Firm Dynamics and Private Credit*

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Abstract

Private credit has emerged as the fastest growing source of firm financing globally, representing as much as one third of non-investment grade corporate debt in the United States. This paper studies how private credit shapes firm dynamics and credit allocation by constructing a novel database from the largest private credit data sources and U.S. Census microdata. Using a synthetic control identification strategy, we find that private credit induces an up-or-out dynamic for borrowing firms by increasing the likelihood of firm exit but also raising employment and innovation among surviving firms. The gains among survivors are driven by the relaxation of borrowing constraints implying segmentation with other financing sources. The increased likelihood of exit stems from active lender credit allocation rather than ex-ante firm risk. Private credit lenders' industry specialization drives these outcomes, suggesting specialized knowledge improves screening. We develop a structural model to explain this implied segmentation, whereby high-risk, high-reward firms sort into private credit. This sorting emerges as an equilibrium outcome of optimal leverage choice across the firm risk distribution.

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1 Introduction

Across the developed and emerging world, private credit lenders—non-bank intermediaries that extend credit directly to firms—have become the fastest-growing segment of the global corporate loan market since the Great Recession. In the United States alone, private credit now represents as much as one third of non-investment grade corporate debt(Board, 2026). Unlike other non-bank lenders such as Collateralized Loan Obligations or marketplace platforms, private credit funds both originate and retain loans on their balance sheets, combining functions that were historically separated across non-bank intermediaries. In addition, they have been shown to closely monitor and actively involve themselves in the governance of their borrowing firms even more than banks (Jang, 2026).

Despite private credit's rapid growth and strong connection with their borrowing firms, the existing literature studying the impact of this lending on firm dynamics remains limited only covers a narrow segment of the borrower distribution. Evidence has been limited by the absence of regulatory disclosure requirements for these lenders and by the private nature of borrowing firms. On account of the opacity of the borrowing firm population, the real impact of private credit on borrowing firms and how it shapes credit allocation more broadly remain largely unidentified. As a result, the extent to which private credit represents a new source of intermediation, as well as, its broader implications for economic growth and financial stability remain open issues.

This paper constructs a novel dynamic dataset of private credit borrowing firms to assess the nature of private credit lending. Using this dataset, it employs a synthetic-control identification strategy to measure the impact of private credit financing on borrowing firms' dynamics. To then interpret the implications of our empirical evidence, we develop a tractable model of heterogeneous firms and lenders that rationalizes the paper's findings and explains interaction with the broader intermediation market. Future extensions of this paper will quantify the impact of private credit on their borrowing firms and overall credit allocation by calibrating the model to our reduced-form estimates

To study the profiles and dynamics of these otherwise opaque borrowing firms, the paper novely links private credit borrowers from the largest private credit datasets, Preqin and PitchBook, to U.S. Census micro-data known as the Longitudinal Business Database (LBD). This linkage provides a time series of firm-level measures of employment, payroll, revenue, industry, age, exit, and patenting. Census longitudinal identifiers allow us to track these outcomes consistently before and after private credit financing. The merged dataset covers more than 17,000 borrowing firms and allows the paper to study the entire distribution of lenders and borrowers in the private credit market.

Through novel stylized facts about borrowing firms, the paper shows private credit is likely expanding overall credit availability and segmenting with other lenders. Over 40% of private credit lending is concentrated in three industries, with particular exposure to technical and scientific services. Firms in these intangible-intensive industries typically face limited access to debt markets due to scarce collateral and

volatile cash flows (Hall and Lerner, 2009) and are empirically not a focus of bank lending (Chodorow-Reich et al., 2022). In addition, we document that the median borrower is comparable in age to the median private U.S. firm but younger than publicly traded firms. They are typically a magnitude larger in size as measured by employees than the typical private firm but a magnitude smaller than the typical public firm.

To plausibly identify private credit's particular impact on a broad set of firm dynamics, the paper adapts the synthetic-control identification strategy of Davis et al. (2025). This approach leverages the universe of firms in the U.S. census data as controls by building a robust set of granular cells to compare to firms treated with private credit in the absence of an exogenous shock. To isolate the effects of receiving financing from this new technology, we focus on each treated firm's first documented private credit loan and require a five-year observation window around the event. We also account for private equity sponsorship through an interaction term that separates its effects from pure private credit treatment.

We find that private credit generates a tradeoff for borrowing firms that differs meaningfully from other financing sources and resembles the "up-or-out" dynamic seen for young firms. For surviving firms, private credit lending boosts short-term employment growth and raises innovation over both short and long horizons, as demonstrated by an increased propensity to both apply for and receive patent grants. However, it also leads to increased firm exit over a two-year horizon. This result is novel, as similar firms in the cross section of the U.S. economy are typically net job destroyers (Haltiwanger, Jarmin and Miranda, 2013), and the literature generally documents negative employment effects following increases in leverage or private equity buyouts (Giroud and Mueller, 2021; Mittal, 2026).

Our analysis of the cross sectional heterogeneity of the baseline impacts identifies the relaxation of borrowing constraints as the dominant channel through which private credit generates real effects. Employment and innovation effects decline monotonically across firm age, size, and liquidation value quartiles. All of which are conventional reduced form proxies for borrowing constraints in the reduced form literature. The gradient across liquidation values is particularly informative given that private credit disproportionately lends to intangible-intensive industries where traditional collateral-based debt is unavailable. Consistent with firms misallocating resources prior to treatment due to binding borrowing constraints, private credit triggers significant internal reallocation of labor across establishments. Together these patterns indicate that private credit reaches firms excluded from debt markets, implying segmentation between lenders rather than substitution. It complements prior evidence that has shown when intangible-intensive firms lose lending relationships their innovation rates decline (Hombert and Matray, 2017).

We further demonstrate that the increased likelihood of exit is driven by active lender credit allocation rather than ex-ante firm risk or leverage. While we do not directly observe firm balance sheets, we construct proxies for average industry-level leverage and liquidation value in a give year to test whether exit is concentrated among firms most exposed to these channels. We find no meaningful gradient across either distribution, ruling out both as primary drivers. As a positive test for the active lender allocation hypothe-

sis, we decompose the treated sample into firms who receive follow-on financing within two years of initial treatment and those who do not. This results reveal a divergence: single-treatment firms exhibit positive exit likelihood near the pooled baseline while multi-treatment firms see a decline in exit likelihood. We interpret this as private credit lenders actively identifying firms with viable continuation prospects while cutting off nonviable counter-parties. In shot, the funds aim to recycle capital toward more productive uses rather than passively absorbing the exit risk of inherently fragile borrowers.

We hypothesize that the mechanism which enables private credit to reach otherwise constrained borrowers is an individual fund's industry concentration and expertise. We adapt the industry specialization measure of Haque, Mayer and Wang (2025) to test whether impacts are differentiated when firms borrow from lenders with expertise in their industry. This is motivated by our evidence that private credit portfolios are highly concentrated in a narrow set of sectors. Across employment growth, innovation, and exit, effects are systematically larger among firms matched to expert lenders. The contrast is sharpest for innovation, where patent applications among firms with an expert lender are nearly double those without, consistent with private credit evaluating and financing innovative activity requiring the deepest sectoral knowledge.

Through a set of robustness checks, we argue that our results are not mechanically driven by private equity sponsorship or selection on factors unrelated to private credit treatment. We augment the baseline specification with an interaction for private equity sponsorship status at treatment and, using a longitudinal record of sponsor relationships, for potential subsequent private equity involvement following treatment. Effects persist across all outcomes with modest attenuation. This provides a lower bound on the true private credit impact given that sponsor status is only observed within the treated sample. We further show through a novel loan intensity measure that effects scale monotonically with financing intensity and differ systematically between single and multi-treated firms, both patterns inconsistent with selection and consistent with private credit as the operative channel. Although we cannot conclusively rule out selection given the opaque nature of these borrowers, our results are robust to observable variation in the U.S. Census data.

To interpret our empirical findings, the paper develops a tractable model of heterogeneous firms and dual lenders. In the model, firms differ in the volatility of their productivities and endogenously choose between two different lenders: traditional banks and private credit. Banks differ from private credit in two key ways: banks face capital regulation that tightens with borrower risk, while private credit operates outside the regulatory purview but faces a higher and endogenously priced cost of debt.

We find that banks serve safer firms while private credit serves riskier ones, a segmentation that arises endogenously from differences in optimal funding structures and binding regulation. The model delivers closed-form expressions for optimal lender leverage and a unique segmentation threshold. Banks optimally operate at higher leverage due to cheap deposit funding, which exposes their equity holders more sharply to default losses on risky borrowers and causes the loan rate they charge to rise steeply with firm risk. Private

credit, with its increasing marginal funding cost, endogenously deleverages away from risk. This generates a crossing of joint surplus curves that partitions the firm distribution: safer firms sort to banks and riskier firms to private credit. Notably, segmentation can arise even without regulation, with capital requirements tightening the boundary but not its source. A planned calibration will discipline the model to the empirical patterns documented in the paper, quantifying the relative contribution of the funding structure and regulatory channels to the observed market segmentation.

This paper contributes to three related literatures: the emerging study of private credit, the literature on non-bank financial intermediation, and the literature on how financing structures shape firm dynamics. There is a voluminous literature on the structure of private credit as a lending technology. One strand of private credit research analyzes how private credit lending compares to bank lending in terms of loan structure and monitoring ability (Jang, 2026; Haque, Stefanescu and Mayer, 2026; Rintamäki and Steffen, 2025). Another focuses on how it may complement and substitute for bank lending (Davydiuk, Marchuk and Rosen, 2024; Chernenko, Ialenti and Scharfstein, 2024; Loumioti, 2022; Haque, Jang and Wang, 2025). Another explores fund performance and market growth (Munday et al., 2018; Erel, Flanagan and Weisbach, 2024), while broader surveys describe the industry's rapid expansion (Block et al., 2024).

However, due to the opacity of the market, much less is known about the nature of the borrowing firms. The closest studies to this are Jang et al. (2026), Haque, Stefanescu and Mayer (2026), and Davydiuk, Marchuk and Rosen (2024). Jang et al. (2026) compares private credit borrowers to firms using other lending types, but does not study firm dynamics. Haque, Stefanescu and Mayer (2026) and Davydiuk, Marchuk and Rosen (2024) examine only a narrow set of outcomes for just BDC-financed or bank-linked firms. By linking borrowing firms to U.S. Census micro-data, this paper is able to measure a broader set of borrower characteristics including wages, revenue, innovation, and exit for the universe of private credit borrowers. In addition and most originally, this paper provides the most robust set of evidence on how private credit shapes these aspects of firm dynamics over time.

A broader literature documents the growth of non-bank financial intermediation across credit markets, including syndicated lending (Irani et al., 2021) and mortgage markets (Buchak et al., 2018; Sarto and Wang, 2022; Buchak et al., 2024), as well as non-bank provision of corporate credit more broadly (Chernenko, Ialenti and Scharfstein, 2024; Haque, Jang and Wang, 2025; Erel, Flanagan and Weisbach, 2024). This paper contributes by identifying where private credit segments itself within this broader landscape, showing how it partitions the firm lending market with banks and how its relationship-intensive model distinguishes it from the role non-banks play in these other markets.

Another literature studies how capital structure and external financing shape firm growth, innovation, and exit across a range of asset classes. This paper contributes most directly to work examining the real effects of PE, VC, and bank financing (Cornaggia et al., 2015; Hombert and Matray, 2017; Lerner, Sorensen and Strömberg, 2011; Puri and Zarutskie, 2012; Davis et al., 2014; Mittal, 2026; Davis et al., 2025). It identifies

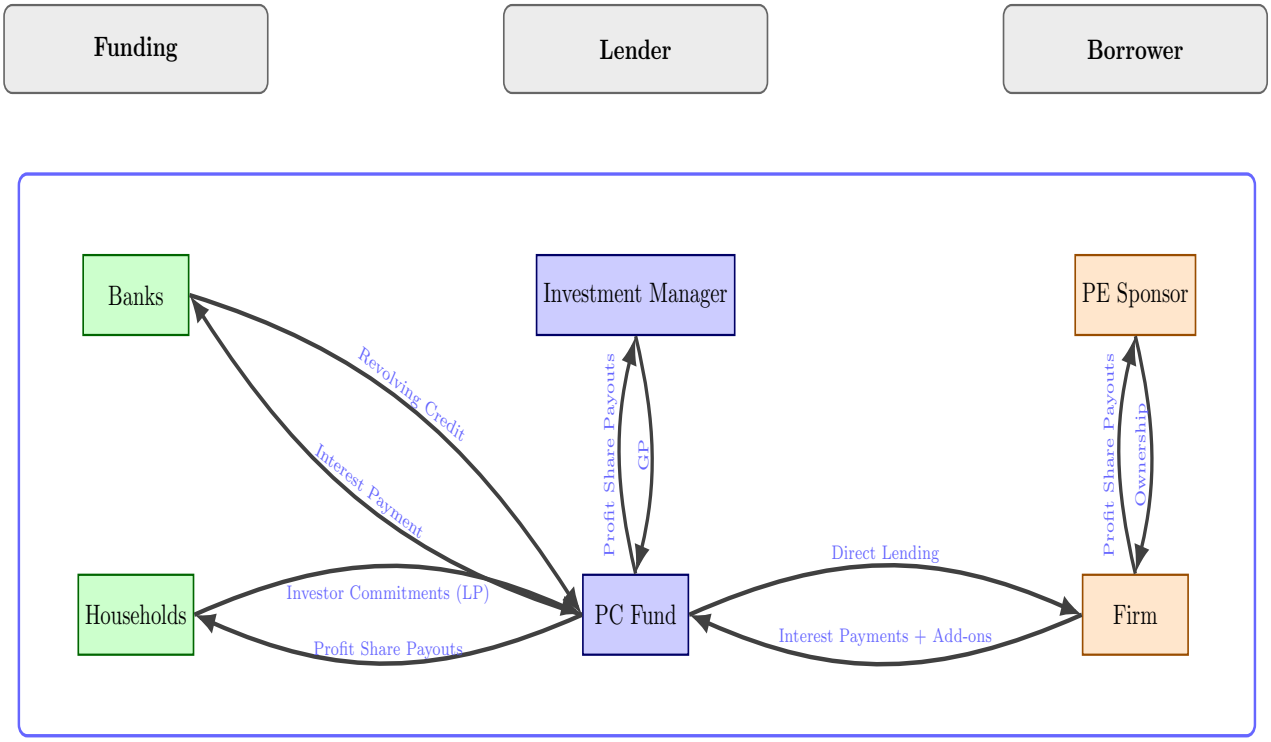
the impact of private credit as a distinct financing source and highlights how its real effects compare to and differ from those documented in these adjacent markets.

2 Institutional Background

We begin with a background to the private credit market, emphasizing two defining features that shape both our identification and modeling strategies: its focus on lending to PE-backed firms and its predominantly equity-funded structure. It also highlights private credit's distinctiveness among non-bank lenders, as it both fully originates its own loans and closely monitors the firms it finances.

Private credit differs from other non-bank lenders by originating and holding its loans on its balance sheet, a feature that may generate real effects for their borrowing firms. As depicted in figure 1, private credit lenders directly lend to firms without a syndicate bank. This is unlike the other main corporate non-bank lender, collateralized loan obligations (CLOs), that typically buy these loans from a bank. In addition, private credit lenders maintain these loans on their balance sheet and typically do not syndicate them (Block et al., 2024). This contrasts with the function of non-banks in other lending markets, notably mortgages, where non-banks syndicate their loans and only banks maintain them on their balance sheets (Buchak et al., 2024).

Figure 1: Structure of Private Credit Lending



Unlike other non-bank lenders that operate at arm's length from their borrowing firms, private credit maintains close, bank-like oversight of its borrowers. Jang (2026) has shown that private debt lenders monitor with the same intensity as banks following origination and show more covenant violations resulting in

alterations of loan structure than bank loans. Furthermore, Block et al. (2024) find that private debt lenders monitor more intensely than syndicated loan lenders. Only 50% of borrowers in this latter market provide information to their lenders on a monthly basis (Gustafson, Ivanov and Meisenzahl, 2021). However, private debt lenders in the survey of Block et al. (2024) typically receive monthly information from nearly 90% of their borrowers.

In isolating private credit's real effects, it is important to note that these lenders disproportionately finance private-equity-sponsored firms as depicted in figure 1. Jang et al. (2026) have shown that 75% of private credit borrowers are backed by a private equity firm, while only 15% of bank borrowing firms are private equity sponsored. This is notable in our context since private equity has been robustly documented as both associated with and a causal factor in a variety of firm dynamics. This includes differential impacts on employment growth and firm productivity depending on the purpose of the private equity buyout (Davis et al., 2025), with public to private transactions showing negative impacts and private to private transactions having positive impacts. It also has been associated with an increase in firm patent quality (Lerner, Sorensen and Strömberg, 2011).

While private credit uses both debt and equity to fund investments (figure 1), it relies overwhelmingly on equity unlike banks, a feature we will exploit in modeling these intermediaries. The equity funding for private credit lenders takes the form of commitments from limited partners in which the investment manager that owns the fund is the general partner. The debt financing for private credit usually consists of revolving credit lines from banks. While public-private credit funds known as Business Development Companies (BDCs) possess an equity share of assets of greater than 50%, privately structured private credit lenders typically use more than 90% equity to fund their assets (Block et al., 2024). Bank equity comes in the form of shareholder equity, while its debt is largely composed of household deposits. Banks in contrast maintain equity at approximately the regulatory minimum of 10%. These features are both persistent aspects of both of these lenders over the last fifteen years as seen in figure A2.

Private credit funds also originate their loans out of a fund structure similar to private equity funds but unlike banks as noted in the central section of figure 1. In particular, private credit lenders hold their investments in pass through investment funds that typically have a lifespan of 5-6 years in which investors have limited or no access at all to their commitments (see figure A1 for a stylized example). Banks, by contrast, hold their loans on the consolidated balance sheet of their bank holding company who is exposed to regulatory oversight.

3 Data Sources and Stylized Facts

To study the profiles and dynamics of private credit borrowing firms, the paper builds a novel merged database. It links private credit borrowers from the largest private credit datasets to U.S. Census microdata. Using this database, the paper finds that these firms are larger than the typical U.S. firm but sit in industries that have not been a focus of traditional debt financing, indicating that private credit relaxes

borrowing constraints and segments with other lenders.

3.1 Data Sources

The paper builds the largest dynamic dataset of private credit borrowing firms by merging two private credit databases, Preqin and Pitchbook, to U.S. Census micro-data that covers the universe of firms in the United States.

In order to identify firms that have received private credit loans, we append two private credit databases, Preqin and Pitchbook. We clean the data to ensure the loan is indeed a directly originated loan from a private credit lender to a U.S. firm without any bank involvement and remove duplicate deals across sources.[1] This results in a dataset of 43,298 private credit loans to 27,625 borrowing firms in the U.S. that were originated from 3,636 lenders. The sample period runs from the earliest documented loan in 1984 through 2022. We stop at this period since this is the final year for which the U.S. Census microdata used in our sample were available at the time of our merge. For each of these loans, we typically observe in the private credit databases borrower and lender names as well as borrower location information. Level of completeness of loan terms varies with about two-thirds of the loans having loan size and a third having maturity and spread information. [2]

However, beyond these details about the borrowing firm, the data rarely contain consistently populated firm-level information, including financials, real characteristics such as age and employment, or any dynamic measures after loan origination. As a result, the data are best suited to identifying which lenders and firms are connected through a given private credit loan, rather than to providing detailed information on the underlying contract or firm outcomes. This limitation reflects the inherent opacity of private credit markets: most lenders and borrowers are privately held and face no regulatory requirement to disclose loan terms or transactions. Despite these constraints, Haque, Mayer and Wang (2025) show that PitchBook is broadly representative, capturing more than 70% of private credit investments through mid 2023.

To better characterize borrowing firms and track them over time, we merge these private credit databases with U.S. Census micro-data. We first link firms to the U.S. business registry known as the Standard Statistical Establishment Listing (SSEL), which contains establishment-level names, addresses, and address geocodes for all U.S. establishments from 1976 to 2022. Using the U.S. Census API, we convert firm addresses in the private credit database into geocodes and implement the fuzzy-matching procedure of Wang and Young (2024), which constructs an aggregate match score as a convex combination of firm-name similarity score and geographic distance between addresses.[3] Finally, to account for potential firm name changes around private credit treatment, we restrict match candidates to a three-year window centered on the year of treatment, following Davis et al. (2025).

[1] See Appendix section A.1 for cleaning process.

[2] See Appendix section B.2 for a detailed breakout of available loan terms and their decomposition across sponsored and unsponsored borrowing firms.

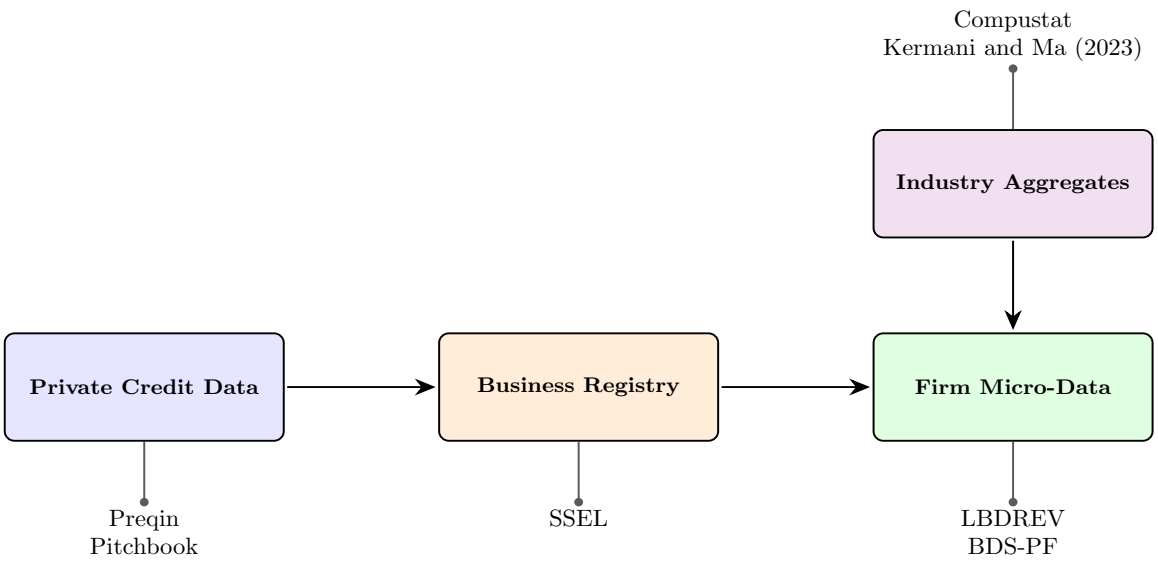
[3] See Appendix A.2 for further details on merge process.

We then link the merged private credit–business registry data to the Longitudinal Business Database Revenue Enhanced (LBDREV). While the lowest level of aggregation in the LBDREV is at an establishment level, the paper focuses on the databases' firm level aggregation as our private credit data is at a firm level. The LBDREV contains detailed information on firm employment, payroll, revenue, age, industry affiliation, exit, and legal form. Crucially, it provides consistent longitudinal firm and establishment identifiers, allowing us to reliably track firms over time following private credit treatment. The LBDREV covers the universe of nonfarm U.S. firms with at least one paid employee which consists of approximately 5.5 million firms as of 2023.

We also link the merged private credit–business registry data to the Business Dynamics Statistics of U.S. Patenting Firms (BDS-PF). This database tracks firms' patenting activity, including the year of patent application and grant. The BDS-PF is constructed by linking the SSEL to administrative data from the United States Patent and Trademark Office (USPTO), following the methodology described in Fort et al. (2025).

Finally, we supplement the firm micro-data with industry-level aggregates at the three-digit NAICS–year level to characterize the typical capital structure and asset composition of each sector. This step is necessary because the LBDREV and BDS-PF contain only limited information on firm financials, preventing us from directly measuring balance-sheet characteristics such as leverage, tangibility, or liquidation value at the firm level. To construct these industry aggregates, we draw on Compustat for standard capital-structure variables (leverage and asset tangibility) and on the asset-level liquidation recovery rates of Kermani and Ma (2023) to construct industry liquidation values. Together, these measures allow us to test for heterogeneity in treatment effects along economically meaningful dimensions of industry financial structure (leverage, tangibility, and liquidation value) that we could not otherwise recover from the Census micro-data alone. Figure 2 illustrates the complete set of linkages among the data sources used.

Figure 2: Data Sources



Through these linkages, the paper constructs the largest dynamic dataset of private credit borrowing firms to date. In total, we observe the real characteristics of approximately 17,500 firms.[4] This coverage allows the analysis to consider the full distribution of private credit borrowing firms, rather than focusing on a narrow segment of the market. By contrast, prior work has been limited to a small set of outcomes for either BDC-financed firms or bank-linked firms (Haque, Stefanescu and Mayer (2026); Davydiuk, Marchuk and Rosen (2024)), which we show correspond to the tails of the private credit borrowing distribution in terms of firm size. While Jang et al. (2026) use a dataset of comparable size to our paper in terms of number of borrowing firms, their data are static rather than dynamic, exclude information on firm wages and revenue, and are not able to situate borrowing firms within the full universe of U.S. firms.

3.2 Descriptive Facts

The paper finds that private credit borrowers tend to be mature and relatively large compared to the U.S. median firm, with meaningful variation by sponsorship status. They are disproportionately located in R&D and intangible-intensive industries that traditional lenders typically underfinance.

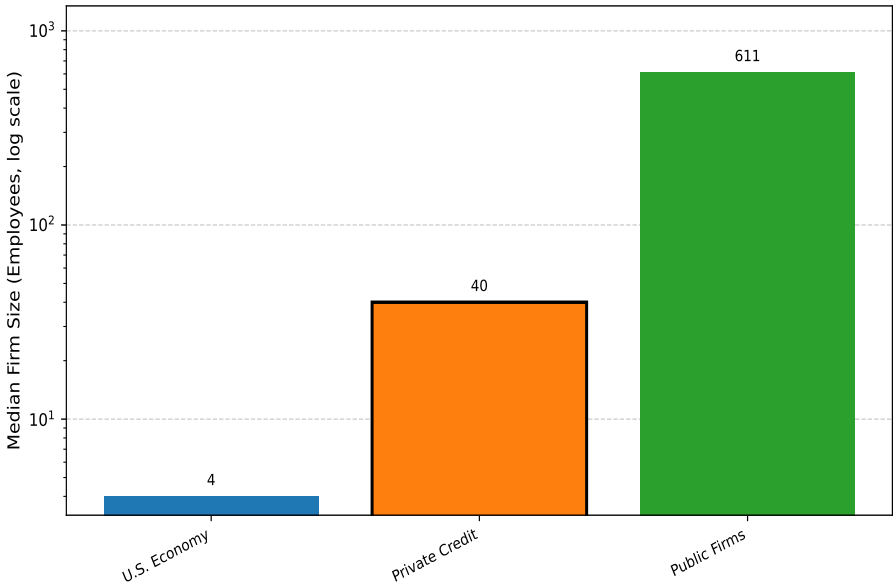
The median borrowing firm in our sample is 10 years old, comparable to the median firm age in the U.S. economy (9 years; Dinlersoz et al. (2024)), but substantially younger than the typical public firm (Kahle and Stulz (2017)). Median ages are similar across sponsored and unsponsored borrowers. This pattern underscores that private credit is not simply a relabeling of venture capital, which typically targets much younger firms (Puri and Zarutskie (2012)). At the same time, the age distribution exhibits substantial dispersion: the 25th and 75th percentiles are 3 and 22 years, respectively. The median age of borrowing firms is notably lower than that reported by Jang et al. (2026), who find a median borrowing firm age of 24 years. Table A4 reports the full set of moments for the borrower age distribution with a decomposition by borrowing firm sponsored status.

As shown in Figure 3, the median private-credit borrowing firm is an order of magnitude larger than the typical U.S. firm, yet substantially smaller than publicly listed firms, placing private credit squarely in the middle market. There is heterogeneity across borrowers: unsponsored firms are much closer in size to the modal U.S. firm at 15 employees, while sponsored borrowers occupy the upper tail of the firm size distribution at 64 employees. This divergence suggests that private credit operates on two distinct margins—financing scaled, sponsor-backed firms and relaxing financing constraints for smaller, non-sponsored borrowers.

Furthermore, the median size of sponsored borrowers closely aligns with the unconditional firm size of sponsored firms reported by Mittal (2026), in her study of private equity target firms, who finds a median of 62 employees. At the same time, borrowing firm size exhibits substantial dispersion, with the 25th and 75th percentiles at 6 and 172 employees, respectively, indicating that private credit serves firms across a

[4] Rounded as per U.S. Census Bureau disclosure guidelines.

Figure 3: Comparison of Firm Sizes



Median firm size by employee count. Private credit results rounded as per U.S. Census Bureau disclosure guidelines. **Source:** Non-private credit data from Dinlersoz et al. (2024).

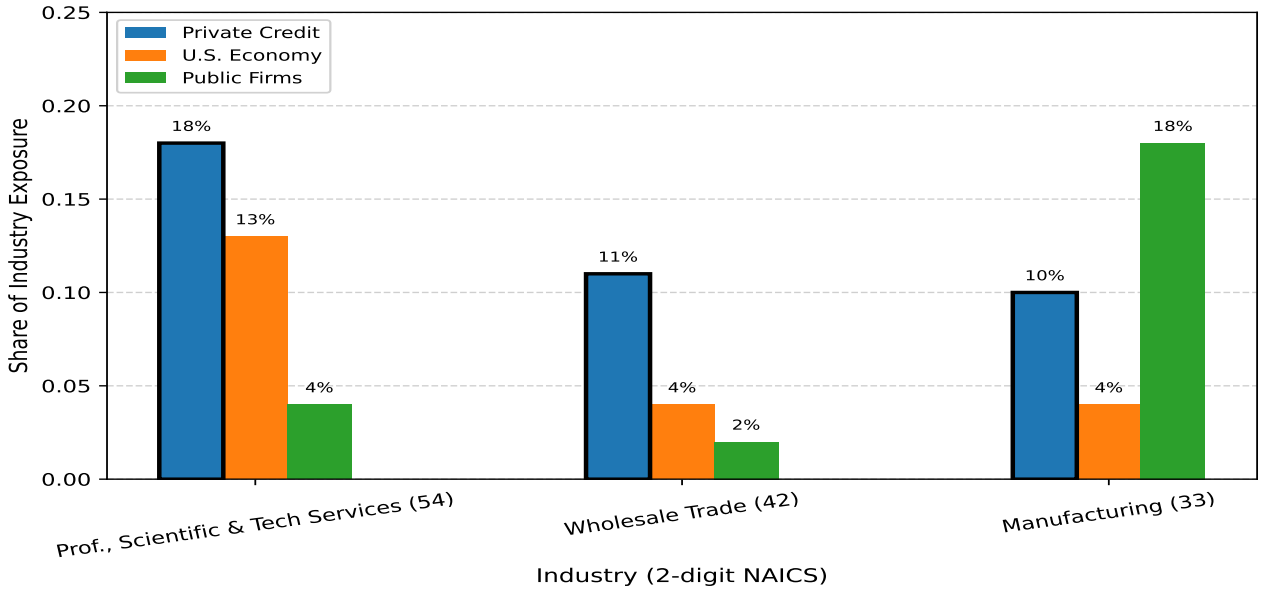
broad range of operational scales. Our median firm size is modestly lower than that reported by Jang et al. (2026), who find a median of 68 employees for private credit borrowing firms. Table A4 reports the full set of moments for the borrower size distribution with a comparison to the cross section of the U.S. economy.

Borrowing firms are highly concentrated in a small number of industries with a strong exposure to industries characterized by having high shares of intangible assets. In particular, 40% of borrowers operate in the three two-digit NAICS industries shown in Figure 4.[5] This aligns with Avalos, Doerr and Pinter (2025) who shows that on a portfolio level that private credit lenders have more than double the industry concentration of banks. Figure 4 points to a clear emphasis on borrowing firms in the professional and scientific services industry. In the cross-section of public firms, this industry is characterized by a high share of intangible assets. As a result, this sector, along with the second most represented industry, relies heavily on cash-flow–based collateral to support borrowing (Caglio, Darst and Kalemli-Özcan (2024)). Relatedly, we find that borrowing firms exhibit substantially higher patenting activity than the broader economy: while only 0.5% of firms have ever filed a patent economy-wide, the mean two-year patenting propensity among borrowers is 16.3%.

The focus on intangible and R&D intensive firms indicates private credit's role in financing firms that are often underserved by traditional lenders and that, in many cases, avoid debt altogether. Banks' exposure to professional and scientific services does not rank among their top five industries in term-loan issuance in the U.S. Y-14 data (Chodorow-Reich et al., 2022), reflecting limited debt supply to these firms. At the same time, private credit's exposure to this industry is disproportionately high relative to the broader U.S.

[5] Table A5 reports the industry exposure by borrowing firm sponsored status.

Figure 4: Comparison of Industry Exposures



Industry classification for private credit firms is defined as the modal industry across a firm's establishments. Private credit results rounded as per U.S. Census Bureau disclosure guidelines. **Source:** Non-private credit data from Compustat, Census BDS.

economy. This overall and relative pattern reflects not only constrained supply from traditional lenders but also limited demand for debt financing: as emphasized by Hall and Lerner (2009), R&D-intensive firms typically favor equity, since debt is poorly suited to financing intangible, highly uncertain innovation and can distort investment incentives.

Last, we find that private credit borrowers are disproportionately organized as corporations, either C- or S-corporations, relative to firms in the broader U.S. economy. In the matched private credit sample, roughly three-quarters of firms are organized as C- or S-corporations, compared with about 40% in the overall U.S. firm distribution (Dinlersoz et al. (2024)), where a much larger share operates as partnership or other legal forms. This disparity is consistent with the earlier finding that private credit borrowers are roughly an order of magnitude larger than the median U.S. firm. Larger firms disproportionately adopt a corporate form as it facilitates access to external capital through standardized equity structures, limits owner liability at scale, and supports the governance and reporting requirements that lenders demand. Table A6 reports the exact exposures to each legal form with a decomposition by borrowing firm sponsored status.

4 Identification Strategy and Firm Dynamics Results

To measure private credit's impact on firm dynamics, the paper employs a synthetic-control strategy, given the absence of exogenous shocks to private credit. We find that private credit financing boosts borrowing firms' growth, raising employment and innovation, while also increasing their exit likelihood.

4.1 Identification Design and Strategy

The paper adapts a synthetic-control–style design to study private credit's impact on firm dynamics in the absence of an exogenous shock. This approach recovers the effect of a firm receiving private credit or not, which reflects a combination of distinct counterfactuals, both the absence of financing and substitution away from alternative funding sources, that we subsequently aim to disentangle later in the paper.

To identify the real effects of private credit lending on borrowing firms, the paper employs the synthetic-control identification strategy of Davis et al. (2025). The ideal experiment would compare firms that exogenously receive private credit financing to otherwise identical firms that do not. In the absence of such broad-based exogenous shocks, this identification instead relies on constructing highly granular control cells from the full universe of U.S. firms, conditioning on observables most closely associated with firm growth and exit dynamics (Haltiwanger, Jarmin and Miranda (2013)). Control cells are defined within each treatment year based on the cross-product of industry (three-digit NAICS), firm size, firm age, and multi-unit status.[6] Firms that ever receive private credit financing in our external database are excluded from the control group.

Within-cell treatment assignment is plausibly quasi-random, as Amiti et al. (2026) document that firm–bank matching in credit markets is largely driven by search frictions rather than systematic selection. This implies that conditional on observable characteristics, which firm receives private credit is close to idiosyncratic.Nonetheless, this strategy cannot fully rule out selection, as we do not observe the private information set of private credit lenders when making lending decisions.

This identification strategy captures the general effect of having private credit versus not having private credit at the time of a firm's earliest documented private credit financing event. We adopt this approach because we do not observe complete balance sheets for treated or control firms. As a result, we cannot directly determine whether the counterfactual of not receiving private credit reflects the absence of financing altogether or the presence of an alternative source of credit. These interpretations imply different mechanisms. Observed real effects may reflect the alleviation of binding borrowing constraints. Alternatively, they may capture the differential impact of private credit relative to other forms of external financing. In the empirical analysis, we attempt to distinguish between these channels by examining heterogeneity in treatment effects across firm characteristics and borrowing intensity.

Nonetheless, the baseline specification can be plausibly interpreted as capturing a transition from prior financing arrangements into private credit, rather than entry into debt financing per se. The median firm age at first observed private credit financing is approximately 10 years as shown earlier, with control firms drawn from comparable age bins, making it likely that many treated firms and similar control firms had

[6] Control cells are constructed based on the cross product of the following categories. Firm size categories are 1–4, 5–9, 10–19, 20–49, 50–99, 100–249, 250–499, 500–999, 1,000–2,499, 2,500–4,999, 5,000–9,999, and greater than 10,000 employees. Firm age categories are 0–5, 6–10, 11–15, 16–20, and 21 or more years. There are 96 industries defined based on three-digit NAICS codes, a dummy for multi-unit status, and the year of treatment.

accessed other financing sources beforehand. By focusing on the earliest observed private credit event, the specification aims to exploit this transition rather than initial debt market entry.

Since we do not observe full balance sheet dynamics or consistently track loan contracts over time, we focus on firm outcomes two years after the initial private credit financing. Although the median stated maturity of these loans is roughly five years, discussions with industry participants suggest that the effective duration is typically closer to two to three years, as loans are frequently called, refinanced, or restructured before maturity. Consequently, estimating effects at the two-year horizon provides a conservative measure of the borrower–lender relationship, capturing the period during which lender control and contractual influence are most likely to be operative.

Formally, we estimate the following specification:

$$Y_{i,t+s} = \beta_{t+s} PC_{it} + \lambda LFIRM_{it} + \sum_{c} \theta_c D_{ict} + \varepsilon_{i,t+s}, \tag{1}$$

where $Y_{i,t+s}$ denotes firm-level outcomes measured s years after the initial private credit treatment year t, including employment growth, patenting propensity, and exit likelihood.

Our dependent variables are constructed as follows. Employment growth is measured using the Davis, Haltiwanger, Schuh (DHS) growth rate, $g_{i,t,t+s} = \frac{Y_{i,t+s} - Y_{i,t}}{0.5(Y_{i,t+s} + Y_{i,t})}$, which provides a symmetric growth measure analogous to a log difference while accommodating firm entry and exit. This measure is bounded between -2 (exit) and $+2$ (entry), limiting extreme distortions and ensuring comparability across firms and time. Innovation outcomes are captured by patenting propensity, defined as $\log\left(1 + \sum_{\tau=t+1}^{t+s} \text{Patents}_{i,\tau}\right)$, which aggregates patent applications or grants over the post-treatment window. The sum begins at $t+1$, excluding the treatment year to address endogeneity concerns arising from deal-coincident patenting activity.

Finally, firm exit is measured using an indicator equal to one if the firm exits by year $t+s$. We measure firm exit using the U.S. Census Longitudinal Business Database (LBD) firm exit flag, which is defined as the disappearance of a firm identifier when all establishments associated with that firm cease operations (Haltiwanger, Jarmin and Miranda, 2013). By requiring the closure of all establishments, it avoids misclassifying ownership changes such as mergers, acquisitions, or restructuring as exits, providing a consistent firm-level measure that is robust to changes in organizational structure.

PC_{it} is an indicator equal to one in the firm's first observed year of private credit financing. The coefficient β_{t+s} captures the average effect of transitioning into private credit relative to otherwise comparable firms. $LFIRM_{it}$ measures pre-treatment firm growth over the two-year period from $t-2$ to t, where the specific measure mirrors the outcome variable. Although for exit regressions, it is lagged employment growth. This allows us to control for differential pre-trends in firm dynamics prior to private credit treatment. It absorbs potential unobservables correlated with both private credit selection and subsequent outcomes.

D_{ict} denotes the saturated set of control-cell fixed effects. Standard errors are clustered at the firm

level. We do not include firm fixed effects, as identification relies on this cross-sectional variation within cells, which firm fixed effects would absorb.

4.2 Impacts on Firm Dynamics

We find that private credit presents a tradeoff for firms, as it leads to higher employment growth and innovation for borrowing firms, while also being associated with higher firm exit rates.

Table 1 reports baseline estimates of employment growth following a firm's first observed private credit financing.[7] Focusing on firms that survive through the first two years following private credit treatment, we find that firms receiving private credit increase employment by 9.0% more over two years relative to firms within the same granular control cell. Among firms that survive and can be tracked for five years, the impact remains persistent and increases by nearly 14% more than comparable firms. We further find that firms receiving multiple private credit treatments within the t to t+2 window experience larger employment gains than those treated only once, growing employment by 17.9% more than control firms[8].

Table 1: Employment Growth Following Private Credit Financing

Employment Growth (g)	*2 years* (1)	*5 years* (2)
Treatment (β)	0.09014***	0.1382***
	(0.005818)	(0.01006)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Firm g	Y	Y
Weighted Employment$_t$	Y	Y
Observations	139,700,000	103,900,000
R^2	0.044	0.053

Sample includes matched two-year and five-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Thus, the majority of the baseline employment growth materializes within the first two years. This is consistent with the realized duration of private credit loans, which based on industry evidence typically runs two to three years. This is despite a median contractual maturity of five years for private credit loans as loans are frequently refinanced or called early. The timing of the employment response is therefore consistent with the effective window of active lender engagement.

These baseline results concerning employment growth are notable for two reasons and indicate that private credit is operating as a distinct source of financing. First, firms with comparable age and size pro-

[7] We also estimate analogous specifications for the overall wage bill (Table A39) and pay per employee (Table A40). We find no significant effect on the overall wage bill, but find a decline in pay per employee.

[8] See appendix table A35 for breakout of results.

files in the broader U.S. economy are, on average, net job destroyers (Haltiwanger, Jarmin and Miranda, 2013). Against that benchmark, positive net employment growth following private credit financing is novel. Second, the estimates differ from standard priors about highly levered and private equity backed firms. Private credit has been shown to be associated with elevated leverage relative to traditional bank lending. Yet higher leverage is typically linked to employment contraction, consistent with debt overhang and financial distress mechanisms (Giroud and Mueller (2021)).

In addition, the private equity literature generally finds statistically insignificant or negative unconditional employment effects on average (e.g., Davis et al. (2014); Davis et al. (2025); Mittal (2026)). In contrast, we estimate positive net employment growth following private credit financing. This pattern suggests that the employment response to private credit provision is not well explained by standard leverage or buyout channels alone but a distinct private credit one.

Table 2 reports the effects of private credit financing on firm-level innovation activity. We find positive and statistically significant increases in both patent applications and grants for firms that survive through the first two years post private credit treatment. Patent application propensity increases by 3.4% and patent grant propensity increases by 6.3% relative to control firms. After five years, applications rise by 7.2% and grants by 14.4%, more than doubling their two-year effect. The dynamic pattern is consistent with an innovation channel operating through hiring and R&D expansion. As shown in the firm profiles, private credit borrowers are disproportionately R&D-intensive firms. Because innovation takes time to translate into observable patent output, the larger increase in applications at the five-year horizon is economically plausible relative to the relative difference in growth at the two and five year horizon for employment growth.

Table 2: Innovation Propensity Following Private Credit Financing

	Applications g		**Grants** g	
	2 years	*5 years*	*2 years*	*5 years*
	(1)	(2)	(3)	(4)
Treatment (β)	0.03365***	0.07197***	0.0631***	0.1444***
	(0.004909)	(0.01058)	(0.00540)	(0.01159)
Industry × Size × Age × Unit-Type × Year FE	Y	Y	Y	Y
Lagged Propensity	Y	Y	Y	Y
Weighted Employment$_t$	Y	Y	Y	Y
Observations	135,900,000	100,900,000	135,900,000	100,900,000
R^2	0.352	0.381	0.352	0.381

Sample includes matched two-year and five-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

These findings complement prior evidence on financing disruptions and innovation. Hombert and

Matray (2017) show that when firms lose bank lending relationships, innovation declines, particularly among smaller and intangible-intensive firms. In contrast, we document increased patenting activity following the formation of a private credit lending relationship. The results are therefore consistent with private credit allowing firms to establish lending relationships that they otherwise may not have obtained, rather than merely substituting for other sources of capital. Although, our findings differ from the private equity innovation literature. Lerner, Sorensen and Strömberg (2011) find little change in overall patent propensity following buyouts, with improvements concentrated in patent quality as measured by citations.

Table 3 reports the effects of private credit financing on firm exit likelihood. It uses a broader sample of treated firms than the previous two estimations as it is not restricted to firms surviving the first two years after receiving private credit treatment. We find that treated firms experience a 2.9% relative increase in exit likelihood within two years following initial private credit treatment and by nearly 6.0% within five years.

Table 3: Firm Exit Likelihood Following Private Credit Financing

Exit Likelihood	*2 years* (1)	*5 years* (2)
Treatment (β)	0.02935***	0.0599***
	(0.002463)	(0.00410)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Firm g	Y	Y
Weighted Employment$_t$	Y	Y
Observations	160,600,000	140,900,000
R^2	0.073	0.154

Sample includes all firms at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

We do not interpret the overall increase in exit likelihood as reflecting a generic leverage effect. Private credit is often associated with higher firm leverage relative to traditional bank financing (Haque, Stefanescu and Mayer (2026)), and higher leverage more broadly is linked to elevated exit risk (Dore and Zarutskie (2023); Campbell, Hilscher and Szilagyi (2008)). One might therefore view our exit results as a mechanical consequence of increased leverage of borrowing firms. However, leverage build-ups are typically accompanied by contraction in firm activity, including declining medium-run employment growth (Giroud and Mueller (2021)). In contrast, private credit borrowers in our sample exhibit rising employment and innovation during the same period in which exit risk increases. The joint pattern of expansion and elevated exit is therefore difficult to reconcile with a standard leverage-fragility mechanism. However, we will robustly test this channel in more detail in section 5.2.

In sum, receipt of private credit presents a trade-off for borrowing firms: conditional on survival, they

experience higher employment growth and patenting propensity, but face a greater likelihood of exit. This dual pattern resembles the "up-or-out" dynamics of young firms documented by Haltiwanger, Jarmin and Miranda (2013), who show that conditional on survival, young firms grow faster than mature firms but also exit at higher rates. Whether private credit amplifies this familiar dynamic or operates through a distinct mechanism, and what more generally drives the real effects we document, is the question we turn to in Section 5.

5 Understanding Mechanisms

We document in the following section that private credit effects are concentrated among the youngest, smallest, and most collateral-constrained firms, identifying relaxation of borrowing constraints as the dominant channel explaining private credit's effects on firm dynamics. In addition, we provide evidence that the increased exit likelihood is driven by active lender credit allocation rather than merely borrower risk selection. Last, we show that private credit's firm industry expertise emerges as the mechanism that enables private credit to reach these constrained firms in the first place and to exercise more informed credit allocation over the lending relationship.

5.1 Relaxing Borrowing Constraints or Credit Substitution

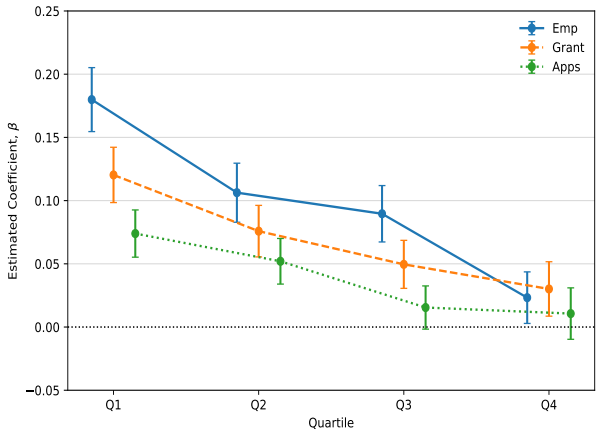
Borrowing constraints emerge as the dominant channel through which private credit shapes firm outcomes, as evidenced by systematic heterogeneity in treatment effects across firm age, size, and liquidation value quartiles.

Our identification admits two counterfactuals concerning our baseline growth results: private credit may relax borrowing constraints for firms without viable outside financing, or provide an incremental benefit to firms that already have access. To examine whether the baseline effects are driven by the relaxation of borrowing constraints, we first analyze heterogeneity in treatment effects across borrower age and firm size at the time of treatment. If private credit primarily operates by expanding access to financing for firms with limited outside options, the effects should be strongest among younger and smaller firms. They are generally more financially constrained since they have shorter credit histories, fewer pledgeable assets, and weaker banking relationships (Berger and Udell (1998), Petersen and Rajan (1994)). Consistent with this view, firm age and size are widely used proxies for financing constraints in the corporate finance literature (Hadlock and Pierce (2010)).

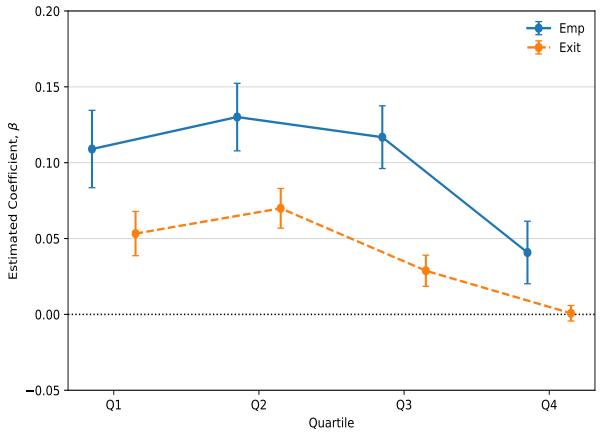
Formally, we estimate equation 1 separately on four subsamples defined by quartiles of firm age and, separately, on four subsamples defined by quartiles of firm employment. Both measures are taken at the treatment year t, the year of the firm's first private credit borrowing event. The resulting sequence $\{\hat{\beta}^{q}_{t+s}\}^{4}_{q=1}$ in Figure 5 traces out the heterogeneity of the private credit effect across the age and size distributions.[9]

[9] See Appendix Tables A7 – A11 for estimates found in these figures.

Figure 5: Heterogeneity in Age-Size Cross Section



Age quartiles for 2 year impact



Employment quartiles for 2 year impact

Consistent with a dominant relaxing of borrowing-constraint mechanism, the treatment effects for employment growth are concentrated among younger and smaller firms as shown in Figure 5. The youngest firms in particular exhibit employment growth rates more than double the pooled baseline estimate of 9.1% in table 1. In contrast, the estimated employment growth across these two dimensions monotonically decline across the age and size distributions, becoming nearly statistically indistinguishable from zero for the largest and oldest firms.

Innovation responses exhibit a similar pattern across the age distribution, while exit likelihood displays this monotonic pattern across the size distribution. In the age-based results on the left-hand side of Figure 5, the effects on grant awards and patent applications are concentrated in the first two quartiles and become statistically insignificant for older firms. On the right-hand side, the effect on exit likelihood declines monotonically across size quartiles and becomes statistically insignificant for the largest firms. The empirical "up-or-out" pattern documented in the firm dynamics literature is driven by firm age, with young firms exhibiting both higher growth conditional on survival and higher exit rates. The fact that our exit effects load on size suggests that the up-or-out dynamic amplified by private credit is driven by a distinct mechanism, one we examine in the following subsection.

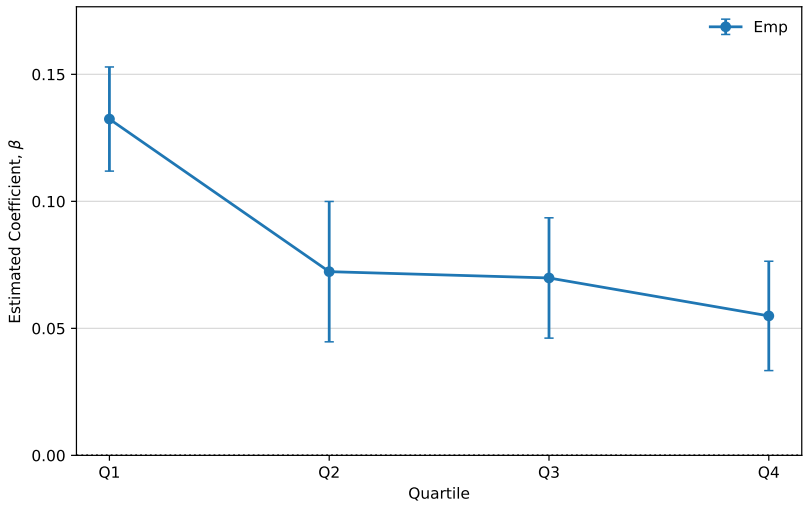
To further examine whether the baseline effects are driven by the relaxation of borrowing constraints, we next analyze heterogeneity in treatment effects across firm liquidation values at the time of treatment. A long theoretical literature establishes that firms with lower liquidation values face tighter borrowing constraints, as the recoverable value of assets in default determines the credit that lenders are willing to extend (Kiyotaki and Moore (1997), Hart and Moore (1994), Shleifer and Vishny (1992)). Empirical work confirms this link: firms with more redeployable assets receive larger loans on better terms, while the secular shift toward intangible capital has shrunk debt capacity across the corporate sector (Benmelech, Garmaise and Moskowitz (2005), Falato et al. (2022)).

This channel is especially relevant in our setting, as the descriptive evidence in Figure 4 shows that private credit disproportionately lends to firms in intangible-intensive industries. This is precisely the firms least able to pledge collateral through traditional channels. If private credit relaxes borrowing constraints, its effects should therefore be most pronounced in industries with the lowest liquidation values.

Although we do not directly observe liquidation values at the firm level, we construct them by using asset-type recovery rates from Kermani and Ma (2023) and then follow the approach in Li et al. (2025) to create aggregate industry-year liquidation values.[10] Specifically, for each 3-digit NAICS industry-year, we compute the mean liquidation value across Compustat firms in that industry-year, and then layer this industry-year mean onto each firm in our sample, across treated and control firms.

We estimate equation 1 separately on four subsamples defined by quartiles of industry liquidation value, measured at the year of the firm's first private credit borrowing event. The resulting sequence $\{\hat{\beta}^{q}_{t+s}\}^{4}_{q=1}$ in Figure 6 ordered from lowest to highest liquidation value, traces out the heterogeneity of the private credit effect across the liquidation-value distribution.[11]

Figure 6: Heterogeneity across Liquidation Values



Liquidation value quartiles for 2 year employment growth.

Consistent with the borrowing-constraint mechanism, the treatment effects are strongest among firms in the lowest liquidation-value industries and decline monotonically across the distribution. As shown in Figure 6, employment growth in the lowest quartile is more than 50% greater than the pooled baseline estimate in Table 1, mirroring the pattern observed for the youngest and smallest firms. Although unlike the

[10] For each 3-digit NAICS industry k in year t, the liquidation value is constructed as $\text{LiqVal}_{kt} = \frac{\sum_j \lambda^j_k A^j_{kt}}{AT_{kt}}$, where λ^j_k is the recovery rate for asset type j in industry k from Kermani and Ma (2023) (cents recovered per dollar of book value upon liquidation), A^j_{kt} is the aggregate book value of asset type j for firms in industry k in year t, with asset types $j \in \{\text{PPE}, \text{inventory}, \text{receivables}, \text{intangibles}\}$, and AT_{kt} is total book assets, which normalizes LiqVal_{kt} to a $[0,1]$ scale. Because recovery rates are provided at the SIC level, we map SIC to 3-digit NAICS using the distribution of SIC codes within each NAICS industry from Compustat.

[11] See Appendix Table A12 for estimates found in this figure.

age-size cross section, even firms in industries with the highest liquidation values exhibit a residual positive effect, indicating that this channel does not absorb the full treatment response.

Given that the relaxation of borrowing constraints emerges as the dominant channel through which firms benefit from private credit, we test whether this translates into meaningful reallocation of resources post-treatment. A large literature documents that borrowing constraints prevent productive firms from reaching their optimal scale, generating dispersion in the marginal products of capital and depressing aggregate TFP (Buera, Kaboski and Shin (2011); Midrigan and Xu (2014); Moll (2014)). This misallocation extends to the liabilities side of the balance sheet (Whited and Zhao, 2021), and recent work shows that firms facing looser constraints charge higher markups and command larger market shares (Li et al., 2025).

While we do not directly observe capital stocks and therefore cannot compute TFP, we examine gross and excess job reallocation as a potential margin of adjustment. We follow the approach and data construction of Davis et al. (2014) and Davis et al. (2025) who in the private equity context document substantial job reallocation at firms following a private equity buyout.

We study both of these margins as net employment growth which we assessed in Table 1 masks two distinct margins of internal adjustment as noted by Davis et al. (2025). Gross job reallocation captures the total volume of simultaneous job creation and destruction within a firm, revealing internal reorganization that net growth alone would obscure. Excess job reallocation strips out the portion of gross churning mechanically required to achieve the observed net change, isolating reshuffling that occurs in excess of what net growth alone would necessitate. Together, these measures reveal the degree to which post-treatment adjustment reflects genuine redeployment of resources across a firm's establishment base, rather than simple expansion or contraction (see Appendix B.4.1 for formal definitions).

We find positive and statistically significant effects on both job reallocation and excess job reallocation following treatment, as reported in Table 4. Specifically, the job reallocation rate rises by nearly 20% relative to control firms 2 years following treatment, while the excess job reallocation rate rises by 14.5%, indicating that treated firms are not merely growing or shrinking uniformly but are genuinely reshuffling employment across their establishment base. These results are robust to controlling for private equity sponsorship (Table A33), suggesting the reallocation effects are not just driven by the involvement of sophisticated financial sponsors but reflect a broader response to the relaxation of borrowing constraints and the role of private credit lenders themselves.

These results are economically meaningful, even when benchmarked against the private equity literature. Davis et al. (2025) find that buyouts raise the overall job reallocation rate by 14.1% and excess reallocation by 9.2% relative to controls over a two-year horizon. Our estimates are larger in magnitude, which is notable given that private credit firms, unlike PE-backed targets, do not undergo the dramatic governance and ownership changes typically associated with buyouts. That the reallocation response among our treated firms approaches the scale documented in the PE literature suggests that the easing of financial

Table 4: Private Credit and Job Reallocation

	Job Reallocation (1)	**Excess Reallocation** (2)
Treatment (β)	0.199***	0.1453***
	(0.00918)	(0.007375)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Firm g	Y	Y
Weighted Employment$_t$	Y	Y
Observations	134,900,000	134,900,000
R^2	0.266	0.406

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year.Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

constraints alone may be sufficient to unlock meaningful reorganization of labor across establishments.

5.2 Active Credit Allocation or External Firm Risk

We demonstrate that the observed increase in exit rates is driven primarily by active lender credit allocation, rather than by borrower risk or leverage.

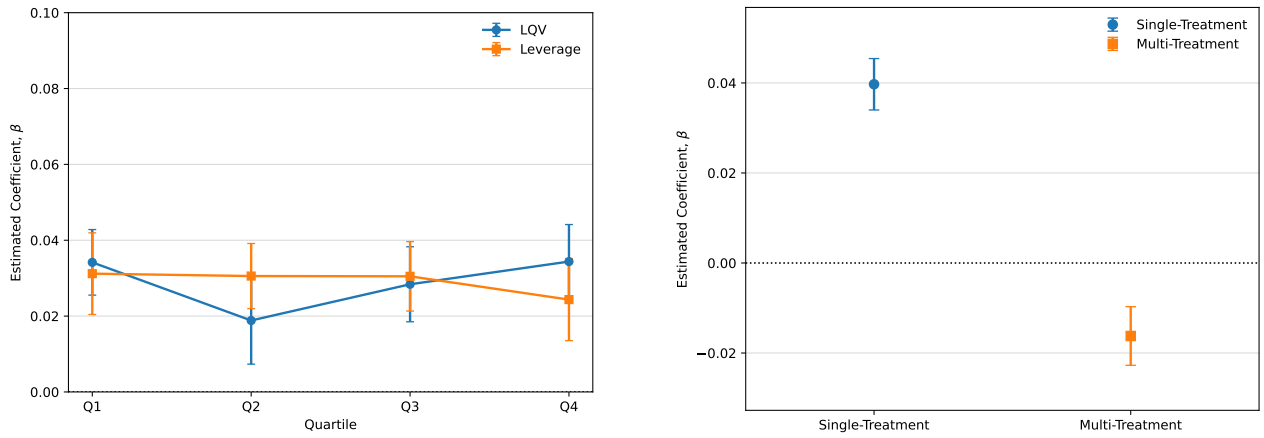
A natural initial concern relating to our finding that private credit is associated with increased exit likelihood is that this result simply reflects ex-ante credit risk or balance sheet risks of firms that receive private credit. Jang et al. (2026) documents that firms borrowing from finance companies and bank lenders have higher credit scores on average relative to firms borrowing from private credit lenders. In addition, private credit arrangements have been shown to accommodate higher leverage multiples than conventional bank financing (Haque, Stefanescu and Mayer, 2026), and elevated leverage is itself a well-established predictor of firm exit(Dore and Zarutskie, 2023).

To assess this, we construct two industry-year proxies from Compustat, aggregated at the three-digit NAICS level for risks relating to credit and leverage. Because we do not directly observe credit scores or leverage levels for treated or control firms, we map each firm to its industry-year average leverage ratio as a proxy for leverage concerns, and adopt the same liquidation value construction as in the previous section as a proxy for underlying firm risk. The identifying assumption follows for each channel's logic. If increased leverage is driving exit, firms in low-leverage industries would be taking on debt well above their industry norm, and we would expect exit likelihood to be elevated here in the cross section of our treated firms. If ex-ante firm risk is the primary driver, exit likelihood should be concentrated among firms in low-liquidation-value industries.

The left hand side of figure 7 shows no meaningful gradient across quartiles for either measure.[12] The treatment coefficient is statistically indistinguishable across the full distribution of both leverage and liquidation values, providing suggestive evidence that neither channel is the primary driver of the exit result.

We next turn to the role of lender behavior. Recent evidence points to private credit lenders as more hands-on counterparties than traditional banks. They exercise closer monitoring, intervene earlier following adverse firm shocks, and retain greater flexibility to restructure or extend credit outside of formal workout processes (Jang, 2026). This active posture raises the possibility that the exit result reflects not passive exposure to risky borrowers, but deliberate credit decisions made over the course of the lending relationship.

Figure 7: Exit Likelihood by Leverage, Liquidation Value Quartile, and Treatment Frequency



Leverage and liquidation value quartiles | Single vs. multiple treatments

Left: Estimated treatment coefficients on 2-year exit likelihood across quartiles of industry-year leverage and liquidation values (LQV). *Right*: Estimated treatment coefficients for firms receiving a single versus multiple loans within outcome window. Whiskers denote 95% confidence intervals.

To test this, we split the treated group according to whether firms received follow-on financing within the two-year outcome window following their initial treatment. We then estimate equation 1 separately on each of these samples.

The results in the right hand side of Figure 7 point to active credit allocation as the primary driver of the increased exit likelihood result.[13] It shows that firms receiving a single treatment exhibit exit likelihood above the baseline in Table 3, while those receiving multiple treatments display a negative treatment coefficient. This divergence is difficult to reconcile with borrower risk or leverage alone, as both groups accessed private credit and face the same ex-ante selection forces. What differs is the lender's decision to recommit capital, with sustained engagement identifying firms whose operations remain viable and exit concentrated among those cut off after an initial transaction.

[12] See Appendix Tables A13–A14 for estimates found in this figure

[13] See Appendix Table **??** for estimates found in this figure.

Our findings are consistent with Jang (2026) in that private credit lenders are more hands-on than traditional banks, yet we arrive at a different conclusion on exit. While that paper documents greater lender flexibility suppressing exit likelihood through renegotiation and covenant relief, our results benchmarked against the broader firm population point in the opposite direction: when distress materializes, resolution through firm exit is more common than renegotiation. Notably, this pattern persists even among sponsored firms, despite evidence in Jang (2026) of private equity sponsors injecting additional capital during distress episodes, underscoring that the exit dynamic we document is not confined to the unsponsored segment of the market.

5.3 Role of Industry Concentration and Expertise

We show that private credit's industry concentration and expertise is the channel through which it is able to effectively allocate credit and relax borrowing constraints

We next ask what enables private credit to extend financing to borrowing-constrained firms that our results imply traditional lenders are unwilling to serve, as well as, efficiently allocate credit which at times results in increased exit rates. A natural candidate is the industry concentration of private credit portfolios documented in our stylized facts (see Figure 4). Sustained sectoral concentration plausibly reflects, and reinforces, the accumulation of industry expertise. This connection is supported by a literature in banking and private equity establishing that lender industry expertise improves real outcomes for borrowers beyond the initial financing decision (Acharya et al. (2013), Bernstein and Sheen (2016), Spaenjers and Steiner (2024),Blickle, Parlatore and Saunders (2026)). If industry expertise is the channel that enables private credit to reach and impact otherwise constrained borrowers, its impact should be greatest among firms borrowing from expert private credit lenders.

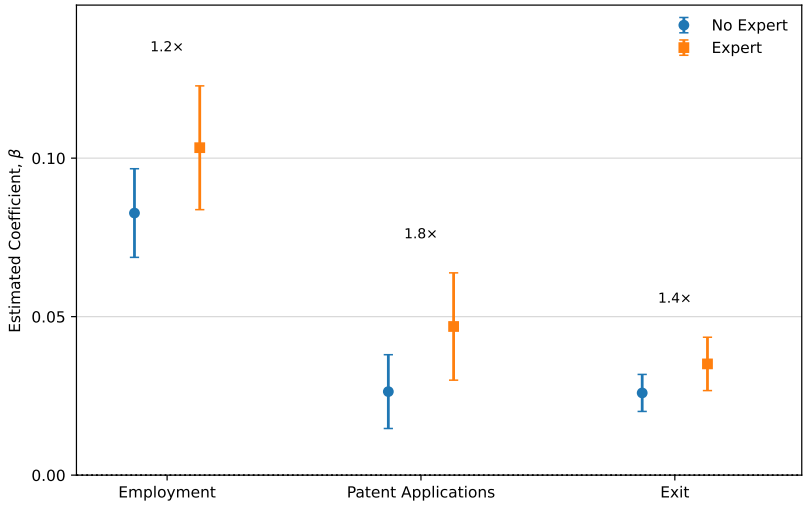
To test this, we adapt the industry specialization measure of Haque, Mayer and Wang (2025), leveraging our external databases, Pitchbook and Preqin, where we observe lender identity for the majority of our borrowers. Lender industry expertise is measured by each lender's share of private credit deals in a borrower's industry over the prior three years. A lender is classified as an expert if that share exceeds the median across active lenders in the same industry-year. A borrower-level dummy is then constructed, equal to one if any of its lenders meets this threshold in its 4-digit NAICS industry.[14] We define lenders at the highest level of aggregation, the parent asset manager (e.g., Ares), rather than at the individual fund or BDC level since industry expertise accumulates at the manager level and is shared across the funds and vehicles it operates.

We then re-estimate equation 1 separately on borrowers with and without an expert lender as seen in Figure 8 across the baseline firm dynamic objects of employment growth, patent propensity, and exit likelihood.

The results presented in Figure 8[15] are consistent with industry expertise serving as a meaningful chan-

[14] See Appendix B.4.2 for details on variable construction.
[15] See Appendix Table A36 for estimates found in this figure.

Figure 8: Role of Industry Expertise



Estimated treatment coefficients on 2-year outcomes. Multiple reflects ratio of expert to no-expert coefficients.

nel through which private credit generates real effects. Firms borrowing from at least one expert lender experience employment growth roughly 1.2 times that of firms without an expert lender. We also observe higher exit rates among firms with an expert lender, at roughly 1.4 times the rate of non-expert-lender borrowers. This pattern is consistent with the active monitoring channel discussed earlier: lenders with deeper industry knowledge are better equipped to identify when continuation is no longer economically viable and to accelerate exit, recycling capital toward more productive uses. The contrast is most striking for innovation: patent applications among firms with an expert lender are roughly 1.8 times those of firms without. This pattern is intuitive as evaluating and supporting innovative activity arguably requires the deepest sectoral knowledge.

6 Robustness

We rule out PE sponsorship dynamics and selection on unobservables as alternative explanations for the baseline results. Effects persist after conditioning on multiple notions of sponsor interaction with borrowing firms. Furthermore, effects scale monotonically with financing intensity and differ systematically by treatment frequency.

6.1 Role of Private Equity Sponsorship

We find that private credit effects persist after conditioning on PE sponsorship among borrowing firms, both at and post the initial private credit treatment.

A concern with our baseline results suggesting the notable real impacts that private credit has on its borrowing firms stems from potential effects from private equity sponsors. This arises due to the close institutional overlap between private credit and PE, as nearly three quarters of borrowing firms have a PE

sponsor. PE sponsorship has well documented effects on the outcomes we study, including employment dynamics (Davis et al. (2025); Mittal (2026)), innovation (Lerner, Sorensen and Strömberg (2011)), and firm exit (Hotchkiss, Smith and Strömberg (2021))."

We first control for this potential concern by augmenting our baseline estimation with an interaction dummy, denoting whether the borrowing firm also is PE-sponsored at the initial private credit treatment. Sponsor status is only observed for private credit borrowers, not for the unconditional universe of PE-backed firms. It ensures our estimated baseline private credit effects are not mechanically capturing the independent impact of PE ownership. Formally, we estimate the following specification (equation 2):

$$Y_{i,t+s} = \beta_{t+s} PC_{it} + \nu_{t+s}(S_{it} \times PC_{it}) + \lambda\, LFIRM_{it} + \sum_{c} \theta_c D_{ict} + \varepsilon_{i,t+s}, \tag{2}$$

S_{it} is an indicator for whether the firm is private-equity sponsored at treatment, while ν_{t+s} isolates the incremental effect for PE-sponsored borrowers.

We continue to find significant real effects of private credit on employment, innovation, and firm exit with heterogeneous effects across firm age, size, and liquidation values, largely mirroring the patterns documented in the previous section. The estimated β coefficients do, however, attenuate in magnitude relative to the baseline specification.

We interpret this attenuation conservatively. Because we observe PE sponsorship only within the private credit sample and not for the unconditional universe of PE-backed firms, we cannot fully separate the incremental private credit effect from the incremental PE ownership effect for sponsored borrowers. Some portion of the estimated ν likely reflects private credit dynamics rather than pure sponsorship effects, meaning our revised β estimates should be viewed as a lower bound on the true private credit impact. We leave to future research the question of how lender–sponsor matching shapes real outcomes across firm types.

We further probe the concern about the role of PE-sponsorship by reconsidering the definition of our sponsorship indicator. The baseline dummy s_{it} in equation 2 equals one if a firm is PE-sponsored in the year it receives private credit. A concern is that firms may obtain PE sponsorship shortly after receiving private credit, in which case estimated effects could partly reflect subsequent sponsor involvement rather than the financing itself. To address this, we redefine the variable as a firm-level indicator s_i that equals one if the firm is PE-sponsored either at treatment or at any point during the following two years. To construct this longitudinal record of sponsorship involvement, we worked with PitchBook to obtain the full time series of sponsor relationships for each of our borrowing firms. As a result, this exercise is conducted only on the PitchBook subsample of borrowing firms.

Re-estimating with this definition yields results that closely mirror the baseline, with all key outcomes remaining statistically significant though magnitudes decline slightly. The one exception is patent applica-

tion propensity, which is no longer significant. Although, this likely reflects the reduced statistical power from the smaller subsample rather than a substantive change in results. Results are in appendix Table A34.

6.2 Treatment Heterogeneity

We show that the baseline impacts are likely inconsistent with selection and consistent with a causal private credit channel as effects scale monotonically with loan size and differ systematically by treatment frequency.

We examine whether results differ for firms receiving a single private credit loan versus multiple loans during the outcome window. If results reflected lender selection on unobservables, the number of loans received should be irrelevant. We find that single-treated firms yield smaller effects on employment growth and patent outcomes relative to the pooled baseline, while multi-treated firms exhibit even larger effects. Exit likelihood increases for single-treated firms, while multi-treated firms exhibit a negative effect on exit likelihood as discussed earlier. Results are in appendix Tables A35 and A36.

Last, we examine whether effects scale with financing intensity. If results reflected selection rather than the private credit channel, we would not expect effects to scale with loan size. We construct a loan intensity measure defined as total private credit loan size in the treatment year divided by the firm wage bill, proxying financing scale relative to firm size using labor expenditures observed in Census data. We sort treated firms into quartiles by loan intensity and focus on employment growth and exit, since patenting may not scale proportionally with short-run financing intensity. The results display a clear monotonic pattern: firms in the top two quartiles experience employment growth effects more than twice as large as those in the bottom two quartiles, and exit probabilities increase steadily across the upper quartiles while remaining insignificant in the lower two. Results are in appendix Tables A37 and A38.

7 Model

Our stylized facts and empirical results about borrowing firms point to segmentation between private credit and other financing sources. With private credit's impact concentrated in intangible intensive sectors and young, smaller firms, we use this reduced form evidence to characterize private credit borrowers as high risk, high reward. However, the data alone cannot tell us what sustains this segmentation in equilibrium, as there are other institutional features that distinguish private credit, most notably its equity structured funding and lack of regulation.

This motivates a structural model of firm choice across heterogeneous lenders. We find that the lenders' optimality conditions deliver different capital structures, with banks choosing higher leverage and private credit more equity. Because banks operate with higher leverage, their equity holders bear greater exposure to default risk. This along with regulatory motivations leads banks to optimally screen toward safer borrowers, generating endogenous segmentation where riskier firms sort into private credit and safer firms into

banks.

7.1 Model Environment

The model consists of heterogeneous firms, differentiated by productivity volatility, and two sources of financing: banks and private credit lenders. The two lender types differ in that banks are subject to regulation while private credit faces a higher cost of debt. The model is designed to capture the key institutional differences between bank lending and private credit markets to study how these differences shape the allocation of credit across firms of varying riskiness.

We consider a two-period economy, $t \in \{0, 1\}$. Throughout, superscripts denote asset or agent type and subscripts denote the time period. For example, X_t^a denotes variable X belonging to agent or asset type a at time t. All asset returns are expressed in gross terms rather than net, so that a return R denotes one plus the net return. The initial period carries subscript X_0 and the second period subscript X_t, with all choice variables subscripted by when they are decided. Despite being set in the initial period, rates carry a X_t subscript, as they are paid in the second period, while shocks carry a X_t subscript as they are realized in the second period.

Firms. The production side of the economy consists of a continuum of measure-one firms, each indexed by i, which are risk neutral and protected by limited liability. Aggregate firm variables are denoted with superscript f. Risk neutrality allows us to frame the firm's problem as one of pure profit maximization. Limited liability is central to the model's mechanism: it means firms internalize only the upside of their investment, creating a wedge between firm and lender incentives that varies systematically with firm riskiness.

At $t = 0$, each firm is endowed with an investment opportunity of scale I_0^i. Firms have no initial internal wealth, so the entire investment must be externally financed. Each firm therefore takes out a loan $L_0^i = I_0^i$ from one of the two lenders. Investment produces output at $t = 1$ according to $Y_t^i = A_t^i \cdot I_0^i$, where A_t^i, the firm's idiosyncratic productivity, is realized at the beginning of the second period.

Firms are heterogeneous in the riskiness of their realized productivity shocks. Each firm is characterized by a volatility parameter σ_i, which indexes the dispersion of its idiosyncratic productivity shock A_t^i. The distribution of σ_i follows the CDF $H(\sigma)$ with associated density $h(\sigma)$, so that $H(\sigma)$ orders firms from safest to riskiest. Specifically, a higher value of σ_i corresponds to a mean-preserving spread of the distribution of A_t^i. This maps naturally to our empirical finding that private credit borrowers exhibit both higher growth rates and greater likelihood of exit, consistent with higher dispersion in realized outcomes rather than lower average productivity.

The parameter σ_i is observed by all agents in the model. This public information assumption eliminates adverse selection as a driver of lender choice, preventing any equilibrium sorting of firms across lenders due to informational frictions.

Dual Lenders. Two types of risk-neutral, competitive lenders with limited liability operate in the economy: a bank ($J = B$) and a private credit lender ($J = P$). Our empirical results and data do not directly identify the agent with which private credit is segmenting. However, we assume without loss of generality that it is a bank, which serves as a reduced form stand-in for an alternate provider of debt financing more broadly.

Similar to firms, lenders are assumed to be risk neutral, so that their objective places no additional weight on variance beyond expected payoffs. Unlike firms, however, lenders have access to equity financing, so their problem is not one of pure profit maximization but rather of maximizing the expected net present value of equity. Limited liability applies symmetrically to lenders: when the realized return on the loan is insufficient to cover debt obligations, equity holders walk away rather than absorbing further losses.

Each lender $J \in \{B, P\}$ invests in a single loan L_0^J, financed with a combination of equit,y E_0^J, and debt, D_0^J. Lenders are endowed with no bounds on the supply of either funding source, so they can freely choose any combination of equity and debt to finance the loan. Restricting each lender to a single loan shuts down diversification across borrowers, allowing us to directly link the lender's balance sheet decision to its lending decision for a given firm. Their balance sheet then can be characterized as:

$$L_0^J = E_0^J + D_0^J. \tag{3}$$

Lender debt holders receive a contractual return and hold seniority over equity in the event of lender default. Equity holders are residual claimants with no contractual returns, absorbing losses when loan returns fall short of what is owed to debt holders. We denote balance-sheet choice of leverage for each lender as $\phi^J \equiv D^J / L^J \in [0, 1]$, so that $\phi^J = 0$ corresponds to all-equity financing and $\phi^J = 1$ to all-debt financing. The cost of debt liabilities $R_t^{d,J}$ is taken as given, and the opportunity cost of equity is β, common across lenders.

The two lenders differ in two key ways following Begenau and Landvoigt (2022). First, banks are subject to capital regulation that places an upper bound on leverage and a risk weight applied to the loan, mirroring the risk-weighted capital requirements under formal regulatory frameworks such as Basel III. Specifically, the bank faces the risk-weighted capital constraint

$$\omega(\sigma^i) \frac{D_0^B}{L_0^B} \leq \frac{\bar{D}^B}{L_0^B}, \tag{4}$$

where $\omega(\sigma^i)$ is a risk weight that is increasing in firm riskiness σ^i, and $\bar{D}^B / L_0^B$ is the maximum permissible risk-weighted leverage ratio. As firm riskiness rises, the effective leverage constraint tightens, raising the cost of extending the loan for the bank. Private credit lenders operate outside the regulatory perimeter and face no analogous constraint.

Second, since banks provide liquidity and benefit from deposit insurance, their debt cost is fixed and

does not vary with leverage or borrowing firm riskiness. Private credit lenders, by contrast, raise debt from institutional investors without such access, so their debt cost is endogenously priced as $R_t^{d,P} = R^{d,P}(\phi_0^P, \sigma^i)$, increasing in both leverage and firm riskiness. We assume private credit debt is unconditionally more expensive than bank debt, $R_t^{d,B} < R_0^{d,P}$, and that private credit debt costs can rise above the cost of equity, while bank debt cost remains strictly below β.

Lenders look to maximize the net present value of equity for their shareholders. In order to do this, at $t = 0$, each lender J extends a loan contract θ^J that matures at $t = 1$. It specifies a gross loan return $R_t^{l,J}$ to be paid by the firm at maturity. The contract requires $R_t^{l,J} \geq 0$. As a result, if a firm cannot meet this obligation, it defaults under limited liability. The lender then seizes the firm's output in the case that realized output falls short of the required repayment.

The loan rate $R_t^{l,J}$ itself is determined by competition between lenders. They employ breakeven pricing whereby each sets loan rates to exactly cover their own funding costs and required returns, extracting no additional rents. In equilibrium, this rate will reflect the leverage of the lender and the riskiness of the borrowing firm.

Each lender itself may also default. This occurs when the realized return on the loan is insufficient to cover its own debt obligations. Following Begenau and Landvoigt (2022), the lender default threshold is denoted by

$$\hat{\sigma}_t^J \equiv \left\{\sigma^i : R_t^{l,J} L_0^J - R_t^{d,J} D_0^J < 0\right\}. \tag{5}$$

When realized firm productivity falls in this region, it is sufficiently low so that the loan return cannot cover the lender's debt liabilities. In such a case, the lender defaults, with equity holders receiving nothing. The probability of lender default can be denoted as $H_{\sigma,t}^J \equiv H\left(\hat{\sigma}_t^J\right)$, the mass of firm types for which the lender would be rendered insolvent should they realize a negative productivity shock. Allowing for lender default links leverage and funding costs directly to equilibrium loan pricing, since a more leveraged lender faces greater default risk and must adjust loan terms accordingly.

Problems of Firm and Lenders. Since firms are identical up to their volatility parameter σ^i and choices scale linearly with investment, the firm's problem aggregates across the continuum. The aggregate firm chooses a lender, subject to limited liability to maximize profits follows:

$$\max_J \ \mathbb{E}\left[\Pi_t^f\right] = \mathbb{E}\left[\max\left(A_t^f I_0^f - R_t^{l,J} L_0^f,\, 0\right)\right]. \tag{6}$$

The $\max(\cdot, 0)$ operator reflects limited liability. Since firms care only about expected profits and the loan rate is the only contract term that varies across lenders, the firm's choice reduces to selecting the lender offering the lower loan rate $R_t^{l,J}$.

Each lender J chooses its balance-sheet composition to maximize the net present value of equity. The

lender's problem is

$$\max_{D_0^J,\, E_0^J} \; V_0^J = -E_0^J + \beta \mathbb{E}\Big[\max\Big(\min\Big(R_t^{l,J},\, A_t^i\Big) L_0^J - R_t^{d,J} D_0^J,\, 0\Big)\Big], \tag{7}$$

subject, for the bank ($J = B$), to the capital regulation constraint in equation 4. The term $-E_0^J$ represents the upfront equity cost of funding the loan. The continuation value discounts future equity payoffs at rate β. The $\min(R_t^{l,J}, A_t^i)$ term captures the fact that the lender receives at most the contractual loan return but may receive less if the firm defaults and output is insufficient. The outer $\max(\cdot, 0)$ reflects lender limited liability. The private credit lender solves the analogous problem without the capital regulation constraint, but faces a higher cost of debt $R_t^{d,P} > R_t^{d,B}$. See figure A3 that outlines the model timing.

Equilibrium. Given the set of exogenous parameters $\{\beta, R_t^{d,B}, R_t^{d,P}, \bar{D}^B, \omega(\cdot)\}$ and firm risk distribution $H(\sigma)$, a competitive equilibrium consists of lender balance sheets $\{E_0^J, D_0^J\}_{J \in \{B,P\}}$, loan contracts $R_t^{l,J}$ for each σ^i, and a lender assignment $\sigma^i \mapsto J$, such that:

- Lenders maximize NPV of equity (7), subject to their balance-sheet (3) and, for banks, capital regulation (4);
- Firms choose the lender with the lower $R_t^{l,J}$ to maximize expected profits (6).

7.2 Analytical Solution

We show that banks optimally operate with higher leverage than private credit, which leaves them more exposed to default on risky borrowers. As a result, banks pass the cost of their leveraged structure into higher loan rates for risky firms, generating segmentation under which safer firms sort to banks and riskier firms to private credit beyond a threshold level of risk.

Tractability Assumptions. We impose a set of assumptions that together deliver a closed-form lender problem and solution to characterize each lender's optimal balance sheet composition.

Assumption 1 (Loan rate covers debt costs). The loan rate $R_t^{l,J}$ is set at the minimum level required to cover the lender's debt obligations,

$$R_t^{l,J} L_0^J \;\geq\; R_t^{d,J} D_0^J. \tag{8}$$

This assumption simplifies the lender's payoff by aligning the loan return with contractual debt servicing, so that the lender's ex post equity payoff depends only on whether firm output is sufficient to cover debt costs. We therefore do not need to separately track the point in the productivity distribution at which the loan rate covers debt costs, which collapses the leverage decision into a single margin, the region where leverage may bind.

Assumption 2 (Default-region focus). We focus on the parameter region in which firm default occurs, $A_t^i \leq R_t^{l,J}$. Restricting attention to this region lets us focus on the scenario in which realized productivity A_t^i

is low enough that the lender's leverage choice ϕ_0^J determines whether debt obligations $R_t^{d,J} D_0^J$ can be met, and thus whether the lender itself defaults.

Assumption 3 (Uniform productivity). Idiosyncratic productivity is uniformly distributed on a symmetric support around a common mean,

$$A_t^i \sim U\left[\mu - \sigma^i,\ \mu + \sigma^i\right]. \tag{9}$$

The uniform assumption preserves the key economic feature of firm heterogeneity while delivering tractability and a closed form solution: a higher σ^i is a mean-preserving spread, with $\mathbb{E}[A_t^i] = \mu$ held constant across firms and variance scaling in σ^i. This isolates the role of dispersion, rather than expected return, in driving lender choice.

Simplified Lender Problem. Under Assumptions 1–2, we recast the problem in terms of leverage $\phi_0^J = D_0^J / L_0^J$ so that the loan size L_0^J scales out. We then normalizing $L_0^J = 1$, so that the lender's problem in (7) reduces to

$$\max_{\phi_0^J}\ V_0^J \ = \ \beta\left[\left(1 - H_\sigma(\phi_0^J)\right)\left(A_t^i - R_t^{d,J}\phi_0^J\right)\right] \ - \ (1 - \phi_0^J), \tag{10}$$

subject, for the bank, to the capital regulation constraint $\phi_0^B \le \bar{\phi}^B$, where $H_\sigma(\phi_0^J)$ denotes the default threshold under firm risk σ^i at lender leverage ϕ_0^J. The first term is the discounted expected equity payoff in the no-default region: $(1 - H_\sigma(\phi_0^J))$ is the probability of survival, and $(A_t^i - R_t^{d,J}\phi_0^J)$ is the per-unit equity payoff conditional on survival. The second term, $-(1 - \phi_0^J)$, is the equity financing cost saved by issuing debt.[16] The lender's problem thus collapses to a single choice of leverage ϕ_0^J that trades off the equity financing saved by issuing more debt against the greater probability of default that leverage induces.

Optimal Leverage. Solving the first-order conditions associated with (10) yields closed-form expressions for optimal leverage:

$$\phi_0^{*,P} \ = \ \underbrace{\frac{\mu + \sigma^i}{R^{d,P}}}_{\substack{\text{(1) upside productivity} \\ \text{to funding cost}}} \ - \ \underbrace{\frac{2\sigma^i}{\beta (R^{d,P})^2} \cdot \frac{1}{1 + \varepsilon^P}}_{\substack{\text{(2) risk-adjusted} \\ \text{cost of leverage}}}, \tag{11}$$

$$\phi_0^{*,B} \ = \ \underbrace{\frac{1}{R^{d,B}}\left(\mu + \sigma^i - \frac{2\sigma^i}{\beta R^{d,B}}\right)}_{\substack{\text{(1) upside net of risk-adjusted} \\ \text{financing cost}}} \ - \ \underbrace{\frac{2\lambda \sigma^i \omega(\sigma^i)}{\beta R^{d,B}}}_{\substack{\text{(2) regulatory} \\ \text{drag}}}, \tag{12}$$

where $\varepsilon^P \equiv \frac{\partial R^{d,P} / \partial \phi_0^P}{R^{d,P}} \phi_0^{*,P}$ is the elasticity of the private credit lender's funding cost with respect to leverage.

The two expressions reflect a fundamental difference in the nature of each lender's optimum. The private credit lender reaches an interior solution, equating the marginal benefit of leverage to its marginal cost. The bank always binds its capital constraint at the corner $\phi_0^{*,B} = \bar{\phi}^B / \omega(\sigma^i)$. Cheap deposit funding that is not endogenously repriced, means the marginal return to leverage exceeds its cost everywhere in

[16] The derivation connecting these assumptions to the revised problem in equation 10 is in Appendix C.2.

the interior, so the constraint alone limits leverage. Equation (12) is therefore not an interior condition for banks but the constraint value expressed in terms of primitives.

For the private credit lender, term (1) measures the borrower's best output relative to the lender's funding cost, increasing in μ and σ^i and decreasing in $R^{d,P}$. Term (2) is the risk-adjusted cost of leverage, rising with firm risk as more mass falls below the repayment threshold. The elasticity correction $1/(1+\varepsilon^P)$ is the key feature: when higher leverage raises the lender's own funding cost, the effective marginal cost of debt is amplified, endogenously dampening leverage. As $\varepsilon^P \to 0$ the expression collapses to the fixed-funding-cost benchmark.

For the bank, term (1) consolidates upside and financing cost into a single expression, while term (2) is the shadow cost of the binding constraint. It rises with both σ^i and $\omega(\sigma^i)$, so riskier borrowers tighten the constraint and impose a steeper regulatory penalty, a key force that causes the bank's effective lending cost to increase more sharply with firm risk than private credit's.

Lender Leverage Ranking. The decomposition in (11) and (12) permit a direct comparison of leverage across lenders, which we summarize in the following proposition.[17]

Proposition 1 (Leverage Ranking). *Let* $\Delta^* \equiv \dfrac{1}{R^{d,B}} - \dfrac{1}{(\tilde{R}^P)'(\phi_0^{*,P})}$ *and* $\Gamma^* \equiv \dfrac{1}{\beta(R^{d,B})^2} - \dfrac{1}{\beta\big((\tilde{R}^P)'(\phi_0^{*,P})\big)^2}$. *For all* $\sigma^i < \mu$, $\phi_0^{*,B} > \phi_0^{*,P}$ *if and only if*

$$\underbrace{\left(\mu+\sigma^i\right)\Delta^*}_{\text{funding advantage}} \quad - \quad \underbrace{2\sigma^i\Gamma^*}_{\substack{\text{default risk cost}\\ \text{to equity holders}}} \quad > \quad \underbrace{\frac{2\sigma^i\,\lambda\,\omega(\sigma^i)}{\beta\,R^{d,B}}}_{\text{regulatory drag}}, \tag{13}$$

where $\Delta^*, \Gamma^* > 0$ *since* $(\tilde{R}^P)'(\phi_0^{*,P}) \equiv (1+\varepsilon^P)R^{d,P}(\phi_0^{*,P}, \sigma^i) > R^{d,P} > R^{d,B}$.

The funding advantage $(\mu+\sigma^i)\Delta^*$ and default risk cost $2\sigma^i\Gamma^*$ both reflect the bank's cheaper flat deposit cost but operate in opposite directions. Cheaper debt raises the upside-to-cost ratio directly, but also induces higher leverage, placing more mass in the default region and raising the risk-adjusted cost borne by equity holders. The regulatory drag is a further offset rising with σ^i through both direct scaling and tightening $\omega(\sigma^i)$. The condition holds for all $\sigma^i < \mu$ because the funding advantage scales with mean productivity μ and dominates the two offsetting terms which scale only with $\sigma^i < \mu$.

Loan Pricing and Segmentation. We now turn to the firm's choice of lender. Each lender sets the loan rate they charge borrowing firms at its own breakeven, earning zero net equity in equilibrium:

$$V_0^J\left(\phi_0^{*,J}, R_t^{l,J}\right) = 0. \tag{14}$$

[17] The proof is in Appendix C.5.

Substituting the closed-form expression for V_0^J from (C.14) and solving for the loan rate yields

$$R_t^{l,J} = \mu + \sigma^i - 2\sqrt{\frac{\sigma^i\left(1-\phi_0^{*,J}\right)}{\beta}}, \tag{15}$$

the rate at which lender J exactly recovers its opportunity cost of funds. The rate is increasing in leverage $\phi_0^{*,J}$: a thinner equity buffer exposes the lender to greater default risk, requiring a higher loan rate to break even. It is also increasing in firm risk σ^i, reflecting greater default probability, and this effect steepens with leverage as the equity buffer offers less protection.

The firm therefore selects the lender with the lower breakeven rate, equivalently the lower total intermediation cost $C^J(\sigma^i;\cdot)$. We then equivalently define the joint surplus associated with lender J and the firm as the sum of expected firm profits and lender net equity:

$$JS^J(\sigma^i;\cdot) = \underbrace{\mathbb{E}[A_t^i] - R_t^{l,J}}_{\text{firm surplus}} + \underbrace{V_0^J}_{\text{lender net equity}} = \mu - C^J(\sigma^i;\cdot). \tag{16}$$

The loan rate cancels as a transfer between firm and lender, so JS^J depends only on model parameters and the firm's choice of lender reduces to a comparison of intermediation costs. The equilibrium assignment is therefore

$$J^*(\sigma^i) = \arg\max_{J\in\{B,P\}} JS^J(\sigma^i;\cdot), \tag{17}$$

even though no agent maximizes joint surplus directly. This representation collapses the firm's lender choice into a single scalar comparison in σ^i.

The joint-surplus representation gives rise to a unique segmentation threshold.[18]

Proposition 2 (Segmentation Threshold). *Let* $\Delta^* \equiv \dfrac{1}{R^{d,B}} - \dfrac{1}{(\tilde{R}^P)'(\phi_0^{*,P})}$ *and* $\Gamma^* \equiv \dfrac{1}{\beta(R^{d,B})^2} - \dfrac{1}{\beta\left((\tilde{R}^P)'(\phi_0^{*,P})\right)^2}$. *There exists a unique threshold* $\hat{\sigma}^i$ *such that* $JS^B(\hat{\sigma}^i;\cdot) = JS^P(\hat{\sigma}^i;\cdot)$, *given by*

$$\hat{\sigma}^i = \frac{\mu\Delta^*}{2\Gamma^* - \Delta^* + \dfrac{2\lambda\,\omega(\hat{\sigma}^i)}{\beta R^{d,B}}}. \tag{18}$$

Firms with $\sigma^i < \hat{\sigma}^i$ *borrow from the bank; firms with* $\sigma^i > \hat{\sigma}^i$ *borrow from private credit.*

Proposition 2 partitions the firm distribution into a safe segment served by banks and a risky segment served by private credit. The numerator $\mu\Delta^*$ reflects the bank's funding-cost advantage; a wider spread raises the threshold and expands bank market share. The denominator captures two forces that erode bank share and increase private credit share: the leverage sensitivity terms $2\Gamma^* - \Delta^*$, through which both lenders deleverage away from riskier borrowers, and the regulatory drag $2\lambda\omega(\hat{\sigma}^i)/(\beta R^{d,B})$, which tightens the con-

[18] The proof is in Appendix C.7.

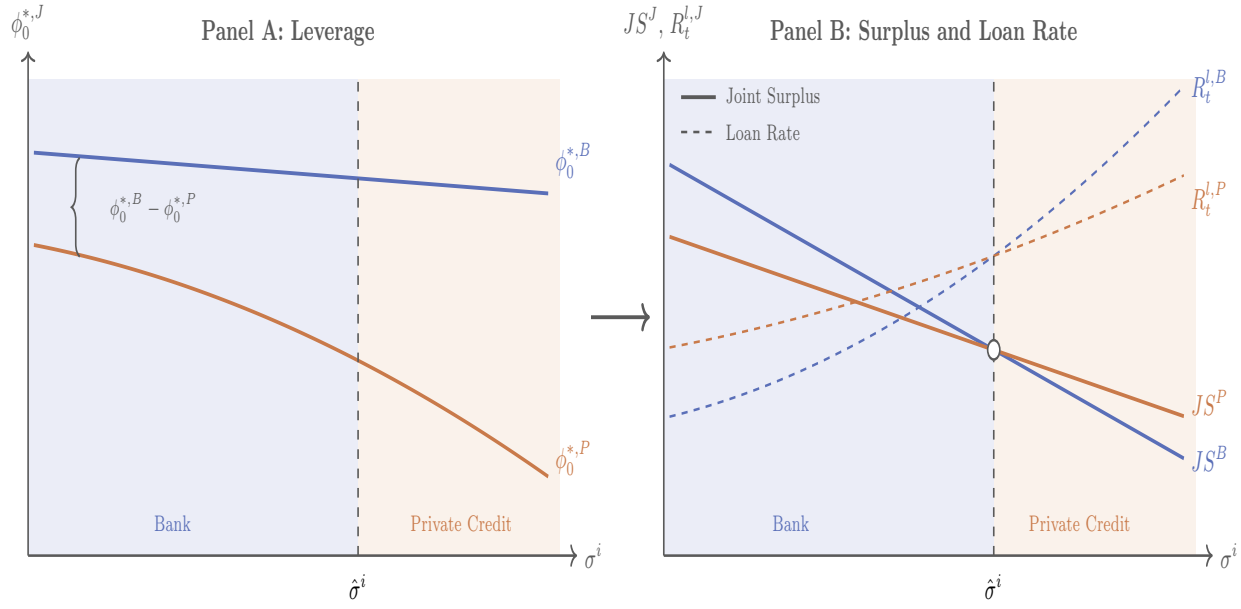
straint on the bank alone.

Crucially, segmentation arises even at $\lambda = 0$: cheap deposit funding induces the bank to lever more heavily than private credit against every borrower, and this leverage differential alone partitions the firm distribution. Regulation tightens the boundary further but is not its source. A planned calibration will quantify the relative contribution of each channel: the funding structure channel, operating through the bank's lower deposit rate and greater leverage, and the regulatory channel, operating through the risk-weighted capital constraint.

Figure 9 visualizes the equilibrium across two panels. Panel A plots optimal leverage as a function of firm risk σ^i. Bank leverage $\phi_0^{*,B}$ is only moderately decreasing, pinned by the binding capital constraint, while private credit leverage $\phi_0^{*,P}$ declines faster and monotonically as rising firm risk raises the marginal cost of leverage through ε^P. The gap $\phi_0^{*,B} - \phi_0^{*,P}$ is positive everywhere, consistent with Proposition 1, and widens as σ^i increases.

Panel B translates the leverage differential into equilibrium pricing. Each lender's breakeven loan rate is increasing in own leverage — higher leverage means less equity injected, so equity holders bear greater default risk and demand compensation through a higher rate. Since the bank carries higher leverage across all σ^i, its loan rate rises faster with firm risk, crossing private credit's at $\hat{\sigma}^i$. The joint surplus curves JS^B and JS^P reflect the same dynamic: both decline in σ^i as intermediation costs rise, but JS^B falls faster through the leverage and regulatory channels, crossing JS^P at the same $\hat{\sigma}^i$.

Figure 9: Model Segmentation



Panel A plots optimal leverage $\phi_0^{*,J}$ against firm risk σ^i for each lender. Panel B plots the corresponding joint surplus JS^J (solid) and loan rate $R_t^{l,J}$ (dashed).

This equilibrium aligns with the empirical patterns documented earlier in the paper. The cross-sectional finding that private credit borrowers exhibit higher growth rates and greater likelihood of exit relative to bank borrowers is consistent with private credit servicing firms drawn from the high-σ^i tail of the distribution: in the model, these are exactly the firms for which realized productivity has greater dispersion, generating both stronger upside and larger downside realizations. More broadly, the model rationalizes the coexistence of the two lender types as a sorting outcome driven by optimal leverage choices and regulation rather than informational frictions, consistent with the publicly-observed risk assumption in the environment and with the institutional view that private credit has grown as a complement to, rather than a substitute for, bank lending.

While the model parameterizes firm heterogeneity through the single dimension of productivity volatility σ^i, the empirical literature provides a direct mapping to observable firm characteristics. Younger and smaller firms exhibit greater idiosyncratic volatility in productivity and growth, consistent with less established business models and thinner operational buffers (Haltiwanger, Jarmin and Miranda, 2013). Intangible-intensive firms face greater dispersion in realized returns because returns to R&D and other intangible investments are inherently uncertain (Hall and Lerner, 2009). The borrower profile and real effects we document, concentrated in intangible-intensive industries with greatest impacts on firms that are younger and smaller than typical public firms, therefore maps cleanly to the high-σ^i tail of the model's distribution.

8 Conclusion

Private credit represents the emergence of a fundamentally new form of corporate financial intermediary, combining the originate-and-hold relationship model of banking with the equity-funded, expertise-driven structure of private equity. Yet its real consequences for borrowing firms have remained largely undocumented. Since the Great Financial Crisis it has become the fastest-growing segment of U.S. corporate lending, but because both lenders and borrowers operate outside the regulatory disclosure purview, the basic questions of who borrows from private credit and how this financing shapes firm outcomes have remained unanswered.

This paper addresses that gap by constructing the largest dynamic dataset of private credit borrowers to date. Merging loan-level data from Preqin and PitchBook with U.S. Census microdata, we link over 17,000 borrowing firms to consistent longitudinal measures of firm outcomes. We adapt a synthetic-control identification strategy to compare treated borrowers to observationally similar control firms drawn from the universe of U.S. firms, and develop a tractable model of heterogeneous firms and dual lenders to explain the segmentation between lenders beyond what the data alone can identify.

Private credit borrowers are shown to be mature, intangible-intensive firms concentrated in industries traditionally underfinanced by debt markets, suggesting that private credit expands credit availability rather than simply substituting for existing debt channels. Following treatment, borrowing firms face a fundamental tradeoff: employment and innovation rise while exit likelihood increases, presenting an up-

or-out dynamic that private credit for its borrowing firms.

Borrowing constraint relaxation emerges as the dominant channel in explaining private credit's real impact on firm dynamics. These patterns imply segmentation between lenders, with private credit reaching firms excluded from debt markets rather than substituting for existing credit sources. We argue that the "out" portion of the documented dynamics are driven by active lender credit allocation rather than primarily ex-ante firm risk or leverage. Industry expertise emerges as the mechanism enabling private credit to reach and impact these otherwise constrained borrowers, with effects systematically larger among firms matched to expert lenders and the contrast sharpest for innovation.

Our model rationalizes the observed segmentation as an equilibrium outcome of optimal lender leverage choices: banks, facing cheap deposit funding but tightening capital regulation with borrower risk, lend to safer firms, while private credit, facing higher and endogenously priced debt costs, absorbs the riskier tail of the firm distribution. Segmentation arises from the funding structure distinction itself, with regulation tightening the boundary but not its source.

Several dimensions of this analysis are ongoing. On the empirical side, we are extending the results to revenue growth and productivity reallocation, which will clarify whether the employment gains we document reflect expansions in productive capacity or compositional shifts. We are also examining the role of loan terms that are distinctive to private credit, including warrants and PIK provisions, to assess whether these contractual features generate real effects beyond the financing itself. We are additionally pursuing an instrumental variable strategy exploiting the 2018 regulatory reform to BDCs as a plausibly exogenous time-series shock to private credit supply, complementing the cross-sectional identification of our baseline design. On the modeling side, we are extending the framework to incorporate the mechanisms identified empirically. A planned calibration will quantify the relative contribution of funding structure and regulatory channels to the observed segmentation.

Looking forward, our findings raise a set of broader questions. How does the fund structure of private credit shape the supply of credit over the cycle? What are the implications for workers at borrowing firms, beyond the firm-level employment effects we document? While we focus on direct firm lending, private credit has expanded rapidly into asset-backed markets; how do its real effects in those markets compare to both traditional lending sources and the firm lending studied here? Finally, if private credit's growth partly reflects regulatory arbitrage, what are the implications of extending prudential oversight to these intermediaries?

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A Data

A.1 Cleaning Private Credit Data

While Pitchbook and Preqin are both databases that contain private credit loans, they do not employ a rigorous and consistent definition of private credit in their debt databases. As a result, we clean each of these databases in the following manner to ensure we are observing a private credit loan to a U.S. firm.

For Pitchbook, we clean the data as follows:

- **Lender type restrictions**
  - Using its LENDER TYPE tag, we remove any deals classified as banks, most notably commercial or investment banks
  - We also exclude non-lender financial service providers such as fund custodians or consultants
- **Lender name filtering**
  - We remove any deal that includes a lender from a comprehensive list of bank names
  - Given the granularity of lender names, this does not exclude deals where banks operate through non-bank arms (e.g., BDCs of institutions such as Goldman Sachs)
- **Deal characteristics**
  - Using the DEAL CHARACTERISTICS tag, we remove syndicated loans
- **Geographic and data availability restrictions**
  - We retain only deals where the borrowing firm is located in the U.S.
  - We require each observation to have at least basic location information, consisting of city and state of headquarters
  - We restrict the sample to deals occurring in 2022 or earlier
  - These restrictions reflect the merge process into the U.S. Census described in the following section, which requires both firm name and location information, as well as the fact that the latest available Census data at the time of merging was 2022

For Preqin, we clean the data as follows:

- **Lender name filtering**
  - We remove any deal that includes a lender from a comprehensive list of bank names

  - As with Pitchbook, this does not exclude deals where banks operate through non-bank arms (e.g., BDCs)

- **Geographic and data availability restrictions**
  - We retain only deals where the borrowing firm is located in the U.S.
  - We require each observation to have at least basic location information, consisting of city and state of headquarters
  - We restrict the sample to deals occurring in 2022 or earlier
  - These restrictions reflect the merge process into the U.S. Census described in the following section, which requires both firm name and location information, as well as the fact that the latest available Census data at the time of merging was 2022

We then append these two datasets and remove duplicate deals between them. Due to inconsistent deal dates and firm names across data sources, we define a loan as a unique cleaned firm name (with the cleaning process outlined in the following section) within a given year-quarter. To compute summary statistics and loan terms, we aggregate within each source by firm and year-quarter, summing loan sizes and taking the median for maturity and spread.

If duplicate observations exist across sources, we first defer to the source that contains populated loan-level detail, as many observations are missing key terms. In cases where both sources report a given loan term for the same firm-quarter, we combine information across sources by averaging the given loan term, reflecting the fact that deal-level reporting can differ across datasets. Similarly, for sponsor status, if a firm is flagged as sponsored in any source within a given firm-year-quarter observation, we classify the firm as sponsored.

See section B.2 for tables summarizing final dataset.

A.2 Merging Private Credit Data with U.S. Census

We merge these external private credit databases to U.S. Census microdata using a multi-stage record linkage procedure that combines exact matching with fuzzy matching. We implement the merge separately for the PitchBook and Preqin components of the private credit data because the two sources differ in the firm names and location identifiers they report. Figure A1 summarizes the main cleaning, matching, and filtering steps.

We begin by standardizing firm names using three nested cleaning transformations. The first converts names to lower case. The second additionally removes punctuation, accents, and extraneous whitespace. The third further strips common corporate suffixes (e.g., “Inc.”, “LLC”, “Ltd.”, and “Corp.”) to isolate the core identifying component of the firm name. We standardize location information separately. When full street

addresses are available, we geocode them using the U.S. Census API Geocoder. When only partial location information is available, we assign the centroid of the reported city. This yields a consistent geographic reference for both external firms and Census establishments.

The first stage of the merge relies on exact matching. For PitchBook, which provides both a standard firm name and a legal firm name, we construct three cleaned versions of each name and concatenate them with the reported city and ZIP code. On the Census side, we construct the same three cleaned name variants and combine two establishment-name fields, Name 1 and Name 2, with location fields drawn from the physical address, mailing address, and address in County Business Patterns (CBP) records. Name 1 is generally the more populated of the two Census establishment-name variables, but we use both fields in order to maximize coverage. Combining Name 1 and Name 2 with each of these location fields yields six Census-side identifiers. We then run exact matches across all combinations of the cleaned PitchBook identifiers and these Census identifiers.

For Preqin, which provides one firm name but not consistently a ZIP code, we construct the same three cleaned name variants and concatenate them with city and state FIPS codes. We then match these to Census Name 1 and Name 2 concatenated with physical and mailing city–state FIPS identifiers. Relative to PitchBook, the exact Preqin merge therefore relies on a narrower set of location-based identifiers because of the more limited address information available in that source.

Matches obtained in the exact stage are then refined using temporal and cleaning-based priorities. Following Davis et al. (2025) for the timing dimension, we prioritize matches in the treatment year t and, if none is found, allow matches in adjacent years $t-1$ and $t+1$. Within this window, we prioritize matches obtained using the most conservative of the three name-cleaning transformations and only use less conservative variants if no higher-priority match is available. At this stage, however, we do not yet remove exact matches from the pool of candidate fuzzy matches, since final selection is based on a comparison of exact and fuzzy candidates after both sets of matches have been filtered.

For all private credit observations, including those with candidate exact matches, we also implement a fuzzy matching procedure following Wang and Young (2024). The basic strategy follows this paper in combining textual similarity in firm names with geographic proximity in location. We use the most comprehensive cleaning transformation as the baseline name definition for the fuzzy stage. For PitchBook, we apply this cleaning to both the standard and legal names and combine each with geocoded location information. We then match each of these against the similarly cleaned Census Name 1 field paired with either the physical-address geocode or the mailing-address geocode. This yields four fuzzy matching strategies for PitchBook: standard name matched to physical geocode, standard name matched to mailing geocode, legal name matched to physical geocode, and legal name matched to mailing geocode. For Preqin, we use the cleaned firm name together with city-centroid geocodes and match it against the cleaned Census Name 1 field paired with either the physical-address geocode or the mailing-address geocode, yielding two fuzzy

matching strategies.

Candidate fuzzy matches are scored using both name similarity and geographic proximity. For each borrowing firm i, we compute

$$\text{aggregate match score}_i = \omega * s_i + (1-\omega) * \left[1 - \left(\frac{d_i}{\bar{d}}\right)^{\eta}\right], \tag{A.1}$$

where s_i is the string similarity score and d_i is the Euclidean distance between geocoded locations. String similarity is computed using a TF–IDF representation of cleaned firm names based on character n-grams, with candidate matches selected through nearest-neighbor search under cosine distance. The parameter ω controls the relative weight placed on name similarity, while η governs how quickly the score declines with geographic distance. We set these parameters as follows: $\omega = 0.7$ and $\eta = 0.5$. In calibrating these parameters, we place greater weight on name similarity (with $\omega > 0.5$) to prioritize textual matches. We then apply a concave distance penalty (with $\eta < 1$) to penalize distant matches while rewarding close geographic proximity, ensuring that high-confidence matches are driven primarily by string alignment but refined by location. We retain only matches with aggregate scores above 0.95, which in our baseline implementation is chosen to isolate high-confidence fuzzy matches.

We then refine fuzzy matches using a temporal and score-based prioritization procedure analogous to the exact stage. We first retain matches in year t that exceed the fuzzy-score threshold. If no such match exists, we allow matches in years $t-1$ through $t+1$ that satisfy the same threshold. This yields a filtered set of exact candidates and a filtered set of fuzzy candidates for each private credit firm.

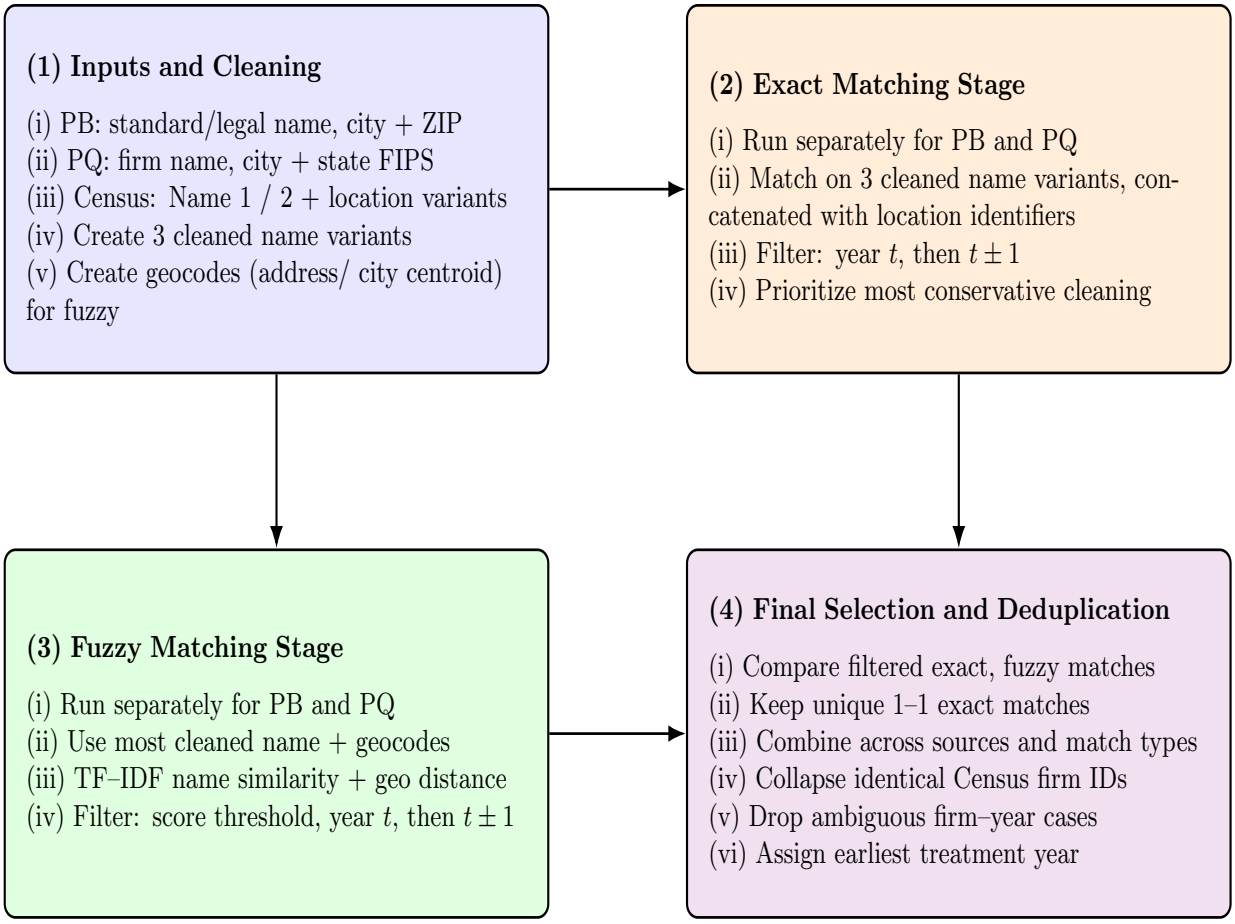
Final match assignment is based on comparing these filtered exact and fuzzy candidates. We first prioritize exact matches when they generate a unique one-to-one link between the private credit observation and a Census firm. We similarly retain fuzzy matches when they generate a unique one-to-one link. If an exact candidate remains duplicated but the fuzzy stage produces a unique one-to-one match, we retain the fuzzy match and discard the duplicated exact candidate. More generally, final selection is based on whether a candidate match can be uniquely resolved, rather than on exact matching alone.

In the final stage, we combine all remaining candidate matches across PitchBook and Preqin, as well as across exact and fuzzy matches within each source, and again search for uniquely resolved one-to-one links. Remaining duplicates arise for several reasons: overlapping coverage across the two private credit databases, multiple Census candidates for a given private credit firm–year within either the exact or fuzzy stage, and cases in which the same private credit firm–year retains both exact and fuzzy candidate matches that point to different Census firms. We first collapse duplicate firm–year observations when the matched Census firm identifier is the same. We then drop ambiguous cases in which multiple Census firms remain linked to the same private credit firm–year after combining across sources and match types.

A final class of duplicates arises when multiple private credit observations match to the same underlying Census firm over time. In practice, this often reflects cases in which a Census firm identifier changes

across years even though the underlying entity remains the same, as indicated by a common employer identification number (EIN), or cases in which the same firm receives multiple private credit deals over time. Since our empirical design focuses on the earliest treatment event, we retain these observations and assign treatment based on the earliest matched year. Figure A1 summarizes the merge process.

Figure A1: Merge Process

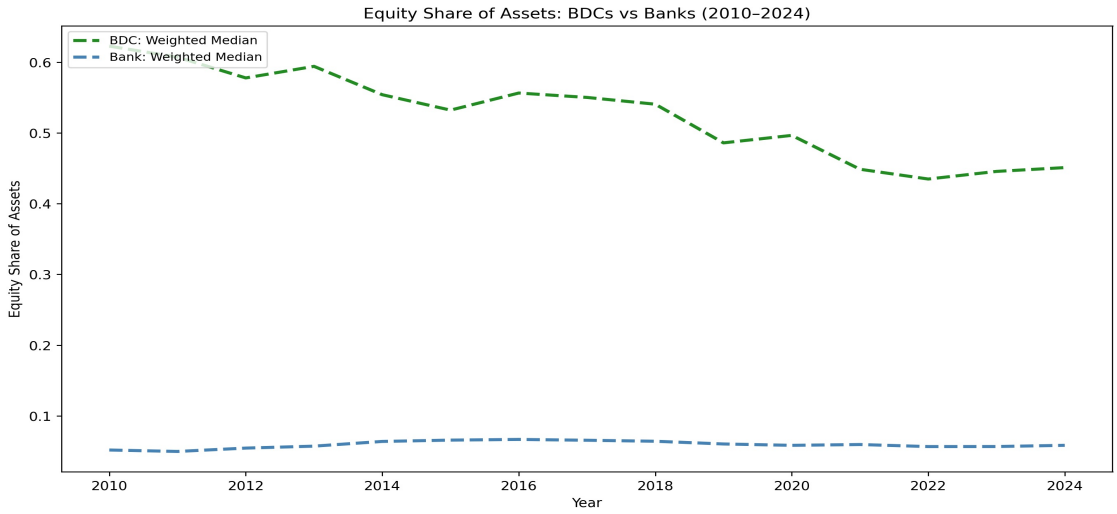


Merge pipeline linking private credit borrowers to Census records.

B Additional Figures and Tables

B.1 Institutional Background

Figure A2: Comparison of Leverage Profiles



Source: Compustat.

Table A1: Stylized Private Credit Fund Structure

Fund Profile	**Details**
Investment Manager	Monroe Capital
Fund Name	Monroe Capital Private Credit Fund III
Fund Size/Funding Source	$1.4bn LP equity across 50–100 investors
Strategy	Direct Lending — Senior Debt
Fund Lifespan	6 yrs (4-yr investment period)

Source: Preqin.

B.2 Private Credit Data

B.2.1 Complete Sample

Table A2: Private Credit Loan Term Characteristics

	Count	Mean	SD	P25	Median	P75
Size	27048	46.5	224.1	1.3	7.5	28
Maturity	19294	7.2	6.0	4.1	5.0	7.3
Spread	14846	6.8	3.0	4.8	6.3	8.5

U.S. private credit deal characteristics. Size is in nominal $mm and maturity in years. An observation is a firm–year-quarter, defined using cleaned firm names and aggregating all loans within that period. **Source**: Preqin and Pitchbook.

B.2.2 Sponsor Decomposition

Table A3: Private Credit Loan Term Characteristics Decomposed by Sponsor Status

	Count	Mean	SD	P25	Median	P75
Size-Unsponsored	10092	31.4	272.6	0.3	1.5	10.6
Size-Sponsored	16960	55.5	189.0	4	12	40
Maturity-Unsponsored	7669	10.9	7.8	5.0	9.9	11.8
Maturity-Sponsored	11626	4.7	2.0	3.6	5.0	5.7
Spread-Unsponsored	6069	6.2	2.8	3.8	6.0	8.0
Spread-Sponsored	8753	7.2	3.0	5.0	6.5	9

U.S. private credit deal characteristics. Size is in nominal $mm and maturity in years. An observation is a firm–year-quarter, defined using cleaned firm names and aggregating all loans within that period. **Source**: Preqin and Pitchbook.

B.3 Descriptive Facts Tables

B.3.1 Age, Size, and Wages

Table A4: Private Credit Firm Characteristics

	Mean	SD	P5	P25	Median	P75	P95
Age	13.8	12.9	0	3	10	22	40
Age – Sponsored	14.2	12.81	0	4	10	23	40
Age – Unsponsored	13.23	13.04	0	2	9	22	40
Employees	831	12020	0	6	40	172	1756
Employees – Sponsored	840	13640	0	13	64	217.1	1805
Employees – Unsponsored	816.9	8680	0	3	15	85.27	1572
Wages	43360	461100	0	294.4	2915	12380	96470
Wages – Sponsored	44820	477500	0	916.6	5080	16240	102400
Wages – Unsponsored	40920	432200	0	116.2	703.9	5340	85510

Percentiles are based on firm-level observations at the year of earliest private credit financing for a given firm. Units for payroll are nominal USD (thousands). Results are rounded, and percentiles are pseudo-percentiles (averages of 10 observations around each percentile) in accordance with U.S. Census Bureau disclosure guidelines.

B.3.2 Industry

Table A5: Private Credit Industry Characteristics

NAICS (2-digit)	**Full**	**Sponsored**	**Unsponsored**
54	18%	21%	14%
42	11%	12%	10%
33	10%	12%	–
62	–	–	8%

Top 3 industry exposure for each sample. Shares within each subsample at year of earliest private credit financing for a given firm. Industry classification is defined as the modal industry across a firm's establishments. Results rounded as per U.S. Census Bureau disclosure guidelines.

B.3.3 Legal Form

Table A6: Private Credit Firm Legal Form Characteristics

Legal Form	Full	Sponsored	Unsponsored
C	47%	56%	33%
Z	28%	21%	40%
P	19%	19%	20%

Shares within each subsample at year of earliest private credit financing for a given firm. Legal form classification is defined as the modal form across a firm's establishments. Results rounded as per U.S. Census Bureau disclosure guidelines.

B.4 Variable Construction

B.4.1 Gross and Excess Job Reallocation

We construct these variables following Davis et al. (2025). For firm i over horizon t to $t+s$, let $\mathscr{E}_{it}$ denote the set of establishments belonging to firm i at time t. For each establishment $e \in \mathscr{E}_{it}$, let $n_{e,t}$ and $n_{e,t+s}$ denote employment at t and $t+s$. The construction accommodates the full range of establishment dynamics: $n_{e,t+s} = 0$ for establishments that close or are divested by $t+s$, and $n_{e,t} = 0$ for establishments that open or are acquired between t and $t+s$. Establishment-level job creation and destruction are

$$jc_{e,t,t+s} = \max(n_{e,t+s} - n_{e,t},\, 0), \qquad jd_{e,t,t+s} = \max(n_{e,t} - n_{e,t+s},\, 0). \tag{B.2}$$

Aggregating to the firm and scaling by base employment E_{it}:

$$JC_{i,t,t+s} = \sum_e jc_{e,t,t+s}, \qquad JD_{i,t,t+s} = \sum_e jd_{e,t,t+s}, \qquad g_{i,t,t+s} = \frac{JC_{i,t,t+s} - JD_{i,t,t+s}}{E_{it}}. \tag{B.3}$$

The gross and excess job reallocation rates are then

$$JR_{i,t,t+s} = \frac{JC_{i,t,t+s} + JD_{i,t,t+s}}{E_{it}}, \tag{B.4}$$

$$XJR_{i,t,t+s} = JR_{i,t,t+s} - |g_{i,t,t+s}| = \frac{2 \cdot \min\left(JC_{i,t,t+s},\, JD_{i,t,t+s}\right)}{E_{it}}. \tag{B.5}$$

XJR is strictly positive whenever both creation and destruction are nonzero, isolating churning that occurs in excess of what net growth alone requires.

B.4.2 Lender Industry Expertise

We construct this variable in the spirit of Haque, Mayer and Wang (2025).For each private credit lender s, industry in, and year t, we compute the lender's share of private credit deals in that industry over the prior

three years:

$$\text{MarketShare}_{s,in,t} = \frac{\#\text{Deals}_{s,in,[t-3,t]}}{\#\text{Deals}_{in,[t-3,t]}}. \tag{B.6}$$

A lender is classified as an industry expert if its market share exceeds the median share among all private credit lenders active in the same industry-year:

$$\text{Expert}_{s,in,t} = \mathbf{1}\left(\text{MarketShare}_{s,in,t} > \underset{s'}{\text{Median}}\left\{\text{MarketShare}_{s',in,t}\right\}\right). \tag{B.7}$$

We then construct a borrower-level indicator equal to one if any lender to firm i in year t is classified as an expert in the firm's 4-digit NAICS industry:

$$\text{Expert}_{i,t} = \mathbf{1}\left(\exists\, s \in \mathscr{L}_{it} \text{ s.t. } \text{Expert}_{s,in(i),t} = 1\right), \tag{B.8}$$

where $\mathscr{L}_{it}$ denotes the set of lenders to firm i in year t and $in(i)$ denotes firm i's 3-digit NAICS industry.

B.5 Estimates from Figures

Table A7: Heterogeneity by Firm Age: Employment Growth

Employment Growth	Age Q1	Age Q2	Age Q3	Age Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.1799***	0.1063***	0.08961***	0.02324**
	(0.01289)	(0.01193)	(0.01138)	(0.01041)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	139,700,000	139,700,000	139,700,000	139,700,000
R^2	0.044	0.044	0.044	0.044

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of firm age measured at treatment year t, ordered from youngest (Q1) to oldest (Q4). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A8: Heterogeneity by Firm Age: Patent Grants

Patent Grants	Age Q1	Age Q2	Age Q3	Age Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.1203***	0.07585***	0.04958***	0.03012***
	(0.01115)	(0.01040)	(0.009688)	(0.01098)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Propensity	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	135,900,000	135,900,000	135,900,000	135,900,000
R^2	0.352	0.352	0.352	0.352

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of firm age measured at treatment year t, ordered from youngest (Q1) to oldest (Q4). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A9: Heterogeneity by Firm Age: Patent Applications

Patent Applications	Age Q1	Age Q2	Age Q3	Age Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.07394***	0.05202***	0.01543*	0.01064
	(0.009536)	(0.009207)	(0.008720)	(0.01038)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Propensity	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	135,900,000	135,900,000	135,900,000	135,900,000
R^2	0.314	0.314	0.314	0.314

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of firm age measured at treatment year t, ordered from youngest (Q1) to oldest (Q4). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A10: Heterogeneity by Firm Size: Employment Growth

Employment Growth	Size Q1	Size Q2	Size Q3	Size Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.1090***	0.1301***	0.1168***	0.04080***
	(0.01299)	(0.01135)	(0.01054)	(0.01052)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	139,700,000	139,700,000	139,700,000	139,700,000
R^2	0.044	0.044	0.044	0.044

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of firm employment measured at treatment year t, ordered from smallest (Q1) to largest (Q4). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A11: Heterogeneity by Firm Size: Exit Likelihood

Exit Likelihood	Size Q1	Size Q2	Size Q3	Size Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.05327***	0.06993***	0.02877***	0.0007493
	(0.007441)	(0.006678)	(0.005240)	(0.002606)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	160,600,000	160,600,000	160,600,000	160,600,000
R^2	0.073	0.073	0.073	0.073

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year.The sample is split into quartiles of firm employment measured at treatment year t, ordered from smallest (Q1) to largest (Q4). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A12: Heterogeneity by Industry Liquidation Value: Employment Growth

Employment Growth	LQV Q1	LQV Q2	LQV Q3	LQV Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.1324***	0.07232***	0.06984***	0.05490***
	(0.01046)	(0.01410)	(0.01209)	(0.01098)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	30,180,000	29,770,000	29,860,000	30,060,000
R^2	0.030	0.045	0.051	0.035

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of industry-year liquidation value (LQV) at treatment year t, ordered from lowest (Q1) to highest (Q4). Liquidation values are constructed following Kermani and Ma (2023) and aggregated at the three-digit NAICS–year level. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A13: Exit Likelihood by Industry Liquidation Value Quartile

Exit Likelihood	LQV Q1	LQV Q2	LQV Q3	LQV Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.03417***	0.01884***	0.02839***	0.03438***
	(0.004411)	(0.005875)	(0.005048)	(0.004983)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	34,360,000	34,320,000	35,110,000	34,820,000
R^2	0.065	0.073	0.065	0.077

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of industry-year liquidation value (LQV), ordered from lowest (Q1) to highest (Q4). Liquidation values are constructed following Kermani and Ma (2023) and aggregated at the three-digit NAICS–year level from Compustat. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A14: Exit Likelihood by Industry Leverage Quartile

Exit Likelihood	Leverage Q1	Leverage Q2	Leverage Q3	Leverage Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.03119***	0.03056***	0.03049***	0.02435***
	(0.005495)	(0.004381)	(0.004672)	(0.005521)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	39,900,000	38,100,000	37,810,000	37,090,000
R^2	0.065	0.069	0.070	0.074

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of industry-year average leverage ratio, ordered from lowest (Q1) to highest (Q4), aggregated at the three-digit NAICS–year level from Compustat. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A15: Role of Industry Expertise: Employment Growth

Employment Growth	Expert	No Expert
	(1)	(2)
Treatment (β)	0.1033***	0.08268***
	(0.009964)	(0.007132)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Firm g	Y	Y
Weighted Emp t	Y	Y
Observations	139,700,000	139,700,000
R^2	0.044	0.044

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Expert denotes borrowing firms with at least one lender whose three-year rolling market share of deals in the borrower's three-digit NAICS industry exceeds the median across all active lenders in that industry-year. No Expert denotes borrowing firms with no expert lender at treatment. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A16: Role of Industry Expertise: Patent Applications and Grants

	Applications g		**Grants** g	
	Expert	*No Expert*	*Expert*	*No Expert*
	(1)	(2)	(3)	(4)
Treatment (β)	0.04689***	0.02635***	0.09093***	0.04814***
	(0.008634)	(0.005937)	(0.009358)	(0.006573)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Propensity	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	135,900,000	135,900,000	135,900,000	135,900,000
R^2	0.314	0.315	0.352	0.352

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Expert denotes borrowing firms with at least one lender whose three-year rolling market share of deals in the borrower's three-digit NAICS industry exceeds the median across all active lenders in that industry-year. No Expert denotes borrowing firms with no expert lender at treatment. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A17: Role of Industry Expertise: Exit Likelihood

Exit Likelihood	Expert	No Expert
	(1)	(2)
Treatment (β)	0.03509***	0.02593***
	(0.004293)	(0.002981)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Firm g	Y	Y
Weighted Emp t	Y	Y
Observations	160,600,000	160,600,000
R^2	0.073	0.073

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Expert denotes borrowing firms with at least one lender whose three-year rolling market share of deals in the borrower's three-digit NAICS industry exceeds the median across all active lenders in that industry-year. No Expert denotes borrowing firms with no expert lender at treatment. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.6 Robustness

B.6.1 Sponsor Interaction

Table A18: Sponsor Interaction: Employment Growth (2-Year and 5-Year)

Employment Growth	2-Year	5-Year
	(1)	(2)
Treatment (β)	0.04373***	0.05109***
	(0.009333)	(0.01670)
Treatment × Sponsored (ν)	0.06933***	0.1290***
	(0.01185)	(0.02078)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Firm g	Y	Y
Weighted Emp t	Y	Y
Observations	139,700,000	103,900,000
R^2	0.044	0.053

Sample includes matched two-year (column 1) and five-year (column 2) continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A19: Sponsor Interaction: Employment Growth by Firm Age Quartile

Employment Growth	Age Q1	Age Q2	Age Q3	Age Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.1194***	0.07700***	0.03320*	−0.01012
	(0.02189)	(0.01871)	(0.01877)	(0.01613)
Treatment × Sponsored (ν)	0.08650***	0.04347*	0.08340***	0.05190**
	(0.02698)	(0.02408)	(0.02349)	(0.02097)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	139,700,000	139,700,000	139,700,000	139,700,000
R^2	0.044	0.044	0.044	0.044

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of firm age measured at treatment year t, ordered from youngest (Q1) to oldest (Q4). Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A20: Sponsor Interaction: Employment Growth by Firm Size Quartile

Employment Growth	Size Q1	Size Q2	Size Q3	Size Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.09721***	0.05909***	0.04096**	0.005757
	(0.01677)	(0.01708)	(0.01940)	(0.01843)
Treatment × Sponsored (ν)	0.02739	0.1106***	0.1022***	0.05018**
	(0.02644)	(0.02260)	(0.02302)	(0.02234)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	139,700,000	139,700,000	139,700,000	139,700,000
R^2	0.044	0.044	0.044	0.044

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of firm employment measured at treatment year t, ordered from smallest (Q1) to largest (Q4). Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A21: Sponsor Interaction: Employment Growth by Liquidation Value Quartile

Employment Growth	LQV Q1	LQV Q2	LQV Q3	LQV Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.08223***	0.04160*	0.01674	0.01849
	(0.01839)	(0.02214)	(0.02007)	(0.01643)
Treatment × Sponsored (ν)	0.06924***	0.04940*	0.08078***	0.05664***
	(0.02226)	(0.02862)	(0.02500)	(0.02181)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	30,180,000	29,770,000	29,860,000	30,060,000
R^2	0.030	0.045	0.051	0.035

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of industry-year liquidation value (LQV) at treatment year t, ordered from lowest (Q1) to highest (Q4), constructed following Kermani and Ma (2023). Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A22: Sponsor Interaction: Employment Growth by Industry Expertise

Employment Growth	Expert	No Expert
	(1)	(2)
Treatment (β)	0.04629***	0.04273***
	(0.01692)	(0.01116)
Treatment × Sponsored (ν)	0.08099***	0.06146***
	(0.02085)	(0.01442)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Firm g	Y	Y
Weighted Emp t	Y	Y
Observations	139,700,000	139,700,000
R^2	0.044	0.044

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Expert denotes borrowing firms with at least one lender whose three-year rolling market share of deals in the borrower's three-digit NAICS industry exceeds the median across all active lenders in that industry-year. Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A23: Sponsor Interaction: Patent Grants (2-Year and 5-Year)

Patent Grants	2-Year	5-Year
	(1)	(2)
Treatment (β)	0.02639***	0.06451***
	(0.008242)	(0.01931)
Treatment × Sponsored (ν)	0.05473***	0.1160***
	(0.01074)	(0.02389)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Propensity	Y	Y
Weighted Emp t	Y	Y
Observations	135,900,000	100,900,000
R^2	0.352	0.381

Sample includes matched two-year (column 1) and five-year (column 2) continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A24: Sponsor Interaction: Patent Grants by Firm Age Quartile

Patent Grants	Age Q1	Age Q2	Age Q3	Age Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.02996***	0.02836**	0.02452	0.02470
	(0.01152)	(0.01328)	(0.01571)	(0.01783)
Treatment × Sponsored (ν)	0.1276***	0.07035***	0.03679*	0.008499
	(0.01880)	(0.01926)	(0.01983)	(0.02253)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Propensity	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	135,900,000	135,900,000	135,900,000	135,900,000
R^2	0.352	0.352	0.352	0.352

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of firm age measured at treatment year t, ordered from youngest (Q1) to oldest (Q4). Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A25: Sponsor Interaction: Patent Grants by Industry Expertise

Patent Grants	Expert	No Expert
	(1)	(2)
Treatment (β)	0.05097***	0.01542
	(0.01417)	(0.01008)
Treatment × Sponsored (ν)	0.05634***	0.05032***
	(0.01841)	(0.01317)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Propensity	Y	Y
Weighted Emp t	Y	Y
Observations	135,900,000	135,900,000
R^2	0.352	0.352

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Expert denotes borrowing firms with at least one lender whose three-year rolling market share of deals in the borrower's three-digit NAICS industry exceeds the median across all active lenders in that industry-year. Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A26: Sponsor Interaction: Patent Applications (2-Year and 5-Year)

Patent Applications	2-Year	5-Year
	(1)	(2)
Treatment (β)	0.01566**	0.02945*
	(0.007752)	(0.01802)
Treatment × Sponsored (ν)	0.02682***	0.06173***
	(0.009935)	(0.02214)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Propensity	Y	Y
Weighted Emp t	Y	Y
Observations	135,900,000	100,900,000
R^2	0.315	0.330

Sample includes matched two-year (column 1) and five-year (column 2) continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A27: Sponsor Interaction: Patent Applications by Firm Age Quartile

Patent Applications	Age Q1	Age Q2	Age Q3	Age Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.02607***	0.02908**	0.002881	0.01091
	(0.01015)	(0.01180)	(0.01445)	(0.01721)
Treatment × Sponsored (ν)	0.06757***	0.03398**	0.01842	−0.0004234
	(0.01629)	(0.01715)	(0.01806)	(0.02151)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Propensity	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	135,900,000	135,900,000	135,900,000	135,900,000
R^2	0.314	0.314	0.314	0.314

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of firm age measured at treatment year t, ordered from youngest (Q1) to oldest (Q4). Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A28: Sponsor Interaction: Patent Applications by Industry Expertise

Patent Applications	Expert	No Expert
	(1)	(2)
Treatment (β)	0.01127	0.01742*
	(0.01372)	(0.009383)
Treatment × Sponsored (ν)	0.05022***	0.01374
	(0.01744)	(0.01205)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Propensity	Y	Y
Weighted Emp t	Y	Y
Observations	135,900,000	135,900,000
R^2	0.314	0.315

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Expert denotes borrowing firms with at least one lender whose three-year rolling market share of deals in the borrower's three-digit NAICS industry exceeds the median across all active lenders in that industry-year. Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A29: Sponsor Interaction: Exit Likelihood (2-Year and 5-Year)

Exit Likelihood	2-Year	5-Year
	(1)	(2)
Treatment (β)	0.01531***	0.04279***
	(0.003825)	(0.006819)
Treatment × Sponsored (ν)	0.02087***	0.02513***
	(0.004944)	(0.008490)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y
Lagged Firm g	Y	Y
Weighted Emp t	Y	Y
Observations	160,600,000	140,900,000
R^2	0.073	0.154

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A30: Sponsor Interaction: Exit Likelihood by Liquidation Value Quartile

Exit Likelihood	LQV Q1	LQV Q2	LQV Q3	LQV Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.01821**	0.01346	0.02135***	0.01211*
	(0.007427)	(0.008816)	(0.008247)	(0.007233)
Treatment × Sponsored (ν)	0.02188**	0.008662	0.01074	0.03424***
	(0.009145)	(0.01174)	(0.01039)	(0.009764)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	34,360,000	34,320,000	35,110,000	34,820,000
R^2	0.065	0.073	0.065	0.077

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of industry-year liquidation value (LQV) at treatment year t, ordered from lowest (Q1) to highest (Q4), constructed following Kermani and Ma (2023). Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A31: Sponsor Interaction: Exit Likelihood by Industry Leverage Quartile

Exit Likelihood	Leverage Q1	Leverage Q2	Leverage Q3	Leverage Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.02373**	0.003499	0.02809***	0.01004
	(0.009569)	(0.006267)	(0.008181)	(0.007274)
Treatment × Sponsored (ν)	0.01014	0.03840***	0.003577	0.02669**
	(0.01163)	(0.008403)	(0.009939)	(0.01086)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	39,900,000	38,100,000	37,810,000	37,090,000
R^2	0.065	0.069	0.070	0.074

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of industry-year average leverage ratio at treatment year t, ordered from lowest (Q1) to highest (Q4), aggregated at the three-digit NAICS–year level from Compustat. Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A32: Sponsor Interaction: Exit Likelihood by Firm Size Quartile

Exit Likelihood	Size Q1	Size Q2	Size Q3	Size Q4
	(1)	(2)	(3)	(4)
Treatment (β)	0.02282***	0.04214***	0.01034	−0.003309
	(0.008872)	(0.009926)	(0.009009)	(0.004316)
Treatment × Sponsored (ν)	0.06896***	0.04322***	0.02478**	0.005786
	(0.01528)	(0.01325)	(0.01099)	(0.005364)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	160,600,000	160,600,000	160,600,000	160,600,000
R^2	0.073	0.073	0.073	0.073

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. The sample is split into quartiles of firm employment measured at treatment year t, ordered from smallest (Q1) to largest (Q4). Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A33: Sponsor Interaction: Reallocation

Reallocation g	Job Reallocation	Excess Reallocation
	(1)	(2)
Treatment (β)	0.166***	0.1168***
	(0.01469)	(0.01169)
Treatment × Sponsored (ν)	0.04999***	0.04305***
	(0.0187)	(0.01496)
Industry × Size × Age × Unit-Type × Year FE	Y	Y
Lagged propensity	Y	Y
Weighted Emp t	Y	Y
Observations	134,900,000	134,900,000
R^2	0.266	0.406

Sample includes matched two-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Specifications augment the baseline with an interaction between the private credit indicator and a dummy for whether the firm is PE-sponsored at treatment (ν). Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.6.2 Alternative Sponsor Interaction

Table A34: Alternative Sponsor Specification Across Outcomes

	Employment	Grants	Applications	Exit
	(1)	(2)	(3)	(4)
Treatment (β)	0.03995***	0.01657**	0.009874	0.01253***
	(0.01018)	(0.008448)	(0.009353)	(0.004512)
Treatment × Sponsored (ν)	0.07339***	0.07247***	0.0364***	0.012**
	(0.01273)	(0.01118)	(0.0114)	(0.005494)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	139,700,000	135,900,000	135,900,000	160,600,000
R^2	0.044	0.352	0.315	0.073

Sample includes matched two-year at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.6.3 Multiple vs. Single Treatment

Table A35: Employment — Multiple vs. Single Treatments

Employment — Multiple vs. Single Treatments	Multiple	Single	Multiple × Sponsored	Single × Sponsored
	(1)	(2)	(3)	(4)
Treatment (β)	0.1785***	0.06827***	0.09219***	0.03635***
	(0.01334)	(0.00642)	(0.02541)	(0.01001)
Treatment × Sponsored (ν)			0.1107***	0.04971***
			(0.02973)	(0.01298)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	139,700,000	139,700,000	139,700,000	139,700,000
R^2	0.044	0.044	0.044	0.044

Sample includes matched two-year at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Multiple and Single denote whether the treatment firm received multiple or single private credit financings. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A36: Exit Likelihood — Multiple vs. Single Treatments

Exit Likelihood — Multiple vs. Single Treatments	Multiple	Single	Multiple × Sponsored	Single × Sponsored
	(1)	(2)	(3)	(4)
Treatment (β)	−0.01622***	0.03971***	−0.02166***	0.02052***
	(0.00332)	(0.00292)	(0.00719)	(0.00424)
Treatment × Sponsored (ν)			0.006976	0.0258***
			(0.00810)	(0.00572)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	160,600,000	160,600,000	160,600,000	160,600,000
R^2	0.073	0.073	0.073	0.073

Sample includes matched two-year at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Multiple and Single denote whether the treatment firm received multiple or single private credit financings. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.6.4 Loan Size to Wage Bill Normalization

Table A37: Employment Growth Robustness: Loan Size Normalized by Wage Bill

Employment	L/P 1	L/P 2	L/P 3	L/P 4
	(1)	(2)	(3)	(4)
Treatment (β)	0.03817**	0.01873	0.1022***	0.05922***
	(0.01949)	(0.02296)	(0.02218)	(0.02203)
Treatment × Sponsored (ν)	0.02311	0.1001***	0.07625***	0.09614***
	(0.02612)	(0.02933)	(0.02924)	(0.03334)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	139,700,000	139,700,000	139,700,000	139,700,000
R^2	0.044	0.044	0.044	0.044

Sample includes matched two-year at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. L/P 1–4 denote quartiles of the loan-to-wage bill ratio at the time of treatment. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A38: Exit Robustness: Loan Size Normalized by Wage Bill

Exit Likelihood	L/P 1	L/P 2	L/P 3	L/P 4
	(1)	(2)	(3)	(4)
Treatment (β)	0.003628	0.004413	0.03433***	0.06162***
	(0.005956)	(0.009155)	(0.01135)	(0.01452)
Treatment × Sponsored (ν)	-0.007368	0.000439	-0.005907	0.03664*
	(0.007696)	(0.01151)	(0.01416)	(0.02053)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	160,600,000	160,600,000	160,600,000	160,600,000
R^2	0.073	0.073	0.073	0.073

Control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. L/P 1–4 denote quartiles of the loan-to-wage bill ratio at the time of treatment. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.7 Pay Results

B.7.1 Total Wage Bill

Table A39: Private Credit Financing and Firm Wage Bill

Pay	2-Year	Alt Sponsor	5-Year	Single
	(1)	(2)	(3)	(4)
Treatment (β)	0.004998	0.004132	0.0147	-0.008051
	(0.009309)	(0.01029)	(0.01674)	(0.01073)
Treatment × Sponsored (ν)	0.03322**	0.03966***	0.09897***	0.01452
	(0.01183)	(0.01285)	(0.02083)	(0.01389)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	139,700,000	139,700,000	103,900,000	135,300,000
R^2	0.035	0.035	0.048	0.0272

Sample includes matched two-year and five-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.7.2 Pay Per Employee

Table A40: Private Credit Financing and Pay Per Employee

Pay per Employee	2-Year	Alt Sponsor	5-Year	Single
	(1)	(2)	(3)	(4)
Treatment (β)	-0.01562***	-0.01302***	-0.01628**	-0.01673***
	(0.004317)	(0.004973)	(0.006575)	(0.005164)
Treatment × Sponsored (ν)	-0.006211	-0.007465	-0.005935	-0.00727
	(0.005543)	(0.00613)	(0.008082)	(0.006677)
Industry × Size × Age × Unit-Type × Transaction Year FE	Y	Y	Y	Y
Lagged Firm g	Y	Y	Y	Y
Weighted Emp t	Y	Y	Y	Y
Observations	139,700,000	139,700,000	103,900,000	135,300,000
R^2	0.041	0.041	0.05	0.0372

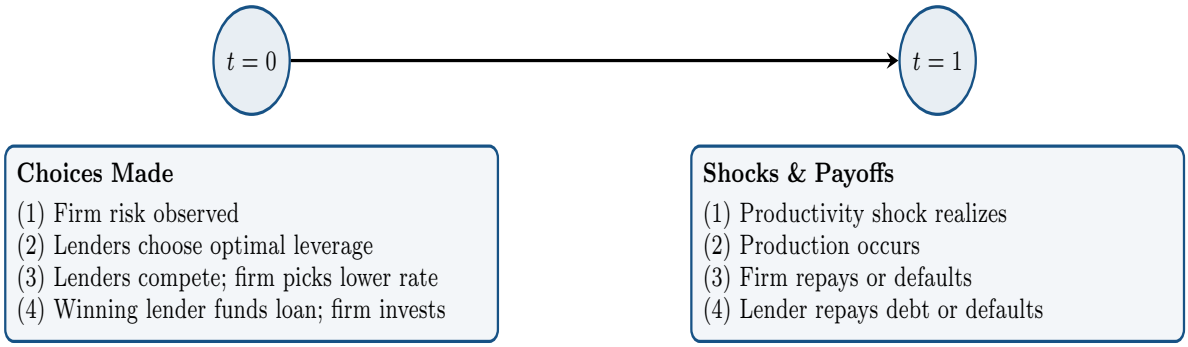
Sample includes matched two-year and five-year continuers at earliest private credit treatment and control firms in the same cells defined by the cross product of age, size, industry, multi-unit status, and treatment year. Regression estimates are weighted by employment in treatment year t. Standard errors are clustered at the firm level to account for potential heterogeneity. Results and observations are rounded as per U.S. Census Bureau disclosure guidelines. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

C Model Appendix: Details and Proofs

This appendix provides the details, derivations and proofs referenced in the model and analytical solution section.

C.1 Model Timing

Figure A3: Sequence of model timing.



C.2 Derivation of the Simplified Lender Problem

We derive equation (10) starting from the lender's problem in (7). The lender's objective is

$$V_0^J = -E_0^J + \beta \mathbb{E}\Big[\max\Big(\min\Big(R_t^{l,J},\, A_t^i\Big) L_0^J - R_t^{d,J} D_0^J,\, 0\Big)\Big]. \tag{C.9}$$

Step 1: Substitute the balance-sheet identity. From (3), $E_0^J = (1-\phi_0^J)L_0^J$. Normalizing $L_0^J = 1$:

$$V_0^J = -(1-\phi_0^J) + \beta \mathbb{E}\Big[\max\Big(\min\Big(R_t^{l,J},\, A_t^i\Big) - R_t^{d,J}\phi_0^J,\, 0\Big)\Big]. \tag{C.10}$$

Step 2: Apply Assumption 1. By (8), $R_t^{l,J} \geq R_t^{d,J}\phi_0^J$, so we can ignore the case in which the firm repays in full but the lender still defaults.

Step 3: Apply Assumption 2. Restricting to the default region $A_t^i < R_t^{l,J}$, the payoff simplifies to

$$V_0^J = -(1-\phi_0^J) + \beta \mathbb{E}\Big[\max\Big(A_t^i - R_t^{d,J}\phi_0^J,\, 0\Big)\Big]. \tag{C.11}$$

Step 4: Recast in terms of the default probability. Define $H_\sigma(\phi_0^J) \equiv \Pr(A_t^i < R_t^{d,J}\phi_0^J)$. Then

$$\mathbb{E}\Big[\mathbf{1}\{A_t^i \geq R_t^{d,J}\phi_0^J\}\Big(A_t^i - R_t^{d,J}\phi_0^J\Big)\Big] = \Big(1 - H_\sigma(\phi_0^J)\Big)\Big(A_t^i - R_t^{d,J}\phi_0^J\Big), \tag{C.12}$$

where A_t^i on the right denotes the conditional value in the no-default region. Substituting yields (10). ■

C.3 Derivation of Optimal Leverage

Under $A_t^i \sim U[\mu-\sigma^i, \mu+\sigma^i]$, the density is $1/(2\sigma^i)$ on the support. Let $a^* \equiv R_t^{d,J}\phi_0^J$ denote the default threshold. Then

$$\mathbb{E}\Big[\mathbf{1}\{A_t^i \geq a^*\}(A_t^i - a^*)\Big] \;=\; \int_{a^*}^{\mu+\sigma^i} \frac{a-a^*}{2\sigma^i}\,da \;=\; \frac{1}{4\sigma^i}\Big(\mu+\sigma^i-a^*\Big)^2. \tag{C.13}$$

Substituting $a^* = R_t^{d,J}\phi_0^J$ yields the closed-form lender value

$$V_0^J \;=\; \frac{\beta}{4\sigma^i}\Big(\mu+\sigma^i - R_t^{d,J}\phi_0^J\Big)^2 \;-\; (1-\phi_0^J). \tag{C.14}$$

Private Credit. The private credit lender solves

$$\max_{\phi_0^P}\; \frac{\beta}{4\sigma^i}\Big(\mu+\sigma^i - R_t^{d,P}(\phi_0^P,\sigma^i)\,\phi_0^P\Big)^2 \;-\; (1-\phi_0^P), \tag{C.15}$$

where $R_t^{d,P}(\phi_0^P,\sigma^i)$ is the endogenous funding cost, increasing in both ϕ_0^P and σ^i. Letting $(\tilde{R}^P)'(\phi_0^{*,P}) \equiv (1+\varepsilon^P)R^{d,P}(\phi_0^{*,P},\sigma^i)$ denote the marginal funding cost evaluated at the optimum, where $\varepsilon^P \equiv \frac{\partial R^{d,P}/\partial\phi_0^P}{R^{d,P}}\phi_0^{*,P}$ is the funding-cost elasticity, the first-order condition is

$$-\frac{\beta\,(\tilde{R}^P)'(\phi_0^{*,P})}{2\sigma^i}\Big(\mu+\sigma^i - R_t^{d,P}\phi_0^{*,P}\Big) \;+\; 1 \;=\; 0. \tag{C.16}$$

Solving for $\phi_0^{*,P}$:

$$\mu+\sigma^i - R_t^{d,P}\phi_0^{*,P} \;=\; \frac{2\sigma^i}{\beta\,(\tilde{R}^P)'(\phi_0^{*,P})}, \tag{C.17}$$

which rearranges to

$$\phi_0^{*,P} \;=\; \frac{\mu+\sigma^i}{R_t^{d,P}} \;-\; \frac{2\sigma^i}{\beta\,(R_t^{d,P})^2}\cdot\frac{1}{1+\varepsilon^P}, \tag{C.18}$$

which is equation (11). The second-order condition is satisfied since V_0^P is strictly concave in ϕ_0^P: the coefficient on the quadratic term is $-\beta((\tilde{R}^P)')^2/(2\sigma^i) < 0$. Since the objective is strictly concave and unconstrained, the private credit lender always reaches an interior optimum.

Bank. The bank's funding cost is flat at $R_t^{d,B}$, independent of leverage, so the marginal funding cost equals the average: $(\tilde{R}^B)'(\phi_0^B) = R_t^{d,B}$. The bank solves

$$\max_{\phi_0^B}\; \frac{\beta}{4\sigma^i}\Big(\mu+\sigma^i - R_t^{d,B}\phi_0^B\Big)^2 \;-\; (1-\phi_0^B), \quad \text{s.t.} \quad \phi_0^B \leq \frac{\bar{\phi}^B}{\omega(\sigma^i)}. \tag{C.19}$$

Because $R_t^{d,B}$ is cheap relative to the opportunity cost of equity, the unconstrained optimum lies above $\bar{\phi}^B/\omega(\sigma^i)$ for all σ^i, so the constraint always binds: $\phi_0^{*,B} = \bar{\phi}^B/\omega(\sigma^i)$. Forming the Lagrangian with multi-

plier $\lambda > 0$,

$$\mathscr{L} = \frac{\beta}{4\sigma^i}\left(\mu + \sigma^i - R_t^{d,B}\phi_0^B\right)^2 - (1-\phi_0^B) - \lambda\left[\omega(\sigma^i)\phi_0^B - \bar{\phi}^B\right], \tag{C.20}$$

the first-order condition is

$$-\frac{\beta R_t^{d,B}}{2\sigma^i}\left(\mu + \sigma^i - R_t^{d,B}\phi_0^{*,B}\right) + 1 - \lambda\,\omega(\sigma^i) = 0. \tag{C.21}$$

Rearranging and isolating $\phi_0^{*,B}$:

$$\phi_0^{*,B} = \frac{1}{R_t^{d,B}}\left(\mu + \sigma^i - \frac{2\sigma^i}{\beta R_t^{d,B}}\right) - \frac{2\lambda\,\sigma^i\,\omega(\sigma^i)}{\beta\,R_t^{d,B}}, \tag{C.22}$$

which is equation (12).[19] ■

C.4 Comparative Statics

This subsection derives each comparative static of optimal leverage by differentiating (11) and (12) with respect to each parameter. For reference, the FOCs are:

$$\phi_0^{*,P} = \frac{\mu+\sigma^i}{R_t^{d,P}} - \frac{2\sigma^i}{\beta\,(R_t^{d,P})^2}\cdot\frac{1}{1+\varepsilon^P},$$
$$\phi_0^{*,B} = \frac{1}{R_t^{d,B}}\left(\mu + \sigma^i - \frac{2\sigma^i}{\beta R_t^{d,B}}\right) - \frac{2\lambda\,\sigma^i\,\omega(\sigma^i)}{\beta\,R_t^{d,B}}.$$

Signs below are evaluated under the standing parameter restrictions that the default threshold lies interior to the productivity support and that $\beta\,R_t^{d,J} > 2$, which ensures the risk-adjusted cost of leverage dominates in the σ^i derivative. For the private credit lender, comparative statics treat ε^P as fixed at its equilibrium value, holding the marginal funding cost schedule constant.

Firm risk σ^i. For private credit, σ^i enters term (1) through the numerator and term (2) through the numerator:

$$\frac{\partial\phi_0^{*,P}}{\partial\sigma^i} = \frac{1}{R_t^{d,P}} - \frac{2}{\beta\,(R_t^{d,P})^2(1+\varepsilon^P)} = \frac{1}{R_t^{d,P}}\left(1 - \frac{2}{\beta R_t^{d,P}(1+\varepsilon^P)}\right). \tag{C.23}$$

Under $\beta R_t^{d,P}(1+\varepsilon^P) < 2$, this is negative. For the bank, since the constraint binds at $\phi_0^{*,B} = \bar{\phi}^B/\omega(\sigma^i)$:

$$\frac{\partial\phi_0^{*,B}}{\partial\sigma^i} = -\frac{\bar{\phi}^B\,\omega'(\sigma^i)}{\omega(\sigma^i)^2} < 0, \tag{C.24}$$

since $\omega'(\sigma^i) > 0$: a riskier firm triggers a tighter effective constraint, reducing permissible bank leverage directly.

[19] The sign on the regulatory-drag term follows the convention in which a tighter constraint raises λ and reduces $\phi_0^{*,B}$. The multiplier $\lambda > 0$ is determined jointly with $\phi_0^{*,B}$ by the complementary slackness condition $\omega(\sigma^i)\phi_0^{*,B} = \bar{\phi}^B$.

Lender debt cost $R_t^{d,J}$. For private credit:

$$\frac{\partial \phi_0^{*,P}}{\partial R_t^{d,P}} = -\frac{\mu + \sigma^i}{(R_t^{d,P})^2} + \frac{4\sigma^i}{\beta\,(R_t^{d,P})^3(1+\varepsilon^P)}. \tag{C.25}$$

The first term dominates under standing restrictions, giving a negative total effect. The bank's leverage is pinned by the constraint and does not depend directly on $R_t^{d,B}$ through an interior condition; its effect operates through the Lagrange multiplier λ, which adjusts to maintain $\phi_0^{*,B} = \bar{\phi}^B/\omega(\sigma^i)$.

Expected productivity μ. Only term (1) of the private credit FOC depends on μ:

$$\frac{\partial \phi_0^{*,P}}{\partial \mu} = \frac{1}{R_t^{d,P}} > 0. \tag{C.26}$$

For the bank, the constraint pins leverage independently of μ, so the effect operates through λ: a higher μ relaxes the shadow cost of the constraint without changing $\phi_0^{*,B} = \bar{\phi}^B/\omega(\sigma^i)$.

Discount factor β. For private credit, only term (2) depends on β:

$$\frac{\partial \phi_0^{*,P}}{\partial \beta} = \frac{2\sigma^i}{\beta^2\,(R_t^{d,P})^2(1+\varepsilon^P)} > 0. \tag{C.27}$$

A more patient lender weights future equity payoffs more heavily, increasing the marginal return to leverage. For the bank, β does not enter the constraint directly, so the effect again operates through the shadow multiplier.

Lagrange multiplier λ. Only the regulatory drag term in the bank FOC depends on λ:

$$\frac{\partial \phi_0^{*,B}}{\partial \lambda} = -\frac{2\sigma^i\,\omega(\sigma^i)}{\beta\,R_t^{d,B}} < 0. \tag{C.28}$$

$\partial \phi_0^{*,P}/\partial \lambda = 0$ since λ does not enter the private credit problem.

Capital cap $\bar{\phi}^B$. Since the constraint always binds, $\omega(\sigma^i)\phi_0^{*,B} = \bar{\phi}^B$, so

$$\frac{\partial \phi_0^{*,B}}{\partial \bar{\phi}^B} = \frac{1}{\omega(\sigma^i)} > 0. \tag{C.29}$$

$\partial \phi_0^{*,P}/\partial \bar{\phi}^B = 0$.

Risk weight $\omega(\sigma^i)$. The constraint gives $\phi_0^{*,B} = \bar{\phi}^B/\omega(\sigma^i)$, so treating ω as a parameter:

$$\frac{\partial \phi_0^{*,B}}{\partial \omega(\sigma^i)} = -\frac{\bar{\phi}^B}{\omega(\sigma^i)^2} < 0. \tag{C.30}$$

$\partial\phi_0^{*,P}/\partial\omega(\sigma^i) = 0$.

Summary. Table A41 collects these results.

Table A41: Comparative Statics of Optimal Leverage

	Bank		Private Credit	
Param.	$\partial\phi_0^{*,B}/\partial x$	Sign	$\partial\phi_0^{*,P}/\partial x$	Sign
$\sigma^i \uparrow$	$-\bar{\phi}^B\omega'(\sigma^i)/\omega(\sigma^i)^2$	$-$	$\frac{1}{R_t^{d,P}} - \frac{2}{\beta(R_t^{d,P})^2(1+\varepsilon^P)}$	$-$
$R_t^{d,J} \uparrow$	via λ	$-$	$-\frac{\mu+\sigma^i}{(R_t^{d,P})^2} + \frac{4\sigma^i}{\beta(R_t^{d,P})^3(1+\varepsilon^P)}$	$-$
$\mu \uparrow$	via λ	$+$	$1/R_t^{d,P}$	$+$
$\beta \uparrow$	via λ	$+$	$\frac{2\sigma^i}{\beta^2(R_t^{d,P})^2(1+\varepsilon^P)}$	$+$
$\lambda \uparrow$	$-2\sigma^i\omega(\sigma^i)/(\beta R_t^{d,B})$	$-$	0	—
$\bar{\phi}^B \uparrow$	$1/\omega(\sigma^i)$	$+$	0	—
$\omega(\sigma^i) \uparrow$	$-\bar{\phi}^B/\omega(\sigma^i)^2$	$-$	0	—

C.5 Proof of Proposition 1

Since the bank always binds the capital constraint, $\phi_0^{*,B} = \bar{\phi}^B/\omega(\sigma^i)$, which is determined independently of $\phi_0^{*,P}$. The leverage gap is

$$\phi_0^{*,B} - \phi_0^{*,P} = \frac{\bar{\phi}^B}{\omega(\sigma^i)} - \left[\frac{\mu+\sigma^i}{R_t^{d,P}} - \frac{2\sigma^i}{\beta(R_t^{d,P})^2(1+\varepsilon^P)}\right]. \tag{C.31}$$

Substituting the bank FOC expression (12) for $\bar{\phi}^B/\omega(\sigma^i)$ and rearranging in terms of Δ^* and Γ^*, where $\Delta^* \equiv 1/R^{d,B} - 1/(\tilde{R}^P)'(\phi_0^{*,P})$ and $\Gamma^* \equiv 1/(\beta(R^{d,B})^2) - 1/(\beta((\tilde{R}^P)'(\phi_0^{*,P}))^2)$, the gap satisfies

$$\phi_0^{*,B} - \phi_0^{*,P} = (\mu+\sigma^i)\Delta^* - 2\sigma^i\Gamma^* - \frac{2\lambda\sigma^i\omega(\sigma^i)}{\beta R_t^{d,B}}. \tag{C.32}$$

Both $\Delta^* > 0$ and $\Gamma^* > 0$ since $(\tilde{R}^P)'(\phi_0^{*,P}) = (1+\varepsilon^P)R^{d,P} > R^{d,P} > R^{d,B}$. The condition $\phi_0^{*,B} > \phi_0^{*,P}$ is then equivalent to (13).

Case 1 ($\lambda = 0$). The regulatory drag vanishes and the condition reduces to $(\mu+\sigma^i)\Delta^* > 2\sigma^i\Gamma^*$, which holds in the natural parameter region where μ is large relative to σ^i.

Case 2 ($0 < \lambda < \lambda^*$). Define λ^* as the value at which equality holds in (13). For $\lambda < \lambda^*$ the bank still levers more than private credit despite the regulatory drag.

Case 3 ($\lambda > \lambda^*$). The regulatory drag dominates and $\phi_0^{*,B} < \phi_0^{*,P}$. Since $\omega(\sigma^i)$ is increasing in σ^i, λ^* is

decreasing in σ^i, so the reversal occurs for the riskiest firms first. ■

C.6 Equilibrium Loan Rate

Each lender prices at its own breakeven, setting $R_t^{l,J}$ such that $V_0^J(\phi_0^{*,J}, R_t^{l,J}) = 0$. Using the closed-form value function (C.14) evaluated at optimal leverage:

$$\frac{\beta}{4\sigma^i}\left(\mu + \sigma^i - R_t^{d,J}\phi_0^{*,J}\right)^2 - (1-\phi_0^{*,J}) = 0. \tag{C.33}$$

Solving for $R_t^{l,J}$ via the no-default payoff:

$$\mu + \sigma^i - R_t^{d,J}\phi_0^{*,J} = 2\sqrt{\frac{\sigma^i(1-\phi_0^{*,J})}{\beta}}, \tag{C.34}$$

which gives

$$R_t^{l,J} = \mu + \sigma^i - 2\sqrt{\frac{\sigma^i(1-\phi_0^{*,J})}{\beta}}, \tag{C.35}$$

which is equation (15). The loan rate is increasing in $\phi_0^{*,J}$: higher leverage thins the equity buffer, raising default exposure and requiring a higher rate to break even. It is also increasing in σ^i, since a wider productivity distribution raises default probability, with the effect steepening in leverage. ■

C.7 Proof of Proposition 2

Using the breakeven loan rate (15) and the value function (C.14), joint surplus for each lender is

$$JS^J(\sigma^i;\cdot) = \mu - R_t^{l,J} + V_0^J = \mu - C^J(\sigma^i;\cdot), \tag{C.36}$$

where the loan rate cancels as a transfer. Substituting the closed-form expressions and evaluating at optimal leverage:

$$JS^P(\sigma^i;\cdot) = \mu - \frac{\sigma^i}{\beta(\tilde{R}^P)'(\phi_0^{*,P})}\cdot f^P(\sigma^i), \tag{C.37}$$

$$JS^B(\sigma^i;\cdot) = \mu - \frac{\sigma^i}{\beta R_t^{d,B}}\cdot f^B(\sigma^i) - \frac{\lambda\omega(\sigma^i)\sigma^i}{\beta R_t^{d,B}}, \tag{C.38}$$

where $f^J(\sigma^i)$ collects the leverage-sensitivity terms for each lender. Setting $JS^B(\hat{\sigma}^i;\cdot) = JS^P(\hat{\sigma}^i;\cdot)$ and rearranging using Δ^* and Γ^* as defined in Proposition 2:

$$\mu\Delta^* = \hat{\sigma}^i\left[2\Gamma^* - \Delta^* + \frac{2\lambda\omega(\hat{\sigma}^i)}{\beta R_t^{d,B}}\right], \tag{C.39}$$

so

$$\hat{\sigma}^i = \frac{\mu\Delta^*}{2\Gamma^* - \Delta^* + \dfrac{2\lambda\,\omega(\hat{\sigma}^i)}{\beta\,R_t^{d,B}}}, \tag{C.40}$$

which is equation (18).

Existence and uniqueness. Define $F(\sigma) \equiv JS^B(\sigma;\cdot) - JS^P(\sigma;\cdot)$. At $\sigma = 0$, $F(0) = \mu\Delta^* > 0$ since $\Delta^* > 0$. As $\sigma \to \infty$, $F(\sigma) \to -\infty$ because C^B grows faster than C^P due to the regulatory drag and the steeper leverage-sensitivity coefficient. By the intermediate value theorem there exists at least one root; strict monotonicity $F'(\sigma) < 0$, which follows from $\omega(\sigma)$ weakly increasing and the leverage-sensitivity coefficient being positive, establishes uniqueness.

Sorting. For $\sigma^i < \hat{\sigma}^i$, $F(\sigma^i) > 0$ so $JS^B > JS^P$ and the firm borrows from the bank. For $\sigma^i > \hat{\sigma}^i$, $F(\sigma^i) < 0$ so $JS^P > JS^B$ and the firm borrows from private credit. ■