Regulation Meets Technology: Evolution of Small Business Lending in Underserved Areas since 2007*

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Abstract

This paper studies how lending to small businesses has evolved since just before the global financial crisis (GFC), paying special attention to underserved borrowers and to nonbank lenders, whose technological advantages are amplified by the enhanced regulation of the largest banks in the aftermath of the GFC. Using millions of manually classified Uniform Commercial Code loan records, this study documents that new lenders, led by fintechs in terms of growth rate, have gained substantial market share in small business lending since 2007 at the expense of banks, especially the largest banks. On the other hand, the largest banks did not particularly curtail credit to small businesses in localities with a high share of low-income and minority households. Finally, by linking records of Paycheck Protection Program loans and small business borrowers' pre-pandemic loans, this study shows that the design of public programs to support small businesses needs to take into account the changing landscape of small business lending.

JEL classification: TBD.

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1 Introduction

The health and growth of small businesses (often referred to as Small and Medium-sized Enterprises, SMEs) is important to the macroeconomy, as they constitute well over 90 percent of employer firms, and account for the majority of employment.[1] Moreover, entrepreneurship as embodied in many small businesses forms the cornerstone of the famed dynamism of the US economy. At the same time, it is well recognized that small businesses generally hold limited liquidity as they face more expensive and often fewer options for credit.[2] This recognition underlies the array of government institutions that promote credit provision to small businesses. It is also a major motivation for the Paycheck Protection Program (PPP) to provide essentially free credit to those firms during the acute phase of the COVID-19 pandemic. Furthermore, it is widely acknowledged that low-income or minority entrepreneurs face even greater barriers to accessing credit.[3] The Community Reinvestment Act (CRA) has formed the center piece of the policy effort to encourage relatively large banks to make loans in those low-to-moderate-income (LMI) localities. The CRA covers only bank lending, reflecting banks' vital role in small business credit supply traditionally.[4] However, there is mounting evidence that nonbank financial institutions (NBFIs, or "nonbanks" for short) have been gaining market share in lending to small firms, especially since the 2007 global financial crisis (GFC, or "the financial crisis" for short).[5] This study seeks to further document the changing landscape of small business lending, using millions of loan records from the Uniform Commercial Code (UCC) filings. Loans underlying UCC filings are all collateralized with some form of business assets, ranging widely from various types of equipment and other tangible assets to monetary claims (such as accounts receivable and expected proceeds) and other intangibles (such as chattel paper). Loans made by private lenders to small businesses are overwhelmingly collateralized, and UCC loans constitute the majority of small-business lending (Gopal and Schnabl, 2022).

We manually classify UCC lenders, distinguishing banks from nonbanks. Within banks, we further single out those large banks subject to the Federal Reserve's stress testing, and analyze them separately from the other banks. Within nonbanks, we distinguish the more established lenders, chiefly finance companies, from the newer entrants, represented by the so-called fintech lenders that use digital technology to automate prospective borrower screening and loan underwriting. With purported superior capability to collect and utilize unconventional (such as social media) data,

[1] According to data from the Census Bureau's Statistics of US Businesses 2021, and the Quarterly Workforce Indicators as of 2023:Q4, for example.

[2] See, for example, the Small Business Administration's 2013 report on small business lending.

[3] These barriers extend to lending under the Community Reinvestment Act (Casey et al., 2023) and even under the PPP, as documented by (Howell et al., 2024; Fairlie and Fossen, 2021; Barkley and Schweitzer, 2022; Atkins et al., 2022; Chernenko and Scharfstein, 2024).

[4] Rupasingha and Wang (2017); Ding et al. (2018); Bhutta (2011).

[5] See, for example, Jagtiani and Lemieux (2016) and Davydiuk et al. (2024).

these "digital-first" lenders likely possess comparative advantage in evaluating borrowers with less or no credit history, offering the potential to expand credit access to underserved populations.[6] By the same logic, "new" lenders should in principle also have a cost advantage over more traditional lenders because of their lower marginal cost of processing each additional loan, just as banks with strong information technology capabilities also have an advantage (Kwan et al., 2021). We document that banks, finance companies and fintechs indeed exhibit different concentration by industry and collateral type, in ways broadly consistent with the understanding of their respective comparative advantage.

Our descriptive analysis documents broad trends in small-business lending since the financial crisis. In particular, between 2007 and 2019 (just before the pandemic),independent finance companies and broadly defined fintech lenders grew steadily, gaining market share nationwide, and disproportionately in underserved areas.[7] In contrast, the average loan growth over 2007–2019 was slightly negative for banks and captive finance companies. Because of these lenders' high market shares in 2007, their contraction offset much of the growth by the other lenders that total small business lending only grew modestly over this post-financial-crisis period.

A natural question is what forces drove such uneven growth. One apparent candidate is bank regulation.[8] In particular, stress testing formed a key component of the additional and strengthened regulatory measures introduced for the largest banks. Previous studies have shown that capital-constrained banks generally curtailed lending in response to the more stringent regulatory scrutiny and enhanced capital requirements in the aftermath of the financial crisis.[9] We thus explore whether the shock of needing to shore up capital after each stress test prompted banks to cut back on making collateralized UCC loans to small firms. Our high-frequency (i.e., annual) analysis complements the previous studies, which examine either small business lending before versus after the financial crisis, or only those banks covered by the CRA filing requirement.[10] Moreover, we pay special attention to the resulting evolution of sources of UCC loans to historically underserved localities, specifically

[6]See, e.g.,Schweitzer and Barkley (2017), Beaumont et al. (2022), and Cornelli et al. (2024), and with specific reference to minority outcomes, Fei and Yang (2021), Atkins et al. (2022), and Erel and Liebersohn (2022).

[7]One noteworthy development is that loans by merchant cash advance (MCA) lenders, a type of new lender classified as fintech by (Gopal and Schnabl, 2022), grew more than ten-fold nationwide over 2007–2019, and almost twenty-fold (equivalent to an average annual growth rate of 25 percent) in counties with very high minority shares.

[8]Another potential explanation is that fintech loans are to an extent different from bank and captive finance company loans. See, e.g., Barkley and Schweitzer (2020).

[9]See, for example, Cortés et al. (2020), Doerr (2021), Gopal and Schnabl (2022), Konietschke et al. (2022), and Berrospide and Edge (2024). For evidence on capital constraints other than stress tests, see Irani et al. (2021), Favara et al. (2021), Chernenko et al. (2022), and Berrospide et al. (2024). Several of these studies also find that small businesses were able to substitute toward unconstrained banks or nonbanks, so that total credit supplied to small businesses did not necessarily fall.

[10]Cortés et al. (2020) analyzes the impact on CRA lending of each stress test, while Gopal and Schnabl (2022) compare UCC lending by stress-tested banks versus other banks. Our measure is most similar to that used by Berrospide and Edge (2024), who focus on larger commercial loans captured by the FR Y-14 disclosures.

those counties with high shares of low-income or minority households, after the financial crisis. In particular, we estimate whether stress-tested banks reduced lending more or less to those low-income or high-minority counties when compared with the other counties. This is an empirical question as the sign of this differential can be ambiguous a priori. On the one hand, these banks could disproportionately cut lending to businesses in underserved areas because it is less profitable. On the other hand, the need to meet CRA requirements may create an incentive for banks to disproportionately preserve lending to such communities.

We find that stress testing shocks have a measurable effect on lending at the county level, similar to findings by previous studies. Our measure of the capital shock induced by each stress test is the unpredictable component, equal to the difference between the current year stress-test capital buffer and the buffer experienced by that lender in the average year. Our estimates indicate that an additional one-percentage point decline in capital projected under the severely adverse scenario predicts a 0.9 percentage point lower growth in small-business lending over the following year. Our estimates show no differential effect for underserved counties. This finding suggests that stress-tested banks seem to simply contract lending across the board.

Having established that stress-testing shocks affected lending by the tested banks, we use the resulting retreat of these lenders as an exogenous supply shock to each local lending market to assess to what extent nonbank lenders (especially the new lenders) stepped in to fill the gap. For each local market, the supply shock equals the weighted average of the capital shocks received by stress-test banks operating in that market. The weights are proportional to the number of loans made by the bank in the previous year in that market. Thus, localities with similar economic conditions or in geographic proximity can receive notably different shocks in any given year. Our instrumental-variable estimates show that other lenders expanded to pick up the slack in loan supply left by the stress-test-induced shocks. However, we again find no differential effect for underserved areas, that is, nonbank lenders as a whole appear to have expanded proportionally across local markets.

Our analysis of the post-financial-crisis reconfiguration of small business lending uses data through 2019. The COVID-19 pandemic was a special period in which credit markets were disrupted—directly, but also indirectly via the government's pandemic responses, especially through credit assistance programs (Hackney, 2023). We thus separately analyze UCC loans from 2020 and 2021 (the last full year of available data). One salient feature of pandemic-era secured small-business lending is substantial heterogeneity across lender groups. Lending by banks, especially stress-test banks, decreased materially. MCA lenders also cut lending by about half, while fintech loans declined by 12 percent (a phenomenon documented by Ben-David et al., 2021). On the other hand, loans by independent and captive finance companies expanded by 26 and 6 percent, respectively, over the two years. One plausible explanation is that the pandemic-induced operational disruptions to many small businesses meant that they did not have enough operation-related assets to secure loans from lenders (such as banks) that rely more on such claims. Moreover, the liquidity small businesses typically source from banks was replaced to a large extent by government subsidized credit such as through the PPP. At the same time, the surplus public liquidity may have afforded some SMEs the down payment needed to purchase equipment, which is more commonly funded by finance companies. Lending by nonprofits also expanded substantially (15 percent).

Interestingly, we also find that nonbank lenders continued to fill supply gaps during the pandemic years, although the pattern of substitution became more nuanced. Specifically, capital shocks stemming from stress tests still deterred lending by the subject banks during the pandemic. The resulting shortfall in credit supply to small business was met foremost by government credit support, primarily through SBA loans, but also filled by MCA and fintech lenders, albeit to a much lesser extent. In other words, even though these new lenders curtailed their lending overall, they selectively cut less in areas hit the hardest by the retreat of large banks.

We next examine how the increased importance of nonbanks in small business lending before the pandemic affected the implementation of the key pandemic program offering credit support to small businesses. During Phase 1 of the PPP between April 3 to 16, 2020, only existing Small Business Administration (SBA) Program 7(a) lenders, primarily depository institutions such as banks, were authorized to make loans. Given the shift toward nonbanks as credit sources, we expect that small businesses that had weaker relationships with banks were at a disadvantage in accessing PPP credit early on.

Using data that link UCC borrower firms by name with PPP borrowers, our estimates show that borrowers having prior relationships with banks, especially those not subject to stress testing, received loans nearly three days earlier on average relative to those PPP borrowers situated in the same industry, county, size, and age class but without prior loans with any UCC lenders. By comparison, UCC borrowers from finance companies and fintechs received PPP loans half to one day *later*, respectively.[11] These timing differentials are nontrivial, considering that over 90 percent of the 2020 PPP funds were disbursed over the first 20 days of its operation (that is, between April 3 and May 3 with a 10-day hiatus due to funding depletion). They are also economically important: Doniger and Kay (2023) show that PPP delays hindered unemployment recovery. The SBA allowed more nonbanks to make loans during Phases 2 and 3 of the program starting April 27, 2020, nonetheless finance companies generally did not participate. Taken together, our results suggest that the design of public programs aiming to support credit provision to SMEs should take into account the substantial changes in the SMEs credit landscape after the financial crisis and expand the scope of covered lenders beyond banks.

[11] The more favorable experience of PPP applicants with nonbank borrowing relationships relative to those with no relationships is consistent with the finding by Erel and Liebersohn (2022) that fintechs helped with PPP access.

The remainder of this paper is organized as follows. Section 2 discusses the data construction. Section 3 documents the post-financial crisis reconfiguration of small business lending, and provides causal evidence for the impact of stress-test capital shocks. Section 4 analyzes lending during the pandemic, including the role of lending relationships in SMEs' chance of rapid access to PPP funding. Section 5 concludes.

2 Data Construction

2.1 Uniform Commercial Code (UCC) Filing Data

The main source of our data are Uniform Commercial Code (UCC) filings. For secured business loans other than real-estate loans, lenders can (and almost always do) file a UCC record to establish a priority claim over specific collateral.[12] UCC records are maintained in state registries, which can then be accessed by prospective counterparties in subsequent secured transactions. Both filing and searching is inexpensive.[13] The value of a priority claim over collateral when the borrower is in distress combined with the negligible transaction costs gives lenders a strong incentive to file, and to do so in a timely manner. As a result, UCC records achieve virtually universal coverage of secured business loans other than real-estate loans. Additional background information on UCC filings can be found in Gopal and Schnabl (2022).

The UCC data also have certain limitations. First, UCC records do not contain the loan amount. We thus have to measure lending activity based on the number of loans, and conduct our analysis under the assumption that total lending is sufficiently correlated with the number of loans. This seems to be a reasonable assumption, at least for the large fraction of UCC loans that are collateralized by (and thus presumably used to purchase) one or a few pieces of industrial equipment, such as tractors or machinery. Gopal and Schnabl (2022) also show formal evidence that using loan counts is a good approximation for loan volume.

Second, UCC records cover both loans and leases, but they are not distinguished in our data. While the distinction between loans and leases seems clear in principle, the boundary can be blurred in practice. In a capital lease, ownership of the asset is transferred to the lessee, so the lessors' position is comparable to that of secured lenders. In an operating lease, ownership remains with the lessor, but it is not uncommon for bankruptcy courts to recast operating leases as capital

[12] The UCC is a set of laws that govern commercial transactions in the United States. "Uniform" refers to the Code's goal to harmonize the legal treatment of sales and other commercial transactions across U.S. states and territories. These lending records are filed under Article 9, which governs secured transactions.

[13] For instance, current fees in the New York State registry are $20 to e-file and $25 to search against one debtor.

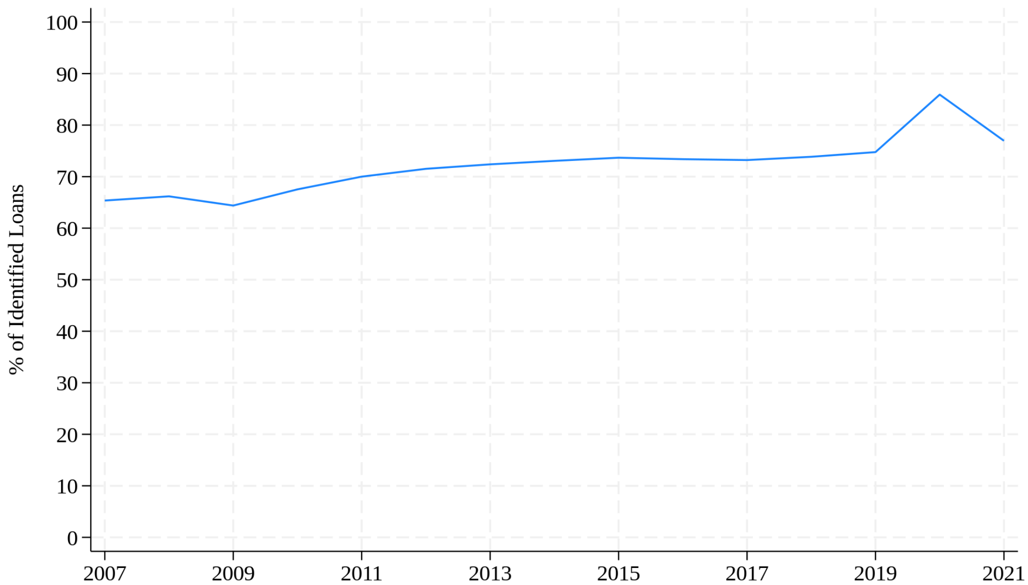

Figure 1: Share of loans by identified & classified lenders

leases. Thus, operating lessors also have an incentive to, and do, file UCC records. Nevertheless, for brevity, we refer to the contract underlying our UCC data as "loans" throughout the paper.

Our data are sourced from the same commercial vendor used by Gopal and Schnabl (2022), and span 2007 to 2021. The raw data contain roughly 41,272,021 filings, covering initial filings that accompany loan originations (commonly known as "UCC 1") and subsequent filings that detail amendments, extensions, terminations, and lien reassignments. We use only the 17,046,745 initial filings for our analysis; all the statistics reported in the remainder of this study pertain only to these original filings unless noted otherwise. The data cleaning procedure is detailed in Section A.1 of the Internet Appendix.

2.2 Identifying Lenders

We identify lenders in the UCC database using a combination of techniques. We aim to cover all lenders with at least 500 loans in the 2007–2021 period. The lenders we have identified account for about 75% of all original filings in the UCC database (see Figure 1). The remaining loans are made by tiny commercial lenders, but also individuals and nonfinancial firms (e.g., as part of trade credit). Our analyses thus cover a large majority of all loans, and all noteworthy lenders. Moreover, in all our analyses of changes in lending by a type of lender, we consider only those loans of which the filer was the secured party, not acting as an agent in some capacity (such as an administrative or collateral agent, or a trustee) for the actual secured party, who is not named in the filing.

Banks We identify banks primarily through fuzzy name matching between UCC lenders and entities covered in the National Information Center (NIC) database, which "provides comprehensive financial and structure information on banks and other institutions for which the Federal Reserve has a supervisory, regulatory, or research interest".[14] NIC data on an entity's charter and type then enable us to establish which lenders are banks or bank holding companies (BHCs), which are collectively referred to as "banks".

Captive and independent finance companies We manually research lenders that made 1,000 or more loans over the 2007–2021 period and whose name does not contain "bank". We identify 877 such lenders and classify most of them as finance companies, defined broadly as for-profit nonbank lenders.

We distinguish among three mutually exclusive categories of finance companies: captive, bank-related, and independent. Captive lenders are defined as those owned by a non-financial corporation, commonly a manufacturer, which provides loans to help buyers of its product to finance the purchases. The most prominent examples are equipment manufacturers such as John Deere and Caterpillar, as well as automakers, all of which provide loans through their financing subsidiaries. Bank-related are simply finance company subsidiaries of BHCs. Some of these are identified through name matching with the NIC data, while others are manually researched. Finally, we classified the remainder as independent, including finance companies owned by an NBFI parent. [15]

Merchant Cash Advance companies Merchant Cash Advance lenders (MCAs for short) advance funds in exchange for a fraction of future sales. For instance, a business could receive $100 today in exchange of paying 2 percent out of all sales until the total repayment amounts $120. This contract is technically not a loan, as it does not specify a well-defined time window for repayment nor an interest rate.[16] Note that Gopal and Schnabl (2022) group MCAs with "fintechs," while we mostly treat MCAs as a separate type of nonbank lenders. We identify MCAs both directly through name searches and using the methodology of Gopal and Schnabl (2022), who focus on lenders that file through filing service providers such as Corporation Service Company's (CSC) Registered Agent Services.

[14] For more information on NIC, see `https://www.ffiec.gov/npw/Home/About`.

[15] In our manual classification, we encountered several borderline cases that we classified as independent finance companies: consulting and similar service firms that cross-sell financing along with other financial services (such as FleetOne Factoring, USI Financial Services, Sterling Resources, and Insight Investments); money transmitters that also lend (e.g., Global Express Money Orders); captive lending shops that expanded to finance third-party equipment (e.g., Sageland).

[16] The most common repayment period is 6 to 18 months, and payments are usually made weekly or daily, and often automatically.

Fintechs To date, there is no consensus on the definition of a "fintech" company. This term is generally used to refer to lenders that rely predominantly or even entirely on digital technology to screen prospective borrowers and underwrite loans. Because of the term's positive association with technological advancement, self-reported "fintech" status is not reliable. We thus curate our list of fintech lenders primarily from a number of reputable neutral third parties, and then manually identify these lenders in the UCC dataset.[17] We also include LEAF Capital, which is mentioned in Gopal and Schnabl (2022) but does not appear in any of the above lists. Finally, our manual inspection of the data turned up a small number of additional lenders, such as Timepayment and FundingMetrics, which we classified as fintechs upon further investigation.[18]

Other lenders Our manual research also unearthed additional lender types. One is what we refer to as nonprofits lenders, which include credit unions, charity lenders (e.g., microlenders), and equipment financing entities set up by industry trade associations. The other category is government lenders, composed of mainly the Small Business Administration, the Department of Agriculture (USDA) and the Farm Credit System (FCS).[19]

Finally, our largely manual classification may be thorough, but it does not systematically account for changes in lender category due to mergers and acquisitions, spinoffs, sales, and other corporate events. By far the most common case is a finance company or fintech being acquired by a bank. However, transitions of every type exist in the data.[20] We handle these cases by classifying a lender by the category to which it belonged in 2017. For instance, if Bank A bought formerly independent Fintech B in 2016 or earlier, we classify Fintech B as bank-related. If the acquisition happened in 2017 or later, we classify Fintech B as a fintech.

Figure 2 presents the percent of loans by lender type. Captive finance companies are the largest category, although banks as a whole have a comparable lending volume. The large spike in the "Other" category in 2020 is largely driven by SBA loans. As part of the government credit support amid the COVID-19 emergency, the SBA administered the Economic Injury Disaster Loan (EIDL)

[17]These sources are: The Forbes Fintech 50 list, published every year from 2015 to 2024; CNBC's "The World's Top 250 Fintech Companies 2024" list, specifically the "alternate finance" section; two 2024 articles by builtin.com ("102 fintech companies and startups to know" and "28 Fintech Lending Companies Upending the Credit Card, Mortgage and Loan Industries"); "The Top 25 Lending Technology Companies of 2024" by The Financial Technology Report; and "Top 36 Digital Lenders to U.S. Small Businesses (SMB)" by Fintech Labs.

[18]Note that fintech lenders would be underrepresented in UCC records to the extent that their superior information obviates the need for collateral.

[19]The FCS comprise a large number of local member-owned agricultural credit cooperatives, which could be classified as either nonprofits or government entities. Ultimately, because of their government-sanctioned nature, we choose to classify FCS co-ops as a type of government lender.

[20]For instance, a former captive finance company was bought by a private-equity firm and recast as a fintech. Another example is a finance-company subsidiary of Signature Bank that was spun off following Signature's demise, and appears to have become an independent finance company.

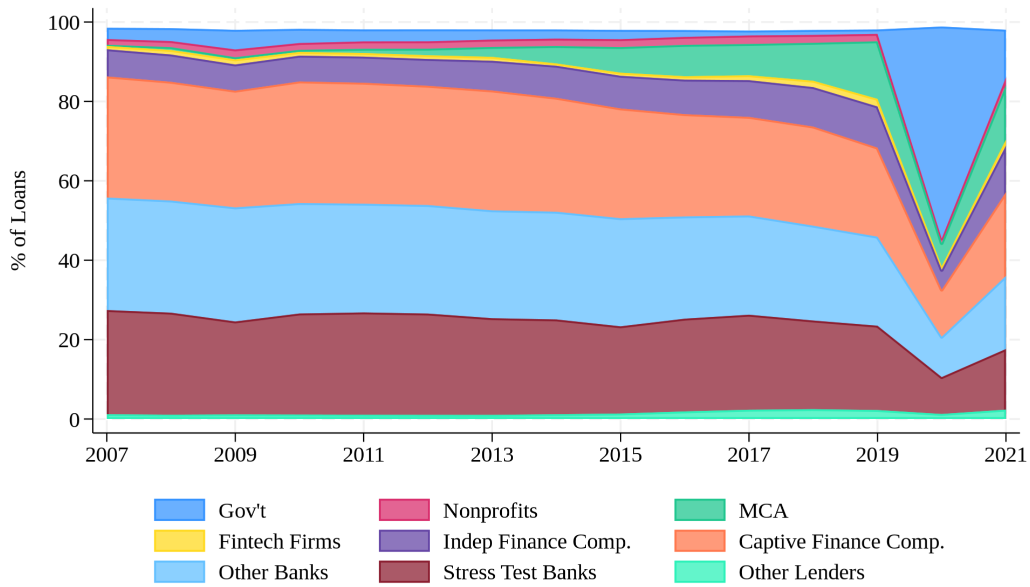

Figure 2: Share of loans by lender type for identified lenders
The lender types depicted in the chart are, from top to bottom: Government, Nonprofits, MCA, Fintechs, Independent Finance Companies, Captive Finance Companies, Other Banks, Stress Test Banks, and Other lenders.

program.[21] EIDL loans above $25,000 are required to be collateralized, and these appear in UCC filings.

2.3 Identifying Supply Shocks to Local Credit Markets

We analyze the post-GFC evolution of small business lending at the level of local lending market, which we operationally define as a county. In order to disentangle the supply from the demand factors that drive fintech lenders' entry into a market, we use shocks to fintech competitors' capacity to lend as a shift in supply. Specifically, similar to the approach used by Gopal and Schnabl (2022) and Cortés et al. (2020), we construct loan supply shocks using shocks to bank capital, under the assumption that banks for which regulatory capital constraints become more binding are likely to reduce their lending. Heterogeneity in these banks' presence at the county level then yields an instrument for loan supply that varies both across local markets and over time.

We rely on capital shocks that result from Federal Reserve stress testing, to which large U.S. banks have been subject since 2012. Stress tests are forward-looking quantitative exercises that assess whether banks are sufficiently capitalized to absorb losses under hypothetical adverse economic

[21] The other, larger, public loan program is the Paycheck Protection Program, which made forgivable, unsecured loans that did not appear in UCC filings.

conditions while meeting obligations to creditors and counterparties and continuing to be able to lend to households and businesses. Together with revised regulatory capital rules and enhancements to the standard supervisory program, stress tests have contributed to a more than two-fold increase in common equity capital at the largest U.S. banks since 2009.[22]

Stress tests create meaningful time variation in the amount of capital a bank needs to maintain above the minimum requirement. They are conducted at annual or biannual frequency, depending on the bank's asset size, and are based on at least two scenarios that are published by bank regulators shortly before each test is carried out. In order to "pass," a bank's capital ratios must remain above the minimal required levels even at the trough corresponding to the worst scenario for the bank—usually the "severely adverse" scenario. As banks learn about the stress scenarios and the resulting hypothetical values of their capital ratios, they may have an incentive to increase or decrease their capital buffer in response—to ensure that they pass the next time while economizing on capital. In fact, because of the continuous evolution of the stress testing regime, banks' stress-test capital ratios varied continually across test rounds, sometimes materially, and many banks may have needed to adjust their capital buffers in multiple years since stress testing commenced in 2012.

The stress tests consider several capital ratios: Tier 1 Common Capital (T1C), Common Equity Tier 1 Capital (CET1), Tier 1 Capital, Total Capital, Tier 1 Leverage, and Supplemental Leverage. Not all of these ratios are tracked at all times. Starting in 2016, the CET1 ratio essentially replaced the Tier 1 Common Capital ratio, and the Supplemental Leverage ratio was included only starting in 2019. In most years, the capital ratios were calculated according to two methodologies simultaneously. Under the Comprehensive Capital and Analysis Review (CCAR) stress test methodology, banks computed the stressed ratios internally. Under the Dodd-Frank Act Stress Test (DFAST) methodology, which was first adopted in 2012 and continues to present, the Federal Reserve computes stressed ratios for the banks. In 2017, CCAR underwent certain reforms, and stopped publishing quantitative results altogether after 2019. To reflect this shift in emphasis, we use the CCAR results until 2016 and the DFAST results from 2017 onwards in our baseline analysis, and conduct robustness checks to examine the sensitivity of our empirical findings to the choice of test results.

For each stress testing cycle, we use published stress test results to compute a "stress capital buffer" for each methodology and each ratio, equal to the decrease or "drop" between the initial value for that ratio and the minimum value projected under the severely adverse scenario. This was the capital buffer actively managed by the tested banks until 2016, and then again starting in 2020, as it resembles the stress-test-based Stress Capital Buffer that was formally implemented since 2020. Over 2017–2019, the 2.5 percent capital conservation buffer (CCB) and the surcharge on

[22]For more detail, see the *Stress Tests* page on the Federal Reserve Board's website (`https://www.federalreserve.gov/supervisionreg/stress-tests-capital-planning.htm`).

Global Systemically Important Bank (GSIB) under the Basel III capital rules) might have instead been the binding constraint.[23] Nevertheless, we use this simple measure for uniformity across all cycles and regimes. Since this buffer was likely not binding before 2020, so our baseline estimates likely provide a lower bound for the impact of capital shocks on lending. We conduct robustness checks to examine the sensitivity of our estimates to alternative measures of capital shock.

We then compute the maximum drop across all applicable stressed capital ratios within a given cycle. We posit that, when it is positive, it forms the binding constraint on a bank's capital buffer that it would want to "cure" until the next stress testing cycle. Since restraining asset growth is the more expedient way to rebuild capital ratio, this would imply a constraint on loan growth. We also produce three alternative shock measures to be employed in robustness checks. First, instead of using the maximum drop across all ratios, we define the buffer as the drop in the risk-weighted capital ratio, the most relevant ratio for risky business loans. This is the CET1 ratio where available, and the T1C ratio otherwise. Second, we also use the first difference, as opposed to the level, of each buffer (either the maximum drop buffer, or the risk-weighted capital buffer).

A number of studies have used stress tests as a source of exogenous capital constraints. Gopal and Schnabl (2022) simply use the introduction of stress tests as a one-time shock and compare lending behavior before and after. Cortés et al. (2020) and Berrospide and Edge (2024) estimate time-varying stress-test capital buffers and show that larger required buffers resulted in reduced lending. Our approach is similar to theirs, but we study a different category of loans that may have behaved differently. In particular, Cortés et al. (2020) mostly examine bank loans reported to fulfill requirements under the Community Reinvestment Act (CRA), while Berrospide and Edge (2024) study all corporate loans reported (in Form FR Y-14) by the stress-test banks.[24] Banks' incentive for making CRA-eligible loans may differ from that for the non-regulated UCC loans, so may their decision rules for making FR Y-14 loans, most of which are to larger companies, including unsecured loans to the largest corporations.

2.4 County Economic and Demographic Data

Given our definition of local lending market, we obtain data on several economic and demographic variables at the county level and yearly frequency as controls.

[23]The Stress Capital Buffer was designed to replace the 2.5 percent capital conservation buffer (CCB) introduced in 2017 (as part of the Basel III capital rules), thus the SCB is set with a minimum floor of 2.5 percent. Since the CCB was in effect until the SCB was formally introduced in 2019, any amount of SCB above the 2.5 minimum was probably not binding before 2020. Berrospide and Edge (2024) also came to the same assessment.

[24]Bord et al. (2021) also use CRA data to study the post-GFC change in small business lending; they find that large banks contracted lending due to capital impairment stemming from mortgage losses suffered during the crisis, and it had a net negative effect on local economies through 2015 because substitutions toward smaller banks only make up for part of the losses.

We create a county-year panel data of racial and ethnic composition by combining decennial Census and annual American Community Survey (ACS) tabulations made available via the National Historical Geographic Information System (NHGIS).[25] Although ACS tabulations are available at annual frequency, the ACS is a 1% sample of the U.S. population. As a result, population counts by race and ethnicity are often missing for smaller counties—either because of privacy concerns, or because the sampling error is too large to produce a meaningful estimate. We thus create a Census-only panel by imputing values for non-Census years (2001–2009, 2011–2019, 2021–2022) using linear interpolation and extrapolation from the 2000, 2010, and 2020 Censuses. We then replace imputed values using ACS values where available.

County-level data on per-capita personal income (PI) and employment is available from the Bureau of Economic Analysis (BEA).[26] We compute the employment rate as total employment divided by total population. For the less than 1% of counties that report employment numbers higher than the population, we cap the employment rate at 100%. The Bureau of Labor Statistics (BLS) also provides monthly Local Area Unemployment Statistics by county. As an alternative measure of the local labor market condition, we add the average annual unemployment rate for each county.[27]

Finally, while UCC loans are not collateralized by real estate, entrepreneurs can, to some degree, substitute between real-estate backed loans and other collateralized business loans. Changes in real estate valuations are, therefore, a potential confounding factor that needs to be controlled for. As our measure of county-level real-estate price appreciation, we adopt the Zillow Home Value Index (ZHVI).[28] We focus on the "All Homes - Top Tier" series which comprises single-family residences and condos priced in the top third of the market, under the assumption that this is the most relevant third for the provision of collateral backing business loans. We use the December index values to compute annual (12-month) home-price growth rates.

2.5 Summary statistics and figures

Table 1 reports summary statistics of the county-level variables, where each observation corresponds to one county-year. Table 2 presents summary statistics of the loan-level data, where each observation is one UCC loan.

Figure 3 shows the collateral mix by type of lender in our loan-level dataset. All three primary types of lenders (banks, finance companies, and fintechs) make a relative majority of their loans

[25] https://www.nhgis.org/. NHGIS is a data product of the Integrated Public Use Microdata Survey (IPUMS).
[26] https://apps.bea.gov/regional/downloadzip.htm, CAINC4 series.
[27] https://www.bls.gov/lau/data.htm, Series 03.
[28] https://www.zillow.com/research/data/.

Table 1: Summary Statistics of County-Level Data

	Mean	SD	Min	Max	N
White Population %	75.2	22.2	0.1	99.2	48,023
Black Population %	8.8	14.2	0.0	87.8	48,023
Hispanic Population %	10.6	18.2	0.0	99.8	48,023
Nonwhite Hispanic Population %	5.8	10.2	0.0	96.5	48,023
Nonblack, Nonhispanic Population %	80.9	21.5	0.2	100.0	48,023
Minority Population %	24.8	22.2	0.8	99.9	48,023
Per-Capita Personal Income USD	40,006	12,564	13,688	362,522	46,694
Per-Capita Income: County / State %	144.9	41.7	47.5	1146.5	46,694
Per-Capita Income: County / National %	134.7	42.3	46.1	1220.9	46,694
Employment Rate (BEA) %	52.0	14.5	11.3	100.0	46,694
Unemployment Rate %	6.5	3.2	0.8	29.4	48,203
Home Price Growth (based on ZHVI) %	3.3	6.4	-40.4	44.7	35,520

Source: Census Bureau/IPUMS, BEA, BLS, Zillow.

Table 2: Summary Statistics of Loan-Level Data

	Mean	SD	Min	Max	N
Borrower Sales Volume ($m)	182	4,206	0	611,289	12,403,676
Borrower Total Employees	130	592	1	5,336	12,403,676
Indicator: Filing as Agent	0.026	0.160	0	1	12,403,676
Indicator: Fintech	0.011	0.104	0	1	12,403,676
Indicator: Stress-Test Bank	0.140	0.347	0	1	12,403,676
Indicator: Other Bank	0.249	0.432	0	1	12,403,676
Indicator: Captive Finance Company	0.262	0.440	0	1	12,403,676
Indicator: Independent Finance Company	0.109	0.312	0	1	12,403,676

Source: UCC 1 filings and authors' manual entity classification.

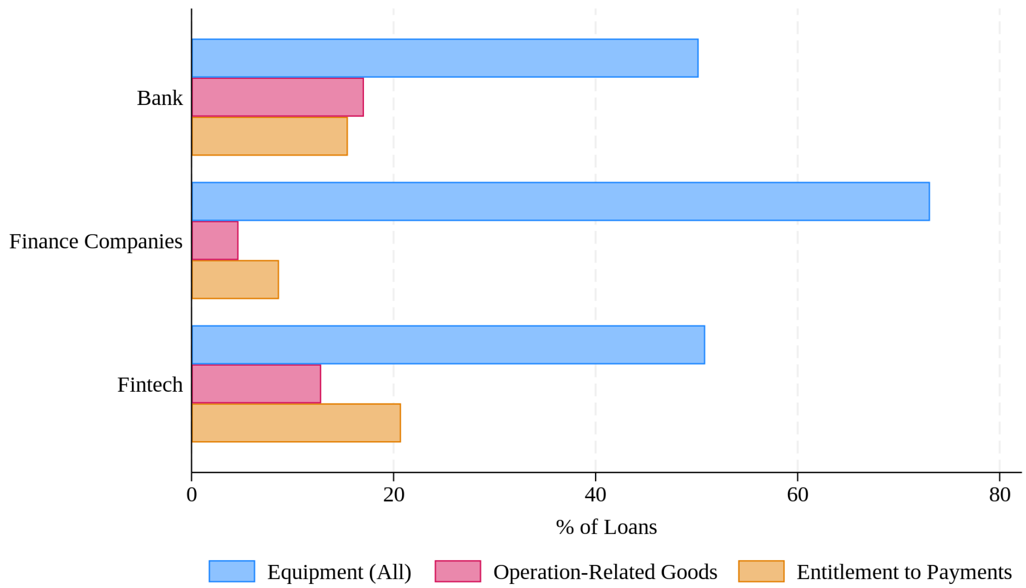

Figure 3: Share of top 3 collateral types for Finance Companies, Banks, and Fintechs

against equipment. Banks and fintechs also have significant shares of lending against other assets, including intangibles.

Figure 4 maps the geographic distribution of loans by lender type.

2.6 Measuring Financial Inclusion

We use the county-level data described above to identify high-minority areas and low-income areas, which are referred to collectively as "underserved" areas. There is ample evidence that supply of credit and other financial services is more scarce in areas with low income, or high minority shares, or both. Our definition of the underserved areas do not perfectly overlap with Community Reinvestment Act (CRA) assessment areas, and thus can provide additional insight on bank lending decisions beyond the influence exerted by regulation.[29]

Both income and the minority share (defined as the share of nonwhite population) have a highly stable geographic distribution throughout our sample period. Figures 5 and 6 map out the respective distribution in 2010 and 2020, which correspond to the two decennial Censuses, when demographic variables are measured with the greatest precision.[30]

[29]Each bank identifies its CRA assessment areas, based on where they have branches, ATMs, or originated or purchased loans. Within an assessment area, specific census tracts are then defined as "low- and moderate-income" (LMI) based on median tract income relative to a reference metropolitan area. Supervisors then assess the bank's compliance with the CRA based on lending in LMI areas.

[30]Figures 6 displays county per capita income normalized by the state average, while Appendix Figure 10 shows the corresponding map with county per capita income normalized by the national average.

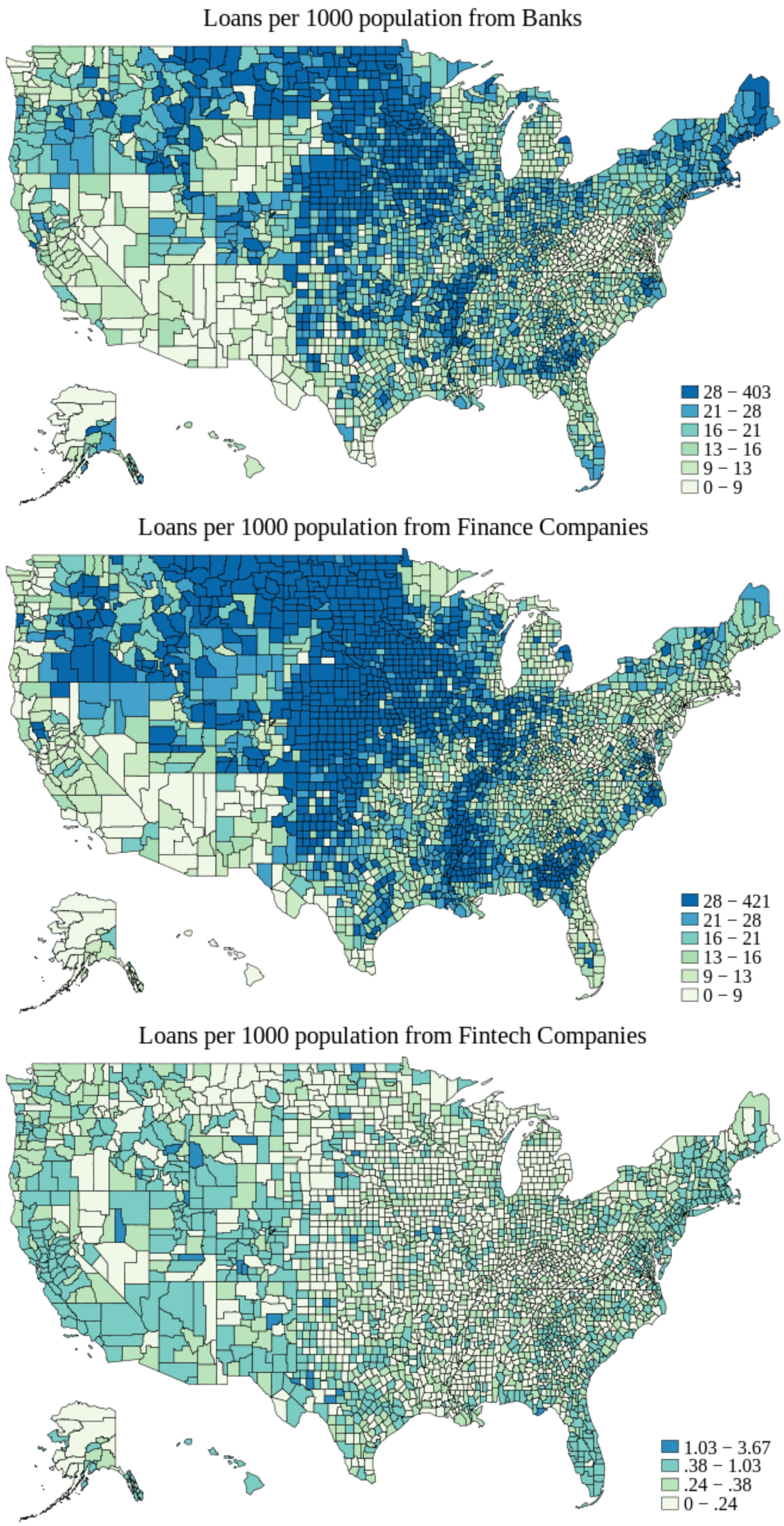

Figure 4: Geographic distribution of loans by Banks, Finance Companies, and Fintechs

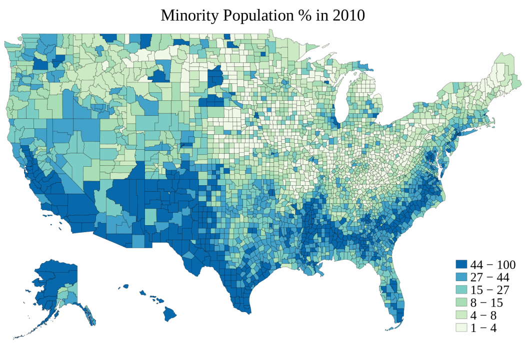

Figure 5: Minority share (percent of nonwhite population) by county, 2010 and 2020

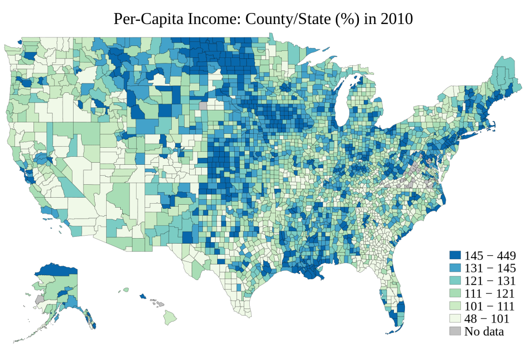

Figure 6: County per-capita income normalized by state average, 2010 and 2020

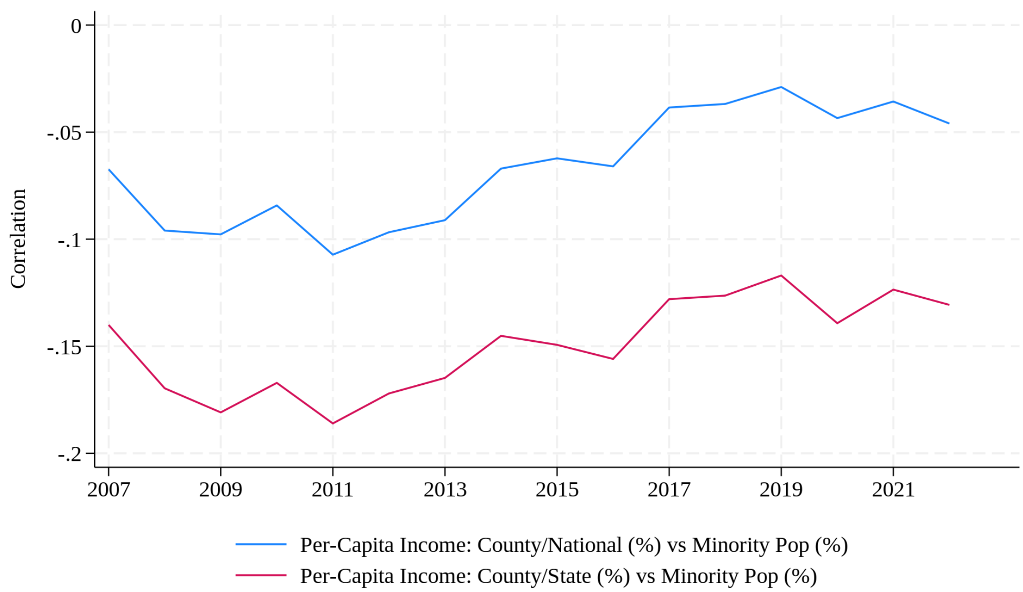

Figure 7: County-level correlation between normalized per-capita income and minority share, 2007–2022

Since the income of whites is, *on average*, higher than that of minorities, one might expect a negative correlation between income and minority share. To test this intuition, we compute the correlation between the minority share and a normalized income by county, with county income normalized as a percentage of either national income or state income per capita. Figure 7 plots the correlations between relative income and minority share over the years, and reveals several interesting patterns. First, the correlation is negative, as expected, but low, indicating that our variables measure two fairly distinct aspects of economic disadvantage. Second, perhaps not surprisingly, state relative income shows a more (albeit still mild) negative correlation with minority share than national relative income, indicating that there are systematic income differences across states uncorrelated with the presence of minorities. Third, regardless of the measure, the correlation is shrinking in absolute magnitude over time. This finding is consistent with Chetty et al.'s (2024) finding that minorities' (specifically, Blacks') income has been catching up to whites' income but has not fully caught up yet. Based on these findings, we will use the county income measure that is normalized by state personal income in our subsequent analysis, and simply refer to it as "normalized income."

2.7 Paycheck Protection Program Data

To study the implications for small businesses during the pandemic of the post-GFC changes in their sources of credit, we link up the UCC data with the PPP loan data by borrower name and address fields. The PPP data we use in this study are from the SBA's data release as of July 2021.[31] These data contain name and address for borrowers and lenders, with more information for the servicing lender than the originating lender. We use the originating lenders in our analysis, although for the majority of loans, the two roles were performed by the same lender. Also provided is information on each loan and the borrower: loan amount, approval date, number of job saved as reported by the borrowers, whether the firm was located in a rural tract or a low-to-moderate-income tract, whether it is a minority firm, etc. Many of the borrower attribute fields contain a high fraction of missing values and are thus not used for analysis.

Table 3: Percent growth in number of loans

2007-2019	Banks ST	Banks Other	Banks All	Non-Banks Indep.	Non-Banks Captive	Non-Banks MCA	Non-Banks Fintech	Non-Banks Nonprofit	Non-Banks All	Gov.	All
All Counties	-4	-1	-2	86	-10	1187	183	60	22	-16	7
Low Income	8	-1	3	119	-8	1220	252	-4	21	27	13
Very Low Income	-4	-15	-11	108	-13	1625	243	28	17	21	4
High Minority	-4	5	0	109	-9	1514	236	72	39	32	16
Very High Minority	5	-1	2	116	-19	1907	285	108	40	15	17

2019-2021	Banks ST	Banks Other	Banks All	Non-Banks Indep.	Non-Banks Captive	Non-Banks MCA	Non-Banks Fintech	Non-Banks Nonprofit	Non-Banks All	Gov.	All
All Counties	-23	-10	-16	26	6	-48	-12	15	6	432	10
Low Income	-16	-11	-13	50	6	-44	-4	20	15	227	16
Very Low Income	-16	-8	-11	68	4	-38	35	7	20	292	23
High Minority	-31	-12	-21	30	1	-49	-15	14	4	1228	14
Very High Minority	-40	-15	-27	29	6	-49	-21	6	4	2768	22

Stress-test (ST) banks are banks that have ever been in at least one stress test as of end 2021. Captive finance companies are those owned by a nonfinancial business such as an equipment manufacturer. Independent (Indep.) finance companies are not owned by either a bank or a nonfinancial business. Merchant Cash Advance (MCA) businesses advance money in exchange for a fraction of future sales, which is legally not a loan. Nonprofits include credit unions, trade association sponsored lenders, and charity lenders. Government (Gov.) lenders include mainly the Small Business Administration and the Farm Credit System.

3 Stress-Testing Shocks and Credit Supply Substitution

3.1 Lending Growth in Underserved Areas

We begin our analysis with a descriptive study of how lending has grown since 2007 in areas with more or less underserved population. To facilitate presentation, we classify counties into discrete bins by normalized income and minority share. Specifically, we define low- (very low-) income counties as those where normalized income is in the bottom 20 percent (5 percent) within a given year. Similarly, we identify high- (very high-) minority counties as ones in which the minority share is in the top 20 percent (5 percent) within a given year. A problem with this classification methodology is that the growth rate for a given category of counties may be distorted if counties of substantially different sizes enter or exit that category from one year to the next over the sample

[31]This release is likely to be the most complete. On one hand, it should contain all the PPP loans disbursed until the program's close date of May 31, 2021. On the other hand, some loans seem to have been removed from later releases for unspecified reasons. The current vintage of PPP data can be downloaded from the SBA at `https://data.sba.gov/dataset/ppp-foia`.

period. For example, if a large county with many loans exits the "low income" group and a small county enters, the total number of loans to low-income counties drops for reasons unrelated to lenders' or borrowers' behavior. To minimize this distortion, we measure lending growth using the growth rate of UCC loans normalized by population.

Table 3 reports the percentage growth in lending by each type of lender. The lender types include banks (Stress Test Banks, Other Banks, All Banks), nonbanks (Independent Finance Companies, Captive Finance Companies, MCAs, Fintechs, Nonprofits, All Nonbanks), and Government. The last column shows the growth rate for All Lenders for reference. The first row presents the nationwide growth rate, while the next four rows detail the growth rates specific to different types of inclusion areas as defined above.

The top panel of Table 3 reports the growth from 2007 to 2019. Overall, the number of loans grew by only 7 percent over this period, which includes the financial crisis and the Great Recession. Lending by banks and government entities shrank by 2 percent and 16 percent, respectively, while nonbank lending expanded massively. Within nonbanks, we further observe that lending by captive finance companies (the largest group among lenders by loan count over the entire period) also shrank by 10 percent, while all other nonbanks experienced sustained growth. In particular, MCAs and fintechs grew at astronomical rates (1187 percent and 183 percent, respectively). While these similar patterns are qualitatively present across all the areas defined above, nonbank lending grew even more in low-income and high-minority counties than on average nationwide. One noteworthy statistic is that lending by MCAs (which Gopal and Schnabl, 2022 classify as "fintechs") grew almost twenty-fold over the thirteen years in very-high-minority counties—an annual compound growth rate of over 25 percent.

The bottom panel of Table 3 shows the growth from 2019 to 2021, an extraordinary period in which the economy was upended by COVID-19. Credit markets were disrupted both directly and indirectly via the government's pandemic response in the form credit assistance programs. As expected, the table confirms a massive expansion of lending by government entities. This can be regarded as an informal validation of our data for the pandemic era, which have not been explored before, to the best of our knowledge. The growth rates over 2020 and 2021 exhibit substantial heterogeneity across lender types. Lending by banks, especially stress-test banks, decreased substantially. "New" lenders also retreated, with MCA lending falling by about half, and fintech lending decreasing by 12 percent. On the other hand, independent and captive finance companies expanded by 26 and 6 percent, respectively, over the two years. Nonprofits also expanded by 15 percent.

These patterns are especially pronounced in high-minority counties. In particular, in very-high-minority counties, stress-test bank lending declined by 40 percent, while government lending expanded almost *28 fold*. Independent finance companies expanded tremendously in low-income (50 percent) and very-low-income (68 percent) areas. In sum, overall lending by all types of lenders actually grew during the pandemic, and it grew even more in underserved areas, which presumably needed credit assistance more.

3.2 Collateral Specialization across Lender Types

To the extent that different lenders have a comparative advantage in lending against different types of collateral, the relative increase in the market share of nonbank lenders can result in lower cost of capital for assets favored by such lenders.[32] For example, captive finance companies tend to be affiliated with manufacturers of vehicles and heavy machinery. They thus specialize in financing the purchase of such assets. Independent finance companies routinely make loans collateralized by equipment more broadly. The relatively faster growth of finance companies thus imply more readily available or cheaper funding for such assets, likely encouraging firms to invest more in such assets or boosting the growth of firms whose operations naturally call for heavy equipment or vehicles, etc. as input, all else being equal. On the other hand, new types of nonbank lenders such as MCAs and especially fintechs appear to specialize in assets or claims that are less tangible and more tied to cash flows generated by the borrower's operation, probably because such lenders possess technology that is superior at tracking borrower cash flow in real time.[33] This suggests that fintech lenders are more likely to accept claims on a firm's cash flow (such as accounts receivable) or other intangibles (such as chattel paper) as collateral. The rise of these more information-based lenders can benefit firms in the service sector. It also suggests that the causality for the changing composition of lender types can go both ways: the growing share of service firms has likely created greater demand for lenders with comparative advantage in lending to them.

We thus examine the pattern of collateral specialization in the UCC data, and whether it has any implications for the evolution of different lenders' market shares in those underserved areas. Collateral backing each loan is classified into one of 33 types, listed in Appendix Table A.1, and the majority of loans are secured by more than one type of collateral. Figure 8 depicts the share of loans by a type of lender backed by each type of collateral for the seven most prevalent types. Consistent with the above discussion, nearly 60% and 20% of finance company loans are backed by general equipment (other than computer and communication equipment) and computer equipment, respectively. By comparison, more of bank loans are secured by assets that arise in the production process (inventory and accounts). Equipment broadly defined constitute a common category of collateral for all of the three primary lender groups.

[32]Gopal (2021), also using UCC data, shows that lenders specialize in different categories of collateral, and firms that borrowed from lenders specializing in its collateral types were able to obtain more credit after the GFC.

[33]Using French data, Beaumont et al. (2022) indeed show that fintech lenders have a comparative advantage over banks in lending to firms with fewer tangible assets.

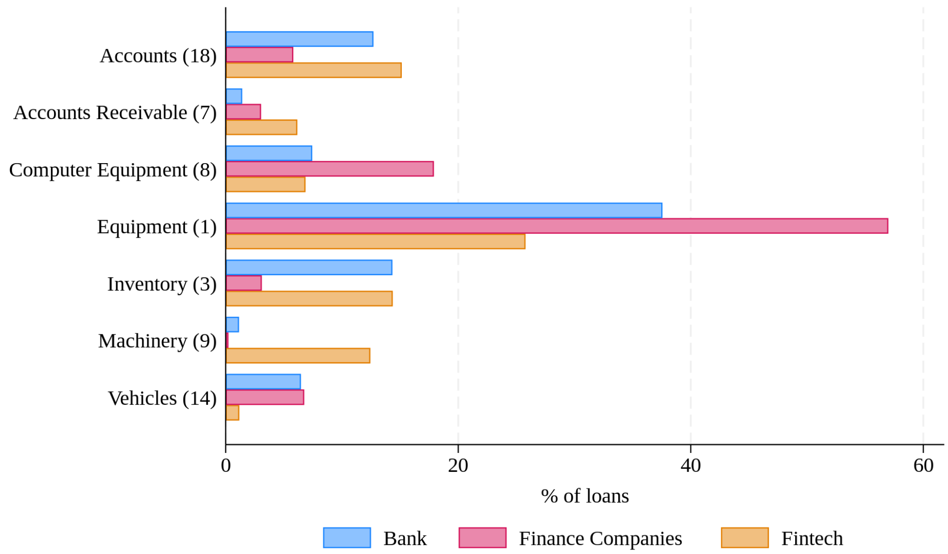

Figure 8: Collateral Composition of Loans by Lender Type

3.3 Industry Specialization across Lender Types

Evidence is accumulating that banks specialize along various dimensions, such as by industry (Blickle et al., 2023) or countries of borrowers' trade relationships (Paravisini et al., 2023). This suggests that the different types of UCC lenders may choose to specialize in different subsets of industries, owing to differences in their funding sources, lending technology, etc. Moreover, to the extent that certain types of assets are used more intensively in some industries than in others, the collateral specialization across types of lenders documented above becomes an additional source of (indirect) industry specialization. Figure 9 depicts the pre-pandemic share of borrower industries (by 2-digit SIC) in the loan portfolio of each type of lender across the industries consistently among the top three in lender portfolios. Two industries, health services and broadly defined business services (spanning advertising, facility management, computing programming, etc.) make up the top two portfolio shares (from 8 to 10 percent) across all three lender groups, while all three invest a relatively small share (around 1 percent) in legal services. In contrast, a few other industries exhibit pronounced heterogeneity across lender types: not surprisingly, finance companies lend more heavily to auto dealers; FinTechs lend much more to restaurants, bars, etc., possibly because they are better able to collect timely information on those businesses' revenue flows by monitoring digital channels such as booking and review platforms.

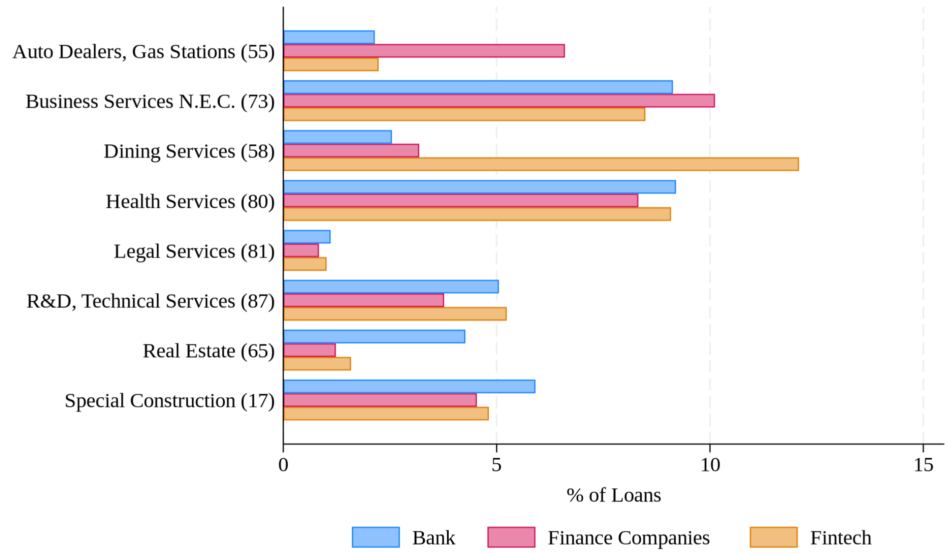

Figure 9: Borrower Industry Composition of Loans by Lender Type

3.4 Did Capital Regulation Contribute to the Changes?

3.4.1 Capital Shocks and Bank Lending

The statistics in Table 3 show that banks' lending grew slowly or even fell, while lending by nonbanks expanded at a sustained pace. However, this is not sufficient evidence that the expansion of nonbanks was a direct response to the retreat of banks. To test such a causal relationship, we identify plausibly exogenous shocks to the supply of bank loans in local lending markets. We then estimate how these supply shocks affected the lending of non-shocked banks as well as nonbanks.

Our identification strategy relies on stress testing as a shock to those banks' capital. Each round of stress test places a constraint on a tested bank's capital that is, at least to some extent, unpredictable. As in several existing studies (e.g., Cortés et al., 2020 and Berrospide and Edge, 2024), we use the annual variation in projected decline in capital ratios as a measure of shocks to bank capital. Then, similar to Gopal and Schnabl (2022), we use these unexpected capital shocks to infer loan supply shocks in local markets, based on the preexisting heterogeneous presence of each stress-test bank across markets.

Our basic identifying assumption is that the size of the stress-test buffer required by a given bank in a given year, *relative to that bank's required stress-test buffer in the average year*, can be considered exogenous. Roughly speaking, a bank's stress-test capital shock is a function of two main factors: the shock scenario and the bank's portfolio. The exogeneity of the shock scenario seems uncontroversial: by nature, scenarios change somewhat from year to year, they are the same for every bank, and they are chosen by the Federal Reserve outside of banks' control. The bank's portfolio, in contrast, is endogenous, but it is also *predetermined* at the time the scenarios are revealed for each round. On the whole, we believe our assumption is reasonable because the composition of individual bank loan portfolios changes slowly, so it is unlikely that a given bank adjusted its loan portfolio in advance to engineer a particular stress test result.[34] Moreover, our specific analysis of UCC loans should be comparatively free from reverse causality because these loans account for a fairly small fraction of the stress-test banks commercial loan portfolio. They are thus unlikely to rely on such loans to position their capital ratios. We measure a bank's shock relative to the average year by using lender fixed effects in all our regressions. Doing so removes any systematic variation in portfolios (and any resultant variation in capital shocks) in the cross-section of banks. Then, since portfolios change slowly within a given bank, the overwhelming majority of the variation in stress-testing capital shocks comes from exogenous changes in the scenarios.

A stress-test capital shock does not automatically imply a cutback in lending. A bank can respond to the shock by shrinking asset size, either by lending less or by selling assets, or it can simply raise more capital. Most studies do find that banks respond to capital shocks by lending less. Nevertheless, it remains an empirical question for any specific asset category. We thus begin by showing that our exogenous shock actually predicts lending growth by the affected banks. To do so, we run the following "first stage" regression:

$$\begin{aligned}\text{Lending Growth}_{l,c,t} &= \alpha + \beta_0 \cdot \text{Stress-Test Buffer}_{l,t} + \beta_1 \cdot I^1_{c,t} \times \text{Stress-Test Buffer}_{l,t} + \\ &+ \beta_2 \cdot I^2_{c,t} \times \text{Stress-Test Buffer}_{l,t} + \gamma \cdot X_{c,t-1} + \lambda_l + \lambda_c + \lambda_t + \varepsilon_{l,c,t}.\end{aligned} \tag{1}$$

In this equation, Lending $\text{Growth}_{l,c,t}$ is lending growth by stress-test bank l in county c during year t, defined as the "symmetric growth rate" in the number of loans:

$$\text{Lending Growth}_{l,c,t} = \frac{\text{N Loans}_{l,c,t} - \text{N Loans}_{l,c,t-1}}{1/2(\text{N Loans}_{l,c,t} + \text{N Loans}_{l,c,t-1})}. \tag{2}$$

This growth rate measure is desirable for two reasons: first, it avoids outliers because it is by definition capped between −2 and +2. Second, it can accommodate 'entry' and 'exit' in that it is defined even when there are no loans in periods $t-1$ or t.

Stress-Test $\text{Buffer}_{l,t}$ is, as discussed in Section 2, the largest drop projected by stress tests across all ratios that apply to bank l in year t. A positive number indicates a drop, i.e., a capital shortfall, so we expect β_0, its effect on lending, to be negative. Stress-Test $\text{Buffer}_{l,t}$ is also multiplied by up to two interaction terms in order to allow for a differential effect of stress-test capital shocks on lending in those specific underserved counties. These terms, $I^1_{c,t}$ and $I^2_{c,t}$, are county-year-level binary indicators. In one specification, I^1 and I^2 take a value of one for low-income and very-

[34] Unlike loan portfolios, banks' trading portfolios do change quickly. However, the stress-test scenario component that applies to these portfolios (the "global market shock" component) is also more unpredictable.

Table 4: Stress-Test Buffers and Stress-Test Banks' Lending.

Dependent Variable: Lending Growth Rate					
	(1)	(2)	(3)	(4)	(5)
Stress-Test Buffer	-0.009**	-0.018***	-0.018***	-0.020***	-0.017***
	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)
Buffer × High Minority				0.005	
				(0.007)	
Buffer × Very High Minority				-0.013	
				(0.015)	
Buffer × Low Income					-0.015
					(0.016)
Buffer × Very Low Income					-0.000
					(0.032)
Housing Price Growth ($t-1$)			-0.000	-0.000	-0.000
			(0.001)	(0.001)	(0.001)
Unemployment Rate ($t-1$)			0.009	0.009	0.009
			(0.008)	(0.008)	(0.008)
Personal Income Growth ($t-1$)			-0.104	-0.105	-0.104
			(0.197)	(0.197)	(0.197)
Constant	0.014	0.042**	-0.004	-0.005	-0.002
	(0.013)	(0.016)	(0.046)	(0.046)	(0.046)
Lender Fixed Effects	No	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Adjusted R-Squared	0.004	0.012	0.012	0.012	0.012
Number of Observations	12,312	12,312	12,160	12,160	12,160

Notes: Robust standard errors are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

low-income counties, respectively, and zero otherwise. In that specification, the total effect of Stress-Test Buffer$_{l,t}$ is equal to $\beta_0 + \beta_1$ for low-income counties and $\beta_0 + \beta_1 + \beta_2$ for very-low-income counties. In another specification, I^1 and I^2 indicate high-minority and very-high-minority counties, respectively, with an analogous interpretation. The effect of Stress-Test Buffer$_{l,t}$ in all other counties is measured by β_0 alone.

Finally, $X_{c,t-1}$ is a vector of lagged, county-level control variables, including Housing Price Growth, Unemployment Rate, and Personal Income Growth. The three λ terms are lender, county, and year fixed effects.

We estimate this regression on a sample of all UCC loans from 2007–2019. (We examine 2020–2021, the pandemic era, in the next section.) To reduce the influence of outliers, we exclude county-lender pairs in which the average number of loans throughout our entire sample (2007–2021) is less than 20.[35]

[35] Our results are qualitatively robust to using a less strict filter (less than 10) or no filter at all, although the distribution of both the dependent and independent variables becomes progressively more non-normal. Our estimates

Table 4 shows the estimates from several versions of Equation (1). Column (1) contains no lender fixed effects and no other controls. Column (2) introduces lender fixed effects. Column (3) introduces the lagged county-level control variables. Finally, Columns (4) and (5) introduce the I^1 and I^2 interactions terms for high-minority and very-high-minority counties, respectively.

Across all specifications, the effect of stress-testing capital shocks is negative and significant: a one percentage point higher stress-test buffer predicts a roughly 1.8 percentage point lower loan growth rate. This is a large effect, as the average lender-county annual loan growth over 2007–2019 is 1.0 percent. The coefficient in column (1), without lender fixed effects, is substantially smaller (0.9 percent), confirming that lender fixed effects are an important component of our identification strategy. Adding control variables, on the other hand, has little impact on the estimated effect of stress-test shocks. We also do not detect a differential effect of our capital shocks across low-income or high-minority counties.

3.4.2 Substitution of Loan Supply at Local Markets

So far, we have argued that stress tests create identifiable and plausibly exogenous shocks to banks' capital and shown that these shocks have a large negative effect on these banks' lending in local markets. We now use these shocks as exogenous *supply* shocks to the local markets to identify the response of other lenders when stress-test banks withdrew from a local market. To this end, we estimate the following equation:

$$\begin{aligned}\text{Lending Growth}_{m,c,t} &= \alpha + \beta \cdot \text{Stress-Test Supply Shock}_{c,t} + \beta_1 \cdot I^1_{c,t} \times \text{Stress Test} \\ &\text{Supply Shock}_{c,t} + \beta_2 \cdot I^2_{c,t} \times \text{Stress-Test Supply Shock}_{c,t} + \gamma \cdot X_{c,t-1} + \lambda_c + \lambda_t + \varepsilon_{m,c,t},\end{aligned} \tag{3}$$

where m indicates the lender type (listed in Section 2 and below). As in Equation (1), Lending Growth$_{m,c,t}$ indicates the symmetric percentage change from year $t-1$ to t in the aggregate number of loans made by lenders of type m in county c. Also as in Equation (1), $X_{c,t-1}$ denote county-level controls while λ_c and λ_t denote county and year fixed effects, respectively.[36]

Stress-Test Supply Shock$_{c,t}$ is computed as the average Stress-Test Buffer in a given county and year, weighted by the small-business loan share of each stress-test bank in that county over year

are also robust under various alternative specifications. One is a weighted least squares estimation, in which each lender-county-year observation is weighted by the square root of the number of loans. This downweights county-lender-year observations with fewer loans, assuming the variance of measurement errors is inversely proportional to loan count. Others use a different definition of stress-test buffer, replacing the maximum drop across all ratios with the drop in risk-based capital ratio (the Tier 1 Capital ratio and later, when introduced, the Common Equity Tier 1 ratio), reflecting the greater relevance of this specific ratio for small business lending. Yet another specification uses the year-over-year change in the stress-test buffer, rather than the level.

[36]Note, however, the absence of lender-type fixed effects, which cannot be used here because the regression uses lender type-year panel data.

Table 5: Stress-test Supply Shocks and Other Lenders in Low-Income Areas

	Dependent Variable: Lending Growth Rate						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Stress-Test Supply Shock	0.209***	0.213***	0.337***	-0.201	0.194***	-0.078	0.329***
	(0.038)	(0.057)	(0.029)	(0.166)	(0.061)	(0.169)	(0.106)
Supply Shock × Low Income	0.072	-0.022	-0.011	-0.571	-0.004	0.065	-0.049
	(0.070)	(0.109)	(0.049)	(0.678)	(0.126)	(0.277)	(0.212)
Supply Shock × Very Low Income	0.063	0.053	-0.134	0.186	0.161	0.999*	-0.126
	(0.157)	(0.189)	(0.092)	(0.459)	(0.189)	(0.558)	(0.393)
Housing Price Growth ($t-1$)	-0.003**	-0.003	-0.000	-0.002	-0.001	-0.017***	0.004
	(0.001)	(0.002)	(0.001)	(0.005)	(0.002)	(0.005)	(0.004)
Unemployment Rate ($t-1$)	0.047***	0.028***	0.037***	0.005	-0.008	0.042	-0.019
	(0.006)	(0.010)	(0.005)	(0.034)	(0.011)	(0.028)	(0.019)
Personal Income Growth ($t-1$)	-0.268**	0.315	0.038	0.222	-0.495**	-1.438**	-0.691**
	(0.131)	(0.231)	(0.092)	(0.800)	(0.250)	(0.696)	(0.336)
Constant	-0.353***	-0.200***	-0.374***	0.264	0.242***	-0.051	-0.112
	(0.039)	(0.064)	(0.029)	(0.214)	(0.070)	(0.169)	(0.109)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Squared	-0.014	-0.050	0.012	0.325	0.231	0.003	-0.115
Number of Observations	8,966	3,596	11,672	564	2,499	997	5,311

Notes: Robust standard errors are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents the estimates for a specific group of lenders: (1) Other (i.e., non-stress-test) Banks; (2) Independent Finance Companies; (3) Captive Finance Companies; (4) Fintechs; (5) MCAs; (6) Nonprofits; (7) Government.

$t-1$:[37]

$$\text{Stress-Test Supply Shock}_{c,t} = \sum_{l \in L_{c,t}} \text{Stress-Test Buffer}_{l,t} \cdot \frac{\text{N Loans}_{l,c,t-1}}{\sum_{j \in L_{c,t-1}} \text{N Loans}_{j,c,t-1}} \tag{4}$$

where $L_{c,t}$ indicates the set of all lenders in county c and year t, and Stress-Test Buffer$_{l,t}$ is set to zero for lenders other than stress-test banks. The lagged market shares as weights should prevent mechanical correlation between our shock variable and the outcome variable.

Tables 5 and 6 present the estimation results for Equation (3), with interaction terms capturing potential differential effects on underserved areas (low-income and high-minority areas, respectively).[38] Each column presents the estimates for a specific group of lenders: (1) Other (i.e.,

[37]It is useful to illustrate the formula using a simple numerical example. Suppose that in county c there are two stress-test banks (A and B). In year $t-1$, a total of 100 loans were originated in county c, of which 10 by A, 25 by B, and 65 by other lenders (either non-stress-test banks or nonbanks). In year t, banks A and B required a stress-test buffer of 2.5 and 4 percentage points, respectively. Our supply shock for year t and county c is computed as $2.5 \cdot 10/100 + 4 \cdot 25/100 = 1.25$.

[38]Similar to the previous subsection, these regressions also exclude county-lender type pairs in which the average number of loans throughout our entire sample (2007-2021) is less than 20.

Table 6: Stress-test Supply Shocks and Other Lenders in High-Minority Areas

Dependent Variable: Lending Growth Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Stress-Test Supply Shock	0.205***	0.196***	0.341***	-0.303	0.197***	-0.155	0.299***
	(0.039)	(0.060)	(0.028)	(0.190)	(0.064)	(0.177)	(0.109)
Supply Shock × High Minority	0.123	0.063	-0.085	0.219	0.011	0.451	0.162
	(0.081)	(0.092)	(0.062)	(0.193)	(0.091)	(0.317)	(0.204)
Supply Shock × Very High Minority	-0.121	0.203	-0.090	-0.532	-0.263	1.613**	-0.238
	(0.242)	(0.252)	(0.203)	(0.708)	(0.235)	(0.804)	(0.584)
Housing Price Growth ($t-1$)	-0.003**	-0.003	-0.000	-0.002	-0.001	-0.016***	0.004
	(0.001)	(0.002)	(0.001)	(0.005)	(0.002)	(0.005)	(0.004)
Unemployment Rate ($t-1$)	0.047***	0.028***	0.037***	0.003	-0.008	0.045	-0.019
	(0.006)	(0.010)	(0.005)	(0.034)	(0.011)	(0.028)	(0.019)
Personal Income Growth ($t-1$)	-0.274**	0.311	0.040	0.214	-0.490*	-1.491**	-0.689**
	(0.131)	(0.231)	(0.092)	(0.800)	(0.250)	(0.695)	(0.336)
Constant	-0.357***	-0.203***	-0.373***	0.260	0.240***	-0.138	-0.117
	(0.039)	(0.064)	(0.029)	(0.213)	(0.070)	(0.171)	(0.109)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Squared	-0.014	-0.050	0.012	0.326	0.231	0.007	-0.115
Number of Observations	8,966	3,596	11,672	564	2,499	997	5,311

Notes: Robust standard errors are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents the estimates for a specific group of lenders: (1) Other (i.e., non-stress-test) Banks; (2) Independent Finance Companies; (3) Captive Finance Companies; (4) Fintechs; (5) MCAs; (6) Nonprofits; (7) Government.

non-stress-test) Banks; (2) Independent Finance Companies; (3) Captive Finance Companies; (4) Fintechs; (5) MCAs; (6) Nonprofits; (7) Government.

The results are once again consistent with expectations. For most lender types, the effect of a supply shock is large, positive, and significant. While the coefficients are difficult to interpret at their face value, a one-standard deviation increase in our shock variable (0.26) predicts a 5–9 percentage point increase in the lending growth rate, depending on lender type. Captive finance companies and government lenders exhibit the greatest gains in market share in response to the supply shock from stress-test banks. The coefficient is negative and insignificant for only two types of lenders: Fintech and Nonprofit. This is likely due to the relatively small number of loans by these two types of lenders. Overall, these estimates suggest that nonbanks fill the credit market supply gaps created by bank capital regulation.

On the other hand, the interaction terms with normalized income and minority share are largely statistically insignificant. This suggests that nonbanks made no evident distinction across the local markets as they stepped in to fill some of the void left by the contraction of lending by stress-test banks. Taken together with the same finding in the previous subsection, these findings indicate that neither banks nor nonbanks appear to treat low-income and high-minority areas differently.

4 Lending During the Pandemic (2020–2021)

4.1 The Effect of Stress-Testing Shocks

Our descriptive analysis in Section 3.1 confirms that credit markets were disrupted by COVID-19 over 2020–2021. A defining characteristic of this period was a surge in lending by government entities through multiple credit-based emergency assistance programs, as evidenced by the spike of government lending share shown in Figure 2. Because of this clear break in small business credit market dynamics, we analyze this period separately from the previous, non-emergency, period.

Tables 7 (stress-test buffers) and 8–9 (stress-test supply shocks) are the pandemic counterparts to Tables 4, 5, and 6, respectively. The "first-stage" regression (Table 7) produces qualitatively similar results, although with a greater magnitude, indicating that stress-test buffers during the pandemic strongly predict which lenders curtailed lending.

Regressions of other lenders' behavior toward low-income counties (Table 8) and high-minority counties (Table 9) both tell roughly the same story. Foremost is the massive substitution from stress-test bank lending to government lending (Column (7)). This goes beyond the results in the bottom panel of Table 3, indicating that not only did government lending grow, but it grew more in markets that suffered the worst supply shocks from the retreat of large bank. In other words, our stress-test supply shock instrument helps reveal that the larger increase in government loans in those underserved areas were in part driven by local small firms' need for a substitute credit source. This finding that public assistance achieved targeted relief even within the underserved areas extends similar findings in existing studies that the 2021 PPP round provided more relief to minority communities (see, e.g., Fairlie and Fossen, 2022).[39] The tables also show some substitution from stress-test banks to other lenders—namely Captive Finance Companies, Fintechs, and MCAs. This result must be interpreted in light of the finding from Table 3 that, in the aggregate, these institutions reduced their lending substantially. Thus, our regression results indicate that Fintechs and MCAs curtailed their lending less in counties that suffered the worst supply shocks. Finally, the estimated coefficients on the low-income and high-minority indicators are for the most part negative and in some cases significantly so, suggesting that lenders filled *less* of the supply gap left by stress-test banks in underserved areas.

[39]According to the Government Accountability Office (2021) and Fairlie and Fossen (2021), Phases 2 and 3 of 2020 PPP already delivered more assistance to minority communities than Phase 1, in part through expanding eligible lenders. Moreover, in Phase 2, the SBA set aside \$10 billion for CDFI lending in order to target minority and other underserved counties.

Table 7: Stress-Test Banks' Lending During the Pandemic

Dependent Variable: Lending Growth Rate

	(1)	(2)	(3)	(4)	(5)
Stress Test Buffer	-0.021	-0.288***	-0.292***	-0.277***	-0.292***
	(0.013)	(0.035)	(0.035)	(0.037)	(0.035)
Buffer × High Minority				-0.036	
				(0.024)	
Buffer × Very High Minority				0.024	
				(0.051)	
Buffer × Low Income					-0.052
					(0.062)
Buffer × Very Low Income					-0.130
					(0.195)
Housing Price Growth ($t-1$)			0.004	0.004	0.004
			(0.006)	(0.006)	(0.006)
Unemployment Rate ($t-1$)			0.013	0.011	0.013
			(0.011)	(0.011)	(0.011)
Personal Income Growth ($t-1$)			-1.690***	-1.622***	-1.725***
			(0.597)	(0.599)	(0.599)
Constant	-0.081***	0.294***	0.291***	0.298***	0.300***
	(0.020)	(0.050)	(0.094)	(0.095)	(0.095)
Lender Fixed Effects	No	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Adjusted R-Squared	-0.020	0.104	0.108	0.108	0.108
Number of Observations	3,401	3,401	3,371	3,371	3,371

Notes: Robust standard errors are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 8: Stress-Test Supply Shocks and Other Lenders in Low-Income Areas During the COVID Pandemic

Dependent Variable: Lending Growth Rate							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Stress-Test Supply Shock	0.175	0.156	0.169	1.517***	0.600***	-0.678	2.679***
	(0.166)	(0.235)	(0.126)	(0.566)	(0.195)	(0.659)	(0.220)
Supply Shock × Low Income	-0.466	-1.341***	0.027	-0.529	-0.148	1.182	0.346
	(0.304)	(0.461)	(0.231)	(1.020)	(0.385)	(1.359)	(0.475)
Supply Shock × Very Low Income	0.226	0.187	-0.296	1.911	-0.537	-1.260	-1.893
	(0.889)	(1.201)	(0.501)	(3.710)	(0.925)	(3.271)	(1.197)
Housing Price Growth ($t-1$)	0.007*	0.017**	-0.001	-0.010	0.003	0.019	0.000
	(0.004)	(0.007)	(0.003)	(0.017)	(0.006)	(0.019)	(0.006)
Unemployment Rate ($t-1$)	-0.000	-0.000	0.006	-0.027	0.008	-0.024	-0.184***
	(0.011)	(0.016)	(0.008)	(0.032)	(0.013)	(0.044)	(0.014)
Personal Income Growth ($t-1$)	-1.815***	-2.481***	-0.447**	-2.426	-0.867	-1.870	2.672***
	(0.350)	(0.681)	(0.219)	(1.798)	(0.546)	(1.838)	(0.388)
Constant	0.024	0.154	0.028	-0.081	-0.091	0.288	0.557***
	(0.084)	(0.129)	(0.060)	(0.277)	(0.107)	(0.358)	(0.108)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Squared	-0.136	0.137	-0.315	-0.104	0.532	-0.355	0.917
Number of Observations	3,092	1,228	4,166	188	842	336	1,878

Notes: Robust standard errors are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents the estimates for a specific group of lenders: (1) Other (i.e., non-stress-test) Banks; (2) Independent Finance Companies; (3) Captive Finance Companies; (4) Fintechs; (5) MCAs; (6) Nonprofits; (7) Government.

4.2 Credit Relationship and Paycheck Protection Program Lending

Shortly after the onset of COVID-19, Congress passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act in in March 2020, dispensing broad-based fiscal assistance on an unprecedented scale. A key CARES provision was the Paycheck Protection Program (PPP), which provided forgivable loans to most small businesses whose operations were disrupted by the pandemic.[40] The loan amount was essentially "prescribed" in that it was capped at 2.5 months of average total monthly payroll costs up to $10 million, and virtually all the borrowers took the maximum allowed. The Small Business Administration (SBA), tasked with administering the PPP, provided the credit guarantee but delegated the underwriting to private lenders, in order to disburse funds as rapidly as possible. Therefore, borrower creditworthiness was not a concern; instead, how quickly a firm could access funding depended on whether its files already existed in an authorized lender's system. The first set of lenders approved were existing lenders under the SBA 7(a) program, consisted primarily of banks, but also other Depository Institutions (DIs) such as credit unions, Minority DIs and Community Development Financial Institutions (CDFIs). An extensive literature has documented that firms with existing relationships with banks were able to access funding earlier, which conferred a

[40] With only a few exceptions, "small" refers to businesses with up to 500 employees.

Table 9: Stress-Test Supply Shocks and Other Lenders in High-Minority Areas During the COVID Pandemic

Dependent Variable: Lending Growth Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Stress-Test Supply Shock	0.273	0.308	0.263**	1.545***	0.708***	-0.503	2.720***
	(0.170)	(0.243)	(0.129)	(0.583)	(0.200)	(0.682)	(0.226)
Supply Shock × High Minority	-0.698***	-0.568**	-0.479**	-0.006	-0.316*	-0.647	-0.114
	(0.240)	(0.249)	(0.192)	(0.383)	(0.183)	(0.634)	(0.266)
Supply Shock × Very High Minority	0.032	-0.846	-0.608	-0.313	-0.160	-0.011	-1.063
	(0.739)	(0.714)	(0.627)	(0.737)	(0.499)	(1.252)	(0.791)
Housing Price Growth ($t-1$)	0.007*	0.018***	-0.001	-0.009	0.003	0.020	0.000
	(0.004)	(0.007)	(0.003)	(0.017)	(0.006)	(0.019)	(0.006)
Unemployment Rate ($t-1$)	-0.008	-0.011	0.001	-0.030	0.003	-0.040	-0.187***
	(0.012)	(0.016)	(0.008)	(0.033)	(0.013)	(0.046)	(0.014)
Personal Income Growth ($t-1$)	-1.757***	-2.189***	-0.447**	-2.158	-0.797	-1.926	2.652***
	(0.349)	(0.674)	(0.219)	(1.690)	(0.531)	(1.824)	(0.388)
Constant	0.057	0.178	0.054	-0.086	-0.071	0.402	0.572***
	(0.085)	(0.130)	(0.060)	(0.273)	(0.106)	(0.364)	(0.108)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Squared	-0.131	0.135	-0.310	-0.109	0.535	-0.352	0.917
Number of Observations	3,092	1,228	4,166	188	842	336	1,878

Notes: Robust standard errors are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents the estimates for a specific group of lenders: (1) Other (i.e., non-stress-test) Banks; (2) Independent Finance Companies; (3) Captive Finance Companies; (4) Fintechs; (5) MCAs; (6) Nonprofits; (7) Government.

special advantage in the first two weeks of the program when there was enormous excess demand. In fact, funding was quickly exhausted after the program opened on April 3, 2020 so that lending had to be halted on April 16, awaiting additional congressional appropriation. The SBA then approved more lenders over Phases 2 and 3 of the program, including mostly nonbank lenders, many with the specific mission to improve credit access to SMEs in underserved areas.[41]

The notable changes in the configuration of credit sources for small businesses after the GFC documented above suggests that a bank-centric implementation scheme could disadvantage those SMEs whose prior credit relationships were primarily or exclusively with nonbank lenders, especially in terms of the speed of credit access. To investigate this hypothesis, we merged the UCC data with the PPP data by matching the borrowing firms' names. We managed to locate UCC records for 75% of PPP borrowers by count (8.8 out of 11.8 millions), as reported in the SBA's July 2021 PPP loan data release. These PPP borrowers represent 92% of the 2020 loan volume, and 81% of the

[41] Specifically, SBA Small Business Lending Companies, SBA Certified Development Companies, SBA Microlenders, Business and Industrial Development Corporations, Farm Credit System lenders, and state-regulated financial companies. Some in the last group were deemed fintech firms, such as Kabbage, which was regulated by MA and TX.

2021 loan volume. This is to be expected because more of the PPP borrowers are self-employed, who are much less likely to have taken out UCC loans.

We then estimate the following regressions to assess the impact of having an existing relationship before the pandemic with a given type of lender on the timing of PPP loan receipts:

$$T_{i,t}^{PPP} = \sum_j \sum_\tau \beta_{j,\tau} \mathbf{I}(\mathrm{UCC}_{j,\tau}) + \gamma X_i + \alpha_n + \alpha_c + \alpha_s + \alpha_a + \varepsilon_{i,t}, \tag{5}$$

where $T_{i,t}^{PPP}$ denotes the date borrower i received a PPP loan in year $t = 2020$ and 2021. For those borrowers that received more than one loan (above the 95^{th} percentile) in each year, it is the date of the first loan. An alternative measure of the 2020 PPP loan timing is the binary indicator that a firm received the first 2020 PPP loan early, defined as during phase 1 by April 16, 2020, when the initially appropriated funding was exhausted. It is well documented there was acute excess demand for PPP funds during this phase. The independent variable of interest $\mathbf{I}(\mathrm{UCC}_{j,\tau})$ is an indicator equal to 1 if firm i had a UCC loan from a type-j lender over period τ before 2020, and 0 otherwise, proxying whether i had pre-pandemic relationship with a type j lender. A pre-COVID period τ closer to 2020 approximates the notion that a borrower had a more active credit relationship when the pandemic hit.

X_i denotes a vector of borrower characteristics, including indicators for whether a borrower was located in an urban or a low-to-moderate-income Census tract. α_n, α_c, α_s, and α_a denote the fixed effects by industry (at the 3-digit NAICS level), county, firm-size bin and firm-age bin. Firms are classified into nine size bins based on the number of jobs reported on the PPP application: 1) single employee (that is, non-employer firm or self-employed), 2) 2–4 employees, 3) 5–9 employees, 4) 10–19 employees, 5) 20–49 employees, 6) 50–99 employees, 7) 100–249 employees, 8) 250–500 employees, 9) missing employment data.[42] Firms are classified into five age bins based on the business start year: 1) 2018 or 2019, 2) 2015–2017, 3) 2010–2014, 4) before 2010, or 5) missing age data.

Table 10 reports the coefficient estimates.[43] Column (1) considers the effect on the 2020 PPP loan date of the existence of a relationship, proxied as a borrower having taken out one or more loans with a type-j lender over the pre-COVID sample years (2007–2019).[44] It shows borrowers

[42]The exact number of jobs saved reported in PPP applications is subject to measurement errors, which should in principle be mitigated by discretizing into a few binned values.

[43]These regressions with multi-dimension fixed effects are estimated using Stata's reghdfe module; see Correia (2017).

[44]For those borrowers that received two loans (above the 95^{th} percentile) in each year, it is the date of the first loan. Firms that received more than two PPP loans or above 95^{th} percentile of pre-COVID UCC loan counts, which are generally much larger or specializing in businesses that rely heavily on UCC loans (such as equipment financing), are excluded, but the results are insensitive to including those large borrowers, as can be seen in Appendix Table A.3. All the standard errors are two-way clustered by industry and county.

Table 10: Effects of Pre-COVID Credit Relationship on PPP Timing

	2020 Loan Date		Early 2020 Loan	2021 Loan Date
	(1)	(2)	(3)	(4)
ST Bank Loans: Any Year	-0.162	-0.090	-0.006	0.430
	(0.163)	(0.142)	(0.004)	(0.320)
Other Bank Loans: Any Year	-3.448***	-2.745***	0.090***	-0.511*
	(0.140)	(0.120)	(0.005)	(0.283)
Fin. Company Loans: Any Year	0.269**	0.396***	0.001	1.697***
	(0.135)	(0.131)	(0.003)	(0.407)
Fintech, MCA Loans: Any Year	1.028***	0.913***	-0.035***	0.753
	(0.128)	(0.162)	(0.003)	(0.548)
Other NBFI Loans: Any Year	-0.615***	-0.443**	0.020***	0.168
	(0.179)	(0.183)	(0.004)	(0.542)
SBA Loans: Any Year	-1.852***	-1.481***	0.058***	0.131
	(0.283)	(0.302)	(0.007)	(1.554)
All Oth. Gov. Loans: Any Year	1.190***	1.642***	-0.038***	-1.647***
	(0.322)	(0.286)	(0.004)	(0.284)
ST Bank Loans: 2017-19		-0.105	-0.001	-0.101
		(0.115)	(0.002)	(0.331)
Other Bank Loans: 2017-19		-1.732***	0.048***	-1.299***
		(0.095)	(0.003)	(0.303)
Fin. Company Loans: 2017-19		-0.198	0.004	-0.654***
		(0.132)	(0.003)	(0.222)
Fintech, MCA Loans: 2017-19		0.237	-0.006*	-0.386
		(0.198)	(0.004)	(0.557)
Other NBFI Loans: 2017-19		-0.394	0.009	0.586
		(0.262)	(0.007)	(0.466)
SBA Loans: 2017-19		-1.497***	0.029**	0.376
		(0.519)	(0.014)	(3.174)
All Oth. Gov. Loans: 2017-19		-1.161***	0.014***	1.419***
		(0.215)	(0.005)	(0.319)
LMI Tract	2.707***	2.707***	-0.016***	4.474***
	(0.328)	(0.328)	(0.003)	(0.510)
Urban Tract	1.907***	1.904***	-0.205***	2.796***
	(0.284)	(0.284)	(0.011)	(0.371)
R^2_a	0.205	0.205	0.217	0.256
Observations	4345348	4345348	4345348	2009302
Industry Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Firm Size Fixed Effects	Yes	Yes	Yes	Yes
Firm Age Fixed Effects	Yes	Yes	Yes	Yes

Notes: This table reports the impact of having pre-COVID relationship with different types of lenders on the timing of receiving Paycheck Protection Program funding. Firms that received more than two PPP loans or above 95^{th} percentile of UCC loan counts are excluded. The dependent variable (LHS) for the first two columns is the date of the first 2020 PPP loan. The LHS for column (3) is the indicator that a firm received the first 2020 PPP loan early (i.e., by April 16, 2020). The dependent variable for column (4) is the date of the first 2021 loan. All the regressions include fixed effects by industry (at the 3-digit NAICS level), county, firm-size bin and firm-age bin. See Section 4.2 for definitions of size and age bins. Two-way clustered standard errors by industry and county in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

with prior relationship with banks other than those subject to stress testing received their 2020 PPP loans 3.5 days sooner than the omitted group—borrowers without prior relationship with any lenders in the UCC data. Relationship borrowers from stress-test banks, however, fared no better than the omitted group. This finding is consistent with results from studies of PPP's implementation early on Granja et al., 2022; Bartik et al., 2020, see, for example. Perhaps not surprisingly, given that PPP applications had to be submitted online to the SBA for approval, the other group of borrowers that received 2020 PPP loans faster were those that had received SBA loans before, often from an authorized SBA lender. In contrast, borrowers that had borrowed from finance companies received their 2020 PPP loans slightly later, while those that had borrowed from fintechs or other government agencies received funding even more slowly—a little over one day later. Recall that many among these credit institutions were not authorized to make PPP loans until at least Phase 2 starting on April 27, 2020.[45] This suggests that, among PPP borrowers without UCC records, some had relationship with banks (such as having their business transaction accounts with a bank), and might have even received credit, just not in the form of loans secured by assets that would warrant a UCC filing. Finally, borrowers from LMI and urban tracts received 2020 PPP funding nearly three days and two days later on average, respectively. The former pattern is consistent with the discrimination documented in Howell et al. (2024) and Chernenko and Scharfstein (2024), for example, while the latter likely reflects the greater funding demand in urban areas, which were much more adversely affected by COVID-19 during the initial surge.

Column (2) then adds a set of indicators for more recent relationship with UCC lenders, defined as having taken out one or more new loans over 2017–2019. As would be expected, a more accurate measure of active relationship with some types of lenders did confer an advantage. Specifically, borrowers with more active relationships with SBA lenders and non-stress-test banks enjoyed further expedited underwriting by 1.5 days or more, while more active pre-COVID borrowers in other government programs also received funds by one day sooner. At the same time, the advantageous effect of having ever borrowed from a UCC lender before 2020 diminishes in general. The delay suffered by LMI and urban borrowers remains the same.

[45]Puzzlingly, few finance companies signed up to make PPP loans, while many fintech lenders did and earned substantial profits. For instance, Blueacorn earned "over $1 billion in taxpayer-funded processing fees" (Congress, 2022) by helping disburse "$12.5 billion in SBA PPP funds" (`https://web.archive.org/web/20221201220227/https://blueacorn.co/about-ppp/`). One reason for this disparity may be that finance companies continued to make sufficient volumes of private loans, as shown above, and thus did not find it worthwhile to participate in PPP lending. Another plausible explanation is that only fintechs could profitably make PPP loans owing to lower processing or underwriting costs due to superior technology or perhaps lower expenditure on fraud prevention (Griffin et al., 2023). An intermediary could still choose to participate in the PPP even if it could not directly profit from making the loans per se, if it expected to benefit from potential future value of developing relationships with the borrowers. The higher the direct profit from making PPP loans, the less such expected future value would matter. Thus, given any potential future value, fintechs would be more likely to participate in the PPP owing to their greater direct profits.

Column (3) uses a linear probability regression and an alternative timing measure for 2020 PPP loans—equal to 1 if a borrower received a loan during phase 1 by April 16, 2020—to further capture the idea that speed carried a special premium during the acute initial phase of the outbreak. In general, the coefficients feature the opposite sign to their counterparts in column (2), except for prior relationships with fintech and MCA lenders, which did hurt a borrower's chance to receive 2020 PPP loans early. The fact that the relative magnitude of the coefficients in column (3) aligns roughly with that in column (2) indicates that the date differences uncovered in column (2) can be largely attributed to the delay during the first-draw phase.

Lastly, for comparison, column (4) reports the corresponding estimates for 2021 PPP dates. As would be expected, the advantage of having borrowed from a non-stress-test bank diminishes substantially, and the drawback of a fintech relationship disappears or even reverses if it was an active relationship. However, being a finance company borrower still meant slower receipt of funds. Somewhat puzzling is the finding that borrowers from urban and LMI areas experienced longer delays in 2021 than 2020. It is possible that a greater fraction of such borrowers in 2021 were self-employed without any prior credit relationship.

Appendix Table A.3 reports estimates from a companion set of regressions based on Equation (5) but with more granular lender types and inclusive of larger borrowers, to confirm that the findings reported in Table are robust. The patterns across the more finely distinguished lender types are broadly similar, with MCA borrowers suffering slightly more delays than the more typical fintech borrowers, while insurance company borrowers in fact close to on par with bank borrowers. Including those largest SMEs with many prior UCC loans or many PPP loans makes little difference.

A natural question is whether the effect of a pre-COVID credit relationship differs for borrowers from LMI areas. For a number of reasons, it is more likely than not that having an existing relationship would matter more for LMI borrowers. First, LMI areas tend to have fewer credit sources, making it more difficult for a SME to find a new lender. Second, most lenders likely expected a low present value of a relationship with most of such new borrowers, and thus had little incentive to make a PPP loan, unless their operation cost was sufficiently low to profit directly from making the PPP loan per se. In contrast, high credit risk, the usual rationale for not lending to such SMEs, should in principle be irrelevant for PPP loans.

To answer this question, we add to Equation (5) the following interaction term:

$$\sum_j \sum_\tau \theta_{j,\tau} \left[\mathbf{I}(\mathrm{UCC}_{j,\tau}) \right] X_i.$$

Table 11 reports the estimates from these regressions. As anticipated, having a prior credit relationship indeed enabled borrowers from LMI areas to access PPP funding sooner relative to their peers

Table 11: Effect of Pre-COVID Credit Relationship on PPP Timing: LMI Borrowers

	2020 Loan Date		Early 2020 Loan	2021 Loan Date
	(1)	(2)	(3)	(4)
ST Bank Loans: Any Year	0.269*	0.351***	-0.006	0.497
	(0.140)	(0.126)	(0.005)	(0.307)
Other Bank Loans: Any Year	-2.976***	-2.291***	0.087***	-0.727***
	(0.121)	(0.114)	(0.005)	(0.261)
Fin. Company Loans: Any Year	0.566***	0.765***	-0.002	1.985***
	(0.159)	(0.162)	(0.003)	(0.524)
Fintech, MCA Loans: Any Year	1.163***	1.042***	-0.031***	1.654***
	(0.128)	(0.147)	(0.003)	(0.630)
Other NBFI Loans: Any Year	-0.359	-0.200	0.017***	-0.148
	(0.220)	(0.202)	(0.004)	(0.512)
SBA Loans: Any Year	-1.565***	-1.146***	0.055***	0.280
	(0.276)	(0.303)	(0.007)	(1.927)
All Oth. Gov. Loans: Any Year	1.209***	1.678***	-0.042***	-1.934***
	(0.307)	(0.285)	(0.004)	(0.393)
ST Bank Loans: Any Year *LMI	-1.722***	-1.749***	0.000	-0.278
	(0.243)	(0.239)	(0.003)	(0.454)
Other Bank Loans: Any Year *LMI	-1.902***	-1.827***	0.012***	0.968**
	(0.229)	(0.217)	(0.002)	(0.394)
Fin. Company Loans: Any Year *LMI	-1.058***	-1.294***	0.010***	-1.183**
	(0.220)	(0.235)	(0.004)	(0.477)
Fintech, MCA Loans: Any Year *LMI	-0.364	-0.334	-0.012***	-2.773***
	(0.232)	(0.256)	(0.003)	(0.650)
Other NBFI Loans: Any Year *LMI	-1.186***	-1.093***	0.012**	1.758
	(0.266)	(0.306)	(0.006)	(1.271)
SBA Loans: Any Year *LMI	-1.046**	-1.181**	0.011	-0.788
	(0.422)	(0.510)	(0.014)	(4.046)
All Oth. Gov. Loans: Any Year *LMI	-1.176***	-1.296***	0.027**	2.269**
	(0.395)	(0.359)	(0.011)	(0.900)
LMI Tract	3.284***	3.285***	-0.018***	4.494***
	(0.392)	(0.392)	(0.003)	(0.514)
Urban Tract	1.923***	1.920***	-0.205***	2.789***
	(0.284)	(0.284)	(0.011)	(0.370)
R^2_a	0.206	0.206	0.217	0.256
Observations	4345348	4345348	4345348	2009302
Industry, County Fixed Effects	Yes	Yes	Yes	Yes
Firm Size, Age Fixed Effects	Yes	Yes	Yes	Yes
Additional Controls	No	Yes	Yes	Yes

Notes: This table reports the impact of having pre-COVID relationship with different types of lenders on the timing of receiving Paycheck Protection Program funding. Firms that received more than two PPP loans or above 95^{th} percentile of UCC loan counts are excluded. The dependent variable (LHS) for the first two columns is the date of the first 2020 PPP loan. The LHS for column (3) is the indicator that a firm received the first 2020 PPP loan early (i.e., by April 16, 2020). The dependent variable for column (4) is the date of the first 2021 loan. All the regressions include fixed effects by industry (at the 3-digit NAICS level), county, firm-size bin and firm-age bin. See Section 4.2 for definitions of size and age bins. Additional Controls: indicators of having one or more loans from a given type of lender over 2017 to 2019, and their interactions with the LMI indicator. Two-way clustered standard errors by industry and county in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

more than it did for non-LMI borrowers. As shown in column (1), for LMI borrowers, relationships with either stress-test banks or other banks in fact conferred the nearly the same timing advantage (receiving 2020 PPP funds faster by 1.7 versus 1.9 days, respectively). In contrast, for non-LMI borrowers, relationships with stress-test banks slowed down their funding receipt by over 3 days. Combining these coefficient differentials with their counterparts in column (3) indicates that LMI borrowers' faster loan receipt from stress-test banks was not because they were more likely to be approved during phase 1, when those banks made a small fraction of PPP loans, but because they received funds faster in phases 2 and 3, when stress-test banks caught up in their lending share. By comparison, the timing advantage enjoyed by LMI borrowers with other lenders was more due to the greater probability of receiving loans in phase 1. Only with SBA lenders did LMI borrowers not enjoy extra benefit of a pre-existing relationship, probably because those lenders were already lending to similar SMEs inside and outside of LMI tracts. Not surprisingly, the delay for those LMI borrowers without pre-existing credit relationships is estimated to be slightly, albeit insignificantly, longer (by half a day).

Moreover, it is the existence of a credit relationship before the pandemic that made the most difference, much more so than having an active relationship. This can be deduced by comparing the coefficients in columns (1) and (2), with the latter regression including additional controls of more recent loans (over 2017–2019) with each type of lender and respective interactions with the LMI indicator. The coefficients on the presence of relationship indicators barely move. By comparison, the patterns across lender types are more varied for 2021 PPP loan timing. Having a pre-existing relationship with banks in fact slowed down loan receipt (by over 1.5 days) by LMI borrowers than their non-LMI counterparts. This is likely because the CDFIs were authorized to start lending on January 11, 2020, while the other lenders started on January 19. The two exceptions are fintechs and finance companies: their existing borrowers in LMI tracts received funding sooner than their non-LMI peers, with the margin especially wide (by over 4 days) for fintech borrowers in LMI tracts.

In sum, these estimates confirm our conjecture that small businesses with little or no credit relationships with banks before the pandemic were at a disadvantage in accessing PPP funding on a timely basis. This handicap was especially meaningful for SMEs located in LMI areas. When combined with the earlier finding that banks, particularly stress-tested banks, retreated from making small business loans over the decade after the GFC, this implies that a larger share of SMEs relied on nonbanks for credit on the eve of the pandemic outbreak, exacerbating their difficulty of accessing the PPP during the initial acute phase. With the expansion of the approved lender pool, this problem was largely resolved in later phases of the program. Nevertheless, this experience offers a lesson on the design of public credit support program going forward: adequate consideration should be given to the composition of existing credit sources for the target group of recipients, assuming that private intermediaries are still expected to underwrite the loans. Alternatively, advances in digital technology, such as electronic payments and distributed ledgers, may enable more small businesses to directly access credit platforms administered directly by government agencies.

5 Conclusion

Using data from Uniform Commercial Code filings, this paper first examines broad trends in bank and nonbank lending to small businesses since the global financial crisis, with a focus on businesses located in underserved areas. Our analysis reveals that banks' role in small business credit diminished continually over the post-crisis era. The same period saw gains in market share for finance companies, merchant cash advance providers, and fintechs, defined as firms relying on new technologies in various aspects of lending such as applicant screen and loan underwriting. Our evidence suggests a causal link at the local market level between the retreat of large banks, owing to capital constraints imposed by stress testing, and the advance of nonbanks in the same market.

We then study small business lending during the COVID-19 pandemic, a massive shock with profound impact on small businesses. We find that, outside of government-sponsored emergency lending programs, both stress-test banks and new lenders contracted, while "traditional" nonbank lenders (chiefly finance companies) expanded. On the other hand, data reveal that the same causal link persisted, with new lenders *retreating less* from areas that were hit hardest by the contraction of credit from large banks. We also examine the experience of borrowers in accessing public credit supply via the Paycheck Protection Program. Our evidence indicates that borrowers with pre-existing relationships with banks, especially those not subject to stress testing, obtained emergency financing noticeably faster than both borrowers with no prior credit relationships or those with relationships with nonbank lenders. Pre-existing relationships were particularly important for borrowers located in low-to-moderate-income areas.

Our findings highlight the increasingly important role played by nonbanks in providing credit to small businesses, including those underserved ones. A general concern about more stringent bank regulation is that it reduces credit supply to businesses. Our analysis yields additional evidence for this adverse effect, consistent with related findings from previous studies. However, this credit-restraining effect was no more acute in underserved communities than in other localities. Moreover, we also find robust evidence that small-business borrowers were able to tap into substitute sources of credit, lessening the adverse impact from large banks' retreat. Nevertheless, our analysis also uncovers a potential drawback of the shift away from banks and toward nonbanks as sources of credit: it hampered some small businesses' access to public credit support during the pandemic because the program relied more on banks to underwrite the loans. Our finding suggests that the existing composition of private lenders should be taken into account in designing public liquidity programs for small businesses, assuming that private lenders are still expected to disburse the funds.

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A Internet Appendix

A.1 Details of the cleaning procedure

Before filtering the UCC data to original loans there were various initial cleaning steps to the raw UCC data worth noting. The data was subset to loan filings between 2006 and 2021 which brings the number of loan filings from 41,272,021 to 36,973,595. Also, since the DUN & Bradstreet DUNS number field for lenders in the UCC data is not well populated, variables containing the lenders' POI information was grouped to form a unique identifier of lenders within the UCC data called lender_geo_name_id. This variable is used extensively to merge lists of classified lenders back to the UCC loans data when categorizing different types of loans.

Since the UCC data contains other various types of loan filings such as amendments and terminations, the data was subject to duplicate loan filings with identical filing numbers and borrowers. The filing type variable was used to filter the data to only original loans by keeping rows where the filing type value was 2. This step brings the data from 36,973,595 loans filings to 17,913,991 filings. However, duplicate loan filings with identical information remained so further filtering was required to have a dataset with unique original loans. After tagging loans with the same filing number and borrower using its DUN & Bradstreet DUNS number, the duplicate where its respective lender is not the assignee of the loan is dropped by dropping duplicates where its assignee indicator value is "N" if there is another duplicate observation where its assignee indicator is "Y." This step removes another 809,501 loan filings. Duplicates with no collateral information or an empty collateral type field are also dropped, which removes an additional 3,931 filings. Finally, if any further duplicate filings with the same borrower DUNS, lender_geo_name_id exist, they are dropped. These cleaning measures result in a dataset with 17,100,559 original loan filings.

A.2 Collateral Codes

Table A.1 lists the types of collateral found in UCC filings.

Table A.1: Collateral Types in the UCC Data

Code	Collateral Type
1	Equipment
2	Fixtures
3	Inventory
4	General Intangibles
5	Chattel Paper
6	Contract Rights
7	Accounts Receivable
8	Computer Equipment
9	Machinery
10	Business Equipment
11	Unspecified
12	Negotiable Instruments
13	Farm Products
14	Vehicles
15	Construction Equipment/Machinery
16	Agricultural Equipment
17	Assets
18	Accounts
19	Notes Receivable
20	Cosigned Merchandise
21	Buildings
22	Real Property
23	As Specified
24	Industrial Equipment/ Machinery
25	Timber
27	Building Materials
28	Communication Equipment
29	Oil, Gas & Minerals
31	Textile Goods
34	Proceeds
37	All Assets
40	Mobile Homes
99	Other

A.3 Additional Statistics

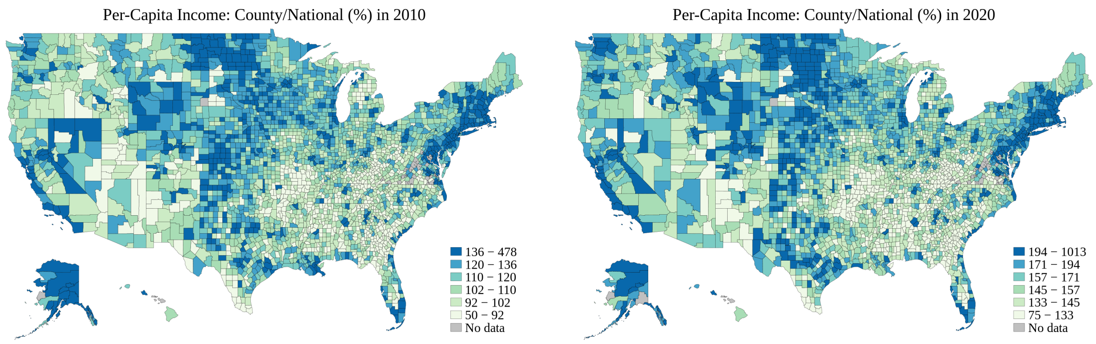

Figure 10: County per-capita income normalized by national average, 2010 and 2020.

A.4 Additional Results of Credit Relationship on PPP Loan Timing

	2020 Loan Date		Early 2020 Loan	2021 Loan Date	
	(1)	(2)	(3)	(4)	(5)
ST Bank Loans: Any Year	-0.066	-0.097	-0.011**	0.130	0.018
	(0.135)	(0.129)	(0.004)	(0.282)	(0.299)
Other Bank Loans: Any Year	-2.468***	-2.577***	0.080***	-0.326	-0.339
	(0.133)	(0.128)	(0.005)	(0.250)	(0.237)
Fin. Company Loans: Any Year	0.445***	0.420***	-0.002	1.587***	2.206***
	(0.124)	(0.126)	(0.003)	(0.302)	(0.349)
Fintech, MCA Loans: Any Year	0.892***	0.995***	-0.037***	0.854	0.806
	(0.164)	(0.149)	(0.002)	(0.519)	(0.532)
Other NBFI Loans: Any Year	-0.164	-0.239	0.014***	0.181	0.497
	(0.175)	(0.169)	(0.004)	(0.567)	(0.539)
SBA Loans: Any Year	-1.370***	-1.524***	0.058***	1.852	-0.134
	(0.347)	(0.322)	(0.007)	(1.405)	(1.568)
All Oth. Gov. Loans: Any Year	1.762***	1.845***	-0.040***	-1.533***	-0.894***
	(0.350)	(0.283)	(0.004)	(0.466)	(0.277)
ST Bank Loans: 2017-19	-0.124	-0.142	-0.005***	0.103	-0.080
	(0.108)	(0.104)	(0.002)	(0.336)	(0.361)
Other Bank Loans: 2017-19	-1.458***	-1.496***	0.038***	-0.905***	-1.208***
	(0.086)	(0.090)	(0.003)	(0.307)	(0.320)
Fin. Company Loans: 2017-19	-0.089	-0.107	-0.001	-0.496**	-0.555**
	(0.086)	(0.089)	(0.002)	(0.199)	(0.225)
Fintech, MCA Loans: 2017-19	0.254	0.271	-0.006	-0.446	-0.323
	(0.202)	(0.216)	(0.004)	(0.500)	(0.536)
Other NBFI Loans: 2017-19	-0.288	-0.330	0.007	0.942*	0.544
	(0.257)	(0.260)	(0.007)	(0.560)	(0.489)
SBA Loans: 2017-19	-1.425***	-1.417***	0.030**	-0.197	0.155
	(0.524)	(0.531)	(0.014)	(3.568)	(3.571)
All Oth. Gov. Loans: 2017-19	-0.990***	-0.980***	0.006	0.756**	0.814**
	(0.207)	(0.222)	(0.005)	(0.326)	(0.325)
ST Bank ≥ 2 Loans: Any Yr	0.081	0.107	0.009***	0.661**	1.606***
	(0.108)	(0.100)	(0.002)	(0.284)	(0.340)
Other Bank ≥ 2 Loans: Any Yr	-0.523***	-0.535***	0.031***	-0.710*	-0.456
	(0.112)	(0.114)	(0.003)	(0.379)	(0.400)
Fin. Company ≥ 2 Loans: Any Yr	-0.018	0.008	0.004	-1.829***	-1.455***
	(0.100)	(0.100)	(0.003)	(0.205)	(0.295)
Fintech, MCA ≥ 2 Loans: Any Yr	-0.239	-0.293	0.002	0.106	-0.303
	(0.170)	(0.184)	(0.004)	(0.538)	(0.561)
Other NBFI ≥ 2 Loans: Any Yr	-0.713***	-0.693***	0.016***	-0.588	-0.759
	(0.196)	(0.218)	(0.005)	(0.546)	(0.496)
SBA ≥ 2 Loans: Any Yr	0.817	0.743	-0.024	1.810	2.129

	(0.685)	(0.688)	(0.018)	(4.257)	(4.543)
All Oth. Gov. ≥ 2 Loans: Any Yr	-0.367	-0.363	-0.001	-1.201***	-1.146***
	(0.282)	(0.258)	(0.005)	(0.295)	(0.214)
ST Bank ≥ 2 Loans: 2017-19	0.129	0.148	0.003	-1.849***	-0.879
	(0.151)	(0.154)	(0.004)	(0.590)	(0.593)
Other Bank ≥ 2 Loans: 2017-19	-0.539***	-0.531***	0.013***	-0.027	-0.098
	(0.135)	(0.130)	(0.003)	(0.371)	(0.347)
Fin. Company ≥ 2 Loans: 2017-19	-0.231	-0.229	0.010*	0.392	0.515
	(0.208)	(0.200)	(0.005)	(0.385)	(0.424)
Fintech, MCA ≥ 2 Loans: 2017-19	0.040	0.036	-0.001	-0.439	-0.127
	(0.175)	(0.176)	(0.004)	(0.825)	(0.877)
Other NBFI ≥ 2 Loans: 2017-19	-0.074	-0.076	0.007	0.227	0.351
	(0.399)	(0.392)	(0.011)	(0.915)	(0.904)
SBA ≥ 2 Loans: 2017-19	-2.175*	-2.066*	0.026	1.691	1.160
	(1.142)	(1.139)	(0.050)	(10.210)	(9.865)
All Oth. Gov. ≥ 2 Loans: 2017-19	-0.299	-0.248	0.020***	2.873***	2.655***
	(0.250)	(0.255)	(0.005)	(0.428)	(0.387)
LMI Tract	3.369***	2.707***	-0.016***	4.012***	4.474***
	(0.469)	(0.328)	(0.003)	(0.568)	(0.510)
Urban Tract	1.851***	1.902***	-0.205***	2.782***	2.806***
	(0.344)	(0.284)	(0.011)	(0.382)	(0.373)
R^2_a	0.225	0.206	0.217	0.250	0.256
Observations	4840288	4345348	4345348	3708086	2009302
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Size Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Age Fixed Effects	Yes	Yes	Yes	Yes	Yes

Table A.3: Effects of Pre-COVID Credit Relationship on PPP Timing

Notes: This table reports coefficient estimates of the impact of having pre-COVID relationship with different types of lenders on the timing of receiving Paycheck Protection Program funding. The sample underlying column (1) includes all borrowers, whereas the sample underlying all the other columns exclue firms that received more than two PPP loans or above 95^{th} percentile of UCC loan counts, which are generally much larger. The dependent variable for the first two columns is the date of the 2020 PPP loan. For those borrowers that received two loans (above the 95^{th} percentile) in each year, it is the date of the first loan. The dependent variable for column (3) is the binary indicator that a firm received the first 2020 PPP loan early, defined as during the first draw by April 16, 2020, when the initially appropriated funding was exhausted. The dependent variable for column (4) is the date of the first 2021 loan. All the regressions include fixed effects by industry (at the 3-digit NAICS level), county, firm-size bin and firm-age bin. See Section 4.2 for definitions of size and age bins. Two-way clustered standard errors by industry and county in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.