The Usability of Bank Capital Buffers and Credit Supply Shocks at SMEs during the Pandemic

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March 3, 2022

Abstract

Were banks reluctant to use Basel-III regulatory capital buffers to support lending to creditworthy SMEs during the COVID-19 pandemic? Confidential US loan-level data show that banks starting the pandemic with "low capital headroom" above the penalty-laden Basel-III regulatory buffers (1) reduced SME loan commitments by 4.9 percent more and (2) were 10 percent more likely to result in borrower exits ex-post, controlling for a host of demand factors. Credit effects across many industries (up to 16% of aggregate SME credit) also suggest real effects on local employment growth during the pandemic (2% slower annually). This study is the first to test the *usability* of Basel-III regulatory buffers *in a downturn*, and contribute a bank capital-based transmission channel to the SME-pandemic literature.

Keywords: Financial institutions, Capital Regulation, Procyclicality, COVID-19, SMEs

JEL classification: G20, G21, G28, D22

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1. Introduction

"Since the onset of the pandemic, however, questions have arisen over banks' ability and willingness to use the regulatory buffers available to them… in a period of stress, banks might react with many of the same procyclical behaviors that we've seen in the past, such as reigning back new business activity." – S&P Global, June 11, 2020[2]

Regulatory reforms implemented after the 2008 global financial crisis (GFC) played a central role in re-building banking system capital to the highest level in decades (nearly double that of 2008). Despite increased banking system capital and significant government support measures, business lending to small and medium sized enterprises (SMEs) was strained during the first few quarters of the COVID-19 pandemic. While much of the decline in business lending is attributable to loan demand and credit quality concerns, a key question remains as to whether banks used their large capital cushions built post-GFC to support lending to creditworthy SMEs during the pandemic. Our paper investigates a novel supply-side transmission channel related to the "usability of regulatory capital buffers" and whether banks that entered the pandemic with capital ratios close to their regulatory capital buffers constrained credit to creditworthy SMEs. Introduced as part of the Basel III capital reforms, regulatory capital buffers are costly regions of "rainy day" equity capital that sit on top of minimum capital requirements and are designed by regulation to act as a buffer to absorb losses and support lending in a downturn.[3] In contrast to minimum capital requirements, which are "hard" mandates that activate resolution procedures when breached, regulatory capital buffers represent a

[2] In addition, Andrea Enria, chair of the European Central Bank's Single Supervisory Mechanism, stated "There has been a concern that the buffers were not being used and there was a reluctance to use them." - Financial Times, January 28, 2021

[3] As part of the Basel III capital reforms, the Basel Committee on Banking Supervision (BCBS) introduced a series of measures to promote the buildup of regulatory capital buffers (i.e., the capital conservation buffer, the countercyclical capital buffer, and the capital surcharge for global systemically important banks) in good times that can be drawn upon in periods of stress to support new lending activity. See, "Consultative document: Strengthening the resilience of the banking sector," BCBS, BIS, December 2009. In the US implementation, the Federal Reserve introduced the stress capital buffer as a replacement for the capital conservation buffer. Institutional details on the implementation of regulatory capital buffers in the U.S. are described in Section 3.

"soft" mandate that limits the bank's ability to pay dividends and bonuses until its capital stock is rebuilt. These penalties were intended to act as a warning signal that disincentivized any unnecessary use of buffers in normal times and allowed banks time to recover from unforeseen shocks.[4]

At the onset of the pandemic, the Federal Reserve publicly encouraged banks to use these buffers to support the economy during the downturn.[5] However, the prospect of large pandemic-related losses during 2020 appears to have caused banks to take steps to reduce the likelihood of dipping into their regulatory buffers in an attempt to avoid incurring associated costs, despite elevated capital levels.[6] Revealed preference appears to suggest that banks found these buffers too costly to use. The proximity of a bank's capital ratio to its regulatory buffer threshold prior to the pandemic can be seen as a bank-specific measure of how binding the costs of the regulatory buffers were. Banks that started the pandemic with a capital ratio relatively close to the regulatory buffer threshold are referred to as "low capital headroom" banks for ease of exposition. In section 6, our event study analysis suggests that the costs of using the buffers can be relatively large during downturns (nearly -300 basis points cumulative abnormal stock return in a 3-day window), implying regulatory capital buffers were too costly to use during the COVID-19 pandemic.

Figure 1 shows an outsized decline in the number of reported private SME exposures from low capital headroom banks during the pandemic whereas the number for high capital headroom banks remained relatively stable.[7] While some lending relationships may have ended due to pandemic-related demand side factors, the relative difference between the two lines (high and low capital headroom banks) suggests that a sizeable number of SMEs may have experienced credit supply shocks during the

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[4] Specifically, there are at least three reasons that the usage of regulatory buffers might prove costly for banks. First, associated dividend restrictions may lead to market stigma concerns for bank shareholders. Second, associated bonus restrictions for executive compensation may prove too costly for bank managers. Third, dipping into capital buffers may lead to downgrade risk from credit rating agencies. On a side note, even if regulators were to release some of the aforementioned costs, banks may still not find it optimal to dip into their buffers to maintain credit if regulators do not provide strong forward guidance as to how long banks would have before they are expected to replenish their buffers (IMF GFSR, 2021).

[5] See https://www.federalreserve.gov/newsevents/pressreleases/monetary20200315b.htm for the official press release.

[6] This regulatory issue expands beyond the case of the United States. In response to the concern that buffers were not be used, the ECB even went as far as to provide pandemic capital relief by temporarily eliminating a significant portion of regulatory capital buffers.

[7] As described in Section 4, we define a firm to be SME if the firm size is less than the median firm size as of 2019Q4.

pandemic due to the un-usability of regulatory capital buffers.[8] We highlight a few facts to better understand the characteristics of firms that lost access to credit with low capital headroom banks during the pandemic period (Figure 1, red line). First, the affected firms were relatively small and bank-dependent – the median borrower had assets of about $6 million, and the largest firm had assets of about $35 million. Nearly all firms in our sample had lending relationships with only one large bank. This suggests these firms may have found it costly to substitute towards other sources of financing. Second, the small firms were highly profitability, demonstrating an average return on assets of about 15 percent pre-pandemic. Third, borrower leverage, measured as debt-to-assets, averaged about 25 percent, suggesting this set of firms were prudently managed and not heavily levered. Lastly, borrowers are spread across all 50 states and a diverse set of industries. Taken together, these facts suggest SMEs that banked with low capital headroom banks were creditworthy and may have lost access to credit for reasons other than demand-side considerations.

To examine the result suggested by Figure 1 more formally, we utilize a novel set of confidential, supervisory loan-level data (FR Y-14Q) between the largest U.S. banks and their corporate borrowers.[9] This granular data provides us with a unique advantage to observe the lending outcomes at an important, yet under-studied segment of the economy, namely, private SMEs. Although identification of a credit supply effect would ideally compare changes in lending across two banks lending to the same firm (Khwaja and Mian, 2008), nearly all private SMEs have a single lender. To overcome the identification challenge for single-bank SMEs, we proceed with two approaches.[10] Firstly, we extend the Khwaja and Mian (2008) approach and compare the lending of low- versus high capital headroom banks to *groups of similar borrower firms*. Specifically, these borrowers are grouped

[8] This is covered more formally in our firm exit specifications.
[9] Given the minimum loan reporting threshold of $1 million USD, this data excludes small business loans (according to the thresholds defined in Call Reports). Y-14 data excludes PPP loan balances.
[10] Another point to note is that while the ex-ante size of excess capital headroom may be endogenous with respect to future lending opportunities, these headroom sizes did not incorporate the arrival of the pandemic recession. Thus, the size of capital headroom is orthogonal with respect to changes in risk or lending opportunities associated with the unexpected arrival of the COVID-19 pandemic recession.

by a variety of firm characteristics in the form of industry * time, location * time, borrower risk * time, and financial constraint status * time fixed effects.[11] In this way, our analysis shows that the relative closeness of a bank's capital ratio to the costly regulatory buffer region ex-ante leads to significant changes in credit growth to a variety of firm groups ex-post, after controlling for time-varying demand shocks that are common to all firms within each group. We also present evidence of parallel trends for credit growth across treatment and control groups prior to the arrival of the pandemic. Secondly, we provide additional robustness for the purpose of identifying credit supply shocks, by showing evidence that low capital headroom banks contract credit to firms *whose pre-pandemic credit lines contractually matured at the start of the pandemic* (as compared to similar firms whose credit lines were contractually locked in). This exercise provides additional robustness for the purpose of identifying credit supply shocks, as the selection rule for these treatment firms (firms with maturing credit lines) comes from a pre-determined variable (i.e., the maturity of a pre-pandemic contract), which was determined several years prior to the unanticipated arrival of the pandemic. This suggests the results are not driven by changes in loan demand, but rather a supply-side change in bank credit policies. Specifically, this result is consistent with the notion that banks needing to shed loan exposures (e.g., to avoid regulatory capital buffer usage) will find it less costly to cut lending to this specific group of firms from a legal and contractual standpoint. In this way, the lender avoids any costs associated with violating contract terms of a pre-existing commitment, and can instead decline credit to firms whose credit lines are up for renegotiation.[12]

We utilize the granularity of this data to explore a second question: did low capital headroom banks curtail lending to certain *types* of firms more than others during the pandemic? First, low capital

[11] In the cross sectional (firm exit) specifications to follow, these firm groups will be implemented through the form of fixed effects, whereas in the panel data specifications, the fixed effects will include an interaction with date, i.e. financial constraint status*date, risk*date, geography*date, and industry*date. More specifically, instead of the firm*time fixed effects (which would absorb single-bank firms) in the Khwaja and Mian approach, we use firm type*time fixed effects.

[12] Any interpretation of this result as being reflective of loan demand is unlikely, as this would require proposing a story for why firms with pre-existing credit lines that happen to mature during the *unanticipated* arrival of the pandemic would have lower loan demand than firms whose credit lines were contractually locked in during the pandemic.

headroom banks disproportionately curtailed lending to private SMEs while leaving their valuable relationships with large public ("core") clients untouched. Second, low capital headroom banks curb lending to firms whose lending relationship is relatively young (less than the median relationship age of 6 years). This is consistent with the literature on relationship lending (Bharath et al. (2011)), which attributes a larger termination cost associated with older relationships.

With parallel trends in pre-pandemic lending growth between low capital headroom banks and high capital headroom banks, we find that SMEs borrowing from low capital headroom banks were up to 10 percent more likely to exit during the pandemic. Furthermore, low capital headroom banks reduced loan commitment growth to SMEs by an average of 4.9 percentage points more annually during the pandemic than high capital headroom banks. In aggregate, these credit effects comprise up to 16 percent in terms of aggregate SME commitments.[13] We also present some evidence that reductions in access to credit is associated with real effects at the industry-county level. Since our data does not contain data on firm-level employment, we show that industry-counties with ex-ante credit exposures to low capital headroom banks demonstrate 2 percent slower annualized employment growth during the pandemic as compared to industry-counties without such exposures.

The evidence presented in our paper suggests regulatory capital buffers may be acting as a "double-edged" policy sword, where the costliness of regulatory capital buffers that incentivized banks to raise their CET1 ratios to historically high levels *during normal times* likely also made buffers *difficult to use during the downturn.* Proposing policy recommendations to improve the usability of capital buffers requires identifying the specific costs associated with their usage that are most binding for banks. As explored in section 6, potential policy recommendations include improving the transparency of the buffer requirement to reduce market stigma – for example, reassuring market

[13] In the appendix, we explore whether borrowers with low capital headroom banks were more likely to substitute toward Paycheck Protection Program (PPP) forms of financing. Our results suggest there is no statistical difference in the likelihood that a low capital headroom bank (versus a high capital headroom bank) substitutes toward PPP lending. This suggests the probability of our credit effects translating into real effects.

participants and credit ratings agencies that bank decisions to dip into their buffers does not necessarily signal weakness – or providing temporarily relief from the buffer constraint in downturns. Beyond this, if some form of buffer relief is granted, banks may still not find it incentive compatible to use buffers in a downturn if clear forward guidance is not provided about the precise timeframe of the relief (Arnold (2021) and IMF GSFR (2021)).

Section 2 summarizes related literature, section 3 provides background on the capital buffer regime under Basel III, section 4 describes our empirical specifications, section 5 discusses our results, section 6 highlights a few policy considerations, and section 7 concludes.

2. Literature Review and Contribution

New to the COVID-19 literature, our paper uncovers the presence of a *transmission channel emanating from regulatory capital buffer constraints* that significantly affected SMEs during the pandemic. Complementing studies that document the performance of SMEs during the pandemic, our paper establishes a supply-side (feedback) transmission channel that likely contributed to these conditions. In this way, our study contributes a new bank capital angle to an expanding literature that studies the various effects of the COVID-19 pandemic shock on the condition of private SMEs. For example, Bloom et al. (2021) use survey data on an opt-in panel of around 2,500 US small businesses to assess the impact of COVID-19 and find a significant negative sales impact that peaked with an average loss of 29 percent in sales. Of these, almost a quarter reported losses of more than 50 percent. In addition, they find these impacts to be persistent, as firms reporting the largest sales drops in mid-2020 were still forecasting large sales losses a year later in mid-2021. Gourinchas et al. (2020) estimate the impact of the COVID-19 crisis on business failures among SMEs in seventeen countries using a large representative firm-level database. They estimate a large increase in the failure rate of SMEs under COVID-19 of nearly 9 percentage points, absent government support. Alekseev et al. (2020) use survey data collected via Facebook and find that about a quarter of small businesses had access to financing from financial institutions, and most small businesses were reliant on personal savings and informal sources of financing during the pandemic. Kapan and Minoiu (2021) find that despite the unexpected surge in credit line drawdowns at the onset of the COVID-19 pandemic, banks with significant exposures to credit lines tightened their lending standards and cut their C&I loan supply to small businesses. Chodorow-Reich et al (2020) document that, unlike large firms, SMEs take loans of shorter maturity, have less active maturity management, post more collateral, pay higher spreads, and have higher utilization rates. These facts, in their view, explain why during the pandemic SMEs did not draw down their credit lines as much as large firms did. Strahan and Li (2021) analyze the bank supply of credit under the emergency Paycheck Protection Program (PPP) and conclude that PPP loans reflect a benefit of bank relationships as they facilitate firms' access to government-subsidized lending. Our results are consistent and complementary to the findings in these papers, and cover a broader class of firms (those with young lending relationships as well as credit lines maturing at the start of the pandemic). In addition, our paper contributes a novel bank capital-based *transmission channel* that affected firms during the pandemic due to the procyclical lending response to the usability of capital buffers.

In relation to the literature studying the credit impacts of *"hard-mandate"* capital requirements (Kashyap, Stein, and Hanson, 2010; Basel Committee on Banking Supervision, 2010; Acharya, Engle, Richardson, 2012; Admati, et al., 2013; Baker and Wurgler, 2015; Sarin and Summers, 2016; Aiyar et al., 2014; Hanson, Kashyap and Stein, 2011; Greenwood et al., 2017; Financial Stability Board, 2020), relatively little is known about the effects of new Basel III "soft mandate" policy tools, such as regulatory capital buffers, *particularly during downturns like the pandemic*.[14] This literature can be categorized into two groups. The first set of papers present evidence on pre-Basel III changes in

[14] Minimum requirements are "hard" mandates that send a bank into resolution when breached. Regulatory capital buffers, on the other hand, are "soft" requirements that allow banks time to try to recover. If the buffer is breached, the bank's ability to pay dividends and bonuses is restricted until its capital stock is rebuilt.

capital regulation and unequivocally find that higher regulatory requirements reduce bank lending. Jiménez et al. (2017) studies bank lending responses to dynamic provisioning experiments in Spain and find that countercyclical regulatory capital buffers help to smooth credit cycles. Gropp et al. (2018) provide evidence for a similar lending response in European banking data to the 2011 European Banking Authority capital exercise, showing that large European banks required to maintain a higher capital ratio in the 2011 capital exercise responded by reducing total asset size, while keeping equity capital and asset risk constant. Behn, Haselmann and Wachtel (2016) and Fraisse, Lé, and Thesmar (2020) use German and French loan-level data, respectively, to show that banks are more likely to cut lending when capital charges on loans, under Basel II rules, increase. Meanwhile, the second set of papers based on US loan-level data explore the impact of Basel III regulatory capital buffers on lending outcomes *during normal times*. Specifically, Berrospide and Edge (2019) find that the introduction of regulatory capital buffers emanating from stress test disclosures led to a lower growth in C&I loan commitments, while Favara, Rezende, and Ivanov (2021) find that time variation in GSIB surcharge regulatory buffers result in significant declines in C&I loan commitments by GSIBs. As both of these papers concentrate on pre-pandemic boom periods, they both contribute the important finding that soft mandate Basel III regulatory capital buffers did in fact play a key role in getting bank system capital to the historically high levels prior to the arrival of the pandemic. Our paper can be seen as a combination of both categories, as it is the first to empirically test whether the *Basel III* regulatory buffers were in fact usable *during a (pandemic) downturn*. We find evidence pointing to procyclical impacts of regulatory capital buffers during the pandemic downturn, particularly on private SMEs and other non-core firms for which it was relatively cheap to cut lending to.

Finally, our results also point to a different interpretation of the Basel III regulatory capital buffers. Rather than seeing the buffers as a cushion to be drawn upon during a downturn, as originally intended by Basel III, banks seem to be treating the regulatory buffers as additional minimum requirements. Several studies have been conducted to enumerate various possible reasons why banks may or may not find buffers expensive. Abad and Pascual (2022) use market expectations to show that banks only decide to use their buffers if the value creation from a larger loan book offsets the costs associated with a capital shortfall, which the authors find to be a rare occurrence. The IMF's Global Financial Stability Report (GFSR) in April 2021 addressed the usability of capital buffers and documents that, despite the vital role of capital buffers to ensure continued supply of credit to the real economy, banks remain reluctant to draw down their buffers.[15] Using a sample of 72 large global banks, representing 60 percent of the global banking system's aggregate market capitalization, the report finds that only banks accounting for 5 percent of market capitalization clear the hurdles to use their buffers. Thus, banks seem to lack the economic incentives to dip into their capital buffers as regulation will require them to rebuild their buffers later. Low returns could make the usability of buffers a costly option if the additional value generated by the new lending does not offset the negative impact from the capital shortfall resulting from using the buffers in the first place. Schmitz et al. (2021) analyze possible stigma effects arising from distribution restrictions associated with a breach in the capital buffers for European banks during the pandemic. Their analysis explores potential negative spillover effects to overall bank funding costs, and finds evidence against this channel. Altogether, this hints that channels associated with ratings agency pressure and the costliness of rebuilding buffers post-crisis may be more probable candidate reasons. Kleinnijenhuis, Kodres, and Wetzer (2020) point to a lack of usable capital and propose several possible improvements in current capital framework that could overcome such issues, such as setting clear expectations about the pathway banks should follow to rebuild their buffers post-crisis.

[15] See, Chapter 1, "An Asynchronous and Divergent Recovery may put Financial Stability at Risk," pages 22-25.

3. Capital Ratios and Basel III Regulatory Capital Buffers

This section outlines some background on the Common Equity Tier 1 (CET1) capital ratio and regulatory capital buffers, as implemented in the U.S. via Basel III. Bank CET1 capital ratios can be split into three parts:

CET1 Capital Ratio = Minimum Requirement + Regulatory Capital Buffers + Excess Headroom

- A regulatory minimum requirement to prevent undercapitalization. Following the Basel III capital rules, this is 4.5 percent for all banks and marks the ("hard" mandate) threshold below which a bank would be deemed insolvent by regulators. If a bank enters this regime, resolution procedures would be set in motion.[16]
- Basel III regulatory capital buffers, such as the Global Systemically Important Bank (GSIB) surcharge, countercyclical capital buffer (CCyB), and the stress capital buffer. These are costly regions of "rainy day" capital that come with payout and bonus restrictions ("soft" mandate). Whereas the CCyB is symmetric across banks, the GSIB surcharge and stress capital buffer vary across banks, depending on the bank's risk profile. These buffers are designed to provide added resilience to absorb bank losses in the event of a stress scenario.
- Excess headroom reflects the CET1 capital ratio level of bank capital in excess of regulatory minimums plus regulatory buffers. For most large firms, this cushion is typically 3 percent or less.

[16] Several papers provide theoretical rationale for why banks find it optimal to maintain an equilibrium level of capital in excess of regulatory minimum requirements. Using a dynamic equilibrium model of relationship lending in which banks are unable to access the equity markets every period and the business cycle determines loans' probabilities of default, Repullo and Suarez (2013) show that banks hold endogenous capital buffers as a precaution against shocks that impair their future lending capacity. Koch et al. (2016) compare optimal capital structure prior to the Great Depression, when no government guarantees existed, versus that of the Great Recession, and suggest that market discipline would have induced the largest US banks to maintain higher capital buffers prior to the 2008 crisis. Baron (2020) further provides support for the case of strengthening countercyclical capital buffers since government guarantees can distort the incentives of banks to raise new equity and affect the dynamics of bank capital structure over the credit cycle. Nier and Zicchino (2008) provide evidence that losses lead to greater pull-back in lending for banks at a lower initial level of capital.

This excess cushion approximates the amount of capital that banks could lose without facing potential payout/bonus restrictions.

For illustrative purposes, Figure 2 depicts a hypothetical bank with a starting CET1 capital ratio of 12 percent. The bank's capital ratio is decomposed into a 4.5 percent Basel III minimum requirement, a 5.5 percent regulatory buffer representing the combination of the stress capital buffer and GSIB surcharge, and an additional 2 percent headroom. As the bank's CET1 capital ratio declines due to the arrival of pandemic losses (downward sloping blue line), the right panel of Figure 2 (in red) highlights an important choice the bank has to make regarding lending decisions. Specifically, the bank has two options:

- Option A: Shrink (e.g., by constraining credit) in order to remain above the regulatory buffer. This saves the bank any costs associated with entering this buffer (payout restrictions, bonus restrictions, etc.).
- Option B: Dip into the regulatory buffers to absorb pandemic losses and continue supporting creditworthy firms through the provision of credit.

Figure 3 presents the evolution of average CET1 headroom through the pandemic. Banks appear quick to replenish their pre-pandemic headroom levels by the third quarter of the pandemic, suggesting credit supply shocks associated with the usability of regulatory capital buffers is likely to be most prominent in the early part of the pandemic.

4. Empirical Approach

a. Data Description and Summary Statistics

To perform our regression analysis, we access novel loan-level information on C&I credit lines (at the bank-firm-quarter level) sourced from the H1 Corporate Schedule of the confidential regulatory filing FR Y-14Q, and combine this with quarterly consolidated bank balance sheet level information at the bank holding company (BHC) level from the FR Y-9C regulatory filing. The main balance sheet variable of interest that separates the set of treatment and control firms in our baseline specification is the lender's pre-pandemic distance to the regulatory buffer (as of 2019Q4). This is equivalent to the size of the green excess capital headroom from Figure 2. As will be elaborated in the next section, we define a bank as being constrained by the regulatory buffer if the distance between its CET1 capital ratio and its regulatory buffer threshold is equal to or less than that of the median (2.14 percent). In other words, we posit that if a bank enters the pandemic with a relatively small headroom to absorb pandemic losses before having to dip into its regulatory buffers (and thereby incur a variety of regulatory costs), that bank may choose to curtail credit in order to avoid incurring any costs from regulatory buffer usage. We consider this a potentially undesirable outcome given that the absolute level of CET1 before the pandemic was historically high, and yet would go un-used.

The FR Y-14Q Corporate Schedule is collected for very large BHCs that participate in the Comprehensive Capital Analysis and Review (CCAR) stress tests. While there are over 30 such BHCs that file, we exclude the filings of the US intermediate holding companies (IHCs) of foreign banks, since the capital ratio of IHCs are internal to the organization and thus not subject to the same incentives.[17] In addition, we drop any BHCs that do not report the FR Y-14 data during the pandemic, or those that have too little C&I loan exposure (i.e. custodian banks). Additionally, to keep the focus on lending outcomes at nonfinancial corporations, we exclude C&I loans to U.S. and foreign banks, other depository financial institutions, non-depository financial institutions, and loans to financial agricultural production and other loans to farmers. This leaves us with quarterly loan information for 16 domestic U.S. BHCs (526,449 bank-firm-time observations) between 2018Q1 and 2020Q3. The data in the FR Y-14 Corporate Schedule includes loan information at the credit facility level for

[17] In addition, the excess headroom of the foreign parent of the IHC (located in the foreign home country) is unknown due to the confidentiality of a particular regulatory capital buffer implemented in Europe, known as the Pillar 2 Guidance.

committed balances greater than or equal to $1 million.[18] The advantage of using loan commitments is that they include both undrawn and drawn portions of credit facilities. This measure of commitments (rather than on-balance sheet outstanding loan amounts) is immune to demand-driven swings in credit line drawdowns and repayments and is thus closer to the idea of bank credit supply decisions, compared to most other studies that use outstanding loan amounts.

Table 1 provides summary statistics at the bank-firm-time level for the control variables in our analysis. C&I commitments have grown on average at an annualized rate of 4.27 percent at the bank-firm level. The median CET1 headroom (not shown) is 2.14 percent, underneath which we denote a bank as having low capital headroom. The average bank primarily funds its assets through deposit funding (66 percent), holds a sizeable amount of illiquid assets on its books (31 percent), and has maintained a quarterly return on assets of about 27 basis points.

One appeal of the FR Y-14Q dataset is that it includes a wide range of firms; that is, small and large firms, as well as publicly traded and private firms. Our use of the FR Y-14 C&I loan level data is quite novel as this is the closest data that we have in the United States to a credit registry data used in the prior literature – see, for example, Jimenez, *et al*. (2017) for banks in Spain.

Figure 4 plots the relationship between the size of the capital headroom, measured as of 2019Q4, versus the subsequent growth in C&I loan commitments during the pandemic period. The figure shows that ex-post commitment growth during the pandemic was weaker among banks that were had low capital headroom ex-ante, a.k.a. those that entered the pandemic with CET1 capital ratios closer to the regulatory buffer.[19]

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[18] For this reason, FR Y-14 does not capture very small business lending (<$1 million USD), and instead captures SME as well as large public firms.
[19] Please refer to Appendix for further analysis showing that this relation cannot be explained by plotting the pre-pandemic level of the CET1 ratio versus the pandemic commitment growth. Counter to intuition, excess capital cushions are not positively correlated with CET1 ratios.

Next, we plot time trends by comparing C&I commitment growth rates across low- versus high capital headroom banks. Suggestive of parallel trends, Figure 5 shows the average commitment growth rates before and after the pandemic for firms that borrow from low capital headroom lenders (red) versus high capital headroom lenders (blue). As shown in the picture, overall C&I commitment growth rates declined significantly after the pandemic, that is, from 2019Q4 to 2020Q3. The contraction was more severe for low capital headroom banks than for high capital headroom banks.

b. Regression specifications

While using consolidated bank balance sheet data is less suitable for disentangling credit supply from credit demand, to overcome this issue, we use loan-level data on C&I credit lines. To account for all changes in lending, the credit effect analysis is broken into a cross-sectional firm exit analysis as well as a panel data intensive margin analysis.[20] Considering that the bulk of firms in the FR Y-14Q data borrow from a single bank, we extend the Khwaja and Mian (2008) approach to compare the lending of low- versus high capital headroom banks to *groups of similar borrowing firms* that are likely to experience common demand shocks (Figure 6). Specifically, our identification strategy replaces firm fixed effects with firm type fixed effects in the cross-sectional firm exit analysis, and replaces firm*time fixed effects with firm type*time fixed effects in the panel data intensive margin analysis. Firm type includes firms grouped by industry, location (zip code), borrower risk (bank-issued investment grade rating), and financial constraint status (firm leverage). We characterize a firm's financial constraint status by creating quintile buckets of firm leverage, and incorporate these as financial constraint status fixed effects in the regression specifications. These firm type and firm type*time fixed effects allow us to control for demand shocks that are common to firms in the same

[20] We use a cross-sectional specification for the firm exit analysis that analyzes the probability that a given pre-pandemic lending relationship ends anytime during the post-pandemic sample period. In this way, coefficients reflect an time-aggregated estimate of the total economic magnitude of firm exits anytime during the pandemic. For robustness, a panel data version of the firm exit analysis is also available upon request and yields consistent results.

group in the cross-sectional and panel data analyses, respectively. Moreover, for the panel data specifications, we add firm*bank fixed effects to control for any unobserved characteristics within that specific bank-firm lending relationship. As additional robustness for the purpose of identifying credit supply shocks, we perform a specification that compares lending outcomes of low capital headroom banks for firms whose pre-pandemic credit lines contractually matured at the start of the pandemic. Here, the selection rule for assigning treatment comes from a pre-determined variable (i.e. the maturity of a previous contract), which was made prior to the unexpected arrival of the pandemic and thus uncorrelated with firm-level demand shocks during the pandemic.

i. Credit effects (firm exits)

Our first set of specifications study bank credit response with regards to firm exits, based on Favara et al. (2021). Figure 1 suggests this effect was significant for low capital headroom banks during the pandemic. We categorize banks as either "low capital headroom" or "high capital headroom" using a dummy variable $LowCapitalHeadroomBank$, which takes the value of 1 for banks that had CET1 capital ratios close to the regulatory buffer right before the onset of the pandemic and 0 for those that had CET1 capital ratios far from it. This threshold is based on whether this headroom is above or below the median headroom (2.14 percent) for CCAR banks as of 2019Q4. Equation (1) below shows our cross-sectional regression:[21]

[21] We use a cross-sectional specification for the firm exit analysis so that coefficients reflect an estimate of the total economic magnitude during the pandemic.

$$FirmExit[0/1]_{b,f,2020Q3}$$
$$= \beta_0 + \beta_1 LowCapitalHeadroomBank_{b,2019Q4} + \beta_2 \theta_{f,2019Q4}$$
$$+ \beta_3 LowCapitalHeadroomBank_{b,2019Q4} * \theta_{f,2019Q4} + \beta_F FirmControls_{f,2019Q4}$$
$$+ \beta_B BankControls_{b,2019Q4} + \alpha_{IndustryFEs} + \gamma_{ZipLocationFEs} + \delta_{InvGradeRatingFE}$$
$$+ \mu_{FirmLeverageFEs} + \varepsilon_{bf}$$

Eq. 1

where $FirmExit[0/1]_{b,f,2020Q3}$ is a binary variable that equals 1 if a given firm exits the FR Y-14Q as of 2020Q3. The interaction coefficient β_3 captures the differential impact that low capital headroom banks have on the probability that a given private SME lending relationship exits during the pandemic (as compared to that of a high capital headroom bank). For Tables 2, 4, and 6, $\theta_{f,2019Q4}$ takes on each respective element of the following set:

$\left\{ \begin{matrix} PrivateSME_{f,2019Q4}, YoungRelationshipFirm_{b,f,2019Q4}, \\ FirmCredLineMaturinginPandemic_{b,f,2019Q4 \to 2020Q2} \end{matrix} \right\}$, where

- $PrivateSME_{f,2019q4}$ is a dummy variable that equals 1 for all firms f existing in 2019Q4 that are smaller than the median firm size in the sample, private, and only borrow from a single lender, as of 2019Q4.
- $YoungRelationshipFirm_{b,f,2019q4}$ is a dummy variable that equals 1 for all firms f that have maintained a lending relationship with their bank b for less than or equal to the median relationship age (6 years), as of 2019Q4.
- $FirmCredLineMaturinginPandemic_{b,f,2019Q4 \to 2020Q2}$ is a dummy variable that equals 1 for all firms f in 2019Q4 whose pre-existing credit facility with bank b is set to contractually mature at the start of the pandemic, 2020Q2.

$BankControls_{b,2019Q4}$ include bank size [$Bank\ Log\ Assets_{b,2019Q4}$], the deposit-to-asset ratio [$Bank\ Deposit\ Ratio_{b,2019Q4}$], the ratio of loan loss provisions to RWA [$Bank\ Provisions\ to\ RWA_{b,2019Q4}$], the share of liquid assets in total assets [$Bank\ Liquid\ Asset\ Ratio_{b,2019Q4}$], and bank profitability [$Bank\ ROA_{b,2019Q4}$], while $Firm\ Controls_{f,2019Q4}$ include firm size [$Firm\ Log\ Assets_{f,2019Q4}$], firm leverage as measured by the ratio of debt to total assets [$Firm\ Leverage_{f,2019Q4}$], firm profitability [$Firm\ ROA_{f,2019Q4}$], and firm sales [$Firm\ Sales\ Ratio_{f,2019Q4}$].

According to our hypothesis, we expect a positive value for the coefficient β_3 on the interaction term, $LowCapitalHeadroomBank_{b,2019Q4} * \theta_{f,2019Q4}$. This would be consistent with the idea that banks entering the pandemic with relatively little capital headroom above the costly regulatory buffer region are more likely ex-post to curtail credit to specific types of firms (i.e., private SMEs, those with relatively young lending relationships, and those whose pre-existing credit lines are maturing at the start of the pandemic) in a way that results in a firm exit.

ii. Credit effects (intensive margin)

Our second set of specifications study bank lending response for borrowers along the intensive margin. We categorize banks as either low capital headroom or high capital headroom using the same dummy variable $LowCapitalHeadroomBank$ as in the previous section. Equation (2) below presents our panel data specification using the growth rate in commitments:

$$\frac{\Delta Commitments_{bft}}{Commitments_{bf,t-1}} = \beta_0 POST_t + \beta_1 LowCapitalHeadroomBank_{b,2019q4} + \beta_2 \theta_{f,2019Q4} + \cdots + \beta_3 POST_t * LowCapitalHeadroomBank_{b,2019q4} * \theta_{f,2019Q4} + \beta_B BankControls_{b,t-1} + \beta_F FirmControls_{f,t-1} + \varphi_{Bank*FirmFEs} + \alpha_{Industry*DateFEs} + \gamma_{Zip*DateFEs} + \delta_{InvGradeRating*DateFE} + \mu_{FirmLeverage*DateFEs} + \varepsilon_{bft}$$

Eq. 2

where the "…" includes all pairwise interactions between the three interacting variables.

$\frac{\Delta Commitments_{bft}}{Commitments_{bf,t-1}}$ is the growth rate in commitments from bank b to firm f at time t. We annualize this measure. $POST_t$ is a dummy variable that equals 1 starting 2020Q1 or later. For regression Tables 3, 5, and 7, θ takes on each respective element of the following set:

$\left\{\begin{matrix} PrivateSME_{f,2019q4}, YoungRelationshipFirm_{b,f,2019q4}, \\ FirmCredLineMaturinginPandemic_{b,f,2019q4\rightarrow 2020Q2} \end{matrix}\right\}$, where the definitions are the same as in Section 4.b.ii.

$BankControls_{b,t-1}$ include $Bank\ Log\ Assets_{b,t-1}$, $Bank\ Deposit\ Ratio_{b,t-1}$, $Bank\ Provisions\ to\ RWA_{b,t-1}$, $Bank\ Liquid\ Asset\ Ratio_{b,t-1}$, $Bank\ ROA_{b,t-1}$, while $FirmControls_{f,t-1}$ include $Firm\ Log\ Assets_{f,t-1}, Firm\ Leverage_{f,t-1}, Firm\ ROA_{f,t-1}, and\ Firm\ Sales\ Ratio_{f,t-1}$.

According to our hypothesis, for the triple difference-in-differences specifications, we expect a negative coefficient on the triple interaction term $POST_t$ *$LowCapitalHeadroomBank_{b,2019q4}$* $\theta_{f,2019Q4}$. A negative coefficient would be consistent with our prediction that low capital headroom banks curb commitments disproportionately to firms with particular characteristics: private SMEs, those with relatively young lending relationships, and those whose pre-existing credit lines were up for renegotiation at the start of the unanticipated pandemic.

iii. Real Effects

Our third set of specifications explore whether the credit effects analyzed in Sections 4.b.i and 4.b.ii translate into real effects, particularly for local employment. Data limitations prevent us from testing the impact of low capital headroom on firm-level real outcomes like corporate investment because these variables are only updated once every 5 to 6 quarters. This is because the majority of firms in the FR Y-14Q are private and thus only produce and submit regular financial firm accounts to lenders for the purpose of obtaining bank loans and satisfying bank monitoring procedures. In addition, the FR Y-14Q does not contain data on firm-level employment. Instead, we utilize panel data on local employment growth rates at the industry-county-month level provided by the Bureau of Labor Statistics Quarterly Census of Employment and Wages (BLS QCEW) as our real outcome variable of interest. We aggregate ex-ante (2019Q4) credit exposures from low capital headroom banks across firms within each industry-county group from the FR Y-14Q dataset, and merge these onto the industry-county employment growth series from the BLS QCEW.

Equation (3) below presents our panel data specification that explores whether the (un-) usability of regulatory buffers led firms located within industry-county groups (with pre-pandemic lending exposures to low capital headroom banks) to reduce employment growth more during the pandemic than firms located within industry-county groups (with no pre-pandemic lending exposures to low capital headroom banks.

$$\frac{\Delta Employment_{c,i,t}}{Employment_{c,i,t-1}} = \beta_0 + \beta_1 POST_t + \beta_2 LowCapHeadroomBankExposure[0/1]_{c,i,2019Q4} + \beta_3 POST_t * LowCapHeadroomBankExposure[0/1]_{c,i,2019Q4} + \alpha_{IndustryDateFEs} + \gamma_{ZipDateFEs} + \varepsilon_{c,i,t}$$

Eq. 3

$\frac{\Delta Employment_{c,i,t}}{Employment_{c,i,t-1}}$ is the growth rate in employment at all firms in industry i located in county c at month t. We annualize this measure. $POST_t$ is a dummy variable that equals 1 starting April 2020 or later. $LowCapHeadroomBankExposure[0/1]_{c,i,2019Q4}$ is a dummy variable that equals 1 if firms in county c and industry i have an aggregated nonzero credit exposure to low capital headroom banks ex-ante to the pandemic (2019Q4). Due to the lack of available macro time series data at the county-month and industry-month level, we include industry*date and county*date fixed effects in order to control for demand-side shocks associated with the local county-month business cycle and industry-month trends.

According to our hypothesis, we expect a negative estimate on the β_2 coefficient for the interaction term $POST_t * LowCapHeadroomBankExposure[0/1]_{c,i,2019Q4}$. This would be consistent with our prediction that low capital headroom banks contract credit during the pandemic, which ultimately translated into real effects via the reduction in employment growth at firms located in exposed industry-counties.g

5. Results

Tables 2 and 3 shows the regression estimates for the firm exit and intensive margin specifications, respectively, where columns gradually add on bank controls and firm controls within each table. Columns 1 through 3 of Table 2 show the negative and statistically significant impact of *LowCapitalHeadroomBank* on the probability that a given private SME exits the FR Y-14Q (as compared to that of similar firm borrowing from a high capital headroom bank). Specifically, firms borrowing from lenders that entered the pandemic with low capital headroom were up to 10 percent more likely to exit during the pandemic. Along the intensive margin, Table 3 shows that low capital headroom banks curtail commitment growth rates to private SMEs by 4.9% annually during the pandemic. These magnitudes are economically meaningful, given that the average growth rate in commitments for all firms across all quarters in the FR Y-14Q is 4.2 percent, as reported in Table 1. In summary, low capital headroom banks appear to be curtailing credit more to firms that do not have the ability to easily substitute toward other forms of financing, suggesting the possibility of subsequent real effects. Our results point to concerns about potential delays in the economic recovery following the peak of the pandemic, as private SMEs incur higher costs in substituting to other sources of financing than do large firms. The fact that low capital headroom banks did not curb credit to large borrowers is consistent with the notion that banks will protect relationships with large public borrowers as they are more valuable (e.g. banks can service multi-line products for large firms).

Figure 7 shows evidence of parallel trends by running a modified panel data version of the firm exit specification. Specifically, we present the date-specific difference-in-difference coefficients (along with 90% confidence intervals) for the private SME subsample.[22] These coefficients capture the relative difference in the probability that any existing firm exits in the next quarter for low- versus high capital headroom banks.

Figure 8 explores firm entrants versus exits in the FR Y-14Q by lender type. While Figure 1 shows the stock of private SME exposures, Figure 8 shows the flow of new firm entrants and old firm exits quarter by quarter. The top panel show that low capital headroom banks show a significant

[22] The modified specification is $FirmExit[0/1]_{b,f,t+1} = \beta_0 + \beta_1 LowCapHeadroomBank_{b,2019Q4} + \sum_{\tau=2018Q3}^{2020Q3} \beta_{2,\tau}\, I(Date = \tau) * LowCapHeadroomBank_{b,2019Q4} + \beta_F FirmControls_{f,t} + \beta_X BankControls_{b,t} + \varphi_{BankFirmFEs} + \alpha_{IndustryDateFEs} + \gamma_{ZipDateFEs} + \delta_{InvGradeDateFE} + \mu_{FirmLeverageDateFEs} + \varepsilon_{bft},\ where\ \tau \neq 2019Q4$

widening, with higher firm exits and lower firm entrants, whereas high capital headroom banks show no relative difference.

It is important to note that these firm exit effects occur across a variety of industries. While Figure 1 plots the total number of private SME exposures by low- versus high capital headroom banks, Figure 9 takes this one step further by breaking these down by industry, and finds that low capital headroom banks show fewer private SME exposures in the FR Y-14Q during the pandemic for a host of sectors including Education, Wholesale Trade, Construction, Food and Textile Manufacturing, Health Care, Information Technology, Technical Services, Retail Trade, Wood, Coal, and Plastics Manufacturing, Transportation, Waste Management, and Mining. This illustrates that the issue of (un-) usability of regulatory buffers had potentially wide-reaching effects, and was not limited to those industries directly affected by the COVID-19 pandemic.

Tables 4 and 5 provide firm exit and intensive margin analysis estimates for credit supply adjustments with respect to borrowers whose lending relationships are relatively young. We define a lending relationship as relatively young if its age is below the median relationship age for all bank-firm pairs in the FR Y-14Q data (6 years or less). Table 4 shows that firms having relatively young lending relationships with low capital headroom banks are 2.2 percent more likely to exit during the pandemic. Additionally, Table 5 shows that low capital headroom banks reduce annual C&I commitment growth to young relationship firms by roughly 4.1 percentage points more during the pandemic. This result is consistent with the idea that curtailing younger relationships is less costly to banks as compared to incurring the reputational costs associated with curtailing older relationships.

Finally, Tables 6 and 7 explore the set of firms that have credit lines originated prior to the pandemic that contractually mature in the first major quarter of the pandemic, 2020Q2. These are the set of firms for which it is least costly (legally and contractually) for a low capital headroom bank to cut lending to, since the bank does not need to break any terms of the pre-existing contract or wait for any covenants to be violated. The bank can simply decline to renew during the contract renegotiation and allow the exposure to costlessly roll off its books. Table 6 shows that such firms borrowing from low capital headroom banks are 6.4 percent more likely to exit, while Table 7 shows that low capital headroom banks reduced annual C&I commitment growth to these contractually maturing firms by 22.3 percentage points more during the pandemic. Note the economic significance of this result. This is expected as it is consistent with the idea that low headroom banks find it contractually cheaper to curtail lending disproportionately to borrowers entering a renegotiation at an unfavorable bargaining time (COVID-19). Note also that this finding provides additional robustness for the purpose of identifying credit supply shocks since the selection rule for this treatment group of firms comes from a pre-determined variable (e.g. the contractual maturity of a pre-pandemic credit line contract), which was set prior to the unexpected arrival of the pandemic downturn. This finding strongly suggests the presence of credit supply effects, as it would be difficult to explain this result using a demand-side story. Tables 4 through 7 show that the credit effects associated with the usability of buffers expands to firms *beyond just SMEs*. Specifically, *any* firm with a young lending relationship or loans maturing at the start of the pandemic qualify as less costly options for low capital headroom banks to curtail lending to in order to preserve bank capital and avoid the costs of buffer usage.

Table 8 shows the results of the employment growth regression, providing suggestive evidence that the credit effects from Tables 2 through 7 likely led to real effects. Specifically, it is likely that SME firms borrowing from low capital headroom banks found it difficult to substitute towards other forms of finance, and thereby, may have had to adjust by reducing the growth rate of employment. The difference-in-difference estimate in Table 8 shows that firms in industry-counties with nonzero ex-ante credit exposures to low capital headroom banks reduce their employment by an annualized growth rate of 1.87% as compared to firms located in other industry-counties. Figure 10 shows evidence of parallel trends by running a modified version of the employment specification. Specifically, we present the date-specific difference-in-difference coefficients (along with 90% confidence intervals) for the employment growth across industry-counties, and show that these pre-pandemic coefficients are not statistically different from zero.[23] Notice that the 1.87 percentage point estimate embeds the fact that the largest difference-in-difference employment growth effect occurs in the month of May 2020, where industry-counties exposed to low capital headroom banks experienced 6 percentage points slower annualized employment growth (Figure 10). It is also interesting to note that the real effects appear to be large but short term in nature (lasting three months from May through July of 2020), consistent with the notion illustrated in Figure 3 that the balance sheet constraints and costs emanating from buffer usability were binding in the short term but not in the long term, as banks were quick to replenish their headroom beyond pre-pandemic levels within a few quarters.

In summary, we find evidence that low capital headroom banks cut lending disproportionately to private SMEs, young relationship firms, and firms whose prior credit lines were set to mature at the start of the pandemic (and thus were up for renegotiation). Altogether, these findings are consistent with the idea that a low capital headroom bank optimizes how best to curtail credit by choosing firms for which it is least costly to curtail lending to (even though the firm is creditworthy), suggesting that regulatory buffers do not appear to be currently working as intended.

We can construct an estimate for the economic magnitude of the credit supply shock associated with un-usability of regulatory buffers by summing USD amounts for the credit adjustment for the intensive margin and firm exit analyses. For the first component, our baseline estimate from Table 3 column 3 states that low capital headroom banks curbed lending to SMEs by roughly 4.9 percentage points more (annually) during the pandemic. For the second component, the baseline estimate from Table 2 column 3 shows that SMEs borrowing from low capital headroom banks were 9.97% more likely to exit the FR Y-14Q during the pandemic. This effect increases to 11.02% for the entire year.[24] The total number of SME borrowers associated with low capital headroom banks in the FR Y-14Q H1

[23] The modified specification is $EmploymentGrowthRate_{c,i,t} = \beta_0 + \beta_1 LowCapHeadroomBankExposure[0/1]_{c,i,2019M12} + \sum_{\tau=2019M9}^{2020M9} \beta_{2,\tau}\, I(Date = \tau) * LowCapHeadroomBankExposure[0/1]_{c,i,2019M12} + \alpha_{IndustryDateFEs} + \gamma_{ZipDateFEs} + \varepsilon_{c,i,t},$ $where\ \tau \neq 2020M3$

[24] This comes from running the same specification until 2020Q4. Results available upon request.

Schedule as of 2019Q4 was 14,155 SMEs comprising $64.8 Billion in total commitments. To establish an economic magnitude of the un-usability of regulatory capital buffers, we estimate that the associated credit supply shock led to roughly 1560 SME exits (= 14155*0.1102) across a diverse set of industries, comprising a credit contraction between $3 Billion to $10.3 Billion in total USD commitments.[25] In aggregate, this credit effect comprises anywhere from 5% (=$3B/$64.8B) to 16% of total SME commitments (=$10.3B/$64.8B).

6. Policy Discussion

As described in the introduction and Section 3, regulatory capital buffers are soft mandate requirements, where banks are allowed to dip into but will incur penalties and costs for doing so. Policy recommendations require understanding why buffer usage are costly from the banks' perspective. In particular, there are at least three potential costs. Firstly, payout restrictions associated with the usage of buffers means banks face potential market stigma.[26] Secondly, dipping into the buffer may lead to the possibility of a downgrade from credit ratings agencies. For example, during April 2020, Moody's released a statement that global investment banks are expected to maintain solid capital buffers *at or above 2019 levels*.[27] The actions of credit ratings agencies can have unintended externalities for the usability of regulatory capital buffers. Thirdly, buffer usage also triggers bonus restrictions with respect to bank executive compensation. Data limitations prevented prior studies from pinning down why capital buffers are costly. While this task is not empirically possible to pin down in the context of the pandemic either, below we provide some historical evidence on the first two costs.

[25] $3 Billion = $64.8 Billion*0.049 (intensive margin), and $10.3 Billion = $64.8 Billion*0.049 (intensive margin) + 64.8 Billion*0.1102 (firm exit). The range is due to the fact that the firm exit estimate of 11.02% is an upper bound estimate of the extensive margin.

[26] For example, Capital conservation buffer, countercyclical capital buffer amount, and GSIB surcharge, 12 C.F.R. § 217.11.

[27] See https://www.moodys.com/research/Moodys-Global-Investment-Banks-Coronavirus-shock-to-profits-should-not--PBC_1224538

To test the impact of market stigma related to a reduction in dividends, we use daily stock price data from 1990 to the present to conduct an event study using a Fama French 3 factor model. For each event i, we estimate coefficients for the Fama French 3 factor model in a 120-day estimation window (130 days before to 10 days before event) as shown in equation 4.

$$R_{it} = \beta_i + \gamma_{it}(Mkt - Rf)_t + \alpha_2 HML_t + \tau_3 SMB_t + \varepsilon_{it}$$

Eq. 4

We then use these coefficients to extract the abnormal stock return of bank i using a (-1,1) 3-day event window around the dividend cut. We find that bank dividend cut events are associated with negative cumulative abnormal stock returns (288 basis points) for banks during stress events such as the 2007-2008 Global Financial Crisis.

	Dividend Cut Events	(-1,1) CAR percent
ALL	42	-2.34 percent**
Normal Times	12	-1.07 percent
GFC Crisis	28	-2.88 percent**

We conduct a second event study, using bank credit rating downgrade events from 1990-present. Overall, we find that credit rating downgrades (specifically in the 2008 crisis) led to negative cumulative abnormal returns of roughly 265 basis points during the 3-day event window.

	Ratings Downgrade Events	(-1,1) CAR percent
All	122	-1.29 percent***
Normal Times	73	-0.43 percent
GFC Crisis	48	-2.65 percent***

The costs associated with rating downgrades and dividend cuts during the GFC are economically large. Despite the potential caveats associated with the limited number of these events, these historical estimates point to the large potential costs banks may have faced had they used their regulatory capital buffers at the onset of the COVID-19 crisis.

Due to data limitations, recommending specific policy remedies requires making strong assumptions about which of these proposed channels for the costliness of buffers was most binding for banks. The large abnormal returns associated with downgrades and dividend cuts suggest it is not possible to eliminate either channel from consideration. However, there are a few policy insights that do emerge from our paper. Firstly, regulatory capital buffers appear to be currently acting as a "double-edged" policy sword, where the costliness of regulatory capital buffers that incentivized banks to raise their CET1 ratios to historically high levels *during normal times* likely also made buffers *difficult to use during the downturn.* Secondly, potential policy recommendations include improving the transparency of the buffer requirement to reduce market stigma – for example, reassuring market participants and credit ratings agencies that bank decisions to dip into their buffers does not necessarily signal weakness – or providing temporarily relief from the buffer constraint in downturns. However, beyond this, there has been evidence suggesting that the action of releasing regulatory buffers in a downturn may not necessarily lead to more *usable* capital, but rather may come with additional unanticipated costs. In particular, on March 12, 2020, the European Central Bank (ECB) posted a press release that allowed banks to operate temporarily below the level of capital defined by the Pillar 2 Guidance (P2G) and the capital conservation buffer.[28] Out of the 115 eurozone banks supervised by the ECB, it is reported that only nine banks took advantage of this capital relief. Possible reasons for this reluctance, as proposed by Arnold (2021), is that "*...some banks have been reluctant to do so, worrying about how long the relief will last and the risk of stigma among investors."* This points to the notion that *forward guidance uncertainty* may be an additional friction associated with banks' incentive to use buffer relief. Corroborated by analysis done in the IMF GSFR (2021), banks may not

[28] See ECB (2020).

take advantage of the buffer relief if clear forward guidance is not provided on how long it will last. In particular, without a specified time frame, banks may be hesitant to use the relief as they could be forced to replenish capital at an unknown future date when the cost of capital is not ideal.

7. Conclusion

Sitting on top of minimum capital requirements, regulatory capital buffers introduced after the 2008 financial crisis are costly regions of "rainy day" equity capital designed to allow banks to absorb losses and support the economy through lending in a downturn. Although the implementation of these Basel III regulatory buffers played a key role in helping build banking system capital to historic levels, it appears this stockpile of capital went effectively unused during the pandemic. Our results suggest that banks were reluctant to use their regulatory buffers to absorb pandemic losses, and instead curtailed lending to SMEs during the pandemic.

To explore this, we employ a novel set of confidential, supervisory loan-level data between the largest U.S. banks and their corporate borrowers during the pandemic. The comprehensive coverage of this data provides us with a unique advantage to observe the lending outcomes at an important, yet under-studied segment of the economy, namely, private SMEs, whose survival was particularly dependent on financing from their lenders.

Controlling for borrower risk, we find that "low capital headroom banks" (e.g. lenders that entered the pandemic with a capital ratio relatively close to the regulatory buffer region) curtailed commitments to creditworthy SMEs along the intensive margin by 4.9% more than "high capital headroom" banks (e.g. lenders that entered the pandemic with a capital ratio relatively far to the regulatory buffer region). It also resulted a 10% higher probability of borrower exits for low capital headroom banks. We further find heterogeneous effects across borrower type. Specifically, our results show that low capital headroom banks disproportionately curtailed lending to: (1) private SMEs (leaving their valuable relationships with large public clients untouched), (2) firms that had a relatively young lending relationship with their bank, and (3) firms whose pre-pandemic credit lines contractually matured at the start of the pandemic (and thus were up for renegotiation). These results are consistent with banks choosing to make cost-minimizing decisions on which firms to curtail credit to, rather than utilizing the buffers for their intended purpose of maintaining the flow of credit to creditworthy businesses in recessions. We estimate that credit effects span a diverse set of industries comprise of up to 16% of aggregate SME credit. We also find suggestive evidence of real effects on local employment growth during the pandemic (2% slower annually).

Our study brings a new angle to the literature on how the pandemic transmitted shocks to SMEs – specifically, these findings uncover a novel transmission channel emanating from constraints related to bank capital which led to credit supply shocks, potentially delaying the economic recovery for private SMEs. Rather than seeing the buffers as a cushion to be drawn upon during a downturn, as intended by Basel III, banks seem to have treated regulatory buffers as de facto minimum requirements.

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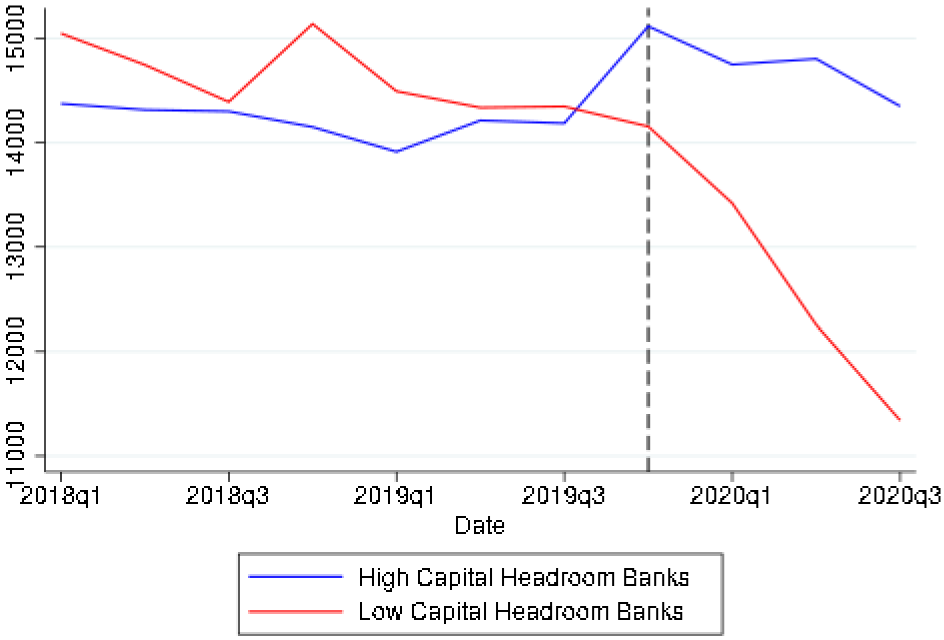
Figure 1: Low Capital Headroom Banks and the Number of SMEs



Note: This plot shows the number of SMEs in the FR Y-14Q by lender type (low versus high capital headroom banks) as of each date. The relative difference between the two lines provides suggestive evidence that many SMEs (borrowing from low capital headroom lenders) exited the FR Y-14Q during the pandemic due credit supply effects related to the un-usability of regulatory capital buffers. Low capital headroom banks are lenders that start the pandemic with a capital ratio relatively close to the regulatory buffer threshold. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

Figure 2: Visualizing the Bank's Decision Whether to Avoid or Use Regulatory Buffers

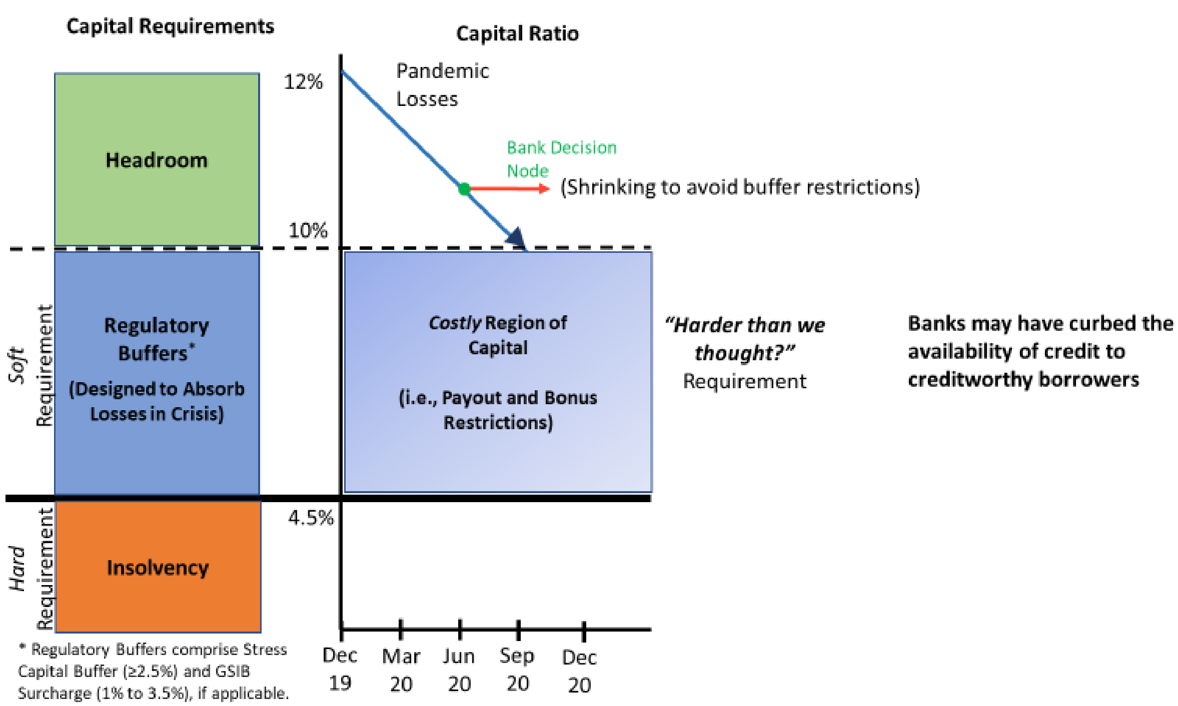
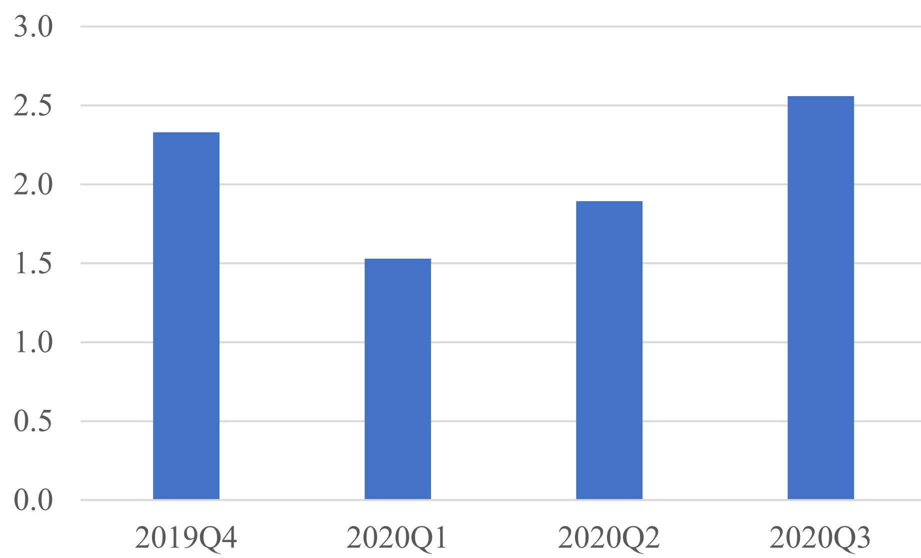
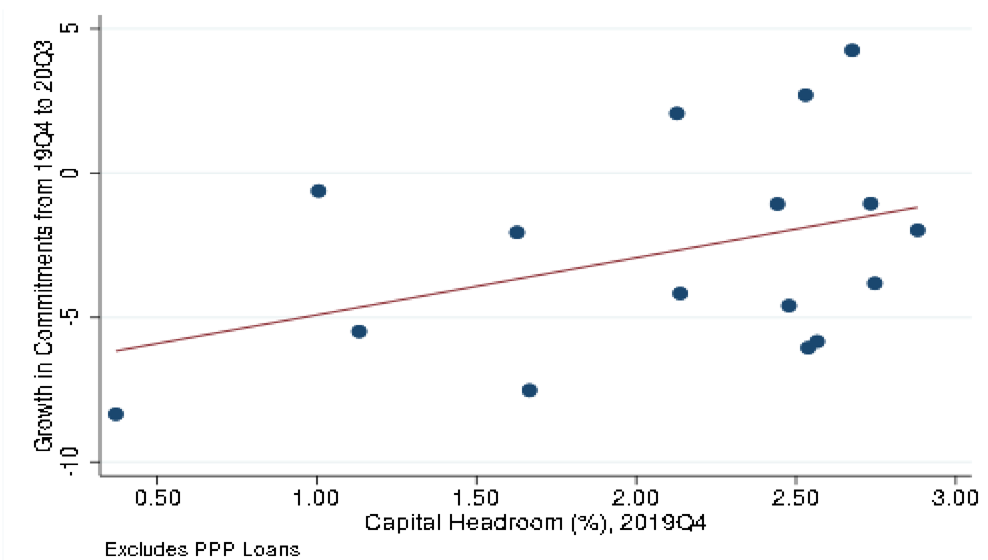


Figure 3: Evolution of CET1 Headroom



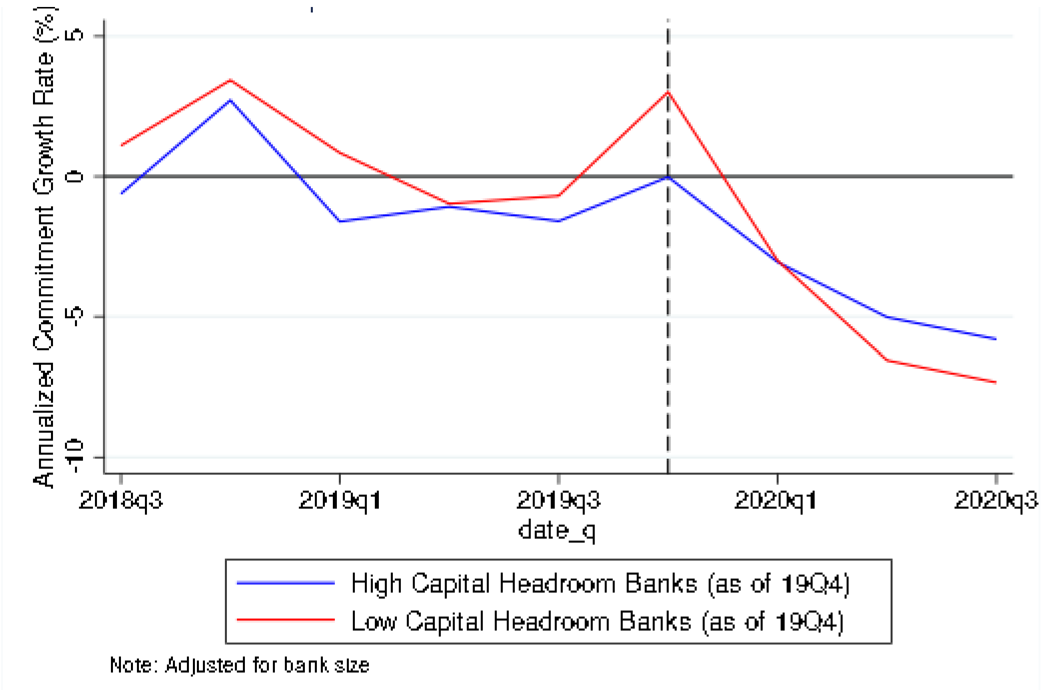
Note: This plot shows the time series evolution of average CET1 headroom across the 16 CCAR banks in the sample (weighted by total assets). Banks are quick to replenish their pre-pandemic headroom levels by the third quarter of the pandemic, suggesting credit supply shocks associated with the usability of regulatory capital buffers is likely to be most prominent in the early part of the pandemic. Source: FR Y-9C, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

Figure 4. Capital Headroom and Pandemic Commitment Growth in the Cross-Section of Banks



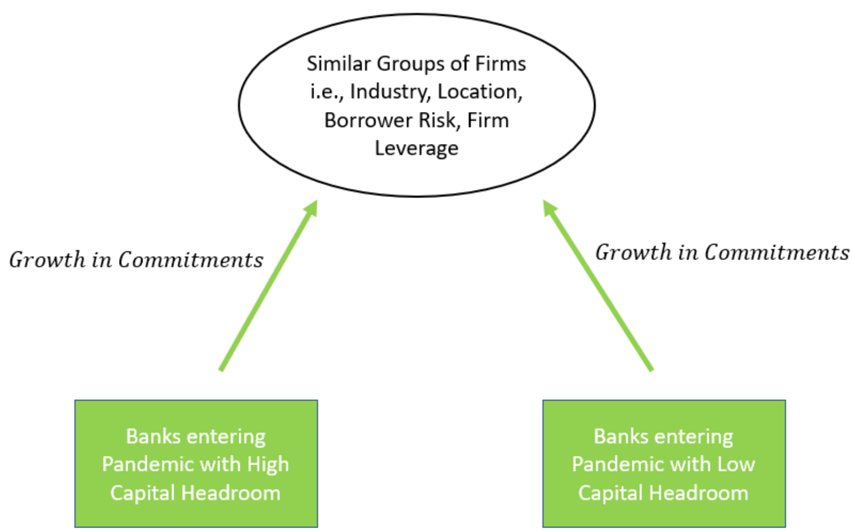
Note: This plot explores credit effects in the cross-section of banks and shows a positive relationship between a bank's capital headroom ex-ante to the pandemic (2019Q4), and its cumulative ex-post percentage growth in loan commitments during the pandemic (from 2019Q4 to 2020Q3). Low capital headroom banks are lenders that start the pandemic with a capital ratio relatively close to the regulatory buffer threshold. Source: FR Y-9C, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

Figure 5: Low Capital Headroom Banks and C&I Commitment Growth through Time



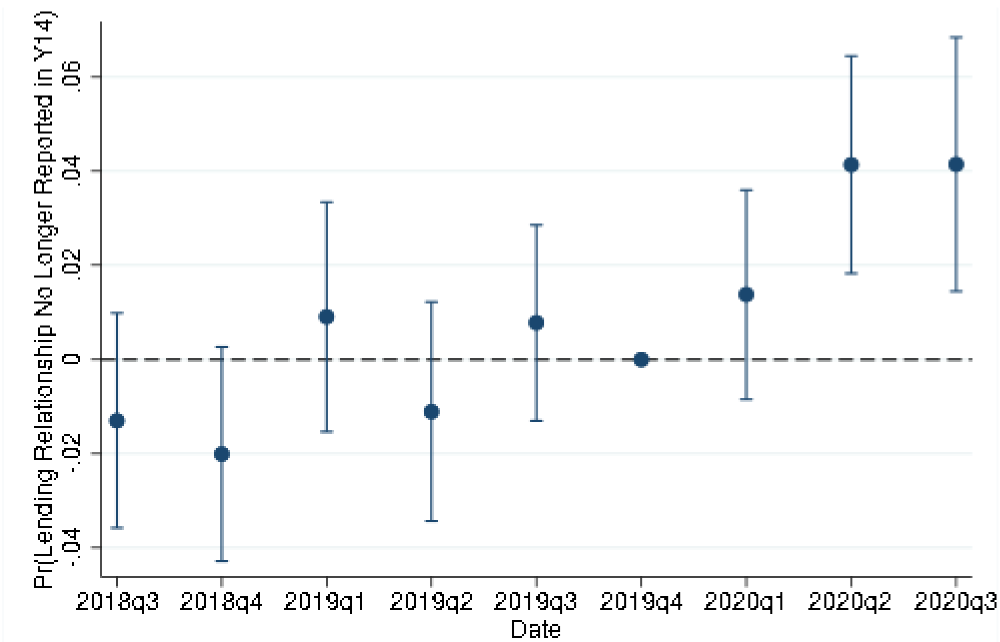
Note: This plot shows the time series variation in average commitment growth across lender type. The average annualized commitment growth rate for low capital headroom banks declines more during the pandemic than that of high capital headroom banks. Low capital headroom banks are lenders that start the pandemic with a capital ratio relatively close to the regulatory buffer threshold. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

Figure 6: Empirical Setup



Note: This diagram illustrates our empirical setup, where we compare differences in pandemic-time commitment growth across low and high capital headroom banks. As SMEs typically only have one lender, we extend the Khwaja and Mian (2008) approach and compare the lending of low- versus high capital headroom banks to *groups of similar borrower firms*, based on location * time, industry * time, firm risk * time, and firm leverage * time fixed effects. Low capital headroom banks are lenders that start the pandemic with a capital ratio relatively close to the regulatory buffer threshold.

Figure 7. Low Capital Headroom Banks and SME Exits

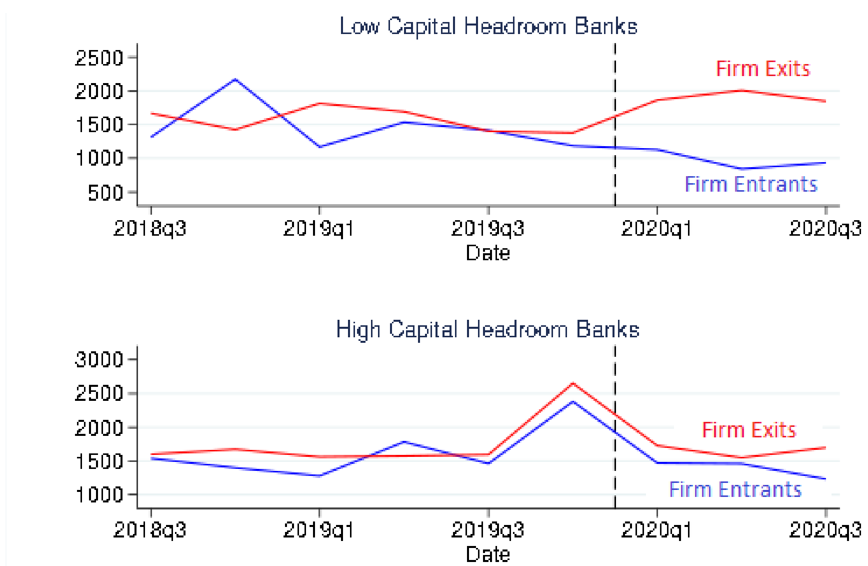


Note: This chart presents the time-specific difference in difference coefficients $\beta_{2,\tau}$ (along with 90% confidence intervals) after estimating the below specification for the private SME subsample. These coefficients capture the relative difference in the probability that a given SME exits the FR Y-14Q in the next quarter across low- versus high capital headroom lenders. Low capital headroom banks are lenders that start the pandemic with a capital ratio relatively close to the regulatory buffer threshold.

$$FirmExit[0/1]_{b,f,t+1} = \beta_0 + \beta_1 LowCapitalHeadroomBank[0/1]_{b,2019Q4} + \sum_{\tau=2018Q3}^{2020Q3} \beta_{2,\tau}\, I(Date = \tau) * LowCapitalHeadroomBank[0/1]_{b,2019Q4} + \beta_F FirmControls_{f,t} + \beta_X BankControls_{b,t} + \varphi_{Bank*FirmFEs} + \alpha_{Industry*DateFEs} + \gamma_{Zip*DateFEs} + \delta_{InvGrade*DateFE} + \mu_{FirmLeverageQuintile*DateFEs} + \varepsilon_{bft},\ where\ \tau \neq 2019Q4$$

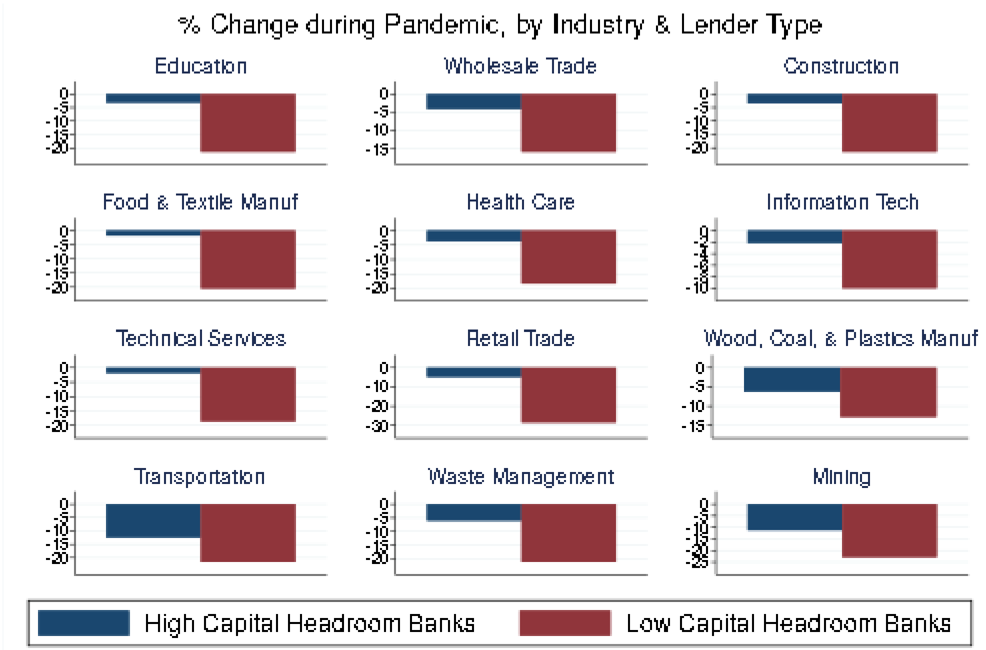
Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

Figure 8: Firm Entry and Exit Flow



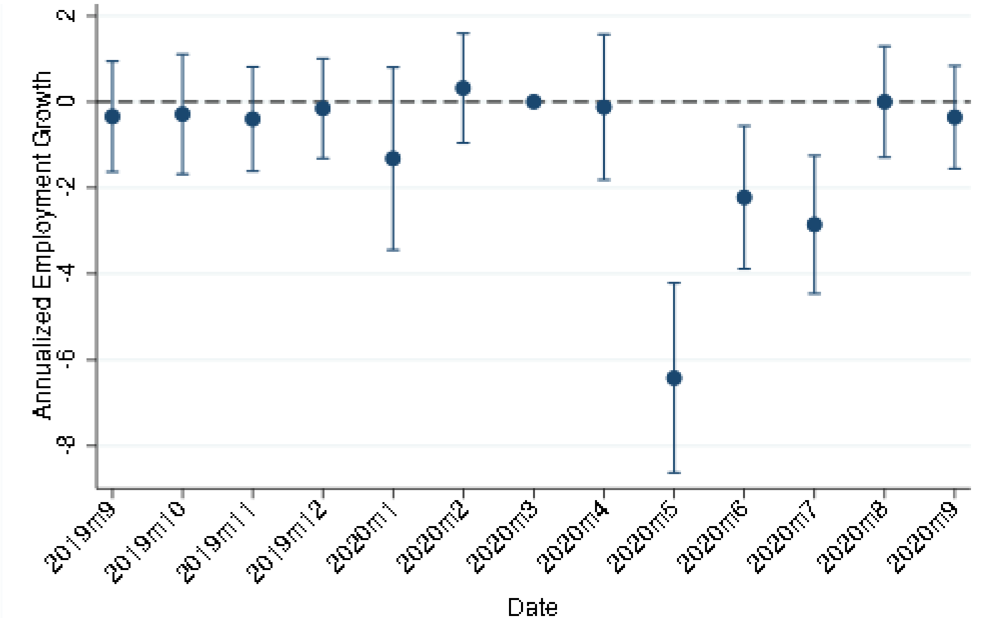
Note: This plot shows the number of firm exits and entrants (flow) in the FR Y-14Q for SMEs across low- versus high capital headroom banks. Low capital headroom banks are lenders that start the pandemic with a capital ratio relatively close to the regulatory buffer threshold. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

Figure 9: Low Capital Headroom Banks and Number of SMEs



Note: This chart presents the percent change in the number of SMEs during the pandemic, by low- versus high capital headroom lender type and by industry. This percent change is measured by counting the total number of private SME borrowers in the FR Y-14Q as of 2019Q4 and 2020Q3. Low capital headroom banks are lenders that start the pandemic with a capital ratio relatively close to the regulatory buffer threshold. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom

Figure 10: Low Capital Headroom Banks and Local Employment Growth



Note: This chart presents the time-specific difference in difference coefficients $\beta_{2,\tau}$ (along with 90% confidence intervals) after estimating the below specification. These coefficients capture the relative difference in employment growth in industry-counties with ex-ante nonzero credit exposures to low capital headroom banks (as of 2019Q4) versus those without such ex-ante exposures. Low capital headroom banks are lenders that start the pandemic with a capital ratio relatively close to the regulatory buffer threshold.

$EmploymentGrowthRate_{c,i,t} = \beta_0 + \beta_1 LowCapitalHeadroomBankExposure[0/1]_{c,i,2019Q4} + \sum_{\tau=2019M9}^{2020M9} \beta_{2,\tau} I(Date = \tau) * LowCapitalHeadroomBankExposure[0/1]_{c,i,2019Q4} + \alpha_{Industry*DateFEs} + \gamma_{Zip*DateFEs} + \varepsilon_{c,i,t}, \text{ where } \tau \neq 2020M3$

Source: FR Y-14Q H1 Schedule, Bureau of Labor Statistics Quarterly Census of Employment and Wages, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

Table 1: Summary Statistics

This table provides summary statistics for key variables in the FR Y-14Q data. The table reports the 10th percentile, mean, 90th percentile, and standard deviation for both BHC variables and firm variables. There are 526,449 bank-firm-time observations, which are spread across 16 lenders and 11 quarters. Source: FR Y-9C, FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

Variable	p10	Mean	p90	Std Dev
Annualized Growth in Commitments (%)	-25.87	4.27	23.44	64.77
CET1 Headroom (%)	1.01	2.06	2.73	0.60
Bank Log Assets	18.74	20.40	21.69	1.19
Bank Deposit Ratio (Dep / Assets) (%)	55.89	65.69	75.82	10.09
Bank Liquid Asset Ratio (Liq Assets / Assets) (%)	21.88	31.31	39.11	7.29
Bank Provisions to RWA (%)	-0.01	0.06	0.28	0.12
Bank ROA (%)	0.12	0.27	0.38	0.11
Firm Leverage (Debt / Assets) (%)	0	33	72	27
Firm ROA (%)	-0.02	0.09	0.24	0.16
Firm Sales Ratio (Net Sales / Assets) (%)	0.32	2.29	4.44	1.97
Firm Log Assets	15.30	18.31	22.42	2.70

Table 2: Credit Effects of Low Capital Headroom Banks on SMEs – Firm Exits
This table presents the regression results for the cross-sectional specification (1), focusing on SMEs. All observations are as of 2019Q4. The left hand side variable is a dummy variable that equals 1 if a given firm no longer exists in the FR Y-14Q at the end of the sample (2020Q3). The interaction coefficient captures the differential impact that a low capital headroom bank has on the probability that a given private SME exits during the pandemic (as compared to that of a high capital headroom bank). LowCapitalHeadroomBank is a 0/1 variable denoting if the firm borrows from a lender whose CET1 capital ratio as of 2019Q4 was relatively close to the costly regulatory capital buffer threshold (headroom ≤ 2.14%). PrivateSME is a 0/1 variable denoting if the firm is private and is smaller than the median firm size in the sample. Controls include lagged firm and bank-level characteristics. All specifications include fixed effects for industry, zip, investment grade rating, and firm leverage quintile. Standard errors are clustered by firm. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent levels, respectively. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

	Pr(Firm Exits during Pandemic)		
VARIABLES	(1)	(2)	(3)
LowCapitalHeadroomBank	-0.000971	-0.0124	-0.0132
LowCapitalHeadroomBank*PrivateSME	**0.0997*** **	**0.106*** **	**0.0997*** **
PrivateSME	0.0194***	0.0165***	0.0153***
Firm ROA			-0.0358**
Firm Leverage			0.00877
Firm Sales Ratio			0.00426***
Bank Log Assets		5.88e-05	-0.00888*
Bank Deposit Ratio		-0.00168***	-0.000687**
Bank Provisions to RWA		-0.101	-0.0756
Bank Liquid Asset Ratio		-0.00130	0.00137
Bank ROA		-0.0513*	-0.0347
Constant	0.117***	0.289***	0.303***
Observations	58741	58741	53256
R-squared	0.214	0.215	0.219
Industry FE	Y	Y	Y
Zip FE	Y	Y	Y
InvGrade FE	Y	Y	Y
FirmLeverage FE	Y	Y	Y
No. of Banks	16	16	16
No. of Firms	43463	43463	39157

Table 3: Credit Effects of Low Capital Headroom Banks on SMEs – Intensive Margin

This table reports the regression results for panel data specification (2), focusing on SMEs. This captures the relative differences in annualized loan commitment growth rates along the intensive margin to private SMEs from low- versus high capital headroom banks after the 2020Q1 arrival of the pandemic. POST is a dummy variable denoting 2020Q1 and after. LowCapitalHeadroomBank is a 0/1 variable denoting if the firm borrows from a lender whose CET1 capital ratio as of 2019Q4 was relatively close to the costly regulatory capital buffer threshold (headroom ≤ 2.14%). PrivateSME is a 0/1 variable denoting if the firm is private and is smaller than the median firm size in the sample. Controls include lagged firm and bank-level characteristics. All specifications are at the bank-firm-date level, span 2018Q1-2020Q3, and include bank*firm, industry*date, zip*date, and investment grade rating*date, and firm leverage quintile*date fixed effects. Standard errors are clustered by bank-date and firm level. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent levels, respectively. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

	C&I Loan Commitment Growth Rate perc pts (Annualized)		
VARIABLES	(1)	(2)	(3)
POST*LowCapitalHeadroomBank	0.992	0.359	0.372
POST*LowCapitalHeadroomBank*PrivateSME	**-4.188****	**-4.427****	**-4.981*****
POST*PrivateSME	3.243***	3.476***	3.778***
Firm ROA			4.708**
Firm Leverage			-6.626***
Firm Sales Ratio			0.609***
Bank Log Assets		-7.291	-9.109
Bank Deposit Ratio		-0.342	-0.388
Bank Provisions to RWA		-2.988*	-3.157*
Bank Liquid Asset Ratio		0.338	0.428
Bank ROA		-1.712	-2.041
Constant	3.963***	165.6	203.1
Observations	526449	526449	487226
R-squared	0.274	0.274	0.281
Bank-Firm FE	Y	Y	Y
Industry-Date FE	Y	Y	Y
Zip-Date FE	Y	Y	Y
InvGrade-Date FE	Y	Y	Y
FirmLeverage-Date FE	Y	Y	Y
No. of Banks	16	16	16
No. of Firms	44753	44753	42632

Table 4: Credit Effects of Low Capital Headroom Banks on Young Relationship Firms – Firm Exits

This table presents the regression results for the cross-sectional specification (1), focusing on young relationship firms. All observations are as of 2019Q4. The left hand side variable is a dummy variable that equals 1 if a given firm no longer exists in the FR Y-14Q at the end of the sample period (2020Q3). The interaction coefficient captures the differential impact that low capital headroom bank has on the probability that a given young relationship firm exits during the pandemic (as compared to that of a high capital headroom bank). LowCapitalHeadroomBank is a 0/1 variable denoting if the firm borrows from a lender whose CET1 capital ratio as of 2019Q4 was relatively close to the costly regulatory capital buffer threshold (headroom ≤ 2.14%). YoungRelationshipFirm is a 0/1 variable denoting if the firm's relationship with its lender (as of 2019Q4) is smaller than the median relationship age in the sample (6 years). Controls include lagged firm and bank-level characteristics. All specifications include fixed effects for industry, zip, investment grade rating, and firm leverage quintiles. Standard errors are clustered by firm. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent levels, respectively. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

	Pr(Firm Exits during Pandemic)		
VARIABLES	(1)	(2)	(3)
LowCapitalHeadroomBank	0.0196***	0.00140	0.0162*
LowCapitalHeadroomBank*YoungRelationshipFirm	**0.0351*** **	**0.0322*** **	**0.0229*** **
YoungRelationshipFirm	0.0417***	0.0429***	0.0349***
Firm Log Assets			-0.0192***
Firm ROA			-0.0444***
Firm Leverage			-0.0176
Firm Sales Ratio			0.00137
Bank Log Assets		0.00665	-0.0141***
Bank Deposit Ratio		-0.000959***	-0.000379
Bank Provisions to RWA		-0.124*	0.00392
Bank Liquid Asset Ratio		-0.000640	0.00301***
Bank ROA		-0.0181	-0.0248
Constant	0.0992***	0.0630	0.690***
Observations	58741	58741	53255
R-squared	0.210	0.211	0.222
Industry FE	Y	Y	Y
Zip FE	Y	Y	Y
InvGrade FE	Y	Y	Y
FirmLeverage FE	Y	Y	Y
No. of Banks	16	16	16
No. of Firms	43463	43463	39155

Table 5: Credit Effects of Low Capital Headroom Banks on Young Relationship Firms – Intensive Margin

This table reports the regression results for panel data specification (2), focusing on young relationship firms. This captures the relative differences in annualized loan commitment growth rates along the intensive margin to young relationship firms from low- versus high capital headroom banks after the 2020Q1 arrival of the pandemic. POST is a dummy variable denoting 2020Q1 and after. LowCapitalHeadroomBank is a 0/1 variable denoting if the firm borrows from a lender whose CET1 capital ratio as of 2019Q4 was relatively close to the costly regulatory capital buffer threshold (headroom ≤ 2.14%). YoungRelationshipFirm is a 0/1 variable denoting if the firm's relationship with its lender (as of 2019Q4) is smaller than the median relationship age in the sample (6 years). Controls include lagged firm and bank-level characteristics. All specifications are at the bank-firm-date level, span 2018Q1 to 2020Q3, and include bank*firm, industry*date, zip*date, and investment grade rating*date, and firm leverage quintiles*date fixed effects. Standard errors are clustered by bank-date and firm. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent levels, respectively. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

	C&I Loan Commitment Growth Rate perc pts (Annualized)		
VARIABLES	(1)	(2)	(3)
POST * LowCapitalHeadroomBank	1.377	0.667	0.622
POST * LowCapitalHeadroomBank * YoungRelationshipFirm	**-3.886*** **	**-3.848*** **	**-4.098*** **
POST * YoungRelationshipFirm	-0.523	-0.500	0.101
Firm Log Assets			-2.030***
Firm ROA			4.478***
Firm Leverage			-6.172***
Firm Sales Ratio			0.614***
Bank Log Assets		-6.800	-8.571
Bank Deposit Ratio		-0.303	-0.317
Bank Provisions to RWA		-2.510	-2.625
Bank Liquid Asset Ratio		0.293	0.387
Bank ROA		-1.707	-1.540
Constant	3.599***	153.9	225.3*
Observations	592113	592113	542468
R-squared	0.279	0.279	0.286
Bank-Firm FE	Y	Y	Y
Industry-Date FE	Y	Y	Y
Zip-Date FE	Y	Y	Y
InvGrade-Date FE	Y	Y	Y
FirmLeverage-Date FE	Y	Y	Y
No. of Banks	16	16	16
No. of Firms	55887	55887	51723

Table 6: Credit Effects of Low Capital Headroom Banks to Firms with Pre-Existing Credit Lines Set to Mature at the Start of the Pandemic – Firm Exits

This table presents the regression results for the cross-sectional specification (1), focusing on firms with pre-existing credit lines that were set to mature at the start of the pandemic. All observations are as of 2019Q4. The left hand side variable is a dummy variable that equals 1 if a given firm no longer exists in the FR Y-14Q at the end of the sample period (2020Q3). The interaction coefficient captures the differential impact that low capital headroom bank has on the probability that a firm (whose pre-existing credit line was set to mature during the pandemic) exits during the pandemic (as compared to that of a high capital headroom bank). LowCapitalHeadroomBank is a 0/1 variable denoting if the firm borrows from a lender whose CET1 capital ratio as of 2019Q4 was relatively close to the costly regulatory capital buffer threshold (headroom ≤ 2.14%). FirmCredLineMaturinginPandemic is a 0/1 variable denoting if any portion of the firm's pre-existing credit lines (as of 2019Q4) was set to mature at the height of the pandemic (2020Q2). Controls include lagged firm and bank-level characteristics. All specifications include fixed effects for industry, zip, investment grade rating, and firm leverage quintiles. Standard errors are clustered at the firm level. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent levels, respectively. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

	Pr(Firm Exits during Pandemic)		
VARIABLES	(1)	(2)	(3)
LowCapitalHeadroomBank	0.0298***	0.0110	0.0232***
LowCapitalHeadroomBank* FirmCredLineMaturinginPandemic	**0.0845*** **	**0.0870*** **	**0.0646*** **
FirmCredLineMaturinginPandemic	0.0126	0.0128	0.00815
Firm Log Assets			-0.0201***
Firm ROA			-0.0383***
Firm Leverage			-0.0171
Firm Sales Ratio			0.000943
Bank Log Assets		0.0104**	-0.0140***
Bank Deposit Ratio		-0.00142***	-0.000645**
Bank Provisions to RWA		-0.148**	-0.00408
Bank Liquid Asset Ratio		-0.00158*	0.00279***
Bank ROA		0.0136	-0.00738
Constant	0.126***	0.0644	0.745***
Observations	58741	58741	53255
R-squared	0.206	0.207	0.220
Industry FE	Y	Y	Y
Zip FE	Y	Y	Y
InvGrade FE	Y	Y	Y
FirmLeverage FE	Y	Y	Y
No. of Banks	16	16	16
No. of Firms	43463	43463	39155

Table 7: Credit Effects of Low Capital Headroom Banks on Firms with Pre-existing Credit Lines Set to Mature at the Start of the Pandemic – Intensive Margin

This table reports the regression results for panel data specification (2), focusing on firms with pre-existing credit lines that were set to mature at the start of the pandemic. This captures the relative differences across low- versus high capital headroom banks in terms of annualized loan commitment growth rates along the intensive margin to firms whose pre-existing credit lines were set to mature during the pandemic. POST is a dummy variable denoting 2020Q2. LowCapitalHeadroomBank is a 0/1 variable denoting if the firm borrows from a lender whose CET1 capital ratio as of 2019Q4 was relatively close to the costly regulatory capital buffer threshold (headroom ≤ 2.14%). FirmCredLineMaturinginPandemic is a 0/1 variable denoting if any portion of the firm's pre-existing credit lines (as of 2019Q4) was set to mature at the height of the pandemic (2020Q2). Controls include lagged firm and bank-level characteristics. All specifications are at the bank-firm-date level, span 2018Q1 to 2020Q2 and include bank*firm, industry*date, zip*date, and investment grade rating*date, and firm leverage quintiles*date fixed effects. Standard errors are clustered by bank-date and firm. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent levels, respectively. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

	C&I Loan Commitment Growth Rate perc pts (Annualized)		
VARIABLES	(1)	(2)	(3)
POST * LowCapitalHeadroomBank	-0.477	-0.441	0.00818
POST * LowCapitalHeadroomBank * FirmCredLineMaturinginPandemic	**-23.03*** **	**-23.17*** **	**-22.38*** **
POST * FirmCreditLineMaturinginPandemic	-2.061	-1.926	-1.891
Firm Log Assets			-1.984***
Firm ROA			4.779**
Firm Leverage			-5.822***
Firm Sales Ratio			0.768***
Bank Log Assets		-7.833	-9.757
Bank Deposit Ratio		-0.258	-0.289
Bank Provisions to RWA		-6.117	-5.754
Bank Liquid Asset Ratio		0.451	0.533
Bank ROA		-4.855	-4.867
Constant	5.211***	169.8	244.7*
Observations	476,858	476,858	438,620
R-squared	0.281	0.282	0.287
Bank-Firm FE	Y	Y	Y
Industry-Date FE	Y	Y	Y
Zip-Date FE	Y	Y	Y
InvGrade-Date FE	Y	Y	Y
FirmLeverage-Date FE	Y	Y	Y
No. of Banks	16	16	16
No. of Firms	44247	44247	39980

Table 8: Real Effects - Impact of Low Capital Headroom Banks on Industry-County Employment Growth

This table reports the regression results for the employment growth regression in specification (3). Observations are at the industry-county-month level. Bank-firm loan exposures ex-ante to the arrival of the pandemic (2019Q4) are aggregated to the industry-county level and merged to the monthly employment growth rates reported in the Bureau of Labor Statistics Quarterly Census of Employment and Wages. POST is a 0/1 variable denoting if the date is April 2020 or later. LowCapHeadroomBankExposure is a 0/1 variable denoting if a given industry-county has non-zero ex-ante exposure to a low capital headroom lender as of 2019Q4. County-date and industry-date fixed effects are included. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent levels, respectively. Source: FR Y-14Q H1 Schedule, BLS QCEW, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

VARIABLES	Employment Growth Rate (Annualized)
LowCapHeadroomBankExposure	-0.214
POST * LowCapHeadroomBankExposure	**-1.867*** **
Constant	10.07***
Observations	4,090,347
R-squared	0.265
Industry-Date FE	Y
County-Date FE	Y

9. Appendix

a. Paycheck Protection Program (PPP)

One related question that arises is whether SMEs impacted by the usability of regulatory buffers were able to substitute some of the loss in funds from their FR Y-14Q lender by participating in the PPP. We explore this possible substitution by matching borrowing firms in the FR Y-14Q to the firms that participated in the PPP, utilizing a fuzzy matching algorithm (based on the text of the firm name) after filtering potential matches based on zip code and industry NAICS.

In order to then test whether firms that experienced negative credit supply shocks (from low capital headroom banks) were able to substitute into the PPP, we run the following cross-sectional specification in Equation (5)h:

$$\begin{aligned} &Participant\ in\ PPP[0/1]_{f,2020} \\ &\quad = \beta_0 + \beta_1 LowCapitalHeadroomBank_{b,2019Q4} + \beta_2 PrivateSME[0/1]_{f,2019Q4} \\ &\quad + \beta_3 LowCapitalHeadroomBank_{b,2019Q4} * PrivateSME[0/1]_{f,2019Q4} \\ &\quad + \beta_F FirmControls_{f,2019Q4} + \beta_B BankControls_{b,2019Q4} + \alpha_{IndustryFEs} \\ &\quad + \gamma_{ZipLocationFEs} + \delta_{InvGradeRatingFE} + \mu_{BankDependenceFEs} + \varepsilon_{bf} \end{aligned}$$

Eq. 5

where $Participant\ in\ PPP[0/1]_{f,2020}$ is a binary variable that equals 1 if a given firm participates in the PPP. The interpretation of any given coefficient would be the impact of that particular right hand side variable on the probability that the firm participates in the PPP. The interaction coefficient captures the difference in the likelihood that private SME borrowing from a low capital headroom bank (ex-ante to the pandemic) will participate in the PPP (as compared to that of a private SME borrowing from a high capital headroom bank). We use the same firm controls, bank controls, and fixed effects as in the firm exit analysis associated with equation (1). The results of Table 9 below show that private SMEs borrowing ex-ante from low capital headroom banks are *neither more nor less* likely to participate in the PPP as compared to private SMEs borrowing ex-ante from high capital headroom banks. Thus, there is no such evidence of credit substitution effects, as the interaction term of interest is not statistically significant. Firms in the FR Y-14Q have minimum credit line balances of \$1 million, which is equivalent to the 99th percentile of PPP loan volume. In other words, firms that considered small and medium size with respect to the FR Y-14Q population of firms are still much larger than the typical firm participating in the PPP, and so it is unlikely that the PPP would have been able to compensate for the loss in credit due to the supply-side credit contraction associated with the usability of regulatory buffers. This lack of differential substitution is also consistent with our employment results from Table 8. Specifically, because firms exposed to the (un-)usability of regulatory buffers via their lenders were not able to secure alternative sources of financing, then firms are likely have to adjust by slowing employment growth during the pandemic.

Table 9: Are firms borrowing from Low Capital Headroom Banks more likely to participate in the PPP?

This table presents the regression results for the cross-sectional specification (4). All observations are as of 2019Q4. The left hand side variable is a dummy variable that equals 1 if a given firm participates in the PPP during the pandemic. The interaction coefficient captures whether a firm borrowing from a low capital headroom bank (as opposed to a high capital headroom bank) in 2019Q4 is more or less likely to substitute its loss in credit by participating in the PPP. LowCapitalHeadroomBank is a 0/1 variable denoting if the firm borrows from a lender whose CET1 capital ratio as of 2019Q4 was relatively close to the costly regulatory capital buffer threshold (headroom ≤ 2.14%). PrivateSME is a 0/1 variable denoting if the firm is private and is smaller than the median firm size in the sample. Controls include lagged firm and bank-level characteristics. All specifications are at the bank-firm-date level and include bank*firm, industry*date, zip*date, and investment grade rating*date, and firm leverage quintiles*date fixed effects. Standard errors are clustered by bank-date and firm. *, **, and *** denote statistical significance at 10 percent, 5 percent, and 1 percent levels, respectively. Source: FR Y-14Q H1 Schedule, aggregated calculations using bank-specific stress capital buffer and GSIB surcharges to calculate the capital headroom.

	Pr(Firm participates in PPP)		
VARIABLES	(1)	(2)	(3)
LowCapitalHeadroomBank	-0.0156***	-0.0397***	-0.0334***
LowCapitalHeadroomBank * PrivateSME	**0.00639**	**-0.00151**	**-0.00380**
PrivateSME	0.318***	0.320***	0.310***
Firm ROA			-0.0184
Firm Leverage			-0.0305
Firm Sales Ratio			0.0166***
Bank Log Assets		0.0268***	0.0314***
Bank Deposit Ratio		4.26e-05	-0.000176
Bank Provisions to RWA		-0.0435	-0.00316
Bank Liquid Asset Ratio		-0.000732	-0.00226*
Bank ROA		0.233***	0.250***
Constant	0.249***	-0.329***	-0.389***
Observations	58741	58741	53256
R-squared	0.436	0.437	0.451
Zip FE	Y	Y	Y
Industry FE	Y	Y	Y
InvGrade FE	Y	Y	Y
FirmLeverage FE	Y	Y	Y
No. of Banks	16	16	16
No. of Firms	43429	43429	39121