How Much Do Small Businesses Rely on Personal Credit?*

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Abstract

In 2021, operational failures at nine Paycheck Protection Program lenders disrupted funding for nearly a million small business loans. Using linked credit records of owners and businesses, we study how owners' personal borrowing responded to the shortfall in business credit. Observationally similar owners approved for the same loan amounts at lenders with failures received nearly $14,000 less in 2021 than those at lenders without failures. On average, owners replaced half of the shortfall with personal borrowing within a year and essentially all of it within three, consistent with personal credit being the marginal funding source for the typical business. Firms owned by renters, who do not replace lost business credit, see a 20 percent decline in employment, while firms owned by homeowners do not contract.

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1 Introduction

Over 80 percent of small businesses report using the owner's personal credit score to obtain financing (Small Business Credit Survey, 2019a,b), suggesting business credit is often underwritten on the owner. Because the same owner can also access consumer credit markets, small-business and personal borrowing are linked, but how they interact is unclear. Owners facing a shortfall in business credit might replace it with personal borrowing, absorbing the shock on their household balance sheet. Alternatively, the shortfall may worsen the owner's risk profile and restrict personal credit access, so that business and personal borrowing contract together. The pass-through from business to personal credit then determines whether owners' household balance sheets dampen or amplify business credit shocks, and whether business credit data over- or understate the financial constraints of small businesses.

There are two challenges in estimating this important pass-through. The first is measuring the borrowing of both the business and its owner, since personal and business credit are recorded separately and typically cannot be linked. The second challenge is identification. Measuring this pass-through requires variation in the supply of business credit that is orthogonal to the owner's personal credit conditions. This precludes the use of shocks to the owner's circumstances—which directly affect both personal and business credit underwriting—and rules out most supply-side contractions, as those can affect households directly through consumer credit supply.

This paper estimates the extent to which small business owners replace lost business credit with personal borrowing and the real effects of this pass-through. We measure both funding sources using 2016–2023 data from the Gies Consumer and Small Business Credit Panel (GCCP), a state-of-the-art credit panel that links personal and business credit records of the same individuals. We supplement this panel with loan-level data from the Paycheck Protection Program (PPP), consisting of government-guaranteed loans to small businesses in 2020–2021. We exploit operational failures at nine PPP lenders, which disrupted the funding of approved business credit. We classify lenders as affected based on documentation of these failures in litigation, federal enforcement actions, and bankruptcy proceedings.

Difference-in-differences estimates comparing similar owners approved for the same PPP loan amounts at lenders with and without failures show that affected owners receive $13,597 less in PPP funds in 2021 than unaffected owners, a magnitude that exceeds the total cash reserves of the typical small business (Farrell and Wheat, 2016). The shortfall narrows as more funding arrives, to an average of $7,863 per year over 2021–22. Other business credit does not respond, implying that the PPP funding shock was a net shortfall in business credit

Owners replace this lost business credit with personal borrowing nearly one-for-one. Personal balances rise by $6,810 in 2021 against the initial $13,597 shock, and by $7,877 on average between 2021 and 2023—essentially dollar-for-dollar against the $7,863 average shortfall. A simple framework rationalizes full substitution to personal credit if that was already the marginal funding source before the shock, consistent with business owners having high levels of home equity before 2021 (Bhutta et al., 2020) and with survey evidence that the typical small business routinely uses personal credit (e.g. Robb and Robinson, 2014)

The implied pass-through, computed as the Wald ratio between the personal and business credit estimates, is thus 0.50 on impact and 1.00 averaged over the post period. Personal borrowing responds primarily through mortgage credit and other personal installment loans. A placebo check confirms no effect on homeownership, consistent with owners tapping into home equity rather than new home purchases. The substitution with personal credit is concentrated among business owners who owned homes in 2020, with a pass-through indistinguishable from zero for renters.

We find evidence that personal credit allows small business owners to smooth the business credit shock and avoid layoffs. PPP failures have no effect on employment for the average firm, consistent with the average dollar-for-dollar pass-through from business to personal credit. This null finding masks substantial heterogeneity, with renter-owned firms—who do not exhibit a significant pass-through to personal credit—contracting by one employee (or 20 percent of the 2020 mean).

Our research design has three identifying assumptions. The first is parallel trends: absent the operational failures, the personal borrowing of owners who obtained PPP loans at affected and unaffected lenders would have evolved in parallel, conditional on controls. While untestable, we support this assumption by showing that, conditional on baseline credit scores and approved PPP amounts, the two groups are balanced on a wide range of personal and business characteristics, including loan balances, repayment, firm size, and demographics, and their outcomes evolve in parallel in the five years preceding the shock. The second assumption is that owners did not anticipate the failures, which is plausible since the failures happened after borrowing decisions had been made, and borrowers would presumably have chosen different lenders had they anticipated not receiving the funds. Our event studies also show no evidence of anticipation, with outcomes for treated and control evolving in parallel through 2020. Finally, interpreting the Wald ratio between our business and personal credit estimates as a pass-through requires that the failures affect personal borrowing only through withheld business credit. The latter highlights the advantage of exploiting operational issues in the implementation of a business credit program inherently separate from consumer credit supply, especially since the majority of affected lenders did not lend to consumers.

We further support these assumptions with a wide range of robustness checks. To address selection into affected lenders, all of which are non-banks, we re-estimate effects using only other non-bank PPP lenders as controls. To alleviate concerns about differential exposure to pandemic-era conditions, we allow counterfactual outcome paths to vary flexibly by state and by two-digit industry. We also show that our results are not driven by firms established after 2019—addressing concerns about PPP fraud involving newly created businesses—or by differential PPP approval timing.

Taken together, our results suggest that household balance sheets play a crucial role in dampening small business credit shocks and that the owner's personal borrowing serves as the marginal funding source for the typical small business. This implies that business credit data alone overstate the financial constraints of small businesses and miss an important mechanism through which business credit shocks propagate through household balance sheets.

Our findings contribute to several strands of literature. The first connects housing collateral to entrepreneurship, using variation in housing equity to study whether personal wealth enables business formation (e.g., Black, Meza and Jeffreys, 1996; Adelino, Schoar and Severino, 2015; Corradin and Popov, 2015; Schmalz, Sraer and Thesmar, 2017; Bracke, Hilber and Silva, 2018; Jensen, Leth-Petersen and Nanda, 2022; Kerr, Kerr and Nanda, 2022). Relatedly, Bahaj, Foulis and Pinter (2020) shows a link between owners' home equity and firm borrowing. Unlike most of this literature, our evidence is on the intensive margin for existing firms. Moreover, we observe business and personal credit for the same individual and measure the household borrowing response to a granular business credit shock, rather than the firm creation or firm borrowing response to home values. A separate literature studies how removing derogatory personal credit information affects entry into self-employment (Bos, Breza and Liberman, 2018; Dobbie, Goldsmith-Pinkham, Mahoney and Song, 2020; Herkenhoff, Phillips and Cohen-Cole, 2021).

We also relate to papers studying the supply of business credit to small firms and substitution across business lenders (Chen, Hanson and Stein, 2017; Cortés, Demyanyk, Li, Loutskina and Strahan, 2020; Bord, Ivashina and Taliaferro, 2021).[1] Gopal and Schnabl (2022) document substitution from banks toward finance companies within business credit, and Robb and Robinson (2014) and Haughwout, Lee, Scally and Van der Klaauw (2021) provide survey and credit-panel evidence that owners draw on personal margins. Berger, Cowan and Frame (2011) and Hair, Howell, Johnson and Matsumoto (2025) doc-

[1] A broader literature studies the credit and liquidity constraints faced by entrepreneurs. Some key topics include relationship lending (Petersen and Rajan, 1994; Berger and Udell, 1995, 2002), banking deregulation (Black and Strahan, 2002; Cetorelli and Strahan, 2006; Zarutskie, 2006; Bertrand, Schoar and Thesmar, 2007; Kerr and Nanda, 2009; Chava, Oettl, Subramanian and Subramanian, 2013; Cornaggia, Mao, Tian and Wolfe, 2015; Hombert and Matray, 2017), and wealth (Hurst and Lusardi, 2004; Cagetti and De Nardi, 2006; Andersen and Nielsen, 2012; Bellon, Cookson, Gilje and Heimer, 2021).

ument that lenders rely on owners' personal credit scores in small business underwriting, with the latter showing that this practice disadvantages young entrepreneurs. Akcigit, Chhina, Cilasun, Miranda and Serrano-Velarde (2025) and Benetton and Buchak (2025) show that credit cards are an important source of short-term financing for small businesses. Kim, Parker and Schoar (2020) and Kim (2022) explore a different aspect of the relationship between personal and business finances by analyzing the bank account transactions of business owners, focusing on consumption and cash flows instead of credit. Our contribution is to estimate the causal effect of a business credit shock on personal borrowing. Our near-complete pass-through estimate suggests that much of the response to this shock occurs outside business data, which is the scope of most of this literature.

Finally, we also relate to evaluations of the PPP and its effects on employment and business outcomes (e.g., Granja, Makridis, Yannelis and Zwick, 2022; Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar and Yildirmaz, 2020; Chetty, Friedman, Hendren, Stepner and Team, 2020; Bartlett III and Morse, 2020; Doniger and Kay, 2023; Gorbachev, Luengo-Prado and Wang, 2026) and of access to the program across lenders and borrowers (e.g., Erel and Liebersohn, 2022; Howell, Kuchler, Snitkof, Stroebel and Wong, 2024). While we do not evaluate the program, we use its operational failures as a source of variation in business credit, and our estimates imply that part of the program's incidence fell on owners' household balance sheets.

The remainder of the paper proceeds as follows. Section 2 presents a conceptual framework. Section 3 provides background on the PPP and its operational failures, and describes the data. Section 4 presents the empirical strategy and Section 5 reports the main results. Section 6 presents robustness checks and additional results, and Section 7 concludes.

2 Conceptual Framework

We start with a simple model to formalize how a small business credit shock can be absorbed through other margins. The framework illustrates two points. First, the shock is fully absorbed by other funding sources, even if more expensive, unless it raises the firm's marginal cost of funding, in which case the shock has real effects. Second, and relatedly, the responses through other business credit, personal credit, and real outcomes such as employment are linked through the firm's financing constraint. Each dollar of lost credit must be replaced by another source of financing or absorbed by reducing the scale of the business, providing a useful frame of reference for interpreting our empirical estimates.

Environment. The model is static. At the start of the period, an owner-managed firm has access to a low-cost source of business credit, rationed up to $\bar{x}$. In our empirical analysis, this source of business credit is PPP funding, but the framework generalizes to non-subsidized credit that is cheaper than other funding sources and sufficiently rationed. A supply shock of size $s \in [0, \bar{x}]$ reduces this funding amount to $\bar{x} - s$. In the baseline model, the shock does not directly change the terms of other business borrowing b and personal financing used to support the firm q.

Firm problem. After observing s, the business chooses employment n and generates value $\Pi(n)$, with $\Pi'(n) > 0$ and $\Pi''(n) < 0$. The owner's problem is

$$\begin{aligned} \max_{n,b,q} \quad & \Pi(n) - r_B b - r_P q \\ \text{subject to} \quad & \omega n \leq \bar{x} - s + b + q, \\ & 0 \leq b \leq \bar{b}, \\ & 0 \leq q \leq \bar{q}, \end{aligned} \tag{1}$$

where ω is the funding need per employee, r_B and r_P are the unit costs of other business credit and effective personal financing, and $\bar{b}$ and $\bar{q}$ are their borrowing limits. We assume the firm uses the cheap business credit in full even if $s = 0$. This is a reasonable assumption for PPP funding, which had statutory caps that were fairly tight for small businesses and was generally convertible into grants.

Marginal cost of funding. We solve the residual financing problem in Appendix B.1, leading to a cost of funding that is piecewise linear. Intuitively, the effect of the business credit shock depends on whether it alters the firm's marginal cost of funding. If the shortfall s can be replaced by the same funding source that was already marginal without its borrowing constraint binding, the marginal cost is unchanged. The owner replaces the missing business credit by borrowing more from the marginal source, and employment does not fall. Employment falls only if the shock moves the owner to a more expensive marginal source or exhausts all borrowing capacity. Thus, a firm can lose a cheaper source of credit and still maintain its scale if the replacement source was already funding the marginal dollar.

Accounting decomposition. The model also gives a simple comparative static decomposition of the credit shortfall. Let n_0, b_0, and q_0 denote choices when $s = 0$, and let n_s, b_s, and q_s denote choices after a shock of size s. Since the financing constraint binds in both allocations,

$$\begin{aligned} \omega n_0 &= \bar{x} + b_0 + q_0, \\ \omega n_s &= \bar{x} - s + b_s + q_s. \end{aligned} \tag{2}$$

Define $\Delta b = b_s - b_0$, $\Delta q = q_s - q_0$, and $\Delta n = n_0 - n_s$. Subtracting the second line of Equation (2) from the first gives

$$\Delta q + \Delta b + \omega \Delta n = s. \tag{3}$$

Each dollar of the shortfall is therefore replaced with other business credit, replaced with effective personal financing, or absorbed through lower employment. Note that our data measure the owner's total personal borrowing p, rather than the amount q that supports the firm. We define $\tau \equiv \Delta q/\Delta p$ as the share of the personal borrowing response that provides effective liquidity to the firm. The remaining personal borrowing is used outside the model, for instance, to finance consumption. Dividing both sides of Equation (3) by s and plugging in $\theta \equiv \Delta p/s$—the pass-through to personal borrowing, which is our main empirical estimate—and the definition of τ gives

$$\tau\theta + \frac{\Delta b}{s} + \frac{\omega \Delta n}{s} = 1. \tag{4}$$

Equation (4) thus serves as a simple frame of reference for our empirical analysis. A high θ along with no business credit response and little change in employment would indicate that personal credit serves as a buffer for the firm. Alternatively, we would expect a small overall borrowing response to be associated with a larger employment decline. In the baseline model, the latter pattern can arise if personal credit is too expensive or personal borrowing constraints bind.[2]

Empirical predictions. The framework predicts that if the credit shock does not alter the marginal source of funding, the firm will fully replace the lost funding with the marginal funding and not change its scale. Alternatively, if the shock changes the marginal funding source to a more expensive one and/or exhausts funding capacity, employment should fall. The framework also implies that the pass-through from business to personal credit is state-dependent. As credit conditions change, the marginal source of funding for the typical business may also change, leading to a different substitution pattern.

As we show in Section 5, we estimate a pass-through to personal credit of 1 and small, insignificant coefficients for Δb and Δn. As discussed above, the model rationalizes a dollar-for-dollar pass-through from cheaper business credit to more expensive personal credit if personal credit was already the marginal funding source, consistent with business owners having high levels of home equity before the pandemic (Bhutta et al., 2020) and with survey evidence that small businesses routinely use personal credit (e.g. Robb and Robinson, 2014). Moreover, assuming businesses do not adjust on dimensions outside the model, such as lower investment, interpreting our estimates through Equation (4) suggests

[2] Appendix B.2 allows the shock to raise the cost of personal credit or reduce its borrowing constraint—proxying for the effect of a business credit shock on the owner's risk profile—and derives when this channel amplifies the employment response.

full business use of the additional personal borrowing (i.e., $\tau = 1$).

3 Background and Data

3.1 The Paycheck Protection Program

The Paycheck Protection Program extended government-guaranteed loans to small businesses through private lenders from April 2020 to May 2021, totaling approximately 11.8 million loans and $799 billion across more than 5,400 lenders (U.S. Small Business Administration, 2021). Loans were eligible for full or partial forgiveness if borrowers used the proceeds for payroll and other eligible expenses. By statute, PPP loans carried no collateral or personal guarantees, and the SBA waived its ordinary creditworthiness and credit-elsewhere requirements, so lenders processed applications without conventional credit screening (U.S. Small Business Administration, 2020). The 2021 wave of the program served smaller businesses, with 87 percent of loans amounting to $50,000 or less (U.S. Small Business Administration, 2021).

Borrowers applied through a lender and self-certified their eligibility, and the lender collected the required documentation and performed eligibility and loan-amount checks. The SBA then processed the loan through its E-Tran system, a largely automated process without traditional underwriting. The lender disbursed the funds, and borrowers later applied for forgiveness through the loan servicer, which reviewed the request and submitted it to the SBA. Each stage of funding therefore depended on the lender's operational capacity. In 2021, much of that capacity came from technology platforms, chiefly Blueacorn and Womply's Fast Lane, which handled application intake, eligibility checks, and customer support for the lenders (Cowley and Koeze, 2021). Borrowers reached these platforms through online marketing and the promise of fast processing, typically without a prior lender relationship (Battisto, Godin, Kramer Mills and Sarkar, 2021).

We searched for documentation of operational failures in federal enforcement actions, litigation, and bankruptcy proceedings across the universe of PPP lenders. We allow for failures in all aspects of funding, including in the timing of approvals and in disbursement. We classify nine PPP lenders as having experienced operational failures: Benworth Capital, Capital Plus Financial, DreamSpring, Fountainhead SBF, Harvest Small Business Finance, Itria Ventures, Kabbage, Prestamos CDFI, and SEDCO. Importantly, this classification is based solely on external documentation of failures, without any reliance on funding outcomes. We reference the documentation underlying our failure classification in Appendix Table A.1.

Most of the affected lenders processed their 2021 loans through one of two technology platforms. Womply's Fast Lane platform served Benworth Capital, DreamSpring, Fountainhead SBF, Harvest Small Business Finance, and SEDCO. The Federal Trade Commission alleged that Womply's technical failures and deficient support delayed the processing of applications and left most of them unfunded, and the SBA suspended it from working with the agency in December 2022 (Federal Trade Commission, 2024; U.S. Small Business Administration, 2022). Blueacorn, suspended at the same time, served Prestamos CDFI and Capital Plus Financial, whose borrowers alleged in class actions that SBA-approved loans were never funded.[3]

The remaining two affected lenders, Itria Ventures and Kabbage, had issues unrelated to these platforms. The FTC charged that Itria Ventures, the lending arm of Biz2Credit, delayed applications for months and blocked borrowers from withdrawing to apply elsewhere, resulting in Biz2Credit and Itria agreeing to pay $33 million in a 2024 settlement (Federal Trade Commission, 2024). Kabbage admitted to miscalculating loan amounts for tens of thousands of borrowers, and its servicing arm filed for bankruptcy in 2022 (U.S. Department of Justice, 2024; Hrushka, 2022).

Several of the affected lenders ranked among the SBA's largest PPP lenders by volume (U.S. Small Business Administration, 2021) and collectively, they extended 947,537 loans to 711,982 distinct businesses in 2021. Of the $17.6 billion in loans approved through

[3] *Marshall v. Prestamos CDFI* (E.D. Pa.) and *Greathouse v. Capital Plus Financial* (N.D. Tex.).

affected lenders, \$1.69 billion was never disbursed. There is no evidence that these failures were anticipated, as breakdowns emerged during the processing of applications or the funding of already-approved loans.

Importantly, the affected lenders largely did not supply consumer credit to their PPP borrowers, making it unlikely that failures directly affected owners' access to personal credit. Seven of the nine affected institutions—Harvest, Fountainhead, DreamSpring, Prestamos, Itria, Kabbage, and SEDCO—lent only to businesses.[4] The remaining two, Benworth and Capital Plus Financial, had small consumer real estate lending businesses in Florida and Texas, respectively, and our estimates are robust to dropping owners with PPP loans from these two institutions (Section 6).

3.2 Data

Our main dataset is the Gies Consumer and Small Business Credit Panel (GCCP), a state-of-the-art credit record panel with information on small businesses and consumers, from one of the three major credit bureaus. While research on consumer credit using credit bureau data has grown dramatically since the 2000s, this is one of the first papers to use small business credit bureau data in the United States.[5]

The GCCP is a one percent random sample of individuals with a credit report, with between 2 and 3 million consumers per year. For those who own a business, the credit bureau links the firm's credit records to the owner's, with both personal and firm records pulled annually as of the end of March of each year. However, the link between owners and firms is refreshed less often. During our 2016–2023 sample period, consumer-business links are measured in 2020 and 2023, so the approximately 400,000 business owners we observe reflect ownership as of those dates. On the consumer side, the data include the

[4] Kabbage discontinued an earlier consumer loan product in 2016.

[5] See Avery, Calem, Canner and Bostic (2003), Lee and Van der Klaauw (2010), and Gibbs, Guttman-Kenney, Lee, Nelson, Van der Klaauw and Wang (2025) for overviews of consumer credit bureau data, and Haughwout, Lee, Scally and Van der Klaauw (2021), Brennecke, Siravyan and Witzen (2021), Bellon, Cookson, Gilje and Heimer (2021), and Benetton and Buchak (2025) for other recent work incorporating small business credit variables.

detailed credit attributes and tradelines of each consumer, including credit score (from the VantageScore model) and debt levels for all major forms of formal debt, such as mortgages, student loans, and credit cards; as well as payment history, bankruptcies, and subprime borrowing history. In addition, it includes basic demographics such as zipcode of residency, age, gender, marital status, and employment status.[6] Using GCCP data since 2004, we additionally impute homeownership status following the procedure in Fonseca, Liu and Mabille (2026), classifying consumers as homeowners if they have a mortgage or have had a mortgage since 2004 and subsequently paid it down, or if they are classified as homeowners by the credit bureau.

On the small business side, the data include credit attributes of each business, including proprietary credit scores used by lenders to assess delinquency and failure risk; debt levels across different categories including term loans, credit lines, credit cards, and leases; and payment history, bankruptcies, liens, UCC filings, and other public records. In addition, it includes the zipcode of each business and information such as firm age, ownership structure, number of employees, and industry classification. These firm attributes are linked to the credit and demographic characteristics of entrepreneurs from the consumer side of the GCCP through an anonymized consumer id provided by the credit bureau. In Figure 1, we show the geographical coverage of the full GCCP (panel a) and the business-owner sample (panel b) in 2020, the year before the PPP operational failures, illustrating that the GCCP has national coverage of both consumers and business owners. In Figure 2, we compare the firm size distribution measured by the number of employees in the GCCP with Census business counts in 2020, and find that the GCCP broadly matches the overall distribution of all U.S. businesses.

Our measurement of the credit shortfall comes primarily from loan-level data from the Paycheck Protection Program. We supplement the GCCP with the SBA's public release loan-level file, which was merged with business credit records by the credit bureau. Of the

[6]The GCCP also includes data on alternative credit products—such as payday loans—from the credit bureau's alternative credit database. For more information on that dimension of the GCCP, see Fonseca (2023).

416,566 businesses in our panel, 30,836 are linked to at least one PPP loan, for a match rate of 7.4 percent. SEDCO, the smallest of the nine affected lenders, does not appear in the matched data. Since the GCCP has snapshots as of the end of March, we use exact dates in the PPP loan-level records to construct cumulative disbursement and forgiveness measures as of the same end-of-March cutoff. Note that our analysis of PPP funding shortfalls is thus restricted to loans that can be reliably linked to an owner's consumer credit record. The incidence of PPP loan fraud, which disproportionately occurred at non-banks (U.S. House Select Subcommittee on the Coronavirus Crisis, 2022; Griffin, Kruger and Mahajan, 2023), should thus be substantially lower in this sample, as most PPP fraud involved non-existent, non-operating, or newly created businesses (U.S. Government Accountability Office, 2023). Non-existent businesses would not link to a consumer or business credit record, and our results are robust to restricting to businesses in the sample before 2020 (Section 6).

Table 1 reports summary statistics in 2020 for our sample of business owners matched to a PPP loan. The average owner has a personal credit score of 749 and consumer balances of $168,380, most of which is mortgage debt. Business credit balances are smaller, with a mean of $25,400, which may partly reflect that loan-level coverage of business credit is less complete than consumer credit (Brennecke, Siravyan and Witzen, 2021).

4 Empirical Strategy

Our research design compares small business owners approved for the same PPP loan amounts at lenders with operational failures in 2021 to those at lenders without them, before and after the failures. The treatment group consists of owners whose firms were approved for a PPP loan originated or serviced by one of the eight affected lenders present in our data, as described in Section 3. The control group consists of all other owners matched to a PPP loan. Our estimand is therefore the effect of a negative credit shock during an economic crisis (the COVID-19 pandemic), as is common in the literature studying disruptions to credit supply (e.g., Ivashina and Scharfstein, 2010; Chodorow-

Reich, 2014; Deyoung et al., 2015; Doniger and Kay, 2023).

In the standard notation, our main specification is

$$Y_{it} \;=\; \alpha_i \;+\; \gamma_t \;+\; \beta\,\mathrm{Treated}_i \times \mathrm{Post}_t \;+\; X_{it} \;+\; \varepsilon_{it}, \tag{5}$$

where Y_{it} is an outcome of owner i in year t, $\mathrm{Treated}_i$ indicates owners at affected lenders, Post_t equals one in 2021 and later years, α_i and γ_t are owner and year fixed effects, respectively, and X_{it} is a vector of controls, which is described below.

The coefficient β measures the average difference in outcomes between affected and unaffected owners over the years after the failures, relative to their difference before. We have a single treatment cohort, and business owners at unaffected lenders form a never-treated control group. To trace the dynamics of the response and assess trends before the failures, we also estimate the event-study specification

$$Y_{it} \;=\; \alpha_i \;+\; \gamma_t \;+\; \sum_{k\neq -1} \beta_k\,\mathrm{Treated}_i \times \mathbb{I}_{t=2021+k} \;+\; X_{it} \;+\; \varepsilon_{it}, \tag{6}$$

where the omitted category $k = -1$ corresponds to 2020, the last year before the failures. Each β_k measures the difference between affected and control owners in year $2021 + k$ relative to 2020, so pre-period coefficients close to zero indicate that the two groups evolved in parallel between 2016 and 2020.

We estimate both specifications with the Callaway and Sant'Anna (2021) panel difference-in-differences estimator, with the same within-owner and year adjustments represented by the owner and year fixed effects in Equations (5) and (6). Relative to OLS, the Callaway-Santanna estimator uses a doubly robust adjustment for the covariates. Treatment is assigned at the PPP lender level, so we cluster standard errors at that level throughout. With eight treated clusters, conventional cluster-robust standard errors are generally unreliable (MacKinnon and Webb, 2018), and we therefore base inference on a wild cluster bootstrap.[7]

Our first set of outcomes measures the shock to business credit. The main outcome is the cumulative amount of PPP funds disbursed to owner i's firm by the end of March year t (to match the timing of the GCCP archives). We also estimate the effect on total non-PPP business credit, to check if the PPP funding shock represents a net shortfall in business credit. The second set of outcomes measures the personal borrowing response, with the main outcome being the owner's total consumer balance. We focus on total balances rather than distinguishing new originations from slower principal repayment, as both would constitute higher net borrowing. We also examine the components of personal borrowing, including mortgage balances, credit card balances, and other personal loans, to characterize the composition of the response.

We estimate the pass-through between business and personal credit with a Wald ratio,

$$\theta = -\frac{\beta^C}{\beta^B}, \tag{7}$$

where β^C is the estimate of equation (5) for total consumer borrowing and β^B is the first-stage estimate for disbursed PPP funds. The minus sign converts the first-stage change in funds received into a shortfall, so θ measures the dollars of additional personal borrowing per dollar of business credit withheld. We compute the pass-through at two horizons, in 2021 and on average over the post period. The post-period ratio uses the average effect over 2021 through 2023 in the numerator and the average over 2021 and 2022 (the duration of the program) in the denominator. We obtain standard errors for θ using the delta method and, as a robustness check, we report Anderson and Rubin (1949) confidence intervals.

The controls in X_{it} are the owner's 2020 personal credit score, the firm's 2020 business credit score, and the firm's total approved PPP amount, all interacted with year dummies.

[7] In Section 6, we show that estimates are very stable when one affected lender is left out at a time, alleviating concerns about the concentration of identifying variation.

All three are upstream of the failures, with scores measured the year before and approved amounts reflecting the funding requested and a size-based statutory cap, both determined before the failures disrupted processing.[8] Conditioning on the approved amount compares owners who were supposed to receive the same funding, but whose lenders differed in whether and when loans were funded. The credit score controls also make the two groups more comparable in terms of credit access, since credit is to a large extent underwritten on credit scores, particularly the mortgage credit that drives the personal borrowing response. As we show in Section 3, conditional on controls, owners in the treated and control groups are balanced on a wide set of characteristics relating to personal and business credit, firm size, and repayment.

Our research design has three key identifying assumptions. The first is parallel trends: absent the operational failures, outcomes of affected and unaffected owners would have evolved in parallel, conditional on the covariates. While the parallel trends assumption is untestable, we provide three types of evidence in its favor, the first of which is covariate balance. Figure 3 compares affected and control owners across several personal and business characteristics measured in their 2020 records, with each difference scaled by the standard deviation of the variable. Without controls, affected owners are smaller, more likely to be female, and have less access to credit, mirroring prior findings about which owners obtained PPP loans through non-bank lenders (e.g., Howell, Kuchler, Snitkof, Stroebel and Wong, 2024). Conditional on baseline credit scores, the two groups are balanced on a wide range of personal and business characteristics, including loan balances, repayment, firm size, and demographics. Note that approved PPP loan amounts are not statistically different between the two groups, but show wide variation. Our final specification thus controls for approved PPP amounts since this outcome is upstream of the operational failures, allowing for a precise comparison of similar business owners whose firms were supposed to receive the same levels of funding. Conditional on the final set

[8] The one lender for whom approved loan amounts are arguably downstream is Kabbage, whose failures included miscalculating loan amounts. In Section 6, we show that our estimates are quantitatively very similar and statistically indistinguishable when we exclude Kabbage.

of controls, we achieve balance on all characteristics except gender, which is marginally significant at 5%, and revolving balances. Both significant differences are economically small, at around 0.08–0.10 standard deviations.

The second piece of evidence is in the event studies in Section 5, showing that the personal and business outcomes of treated and control owners evolved in parallel in the five years preceding the failures. Finally, we also report robustness checks directly addressing potential violations of the parallel trends assumption. For instance, a key concern is that affected lenders are all non-banks and may serve borrowers who are unobservably different even conditional on controls, and so we show that our results are robust to restricting the control group to owners who obtained PPP loans from non-banks (Section 6). Another concern is that treated and control owners could be differentially exposed to the COVID-19 pandemic. Because that exposure is most likely to operate through regional and sectoral conditions, as states had different pandemic-era policies and sectors differed markedly in their exposure to these policies and to the crisis itself, Section 6 also reports results flexibly controlling for state- and sector-specific shocks.

The second assumption is that owners did not anticipate the failures. This is plausible in our setting, as the failures arose in the lenders' processing of loans after borrowing decisions had been made, and borrowers would presumably have chosen different lenders had they anticipated not receiving the funds. Moreover, the event studies in Section 5 show no evidence of anticipation.

Finally, interpreting the ratio of the personal credit response to the business credit shortfall as a pass-through requires a third assumption, namely that the failures affect personal borrowing only through withheld business credit. This exclusion restriction would be highly implausible for most credit supply shocks and highlights the advantage of our setting, which is unusually isolated from consumer credit supply. The PPP was a business credit program and affected lenders largely did not supply consumer credit (Section 3.1), so the failures and their consequences to lenders are unlikely to directly affect owners' access to personal credit.

5 Main Results

5.1 Business Credit Shortfall

We start by documenting the effect of PPP failures on the business credit received by affected firms. Figure 4 plots estimates of the event study in Equation (6) for disbursed PPP funding (panel (a)) and other, non-PPP business credit (panel (b)). PPP funding is mechanically zero before the program, so pre-2021 estimates in panel (a) should not be interpreted as a pre-trends check. In 2021, affected owners receive \$13,597 less in disbursed funds than control owners approved for the same PPP loan amounts. This is a sizable shock, exceeding the total cash reserves of the typical small business (Farrell and Wheat, 2016). The gap narrows as additional funding arrives but remains large, averaging \$7,863 per year over 2021 and 2022, the two years of the program (column 1 of Table 2).

Owners do not make up the shortfall by borrowing elsewhere through their firms. Panel (b) of Figure 4 shows that business balances of affected owners evolve in parallel with control owners from 2016 to 2023, showing no effect of the failures. On average, non-PPP business credit falls by \$277 for treated owners relative to the control, an effect both economically small and statistically insignificant (column 2 of Table 2). The shock to PPP funding thus represents a net shortfall in business credit for affected firms. This implies that the term $\Delta b/s$ in Equation (4) is close to zero (and, if anything, negative), and thus the shortfall must be absorbed through personal borrowing, lower employment, or margins outside of the model in Section 2.

5.2 Pass-Through to Personal Borrowing

Figure 5 shows event studies (Equation (6)) for owners' consumer balances, with pooled estimates reported in Table 2. Treated and control owners evolve in parallel from 2016 through 2020 (Figure 5), supporting the parallel-trends assumption and showing no evidence that owners anticipated the failures. Personal balances then rise by \$6,810 on impact in 2021 and by \$7,877 on average over the post period (column 3 of Table 2), offsetting the \$7,863 average shortfall in business credit essentially dollar-for-dollar. Interpreted through the conceptual framework in Section 2, this dollar-for-dollar pass-through to personal credit suggests that personal credit was already the marginal funding source for the average business in our sample, consistent with survey evidence that small businesses routinely use personal credit (e.g. Robb and Robinson, 2014).

The response is largely driven by mortgage and other installment credit rather than credit cards. Mortgage balances rise by \$4,080 (column 4 of Table 2) and other personal installment loans by \$1,011 (column 6), while credit card balances rise by a much smaller \$346 (column 5). As a placebo check, we show that the failures have a small and statistically insignificant effect on homeownership (Appendix Figure A.1), suggesting higher mortgage balances reflect owners tapping into equity—either through refinancing or slower amortization.

Table 3 reports the pass-through from business to personal credit, computed as the Wald ratio of the personal borrowing response to the business credit shortfall, with standard errors obtained using the delta method. Owners replaced half of each withheld dollar within the first year and essentially all of it over the post period. The pass-through is 0.50 in 2021 (column 5) and 1.00 on average (column 6), both statistically significant at the 1% level. As a robustness check, we report Anderson–Rubin confidence intervals, which also exclude zero.

Through the lens of our conceptual framework, this pass-through measures the owner's total personal borrowing response, $\theta = \Delta p/s$ in Equation (4). The amount that effectively supports the firm is $\tau\theta$, where τ is the share of additional personal borrowing that relaxes the firm's financing constraint, which we do not observe. We return to what, under some assumptions, we can infer about τ in Section 5.3 below, once we have estimates that map to all of the terms in Equation (4).

5.3 Real Effects and Heterogeneity by Homeownership Status

Next, we show evidence that personal borrowing helped owners successfully smooth the business credit shock and avoid layoffs. Panel (a) of Figure 6 shows the event study for the number of employees. Treated and control firms evolve in close parallel before 2021, again supporting the parallel trends assumption. Employment then declines by 0.16 employees in 2021–2023 for treated firms relative to the control, a small decline that is not statistically distinguishable from zero. There is thus no average effect of the negative credit shock on employment, consistent with the average owner replacing the business credit shortfall with personal credit dollar-for-dollar. Assuming businesses do not adjust on dimensions outside the framework in Section 2, such as lower investment, and taking our small and insignificant estimates for other business borrowing and employment as zeros, the decomposition in Equation (4) suggests that the additional personal borrowing is fully used by the business (i.e., $\tau = 1$).

According to the framework in Section 2, the fact that firms did not contract despite replacing cheaper, subsidized funding with personal credit also suggests that personal credit was already the marginal funding source for the average business in our sample, consistent with survey evidence that small businesses routinely use personal credit (e.g. Robb and Robinson, 2014). Recall that this result also requires personal borrowing constraints to be slack enough that the shortfall can be replaced without exhausting that funding source, so that the marginal cost of funding remains constant (Appendix B.1). We test this mechanism further by exploiting variation in personal borrowing capacity

Since the personal credit substitution runs primarily through mortgages (Table 2)—which is generally the cheapest form of personal credit—we estimate effects separately for business owners with and without home equity. Note that we do not observe home values, addresses, or property characteristics in credit bureau data, and thus cannot measure or impute home equity. Moreover, while our estimated business credit shortfalls of \$8–14k are large compared to the cash reserves of small businesses, they are small compared to typical levels of home equity. Even before the pandemic era house-price appreciation, the median home equity was $120,000 overall and $89,000 for homeowners at the bottom of the income distribution (Bhutta et al., 2020). Thus, even homeowners with relatively low incomes or who experienced relatively low levels of house price growth in the years preceding the failures would likely have sufficient personal borrowing capacity to fully smooth the business credit shock.

Therefore, to isolate business owners with no available home equity to borrow against, we estimate results separately for homeowners and renters, based on homeownership status in 2020. Table 4 reports estimates for homeowners in columns 1–3 and for renters in columns 4–6. We find a smaller increase in personal borrowing for renters (column 4 vs 1), which is not statistically significant. Similarly, the pass-through for renters is smaller and not statistically distinguishable from zero (column 6), suggesting that these business owners are not able to fully smooth the business credit shock.

Consistent with the hypothesis that the full pass-through to personal credit insulates the firm, panel (b) of Figure 6 shows a striking pattern in firm employment by 2020 homeownership status. Treated firms owned by homeowners show no decline in employment relative to the control, while firms owned by renters contract on average by one employee, a sizable 20 percent decline relative to the average number of employees of renter-owned firms in 2020. This pattern suggests that household balance sheets play a crucial role in dampening the impacts of credit shocks on small businesses.

To test whether insulating the firm comes at the cost of increased household financial distress, Table A.2 reports estimates for balances and accounts 90 days or more past due and balances and accounts in collections, for both homeowners (columns 1–4) and renters (columns 5–8). We find no significant effects for homeowners, though confidence intervals for balances past due include economically large effects (column 1). For renters, we estimate a sizable increase in past-due balances, which more than doubles relative to their 2020 mean. The credit shock thus seems to lead to distress for owners with tighter personal borrowing constraints, rather than for owners who smooth the shock with personal credit.

6 Robustness and Additional Results

6.1 Restricting the Control Group to Non-Banks

A key concern with our research design is that firms that applied for PPP funding from non-banks might be unobservably different than firms that borrowed from banks, even conditional on controls. While the balance test in Figure 3 and the flat pre-trends in Section 5 help alleviate this concern, the possibility of selection on unobservables remains. We thus re-estimate our main results restricting the control group to owners who also obtained PPP loans from non-banks. Our definition of non-banks includes fintech lenders, community development financial institutions, small business finance companies, and fintech-conduit banks (Cross River, Celtic, WebBank, and Customers Bank). All affected lenders are non-banks under this classification.

Table A.3 reports the results of this exercise, which excludes 82 percent of the sample and 91 percent of the control group. Within non-banks, the pass-through is 0.87 in 2021 (column 5) and 1.49 averaged over the post period (column 6), both statistically significant. Our baseline estimates of 0.50 (Table 3, column 5) and 1.00 (Table 3, column 6), respectively, are thus smaller than the within-non-bank estimates, but are comfortably within 95 percent confidence intervals.

6.2 Controlling for Region- and Sector-Specific Shocks

A related concern is that our estimates may capture the uneven regional and sectoral effects of the pandemic rather than the contraction in business credit. States differed in the timing and intensity of business restrictions and in their reopening policies, while sectors differed in their exposure to shutdowns and shifts in demand. We address this concern within the Callaway–Sant'Anna framework by conditioning, separately, on state and two-digit-industry indicators. These enter with period-specific coefficients in the doubly robust adjustment and thus allow the counterfactual outcome path to vary flexibly by state or industry, analogous to absorbing state-by-year or industry-by-year shocks in a fixed-effects specification. Because the treatment arises from a small set of national lender-level processing failures affecting approximately 2,000 business owners in our sample, allowing for this rich set of state-time and sector-time shocks is a demanding test of whether the estimates are driven by pandemic-era regional or industry conditions.

Table A.4 reports the results of this exercise, with Panel A absorbing state-specific shocks and Panel B absorbing industry-specific shocks. The pass-through ranges from 0.44 to 0.54 on impact (column 5) and from 0.91 to 0.95 in the post period (column 6), quantitatively similar and statistically indistinguishable from our baseline of 0.50 (Table 3, column 5) and 1.00 (Table 3, column 6), respectively.

6.3 Controlling for Approval Timing

A separate concern is that affected lenders may have attracted borrowers at different points in the program, and loans approved closer to our end-of-March cutoff would have had less time to be funded before measurement. We observe the loan approval date, not the application date, and we do not condition on approval timing in the baseline specification because delayed approval could itself be a manifestation of the operational failures. If the failures delayed approval and disbursement, our baseline estimates correctly capture the concurrent personal borrowing response to that shortfall.

If, instead, these borrowers were approved at different points in time for reasons unrelated to the failures, controlling for application date would be appropriate and would isolate the confounding timing effect. Table A.5 reports estimates of Equation (5) additionally controlling for PPP approval timing, measured as the number of days between the start of the program and the firm's approval date. The pass-through remains similar to our baseline and statistically significant, at 0.58 in 2021 (column 5) and 1.28 over the post period (column 6).

6.4 Leaving Out One Affected Lender at a Time

Our identifying variation comes from eight affected lenders, which is a small number of treated clusters. To address concerns that the identifying variation may be driven by one large cluster, Figure A.2 re-estimates our main results, leaving out one affected lender at a time. Estimates are stable, with the personal borrowing response ranging from $6,973 to $8,232 and the business credit shortfall from $6,991 to $8,698, implying that our findings are not driven by any one lender. The resulting pass-through estimates are also very stable, ranging from 0.93 to 1.17 and equaling our 1.0 baseline estimate (Table 3, column 6) in five out of eight exclusions.

6.5 Leaving Out One Shock Family at a Time

The eight affected lenders are arguably not eight independent shocks. Most processed their 2021 loans through one of two technology platforms, and so the operational failures could be categorized into four episodes instead of eight. The Womply platform served Benworth, DreamSpring, Fountainhead, and Harvest, while the Blueacorn platform served Prestamos and Capital Plus. The remaining two episodes are Biz2Credit's Itria Ventures and Kabbage, which are not directly related to either platform.

Figure A.3 re-estimates the business credit shortfall and the personal borrowing response, leaving out one shock family at a time. The estimates are again stable across all exclusions, with the personal borrowing response ranging from $7,423 to $8,191, the business credit shortfall from $6,834 to $8,698, and the implied pass-through from 0.93 to 1.19. The rightmost estimate drops Benworth and Capital Plus together, the two affected lenders with local consumer real estate lending businesses, also leaving the response largely unchanged. Our findings are thus not driven by any single shock family or by the two lenders with consumer portfolios.

6.6 Restricting to Owners in the Sample Before 2020

Another concern is that a prominent form of PPP loan fraud involved the creation of shell corporations during the pandemic for the purpose of obtaining PPP funding (e.g. U.S. House Select Subcommittee on the Coronavirus Crisis, 2022). We address this possibility by restricting the sample to owners whose firm is observed in or before 2019, dropping a total of 1,636 owners. Table A.6 shows that the pass-through is virtually unchanged, at 0.51 on impact (column 5) and 0.98 over the post period (column 6).

7 Conclusion

This paper measures how much small business owners rely on personal credit when business credit contracts. In 2021, operational failures at nine Paycheck Protection Program lenders disrupted funding from nearly a million approved loans, a shock to business credit that is isolated from consumer credit supply. Comparing owners approved for the same loan amounts at lenders with and without failures, we find that they replace the lost business credit nearly one-for-one with personal borrowing—mostly through mortgages and other personal installment loans—consistent with personal credit being the marginal funding source for the typical small business.

Firms owned by renters, who do not have a significant pass-through to personal credit, contract by one employee (or 20 percent of the 2020 mean), while firms owned by homeowners see no decline in employment. Our findings are thus consistent with owners using personal credit to successfully smooth the business credit shock and avert firm distress. This suggests that business credit data overstate the financial constraints faced by small firms, and miss an important channel through which credit shocks are dampened by household balance sheets.

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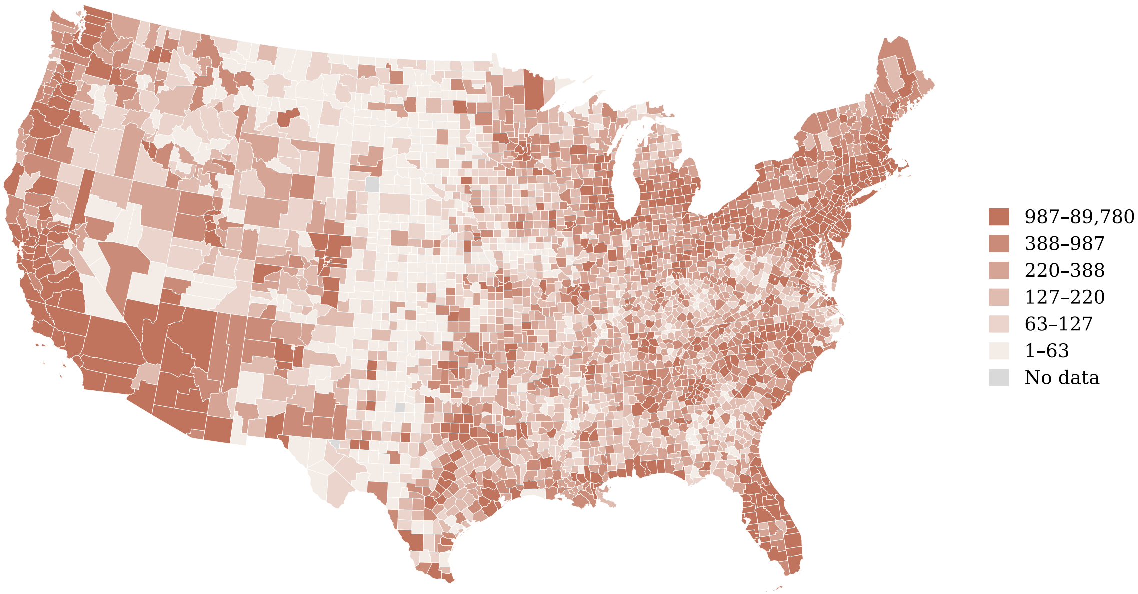
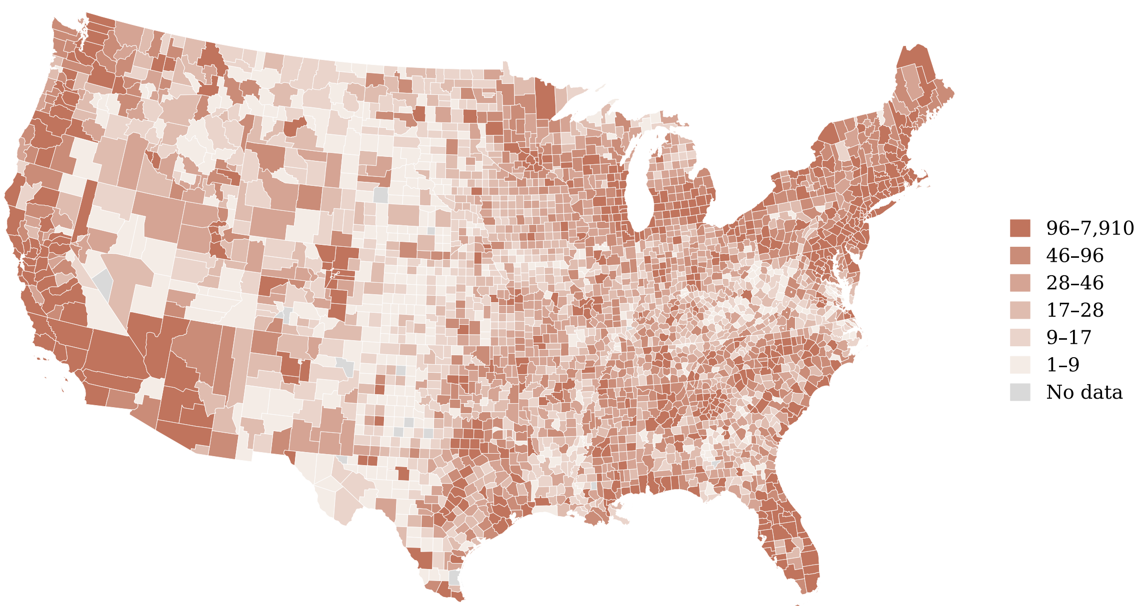
Figure 1: Geographical Coverage in 2020



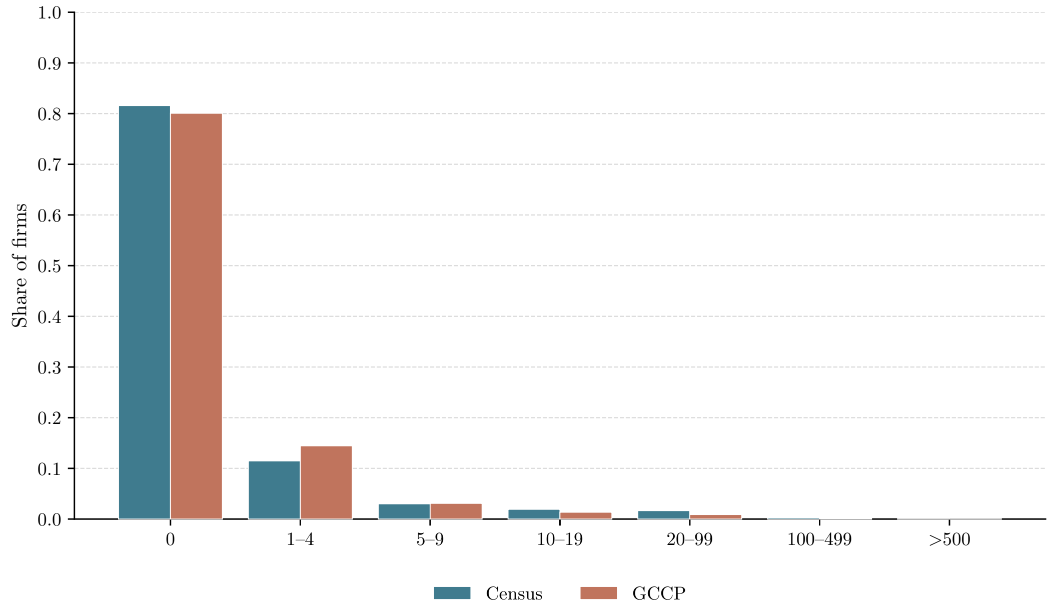
(a) All consumers



(b) Business owners

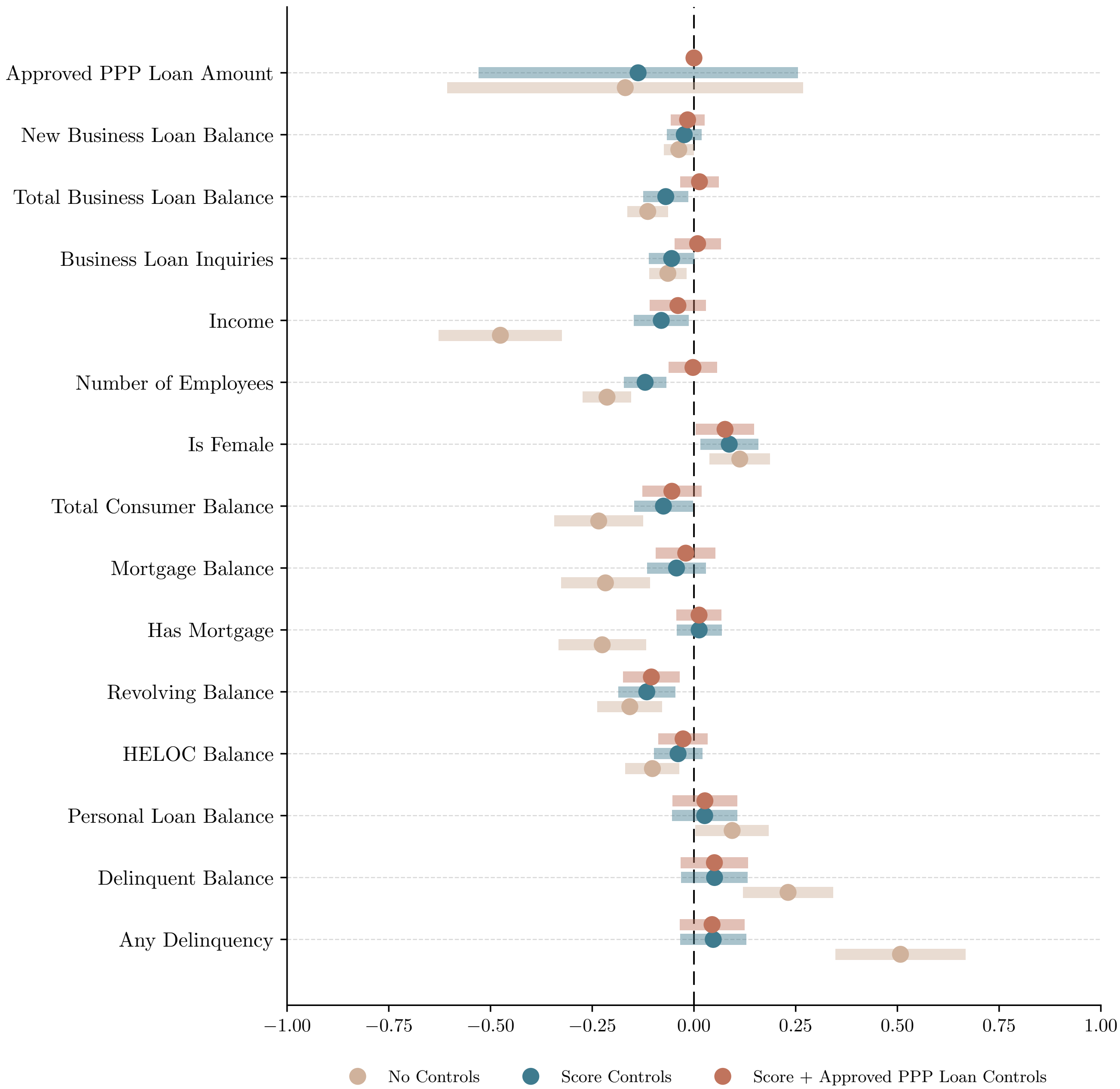
This figure shows the geographical coverage of the GCCP in 2020 by plotting the number of consumers (panel a) and business owners (panel b) in our sample by county.

Figure 2: Firm Size Distribution in 2020



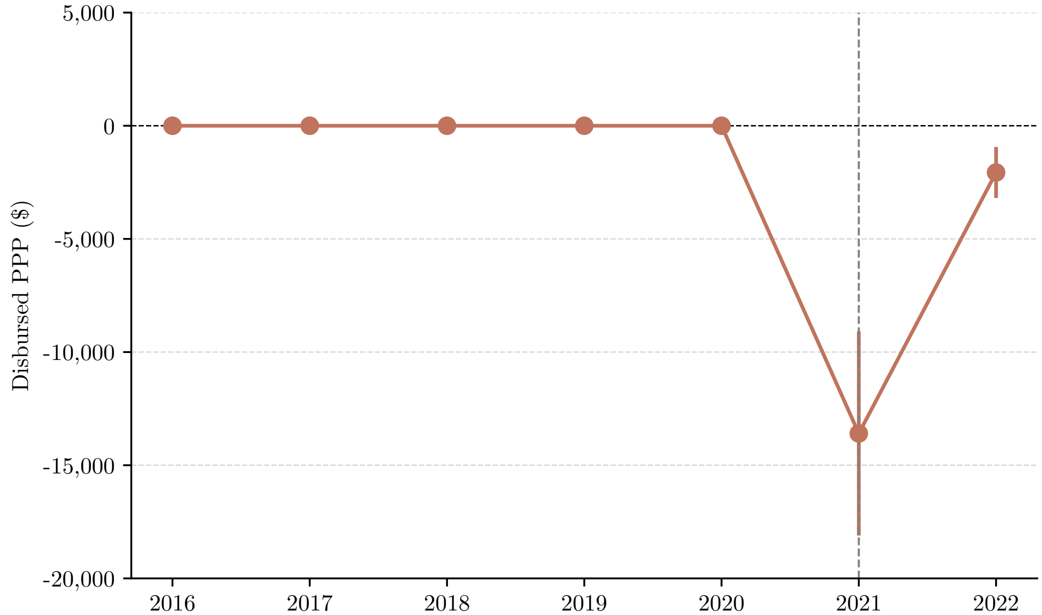
This figure shows the distribution of firms across firm size in 2020. We plot the share of firms in each firm size bin, measured by the number of employees, in the GCCP and in Census data. The Census benchmark combines the number of non-employer firms from the 2020 Non-Employer Statistics with the number of employer firms by employment size from the 2020 County Business Patterns.

Figure 3: Balance Between Affected and Control Owners in 2020

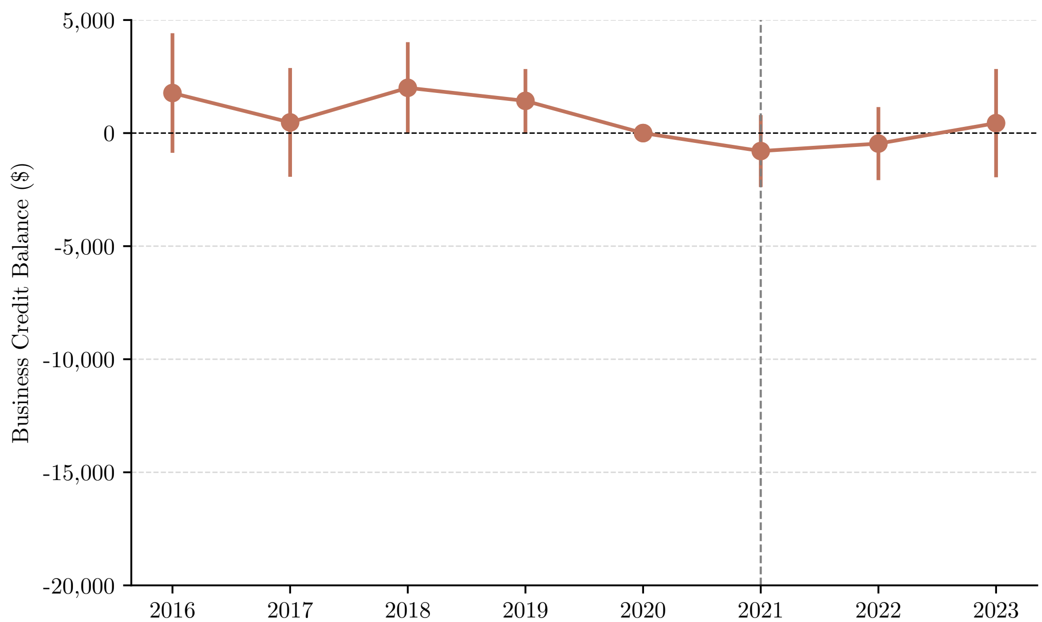


This figure shows coefficients of balancing regressions for a range of variables, denoted on the y-axis. All variables are measured in 2020, prior to the PPP operational failures, and standardized to have a mean of zero and a standard deviation of one. Each dot shows the coefficient from a regression of the y-axis variable on an affected-owner indicator. No Controls reports regression coefficients without any controls. Score Controls controls for the 2020 consumer and business credit scores. Score + Approved PPP Loan Controls additionally controls for the approved PPP amount. Bars show 95 percent confidence intervals from a wild cluster bootstrap, clustered on PPP lender.

Figure 4: Effect of PPP Failures on Business Credit



(a) Disbursed PPP Funds



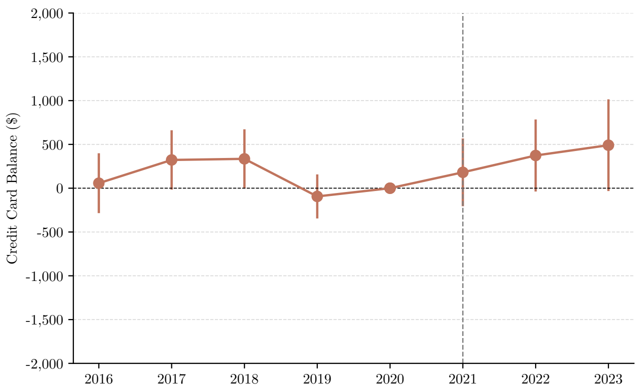
(b) Non-PPP Business Balance

This figure plots coefficient estimates from Equation (6), in dollars. Panel (a) shows estimates for the cumulative PPP funds disbursed to the owner's firm, which are mechanically zero before the program. Panel (b) shows estimates for the firm's total non-PPP business credit balance. Solid vertical lines denote the 95 percent confidence interval from a wild cluster bootstrap, clustered on PPP lender. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Year labels denote end-of-March snapshots. Disbursed PPP Funds is constructed using the same end-of-March cutoff as the GCCP.

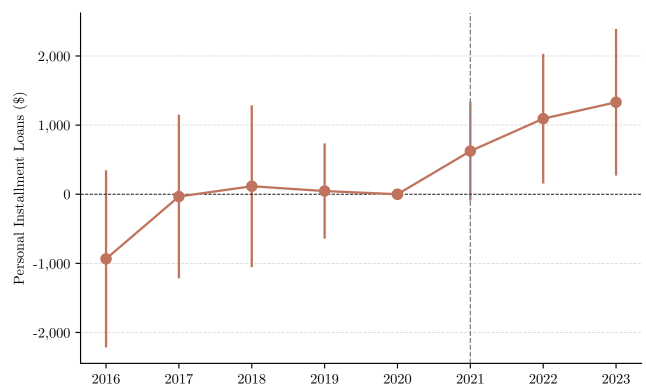
Figure 5: Personal Borrowing Response

(a) Total Consumer Balance

(b) Mortgage Balance



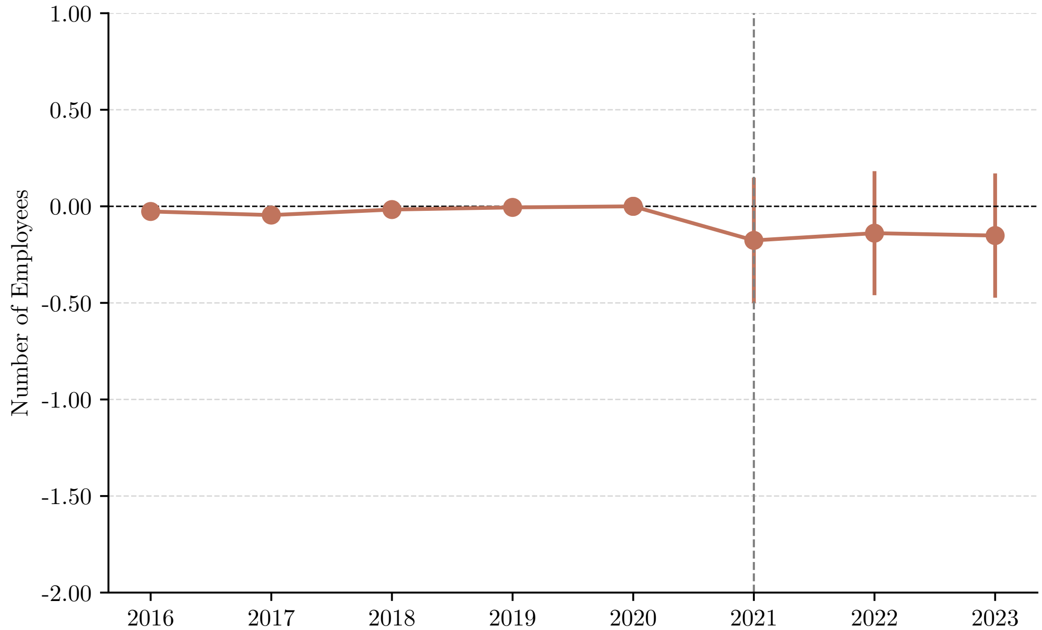
(c) Credit Card Balance



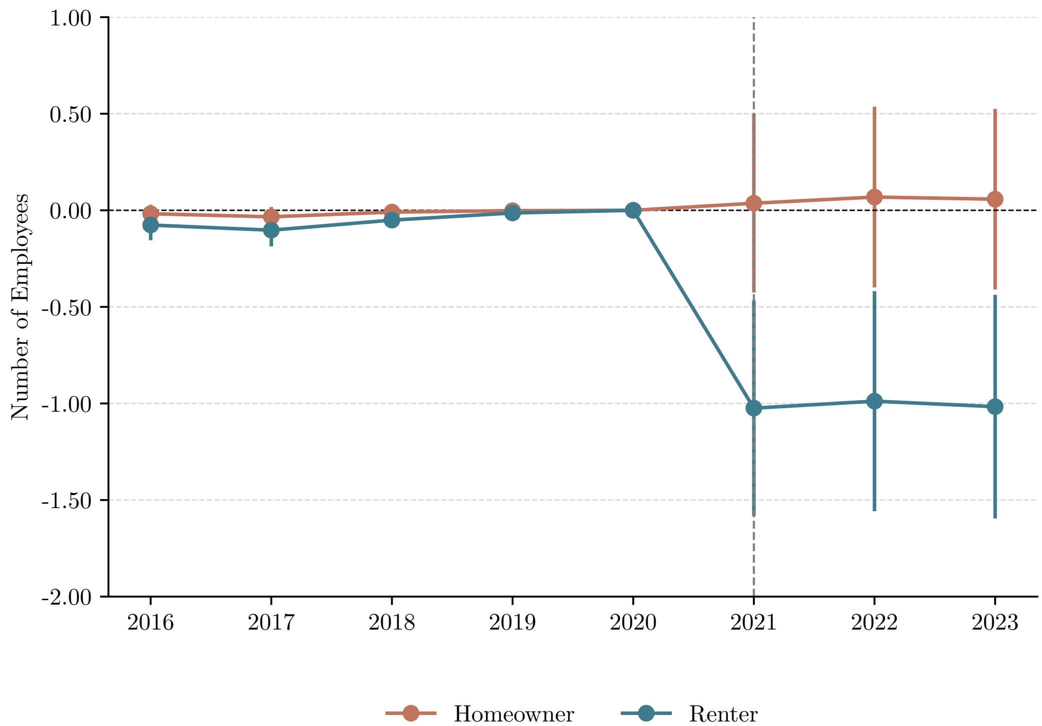
(d) Personal Installment Loans

This figure plots coefficient estimates from Equation (6). Panel (a) shows estimates for the owner's total consumer balance, panel (b) for the mortgage balance, panel (c) for the credit card balance, and panel (d) for other personal installment loans, in dollars. Solid vertical lines denote the 95 percent confidence interval from a wild cluster bootstrap, clustered on PPP lender. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Year labels denote end-of-March snapshots.

Figure 6: Real Effects: Firm Employment



(a) Full Sample



(b) By Homeownership Status

This figure plots coefficient estimates from Equation (6) for the number of employees at the owner's firm. Panel (a) shows estimates for the full sample. Panel (b) shows estimates for homeowners and renters, based on homeownership status in 2020. Solid vertical lines denote the 95 percent confidence interval from a wild cluster bootstrap, clustered on PPP lender. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Year labels denote end-of-March snapshots.

Table 1: Summary Statistics in 2020

	(1)	(2)	(3)
	Mean	St. Dev.	Median
A. Owner Demographics			
Income ($1,000)	164.29	123.67	120.00
Age (years)	56.81	13.60	58.00
Female (%)	27.71	44.76	0.00
Homeowner (%)	86.75	33.90	100.00
B. Personal Credit			
Personal Credit Score	749.36	85.13	782.00
Total Consumer Balance ($1,000)	168.38	245.78	56.12
Mortgage Balance ($1,000)	139.69	255.27	0.00
HELOC Balance ($1,000)	10.28	47.05	0.00
Credit Card Balance ($1,000)	8.51	13.17	3.10
Personal Loan Balance ($1,000)	0.46	3.43	0.00
Has Mortgage (%)	46.77	49.90	0.00
C. Business Credit and Size			
Business Credit Score	51.25	26.45	46.00
Total Business Loan Balance ($1,000)	25.40	83.87	0.20
New Business Loan Balance ($1,000)	0.34	5.74	0.00
Total Business Tradelines	3.28	4.16	2.00
New Business Tradelines	1.84	2.71	1.00
Business Loan Inquiries	0.75	2.51	0.00
Number of Employees	5.40	12.50	2.00
D. Financial Distress			
Delinquent Balance ($1,000)	0.08	1.63	0.00
Delinquent Trades	0.19	1.00	0.00
Collection Balance ($1,000)	0.24	1.50	0.00
Collection Trades	0.27	1.12	0.00
Business Delinquent Balance ($1,000)	1.77	12.67	0.00
Business Collection Balance ($1,000)	0.24	5.89	0.00
E. PPP Funding			
Approved PPP Amount ($1,000)	38.64	189.87	6.75
Disbursed PPP Amount ($1,000)	32.51	98.47	6.20
Affected by Failures (%)	9.84	29.79	0.00
Observations	18,423		

This table reports summary statistics. The unit of observation is the owner, measured in the March 2020 records. We restrict to owners with non-missing personal and business credit scores, so as to reflect the estimation sample. Dollar amounts are in thousands of dollars. Indicator variables are in percent. The approved and disbursed PPP amounts are the owner's totals across all of the owner's PPP loans for the duration of the program. Affected by Failures indicates owners whose firms borrowed from the lenders with documented operational failures.

Table 2: Effect of PPP Failures on Business Credit and Personal Credit

	Business Credit		Personal Credit			
	(1) Disbursed PPP Funds	(2) Non-PPP Business Balance	(3) Total Consumer Balance	(4) Mortgage Balance	(5) Credit Card Balance	(6) Personal Installment Loans
Treated × Post	-7,863.01***	-276.89	7,876.80***	4,080.29**	346.33*	1,010.88***
	(1,569.15)	(846.11)	(1,439.25)	(2,054.24)	(186.65)	(371.62)
Dependent Var Mean	0.00	16,814.84	116,514.53	89,621.22	6,394.92	4,379.76
Observations	111,483	123,469	121,675	121,675	121,675	121,675

This table reports coefficient estimates from Equation (5). Standard errors from a wild cluster bootstrap, clustered on PPP lender, are in parentheses. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Dependent Var Mean is the affected group's 2020 mean of the outcome. It is zero for disbursed PPP funds, whose March 2020 records predate the program. Disbursed PPP Funds is constructed using the same end-of-March cutoff as the GCCP. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Pass-Through from Business to Personal Credit

	β^C: Consumer Balance		β^B: Disbursed PPP		θ: Pass-Through	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × 2021	6,810.37***		-13,597.29***		0.50***	
	(1,519.53)		(2,507.98)		(0.14)	
					[0.25, 0.85]	
Treated × Post		7,876.80***		-7,863.01***		1.00***
		(1,439.25)		(1,569.15)		(0.27)
						[0.50, 1.30]
Dependent Var Mean	116,514.53	116,514.53	0.00	0.00	–	–
Observations	121,675	121,675	111,483	111,483	–	–

This table reports the pass-through from business to personal credit, $\theta = -\beta^C/\beta^B$ (Equation (7)), where β^C is the estimate of Equation (5) for the owner's total consumer balance and β^B is the estimate for disbursed PPP funds. Odd columns report the effect on impact in 2021, and even columns report the post-period average. For columns 1–4, wild cluster bootstrap standard errors, clustered on PPP lender, are in parentheses. For columns 5–6, delta-method standard errors are in parentheses and the Anderson–Rubin 95 percent confidence set is in brackets. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Dependent Var Mean is the affected group's 2020 mean of the β-column outcome, the total consumer balance for β^C and disbursed PPP funds for β^B. It is zero for PPP funds, whose March 2020 records predate the program, and is omitted for the pass-through columns. Disbursed PPP Funds is constructed using the same end-of-March cutoff as the GCCP. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Pass-Through to Personal Credit by Homeownership Status

	Homeowner			Renter		
	(1) Consumer Balance	(2) Disbursed PPP	(3) Pass-through	(4) Consumer Balance	(5) Disbursed PPP	(6) Pass-through
Treated × Post	7,631***	-7,962***	0.96**	5,371	-7,192***	0.75
	(2,641)	(1,592)	(0.38)	(7,150)	(1,254)	(1.00)
Dependent Var Mean	144,301.46	0.00	–	25,995.05	0.00	–
Observations	105,125	95,982	–	16,110	14,984	–

This table reports the pass-through from business to personal credit, $\theta = -\beta^C/\beta^B$ (Equation (7)), separately for homeowners and renters, based on homeownership status in 2020. Columns 1–3 report estimates for homeowners and columns 4–6 for renters. For columns 1–2 and 4–5, wild cluster bootstrap standard errors, clustered on PPP lender, are in parentheses. For columns 3 and 6, delta-method standard errors are in parentheses. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Dependent Var Mean is the affected group's 2020 mean of the β-column outcome and is zero for disbursed PPP funds, whose March 2020 records predate the program. Disbursed PPP Funds is constructed using the same end-of-March cutoff as the GCCP. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix

"How Much Do Small Businesses Rely on Personal Credit?"

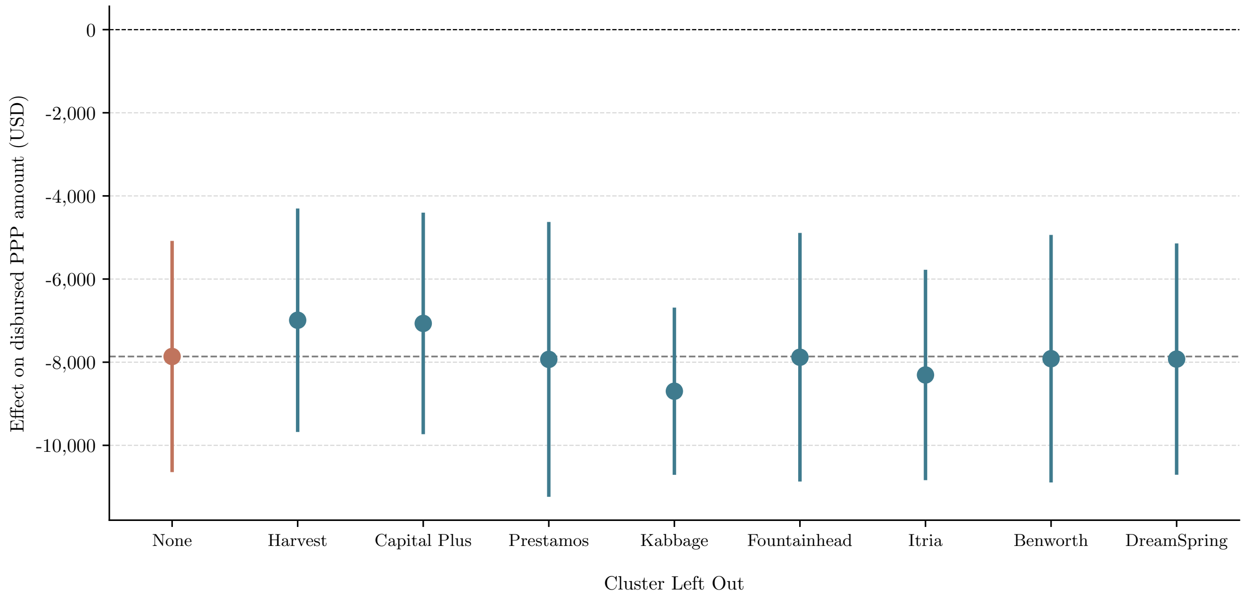
Julia Fonseca and Jialan Wang

A Additional Figures and Tables

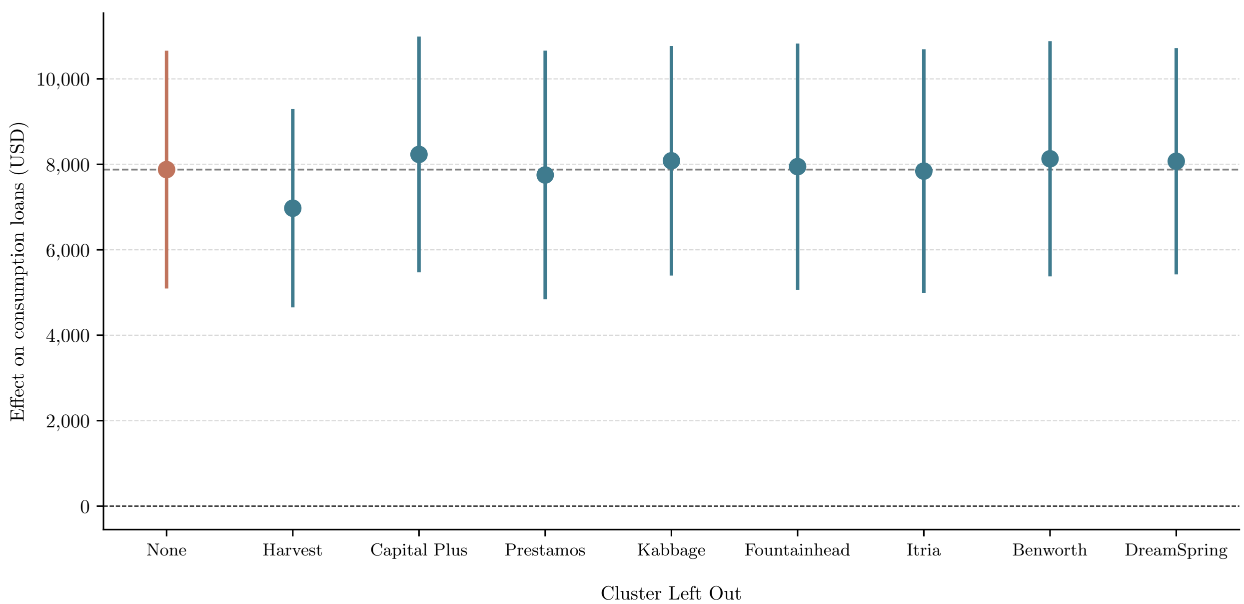
Figure A.1: Placebo Check: Effect of Failures on Homeownership

This figure plots coefficient estimates from Equation (6) with homeownership status as the outcome variable. Solid vertical lines denote the 95 percent confidence interval from a wild cluster bootstrap, clustered on PPP lender. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Year labels denote end-of-March snapshots. The average effect over the post period is 0.73 percentage points (standard error 0.76) and is not statistically significant. Homeownership does not respond to PPP disbursement failures, consistent with existing homeowners tapping into home equity rather than new home purchases.

Figure A.2: Robustness to Leaving Out Affected Lenders



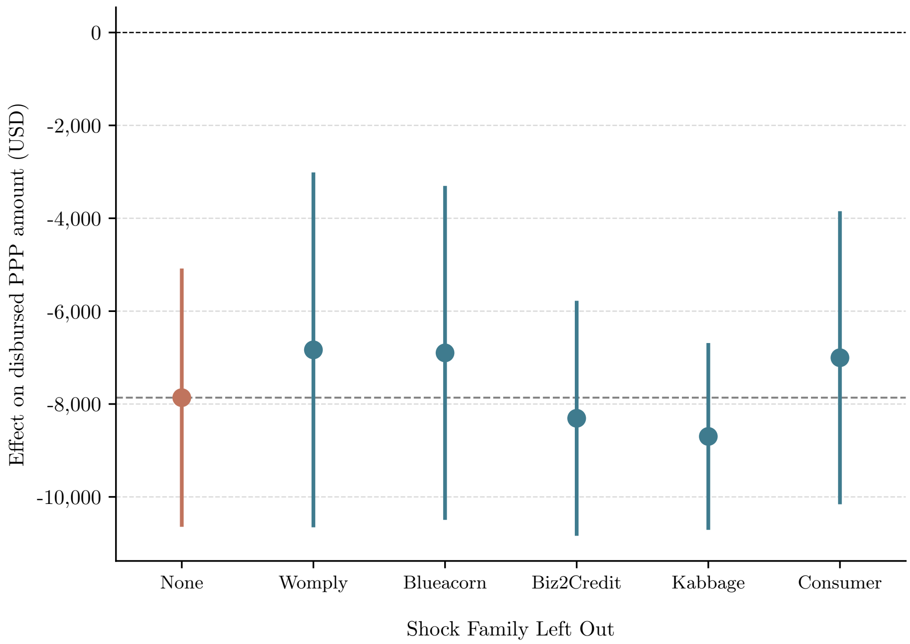
(a) Disbursed PPP Amount



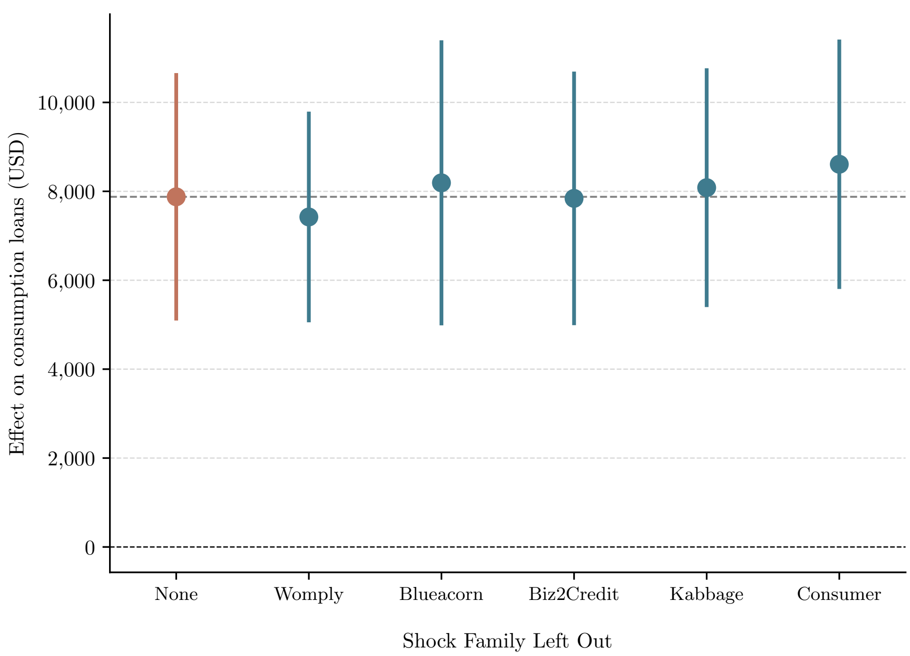
(b) Consumer Borrowing

This figure re-estimates the coefficient β from Equation (5), leaving out one affected lender at a time. Panel (a) shows estimates for the disbursed PPP amount over 2021–22 and panel (b) for consumer borrowing over 2021–23, in dollars. None is the baseline estimate with all eight clusters, also denoted by the horizontal dashed line. Solid vertical lines are 95 percent confidence intervals from a wild cluster bootstrap, clustered on PPP lender. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year.

Figure A.3: Robustness to Leaving Out Shock Families



(a) Disbursed PPP Amount



(b) Consumer Borrowing

This figure re-estimates the coefficient β from Equation (5), leaving out one shock family at a time. The eight affected lenders cluster into four operational episodes by technology platform. The Womply platform served Benworth, DreamSpring, Fountainhead, and Harvest. The Blueacorn platform served Prestamos and Capital Plus. The remaining two episodes are Biz2Credit's Itria Ventures and Kabbage. Consumer drops Benworth and Capital Plus, the two affected lenders that also operated consumer real estate lending. Panel (a) shows estimates for the disbursed PPP amount over 2021–22 and panel (b) for consumer borrowing over 2021–23, in dollars. None is the baseline estimate with all shock families, also denoted by the horizontal dashed line. Solid vertical lines are 95 percent confidence intervals from a wild cluster bootstrap, clustered on PPP lender. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year.

Table A.1: Documentation of the Operational Failures by Affected Lender

Lender	Documentation
Benworth Capital	Womply Fast Lane partnership, FTC v. Womply, SBA suspension
Capital Plus Financial	*Greathouse v. Capital Plus*, Blueacorn partnership
DreamSpring	Womply Fast Lane partnership, FTC v. Womply, SBA suspension
Fountainhead SBF	Womply Fast Lane partnership, FTC v. Womply, SBA suspension
Harvest Small Business Finance	Womply Fast Lane partnership, House report, FTC v. Womply
Itria Ventures	FTC v. Biz2Credit, FTC settlement
Kabbage	DOJ settlement, K Servicing bankruptcy, *Carr v. Kabbage*
Prestamos CDFI	*Marshall v. Prestamos*, House report, SBA suspension
SEDCO	Womply Fast Lane partnership, FTC v. Womply, SBA suspension

This table links to documentation supporting each lender's classification as affected. The underlying documentation comes primarily from federal enforcement actions, class-action litigation, and bankruptcy proceedings

Table A.2: Effect on Household Financial Distress

	Homeowner				Renter			
	(1) Delinquent Balance	(2) Delinquent Trades	(3) Collection Balance	(4) Collection Trades	(5) Delinquent Balance	(6) Delinquent Trades	(7) Collection Balance	(8) Collection Trades
Treated × Post	104.20	-0.03	36.17	-0.07	230.31**	0.08	-247.38	-0.00
	(84.23)	(0.04)	(83.68)	(0.04)	(97.87)	(0.13)	(199.49)	(0.14)
Dependent Var Mean	269.85	0.61	656.77	0.72	148.66	1.14	1,624.97	1.47
Observations	105,125	105,125	105,125	105,125	16,110	16,110	16,110	16,110

This table reports coefficient estimates from Equation (5) for measures of financial distress. The delinquent balance is the personal balance 90 days or more past due and delinquent trades is the number of personal trades 90 or more days past due in the prior 24 months. Collection balance is the balance across all accounts in collections and collection trades is the number of accounts in collections. We split business owners into homeowners (columns 1–4) and renters (columns 5–8) based on homeownership status in 2020, prior to the PPP failures. Standard errors from a wild cluster bootstrap, clustered on PPP lender, are in parentheses. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Dependent Var Mean is the affected group's 2020 mean of the outcome. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Pass-Through Within Non-Banks

	β^C: Consumer Balance		β^B: Disbursed PPP		θ: Pass-Through	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × 2021	9,491.20***		-10,907.16***		0.87**	
	(3,262.62)		(2,262.33)		(0.35)	
Treated × Post		8,919.78***		-5,977.89***		1.49***
		(2,514.62)		(1,399.14)		(0.55)
Dependent Var Mean	116,514.53	116,514.53	0.00	0.00	–	–
Observations	21,827	21,827	19,720	19,720	–	–

This table reports the pass-through from business to personal credit, $\theta = -\beta^C/\beta^B$ (Equation (7)), where β^C is the estimate of Equation (5) for the owner's total consumer balance and β^B is the estimate for disbursed PPP funds, restricting the control group to non-banks. Odd columns report the effect on impact in 2021, and even columns report the post-period average. For columns 1–4, wild cluster bootstrap standard errors, clustered on PPP lender, are in parentheses. For columns 5–6, delta-method standard errors are in parentheses and the Anderson–Rubin 95 percent confidence set is in brackets. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Dependent Var Mean is the affected group's 2020 mean. It is zero for PPP funds, whose March 2020 records predate the program, and is omitted for the pass-through columns. Disbursed PPP Funds is constructed using the same end-of-March cutoff as the GCCP. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Pass-Through Controlling for State- and Sector-Specific Shocks

Panel A: State-Specific Shocks

	β^C: Consumer Balance		β^B: Disbursed PPP		θ: Pass-Through	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × 2021	7,168.50***		-13,252.49***		0.54***	
	(1,781.40)		(2,389.89)		(0.17)	
Treated × Post		7,151.63***		-7,558.10***		0.95***
		(1,718.30)		(1,461.10)		(0.29)
Dependent Var Mean	116,508.19	116,508.19	0.00	0.00	–	–
Observations	120,660	120,660	110,515	110,515	–	–

Panel B: Sector-Specific Shocks

	β^C: Consumer Balance		β^B: Disbursed PPP		θ: Pass-Through	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × 2021	5,935.81***		-13,534.66***		0.44***	
	(1,438.94)		(2,469.88)		(0.13)	
Treated × Post		7,144.87***		-7,840.79***		0.91***
		(1,457.32)		(1,521.68)		(0.26)
Dependent Var Mean	116,514.53	116,514.53	0.00	0.00	–	–
Observations	121,675	121,675	111,483	111,483	–	–

This table reports the pass-through from business to personal credit, $\theta = -\beta^C/\beta^B$ (Equation (7)), where β^C is the estimate of Equation (5) for the owner's total consumer balance and β^B is the estimate for disbursed PPP funds, adding controls for state- and sector-specific shocks. Odd columns report the effect on impact in 2021, and even columns report the post-period average. For columns 1–4, wild cluster bootstrap standard errors, clustered on PPP lender, are in parentheses. For columns 5–6, delta-method standard errors are in parentheses. Other controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Dependent Var Mean is the affected group's 2020 mean. It is zero for PPP funds, whose March 2020 records predate the program, and is omitted for the pass-through columns. Disbursed PPP Funds is constructed using the same end-of-March cutoff as the GCCP. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Pass-Through Controlling for Approval Date

	β^C: Consumer Balance		β^B: Disbursed PPP		θ: Pass-Through	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × 2021	6,595.77***		-11,355.71***		0.58***	
	(1,852.31)		(1,984.68)		(0.19)	
Treated × Post		8,602.92***		-6,745.20***		1.28***
		(1,717.04)		(1,232.27)		(0.35)
Dependent Var Mean	116,514.53	116,514.53	0.00	0.00	–	–
Observations	121,675	121,675	111,483	111,483	–	–

This table reports the pass-through from business to personal credit, $\theta = -\beta^C/\beta^B$ (Equation (7)), where β^C is the estimate of Equation (5) for the owner's total consumer balance and β^B is the estimate for disbursed PPP funds, adding a control for approval date measured as days since the start of the program. Odd columns report the effect on impact in 2021, and even columns report the post-period average. For columns 1–4, wild cluster bootstrap standard errors, clustered on PPP lender, are in parentheses. For columns 5–6, delta-method standard errors are in parentheses. Other controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Dependent Var Mean is the affected group's 2020 mean. It is zero for PPP funds, whose March 2020 records predate the program, and is omitted for the pass-through columns. Disbursed PPP Funds is constructed using the same end-of-March cutoff as the GCCP. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Pass-Through for Owners in the Sample Before 2020

	β^C: Consumer Balance		β^B: Disbursed PPP		θ: Pass-Through	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × 2021	6,928.69***		-13,495.85***		0.51***	
	(1,688.15)		(2,294.42)		(0.15)	
Treated × Post		7,603.28***		-7,783.26***		0.98***
		(1,767.08)		(1,407.52)		(0.29)
Dependent Var Mean	117,012.98	117,012.98	0.00	0.00	–	–
Observations	116,841	116,841	108,061	108,061	–	–

This table reports the pass-through from business to personal credit, $\theta = -\beta^C/\beta^B$ (Equation (7)), where β^C is the estimate of Equation (5) for the owner's total consumer balance and β^B is the estimate for disbursed PPP funds, , for the sample of owners whose firm link is observed in or before 2019. Odd columns report the effect on impact in 2021, and even columns report the post-period average. For columns 1–4, wild cluster bootstrap standard errors, clustered on PPP lender, are in parentheses. For columns 5–6, delta-method standard errors are in parentheses. Controls include the 2020 personal and business credit scores and the approved PPP amount, each interacted with year. Dependent Var Mean is the affected group's 2020 mean. It is zero for PPP funds, whose March 2020 records predate the program, and is omitted for the pass-through columns. Disbursed PPP Funds is constructed using the same end-of-March cutoff as the GCCP. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Model Appendix

This appendix contains derivations for the model of Section 2.

B.1 The Marginal Funding Source

Deriving the piecewise linear cost of funding. For a given employment choice n, let ℓ denote the amount the owner must raise outside the cheaper source,

$$\ell = \omega n - \bar{x} + s, \tag{8}$$

where ω is the funding need per employee, $\bar{x}$ is the amount expected from the cheaper source, and s is the shortfall. The owner raises this residual amount from other business credit b and effective personal financing q,

$$\begin{aligned} \mathcal{R}(\ell) = \min_{b,q} \quad & r_B b + r_P q \\ \text{subject to} \quad & b + q \geq \ell, \\ & 0 \leq b \leq \bar{b}, \\ & 0 \leq q \leq \bar{q}, \end{aligned} \tag{9}$$

where $\mathcal{R}(\ell)$ is the minimum cost of raising ℓ, r_B and r_P are the unit costs of other business credit and effective personal financing, and $\bar{b}$ and $\bar{q}$ are their borrowing limits. If $\ell < 0$, no residual financing is needed. If $0 \leq \ell \leq \bar{b} + \bar{q}$ and effective personal financing is cheaper, $r_P < r_B$, the owner uses q first. The residual financing cost is

$$\mathcal{R}(\ell) = \begin{cases} r_P \ell, & 0 \leq \ell \leq \bar{q}, \\ r_P \bar{q} + r_B(\ell - \bar{q}), & \bar{q} < \ell \leq \bar{q} + \bar{b}. \end{cases} \tag{10}$$

If other business credit is cheaper, $r_B < r_P$, the owner uses b first. The residual financing cost is

$$\mathcal{R}(\ell) = \begin{cases} r_B \ell, & 0 \leq \ell \leq \bar{b}, \\ r_B \bar{b} + r_P(\ell - \bar{b}), & \bar{b} < \ell \leq \bar{b} + \bar{q}. \end{cases} \tag{11}$$

In the knife-edge case in which $r_B = r_P$, the owner is indifferent between the two sources until one constraint binds.

Equations (10) and (11) show that the residual financing cost schedule is piecewise linear. Each segment corresponds to one active funding source. The length of the segment is the amount available from that source, and the slope is its unit cost.

Deriving the marginal funding result. Let r_x denote the unit cost of the cheaper credit. Since the owner uses the cheaper source up to its available amount, employment solves

$$W(s) = \max_n \left\{ \Pi(n) - r_x(\bar{x} - s) - \mathcal{R}(\omega n - \bar{x} + s) \right\}. \tag{12}$$

At any point where $\mathcal{R}$ is differentiable, employment satisfies

$$\Pi'(n) = \omega \mathcal{R}'(\ell). \tag{13}$$

The right-hand side is the funding need per employee multiplied by the unit cost of the marginal residual funding source.

Now compare the no-shock allocation, indexed by 0, to the allocation after a shock of size s. Suppose effective personal financing is the active residual source in both allocations and remains below its limit. Then $\mathcal{R}'(\ell_0) = \mathcal{R}'(\ell_s) = r_P$, so Equation (13) implies

$$\Pi'(n_0) = \omega r_P = \Pi'(n_s). \tag{14}$$

Since Π is strictly concave, $n_s = n_0$. The financing constraint then gives

$$q_s - q_0 = s, \qquad b_s = b_0. \tag{15}$$

The shock is absorbed by effective personal financing, and employment does not change.

The same logic applies when other business credit is the active residual source in both allocations and remains below its limit. In that case, $\mathcal{R}'(\ell_0) = \mathcal{R}'(\ell_s) = r_B$, so $n_s = n_0$, and

$$b_s - b_0 = s, \qquad q_s = q_0. \tag{16}$$

The shock is absorbed by other business credit, and employment again does not change.

The business credit shock thus reduces employment only if it changes the marginal residual funding source or exhausts remaining residual borrowing capacity. If the same residual source remains active, the unit cost that determines employment is unchanged. The owner replaces the missing business credit by borrowing more from that source. If the shock moves the owner to a higher-cost segment, then $\mathcal{R}'(\ell_s) > \mathcal{R}'(\ell_0)$, so Equation (13) implies $n_s < n_0$. If neither residual source can expand between the two allocations, so $b_s = b_0$ and $q_s = q_0$, the binding financing constraint gives

$$\omega(n_0 - n_s) = s. \tag{17}$$

The shortfall is then absorbed by reducing the firm's funding need through lower employment.

B.2 Extension: Endogenous Personal Credit Terms

In the benchmark model, the shortfall s does not change the terms of residual financing. In practice, a business-credit shortfall may worsen the owner's risk profile and tighten personal credit. To capture this possibility, let the cost and limit of effective personal financing depend on the shortfall,

$$r_P(s) = r_P + \kappa s, \qquad \bar{q}(s) = \bar{q} - \chi s, \qquad \kappa \geq 0, \quad \chi \geq 0. \tag{18}$$

We interpret the declining-limit case locally, for shocks such that $\bar{q}(s) > 0$. These changes matter only when effective personal financing is part of the relevant residual funding frontier. A higher $r_P(s)$ matters when q is active or when it changes the ordering of financing sources. A lower $\bar{q}(s)$ matters when the personal limit binds or becomes binding.

Suppose effective personal financing is active and its limit is slack both before and after the shock. Employment satisfies

$$\Pi'(n_s) = \omega r_P(s), \qquad \Pi'(n_0) = \omega r_P(0). \tag{19}$$

If $\kappa > 0$, then $r_P(s) > r_P(0)$, so strict concavity of Π implies $n_s < n_0$. The binding financing constraint gives

$$q_s - q_0 = s - \omega(n_0 - n_s). \tag{20}$$

A deterioration in personal credit terms therefore shifts part of the response from personal financing toward lower employment. If the employment decline is large enough that $\omega(n_0 - n_s) > s$, effective personal financing falls with the business credit shock, flipping the sign of the pass-through.

A decline in the personal financing limit matters when the limit binds and other sources do not replace the lost capacity. Suppose $q_0 = \bar{q}$, $q_s = \bar{q}(s)$, and other business borrowing is fixed locally. The financing constraint gives

$$\omega n_0 = \bar{x} + b + \bar{q}, \qquad \omega n_s = \bar{x} - s + b + \bar{q}(s). \tag{21}$$

Using Equation (18),

$$\omega(n_0 - n_s) = (1 + \chi)s. \tag{22}$$

The decline in personal financing capacity thus adds to the original shortfall. If other business credit remains available at a cost that supports the existing scale, the owner can substitute toward that source and employment need not fall.