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

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Abstract

We assess how debt affects firm, employee, investor, and creditor outcomes in the setting of dividend recapitalizations among private equity (PE)-backed firms. Debt is isolated in these deals because ownership remains constant, leverage increases substantially, and loan proceeds are paid to a PE fund rather than deployed within the firm. We show that PE firms select high-quality deals for dividend recapitalizations. Causal analysis finds that the new debt makes firms riskier, dramatically raising bankruptcy and failure rates, but also increasing IPOs and spurring revenue growth. It also reduces wages and fund returns, potentially to employees' and limited partner investors' detriment.

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1 Introduction

What is the impact of capital structure on firm outcomes? To isolate the role of capital structure, we require a setting with two features: a change in liability composition without a corresponding change in assets, and a change in leverage that is meaningful enough to affect firm outcomes. In other words, we need a large, exogenous increase in debt that is not deployed within the firm. In this paper, we find such a real-world setting in the form of dividend recapitalizations (or "recaps") among private equity (PE)-backed firms. Over the past two decades, dividend recaps have become a significant tool in the PE playbook. In these transactions, the PE firm sponsors a loan on behalf of its portfolio company. The loan proceeds are paid as dividends to the PE fund. In other words, the company takes on new debt, which is used to provide early cash returns to investors.

We offer causal estimates of how dividend recaps affect relevant stakeholders: the portfolio company itself, employees of the portfolio company, investors, and pre-existing creditors. In doing so, we make progress towards understanding how capital structure affects the firm and we shed light on the role of debt in PE. From the portfolio company's perspective, a dividend recap creates significant additional debt for an already highly levered company. This differs from dividend recaps among publicly traded companies, where the new debt issuance and total debt are small relative to assets. We shed light on the effects of additional debt where we expect them to be dramatic; among levered firms without access to public markets.

The population of PE-owned firms is diverse and reasonably representative of a large share of U.S. employer firms along measures such as size, sector, and location. Research on capital structure has focused on publicly traded firms, which account for less than 1% of firms, less than one third of non-farm employment, and have uniquely dispersed ownership and disclosure requirements.[1] In contrast, the vast majority of firms are privately owned with less than 100 employees. Our study represents to our knowledge a novel effort to rigorously examine how additional debt affects private firms.

How do we expect new debt to affect the PE-backed firm? On the one hand, PE managers may over-invest when credit is cheap, leading additional debt to increase the chance of bankruptcy without benefiting the portfolio company or the fund (Axelson et al., 2009). On the other hand, 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 predicts that dividend recaps will always occur at strong companies and will benefit financial stakeholders. Two anecdotes exemplify this debate:

> "[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

[1] See https://www.nber.org/digest/apr07/changing-business-volatility.

and financial performance."
– Scott Sperling, co-president of Thomas H Lee Partners.[2]

"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."
– From a lawsuit filed on behalf of restaurant chain Buffets Holdings. Buffets had been acquired in a PE deal with $130 million of equity (i.e., investment by the PE firm) and $515 million of debt. In a dividend recap two years later, the company distributed $150 million to the PE fund, more than the initial investment. Six years later, Buffets filed for bankruptcy.[3]

While PE-owned firms are in some ways representative of U.S. firms overall, PE ownership brings unique operational and financial strategies (Kaplan and Strömberg, 2009; Brown et al., 2021; Gupta et al., 2023). PE owners have been shown to create value across a range of industries, to have expertise in managing firms through distress, and to rely on long term reputation-based relationships with banks (Cohn et al., 2014; Hotchkiss et al., 2021; Ivashina and Kovner, 2011; Johnston-Ross et al., 2021; Gompers and Kaplan, 2022). These factors suggest that, conditional on pre-existing leverage, the effect of new debt on distress is likely lower at PE-owned relative to other private firms. PE also has a growing footprint in the economy. PE-owned firms directly employ over 12 million U.S. workers and account for 6.5% of U.S. GDP. We should expect this footprint to grow, as PE funds have $2.6 trillion in "dry powder," or funds waiting to be invested, in addition to their $4.4 trillion in U.S. assets under management.[4]

In this paper, we establish a causal effect of dividend recaps on real and financial outcomes. Our empirical strategy is based on opportunistic deals that occur when a particular PE firm has access to relatively cheaper credit than its peer PE firms. Low interest rates create an arbitrage opportunity between debt and equity markets, making dividend recaps an appealing way to deliver returns to investors. In choosing this design, we are motivated by evidence that PE managers use more leverage when debt is cheap (Kaplan and Stein, 1993; Kaplan and Schoar, 2005; Ivashina and Kovner, 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."[5]

Specifically, we instrument for dividend recaps using PE relationship banks' collateralized loan obli-

[2] See https://www.privateequityinternational.com/firms-turn-to-dividend-recaps-for-exits/.

[3] See https://www.wsj.com/articles/DJFDBR0020100409e649000dy and https://www.bloomberg.com/news/articles/2008-12-04/private-equity-s-year-from-hell.

[4] See AIC (2023) for employment and GDP statistics, which are for 2022, https://pitchbook.com/news/reports/q1-2023-us-pe-breakdown for AUM, which is for 2023, and https://www.spglobal.com/marketintelligence/en/news-insights/latest-news-headlines/private-equity-firms-face-pressure-as-dry-powder-hits-record-2-59-trillion-79762227 for dry powder statistics, which is also for 2023.

[5] See https://www.privateequityinternational.com/firms-turn-to-dividend-recaps-for-exits/.

gation (CLO) underwriting. CLOs are actively managed, highly diversified portfolios of leveraged loans, mostly to PE-backed firms. Loans issued by at least 100 unique companies compose the CLO, so no one company motivates CLO formation or determines CLO performance. When a PE firm's relationship bank underwrites a new CLO, the firm has better access to credit during the window when 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 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 the heart of our identification strategy.

Our instrument is the outstanding value of CLOs underwritten by the lead PE firm's relationship banks. 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 IV regression design. Within each stack, we compare treated firms that receive a dividend recap to control firms in a similar industry and that experienced an LBO at a similar time, but which 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 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 PE leveraged buyouts (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. As Figure 1 shows, dividend recaps have become a significant part of the LBO playbook since the mid-2000s. To construct our instrument, we use information on loans, bank relationships, and CLOs using data from LCD, Dealscan, Creditflux CLO-i, and Capital IQ. We obtain deal- and fund-level returns from Stepstone SPI and Burgiss, respectively. 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 the new debt increases firm risk, consistent with theories predicting agency problems of debt (Axelson et al., 2009, 2013). We focus first on the firm. We show that dividend recaps increase the chance of bankruptcy dramatically. For example, in our preferred model, the effect after eight years is 8.7 percentage points (pp), relative to a mean of just

0.2%. 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). Meanwhile, 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 and possibly leading to more disciplined management, 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 focus in on owners by examining returns to investors, our third stakeholder group. We find that the increased risk based on the real outcomes is paralleled by wider dispersion in deal returns. At the deal level, we find that dividend recaps do not have significant average effects, though the coefficients are positive. Dividend recaps do increase the chance of very good returns, of 20%-40% internal rate of return (IRR) or 2-4x total value multiple (TVM, or cash-on-cash returns). They decrease the chance of poor or moderate returns.

By bringing forward returns, dividend recaps may enable a longer holding period, which could have beneficial effects on the firm for two reasons. First, one critique of PE is short termism. Second, there is evidence that PE ownership improves productivity and operational efficiency. 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. Consistent with this hypothesis, we find that dividend recaps increase the holding period.

At the fund level, we show that dividend recaps decrease both the TVM and the public market equivalent (PME). There is no effect on IRR, a discrepancy consistent with bringing cash flows forward in the fund’s life. Meanwhile, dividend recaps sharply increase the chance of launching a new fund in the quarters immediately following the deal. Altogether, this points to dividend recaps being used to benefit general partners (GPs) by enabling early cash and easier fundraising; they may then focus more on the new fund or become more complacent about subsequent deals, reducing returns to the fund.

Finally, we study whether dividend recaps represent value shifting away from pre-existing creditors. We cannot observe returns to pre-existing private or bank debt, but we can observe a small sample of dividend recap targets with bonds outstanding. This analysis is ongoing and will be included in a future draft.

Descriptive analysis yields quite a different picture. We expect that the average, non-instrumented dividend recap will target a high-quality firm on a trajectory to generate the cash to pay off new debt. Consistent with this, in OLS regressions, the average dividend recap is associated with better outcomes. While there is a significant increase in bankruptcy, it is much smaller, and there is a lower chance of exit than at control firms. Similarly, we find that 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. Our causal effects represent the impacts of debt after eliminating this selection bias.

Overall, our paper is unique in considering the implications of additional debt for stakeholders on both the real and financial side. To our knowledge, this paper is the first attempt to study how debt causally affects the firm independently of changes to the asset side of the balance sheet; that is, because the debt is not used within the firm, we hold fixed its ability to enable investment and firm growth. Our results support the idea that financing structures have important implications for firm outcomes. Much existing work has focused on evaluating pecking order and tradeoff theories for whether firms will choose to finance themselves with debt, equity, or internal cash flow (Myers, 1984; Fischer et al., 1989). Our contribution is to focus on the implications of a given capital structure outcome. Heterogeneous 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 predict capital structure consistently.

While dividend recap targets are selected on being PE-owned, the control group in our analysis are also PE-owned LBO targets, so the effects do not reflect a “PE treatment." While the particular magnitude of our results may not generalize to other contexts, we believe they show—for the first time—that debt causally increases the chance of financial distress and negatively impacts wages conditional on firm survival. While we cannot calculate welfare for any stakeholder, substantial additional debt does not appear to benefit the limited partner investors who provide the equity for PE funds, appears to lead to worse outcomes for employees, and likely benefits the GP and possibly the firm (through higher IPOs).

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), Giroud and Mueller (2021), 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. This literature focuses on how leverage predicts outcomes following a negative shock (e.g., a financial crisis). Our instrument is related to work on determinants of leverage, including Benmelech and Bergman (2009), Eisfeldt and Rampini (2009), Rauh and Sufi (2010), Rauh and Sufi (2012), and De Maeseneire and Brinkhuis (2012).[6]

[6]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).

This paper also contributes to work on PE. Our IV strategy is motivated by evidence that PE firm reputation and relationships with 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). Unlike most of the literature focusing on effects of ownership transitions to PE (e.g., LBO effects), we focus on the effects of additional debt. Our paper is also one of a few to consider both returns and real outcomes. 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). The literature on the real outcomes side includes Boucly et al. (2011), Davis et al. (2014), Bernstein and Sheen (2016), Fracassi et al. (2022), and Howell et al. (2022), among many others.

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).[7] Some studies in PE have touched on dividend recaps; they have observed very small samples of these deals and have not identified causal effects; they generally find 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.

2 What Dividend Recaps Can They Tell us About Capital Structure

Testing capital structure theories, especially the impact of debt on firm outcomes, is challenging because higher leverage is endogenous, debt proceeds are usually deployed in new investment, and new debt is small relative to assets in samples of public firms. To evaluate the effects of additional debt, we must choose a setting where the increase in debt is economically meaningful for the firm. For most firms, observed leverage changes are too small to significantly impact firm value. Furthermore, there is reason to believe that for most firms, there is little value from optimizing capital structure, which may be one reason capital structure variation remains unexplained (Van Binsbergen et al., 2010; Graham and Leary, 2011). Thus, if faced with even relatively small transaction costs, most firms will not optimize or do so very irregularly (Strebulaev, 2007; Leary and Roberts, 2005; Korteweg et al., 2022). This, together with the effects of new debt on investment outcomes, makes it difficult to test how leverage affects the firm. We overcome these challenges by isolating a large increase in leverage that is not tied to investment.

As the name "Leveraged Buyout" reveals, PE-backed firms acquired in LBOs are more levered than

[7]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).

their industry peers. This is because the debt that financed the deal is passed to the target company, enabling the higher returns demanded by PE investors. In our deal-level data from the fund-of-fund Stepstone (Table 1, described below), the average (median) debt-to-EBIDTA ratio is 3.9 (4.1), which is roughly in line if slightly lower than industry standards according to LCD (a platform which aggregates but does not sell portfolio company-level data).[8] This higher debt load may discipline managers via the claim on cash flows and offers tax benefits (Jensen, 1986; Cohn et al., 2014). In a dividend recap, a firm takes on new debt that is used to pay a return to its equity owners. This departs from the standard use of loan proceeds, which is either to fund new firm capital or operating expenditures, or to repay pre-existing debt. Dividend recaps are large enough to significantly increase firm leverage. Relative to non-DR deals in the Stepstone subsample merged to Pitchbook, we see a 11% increase in debt-to-EBIDTA. This creates an opportunity to study how leverage independently affects firm outcomes.

Of course, our sample is selected on being PE-owned. Before we discuss the particularities of PE, however, it is worth noting that the public firms which capital structure research typically considers are quite selected, accounting for less than 1% of firms and less than one third of non-farm employment.[9] It is not at all obvious that the dispersed ownership, disclosure requirements, and other features of being a publicly traded firm allow us to learn about how debt works for the vast majority of firms (and employment) in the economy, which is privately owned. PE funds are financial intermediaries, with capital raised from limited partners such as pension funds and endowments. The general partners, 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. Kaplan and Stromberg (2009), Jenkinson et al. (2021) and Gompers and Kaplan (2022) provide detailed discussions of the PE business model and review the academic evidence on their effects.[10] PE is associated with particularly high-powered incentives to maximize profits both because of the large share of debt on the balance sheet and because the general partners are compensated through a call option-like share of the profits (Kaplan and Stromberg, 2009).[11]

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.[12]

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

[9] See here.

[10] See also **?**, Kaplan and Schoar (2005), Guo et al. (2011), Harris et al. (2014), Robinson and Sensoy (2016) and Korteweg and Sorensen (2017).

[11] Specifically, their compensation stems primarily from the right to 20% of profits from increasing portfolio company value between the time of the buyout and an exit, when the company is sold to another firm or taken public. GPs also can receive transaction and monitoring fees, which are not tied to performance.

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

As discussed above, PE ownership affects companies beyond simply bringing additional debt, and these treatment effects likely impact how the greater debt affects firm outcomes. First, while higher debt loans should increase firm risk, thus raising the chance of financial distress, the literature has not found that PE-backed firms face distress more often than their peers, and 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 private capital (Tykvová and Borell, 2012; Hotchkiss et al., 2021; Johnston-Ross et al., 2021). We expect that these special skills of PE owners 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. Second, PE backing typically brings greater operational efficiency and management with higher-powered incentives (Acharya et al., 2012; Bloom et al., 2015; Gompers et al., 2016; Howell et al., 2022). This may lead debt to have a more sharply disciplining effect than it would at other firms. Finally, overall PE leads to higher-powered incentives than exist at other firms. This in part derives from higher leverage, which is what we are interested in, but it also emerges from the call-option nature of PE manager compensation (Metrick and Yasuda, 2010).

Overall, our study offers a first step towards examining the independent effect of leverage, with the important caveat of selection into and other treatment effects of PE ownership. In addition, it is independently important to understand the effect of dividend recaps in PE. As Figure 1 shows, 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. This is the intuition behind our PE-firm level instrument. Dividend recaps in PE are often vilified as an extreme form of asset-stripping, representing the “worst" of an extractive sector. Yet it is not at all obvious that they will have negative effects; first, if they typically led to distress, it is not plausible that creditors would 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.

3 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 this section, we briefly describe each dataset that we use in the analysis.

PE Deals from Pitchbook. We begin with a comprehensive dataset of PE deals from Pitchbook. We focus analysis on 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 (a small portion of the dividend recaps are sourced from LCD, described below).

The Pitchbook data also includes detailed information about the PE fund and firm.

Firm Outcomes: Bankruptcies and IPOs. We gather data on bankruptcies and IPOs from Preqin and Pitchbook. These firms source bankruptcy events from court records. This is our central outcome variable, where we have comprehensive coverage and no concern about selection or noise that may be induced by matching. We match Preqin to Pitchbook for each portfolio company, and append the two datasets together to create a file containing the universe of publicly available 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 B. Here, we provide a brief summary. We first match the Pitchbook deals to the County Business Patterns Business Register (CBPBR), which is a internal Census registry of establishments. Establishments represent the smallest unit of a company, corresponding to a particular facility or location. We developed a new, multi-step rigorous matching approach that makes use of 12 crosswalks between Pitchbook and Census variables as well as the firm EIN where available (though EIN is never relied upon alone).[13] 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, and exit, aggregating up to the firm level where necessary. 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 deal.

Investor Outcomes: Burgiss & Stepstone We are one of a few papers on PE to study both financial returns and real outcomes. To our knowledge, we are the only paper to observe both fund- and deal-level performance. For deal-level performance, we use data from Stepstone Group, a large fund of funds. This firm has built its Stepstone Private Market Intelligence (SPI) database while 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)

[13] 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).

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. For fund-level data, we employ the Burgiss database. 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 & Dealscan, We construct the sample of loans taken by PE-backed companies by combining two sources: Leveraged Commentary & Data (LCD, now owned by Pitchbook) and Refinitiv Dealscan. Both sources provide loan-level information on borrowers, lenders, and PE sponsors. They also provide details on loan amount, maturity, interest rate spread, loan covenants, etc. However, LCD and Dealscan differ in their coverage and do not fully overlap with each other. E.g., Dealscan widely covers the broadly syndicated loan market. However, several studies express concern that Dealscan has poor reporting quality in the leveraged loan market and often mis-classifies covenant-lite loans. (Becker and Ivashina (2016); Bräuning et al. (2022)).[14] 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,

[14]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.

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.

Collateralized Loan Obligations from CLO-i. We construct the shocks for our instrumental variables analysis by combining the PE-bank relationship data with CLO issuance data from the Creditflux CLO-i database. CLO-i includes information about the CLO manager, the CLO portfolio, and the underwriting bank. We use this detailed data on CLO funds to quantify banks' CLO underwriting activity and to examine purchase of dividend recap loans by CLO managers. This CLO data comes from Acuris CLO-i database which 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.

3.1 Summary Statistics: Understanding Dividend Recaps

One contribution of our study is to offer a first academic look at dividend recaps. Figure 1 shows the number of dividend recaps over time (Panel A). This deal time essentially did not exist in PE before the mid-2000s. With a hiatus during the Financial Crisis, it has since become a part of the playbook, with 100-200 years per year. We relate this to the overall number of firms entering PE ownership in Panel B, where we see that the number of dividend recaps is meaningful relative to the universe of firms that undergo LBOs, at about 5-10$\%$. Figure 2 Panel A compares the industry composition of firms with dividend recaps to the overall sample with LBOs. The distribution is relatively similar, though with a higher fraction in consumer-facing firms, and a lower fraction in financial and business-facing firms. Dividend recaps dtend to occur one to three years after the LBO, peaking at two years (Panel B of Figure 2).

We expect that dividend recaps may be associated with longer holding period. This is an interesting point because overall in PE, the rise of dividend recaps has coincided with longer holding periods. 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. Dividend recaps may make longer holding periods feasible within the existing fund economics structure. This could be a positive effect of dividend recaps, 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.[15] If this is the case, then dividend recaps might 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.

Summary statistics at the portfolio company level, pertaining to the LBO deal, the deal outcomes, and the portfolio company outcomes, are in Table 1. We provide the full sample result as well as separate statistics for dividend recap targets and other LBO targets (which are used in our analysis to develop a control firm sample). Our data sources are perhaps uniquely comprehensive in the literature on PE, in terms of including deal-level real and financial measures. However, a downside is that the various datasets and often variables within the datasets have different degrees of coverage. To the extent possible, we explore likely selection biases and test the robustness of key results to overlapping samples.

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. Note that Pitchbook reports deal size only for 12,400 of the 61,600 LBOs that we use from its database. Dividend recap targets are much larger than their counterparts in both datasets as measured by deal size and total enterprise value (TEV). Following the deal, they have higher debt loads and higher gross profits.

The second set of variables on deal outcomes (all from Stepstone) show that relative to the overall average IRR of about 27%, dividend recap targets have much higher IRRs of 43%. 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.

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; this is likely biased downward as many of the LBOs are recent. If we consider only LBOs before 2015, giving firms at least eight years under observation, the rate is 0.94%. Existing literature has typically found higher rates of bankruptcy post-LBO, between 10% and 20% (Kaplan and Stein, 1993; Strömberg, 2008; Kaplan and Stromberg, 2009; Braun et al., 2011; Cohn et al., 2014; Ayash and Rastad, 2021). These previous studies generally use small samples of pre-Financial Crisis public-to-private deals. In contrast, our sample of around 61,000 LBOs is composed overwhelmingly of private-to-private LBOs, where firms are smaller and thus less likely to file for bankruptcy. Note in the overall U.S. during our sample period, there were 20,000-40,000 business bankruptcies each year, representing just 0.3%-0.7% of the roughly six million employer firms.[16] Therefore, the bankruptcy rate among LBOs in our

[15]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

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

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, since PE firms specialize in taking successful firms public as an exit strategy. Both bankruptcy and IPO exhibit much higher means for dividend recap targets, consistent with higher risk.

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. 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. We impose a stringent requirement that employment be observed for all years between $t-1$ and $t+4$ in order to retain the firm in this survivor sample. This ensures consistency across the outcome variables with no intermittency. We see that 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 deal. For example, employment growth through the third year after the deal is defined as $\frac{(Emp_{t+3}-Emp_{t-1}}{Emp_{t-1}}$. 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, 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).

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. The final set of variables concerns the PE firm; while the age is fairly similar, we see that firms which undertake any dividend recaps are on average have more investments than firms that never do a dividend recap, but have smaller assets under management (AUM).

Last, summary statistics about the loans are in Table A1. Altogether, our sample has 15,627 loans containing a total of 11,714 prorata tranches (which are typically held by banks) and 12,517 institutional tranches (which are typically held by institutional investors like the CLOs). The average loan in the sample has a size of $216 Million and a maturity of 5 years. It has a interest rate spread (over the benchmark rate) of 403 basis points and has a 16% probability 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.

4 Empirical Strategy

The intuition for our instrument is that when a PE firm is exogenously shocked with a short-term opportunity to access the leveraged loan market at lower cost than usual, it may conduct an opportunistic dividend recap with one of its portfolio companies. Our instrument relies on two relationships: (i) Between a CLO manager and the bank underwriting the CLO; (ii) Between a PE firm and their relationship bank. The exclusion restriction is that the CLO volume underwritten by the relationship bank cannot be independently related to the trajectory of the targeted portfolio company. That said, we do not assert that the choice of portfolio company is exogenous. Instead, we allow that the portfolio company chosen may be the one most amenable to a dividend recap within the PE firm's portfolio. The fact that higher quality firms tend to be chosen for dividend recaps means that any bias from within-fund selection should push us to find more positive effects. We show that our results are robust to restricting to PE portfolios with only two firms that could plausibly conduct a dividend recap. While we restrict the sample to targets at similar stages, sectors, and buyout years, our causal identification is not based on this match. Our causal effect is identified by comparing two similar LBO targets across PE firms, and is based on the PE firm's exogenous and time varying access to credit based on their relationship banks' CLO underwriting.

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 dividend 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.

4.1 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 (i.e., securitize 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, with which she buys an initial set of loans during a warehousing phase of about six to nine months. After the warehousing phase, the deal officially closes and the bank starts marketing it to potential investors. The investors (typically banks, insurance companies, pension funds, etc.) provide the manager with long-term financing, which both pays off the line of credit and is used to purchase additional loans over the next six months (the ramp-up phase) until the target asset volume is achieved and the CLO becomes effective (Cordell et al., 2023). 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 starts to wind down. The managers stops trading, and the fund winds down mechanically as the maturing loans are used to pay out the remaining investors. This amortization phase can last a long time (6-10 years) after which the CLO matures and the fund is closed.

Loans issued by at least 100 unique companies compose the CLO. This means that no one company can determine CLO performance, and 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. This requires the company to have strong cash flows or other assets to serve as collateral, beyond that required for pre-existing debt. At the same time, 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 typically is 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, 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 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.

Summary statistics about the loans are in Table A1. Altogether, our sample has 15,627 loans containing a total of 11,714 prorata tranches (which are typically held by banks) and 12,517 institutional tranches (which are typically held by institutional investors like the CLOs). The average loan in the sample has a size of \$216 Million and a maturity of 5 years. It has a interest rate spread (over the benchmark rate) of 403 basis points and has a 16% probability 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 construct the shocks for our instrumental variables analysis by combining the PE-bank relationship data with CLO issuance data from the Creditflux CLO-i database. CLO-i includes information about the CLO manager, the CLO portfolio, and the underwriting bank. We use this detailed data on CLO funds to quantify banks' CLO underwriting activity and to examine purchase of dividend recap loans by CLO managers. This CLO data comes from Acuris CLO-i database which 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.

4.2 PE-Bank-CLO Manager Relationships

The above discussion explains that CLOs demand risky debt issued by PE-backed companies, but do not make investment decisions for their new funds in isolation. Instead, they propose a list of potential loans to the underwriting bank who screens the borrowers and provides the manager with the list of approved loans. This helps the underwriter ensure that the CLO securities backed by those loans are rated and priced appropriately for the potential investors. Also, the underwriting bank provides bridge loans to finance the purchase of these loans. The implication of such contractual features is that the underwriting bank is deeply involved in loan selection process of a new CLO.

The key takeaway from the institutional context described above is that *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*. This can happen for two reasons. First, the underwriter may have private information about its related PEs and is more likely to screen their loans favorably (Ivashina and Kovner (2011)). Alternatively, banks may facilitate related PEs access to the CLO market in order to secure future lending business from such PEs.

As introduced in Section 3, we create measures of PE-bank relationships using the set of loans in the Dealscan-LCD combined dataset. Specifically, we define that a PE firm p has a relationship with a bank b in month t, if one or more companies sponsored by p took a loan from bank b (as lead bank) during that month.[17] Thus, our relationship variable 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 our sample period, an average PE firm had a relationship with two banks, and the average bank had a relationship with three PE firms. Among the set of banks that are in the CLO underwriting business, an average bank has relationship with four PE firms, highlighting that CLO underwriting banks are typically larger in size.

We find descriptive support in the data for the PE-Bank-CLO channel as a motivating factor behind dividend recaps. Specifically, out of 782 dividend recap loans in our data that were financed by CLOs, more than 66% were bought by CLOs underwritten by a bank related to the PE. We formally show that CLOs are more likely to buy the dividend recaps of PE firms related their underwriter bank using the methodology of Bharath et al. (2011) and Chodorow-Reich (2014). For a given dividend recap loan, each observation in the sample corresponds to a potential CLO buyer. We define the potential CLO buyers as CLOs that are actively purchasing loans (i.e., are in their warehousing or ramp-up phase) at the time the dividend recap loan was issued. We then estimate the following specification:

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

Purchased by CLO$_{d(p),k(b,t)}$ 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. PE-Bank Relationship$_{p,b,t-1}$ indicator is one if p has a lending relationship with bank b in year $t-1$, and zero otherwise.

Table 2 presents our results. In Column (1), we employ PE fixed effects to address the concern that some PE managers may issue loans whose contractual terms are more amenable to the CLO industry. We also employ CLO fixed effects as higher market share my correlate with the likelihood of acquiring PE-backed loans. We find that, after controlling for such factors, loans of PE firms 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. In Column (2), we control for the variation in CLO market share over time and across industry by including CLO $\times$ Year and CLO $\times$ Industry fixed effects and find similar results. In sum, 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

[17] 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.

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.

4.3 Instrumental Variable

Our primary instrument for a dividend recap deal is constructed as follows. For each PE, we measure exposure to the CLO market by aggregating the CLO underwriting activity of all banks related to that PE. We first 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 4.1, 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. In other words, we only consider the CLOs for which month t falls within the window of 6 months prior to their closing date up until their effective date. Next, we aggregate CLO underwriting by across all the banks related to PE firm p and average it over the past-12 months to create our instrument (denoted by R-Banks CLO Volume), which is calculated using Equation 2.

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

Here, 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. 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. We use this measure as a proxy for how likely it is for the firm p to get their dividend recap loans financed by the CLO industry.

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 deals that featured a dividend recap and 0.05 among deals that did not. This simple univariate comparison is consistent with the idea that PE firms more exposed to the CLO industry show a higher probability of doing a dividend recap. We formally test this idea in Section 4.5.

4.4 Building Stacks for Analysis

To avoid concerns about staggered treatment bias, and to establish a more homogeneous sample, we take 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 LBOs. The target with the dividend recap is the treated LBOs. When a company has multiple dividend recap deals, we use the first. We drop dividend recaps that are within a year of exit, because these tend to be part of the exit transaction. We also exclude secondary LBOs for companies already in bankruptcy, or in early investment stages at the time of the LBO.

We require the control companies in each stack to be similar to the dividend recap target in their LBO date, industry, deal size, and PE firm type. 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.[18] The control LBO deal values 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. On the PE firm side, we require deal sponsors to be within a range of 10% to 10 times as large in both number of investments and AUM, and require that they be founded within a period of five years around the PE of the treated LBO. Finally, we drop any control LBOs which occurred after the dividend recap date.

Overall, this sample helps to address issues of selection. We show that our main results are robust to alternative approaches to stacking. All of them address the concern that fixed effects that are shared across treatments can bias estimates of the average treatment effect (Baker et al., 2022; Cengiz et al., 2019).

4.5 Estimating Equations and First Stage Analysis

The first stage analysis documents 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. (Below, we discuss robustness tests, including around the question of selection within the portfolio.) Using the stacked deal-level data described above in Section 4.4, the empirical specification is:

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

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, $\mathbb{1}(\text{Dividend Recap})_{s,d(c,p,t)}$ is an indicator variable that is one for the treated deal (i.e., the deal that had the dividend recap), and zero otherwise. The instrument ($\text{R-Banks CLO Volume}_{p,t}$) 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.

[18] Pitchbook defines 40 industry groups

Next, 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 ways to define banks' underwriting activity. Instead of looking at the value of CLOs underwritten by the bank, we now look at the number of CLO deals underwritten by the bank and the entry of bank in the CLO underwriting business. Columns (4) and (5) show that we get similar results when we use these alternative instruments.

We also investigate the timing of the relationship between PE firms' access to CLO funding and their probability of executing a dividend recap using a dynamic empirical specification. We aggregate the deal-level data to create a panel data at the PE-firm×year-month level. We then create a PE-firm-level "shock" as an indicator variable (D(ΔR-Banks CLO Volume>25%)$_{p,t}$) 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. We then use a finite distributed lag model to accommodate for the possibility that new CLO underwriting shocks may have a delayed impact on loan issuance. The empirical specification is:

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

$\mathbb{1}$(Dividend Recap)$_{p,t}$ is an indicator variable that is equal to one if PE-Firm p took a dividend recap loan in month t, and zero otherwise. D(ΔR-Banks CLO Volume>25%)$_{p,t-h}$ is an indicator variable 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 employ PE-firm (α_p) and year-month (α_t) fixed effects to absorb cross-sectional and aggregate time-series variation in our variables.

Figure 4 shows the estimated coefficients (β_h) plotted against the corresponding time difference (h). The results highlight three important points. First, coefficients corresponding to the periods before the shock (i.e., with $h < 0$) are not statistically different from zero, indicating that there are no pre-trends across PE-firms that received a shock versus those that did not. Second, the increase in CLO access in a given month increases the probability of dividend recap by 1 pp in that month. This increase in economically significant relative to the unconditional probability of 0.4%. Third, the impact of new CLO access reverses back to zero in approximately 12 months, which is consistent with the idea that most CLOs actively buy loans during the first 12 months of their life (i.e., during the warehousing and the ramp-up phase). In sum, we observe a strong first stage with consistent dynamics, suggesting we can instrument for dividend recaps with CLO funding shocks.

The second stage of the 2SLS empirical specification is:

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

Here, $\mathbb{1}(\widehat{\text{Dividend Recap}})_{s,d(c,p,t)}$ is the predicted value of dividend recap from the first stage (Equation 3 that we use as the explanatory variable in this second stage.

4.6 Instrument Assumption and Validation

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 economic magnitude. There are three potential concerns with 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 the 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 4.1. First, in our CLO-month level data, the average dividend recap target portfolio company is 1.16% of the CLO, too small a share to drive the whole vehicle's creation. This is by design to obtain the high level of diversification that underlies the return proposition for the CLO securities.

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. To show this in practice, in Figure 4 we provide an event study analysis in which we show that sharply higher CLO volume predicts a dividend recap. We do not see pre-trends, which we would expect if it were also the case that dividend recaps sometimes drove CLO volume.

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. This may reflect the agency problems formalized in Axelson et al. (2009). Therefore, there may be concern that compliers with our instrument–opportunistic dividend recaps–could be lower quality than dividend recaps that are selected under normal or tight credit conditions. However, our main result is not about the effect of easy vs. tight credit and is thus orthogonal to this existing literature. We do not compare opportunistic dividend recaps to the average dividend recap, which would be the analogy to the previous literature. Instead, we compare firms that experience opportunistic dividend recaps to other PE-backed firms. This makes it unlikely that the results reflect moving down the firm's demand curve, since in practice dividend recap targets in general have stronger cash flows (to support additional debt) relative to the average company at the same point in its lifecycle. While we cannot observe these cash flows, it is apparent from Table 1 that LBOs with DRs are much larger and have higher profits and debt to Ebitda ratios relative to the average LBO.

From another perspective, however, our empirical design is the first to establish a causal relationship between lower cost credit and PE dealmaking that is not tied to macroeconomic factors. The literature thus far has only established an association between easy credit conditions and lower quality deals, noting that these credit cycles are tied to broader business cycles, which affect company operations and financing through many channels, including demand.

Selection Within the Portfolio An alternative concern pushes in the opposite direction, potentially biasing us to find more positive results relative to an assessment of truly random dividend recaps. Our instrument is at the level of the PE firm, since it is based on the PE firm's bank relationships. Therefore, conditional on a random shock to capital supply, the PE firm selects a company in their portfolio for an opportunistic dividend recap. To the degree that higher-quality companies are selected for dividend recaps, since as mentioned above these deals require strong cash flows that can, at least in theory, service additional debt, this should bias us toward finding more positive effects,

We cannot fully address this possibility. However, two tests suggest it is not a first order issue in our analysis. First, we test this by restricting 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 broadly 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.

A second way that we partially address the issue of selection is through the choice of control sample. As discussed above in Section 4.4, 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 these observables is not central to the main finding.

5 Results: The Effect of Dividend Recaps on Key Stakeholders

This section presents the IV effects of additional debt via dividend recaps on the relevant stakeholders: the target company, the investors, and the bondholders.

5.1 The Target Company

The first stakeholder we consider is the firm, for which we examine both bad and good outcomes.

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 firms are generally better at managing portfolio company distress by renegotiating with creditors or injecting capital themselves, 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. Our preferred specification, in column (1), uses a 8-year horizon. This is the longest we can reasonably track firms given our sample period. (Recall from Section 4.4 that we define all outcomes around the dividend recap year, where all time-varying firm variables in a stack are centered around the dividend recap year for that stack.) The effect of 8.7 percentage points (pp) is about 45 times the mean, a dramatic effect. The results are similar but slightly lower at the six-year horizon (column (2)).

We turn to firm exit in columns (3)-(4). This employs the U.S. Census Bureau-matched dataset, which is much smaller. In this sample, we find similar effects on bankruptcy, shown in Appendix Table A3. The effect on exit 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 8-year or 10-year outcome, and the results are noisier for the six-year horizon.

Robustness Tests. We present a range of robustness on the bankruptcy result. First, in Table A3 we show that the effect of bankruptcy is quite similar and statistically significant in the real outcomes sample (i.e., within the Census dataset) and in the Stepstone-matched sample that we use to estimate effects on deal-level returns.

Further robustness tests of the bankruptcy result are in Table 4. In this draft, we minimize reporting of

Census-matched results.[19] 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 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.

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. Above, we showed a marked increase in bad outcomes for the firm. But 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 (Jensen, 1986). In this case, we might also expect to see a positive effect on right-tail outcomes. We see this in Table 5, where we first consider the chances of the firm having an IPO over six-, eight- 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.

[19]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).

For example, over a 10-year horizon, the effect is about 80 times the mean (Panel A column (3)).

In Panel B, we turn to revenue growth among survivors, using data from the U.S. Census Bureau. There would be a large mechanical negative effect on growth measures if we included the whole sample, since there is such a large, positive effect on exit (Table 3). Therefore, we focus on growth in the sample of survivors. Note that revenue in Census is available only for a subset of firms in the LBD, reflecting how Census collects data from tax forms. Furthermore, as explained in Section B, we require a firm's panel to be fully populated across the four years between $t-1$ and $t+3$ to calculate growth statistics. Therefore, we should consider this population to be selected on growth.

5.2 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. We use the growth measures described in Section 3.1, which are calcualted between the year before the dividend recap year and the fourth year after (i.e., $\frac{(Emp_{t+3}-Emp_{t-1})}{Emp_{t-1}}$), where $t=0$ is the dividend recap year. The results are in Table 6. Panel A shows a negative but insignificant effect of dividend recaps on average employment growth. In the following columns, we unpack this to reveal an interesting distributional effect. Column 2 shows that there is a positive effect on having a large contraction in employment growth; the probability that employment growth declines by more than 75% increases by about 20 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. The coefficient on a smaller contraction, where employment growth is between zero and -75%, is positive but insignificant. We see a large decline of about 52 pp in the chance of moderate growth between zero and 75% in column 4, though this is much 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%. 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. The coefficients on all the remaining outcomes are negative and insignificant, pointing to an overall more negative effect on payroll than on employment; that is, there dividend recaps do not increase the chance of payroll growth (columns 3-4). 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).

5.3 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 sufficiently large dividend may allow the investors to extract value even before exiting the deal, thereby increasing deal returns. We use the Stepstone SPI data, which provides deal-level internal rate of return (IRR) and total value multiple (TVM). We construct variables that parallel the continuous real outcomes from the previous analysis, allowing us to observe average and distributional effects.

The results are presented in Table 7. Measured both with IRR (Panel A) and TVM (Panel B), we find that dividend recaps do not significantly affect average deal returns, though the coefficients are positive. In terms of the distribution, dividend recaps increase the probability of deals falling in high-return bins and lower the probability of them falling into low-return bins. More specifically, dividend recaps shifts the distribution of IRR from the 0-20% bin to the 20-40% bin. The chance of decent returns between zero and 20% falls by 5.4 pp, while the chance of a 20-40% return increases by 2 pp (Panel A columns 3-4). Similarly, they reduce the likelihood of a deal returning less than two times the initial investment and increase the likelihood of it delivering returns between two and four times (Panel B columns 3-4). Collectively, these results indicate that dividend recaps have a moderate and positive impact of deal returns. They are consistent with the idea that PE firms are increasingly opting for dividend recaps as a tool to generate higher returns for themselves and their investors.

Fund-Level Returns We study the effect at the fund level using data from Burgiss. 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. The fund-level effects are in Table 8. There is no significant effect on average IRR, though the coefficient is negative (Panel A, column 1). When we turn to TVM and PME, we see large, negative effects; TVM declines by 87% of the mean, and PME declines by 41% of the mean. 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.

Distributionally, all three measures paint a similar picture. Dividend recaps decrease the chance that a fund has either very low or very strong returns, with the coefficients in columns 1, 3, and 4 of Panels A, B, and C all negative and generally highly significant. For example, a dividend recap reduces the chance of a fund returning between twice and four times its money by 94 pp, which is important since this category is where 38% of funds turn out (Panel B column 3, with mean in bottom row). Instead, dividend recaps increase the chance of being in the “OK" return bucket, which we define as IRR between zero and 20% (Panel A column 3), TVM of 1-2x (Panel B column 3), and PME of 1-2x (Panel C column 3).

The negative effects on fund returns are perhaps surprising given the positive coefficients at the deal level from Table 7. 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 A8). 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 A9). In other words, although the dividend recap if anything increases the deal-level return, it reduces the fund-level return. Brown et al. (2023) show that within PE funds, early deals tend to have higher return and higher risk relative to later deals. Our results are very consistent with this, in that dividend recaps tend to have higher returns and are concentrated early in the fund’s life.

Why would dividend recaps reduce fund-level returns? While we cannot pin down a particular mechanism, one reason PE managers may want to bring cash flows forward in the funds life is to improve their ability to raise the next fund. Fundraising typically occurs in cycles, with the next fund raised midway through the previous fund, making interim returns important. Chung et al. (2012) document the importance of current fund performance for the ability to raise future funds, affecting lifetime total pay; in fact, this indirect pay for performance stemming from the current fund’s impact on future fundraising is about the the same as the direct pay for performance of the current fund.

Motivated by Chung et al. (2012), we consider the effect of dividend recaps on new fund launches. First, Figure 5 shows the effect using a dynamic OLS differences-in-differences specification. We estimate the equation:

$$\begin{aligned}\text{New Fund Launch}_{s,d(c,p,t),h} = &\sum_{h=-8}^{8} \beta_h \times \mathbb{1}(\text{Dividend Recap})_{s,d(c,p,t)} \\ &+ \alpha_d(c,p,t) \times \alpha_s + \alpha_h \times \alpha_s + \varepsilon_{s,d(c,p,t),h}\end{aligned} \tag{6}$$

Here, $d(c, p, t)$ denotes an LBO deal where the PE firm p took a DR loan at time t for target company c. For each stack s, New Fund Launch$_{s,d(c,p,t),t}$ is the count of new funds launched by PE-Firm p in h quarters after time t. $\mathbb{1}$(Dividend Recap)$_{s,d(c,p,t)}$ is an indicator variable that is one for the treated deal (i.e., the deal that had the dividend recap), and zero otherwise. We control for deal-stack($\alpha_d(c, p, t) \times \alpha_s$) and stack-lag($\alpha_t \times \alpha_s$) fixed effects to absorb any deal and time specific variation within a stack.

We see a marked increase in launching a new fund in the quarter after the DR, and generally elevated rates for many of the subsequent quarters. (Note that we do not present event studies for other outcomes because bankruptcy and IPO are too rare and there are disclosure restrictions for the Census results.) We estimate the IV counterpart of this specification by estimating the equation:

$$\begin{aligned}\text{Number of Fund Launch}_{s,d(c,p,t),h} = \beta \times \mathbb{1}(\widehat{\text{Dividend Recap}})_{s,d(c,p,t)} \times \text{Post}_{s,h} \\ + \alpha_d(c,p,t) \times \alpha_s + \alpha_h \times \alpha_s + \varepsilon_{s,d(c,p,t),h}\end{aligned} \tag{7}$$

Here, $d(c, p, t)$ denotes an LBO deal where the PE firm p took a DR loan at time t for target company c. For each stack s, New Fund Launch$_{s,d(c,p,t),t}$ is the count of new funds launched by PE-Firm p in h lags after time t. $\mathbb{1}(\widehat{\text{Dividend Recap}})_{s,d(c,p,t)}$ is the predicted value of dividend recap from the first stage that we use as the explanatory variable in this second stage. Post$_{s,h}$ is an indicator variable which switches on after the DR date. We control for deal-stack($\alpha_d(c, p, t) \times \alpha_s$) and stack-lag($\alpha_t \times \alpha_s$) fixed effects to absorb any deal and time specific variation within a stack. The IV results using both firm-quarter and firm-year panel data structures are in Table 9 We see that dividend recaps increase the fund launches in a particular quarter by 0.12, which is about twice the mean of 0.06. At the annual level the result is similar, at 0.78 relative to a mean of 0.21. In sum, our results together with the literature suggest that dividend recaps reduce future risk-taking by PE managers, in part by helping them launch the next fund. Once the next fund is launched, the GPs may take less risk or put less effort in other deals in the fund as they focus on the new fund.

5.4 The Creditors

The final stakeholder we consider are the lenders. The main lenders for both LBOs and dividend recaps are banks and private lenders. As discussed above, the 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. The type of debt with observable performance is bonds. Below, we consider a small sample of dividend recaps with bonds outstanding.

Loan Spreads We expect that if dividend recaps increase firm risk, they will be accompanied by higher interest rate relative to loans taken for other purposes, such as to finance positive NPV projects, where the future cash flows from the project may mitigate the risk of bankruptcy stemming from higher indebtedness. In the case of dividend recap loans, the loan proceeds are not utilized to grow the firm's business. Thus, these loans may require higher spread to compensate the investors for the increased bankruptcy risk.

We verify this conjecture in the loan sample based on 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:

$$\text{Loan Spread}_{l(p,b,t)} = \mathbb{1}(\text{Dividend Recap})_l + \alpha_p + \alpha_b + \alpha_t + \varepsilon_l \tag{8}$$

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)$_l$ is an indicator variable that is one if the loan l indicates dividend recap as its purpose, and zero otherwise. We include PE-firm (α_p), bank (α_b), and year-month (α_t) fixed effects.

The results are reported in Table 10. 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 possibly increasing the risk of bankruptcy.

Preexisting Bondholders The large effect of dividend recaps on bankruptcy implies some degree of value-shifting away from pre-existing creditors. Dividend recap loans that are sold to CLOs are senior secured, and thus 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. Bonds are not secured, and thus leveraged loans are more senior than bonds. This implies that bondholders would almost certainly lose out in a dividend recap-induced bankruptcy.

Unfortunately, we do not observe enough firms with publicly traded bonds outstanding at the time of the dividend recap to conduct the IV analysis. However, in OLS results we see a null effect. More specifically, we compare the monthly bond returns of companies that underwent a dividend recap to a matched sample of firms that did not and find no differential pattern in bond returns within a 2-year window. These results are consistent with the idea that while bondholders suffer over a long-run due to higher bankruptcies, there is no impact in the short run. However, these results are derived from a small sample and should be interpreted as suggestive.

6 The Role of Selection

In this section we present and discuss OLS effects, which differ markedly in certain cases 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 A4 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 A5, 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).

Finally, the OLS relationships for deal returns and financials are in Table A6. 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.

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.

7 Conclusion

This paper offers to our knowledge the first effort to understand how new debt affects real and financial outcomes in a setting where it is possible to (a) isolate debt on the balance sheet; and (b) identify causal effects that control for the strong positive selection bias into new debt. We use the setting of dividend recaps in PE, which also allows us to make the secondary contribution of providing the first systematic analysis of these deals, which have become a significant part of the PE playbook.

After instrumenting for dividend recaps using CLO volume underwritten by PE firms’ relationship banks, we show that these deals increase firm risk. They dramatically raise the chance of bankruptcy, which we might expect from higher leverage and interest burdens holding all else equal and in a case where PE firms are not able to counter this higher risk with better distress management or injections of new capital. Note that the existing literature has found mixed results on whether, in general, PE ownership is associated with higher or lower chances of bankruptcy. However, we also find that the deals increase the good outcomes of IPOs and incidence of high revenue growth. For employees, the effects appear largely negative, with employment and payroll growth falling, 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. At the fund level, we see evidence that while there is no effect on IRR, the total value multiple (TVM, or cash-on-cash returns) and the public market equivalent (PME) both decrease. These results are consistent with bringing cash flows forward in the fund's life. Meanwhile, dividend recaps sharply increase the chance of launching a new fund. Altogether, this points to dividend recaps being used to benefit general partners (GPs) by enabling early cash and easier fundraising; they may then focus more on the new fund or become more complacent about subsequent deals, reducing returns to the fund.

Overall, our paper is unique in considering the implications of a PE transaction for stakeholders on both the real and financial side, in contrast to most existing literature on PE that considers only one, such as customers, investors, or employees. We exploit this context to shed light on how a large amount of new debt affects an already leveraged firm. While of course PE-owned firms are different from other types of firms, they are also more representative of most firms in the economy than the public firms who we usually focus on in capital structure studies. We expect that if anything, the ability of PE owners to negotiate with creditors and inject capital (Hotchkiss et al., 2021) should push against finding significant effects of new debt on bankruptcy, and our control group is also LBO targets that are under PE ownership during our period of study, so any effects do not reflect a "PE treatment" effect. Therefore, while the particular magnitude of our results may not generalize to other contexts, we believe they show—for the first time—that when new debt is added in significant amounts, firm risk increases, with a dramatic, positive effect on the chance of financial distress, and negative impacts on wages for employees.

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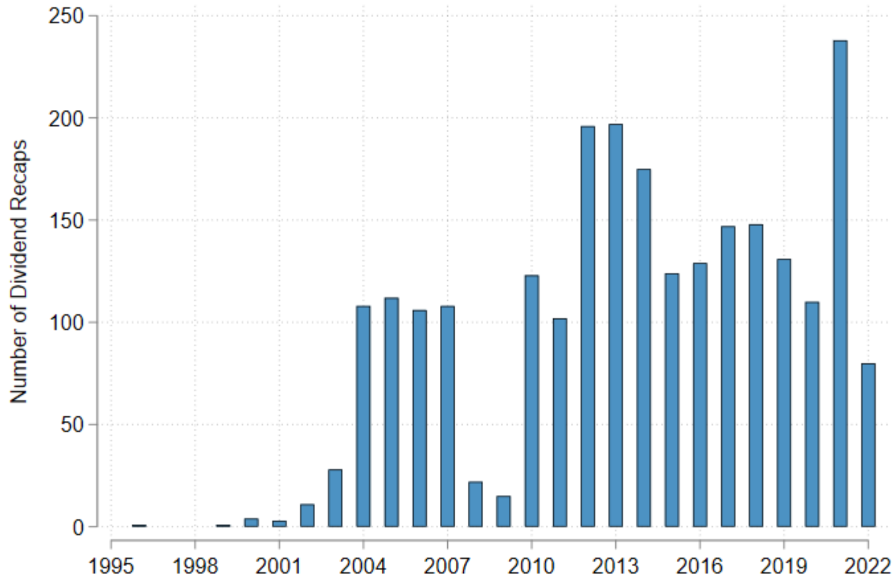
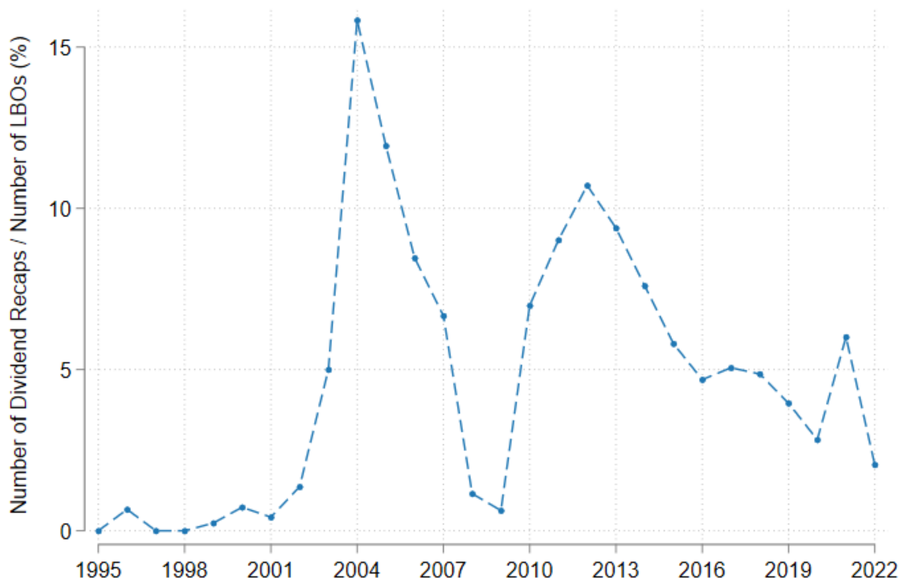
Figure 1: **Dividend Recaps Over Time**

Panel (A): Number of Dividend Recaps by Year

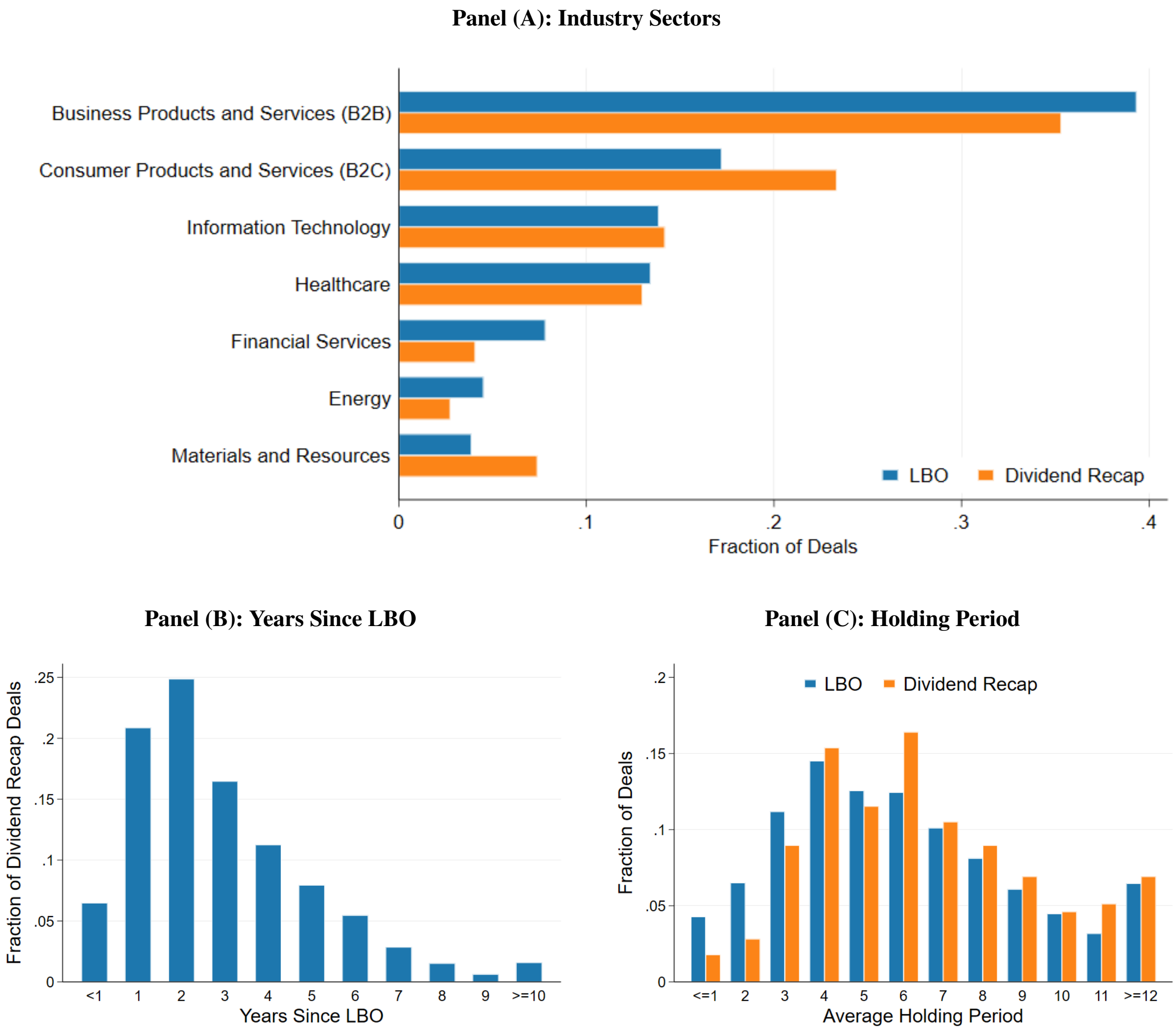


Panel (B): Dividend Recaps as Fraction of LBOs$_t - 2$



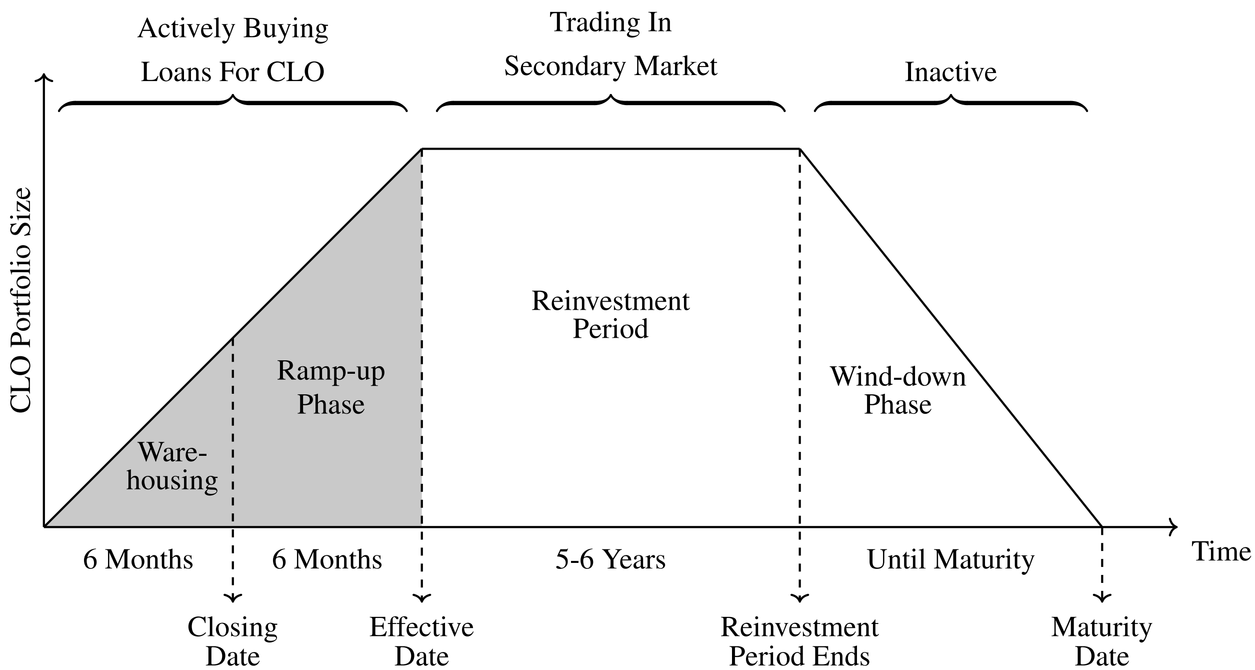
Notes: Figure 1 shows the trend of dividend recapitalizations over the years. Panel (A) shows the number of dividend recap loans by year. Panel (B) shows the number of dividend recap loans each year scaled by the number of LBO deals executed two years ago. We scale by LBO deals two years ago because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

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



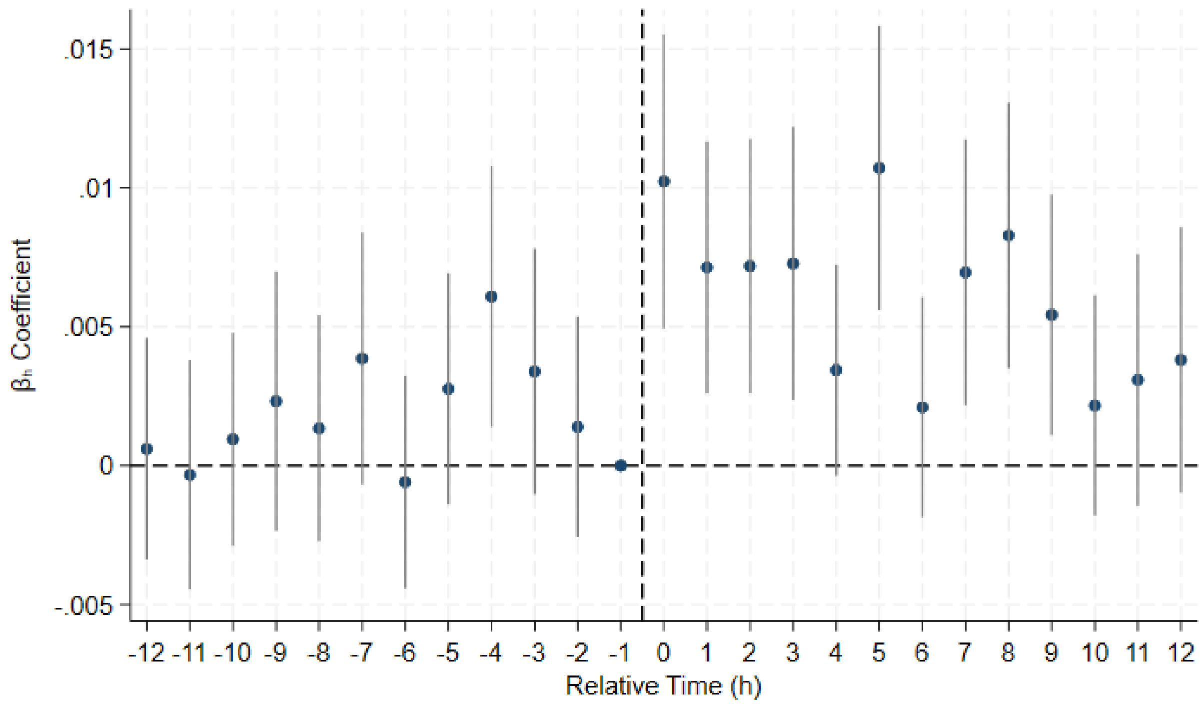
Notes: Figure 2 shows the cross-sectional distribution of all LBO deals and deals with dividend recaps. Panel (A) shows the distribution across the broad industry sectors. Panel (B) shows the distribution of duration between dividend recap loan and corresponding LBO deal date in years. Panel (C) shows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

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



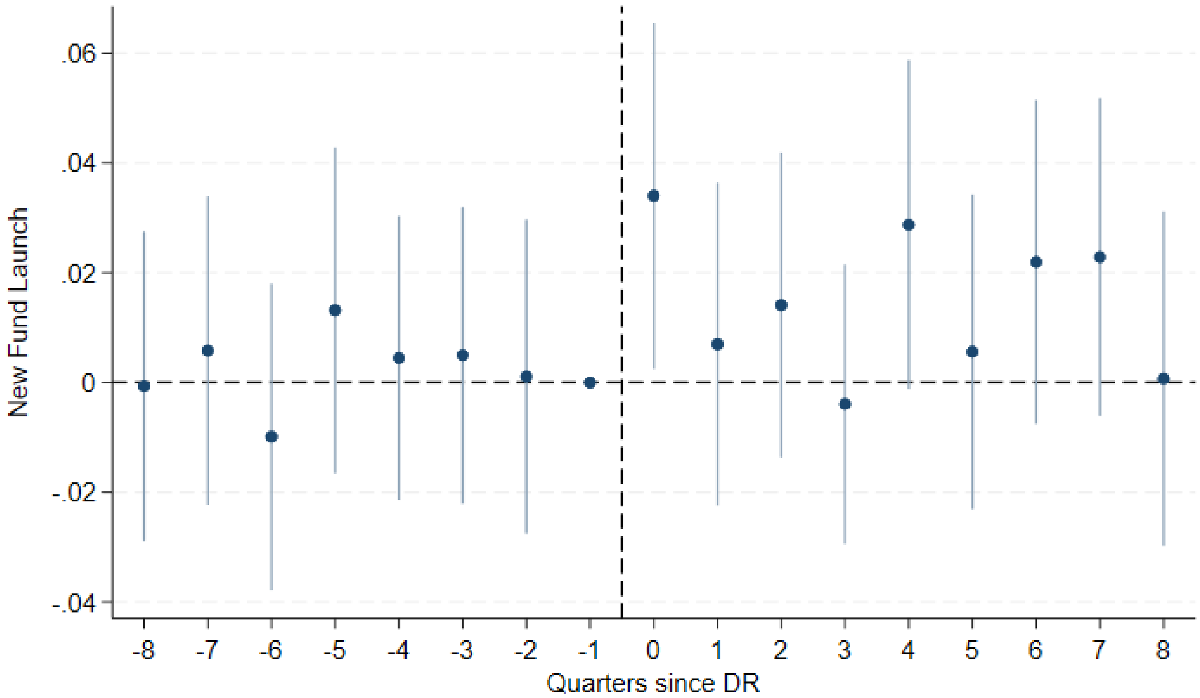
Notes: Figure 1 describes the life cycle of a typical Collateralized Loan Obligation (CLO). The X-axis plots the time since the CLO starts its operation and the Y-axis plots the size of its portfolio over time. CLOs actively purchase loans to fill their portfolio starting 6 months before the closing 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.

Figure 5: **Effect of DR on New Fund Launch**



Notes: Figure 5 shows the impact of a DR on new fund launch. We plot the regression coefficients obtained from a distributed lag model given by Equation 6. the dependent variable is the count of new funds launched by PE-Firm p in h quarters after time t. The main explanatory variable is an indicator variable that is one for the treated deal (i.e., the deal that had the dividend recap), and zero otherwise. We control for stack interacted deal, and stack interacted lag fixed effects. Standard errors are clustered at the stack 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								
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
PE Ownership (%), Entry	3,328	66.81	70	24.32	448	64.82	2,880	67.12
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
Δ Log(Debt) (%)	1,259	27.32	20.7	84.77	232	62.33	1,027	19.41
Δ PE Ownership (%)	1,172	-5.92	-1.17	13.9	184	-6.76	988	-5.76
Portfolio Company Outcomes								
Bankruptcy (%)	61,628	0.47	0	6.84	1,572	1.34	60,056	0.45
IPO (%)	61,628	0.66	0	8.09	1,572	5.47	60,056	0.53
Exit (4-Yr) (%)	24,500	15.90						
Exit (6-Yr) (%)	24,500	18.50						
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				

Continued on next page

Table 1 – continued from previous page

	All				DR		Non-DR	
	N	Mean	Median	SD	N	Mean	N	Mean
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

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	
	8-Year	6-Year	4-Year	6-Year
	(1)	(2)	(3)	(4)
Dividend Recap	8.71**	8.06**	46.79***	33.49*
	(3.50)	(3.14)	-17.85	-18.6
Stack FE	Y	Y	Y	Y
Obs	53539	53539	24500	24500
Y-Mean	0.21	0.17	15.9	18.5
F-Stat	71.97	71.97	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) and (2), the outcome variables are bankruptcy over a 8-year and 6-year horizon, respectively. In columns (3) and (4), 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 8-year or 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: **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 Industry 3-Year Window
	(1)	(2)	(3)	(4)	(5)	(6)
𝟙(Dividend Recap)	10.41**	12.10**	10.47***	12.54***	18.86***	15.14***
	(4.11)	(5.37)	(3.63)	(4.25)	(4.69)	(3.85)
Stack FE	Y	Y	Y	Y	Y	Y
Obs	53539	53539	53539	53539	133562	123614
Y-Mean	0.21	0.21	0.21	0.21	0.26	0.19
F-Stat	58.15	44.06	48.82	46.21	57.26	81.21

Panel (B): Alternative Deal, and PE-Firm Filters and Number of 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)
𝟙(Dividend Recap)	12.70***	11.15***	18.46***	20.26**	11.03*	9.87*	9.12**
	(3.95)	(3.55)	(5.70)	(9.57)	(6.32)	(5.20)	(4.63)
Stack FE	Y	Y	Y	Y	Y	Y	Y
Obs	55646	79416	170949	9988	14829	18677	20864
Y-Mean	0.23	0.20	0.18	0.47	0.39	0.35	0.35
F-Stat	61.03	60.91	47.99	16.08	27.06	37.69	44.50

Notes: This table shows robustness tests of the 2SLS effect of dividend recaps on the probability of bankruptcy using Equation 5. In the first stage, the endogenous variable is a binary variable that is one if the deal d featured a dividend recap transaction, and zero otherwise. The instrument is R-Banks CLO Volume$_{p,t-1}$, which is defined as the average outstanding volume of CLOs underwritten by the PE-Firm p's relationship banks in the month $t-1$. In Panel (A) Columns (1) to (4), we show our results using an alternative set of instruments in the first stage. Panel (A) Columns (5) and (6) show our results using an alternative set of filters on industry (8 industry sectors 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 5: **IV Effect of Dividend Recaps on IPO and Revenue Growth**

Panel (A): Probability Of IPO

	6-Year	8-Year	10-Year
	(1)	(2)	(3)
$\mathbb{1}$(Dividend Recap)	9.43***	10.97***	10.97***
	(3.28)	(3.40)	(3.40)
Stack FE	Y	Y	Y
Obs	53539	53539	53539
Y-Mean	0.12	0.13	0.13
F-Stat	71.97	71.97	71.97

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 6: **IV Effect of Dividend Recaps on Employees**

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 7: **IV Effect of Dividend Recaps on 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)	0.362	-0.920	-5.371***	2.016**	1.093
	(0.611)	(0.637)	(1.211)	(0.832)	(0.682)
Stack FE	Y	Y	Y	Y	Y
Obs	29321	29321	29321	29321	29321
Y-Mean	0.26	0.20	0.30	0.26	0.28
F-Stat	43.76	43.76	43.76	43.76	43.76

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)	4.902	-1.196**	-8.996***	7.062***	0.218
	(3.425)	(0.606)	(1.598)	(1.361)	(0.556)
Stack FE	Y	Y	Y	Y	Y
Obs	30738	30738	30738	30738	30738
Y-Mean	2.72	0.22	0.26	0.34	0.20
F-Stat	44.96	44.96	44.96	44.96	44.96

Panel (C): Deal Financials

	Holding Period	Δ Gross Profit	Δ Debt/Ebitda	Δ Log(Debt)	Δ PE Ownership
	(1)	(2)	(3)	(4)	(5)
Dividend Recap	9.975*	0.602*	45.253***	0.888	-3.571***
	(5.509)	(0.332)	(15.554)	(1.920)	(1.006)
Stack FE	Y	Y	Y	Y	Y
Obs	16842	11975	11321	10690	9126
Y-Mean	5.75	-0.00	-0.32	-0.11	-0.05
F-Stat	31.80	18.83	19.27	15.08	13.93

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

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

Panel (A): Fund IRR

	Average	$\mathbb{1}$[<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 9: **Effect of Dividend Recaps on Fund Launch**

	Number of Funds Launched	
	Quarterly	Annual
	(1)	(2)
$\mathbb{1}$(Dividend Recap) $\times$ Post	0.1218*	0.7789***
	(0.0687)	(0.2228)
Stack-Deal FE	Y	Y
Stack-Year-Quarter FE	Y	
Stack-Year FE		Y
Obs	1,290,691	637,907
Y-mean	0.06	0.21
X-mean	0.01	0.01
F Stat	64.73	64.58

Notes: This table shows the 2SLS effect of dividend recaps on new fund launches using Equation 7. In the first stage, the endogenous variable is an indicator variable that is one for the treated deal (i.e., the deal that had the dividend recap), and zero otherwise, interacted with an indicator variable which switches on after the DR date. 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 $t-1$, interacted with an indicator variable which switches on after the DR date. The outcome variable is the count of new funds launched by PE-Firm p in h lags after time t. Column 1 has data collapsed at the stack-deal-quarter level (with a 8 quarter window around each DR), while Column 2 shows data collapsed at the stack-deal-year level (with a 4 year window around each DR). All models include stack-deal and stack-time fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 10: **Spread on Dividend Recap Loans**

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

Notes: This table shows the effect of dividend recaps on fund-level returns using Equation 8. The endogenous variable is the spread on the loan in bps. The main explanatory variable 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. . ***, **, * 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: Effect On Bankruptcy in Overlapping Samples

	Bankruptcy				
	Census-Sample		Stepstone Sample		
	8-Year	6-Year	8-Year	6-Year	10-Year
	(1)	(2)	(1)	(2)	(3)
$\mathbb{1}$(Dividend Recap)	12.22**	10.67**	11.87*	9.98*	11.87*
	(5.421)	(4.699)	(6.24)	(5.46)	(6.24)
Stack FE	Y	Y	Y	Y	Y
Obs	24500	24500	2066	2066	2066
Y-Mean	0.462	0.367	0.87	0.68	0.87
F-Stat	45.16	45.16	19.24	19.24	19.24

Notes: Table A3 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 and Columns (4)-(6) correspond to the Stepstone-Pitchbook matched sample.

Table A4: OLS Relationship between Dividend Recaps and Portfolio Company Outcomes

Panel (A): Bankruptcy and IPO

	Bankruptcy			IPO		
	8-Year	6-Year	10-Year	8-Year	6-Year	10-Year
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(Dividend Recap)	0.98**	0.63**	1.01**	2.76***	2.68***	2.76***
	(0.38)	(0.32)	(0.40)	(0.55)	(0.54)	(0.55)
Stack FE	Y	Y	Y	Y	Y	Y
Obs	53539	53539	53539	53539	53539	53539
Y-Mean	0.21	0.17	0.23	0.13	0.12	0.13
Adj. R-Sq	0.04	0.04	0.04	0.06	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 A4 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 Employee Outcomes

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

	Average	𝟙[<-75%]	𝟙[-75,0%]	𝟙[0,75%]	𝟙[>75%]
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	𝟙[<-75%]	𝟙[-75,0%]	𝟙[0,75%]	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	𝟙[<-75%]	𝟙[-75,0%]	𝟙[0,75%]	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 A4 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 A6: 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)	0.082***	-0.105***	-0.030	0.086***	0.047*
	(0.023)	(0.018)	(0.027)	(0.028)	(0.028)
Stack FE	Y	Y	Y	Y	Y
Obs	29321	29321	29321	29321	29321
Y-Mean	0.26	0.20	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)	0.896***	-0.135***	-0.021	0.019	0.128***
	(0.157)	(0.017)	(0.025)	(0.029)	(0.027)
Stack FE	Y	Y	Y	Y	Y
Obs	30738	30738	30738	30738	30738
Y-Mean	2.72	0.22	0.26	0.34	0.20

Panel (C): Deal Financials

	Holding Period	Δ Gross Profit	Δ Debt/Ebitda	Δ Log(Debt)	Δ PE Ownership
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Dividend Recap)	1.393***	0.015	1.050***	0.368***	0.008
	(0.226)	(0.010)	(0.370)	(0.071)	(0.014)
Stack FE	Y	Y	Y	Y	Y
Obs	16842	11975	11321	9133	9126
Y-Mean	5.75	-0.00	-0.32	0.60	-0.05

Notes: Table A6 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 A7: OLS Relationship between Dividend Recaps and Fund 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)	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): 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)	0.06*	-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): Deal 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 A7 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 A8: **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 A8 shows how dividend recaps affect fund-level returns using the IV approach. The second stage of the 2SLS empirical specification is:

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

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. $\widehat{\mathbb{1}(\text{Dividend Recap})}_{s,f}$ 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 A9: **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 A9 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 A10: **OLS Relationship of Dividend Recaps on Fund Launch**

	Number of Funds Launched	
	Quarterly	Annual
	(1)	(2)
$\mathbb{1}$(Dividend Recap) $\times$ Post	0.0122**	0.0551***
	(0.0053)	(0.0153)
Stack-Deal FE	Y	Y
Stack-Year-Quarter FE	Y	
Stack-Year FE		Y
Obs	1,296,675	640,135
Y-mean	0.06	0.22
X-mean	0.01	0.01

Notes: This table shows the OLS effect of dividend recaps on new fund launches using Equation 6. The outcome variable is the count of new funds launched by PE-Firm p in h lags after time t. The main explanatory variable is an indicator variable that is one for the treated deal (i.e., the deal that had the dividend recap), and zero otherwise, interacted with an indicator variable which switches on after the DR date. Column 1 has data collapsed at the stack-deal-quarter level (with a 8 quarter window around each DR), while Column 2 shows data collapsed at the stack-deal-year level (with a 4 year window around each DR). All models include stack-deal and stack-time fixed effects and cluster standard errors at the stack level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix B 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.[20] 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.

B.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

[20] 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).[21] 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

[21] 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.

B.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.[22] 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.[23] 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 deal.

We then construct our outcome variables. For exit, we simply consider years from the deal, for example

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

[23] 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)).

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.