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# Pre-LBO Credit Market Conditions and Post-LBO Target Behavior

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**Abstract**

In the context of leveraged buyouts (LBOs), this paper empirically studies the relation between pre-buyout credit market conditions and the post-buyout behavior of target companies, employing a supervisory dataset to overcome limited data availability for post-buyout target financial information. We propose an LBO-specific measure of (changes of) credit market conditions—the short-term (6-month) change of credit spreads leading up to buyout close. Using this proposed measure, we show that loosening pre-LBO credit market conditions, which are related to higher buyout leverage consistent with the literature, are associated with poor post-LBO (operating) performance of the target company. These results support the narrative of agency costs of debt such as risk shifting and debt overhang but are inconsistent with theories of disciplinary effects of debt. We provide further evidence supportive of the theories of agency costs of debt and some results favorable to the risk shifting story.

JEL: G00, G12, G24, G34.

Keywords: Private equity, leveraged buyout, credit market condition, agency cost.

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# 1 Introduction

Leveraged buyouts (LBOs) are a type of transaction employed by private equity (PE) firms when acquiring companies. Since LBOs involve significant usage of external debt financing, research often finds that credit market conditions and buyout transaction outcomes are related. Yet, there are few studies on how pre-LBO credit market conditions affect the post-LBO behavior of target companies acquired by PE firms, partly due to limited available systematic data for PE-backed companies, which are private. We fill this gap by utilizing a supervisory dataset, FR Y-14Q, which provides financial information for private firms. As far as the authors are aware, this paper is the first study that uses post-buyout financial information for target companies to investigate the impact of pre-buyout credit market conditions.

Theoretical studies provide divergent predictions on how credit market conditions influence the post-buyout behavior of the target companies. Jensen (1989) argues that a large amount of debt involved in LBOs has a disciplining effect on the acquired company, based on the prior literature including his own work. From this perspective, loose credit market conditions allow a target company to take more external debt and, consequently, disincentivize the managers from wasting excess cash (Jensen (1986)) and/or provide an incentive structure that better aligns with improving efficiency for the managers and the PE sponsor (Jensen and Meckling (1976)). On the contrary, theories of agency costs of debt, such as risk shifting (Jensen and Meckling (1976)) and debt overhang (Myers (1977)), suggest that excessive debt issuance under loose credit market conditions may lead to inefficiency in terms of the value of the target company.

To study the relation between pre-buyout credit market conditions and post-buyout target behavior, this paper proposes an LBO-specific measure of pre-buyout credit market conditions—the short-term (6-month) change of credit spreads leading up to a buyout's

close. Our proposed measure has two additional advantages over the level of spreads at a buyout's close, which is often used in the literature (e.g., Axelson, Jenkinson, Strömberg, and Weisbach (2013)). First, as opposed to the level of credit spreads, the short-term spread change is to a large extent uncorrelated with the valuation of the target company. Second, the short-term spread change is unlikely to be correlated with possible selection biases while the level of credit spreads may. Empirical evidence to support these assertions is provided.

We first examine the relation between the proposed measure of credit market conditions (changes of), the short-term change of credit spreads leading up to buyout close, and buyout leverage, defined by the amount of buyout debt to the target enterprise value. Consistent with the literature (e.g., Axelson et al. (2013)), credit market conditions are positively related to buyout leverage: the short-term change of credit spreads leading up to LBO close is negatively associated with buyout leverage. Furthermore, the short-term spread change contributes to the vast majority of the effect of the level of spread at buyout on buyout leverage.

Then this paper studies how the proposed measure—6-month credit spread changes leading up to buyout close—affects the post-LBO behavior of target companies, utilizing FR Y-14Q. The supervisory data provides financial information, particularly financial statement variables, for firms that have outstanding loans from large banks in the United States. Combined with buyout transactions in S&P Capital IQ (CIQ), our main sample covers slightly under 900 LBO deals denominated in United States dollars completed between 2011 and 2022—including most periods after the Global Financial Crisis (GFC)—for target companies in the United States. A clear advantage of using this dataset is that the vast majority of the sample is comprised of private-to-private LBO transactions, consistent with the composition of LBO deals in the United States.[1] Our sample is likely more representative of the US LBO

[1] Bain & Company (2020) shows that public-to-private buyout transactions only make up a small portion of all buyout transactions since 2005 in North America and globally.

population compared with samples of previous studies primarily involving public-to-private LBOs.

Employing a (version of) difference-in-difference estimation approach, we find that pre-LBO narrowing of credit spreads (loosening credit market conditions) leads to poor post-LBO target (operating) performance. Target performance is measured by nine different variables to address possible different opinions on what variable best measures target performance: three different cash flows variables (net income, operating income, and EBITDA) scaled by three different denominators (net sales, total assets, and book equity). These results are not supportive of the theories of disciplinary effects of debt but are consistent with the narrative of agency costs of debt; LBO target companies acquired after narrowing credit spreads take on more debt than those after widening credit spreads, yet they exhibit worse post-buyout performance. Therefore, our findings suggest that agency problems associated with excessive debt dominate the disciplining effect of debt, on average.

Lastly, we pursue further evidence that supports the narrative of agency costs of debt. We find that narrowing credit spreads leading up to buyout close are associated with high post-buyout probabilities of default assigned by banks to target companies. This result is consistent with the implication of theories of agency costs of debt that highly levered firms make suboptimal decisions that also harm debtholders' value. Next, we examine post-buyout covenant compliance of target companies and find favorable evidence to the risk-shifting theory: pre-buyout credit spread tightening is associated with more covenant breaches and less covenant compliance after waivers/amendments are granted.

This paper's findings suggest that the degree of agency problems between PE firms and creditors to their portfolio companies varies considerably over the credit cycle. A caveat is warranted that our results are not necessarily inconsistent with earlier findings that LBO target companies experience post-buyout improvements in their performance as Jensen (1989) argues. However, our findings indicate that such benefits of LBOs on acquired companies

may be undermined by loose credit market conditions to a notable extent. These implications might be useful to lenders, institutional investors, regulators and policymakers in relevant areas.

## Related Literature

There has been a great amount of research that supports a positive role by PE firms in improving the efficiency of their portfolio companies. A seminal paper by Jensen (1989) particularly advocates LBOs in that their "management, compensation, and financial structures" help to reduce inefficiency by better aligning the incentive structure for the management and the PE sponsor with such behavior.[2] Empirically, many studies show that LBOs lead to efficiency enhancement. For operating performance, Kaplan (1989) shows that management buyouts (MBOs)—a type of LBOs—in the 80s, during the first buyout wave in the United States, improved operating performance of public target companies by a large magnitude. Yet, Guo, Hotchkiss, and Song (2011) provides evidence that such improvements in operating performance largely disappeared for buyouts between 1990 and 2006. Regarding productivity, Lichtenberg and Siegel (1990) shows that plant productivity increased for buyouts (particularly MBOs) during 1983 to 1986. Similarly, Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014) finds productivity gains at target firms, primarily through exits from less productive establishments and entries to more productive ones. Bernstein, Lerner, Sorensen, and Strömberg (2017) provides some evidence that industries with more PE investment exhibit higher total production growth while being less exposed to aggregate shocks. Including some of above studies, there are many papers that find PE involvement improves other aspects of the portfolio companies: Bernstein and Sheen (2016), Bellon (2020), Cohn, Nestoriak, and Wardlaw (2021), and Fracassi, Previtero, and Sheen

[2] Jensen (1989) argues that there is a possibility of risk shifting but reputation concerns for PE firms would alleviate such issues.

(2022), among others. Relatively fewer studies find negative impacts of PE sponsorship, such as Eaton, Howell, and Yannelis (2019) and Gupta, Howell, Yannelis, and Gupta (2020), among others.

A tremendous amount of literature has been devoted to studying agency costs of debt since Jensen and Meckling (1976). While it would be almost an impossible task to summarize the entire literature here, the theory of debt overhang by Myers (1977) is particularly relevant as LBOs involve intensive use of debt. One empirical challenge to those theories of agency costs of debt is that capital structure decisions are often highly endogenous. Surprisingly yet, there are not many studies on agency costs of debt in the context of LBOs despite their significant usage of debt; Axelson et al. (2013) is one of such studies.

Including Axelson et al. (2013), a few studies examine the relation between credit market conditions and LBO outcomes. Kaplan and Stein (1993) is an early study on MBOs that investigates the evolution of buyout pricing and financial structure over time during the first buyout wave in the 80s, providing some evidence for the "overheated buyout market." Axelson et al. (2013) shows that the level of credit spreads at buyout is the primary determinant of buyout leverage, which is associated with higher buyout pricing and lower buyout fund returns. Their study attempts to link those results to agency problems. Davis, Haltiwanger, Handley, Lerner, Lipsius, and Miranda (2021), while covering quite broad types of heterogeneity on the effect of buyouts, finds that post-buyout productivity gains at target firms are (much) larger for deals closed under tight credit market conditions. However, Haddad, Loualiche, and Plosser (2017) provides some evidence that "the equity risk premium is the primary determinant of buyout activity rather than credit-specific conditions," displaying that the impact of credit market conditions on LBO outcomes is likely confounded by comovements between credit and equity market conditions.

A related topic is PE fund performance, studies on which include an influential paper by Kaplan and Schoar (2005), among many others. Buyout fund returns depend on the fund's

equity contribution, dividend distributions, the exit value of the target portfolio companies, and the timing of their occurrence—returns to equity for individual buyout deals drive the fund returns. By contrast, our focus is on post-buyout (operating) performance of target companies, the bottom line of the company from the perspective of firm value maximization. Those two are connected but not perfectly aligned; for example, Guo et al. (2011) shows that the average deal-level (equity) return was large for buyouts completed in the US between 1990 and 2006, while operating performance gains relative to benchmark firms were small.[3] Since target pricing (both at buyout and exit) is closely related to deal-level equity returns, buyout fund performance is affected by fluctuations in the overall market valuation. Thus, although buyout fund performance captures target "fundamentals" to a certain extent, the measure is likely confounded with unrelated factors influencing the target's valuation. By focusing on target (operating) performance, our study is relatively free of such problems.

# 2 Data and Sample Description

This section covers data sets used in this paper and a brief summary of the characteristics of the sample.

## 2.1 Data

In this subsection, a list of data sets that are used in this paper is provided. Some of the data cleaning procedures are described, but most details can be found in Appendix Section A.2.

[3]Yet, Acharya, Gottschalg, Hahn, and Kehoe (2012) finds that buyout deals in Western Europe during a similar period exhibited both notable operating performance gains and abnormal (positive) deal IRR, emphasizing possible international differences.

### Credit Market Conditions

The main credit market conditions metric in this paper is credit spreads for high yield bonds, the ICE BofA US High Yield Index Option-Adjusted Spread (HY OAS) from ICE Data Indices. The series begins at the end of 1996, and, as a result, our empirical analysis does not cover periods before 1997. For robustness tests, we use a couple other measures including the ICE BofA US Corporate Index Option-Adjusted Spread (Corp OAS) from ICE and the loan spread-to-maturity (Loan STM) from PitchBook LCD.

### LBO Sample Construction

We primarily use S&P CIQ, specifically the M&A package, to compile a sample of roughly 26,000 LBO transactions by filtering on: (i) completed transactions (i.e., closed deals), (ii) transaction feature flagged as LBO, (iii) denominated in US dollars (USD), and (iv) US target companies. Since our focus is on the US corporate credit market, it is reasonable to filter for buyout deals denominated in the USD and those with US target companies—deals denominated by other currencies or those with non-US target companies are more likely to obtain financing outside the United States. The sample coverage starts to become comprehensive for observations around the late 1990s to the early 2000s, although the data goes as far as back to 1967.

CIQ provides dates on which the transactions were announced, the dates on which the transactions were finalized (i.e., closed date), as well as other buyout-related dates (e.g., deal signing date). Many of these "other" dates are missing from the database and some that are provided are often inaccurate. CIQ also provides deal terms such as transaction value, (implied) enterprise value, financial ratios (e.g., enterprise value/EBITDA). These deal terms are often not disclosed, particularly for private-to-private deals where only a small portion of the observations have such information.[4]

[4]For transaction value and enterprise value, roughly a quarter of the sample has the information. For the

### Pre-LBO Target Stock Prices

We study how pre-buyout target stock prices respond to credit spread changes in Appendix Section B.2. For this investigation, the LBO transaction sample from CIQ is matched to CRSP. By definition, only public-to-private transactions are matched to CRSP to obtain the target's pre-LBO stock prices. Using the GVKEY table provided by CIQ and the CRSP-Compustat Linking Table, 714 LBO transactions between 1997 and 2022 are matched to CRSP.

### LBO Debt Financing

In Section 3.2, this paper examines the relation between credit market conditions and buyout leverage, defined by the amount of buyout debt to the target enterprise value. The amount of buyout debt is not readily available in CIQ, from which the target enterprise value is taken. Thus, to obtain the issuance amounts of buyout debt for corporate bonds and syndicated loans, we utilize Mergent Fixed Income Securities Database (FISD) and Refinitiv DealScan, respectively, both of which are widely used in the literature and known to be comprehensive.

### FR Y-14Q Data

By definition, LBO target companies are private after buyout and, as a result, do not publicly disclose their financial information, including their balance sheet and cash flow variables. Although private companies occasionally reveal such information, there are limited commercially available databases that systematically covers their financial information. Due to this data limitation, previous studies on target (operating) performance either rely on proprietary data (e.g., Acharya et al. (2012)) or focus on a subset of target companies

financial ratios, less than 10% (close to 6%) of the sample has the information.

that disclose financial information (e.g., Guo et al. (2011)).[5] A possible concern with these approaches is that the sample may not be representative of the LBO population—for the former, to the extent that the data provider is not representative in the buyout market, and for the latter, to the extent that the disclosing target companies are different from non-disclosing ones.

To overcome such data limitation, we obtain LBO target companies' financial statement information from a confidential supervisory dataset, the FR Y-14Q, particularly Schedule H.1. The FR Y-14Q collects detailed data on banks portfolio holdings for annual stress tests; reporting banks are comprised of U.S. bank holding companies (BHCs) and U.S. intermediate holding companies (IHCs) of foreign banking organizations (FBOs) with $100 billion or more in total consolidated assets.[6] These institutions originate a significant portion of the loans that finance leveraged buyouts and are very likely to hold part of them, and, thus, the data providers are arguably representative (as underwriters/lenders) in the market of LBOs. Since it is mandatory (by regulation) for those institutions to participate in FR Y-14Q reporting, sample selection issues associated with financial information disclosure are quite unlikely to exist for this dataset.

Reporters to FR Y-14Q are required to provide information on borrowers of corporate loans that they hold on their balance sheet. The reported information includes rich details on loans and the borrowers: loan attributes, risk metrics—such as probability of default and loss given default—assessed by reporting banks, and borrowers' financial statement items for major balance sheet, income, and cash flow variables. Because our focus is on the behavior of LBO target companies, we only use company-level information from FR Y-14Q—financial statement variables and probability of default.

[5] Guo et al. (2011) considers a subset of (less than a hundred) target companies that either have widely held public debt outstanding—hence are required to report their financial information to the SEC—or provide historical financial statements at the time of a subsequent IPO, acquisition, or public debt financing.

[6] Before 2020, the total consolidated asset threshold had been $50 billion. In 2020, savings and loan holding companies (SLHCs) were included as reporters.

### Covenant Information at Issuance

For covenant information at issuance, we mainly rely on DealScan. In particular, the legacy DealScan data is used because the new version (LoanConnector DealScan) has relatively poor coverage of covenants, particularly on those in the early part of the sample, compared with the legacy data.[7] The "financial covenant" table and "net worth covenant" table are utilized to assess covenants for each loan deal package. Among nearly 270,000 completed deals denominated in the USD for US companies, roughly 10% of the deals—about 28,000 deals—have covenant information. Yet, for LBO loan deals, which are identified based on the "deal purpose" field being LBO, SBO, or MBO, there are only 645 deals that have covenant information in the entire database.

### Covenant Compliance

In Section 4.2, we further investigate drivers of the relation between pre-buyout credit market conditions and post-buyout target performance using a confidential supervisory data on covenant compliance: the Shared National Credit (SNC) database. The SNC data includes all loans equal to or greater than $100 million lent from federally supervised banking institutions that are shared between three or more institutions.[8] Bank regulators review those loans (either semiannually or annually) at a designated bank, called a "review bank," which is usually the agent bank of the loan. Starting from 2006, the SNC program started to collect covenant compliance information on a subset of the loans under their purview, where

[7]The legacy DealScan data stops at around early 2020, so we end up losing two to three years of data. Yet, the loss is quite small, particularly covenant information for LBO deals, because both the old and new version of DealScan mostly do not report covenants for so-called "covenant-lite" loans, which became the majority of (institutional) leveraged loans issued after the GFC. As a result, post-GFC data contributes to less than 10% of the LBO deal covenant sample. Berlin, Nini, and Yu (2020) shows that such "covenant-lite" loans mostly have split control rights with financial covenants.

[8]The SNC program is governed by an interagency agreement among the Board of Governors of the Federal Reserve System, the Federal Deposit Insurance Corporation, and the Office of the Comptroller of the Currency. The $100 million threshold was $20 million until 2017, and the minimum number of shared institutions was two or more until 1998.

details on covenant compliance information can be found in Chodorow-Reich and Falato (2022).

## 2.2 Summary Statistics for FR Y-14Q sample

Table 1 shows the summary statistics for the baseline FR Y-14Q sample. Since a very small portion of the sample has severe outliers, instead of showing the average, we show the truncated mean after trimming the top and bottom 1% of each variable of interest. For the same reason, instead of the standard deviation, the interquartile range (IQR) is displayed, which is 1.35 times the standard deviation if the variable of interest follows a normal distribution. Yet, the quartiles are computed without trimming and shown in the table. Based on the size of total assets and net sales (revenues), the vast majority of the borrowers are middle-market companies.[9] As typical, the size of companies in the three dimensions—total assets, total liabilities, and net sales—are quite skewed to the right. Book leverage, defined by total liabilities to total (book) assets, is on average close to 2/3, and the average revenues are roughly twice the size of the book assets. Interest expenses are on average 1.6% of the total assets and 2.4% of the total liabilities. The composition of the liabilities is roughly 60% current liabilities—maturing in a year— and 40% long-term liabilities. The share of current assets are 54%, similar to that of current liabilities, presumably for firms to match the term structure of cash flows between the asset side and the liability side. The share of cash and marketable securities, a component of current assets, is on average 11%, but skewed a fair bit. Next, the vast majority of the total assets are tangible assets—even the 25th percentile (first quartile) is 84%. Retained earnings scaled by total assets have a quite large cross sectional variation. Capital expenditures are somewhat skewed to the right.

For (operating) performance variables, we consider 9 different variables: three cash flow

[9]While there are many different definitions of middle-market companies, a typical definition is companies with (annual) revenues between $10 million to $1 billion. See https://www.middlemarketcenter.org/ for further information.

variables (net income, operating income, and EBITDA) divided by three scaling variables (net sales, total assets, and book equity, defined by total assets minus total liabilities). Each of these numerators and denominators has its own strengths and weaknesses: for example, net income is commonly used for measuring corporate earnings, but the measure excludes non-operating expenses such as interest and taxes. One may argue that firms may not need to incur such non-operating expenses if structured properly, and operating income—which is after depreciation and amortization—or EBITDA can be better performance measures. Similarly, some may claim that revenues (net sales) are a better denominator as they are the starting point of firm cash flows. Yet, in the literature total assets are often used to scale cash flow variables, and book equity can be useful to gauge cash flows per a unit of equity to the extent that the size of book equity is comparable to that of market equity.

Consistent with definition of the three cash flows, the average net income scaled by three different denominators is smaller than the average operating income, respectively. Similarly, the average operating income scaled by those denominators is smaller than the average EBITDA. The average net sales are larger than the average total assets, and the average total assets are larger than the average book equity; each cash flow variable scaled by book equity is the largest, that scaled by total assets is the next largest, and that scaled by net sales is the smallest, respectively.

While Table 1 provides the distribution of the level of performance variables, our main measures of performance variables are 3-year changes from their pre-buyout levels to post-buyout levels. To better assess the magnitude of the effect of credit market conditions (changes of), the IQR of 3-year changes of each performance variable is provided in Table A1a. These IQRs are smaller than those of the level in Table 1, and the difference is notable for some of the performance variables.

One of the concerns regarding the 9 different performance variables used in this paper is that those variables might be highly correlated. If those 9 variables are all highly correlated,

we are essentially not very far from taking a single performance variable. In Table A1b, the correlation matrix of the 3-year change of all performance variables is presented. A few of the variables are highly correlated—particularly due to a high correlation between EBITDA and operating income—but not all of the performance variables are highly correlated. Only considering the off-diagonal correlations, the mean and median correlation is about 0.5, the first quartile is about 0.4, and the third quartile is about 0.6. Therefore, those 9 performance variables provide sufficient degrees of freedom for assessing the relation between pre-buyout credit market conditions (changes of) and post-buyout target performance.

# 3 Pre-LBO Credit Market Conditions and post-LBO Target Behavior

In this section, we utilize our proposed measure—the short-term (6-month) change of the high-yield option-adjusted spread (HY OAS) leading up to LBO—of pre-buyout credit market conditions (changes of) to assess how those conditions affect LBO leverage and post-buyout target company behavior. The usage of this measure is justified in Appendix Section B based on two main advantages over the level of credit spreads: 1) target valuations are largely uncorrelated (or at most weakly correlated) with the measure of spread changes, and 2) target selections are unlikely to depend on the short-term change of credit spreads. For post-LBO target behavior, we particularly focus on the post-buyout performance of the target companies.

## 3.1 Illustrative LBO Example

In Appendix Section B, we provide extensive justifications for taking the short-term change of credit spreads leading up to buyout close as a measure of pre-buyout credit mar-

ket conditions (changes of). Yet, an illustrative example might be helpful for readers to understand the motivation behind the choice. Figure 1 shows an LBO example of Staples Inc. (target) by Sycamore Partners Management L.P. (PE sponsor) in 2017. The timeline of the LBO example based on unofficial/official news and stock prices of the target are presented in the figure.

The target company's stock prices appear to respond to buyout rumors, which began to spread in early April, more than 5 months before the buyout deal's close in September.[10] The target's stock price rose to close to $10 per share, which is not very far from the actual buyout pricing of $10.25 per share. As time evolved and further rumors/unofficial news were revealed, target stock prices stayed a bit lower. Right after the deal was signed on June 28 at the pricing of $10.25 per share, where the deal was also announced on the exact same date, stock prices of Staples Inc. increased to above $10 per share and did not change much thereafter. As in this example, in general, there is a substantial buyout premium to public shareholders, as documented in previous studies (e.g., DeAngelo, DeAngelo, and Rice (1984) and Kaplan (1989)).

The timeline of the example transaction is particularly worth discussing because it is relevant to our choice of the short-term change of the HY OAS as a measure of credit market conditions.[11] First, the seller of the target and (potential) buyers initiate talks some time before reaching an agreement and signing the deal. For the example LBO deal, the first rumor of such talks was reported on April 4, and, hence, discussions on a (potential) deal likely started before that date—3 months or more before signing. Second, buyout deal terms including target pricing are finalized at signing—stock prices of Staples Inc. did not move much after the definitive agreement was made and announced.

Third and lastly, financing a buyout deal through external debt occurs after signing

[10] https://www.wsj.com/articles/staples-explores-sale-1491313493.

[11] Institutional details on the timeline of a typical LBO transaction can be found in Appendix Section A.1.

and is subject to change. In the Staples Inc. buyout example, the external debt package comprised of term loan B (TLB) and unsecured senior notes was completed—commitments by lenders were made at determined terms and pricing—on August 15, about one and a half months after the definitive agreement. The total amount and composition of the debt package changed from the initial plan reported around deal signing—from $4 billion ($2.4 billion TLB and $1.6 billion unsecured bridge) to $3.9 billion ($2.9 billion TLB and $1 billion unsecured notes).[12] The actual close of the debt deals, particularly of the TLB portion, occurred on September 12, which is the buyout deal close date.

Since the timing of debt financing is after that of target valuation, the short-term change of credit spreads can be used as a measure of credit market conditions controlling for changes of target valuation over the same period. While target pricing is finalized at deal signing, as long as target valuation is not (or weakly) correlated with credit spread changes for a certain period of time before deal signing, spread changes over that period can serve as part of the measure. In the Staples Inc. example, stock price movements for a few months before the announcement/definitive agreement date seemed to be largely affected by buyout rumors, and its pre-signing stock price peaked at a level close to the actual buyout stock price.

Similarly, the short-term change of credit spreads leading up to LBO close is unlikely to be correlated with possible selection biases. Buyout targets are selected following lengthy and complicated interactions between the (potential) buyers and the seller, which we refer to as a target selection process. Given the timeline of the process, credit spread changes between a few months before buyout close and buyout close are unlikely to affect the target selection process. In the Staples Inc. example, LBO talks between the (potential) buyers and the seller started at least 5 months before the buyout's close.

To a certain extent, the Staples Inc. LBO example elucidates the rationale behind our

[12]Note that there was also an additional $1.2 billion asset-backed lending (ABL) facility unchanged from the initial plan.

choice of the short-term change of credit spreads leading up to LBO as a measure of pre-buyout credit market conditions (changes of). Yet, more detailed and rigorous justifications for this choice are provided in Appendix Section B.

## 3.2 Pre-LBO Credit Market Conditions and LBO Leverage

In this subsection, we examine the relation between pre-buyout credit market conditions and LBO transaction outcomes, particularly buyout leverage, by taking the proposed measure of credit market conditions—the 6-month change of credit spreads leading up to buyout close. Since a positive relation between pre-buyout credit market conditions and LBO leverage is established in the literature (e.g., Axelson et al. (2013)) to a certain extent, our investigation focuses on validating the proposed measure as a measure of credit market conditions and justifying (some of) the arguments in Appendix Section B.

The level of credit spreads at buyout close, which is a widely used measure of credit market conditions in the literature, can be decomposed into the level 6 months before LBO close and the 6-month change of credit spreads as in Equation B.1. Table 2 shows the regression coefficient of (log) buyout leverage, defined by the amount of debt raised for the LBO to the enterprise value of the target, on each of those three credit spread variables.[13]

The first column of Table 2 confirms that the level of the HY OAS at LBO close is negatively associated with buyout leverage, consistent with Axelson et al. (2013). The second column shows that the level 6 months before buyout close is not related to buyout leverage in a statistically meaningful way—also the magnitude is quite small. The third (and last) column exhibits that the relation between the level of the HY OAS at LBO close and the LBO leverage largely comes from the 6-month change of the HY OAS. The regression coefficient on the change of the HY OAS is larger in magnitude and statistically more significant than

[13]We take log of LBO leverage as the dependent variable as this specification gives a considerably better fit measured by adjusted-$R^2$ and a decomposition of log of LBO leverage in a linear form is used in later part.

that on the level at close—a 1 p.p. increase in the HY OAS is associated with a 4.1% lower buyout leverage.[14] This result gives us some comfort in taking the short-term change of the HY OAS as a measure of credit market conditions (changes of) instead of its level.

Figure 2 shows the regression coefficient of (log) LBO leverage on the $n$-month change of credit spreads leading up to buyout close (left column) as well as that on the level at $n$ months before close (right column). The top charts display regression results without any fixed effects and clustering of standard errors. The bottom results exhibit those with industry-fixed effects and double-clustering of standard errors in the industry and the year-quarter (of buyout close) dimensions—the specification of Table 2. Adding industry-fixed effects does not change the regression coefficients in a noticeable way, but, as expected, double-clustering increases the magnitude of standard errors notably. We maintain those elements in regression specifications throughout this paper, although other fixed effects and control variables may be added when appropriate.

The magnitude of the regression coefficient on changes of the HY OAS is maximized at around $n = 4$, and the magnitude declines somewhat as $n$ increases. Yet, at the same time, the standard error of the coefficient decreases a fair bit for higher $n$. This is exactly the trade-off discussed in Appendix Section B.4 and justifies our choice of $n = 6$ for the baseline measure of credit market conditions (changes of). At around $n = 6$, the regression coefficient is still close to the minimum (maximum in magnitude), but the standard error is modestly smaller. Furthermore, the regression coefficient on the level of the HY OAS (right column) dissipates almost fully around $n = 7$ and after.

Next we decompose LBO leverage into two parts: the amount of debt and target valuation. The log of LBO leverage $\log\left(\frac{D}{EV}\right)$, where $D$ is the amount of LBO debt and $EV$ is

[14] A 4.1% lower buyout leverage can lead to a substantially higher equity contribution to a buyout. For an LBO with the average sample buyout leverage (0.71; median 0.66), a 4.1% lower LBO leverage implies a 10% higher equity-to-enterprise value: buyout leverage decreases to 0.68, and equity-to-enterprise value increases from 0.29 to 0.32, a 10% increase.

the enterprise value of the target, can be linearly decomposed into the $D$ and $EV$ part:

$$\log\left(\frac{D}{EV}\right) = \log\left(\frac{D}{\text{Scaling Variable}}\right) - \log\left(\frac{EV}{\text{Scaling Variable}}\right), \tag{1}$$

where the scaling variable is needed to scale both $D$ and $EV$ as they are not stationary variables. We take two widely used scaling variables for $D$ and $EV$ in practice: EBITDA and net sales. Through this decomposition, the effect of credit market conditions on LBO leverage can be thought of as difference between their effect on the LBO debt and target valuation.

Table 3 repeats regressions in Table 2 for the decomposition: taking EBITDA as the scaling variable in Table 3a and net sales in Table 3b. In both tables, the level of the HY OAS at buyout close is negatively associated with both the debt and target value components but with (log) buyout leverage as well because its impact on the debt component is larger. In contrast, the regression coefficients of the debt and target value components on the level of the HY OAS 6 months before LBO close are similar. As a result, the regression coefficient of the LBO leverage on the level 6 months before close is small in magnitude and statistically not significant in both tables. Yet, the effect of the 6-month change of the HY OAS on buyout leverage is mostly through the debt component, and its effect on the target value component is small in magnitude and statistically not significant.

Therefore, the relation between the level of credit spreads at deal close and LBO leverage largely comes from the change of credit spreads leading up to LBOs because the change of credit spreads disproportionately affects the amount of debt taken for LBOs than target valuation. This result is consistent with the argument in Appendix Section B.2 that the short-term change of credit spreads leading up to LBO close is largely uncorrelated with target valuation. By contrast, the level of credit spreads at the start of the change is related to both the debt portion of LBO leverage and buyout pricing and, as a result, has a much

smaller effect on LBO leverage.

Lastly, as described in Appendix Section A.1 and shown in the Staples Inc. example in Figure 1, target valuation is (mostly) finalized at deal signing. The logic of the previous paragraph implies that credit spread changes between deal signing and close affects buyout debt but not target valuation and, as a result, has a large impact on buyout leverage. Table 4 repeats regressions in Table 2 with the change of the HY OAS between signing and close as a regressor. Indeed, the regression coefficient of (log) buyout leverage on the spread change is substantially larger than the coefficient on the level at close. This result supports that the relation between the short-term change of credit spreads and buyout leverage likely comes from the lack of (or at most weak) correlation between the spread change and target valuation.[15]

## 3.3 Post-buyout Changes of Financial Variables

To begin with, we run regressions that are similar to those in Section 3.2—taking post-buyout changes of financial variables as the dependent variables instead. In our setup, post-LBO changes are defined as the difference between the post-buyout level of the variable of interest and its pre-buyout level: post-buyout levels are taken as of the latest observation between 1 and 3 years after buyout close, and pre-buyout levels are as of the latest observation between 2 years before buyout close and buyout close.[16] In addition, as previously discussed,

[15]The change of credit spreads between deal signing and close may be considered as an alternative measure of credit market conditions (changes of). However, the majority (over 70%) of the CIQ LBO sample is missing deal signing date information and, consequently, using this measure instead limits the sample (particularly the FR Y-14Q sample) quite a bit. Yet, for the sample in Table 4, the vast majority of deals have signing date information because the extent to which LBO valuation and debt information is known is highly correlated with other details of the LBOs being disclosed.

[16]3 years at maximum after LBOs for post-buyout levels were chosen based on the fact that LBO exits typically occur between 3 years and 7 years after the buyout—we are mainly interested in post-buyout target behavior while the target is under the control of the sponsor PE firm, not after the target is sold to another entity. We also allow 2-year windows for both the pre-LBO and post-LBO levels, respectively, to increase the size of the sample. The reason for limiting post-buyout levels to be at least a year after the LBO is that cash flow variables in FR Y-14Q are the sum of the past 12-month values—post-buyout variables within a year after the LBO may partly reflect pre-LBO values.

we only consider changes of financial variables reported by the same bank for both the pre-LBO and post-LBO levels.

Table 5 shows results for the regressions of changes of financial variables on 6-month changes of HY OAS leading up to LBOs, using LBO target company data only. Conceptually, these regressions compare post-buyout changes of the outcome variables for LBOs closed during credit spread widening with those for LBOs done during credit spread tightening. Yet, post-buyout behavior of targets of LBOs closed during credit spread widening may be different from those done during credit spread tightening because 1) target company selections by PE firms may depend on pre-buyout credit market conditions and/or 2) post-buyout business conditions may be correlated with pre-buyout credit market conditions. We come back to these identification issues later in this section.

With these caveats in mind, to somewhat address (observable) differences among target companies of LBOs done during different credit market conditions, we add (pre-LBO levels of) several control variables—log of total assets, net sales to total assets, book leverage (total liabilities to total assets), EBITDA to total assets, and tangible assets to total assets—to the regressions. In addition, similar to regressions in Section 3.2, industry-fixed effects based on target companies' two-digit NAICS code are included.[17] Standard errors of the regressions are two-way clustered in the industry and year-quarter dimensions. Dependent variables are primarily changes of financial variables other than performance variables in Table 5a and, separately, target performance changes in Table 5b.

In Table 5a, we find that pre-buyout credit spread tightening (loosening credit market condition) is associated with higher post-buyout growth of (i.e., the change of log) total liabilities and, relatedly, higher post-buyout book leverage compared to its pre-buyout level. Interestingly, interest expenses denominated neither by total assets nor by total liabilities are

[17] Yet, time-fixed effects are not included as the regressions rely primarily on time variations of (changes of) the outcome variables for identification, similar to those in Section 3.2.

affected by credit spread tightening, in comparison with their pre-LBO levels. The portion of current assets and that of cash and marketable securities out of total assets seem positively associated with widening credit spread (tightening credit market condition) leading up to the LBO, but these relations are not robust as can be seen later in the subsection. Lastly, capital expenditures denominated by total assets are not related to the short-term (6-month) changes of credit spreads.

Table 5b displays results for the regressions of post-buyout changes of target performance variables on pre-buyout credit spread changes, where all results are statistically significant. We utilize three different cash flow variables—net income, operating income, and EBITDA (operating income before depreciation)—respectively denominated by three different variables—net sales, total assets, and book equity (total assets minus total liabilities)—that are commonly used in the literature to denominate cash flows. Post-buyout changes of the 9 performance variables in total are positively and statistically significantly associated with pre-buyout credit spread widening: target companies of LBOs done during loosening credit market conditions tend to perform worse than those closed during tightening credit market conditions.

These results are consistent with theories of agency costs of debt: loosening pre-buyout credit market conditions are associated with more debt take-ups and higher leverage but lead to worse post-buyout performance of target companies. The narrative of risk shifting by Jensen and Meckling (1976) or debt overhang by Myers (1977) are consistent with the results. By contrast, the disciplinary effect of debt as argued by Jensen (1989) is not supported by the evidence as more debt usage does not lead to better target performance in data.

While it is encouraging to have results that are strongly supportive of one of the hypotheses but not the other, as previously noted, there are a couple of identification concerns with regards to the empirical setup for Table 5. In particular, LBO target companies are not randomly chosen by PE firms, and even if those companies are randomly chosen, post-buyout

target behavior can depend on post-buyout business conditions that may be correlated with pre-buyout credit market conditions.

To better address those identification concerns, we match each LBO target company with firms of similar (observable) pre-buyout characteristics and take the matched firms as the control group. Specifically, taking a similar approach as in Bernstein, Lerner, and Mezzanotti (2018), we match each LBO target company to non-LBO firms that (i) belong to the same industry based on two-digit NAICS code and have (ii) total assets, (iii) book leverage (total liabilities to total assets), and (iv) EBITDA in the same quintile as the pre-LBO level of those variables for the LBO target within the same year.[18] While this procedure guarantees that the matched firms have data points in the same year as the pre-LBO data point for the target company, we also impose further conditions that (i) matched firms should have at least one observation before and after the LBO close of the corresponding target company, respectively, and (ii) those (at least two) observations need to be from the same bank's reporting, similar to the condition imposed on LBO target observations. Then, among the matched firms, those that have the closest observation dates (minimum sum of the absolute deviations from pre- and post-buyout target observation dates) to LBO target observation dates are selected. Finally, if more than 5 firms are identified for an LBO target after all the above procedures, we choose only 5 firms that have the closest total assets, book leverage, and EBITDA to the pre-LBO level of those variables for the target.[19] This last step limits the size of the control group for each LBO to 5 at maximum, similar to Haque, Jang, and Mayer (2022).

Ideally, if an LBO target is matched to identical firms where the only difference is that the target company is acquired by a PE firm through the LBO while the matched firms

[18] Quintiles of those variables are computed by each year based on (cleaned) FR Y-14Q data of all firms. Note that this procedure guarantees that the matched firms have records within the year of the pre-LBO target observation date.

[19] We take the sum of the absolute deviation over the range of the quintile for the three variables as the measure of proximity.

are not, we will be able to identify *relative behavior* of the target firm to their matched firms—the effect of LBOs on post-buyout target behavior. What the ideal matching does is to eliminate biases associated with post-buyout behavior of non-LBO counterfactual firms: post-buyout behavior of targets that would occur absent buyouts. Therefore, to the extent that the counterfactual post-buyout target behavior selected for LBOs during improving credit market conditions differs from that for LBOs during deteriorating conditions, the matching in the ideal form takes care of such biases. Similarly, differences in the counterfactual post-buyout target behavior associated with post-buyout business conditions, which are possibly correlated with pre-buyout credit market conditions, are controlled for by the ideal matching.

Table 6 exhibits the result of the following regressions using the matched sample:

$$\begin{aligned} \Delta Y_{i,k} &\equiv Y_{i,k,post} - Y_{i,k,pre} \\ &= \alpha + \beta_1 (\text{LBO})_{i,k} + \beta_2 (\text{HY OAS Change})_k + \\ &\beta_3 (\text{LBO})_{i,k} \times (\text{HY OAS Change})_k + \gamma' X_{i,k,pre} + \zeta_j + \eta_t + \epsilon_{i,k} \ , \end{aligned} \tag{2}$$

where $k$ is an index for each LBO transaction, and $i$ is a firm index for a group of firms, including both the treatment firm (LBO target) and control firms (non-LBO matches), corresponding to the LBO transaction. *post* indicates the post-buyout level of a variable, and similarly *pre* indicates the pre-buyout level. $\Delta Y_{i,k}$ is the pre-post change of the financial variable of interest, $(\text{LBO})_{i,k}$ is a dummy variable that takes the value of 1 if company $i$ is the LBO target of transaction $k$ and 0 otherwise, $(\text{HY OAS Change})_k$ is the 6-month change of HY OAS leading up to the close of LBO transaction $k$, and $X_{i,k,pre}$ is pre-buyout levels of control variables—log of total assets, net sales to total assets, book leverage, EBITDA to total assets, and tangible assets to total assets. Finally, $\zeta_j$ is industry-fixed effects based on two-digit NAICS code, and $\eta_t$ is time-fixed effects based on the year-quarter of the LBO

close.[20] Our coefficient of interest is $\beta_3$, which estimates the relation between the post-buyout change of a financial variable of the target company and the 6-month change of credit spreads leading up to the buyout compared with that of non-LBO control firms. These regressions are a version of difference-in-difference estimations, which we revisit in the next subsection, and will be taken as the baseline specification throughout the section.[21]

Table 6a shows that loosening credit market condition leading up to the buyout is (statistically) significantly associated with more growth of total liabilities and higher book leverage, consistent with the relations in Table 5a. Many other financial variables are not affected by pre-buyout credit spread changes, similar to results for those in Table 5a. In contrast, some statistically significant results in Table 5a no longer hold once the matched non-LBO firms are used as a control group—the portion of current assets and that of cash and marketable securities out of total assets seem unrelated to pre-LBO credit market conditions. Also, as pre-buyout credit spread widens, the share of tangible assets increases, although the statistical relations are not strong and do not even exist in Table 5a.

Using matched non-LBO firms as control groups, Table 6b shows that post-buyout performance of targets worsens for LBOs done during improving credit market conditions, consistent with Table 5b. Therefore, loosening credit market conditions leading up to buyouts are associated with higher debt take-ups and worse post-buyout performance, even after controlling for counterfactual behavior of the LBO targets. Yet, the legitimacy of this interpretation depends on the validity of the counterfactual control groups, which we revisit in the next subsection. Again, these results are consistent with agency costs of debt (Jensen and Meckling (1976) and Myers (1977)), but not with disciplinary effects of debt (Jensen (1989)).

[20] Note that we are now able to include time-fixed effects in this empirical setup as the identification relies on differences between LBO targets and their non-LBO control groups. As opposed to the setup, all previous regressions rely on the time variation of the dependent variables.

[21] For each regression, we trim observations where the dependent variable (the change of a financial variable) is either larger than top 1% or smaller than bottom 1% of the sample to remove outliers.

The magnitude of the estimated effect of pre-buyout spread changes on post-buyout target performance is economically large. Compared with the IQRs in Table A1a, the regression coefficients are in the range of 10% to 36% of the IQR—a 1 p.p. increase in the HY OAS is associated with a performance increase of 10% to 36% of the IQR of 3-year changes of the corresponding performance variable. If those 3-year changes follow a normal distribution, the increase is 14% to 50% of the standard deviation of the corresponding performance variable (3-year changes of). The magnitude differs quite a bit across variables, particularly depending on the denominator. When scaled by net sales, the regression coefficient is in the range of 16% to 23% of the IQR. For those scaled by total assets, the magnitude of the effect is about 10% of the IQR. Lastly, for cash flow variables denominated by book equity, the range is from 28% to 36% of the IQR.

Then, Table A6 repeats regressions in Table 6b with taking the 4-month change of credit spread leading up to LBO close as the main regressor instead of the 6-month change in the baseline setup. Largely consistent with the discussion in Appendix Section B.4, the regression coefficients (on the interaction term) are overall larger—except for two variables: EBITDA-to-net sales and EBITDA-to-total assets—but their statistical significance is noticeably weaker due to larger standard errors, compared with those in Table 6b.

It is worth noting that the relations between pre-buyout credit market conditions and post-buyout target behavior that we find in Table 6 do not hold when we use the level of credit spreads at buyout instead of the change of credit spreads leading up to LBOs. Table A5 shows the result of regressions that are same as the specification in Equation 2 with the short-term change of the HY OAS $(\text{HY OAS Change})_k$ replaced by the level of HY OAS at close $(\text{HY OAS at Close})_k$. As can be seen in Table A5a, all (non-performance) post-LBO financial variables do not have a statistically significant relation with the level of credit spreads at LBO close. Furthermore, as shown in Table A5b, none of the post-buyout performance variables are associated with the level of HY OAS at buyout in a statistically

significant way. Therefore, we find it crucial to correctly identify pre-LBO credit market conditions (or changes thereof) to uncover their impact on post-buyout target behavior.

Finally, $\beta_1$, the regression coefficient on LBO dummies, of Equation 2 might also be of interest as the coefficient captures the impact of LBOs on post-buyout target behavior if the non-LBO counterfactual firms are perfectly identified.[22] Assuming that is the case, in Table 6a, we find that LBOs are associated with substantial growth in total assets and liabilities, higher leverage and interest expenses, lower share of current assets, cash and marketable securities, and tangible assets, and lower retained earnings and capital expenditure when denominated by total assets. The net sales of the target company increase in amount after buyouts but much less so than total assets; The post-buyout change of net sales scaled by total assets is negative for LBO target companies compared with their non-LBO control firms. Lastly, the composition of target liabilities shifts towards longer maturities after LBOs. Many of these results are consistent with the fact that an LBO involves a heavy usage of external debt, which tends to be long-term financing. Yet, the impact of LBOs on post-buyout performance looks negative as can be seen in Table 6b. This result is not consistent with Kaplan (1989), which shows substantial gains in post-buyout operating performance for LBOs done in the 1980s, but consistent with the trend of declining gains of LBOs in operating performance reported in Guo et al. (2011).[23] However, as discussed in the next subsection, we believe that it is difficult to justify that non-LBO control firms that we identified reflect the true non-LBO counterfactual firms and, hence, prefer not to take too much inference out of the regression coefficients on LBO dummies.

[22]The 6-month credit spread changes $(\text{HY OAS Change})_k$ leading up to LBOs are demeaned so that $\beta_3$ estimation may not affect the point estimate of $\beta_1$.

[23]Such declines in operating performance gains are likely related to a concept that as the PE industry becomes larger (and more mature), available investment opportunities (per fund) decrease (e.g., Pástor and Stambaugh (2012)). Consistent with this concept, Harris, Jenkinson, Kaplan, and Stucke (2023) reports weaker performance persistence for buyout funds over time.

## 3.4 Pre-LBO Trend Tests

As discussed in the previous subsection, the legitimacy of the regression results in Table 6 depends on if non-LBO control firms are chosen appropriately. Ideally, we would want those control firms to be the non-LBO counterfactual firms of LBO targets. Our matching procedure is based on similarity of several (observable) financial variables of non-LBO firms to those of LBO targets before buyouts. However, this procedure is unlikely to identify the true counterfactual firms of LBO targets since it is not plausible that the selection of LBO targets by PE sponsors is merely based on pre-buyout financial variables of potential targets. Hence, there may be potential selection biases in post-buyout target behavior, to the extent that firms in our control group deviate from the true counterfactual firms.

Yet, our coefficient of interest, $\beta_3$ in Equation 2, can still be unbiased even if there are selection biases in post-buyout target behavior—as long as the biases are not correlated with the measure of (changes of) credit market conditions. A formal proof of this statement is provided in Appendix Section C.1. Therefore, in this subsection, we focus on validating that possible deviations of non-LBO control firms from the true non-LBO counterfactual firms are not systematically associated with short-term credit spread changes leading up to LBO close. As discussed in Appendix Section B.3, a target selection process is presumably not associated with the short-term spread change. As a result, the difference between non-LBO control firms and the true counterfactual firms—driven by factors that are not captured by our matching procedure but govern the target selection process—is unlikely to be correlated with the credit spread changes. Nonetheless, additional pre-LBO trend tests are conducted to further support the legitimacy of our empirical approach.

As briefly mentioned in the previous subsection, the regression specification in Equation 2 is a version of a difference-in-difference estimation. In this specification, we take the change of financial variables from pre-buyout to post-buyout as the dependent variables mainly because 1) those financial variables are generally persistent, and, as a result, taking changes

are likely to reduce correlations of the residuals over time, 2) adding the pre-buyout level of financial variables as control variables is straightforward since none of those variables are (part of) dependent variables, and 3) the specification is simpler and easier to display (and to some extent to interpret) the results. Yet, a more typical regression specification for difference-in-difference estimations gives largely the same results as shown in Appendix Section C.2. In a difference-in-difference approach, the most important assumption is a parallel trend assumption, where the difference between the treatment and control group is constant over time in the absence of treatment. This assumption is well satisfied if true non-treatment (non-LBO) counterfactual firms are selected as the control group.

While the parallel trend assumption is not directly testable, researchers taking a difference-in-difference estimation approach often test if there is a pre-treatment trend to justify their empirical setup. The existence of a pre-treatment trend for the difference between the treatment and control group indicates that the control group is unlikely to represent the true conterfactual of the treatment group. We take a similar approach and examine the existence of pre-treatment trends: we investigate the treatment-control group difference at each point of time from 6 years before LBO to 5 years after LBO, relative to the difference in a year before LBO.[24] The relative treatment-control group differences before LBOs are used for assessing pre-treatment trends, and the relative differences after LBO for confirming treatment effects.

When examining pre-treatment trends, one challenge is that our panel data is quite unbalanced; it is not a rare occasion that a firm in our sample misses several years of reporting between adjacent observations. As a result, if we estimate the pre-treatment trends using the full unbalanced panel, there might be biases related to those missing observations. For

[24] "A year before LBO" indicates financial variables reported (as of) between 12 months before LBO close and LBO close. Similarly, "a year after LBO" indicates those reported between LBO close and 12 months after LBO, "2 years after LBO" indicates those reported between 12 months after LBO and 24 months after LBO, and so forth.

example, suppose that we estimate, say, the treatment-control group difference in 3 years before LBO relative to the difference in a year before LBO using the full unbalanced panel data. Then, many observations for firms that have data in 3 years before LBO but not in a year before LBO, and vice versa, are included. To the extent those firms that partially miss data in one of the two years differ from firms that have data in both years, there could be biases in our estimation.

To address such possible biases, when estimating the treatment-control difference relative to the difference in a year before LBO, we only take observations from firms that report both in the year of interest and a year before LBO and run separate regressions for each year of interest. Also, similar to previous specifications, financial information in both years needs to come from the same bank in order to be included in the sample. When there are multiple observations for each firm in each year (from LBO), the median of the financial variable of interest is taken for the corresponding firm-year. In this setup, we first show that pre-treatment trends clearly exist for the LBO effect—the treatment-control group difference—on target performance in Table A8. Details on the regression specification and discussions are provided in Appendix Section C.2.

Such pre-treatment trends indicate that the control non-LBO firms that we match to LBO targets are not representative of true counterfactual non-LBO firms. Therefore, our estimate of the LBO effect—$\beta_1$ of Equation 2: the regression coefficient on LBO dummies in Table 6—cannot be taken at the face value since the parallel trend assumption is unlikely to hold. These results are to some extent expected as it is not plausible that PE firms select target companies merely based on their pre-buyout (observable) financial information.

Yet, our main question of interest is how LBO effects differ between those under different pre-buyout credit market conditions. As discussed above, as long as the deviation of the control non-LBO firms from the true counterfactual non-LBO firms is systematically not different between target companies of LBOs done during widening credit spreads and those

closed during narrowing credit spreads, our estimate of the differential LBO effects under different pre-LBO credit market conditions would not suffer from similar issues of selection biases.

If differences between the matched control firms and the true counterfactual firms are not correlated with (changes of) pre-buyout credit market conditions, we would see no difference between the pre-buyout treatment-control group difference for LBOs done during improving credit market condition and that for LBOs closed during deteriorating conditions. To test this prediction, we run the following regression:

$$
\begin{aligned}
Y_{i,k,\tau} = \alpha + \beta_1 (\text{LBO})_{i,k} + \beta_2 (\text{Non-reference})_{\tau} + \beta_3 (\text{HY OAS Change})_k + \\
\beta_4 (\text{LBO})_{i,k} \times (\text{Non-reference})_{\tau} + \beta_5 (\text{LBO})_{i,k} \times (\text{HY OAS Change})_k + \\
\beta_6 (\text{Non-reference})_{\tau} \times (\text{HY OAS Change})_k + \beta_7 (\text{LBO})_{i,k} \times (\text{Non-reference})_{\tau} \times \\
(\text{HY OAS Change})_k + \zeta_j + \eta_t + \epsilon_{i,k,\tau} \ ,
\end{aligned} \tag{3}
$$

where indexes and variables are largely the same as those in Equation C.16, and $(\text{HY OAS Change})_k$ is the 6-month change of HY OAS leading up to the close of LBO transaction $k$. Our coefficient of interest is $\beta_7$, which measures the sensitivity of the difference between LBO targets and non-LBO control firms in the year of interest, relative to the reference year, to the 6-month change of HY OAS leading up to LBO. Table 7 displays the regression results for $\beta_7$. Unlike in Table A8, we do not find pervasive pre-treatment trends for the difference of differential performance between the treatment and control group by (changes of) pre-buyout credit market conditions; only two regression coefficients (for operating income to sales and operating income to total assets) out of 45 are statistically significant for pre-treatment trends as opposed to 28 coefficients being statistically significant for post-treatment trends.

Therefore, we do not find systematic pre-LBO trends for the sensitivity of the treatment-control group performance difference to changes of credit spreads leading up to LBOs. This

finding, along with prevalent pre-LBO trends for the treatment-control group performance difference, supports that the deviation of the non-LBO control firms from the true counterfactual firms does not systematically differ by (changes of) pre-buyout credit market conditions.

It is worth noting that replacing the 6-month HY OAS change leading up to LBOs with the level of HY OAS at 6 month before LBOs in the regression specification from Equation 3 gives quite different results. As shown in Table A9, in this specification, 13 regression coefficients (particularly for operating income and EBITDA) out of 45 are statistically significant for pre-treatment trends, but 8 coefficients are statistically significant for post-treatment trends. Hence, we do see notable pre-treatment trends for the sensitivity of the treatment-control group performance difference to the level of credit spreads at 6 months before LBO close.

We interpret such pre-treatment trends as the level of credit spreads (at 6 months before LBOs) being correlated with selection biases—the deviation of the non-LBO control firms from the true counterfactual firms. As can be seen in Table A10, showing the results of regressions specified in Equation 2 ($\beta_3$ in particular) with the change of credit spreads replaced by the level of credit spreads at 6 months before LBOs, wide credit spreads (tight credit market conditions) are associated with higher liability growth and leverage and worse post-buyout performance by some measures. These results, particularly those on liability growth and leverage, are inconsistent with the relation between pre-buyout credit market conditions and buyout leverage established in Section 3.2 and in the literature (e.g., Axelson et al. (2013)). The conceptually implausible results likely reflect the issue of selection biases and provide another reason that the level of credit spreads (at 6 months before LBOs) is not taken as a control variable—the conditional independence assumption is unlikely to hold with its inclusion.

## 3.5 Do Post-LBO Business Conditions Drive the Results?

So far, we show that short-term credit spread changes leading up to buyout close are likely independent of the buyout target selection process and, as a result, are unlikely correlated with selection biases, the existence of which seem natural. No evidence of broad pre-treatment trends for the sensitivity of the treatment-control group difference to pre-buyout credit spread changes is supportive of this rationale as well. To the extent that this argument holds, our baseline results in Table 6 are not driven by a correlation between pre-buyout credit market conditions and selections of targets for which the effects of LBOs may differ.

Still, simply put, the baseline results in Table 6 capture the differential effect of LBOs on post-buyout target behavior by (changes of) pre-buyout credit market conditions—difference between the effect of LBOs done during credit spread widening and that of LBOs closed during credit spread tightening. The investigations in the previous subsection help exclude target selections as the primary driver of the baseline results but they do not exclude a possibility that pre-buyout credit spread changes are proxies for post-buyout business conditions: the differential effect of LBOs on post-buyout target behavior captured in Table 6 may reflect the impact of post-buyout business conditions on the effect of LBOs rather than that of pre-LBO credit market conditions, which are possibly correlated with post-buyout business conditions.

To address the possibility that our main results might come from a correlation between pre-buyout credit spread changes and post-buyout business conditions, we include 6-month spread changes right before (lag) and right after (lead) the baseline 6-month change window—6 months leading up to LBO close—as additional regressors. Our time window for measuring (changes of) performance variables is approximately 3 years—if the correlation between credit spread changes and future business conditions is the main driver of our results, spread changes in the neighboring periods are very likely to have a similar relation to target perfor-

mance. Thus, one would expect to see that regression coefficients on (LBO dummies times) the lag and lead 6-month spread changes are similar to those on the baseline 6-month spread change, both in magnitude and statistical significance. However, it turns out not to be the case.

As shown in Table 8, even after including the lag and lead 6-month credit spread changes as additional regressors, the regression coefficients of performance variables are only broadly significant for those on (LBO dummies times) the baseline 6-month spread change. Furthermore, many of the coefficients on (LBO dummies times) the lag and lead 6-month spread changes are negative as opposed to the baseline coefficients being all positive. Table A11, which only includes either the lag or lead 6-month credit spread change in each regression, also exhibits very similar results. Therefore, these results support that a correlation between pre-buyout (changes of) credit market conditions and post-buyout business conditions is not a primary driver of our baseline results in Table 6.

While unlikely, there is one last explanation that a correlation between pre-LBO credit spread changes and post-LBO business conditions may drive our main results. That is, the exact timing of (6-month) credit spread changes can be crucial for its correlation with post-buyout business conditions, and a (slight) timing mismatch by 6 months—relative to the time window for performance change measurement—for the lag or lead spread changes may weaken the relation substantially. To address this possibility, we shift the time window for measuring performance variable changes by 6 months (either backward or forward) so that the relative timing between the lag or lead spread change and the performance variable changes can be (almost) equivalent to that of the baseline setup in Table 6.

By making these timing shifts, the only important timing difference between those setups and the baseline setup is the buyout close date. In the baseline setup, the buyout close date is right at the end of the window for credit spread changes. In those (shifted) setups, the buyout close date is either 6 months after the end of the window for credit spread changes,

or at the start of the window. If the hypothesis on the exact timing is correct, regression coefficients in these settings should be largely the same as those in the baseline result. As shown in Table A12, statistical significance of the coefficients is not as comprehensive as that in the baseline result, and even those that are statistically significant are all negative.

Above results do not appear to support that a correlation between credit spread changes and (future) business conditions drives the main result in Table 6. Rather, those results suggest that the 6-month credit spread change leading up to LBO close has a distinctive relation to post-buyout target performance, even compared with spread changes in adjacent periods. As a result, while we do not make a strong claim that pre-buyout credit spread widening (tightening) causes better (worse) post-buyout target performance, the implication of those results sets the bar high for some other variables to be a primary driver of the baseline result in Table 6. Such a variable, if anything, has to be highly correlated with the 6-month credit spread change leading up to buyout close but not (or much less) correlated with spread changes in neighboring periods.[25]

Therefore, it is implausible that the baseline result in Table 6 is driven by other variables than the 6-month spread change leading up to LBO close. Having established that the baseline results are likely a consequence of (changes of) pre-buyout credit market conditions, other robustness checks are performed in Appendix Section C.3.

# 4 Do Agency Costs of Debt Explain the Results?

In this section, this paper pursues further evidence that the results in Section 3 are explained by the narrative of agency costs of debt, particularly risk shifting by Jensen and Meckling (1976) and debt overhang by Myers (1977). Then, we explore which of the two main theories of agency costs of debt is better supported by data. To examine possible alternative

[25]This precondition naturally excludes any post-buyout macroeconomic and financial variables that have a stable relation to (past) credit spread changes from being a candidate.

explanations, post-LBO target strategies based on a couple of observable dimensions are separately examined in Appendix Section C.4.

## 4.1 Agency Costs of Debt: Bank-Assigned Probability of Default

Our main results in Section 3 are consistent with the narrative of agency costs of debt: pre-buyout credit spread tightening, which leads to higher buyout leverage, is associated with worsening post-buyout (operating) performance of the targets. While those results are supportive of the narrative, we further investigate if pre-LBO spread changes are related to agency costs of debt.

Among theories of agency costs (of debt), our focus is particularly on the theory of risk shifting by Jensen and Meckling (1976) and that of debt overhang by Myers (1977). While there are other agency theories, those are not necessarily consistent with our findings. For example, Jensen and Meckling (1976), where the theory of risk shifting comes from, also studies a moral hazard problem where the entrepreneur chooses the level of "effort" that is not directly contractible. The paper shows that a debt contract solves such a moral hazard problem absent a risk-shifting issue; this result serves as one of the rationales for advocating LBOs in Jensen (1989). Thus, not all theories of agency costs imply that higher leverage is associated with worse firm performance, and we focus on those that are consistent with our results.

In the theory of risk shifting and debt overhang, leverage is important because higher leverage leads to less "skin in the game" for shareholders, assuming that a firm's objective is to maximize the shareholders' value.[26] A negative relation between pre-buyout (short-term) credit spread change and LBO (market) leverage is established in Section 3.2. A similar negative relation between the spread change and pre-post book leverage change,

[26]This assumption is crucial for those theories as its violation may result in a different or even opposite result. For example, the free cash flow theory by Jensen (1986) assumes conflicts of interest between managers and shareholders (and other stakeholders who may benefit by taking positive NPV projects).

although presumably market leverage is more relevant in those theories, is shown in Section 3, specifically in Table 6a. Therefore, results in the previous sections are supportive of the narrative that loosening credit market conditions are associated with less skin in the game by the shareholders, which are the PE sponsors in the context of LBOs. This narrative is particularly more relevant to the short-term spread change leading up to buyout close, mainly because target valuations are largely uncorrelated with the change and, as a result, higher (market) leverage directly translates into lower equity contributions by the buyer.

Both theories—risk shifting and debt overhang—imply that highly levered firms do not make optimal decisions that maximize the firm value. The theory of risk shifting claims that firms take negative-NPV projects with excessive risk as the shareholders only take the upside while the debtholders suffer from the downside. In contrast, the theory of debt overhang argues that firms do not take positive-NPV projects, which they would take absent any debt, as part of the gain goes to the debtholders. Hence, both theories imply worse firm performance (and, consequently, firm value) and the erosion of debtholders' value for highly levered firms, compared with less levered firms. The former implication is consistent with results in Section 3.

To further test if short-term credit spread changes leading up to buyout close are associated with agency costs of debt, we investigate if pre-buyout credit spread tightening is related to more post-buyout erosion of debt value. To address this question, we utilize information in FR Y-14Q, in particular, the probability of default for borrowers that is assigned by banks which report in FR Y-14Q. Because most banks start to report probability of default from the end of 2012, our sample for this exercise involves buyouts from 2013. Also, probability of default is as of the reporting date as opposed to financial variables mostly being as of some time before the reporting date.

Similar to the baseline setup in Section 3, a time window between 24 months before buyout close and buyout close is used for the pre-buyout levels, and that between buyout

close and 36 months after buyout close for the post-buyout levels—the latest values within each window are chosen.[27] In addition, only the probability of default reported by banks that have nonzero exposure to the target is taken—banks are unlikely to pay much attention to firms that have zero (utilized) amount of liability to the banks. Lastly, since the probability of default is a bank-specific subjective measure, we take the probability of default for target firms for all possible buyout-bank pairs.[28] The final sample only has observations of a few hundred LBO-bank pairs, and control firms (of at most 5 per each buyout) are matched similarly as in the baseline setup.

Table 9 shows the result of regressions of the pre-post buyout change of (log of) the probability of default on credit spreads.[29] When taking the change of the (log) default probability, firms that are already in default at the start of the change are excluded. As the probability of default is a bank-specific measure, we only take changes of the (log) probability of default for those assigned by the same bank. Bank-fixed effects are added to control for (static) differences among banks, and the pre-buyout level of the (log) probability of default is added as a control variable to address its mean reversion. The regression result shows that the 6-month change of credit spread leading up to LBO close is negatively associated with the pre-post LBO change of the (log) probability of default. Thus, pre-buyout spread tightening is associated with higher post-buyout probability of default as opposed to the level of credit spreads being not much related to post-buyout probability of default. This result is consistent with a narrative that loosening credit market conditions induce less "skin in the game" for buyout targets, leading to more erosion of debtholders' value.

[27] Unlike the baseline setup, for the post-buyout levels, the period between buyout and 12 months after buyout is included because the probability of default is an as-of variable, not a sum over the past 12 months as for some financial variables.

[28] When there are multiple default probabilities assigned by the same bank to the same company for different facilities, the highest probability is taken. Furthermore, we exclude companies that are already in default (probability of default at 1) before buyout.

[29] Log is taken since the probability of default follows an extremely skewed distribution; roughly, the 75th percentile is 2%, the 90th percentile is 5%, and the 95th percentile is 12%.

Yet, this result might be already expected to a large extent—the probability of default for a borrower is likely to increase when its leverage increases. Since it is shown that buyout leverage increases for LBOs closed during credit spread tightening in Section 3.2, it is natural that the target's post-buyout default probability increases as well. To separate out the impact of the buyout leverage, we run the same regressions in Table 9 with two different specifications for the dependent variable: 1) changes of the (log) default probability from the pre-buyout level to levels shortly after LBO close (defined by a time window between buyout close and 12 months after close) and 2) changes of the (log) default probability from shortly after LBO close (the same definition) to 36 months after close (defined by a time window between 24 months after and 36 months after close). These two separate specifications can be thought of as a decomposition of the dependent variable in Table 9, although not exactly because the sample differs and control variables, particularly their timing, are also different.

The regression results of those two specifications are displayed in Table A15. The former specification—change of the (log) default probability until shortly after LBO close—in the first three columns confirms that loosening pre-buyout credit market conditions are associated with higher default probability even shortly after buyout close (the third column). Such higher default probability is in part likely related to higher buyout leverage induced by decreasing credit spreads leading up to buyout close. Yet the regression coefficient on the 6-month spread change is notably smaller than the coefficient in Table 9.

The result of the latter specification—change of the (log) default probability after buyout close (right after to 3 years after)—in the last three columns of Table A15 presumably excludes the direct effect of LBO leverage on targets' probability of default since the change of default probability occurs after buyout leverage is determined. Still, we find that the relation between pre-buyout credit spread change and the change of the (log) default probability—the last column—is negative and much larger (about twice to three times) in magnitude compared to that in the third column. Therefore, the "erosion" of debtholders' value measured

by the probability of default for the target company largely takes place after buyout close, consistent with the theories of agency costs of debt.

As discussed earlier in Section 3, even though LBO target companies are matched to similar non-LBO control firms based on their pre-buyout characteristics, selection biases are likely to exist, as it is the case for performance variables. Yet, our baseline measure of (changes) of credit market conditions, which is 6-month changes of credit spreads leading up to buyout close, are presumably uncorrelated with such selection biases. As a result, the relation between the short-term credit spread change and the (log) default probability change shown above is unlikely to be biased.

The second column of Table A16 confirms that there is no statistically meaningful pre-treatment (pre-LBO) trend for the sensitivity of the (log) default probability to the 6-month HY OAS change leading up to buyout close. This sensitivity is basically $\beta_7$ in Equation 3 where the financial variable $Y$ is replaced by log(Probability of Default), with bank-fixed effects added. This result supports that the association between the 6-month spread change and the change of the (log) default probability is not driven by selection biases. Note that consistent with the results in Table A15, the regression coefficients start to increase in magnitude (with a negative sign) after a year after buyout close, but the most stark increase occurs from two years after.

In contrast, in Table A16, the first column, which displays $\beta_3$ in Equation C.16 where $Y$ is replaced by log(Probability of Default), shows that the estimated LBO effect on the (log) probability of default exhibits a certain degree of selection biases; a pre-treatment trend exists in a statistically significant way. The third column shows the result of the same specification as in the second column with the 6-month HY OAS change replaced by the (level of) HY OAS 6 months before buyout close. This result proves that the level of credit spreads, particularly that 6 months before LBO close, is correlated with the selection biases coming from that our matching process does not capture the true (non-LBO) counterfactual firms.

Therefore, while we show regression results that include the level of credit spreads 6 months before LBO close as regressors—the level itself and its interaction with LBO dummies—in (the last column of) Table 9 and Table A15, estimates under such a specification are likely to be biased.

As a final note, one might argue that poor post-buyout performance coming from loosening pre-buyout credit market conditions is the cause of high post-LBO default probability; naturally, poor firm performance is likely associated with higher default probability. However, such a claim on causality should be made with a great deal of caution; a good example is the theoretical mechanism of risk shifting or debt overhang we focus on. The theories of agency costs of debt suggest that firm behavior—taking negative NPV projects or forgoing positive NPV projects at debtholders' expense—changes the distribution of cash flows. This change of the cash flow distribution caused by the firm behavior generates a negative relation between firm performance and its default probability.[30] In this example, firm behavior is the fundamental cause of both its performance and default probability.

Therefore, if a certain type of firm behavior (other than risk shifting and debt overhang) in an alternative explanation causes changes of the distribution of the cash flows which affect the firm's performance and default probability, the mechanism is largely the same as that of risk shifting or debt overhang. In this case, the same causality claim also applies to the theory of risk shifting and debt overhang to the extent the claim is valid for the alternative explanation.

Yet, one can still argue that high post-buyout target default probability is caused by negative shocks to target cash flows, not by post-buyout target behavior. In this case, our statistical approach only leaves a possibility that the cash flow shocks are systematic, and such systematic shocks may exist to the extent that there are selection biases. Since we

[30]Firm (operating) performance is a variable that measures (scaled) cash flows at a certain point of time while the cash flow distribution entails cash flows over the life of the firm.

address the issue of selection biases in Section 3 and in this subsection to a large extent, such selection biases are an unlikely possibility. Nonetheless, the causality claim that high post-buyout target default probability is caused by its poor post-LBO target performance is conceptually more viable in this context.

To address the impact of those cash flow shocks, Table A17 adds the change of each performance variable as an additional control variable to the regression specification (in the last column) of Table A15. The regression results show that even after controlling for performance change, the 6-month change of credit spreads leading up to LBO close is negatively associated with the post-buyout change of the (log) default probability (from a year after buyout close to 3 years after). Note that the magnitude of the regression coefficient is largely similar across columns which take a different performance variable as an additional control. Hence, post-buyout cash flow shocks to target companies do not fully explain the results of Table A15, particularly those of the specification for the last three columns.

## 4.2 Risk Shifting versus Debt Overhang: Evidence from Covenants

The theories of risk shifting and debt overhang have a subtle difference in their implications. In the risk shifting theory, firms actively take negative-NPV projects while firms passively forgo positive-NPV projects in the debt overhang theory. Yet, investment decisions by a firm are not observed at the project level, and even if so, it would be quite difficult to discern the NPV of each project unless the projected cash flows from the project and its discount rate are provided. Thus, directly testing those implications of the two theories would not be feasible.

Instead of directly testing what type of projects target companies take after buyout, we indirectly test those two theories utilizing data on loan covenants. Since covenants are designed to protect lenders from borrowers' behavior that is likely to harm the value of the debt, covenant breaches can be thought of as firm behavior that is more consistent with the

narrative of risk shifting than with that of debt overhang. Also, upon covenant violations, lenders may take control of the firm or waive/amend the violated covenants so that the current management can maintain its control over the firm. If the firm is likely to engage in activities that damage the debtholders' value, which are firm behavior more consistent with the risk shifting theory, the lenders would tend not to grant a covenant waiver/amendment that leads to covenant compliance.

Before studying covenant violations and waivers, we examine a relation between pre-buyout credit market conditions and the strictness of covenants for buyout loans. Ex-post covenant violations (and potentially their waivers/amendments) are largely dependent on ex-ante strictness of the covenants; if covenants are written very strictly at the issuance of LBO loans, firms are highly likely to violate the covenants in the future. Consequently, it would be to a large extent futile to discuss covenant violations and waivers without considering the strictness of covenants for buyout loans.

We test if pre-buyout credit spread changes are associated with measures of covenant strictness for buyout loans, utilizing covenant information at loan issuance from Refinitiv DealScan data. LBO loan deals are matched to non-LBO loan deals in the same quarter for firms that are similar to LBO target companies, and covenants of the matched non-LBO deals are used as the control group. Two different measures of covenant strictness are employed: a) the number of covenants and b) the loan contract strictness developed by Murfin (2012).

Table 10 displays the result of the regression of each measure of covenant strictness on the pre-buyout level and change of credit spreads. Since non-LBO loan deals are used as the control group, our coefficients of interest are those on the interaction term between LBO dummies and the level/change of the HY OAS. The level of the HY OAS, both at the loan deal close and at 6 months before deal close, is positively associated with measures of covenant strictness. In contrast, the 6-month change of the HY OAS leading up to deal close does not seem related to how strict the covenants of the buyout loan deal are. The former

result, the positive relation between the level of credit spreads and covenant strictness, is largely expected because credit market conditions, which are measured by credit spreads, are correlated with lenders' attitude; lenders require stricter covenants for buyout loans when credit market conditions are tight.

For the latter result, the lack of a relation between the 6-month change of credit spreads and covenant strictness contrary to the former result, there are two possible interpretations. The first interpretation is that for LBOs done during credit spread widening, the lower probability of the borrower engaging in activities that harm the loan value allows the lenders not to tighten terms for the loan as much as the lenders would. This interpretation is more in line with the narrative of risk shifting. Yet, the other interpretation is that loan covenants are determined some time before the deal close date—similar to buyout target valuations being determined a few months before LBO close—and, consequently, are not much affected by the short-term change of credit spreads leading up to deal close. Murfin (2012) assumes that loan contracts are typically determined 90 days before the close date.

Regardless of the interpretation, the strictness of LBO loan covenants is shown to be largely unrelated to the 6-month credit spread change. While the control group in Table 10 is selected following a matching process similar to that in Section 3, since covenants for non-LBO loans are likely to be different in nature from those for LBO loans, the validity of the control group in this exercise may be weaker. Thus, we run the same regressions in Table 10 for LBO deals only without matching to non-LBO control deals. The result of these regressions is shown in Table A18, and we still do not find evidence for a relation between pre-buyout credit spread changes and covenant strictness.

Having established that the ex-ante strictness of covenants for buyout loans is not associated with the short-term credit spread change leading up to deal close, now we examine covenant breaches and waivers using the SNC data. The data provides information on covenant compliance for a subset of (syndicated) loans under the purview of the SNC pro-

gram. One data issue with our empirical investigation is that only a minor fraction of loans are examined on their covenants over multiple years—more than 60% of the loan facilities are examined only once in the covenant compliance data. As a result, a comparison of post-buyout covenant compliance with pre-buyout compliance for the same firm would limit the size of the sample quite a bit.[31]

Due to such data limitation, we compare post-buyout covenant compliance of LBO loan deals (treatment deals) with that of non-LBO loan deals (control deals)—for covenant reviews up to 3 years after buyout close. Even without a restriction that deals need to have both pre- and post-buyout information, the number of LBO deals in the sample is still quite small at a few hundreds. Since balance sheet information of the borrowers is not readily available in the SNC data, the LBO deals are matched to non-LBO deals of firms in the same industry (represented by the first two-digit of the NAICS code), covenants of which are reviewed by SNC examiners for the same review bank on the same date. Limiting the control deals to those with the same review bank and the same review date (as those of the corresponding LBO deal) is to control for possible heterogeneity among review banks and that among SNC examiners.

Table 11 shows the the relation between covenant compliance and credit spreads. Note that each covenant compliance information is aggregated at the borrower level—if a borrower breaches one of the covenants for a single credit facility out of multiple outstanding credit facilities, the occasion is recorded as a covenant breach for that borrower (for the corresponding review date). We run regressions of the following type:

$$Y_{i,k,b,\tau} = \alpha + \beta_1 (\text{LBO})_{i,k} + \beta_2 (\text{HY OAS})_k + \beta_3 (\text{LBO})_{i,k} \times (\text{HY OAS})_k + \gamma' X_{i,\tau} + \zeta_j + \eta_t + \xi_b + \mu_\tau + \epsilon_{i,k,b,\tau} \,, \tag{4}$$

[31] With this restriction that both pre-buyout and post-buyout covenant compliance information must exist, less than 100 LBO deals remain in the sample. A statistical analysis based on the sample of such a small size are unlikely to be useful.

where $k$ is an index for each LBO transaction, $i$ is a firm index for a group of firms, including both the treatment firm (LBO target) and control firms (non-LBO matches), corresponding to the LBO transaction, $b$ is a review bank index, and $\tau$ is an index for the timing of the review date. $Y_{i,k,b,\tau}$ is the dependent variable related to covenant compliance, $(\text{LBO})_{i,k}$ is a dummy variable that takes the value of 1 if company $i$ is the LBO target of transaction $k$ and 0 otherwise, $(\text{HY OAS Change})_k$ is the 6-month change of HY OAS leading up to the close of LBO transaction $k$, and $X_{i,\tau}$ is control variables that are known to affect debt renegotiation—the (log) number of participants, the agent bank share, and the nonbank share—aggregated at the firm level. Lastly, $\zeta_j$ is industry-fixed effects based on two-digit NAICS code, $\eta_t$ is time-fixed effects based on the year-quarter of the LBO close, $\xi_b$ is review bank-fixed effects, and $\mu_\tau$ is review date-fixed effects. There are no dummies on pre- and post-buyout as only post-buyout covenant compliance is considered in this specification. Our coefficient of interest is $\beta_3$.

Results in Table 11 are consistent with the implications of the risk shifting theory. As displayed in the first three columns, the 6-month credit spread change leading up to buyout close is negatively associated with covenant breach dummies: loan covenants for LBO deals done during credit spread widening are less likely to be breached. At the same time, the next three columns exhibits that the 6-month spread change is positively associated with covenant compliance with waivers/amendments dummies: target companies for LBO deals done during credit spread widening are more likely to receive covenant waivers/amendments that lead to compliance. The last three columns shows that LBO loan deals are not granted more (or less) waivers/amendments, compared to non-LBO control deals, depending on pre-buyout credit spread changes. Therefore, the middle three columns can also be interpreted as the conditional likelihood of covenant compliance given a waiver/amendment being higher for buyout deals closed during credit spread widening.

Lastly, to assure that the result of Table 11 is not driven by the choice of the control

group, we run the same regressions with only LBO deals. The result of this LBO deal-only specification is displayed in Table A19. While the magnitude of the regression coefficients are smaller and the statistical significance is also a bit weaker, the results are qualitatively similar to those in Table 11.

It is worth noting that empirical strategies employed in this subsection are not as robust as those in the previous subsection and Section 3, where (a version of) difference-in-difference estimation techniques are utilized. Because covenants are determined at loan issuance, at a certain point of time rather than continuously over time, we cannot use a similar approach on measures of covenant strictness. Similarly, due to data limitation, a difference-in-difference estimation strategy cannot be utilized for covenant compliance measures. With these caveats in mind, above results provide evidence more in line with the theory of risk shifting. Yet, these results do not necessarily reject the debt overhang narrative.

# 5 Conclusions

We study a relation between pre-buyout credit market conditions and post-buyout firm behavior, particularly (operating) performance. To overcome the lack of (systematic) data on post-LBO target financial information, a supervisory dataset (FR Y-14Q) is employed.

An LBO-specific measure of (changes of) pre-buyout credit market conditions, which is the short-term (6-month) credit spread change leading up to buyout close, is proposed. The short-term spread change has two advantages over the level of credit spreads, which is a widely used measure of credit market conditions in the literature. First, the proposed measure is largely uncorrelated with the valuation of the target company and, hence, can be interpreted as (changes of) pre-LBO credit market conditions controlling for target valuation. This property of the measure helps us to separate out the impact of credit market conditions from that of equity market valuations, which are often thought of as correlated with credit

market conditions. Second, the short-term change of credit spreads leading up to buyout close is unlikely to affect the target selection process and, consequently, unlikely to be correlated with possible selection biases.

Using the proposed measure as a proxy of (changes of) pre-LBO credit market conditions, we first find that the short-term change of credit spreads is negatively associated with buyout leverage, defined by the amount of debt used for the LBO to the enterprise value of the target. This result is largely consistent with previous studies including Axelson et al. (2013) that show the level of credit spreads is negatively correlated with buyout leverage. It is further shown that when decomposing the level of credit spreads at buyout close into the level 6 months before buyout close and the 6-month change, the latter, which is also our proposed measure, explains the vast majority of the impact of the level of credit spreads at buyout on buyout leverage. Then, we show that pre-buyout credit spread tightening (loosening credit market conditions) is related to poor post-buyout target performance. This result is largely consistent with theories of agency costs of debt such as risk shifting and debt overhang but not supportive of the disciplinary effects of debt narrative. We provide further evidence that supports the narrative of agency costs of debt—the post-buyout probability of default for target companies is higher for LBOs done during credit spread tightening. Our investigation of post-buyout covenant compliance gives somewhat favorable results for the risk-shifting channel in particular.

As a final note, the main focus of this paper is the intensive margin: how pre-buyout credit market conditions affect the performance of targets of *already closed* LBO deals. The short-term change of credit spreads is proposed as a measure of pre-buyout credit market conditions to address that particular aspect. However, the extensive margin—how pre-buyout credit market conditions affect the *population* of LBO deals over the credit cycle—, which is a valid channel through which credit market conditions may affect the economy, is not a subject of this study and can be a topic for future research.

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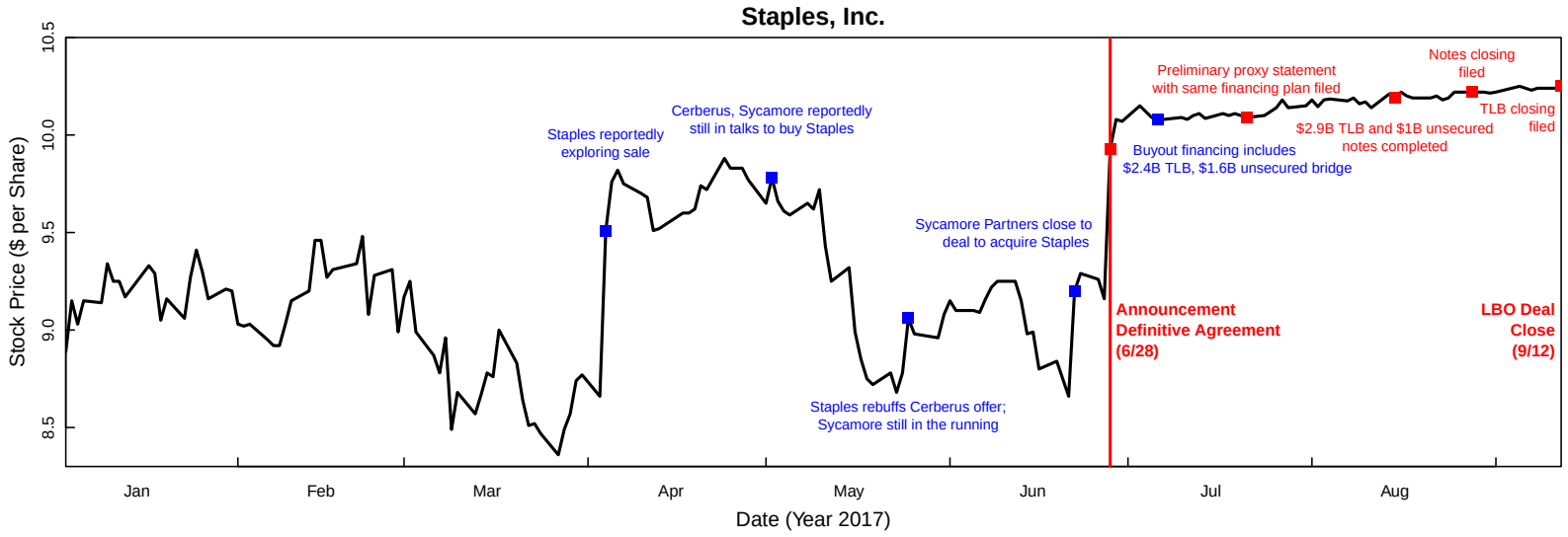
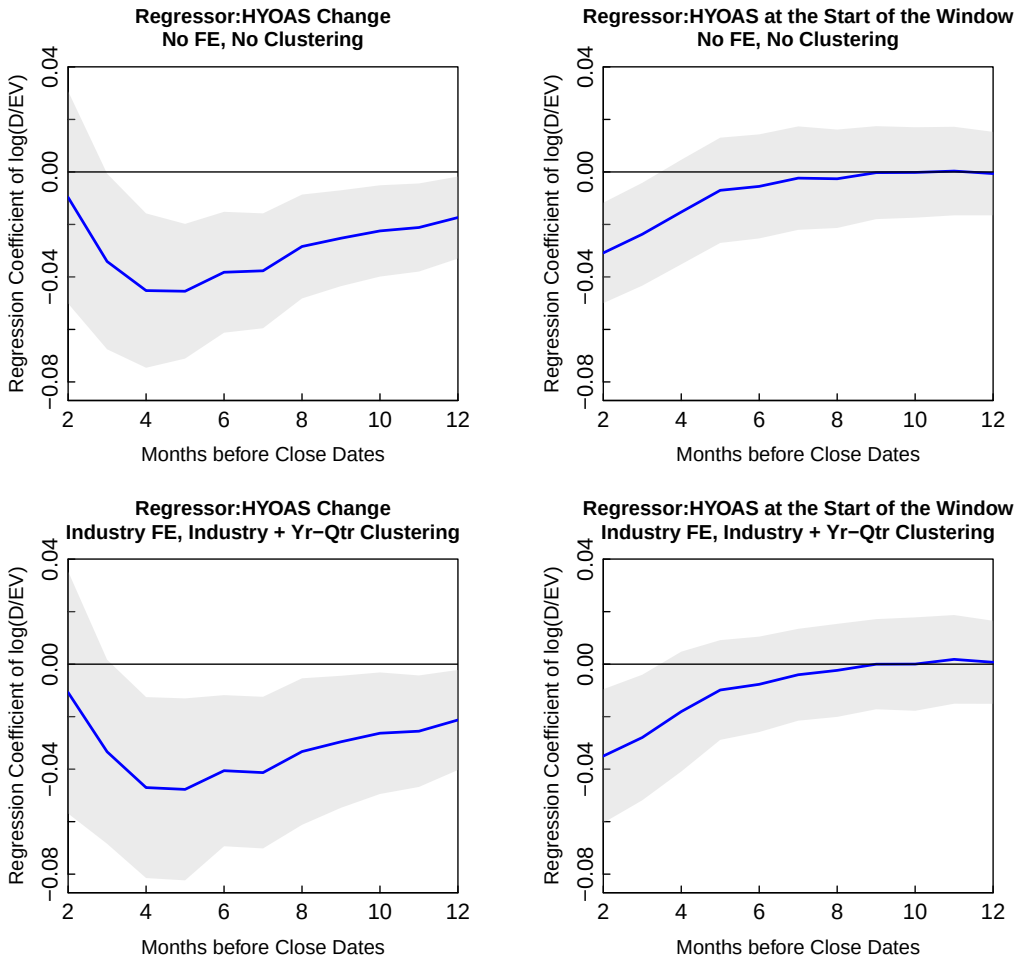
Figure 1: Example - Staples, Inc. LBO Timeline and Stock Prices




Note: This figure plots stock prices of Staples, Inc. (NASDAQ: SPLS) before its buyout by Sycamore Partners Management, L.P. in 2017. The chart starts at the beginning of 2017, the red vertical line indicates the date of the LBO announcement/definitive agreement, and the chart ends at the buyout's close on 12 September 2017. Blue square dots indicate unofficial rumors/news, and red square dots indicate official news/SEC filings.

Figure 2: Univariate Regression Coefficient of $\log\left(\frac{D}{EV}\right)$




Note: Sample period is from 1997 through 2022 with 901 LBO transactions. This figure displays regression coefficients for a univariate regression of $\log\left(\frac{D}{EV}\right) = \beta(\text{HY OAS Change}) + \epsilon$ on the left column and those of $\log\left(\frac{D}{EV}\right) = \beta(\text{HY OAS at Start}) + \epsilon$ on the right column. (HY OAS Change) refers to the change of the ICE BofA US High Yield Index Option-Adjusted Spread from $n$ months before the LBO close date through the LBO close date, where $n$ is indicated in the $x$-axis of each chart. Similarly, (HY OAS at Start) refers to the level of the ICE BofA US High Yield Index Option-Adjusted Spread at $n$ months before the LBO close date. The gray area represents a 95% confidence interval.

Table 1: Summary Statistics for the FR Y-14Q Sample

| | Truncated Mean | 25th Pctl | Median | 75th Pctl | IQR | N Obs |
|---|---|---|---|---|---|---|
| Total Assets ($mil) | 2269.195 | 10.108 | 39.473 | 633.45 | 623.342 | 1549903 |
| Total Liabilities ($mil) | 1500.392 | 5.686 | 22.996 | 375.583 | 369.897 | 1547491 |
| Net Sales ($mil) | 1522.939 | 22.161 | 76.303 | 598.29 | 576.129 | 1549903 |
| Book Leverage | 0.637 | 0.466 | 0.643 | 0.797 | 0.331 | 1547491 |
| $\frac{\text{Sale}}{\text{Assets}}$ | 2.077 | 0.813 | 1.757 | 2.976 | 2.163 | 1549903 |
| $\frac{\text{Interest}}{\text{Assets}}$ | 0.016 | 0.006 | 0.012 | 0.022 | 0.016 | 1449296 |
| $\frac{\text{Interest}}{\text{Liabilities}}$ | 0.024 | 0.011 | 0.021 | 0.033 | 0.022 | 1447686 |
| $\frac{\text{Current Liabilities}}{\text{Liabilities}}$ | 0.585 | 0.283 | 0.588 | 0.903 | 0.62 | 1543178 |
| $\frac{\text{Long-term Liabilities}}{\text{Liabilities}}$ | 0.4 | 0.148 | 0.389 | 0.625 | 0.476 | 1260847 |
| $\frac{\text{Current Assets}}{\text{Assets}}$ | 0.542 | 0.265 | 0.557 | 0.831 | 0.566 | 1547018 |
| $\frac{\text{Cash}}{\text{Assets}}$ | 0.113 | 0.02 | 0.065 | 0.16 | 0.14 | 1486122 |
| $\frac{\text{Tangible Assets}}{\text{Assets}}$ | 0.884 | 0.844 | 0.983 | 1 | 0.156 | 1533864 |
| $\frac{\text{Retained Earning}}{\text{Assets}}$ | 0.265 | 0.073 | 0.266 | 0.486 | 0.413 | 1487470 |
| $\frac{\text{CapEx}}{\text{Assets}}$ | 0.055 | 0.011 | 0.031 | 0.072 | 0.061 | 1031061 |
| $\frac{\text{Net Income}}{\text{Sale}}$ | 0.049 | 0.008 | 0.033 | 0.083 | 0.075 | 1544965 |
| $\frac{\text{Net Income}}{\text{Assets}}$ | 0.08 | 0.015 | 0.052 | 0.117 | 0.102 | 1544965 |
| $\frac{\text{Net Income}}{\text{Book Equity}}$ | 0.248 | 0.051 | 0.152 | 0.337 | 0.286 | 1439827 |
| $\frac{\text{Operating Income}}{\text{Sale}}$ | 0.073 | 0.012 | 0.047 | 0.113 | 0.102 | 1411726 |
| $\frac{\text{Operating Income}}{\text{Assets}}$ | 0.093 | 0.023 | 0.068 | 0.136 | 0.113 | 1411726 |
| $\frac{\text{Operating Income}}{\text{Book Equity}}$ | 0.302 | 0.066 | 0.191 | 0.39 | 0.324 | 1314335 |
| $\frac{\text{EBITDA}}{\text{Sale}}$ | 0.122 | 0.026 | 0.077 | 0.168 | 0.142 | 1506156 |
| $\frac{\text{EBITDA}}{\text{Assets}}$ | 0.134 | 0.055 | 0.107 | 0.183 | 0.129 | 1506156 |
| $\frac{\text{EBITDA}}{\text{Book Equity}}$ | 0.455 | 0.144 | 0.29 | 0.532 | 0.388 | 1402895 |

Note: Sample period is from 2011 through 2022. For the computation of the truncated mean, the top and bottom 1% of each variable are trimmed to remove severe outliers. For the computation of the quartiles, the untrimmed full sample is used. "Book Leverage" refers to total liabilities to total assets. In denominators (and some in numerators), "Assets" indicate total (book) assets, "Liabilities" total liabilities, and "Sale" net sales, and "Book Equity" total assets minus total liabilities. For book equity, zero or negative values are discarded.

Table 2: Regression of LBO Transaction Leverage on the Level and Change of the HY OAS

| Dependent Variable: $\log\left(\frac{D}{EV}\right)$ | | | |
|---|---|---|---|
| HY OAS at close | -0.033 ** | | |
| | (0.013) | | |
| HY OAS at start | | -0.008 | |
| | | (0.009) | |
| HY OAS change from 6mo before close | | | -0.041 *** |
| | | | (0.015) |
| Fixed effects | Industry | | |
| Clustering | Industry, Yr-Qtr | | |
| N | 882 | 882 | 882 |
| Adj-$R^2$ | 0.039 | 0.026 | 0.039 |

Note: Sample period is from 1997 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. $D$ refers to the total amount of debt, the sum of the loan amount from Dealscan and the bond amount from Mergent FISD, used for the corresponding LBO transaction. $EV$ refers to the (estimated) enterprise value of the target company at LBO. Industry refers to 69 industries based on the Global Industry Classification Standard (GICS).

Table 3: Regression of the LBO Transaction Leverage Decomposition on the Level and Change of the High-Yield Option-Adjusted Spread

(a) Scaled by EBITDA

| Dependent Variable: | $\log\left(\frac{D}{EV}\right)$ | | | $\log\left(\frac{D}{EBITDA}\right)$ | | | $\log\left(\frac{EV}{EBITDA}\right)$ | | |
|---|---|---|---|---|---|---|---|---|---|
| HY OAS at close | -0.039 * | | | -0.081 *** | | | -0.042 ** | | |
| | (0.02) | | | (0.024) | | | (0.016) | | |
| HY OAS at start | | -0.01 | | | -0.037 * | | | -0.027 | |
| | | (0.015) | | | (0.02) | | | (0.021) | |
| HY OAS change from 6mo before close | | | -0.044 ** | | | -0.068 * | | | -0.024 |
| | | | (0.02) | | | (0.036) | | | (0.028) |
| Fixed effects | Industry | | | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | | | |
| N | 474 | 474 | 474 | 474 | 474 | 474 | 474 | 474 | 474 |
| Adj-$R^2$ | 0.054 | 0.042 | 0.053 | 0.159 | 0.136 | 0.144 | 0.215 | 0.208 | 0.206 |

(b) Scaled by Net Sales

| Dependent Variable: | $\log\left(\frac{D}{EV}\right)$ | | | $\log\left(\frac{D}{Sale}\right)$ | | | $\log\left(\frac{EV}{Sale}\right)$ | | |
|---|---|---|---|---|---|---|---|---|---|
| HY OAS at close | -0.025 * | | | -0.065 *** | | | -0.04 ** | | |
| | (0.015) | | | (0.02) | | | (0.019) | | |
| HY OAS at start | | -0.003 | | | -0.05 *** | | | -0.047 ** | |
| | | (0.011) | | | (0.017) | | | (0.019) | |
| HY OAS change from 6mo before close | | | -0.031 ** | | | -0.032 | | | 0 |
| | | | (0.013) | | | (0.02) | | | (0.015) |
| Fixed effects | Industry | | | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | | | |
| N | 582 | 582 | 582 | 582 | 582 | 582 | 582 | 582 | 582 |
| Adj-$R^2$ | 0.088 | 0.081 | 0.089 | 0.295 | 0.285 | 0.279 | 0.345 | 0.347 | 0.336 |

Note: Sample period is from 1997 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. $D$ refers to the total amount of debt, the sum of the loan amount from Dealscan and the bond amount from Mergent FISD, used for the corresponding LBO transaction. $EV$ refers to the (estimated) enterprise value of the target company at LBO. $Sale$ refers to net sales. Industry refers to 69 industries based on the Global Industry Classification Standard (GICS).

Table 4: Regression of LBO Transaction Leverage on the Level and Change of the High-Yield Option-Adjusted Spread between Signing and Close

| Dependent Variable: $\log\left(\frac{D}{EV}\right)$ | | | |
|---|---|---|---|
| HY OAS at close | -0.033 ** | | |
| | (0.015) | | |
| HY OAS at signing | | -0.015 | |
| | | (0.014) | |
| HY OAS change from signing to close | | | -0.066 ** |
| | | | (0.028) |
| Fixed effects | Industry | | |
| Clustering | Industry, Yr-Qtr | | |
| N | 776 | 776 | 776 |
| Adj-$R^2$ | 0.043 | 0.032 | 0.048 |

Note: Sample period is from 1997 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. $D$ refers to the total amount of debt, the sum of the loan amount from Dealscan and the bond amount from Mergent FISD, used for the corresponding LBO transaction. $EV$ refers to the (estimated) enterprise value of the target company at LBO. Industry refer to 69 industries based on the Global Industry Classification Standard (GICS). To be included in the sample, the deal close date needs to be after the deal signing date.

Table 5: Regression of the Changes of Financial Variables after LBOs on the 6-month Change of High-Yield Option-Adjusted Spread; LBO Targets Only

(a) Changes of Balance Sheet and Cash Flow Variables

| | log(Assets) | log(Liabilities) | log(Sale) | Leverage | $\frac{\text{Sale}}{\text{Assets}}$ | $\frac{\text{Interest}}{\text{Assets}}$ | $\frac{\text{Interest}}{\text{Liabilities}}$ |
|---|---|---|---|---|---|---|---|
| HY OAS Change | -0.02 | -0.044 ** | 0.015 | -0.014 *** | 0.017 | -0.001 | -0.001 |
| | (0.016) | (0.018) | (0.017) | (0.004) | (0.014) | (0.001) | (0.001) |
| Firm-level controls | | | | Yes | | | |
| Fixed effects | | | | Industry | | | |
| Clustering | | | | Industry, Yr-Qtr | | | |
| N | 854 | 854 | 854 | 854 | 854 | 760 | 761 |
| Adj-$R^2$ | 0.176 | 0.259 | 0.039 | 0.575 | 0.507 | 0.251 | 0.084 |

| | $\frac{\text{Current Liabilities}}{\text{Liabilities}}$ | $\frac{\text{Long-term Liabilities}}{\text{Liabilities}}$ | $\frac{\text{Current Assets}}{\text{Assets}}$ | $\frac{\text{Cash}}{\text{Assets}}$ | $\frac{\text{Tangible Assets}}{\text{Assets}}$ | $\frac{\text{Retained Earning}}{\text{Assets}}$ | $\frac{\text{CAPX}}{\text{Assets}}$ |
|---|---|---|---|---|---|---|---|
| HY OAS Change | 0.015 | -0.013 | 0.011 ** | 0.004 *** | 0.004 | 0.023 | 0 |
| | (0.01) | (0.011) | (0.004) | (0.001) | (0.006) | (0.019) | (0.002) |
| Firm-level controls | | | | Yes | | | |
| Fixed effects | | | | Industry | | | |
| Clustering | | | | Industry, Yr-Qtr | | | |
| N | 854 | 670 | 853 | 746 | 854 | 731 | 622 |
| Adj-$R^2$ | 0.186 | 0.074 | 0.296 | 0.112 | 0.331 | 0.276 | 0.022 |

(b) Changes of Target Performance Variables

| | $\frac{\text{Net Income}}{\text{Sale}}$ | $\frac{\text{Net Income}}{\text{Assets}}$ | $\frac{\text{Net Income}}{\text{Book Equity}}$ | $\frac{\text{Operating Income}}{\text{Sale}}$ | $\frac{\text{Operating Income}}{\text{Assets}}$ | $\frac{\text{Operating Income}}{\text{Book Equity}}$ | $\frac{\text{EBITDA}}{\text{Sale}}$ | $\frac{\text{EBITDA}}{\text{Assets}}$ | $\frac{\text{EBITDA}}{\text{Book Equity}}$ |
|---|---|---|---|---|---|---|---|---|---|
| HY OAS Change | 0.008 ** | 0.015 *** | 0.056 ** | 0.009 * | 0.012 *** | 0.117 * | 0.008 ** | 0.01 *** | 0.056 ** |
| | (0.003) | (0.004) | (0.025) | (0.004) | (0.003) | (0.057) | (0.004) | (0.003) | (0.025) |
| Firm-level controls | | | | | Yes | | | | |
| Fixed effects | | | | | Industry | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |
| N | 848 | 848 | 678 | 729 | 729 | 581 | 852 | 852 | 683 |
| Adj-$R^2$ | 0.112 | 0.338 | 0.077 | 0.14 | 0.543 | 0.125 | 0.096 | 0.6 | 0.136 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table 6: Regression of the Changes of Financial Variables after LBOs on the 6-month Change of High-Yield Option-Adjusted Spread; LBO Targets and Control Firms

(a) Changes of Balance Sheet and Cash Flow Variables

| | log(Assets) | log(Liabilities) | log(Sale) | Leverage | $\frac{\text{Sale}}{\text{Assets}}$ | $\frac{\text{Interest}}{\text{Assets}}$ | $\frac{\text{Interest}}{\text{Liabilities}}$ |
|---|---|---|---|---|---|---|---|
| LBO Dummy | 0.382 *** | 0.566 *** | 0.043 ** | 0.084 *** | -0.432 *** | 0.01 *** | 0.013 *** |
| | (0.036) | (0.03) | (0.019) | (0.012) | (0.045) | (0.001) | (0.001) |
| HY OAS Change | 0.021 * | 0.033 ** | 0 | 0.012 *** | -0 | 0.001 | 0 |
| | (0.011) | (0.014) | (0.012) | (0.003) | (0.022) | (0.001) | (0.001) |
| LBO Dummy × | -0.02 | -0.058 *** | 0.023 | -0.018 ** | 0.032 | -0.001 | 0 |
| HY OAS Change | (0.017) | (0.014) | (0.014) | (0.008) | (0.02) | (0.001) | (0.001) |
| Firm-level controls | | | | Yes | | | |
| Fixed effects | | | | Industry, Yr-Qtr | | | |
| Clustering | | | | Industry, Yr-Qtr | | | |
| N of LBOs | 842 | 842 | 851 | 822 | 851 | 726 | 728 |
| N | 4103 | 4101 | 4158 | 4012 | 4130 | 3400 | 3390 |
| Adj-$R^2$ | 0.136 | 0.185 | 0.041 | 0.195 | 0.252 | 0.126 | 0.125 |

| | $\frac{\text{Current Liabilities}}{\text{Liabilities}}$ | $\frac{\text{Long-term Liabilities}}{\text{Liabilities}}$ | $\frac{\text{Current Assets}}{\text{Assets}}$ | $\frac{\text{Cash}}{\text{Assets}}$ | $\frac{\text{Tangible Assets}}{\text{Assets}}$ | $\frac{\text{Retained Earning}}{\text{Assets}}$ | $\frac{\text{CAPX}}{\text{Assets}}$ |
|---|---|---|---|---|---|---|---|
| LBO Dummy | -0.163 *** | 0.109 *** | -0.131 *** | -0.032 *** | -0.181 *** | -0.131 *** | -0.004 *** |
| | (0.008) | (0.006) | (0.009) | (0.004) | (0.011) | (0.02) | (0.001) |
| HY OAS Change | -0.013 ** | 0.002 | -0.005 | -0.002 | 0.001 | -0.016 * | -0.002 |
| | (0.006) | (0.005) | (0.004) | (0.002) | (0.005) | (0.008) | (0.001) |
| LBO Dummy × | 0.014 | -0.014 | 0.009 | -0 | 0.008 * | 0.016 | 0.002 |
| HY OAS Change | (0.008) | (0.009) | (0.007) | (0.004) | (0.004) | (0.012) | (0.002) |
| Firm-level controls | | | | Yes | | | |
| Fixed effects | | | | Industry, Yr-Qtr | | | |
| Clustering | | | | Industry, Yr-Qtr | | | |
| N of LBOs | 844 | 628 | 833 | 722 | 825 | 696 | 576 |
| N | 4100 | 2680 | 4060 | 3400 | 4022 | 3411 | 2404 |
| Adj-$R^2$ | 0.131 | 0.077 | 0.196 | 0.051 | 0.273 | 0.094 | 0.013 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

(b) Changes of Target Performance Variables

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets |
|---|---|---|---|---|---|
| LBO Dummy | -0.055 *** | -0.057 *** | -0.209 *** | -0.048 *** | -0.064 *** |
| | (0.008) | (0.007) | (0.046) | (0.008) | (0.005) |
| HY OAS Change | 0.002 | 0.004 | -0.003 | 0.002 | 0.003 |
| | (0.003) | (0.004) | (0.014) | (0.002) | (0.006) |
| LBO Dummy × | 0.009 ** | 0.01 *** | 0.063 *** | 0.012 *** | 0.01 *** |
| HY OAS Change | (0.003) | (0.003) | (0.019) | (0.004) | (0.003) |
| Firm-level controls | | | Yes | | |
| Fixed effects | | | Industry, Yr-Qtr | | |
| Clustering | | | Industry, Yr-Qtr | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 |
| Adj-$R^2$ | 0.091 | 0.148 | 0.129 | 0.102 | 0.229 |

| | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|
| LBO Dummy | -0.143 *** | -0.007 | -0.051 *** | -0.08 ** |
| | (0.024) | (0.006) | (0.005) | (0.033) |
| HY OAS Change | -0.015 | 0.004 ** | 0.002 | -0.014 |
| | (0.03) | (0.002) | (0.003) | (0.025) |
| LBO Dummy × | 0.089 *** | 0.008 ** | 0.009 *** | 0.074 *** |
| HY OAS Change | (0.023) | (0.003) | (0.002) | (0.019) |
| Firm-level controls | | | Yes | |
| Fixed effects | | | Industry, Yr-Qtr | |
| Clustering | | | Industry, Yr-Qtr | |
| N of LBOs | 537 | 857 | 849 | 664 |
| N | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.147 | 0.056 | 0.219 | 0.147 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table 7: Pre- and Post-buyout Trends: Sensitivity of Difference of Performance Variables between the LBO Targets and non-LBO Control Firms, Relative to a Year before LBO, to HY OAS Change

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| 6 yrs before LBO | -0.002 | -0.006 | -0.046 | 0.002 | -0.004 | -0.055 | -0 | -0.003 | -0.081 |
| | (0.007) | (0.008) | (0.055) | (0.005) | (0.006) | (0.053) | (0.004) | (0.005) | (0.079) |
| | 273/1896 | 262/1838 | 211/1488 | 219/1544 | 207/1458 | 173/1228 | 267/1860 | 254/1782 | 203/1430 |
| 5 yrs before LBO | -0.002 | -0.006 | -0.006 | 0.003 | 0.007 | 0.03 | 0.002 | 0.001 | 0.004 |
| | (0.004) | (0.006) | (0.026) | (0.003) | (0.007) | (0.04) | (0.002) | (0.004) | (0.05) |
| | 421/3096 | 403/2966 | 336/2466 | 331/2448 | 322/2378 | 265/1974 | 414/3032 | 401/2956 | 325/2386 |
| 4 yrs before LBO | 0.003 | 0.003 | 0.02 | 0.005 * | 0.005 | 0.031 | 0.002 | 0.003 | 0.005 |
| | (0.003) | (0.003) | (0.02) | (0.003) | (0.004) | (0.037) | (0.003) | (0.004) | (0.052) |
| | 557/4466 | 546/4414 | 457/3634 | 484/3888 | 471/3800 | 394/3158 | 554/4442 | 535/4324 | 453/3594 |
| 3 yrs before LBO | 0 | 0.002 | 0.004 | 0 | 0.005 *** | 0.023 | 0.001 | 0.004 | -0.01 |
| | (0.002) | (0.002) | (0.016) | (0.002) | (0.002) | (0.018) | (0.002) | (0.002) | (0.029) |
| | 791/6904 | 769/6758 | 650/5604 | 697/6098 | 678/5972 | 577/4996 | 786/6826 | 762/6654 | 647/5558 |
| 2 yrs before LBO | 0 | -0.002 | 0 | 0.002 | 0.001 | -0.004 | 0.002 | 0.001 | -0.027 |
| | (0.002) | (0.003) | (0.011) | (0.001) | (0.002) | (0.019) | (0.002) | (0.003) | (0.03) |
| | 1045/10030 | 1013/9762 | 868/8194 | 943/9104 | 916/8860 | 785/7472 | 1036/9954 | 1007/9682 | 861/8146 |
| 1 yr after LBO | 0.008 * | 0.004 | 0.011 | 0.013 *** | 0.007 ** | 0.043 *** | 0.008 *** | 0.009 *** | 0.039 *** |
| | (0.004) | (0.003) | (0.014) | (0.002) | (0.003) | (0.014) | (0.002) | (0.002) | (0.013) |
| | 894/8306 | 878/8220 | 766/6944 | 740/6924 | 738/6950 | 639/5842 | 880/8190 | 876/8206 | 753/6854 |
| 2 yrs after LBO | 0.006 | 0.001 | 0.021 | 0.012 *** | 0.005 | 0.043 *** | 0.003 | 0.005 | 0.03 |
| | (0.005) | (0.006) | (0.016) | (0.003) | (0.004) | (0.011) | (0.003) | (0.003) | (0.025) |
| | 792/6734 | 771/6576 | 658/5420 | 679/5658 | 670/5634 | 563/4576 | 801/6826 | 787/6702 | 659/5422 |
| 3 yrs after LBO | 0.007 | -0.001 | 0.02 | 0.014 *** | 0.006 * | 0.07 *** | 0.002 | 0.002 | 0.07 * |
| | (0.005) | (0.005) | (0.019) | (0.002) | (0.003) | (0.019) | (0.003) | (0.004) | (0.038) |
| | 627/4846 | 617/4780 | 489/3708 | 524/4026 | 526/4028 | 410/3050 | 632/4944 | 628/4904 | 494/3756 |
| 4 yrs after LBO | 0.016 *** | 0.006 | 0.059 *** | 0.01 *** | 0.015 *** | 0.069 ** | 0.003 * | 0.008 *** | 0.13 *** |
| | (0.003) | (0.005) | (0.02) | (0.002) | (0.005) | (0.027) | (0.002) | (0.003) | (0.027) |
| | 470/3360 | 464/3282 | 344/2440 | 391/2692 | 385/2664 | 278/1914 | 481/3436 | 475/3376 | 355/2522 |
| 5 yrs after LBO | 0.008 | 0.01 * | 0.066 * | 0.01 ** | 0.011 ** | 0.134 ** | 0.008 | 0.011 *** | 0.114 ** |
| | (0.007) | (0.005) | (0.034) | (0.004) | (0.005) | (0.052) | (0.005) | (0.003) | (0.047) |
| | 320/2134 | 318/2112 | 210/1414 | 255/1658 | 251/1632 | 166/1090 | 325/2176 | 325/2172 | 216/1444 |
| Firm-level controls | | | | | No | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. HY OAS Change refers to the 6-month change of the HY OAS leading up to buyout close. Industry refers to 2-digit NAICS code. Dependent variables are the level at the corresponding period. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table 8: Regression of the Changes of Post-LBO Target Performance Variables on the Pre-LBO 6-month Change of High-Yield Option-Adjusted Spread and the Lag and Lead 6-month Changes; LBO Targets and Control Firms

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy | -0.054 *** | -0.057 *** | -0.21 *** | -0.05 *** | -0.064 *** | -0.143 *** | -0.008 | -0.051 *** | -0.078 ** |
| | (0.007) | (0.007) | (0.047) | (0.008) | (0.005) | (0.018) | (0.005) | (0.005) | (0.028) |
| Lag HY OAS Chg | 0.001 | 0.004 | -0.019 | 0.001 | -0 | -0.044 | 0 | 0.002 | -0.025 |
| | (0.003) | (0.002) | (0.027) | (0.004) | (0.004) | (0.047) | (0.002) | (0.003) | (0.031) |
| HY OAS Chg | 0.004 | 0.004 | -0.017 | 0.001 | 0.002 | -0.013 | 0.004 * | 0.002 | -0.008 |
| | (0.003) | (0.005) | (0.014) | (0.002) | (0.007) | (0.038) | (0.002) | (0.003) | (0.03) |
| Lead HY OAS Chg | 0.004 | -0.007 | -0.005 | -0 | -0.003 | 0.035 | 0.002 * | -0.003 | 0.031 |
| | (0.004) | (0.005) | (0.02) | (0.003) | (0.004) | (0.033) | (0.001) | (0.003) | (0.022) |
| LBO Dummy × | 0.005 | 0.002 | -0.003 | -0.007 ** | -0.004 | -0.008 | -0.006 ** | 0 | 0.013 |
| Lag HY OAS Change | (0.004) | (0.006) | (0.027) | (0.003) | (0.003) | (0.03) | (0.002) | (0.003) | (0.033) |
| LBO Dummy × | 0.009 ** | 0.01 *** | 0.063 *** | 0.012 *** | 0.01 *** | 0.088 *** | 0.007 ** | 0.009 *** | 0.072 *** |
| HY OAS Chg | (0.003) | (0.002) | (0.02) | (0.003) | (0.003) | (0.022) | (0.003) | (0.002) | (0.016) |
| LBO Dummy × | -0.004 | -0.004 | -0.012 | 0.002 | -0.001 | 0.044 ** | 0.001 | 0.001 | 0.073 *** |
| Lead HY OAS Chg | (0.004) | (0.005) | (0.026) | (0.003) | (0.003) | (0.019) | (0.002) | (0.003) | (0.015) |
| Firm-level controls | | | | | Yes | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 | 537 | 857 | 849 | 664 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.091 | 0.149 | 0.128 | 0.103 | 0.228 | 0.15 | 0.057 | 0.219 | 0.15 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Lag HY OAS Change refers to the change of the HY OAS from 12 months before LBO close through 6 months before close. Lead HY OAS Change refers to the change of the HY OAS from LBO close through 6 months after close. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table 9: Regression of the Pre-Post LBO Change of Log of Probability of Default Assigned by Banks on the High-Yield Option-Adjusted Spread

| Dependent Variable: $\Delta \log$(Probability of Default) | | | |
|---|---|---|---|
| LBO Dummy | 0.552 *** | 0.554 *** | 0.552 *** |
| | (0.109) | (0.102) | (0.096) |
| HY OAS at Close | 0.03 | | |
| | (0.184) | | |
| HY OAS at Start | | -0.163 | |
| | | (0.13) | |
| HY OAS Change | | | 0.134 |
| | | | (0.123) |
| LBO Dummy × | -0.125 * | | |
| HY OAS at Close | (0.064) | | |
| LBO Dummy × | | 0.077 | |
| HY OAS at Start | | (0.051) | |
| LBO Dummy × | | | -0.194 *** |
| HY OAS Change | | | (0.054) |
| Firm-level controls | Yes | | |
| Additional controls | $\log$ ((Probability of Default)$_{start}$) | | |
| Fixed effects | Industry, Bank, Yr-Qtr | | |
| Clustering | Industry, Yr-Qtr | | |
| N of LBOs | 275 | 275 | 275 |
| N of LBO-Bank Pairs | 345 | 345 | 345 |
| N | 1339 | 1339 | 1339 |
| Adj-$R^2$ | 0.161 | 0.16 | 0.166 |

Note: Sample period is from 2012 through 2022, but almost all of the sample is from the end of 2014. Probability of default refers to default probability for firms assigned by reporting banks in FR Y-14Q. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. The dependent variable is the change of log probability of default from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. (Probability of Default)$_{start}$ is the probability of default at the start of the window for estimating the change of log probability of default.

Table 10: Regression of the Strictness of Financial Covenants for LBO Loan Deals on the High-Yield Option-Adjusted Spread; LBO Deals and Control Deals

| | Dependent Variable: | | | | | |
|---|---|---|---|---|---|---|
| | Number of Covenants | | | Covenant Strictness Measure | | |
| LBO Dummy | 0.161 | 0.152 | 0.15 | -0.091 ** | -0.092 ** | -0.093 ** |
| | (0.168) | (0.175) | (0.183) | (0.035) | (0.038) | (0.04) |
| HY OAS at Close | -0.15 | | | -0.026 | | |
| | (0.137) | | | (0.05) | | |
| HY OAS at Start | | -0.139 | | | -0.072 | |
| | | (0.207) | | | (0.073) | |
| HY OAS Change | | | 0.01 | | | 0.019 |
| | | | (0.074) | | | (0.027) |
| LBO Dummy × | 0.179 * | | | 0.04 * | | |
| HY OAS at Close | (0.09) | | | (0.023) | | |
| LBO Dummy × | | 0.189 ** | | | 0.037 * | |
| HY OAS at Start | | (0.071) | | | (0.018) | |
| LBO Dummy × | | | -0.077 | | | -0.006 |
| HY OAS Change | | | (0.075) | | | (0.021) |
| Firm-level controls | Yes | | | | | |
| Additional controls | log(N of Lenders) | | | | | |
| Fixed effects | Industry, Lead Agent, Yr-Qtr | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | |
| N of LBO Loan Deals | 191 | 191 | 191 | 191 | 191 | 191 |
| N of LBO Obs | 219 | 219 | 219 | 219 | 219 | 219 |
| N | 676 | 676 | 676 | 676 | 676 | 676 |
| Adj-$R^2$ | 0.309 | 0.313 | 0.292 | 0.322 | 0.325 | 0.313 |

Note: Sample period is from 1997 through 2019. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Observations are at the level of deal-lead agent pairs. Lead agents are identified following Ivashina (2009). The covenant strictness measure is computed following Murfin (2012).

Table 11: Regression of Post-LBO Covenant Compliance on the High-Yield Option-Adjusted Spread; LBO Deals and Control Deals

| | Dependent Variable: | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Post-LBO Covenant Breach Dummy | | | Post-LBO Covenant Compliance with Waiver/Amendment Dummy | | | Post-LBO Waiver/Amendment Dummy | | |
| LBO Dummy | -0.03 ** | -0.028 ** | -0.029 ** | -0.022 | -0.023 | -0.025 | -0.066 ** | -0.065 ** | -0.067 ** |
| | (0.012) | (0.011) | (0.011) | (0.017) | (0.018) | (0.017) | (0.029) | (0.029) | (0.029) |
| HY OAS at Close | 0.007 | | | -0.004 | | | 0.004 | | |
| | (0.011) | | | (0.011) | | | (0.01) | | |
| HY OAS at Start | | -0.025 ** | | | 0.006 | | | 0.014 | |
| | | (0.011) | | | (0.014) | | | (0.022) | |
| HY OAS Change | | | 0.015 * | | | -0.004 | | | 0.002 |
| | | | (0.008) | | | (0.008) | | | (0.007) |
| LBO Dummy × | 0.005 | | | 0.023 *** | | | 0.019 | | |
| HY OAS at Close | (0.004) | | | (0.006) | | | (0.012) | | |
| LBO Dummy × | | 0.017 *** | | | 0.007 | | | 0.022 *** | |
| HY OAS at Start | | (0.005) | | | (0.006) | | | (0.008) | |
| LBO Dummy × | | | -0.016 *** | | | 0.016 ** | | | -0.009 |
| HY OAS Change | | | (0.005) | | | (0.006) | | | (0.011) |
| Additional controls | log(N of Participants), Agent Bank Share, Nonbank Share | | | | | | | | |
| Fixed effects | Industry, Review Bank, Review Date, Yr-Qtr | | | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | | | |
| N of LBOs | 412 | 412 | 412 | 412 | 412 | 412 | 412 | 412 | 412 |
| N of LBO Obs | 630 | 630 | 630 | 630 | 630 | 630 | 630 | 630 | 630 |
| N | 3535 | 3535 | 3535 | 3535 | 3535 | 3535 | 3535 | 3535 | 3535 |
| Adj-$R^2$ | 0.059 | 0.062 | 0.062 | 0.04 | 0.038 | 0.039 | 0.057 | 0.057 | 0.056 |

Note: Covenant compliance sample period is from 2007 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Observations are at the level of company-review bank-review date pairs. N of Participants is the number of unique entities that participate in any syndicated loans in SNC to the obligor. Agent bank share is the share of the agent bank's commitment out of the entire loan commitments (in SNC) to the obligor. Nonbank Share is the share of nonbanks' (identified by SNC) commitment out of the entire loan commitments (in SNC) to the obligor.

# Appendix A Institutional Details and Data Processing

## A.1 Institutional Details on LBO Timeline

An LBO is a type of financial transaction whereby an outside entity—usually a private equity firm—purchases a target company using significant amounts of external debt to finance the acquisition. An LBO is typically effectuated through a series of mergers and acquisitions (M&As) through newly-created companies and subsidiaries of PE funds. The simplified structure of an LBO is depicted in Figure A1.[32] PE funds acquiring the target of an LBO contribute to the equity portion of the buyout financing where the capital is coming from the limited partners and the PE sponsors. Banks (and sometimes non-banks) provide debt financing through a combination of term loans, high-yield bonds, and/or subordinated debt, where the vast majority of the LBO debt package is often sold to institutional investors.

The LBO transaction timeline is particularly important for justifying our measure of credit market conditions (changes of): the short-term (6-month) change of the HY OAS. Before describing the detailed timeline of a typical LBO deal, it is worth emphasizing that effectuating an LBO transaction is a quite lengthy and complex processes. There are various legal and regulatory requirements as well as financial and governance processes that need to be accomplished. Some steps occur prior to a definitive agreement (deal signing) while others occur afterwards. The timeline of typical LBO transactions are described in Figure A2.

Note that there is a positive relationship between deal size and deal complexity. Increased complexity generally leads to a longer time taken for completing the entire LBO process. Yet, LBO transactions are heterogeneous in many dimensions, making it difficult to specify a time frame for each process.

[32]For further information, see Levin and Rocap (2014).

### Pre-signing Period

Buyers, for the purpose of our paper, PE Firms, source deals by looking for companies that might be attractive targets in their portfolio. Once potential targets are identified, the buyer will contact the company directly or be introduced by an intermediary. The initial meetings are a chance for the two parties to gauge interest and viability on a potential transaction. Such introductions can take place quite a while before deal closing, possibly even a year before.

After initial meetings, if the (potential) target company wishes to engage, non-public information is provided to the potential acquirer to perform more specific due diligence. Based on this diligence, the buyer will decide if it wants to make an acquisition offer. The target company can accept, decline, or continue negotiating. Eventually, the buyer and the seller reach a merger/purchase agreement that is a fully binding contract. This contract explicitly states the rights and obligations for each party, the conditions for breaching those requirements, and the remedies in the event of such breach. For instance, the right to terminate a deal and the remediation that is necessary for a rightful termination are described. These points typically undergo material negotiation in finalizing the merger/purchase agreement.

### Post-signing Period

Once the agreement is finalized, a definitive agreement is signed. At this point buyer and seller begin to complete closing requirements. This can include, but is not limited to, completing the financing process, obtaining regulatory approvals, and getting shareholder approval. In terms of regulatory approvals, the size of the target as well as the jurisdictions in which it operates dictates the number and complexity of the approvals needed. Small target companies may be exempt from obtaining such regulatory approvals. For public-to-private deals, shareholders must approve of the LBO, which may take some time to operationalize and complete.

### Termination

Either the buyer or potential target company can terminate a deal. In the pre-signing period, a target company might reject a buyout proposal, or a buyer may determine not to bid on the company after due diligence. However, it becomes substantially more challenging to terminate after reaching a definitive agreement. Acceptable reasons for termination are agreed to in the merger agreement, and reasons outside of those are generally not permitted. Any party in breach of the deal terms would be liable for breakup fees. In additional to these fees, the terminating party is likely to be subject to a lawsuit and may still need to close the deal.[33]

## A.2 Data Cleaning, Filtering, and Matching Procedure

### LBO Debt Financing

For Mergent FISD, our first step is to use the issuer part—first six digits—of a corporate bond CUSIP to match to buyout targets in CIQ. For the remaining LBO transactions name matching is employed to match to bonds in FISD, although less than 10% of the matches are via name matching.[34] Corporate bond issuance within 60 days from the LBO close date is considered as (part of) the buyout debt. Yet, if the bond issuance is more than 30 days after the buyout close date, the issuance is only added if 1) there is an LBO loan deal closed within 30 days from the bond issuance date, and 2) the bond offering is not an exchange offering. If the maturity of the bond is longer than 20 years, the issuance is excluded. If there are multiple bond issuance on multiple dates for a single buyout deal, exchange offerings are excluded.

[33]The legal dispute between LVMH and Tiffany is a good example of such cases: https://www.reuters.com/article/tiffany-m-a-lvmh-idINKBN27E13L.

[34]Throughout this paper, our name matching algorithm is based on exact matching (after removing special characters and punctuation) instead of fuzzy matching. The exact matching is executed through three steps: 1) raw matching, 2) removing common non-informative words (e.g., "CO" and "INC") and matching, and 3) taking care of abbreviated words with punctuation (e.g., "U.S.") and matching.

For DealScan, multiple types of company IDs are utilized to match to buyout targets in CIQ: (a) CIK, (b) LEI, (c) Ticker, and (d) CUSIP in sequential order—for example, if an LBO deal is matched to DealScan based on the target's CIK number, the matched deal is excluded from LEI-based matching, and so forth. If there is no match based on the four types of company IDs, name matching is employed; for DealScan matching, roughly 30% of matches are done through name matching. Since loan purpose is provided in DealScan, only loan deals in three categories—leveraged buyout, sponsored buyout, management buyout—are taken. Also, similar to FISD-CIQ matches, loan deals closed outside 60 days from the LBO close date are excluded.

Roughly a quarter (∼6,700) of the nearly 26,000 LBO transactions have the target enterprise value. After those matching processes to FISD and DealScan, we are left with slightly below a thousand buyout deals from 1986 through 2022. The vast majority (around 90%) of those involve syndicated loans from DealScan, a much lower portion (around 30%) take corporate bonds in FISD, and only approximately 20% of the deals use both syndicated loans and corporate bonds for buyout debt. The buyout leverage is calculated as the sum of the amount of syndicated loans and corporate bonds used for the buyout divided by the target's enterprise value.

**FR Y-14Q Data Cleaning and Filtering**

To clean the data and filter out erroneously reported observations, we take quite a few steps along the lines of Greenwald, Krainer, and Paul (2020). Since the main company identifier that is common across multiple banks' reporting in FR Y-14Q is Taxpayer Identification Number (TIN)—for businesses, equivalent to Employer Identification Number (EIN)—, observations that do not have a TIN, roughly a quarter of the entire observations, are excluded. Then, observations that do not have the as-of date of financial information, roughly a half of the remaining observations, are deleted. These two steps get rid of roughly 60% of the

initial ∼18 million observations and leave us ∼7.2 million observations. Taking further steps remove roughly 1.2% of the remaining observations by deleting observations which: 1) have invalid or likely erroneous TINs, 2) are for non-US borrowers, 3) do not report any financial information of the borrower, and 4) report invalid date of financial information—those later than the corresponding FR Y-14Q reporting date.

Next, we apply a conservative threshold for the size of the total assets and net sales of borrowers: larger than $1 million both in total assets and net sales. Companies of a smaller size are very unlikely to be considered as a buyout target. This filter removes approximately 8% of the remaining observations, leaving ∼6.5 million observations. Then the sign of non-negative variables are fixed when negative numbers are reported for those variables.[35] Observations that are inconsistent with accounting rules are also excluded:

- Components of total assets—current assets, fixed assets (corresponding to PPENT in Compustat), tangible assets—must be not larger than total assets; the sum of current assets and fixed assets must be not larger than total assets.

- Components of current assets—inventories, cash and marketable securities, and accounts receivable—must be not larger than current assets; the sum of those three components must be not larger than current assets.

- Components of total liabilities—long-term debt and current liabilities—must be not larger than total liabilities; the sum of long-term debt and current liabilities must be not larger than total liabilities.

- A component of current liabilities—accounts payable—must be not larger than current liabilities; components of debt in current liabilities (another component of current

[35]Non-negative variables in FR Y-14Q are net sales, interest expenses, inventories, cash and marketable securities, fixed assets, tangible assets, current assets, total assets, long-term debt, current maturities of long-term debt, short-term debt, current liabilities, total liabilities, accounts receivable, accounts payable, capital expenditure, and depreciation and amortization.

liabilities)—current maturities of long-term debt (corresponding to DD1 in Compustat) and short-term debt (corresponding to NP in Compustat)—must be not larger than current liabilities; the sum of accounts payable, current maturities of long-term debt, and short-term debt must be not larger than current liabilities.

- EBITDA (corresponding to OIBDP in Compustat) and operating income (corresponding to OIADP in Compustat) must be not larger than net sales (corresponding to SALE in Compustat).

This procedure removes roughly 2.8% of the remaining observations, leaving ∼6.3 million observations. Then, among multiple observations for the same credit facility on the same date of financial information reported by the same bank, we exclude observations that are missing notable financial variables—more than 3 financial variable than the median number of non-missing financial variables for those multiple observations.

Subsequently, several steps are carried out to address possible "fat finger" errors. For each financial variable, a transformation of log base 10 $\frac{\log(x)}{\log(10)}$ is taken to capture its "digit" (e.g., $\frac{\log(1{,}000)}{\log(10)} = 3$). Within each bank-facility-date (of financial information) pair, observations with the average log value (across financial variables) deviating from the median of the average log value by more than 2 are excluded. Then, similarly within each bank-company-date pair, observations with the average log value (across financial variables) deviating from the median of the average log value by more than 2 are excluded. Lastly, within each bank-company-date pair, for each financial variable, observations with the log value deviating from the median of the log value (across multiple observation within the bank-company-date pair) by more than 2 are excluded. These steps remove roughly 1.6% of remaining observations, leaving ∼6.2 million observations.

Next we take unique observations for each bank-facility-date pair. First, we take observations with the most recent reporting date within each bank-facility-date (of financial

information) pair. This step removes quite a bit of observations—about 60% of the remaining observations. Then, among observations that still have multiple records within a bank-facility-date pair, those with the most recent "data update" date-time are taken. This step only removes a small portion (0.5%) of the remaining observations, leaving ∼2.5 million observations, but guarantees unique observations for each bank-facility-date pair. These steps are conducted based on a rationale that most recent reporting with most recent data update is likely to be more accurate.

Finally, the last step is to obtain unique observations for each bank-company-date pair. One issue is that sometimes different financial information is reported for multiple facilities of a single borrower by the same bank—clearly an error as financial information is at the company level. Another issue is that occasionally the value of 0 is reported instead of "NA" for some financial variables. To address these issues, within each bank-company-date pair, we take the median of non-zero values of each financial variable. If the (strict) majority of observations within a bank-company-date pair report non-zero values for the financial variable, the non-zero median is assigned to the variable, and otherwise 0.

Following all those steps, we end up with the sample of ∼1.6 million observations of unique bank-company-date pairs. There are unique ∼1.3 million company-date pairs in this sample—only a minor portion of the sample is reported by multiple banks. This final sample is taken as the baseline FR Y-14Q sample.

**LBO Target Financial Information**

The sample of LBO transactions of roughly 26,000 deals from CIQ is matched to the baseline FR Y-14Q sample of roughly 1.56 million observations for approximately 200,000 companies. For the FR Y-14Q sample period from 2011 through 2022, the number of LBO transactions in the CIQ sample is reduced to near 11,000 deals. Among those companies in the CIQ sample, only about 3,000 targets—a bit more than a quarter—are matched to the

FR Y-14Q sample based on TINs from various sources and name matching.[36]

Since target financial information around 3 years out from buyout close—a significant amount of time after buyout but short enough for the vast majority not to exit yet—is particularly considered for post-buyout behavior, we take a subset of LBO transactions through the end of 2019. Taking this subset reduces the number of matches between CIQ and FR Y-14Q to approximately 2,700 transactions for roughly 2,500 companies. Requiring the matched sample to have financial information at least once between 2 years before buyout close to 3 years after makes the sample slightly smaller to about 2,300 transactions.

Yet, further demanding the sample to have financial information both before and after buyout close reduces the size of the sample substantially to about 1,100 transactions. We also further restrict post-buyout financial information to be at least a year after buyout close because many cash flow variables in the FR Y-14Q data are the sum of the last 12 month values—those variables within a year after buyout close contains pre-buyout cash flows. This restriction decreases the sample size to about 1,000 transactions. Lastly, we limit pre- and post-buyout financial variables for each target company to those reported by the same bank. This filter is based on our observation that different banks sometimes report similar but slightly different values for the same financial variable.[37] This final step leaves us slightly less than 900 buyout transactions in the sample. In actual regressions in later part of the paper, observations with top and bottom 1% of each dependent variable are trimmed because all these procedures still leave a very small portion of outliers. In addition, the number of observations in each regression depends on the extent of how many observations are missing the financial variable of interest as well as control variables.

[36]Sources of TINs for CIQ data is largely from CIQ itself, but we complimented them with several other sources: S&P Cross Reference Services, Compustat, EDGAR Public Dissemination Service, and Bloomberg Credit Risk Corporate Structure. The (exact) name matching process only contributes to less than 10% of the matches.

[37]For instance, for net income, some banks report a non-adjusted value corresponding to NI in Compustat, while other banks report an adjusted value corresponding to NIADJ in Compustat.

### Covenant Information at Issuance

We match DealScan to Compustat to obtain financial information to 1) compute one of the covenant strictness measures—the loan contract strictness measure developed by Murfin (2012)—and 2) identify control firms and get control variables. CIK is the main identifier that links Compustat to DealScan in our match; less than a quarter of the firms in Compustat are missing CIK when the identifier is collected from WRDS SEC Analytics Suite and CIQ, in addition to Compustat itself.[38] The number of LBO deals that have covenant information becomes 380 when matched to Compustat, and requiring those LBO deals to have financial information within a year before the loan deal close reduces the number further to 259.

For the computation of the loan contract strictness measure largely following Murfin (2012), 11 types of financial ratios/variables are considered: debt-to-ebitda ratio, interest coverage ratio, fixed coverage ratio, current ratio, debt-to-tangible net worth ratio, debt-to-equity ratio, quick ratio, net worth, tangible net worth, EBITDA, and capital expenditure. Both the annual and quarterly version of Compustat are taken by augmenting the annual version by the quarterly one—this procedure helps to increase the number of covenants with the corresponding financial ratio/variable in data before the loan deal close.[39] The most recent financial ratios/variables within a year before a deal's close are used for computing the log difference between the current ratio and the "initial" ratio in the covenant data. To normalize the log difference, the variance-covariance matrix of the (quarterly difference of log) relevant financial ratios/variables is computed for each rolling past 10 years within each industry represented by the first digit of the SIC code. These procedures closely follow that of Murfin (2012).

The number of LBO sample decreases to 210 from 259 when limiting the sample to those

[38] Compustat contributes to ∼69% of the matches, WRDS SEC Analytics Suite ∼18%, and CIQ ∼13%.

[39] For deals with the strictness measure, the average number of covenants is 2.47, but the average number of covenants with their corresponding ratio is only 1.92 when only Compustat Quarterly is used. The number increases to 2.04 when Compustat Annual is taken together.

having the loan contract strictness measure. Then the lead bank of each deal is identified to address the lead bank's role in covenant determination, following Ivashina (2009). The final sample is comprised of 238 LBO loan deal-lead bank pairs. Note that we do not attempt to control for the lead bank share simply because only a small portion—less than 40 deals—of the already small number of observations are not missing their lead bank's allocation information.

**Covenant Compliance**

We match the LBO transaction sample from CIQ to SNC based on TIN and name matching, similar to the matching between CIQ and FR Y-14Q. Based on TIN, roughly 2,400 target companies in CIQ are matched, and (exact) name matching contributes to approximately 700 matches—less than a quarter of the entire matches. For LBO transactions that are completed during the SNC covenant data period from 2007 to 2022, the number of matches decreases to close to 2,100 target companies. Further allowing at least three years of post-buyout period—restricting the sample to those closed before the end of 2019—reduces the number to roughly 1,900 target companies. Among those, approximately 1,600 targets have matched SNC loans in the database, and close to 1,100 companies of those are matched to the covenant compliance data in SNC. For post-buyout covenant compliance from buyout close to 3 years out from buyout close, only 480 target companies for 510 LBO transactions are matched to the covenant data.

# Appendix B Measure of Credit Market Conditions

## B.1 Credit Spreads: Level v.s. Change

The top chart of Figure A3 shows the historical series—level and 6-month change—of the HY OAS through 2022. Both the level and change display a fair bit of variations across

time for the full sample. The black thin line plots the level of the HY OAS, and the red thick line shows the 6-month change. The standard deviations of both the level and change are similar in magnitude. Yet, as the bottom chart for the main FR Y-14Q sample—from 2011 through 2019, where the end of the period is set to allow at least three years after buyout close—shows, there are not too many ups and downs during those periods, particularly for the level of credit spreads.

In the bottom chart of Figure A3, both the level and 6-month change are standardized (subtracting the mean and dividing by the standard deviation), and the blue dashed line indicates one standard deviation—which is 1 for any standardized series. For a normal distribution, the probability of being outside one standard deviation from the mean is approximately 32%, adding 16% from each side of the distribution. Since the level of credit spreads is persistent, while the number of days where the level stays outside 1 or -1 is roughly 30% of the all days in the sample, those occasions tend to be clustered over adjacent periods. Qualitatively, the level of credit spreads captures two episodes of stress events in the credit market between 2011 and 2019: one in 2011, which is often attributed to the Euro area debt crisis and/or US sequestration, and the other in 2015-2016, which is thought of as related to stress in the oil and gas industry due to a substantial drop in oil prices.

By contrast, in the same chart, the 6-month change of the HY OAS tends to move outside one standard deviation more frequently but for a shorter duration compared with the level. The total number of days where the change goes outside 1 or -1 is approximately 29% of the entire days in the sample, similar to that of the level. Qualitatively, the short-term change of credit spreads picks up two additional stress episodes to the two captured by the level: one just before 2015-2016 spread widening and the other around the end of 2018. Therefore, overall, the 6-month change of credit spreads exhibits notably more variations over the main FR Y-14Q sample period than the level does—a desirable feature for the main regressor.

Next we examine statistical properties of the level and short-term change of the HY OAS.

A straightforward relation between the level and 6-month change of the HY OAS is:

$$s_t = s_{t-6} + (s_t - s_{t-6}) \equiv s_{t-6} + \Delta s_t \ , \tag{B.1}$$

where $s_t$ is the level of the HY OAS at time $t$, $s_{t-6}$ is the level at 6 months before $t$, and $\Delta s_t$ is the change of the HY OAS between 6 months before $t$ and $t$. This relation helps to understand some of the statistical properties shown in Table A2. To avoid possible issues with making statistical inferences out of overlapping periods, every non-overlapping 6-month period is taken between the end of 1996 and the end of 2022, giving us 53 (independent) observations in total.

As can be seen in the first column, the (6-month) serial correlation of the level of the HY OAS is positive—at roughly 0.59—and statistically significant, indicating that the level of the HY OAS is persistent. Denote this (6-month) serial correlation by $\rho$. If the level of the HY OAS follows an AR(1) process where the time interval between each period is 6 months, the 12-month serial correlation $Cor(s_{t-6}, s_{t+6})$ should be $\rho^2 \approx 0.35$. Yet, as shown in the second column, the 12-month serial correlation is approximately 0.29, which is somewhat lower. This result indicates that the level of the HY OAS follows a more complex process than the AR(1) process at the 6-month interval.

The serial correlations of the level of the HY OAS governs the correlation between the level and change of the HY OAS to a large extent. Negative correlations in the third and fourth column can be mostly inferred from the serial correlations of the level of the HY OAS (first and second column of Table A2). First note that the standard deviations of $s_t$ and $\Delta s_t$ are similar in magnitude, as displayed in the last two columns of Table A2—$Std(s_t)$ is roughly 1.09 times $Std(\Delta s_t)$. Thus, Equation B.1 tells us that the correlation between $s_{t-6}$—the level of the HY OAS at the beginning of the 6-month change $\Delta s_t$—and $\Delta s_t$ is

negative, and the magnitude is close to $(1-\rho)$:

$$Cov(s_{t-6}, s_t) = Var(s_{t-6}) + Cov(s_{t-6}, \Delta s_t) \tag{B.2}$$
$$\implies \quad Cor(s_{t-6}, \Delta s_t) = \frac{Cov(s_{t-6}, \Delta s_t)}{Std(s_{t-6})Std(\Delta s_t)} = \frac{Cov(s_{t-6}, \Delta s_t)}{Var(s_{t-6})} \frac{Std(s_t)}{Std(\Delta s_t)} = -(1-\rho)\frac{Std(s_t)}{Std(\Delta s_t)} ,$$

which is consistent with the third column as $-(1-0.589)*1.09 \approx -0.45$. The identity $Std(s_t) = Std(s_{t-6})$ is used in this derivation. The correlation between the current level of the HY OAS and its change from 6 months from now to 12 months is given by:

$$Cov(s_{t-6}, s_{t+6}) = Cov(s_{t-6}, s_t) + Cov(s_{t-6}, \Delta s_{t+6}) \tag{B.3}$$
$$\implies \quad Cor(s_{t-6}, \Delta s_{t+6}) = \frac{Cov(s_{t-6}, \Delta s_{t+6})}{Var(s_{t-6})} \frac{Std(s_t)}{Std(\Delta s_t)} = -(\rho - Cor(s_{t-6}, s_{t+6})) \frac{Std(s_t)}{Std(\Delta s_t)} .$$

Based on the numbers in Table A2, we obtain $-(0.589-0.289)*1.09 \approx -0.33$, which is close to the actual sample correlation displayed in the fourth column of the table. Those negative correlations in the third and fourth column of Table A2 indicate that the level of the HY OAS tends to revert to its mean in the next 6 months (third column), and even through the next 12 months (fourth column). Therefore, the level of the HY OAS has a predictive power on future changes of its level, at least over the next 12 months. As a result, effects of the level of the HY OAS are difficult to interpret because it is unclear if the effects come from the current level or from its future change.

As opposed to the level of the HY OAS being (negatively) correlated with its future change, the change of the HY OAS over the past 6 months is not correlated with the change over the next 6 months in a statistically significant way, as can be seen in the fifth column in Table A2. Similarly, the change of the HY OAS over the past 6 months is not correlated with the change from 6 months from now to 12 months, as displayed in the sixth column in the same table. Therefore, the short-term change of the HY OAS is not predictive of the

future change, which makes the interpretation of the effect of the change of the HY OAS relatively straightforward compared with that of the level of the HY OAS.

Lastly, but importantly, the correlation between the level and short-term change of the HY OAS mechanically generates a discrepancy for the estimated effect of the short-term change of the HY OAS when the level of the HY OAS is added as a control variable. To illustrate this point, consider a simple model:

$$y = \alpha + \beta \Delta s_t + \epsilon \ , \tag{B.4}$$

where $y$ is the dependent variable of interest, and the residual $\epsilon$ is idiosyncratic. For simplicity, all regressors are demeaned. Then $\beta$ can be computed from the population as follows:

$$\beta = \frac{Cov(y, \Delta s_t)}{Var(\Delta s_t)} \ . \tag{B.5}$$

Now adding the level $s_{t-6}$ as a control changes the estimate of $\beta$ as follows:

$$\tilde{\beta} = \frac{Cov(y, \Delta \tilde{s}_t)}{Var(\Delta \tilde{s}_t)} \ , \tag{B.6}$$

where $\Delta \tilde{s}_t$ is the component of $\Delta s_t$ that is orthogonal to $s_{t-6}$, which is given by

$$\Delta \tilde{s}_t = \Delta s_t - \frac{Cov(\Delta s_t, s_{t-6})}{Var(s_{t-6})} s_{t-6} = \Delta s_t + (1-\rho) s_{t-6} \ . \tag{B.7}$$

Then $\tilde{\beta}$, the regression coefficient on $\Delta s_t$ controlling for the level $s_{t-6}$, can be rewritten as

$$\begin{aligned} \tilde{\beta} &= \frac{Var(\Delta s_t)}{Var(\Delta \tilde{s}_t)} \frac{Cov(y, \Delta s_t)}{Var(\Delta s_t)} + (1-\rho) \frac{Var(s_{t-6})}{Var(\Delta \tilde{s}_t)} \frac{Cov(y, s_{t-6})}{Var(s_{t-6})} \\ &= \frac{Var(\Delta s_t)}{Var(\Delta \tilde{s}_t)} \beta + (1-\rho) \frac{Var(s_t)}{Var(\Delta \tilde{s}_t)} \beta_{lev} \ , \end{aligned} \tag{B.8}$$

where $\beta_{lev} = \frac{Cov(y, s_{t-6})}{Var(s_{t-6})}$ is a univariate regression coefficient of $y$ on $s_{t-6}$. Note that $Var(\Delta\tilde{s}_t)$ is smaller than $Var(\Delta s_t)$ because $\Delta\tilde{s}_t$ and $s_{t-6}$ are orthogonal. Based on in-sample estimates, this equation can be rewritten as

$$\tilde{\beta} \approx 1.1 \times \beta + 0.5 \times \beta_{lev} \ , \tag{B.9}$$

where $\tilde{\beta}$ is the regression coefficient on $\Delta s_t$ when $s_{t-6}$ is added as a control variable, $\beta$ is a univariate regression coefficient on $\Delta s_t$, and $\beta_{lev}$ is a univariate regression coefficient on $s_{t-6}$.

Therefore, quite mechanically, when $\beta_{lev}$ is not close to zero, adding the level $s_{t-6}$ of the HY OAS as a control variable may give a quite different estimate of $\beta$ (which is $\tilde{\beta}$) from the initial estimate of $\beta$ without it. Consequently, we do not attempt to control for the level of the HY OAS throughout this paper when using the (6-month) change as the main regressor.

## B.2 LBO Target Stock Price Sensitivity to Credit Spreads

Axelson et al. (2013) finds that credit market conditions measured by the level of the HY OAS at buyout are the predominant determinant of the leverage and pricing of the LBOs—LBO leverage and valuation are high when the HY OAS is low, and vice versa. Yet, equity market valuation, which is a crucial factor for buyout activity as documented in Haddad et al. (2017), often moves closely with credit market conditions, confounding the impact of credit market conditions on LBO outcomes through target valuation. Therefore, ideally, assessing the effect of credit market conditions on LBO outcomes needs to involve separating out the impact of target valuation on those outcomes.

In this subsection, we show that target stock prices around buyout close become (relatively) insensitive to credit spread changes. Target stock prices reflect the valuation of the target company from the perspective of outside investors. As a result, taking the short-term change of credit spreads leading up to buyout close as a measure of credit market conditions

provides a way to estimate the effect of credit market conditions on LBO outcomes while buyout target valuation being unaffected. Since target companies' stock prices only exist for public companies, this exercise is limited to public-to-private transactions.

We examine the sensitivity of daily target stock returns to daily HY OAS changes in Figure A4. The beta of daily stock returns on daily HY OAS changes is computed for each target company of LBO transactions, and the cross sectional statistics of the beta are shown. The top two charts are based on the full sample of daily stock returns for LBO targets, including pre-announcement returns. Since only part of buyouts have rumors/unofficial news preceding their announcement, these two charts include target stock price movements without any information about the buyout "known" to the investors. To exclude such price movements, in the bottom two charts, we only consider price movements after the announcement of the LBO deal.

The top left chart (left chart of Figure A4a) shows the beta—the median (dot) and the interquartile range (bar range)—over the period from 24 months before buyout close to 12 months before. The beta is negative for the majority of target companies, by definition of the interquartile range, and the median is approximately -4.2: an increase of the HY OAS by 1 p.p. is associated with a target stock return decrease by slightly more than 4 p.p.. Therefore, over the 12-month period where the buyout transaction occurring after (more than) a year is unlikely to affect target stock prices, those prices are quite sensitive to changes of credit spreads. We take this median beta of -4.2 as the benchmark sensitivity of daily target stock returns to daily HY OAS changes—reflecting how target stock prices would respond to credit spread changes absent the buyout.

In the top right chart (right chart of Figure A4a), we compute the same daily beta for the time window between $n$ months before close and close for $n = 1$ through $n = 12$. Note that to be included in the sample, a target company needs to have stock prices going back to $n$ months before close. For example, target companies with stock prices from 3 months

before close to close are excluded from the sample for $n = 4$ but are included in the sample for $n = 3$. Even not excluding price movements absent any buyout information, the median beta coefficient is relatively small in magnitude at about -0.7 for $n = 3$, -1.1 for $n = 4$, and -1.5 for $n = 5$. Therefore, compared to the median beta coefficient of -4.2 in the top left chart, within a sufficiently short time window leading up to LBO close, target stock prices are relatively insensitive to the change of credit spreads.

The bottom left chart (left chart of Figure A4b) shows the median and interquartile range of beta of daily target stock returns on daily changes of HY OAS for the entire periods after the announcement of buyout deals. The median estimate is about -0.5, and the interquartile range is substantially narrower than that of the top left chart. This narrow band suggests that the vast majority of target companies' valuations from outside investors' perspective do not respond much to credit spread changes once the LBO deal is announced.

The bottom right chart (right chart of Figure A4b) computes daily beta similarly as for the top right chart, but the sample is limited to daily stock returns after buyout announcements. Since only a small portion of target companies make announcements on their buyout deals prior to 6 months before buyout close, the number of LBO transactions included in the sample is less than 100 for $n > 6$. Yet, up to $n = 5$, the median coefficient is small in magnitude (about -0.5 or smaller in magnitude), and the interquartile range is also quite narrow, compared with those in the top left chart. Therefore, after buyout announcements, target stock prices become (relatively) insensitive to changes of credit spreads, at least from 5 months before the LBO close.

A caveat is warranted for interpreting the bottom two charts of Figure A4. Most LBO deals in CIQ have their announcement date on (or around) the definitive agreement date, implying that the insensitivity of target stock prices on credit spreads in those bottom charts is likely due to target valuation being largely finalized after the announcement. Yet, as shown in the top right chart, over a 4-month or shorter window leading up to buyout close,

target stock prices are still quite insensitive to credit spread changes even after including pre-announcement stock returns—these pre-announcement returns primarily involve target stock returns before its LBO valuation is finalized. Note that those pre-announcement returns also include price movements absent any information about the buyout deal (known to outside investors), and, as a result, betas in the top right chart presumably overstates the sensitivity of stock returns to credit spread changes. Therefore, target valuation is unlikely to respond to the (sufficiently) short-term change of credit spreads leading up to buyout close.

## B.3 LBO Target Selection and Credit Spreads

In this subsection, we argue that the determination process of LBO targets in our sample is largely unrelated to the short-term credit spread changes leading up to LBO close, based on institutional details of the buyout process. Consequently, the "target selection" process, which refers to the entire process through which the buyout target of a closed deal is determined out of the entire universe of potential targets, is uncorrelated with short-term credit spread changes leading up to the buyout. Some evidence for the claim is provided.

Buyout targets in our sample—those that survive through the completion of the LBO deals—are determined after going through lengthy and complex processes. Details on those processes are provided in Appendix Section A.1. Until the buyout deal closes, "target selection" occurs through two channels: the buyer sources and screens potential targets, or the deal is canceled/withdrawn by either the buyer or the seller. The initial sourcing/screening of potential targets by a PE firm mostly takes place at least a few months before buyout close, and, as a result, the (sufficiently) short-term change of credit spreads leading up to the close is very unlikely to have any causal effect on the screening process. Once the sourcing and screening process is over, the buyer initiates an official buyout process by meeting the seller. From this point on, "target selection" only occurs through terminating the deal by either party.

As described in Appendix Section A.1, before signing a definitive agreement, the (potential) buyer performs due diligence and decides on making an offer based on the diligence. While unsatisfactory diligence results can lead to a deal termination, such occasions are largely idiosyncratic and unlikely to be associated with credit market conditions. Also, since due diligence process is costly in terms of time and resource committed to those investigations, moderate changes of credit market conditions are unlikely to lead to a deal termination by the buyer. Once an offer is made, the seller can accept, decline, or continue negotiating the offer. Since the seller's objective—to close the deal at the highest price—does not directly involve credit market conditions, presumably the seller's deal termination decision is not much affected by credit market conditions (changes of). After terms and conditions of a merger/purchase agreement are agreed on, the definitive agreement is signed.

Once the deal is signed, a termination decision largely depends on conditions on a rightful termination specified in the contract. Outside of those conditions, which are not dependent on credit market conditions in most cases, a deal termination by either party incurs substantial costs as discussed in Appendix Section A.1. Therefore, after deal signing, it is highly unlikely that credit market conditions affect buyout deal termination decisions.

Overall, the timeline of LBO transactions implies that the "target selection" process is unlikely to be (or at most limitedly) impacted by the (sufficiently) short-term change of credit spreads leading up to buyout close. The initial sourcing/screening process ends (mostly) at least a few months before buyout close, and credit spread changes occurring after that process presumably do not affect the sourcing/screening decisions. For termination decisions after initial meetings, many factors that are unrelated to credit market conditions affect such decisions, and a deal termination is costly although associated costs depend on the stage of the deal.

This paper provides empirical evidence supporting that the short-term credit spread change leading up to LBO close is not correlated with the "target selection" process. To

properly test this hypothesis, we need to know how many buyout deals out of the entire universe of potential buyout targets are closed at each point of time.[40] While closed buyout deals are recorded in the CIQ database, potential buyout targets are difficult to identify without making substantial assumptions.

To proceed further, we assume that the number of closed LBO deals in the near past is proportional to the number of the outstanding potential buyout targets. More specifically, we define the (monthly) rate of LBO activities as the number of closed LBO deals in the month divided by the monthly (average) number of closed deals between 2 years and a year before the corresponding month. We avoid using the monthly average number for the past year as the denominator since that number is likely correlated with the current level of credit spreads—the level of credit spreads is quite persistent. If the rate is higher than 1, this month's number of closed buyout deals is larger than the monthly average between 2 years and a year ago, and vice versa. Under our assumption that the the number of recent closed deals is proportional to the number of potential targets, the rate of LBO activities is proportional to the rate of actual target selections.

In the left chart of Figure A5a, the regression coefficient of the rate of LBO activities on the $n$-month change of the HY OAS leading up to the corresponding month where the rate of LBO activities is measured. Overall, the regression coefficients are small in magnitude—close to zero—up to the 7-month change of credit spreads but start to decline thereafter. Still, the coefficient is statistically not significant even up to the 12-month change. The decreasing regression coefficients beyond the 7-month change are consistent with our rationale that credit market conditions are unlikely to matter after the sourcing/screening process but may affect the target selection process beforehand.

In contrast, the right chart of Figure A5b shows that the level of the HY OAS is negatively

[40] Even if the portion of closed buyout deals does not change, it is still possible that the composition of the LBO target companies changes. However, such compositional changes are hard to test with the available datasets as information on characteristics of the target companies are either very limited or largely missing.

related to the rate of LBO activities—a 1 p.p. higher level of the HY OAS is associated with about a 4% lower rate of LBO activities. The regression coefficient is economically large in magnitude and statistically significant, up to the level at 10 months before the corresponding month of LBO activities. Also, the regression coefficient does not change noticeably up to 7 months before the corresponding month, consistent with the main takeaway of the left chart.

Therefore, as argued based on the institutional details on LBOs, the (sufficiently) short-term change of credit spreads does not affect the target selection process, to the extent the rate of LBO activities is capturing the outcome of the process. To corroborate the rationale, we investigate if the short-term credit spread change affects buyout deal termination decisions. Unlike other part of the target selection process that is mostly not observed in data (e.g., sourcing and screening process), deal withdrawal/termination decisions are recorded in the S&P Capital IQ data. Furthermore, as opposed to the rate of LBO activities, where substantial assumptions need to be made to set the benchmark for monthly LBO activities, there is a fairly straightforward set of benchmark deals for withdrawn buyout deals: successfully closed deals within the same time frame.

Based on our rationale, LBO deal termination decisions should not be associated with the short-term change of credit spreads. The (monthly) deal termination rate, defined as the ratio of the number of terminated deals to the number of closed deals in the month, is computed to measure those LBO termination decisions. The left chart of Figure A5b shows the regression coefficient of the deal termination rate on the $n$-month change of the HY OAS leading up to the corresponding month of the deal termination rate. Overall, the regression coefficient is small in magnitude and statistically not significant even up to the 12-month change—the coefficient increases a bit as $n$ increases but by a small magnitude.

The right chart of Figure A5b shows the regression coefficient of the deal termination rate on the level of the HY OAS at $n$ months before the corresponding month of the deal termination rate. The level of the HY OAS is positively associated with the LBO termination

rate—a 1 p.p. higher level of the HY OAS is associated with about a 0.5% higher termination rate. Since the number of terminated LBO deals is roughly 3.5% of the number of closed LBO deals in the entire sample, the regression coefficient is economically large in magnitude. While the coefficient decreases somewhat as $n$ increases, those coefficients are all statistically significant.

Results in Figure A5 support that the target selection process is likely unrelated to the short-term change of credit spreads leading up to buyout close. On the contrary, the level of credit spreads, which is quite persistent over time, is correlated with the rate of LBO activities and the LBO termination rate. Therefore, our proposed measure of the short-term change of credit spreads is much less likely to be subject to target selection biases than the level of credit spreads is.

## B.4 Choice of Time Window for Spread Change

This paper chooses a 6-month window leading up to LBO close for the short-term change of credit spreads. In this subsection, we briefly justify the choice of the time window. The timeline of LBO transactions described in Appendix Section A.1 implies that there is a gap between the timing of target selection and/or valuation and that of buyout debt issuance, where the latter occurs near the buyout's close. Since LBO deals are heterogeneous in many dimensions and, consequently, in their timeline, it would presumably not be feasible to find a single time window that perfectly captures such a gap. Yet, for our purpose, choosing a time frame where target selection and valuation are not or very weakly correlated with the credit spread change within that window would be sufficient.

A particularly useful set of information for this purpose is deal signing dates. As discussed in Appendix Section A.1 and displayed in the Staples Inc. example in Figure 1, target valuation is largely finalized at deal signing. Also, a deal termination by any party becomes quite costly and difficult after signing. Yet, the signing date information is available only

for a portion of LBO transactions in data: out of ∼30,000 LBO transactions between 1997 and 2022, only ∼8,200 deals have the information.[41] Based on a subset of LBO deals that have signing date information, the quartiles of the day difference between signing and close are 33 days (25th percentile), 69 days (median), and 137 days (75th percentile). Therefore, a typical range of the time window between deal signing and close is 1 to 4 months. Within this window, target selection and valuation are very unlikely to be associated with credit spread changes.

As shown in the Staples Inc. buyout example, target selection and/or valuation often become insensitive to credit market conditions before deal signing. Consequently, to better capture the impact of credit spread changes, we may want to extend the time window beyond deal signing dates. Results in Appendix Section B.2 and B.3 suggest that a 4-month window would be a safe choice where target selection and valuation are largely uncorrelated with credit spread changes within that window. However, our choice of a time window for credit spread changes is 6 months, fairly longer than 4 months.

We take a longer time window of 6 months because this choice helps to improve the signal-to-noise ratio of regression coefficients on the credit spread change measure. The effect of credit spread changes between 6 months before LBO close and 4 months before is likely similar to that of spread changes between 4 months before close to close, although the magnitude of the effect may be smaller. Then, not including the additional two-month spread changes would increase the standard error of the estimated effect. To show this more formally, a simple model illuminating the point is presented below.

[41] Here we use data directly downloaded from S&P Capital IQ Pro website portal because 1) the portal has a bit better coverage compared with the CIQ data feed (30,000 v.s. 26,000 LBO transactions) and 2) the portal does not fill in missing deal signing dates with placeholder dates that are not actually the singing date as seen in the CIQ data feed (the close date is the most commonly used placeholder date). The latter is particularly useful as we do not need to make further assumptions on how CIQ treats missing signing dates.

Consider the following (demeaned) simplified model:

$$y = \sum_{n=1,2,...,\bar{n}} \beta_n \Delta s_{t-n,t-n+1} + \epsilon \ , \tag{B.10}$$

where $y$ is the dependent variable of interest, $t$ is the buyout close date, $\Delta s_{t-n,t-n+1}$ is credit spread changes between $n$ months before $t$ and $n-1$ months before $t$, $\bar{n}$ is the maximum month before buyout close where the spread change within the month affects the dependent variable, $\beta_n$ is the impact of each (monthly) spread change, and $\epsilon$ is an idiosyncratic error term. Spread changes are not correlated with each other—close to their statistical properties in data. For the sake of further simplicity, we assume $\bar{n} = 6$ and $\beta_n = \beta$ for $n = 1, ..., 4$ but $\beta_n = \beta - \delta$ for $n = 5, 6$.

In this setup, taking a 4-month window gives the right (mean) estimate of $\beta$ when running an OLS regression, but the (asymptotic) variance of the coefficient becomes

$$Var(\hat{\beta}_4) = \frac{1}{N} \frac{Var((\beta - \delta)\Delta s_{t-6,t-4}) + Var(\epsilon)}{Var(\Delta s_{t-4,t})} \ , \tag{B.11}$$

where $\hat{\beta}_4$ is the estimated regression coefficient on the 4-month change of credit spreads, and $N$ is the number of observations. On the other hand, taking a 6-month window gives the mean estimate of

$$E[\hat{\beta}_6] = \beta - \frac{1}{3}\delta \ , \tag{B.12}$$

where $\hat{\beta}_6$ is the estimated regression coefficient on the 6-month change of credit spreads. Its (asymptotic) variance now reads

$$Var(\hat{\beta}_6) = \frac{1}{N} \frac{\frac{1}{9}Var(\delta \Delta s_{t-4,t}) + \frac{4}{9}Var(\delta \Delta s_{t-6,t-4}) + Var(\epsilon)}{Var(\Delta s_{t-6,t})} \ . \tag{B.13}$$

As long as $\delta$ is sufficiently small ($\delta < (1+\sqrt{2/3})^{-1}\beta \approx 0.55\beta$), the numerator of $Var(\hat{\beta}_6)$ is smaller than the numerator of $Var(\hat{\beta}_4)$, and the denominator is notably larger: $Var(\Delta s_{t-6,t}) = 1.5Var(\Delta s_{t-4,t})$. Hence, in this setup, the standard error of $\hat{\beta}_6$ is smaller than $\sqrt{2/3} \approx 0.82$ of the standard error of $\hat{\beta}_4$ if $\delta$ is small enough.[42]

Therefore, there is a notable gain in terms of the statistical significance of regression coefficients, at a (relatively) small cost of the estimates being a bit smaller in magnitude, by taking a 6-month window instead of a 4-month window. Based on the authors' institutional knowledge, the 6-month change of credit spreads leading up to LBO close is still in the realm where target selection and valuation are only weakly correlated with the spread change. Yet, to validate our results further, we also provide estimates taking a 4-month window instead, at least for the main results.

# Appendix C Proof and Additional Results

## C.1 Proof of Unbiasedness

In this subsection, we provide a formal proof that the estimated regression coefficient of a dependent variable (pre-post buyout change) on the interaction term between the LBO dummy and the short-term change of credit spreads is unbiased if certain conditions are met: if selection biases—defined by deviations of non-LBO control firms from the true non-LBO counterfactual firms in this context—are uncorrelated with the short-term change of credit spreads. The purpose of this proof is to show that the empirical estimate of $\beta_3$ in Equation 2 is unbiased under those conditions. In this proof, we consider a simplified setup that excludes control variables and fixed effects, but it is straightforward to extend the proof to the setup in Equation 2.

[42] A regression coefficient that is at the 5% significance level becomes statistically not significant at the 10% level if the standard error becomes $\sqrt{3/2}$ larger. Thus, this (minimum) difference in standard errors can be substantial.

The simplified setup of Equation 2 implicitly assumes the following model:

$$\Delta Y_i = \alpha + \beta_1 D_i + \beta_2 \Delta s_t + \beta_3 D_i \Delta s_t + \epsilon_i \ , \tag{C.1}$$

where $D_i$ is a simplified notation for the LBO dummy, and $\Delta s_t$ is the (demeaned) 6-month credit spread change leading up to LBO close at time $t$. This model is assumed to hold in the population and the sample, but $\epsilon_i$ is likely to be correlated with (some of) the regressors as companies in the treated group (LBO targets) are deliberately selected through a target selection process. Thus, the full sample OLS estimate of $\beta_1$, which captures the effect of LBOs, is very likely biased:

$$E_s[\hat{\beta}_{1,s}] = \frac{Cov_s(\tilde{D}_i, \Delta Y_i)}{Var_s(\tilde{D}_i)} = \beta_1 + \frac{Cov_s(\tilde{D}_i, \epsilon_i)}{Var_s(\tilde{D}_i)} \ , \tag{C.2}$$

where the subscript $s$ indicates the (full) sample estimate, and $\tilde{D}_i$ is the component of $D_i$ that is orthogonal to other regressors.

For the purpose of a better identification, we match each company in the treated group (LBO targets) to control firms, which are taken as the control group. By assigning (non-treated) control firms independently for each transaction, $D_i$ is uncorrelated with $\Delta s_t$. For example, by assigning one control firm to each treated company, regardless of $\Delta s_t$, there always exists a pair of a treated and control company for each transaction. Yet, to the extent the control firms deviate from the true non-LBO counterfactual firms, there may still exist selection biases.

To show this formally, the outcome of a treated firm can be written as

$$\Delta Y_{1i} \equiv \Delta Y_i | (D_i = 1) = \alpha + \beta_1 + \beta_2 \Delta s_t + \beta_3 \Delta s_t + \epsilon_i \ , \tag{C.3}$$

and the outcome of its true (non-treated) counterfactual firm is

$$\Delta Y_{0i} \equiv \Delta Y_i | (D_i = 0) = \alpha + \beta_2 \Delta s_t + \epsilon_i \ . \tag{C.4}$$

Hence, the average difference between the outcome of treated firms and that of the true counterfactual firms is $\beta_1$, which is what a difference-in-difference approach ideally aims to achieve:

$$E_m^{ideal}[\Delta Y_{1i} - \Delta Y_{0i}] = \beta_1 + \beta_3 E_m^{ideal}[\Delta s_t] = \beta_1 \ , \tag{C.5}$$

where the subscript $m$ indicates the matched sample estimate, the superscript *ideal* indicates a hypothetical ideal matching where the matched control firms are the true non-LBO counterfactual firms, and $E_m^{ideal}[\Delta s_t] = 0$ as $\Delta s_t$ is demeaned. The average outcome difference captures the average LBO effect on the treated and is identical to the (mean) OLS estimate of $\beta_1$ for the ideally matched sample:

$$E_m^{ideal}[\hat{\beta}_{1,m}^{ideal}] = \frac{Cov_m^{ideal}(D_i, \Delta Y_i)}{Var_m^{ideal}(D_i)} = \beta_1 = E_m^{ideal}[\Delta Y_{1i} - \Delta Y_{0i}] \ , \tag{C.6}$$

where the derivation comes from $Cov_m^{ideal}(D_i, \Delta s_t) = Cov_m^{ideal}(D_i, D_i \Delta s_t) = 0$ due to the independence of the matching process.

However, the actual control firms are quite likely to deviate from the true counterfactual firms. Then the outcome of control firms can be written as

$$\Delta Y_{0i} = \alpha + \beta_2 \Delta s_t + \epsilon_i + \eta_i \ , \tag{C.7}$$

where the last term $\eta_i$ represents the deviation and is referred to as selection biases. If the

selection biases exist, the outcome $\Delta Y_i$ in the matched sample can be rewritten as:

$$\Delta Y_i = \alpha + \beta_1 D_i + \beta_2 \Delta s_t + \beta_3 D_i \Delta s_t + \epsilon_i + (1 - D_i)\eta_i \ . \tag{C.8}$$

To the extent the (matched) sample mean of $\eta_i$ is different from zero, the estimate of $\beta_1$ is biased:

$$E_m[\hat{\beta}_{1,m}] = \frac{Cov_m(D_i, \Delta Y_i)}{Var_m(D_i)} = \beta_1 - E_m[\eta_i] = E_m[\Delta Y_{1i} - \Delta Y_{0i}] \ , \tag{C.9}$$

where the second equality is derived from

$$\begin{aligned} \frac{Cov_m(D_i, \Delta Y_i)}{Var_m(D_i)} &= \beta_1 + \frac{Cov_m(D_i, (1 - D_i)\eta_i)}{Var_m(D_i)} = \beta_1 - \frac{E_m[D_i]E_m[(1 - D_i)\eta_i]}{E_m[D_i](1 - E_m[D_i])} \\ &= \beta_1 - \frac{E_m[1 - D_i]E_m[\eta_i]}{1 - E_m[D_i]} = \beta_1 - E_m[\eta_i] \ , \end{aligned} \tag{C.10}$$

where the first equality in the second line comes from the independence of the matching process. For instance, in the previously mentioned example where one control firm is assigned to each treated company, $E_m[(1 - D_i)\eta_i] = \frac{1}{2}E_m[\eta_i]$.

Now we show that if the selection biases are uncorrelated with the spread change $\Delta s_t$, the estimate of $\beta_3$ using the matched sample is unbiased. First note that the component of $D_i \Delta s_t$ that is orthogonal to $D_i$ and $\Delta s_t$ is $(D_i - E_m[D_i])\Delta s_t$:

$$Cov_m((D_i - E_m[D_i])\Delta s_t, D_i) = 0 \quad , \quad Cov_m((D_i - E_m[D_i])\Delta s_t, \Delta s_t) = 0 \ , \tag{C.11}$$

where both equalities come from the independence of the matching process. Also note that

the mean of $(D_i - E_m[D_i])\Delta s_t$ is zero. Then, the estimate of $\beta_3$ in the matched sample reads

$$\begin{aligned} E_m[\hat{\beta}_{3,m}] &= \frac{Cov_m((D_i - E_m[D_i])\Delta s_t, \Delta Y_i)}{Var_m((D_i - E_m[D_i])\Delta s_t)} \\ &= \beta_3 + \frac{Cov_m((D_i - E_m[D_i])\Delta s_t, (1 - D_i)\eta_i)}{Var_m((D_i - E_m[D_i])\Delta s_t)} = \beta_3 \ , \end{aligned} \tag{C.12}$$

where the last equality is derived from

$$Cov_m((D_i - E_m[D_i])\Delta s_t, (1 - D_i)\eta_i) = E_m[(D_i - E_m[D_i])(1 - D_i)]E_m[\Delta s_t \eta_i] = 0 \ , \tag{C.13}$$

where the separated expectations come from the independence of the matching process. By assumption

$$E_m[\Delta s_t \eta_i] = Cor_m(\Delta s_t, \eta_i) = 0 \ , \tag{C.14}$$

since $\Delta s_t$ is demeaned. Therefore, as long as the selection biases—deviations of control firms from the true non-LBO counterfactual firms—represented by $\eta_i$ is not correlated with the short-term change of credit spreads, the estimate of $\beta_3$ in the matched sample is unbiased.

## C.2 Difference-in-Difference Approach-related Exercises

### Alternative Difference-in-Difference Estimates for Post-LBO Target Performance

Using the same sample as for Table 6b, we take a regression specification that is more typical for difference-in-difference estimations:

$$\begin{aligned} Y_{i,k,\tau} = \alpha &+ \beta_1(\text{LBO})_{i,k} + \beta_2(\text{Post})_\tau + \beta_3(\text{HY OAS Change})_k + \beta_4(\text{LBO})_{i,k} \times \\ &(\text{Post})_\tau + \beta_5(\text{LBO})_{i,k} \times (\text{HY OAS Change})_k + \beta_6(\text{Post})_\tau \times (\text{HY OAS Change})_k + \\ &\beta_7(\text{LBO})_{i,k} \times (\text{Post})_\tau \times (\text{HY OAS Change})_k + \zeta_j + \eta_t + \epsilon_{i,k,\tau} \ , \end{aligned} \tag{C.15}$$

where $k$ is an index for each LBO transaction, $i$ is a firm index for a group of firms, including both the treatment firm (LBO target) and control firms (non-LBO matches), corresponding to the LBO transaction, and $\tau \in \{Pre, Post\}$ is an index if the observation is before or after the corresponding buyout. $Y_{i,k,\tau}$ is the level of the financial variable of interest, and $(\text{Post})_\tau$ is a dummy variable that takes the value of 1 if $\tau = Post$ and 0 otherwise. Other notations mostly stay the same as those in Equation 2. Note that we include industry- and time-fixed effects but exclude control variables $X_{i,k,pre}$ as those variables are part of the dependent variables in this specification. Our coefficient of interest is $\beta_7$, which measures the extent to which the difference of the dependent variable between the treatment group (LBO targets) and the control group (non-LBO matches) after LBOs, compared to their pre-LBO difference, depends on pre-buyout credit market conditions.

In Table A7, we show that post-buyout performance variables are all positively related to pre-LBO credit spread changes—better post-buyout performance for LBOs done during deteriorating credit market conditions. As expected, these results are largely the same as those in Table 6b, since the regression specification in Equation 2 is a version of a difference-in-difference estimation as specified in Equation C.15.

**Pre-trends for LBO Effect**

To capture pre-treatment trends for the LBO effect—the treatment-control group difference—we run the following regressions:

$$Y_{i,k,\tau} = \alpha + \beta_1(\text{LBO})_{i,k} + \beta_2(\text{Non-reference})_\tau + \beta_3(\text{LBO})_{i,k} \times (\text{Non-reference})_\tau + \zeta_j + \eta_t + \epsilon_{i,k,\tau} \,, \tag{C.16}$$

where $\tau \in \{\text{Reference}, \text{Non-reference}\}$ is an index if the observation is in the reference year (a year before LBO) or not, and other indexes and variables are largely the same as those

in Equation C.15. Note that we take only two years of observations for each regression: a year before LBO as the reference year and the year of interest. $(\text{Non-reference})_\tau$ is a dummy variable that takes the value of 1 if $\tau = (\text{Non-reference})$ and 0 otherwise. Our coefficient of interest is $\beta_3$, which measures the difference between LBO targets and non-LBO control firms in the year of interest compared to the reference year. Results of this regression specification for operating performance variables are displayed in Table A8.

Table A8 shows that there are considerable pre-treatment (pre-buyout) trends for performance variables; pre-buyout performance differences between LBO targets and the control firms exist, relative to those in a year before LBO. Except for net income to book equity (total assets minus total liabilities) and operating income to book equity, the pre-buyout treatment-control group difference is broadly statistically significant, although the magnitude is overall smaller than the post-buyout treatment-control group difference. Out of the 45 regression coefficients estimated for the pre-buyout treatment-control group difference, we find 23 coefficients statistically significant. Compared with 39 coefficients being statistically significant for the post-buyout treatment-control group difference, which is supposed to capture the effect of LBOs on target performance under the ideal matching assumption, pre-buyout trends are not as stark but exist in a statistically meaningful way.

## C.3 Robustness Checks and Alternative Explanations

In this subsection, we perform robustness checks on the relation between pre-LBO credit market conditions and LBO leverage as well as post-LBO target performance. Some alternative explanations for the main results are explored as well.

### Pre-LBO Credit Market Conditions and LBO Leverage

In Table A3a, we consider credit spreads for a broader corporate bond index, the ICE BofA US Corporate Index Option-Adjusted Spread (Corporate OAS), instead of the HY

OAS. Regression results are qualitatively similar with those in Table 2, except that the magnitude of the coefficients for the Corporate OAS is roughly three times larger because the HY OAS is three times more volatile.[43] While the HY OAS is more relevant to LBOs as typical debt used for LBOs is below investment grade, the results are largely similar for the broader corporate bond index. Since the debt composition used for LBOs in the past two decades was tilted towards leveraged loans rather than high yield bonds, we also take loan spreads, spread-to-maturity (STM) for all loans published by PitchBook LCD, as the regressor instead of the HY OAS.[44] Table A3b shows that only the 6-month change of the loan STM has a statistically significant negative relation to LBO leverage. As the regression coefficient of LBO leverage on the level of the loan STM at close is statistically not significant, these results are somewhat stronger than our baseline results in Table 2.

Next we consider interest rates, 2-year and 10-year Treasury yields, as additional regressors. Since credit cycles tend to comove with interest rate cycles and monetary policy, the relation between changes of credit spreads and LBO leverage might be confounded by interest rate movements. Yet, regression results in Table A4a show that both the level or change of interest rates before LBOs are not related to LBO leverage.

The relation between credit spreads and LBO leverage might be still driven by the equity risk premium rather than credit spreads since credit spreads and the equity discount rate are correlated. Similar to the setting in Table A4a, price-to-dividend (PD) ratio of the S&P 500 portfolio is added as a regressor in Table A4b. The 6-month change of the PD ratio before LBOs has a positive relation to LBO leverage when credit spreads are not added to the regression, as can be seen in the fifth column of Table A4b. Yet, when both credit

[43]The daily correlation between the Corporate OAS and HY OAS is quite high at near 93%.

[44]While it might make more sense to use loan spreads given the argument, the loan STM series published by LCD is monthly at the beginning of the series and becomes daily only after the GFC. As a result, at least for some of our analysis the HY OAS series, which is daily, is a better alternative. Another reason for favoring the HY OAS is because loans are less liquid instruments than bonds, and loan prices may be more stale. Nonetheless, the monthly correlation between the HY OAS and the loan STM is high at 84%.

spreads and the PD ratio are taken as regressors, as can be seen in the last column, only the 6-month change of the HY OAS has a statistically significant relation with LBO leverage.

**Pre-LBO Credit Market Conditions and Post-LBO Target Performance**

First, we consider credit spreads for a broader set of corporate bonds than high-yield bonds by taking the Corporate OAS as the regressor instead. Table A13a shows the result: regression coefficients of performance variables are qualitatively similar to those in the baseline result in Table 6, with roughly 3 times the magnitude due to the 1/3 times volatility of the Corporate OAS compared to the HY OAS. Statistical significance is overall a bit weaker, and the coefficient for EBITDA to net sales does not exhibit statistical significance. These qualitatively similar but weaker results are to some extent expected since below-investment grade debt is typically used for LBOs given high leverage, and, thus, the HY OAS is a more relevant measure of credit market conditions for buyouts. Since leveraged loans were the primary source of LBO financing in the past decade, we take the spread-to-maturity (STM) for all loans as the regressor: the result is displayed in Table A13b and also qualitatively similar to the baseline result in Table 6 with sightly weaker statistical significance.

Next we include 2-year and 10-year Treasury yield changes as regressors to test if interest rate movements instead of credit spread changes drive our main result. As can be seen in Table A14a, Treasury yield changes seem to have limited impacts on target performance variables while the credit spread change exhibits similar effects as for the baseline result in Table 6. Then we add changes of the price-to-dividend (PD) ratio of the S&P 500 portfolio as a regressor to the baseline setup. Table A14b also shows that pre-buyout PD ratio changes are limitedly associated with post-buyout target performance, but pre-buyout credit spread changes are firmly related to post-buyout performance, similarly as in Table 6. Note that we omit to report regression coefficients on several regressors in Table A14, which are specified in each subtable.

## C.4 Pre-LBO Credit Market Conditions and Target Strategy

We examine if the post-buyout strategy of target companies is associated with (changes of) pre-buyout credit market conditions. While data is fairly limited on actions taken by the target companies after buyout close, there are a few dimensions that we can investigate. We consider 1) how much time is taken between a buyout close and the sale of the target company and 2) how many firms (or entities) a target company acquired between the buyout close and 3 years after the buyout. For the latter, 3 years are chosen as the measurement window because that is the main estimation window for our baseline result in Table 6, and the choice helps us to avoid counting acquisition transactions that occur after the buyout exit, which typically occur between 3-7 years from the buyout.

Table A20a shows that the 6-month credit spread change indeed affects time taken until the sale of target companies—pre-buyout credit spread widening increases the amount of time it takes for a target company to be sold. Note that the time computation is based on if the sale of the target is reported in Capital IQ and, as a result, can be overestimated if the actual exit sale is not recorded.[45] Also, for a non-negligible portion (roughly 5%) of the sample, the time taken between the buyout and the target sale is less than a year, which is likely data errors. To address those issues, the top and bottom 5% of the sample based on the amount of time between the LBO and the target sale are trimmed. Based on the trimmed data, the lower, median, and upper quartile are 3.18 years, 4.85 years, and 7.16 years, respectively.[46] Based on the regression result, one p.p. increase in the HY OAS before an LBO close leads to 0.05 years (0.6 months) longer time taken for the target company's sale, which is roughly 1.3% of the interquartile range of time taken for target sales.

While the regression coefficients on the 6-month spread change in Table A20a are all

[45] This methodology naturally excludes buyout exits through initial public offerings (IPOs). Since the share of LBO exits through IPOs was small in the past two decades, this is not too much of a concern.

[46] Those quartiles are not very different for untrimmed data: 3.03 years, 4.89 years, and 7.64 years. Also, running the same regressions in Table A20a for untrimmed data gives largely similar results (not reported).

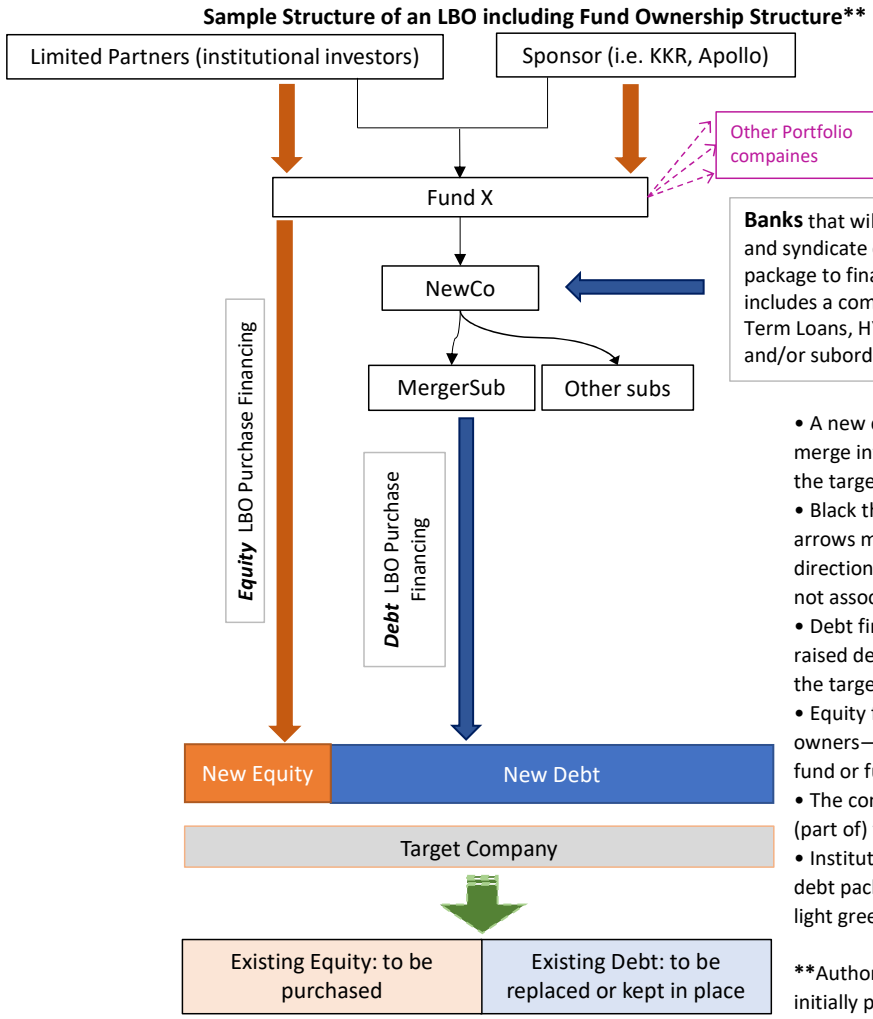
statistically significant, the magnitude of the effect may not be very large: for an HY OAS increase of 2.36 p.p., which is the historical standard deviation of the 6-month change of the HY OAS, the target company takes 0.13 longer years (1.5 longer months) to be sold (roughly 3% of the interquartile range). It is probably unlikely that a one- or two-month difference in exit timing reflects a meaningful difference in target strategy. With this caveat in mind, longer time to sell for targets of buyouts done during spread widening is consistent with a narrative that targets of LBOs closed during deteriorating credit market conditions tend to focus more on improving operating performance—such improvements are likely to take more time and effort and to take longer time to materialize.

Next we investigate if targets of LBOs done during credit spread widening are involved more in acquiring other companies (and/or subsidiaries) after buyouts, but until 3 years after the buyout, compared with those of LBOs done during credit spread tightening. The vast majority (roughly 80%) of the LBO transactions in the sample do not have a record of subsequent acquisitions of other entities (within the 3 year window), presumably reflecting limited coverage of such acquisitions due to the opaque nature of this market. Thus, we limit our sample to target companies of LBO transactions that acquire at least one entity within 3 years from the buyout close. Still, the number of acquisitions by LBO target companies is highly skewed: for the majority of LBO targets, the number is 1.

As Table A20b shows, while the pre-LBO level of credit spreads is negatively related to the number of subsequent acquisitions by the target, the 6-month spread change leading up to buyout close is not associated with the number of acquisitions. Therefore, the short-term credit spread change leading up to LBO close is not related to the number of the target's subsequent acquisitions. While many buyout targets take a so-called "buy-and-build" strategy, where the buyout target serves as a platform company by acquiring other companies/entities, these results partly assure that the 6-month spread change is likely not associated with the target's employing such a strategy.

In summary, in a few observable dimensions based upon data availability, the post-buyout strategy of LBO targets does not seem to materially differ between buyouts done during credit spread widening and those closed during credit spread tightening. Therefore, the dependence of post-buyout target performance on pre-buyout (changes of) credit market conditions is likely driven by a mechanism that works through not readily observable dimensions.

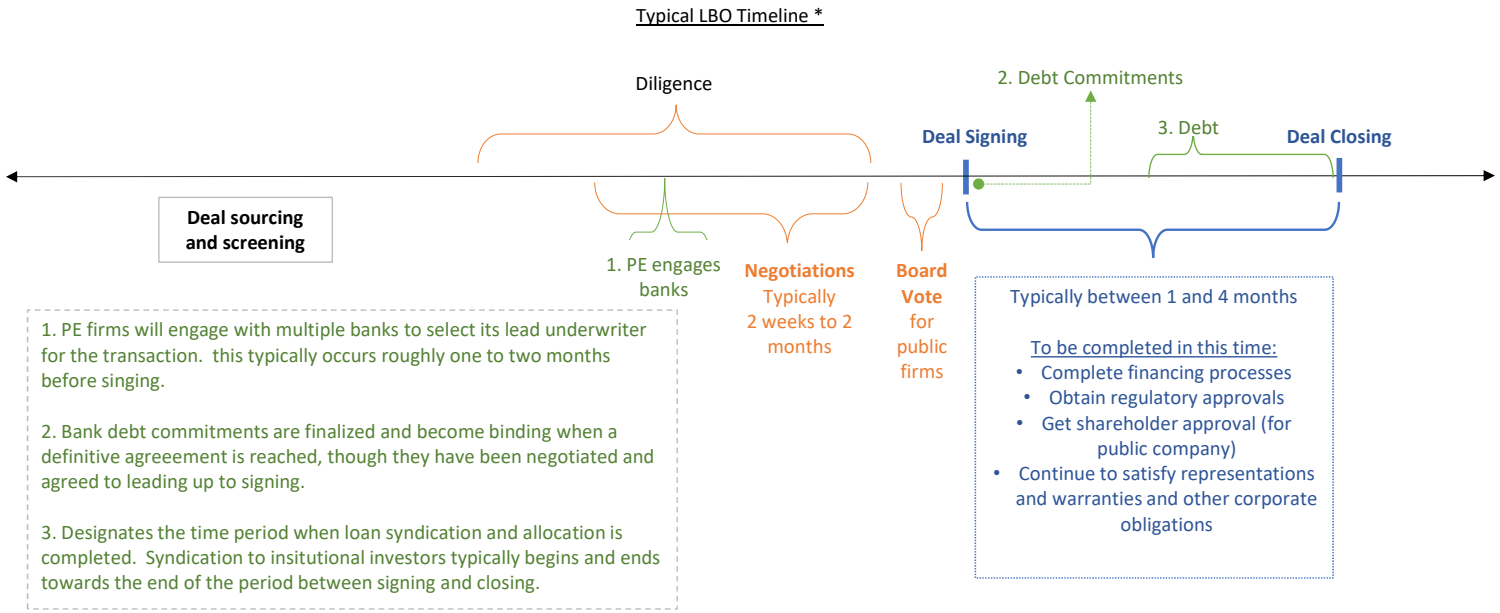
Figure A1: Illustration of Simplified Structure of an LBO




- A new company (NewCo) and its subsidiaries are created to merge into or with the target. The surviving entity is commonly the target.
- Black thin arrows indicate the ownership structure (direction of arrows means subordination); Thick colored arrows indicate the direction of cash flows; Dashed arrows indicate ownership that is not associated with this LBO transaction.
- Debt financing is issued by the NewCo entities. The newly raised debt (blue arrows) is used as financing for the purchase of the target.
- Equity financing (orange arrows) is provided from the owners—limited partners and sponsoring GP—of the acquiring fund or funds.
- The combined debt and equity financing is used to purchase (part of) the existing debt and equity (dark green arrow).
- Institutional investors are often the ultimate holders of LBO debt packages but do not directly provide LBO funding (double light green arrows).

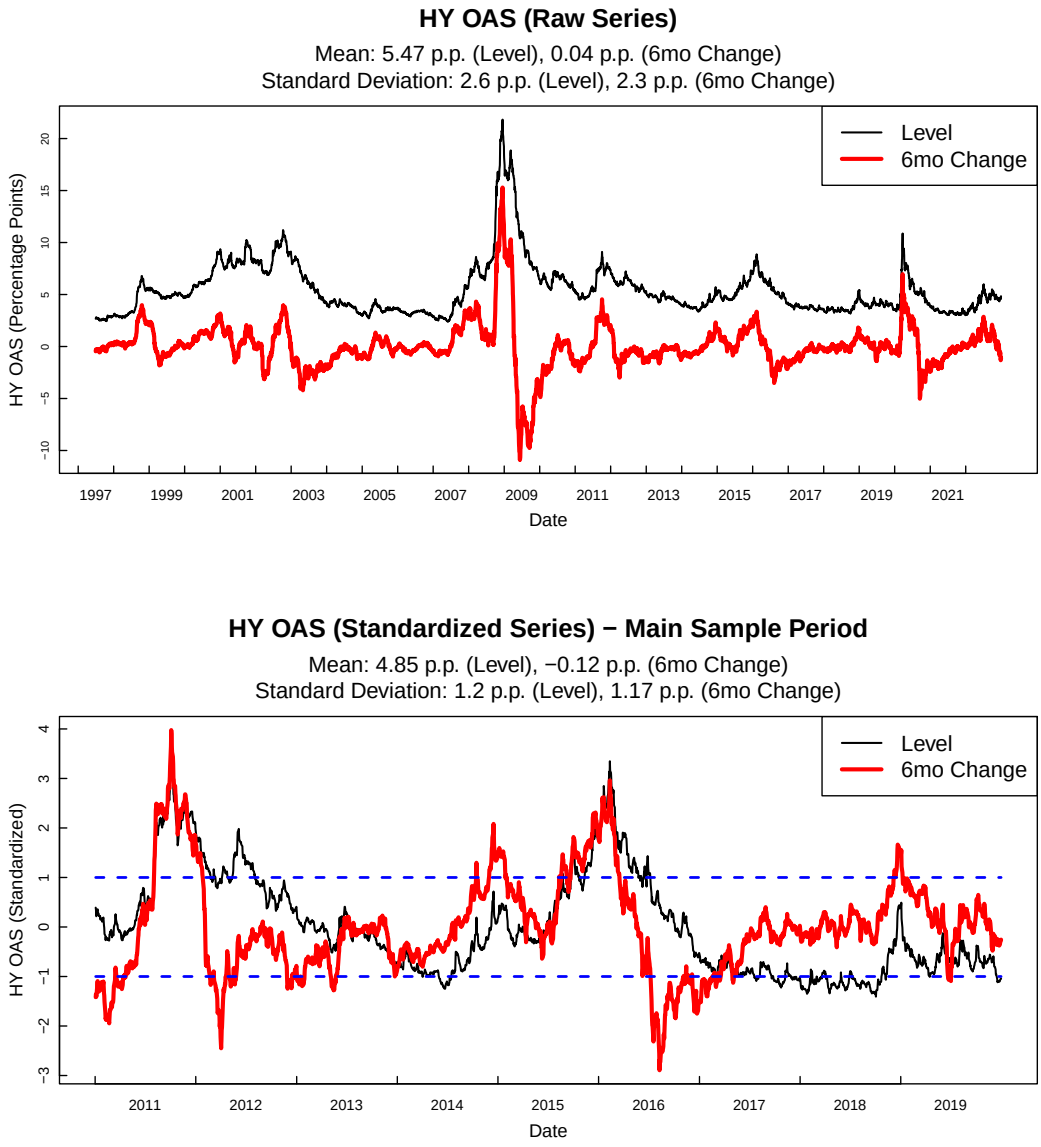
**Authors' depictions; also see: Levin and Rocap (2014 Edition, initially published in 1994).

Figure A2: Illustration of the Timeline of Typical LBOs




*Authors' depictions.

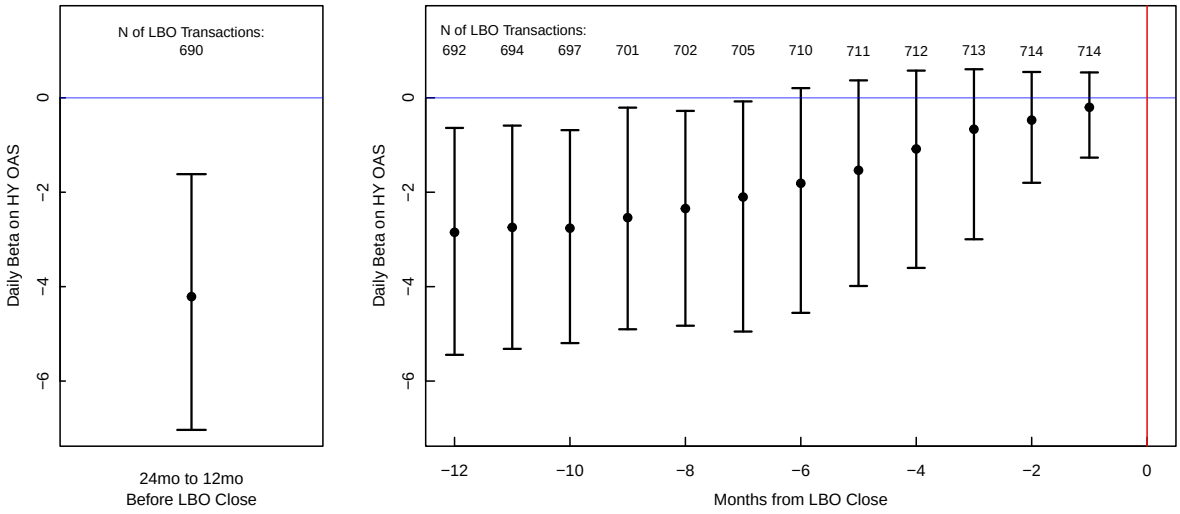
Figure A3: Level and Change of the HY OAS



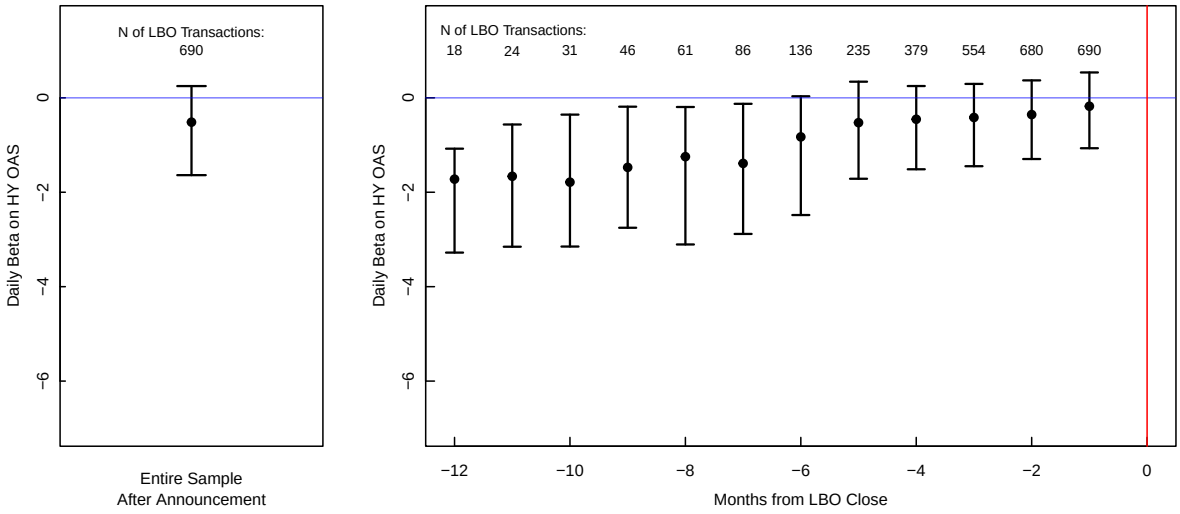

Note: For the top chart, the sample period is from the middle of 1997 through the end of 2022. For the bottom chart, the sample period is from 2011 through 2019. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread. The mean and standard deviation of each raw series (before being standardized) are separately displayed at the top of each chart.

Figure A4: Public-to-Private LBO Target Stock Return Beta on the Daily Change of the HY OAS

(a) Full Sample: Including Pre-Announcement Returns



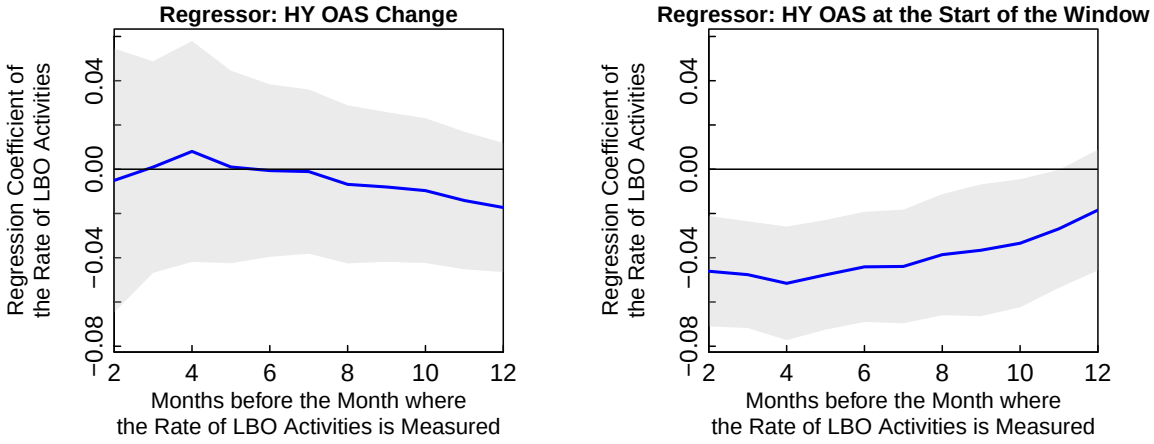

(b) Post-Announcement Returns Only



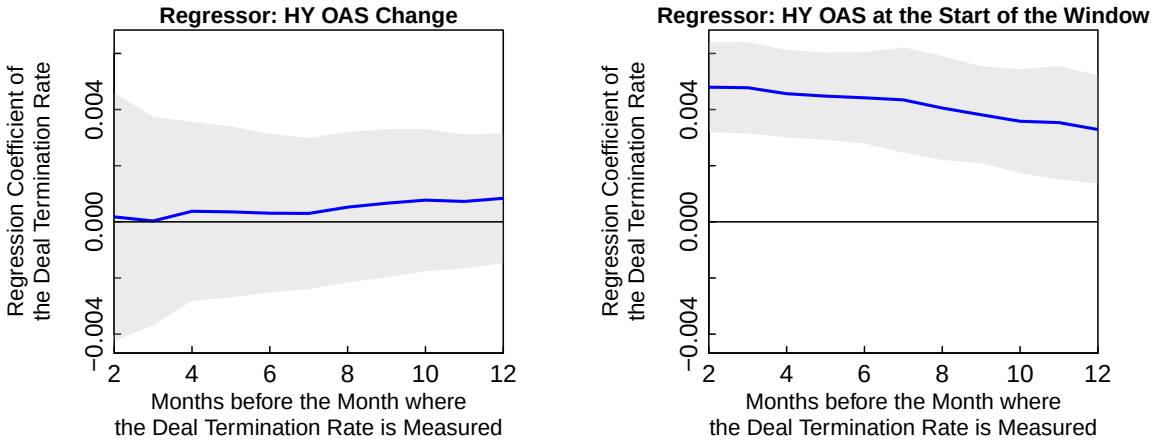

Note: Sample period is from 1997 through 2022 and includes public-to-private LBO transactions that are matched to CRSP. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread. For each target companies, beta of its daily stock returns on daily changes of the HY OAS is computed. The cross sectional median is plotted as a circle dot, and the interquartile range is displayed as a bar range. For the top right and bottom right chart, beta is computed over a time window between $n$ months from buyout close, where $n$ is displayed on the x-axis, and buyout close.

Figure A5: Effect of Credit Spreads on the Monthly Rate of LBO Activities and LBO Terminations

(a) Monthly Rate of LBO Activities




(b) LBO Termination Rate




Note: Sample period is from 1997 through 2022 with 312 observations. The monthly rate of LBO activities is defined by the number of LBOs in the corresponding month divided by the monthly average number of LBOs between 2 years (24 months) before the (start of the) corresponding month and a year (12 months) before. The top charts display regression coefficients for a univariate regression of (Monthly Rate of LBO Activities) $= \beta$(HY OAS Change) $+ \epsilon$ on the left column and those of (Monthly Rate of LBO Activities) $= \beta$(HY OAS at Start) $+ \epsilon$ on the right column. The monthly deal termination rate is defined by the ratio of the number of terminated deals to the number of closed deals in the same month. The bottom charts display regression coefficients for a univariate regression of (Monthly Deal Termination Rate) $= \beta$(HY OAS Change) $+ \epsilon$ on the left column and those of (Monthly Deal Termination Rate) $= \beta$(HY OAS at Start) $+ \epsilon$ on the right column. (HY OAS Change) refers to the change of the ICE BofA US High Yield Index Option-Adjusted Spread from $n$ months before the start of the corresponding month—month where the monthly rate is measured—through right before the start of the corresponding month, where $n$ is indicated in the $x$-axis of each chart. Similarly, (HY OAS at Start) refers to the level of the ICE BofA US High Yield Index Option-Adjusted Spread at $n$ months before the start of the corresponding month. The gray area represents a 95% confidence interval. For computation of the standard error of the regression coefficients, a Newey-West HAC covariance matrix (with Bartlett Kernel) is used, with the optimal bandwidth chosen following Newey and West (1994).

Table A1: Interquartile Range and Correlation of the 3-year Change of Performance Variables

(a) Interquartile Range

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| IQR | 0.049 | 0.086 | 0.224 | 0.051 | 0.088 | 0.244 | 0.05 | 0.089 | 0.256 |
| N Obs | 818623 | 818623 | 749188 | 729085 | 729085 | 666031 | 791882 | 791882 | 724079 |

(b) Correlation

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| Net Income / Sale | 1 | | | | | | | | |
| Net Income / Assets | 0.63 | 1 | | | | | | | |
| Net Income / Book Equity | 0.46 | 0.65 | 1 | | | | | | |
| Operating Income / Sale | 0.66 | 0.53 | 0.38 | 1 | | | | | |
| Operating Income / Assets | 0.45 | 0.83 | 0.54 | 0.66 | 1 | | | | |
| Operating Income / Book Equity | 0.26 | 0.46 | 0.75 | 0.42 | 0.6 | 1 | | | |
| EBITDA / Sale | 0.53 | 0.48 | 0.33 | 0.86 | 0.61 | 0.38 | 1 | | |
| EBITDA / Assets | 0.4 | 0.79 | 0.5 | 0.6 | 0.96 | 0.57 | 0.63 | 1 | |
| EBITDA / Book Equity | 0.16 | 0.33 | 0.61 | 0.31 | 0.47 | 0.89 | 0.34 | 0.51 | 1 |

Note: Sample period is from 2011 through 2022. The untrimmed full sample is used. In denominators, "Assets" indicate total (book) assets, "Sale" net sales, and "Book Equity" total assets minus total liabilities. For book equity, zero or negative values are discarded. In the bottom table, for the computation of correlations, the top and bottom 1% of each variable are trimmed, resulting in 606,893 observations.

Table A2: Correlations of HY OAS ($s_t$)

| | $Cor(s_{t-6}, s_t)$ | $Cor(s_{t-6}, s_{t+6})$ | $Cor(s_{t-6}, \Delta s_t)$ | $Cor(s_{t-6}, \Delta s_{t+6})$ | $Cor(\Delta s_{t-6}, \Delta s_t)$ | $Cor(\Delta s_{t-6}, \Delta s_{t+6})$ | $Std(s_t)$ | $Std(\Delta s_t)$ |
|---|---|---|---|---|---|---|---|---|
| | 0.589 *** | 0.289 ** | -0.459 *** | -0.336 ** | -0.142 | -0.146 | 2.58 | 2.36 |
| N | 52 | 51 | 52 | 51 | 51 | 50 | 53 | 52 |

Note: Sample period is from 1997 through 2022. HY OAS denoted by $s_t$ refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. $s_t$ is the level of HY OAS at month $t$, and $s_{t-6}$ is the level at 6 months before $t$. $\Delta s_t$ is the change of HY OAS from 6 months before $t$ to $t$. To eliminate overlaps between each 6-month period, we only take values at every six months (53 observations for $s_t$). Statistical significance of each correlation is computed based on a $t$-distribution with $N-2$ degrees of freedom, where $N$ is the number of observations.

Table A3: Regression of LBO Transaction Leverage on the Level and 6-month Change of Alternative Spreads

(a) Corporate Index Option-Adjusted Spread

| Dependent Variable: $\log\left(\frac{D}{EV}\right)$ | | | |
|---|---|---|---|
| Corp OAS at close | -0.104 ** | | |
| | (0.043) | | |
| Corp OAS at start | | -0.035 | |
| | | (0.036) | |
| Corp OAS change from 6mo before close | | | -0.127 *** |
| | | | (0.045) |
| Fixed effects | Industry | | |
| Clustering | Industry, Yr-Qtr | | |
| N | 882 | 882 | 882 |
| Adj-$R^2$ | 0.038 | 0.026 | 0.037 |

(b) Loan Spread-to-Maturity

| Dependent Variable: $\log\left(\frac{D}{EV}\right)$ | | | |
|---|---|---|---|
| Loan STM at close | -0.026 | | |
| | (0.018) | | |
| Loan STM at start | | 0.01 | |
| | | (0.015) | |
| Loan STM change from 6mo before close | | | -0.063 *** |
| | | | (0.022) |
| Fixed effects | Industry | | |
| Clustering | Industry, Yr-Qtr | | |
| N | 881 | 881 | 881 |
| Adj-$R^2$ | 0.028 | 0.024 | 0.038 |

Note: Sample period is from 1997 through 2022. Corp OAS refers to the ICE BofA US Corporate Index Option-Adjusted Spread (BAMLC0A0CM) from FRED. Loan STM refers to spread-to-maturity for all loans provided by PitchBook LCD. $D$ refers to the total amount of debt, the sum of the loan amount from Dealscan and the bond amount from Mergent FISD, used for the corresponding LBO transaction. $EV$ refers to the (estimated) enterprise value of the target company at LBO. Industry refers to 69 industries based on the Global Industry Classification Standard (GICS).

Table A4: Regression of the LBO Transaction Leverage on the High-Yield Option-Adjusted Spread Controlling for Other Rate/Ratio

(a) Adding Treasury Yields

| Dependent Variable: $\log\left(\frac{D}{EV}\right)$ | | | | | | |
|---|---|---|---|---|---|---|
| HY OAS change from | -0.041 *** | | | -0.04 ** | -0.044 *** | -0.044 ** |
| 6mo before close | (0.015) | | | (0.015) | (0.016) | (0.017) |
| 2yr Tsy yield at start | | -0.005 | | 0.001 | | 0.007 |
| | | (0.012) | | (0.012) | | (0.02) |
| 2yr Tsy yield change from | | 0.038 | | 0.005 | | 0.035 |
| 6mo before close | | (0.042) | | (0.039) | | (0.043) |
| 10yr Tsy yield at start | | | -0.005 | | 0 | -0.009 |
| | | | (0.018) | | (0.017) | (0.032) |
| 10yr Tsy yield change from | | | 0.029 | | -0.02 | -0.053 |
| 6mo before close | | | (0.049) | | (0.048) | (0.057) |
| Fixed effects | | | Industry | | | |
| Clustering | | | Industry, Yr-Qtr | | | |
| N | 882 | 882 | 882 | 882 | 882 | 882 |
| Adj-$R^2$ | 0.039 | 0.026 | 0.025 | 0.036 | 0.037 | 0.035 |

(b) Adding S&P 500 Price-to-Dividend Ratio

| Dependent Variable: $\log\left(\frac{D}{EV}\right)$ | | | | | | |
|---|---|---|---|---|---|---|
| HY OAS at close | -0.033 ** | | -0.033 ** | | | |
| | (0.013) | | (0.013) | | | |
| HY OAS change from | | | | -0.041 *** | | -0.035 ** |
| 6mo before close | | | | (0.015) | | (0.015) |
| $PD$ at close | | 0 | 0 | | | |
| | | (0.002) | (0.002) | | | |
| $PD$ at start | | | | | -0.002 | -0.001 |
| | | | | | (0.002) | (0.002) |
| $PD$ change from | | | | | 0.005 * | 0.003 |
| 6mo before close | | | | | (0.002) | (0.002) |
| Fixed effects | | | Industry | | | |
| Clustering | | | Industry, Yr-Qtr | | | |
| N | 882 | 882 | 882 | 882 | 882 | 882 |
| Adj-$R^2$ | 0.039 | 0.025 | 0.038 | 0.039 | 0.031 | 0.039 |

Note: Sample period is from 1997 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Tsy refers to U.S. Treasury securities. $PD$ refers to the price-to-dividend ratio of the S&P 500 Index portfolio, where dividends are computed daily for the past 63 trading days (approximately a quarter) on a rolling basis and annualized. $D$ refers to the total amount of debt, the sum of the loan amount from Dealscan and the bond amount from Mergent FISD, used for the corresponding LBO transaction. $EV$ refers to the (estimated) enterprise value of the target company at LBO. Industry refers to 69 industries based on the Global Industry Classification Standard (GICS).

Table A5: Regression of the Changes of Financial Variables after LBOs on the level of High-Yield Option-Adjusted Spread at LBO Close; LBO Targets and Control Firms

(a) Changes of Balance Sheet and Cash Flow Variables

| | log(Assets) | log(Liabilities) | log(Sale) | Leverage | $\frac{\text{Sale}}{\text{Assets}}$ | $\frac{\text{Interest}}{\text{Assets}}$ | $\frac{\text{Interest}}{\text{Liabilities}}$ |
|---|---|---|---|---|---|---|---|
| LBO Dummy | 0.382 *** | 0.566 *** | 0.043 ** | 0.084 *** | -0.432 *** | 0.01 *** | 0.013 *** |
| | (0.036) | (0.031) | (0.021) | (0.012) | (0.045) | (0.001) | (0.001) |
| HY OAS at Close | 0.013 | 0.002 | -0.021 | 0 | -0.015 | -0 | -0 |
| | (0.03) | (0.045) | (0.017) | (0.008) | (0.029) | (0.001) | (0.001) |
| LBO Dummy × | -0.005 | -0.006 | 0.014 | 0.007 | -0.008 | 0.002 | 0.001 |
| HY OAS at Close | (0.023) | (0.028) | (0.017) | (0.006) | (0.022) | (0.001) | (0.001) |
| Firm-level controls | | | | Yes | | | |
| Fixed effects | | | | Industry, Yr-Qtr | | | |
| Clustering | | | | Industry, Yr-Qtr | | | |
| N of LBOs | 842 | 842 | 851 | 822 | 851 | 726 | 728 |
| N | 4103 | 4101 | 4158 | 4012 | 4130 | 3400 | 3390 |
| Adj-$R^2$ | 0.135 | 0.183 | 0.041 | 0.193 | 0.251 | 0.128 | 0.126 |

| | $\frac{\text{Current Liabilities}}{\text{Liabilities}}$ | $\frac{\text{Long-term Liabilities}}{\text{Liabilities}}$ | $\frac{\text{Current Assets}}{\text{Assets}}$ | $\frac{\text{Cash}}{\text{Assets}}$ | $\frac{\text{Tangible Assets}}{\text{Assets}}$ | $\frac{\text{Retained Earning}}{\text{Assets}}$ | $\frac{\text{CAPX}}{\text{Assets}}$ |
|---|---|---|---|---|---|---|---|
| LBO Dummy | -0.163 *** | 0.108 *** | -0.131 *** | -0.032 *** | -0.181 *** | -0.131 *** | -0.004 *** |
| | (0.008) | (0.007) | (0.009) | (0.004) | (0.011) | (0.02) | (0.001) |
| HY OAS at Close | -0.01 | -0.011 | -0.004 | -0.001 | 0 | -0.013 | -0.003 |
| | (0.01) | (0.013) | (0.004) | (0.004) | (0.007) | (0.017) | (0.002) |
| LBO Dummy × | 0.011 | -0.009 | 0.007 | 0.003 | 0.008 | -0.012 | 0.001 |
| HY OAS at Close | (0.007) | (0.009) | (0.006) | (0.003) | (0.006) | (0.013) | (0.002) |
| Firm-level controls | | | | Yes | | | |
| Fixed effects | | | | Industry, Yr-Qtr | | | |
| Clustering | | | | Industry, Yr-Qtr | | | |
| N of LBOs | 844 | 628 | 833 | 722 | 825 | 696 | 576 |
| N | 4100 | 2680 | 4060 | 3400 | 4022 | 3411 | 2404 |
| Adj-$R^2$ | 0.13 | 0.077 | 0.196 | 0.051 | 0.273 | 0.094 | 0.013 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

(b) Changes of Target Performance Variables

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets |
|---|---|---|---|---|---|
| LBO Dummy | -0.055 *** | -0.057 *** | -0.209 *** | -0.048 *** | -0.064 *** |
| | (0.008) | (0.007) | (0.047) | (0.008) | (0.005) |
| HY OAS at Close | -0.002 | 0.007 | 0.001 | 0.003 | 0.001 |
| | (0.008) | (0.005) | (0.018) | (0.006) | (0.006) |
| LBO Dummy × | 0.009 | 0.003 | 0.009 | 0.002 | -0 |
| HY OAS at Close | (0.006) | (0.006) | (0.025) | (0.006) | (0.003) |
| Firm-level controls | | | Yes | | |
| Fixed effects | | | Industry, Yr-Qtr | | |
| Clustering | | | Industry, Yr-Qtr | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 |
| Adj-$R^2$ | 0.09 | 0.147 | 0.126 | 0.099 | 0.227 |

| | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|
| LBO Dummy | -0.144 *** | -0.007 | -0.051 *** | -0.079 ** |
| | (0.027) | (0.006) | (0.005) | (0.036) |
| HY OAS at Close | -0.02 | 0.003 | 0.002 | -0.033 |
| | (0.019) | (0.003) | (0.004) | (0.024) |
| LBO Dummy × | 0.004 | 0.002 | 0.001 | -0.021 |
| HY OAS at Close | (0.021) | (0.004) | (0.003) | (0.027) |
| Firm-level controls | | | Yes | |
| Fixed effects | | | Industry, Yr-Qtr | |
| Clustering | | | Industry, Yr-Qtr | |
| N of LBOs | 537 | 857 | 849 | 664 |
| N | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.142 | 0.053 | 0.217 | 0.146 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table A6: Regression of the Changes of Performance Variables after LBOs on the 4-month Change of High-Yield Option-Adjusted Spread; LBO Targets and Control Firms

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets |
|---|---|---|---|---|---|
| LBO Dummy | -0.055 *** | -0.057 *** | -0.209 *** | -0.048 *** | -0.063 *** |
| | (0.008) | (0.006) | (0.046) | (0.008) | (0.005) |
| HY OAS Change | 0.001 | 0.003 | 0.006 | 0.001 | -0.005 |
| | (0.004) | (0.004) | (0.009) | (0.004) | (0.005) |
| LBO Dummy × | 0.01 * | 0.013 *** | 0.088 ** | 0.014 * | 0.012 * |
| HY OAS Change | (0.006) | (0.004) | (0.032) | (0.007) | (0.006) |
| Firm-level controls | | | Yes | | |
| Fixed effects | | | Industry, Yr-Qtr | | |
| Clustering | | | Industry, Yr-Qtr | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 |
| Adj-$R^2$ | 0.09 | 0.148 | 0.13 | 0.102 | 0.228 |

| | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|
| LBO Dummy | -0.143 *** | -0.007 | -0.051 *** | -0.079 ** |
| | (0.023) | (0.006) | (0.005) | (0.034) |
| HY OAS Change | -0.027 *** | 0.002 | -0.002 | -0.032 |
| | (0.008) | (0.003) | (0.004) | (0.022) |
| LBO Dummy × | 0.113 ** | 0.007 * | 0.008 * | 0.075 * |
| HY OAS Change | (0.041) | (0.004) | (0.004) | (0.042) |
| Firm-level controls | | Yes | | |
| Fixed effects | | Industry, Yr-Qtr | | |
| Clustering | | Industry, Yr-Qtr | | |
| N of LBOs | 537 | 857 | 849 | 664 |
| N | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.147 | 0.054 | 0.218 | 0.147 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table A7: Difference-in-Difference Regression of the Post-LBO Target Performance Variables

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO × Post × | 0.011 *** | 0.006 * | 0.037 ** | 0.013 *** | 0.012 *** | 0.087 *** | 0.008 ** | 0.011 *** | 0.066 ** |
| HY OAS Change | (0.003) | (0.003) | (0.017) | (0.002) | (0.004) | (0.025) | (0.003) | (0.003) | (0.029) |
| Non-reported regressors | LBO, Post, HY OAS Change, LBO × Post, LBO × HY OAS Change, Post × HY OAS Change | | | | | | | | |
| Firm-level controls | No | | | | | | | | |
| Fixed effects | Industry, Yr-Qtr | | | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | | | |
| N of LBOs | 826 | 814 | 637 | 671 | 669 | 524 | 838 | 834 | 648 |
| N | 7916 | 7876 | 6030 | 6436 | 6428 | 4944 | 8122 | 8108 | 6154 |
| Adj-$R^2$ | 0.092 | 0.111 | 0.08 | 0.113 | 0.115 | 0.051 | 0.188 | 0.083 | 0.038 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. HY OAS Change refers to the 6-month change of the HY OAS leading up to buyout close. Industry refers to 2-digit NAICS code. Dependent variables are the level at the corresponding period. For each dependent variable, top and bottom 1% of the sample are trimmed. LBO refers to LBO dummies, and Post refers to post dummies.

Table A8: Pre- and Post-buyout Trends: Difference of Performance Variables between the LBO Targets and non-LBO Control Firms, Relative to a Year before LBO

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| 6 yrs before LBO | -0.003 | 0.002 | -0.072 | 0.003 | 0.004 | -0.066 * | 0.001 | 0 | -0.108 ** |
| | (0.007) | (0.011) | (0.054) | (0.003) | (0.005) | (0.036) | (0.003) | (0.007) | (0.044) |
| | 273/1896 | 262/1838 | 211/1488 | 219/1544 | 207/1458 | 173/1228 | 267/1860 | 254/1782 | 203/1430 |
| 5 yrs before LBO | -0.008 * | -0.009 | -0.002 | -0.006 | -0.005 | -0.052 | -0.005 | -0.012 * | -0.082 * |
| | (0.004) | (0.007) | (0.037) | (0.005) | (0.007) | (0.045) | (0.004) | (0.007) | (0.041) |
| | 421/3096 | 403/2966 | 336/2466 | 331/2448 | 322/2378 | 265/1974 | 414/3032 | 401/2956 | 325/2386 |
| 4 yrs before LBO | -0.016 *** | -0.017 *** | -0.041 | -0.012 ** | -0.015 * | -0.044 | -0.014 *** | -0.016 ** | -0.103 ** |
| | (0.004) | (0.005) | (0.029) | (0.004) | (0.008) | (0.032) | (0.004) | (0.007) | (0.039) |
| | 557/4466 | 546/4414 | 457/3634 | 484/3888 | 471/3800 | 394/3158 | 554/4442 | 535/4324 | 453/3594 |
| 3 yrs before LBO | -0.013 *** | -0.016 *** | -0.038 * | -0.009 * | -0.016 ** | -0.032 | -0.006 | -0.019 *** | -0.103 ** |
| | (0.004) | (0.004) | (0.021) | (0.004) | (0.006) | (0.025) | (0.005) | (0.006) | (0.043) |
| | 791/6904 | 769/6758 | 650/5604 | 697/6098 | 678/5972 | 577/4996 | 786/6826 | 762/6654 | 647/5558 |
| 2 yrs before LBO | -0.005 * | -0.007 * | -0.017 | -0.004 | -0.007 * | -0.017 | -0.004 | -0.006 | -0.04 * |
| | (0.003) | (0.004) | (0.015) | (0.003) | (0.004) | (0.017) | (0.003) | (0.004) | (0.02) |
| | 1045/10030 | 1013/9762 | 868/8194 | 943/9104 | 916/8860 | 785/7472 | 1036/9954 | 1007/9682 | 861/8146 |
| 1 yr after LBO | -0.064 *** | -0.068 *** | -0.19 *** | -0.043 *** | -0.066 *** | -0.194 *** | -0.021 *** | -0.072 *** | -0.255 *** |
| | (0.008) | (0.005) | (0.019) | (0.005) | (0.005) | (0.025) | (0.004) | (0.005) | (0.035) |
| | 894/8306 | 878/8220 | 766/6944 | 740/6924 | 738/6950 | 639/5842 | 880/8190 | 876/8206 | 753/6854 |
| 2 yrs after LBO | -0.055 *** | -0.066 *** | -0.2 *** | -0.045 *** | -0.069 *** | -0.206 *** | -0.006 | -0.055 *** | -0.171 *** |
| | (0.008) | (0.005) | (0.027) | (0.008) | (0.006) | (0.032) | (0.004) | (0.005) | (0.04) |
| | 792/6734 | 771/6576 | 658/5420 | 679/5658 | 670/5634 | 563/4576 | 801/6826 | 787/6702 | 659/5422 |
| 3 yrs after LBO | -0.054 *** | -0.061 *** | -0.18 *** | -0.042 *** | -0.064 *** | -0.118 *** | -0.007 | -0.05 *** | -0.015 |
| | (0.005) | (0.007) | (0.022) | (0.005) | (0.005) | (0.022) | (0.005) | (0.005) | (0.051) |
| | 627/4846 | 617/4780 | 489/3708 | 524/4026 | 526/4028 | 410/3050 | 632/4944 | 628/4904 | 494/3756 |
| 4 yrs after LBO | -0.05 *** | -0.064 *** | -0.158 *** | -0.04 *** | -0.064 *** | -0.111 *** | -0.01 * | -0.048 *** | -0.001 |
| | (0.004) | (0.006) | (0.021) | (0.004) | (0.007) | (0.028) | (0.006) | (0.005) | (0.051) |
| | 470/3360 | 464/3282 | 344/2440 | 391/2692 | 385/2664 | 278/1914 | 481/3436 | 475/3376 | 355/2522 |
| 5 yrs after LBO | -0.057 *** | -0.065 *** | -0.149 *** | -0.035 *** | -0.052 *** | 0.008 | -0 | -0.039 *** | 0.195 *** |
| | (0.008) | (0.009) | (0.029) | (0.007) | (0.009) | (0.049) | (0.006) | (0.007) | (0.054) |
| | 320/2134 | 318/2112 | 210/1414 | 255/1658 | 251/1632 | 166/1090 | 325/2176 | 325/2172 | 216/1444 |
| Firm-level controls | | | | | No | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |

Note: Sample period is from 2011 through 2022. Industry refers to 2-digit NAICS code. Dependent variables are the level at the corresponding period. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table A9: Pre- and Post-buyout Trends: Sensitivity of Difference of Performance Variables between the LBO Targets and non-LBO Control Firms, Relative to a Year before LBO, to the Level of HY OAS at 6 Months before LBO Close

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| 6 yrs before LBO | -0.004 | 0.006 | 0.013 | 0.003 | 0.012 *** | 0.02 | 0 | 0.005 | 0.006 |
| | (0.006) | (0.007) | (0.048) | (0.003) | (0.004) | (0.036) | (0.005) | (0.004) | (0.046) |
| | 273/1896 | 262/1838 | 211/1488 | 219/1544 | 207/1458 | 173/1228 | 267/1860 | 254/1782 | 203/1430 |
| 5 yrs before LBO | 0.002 | 0.006 | -0.008 | -0.005 | -0.014 | -0.014 | -0.005 *** | -0.008 *** | -0.013 |
| | (0.004) | (0.005) | (0.027) | (0.006) | (0.01) | (0.04) | (0.001) | (0.002) | (0.042) |
| | 421/3096 | 403/2966 | 336/2466 | 331/2448 | 322/2378 | 265/1974 | 414/3032 | 401/2956 | 325/2386 |
| 4 yrs before LBO | 0.002 | -0.002 | -0.019 | -0.003 *** | -0.008 ** | -0.032 | -0.003 ** | -0.007 ** | -0.031 |
| | (0.004) | (0.002) | (0.014) | (0.001) | (0.004) | (0.026) | (0.001) | (0.003) | (0.035) |
| | 557/4466 | 546/4414 | 457/3634 | 484/3888 | 471/3800 | 394/3158 | 554/4442 | 535/4324 | 453/3594 |
| 3 yrs before LBO | 0 | -0.004 | -0.008 | -0.003 | -0.008 ** | -0.042 *** | -0.003 | -0.008 ** | -0.011 |
| | (0.003) | (0.003) | (0.015) | (0.002) | (0.003) | (0.015) | (0.002) | (0.003) | (0.022) |
| | 791/6904 | 769/6758 | 650/5604 | 697/6098 | 678/5972 | 577/4996 | 786/6826 | 762/6654 | 647/5558 |
| 2 yrs before LBO | 0.001 | 0.002 | -0.006 | -0.003 ** | -0.004 * | -0.023 | -0.003 ** | -0.002 | -0.001 |
| | (0.002) | (0.003) | (0.011) | (0.001) | (0.002) | (0.017) | (0.001) | (0.003) | (0.022) |
| | 1045/10030 | 1013/9762 | 868/8194 | 943/9104 | 916/8860 | 785/7472 | 1036/9954 | 1007/9682 | 861/8146 |
| 1 yr after LBO | 0.001 | -0.003 | -0.006 | -0.005 | -0.002 | -0.004 | -0.002 | -0.002 | -0.001 |
| | (0.004) | (0.004) | (0.016) | (0.005) | (0.004) | (0.018) | (0.003) | (0.003) | (0.006) |
| | 894/8306 | 878/8220 | 766/6944 | 740/6924 | 738/6950 | 639/5842 | 880/8190 | 876/8206 | 753/6854 |
| 2 yrs after LBO | -0.001 | -0.002 | -0.015 | -0.007 | -0.003 | -0.008 | -0.001 | -0.002 | 0.012 |
| | (0.005) | (0.005) | (0.014) | (0.005) | (0.005) | (0.025) | (0.005) | (0.003) | (0.022) |
| | 792/6734 | 771/6576 | 658/5420 | 679/5658 | 670/5634 | 563/4576 | 801/6826 | 787/6702 | 659/5422 |
| 3 yrs after LBO | -0.001 | -0.003 | -0.017 | -0.007 ** | -0.003 | -0.063 ** | -0.002 | 0 | -0.039 |
| | (0.005) | (0.005) | (0.017) | (0.003) | (0.004) | (0.025) | (0.005) | (0.004) | (0.034) |
| | 627/4846 | 617/4780 | 489/3708 | 524/4026 | 526/4028 | 410/3050 | 632/4944 | 628/4904 | 494/3756 |
| 4 yrs after LBO | -0.01 ** | -0.004 | -0.017 * | -0.008 *** | -0.009 | -0.042 | -0.004 | -0.001 | -0.072 * |
| | (0.004) | (0.003) | (0.01) | (0.002) | (0.007) | (0.028) | (0.004) | (0.003) | (0.038) |
| | 470/3360 | 464/3282 | 344/2440 | 391/2692 | 385/2664 | 278/1914 | 481/3436 | 475/3376 | 355/2522 |
| 5 yrs after LBO | 0 | -0.004 | -0.042 ** | 0.001 | -0.001 | -0.054 | -0.004 | -0.003 | -0.12 *** |
| | (0.007) | (0.005) | (0.018) | (0.004) | (0.007) | (0.059) | (0.006) | (0.003) | (0.034) |
| | 320/2134 | 318/2112 | 210/1414 | 255/1658 | 251/1632 | 166/1090 | 325/2176 | 325/2172 | 216/1444 |
| Firm-level controls | | | | | No | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Dependent variables are the level at the corresponding period. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table A10: Regression of the Changes of Post-LBO Target Financial Variables on the Level of High-Yield Option-Adjusted Spread at 6 Months before LBO Close; LBO Targets and Control Firms

(a) Changes of Balance Sheet and Cash Flow Variables

| | log(Assets) | log(Liabilities) | log(Sale) | Leverage | $\frac{\text{Sale}}{\text{Assets}}$ | $\frac{\text{Interest}}{\text{Assets}}$ | $\frac{\text{Interest}}{\text{Liabilities}}$ |
|---|---|---|---|---|---|---|---|
| LBO Dummy × | 0.011 | 0.038 * | -0.006 | 0.019 *** | -0.031 | 0.002 *** | 0.001 |
| HY OAS at 6mo bef Close | (0.019) | (0.021) | (0.014) | (0.003) | (0.019) | (0) | (0.001) |
| Non-reported regressors | LBO Dummy, HY OAS at 6mo bef Close | | | | | | |
| Firm-level controls | Yes | | | | | | |
| Fixed effects | Industry, Yr-Qtr | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | |
| N of LBOs | 842 | 842 | 851 | 822 | 851 | 726 | 728 |
| N | 4103 | 4101 | 4158 | 4012 | 4130 | 3400 | 3390 |
| Adj-$R^2$ | 0.136 | 0.184 | 0.041 | 0.196 | 0.252 | 0.129 | 0.126 |

| | $\frac{\text{Current Liabilities}}{\text{Liabilities}}$ | $\frac{\text{Long-term Liabilities}}{\text{Liabilities}}$ | $\frac{\text{Current Assets}}{\text{Assets}}$ | $\frac{\text{Cash}}{\text{Assets}}$ | $\frac{\text{Tangible Assets}}{\text{Assets}}$ | $\frac{\text{Retained Earning}}{\text{Assets}}$ | $\frac{\text{CAPX}}{\text{Assets}}$ |
|---|---|---|---|---|---|---|---|
| LBO Dummy × | -0.002 | 0.004 | -0.001 | 0.003 | 0 | -0.021 | -0 |
| HY OAS at 6mo bef Close | (0.006) | (0.008) | (0.004) | (0.004) | (0.003) | (0.013) | (0.002) |
| Non-reported regressors | LBO Dummy, HY OAS at 6mo bef Close | | | | | | |
| Firm-level controls | Yes | | | | | | |
| Fixed effects | Industry, Yr-Qtr | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | |
| N of LBOs | 844 | 628 | 833 | 722 | 825 | 696 | 576 |
| N | 4100 | 2680 | 4060 | 3400 | 4022 | 3411 | 2404 |
| Adj-$R^2$ | 0.13 | 0.076 | 0.195 | 0.052 | 0.273 | 0.095 | 0.012 |

(b) Changes of Target Performance Variables

| | $\frac{\text{Net Income}}{\text{Sale}}$ | $\frac{\text{Net Income}}{\text{Assets}}$ | $\frac{\text{Net Income}}{\text{Book Equity}}$ | $\frac{\text{Operating Income}}{\text{Sale}}$ | $\frac{\text{Operating Income}}{\text{Assets}}$ | $\frac{\text{Operating Income}}{\text{Book Equity}}$ | $\frac{\text{EBITDA}}{\text{Sale}}$ | $\frac{\text{EBITDA}}{\text{Assets}}$ | $\frac{\text{EBITDA}}{\text{Book Equity}}$ |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy × | 0.001 | -0.005 | -0.038 * | -0.009 *** | -0.01 * | -0.08 *** | -0.005 | -0.006 | -0.071 *** |
| HY OAS at 6mo bef Close | (0.005) | (0.006) | (0.019) | (0.003) | (0.005) | (0.019) | (0.004) | (0.004) | (0.024) |
| Non-reported regressors | LBO Dummy, HY OAS at 6mo bef Close | | | | | | | | |
| Firm-level controls | Yes | | | | | | | | |
| Fixed effects | Industry, Yr-Qtr | | | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 | 537 | 857 | 849 | 664 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.09 | 0.147 | 0.128 | 0.101 | 0.229 | 0.146 | 0.055 | 0.219 | 0.148 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table A11: Regression of the Changes of Post-LBO Target Performance Variables on the Lag and Lead 6-month Changes of High-Yield Option-Adjusted Spread; LBO Targets and Control Firms

(a) Regressor: Lag 6-month HY OAS Change

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy | -0.055 *** | -0.057 *** | -0.208 *** | -0.048 *** | -0.064 *** | -0.143 *** | -0.007 | -0.051 *** | -0.079 ** |
| | (0.008) | (0.006) | (0.047) | (0.008) | (0.005) | (0.028) | (0.005) | (0.005) | (0.037) |
| Lag HY OAS Chg | -0.001 | 0.001 | -0.017 | -0.001 | -0.001 | -0.048 | -0.002 | 0 | -0.026 |
| | (0.003) | (0.002) | (0.025) | (0.004) | (0.005) | (0.045) | (0.002) | (0.002) | (0.022) |
| LBO Dummy × | 0.004 | 0.001 | -0.007 | -0.007 | -0.004 | -0.004 | -0.006 ** | -0.001 | 0.005 |
| Lag HY OAS Chg | (0.004) | (0.007) | (0.028) | (0.004) | (0.004) | (0.04) | (0.003) | (0.003) | (0.035) |
| Firm-level controls | | | | | Yes | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 | 537 | 857 | 849 | 664 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.089 | 0.146 | 0.127 | 0.1 | 0.227 | 0.143 | 0.055 | 0.217 | 0.145 |

(b) Regressor: Lead 6-month HY OAS Change

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy | -0.055 *** | -0.057 *** | -0.209 *** | -0.048 *** | -0.064 *** | -0.142 *** | -0.007 | -0.051 *** | -0.079 ** |
| | (0.008) | (0.006) | (0.047) | (0.008) | (0.005) | (0.024) | (0.006) | (0.005) | (0.032) |
| Lead HY OAS Chg | 0.002 | -0.008 ** | -0.005 | -0.001 | -0.004 | 0.034 | 0 | -0.004 | 0.027 |
| | (0.004) | (0.003) | (0.018) | (0.003) | (0.004) | (0.028) | (0.001) | (0.003) | (0.019) |
| LBO Dummy × | -0.004 | -0.004 | -0.009 | 0.002 | -0.001 | 0.045 * | 0.001 | 0.001 | 0.075 *** |
| Lead HY OAS Chg | (0.004) | (0.005) | (0.027) | (0.004) | (0.003) | (0.023) | (0.003) | (0.003) | (0.018) |
| Firm-level controls | | | | | Yes | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 | 537 | 857 | 849 | 664 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.089 | 0.148 | 0.126 | 0.099 | 0.227 | 0.145 | 0.052 | 0.218 | 0.149 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Lag HY OAS Change refers to the change of the HY OAS from 12 months before LBO close through 6 months before close. Lead HY OAS Change refers to the change of the HY OAS from LBO close through 6 months after close. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table A12: Regression of the Shifted Changes of Post-LBO Target Performance Variables on the Lag and Lead 6-month Changes of High-Yield Option-Adjusted Spread; LBO Targets and Control Firms

(a) Regressor: Lag 6-month HY OAS Change

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy | -0.056 *** | -0.054 *** | -0.149 *** | -0.04 *** | -0.057 *** | -0.158 *** | -0.005 | -0.053 *** | -0.136 *** |
| | (0.009) | (0.006) | (0.035) | (0.008) | (0.005) | (0.03) | (0.006) | (0.005) | (0.034) |
| Lag HY OAS Chg | -0.004 ** | -0.005 *** | 0.019 | -0.007 *** | -0.003 | -0.001 | -0.006 *** | -0.006 *** | 0.018 |
| | (0.002) | (0.001) | (0.021) | (0.002) | (0.004) | (0.025) | (0.002) | (0.002) | (0.014) |
| LBO Dummy × | 0.001 | -0.001 | -0.029 | -0.005 | -0.008 | -0.014 | 0.001 | -0.002 ** | 0.011 |
| Lag HY OAS Chg | (0.008) | (0.005) | (0.023) | (0.007) | (0.005) | (0.029) | (0.003) | (0.001) | (0.03) |
| Firm-level controls | | | | | Yes | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |
| N of LBOs | 632 | 632 | 517 | 510 | 514 | 425 | 646 | 643 | 522 |
| N | 3142 | 3155 | 2480 | 2540 | 2559 | 2033 | 3235 | 3233 | 2533 |
| Adj-$R^2$ | 0.108 | 0.139 | 0.099 | 0.085 | 0.152 | 0.108 | 0.054 | 0.167 | 0.088 |

(b) Regressor: Lead 6-month HY OAS Change

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy | -0.034 *** | -0.039 *** | -0.147 *** | -0.036 *** | -0.04 *** | -0.104 *** | -0.003 | -0.023 *** | 0.017 |
| | (0.005) | (0.005) | (0.04) | (0.006) | (0.004) | (0.036) | (0.003) | (0.004) | (0.029) |
| Lead HY OAS Chg | -0.003 | -0.002 | -0.02 * | -0.003 | 0.002 | 0.02 | 0.001 | 0.001 | -0.009 |
| | (0.004) | (0.003) | (0.01) | (0.004) | (0.003) | (0.021) | (0.002) | (0.002) | (0.02) |
| LBO Dummy × | -0.001 | -0.007 *** | -0.009 | -0.005 | -0.008 *** | -0.025 ** | 0 | -0.006 *** | -0.009 |
| Lead HY OAS Chg | (0.002) | (0.001) | (0.019) | (0.003) | (0.002) | (0.012) | (0.004) | (0.001) | (0.02) |
| Firm-level controls | | | | | Yes | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |
| N of LBOs | 883 | 875 | 712 | 742 | 735 | 597 | 890 | 880 | 717 |
| N | 4252 | 4223 | 3383 | 3552 | 3537 | 2815 | 4311 | 4286 | 3410 |
| Adj-$R^2$ | 0.068 | 0.131 | 0.052 | 0.086 | 0.162 | 0.068 | 0.066 | 0.177 | 0.082 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Lag HY OAS Change refers to the change of the HY OAS from 12 months before LBO close through 6 months before close. Lead HY OAS Change refers to the change of the HY OAS from LBO close through 6 months after close. Industry refers to 2-digit NAICS code. Dependent variables are changes over the time window shifted by 6 months from the pre-post buyout change time window so that the relative timing to the lag (lead) HY OAS change may remain (almost) the same. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table A13: Regression of the Changes of Post-LBO Target Performance Variables on the Pre-LBO 6-month Change of Alternative Spreads; LBO Targets and Control Firms

(a) Corporate Index Option-Adjusted Spread

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy | -0.055 *** | -0.057 *** | -0.209 *** | -0.048 *** | -0.063 *** | -0.142 *** | -0.007 | -0.051 *** | -0.079 ** |
| | (0.007) | (0.007) | (0.048) | (0.008) | (0.005) | (0.025) | (0.006) | (0.005) | (0.032) |
| Corp OAS Change | 0.001 | 0.009 | -0.028 * | -0.021 | -0.013 | -0.194 | 0.011 | 0.001 | -0.121 |
| | (0.01) | (0.011) | (0.016) | (0.016) | (0.026) | (0.114) | (0.007) | (0.01) | (0.093) |
| LBO Dummy × | 0.033 *** | 0.02 * | 0.198 *** | 0.047 *** | 0.039 ** | 0.362 *** | 0.016 | 0.026 *** | 0.299 *** |
| Corp OAS Change | (0.011) | (0.011) | (0.054) | (0.004) | (0.014) | (0.062) | (0.01) | (0.007) | (0.067) |
| Firm-level controls | | | | | Yes | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 | 537 | 857 | 849 | 664 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.091 | 0.147 | 0.129 | 0.102 | 0.228 | 0.147 | 0.054 | 0.218 | 0.148 |

(b) Loan Spread-to-Maturity

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy | -0.055 *** | -0.057 *** | -0.21 *** | -0.048 *** | -0.064 *** | -0.144 *** | -0.007 | -0.051 *** | -0.08 ** |
| | (0.008) | (0.007) | (0.047) | (0.008) | (0.005) | (0.023) | (0.006) | (0.005) | (0.034) |
| Loan STM Change | 0.002 | 0.008 | 0.006 | -0.001 | 0.002 | -0.034 | 0.006 * | 0.001 | -0.032 |
| | (0.004) | (0.005) | (0.017) | (0.004) | (0.014) | (0.059) | (0.003) | (0.005) | (0.037) |
| LBO Dummy × | 0.013 * | 0.019 ** | 0.115 *** | 0.016 ** | 0.016 * | 0.155 *** | 0.011 ** | 0.016 ** | 0.121 *** |
| Loan STM Change | (0.007) | (0.008) | (0.026) | (0.007) | (0.009) | (0.046) | (0.006) | (0.007) | (0.036) |
| Firm-level controls | | | | | Yes | | | | |
| Fixed effects | | | | | Industry, Yr-Qtr | | | | |
| Clustering | | | | | Industry, Yr-Qtr | | | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 | 537 | 857 | 849 | 664 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.09 | 0.148 | 0.129 | 0.101 | 0.228 | 0.146 | 0.055 | 0.219 | 0.147 |

Note: Sample period is from 2011 through 2022. Corp OAS refers to the ICE BofA US Corporate Index Option-Adjusted Spread (BAMLC0A0CM) from FRED. Loan STM refers to spread-to-maturity for all loans provided by PitchBook LCD. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table A14: Regression of the Changes of Post-LBO Target Performance Variables on the Pre-LBO 6-month Change of High-Yield Option-Adjusted Spread Controlling for Other Rate/Ratio Changes; LBO Targets and Control Firms

(a) Adding Treasury Yield Changes

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy × | 0.011 ** | 0.011 *** | 0.063 ** | 0.016 *** | 0.013 *** | 0.097 *** | 0.01 ** | 0.009 ** | 0.091 *** |
| HY OAS Change | (0.004) | (0.004) | (0.023) | (0.005) | (0.003) | (0.02) | (0.004) | (0.004) | (0.02) |
| LBO Dummy × | -0.014 | 0.001 | -0.029 | -0.034 * | -0.012 | -0.089 | -0.011 | -0.003 | 0.015 |
| 2yr Tsy Yield Change | (0.012) | (0.021) | (0.131) | (0.016) | (0.019) | (0.154) | (0.013) | (0.016) | (0.154) |
| LBO Dummy × | 0.01 | 0.005 | 0.004 | 0.034 *** | 0.018 ** | 0.061 | 0.013 | 0.002 | 0.1 |
| 10yr Tsy Yield Change | (0.009) | (0.008) | (0.086) | (0.012) | (0.008) | (0.063) | (0.008) | (0.009) | (0.062) |
| Non-reported regressors | LBO Dummy, HY OAS Change, 2yr Tsy Yield Change, 10yr Tsy Yield Change | | | | | | | | |
| Firm-level controls | Yes | | | | | | | | |
| Fixed effects | Industry, Yr-Qtr | | | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 | 537 | 857 | 849 | 664 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.091 | 0.148 | 0.129 | 0.105 | 0.228 | 0.146 | 0.056 | 0.218 | 0.147 |

(b) Adding S&P 500 Price-to-Dividend Ratio Changes

| | Net Income / Sale | Net Income / Assets | Net Income / Book Equity | Operating Income / Sale | Operating Income / Assets | Operating Income / Book Equity | EBITDA / Sale | EBITDA / Assets | EBITDA / Book Equity |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy × | 0.008 ** | 0.007 *** | 0.061 ** | 0.01 ** | 0.009 *** | 0.076 *** | 0.006 ** | 0.008 *** | 0.071 *** |
| HY OAS Change | (0.004) | (0.001) | (0.026) | (0.004) | (0.002) | (0.02) | (0.003) | (0.002) | (0.016) |
| LBO Dummy × | -0.001 | -0.002 ** | -0.002 | -0.001 | -0 | -0.007 | -0.001 | -0.001 | -0.002 |
| *PD* Change | (0.001) | (0.001) | (0.007) | (0.001) | (0.001) | (0.005) | (0.001) | (0.001) | (0.004) |
| Non-reported regressors | LBO Dummy, HY OAS Change, *PD* Change | | | | | | | | |
| Firm-level controls | Yes | | | | | | | | |
| Fixed effects | Industry, Yr-Qtr | | | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | | | |
| N of LBOs | 840 | 832 | 661 | 686 | 683 | 537 | 857 | 849 | 664 |
| N | 4055 | 4028 | 3119 | 3298 | 3302 | 2529 | 4159 | 4133 | 3157 |
| Adj-$R^2$ | 0.091 | 0.15 | 0.129 | 0.105 | 0.231 | 0.148 | 0.057 | 0.22 | 0.147 |

Note: Sample period is from 2011 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Tsy refers to U.S. Treasury securities. *PD* refers to the price-to-dividend ratio of the S&P 500 Index portfolio, where dividends are computed daily for the past 63 trading days (approximately a quarter) on a rolling basis and annualized. Industry refers to 2-digit NAICS code. Dependent variables are changes from pre-buyout to post-buyout. Firm-level controls are pre-buyout levels. For each dependent variable, top and bottom 1% of the sample are trimmed.

Table A15: Regression of the LBO Change of Log of Probability of Default Assigned by Banks on the High-Yield Option-Adjusted Spread; Splitting Periods

| | Dependent Variable: $\Delta$ log(Probability of Default) | | | | | |
|---|---|---|---|---|---|---|
| | Pre-Post Change: Shorter Post Window | | | Post-Only Change | | |
| LBO Dummy | 0.352 *** | 0.352 *** | 0.353 *** | 0.39 *** | 0.388 *** | 0.392 *** |
| | (0.055) | (0.05) | (0.05) | (0.072) | (0.06) | (0.057) |
| HY OAS at Close | 0.072 | | | -0.065 | | |
| | (0.055) | | | (0.139) | | |
| HY OAS at Start | | -0.063 | | | -0.203 *** | |
| | | (0.062) | | | (0.058) | |
| HY OAS Change | | | 0.078 * | | | 0.1 |
| | | | (0.044) | | | (0.078) |
| LBO Dummy × | -0.104 *** | | | -0.12 ** | | |
| HY OAS at Close | (0.029) | | | (0.049) | | |
| LBO Dummy × | | -0.009 | | | 0.098 ** | |
| HY OAS at Start | | (0.012) | | | (0.046) | |
| LBO Dummy × | | | -0.069 *** | | | -0.219 *** |
| HY OAS Change | | | (0.02) | | | (0.043) |
| Firm-level controls | Yes | | | | | |
| Additional controls | $\log\left((\text{Probability of Default})_{start}\right)$ | | | | | |
| Fixed effects | Industry, Bank, Yr-Qtr | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | |
| N of LBOs | 358 | 358 | 358 | 451 | 451 | 451 |
| N of LBO-Bank Pairs | 456 | 456 | 456 | 555 | 555 | 555 |
| N | 1856 | 1856 | 1856 | 2130 | 2130 | 2130 |
| Adj-$R^2$ | 0.109 | 0.105 | 0.108 | 0.126 | 0.127 | 0.132 |

Note: Sample period is from 2012 through 2022, but almost all of the sample is from the end of 2014. Probability of default refers to default probability for firms assigned by reporting banks in FR Y-14Q. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. "Pre-Post Change: Shorter Post Window" means that the dependent variable is the change of the (log) default probability from pre-buyout to a year after buyout close. "Post-Only Change" means that the dependent variable is the change of the (log) default probability from a year after to 3 years after buyout close. Firm-level controls are levels as of the start of the change of the (log) default probability. $(\text{Probability of Default})_{start}$ is the probability of default at the start of the window for estimating the change of log probability of default.

Table A16: Pre- and Post-buyout Trends: Difference of (log) Default Probability between the LBO Targets and non-LBO Control Firms, Relative to a Year before LBO

| Dependent Variable: log(Probability of Default) | | | | |
|---|---|---|---|---|
| | Regression Coefficients on: | | | |
| | LBO Dummy (No HY OAS) | LBO Dummy × 6mo HY OAS Change | LBO Dummy × HY OAS at 6mo bef LBO | N LBOs/N LBO-Bank Pairs<br>N Obs |
| 5 yrs before LBO | -0.039 | 0.029 | -0.173 *** | 136/217 |
| | (0.081) | (0.021) | (0.034) | 1460 |
| 4 yrs before LBO | 0.071 ** | 0.054 | -0.082 *** | 247/389 |
| | (0.03) | (0.039) | (0.017) | 2692 |
| 3 yrs before LBO | 0.058 | -0.019 | 0.041 | 382/581 |
| | (0.058) | (0.04) | (0.024) | 4308 |
| 2 yrs before LBO | 0.109 *** | -0.038 | 0.036 * | 601/954 |
| | (0.025) | (0.028) | (0.02) | 7566 |
| 1 yr after LBO | 0.315 *** | -0.061 *** | 0.006 | 382/483 |
| | (0.053) | (0.013) | (0.016) | 3968 |
| 2 yrs after LBO | 0.471 *** | -0.214 *** | 0.128 *** | 275/338 |
| | (0.093) | (0.043) | (0.028) | 2554 |
| 3 yrs after LBO | 0.661 *** | -0.236 *** | 0.226 *** | 180/214 |
| | (0.142) | (0.054) | (0.053) | 1496 |
| 4 yrs after LBO | 0.674 *** | -0.295 *** | 0.218 * | 135/164 |
| | (0.143) | (0.081) | (0.105) | 1076 |
| Firm-level controls | | Yes | | |
| Fixed effects | | Industry, Bank, Yr-Qtr | | |
| Clustering | | Industry, Yr-Qtr | | |

Note: Sample period is from 2012 through 2022, but almost all of the sample is from the end of 2014. Probability of default refers to default probability for firms assigned by reporting banks in FR Y-14Q. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. The dependent variable is the level of the (log) probability of default at the corresponding period. Firm-level controls are levels at the reference year (a year before LBO).

Table A17: Regression of the LBO Change of Log of Probability of Default Assigned by Banks on the 6-month Change of High-Yield Option-Adjusted Spread: Controlling for Performance Change

| | Dependent Variable: $\Delta$ log(Probability of Default) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| LBO Dummy | 0.331 *** | 0.252 *** | 0.4 *** | 0.307 *** | 0.21 *** | 0.419 *** | 0.364 *** | 0.254 *** | 0.428 *** |
| | (0.052) | (0.053) | (0.079) | (0.045) | (0.052) | (0.084) | (0.046) | (0.05) | (0.084) |
| HY OAS Change | 0.112 | 0.088 | 0.103 | 0.112 | 0.079 | 0.107 | 0.118 | 0.08 | 0.107 |
| | (0.069) | (0.077) | (0.081) | (0.069) | (0.077) | (0.082) | (0.071) | (0.079) | (0.084) |
| LBO Dummy × | -0.193 *** | -0.194 *** | -0.235 *** | -0.196 *** | -0.191 *** | -0.234 *** | -0.201 *** | -0.189 *** | -0.235 *** |
| HY OAS Change | (0.04) | (0.038) | (0.044) | (0.045) | (0.039) | (0.046) | (0.041) | (0.034) | (0.043) |
| Performance control (change) | $\frac{\text{Net Income}}{\text{Sale}}$ | $\frac{\text{Net Income}}{\text{Assets}}$ | $\frac{\text{Net Income}}{\text{Book Equity}}$ | $\frac{\text{Operating Income}}{\text{Sale}}$ | $\frac{\text{Operating Income}}{\text{Assets}}$ | $\frac{\text{Operating Income}}{\text{Book Equity}}$ | $\frac{\text{EBITDA}}{\text{Sale}}$ | $\frac{\text{EBITDA}}{\text{Assets}}$ | $\frac{\text{EBITDA}}{\text{Book Equity}}$ |
| Firm-level controls | Yes | | | | | | | | |
| Additional controls | $\log\left((\text{Probability of Default})_{start}\right)$ | | | | | | | | |
| Fixed effects | Industry, Bank, Yr-Qtr | | | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | | | |
| N of LBOs | 431 | 431 | 362 | 432 | 432 | 363 | 431 | 431 | 362 |
| N of LBO-Bank Pairs | 524 | 524 | 438 | 525 | 525 | 439 | 524 | 524 | 438 |
| N | 1974 | 1974 | 1630 | 1979 | 1979 | 1634 | 1976 | 1976 | 1631 |
| Adj-$R^2$ | 0.148 | 0.195 | 0.12 | 0.161 | 0.207 | 0.114 | 0.148 | 0.192 | 0.111 |

Note: Sample period is from 2012 through 2022, but almost all of the sample is from the end of 2014. Probability of default refers to default probability for firms assigned by reporting banks in FR Y-14Q. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. The dependent variable is the change of the (log) default probability from 1 year after to 3 years after buyout close. Firm-level controls are levels as of the start of the change of the (log) default probability. $(\text{Probability of Default})_{start}$ is the probability of default at the start of the window for estimating the change of log probability of default.

Table A18: Regression of the Strictness of Financial Covenants for LBO Loan Deals on the High-Yield Option-Adjusted Spread; LBO Deals Only

| | Dependent Variable: | | | | | |
|---|---|---|---|---|---|---|
| | Number of Covenants | | | Covenant Strictness Measure | | |
| HY OAS at Close | 0.179 *** | | | 0.027 | | |
| | (0.053) | | | (0.022) | | |
| HY OAS at Start | | 0.218 *** | | | 0.032 | |
| | | (0.063) | | | (0.019) | |
| HY OAS Change | | | -0.117 | | | -0.016 |
| | | | (0.113) | | | (0.044) |
| Firm-level controls | Yes | | | | | |
| Additional controls | log(N of Lenders) | | | | | |
| Fixed effects | Industry, Lead Agent | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | |
| N of LBO Loan Deals | 193 | 193 | 193 | 193 | 193 | 193 |
| N of LBO Loan Deal-Lead Agent Pairs | 221 | 221 | 221 | 221 | 221 | 221 |
| N | 221 | 221 | 221 | 221 | 221 | 221 |
| Adj-$R^2$ | 0.433 | 0.467 | 0.39 | 0.303 | 0.313 | 0.288 |

Note: Sample period is from 1997 through 2019. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Observations are at the level of deal-lead agent pairs. Lead agents are identified following Ivashina (2009). The covenant strictness measure is computed following Murfin (2012).

Table A19: Regression of Post-LBO Covenant Compliance on the High-Yield Option-Adjusted Spread; LBO Deals Only

| | Dependent Variable: | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Post-LBO Covenant Breach Dummy | | | Post-LBO Covenant Compliance with Waiver/Amendment Dummy | | | Post-LBO Waiver/Amendment Dummy | | |
| HY OAS at Close | -0.001 | | | 0.015 ** | | | 0.012 | | |
| | (0.003) | | | (0.006) | | | (0.008) | | |
| HY OAS at Start | | 0.017 ** | | | 0.003 | | | 0.01 | |
| | | (0.008) | | | (0.003) | | | (0.006) | |
| HY OAS Change | | | -0.011 * | | | 0.011 ** | | | 0.003 |
| | | | (0.005) | | | (0.005) | | | (0.005) |
| Additional controls | log(N of Participants), Agent Bank Share, Nonbank Share | | | | | | | | |
| Fixed effects | Industry, Review Bank, Review Date | | | | | | | | |
| Clustering | Industry, Yr-Qtr | | | | | | | | |
| N of LBOs | 510 | 510 | 510 | 510 | 510 | 510 | 510 | 510 | 510 |
| N of LBO Obs | 857 | 857 | 857 | 857 | 857 | 857 | 857 | 857 | 857 |
| N | 857 | 857 | 857 | 857 | 857 | 857 | 857 | 857 | 857 |
| Adj-$R^2$ | 0.152 | 0.167 | 0.163 | 0.119 | 0.112 | 0.116 | 0.127 | 0.126 | 0.125 |

Note: Covenant compliance sample period is from 2007 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 2-digit NAICS code. Observations are at the level of company-review bank-review date pairs. N of Participants is the number of unique entities that participate in any syndicated loans in SNC to the obligor. Agent bank share is the share of the agent bank's commitment out of the entire loan commitments (in SNC) to the obligor. Nonbank Share is the share of nonbanks' (identified by SNC) commitment out of the entire loan commitments (in SNC) to the obligor.

Table A20: Regression of the Variables Related to Target Strategies on the Level and Change of the High-Yield Option-Adjusted Spread

(a) Time (in Years) between LBOs and the Sale of the LBO Targets

| Dependent Variable: Time from LBO to Sale | | | |
|---|---|---|---|
| HY OAS at close | 0.011 | | |
| | (0.027) | | |
| HY OAS at start | | -0.029 | |
| | | (0.027) | |
| HY OAS change from 6mo before close | | | 0.053 ** |
| | | | (0.024) |
| Fixed effects | Industry | | |
| Clustering | Industry, Yr-Qtr | | |
| N | 6464 | 6464 | 6464 |
| Adj-$R^2$ | 0.006 | 0.007 | 0.008 |

(b) Number of Post-LBO Acquisitions by LBO Targets

| Dependent Variable: N of Acquisitions | | | |
|---|---|---|---|
| HY OAS at close | -0.039 *** | | |
| | (0.012) | | |
| HY OAS at start | | -0.037 *** | |
| | | (0.013) | |
| HY OAS change from 6mo before close | | | 0.003 |
| | | | (0.02) |
| Fixed effects | Industry | | |
| Clustering | Industry, Yr-Qtr | | |
| N | 3654 | 3654 | 3654 |
| Adj-$R^2$ | 0.069 | 0.069 | 0.068 |

Note: Sample period is from 1997 through 2022. HY OAS refers to the ICE BofA US High Yield Index Option-Adjusted Spread (BAMLH0A0HYM2) from FRED. Industry refers to 69 industries based on the Global Industry Classification Standard (GICS). For the time between an LBO and the sale of the target, the top and bottom 5% of the sample are trimmed. The number of post-LBO acquisitions counts the number of acquisitions by the LBO target company between buyout close and 3 years after close.