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Abhishek Bhardwaj
Abhinav Gupta
Sabrina T. Howell

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Capital Structure & Firm Outcomes: Evidence from Dividend Recapitalizations in Private Equity
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ABSTRACT

We study the causal effect of a large increase in firm leverage. Our setting is dividend recapitalizations in private equity (PE), where portfolio companies take on new debt to pay investor returns. After accounting for positive selection into more debt, we show that dramatically increasing leverage makes firms much riskier. The debt-bankruptcy relationship is in line with Altman-Z model predictions. Dividend recapitalizations increase deal returns but reduce: (a) wages among surviving firms; (b) pre-existing loan prices; and (c) fund returns, which seems to reflect moral hazard via new fundraising. These results suggest negative implications for employees, pre-existing creditors, and investors.

Abhishek Bhardwaj
A. B. Freeman School of Business
Tulane University
7 McAlister Dr
New Orleans, LA 70118
United States
abhardwaj@tulane.edu

Sabrina T. Howell
NYU Stern School of Business
KMC 9-93
44 West 4th Street
New York, NY 10012
and NBER
showell@stern.nyu.edu

Abhinav Gupta
UNC Chapel Hill
abhinav_gupta@kenan-flagler.unc.edu

A feature of all capital structure models is a causal, positive relationship between debt ratios and the chance of bankruptcy. For example, trade-off theory describes how firms weigh the tax benefits of new debt against its positive effect on bankruptcy risk (Leland, 1994; Fan and Sundaresan, 2000). However, the debt-bankruptcy relationship is challenging to document because firms that are less risky choose higher debt ratios, so the correlation between debt and bankruptcy can reverse (Myers, 2001). Also, new debt typically expands the asset side of the balance sheet by adding cash, which influences outcomes such as investment, employment, and bankruptcy.

We shed light on the independent effect of a large increase in leverage that occurs when private equity (PE) managers leverage company assets or cash flows to deliver financial returns to their funds without selling the portfolio company. We term these transactions “leveraged payouts." Dividend recapitalizations (or “recaps”) are one such common tool in the PE playbook, in which the proceeds of new debt backed by expected cash flows is paid as returns to investors. Leveraged payouts tilt the capital structure towards more debt without injecting more resources into the borrowing company. They are not like dividend recaps in publicly traded companies, where new debt issuance and total debt are small relative to assets, investors can easily sell ownership, and disclosure requirements limit agency problems (Kaplan and Stein, 1990; Gupta and Rosenthal, 1991; Peyer and Shivdasani, 2001). They also differ from leveraged buyouts (LBOs), where leverage increases but the company experiences the various treatment effects of new PE ownership (Boucly et al., 2011; Bloom et al., 2015; Agrawal and Tambe, 2016; Fracassi et al., 2022).

Despite the growing use of leveraged payouts, there is to our knowledge no rigorous study of these transactions. Understanding them is important for policy, as media reports and bankruptcy proceedings often allege that dividend recaps cause insolvency (Lim and Weiss, 2024). However, PE industry representatives argue that leveraged payouts are one element of the bidirectional capital flows between a PE fund and its portfolio companies, and are justified when companies perform well (AIC, 2021). This paper provides the first comprehensive analysis of leveraged payouts, shedding light on the causal relationship between significant debt increases and financial distress, with implications extending beyond the PE sector.

We obtain real and financial outcome data in what we believe to be the most comprehensive analysis of a PE sample to date. Our paper is also relatively rare in analyzing a sample that includes all industries rather than focusing on a single sector. Using PitchBook, LCD, and Dealscan data, we focus on a set of 61,628 unique U.S. buyout deals, of which we identify subsequent dividend recaps for 1,572, or 2.6% of deals 9.3% of LBO volume. The outcomes include bankruptcy from LexisNexis; fund returns from Burgiss; deal returns from a fund-of-funds[1]; and exit, employment, and revenue from the U.S. Census Bureau.

We show that on average, dividend recaps are conducted on large and seemingly healthy firms. To establish a causal effect of dividend recaps, we exploit how PE managers respond opportunistically to cheaper credit. We isolate deals that occur when a particular PE firm has access to relatively cheaper credit than its

[1]This is a large fund-of-funds and advisory services firm, which has built a private market database since 2006. This firm wishes to be anonymous, so we describe it as the fund-of-funds below.

peer firms. Our design is motivated by extensive evidence that cheap credit conditions cause PE managers to use more debt (Kaplan and Stein, 1993; Kaplan and Schoar, 2005; Shivdasani and Wang, 2011; Axelson et al., 2013; Davis et al., 2021), as well as broader work on how supply-side channels play a role in firm leverage (Baker and Wurgler, 2002; Leary, 2009; Haddad et al., 2017). There is also evidence from practitioners; for example, Fitch Ratings notes that it expects PE firms to "opportunistically tap windows of high credit market demand to seek cheap funding for a dividend recap on their legacy assets" (Reuters, 2012).

PE firm relationships with bank lenders affect debt financing for portfolio companies.[2] We make use of this fact to instrument for dividend recaps using PE relationship banks' collateralized loan obligation (CLO) underwriting. CLOs are actively managed, highly diversified portfolios of leveraged loans; they are the main investors in PE-sponsored bank loans. Banks play crucial roles in the CLO market. A lead arranger bank originates the loan in collaboration with a PE sponsor firm, then sells part or all of the loan to CLOs and other buyers (Bord and Santos, 2015; Blickle et al., 2020). A bank underwrites the CLO, which includes approving every loan in the portfolio (Benmelech et al., 2012). When the bank has a relationship with the loan's PE sponsor, it is likely easier for the CLO to acquire the loan. This implies that the bank has more information about and is incentivized to place loans that it has originated in the CLO, and the CLO manager is incentivized to purchase them because it is beholden to the bank for the overall CLO process. Motivated by this relationship, we instrument for a portfolio company dividend recap using the outstanding value of CLOs that are (a) in the loan purchasing phase; and (b) underwritten by the PE firm's relationship banks.[3]

We then construct a stacked instrumental variables (IV) regression design. The stacked approach ensures there is no staggered treatment bias (Baker et al., 2022). Within each stack, we compare treated firms that receive a dividend recap to control firms in a similar industry, backed by a similarly-sized PE firm, and that had their LBO at a similar time, but never experienced a dividend recap. We then look at outcomes centered around the dividend recap year for all the companies in the stack. We show a robust first stage and support the intuition of the instrument by documenting that a dividend recap loan is much more likely to end up in a CLO underwritten by the sponsoring PE firm's relationship bank than a random CLO.

The exclusion restriction is that relationship bank CLO volume must not affect the trajectory of the targeted portfolio company except through opportunistic dividend recaps. There are good reasons to think this is the case. First, loans issued by at least 100 unique companies compose the CLO, so no one company motivates CLO formation or determines CLO performance. Second, our results do not seem to reflect the PE firm forming relationships with banks it expects to underwrite more CLOs in the near term, because our results are robust to using PE firm-bank relationships created long before the CLO launch as well as to instrumenting with relationship bank entry into the CLO underwriting business. Third, there are no pre-trends in a dynamic first stage, so PE firms are not shifting the timing of dividend recap deals that would

[2]See Drucker and Puri (2009); Demiroglu and James (2010); Ivashina and Kovner (2011); Malenko and Malenko (2015); Shive and Forster (2022).

[3]To construct the instrument, we use information on loans, bank relationships, and CLOs with data from LCD, Dealscan, Acuris CLO-i, and Capital IQ.

occur anyway to wait for the shock (else we would expect an anticipatory dip). More intuitively, since there are strong incentives for fund managers to conduct dividend recaps, credit constraints likely explain the absence of dividend recaps for many portfolio companies. Our data point to a pecking order, where some of the healthiest portfolio companies experience dividend recaps, while compliers with our instrument become creditworthy when debt becomes exogenously cheaper.

The causal analysis paints a picture in which new debt induced by cheap credit dramatically increases firm risk. The outcome we are most interested in is bankruptcy, which is central to capital structure theory. Dividend recaps increase the chance of bankruptcy by 31 percentage points (pp) in the following six years. This is a large effect relative to the sample mean of 1.3%. On the intensive margin, a 1 pp increase in the leverage ratio increases the chance of bankruptcy by 3.5 pp, or in dollar units, a separate estimate shows that $1 million in additional debt increases the chance of bankruptcy by 0.3 pp. To our knowledge, these regressions offer the first causal quantified estimate of the effect of additional leverage on bankruptcy.

We ask whether the main result's magnitude is what we would expect from theory. We use Altman's Z score model for private firms to benchmark our findings against those from standard prediction models (Altman, 1993). A typical dividend recap increases a company's total debt by 84%.[4] At the same time, we find that while pre-dividend recap LBO targets have leverage typical of safer companies, post-dividend recap leverage is close to those of failed firms, as estimated by Altman et al. (2017). Finally, we find that the change in Z score around the dividend recap is roughly a third of the difference between normal and failed firms, which is consistent with our estimate of a 31 pp increase in bankruptcy likelihood. Overall, dividend recaps cause a large increase in leverage, and the dramatic effect on bankruptcy that we find in causal analysis is in line with what we would expect from standard bankruptcy prediction models.

Consistent with the bankruptcy results, using the U.S. Census Bureau's data we find that dividend recaps increase the chance that a firm exits within six years by 33 pp, compared to a sample mean of 18.5%. (We use the dividend recap indicator rather than amount for analysis due to data coverage.) Yet at the same time, dividend recaps increase the chances of exceptionally good outcomes, in the form of IPOs and the incidence of especially high revenue growth among survivor firms. This suggests that the additional leverage makes the firm riskier in the sense that the tails of its outcome distribution become fatter.

Turning to employees, we examine the effects of a dividend recap on four-year growth in firm employment, payroll, and average wages relative to the year before the event, after restricting the sample to firms that survived at least to the fourth year after the dividend recap. We find a large negative effect on wage growth of -53%, relative to a mean of -4%. This is driven by declining payroll, especially at the left tail (i.e., the worst performers among survivors). There is a negative albeit insignificant effect on employment growth, driven by greater chances of being in the tails of the distribution, with a significantly lower chance

[4]Dividend recaps do not simply shift debt from the LBO to later in time. We show that on average, dividend recap targets experience a dramatic increase in the average ratio of debt to earnings (EBIDTA) over the entire deal lifecycle, and have higher leverage at the time of the LBO relative to other LBO targets.

of modest employment growth. Overall, the results suggest that by making firms riskier, dividend recaps raise the specter of bad outcomes for workers—exit, bankruptcy, and significant wage declines—but also increase the chance that the firm experiences a good outcome for owners (IPO, large revenue increases).

We next consider the returns to investors. Dividend recaps could increase deal returns for at least two reasons. One is compensation for the higher risk associated with more debt. The other is that we find dividend recaps extend holding periods, which could mitigate any short-termism (e.g., Konczal (2015)) and extend access to PE operational improvements. Also, we expect dividend recaps to mechanically boost the Internal Rate of Return (IRR) by bringing returns forward in time. Indeed, dividend recaps increase deal IRR. The effect on the deal cash-on-cash multiple (i.e., Total Value Multiple or TVM) is positive but not statistically significant, suggesting that the IRR result reflects earlier realization of cash flows. Also, the dispersion of returns increases, paralleling the higher risk in real outcomes.

At the fund level, we show that dividend recaps decrease the fund's cash-on-cash multiple and public market equivalent (PME) return measures. There is no significant effect on IRR, consistent with bringing cash flows forward in the fund's life. What might explain a positive effect on deal returns yet a negative effect on fund returns? We show that dividend recaps dramatically increase short-term distributions paid out to the fund, which could incentivize the general partners (GPs) to raise a new fund based on good interim returns, consistent with Gompers (1996) and Barber and Yasuda (2017). Indeed, dividend recaps sharply increase the chance of launching a new fund. These results suggest that dividend recaps benefit GPs by enabling early distributions and new fundraising. GPs then may focus effort on the new fund. Supporting this conjecture, we observe that dividend recaps cause lower returns for subsequent LBOs within the fund and reduce the number of new LBOs pursued, relative to other funds of the same vintage.

Last, we study whether dividend recaps shift value away from pre-existing creditors. We cannot conduct a robust instrumented analysis, but we observe in OLS models that loan prices significantly decrease in the months around the dividend recap, consistent with value shifting away from pre-existing creditors. Given the positive selection in OLS, these results point to greater value shifting in the more opportunistic deals that are the compliers with our instrument. Dividend recap creditors appear at least partially compensated for higher risk, because dividend recap loans have higher interest rates than other leveraged loans.

Dividend recap targets are typically high-quality firms on growth trajectories. This selection is crucial to understanding our results, especially the difference between the causal analysis and OLS results, which capture both the positive selection and any causal impact. In OLS, we find a smaller and weaker increase in bankruptcy and lower chances of exit. Employment and payroll increase, while wages and revenue stay constant. Therefore, contrary to some media narratives, dividend recaps are not in general associated with bad outcomes, because they are usually performed on larger and stronger firms. The IV analysis significantly reduces this selection bias. The large causal effects are likely to become increasingly important amid dramatic growth of CLOs and a decline in conventional exit opportunities for PE owners. The industry press noted in mid-2024 that "US CLO issuance continues at an unprecedented rate as investor demand for leveraged loan assets swamps new issuance" (PitchBook, 2024). More supply-side driven CLO issuance should create demand for risky firms. PE funds fulfill this demand by increasing leverage in existing portfolio companies through dividend recaps, which can have negative implications for some portfolio company stakeholders.

We contribute to work on how capital structure affects the firm. We analyze a change in debt that is not deployed within the firm and thus is independent of changes to the asset side of the balance sheet. The results could reasonably generalize because PE-owned firms are representative of a large share of U.S. employer firms along measures such as size, sector, and location. Most existing research on capital structure focuses on publicly traded firms, which account for less than 1% of firms, less than one third of employment, and which have unique disclosure obligations and highly dispersed ownership (Francis, 2007). Related work on debt and the implications of bankruptcy for capital structure includes Myers and Majluf (1984), Bolton and Scharfstein (1996), Faulkender and Petersen (2006), Rice and Strahan (2010), and Antill and Grenadier (2019). Focusing on leverage following a negative macro shock, Giroud and Mueller (2017) and Sever (2023) show that higher leverage predicts that employment initially expands but ultimately declines, while Kalemli-Özcan et al. (2022) shows that higher leverage predicts lower investment.

This paper also offers a tool to correct for the endogeneity between leverage and firm risk, which pushes the correlation in the opposite direction from what theory would predict. For example, Titman et al. (2005) show that there is only a weak positive relationship between loan-to-value (LTV) for commercial mortgages, inconsistent with theory, but that it becomes 10 times larger after partially correcting for selection. The magnitude aligns with our results. Our empirical design also connects to work on determinants of leverage, including Benmelech and Bergman (2009), Lemmon and Roberts (2010), Eisfeldt and Rampini (2009), Rauh and Sufi (2010), and De Maeseneire and Brinkhuis (2012).[5] Our results suggest that responsiveness to credit supply shocks may help to explain why there is so much variation in capital structure across firms, and why various theories anchored in credit demand fail to consistently predict capital structure.

While our results might generalize to private firms more broadly, understanding capital structure in PE is independently important. With $4.4 trillion in U.S. assets under management, PE funds own firms that employ over 12 million U.S. workers and account for 6.5% of U.S. GDP.[6] We contribute to the PE literature by exploring capital structure under PE ownership and connecting it to value creation. Existing research has established that PE managers aim to target under-performing firms for LBOs, improve their performance, and ultimately generate returns by selling for more than the purchase price (Gompers et al., 2016; Bernstein et al., 2019; Howell et al., 2022; Fracassi et al., 2022).[7] In contrast, PE firms target high quality companies

[5] This also relates to work on the syndicated loan market, lead arranger incentives, and corporate debt securitization (Ivashina and Scharfstein, 2010; Benmelech et al., 2012; Nadauld and Weisbach, 2012; Wang and Xia, 2014; Lee et al., 2022; Griffin and Nickerson, 2023).

[6] We should expect this footprint to grow, as PE funds have $2.6 trillion in funds waiting to be invested. See AIC (2023) for employment and GDP statistics, which are for 2022, Pitchbook (2023) for AUM, which is for 2023, and Asif and Sabater (2023) for dry powder statistics, which is also for 2023.

[7] Further work on the real outcomes side includes Acharya et al. (2012), Davis et al. (2014), Agrawal and Tambe (2016), Eaton et al. (2020), Cohn et al. (2021), Ewens et al. (2022) and Howell et al. (2022), among many others. The literature on returns includes Kaplan and Schoar (2005), Phalippou and Gottschalg (2009), Harris et al. (2014), Korteweg and Sorensen (2017), Brown

within the portfolio for dividend recaps. Also, there is evidence that PE firms specialize in managing firms through distress to avoid bankruptcy (Tykvová and Borell, 2012; Hotchkiss et al., 2021; Johnston-Ross et al., 2021). These PE-specific skills should bias downward any potential negative effects of debt on bankruptcy. While there is some work describing capital structure in PE, this is the first paper to try to isolate the real effects of debt in PE (Cohn et al., 2014; Brown et al., 2021).

Finally, this paper contributes to a small dividend recap literature. Early work on dividend recaps studied small samples of public firms between the 1970s and early 1990s, without causal identification (Masulis, 1983; Kaplan and Stein, 1990; Denis and Denis, 1993; Gupta and Rosenthal, 1991; Peyer and Shivdasani, 2001). Denis (1994) conducts a case study on dividend recaps in PE. More recently, some PE research includes small samples of dividend recaps in descriptive analysis (Cohn et al., 2014; Harford and Kolasinski, 2014; Ayash et al., 2017; Hotchkiss et al., 2021). Kaplan and Stein (1993) study the first PE boom-bust period in the 1980s. They show how the junk bond market led to over-leveraging and unsustainable debt burdens in LBOs, which precipitated a market collapse. We find evidence that in a different lending market—leveraged loans—history does not repeat but it does rhyme. We have yet to see whether rising interest rates will lead to a wave of defaults among PE-backed firms who benefited from opportunistic leverage during the low rate period, but our results do suggest that opportunistic leverage increases the chance of distress, holding all else equal.

1 Context, Data Sources, and Summary Statistics

In this section, we first describe dividend recaps in PE, connect them to the larger class of leveraged payout transactions, and explain how they are different from dividend recaps at public companies (Section 1.1). We introduce the data sources in Section 1.2. We describe summary statistics in Section 1.3, shedding new light on dividend recaps and PE more broadly.

1.1 Leveraged Payouts and Dividend Recaps

We will first introduce the basic PE operating model. PE funds are financial intermediaries that source capital from limited partners (LPs) such as pension funds and endowments. The GPs, who own the PE firm and manage its funds, are responsible for the lifecycle of a deal: choosing the company to acquire, negotiating the transaction, adjusting operations at the target firm, and finally harvesting value, usually via a liquidation event in which they sell the portfolio company.[8] PE is associated with high-powered incentives to maximize profits because of the large share of debt on the balance sheet and because GPs are compensated with a call option-like share of profits (Kaplan and Stromberg, 2009).

et al. (2019), and Gupta and Van Nieuwerburgh (2021).

[8]For details on the PE business model, see Kaplan and Stromberg (2009), Robinson and Sensoy (2016), Korteweg and Sorensen (2017), Jenkinson et al. (2021), and Gompers and Kaplan (2022).

In the traditional PE business model, fund managers target an under-performing firm via an LBO, improve it, and make money by selling it for a higher price.[9] Over the past two decades a new class of transaction emerged—which we term the "leveraged payout"—where PE managers leverage company assets or cash flows to deliver financial returns to their funds without selling the portfolio company. There are at least three such strategies. One is the sale of real estate, where the portfolio company takes on new lease obligations. Another is stock-backed loans, where company-owned stock is posted as collateral. The third is the dividend recap, where the proceeds of new debt backed by expected cash flows is paid as returns to investors. Figure 1 presents a diagram showing how dividend recaps (and other leveraged payouts) affect firm capital structure. Dividend recaps have become a significant tool in the PE playbook, as shown in Figure 2.

A positive view is that leveraged payouts might permit longer holding periods, extending the benefits of PE "treatment" for the company while also increasing returns to investors. Additional debt may further discipline management, thereby improving company performance (Jensen, 1986). Finally, sophisticated creditors may restrict debt for payouts to extremely strong companies. This perspective predicts that leveraged payouts will benefit the company and financial stakeholders. Alternatively, these deals might represent excessive debt, reflecting agency problems between fund managers and their investors (Axelson et al., 2009, 2013). The new debt may reduce company resources and increase risk, leading to detrimental outcomes for the company and its stakeholders.

This more negative view is highlighted by media stories and by creditors in bankruptcy proceedings (Lim and Weiss, 2024). For example, creditors filing a lawsuit against Caxton-Iseman Capital, which had owned the restaurant chain Buffets, claimed that "The principal purpose of these transactions was to pay huge dividends to defendants by borrowing huge amounts of money that left Buffets insolvent and on a path to bankruptcy."[10] Similarly, when Bain Capital-owned KB Toys went bankrupt in 2012, creditors claimed an earlier dividend recap rendered the firm insolvent.[11] And Cerberus' sale of Steward Health Care System's real estate created rent obligations that were later blamed for the hospital system's bankruptcy.[12] In contrast, the PE industry contextualizes leveraged payouts within the bidirectional capital flows between a PE fund and a company (AIC, 2021). Scott Sperling, co-president of Thomas H Lee Partners, said:

> "[Simmons Bedding], during our ownership, increased its investment level, built numerous new plants and took market share from its competitors. If you run a company well like that, it generally allows you to do recaps, and when the recaps were done, nobody complained about them. S&P and Moody's didn't complain at the time; they noted the company's strong operating and financial performance" (Bobeldijk, 2012).

By studying dividend recaps in a causal analysis, we shed light on optimal capital structure post-LBO, and

[9] See Kaplan and Stromberg (2009); Lerner et al. (2011); Bernstein and Sheen (2016); Gupta et al. (2023)

[10] Buffets was bought in an LBO with $130 million in equity and $515 million in debt. In a dividend recap two years later, the company distributed $150 million to the PE fund. Six years later, Buffets filed for bankruptcy (Bogoslaw, 2008; Fitzgerald, 2010).

[11] Bain Capital invested $18 million in equity (alongside $237 million in debt) to acquire KB Toys in 2000. Two years later, they employed a dividend recap to fund an $85 million payout, for a 370% return on equity (Vardi, 2013).

[12] See Cerberus (2016); Smallwood (2022); Phakdeetham and Shah (2024).

contribute to understanding capital structure and the rise of private credit in the economy more broadly.

1.2 Data Sources and Collection

We believe that our real and financial outcomes represent the most comprehensive picture of a PE sample to date. In Appendix B, we explain each dataset that we use in the analysis and our filtering and matching strategies in detail. Here, we provide a brief overview.

We begin with a dataset of PE deals, funds, and firms from Pitchbook through 2024. We restrict the deals to LBOs and remove those with missing investor or deal date, leaving roughly 110,000 deals. We next retain deals between 1985, when our Pitchbook dataset starts, and 2020 when the CLO underwriting data ends. Ending in 2020 also allows us to observe outcomes in the following years. Next, we identify lead investors and map them to the sponsors in the LCD-Dealscan combined database. We retain only those Pitchbook deals for which we can verify in LCD-Dealscan that at least one investor is a PE firm, because some investors in the PitchBook PE universe are not PE firms. There are in total 1,232 investors in the data which we manually verify to be PE firms. Finally, we drop any deal in which the only investor is an add-on platform. Our resulting core dataset contains 61,628 deals across 54,790 unique companies. We then add information about subsequent dividend recaps, which are drawn from the combined Pitchbook and LCD-Dealscan database. 1,572 of these LBOs are followed by a dividend recap. Deals followed by dividend recaps are larger, accounting for 9.3% of all leveraged loans in our data by volume. There were 442 PE firms and 1,440 portfolio companies associated with these dividend recap deals.

We collect portfolio company bankruptcy and IPO data from LexisNexis, Preqin and Pitchbook. To access administrative information on real outcomes, we match the Pitchbook LBO target companies to the U.S. Census Bureau's Business Register. The matching exercise is summarized in Appendix B and described in detail in Appendix C. We match 33,500 unique firms with reasonable confidence. We use time series data that appear in the Longitudinal Business Database (LBD) on employment, payroll, revenue, average wage, and exit. We structure the dataset at the LBO level to align with the rest of our analysis, with time-varying outcome variables centered around the deal year. For example, we create the variable Emp_{t-1} to represent employment in the year before the deal. We match 1,888 funds from PitchBook to fund data from Burgiss (44%) and 9,780 LBOs to deal data from the fund-of-funds provider (16%).

To define lending relationships between PE firms and banks, we gather loans taken by PE-backed companies using LCD (now owned by Pitchbook) and Refinitiv Dealscan. The raw loan sample has 28,421 loans corresponding to 12,925 borrowers, 4,751 PE firms, and 798 banks. We construct the shocks for our instrumental variables analysis at the PE-firm level by combining the PE-bank relationship data with banks' CLO underwriting data from the Acuris CLO-i database.[13] Of all the banks in our loan sample, 29 have

[13]This has been used by Ivashina and Sun (2011), Benmelech et al. (2012), Loumioti and Vasvari (2019a), and Elkamhi and Nozawa (2022), among others.

underwritten a CLO. These banks are large, accounting for 84% of the total lending volume to PE-backed companies in our raw loan sample. As discussed above, we also limit the set of PE firms to those common in the Pitchbook and LCD-Dealscan data. To study CLO investment in the dividend recap loans, we combine LBO deals from Pitchbook with CLO holdings data from Acuris CLO-i database. Of all dividend recap loans in our data, 777 were financed by one or more CLOs. Finally, we study the secondary market performance of loans issued by PE-backed companies using daily quotes from the Loan Syndications and Trading Association (LSTA) loan pricing service. The dataset covers almost 80% of the loan trading activity in the U.S. and has been used by Saunders et al. (2020) among others. We supplement LSTA data with CLO transactions data containing loan transaction prices from Acuris CLO-i database. We match the LSTA/CLO-i data to 2,227 Pitchbook companies, of which 718 have a dividend recap.

This paper benefits from data on multiple deal dimensions, including both real and financial outcomes. However, the private nature of the industry means the sources are subject to access restrictions, making it impossible in some cases to combine them. Furthermore, since the samples vary depending on the match, we cannot always test whether we see the same effects on the overlapping sample or assert that results in a given matched sample would be the same in the complementary non-matched sample. While this creates some caveats to interpretation, we believe that our results taken together paint a consistent picture.

1.3 Summary Statistics: Understanding Dividend Recaps

One contribution of our study is to provide the first academic analysis of dividend recaps. In our sample of unique LBO targets, 2.6% experience dividend recaps. Among the 1,232 unique GPs in the raw data, 36% have executed at least one dividend recap (the figure is 43% using our estimation sample). Figure 2 describes the number and value of dividend recaps over time.[14] These deals became popular during the PE boom of the mid-2000s, reaching 10% of LBOs in 2004, then declined sharply during the Financial Crisis before rising again. In 2024, there were about 200 dividend recaps with a combined value of roughly $125 billion, accounting for around 4% of lagged LBOs by count and close to 30% by value.

We compare the industry composition of firms with dividend recaps to the overall sample with LBOs in Figure 3 Panel A. The distribution is similar, albeit with a higher fraction of consumer-facing firms and a lower fraction of financial and business-facing firms. Dividend recaps tend to occur one to three years after the LBO, peaking at two years (Panel B of Figure 3). Dividend recaps are associated with longer holding periods; Panel C of Figure 3 shows that the distribution shifts to the right when comparing dividend recap targets to LBO targets overall. The mean is 7.3 years vs. 5.8 years.

Summary statistics on outcome variables from the stacked analysis sample—which also conditions on observing the instrument—are in Table 1, with a first set of columns for all deals and subsequent ones that

[14] For this figure, we include deals through 2024, though our analysis stops in 2020 to have time to observe outcomes. The values require observing both loan size in LCD and deal size in Pitchbook.

divide the sample by ultimate dividend recap status. The data are reported relative to the date of the focal dividend recap year in the stack (as we will explain below, this means that if the dividend recap target had the dividend recap in 2010, then all other control firms in the stack have outcomes measured relative to 2010). The statistics for the full, unstacked sample are in Table A1. The chance of bankruptcy within six years after the focal dividend recap year is 1.31%; it is higher for the firms that experience a dividend recap, at at 2.14%, relative to 1.29% for the control firms. Note that many LBOs occur later in the sample (e.g., in 2019) and may go bankrupt after our sample period ends. The bankruptcy rates in our data are somewhat lower than in earlier PE literature, which focused on small samples of mostly pre-Financial Crisis public-to-private deals.[15] Our sample is dominated by private-to-private LBOs, where firms are smaller and more likely to restructure than to file for bankruptcy.

The next set of variables concerns real outcomes from the U.S. Census Bureau-matched sample. We are required to round observation counts, so the last two columns do not add up to the first. For exit, we calculate whether the firm has exited as of four and six years following the dividend recap. The means are 16% and 19%, respectively. Dividend recap targets have lower chances of exit. For continuous outcomes, we restrict the analysis to survivor firms that are observed each year from $t-1$ to $t+4$, where we center all LBO firms around the focal target firm's dividend recap year, which is $t=0$. Conditional on survival, we see substantial growth in employment, payroll, and revenue. For example, the average (median) payroll is \$45 (\$7) million in $t-1$ and \$52 (\$14) million in $t+3$. This increase is driven by large employment gains. The average wage falls from \$63,000 to \$57,000, though the median rises from \$53,000 to \$56,000. These patterns could reflect greater unrealized equity-based compensation for senior employees. Finally, average (median) revenue is \$392 (\$21) million in $t-1$ and \$764 (\$158) million in $t+3$.

For our analysis, we use outcomes representing four-year growth relative to $t-1$. For example, employment growth is defined as $\frac{(Emp_{t+3}-Emp_{t-1})}{Emp_{t-1}}$ and has a mean of 18%. Average payroll, wage, and revenue growth are 13%, -0.4%, and 39%, respectively. Therefore, on average following LBOs we see increases in firm growth and a slight decline in wages. We focus on categorical variables capturing the nature of growth: Did firms experience a very good outcome, a good outcome, a poor outcome, or a very poor outcome? We approximate these with indicators for growth greater than 75% (very good), between 0 and 75% (good), between 0 and negative 75% (poor), and less than negative 75% (very poor).

The statistics suggest that PE-backed firms in general are relatively representative of the overall distribution of U.S. firms. For example, the median PE-backed business employed 69 workers in 2022 (AIC, 2023). In our Census-matched dataset, the median is 110. Overall in the economy, about 96% of all C-corporation employer firms have fewer than 100 employees, and these firms account for 32% of all private sector employment.[16]

[15] See Kaplan and Stein (1993); Strömberg (2008); Kaplan and Stromberg (2009); Braun et al. (2011); Cohn et al. (2014); Ayash and Rastad (2021).

[16] See https://www.census.gov/data/tables/2019/econ/susb/2019-susb-annual.html

The following two sets of variables on deal characteristics are from the fund-of-funds and Burgiss samples. The deal outcomes describe the overall deal, from LBO entry to exit. They show that dividend recap targets tend to have higher returns. For example, the average deal returns 2.7 times the initial investment (total value multiple, or TVM); for dividend recap targets, average TVM is 3.6. Dividend recap targets also have much larger changes in average gross profit, at 105% vs. -18%. Notably, they exhibit a 67 pp increase in Debt/EBITDA between the LBO and exit, vs. a 34 pp decline for other firms, consistent with the dividend recap significantly increasing debt loads. This fact together with the higher leverage at the time of the LBO shows that dividend recaps do not represent a shifting of debt from the LBO to later in time.

Summary statistics about the LBO deal and leveraged loans from our analysis sample—which again requires a match to the instrument—are in Table 2. In Panel A, the sample is divided according to whether the LBO was followed by a dividend recap or not. Dividend recap targets tend to be substantially larger than their counterparts, with an LBO deal size of $676 million compared to $294 million. In the raw sample, these figures are $755 million and $470 million (Table A1).[17] Dividend recap targets are also larger in the fund-of-funds data using total enterprise value (TEV), and note this is also consistent with the employment and revenue figures. Following the LBO, they have higher debt loads and higher gross profits. The average debt-to-EBIDTA ratio is 3.9, which is roughly in line with industry standards according to LCD.[18] Funds pursuing dividend recaps tend to be larger. At the firm level, we see that firms doing dividend recaps tend to have more investments and assets under management (AUM). Panel B shows summary statistics about the leveraged loans in the Dealscan-LCD combined sample. The average loan is for $216 million and has a five-year maturity. Dividend recap loans are about the same size as non-dividend recap loans but have a higher spread, at 440 vs. 399 basis points. They are also more likely to be covenant light.

2 Empirical Strategy

The intuition for our approach is that when a PE firm has short-term exogenously lower-cost access to the leveraged loan market, it is more likely to undertake an opportunistic dividend recap with one of its portfolio companies. The instrument relies on two relationships: (i) Between a CLO manager and the bank underwriting the CLO; (ii) Between a PE firm and their relationship bank. The exclusion restriction is that CLOs underwritten by the relationship bank cannot be independently related to the trajectory of the targeted portfolio company. At the time of the credit shock, the chosen company may be more amenable to a dividend recap relative to others in the PE firm's portfolio. Within-fund selection should bias towards more positive results, since higher quality firms tend to be chosen for dividend recaps. However, we show similar results after restricting the sample to PE firms with only one company that could plausibly have a dividend recap.

In the remainder of this section, we first explain how CLOs operate (Section 2.1). Next, we describe

[17] Note that Pitchbook reports deal size only for 5,168 out of the 53,539 stack-deal observations in our sample.

[18] https://pitchbook.com/news/articles/with-lbos-scarce-leverage-in-syndicated-us-loan-market-sinks-to-7-year-low

why a relationship bank-underwritten new CLO would exogenously reduce the cost of credit for the PE fund and lead to an opportunistic dividend recap (Section 2.2). We then explain the instrument (Section 2.3) and present the estimating equations and first stage results (Section 2.4). Finally, we provide empirical evidence for the mechanism and validation tests (Section 2.5).

2.1 Background on Collateralized Loan Obligations (CLOs)

The leveraged loan market, which includes essentially all LBO and dividend recap financing, depends primarily on CLOs for funding; indeed, roughly two-thirds of leveraged loan issuance since 2008 has been funded by the CLO industry (Cordell et al., 2023). CLOs are special-purpose vehicles that acquire a highly diversified pool of leveraged loans and repackage them into a set of securities with varying risk levels, or tranches. Like a PE fund, a CLO has a manager, which is often a private lender such as Golub Capital or the private credit wing of a large PE firm such as Blackstone. The vast majority of loans purchased by CLOs are syndicated, with a lead arranger bank who originates the loan in collaboration with the PE sponsor firm. In what has become a standard originate-to-distribute model, the bank sells part or all of the loan to CLOs and other buyers (Bord and Santos, 2015; Blickle et al., 2020).

The life cycle of a typical CLO is illustrated in Figure 4. At inception, the manager approaches a bank to obtain a line of credit, which she uses during a warehousing phase of six to nine months to acquire an initial set of loans. After the warehousing phase, the deal formally closes and the bank begins to market it to investors. The investors give the manager long-term financing, which is used to pay down the line of credit and to purchase additional loans over the next six months (the ramp-up phase) until the manager reaches her target asset volume and the CLO becomes effective. The CLO then enters the reinvestment phase and starts trading loans in the secondary market according to the contractually mandated risk profile and portfolio concentration limits. This phase lasts five to six years, after which the CLO winds down. The manager stops trading and maturing loans pay out remaining investors. This amortization phase can last six to ten years, at which point the CLO matures and the fund is closed.

A CLO contains loans issued by at least 100 unique companies, so no one company can determine CLO performance; indeed, the CLO contract generally restricts company and industry-level exposures. CLOs purchase floating-rate, senior-secured term loans (either the whole loan or part of it), which are fully collateralized, implying the company has strong cash flows or other assets. However, the loans are generally high-risk and not investment grade, with ratings at B+ or below. The magic of diversification and tranche securitization is that some debt tranches are rated highly (AAA and AA) and thus suitable for institutional buyers such as banks and insurance companies. The equity tranche is usually owned by the CLO manager and its private credit fund. Despite the higher risk, Benmelech et al. (2012) finds that there is little adverse selection in securitization by CLOs. Furthermore, CLO managers earn excess returns not through skill at selecting loans, but rather by pricing the debt tranches to benefit the equity tranche (Nickerson and Griffin, 2017; Cordell et al., 2023).

2.2 PE-Bank-CLO Manager Relationships

The discussion so far explains that CLOs demand risky debt issued by PE-backed companies and make investment decisions in collaboration with underwriting banks, who screen and approve borrowers. The bank can thus ensure that CLO securities backed by the loans are rated and priced appropriately for the potential investors. The underwriting bank also provides bridge loans to finance loan purchases. In sum, the underwriting bank is deeply involved in a new CLO's loan selection process. Simultaneously, the bank may have private information about its client PE firms, leading it to screen their loans favorably (Ivashina and Kovner, 2011), or it may give client PE firms privileged access to new CLOs in order to secure future lending business. Therefore, *when a PE firm has a relationship with a CLO underwriting bank, it should be easier to place a new portfolio company loan with the new CLO*. Shivdasani and Wang (2011) provide evidence for this channel by documenting the within-bank correlation between LBO lending and CLO underwriting.

To construct an indicator for PE-bank relationships, we focus on the lead PE firm in the company's LBO and identify its relationship banks based on recent non-dividend recap loans it has sponsored for portfolio companies. We define a PE firm p as having a relationship with a bank b in year t if at least one company sponsored by p took a loan from bank b (as lead bank) during year t. During the sample period, PE firms have relationships with two banks on average, and banks have relationships with three PE firms on average. The banks in the CLO underwriting business have relationships with four PE firms on average.

The data support a PE-Bank-CLO channel as a driver of dividend recaps. Out of 782 dividend recap loans in our data that were financed by CLOs, more than 66% were bought by CLOs underwritten by a bank related to the PE. To formally show that CLOs are more likely to buy the dividend recaps of PE firms related to their underwriter bank, we borrow a method from Bharath et al. (2011) and Chodorow-Reich (2014). Here, we use a stacked sample, where each dividend recap has its own stack consisting of all the CLOs actively purchasing loans (i.e., are in their warehousing or ramp-up phase) in the same year as the dividend recap loan was issued. We then estimate the following specification:

$$\mathbb{1}(\text{DR Purchased by CLO})_{d(p),k(b,t)} = \lambda \mathbb{1}(\text{PE-Bank Relationship})_{p,b,t-1} + \alpha_p + \alpha_k + \varepsilon_{d,k} \tag{1}$$

$\mathbb{1}(\text{DR Purchased by CLO})_{d(p),k(b,t)}$ equals one if CLO k (underwritten by bank b in year t) purchased a DR loan d sponsored by a PE firm p, and equals zero otherwise. $\mathbb{1}(\text{PE-Bank Relationship})_{p,b,t-1}$ equals one if p has a lending relationship with bank b in year $t-1$, and equals zero otherwise. The results are in Table 3 Panel A. We include PE fixed effects in case some PE managers may sponsor loans more amenable to CLOs. In Column (1), we also employ CLO fixed effects, while in Column (2), we include CLO $\times$ Year and CLO $\times$ Industry fixed effects. These address the concern that higher market share correlates with acquiring PE-backed loans. With these controls, we observe that PE firm loans that are related to the CLO underwriter have a 1.1 pp higher probability of being acquired by the CLO. This represents a 23% increase over an unconditional likelihood of a DR loan purchase, consistent with our proposed channel.

Our approach relies on the more opportunistic nature of dividend recaps relative to the debt financing undertaken at the time a PE fund acquires a new portfolio company in an LBO. In a dividend recap, the PE fund already owns the company and may take advantage of an opportunity to pull forward returns.

2.3 Instrumental Variable and Stacks

To construct the instrumental variable, we combine the PE-bank relationships with CLO issuance data, which includes the CLO's manager, portfolio, and underwriting bank. We can then quantify banks' CLO underwriting activity and CLO acquisitions of dividend recap loans (more details on these data are in Appendix B). First, we calculate prior exposure to the CLO market for each PE firm-month using the CLO underwriting activity of all banks related to that PE firm, using the definition of relationship defined in Section 2.2. We measure each bank b's underwriting activity in any given month t as the total outstanding amount of CLOs underwritten by the bank in that month (denoted by Bank CLO Volume$_{b,t}$). We only consider CLOs in the warehousing and the ramp-up phases because CLO managers purchase loans for a new CLO during these periods (Section 2.1). Specifically, we use the CLOs for which month t falls between six months before the closing date and the effective date. Next, we aggregate the CLO underwriting volume across the banks related to PE firm p and average it over the past 12 months to create the instrument.

The instrument for a dividend recap deal by PE firm p in month t is this aggregated CLO underwriting volume for p as of the previous month $t-1$, calculated as:

$$\text{R-Banks CLO Vol.}_{p,t-1} = \log\left(1 + \frac{1}{12}\sum_{\tau=t-1}^{\tau=t-13}\left(\sum_{b}\mathbb{1}(\text{PE-Bank Rel.})_{p,b,\tau} \times \text{Bank CLO Vol.}_{b,\tau}\right)\right). \quad (2)$$

Here, $\mathbb{1}(\text{PE-Bank Rel.})_{p,b,\tau}$ equals one if the PE firm p and the bank b had a lending relationship at time τ, and equals zero otherwise.[19] R-Banks CLO Vol.$_{p,t-1}$ measures the average CLO volume underwritten by p's relationship banks in the 12-month period prior to the DR month t. When it is high, firm p's cost of accessing the leveraged loan market is exogenously lower. We present summary statistics related to the instrument in Table A2. The average value of R-Banks CLO Volume$_{p,t-1}$ is 2.08 across our sample. Notably, the average value of the instrument is 3.34 among dividend recap deals and 2.06 among other deals. This simple comparison is consistent with PE firms that are more exposed to the CLO industry being more likely to undertake a dividend recap. We present a formal test of this in in Section 2.4.

To avoid concerns about staggered treatment bias and to establish a more homogeneous sample, we use a stacked approach in our regression analysis (Baker et al., 2022). For each dividend recap target portfolio company in our dataset, we create a matched stack of control LBOs. We require the control companies in each stack to be similar to the dividend recap target in their LBO date, industry, and deal size, to the degree

[19] To minimize the effect of extremely large values, we log transform volume. Since we do not use R-Banks CLO Vol.$_{p,t-1}$ as an outcome variable in our empirical analysis, this transformation does not lead to bias that occurs when an outcome variable with zeros is log transformed (Chen and Roth, 2024).

the data permit.[20] We also require control companies to have PE firm owners within a range of 10% to 10 times as large in both number of investments and AUM, and that were founded within a period of five years around the PE of the treated LBO. Finally, we drop LBOs which occurred after the dividend recap date. In our main stacked analysis sample, there are 21,439 unique deals. In robustness tests, we show that the results are not dependent on the approach to stacking or the control sample. It is important to note that our causal identification is not based on the matching between DR- and non-DR-deals, but rather exogenous variation in PE firm exposure to lower-cost credit based on their relationship banks' CLO underwriting

2.4 Estimating Equations and First Stage Analysis

The first stage—with the stacked deal-level data described above—has the following specification:

$$\mathbb{1}(\text{Dividend Recap})_{s,d(c,p,t)} = \gamma \text{R-Banks CLO Volume}_{p,t-1} + \alpha_s + \varepsilon_{s,d}. \tag{3}$$

Here, $d(c, p, t)$ denotes an LBO deal where the PE firm p acquired the target company c. Month t is the date the focal firm in the stack took out a dividend recap loan. For each stack s, $\mathbb{1}(\text{Dividend Recap})_{s,d(c,p,t)}$ equals one if the deal had a dividend recap, and equals zero otherwise.[21] The instrumental variable R-Banks CLO Volume$_{p,t-1}$ is the total CLO volume underwritten by the PE firm p's relationship banks during the month $t-1$ (expressed in logs). Stack fixed effects α_s compare the treated deal with the comparable set of control deals within the same stack.[22] Table 3 Panel B presents the results. Column (1) shows that a one standard deviation increase in the instrument increases the likelihood of a dividend recap by 13.5 pp (i.e., the coefficient of 0.04 times the instrument's standard deviation of 3.37), which is seven times the unconditional likelihood of a dividend recap (2%). Thus, as in the raw summary statistics, we observe that PE firms with a greater exposure to the CLO industry are more likely to conduct a dividend recap.

The first-stage result is robust to alternative definitions of the instrument. First, we vary how the PE firm-bank relationship is defined to address concern that PE firms with weak portfolio companies match endogenously with banks they expect to soon underwrite more CLOs. We use relationships formed during one and five years before the CLO creation and find consistent results in Columns (2) and (3) of Table 3. Next, we use alternative measures of bank underwriting activity. Instead of the value of CLOs underwritten

[20]Specifically, the control companies must have had their LBO within one year before or after the treated company. They must also be in the same industry group, using the 40 groups from Pitchbook. The control LBO deals must be at least half as large or at most twice as large as the treated company's LBO deal. Further, we drop deals with values of less than \$10 million, as the size of the smallest LBO with a dividend recap is \$13 million.

[21]When a company has multiple dividend recapitalization deals, we consider only the first one. Additionally, we exclude recaps that occur within two years of an exit, as these are often part of the exit transaction rather than standalone events.

[22]To instrument for a binary endogenous variable, we follow the recommendations of Wooldridge (2010) (on page 623, procedure 18.1; also see Adams et al. (2009)). The process is to first predict dividend recaps in a logistic regression model using total CLO issuance by relationship banks in the previous month as the instrument. We use the fitted probabilities as instruments in a two-stage least squares (2SLS) regression, as specified in Equation 3. In addition to accounting for binary variables, this method reduces concern that first-stage misspecification might influence finite-sample bias. Note that standard IV standard errors remain asymptotically valid (Wooldridge, 2010).

by the bank, we use the number of CLO deals underwritten by the bank and find consistent results (Column 4). We might worry that banks related to weak PE firms may choose to underwrite more CLOs in order to cater to their more urgent demand. To address this, we use the relationship bank's entry in the CLO underwriting business as an instrument, since a bank's decision to start underwriting CLOs is unlikely to be driven by demand for dividend recaps from a particular PE firm. We find consistent results (Column 5).

To explore the timing of the relationship between PE firms' access to CLO funding and the chances of a dividend recap, we use a stacked difference-in-differences event study model at the PE firm level. Each year-month corresponds to a unique stack (indexed by a stack date t_0). Within each stack, we restrict our sample to 24 months around the stack date and define the treated group (denoted by Treated PE $\text{Firm}_{t_0,p} = 1$) as the set of PE firms which experience more than 25% month-over-month increase in the instrument at t_0 (i.e., $\Delta(\text{R-Banks CLO Volume})_{p,t_0} \geq 25\%$). The control group is all PE firms that did not experience any such increase during the corresponding 24-month window.[23] We then estimate the following specification:

$$\mathbb{1}(\text{Dividend Recap})_{p,t} = \sum_{h=-12}^{12} \beta_h \times \text{Treated PE Firm}_{t_0,p} \times \mathbb{1}_{t-t_0=h} + \alpha_{t_0,p} + \alpha_{t_0,t} + \varepsilon_{t_0,p,t} \qquad (4)$$

The outcome variable is $\mathbb{1}(\text{Dividend Recap})_{p,t}$, which is one if PE Firm p took a DR loan in year-month t, and 0 otherwise. We include stack–PE firm ($\alpha_{t_0,p}$) and stack–year-month ($\alpha_{t_0,t}$) fixed effects to absorb cross-sectional and aggregate time-series variation in our variables.

Figure 5 reports the estimated coefficients (β_h) plotted against the corresponding time difference (h). Coefficients on the periods before the shock (i.e., with $h < 0$) indicate no pre-trends across PE firms that received a shock versus those that did not. The flat pattern before the shock also shows that PE firms are not waiting for a credit shock to execute a dividend recap on a particular firm, because otherwise we would expect to see a decline leading up to the shock. Exposure (i.e., post-shock) is associated with a 1 pp higher chance of a dividend recap in each month for about 12 months, at which point it reverts to zero. This is consistent with CLOs acquiring loans during their first 12 months (see Section 2.1). The increase is economically significant relative to the unconditional probability of 0.4%. Overall, we observe a strong first stage with dynamics that are consistent with the underlying economics. This permits us to estimate the second stage, which is:

$$y_{s,c} = \rho \widehat{\mathbb{1}(\text{Dividend Recap})}_{s,d(c,p,t)} + \alpha_s + \varepsilon_{s,c}. \qquad (5)$$

Here, $\widehat{\mathbb{1}(\text{Dividend Recap})}_{s,d(c,p,t)}$ is the predicted dividend recap from the first stage (Equation 3).

[23] We drop very small PE firms (defined as those with total investments fewer than 100) and PE firms for which the instrument value is always 0. Doing this removes PE firms that rarely do a dividend recap or are picked by our instrument. However, our results are not sensitive to this exclusion.

2.5 Instrument Assumptions and External Validity

An instrument must satisfy the relevance, exclusion restriction, and exogeneity assumptions to be valid. Above, we documented relevance through a strong first stage with meaningful magnitude. The exclusion restriction is not formally testable but based on our knowledge of the institutional context (see Sections 2.1 and 2.2), we see no means for relationship bank CLO underwriting to affect the target company besides access to low-cost credit leading to a dividend recap. There remain three potential questions about our approach:

1. Could there be reverse causality where dividend recap opportunities drive CLO creation, potentially violating the exogeneity assumption?

2. Are we focusing on an effect among low quality deals as PE firms move down their demand curves?

3. Are we focusing on an effect among high quality deals because treated PE firms select the best performers in their portfolio for dividend recaps?

Reverse Causality. There may be concern that PE firm demand for dividend recap debt for a particular company drives CLO creation. There are three reasons why this is almost certainly not occurring, all of which follow from the discussion in Section 2.1. First, the average dividend recap loan is 1.16% of the CLO, too small a share to drive the whole vehicle's creation. This is by design to ensure sufficient diversification. Second, CLO managers approach banks to underwrite a new vehicle, not vice-versa. Third, and most important, the timing of the CLO process precludes reverse causality. Our instrument is constructed using CLOs that are effective before the dividend recap loan that we are trying to predict. Therefore, the CLO warehousing period—in which the bank underwrites the CLO and the manager obtains a credit line from the bank—occurs many months before the leveraged loan, and it is implausible that the loan caused the CLO. The absence of pretrends in Figure 5 shows that this is true in practice.

Easy Financing May Lead to Lower Quality Deals. As noted in studies on PE from Kaplan and Stein (1993) to Davis et al. (2021), easy financing may lead PE firms to move "down their own demand curve" and invest in lower quality deals. In this case, compliers with our instrument–opportunistic dividend recaps– could be lower quality than dividend recaps that are selected under normal or tight credit conditions. Note that this is a question about external validity, not identification. However, our analysis is not about the effect of easy vs. tight credit, which has been studied descriptively in the existing literature. We do not compare opportunistic dividend recaps to the average dividend recap. Instead, we compare firms that experience opportunistic dividend recaps to other PE-backed firms at the same moment in time. This makes it unlikely that the results reflect moving down the firm's demand curve because in practice dividend recap targets tend to be larger and have more free cash flows to support additional debt relative to the average company at the same point in its lifecycle (see Section 1.3). They are thus if anything higher quality than the control firms.

This should assuage any concern that opportunistic dividend recaps reflect excessively lax screening on the part of the underwriting bank. Furthermore, Shivdasani and Wang (2011) show that banks' access to the CLO market—which enabled the LBO boom of the mid-2000s through the same channel as our instrument—did not lead underwriting banks to fund lower quality deals but rather to fund bigger LBO deals. Note that their sample consists of only publicly traded firms and they use a different source of variation, focusing on bank-level OLS analysis. That said, we similarly find that opportunistic dividend recaps are if anything larger than the average deal. The mechanism, therefore, does not seem likely to reflect moral hazard on the part of the underwriter.

Selection Within the Portfolio. An opposite perspective on external validity is that compliers may be higher quality relative to true random assignment, biasing our results in a positive direction. Conditional on a random shock to capital supply, the PE firm selects a company in their portfolio for an opportunistic dividend recap. If higher-quality companies are selected for dividend recaps, which seems to be the case on average, this should lead to upward bias. Two tests suggest this is not a first order issue. First, our results are robust to including only PE firms with just one portfolio company at plausible hazard of a dividend recap across all their funds. This approach also restricts the sample to small PE firms, addressing any concerns that our results are specific to large firms. Second, we partially address this issue with the control sample. As discussed in Section 2.3, we compare dividend recap targets to other PE-backed companies that are similar along many dimensions, including the timing of their LBO, their deal size and industry, and the size of the PE firm. Moreover, when we adjust these stacks to include a range of different control firms, the results are qualitatively similar, suggesting that selection on observables is not a major driver of the main finding.

External Validity Test. Dividend recaps which depend on access to relatively cheap credit may be different from the average dividend recap, limiting the external validity of our results. This issue is common to many IV settings (Bennedsen et al., 2007). We test whether this is likely to be a significant concern in our setting by comparing dividend recaps that are more and less affected by the instrument. To do so, we use the residuals of the first stage estimating equation, following the suggestion in Roberts and Whited (2013). Dividend recaps with low first stage residuals are more likely to be compliers. Therefore, we compare deals with below-median residuals to those with above-median ones. Table A3 shows that the two groups are generally similar, except that more affected dividend recaps tend to be associated with larger funds and firms. This may reflect stronger bank relationships.

3 The Effect of Dividend Recaps on Bankruptcy

This section presents the effects of instrumented dividend recaps on bankruptcy—our most important outcome—and calibrates the estimated impact of leverage on bankruptcy against predictions from theory.

Bankruptcy represents firm distress with significant private and social costs (Bernstein et al., 2019; Dou et al., 2021; Antill, 2022). For example, Davydenko et al. (2012) calculate that the cost of bankruptcy is over 30% of assets. We show in Table 4 Columns (1)-(3) that dividend recaps increase the chance of bankruptcy dramatically. Recall from Section 2.3 that we center all outcomes for firms in the stack around the dividend recap year, so bankruptcy within six years considers the chance of bankruptcy for all firms in the six years after the treated firm had its dividend recap. Since all the firms in the stack had their LBOs around the same time, the deals are at a similar stage in their lifecycle at time "zero" when the dividend recap occurs. We observe significant effects at four, six, and 10 year horizons. The first stage F-stat is 69.62, suggesting that the instrument explains the variation in the dividend recap likelihood reasonably well. Our preferred estimate at six years of 31 pp is just over twenty times the mean.

The large coefficient should be interpreted as a LATE among compliers that shifts a firm about one-third of the way (i.e., 31 pp relative to 100 pp) from very safe to failed. The reason for this interpretation connects to the way our IV is constructed. The dividend recap treatment is a binary and rare event; as shown in Table 1, it occurs for just 1.3% of the analysis sample, with even smaller rates among the healthy control firms that are ex-ante similar to dividend recap targets. When the instrument increases, the predicted probability of treatment rises (as shown in Table 3), but from a small base. The second stage regresses the outcome of bankruptcy resulting from a one-unit shift in the treatment probability; i.e., a change from 0 to 100%. The coefficient is extrapolating from a small increase in treatment probability to a one-unit shift. Below, we connect this to economic theory.

We consider the intensive margin effect of the amount of debt in Table 4 Panel B. To our knowledge, these regressions offer the first causal quantified estimate of the effect of additional leverage on bankruptcy. Our main models and real effects analysis use an indicator to maximize the sample size because we observe the amount of debt only for LCD-derived transactions, not for Pitchbook. We present four specifications with different interpretations, which together demonstrate the robustness of the general result. Column (1) uses the log of one plus the amount of debt in millions of dollars. The coefficient indicates that 1% more debt from a dividend recap increases the chance of bankruptcy by 0.19 pp.[24] Column (2) repeats without the log transformation and shows that $1 million in additional debt increases bankruptcy rates by 0.3 pp (note an average dividend recap is $214 million). We turn to leverage ratios in Columns (3)-(4), where we divide the dividend recap amount by the size of the LBO deal. Column (3) ((4)) shows that a 1% (1 pp) increase in the leverage ratio increases bankruptcy rates by 5 pp (3.5 pp).[25]

Benchmarking Against Theory We find a dramatic effect of dividend recaps on bankruptcy. Is the magnitude reasonable? To answer, we turn to the theory that motivates our research, which predicts that higher leverage increases the chance of bankruptcy, *ceteris paribus*. The most prominent model to quantify this

[24]Specifically, a 1% increase in dividend recap size increases bankruptcy rates by 18.79 $\times$ 1% = 0.1879 pp

[25]Specifically, a 1% increase in leverage ratio increases bankruptcy rates by 502$\times$ 1% = 5.02 pp for column 3, and a 1 pp increase in the leverage ratio increases bankruptcy rates by 349$\times$ 1% = 3.49 pp for column 4

relationship is Altman's Z-score, introduced in Altman (1968), and used in a wide range of settings (see Altman et al. (2017) for a review).

In a thought experiment, we ask what theory would predict for an archetypal random firm experiencing the increase in leverage associated with an average dividend recap in our sample. To do this, we use the full sample average increase in debt over the course of a deal with a dividend recap relative to one without. The average dividend recap target has starting debt of $256 million at the time of LBO, and a $214 million dividend recap loan. Therefore, absent interim loans, the dividend recap increases total debt by 84%. Does this lead to the 31 pp increase in bankruptcy rates that we find in the causal analysis, after controlling for strong positive selection? To assess, we employ the Z''-score model, which is appropriate for both private and publicly traded manufacturing and non-manufacturing firms (see e.g. Altman (1993)).[26]

The Z''-score is calculated as follows:

$$Z'' = 3.25 + 6.56X_1 + 3.26X_2 + 6.72X_3 + 1.05X_4, \text{ where:} \tag{6}$$

- X_1 = working capital / total assets
- X_2 = retained earnings / total assets
- X_3 = EBIT / total assets
- X_4 = book value of equity / book value of total liabilities

The variable affected by a dividend recap is X_4, the ratio of book value of equity to book value of total liabilities.[27] We take two approaches to benchmark this calculation against the estimates for U.S. companies in Altman et al. (2017). One uses the raw average change in leverage and the other uses our IV estimates. Both require evaluating changes in X_4. A typical firm in our sample has a starting enterprise value of $501 million, implying a book equity value of $245 million (enterprise value minus debt of $256 million).[28] If the entire $214 million dividend recap loan is used to dilute equity, the book equity decreases by $214 million, and debt rises by the same amount. Thus, X_4 declines from $\frac{\$245 \text{ million}}{\$256 \text{ million}} = 0.96$ to $\frac{\$31 \text{ million}}{\$470 \text{ million}} = 0.07$. In Altman et al. (2017)'s sample, the X_4 gap between failed and non-failed U.S. firms in Altman et al. (2017) is 0.72, comparable to the 0.89 difference between pre- and post-dividend recap firms in our sample.

The second approach considers our IV estimate and compares it to the overall Z''-score. Our baseline effect is 31 pp, $\frac{1}{3}$ of the 100 pp effect that would take firms all the way to bankruptcy. In Altman et al. (2017),

[26]We do not use the Z-score model as it is applicable for public firms, whereas all firms in our sample are private. Similarly, we do not use the Z'-score model because it applies to private manufacturing firms, whereas dividend recap firms in our sample belong to diverse set of industries (Figure 3).

[27]Since loan proceeds are paid out as dividends, total assets are unchanged. Similarly, there should be limited impact on working capital and earnings before interest and taxes (thus on X_1 and X_2). To isolate the impact of leverage and for simplicity, we assume that dividend recaps do not affect the level of retained earnings. Higher interest expense would likely lower retained earnings, so ignoring that change may underestimate the dividend recap's impact on bankruptcy.

[28]Since all the firms in our sample are private are valued using a third-party service, we assume that their reported enterprise value is the sum of their total debt and total book equity. We exclude cash holdings in the enterprise value calculation due to lack of data.

the Z''-score gap between failed and non-failed U.S. firms is 4.93. In other words, a Z''-score decline of 4.93 pushes a safe firm into failure with certainty, corresponding to a 100 pp effect. A typical dividend recap lowers the Z''-score of a firm by 0.93 (1.05×0.89). Thus, a single DR loan in our sample accounts for about $\frac{1}{5}$ of the gap between a failed and a non-failed firm in Altman et al. (2017), which is roughly consistent with the 31 pp (i.e., $\frac{1}{3}$) effect we find in the IV analysis.

In sum, this analysis shows that the raw changes in leverage that accompany a dividend recap as well as the large causal estimates of the additional leverage on bankruptcy are both in line with what we would expect from prediction models based on theory. By massively increasing leverage, dividend recaps should have a dramatic causal effect on the chance of bankruptcy.

4 The Effect of Dividend Recaps on Real Outcomes

In this section, we examine real outcomes for the firm and its employees.

Firm Exit. The effect on exits, using data from the U.S. Census Bureau, is reported in Table 4 Panel A Columns (3)-(4). Like the bankruptcy result, the effect is large, at 47 pp (about three times the mean) over a four-year horizon and 33 pp (close to two times the mean) over a six-year horizon. As the Census panel is shorter, ending in 2021, we cannot use a 10-year horizon and the results are noisier for the six-year horizon. Note that the U.S. Census Bureau-matched dataset is smaller. To ensure consistency across the two samples, we also estimate the effect on bankruptcy in the Census sample. The results, reported in Table A4, are similar to the main estimates.

IPOs and Firm Growth. If failure and bankruptcy represent the left tail of possible firm outcomes, exit to public markets via an IPO and firm growth represent the right tail of good firm outcomes. Dividend recaps may permit longer holding periods and possibly offer other benefits, such as more disciplined management. This predicts a positive effect on right-tail outcomes. Table 5 first considers the chance of a firm having an IPO over four-, six- and 10-year horizons from the dividend recap deal (Panel A). There is a dramatic, positive effect. Over the six-year horizon, the effect is roughly 40 times the mean (Panel A column (2)).

Next, we turn to revenue growth among survivors, using data from the U.S. Census Bureau. We expect this population to be selected on growth since the panel must be fully populated for four years between $t-1$ and $t+3$ (here, growth is defined as $\frac{(Rev_{t+3}-Rev_{t-1})}{Rev_{t-1}}$). Mechanically, there would be a large negative effect on growth measures if we included the whole sample, since there is such a large effect on exits. Also, as explained in Appendix C, revenue is only available for a subset of firms in the LBD, reflecting the Census Bureau's reliance on tax forms (see Haltiwanger et al. (2017) and Basker et al. (2024) for details). These restrictions lead to a much smaller sample. There is a positive coefficient for average growth in Table 5 Panel B Column (1). The four categorical outcome variables reflecting very poor, poor, good, and very good outcomes are in the following columns. Dividend recaps decrease the probability of each of the bottom three outcomes (though with no statistical significance), while there is a large, positive effect significant at the 10% level for very good realizations, where growth increases by more than 75% over the four years after a dividend recap. Overall, the results in Table 5 point to a fat right tail of good outcomes from dividend recaps that mirrors the fat left tail, though the coefficients are imprecise.

Employees To explore implications for employees, we consider growth in employment, payroll, and average wages among survivor firms, using data from the U.S. Census Bureau. The results are in Table 6. Panel A shows a negative but insignificant effect of dividend recaps on average employment growth. In the following columns, we unpack this to reveal an interesting distributional effect. Column (2) shows that there is a positive effect on having a large contraction in employment growth; the probability that employment growth declines by more than 75% increases by about 20 pp, relative to a mean of 4.5%. There is also a large decline of about 52 pp in the chance of moderate growth between zero and 75% in Column (4), though this is smaller relative to the mean, since about 40% of firms are in this category. There is a large positive but insignificant effect on high growth outcomes, of more than 75% (Column (5)). The pattern is similar in Panel B for payroll growth. Average payroll growth is large and negative but insignificant (Column (1)), driven by a large increase in left-tail outcomes; the chance of payroll contracting by more than 75% increases by 40 pp, which is about five times the mean (Column (2)). The coefficients on all the remaining outcomes are negative and insignificant, indicating a more negative effect on payroll than on employment.

This points to a negative effect on wages, which we report in Panel C. Here we see that dividend recaps reduce wage growth by 53 pp, significant at the 5% level. This is large relative to the sample mean of a 4 pp reduction. This is driven by higher chances of negative wage growth (Columns (2)-(3)) and lower chances of positive wage growth (Columns (4)-(5)). Overall, these results are consistent with dividend recaps increasing firm risk and generally reducing employment and wages, especially through large contractions, which parallels the bankruptcy and exit results.

5 The Effect of Dividend Recaps on Financial Outcomes

Thus far, we have shown that the additional debt brought on by a dividend recap increases firm risk, leading to much higher chances of firm failure but also higher chances of good outcomes, and having generally negative impacts on employees. We now turn to the third stakeholder: investors.

Deal-Level Returns. How might dividend recaps affect deal-level returns? On the one hand, dividend recaps may lower deal returns by increasing the likelihood of bankruptcy and the associated costs borne by the equity-holders. On the other hand, a large dividend may be sufficient to increase deal returns even with a poor exit outcome. To analyze deal-level returns, we construct variables with the fund-of-funds data that parallel those used above for real outcomes, allowing us to observe average and distributional effects.

The results are presented in Table 7. Measured both with IRR (Panel A) and TVM (Panel B), we find that dividend recaps increase average deal returns, with a large effect of 100 pp on IRR (Panel A Column (1)), though in both cases the coefficients are noisy. The subsequent columns in both panels suggest that dividend recaps increase the tails of the distribution, with particularly strong positive effects on very good returns. Specifically, we see significantly higher chances of good IRRs of more than 20% (Panel A Columns (4)-(5)), and dramatically higher chances of good multiples of between two and four times the investment (Panel B Column (4)). There is also a higher chance of a bad outcome (IRR less than zero, or multiple less than one), shown in Column (1) of Panels A and B. The chance of "OK" outcomes declines (Column (3) of Panels A and B). Collectively, these results indicate that dividend recaps have positive impact of deal returns largely because they increase the chance of extremely good realizations, consistent with the results for revenue, IPO, and distress above.

We consider other deal-level outcomes in Panel C of Table 7. We see that dividend recaps increase holding period by almost 13 years, compared to a mean of nearly six years (Column (1)). Between entry and exit, there is a negative but insignificant effect on gross profit, but a very large increase in absolute debt as well as in the debt/EBITDA ratio. This is what we would expect given that substantial new leverage is being used to generate returns. We also see a strong negative effect on the total enterprise value, both in absolute terms and relative to EBITDA. This is consistent with the idea that leverage increase due to dividend recap is lowering firm value, as measured by the enterprise value.

Fund-Level Returns. We next turn to financial returns at the fund level using data from Burgiss. These outcomes are of paramount importance to the LPs who provide the equity for PE funds. Table 8 shows that dividend recaps have a negative effect on fund returns.[29] The effect on average IRR is insignificant (Panel A Column (1)), but there are large, significant negative effects on the cash-on-cash multiple (TVM) and the Public Market Equivalent (PME). TVM declines by 97% of the mean, and PME declines by 41% of the mean (Column (1) of Panels B and C, respectively). The larger decline for TVM than IRR is consistent with the dividend recap bringing cash flows forward in the fund life, since the IRR places larger weights on earlier cash flows while the TVM does not account for the time value of money. In the subsequent columns of each panel, we report the distributional results. Across all three panels, dividend recaps increase the chance of a relatively poor outcome (of 0-20% IRR, 1-2x multiple, and 1-2x PME from Column (3)). They reduce all other outcomes. For example, they reduce the chance of a 20-40% IRR (which comprises almost 30% of the sample) by 71 pp (Panel A Column (4)).

The negative effects on fund returns contrast with the positive effects at the deal level from Table 7. Two exercises show that this does not reflect different selection of deals into the samples matched to the fund-of-

[29]We do not observe deal-level cash flows in Burgiss nor fund-level cash flows in the fund-of-funds data, thus requiring separate datasets for the two analyses.

funds and to Burgiss. First, we find similar fund-level results in the subset of the Burgiss sample that also appears in the fund-of-funds sample (Table A5). Second, when we aggregate the return to deals within a fund in the fund-of-funds data, there are similar results as in the Burgiss data (Table A6). In other words, although the dividend recap if anything increases the deal-level return, it reduces the fund-level return. In Section 6 below, we provide evidence for a mechanism underlying these diverging impacts.

Creditor Returns The last stakeholders that we consider are the lenders to the portfolio company. As discussed above, dividend recap loans are typically securitized in CLOs. Cordell et al. (2023) show that CLOs in general perform well. Unfortunately, ex-post performance for loans acquired by CLOs is not available. However, we do not expect that dividend recaps will meaningfully impact performance because CLOs are highly diversified and, as we will show below, additional risk is at least to some degree incorporated via the spread. Instead, pre-existing creditors of the portfolio company may lose out.

If dividend recaps increase firm risk, they should be accompanied by higher interest rates relative to loans that are used to finance projects with positive net present value, where the project's future cash flows would reduce the risk stemming from higher indebtedness. We verify this conjecture by studying loans in our sample associated with LBOs and dividend recaps. The secondary market loan data are from Dealscan and LCD data. For each loan, we observe information on borrowers, lenders, and the PE sponsors, as well as contractual terms (spreads, covenants, etc.). We observe 24,202 loans, of which 11.7% (2,808) are for a dividend recap. Table A2 reports summary statistics about these loans, divided by DR status.

We estimate the following OLS specification, which is cross-sectional at the loan level:

$$\text{Loan Spread}_{l(p,b,t)} = \mathbb{1}(\text{Dividend Recap})_l + \alpha_p + \alpha_b + \alpha_t + \varepsilon_l. \tag{7}$$

Loan Spread$_{l(p,b,t)}$ is the spread on the loan l taken by PE firm p from bank b at time t. The spread is paid over the benchmark interest rate (LIBOR or SOFR) and is expressed in basis points. $\mathbb{1}(\text{Dividend Recap})_l$ equals one if loan l's purpose is a dividend recap, and equals zero otherwise. We include PE firm (α_p), bank (α_b), and year-month (α_t) fixed effects. The results are in Table 9 Panel A. Column (1) shows that the spread on dividend recap loans is 21 bps higher than that on other loans. This is a 5% difference relative to the average spread in our loan sample. In Column (2), we control for loan characteristics that may affect the spread, including size, maturity, and covenant-lite status, and find a similar relationship. This indicates that dividend recap loans are riskier and burden the firms with higher interest expenses relative to other types of loans, which in turn increases the chance of distress.

The large effect of dividend recaps on bankruptcy suggests that these deals could shift value away from pre-existing creditors. Pre-existing loan and bond covenants might restrict dividend recaps.[30] Observing a dividend recap with existing loans outstanding implies one or more of three things: First, the company

[30]Covenants are conditions on the borrower's activities during the life of the loan; for example, a debt service coverage ratio covenant requires the borrower to maintain funds to cover all debt payments. Covenants can also limit new debt issuance.

has sufficient cash flows to increase leverage without breaking covenants. Second, the new creditors may be junior to pre-existing ones. Third, the pre-existing debt may be renegotiated to have looser covenants. In practice, dividend recap loans that are sold to CLOs are senior secured. This suggests that the second possibility is unlikely. It also implies that in a bankruptcy these creditors are paid out first in a *pro rata* fashion along with the other senior secured creditors, such as those that financed the original LBO. Preexisting creditors, including bondholders, would likely lose out in a dividend recap-induced bankruptcy.

For a subsample of our LBO targets, we observe loan price data from secondary market trading. We apply these data to our main stacked analysis approach. The loan trading data can only be matched at the company level rather than the loan level. We identify pre-existing loans as those that originated before the stack's dividend recap date, and average across all such loans to the company to obtain price series for pre-existing loans. In this subsample, we observe 541 dividend recap deals with loan price data within a three-month window on either side of the transaction. Thus there are 541 stacks. Unfortunately, the instrument is too weak in this sample to obtain causal effects. However, we can examine the OLS relationship between dividend recaps and the change in price and liquidity measures.

The results are presented in Table 9 Panel B. Using windows of one and three months on either side of the dividend recap, there is a negative association between a dividend recap deal and the percent price change (Columns (1)-(2)). Specifically, within three months of the dividend recap the average firm experiences a 13 pp price decrease, significant at the 5% level. This is about 75% of the mean. In the remaining columns of Table 9 Panel B, we consider liquidity. There is a decline in both the bid-ask spread (Columns (3)-(4)) and the number of quotes (Columns (5)-(6)). These results suggest the causal effect would be larger, since dividend recap targets tend to be higher quality than the average PE-owned firm (see Section 1.3). In sum, even for the average dividend recap, there is value-shifting away from pre-existing creditors.

6 Mechanisms

Why do dividend recaps negatively affect firm outcomes? Why do GPs undertake them? And how can they increase deal returns yet reduce fund returns? In this section, we begin to answer these questions.

Taken together, our evidence suggests that dividend recaps lead GP incentives to diverge from the interests of current fund limited partner (LP) investors, portfolio company employees, and creditors. A leveraged payout delivers cash to the fund, incentivizing the GP to raise a new fund based on good interim returns. After raising a new fund, the GP—whose attention is limited—prioritizes the new fund at the expense of the current fund, ultimately leaving it with lower returns. Meanwhile, having realized good returns from the targeted portfolio company, the GP may take more risk in the investment because its payoff has become more call option-like. The portfolio company is also inherently riskier and weaker because of dividend recap, which creates additional debt not deployed within the firm, leading to higher chances of distress and poorer returns for pre-existing creditors. In the remainder of this section, we elaborate on each step in this moral hazard story through new analysis and support from the literature.

Paying out the Dividend Recap via Distributions. We first establish that GPs use dividend recaps to deliver cash returns to the fund. They could alternatively recycle it into new deals, which would not increase the fund's interim IRR. In Table 10 Columns (1)-(2), we show that the dividend recap has a large causal effect on distributions to the fund in the first quarter and year following the dividend recap quarter. Here and below, we use long differences to accommodate sparse outcomes that occur on either side of the transaction. The effect is 1.3 in the first quarter and 2.0 in the first year, relative to average changes in payout chances of 0.5 in these time frames.

Raising New Funds. One benefit to GPs of bringing cash flows forward in the fund's life is that it will improve follow-on fundraising. Interim returns are important because PE fundraising is cyclical, with the next fund typically raised midway through the previous fund. Harris et al. (2023) explain that GPs "tend to avoid fundraising when the interim performance of their current fund is weak." Chung et al. (2012) document the importance of current fund performance for future fundraising. They show that indirect pay for performance stemming from the current fund's impact on future fundraising affects the GP's lifetime total pay about the the same as the direct pay for performance of the current fund. Chakraborty and Ewens (2018) show that GPs delay revealing negative information about fund performance until they have raised the new fund, at which point they write off or reinvest in bad companies. Especially high interim returns can lead LPs to perceive the fund and its managers as higher quality than they truly are.

Motivated by this literature, we test whether dividend recaps enable new fund launches. In Columns (3)-(4) of Table 10, we report the causal effect on new funds launched by the PE firm in the first quarter and first year following the dividend recap transaction as compared to the same period before the deal. We find a statistically insignificant result at one quarter, consistent with GPs needing time to close a new fund. We find a large effect of about 12 times the mean in Column (4) for extra new funds launched in the subsequent year. These results suggest that bringing forward distributions via dividend recaps enables opportunistic fundraising. This is one reason, in addition to potentially accessing liquidity early themselves, that GPs are likely motivated to do as many dividend recaps as possible.

Declining Attention to the Current Fund. We next address why dividend recaps reduce fund returns. Our data suggest that by yielding early distributions, dividend recaps reduce GP attention and effort to the current fund, which is ultimately to its detriment. There are three pieces of evidence for this channel. First, we find in Table 10 Columns (5)-(6) that dividend recaps reduce returns for subsequent LBOs within the same fund. Here, the dependent variable is the average return of within-fund LBOs conducted after the dividend recap. Note that this specification continues to use the stacked model in which control firms have their LBOs at similar times as the dividend recap target, with the coefficient representing the causal effect of a dividend recap. This means that the result does not reflect deals which are later in the fund generally having lower returns, as in Brown et al. (2023).

Second, we show that funds with dividend recaps do fewer LBOs over the following two and four years. Table 10 Columns (7)-(8) show a large decline in the number of new deals relative to control funds. This is consistent with GPs paying less attention to these funds, and it is inconsistent with recycling returns into new deals. Third, the negative impacts of dividend recaps on target portfolio companies documented above is evidence of inattention or de-prioritization, especially given existing evidence that in general PE owners have expertise managing firms through distress (Hotchkiss et al., 2021). In sum, GPs appear to reduce attention to the current fund after a leveraged payout in the middle of the deal lifecycle.

More Risk for the Portfolio Company. After a leveraged payout, the portfolio company suffers both from lower priority in the eyes of fund managers and the ongoing costs of the new debt. Unlike conventional debt, the proceeds from a dividend recap loan are not deployed within the firm (and dividend recap loans have higher interest rates than other loans to PE-owned companies). The firm has a new obligation with no counteracting benefit. Furthermore, the investment has become more call option-like since earning some return on equity helps to cover the downside. Company management may be encouraged to take more risk, manifesting in the fatter-tailed distributions we observe in financial and real outcomes across Tables 4-7. Higher risk need not be bad for investors *per se*, but it is likely bad for employees and creditors. Exit is much more common than IPO and has strongly adverse outcomes for most employees. Employees of failed firms face frictions finding a new job and lose lifetime earnings (Berk et al., 2010; Graham et al., 2023; Gornall et al., 2024). This is compounded by the negative effect on wages among surviving firms.

7 Supplementary Tests

We described a number of robustness tests together with the main results in Sections 3-5. In Section 7.1, we present further tests. OLS results are in Section 7.2.

7.1 Robustness Tests

Our robustness tests focus on bankruptcy because it is the primary outcome and is estimated on the full sample.[31] First, we restrict the sample to PE funds with only a small number of portfolio companies that are at risk of experiencing a dividend recap. This serves to both limit the sample to smaller PE firms, ensuring that the largest firms do not drive our results, and explores whether selection within the portfolio seems to drive the estimates. Specifically, we consider the LBOs that PE firm conducted during the past seven years (nearly all dividend recaps occur within seven years of the LBO, as shown in Figure 3), and that are in the

[31] We have avoided doing more tests using the Census Bureau data as we are sharply limited in the samples (and implicit samples) that we may disclose. However, we can conduct more tests in a revision.

same industry as the dividend recap (since banks—even large ones—typically specialize in lending to certain industries (Blickle et al., 2023)). When the number of portfolio companies meeting these requirements is larger than one for a given dividend recap, we remove that company and its stack from our analysis. We limit the sample to PE firms with only one, two, three, or four portfolio companies in this category. The results are in Table 11 columns (1)-(4). We observe effects on bankruptcy that are consistent with our main findings, though the magnitude of the effect is larger as the number of at-risk portfolio companies declines. This is consistent with any selection bias pushing the effect on bankruptcy down.

Second, we show robustness to four alternative instruments (which were also reported in the first stage analysis in Table 3). As Table 11 columns (5)-(8) shows, they all yield similar results, indicating that the main finding does not spuriously reflect a particular approach to constructing relationship bank CLO activity. Last, we adjust the stacking algorithm to change the set of control firms in seven ways, which also changes the size of each stack and thus the overall sample size. The results are reported in Table A7. Columns (1) and (2) replaces the eight sectors with 40 industries and 200 sub-industries (these classifications are all from Pitchbook). Column (3) uses a three- rather than one-year window around the LBO. Columns (4)-(6) omit three types of variables from the matching process: Deal size (column (4)), PE firm assets under management (column (5)), and PE firm age (column (6)). Column (7) excludes add-on deals, where portfolio firms are added to a pre-existing PE owned vehicle. In all cases, the results are qualitatively similar to the main model, with dramatic positive effects on bankruptcy.

7.2 Descriptive Analysis

Targets of dividend recaps tend to be much larger and more profitable, and to experience increases in profits and returns on average (Tables 1 and 2). Therefore, we expect OLS estimates to be much more positive than the causal estimates. First, Table A8 shows that dividend recaps are associated with a negative but statistically insignificant change in the chance of bankruptcy over six years. Consistent with the same strong selection effect, we see that there is a negative OLS relationship between dividend recaps and subsequent exit. There is a positive relationship for IPOs (Columns (6)-(8)), and a positive but insignificant relationship for revenue growth (Panel B). Table A9 contains employee outcomes. There are positive associations between dividend recaps and employment and payroll growth (Column (1) of Panels A and B). These do not exhibit the same pattern of changes driven by the tails that we see for the causal estimate. For wages, there is a negative but small and insignificant effect (Panel C).

The OLS relationships for deal returns and financials are in Table A10. Again consistent with Table 1 Panel (B), dividend recaps are associated with higher average IRR and TVM. As we would expect given that cash is brought forward in time, the effect for IRR serves to reduce the chance of a very bad IRR outcome, but increases are driven by “good" deals with 20-40% returns (Panel A). For TVM, the positive relationship is driven by the tails of the distribution. Finally, we see that holding periods and debt relative to EBITDA increase substantially on average, while consistent with the null effect for revenue we do not see a change in gross profit. The OLS effect on fund-level returns is shows in Table A11. In contrast to the IV estimates, dividend recaps are associated with higher fund-level returns, both in terms of IRR, TVM, and PME. Last, we present the OLS results for distributions and subsequent fund launches in Table A12. As in the IV analysis, we observe a positive average effect on distributions (Columns (1)-(2)). In Columns (3)-(4), there is a smaller positive effect on launching new funds. We do not find any significant impact on returns of other deals within the fund, and instead find a positive impact on the number of new LBOs launched after a DR.

Together, these results suggest that PE funds select firms on more positive trajectories for dividend recaps. In this population, the new debt may increase the risk of distress, but it is much more muted. PE firms are not—as they are sometimes accused—using dividend recaps to drive firms towards failure. Instead, there is strong selection of good deals into dividend recaps, helping to explain why creditors are willing to lend for this purpose.

8 Conclusion

This paper offers to our knowledge the first effort to understand how new debt affects real and financial outcomes in a setting where it is possible to (a) isolate debt on the balance sheet; and (b) identify causal effects that control for the strong positive selection bias into new debt. Beyond the contribution to capital structure, we offer the first analysis of leveraged payouts in PE, which are deals in which an already PE-owned portfolio company takes on new debt or debt-like obligations (such as a lease after real estate is sold) and pays the proceeds of the debt to the PE fund as returns to equity. The media has vilified leveraged payouts as an extreme form of asset-stripping, representing the “worst" of an extractive sector (Bogoslaw, 2008; Fitzgerald, 2010; Lim and Weiss, 2024). Yet it is not obvious that these deals will generally have negative effects. First, if they typically cause distress, creditors would be unlikely to offer loans (or sale-leasebacks in the case of real estate) for this purpose. Second, if PE ownership brings better management and value creation, dividend recaps might enable longer holding periods, which could benefit the firm.

We document that consistent with positive selection into debt—which has been shown in the broader economy (Titman et al., 2005)—PE firms tend to target large, healthy portfolio companies for dividend recaps. The deals do not in general lead to bad outcomes. To address the selection challenge, we instrument for dividend recaps using CLO volume underwritten by PE firms' relationship banks. This empirical design allows us to also shed light on an indirect effect of the burgeoning CLO industry.

After accounting for selection, we show that cheap credit-induced dividend recaps increase firm risk. The large effect on the chance of bankruptcy is in line with what Altman Z-score theory predicts. At the same time, there is also some evidence of positive effects, such as on IPOs. For employees, the effects appear largely negative even among survivor firms, driven by realizations of large contractions. Wage growth among survivor firms falls by over 50%. Higher risk in real outcomes is paralleled on the investor side by wider dispersion in deal returns. Although dividend recaps increase deal returns, they reduce fund returns.

Managers seem to make use of higher interim returns to raise new funds, focusing less on the current fund. Finally, dividend recaps reduce preexisting loan prices.

While more debt can benefit firms by disciplining managers and offering tax benefits (Jensen, 1986; Cohn et al., 2014), our results overall suggest that dividend recaps create misaligned incentives and moral hazard problems for GPs, leading to activities that diverge from the interests of fund investors, company employees, and pre-existing creditors. Dividend recaps increase the firm's risk firstly because the new debt—with no compensating cash infusion—inherently raises the chance of distress. Indeed, dividend recap loans have higher interest rates than other leveraged loans, adding to the debt service burden. Second, a return realization makes the deal's payoff more call option-like, which may lead GPs to encourage executives to take more risks, creating the fatter-tailed outcome distribution we observe. While higher risk need not be bad for investors *per se*, it is likely bad for employees and creditors (or any risk-averse stakeholder).[32] Furthermore, while DRs accelerate deal returns to improve IRRs and assist GPs in launching new funds, they negatively impact overall fund returns. The increased risk appears to benefit GPs while reducing value for LP investors.

[32]Exits and bankruptcies are much more common than IPOs, with detrimental consequences for employees, who face job search frictions and lose lifetime earnings (Berk et al., 2010; Graham et al., 2023). This is compounded by the negative effect on wages among surviving firms.

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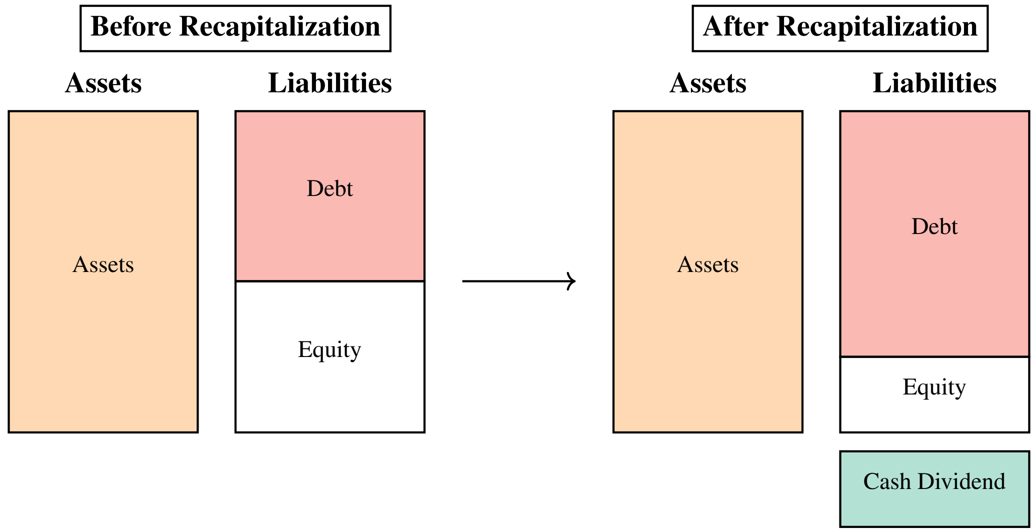
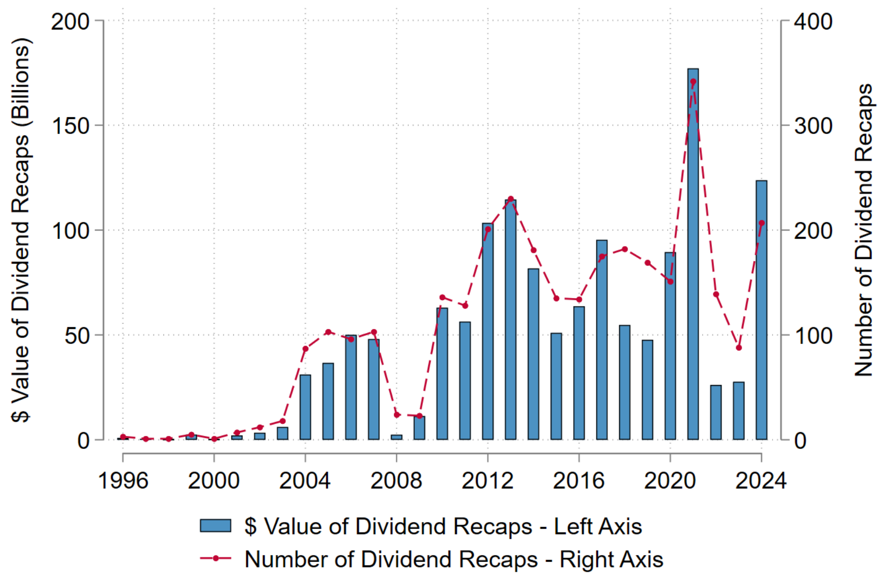
Figure 1: **Dividend Recapitalization in the Capital Structure**



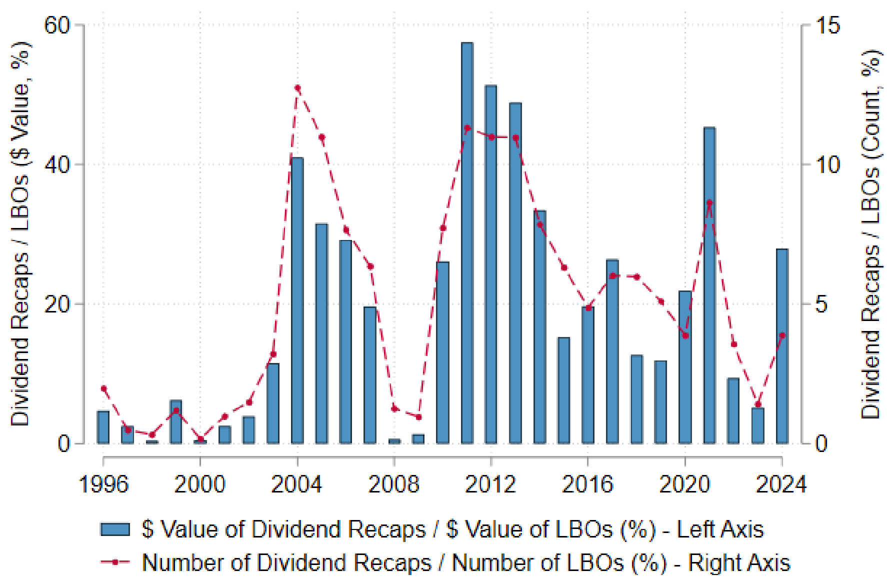
Notes: This figure describes how a dividend recap loan affects the balance sheet of the company. For a typical dividend recap in our data, the ratio of book equity to book debt before the recapitalization is 0.96. After the recapitalization, the ratio is 0.07, with the additional debt paid off as cash dividend to the shareholders.

Figure 2: **Dividend Recaps Over Time**

Panel (A): Number of Dividend Recaps by Year

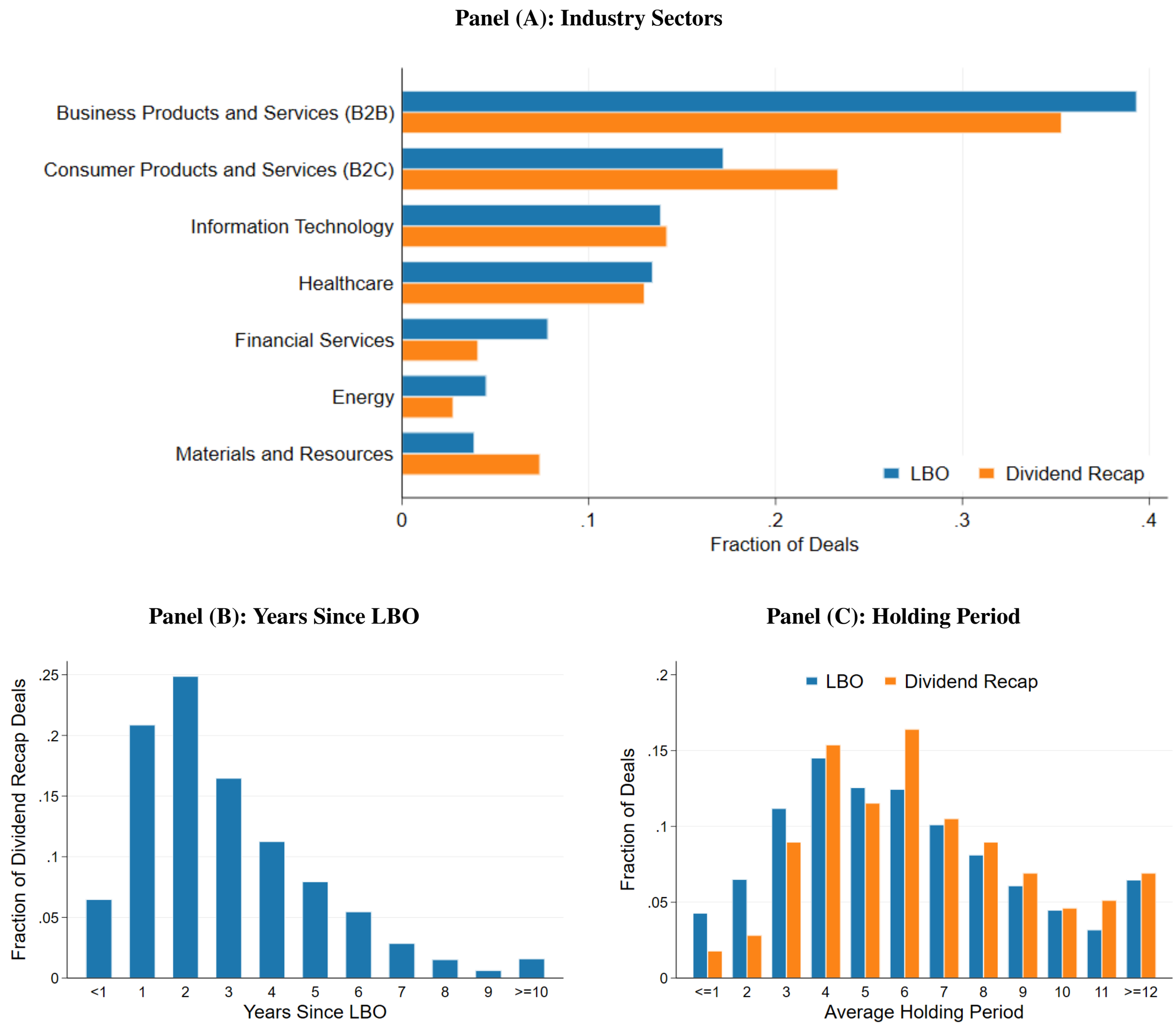


Panel (B): Dividend Recaps as Fraction of LBOs$_{t-2}$



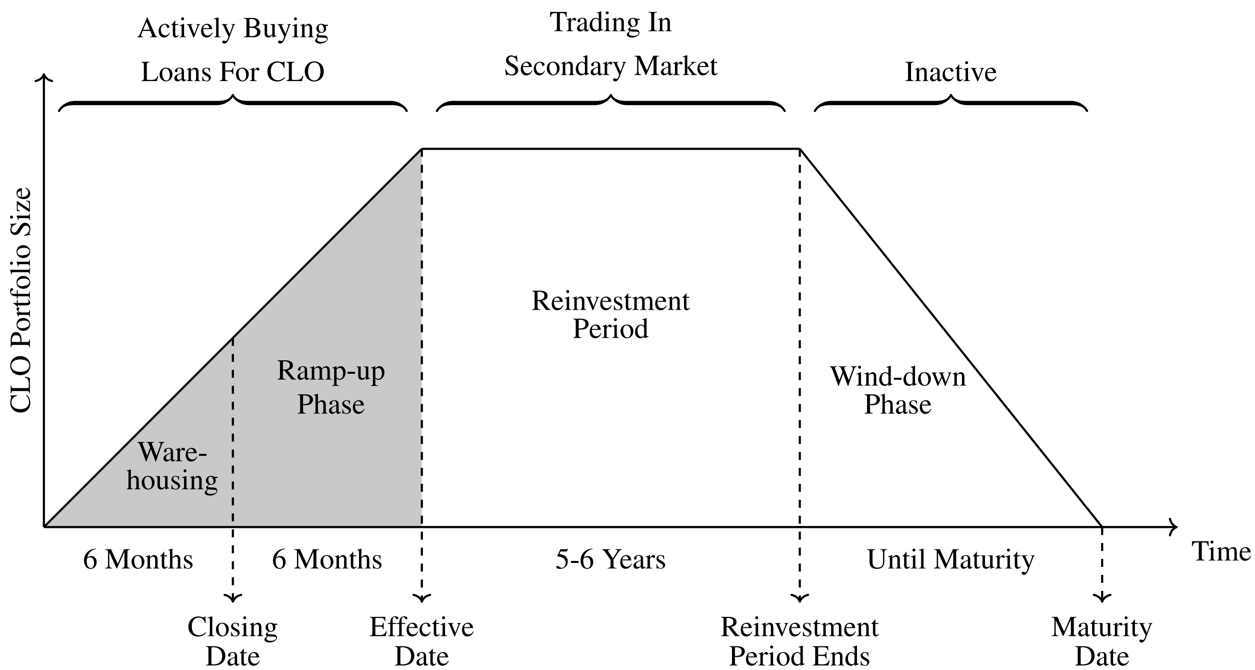
Notes: This figure shows dividend recap trends over time. Panel (A) contains the number and dollar value of dividend recap deals by year. Panel (B) scales this by the number and dollar value of LBO deals executed two years ago, respectively (we use this normalization because most dividend recap loans are taken two years after the LBO). The dollar value figures are adjusted for inflation using the GDP deflator with 2023 as the base year and are calculated using the loan size variable in LCD and the deal size variable in Pitchbook. Hence, these numbers are conditional on the deals with non-missing values for loan size and deal size in the respective datasets.

Figure 3: **Cross-sectional Differences in Dividend Recaps**



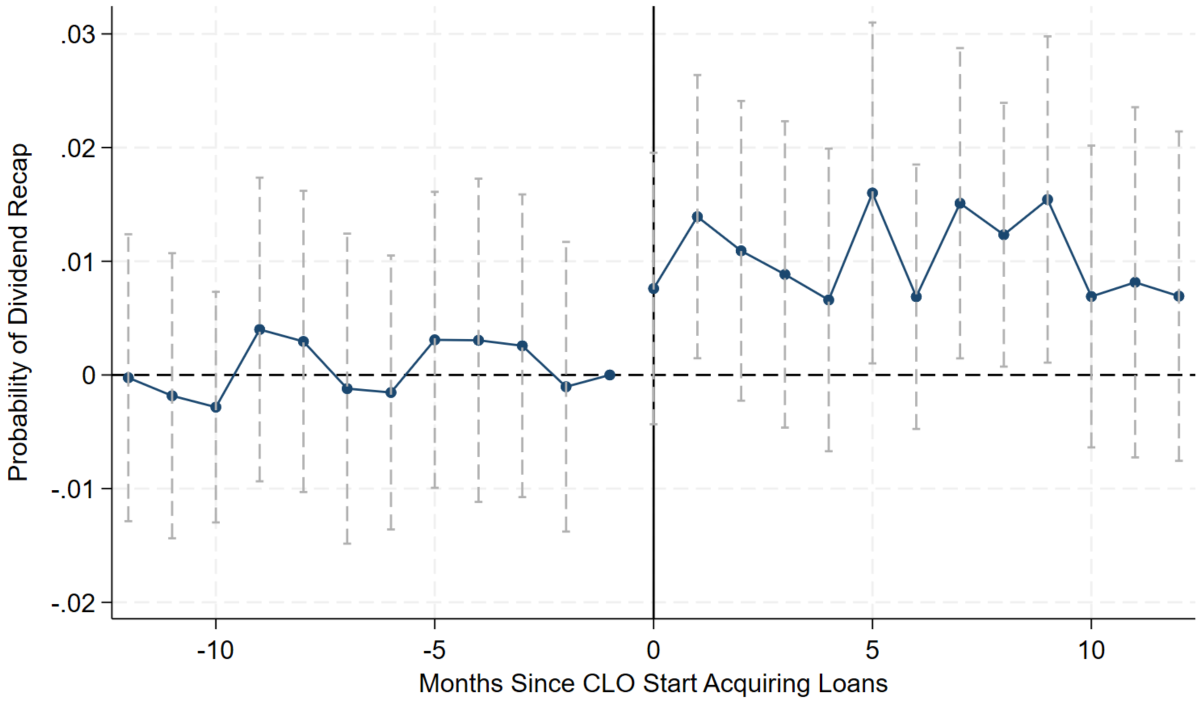
Notes: This figure compares the cross-sectional distribution of all LBO deals against those that are followed by dividend recaps. Panel A shows the distribution across the broad industry sectors. Panel B shows the distribution of duration between dividend recap loan and corresponding LBO deal date in years. Panel C shows the distribution of holding period (in years) for all LBO deals and deals with dividend recap.

Figure 4: **Life Cycle Of A Collateralized Loan Obligation (CLO)**



Notes: This figure describes the life cycle of a typical CLO. The X-axis plots the time from the CLO's start of operation. The Y-axis plots the size of its portfolio over time. CLOs actively purchase loans to fill their portfolio six months before the closing date through the effective date. The CLO then enters the reinvestment stage in which it trades in the secondary loan market. After the reinvestment phase, the CLO mechanically winds down and repays the investors as the portfolio loans mature over time. We classify all CLOs actively buying loans (shaded region) as "active CLOs". We use the total volume of active CLOs underwritten by each PE firm's relationship banks to proxy for dividend recap loan demand.

Figure 5: **Effect of CLO Underwriting on DR issuance: Event Study**



Notes: This figure shows the relationship between PE firms' access to CLO funding and the chances of a dividend recap using a stacked event study design. We plot the regression coefficients estimated in a stacked event study model (Equation 4). The dependent variable is an indicator that is equal to one if PE firm p sponsored a dividend recap loan in month t, and zero otherwise. The independent variables are a set of time dummies with respect to the stack date t_0. Treated firms are the set of PE firms which experienced more than 25% month-over-month increase in the instrument at t_0. The control group is all PE firms that did not experience any such increase during the 24-month window around the stack date. We include stack–PE firm and stack–year-month fixed effects. Standard errors are two-way clustered at the PE firm and year-month level.

Table 1: **Summary Statistics about Outcome Variables**

	All				DR		Non-DR	
	N	Mean	Median	SD	N	Mean	N	Mean
Panel (A): Portfolio Company Outcomes								
Bankruptcy (6-Yr) (%)	53,539	1.31			935	2.14	52,604	1.29
IPO (6-Yr) (%)	53,539	0.98			935	2.99	52,604	0.94
Conditional on Census Bureau Match								
Exit (4-Yr) (%)	24,500	15.90			500	9.8	23,500	16
Exit (6-Yr) (%)	24,500	18.50			500	16	23,500	18.5
Conditional on Census Bureau Match & Survival								
Employment$_{t-1}$	7,700	1,313	110	5,109	300	2,253	7,400	1,273
Employment$_{t+3}$	7,700	1,761	243	6,182	300	3,449	7,400	1,688
Payroll$_{t-1}$ ($, Millions)	7,700	45	7	127	300	91	7,400	43
Payroll$_{t+3}$ ($, Millions)	7,700	52	14	143	300	121	7,400	49
Wage$_{t-1}$ ($, Thousands)	7,700	63	53	31	300	64	7,400	63
Wage$_{t+3}$ ($, Thousands)	7,700	57	56	31	300	58	7,400	57
Revenue$_{t-1}$ ($, Millions)	3,600	392	21	1,637	150	784	3,500	377
Revenue$_{t+3}$ ($, Millions)	3,600	764	158	2,323	150	1,152	3,500	749
ΔEmployment$_{t-1,t+3}$	7,700	0.18	0.12	0.54	300	0.30	7400	0.17
ΔPayroll$_{t-1,t+3}$	7,700	0.13	0.11	0.57	300	0.24	7400	0.13
ΔWage$_{t-1,t+3}$	7,700	-0.04	-0.03	0.38	300	-0.04	7400	-0.04
ΔRevenue$_{t-1,t+3}$	3,600	0.39	0.55	0.66	150	0.32	3500	0.39
Panel (B): Deal and Fund Outcomes								
Deal Outcomes: Conditional on Fund-of-funds Match								
Gross IRR (%)	29,321	25.79	22.52	47.09	284	33.34	29,037	25.72
Gross TVM	30,738	2.72	2.2	2.45	288	3.6	30,450	2.71
Holding Period (Years)	16,844	5.75	5	2.97	163	7.27	16,681	5.73
Δ Gross Profit (%)	11,977	-0.17	0.57	12.26	129	1.05	11,848	-0.18
Δ Debt/Ebitda	11,324	-0.32	-0.62	5.98	129	0.67	11,195	-0.34
Δ Debt/TEV	10,693	-0.11	-0.15	0.28	123	-0.09	10,570	-0.11
Δ Log(Debt) (%)	8,841	30.42	23.37	85.02	116	60.09	8,725	30.02
Δ Log(TEV) (%)	11,155	0.81	0.83	0.82	126	0.89	11,029	0.8
Δ TEV/Ebitda	10,842	2.18	2.21	15.37	126	2.7	10,716	2.18
Fund Outcomes: Conditional on Burgiss Match								
Total Value Multiple	17,599	1.95	1.81	0.81	620	1.94	16,979	1.95
Public market Equivalent	17,599	1.26	1.19	0.46	620	1.29	16,979	1.25
IRR (%)	17,598	16.85	15.38	13.54	620	17.97	16,978	16.81

Notes: This table contains summary statistics about key outcomes in our analysis for the portfolio company (Panel (A)) and for the deal and its fund (Panel (B)). The dataset is stacked, with each stack containing a treated deal (i.e., DR deal) and a set of control deals (i.e., matched non-DR deals). Outcomes are measured relative to the DR date corresponding to the stack. Census sample sizes are rounded.

Table 2: **Summary Statistics about LBO Deal and Primary Market Loans**

	All				DR		Non-DR	
	N	Mean	Median	SD	N	Mean	N	Mean
Panel (A): Deal and Fund Characteristics								
Deal Characteristics								
Deal Size ($, Millions)	5,168	325.08	103.12	597.02	413	675.52	4,755	294.64
Conditional on Fund-of-funds Match								
TEV ($, Millions), Entry	13,038	464.84	120	1,779.36	133	500.99	12,905	464.46
TEV ($, Millions), Exit	11,812	752.22	325	1,282.88	127	1,056.85	11,685	748.9
Debt, Entry	12,277	162.24	45	351.11	129	256.29	12,148	161.25
Debt, Exit	11,163	233.07	71.98	430.58	121	406.08	11,042	231.18
Debt/Ebitda, Entry	12,117	3.45	3.73	2.73	134	3.87	11,983	3.45
Debt/Ebitda, Exit	11,739	3.16	2.89	4.22	129	4.54	11,610	3.15
Debt/TEV (%), Entry	12,259	41.23	47.9	26.75	130	52.37	12,129	41.11
Debt/TEV (%), Exit	11,062	29.59	26.67	24.77	123	42.68	10,939	29.44
Gross Profit (%), Entry	12,617	18.15	17.3	16.78	131	21.57	12,486	18.11
Gross Profit (%), Exit	12,322	18.01	17	15.63	130	22.69	12,192	17.96
TEV/Ebitda, Entry	12,213	8.19	7.61	8.72	133	7.81	12,080	8.19
TEV/Ebitda, Exit	11,391	10.85	10.13	16.83	128	10.51	11,263	10.86
Conditional on Burgiss Match								
Fund Size ($, Billions)	17,599	1.44	0.73	2.19	620	2.45	16,979	1.41
Panel (B): Loan Characteristics (Conditional on LCD Match)								
	All				DR Loan		Non-DR Loan	
	N	Mean	Median	SD	N	Mean	N	Mean
Loan Amount ($, Millions)	29,107	216.01	93.8	334.6	3,202	214.02	25,905	216.25
Loan Spread (bps)	26,704	403.59	375	151.75	2,914	440.84	23,790	399.03
Maturity (Years)	28,991	5.44	5.01	1.3	3,196	5.6	25,795	5.42
Cov-Lite Indicator	29,588	0.16			3,228	0.21	26,360	0.16

Notes: This table contains summary statistics about the LBO deals, PE funds, and leveraged loans in our sample. Panel A contains data about the LBO and fund size, divided by whether the LBO was followed by a dividend recap or not. Panel B contains data at the individual loan level. Here, “DR Loan" corresponds to loans for the purpose of dividend recaps.

Table 3: **First Stage Analysis**

Panel (A): Effect of PE-Bank Relationships

	$\mathbb{1}$(DR Purchased by CLO)	
	(1)	(2)
PE-Bank Relation	0.011***	0.011***
	(0.001)	(0.002)
PE FE	Y	Y
CLO FE	Y	
CLO $\times$ Year FE		Y
CLO $\times$ Industry FE		Y
Obs	393513	393513
Y-Mean	.047	.047

Panel (B): First Stage Results

	$\mathbb{1}$(Dividend Recap)				
	(1)	(2)	(3)	(4)	(5)
R-Banks CLO Volume	0.04***				
	(0.01)				
R-Banks CLO Volume (1-Yr)		0.03***			
		(0.00)			
R-Banks CLO Volume (5-Yr)			0.02***		
			(0.00)		
R-Banks CLO Count				0.12***	
				(0.02)	
R-Banks CLO Underwriting					0.43***
					(0.06)
Stack FE	Y	Y	Y	Y	Y
Obs	53539	53539	53539	53539	53539
Y-Mean	.02	.02	.02	.02	.02

Notes: This table shows how PE-Bank relationships affect DR purchase by CLOs and new DR issuance. Panel A estimates Equation 1. The dependent variable is an indicator variable that equals one if CLO k (underwritten by bank b in year t) purchased a DR loan d sponsored by a PE firm p, and zero otherwise. The main explanatory variable is indicator which is one if p has a lending relationship with bank b in year $t-1$, and zero otherwise. We include PE and CLO fixed effects and cluster standard errors at the CLO level. Panel (B) shows the relationship between CLO underwriting activity of PEs related banks and their likelihood of doing a dividend recap, using Equation 3. Here, we use the stacked dataset, where each stack has a treated deal (i.e., deal with dividend recap) and a set of control deals matched on ex-ante characteristics. Column (1) shows the results with our main measure (R-Banks CLO Volume) and columns (2) to (5) shows the corresponding results with alternative measures of CLO activity. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4: **IV Effect of Dividend Recaps on Bankruptcy and Exit**

Panel (A): Effect of Dividend Recap Indicator

	Bankruptcy			Exit	
	4-Year	6-Year	10-Year	4-Year	6-Year
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	14.99**	30.90***	40.32***	46.79***	33.49*
	(5.92)	(8.14)	(10.04)	(17.85)	(18.6)
Stack FE	Y	Y	Y	Y	Y
Obs	53539	53539	53539	24500	24500
Y-Mean	0.91	1.31	1.79	15.9	18.5
F-Stat	69.62	69.62	69.62	45.16	45.16

Panel (B): Leverage Increase From Dividend Recap

	Bankruptcy			
	(1)	(2)	(3)	(4)
Log(1+DR Size)	18.792***			
	(3.953)			
DR Size		0.257***		
		(0.059)		
Log(1+DR Size/Deal Size)			502.397***	
			(143.722)	
DR Size/Deal Size				349.570**
				(142.389)
Stack FE	Y	Y	Y	Y
Obs	50516	50516	50166	50166
Y-Mean	1.40	1.68	1.68	1.68
F-Stat	81.36	46.81	19.67	7.44

Notes: This table shows the 2SLS effect of dividend recaps on the probability of bankruptcy and firm exit using Equation 5. In the first stage, the endogenous variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The instrument is R-Banks CLO Volume$_{p,t-1}$, which is defined as the average outstanding volume of CLOs underwritten by the PE firm p's relationship banks in the month $t-1$. In panel (A) columns (1), (2), and (3), the outcome variables are bankruptcy over a 4-year, 6-year, and 10-year horizon, respectively. In panel (A) columns (4) and (5), the Census sample is employed and the outcomes are company exit at 4-year and 6-year horizons. As the Census panel is shorter, ending in 2021, we do not have enough time to estimate 10-year outcomes. In panel (B), we focus on change in debt induced by DR loan. The outcome variable is the indicator for bankruptcy over a 6-year horizon. In columns (1) and (2), we use the size of DR loan as the endogenous variable, expressed in Log(1+.) and in $ Millions, respectively. In columns (3) and (4), we scale the size of the DR loan by the size of the LBO deal. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5: **IV Effect of Dividend Recaps on IPO and Revenue Growth**

Panel (A): IPO

	4-Year	6-Year	10-Year
	(1)	(2)	(3)
$\mathbb{1}$(Dividend Recap)	31.76***	38.46***	43.74***
	(6.86)	(7.74)	(8.21)
Stack FE	Y	Y	Y
Obs	53539	53539	53539
Y-Mean	0.82	0.98	1.09
F-Stat	69.62	69.62	69.62

Panel (B): Revenue Growth (4-year horizon)

	Average	$\mathbb{1}$[<-75%]	$\mathbb{1}$[-75,0%]	$\mathbb{1}$[0,75%]	$\mathbb{1}$[>75%]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	.709	-.024	-.158	-.705	0.886*
	(0.637)	(0.233)	(0.383)	(0.436)	(0.511)
Stack FE	Y	Y	Y	Y	Y
Obs	3600	3600	3600	3600	3600
Y-Mean	0.387	0.0746	0.246	0.212	0.467
F-Stat	11.21	11.21	11.21	11.21	11.21

Notes: This table shows the 2SLS effect of dividend recaps on the probability of IPO and revenue growth using Equation 5. In the first stage, the endogenous variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The instrument is R-Banks CLO Volume$_{p,t-1}$, which is defined as the average outstanding volume of CLOs underwritten by the PE firm p's relationship banks in the month $t-1$. In Panel (A), the outcome variable $y_{s,c}$ is the probability of an IPO in the next 4-, 6-, and 10-year period. In Panel (B) Column (1), the outcome variable is the revenue growth over a 4-year horizon around the dividend recap year, measured as the percent change between the 3rd year after the dividend recap and the year before the dividend recap. Only survivor firms with revenue populated across all four years are included. The dependent variables in Panel (B) columns 2-5 are indicators for growth falling into a particular bin. For example, in column 2 the dependent variable is one if revenue shrank such that growth was less than -75%. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6: **IV Effect of Dividend Recaps on Employees among Survivor Firms**

Panel (A): Employment Growth (4-Year horizon)

	Average	$\mathbb{1}$[<-75%]	$\mathbb{1}$[-75,0%]	$\mathbb{1}$[0,75%]	$\mathbb{1}$[>75%]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	-.2916	.1957*	.1194	-.5175*	.2024
	(.3072)	(.114)	(.2693)	(.2843)	(.2419)
Stack FE	Y	Y	Y	Y	Y
Obs	7700	7700	7700	7700	7700
Y-Mean	.1801	0.045	0.337	0.403	0.216
F-Stat	25.92	25.92	25.92	25.92	25.92

Panel (B): Payroll Growth (4-Year horizon)

	Average	$\mathbb{1}$[<-75%]	$\mathbb{1}$[-75,0%]	$\mathbb{1}$[0,75%]	$\mathbb{1}$[>75%]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	-.4669	.4083**	-.1785	-.1703	-.05948
	(.3314)	(.1664)	(.2637)	(.2589)	(.2338)
Stack FE	Y	Y	Y	Y	Y
Obs	7700	7700	7700	7700	7700
Y-Mean	.1309	0.0646	0.38	0.351	0.205
F-Stat	25.92	25.92	25.92	25.92	25.92

Panel (C): Wage Growth (4-Year horizon)

	Average	$\mathbb{1}$[<-75%]	$\mathbb{1}$[-75,0%]	$\mathbb{1}$[0,75%]	$\mathbb{1}$[>75%]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	-.5339**	.1711*	.154	-.1978	-.1274
	(.2656)	(.08771)	(.2911)	(.293)	(.1092)
Stack FE	Y	Y	Y	Y	Y
Obs	7700	7700	7700	7700	7700
Y-Mean	-.03893	0.0369	0.508	0.42	0.035
F-Stat	25.92	25.92	25.92	25.92	25.92

Notes: This table shows the 2SLS effect of dividend recaps on employment, payroll, and wage growth using Equation 5. In the first stage, the endogenous variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The instrument is R-Banks CLO Volume$_{p,t-1}$, which is defined as the average outstanding volume of CLOs underwritten by the PE firm p's relationship banks in the month $t-1$. The outcome variables in Panels (A), (B), and (C) are employment growth, payroll growth, and wage growth over a 4-year horizon around the dividend recap year, measured as the percent change between the 3rd year after the dividend recap and the year before the dividend recap. Only survivor firms with employment and payroll populated across all four years are included. In each panel, the dependent variable in Column (1) is the average outcome. The dependent variables in columns (2)-(5) are indicators for growth falling into a particular bin. For example, in Panel (A) column 2 the dependent variable is one if employment shrank such that growth was less than -75%. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7: **IV Effect of Dividend Recaps on Deal Returns**

Panel (A): Deal IRR

	Average	𝟙[<0%]	𝟙[0,20%]	𝟙[20,40%]	𝟙[>=40%]
	(1)	(2)	(3)	(4)	(5)
𝟙(Dividend Recap)	99.99*	0.98*	-3.21***	1.13*	1.10**
	(59.74)	(0.57)	(0.94)	(0.66)	(0.53)
Stack FE	Y	Y	Y	Y	Y
Obs	29144	29144	29144	29144	29144
Y-Mean	25.35	0.16	0.30	0.26	0.28
F-Stat	25.33	25.33	25.33	25.33	25.33

Panel (B): Deal TVM

	Average	𝟙[<1x]	𝟙[1,2x]	𝟙[2,4x]	𝟙[>=4x]
	(1)	(2)	(3)	(4)	(5)
𝟙(Dividend Recap)	1.44	0.84	-5.63***	4.56***	0.24
	(2.79)	(0.57)	(1.28)	(1.09)	(0.48)
Stack FE	Y	Y	Y	Y	Y
Obs	29144	29144	29144	29144	29144
Y-Mean	2.78	0.17	0.27	0.36	0.21
F-Stat	25.33	25.33	25.33	25.33	25.33

Panel (C): Deal Financials

	Holding Period	Δ Gross Profit	Δ Debt/Ebitda	Δ Log(Debt)	Δ Log(TEV)	Δ TEV/EBITDA
	(1)	(2)	(3)	(4)	(5)	(6)
𝟙(Dividend Recap)	12.77**	-0.11	17.44**	2.97**	-12.18***	-34.59*
	(6.25)	(0.21)	(7.66)	(1.48)	(3.28)	(19.14)
Stack FE	Y	Y	Y	Y	Y	Y
Obs	16842	11975	11017	8659	11152	10838
Y-Mean	5.75	-0.00	-0.29	0.30	0.80	2.13
F-Stat	15.47	17.91	17.30	11.62	16.67	17.25

Notes: This table shows the 2SLS effect of dividend recaps on deal-level returns and deal financials using Equation 5. In the first stage, the endogenous variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The instrument is R-Banks CLO Volume$_{p,t-1}$, which is defined as the average outstanding volume of CLOs underwritten by the PE firm p's relationship banks in the month $t-1$. In Panels (A) and (B), the outcome variables are the Internal Rate of Return (IRR) and Total Value Multiple (TVM) for deal d. In both these panels, we first show the effect on the average outcome of the deal and then show the probability of the deal falling within four bins corresponding to each of the three outcome variables. In Panel (C), we use the change in several financial characteristics from the time the PE firm entered the deal to the time of them exiting the deal. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8: **IV Effect of Dividend Recaps on Fund Returns**

Panel (A): Fund IRR

	Average (1)	$\mathbb{1}$[<0%] (2)	$\mathbb{1}$[0,20%] (3)	$\mathbb{1}$[20,40%] (4)	$\mathbb{1}$[>40%] (5)
$\mathbb{1}$(Dividend Recap)	-7.64	-0.74***	1.89***	-0.71***	-0.44***
	(6.38)	(0.17)	(0.42)	(0.25)	(0.15)
Stack FE	Y	Y	Y	Y	Y
Obs	12285	12477	12477	12477	12477
Y-Mean	17.29	0.04	0.60	0.31	0.05
F-Stat	30.52	30.77	30.77	30.77	30.77

Panel (B): Fund TVM

	Average (1)	$\mathbb{1}$[<1x] (2)	$\mathbb{1}$[1,2x] (3)	$\mathbb{1}$[2,4x] (4)	$\mathbb{1}$[>4x] (5)
$\mathbb{1}$(Dividend Recap)	-1.89***	-0.75***	2.23***	-1.00***	-0.49***
	(0.56)	(0.17)	(0.48)	(0.30)	(0.15)
Stack FE	Y	Y	Y	Y	Y
Obs	12286	12477	12477	12477	12477
Y-Mean	1.95	0.04	0.55	0.38	0.03
F-Stat	30.52	30.77	30.77	30.77	30.77

Panel (C): Fund PME

	Average (1)	$\mathbb{1}$[<1x] (2)	$\mathbb{1}$[1,2x] (3)	$\mathbb{1}$[2,4x] (4)	$\mathbb{1}$[>4x] (5)
$\mathbb{1}$(Dividend Recap)	-0.52**	-0.14	0.67**	-0.22*	-0.31***
	(0.25)	(0.23)	(0.28)	(0.12)	(0.11)
Stack FE	Y	Y	Y	Y	Y
Obs	12286	12477	12477	12477	12477
Y-Mean	1.26	0.28	0.65	0.05	0.02
F-Stat	30.52	30.77	30.77	30.77	30.77

Notes: This table shows the 2SLS effect of dividend recaps on fund-level returns using Equation 5. In the first stage, the endogenous variable is a binary variable that is one if the fund f featured a dividend recap transaction, and zero otherwise. The instrument is R-Banks CLO Volume$_{p,t-1}$, which is defined as the average outstanding volume of CLOs underwritten by the PE firm p's relationship banks in the month $t-1$. In Panels (A), (B), and (C) the outcome variables are the Internal Rate of Return (IRR), Total Value Multiple (TVM), and PME Market Equivalent (PME) for fund f. In these panels, we first show the effect on the average outcome of the deal and then show the probability of the deal falling within four bins corresponding to each of the three outcome variables. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9: **OLS Relationships between Dividend Recaps and Credit Outcomes**

Panel (A): Loan Spread in Primary Market

	Loan Spread (bps)	
	(1)	(2)
𝟙(Dividend Recap)	20.78***	19.75***
	(3.59)	(3.41)
Loan Size		-23.93***
		(1.40)
Maturity		25.08***
		(1.59)
Cov-Lite Indicator		21.24***
		(4.14)
PE FE	Y	Y
Bank FE	Y	Y
Year-Month FE	Y	Y
Obs	24202	24202
Y-Mean	414.21	414.21

Panel (B): Pre-existing Loan Outcomes

	ΔPrice		ΔBid-Ask		Δ # Quotes	
	[-1,+1]	[-3,+3]	[-1,+1]	[-3,+3]	[-1,+1]	[-3,+3]
	(1)	(2)	(3)	(4)	(5)	(6)
𝟙(Dividend Recap)	-0.06*	-0.13**	-0.02*	-0.03**	-2.34**	-5.76***
	(0.03)	(0.05)	(0.01)	(0.02)	(0.96)	(1.28)
Stack FE	Y	Y	Y	Y	Y	Y
Obs	4541	4547	4541	4547	4713	4724
Y-Mean	0.06	0.17	-0.02	-0.04	-0.66	-1.61

Notes: This table uses OLS models to describe the relationship between dividend recaps and credit-related outcomes. In Panel (A), the outcome variable is the spread on the loan in basis points (bps). The independent variable of interest is an indicator variable that equals one if the loan purpose is specified as dividend recap, and zero otherwise. We employ PE, bank, and year-month fixed effects. Standard errors are clustered at the PE level. In Panel (B), we describe the relationship between dividend recaps and secondary market outcomes of the portfolio company's pre-existing loans. We examine change in loan price (Columns (1) and (2)), bid-ask spreads (Columns (3) and (4), and number of quotes (Columns (5) and (6)). The sample is somewhat larger for number of quotes because this outcome can be zero. We examine such changes 1 month and 3 months before and after the Dividend Recap transaction. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 10: **IV Effect of Dividend Recaps on Distributions, Fund Launch, Peer Deal Returns, and new LBOs**

	Δ Distribution Transactions		Δ New Funds Launched		Within-Fund Peer Deal IRR		Δ New LBOs	
	1-Quarter	1-Year	1-Quarter	1-Year	Pre-DR	Post-DR	2-Year	4-Year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(Dividend Recap)	1.3634**	2.0276**	0.307	0.743**	3.28	-91.82**	-3.82	-36.70***
	(0.5675)	(0.9048)	(0.210)	(0.356)	(41.24)	(44.36)	(3.96)	(10.11)
Stack FE	Y	Y	Y	Y	Y	Y	Y	Y
Obs	25,397	25,397	75,923	75,923	28,715	2004	75,923	75,923
Y-Mean	0.54	0.55	0.055	0.062	26.11	27.51	-1.03	-6.46
F-Stat	32.5	32.5	64.7	64.7	26.6	16.65	65	65

Notes: This table shows the 2SLS effect of dividend recaps on several fund and deal level outcomes using Equation 5. In the first stage, the endogenous variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The instrument is R-Banks CLO Volume$_{p,t-1}$, which is defined as the average outstanding volume of CLOs underwritten by the PE firm p's relationship banks in the month $t-1$. The dependent variable is the change in the count of distribution transactions by PE firm p in 1 and 4 quarters after time t compared to 1 and 2 quarters before time t for columns (1)-(2), the change in the count of new funds launched by PE firm p in 1 and 4 quarters after time t compared to 1 and 2 quarters before time t for columns (3)-(4), the average return of peer deals, i.e., other deals in the fund with the DR (or control) deals before and after time t in columns (5)-(6), and the change in the count of new LBOs launched by PE firm p in 2 and 4 years after time t compared to 1 and 2 quarters before time t for columns (7)-(8). We control for stack fixed effect. Standard errors are clustered at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 11: **Robustness Tests of Dividend Recap IV Effect on Bankruptcy: Number of At-Risk Deals and Alternative Instruments**

	Number of At-Risk Deals				Alternative Instruments: R-Banks CLO			
	One	Two	Three	Four	Vol (1-Yr)	Vol (5-Yr)	Count	Underwriting
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(Dividend Recap)	86.11*	61.52**	37.54**	30.48***	39.79***	41.43***	21.78***	26.03***
	(50.81)	(24.45)	(14.63)	(10.99)	(9.95)	(11.46)	(7.47)	(8.72)
Stack FE	Y	Y	Y	Y	Y	Y	Y	Y
Obs	4941	9988	14829	18677	53539	53539	53539	53539
Y-Mean	2.59	2.17	1.89	1.69	1.31	1.31	1.31	1.31
F-Stat	4.41	13.45	24.36	35.76	56.79	40.15	48.83	43.85

Notes: This table shows robustness tests of the 2SLS effect of dividend recaps on the probability of bankruptcy using Equation 5. The outcome variable is bankruptcy over a 6-year horizon. In the first stage, the endogenous variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The instrument is R-Banks CLO Volume$_{p,t-1}$, which is defined as the average outstanding volume of CLOs underwritten by the PE firm p's relationship banks in the month $t-1$. In Columns (1) to (4), we re-estimate our results by only considering stacks where the PE firm associated with the treated deal only had one to four at-risk deals in their portfolio. In Columns (5) to (8), we show our results using an alternative set of instruments in the first stage. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

ONLINE APPENDICES

Appendix A Supplementary Tables and Figures

Table A1: **Summary Statistics on LBOs With and Without Dividend Recaps (Full Sample)**

	All				DR		Non-DR	
	N	Mean	Median	SD	N	Mean	N	Mean
Panel (A): Portfolio Company and Deal-Level Variables								
Portfolio Company Outcomes								
Bankruptcy (6-Yr) (%)	61,628	0.47			1572	1.34	60,056	0.45
IPO (6-Yr) (%)	61,628	0.66			1572	5.47	60,056	0.53
Deal Characteristics								
Deal Size ($, Millions)	12,408	487.29	100	1681.63	743	755.1	11,665	470.23
Conditional on Fund-of-funds Match								
TEV ($, Millions), Entry	5,801	477.19	119.75	1423.15	523	734.8	5,278	451.66
Debt/Ebitda, Entry	5,267	3.42	3.73	3.22	516	4.15	4,751	3.34
Debt/TEV (%), Entry	5,323	36.68	43.01	27.38	511	48.73	4,812	35.4
Gross Profit (%), Entry	5,526	18.1	17.7	18.89	515	22.25	5,011	17.68
Deal Outcomes (Conditional on Fund-of-funds Match)								
Gross IRR (%)	9,162	26.92	20.44	52.74	652	43.12	8,510	25.68
Gross TVM	9,670	2.59	1.86	2.64	658	3.69	9,012	2.51
Holding Period (Years)	2,289	5.94	6	3.06	339	6.46	1,950	5.85
Δ Gross Profit (%)	3,359	-0.65	0.05	8.95	454	1.93	2,905	-1.05
Δ Debt/Ebitda	3,288	-0.04	-0.35	4.75	456	0.04	2,832	-0.06
Δ Log(Debt) (%)	2,759	32.22	18.91	77.18	403	60.2	2,356	27.43
Panel (B): PE Firm- and Fund-Level Variables								
PE Fund Variables (Conditional on Burgiss Match)								
Fund Size ($, Billions)	1,064	1.41	0.58	2.22	492	1.72	572	1.15
Total Value Multiple	1,888	1.77	1.64	0.78	574	1.31	1,314	1.19
Public market Equivalent	1,888	1.23	1.16	0.5	574	1.31	1,314	1.19
IRR (%)	1,886	16.75	14.53	20.19	574	18.48	1,312	15.99
PE Firm Variables								

Continued on next page

Table A1 – continued from previous page

	All				DR		Non-DR	
	N	Mean	Median	SD	N	Mean	N	Mean
Age (Years)	1,141	27.62	25	14.84	418	27.5	723	27.68
PE No. of Investments	1,212	138.33	53	245.93	423	243.42	789	81.99
AUM ($, Billions)	853	39.63	2.35	149.5	363	36.23	490	42.14
R-Banks CLO Volume	173,798	1.21	0	2.53	1292	3.18	172,506	1.19
R-Banks CLO Volume (1-Yr)	173,797	2.16	0	3.61	1303	4.84	172,494	2.14
R-Banks CLO Volume (5-Yr)	173,797	3.82	0	4.33	1304	6.38	172,493	3.8
R-Banks CLO Count	173,802	0.19	0	0.52	1292	0.56	172,510	0.19
R-Banks CLO Underwriting	173,822	0.04	0	0.11	1284	0.15	172,538	0.04

Notes: This table shows the summary statistics of the leveraged buyout deals in our full sample. We also show the key statistics separately for LBOs that featured a dividend recap transaction and the other LBOs that did not. Panel A contains data at the target portfolio company level. Panel B contains data at the PE firm and fund levels.

Table A2: **Summary Statistics on PE Firm and Secondary Market Loan Characteristics**

	All				DR		Non-DR	
	N	Mean	Median	SD	N	Mean	N	Mean
Panel (A): PE Firm Variables								
Age (Years)	52,487	27.68	26	8.76	917	30.2	51,570	27.63
PE No. of Investments	53,524	403.49	235	426.11	923	452.81	52,601	402.63
AUM ($, Billions)	48,637	29.93	7.04	75.67	872	51.97	47,765	29.53
R-Banks CLO Volume	53,539	2.08	0	3.37	935	3.34	52,604	2.06
R-Banks CLO Volume (1-Yr)	53,539	3.11	0	4.35	935	4.7	52,604	3.09
R-Banks CLO Volume (5-Yr)	53,539	4.57	0	4.86	935	5.96	52,604	4.54
R-Banks CLO Count	53,539	0.46	0	0.94	935	0.71	52,604	0.46
R-Banks CLO Underwriting	53,539	0.1	0	0.23	935	0.18	52,604	0.1
Panel (B): Loan Variables (Conditional on LSTA Match)								
ΔPrice$_{-1,1}$	4,665	0.06	0	0.49	207	0	4,458	0.07
ΔPrice$_{-3,3}$	4,671	0.17	0	1.01	207	0.07	4,464	0.18
ΔBid-Ask$_{-1,1}$	4,665	-0.02	0	0.14	207	-0.03	4,458	-0.02
ΔBid-Ask$_{-3,3}$	4,671	-0.04	0	0.26	207	-0.05	4,464	-0.04
Δ# Quotes$_{-1,1}$	4,830	-0.66	0	12.22	265	-2.82	4,565	-0.54
Δ# Quotes$_{-3,3}$	4,840	-1.65	0	15.51	267	-7.98	4,573	-1.28

Notes: This table shows the summary statistics of the PE firms and leveraged loans in our sample. We also show the key statistics separately for PE firms and loans that correspond to a dividend recap transaction and others that do not. Panel A contains data at the PE firm level and Panel B contains data at the individual loan level.

Table A3: **External Validity Test for IV Analysis**

	All DRs		1st-Stage Residual Low	1st-Stage Residual High	Difference
	N	Mean	Mean	Mean	T-Test
	(1)	(2)	(3)	(4)	(5)
Deal Size ($ Millions)	413	491.92	510.05	464.68	45.37
TEV ($ Millions) Entry	302	423.75	417.63	428.7	-11.07
Debt/Ebitda Entry	301	4.09	3.96	4.2	-0.24
Debt/TEV (%) Entry	300	0.46	0.48	0.45	0.03
Gross Profit (%) Entry	303	0.23	0.22	0.24	-0.01
PE Ownership (%) Entry	253	0.67	0.69	0.66	0.03
Fund Size ($, Billions)	742	1.90	2.17	1.64	0.53***
Fund No. of Investments	758	56.94	57.04	56.85	0.19
Age (Years)	917	29.09	30.39	27.76	2.63***
PE Number of Investments	923	431.7	491.05	370.65	120.40***
AUM ($ Billions)	872	30.67	36.83	24.31	12.52***

Notes: This table shows the difference between marginal DRs (i.e., the ones affected by R-Banks CLO Volume) and other DRs in our sample. We divide all DRs into two groups based on their absolute value of residuals from the first-stage IV specification shown in Equation (4). We show average characteristics of an average DR in Column (2), DRs with low residual (i.e., marginal DRs) in Column (3), and DRs with high residuals in Column (4). Column (5) shows the difference between the two groups.

Table A4: Effect On Bankruptcy in Overlapping Samples

	Bankruptcy		
	Census Sample	Fund-of-funds Sample	Burgiss Sample
	6-Year	6-Year	6-Year
	(1)	(2)	(3)
$\mathbb{1}$(Dividend Recap)	22.65***	14.95*	32.92***
	(7.85)	(8.34)	(9.95)
Stack FE	Y	Y	Y
Obs	24500	2066	28164
Y-Mean	0.98	1.50	1.22
F-Stat	45.16	14.31	44.53

Notes: Table A4 shows the relationship between dividend recaps and portfolio company outcomes. The empirical specification is:

$$y_{s,c,t} = \text{DR}_{s,d(c,f,t)} + \alpha_s + \varepsilon_{s,c,t}$$

s denotes a stack, d denotes a deal, c denotes a portfolio company, f denotes a PE firm, and t denotes the deal year. Column (1) corresponds to the Census-Pitchbook matched sample, Column (2) corresponds to the Pitchbook sample matched to the fund-of-funds data, and Columns (3) corresponds to the Burgiss-Pitchbook matched sample.

Table A5: **IV Effect of Dividend Recaps on Fund Returns - Fund-of-funds Sample**

Panel (A): Fund IRR

	Average	$\mathbb{1}[<0\%]$	$\mathbb{1}[0,20\%]$	$\mathbb{1}[20,40\%]$	$\mathbb{1}[>40\%]$
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	-21.83***	-0.02	0.95**	-0.63**	-0.30**
	(8.37)	(0.07)	(0.38)	(0.31)	(0.14)
Stack FE	Y	Y	Y	Y	Y
Obs	835	849	849	849	849
Y-Mean	21.10	0.02	0.49	0.42	0.07
F-Stat	11.67	10.89	10.89	10.89	10.89

Panel (B): Fund TVM

	Average	$\mathbb{1}[<1x]$	$\mathbb{1}[1,2x]$	$\mathbb{1}[2,4x]$	$\mathbb{1}[>4x]$
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	-0.83**	-0.02	0.46*	-0.35	-0.10
	(0.41)	(0.07)	(0.28)	(0.27)	(0.09)
Stack FE	Y	Y	Y	Y	Y
Obs	835	849	849	849	849
Y-Mean	2.06	0.02	0.52	0.42	0.03
F-Stat	11.67	10.89	10.89	10.89	10.89

Panel (C): Fund PME

	Average	$\mathbb{1}[<1x]$	$\mathbb{1}[1,2x]$	$\mathbb{1}[2,4x]$	$\mathbb{1}[>4x]$
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	-0.61**	0.02	0.12	-0.08	-0.06
	(0.26)	(0.21)	(0.22)	(0.11)	(0.07)
Stack FE	Y	Y	Y	Y	Y
Obs	835	849	849	849	849
Y-Mean	1.35	0.17	0.76	0.05	0.01
F-Stat	11.67	10.89	10.89	10.89	10.89

Notes: Table A5 shows how dividend recaps affect fund-level returns using the IV approach. The second stage of the 2SLS empirical specification is:

$$y_{s,f} = \mathbb{1}(\widehat{\text{Dividend Recap}})_{s,f} + \alpha_s + \varepsilon_{s,f}$$

In Panels (A), (B), and (C) the outcome variables are the Internal Rate of Return (IRR), Total Value Multiple (TVM), and PME Market Equivalent (PME) for fund f. In these panels, we first show the effect on the average outcome of the deal and then show the probability of the deal falling within four bins corresponding to each of the three outcome variables. $\mathbb{1}(\widehat{\text{Dividend Recap}})_{s,f}$ is the predicted value of dividend recapin the fund f from the first stage that we use as the explanatory variable in this second stage. We employ stack fixed effects and cluster standard errors at the stack level.

Table A6: **IV Effect of Dividend Recaps on Fund Returns - Fund-of-funds Returns**

Panel (A): Fund IRR

	Average	$\mathbb{1}$[<0%]	$\mathbb{1}$[0,20%]	$\mathbb{1}$[20,40%]	$\mathbb{1}$[>40%]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	-4.27*	-2.19***	5.08***	-0.77	-2.13***
	(2.19)	(0.61)	(1.25)	(0.74)	(0.82)
Stack FE	Y	Y	Y	Y	Y
Obs	29144	29144	29144	29144	29144
Y-Mean	0.35	0.10	0.30	0.39	0.21
F-Stat	25.33	25.33	25.33	25.33	25.33

Panel (B): Fund TVM

	Average	$\mathbb{1}$[<1x]	$\mathbb{1}$[1,2x]	$\mathbb{1}$[2,4x]	$\mathbb{1}$[>4x]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	-1.41	-1.95***	-6.14***	10.56***	-2.47***
	(2.12)	(0.42)	(1.38)	(2.18)	(0.71)
Stack FE	Y	Y	Y	Y	Y
Obs	29144	29144	29144	29144	29144
Y-Mean	2.68	0.02	0.29	0.57	0.12
F-Stat	25.33	25.33	25.33	25.33	25.33

Notes: Table A6 shows how dividend recaps affect fund-level returns using the IV approach. The second stage of the 2SLS empirical specification is:

$$y_{s,f} = \mathbb{1}(\widehat{\text{Dividend Recap}})_{s,f} + \alpha_s + \varepsilon_{s,f}$$

In Panels (A), and (B) the outcome variables are the Internal Rate of Return (IRR), and Total Value Multiple (TVM), for fund f. In these panels, we first show the effect on the average outcome of the deal and then show the probability of the deal falling within four bins corresponding to each of the three outcome variables. $\mathbb{1}(\widehat{\text{Dividend Recap}})_{s,f}$ is the predicted value of dividend recapin the fund f from the first stage that we use as the explanatory variable in this second stage. We employ stack fixed effects and cluster standard errors at the stack level.

Table A7: **Robustness Tests of Dividend Recap IV Effect on Bankruptcy: Alternative Filters**

	Alternative Industry and Time Window Filters			Alternative Deal and PE firm Filters			
	Same Sector 1-Year Window (1)	Same Sub-Industry 1-Year Window (2)	Same Industry 3-Year Window (3)	Without Deal Size (4)	Without PE firm AUM (5)	Without PE firm Age (6)	Without Add-On Deals (7)
$\mathbb{1}$(Dividend Recap)	70.25***	12.96**	43.72***	33.55***	35.86***	77.48***	20.63***
	(14.92)	(6.34)	(9.37)	(9.38)	(8.85)	(16.08)	(6.13)
Stack FE	Y	Y	Y	Y	Y	Y	Y
Obs	133562	10364	123614	55646	79416	170949	15084
Y-Mean	1.68	1.51	1.27	1.38	1.31	1.26	1.82
F-Stat	58.97	36.42	79.51	59.38	57.46	46.97	67.20

Notes: This table shows robustness tests of the 2SLS effect of dividend recaps on the probability of bankruptcy using Equation 5. The outcome variable is bankruptcy over a 6-year horizon. In the first stage, the endogenous variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The instrument is R-Banks CLO Volume$_{p,t-1}$, which is defined as the average outstanding volume of CLOs underwritten by the PE firm p's relationship banks in the month $t-1$. In Columns (1) to (3) show our results using an alternative set of filters on industry (8 industry sectors and 156 sub-industry codes instead of 40 industry groups) and time window (3 years instead of 1 year) to choose our control deals. In Columns (4) to (6), we omit filtering on deal size, PE firm AUM, and PE firm age, to choose our control deals. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A8: OLS Relationship between Dividend Recaps and Portfolio Company Outcomes

Panel (A): Bankruptcy, Exit, and IPO

	Bankruptcy			Exit		IPO		
	4-Year	6-Year	10-Year	4-Year	6-Year	4-Year	6-Year	10-Year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
𝟙(Dividend Recap)	-0.56*	-0.04	0.58	-.1154***	-.1348***	0.65	1.44***	1.33**
	(0.29)	(0.47)	(0.61)	(.01805)	(.01557)	(0.44)	(0.54)	(0.55)
Stack FE	Y	Y	Y	Y	Y	Y	Y	Y
Obs	53539	53539	53539	24500	24500	53539	53539	53539
Y-Mean	0.91	1.31	1.79			0.82	0.98	1.09

Panel (B): Revenue Growth (4-year horizon)

	Average	𝟙[<-75%]	𝟙[-75,0%]	𝟙[0,75%]	𝟙[>75%]
𝟙(Dividend Recap)	0.18	-0.024	0.025	-0.005	0.004
	(0.064)	(0.025)	(0.043)	(0.041)	(0.048)
Stack FE	Y	Y	Y	Y	Y
Observations	3600	3600	3600	3600	3600
Y-mean	0.387	0.0746	0.246	0.212	0.467
Adj. R-Sq	0.16	0.12	0.13	0.18	0.2

Notes: This table shows the OLS effect of dividend recaps on the probability of bankruptcy, IPO, and revenue growth. The independent variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. In Panel (A) columns (1), (2), and (3), the outcome variables are bankruptcy over a 4-year, 6-year, and 10-year horizon, respectively. In columns (4), (5), and (6), the outcome variables are IPO over a 4-year, 6-year, and 10-year horizon, respectively. In Panel (B) Column (1), the outcome variable is the revenue growth over a 4-year horizon around the dividend recap year, measured as the percent change between the 3rd year after the dividend recap and the year before the dividend recap. Only survivor firms with revenue populated across all four years are included. The dependent variables in Panel (B) columns 2-5 are indicators for growth falling into a particular bin. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A9: OLS Relationship between Dividend Recaps and Employee Outcomes

Panel (A): Employment Growth (4-year horizon)

	Average	$\mathbb{1}$[<-75%]	$\mathbb{1}$[-75,0%]	$\mathbb{1}$[0,75%]	$\mathbb{1}$[>75%]
$\mathbb{1}$(Dividend Recap)	0.11***	-0.013	-0.063**	0.021	0.056*
	(0.033)	(0.01)	(0.028)	(0.031)	(0.028)
Stack FE	Y	Y	Y	Y	Y
Observations	7700	7700	7700	7700	7700
Y-mean	.1801	0.0646	0.38	0.351	0.205
Adj. R-Sq	0.081	0.074	0.079	0.075	0.083

Panel (B): Payroll Growth

	Average	$\mathbb{1}$[<-75%]	$\mathbb{1}$[-75,0%]	$\mathbb{1}$[0,75%]	$\mathbb{1}$[>75%]
$\mathbb{1}$(Dividend Recap)	0.098***	-0.029**	-0.039	0.033	0.035
	(0.033)	(0.011)	(0.029)	(0.03)	(0.026)
Stack FE	Y	Y	Y	Y	Y
Observations	7700	7700	7700	7700	7700
Y-mean	.1309	0.0646	0.38	0.351	0.205
Adj. R-Sq	0.089	0.081	0.083	0.08	0.082

Panel (C): Wage Growth (4-year horizon)

	Average	$\mathbb{1}$[<-75%]	$\mathbb{1}$[-75,0%]	$\mathbb{1}$[0,75%]	$\mathbb{1}$[>75%]
$\mathbb{1}$(Dividend Recap)	-.0059	0.0126	0.0066	-0.014	-0.0047
	(0.024)	(0.012)	(0.031)	(0.03)	(0.011)
Stack FE	Y	Y	Y	Y	Y
Observations	7700	7700	7700	7700	7700
Y-mean	-.03893	0.0369	0.508	0.42	0.035
Adj. R-Sq	0.077	0.077	0.079	0.077	0.08

Notes: This table shows the OLS effect of dividend recaps on employment, payroll, and wage growth. The independent variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The outcome variables in Panels (A), (B), and (C) are employment growth, payroll growth, and wage growth over a 4-year horizon around the dividend recap year, measured as the percent change between the 3rd year after the dividend recap and the year before the dividend recap. Only survivor firms with employment and payroll populated across all four years are included. In each panel, the dependent variable in Column (1) is the average outcome. The dependent variables in columns (2)-(5) are indicators for growth falling into a particular bin. For example, in Panel (A) column 2 the dependent variable is one if employment shrank such that growth was less than -75%. All models include stack fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A10: OLS Relationship between Dividend Recaps and Deal Returns

Panel (A): Deal IRR

	Average	$\mathbb{1}$[<0%]	$\mathbb{1}$[0,20%]	$\mathbb{1}$[20,40%]	$\mathbb{1}$[>=40%]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	7.325***	-0.103***	-0.029	0.087***	0.045
	(1.993)	(0.016)	(0.027)	(0.028)	(0.028)
Stack FE	Y	Y	Y	Y	Y
Obs	29144	29144	29144	29144	29144
Y-Mean	25.35	0.16	0.30	0.26	0.28

Panel (B): Deal TVM

	Average	$\mathbb{1}$[<1x]	$\mathbb{1}$[1,2x]	$\mathbb{1}$[2,4x]	$\mathbb{1}$[>=4x]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	0.832***	-0.106***	-0.032	0.013	0.125***
	(0.156)	(0.016)	(0.025)	(0.029)	(0.028)
Stack FE	Y	Y	Y	Y	Y
Obs	29144	29144	29144	29144	29144
Y-Mean	2.78	0.17	0.27	0.36	0.21

Panel (C): Deal Financials

	Holding Period	Δ Gross Profit	Δ Debt/Ebitda	Δ Log(Debt)	Δ Log(TEV)	Δ TEV/EBITDA
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(Dividend Recap)	1.393***	0.015	0.923***	0.366***	0.134**	1.179**
	(0.226)	(0.010)	(0.337)	(0.065)	(0.054)	(0.476)
Stack FE	Y	Y	Y	Y	Y	Y
Obs	16842	11975	11076	8686	11152	10838
Y-Mean	5.75	-0.00	-0.28	0.30	0.80	2.13

Notes: Table A10 shows how dividend recaps affect deal-level returns and deal financials using the OLS approach. The empirical specification is:

$$y_{s,c} = \mathbb{1}(\text{Dividend Recap})_{s,d(c,p,t)} + \alpha_s + \varepsilon_{s,c}$$

In Panels (A) and (B), the outcome variables are the Internal Rate of Return (IRR) and Total Value Multiple (TVM) for deal d. In both these panels, we first show the effect on the average outcome of the deal and then show the probability of the deal falling within four bins corresponding to each of the three outcome variables. In Panel (C), we use the change in several financial characteristics from the time the PE firm entered the deal to the time of them exiting the deal. $\mathbb{1}(\text{Dividend Recap})_{s,d(c,p,t)}$ is an indicator variable that is one if the deal d experienced a dividend recapitalization, and zero otherwise. We employ stack fixed effects and cluster standard errors at the stack level.

Table A11: OLS Relationship between Dividend Recaps and Fund Returns

Panel (A): Fund IRR

	Average	$\mathbb{1}$[<0%]	$\mathbb{1}$[0,20%]	$\mathbb{1}$[20,40%]	$\mathbb{1}$[>40%]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	2.29***	-0.03***	-0.04*	0.03	0.05***
	(0.57)	(0.01)	(0.02)	(0.02)	(0.01)
Stack FE	Y	Y	Y	Y	Y
Obs	17,513	17,787	17,787	17,787	17,787
Y-Mean	16.85	0.05	0.60	0.31	0.04

Panel (B): Fund TVM

	Average	$\mathbb{1}$[<1x]	$\mathbb{1}$[1,2x]	$\mathbb{1}$[2,4x]	$\mathbb{1}$[>4x]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	0.06*	-0.03***	0.01	-0.02	0.04***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
Stack FE	Y	Y	Y	Y	Y
Obs	17,514	17,787	17,787	17,787	17,787
Y-Mean	1.95	0.05	0.54	0.38	0.03

Panel (C): Fund PME

	Average	$\mathbb{1}$[<1x]	$\mathbb{1}$[1,2x]	$\mathbb{1}$[2,4x]	$\mathbb{1}$[>4x]
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	0.03*	-0.08***	0.04*	0.01	0.04***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Stack FE	Y	Y	Y	Y	Y
Obs	17,514	17,787	17,787	17,787	17,787
Y-Mean	1.26	0.29	0.64	0.05	0.02

Notes: Table A11 shows how dividend recaps affect fund-level returns using the OLS approach. The empirical specification is:

$$y_{s,f} = \mathbb{1}(\text{Dividend Recap})_{s,f} + \alpha_s + \varepsilon_{s,c}$$

In Panels (A), (B), and (C) the outcome variables are the Internal Rate of Return (IRR), Total Value Multiple (TVM), and PME Market Equivalent (PME) for fund f. In these panels, we first show the effect on the average outcome of the deal and then show the probability of the deal falling within four bins corresponding to each of the three outcome variables. $\mathbb{1}(\widehat{\text{Dividend Recap}})_{s,f}$ is the predicted value of dividend recapin the fund f from the first stage that we use as the explanatory variable in this second stage. We employ stack fixed effects and cluster standard errors at the stack level.

Table A12: **OLS Effect of Dividend Recaps on Distributions, Fund Launch, Peer Deal Returns, and LBOs**

	Δ Distribution Transactions		Δ New Funds Launched		Within-Fund Peer Deal IRR		Δ New LBOs	
	1-Quarter	1-Year	1-Quarter	1-Year	Pre-DR	Post-DR	2-Year	4-Year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(Dividend Recap)	0.2436***	0.3547***	0.053***	0.096***	0.017	0.056	1.06***	3.14***
	(0.03)	(0.05)	(0.015)	(0.026)	(0.015)	(0.076)	(0.22)	(0.46)
Stack FE	Y	Y	Y	Y	Y	Y	Y	Y
Obs	35,974	35,974	76,275	76,275	28,715	4,571	75,923	75,923
Y-Mean	0.54	0.55	0.05	0.06	0.26	0.33	-1.03	-6.46

Notes: This table shows the OLS effect of dividend recaps on distributions and new fund launches. The empirical specification is:

$$y_{s,f} = \mathbb{1}(\text{Dividend Recap})_{s,f} + \alpha_s + \varepsilon_{s,c}$$

The variable of interest is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The dependent variable is the change in the count of distribution transactions by PE firm p in 1 and 4 quarters after time t compared to 1 and 2 quarters before time t for columns (1)-(2), the change in the count of new funds launched by PE firm p in 1 and 4 quarters after time t compared to 1 and 2 quarters before time t for columns (3)-(4), the average return of peer deals, i.e., other deals in the fund with the DR (or control) deals before and after time t in columns (5)-(6), and the change in the count of new LBOs launched by PE firm p in 2 and 4 years after time t compared to 1 and 2 quarters before time t for columns (7)-(8). We control for stack fixed effect. Standard errors are clustered at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix B Description of Data Sources and Matching

To conduct our analysis, we obtain both administrative real outcome and proprietary financial outcome data in what we believe to be the most comprehensive analysis of a PE sample to date. In this section, we describe each dataset that we use in the analysis and then use summary statistics to shed initial light on dividend recaps and PE more broadly.

PE Context and Deals from Pitchbook. It is useful to briefly introduce the PE model for those who may not be familiar. PE funds are financial intermediaries, with capital raised from limited partners such as pension funds and endowments. The general partners (GPs), who own the PE firm and manage its funds, are responsible for the lifecycle of a deal: choosing the company to acquire, negotiating the transaction, adjusting operations at the target firm, and finally harvesting value, usually via a liquidation event in which they sell the portfolio company.[33] PE is associated with high-powered incentives to maximize profits because of the large share of debt on the balance sheet and because GPs are compensated with a call option-like share of profits (Kaplan and Stromberg, 2009).

We begin with a comprehensive dataset of PE deals, funds, and firms from Pitchbook, which is about 250,000 deals through June of 2023. We start with all completed deals for which we can see an investor and deal date in Pitchbook. We remove deals in which either the deal date or the investor fields are missing, leaving us with about 157,000 deals. We next filter for Leveraged Buyouts (LBOs) by keeping just over 110,000 deals which are coded as Buyout/LBO, Secondary Transaction - Open Market, Secondary Transaction - Private, Merger/Acquisition, and Platform Creation in deal type in Pitchbook. We keep these extra deal types as we have seen these deals sometimes actually being LBOs in other databases we match to.

We next retain deals between 1995, when the data for our instrument becomes available, and 2020, to have enough time to observe outcomes. The next step is to identify lead investors, which we do using the classification from Pitchbook. In case no investor is classified as lead, we assume the first investor listed in the deal synopsis is the lead. We only keep the lead investor for each deal. We merge the Pitchbook deals to the LCD database on the investor. We then retain only those Pitchbook deals for which we can verify in LCD that at least one investor is PE, because there are some investors in the Pitchbook "PE universe" which are not true PE firms. There are in total about 1,200 investors in the data which we manually verify to be PE firms. Last, we drop any deal in which the only investor is an add-on platform. If both an add-on platform and PE investor are listed as the investor in the data, we keep the deal with the PE as the investor. After these filters, we have our key starting dataset of 47,401 LBO deals, which concern 42,055 unique firms.

Finally, data on dividend recaps are from Pitchbook and LCD. We combine these sources on dividend recaps and deduplicate them. Among the final LBO deals, 1,572 were followed by a dividend recap debt deal.

[33] For details on the PE business model, see Kaplan and Stromberg (2009), Robinson and Sensoy (2016), Korteweg and Sorensen (2017), Jenkinson et al. (2021), and Gompers and Kaplan (2022).

Firm Outcomes: Bankruptcies and IPOs. We gather data on bankruptcies and IPOs from LexisNexis, Preqin and Pitchbook. These firms source bankruptcy events from court records. Bankruptcy is our central outcome variable because it offers comprehensive coverage and no concern about selection into the dataset. In matching to LexisNexis, we clerically confirm matches and ensure an exact match on cleaned name and state. We also match Preqin to Pitchbook on portfolio companies in order to obtain more comprehensive exit information.

Firm Outcomes: U.S. Census Bureau Data. To access administrative information on real outcomes, we match the Pitchbook LBO target companies to the U.S. Census Bureau data. This complicated matching exercise is described in detail in Appendix C. Here, we provide a brief summary. We first match the Pitchbook deals to the County Business Patterns Business Register (CBPBR), which is a internal Census registry of establishments. Establishments represent the smallest unit of a company, corresponding to a particular facility or location. We developed a new, multi-step rigorous matching approach that makes use of 12 crosswalks between Pitchbook and Census variables as well as the firm EIN where available (though EIN is never relied upon alone).[34] The second step is to link the resulting crosswalk to the LBD, where we make use of both Pitchbook's concept of a firm and the LBD's concept of a firm in order to create a best-possible panel dataset at the firm-establishment-year level. We are able to match with reasonable confidence 33,500 unique firms.

We use time series data that appear in the LBD on employment, payroll, revenue, average wage, and exit, aggregating up to the firm level where necessary. With these in hand, we structure the dataset at the LBO level (i.e., so a firm appears once), to align with the rest of our analysis. This involves reshaping the data to create variables for time-varying outcomes centered around the deal year. For example, we create Emp_{t-1} to represent employment in the year before the deal.

Investor Outcomes: Burgiss & Fund-of-funds. To our knowledge, this is the first paper on PE to observe both fund- and deal-level performance. For deal-level performance, we use data from a large fund-of-funds and advisory services firm, which has built a private market database since 2006. These data come from performing due diligence and monitoring investments, similar to other academic sources of deal-level PE return data (Robinson and Sensoy, 2013; Degeorge et al., 2016; Braun et al., 2017). As this source wishes to remain anonymous, we call it the "fund-of-funds" data. The fund-of-funds requires fund managers to report returns from all deals and reconcile them with fund-level performance, which mitigates the bias towards more successful deals that is suffered by datasets that allow selective reporting. We use deal-level internal rate of return (IRR) and total value multiple (TVM) as the key deal-level return variables. The fund-of-funds does not have contributions from or distributions to LPs, so it is not possible to calculate a precise fund-level return, since IRRs at the deal level may be quite different from the overall fund IRR depending on

[34] A firm may change the EIN they use for reasons unrelated to ownership, such as switching to a new accountant. The Census concept of a firm, captured in the *lbdfid* variable, is "an economic unit comprising one or more establishments under common ownership or control"; see Chapter 3 in National Academies of Sciences et al. (2018).

how value is returned to LPs. The fund-of-fund's lack of cash flow data also prevents calculating deal-level public market equivalents (PMEs).

While the fund-of-fund provides deal level data on IRR and TVM, it does not have information on all the contributions and distributions between limited partners and general partners. This makes it difficult to aggregate returns across various deals and calculate fund-level returns accurately. Thus, we employ the Burgiss database to calculate fund-level return variables. Burgiss collects detailed information for each distribution and contribution in each PE fund. This detailed time varying cash flow information allows us to calculate common fund level returns, including IRR, TVM, and PMEs. We are able to match 9,780 (20%) of the Pitchbook LBOs to the fund-of-fund data, and 1,888 (44%) of Pitchbook funds to Burgiss.

Loans from LCD and Dealscan. We construct the sample of loans taken by PE-backed companies by combining two sources: Leveraged Commentary & Data (LCD, now owned by Pitchbook) and Refinitiv Dealscan. Both sources provide loan-level information on borrowers, lenders, and PE sponsors. They also provide details on loan amount, maturity, interest rate spread, loan covenants, etc. However, LCD and Dealscan differ in their coverage and do not fully overlap with each other. E.g., Dealscan widely covers the broadly syndicated loan market. However, several studies express concern that Dealscan has poor reporting quality in the leveraged loan market and often mis-classifies covenant-lite loans. (Becker and Ivashina (2016); Bräuning et al. (2022)).[35] This is an important concern because CLOs predominantly buy leveraged loans. Thus, we supplement the Dealscan sample with LCD, which provides comprehensive data on U.S.-issued leveraged loans and has been used in several recent studies (Bruche et al. (2020); Ivashina and Vallee (2020)). Combining the two datasets provide us a more detailed picture of lending relationships between banks and PE-sponsors.

We create a combined sample of loans by first matching borrowers, lenders, and PE sponsors across Dealscan and LCD. Each loan in both datasets consists of several tranches. We categorize tranches into two groups – the *prorata* tranches consist of revolvers and amortizing loan facilities, whereas the *institutional* tranches consist of Term-B and other term-loan facilities. We aggregate loans at borrower-monthly date combination and define a tranche as the loan-tranche-type combination. If a loan is present in both datasets, we only keep the LCD entry to avoid the double-counting of loans in our sample. The combined LCD-Dealscan sample contains 15,627 loans containing 26,388 tranches by 7,877 companies between 1986 and 2020. Of these, we can match 5,973 to LBO targets from Pitchbook, of which 1,069 had a dividend recap. After removing non-U.S. companies and instances of spurious double-counting across Dealscan and LCD, we are left with 5,081 companies. There are 1,156 unique PE firm sponsors and 180 lead arranger banks. We use this sample of loans to define lending relationships between PE firms and banks.

[35] Another issue with the Dealscan data is that its older version did not adequately differentiate between loan originations and amendments (Roberts (2015)). However, we use the new version (called Refinitiv LoanConnector Dealscan) which contains a variable called Tranche O/A which identifies originations in the sample.

Secondary loan market outcomes from LSTA. Secondary market data on leveraged loans comes from Loan Syndications and Trading Association (LSTA) loan pricing service. It provides loan characteristics (issuer name, loan type, and loan maturity) along with daily price, number of quotes, and bid/ask in the OTC market. LSTA receives quotes from over 35 dealers that represent almost all major commercial and investment banks. It represents over 80% of the entire secondary market trading for syndicated loans and is representative of the secondary loan market conditions for large corporate loans.More information about the LSTA data is provided by Berndt and Gupta (2009) and Saunders, Spina, Steffen, and Streitz (2020). We are able to identify 2,227 Pitchbook LBO targets in the LSTA data. Out of these, 718 were involved in a Dividend Recap transaction. We use this sample to examine the impact on DR on the companies' pre-existing creditors.

Collateralized Loan Obligations from Acuris CLO-i. We construct the shocks for our instrumental variables analysis by combining the PE-bank relationship data with CLO issuance data from the Acuris CLO-i database. CLO-i includes information about the CLO manager, the CLO portfolio, and the underwriting bank. We use this detailed data on CLO funds to quantify banks' CLO underwriting activity and to examine purchase of dividend recap loans by CLO managers. It provides comprehensive information on investment portfolios and trading activities of US and European CLOs. The database has information on about 3,000 CLOs managed by 228 managers and arranged by 47 banks. The CLOs in the sample hold loans of 13,800 firms belonging to 35 broad industries. The sample time period ranges from 1998 to 2020. This information is sourced directly from over $45,000$ trustee reports and CLO prospectuses. CLO-i data has been used by Ivashina and Sun (2011), Benmelech et al. (2012), Loumioti and Vasvari (2019a), Loumioti and Vasvari (2019b), Elkamhi and Nozawa (2022), among others. While the CLO-i data is not exhaustive, it captures a substantial portion of overall holdings and trading in the corporate leveraged loan market. Acuris's coverage of the CLO market has increased steadily from about $45\% - 60\%$ prior to 2009 to near-comprehensive coverage after that. Recall from above that we observe loans for 1,069 LBO portfolio companies with dividend recap. Of these, 782 were financed by CLOs. In a final step, we connect the relationship banks with CLO issuance. Of the 636 relationship banks in our loan sample, 35 ever underwrite a CLO.

Challenges from Many Samples. This paper benefits from an unprecedented combination of data describing PE funds, deals, and portfolio company real outcomes. To our knowledge, this is the widest set of variables capturing the most comprehensive financial and economic picture of PE deals in the literature to date. For example, it is rare but important to observe both administrative data on employees and financial returns. Combining these data in common causal analytical models is crucial to push forward in understanding how all stakeholders in this ecosystem are affected. However, the private nature of the industry means the sources for these datasets are necessarily diverse and subject to significant access restrictions, making it impossible in some cases to combine them. Furthermore, the samples for analysis vary depending on the matched subset. This means that we cannot in all cases test whether we see the same effects on the overlap sample, or to assert that results in a given matched sample would be same in the complement non-matched sample. This creates necessary caveats to our interpretation, but as mentioned above, we believe that our results taken together paint a consistent picture and we provide evidence that the various samples are similar on observables, suggesting the results are valid beyond the matched subsets.

Appendix C Matching Process to U.S. Census Bureau Data

The matching exercise has two broad steps. The first is to match the Pitchbook deals to the County Business Patterns Business Register (CBPBR), which is a internal Census registry of establishments. Establishments represent the smallest unit of a company, corresponding to a particular facility or location. The CBPBR is a cleaned and processed combination of the Business Register (BR) and County Business Patterns (CBP) microdata, spanning 1976 to 2020. It provides consistent establishment level information, including name, address, zip code, and state.[36] The second step is to link the resulting crosswalk to the Longitudinal Business Database (LBD), and to make use of both Pitchbook's concept of a firm (*pbid*) and the LBD's concept of a firm, which is identified by their *lbdfid* variable, in order to create a best-possible panel dataset at the firm-establishment-year level, in which the Census work that underlies the *lbdfid* variable allows us to see dynamically establishments being added to the firm (e.g. buy-and-build), created de novo, or sold to another firm.

In what follows, we first describe the different datasets that we employ. Then we explain the matching process in detail. Finally, we provide summary statistics about the match results.

C.1 Matching to the CBPBR

We begin with a set of about 86,000 unique companies in Pitchbook's private equity universe based on Pitchbook's firm ID, which we call *pbid*. Each deal has a deal year, several addresses, and company name variables. Deal year varies at the deal-level, address and company name vary at the company-level.

We match the Pitchbook data to the CBPBR. In the CBPBR, Each file is one year, where the level of observation is the unique establishment ID which applies only to that year, called *id* (also known as *estabid*). Importantly, this *estabid* is not the same for the same establishment across years; it is year-specific. We divide each year file into separate states. We match to the CBPBR in the year before the deal year and in the deal year if there is no match in the we don't find it in the year before). We create the following 12 crosswalks, where the left object is from Pitchbook and the right object is is from the CBPBR:

1. Address 1 to Physical Address
2. Address 1 to Mailing Address
3. Full Address to Physical Address
4. Full Address to Mailing Address
5. Company Name to Name 1
6. Legal Name to Name 1

[36] More on its creation and usage can be found in Chow et al. (2021).

7. Alternate Name to Name 1
8. Former Name to Name 1
9. Company Name to Name 2
10. Legal Name to Name 2
11. Alternate Name to Name 2
12. Former Name to Name 2

We run three matching exercises, named "Fuzzy1", "EIN", and "Fuzzy2". For "fuzzy" matches, we read in the CBPBR data, subset to the state, year, and if either the mailing or physical zip matches. For Fuzzy1, the zip refers to 5-digit zip. For Fuzzy2, the zip refers to 3-digit zip, which is a less stringent location criteria. For EIN matches, we match Pitchbook companies to Dun and Bradstreet to obtain the EIN, requiring an exact match on name and address in Dun and Bradstreet. Since EINs are longitudinally consistent, we then match EINs from Pitchbook directly to EINs in the CBPBR on any year. However, we recognize that EIN matches can be unreliable, as changing the accountant can constitute a change in EIN. Therefore, EIN matches only contribute to the overall score, instead of determining a match fully.

We then use Term Frequency – Inverse Document Frequency (TFIDF) to remove rows where neither the physical or mailing address have a remote similarity to the full address. We use TFIDF because it is comparatively fast. TFIDF is a standard natural language processing technique that measures how important a term is. It weights terms by how frequently they appear in a string by how frequently they appear in the dataset as a whole. Each string is split into n-grams, which may capture more information about text than the text itself (e.g. accounting for errors).[37] We impose a low threshold here of 40; this includes many obviously fals matches, so it is highly unlikely that a true match is removed at this stage.

Then, for each of the 12 crosswalks listed above, we compute 6 match scores: the Levenshtein, Damerau-Levenshtein, Jaro, JaroWinkler, Qgram, and Cosine distances, and save these scores. When filtering, we don't know if the address in Pitchbook maps to the mailing or physical address, so we don't consider an aggregate score of the two. Instead, it is enough if the either mapping has a high score. In the same way, either the shorter Address 1 or Full Address having a sufficient score is enough. We perform the same filtering on name, that is, any name match is good. We apply further filters to the address match. The first and trailing numbers must match, if they exist. This is meant to prevent spurious matches like 1 Waverly Place and 2 Waverly Place. Each of the six scores is assigned a weight, normalized to sum to 1. Visual inspection indicates that that Damerau and JaroWinkler perform the best, so they have the highest weights. We then determine the threshold of the 6 weighted averaged scores that will define a successful match. This

[37]For example, the bigram for "independence" is ["in", "nd", "de", "ep", "pe", "en", "nd", "de", "en", "nc", "ce"]. Anecdotally, bigrams and trigrams perform the best. We follow tfidf-matcher 0.3.0, which uses trigrams as the default.

is arrived at by clerical examination of the data. Matches are ranked based on a combination of factors: the address score, the name score, if it matches on EIN, if it has the same geography.

Overall, a match type is a combination of name, address, EIN, and geography, for a total of 5 * 5 * 2 * 4 = 200 match types. An example of a match type is “exact name:confident address:no match ein:same zip5”. The EIN factor is a dummy for whether an EIN match is present. The address and name scores are broken down into 5 components:

1. Exact match (score = 1)
2. Confident match (score >= .8)
3. Fairly confident match (score >= .7)
4. Maybe confident match (score >= .55)
5. No match (score < .55)

The geography factor is broken down into:

1. Same 5-digit zip
2. Same 3-digit zip
3. Same state
4. No match

We then weight the factors. An exact match on name holds the highest weight, then a confident match on name, and so on. The exact rankings are:

1. Exact name
2. Confident name
3. Exact address
4. Confident address
5. Fairly confident name
6. Fairly confident address
7. Same EIN
8. Maybe name

9. Same 5-digit zip
10. Maybe address
11. Same 3-digit zip
12. Same state

A match type score is then computed using these weights. For example, a match type of "exact name:confident address:no match ein:same zip5" will rank higher than "exact name:confident address:no match ein:same zip3". This allows us to filter on match quality. Finally, we construct a condensed match type, with the following tiers:

1. Very confident
    (a) If confident name or above is combined with at least one of: EIN, fairly confident address or above, same 5-digit zip
    (b) If fairly name is combined with two of: EIN, fairly confident address or above
    (c) If maybe name is combined with fairly address, same zip5, and same EIN
2. Confident
    (a) If confident name or above is combined with same state or above
    (b) If fairly name is combined with at least one of: EIN, fairly confident address or above
    (c) If maybe name is combined with at least one of: confident address or above
    (d) If maybe name is combined with two of: EIN, maybe address or above
3. Somewhat Confident
    (a) If maybe name is combined with EIN
    (b) If fairly name is combined with fairly address or above
    (c) If fairly address is combined with EIN
4. Borderline
    (a) If same EIN
    (b) If maybe name is combined with maybe address or above
5. Likely not a match: All others

We retain matches in the top three tiers, which in manual inspection appear to have high rates of accuracy. There are rare cases where we obtain different but apparently successful matches in both years considered (deal year-1, and deal year). In this case, we impose the following rule: keep the match in the year before the deal year unless the match in the deal year is significantly better, where "significantly" is defined as having a greater than .1 combined address and name score.

C.2 Bringing in the LBD

With this match in hand, we bring in data from the LBD. In the LBD, each file is one year. The level of observation is the LBD establishment (*lbdnum*) which is consistent across years. These data also include *estabid* to match to CBPBR. Further, they include the LBD FirmID, which is a carefully constructed Census variable that corresponds to a firm, incorporating name changes and restructuring, as well as additions and subtractions of establishments, to the greatest extent possible. Note that *lbdfid* defines firms, which Census defines as "an economic unit comprising one or more establishments under common ownership or control" (see Chapter 3 in National Academies of Sciences et al. (2018)). It is longitudinally consistent across years for firms, but is not consistent at the enterprise-level (*ein*). That is to say, a firm may change the EIN they use for reasons unrelated to ownership, such as switching to a new accountant. In this way, the LBD offers a high-quality firm identifier.

We mach the CBPBR to LBD on *year* and *estabid*. Not all establishments found in the CBPBR match to the LBD perfectly, as the LBD implements re-timing algorithms that the CBPBR does not.[38] If there is no match on *estabid*, we match on *estabid-rorg*. If there is still no match, we repeat the process, but look in the year before and after. While *estabid* is not intended to be longitudinal, it is not uncommon that it is. After the match, we check the quality of these matches and retain only those that satisfy a high bar, with minimum name and address scores of .8 and .95, respectively).

Our final dataset in the LBD has about 58,500 unique firms matched using the top two tiers. We restrict to 33,500 that are in the LBO dataset that we use for the bankruptcy analysis, for a match rate of about 55%. We then aggregate the data from the establishment level up to the firm level. We make use of time series data on firm-level (*lbdfid*) employment, payroll, revenue, and exit that appear in the LBD. There are both quarterly and annual variables for employment and payroll. For each variable, we take the maximum of the four-quarter sum and the annual measure. Revenue is only available for a subset of the sample. This is because revenue is added to the LBD using income tax receipts that are gathered and matched by U.S. Census Bureau staff in a separate exercise from original LBD construction, where information with payroll and employment attached form the backbone of the time series (for more information, see Haltiwanger et al. (2019)). With these in hand, we structure the dataset to align with the rest of our analysis, which is to say at the one-per-LBO level. This requires reshaping to make new variables for each time-varying outcome, centered around the deal year. For example, we create Emp_{t-1} to be employment in the year before the

[38] Chow et al. (2021) describes this issue in more detail.

deal.

We then construct our outcome variables. For exit, we simply consider years from the deal, for example whether the firm has exited as of four and six years following the deal. For the continuous variables, we restrict the analysis to survivor firms and construct growth relative to the year before the deal. For example, employment growth through the third year after the deal is defined as $\frac{Emp_{t+3}-Emp_{t-1}}{Emp_{t-1}}$. Note that the deal year is $t = 0$, so we look four years after relative to one year before. We impose a stringent requirement that employment be observed for all years between $t-1$ and $t+3$ in order to retain the firm in this survivor sample. This ensures consistency across the outcome variables with no intermittency. Finally, we focus analysis on categorical variables capturing the nature of growth: Was this a very good outcome, an OK outcome, a poor outcome, or a very poor outcome? We approximate these with indicators for growth greater than 75% (very good), between 0 and 75% (OK), between 0 and negative 75% (poor), and less than negative 75% (very poor). Summary statistics at the company level about the real outcomes from the Census-matched sample are in Table 1.