LEVERAGED PAYOUTS:
How Using New Debt to Pay Returns in Private Equity Affects Firms, Employees, Creditors, and Investors *

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Abstract

Private equity (PE) managers often generate financial returns without selling the portfolio company by leveraging company assets or cash flows. We study one such "leveraged payout" transaction, the dividend recapitalization (DR). As large, high-quality firms are selected for DRs, we identify causal effects using PE relationship bank CLO underwriting. DRs induced by cheap credit make firms riskier, with higher bankruptcy and failure rates, but also more IPOs and revenue growth. While DRs increase deal returns, they reduce wages, pre-existing loan prices, and fund returns (possibly reflecting moral hazard via new fundraising), pointing to negative implications for employees, creditors, and investors.

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Private equity (PE) has a growing footprint in the economy. 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.[1] It is thus important to understand how PE managers create value. Under the traditional (and well-studied) business model, PE managers target an under-performing firms via leveraged buyouts (LBOs), improve them, then earn financial returns by selling them for a higher price (Kaplan and Stromberg, 2009; Lerner et al., 2011; Bernstein and Sheen, 2016; Gupta et al., 2023). Over the past two decades, a new class of transactions has emerged, which we term the “leveraged payout," in which PE managers leverage company assets or cash flows to generate 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 recapitalization (or “recap”), in which the proceeds of new debt backed by expected cash flows is used to pay returns to investors. Understanding these deals is relevant not only to the PE industry but to capital structures and the rise of private credit in the economy more broadly.

There is increasing and largely negative attention to leveraged payouts in the media and from creditors in bankruptcy proceedings (Lim and Weiss, 2024). For example, creditors filing a lawsuit against Caxton-Iseman Capital, which had owned Buffets, a restaurant chain, 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."[2] Similarly, when Bain Capital-owned KB Toys went bankrupt in 2012, creditors claimed an earlier dividend recap rendered the firm insolvent.[3] And Cerberus’ sale of Steward Hospital system’s real estate created rent obligations that were later blamed for the hospital system’s bankruptcy.[4] The PE industry has a different perspective, contextualizing 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).

Leveraged payouts are not like dividend recaps in publicly traded companies, where new debt issuance and total debt are small relative to assets. They also differ from LBOs, where leverage increases but the company benefits from efficiency improvements under new PE ownership and new capital injections (Boucly

[1] 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.

[2] 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).

[3] 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).

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

et al., 2011; Bloom et al., 2015; Agrawal and Tambe, 2016; Fracassi et al., 2022). Theory points to potentially mixed impacts of leveraged payouts. A positive view is that they can permit longer holding periods, extending the benefits of PE “treatment" for the company while also increasing returns to investors. While new debt increases risk, holding all else equal, creditors may be unwilling to supply excessive debt and PE managers may choose leverage to maximize firm value, exploiting the operationally disciplining effect of debt (Jensen, 1986). This perspective predicts that leveraged payouts will always occur at strong companies and will benefit financial stakeholders. Alternatively, the deals might represent a market failure in which PE managers use excessive debt (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, and ultimately need not increase overall fund returns.

Despite the increased prevalence of deploying debt in the middle of the deal lifecycle to deliver cash to investors, there is to our knowledge no rigorous study of these transactions. In this paper, we examine dividend recaps, where a PE firm sponsors a loan on behalf of its portfolio company and uses that loan to pay a dividend to its fund. We offer causal estimates of how dividend recaps affect relevant stakeholders: the portfolio company itself, its employees and pre-existing creditors, and finally fund investors. Dividend recaps have become a significant tool in the PE playbook, as shown in Figure 1. As we discuss further below, our analysis not only sheds light on the role of debt in PE, but also makes progress towards understanding how capital structure affects the firm.

To establish a causal effect of dividend recaps, we make use of the fact that 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 peer firms. In choosing this design, we are 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 the broader literature showing a role for supply-side channels in determining firm leverage (Baker and Wurgler, 2002; Leary, 2009). 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” (Bobeldijk, 2012). Specifically, we instrument for dividend recaps using PE relationship banks’ collateralized loan obligation (CLO) underwriting. CLOs are actively managed, highly diversified portfolios of leveraged loans, mostly to PE-backed firms. They are the main investors in leveraged PE-sponsored bank loans. This market has grown dramatically, from about \$500 billion in outstanding leveraged loans as of 2010 to \$1.4 trillion in 2023, roughly the size of the U.S. high-yield bond market (Fidelity, 2024). There is industry consensus that this growth reflects investor demand and “search for yield" (Johnson, 2018).

When a PE firm’s relationship bank underwrites a new CLO, the firm has better access to credit while the CLO manager is building his book. Banks play crucial roles in the CLO market. First, 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). Second, a bank underwrites the CLO, which includes negotiating contract terms and assessing the creditworthiness of the borrowers whose loans are being purchased. Importantly, the underwriting bank must approve every loan in the portfolio. If the bank has a relationship with the loan's PE sponsor, it is likely easier for the CLO to acquire the loan. Shivdasani and Wang (2011) provide evidence for this channel by documenting the within-bank correlation between LBO lending and CLO underwriting. Furthermore, PE firm relationships with bank lenders affect debt financing for portfolio companies (Drucker and Puri, 2009; Demiroglu and James, 2010; Ivashina and Kovner, 2011; Malenko and Malenko, 2015; Shive and Forster, 2022). To put this another way, the bank has more information about and is incentivized to place loans that it has originated, and the CLO manager in turn is incentivized to purchase these loans given the underwriting relationship with the bank. This relationship is the heart of our identification strategy.

Our instrument is the outstanding value of CLOs underwritten by the LBO deal's lead PE firm's relationship banks. Loans issued by at least 100 unique companies compose the CLO, so no one company motivates CLO formation or determines CLO performance. We show a robust first stage and support the intuition of the instrument by documenting that treated dividend recap loans are much more likely to end up in the most recent CLO of the sponsoring PE firm's relationship bank than a random dividend recap target. 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 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 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 particular sector. We begin with data on LBOs and dividend recaps from Pitchbook. Our LBO sample includes about 61,000 LBOs sponsored by over 1,200 unique PE firms between 1985 and 2023. We observe nearly 1,600 dividend recaps that we connect via the sponsor PE firm to a previous LBO. To construct the instrument, we use information on loans, bank relationships, and CLOs with data from LCD, Dealscan, Acuris CLO-i, and Capital IQ. We obtain deal- and fund-level returns from Stepstone SPI and Burgiss, respectively, and bankruptcies from LexisNexis. Finally, we study real outcomes in the U.S. Census Bureau's Longitudinal Business Dynamics dataset.

We find that on average, dividend recaps are conducted on large and seemingly healthy firms. Once we control for this selection, the causal analysis paints a picture in which new debt induced by cheap credit increases firm risk, consistent with theories predicting agency problems of debt. We focus first on the firm. We show that dividend recaps dramatically increase the chance of bankruptcy, for example by 17pp in the following six years, relative to an overall sample mean of 1%.[5] Using U.S. Census Bureau data, we find that dividend recaps also increase exit within six years by about 200% of the mean. 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.

Turning to employees, we examine the effects on four-year growth in employment, payroll, and average wages relative to the year before the dividend recap, 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%, which is many times the 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 of modest positive employment growth. Overall, these results suggest that by increasing firm risk, dividend recaps increase the chance of the firm experiencing 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 returns to investors. The increased risk measured in real outcomes is paralleled by wider dispersion in returns. Dividend recaps increase deal IRR, while the effect on the deal multiple is positive but not statistically significant. By bringing forward returns, dividend recaps may enable a longer holding period. We find that this is indeed the case. Longer holding periods could benefit the firm, as one critique of PE owners is short termism (Konczal, 2015). If dividend recaps increase holding periods, they could minimize one "dark side" of PE while allowing the firm to benefit from the PE managers' operational engineering for a longer time period.

At the fund level, we show that dividend recaps decrease the fund's Total Value Multiple (TVM) and public market equivalent (PME) return measures. There is no effect on IRR, consistent with bringing cash flows forward in the fund's life. 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. It is remarkable to see this value-shifting even in the OLS models. The results point to much larger reallocation in the more opportunistic deals that are compliers with our instrument.

Our results point to a mechanism in which dividend recaps lead to misaligned incentives and moral hazard problems for GPs, causing them to pursue activities that diverge from the interests of fund investors, company employees, and pre-existing creditors. We show that dividend recaps dramatically increase short-term distributions paid out to the fund. This could incentivize the GP to raise a new fund on the basis of 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 are used to

[5]The overall sample mean with each deal repeated once, is different from the stacked sample mean of 0.75%, where each firm may appear as control in multiple stacks

benefit GPs by enabling early distributions and new fundraising. In turn, they may focus their effort more on the new funds. Consistent with this, we observe that dividend recaps cause lower returns for subsequent LBOs within the fund, relative to funds of the same vintage. This helps to explain the negative effect on overall fund returns.

Meanwhile, having realized good returns from the targeted portfolio company, the GP may encourage its managers to take more risk because the investment's payoff has become more call option-like. This the fatter-tailed outcome distribution we observe. GPs are also less incentivized to create value in the company in order to accomplish a strong exit. Finally, the company is inherently riskier and weaker because of the dividend recap, leading to higher chances of distress and poorer returns for pre-existing creditors. Consistent with more risk, dividend recap loans have higher interest rates than other leveraged loans, which adds to the debt service burden. While higher risk need not be bad for investors *per se*, it is likely bad for employees and creditors (and, generically, any risk-averse stakeholder). The negative outcomes of exit and bankruptcy are much more common than IPO and have detrimental consequences for employees, who face frictions finding a new job and lose lifetime earnings (Berk et al., 2010; Graham et al., 2023). This is compounded by the negative effect on wages among surviving firms.

This empirical design serves to document an indirect outcome of the dramatically increasing demand for CLOs: More dividend recaps and their attendant real effects. 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, 2021). More supply-side driven CLO issuance should create demand for risky firms. PE funds fulfil this demand by increasing leverage in existing portfolio companies through dividend recaps, which can have negative implications for many portfolio company stakeholders.

Our causal effects differ from descriptive analysis in OLS regressions. Dividend recap targets are typically high-quality firm on growth trajectories. OLS regressions capture this positive selection as well as any causal impact. We find that the average dividend recap is associated with a much smaller and weaker increase in bankruptcy and lower chances of exit. Similarly, employment and payroll increases, while average wages and revenue stay constant. Therefore, contrary to media narratives, dividend recaps are not in general associated with bad outcomes, as these deals are usually carried out on larger and stronger firms. The IV effects represent the causal impacts of cheap credit-induced dividend recaps, with selection bias significantly reduced.

The data we use are, to our knowledge, unprecedented in bringing together information on PE funds, deals, and portfolio company real outcomes. By combining financial and economic pictures, not only for GPs but also for LPs, creditors, and employees, we are able to shed light on the implications of new debt in PE in a more holistic way than has been possible in most research on PE. The downside is that the sources have different matched sample sizes and suffer from access restrictions and limits on combination. However, the consistency of our results, in particular for the central finding that dividend recaps increase firm risk without a commensurate benefit for LPs, appears in multiple datasets—including in the full sample for the most important result on bankruptcy—and seems unlikely to reflect a particular subsample.

This paper complements the rich literature on PE. There is much evidence that following LBOs, PE managers improve company performance, enabling them to earn returns by selling the company for more than its purchase price (Cohn et al., 2014; Hotchkiss et al., 2021; Brown et al., 2021; Gompers and Kaplan, 2022).[6] In a dividend recap, the strategy is different, targeting high quality companies within the portfolio rather than external and underperforming companies. The treatment effects of PE likely impact how debt affects firm outcomes. While more debt should raise the chance of financial distress holding all else equal, there is evidence that PE firms specialize in managing firms through distress to avoid bankruptcy, for example by developing expertise in negotiating with creditors or by injecting additional capital (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 relative to a counterfactual where a non-PE-owned firm takes on a similar amount of debt.

We also shed light on how capital structure affects the firm because we analyze a change in debt that is not deployed within the firm and thus occurs independently of changes to the asset side of the balance sheet. Further, we study the implications of debt for both the real and financial sides of the firm. 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 non-farm employment, and which have unique disclosure obligations and highly dispersed ownership (Francis, 2007). Also, existing work focuses on evaluating pecking order and tradeoff theories in determining the source of financing (Myers, 1984; Fischer et al., 1989). In contrast, we study the effects of a specific change in capital structure, and our results support the idea that financing structures affect firm outcomes. 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.

Our study joins existing work on the relationships between capital supply, firm leverage, and firm outcomes. Much evidence suggests that firms take on more debt when credit supply increases (Faulkender and Petersen, 2006; Sufi, 2009; Rice and Strahan, 2010; Lemmon and Roberts, 2010). There is some work on the macroeconomic effects of corporate debt, including Mian et al. (2017), Greenwood et al. (2022), Jordà et al. (2022), and Ivashina et al. (2024). At the firm level, Giroud and Mueller (2017) and Sever (2023) show that higher leverage predicts initial employment expansions but then greater employment losses, while Kalemli-Özcan et al. (2022) shows that higher leverage predicts lower investment. These papers study leverage following a negative shock (e.g., a financial crisis). Our instrument is closer to work on determinants of leverage, including Benmelech and Bergman (2009), Eisfeldt and Rampini (2009), Rauh and Sufi (2010),

[6]Further work on the real outcomes side includes Acharya et al. (2012), Davis et al. (2014), Agrawal and Tambe (2016), Bernstein et al. (2019), Eaton et al. (2020), Cohn et al. (2021), 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 et al. (2019), and Gupta and Van Nieuwerburgh (2021).

Rauh and Sufi (2012), and De Maeseneire and Brinkhuis (2012).[7]

Finally, there is a small literature on dividend recaps, which has studied mostly public firms and in small samples (see Eckbo and Thorburn (2007) for a review).[8] Some studies in PE have touched on dividend recaps. With very small samples of these deals and no causal interpretation, they have generally found no significant associations (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 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

Before describing our data, it is useful to 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.[9] 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).

1.1 Data Sources

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 Appendix B, we explain each dataset that we use in the analysis in detail. Here, we provide a very brief overview. Then, we describe summary statistics to shed initial light on dividend recaps and PE more broadly.

We begin with a comprehensive dataset of PE deals, funds, and firms from Pitchbook. We focus analysis

[7] More generally, our empirical design also connects this paper to work on the syndicated loan market, lead arranger incentives, and securitization of corporate debt (Ivashina and Scharfstein, 2010; Benmelech et al., 2012; Nadauld and Weisbach, 2012; Wang and Xia, 2014; Lee et al., 2022).

[8] This literature begins with Masulis (1980) and Masulis (1983), who study instances of "pure" capital structure changes where firms substitute equity with debt or vice versa (also see Pinegar and Lease (1986) and Cornett and Travlos (1989)). Early work on high leverage transactions include Kaplan and Stein (1990) and Denis and Denis (1993).

[9] 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).

on PE transactions between 2000 and 2023. This includes about 61,000 unique firms undergoing LBOs, sponsored by over 1,200 unique PE firms. Among these LBOs, about 1,600 were followed by a dividend recap debt deal. Data on dividend recaps are from Pitchbook and LCD. We collect portfolio company outcome data on bankruptcies and IPOs 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 data. The matching exercise is summarized in Appendix B and described in exhaustive detail in Appendix C. We are able to match with reasonable confidence 33,500 unique firms, or about 55% of the LBO sample. 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 gather fund data from Burgiss (we match 1,888 funds, or 44% of the Pitchbook sample) and deal data from Stepstone Group (we match 9,780, or 16%, of the Pitchbook LBOs). 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. In our final dataset, 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. 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, which has been used by Ivashina and Sun (2011), Benmelech et al. (2012), Loumioti and Vasvari (2019a), Loumioti and Vasvari (2019b), and Elkamhi and Nozawa (2022), among others. 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. 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 are able to match our LSTA sample with 2,227 Pitchbook companies, among which, 718 have done a dividend recap transaction during our sample period.

This paper benefits from an unprecedented combination of data describing PE funds, deals, and portfolio company real 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, the samples 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. While this creates some caveats to interpretation, we believe that our results taken together paint a consistent picture.

1.2 Summary Statistics: Understanding Dividend Recaps

One contribution of our study is to provide the first academic look at dividend recaps. Figure 1 shows the number of dividend recaps over time. These deals became popular during the PE boom of the mid-2000s, reaching 15% of LBOs in 2004, then declined sharply during the financial crisis, and subsequently have been 5-10% of LBOs, or 100-200 deals per year. They appear to be more common during low interest rate periods, when credit is cheap. Next, we compare the industry composition of firms with dividend recaps to the overall sample with LBOs in Figure 2 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 2).

Dividend recaps may make longer holding periods feasible. This could represent a benefit, as there are calls for LPs to prioritize long-term and evergreen funds under the assumption that short-termism is bad for companies and undermines the ability of PE managers to create long term value.[10] This could lead dividend recaps to have a positive effect. In practice we see that dividend recaps are associated with longer holding periods; Panel C of Figure 2 shows that the distribution shifts to the right when comparing dividend recap targets to LBO targets overall. The mean is 6.5 years vs. 5.7 years. The rise of dividend recaps has coincided with longer holding periods more generally in PE. Before the Financial Crisis, the rolling 3-year average holding period was just under four years. Over the past four years since 2019, this has increased to about 5.5 years.

Summary statistics at the portfolio company and LBO deal level are in Table 1. These are collected at the time of the LBO, not the subsequent dividend recap for affected firms. The overall picture that emerges from Table 1 is that PE firms target larger and higher-quality firms for dividend recaps, and they ultimately have better outcomes on average albeit higher bankruptcy rates. The first set of variables on deal characteristics are primarily from Stepstone, except for deal size which is from Pitchbook.[11] Dividend recap targets are much larger than their counterparts in deal size and total enterprise value (TEV). Following the LBO, they have higher debt loads and higher gross profits. The average (median) debt-to-EBIDTA ratio is 3.9 (4.1), which is roughly in line with industry standards according to LCD.[12] A high debt load may discipline managers via the claim on cash flows and offers tax benefits (Jensen, 1986; Cohn et al., 2014).

The second set of variables on deal outcomes (all from Stepstone) describe outcomes for the overall deal, from entry to exit. They show that dividend recap targets have higher returns. For example, the average deal returns 2.6x times the initial investment (total value multiple, or TVM); for dividend recap targets, this is much higher at 3.7. Dividend recap targets also have much larger changes in average gross profit. Notably, they exhibit a 22% increase in Debt/Ebitda between the LBO and exit, vs. a 54% decline for other firms,

[10] For example, see https://www.reuters.com/breakingviews/private-equitys-short-termism-has-rising-cost-2022-06-23/ and https://www.familywealthreport.com/article.php?id=198731

[11] Note that Pitchbook reports deal size only for 12,400 of the 61,600 LBOs that we use from its database.

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

consistent with the dividend recap significantly increasing debt loads.

The third set of variables concern portfolio company outcomes. About 0.5% of the firms in our sample ever experience a bankruptcy after their LBO, but this reflects many firms not having time to experience exit outcomes. If we consider LBOs before 2015, observing them for at least eight years, the rate is 0.94%. Existing literature using small samples of mostly pre-Financial Crisis public-to-private deals has found higher rates of bankruptcy post-LBO.[13] Our sample is overwhelmingly private-to-private LBOs, where firms are smaller and less likely to file for bankruptcy rather than restructure. During our sample period, there were 20,000-40,000 business bankruptcies in the U.S. each year, which is 0.3%-0.7% of the roughly six million employer firms.[14] Therefore, the bankruptcy rate among LBOs in our sample is higher than the overall rate in the economy. The rate of IPO is 0.7%, which is also much higher than the overall rate in the economy. Both bankruptcy and IPO exhibit much higher means for dividend recap targets.

The final variables concern real outcomes from the Census-matched sample. We construct real outcome variables that are parallel to the bankruptcy and IPO analysis. PE-backed firms are relatively representative of the overall distribution of U.S. firms in their size, location, and industry composition. For example, The median PE-backed business employed 69 workers in 2022 (AIC, 2023). In our Census-matched dataset, the median is 110 (Table 1). Overall in the economy, about 98% of all employer firms have less than 100 employees, and these firms account for 32% of all private sector employment.[15] We have not yet disclosed from the U.S. Census data environment separate statistics for targets with and without dividend recaps because there are strict limits to the number of samples and the potential for implicit samples to create disclosure violations. We will do so in a future draft. For exit, we calculate whether the firm has exited as of four and six years following the LBO. These means are 16% and 19%, respectively.

For the continuous variables, we restrict the analysis to survivor firms that are observed from $t-1$ and $t+4$. The average firm in the data has about 1,300 employees in the data in the year before the buyout and 1,761 after the third year subsequently (where the buyout is year zero, so this is the fourth year after the buyout), conditional on surviving. The medians are much lower, at 110 and 243 employees respectively. The average (median) payroll is \$45 (\$7) million before the dividend recap year and \$52 (\$14) million in $t+3$. The average (median) wage is \$63 (\$53) before the dividend recap year and \$57 (\$56) million in $t+3$. The fact that average wages go down could be consistent either with greater unrealized equity-based compensation or with reducing costs by cutting wages at firms that were previously paying inefficiently high wages. Finally, average (median) revenue is \$392 (\$21) million before the dividend recap year and \$764 (\$158) million in $t+3$.

For our outcome variables, we construct growth relative to the year before the dividend recap. For

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

[14] See https://www.uscourts.gov/statistics-reports/caseload-statistics-data-tables and https://sbecouncil.org/about-us/facts-and-data/.

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

example, employment growth through the third year after the deal is defined as $\frac{(Emp_{t+3}-Emp_{t-1})}{Emp_{t-1}}$. The average in the data is 18% growth, with a median of 54%. Average payroll, wage, and revenue growth are 13%, -0.04%, and 39%. Therefore, on average following LBOs we see increases in firm growth and a slight decline in wages. We focus analysis on categorical variables capturing the nature of growth: Was this 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).

Panel B of Table 1 contains PE fund and firm variables. Funds pursuing dividend recaps tend to be larger, at about \$2 billion relative to \$1.3 billion at funds that never do a dividend recap on a portfolio company. At the firm level, we see that firms which undertake any dividend recaps tend to have more investments than firms that never do a dividend recap, but have lower assets under management (AUM). Next, summary statistics about the loans are in Table A1.[16] The average loan is for \$216 million and has a five-year maturity. It has a interest rate spread (over the benchmark rate) of 403 basis points and has a 16% chance of being covenant-lite. In terms of the stated purpose, 40% of loans specify LBO, 16% of loans specify Capital Investments, and 18% of loans specify Refinancing. Notably, 10% of loans specify Dividend Recap as their stated purpose. We also examine secondary market outcomes for loans. Panel C of Table 1 shows the change in price, bid-ask spread, and number of quotes for firms one and three month before and after the dividend recap transaction. There are no significant differences between the DR and the non-DR deals, except that the change in the number of quotes is smaller for DR deals.

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. We construct an instrument that 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 the CLO volume underwritten by the relationship bank cannot be independently related to the trajectory of the targeted portfolio company. This allows the chosen company to be the one most amenable to a dividend recap within the PE firm's portfolio. Since higher quality firms tend to be chosen for dividend recaps, bias from within-fund selection should push us to find more positive effects. However, we show that our results are similar when restricting analysis to PE portfolios with only two firms that could plausibly undergo a dividend recap.

In this section, we first explain how CLOs operate. Next, we describe why a relationship bank-underwritten new CLO would exogenously reduce the cost of credit for the PE fund and lead to an opportunistic divi-

[16] Altogether, the sample has 15,627 loans containing a total of 11,714 *pro-rata* tranches (which are typically held by banks) and 12,517 institutional tranches (which are typically held by institutional investors like the CLOs).

dend recap. We then present our specific empirical design. Finally, we present empirical evidence for this mechanism as well as the results from our first stage estimation.

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 the private credit wing of a large PE firm such as Carlyle or Blackstone, or a private lender such as Golub Capital.

The life cycle of a typical CLO fund is illustrated in Figure 3. At inception, the manager approaches a bank to obtain a line of credit, which she uses during a warehousing phase of about 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 pays off the line of credit and is used 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 for 5-6 years, after which the CLO winds down. The manager stops trading and maturing loans pay out remaining investors. This amortization phase can last between six and 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 exposure to any specific company or industry. CLOs purchase floating-rate, senior-secured term loans, and the debt securities they issue are also floating rate. Senior secured means that the loan is fully collateralized, requiring the company to have strong cash flows or other assets to serve as collateral. 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 seeking investment-grade assets. Insurance companies, pension funds, hedge funds and a range of other institutions around the world also purchase CLO securities. 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 through underpricing the debt tranches relative to their risk-adjusted performance, which ultimately benefits the equity tranche (Cordell et al., 2023).

Banks play crucial roles in the CLO market. First, almost all the loans that CLOs buy are syndicated, with a lead arranger bank who originates the loan in collaboration with a 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). Second, a bank also underwrites the CLO, which includes both arranging the contract terms and assessing the creditworthiness of the borrowers whose loans are being purchased. Importantly, the underwriting bank must approve every loan in the portfolio. If the bank has a relationship with the loan's PE sponsor, it is likely easier for the CLO to acquire the loan. To put this another way, the bank has more information about and is incentivized to place loans that it has originated, and the CLO manager in turn is incentivized to purchase these loans given the underwriting relationship with the bank. This relationship is at the heart of our identification strategy.

We construct the shocks for our instrumental variables analysis by combining the PE-bank relationships with information about CLO issuances, which includes the CLO's manager, portfolio, and underwriting bank. This allows us to quantify banks' CLO underwriting activity and CLO acquisitions of dividend recap loans. (As mentioned above, more details on these data, including the number of managers and banks, are in Appendix B.)

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. Simulatenously, 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*.

To construct measures of PE-bank relationships, we define a PE firm p as having a relationship with a bank b in month t if at least one company sponsored by p took a loan from bank b (as lead bank) during that month.[17] Thus, PE-Bank Relationship$_{p,b,t}$ is an indicator variable that is equal to one if the PE firm p and the bank b had a lending relationship at time t, and zero otherwise. We exclude dividend recap loans when calculating the PE-bank lending relationships. 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, reflecting the larger size of CLO underwriting banks.

The data support a PE-Bank-CLO channel as a driver of dividend recaps. Out of 782 dividend recap

[17]This makes use of the loan data from Dealscan and LCD, described in Appendix B. We use alternative definitions of lending relationships based on the loans issued by the sponsor p in past one year and past five years in our robustness tests.

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 their underwriter bank, we borrow a method from Bharath et al. (2011) and Chodorow-Reich (2014). Here, we use a stacked sample (as elsewhere in analysis), 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 month as the dividend recap loan was issued. We then estimate the following specification:

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

$\mathbb{1}(\text{Purchased by CLO})_{d(p),k(b,t)}$ 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 2 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 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, and it shows that PE-Bank relationships are important for dividend recap financing.

This mechanism 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 through a dividend recap. In contrast, an LBO involves a greater degree of selection. While there are no doubt cases in which changes in the cost of financing affect LBOs on the extensive and intensive margins (i.e., whether the deal is done and how much debt is used), the CLO channel is unlikely to be first order. Consistent with this, we do not observe a strong first stage for LBOs.

2.3 Instrumental Variable and Stacks

Next we describe the primary instrument for a dividend recap deal. For each PE firm, we calculate exposure to the CLO market by aggregating the CLO underwriting activity of all banks related to that PE. 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 CLO Volume$_{b,t}$). As described in Section 2.1, CLO managers purchase loans for a new CLO during the warehousing and the ramp-up phase. Thus, we only consider CLOs in these phases to capture banks' recent underwriting activity. Specifically, we use the CLOs for which month t falls within the period from 6 months before the closing date up to the effective date. Next, we aggregate CLO underwriting across the banks related to PE firm p and average it over the past 12 months to create the instrument, which we denote by R-Banks CLO Volume. The formula for this measure is in Equation 2.

$$\text{R-Banks CLO Volume}_{p,t} = \log(1 + \sum_b \mathbb{1}(\text{PE-Bank Relationship})_{p,b,t} \times \text{CLO Volume}_{b,t}). \qquad (2)$$

Here, $\mathbb{1}(\text{PE-Bank Relationship})_{p,b,t}$ equals one if the PE firm p and the bank b had a lending relationship at time t, and equals zero otherwise.

Thus, R-Banks CLO Volume$_{p,t}$ measures the amount of CLO underwriting done by all of p's relationship banks during the 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 A1. The average value of R-Banks CLO Volume$_{p,t}$ is 0.05 across our sample. Notably, the average value of the instrument is 0.31 among dividend recap deals and 0.05 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 to regression analysis (Baker et al., 2022). For each dividend recap target portfolio company in our dataset, we create a matched stack of control LBOs. When a company has multiple dividend recap deals, we use only the first. We also drop dividend recaps that are within a year of exit, because these tend to be part of the exit transaction. 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 the data permit.[18] 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 robustness tests, we show that the results are not dependent on the control sample. Note that causal identification is not based on this match, 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 shows that CLO underwriting by a PE firm's relationship banks increases the chance of a dividend recap at a firm in the PE's portfolio. The specification, which uses the stacked deal-level data

[18] 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. Pitchbook defines 40 industry groups. 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.

described above, is in Equation 3:

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

Here, $d(c,p,t)$ denotes an LBO deal where the PE firm p acquired the target company c during time t. 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. The instrumental variable R-Banks CLO Volume$_{p,t}$ is the total CLO volume underwritten by the PE firm p's relationship banks during the month t (expressed in logs). Stack fixed effects α_s compare the treated deal with the comparable set of control deals within the same stack.

Table 2 Panel B presents the results. Column (1) shows that one standard deviation increase in the instrument increases the likelihood of a dividend recap by 4 pp. The effect is economically large; it is 200% of the unconditional likelihood of a dividend recap (2%), and a one standard deviation increase in the instrument increases the likelihood of a dividend recap by 14 pp, which is seven times the mean. This result highlights that PE firms with a greater exposure to the CLO industry are more likely to do a dividend recap on their portfolio companies.

We test whether this first-stage result is robust to alternative definitions of the instrument. First, we vary how we define a relationship between the PE firm and the CLO underwriting banks. In our main measure, the PE-Bank relationship at time t is defined on the basis of all the non-dividend-recap loans taken by the PE firm p during the same month. We create alternative instruments (R-Banks CLO Volume (1-Yr)$_{p,t}$ and R-Banks CLO Volume (5-Yr)$_{p,t}$) by defining the PE-Bank relationship on the basis of all loans taken by the PE firm in the last one year and the last five years, and find consistent results (Columns (2) and (3)). Next, we use alternative measures of bank underwriting activity. Instead of the value of CLOs underwritten by the bank, we use the number of CLO deals underwritten by the bank and the entry of the bank into the CLO underwriting business. Columns (4) and (5) show that we get similar results with these alternative instruments.

To explore the timing of the relationship between PE firms' access to CLO funding and the chances of a dividend recap, we use a dynamic model. First, we aggregate the deal-level data to create a panel data at the PE-firm$\times$year-month level. We then create a PE-firm-level shock with a variable that is equal to one if the instrument (i.e., R-Banks CLO Volume) for p increased by more than 25% in a given month ($\mathbb{1}(\Delta\text{R-Banks CLO Volume}{>}25\%)_{p,t}$). We use a finite distributed lag model to account for delay between new CLO underwriting shocks and loan issuance, and include PE-firm (α_p) and year-month (α_t) fixed effects to absorb cross-sectional and aggregate time-series variation in our variables. The empirical specification is:

$$\mathbb{1}(\text{Dividend Recap})_{p,t} = \sum_{h=-12}^{12} \beta_h \times \mathbb{1}(\Delta\text{R-Banks CLO Volume}{>}25\%)_{p,t-h} + \alpha_p + \alpha_t + \varepsilon_{p,t} \tag{4}$$

Figure 4 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. After the shock, the new CLO access is associated with a 1 pp higher chance of a dividend recap by 1 pp each month. This increase is economically significant relative to the unconditional probability of 0.4%. It reverts to zero after about 12 months, consistent with CLOs acquiring loans during their first 12 months (see Section 2.1). In sum, we observe a strong first stage with dynamics that are consistent with the underlying economics.

The second stage is as follows, where $\mathbb{1}(\widehat{\text{Dividend Recap}})_{s,d(c,p,t)}$ is the predicted dividend recap from the first stage (Equation 3):

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

2.5 Instrument Assumptions and External Validity

An instrument must satisfy the relevance, exogeneity, and exclusion restriction assumptions to be valid. Above, we have documented relevance through a strong first stage with meaningful magnitude. There are three potential questions about our approach:

1. Could there be reverse causality where the dividend recap opportunity drives CLO creation, potentially violating the exogeneity assumption?
2. Are we focusing on an effect among low quality deals as the PE firm moves down its demand curve?
3. Are we focusing on an effect among high quality deals because the treated PE firm selects the best performer in its portfolio for a dividend recap?

Reverse Causality One concern is that a dividend recap opportunity could drive the creation of the CLO (i.e., reverse causation). This might occur if a bank seeks out CLOs to underwrite because a PE sponsor with whom the bank has a lucrative relationship has notified the bank of its interest in a dividend recap. There are three reasons why this is almost certainly not occurring, all of which emerge 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. As explained above, 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 4 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 main result is not about the effect of easy vs. tight credit, which has been studied descriptively in the existing literatures. 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 1.2). They are thus if anything higher quality than the control firms.

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. We cannot fully address this possibility. However, two tests suggest it is not a first order issue in our analysis. First, we restrict the sample of PE funds to those with few portfolio companies that are at risk of experiencing a dividend recap. As shown below, we observe effects on bankruptcy that are consistent with our main findings. This approach also restricts the sample to small PE firms, addressing any concerns that our results reflect something spurious about only large firms. We also 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. While our IV estimates have a causal interpretation within the complier population, it is possible that dividend recaps which depend on access to relatively cheap credit are 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 A13 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 Results: The Effect of Dividend Recaps on Key Stakeholders

This section presents the effects of instrumented dividend recaps on the relevant stakeholders: the target company, employees, investors, and pre-existing creditors.

3.1 The Target Company

The first stakeholder we consider is the firm. We primarily focus on the adverse outcome of bankruptcy, but also consider exit and the more positive outcomes of IPO and revenue growth.

Bankruptcy and Firm Exit Our most important outcome is bankruptcy; this is a measure of firm distress that imposes significant social costs (Bernstein et al., 2019; Dou et al., 2021; Antill, 2022) and is a measure that is observable for the whole sample. To the degree PE owners can better manage portfolio company distress by renegotiating with creditors or injecting new capital, we do not expect to see any significant effect of opportunistic dividend recaps on bankruptcy, especially given the discussion above about selection-within-the-portfolio biasing results in a more positive direction.

Instead, we show in Table 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. We observe significant effects at four, six, and ten year horizons. Our preferred estimate at six years of 17 pp is about twenty times the mean, a dramatic effect. Note that the mean in the stacked sample is lower than the mean in the overall sample because of selection into the matched sample. As explained in Section 1.2, dividend recap targets tend to be larger, higher quality firms, characteristics that in the overall sample are less associated with bankruptcy.

The effect on exit, using data from the U.S. Census Bureau, is reported in Table 3 Columns (3)-(4). It is also 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 do not have enough time to estimate a 10-year horizon and the results are noisier for the six-year horizon. Note also 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 A9, are similar to the main estimates. In sum, consistent with higher risk and difficulty servicing additional debt, we find a strong negative effect of cheap credit-induced leveraged payouts on the firm.

IPOs and Firm Growth If failure and bankruptcy represent the bad end–i.e., the left tail–of possible firm outcomes, exit to public markets via an IPO and firm growth represent the opposite right tail of good firm outcomes. Dividend recaps may increase the overall risk level and permit longer holding periods as well as other benefits of debt in the PE model, such as more disciplined management. In this case, we might also expect to see a positive effect on right-tail outcomes. We see this in Table 4, where we first consider the chances of the firm having an IPO over four-, six- and ten-year horizons from the dividend recap deal (Panel A). We see a dramatic, positive effect that is the same order of magnitude relative to the man as the bankruptcy result. Over the six-year horizon, the effect is about 80 times the mean (Panel A column (3)).

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. First, as explained in Section 1.2 and Appendix C, we require a firm's panel to be fully populated across the four years between $t-1$ and $t+3$ to calculate growth statistics (here, growth is defined as $\frac{(Rev_{t+3}-Rev_{t-1})}{Rev_{t-1}}$). There would be a large mechanical negative effect on growth measures if we included the whole sample, since there is such a large effect on exit. Also, as explained in Appendix C, revenue is only available for a subset of firms in the LBD, reflecting how the Census Bureau collects data from tax forms. These two restrictions considerably constrain the sample. With that said, we observe a positive coefficient for average growth in Table 4 Panel B Column (1). The four categorical outcome variables reflecting very poor, poor, good, and very good outcomes are in the subsequent columns. We see that the chances of the first three seem to decline, 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 4 point to a fat right tail of good outcomes from dividend recaps that mirrors the fat left tail documented above.

3.2 Employees

The next stakeholder we turn to is employees. Here we consider growth in employment, payroll, and average wages among survivor firms, using data from the U.S. Census Bureau and analogous growth measures as described above for revenue. The results are in Table 5. 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 percentage points (pp). Since only 4.5% of firms have such a bad outcome (see mean at the bottom of the table), this is a dramatic effect. We also see 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. Finally, we see a large positive but insignificant effect on high growth outcomes, of more than 75% (Column (5)). Overall, these results are consistent with dividend recaps increasing firm risk and generally reducing employment, in particular via very large contractions, which parallels the bankruptcy and exit results.

Next, Panel B conducts the same analysis for payroll growth. We see a similar pattern where 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, pointing to an overall 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)).

3.3 The Investors

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, as in the anecdotes described above in the Introduction. To analyze deal-level returns, we use the Stepstone SPI data and construct variables that parallel those used above for real outcomes, allowing us to observe average and distributional effects.

The results are presented in Table 6. Measured both with IRR (Panel A) and TVM (Panel B), we find that dividend recaps appear to 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. We see a decline in the merely good outcomes of IRR between zero and 20% or TVM between one and two times the investment (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 6. Consistent with leveraged payouts enabling longer holding periods, dividend recaps increase the 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 debt relative to Ebitda. This is what we would expect given that substantial new leverage is being used to generate returns. We also see a large but insignificant increase in total debt (Column (4)).

Fund-Level Returns We next turn to financial returns at the fund level, which is the outcome of interest to the LPs who invest in PE. The results, using data from Burgiss, are reported in Table 7.[19] We begin with the IRR in Panel A. There is no significant effect on average IRR, though the coefficient is negative (Column (1)). For TVM and PME, we see large, negative effects; TVM declines by 87% 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 at all. In the subsequent columns of each panel, we report the distributional results, which paint a consistent picture. Dividend recaps increase the chance of a relatively poor outcome of zero to 20% IRR, 1-2x multiple, and 1-2x PME (Column (3) of Panels A, B, and C). They reduce all other outcomes. For example, they reduce the chance of a 2-4x multiple (which comprises almost 40% of the sample) by 94 pp (Panel B Column (4)).

The negative effects on fund returns are perhaps surprising given the positive coefficients at the deal level from Table 6. The pattern does not reflect different selection of deals into the Stepstone- and Burgiss-matched samples. We find very similar results in the Stepstone-matched sub-sample of the Burgiss sample (Table A11). Furthermore, in the Stepstone data, when we aggregate the return to deals within a fund, we find similar results as in the Burgiss data (Table A12). In other words, although the dividend recap if anything increases the deal-level return, it reduces the fund-level return.

3.4 The Creditors

The final stakeholder we consider are the portfolio company's lenders. The main lenders for both LBOs and dividend recaps are banks and private lenders. As discussed above, the dividend recap loans are typically packaged together and securitized in CLOs. Unfortunately, loan-level performance is unavailable from LCD. Cordell et al. (2023) show that CLOs perform well, and they are highly diversified. Therefore, we do not expect that dividend recaps will meaningfully impact CLO performance, especially once any additional risk has been incorporated via the spread, which we consider below. Instead, pre-existing creditors of the portfolio company may lose out. We consider them in the second analysis below.

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

Loan Spreads We expect that if dividend recaps increase firm risk, they will be accompanied by higher interest rates relative to loans taken for other purposes, such as to finance positive NPV projects, where the future cash flows from the project would reduce the risk of bankruptcy stemming from higher indebtedness. We verify this conjecture by studying loans associated with LBOs and dividend recaps in our sample (drawn from Dealscan and LCD data). For each loan, we observe information on borrowers, lenders, and the PE sponsors, as well as the loan purpose (LBO, capital investments, dividend recap, etc.) and contractual terms (spreads, covenants, etc.). We use the following OLS specification:

$$\text{Loan Spread}_{l(p,b,t)} = \mathbb{1}(\text{Dividend Recap})_l + \alpha_p + \alpha_b + \alpha_t + \varepsilon_l \tag{6}$$

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. $\mathbb{1}$(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 reported in Table 9 Panel A. Note that as described above, this regression is cross-sectional at the loan level. We observe a sample of 24,202 loans, of which 11.7% (2,808) are for the purpose of a dividend recap. Column (1) shows that 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 several loan characteristics (size, maturity, and covenant-lite status of the loan) that may affect the spread but find similar results. These results indicate that PE firms have to pay higher interest when they take a loan to pay dividends to themselves. This is consistent with the notion that dividend recap loans are riskier and burden the firms with higher interest expenses, which in turn further increases the chance of distress.

Preexisting Creditors The large effect of dividend recaps on bankruptcy suggests that these deals could shift value away from pre-existing creditors. Typically, covenants in the pre-existing loans and bonds covenants would restrict dividend recaps. Covenants are conditions on the borrower's activities during the life of the loan; for sample, a debt service coverage ratio covenant requires the borrower to maintain funds to cover all debt payments. Covenants can also limit new debt issuance. Observing a dividend recap with existing loans outstanding implies one or more of three things: First, the company 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 data, we observe secondary market trading for pre-existing loans. We study them using our baseline approach of stacking LBOs matched to the focal dividend recap target. Each observation is therefore at the stack-deal level. 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. This is a very useful exercise because if we see a negative impact on creditors in an OLS model, it is almost certainly much worse in a causal model, since dividend recap targets are higher quality than the average PE-owned firm (see Section 1.2).

The results are presented in Table 9 Panel B. The first and most important outcome is price. Using windows of one and three months on either side of the dividend recap, we see a negative association between a dividend recap and the percent change in price (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)). The sample is somewhat larger for number of quotes because it can be zero. In sum, this analysis shows that even for the average dividend recap—where, as we will show below, there is only weak evidence of higher rates of bankruptcy—there is value-shifting away from pre-existing creditors.

4 Mechanisms

Why do dividend recaps negatively effect firm outcomes? Why do GPs undertake them? And how can they increase deal returns yet reduce fund returns? In this section, we seek to answer these questions. Taken together, our evidence suggests that dividend recaps cause the GP's incentives to diverge from the interests of current fund investors, portfolio company employees, and creditors. A leveraged payout delivers cash to the fund, incentivizing the GP to raise a new fund on the basis of good interim returns. After raising a new fund, the GP—whose attention is limited—prioritizes the new fund at the expense of the old, reducing returns for the current fund. 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 created additional debt that was 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 flesh out each step in this moral hazard story with new analysis and support from the literature.

Paying out the Dividend Recap via Distributions. We must 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 8 Columns (1)-(2), we show that the dividend recap has a large causal effect on distributions to the fund in the first and second quarters following the dividend recap quarter. The effect of about 10 on log distributions is very large relative to the mean, implying a more than 20,000% increase. This reflects the fact that most quarters have no distributions, while quarters following complier dividend recaps almost always have them. We also directly test for new LBO launches after gthe dividend recapitalization transaction in Appendix table A8. We find that funds with DR transactions do fewer LBO transactions in the next 2 and 4 year period, also going against the recycling story.

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 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. Overall, 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, likely via the distributions channel documented above. In Columns (3)-(4) of Table 8, we report the causal effect on the extra 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 for 1 quarter, consistent with GPs needing time to close a new fund. We find a large effect of about twelve times the mean in column 6 for extra new funds launched in the subsequent year. These results suggest that bringing forward distributions via dividend recaps enables opportunistic fundraising.

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, the negative impacts of dividend recaps on target portfolio companies is some evidence of inattention or less prioritization, especially given existing evidence that in general PE owners have expertise managing firms through distress (Hotchkiss et al., 2021). Second, we find in Table 8 Columns (5)-(6) that dividend recaps reduce returns in subsequent LBOs in the 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). Third, we also test for the change in number of deals in Appendix table A8. We find that funds with DR transactions do fewer LBO transactions in the next 2 and 4 year period, consistent with GPs paying lower attention to these funds. In sum, these results suggest that GPs reduce attention to the current fund after a leveraged payout in the middle of the deal lifecycle. However, an important caveat is that we do not assess welfare for any stakeholder. While dividend recaps lead LPs to earn lower fund returns, they may benefit from early liquidity.

Higher 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, so they cannot be used to fund NPV positive investments that might offset the additional costs of higher leverage. Moreover, as shown in Section 3.4, dividend recap loans have higher interest rates than other loans to PE-owned companies, which further increases the burden of servicing the additional debt and adds to the chances of distress. Overall, the firm has a new regular obligation that depletes free cash flows with no meaningful commensurate benefit to counteract this new cost. Moreover, the PE managers have no incentive to avoid distress because they already have their return. The result is more bankruptcy.

At the same time, firm executives and fund managers have an incentive to take more risks with the firm. This is a standard outcome of more leverage and also reflects the investment being more like a call option than it was before because the GP's downside is covered, as he has already earned at least some return on equity. Greater risk taking manifests in the fatter tailed distributions we observe in outcomes across Tables 3-6. We see strong evidence of very poor outcomes—most strikingly bankruptcy and lower wages conditional on surviving—but also a higher chance of very good outcomes, such as an IPO. Higher risk need not be bad for investors *per se*, but it is likely bad for employees and creditors (and, generically, any risk-averse stakeholder). First, the negative outcomes of exit and bankruptcy are much more common than IPO. Second, they have much worse consequences for employees. Employees of bankrupt firms face frictions finding a new job and lose lifetime earnings (Berk et al., 2010; Graham et al., 2023). This is compounded by the negative effect on wages among surviving firms.

An alternative possibility is that opportunistic dividend recaps are lower quality and reflect excessively lax screening on the part of the underwriting bank. However, we find that if anything, dividend recaps with high values of the instrument are higher quality (see Section 2.5). 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. 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.

In sum, our evidence suggests that leveraged payouts give rise to incentive misalignment, leading to moral hazard problems. When GPs extract early returns from a portfolio company, they are less incentivized to create value in the company in order to accomplish a strong exit, and instead take excessive risk. Further, the dividend recap's distributions create good interim returns, motivating the GPs to raise new funds. As a result, they reduce effort in the current fund, which experiences muted further LBO activity and lower returns.

4.1 OLS Results: The Role of Selection

In this section we present and discuss OLS effects, which differ markedly from the causal estimates. Generally, this reflects the selection bias that is easily seen from Table 1: targets of dividend recaps are much larger and more profitable, making them less likely to experience deleterious outcomes of additional debt. Consistent with this, they have much higher increases in profit and returns on average (second set of variables in Table 1).

The OLS results reflect these patterns. First, Table A3 shows that dividend recaps are associated with about a 1 pp increase in the chance of bankruptcy, which is about five times the mean, though it is much lower than the IV estimate. This points to risk increasing. However, consistent with a strong selection effect and the smaller magnitude of the IV effect on exit relative to bankruptcy, in columns 4-5 we see that there is a negative OLS relationship between dividend recaps and subsequent exit. We see a strong positive relationship for IPOs (columns 6-9), and a positive but insignificant relationship for revenue growth (Panel B).

In Table A4, we turn to employee outcomes. There are positive associations between dividend recaps and both 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 A5. Again consistent with Table 1, dividend recaps are associated with higher average IRR and TVM (column 1 of Panels A and B). 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 the "good" deals with 20-40% returns rather than the "great" deals with more than 40% returns (Panel A). For TVM, the positive relationship is driven by the tails of the distribution. Finally, we see that the holding period and debt relative to EBITDA increase substantially on average, while consistent with the null effect for revenue, we do not see a chance in gross profit.

Last, we present the OLS results for distributions and subsequent fund launches in Table **??**. As in the IV analysis, we observe a positive average effect on distributions (columns 1-2), consistent with managers using the average dividend recap at least in part to deliver cash to fund investors. The magnitude is again much smaller than in the causal model. However, in columns 3-4, we do not see any effect on launching new funds. Together with the other results, this suggests that the timing of the average dividend recap is tied more to company fundamentals than to fundraising strategy. In contrast, dividend recaps induced by cheaper capital enable the managers to raise new funds.

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 (bankruptcy), but it is much more muted. PE firms are not–as they are sometimes accused–using dividend recaps as a means to drive firms toward failure and profit along the way. Instead, there is strong selection of good deals into dividend recaps, helping to explain why creditors are willing to lend for this purpose.

5 Robustness Tests

We present a range of robustness tests, focusing on the bankruptcy result because it is our primary outcome and is estimated on the full sample. 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 are open to parsimonious requests in a revision.

The first test assesses whether the effect on bankruptcy is replicable in the other key subsamples with limited matches to the main sample. The results are in Table A9. It shows that effect of bankruptcy over the six-year horizon is quite similar and statistically significant in the Census Bureau-matched dataset that we use to analyze real outcomes sample, in the Stepstone-matched sample that we use to analyze deal returns, and the Burgiss-matched sample that we use to analyze fund returns. We also show, as mentioned above, that the fund returns analysis is robust to estimation on the Stepstone-matched subsample (Table A11), and the Stepstone deal-level data when aggregated yields similar fund-level results as the Burgiss data (Table A12).

Further robustness tests of the bankruptcy result are in Table A10. In this draft, we minimize reporting of Census-matched results.[20] The first test is to ensure our result is robust to alternative instruments. In Panel A columns (1)-(4), we employ the four alternative instruments that we also reported in the first stage analysis (Table 2). We find similar results with all of them, indicating that the result is not a spurious result of the particular way we construct the CLO activity of the relationship bank.

Next, we adjust the stacking algorithm to change the set of control firms, which also changes the size of each stack and thus the overall sample size. First, in Panel A column (5), we use sector (where Pitchbook provides eight sectors) rather than the 40 industries. Here and in what follows, all other requirements remain intact from the baseline model. Second, in column (6) we use a three rather than one-year window around

[20]Unfortunately, the U.S. Census Bureau is imposing increasingly narrow restrictions on disclosure, including on the overall number of results and especially around different samples. Therefore, we minimize reporting of the Census results, but can produce desired additional results from that sample in future drafts as requested (essentially, we maintain ability to do more tests in the future with minimal reporting in this draft).

the LBO. Third, in Panel B we omit three types of variables from the matching process: Deal size (column (1)), PE firm assets under management (column (2)), and PE firm age (column (3)). In all cases, the results are qualitatively similar to the main model, with dramatic positive effects on bankruptcy.

Last, 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 sheds light on whether selection within the portfolio matters to our findings. 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 2), and that are in the same industry as the dividend recap. Banks—even large ones—typically specialize in lending to certain industries, and they may transmit the credit-shock selectively in the industry of the DR (Blickle et al., 2023). When the number of portfolio companies meeting these requirements is larger than two for a given dividend recap, we remove that company and its stack from our analysis. We also limit the sample to PE firms with only three, four, or five portfolio companies in this category. The results are in columns 4-7 of Panel B. 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.

6 Conclusion

This paper offers 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 the proceeds of the debt are paid to the PE fund as returns to equity. We focus on dividend recaps, the most readily observed of these transactions. The media has vilified leveraged payouts as an extreme form of asset-stripping, representing the “worst" of an extractive sector (see examples in the Introduction). Yet it is not obvious that they will have negative effects; first, if they typically cause distress, creditors would be unlikely to offer affordable loans 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.

Our analysis not only represents the first systematic, causal analysis of leveraged payouts, but also 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. Finally, our analysis sheds light on an indirect effect of the burgeoning CLO industry.

We first document that PE funds tend to target large, healthy portfolio companies for dividend recaps. While these targets experience higher subsequent bankruptcy rates on average relative to other portfolio companies, they do not experience other more negative outcomes. To address the selection of high-quality firms, we instrument for dividend recaps using CLO volume underwritten by PE firms’ relationship banks

Our main finding is that cheap credit-induced dividend recaps increase firm risk and have negative implications for investors, creditors, and employees. In particular, the causal analysis shows that dividend recaps dramatically increase the chance of bankruptcy and firm exit, while also increasing the more positive outcomes of IPOs and especially high revenue growth. For employees, the effects appear largely negative, with employment and payroll growth appearing to fall, driven by realizations of large contractions. Wage growth falls substantially, by over 50%. On the investor side, the increased risk from the real outcomes is paralleled by wider dispersion in deal returns. Although DRs increase deal returns, they reduce fund returns. Managers seem to make use of higher interim to returns to raise new funds, focusing less on the current fund. Finally, DRs reduce employee wages and preexisting loan prices. Our findings imply that rising CLO demand will increase opportunistic DRs, with negative implications for portfolio company stakeholders such as employees, creditors, and fund investors.

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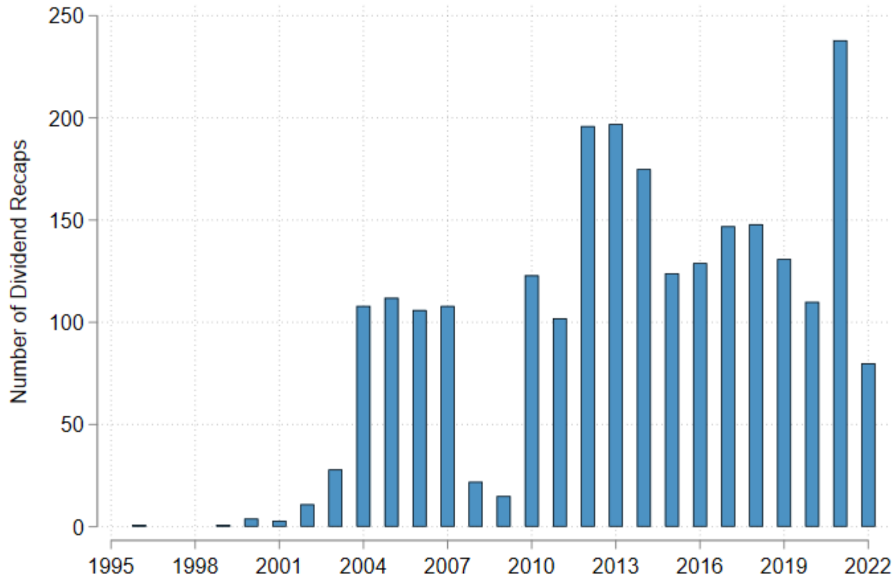
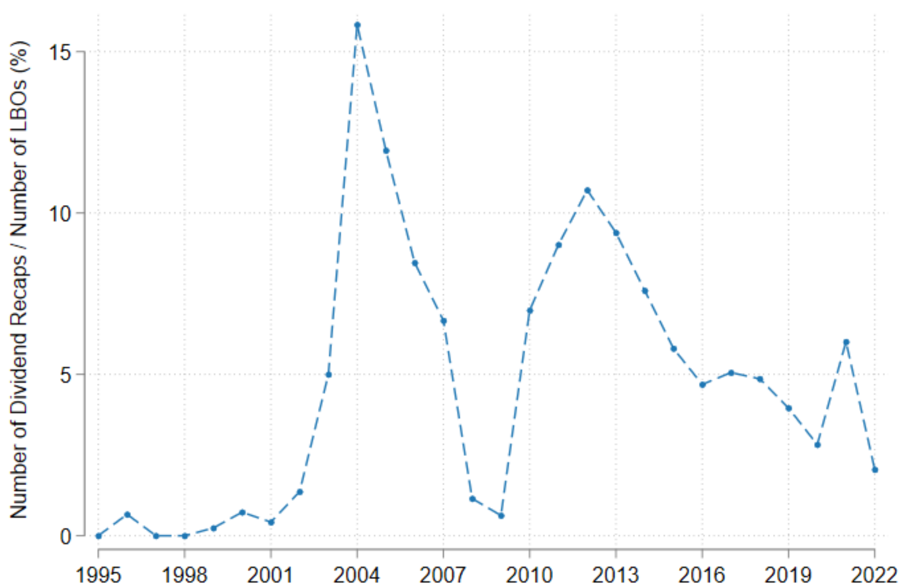
Figure 1: **Dividend Recaps Over Time**

Panel (A): Number of Dividend Recaps by Year



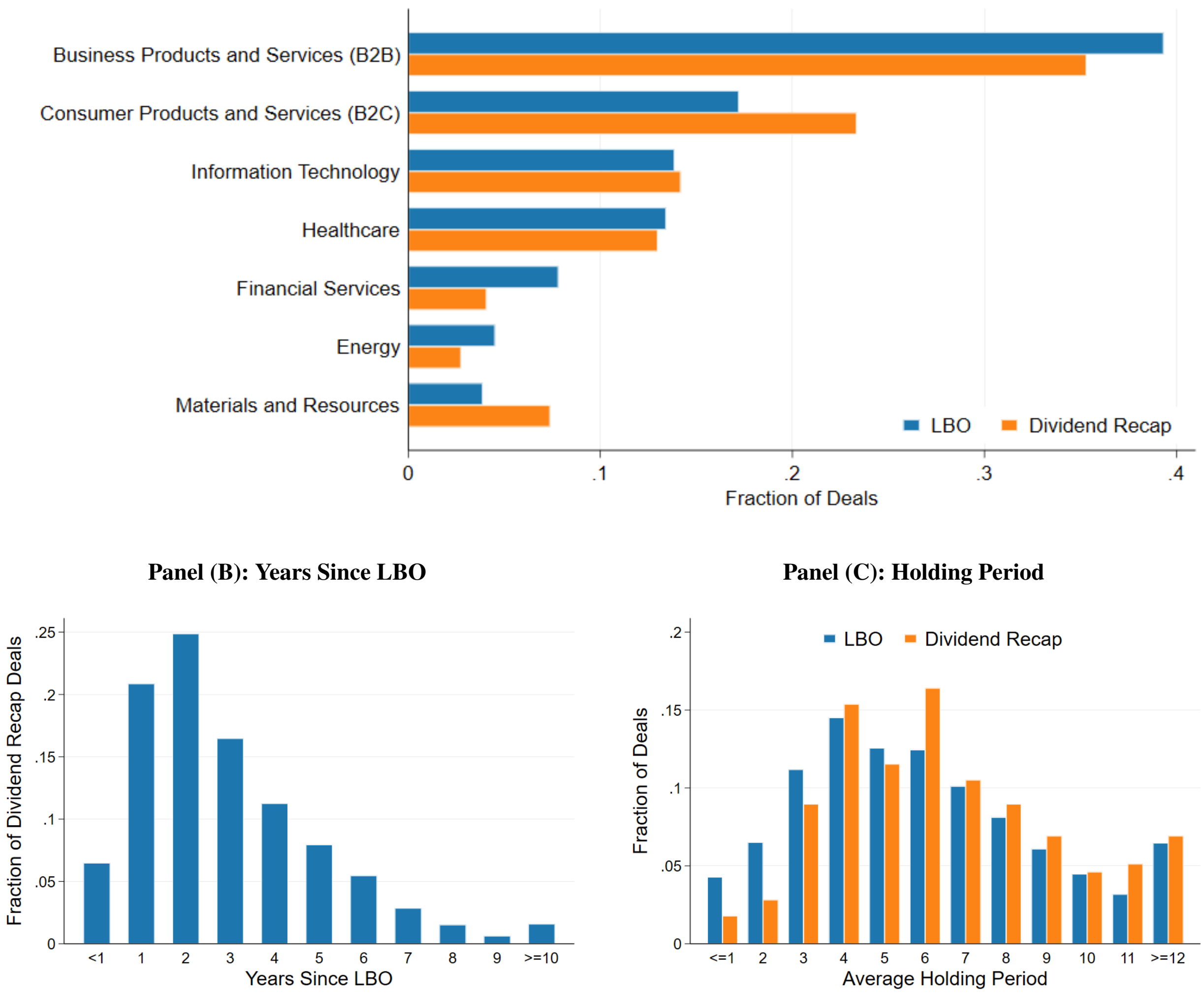
Panel (B): Dividend Recaps as Fraction of LBOs$_t - 2$



Notes: Figure 1 shows the trend of dividend recapitalizations over the years. Panel (A) shows the number of dividend recap loans by year. Panel (B) shows the number of dividend recap loans each year scaled by the number of LBO deals executed two years ago. We scale by LBO deals two years ago because most dividend recap loans are taken two years after the LBO.

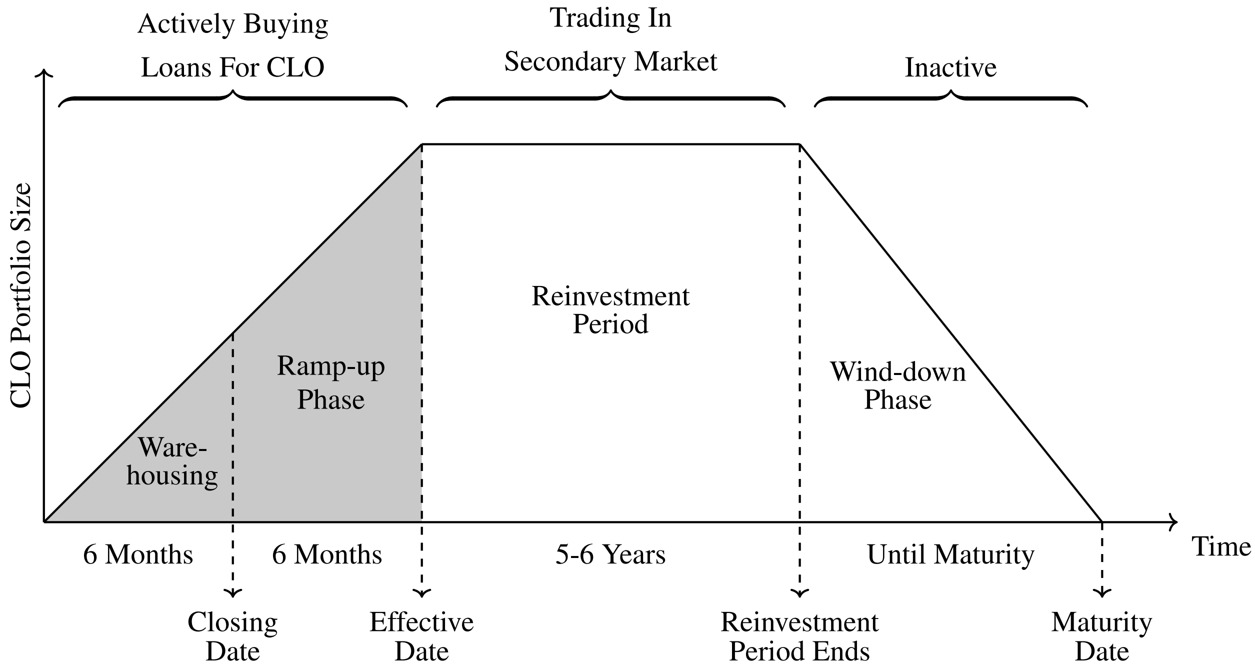
Figure 2: **Cross-sectional Differences in Dividend Recaps**

Panel (A): Industry Sectors



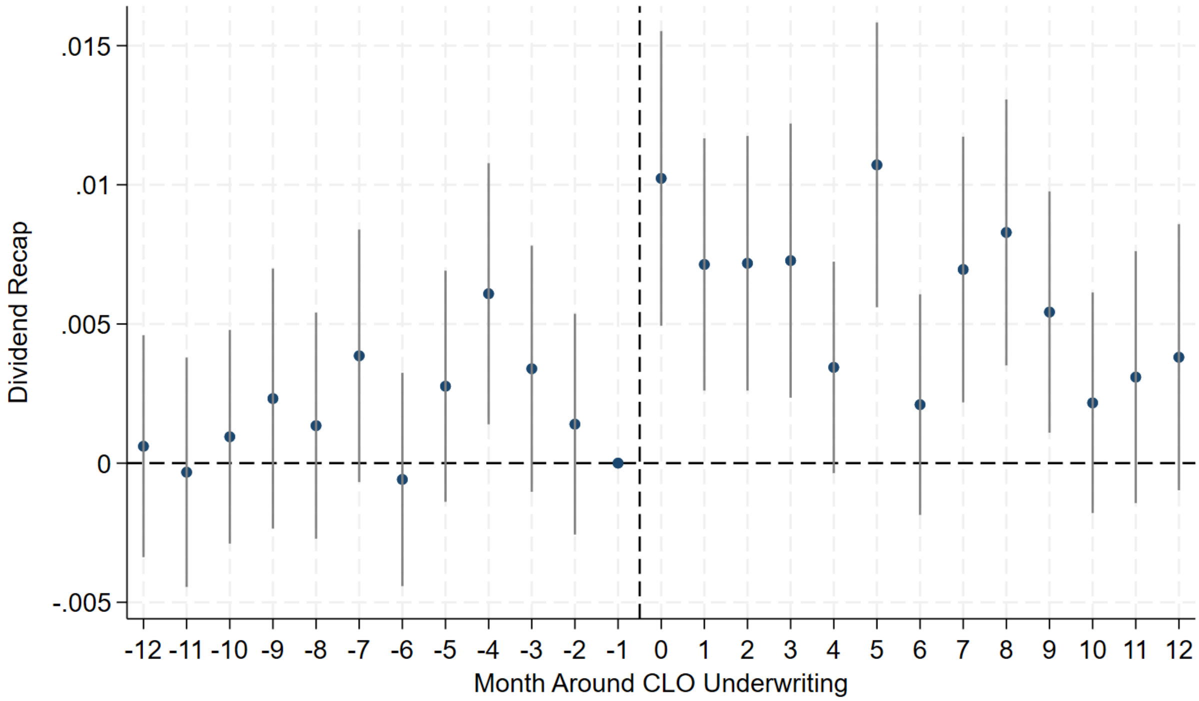
Notes: Figure 2 shows the cross-sectional distribution of all LBO deals and deals with dividend recaps. Panel (A) shows the distribution across the broad industry sectors. Panel (B) shows the distribution of duration between dividend recap loan and corresponding LBO deal date in years. Panel (C) shows the distribution of holding period (in years) for all LBO deals and deals with dividend recap.

Figure 3: **Life Cycle Of A CLO**



Notes: Figure 1 describes the life cycle of a typical Collateralized Loan Obligation (CLO). The X-axis plots the time since the CLO starts its operation and the Y-axis plots the size of its portfolio over time. CLOs actively purchase loans to fill their portfolio starting 6 months before the closing date up until the effective date. After that, the CLO 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". For each PE-firm, we use the total volume of active CLOs underwritten by their relationship banks as a proxy for CLO demand for DR loans.

Figure 4: **Effect of CLO Underwriting on DR issuance: Dynamic Specification**



Notes: Figure 4 shows how CLO underwriting of relationship banks affects a PE's probability of taking a DR loan. We plot the regression coefficients obtained from a distributed lag model given by Equation 4. The dependent variable is an indicator that is equal to one if PE-Firm p took a dividend recap loan in month t, and zero otherwise. The dependent variable of interest is an indicator that equals one if the value of CLOs underwritten by p's related banks increased by 25% or more in month $t-h$, and zero otherwise. We control for PE-Firm and monthly date fixed effects. Standard errors are clustered at the PE-Firm level.

Table 1: **Summary Statistics: LBOs With and Without Dividend Recaps**

	All				DR		Non-DR	
	N	Mean	Median	SD	N	Mean	N	Mean
Panel (A): Portfolio Company and Deal-Level Variables								
Portfolio Company Outcomes								
Full Sample								
Bankruptcy (%)	61,628	0.47			1,572	1.34	60,056	0.45
IPO (%)	61,628	0.66			1,572	5.47	60,056	0.53
Census Sample								
Exit (4-Yr) (%)	24,500	15.90						
Exit (6-Yr) (%)	24,500	18.50						
Census Sample (Survivors Only)								
Employment$_{t-1}$	7,700	1,313	110	5,109				
Employment$_{t+3}$	7,700	1,761	243	6,182				
Payroll$_{t-1}$	7,700	44,690	6,942	126,700				
Payroll$_{t+3}$	7,700	52,170	14,400	142,700				
Wage$_{t-1}$	7,700	63	53	31				
Wage$_{t+3}$	7,700	57	56	31				
Revenue$_{t-1}$	3,600	391,900	20,550	1,637,000				
Revenue$_{t+3}$	3,600	764,000	158,000	2,323,000				
ΔEmployment$_{t-1,t+3}$	7,700	0.18	0.54	0.12				
ΔPayroll$_{t-1,t+3}$	7,700	0.13	0.57	0.11				
ΔWage$_{t-1,t+3}$	7,700	-0.04	0.38	-0.03				
ΔRevenue$_{t-1,t+3}$	3,600	0.39	0.66	0.55				
Deal Characteristics								
Deal Size ($, Millions)	12,408	487.29	100	1,681.63	743	755.1	11,665	470.23
TEV ($, Millions), Entry	3,885	499.64	143	1,385.42	523	734.8	3,362	463.06
Debt/Ebitda, Entry	3,706	3.93	4.12	2.59	513	4.18	3,193	3.89
Debt/TEV (%), Entry	3,813	42.69	47.94	29.46	517	47.29	3,296	41.97
Gross Profit (%), Entry	3,835	19.61	18.81	82.65	523	23.04	3,312	19.07
Deal Outcomes								
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)	2,267	5.96	6	3.06	337	6.46	1,930	5.87
Δ Gross Profit (%)	1,557	-0.39	0.59	9.49	257	2.25	1,300	-0.91
Δ Debt/Ebitda	1,531	-0.41	-0.72	4.74	261	0.22	1,270	-0.54

Continued on next page

Table 1 – continued from previous page

	All				DR		Non-DR	
	N	Mean	Median	SD	N	Mean	N	Mean
Δ Log(Debt) (%)	1,259	27.32	20.7	84.77	232	62.33	1,027	19.41

Panel (B): PE Firm- and Fund-Level Variables								
PE Fund Variables								
Fund Size ($, Billions)	3,954	1.47	0.51	2.77	772	2.06	3,182	1.33
No. of Investments	4,230	24.94	14	35.72	790	46.84	3,440	19.91
Total Value Multiple	1,888	1.77	1.64	0.78	574	1.96	1,314	1.69
Public market Equivalent	1,888	1.23	1.16	0.5	574	1.31	1,314	1.19
IRR (%)	1,886	16.75	14.53	20.19	574	18.48	1,312	15.99
PE Firm Variables								
Age (Years)	1,150	31.03	25	27.59	427	28.95	723	32.25
No. of Investments	1,221	138.4	53	266.41	432	248.86	789	77.91
AUM ($, Billions)	862	68.77	2.42	455.5	372	39.33	490	91.11

Panel (C): Secondary Loan Market Outcomes								
Loan Variables								
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 leveraged buyout deals in our sample. We also show the key statistics separately for LBOs that featured a dividend recap transaction and the other LBOs that did not. The first set of variables correspond to deal characteristics. The second and the third set of variables correspond to the PE fund and the PE firm, respectively. The last three sets of variables are related to the portfolio company, deal outcomes, and fund outcomes, respectively.

Table 2: **First Stage Analysis**

Panel (A): Effect of PE-Bank Relationships

	Purchased by CLO	
	(1)	(2)
PE-Bank Relationship	0.011***	0.011***
	(0.001)	(0.002)
PE FE	Y	Y
CLO FE	Y	
CLO × Year FE		Y
CLO × Industry FE		Y
Obs	393513	393513
Adj. R-squared	0.117	0.147
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				0.13***	
				(0.02)	
R-Banks CLO Underwriting					0.45***
					(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. 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 3: **IV Effect of Dividend Recaps on Bankruptcy and Exit**

	Bankruptcy			Exit	
	4-Year	6-Year	10-Year	4-Year	6-Year
	(1)	(2)	(3)	(4)	(5)
Dividend Recap	7.40*	17.03***	15.51**	46.79***	33.49*
	(4.35)	(5.77)	(6.86)	(17.85)	(18.6)
Stack FE	Y	Y	Y	Y	Y
Obs	53539	53539	53539	24500	24500
Y-Mean	0.52	0.75	0.95	15.9	18.5
F-Stat	69.6	69.6	69.6	45.16	45.16

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 columns (1), (2), and (3), the outcome variables are bankruptcy over a 4-year, 6-year, and 10-year horizon, respectively. In 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. 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 IPO and Revenue Growth**

	IPO		
	4-Year	6-Year	10-Year
	(1)	(2)	(3)
$\mathbb{1}$(Dividend Recap)	5.63**	10.00***	11.53***
	(2.24)	(3.38)	(3.50)
Stack FE	Y	Y	Y
Obs	53539	53539	53539
Y-Mean	0.09	0.12	0.13
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 6 year, 8 year, 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 5: **IV Effect of Dividend Recaps on Employees among Survivor Firms**

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

	Average	𝟙[<-75%]	𝟙[-75,0%]	𝟙[0,75%]	𝟙[>75%]
	(1)	(2)	(3)	(4)	(5)
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	𝟙[<-75%]	𝟙[-75,0%]	𝟙[0,75%]	𝟙[>75%]
	(1)	(2)	(3)	(4)	(5)
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	𝟙[<-75%]	𝟙[-75,0%]	𝟙[0,75%]	𝟙[>75%]
	(1)	(2)	(3)	(4)	(5)
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 6: **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)	1.00*	0.98*	-3.21***	1.13*	1.10**
	(0.60)	(0.57)	(0.94)	(0.66)	(0.53)
Stack FE	Y	Y	Y	Y	Y
Obs	29144	29144	29144	29144	29144
Y-Mean	0.25	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)
	(1)	(2)	(3)	(4)
𝟙(Dividend Recap)	12.77**	-0.11	23.01**	1.08
	(6.25)	(0.21)	(10.18)	(1.37)
Stack FE	Y	Y	Y	Y
Obs	16842	11975	11321	8837
Y-Mean	5.75	-0.00	-0.32	0.30
F-Stat	15.47	17.91	18.07	12.14

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 7: **IV Effect of Dividend Recaps on 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)	-6.54	-0.71***	1.79***	-0.65***	-0.42***
	(6.25)	(0.17)	(0.40)	(0.24)	(0.14)
Stack FE	Y	Y	Y	Y	Y
Obs	12,295	12,481	12,481	12,481	12,481
Y-Mean	17.30	0.04	0.60	0.32	0.05
F-Stat	31.27	31.98	31.98	31.98	31.98

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)	-1.78***	-0.72***	2.12***	-0.94***	-0.47***
	(0.53)	(0.17)	(0.45)	(0.29)	(0.14)
Stack FE	Y	Y	Y	Y	Y
Obs	12,296	12,481	12,481	12,481	12,481
Y-Mean	1.95	0.04	0.55	0.38	0.03
F-Stat	31.27	31.98	31.98	31.98	31.98

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)	-0.52**	-0.09	0.60**	-0.21*	-0.30***
	(0.24)	(0.22)	(0.26)	(0.12)	(0.11)
Stack FE	Y	Y	Y	Y	Y
Obs	12,296	12,481	12,481	12,481	12,481
Y-Mean	1.26	0.28	0.65	0.05	0.02
F-Stat	31.27	31.98	31.98	31.98	31.98

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 8: **IV Effect of Dividend Recaps on Distributions, Fund Launch, and Peer Deal Returns**

	Log(1+Distributions)		Δ New Funds Launched		Within-Fund Peer Deal IRR	
	1-Quarter	2-Quarter	1-Quarter	1-Year	Pre-DR	Post-DR
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(Dividend Recap)	9.9499***	9.8987***	0.307	0.743**	0.03	-4.66***
	(0.005)	(0.005)	(0.210)	(0.356)	(0.41)	(1.65)
Stack FE	Y	Y	Y	Y	Y	Y
Obs	24,004	23,999	75,923	75,923	28715	4571
Y-Mean	2.84	3.37	0.055	0.062	0.26	0.33
F-Stat	27	29	64.7	64.7	26.59	13.72

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 logarithm of one plus total distributions (in$ millions) made by PE-Fund f in 1 and 2 quarters after time t for columns (1)-(2), the change in the count of new funds launched by PE-Firm p in 1 and 2 quarters after time t compared to 1 and 2 quarters before time t for columns (3)-(4), and 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). We control for stack fixed effects. We control for the logarithm of one plus total distributions (in $ millions) by fund f in 1 and 2 quarters before the DR in columns (1)-(2). Standard errors are clustered 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)
$\mathbb{1}$(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

	ΔPrice		ΔBid-Ask		Δ # Quotes	
	[-1,+1]	[-3,+3]	[-1,+1]	[-3,+3]	[-1,+1]	[-3,+3]
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(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)). 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.

ONLINE APPENDICES

Appendix A Supplementary Tables and Figures

Table A1: **Summary Statistics of Instrument and Loan Variables**

	All				DR		Non-DR	
	N	Mean	Median	SD	N	Mean	N	Mean
PE-Firm Variables								
R-Banks CLO Volume	173,798	255	0	977	1,292	841	172,506	251
R-Banks CLO Volume (1-Yr)	173,798	2,373	0	8,052	1,303	6,984	172,495	2,338
R-Banks CLO Volume (5-Yr)	173,798	7,059	0	18,784	1,304	16,374	172,494	6,989
R-Banks CLO Count	173,802	0.55	0	2.08	1,292	1.82	172,510	0.54
R-Banks CLO Underwriting	173,822	0.05	0	0.15	1,284	0.18	172,538	0.05
Loan Characteristics								
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	0	0.37	3,228	0.21	26,360	0.16

Notes: Table A1 shows the summary statistics of key variables used in the analysis.

Table A2: **Effect of CLO underwriting on DR issuance**

	𝟙(Dividend Recap)				
	(1)	(2)	(3)	(4)	(5)
R-Banks CLO Volume	0.08*	0.09***	0.05***	0.05	0.03
	(0.04)	(0.02)	(0.02)	(0.03)	(0.07)
R-Banks CLO Volume × Size	-0.02*				-0.01
	(0.01)				(0.01)
R-Banks CLO Volume × Debt/Ebitda		-0.01***			-0.01**
		(0.00)			(0.00)
R-Banks CLO Volume × Gross Profit			0.04		0.12
			(0.07)		(0.09)
R-Banks CLO Volume × PE Ownership				0.01	0.02
				(0.04)	(0.05)
Size	0.34***				0.41***
	(0.05)				(0.07)
Debt/Ebitda		0.05***			-0.01
		(0.02)			(0.02)
Gross Profit			0.66*		0.26
			(0.34)		(0.47)
PE Ownership				-0.48**	0.01
				(0.23)	(0.25)
Observations	6119	6171	6038	4692	3766
Y-mean	0.05	0.05	0.05	0.05	0.06
Stack FE	Y	Y	Y	Y	Y

Notes: This table shows the relationship between CLO underwriting activity of PEs related banks and their likelihood of doing a dividend recap across different types of firms. First row shows the coefficient of our main measure (R-Banks CLO Volume) and the following rows shows the corresponding results across various types of firms. All models include stack fixed effects and cluster standard errors at the stack level.

Table A3: OLS Relationship between Dividend Recaps and Portfolio Company Outcomes

Panel (A): Bankruptcy and IPO

	Bankruptcy		IPO	
	8-Year	6-Year	8-Year	6-Year
	(1)	(2)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	0.98**	0.63**	2.76***	2.68***
	(0.38)	(0.32)	(0.55)	(0.54)
Stack FE	Y	Y	Y	Y
Obs	53539	53539	53539	53539
Y-Mean	0.21	0.17	0.13	0.12
Adj. R-Sq	0.04	0.04	0.06	0.06

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

	Average	$\mathbb{1}$[<-75%]	$\mathbb{1}$[-75,0%]	$\mathbb{1}$[0,75%]	$\mathbb{1}$[>75%]
1(DR)	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: Table A3 shows the relationship between dividend recaps and portfolio company outcomes. The empirical specification is:

$$y_{s,c,t} = 1(\text{DR})_{s,d(c,f,t)} + \alpha_s + \varepsilon_{s,c,t}$$

s denotes a stack, d denotes a deal, c denotes a portfolio company, f denotes a PE firm, and t denotes the deal year.

Table A4: 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%]
$\mathbb{1}$(DR)	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%]
1(DR)	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%]
1(DR)	-.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: Table A3 shows the relationship between dividend recaps and portfolio company outcomes. The empirical specification is:

$$y_{s,c,t} = 1(\text{DR})_{s,d(c,f,t)} + \alpha_s + \varepsilon_{s,c,t}$$

s denotes a stack, d denotes a deal, c denotes a portfolio company, f denotes a PE firm, and t denotes the deal year.

Table A5: 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)	0.073***	-0.103***	-0.029	0.087***	0.045
	(0.020)	(0.016)	(0.027)	(0.028)	(0.028)
Stack FE	Y	Y	Y	Y	Y
Obs	29144	29144	29144	29144	29144
Y-Mean	0.25	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)
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Dividend Recap)	1.393***	0.015	1.050***	0.368***
	(0.226)	(0.010)	(0.370)	(0.071)
Stack FE	Y	Y	Y	Y
Obs	16842	11975	11321	8837
Y-Mean	5.75	-0.00	-0.32	0.30

Notes: Table A5 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 A6: 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)	2.31***	-0.04***	-0.04**	0.03	0.05***
	(0.57)	(0.01)	(0.02)	(0.02)	(0.01)
Stack FE	Y	Y	Y	Y	Y
Obs	17,522	17,787	17,787	17,787	17,787
Y-Mean	16.84	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*	-0.04***	0.01	-0.02	0.05***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
Stack FE	Y	Y	Y	Y	Y
Obs	17,523	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	17,523	17,787	17,787	17,787	17,787
Y-Mean	1.25	0.29	0.64	0.05	0.02

Notes: Table A6 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 A7: **OLS Relationship between Dividend Recaps and Distributions, Subsequent Fund Launches**

	Log(1+Distributions)		Δ New Funds Launched	
	1-Quarter	2-Quarter	1-Quarter	1-Year
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Dividend Recap)	0.21***	0.12*	0.053***	0.096***
	(0.06)	(0.07)	(0.015)	(0.026)
Stack FE	Y	Y	Y	Y
Obs	24,004	23,999	76,275	76,275
Y-Mean	2.84	3.37	0.05	0.06

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 logarithm of one plus total distributions (in$ millions) made by PE-Fund f in h quarters after time t for columns (1)-(2) , and the change count of new funds launched by PE-Firm p in 1 and 4 quarters after time t, compared to 1 and 4 quarters before time t for columns (3)-(4) . We control for stack fixed effects. We also control for the logarithm of one plus total distributions (in $ millions) by fund f in columns (1)-(2). Standard errors are clustered at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A8: **Relationship between Dividend Recaps and New LBOs**

	IV Specification		OLS Specification	
	2-Year	4-Year	2-Year	4-Year
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Dividend Recap)	-3.82	-36.70***	1.06***	3.14***
	(3.96)	(10.11)	(0.22)	(0.46)
Stack FE	Y	Y	Y	Y
Obs	75,923	75,923	75,923	75,923
Y-Mean	-1.03	-6.46	-1.03	-6.46
F-Stat	65	65		

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 new LBO transactions by PE-Firm p in 2 and 4 years after time t, compared to 2 and 4 years before time t for columns (1) and (2) , and (2) and (4). Columns (1)-(2) present the IV results, while columns (3)-(4) present the OLS statistics. We control for stack fixed effects. Standard errors are clustered at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A9: Effect On Bankruptcy in Overlapping Samples

	Bankruptcy					
	Census-Sample		Stepstone Sample		Burgiss Sample	
	8-Year	6-Year	8-Year	6-Year	8-Year	6-Year
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(Dividend Recap)	12.22**	10.67**	11.87*	9.98*	6.47	6.87*
	(5.42)	(4.70)	(6.24)	(5.46)	(4.19)	(4.04)
Stack FE	Y	Y	Y	Y	Y	Y
Obs	24,500	24,500	2,066	2,066	28,164	28,164
Y-Mean	0.46	0.37	0.87	0.68	0.22	0.18
F-Stat	45.16	45.16	19.24	19.24	44.53	44.53

Notes: Table A9 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. Columns (1) and (2) correspond to the Census-Pitchbook matched sample, Columns (3) and (4) correspond to the Stepstone-Pitchbook matched sample, and Columns (5) and (6) correspond to the Burgiss-Pitchbook matched sample.

Table A10: **Robustness Tests of Dividend Recap IV Effect on Bankruptcy**

Panel (A): Alternative Instruments, Industry, and Window Filters

	Alternative Instruments				Alternative Industry, and Time Window Filters		
	R-Banks CLO Volume (1-Yr)	R-Banks CLO Volume (5-Yr)	R-Banks CLO Count	R-Banks CLO Underwriting	Same Sector 1-Year Window	Same Sub-Industry 1-Year Window	Same Industry 3-Year Window
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dividend Recap	20.08***	22.37***	11.59**	13.42**	21.57***	9.64**	22.30***
	(6.78)	(8.19)	(5.75)	(6.14)	(7.08)	(4.63)	(5.74)
Stack FE	Y	Y	Y	Y	Y	Y	Y
Obs	53539	53539	53539	53539	133562	10364	123614
Y-Mean	0.66	0.66	0.66	0.66	0.85	0.80	0.62
F-Stat	56.79	40.15	48.83	43.85	58.97	36.42	79.51

Panel (B): Adjusting the Sample with Alternative Filters and At-Risk Deals

	Alternative Deal and PE-Firm Filters			Number of At-Risk Deals			
	Without Deal Size	Without PE-Firm AUM	Without PE-Firm Age	Two	Three	Four	Five
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dividend Recap	17.08**	18.96***	32.42***	41.07**	23.98**	19.05**	16.41**
	(6.88)	(6.03)	(9.29)	(17.75)	(11.20)	(8.47)	(7.47)
Stack FE	Y	Y	Y	Y	Y	Y	Y
Obs	55646	79416	170949	9988	14829	18677	20864
Y-Mean	0.72	0.64	0.66	1.21	1.09	0.99	0.95
F-Stat	59.38	57.46	46.97	13.45	24.36	35.76	41.76

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 Panel (A) Columns (1) to (4), we show our results using an alternative set of instruments in the first stage. Panel (A) Columns (5) to (7) 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 Panel (B) Columns (1) to (3), we omit filtering on deal size, PE-Firm AUM, and PE-Firm age to choose our control deals. In Panel (B) Columns (4) to (7), we re-estimate our results by only considering stacks where the PE firm associated with the treated deal only had two to five at-risk deals in their portfolio. 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 A11: **IV Effect of Dividend Recaps on Fund Returns - Stepstone 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)	-21.71**	0.05	1.00**	-0.16	-0.89***
	(9.19)	(0.07)	(0.39)	(0.26)	(0.29)
Stack FE	Y	Y	Y	Y	Y
Obs	1,380	1,478	1,478	1,478	1,478
Y-Mean	21.30	0.02	0.47	0.40	0.11
F-Stat	12.58	15.74	15.74	15.74	15.74

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)	-0.93*	0.05	0.66**	-0.03	-0.68***
	(0.49)	(0.07)	(0.31)	(0.26)	(0.25)
Stack FE	Y	Y	Y	Y	Y
Obs	1,380	1,478	1,478	1,478	1,478
Y-Mean	2.06	0.02	0.50	0.40	0.09
F-Stat	12.58	15.74	15.74	15.74	15.74

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)	-0.66**	0.04	0.72**	-0.13	-0.63***
	(0.31)	(0.19)	(0.28)	(0.12)	(0.22)
Stack FE	Y	Y	Y	Y	Y
Obs	1,380	1,478	1,478	1,478	1,478
Y-Mean	1.34	0.15	0.73	0.05	0.06
F-Stat	12.58	15.74	15.74	15.74	15.74

Notes: Table A11 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 A12: **IV Effect of Dividend Recaps on Fund Returns - Stepstone 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)	-7.475**	-2.677***	3.789***	2.456**	-4.093***
	(3.523)	(0.659)	(1.068)	(1.010)	(1.001)
Stack FE	Y	Y	Y	Y	Y
Obs	30478	30478	30478	30478	30478
Y-Mean	0.36	0.10	0.30	0.39	0.22
F-Stat	42.27	42.27	42.27	42.27	42.27

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)	0.231	-1.978***	-9.801***	14.348***	-2.757***
	(2.713)	(0.385)	(1.758)	(2.352)	(0.755)
Stack FE	Y	Y	Y	Y	Y
Obs	30973	30973	30973	30973	30973
Y-Mean	2.70	0.02	0.29	0.56	0.12
F-Stat	43.40	43.40	43.40	43.40	43.40

Notes: Table A12 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 A13: **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.

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.[21] 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. We focus analysis on PE transactions between 2000 and 2023. This includes about 61,000 unique firms undergoing LBOs, sponsored by over 1,200 unique PE firms.[22] Among these LBOs, about 1,600 were followed by a dividend recap debt deal. Data on dividend recaps are from Pitchbook and LCD.

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

[21] 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).

[22] We exclude a small number of secondary LBOs for companies already in bankruptcy or that are in startup stage at the time of the LBO.

never relied upon alone).[23] The second step is to link the resulting crosswalk to the Longitudinal Business Database (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, or about 55% of the LBO sample.

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 & Stepstone. 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 Stepstone Group. This firm has built its Stepstone Private Market Intelligence (SPI) database through providing fund-of-fund and advisory services in private markets 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). Stepstone 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. Stepstone 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 how value is returned to LPs. Stepstone's lack of cash flow data also prevents calculating deal-level public market equivalents (PMEs).

While Stepstone 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 (16%) of the Pitchbook LBOs to Stepstone, 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

[23] 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).

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)).[24] 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.

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

[24]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.

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.[25] 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

[25] 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).[26] 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

[26]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.[27] 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

[27] 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.