The Real Effects of Ratings Actions: Evidence from Corporate Asset Sales

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Keywords: Credit ratings, asset sales, asset allocation, financial constraints

JEL Classifications: G34

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1. Introduction

Understanding the existence and nature of real effects of credit ratings is important for several actors in financial markets. Such understanding is critical in making investment decisions, but also in the context of regulatory changes regarding institutional reliance on credit ratings. Moreover, as survey evidence consistently shows, credit ratings play a major role in corporate capital structure decisions, and access to capital and the cost of borrowing are among the most pressing concerns for corporate executives (see *e.g.*, Graham and Harvey (2001) and subsequent surveys by Graham, Meyer, Parker and Waddell (2020)).[1]

We study whether, how, and why negative rating actions affect subsequent corporate decision making. Negative rating actions have been shown to deteriorate a firm's financial condition, tighten its financial constraints, and increase its cost of capital. These may trigger a response by the affected firm (Fracassi et al., 2016; Goldstein and Huang, 2020; Kisgen, 2007; Manso, 2013). We present novel and robust evidence of corporate responses to negative ratings actions and on the channels that motivate these responses. Firms may respond to negative ratings actions by addressing pre-existing (or resulting) inefficiencies in their asset allocations, which we call the discipline channel. This channel is in line with the disciplinary role of credit rating agencies (CRAs) in Boot et al. (2005). Firms may also respond by generating more financial slack to address their tightened financial constraints, which we call the financial constraints channel. These channels are not mutually exclusive.

To test the discipline channel in conjunction with the financial constraints channel, we study corporate restructuring decisions induced by ratings actions. Specifically, we focus on corporate asset sales. Among material corporate decisions, asset sales provide an attractive and novel laboratory to test the channels underlying real effects of negative ratings actions.[2] Both of the aforementioned channels predict that negative rating actions could have real effects, but each channel yields distinct predictions with respect to observable characteristics

[1] https://www.frbatlanta.org/blogs/macroblog/2020/05/15/introducing-the-cfo-survey?item=50F08AC3-1109-4797-AB24-133E6209C2EE

[2] Alternative responses to rating actions, such as capital expenditures (CAPEX) decisions, are more difficult to interpret in terms of asset allocation decisions within and across firms. For example, CAPEX within a firm may be sub-optimal even under the most efficient owner.

of the resulting asset sales (if any).[3] For example, the discipline channel predicts that firms sell non-core assets and assets that underperform relative to their industry peers in order to (partially) revert negative ratings actions. Absent rating changes, managers may resist such efficiency-enhancing actions in the face of empire building concerns or due to reluctance to recognize losses. Hence, the disciplinary channel reduces agency problems in the spirit of Boot (1992), who argues that managers may hold on to certain assets for too long in the absence of governance mechanisms. Consistently, Denis and Kruse (2000) show that increases in managerial discipline result in more corporate restructuring events. The financing constraints channel predicts that firms primarily focus on generating financial slack in response to downgrades, even if this involves the sale of productive or efficiently operated assets. Firms may ultimately not respond to rating downgrades with asset sales if there are less costly alternatives available or if managers are unwilling to part with certain assets. If anything, this would empirically bias against finding a relationship between rating downgrades and subsequent asset sales, irrespective of the channel.

Our empirical strategy uses recent credit rating downgrades as a key explanatory variable for the incidence of asset sales. A necessary (albeit not sufficient) condition for identification is that ratings do not perfectly correlate with credit risk. Otherwise, we would be unable to separate effects of rating changes from effects of credit risk changes. In other words, our analysis depends on imperfections in the rating process, for example due to personal biases of analysts, such that downgrades can occur for non-fundamental reasons. We confirm that downgrades in our sample display a strong noise component. Specifically, the "political bias" measure of Kempf and Tsoutsoura (2021), which is orthogonal to firm fundamentals, is a strong predictor of rating downgrades in our setting too.[4]

Another requirement for our study to be valid and meaningful is for credit rating changes to have a material effect on firm value, for example through financial distress. Using an event study around rating downgrades, we present evidence that is consistent with credit

[3]Another benefit of focusing on discrete and (largely)irreversible stock-based events is that the use of these typically generates superior signal-to-noise ratios compared to changes in continuous flow variables.

[4]Given the proprietary and confidential nature of their data, we asked the authors to verify whether the results in Kempf and Tsoutsoura (2021) are also present in our sample. We are extremely grateful to Elizabeth Kempf and Margarita Tsoutsoura for running this analysis for us with our sample.

rating changes having a material effect on firm values.

One of the main empirical predictions of both channels is that firms, on average, respond to negative ratings actions through corporate asset sales. Using several empirical approaches, we show that the incidence of asset sales indeed increases sharply in response to rating downgrades for the sample period 1990-2015. A visual event study approach demonstrates these findings in an intuitive manner. Figure 1 plots the difference in the incidence of asset sales between firms that experience a rating downgrade and matched control firms that did not experience a downgrade during the six months prior and after the rating downgrade announcement. During the pre-announcement period, the trend in the difference in asset sales incidence between treated and control firms is flat. In stark contrast, in the post-announcement period, there is a strong and increasing difference in the incidence rate for treated firms relative to the control group. We deploy several alternative empirical methods, which we describe in more detail below, and arrive at the same conclusion that the incidence of asset sales increases after rating downgrades. These results are new to the literature, statistically significant, and robust to the inclusion of a comprehensive set of credit risk-related variables, other firm and deal characteristics, and various fixed effects. The results are also economically significant. We estimate asset sales to be 45% more likely after a rating downgrade. These results continue to hold when we control for covenant violations (to control for private creditor monitoring), prior credit watchlist placements, or when we measure negative rating actions by negative watchlist events (39% increase in incidence rate of asset sales).[5]

Our conclusions are robust to a variety of tests aimed at alleviating identification concerns, especially in the context of omitted variables related to credit risk.[6] Our empirical tests focus on the identification of restructuring events induced by rating actions. We want to rule out that the same firm fundamentals (e.g., a drop in demand for the issuer's products or services), cause both a downgrade and a divestiture.

[5]Watchlists indicate planned rating changes in the absence of improvements (negative watch) or adverse events (positive watch). As such, negative watch events often precede downgrades.

[6]Other studies, like Almeida et al. (2017) and Begley (2015) manage to obtain exogenous sources of variation in (the risk of) credit rating changes. We explain at the end of the introduction why their settings are less suited to study our research question.

Our first approach to address omitted variables is to include special controls. Specifically, we include covenant violations, credit spreads, and Moody's KMV Expected Default Frequencies^{TM} (EDFs) as "catch all" controls. These catch-all controls are strongly driven by credit risk, which means that they are also affected by any credit risk-related omitted variables. Our catch-all controls then proxy for such omitted variables. Credit spreads and EDFs are particularly appealing because they are based on prices and therefore reflect aggregate and forward looking market opinions. Our results continue to be statistically and economically significant when we include these catch-all controls. The results are also robust when we include multiplicative fixed effects (industry×year) to control for potential industry-specific shocks (Gormley and Matsa, 2013). Additional control variables and sub-sample analyses allow us to reject a variety of alternative channels that could have explained or affected our results, such as historical acquisition or LBO activity, anticipated asset sales (which could give rise to reverse causality), and ratings conveying private information.[7]

Our second approach is to calculate the average treatment effect of downgrades on asset sales using the non-parametric matched sample methodology of Abadie and Imbens (2011). This approach addresses concerns that non-linearities in our control variables could act as omitted variables. This strategy yields consistent and qualitatively similar results.

Our third approach to alleviate omitted variable concerns is based on a novel placebo test. In this test we exploit the well-documented phenomenon of sluggishness and conservatism in rating updates.[8] The through-the-cycle approach employed by CRAs, combined with conservatism in rating migrations, makes it highly likely that an important part of the information underlying rating changes was already publicly available earlier. Accordingly, we define placebo downgrade and upgrade indicator variables, which we shift one year prior to the actual rating change (see Fig. 3). Based on the premise that downgrades cause asset sales (which typically take less than a year to organize), we should not see an increase in

[7]CRAs may have access to private information and by issuing their rating reveal this information to the market. To address this, we show that our results are similar in a sub-sample analysis during the period in which CRAs were not exempted from Regulation Fair Disclosure (Reg-FD). During this period, it would be illegal for issuers to share material private information with CRAs without disclosing it publicly.

[8]See *e.g.*, (Altman and Rijken, 2004, 2006; Baghai et al., 2014; Beaver et al., 2006; Cheng and Neamtu, 2009; Kiff and Kisser, 2020; Loffler, 2004; White, 2010).

asset sales in the year prior to the rating change, but only in the period following the rating change.[9] Alternatively, if omitted public information drives our results, we expect to see an increase in asset sales in the year prior to the rating change, and not per se for the year after. We find a robust and large increase in asset sales (45%) in the post-downgrade period, but no material increase (1%) in the pre-downgrade period. The placebo results are robust to excluding larger asset sales, which arguably would take longer to organize, excluding asset sales announced within six months after the placebo date, or using a half-year shift.

Together, these tests support the conclusion that downgrades positively affect the incidence of subsequent asset sales. Naturally, the interpretation of our estimates rests on identification assumptions, which cannot be tested directly. However, while each of the robustness tests for identification may indeed not be 100% perfect individually, the hurdle to invalidate these robustness tests *jointly* is much higher, as weaknesses of a given test are covered by some of the others.

In the remainder of the paper, we analyze whether rating downgrades induce firms to sell or restructure assets to improve asset allocations, relax financial constraints, or both. We formulate several empirical predictions that help to delineate between the two channels and assess their relative importance. Our first test considers the reported use of the proceeds.[10] Following a downgrade, the discipline channel predicts an increased likelihood of asset sales with a reported purpose of efficiency improvements in productive asset configurations (Hite et al., 1987). In contrast, the financial constraints channel predicts an increased likelihood of asset sales with a reported purpose of raising cash, repaying debt, and addressing other financing frictions (Lang et al., 1995). At best, we find a weak and non-robust effect of downgrades on asset sales motivated by efficient asset reallocation or discipline. In fact, depending on the specification, the coefficient for a recent downgrade turns (insignificantly)

[9]Because contract law and the business judgment rule govern regular asset sales, as opposed to corporate law, asset sales are a relatively quick divestiture mechanism with few disclosure requirements and minimal need for shareholder approval or participation (*e.g.*, Hege et al., 2008). Firms are able to conduct (partial) asset sales typically in a matter of just a few months and much quicker than other forms of corporate divestitures. See, *e.g.*, `https://corpgov.law.harvard.edu/2017/07/27/when-a-piece-of-your-company-no-longer-fits-what-boards-need-to-know-about-divestitures/`

[10]Self-reported purposes can be biased. We find this bias to be limited (see Internet Appendix Table B.5).

negative for discipline-based asset sales. In contrast, we find that asset sales aimed to relax financial constraints are particularly more likely after a rating downgrade (75% to 156%). A potential concern could be that the discipline channel manifests itself when a firm is placed on a negative watchlist, which often precedes a downgrade. To exclude that possibility, we include watchlist placements in our specification. We find no link between negative watchlists and the incidence of discipline-based asset sales. Yet, we do find a positive association between both negative watchlists and downgrades and the incidence of asset sales aimed to relax financial constraints.

Corporate actions without cash inflow, such as spinoffs or payouts would not be a logical response to downgrades under the financial constraints channel. By contrast, if rating actions affect corporate restructuring decisions through the disciplinary channel, spinoffs may be a logical alternative to asset sales to improve asset allocations.[11] Yet, we find no relation between spinoffs and preceding downgrades or negative watchlist placements. In fact, conditional on a negative rating action, the average rating for firms that announce spinoffs and for firms that announce discipline-based asset sales are similar and three notches higher than firms that announce asset sales aimed at relaxing financial constraints. This difference is economically and statistically significant. Consistent with the literature, we do find a strong positive relation between downgrades and reductions in payouts (Bliss et al., 2015). If anything, these findings only corroborate the financial constraints hypothesis.

In the final section, we delineate between the two channels by asking whether rating actions affect the selection of assets to be sold in the asset sale. We match asset sales to segment data and conduct inter- and intra-firm analyses. Again, we fail to find support for the discipline channel. For example, we find no relation between rating actions and the likelihood of selling non-core segments or segments that underperform their industry peers. This lack of evidence persists when accounting for prior watchlists. By contrast, recent downgrades do increase the likelihood of selling segments with poor cash flow generation and the highest growth opportunities, each by ten percentage points. This suggests that

[11] We are grateful to our AFA discussant, Mariassunta Giannetti, for this suggestion.

after negative rating actions firms respond by trying to relax financial constraints, but hardly seek to improve the allocation of productive assets. To summarize, our analyses provide consistent and robust evidence in line with the financial constraints channel being the more dominant factor compared to the discipline channel in driving firms' restructuring responses to downgrades.

Our study contributes to several strands of literature. Foremost, our work complements and adds to the literature on the real effects of credit ratings by empirically analyzing the relative importance of the financial constraint channel and the discipline channel. On the theory side, Boot et al. (2005) suggest that CRAs have a monitoring role. We investigate this empirically. Goldstein and Huang (2020) and Manso (2013) derive feedback effects of ratings which provide micro-foundations for ratings tightening financial constraints. Yet, these studies make no predictions about the nature of corporate responses to negative rating changes. On the empirical side, Almeida et al. (2017) and Begley (2015) find that rating deteriorations reduce investment and lead to value-destroying earnings management. These studies are appealing from an identification perspective, but our focus on corporate restructuring events and our proposed empirical methodologies are better suited to understand the relative importance of these two channels. Almeida et al. (2017) exploit CRAs' sovereign ceiling rules to create a quasi-natural experiment around sovereign downgrades, which severely limits the sample to include 73 treated firms overall and just four U.S. firms. This sample would lack the necessary power to understand the relative importance of the underlying channels. Moreover, the sovereign ceiling is most likely to bind for creditworthy issuers for which discipline is arguably less important. Consistent with the idea that rating downgrades tighten financial constraints, they show a decline in subsequent debt issuance. Begley (2015) relies for identification on key Debt/EBITDA thresholds used by CRAs and presents results that suggest a relaxing of financial constraints. However, relaxing financial constraints around these thresholds is relatively easy and could easily lead to biased results given that rating actions could still induce discipline away from such thresholds.[12] Finally, Bannier et al. (2012) find that CAPEX reduces after downgrades, and especially for firms with more severe agency conflicts, which they interpret as evidence for discipline, but contrasts with our findings.

Compared to the aforementioned studies we use stock instead of flow variables. Stock variables arguably generate a higher signal to noise ratio. There are other studies that also use stock variables and show that ratings affect capital structure (Kisgen, 2006), the cost of capital (Kisgen, 2009), and acquisitiveness (Aktas et al., 2021). However, these studies do not explicitly study the discipline channel.[13] Overall, we contribute to the empirical literature by showing novel evidence on the relative importance of the financing constraints channel and the discipline channel based on corporate asset sales.

Our paper also contributes to the growing literature on non-fundamental determinants and biases of credit ratings. These biases include partisan biases (Kempf and Tsoutsoura, 2021), the home bias (Cornaggia et al., 2020), competitive effects (Becker and Milbourn, 2011), revolving door effects (Cornaggia et al., 2016), catering and ratings shopping (Griffin et al., 2013; Skreta and Veldkamp, 2009), and career concerns (Kisgen et al., 2020). We add to this literature and show that rating biases, which are orthogonal to firm fundamentals, are a strong predictor of rating downgrades in our setting as well.

Our results also add to the literature on creditor governance, which focuses mainly on private credit agreements and discipline imposed by debt covenants (e.g., Becher et al., 2021; Chava and Roberts, 2008; Nini et al., 2012; Roberts and Sufi, 2009). Covenants are less relevant for dispersed public debt, as these are typically not performance contingent but rather impose restrictions on the managerial action space (e.g., Chava et al., 2009; Smith and Warner, 1979). Our paper complements this literature by studying the role of credit rating changes in fostering public creditor governance. Our results are relevant against the

[12] Liu and Shivdasani (2019) bypass this issue by looking at exogenous variation in ratings-induced debt capacity following a methodological change by S&P. Yet, their events only include rating changes that relax, but not tighten financial constraints and are therefore less suitable to analyze discipline-based effects.

[13] Harford and Uysal (2014) show that not having a rating leads to under-investment in acquisitions, but also higher announcement returns. As such, their result could be interpreted as evidence at the extensive margin in support of both channels.

backdrop of a trend towards market-based financing and a diminished reliance on bank financing.[14] We find little support for the discipline channel and conclude that rating actions appear to be poor substitutes for monitoring actions around covenant breaches.

Finally, our results contribute to the asset sales literature, which shows that firms sell small, liquid, and non-core assets, and experience improved firm performance and reduced costs associated with cross-subsidization.[15] As such, the motivation for asset sales can originate from either discipline reflecting efficient investment decisions or from financial constraints (Lang et al., 1995; Warusawitharana, 2008). We provide evidence that in context of rating changes, the financial constraints channel dominates.

2. Real effects of rating changes

This section sets the stage for our empirical analysis in the subsequent sections where we try to understand whether and through which channel credit rating downgrades induce corporate restructurings. In Section 2.1 we present testable hypotheses that link rating changes to asset sales and delineate between the different channels. In Section 2.2 we present conditions that need to be satisfied for us to be able to empirically test these hypotheses.

2.1. Hypothesis development

Credit ratings can affect issuers in many ways, both ex-ante (anticipatory) as well as ex-post (reactive). The focus in this study is on ex-post effects as our empirical methodology is not designed to identify ex-ante effects. In Internet Appendix A, we present a simple model to formalize some of the main effects described below.

A deterioration in a credit rating is likely to increase a firm's cost of debt and financial distress costs, and tighten financial constraints, for example due to institutional reliance

[14] Recently the European Commission, through the Capital Markets Union Initiative, has aimed to stimulate market-based lending in favor of bank-based lending (`https://ec.europa.eu/info/business-economy-euro/growth-and-investment/capital-markets-union/what-capital-markets-union_en`). Bank debt too, has seen a recent emergence of credit agreements sans protective covenants ("cov-lite" loans), which may erode the effectiveness and relative importance of private credit monitoring *vis-a-vis* public credit monitoring (Becker and Ivashina, 2016).

[15] Eckbo et al. (2013) provide a detailed summary of this literature.

on credit ratings (see e.g., Bongaerts et al., 2012; Opp et al., 2013).[16] As a result, rating changes may trigger issuers to re-assess their portfolio of activities (*e.g.*, restructure assets) and/or to their financing choices (*e.g.*, reduce leverage). Importantly, such effects would manifest themselves even if rating changes are purely driven by changes in credit risk.

A firm may choose to restructure its portfolio of activities by selling assets for which the firm is not the best user relative to other firms. Such a restructuring would constitute an efficiency enhancement. Naturally, the feasibility of this transaction depends on whether the firm currently operates assets that underperform relative to its peers, the availability of suitable buyers, and the willingness of the CEO to implement this strategy since this may hurt her private interests.[17] Restructuring the firm's portfolio of activities would typically be an (ex-post) efficiency improvement. A restructuring of business activities could be conducted through an asset sale and its proceeds could, for example, be paid as dividends. Alternatively a spinoff could be undertaken (typically without cash transfer). In rare cases, a rating downgrade itself may make it optimal to sell assets even when the firm is ex-ante the best user of these assets. This could happen when a rating downgrade increases the cost of capital to levels at which it exceeds asset profitability. This would be most likely to happen with very distressed firms (which are excluded from our sample), or with assets that are hardly viable to begin with and that afterwards would most likely be put to alternative use (*e.g.*, coal mines). Even in this scenario, selling the relatively least profitable assets a firm owns would typically be most efficient (assuming that the relative efficiency ranking of segments is unaffected by a downgrade).

Alternatively, the firm may choose to make changes to its financial structure and decide to de-lever and/or increase liquidity buffers to (partially) undo the rating change or to re-optimize leverage, taking into account the additional financial distress costs from the lower rating level. Both actions would require cash. To de-lever, the firm would cut payouts (especially discretionary ones) and either issue additional equity or sell assets to pay down

[16] Another mechanism would be feedback effects (Goldstein and Huang, 2020; Manso, 2013).

[17] For instance, empire building, personal tastes, risk preferences, and a reluctance to admit failure or recognize an (accounting) loss affect this willingness (Boot, 1992).

debt, depending on the relative transactions costs involved with either strategy. The more liquid the assets are the firm could sell, the more likely it would prefer asset sales over equity issuance. In the absence of under-performing assets or CEO willingness to part with under-performing assets, such asset sales would constitute ex-post welfare losses.

The two channels are not mutually exclusive. A firm may for example sell under-performing assets and use the proceeds to pay down debt. As a result, tests that confirm or reject only one mechanism cannot be interpreted as evidence regarding the other.

The two channels are also not exhaustive. In case transaction costs are prohibitively high, and/or a CEO would be too reluctant to let go of an inefficient asset, doing nothing may also be a choice. If anything, this scenario would bias against finding any empirical relationship between rating deteriorations and asset sales. If so, discipline effects, if any could be concentrated among young CEOs and CEOs with shorter tenure because any underperforming asset would most likely be bought by a predecessor and these CEOs have had less time to build an emotional bond with certain assets.

Assets may have certain characteristics, such as cash flow generation features that would alleviate or add to financial distress when they are sold. All else equal, these features are likely to be more relevant under worsening financial and business conditions. When an inefficient asset is sold, the business prospects of the firm improve along with the proceeds from the sale, steering the firm further away from financial distress than when a productive or efficient asset of similar size is sold. As a result, these other characteristics would, all else equal, matter more for the financial constraint channel than for the discipline channel.

Our analysis focuses on the ex-post effects of rating changes. We purposefully abstract from ex-ante effects that result from the anticipation/risk of future rating changes, as our methodology is ill-suited to identify such effects.

2.2. Conditions for empirical identification

In this section, we present necessary conditions for empirical testability of our hypotheses and assert that these conditions are satisfied. The key testability condition is that credit ratings, despite their non-randomness, are not perfectly correlated with credit quality. If so, perfect multi-collinearity would prevent us from telling effects resulting from changes in credit quality apart from those resulting form rating changes themselves, even if perfect controls were available.

There are several reasons for credit ratings and credit quality to be imperfectly correlated. To start with, credit ratings have a through-the-cycle perspective and, consequently, adjust to information sluggishly and conservatively (Altman and Rijken, 2004, 2006; Baghai et al., 2014; Beaver et al., 2006; Cheng and Neamtu, 2009; Kiff and Kisser, 2020; Loffler, 2004; White, 2010). As a result, the rating path is smoother than the credit quality path and ratings lags behind. Therefore, ratings do not always perfectly reflect fundamentals. In Section 4.3.2, we exploit this sluggishness for a placebo test. Second, ratings are granular. As a result, divergences in rating actions may arise between similar but not identical firms that experiences the same deterioration in credit quality. Due to rating granularity, the same shock to credit quality may trigger a rating change for one firm and not for another. Virtually all CRAs use granular rating scales. However, rating granularity does not generate exogenous variation in ratings among identical firms that receive identical fundamental shocks. In this extreme situation one could still identify effects if there is noise in credit ratings that causes a rating change for one firm and not for another. In Section 4.1, we provide evidence that in our sample noise is indeed present in ratings. This evidence also conceptually validates the matched sample analysis in Section 4.3.2.

3. Data and sample design

In this section we describe the data sources and explain the formation of the full sample (all U.S. listed corporates), the asset sale sample, and the sample with segment level data.

3.1. Full sample

Our full sample includes the Compustat universe of listed corporates in the U.S., excluding firms with total book assets less than $75 million (measured in 1990 inflation-adjusted dollars), financial firms (SIC 6000 to 6999), and regulated utilities (SIC 4900 to 4999) during the period 1990-2015. We collect accounting data from Compustat and data on the top-5 executive shareholdings (in %) excluding options from ExecuComp. We collect S&P long-term credit ratings from Compustat Ratings and use CRSP data to construct the size-age (SA) index to proxy for financial constraints following Hadlock and Pierce (2010). To control for asset liquidity, we use the asset redeployability measure from Kim and Kung (2016).[18]

Finally, we collect watchlist data from Bloomberg for our entire sample period. Since Bloomberg does not have the same company identifiers, we match these data by name. We manage to automate this process for a sizeable fraction of rated firms in our sample. The remaining firms we match by hand. We cross-check with the letter rating and only include matches that we are certain about. Naturally, this procedure is far from ideal and we only match a subset of rated firms. Therefore, we only use rating changes in our baseline specification but we do provide additional specifications that include watchlist placements (see Table 5 and Internet Appendix Tables B.2 to B.4).

Our sample contains 835,926 firm-month observations, with 8,980 unique firms. Out of these, 3,253 firms (36.2%) have an S&P credit rating at any point of time in the sample period. Of the firms with a credit rating, 58% (47.8%) experience at least one credit rating downgrade (upgrade) during the sample period. Table 1 provides variables definitions and the first four columns in Table 2 provide summary statistics for our full sample.

3.2. Corporate restructuring sample

We consider two common types of corporate restructuring events. First, we collect corporate asset sales announced during the period 1990-2015 from the Special Mergers Sectors Database of Refinitiv Securities Database Corporation (SDC). We include all target firms for which the ultimate parent is a publicly listed firm in the U.S. and the restructuring event is completed and listed as a restructuring, equity carve-out, subsidiary acquisitions,

[18] We are grateful to Hyunseob Kim and Howard Kung for making these data available to us.

or divestiture. We also include deals labeled as seeking buyer, but exclude stock swaps, pooling of interest, reverse take-overs, and reverse Morris trusts. We remove observations if the transaction value is less than $10 million (in 1990 inflation-adjusted dollars), or if we cannot match the ultimate or immediate parent of the target with our full sample. Second, we collect a sample of spinoffs from SDC based on the same criteria and verify that these do not coincide with security issuances to outside investors or the sale of other assets for cash. Our main analyses focus on a merge of the full sample and the asset sale sample. We use the spinoff sample in separate analyses to identify the nature of asset sales.

Our selection criteria yield 16,304 completed assets sales. We match these deals to parent companies as follows. We take the target immediate parent and ultimate parent CUSIP and match those to CRSP in the fiscal year before the announcement. Usually the immediate and ultimate parent coincide. If not, we take the immediate parent. We delete deals that match only on the Target CUSIP field in SDC. Our final restructuring sample includes 4,974 asset sales, performed by 1,657 firms.

For this sample, we also collect data on the self-reported use of proceeds from SDC. We label the asset sales as financial constraints motivated if the firm (seller) intends to use the asset sale proceeds to pay down existing debt or to raise cash (247 deals or 5%). We label the asset sale as disciplinary if the firm intents to concentrate on core business or assets or if the sale involves a loss making or bankrupt operation (431 deals or 9%). For all remaining cases (over 80%), SDC reports no seller's purpose (either no data or only buyer purpose), only other seller purposes (approximately 5% of asset sales), or reports both types of purposes (<0.5% of asset sales).[19] We label those asset sales as ambiguous.

We also collect data on rumored, uncompleted, pending, intended, partially completed deals, and deals with seeking buyer announcements. We use these in robustness tests.

We also include non-rated firms and asset sales by non-rated firms and include rating fixed effects (having a rating at all or the particular rating). These observations help to estimate the effects of other covariates more precisely. Columns (4) to (8) in Table 2 show

[19]Most of these "other motivations" are either regulatory or classified as general purposes.

summary statistics for the asset sale sample.

3.3. Asset sales with segment-level data

Finally, we construct a sub-sample of firms that conduct asset sales for which we have matching segment data. To do so, we match each asset sale by a multi-segment firm to a corporate segment using the Compustat Segment File. There are 2,023 asset sales done by multi-segment firms. As SDC does not contain segment identifiers, we match the 4-digit (or, if unsuccessful, 3-digit) SIC code of the asset sale with the primary or secondary 4-digit (3-digit) SIC code in the Compustat segment file. This procedure yields 625 (118) unique matches. We match deals with multiple or no matches by hand using deal synopsis from SDC and 10-K filings from EDGAR (867 matches). We match 1,610 asset sales in total.

For each matched segment, we construct the following performance measures: operating profitability, profit margin, turnover, Cash Flow ratio, and Net Cash Flow ratio (definitions in Table 1). For each measure we determine whether each segment is in the top or bottom half of segments in the same 2-digit SIC industry. We also determine whether each matched segment is the highest or lowest, or in the upper or lower half within the firm regarding each of the performance measures, growth opportunities (segment industry average Tobin's Q), and whether the segment is non-core (two, three, or four-digit SIC industry different from parent). The final four columns in Table 2 present summary statistics for the segment sample.

4. Results

4.1. Validation of necessary identification conditions and relevance

We start our empirical investigation by showing that rating changes are in part driven by noise. This validates the necessary conditions for identification, as spelled out in Section 2.2. To this end, we present a replication of the results of Kempf and Tsoutsoura (2021) for our sample in Table 3. Kempf and Tsoutsoura (2021) regress rating actions, such as downgrades, from multiple CRAs at the firm-analyst-quarter level, on partisan biases of analysts involved in producing these ratings (political orientation of the analyst compared to the sitting president). They include firm-quarter fixed effects and include analyst covariates to prevent fundamentals or variables at the analyst level from driving their results. Table 3 indeed shows, as in their original paper, that political biases of analysts are an important driver of rating actions in our sample too. Since firm-quarter fixed effects are included, it is very unlikely that these results are driven by omitted variable related to (changes in) credit risk.

Moreover, one would only expect a causal relationship between rating changes and asset sales if rating changes are material drivers of firm value, for example through financial distress. We conduct an event study in which we look at cumulative abnormal stock returns (CARs) of downgraded firms in a window of -5 to $+5$ days around the downgrade announcement event to show that it is plausible for this condition to hold. Figure 2 presents the results for all rating downgrades (Panel A) and all rating downgrades with an asset sale within one year (Panel B). In both panels, there is a negative and consistently significant CAR from the announcement date onwards that does not revert afterwards. These findings are consistent with downgrade announcements negatively affecting enterprises value.

4.2. Credit rating changes as a determinant of asset sales

We now continue with our main analysis. Our first prediction is that negative rating actions have real effects. Specifically, we predict that the incidence of asset sales increases following negative rating actions. To show this, we model the waiting time for asset sales as a function of covariates using a duration model. Duration models are designed to model the arrival intensity of (conditionally random) events and are the optimal tool for our setting. Specifically, we choose the Cox proportional hazard model due to its convenient property that one only needs to estimate a partial likelihood. Our baseline model is as follows:

$$\lambda(s|\mathbf{X}_{i,t}) = \lambda_0(s)\exp\Big(\beta_1 \cdot \mathit{Rating\ downgrade}_{i,t}\,(0,1) + \beta_2 \cdot \mathit{Rating\ upgrade}_{i,t}\,(0,1) + \gamma' \cdot \mathbf{Y}_{i,t} + \mathbf{z}'_{i,t}\Big), \quad (1)$$

where $\lambda(s|\mathbf{X}_{i,t})$, is the asset sale hazard rate for firm i at time t that has survived for s periods and $\mathbf{X}_{i,t}$ is a vector of covariates. $\lambda_0(s)$ is the (common) baseline hazard rate for any firm that has not conducted an asset sale for s periods ($\lambda_0(s)$ is common and falls out, which facilitates estimation (Cox, 1972, 1975)). $\mathit{Rating\,downgrade}_{i,t}\,(0,1)$ ($\mathit{Rating\,upgrade}_{i,t}\,(0,1)$) is a dummy that equals 1 in case of an S&P credit rating downgrade (upgrade) over the 12 months preceding month t, and 0 otherwise. $\mathbf{Y}_{i,t}$ is a vector that contains time-varying firm-specific and potentially industry-wide controls; $\mathbf{z}_{i,t}$ is a vector of fixed effects. A firm is at risk from its first observation, or in case of multiple events per firm the month after the previous event. We properly account for censoring and truncation.

We estimate our Cox model on the whole universe of U.S. corporates. Our key explanatory variable is $\mathit{Rating\ downgrade}_{i,t}\,(0,1)$. Since several downgrades are preceded by negative watchlist placements, we also include specifications that specifically investigate the effects of negative credit watches. As our rating data have better coverage than our credit watch data, our main specification does not include watchlist placements. Our specifications include a comprehensive set of controls for credit risk, asset liquidity, firm performance, leverage, alternative forms of credit monitoring, and several fixed effects (see Table 1 for variable definitions). In our main specification, we include levels of all controls. We also include a specification in which we include changes rather than levels of all our controls. Depending on the specification, we include industry, time, industry $\times$ time fixed effects, and industry medians of all covariates. We update all covariates on a monthly basis and lag all covariates by a month to ensure that controls were public information. We first estimate the baseline model. In the next sections, we employ a comprehensive identification strategy to mitigate concerns about potential omitted variables biases.

Table 4 presents the baseline results of our duration analysis based on Eq. (1). In model (1), we include the respective dummy variables for credit rating downgrades and upgrades, a dummy for having a bond rating, and the rating level (if any). In this model, β_1 is positive and highly significant, which indicates that rating downgrades significantly increase the likelihood of asset sales. Rating upgrades on the other hand have a significantly negative coefficient and decrease the likelihood of asset sales. In model (2), we include a large set of controls related to the firm's credit risk profile and financial performance. Specifically, we add variables previously used in the literature as proxies for credit risk, such as profitability, leverage, size, tangibility, asset redeployability, financial constraints, governance, executive ownership, cash buffers, cash flow, growth opportunities, capital expenditures, R&D, and debt maturity structure. We continue to find a strong positive (negative) relation between the hazard rate of asset sales and credit rating downgrades (upgrades). In model (3) we add time fixed effects, in model (4) industry fixed effects (2-digit SIC), and in model (5) both industry and time fixed effects as well as industry covariates (industry medians of all covariates). In each specification, the relation between credit rating changes and the hazard rate of asset sales continues to be economically and statistically significant. For example, in model (5), a rating downgrade increases the hazard rate of an asset sale by about 45% ($\exp(0.369) - 1$). In model (6), we include multiplicative fixed effects (industry$\times$year) as a first way to mitigate unobserved heterogeneity (see also Gormley and Matsa, 2013) and β_1 remains positive and highly significant. This suggests that our estimates are not driven by unobservable industry shocks. We return to the important issue of omitted variables and identification more extensively in section 4.3.2.

Coefficient estimates for most control variables are consistent with tighter financial constraints. For example, higher leverage and more severe financial constraints trigger asset sales, while high ROA, a high Altman z-score and high cash holdings lower the incidence rate of asset sales. Model (7) matches model (5), but models only the first asset sale of each firm. The occurrence of an asset sale could influence the likelihood of future asset sales and hence, treating all asset sales the same might lead to biases. The coefficient on credit rating downgrades remains statistically and economically similar to that in model (5), but the coefficient on the credit rating upgrades switches sign and loses significance. In model (8), we include changes rather than levels of all covariates and results are similar to the other specifications. Across all specifications, the association between rating downgrades and subsequent asset sales is both economically and statistically material and robust.

We next consider the impact of being placed on a credit watchlist. CRAs put a firm on a negative (positive) watchlist to indicate an intention to downgrade (upgrade) if creditworthiness does not improve (deteriorate). Under the discipline channel, firms may react to negative watchlist placements (for example by selling non-performing assets) and preventing being downgraded (see e.g., Boot et al., 2005). Focussing solely on actual rating changes may incorrectly attribute such effects to rating changes rather than watchlist events. Therefore, we also run specifications controlling for recent watchlist placements.

In models (1) through (4) of Table 5, we re-estimate models (2) through (5) from Table 4, where we include separate indicator variables for negative and positive watchlist placements in the 12 months before in addition to the rating downgrade and upgrade indicator variables. For brevity, we suppress the remaining covariates in the subsequent tables. For each model, we find that the coefficients on the negative watchlist dummy are positive and significant at the one percent level. In other words, a negative watchlist, like a rating downgrade, increases the likelihood of asset sales. In terms of economic significance, the estimated coefficient from model (4), for example, translates into a 39% higher likelihood of an asset sales after a negative watchlist. The inclusion of watchlist dummies reduces the coefficients on the rating downgrade variable a little bit compared to Table 4. This is natural given the positive correlation (0.36) between the two. However, β_1 remains statistically and economically highly significant. For example, based on model (4), the likelihood of an asset sales still increases by 31% after a rating downgrade over and above the effect of watchlists. Therefore, our evidence shows that rating downgrades and credit watchlists are far from perfect substitutes and that downgrades matter over and beyond watchlists. In models (5) and (6), we confirm these conclusions for specifications modeling only the first asset sale and with changes (as opposed to levels) of the controls, respectively.

4.3. Alternative forms of creditor governance and identification

Overall, the results in Tables 4 and 5 show that negative rating actions are positively associated with subsequent asset sales. This is consistent with our predictions and with the prior literature, which consistently shows that ratings correlate with strategic corporate decisions beyond the effect of credit risk itself (e.g., Aktas et al., 2021; Kisgen, 2006, 2009). In this section, we address relevance and identification concerns. First, we analyze whether credit ratings matter over and beyond other forms of creditor governance. Second, we address omitted variables and present several approaches to mitigate identification concerns.

4.3.1. Creditor governance from covenants

We start with the question of whether our results are incrementally relevant beyond alternative mechanisms for creditor governance. The literature on creditor governance focuses primarily on private credit agreements and finds that covenants constitute binding constraints (e.g., Chava and Roberts, 2008; Roberts and Sufi, 2009). While firms with access to public debt markets typically satisfy their long-term financing needs with bonds, some exposure to private credit arrangements may remain (e.g., through credit lines). To the extent that this is the case, covenant breaches may render rating downgrades redundant and ineffective in terms of affecting corporate behavior. To address this issue, we download the updated covenant violation data from Michael Roberts' website and re-estimate Eq. (1), where we include a dummy variable that equals one if a covenant took place during the last 12 months and zero otherwise. Since the covenant violation data do not cover our entire sample period, we shorten our sample for this analysis and also run a specification without the covenant violation dummies to prevent sample differences from driving results.

Table 6 presents the results of this analysis. Model (1) shows robustness with respect to shortening the sample period. Models (2) to (5) include the covenant violation dummy and show that covenant violations are statistically and economically significant, corroborating findings by Chava and Roberts (2008) and Roberts and Sufi (2009). Moreover, including covenant violations leaves the effect of rating downgrades intact in all specifications. In short, rating downgrades seem to matter over and beyond covenant violations.

4.3.2. Identification

In Section 2.1, we present two important conceptual arguments that diminish identification concerns: (*i*) firms alleviating financial constraints in other ways would, if anything, work against us finding anything, and (*ii*) the likelihood of asset sales under both channels is incrementally affected by rating changes induced by creditworthiness deteriorations over and beyond the effect of creditworthiness deteriorations on asset sales themselves. Notwithstanding these arguments, we present several empirical approaches which, we believe, will further help mitigate identification concerns, specifically with respect to omitted variables.

First, we include special "catch-all" controls. These strongly correlate with credit risk. Omitted variables related to credit risk, would arguably also affect these catch-all controls. The catch-all controls then proxy for such omitted variables, even if these omitted variables are not observable or even known. The first catch-all control is the covenant violation dummy used above. The results in Table 6 show that omitted variables that induce both credit rating downgrades and covenant violations do not appear to drive our results.

We also include two market-based catch-all controls: Moody's KMV Expected Default Frequencies (EDF) and credit spreads. Since these are market-based, they reflect information in a forward looking way (as opposed to backward looking controls such as accounting ratios). Conceptually, we prefer credit spreads as these are more direct measures of credit risk. While widely accepted and used, Moody's KMV EDFs require more assumptions. Yet, Moody's KMV EDF data cover our entire sample period (as opposed to credit spread data, which only start in 2002).

We begin with the specifications that include credit spreads. We download bond yields for senior unsecured corporate debentures and corporate medium-term notes from the WRDS Bond Return Data (based on the TRACE Enhanced data that covers the entire U.S.

market and cleaned by WRDS). We require bonds to have at least one year to maturity left, have a strictly positive yield, and discard any yields larger than 30%. To construct credit spreads, we deduct an interpolation of the yields of two treasuries bracketing the bond in terms of duration as in Bongaerts et al. (2012). We match bonds to PERMCOs using the WRDS Bond Return Data matching table and create an issue-size-weighted average credit spread for every firm $\times$ month observation.

Since TRACE starts from July 2002, we provide results with and without credit spreads as controls over the sub-sample that starts in July 2002. The results are in Table 7. Model (1) shows that the results of our baseline estimation of Eq. (1) are robust for the sub-sample from July 2002 onwards. In models (3) and (4) we include credit spreads ($avgCS$) and an indicator variable that equals 1 if the credit spread is missing. The coefficients on $avgCS$ are insignificant while those on rating downgrades remain significant.

Moody's KMV EDFs represent expected default frequencies, generated by a modified version of the Merton (1974) model (see Crosbie and Bohn, 2003). The model behind the EDFs is calibrated to stock prices and return volatilities, because of which the forward looking nature of stock prices is inherited. Several publications by Moody's KMV show the usefulness of their EDF measure in predicting defaults.[20] We download one- and five-year EDFs from Moody's KMV and match these to our other data using CUSIPs. We run specifications with the one-year EDFs, five-year EDFs, and with both. Model (6) of Table 7 presents the most parsimonious model with both EDFs (the others are similar). The results are consistent with our baseline results and coefficients on EDFs are insignificant. The results with EDFs and credit spreads further mitigate omitted variable concerns.

Second, to exclude the possibility that non-linear terms of our control variables constitute a source of omitted variables, we also calculate the average treatment effect of downgrades on asset sales using the methodology of Abadie and Imbens (2011). In doing so, we use the control variables of our baseline specification plus the one and five year Moody's

[20] See e.g., https://www.moodys.com/sites/products/productattachments/riskcalc%203.1%20whitepaper.pdf

KMV EDF for matching.[21] We find a statistically significant average treatment effect of the treated (z-stat=3.87; p-value< 0.0001) that is qualitatively similar to the results of our duration analyses.

Third, we conduct a placebo test, in which we exploit the empirical fact that CRAs are relatively sluggish in their rating updates (Altman and Rijken, 2004, 2006; Baghai et al., 2014; Beaver et al., 2006; Cheng and Neamtu, 2009; Kiff and Kisser, 2020; Loffler, 2004; White, 2010). The through-the-cycle approach employed by CRAs combined with their conservatism in rating migrations make it highly likely that a material part of the information underlying rating changes would have been publicly available before these rating changes. In contrast, firms can conduct (partial) asset sales relatively quickly. A recent study by PricewaterhouseCoopers LLP reports that organizing an asset sale typically takes only a few months to a year, which is faster than for other types of corporate divestments.[22] This agility in asset sale decisions is important for our placebo design, because if asset sales are sluggish too, our placebo test may give spurious results. To implement this strategy, we construct a placebo downgrade and placebo upgrade indicator variable set equal to one one year prior ($t-1$) to the corresponding rating change at time t (see Figure 3).[23] Based on our hypothesis that asset sales are caused by downgrades and field evidence that asset sales typically take less than a year to organize, we should not see an increase in asset sales in the period from $t-1$ to t, but only for the period from t to t+1. Alternatively, if omitted public information drives our results, we expect to see an increase in asset sales in the period from $t-1$ to t and not, or at least much less so, for the period from t to t+1.

We report the results for the placebo test in Table 8. In model (1), we replicate model (5) from Table 4, but replace the rating change dummies with their placebo counterparts.

[21] We match exactly on 2-digit SIC, month, and whether R&D expenses and executive ownership are disclosed. We match as well as possible on all other covariates including credit rating and 4-digit SIC. We apply the standard bias adjustment and use a diagonal inverse variance weighting matrix.

[22] See https://corpgov.law.harvard.edu/2017/07/27/when-a-piece-of-your-company-no-longer-fits-what-boards-need-to-know-about-divestitures/

[23] Arguably, the period of twelve months is arbitrary. We choose this length because i) most companies review financial budgets and strategies annually, ii) credit ratings within CRAs tend to be reviewed (at least) annually (S&P Global, 2018), and iii) ideally we have no overlap between our regular measurements of rating changes and our placebos for these to be true placebos. If the period of twelve months in some cases were too long, we would still expect a sizable positive coefficient on placebo downgrades (albeit somewhat smaller than when the 12 months period would be 'spot on'). For robustness, we also present results with a smaller shift (half a year).

The coefficients on the placebo dummies are economically and statistically insignificant and considerably smaller than the coefficient reported in the baseline regression (0.008 vs 0.369). This suggests that information available prior to the downgrade does not drive the relation between downgrades and assets sales. Even if a 12-month shift is too large, we would still expect a much larger and more significant coefficient here if our results were driven by omitted variables. In model (2), we add the actual downgrade and upgrade dummies and find that β_1 continues to be positive and highly significant, while its placebo counterpart loads negatively and insignificantly. Economically, these results translate into a 45% increase in the asset sale likelihood following an actual downgrade versus a 3% decrease following a placebo. To avoid potential misclassification caused by sluggishness in asset sales, we drop asset sales announced within half a year of the downgrade and half a year after the placebo date (model (3)) and only consider below median-size asset sales which are arguably faster to organize ($\approx$ 2.7% relative to the seller's book value of assets; see model (4)). For both specifications, we continue to find positive and significant coefficients on the actual downgrade dummy and insignificant coefficients on the placebo. In model (5), we shorten the difference between the placebo and actual rating downgrade to half a year, which mitigates any concerns about a one-year shift being too large. These results are robust to modeling only the first asset sale of each firm or using changes rather than levels of controls (models (6) and (7), respectively). Summarizing, we find a sharp and robust increase in asset sales following downgrades, but not before, consistent with downgrades leading to asset sales.

Finally, we strengthen our identification by showing that our results are stronger when ratings matter more. Kisgen and Strahan (2010) and Bongaerts et al. (2012) show that the effect of ratings on the cost of capital is larger for firms around the Investment Grade-High Yield (IG-HY) boundary. We exploit this prediction as a fourth way to help assure that our duration models capture the effects of credit rating downgrades on the likelihood of asset sales. Accordingly, we expect a stronger effect of rating downgrades on the incidence of asset sales around the IG-HY boundary. To test this prediction, we weigh observations by the inverse distance in (absolute) number of notches between the firm's rating and the IG-HY boundary. Non-rated firms receive the minimum weight among the rated firms. We normalize weights such that the average weight over the whole sample equals 1. We report the results in Internet Appendix Table B.1. In models (1) and (2) this is done symmetrically. Weights on the HY side are multiplied by 2 in models (3) and (4), and by 5 in models (5) and (6) since rating notches represent larger differences in default probabilities on the HY side. On par with our prediction, the coefficients on the downgrade indicator variable are larger, and statistically substantially stronger than those reported in Table 4.

4.4. Channels through which firms respond to rating downgrades

One of the main objectives of the paper is to understand through which channel credit rating downgrades may induce corporate restructurings. To that end, we present in the next sections various approaches to test whether, on average, managerial discipline or financial constraints is the predominant channel through which firms respond to rating actions. To do so, we analyze the (self-reported) intended use of proceeds from the asset sales, equity-based spinoffs, payouts to equity holders, asset liquidity, and, finally, the choice of assets to divest based on inter-firm and intra-firm comparisons against the backdrop of the predictions under the different channels formulated in Section 2.1.

4.4.1. Use of proceeds

In this section, we test whether negative ratings actions associate with asset sales aimed at restructuring assets (discipline channel), raising cash or reducing leverage (financial constraints channel), or both. We use self-reported intended use of proceeds to estimate purpose-specific hazard rates. We estimate two types of specifications. First, we assume that the intensity of any type of asset sale is unaffected by previous asset sale events. In this case, we estimate purpose-specific hazard rate functions, where other types of events are assumed censored data points. Models (1) to (3) in Table 9 present the results. Alternatively, we recognize that an asset sale could affect the intensity future asset sales. To prevent any estimation bias, we only model the first asset sale (as in model (7) of Table 4). Yet, as a by-product, events become mutually exclusive, which we incorporate explicitly by using the competing risk framework of Fine and Gray (1999). Models (4) to (6) of Table 9 presents these results according. Without mutual exclusivity, the coefficient on the credit rating downgrades is positive and significant for all types of asset sales. However, for the financial constraints (model (1)), the coefficient is 30% larger and statistically more significant than for the discipline (model (2)). The competing risk model shows even more pronounced effects: asset sales are more prevalent following downgrades when aimed at relaxing financial constraints when aimed at efficient asset reallocation.

An important caveat is in order when using self-reported purpose statements as reporting may be biased towards managerial self-preservation. For example, after a downgrade CEOs may be reluctant to admit mismanagement of assets as a reason for selling. Instead they may justify asset sales as a way to alleviate tightened credit constraints. To at least partially address this potential bias, we re-estimate our model for firms with young CEOs (below median of 56 years old) and with CEOs with short tenure (below median of 5.91 years). Younger CEOs and those with shorter tenures, if anything, would be less likely to have been involved in the acquisition or mismanagement of assets, reducing this potential bias. The results are in Internet Appendix Table B.5. If anything, following rating changes young CEOs and CEOs with a short tenure are even less likely to engage in asset sales with discipline purpose. In the same context, the average ratings for firms announcing spinoffs or discipline-based asset sales following downgrades are three notches higher than firms announcing asset sales with aimed at relaxing financial constraints. This difference is statistically and economically significant and suggest that there is a fundamental difference in the credit situation of firms that report one purpose versus the other. This also alleviates concerns about self-reporting biases.

4.4.2. Spinoffs, CEO characteristics, and corporate payout ratios

It may be that firms avoid or choose a specific restructuring method in response to rating actions depending on the underlying channel. We use this logic to disentangle the underlying channels. Spinoffs, for example, are restructuring events like asset sales, but unlike asset sales, they do not generate cash for the firm or its shareholders. Under the financial constraints channel, spinoffs would not be a logical response to rating actions but under the discipline channel these would be. Of course, the discipline channel does not predict that firms must respond with spinoffs (regular asset sales would also be an option). Therefore, we would interpret an increase in spinoffs after negative rating actions as evidence for the discipline channel, but cannot reject the discipline channel if we find no effect.

We add 268 spinoffs events to our original sample and redo our analyses. The results are in Table 10. Model (1) allows for multiple events and implicitly assumes that hazard rates are unaffected by past events. We find that β_1 is insignificant for spinoffs compared to a consistently positive and significant estimate for asset sales. Models (2) to (5) present sub-hazard estimates for spinoffs and asset sales by their intended use of proceeds using the competing risk model by Fine and Gray (1999). Consistent with financial constraints, we find that rating downgrades significantly increase the likelihood of asset sales with the purpose to relax financial constraints, but do not affect the likelihood of spinoffs or asset sales with the purpose of efficient asset reallocating. To account for the possibility that firms respond to watchlist placements rather than downgrades, we re-estimate all models controlling for watchlist dummies (see Internet Appendix Table B.2). The results remain mostly the same, with the exception that for relaxing credit constraints negative watchlist placements now load positively and significantly, at the expense of downgrades (but β_1 is economically still large and with a z-statistic of 1.600 close to significant).

As outlined in Section 2.1, it is likely under the discipline hypothesis, that the willingness for managers to sell assets depends on whether these assets were acquired by their predecessors. Hence, we can re-use the results in Internet Appendix Table B.5. We find that the coefficients on the rating downgrade dummy for younger CEOs and those with shorter tenures are smaller and less significant than the full sample estimates in Table 9. This evidence is consistent with our general lack of evidence for the discipline channel.

To further asses the validity of our findings, we also consider payout reductions as an alternative response to downgrades. Under the financial constraints channel, one of the responses to rating downgrades would be to cut payouts to equity holders, especially discretionary ones. The discipline hypothesis does not make such a prediction. Therefore, we analyze the effect of rating changes on payout reductions to equity holders. Following Bliss et al. (2015), we define a payout reduction as a reduction in the sum of dividends and repurchases by more than five percent compared to the previous year and estimate logit models explaining payout reductions by all their covariates (excluding crisis interactions) plus our rating down- and upgrade dummies (see Internet Appendix Table B.6). We find a positive and, both economically and statistically, significant effect of rating downgrades on subsequent payout reductions. These findings corroborate our other results based on asset sales.

4.4.3. Asset selection

We next explore whether rating actions affect which assets firms sell. Our tests are based on the prediction that (i) under the discipline channel, firms would choose to sell off poorly performing or non-core segments following negative rating actions, and (ii) under the financial constraints channel firms would be more inclined to sell segments that impair creditworthiness more. We start with an inter-firm analysis, in which we compare divested segments to their industry peers to see whether under-performing segments are sold. Next, we present an intra-firm analysis based on performance measures of the divested segment relative to all non-divested segments within the same firm, whether the divested segment is a core segment or not, and whether it contributes to debt service.

Table 11 presents marginal effects of cross-sectional logit regressions. The dependent variable is an indicator variable equal to one if the corresponding performance measure is below the median of its industry peers and zero otherwise. In models (1) through (5), we focus on (Operating) Profitability, Profit Margin, Asset Turnover, Operating Cash Flow, and Net Cash Flow (all defined in Table 1). The discipline hypothesis predicts that poorly-fitting segments are sold following downgrads, and hence a positive and significant marginal effect on the downgrade dummy. However, the marginal effects of the downgrade dummy are insignificant for all performance measures and even have the opposite sign for Profitability and Profit Margin. Interestingly, the marginal effects of rating upgrades are mostly negative and significant, which suggests a reduction in managerial discipline following rating upgrades. In terms of credit rating downgrades, our main variable of interest, the results in Table 11 do not support the discipline hypothesis.

The financial constraints hypothesis is ambiguous in terms of predicting a sign on the rating downgrade dummy in the inter-firm analysis. Firms seeking to relax financial constraints are more likely to consider cash flow generation, contribution to collateral, and performance relative to internal segments rather than industry peers.

Table 12 presents marginal effects of cross-sectional logit regressions, but now for the intra-firm analysis. The dependent variable in each specification is a dummy that equals one if the divested segment has below median operating cash flow, has the highest collateral value (measured by Tobin's Q), is not a core segment, or has below median profitability, profit margins, or asset turnover relative to the other segments in the firm.

Under the financial constraints channel, we predict that other credit-risk related variables matter more under the financial constraints channel. Moreover, Shleifer and Vishny (1992) state that financially constrained firms are more likely to sell off segments that are valuable, but do not contribute to current cash flow generation. The results from models (1) and (2) are consistent with this prediction. Model (1) shows that segments that generate little cash are ten percentage points more likely to be sold following rating downgrades. Model (2) indicates that the probability that the divested segment has the highest Tobin's Q of the firm is almost ten percentage points higher if the asset sale follows a rating downgrade. Such segments typically require substantial investments in intangible assets, provide little to no collateral, and generate little current cash flow. Moreover, it is easier to convert high growth opportunity segments into cash and these are therefore prime candidates to divest in case of tighter financial constraints.[24]

The result of model (1) could also reflect selling off less productive assets in order to increase efficiency as predicted by the discipline hypothesis. By contrast, the result from model (2) is inconsistent with the predictions from the discipline hypothesis insofar as firms cherish high growth segments in favor of low growth opportunity segments that the firm can sell to a more efficient user of these assets. Note that we use the highest Q rather than the above-median Q in model (2) because of the relatively flat distribution among the segments' industry-median Q values. The coefficient on the rating downgrade variable is insignificant when we use the above-median Q instead (unreported). The results for models (1) and (2) support the financial constraints hypothesis and show that firms prefer to sell assets that generate low current cash flows and have high growth opportunities.

In model (3), we test another prediction of the discipline hypothesis: we would expect a higher likelihood for selling non-core segments following downgrades as a firm is less likely to be the optimal user for such segments. Inconsistent with this prediction, we find no evidence that the likelihood of selling a non-core segment (based on two digit SIC code) is significantly higher after a credit rating downgrade. We re-estimate, but do not tabulate, model (3) where we use a more granular definition of a non-core segment, (based on three and four-digit SIC codes) and confirm this result. In models (4)-(6), we test if segments with below-median performance are more likely to be sold following rating downgrades. Similar to the results from the inter-firm segment analysis, the coefficient on the rating downgrade dummy in these models is insignificant for profitability and profit margin as performance measures. Together with the result on operating cash flows from model (1), this suggests that selling low cash flow generating segments is not a reflection of selling unproductive assets, as would be predicted by the discipline hypothesis. We do find a positive and significant coefficient on the rating downgrade dummy for low asset turnover. This is the

[24] Our interpretation is valid under the assumption that assets are fairly valued. If not, high Q segments may reflect overvalued segments, in which case selling these assets could be consistent with either channel.

lone evidence we can find in the segment analyses consistent with a discipline channel. Yet, it could also be consistent with a financial constraints channel insofar higher asset turnover allows for more efficient working capital management.[25] Taken together, the results in models (4) through (6) do not provide much support for the discipline hypothesis.[26]

Finally, we re-estimate all models in Tables 11 and 12 controlling for watchlist dummies (see Internet Appendix Tables B.3 and B.4). For the inter-firm analyses, results are unaffected. The watchlist variables do not load in any of the models either. The results for the rating downgrade dummy in the intra-firm analysis strengthen with the inclusion of the watchlist dummies. Interestingly, we find that both negative and positive watchlist events have a negative relation with our first performance measure (low operating cash flow) in model (1).

4.5. Robustness and alternative channels

In this section, we further challenge the robustness of our results and ask whether alternative channels can explain our results. First, the control variables we include for credit risk in our previous analyses, such as covenant violations, credit spreads, and Moody's KMV EDFs, may be incomplete to the extent that rating analysts may base their ratings actions also on private information, for example, as provided by issuers under non-disclosure agreements. To test whether our results are affected by such asymmetric information, we run our tests for the time-period starting after September 2010, when the Securities and Exchange Commission amended Regulation FD to remove the specific exemption for disclosures made to nationally recognized statistical rating organizations for the purpose of determining credit ratings.[27] We report the results in Internet Appendix Table B.7. Despite the much shorter sample period, our results are consistent with and highly similar to the results we estimated for the full sample. This mitigates concerns that our results are materially affected by CRAs transmitting private information through their ratings.

[25] E.g., for the same collection period, a larger volume of outstanding invoices could be delayed on.

[26] We recognize that failing to reject the null hypothesis (β_1=0) may be caused by a lack of power of the test and/or an attenuation bias in the presence of measurement error and/or multicollinearity. Yet, the results in models (1) and (2) should suffer from the same problem.

[27] See https://www.sec.gov/rules/final/2010/33-9146.pdf

Second, we ask if historical acquisition activity can explain our findings. Kaplan and Weisbach (1992), for example, show that a substantial fraction of their sample of large acquisitions is subsequently divested. Firms may divest certain parts of recently acquired companies for disciplinary reasons especially if these assets do not provide a good fit with the new owners (Hite et al., 1987). Similarly, acquisitions may trigger rating downgrades, especially if they are associated with cash payments, significant increases in debt, or other outcomes that deteriorate the growth prospects of the firm. In this sense, asset sales may simply be a disciplinary or financial constraints-induced response to acquisitions, rather than caused by credit rating downgrades per se.

To address these alternative mechanisms, we collect all majority acquisitions from SDC and match these with our full sample. We then construct the following measures of acquisition activity. First, for each firm i in month t, we calculate the ratio of aggregated deal values with firm i as a buyer from month $t-24$ to $t-6$ to the most recent book value of assets prior to month t (Acquisition spending). We also define an indicator variable (Acquisition) equal to one if Acquisition spending > 0 and zero otherwise. Similarly, we calculate the aggregated dollars spent with cash [equity] on acquisitions as a fraction of the most recent book value of assets prior to month t (Cash [Equity] acquisition spending). We include these measures of acquisition activity as controls in our baseline Cox model (see Internet Appendix Table B.8). The coefficients on all these acquisition-related variables are positive and significant. An F-test indicates that the coefficient for equity used in acquisitions is marginally higher than the coefficient for cash spending on acquisitions (p-value $= 0.085$). In all specifications, the coefficient on our rating downgrades variable remains positive, strongly significant, and economically similar to our baseline specification (model (5) of Table 4). Hence, acquisition activity helps to explain the likelihood for asset sales, but does not change our conclusions with respect to rating downgrades. We also run a subsample analysis in which we split the sample into observations with (Acquisition spending > 0) and without (Acquisition spending=0) prior acquisition activity and find that the coefficient on the rating downgrade dummy is positive and significant in both models.

Leveraged buy-outs (LBOs) are another example of events in which the corporate structure of a firm is heavily affected. Asset sales and rating changes may occur concurrently as a result of the LBO. To make sure LBOs do not drive our results, we re-estimate our duration analysis in which we exclude all deals that are part of an LBO transaction. This includes straight up LBOs and deals conducted by an LBO firm, where the parent firm is an LBO firm, or deals in which the acquirer is an LBO firm (data from SDC). The results are qualitatively and quantitatively similar to our baseline results and reported in Table B.9 in the Internet Appendix.

Since we only consider completed deals in our analyses, there is a potential concern about selection bias. In particular, there may be endogenous selection effects that impair the external validity of our results. To alleviate such concerns, we provide a robustness test in Table B.10 in the Internet Appendix in which we re-estimate our duration analysis and include deals that never (fully) completed. These deals include rumored, pending, intended, and partially completed deals. We label these as 'failed deals' and include these as a specific type of deals in the same way we do with spin-offs. We re-estimate separate sub-hazards for these deal types, both in an unconstrained setting as well as in a competing risk setting (similar to the analysis in Table 9). We conclude that the degree of such bias, if any, is small.

Finally, reverse-causality may become an issue if rating changes already incorporate expected asset sales. We would expect this issue to be most relevant for rumored deals and for cases with a prior "seeking a buyer" announcement. Therefore, we re-run our duration analysis where we exclude completed deals that before completion had rumors, seeking buyer announcements, or both. We report our results in Table B.11 in the Internet Appendix. These are qualitatively and quantitatively similar as our baseline results.

5. Conclusion

In this paper, we provide strong and novel evidence on the existence of real effects of credit rating downgrades on the basis of corporate restructuring decisions in general, and asset sales in particular. We show that firms, on balance, respond to rating actions primarily in accordance to a financial constraints hypothesis, while we hardly find any evidence for responses in accordance to a discipline hypothesis.

Identification is a prime concern in the absence of a strong instrument or natural experiment. Yet, a battery of robustness tests mitigate these concerns significantly, while preserving a large and comprehensive sample. Moreover, the different tests in concert paint a coherent picture. While one could challenge each of these tests individually, it would be more challenging to come up with alternative mechanisms that would predict similar empirical patterns jointly and consistently.

While comprehensive, our findings leave room for future research as we are only able to analyze real effects of ratings to the extent that asset sales are not prohibitively expensive. Firms are likely to resort to other means to relax financial constraints (or foster discipline) following rating downgrades as their asset liquidity deteriorates. Hence, our findings are particularly relevant for firms with sufficiently high asset liquidity. One would need to investigate other ways of relaxing financial constraints (or fostering discipline) to say more about the external validity of our results in case of highly illiquid assets. Similarly, discipline could manifest itself in other forms than through corporate restructuring events, such as asset sales (e.g., cost-cutting and layoffs; see Denis and Kruse, 2000). Yet, our empirical tests imply that managerial private benefits are high, which suggests that these other forms of discipline are also less likely. Moreover, our methodology is aimed at identifying ex-post effects of rating changes, but is less suited to analyze ex-ante effects that result from the anticipation of the risk of being downgraded.

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Figures

Figure 1: Difference in asset sales incidence rate between treated and control firms
The figure plots the difference in the cumulative daily incidence rate of asset sales between firms experiencing a rating downgrade announcement (treated firms) and matched control firms from 180 days before to 180 days after the rating downgrade announcement day (t=0). Control firms are matched based on industry, the baseline firm-level control variables from models (2) to (8) in Table 4, and on either Moody's KMV one and five year EDFs (Panel A) or credit spreads (Panel B).

Panel A: Matching on firm characteristics & Moody's KMV one and five year EDFs

Panel B: Matching on firm characteristics & credit spreads

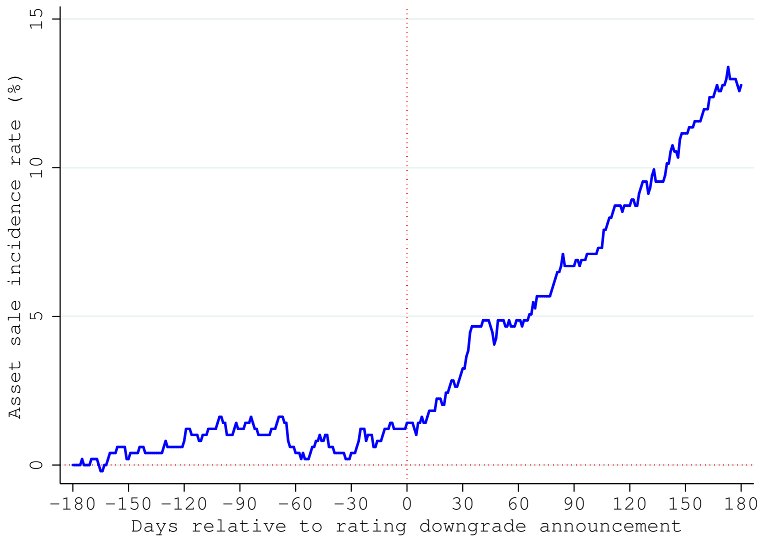
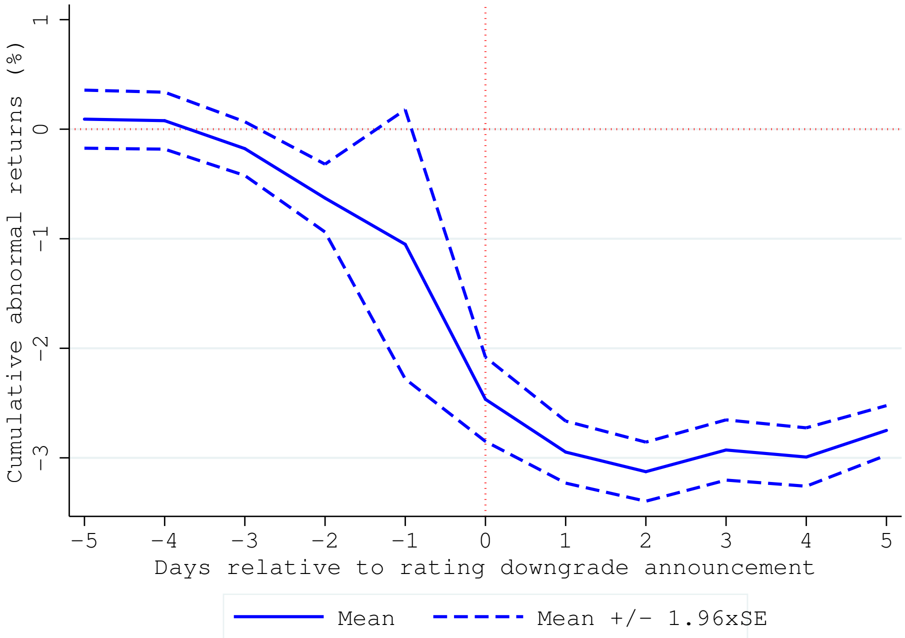


Figure 2: Cumulative abnormal returns around downgrades
The figure plots average cumulative abnormal returns (CAR) around downgrade announcement event days (t=0). Returns on day t are based on end-of-day t closing prices. We use a market model with an estimation window of 100 trading days (with a minimum of 70)and a gap between the estimation and event windows of 20 trading days. The dashed lines are the 95% confidence bounds, calculated as the mean +/− 1.96 times the standard error (SE). Panel A shows the CAR for all downgraded firms in our sample that we were able to match to rating event date from Bloomberg and Panel B shows the CAR for all firms that had a subsequent asset sale within one year of the downgrade.

Panel A: All downgraded firms



Panel B: Downgraded firms with subsequent asset sales

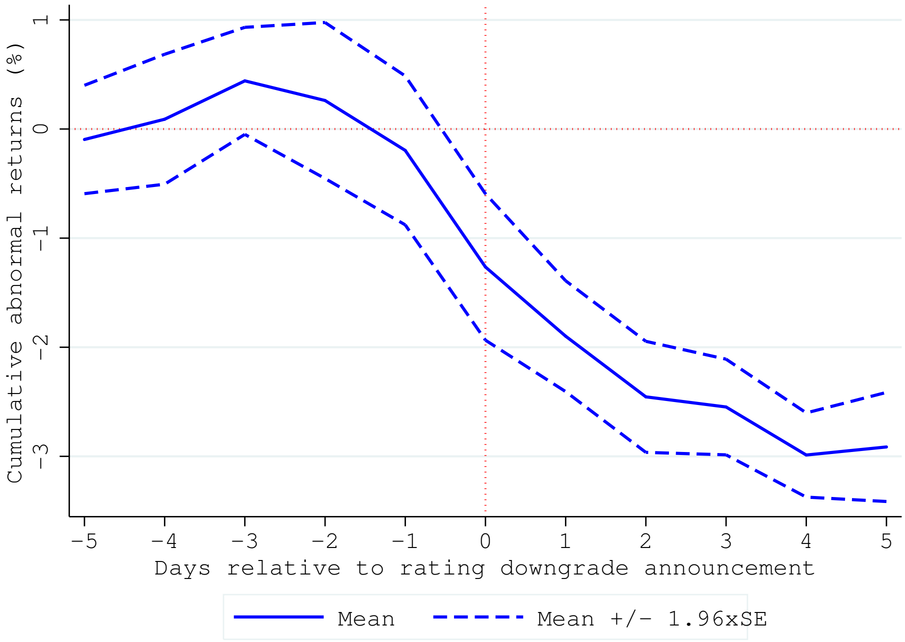
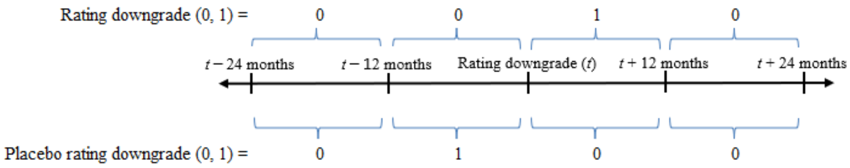


Figure 3: Placebo timeline

This figure shows the time-line around month t, during which the rating downgrade occurs and how the indicator variable for a (placebo) rating downgrade, (Placebo) Rating downgrade (0, 1), is set equal to one during the 12 months period following (prior to) month t and set to zero elsewhere.



Tables

Table 1: Variable definitions

Variable	Description
Segment profitability	Segment ROA (operating income over identifiable assets)
Segment turnover	Segment sales over identifiable assets
Segment profit margin	Segment operating income over sales
Segment operating cash flow	Segment operating income plus depreciation over identifiable assets
Segment net cash flow	Segment operating income plus depreciation minus CAPEX over identifiable assets
Rating downgrade (0,1)	Indicator variable=1 if downgraded by S&P in the past year (=0 otherwise)
Rating upgrade (0,1)	Indicator variable=1 if upgraded by S&P in the past year (=0 otherwise)
S&P credit rating	S&P credit rating (AAA=22; declining by 1 per notch)
Positive watchlist (0,1)	Indicator variable=1 if put on positive watch by S&P in the past year (=0 otherwise)
Negative watchlist (0,1)	Indicator variable=1 if put on negative watch by S&P in the past year (=0 otherwise)
Assets – Total (1990 $ billion)	Total (book) assets (inflation-adjusted, in 1990 $ billions)
Ln(Assets)	Natural logarithm of Total (book) assets (in 1990 $ millions)
Asset redeployability	Average asset redeployability as defined as in Kim and Kung (2016)
Cash holdings	Cash over total assets
Interest coverage	Operating income before depreciation over interest expense
Leverage	Total debt over total assets
Rollover	Short-term debt over total assets
Tangibility	PPE over total assets
Altman Z-score	Altman's Z-score
Tobin's Q	(Equity market cap + book assets – book equity and deferred taxes) / total assets
Profitability	Operating profit over total assets
ROA	Net income over total assets
Cash Flow	(EBIDA – interest expense, dividends and taxes) / total assets
CAPEX	Capital expenditures over total assets
PPE growth	Growth rate of PPE over the past year
Sales growth	Growth rate of sales over the past year
R&D/assets	R&D expense over total assets
Number of segments	Number of business or operating segments
Stock ownership	Ownership top-5 compensated executives excluding options (in % points)
SA index	SA index of financial constraints (Hadlock and Pierce, 2010)
Same two-digit SIC (0,1)	Divested segment same industry as target parent (two-digit SIC)
Deal Value	Deal value in $ millions
Relative size	Deal value over total assets

Table 2: Summary statistics

The table presents the number of observations (n), means, standard deviations (SD), and medians for the variables used in the analysis for the three samples used in the analysis. The 'Full Sample' consists of the universe of Compustat firms over the period 1990 to 2015, the 'Asset Sale Sample' consists of all matched asset sales over our sample period, and the 'Segment Sample' consists of all asset sales over our sample period that could be traced back to one of the segments in the Compustat Segment file. Variable descriptions are in Table 1.

	Full sample				Asset sales				Segment-matched subsample			
	n	Mean	SD	Median	n	Mean	SD	Median	n	Mean	SD	Median
Rating downgrade (0, 1)	835,926	0.048	0.213	0.000	4,974	0.144	0.351	0	1,610	0.157	0.363	0
Rating upgrade (0, 1)	835,926	0.038	0.192	0.000	4,974	0.050	0.217	0	1,610	0.045	0.207	0
Positive watchlist (0,1)	835,926	0.009	0.094	0.000	4,974	0.020	0.140	0	1,610	0.026	0.159	0
Negative watchlist (0,1)	835,926	0.028	0.164	0.000	4,974	0.093	0.290	0	1,610	0.114	0.318	0
S&P credit rating	322,911	12.550	3.619	12.000	3,793	14.022	4.165	14.000	1,381	14.825	4.028	15
Asset redeployability	808,271	0.419	0.135	0.416	4,872	0.379	0.128	0.403	1,593	0.391	0.115	0.406
Assets - Total	835,926	5,797	27,572	736	4,974	40,392	122,473	4,472	1,610	59,424	146,904	9,310
Cash holdings	825,999	0.094	0.107	0.052	4,825	0.062	0.072	0.037	1,555	0.058	0.063	0.038
Interest coverage	697,121	20.980	35.633	6.955	4,824	12.016	21.693	5.405	1,580	13.440	22.068	6.489
Leverage	832,365	0.244	0.194	0.222	4,967	0.320	0.179	0.305	1,608	0.313	0.170	0.292
Rollover	834,526	0.039	0.058	0.014	4,972	0.050	0.067	0.021	1,610	0.052	0.068	0.024
Tangibility	834,835	0.287	0.237	0.222	4,973	0.335	0.233	0.278	1,610	0.320	0.221	0.268
Altman Z-score	689,412	3.828	3.561	2.996	4,422	2.624	2.380	2.319	1,412	2.707	2.055	2.484
Tobin's Q	736,461	1.774	1.019	1.432	4,743	1.602	0.762	1.370	1,542	1.636	0.732	1.406
Profitability	835,926	0.077	0.114	0.079	4,974	0.075	0.093	0.075	1,609	0.089	0.083	0.085
ROA	835,926	0.026	0.117	0.038	4,974	0.014	0.109	0.030	1,609	0.035	0.087	0.041
Operating cash flow	717,149	0.075	0.102	0.081	4,792	0.068	0.078	0.068	1,570	0.072	0.066	0.071
CAPEX/assets	762,354	0.070	0.065	0.048	4,888	0.070	0.064	0.047	1,594	0.062	0.056	0.044
PPE growth	832,570	0.142	0.335	0.052	4,967	0.078	0.295	0.023	1,607	0.072	0.273	0.025
Sales growth	830,970	0.136	0.274	0.084	4,972	0.085	0.256	0.047	1,609	0.074	0.222	0.049
R&D/assets	434,303	0.058	0.084	0.024	3,001	0.038	0.057	0.017	1,055	0.032	0.045	0.015
Number of segments	763,933	2.175	1.537	2.000	4,933	3.129	1.991	3.000	1,610	3.973	1.868	4.000
Stock ownership	255,248	4.557	6.912	1.300	1,741	2.876	5.318	0.688	618	2.754	5.379	0.475
SA index	835,926	-3.702	0.635	-3.593	4,974	-3.939	0.642	-3.997	1,610	-4.119	0.594	-4.437
Deal Value	-	-	-	-	4,974	324	875	92	1,610	565	1,041	220
Relative Size	-	-	-	-	4,974	0.082	0.153	0.027	1,542	0.070	0.127	0.021
Same two-digit SIC (0,1)	-	-	-	-	-	-	-	-	1,610	0.620	0.485	1
Segment profitability	-	-	-	-	-	-	-	-	1,192	0.112	0.220	0.097
Segment asset turnover	-	-	-	-	-	-	-	-	1,472	1.223	1.316	0.973
Segment profit margin	-	-	-	-	-	-	-	-	1,293	0.059	1.892	0.110
Segment operating cash flow	-	-	-	-	-	-	-	-	1,134	0.164	0.225	0.143
Segment net cash flow									1,068	0.104	0.224	0.091

Table 3: Non-fundamental rating changes

This table replicates the results from Table 3 of Kempf and Tsoutsoura (2021). It presents estimates from an OLS regression (linear probability model) of rating downgrade actions at the analyst/firm/quarter level on an ideological mismatch variable (equal to 1 if sitting president's political orientation is opposite of analyst's and 0 otherwise), firm × quarter fixed effects, analyst covariates (tenure at CRA and # of firms covered) and in column (1) CRA fixed effects, in column (2) CRA × industry fixed effects, and in column (3) agency × quarter fixed effects. The sample is the intersection of our main sample and the sample of Kempf and Tsoutsoura (2021). Analysts employed by S&P, Moody's and Fitch are covered. Standard errors are clustered by firm and analyst. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variable	Downgrade		
	(1)	(2)	(3)
Ideological mismatch	0.0121***	0.0114***	0.0071***
	(3.81)	(3.68)	(2.81)
Tenure	-0.0084***	-0.0091***	-0.0076***
	(-3.61)	(-3.93)	(-3.72)
Number of firms covered	0.0019	0.0023**	0.0026**
	(1.64)	(2.18)	(2.46)
Firm × Quarter FEs	Yes	Yes	Yes
Agency FEs	Yes	No	No
Agency × Sector FEs	No	Yes	No
Agency × Quarter FEs	No	No	Yes
Number of observations	33,815	33,815	33,813
R^2	0.812	0.812	0.817

Table 4: Basic Cox regressions

The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis over the entire sample. R&D missing (0, 1) and Stock ownership missing (0, 1) are equal to one if, respectively, R&D intensity or executive ownership is missing, and zero otherwise. We define all other covariates in Table 1. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	Levels						1st Event	Changes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rating downgrade (0, 1)	0.597***	0.392***	0.355***	0.401***	0.369***	0.360***	0.412***	0.446***
	[9.75]	[5.92]	[5.49]	[6.11]	[5.77]	[5.62]	[4.16]	[6.75]
Rating upgrade (0, 1)	-0.264***	-0.208**	-0.224***	-0.191**	-0.183**	-0.160*	0.0644	-0.177**
	[-3.16]	[-2.46]	[-2.64]	[-2.30]	[-2.17]	[-1.87]	[0.48]	[-2.07]
Rated (0, 1)	0.0244	-0.191	0.144	-0.124	0.246	0.224	0.270	0.507***
	[0.11]	[-1.11]	[0.83]	[-0.74]	[1.47]	[1.33]	[1.45]	[3.10]
S&P credit rating	0.106***	0.0640***	0.0222	0.0573***	0.0126	0.0158	0.00608	0.0368***
	[5.80]	[4.76]	[1.62]	[4.34]	[0.95]	[1.18]	[0.41]	[2.97]
Redeployability		-1.394***	-1.728***	-0.821*	-1.223**	-1.533***	-1.330***	0.670
		[-3.65]	[-4.62]	[-1.69]	[-2.52]	[-3.00]	[-2.66]	[0.75]
Altman Z-score		-0.0583***	-0.0877***	-0.0358*	-0.0589***	-0.0592***	-0.0312	0.00106
		[-2.92]	[-4.19]	[-1.76]	[-2.74]	[-2.80]	[-1.50]	[0.06]
Cash holdings		-2.914***	-2.286***	-2.476***	-1.790***	-1.652***	-1.857***	0.248
		[-7.27]	[-5.70]	[-6.42]	[-4.80]	[-4.39]	[-4.37]	[0.73]
Interest coverage		-0.00333**	-0.00106	-0.00379***	-0.00107	-0.00129	-0.000680	0.00109
		[-2.21]	[-0.75]	[-2.66]	[-0.82]	[-1.07]	[-0.47]	[1.05]
Rollover		-0.732	-1.101**	-0.232	-0.647	-0.719	-0.753	0.498
		[-1.34]	[-1.97]	[-0.42]	[-1.18]	[-1.35]	[-1.32]	[1.11]
Leverage		-0.0387	0.0453	-0.0786	0.125	0.151	0.420*	0.988***
		[-0.17]	[0.20]	[-0.35]	[0.56]	[0.68]	[1.73]	[3.42]
Tangibility		-0.760***	-0.852***	-0.723***	-0.965***	-0.973***	-0.796***	-1.065**
		[-3.58]	[-3.91]	[-3.40]	[-4.33]	[-4.40]	[-3.61]	[-2.10]
Tobin's Q		0.0428	0.120**	-0.00883	0.0312	0.0445	-0.0772	-0.0890*
		[0.93]	[2.52]	[-0.19]	[0.65]	[0.93]	[-1.52]	[-1.84]
Profitability		0.981*	0.192	1.083**	0.00798	-0.00816	-0.694	-0.133
		[1.80]	[0.33]	[2.02]	[0.01]	[-0.01]	[-1.07]	[-0.25]
ROA		-1.594***	-1.457***	-1.830***	-1.692***	-1.649***	-1.675***	-0.227
		[-4.85]	[-4.37]	[-5.74]	[-5.17]	[-5.04]	[-4.05]	[-0.70]
Operating cash flow		-0.551	-0.265	-0.493	0.104	0.189	0.954*	-0.317
		[-1.10]	[-0.50]	[-1.05]	[0.22]	[0.38]	[1.72]	[-0.72]
CAPEX/assets		3.104***	2.668***	2.585***	1.469**	1.199*	0.449	-0.308
		[5.11]	[4.30]	[4.23]	[2.34]	[1.90]	[0.64]	[-0.63]
PPE growth		-0.511***	-0.460***	-0.456***	-0.395***	-0.359***	-0.292**	-0.228
		[-4.46]	[-3.99]	[-4.11]	[-3.67]	[-3.35]	[-2.28]	[-1.50]
Sales growth		-0.307**	-0.393***	-0.332***	-0.459***	-0.519***	-0.399***	-0.389***
		[-2.50]	[-3.00]	[-2.86]	[-3.71]	[-4.21]	[-2.78]	[-2.63]
R&D/assets		0.570	-0.00185	0.240	-0.372	-0.503	-0.149	-0.186
		[0.94]	[-0.00]	[0.42]	[-0.64]	[-0.85]	[-0.26]	[-0.26]
R&D missing (0, 1)		-0.0221	-0.0252	0.0823	0.0593	0.0447	0.115	-0.0208
		[-0.33]	[-0.39]	[1.08]	[0.80]	[0.60]	[1.56]	[-0.28]
Number of segments		0.102***	0.109***	0.105***	0.105***	0.113***	0.106***	0.150***
		[5.84]	[6.16]	[6.05]	[5.99]	[6.38]	[5.23]	[8.12]
Ln(Assets)		0.297***	0.374***	0.290***	0.374***	0.363***	0.175***	0.131
		[10.71]	[13.27]	[9.65]	[12.51]	[12.34]	[5.54]	[0.67]
SA index		0.292***	0.412***	0.247***	0.383***	0.381***	0.510***	2.196***
		[4.36]	[6.86]	[3.78]	[6.68]	[6.76]	[8.75]	[3.01]
Stock ownership		-0.0149*	-0.0256***	-0.0127*	-0.0243***	-0.0252***	-0.0241***	-0.00831
		[-1.67]	[-2.80]	[-1.68]	[-3.14]	[-3.21]	[-2.67]	[-0.53]
Stock ownership missing (0, 1)		-0.145**	-0.231***	-0.138**	-0.222***	-0.240***	-0.341***	-0.0363
		[-2.41]	[-3.86]	[-2.31]	[-3.76]	[-4.05]	[-4.37]	[-0.55]
Industry FEs	No	No	No	Yes	Yes	No	Yes	Yes
Time FEs	No	No	Yes	No	Yes	No	Yes	Yes
Industry covariates	No	No	No	No	Yes	No	Yes	Yes
Industry×Time FEs	No	No	No	No	No	Yes	No	No
Number of observations	835,269	587,043	587,043	586,055	578,306	586,055	453,546	495,144
Pseudo R^2	0.037	0.055	0.074	0.063	0.085	0.104	0.065	0.069
Number of clusters	8,980	6,406	6,406	6,401	6,364	6,401	6,302	5,472

Table 5: Cox regressions including watchlist placements.
The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis over the entire sample. Negative (Positive) watchlist (0,1) refers to a dummy variable that equals 1 in case the firm was put on negative (positive) watch during the 12 months preceding the observation. The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	Levels				1st Event	Changes
	(1)	(2)	(3)	(4)	(5)	(6)
Rating downgrade (0,1)	0.291***	0.267***	0.293***	0.270***	0.326***	0.343***
	[4.38]	[4.11]	[4.43]	[4.16]	[3.10]	[4.86]
Rating upgrade (0,1)	-0.223**	-0.239***	-0.208**	-0.200**	0.0523	-0.209**
	[-2.57]	[-2.75]	[-2.42]	[-2.32]	[0.37]	[-2.36]
Negative watchlist (0,1)	0.337***	0.283***	0.366***	0.329***	0.259*	0.357***
	[4.13]	[3.42]	[4.47]	[3.96]	[1.93]	[4.17]
Positive watchlist (0,1)	0.220	0.188	0.236	0.208	0.202	0.290*
	[1.49]	[1.27]	[1.63]	[1.45]	[0.82]	[1.90]
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	Yes	No	Yes	Yes	Yes
Time FEs	No	No	Yes	Yes	Yes	Yes
Industry covariates	No	No	No	Yes	Yes	Yes
Number of observations	586,927	586,927	585,939	578,190	453,522	495,164
Pseudo R^2	0.054	0.072	0.062	0.083	0.064	0.07
Number of clusters	6,406	6,406	6,401	6,364	6,302	5,472

Table 6: Cox regressions including covenants.
The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis over the sample up to 2011. Covenant violation (0,1) is a dummy variable that equals 1 in case of a covenant violation during the 12 months prior to the observation in question. The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	Levels			1st Event	Changes
	(1)	(2)	(3)	(4)	(5)
Rating downgrade (0, 1)	0.382***	0.421***	0.374***	0.388***	0.479***
	[5.29]	[5.77]	[5.18]	[3.67]	[6.42]
Rating upgrade (0, 1)	-0.212**	-0.221**	-0.213**	0.0399	-0.143
	[-2.24]	[-2.33]	[-2.25]	[0.27]	[-1.48]
Covenant violation (0,1)		0.569***	0.587***	0.522***	0.567***
		[4.52]	[4.58]	[3.65]	[4.47]
Other covariates	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	No	Yes	Yes	Yes
Time FEs	Yes	No	Yes	Yes	Yes
Industry covariates	Yes	No	Yes	Yes	Yes
Number of observations	441,230	447,415	441,230	355,205	383,490
Pseudo R^2	0.084	0.06	0.085	0.063	0.066
Number of clusters	5,807	5,844	5,807	5,760	5,011

Table 7: Cox regressions including credit spreads Moody's KMV EDFs.
The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis. Models (1) to (4) use data from July 2002 onwards. avgCS is the average credit spread and CS Missing (0,1) equals 1 if credit spread data is missing and 0 otherwise. EDF1 and EDF5 are 1 and 5 year Moody's KMV EDFs, respectively. The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	(1)	(2)	(3)	(4)	(5)	(6)
Rating downgrade (0,1)	0.261***	0.252***	0.257***	0.249***	0.321***	0.316**
	[2.76]	[2.62]	[2.71]	[2.59]	[4.61]	[4.66]
Rating upgrade (0,1)	-0.287**	-0.269**	-0.278**	-0.262**	-0.253***	-0.217**
	[-2.58]	[-2.43]	[-2.48]	[-2.36]	[-2.76]	[-2.39]
avgCS			-2.873	-2.257		
			[-1.46]	[-1.17]		
CS missing (0,1)			-0.838***	-0.654***		
			[-6.84]	[-5.44]		
EDF1					-0.00471	-0.00113
					[-0.47]	[-0.11]
EDF5					0.0103	0.00962
					[0.53]	[0.48]
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	Yes	No	Yes	No	Yes
Time FEs	No	Yes	No	Yes	No	Yes
Industry covariates	No	No	No	No	No	Yes
Number of observations	292,840	292,518	292,840	292,518	406,837	400,809
Pseudo R^2	0.071	0.097	0.071	0.097	406,837	400,809
Number of clusters	3,946	3,944	3,946	3,944	406,837	400,809

Table 8: Cox hazard regressions with placebo indicator variables for rating changes
The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis over the entire sample. We cluster standard errors by firm and report t-statistics in brackets. The same covariates are included as in Table 4. In models (1), (2), (5), and (6) placebo dummies are set according to Fig. 3. In model (3) placebo dummies are set to zero between $t-12$ and $t-6$. In model (4), only asset sales with a relative size below the median of 2.7% are classified as events. The offset between placebo and actual rating in model (5) is half a year. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	Levels					1st Event	Changes
			Excluding 1st half year	Small deals only	Half year Difference		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Placebo Rating downgrade (0, 1)	0.00801	-0.0321	-0.0283	0.0776	0.0495	-0.144	0.0338
	[0.11]	[-0.44]	[-0.35]	[0.77]	[0.68]	[-1.23]	[0.42]
Placebo Rating upgrade (0, 1)	-0.0707	-0.0322	-0.0506	-0.239**	-0.0706	-0.0291	0.0215
	[-0.91]	[-0.42]	[-0.54]	[-2.13]	[-0.85]	[-0.22]	[0.26]
Rating downgrade (0, 1)		0.370***	0.305***	0.263***	0.337***	0.419***	0.444***
		[5.81]	[4.16]	[2.99]	[4.81]	[4.20]	[6.80]
Rating upgrade (0, 1)		-0.184**	-0.211**	-0.348***	-0.135	0.0564	-0.176**
		[-2.19]	[-1.96]	[-2.95]	[-1.51]	[0.42]	[-2.05]
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	578,306	578,306	578,306	578,190	578,190	453,546	495,144
Pseudo R^2	0.084	0.085	0.084	0.195	0.081	0.065	0.069
Number of clusters	6,364	6,364	6,364	6,364	6,364	6,302	5,472

Table 9: Hazard rate regressions by purpose

The table presents coefficient estimates for the hazard rates of asset sale events by self-reported purposes: Relaxing credit constraints (Relax), Discipline, or Ambiguous. Specification (1) to (3) are Cox proportional hazard regressions on a monthly basis over the entire sample. These specifications assume that multiple events can happen to a subject and that hazard rates are unaffected by such events. Specifications (4) to (6) present sub-hazard estimates for asset sales using the competing risk model by Fine and Gray (1999), in which subjects leave the sample after the first event takes place. The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	Multiple events with others as censored			Competing risk (1st only) subhazard		
	Relax	Discipline	Ambiguous	Relax	Discipline	Ambiguous
	(1)	(2)	(3)	(4)	(5)	(6)
Rating downgrade (0, 1)	0.559***	0.430**	0.379***	0.868**	-0.122	0.441***
	[2.88]	[2.41]	[5.26]	[2.19]	[-0.31]	[4.14]
Rating upgrade (0, 1)	-0.269	-0.629**	-0.178**	0.488	-1.006	0.103
	[-0.82]	[-2.24]	[-2.03]	[0.77]	[-1.39]	[0.74]
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	No	No	No	No	No
Time FEs	No	No	No	No	No	No
Industry covariates	No	No	No	No	No	No
Number of observations	606,620	606,620	606,620	475,654	475,654	475,654
Pseudo R^2	0.117	0.045	0.055	-	-	-
Number of clusters	6,550	6,550	6,550	6,489	6,489	6,489

Table 10: Spinoff hazard rate regressions

The table presents coefficient estimates for the hazard rates of spinoffs and asset sale events by self-reported purposes: Relaxing credit constraints (Relax), Discipline, or Ambiguous. Specification (1) refers to Cox proportional hazard regressions on a monthly basis over the entire sample for spinoffs. This specification implicitly assumes that multiple events can happen to a subject and that hazard rates are unaffected by past events. Specifications (2) to (5) present sub-hazard estimates for spinoffs and asset sales by purpose using the competing risk model by Fine and Gray (1999). The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	Multiple events with others as censored	Competing risk (1st only) subhazard			
	Spinoff	Relax	Disicpline	Ambiguous	Spinoff
	(1)	(2)	(3)	(4)	(5)
Rating downgrade (0, 1)	0.188	0.941**	-0.166	0.382***	-1.028
	[0.68]	[2.29]	[-0.40]	[3.44]	[-1.44]
Rating upgrade (0, 1)	-0.814**	0.572	-0.921	0.119	-0.714
	[-2.12]	[0.90]	[-1.27]	[0.83]	[-1.20]
Other covariates	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	No	No	No	No
Time FEs	No	No	No	No	No
Industry covariates	No	No	No	No	No
Number of observations	606,620	470,476	470,476	470,476	470,476
Pseudo R^2	0.058	-	-	-	-
Number of clusters	6,550	6,482	6,482	6,482	6,482

Table 11: Inter-firm segment performance analysis

The table presents average marginal effects of logit regressions of dummy variables equal to 1 for inter-firm segment underperformance and 0 otherwise, in comparison to medians of peer segments in other multi-segment firms with matching 2-digit SIC codes for Profitability, Profit Margin, Asset Turnover, Operating Cash Flow, and Net CF using the Segment Sale sample. t-statistics are in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level. Variable descriptions are in Table 1.

	Profitability (1)	Profit Margin (2)	Asset Turnover (3)	Operating CF (4)	Net CF (5)
Rating downgrade (0, 1)	0.00143	-0.0243	0.0122	0.0219	0.0329
	[0.034]	[-0.592]	[0.288]	[0.511]	[0.761]
Rating upgrade (0, 1)	-0.154**	-0.112*	0.0270	-0.110	-0.118*
	[-2.315]	[-1.708]	[0.357]	[-1.516]	[-1.656]
Other covariates	Yes	Yes	Yes	Yes	Yes
Rating FEs	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes
Number of observations	874	978	1,113	853	801
Pseudo R^2	0.124	0.109	0.0829	0.150	0.161

Table 12: Intra-firm segment performance analysis

The table presents average marginal effects of logit regressions of dummy variables indicating intra-firm segment under- or over-performance on covariates described in Table 1 using the Segment Sale sample. In specification (4) we include a variable indicating the fraction of segments that are core segments to control for mechanical effects. The same covariates are included as in Table 11. t-statistics are in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	Low Operating Cash flow (1)	Highest Tobin's q (2)	Non-Core Segment (3)	Low Profitability (4)	Low Profit Margin (5)	Low Asset Turnover (6)
Rating downgrade (0, 1) = 1	0.104**	0.0977***	0.0209	0.0495	0.0193	0.0704*
	[2.262]	[2.651]	[0.457]	[1.063]	[0.431]	[1.760]
Rating upgrade (0, 1) = 1	0.167**	-0.0463	-0.0179	0.105	-0.0310	-0.0829
	[2.159]	[-0.688]	[-0.243]	[1.375]	[-0.391]	[-1.186]
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes
Rating FEs	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	908	1,308	989	957	1,046	1,185
Pseudo R^2	0.0478	0.0988	0.164	0.0384	0.0424	0.0491

Internet Appendix for The real effects of ratings actions: Evidence from corporate asset sales

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A. A simple model

In this section, we introduce a simple agency model in the context of the classic tradeoff theory to derive real effects of rating actions on asset allocation and capital structure decisions (proofs in Appendix A.5). This framework yields testable implications that largely align with the more informal implications obtained in Section 2.1, which steer our subsequent empirical analysis.

A.1. Model setup

Consider a multi-segment firm with a core and a non-core segment, indexed by c and nc, respectively. We also assume that the risk of both segments is identical, for expositional purposes. Finally, we assume that the firm is fully proficient in its core activity, but receives a discount $\delta \in (0,1)$ on the profits of its non-core activities, such that $IRR_c = \delta IRR_{nc}$, where IRR_x refers to the internal rate of returns for a segment x. The (expected) internal rate of return for the company (IRR) is given by

$$IRR = \frac{IC_c}{IC} IRR_c + \frac{IC_{nc}}{IC} IRR_{nc}, \tag{2}$$

where IC_x refers to segment-specific invested capital and $IC = IC_c + IC_{nc}$. The cost of capital r is defined as

$$r = r_a - \frac{D}{V} T r_d + \gamma \left(a\frac{D}{V} + bCR \right)^2, \tag{3}$$

where T is the corporate tax rate, r_a is the cost of capital for an unlevered firm, $\frac{D}{V}$ is the leverage ratio, CR is the rating level (where a higher value for CR corresponds to a worse rating), r_d is the pre-tax cost of debt, γ is a weighting parameter on financial distress costs, and a and b are weight parameters that determine the contribution of leverage and rating levels to financial distress costs, respectively. The second term in Eq. (3) refers to the tax shield and the third to financial distress costs. The firm creates economic value

$$EVA = (IRR - r)IC \tag{4}$$

per annum. We assume the following expected utility function of the CEO:

$$U_m = \beta IC_{nc} + EVA, \tag{5}$$

where β reflects private benefits of non-core operations (e.g., empire building). The EVA term reflects performance-based CEO incentives (normalized to 1 without loss of generality). For the moment, we assume transaction costs to be zero for asset sales and to be strictly positive for any other means of adjusting leverage.

A.2. CEO private value maximization

We derive the (privately) optimal financing structure as well as conditions under which the CEO invests in the non-core segment. Investing in the non-core segment may be privately optimal if private benefits from doing so exceed the associated loss in EVA.

Lemma 1. *Given a rating, the optimal leverage ratio is given by*

$$\left(\frac{D}{V}\right)^* = \frac{-2abCR + \gamma^{-1} T r_d}{2a^2}. \tag{6}$$

The CEO optimally holds on to the non-core segment if

$$r^* \leq IRR_{nc} + \beta, \tag{7}$$

where r^* is given by Eq. (3) evaluated at $\left(\frac{D}{V}\right)^*$.

Now assume that $IRR_{nc} < r^*$, but that the CEO holds on to segment nc (Eq. (7) is satisfied). Moreover, assume that owning segment nc would initially be irrelevant for the credit rating due to rating coarseness (see Goel and Thakor, 2015). Now exogenous economic adversity causes the rating to deteriorate to $CR' > CR$. Moreover, assume that this shock just puts the firm over a rating threshold, such that the negative rating action can be undone by selling segment nc to a better user. We can now derive whether the CEO optimally sells the non-core segment or reduces leverage in response of the changed rating.

Proposition 1. *In response to the rating deterioration, the CEO lowers leverage to*

$$\left(\frac{D}{V}\right)^{*'} = \frac{-2abCR' + \gamma^{-1}Tr_d}{2a^2}, \tag{8}$$

if

$$r^{*'} \leq \frac{(\beta + IRR_{nc})IC_{nc} + IC_c r^*}{IC_{nc} + IC'_c}, \tag{9}$$

where $r^{*'}$ is given by Eq. (3) evaluated at $\left(\frac{D}{V}\right)^{*'}$ and CR', and IC'_c is the invested capital of segments c after the leverage reduction. Otherwise the CEO sells segment nc and leaves leverage unchanged.

Proposition 1 shows that if Condition (9) is satisfied (i.e., the financial constraints channel dominates), negative rating actions lead to leverage reductions, which in this case are funded by asset sales.[1] Proposition 1 also shows that the negative rating action increases pressure to sell segment nc in two ways. Even if financial constraints are partially alleviated by reducing leverage, the reduced tax-deductability further impairs the profitability of segment nc, thereby tightening Condition (9). The reduced tax-deductability also gives rise to an intra-firm spillover effect by making segment c less profitable. These effects are broadly consistent with Boot et al. (2005), who shows theoretically that ratings can act as coordination mechanisms and that negative watch lists of CRAs serve as implicit monitoring contracts. By assumption, transaction costs are zero for asset sales and strictly positive for other ways of reducing leverage. Therefore, we get the following testable implications:

Implication 1. *Negative rating actions increase the likelihood of asset sales, irrespective of whether the financial constraints or discipline channel is at work.*

Implication 2. *Under the discipline channel, non-core, and poorly performing segments are more likely to be sold following negative rating actions.*

Under the assumption that companies truthfully report the purpose of their asset sales we immediately get another implication:

Implication 3. *Negative rating actions primarily induce asset sales with restructuring purpose when the discipline channel dominates and primarily induce asset sales with leverage reduction purpose when the financial constraints channel dominates.*

Naturally, selling segment nc to a better user is a net welfare improvement since the buyer can generate more economic surplus from it. If corporate taxes are distortive, tax shields reduce the distortion imposed by taxes. Selling segment nc therefore also positively contributes to welfare as it prevents the need to delever, which is associated with increased tax-based distortions.

[1] Empirically, Lang et al. (1995) show that asset sales are an important source of financing when firms are otherwise financially constrained and Officer (2007) show that firms who announce asset sales have lower bond ratings and are financially more constrained than firms that do not.

A.3. Transaction costs and other ways of reducing leverage

Now assume that there are two core segments, $c1$ and $c2$. Denote the asset liquidity of segments x by l_x. Liquidity l_x is defined as the proportional secondary market value (relative to true value), such that proportional transaction cost are given by $1 - l_x$. For tractability, assume that segments are infinitely divisible, such that the desired reduction in leverage can be exactly achieved. Also assume that there is an outside option to reduce leverage, such as a Seasoned Equity Offering (SEO), denoted by s with proportional transaction cost $1 - l_s$.

We now derive the main proposition of this section.

Proposition 2. *In response to the rating deterioration, the CEO lowers leverage to*

$$\left(\frac{D}{V}\right)^{**} = \min\left(\frac{D^*}{V}, \frac{-2abCR' + \gamma^{-1}(Tr_d + l_{k^*}^{-1} r_f V)}{2a^2}\right), \tag{10}$$

if

$$r^{**} \leq \frac{(\beta + IRR_{nc})IC_{nc} + r^* IC_c + IRR_c(IC'_c - IC_c) + (1 - l_{nc})IC_{nc} r_f}{IC_{nc} + IC'_c}, \tag{11}$$

where

$$r^{**} = r_a - \frac{D^{**}}{V} Tr_d + \frac{V r_f |\frac{D^{**}}{V} - \frac{D^*}{V}|}{l_x} + \gamma\left(a\frac{D^{**}}{V} + bCR'\right)^2, \tag{12}$$

$$k^* = \arg\max_{k \in \{c1, c2, nc, s\}} l_k, \tag{13}$$

V is firm value, r_f is the risk-free rate, and IC'_c is the invested capital of segments $c1$ and $c2$ together after the leverage reduction. Otherwise the CEO sells segment nc and leaves leverage unchanged.

Proposition 2 shows that if Condition (11) is violated, the non-core segment is sold and leverage is unaffected. An efficient way to achieve this is by doing a spinoff.

Implication 4. *The likelihood of spinoffs following negative rating actions is higher only under the discipline channel.*

Proposition 2 accounts for other ways to reduce leverage than asset sales. Hence, our focus on asset sales may only allow us to capture part of the real effect of rating changes. This leads to the following robustness result.

Robustness Result 1. *Transaction costs would prevent some assets sales from happening. This works against finding empirical evidence for Implication 1.*

A.4. Incorporating other characteristics

Now assume that financial distress costs are not only driven by ratings and leverage, but also by another characteristic X. To focus on the effect of X, we assume markets to be perfectly liquid and no outside options to be available. We have that

$$r = r_a - \frac{D}{V} Tr_d + \gamma\left(a\frac{D}{V} + bCR + cX\right)^2, \tag{14}$$

$$= r_a - \frac{D}{V} Tr_d + \gamma\left(\left(a\frac{D}{V}\right)^2 + b^2CR^2 + c^2X^2 + 2ab\frac{D}{V}CR + 2ac\frac{D}{V}X + 2bcCRX\right). \tag{15}$$

We derive the optimal leverage and the condition keep the non-core asset as before.

Lemma 2. *Given a rating, the optimal leverage ratio is given by*

$$\left(\frac{D}{V}\right)^{***} = \frac{-2a(bCR + cX) + \gamma^{-1}Tr_d}{2a^2}. \quad (16)$$

In this setting, the CEO optimally holds on to the non-core segment if

$$r^{***} \leq IRR_{nc} + \beta, \quad (17)$$

where r^{***} is given by (14) evaluated at $\left(\frac{D}{V}\right)^{***}$.

Other characteristics may be important as these could give rise to omitted variables concerns or interfere with the predictions derived before (confounding characteristics effects). We first address the omitted variable concern. Because ratings are coarse, a deterioration in X to a level X' affects the credit rating with strictly positive probability. It follows from (16) that under the financial constraints channel, the optimal leverage adjustment is larger if X' results in a rating deterioration. It follows from (17) that under the discipline channel, the sale of the under-performing segment is also more likely if X' results in a rating deterioration (as selling the non-core asset offsets a larger negative utility effect).

Proposition 3. *In response to the rating deterioration induced by X', the CEO lowers leverage to*

$$\left(\frac{D}{V}\right)^{***'} = \frac{-2a(b(CR + \Delta CRI_{ch}) + cX') + \gamma^{-1}Tr_d}{2a^2}, \quad (18)$$

if

$$r^{***'}_{I_{ch}=1} \leq \frac{(\beta + IRR_{nc})IC_{nc} + IC_c r^{***'}_{I_{ch}=0}}{IC_{nc} + IC'_c}, \quad (19)$$

where I_{ch} is an indicator function that equals 1 in case of a rating change and 0 otherwise, $\Delta CR = CR' - CR$, and $r^{***'}_{I_{ch}}$ is given by Eq. (14) evaluated at $\left(\frac{D}{V}\right)^{***'}$, X' and I_{ch}.

Hence, even if a rating change is induced by a change in credit quality that matters by itself for financial distress costs, there is an incremental effect on the likelihood of asset sales under both channels caused by the rating change.

Robustness Result 2. *Rating changes driven by deteriorations in credit risk induce asset sales over and above the effect of credit risk deteriorations on their own, irrespective of the channel at work.*

Next, we address the confounding characteristics effect. We show the effect of confounding characteristics for both the discipline channel and the financial constraints channel. In the financial distress component of Eq. (15), there is an interaction term with leverage, which influences leverage choices. As a result, asset sales that attenuate financial distress by improving other firm characteristics require smaller leverage decreases than asset sales that worsen financial distress through their effect on firm characteristics. Since leverage reductions are costly due to the loss of tax shields, the financial constraints channel predicts that assets are sold that aggravate financial distress and that assets are kept that mitigate it. Yet, the sale of the non-core segment involves similar effects (it may require an additional leverage reduction with associated costs). Yet, Eq. (15) also contains the interaction term $2bcCRX$. Under the financial constraints hypothesis, the rating is likely to stay poor, so the effect of this interaction term is large. By contrast, under the discipline channel the asset sale is likely to improve the rating, and hence, these characteristics are likely to matter less. We work this out formally below.

Proposition 4. *Assume that selling (from) segment k induces X to change to X_{-k}. In response to the rating deterioration, the CEO lowers leverage to*

$$\left(\frac{D}{V}\right)^{***''} = \frac{-2ab(CR + \Delta CRI_{ch} + cX_{-k^*}) + \gamma^{-1}Tr_d}{2a^2}, \tag{20}$$

if

$$r^{***''} \leq \frac{(\beta + IRR_{nc})IC_{nc} + r^{***}_{-nc}IC_c + IRR_c(IC'_c - IC_c)}{IC_{nc} + IC'_c}, \tag{21}$$

where $r^{***''}$ is given by Eq. (14) evaluated at $\left(\frac{D}{V}\right)^{***''}$ and X_{-k^*}, r^{***}_{-nc} is given by (14) evaluated at $\left(\frac{D}{V}\right)^{***''}$ with $k = nc$ and $I_{ch} = 0$, and

$$k^* = \arg\min_{k \in \{c1,c2,nc,s\}} X_{-k}. \tag{22}$$

Otherwise the CEO sells segment nc and changes leverage to a level that incorporates X_{-nc}.

Implication 5. *Confounding characteristics matter more for leverage-reducing assets sales than for asset sales aimed at improving asset configurations.*

In summary, our model predicts that negative rating actions lead to an increase in asset sales. It also generates several robustness results that mitigate concerns about omitted variables and alternative responses to negative rating actions in our subsequent empirical estimations. Finally, the model generates several predictions that can help us delineate between a discipline channel and a tightening of financial constraints channel.

A.5. Proofs

Proof of Lemma 1. Minimizing the discount rate r with respect to $\frac{D}{V}$ yields the first-order condition

$$0 = \gamma\left(2a^2\frac{D}{V} + 2abCR\right) - Tr_d. \tag{23}$$

Solving w.r.t. $\frac{D}{V}$ yields Eq. (6).

Now we denote Eq. (3) evaluated at $\left(\frac{D}{V}\right)^*$ as r^*. We can write the utility function of the CEO as

$$U_m = \beta IC_{nc} + (IRR_{nc} - r^*)IC_{nc} + (IRR_c - r^*)IC_c, \tag{24}$$
$$= (\beta + (IRR_{nc} - r^*))IC_{nc} + (IRR_c - r^*)IC_c. \tag{25}$$

This is maximized by holding on to segment nc when

$$(\beta + (IRR_{nc} - r^*)) \geq 0, \tag{26}$$
$$r^* \leq IRR_{nc} + \beta. \tag{27}$$

□

Proof of Proposition 1. If segment nc is held on to, Lemma 1 provides the new optimal leverage ratio in Eq. (8). If segment nc is sold, optimal leverage is unchanged as the rating is reverted to level CR in that case. To find the optimal solution, we need to compare CEO utility when selling with keeping segment nc. We have that keeping nc is optimal when

$$(IRR_c - r^*)IC_c \leq (\beta + IRR_{nc} - r^{*'})IC_{nc} + (IRR_c - r^{*'})IC_c', \Rightarrow \tag{28}$$

$$r^{*'}(IC_{nc} + IC_c') \leq (\beta + IRR_{nc})IC_{nc} + r^* IC_c, \Rightarrow \tag{29}$$

$$r^{*'} \leq \frac{(\beta + IRR_{nc})IC_{nc} + IC_c r^*}{IC_{nc} + IC_c'}. \tag{30}$$

□

Proof of Proposition 2. We can transform one-off transaction costs to a perpetuity. Since there is no uncertainty about these costs, the discount rate is the risk-free rate. We have that

$$\frac{(D^{**} - D^*)}{l_x} = \frac{r_f(D^{**} - D^*)}{l_x r_f} = \frac{V r_f(\frac{D^{**}}{V} - \frac{D^*}{V})}{l_x r_f}, \tag{31}$$

$$(1 - l_{nc})IC_{nc} = \frac{(1 - l_{nc})r_f IC_{nc}}{r_f}. \tag{32}$$

Hence, per period transaction costs for reducing leverage are $\frac{V r_f(\frac{D^{**}}{V} - \frac{D^*}{V})}{l_x}$ and for selling segment nc they equal $(1 - l_{nc})r_f IC_{nc}$. We now have that the cost of capital accounting for transaction costs of adjusting leverage is given as a function of leverage by

$$r = r_a - \frac{D}{V}Tr_d + \frac{V r_f|\frac{D}{V} - \frac{D^*}{V}|}{l_x} + \gamma\left(a\frac{D}{V} + bCR'\right)^2. \tag{33}$$

Imposing a first-order condition and minimizing w.r.t. $\frac{D}{V}$ yields Eq. (10) for a given l_x. Since r is decreasing in l_x, r is minimized by maximizing l_x, giving rise to Eq. (13). It is optimal for the CEO to keep segment nc when

$$(IRR_c - r^*)IC_c - (1 - l_{nc})r_f IC_{nc} \leq (\beta + IRR_{nc} - r^{**})IC_{nc} + (IRR_c - r^{**})IC_c'. \tag{34}$$

Re-writing yields Eq. (11). □

Proof of Lemma 2. Minimizing the discount rate r with respect to $\frac{D}{V}$ yields the first-order condition

$$0 = \gamma\left(2a^2\frac{D}{V} + 2a(bCR + cX)\right) - Tr_d. \tag{35}$$

Solving w.r.t. $\frac{D}{V}$ yields Eq. (16).

Now we denote Eq. (14) evaluated at $\left(\frac{D}{V}\right)^{***}$ as r^{***}. The utility function of the CEO is given by

$$U_m = \beta IC_{nc} + (IRR_{nc} - r^{***})IC_{nc} + (IRR_c - r^{***})IC_c, \tag{36}$$

$$= (\beta + (IRR_{nc} - r^{***}))IC_{nc} + (IRR_c - r^{***})IC_c. \tag{37}$$

This is maximized by holding on to segment nc when

$$(\beta + (IRR_{nc} - r^{***})) \geq 0, \tag{38}$$

$$r^{***} \leq IRR_{nc} + \beta. \tag{39}$$

□

Proof of Proposition 3. If segment nc is held on to, Lemma 2 provides the new optimal leverage ratio in Eq. (16) given rating level CR'. If segment nc is sold, optimal leverage Lemma 2 provides the new optimal leverage ratio in Eq. (16) with rating level CR as the rating is reverted to level CR in that case. To find the optimal action for the CEO, we need to compare CEO utility when selling with keeping segment nc. We have that keeping segment nc is optimal when

$$(IRR_c - r^{***'}_{I_{ch}=0})IC_c \leq (\beta + IRR_{nc} - r^{***'}_{I_{ch}=1})IC_{nc} + (IRR_c - r^{***'}_{I_{ch}=1})IC'_c, \Rightarrow \tag{40}$$

$$r^{***'}_{I_{ch}=1}(IC_{nc} + IC'_c) \leq (\beta + IRR_{nc})IC_{nc} + r^{***'}_{I_{ch}=0}IC_c, \Rightarrow \tag{41}$$

$$r^{***'}_{I_{ch}=1} \leq \frac{(\beta + IRR_{nc})IC_{nc} + IC_c r^{***'}_{I_{ch}=0}}{IC_{nc} + IC'_c}. \tag{42}$$

□

Proof of Proposition 4. The optimal leverage with characteristics X_{-k} is given by Lemma 2. Since a smaller X_{-k} leads to a lower discount rate and therefore higher EVA, this is optimized by choosing the k that minimizes X_{-k}. This gives rise to Eq. (22). When segment nc is sold, the optimal new leverage is given by Lemma 2.

It is then optimal for the CEO to keep segment nc when

$$(IRR_c - r^{***}_{-nc})IC_c \leq (\beta + IRR_{nc} - r^{***"})IC_{nc} + (IRR_c - r^{***"})IC'_c. \tag{43}$$

Re-writing yields Eq. (11). □

B. Supplementary Tables

Table B.1: Cox hazard regressions using distance-weighted credit rating downgrades
The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis over the entire sample. In each specification, we weigh all observations of a firm by the sample-period average of the absolute inverse distance of that firm's rating relative to the IG-HY boundary (in notches). Non-rated firms receive the minimum weight among the rated firms. We re-scale all weights to get a sample average weight of 1. In specifications (1) and (2) this is done symmetrically. In specifications (3) and (4), weights on the HY side of the spectrum are multiplied by 2 and in specifications (5) and (6) these are multiplied by 5. The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	(1)	(2)	(3)	(4)	(5)	(6)
Rating downgrade (0,1)	0.433***	0.393***	0.436***	0.402***	0.435***	0.410***
	[11.16]	[9.91]	[11.28]	[10.17]	[11.31]	[10.38]
Rating upgrade (0,1)	-0.220***	-0.175***	-0.240***	-0.185***	-0.257***	-0.195***
	[-4.11]	[-3.22]	[-4.45]	[-3.40]	[-4.77]	[-3.58]
Other Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	Yes	No	Yes	No	Yes
Time FEs	No	Yes	No	Yes	No	Yes
Industry covariates	No	Yes	No	Yes	No	Yes
Ratio HY vs IG weights	1	1	2	2	5	5
Number of observations	634,319	624,012	640,069	629,820	646,724	636,542
Pseudo R^2	0.038	0.076	0.036	0.076	0.034	0.077

Table B.2: Hazard rate regressions by purpose including watchlist placements

The table presents coefficient estimates for the hazard rates of asset sale events by self-reported purposes: Relaxing credit constraints (Relax), Discipline, Ambiguous, or spinoff. Specification (1) to (4) are Cox proportional hazard regressions on a monthly basis over the entire sample. These specifications assume that multiple events can happen to a subject and that hazard rates are unaffected by such events. Specifications (5) to (8) present sub-hazard estimates for asset sales using the competing risk model by Fine and Gray (1999), where subjects leave the sample after the first event takes place. The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	Multiple events with others as censored				Competing risk (1st only) subhazard			
	Relax	Disicpline	Ambiguous	Spinoff	Relax	Discipline	Ambiguous	Spinoff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rating downgrade (0, 1)	0.333*	0.311*	0.280***	0.123	0.655	-0.312	0.335***	-0.864
	[1.71]	[1.78]	[3.82]	[0.41]	[1.60]	[-0.76]	[2.84]	[-1.15]
Rating upgrade (0, 1)	-0.167	-0.579*	-0.206**	-0.710*	0.716	-0.888	0.0751	-0.725
	[-0.51]	[-1.94]	[-2.29]	[-1.90]	[1.12]	[-1.19]	[0.50]	[-1.30]
Negative watchlist (0,1)	0.779***	0.422**	0.322***	0.275	0.831**	0.458	0.193	-0.913
	[3.81]	[2.00]	[3.58]	[0.81]	[2.13]	[1.08]	[1.27]	[-0.86]
Positive watchlist (0,1)	-0.357	-0.0644	0.284*	-1.133	-13.00***	-0.168	0.393	-0.0239
	[-0.71]	[-0.14]	[1.88]	[-1.16]	[-35.59]	[-0.16]	[1.57]	[-0.02]
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	No	No	No	No	No	No	No
Time FEs	No	No	No	No	No	No	No	No
Industry covariates	No	No	No	No	No	No	No	No
Number of observations	606,503	606,503	606,503	606,503	470,476	470,476	470,476	470,476
Pseudo R^2	0.119	0.045	0.054	0.058	-	-	-	-
Number of clusters	6,550	6,550	6,550	6,550	6,482	6,482	6,482	6,482

Table B.3: Inter-firm segment performance analysis including watchlist placements
The table presents average marginal effects of logit regressions of dummy variables equal to 1 for inter-firm segment underperformance and 0 otherwise, in comparison to medians of peer segments in other multi-segment firms with matching 2-digit SIC codes for Profitability, Profit Margin, Asset Turnover, Operating Cash Flow, and Net CF using the Segment Sale sample. Negative (Positive) watchlist (0,1) refers to a dummy variable that equals 1 in case the firm was put on negative (positive) watch during the 12 months preceding the observation. t-statistics are in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level. Variable descriptions are in Table 1.

VARIABLES	Profitability (1)	Profit Margin (2)	Asset Turnover (3)	Operating CF (4)	Net CF (5)
Rating downgrade (0, 1) = 1	-0.00894	-0.0383	0.0468	0.0315	0.0556
	[-0.195]	[-0.861]	[1.005]	[0.662]	[1.138]
Rating upgrade (0, 1) = 1	-0.170**	-0.110	0.00760	-0.103	-0.128*
	[-2.522]	[-1.616]	[0.099]	[-1.336]	[-1.819]
Negative watchlist (0,1)	0.0338	0.0384	-0.0784	-0.0273	-0.0530
	[0.665]	[0.761]	[-1.597]	[-0.537]	[-1.013]
Positive watchlist (0,1)	0.0785	-0.00547	0.116	-0.0410	0.0569
	[0.790]	[-0.056]	[1.342]	[-0.407]	[0.603]
Other covariates	Yes	Yes	Yes	Yes	Yes
Rating FEs	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes
Number of observations	874	978	1,113	853	801
Pseudo R^2	0.125	0.109	0.0859	0.150	0.162

Table B.4: Intra-firm segment performance analysis including watchlist placements
The table presents average marginal effects of logit regressions of dummy variables indicating intra-firm segment under- or over-performance on covariates described in Table 1 using the Segment Sale sample. In specification (4) we include a variable indicating the fraction of segments that are core segments to correct for mechanical effects. The same covariates are included as in Table B.3. t-statistics are in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

VARIABLES	Low Operating Cash flow (1)	Highest Tobin's q (2)	Non-Core Segment (3)	Low Profitability (4)	Low Profit Margin (5)	Low Asset Turnover (6)
Rating downgrade (0, 1) = 1	0.139***	0.112***	0.0345	0.0766	-0.0174	0.0845**
	[2.861]	[2.817]	[0.697]	[1.545]	[-0.350]	[1.982]
Rating upgrade (0, 1) = 1	0.204***	-0.0484	-0.0318	0.136*	0.00593	-0.0900
	[2.754]	[-0.712]	[-0.430]	[1.790]	[0.074]	[-1.270]
Negative watchlist (0,1)	-0.118**	-0.0419	-0.0240	-0.0917	0.0823	-0.0373
	[-2.011]	[-0.931]	[-0.471]	[-1.602]	[1.493]	[-0.774]
Positive watchlist (0,1)	-0.281**	-0.00996	0.100	-0.214*	-0.206*	0.0486
	[-2.413]	[-0.124]	[1.106]	[-1.912]	[-1.953]	[0.542]
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes
Rating FEs	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	908	1,308	989	957	1,046	1,185
Pseudo R^2	0.0554	0.0993	0.165	0.0430	0.0468	0.0497

Table B.5: Hazard rate regressions by purpose for firms with young CEOs and CEOs with short tenures
The table presents coefficient estimates for the hazard rates of asset sale events by self-reported purposes: Relaxing credit constraints (Relax) or Discipline on a monthly basis over the entire sample. These specifications assume that multiple events can happen to a subject and that hazard rates are unaffected by such events. Columns (1) and (3) are restricted to firms with young CEOs (below the median of 56 years old). Columns (2) and (4) are restricted to firms with short tenures (below the median of 5.91 years). The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	Relax		Discipline	
	Young (1)	Short tenure (2)	Young (3)	Short tenure (4)
Rating downgrade (0, 1)	0.310	0.486*	0.139	0.356
	[0.92]	[1.67]	[0.54]	[1.56]
Rating upgrade (0, 1)	0.0619	-0.491	-0.321	-0.505
	[0.15]	[-0.87]	[-0.77]	[-1.32]
Other covariates	Yes	Yes	Yes	Yes
Industry FEs	No	No	No	No
Time FEs	No	No	No	No
Industry covariates	No	No	No	No
Number of observations	135,819	135,819	135,819	147,467
Pseudo R^2	0.169	0.11	0.077	0.065
Number of clusters	2,057	2,057	2,057	2,082

Table B.6: Payout regressions

The table presents average marginal effects logit regressions of reductions in total payout to equity holders on all explanatory variables in Bliss et al. (2015) as well as same year and previous year rating actions at the firm/year level. See the Appendix of Bliss et al. (2015) for variable definitions. t-statistics are in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variable	Payout reduction	
	(1)	(2)
Rating downgrade (0,1)	0.0575***	0.0566***
	[9.066]	[8.899]
Rating downgrade (t-1)	0.0446***	0.0455***
	[6.623]	[6.739]
Rating upgrade (0,1)		-0.0184*
		[-1.827]
Rating upgrade (t-1)		0.0123
		[1.429]
Age (t-1)	0.000142	0.000142
	[0.881]	[0.884]
Log assets (t-1)	-0.00588***	-0.00583***
	[-3.587]	[-3.532]
Loss making (t-1)	-0.0127***	-0.0127***
	[-7.349]	[-7.300]
Investment ratio (t-1)	0.0228	0.0217
	[0.836]	[0.796]
Market Leverage (t-1)	-0.000456	-0.000430
	[-0.421]	[-0.397]
Cash flow ratio	-0.330***	-0.329***
	[-14.201]	[-14.151]
CashRatio (t-1)	-0.128***	-0.127***
	[-7.365]	[-7.361]
Tobin's q	-0.109***	-0.109***
	[-18.842]	[-18.802]
Volatility (t-1)	-0.0635**	-0.0622**
	[-2.089]	[-2.046]
Cash flow volatility	-0.102***	-0.101***
	[-3.081]	[-3.074]
Payout Ratio (t-1)	3.956***	3.952***
	[49.832]	[49.727]
Payout Reduction (t-1)	0.0444***	0.0444***
	[11.250]	[11.256]
Industry FEs	Yes	Yes
Year FEs	Yes	Yes
Number of observations	40,420	40,420
Pseudo R^2	0.286	0.286
Number of clusters	5,157	5,157

Table B.7: Post Regulation FD removal sample

The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis over the sample period starting from September 2010 (after which CRAs were not exempted from Regulation FD anymore). The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	(1)	(2)	(3)	(4)	(5)
Rating downgrade (0,1)	0.470***	0.284*	0.292*	0.294**	0.310**
	[3.06]	[1.87]	[1.92]	[1.99]	[2.05]
Rating upgrade (0,1)	-0.592***	-0.443**	-0.443**	-0.417*	-0.353
	[-2.95]	[-2.01]	[-2.00]	[-1.87]	[-1.62]
Other covariates	No	Yes	Yes	Yes	Yes
Industry FEs	No	No	No	Yes	Yes
Time FEs	No	No	Yes	No	Yes
Industry covariates	No	No	No	No	Yes
Number of observations	132,674	97,123	97,123	97,100	94,944
Pseudo R^2	0.042	0.074	0.082	0.098	0.109
Number of clusters	3,241	2,469	2,469	2,468	2,437

Table B.8: Cox hazard regressions and acquisition activity

The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis over the entire sample. Acquisition spending is the monthly ratio of aggregated deal values for firm i from month t–24 to t–6 to the most recent book value of assets prior to month t. Acquisition activity is an indicator variable equal to one if Acquisition spending > 0 and zero otherwise. We define Cash (equity) acquisition spending as the aggregate dollars spent with cash (equity) on acquisitions for firm i from month t–24 to t–6 to the most recent book value of assets prior to month t. All acquisition activity and spending is from SDC. Models (1) to (5) utilize the full sample of observations. Model (6) is for the sub-sample where Acquisition activity equals one and model (7) is for the sub-sample where Acquisition activity equals zero. The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

						Acquisition Activity	
						Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rating downgrade (0,1)	0.360***	0.335***	0.352***	0.352***	0.344***	0.333***	0.317***
	[5.61]	[5.18]	[5.52]	[5.48]	[5.40]	[3.78]	[3.76]
Rating upgrade (0,1)	-0.161*	-0.190**	-0.158*	-0.167**	-0.163**	-0.231**	-0.115
	[-1.95]	[-2.27]	[-1.91]	[-2.03]	[-1.98]	[-1.97]	[-1.02]
Recent M&A Activity	0.926***		0.866***	0.878***	0.817***		
	[19.27]		[15.38]	[17.81]	[14.37]		
Aggregate value Recent M&A Activity		2.476***					
		[16.95]					
Aggregate cash in Recent M&A Activity			0.747**		0.751**		
			[2.49]		[2.56]		
Aggregate stock in Recent M&A Activity				1.534***	1.540***		
				[4.26]	[4.34]		
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	No	No	No	No	No	No
Time FEs	No	No	No	No	No	No	No
Industry covariates	No	No	No	No	No	No	No
Number of observations	587,043	587,043	587,043	587,043	587,043	134,596	452,447
Pseudo R^2	0.066	0.061	0.067	0.067	0.067	0.083	0.053
Number of clusters	6,406	6,406	6,406	6,406	6,406	3,305	6,087

Table B.9: Cox hazard regressions excluding LBOs
The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis over the entire sample. We exclude asset sale events that are LBO deals (model (1)), where the seller is an LBO firm (model (2)), where the seller parent is an LBO firm (model (3)), and where the acquirer is an LBO firm (model (4)). The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	No LBO (1)	Target no LBO (2)	Target parent no LBO (3)	Acquirer no LBO (4)
Rating downgrade (0, 1)	0.391***	0.392***	0.396***	0.399***
	[5.71]	[5.92]	[5.97]	[5.94]
Rating upgrade (0, 1)	-0.252***	-0.207**	-0.216**	-0.209**
	[-2.83]	[-2.46]	[-2.54]	[-2.44]
Other covariates	Yes	Yes	Yes	Yes
Industry FEs	No	No	No	No
Time FEs	No	No	No	No
Industry covariates	No	No	No	No
Number of observations	587,043	587,043	587,043	587,043
Pseudo R^2	0.055	0.055	0.055	0.056
Number of clusters	6,406	6,406	6,406	6,406

Table B.10: Cox hazard regressions for failed asset sales and asset sales by purpose
The table presents coefficient estimates for the hazard rates of failed asset sale events by self-reported purposes: Relaxing credit constraints (Relax), Discipline, or Ambiguous. Specification (1) presents Cox proportional hazard regressions on a monthly basis over the entire sample for never completed deals that were rumored, intended, pending, were seeking a buyer or only partially completed (failed deals). This specification implicitly assumes that multiple events can happen to a subject and that hazard rates are unaffected by past events. Specifications (2) to (5) present sub-hazard estimates for failed deals and asset sales by purpose using the competing risk model by Fine and Gray (1999). The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

		Competing risk (1st only) subhazard			
	Failed asset sale (1)	Relax (2)	Discipline (3)	Ambiguous (4)	Failed asset sale (5)
Rating downgrade (0, 1)	0.301***	0.830	-0.336	0.381***	0.223
	[3.03]	[1.56]	[-0.70]	[3.02]	[1.26]
Rating upgrade (0, 1)	-0.313**	0.998	-0.777	0.204	-0.0639
	[-2.23]	[1.49]	[-1.07]	[1.32]	[-0.27]
Other covariates	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	No	No	No	No
Time FEs	No	No	No	No	No
Industry covariates	No	No	No	No	No
Number of observations	606,620	444,579	444,579	444,579	444,579
Pseudo R^2	0.036	-	-	-	-
Number of clusters	6,550	6,467	6,467	6,467	6,467

Table B.11: Cox hazard regression excluding anticipated deals

The table presents coefficient estimates for the hazard rates of asset sale events using Cox proportional hazard regressions on a monthly basis over the entire sample. We exclude asset sale events that were rumored before completion (model (1)), that had announced that they were seeking a buyer prior to completion (model (2)), and both (model (3)). The same covariates are included as in Table 4. We cluster standard errors by firm and report t-statistics in brackets. Respectively, *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

	(1)	(2)	(3)
	Excluding Rumors	Excluding seeking buyer	Excluding both
Rating downgrade (0, 1)	0.379***	0.357***	0.355***
	[5.81]	[5.32]	[5.23]
Rating upgrade (0, 1)	-0.188**	-0.145	-0.158*
	[-2.19]	[-1.63]	[-1.76]
Other covariates	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes
Industry covariates	Yes	Yes	Yes
Number of observations	578,306	578,306	578,306
Pseudo R^2	0.084	0.082	0.081
Number of clusters	6,364	6,364	6,364

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