next specification includes all firm-quarter instead of just negative return event quarters, and the final one examines notches downgraded instead of a downgrade dummy with similar results. In terms of economic magnitude, a move from the 25th to the 75 percentile of fire sales translates into 2% decline in downgrade rates in a given quarter. With about a 1000 rated firms, this translates to about 80 fewer downgrades a year.

The next two panels examine rating downgrades over the next quarter ($EQ+1$), and over the two quarters starting with EQ ($EQ : EQ+1$). These regressions allow us to examine any effects that may occur in the next quarter if rating agencies take some time to respond to new information. Here, all that changes from Panel A is the dependent variable, the explanatory variable remain the same as before in all aspects including timing. These results are consistent with those in Panel A. CRAs appear to be slow to respond to information because lagged returns predict downgrades. However, CRAs also continue to downgrade firms with fire-selling pressure at a lower rate. Although the coefficient on $MFFlow$ is somewhat smaller, it does not reverse indicating that CRAs are not simply waiting longer to downgrade fire-sold firms. Panel C presents results in which the dependent variable is downgrades over the event and subsequent quarter. These results are consistent with those in Panels A and B.

3.2. Can markets see through fire-sale shocks?

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

3.2.1. CDS markets

Panel C in Figure 1 examines how CDS spreads respond to fire sales. The figure presents cumulative CDS spread changes over the two years around EQ. The figure computes the cross-sectional mean of the outcome variable in each quarter, followed by means in event time as in Coval and Stafford (2007). We see that CDS spreads rise for treated firms in EQ and $EQ+1$. CDS spreads appear to lag stock markets—about half the increase in spreads takes place during EQ+1 rather than 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.[13] Figure 1, Panel B also shows that increases in CDS spreads for treated firms are indeed transitory; spreads reverse back to their pre-EQ levels over the next two years.

Panel A of Table 5 examines the response of CDS markets to fire sales in a regression setting. The first three specifications examine changes over EQ and the next three examine changes over EQ and $EQ+1$.

[13] 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").

Consistent with the figure, we see in specification 1 that CDS spreads increase (statistically insignificantly) in response to fire sales. However, the next specification shows that this increase is due to the low returns that accompany fire sales. Once the EQ return is included as a control, the coefficient on $MFFlow$ becomes effectively zero and the EQ return is significant. Thus, CDS markets do not appear to distinguish between returns that are accompanied with fire sales and those that are not. The next specification examines if CRA actions help CDS markets discern transitory shocks, by including a dummy that is 1 if the firm is downgraded in EQ. This does not affect the coefficient on the fire sales variable, suggesting that the action (or the lack thereof) does not affect the CDS spread response to fire sales. Specifications 4 to 6 show that results are similar over EQ and $EQ+1$. Panel B examines downgrades implied from CDS prices instead of raw CDS spreads. These downgrades are similar to rating downgrades in that they are discrete binary variables and hence relatively immune to the effect of outliers. Results for CDS implied downgrades are similar to those for spreads in Panel A.

Requiring CDS data to be available reduces the sample to about one-fifth of the full sample, with 21,000 firm-quarters and increases average firm size. To ensure that the difference in results between CDS markets and CRAs are not driven by differences in samples, we redo the analysis in Table 4 on CRA downgrades for the subsample with available CDS data in Panel C. We find similar results as in Table 4. As before, a greater $MFFlow$ is associated with lower downgrade rates even in this smaller subsample.

Figure 1, Panel D also examines another market based measure, the model implied default probability from the CHS model. This model combines information from equity markets and accounting statements to predict defaults. Similar to CDS spreads, CHS model default probabilities also increase in the event quarter and then slowly reverse over the next few quarters. These results are confirmed in Panel D of Table 5.

Thus two important indicators of credit risk based on market prices do not distinguish between permanent and transitory shocks to equity prices. The question of whether market-based estimates of default risk *should* react to transitory equity price shocks is a thorny one. At one level, the market value of equity has just fallen, thereby increasing perceived leverage, so perhaps an increase in CDS spreads is warranted. But this increase is transitory and reverses over the next few quarters. It seems unlikely that spreads on five year CDS contracts would increase, if credit market participants expect the shock to reverse over the next two years.

3.2.2. *Realized defaults after fire sales*

Panel E in Table 5 provides evidence on whether the increase in credit spreads and predicted default probabilities is justified by realized future defaults. The first three specifications examine bankruptcies over 1 year, while the next three examine bankruptcies over 5 years after the fire sale quarter. Overall, the table is consistent with the motivation underlying our empirical design. Controlling for firm characteristics and default probabilities at the start of the event quarter, more negative returns in the event quarter predict a greater bankruptcy probability. The coefficient on $MFFlow$ is marginally insignificant in predicting bankruptcies over the next year, but significantly predicts lower bankruptcies over 5 years even after controlling for EQ returns and changes in CHS default probability. Thus, markets ignore relevant information when they do not take into account the effect of fire sales.

3.3. *The CRA information advantage channel*

Why do CRAs succeed in discerning that fire-sale shocks to market prices are transitory when markets fail? One possible explanation that does not violate semi-strong form market efficiency is that CRAs possess nonpublic information about a firm's prospects. As discussed in the introduction, CRAs claim that they routinely receive nonpublic information including budgets, internal capital allocation schedules, potential acquisitions, and restructurings in the process of rating a firm. Thus, after seeing a shock to prices, CRAs can seek information to verify whether fundamentals have indeed deteriorated before issuing a downgrade. Consistent with the existence of information advantage for CRAs, prior research finds that markets react to rating downgrades, and Jorion, Liu, and Shi (2005) show that this reaction increased after information advantage of CRAs increased with the enactment of Reg FD.

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

3.3.1. *Uncertainty in public information and fire sale downgrades*

For firms with a more uncertain information environment, it is likely that market participants will find it more difficult to tell if a given shock to prices is due to fire sales or changes in fundamentals. For such firms, we expect deeper fire-sale shocks and a greater information advantage of CRAs relative to markets.

We test if the treatment effect is greater for such firms. We use measures of uncertainty derived from analyst estimates: the cross-sectional dispersion in analyst forecasts and the time-series standard deviation in forecast errors over a rolling 3-year window before the event quarter.

Table 6 shows results for tests that regress downgrades on $MFFlow$ and interactions with uncertainty variables. All specifications includes time and rating fixed effects. The first specification confirms that our main result holds for the sample of firms with I/B/E/S analyst coverage. The coefficient on $Mfflow$ at -0.0457% is similar to that in Table 4. Specification 2 introduces a dummy variable that is 1 if the number of analysts covering the firm is less than the sample median (4 analysts). This specification shows that just increased coverage by itself does not change the effect of fires sales on downgrade rates. Specification 3 includes a dummy variable that equals 1 if the cross-sectional standard deviation of analyst Earnings Per Share (EPS) estimates is above the sample median. Firms with more analyst disagreement are more likely to be downgraded, consistent with such firms having more uncertain fundamentals and hence being riskier borrowers. The interaction between fire sales and disagreement is large in magnitude and significant, suggesting that information advantage of CRAs relative to markets is larger in firms with more analyst disagreement. The next specification shows that results are similar if the uncertainty dummy variable is based on the time-series standard deviation of median forecast errors instead of analyst disagreement. The final specification uses stock volatility as a measure of uncertainty with similar results.

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

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

To further identify the channel through which CRAs discern transitory shocks, we use Reg FD as an exogenous shock to the information environment of CRAs. In October 2000, the enactment of Reg FD prohibited firms from selectively disclosing material information to investors or market professionals such as equity analysts. However, disclosure to CRAs was exempt from its provisions. Thus, we expect that the information advantage of CRAs relative to the market increased after the enactment of Reg FD. Jorion, Liu, and Shi (2005) find evidence consistent with this hypothesis: stock price reactions to downgrades increased significantly after Reg FD. Hence, we use Reg FD as a shock to the information advantage of CRAs and test whether the ability of CRAs to discern transitory shocks to stock prices improved after it was enacted.

These results can be interpreted causally, if we assume that the enactment of Reg FD was exogenous and hence, the assignment of fire sales to firms did not change after Reg FD.

To minimize the effect of other potential changes, we restrict our sample in this test to a relatively short period around Reg FD. In particular, we use exactly the same sample period as Jorion et al. (2005)—9 quarters before and 9 quarters after October 2000. We also do not include $RegFD_t$ dummy variable by itself as it is subsumed by the time fixed effects.

The last specification in Table 6 reports results from this regression. Consistent with RegFD improving the information advantage of CRAs relative to markets, we find that CRAs ignore fire sales to greater extent just after RegFD. These results are stronger when we consider notches downgraded in Panel B. Because the Reg FD shock is a time-series shock it is also amenable for matched sample analysis (described below).

In October 2010, the Dodd Frank Act removed the explicit exemption for CRAs from Reg FD. We do not use this second shock as an additional test for two reasons. First, it is not clear whether this change has had any material effect on the access of CRAs to nonpublic information. CRAs argue the removal of the specific exemption does not affect their access to nonpublic information because they meet other criteria for exemption in Reg FD: they do not seek to make investment decisions based on the private information, and their engagement letters with firms contain confidentiality agreements (Carbone, 2010). Second, the Dodd Frank Act is not a clean shock to the information environment because it also made other changes to the legal environment of CRAs. These changes include increasing the legal liability for issuing inaccurate ratings, and making it easier for the SEC to impose sanctions against CRAs. Dimitrov, Palia, and Tang (2015) find that these changes affected the information content of ratings for equity and bond markets.

4. Robustness

In this section, we perform a series of tests that examine whether our results are robust to changes in our testing methodology and over time and rating levels. We consider two changes to our methodology. First, we use a matched-sample difference-in-difference approach rather than the regression setup. Second, we conduct a hazard rate analysis to examine whether CRAs use information in returns from within a quarter to determine which shocks are transitory. We also use this setup to test whether other rating actions of CRAs such as rating watches or outlook changes respond to fire sales. We also examine whether the effects are consistent over rating categories and time. Finally we consider if our results are robust to using the Lou measure of fire sales.

4.1. Matched sample analysis

In this section we employ a matched sample approach instead of regressions for our test. We test whether realized downgrade probabilities are different for treated firms relative to controls over the fire sale and subsequent quarters. This is a 'difference-in-difference' test in that it is the difference in the change in credit ratings between treated and matched firms over the event and subsequent quarter. This approach allows to control for any non-linear effects of our controls on fire sales and downgrades. We also are better able to control for unmodelled differences between fire sold and other firms, by requiring our matches to have been fire sold at some point in the past three years, but not in the current quarter as suggested by Berger (2017).

As discussed above, treated firms are in the top decile of fire sales that quarter. Control firms are chosen to have similar characteristics as treated firms at the start of, and similar returns during, the fire-sale quarter. In particular, our matching procedure consists of the following steps.

As of the beginning of EQ, we search for controls that have:

1. the same narrow credit rating,
2. the same Fama-French five industry classification,
3. a propensity to be a fire-sale stock within one standard deviation of that of the treated firm, and
4. below median fire selling pressure in EQ, but have been in the top fire selling quintile at some point in the past three years.

From these potential matches, we pick the firm with the minimal absolute distance in stock return from the treated firm in EQ, with the ratio of gross returns (1+returns) of treated to control firms being within 0.8 and 1.2. 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 '+','-'). We also require treated and control firms to both not have been classified as treated in EQ-1 to eliminate pre-trends due to treatment in the previous quarter.

The propensity score model for a stock to be a fire sale is a logit model that predicts whether a stock is treated using all the firm specific controls used in Table 4 and year quarter fixed effects (this is reported in Appendix Table A1). Our matching criteria are chosen to balance the need for a tight match and a large sample. We show below that this procedure results in treated and control firm samples that are similar across a variety of dimensions.

4.1.1. Covariate balance

Table 7 shows that our matching procedure achieves reasonable covariate balance. Because of our stringent matching criteria, we are able to find matches for 4,096 firm-quarters. Panel A shows that treated and control firms have similar means and standard deviations for all variables in the propensity score model. In particular, means for size, leverage, mutual fund ownership, and past returns are not different between treated and control firms in economic or statistical terms. The Amihud ratio is statistically higher for treated firms than for controls. However, the difference is economically small (about 0.006 or one-seventh of a standard deviation in the treatment sample). There appears to be no difference in the average change in credit rating between treated and control firms in the quarter before the event.

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

Consistent with our matching design, Panel B also shows that DGTW-adjusted event quarter returns for treated and control firms are similar in magnitude. If anything treated firms have lower returns than controls biasing us against finding that they are less likely to be downgraded. The table also shows that treated firm returns are transitory: returns almost entirely reverse over the next 21 months. However, control firm returns do not reverse at all, and are significantly different from treated firm returns over the next 21 months. The EQ return shock and subsequent recovery are relatively modest in absolute magnitude. This is the price we pay for using hypothetical instead of actual fire sales to mitigate selection biases that could arise from manager discretion, examining rated firms that are typically larger and less susceptible to fire sale pressure, and also for choosing stringent matching criteria that result in a well-balanced sample, but also increase the likelihood that firms with more extreme EQ returns remain unmatched.

4.1.2. Downgrade rates

Panels C and D presents the main results of this paper redone using the matching design. Panel C 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. The realized downgrade probability is the fraction of firms in the relevant sample (treated or control) that experience a downgrade over a given period. The third column presents the Average Treatment effect on Treated (henceforth, 'Treatment Effect'), or the mean difference in the outcome variable between treated and control firms. Over the three-month period EQ-1 (six-month period EQ-2 and EQ-1), treatment and control firms exhibit parallel trends with similar downgrade probabilities of 3% (5.7%) for treated firms and 3.1% (5.7%) for controls. During the event quarter, treated firms have a much lower downgrade probability (2.9%) than controls (3.9%). The difference of -0.95% is significant statistically (heteroskedasticity robust t-statistic of -2.8).[14] The treatment effect is present one quarter after EQ as well, with the difference in downgrade probabilities between treated (3.2%) and control firms (4.2%) of -1.03%. Overall, for the six month period starting with EQ, the difference between treated and control firms is -1.7% (t-statistic of -3.6), about a third of the pre-EQ downgrade rate for treated firms.

Although the average realized downgrade probability for treated firms during EQ is not zero, this does not necessarily show that CRAs have failed to identify some transitory shocks. Because treated firms are also subject to fundamental shocks, zero is not the relevant benchmark. The correct benchmark reflects the rate of arrival of fundamental shocks to a sample of firms that is similar to the treated sample and does not condition on contemporaneous returns. A simple estimate that meets this criteria is the downgrade probability for treated (or control) firms in the quarter prior to the event. We see that downgrade probabilities for treated firms are similar in EQ and EQ-1 (3.0% and 2.9%), suggesting that CRAs do ignore transitory price pressure.

Next, we incorporate the severity of rating downgrades in our analysis. Panel D reports results of tests that use the number of notches downgraded as the dependent variable. This variable is zero for upgrades or if there is no change in the credit rating, and equals the number of notches downgraded if there is a downgrade over the test period. These results are similar to those for realized downgrade probabilities considered in Panel C. Treatment and control samples again display parallel trends before the event quarter, with average

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

downgrade notches being insignificantly diffrent over the six moths prior to the fire-sale quarter. Over the following two quarters, treated firms have significantly lower average downgrade notches as compared with controls. This difference of 0.05 downgrade notches is large relative to the average downgrade notches of 0.132 over the six months before EQ for treated firms and almost five times as large as it is during the six months prior to EQ.

Finally, Panel E of Table 7 examines the effects of RegFD-adoption in the matched sample settings which is further constrained to just 9 quarters before and after the RegFD enactment in October 2000, as in Jorion et al. (2005). This limits our sample to just under 800 treated-quarters and 1,473 firm-quarters including matched controls. Nevertheless, we continue finding a statistically significant increase in the treatment effect during EQ—consistent with the unmatched sample regression reported in specification (6) of Table 6—despite additional controls for company characteristics and time fixed effects. The increase in the treatment effect attenuates by EQ+1 as follows from specifications (4)–(6) of Table 7.

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

4.2. Hazard model analysis

In this section, we examine three alternative explanations for our findings that CRAs distinguish between transitory and permanent shocks to prices:

1. CRAs learn which shocks are transitory by observing high frequency patterns in returns. For example, firms that begin to recover within the quarter are not downgraded.
2. Downgrades (or lack thereof) cause EQ returns and thus treated and control firm returns over the full quarter are not directly comparable.
3. Other ratings actions (e.g. Upgrades, Outlook issuance) attenuate the effects of the lack of downgrades in response to fire sales.

To test these alternatives, we estimate a logistic hazard model for CRA actions at the daily frequency for the matched sample of treated and control firms. The rating action on each day conditional on no action up to that day is the dependent variable and the treatment dummy along with firm-specific controls are independent variables. As is standard in hazard models, we drop the firm after the action occurs and limit the sample to the end of EQ+1 if the action does not occur.

Table 9 reports the results of this analysis. In all specifications, we include the treatment dummy and EQ- and pre-EQ covariates as in specification (4) of table 4. In odd specifications, the only additional control is the number of days elapsed since start of the quarter. In even specifications, we also include the return and squared return since the start of the quarter, as well as average returns and excess turnover over the past 10 days. The excess turnover is the turnover demeaned by the firm's average turnover over the past year.

In Panel A, we focus on negative rating actions—Downgrades, Negative Outlook, Negative CreditWatch. The negative coefficient on treated dummy is significant across specifications and is somewhat stronger for Outlook and CreditWatch actions. These results imply that the treatment effect is not subsumed by information contained in returns up to the point of the rating action. It is also not driven by returns after the rating action. Also, CRAs do not seem to "hedge their bets", for example by issuing watches for firms they are not downgrading in response to fire sales.

In Panel B, we focus on the corresponding positive rating actions. The coefficient on Treated is positive but small and insignificant for upgrades and positive outlook issuance, and negative but insignificant for negative credit watch issuance. This is consistent with two explanations: (i) CRAs are reluctant to "actively oppose" market prices, (ii) the positive fundamental shock arrival rate is the same in treated and control samples.

4.3. The effect across rating categories and over time

Figure 3 shows downgrade probabilities for treated and control firms by broad rating category. Realized downgrade probabilities continue to measure downgrades across narrow categories—we merely present results by broad rating categories (i.e., ignoring '+' and '-') in the figure to ensure sufficiently large samples within each rating category. Panels A and B show that in general across all firms, whether treated or control, downgrade probabilities follow a 'U'-shaped pattern. Downgrade probabilities decrease as credit risk increases from AA, reaching a minimum at BBB, and increase thereafter.[15]

Panel B shows that the treatment effect is present across rating categories. In particular, treated firms

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

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

We next examine time-variation in the treatment effect to test whether the results are driven by a few years, which might imply that they are due to specific events such as the financial crisis. Figure 4 shows downgrade probabilities for treated and control firms in the event quarter (Panel A) and in the event quarter and subsequent quarter (Panel B). We split the sample into pre and post the onset of the 2007–2009 financial crisis. Overall, although the treatment effect varies over time, the figure shows that our results are not driven by a few points in time. The treatment effect is highest in the original Reg FD period when CRAs had privileged access to information and lowest during the crisis. The effect is present, but appears smaller in the post-crisis, modified-Reg FD period.

4.4. Alternative proxy of fire sales

Finally, we examine the robustness of the main tests to using the fire-sale pressure based on the methodology in Lou (2012) that does not contain contemporaneous dollar volume. Table 10 shows that our main conclusions from the EGJ setup continue to hold with the Lou setup. Panel A examines the effect on CRA actions across different samples and outcome definitions. Panel B examines the response of CDS markets, while Panel C—the associations with CHS probability changes and realized defaults. There are two differences from our main tests. First, CRA downgrades are not significantly related to fire sales in the subsample with available CDS data. Second, CDS implied downgrades are now significantly increasing in fire-selling pressure.

5. Conclusion

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

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

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

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Table 1: Summary statistics

This table reports summary statistics for the data used in this study at the firm-quarter frequency. The sample includes all firms with Standard&Poor's issuer long-term credit rating and positive debt as of the quarter-end that have CRSP share code of 10, 11, or 12 over the 1990–2016 period. See Appendix B for variable definitions.

	Firm-Qtrs	mean	sd	p1	p25	p50	p75	p99
Return (Raw)	126,058	0.031	0.227	−0.578	−0.082	0.027	0.134	0.782
Return (DGTW)	116,343	0.001	0.184	−0.510	−0.092	−0.005	0.085	0.590
log(realized variance)	126,263	−7.672	1.132	−9.883	−8.452	−7.766	−6.997	−4.565
CAPM β	120,782	1.120	0.709	−0.113	0.638	1.025	1.466	3.389
log(Market Cap)	126,263	7.543	1.809	3.089	6.398	7.572	8.725	11.741
Book-to-Market	125,089	0.804	1.078	0.015	0.341	0.582	0.926	5.070
Debt-to-EV	126,263	0.374	0.233	0.014	0.185	0.338	0.528	0.946
Amihud ratio	126,255	0.021	0.049	3.071	0.000	0.002	0.010	0.234
Mutual fund ownership	123,968	0.154	0.113	0.001	0.060	0.131	0.232	0.451
Rating change	125,168	−0.074	1.138	−3.000	0.000	0.000	0.000	2.000
CHS default probability (%)	121,028	0.086	0.198	0.015	0.029	0.042	0.067	0.962
Number of Analyst estimates	109,760	8.032	6.295	1.000	3.000	6.000	11.000	29.000
Analyst Fforecast st.dev.	99,414	0.096	3.317	0.000	0.014	0.028	0.058	0.523
Analyst forecast error	106,145	0.223	4.492	0.007	0.032	0.067	0.139	1.651
5y CDS spread level	23,025	1.945	4.550	0.120	0.443	0.879	2.008	15.588
5y CDS spread change	23,563	0.020	1.199	−3.096	−0.126	−0.006	0.083	4.550
MFFlow rank	108,835	0.489	0.260	0.020	0.280	0.490	0.700	0.970
FIT rank	119,670	0.511	0.253	0.020	0.310	0.520	0.710	0.980

Table 2: Simple tests

This table reports downgrade rates for firms that receive substantial negative shocks—firms in the bottom quintile of DGTW adjusted returns in a given quarter and rating category—in an event quarter (EQ). We split this sample into two groups based on variables that measure how transitory the shock is likely to be. The 'perfect foresight' split sorts firms into two groups based on whether their DGTW adjusted returns over the next year are in the top ('More Transitory') or bottom ('Less Transitory') quntile. The Amihud ratio split sorts firms based on whether their Amihud illiquidity ratio in the quarter before the shock is in the top ('More Transitory', most illiquid) or bottom ('Less Transitory', most liquid) quintile. To ensure that the groups are matched by event quarter returns, for each firm in the more transitory group we find a firm in the less transitory with the same rating and the closest event quarter abnormal return. We exclude any returns with absolute differences of more than 10%.

	Less Transitory		More Transitory		More minus Less	
	Mean	N	Mean	N	Mean	p-value
Panel A: Perfect Foresight split						
Downgrade (EQ)	0.0629	4,306	0.0507	4,514	−0.0122	0.013
DGTW Return (EQ)	−0.1894	4,433	−0.1910	4,624	−0.0016	0.473
DGTW Return (EQ+1:EQ+4)	−0.4027	4,433	0.5146	4,624	0.9173	<0.001
Panel B: Amihud ratio split						
Downgrade (EQ)	0.0518	3,767	0.0378	3,939	−0.0139	0.003
DGTW Return (EQ)	−0.1753	3,807	−0.1776	3,998	−0.0023	0.276
DGTW Return (EQ+1:EQ+4)	−0.0131	3,801	0.0270	3,979	0.0401	0.001

Table 3: Fire selling pressure persistence and abnormal returns

This table reports transition probabilities between $EQ-1$ and EQ fire-selling quintiles, abnormal returns over EQ, and over the subsequent seven quarters. Abnormal returns are measured relative size, book-to-market, and momentum benchmarks as in Daniel et al. (1997) (DGTW). 'Low'['High'] denotes lowest [highest] fire selling pressure. The penultimate row in each panel also reports results for firms in the highest EQ decile. The last row reports summaries for the full sample. Panel A uses the Edmans et al. (2012) methodology (EGJ) to construct fire sale pressure, while Panel B uses the Lou (2012) methodology (Lou).

Panel A: The EGJ method

		Firm-Qtr Count	Fraction in EQ-1 MFFlow quintile					DGTW returns (%)	
			Min	2	3	4	Max	EQ	7Q after
EQ MFFlow quintile	Low	19,613	0.384	0.223	0.134	0.083	0.063	3.21	−0.71
	2	22,221	0.196	0.290	0.225	0.152	0.082	1.33	0.79
	3	22,802	0.108	0.218	0.276	0.232	0.124	−0.03	2.08
	4	22,928	0.074	0.141	0.228	0.295	0.219	−0.86	2.49
	High	22,801	0.057	0.083	0.121	0.217	0.457	−2.40	2.64
High EQ decile		11,287	0.054	0.065	0.089	0.163	0.547	−3.31	2.72
MFFlow sample		110,365	0.182	0.205	0.211	0.212	0.211	0.14	1.55

Panel B: The Lou method

		Firm-Qtr Count	Fraction in EQ-1 FIT quintile					DGTW returns (%)	
			Min	2	3	4	Max	EQ	7Q after
EQ FIT quintile	Low	23,122	0.468	0.223	0.119	0.083	0.086	1.31	−0.56
	2	24,918	0.218	0.330	0.234	0.131	0.076	0.49	1.44
	3	25,257	0.107	0.238	0.302	0.230	0.113	0.30	2.50
	4	24,908	0.077	0.133	0.240	0.329	0.211	−0.17	2.26
	High	22,949	0.082	0.081	0.121	0.238	0.456	−1.16	2.18
High EQ decile		10,951	0.094	0.074	0.099	0.176	0.528	−1.38	2.43
FIT sample		121,154	0.197	0.210	0.213	0.210	0.194	0.16	1.60

Table 4: Fire selling pressure and credit rating downgrades

This table reports OLS estimates of rating downgrades for the sample of firm-quarters described in Table 1. '*MFFlowpercentile*' is the percentile rank of fire-selling pressure as defined in section 2.1, 'EQ Stock Return' is the stock return during the fire sale Event Quarter (EQ). 'CHS dp' is the default probability estimate based on the model in Campbell et al. (2008). 'Past Fire Sale FE' includes fixed effects for the highest fire selling pressure decile that the firm has been over the past 3 years. Specifications (1)-(4) limit the sample to firm-quarters with *negative* DGTW-adjusted returns only, while (5) and (6) include all firm-quarters with non-missing covariates. Other variables are defined in Appendix B. In Panel A (B) [C], the dependent variable is the credit rating downgrade event during EQ ($EQ+1$) [EQ and $EQ+1$]. In specification (6), it is measured as a change in number of notches and as a binary variable elsewhere. T-statistics in parentheses are based on standard errors clustered by industry. */**/*** denote significance at the 10/5/1% confidence level.

Panel A: EQ downgrade probability / notches downgraded

	Downgrade (1/0)					# Notches
	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	−0.0467***	−0.0425***	−0.0407***	−0.0394***	−0.0343***	−0.1003***
	(−9.29)	(−8.25)	(−8.44)	(−8.28)	(−10.31)	(−8.23)
EQ Stock Return	−0.3317***	−0.2919***	−0.2690***	−0.1489***	−0.0383***	−0.0714**
	(−18.47)	(−18.07)	(−17.82)	(−11.19)	(−7.40)	(−2.58)
CHS dp (EQ change)				0.2402***	0.2633***	1.3910***
				(10.79)	(9.39)	(7.76)
CHS dp (EQ-1 level)				0.3123***	0.2842***	1.5745***
				(11.35)	(10.07)	(13.36)
log(Market Cap)		−0.0109***	−0.0083***	−0.0050***	−0.0051***	−0.0137***
		(−6.37)	(−5.83)	(−4.17)	(−5.22)	(−4.93)
Market Leverage		0.1357***	0.0899***	0.0673***	0.0506***	0.0525***
		(9.59)	(7.98)	(8.12)	(7.36)	(2.65)
MF Ownership		0.0232	0.0221	0.0181	0.0174**	0.0260
		(1.45)	(1.53)	(1.24)	(2.11)	(1.01)
log(Realized Variance)		0.0308***	0.0270***	0.0163***	0.0190***	0.0293***
		(11.08)	(10.85)	(7.17)	(11.80)	(4.88)
Amihud Ratio		0.0097	−0.0274	−0.2216***	−0.1911***	−0.7686***
		(0.18)	(−0.55)	(−4.23)	(−4.87)	(−4.16)
Return (q-2:q-1)			−0.0870***	−0.0475***	−0.0409***	−0.0627***
			(−18.00)	(−10.66)	(−12.62)	(−5.67)
Rating Downgrade last 6 months			0.0487***	0.0257***	0.0187***	0.0765***
			(7.76)	(4.35)	(4.55)	(4.59)
Rating Upgrade last 6 months			−0.0168***	−0.0175***	−0.0136***	−0.0275***
			(−7.65)	(−8.71)	(−10.70)	(−6.03)
Return (q-4:q-3)			−0.0477***	−0.0312***	−0.0306***	−0.0485***
			(−11.07)	(−7.32)	(−10.57)	(−3.42)
Rating Downgrade in (q-4:q-3)			0.0493***	0.0400***	0.0320***	0.0890***
			(7.42)	(6.46)	(7.31)	(5.73)
Rating Upgrade in (q-4:q-3)			−0.0089***	−0.0093***	−0.0116***	−0.0232***
			(−2.86)	(−2.96)	(−6.29)	(−4.27)
Rating & Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Past Fire Sale FE	No	No	No	Yes	Yes	Yes
Industry FE	No	No	No	Yes	Yes	Yes
Sample	EQret<0	EQret<0	EQret<0	EQret<0	Full	Full
Observations	53,672	52,548	51,895	49,677	101,339	101,339
R^2	0.043	0.075	0.100	0.127	0.093	0.140

Table 4: Fire selling pressure and Credit Rating Downgrades (Continued)

Panel B: EQ+1 downgrade probability / notches downgraded

	Downgrade (1/0)					# Notches
	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	−0.0399***	−0.0345***	−0.0323***	−0.0263***	−0.0256***	−0.0843***
	(−7.77)	(−6.30)	(−6.16)	(−5.10)	(−7.78)	(−5.36)
EQ Stock Return	−0.3097***	−0.2765***	−0.2541***	−0.1804***	−0.0817***	−0.2205***
	(−19.04)	(−19.02)	(−18.64)	(−11.46)	(−15.27)	(−5.98)
CHS dp (EQ change)				0.1521***	0.1982***	1.8061***
				(5.87)	(11.00)	(9.74)
CHS dp (EQ-1 level)				0.1915***	0.2239***	1.8416***
				(8.59)	(11.95)	(11.24)
Rating & Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Past Fire Sale FE	No	No	No	Yes	Yes	Yes
Industry FE	No	No	No	Yes	Yes	Yes
Past returns	No	No	Yes	Yes	Yes	Yes
Past rating changes	No	No	Yes	Yes	Yes	Yes
Firm characteristics	No	Yes	Yes	Yes	Yes	Yes
Sample	EQret<0	EQret<0	EQret<0	EQret<0	Full	Full
Observations	53,225	52,108	51,470	49,595	101,245	101,245
R^2	0.032	0.057	0.078	0.089	0.073	0.113

Panel C: EQ:EQ+1 downgrade probability / notches downgraded

	Downgrade (1/0)					# Notches
	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	−0.0743***	−0.0654***	−0.0618***	−0.0573***	−0.0539***	−0.1828***
	(−10.49)	(−9.18)	(−9.43)	(−8.60)	(−10.85)	(−8.20)
EQ Stock Return	−0.5193***	−0.4553***	−0.4151***	−0.2815***	−0.1062***	−0.2857***
	(−21.87)	(−22.91)	(−23.01)	(−15.94)	(−15.13)	(−5.98)
CHS dp (EQ change)				0.2750***	0.3345***	3.0028***
				(11.53)	(12.22)	(12.54)
CHS dp (EQ-1 level)				0.3158***	0.3440***	3.0942***
				(10.20)	(10.10)	(15.07)
Rating & Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Past Fire Sale FE	No	No	No	Yes	Yes	Yes
Industry FE	No	No	No	Yes	Yes	Yes
Past returns	No	No	Yes	Yes	Yes	Yes
Past rating changes	No	No	Yes	Yes	Yes	Yes
Firm characteristics	No	Yes	Yes	Yes	Yes	Yes
Sample	EQret<0	EQret<0	EQret<0	EQret<0	Full	Full
Observations	53,672	52,548	51,895	49,677	101,339	101,339
R^2	0.057	0.104	0.142	0.160	0.127	0.208

Table 5: Market-based downgrades and Realized defaults

This table compares the effect of mutual fund fire sales on credit ratings and market-based measures of credit quality in the sample with negative DGJW-adjusted returns. Specifically, the first 3 panels contrast the response of CDS spreads (Panel A) and CDS-implied rating downgrades (Panel B) with CRA actions (Panel C) over the sample of firms with available CDS data. Panel D examines the effects on CHS default probability changes during EQ, while Panel E presents realized defaults (as measured by bankruptcy filing). The suppressed firm-level control variables are as in specification (4) of table 4. Specifications (1) through (3) examine outcomes over EQ, while specifications (4) through (6) are over $EQ+1$ for Panels A throug D. T-statistics in parentheses are based on standard errors clustered by industry. */**/*** denote significance at the 10/5/1% confidence level.

Panel A: CDS spread changes

	EQ			EQ:EQ+1		
	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	0.0869	−0.0002	0.0334	0.1247	0.0055	0.0188
	(1.34)	(−0.00)	(0.51)	(1.38)	(0.06)	(0.21)
EQ Stock Return		−2.1385***	−2.0791***		−2.7193***	−2.6987***
		(−7.23)	(−6.96)		(−7.71)	(−7.64)
EQ CRA DownGrade			0.1988***			0.0722
			(3.42)			(1.20)
CHS dp level & change	No	No	Yes	No	No	Yes
Controls in each Spec.	Past returns and Rating changes, Firm characteristics, Baseline FE					
Observations	10,537	10,454	10,434	10,537	10,454	10,434
R^2	0.061	0.161	0.167	0.049	0.141	0.141

Panel B: CDS implied downgrades

	EQ			EQ:EQ+1		
	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	0.0068	0.0050	0.0046	0.0124	0.0105	0.0097
	(0.61)	(0.45)	(0.41)	(0.79)	(0.65)	(0.60)
EQ Stock Return		−0.0716***	−0.0723***		−0.0845***	−0.0860***
		(−3.30)	(−3.34)		(−2.80)	(−2.84)
EQ CRA DownGrade			−0.0019			−0.0039
			(−1.04)			(−1.62)
CHS dp level & change	No	No	Yes	No	No	Yes
Controls in each Spec.	Past returns and Rating changes, Firm characteristics, Baseline FE					
Observations	10,701	10,600	10,580	10,701	10,600	10,580
R^2	0.012	0.014	0.014	0.018	0.021	0.021

Panel C: CRA downgrades—CDS sample

	EQ			EQ:EQ+1		
	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	−0.0684***	−0.0748***	−0.0678***	−0.1014***	−0.1114***	−0.1020***
	(−5.45)	(−5.75)	(−5.48)	(−6.03)	(−6.47)	(−5.94)
EQ Stock Return		−0.1755***	−0.1401***		−0.3612***	−0.3120***
		(−5.14)	(−4.00)		(−9.24)	(−7.70)
EQ CDS Spread Chge			0.0158***			0.0239***
			(3.75)			(6.45)
CHS dp level & change	No	No	Yes	No	No	Yes
Controls in each Spec.	Past returns and Rating changes, Firm characteristics, Baseline FE					
Observations	10,696	10,595	10,434	10,696	10,595	10,434
R^2	0.091	0.138	0.136	0.136	0.172	0.175

Table 5: Market-based downgrades and Realized defaults (*Continued*)

Panel D: CHS default probability changes

	EQ			EQ:EQ+1		
	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	0.0032	−0.0031	0.0004	−0.0000	−0.0056	−0.0048
	(0.97)	(−1.16)	(0.14)	(−0.00)	(−0.74)	(−0.67)
EQ Stock Return		−0.4277***	−0.3946***		−0.3909***	−0.3750***
		(−21.19)	(−21.79)		(−11.10)	(−10.98)
EQ CRA DownGrade			0.0265***			0.0216***
			(9.28)			(5.66)
CHS dp (EQ-1 level)			−0.0602			−0.2883***
			(−1.35)			(−3.69)
Controls in each specification	Past returns and Rating changes, Firm characteristics, Baseline FE					
Observations	51,130	51,130	50,142	51,447	51,447	50,198
R^2	0.061	0.224	0.265	0.047	0.085	0.104

Panel E: Actual default realizations

	Bankruptcy in 1 year			Bankruptcy in 5 years		
	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	−0.0090***		−0.0032	−0.0230***		−0.0185***
	(−3.96)		(−1.49)	(−3.59)		(−3.03)
EQ Stock Return		−0.0309***	−0.0242***		−0.1444***	−0.1377***
		(−3.87)	(−2.96)		(−9.83)	(−9.38)
CHS dp (EQ change)		0.1435***	0.1308***		0.1440***	0.1400***
		(10.97)	(7.66)		(9.92)	(7.25)
CHS dp (EQ-1 level)		0.2028***	0.1925***		0.2319***	0.2404***
		(9.38)	(6.47)		(8.74)	(6.61)
Controls in each specification	Past returns and Rating changes, Firm characteristics, Baseline FE					
Observations	50,119	53,147	48,692	48,085	51,118	46,685
R^2	0.104	0.194	0.170	0.126	0.166	0.149

Table 6: Fire sales and CRA downgrades: channels

This table examines how the effect of mutual fund fire sales on rating downgrade probabilities varies with the information environment of firms by examining interactions between fire sales and proxies for analyst coverage and forecast uncertainty (specifications 1 through 5), and by examining the changes in the relative informational advantage of CRAs after the adoption of Regulation Fair Disclosure (Reg FD, specification 6). The sample in specifications in trhough 5 is all firm-quarters with credit rating and analyst coverage, and 9 quarters before and after the 'Reg FD' as in Jorion et al. (2005) in specification 6. In Panel A [B], the dependent variable equals 1 [number of rating notches change] if the firm was downgraded during the current quarter and zero otherwise. T-statistics in parentheses are based on standard errors clustered by industry. */**/*** denote significance at the 10/5/1% confidence level.

Panel A: EQ Downgrade Probability

	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	−0.0457***	−0.0494***	−0.0264***	−0.0340***	−0.0342***	−0.0379***
	(−10.72)	(−8.62)	(−5.88)	(−6.47)	(−8.84)	(−4.28)
Low Coverage=1		−0.0190***				
		(−4.28)				
Low Coverage=1 × MFFlow		0.0110				
		(1.55)				
AF Disagreement=1			0.0301***			
			(9.26)			
AF Disagreement=1 × MFFlow			−0.0296***			
			(−5.42)			
AF StdError=1				0.0166***		
				(4.16)		
AF StdError=1 × MFFlow				−0.0203***		
				(−3.68)		
Idiosync. Volatility=1					0.0506***	
					(7.93)	
Idiosync. Volatility=1 × MFFlow					−0.0452***	
					(−4.56)	
postFD=1 × MFFlow						−0.0330*
						(−1.74)
EQ Stock Return	−0.1002***	−0.1025***	−0.0666***	−0.0698***	−0.0691***	−0.0995***
	(−13.19)	(−12.34)	(−8.95)	(−7.55)	(−9.74)	(−8.29)
Interaction with EQ returns	No	Yes	Yes	Yes	Yes	Yes
Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Baseline FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	89,793	89,793	89,793	88,218	89,793	21,512
R^2	0.047	0.048	0.049	0.048	0.051	0.069

Table 6: Fire sales and CRA Downgrades—Channels (*Continued*)

Panel B: Downgrade Notches

	(1)	(2)	(3)	(4)	(5)	(6)
MFFlow percentile	−0.1532***	−0.1503***	−0.0802***	−0.1028***	−0.0905***	−0.1718***
	(−8.76)	(−7.18)	(−5.18)	(−5.37)	(−6.99)	(−3.56)
Low Coverage=1		−0.0343**				
		(−1.98)				
Low Coverage=1 × MFFlow		0.0006				
		(0.03)				
AF Disagreement=1			0.1019***			
			(8.15)			
AF Disagreement=1 × MFFlow			−0.1148***			
			(−5.80)			
AF StdError=1				0.0634***		
				(4.01)		
AF StdError=1 × MFFlow				−0.0896***		
				(−4.20)		
Idiosync. Volatility=1					0.2169***	
					(8.42)	
Idiosync. Volatility=1 × MFFlow					−0.2717***	
					(−6.49)	
postFD=1 × MFFlow						−0.2019**
						(−1.98)
EQ Stock Return	−0.4234***	−0.4169***	−0.2573***	−0.2930***	−0.2418***	−0.4229***
	(−11.22)	(−11.07)	(−6.68)	(−6.30)	(−8.25)	(−4.38)
Interaction with EQ returns	No	Yes	Yes	Yes	Yes	Yes
Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Baseline FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	89,793	89,793	89,793	88,218	89,793	21,512
R^2	0.050	0.050	0.052	0.052	0.056	0.070

Table 7: Matched Sample Analysis

This table presents means and standard deviations of selected variables for fire-sale ('treated') stocks and controls. A 'treated' firm experiences a fire sale in a given event quarter (EQ). Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 2 for details. The fewer number of control relative to treatment firm-quarters indicates that some controls are matched to multiple treated firms. Panel A reports variables used in the propensity score model (Table A1A) while Panel B examines other variables of interest. Panels C and D show downgrades before and after the EQ— the difference in the outcome variable between treated and control firms ('Average Treatment effect on Treated' in column 3); a negative number indicates lower mean outcomes for treated firms relative to controls. In Panel C, the outcome variable equals 1 if the credit rating was downgraded during the period and zero otherwise. In Panel D, we take into account the severity of downgrades by reporting the average number of notches downgraded ($E[\#NotchesDown]$). Standard errors are robust to heteroskedasticity as in Abadie and Imbens (2006) in both panels. Finally, Panel F examines the effect of the adoption of Regulation Fair Disclosure (Reg FD) on the matched sample during 9 quarters before and after the 'Reg FD' as in Jorion et al. (2005) by regressing the EQ (or $EQ : EQ+1$) CRA downgrade indicator on the treatment indicator and its interaction with 'PostRegFD' while controlling for firm characteristics and time fixed effects.

Panel A: Covariate balance—featured in Propensity score

	N(Treated)=4,096, N(Control)=3,313				
	Means			St.Deviations	
	Treated	Control	P-value	Treated	Control
	(1)	(2)	(3)	(4)	(5)
MCap(USD bln)	3.421	3.377	0.837	8.600	9.746
Debt-to-EV	0.347	0.350	0.825	0.216	0.222
Mutual Fund Ownership	0.182	0.168	0.016	0.119	0.109
Realized Volty past 12 month	0.067	0.068	0.239	0.038	0.041
Amihud Ratio	0.020	0.014	0.001	0.040	0.032
Rating Change past 6 months	-0.056	-0.078	0.457	0.768	0.780

Panel B: Covariate balance—Other characteristics and Trends

		Means			St.Deviations	
		Treated	Control	P-value	Treated	Control
		(1)	(2)	(3)	(4)	(5)
pre EQ	CAPM β	1.003	1.067	0.006	0.638	0.675
	Book-to-Market	0.788	0.776	0.739	0.956	0.970
	Raw return past 6 months	0.078	0.080	0.844	0.267	0.305
	DGTW return past 6 months	0.011	0.010	0.777	0.211	0.242
	CHS default prob.	0.061	0.061	0.838	0.074	0.081
EQ	Raw return	0.001	0.009	0.039	0.166	0.170
	Excess return vs Mkt	-0.025	-0.017	0.012	0.141	0.146
	Excess return vs DGTW	-0.023	-0.018	0.136	0.132	0.135
	CHS default prob.	0.071	0.068	0.228	0.142	0.119
post EQ	21-month return vs Mkt	0.066	0.040	0.129	0.543	0.539
	21-month Return vs DGTW	0.023	-0.004	0.030	0.434	0.442
	CHS Default Prob. 21-mo after	0.069	0.077	0.042	0.107	0.131
	Bankruptcy w/n 1 year	0.001	0.003	0.162	0.038	0.052
	Bankruptcy w/n 5 years	0.025	0.031	0.116	0.157	0.174

Table 8: Matched Sample Analysis (*Continued*)

Panel C: $Pr\{Downgrade\}$

	N(Treated) = 4,096				
	Treated	Control	ATT	SE	t-statistic
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.057	0.057	−0.0002	0.0042	−0.06
EQ-1	0.030	0.031	−0.0002	0.0031	−0.08
Event Quarter	0.029	0.039	−0.0095	0.0034	−2.79
EQ+1	0.032	0.042	−0.0103	0.0036	−2.89
EQ and EQ+1	0.059	0.075	−0.0166	0.0046	−3.58

Panel D: $E[\#NotchesDown]$

	N(Treated) = 4,096				
	Treated	Control	ATT	SE	t-statistic
	(1)	(2)	(3)	(4)	(5)
EQ-2 and EQ-1	0.132	0.143	−0.0110	0.0109	−1.01
EQ-1	0.070	0.074	−0.0044	0.0073	−0.60
Event Quarter	0.067	0.096	−0.0291	0.0091	−3.20
EQ+1	0.087	0.111	−0.0244	0.0169	−1.44
EQ and EQ+1	0.151	0.201	−0.0498	0.0200	−2.49

Panel E: 'RegFD' adoption effects

	EQ			EQ and EQ+1		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	−0.016*	−0.018*	0.001	−0.026*	−0.035**	−0.018
	(−1.74)	(−1.97)	(0.11)	(−1.74)	(−2.47)	(−0.86)
Treated × PostRegFD			−0.042**			−0.036
			(−2.72)			(−1.44)
Industry & Rating FE	No	Yes	Yes	Yes	Yes	Yes
Firm characteristics	Yes	Yes	Yes	No	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,473	1,473	1,473	1,473	1,473	1,473
R^2 / R^2	0.028	0.045	0.048	0.029	0.070	0.071

Table 9: Rating actions hazard model

This table presents a discrete time hazard model analysis of CRA actions at he daily frequency during EQ and $EQ+1$. A 'treated' firm experiences a fire sale in a given event quarter (EQ). Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and cumulative return over EQ as in Table 7. The outcome variables in Panel A [B] are based on either rating downgrade [upgrade], or negative [positive] 'Outlook', or 'Credit Watch' negative [positive] issuance. The sample is all firm days in $EQ : EQ+1$, and is right-censored—i.e. observations after the event occurs (including the day of the event if announcement is made during the business hours) are dropped. Standard errors are clustered at EQ-level.

Panel A: Negative Rating Actions

	Downgrade		Outlook		CreditWatch	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated (d)	−0.1830*	−0.2040**	−0.2332**	−0.2410**	−0.3764***	−0.3622***
	(−1.92)	(−2.20)	(−2.47)	(−2.44)	(−4.14)	(−3.92)
Days since EQ start	0.0017*	−0.0010	−0.0004	−0.0037***	0.0007	−0.0034**
	(1.89)	(−0.74)	(−0.45)	(−2.75)	(0.65)	(−2.13)
Return since EQ start		−1.5415***		−1.2579***		−0.8687**
		(−4.73)		(−4.06)		(−2.49)
(Return since EQ start)^2		0.1502**		0.1779***		0.2628***
		(2.31)		(2.89)		(3.28)
Last 10-day return		−1.0938***		−0.9626***		−0.0488
		(−2.97)		(−3.74)		(−0.09)
Last 10-day turnover		−0.0217		−0.0256		0.0242
		(−0.21)		(−0.20)		(0.30)
Pre-EQ returns and rating changes	Yes	Yes	Yes	Yes	Yes	Yes
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Rating and industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	864,042	863,506	859,179	858,643	860,819	860,289
Pseudo R^2	0.054	0.071	0.046	0.060	0.035	0.042

Panel B: Positive Rating Actions

	Upgrade		Outlook		CreditWatch	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated (d)	0.0395	0.0546	0.0577	0.0686	−0.1197	−0.0724
	(0.31)	(0.43)	(0.39)	(0.47)	(−0.69)	(−0.42)
Days since EQ start	0.0007	−0.0004	−0.0009	−0.0020	0.0026	−0.0001
	(0.65)	(−0.27)	(−0.62)	(−0.96)	(1.28)	(−0.05)
Return since EQ start		1.0320***		1.0105**		0.5557
		(2.74)		(2.38)		(1.64)
(Return since EQ start)^2		0.0356		0.0509		0.1211
		(0.53)		(0.63)		(0.88)
Last 10-day return		0.3440		−0.8499		1.0434***
		(0.60)		(−1.14)		(2.95)
Last 10-day turnover		−0.1530*		−0.0589		0.1666
		(−1.84)		(−0.27)		(1.37)
Pre-EQ returns and rating changes	Yes	Yes	Yes	Yes	Yes	Yes
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Rating and industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	859,562	859,045	860,293	859,771	828,708	828,216
Pseudo R^2	0.051	0.054	0.041	0.042	0.034	0.043

Table 10: Robustness to alternative definition of fire sales

This table examines robustness of the main results to using alternative definitions of fire sale pressure as in Lou (2012). Panel A examines the effect on CRA actions across different samples and outcome definitions. Panel B examines the response of CDS markets, while Panel C examines CHS default probability changes as well as realized defaults. Specifications 1 and 4 in Panel A contain all firm-quarters, specification (2) and (5) contain only EQs with negative returns, and 3 is restircted to the CDS subsample. Panels B and C also restrict the sample not negative EQ returns. The firm-level control variables are as in specification (4) of table 4. T-statistics in parentheses are based on standard errors clustered by industry. */**/*** denote significance at the 10/5/1% confidence level.

Panel A: CRA actions

	Pr{Downgrade}			E[NotchesDown]		
	(1)	(2)	(3)	(4)	(5)	(6)
FIT percentile	−0.0096***	−0.0172***	−0.0146	−0.0344***	−0.0589***	−0.0192
	(−3.34)	(−3.59)	(−0.99)	(−2.72)	(−2.77)	(−0.50)
EQ Stock Return	−0.0359***	−0.1490***	−0.1704***	−0.0715***	−0.4140***	−0.2818***
	(−7.88)	(−11.45)	(−4.99)	(−2.67)	(−5.04)	(−2.75)
CHS dp (EQ change)	0.2393***	0.2248***	0.3178***	1.3915***	1.4429***	1.4431***
	(10.59)	(11.93)	(5.69)	(8.43)	(8.50)	(7.57)
CHS dp (EQ-1 level)	0.2680***	0.2800***	0.4458***	1.6388***	1.7680***	1.4582***
	(12.39)	(12.52)	(5.63)	(15.10)	(9.67)	(7.16)
Controls in each Spec.	Past returns and Rating changes, Firm characteristics, Baseline FE					
Observations	110,481	53,964	10,653	110,481	53,964	10,653
R^2	0.095	0.128	0.131	0.143	0.175	0.178
Sample	Full	negEQ	CDSnegEQ	Full	negEQ	CDSnegEQ

Panel B: CDS markets

	CDS spread Δ			CDS-impl. Rating Downgrade		
	(1)	(2)	(3)	(4)	(5)	(6)
FIT percentile	−0.3900	−5.3456	−4.1234	0.0224***	0.0197**	0.0200**
	(−0.05)	(−1.10)	(−0.86)	(2.68)	(2.33)	(2.33)
EQ Stock Return		−240.5356***	−174.3056***		−0.0412***	−0.0485***
		(−12.40)	(−11.03)		(−3.78)	(−4.25)
CHS dp (EQ change)			302.1989***			−0.0207
			(4.03)			(−1.56)
CHS dp (EQ-1 level)			150.1889**			−0.0502***
			(2.39)			(−3.48)
Controls in each specification	Past returns and Rating changes, Firm characteristics, Baseline FE					
Observations	10,758	22,228	22,038	22,228	22,644	22,038
R^2	0.057	0.130	0.168	0.007	0.008	0.009

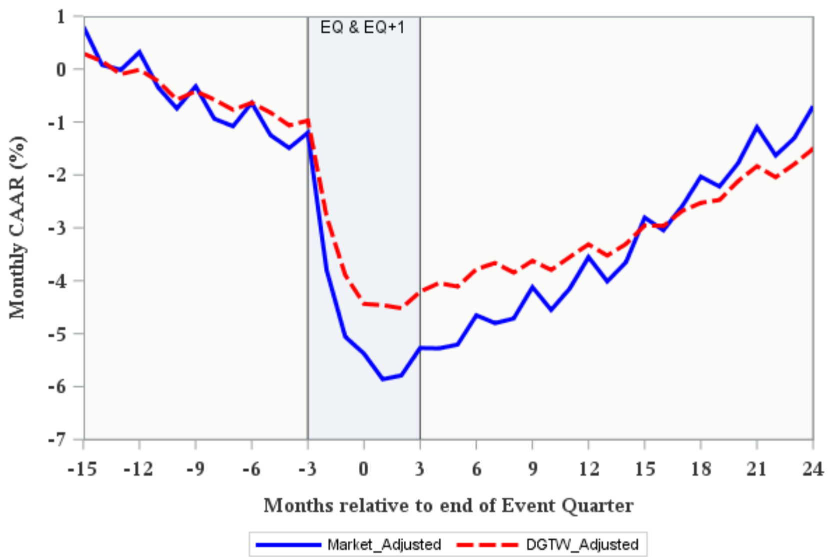
Panel C: Change in CHS Default probability and bankruptcy filings

	CHS dp Δ			Bankruptcy in 1 year		
	(1)	(2)	(3)	(4)	(5)	(6)
FIT percentile	0.0051*	−0.0064**	−0.0078**	−0.0050*	−0.0081***	−0.0066**
	(1.72)	(−2.21)	(−2.52)	(−1.74)	(−2.75)	(−2.34)
EQ Stock Return		−0.4732***	−0.4748***		−0.1243***	−0.1089***
		(−23.36)	(−23.16)		(−9.86)	(−10.00)
CHS dp (EQ-1 level)			−0.0336			0.2038***
			(−0.95)			(8.82)
Controls in each specification	Past returns and Rating changes, Firm characteristics, Baseline FE					
Observations	55,836	55,836	55,094	54,932	54,932	53,742
R^2	0.061	0.225	0.236	0.104	0.126	0.174

Figure 1: Mutual fund stock fire sales and market prices

This figure plots changes in market-based variables related to credit risk for firms that experience substantial fire-sales ('treated firms') as per Edmans et al. (2012) methodology. A treated firm is in the top decile of mutual fund fire-sales in an event quarter (EQ). Panels A and B plot Cumulative Average Abnormal Returns (CAAR) relatively to the market return with a solid line and the characteristics-matched portfolio as in Daniel et al. (1997) (DGTW) with a dashed line. While Panel A plots calendar time weighted returns for all treated firms, Panel B restricts the sample to treated firm were were not also treated in the quarter before the EQ. Panel C plots 5-year Credit Default Swap (CDS) spreads. Panel D plots cumulative changes in default probability based on the model in Campbell et al. (2008) (CHS) that combines equity prices and accounting data relevant for default prediction. In all panels, the sample is limited to the firms with credit rating (see Table 1). In addition, the sample is limited to firm-quarters with CDS data in Panel C, and to firm-quarters with all the accounting and market information required to compute the CHS measure in Panel D.

Panel A: Stock Returns—full rated sample



Panel B: Stock Returns—excluding back-to-back

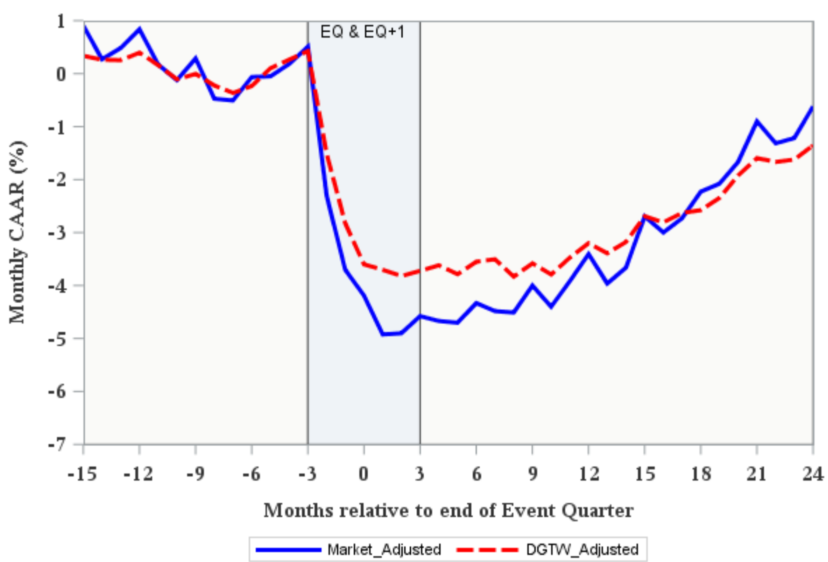


Figure 1: Mutual fund stock fire sales and market prices (*Continued*)

Panel C: CDS spreads

Panel D: CHS default probability

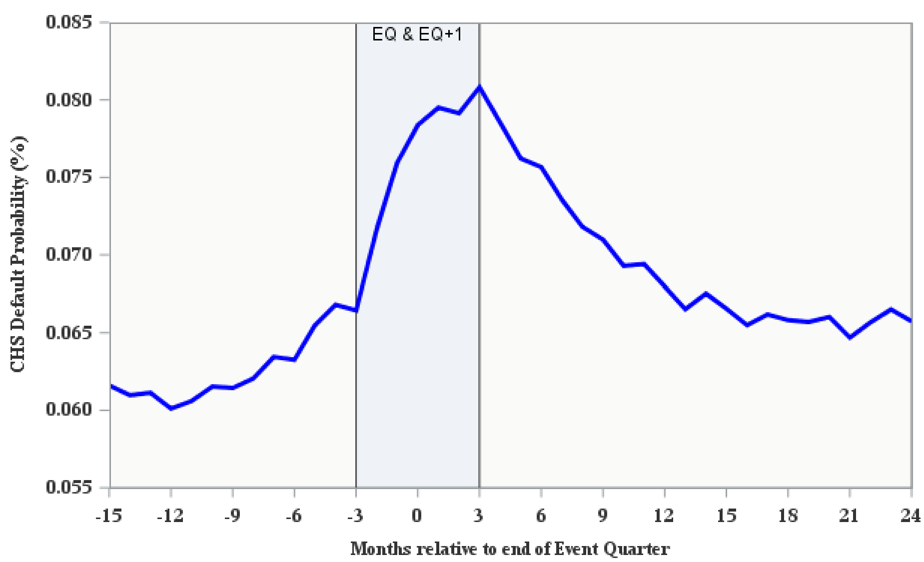
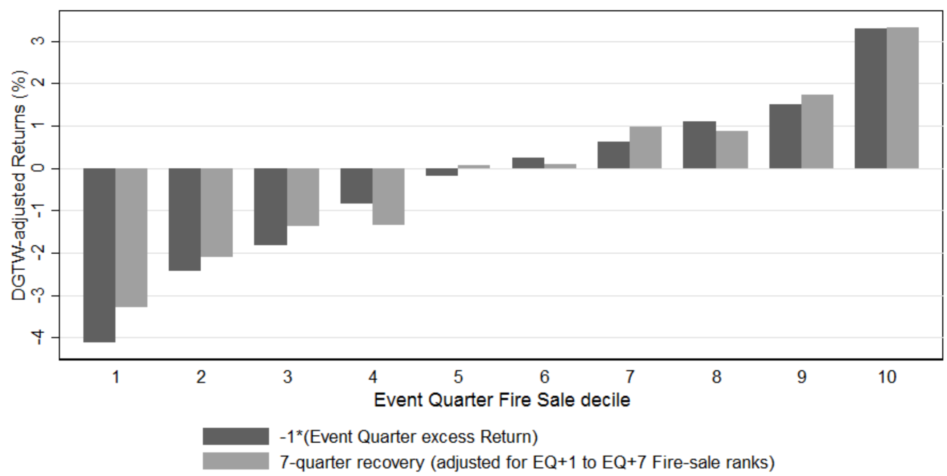


Figure 2: Recovery pattern by fire sale rank

This figure compares DGTW-adjusted returns in a given quarter (Event Quarter) with a recovery over the subsequent seven quarters by Event Quarter fire sale pressure decile. Panel A uses Edmans et al. (2012) methodology (EGJ) to construct the fire sale pressure, while Panel B uses the method of Lou (2012). The ranking into deciles is done within each calendar quarter over the mutual fund held rated firms (see Table 1 for sample description) such that the 1st [10th] decile experiences lowest [highest] fire sale pressure. The recovery is defined as the residual from a regression of the cumulative DGTW-adjusted returns over the next seven quarters on indicators of the fire sale decile in each of the following 7 quarters: $eR_i^{(q+1:q+7)} = \sum_{t=1}^{7}\sum_{j=1}^{10} \mathbb{1}[\text{FS-decile}_{i,q+t}=j]_i + \varepsilon_i$. Thus, the residual recovery ε_i can be thought of as future excess return unexplained by the pattern in future fire selling pressure in that stock. It is plotted with the light bars against the dark bars which indicate the negative of the Event Quarter return in the respective decile.

Panel A: The EGJ method



Panel B: The Lou method

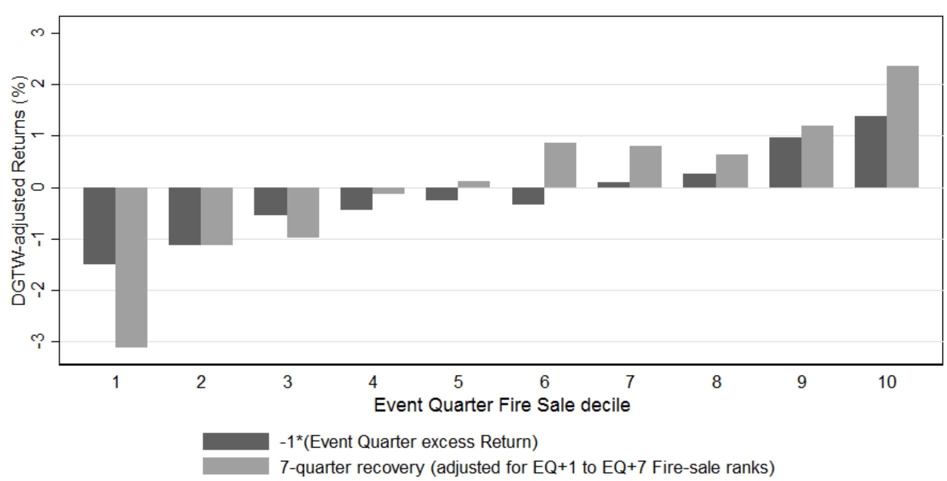
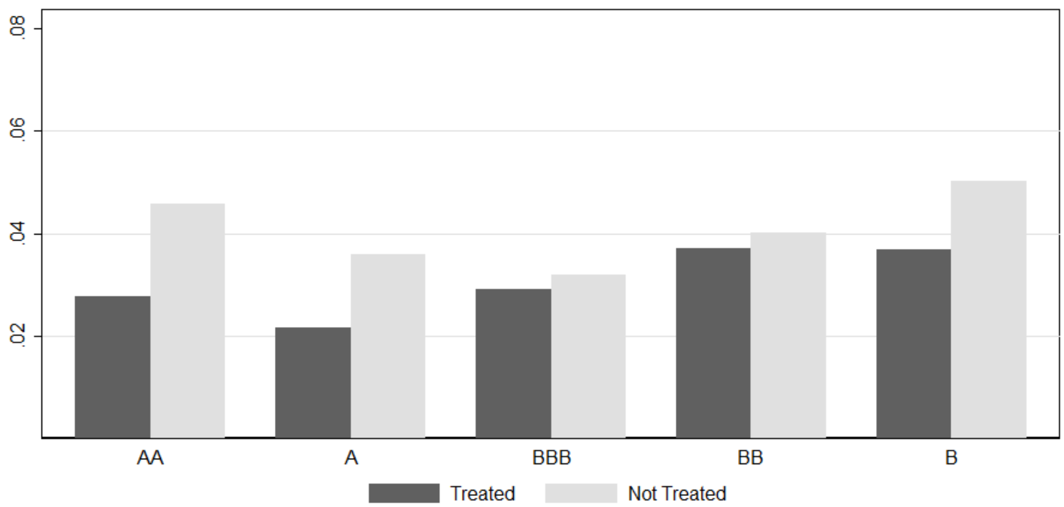


Figure 3: Fire-sale effects by rating level

This figure compares credit rating downgrade probability for fire-sale stocks (as defined in Section 2.1) to all other stocks (Panel A), and to matched controls (Panel B) by rating category. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of EQ), and return in EQ. See Section 2 for details.

Panel A: Full sample



Panel B: Matched sample

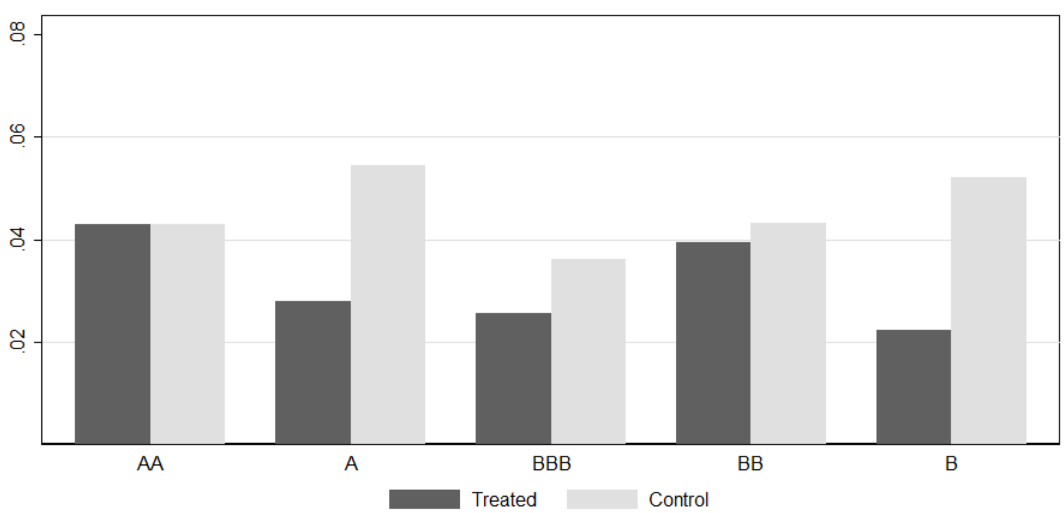
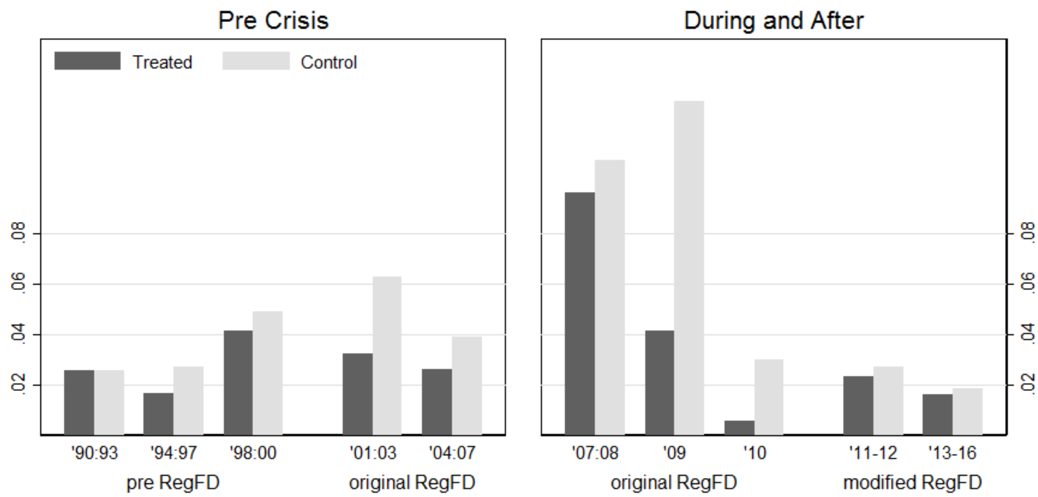


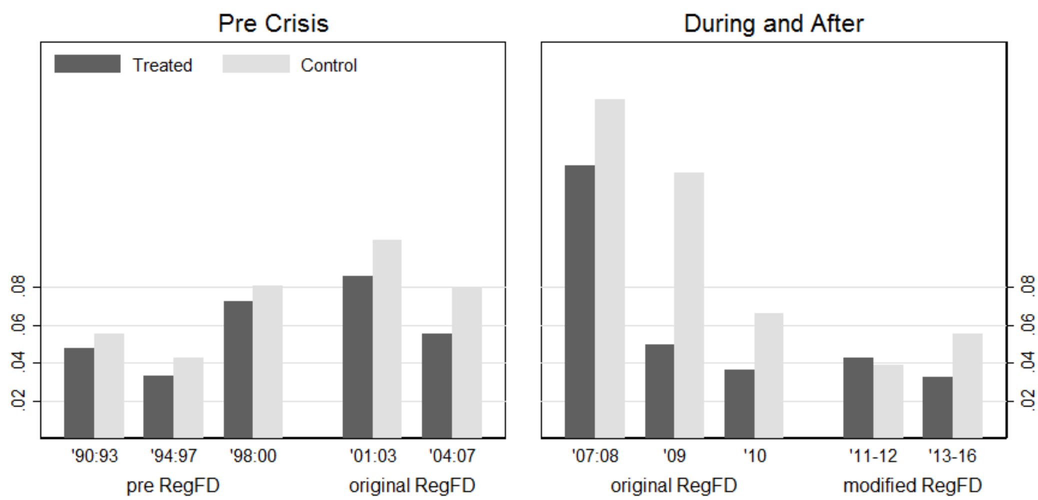
Figure 4: Matched sample fire-sale effects over time

This figure compares realized rating downgrade probabilities for treated firms to matched controls over different time periods and regulatory regimes. A treated firm is in the top decile of mutual fund fire-sales in an event quarter (*EQ*). See Section 2 for details. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of *EQ*), and return in *EQ*. Panel A reports results for the Event Quarter (*EQ*), whereas Panel B reports results for 6 months starting at the beginning of *EQ*.

Panel A: Event Quarter (EQ) ATT



Panel B: EQ to EQ+1 ATT



Appendix A. Data used to compute fire sales

This section describes the data used to calculate mutual fund fire sales in Edmans et al. (2012). The CRSP Survivorship Bias Free Mutual Fund database provides data at the mutual fund share class level. We use the MFLINKS file provided by Wharton Research Data Services (WRDS) to aggregate data to the fund level. For any observations not matched to MFLINKS, we use the CRSP portfolio number to aggregate the different share classes. We then merge the CRSP mutual fund database with the Thompson Financial CDA/Spectrum holdings database. We use the holdings data from CDA/Spectrum to compute the number of shares and value of equity holdings of mutual funds as of the quarter end.

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

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

Next, we describe the procedure used to compute the flow-induced price pressure measure suggested by Lou (2012). This replication employs the same dataset as the one used for calculating the above mutual fund fire sales measure. First, we estimate the following equation from Lou (2012) to measure the effect of holdings level liquidity and other constraints on the extent of partial scaling:

$$trade_{i,j,t} = \beta_0 + \beta_1 \cdot flow_{i,t} + \Gamma_2 \cdot X + \Gamma_3 \cdot flow_{i,t} \cdot X + \varepsilon_{i,t}. \tag{A.1}$$

The dependent variable is the percentage trading of stock *j* by fund *i* during quarter *t*. The key independent variable is $flow_{i,t}$, which is the capital flow in and out of fund *i* during quarter *t* expressed as a percentage of the funds TNA at the end of previous quarter. *X* includes variables that captures liquidity and trading costs: (i) the ownership share of fund *i* in stock /textitj and (ii) the effective half bid-ask spread estimated from the Basic Market-Adjusted model. These two control variables are the portfolio-weighted ownership share and liquidity cost, and therefore they are the fund level control variables. We use the above regression specification, which correspond to Columns 3 and 7 of Table 2 in Lou (2012).

Based on the above regression estimation results, flow-induced trading (FIT) for each stock in each quarter is defined as:

$$FIT_{j,t} = \frac{\sum_i shares_{i,j,t-1} \cdot flow_{i,t} \cdot PSF_{i,t-1}}{\sum_i shares_{i,j,t-1}}. \tag{A.2}$$

PSF is the partial scaling factor computed based on the above regression model. PSF is estimated for funds with capital inflows and outflows separately. Flows is the capital flow to fund i during quarter t expressed as a percentage of the funds lagged TNA, and shares is the number of shares held by fund i as of the end of the previous quarter.

Table A1: Fire sale probability model

We estimate models for the probability of a stock to experience a fire sale as a function of one-quarter lagged firm characteristics, past rating changes, stock returns, and year-quarter fixed effects. The outcome is one if the firm-quarter meets the criteria to be a fire sale according to Edmans et al. (2012) [Lou (2012)] methodology in Panel A [B] as described in Section 2.1, and zero otherwise. Specification (1) through (4) report linear probability model estimates. Specification (4) reports marginal effects estimated at means from a conditional logit model. In all specifications, the sample is restricted to firm-quarters that did not meet the fire sale criteria in the previous quarter. See table 1 for sample description. Standard errors for t-statistics (reported in parentheses) are clustered by firm, and */**/*** denote significance at 10/5/1% confidence level.

Panel A: The EGJ method-based Treatment definition

	OLS				Logit
	(1)	(2)	(3)	(4)	(5)
log(Market Cap)	−0.0201***	−0.0203***	−0.0202***	−0.0200***	−0.1030***
	(−19.47)	(−19.53)	(−19.41)	(−19.34)	(−31.87)
Leverage	−0.0291***	−0.0271***	−0.0259***	−0.0277***	−0.1426***
	(−5.28)	(−4.85)	(−4.57)	(−4.95)	(−7.92)
MF Ownership	0.3017***	0.3010***	0.3010***	0.3016***	1.1048***
	(18.78)	(18.68)	(18.61)	(18.70)	(29.45)
Amihud Ratio	1.1090***	1.0945***	1.0934***	1.1079***	2.0759***
	(14.64)	(14.43)	(14.32)	(14.53)	(18.02)
log(Realized Variance)	−0.0307***	−0.0310***	−0.0308***	−0.0304***	−0.1279***
	(−18.98)	(−19.07)	(−18.85)	(−18.73)	(−26.40)
Return (q-1)		0.0206***	0.0203***		
		(5.17)	(5.09)		
Return (q-2)		−0.0034	−0.0038		
		(−0.84)	(−0.94)		
Rating Downgrade last 6 months			−0.0056	−0.0059	−0.0271*
			(−1.52)	(−1.61)	(−1.83)
Rating Upgrade last 6 months			0.0011	0.0014	0.0095
			(0.29)	(0.35)	(0.62)
Observations	89,925	89,377	88,850	89,393	89,393
R^2 / Pseudo R^2	0.0367	0.0367	0.0366	0.0366	0.0740

Table A1: Fire Sale probability model (*Continued*)

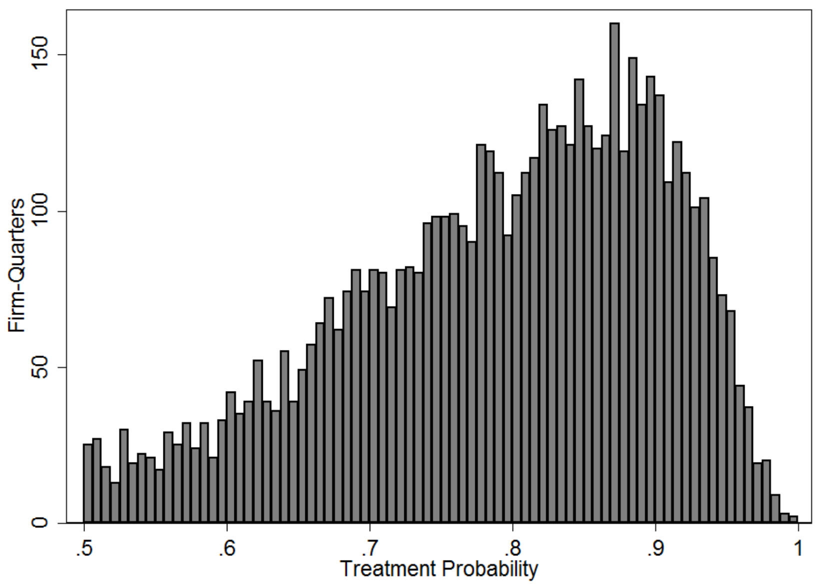
Panel B: The Lou method-based Treatment definition

	OLS				Logit
	(1)	(2)	(3)	(4)	(5)
log(Market Cap)	−0.0158***	−0.0153***	−0.0152***	−0.0157***	−0.0039***
	(−16.30)	(−15.77)	(−15.60)	(−16.14)	(−22.76)
Leverage	0.0093	0.0025	0.0030	0.0090	0.0006
	(1.42)	(0.39)	(0.45)	(1.37)	(0.66)
MF Ownership	−0.2824***	−0.2795***	−0.2780***	−0.2807***	−0.0593***
	(−16.68)	(−16.57)	(−16.40)	(−16.51)	(−25.37)
Amihud Ratio	0.2408***	0.2041***	0.2189***	0.2553***	−0.0160***
	(3.64)	(3.09)	(3.28)	(3.82)	(−2.68)
log(Realized Variance)	0.0185***	0.0195***	0.0196***	0.0185***	0.0031***
	(10.03)	(10.47)	(10.41)	(9.92)	(12.37)
Return (q-1)		−0.0421***	−0.0423***		
		(−7.95)	(−7.98)		
Return (q-2)		−0.0364***	−0.0359***		
		(−7.30)	(−7.15)		
Rating Downgrade last 6 months			−0.0033	0.0002	−0.0005
			(−0.71)	(0.05)	(−0.75)
Rating Upgrade last 6 months			−0.0086**	−0.0101**	−0.0023**
			(−2.13)	(−2.50)	(−2.41)
Observations	94,345	93,663	93,072	93,748	93,748
R^2 / Pseudo R^2	0.0324	0.0337	0.0338	0.0325	0.0576

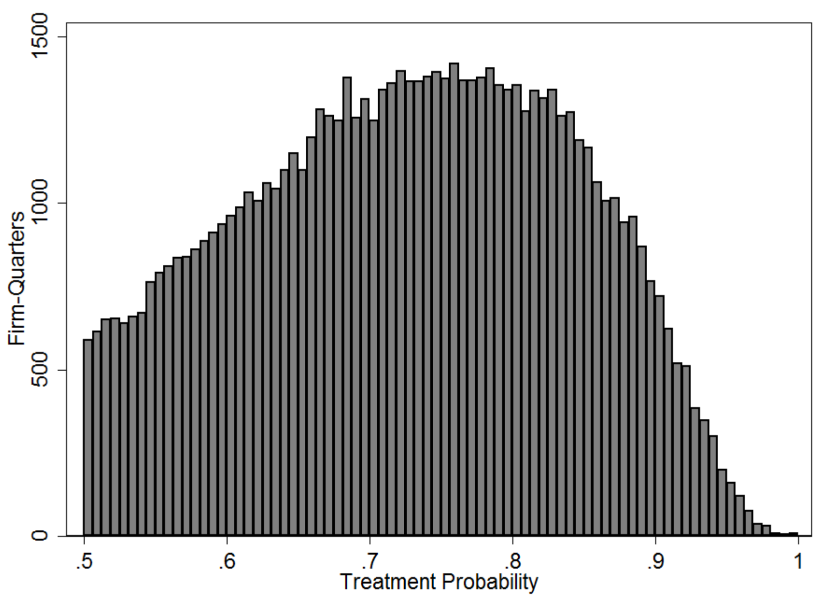
Figure A1: Propensity scores

This figure plots the propensity score estimates for being a fire-sale stock (as defined in Section 2.1) from the conditional logit model in Table A1 panel A for the fire-sale firm-quarters ('treated') in Panel A and all others in Panel B. We set year-quarter fixed effects to zero for comparability of scores across time. See Sections 2.1 for details.

Panel A: Treated



Panel B: Not Treated



Appendix B. Variables Definitions

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.
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 following Edmans et al. (2012) methodology. See Appendix A for details. Sources: CRSP, Thompson Reuters.
FIT	Flow induced trading defined as the predicted trade in a stock by the inflows and outflows across mutual funds using the specifications 3 and 7 of Table 2 in Lou (2012). See Appendix A for details. Sources: CRSP, Thompson Reuters.
Treated (1/0)	All firm-quarters were *MFFlow* is below the 10th percentile for that quarter within firms with non-zero *MFFlow* (local cutoff).
Event Quarter	The quarter for which the treated firm's *MFFlow* is below the local cutoffs.
Control Firm	Defined for each treated firm. Must have similar characteristics as the treated firm as of the start of the event quarter and the closest return to the treated firm during the event quarter. In particular, (i) the control must be in the same industry as the treated firm, (ii) have a similar propensity to be treated, (iii) the same credit rating at the beginning of the quarter, and (iv) closest stock return during the event quarter. We pick one control within one standard deviation of propensity score and also require that the ratio of the gross returns (i.e. 1+r) to that of the treated firm is within 1/1.2 to 1.2 while returns being of the same sign. 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.

Variable	Description
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.
Return (DGTW)	Stock return, including dividends, minus the return on the characteristics-matched portfolio following the methodology of Daniel, Grinblatt, Titman, and Wermers (1997). We rematch the momentum benchmarks every month. 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.
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. Quarterly changes are the sum of monthly changes over the quarter. Source: Markit
CDS Implied Downgrades	Based on ratings implied by five-year CDS contracts on a firm as computed by Markit.
Realized default probabilities	Binary variable based on bankruptcy filing data as reported by Capital IQ.