The Credit Line Channel

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November 1, 2021

Abstract

Aggregate U.S. bank lending to firms expands following several adverse macroeconomic shocks, such as the outbreak of COVID-19 or a monetary policy tightening. Using loan-level supervisory data, we show that these dynamics are driven by draws on credit lines by large firms. Banks that experience larger drawdowns during the COVID-19 crisis restrict term lending more, crowding out credit to smaller firms. A structural model indicates that credit lines are central to reproducing this flow of credit toward less constrained firms. While credit lines increase total credit growth in bad times, their redistributive effects can exacerbate the fall in investment.

1 Introduction

What role does firm credit play in the transmission of macroeconomic shocks? This question is at the heart of the financial accelerator and the credit channel, among the most influential mechanisms in modern macroeconomics. These theories posit that, due to financial frictions, the investment and output decisions of firms depend on credit availability and pricing. As a result, a macroeconomic shock that increases spreads or tightens credit constraints should create downward pressure on firm borrowing, worsening the drop in real activity.

A central feature of these mechanisms is that they require lenders to be able to control the price and quantity of new borrowing. But importantly, not all forms of credit satisfy these conditions. In particular, credit line facilities allow borrowers to draw credit up to a precommitted amount at a predetermined spread.[1] As a result, firms with unused credit line capacity have the ability to sidestep adverse changes in lending conditions, potentially neutralizing these amplification mechanisms central to macrofinancial theory.

This richer look at the structure of corporate credit raises a number of salient questions. First, are undrawn credit line balances available and used in sufficient quantities to be important for macrofinancial dynamics? Second, how are credit lines allocated across firms, and what does this imply for the response of investment and output to macroeconomic disturbances? Third, how do credit line drawdowns affect the banking sector and its ability to intermediate funds in bad times?

In this paper, we seek to answer these questions using detailed U.S. loan-level data to document empirical relationships and a structural model to interpret them. To preview, we show that the corporate sector has vast amounts of undrawn credit line commitments available. These credit lines are drawn heavily following a number of adverse shocks, to such an extent that they explain virtually all of the rise of bank-firm credit in response to both the outbreak of COVID-19 and to contractionary monetary policy shocks. At the same time, unused credit line capacity is overwhelmingly held by the largest and least financially constrained firms in the economy. As a result, we find that large firms not only receive the vast majority of credit following the adverse shocks that we study, but can in fact crowd out lending to smaller firms by putting pressure on bank balance sheets with their drawdowns. By shifting the allocation of credit from the most constrained firms in the economy to the least constrained, credit lines may actually worsen the drop in aggregate investment following a negative shock like the COVID-19 outbreak, despite increasing the total amount of credit flowing to the corporate sector. Combining these results, we refer to this influence of credit line facilities on macroeconomic transmission as the *credit line channel*.

Our empirical study of the credit line channel centers on the FR Y-14Q data set (Y14), created by the Federal Reserve System for the purpose of conducting bank stress tests. This data set contains loans made to firms by large U.S. commercial banks over the period 2012 to 2020. Compared with standard U.S. data sets, which are often restricted to public firms and available at low frequency or at time of origination only, our data cover more than 200,000 private firms and are updated

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[1]In principle, debt covenants can be written to constrain borrowing even on precommitted credit lines. We show in Section 3 that the vast majority of unused credit line balances can be drawn without violating typical debt covenants.

quarterly. Importantly, the Y14 data offer detailed information on loan characteristics unavailable in alternative data sets, the most relevant being the distinction between term loans and credit lines, and between used and undrawn credit.[2] Our Y14 data have widespread coverage, on the order of half of all C&I lending by U.S. commercial banks over our sample. In addition, we refine and expand the data on firm financials using information from Compustat and Orbis.[3] Equipped with this unique merged data set, we are able to provide a detailed empirical account of bank credit for U.S. firms, and investigate the role of credit lines at both the aggregate and cross-sectional levels.

Our main empirical results are as follows. First, we document that undrawn credit line balances are vast, with a volume more than 40 percent larger than the total used balances on bank credit lines and term loans *combined*. The size of undrawn balances is stable over time and is robust to adjusting for effective capacity using typical covenant ratios. At the same time, we find that the distribution of undrawn credit is highly skewed, with more than 70 percent of undrawn credit (compared to around 40 percent of used credit), accruing to the top 10 percent of the firm size distribution. Beyond size, firms with more unused credit line capacity exhibit a number of other characteristics associated with being less financially constrained — including being more profitable, less levered, investment grade, public, and older — confirming earlier findings by Sufi (2009), Acharya et al. (2014), and others on a broader sample of public and private firms.

Next, we study how credit lines are dynamically utilized, finding that they are the primary instrument used to respond to a number of shocks at both the idiosyncratic and aggregate levels. We first estimate the response of credit to an idiosyncratic change in cash flows. We find that firms increase credit by 33 cents over the first year following a \$1 drop in cash flows, primarily driven by higher use of existing credit lines. The response of term loans is both economically and statistically insignificant, pointing to credit lines as the key margin of adjustment in response to firm cash flow shocks.

Turning to macroeconomic shocks, we revisit a counterintuitive feature of the data: bank-firm credit typically rises following a contractionary monetary policy shock (Gertler and Gilchrist, 1993a). We reproduce this finding in Figure 1.1 using the more recent identification approach by Romer and Romer (2004). While consumer and real estate lending decline following a contractionary shock, we observe a significant rise in C&I lending.[4] Given our ability to distinguish credit types in the Y14 data, we show that the rise in bank-firm credit is entirely accounted for by

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[2]While some loan- or firm-level data sets like the Shared National Credit Program ("SNC"), Reuter's Dealscan Database, and Compustat Capital IQ allow for distinctions by loan type, and partly for separations into committed and utilized exposure, they are either available only at an annual frequency and for large syndicated loans (SNC), at origination (Dealscan), or for public firms (Capital IQ). Commonly used bank-level data such as the H.8 releases for commercial banks, the Consolidated Reports of Condition and Income ("Call Reports"), or the Consolidated Financial Statements for Bank-Holding Companies (FR Y-9C) do not separate used credit into credit lines and term loans.

[3]Besides the information on credit arrangements, an additional advantage of the Y14 data is its wide coverage on balance sheets and income statements of private firms. Such information is typically difficult to obtain, and our data coverage substantially exceeds that of other data sets that provide such information, such as Dun & Bradstreet or Orbis.

[4]This pattern is robust to a wide array of specifications, identification schemes, and more recent samples, presented in Appendix B. The distinctive response of C&I loans to a monetary policy shock has been studied previously (see, e.g., Gertler and Gilchrist, 1993b, Kashyap and Stein, 1995, and den Haan et al., 2007).
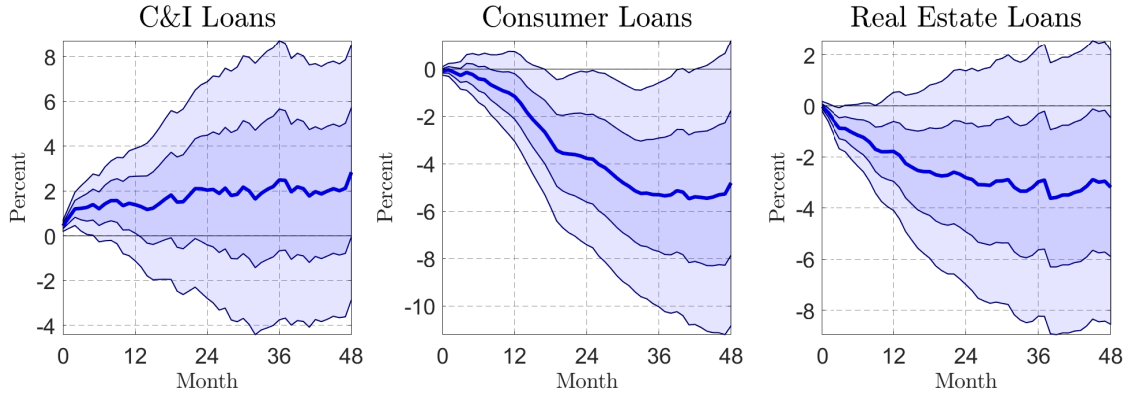
Figure 1.1: Impulse Responses to a Monetary Policy Shock.

Notes: Impulse responses to a 1 percentage point contractionary monetary policy shock based on the identification approach by Romer and Romer (2004). The shock series is taken from Coibion et al. (2017) and the credit series are based on the H.8 releases for U.S. commercial banks from the Board of Governors of the Federal Reserve (see Appendix Table C.1 for details about the data). Sample: 1970:M1 - 2007:M12, the shocks in 1980:M4 - 1980:M6 and 1980:M9 - 1980:M11 are excluded following Coibion (2012). 95 and 68 percent confidence bands are shown using Newey and West (1987) standard errors. See Appendix B for details on the estimation, robustness, and further evidence.

an increase in credit lines, while term loans actually decrease.[5] Decomposing this result further by firm characteristics, we show that this increase in credit lines balances is almost completely driven by large firms with extensive preexisting borrowing capacity.

While credit lines are quantitatively important for the transmission of typical business cycle shocks, they play a particularly important role during crisis episodes. We examine the behavior of credit lines following the COVID-19 outbreak, a macroeconomic event that caused a steep and unexpected decline in cash flows for many firms. Figure 1.2 shows that bank-firm credit sharply increased over this period, while other major credit categories like consumer and real estate lending showed no such surge. This rise in bank credit is almost completely explained by an increased drawdown of existing credit facilities, rather than new credit issuance (Acharya and Steffen, 2020; Li et al., 2020) — a pattern similar to what was observed during the 2007-09 financial crisis (Ivashina and Scharfstein, 2010). Our data further show that these drawdowns were not evenly distributed across the size distribution but instead flowed overwhelmingly to the largest 10 percent of firms (see also Chodorow-Reich et al., 2020).

In our final set of empirical results, we investigate the spillover effects of credit line draws through the bank-firm network. Specifically, we study whether the large drawdowns of existing credit lines in 2020:Q1 resulted in a crowding out of lending for firms that rely on term loans. Using the fixed effects approach of Khwaja and Mian (2008) on the population of firms with term loans from multiple banks, we find that banks experiencing larger credit line drawdowns contract their term lending by more, especially on smaller and non-syndicated loans. Surprisingly, the negative effect of drawdowns on term lending is not offset at all by the large deposit inflows observed

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[5]These differential credit responses are not a recent phenomenon. Using survey data on loans under commitment from the 1970s and 1980s, Sofianos et al. (1990) and Morgan (1998) were able to uncover similar patterns, and these results are confirmed on more recent survey data by Barraza et al. (2019).
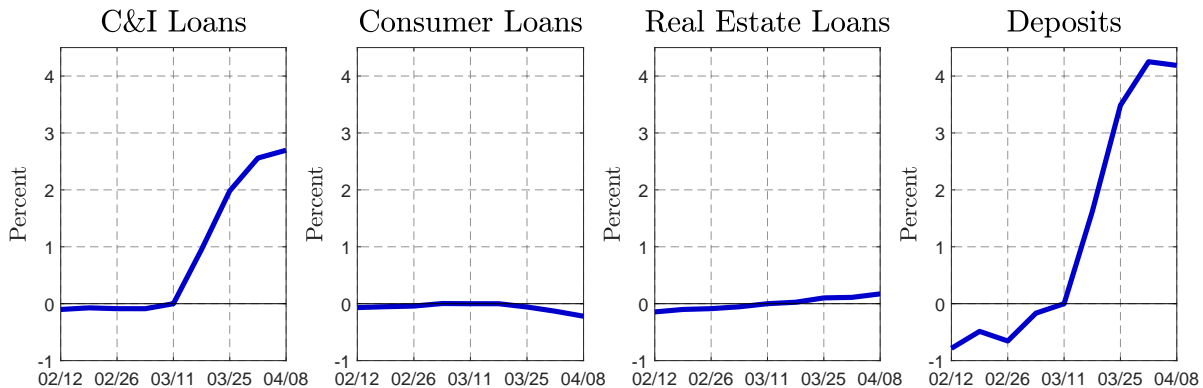
Figure 1.2: U.S. commercial banks' balance sheets around the start of COVID-19.

Notes: The figures show changes in credit relative to total assets on 03/11/2020 around the start of the COVID-19 pandemic in the United States. The series are based on the H.8 releases for U.S. commercial banks from the Board of Governors of the Federal Reserve. See Appendix Table C.1 for further details.

over this period (right panel of Figure 1.2). Instead, the credit supply effect appears more likely driven by non-liquidity concerns such as regulatory limits, as banks with lower pre-crisis capital buffers displayed larger spillovers to term lending. Last, we test whether these crowding-out effects also translated into changes in total firm debt and investment, as firms may have obtained additional credit from other lenders or less constrained firms, or smoothed investment using their cash holdings. We find that the spillover effects influenced total firm debt almost one-for-one and sharply reduced investment, even though affected firms used some of their cash to buffer the lending cuts. However, we obtain these results only for smaller and medium-sized enterprises (SMEs), reinforcing our results on heterogeneity in transmission.

Taken together, our empirical results suggest that credit lines are central to the pass-through of macroeconomic shocks to firm credit. To connect our results to the general equilibrium implications of credit lines on firm credit and investment, we develop a structural model that highlights the interplay between bank term loans and credit lines. Our setup features two types of firms to accommodate our results on firm heterogeneity: smaller "constrained" firms with high exit rates that result in a binding minimum on their dividend payouts, and larger "unconstrained" firms with low exit rates. Each type of firm prefers debt finance due to a tax shield but faces covenant violation risk that increases with firm leverage. Lenders face convex funding costs, so that spreads on new term loans increase as firms obtain more term loans or draw their credit lines.

To mimic the COVID-19 episode, we study the model's response to an adverse shock to aggregate total factor productivity (TFP). To closely connect the model with our empirical findings, we calibrate the model's key parameter that governs the crowding out effect — the elasticity of spreads to changes in the volume of bank credit — to directly match our empirical regression estimates (see Chodorow-Reich (2014), Herreño (2020), and Martín et al. (2020) for similar approaches). We first show that a model in which both types of firms use only term loans fails to match our empirical findings, counterfactually predicting that the relative share of credit held by constrained firms increases following an adverse shock. This occurs because unconstrained firms

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have a more elastic demand for credit due to their flexible dividend margin, leading to a relative decline in their share of credit as spreads rise. Beginning from this baseline, we introduce credit lines, which allow unconstrained firms to borrow at a predetermined, fixed spread. Insulated from rising spreads, these firms borrow heavily, crowding out lending to constrained firms, and reproducing the pattern observed in the data.

In aggregate, the presence of credit lines sharply increases total credit growth to firms following the negative shock, reproducing the pattern documented in Figure 1.2. This occurs because unconstrained firms, now insulated from deteriorating credit conditions by the ex-ante fixed spreads on credit line facilities, borrow heavily, while constrained firms, in an effort to avoid further costly disinvestment, do not decrease their borrowing by an offsetting amount, despite high spreads. But while aggregate credit increases, aggregate investment declines more in an economy with credit lines. This occurs due to a large flow of credit away from constrained firms with a high marginal propensity to invest, and toward unconstrained firms with a low marginal propensity to invest.

Quantitatively, our model implies that the capital stock contracts over the 20Q following the shock more than twice as much in our benchmark economy with credit lines (0.56%) compared to a counterfactual economy without credit lines (0.26%). Instead, much of the additional credit is used to dampen the decline in dividends paid by unconstrained firms, which fall more than seven times less on impact relative to the counterfactual economy (4.63% vs. 35.70%). We show that these same qualitative results carry over in an extension to the model with policy interventions in the bond market, but are quantitatively muted. Thus, one of the most important impacts of bond market interventions over this period may have been indirect, by improving credit access to small firms dependent on bank credit.

In summary, our results point to a world in which the corporate sector as a whole has access to large amounts of credit, even in bad times, but where cross-sectional disparities in access to precommitted credit have powerful implications for the transmission of macroeconomic shocks into corporate debt and investment.

Related Literature.   Our paper relates to a large literature on the transmission of macroeconomic shocks through credit markets.[6] For example, the credit channel of monetary policy posits that the "direct effects of monetary policy on interest rates are amplified by endogenous changes in the external finance premium" (Bernanke and Gertler, 1995). Importantly, an increase in bank-firm credit after a monetary tightening should not be taken as evidence against such a channel. Instead, our main contribution is the finding that amplification mechanisms such as the credit channel are mitigated for a subset of firms — those with credit lines — and, as a result, are *even stronger* for other firms. In this regard, we contribute to a growing literature that emphasizes the heterogeneous effects of macroeconomic shocks, with several recent contributions focusing on

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[6]See e.g., Bernanke et al. (1999), Kiyotaki and Moore (1997), Gertler and Gilchrist (1993a) Gertler and Gilchrist (1994), among many others, and Lian and Ma (2021) for a recent example.

firm responses to changes in monetary policy.[7] Our paper complements this work by demonstrating the centrality of credit lines in driving this phenomenon, which has distinct aggregate and cross-sectional consequences for firm credit and investment.

Turning to the corporate finance literature, we relate to an extensive body of work finding that the pricing and availability of credit lines depend on the risk exposure of both lenders and borrowers.[8] In addition, several papers have shown that credit lines are an important source of funding for firms in times of distress.[9] For example, Brown et al. (2020) use weather events as instruments for cash flow shocks and find that firms use their credit lines to smooth out such shocks. We find estimated responses close to theirs, and further show that firms do not smooth using other types of bank credit in a significant way. Relative to this literature, we take a more macroeconomic perspective that focuses on the aggregate implications of credit lines, made possible by our comprehensive administrative data and the general equilibrium model.

We also obtain important context for our work from the literature on covenant violations. Sufi (2009) and Chodorow-Reich and Falato (2021) show that firms can lose access to their credit lines when they violate their financial covenants. As a result, our findings on the credit line channel should be valid in environments where large, unconstrained firms do not violate their covenants, or when banks are unable or unwilling to restrict credit line access to these firms.

We also connect to a large literature that measures the effects of bank health on the allocation of firm credit (e.g., Khwaja and Mian, 2008) and regional or firm outcomes (e.g., Peek and Rosengren, 2000; Chodorow-Reich, 2014).[10] Our contribution to this literature is to present new evidence that precommitted credit lines can drive a quantitatively important shock to bank balance sheets when a large number of firms draw on their existing credit lines simultaneously. We build on Ivashina and Scharfstein (2010) and Cornett et al. (2011), who provide similar evidence for the 2007-09 financial crisis.[11] These papers use DealScan and Call Reports data, respectively, to document that banks with larger committed credit line exposure cut lending more in the financial crisis. Compared to these works, our matched bank-firm data set allows us to: (i) measure the actual drawdowns of credit lines at each bank, rather than using exposure to proxy for this variable; (ii) document which firms and loan types are crowded out; (iii) control for endogenous matching between banks and firms using the Khwaja and Mian (2008) borrower fixed effects approach; and (iv) trace out the real effects of crowding out at the firm level. Our cross-sectional regression estimates further enable us to calibrate a macroeconomic model and derive general equilibrium

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[7]Examples of recent papers are Ottonello and Winberry (2020), Crouzet and Mehrotra (2020), Jeenas (2019), Cloyne et al. (2019), Bahaj et al. (2020), Darmouni et al. (2020), and Anderson and Cesa-Bianchi (2020), among others, which build on the work by Gertler and Gilchrist (1994).

[8]See, e.g., Campello et al. (2011), Acharya et al. (2013), Acharya et al. (2014), Ippolito et al. (2016), Berg et al. (2017), Acharya et al. (2019), and Acharya et al. (2021a).

[9]See, e.g., Jiménez et al. (2009), Lins et al. (2010), Campello et al. (2010), Berrospide and Meisenzahl (2015), Berg et al. (2016), and Nikolov et al. (2019).

[10]See e.g., Ashcraft (2005), Gan (2007), Paravisini (2008), Schnabl (2012), Jiménez et al. (2012, 2014), Iyer et al. (2014), Huber (2018), Jiménez et al. (2020), Paravisini et al. (2020), Blattner et al. (2020), and Martín et al. (2020), as well as Luck and Zimmermann (2020), Caglio et al. (2021), and Bidder et al. (2020) who also use the Y14 data.

[11]See also the debate between Chari et al. (2008) and Cohen-Cole et al. (2008).

implications, an approach that we share with Chodorow-Reich (2014), Herreño (2020), and Martín et al. (2020).

In addition to this past research, a number of contemporaneous papers have used the same Y14 dataset to document credit flows during the COVID-19 episode, the closest being a study by Chodorow-Reich et al. (2020). While our work focuses on the cross-sectional allocation of unused credit line commitments, arguing that they are plentiful for large firms and scarce for small firms, leading to crowding out, Chodorow-Reich et al. (2020) instead focus on the intensive margin, documenting that smaller firms also drew less of what unused credit line capacity they had. They attribute this finding to the shorter maturities typically found on credit lines to SMEs, which increase lenders' discretion and bargaining power — a separate and parallel channel impinging smaller firms' credit access over this period.

Building on the same data set, Caglio et al. (2021) document several facts about the composition of credit by firm size and investigate the implications for the transmission of monetary policy, finding evidence of firm risk-taking behavior. Kapan and Minoiu (2021) use syndicated loan data from Dealscan to find evidence of crowding out effects similar to those documented in this paper. Finally, Acharya et al. (2021b) show that the liquidity risk posed by credit line drawdowns has explanatory power for bank stock returns during the pandemic.

Overview. The rest of the paper proceeds as follows. Section 2 describes the data, while Section 3 establishes several key stylized facts. Section 4, provides evidence on the use of credit lines and borrowing capacity in the cross-section of firms and show how firms adjust their credit usage in response to cash flow changes. Section 5, revisits the evidence in Figures 1.1 and 1.2, and studies the behavior of firm credit in response to a monetary policy shock and to the outbreak of COVID-19. Section 6 presents a macroeconomic model with credit lines. Section 6.6 provides extensions and robustness. Section 7 concludes.

2   Data

We assemble the data for our empirical analysis from a variety of sources. All loan information on bank-firm relationships and contract terms comes from the FR Y-14Q H.1 collection for commercial loans. The Y14 data consists of information on loan facilities with over $1 million in committed amount, held in the portfolios of bank holding companies (BHCs) subject to the Dodd-Frank Act Stress Tests.[12] The number of BHCs in the Y14 has varied over time, starting with 18 BHCs at inception in 2011:Q3 and peaking at 38 BHCs in 2016:Q4.[13]

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[12]A loan facility is a lending program between a bank and a borrower and can include more than one distinct loan, and possibly contain more than one loan type (e.g., credit line or term loan). Banks classify the facility type according to the loan type with the majority of total committed amount. Since term loans are typically fully used immediately after their issuance, the majority of unused term loan borrowing capacity is likely accounted for by unused credit lines. We therefore assume throughout that unused term loans represent unused credit lines, or "unused credit" for short.

[13]The Federal Reserve requires U.S. BHCs, savings and loan companies, and depository institutions with assets exceeding given thresholds, and also some foreign banking organizations, to comply with the stress test rules. For

We restrict the sample to 2012:Q3 - 2020:Q3. The starting point gives a more even distribution of BHCs across quarters and affords a short phase-in period for the structure of the collection and variables to stabilize. We select facilities to firms that are identified as commercial and industrial, "other loans," and loans secured by owner-occupied commercial real estate. We drop all loans to financial firms and firms in the real estate sector. Appendix C.3 describes these and other sample restrictions in detail. Our analysis therefore focuses on bank credit to nonfinancial firms and does not cover nonbank credit, bank credit extended by non-Y14 banks, or unobserved firms.

The great strength of the data is its rich cross-sectional information and its unparalleled view into loan contracting arrangements for a broad spectrum of firms, especially firms that are smaller and non-publicly traded. In particular, we observe not only the committed amount of the facility, but also the amount utilized in each quarter, allowing us to precisely measure a firm's unused borrowing capacity. Our primary way of identifying a distinct firm is through the Taxpayer Identification Number (TIN). There are 207,505 distinct TINs observed in the Y14 over the sample period, among which we identify 3,222 public firms that can be matched to Compustat data.

The firm financial statement variables are combined from three sources: Compustat, the Y14, and Orbis. We use financial statement data from the quarterly Compustat files whenever possible because publicly traded firms have accurate and uninterrupted quarterly data for the key variables of interest. For all other firms we default to the Y14 financials data, which is typically recorded annually. Since firm financial data are reported at the facility level in the Y14 data, we measure financial variables for a given firm as the medians of those variables over all facilities held by that firm. In addition, if a variable is also observed for a private firm in Orbis, we average the variables from the two sources as a way of further reducing measurement error.[14] The Orbis data also provides us with a measure of firm age for a wide range of private and public firms, defined as the number of years between the data observation date and the firm incorporation date.[15]

Our data sources do not include information on lending covenants. To bridge this gap, we use heuristic covenant formulae taken from Dealscan and apply them to the firm financial statement data (see Appendix C.2 and Section 3 for details). All nominal variables are deflated using the consumer price index for all items. Variable descriptions, data sources, and a list of cleaning and data filtering steps can be found in Appendix C.

3   Descriptive Evidence

In this section, we establish several key stylized facts demonstrating the importance of credit lines for aggregate and cross-sectional credit patterns. We focus on the portion of the sample before the start of the outbreak of COVID-19 in the United States (2012:Q3-2019:Q4), and therefore more

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most of the sample period, the size threshold was set at $50 billion. In 2019, the threshold was increased to $100 billion.

[14]If the Y14 and Orbis data do not differ by more than 5 percent for a particular firm-date observation, then we average the variables from the two sources but exclude the observation otherwise.

[15]To supplement our age data, we use the Field-Ritter data set of company founding dates for public firms (Field and Karpoff, 2002; Loughran and Ritter, 2004), using the Field-Ritter date whenever the value in the Orbis data is missing or the Field-Ritter founding date is older than the one according to Orbis.

Table 3.1: Summary Statistics.

	Total	Credit Lines	Term Loans
Loan Facility Observations	4,496,353	58%	42%
Used Credit	$941B	53%	47%
Committed Credit	$2,231B	78%	22%

Notes: Credit amounts are quarterly averages in billion U.S. Dollars (2015 consumer prices). Sample: 2012:Q3-2019:Q4.

representative of an economy in "normal times." For this period, the data cover around 4.5 million loan facility observations. Summary statistics, presented in Table 3.1, show that credit lines are a central form of bank-firm credit. In terms of counts, 58 percent of the observed facilities are labeled as credit lines, and 53 percent of total used credit. Further, we document that credit lines have enormous quantities of credit that have been committed by lenders but not yet drawn. These undrawn balances are in fact nearly 40 percent larger than total used credit on both credit lines and term loans, representing a vast source of potential financing. Combining used and unused credit, credit lines account for the majority of credit committed by banks (78 percent).

Figure 3.1 shows that these patterns are stable over time, and that unused borrowing capacity substantially exceeds actual used credit throughout the sample. We note, however, that not all of this capacity may be freely drawn in practice, since banks frequently include loan covenants in their lending facilities that restrict further drawdowns if a firm's condition deteriorates in some observable way. To address this, we apply typical ratios on the most common financial covenants found in Dealscan: interest-coverage and debt-to-earnings covenants. Firm-by-firm, we compute the additional amount that could be drawn from credit lines without violating these typical covenant limits as a firm's effective borrowing capacity (see Appendix C.2 for details about the calculations). We aggregate this covenant-adjusted undrawn borrowing capacity, and plot it as the red line in Figure 3.1. While covenant restrictions are nontrivial, roughly two-thirds of unused credit could still be drawn without violating typical covenants, resulting in an aggregate borrowing capacity roughly the same size as total used credit.

The data cover a large number of SMEs. For example, ordering firms by size, the threshold for the top ten percent of the size distribution is $582 million in total assets, $21.5 million for the top 50 percent, and $3.6 million for the bottom 10 percent in 2016:Q4. Figure 3.2 shows how credit, both used and unused, is allocated across the firm size distribution. The largest 10 percent of firms account for around 48 percent of total used term loans and approximately 39 percent of total used credit lines. To some degree, this skew reflects the fact that firm size itself is a skewed distribution. However, in comparison, unused credit is even more skewed than used credit, with the top 10 percent of firms accounting for more than 71 percent of the total unused credit available.[16]

Figure 3.3 displays additional statistics across the firm size distribution, showing that this positive relationship between undrawn balances and size stems from two forces. First, small firms

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[16]One possible use of credit lines is that they serve as a backup for commercial paper. However, we find that firms report such a purpose for only 5.3 percent of committed and 0.4 percent of used credit lines.
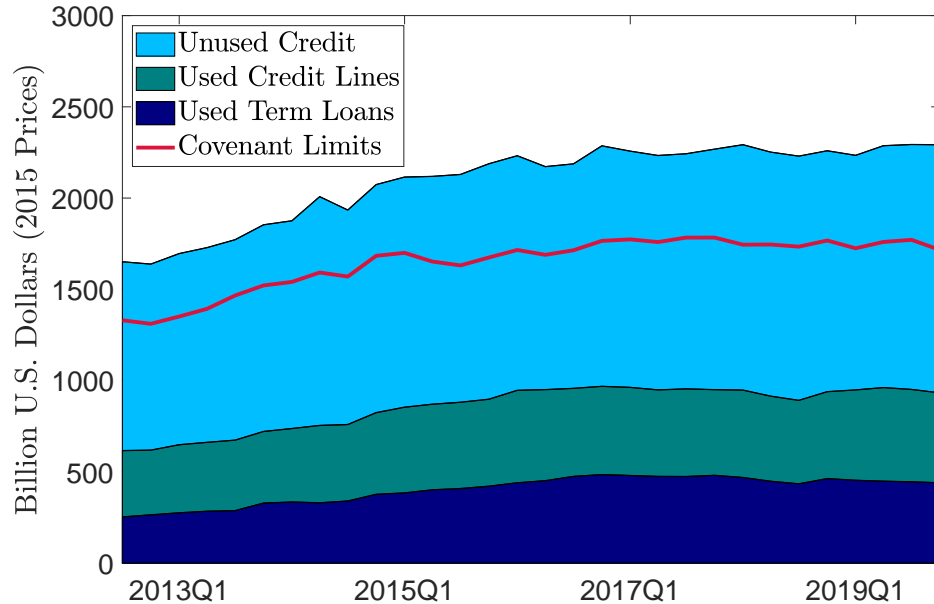
Figure 3.1: Aggregate term loans and credit lines.

Notes: The figure shows the total amount of term loans and credit lines across all banks in billion U.S. dollars. Unused credit is the difference between committed and used credit of credit lines and term loans. The red line indicates covenant-adjusted undrawn borrowing capacity (see text and Appendix C.2 for details). Sample: 2012:Q3 - 2019:Q4.

have lower committed credit line balances. Panel (a) shows that while nearly 100 percent of the largest firms have a credit line facility, that share drops close to 60 percent for the smallest firms. Panel (b) shows that the share of credit lines to total committed credit also trends upward with size.[17] Since firms have incentives to draw credit lines in periods of distress, these patterns may reflect banks' preference for allocating credit line facilities and balances to larger, more profitable firms that are further from the distress boundary (Sufi, 2009).

Second, small firms have lower *undrawn* credit balances because they utilize more of their committed credit. Panel (c) shows that, while firms below the 80th size percentile use a sizable and stable fraction of their committed credit balances, the very largest firms use little of their committed credit line balances, a pattern that is even stronger adjusting for typical covenants (panel (d)). These results point to an additional key reason why small firms lack credit line capacity after adverse shocks. Faced with a trade-off between reserving committed balances to insure against shocks and drawing down committed balances today (e.g., for investment), these smaller, financially constrained firms choose to use more credit in normal times. As a result, even when small firms have access to credit line facilities, their utilization policy may prevent them from drawing additional balances following negative shocks.

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[17]Since we observe only borrowing at a subset of banks, a potential concern is that smaller firms may obtain credit lines from banks that fall outside our data. Based on a firm's total debt from all sources, we are able to verify that our Y14 bank debt contains the majority of total debt for the smaller firms in our sample (see Appendix Figure D.2). The documented patterns can therefore only change for a larger set of firms if the small firms outside of our data are substantially different than the ones that we observe.
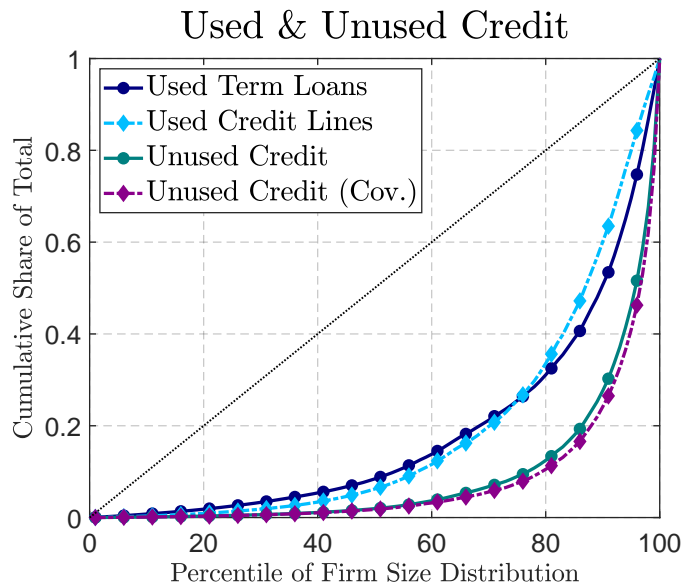
Used & Unused Credit

Figure 3.2: Cumulative Shares across Firm Size Distribution.

Notes: The figure shows the cumulative shares of used term loans, used credit lines, unused credit, and unused credit adjusted for generic covenant rules ("Cov.") across the firm size distribution. Unused credit is the difference between all committed and used credit, which is additionally adjusted by applying generic covenant rules (see Appendix C.2 for details). The firm size distribution is obtained for each date according to firms' total assets. Sample: 2012:Q3 - 2019:Q4.

Appendix Figures D.1-D.3 further show that larger firms receive lower interest rates, are rated more creditworthy according to the banks' internal ratings, are less likely to post collateral, and post less collateral relative to the size of the loan when they do. Smaller firms obtain more fixed-rate and nonsyndicated loans, show higher probabilities of default, often use real estate as a form of collateral, and take on longer-maturity term loans but shorter-maturity credit lines (see also Chodorow-Reich et al., 2020; Caglio et al., 2021).

Last, we investigate credit line pricing. The majority of credit line facilities in the Y14 data are variable-rate loans with fixed spreads. While spreads can in principle be renegotiated, we find that the spread on more than 90 percent of credit lines remains completely unchanged throughout their history, implying that it is safe to consider these spreads as constant over time.

Taken together, the data offer a detailed view into the composition of bank credit for a much larger set of U.S. firms than is typically studied. We show that credit lines account for the majority of used and committed firm credit held by large banks. Cross-sectionally, credit line access and unused borrowing capacity are overwhelmingly concentrated among the largest, most creditworthy firms, and exhibit skewness even beyond what is observed for total credit.

4 Determinants and Use of Firm Credit

In this section, we present results from simple empirical models measuring what determines a firm's unused borrowing capacity beyond its size. We show that various proxies for a borrower's
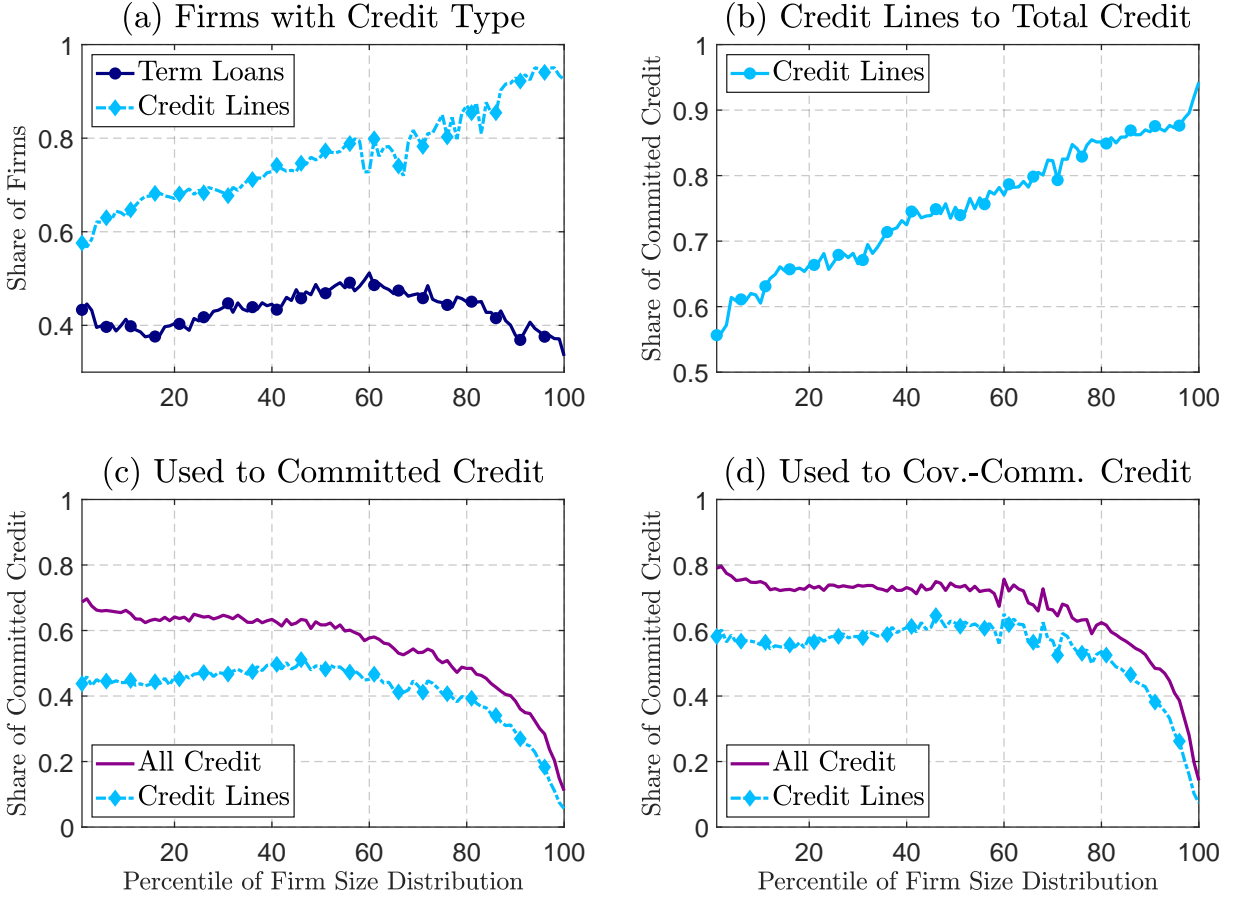
Figure 3.3: Credit Characteristics across Firm Size Distribution.

Notes: The figures show various credit characteristics for percentiles across the firm size distribution. Panel (a) portrays the share of firms that have some committed credit line or term loan. Panel (b) shows the share of committed credit lines relative to all committed credit. Panel (c) displays the share of used relative to committed credit for credit lines and combined credit lines and term loans. Panel (d) shows similar ratios, but additionally adjusts a firm's committed credit for covenant limits, following the computations described in Appendix C.2. The firm size distribution is computed for each date according to firms' total assets. Sample: 2012:Q3 - 2019:Q4.

ability to repay are significant predictors. We also investigate how credit usage adjusts to changes in firm cash flows and find that most of the adjustment is accomplished through credit lines.

4.1 Which firms have credit line borrowing capacity?

We explore specifications related to those in Sufi (2009) and Campello et al. (2011) to understand which type of firms possess credit line borrowing capacity. To this end, we aggregate all credit indicators at the firm level and estimate

$$\frac{\text{Unused Credit}_{i,t}}{\text{Committed Credit}_{i,t}} = \alpha_t + \tau_k + \boldsymbol{\beta} \boldsymbol{X_{i,t-4}} + u_{i,t} \ , \tag{4.1}$$

Table 4.1: Credit Line Borrowing Capacity.

	Size	Age	Public	EBITDA	Leverage	Tangible Assets	Inv. Grade
Coeff.	0.02***	0.02***	0.13***	0.28***	-0.58***	0.18***	0.12***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
R-squared	0.27						

Notes: Estimation results for regression (4.1). Standard errors in parentheses are clustered by firm. Sample: 2012:Q3 - 2019:Q4. Observations: 156,010. Number of firms: 31,209. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

where the dependent variable is the firm's level of unused borrowing capacity on credit lines (1-used credit/committed credit). All specifications include time (α_t) and industry (τ_k) fixed effects. The vector $X_{i,t-4}$ collects several controls that are lagged by four quarters. Firm size is defined as the natural log of a firm's noncash assets. EBITDA and tangible assets are scaled by a firm's noncash assets, while leverage is defined as total liabilities over total assets.[18] Investment Grade and Public are dummy variables denoting whether a firm has an internal rating of BBB or better, and is publicly traded, respectively. For the estimation, we also adjust firms' unused and committed balances for covenants as described above in Section 3.

The results in Table 4.1 show that a higher unused borrowing capacity is more commonly observed among large, old, public, and profitable firms with low leverage that possess more tangible assets and are well rated. These are all well-known proxies for firm credit constraints (see, e.g., Cloyne et al., 2019). These results are consistent with theoretical models that stress the interplay between firm demand for liquidity insurance with lender concerns about moral hazard and other agency problems (e.g., Holmström and Tirole, 1998; Acharya et al., 2014).

As shown in Appendix Table E.1, we obtain estimated coefficients with largely the same signs for two alternative dependent variables in regression (4.1): (i) the log-odds ratio of a 0-1 variable, denoting whether a firm has a credit line facility with a positive committed balance, and (ii) a credit line intensity variable (unused credit/(unused credit + cash)) that measures the extent to which a firm relies on its observed credit line capacity relative to cash as a source of liquidity. Older, public, profitable firms with low leverage are more likely to possess credit line access and tend to rely more heavily on unused credit lines as a potential source of liquidity.[19]

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[18]To eliminate outliers and data entry errors, we exclude observations within the 1 percent tails of the distributions for EBITDA, tangible assets (both relative to noncash assets), and leverage.

[19]In Appendix E.1, we also compare estimations with and without covenant adjustments (Table E.2) and split the sample into private and public firms for the specification without covenant adjustments (Table E.3). Broadly, specifications without covenant adjustments give similar results. In the private-public firm split, firm profitability is particularly important for explaining whether private firms have credit lines and how much unused capacity they possess.

4.2 Credit Responses to Cash Flow Changes

We next investigate how firms use various bank credit instruments to smooth through shocks to their cash flows. We estimate credit responses using the local projections

$$\frac{L_{i,t+h-3} - L_{i,t-4}}{0.5\left(L_{i,t+h-3} + L_{i,t-4}\right)} = \alpha_i^h + \tau_{t,k}^h + \beta^h \frac{\Delta^4 CF_{i,t}}{Assets_{i,t-4}} + \gamma^h X_{i,t-4} + u_{i,t-3}^h \ , \tag{4.2}$$

where $h = 0, 1, 2, ..., 8$ and $L_{i,t}$ denotes credit of firm i at time t. In this regression setup, we use the symmetric growth rate of firm i's credit as the dependent variable.[20] This symmetric growth rate is able to accommodate changes in credit from a starting level of zero, and is always bounded between -2 and 2, removing the typical challenge of extreme outliers and the need to winsorize.

We measure growth between $t - 4$ and $t + h - 3$ due to a timing feature of our data. Specifically, our main cash flow variable records total net income over the preceding 12 months. As a result, the 4Q change in this variable at time t reflects changes in the period from $t - 3$ to t relative to the period from $t - 7$ to $t - 4$. Since the change in cash flows could thus have occurred as early as $t - 3$, we begin our impulse response at that time ($h = 0$). At the same time, $\Delta^4 CF_{i,t}$ reflects changes in cash flows as late as time t, we should expect the estimated effects to build between $h = 0$ (time $t - 3$) and $h = 3$ (time t) as additional shocks arrive.

Our coefficient of interest is β^h, associated with a firm's change in cash flow $\Delta^4 CF_{i,t}$ scaled by total assets. In addition, all specifications include a firm-horizon fixed effect (α_i^h) and an industry-time-horizon fixed effect ($\tau_{t,k}^h$). The vector $X_{i,t-4}$ contains several firm controls: log of total assets, (cash and marketable securities)/total assets, tangible assets/total assets, and leverage. All firm financial variables are lagged by four quarters. In addition, $X_{i,t-4}$ includes two lagged values of the change in the cash flow variable and two lags of the four-quarter change in the dependent variable to account for possible serial correlation. Moreover, to address outliers and measurement error in $\Delta^4 CF_{i,t}/Assets_{i,t-4}$, as well as to focus on typical cash flow changes, we exclude absolute annual changes of $\Delta^4 CF_{i,t}/Assets_{i,t-4}$ that are larger than 5 percentage points.[21]

The various control variables are intended to absorb non-cash flow drivers of firm credit, so that β^h captures the remaining variation due to cash flow changes. Even so, interpreting β^h as a causal estimate would face identification challenges. Instead, our results focus on the differences in β^h across credit categories to decompose the roles of credit lines and term loans in driving the observed correlations of cash flow changes and credit growth at various horizons.

Figure 4.1 shows the negative of the estimated coefficients β^h to facilitate the interpretation. After a fall in net income, firms increase their total use of credit immediately (panel (a)). The rise in credit to a negative cash flow change reaches a peak after three quarters, and actually

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[20]The symmetric growth rate is the second-order approximation of the log-difference for growth rates around zero and has been used in a variety of contexts (see, e.g., Gomez et al., 2020).

[21]This assumption approximately corresponds to excluding observations below the 15th and above the 85th percentiles of the sample distribution. In addition, the sample is also constrained to a balanced panel and loan histories with time gaps are excluded. Our results are robust to considering absolute annual changes in net income relative to assets that are smaller than 10 p.p.
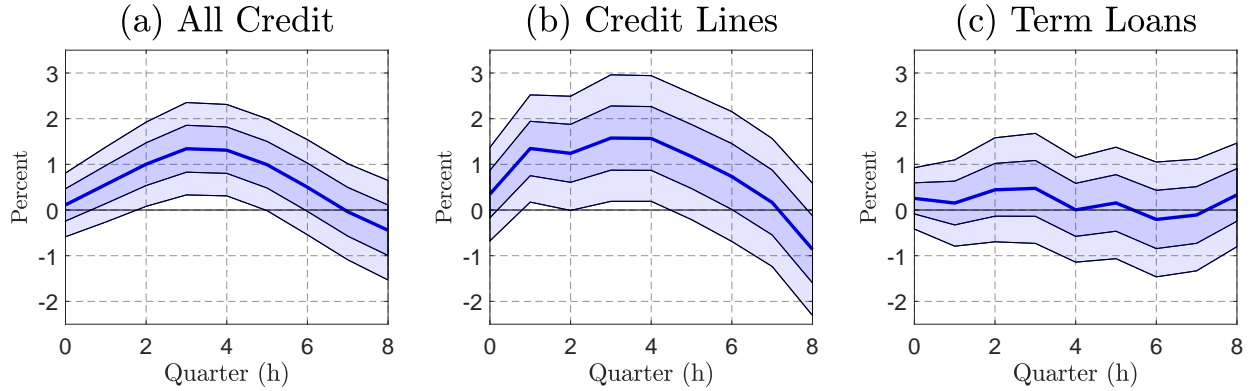
Figure 4.1: Credit Responses to a Cash-Flow Change.

Notes: Responses of firms' total used credit, credit lines, and term loans to a one-unit decrease in net income relative to assets, based on the local projection approach in (4.2). Plots display estimates $-\beta^h$, corresponding to a decline in cash flows. Observations with absolute annual changes in net income relative to assets larger than 5 percent are excluded. The estimations are based on a balanced panel for each credit type and include 9448 (panel (a)), 6751 (panel (b)), and 3913 (panel (c)) observations for each impulse response horizon. 95 and 68 percent confidence bands are shown using standard errors that are clustered by firm. Sample: 2012:Q3 - 2019:Q4.

becomes negative after around six quarters, indicating that firms' creditworthiness deteriorates in the medium run. Panels (b) and (c) show that the rise in total credit is completely accounted for by the adjustment in credit lines. By contrast, there is no statistically significant adjustment in term loan usage, with point estimates close to zero at all horizons.

To understand the quantitative importance of these effects, we re-estimate regression (4.2) using the one-year change in total firm debt relative to assets as a dependent variable. We find that a \$1 drop in net income is associated with an increase in total debt of around 33 cents, which is statistically different from zero at the 5 percent level.[22] Most important for our analysis, we find that more than half of this change can be accounted for by credit lines drawn in our data, a lower bound given that we observe only a subset of banks.

In Appendix E.2, we provide two refinements of these results. First, interacting the firm's cash flow change variable with lagged borrowing capacity shows that the adjustment in credit line usage is strongest for firms that have relatively more capacity prior to the cash flow change (Figure E.1). Second, there is relatively little adjustment in committed credit lines to changes in cash flow. Instead, the response of credit is most clearly detected in a change in utilization rates of existing credit lines (Figure E.2).

5 Behavior of Firm Credit around Macroeconomic Events

The evidence so far shows that credit lines account for the majority of bank-firm credit in the aggregate and enable firms to meet their short-run liquidity needs following changes to their cash

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[22]This estimate is very close to the one by Brown et al. (2020) who use weather events to instrument for cash-flow shocks and find a total debt increase of 35 cents for a \$1 drop in net income.

flows. In this section, we study the role of credit lines in shaping the response of firm borrowing to macroeconomic shocks in the aggregate and the cross-section. In particular, we revisit the evidence presented in the introduction on the responses of bank-firm credit to monetary policy shocks and to the outbreak of COVID-19. We show that credit lines are the main driver of the increase in overall credit to these two types of adverse macroeconomic shocks.

5.1 Credit Responses to Monetary Policy Surprises

We take two approaches to understand whether the responses in Figure 1.1 can be explained by an increase in credit lines after a monetary policy tightening. First, we construct aggregate time series for term loans and credit lines based on the micro-data and estimate separate responses for each. Second, to study the cross-sectional reallocation induced by these shocks, we replicate the responses using firm level data in Appendix F.2, which also allows us to decompose the aggregate response by prior firm characteristics.

Denote the total loan volume of some credit type at time t across all firms $i = 1, ..., N$ and banks $j = 1, ..., J$ by $L_t = \sum_{i=1}^{N} \sum_{j=1}^{J} L_{i,t}^j$. Based on these quarterly "aggregate" time series, we estimate impulse responses using the specification

$$\frac{L_{t+h} - L_{t-1}}{L_{t-1}} = \alpha^h + \beta^h \epsilon_t^{MP} + \gamma^h X_{t-1} + u_t^h \ , \tag{5.1}$$

where $h = 0, 1, ..., 8$ and ϵ_t^{MP} denotes the monetary policy shock at time t. Since the short-term policy rate was expected to remain at its lower bound for a large part of the sample for which the Y14 data is available, we use surprise movements in the two-year government bond yield as a measure of the shock. In particular, we employ the high-frequency identification approach as in Gürkaynak et al. (2005) based on the assumption that monetary policy news dominates over other factors in such tight windows.[23] To match the frequency of the Y14 data, we convert the surprises from a meeting-by-meeting frequency to a quarterly frequency by summing all meeting surprises within a quarter. The resulting series is shown in Appendix Figure F.1. The vector X_{t-1} collects several controls: two lagged values of the one-quarter growth rate of the dependent variable and two lags of the monetary policy shock.[24]

The coefficient of interest in (5.1) is β^h, which captures the response of credit at horizon h to a monetary policy shock. For the sample 2012:Q3 - 2019:Q4, Figure 5.1 shows the estimated coefficients, depending on whether the credit type are credit lines, term loans, or the sum of the two. Reassuringly, the response of all credit (panel (a)) takes a similar shape as that of Figure 1.1 for a more recent sample, showing an expansion of aggregate bank-firm credit following a

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[23]The surprises are given by changes in the two-year Treasury note over a 30-minute-window around policy announcements: 10 minutes before, 20 minutes after an announcement.

[24]The lag length is chosen to yield good performance, as measured by the Aikake and Bayes information criteria, across outcome variables and impulse response horizons. In unreported results, we find that the results are similar without any controls or using four lagged values of both the one-quarter growth rate of the dependent variable and the monetary policy shock.
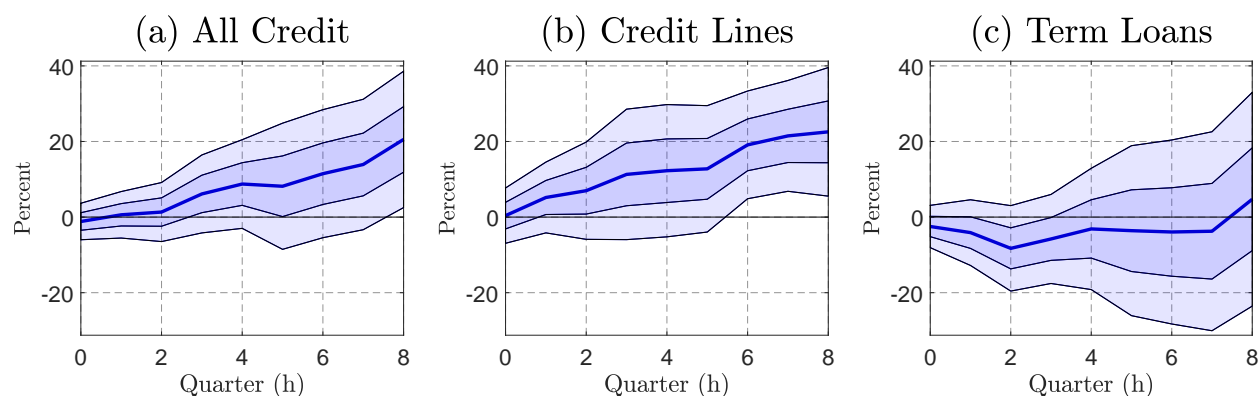
Figure 5.1: Aggregate Credit Responses to a Monetary Policy Surprise.

Notes: Impulse responses to a 25 basis point surprise increase in the two-year government bond yield based on the local projection approach in (5.1), multiplied by 100. 95 and 68 percent confidence bands are shown using Newey and West (1987) standard errors. Sample: 2012:Q3 - 2019:Q4.

surprise monetary tightening.[25] The two other panels decompose the drivers of this response into changes in credit lines and term loans, showing that the aggregate increase is entirely accounted for by an increase in credit lines, while term loans actually decrease.[26] These results suggest that firms strongly prefer to smooth borrowing through their pre-negotiated credit lines, rather than returning to the market for new credit facilities.

In Appendix F.1, we provide additional robustness checks and refinements of our findings. First, we show that the results are similar when using the monetary policy surprise series by Nakamura and Steinsson (2018), shown in Figure F.1, which loads more heavily on the short end of the yield curve (see Figure F.3). Second, the monetary policy announcements may entail a release of private information by the Federal Reserve (Nakamura and Steinsson, 2018), and our results may reflect responses to the information news instead. While various macroeconomic indicators in Appendix Figure F.2 show typical reactions to monetary policy shocks and therefore speak against such a concern, we also follow Jarociński and Karadi (2020) and exclude policy meeting surprises that are associated with nonstandard stock price responses.[27] Figure F.3 shows the impulse responses based on this series, which are much the same as the ones in Figure 5.1. Last,

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[25]Compared with the estimates in Figure 1.1, the responses are relatively large: total credit rises by more than 50 percent to a 100 basis point increase in the two-year government yield. These differences can be explained by (i) the larger size of the shock (a one percentage point increase in the federal funds rate versus the two-year government bond yield), (ii) the fact that the high-frequency shocks come as true surprises whereas the shocks by Romer and Romer (2004) could partly be anticipated, and (iii) that credit lines were less common for the early part of the sample that is used in Figure 1.1 (1970:M1 - 2007:M12). In line with these explanations, Appendix Figure B.3 also shows larger responses of C&I loans based on the shock series by Nakamura and Steinsson (2018) for the sample 1994:M1 - 2007:M12.

[26]Appendix Figure F.2 shows the corresponding responses of several other aggregate indicators. To a monetary tightening, economic activity and prices decline, while corporate bond spreads rise. Similar to Figure 1.1, C&I loans increase, whereas consumer and real estate credit fall. The delayed response of credit lines coincides with the observed inertia in the decline of economic activity and firm cash-flows (Christiano et al., 2005). The credit response also moves in tandem with the rise of the Gilchrist and Zakrajšek (2012) spread, consistent with the relative pricing of credit line facilities with predetermined spreads driving the flow of credit.

[27]We define nonstandard responses as tightening (easing) surprises that show a simultaneous increase (decrease) in the S&P500 stock index. The updated results are shown in Appendix Figure F.1.

we use the full scope of the micro-data in Appendix F.2 to understand which firms account for the aggregate responses. We find that the total response of credit lines is almost entirely explained by firms that are large and have substantial ex-ante borrowing capacity. Taken together, these results show that credit lines are quantitatively important for the transmission of monetary policy through bank lending.

5.2 Credit Movements during the COVID-19 Pandemic

The outbreak of the COVID-19 pandemic and the closure of certain businesses entailed a sharp fall in (expected) cash flows for the majority of firms in the United States. It therefore represents a unique setting for studying changes in firm credit, in particular because the outbreak was largely unanticipated. The pandemic started in mid-March 2020 in the United States. Comparing end-of-quarter stocks, the points at which the credit variables are measured in the Y14 data, between 2019:Q4 and 2020:Q1 therefore provides an accurate depiction of the immediate changes in credit that likely resulted from the shock. In Figure 5.2, we plot differences in used and committed credit between 2019:Q4 and 2020:Q1 for all firms (blue bars) and separately for firms within the top 10 percent and the bottom 90 percent of the firm size distribution (orange and yellow bars), with a cutoff between the two groups of around \$1.2 billion in total assets. That is, we compute

$$\frac{\left(L^{k,g}_{2020:Q1} - L^{k,g}_{2019:Q4} \right)}{\text{Total Used Credit}_{2019:Q4}} \quad ,$$

where $L^{k,g}_{t}$ denotes the amount of credit type k at time t of group g (all firms, top 10 percent, bottom 90 percent).[28] All changes are scaled by total used bank-firm credit in 2019:Q4, implying an additive decomposition. The quantitative differences relative to Figure 1.2 are due to the alternative scaling by bank total assets.

Panel (a) of Figure 5.2 shows that the overwhelming majority of the change in credit, around 90 percent, stems from existing credit lines. Issuance of new term loans or new credit lines, while positive, played a minor role. Breaking down these effects by firm size, we find that roughly 95 percent of the additional credit issued over this period flowed to the top 10 percent of the size distribution, even though these firms hold less than half of all used bank-firm credit in normal times (see Figure 3.2). Existing credit lines of large firms alone explain around 77 percent of the increase in total credit. In contrast, the bottom 90 percent of firms saw a modest increase in credit from existing lines, as well as small decreases in the issuance of new lines and loans.

Panel (b) of Figure 5.2 shows changes in committed, rather than used, credit. In aggregate, committed credit barely increased over this period, showing that credit growth was almost completely accounted for by an increased utilization of existing credit line commitments. While the

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[28]The firm size distribution for both quarters is computed according to firms' total assets in 2019:Q4, meaning that firms remain within the same group across the two quarters. Credit of type k for observations with missing total assets in 2019:Q4 is allocated to the top 10 percent or the bottom 90 percent according to the share of each group of total credit of type k across nonmissing observations.
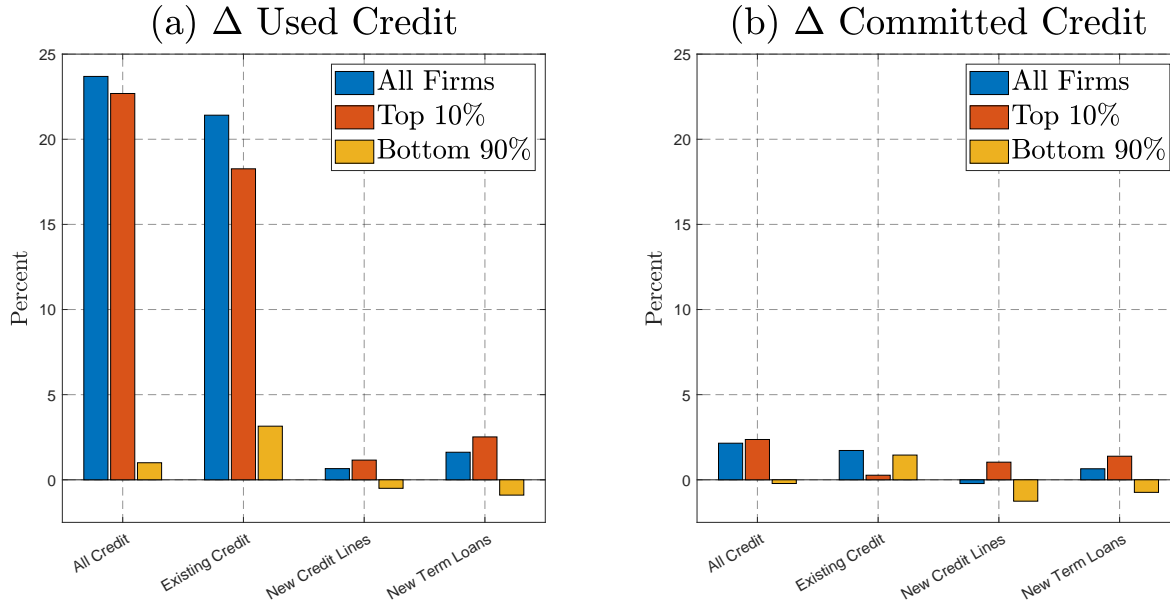
Figure 5.2: Changes in Used and Committed Credit for 2019:Q4 - 2020:Q1.

Notes: The blue bars show aggregate changes in used and committed credit across all banks between 2019:Q4 and 2020:Q1, relative to total used credit in 2019:Q4. The orange and yellow bars display equivalent changes for the 10 percent and the bottom 90 percent of the firm size distribution, also relative to total used credit in 2019:Q4. The changes are further separated into differences in existing credit, new credit line issuances, and new term loans (all in percent relative to all used credit in 2019:Q4). The firm size distribution is computed according to firms' total assets in 2019:Q4.

largest 10 percent of firms were able to slightly increase the balances of committed credit, the bottom 90 percent displayed a small drop stemming from lower commitments on new facilities.

Appendix Figure F.7 repeats these calculations for changes from 2019:Q4 to 2020:Q2 and to 2020:Q3. While more than half of the initial increase of used existing credit tapers off in 2020:Q2, the change for the bottom 90 percent of firms actually turns negative for the two-quarter comparison and the one for the top 10 percent remains elevated. Similar heterogeneity is present for new credit issuances. By the end of 2020:Q3, the initial increase in bank-firm credit reversed in the aggregate, with some of the distributional differences still remaining.[29]

5.3 Credit Supply during the COVID-19 Pandemic

While these patterns indicate that access to credit differed in the cross-section of firms, they do not distinguish between credit demand and supply, and possible spillovers between firms. In particular, the large withdrawal of existing credit lines may have put pressure on bank balance sheets. In turn, banks may have reduced their supply of term loans, an important source of credit to SMEs. We test for such crowding out effects by employing a fixed effect regression similar to Khwaja and Mian (2008). The methodology for estimating a credit supply channel focuses on

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[29]In Appendix F.3, we provide further evidence that credit shifted toward less financially constrained firms. Firms that accessed their credit lines in 2020:Q1 were disproportionately large, profitable, and public firms compared to the 2012:Q3 - 2019:Q4 presample (see also Chodorow-Reich et al., 2020).

firms borrowing from multiple banks, where banks differ in their exposure to the outbreak of COVID-19. As a measure of exposure, we use variation in drawdowns on existing credit lines.

The approach relies on two key identifying assumptions. First, the shock must be exogenous, an assumption that we believe is satisfied, since the outbreak was largely unanticipated at the end of 2019. Second, a firm's demand for term loans should not depend on its bank's differential exposure to the shock, holding the terms of the loan fixed. This second assumption would, for example, be violated if firms substitute between credit lines and term loans at some bank. One possible reason for such a relation is that credit lines become relatively cheaper when the general cost of credit increases, since they feature fixed, predetermined spreads. To ensure that the second identifying assumption is satisfied, we restrict the sample to term loans only, and exclude cases where firms have both term loans and credit lines at the same bank, so that our results are not driven by substitution between the two. Based on the restricted data set, we estimate

$$\frac{L_{i,t+h}^{j,k} - L_{i,t-1}^{j,k}}{0.5\left(L_{i,t+h}^{j,k} + L_{i,t-1}^{j,k}\right)} = \alpha_{i,h}^k + \beta^h \frac{\Delta Credit\ Line\ Usage_t^j}{Assets_{t-1}^j} + \mu^h \frac{\Delta Deposits_t^j}{Assets_{t-1}^j} + \gamma^h X_{t-1}^j + u_{i,h}^{j,k}, \qquad (5.2)$$

for $h = 0, 1, 2$, where $t - 1$ denotes 2019:Q4 and $t + h$ is given by one of the following three quarters. For the dependent variable, we use the same formulation as in Section 4.2, which allows for possible zero-observations in $t - 1$ or $t + h$ and is bounded in the range $[-2, 2]$. $L_{i,t}^{j,k}$ is the aggregated loan amount of type k between bank j and firm i at time t. We consider variable- and fixed-rate loans as separate types k to account for possible differences in the demand for such loans due to changes in short-term interest rates between $t - 1$ and $t + h$ that may be correlated with the drawdowns at the bank-level, and we again exclude observations with both types at the same bank. The firm-specific fixed effect $\alpha_{i,h}^k$ absorbs a firm's common demand for credit type k. The estimated coefficient β associated with the change of used existing credit lines between $t - 1$ and t at bank j, relative to total bank assets in $t - 1$, therefore captures credit supply effects: banks may differ in their supply of term loans due to their differential intensity of credit line withdrawals.[30] X_{t-1}^j represents a vector of controls, which are omitted in the baseline specification and added subsequently to test the robustness of the results.

The estimation results are shown in Table 5.1. Column (i) shows the results for used term loans between 2019:Q4 and 2020:Q1. The negative sign of the coefficient β implies that a bank that experiences a larger drawdown of credit lines restricts its supply of term loans by more and this effect is statistically different from zero at the 5 percent level.[31] In column (ii), we extend the

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[30]Drawdowns on precommitted credit lines cannot generally be refused by banks, unless the borrower has violated its debt covenants or "material adverse change clauses" (Demiroglu and James, 2011). However, banks may use informal bargaining power to pressure firms not to draw their credit lines (Chodorow-Reich et al., 2020) or react to covenant violations on other credit lines more strongly when they experience large drawdowns (Chodorow-Reich and Falato, 2021). If banks discourage drawdowns more when their own balance sheets are impaired, then the estimated effects in Table 5.1 can be seen as a lower bound on the strength of the crowding out effect.

[31]Alongside these quantity responses, we test for price responses using the change in the interest rate, weighted by used term loans, as a dependent variable in (5.2), and report the results in Appendix Table F.2. For 2020:Q2/Q3, we find a positive and statistically significant coefficient β. However, these results are sensitive to excluding outliers, and generally less precise than our quantity responses.

Table 5.1: COVID-19 − Credit Supply.

	(i) 2020:Q1	(ii) 2020:Q1	(iii) 2020:Q1	(iv) 2020:Q2	(v) 2020:Q3	(vi) 2020:Q1
Δ Credit Line Usage	-1.96**	-2.28***	-2.57***	-3.03**	-3.63**	-1.70**
	(0.72)	(0.65)	(0.91)	(1.14)	(1.62)	(0.66)
Δ Deposits						0.14
						(0.20)
Fixed Effects						
Firm × Rate	✓			✓	✓	✓
Firm × Rate × Maturity		✓				
Firm × Rate × Purpose			✓			
Bank Controls			✓			✓
R-squared	0.51	0.51	0.55	0.51	0.53	0.51
Observations	1,678	1,596	1,007	1,519	1,390	1,638
Number of Firms	749	712	464	682	624	733
Number of Banks	28	28	27	28	28	26

Notes: Estimation results for regressions (5.2), where the dependent variable is given by changes in credit between 2019:Q4 and 2020:Q1 in columns (i)-(iii) and (vi), from 2019:Q4 to 2020:Q2 in column (iv), and between 2019:Q4 and 2020:Q3 in column (v). The regressors "Δ Credit Line Usage" and "Δ Deposits" denote the change of a bank's used existing credit lines or deposits from 2019:Q4 to 2020:Q1, relative to total assets in 2019:Q4. All regressions include firm-specific fixed effects that additionally vary by rate type (adjustable- or fixed-rate) and the remaining maturity (column ii) or the loan purpose (column iii). Maturity fixed effects take the form of three bins according to their remaining maturity in 2019:Q4: (i) less than one quarter, (ii) less than one year, and (iii) more than one year. Columns (iii) and (vi) include various bank controls for 2019:Q4: bank size (natural log of total assets), return on assets (net income/total assets), leverage (total liabilities/total assets), deposit share (total deposits/total assets), and banks' income gap (see Appendix Table C.5 for details on the data). All specifications are estimated using OLS. Standard errors in parentheses are clustered by bank. Sample: 2019:Q4 - 2020:Q3. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

fixed effect to cover not only loan types according to the flexibility of their interest rate but also by their remaining maturity. This extension checks the robustness of the results for the possibility that the amount of credit line drawdowns and the maturity profile of a bank's term loan portfolio are correlated, and a firm's credit demand depends on the remaining maturity (see also Khwaja and Mian, 2008). If anything, the results become stronger with the extended fixed effect.

    Another potential identification concern may be that banks specialize in certain types of lending and the associated credit demand for such borrowing is correlated with the credit line drawdowns across banks (Paravisini et al., 2020). For bank specialization to explain our results, banks would have to hedge their lending activities across loan types, such that banks with larger credit line drawdowns specialize in providing term loans that are less likely to be associated with firms' short-run liquidity needs. To address this concern, we additionally allow for the firm fixed effect in regression (5.2) to vary with the loan purpose that firms report.[32] To account for other pre-crisis

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[32]We distinguish between the purposes "Working Capital," "Capital Expenditures" (including real estate), "M&A Financing," and "All Other Purposes" (see also Appendix Table C.2).

differences across banks, we also include various bank-specific controls that are collected in the vector X_{t-1}^j in regression (5.2). Among these, bank size could account for the possibility that firms may prefer to borrow from smaller relationship banks that offer fewer credit lines during a crisis. Column (iii) in Table 5.1 shows that the results actually intensify with the extended fixed effect and the additional control variables.

In Appendix F.3, we show that these findings are robust to various modifications of the regression specification. We first test whether our results hold in the absence of the firm fixed effect $\alpha_{i,h}^k$. Table F.3 presents these results for the multi-lender subsample, showing that the resulting coefficients are close to those in Table 5.1. Table F.4 removes the restriction that firms borrow from multiple lenders, estimating coefficients that are slightly attenuated but highly statistically significant for this extended sample of nearly 30,000 firms. Table F.4 further shows strong spillovers for firms with a single lender, a subsample that includes many of the smallest firms that we observe. These results illustrate that firm credit demand and bank credit supply shocks are relatively uncorrelated for that sample of firms, and that the firm fixed effect is not critical to our results.

The crowding out effects that we uncover are potentially a smaller concern if they are relatively short-lived. To test for the persistence of the results, we consider changes in the dependent variable from 2019:Q4 to 2020:Q2/Q3 in regressions (5.2). As shown in columns (iv) and (v) of Table 5.1, the effects not only remain but actually intensify towards the end of 2020:Q3, even though the initial drawdowns were largely repaid by 2020:Q3. Our results are therefore consistent with inertia or planning time in bank lending decisions, or persistence in the uncertainty of additional drawdowns. Tables F.5 and F.6 compare alternative fixed effect and control specifications for 2020:Q2/Q3, showing that the results are robust across these quarters as well.

To measure economic significance, we combine our results using a back-of-the-envelope calculation. Given the average ratio of term lending to bank assets that we observe, these estimates imply a term lending cut of around 10-30 cents for a $1 drawdown of credit lines.[33] While these spillover effects are already substantial, we consider them a lower bound on the total crowding out effect, which likely extends to other forms of credit not present in our sample such as small business, consumer, and real estate credit.

Finally, we test how these spillover effects differ by loan characteristics, with the results shown in Appendix Table F.7. Our findings are largely explained by a supply contraction of smaller, fixed-rate, and non-syndicated loans. All of these characteristics are more prevalent among SMEs, implying that these smaller firms faced a sharper lending cut due to drawdowns.

Liquidity and Bank Constraints. These spillover effects are perhaps surprising given that aggregate bank deposits increased by more than C&I loans over this period (see Figure 1.2) and, as pointed out in Gatev and Strahan (2006), banks generally have an incentive to match the cyclicality of their deposit flows and credit line draws. Even if banks were short on deposits, they could

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[33]This is computed by multiplying the typical ratio of term lending to bank assets across the Y14 banks ($\sim$5 percent) with the range of estimates for β in Tables 5.1, F.5, and F.6, which lie between -2 and -6.

have obtained additional liquidity through the interbank market. Taken together, this suggests that banks should have had more than sufficient funding to cover their credit line draws without contracting their other lending activity. Instead, we find strong evidence of a credit crunch in the market for term loans depending on banks' credit line exposure, a result that we should not obtain if the balance sheet pressures were offset by the available liquidity.

To isolate the role of deposits, we modify the baseline specification and additionally control for the deposit inflow in the first quarter of 2020, denoted by $\Delta Deposits_t^j / Assets_{t-1}^j$ in regressions (5.2), as well as various other bank characteristics collected in X_{t-1}^j.[34] Column (vi) in Table 5.1 shows the estimation results. Despite the additional controls, the estimated coefficient β remains nearly unchanged compared with the baseline in column (i). At the same time, we find a coefficient close to zero on the change in deposits, and can easily reject the hypothesis $\beta + \mu = 0$. In other words, our estimates imply that the combination of a \$1 deposit inflow, paired with a simultaneous \$1 outflow on a drawn credit line is not neutral, but instead causes a significant decrease in that bank's supply of term loans. This lack of equivalence between deposit inflows and credit line outflows can explain our findings of a term loan crunch in an environment of plentiful deposits.

Aware of the pressure on banks' balance sheets, policymakers provided liquidity to financial markets, established lending programs targeting in particular SMEs, eased restrictions on banks, and also indirectly supported them through various monetary and fiscal actions.[35] While our findings can be understood as a rationale for such interventions, they also show that the policy actions did not completely offset the pressure from the drawdowns on bank balance sheets, which would have instead led us to find $\beta \approx 0$.

We interpret these results as providing strong evidence that banks were averse to taking on additional risk or were facing constraints on lending in spite of the direct availability of funds in the weeks following the outbreak of the pandemic. A particular mechanism that can explain our findings works through bank regulatory capital. Undrawn balances on credit lines typically have

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[34]The regressors of interest in equation (5.2) show substantial variation across banks. The drawdown on existing credit lines relative to lagged assets ranges from -0.2 to around 3.6 percent with a standard deviation of around 1 percent. The change in deposits relative to lagged assets ranges from 0.9 percent to around 31 percent with a standard deviation of around 6.7 percent. The two variables are negatively correlated with a correlation coefficient of -0.16, suggesting that a mechanism by which credit line drawdowns are immediately re-deposited at the same bank was not a dominant driver of deposit flows. In addition, using weekly deposit rate data from Ratewatch and weekly balance sheet data for U.S. commercial banks from the FR-2644 forms, we find no evidence that banks that paid higher deposit rates attracted more deposits over the period that is shown in Figure 1.2 from 2/12/2020 to 4/8/2020 (results not reported), suggesting that the deposit inflow was not strongly influenced by individual bank policies.

[35]In response to the outbreak of the pandemic, the Economic Injury Disaster Loans, the Paycheck Protection Program (PPP), and the Main Street Lending Program (MSLP) provided credit to SMEs. With respect to our results, we note that PPP are not part of the Y14 data, and that while MSLP loans do appear in the Y14 data, our subsample used for our results in Table 5.1 does not contain any, likely due to the relatively low initial usage of this program. We believe these lending programs are unlikely to confound our results for three reasons. First, both programs begin in 2020:Q2, while our results are already visible in 2020:Q1. Second, the larger banks that experienced a higher rate of credit line drawdowns in our data also extended fewer PPP loans on average (Granja et al., 2020). As a result, it is unlikely that these banks reduced term lending to our subsample of firms due to substitution into PPP loans. Third, we show in Table 5.2 that our results carry through to total firm debt from all sources, implying that incorporating PPP lending would not materially change our findings.

substantially lower regulatory risk-weights than drawn balances (Pelzl and Valderrama, 2019).[36] When credit lines are used and appear on bank balance sheets, they may therefore tie up bank capital, leading banks to compensate by cutting their term lending supply. To test whether regulatory constraints can explain our results, we consider alternative specifications of regression (5.2) that allow for interactions between the credit line drawdowns and bank capital buffers in 2019:Q4. The results are reported in Appendix Table F.8. Consistent with the described mechanism, we find that banks with lower pre-crisis capital buffers restricted their term lending supply to a greater degree in response to drawdowns on their credit lines.

A potential alternative explanation for our results is that banks with larger credit line drawdowns also experienced a stronger decline in the expected profitability of their legacy loans. In principle, declining health could both induce large drawdowns as firms "run" on their credit lines, and also lead the bank to reduce term lending, without any causal link between the two. To address this concern, we follow two approaches. First, we use an instrumental variable estimation for regression (5.2). As an instrument for the credit line drawdowns, we use banks' ratio of unused credit commitments relative to assets in 2019:Q4 with the identifying assumption that banks with different ratios have otherwise similar loan portfolios.[37] Second, we directly control for the change in the quality of a bank's existing term loan portfolio using banks' reported probabilities of default and provision for loan losses from banks' income statements. The results are reported in Appendix Table F.9 and are close to our baseline estimates.

Crowding Out Effects on Total Debt and Investment. Our estimates so far reflect relative responses of credit at banks in our sample. Thus, to the extent that firms may substitute across banks, toward non-bank lenders, or receive credit from other firms, the effects of drawdowns on total firm debt could differ. The ultimate impact on investment is similarly unclear if firms use other margins, like cash holdings, to replace declines in credit. To address these concerns, we now turn to the firm financial data, which measures total firm borrowing from all sources, as well as investment. Using these data, we estimate firm level regressions of the form

$$\frac{D_{i,t+1} - D_{i,t-1}}{0.5\left(D_{i,t+1} + D_{i,t-1}\right)} = \alpha_m + \beta \sum_{j=1}^{J} \omega_{i,t-1}^{j} \left(\frac{\Delta Credit\ Line\ Usage_t^j}{Assets_{t-1}^j}\right) + \gamma\, X_{i,t-1} + u_{i,t+1}\ , \qquad (5.3)$$

where $D_{i,t}$ denotes total debt of firm i at time t and α_m is an industry fixed effect. The coefficient of interest is β related to the weighted sum of firms' exposure across all banks in our data. The associated interaction terms consist of the observed share of term borrowing to total debt $\omega_{i,t}^j = (Term\ Loan_{i,t}^j)/D_{i,t}$ between firm i and bank j and bank j's exposure to credit line drawdowns that

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[36]Under the Basel framework's standardized approach to calculating risk-based capital requirements, off-balance-sheet commitments are assigned credit conversion factors (CCFs) depending on maturity. Exposures with original maturity under one year receive a CCF of 20 percent, while exposures with maturities over one year receive a 50 percent CCF. If the commitment can be unconditionally canceled at any time, the exposure receives a zero percent CCF, or a zero-risk weighting.

[37]Bank credit line drawdowns in 2020:Q1 and their unused commitments before the outbreak of the pandemic are strongly positively correlated with a correlation coefficient of 0.67, showing that much of banks' differential exposure in regressions (5.2) originates from differences in prior commitments.

Table 5.2: COVID-19 − Total Debt & Capital Expenditures.

| | Δ Total Debt | | Capital Expenditures | | |
| | All | Small/Large | All | Small | Large |
	(i)	(ii)	(iii)	(iv)	(v)
Δ Credit Line Usage	-2.63***				
	(0.69)				
Δ Credit Line Usage × Small		-2.61***			
		(0.68)			
Δ Credit Line Usage × Large		0.97			
		(5.72)			
Δ Total Debt			0.06**	0.07***	-0.05
			(0.02)	(0.03)	(0.27)
Industry Fixed Effects	✓	✓	✓	✓	✓
Firm Controls	✓	✓	✓	✓	✓
Estimator	OLS	OLS	IV	IV	IV
First Stage F-Stat.			248	183	10.6
R-squared	0.08	0.08	0.03	0.05	0.03
Observations	3,164	3,164	2,717	1,166	1,550
Number of Firms	3,164	3,164	2,717	1,166	1,550
Number of Banks	28	28	28	24	28

Notes: Columns (i) and (ii) report estimation results for regressions (5.3). "Δ Credit Line Usage" denotes the estimation results for β. Column (ii) additionally distinguishes the effect by firm size, where a large firm is defined as one with total assets within the top 20 percent of the firm size distribution in 2019 and the indicator variable is included in the set of firm controls. Columns (iii)-(v) report estimation results for the instrumental variable regressions $CAPEX_{i,t+1}/Assets_{i,t-3} = \tilde{\alpha}_m + \tilde{\beta} \cdot 2\left(D_{i,t+1} - D_{i,t-1}\right) / \left(D_{i,t+1} + D_{i,t-1}\right) + \tilde{\gamma} X_{i,t-1} + \tilde{u}_{i,t+1}$, where $CAPEX_{i,t+1}$ denotes capital expenditures over the period $t-3$ to $t+1$, which is measured at an annual frequency within our data and we report the annualized coefficients. The variables $\sum_{j=1}^{J} \omega_{i,t-1}^{j}(\Delta Credit Line Usage_t^j)/Assets_{t-1}^j$ and $\sum_{j=1}^{J} \omega_{i,t-1}^{j}$ are used as instruments for $2\left(D_{i,t+1} - D_{i,t-1}\right) / \left(D_{i,t+1} + D_{i,t-1}\right)$. "Δ Total Debt" denotes the estimation results for $\tilde{\beta}$. $X_{i,t-1}$ includes $\sum_{j=1}^{J} \omega_{i,t-1}^{j}$ in columns (i) and (ii) and various firm indicators in 2019:Q2: net income, cash, tangible assets, total liabilities (all relative to total assets), firm size (natural log of total assets), and a binary variable that indicates whether a firm is publicly traded. Columns (iv) and (v) restrict the sample by firm size following the definition in column (ii). All specifications include industry fixed effects (two-digit NAICS code). Standard errors in parentheses are clustered by the bank with the largest term loan to firm i. Sample: 2019:Q2 - 2020:Q2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

is used thus far. The vector $X_{i,t}$ collects various firm controls, including the share of all observed term loans to total debt. We note that, unlike in (5.2), we are unable to include a firm fixed effect, as regression (5.3) covers only a single firm observation over the COVID-19 episode of interest. We estimate regression (5.3) for a change in firm debt from 2019:Q4 to 2020:Q2, the latest quarter with sufficient data on firm balance sheets, and report the results in Table 5.2. Column (i) shows that firms with higher term loan exposure were unable to substitute into alternative financing and experienced a contraction in their total debt. The magnitude of the effect is slightly lower but comparable to that of Table 5.1 column (iv), which controls for credit demand. Column (ii)

in Table 5.2 interacts firm term borrowing exposure with measures of firm size, showing that the estimated effects are strongest among SMEs.

In a second step, we employ an instrumental variable regression to investigate whether the reductions in firm debt also affected firm investment decisions. The first stage is given by regression (5.3), where firms' term loan exposure serves as an instrument to estimate the relation between firm debt and capital expenditures in the second stage. Columns (iii)-(v) in Table 5.2 report the results for all firms, and separately for small and large ones. The positive coefficient in column (iii) shows that reductions in total debt resulted in lower capital expenditures. The relation is again driven by smaller firms (column (iv)). In contrast, the estimated coefficient is not statistically different from zero for large firms (column (v)), in part because of the weak link between term loan exposure and total debt in the first stage. The effects are also economically sizable. The estimated coefficient in column (iv) implies a reduction in capital expenditures of around 23 cents for every $1 decline in firm debt, given the typical debt-to-asset ratio in the data.

These results show that firms were unwilling or unable to buffer the lending cuts by using their cash holdings or other resources, leading to a fall in firm investment. To isolate the response of firm cash holdings, we rerun the IV-regressions using the change in cash holdings as a dependent variable. The results, reported in Appendix Table F.10, show that SMEs partly reacted to the lending cuts by reducing their cash buffers. Without these adjustments, the fall in investment would have likely been even stronger. Taken together, our results show that the credit line drawdowns of large firms following the outbreak of COVID-19 crowded out term lending to SMEs, which in turn resulted in a decline of their total debt and investment.

6   Model

In this section we derive a theoretical model to study the general equilibrium implications of the credit line channel for firm borrowing and investment. We briefly summarize the key ingredients of the model, present the detailed structure, calibrate the model, and describe our findings.

6.1   Model Overview

Our model is designed to capture the main empirical patterns we document in Sections 3 - 5. To account for heterogeneity in credit line access in a tractable way, we allow for two types of firms: constrained firms that face a binding lower limit on their dividend payouts, and unconstrained firms that do not. Inspired by our empirical findings, the unconstrained firms borrow using credit lines, while the constrained firms borrow using term loans. Importantly, neither type of firm is literally *credit* constrained, and both types are able to obtain additional debt at the margin. Instead, all firms find an interior solution for debt that balances the benefits of debt against the increased covenant violation risk that comes with higher leverage. As a result, constrained primarily differ from unconstrained firms in that they use their debt margin exclusively to fund new investments, while unconstrained firms use debt to both invest and pay dividends at the margin.

To introduce credit lines in a parsimonious manner, we make the simple assumption that term loans are priced at the market-clearing rate, while credit lines always have a fixed, predetermined spread over the risk-free interest rate. Lenders face convex funding costs for providing either form of credit, implying that spreads increase with the quantity of credit demanded. As a result, borrowing by one type of firm (i.e., unconstrained firms drawing on credit lines) can crowd out credit supply for the other firms in the economy. Superficially, this assumption is at odds with our findings in Tables 5.1 and F.8 showing that spillovers are more consistent with constraints on capital requirements than on the direct need to obtain funds. However, we view this convex funding cost as a parsimonious approximation of a richer model in which banks face capital constraints but face convex costs of raising capital, where the degree of frictions on equity issuance in this richer model should map directly into the curvature of our model's convex funding cost.

To discipline debt accumulation, we impose debt-to-EBITDA covenants that are costly to violate following Greenwald (2019). Firms face idiosyncratic risk, leading them to reduce their probability of violation by keeping a precautionary buffer between their debt-to-EBITDA ratio and the covenant threshold. This specification matches the findings by Sufi (2009) that firms with credit lines are typically limited by their covenants rather than the amount of committed credit. Our model also realistically ensures that firms are not literally constrained from obtaining credit at equilibrium, but instead choose not to do so to reduce the expected costs of violation and distress.

Finally, to discipline the model to directly match our empirical findings, we introduce additional heterogeneity by banking sector, so that following an adverse shock, some constrained firms are more exposed to credit line drawdowns by unconstrained firms. This variation allows us to compute cross-sectional regression coefficients, and calibrate the key parameter governing crowding out in the model so that they exactly match their empirical equivalents.

We embed this financial structure into a macroeconomic environment with dividend smoothing incentives and capital adjustment costs. Following an adverse shock, firms must balance the drop in resources among three costly margins: reducing dividends, which impairs smoothing; reducing investment, which incurs adjustment costs; or increasing debt, which increases covenant violation risk. At equilibrium, the more flexible dividend margin for the unconstrained firms will make their credit demand more sensitive to spreads, a key driver of our results.

6.2 Model Structure

Bank-Firm Organization. Our model features a continuum of banking sectors, indexed by b, each of which lends to a continuum of firms, which are indexed by i.[38] Each firm has a financial type $f \in \{C, U\}$, corresponding to the constrained and unconstrained types, respectively. We assume that type U firms borrow from all banks, which is without loss of generality in our experiments.[39] Type C firms are matched to a single banking sector b. Given the firm's identify i, we

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[38]We use the terminology "banking sectors" instead of "banks" because each sector still prices loans competitively, whereas a single bank would have market power.

[39]All of our experiments either feature banks that behave symmetrically, or unconstrained firms that have access to credit lines at fixed spreads. In either case, unconstrained firm borrowing behavior does not depend on the banking

can match it to the type $f(i)$, and for constrained firms, its banking sector $b(i)$. For parsimony, we will use the single index $j \in \{U, (C, b)\}$, to denote a firm's type, which is either the single financial type U or the financial type-sector pair (C, b). Since firms behave symmetrically in equilibrium, we typically index firm variables by j instead of i. We note that the presence of bank heterogeneity through b is important only for our calibration strategy, since it gives us the needed cross-sectional variation to match our empirical regressions, and that our core results would hold in an environment with homogeneous banks.

Demographics and Preferences. Households exist in three types: unconstrained entrepreneurs (denoted U), constrained entrepreneurs (denoted C), and savers (denoted S). Each agent has trades a complete set of contracts among other agents of that type, but not across types, allowing aggregation and a representative agent for each type.[40]

An entrepreneur of type j has preferences over nondurable consumption $C_{j,t}$ given by

$$U_{j,t} = E_t \sum_{k=0}^{\infty} \beta_j^k \frac{C_{j,t}^{1-\psi_j}}{1 - \psi_j}. \tag{6.1}$$

The saver type has preferences over nondurable consumption $C_{S,t}$ and labor hours $N_{S,t}$ given by

$$U_{S,t} = E_t \sum_{k=0}^{\infty} \beta_S^k \left(C_{S,t} - \nu \frac{N_{S,t}^{1+\varphi}}{1 + \varphi} \right). \tag{6.2}$$

These risk-neutral saver preferences simplify our analysis, implying an exogenous risk-free rate and removing wealth effects on labor supply. Following Herreño (2020), we specify total labor supply as a CES aggregate of labor supplied to the constrained and unconstrained sectors, so that

$$N_{S,t} = \left(\chi_U N_{U,t}^{\frac{\epsilon_n - 1}{\epsilon_n}} + \chi_C \int_b N_{C,b,t}^{\frac{\epsilon_n - 1}{\epsilon_n}} d\Gamma_b(b) \right)^{\frac{\epsilon_n}{\epsilon_n - 1}}$$

where χ_U and χ_C are the measures of unconstrained and constrained firms, respectively, and Γ_b is the c.d.f over banking sector types b.

Productive Technology and Labor Demand. The production function for firm type j is

$$Y_{j,t} = Z_t \left(K_{j,t-1} \right)^{\alpha} N_{j,t}^{1-\alpha}$$

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sector they borrow from. Matching between unconstrained firms and banks is therefore irrelevant for equilibrium allocations, except through its effects on constrained firm spreads.

[40]This assumption, made for parsimony, implies that constrained firms in different banking sectors use the same constrained entrepreneur stochastic discount factor (SDF). This is largely irrelevant since the smoothing incentives created by the SDFs are much less important for constrained firms that cannot adjust their dividend margin. Specifying a separate representative entrepreneur for each type-banking sector pair yields quantitatively identical results.

where Z_t is aggregate productivity, $K_{j,t-1}$ is capital, and $N_{j,t}$ is labor. We assume that Z_t follows an AR(1) in logs

$$\log Z_t = (1 - \rho_Z) \log \bar{Z} + \rho_Z \log Z_{t-1} + \varepsilon_{Z,t}, \qquad \varepsilon_{Z,t} \sim N(0, \sigma_Z^2)$$

which represents the only source of aggregate risk in the economy.

Debt Contracts. Firms borrow from their respective banks using one-period risk-free debt, either in the form of term loans or credit lines, described in further detail below.

Corporate Bonds. An important feature of the data is that large firms typically borrow through non-bank channels like corporate bonds in normal times but draw heavily on the banking sector in crises. As a result, the proportional increase in bank loans can be much larger than the proportional increase in total credit during a crisis, explaining the very large growth in bank-firm credit in the 2020:Q1 Y14 data (23.7%) compared to the rise in total firm credit observed in the 2020:Q1 Flow of Funds (4.7%). To capture this, we allow each type of firm to have a fixed stock of corporate bonds B_j, which is not adjusted over the crisis episode we consider. The bond interest rate is equal to the risk-free short rate plus a fixed spread $\bar{s}_{bond}$

$$\underbrace{1 + r_{bond,t}}_{\text{gross bond rate}} = \underbrace{(1 + r_t)}_{\text{risk-free rate}} \underbrace{(1 + \bar{s}_{bond})}_{\text{spread}}.$$

Debt Covenants. We impose debt-to-EBITDA covenants — the most relevant financial covenants over our sample — by requiring each firm to pay a penalty if its total debt on both loans and bonds exceeds a multiple of smoothed EBITDA.[41] We define EBITDA $X_{j,t}$ as revenue net of the wage bill

$$X_{j,t} = P_{j,t} Y_{j,t} - w_t N_{j,t}.$$

Since covenants measure smoothed EBITDA, we define this quantity, denoted $X_{j,t}^*$ as

$$X_{j,t}^* = \rho X_{j,t} + (1 - \rho) \bar{\pi}^{-1} X_{j,t-1}^*.$$

where the economy's (constant) inflation rate $\bar{\pi}$ adjusts for the convention of computing the smoothed average in nominal terms.[42]

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[41] Greenwald (2019) shows that the most common form of covenant is an interest coverage covenant, which limits the ratio of interest payments to EBITDA, followed by the debt-to-EBITDA covenant described above. However, the very low interest rates observed entering and following the COVID-19 pandemic imply that interest coverage ratios were largely slack over this period, motivating our choice to focus on the debt-to-EBITDA covenants.

[42] Using a constant inflation rate rather than framing debt in real terms is irrelevant for our main mechanisms, but allows us to more realistically match the weights on lagged vs. current EBITDA in the moving average $X_{j,t}^*$, as well as the opportunity cost of holding cash in our model extension with cash presented in Appendix A.3.

We assume that a firm violates its covenant if

$$\bar{\pi}^{-1}(L_{j,t-1} + B_j) > \omega_{i,t}\theta X^*_{j,t} \tag{6.3}$$

where $L_{j,t}$ denotes bank debt, and $\omega_{i,t}$ is drawn i.i.d. from a distribution with mean unity and c.d.f. $\Gamma_{\omega,j}$. The shock $\omega_{i,t}$ represents idiosyncratic risks that unexpectedly shift a firm's EBITDA, potentially sending it into violation. Although we model this shock as affecting only the probability of violation, and not the firm's cash flows, we note that the scale of the individual firms is indeterminate, as each firm's problem is linear in its capital stock. As a result, similar implications would be found in a model featuring the capital quality shocks used in Bernanke et al. (1999) and much of the subsequent literature.[43] Rearranging (6.3), a firm of type j violates its covenant if and only if $\omega_{i,t} < \bar{\omega}_{j,t}$, for

$$\bar{\omega}_{j,t} = \frac{\bar{\pi}^{-1}(L_{j,t-1} + B_j)}{\theta X^*_{j,t}}$$

so that the probability of violation is equal to $\Gamma_{\omega,j}(\bar{\omega}_{j,t})$. As a result, the firm's probability of violation is smoothly increasing with its expected ratio of debt to EBITDA. For the violation penalty, we assume that violating firms pay a cost equal to fraction κ_j of their face value debt $\bar{\pi}^{-1}L_{j,t-1}$.

Firms. Each member of entrepreneur family j owns a firm, which uses capital and labor to produce, and maximizes the present value of dividends $D_{j,t}$ to entrepreneur type j,

$$V_{j,t} = \max D_{j,t} + E_t\Big[\Lambda_{j,t+1}V_{j,t+1}\Big], \tag{6.4}$$

where $\Lambda_{j,t+1}$ is the stochastic discount factor of the type j entrepreneur

$$\Lambda_{j,t+1} = \beta_j \left(\frac{C_{j,t+1}}{C_{j,t}}\right)^{-\psi_j}.$$

We note that the concave utility captured by ψ_j will provide firms an incentive to smooth dividends. The budget constraint for a firm of type j is

$$D_{j,t} \leq \underbrace{A_{j,t}}_{\text{cash on hand}} - \underbrace{Q_{j,t}K_{j,t}}_{\text{new capital}} + \underbrace{L_{j,t}}_{\text{new debt}} + B_j \tag{6.5}$$

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[43] For covenants written on non-smoothed EBITDA $X_{i,t}$, this model with capital quality shocks would be isomorphic to our baseline model, offering a simple microfoundation. When EBITDA is smoothed, a model with richer shocks would face the serious complications of tracking the history of past $\omega_{i,t}$ shocks for each firm. We therefore consider (6.3) as a parsimonious approximation to this richer model.

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where $D_{j,t}$ is dividends paid to the type j entrepreneur, $Q_{j,t}$ is the price of capital, $L_{j,t}$ is debt, and $A_{j,t}$ is cash on hand, defined by

$$A_{j,t} = \underbrace{\left[(1-\tau)x_{j,t} + \left(1 - (1-\tau)\delta\right)\bar{Q}_{j,t}\right]K_{j,t-1}}_{\text{income from capital}} - \underbrace{\bar{\pi}^{-1}\left(1 + (1-\tau)r_{j,t-1} + \kappa_j\Gamma_{\omega,j}(\bar{\omega}_{j,t})\right)L_{j,t-1}}_{\text{payment on existing loans}}$$
$$- \underbrace{\bar{\pi}^{-1}\left(1 + (1-\tau)r_{bond,t} + \kappa_j\Gamma_{\omega,j}(\bar{\omega}_{j,t})\right)B_j}_{\text{payment on existing bonds}}$$

(6.6)

where τ is the corporate tax rate, δ is the depreciation rate, $\bar{Q}_{j,t}$ is the resale price of old capital, $\bar{\pi}$ is the inflation rate, and $r_{j,t}$ is the interest rate on bank debt. The terms $\kappa_j\Gamma_{\omega,j}(\bar{\omega}_{j,t})$ represent the expected violation costs on the debt. This constraint also captures that both depreciation and interest payments on debt are tax-deductible by the firm.

Following Bernanke et al. (1999), we use a combination of firm exit and a non-negativity constraint on dividends for surviving firms, to generate a sector of constrained firms. We assume that firms of type j exogenously exit the market at rate $1 - \gamma_j$ each period, at which point they all their remaining resources as a dividend. For intraperiod timing, the exit occurs after production and repayment of debt, but before the firm's choices of new debt and capital. Non-exiting firms face a payout constraint implying that dividends cannot be negative. Aggregating over exiting and surviving firms, we obtain the minimum payout constraint

$$D_{j,t} \geq (1 - \gamma_j)A_{j,t}.$$

(6.7)

The key difference between the constrained and unconstrained sectors is the survival rate γ_j. We calibrate a lower survival rate for the constrained sector, causing (6.7) to bind at equilibrium.[44] We then calibrate a higher survival rate for the unconstrained sector, causing (6.7) to be slack at equilibrium, as unconstrained firms survive long enough to outgrow the constraint.[45]

For implications on firm debt and investment, we can combine (6.5) and (6.7) to obtain

$$Q_{j,t}K_{j,t} \leq \gamma_j A_{j,t} + L_{j,t} + B_j.$$

(6.8)

Equation (6.8) shows that when the payout constraint binds, firm investment moves one-for-one with new debt financing $L_{j,t}$. As a result, constrained firms have a much higher marginal propensity to invest out of debt than unconstrained firms at equilibrium.

Government Sector. The monetary authority targets and achieves a constant inflation rate $\bar{\pi}$, while the fiscal authority spends corporate tax revenues on government spending G_t that has no

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[44]Even though many firms pay zero dividends, the constrained entrepreneur who prices the firms' cash flows still has nonzero consumption at equilibrium due to the payouts of exiting firms, implying that paying zero dividends can be optimal even when entrepreneur utility is concave.

[45]At equilibrium, (6.7) therefore implies that the constrained sector devotes a larger share of its resources to payouts. We note, however, that *surviving* firms in this sector face a binding minimum of zero dividends, consistent with the empirical evidence on constrained firms in, e.g., Cloyne et al. (2019).

effect on household utility.

Entrepeneurs' Problems. The unconstrained and constrained entrepreneurs choose consumption $C_{j,t}$ to maximize (6.1) subject to the budget constraint $C_{j,t} \leq D_{j,t}$.

Firm's Problem. The sector-type j firm maximizes (6.4) subject to (6.5) and (6.8).

Bank's Problem. Each banking sector b contains a continuum of competitive banks that lend to a set of firms S_b, pays a convex funding cost, and returns the proceeds to the saver as a dividend $D_{S,b,t}$. The representative bank in sector b solves the problem

$$V_{b,t} = \max D_{S,b,t} + E_t \left[\Lambda_{S,t+1} V_{b,t+1} \right]$$

subject to

$$D_{S,b,t} \leq \underbrace{\bar{\pi}^{-1} (1 + r_{b,t-1}) L_{b,t-1} - L_{b,t}}_{\text{net lending income}} - \underbrace{\left(\frac{\eta}{1 + \zeta_L} \right) \left(\frac{L_{b,t}}{L_b} \right)^{1+\zeta_L}}_{\text{funding cost}}$$

where $L_{b,t}$ is total lending to all types of firms, and L_b is its steady state value.

Each bank's optimality condition with respect to $L_{b,t}$ is

$$(1 + r_{b,t}) = \underbrace{(1 + r_t)}_{\text{risk-free rate}} \underbrace{(1 + s_{b,t})}_{\text{spread}}, \qquad\qquad s_{b,t} \equiv \eta \left(\frac{L_{b,t}}{L_b} \right)^{\zeta_L}. \tag{6.9}$$

When unconstrained and constrained firms are mixed in the set S_b, (6.9) implies that borrowing by one type raises spreads for the other type, inducing crowding out.

In our version of the model in which unconstrained firms have access to credit lines, their interest rate is instead given by

$$(1 + \bar{r}_t) = (1 + r_t)(1 + \bar{s}) \tag{6.10}$$

where the constant $\bar{s}$ represents the predetermined spreads on credit lines facilities.

For the matching structure S_b, we assume that each firm is randomly matched with a banking sector b for their term lending. Absent credit lines, all banking sectors would therefore be symmetric. However, for the model with credit lines, we assume that undrawn credit line capacity, and therefore drawdowns, vary across banking sectors (see Appendix A.2 for details). This cross-sectional variation allows us to replicate our empirical regression (5.3) in our calibration below.

Saver's Problem. The saver chooses consumption $C_{S,t}$ and labor to each firm $N_{S,i,t}$, and risk-free bonds A_t to maximize (6.2) subject to the budget constraint

$$C_{S,t} \leq w_t N_{S,t} + \underbrace{\int_b D_{S,b,t}\, d\Gamma_b(b)}_{\text{bank dividends}} + \underbrace{(1+r_{t-1})\bar{\pi}^{-1}A_{t-1} - A_t}_{\text{risk-free bonds}} + \underbrace{\left(\bar{\pi}^{-1}(1+r_{bond,t-1}) - 1\right)B}_{\text{corporate bonds}} + \underbrace{T_{S,t}}_{\text{rebate}}.$$

where A_t denotes risk-free bonds in zero net supply, $B = B_U + B_C$ denotes total corporate bonds, and $T_{S,t}$ rebates the funding cost lump sum to savers to ensure that this cost has no effect on the economy's total supply of resources.

Capital Producers. Competitive producers create capital for each sector using

$$K_{j,t} = \Phi(i_{j,t})K_{j,t-1} + (1-\delta)K_{j,t-1}$$

where $i_{j,t} = I_{j,t}/K_{j,t-1}$ is the share of investment expenditures to existing capital in sector j. The capital producers buy existing capital at price $\bar{Q}_{j,t}$ and sell new capital at price $Q_{j,t}$. The capital producer's problem is therefore given by

$$\max_{i_{j,t}, K_{j,t-1}} Q_{j,t}\left[\Phi(i_{j,t})K_{j,t-1} + (1-\delta)K_{j,t-1}\right] - i_{j,t}K_{j,t-1} - \bar{Q}_{j,t}(1-\delta)K_{j,t-1}.$$

Final Good Producers. The intermediate goods from each sector are packaged by competitive final goods producers using the technology

$$Y_t = \left(\chi_U Y_{U,t}^{\frac{\epsilon_y-1}{\epsilon_y}} + \chi_C \int_b Y_{C,b,t}^{\frac{\epsilon_y-1}{\epsilon_y}}\, d\Gamma_b(b)\right)^{\frac{\epsilon_y}{\epsilon_y-1}}$$

where χ_U and χ_C are the measures of unconstrained and constrained firms, respectively. The price of the final good is normalized to unity, while the price of the intermediate good produced by firm j is $P_{j,t}$. Combining, the final good producer's problem is

$$\max_{Y_{j,t}} \left(\int_j Y_{j,t}^{\frac{\epsilon_y-1}{\epsilon_y}}\right)^{\frac{\epsilon_y}{\epsilon_y-1}} - \int_j P_{j,t} Y_{j,t}.$$

6.3 Equilibrium

Competitive equilibrium in this model is an allocation of endogenous states $(K_{j,t}, X^*_{j,t}, r_{j,t})$, policies $(N_{j,t}, i_{j,t}, n_{S,t}, A_t)$, and prices $(r_{j,t}, Q_{j,t}, \bar{Q}_{j,t}, r_t, P_{j,t}, , w_t)$, for $j \in \{U, (C,b)\}$, such that all agents' problems are optimized, and the markets for labor, capital goods, intermediate goods, final goods, and loans all clear. We provide (6.9) and (6.10) in the main text above, and relegate the remaining equilibrium conditions to Appendix A.1.

6.4 Calibration

Our quarterly calibration is displayed in Table 6.1. To stay parallel to our empirical findings, we match unconstrained firms to data on firms in the top 10% of the size distribution and constrained firms to data on firms in the bottom 90% of the size distribution.

Table 6.1: Parameter Values: Baseline Calibration (Quarterly)

Parameter	Name	Value	Internal	Target/Source
Preferences				
Entrepreneur Discount Factor (U)	β_U	0.992	N	See text
Entrepreneur Discount Factor (C)	β_C	0.992	N	See text
Entrepreneur RRA (U)	ψ_U	0.01	N	See text
Entrepreneur RRA (C)	ψ_C	0.01	N	See text
Saver Discount Factor	β_S	0.995	N	See text
Saver Labor Disutility	φ	0.5	N	Standard
Saver Labor Disutility	ν	0.536	Y	$N = 1$
Funding Cost				
Funding Cost Curvature	ζ_L	4.606	N	See text
Funding Cost Level	η	0.625%	Y	250bp Spread (Ann.)
Credit Line Spread	$\bar{s}$	0.625%	N	250bp Spread (Ann.)
Bond Spread	$\bar{s}_{bond}$	0.625%	N	250bp Spread (Ann.)
Financial				
Minimum Payout	$1 - \gamma_C$	0.02	N	See text
Minimum Payout	$1 - \gamma_U$	0.00	N	See text
Debt-to-EBITDA Limit	θ	15	N	Dealscan
Covenant Smoothing	ρ_L	0.75	N	4Q smoothing
Covenant Violation Fee (U)	κ_U	0.000272	N	See text
Covenant Violation Fee (C)	κ_C	0.0253	N	See text
Idio. EBITDA Vol. (U)	$\sigma_{\omega,U}$	0.660	N	See text
Idio. EBITDA Vol. (C)	$\sigma_{\omega,C}$	0.882	N	See text
Corporate Bonds (U)	B_U	2.944	N	See text
Corporate Bonds (C)	B_C	0	N	See text
Technology and Government				
Capital Share	α	0.33	N	Standard
Capital Adjustment Cost	ζ_K	0.5	N	Standard
Variety Elasticity	ϵ_y	6	N	See text
Labor Sector Elasticity	ϵ_n	6	N	See text
Unconstrained Share	a_U	0.752	N	Asset shares
Productivity	$\log \bar{Z}$	-0.715	Y	$Y = 1$
Productivity	ρ_Z	0.75	N	SPF Forecast
Corporate Tax Rate	τ	0.21	N	Standard
Inflation Rate	$\bar{\pi}$	1.005	N	2% inflation

Preferences. We assume that constrained and unconstrained entrepreneurs have the same preference parameters, so that the only difference between the two types come from their exit rates and credit technologies. Our preference specification requires calibration of two parameters, ψ and β, which control the entrepreneur dividend smoothing incentive and discount factor, respectively. In principle, we could calibrate these parameters to match the growth in payouts and credit over the COVID-19 episode. However, it turns out that our model is unable to fully match the decline in payouts or rise in credit for any feasible value of these parameters, given our remaining calibration strategy. We believe this is due to a strong precautionary motive to amass financial resources in uncertain times that is not present in our model's perfect foresight experiments. As a result, we instead choose each parameter to get as close as possible to the true moments within that parameter's feasible range. We note that in both cases our core results strengthen as we get closer to the true data moment, implying our results are likely conservative.

For the β parameters, our baseline calibration $\beta_U = \beta_C = 0.992$ falls at the upper end of the feasible range, implying that unconstrained firms are close to indifferent between borrowing and internal finance. For the ψ parameters, our baseline calibration $\psi_U = \psi_C = 0.01$ falls at the lower end of the feasible range, representing a weak but nonzero motivation to smooth payouts. This weak smoothing incentive reflects the various flexible financial margins available to large firms, such as repurchasing stock, spending out of cash, and issuing new non-bank debt. Further details about the calibration of these parameters, as well as sensitivity results using a range of parameters for both ψ and β, can be found in Appendix A.5.

For the saver, we set β_S to 0.995 to target a steady state annualized real discount rate of 2%. Saver labor disutility is calibrated so that φ, the inverse Frisch elasticity, is equal to 0.5, while the multiplicative term χ is set so that $N = 1$ in steady state.

Funding Cost. We next calibrate the funding cost parameters. The key parameter here is the elasticity ζ_L, which determines the strength of the crowding out effect. We calibrate this parameter so that estimating the cross-sectional regression (5.3) on constrained firms in the model delivers the exact coefficient implied by Table 5.2, yielding a value of 4.606. Full details on this procedure can be found in Appendix A.2. Once we have obtained our value for ζ_L, the coefficient η is set to ensure a steady-state spread of 250bp, while $\bar{s}$ and $\bar{s}_{bond}$ are chosen to ensure the same 250bp spread on credit lines and corporate bonds, respectively. For intuition, these parameters imply that a 1% increase in bank-firm credit raises credit spreads on bank loans by 11.7bp. Finally, for banking sector heterogeneity, we assume in our benchmark economy that log undrawn credit line balances (and therefore log credit line draws) are normally distributed across banking sectors with standard deviation $\sigma_b = 0.914$, calibrated to match the dispersion across banks in our sample.

Financial. We set the exit rate for unconstrained firms to zero, and set the exit rate for constrained firms to 2 percent, close to the value used in Bernanke et al. (1999). For the debt covenants, we choose a debt-to-EBITDA limit of 3.75 (annual), in line with the evidence in Greenwald (2019). We

set the smoothing parameter ρ_L to 0.75, consistent with covenants averaging EBITDA over four quarters. We parameterize the $\omega_{i,t}$ distribution as a lognormal, so that

$$\log \omega_{j,t} \sim N\left(-\frac{1}{2}\sigma_{\omega,j}^2, \sigma_{\omega,j}^2\right).$$

We calibrate the violation costs κ_U, κ_C and the idiosyncratic volatilities $\sigma_{\omega,U}$ and $\sigma_{\omega,C}$ for each sector to match four targets: the ratio of debt to capital in each sector, equal to 32% for the unconstrained sector, and 28% for the constrained sector; and the rate at which firms exceed the model debt-to-EBITDA threshold of 15 (quarterly), equal to 34% for the unconstrained sector and 32% for the constrained sector, rates similar to those found in Chodorow-Reich and Falato (2021). Experimenting with alternative values for these parameters had little influence on the results provided they yielded reasonable values for firm leverage. Finally, we assume that constrained firms do not issue corporate bonds ($B_C = 0$) and choose the level of bonds held by unconstrained firms (B_U) to ensure that 40% of bank debt is held by the unconstrained sector in steady state, consistent with the share held by the 10% largest firms in the Y14 data.[46]

Technology and Government. We set the capital share to $\alpha = 0.33$, a standard value. We parameterize the investment adjustment cost as

$$\Phi(i_{j,t}) = \phi_0 + \phi_1 \frac{i_{j,t}^{1-\zeta_K}}{1 - \zeta_K}.$$

We set $\zeta_K = 0.5$, which is in the typical range used by the literature and generates a reasonable investment response to a TFP decline. For the other coefficients, we set

$$\phi_0 = \delta\left(\frac{\zeta_K}{\zeta_K - 1}\right), \qquad\qquad \phi_1 = \delta^{\zeta_K}$$

to ensure that $\Phi(i) = i$ and $\Phi'(i) = 1$ in steady state.

For productivity, we set the average level $\bar{Z}$ so that steady-state output is normalized to unity, and the persistence $\phi_Z = 0.75$ to match the COVID-19 output scenario implied by the 2020:Q2 Survey of Professional Forecasters.[47] For the final goods aggregator, we choose a typical elasticity value $\epsilon_y = 6$ and symmetrically set $\epsilon_n = 6$. Experimentation has shown that these elasticity parameters are quantitatively unimportant for our main results. We choose the weights $a_U = 0.752$ and $a_C = 1 - a_U$, so that unconstrained firms account for 86% of sales in steady state, consistent with the share produced by the top 10% largest firms in our data.

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[46]Assuming that the constrained sector holds a positive amount of corporate bonds would require us to assign a larger share of bonds to the unconstrained sector, implying that the proportional growth of bank credit would be even larger over the COVID-19 episode. We therefore consider this to be a conservative calibration.

[47]This forecast projected 2020:Q2 output growth of -32.2 percent and 2020:Q3 output growth of 10.6 percent, both annualized. The implied quarterly log growth rates are -0.0972 and 0.0252, respectively. Under an AR(1) process beginning from steady state, these rates should correspond to $\varepsilon_{Z,t}$ and $-(1-\phi_Z)\varepsilon_{Z,t}$, respectively. Solving for two equations in two unknowns yields $\phi_Z = 0.741$.

For the government sector we set τ to 0.21, matching the US corporate tax rate, and the inflation rate to 1.005, implying an annual inflation rate of 2%.

6.5 Results

We study the response of the economy to an adverse TFP shock, designed to mimic the COVID-19 episode. Specifically, we compute perfect foresight paths in response to the unexpected shock $\varepsilon_Z = -0.0552$, a magnitude chosen to match the decline in GDP in 2020:Q2 in our benchmark ("Credit Lines") specification.[48] For each experiment, we apply an unexpected shock, then trace the nonlinear transition back to steady state.

To shed light on the specific contributions of the credit line channel and of cross-type spillovers more generally, we compare outcomes under two different models. To set a baseline, we consider a "Term Loans" economy in which all firms borrow in one period debt at the current market spread, given by (6.9). From this starting point, we move to a "Credit Lines" economy in which unconstrained firms borrow on credit lines at a fixed spread according to (6.10), while constrained firms borrow using term loans at the current market spread according to (6.9).

We begin with Figure 6.1, which shows the separate responses for unconstrained (U) and constrained (C) firms in each economy, where results for constrained firms aggregate over all possible banking sectors b. To set a baseline, we can begin with the Term Loans economy. Following a negative shock to TFP, profits fall for both types of firms. As a result, each firm is forced to adjust by balancing three costly margins: (i) cutting dividends (if unconstrained), subject to dividend smoothing motives; (ii) cutting investment, subject to adjustment costs; or (iii) increasing debt, subject to increasing expected violation costs.

Importantly, unconstrained firms are able to adjust their dividend, and do so heavily, reducing dividends 35.7% on impact. As a result, unconstrained firms are able to balance their budget constraint with only a very small decline in investment, with capital falling by only 0.18% over the 20Q following the shock. In contrast, constrained firms already face a binding minimum for dividends, and cannot voluntarily cut them further.[49] Instead, constrained firms are forced to cut investment, leading to a much larger drop in capital of 0.74% in the 20Q following the shock.

At equilibrium, unconstrained firms also have a more price sensitive demand for debt, since they would rather reduce their dividend than pay high spreads. In contrast, constrained firms have a more inelastic demand for debt, since decreasing debt requires them to cut investment — a more costly margin of adjustment. At equilibrium, this drives constrained firms to substantially increase their borrowing following the shock, while unconstrained firms cut their borrowing by a similar margin to avoid the increase in spreads. As a result, the Term Loans economy delivers a relative flow of credit from unconstrained toward constrained firms following the negative shock, generating the opposite pattern compared to what we observed empirically in Figure 5.2.

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[48]This decline in productivity is smaller than the 9.5 percent target drop in GDP due to the endogenous reduction in labor hours, which amplifies the impact of the shock on output.

[49]Constrained firm payouts still exhibit some decline due to the lower values of exiting firms.
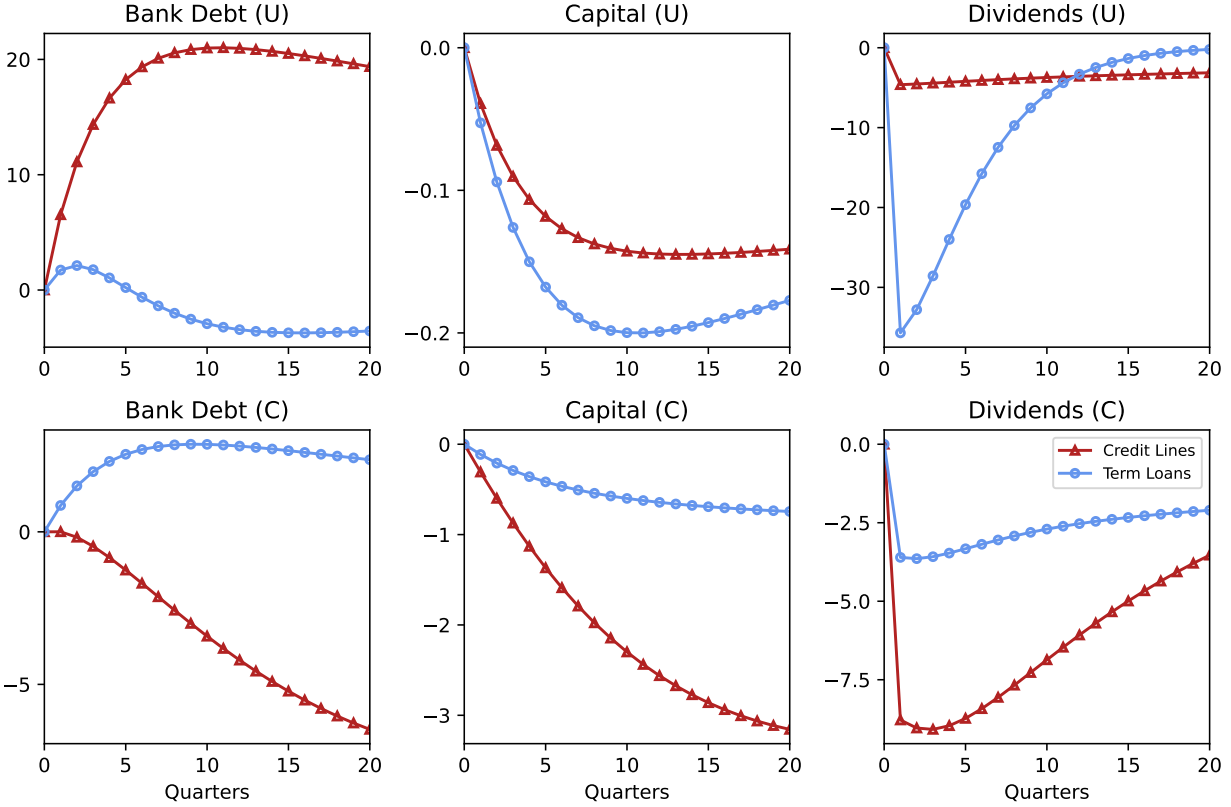
Figure 6.1: Responses by Type, Credit Line vs. Term Loan Economies

Notes: This figure plots the impulse response to the productivity shock $\varepsilon_Z = -0.0552$. Variable definitions are as follows: "Debt (U)" is $L_{U,t}$; "Capital (U)" is $K_{U,t}$; "Dividends (U)" is $D_{U,t}$; "Debt (C)" is $L_{C,t}$; "Capital (C)" is $K_{C,t}$; "Dividends (C)" is $D_{C,t}$. All variables are in logs and are displayed in percent.

From this baseline, we now turn to the Credit Lines economy, which allows unconstrained firms, and only unconstrained firms, to borrow through credit lines at a fixed spread $\bar{s}$. Figure 6.2 shows that unconstrained firms now increase, rather than decrease their borrowing. Although unconstrained firms are still highly sensitive to the price of credit, the fixed spread on credit lines insulates them from any rising spreads. As a result, unconstrained firms now primarily adjust by borrowing much more (at ex-post underpriced rates), while decreasing their dividend payout nearly four times less on impact (4.6%) as in the Term Loans economy. Unconstrained firm investment is largely unaffected by the inflow of credit, falling by a virtually identical 0.14% after 20Q, demonstrating these firms' low marginal propensities to invest.

This large increase in unconstrained firm borrowing puts pressure on bank balance sheets due to banks' convex funding costs, raising the spreads on new debt and crowding out borrowing by constrained firms. Constrained firm borrowing now falls where it had previously risen in the Term Loans economy. Since constrained firms are forced to offset this change in debt with reduced investment, their capital now falls by roughly five times more after 20Q compared to the Term Loans economy (3.16% vs. 0.74%).
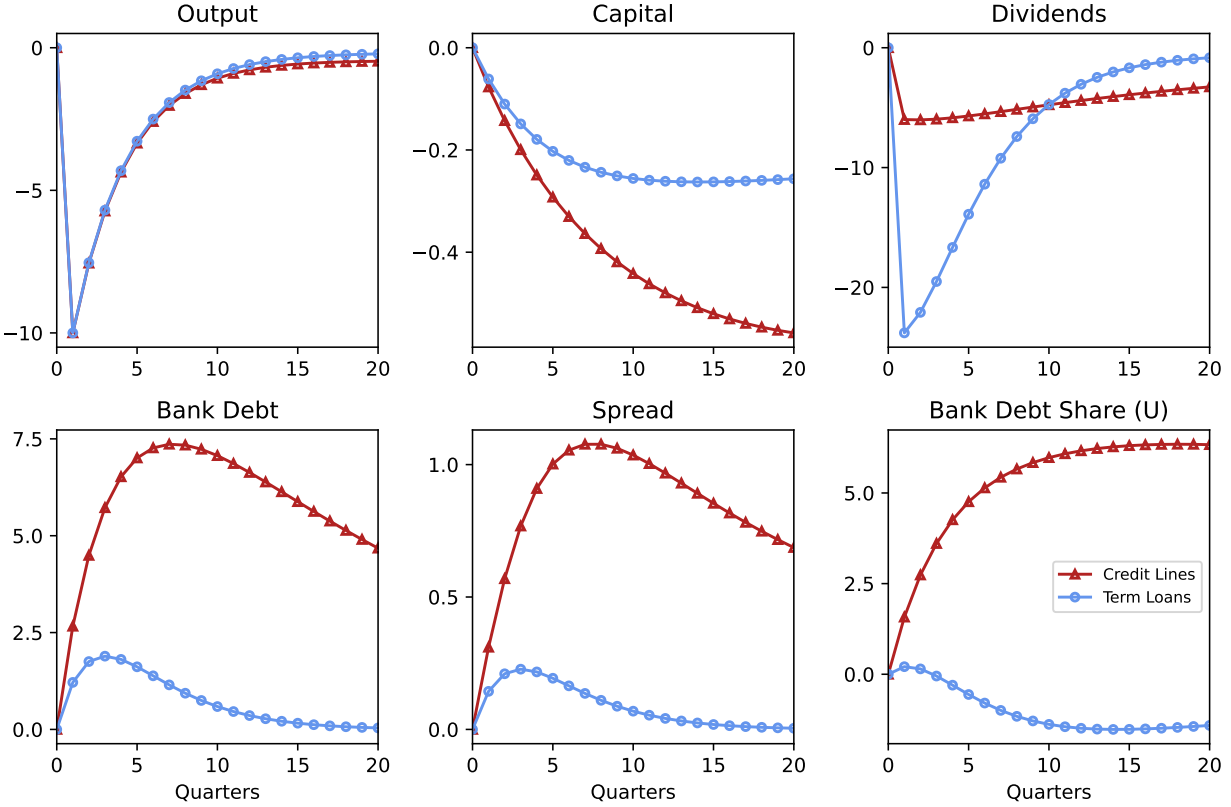
Figure 6.2: Aggregate Responses, Credit Line vs. Term Loan Economies

Notes: This figure plots the impulse response to the shock $\varepsilon_Z = -0.0552$. Variable definitions are as follows: "Output" is Y_t; "Capital" is $a_U K_{U,t} + a_C K_{C,t}$; "Dividends" is $a_U D_{U,t} + a_C D_{C,t}$; "Debt" is $a_U L_{U,t} + a_C L_{C,t}$; "Spread" is $s_{C,t}$, equivalently $s_{U,t}$ in the All Unconstrained and Term Loans economies; "Debt Share (U)" is $a_U L_{U,t}/(a_U L_{U,t} + a_C L_{C,t})$. The responses of "Spread" and "Debt Share (U)" are displayed in levels in percentage points, while the responses of the remaining variables are displayed in logs in percent.

To study the model's macroeconomic implications, Figure 6.2 displays aggregate series for our Term Loans and Credit Lines economies. The bottom left panel shows that the introduction of credit lines substantially increases aggregate debt issuance following the negative shock, peaking at over 7% growth, compared to just over 1% peak growth in the Term Loans economy. This occurs because unconstrained firms borrow much more than in the Term Loans economy, while the highly inelastic credit demand of the constrained firms does not fall sufficiently to offset it. Further, the bottom right panel shows that the addition of credit lines causes credit to flow toward unconstrained firms following the shock, matching our empirical findings. The model therefore implies that credit lines were central to both the aggregate and cross-sectional patterns observed following the outbreak of COVID-19 in Figures 1.2 and 5.2.

Despite boosting aggregate credit growth, credit lines worsen aggregate disinvestment following this shock. Figure 6.2 shows that aggregate capital falls by 0.56% in the 20Q following the shock, nearly tripling the decline in the Term Loan economy (0.26%). Although total credit issuance is larger compared to the Term Loans economy, credit lines lead to a large transfer of credit

from constrained firms, who have the highest marginal propensities to invest in the economy, toward unconstrained firms, who have the lowest. As a result, the decline in investment by the constrained firms is not offset by increases in investment by the unconstrained firms, who invest virtually none of these resources. Instead, larger credit flows to unconstrained firms primarily prop up aggregate dividends, which now fall by 6.0% on impact, a decline more than four times less than observed in the Term Loans economy (23.8%).

In summary, our model indicates that credit lines are structurally important to the patterns we observed in the data, and that it would be challenging to replicate the strong flow of bank credit toward unconstrained firms in bad times without them. But despite increasing overall lending to the corporate sector in bad times, the fact that these credit lines are overwhelmingly held by unconstrained firms can worsen the economic consequences of the recession. In closing, we note that while our model lacks any mechanism for this channel to influence output beyond excessive disinvestment, parallel phenomena could easily occur if constrained firms were forced to lose match surplus by laying off workers or destroy value by inefficiently liquidating in default. As a result, we believe our quantitative results could be substantially amplified by considering the impact of these countercyclical credit flows in richer models of firm production and investment.

6.6 Extensions

This section presents several extensions to our baseline model. We summarize the specifications and results here, and relegate additional details to Appendix A.

Cash Holdings. While our baseline model abstracts from cash holdings for parsimony, in practice cash represents an alternative store of precautionary resources that small firms could use to smooth investment following an adverse shock. In Appendix A.3, we extend the model to allow both types of firms to hold cash. Since our results largely hinge on how flexibly cash is used to smooth investment, we proxy for the precautionary value of cash using a simple specification in which cash enters the utility function of the firm, so that the curvature of utility from cash will map directly into the flexibility of this margin. Our estimates in Tables 5.2 and F.10 imply that small firms react to crowding out by reducing investment and cash holdings equally, we reproduce these IV estimates in the model, and calibrate the key curvature parameter so that the model exactly reproduces the equality of these IV coefficients.[50]

The impulse responses from the model with cash, shown in Figure A.1, illustrate that the ability of small firms to spend down cash does reduce the quantitative fall in investment, with the aggregate capital stock now falling by 0.39% rather than 0.56% after 20Q. However, this decline

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[50]Both of these IV coefficients are 0.12 in the model, reasonably close to the 0.07 found in the data. To the extent that the difference is due to a decline in resources used for productive purposes such as working capital, or represents future investment delayed by time-to-build frictions not present in the model, the model with cash will somewhat underestimate the strength of the credit line channel. To the extent that the difference is instead accounted for by decreased dividend payouts by small firms, the model with cash will somewhat overestimate the strength of the credit line channel. This distinction would require additional data and is left for future work.

in the capital stock in the Credit Lines economy with cash is still more than 60% larger than the corresponding 0.24% decline in the Term Loans economy with cash, due to the influence of the credit line channel. As a result, we conclude that the cash margin is quantitatively relevant, but insufficient to fundamentally alter our core results based on our empirically estimated elasticities.

Bond Market Interventions. Although our model correctly captures a strong rise in bank-firm credit following the outbreak of COVID-19, it predicts that bank-firm credit remains elevated for several years. In the data, however, much of the change in bank-firm credit following the COVID-19 outbreak reverted by 2020:Q3. We believe that this discrepancy is due to changes in bond market conditions, largely due to interventions by the Federal Reserve, that restored bond markets as a conduit for large firms to obtain credit at affordable rates. The resulting wave of bond issuance, which increased the stock of corporate bonds by over 7% between 2020:Q1 and 2020:Q2, substituted for bank credit, allowing large firms to pay down their credit line balances (Darmouni and Siani, 2020).

To investigate this in the context of our model, Appendix A.4 extends our framework to allow our previously static bond holdings B_j to instead follow a stochastic process. Figure A.2 shows that a modest increase in bond holdings can lead large firms to fully pay down their credit lines, as observed in the data. Importantly, this repayment removes pressure on bank balance sheets, alleviating the crowding out effects harming small firms and increasing aggregate investment. We therefore conclude that one of the most important effects of bond market interventions may have been indirect, by improving credit access to small firms that do not use corporate bonds, but nonetheless benefited from better access to bank term lending.

Still, aggregate capital and constrained firm debt remain persistently depressed in the Bond Market Shock extension relative to the Term Loans economy without credit lines. Since constrained firms have already disinvested substantially by the intervention date, capital adjustment costs and tighter covenant limits due to lower EBITDA prevent them from rapidly reaccumulating debt and investment. These results imply that ex-post bond market interventions may not be a complete solution, and may also explain why we find continuing crowding out effects in 2020:Q3, even after many credit lines have been repaid.

7 Conclusion

In this paper, we have argued that credit lines are central to the transmission of macroeconomic shocks to firm credit, at both the aggregate and cross-sectional levels. Using a highly granular data set, we are able to open the black box of U.S. bank balance sheets to show that unused credit line capacity is vast, but overwhelmingly concentrated among the largest, least financially constrained firms. As a result, while credit lines allow for a large expansion of aggregate firm credit following adverse shocks, they also crowd out credit to constrained firms in favor of unconstrained firms, potentially depressing firm investment. Our theoretical results show that the predetermined pric-

ing and terms of credit lines are key to this relative flow of credit, which would otherwise favor constrained firms following adverse shocks to productivity. Moreover, this cross-sectional pattern has important aggregate implications, severely worsening the drop in investment following negative shocks, despite increasing the aggregate flow of credit.

Looking ahead, our work has implications for both research and policy. On the research side, workhorse macroeconomic models, such as Bernanke et al. (1999) and Kiyotaki and Moore (1997), impose that the corporate sector is financially constrained, and is either unable to borrow further or dissuaded from doing so by rising credit spreads. Our data show instead that, in the aggregate, the corporate sector is far from constrained, with access to large amounts of unused credit under predetermined conditions. The financial accelerator mechanism therefore depends crucially on the allocation of credit across the firm distribution, and not merely on aggregate quantities. We encourage researchers to view the reallocation of credit between firms as of primary importance in driving macrofinancial dynamics.

For policymakers, these findings highlight the potential risks inherent in banks' undrawn credit lines. Because committed credit line balances have lower risk weights when undrawn, a sudden wave of drawdowns can tie up bank capital in bad times, leading banks to reduce other lending in an attempt to improve their capital ratios. Regulatory treatment of unused credit capacity should be carefully calibrated to account for these macroeconomic externalities. Our findings could also motivate various policy interventions during severe crises, particularly ones that directly provide credit to SMEs, such as the Paycheck Protection Program. Finally, the effects that we document could have been even stronger without some of the policy interventions put in place over the COVID-19 outbreak period. For example, we find that a quick recovery in the corporate bond market may have had strong indirect effects by improving bank credit conditions for small firms that do not issue corporate bonds. To the extent that this recovery was due to Federal Reserve interventions, our results provide additional motivation for such a policy.

Overall, the central role of credit lines in driving credit flows in bad times makes them a highly salient target for future academic and policy research.

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Internet Appendix

A   Model Appendix

A.1   Model Optimality Conditions

This section derives the optimality conditions that must hold at equilibrium.

Firms.   Define

$$\xi_{j,t} = \kappa_j \Gamma_{\omega,j}(\bar{\omega}_{j,t})$$

to be the expected violation cost per unit of debt, equal to the product of the cost and probability of violation. The optimality condition for capital for a firm of type j is

$$(1 + \mu_{j,t})Q_{j,t} = E_t \left\{ \Lambda_{j,t+1} \left[(1 + \gamma_j \mu_{j,t+1}) \left((1 - \tau)x_{j,t+1} + (1 - \delta)\bar{Q}_{t+1} \right) + \Psi_{j,t+1} \frac{\partial \bar{L}_{j,t+1}}{\partial K_{j,t}} \right] \right\}$$

where

$$\Psi_{j,t} = -(1 + \gamma \mu_{j,t})\bar{\pi}^{-1} L_{j,t} \frac{\partial \xi_{j,t}}{\partial \bar{L}_{j,t}} + E_t \left\{ \Lambda_{j,t+1} \Psi_{j,t+1} \frac{\partial \bar{L}_{j,t+1}}{\partial \bar{L}_{j,t}} \right\}$$

and where $\mu_{j,t}$ is the multiplier on the payout constraint (6.7). The condition for debt is

$$1 + \mu_{j,t} = E_t \left\{ \Lambda_{j,t+1} \bar{\pi}^{-1}(1 + \gamma \mu_{j,t+1}) \left[\left(1 + (1 - \tau)r_t + \xi_{j,t+1} \right) + \frac{\partial \xi_{j,t+1}}{\partial L_{j,t}} L_{j,t} \right] \right\}.$$

At equilibrium, we have $\mu_{j,t} > 0$ for constrained firms, and $\mu_{j,t} = 0$ for unconstrained firms.

Saver.   The saver's optimality condition for labor is

$$\nu N_{S,t}^{\varphi} = C_{S,t}^{-\psi_S} w_t$$

while the optimality condition for bonds is

$$1 + r_t = E_t \left[\Lambda_{S,t+1} \bar{\pi}^{-1} \right]^{-1}. \tag{A.1}$$

where $\Lambda_{S,t+1}$ is the saver's stochastic discount factor

$$\Lambda_{S,t+1} = \beta_S \left(\frac{C_{S,t+1}}{C_{S,t}} \right)^{-\psi_S}.$$

Capital Producer. The optimality condition for a capital producer of type j is

$$Q_{j,t} = \Phi'(i_{j,t})^{-1}$$

$$\bar{Q}_{j,t} = Q_{j,t} + \frac{Q_{j,t}\Phi(i_{j,t}) - i_{j,t}}{1-\delta}$$

where $i_{j,t} \equiv I_{j,t}/K_{j,t-1}$.[51]

Final Goods Producer. The optimality condition for the final goods producer's problem is

$$P_{j,t} = \left(\frac{Y_{j,t}}{Y_t}\right)^{-\frac{1}{\epsilon_y}}$$

which pins down the relative prices of the different types of goods.

Aggregation. Values for the total constrained sector are obtained by integrating over banks. For example, aggregate bank debt of constrained firms is computed as

$$L_{C,t} = \int_b L_{C,b,t}\, d\Gamma_b$$

where Γ_b is the c.d.f. of the bank distribution (see Appendix A.2). In practice we compute these integrals using three-point Gauss-Hermite quadrature.

Values for the aggregate economy are obtained by combining unconstrained and constrained firm values, weighting by their respective measures. For example, aggregate bank debt is computed as

$$L_t = \chi_U L_{U,t} + \chi_C L_{C,t}.$$

A.2 Matching our Cross-Sectional Estimates

To calibrate the strength of our spillover channel, we directly set the funding cost parameter ζ_L so that a simulated regression from our model with credit lines is able to exactly reproduce our cross-sectional estimate in Table 5.2. We first provide more detail on our parametrization of bank heterogeneity, before turning to the calibration itself.

Bank Heterogeneity. To implement bank heterogeneity, we assume that banks vary in their committed but undrawn credit line balances, so that undrawn balances at a bank in sector b, denoted $\hat{L}_b$ are given by

$$\log \hat{L}_b = \text{const} + e_b, \qquad e_b \sim N(0, \sigma_b^2). \tag{A.2}$$

We assume that when unconstrained firms draw their credit lines, they randomly draw credit among their open lines, implying that actual credit line draws at each bank are proportional to $\hat{L}_b$. The constant term in the equation (A.2) is only relevant if the unconstrained sector exhausts its undrawn balances. Motivated by our empirical results, we assume this never occurs, implying that the value of const is irrelevant. We

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[51]The difference between $Q_{j,t}$ and $\bar{Q}_{j,t}$ is second order and disappears in the linearized solution.

further assume that the distribution of used credit by unconstrained firms is equal across banking sector types in steady state.[52]

Combining these assumptions, the total used credit balances by unconstrained firms at a bank of type b are

$$L_{U,t}^b = \underbrace{L_U}_{\text{old credit}} + \underbrace{\left(\frac{\hat{L}_b}{\sum_b \hat{L}_b}\right)(L_{U,t} - L_U)}_{\text{new credit line draws}}$$

where L_U represents per-bank credit in the steady state, and the second term represents new credit line draws by unconstrained firms, equal to the relative share of draws at a bank of that type multiplied by the average amount drawn net of the steady state.

To calibrate bank heterogeneity, we choose σ_b to directly target the cross-sectional variance of the share of drawdowns, equivalent to

$$\text{Var}\left(\frac{\hat{L}_b}{\sum_b \hat{L}_b}\right)$$

in the model. Matching this to the corresponding moment in the data yields $\sigma_b = 0.914$.

For our Term Loans economy counterfactual, we assume that term lending is done symmetrically across banks, so that $L_{U,t}^b = L_{U,t}$.

Calibration Step. To compute the model-implied version of our regression coefficient, we first define drawdown exposure at bank b by

$$\text{Drawdown}_{b,t} = \frac{L_{U,t}^b - L_U^b}{L_U^b + L_C^b} = \frac{L_{U,t}^b - L_U}{L_U + L_C}$$

where variables without time indices indicate steady states, and where the second equality imposes that all banks are identical in steady state. We similarly compute the debt response of a constrained firm matched with bank b as

$$\text{DebtResponse}_{b,t} = \frac{L_{C,t}^b - L_C^b}{L_C^b} = \frac{L_{C,t}^b - L_C}{L_C}.$$

Given these variables, we compute the model regression coefficient as

$$\beta_t^{model} = \frac{\text{Cov}(\text{Drawdown}_{b,t}, \text{DebtResponse}_{b,t})}{\text{Var}(\text{Drawdown}_{b,t})}.$$

To compute these integrals (variances and covariances) in practice, we employ three-point Gauss-Hermite quadrature.[53] With β^{model} computed, we next match it to our empirical results. Because banks in our model

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[52]This assumption is without loss of generality, since what drives heterogeneity in our bank drawdown measures is large drawdowns relative to the existing stock of credit. As a result, a model with pre-existing heterogeneity in used credit, along with additional variation in drawdowns would deliver identical results.

[53]This number of quadrature nodes extends out 1.73 standard deviations in each direction. While we could computationally handle more quadrature nodes, this would incorporate more extreme observations beyond anything that actually occurred at the banks that make up our sample. Because some of the impacts of bank spillovers are nonlinear, this would potentially bias our results, undoing any benefit of additional quadrature accuracy.

have no assets besides loans to firms, we first adjust our target coefficient. Our original regression implies that a 1 percent shock to the balance sheet of the entire banking sector (as a share of total assets) should reduce bank credit to small (constrained) firms by 2.61 percent. Adjusting for the fact that firm credit is only 9.3 percent of bank assets implies that drawdowns equal to 1% of firm credit at bank b reduces lending to constrained firms by $0.093 \times 2.61\% = 0.243\%$ on impact. We calibrate $\zeta_L = 4.606$ to exactly match this cross-sectional regression coefficient.

As a final remark, note that we can also compute model equivalents to our IV estimates in Tables 5.2 and F.10. To match the IV regression of Table 5.2, we can compute the CapEx coefficient as

$$\beta_{capex,t}^{model} = \frac{\text{Cov}\left(\text{Drawdown}_{b,t}, (\sum_{j=-3}^{0} \text{CapEx}_{b,t-j})/\text{Assets}_{b,t-4}\right)}{\text{Cov}\left(\text{Drawdown}_{b,t}, \text{DebtResponse}_{b,t}\right)}$$

and for the IV regression of Table F.10, we can compute the cash coefficient as

$$\beta_{cash,t}^{model} = \frac{\text{Cov}\left(\text{Drawdown}_{b,t}, (\Delta_4 \text{Cash}_{b,t})/\text{Assets}_{b,t-4}\right)}{\text{Cov}\left(\text{Drawdown}_{b,t}, \text{DebtResponse}_{b,t}\right)}.$$

A.3 Extension: Model with Cash Holdings

An important feature missing from our benchmark model is that the constrained firms can use cash to insure themselves in the absence of credit lines. To address this, we now incorporate cash explicitly into the model. To parsimoniously include cash, we update the firm objective function (6.4) to read

$$V_{j,t} = \max D_{j,t} + \nu_{H,j} \frac{H_{j,t}^{1-\psi_H}}{1-\psi_H} + E_t\left[\Lambda_{j,t+1} V_{j,t+1}\right]$$

where $H_{j,t}$ is cash holdings. The concave benefit derived from cash can be interpreted as either a reduced form for investment opportunities the firm can take advantage of through its flexibility, or as a reduction of dividend risk that leads to a corresponding increase in the certainty equivalent dividend. In practice, this functional form represents a parsimonious way to introduce a cash margin with an arbitrary degree of flexibility via the curvature parameter ψ_H that we can use to match our cash regression in Table F.10.

With the addition of cash, several equations from Sections 6 and A.1 must be updated. We now have

$$D_{j,t} \leq \underbrace{A_{j,t-1}}_{\text{cash on hand}} - \underbrace{Q_{j,t}K_{j,t}}_{\text{new capital}} + \underbrace{L_{j,t}}_{\text{new debt}} - \underbrace{H_{j,t}}_{\text{new cash holdings}}$$

$$A_{j,t} = \underbrace{\left[(1-\tau)x_{j,t} + \left(1-(1-\tau)\delta\right)\bar{Q}_{j,t}\right]K_{j,t-1}}_{\text{income from capital}} - \underbrace{\bar{\pi}^{-1}\left(1 + (1-\tau)r_{j,t-1} + \kappa_j\Gamma_{\omega,j}(\bar{\omega}_{j,t})\right)L_{j,t-1}}_{\text{payment on existing loans}}$$

$$- \underbrace{\bar{\pi}^{-1}\left(1 + (1-\tau)r_{bond,t} + \kappa_j\Gamma_{\omega,j}(\bar{\omega}_{j,t})\right)B_j}_{\text{payment on existing bonds}} + \underbrace{\bar{\pi}^{-1}H_{j,t-1}}_{\text{start of period cash}}$$

in place of (6.5), (6.6), where we note that "cash on hand" $A_{j,t}$ is defined as in Section 6 and is distinct from

cash holdings $H_{j,t}$. We also obtain the new equilibrium condition

$$1 + \mu_{j,t} = \nu_{H,j}H_{j,t}^{-\nu_H} + \pi^{-1}E_t\left[(1 + \gamma_j\mu_{j,t+1})\Lambda_{j,t+1}\right]$$

that sets the current cost of holding \$1 of cash (\$1 plus the shadow cost of tightening the dividend constraint) equal to the marginal utility of holding cash plus the continuation value of cash in the next period (sum of direct value and shadow value of relaxing the dividend constraint).

Calibration. For this extension, we need to calibrate three new parameters: $\nu_{H,U}, \nu_{H,C}$ and ψ_H. We use a common curvature parameter for both constrained and unconstrained firms since we only have a single target to match, as the first stage in the IV cash regression in Table F.10 is not significant for large firms. This assumption is of little importance since the flexible dividend margin available to unconstrained firms makes their cash margin much less relevant to begin with. We calibrate $\nu_{A,U} = 0.00675$ to target a cash-to-assets ratio for large firms of 7.4%, and $\nu_{A,C} = 0.01291$ for small firms, to target a cash-to-assets ratio for small firms of 9.6%, with both targets obtained from the Y14 data. This stronger cash-holding motive for small firms generally have less access to credit lines is consistent with results from Sufi (2009) showing that credit lines are a substitute for cash as an insurance mechanism.

For the curvature parameter ψ_A, we calibrate this parameter so that the two IV regressions for CapEx (Table 5.2) and the change in cash (Table F.10) are equal, as we find in our empirical analysis, implying that the two margins are used equally following the COVID-19 shock. In the model, these coefficients are slightly larger than the data, each taking the value 0.12 instead of 0.07 as in our empirical estimates. Last, recalibrating our spillover parameter ζ_L to match our debt regression in the model with cash yields the value $\zeta_L = 2.490$.

Results. The results of the COVID-19 experiment for the Credit Lines and Term Loans economies with cash can be seen in Figure A.1. Overall, the plot shows a very similar qualitative story to the one described in Section 6.5. In the Term Loans economy with cash, credit once again flows from unconstrained firms, who prefer to cut dividends rather than pay high spreads, toward constrained firms who lack a flexible dividend margin. In the Credit Lines economy, unconstrained firms draw heavily on credit lines used mainly to increase payouts, while constrained firms are crowded out of bank credit and decrease investment, leading to a larger fall in the capital stock.

The main quantitative difference comes from the fact that constrained firms now split their adjustments between their investment and cash margins following a change in debt. As a result, while bank debt falls for constrained firms by a similar amount in the Credit Lines economy for both our cash extension and baseline model, constrained firms in the extended model absorb part of the change by spending cash, leading their cash holdings to fall by more than 4%. This cash financing allows firms in the cash extension to cut investment by less, with the capital stock declining by 2.00% at the 20Q horizon, compared to 3.16% in the Credit Lines economy without cash. Correspondingly, the aggregate capital stock falls by 0.39% after 20Q in the cash extension, compared to 0.56% in the baseline Credit Lines economy. While adding cash therefore quantitatively dampens our result, our finding that credit lines amplifies disinvestment is robust, as this 0.39% decline is still more than 60% larger than the 0.24% decline observed under the Term Loans economy with cash.
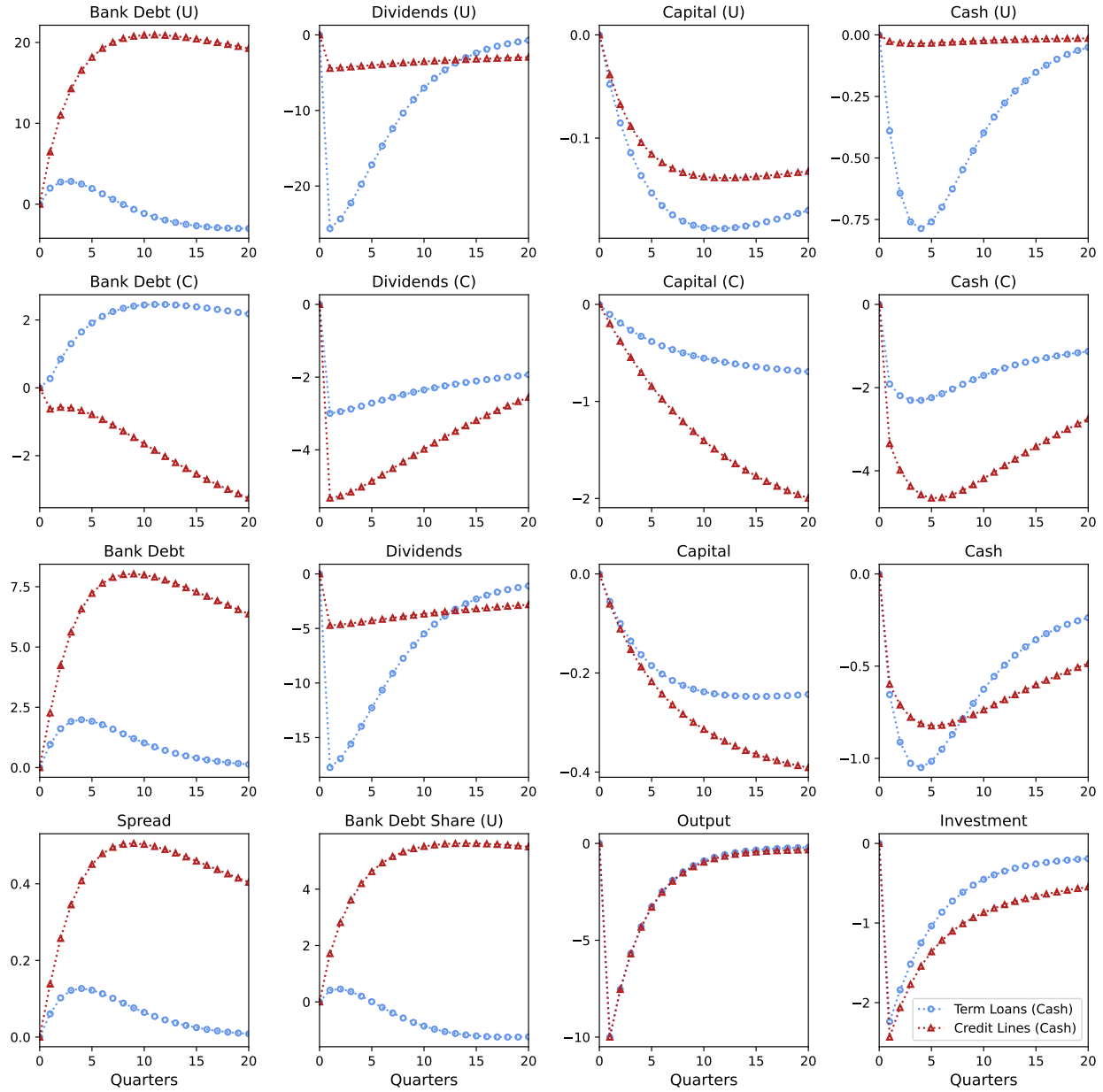
Figure A.1: Responses, Cash Extension Models

Notes: This figure plots the impulse response to the productivity shock $\varepsilon_Z = -0.0552$. Variable definitions are as follows. Bank Debt, Dividends, Capital, and Cash refer to the variables L, D, K, and A in the model, respectively. The label "(U)" or "(C)" refers to those variables for unconstrained and constrained firms, respectively, while variables without a label refer to the aggregate economy. All variables are in logs and are displayed in percent.

A.4   Extension: Bond Market Shock

While bank debt increases and remains elevated for a long period in our baseline model, in practice bank debt mostly reverted to its pre-COVID-19 value in 2020:Q3, with depository institution loans to the corporate sector declining from \$1.36T to \$1.12T between 2020:Q2 and 2020:Q3 (source: Flow of Funds). This repayment of bank debt follows an increase in corporate bond liabilities to the corporate sector from \$5.98T

to \$6.42T between 2020:Q1 and 2020:Q2, implying that firms may have used new corporate bond issuances as a cheaper substitute for their credit lines, allowing them to rapidly repay their lines.

While bond markets were jittery in 2020:Q1, following the initial outbreak, it is widely believed that the Federal Reserve interventions stabilized the markets, allowing firms to issue bonds at reasonable rates. To model the effect of this intervention in a parsimonious way, we extend our baseline model so that static bondholdings B_j instead follow the stochastic process

$$B_{j,t} = \exp(\Delta_{j,t})\bar{B}_j$$
$$\Delta_{j,t} = \rho_\Delta \Delta_{j,t-1} + \varepsilon_{\Delta,j,t}.$$

The only additional update needed is to explicitly clarify which of the B_j terms in the original model correspond to $B_{j,t}$ and which correspond to $B_{j,t-1}$. Equation (6.3) becomes

$$\bar{\pi}^{-1}(L_{j,t-1} + B_{j,t-1}) > \omega_{i,t}\theta X_{j,t}^*$$

while equations (6.5) and (6.6) become

$$D_{j,t} \leq \underbrace{A_{j,t}}_{\text{cash on hand}} - \underbrace{Q_{j,t}K_{j,t}}_{\text{new capital}} + \underbrace{L_{j,t}}_{\text{new debt}} + B_{j,t}$$

$$A_{j,t} = \underbrace{\left[(1-\tau)x_{j,t} + \left(1-(1-\tau)\delta\right)\bar{Q}_{j,t}\right]K_{j,t-1}}_{\text{income from capital}} - \underbrace{\bar{\pi}^{-1}\left(1+(1-\tau)r_{j,t-1} + \kappa_j\Gamma_{\omega,j}(\bar{\omega}_{j,t})\right)L_{j,t-1}}_{\text{payment on existing loans}}$$

$$- \underbrace{\bar{\pi}^{-1}\left(1+(1-\tau)r_{bond,t} + \kappa_j\Gamma_{\omega,j}(\bar{\omega}_{j,t})\right)B_{j,t-1}}_{\text{payment on existing bonds}}$$

and the saver budget constraint becomes

$$C_{S,t} \leq w_t N_{S,t} + \underbrace{\int_b D_{S,b,t}\,d\Gamma_b(b)}_{\text{bank dividends}} + \underbrace{(1+r_{t-1})\bar{\pi}^{-1}A_{t-1} - A_t}_{\text{risk-free bonds}} + \underbrace{\left(\bar{\pi}^{-1}(1+r_{bond,t-1})B_{t-1} - B_t\right)}_{\text{corporate bonds}} + \underbrace{T_{S,t}}_{\text{rebate}},$$

where $B_t = \sum_j B_{j,t}$.

To provide an example of the effects of such a policy, we repeat our baseline COVID-19 experiment for our Credit Lines model, but add in an unexpected shock $\varepsilon_{\Delta,U,t+8} = 0.02$ that occurs 8 quarters after the initial shock. We choose this timing because our bank credit series does not increase as rapidly as the actual bank credit series in the data, so this timing shows the effect of an intervention when bank credit is at its peak.

The resulting responses are plotted in Figure A.2. The figure displays the responses of the Credit Lines and Term Loans economies from the main text alongside the "Bond Market Shock" extension of the Credit Lines economy. The figure shows that an intervention leading large firms to increase their bond holdings by 2% is able to completely undo the rise in bank debt driven by credit lines, as bonds act as direct substitutes for bank debt. This policy is also effective at mitigating the decline in investment caused by credit lines, as lower bank balances by large firms reduce crowding out of credit to small firms, allowing them to better maintain their rates of capital investment. Overall, these results indicate that an important indirect effect of stabilizing bond markets may be improved credit access to small firms via these banking spillovers, even when those small firms do not themselves issue corporate bonds.
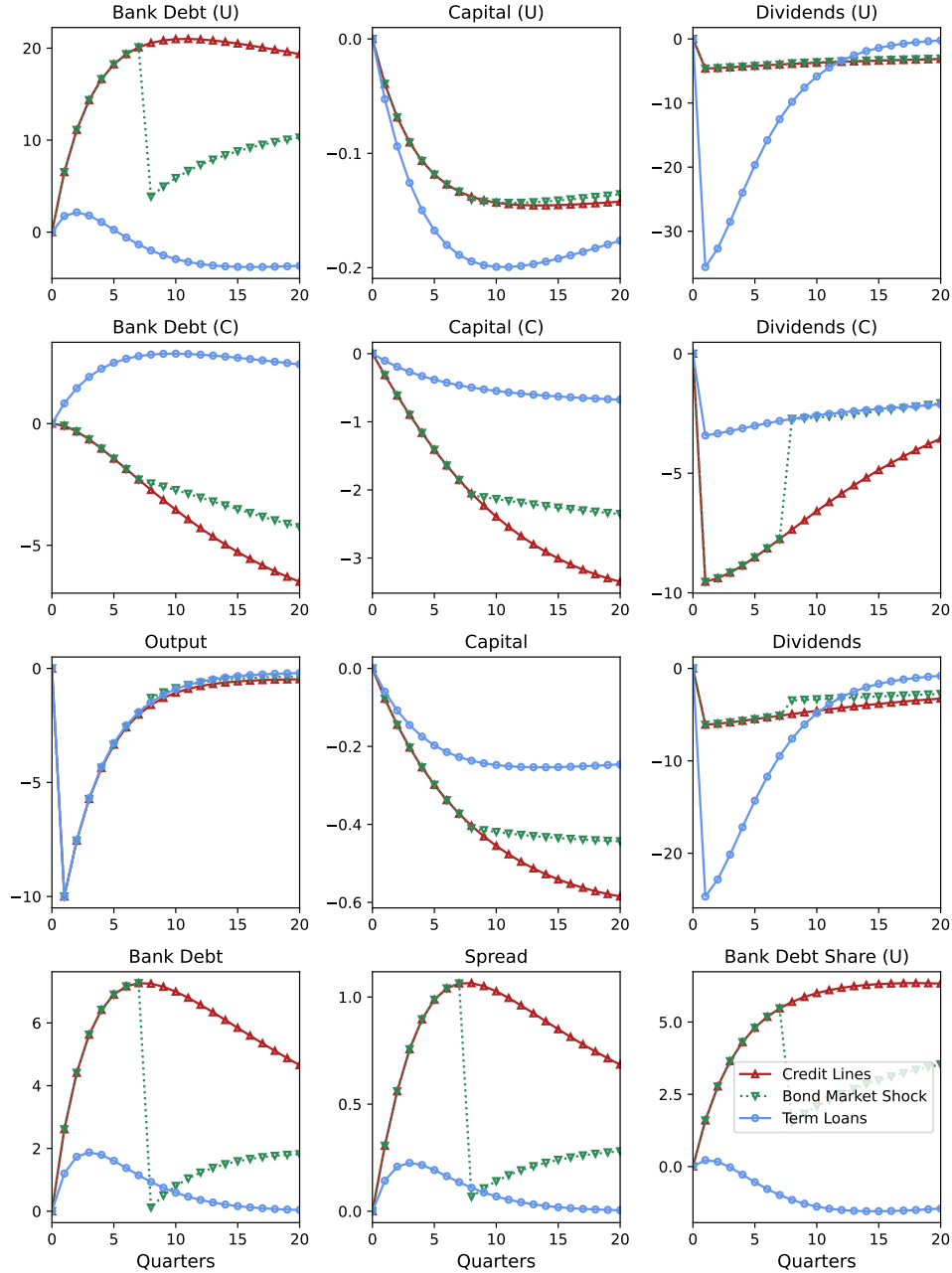
Figure A.2: Responses, Baseline Experiment vs. Bond Market Shock

Notes: The "Bond Market Shock" plots the impulse response to the productivity shock $\varepsilon_Z = -0.0552$ at time 0 in the Credit Lines economy, followed by the bond market shock $\varepsilon_{\Delta,U,t} = 0.02$ at time 8. The Credit Lines and Term Loans economies are defined as in Section 6.5. Variable definitions are as follows: "Debt (U)" is $L_{U,t}$; "Capital (U)" is $K_{U,t}$; "Dividends (U)" is $D_{U,t}$; "Debt (C)" is $L_{C,t}$; "Capital (C)" is $K_{C,t}$; "Dividends (C)" is $D_{C,t}$; "Output" is Y_t; "Capital" is $a_U K_{U,t} + a_C K_{C,t}$; "Dividends" is $a_U D_{U,t} + a_C D_{C,t}$; "Debt" is $a_U L_{U,t} + a_C L_{C,t}$; "Spread" is $s_{C,t}$, equivalently $s_{U,t}$ in the All Unconstrained and Term Loans economies; "Debt Share (U)" is $a_U L_{U,t}/(a_U L_{U,t} + a_C L_{C,t})$. All variables except for "Spread" and "Debt Share (U)" are in logs, and all variables are displayed in percent.

At the same time, the capital stock still remains depressed following the intervention relative to the Term Loans economy. While total bank debt returns to roughly its steady state level following the interven-

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tion, constrained firm bank debt is not restored, with elevated credit line debt by unconstrained firms making up the difference. This occurs because of two factors. First, following the initial disinvestment, capital adjustment frictions slow the reaccumulation of capital, even once spreads are reduced. Second, disinvestment in the initial period reduces the EBITDA of constrained firms, tightening these firms' covenants, and constraining debt accumulation in the recovery. These results indicate that ex-post bond market interventions may not be a complete cure for crowding out, and may help explain our results that crowding out persisted into at least 2020:Q3, even as many credit lines were repaid following the stabilization of the bond market.

A.5   Sensitivity Analysis

As mentioned in Section 6.4, there are two parameters that cannot exactly match their target moments: the preference for dividend smoothing for the entrepreneurs (ψ_U, ψ_C), and the discount rate of the entrepreneurs (β_U, β_C). Although this technically makes four parameters, we assume that $\psi_U = \psi_C$ and $\beta_U = \beta_C$, reducing the number of free parameters to two.

To understand the role that these parameters play, we perform a sensitivity analysis, varying each set of parameters (ψ or β), recalibrating the remaining parameters as in Section 6.4, and then recomputing our deterministic transition paths as in Figures 6.1 and 6.2. For the ψ parameter we consider a grid varying between our baseline of 0.01, a low smoothing incentive equivalent to an EIS of 100, and an upper limit of 1.0, a high smoothing incentive equivalent to an EIS of unity (log utility). For the β parameter we consider a grid varying between our baseline of 0.992, making the unconstrained entrepreneurs close to indifferent between financing the firm with debt and equity, and a lower limit of 0.985, implying a much stronger preference for debt finance.

Figure A.3 displays the results under the various parameters, with the left panels (a), (c), (e) showing variation over ψ and the right panels (b), (d), (f) showing variation over β. To better understand these results, we also reproduce the IRFs at the opposite ends of the ranges we consider: for $\psi_U = \psi_C = 1$ in Figures A.4, and for $\beta_U = \beta_C = 0.985$ in Figures A.5.

Beginning with variation over ψ, Figure A.3 panel (a) shows that while altering ψ has only a modest impact on capital growth at the 20Q horizon in the Credit Lines economy, it has a very large impact on the counterfactual growth in the Term Loans economy. For low values of ψ, introducing credit lines leads to a larger decline in capital in the Credit Lines economy relative to the Term Loans economy, whereas for high values of ψ the relationship is reversed.

These differences are explained by the firms' use of the dividend margin. Figure A.3c shows that when incentives to smooth dividends are weak (ψ is low), unconstrained firms primarily adjust along the dividend margin in the Term Loans economy. As a result, the additional borrowing in the Credit Lines economy largely substitutes for these dividend cuts, leaving unconstrained firm investment largely unchanged. In contrast, when the incentives to smooth dividends are strong (ψ is high), then unconstrained firms are unwilling to cut dividends much even in the Term Loans economy, with dividends falling by less than half as much as output on impact for $\psi = 1$. Under this calibration, unconstrained firms are primarily adjusting along the investment margin in Term Loans economy. Increasing unconstrained firm borrowing in the Credit Lines economy therefore substitutes largely for foregone investment, leading to a large overall rise in total investment and capital accumulation in the Credit Lines economy relative to the Term Loans economy.

Under any of our considered choices of ψ, including the baseline, the model underpredicts the actual decline in firm payouts following the COVID-19 outbreak, which is 45% in the data. As can be seen in Figure (c), dividend growth in the quarter following the shock is lowest for the smallest values of ψ, and
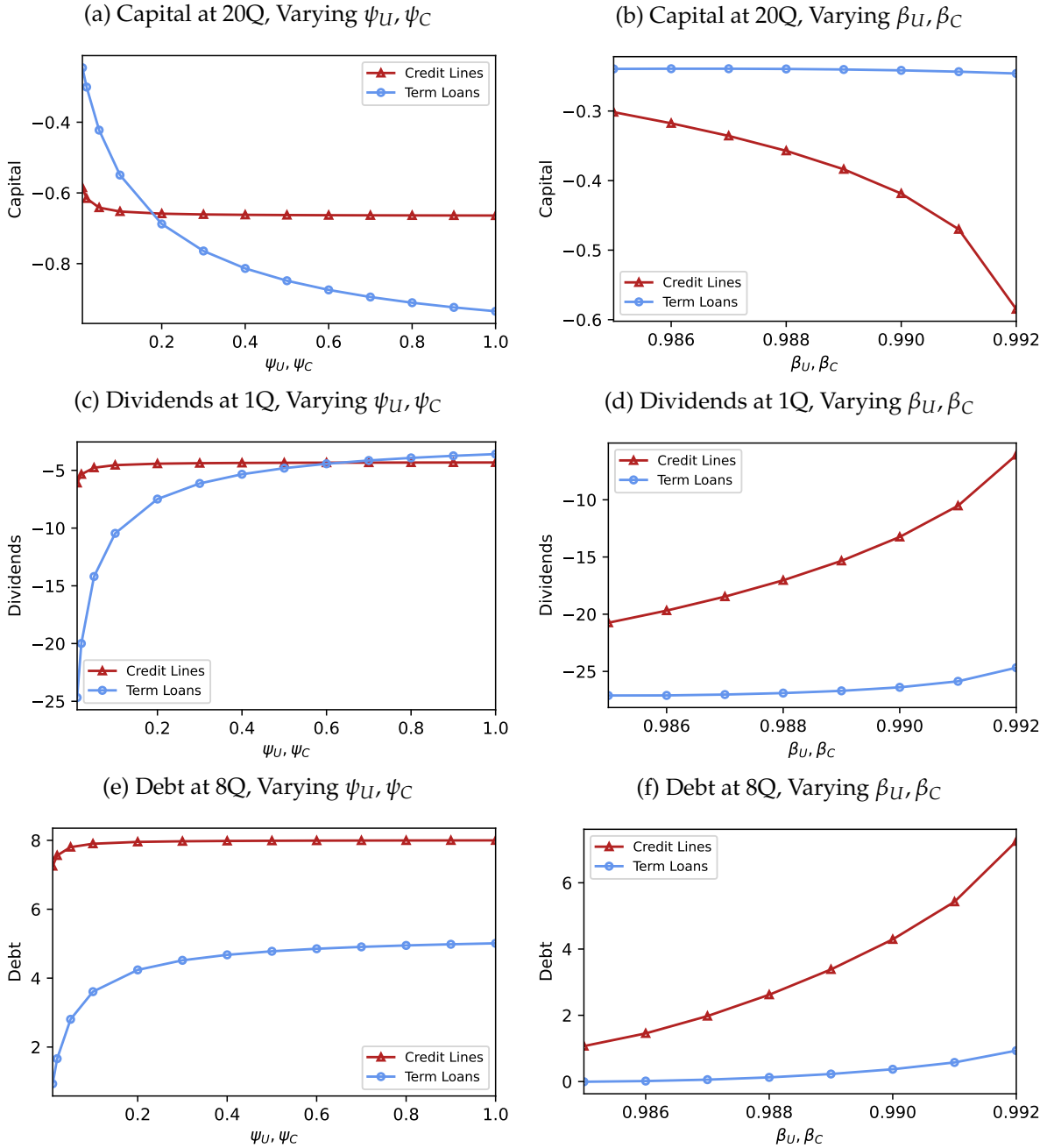
Figure A.3: Sensitivity Analysis: IRF Responses

Notes: The plot above displays selected results from deterministic transition paths calculated as in Figure 6.2, while varying the values of the ψ_U, ψ_C and β_U, β_C parameters. Panels (a), (c), and (e) display results setting both ψ_U and ψ_C to the value given on the x-axis, while Panels (b), (d), and (f) display results setting both β_U and β_C to the value given on the x-axis. For each set of parameter values, we recalibrate the remaining parameters as in Section 6.4. Each panel displays the response of a single variable at a single horizon.

increases from there, implying that our baseline calibration of $\psi_U = \psi_C = 0.01$ comes closest, delivering a decline of 6.00%. This value of ψ also comes closest to matching the fall in dividends among unconstrained (large) firms, which is 11.7% in our data, and 4.63% under our baseline calibration. As a result, we conclude

that our calibration with the most flexible dividend margin (lowest ψ) comes closest to fitting the data.

Beyond moment matching, a more general justification comes from a broader look at corporate finance. Although we refer to firm payouts as "dividends" in our model, this margin really reflects any financial transactions firms could use to avoid reducing either bank debt or investment. For example, while firms seem to make efforts to keep quarterly dividends smooth, they distribute a comparable amount of payouts in the form of repurchases, the size of which is both volatile and irregular. Firms that utilize repurchases should therefore be able to adjust their payouts very flexibly. Adjustments to nonbank credit or cash would similarly be reflected by this margin. Such transactions, potentially carry a small but nontrivial cost in terms of fees, flexibility, or potential negative signals to the market, are an alternative interpretation of the dividend margin in our model, and point to a low but nonzero value for ψ.

Next, we can consider variation over β. Panel (b) shows that as the calibrated value for β_U and β_C decreases, the difference in capital accumulation between the Credit Lines and Term Loans economies at the 20Q horizon falls sharply. The mechanism behind this result is a bit subtle, and largely occurs through indirect effects on how other parameters are internally calibrated. Recall that the violation cost κ_j is calibrated to match the leverage ratio of each type of firm in the data. When β_U falls, the value of borrowing from the entrepreneurs' perspective rises, and so the model must calibrate a higher violation cost κ_U to keep unconstrained leverage at the target level. With higher violation costs, unconstrained firms' capital structure decisions are effectively driven more by the marginal cost of increased violation costs $\xi_{U,t}$ and less by the direct cost of credit $r_{j,t}$. Because of this, introducing credit lines have a much smaller impact on firm borrowing, since their main benefit (lower rates) are less important in a model where lending is primarily driven by covenant concerns.

This mechanism implies that aggregate borrowing, shown at the 8Q horizon in Panel (f), is strongly increasing in β_U in the Credit Lines economy. Since our model underpredicts the actual growth of bank-firm debt, which was 23.7% larger in 2020:Q1 than in 2019:Q4, we come closest by choosing a value of β_U as high as possible, yielding a peak increase in bank-firm credit of around 7%. This calibration implies that unconstrained firms are close to indifferent between financing with bank debt or internally in steady state, which we believe is reasonable considering that firms have access to many forms of nonbank credit in practice.
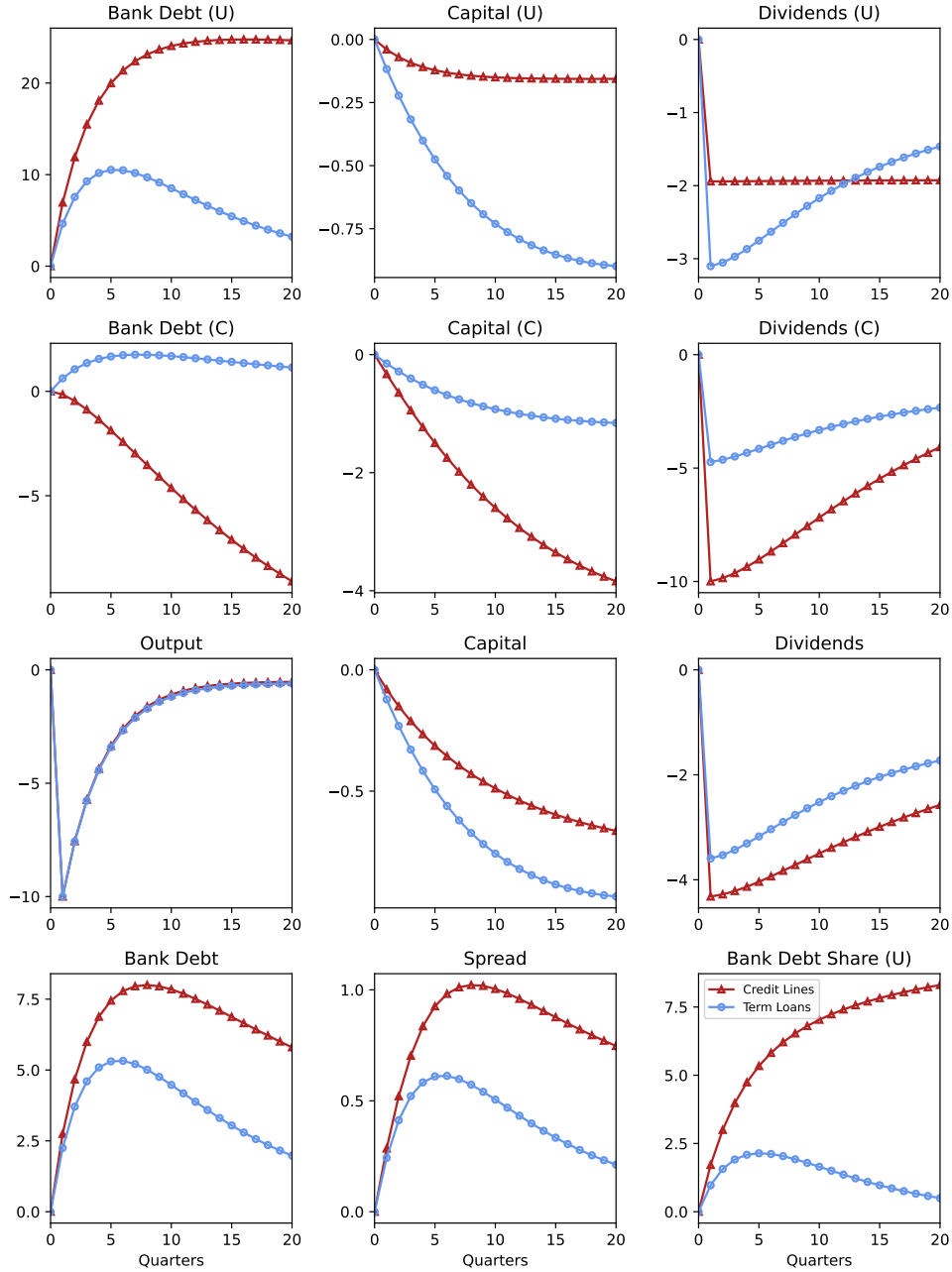
Figure A.4: Responses: Credit Line vs. Term Loan Economies, $\psi_U = \psi_C = 1$

Notes: This figure plots the impulse response to the productivity shock $\varepsilon_Z = -0.0552$. Variable definitions are as follows: "Debt (U)" is $L_{U,t}$; "Capital (U)" is $K_{U,t}$; "Dividends (U)" is $D_{U,t}$; "Debt (C)" is $L_{C,t}$; "Capital (C)" is $K_{C,t}$; "Dividends (C)" is $D_{C,t}$; "Output" is Y_t; "Capital" is $a_U K_{U,t} + a_C K_{C,t}$; "Dividends" is $a_U D_{U,t} + a_C D_{C,t}$; "Debt" is $a_U L_{U,t} + a_C L_{C,t}$; "Spread" is $s_{C,t}$, equivalently $s_{U,t}$ in the All Unconstrained and Term Loans economies; "Debt Share (U)" is $a_U L_{U,t}/(a_U L_{U,t} + a_C L_{C,t})$. All variables except for "Spread" and "Debt Share (U)" are in logs, and all variables are displayed in percent.

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Figure A.5: Responses by Type, Credit Line vs. Term Loan Economies, $\beta_U = \beta_C = 0.985$

Notes: This figure plots the impulse response to the productivity shock $\varepsilon_Z = -0.0552$. Variable definitions are as follows: "Debt (U)" is $L_{U,t}$; "Capital (U)" is $K_{U,t}$; "Dividends (U)" is $D_{U,t}$; "Debt (C)" is $L_{C,t}$; "Capital (C)" is $K_{C,t}$; "Dividends (C)" is $D_{C,t}$; "Output" is Y_t; "Capital" is $a_U K_{U,t} + a_C K_{C,t}$; "Dividends" is $a_U D_{U,t} + a_C D_{C,t}$; "Debt" is $a_U L_{U,t} + a_C L_{C,t}$; "Spread" is $s_{C,t}$, equivalently $s_{U,t}$ in the All Unconstrained and Term Loans economies; "Debt Share (U)" is $a_U L_{U,t}/(a_U L_{U,t} + a_C L_{C,t})$. All variables except for "Spread" and "Debt Share (U)" are in logs, and all variables are displayed in percent.
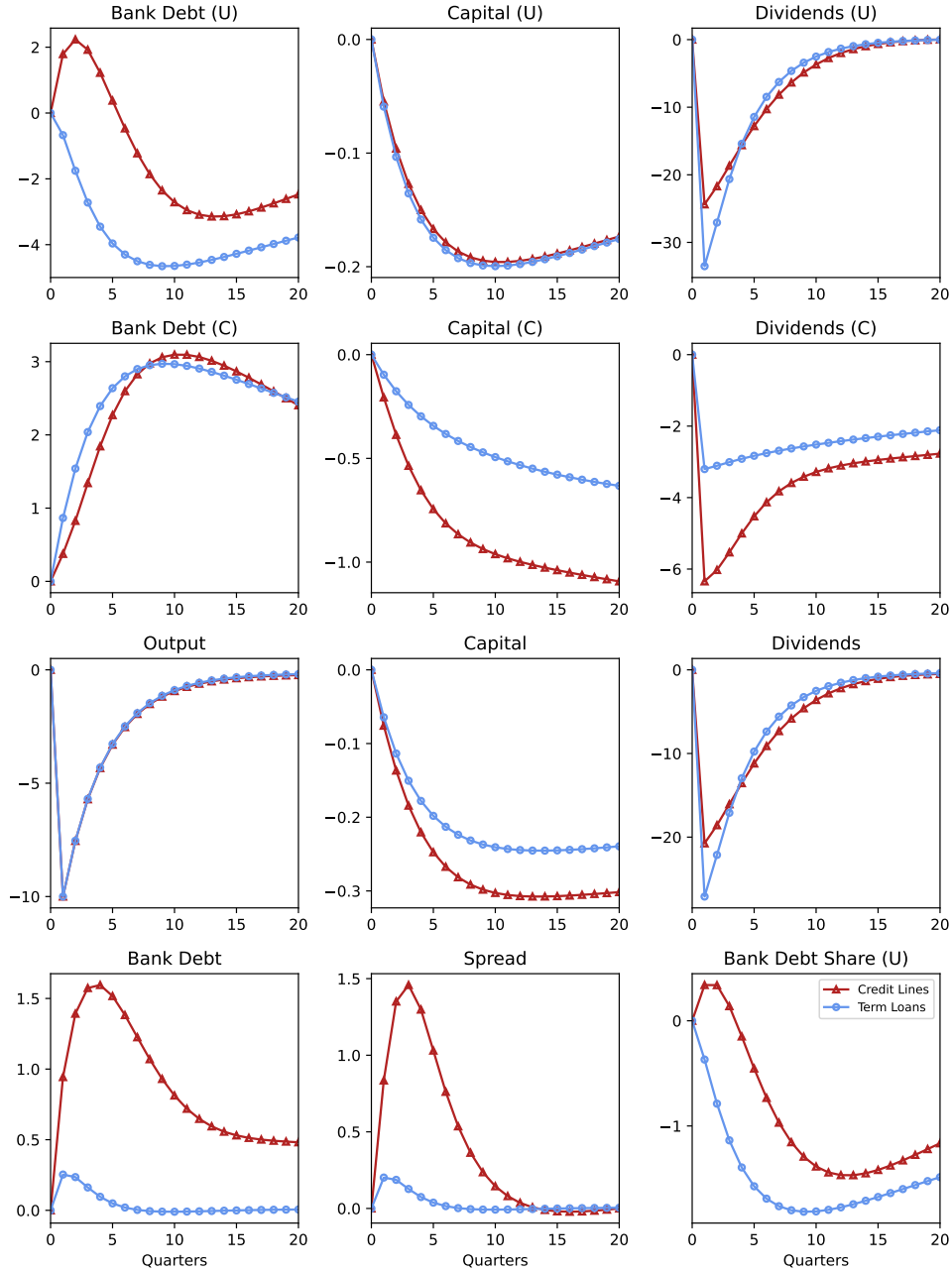
B   Aggregate Responses to Monetary Policy Shocks

B.1   Further Evidence and Robustness

In this Appendix, we describe the estimations of the impulse responses in Figure 1.1 and provide further evidence and robustness checks. Given an identified series of monetary policy shocks, we run several local projections. Let y_t be the outcome variable at time t, e.g., (log) real credit or the federal funds rate. Following Jordà (2005), we estimate

$$y_{t+h} - y_{t-1} = \alpha^h + \beta^h \cdot \epsilon_t^{MP} + \gamma^h X_{t-1} + u_t^h \ , \tag{B.1}$$

where $h = 0, 1, ..., 48$. The estimated coefficients β^h give the percentage (point) change at horizon h to a 100-basis-point monetary policy shock ϵ_t^{MP}. X_{t-1} denotes a vector of controls. The specification in Figure 1.1 includes one year of lagged values of the monetary policy shock and one year of lagged values of the one-month change in the respective dependent variable. We check and confirm the robustness of the results to the choice of the lag length, as explained below.

Figure B.1 replicates the results in Figure 1.1 and additionally shows the responses for the "Federal Funds Rate," "Industrial Production," "Consumer Price Index," "Deposits," "Loans & Leases," and "Securities."[54] Importantly, in response to a monetary policy tightening, deposits flow out of the banking sector (Drechsler et al., 2017), in contrast to the behavior of deposits around the outbreak of COVID-19 (see Figure 1.2). This deposit outflow may be an additional source of bank credit supply contraction after a monetary policy tightening.

In Figures B.2-B.4, we provide further evidence. First, we test whether the findings depend on the choice of the lag length. Figure B.2 shows that the results remain much the same whether any, one year, or two years worth of controls of the shocks and the one-month change in the dependent variable are added. Second, we check whether the responses depend on the identification approach. Using the high-frequency identification approach (see e.g., Gürkaynak et al., 2005), Figure B.3 shows impulse responses to the shock series from Nakamura and Steinsson (2018) for the sample 1994:M1 - 2007:M12.[55] Apart from the CPI response, which shows a price puzzle initially, the remaining responses are similar compared with the ones in Figure 1.1.

Last, we investigate the response of total firm credit using quarterly data from the Flow of Funds (see Appendix Table C.1 for details about the data). Figure B.4 shows the impulse responses of various types of credit to nonfinancial businesses following a contractionary monetary policy shock.[56] Nonfinancial businesses can be separated into corporate and noncorporate categories. Nonfinancial noncorporate businesses (e.g., sole proprietorships and limited partnerships) only borrow using loans, and panel (b) shows that such credit contracts to a monetary policy tightening. These loans likely consist of term loans, so this response is consistent with the results in Section 5.1. In contrast, corporate loans, which include more credit lines, increase as shown in panel (c), again in line with the findings in Section 5.1. In addition, corporate debt

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[54]We do not include controls in the equation for the federal funds rate, since the rate responds on impact to the shock. The responses of the federal funds rate are largely unaffected by including additional controls.

[55]The policy news shock series by Nakamura and Steinsson (2018) is the first principal component across surprise changes of five futures contracts around scheduled policy announcements: the one with respect to the Fed funds rate immediately following a meeting by the Federal Open Market Committee (FOMC), the expected federal funds rate immediately following the next FOMC meeting, and expected three-month eurodollar interest rates at horizons of two, three, and four quarters. These are the same contracts that are used in the rotations by Gürkaynak et al. (2005). The scale of the shock series is arbitrary, and we rescale it such that its effect on the two-year government bond yield is one and therefore comparable to the results in Section 5.1.

[56]All local projections include one year of lagged values of the monetary policy shock and the one-month change in the respective dependent variable as controls.

securities also rise after an initial dip (see panel (d)). Commercial paper and corporate bonds, which are both part of corporate debt securities, also increase as shown in panels (e) and (f), after a drop over the first quarters for corporate bonds. Taking all corporate and noncorporate loans and debt securities together, total firm credit also rises following a monetary policy tightening, as shown in panel (a).

Figure B.1: Impulse Responses to a Monetary Policy Shock.

Notes: Impulse responses to a 1 percentage point contractionary monetary policy shock based on the identification approach by Romer and Romer (2004). The shock series is taken from Coibion et al. (2017) and the remaining data are obtained from St. Louis Fed's FRED database. The credit series are based on the H.8 releases for U.S. commercial banks from the Board of Governors of the Federal Reserve (see Appendix Table C.1 for details about the data). Sample: 1970:M1 - 2007:M12. 95 and 68 percent confidence bands are shown using Newey and West (1987) standard errors.

Figure B.2: Impulse Responses to a Monetary Policy Shock − Lag Length.

Notes: Impulse responses to a 1 percentage point contractionary monetary policy shock based on the identification approach by Romer and Romer (2004) and the local projection specification in (B.1). The estimations differ according to the controls that are included (no controls, one year, or two years). The shock series is taken from Coibion et al. (2017) and the remaining data are obtained from St. Louis Fed's FRED database (see Appendix Table C.1 for details about the data). Sample: 1970:M1 - 2007:M12, the shocks in 1980:M4 - 1980:M6 and 1980:M9 - 1980:M11 are excluded following Coibion (2012). 95 and 68 percent confidence bands are shown using Newey and West (1987) standard errors.
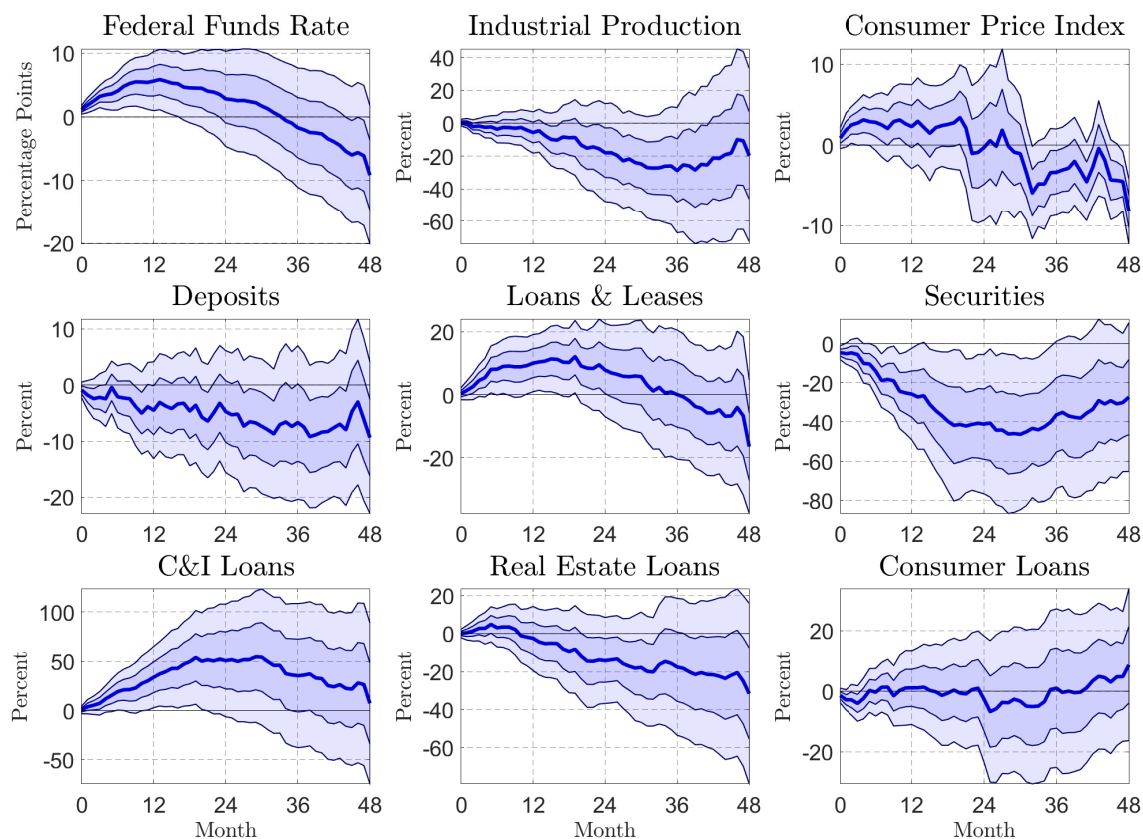
Figure B.3: Impulse Responses to a Monetary Policy Shock − High-Frequency Surprises.

Notes: Impulse responses to a 1 percentage point contractionary monetary policy shock based on the local projection specification in (B.1). The shock series follows the computations in Nakamura and Steinsson (2018) (see also footnote 55) and the remaining series are obtained from St. Louis Fed's FRED database (see Appendix Table C.1 for details about the data). All specifications exclude additional controls, apart from the estimations for industrial production and the consumer price index, for which two years worth of the shocks and the one-month change in the respective dependent variable are included. Sample: 1994:M1 - 2007:M12. 95 and 68 percent confidence bands are shown using Newey and West (1987) standard errors.
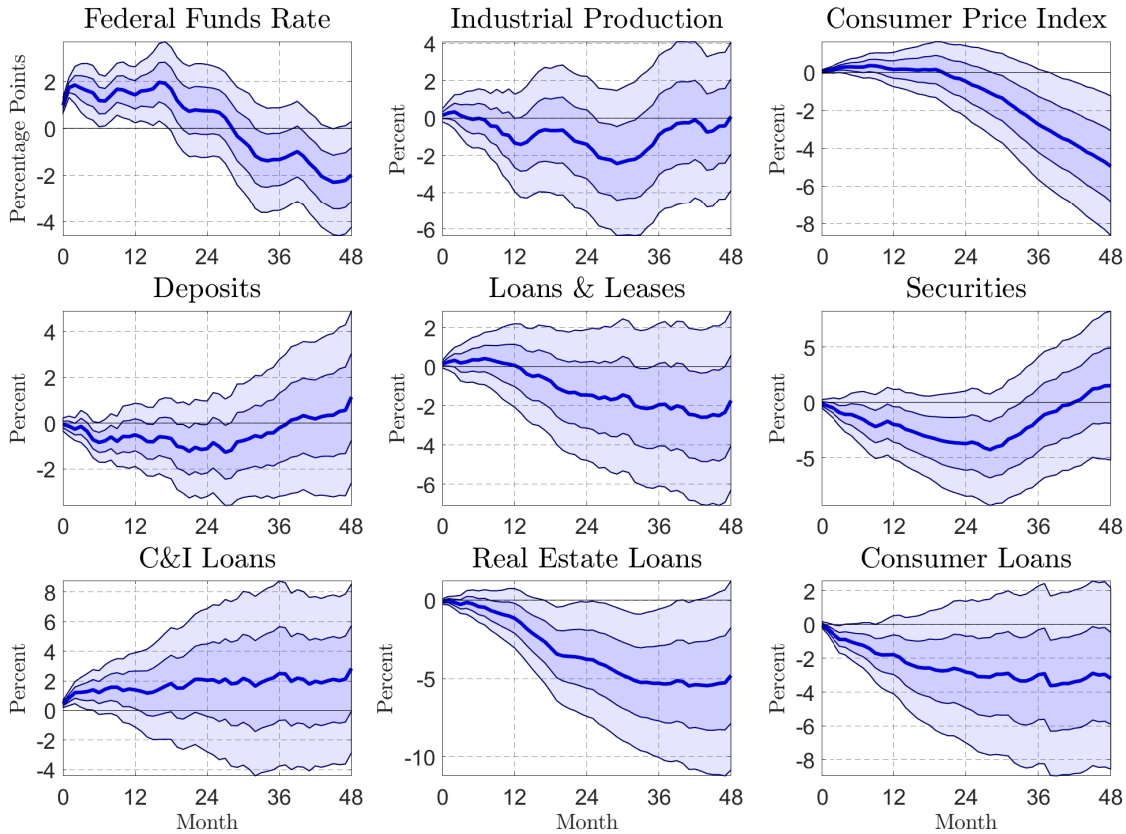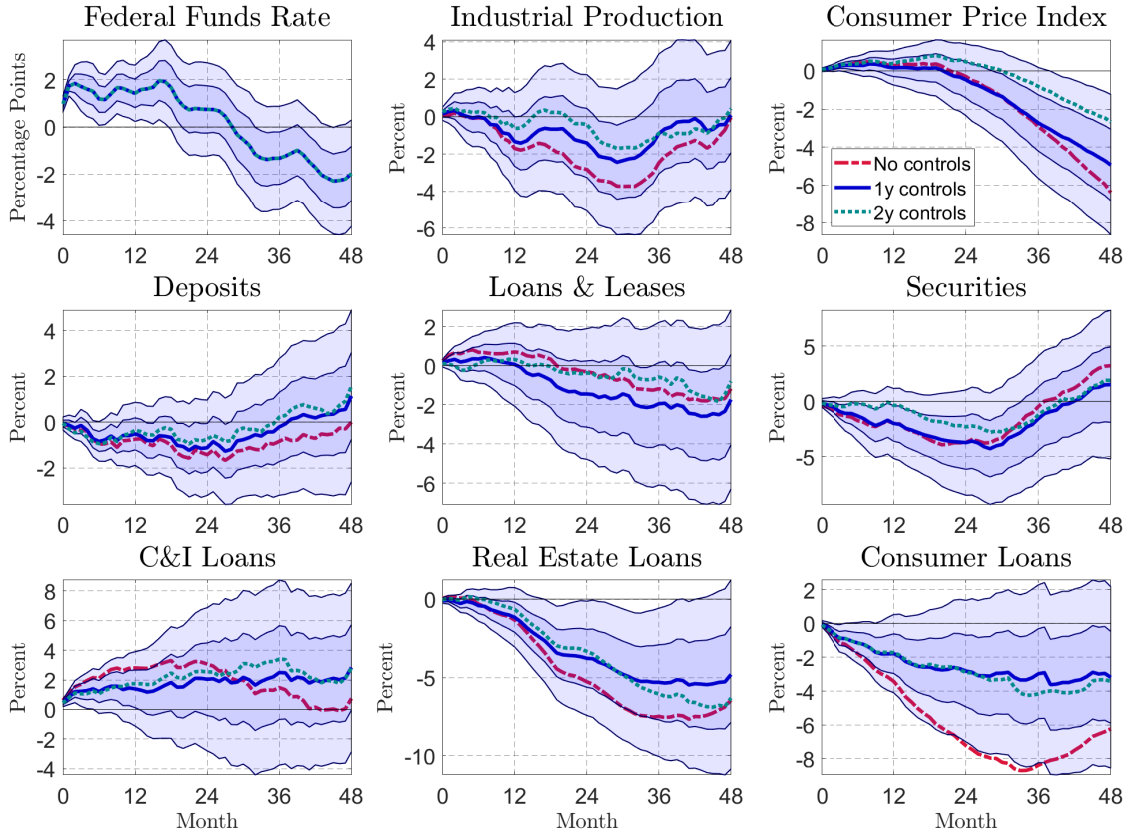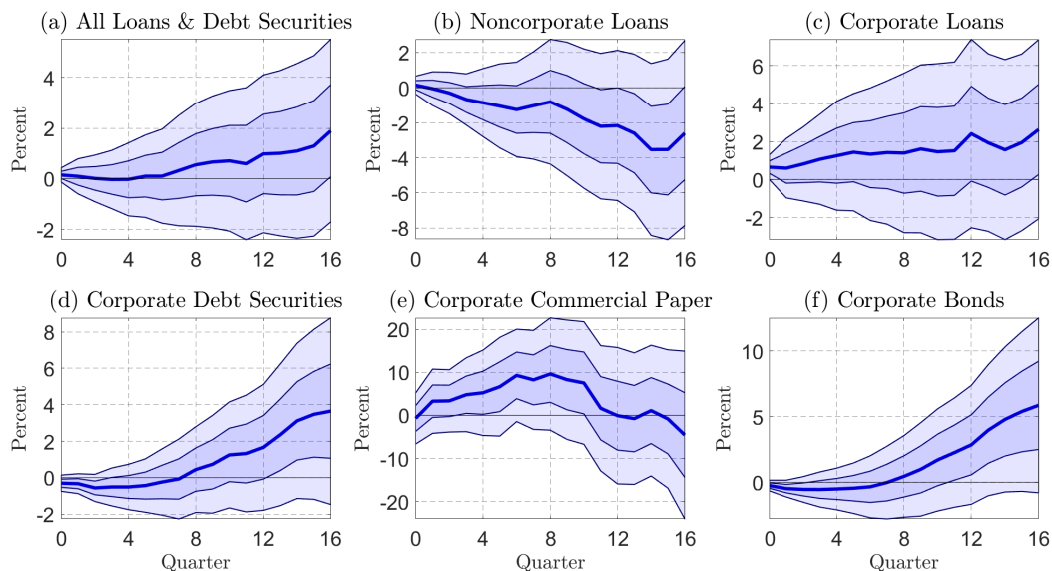
Figure B.4: Impulse Responses to a Monetary Policy Shock − Flow of Funds.

Notes: Impulse responses to a 1 percentage point contractionary monetary policy shock based on the identification approach by Romer and Romer (2004) at a quarterly frequency and the local projection specification in (B.1). The shock series is taken from Coibion et al. (2017) and the remaining data are obtained from St. Louis Fed's FRED database (see Appendix Table C.1 for details about the data). Sample: 1970:M1 - 2007:M12, the shocks in 1980:M4 - 1980:M6 and 1980:M9 - 1980:M11 are excluded following Coibion (2012). 95 and 68 percent confidence bands are shown using Newey and West (1987) standard errors.

C Data

C.1 Variable Definitions and Data Sources

In Tables C.1-C.5, we provide names, definitions, and sources for all variables that are used in the empirical analysis. Table C.1 reports the macro time series that are used in Section 1 and Appendix B. Table C.2 collects all variables that are used from the FR Y-14Q H.1 data, Table C.3 the ones from Compustat, and Table C.4 reports the ones from Orbis. These variables are employed in Sections 3-5 and the Appendix. The variables from the FR Y-9C Filings are described in Table C.5.

Table C.1: Macro Time Series.

Variable Name	Description	Source
Bank Credit	H.8 releases, All U.S. commercial banks, weekly, SA	FRED
Loans and Leases	H.8 releases, All U.S. commercial banks, weekly, SA	FRED
Securities	H.8 releases, All U.S. commercial banks, weekly, SA	FRED
C&I Loans	H.8 releases, All U.S. commercial banks, weekly, SA	FRED
Real Estate Loans	H.8 releases, All U.S. commercial banks, weekly, SA	FRED
Consumer Loans	H.8 releases, All U.S. commercial banks, weekly, SA	FRED
Total Assets	H.8 releases, All U.S. commercial banks, weekly, SA	FRED
Deposits	H.8 releases, All U.S. commercial banks, weekly, SA	FRED
Federal Funds Rate	Effective funds rate, daily, NSA	FRED
Consumer Price Index	All Items for the United States, SA, 2015=100	FRED
Industrial Production	Real Index, 2012=100, SA	FRED
Gross Domestic Product	Real, Billions of Chained 2012 Dollars, SA	FRED
Gilchrist-Zakrajsek Spread	Based on Gilchrist and Zakrajšek (2012), NSA	FEDS Notes
Nonfinancial Business; Debt Securities and Loans	Flow of Funds, NSA	FRED
Nonfinancial Noncorporate Business; Loans	Flow of Funds, NSA	FRED
Nonfinancial Corporate Business; Loans	Flow of Funds, NSA	FRED
Nonfinancial Corporate Business; Debt Securities	Flow of Funds, NSA	FRED
Nonfinancial Corporate Business; Commercial Paper	Flow of Funds, NSA	FRED
Nonfinancial Corporate Business; Corporate Bonds	Flow of Funds, NSA	FRED

Notes: All nominal credit series are converted into real series using the consumer price index. All weekly or monthly time series are averaged to monthly or quarterly frequency for purposes of computing impulse responses in Figure 1.1 and Appendix B. Notation: "FRED" = St. Louis Fed's FRED Database, "SA" = seasonally-adjusted, "NSA" = non-seasonally-adjusted.

Table C.2: FR Y-14 Variable Definitions.

Variable Name	Description / Use	Field No.
Zip code	Zip code of headquarters	7
Industry	Derived 2-Digit NAICS Code	8
Internal risk rating	Internal risk rating mapped to S&P scale	10
TIN	Taxpayer Identification Number	11
Internal Credit Facility ID	Used together with BHC and previous facility ID to construct loan histories	15
Previous Internal Credit Facility ID	Used together with BHC and facility ID to construct loan histories	16
Origination Date	Used to distinguish new and existing loans	18
Maturity Date	Used to determine remaining maturity	19
Term Loan	Loan facility type reported as Term Loan, includes Term Loan A-C, Bridge Loans, Asset-Based, and Debtor in Possession.	20
Credit Line	Loan facility type reported as revolving or non-revolving line of credit, standby letter of credit, fronting exposure, or commitment to commit.	20
Purpose	Credit facility purpose	22
Committed Credit	Committed credit exposure	24
Used Credit	Utilized credit exposure	25
Line Reported on Y-9C	Line number reported in HC-C schedule of FR Y-9C	26
Secured Credit	Security type of credit	36
Variable Rate	Interest rate variability reported as "Floating" or "Mixed"	37
Interest Rate	Current interest rate	38
Date Financials	Financial statement date used to match firm financials to Y-14 date	52
EBITDA	Derived from operating income plus depreciation and amortization	56, 57
Interest Expense	Used in calculating implied covenants	58
Net Income	Current and prior year net income for trailing 12-months used to construct cash flow changes	59, 60
Cash and Securities	Cash and marketable securities	61
Tangible Assets	Tangible assets	68
Total Assets	Total assets, current year and prior year	70
Short Term Debt	Used in calculating implied covenants & total debt	74
Long Term Debt	Used in calculating implied covenants & total debt	78
Total Liabilities	Total liabilities	80
Capital Expenditures	12-month trailing CAPEX	82
Probability of Default	Probability of default for firms	88
Collateral Value	Collateral market value	93
Syndicated Loan	Syndicated loan flag	100

Notes: All nominal series are converted into real series using the consumer price index (see Table C.1). The corresponding "Field No." can be found in the data dictionary (Schedule H.1, pp. 162-217): https://www.federalreserve.gov/reportforms/forms/FR_Y-14Q20200331_i.pdf

Table C.3: Compustat Variable Definitions.

Variable Name	Description	Compustat Name
Total Assets	Total firm assets	atq
Cash and Short-Term Investments	Cash and short-term investments	cheq
Tangible Assets	Constructed from cash, fixed assets, receivables, and inventories	cheq + invtq + ppentq + rectq
EBITDA	Earnings before interest, taxes, and depreciation and amortization, annual series (only matched to Y14 for Q4-observations)	ebitda
Employer Identification Number	Used to match to TIN in Y14, successful merges are basis for publicly traded designation	ein
Total Liabilities	Total firm liabilities	ltq
Net Income	Firm net income (converted to 12-month trailing series)	niq
Total Debt	Debt in current liabilities + long-term debt	dlcq + dlttq
Capital Expenditures	CAPEX (converted to 12-month trailing series)	capxy

Notes: All data are obtained from the Wharton Research Data Services. Nominal series are converted into real series using the consumer price index (see Table C.1).

Table C.4: Orbis - Bureau van Dijk Variable Definitions.

Variable Name	Description	BvD Name
Employer Identification Number	Used to match to TIN in Y14	EIN
Cash	Cash and cash equivalent assets	CASH
Incorporation date	Date of firm incorporation	DATEINC, DATEINC_YEAR
EBITDA	Earnings before interest, taxes, and depreciation and amortization	EBTA
Total Liabilities	Non-current liabilities + current liabilities	NCLI + CULI
Net Income	Firm net income	ONET
Total Assets	Total firm assets	TOAS

Notes: All data are obtained from Orbis - Bureau van Dijk. Nominal series are converted into real series using the consumer price index (see Table C.1).

Table C.5: Variables from Y-9C filings.

Variable Code	Variable Label
BHCK 2170	Total Assets
BHCK 2948	Total Liabilities
BHCK 4340	Net Income
BHCK 3197	Earning assets that reprice or mature within one year
BHCK 3296	Interest-bearing deposit liabilities that reprice or mature within one year
BHCK 3298	Long-term debt that reprices within one year
BHCK 3408	Variable-rate preferred stock
BHCK 3409	Long-term debt that matures within one year
BHDM 6631	Domestic offices: noninterest-bearing deposits
BHDM 6636	Domestic offices: interest-bearing deposits
BHFN 6631	Foreign offices: noninterest-bearing deposits
BHFN 6636	Foreign offices: interest-bearing deposits
BHCK JJ33	Provision for loan and lease losses
BHCA P793	Common Tier 1 Capital Ratio

Notes: The table lists variables that are collected from the Consolidated Financial Statements or FR Y-9C filings for Bank-Holding Companies from the Board of Governors' National Information Center database. The one-year income gap is defined as $(\text{BHCK 3197} - (\text{BHCK 3296} + \text{BHCK 3298} + \text{BHCK 3408} + \text{BHCK 3409}))/\text{BHCK 2170}$. Total deposits are given by $(\text{BHDM 6631} + \text{BHDM 6636} + \text{BHFN 6631} + \text{BHFN 6636})$. Nominal series are converted into real series using the consumer price index (see Table C.1). The FR Y-9C form for March 2020 can be found at: https://www.federalreserve.gov/reportforms/forms/FR_Y-9C20200401_f.pdf.

C.2   Covenants

Bank credit facilities often come with debt covenants that can effectively limit firm borrowing, even on precommitted credit lines. To account for these, we adjust firms' unused borrowing capacity to account for possible covenant limits that are unobserved in our data sources. As shown by Greenwald (2019), the two most frequently applied covenants are the "Interest-Coverage" (IC) and "Debt-to-Earnings" (DE) covenants (see, e.g., Figure 1 therein). The IC covenant demands that

$$\frac{EBITDA}{Interest\ Expenses} \geq \kappa \,,$$

whereas the DE covenant requires that

$$\frac{Debt}{EBITDA} \leq \tau \ .$$

Based on data from Dealscan, Greenwald (2019) shows that κ and τ are relatively stable over time (see, e.g., Figure 2 therein). In particular, weighting loans by the deal-amount, κ is around 2.75 and τ is approximately 3.75. We use these two covenant rules and the calibrations for κ and τ to adjust firms' borrowing capacity. To this end, we apply the following steps. Based on firms' EBITDA, stock of debt (short-term debt + long-term debt), and interest expenses, we compute the "debt room" that a firm has until either of the two constraints binds. For the IC covenant, we calculate the debt room based on the average interest rate on a firm's outstanding debt. If a firm's debt room is smaller than its unused capacity, then we assume that

a firm's actual unused capacity is equal to the debt room.[57] Based on this procedure, we find that around 37 percent of firms violate one of the two constraints in normal times (2012:Q3-2019:Q4). Chodorow-Reich and Falato (2021) find a slightly lower share of violations across loans (around 25 percent). Hence, while in the same range, our procedure can be viewed as conservative, since firms with looser limits or without the type of covenants that we assume could in fact be non-violaters.

C.3 Data Cleaning and Sample Restrictions

We apply the following set of sample restrictions to the Y14 data:

1. We restrict the sample to begin in 2012:Q3. The Y14 collection began in 2011:Q3, but there was a significant expansion in the number of BHCs required to submit Y14 commercial loan data until 2012:Q3. Moreover, the starting date in 2012:Q3 also affords a short phase-in period for the structure of the collection and variables to stabilize.

2. We constrain the sample to loan facilities with line reported on the HC-C schedule in the FR Y9-C filings as commercial and industrial loans, "other" loans, "other" leases, and owner-occupied commercial real estate (corresponding to Field No. 26 in the H.1 schedule of the Y14 to be equal to 4, 8, 9, or 10; see Table C.2). In addition, we drop all observations with NAICS codes 52 and 53 (loans to financial firms and real estate firms).

3. When we use information about the facility type (credit line or term loan) or interest rate variability type (i.e., fixed or floating), we exclude observations for which this information is missing or changing over the facility history.

4. Drop all facility records with origination dates before 1990 and maturities greater than 30 years, to minimize the potential influence of data entry errors.

5. Observations with negative or zero values for committed exposure, negative values for utilized exposure, and with committed exposure less than utilized exposure are excluded.

6. When aggregating loans at the firm-level, we exclude observations for which the firm identifier "TIN" is missing. To preserve some of these missing values, we fill in missing TINs from a history where the non-missing TIN observations are all the same over a unique facility ID.

7. When using information on firms' financials in the analysis, we apply a set of filters to ensure that the reported information is sensible. We exclude observations (i) if total assets, total liabilities, short-term debt, long-term debt, cash assets, tangible assets, or interest expenses are negative, (ii) if tangible assets, cash assets, or total liabilities are greater than total assets, and (iii) if total debt (short term + long term) is greater than total liabilities.

8. In parts of the empirical analysis, we differentiate between new and existing loans. In some instances, the reporting banks change the IDs for the same facility over time, which would lead to an incorrect classification of such loans as newly issued. To address this issue, we use information on whether a credit facility previously had a different ID, which banks have to report in the Y14 (see Table C.2). If we can find a record for the prior ID, we append the history of the new ID onto the history of the prior ID.

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[57]To account for covenant limits in Figure 3.1, we adjust the total amount of unused credit based on the ratio of debt room to unused credit for firms for which we observe all balance sheet and income information within a period.

9. While a loan facility may include both credit lines and term loans, we observe a binary facility type designation, corresponding to which loan type constitutes the majority share. As a result, we observe facilities designed as term loan facilities, but that nonetheless contain committed but unused credit, strongly implying that the facility contains a credit line. To address this, we assume that all unused credit (i.e., committed exposure net of utilized exposure) represents unused capacity on the firm's credit lines. In other words, we count unused credit on facilities that designed as term loans as part of that firm's unused credit line balances.

10. When using the interest rate on loans in our calculations, we exclude observations with interest rates below 0.5 or above 50 percent to minimize the potential influence of data entry errors.

D Additional Descriptive Evidence

Figure D.1: Credit Characteristics across Firm Size Distribution.

Notes: The figures show various credit characteristics for percentiles across the firm size distribution. Weighted by used credit, panel (a) portrays firms' interest rate and panel (b) shows banks' internal credit rating (we assign a number to each rating ranging from 10 (AAA) to 1 (D)). Panel (c) displays the share of credit that is secured by collateral (value of collateral is set to loan commitment amount if it exceeds this amount). Panel (d) shows the value of collateral relative to committed credit. The firm size distribution is computed for each date according to firms' total assets. Sample: 2012:Q3 - 2019:Q4.

Variable Rate Loans

Probability of Default

Syndicated Loan

Remaining Maturity

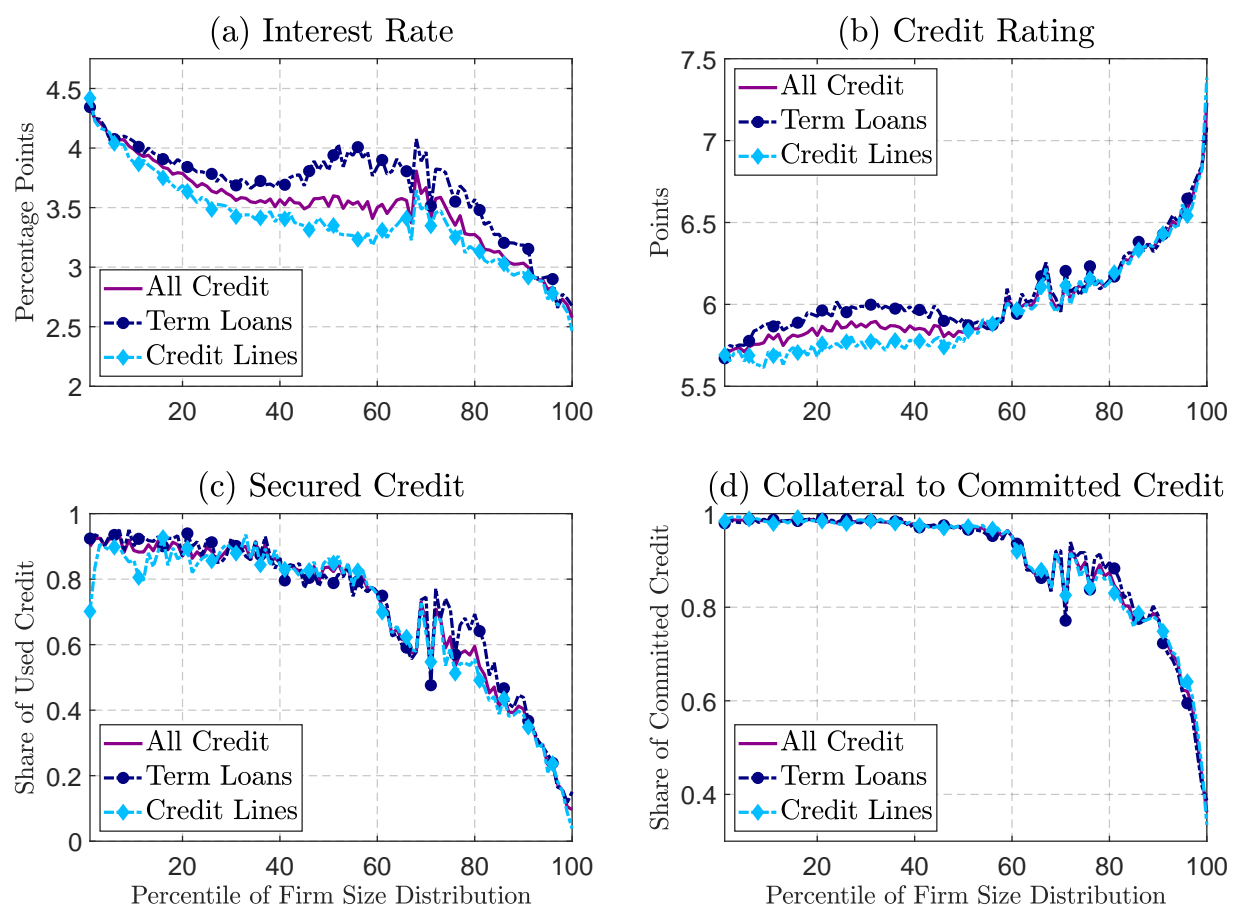## Firms with High Credit Use

Observed Credit to Debt

Figure D.2: Credit Characteristics across Firm Size Distribution.

Notes: The figures show various credit characteristics for percentiles across the firm size distribution. The top left gives the share of loans that carry a variable rate. The top right shows banks' assessed probability of default. The middle left gives the share of used credit that is syndicated and the middle right shows remaining maturity weighted by all used credit. The bottom left gives the share of firms that use at least 90 percent of their committed credit, which is additionally adjusted for covenants (see Appendix C.2). The bottom right graph shows the average share of observed credit in our data relative to total debt. The firm size distribution is computed for each date according to firms' total assets. Sample: 2012:Q3 - 2019:Q4.
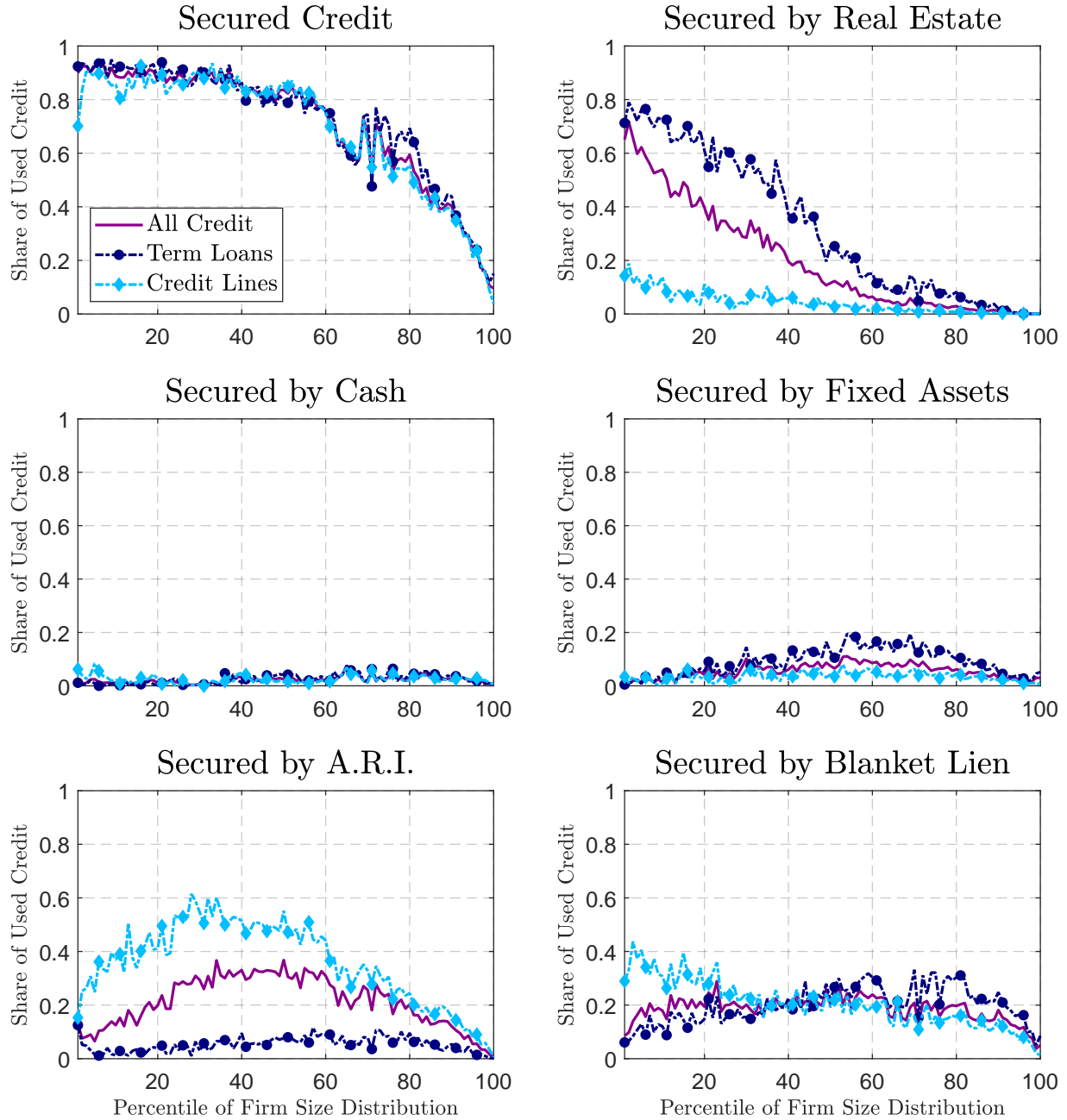
Figure D.3: Credit Characteristics across Firm Size Distribution.

Notes: The figures show the share of used credit that is secured by collateral for percentiles across the firm size distribution. The top left gives the share of loans that is secured by some type of collateral. The remaining graphs show the share of used credit secured by real estate (top right), cash and marketable securities (middle left), fixed assets excluding real estate (middle right), accounts receivables and inventory (A.R.I., bottom left), or by a blanket lien (bottom right). The firm size distribution is computed for each date according to firms' total assets. Sample: 2012:Q3 - 2019:Q4.

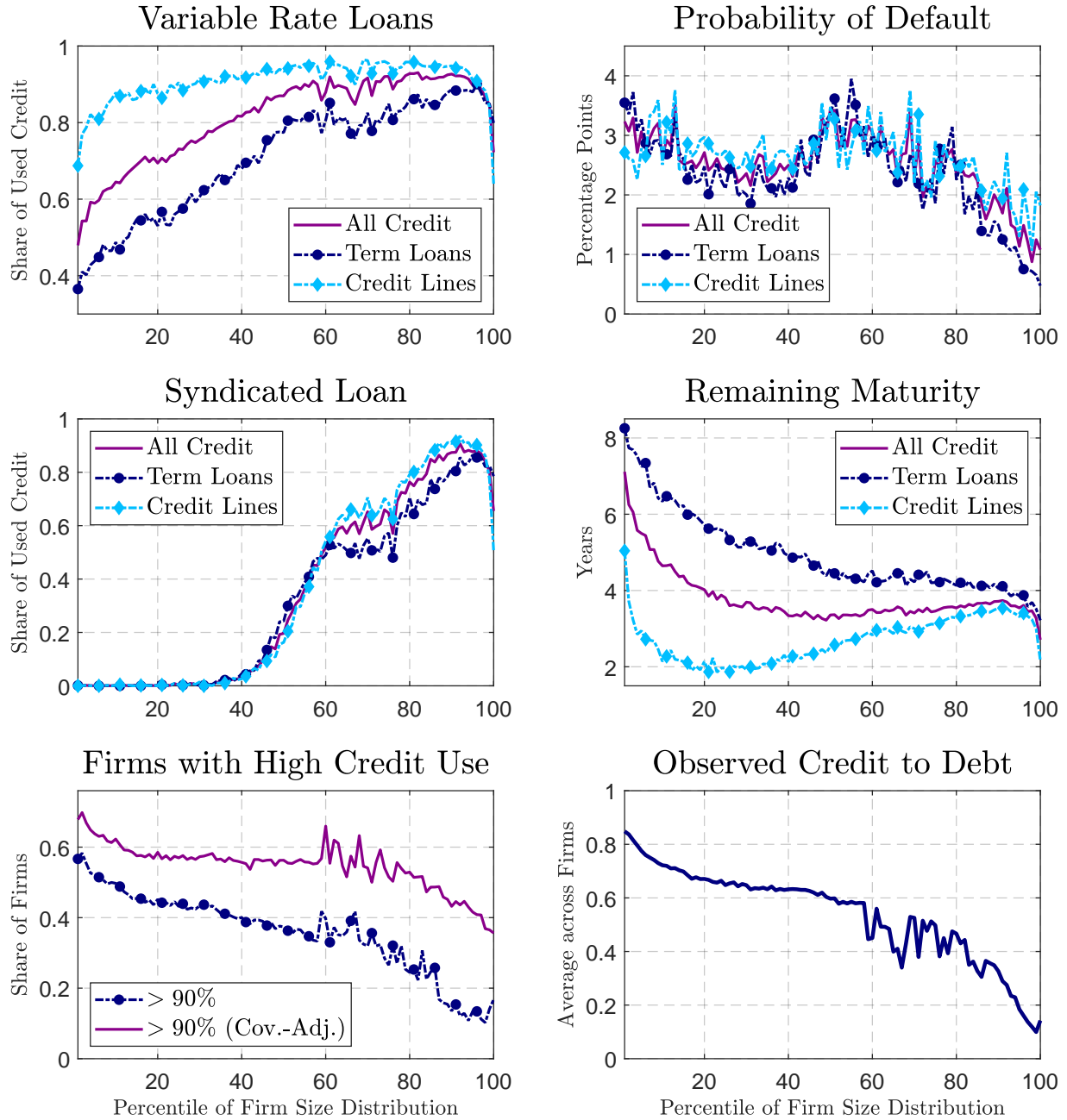# E Determinants and Use of Firm Credit

E.1 Which firms have credit lines and borrowing capacity?

Table E.1: Credit Line Regressions.

	(i) Firm has Credit Line (Committed>0)	(ii) Unused Capacity (Unused/Committed)	(iii) Credit Intensity (Unused/(Unused+Cash))
EBITDA	1.53***	0.28***	0.25***
	(0.10)	(0.01)	(0.01)
Tangible assets	-0.41***	0.18***	-0.29***
	(0.04)	(0.01)	(0.01)
Size	0.08***	0.02***	-0.03***
	(0.01)	(0.00)	(0.00)
Leverage	-0.85***	-0.58***	-0.21***
	(0.06)	(0.01)	(0.01)
Investment grade	-0.09***	0.12***	0.03***
	(0.03)	(0.00)	(0.00)
Public firm	0.61***	0.13***	0.00
	(0.06)	(0.01)	(0.01)
Firm age	0.11***	0.02***	0.03***
	(0.01)	(0.00)	(0.00)
R-squared	0.10	0.27	0.12
Observations	194,136	156,010	152,607
Number of Firms	38,025	31,209	30,841

Notes: Estimation results for regressions (4.1). Dependent variables are {0,1}-indicator measuring whether a firm has a credit line in column (i), the share of unused borrowing capacity, defined as 1 minus the ratio of used balances to committed balance in column (ii), and a credit line borrowing intensity measure defined as the ratio of unused balances to unused balances plus cash in column (iii). The specifications include adjustments to borrowing capacity to reflect generic covenant restrictions as described in Appendix C.2. EBITDA and tangible assets are scaled by noncash assets (total assets minus cash and marketable securities). Size is defined as the natural log of noncash assets. Leverage is the ratio of total liabilities to total assets. Firm age is the natural log of the number of periods between the observation date and firm incorporation date, annualized. All regressors are lagged by four quarters. The incidence of a firm having a credit line is estimated as a logit regression, reporting a Pseudo R^2. All other specifications are estimated using OLS. Sample: 2012:Q3 - 2019:Q4. All estimations include industry and time fixed effects. Standard errors in parentheses are clustered by firm. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table E.2: Credit Line Regressions: Covenant Adjustments.

| | (i) | (ii) | (iii) | (iv) | (v) | (vi) |
| | Firm has credit line | | Unused capacity | | Credit intensity | |
	Commitment	Covenants	Commitment	Covenants	Commitment	Covenants
EBITDA	0.92***	1.53***	0.14***	0.28***	0.02***	0.25***
	(0.08)	(0.10)	(0.01)	(0.01)	(0.01)	(0.01)
Tangible assets	-0.36***	-0.41***	0.13***	0.18***	-0.34***	-0.29***
	(0.05)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)
Size	0.15***	0.08***	0.01***	0.02***	-0.04***	-0.03***
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Leverage	-0.91***	-0.85***	-0.41***	-0.58***	0.09***	-0.21***
	(0.07)	(0.06)	(0.01)	(0.01)	(0.01)	(0.01)
Investment grade	-0.17***	-0.09***	0.07***	0.12***	-0.05***	0.03***
	(0.03)	(0.03)	(0.00)	(0.00)	(0.00)	(0.00)
Public firm	1.74***	0.61***	0.15***	0.13***	-0.00	0.00
	(0.11)	(0.06)	(0.01)	(0.01)	(0.01)	(0.01)
Firm age	0.10***	0.11***	0.02***	0.02***	0.02***	0.03***
	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
R-squared	0.14	0.10	0.22	0.27	0.20	0.12
Observations	194,136	194,136	156,010	156,010	152,607	152,607
Number of Firms	38,025	38,025	31,209	31,209	30,841	30,841

Notes: Estimation results for regressions (4.1). "Commitment" refers to the unadjusted committed balances, specifications labeled "Covenants" include adjustments to borrowing capacity to reflect generic covenant restrictions as described in Appendix C.2. Dependent variables are {0,1}-variable measuring whether firm has a credit line in columns (i) and (ii), the share of unused borrowing capacity, defined as 1 minus the ratio of used balances to committed balance in columns (iii) and (iv), and a credit line borrowing intensity measure defined as the ratio of unused balances to unused balances plus cash in columns (v) and (vi). The two specifications "Commitment" and "Covenants" are estimated on the same sample for each dependent variable. EBITDA and tangible assets are scaled by noncash assets (total assets minus cash and marketable securities). Size is defined as the natural log of noncash assets. Leverage is the ratio of total liabilities to total assets. Firm age is the natural log of number of periods between the observation date and the firm incorporation date, annualized. All regressors are lagged by four quarters. The incidence of a firm having a credit line is estimated as a logit regression, reporting a Pseudo R^2. All other specifications are estimated using OLS. Sample: 2012:Q3 - 2019:Q4. All estimations include industry and time fixed effects. Standard errors in parentheses are clustered by firm. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table E.3: Credit Line Regressions: Private versus Public Firms.

| | (i) | (ii) | (iii) | (iv) | (v) | (vi) |
| | Firm has credit line | | Unused capacity | | Credit intensity | |
	Private	Public	Private	Public	Private	Public
EBITDA	0.82***	-0.23	0.10***	0.06	0.02***	-0.06
	(0.07)	(1.16)	(0.01)	(0.03)	(0.01)	(0.04)
Tangible assets	-0.27***	-0.10	0.11***	0.14***	-0.33***	-0.32***
	(0.04)	(0.29)	(0.01)	(0.01)	(0.01)	(0.02)
Size	0.11***	0.36***	0.01***	0.04***	-0.04***	-0.06***
	(0.01)	(0.06)	(0.00)	(0.00)	(0.00)	(0.00)
Leverage	-0.94***	-0.50	-0.43***	-0.14***	0.10***	0.12***
	(0.07)	(0.43)	(0.01)	(0.02)	(0.01)	(0.03)
Investment grade	-0.19***	0.22	0.06***	0.05***	-0.05***	0.00
	(0.03)	(0.18)	(0.00)	(0.01)	(0.00)	(0.01)
Firm age	0.11***	-0.11	0.01***	0.01**	0.02***	0.03***
	(0.02)	(0.09)	(0.00)	(0.00)	(0.00)	(0.01)
R-squared	0.10	0.06	0.17	0.14	0.20	0.27
Observations	210,532	32,578	172,149	31,928	137,150	31,470
Number of Firms	41,910	2,347	34,510	2,298	30,458	2,269

Notes: Estimation results for regressions (4.1). "Private" and "Public" refer to samples restricted to only private or only public firms, respectively. Committed and unused balances are computed without covenant adjustments. Dependent variables are {0,1}-variable measuring whether firm has a credit line in columns (i) and (ii), the share of unused borrowing capacity, defined as 1 minus the ratio of used balances to committed balance in columns (iii) and (iv), and a credit line borrowing intensity measure defined as the ratio of unused balances to unused balances plus cash in columns (v) and (vi). EBITDA and tangible assets are scaled by noncash assets (total assets minus cash and marketable securities). Size is defined as the natural log of noncash assets. Leverage is the ratio of total liabilities to total assets. Firm age is the natural log of number of periods between the observation date and the firm incorporation date, annualized. All regressors are lagged by four quarters. The incidence of a firm having a credit line is estimated as a logit regression, reporting a Pseudo R^2. All other specifications are estimated using OLS. Sample: 2012:Q3 - 2019:Q4. All estimations include industry and time fixed effects. Standard errors in parentheses are clustered by firm. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

E.2 Credit Responses to Cash-Flow Changes

We extend regressions (4.2) to investigate whether firms with different levels of borrowing capacity prior to a change in cash flow show distinct credit line responses. To this end, we estimate

$$\frac{L_{i,t+h-3} - L_{i,t-4}}{0.5\left(L_{i,t+h-3} + L_{i,t-4}\right)} = \alpha_i^h + \tau_{t,k}^h + \beta_1^h \frac{\Delta^4 CF_{i,t}}{Assets_{i,t-4}} + \beta_2^h \frac{\Delta^4 CF_{i,t}}{Assets_{i,t-4}} \cdot Cap_{i,t-4} + \gamma^h X_{i,t-4} + u_{i,t-3}^h \ , \qquad \text{(E.1)}$$

where $h = 0, 1, ..., 8$ and $Cap_{i,t-4}$ is the ratio of unused to committed credit of firm i at time $t - 4$. Unused credit is the sum of unused credit lines and unused term loans. Figure E.1 shows the negative of the estimated coefficients β_1 and β_2. To determine firms' borrowing capacity, we also consider a version that additionally adjusts for covenants as described in Appendix C.2.

In addition, we estimate the local projections (4.2) using the committed instead of the used amount of credit for the dependent variable, and also consider the specification

$$\frac{L_{i,t+h-3}}{C_{i,t+h-3}} - \frac{L_{i,t-4}}{C_{i,t-4}} = \alpha_i^h + \tau_{t,k}^h + \beta_1^h \frac{\Delta^4 CF_{i,t-4}}{Assets_{i,t-4}} + \gamma^h X_{i,t-4} + u_{i,t-3}^h \ , \qquad \text{(E.2)}$$

where $h = 0, 1, ..., 8$ and $L_{i,t}/C_{i,t}$ is the ratio of used to committed credit of firm i at time t. The estimation results to a negative cash flow change for (E.2) are shown in Figure E.2, again considering a version that additionally adjusts for covenants.
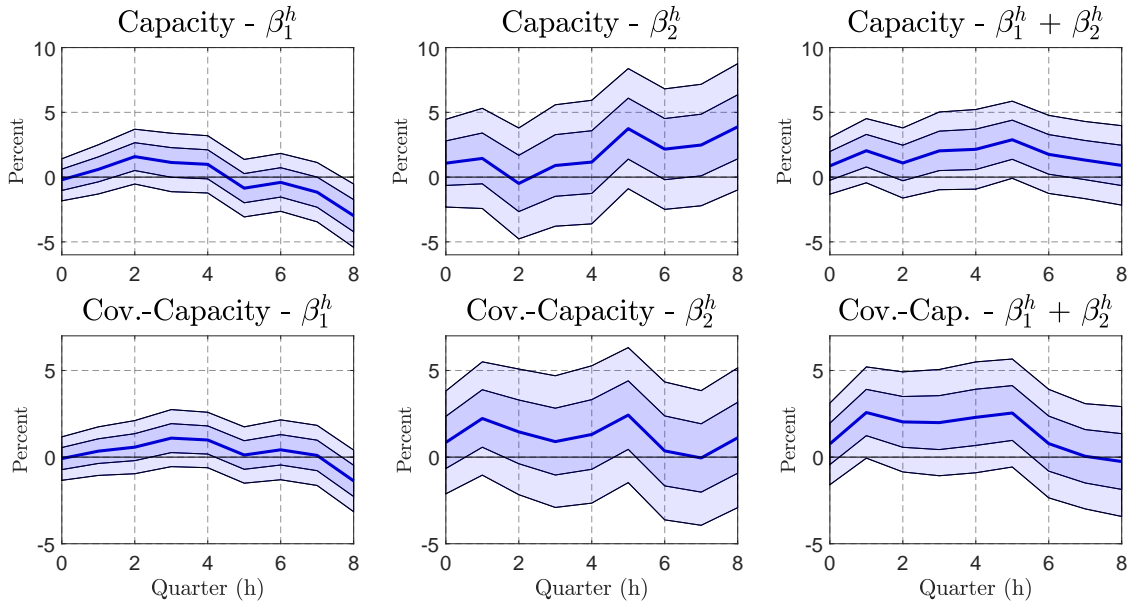

Figure E.1: Credit Responses to a Cash-Flow Change − Borrowing Capacity.

Notes: Responses of firms' used credit lines to a one-unit decrease in net income relative to assets, based on the local projection approach in (E.1). The figures at the top show the estimated coefficients for specifications that use firms' lagged borrowing capacity, defined as the ratio of unused credit to committed credit, and the ones at the bottom adjust firms' borrowing capacity for generic covenant limits (see Appendix C.2). The left and the middle graphs give the estimated coefficients β_1^h and β_2^h, and the right figure indicates the sum of the two. Observations with absolute annual changes in net income relative to assets larger than 5 percent are excluded. The estimations are based on a balanced panel with 6,751 observations (top) and 6,710 observations (bottom), respectively, for each impulse response horizon. 95 and 68 percent confidence bands are shown using standard errors that are clustered by firm. Sample: 2012:Q3 - 2019:Q4.
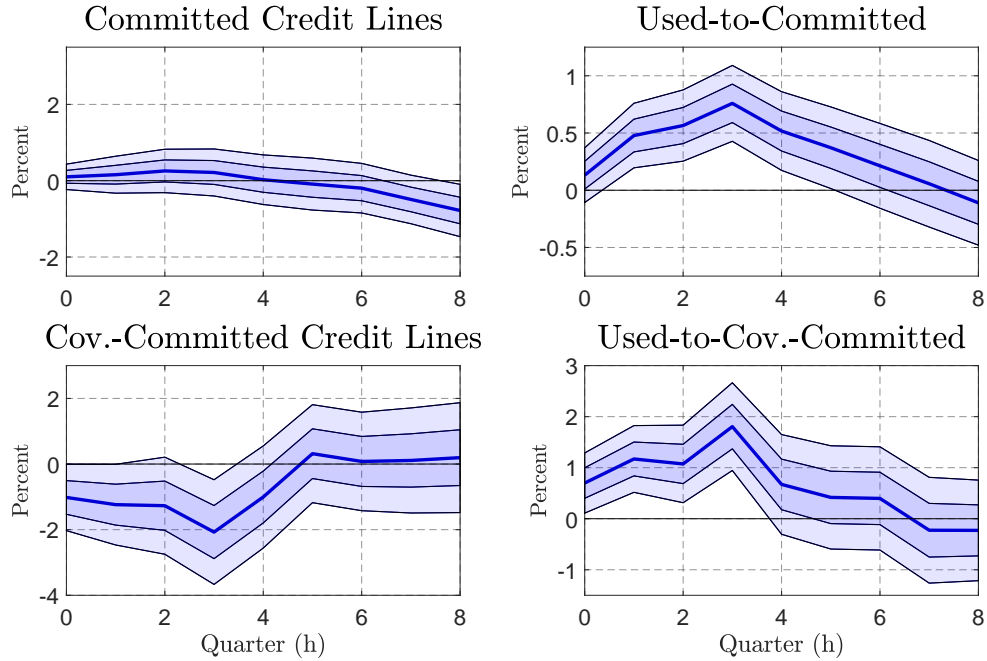
Figure E.2: Credit Responses to a Cash-Flow Change − Committed Credit.

Notes: Responses of firms' committed credit lines (left) and the ratio of used-to-committed credit lines (right) to a one-unit decrease in net income relative to assets, based on the local projection approaches in (4.2) and (E.2). Observations with absolute annual changes in net income relative to assets larger than 5 percent are excluded. The estimations are based on a balanced panel with 6,751 observations (top) and 2,744 observations (bottom), respectively, for each impulse response horizon. 95 and 68 percent confidence bands are shown using standard errors that are clustered by firm. Sample: 2012:Q3 - 2019:Q4.

F Behavior of Firm Credit around Macroeconomic Events

F.1 Credit Responses to Monetary Policy Surprises - Aggregate Data
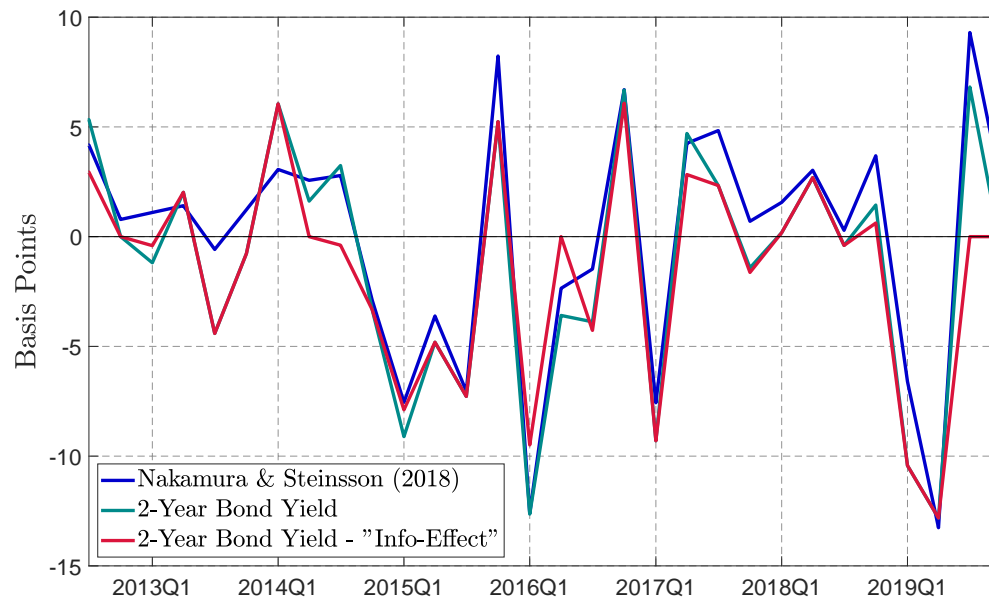
Figure F.1: Monetary Policy Surprises.

Notes: The figure shows monetary policy surprises that are measured within a 30-minute window around policy announcements of the Federal Reserve (10 minutes before and 20 minutes after each announcement) and aggregated to a quarterly frequency by summing up the individual surprises within a quarter (see also Romer and Romer, 2004, regarding the aggregation). The shock series displayed by the blue line follows the computations in Nakamura and Steinsson (2018) (see also footnote 55). The green line shows the surprises of the two-year government bond yield and the red line uses the same surprises but excludes the ones that are associated with nonstandard stock price responses following Jarociński and Karadi (2020). Sample: 2012:Q3 - 2019:Q4.
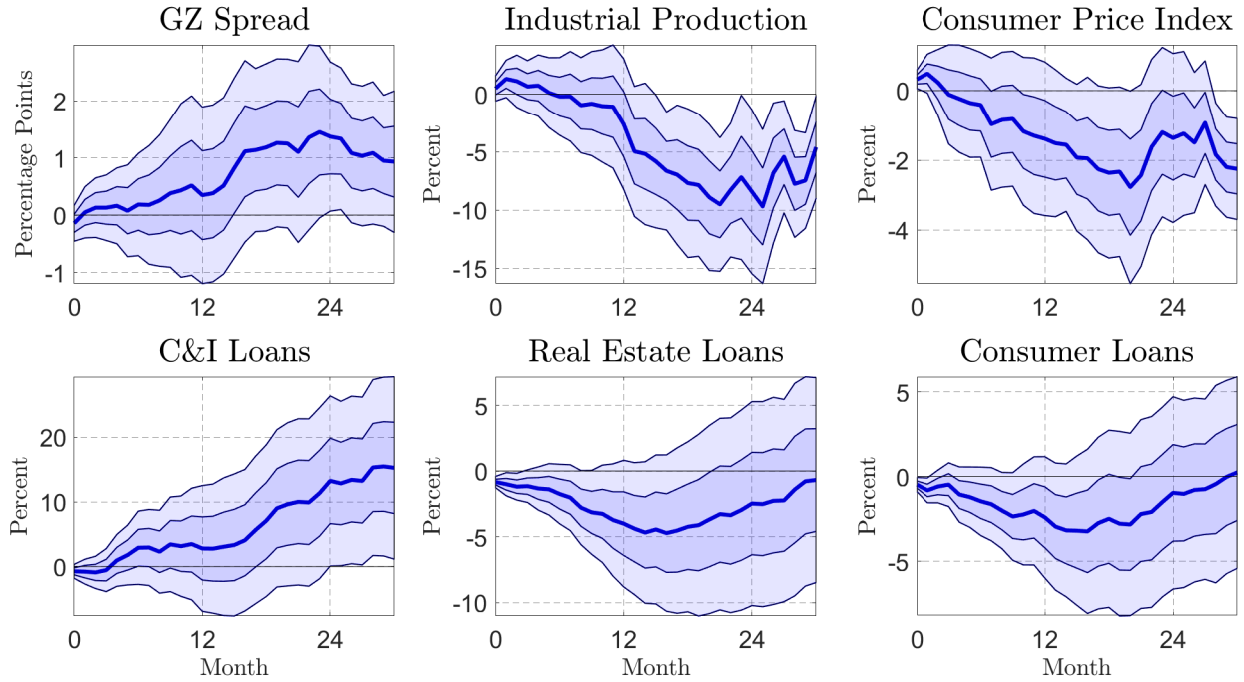
Figure F.2: Responses of Aggregate Indicators to MP Surprises.

Notes: Impulse responses at the monthly frequency to a 25 basis point surprise increase in the two-year government bond yield. "GZ Spread" denotes the corporate bond spread series by Gilchrist and Zakrajšek (2012). All specifications exclude additional controls, apart from the estimations for industrial production and the consumer price index, for which one year worth of the shocks and the one-month change in the respective dependent variable are included. The remaining data are obtained from St. Louis Fed's FRED database (see Appendix Table C.1 for details about the data). 95 and 68 percent confidence bands are shown using Newey and West (1987) standard errors. Sample: 2012:M7 - 2019:M12.
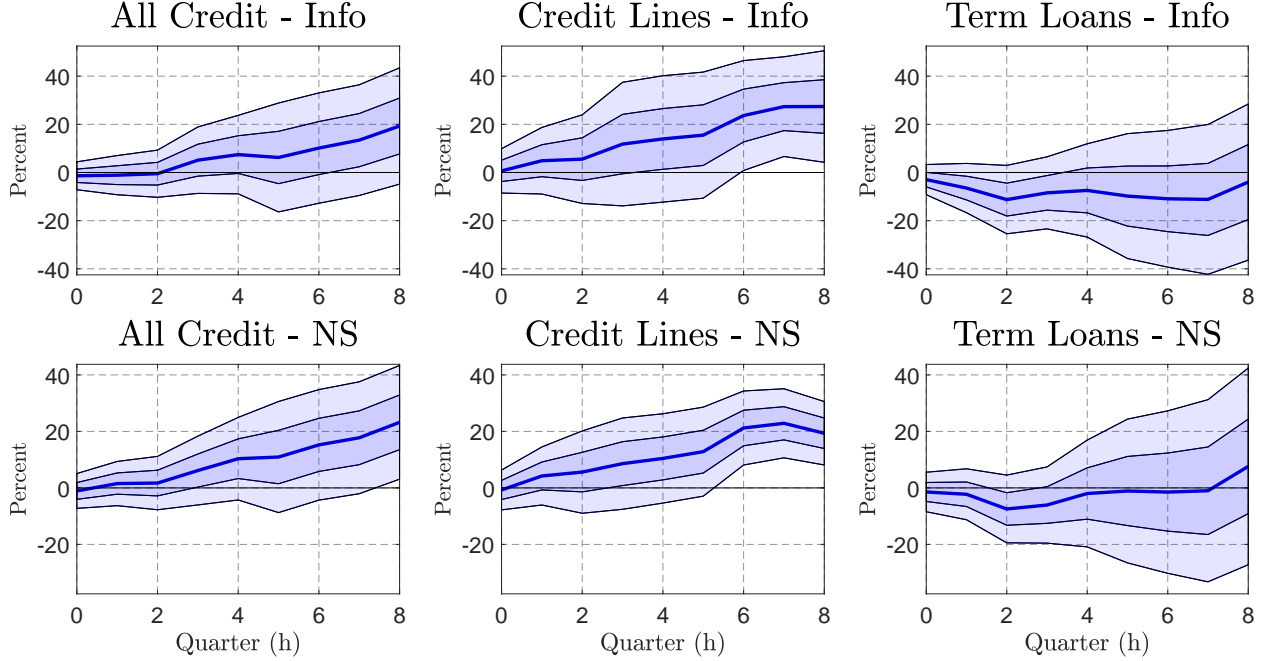
Figure F.3: "Information Effect" and Nakamura and Steinsson (2018)-surprises.

Notes: Impulse responses to a 25 basis point surprise increase in the two-year government bond yield that excludes surprises with nonstandard stock price responses (top graphs, "Info") and the shock series that follows the computations in Nakamura and Steinsson (2018) (bottom graphs, "NS", see also footnote 55). All estimations are based on the local projection approach in (5.1) that uses aggregated data, multiplied by 100. 95 and 68 percent confidence bands are shown using Newey and West (1987) standard errors. Sample: 2012:Q3 - 2019:Q4.

F.2 Credit Responses to Monetary Policy Surprises - Micro Data

At the firm-level, we estimate local projections

$$\frac{L_{i,t+h} - L_{i,t-1}}{\overline{L}_{t-1}} = \alpha_i^h + \beta^h \epsilon_t^{MP} + \gamma^h X_{i,t-1} + u_{i,t}^h \ , \tag{F.1}$$

where $h = 0, 1, ..., 8$ and $L_{i,t} = \sum_{j=1}^{J} L_{i,t}^j$ is the amount of some credit type by firm i at time t across all banks $j = 1, ..., J$. Note that the dependent variable takes a particular form. The numerator is given by the change in credit of firm i between $t-1$ and $t+h$, while the denominator $\overline{L}_{t-1}$ is the average amount of a particular type of credit across all firms $i = 1, ..., N$ at $t-1$, that is, $\overline{L}_{t-1} = (1/N)\sum_{i=1}^{N} L_{i,t-1}$. Given this setup, the estimated coefficients β^h are comparable to the ones estimated with aggregate time series in (5.1). As above, we use surprise movements in the two-year Treasury note as a measure of the shock ϵ_t^{MP}, $X_{i,t-1}$ again includes two lagged values of the one-quarter growth rate of the dependent variable and two lags of ϵ_t^{MP}, and α_i^h is a firm-horizon fixed effect.

Estimating F.1 on the full sample gives the results that are shown in Figure F.4. They are similar to the ones in Figure 5.1, with the difference that the confidence intervals are slightly wider. The additional advantage of the firm-level approach is that it allows us to decompose the aggregate response by firm characteristics. In particular, we estimate the local projections in (F.1) for credit lines by groups according to their position along the firm size distribution and the borrowing capacity distribution in the quarter before

the shock occurs.[58] We separate firms into three groups of similar size and rescale their response by the number of firms in each group relative to the total number of firms for each respective estimation. The results are shown in Figures F.5 and F.6. The total response of credit lines is almost entirely explained by firms that are large and have substantial ex-ante borrowing capacity.[59]

| (a) All Credit | (b) Credit Lines | (c) Term Loans |
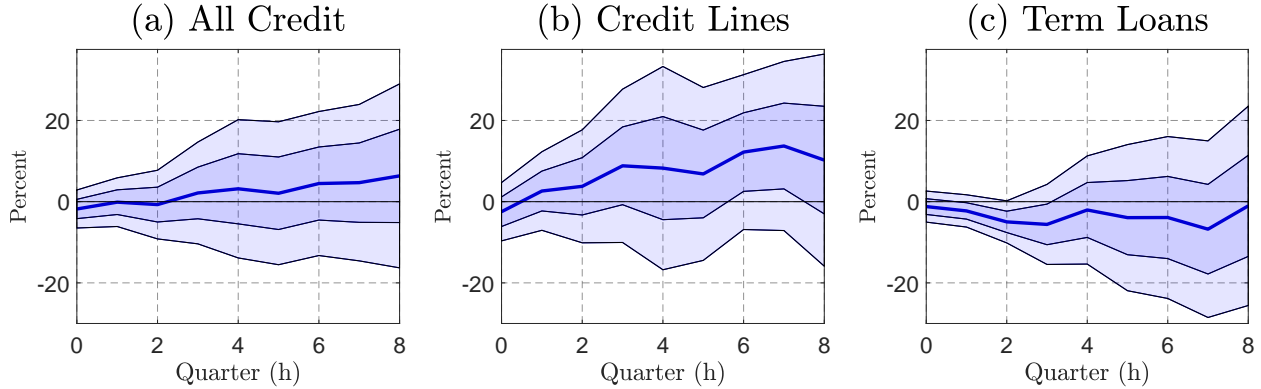
Figure F.4: Firm-Level Credit Responses to a Monetary Policy Surprise.

Notes: Impulse responses of firms' total used credit, credit lines, and term loans to a 25 basis point surprise increase of the two-year government bond yield based on the local projection approach in (F.1), multiplied by 100. The estimations are based on a balanced panel and include 235,225 observations for credit lines, 286,934 for term loans, and 498,384 for all credit for each impulse response horizon. 95 and 68 percent confidence bands are shown using Driscoll and Kraay (1998) standard errors. Sample: 2012:Q3 - 2019:Q4.

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[58]Firm size is measured by total assets, and borrowing capacity is the amount of unused credit, combining unused credit lines and term loans. In Figure F.6, we consider two versions for firms' borrowing capacity, an unadjusted one, where unused credit is given by the difference between committed credit and used credit, and one that additionally adjusts unused credit for generic covenant rules (see Appendix C.2 for details).

[59]In separate regressions, we confirm that the responses of large firms and the ones with large unused borrowing capacity are also statistically different from the responses of either of the two other groups around the seven-quarter-ahead impulse response horizon at the one standard deviation confidence intervals. Differences in the magnitude of the total responses between Figures 5.1, F.4, F.5, and F.6 may arise because of variations in the sample of firms arising from differences in the data availability that is used in each respective estimation.
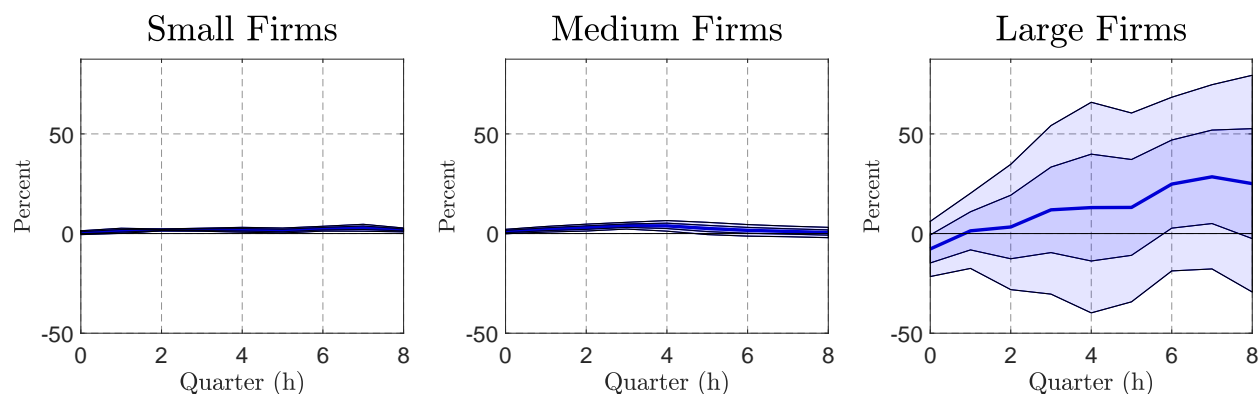
Figure F.5: Credit Line Responses by Firm Size.

Notes: Impulse responses of firms' credit lines to a 25 basis points surprise increase of the two-year government bond yield based on the local projection approach in (F.1), multiplied by 100. The estimations are based on a balanced panel and include a total of 88,941 observations for each respective impulse response horizon. Firms are separated into three bins of equal size according to their position along the firm size distribution in the quarter before the shock occurs. 95 and 68 percent confidence bands are shown using Driscoll and Kraay (1998) standard errors. Sample: 2012:Q3 - 2019:Q4.
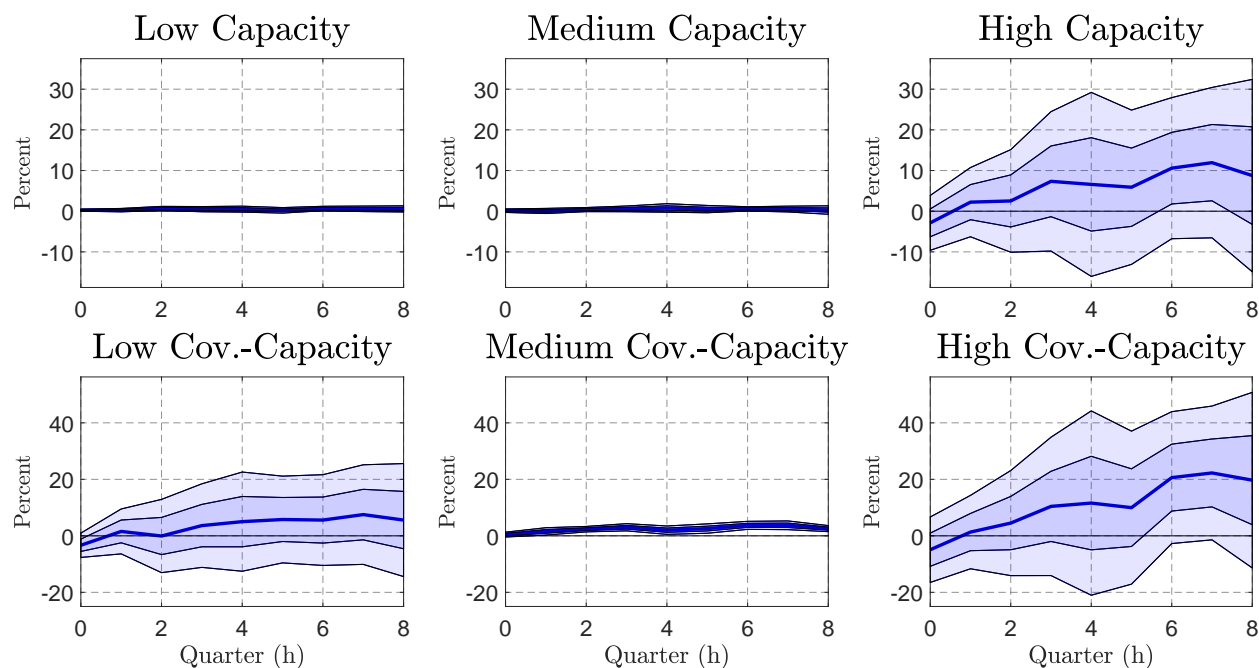
Figure F.6: Credit Line Responses by Borrowing Capacity.

Notes: Impulse responses of firms' credit lines to a 25 basis points surprise increase of the two-year government bond yield based on the local projection approach in (F.1), multiplied by 100. Firms are separated into three bins of similar size according to their position along the borrowing capacity distribution in the quarter before the shock occurs. The top graphs use firms' total unused borrowing as a measure of borrowing capacity, and the bottom graphs additionally adjust this measure for generic covenant rules (see Appendix C.2 for details). The estimations are based on a balanced panel and include a total of 235,225 observations (top figures), and 84,088 observations (bottom figures), respectively, for each impulse response horizon. 95 and 68 percent confidence bands are shown using Driscoll and Kraay (1998) standard errors. Sample: 2012:Q3 - 2019:Q4.

F.3 Credit Movements during the COVID-19 Pandemic

To further understand which firms accounted for the drawdowns on existing credit lines and how those firm characteristics changed relative to other periods, we estimate a set of regressions,

$$Y_{i,t} = \alpha_t + \tau_k + \boldsymbol{\beta_1} X_{i,t-1} + \boldsymbol{\beta_2} X_{i,t-1} \cdot I_t + u_{i,t} \quad , \tag{F.2}$$

where $X_{i,t-1}$ is a vector of firm characteristics, I_t is an indicator variable that is equal to one in 2020:Q1 and zero otherwise, and $Y_{i,t}$ is one of three possible dependent variables. In the first specification, it is given by the log-odds ratio of a (0-1)-indicator, determining whether a firm draws on an existing credit line from one quarter to the next (column (i) of Table F.1). Along the intensive margin, column (ii) considers the change in the use of an existing credit line, where the functional form is again given by $2 \cdot (L_{i,t} - L_{i,t-1}) / (L_{i,t} + L_{i,t-1})$ to account for possible zero observations and diminish the influence of extreme observations. We also specify a change of an existing credit line measure that is scaled by lagged average credit line use across all firms as a way of detecting which type of firms account for the aggregate change in utilization, $(L_{i,t} - L_{i,t-1}) / \overline{L}_{t-1}$ (column (iii) in Table F.1).[60] Besides the time- and industry-fixed effects α_t and τ_k, the same set of explanatory variables is used as in Section 4.1, apart from using net income instead of EBITDA as a measure of profitability.[61] The interaction terms are intended to uncover whether the usual financing patterns may have changed in 2020:Q1.

Similar to the findings in Section 4.2, the negative coefficient on "Net Income" suggests that firms draw on their credit lines when they experience low profits in normal times. However, at the onset of the pandemic, this relationship flips, such that previously profitable firms access their credit lines. A similar pattern occurs for the variables "Size" and "Public," as well as for "Borrowing Capacity" at the extensive margin. Thus, in 2020:Q1, the type of firms that access their existing credit lines are different than in normal times: large, profitable, publicly traded firms with preestablished borrowing capacity draw on their available funding. In addition, highly leveraged firms and firms rated below investment grade are more likely to use their credit lines.

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[60] $L_{i,t}$ and $L_{i,t-1}$ in columns (ii) and (iii) of Table F.1 denote credit lines that were established in $t-1$ or prior to that date. $\overline{L}_{t-1}$ is the average credit line use across all firms in $t-1$, that is, $\overline{L}_{t-1} = (1/N)\sum_{i=1}^{N} L_{i,t-1}$.

[61] To eliminate outliers and data entry errors, observations within the 1 percent tails of the distributions for net income, tangible assets (both relative to noncash assets), and leverage are excluded.

Table F.1: COVID-19 − Changes in Existing Credit Lines.

	(i) Draw Existing Line (0,1)-Dummy		(ii) Δ Existing Line $2 \cdot (L_{i,t} - L_{i,t-1})/(L_{i,t} + L_{i,t-1})$		(iii) Δ Existing Line $(L_{i,t} - L_{i,t-1})/\bar{L}_{t-1}$	
	Coeff.	Std.Err.	Coeff.	Std.Err.	Coeff.	Std.Err.
Borrowing Capacity	-0.34***	0.02	0.77***	0.01	1.49***	0.05
$\cdot \times I_t$	0.53***	0.06	0.59***	0.03	0.51***	0.19
Net Income	-0.11**	0.05	-0.04**	0.02	-0.31***	0.04
$\cdot \times I_t$	0.44***	0.12	0.22***	0.06	1.65***	0.26
Tangible Assets	-0.73***	0.03	0.08***	0.01	0.05	0.03
$\cdot \times I_t$	-0.59***	0.08	0.00	0.04	-0.19	0.29
Size	-0.02***	0.00	-0.00***	0.00	-0.09***	0.01
$\cdot \times I_t$	0.05***	0.01	0.04***	0.01	0.82***	0.09
Leverage	0.77***	0.04	0.07***	0.01	0.17***	0.04
$\cdot \times I_t$	0.01	0.09	0.06	0.05	1.90***	0.42
Investment Grade	-0.16***	0.01	-0.02***	0.01	-0.13***	0.03
$\cdot \times I_t$	-0.04	0.05	-0.04*	0.02	0.09	0.23
Public	-0.17***	0.03	-0.12***	0.01	-0.34***	0.04
$\cdot \times I_t$	0.39***	0.07	0.15***	0.04	5.59***	0.61
Firm Age	0.02***	0.01	-0.00*	0.00	-0.03***	0.01
$\cdot \times I_t$	-0.02	0.02	-0.02*	0.01	0.09	0.14
Sum of Coefficients 2020:Q1						
Borrowing Capacity	0.19***	0.06	1.37***	0.03	2.00***	0.18
Net Income	0.33***	0.11	0.17***	0.06	1.35***	0.25
Tangible Assets	-1.32***	0.08	0.09**	0.04	-0.13	0.28
Size	0.03***	0.01	0.03***	0.01	0.73***	0.09
Leverage	0.79***	0.09	0.13***	0.05	2.06***	0.42
Investment Grade	-0.20***	0.04	-0.06***	0.02	-0.04	0.22
Public	0.22***	0.07	0.03	0.04	5.25***	0.61
Firm Age	0.00	0.02	-0.03**	0.01	0.06	0.14
R-squared	0.04		0.08		0.03	
Observations	260,766		183,457		260,766	
Number of Firms	41,563		32,900		41,563	

Notes: Estimation results for firm credit regressions (F.2) using changes in existing credit lines from one quarter to the next. Dependent variables are a {0,1}-variable measuring whether a firm drew on an existing credit line in column (i), the change in used credit of an existing credit line, relative to a firm's own stock over two quarters in column (ii) or the average existing credit line stock in the previous quarter in column (iii). All explanatory variables are given by the most recent observations over the previous four quarters. Borrowing capacity is the share of unused borrowing to total commitments. Net income and tangible assets are scaled by noncash assets (total assets minus cash and marketable securities). Size is defined as the natural log of noncash assets. Leverage is the ratio of total liabilities to total assets. Firm age is the natural log of the number of periods between the observation date and firm incorporation date, annualized. The incidence of a firm drawing on an existing credit line is estimated as a logit regression, reporting a Pseudo R^2. All other specifications are estimated using OLS. All estimations include time and industry fixed effects. Standard errors in parentheses are clustered by firm. Sample: 2012:Q3 - 2020:Q1. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table F.2: COVID-19 Credit Supply − Interest Rates.

	Δ Interest Rate			Δ Interest Rate (Wins.)		
	(i) 2020:Q1	(ii) 2020:Q2	(iii) 2020:Q3	(iv) 2020:Q1	(v) 2020:Q2	(vi) 2020:Q3
Δ Credit Line Usage	-0.51	1.68**	2.22**	0.85	0.94	1.41
	(1.02)	(0.75)	(0.98)	(0.63)	(0.66)	(0.84)
Fixed Effects						
Firm × Rate	✓	✓	✓	✓	✓	✓
R-squared	0.76	0.9	0.92	0.83	0.94	0.94
Observations	1,644	1,487	1,359	1,598	1,444	1,319
Number of Firms	733	667	609	714	648	591
Number of Banks	28	28	28	28	28	28

Notes: Estimation results for regressions (5.2), where the dependent variable is given by changes in interest rates ($L_{i,t+h}^{j,k} - L_{i,t-1}^{j,k}$) between 2019:Q4 and 2020:Q1 in columns (i) and (iv), from 2019:Q4 to 2020:Q2 in columns (ii) and (v), and between 2019:Q4 and 2020:Q3 in columns (iii) and (vi). Columns (iv)-(vi) exclude observations within the 1 percent tails of the distribution of the dependent variable. The regressor "Δ Credit Line Usage" denotes the change of a bank's used existing credit lines from 2019:Q4 to 2020:Q1, relative to total assets in 2019:Q4. All specifications are estimated using OLS. Standard errors in parentheses are clustered by bank. Sample: 2019:Q4 - 2020:Q3. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table F.3: COVID-19 Credit Supply − Firm Fixed Effect.

	(i) 2020:Q1	(ii) 2020:Q1	(iii) 2020:Q1	(iv) 2020:Q2	(v) 2020:Q3	(vi) 2020:Q1
Δ Credit Line Usage	-1.81***	-1.81***	-2.61***	-3.19***	-3.72**	-1.54**
	(0.55)	(0.51)	(0.69)	(0.73)	(1.36)	(0.62)
Δ Deposits						0.19
						(0.17)
Fixed Effects						
Maturity		✓				
Purpose			✓			
Bank Controls			✓			✓
R-squared	0.01	0.01	0.02	0.01	0.01	0.01
Observations	1,678	1,596	1,010	1,519	1,390	1,638
Number of Firms	749	712	464	682	624	733
Number of Banks	28	28	27	28	28	26

Notes: Estimation results for regressions (5.2) that omit the firm fixed effect. The dependent variable is given by changes in credit between 2019:Q4 and 2020:Q1 in columns (i)-(iii) and (vi), from 2019:Q4 to 2020:Q2 in column (iv), and between 2019:Q4 and 2020:Q3 in column (v). The regressors "Δ Credit Line Usage" and "Δ Deposits" denote the change of a bank's used existing credit lines or deposits from 2019:Q4 to 2020:Q1, relative to total assets in 2019:Q4. Column (ii) includes a fixed effect that varies by the remaining maturity and column (iii) includes a fixed effect that varies by the loan purpose. Maturity fixed effects take the form of three bins according to their remaining maturity in 2019:Q4: (i) less than one quarter, (ii) less than one year, and (iii) more than one year. Columns (iii) and (vi) include various bank controls for 2019:Q4: bank size (natural log of total assets), return on assets (net income/total assets), leverage (total liabilities/total assets), deposit share (total deposits/total assets), and banks' income gap (see Appendix Table C.5 for details on the data). All specifications are estimated using OLS. Standard errors in parentheses are clustered by bank. Sample: 2019:Q4 - 2020:Q3. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table F.4: COVID-19 Credit Supply − Alternative Fixed Effects.

	(i)	(ii)	(iii)	(iv)
Δ Credit Line Usage	-0.86***	-0.87***	-0.97**	-1.63***
	(0.30)	(0.26)	(0.36)	(0.56)
Fixed Effects				
Rate	✓	✓		
Rate × Industry × Location			✓	
Rate × Industry × Location × Size				✓
Single Lender		✓		
R-squared	0	0	0.44	0.49
Observations	31,246	23,444	11,399	5,266
Number of Firms	28,569	23,444	9,569	3,538
Number of Banks	29	29	29	29

Notes: Estimation results for regressions (5.2), where the dependent variable is given by changes in credit between 2019:Q4 and an average across non-missing observations for 2020:Q1-Q3. The regressor "Δ Credit Line Usage" denotes the change of a bank's used existing credit lines from 2019:Q4 to 2020:Q1, relative to total assets in 2019:Q4. All regressions omit firm-specific fixed effects and include single bank-firm relations. Columns (i) and (ii) include fixed effects that vary by rate type (adjustable- or fixed-rate). Columns (iii) and (iv) additionally allow the fixed effects to vary by industry (two-digit NAICS code), location (zip code of a firm's headquarter), and ten equally sized groups of the distribution of total observed credit to proxy for firm size. Column (ii) considers only firms with a single lender within our data. All specifications are estimated using OLS. Standard errors in parentheses are clustered by bank. Sample: 2019:Q4 - 2020:Q3. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table F.5: COVID-19 − Credit Supply in 2020:Q2.

	(i)	(ii)	(iii)	(iv)
Δ Credit Line Usage	-3.03**	-3.40***	-4.21***	-2.69**
	(1.14)	(1.05)	(1.26)	(1.05)
Δ Deposits				0.18
				(0.26)
Fixed Effects				
Firm × Rate	✓			✓
Firm × Rate × Maturity		✓		
Firm × Rate × Purpose			✓	
Bank Controls			✓	✓
R-squared	0.51	0.52	0.54	0.51
Observations	1,519	1,472	914	1,483
Number of Firms	682	661	421	668
Number of Banks	28	28	27	26

Notes: Estimation results for regressions (5.2), where the dependent variable is given by changes in credit between 2019:Q4 and 2020:Q2. The regressors "Δ Credit Line Usage" and "Δ Deposits" denote the change of a bank's used existing credit lines or deposits from 2019:Q4 to 2020:Q1, relative to total assets in 2019:Q4. All regressions include firm-specific fixed effects that additionally vary by rate type (adjustable- or fixed-rate) and the remaining maturity (column (ii)) or the loan purpose (column (iii)). Maturity fixed effects take the form of three bins according to their remaining maturity in 2019:Q4: (i) less than one quarter, (ii) less than one year, and (iii) more than one year. Columns (iii) and (iv) include various bank controls for 2019:Q4: bank size (natural log of total assets), return on assets (net income/total assets), leverage (total liabilities/total assets), deposit share (total deposits/total assets), and banks' income gap (see Appendix Table C.5 for details on the data). All specifications are estimated using OLS. Standard errors in parentheses are clustered by bank. Sample: 2019:Q4 - 2020:Q2. $^{***}p < 0.01$, $^{**}p < 0.05$, $^*p < 0.1$.

Table F.6: COVID-19 − Credit Supply in 2020:Q3.

	(i)	(ii)	(iii)	(iv)
Δ Credit Line Usage	-3.63**	-3.72**	-6.03***	-3.17*
	(1.62)	(1.66)	(1.46)	(1.54)
Δ Deposits				0.36
				(0.31)
Fixed Effects				
Firm × Rate	✓			✓
Firm × Rate × Maturity		✓		
Firm × Rate × Purpose			✓	
Bank Controls			✓	✓
R-squared	0.53	0.53	0.55	0.53
Observations	1,390	1,358	845	1,355
Number of Firms	624	610	390	610
Number of Banks	28	28	27	26

Notes: Estimation results for regressions (5.2), where the dependent variable is given by changes in credit between 2019:Q4 and 2020:Q3. The regressors "Δ Credit Line Usage" and "Δ Deposits" denote the change of a bank's used existing credit lines or deposits from 2019:Q4 to 2020:Q1, relative to total assets in 2019:Q4. All regressions include firm-specific fixed effects that additionally vary by rate type (adjustable- or fixed-rate) and the remaining maturity (column (ii)) or the loan purpose (column (iii)). Maturity fixed effects take the form of three bins according to their remaining maturity in 2019:Q4: (i) less than one quarter, (ii) less than one year, and (iii) more than one year. Columns (iii) and (iv) include various bank controls for 2019:Q4: bank size (natural log of total assets), return on assets (net income/total assets), leverage (total liabilities/total assets), deposit share (total deposits/total assets), and banks' income gap (see Appendix Table C.5 for details on the data). All specifications are estimated using OLS. Standard errors in parentheses are clustered by bank. Sample: 2019:Q4 - 2020:Q3. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table F.7: COVID-19 Credit Supply − Sample Splits.

	(i) Fixed-Rate	(ii) Adj.-Rate	(iii) Small Loans	(iv) Large Loans	(v) Non-Synd.	(vi) Synd.
Δ Credit Line Usage	-3.53**	0.14	-3.19***	0.03	-3.11**	-0.29
	(1.29)	(0.97)	(1.11)	(1.67)	(1.18)	(1.98)
Fixed Effects						
Firm	✓	✓				
Firm × Rate			✓	✓	✓	✓
R-squared	0.5	0.55	0.49	0.68	0.48	0.67
Observations	1,312	366	1,270	165	1,378	186
Number of Firms	587	166	573	77	618	82
Number of Banks	22	26	26	22	23	21

Notes: Estimation results for regressions (5.2), where the dependent variable is given by changes in credit between 2019:Q4 and an average across non-missing observations for 2020:Q1-Q3. The regressor "Δ Credit Line Usage" denotes the change of a bank's used existing credit lines from 2019:Q4 to 2020:Q1, relative to total assets in 2019:Q4. All regressions include firm-specific fixed effects that additionally vary by rate type (adjustable- or fixed-rate) in columns (iii) - (vi). Columns (i) and (ii) split the sample into fixed-rate and adjustable-rate loans. Columns (iii) and (iv) divide the sample into small and large loans according to the threshold between the bottom 90 percent and the top 10 percent of the unconditional loan size distribution in 2019:Q4. Columns (v) and (vi) split the sample into non-syndicated and syndicated loans. All specifications are estimated using OLS. Standard errors in parentheses are clustered by bank. Sample: 2019:Q4 - 2020:Q3. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table F.8: COVID-19 Credit Supply − Bank Capital.

	(i) 2020:Q1	(ii) 2020:Q2	(iii) 2020:Q3
Δ Credit Line Usage	-5.16***	-8.72***	-13.95***
	(1.15)	(1.94)	(3.08)
Δ Credit Line Usage × Cap-Buffer	1.29**	2.30**	3.88***
	(0.52)	(0.88)	(1.34)
Cap-Buffer	-0.01**	-0.02**	-0.04***
	(0.01)	(0.01)	(0.02)
Fixed Effects: Firm × Rate	✓	✓	✓
Bank Controls	✓	✓	✓
R-squared	0.51	0.51	0.53
Observations	1,650	1,493	1,365
Number of Firms	738	672	614
Number of Banks	27	27	27

Notes: Estimation results for regressions (5.2), where the dependent variable is given by changes in credit between 2019:Q4 and 2020:Q1 (column (i)), 2020:Q2 (column (ii)), or 2020:Q3 (column (iii)). The regressor "Δ Credit Line Usage" denotes the change of a bank's used existing credit lines from 2019:Q4 to 2020:Q1, relative to total assets in 2019:Q4. "Cap-Buffer" denotes banks' voluntary capital buffers in 2019:Q4, which are defined as the difference between common Tier 1 capital ratios and (4.5 + SCB + GSIB) where SCB is the stress capital buffer and GSIB is the additional buffer for global systemically important banks. "Δ Credit Line Usage × Cap-Buffer" denotes the interaction between the two variables. All specifications include various bank controls for 2019:Q4: bank size (natural log of total assets), return on assets (net income/total assets), leverage (total liabilities/total assets), deposit share (total deposits/total assets), and banks' income gap (see Appendix Table C.5 for details on the data). Standard errors in parentheses are clustered by bank. Sample: 2019:Q4 - 2020:Q3. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table F.9: COVID-19 Credit Supply − IV-Estimation & Portfolio Losses.

	Baseline (i)	IV-Estimation (ii)	Portfolio Losses (iii)
Δ Credit Line Usage	-2.47**	-2.90*	-2.48***
	(1.07)	(1.68)	(0.88)
Δ Probability Default			0.02
			(0.72)
Δ Provision Losses			3.81
			(9.07)
Fixed Effects: Firm × Rate	✓	✓	✓
Bank Controls	✓	✓	✓
Estimator	OLS	IV	OLS
First Stage F-Stat.		31	
R-squared	0.5	0.5	0.5
Observations	1,650	1,650	1,650
Number of Firms	738	738	738
Number of Banks	27	27	27

Notes: Estimation results for regressions (5.2), where the dependent variable is given by changes in credit between 2019:Q4 and an average across non-missing observations for 2020:Q1-Q3. Column (i) reports the baseline specification corresponding to column (i) in Table 5.1. The regressor "Δ Credit Line Usage" denotes the change of a bank's used existing credit lines from 2019:Q4 to 2020:Q1, relative to total assets in 2019:Q4, and is instrumented with a bank's ratio of unused credit commitments relative to assets in 2019:Q4 in column (ii). The regressor "Δ Probability Default" denotes the reported change in the probability of default of a bank's existing term loan portfolio between 2019:Q4 and 2020:Q1, relative to total assets in 2019:Q4 and excluding the loan related to the dependent variable. The regressor "Δ Provision Losses" denotes the change in the provision for loan and lease losses reported in banks' income statement between 2019:Q4 and 2020:Q1, relative to total assets in 2019:Q4. All specifications include various bank controls for 2019:Q4: bank size (natural log of total assets), return on assets (net income/total assets), leverage (total liabilities/total assets), deposit share (total deposits/total assets), and banks' income gap (see Appendix Table C.5 for details on the data). Standard errors in parentheses are clustered by bank. Sample: 2019:Q4 - 2020:Q3. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table F.10: COVID-19 − Total Debt & Cash.

	Δ Total Debt		Δ Cash		
	All (i)	Small/Large (ii)	All (iii)	Small (iv)	Large (v)
Δ Credit Line Usage	-2.63***				
	(0.69)				
Δ Credit Line Usage × Small		-2.61***			
		(0.68)			
Δ Credit Line Usage × Large		0.97			
		(5.72)			
Δ Total Debt			0.07***	0.07***	-0.87
			(0.01)	(0.01)	(0.78)
Industry Fixed Effects	✓	✓	✓	✓	✓
Firm Controls	✓	✓	✓	✓	✓
Estimator	OLS	OLS	IV	IV	IV
First Stage F-Stat.			233	167	10.9
R-squared	0.08	0.08	0.02	0.18	0.02
Observations	3,164	3,164	3,163	1,572	1,590
Number of Firms	3,164	3,164	3,163	1,572	1,590
Number of Banks	28	28	28	23	28

Notes: Columns (i) and (ii) report estimation results for regressions (5.3). "Δ Credit Line Usage" denotes the estimation results for β. Column (ii) additionally distinguishes the effect by firm size, where a large firm is defined as one with total assets within the top 20 percent of the firm size distribution in 2019 and the indicator variable is included in the set of firm controls. Columns (iii)-(v) report estimation results for the instrumental variable regressions $(Cash_{i,t+1} - Cash_{i,t-3})/Assets_{i,t-3} = \tilde{\alpha}_m + \tilde{\beta} \cdot 2\left(D_{i,t+1} - D_{i,t-1}\right)/\left(D_{i,t+1} + D_{i,t-1}\right) + \tilde{\gamma} X_{i,t-1} + \tilde{u}_{i,t+1}$, where $Cash_{i,t}$ denotes cash holdings at time t and the timing is chosen to match the one in Table 5.2. The variables $\sum_{j=1}^{J} \omega_{i,t-1}^{j}(\Delta Credit\ Line\ Usage_t^j)/Assets_{t-1}^j$ and $\sum_{j=1}^{J} \omega_{i,t-1}^{j}$ are used as instruments for $2\left(D_{i,t+1} - D_{i,t-1}\right)/\left(D_{i,t+1} + D_{i,t-1}\right)$. "Δ Total Debt" denotes the estimation results for $\tilde{\beta}$. $X_{i,t-1}$ includes $\sum_{j=1}^{J} \omega_{i,t-1}^{j}$ in columns (i) and (ii) and various firm indicators in 2019:Q2: net income, cash, tangible assets, total liabilities (all relative to total assets), firm size (natural log of total assets), and a binary variable that indicates whether a firm is publicly traded. Columns (iv) and (v) restrict the sample by firm size following the definition in column (ii). All specifications include industry fixed effects (two-digit NAICS code). Standard errors in parentheses are clustered by the bank with the largest term loan to firm i. Sample: 2019:Q2 - 2020:Q2. $^{***}p < 0.01$, $^{**}p < 0.05$, $^*p < 0.1$.
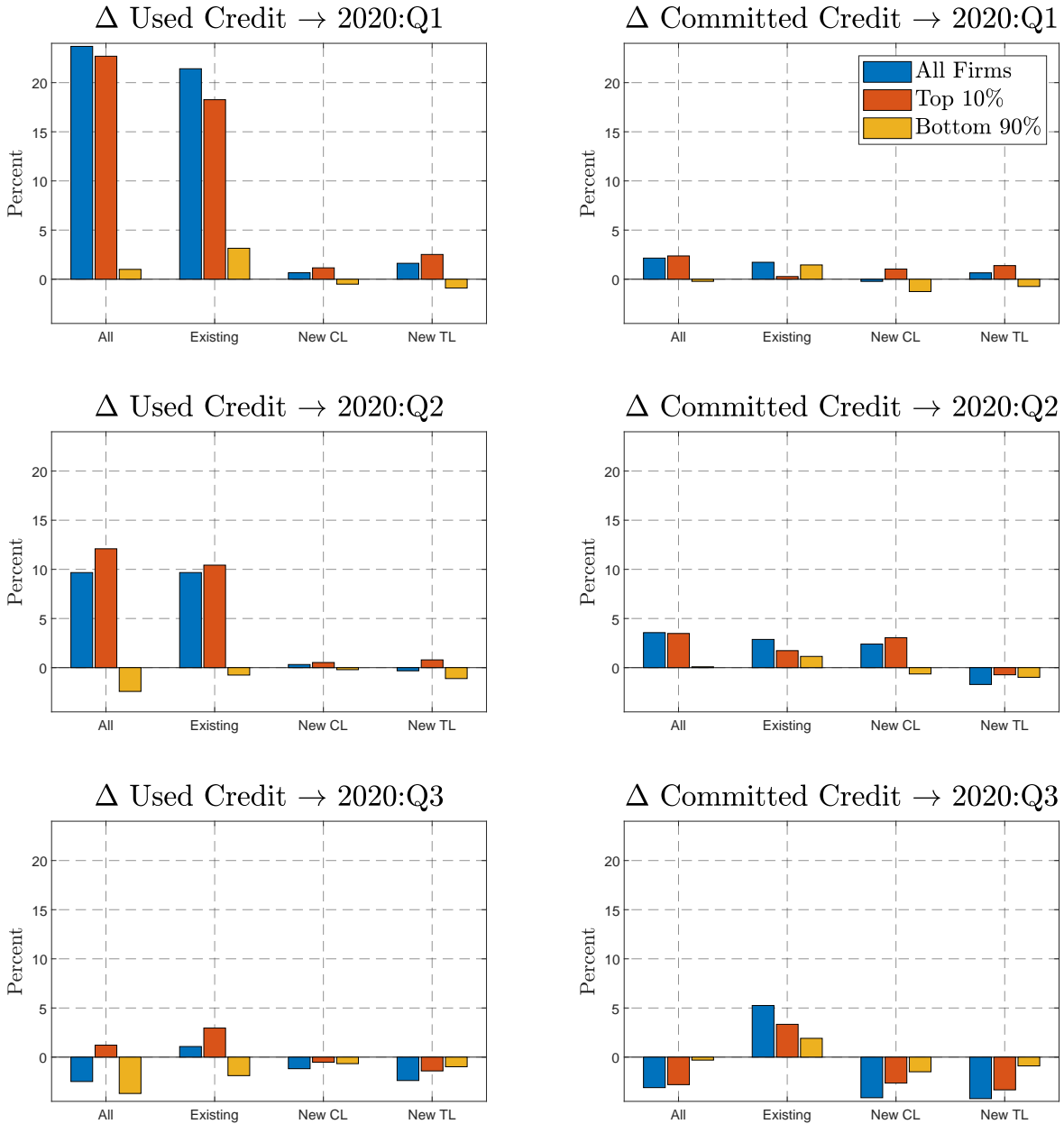
Figure F.7: Changes in Used and Committed Credit for 2019:Q4 - 2020:Q3.

Notes: The blue bars show aggregate changes in used and committed credit across all banks and firms from 2019:Q4 to 2020:Q1 (top), 2020:Q2 (middle), or 2020:Q3 (bottom), all relative to total used credit in 2019:Q4. The orange and yellow bars display equivalent changes for the top 10 percent and the bottom 90 percent of the firm size distribution, also relative to total used credit in 2019:Q4. The changes are further separated into differences in existing credit ("Existing"), new credit line issuances ("New CL"), and new term loans ("New TL"), all in percent relative to all used credit in 2019:Q4. The firm size distribution is computed according to firms' total assets in 2019:Q4 for each quarter.