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Did High Leverage Render Small Businesses Vulnerable to the COVID-19 Shock?

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Abstract:
Using supervisory data on small and mid-sized nonfinancial enterprises (SMEs), we find that those SMEs with higher leverage faced tighter constraints in accessing bank credit after the COVID-19 outbreak in spring 2020. Specifically, SMEs with higher pre-COVID leverage obtained a smaller volume of new loans and had to pay a higher spread on them during the pandemic period. Consistent with an inward shift in loan supply, these effects were concentrated in loans originated by banks with below-median capital buffers. Highly levered SMEs that relied on low-capital large banks for funding before the pandemic were not able to substitute to other sources of debt financing and thus experienced more of a reduction in total debt as well as a decline in investment and employment. On the other hand, the unprecedented public support, especially the Paycheck Protection Program (PPP), mitigated the adverse real effect stemming from bank credit constraints.

JEL Classifications: G21, G28, G32
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Introduction

The nonfinancial business sector entered the COVID-19 pandemic with an unprecedented high level of leverage, based on the debt-to-income ratio.[1] Arguably, the level of pre-pandemic debt was supported by adequate income. However, the sharp contraction in economic activity after the onset of COVID-19 in 2020 caused widespread concern that high leverage could amplify the adverse impact of the initial shock. Small and mid-sized enterprises (SMEs) were particularly exposed to liquidity risk, since it is well documented that they face greater financial frictions (see, for example, Gertler and Gilchrist, 1994) that hamper their ability to access credit needed to smooth their cash flows.[2] Thus, for SMEs, such credit constraints carry the risk of excessive liquidation, as they might be forced to close even if they are viable over the long run. The resulting deadweight loss could be considerable, since SMEs account for a nontrivial share of investment and employment.

Even though the role of nonfinancial business leverage in the transmission of shocks has important policy implications, empirical findings about this role are relatively scarce, in part due to the lack of firm-level data for SMEs. In this paper, we fill this gap by studying how leverage affected SMEs' ability to access bank credit, as well as their investment and employment, in the aftermath of a severe macroeconomic shock—the COVID outbreak.[3] Our empirical analysis exploits detailed financial information on nonfinancial business borrowers obtained from confidential supervisory data (FR Y-14Q) that contain virtually all commercial and industrial (C&I) loans originated by large US banks.[4] Important for our goal of analyzing SMEs, this data set contains balance sheet and income statement information of borrowers, most of which are *private* firms, allowing us to study the role of leverage in the propagation of shocks for these typically hard-to-observe firms.

We exploit the large exogenous shock related to the COVID pandemic to identify the role of leverage in the amplification of shocks. The value of this "natural experiment," however, is tempered by the unprecedented magnitude of the public policy response: The short-lived but extremely deep crisis induced by the COVID shock elicited extraordinary measures by

[1] Throughout this paper, our primary measure of leverage is total debt over income, with income equal to earnings before income, taxes, depreciation, and amortization (EBITDA) by convention.

[2] Not only do SMEs generally have no access to public debt markets, recent studies (Chodorow-Reich et al., 2021, in particular,) also show that, when compared with large firms, smaller firms are more subject to bank discretion in accessing liquidity: They are less likely to have lines of credit, face shorter maturities, and routinely utilize higher fractions of such lines.

[3] There is no standard definition of SMEs across different studies. As we describe below, we mostly identify SMEs as those firms with annual revenues of no more than $500 million.

[4] These data cover every corporate loan and lease with a committed balance greater than or equal to $1 million originated by US bank holding companies with $50 billion or more in total assets. Throughout the paper, we refer to "Y-14 banks" as those US bank holding companies required to report FR Y-14Q data to the Federal Reserve.

both fiscal and monetary authorities. Recognizing the particular vulnerability of SMEs, in part due to their greater difficulty in accessing private funding, policymakers enacted several novel public support programs targeting such nonfinancial firms. Particularly relevant were the Paycheck Protection Program (PPP), which issued loans-cum-grants to businesses with 500 or fewer employee; loan modification programs, which incentivized banks to work with borrowers in preventing delinquencies; and the Main Street Lending Program (MSLP), which provided credit to mid-sized firms. In our analysis, we account for the first two of these public programs in response to the pandemic; data limitations prevent us from evaluating the role of the MSLP. This helps us estimate whether the funding support from the public sector mitigated the impact of the shock on firms' real outcomes. Moreover, it allows us to identify the effects of leverage as a shock amplifier more generally, so that our findings can be applicable to more ordinary circumstances.

Our key results are as follows. First, we find that, unconditionally, the volume of new loan originations contracted by 50 percent after the COVID shock in March 2020, while loan interest rate spreads on newly originated loans increased by 20 basis points (bps), from a 200 bps pre-pandemic average to 220 bps in the second quarter of 2020. Comparing loans made to firms in the same industry and month, priced to the same type of base rate (such as the LIBOR or prime rate), and accounting for differences in other financial ratios, we find that SMEs with higher leverage obtained lower volume and paid higher spreads on new loans during the COVID crisis. Moreover, these effects were concentrated in loans from banks with relatively low capital buffers (relative to the required ratio), consistent with the interpretation of a more severe credit supply contraction for highly levered SMEs. This occurred even though bank capital ratios seemed generally quite healthy relative to the required level and much higher than the level during the financial crisis in 2008. Our estimates suggest that "excess" bank capital—relative to the regulatory requirement—mattered independently of the absolute level of the capital ratio. According to our baseline estimates, loan volume contracted by an additional 5 percentage points, while loan spreads rose by 4 basis points more, for a one standard deviation (3.42) increase in borrowers' pre-pandemic leverage.

Further analysis shows that the differential effect of leverage on the volume and spread of new SME loans cannot be explained by firms' credit ratings (assigned by the loan-originating bank) or differences in loan collateral. This finding suggests that low-capital banks reduced credit to firms with high leverage not simply because those loans had worse credit ratings that triggered additional regulatory capital charges, but because leverage provided a finer measure of credit risk. Our results are also robust to controlling for differences in the liquidity position of the borrower (including undrawn loan commitments), confirming a distinct effect of leverage on loan volume and spread. Moreover, the adverse effects of high leverage during the COVID crisis were not concentrated in industries especially susceptible to the pandemic-induced disruption to economic activity, but were instead present across all industries.

At the firm level, we find that the supply contraction in bank lending binds and reduces overall credit availability for highly levered SMEs. This effect is especially pronounced for high-leverage SMEs that have stronger ties to less capitalized banks—that is, those firms obtaining more than half of their total debt from low-capital banks. We find that after the onset of COVID, these SMEs suffered a substantial decline in both loans from FR Y-14Q banks and total debt, even when we control for other firm characteristics and the industry and state in which a firm operates. This result bolsters the case that the decline in firm borrowing was due to a contraction in bank credit supply. At the same time, the decline in debt funding did not coincide with a commensurate reduction in the SMEs' cash and cash-equivalent holdings (including unused lines of credit). This suggests the possibility that firms deemed it important to preserve a certain degree of liquidity and thus had to forego some of the funding for other purposes.

Last, but just as importantly, we examine the effects of high leverage on real activity by comparing firms' investment and employment decisions before and after the COVID crisis depending on their preexisting leverage. For highly levered SMEs that were subject to the bank credit crunch, we find a larger decline in investment and employment in 2020 after the onset of COVID. Interestingly, we find that funding from the PPP and credit modifications, including extensions, were able to mitigate—entirely offset, in fact—the adverse effects of leverage on investment and employment among these SMEs. For identification of these effects, we exploit the PPP's eligibility criterion to instrument program participation and the remaining maturity of firm loans coming into the pandemic to instrument modifications. Our IV estimates are larger than their OLS counterparts and point to a severe contraction in real activity of highly levered firms that could not extend bank debt or substitute public money for lost bank funding: Firms with a 1x higher leverage ratio experienced a 1.6 percentage point larger drop in investment and a 5 percentage point larger drop in employment.

The remainder of the paper proceeds as follows. Section 1 reviews the related strands of literature to which we contribute. Section 2 describes the data sets used in this study. Section 3 reports our empirical findings. Section 4 concludes.

1 Related Literature

Our study relates to an extensive body of research examining the impact of business leverage and the associated financial frictions for businesses' ability to access new credit, sensitivity to shocks, cyclical fluctuations, growth prospects, and survival probability. It also joins a growing number of studies analyzing the impact of the extraordinary array of public support programs enacted in the wake of the COVID-19 outbreak.

First, our paper follows in the footsteps of several studies demonstrating that high leverage, and hence the heavy reliance on external finance, renders firms' real activity more vulnerable to disruptions to credit supply. For example, Almeida et al. (2012) show that firms with long-term bonds maturing at the peak of the Global Financial Crisis (GFC) in late 2008 cut investment significantly more than those with bonds maturing afterward. Furthermore, Duchin et al. (2010) find that, following the onset of the GFC, investment declined more for firms that had low cash reserves or high net short-term debt, were financially constrained, or operated in industries dependent on external finance. Kalemli-Özcan et al. (2018) quantify how much of the sluggish post-crisis investment by European firms can be attributed to high leverage, versus credit constraints due to banks being weakened by sovereign default risk, versus weak aggregate demand. Chodorow-Reich (2013) presents evidence that supply-side credit constraints can also curb employment. Similar to these existing studies, we also find evidence that credit supply tightened more for firms with higher leverage after an adverse shock. Unlike these studies, most of which rely on the clear shock to bank capital and hence loan supply stemming from the GFC, we examine bank credit contraction that occurred at a time when bank capital mostly was deemed adequate (relative to the minimum requirement, which will be discussed further below) and in response to a public health scare.

Leverage was also recognized as a source of general vulnerability in the early days of the COVID crisis, based on stock prices and public filings of large firms. Ding et al. (2021) and Alfaro et al. (2020), for instance, find that higher leverage was associated with steeper stock price declines early in the COVID outbreak. Similarly, Fahlenbrach et al. (2020) find that firms with greater financial flexibility (chiefly less debt or more cash) experienced milder stock price declines. Kovner et al. (2020) estimate that highly levered firms faced a higher risk of becoming insolvent compared with their less levered peers because their debt repayment obligations constituted additional fixed costs that their COVID-induced diminished income might not have been able to cover. Our study, in contrast, seeks to estimate the impact of the COVID pandemic on the numerous small, private firms by utilizing a data source that covers a large share of such firms.

Second, our paper also contributes to an extensive literature that studies financing frictions faced by SMEs and their consequences. It has long been established that borrowing constraints are more likely to be binding for small firms, most of which rely on bank loans for external financing. The debt-based propagation mechanism, partly intermediated through banks, thus operates more prominently with small firms. Gertler and Gilchrist (1994) show that small firms account for a disproportionate share of the manufacturing decline and inventory slowdown that follows monetary policy tightening. Bord et al. (2021) find that the supply shock to small-business bank loans crimped the launch of new firms and hurt the growth of firms with fewer than 20 employees.[5] More recently, using FR Y-14Q data, Chodorow-Reich et al. (2021) find that, compared with large firms, SMEs are less likely to have committed lines of credit, face shorter maturities on such lines, post more collateral, have higher utilization rates, and pay higher spreads.[6] Similarly, Greenwald et al. (2021) document that, unlike SMEs, large firms are able to tap into their existing credit lines following adverse shocks. Unlike these studies, we focus on the quantity and terms of *new* loan originations to SMEs during a crisis, paying special attention to the role of borrower leverage. Moreover, we analyze how shifts in credit conditions affect SMEs' investment and employment.

Third, our findings extend the understanding of the importance of bank capital for loan supply, especially following adverse shocks. Pioneering studies in this literature include those of Peek and Rosengren (1997) and Peek and Rosengren (2000), who provide causal evidence that an adverse shock to bank capital reduces lending.[7] Several studies conducted in the years after the financial crisis estimate the effect of enhanced capital regulation on banks' lending decisions; most of them examine specifically the effect of the additional capital buffer implied by the Federal Reserve's stress test. In particular, both Cortés et al. (2020) and Berrospide and Edge (2019) find that banks facing higher de facto required capital ratios (due to larger forward-looking capital losses given the stress scenarios) cut their commercial and industrial (C&I) lending.[8] Acharya et al. (2018) explicitly test between two competing explanations for banks' lending response to stress tests and find evidence for what they term the Risk Management Hypothesis, whereby banks reduce loans to more risky firms in order to manage credit risk, instead of increasing loan supply to riskier firms (referred to as the Moral Hazard Hypothesis).[9] Most closely related to our study is the one by Berrospide

[5]Greenstone et al. (2020), on the other hand, find that contraction in banks' small-business lending exerted no significant negative impact on employment at local small businesses.

[6]Consistent with the hypothesis that SMEs are subject to greater lender discretion, SMEs were less likely to draw down available lines even when they faced the same abrupt disruption of revenues as large firms did when COVID-induced lockdown measures were put in place. PPP recipients reduced non-PPP loan balances, indicating the program alleviated the liquidity shortfall of SMEs that received the loans.

[7]It is beyond the scope of this paper to provide a comprehensive survey of this literature, so we note only a few important studies, including Ivashina and Scharfstein (2010).

[8]Berrospide and Edge (2019) use FR Y-14Q data, as we do, but they do not distinguish between SMEs and large firms, nor do they explore heterogeneous impact by borrower characteristics, such as leverage.

[9]This risk-management behavior is the opposite of zombie lending (also referred to as evergreening) by banks to preserve capital, as is demonstrated in Caballero et al. (2008). These two mechanisms are not necessarily exclusive. In fact, for the purpose of preserving capital, banks may well combine rolling over loans that are *already troubled* (that is, evergreening) with reducing the share of *new originations* to risky borrowers.

et al. (2021), who also explore the role of total bank capital buffers in SME lending after the COVID outbreak. They find that, even though all banks entered the pandemic with capital ratios clearly above the regulatory requirement, those with relatively lower ratios vis-à-vis the required buffers nonetheless reduced loan commitments to SMEs. We likewise consider the impact of different levels of capital buffers across banks on their lending decisions, although we focus on new originations, which are more subject to bank discretion. Furthermore, we show the interactive effect between bank equity and firm equity: During a crisis, lower bank capital also leads to a *differential* change of bank loan supply to firms of differing default risk—low-capital banks exhibited a more pronounced reduction in credit to highly levered firms.

Fourth, our paper also joins a growing number of studies analyzing the effect of the array of public funding and lending programs put in place in the wake of the COVID outbreak to help firms weather the income shock and retain workers. By far the best-studied public funding program is the Paycheck Protection Program (PPP). In one of the earliest studies, Bartik et al. (2020) use an instrumental variables (IV) approach to better estimate the causal effect of the PPP and find a modest boost to employment, as do Granja et al. (2020). Joaquim and Netto (2021) develop a model to formalize the intuition that banks would optimally choose to make PPP loans first to firms that benefit those banks the most. They find that these firms happen to be existing customers, larger firms, and firms less affected by the pandemic. This reconciles the seemingly disparate estimates in the existing studies attributed to differing degrees of bias due to the endogeneity. Doniger and Kay (2021) use the 10-day delay in appropriation for the PPP as an instrument and estimate a large impact of PPP on employment, especially for the smallest firms and the self-employed.[10] Our study is the first, as far as we are aware, to estimate the impact of the PPP on SMEs' investment, in addition to its impact on SMEs' employment, using an IV approach. We also analyze the effect of the debt relief measures authorized by the Coronavirus Aid, Relief, and Economic Security (CARES) Act and supported by banking regulators.

2 Data Sources

The primary data source for this study is the financial information on borrowers reported in the supervisory FR Y-14Q (henceforth Y-14) data set on mostly commercial and industrial (C&I) loans, because it enables us to analyze not only public firms but also a large number

[10]Using the same instrument, Kurmann et al. (2021) further show that the decline in employment is mostly accounted for by temporary closures of the smallest firms. By comparison, Autor et al. (2022), using ADP data, estimate a much smaller PPP impact on employment for firms near the 500-employee threshold.

of private, and often small, firms. Banks with more than $100 billion in total consolidated assets are subject to the mandated Dodd-Frank Act Stress Tests (DFAST) and are required to report facility-level information on corporate loans and leases on a quarterly basis to the Federal Reserve.[11] Specifically, reporting of the Y-14 C&I schedule started in 2012:Q2 and covers corporate loans and leases with a committed balance of at least $1 million. These data include detailed facility-level information such as type of loan, interest rate, and maturity. More importantly, they also cover borrower balance sheet and income statement information, irrespective of borrower size or ownership status.[12]

Borrowers covered in the Y-14 data span a wide range of the firm-size distribution, and most are small and mid-sized private businesses, which in this study we define as firms with annual sales of no more than $500 million. In contrast to this paper, most previous studies of firm financing and investment rely largely on Compustat data on financial statements of public firms filed with the Securities and Exchange Commission (SEC), along with Dealscan data on individual loan attributes. These data thus cover only public firms, and their borrowing from banks is dominated by the largest loans. While public firms represent most of the bank credit volume, the sample of firms in Compustat leaves out an important segment of the firm-size distribution and thus cannot be used to study small or even mid-sized firms, many of which rely on bank loans as a primary source of external funding. The Y-14 data set therefore is necessary for the purpose of this study, which is to understand the effects of leverage on access to credit and real activity for firms across the size distribution, especially small and mid-sized private businesses, which generally do not have access to market finance.

In our analysis of bank lending, we study only loans that were originated in 2019 and 2020, in order to compare loan amounts and terms before and after the COVID outbreak using a relatively symmetric length of time around the shock. Furthermore, we restrict our sample to nonfinancial firms (that is, we exclude firms with the two-digit NAICS industry code of 52) to focus on the effects of the pre-pandemic leverage ratio on the credit access and real activity of nonfinancial firms after the pandemic started.

Data on Y-14 borrowers that also borrowed from the Paycheck Protection Program come from the Small Business Administration (SBA) data release on July 2, 2021. We use information on only those borrowers that received loans in 2020, since our sample period ends in 2020:Q4. Employment data on the Y-14 borrowers were obtained from the Dun & Bradstreet

[11] "Banks" here is a shorthand for large bank holding companies (BHCs) domiciled in the United States and foreign-owned intermediate holding companies (IHCs) with substantial presence in the United States that are subject to the DFAST.

[12] The reported data are confidential supervisory information, but the Y-14 reporting form and instructions, including the list of variables collected by the Federal Reserve, are publicly available at `https://www.federalreserve.gov/apps/reportforms/Default.aspx`.

(D&B) database.

3 Empirical Findings

3.1 Leverage and New Loan Originations

The COVID-19 outbreak and the drastic containment measures enacted in response to it caused an abrupt plummet in sales and income for many firms. Some degree of income loss may be mostly an inconvenience for a lightly levered firm, especially if it has a preexisting credit line to tap into or can easily access credit markets, but the same magnitude of income loss can spell disaster for a firm that is already highly levered. These concerns fueled, and were at the same time exacerbated by, the turmoil in credit markets at the onset of the pandemic, when corporate bond spreads increased dramatically.

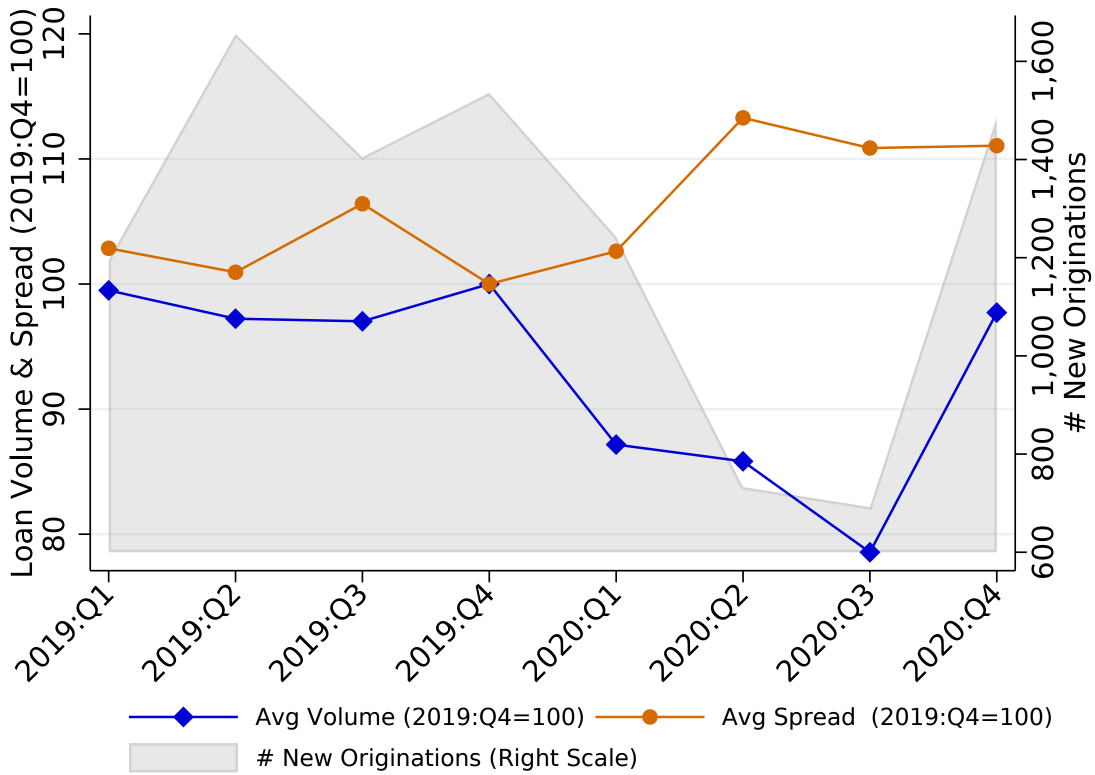
In response to the credit market turmoil, the Federal Reserve, with the approval of the US Treasury, quickly established several liquidity and debt purchase facilities to restore the normal functioning of the credit markets. Indeed, several studies find that conditions in the corporate bond market normalized rapidly after the announcement of the primary and secondary corporate credit facilities (PMCCF and SMCCF, respectively) on March 23, 2020.[13] Subsequently, conditions in the corporate bond market improved so much that the overall volume of new issuance in 2020 surpassed that of 2019.

In contrast, survey evidence from the same period—for example, the Senior Loan Officer Opinion Survey on Bank Lending Practices—suggests that banks tightened their lending standards and terms noticeably over 2020:Q2 and 2020:Q3. This implies that credit conditions for small firms most likely deteriorated notably relative to the conditions for large firms, with credit conditions easing back to normal for those large firms that were able to access the bond market but tightening for smaller firms, which typically do not have access to public debt markets. We thus start our analysis with an empirical examination of the behavior of bank lending to SMEs following the onset of the COVID pandemic to evaluate the extent to which the credit market may have bifurcated.

Figure 1 shows a substantial drop in the number of new loan originations to SMEs by the largest US banks after the COVID shock hit the United States in 2020:Q1. The sharp decline in the number of new loans (extensive margin of credit) is accompanied by a reduction in average loan volume and an increase in the average loan spread on granted loans (intensive margins). During the COVID period, the number of quarterly loan originations dropped

[13] O'Hara and Zhou (2021), in particular, examine the Federal Reserve's role as market maker of last resort during the COVID-19 corporate bond liquidity crisis.

Figure 1: Bank Loan Volume and Interest Rate Spread for SMEs



Note: The figure plots the average volume (blue diamonds) and average interest rate spread (orange dots) of new loan originations to firms with less than $500 million in annual sales (SMEs). Both series are at the quarterly frequency and indexed to 100 in 2019:Q4. The shaded area shows the number of quarterly new originations (right scale).

by more than 50 percent relative to 2019:Q4. At the same time, the data show that, on average, the new SME loans made each quarter during the COVID period were as much as 20 percent smaller by volume and had spreads that were as much as 12 percent greater. (The figure is based on a sample of all loans with different types of rate indexes, but results hold if we restrict the sample to loans with similar indexes, such as the three-month LIBOR.) The tightening of SMEs' bank credit extended from the initial shock throughout 2020, although in 2020:Q4 conditions improved somewhat. In contrast to SMEs, large firms initially increased their bank borrowing (both in terms of the number of loans and the size of the loans) at constant rates in 2020:Q1 but then faced a contraction on all three margins of credit, as shown in Appendix Table D.1. Taken together, the evidence on prices and the evidence on quantities suggest that the COVID shock induced a bank credit supply shock, specifically for SMEs, that lasted through the autumn of 2020, substantially longer than the brief bond market turmoil at the onset of the crisis.

Our first set of regression analyses addresses the core objective of this study: to determine if or how conditions of bank credit during the pandemic evolved differently for SMEs depending on their pre-pandemic leverage. The empirical analysis is grounded on a simple model that focuses on the interaction between firm leverage and bank capital in response to a negative shock, which we elaborate on in Appendix A.[14] We estimate how much the volume and interest rate spread of newly originated loans are affected by a borrowing firm's pre-COVID leverage, controlling for other relevant loan, firm, and bank characteristics. We analyze newly originated loans instead of outstanding loans because loan terms (such as the total committed amount of a credit line and the interest rate spread) are set at the time of origination. New originations are thus much more likely to be subject to supply constraints, as lenders have much greater discretion regarding loan size and terms than with drawdowns under existing credit lines, which are largely determined by credit demand.[15] We estimate total changes in debt, that is, including adjustments on the extensive margin, in the next section.

Our main regression at the individual loan level is given by:

$$\text{Credit Margin} = \beta \cdot \text{COVID} \times \text{Leverage} + X'\gamma + \epsilon, \tag{1}$$

where Credit Margin refers to loan amount (in logs), or interest rate spread (in basis points over the LIBOR) of a newly originated loan. Leverage is the borrower's debt-to-income ratio measured using data from financial statements as of the year-end prior to the loan origination, with income equal to earnings before interest, taxes, depreciation, and amortization (EBITDA).[16] We focus on this measure of leverage because Greenwald (2019) shows that most loan contracts feature a covenant that places a ceiling on debt balance over earnings, and this debt-over-earnings covenant is more likely to be binding compared with other covenants (such as one on the interest coverage) during periods of low interest rates. COVID is a dummy variable that equals 1 after March 15, 2020, and 0 otherwise. Our key coeffi-

[14] An important element of the model is to explain why banks are more likely to cut lending to riskier borrowers if they are concerned about falling below the required capital buffers not only today but also, or plausibly even more so, in the *future*. We show that the three types of costs that may be incurred by banks dipping into their capital buffer as described in Berrospide et al. (2021) do not necessarily imply a reduction in loans to riskier firms. Instead, a more plausible explanation rests on riskier loans raising the odds of a bank's capital buffer eroding in the future.

[15] This does not preclude some lender discretion over drawdowns, as shown in Chodorow-Reich et al. (2021). Nevertheless, the lending banks are bound to varying degrees by terms set at the time of origination.

[16] Note that we focus on gross leverage as the key leverage variable of interest, while controlling for borrower liquidity (that is, cash and market securities) simultaneously in all the regressions. This is a more flexible specification than using net leverage (that is, debt net of cash and securities) instead as the regressor, which would impose the implicit constraint that the coefficients on gross leverage and on cash have the same magnitude but opposite signs. In unreported results, we find that this implicit constraint is generally not satisfied, and the coefficient on net leverage is mostly insignificant.

cient of interest is β, which measures the change in bank credit conditions after the COVID outbreak in relation to a firm's pre-COVID leverage.[17]

The vector X collects the control variables along with leverage and the COVID indicator individually. First, in all the regressions, we control for other firm financial information that is considered relevant for banks' credit decisions. This includes both the (log) level and the annual growth rate of sales, profit margin (defined as the ratio of operating income over total assets), asset tangibility (defined as the share of property, plant, and equipment in total assets), and liquidity (defined as the share of cash plus marketable securities in total assets). All these firm attributes are measured as of the last year-end prior to the loan origination date, in order to maximize data availability and consider them pre-determined.[18] We also control for the type of base rate (such as the LIBOR or prime rate) in all the regressions, since different types of rates correspond to noticeably different average levels of the rate spread.

Perhaps more importantly, we include in most of the regressions a full set of three-digit NAICS industry-by-month fixed effects. These specifications thus estimate the effects of firm leverage on bank loan quantity and spread *within* a given industry*month, as the industry-by-month fixed effects account for any systematic differences across industries over time. Industry-by-month fixed effects absorb not only unobserved steady-state average differences across industries, but also differences in leverage due to industry-specific pledgeability of assets or differences in revenue cyclicality, for example. The industry-by-month fixed effects also account for the possibly of a different evolution of industry conditions during the pandemic.

We also include state-by-month dummy variables to account for time-varying state heterogeneity in economic conditions during the pandemic (such as those related to differences in mobility restrictions) that may influence a firm's productive activity and thus credit demand. In addition, we include bank*month fixed effects to absorb heterogeneous time-varying supply effects across banks, which encompass, but are not limited to, the impact of a high volume of drawdowns on credit lines to large firms at the onset of the pandemic (as documented in Greenwald et al., 2021) and the capital buffer constraint analyzed in Berrospide et al. (2021).

Because the dependent variables as well as several explanatory variables of firm financial ratios (including leverage) contain extreme outlier values, we trim the sample to remove observations with values in the top and bottom 1 percentiles. We also focus on firms with

[17] Because the COVID indicator equals 1 after March 15, 2020, its own coefficient is identified with only loans originated from March 15 through April 1, 2020, in any specifications that include time fixed effects.

[18] In Y-14 data, firm financial data as of Q4 each year are by far the most available. All our specifications include time fixed effects to control for the effect of information staleness.

Table 1: Sample Statistics for Regressions of New Loans to SMEs

	Count	Mean	Std. Dev.	p5	p25	p50	p75	p95
Loan Outcomes								
Volume ($M)	8,292	11.34	17.18	1.00	2.00	4.50	13.62	43.00
Spread (BPS)	8,292	205.66	108.84	0.00	150.00	200.00	275.00	400.00
Borrower Characteristics								
Leverage (Debt/EBITDA)	8,292	3.40	4.44	0.00	0.83	2.22	4.34	10.42
Net Sales ($M)	8,292	126.7	128.8	7.7	27.0	72.7	190.6	408.3
Profit Margin (Income/Assets)	8,292	0.13	0.15	-0.01	0.04	0.09	0.17	0.43
Tangibility (PPE/Assets)	8,292	0.33	0.28	0.01	0.07	0.26	0.53	0.88
Liquidity (Cash+Equiv./Assets)	8,292	0.08	0.10	0.00	0.01	0.04	0.11	0.31

Note: Loan volume and sales are in million dollars. Spread is in basis points (BPS). Leverage is the ratio of total debt over EBITDA. Profit margin equals operating income over total assets. Tangibility equals the value of property, plant, and equipment over total assets. Liquidity equals cash plus marketable securities over total assets.

positive leverage (that is, positive income), since loans to firms with negative income, which tend to exhibit notably higher growth rates, may be structurally different. See Table 1 for the summary statistics of loan characteristics, along with the borrowing firms' financial ratios for the new-loans sample underlying the regressions for loan volume and interest rate spread.[19] Overall, our sample includes more than 8,000 loans to SMEs, with an average loan value of $11 million (median of $4.5 million) and an average spread of 205 basis points (median of 200 basis points) over the base rate. The average annual sales value of the borrowers in our data set is $127 million (median of $73 million), and the average leverage, measured as debt to income, is 3.4 (median 2.2). We next present the regression output that links credit conditions to borrower leverage. We omit estimated coefficients of all fixed effects and control variables from the output tables for brevity in order to focus on the main coefficients of interest—those measuring the effects of leverage on bank credit during the COVID pandemic. All standard errors are multi-way clustered at the industry, state, and bank levels, allowing for a wide range of correlation structures in the residual terms.

In Table 2, we present the regression results for the loan volume and interest rate spread of newly originated loans to SMEs during the crisis depending on firm leverage. Column (1) shows that higher leverage is associated with a significantly smaller loan volume after we control for other relevant firm financial ratios and the interest rate index type of the loan. Column (2) demonstrates that this result is robust to the inclusion of the large set of fixed effects described above, in particular industry*month and state*month dummy variables to account for the heterogeneous impact of the pandemic across industries and locales. By com-

[19] See Appendix B for a detailed list of the cleaning steps taken to prepare the data for regression analyses.

Table 2: Effects of Leverage on New Bank Loan Originations to SMEs

	Log(Volume), x100			Spread, BPS		
	(1)	(2)	(3)	(4)	(5)	(6)
COVID*Leverage	-2.135***	-1.590***		1.531*	1.254*	
	(0.604)	(0.522)		(0.787)	(0.640)	
COVID*Leverage*Low Capital Bank			-1.872***			2.270***
			(0.644)			(0.597)
COVID*Leverage*High Capital Bank			0.155			0.248
			(0.857)			(0.840)
Observations	8,292	8,292	8,292	8,292	8,292	8,292
R-squared	0.278	0.581	0.582	0.201	0.617	0.618
Industry*Time FE	No	Yes	Yes	No	Yes	Yes
State*Time FE	No	Yes	Yes	No	Yes	Yes
Bank*Time FE	No	Yes	Yes	No	Yes	Yes
Rate Index Type FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Group Difference Est.			2.027			-2.022
Group Difference P-value			0.0331			0.0091

Note: Coefficient estimates for Log(Volume) regressions are multiplied by 100. Spread is in basis points (BPS). Leverage is the ratio of total debt over EBITDA. COVID is an indicator that equals 1 from March 15, 2020, to the end of 2020, and 0 otherwise. Low Capital Bank is an indicator that equals 1 if a bank's capital buffer is below the median. Robust standard errors multi-way clustered at the state, industry, and bank level are in parentheses; ***, ** and * denote significance at 1%, 5% and 10% levels, respectively.

parison, columns (4) and (5) report that borrowers with higher leverage paid higher spreads on loans originated during the COVID period. The point estimate, in fact, turns significant at the 10 percent level once we control for our full set of fixed effects. The estimated coefficients in columns (2) and (5) indicate that a one-standard-deviation higher leverage (an increase in debt-to-EBITDA by 3.42) is associated with an additional 5 percentage point relative decline in loan size during the pandemic and an additional 4 basis point increase in spreads, respectively.[20]

In Appendix Figure D.2, we show the dynamic effect of leverage on the volume and spread of new loan origninations in every quarter over the sample period. Specifically, we refine our empirical model underlying columns (2) and (4) of Table 2 by interacting leverage and the firm controls with quarterly dummy variables instead of the single COVID indicator. This approach allows us to test for the parallel pre-trend assumption and to study the richer dynamics in SMEs' credit condition around the outbreak of the COVID crisis. The estimates

[20]Unlike with SMEs, leverage does not matter for the volume and spread of new loans to large firms (those with annual sales exceeding $500 million) during the pandemic, as shown in Appendix Table D.1. This is consistent with the aggregate finding that credit conditions during COVID deteriorated less so for large firms.

indeed show no significant pre-trend, validating our empirical approach. Moreover, the results highlight the decrease in loan volume and the increase in loan spreads as of 2020:Q2. Point estimates for 2020:Q2 and 2020:Q4 indicate a stronger decline in loan volume and increase in spreads than the average change after the COVID outbreak reported in Table 2.[21] Overall, this dynamic analysis validates our inference from Table 2. Nevertheless, in all subsequent analyses, we continue to focus on the specification with dichotomous pre- versus post-COVID period as our baseline, because such pooling allows for a more robust analysis.

We also consider alternative measures of leverage. Tables D.2 and D.3 in the Appendix report the estimates when leverage is measured by the interest coverage ratio (ICR) or the debt-to-assets ratio, respectively. A higher pre-COVID ICR exhibited no impact on the loan size and interest rate spread on new loans originated after the COVID shock hit, whereas a higher pre-COVID debt-to-assets ratio significantly curtailed loan size and raised loan rate spread (albeit not significantly). Combining these patterns across the three leverage measures, it appears that the banks were concerned about borrowers' preexisting high debt balance (relative to income especially), but not high pre-COVID debt servicing burden. One likely reason that the ICR exerted insignificant impact is that, unlike the debt-to-income ratio, the ICR is also affected by the interest rate charged on loans, and interest rates tend to be compressed by Fed interventions in response to adverse shocks. This dynamic became even more pronounced following the COVID outbreak: The risk-free base rate fell with the policy rate cuts, while sharp increases in the interest rate premium were contained and later (by the end of 2020) fully reversed by measures taken by the Fed to stabilize the risky assets markets (such as the corporate bond purchase facilities). This interest-rate-based mitigating effect is absent for both the debt-to-income and the debt-to-assets ratios. Between the two measures, a high debt-to-income ratio showed greater impact during this episode, likely because the pandemic was primarily a massive adverse shock to income. We therefore focus on the debt-to-income leverage ratio exclusively in all the subsequent analyses.

Our finding that highly levered borrowers experienced a larger decrease in volume for newly originated loans along with a larger increase in spreads during the pandemic suggests that the loan market dynamics after the COVID shock hit were, on net, dominated by a credit supply contraction.[22] To further substantiate this interpretation, we next examine more closely the role of loan supply factors, that is, the condition of banks such as their capital and liquidity ratios. Indeed, a large literature highlights the importance of bank capital for lending, as reviewed above. In particular, better bank capitalization tends to

[21] On the other hand, there was no significant change in loan spreads in 2020:Q3.

[22] While demand may well have also shifted inward more for the highly levered SMEs, the extra increase in loan spreads suggests that the contraction in the supply of loans to them dominated.

foster greater willingness to supply credit following adverse shocks. Given our regression specification, such general effects of a bank's health (as reflected in bank capital or liquidity ratios) on its overall credit supply (as found in, for example Berrospide et al., 2021), are subsumed in the estimated bank fixed effects.

Instead, we focus on the role of bank capital in driving the differential change in loan volume and spread after the COVID shock in connection with borrower leverage. This analysis, which emphasizes the differential variation in credit conditions across borrowers within a bank, follows the same logic as in those existing studies that highlight the risk-taking channel whereby poorly capitalized banks reduce exposure more substantially to riskier borrowers.[23] Specifically, we allow the baseline effect of firm leverage to differ depending on a bank's capitalization. Our measure of bank capital is the so-called management—or excess—capital buffer, defined as the common equity tier 1 capital ratio net of all the required capital buffer for a given bank, which equals the sum of the minimum regulatory capital ratio (of 4.5 percent), the capital conservation buffer as determined by the US implementation of the Basel III Capital Accord, and the G-SIB surcharge if applicable.

The resulting estimates, as reported in columns (3) and (6) of Table 2, show that the adverse effects of leverage found so far on credit to highly levered SMEs are concentrated entirely in loans made by banks with a relatively low capital buffer (that is, below the median value). In contrast, well-capitalized banks (that is, those with an above-median excess buffer) did not curtail supply more severely to borrowers with higher leverage, all else being equal. This finding indicates that, even though banks went into the COVID-induced downturn with notably higher levels of capital when compared with previous downturns (especially the financial crisis), low levels of bank capital relative to the required buffer still appear to have restrained bank credit supply in the aftermath of a major adverse shock.[24]

One potential concern with the supply-constraint interpretation is that borrower characteristics, in particular their sensitivity to adverse shocks, may differ systematically across low- versus high-capital banks. Such differences in borrower composition across banks could, in principle, explain the estimated differential effects of leverage across banks. Note, however, that our regressions are already saturated with granular industry*time and location*time

[23] As noted above, cutting back on new loans to riskier but still solvent borrowers (which is the focus of our analysis) can coexist with bank loan evergreening, whereby banks keep rolling over already delinquent loans, as both actions help banks preserve capital. The rate of delinquency is exceedingly low in the Y-14 data, so evergreening is likely to be a rather minor consideration in general for Y-14 loans. During the COVID crisis, evergreening may have been even less likely owing to the unprecedented scale of public sector support, which made it less costly for banks to drop riskier borrowers because such firms could obtain public funding assistance.

[24] See Berrospide et al. (2021) for a discussion of several likely costs to banks that resulted in their reluctance to run down the excess buffer in order to expand the credit supply.

fixed effects, so any residual systematic difference would have to be within each industry-month or state-month.[25] In Appendix Table D.4, we report conditional summary statistics of the loan portfolio of high- versus low-capital banks to assess the degree of compositional differences in borrowers. While we do find that firms borrowing from low capital banks tend to be less levered than borrowers from high capital banks, we find no economically relevant evidence of sorting of borrowers across low- versus high-capital banks along the profitability, tangibility, or liquidity dimension.[26] Moreover, we also control for several borrower attributes relevant for credit risk, in addition to the large set of fixed effects, which should further mitigate concerns about unobserved borrower heterogeneity. Finally, our joint investigation of prices and quantities further provides insights into supply versus demand factors. In sum, the lack of systematic differences in borrower attributes between the two groups of banks along with the extensive set of controls support a supply-side interpretation of our findings above: The larger decline in quantity and increase in spread experienced by borrowers with higher leverage is more attributable to lower-capital banks restricting credit supply due to concerns about maintaining their capital buffers.

A natural question that emerges asks why low-capital banks reduced their lending to more levered SMEs. An obvious answer would be that higher leverage raised the default risk of a loan more in the wake of COVID, and the higher odds of expected future loss may have been particularly costly to low-capital banks, as they were already closer to the required capital buffer.[27] Put differently, what is restraining *current* lending is the greater likelihood of breaching required capital buffers in the *future*. The logic is similar to the Risk Management Hypothesis described in Acharya et al. (2018), in that banks limit risk today in order to lower the odds of experiencing future losses and reduce the size of any losses that are sustained. It is worth noting, however, that there is a subtle distinction. Even though the capital buffer associated with stress testing is based on forward-looking losses that could materialize under the stress scenario over the forecast horizon, such losses are nonetheless built into the de facto capital buffer and thus become *known* information at

[25]For instance, one may be concerned that firms that borrowed from low-capital banks may have had demand that was somehow, on average, more sensitive to the COVID shock such that their loan demand also tended to contract more in the aftermath. Note that the COVID shock led to highly unusual disruptions when compared with the typical economic downturn: Services that involve personal contact were hit especially hard, contracting much more severely than manufacturing, which is exactly contrary to what happens in a typical recession. Given that we are already controlling for industry-month fixed effects, any remaining difference between high- versus low-capital banks in terms of borrower vulnerability to COVID would have to be within each three-digit NAICS industry and month.

[26]For some of the variables, we find statistically significant means, but the economic magnitudes are very similar across the two groups.

[27]As noted in Berrospide et al. (2021), the cost of breaching the capital buffer consists largely of restrictions on capital distribution and compensation.

the moment when the stress test results are disclosed to banks. In the aftermath of the COVID outbreak, however, potential future losses were not known to banks, or anyone else. Therefore, any bank actions in connection with their current capital positions were taken based on *unobserved* expectations of future losses.

As discussed above, our chosen measure of leverage is more likely to become binding following income loss, especially in a low-interest rate environment, compared with other measures of leverage such as interest coverage. While covenant violations do not have a mechanical impact on capital, banks need to recognize a higher likelihood of default when a covenant is breached, since the borrower is now in technical default. Banks thus generally have to increase provisions in order to accumulate more allowances for loan losses, which reduces net income and and in turn lowers capital accumulation. Moreover, greater default risk also could lead to a credit rating downgrade, resulting in higher regulatory capital charges. Both types of negative prospective impact on capital are likely more costly for banks with a comparatively low capital buffer.

To directly test whether higher leverage is serving as a proxy for worse credit risk, we add as additional controls each loan's credit rating (as a dummy variable) assigned by the lending bank (according to its internal risk model) at origination. The rating information reported in the Y-14 data is based on a common scale comparable to those used by the rating agencies, mapped from equivalent bank-specific rating grades. As with leverage, these rating indicators are also interacted with the COVID indicator to capture the potentially differential impact of loans' ratings during the pandemic.

In our investigation of the role of leverage as a proxy for default risk, we facilitate the interpretation of coefficients by restricting the sample to loans originated by low-capital banks, since the detrimental effects of leverage found so far are concentrated in these loans.[28] Columns (1) and (5) in Table 3 report the estimates for the subsample of loans from banks with a low capital buffer using the same baseline specification as in Table 2. The estimated effects of leverage are somewhat smaller (in absolute value) than those of their counterparts in Table 2 but still highly significant both statistically and economically. Columns (2) and (6) show that the inclusion of ratings-based controls does not substantially change the point estimate and significance of the deleterious effects of leverage during the pandemic. That is, even within the same rating grade, SMEs with higher leverage obtained smaller loans and paid higher spreads in 2020 after COVID hit. Thus, the regulatory cost due to higher-risk weights and associated capital charges on lower-rated loans is unlikely to be the main reason why low-capital banks were reluctant to lend to firms that had higher leverage. More likely,

[28]Econometrically, this is, in fact, a more stringent specification because it allows all coefficients to vary between the subsamples of loans from low- versus high-capital banks.

Table 3: Role of Leverage, Credit Rating, and Loan Collateral in Bank Loans to SMEs

	Log(Volume), x100			Spread, BPS		
	(1)	(2)	(3)	(4)	(5)	(6)
COVID*Leverage	-1.713**	-1.741*	-1.553**	2.069***	1.814***	2.030***
	(0.582)	(0.931)	(0.661)	(0.602)	(0.553)	(0.613)
Observations	5,765	5,765	5,765	5,765	5,765	5,765
R-squared	0.613	0.614	0.617	0.630	0.642	0.632
Industry*Time FE	Yes	Yes	Yes	Yes	Yes	Yes
State*Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank*Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Rate Index Type FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
COVID*Rating	No	Yes	No	No	Yes	No
COVID*Collateral Type	No	No	Yes	No	No	Yes

Note: This table shows the robustness of the baseline estimates to controlling for credit rating and loan collateral type. Coefficient estimates for Log(Volume) regressions are multiplied by 100. Spread is in basis points (BPS). Leverage is the ratio of total debt over EBITDA. COVID is an indicator that equals 1 from March 15, 2020, to the end of 2020, and 0 otherwise. The sample is restricted to loans originated by low-capital-buffer banks. Robust standard errors multi-way clustered at the state, industry, and bank level are in parentheses; ***, ** and * denote significance at 1%, 5% and 10% levels, respectively.

it is because leverage provides a more granular signal about default risk or expected loss beyond what is embedded in the comparatively coarser rating system.[29]

In exploring the role of leverage as a proxy for default risk, we also add controls for the type of collateral backing a loan (along with their interactions with the COVID indicator), since loans to more levered firms may have different types of collateral pledged, which in turn can affect loan volume and spread. Moreover, previous studies show that collateral is a prevalent risk mitigant for loans to SMEs, and loans backed by different types of collateral react differently to shocks (see, for example, Caglio et al., 2021).[30] The types of collateral as

[29]Another possible, data-specific explanation is that the ratings data (from banks' internal risk models) in Y-14 are somehow less timely than the data on firm leverage (from financial statements on borrowers). While the exact dates of the financial reports on borrowers are provided in Y-14, it unfortunately does not report the dates when loans' ratings were assigned. Even though we would expect a loan rating to be up to date at the time of a new origination, we cannot rule out the possibility that the internal rating is more stale than the borrower leverage ratio.

[30]Also using Y-14 data, Caglio et al. (2021) document that essentially all bank loans to private firms, most of which are SMEs, are secured by some form of collateral, the most important types being accounts receivable and inventory, and blanket liens. The common property uniting these types of collateral is that they are all created intrinsically as a result of a firm's operations; that is, they are embedded in the firm's value as a going concern instead of tied to an asset (such as equipment or real estate) with independent value even without the firm's operations.

classified in Y-14 data include real estate and fixed assets, cash and marketable securities, accounts receivable and inventory, and blanket liens. Interestingly, for small and large private firms, each type of collateral's share in newly originated loans remained roughly the same during the COVID period as it was before the pandemic (see Figure D.4 in the Appendix), even though a priori one might have expected the accounts receivable and inventory share as collateral to decline given the COVID-induced damage to firm operations.[31] In contrast, there was a noticeable increase during the pandemic in the share of loans to large public firms that were unsecured (most of which was offset by the lower share of loans secured with blanket liens). This hints at a change in the extensive margin of bank lending after the COVID shock, in that more loans were shifted to large public firms that could borrow without posting collateral.

Columns (3) and (7) in Table 3 report the effects of leverage during the pandemic, controlling for the type of collateral. We again find that the main coefficient of interest remains quantitatively unchanged, as in the case when ratings-based controls are added.[32] Taken together, the estimates reported in Table 3 indicate that whatever risk signal borrower leverage represented was above and beyond (more granular, for example) the information content already embedded in the loan risk rating and the type of collateral pledged. It is nevertheless plausible that higher leverage was regarded as an extra indicator of greater risk, which discouraged banks with a comparatively weaker capital buffer from lending to those firms during the pandemic.

Since the pandemic was much more disruptive to some industries (such as those involving in-person services) than to others, we might expect that banks exhibited greater aversion to lending to highly levered SMEs in those industries damaged by COVID. On the other hand, firms in such industries may have received more financial support from public programs (such as the PPP) and thus did not become more risky on net than firms in other industries. We consider the effects of the public support in the next section, but first we explore if high leverage impeded credit availability more for SMEs in industries hit harder by the pandemic by interacting our main variable of interest—the interaction between leverage and the COVID-period indicator—with a dummy variable that flags whether a firm belongs to an industry that was severely affected by the pandemic. Table 4 reports the estimates when we consider travel and leisure, oil and gas, transportation, entertainment and recreation, apparel

[31] One likely explanation is the anecdotal evidence that, after COVID broke out, banks lengthened the period over which the value of accounts receivable would be assessed, effectively making it easier for firms to satisfy this collateral requirement.

[32] When collateral and rating controls are simultaneously included, the point estimates remain essentially the same and still highly significant for spreads. But the coefficient becomes insignificant for explaining loan amount, as the standard error widens by 50 percent. These estimates are omitted for brevity.

Table 4: Differential Effects by Industry due to COVID Vulnerability

	Log(Volume), x100	Spread, BPS
	(1)	(2)
COVID*Leverage	-1.919***	2.011***
	(0.559)	(0.564)
COVID*Leverage*Impacted Industry	-4.880	4.092
	(3.431)	(3.724)
Observations	5,765	5,765
R-squared	0.614	0.631
Industry*Time FE	Yes	Yes
State*Time FE	Yes	Yes
Bank*Time FE	Yes	Yes
Rate Index Type FE	Yes	Yes
COVID*Firm Controls	Yes	Yes

Note: Coefficient estimates for Log(Volume) regressions are multiplied by 100, thus interpreted as percentage difference. Spread is in basis points (BPS). Leverage is the ratio of total debt over EBITDA. COVID is an indicator that equals 1 from March 15, 2020, to the end of 2020, and 0 otherwise. Impacted Industry is an indicator that equals 1 if a borrower's six-digit NAICS industry code is classified by the Chicago Fed as having had "severe" or "substantial" impact from COVID-19 shock, and 0 otherwise. The sample is restricted to loans originated by low-capital-buffer banks. Robust standard errors multi-way clustered at the state, industry, and bank level are in parentheses; ***, ** and * denote significance at 1%, 5% and 10% levels, respectively.

and textile manufacturing, automobile production, retail, media, and services as industries severely affected by COVID. Coefficients on the baseline term (COVID*Leverage) retain both the magnitude and significance, whereas coefficients on the triple interaction term are insignificant for both loan volume and interest rate spread (albeit of the same sign as the main term). This indicates that low-capital banks reduced credit to highly levered SMEs across all industries, not just those particularly vulnerable to COVID-induced disruptions.

3.2 Impact of Firm Leverage on Total Debt Financing, Investment, and Employment

Our analysis so far has uncovered evidence of supply constraints faced by highly levered firms in obtaining bank credit, which is by far the primary source of external financing for SMEs (e.g., Bräuning et al., 2021a). Financing, however, is ultimately a means to an end, enabling businesses to carry out the optimal level of productive activity efficiently. We also care about leverage because of the potentially deleterious effect of high leverage on firms' optimal decision regarding real activity, particularly in response to a negative shock. We therefore move on to analyze whether high pre-COVID leverage hampered the overall availability of debt to SMEs at the firm level and in turn adversely affected their real activity, especially investment and employment.

By examining the dynamic of total debt at the firm level following the onset of the COVID-19 pandemic, we gain an essentially complete picture of the evolution of external financing for these SMEs. This external financing includes not only loans from Y-14 banks but also loans from other banks, as well as debt from all other sources.[33] Note that our analysis so far pertains only to the intensive margin of newly originated Y-14 bank loans, as they are based on loans *actually made*, whereas the net change in total Y-14 loans outstanding also includes the extensive margin—the balance would fall if a firm did not renew a maturing loan.[34]

In addition to analyzing total debt changes, we devote the most attention to the impact of high leverage on real outcomes at the firm level—specifically investment and employment. We also examine how firms adjusted internal funds (represented by cash and marketable securities) in conjunction with the change in total debt, as well as changes along the other dimensions that require funding, accounts receivable and inventory in particular.

Given the evidence presented above of a reduction in credit supply to highly levered firms *at the loan level* emanating from low-capital Y-14 banks, we hypothesize that highly levered SMEs relying more heavily on Y-14 banks for credit were more likely to be constrained in overall external financing of real activity during the pandemic. The main reason for this constraint was that these firms would have had difficulty switching to other sources of funds. Thus, for identification, we focus our analysis on the Y-14–reliant SMEs, defined as those SMEs with 50 percent or more of their total debt in 2019 consisting of loans from one or more Y-14 banks.[35]

When analyzing the relationship between pre-crisis levels of leverage and firm-level real outcomes, we must take into account the public programs that were designed to provide economic relief to firms (and households) after the COVID outbreak. Indeed, the unprecedented scale and speed of support provided by the public sector in response to the pandemic is arguably a unique feature of the ensuing downturn. Specifically, Congress passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act swiftly, providing funding

[33]These other sources include loans and advances from nonbank financial intermediaries and the bond market, although bond financing is generally irrelevant for SMEs. Equity issuance is also rarely available to SMEs for external financing.

[34]On the other hand, the change in total loan balance also includes changing drawdowns under existing commitments, which are determined more by a borrower's demand for funds.

[35]As a placebo test, we also estimate the same regressions for firms that obtained only a small share of their debt in 2019 from Y-14 banks and find little evidence of overall credit constraint, as expected. These results are presented in the appendix. While other, non-Y14 banks may also have been capital constrained, we do not observe the loans by these banks and thus cannot cleanly test credit supply shifts.

and relief to businesses, especially SMEs, experiencing hardship due to the imposition of containment measures. Three of the public programs in particular targeted SMEs to varying degrees: the Paycheck Protection Program (PPP), the Main Street Lending Program (MSLP), and the loan forbearance measures.

In our analysis, we consider the effects of two programs most pertinent for the SMEs in our data: loan forbearance measures and the PPP. Loan forbearance measures, in the form of extensions or modifications, were introduced to preserve the existing loan balances by incentivizing lenders to prevent some of the existing loans from being accelerated or terminated due to covenant violations or missed payments during the pandemic.[36] As a result, a firm's outstanding balance would stay higher than otherwise, even without the injection of new debt. Particularly relevant for our analysis is the possibility that the incidence of such loan extensions or modifications depended on borrower leverage or lender capitalization, since the banks had discretion over whether and which forbearance measures to offer. By preventing some of the existing loans from becoming delinquent, such forbearance likely in turn affected borrowers' investment and employment decisions.[37] It may have diminished the degree to which firms cut back on their real activity.

By comparison, the PPP is by far the better-studied program to date, as noted above. It offered low-cost loans-cum-grants to small businesses; it was widely anticipated that most of the loans would be forgiven because the conditions for forgiveness were deemed easy to meet (see Appendix C for details on the features of the program). As a result, even though a PPP loan would increase the borrower's debt balance before it was formally forgiven, a loan did not raise the true debt burden, or it did so only minimally at most. Therefore, the marginal value of the fresh injection of funds was likely higher for SMEs that entered the pandemic with a higher leverage, because the PPP substituted for private funding that otherwise would have cost more or even been unavailable.

The third credit-support program that may have had an impact on SMEs is the MSLP, which extended a total of \$17.5 billion in loans to nearly 2,500 borrowers (e.g., Bräuning et al., 2021b). While substantial when compared with that of the bond-buying programs, the total uptake of MSLP funding was modest when compared with its maximum capacity or the PPP uptake. Moreover, the Y-14 banks were not active participants in the MSLP, thus the number of matched MSLP borrowers in our data is too small to obtain any meaningful

[36]For details about these forbearance programs, which consist of a CARES Act provision and an interagency statement offering further guidance, see Appendix C.

[37]During periods of low interest rates, debt-over-earnings covenants are more likely to be binding compared with other covenants ((see Greenwald, 2019)). Furthermore, earnings-based covenants are likely to trigger widespread technical defaults on loan contracts in the wake of a shock such as the COVID-19 pandemic that hinders some firms' ability to generate sales or even halts revenue inflow entirely. Such covenant violations per se could have had a significantly disruptive impact on the operations of borrower firms.

results, and so we omit the MSLP from the baseline analysis.

Considering all of the aforementioned factors, we study the impact of high pre-COVID leverage on firm-level outcomes after the outbreak with the following regression specification:

$$\text{Firm Outcome} = \beta \cdot \text{COVID} \times \text{Leverage} + X'\gamma + \epsilon, \tag{2}$$

where Firm Outcome includes the total number of Y-14 bank loans, total debt, cash, accounts receivable and inventory, capital expenditures, and employment. To facilitate comparing the magnitude of coefficients across different uses of funds, all the dependent variables except employment are normalized by the stock of plants, property, and equipment (PPE) at the end of the preceding year.[38] In these firm-level regressions, we include as controls essentially the same set of firm characteristics as in the loan-level analysis and interact all of these controls with the COVID-period indicator.[39] Because firm financial statements for the majority of companies are available only at year-ends (that is, Q4 of each year), we use annual data in 2020 and 2019 for our analysis based on Equation (2).[40] The COVID-period indicator thus equals 1 in 2020 and 0 in 2019.[41] All the firm-level controls are lagged by one year. As in the loan-level analysis, we saturate the firm-outcome regressions with industry*time fixed effects and state*time fixed effects to account for the influence of time-varying conditions across industries and states on the outcome variables during the COVID period. Thus, we identify the effect of leverage on firm-level outcomes during the pandemic from variations in leverage within the same industry and state. Consistent with the loan-level analysis, industry groups are based on three-digit NAICS codes in the baseline specifications. Finally, in all firm-level regressions, we compute two-way clustered standard errors at the industry and state levels.

Table 5 presents summary statistics of the sample used in the firm-level analysis. This sample consists of all SMEs with 50 percent or more of their total debt accounted for by loans from Y-14 banks (Y-14–reliant SMEs), given our prior that credit constraints due to Y-14 banks' concern about their capital buffer levels are more likely to restrict overall funding

[38]We use property plant and equipment (PPE) to normalize most dependent variables because our main variable of interest is investment, and it is commonly measured as an investment rate, defined as annual capital expenditures normalized by the stock of PPE at the end of the preceding year.

[39]Two additional controls are included here: annual sales growth to account for growth prospect and unutilized Y-14 loans (normalized by PPE) to help account for available liquidity. Sales growth has to be omitted from the new loan regressions earlier because it requires sales data for two consecutive years and thus results in a sample of new loans over the two sample years that is too small. Conditional on that small sample, however, we verified that controlling for sales growth makes little difference for the coefficients.

[40]All the flow variables, such as sales, cost, and investment, are reported on a trailing 12-month basis, so the value reported in Q4 each year is essentially annual data.

[41]Arguably, the COVID-19 outbreak did not become a pandemic in the United States until March 2020, so firm financial conditions in the first two or even three months of 2020 should not have been affected by the shock. Hence, our estimates of the pandemic's impact on the 12-month activity level may be, if anything, an underestimate of the full-year impact of the shock.

Table 5: Summary Statistics for Firm-Level Estimation Sample

	Count	Mean	Std. Dev.	p5	p25	p50	p75	p95
Employees	5,236	172	533	2	20	56	148	600
Investment Rate (%)	6,396	29.33	64.42	0.00	0.00	9.95	29.99	114.04
Y14 Debt/PPE	6,446	6.22	14.25	0.20	0.56	1.43	4.89	28.41
Non-Y14 Debt/PPE	6,443	1.33	3.85	0.00	0.09	0.28	0.87	6.06
Total Debt/PPE	6,446	7.59	16.96	0.26	0.76	1.85	6.07	34.29
Leverage (Debt/EBITDA)	6,446	3.90	5.52	0.11	1.04	2.27	4.44	12.98
Net Sales ($M)	6,446	86.62	191.04	8.98	20.83	43.16	100.75	312.76
Sales Growth	6,446	0.11	0.30	-0.17	-0.01	0.06	0.17	0.48
Profit Margin (Income/Assets)	6,446	0.12	0.13	-0.00	0.04	0.09	0.16	0.36
Tangibility (PPE/Assets)	6,446	0.23	0.22	0.01	0.05	0.16	0.36	0.71
Liquidity (Cash+Equiv./Assets)	6,446	0.07	0.10	0.00	0.01	0.03	0.10	0.27
Unutilized Exposure/PPE	6,446	2.99	9.33	0.00	0.00	0.32	1.67	14.66
PPP Loan Amount/PPE	1,515	1.43	4.70	0.03	0.16	0.44	1.21	5.13

Note: Y14 Debt is the utilized dollar amount of loans from Y-14 banks. Sales are in million dollars. Investment Rate is defined as 12-month trailing capital expenditures reported in Q4 of year t ($t = 2019, 2020$) normalized by the preceding year's Q4 capital stock of property, plant, and equipment (PPE). Leverage is the ratio of total debt over EBITDA. Sales growth is the change in 12-month net sales over the preceding year's net sales. Profit margin is operating income over assets. Tangibility is PPE over total assets. Liquidity is cash plus marketable securities over total assets. Unutilized Exposure is the volume of credit lines from Y-14 banks not yet drawn (utilized). Debt/Income, Net Sales, Sales Growth, Profit Margin, Tangibility, Liquidity, and Unutilized Exposure/PPE are all lagged by one year vis-à-vis investment and employment. In our sample, 95 percent of firms were eligible for a PPP loan. Of those that were eligible, 67 percent received a PPP loan.

availability for these firms and in turn restrain their ability to invest and hire. As with the loan-level analysis, the majority of firms are fairly small, somewhat smaller than the SMEs in the earlier sample of firms used to analyze new loan originations. On average, the firms in the sample have sales of $86.6 million (median of $43.2 million) and 172 employees (median of 56 employees).[42] This sample also has a slightly lower share of PPE in total assets compared with the loan-analysis sample (23 percent versus 33 percent). The two samples are quite similar along the other dimensions (that is, leverage, profit margin, and liquidity). There is substantial dispersion in the investment rate (defined as capital expenditures as a percentage of the preceding year-end PPE) around a mean of 29.33 percent (and a median of 10 percent). Because of our focus on SMEs that rely mostly on Y-14 banks for debt financing, Y-14 debt is a large share of total debt.

We start our analysis of firm-level effects with total debt funding. Table 6 reports the effects of leverage on firms' debt financing during the COVID period. Estimates for total loans from Y-14 banks are reported in columns (1) through (3), while those for firms' total

[42] The mean and median sales for the loan-analysis sample are $126.7 million and $72.7 million, respectively.

Table 6: Effect of Leverage on Debt Financing at the Firm Level

	Y14 Debt/PPE, %			Total Debt/PPE, %		
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	-93.360***			-59.015*		
	(29.932)			(31.494)		
COVID*Leverage		-17.264*			-22.804**	
		(8.808)			(10.113)	
COVID*Leverage*Reliance on Low Cap. Bank			-19.731**			-26.288**
			(8.695)			(9.820)
COVID*Leverage*Reliance on High Cap. Bank			-0.819			3.457
			(24.341)			(30.463)
Observations	6,446	6,446	6,446	6,446	6,446	6,446
R-squared	0.331	0.344	0.347	0.336	0.347	0.350
Industry FE	Yes	No	No	Yes	No	No
State FE	Yes	No	No	Yes	No	No
Firm Controls	Yes	No	No	Yes	No	No
COVID*Industry FE	No	Yes	Yes	No	Yes	Yes
COVID*State FE	No	Yes	Yes	No	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	No	Yes	Yes
Group Difference Est.			-18.912			-29.744
Group Difference P-value			0.4481			0.3253

Note: This table reports the effect of leverage during the pandemic on debt funding at the firm level. The dependent variables are (i) total outstanding balance of bank loans from all Y-14 banks (columns 1 through 3) and (ii) total level of all debt (columns 4 through 6). All dependent variables are scaled by the PPE capital stock at the preceding year-end to facilitate coefficient comparison with regressions of firms' real outcomes. Leverage is the ratio of total debt over EBITDA measured at the preceding year-end. COVID is an indicator equal to 1 for 2020 and 0 otherwise. The sample consists of all SMEs with 50 percent or more of their total debt accounted for by loans from Y-14 banks (Y-14–reliant SMEs). In columns (3) and (6), the effect of leverage during the pandemic is allowed to differ depending on whether a firm obtained more than half of its Y-14 loans from low-capital banks (for which we find in the previous section evidence of credit supply shock). Robust standard errors multi-way clustered at the state and industry levels are in parentheses; ***, ** and * denote significance at 1%, 5%, and 10% levels, respectively.

debt balance are in columns (4) through (6). The estimated coefficient in column (1) shows that total loans from Y-14 banks contracted significantly during the pandemic, by 93 percent of the PPE balance. More importantly, column (2) shows that this contraction was greater after COVID hit for firms with higher leverage among the Y-14–reliant SMEs, by nearly 20 percent of the unconditional decline in column (1). Column (4) reports that the decline in total debt was close to two-thirds of the decline in Y-14 loans, indicating that these SMEs were able to obtain credit from other sources (including the PPP) to partially offset the contraction in credit from Y-14 banks. Column (5) reveals that total debt also shrank more for firms with higher leverage. The magnitude of the reduction indicates that the more levered firms were also less able to find funding from non–Y-14 lenders, although the differential impact due to leverage was much smaller on non–Y-14 debt than on Y-14 loans (less than -5.5 versus -17.3 in column (2)).

Because our earlier analysis reveals that the retrenchment in new loan origination can be fully attributed to those Y-14 banks with a low capital buffer, we next distinguish between the two groups of banks in estimating the impact of leverage. To this end, we further interact the main covariate of interest with an indicator of a firm's reliance on low-capital banks, which is defined as equal to 1 if more than 50 percent of a firm's total Y-14 loans outstanding at the end of 2019 were from one or more of the low-capital banks and 0 otherwise. The indicator of a firm's reliance on high-capital banks is defined analogously. Columns (3) and (6) of Table 6 report the coefficients on these two triple-interaction terms for the two dependent variables.

Similar to our loan-level findings, these estimates show that the adverse effects of pre-shock leverage on overall Y-14 loans outstanding and total debt balance after the COVID shock were entirely driven by firms that were more reliant on Y-14 banks with low capital buffers before the pandemic. In contrast, we find no impact from leverage on firms that relied on well-capitalized Y-14 banks.[43] The estimate in column (6) suggests that a 1x higher leverage ratio reduced total debt balance (relative to lagged PPE) at the end of 2020 by a substantial 26 percentage points, with the Y-14 loan balance accounting for the bulk of the reduction (19 percentage points; see column (3)). Again, the evidence points to a total lack of substitution of alternative sources of debt funding, in this case specifically for those highly levered SMEs. Consequently, their total debt declined by slightly more than the volume of Y-14 loans.

Given the contraction in total debt balance, we next investigate how firms adjusted the various uses of funds during the same time period, paying special attention to their capital expenditures (investment) and employment. Since the reduction in total debt funding was concentrated in SMEs that were more reliant on low-capital Y-14 banks, it is most likely that any deleterious effect on real activity would have manifested most clearly with these firms as well. We thus further narrow our focus to this subset of firms (that is, those with Y-14 loans accounting for more than 50 percent of their total debt and with more than 50 percent of their Y-14 loans from the low-capital banks, both as of the end of 2019) in the following analysis.

[43] However, the differential effect is not statistically significant due to the large standard error for the firms not reliant on low-capital banks (see the test statistic in the last row). In Appendix Table D.8, we also show that total debt did not contract as strongly during the COVID period for firms that did not rely on Y-14 banks. Recall that, for these firms, the impact of leverage on new loan originations was also insignificant due to the large standard errors.

3.3 Public Credit Programs Supported Real Activity during the Pandemic

We now turn to our core results regarding the impact of pre-crisis leverage on firms' real activity during the COVID period. As discussed above, any analysis of credit conditions and their consequences after the COVID outbreak should take into account the extraordinary public support. Two programs exemplifying that support are especially relevant for our sample of SMEs: loan forbearance through the debt-relief program and direct lending through the PPP. Thus, a natural question for our analysis is whether or by how much the PPP and loan forbearance mitigated the adverse effects of leverage on SME investment and employment documented so far. Indeed, if the PPP, for example, did make a difference, then a regression that omits it would suffer from an attenuation of the coefficient on leverage during the COVID period because at least some firms that received PPP loans could have afforded not to cut their investment and employment as much despite their high leverage.

To formally estimate the extent to which either loan modifications or PPP loans may have alleviated the funding constraint on SME investment and employment due to high leverage, we allow the effect of COVID*Leverage to vary depending on whether a firm participated in either program. Specifically, we estimate the following regression:

$$\text{Firm Outcome} = \beta \cdot \text{COVID} \times \text{Leverage} + \theta \cdot \text{COVID} \times \text{Leverage} \times \text{Public Program} + X'\gamma + \epsilon. \tag{3}$$

The marginal effect of a public program is captured by the triple interaction term between a Public Program and COVID*Leverage, which is the key term analyzed so far. Depending on the program studied, Public Program is an indicator variable that equals 1 if either (i) a firm received one or more loan modifications, or (ii) a firm received one or more PPP loans, and 0 otherwise. In estimations according to Equation (3), we keep all the other terms, including the (COVID) interaction terms, in the previous specifications according to Equation (2), but omit these from the result tables for brevity. We relegate to the Appendix Table D.5 the descriptive statistics of the sample of firms included in our (3) estimations. We separately report statistics for firms that received PPP loans and firms that did not receive PPP loans.

Table 7 reports the estimates according to Equation (3) for investment as the real outcome variable. To set the baseline for comparison, column (1) presents the effect of COVID on the investment rate of those SMEs reliant on low-capital banks for credit, regardless of their leverage. It shows that during the COVID period, these firms' investment rate fell significantly by about 3 percentage points on average for a given industry and state,

Table 7: Effects of Leverage and Credit Support Programs on Firm Investment

	Investment Rate, %					
				IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	-3.266*					
	(1.755)					
COVID*Leverage		-0.715***	-0.202	-2.349**	-1.037***	-1.535***
		(0.222)	(0.667)	(1.134)	(0.251)	(0.521)
COVID*Leverage*Loan Modification			-0.605	1.944*		
			(0.667)	(0.973)		
COVID*Leverage*PPP Loan					0.536*	1.486***
					(0.270)	(0.311)
Observations	5,645	5,645	5,645	5,645	5,645	5,645
R-squared	0.079	0.102	0.103	0.041	0.102	0.044
Industry FE	Yes	No	No	No	No	No
State FE	Yes	No	No	No	No	No
Firm Controls	Yes	No	No	No	No	No
COVID*Industry FE	No	Yes	Yes	Yes	Yes	Yes
COVID*State FE	No	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	Yes	Yes	Yes
First-stage Robust F statistics				60.717		43.742
P-value for H_0: Underidentified				0.0291		0.0315

Note: This table reports the effect of leverage during the COVID period on firm investment rate, along with the effect of two credit support programs. The dependent variable is defined as 12-month trailing capital expenditures reported in Q4 of year $t, t = 2019, 2020$ normalized by the preceding year-end's PPE. Leverage is the ratio of total debt over EBITDA measured at the preceding year-end. COVID is an indicator that equals 1 for 2020 and 0 otherwise. Columns (3) and (4) consider loan extensions, with Modification Flag being an indicator equal to 1 if one or more of a firm's Y-14 loans were modified or extended within the year and 0 otherwise. Columns (5) and (6) consider the PPP, with PPP Loan being an indicator equal to 1 if a firm received a PPP loan and 0 otherwise. Column (5) uses binned minimum number of days to maturity as instruments for actual extension. Column (6) uses eligibility for the PPP as an instrument for actual uptake. The sample includes all firms with more than 50 percent of their total debt coming from Y-14 banks (Y-14–dependent firms) and more than half of their Y-14 debt from low-capital banks (those banks where the credit supply shock was concentrated). Robust standard errors multi-way clustered at the state and industry levels are in parentheses; ***, ** and * denote significance at 1%, 5%, and 10% levels, respectively.

controlling for firm characteristics to account for compositional shifts in the sample.

In column (2), we zero in on the differential effect of leverage on investment during the COVID period. For this purpose, we saturate this regression with all of the controls and fixed effects along with their interactions with the COVID indicator, and our key coefficient of interest is the interactive effect between leverage and COVID. Column (2) shows a significantly stronger decline in the investment rates of firms that entered the COVID downturn with higher leverage. The estimated effect is also economically sizable: A 1x higher leverage is associated with an additional drop in the investment rate of seven-tenths of a percentage point, nearly a quarter of the unconditional decline in investment rate during the pandemic.[44] Note again that this effect is identified from variation within the same industry and state and while controlling for other firm financial attributes. It is worth contrasting our finding of a noticeably negative impact of leverage on bank lending and in turn on firm investment during the COVID-induced recession with the finding of no investment effect in Berrospide and Edge (2019).[45] These contradictory findings suggest that the effect of credit constraints from reductions in bank lending likely depends on the state of the economy. During normal times, a pullback in credit supply by banks can be largely offset by additional credit from other sources, resulting in little to no restraints on firms' real economic decisions. In contrast, in the aftermath of an aggregate adverse shock, firms that face reduced credit supply from their regular banks are much more likely to have difficulty obtaining funds from other sources to make up for the shortfall.

The OLS estimate for the effect of loan modifications on the investment rate is reported in column (3). It shows that for a firm, receiving one or more loan modifications or extensions in 2020 did not mitigate the adverse effect of high leverage on its investment, as the OLS coefficient on the triple-interaction term is negative albeit insignificant. At the same time, the baseline effect of leverage during the COVID period also becomes insignificant. These estimates, however, may suffer from an endogeneity bias, in that firms' decisions to seek loan forbearance and banks' decisions to grant forbearance are both endogenous. In the context of our analysis, we are particularly concerned about how a bank and borrower's joint choice to modify a loan depends on the firm's leverage.

Specifically, a higher pre-COVID leverage would increase the likelihood of the firm violating a loan covenant (such as the interest coverage or the debt-to-income ratio), even while it had a greater need to maintain existing funding, because it would find new funding more difficult or costly to obtain, as our analysis so far has revealed. If the bank then chose to accelerate repayment or even terminate the loan prematurely, it would heighten the probability of default. This would damage the bank's financial performance, and the resulting charge-offs would impair its capital ratio. Banks, especially those with a relatively low capital buffer, would find such impairment costly. Therefore, when the firm's leverage is higher, both parties have greater incentive to modify the loan. To the extent that highly levered firms were more likely to invest less in the aftermath of COVID for other reasons (such as debt overhang even without any financing frictions), this endogeneity would bias

[44]For this sample, the inter-quartile range of leverage is 3.4, which translates into an investment rate 2.4 percentage points lower for firms at the 75th versus the 25th percentile of the leverage distribution.

[45]Cortés et al. (2020) also find little impact of credit reduction by the Y-14 banks on small firms' total credit and local employment growth during normal times using a different data set.

down the OLS estimate of the mitigating effect of loan forbearance during the pandemic.[46]

The instrumental variable (IV) we use to solve this endogeneity problem is a function of the remaining number of days to maturity in 2020 on a firm's loans that were outstanding at the end of 2019:Q4. Obviously, the maturity dates were set prior to the COVID outbreak, and it is hard to imagine that the dates were chosen with any reference to a firm's possible reaction to such a shock. Hence, this variable should be uncorrelated with the residual of the investment rate equation in 2020. At the same time, this IV is likely to be correlated with the odds of a firm being granted a loan modification or extension, because the pre-COVID data reveal that a loan was much more likely to be extended in the quarter when it was scheduled to mature. In other words, a shorter (longer) time to maturity is related to a firm's real outcome only through its influence raising (lowering) the odds of a loan being modified or extended. Given that the endogenous variable is an indicator equal to 1 if *any* of a firm's loans were granted forbearance, we define the IV based on the minimum number of days from the end of each quarter in 2020 to the maturity date of every loan still outstanding as of that quarter. Furthermore, since the regressor is a binary variable, we transform the minimum number of days to maturity into a categorical variable as follows to improve the first-stage fit: (1) up to one month remaining until maturity, (2) one month to two years until maturity, and (3) more than two years until maturity.

Column (4) of Table 7 reports the coefficients from this IV estimation, along with first-stage diagnostic statistics supporting the validity of the instrument.[47] The baseline effect of leverage on investment during the COVID period is now significantly more negative, a bit more than two-thirds of the unconditional impact of COVID reported in column (1) (2.3 versus 3.3 percentage points). In contrast, as surmised, the coefficient on the triple-interaction term involving loan forbearance is now positive at the 10 percent significance level. Its magnitude is also nontrivial—nearly offsetting the negative coefficient on COVID*leverage. In short, the IV estimates indicate that the endogeneity of forbearance decisions on net biased down the positive effect of loan modifications. Once corrected, the evidence becomes favorable to the prior that loan forbearance largely mitigated the deleterious effect of leverage on investment during the pandemic.

Next, we consider how funding from the PPP may have supported investment by highly levered SMEs. Column (5) presents the OLS estimate suggesting that PPP recipients cut

[46] A bias of the opposite sign is also possible, albeit less likely, if higher leverage is the result of a firm having enjoyed better than average investment opportunities and thus having invested heavily before the COVID outbreak hit and continuing to invest despite COVID. The sign of the net bias is an empirical matter.

[47] The first-stage robust F statistic is substantially larger than the commonly applied threshold of 10, and a formal test of underidentification can be rejected at conventional significance levels, as reported in the last two rows of column (4).

their investment rate 50 percent less than non-recipients for any given level of leverage. However, similar to the situation with loan forbearance, a firm's decision to obtain a PPP loan was likely endogenous. More importantly for our analysis, this endogenous choice may have been influenced by a firm's leverage. A higher pre-COVID leverage would have made it more difficult or costly for a firm to obtain funding from private lenders, as revealed by our analysis so far of loans from low-capital Y-14 banks, and this would have raised the marginal value of funding from the PPP. The implication is that firms with higher leverage were more motivated to seek PPP loans, all else being equal. At the same time, the marginal value and hence the selection effect of PPP uptake also may well have depended on a firm's investment opportunity. A firm that took out a PPP loan may have had relatively better business prospects and therefore would have cut its employment and investment less even absent the PPP. In sum, the observed net selection effect depends on which effect dominates—the "pull" from brighter growth prospects or the "push" from funding constraint or cost.

We tackle this identification challenge by exploiting an eligibility criterion of the PPP to instrument the actual decision to take out a PPP loan. As discussed above, PPP loans were available only to firms with 500 or fewer employees, and this threshold was exogenous to an individual firm's growth prospects or financial health at the outset of the COVID pandemic. In our 2020 sample, 95 percent of the SMEs qualified for the PPP based on their employment level in March 2020, and two-thirds of the eligible firms received PPP loans (see notes under Table 5). Hence, conditional on firm characteristics and industry fixed effects, the variation in eligibility can serve as an instrument that enables us to estimate the causal effect of the PPP program on recipient firms' real outcomes, particularly the extent to which the PPP helped alleviate the detrimental effect of high leverage on SMEs' access to private funding and in turn their real activity. It should be noted that estimates identified via this IV may be tilted toward the lower end of the PPP's "treatment effects," because they are identified more through variations around the 500-employee threshold and other studies find the boost to employment from the PPP was much larger for the smallest firms (those with fewer than 10 employees) than for larger firms.[48]

Column (7) of Table 7 presents the estimates using this IV.[49] The estimates suggest that once the endogeneity of the PPP uptake is taken into account, leverage exhibits a noticeably larger adverse effect on investment. The coefficient on COVID*Leverage implies that a 1x higher preexisting leverage led to an additional contraction in investment during the COVID period of about 1.5 (versus the OLS estimate of 1.0) percentage points. At the same time,

[48] See, for example, the estimate differences noted above between Doniger and Kay (2021) and Autor et al. (2022).

[49] Again, the first-stage robust F statistic substantially exceeds the common threshold of 10, and the p value for the underidentification test is 0.03.

Table 8: Effects of Leverage and Credit Support Programs on Firm Employment

	Log(Employees), x100					
				IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	-6.407					
	(4.158)					
COVID*Leverage		-0.346	-1.316	-0.628	0.898	-5.004*
		(0.467)	(1.131)	(3.805)	(0.906)	(2.614)
COVID*Leverage*Loan Modification			1.060	0.057		
			(1.005)	(3.940)		
COVID*Leverage*PPP Loan					-1.808**	7.473*
					(0.748)	(3.961)
Observations	4,606	4,606	4,606	4,606	4,606	4,606
R-squared	0.339	0.355	0.356	0.243	0.359	-0.141
Industry FE	Yes	No	No	No	No	No
State FE	Yes	No	No	No	No	No
Firm Controls	Yes	No	No	No	No	No
COVID*Industry FE	No	Yes	Yes	Yes	Yes	Yes
COVID*State FE	No	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	Yes	Yes	Yes
First-stage Robust F statistics				32.558		90.017
P-value for H_0: Underidentified				0.0299		0.0375

Note: This table reports the effect of leverage during the COVID crisis on (log) firm employment, along with the effects of two credit support programs. The dependent variable is the logarithm of total employment as of December 2019 and December 2020. Leverage is the ratio of total debt over EBITDA measured at the preceding year-end. COVID is an indicator that equals 1 for 2020 and 0 otherwise. Columns (3) and (4) consider loan extensions, with Loan Modification being an indicator equal to 1 if one or more of a firm's Y-14 loans were extended within the year and 0 otherwise. Columns (5) and (6) consider the PPP, with PPP Loan being an indicator equal to 1 if a firm received a PPP loan and 0 otherwise. Column (5) uses binned minimum number of days to maturity as instruments for actual extension. Column (6) uses eligibility for the PPP as an instrument for actual uptake. The sample includes all firms with more than 50 percent of their total debt coming from Y-14 banks (Y-14–dependent firms) and more than half of their Y-14 debt from low-capital banks (those banks where the credit supply shock was concentrated). Robust standard errors multi-way clustered at the state and industry levels are in parentheses; ***, **, and * denote significance at 1%, 5% and 10% levels, respectively.

the PPP raised the recipients' investment rate by 1.4 percentage points more than that of the non-recipients. The magnitude of the IV coefficients is such that the PPP almost entirely offset the average adverse impact of higher leverage on investment. In short, our estimates indicate that the PPP provided much needed funding to SMEs that were reliant on low-capital Y-14 banks for funding and thus faced greater difficulty and a higher cost in accessing private bank loans. As a result, higher leverage did not exert any negative influence on investment by these SMEs during the pandemic.

Last, we study the effect of leverage on SMEs' employment outcome during the pandemic. Table 8 presents the results following the same structure as in Table 7.[50] First, column (1) shows that employment fell by 6.4 percent during the pandemic, although this average effect is statistically insignificant at standard levels of confidence because of the large standard error. Column (2) reports that the employment change in 2020 is not negatively associated with a firm's pre-COVID leverage. Columns (3) and (4) estimate the effect of loan forbearance on SMEs' employment. The OLS coefficients (column (3)) indicate that, once loan modifications are taken into account, there is more evidence that leverage exerted a negative impact on employment, which was largely offset by loan forbearance. This pattern conforms closely to that found for investment using the IV estimation, even though the coefficients for employment are statistically insignificant. Interestingly, this pattern nearly disappears in the IV estimation (column (4)), even though the first-stage diagnostic statistics remain adequate. This implies that the endogeneity of forbearance decisions may have led to an upward bias of the OLS coefficient on the triple-interaction term, meaning that highly levered firms were more likely to have their loans modified but also tended to reduce employment by less. In combination with the findings for investment, this suggests that the firms may have practiced capital-labor substitution to some degree. That is, firms that had greater difficulty obtaining funding curtailed investment more than they cut labor input.

Columns (5) and (6) of Table 8 present findings for the effect of the PPP on employment. According to the OLS estimates in column (5), the more levered SMEs experienced a greater decline in employment during the COVID period if they also received PPP loans, while the baseline effect of leverage on employment was insignificantly different from zero. This result is most likely due to the endogeneity of the PPP uptake decision, and this confounding effect may be particularly relevant for employment, because the foremost objective of the PPP was to fund payroll and thus support employment. That is, firms that had greater difficulty maintaining their employment levels due to costly or insufficient private funding were more likely to seek and qualify for a PPP loan. Such a selection effect can explain the negative OLS coefficient on the triple-interaction term in column (5). To correct for this potential bias, we apply the same IV estimation strategy for employment that we used for investment. These IV estimates, reported in column (6), confirm our conjecture that the PPP significantly mitigated the damaging effect of leverage on employment during the pandemic: A 1x higher leverage lowered employment by 5.0 percent, but this was more than fully offset by a 7.5 percent boost to employment if the firm received a PPP loan. In fact, the supportive effect of the PPP even slightly exceeded in magnitude the unconditional

[50] The sample size is somewhat smaller because employment is missing for some of the firms underlying Table 7.

deleterious effect of the pandemic on employment (shown in column (1)).

The notably larger magnitude of these IV estimates of the PPP's impact on employment compared with the estimates of its impact on investment is consistent with the stated objective of the PPP: to encourage and enable firms to retain workers. At the same time, we note again that our IV estimates may still be toward the lower end given that our eligibility-based IV estimates are identified more through variations around the 500-employee threshold of firm size. Just as important, in terms of the overall average effect on firms' productive activity, we find evidence that the PPP achieved its goal in one particular way: It prevented preexisting high leverage from impeding SMEs' ability to retain workers or invest in capital during the pandemic, thereby limiting the damage to the real economy. We also find that PPP loans did not significantly contribute to an increase in cash holdings or inventory (see Tables D.6 and D.7 in the Appendix). Moreover, the assistance is found to have been effective precisely for the SMEs that were most susceptible to credit constraint—those that rely heavily on loans from banks concerned about capitalization. We find no effect of the PPP on either employment or investment for large firms or for SMEs that do not rely on low-capital Y-14 banks (see Tables D.10–D.13. This validates the design philosophy underlying the PPP: to provide public funding to small businesses because they faced greater financing frictions that were likely to be exacerbated by sizable adverse shocks to the economy as a whole.

4 Conclusion

Using a supervisory loan-level data set containing information about the majority of commercial and industrial (C&I) loans issued by large US banks, we analyze the impact of pre-pandemic corporate leverage on the credit conditions faced by borrower firms during the COVID crisis. We find heterogeneous effects of leverage on credit outcomes across firms. Credit conditions of small firms with high pre-COVID leverage were most negatively affected during the pandemic. These firms received smaller loans and paid higher spreads on new loans when they were borrowing from banks with low capital buffers. Consistent with the interpretation that such differential impact was due to credit market frictions, especially for small businesses, those affected firms were unable to fully offset the lost funding by borrowing more from other banks (or other sources of financing).

As a result of the credit crunch, investment and employment at SMEs relying on low-capital-buffer banks contracted more strongly during the COVID period, highlighting the importance of high corporate leverage as a shock amplifier that can jeopardize financial stability. However, the unprecedented public support provided by the US government and the Federal Reserve ameliorated the credit conditions for the most affected firms, especially those small firms that would have otherwise had difficulty accessing liquidity. Specifically, we find that the PPP targeted at small firms mitigated the adverse effects of the lack of private funding (through bank loans) for leveraged SMEs, thereby containing the would-be adverse effects of the credit crunch on those firms' employment and investment.

Overall, our findings highlight the importance of policy measures that support the flow of credit (or even grants) to small businesses, including those that are relatively highly levered but are nevertheless viable in the long run, following a severe exogenous adverse shock. Our analysis also provides evidence that a well-capitalized banking sector, owing to rigorous capital regulation during normal times, for example, would be more capable of supplying much-needed credit to bank-dependent SMEs following adverse shocks. This would be particularly valuable in the absence of public liquidity facilities directly targeted at more constrained small businesses. On the other hand, our results also suggest that bank capital, while exceeding the minimum regulatory requirement by a much wider margin relative to historical standards, can still become a constraint on bank lending during periods of distress when bank substantially revise upward expectations of capital losses. To offset this tendency, countercyclical capital buffers could, in principle, improve bank credit supply, especially to the more constrained SMEs, following severe adverse shocks.

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Appendix (Not for Publication)

A A Simple Model of Firm Leverage, Bank Capital, and Investment

We sketch a simple model to provide the rationale for interpreting our empirical analysis. We pay special attention to the distinction between large and small firms, with firm size serving as a proxy for firms' access to external funding markets, since firms that can readily access the corporate bond or stock markets tend to be much larger than firms that have access only to bank loans. Moreover, this model focuses on the interaction between firm leverage and bank capital, especially in response to an adverse shock. It illustrates the mechanism by which banks with adequate capital but a *low buffer* vis-à-vis the required ratio are less willing to lend to riskier firms. Furthermore, higher leverage is more likely to adversely affect small firms' real activity in the wake of a negative shock by impairing their ability to obtain additional credit from private lenders.The resulting credit constraint faced by such firms can limit their spending on capital and labor suboptimally, therefore additional funding should have a greater impact, given their higher shadow cost of capital.[1] The extraordinary public funding provided through programs such as the Paycheck Protection Program (PPP) shortly after the onset of COVID was thus likely to be particularly valuable to SMEs with higher leverage and therefore deemed to pose greater default risk. In fact, if government intervention is designed to target SMEs, then higher leverage may distort their investment to a smaller degree than it does with large firms, even if SMEs still receive less private credit on net than large firms.

To focus on studying the impact of a likely temporary shock, we include two dates in the model, $t = 1$ and $t = 2$. A firm needs additional funding, beyond its available cash holdings, to maintain operations that will generate surplus in the future, $t = 2$. The firm has cash holdings of a and a gross expenditure need, e, to produce its output. The net funding need is $e - a$ if the firm chooses to use internal funds first. If the firm decides to spend and produce, the future surplus is eR, with R denoting the gross rate of return of the new expenditure. For simplicity, we assume that the return R is certain, and $R > r$, with r denoting the gross risk-free interest rate.

The firm's existing assets will have a stochastic value of $\tilde{A}$ at $t = 2$, with a cumulative distribution function $F(\tilde{A})$. If the shock is structural and changes the value of the firm's existing assets, then $\tilde{A}$ can be construed as the post-shock value. For example, COVID raised

[1] Note that this type of underinvestment is separate from, but can coexist with, the problem of debt overhang, which also results in inefficiently low capital investment and worker retention when a firm is too heavily indebted. However, debt overhang can manifest even without credit constraints.

the value of e-commerce–centric Amazon while devastating brick-and-mortar retailers. We assume that the firm's outstanding debt requires a gross payment of D in $t = 2$. The probability that $\tilde{A} < D$ is denoted by δ; δ is the default probability in the absence of additional borrowing, or approximates it if only a sufficiently small amount of debt is added.[2] This implies that, for a given D, a negative (positive) structural shock to $\tilde{A}$ increases (decreases) the firm's default probability and thus is equivalent to raising (lowering) its leverage.

A.1 A Small Firm's Optimization Problem

Now consider a small firm's optimal decision to invest or retain workers. The key feature that distinguishes an SME from a large firm is that the former faces borrowing constraints, as the literature has long recognized. This limits the amount an SME can borrow to no more than a certain multiple of its owner's net worth (or, equivalently, a fraction of the firm's asset value, with the assets serving as collateral) used as a mechanism to mitigate the agency problem.

We follow the convention and model the borrowing constraint based on asset value: Assume a small firm's owner is endowed with starting wealth (net worth) N and can borrow D from banks to buy capital K, which serves as collateral for D. The liquidation value of K is θK, the borrowing constraint is thus set as $D \leq \theta K$, and the firm pays a gross interest rate of γ on the loan. Assume the gross rate of return on K is R in a steady state, and $R > \gamma$, then the firm will borrow as much as possible, and its balance sheet identity implies:

$$K = N + D, D = \theta K \quad \Longrightarrow \quad K = N/(1-\theta). \tag{A.1}$$

If the firm expects to run at this scale forever, then the (private) value of the firm to the owner is

$$V_E = N\frac{R - \gamma\theta}{r(1-\theta)}. \tag{A.2}$$

Now assume a negative shock such as the pandemic shock causes the cash flow process to become $\tilde{R}_1$ with CDF $F(\tilde{R}_1)$, so that the default probability becomes $F(\gamma D)$. Once the firm defaults, the bank will take over, sell the capital, and receive θK. So the deadweight loss to society is $V_E - \theta K$, since the firm is more efficiently operated by the owner/manager. Now further assume that the liquidation value is also reduced by the shock, and that the shock especially elevates downside risk so that in the worst-case scenario the bank receives only $\underline{\theta} K$ from selling the capital, with $\underline{\theta} < \theta$. This can lead to a credit constraint on the firm even without it defaulting on the existing loan contract if its loan comes due and has

[2] By the same logic, cash holding a is assumed to be sufficiently small and thus has negligible effect on δ.

to be renewed, because the bank will want to tighten the borrowing limit down to $\underline{\theta}K$. This probability is higher for small firms because they tend to have loan contracts with shorter maturities (Chodorow-Reich et al. (2021)). This constraint can be mitigated to some extent if the firm has unpledged assets that can be tapped as collateral to minimize the reduction in loan balance, despite the lower limit $\underline{\theta}K$. A policy measure that grants loan extensions upon borrower requests can also alleviate the problem.

Even if the adverse shock does not directly diminish the fundamental value of firm i, it can still be subject to credit constraints if defaults by other firms impair the bank's capital and hence its risk-bearing capacity, or if the resale value of assets serving as collateral deviates from the fundamentals because the secondary market suffers from a fire sale or excess supply. These factors were the main culprits during and after the 2008 financial crisis, but they were largely absent during the COVID-19 episode because banks were well capitalized (especially vis-à-vis the minimum requirement) prior to the outbreak. Nevertheless, as we will show later in the bank's optimization problem, concerns about possible *future* capital shortfall can restrain banks' lending today.

If the credit tightening is sufficiently severe, it could precipitate default if the firm cannot find enough internal liquidity to cover the gap between the smaller new loan and the larger repayment needed on the old loan. Sources of internal funds include cash flow from operations (net income, non-cash expenses, and a change in working capital), additional cost cutting, or cash reserves. Given that most small firms have little cash on hand, they most likely would have to resort to diverting depreciation or cutting back on working capital or even employment.[3] This implies that even if the firm can gather enough internal funds to cover the shortfall, the credit constraint will likely damage the firm's long-term value because the funds diverted to loan repayment would have had more productive uses, such as to maintain capital or retain workers.

Even if the firm has no loans maturing in the near term after the adverse shock hits, the owner/manager's desire to preserve enterprise value by lowering the default risk will likely still lead to value-destroying resource allocations, because the value of liquidity is high for a typical small firm in the wake of a negative shock. Suppose the firm cuts back on necessary (but not critical) maintenance, suspends worker training, or even lays off some employees, so that it raises $\tilde{R}_1$ by e but at the cost of lowering revenue at $t = 2$ by e. This reduction in today's default probability raises the enterprise value by $V_E f(\gamma D)e$, compared with a present value of future revenue loss equal to $(1 - F(\gamma D)e)/r$. It is clear that the gain is proportional to the stock of enterprise value, whereas the loss is proportional to the flow of revenue, and

[3] According to Bartik et al. (2020), among firms in the survey, the median amount of cash on hand covers 5.3 months of expenses.

this suggests that many small firms would be willing to cut expenses today by a nontrivial margin to preserve V_E.[4]

The high market value of liquidity for small businesses provides a clear rationale for policy measures that help preserve borrower liquidity. For example, lender forbearance in the form of temporary postponement of partial or all loan repayment can alleviate the liquidity demand. Supply of funding by the public sector at low or no cost, such as through the Paycheck Protection Program, to cover at least part of small businesses' credit demand should also mitigate, or even eliminate, the deadweight loss if some small firms were to go bankrupt, or the damage to their enterprise values that would have been inflicted by credit constraints had these firms needed to seek credit with private lenders.

Public funding support becomes even more important if the revenue loss due to the adverse shock persists for a period, since the more substantial cumulative loss of income means that the borrowers need debt relief beyond the temporary delay of private debt repayment. This argument applies to the COVID-19 period, as the pandemic lingered much longer than initially anticipated, especially for firms in some industries and regions.

A.2 A Bank's Optimization Problem

Here we sketch a simple model describing the likely mechanism by which a bank would choose to restrict risky lending, such as issuing C&I loans to a firm that is deemed riskier, plausibly because the bank is more concerned about potential *future* capital shortfalls than its capital ratio today, especially when its current capital level is adequate. This is a distinction that is not highlighted in existing studies, most of which ((such as, Peek and Rosengren, 1997)) consider the impact of *current* capital shortfall on lending, a situation that clearly did not apply at the onset of the COVID crisis. The three likely costs cited in Berrospide et al. (2021) that may be incurred by a bank and erode its capital buffer do not necessarily imply that the bank should cut back on lending to more risky firms, especially if the greater risk is not explicitly accounted for by a higher capital charge. Instead, our formulation here, which hinges on the logic that de facto riskier loans raise the expected credit losses and in turn the probability of dipping into the capital buffer in the future, offers a more persuasive explanation.

There are two dates in the firm's problem, and the bank makes its investment decision on $t = 1$ with the goal of maximizing expected payoff on $t = 2$. Its problem can be described by the following equations:

[4] Here, the assumption is that today's cost-cutting is by and large marginal and thus will reduce future revenue only temporarily. If the cost-cutting is too deep, it can inflict longer-lasting damage on the enterprise value.

$$\max_{L^S,L,B} \{-C(E_1 - E_{min}) + \beta\mathbb{E}[R^S \cdot L^S + (\gamma \cdot \mathbf{1}_{\gamma\theta \leq \tilde{R}_1})L - R^B \cdot B - C(E_2 - E_{min})]\} \quad \text{(A.3)}$$

$$L^S + L = B + E_1, \quad \text{(A.4)}$$

$$R^B = g(B),\ g'(\cdot) > 0, \quad \text{(A.5)}$$

$$E_1,\ E_2 \geq E_{min},\ E_{min} = \eta(L^S + L), \quad \text{(A.6)}$$

$$E_2 = E_1 + R^S \cdot L^S + (\gamma \cdot \mathbf{1}_{\gamma\theta \leq \tilde{R}_1})L - R^B \cdot B, \quad \text{(A.7)}$$

$$C(\cdot) \geq 0\ ,\ C'(\cdot) < 0,\ C''(\cdot) > 0. \quad \text{(A.8)}$$

Equation (A.3) is the bank's objective function, with β being its discount factor. Equation (A.4) is the bank's balance sheet identity on $t = 1$. The bank starts with given equity capital E_1 and raises additional funding by borrowing (such as through deposits) B. It has to pay a higher interest rate in order to attract more deposits (Equation (A.5)). The bank can choose between two assets: A safer loan L^S pays a (gross) interest rate R^S, while a risky loan L pays a rate γ if the borrower stays solvent on $t = 2$, denoted by the indicator function $\mathbf{1}_{\gamma\theta \leq \tilde{R}_1}$, where θ and $\tilde{R}_1$ are, respectively, the bank borrower's leverage and total return (see the above section on the firm's problem).[5] The probability of a borrower being solvent is denoted by ν, with $\nu = \mathbb{E}(\mathbf{1}_{\gamma L \leq \tilde{R}_1}) = 1 - F(\gamma D)$. The loan pays nothing if the firm defaults. Here we model the two types of loans with a stark contrast—only one is subject to default risk—to gain clarity and focus on the key aspect of interest and highlight its implications: Two types of loans both incur the same capital charge, but one is riskier along dimensions not recognized by the capital requirement, and banks cut back more on making the riskier loan when its risk rises.[6] We can make loan L^S subject to default risk as well, but it will not alter the conclusion qualitatively as long as it is less risky both before and after the shock.

The bank faces capital requirement on both dates. Here we focus on the risk-based capital ratio, which is more relevant for decisions regarding risky loans. On both dates, the bank has to hold capital that is no less than η fraction of its loan portfolio.[7] Moreover, we posit a convex cost of reducing the capital cushion, as expressed in Equation (A.8). That is, the lower the capital relative to the required amount, the higher the cost, and the thinner the capital cushion, the higher the marginal cost. Given the initial capital E_1, the bank can

[5] For SMEs, typically the only source of credit is bank loans, thus $L = D$, although a firm can borrow from multiple banks.

[6] Also for the sake of brevity, we omit securities from the bank's balance sheet.

[7] This specification is standard for $t = 1$, while the requirement on $t = 2$ is modeled in the spirit of maintaining bank credit to firms, similar to the principle used to set a capital buffer in connection with the stress test, whereby projected capital shortfalls translate into a higher capital buffer.

choose only L^S, L, and B to influence E_2 and maximize the total expected payoff in (A.3).[8] Equation (A.7) describes the accumulation of capital over time: Capital on $t = 2$ equals the initial capital E_1 plus retained earnings.

Denoting the Lagrangian multiplier associated with bank capital E_t by λ_t ($t = 1, 2$), and substituting out B using the balance sheet identity, we obtain the first-order conditions with regard to L^S and L, respectively, as follows:

$$\eta C'(\hat{E}_1) + \lambda_1 + \beta\mathbb{E}[(R^S - \epsilon R^B(\cdot))(1 - C'(\hat{E}_2)) + \eta C'(\hat{E}_2) + \lambda_2] = 0, \quad \text{(A.9)}$$
$$\eta C'(\hat{E}_1) + \lambda_1 + \beta\mathbb{E}[(\gamma \cdot \mathbf{1}_{\gamma\theta \leq \tilde{R}_1} - \epsilon R^B(\cdot))(1 - C'(\hat{E}_2)) + \eta C'(\hat{E}_2) + \lambda_2] = 0. \quad \text{(A.10)}$$

$\hat{E}_t := E_t - E_{min}$ ($t = 1, 2$) denotes the capital cushion (in excess of requirement), while $\epsilon := 1 + B(dR^B/dB)/R^B > 1$ denotes the markup on the deposit rate (so that ϵB is the marginal cost of deposits, with the second term being the elasticity of deposit supply, which is a function of B, almost certainly increasing in B and hence in L and L^S). Both first-order conditions indicate that, because of the marginal cost of running down capital cushions $C'(\cdot)$, the bank would not willingly operate at the minimum capital level (that is, with binding capital requirement so that $\lambda_1 > 0$ and $\lambda_{\not{2}} > 0$) unless $R^S > \epsilon R^B$ at that point, which could happen if the bank possesses large market power in the loan market or deposits market or both. This implication of the model, that banks generally operate with more than the minimum required capital, is strongly supported by the data.

Comparing the two first order conditions, the only difference lies in the expected value of the product between the loan return and the marginal cost of capital:

$$(\nu\gamma - R^S)(1 - \mathbb{E}(C'(\hat{E}_2)) - cov(C'(\hat{E}_2), \mathbf{1}_{\gamma\theta \leq \tilde{R}_1}). \quad \text{(A.11)}$$

Since $\mathbb{E}(C'(\hat{E}_2)) \leq 0$, the sign of the first product term is determined by the sign of $(\nu\gamma - R^S)$, which is the difference in the expected rate of return on the two loans. If, for simplicity, we assume the bank takes loan interest rates as exogenously given, such as for a bank with little market power, and pegs each loan rate to the yield on a corporate bond with the same default risk, and we consider an adverse shock that raises the default probability (that is, a lower ν). If the expected return $\nu\gamma$ (that is, the yield scaled down by default probability) in fact rises, then the first two terms' contribution is positive, meaning the bank will want to make more of the riskier loan L^S, all else being equal. But that incentive is at least partly offset by the last term, because the marginal cost of capital and default risk are positively

[8] We can add to period 2's payoff a value function representing the expected present value of all optimized payoffs on later dates $t > 2$. It will not alter any of the results qualitatively as long as the value function does not more than offset the convexity in $C(E_2)$.

correlated, since[9]

$$\frac{\partial C'(\hat{E}_2)}{\partial \mathbf{1}_{\gamma\theta \le \tilde{R}_1}} = C''(\hat{E}_2)\frac{\partial \hat{E}_2)}{\partial \mathbf{1}_{\gamma\theta \le \tilde{R}_1}} > 0. \tag{A.12}$$

Thus, when the default probability of L rises, the marginal cost of capital shortfall also rises. To the extent this covariance term dominates, the bank will make less of the riskier loan L (relative to L^S) following such an adverse shock. This outcome does not need the bank to be capital constrained on $t = 1$ or even expect to be constrained on $t = 2$. If we instead assume that the bank can set its own interest rate on L, because, for example, information on riskier borrowers is more opaque and so it is harder for them to switch lenders. Given a downward sloping loan demand curve, that is, $\gamma = \mathfrak{L}(L)$, with $\mathfrak{L}' < 0$, the bank will have incentive to cut back on the amount of L in order to earn a higher expected rate of return to offset the greater expected capital cost due to the greater default risk.[10]

Another way to interpret the intuition is to recognize that, given the convexity of the cost $C(\cdot)$ of running down capital, the bank has an incentive to limit the fluctuation of its capital level (beyond satisfying the capital requirement). It thus will reduce its exposure to risky assets that experience an increased probability of incurring losses that will eat into its capital cushion in the future, all else being equal (such as earning the same relative expected rate of return on the assets).

Berrospide et al. (2021) proposes three types of costs to explain why a bank would not want to lend more but instead want to maintain its capital buffer: (1) banks do not want to incur the cost of building back the buffer later; (2) banks are concerned about the rating downgrade from dipping into their capital buffer; and (3) banks want to avoid restrictions on earnings distribution that can follow an erosion of capital buffers. It is worth noting, however, that these costs do not necessarily explain why a bank would refrain from making the type of loans that may be riskier but do not incur higher capital charges, such as the type of loans studied here—C&I loans to SMEs with higher leverage.[11] Our model can be regarded as formalizing the first cost with an additional ingredient: To the extent those loans with unobserved greater risk will lead to larger losses (that is, risk not adequately covered by the higher interest rate) and hence a larger capital shortfall tomorrow, it will likely raise the

[9]This positive covariance term also means that in the bank's optimally chosen portfolio, it requires a higher expected rate of return on L than on L^S.

[10]The bank may, in fact, choose not to raise the loan rate as much as the increase in default risk following the adverse shock because doing so will worsen the adverse selection or moral hazard problem ((see, for example, Stiglitz and Weiss, 1981)) and will result in a lower expected rate of return. In other words, an increase in γ, the contractual loan rate, can be more than offset by a decrease in ν such that the product $\nu\gamma$ declines.

[11]It is just remotely possible that if the rating agencies observe all the characteristics on every loan made by each of the Y-14 banks, they may be more likely to downgrade a bank that made more loans since the onset of the pandemic to firms with higher leverage.

cost of rebuilding the capital buffer.Such an expectation therefore deters banks from lending today, especially to riskier firms, even if the extra risk does not carry an extra capital charge.

B Data Cleaning

For observations in the regressions for new loan originations (with dependent variables Log(Volume) and Spread (BPS)): We begin with the full Y-14 data with $13,753,768$ observations/loans from 2011:Q3 through 2020:Q4. There is a total of $2,568,583$ loans in the period from 2019:Q1 through 2020:Q4, of which $123,465$ are new originations. The number of loans that remains after each cleaning step is:

1. $92,469$ new originations remain after dropping records missing obligor TIN.
2. $66,011$ new originations remain after dropping records missing interest rate spread.
3. $32,147$ new originations remain after dropping records missing the preceding year's Q4 financial data (any among leverage, sales, profit margin, tangibility, or liquidity) or missing state or industry.
4. $18,371$ new originations remain after dropping records with fully undrawn commitments (because these do not report interest rate spread).
5. $17,671$ new originations remain after keeping only records with valid maturity dates.
6. $17,039$ new originations remain after dropping firms in NAICS Sector 52 (Finance and Insurance).
7. $15,448$ new originations remain after removing outliers from Leverage, Log(Sales), Profit Margin, Tangibility, and Liquidity, and also from the variables of interest Log(Volume) and Spread. We exclude values that are below the 1st percentile or above the 99th percentile for each variable.
8. $15,111$ new originations remain after dropping records with negative leverage.
9. $14,827$ new originations remain after dropping records where a firm is in NAICS Sector 531 (Real Estate) and the collateral type is Real Estate.
10. $8,889$ new originations remain after keeping only loans to small firms (with net sales of $500 million or less).

For regressions limited to SMEs, in addition to 1 through 10 above:

1. $8,292$ new originations remain after dropping singleton observations for regressions limited to SMEs.

For regressions limited to SMEs and loans originated by low-capital-buffer banks, in addition to 1 through 10 above:

1. $6,379$ new originations remain after keeping only loans originated by low-capital-buffer banks.

2. 5,765 new originations remain after dropping records missing collateral or missing ratings, and additional singleton observations for regressions limited to SMEs and loans originated by low-capital-buffer banks.

For observations in the firm-level regressions (with dependent variables Y14 Debt/PPE, Total Debt/PPE, Investment Rate, and Log(Employees)): There is a total of 67,671 borrowers with legitimate financial data in either 2019:Q4 or 2020:Q4, a total of 98,983 borrower-quarters. The number of borrower-quarters that remain after each cleaning step is:

1. 58,579 borrower-quarters remain after dropping records missing the preceding year's Q4 financial data (any among leverage, sales, sales growth, profit margin, tangibility, liquidity) or missing unutilized exposure, state, or industry.
2. 58,290 borrower-quarters remain after dropping firms in NAICS Sector 52 (Finance and Insurance).
3. 31,430 borrower-quarters remain after keeping only records with a loan that matures sometime within the same year.
4. 29,990 borrower-quarters remain after dropping records missing investment rate.

For regressions with dependent variables either Y14 Debt/PPE or Total Debt/PPE, in addition to 1 through 4 above:

1. 26,706 borrower-quarters remain trimming RHS variables Leverage, Log(Sales), Sales Growth, Profit Margin, Tangibility, Liquidity, and Unutilized Exposure/PPE, and LHS variables Y14 Debt/PPE and Total Debt/PPE (excluding values that are below the 1st percentile or above the 99th percentile for each variable, or if the variable is missing).
2. 25,579 borrower-quarters remain after dropping records with negative leverage.
3. 19,182 borrower-quarters remain after dropping records in which the ratio of Y-14 debt over total debt is less than 0 or greater than 1.
4. 19,179 borrower-quarters remain after dropping records with negative sales.
5. 18,916 borrower-quarters remain after dropping records missing a low-capital-buffer flag.
6. 7,137 borrower-quarters remain after keeping only records in which 50 percent or more of the total debt is accounted for by loans from Y-14 banks.
7. 6,458 borrower-quarters remain after keeping only small firms (with net sales of $500 million or less).
8. 6,446 borrower-quarters remain after dropping singleton observations.

For regressions with the dependent variable Investment Rate, in addition to 1 through 4 above:

1. 26,753 borrower-quarters remain trimming RHS variables Leverage, Log(Sales), Sales Growth, Profit Margin, Tangibility, Liquidity, and Unutilized Exposure/PPE, and LHS

variable Investment Rate (excluding values that are below the 1st percentile or above the 99th percentile for each variable, or if the variable is missing).

2. 25, 609 borrower-quarters remain after dropping records with negative leverage.
3. 19, 139 borrower-quarters remain after dropping records in which the ratio of Y-14 debt over total debt is less than 0 or greater than 1.
4. 19, 136 borrower-quarters remain after dropping records with negative sales.
5. 7, 181 borrower-quarters remain after keeping only records in which 50 percent or more of the total debt is accounted for by loans from Y-14 banks.
6. 6, 499 borrower-quarters remain after keeping only small firms (with net sales of $500 million or less).
7. 5, 699 borrower-quarters remain after keeping only firms where more than half of the Y-14 debt came from low-capital banks.
8. 5, 645 borrower-quarters remain after dropping singleton observations.

For regressions with dependent variable Log(Employees), in addition to 1 through 4 above:

1. 19, 947 borrower-quarters remain after trimming RHS variables Leverage, Log(Sales), Sales Growth, Profit Margin, Tangibility, Liquidity, and Unutilized Exposure/PPE, and LHS variable Log(Employees) (excluding values that are below the 1st percentile or above the 99th percentile for each variable, or if the variable is missing).
2. 19, 067 borrower-quarters remain after dropping records with negative leverage.
3. 13, 914 borrower-quarters remain after dropping records in which the ratio of Y-14 debt over total debt is less than 0 or greater than 1.
4. 13, 913 borrower-quarters remain after dropping records with negative sales.
5. 5, 688 borrower-quarters remain after keeping only records in which 50 percent or more of their total debt is accounted for by loans from Y-14 banks.
6. 5, 323 borrower-quarters remain after keeping only small firms (with net sales of $500 million or less).
7. 4, 660 borrower-quarters remain after keeping only firms where more than half of the Y-14 debt came from low-capital banks.
8. 4, 606 borrower-quarters remain after dropping singleton observations.

C Public Response to COVID Crisis

A distinguishing feature of the pandemic-induced downturn in 2020 was the unprecedented scale and speed of support provided by the public sector, especially from the fiscal authority. Congress passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act swiftly, and it was signed into law on March 27, 2020. In this section, we focus on two important policy responses covered under the act. First, we look at the loan forbearance program, and then we evaluate the effects of the Paycheck Projection Program (PPP). These measures were aimed at supporting business operations, especially those of small firms, by stimulating private bank credit supply as well by supporting business directly with public money. Our focus is on how these two important components of the CARES Act affected credit and real outcomes of firms depending on their pre-crisis leverage. We begin by discussing the impact of the PPP before moving on to the effects of loan extensions on debt.

Paycheck Protection Program Given the unprecedented scale and speed of the public sector support in response to the COVID-19 outbreak, we must consider the role of such support programs when studying firms' real activity during the pandemic. For SMEs, arguably the most important is the CARES Act provision that authorized the Paycheck Protection Program, which offered low-cost loans to small businesses, defined as businesses that were in operation by February 15, 2020, and had 500 or fewer employees.[12] We therefore use the 500-employee threshold for eligibility as a binary instrumental variable for the actual PPP status. Not surprisingly, 95 percent of the firms in our SME sample qualified for the PPP based on this criterion. [13] Actual PPP uptake was widespread, albeit not universal, among eligible small businesses. The uptake rate was 67 percent of the eligible firms in our SME sample, roughly in line with the uptake rate estimated in Dalton (2021) using the Bureau of Labor Statistics microdata. It is likely that all the borrowers were adversely affected by the pandemic to some degree. At the same time, there was minimal screening, and it was realistic for most small-business borrowers to expect their PPP loans to be forgiven, since they could readily satisfy the requirements for forgiveness.[14] A PPP loan on average

[12]There are some exceptions to the 500-employee cutoff. In general, a business could qualify if it met the Small Business Administration's (SBA) employee-based or revenue-based size standard corresponding to its primary industry, or if it met both tests in the SBAs "alternative size standard" (based on maximum tangible net worth or average net income) as of March 27, 2020. Moreover, firms whose industry falls under the two-digit NAICS Sector 72 could qualify as long as the number of employees at each location was 500 or fewer. For more details, see the FAQs posted by the SBA.

[13]Employment data are from the D&B database. For NAICS 72, we use the employee count as reported for each location of operation. For all the other industries, we use the total employee count at the firm level.

[14]The uptake was less than 100 percent likely because borrowers had to certify that in principle "current economic uncertainty makes this loan request necessary to support the ongoing operations of the Applicant." For more details, see the FAQs posted by the SBA.

amounted to about one-third of the value of a firm's property, plant, and equipment (PPE) (see Table 5).

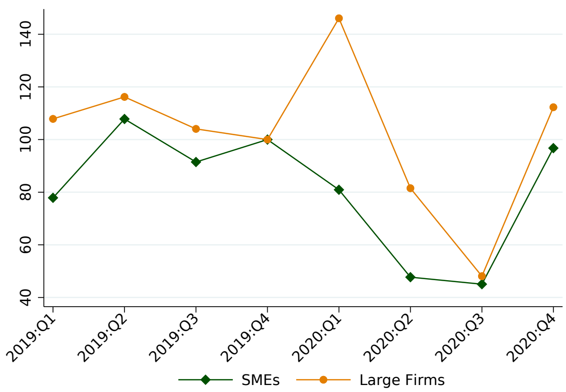
Loan Modifications Section 4013 of the CARES Act introduced loan forbearance measures to mitigate the potential impact on credit availability to firms and the consequent losses to lenders, while an interagency statement issued by the regulatory agencies in April 2020 provided further guidance. The agencies encouraged financial institutions to work with borrowers that faced difficulties meeting their contractual payment obligations due to the effects of the COVID-19 outbreak. In particular, the agencies viewed loan modification programs as positive actions to mitigate adverse effects on borrowers due to the pandemic. The main implication for lenders was that they did not need to categorize COVID-19–related modifications as troubled debt relief measures, with the corresponding provisioning implications.

According to the CARES Act, a loan modification had to be (1) related to COVID-19 and (2) executed on a loan that was not more than 30 days past due as of December 31, 2019, to be an eligible loan under section 4013 (section 4013 loan).

D Additional Tables and Figures

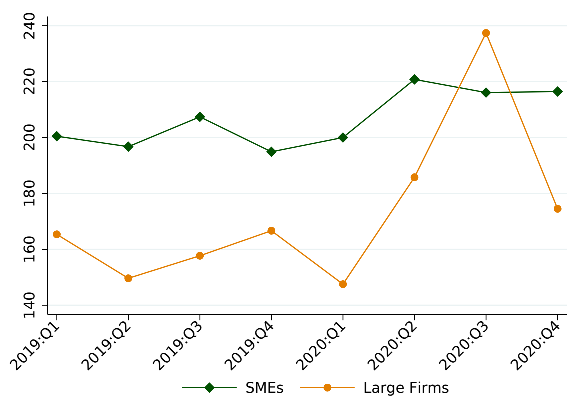
Figure D.1: Bank Loan Volume and Interest Rate Spread, 2019:Q1–2020:Q4

(a) Number of Loans (2019Q4=100)



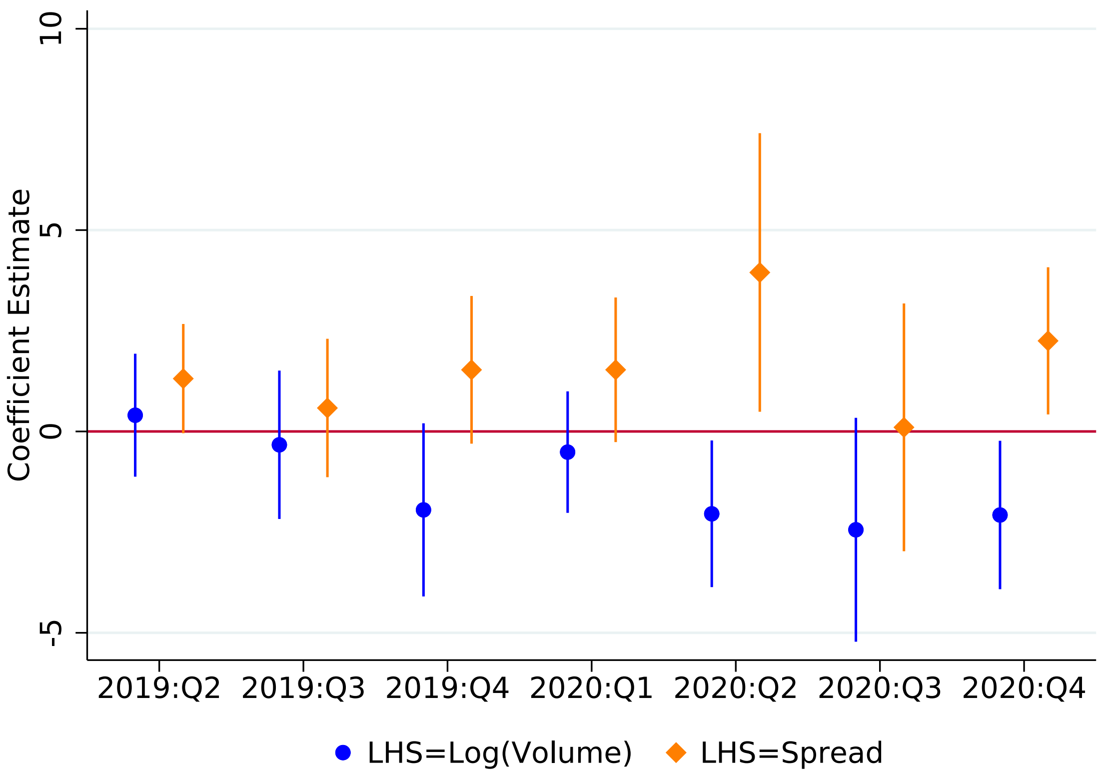
(b) Loan Volume (2019Q4=100)

(c) Spread (Basis Points)



Note: Panel (a) plots the quarterly number of new bank loans originated by Y-14 banks (indexed to 100 in 2019:Q4). Panel (b) plots the average volume of the loans (indexed to 100 in 2019:Q4). Panel (c) plots the average interest rate spread of the loans (in basis points). The figure breaks down new originations and spreads for loans to firms with up to \$500 million in annual sales (SMEs) versus firms with sales exceeding \$500 million.

Figure D.2: Dynamic Effects of Leverage on SME Loan Volumes and Spreads



Note: The figure shows the effects of leverage on log loan volume and spread of new loan origination to SMEs. The estimated model is based on that of Table 2, columns 2 and 4, but instead of pooling the effects of leverage and other firm controls within the pre- and post-COVID period, we estimate time-varying coefficients for each quarter in our sample. 90% confidence bounds are shown along with point estimates of leverage*quarter. All effects are relative to the omitted quarter, 2019:Q1.

Figure D.3: Rating Distribution of New Originations, Pre-COVID versus COVID Periods

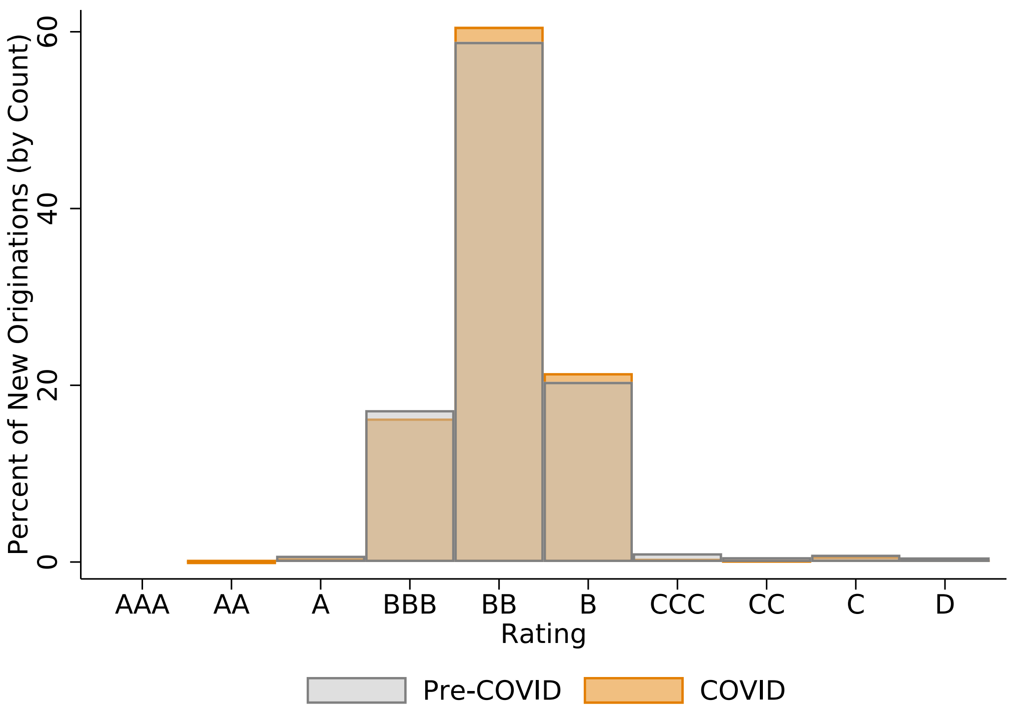


Figure D.4: Distribution of Collateral Types among New Originations, Pre-COVID versus COVID Periods

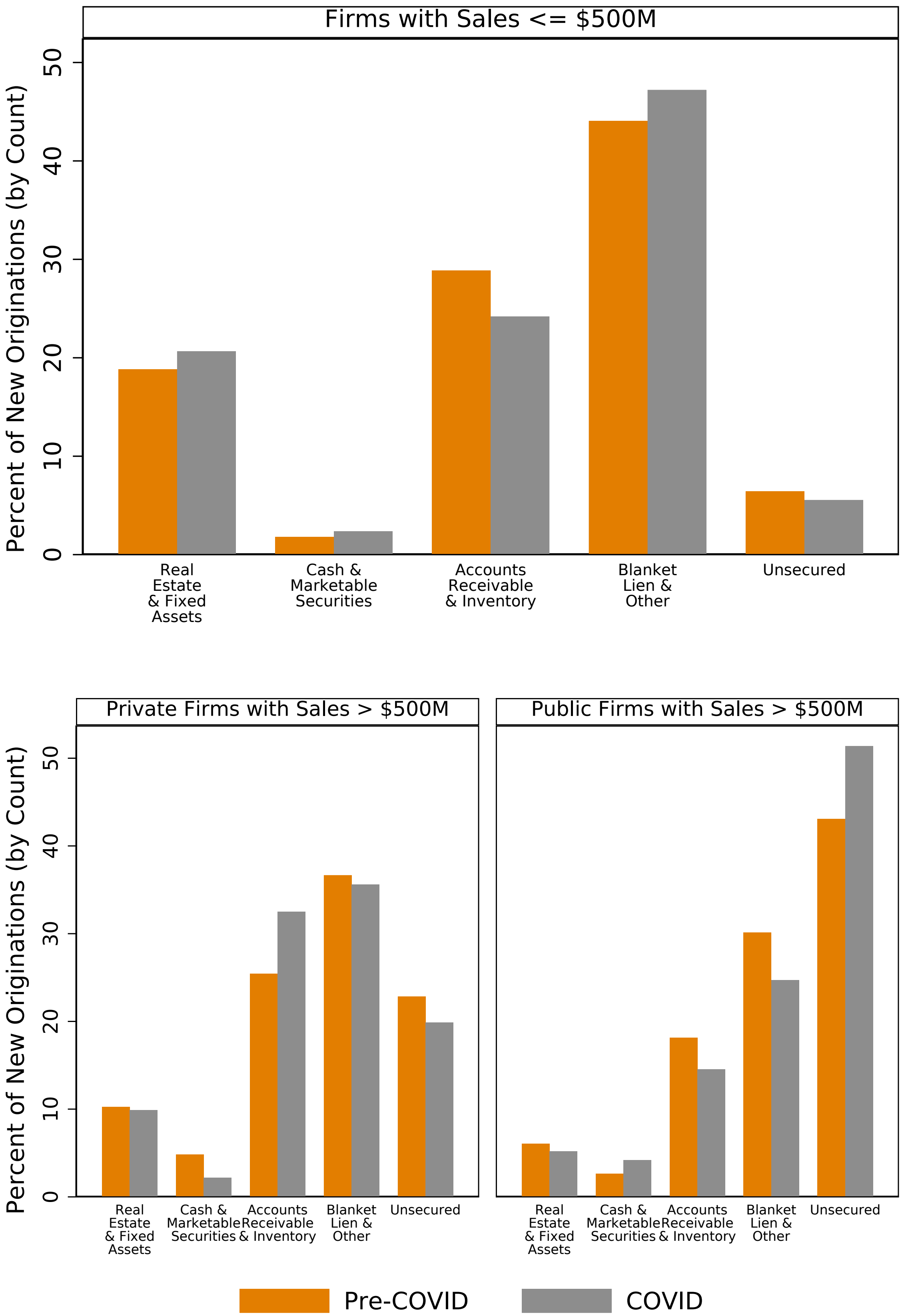


Table D.1: Effects of Leverage on Bank Credit to Large Firms

	Log(Volume), x100			Spread, BPS		
	(1)	(2)	(3)	(4)	(5)	(6)
COVID*Leverage	-0.843	1.079		-1.145	2.375	
	(2.706)	(2.245)		(1.450)	(2.395)	
COVID*Leverage*Low Capital Bank			1.802			2.324
			(2.174)			(2.496)
COVID*Leverage*High Capital Bank			0.780			2.046
			(2.295)			(2.791)
Observations	5,397	5,397	5,397	5,397	5,397	5,397
R-squared	0.060	0.728	0.728	0.105	0.714	0.715
Industry*Time FE	No	Yes	Yes	No	Yes	Yes
State*Time FE	No	Yes	Yes	No	Yes	Yes
Bank*Time FE	No	Yes	Yes	No	Yes	Yes
Rate Index Type FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Group Difference Est.			-1.022			-0.278
Group Difference P-value			0.6308			0.8833

Note: Coefficient estimates for Log(Volume) regressions are multiplied by 100. Spread is in basis points (BPS). Leverage is the ratio of total debt over EBITDA. COVID is an indicator that equals 1 from March 15, 2020, to the end of 2020 and 0 otherwise. Low Capital Bank is an indicator that equals 1 if a bank's capital buffer is below the median. Robust standard errors multi-way clustered at the state, industry, and bank levels are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table D.2: Effects of ICR-based Leverage Ratio on Bank Credit to Small Firms

	Log(Volume), x100			Spread, BPS		
	(1)	(2)	(3)	(4)	(5)	(6)
COVID*Leverage	0.002	0.006		-0.000	-0.003	
	(0.005)	(0.007)		(0.003)	(0.003)	
COVID*Leverage*Low Capital Bank			0.001			-0.003
			(0.005)			(0.003)
COVID*Leverage*High Capital Bank			-0.009			-0.002
			(0.034)			(0.017)
Observations	7,863	7,829	7,829	7,863	7,829	7,829
R-squared	0.269	0.586	0.586	0.201	0.628	0.628
Industry*Time FE	No	Yes	Yes	No	Yes	Yes
State*Time FE	No	Yes	Yes	No	Yes	Yes
Bank*Time FE	No	Yes	Yes	No	Yes	Yes
Rate Index Type FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Group Difference Est.			-0.010			0.002
Group Difference P-value			0.7819			0.9211

Note: Coefficient estimates for Log(Volume) regressions are multiplied by 100. Spread is in basis points (BPS). Leverage is the ICR—ratio of EBITDA over interest payment. The sample for these regressions is the same as that for Table **??**, except some firms have missing values for ICR. COVID is an indicator that equals 1 from March 15, 2020, to the end of 2020 and 0 otherwise. Low Capital Bank is an indicator that equals 1 if a bank's capital buffer is below the median. Robust standard errors multi-way clustered at the state, industry, and bank levels are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table D.3: Effects of Debt-to-Assets Leverage Ratio on Bank Credit to Small Firms

	Log(Volume), x100			Spread, BPS		
	(1)	(2)	(3)	(4)	(5)	(6)
COVID*Leverage	5.069	-2.575		14.272*	30.027*	
	(19.984)	(18.212)		(6.974)	(15.283)	
COVID*Leverage*Low Capital Bank			-12.395			33.028**
			(22.051)			(14.758)
COVID*Leverage*High Capital Bank			60.718			44.263
			(38.682)			(26.824)
Observations	8,292	8,292	8,292	8,292	8,292	8,292
R-squared	0.272	0.580	0.581	0.203	0.621	0.621
Industry*Time FE	No	Yes	Yes	No	Yes	Yes
State*Time FE	No	Yes	Yes	No	Yes	Yes
Bank*Time FE	No	Yes	Yes	No	Yes	Yes
Rate Index Type FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Group Difference Est.			73.113			11.235
Group Difference P-value			0.1210			0.6154

Note: Coefficient estimates for Log(Volume) regressions are multiplied by 100. Spread is in basis points (BPS). Leverage is the debt-to-assets ratio. COVID is an indicator that equals 1 from March 15, 2020, to the end of 2020 and 0 otherwise. The sample for these regressions is the same as that for Table **??**. COVID is an indicator that equals 1 from March 15, 2020, to the end of 2020 and 0 otherwise. Low Capital Bank is an indicator that equals 1 if a bank's capital buffer is below the median. Robust standard errors multi-way clustered at the state, industry, and bank levels are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table D.4: Summary Statistics for Loan-Level New-Origination Estimation Sample, by Bank Capital Buffer Status

	Low Capital Buffer				High Capital Buffer				P-value
Variable	Count	Mean	Std. Dev.	Median	Count	Mean	Std. Dev.	Median	(H_0: Equal Means)
Volume ($M)	5,986	11.33	17.00	4.50	2,306	11.36	17.65	4.56	0.9430
Spread (BPS)	5,986	212.97	109.30	210.00	2,306	186.70	105.33	195.50	0.0000
Leverage (Debt/EBITDA)	5,986	3.30	4.37	2.11	2,306	3.65	4.60	2.48	0.0010
Net Sales ($M)	5,986	129.59	129.73	74.89	2,306	119.26	125.95	67.54	0.0011
Profit Margin (Income/Assets)	5,986	0.13	0.15	0.09	2,306	0.12	0.14	0.08	0.0033
Tangibility (PPE/Assets)	5,986	0.33	0.29	0.25	2,306	0.33	0.28	0.28	0.4111
Liquidity (Cash+Equiv./Assets)	5,986	0.08	0.11	0.04	2,306	0.08	0.10	0.04	0.0267

Note: This table compares the distribution of firm attributes for firms that borrowed from banks with above- or below-median levels of capital buffer (that is, high- or low-capital-buffer banks), respectively. Together, these firms constitute the sample for the new-origination regressions. PPP Loan Amount and Sales are in million dollars. Investment Rate is defined as 12-month trailing capital expenditures reported in Q4 of year t ($t = 2019, 2020$) normalized by the preceding year's Q4 capital stock of property, plant, and equipment (PPE). Debt/Income is the ratio of total debt over EBITDA. Sales growth is the change in 12-month net sales over the preceding year's net sales. Profit margin is operating income over assets. Tangibility is PPE over total assets. Liquidity is cash plus marketable securities over total assets. Unutilized exposure is the amount of credit lines not yet drawn (utilized). Debt/Income, Net Sales, Sales Growth, Profit Margin, Tangibility, Liquidity, and Unutilized Exposure/PPE are all lagged by one year vis-à-vis investment.

Table D.5: Summary Statistics for Firm-Level Investment Estimation Sample

(a) PPP Borrowers, 2020Q4

	Count	Mean	Std. Dev.	p5	p25	p50	p75	p95
PPP Loan Amount	1,326	1.74	1.93	0.15	0.53	1.08	2.18	5.93
Employees	1,184	115.41	201.69	4.00	25.00	60.00	130.00	400.00
Investment Rate (%)	1,326	0.27	0.58	0.00	0.00	0.09	0.30	1.09
Leverage (Debt/EBITDA)	1,326	4.49	5.93	0.17	1.28	2.60	5.25	15.09
Net Sales ($M)	1,326	69.8	78.8	10.0	21.9	43.5	84.5	228.2
Sales Growth	1,326	0.06	0.24	-0.20	-0.03	0.04	0.13	0.36
Profit Margin (Income/Assets)	1,326	0.11	0.12	0.00	0.04	0.08	0.15	0.33
Tangibility (PPE/Assets)	1,326	0.22	0.21	0.01	0.05	0.14	0.34	0.66
Liquidity (Cash+Equiv./Assets)	1,326	0.07	0.10	0.00	0.01	0.03	0.10	0.28
Unutilized Exposure/PPE	1,326	3.81	10.51	0.00	0.04	0.52	2.43	18.21

(b) Non-PPP Borrowers, 2020Q4

	Count	Mean	Std. Dev.	p5	p25	p50	p75	p95
PPP Loan Amount	777	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Employees	524	328.62	947.14	2.00	10.00	80.00	314.50	1,224.00
Investment Rate (%)	777	0.27	0.62	0.00	0.00	0.10	0.27	0.98
Leverage (Debt/EBITDA)	777	3.95	6.20	0.12	1.02	2.23	4.17	14.85
Net Sales ($M)	777	154.2	205.4	11.2	37.6	92.3	199.2	472.2
Sales Growth	777	0.09	0.30	-0.16	-0.02	0.05	0.13	0.49
Profit Margin (Income/Assets)	777	0.12	0.12	-0.01	0.04	0.09	0.15	0.38
Tangibility (PPE/Assets)	777	0.29	0.25	0.01	0.08	0.22	0.45	0.80
Liquidity (Cash+Equiv./Assets)	777	0.07	0.10	0.00	0.01	0.04	0.10	0.30
Unutilized Exposure/PPE	777	3.02	10.55	0.00	0.00	0.33	1.31	13.34

Note: This table compares the distribution of firm attributes for firms that received PPP loans versus firms that did not, respectively. Together, they constitute the sample for the firm-level investment regressions. PPP Loan Amount and Sales are in million dollars. Investment Rate is defined as 12-month trailing capital expenditures reported in Q4 of year t ($t = 2019, 2020$) normalized by the preceding year's Q4 capital stock of property, plant, and equipment (PPE). Debt/Income is the ratio of total debt over EBITDA. Sales growth is the change in 12-month net sales over the preceding year's net sales. Profit margin is operating income over assets. Tangibility is PPE over total assets. Liquidity is cash plus marketable securities over total assets. Unutilized exposure is the amount of credit lines not yet drawn (utilized). Debt/Income, Net Sales, Sales Growth, Profit Margin, Tangibility, Liquidity, and Unutilized Exposure/PPE are all lagged by one year vis-à-vis investment.

Table D.6: Effect of Leverage on SME Holdings of Cash and Cash Equivalents

	Cash/PPE, %					
				IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	0.880***					
	(0.260)					
COVID*Leverage		-0.050	-0.254***	-0.347	-0.066**	-0.029
		(0.034)	(0.062)	(0.273)	(0.032)	(0.118)
COVID*Leverage*Loan Modification			0.228***	0.337		
			(0.049)	(0.310)		
COVID*Leverage*PPP Loan					0.027	-0.021
					(0.039)	(0.186)
Observations	5,691	5,691	5,691	5,691	5,691	5,691
R-squared	0.242	0.269	0.271	0.215	0.269	0.212
Industry FE	Yes	No	No	No	No	No
State FE	Yes	No	No	No	No	No
Firm Controls	Yes	No	No	No	No	No
COVID*Industry FE	No	Yes	Yes	Yes	Yes	Yes
COVID*State FE	No	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	Yes	Yes	Yes
First-stage Robust F statistics				66.333		43.986
P-value for H_0: Underidentified				0.0152		0.0313

Note: PPP Loan is 0 if a firm did not receive a PPP loan, and 1 if a firm received a PPP loan. Columns (4) and (8) use eligibility as an instrument for PPP Loan (and the interactions).

Table D.7: Effect of Leverage on SME Inventory and Accounts Receivables

	Inventory+AR/PPE, %					
				IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	-1.061					
	(0.851)					
COVID*Leverage		-0.396***	-0.626***	-0.326	-0.334*	0.066
		(0.122)	(0.200)	(1.000)	(0.195)	(0.400)
COVID*Leverage*Loan Modification			0.251	-0.111		
			(0.277)	(1.104)		
COVID*Leverage*PPP Loan					-0.102	-0.801
					(0.197)	(0.702)
Observations	5,668	5,668	5,668	5,668	5,668	5,668
R-squared	0.382	0.394	0.395	0.332	0.395	0.330
Industry FE	Yes	No	No	No	No	No
State FE	Yes	No	No	No	No	No
Firm Controls	Yes	No	No	No	No	No
COVID*Industry FE	No	Yes	Yes	Yes	Yes	Yes
COVID*State FE	No	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	Yes	Yes	Yes
First-stage Robust F statistics				66.639		44.042
P-value for H_0: Underidentified				0.0023		0.0315

Note: PPP Loan is 0 if a firm did not receive a PPP loan, and 1 if a firm received a PPP loan. Columns (4) and (8) use eligibility as an instrument for PPP Loan (and the interactions).

Table D.8: Effect of Leverage on Debt Financing at SMEs Not Reliant on Y-14 Banks

	Y14 Debt/PPE, %			Total Debt/PPE, %		
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	-22.736*			-24.060		
	(13.259)			(60.697)		
COVID*Leverage		1.861			5.530	
		(4.809)			(7.943)	
COVID*Leverage*Reliance on Low Cap. Bank			3.065			9.319
			(5.271)			(9.332)
COVID*Leverage*Reliance on High Cap. Bank			-8.213			-17.450
			(5.463)			(27.384)
Observations	8,604	8,604	8,604	8,604	8,604	8,604
R-squared	0.130	0.146	0.147	0.253	0.267	0.268
Industry FE	Yes	No	No	Yes	No	No
State FE	Yes	No	No	Yes	No	No
Firm Controls	Yes	No	No	Yes	No	No
COVID*Industry FE	No	Yes	Yes	No	Yes	Yes
COVID*State FE	No	Yes	Yes	No	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	No	Yes	Yes
Group Difference Est.			11.279			26.769
Group Difference P-value			0.1924			0.4085

Note: This table reports the effect of leverage during the pandemic on debt funding at the firm level. The dependent variables are (i) total outstanding balance of bank loans from all Y-14 banks (columns 1 and 2), (ii) total balance of debt from sources other than Y-14 banks (columns 3 and 4), and (iii) total level of all debt (columns 5 and 6). All dependent variables are scaled by the PPE capital stock at the preceding year-end. Leverage is the ratio of total debt over EBITDA measured at the preceding year-end. COVID is an indicator equal to 1 for 2020 and 0 otherwise. The sample includes all SMEs with less than 50 percent of their total debt accounted for by loans from Y-14 banks. In columns (2), (4), and (6), the effect of leverage during the pandemic is allowed to differ depending on whether a firm obtained more than half of its Y-14 loans from low-capital banks (for which we find in the previous section evidence of credit supply shock). Robust standard errors multi-way clustered at the state and industry levels are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table D.9: Effect of Leverage on Debt Financing at Large Firms

	Y14 Debt/PPE, %			Total Debt/PPE, %		
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	-3.246			-21.165		
	(9.890)			(32.883)		
COVID*Leverage		6.453*			7.923	
		(3.476)			(8.367)	
COVID*Leverage*Reliance on Low Cap. Bank			6.088*			7.646
			(3.571)			(12.293)
COVID*Leverage*Reliance on High Cap. Bank			6.957			-1.085
			(5.467)			(22.433)
Observations	3,829	3,829	3,829	3,829	3,829	3,829
R-squared	0.281	0.315	0.324	0.246	0.280	0.290
Industry FE	Yes	No	No	Yes	No	No
State FE	Yes	No	No	Yes	No	No
Firm Controls	Yes	No	No	Yes	No	No
COVID*Industry FE	No	Yes	Yes	No	Yes	Yes
COVID*State FE	No	Yes	Yes	No	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	No	Yes	Yes
Group Difference Est.			-0.869			8.731
Group Difference P-value			0.8900			0.7630

Note: This table reports the effect of leverage during the pandemic on debt funding at the firm level. The dependent variables are (i) total outstanding balance of bank loans from all Y-14 banks (columns 1 and 2), (ii) total balance of debt from other sources than Y-14 banks (columns 3 and 4), and (iii) total level of all debt (columns 5 and 6). All dependent variables are scaled by the PPE capital stock at the preceding year-end. Leverage is the ratio of total debt over EBITDA measured at the preceding year-end. COVID is an indicator equal to 1 for 2020 and 0 otherwise. The sample include only large firms. In columns (2), (4), and (6), the effect of leverage during the pandemic is allowed to differ depending on whether a firm obtained more than half of its Y-14 loans from low-capital banks (for which we find in the previous section evidence of credit supply shock). Robust standard errors multi-way clustered at the state and industry levels are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table D.10: Effects of Leverage and Credit Support Programs on Investment at SMEs Not Reliant on Low-Capital Y-14 Banks

	Investment Rate, %					
				IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	-3.702***					
	(1.338)					
COVID*Leverage		0.166	0.658	0.994	-0.030	0.484
		(0.437)	(0.748)	(1.343)	(0.444)	(0.952)
COVID*Leverage*Loan Modification			-0.564	-0.888		
			(0.725)	(1.901)		
COVID*Leverage*PPP Loan					0.333	-0.569
					(0.404)	(1.425)
Observations	9,401	9,401	9,401	9,401	9,401	9,401
R-squared	0.079	0.090	0.090	0.043	0.090	0.047
Industry FE	Yes	No	No	No	No	No
State FE	Yes	No	No	No	No	No
Firm Controls	Yes	No	No	No	No	No
COVID*Industry FE	No	Yes	Yes	Yes	Yes	Yes
COVID*State FE	No	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	Yes	Yes	Yes
First-stage Robust F statistics				.		45.269
P-value for H_0: Underidentified				.		0.0035

Note: This table reports the effect of leverage during the crisis on firm investment, along with the effect of two credit support programs. The dependent variable investment rate is defined as 12-month trailing capital expenditures reported in Q4 of year $t, t = 2019, 2020$ normalized by the preceding year-end's plant, property, and equipment. Leverage is the ratio of total debt over EBITDA measured at the preceding year-end. COVID is an indicator that equals 1 for 2020 and 0 otherwise. Columns (3) and (4) consider loan extensions, with Loan Modification being an indicator equal to 1 if one or more of a firm's Y-14 loans were extended within the year and 0 otherwise. Columns (5) and (6) consider the PPP, with PPP Loan being an indicator equal to 1 if a firm received a PPP loan and 0 otherwise. Column (5) uses binned minimum number of days to maturity as instruments for actual extension. Column (6) uses eligibility for the PPP as an instrument for actual uptake. The sample consists of SMEs with less than 50 percent of their total debt accounted for by loans from Y-14 banks or Y-14–dependent firms with less than 50 percent of their Y-14 loans from low-capital banks. Robust standard errors multi-way clustered at the state and industry levels are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table D.11: Effects of Leverage and Credit Support Programs on Employment at SMEs Not Reliant on Low-Capital Y-14 Banks

	Log(Employees), x100					
				IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	-2.290					
	(2.727)					
COVID*Leverage		-0.400	-0.401	-1.714	-1.374	1.322
		(1.308)	(2.041)	(2.945)	(1.874)	(3.341)
COVID*Leverage*Loan Modification			-0.078	1.280		
			(1.448)	(3.864)		
COVID*Leverage*PPP Loan					1.666	-3.442
					(1.697)	(5.133)
Observations	7,567	7,567	7,567	7,567	7,567	7,567
R-squared	0.284	0.295	0.295	0.176	0.307	-0.620
Industry FE	Yes	No	No	No	No	No
State FE	Yes	No	No	No	No	No
Firm Controls	Yes	No	No	No	No	No
COVID*Industry FE	No	Yes	Yes	Yes	Yes	Yes
COVID*State FE	No	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	Yes	Yes	Yes
First-stage Robust F statistics				36.083		87.780
P-value for H_0: Underidentified				0.0243		0.0026

Note: This table reports the effect of leverage during the COVID-19 crisis on (log) firm employment, along with the effect of two credit support programs. The dependent variable is the logarithm of total employment as of December 2019 and 2020. Leverage is the ratio of total debt over EBITDA measured at the preceding year-end. COVID is an indicator that equals 1 for 2020 and 0 otherwise. Columns (3) and (4) consider loan extensions, with Loan Modification being an indicator equal to 1 if one or more of a firm's Y-14 loans were extended within the year and 0 otherwise. Columns (5) and (6) consider the PPP, with PPP Loan being an indicator equal to 1 if a firm received a PPP loan and 0 otherwise. Column (5) uses binned minimum number of days to maturity as instruments for actual extension. Column (6) uses eligibility for the PPP as an instrument for actual uptake. The sample consists of SMEs with less than 50 percent of their total debt accounted for by loans from Y-14 banks or Y-14–dependent firms with less than 50 percent of their Y-14 loans from low-capital banks. Robust standard errors multi-way clustered at the state and industry levels are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table D.12: Effects of Leverage and the Paycheck Protection Program on Investment (Large Firms)

	Investment Rate, %					
				IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	-4.547*					
	(2.272)					
COVID*Leverage		-0.162	-0.366	1.270	-0.106	0.257
		(0.565)	(0.768)	(1.459)	(0.581)	(0.594)
COVID*Leverage*Loan Modification			0.377	-2.156		
			(0.734)	(2.607)		
COVID*Leverage*PPP Loan					-0.888	-3.370
					(2.016)	(2.940)
Observations	3,745	3,745	3,745	3,745	3,745	3,745
R-squared	0.093	0.128	0.129	0.007	0.129	0.027
Industry FE	Yes	No	No	No	No	No
State FE	Yes	No	No	No	No	No
Firm Controls	Yes	No	No	No	No	No
COVID*Industry FE	No	Yes	Yes	Yes	Yes	Yes
COVID*State FE	No	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	Yes	Yes	Yes
First-stage Robust F statistics				11.913		8.343
P-value for H_0: Underidentified				0.0405		0.0045

Note: This table reports the effect of leverage during the crisis on real variables at the firm level and shows the effect of the public support program. The dependent variable in columns (1) through (4) is defined as 12-month trailing capital expenditures reported in Q4 of year $t, t = 2019, 2020$ normalized by the preceding year-end's plant, property, and equipment. The dependent variable in columns (5) through (8) is defined as the logarithm of total employment as of December 2019 and 2020. Leverage is the ratio of total debt over EBITDA measured at the preceding year-end. COVID is an indicator that equals 1 for 2020 and 0 otherwise. PPP Loan is an indicator variable equal to 1 if a firm received a PPP loan and 0 otherwise. Columns (4) and (8) use eligibility for the PPP as an instrument for actual uptake. The sample includes only large firms. Robust standard errors multi-way clustered at the state and industry levels are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table D.13: Effects of Leverage and the Paycheck Protection Program on Employment (Large Firms)

	Log(Employees), x100					
				IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)
COVID	7.492					
	(8.930)					
COVID*Leverage		-1.342	1.222	17.503**	-1.846	5.003
		(2.476)	(4.508)	(7.626)	(2.762)	(4.191)
COVID*Leverage*Loan Modification			-4.966	-35.565**		
			(5.158)	(14.025)		
COVID*Leverage*PPP Loan					3.908	41.617
					(4.361)	(35.573)
Observations	1,452	1,452	1,452	1,452	1,452	1,452
R-squared	0.299	0.341	0.342	-0.065	0.341	-2.466
Industry FE	Yes	No	No	No	No	No
State FE	Yes	No	No	No	No	No
Firm Controls	Yes	No	No	No	No	No
COVID*Industry FE	No	Yes	Yes	Yes	Yes	Yes
COVID*State FE	No	Yes	Yes	Yes	Yes	Yes
COVID*Firm Controls	No	Yes	Yes	Yes	Yes	Yes
First-stage Robust F statistics				.		13.777
P-value for H_0: Underidentified				.		0.0072

Note: This table reports the effect of leverage during the crisis on real variables at the firm level and shows the effect of the public support program. The dependent variable in columns (1) through (4) is defined as 12-month trailing capital expenditures reported in Q4 of year $t, t = 2019, 2020$ normalized by the preceding year-end's plant, property, and equipment. The dependent variable in columns (5) through (8) is defined as the logarithm of total employment as of December 2019 and 2020. Leverage is the ratio of total debt over EBITDA measured at the preceding year-end. COVID is an indicator that equals 1 for 2020 and 0 otherwise. PPP Loan is an indicator variable equal to 1 if a firm received a PPP loan and 0 otherwise. Columns (4) and (8) use eligibility for the PPP as an instrument for actual uptake. The sample includes only large firms. Robust standard errors multi-way clustered at the state and industry levels are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.