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Credit Supply Shocks and Prices: Evidence from Danish Firms†

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We study the response of firms' output prices to a cut in credit supply. We combine data on loans between Danish firms and banks with survey-based producer prices and transaction-based export unit values. Exploiting banks' heterogeneous exposure to the global financial crisis, we show that loans to firms with relationships to exposed banks drop and lending rates increase. In response, firms raise prices by 3–5 percent. This effect is decreasing in the elasticity of firms' demand but positive for most industrial production. Our results show that firms increase prices to raise cash when external sources of liquidity dry up. (*JEL* D22, E23, E31, E32, E44, G01, G21)

In this paper, we study the relationship between loan supply and firms' output prices in the aftermath of the 2007–2008 global financial crisis. During the Great Recession triggered by the financial crisis, inflation did not fall as much as many economists would have expected based on the historic relationship between output and prices.[1] A natural explanation for this "missing disinflation" is that the financial crisis had a direct impact on prices through bank lending markets. Such a channel is an important component of efforts to model price developments over the Great Recession (see Christiano, Eichenbaum, and Trabandt 2015; Gilchrist et al. 2017), but direct evidence on the effect of loan supply on prices remains scarce.

We aim to provide such evidence in this paper. We identify the causal effect of loan supply on prices using a strategy based on firms' preexisting relationships with banks who are more or less exposed to the financial crisis, depending on banks' precrisis

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[1] See, e.g., Hall (2011) for a discussion of the US case, Friedrich (2016) for a cross-country perspective, and online Appendix D.1 for the Danish case.

funding strategy through either deposits or wholesale interbank loans. Our empirical analysis is based on a new dataset covering all loan relationships between Danish firms and banks, combined with price information from producer price index (PPI) survey data and transaction-based export unit values. We show that the loan balances of firms with preexisting relationships with "wholesale-funded" banks decrease sharply after 2007, while the interest rates they pay on the remainder increase. At the same time, these firms raise their domestic and export prices by 5 percent and 3 percent relative to other firms in the same sector. Our IV estimates of the loan supply elasticities of domestic and export prices amount to between −0.06 and −0.26. We find that the short-term profitability of exposed firms increases, while their longer-run market share falls. Overall, our results are consistent with an important role of the liquidity channel of Gilchrist et al. (2017), who suggest that liquidity-constrained firms trade off short-term profits against longer-term market share.

Our identification strategy follows Jensen and Johannesen (2017) and is similar to identification schemes based on US banks' exposure to the financial crisis, such as Chodorow-Reich (2014). Like Jensen and Johannesen, we distinguish between banks who rely more on deposits ("deposit funded") and banks who rely more on interbank wholesale markets ("wholesale funded") for funding before the financial crisis. After the interbank market froze up at the end of 2007, wholesale-funded banks faced a severe funding shortage and reduced their loan supply relative to deposit-funded banks. This results in substantial variation in firms' access to credit over the crisis depending on their precrisis banking relationships. We find that after 2007, the loan balances of firms with precrisis relationships with wholesale-funded banks decrease by roughly 20–30 percent, while the interest rate paid on the remainder increases by up to 0.5 percentage points (pp) relative to other firms.

The main challenge to our identification strategy is the possibility of sorting between firms and banks—wholesale-funded banks could attract customer firms who are more exposed to the Great Recession through other channels as well. We address this concern in several ways: first, we show that firms borrowing from deposit-focused and wholesale-funded banks are very similar in terms of observable characteristics. Second, we show that prior to the crisis, price and loan outcomes of both groups of firms align very closely. Third, we show that our results are robust to controlling for the dynamic impact of firm characteristics over the crisis by controlling for the dynamic impact of ex ante selected controls and picking controls using the PDSLASSO method proposed by Belloni, Chernozhukov, and Hansen (2014) and Belloni et al. (2017) in robustness checks.

Our main contribution to the existing literature are causally identified estimates of the effect of loan supply on producer prices of a diverse and broadly representative sample of firms. Kim (2021) uses a similar identification strategy to identify the effect of credit supply on prices of US consumer packaged goods producers—mostly large food manufacturing firms—and finds that a negative credit supply shock leads to lower prices in this sample. Our analysis is based on prices of firms that participate in the Danish PPI survey—which is representative of the sectoral composition of Danish manufacturing—and export unit values for the universe of Danish exporters in manufacturing sectors. In contrast to Kim (2021), we find that negative credit supply shocks lead to price increases. We reconcile these two different results through differences in the elasticity of market demand between consumer packaged goods producers and manufacturing overall. We show that the price effect of a negative credit supply shock decreases with the elasticity of market demand, and that consumer packaged goods producers face much more elastic demand than manufacturing firms overall. For the high demand elasticities faced by consumer packaged goods producers, our estimates imply a modestly negative effect consistent with Kim (2021). However, given the distribution of demand elasticities in Danish and European manufacturing, our estimates imply that negative credit supply shocks lead to price increases for most firms.

Our results are consistent with the evidence from Gilchrist et al. (2017), who show that US manufacturing firms with lower initial liquidity buffers (who are more likely to hit liquidity constraints) increase prices relative to other firms over the course of the Great Recession. They are also consistent with Montero and Urtasun (2021), who show that estimated markups of Spanish manufacturing firms in sectors with higher initial average debt-to-cash flow ratios increase over the Great Recession, and with Duca et al. (2018), who provide survey evidence that Italian firms that report financial constraints are also more likely to report increases in markups. These contributions present results consistent with ours, but they are subject to important endogeneity concerns that our identification strategy is designed to address: initial liquidity reserves and debt correlate with access to external financing (see, e.g., Bates, Kahle, and Stulz 2009), and firms that hold little liquidity or high debt initially may also have easier access to credit during the financial crisis.

Our second contribution is evidence of the relevance of different channels behind the overall positive effect we find. The two mechanisms consistent with a positive relationship between credit supply and prices are the "liquidity channel" of Gilchrist et al. (2017); Chevalier and Scharfstein (1996); and Gottfries (1991) and the "working capital channel" (see, e.g., Christiano and Eichenbaum 1992; Bigio 2015; Christiano, Eichenbaum, and Trabandt 2015). The working capital channel is based on the idea that when firms have to prefinance part of their variable production cost, an increase in interest rates can affect marginal cost beyond what is implied by the rental cost of physical capital. Such a cost increase would then be passed through into prices. The "liquidity channel" is based on the idea that firms operate in markets where short-run demand elasticities are lower than the longer-run demand elasticities—for example, due to habits or search frictions. In this context, firms can raise internal liquidity by increasing their markups when external credit becomes more costly or difficult to obtain. This leads to an increase in short-run profits at the cost of a loss of future market share. Our results suggest an important role for the liquidity channel: we find that the gross profit margin of firms exposed to a negative credit supply shock increases by around 3 pp, while their European market share decreases by about 10 percent in the longer run. This is inconsistent with the pass-through of higher working capital cost alone, which would imply constant (full pass-through) or lower (incomplete pass-through) profitability. We do find that exposed firms that use more working capital initially raise their prices more strongly than other firms, suggesting that pass-through of working capital cost adds to the liquidity channel. However, in back-of-the-envelope calculations, we can attribute at most one-tenth of the price increase to a higher cost of working capital.

In addition to the microeconometric evidence already discussed above, our results relate to recent macroeconometric work that has integrated measures of financial shocks into structural VARs. The results in this literature are mixed and depend on the identifying restrictions imposed on the data. For example, Gilchrist and Zakrajšek (2012) find insignificantly negative effects of a financial shock on inflation in a VAR identified through imposing a zero effect on impact. Several other papers identify the effects of financial shocks on inflation through sign restrictions on the relationship between output and inflation (Hristov, Hülsewig, and Wollmershäuser 2012; Darracq-Paries and De Santis 2015; Gambetti and Musso 2017; Furlanetto, Ravazzolo, and Sarferaz 2019). While the main focus of these papers lies in estimating the impact of financial shocks on output, they also tend to find that financial shocks decrease inflation. In contrast, Abbate, Eickmeier, and Prieto (2023) present evidence based on a structural VAR that leaves the response of inflation unrestricted and find that negative financial shocks raise prices. Compared to the macroeconometric approach, we do not rely on structural restrictions but instead work under the assumption that precrisis banking relationships are independent of exposure to the Great Recession through nonfinancial channels.

Our results on the relationship between loan supply and prices also contribute more broadly to understanding the unusual price dynamics over the Great Recession. We show that in a partial equilibrium counterfactual scenario in which the credit supply of all Danish banks mirrors the credit supply of the deposit-funded banks less exposed to the financial crisis, the aggregate PPI would have fallen about 3 percent below its actual values over the 2008–2010 period. This brings the behavior of aggregate prices closer to a conditional forecast based on a simple VAR estimated on precrisis data and can explain some of the missing disinflation in Denmark. Moreover, our results contribute to the discussion of the interaction between macroprudential policy measures, traditional monetary policy, and real economic activity. In the aftermath of the financial crisis, Farhi and Werning (2016) and Korinek and Simsek (2016) argue that policies that limit borrowing during a boom and support lending during recessions could improve welfare due to aggregate demand externalities. We show that such policies can be in conflict with price stability targets of traditional monetary policy: restricting credit availability can be inflationary during a boom, and supporting credit supply during the bust enhances deflationary tendencies.

The rest of the paper is structured as follows. In Section I we present the data we use in our analysis. We discuss estimation and identification assumptions in Section II. Section III shows that our measure of exposure to bank-level shocks substantially affected loan balances and interest rates paid by firms. In Section IV we present the main results on the effects on prices. In Section V we discuss evidence for the importance of the liquidity and working capital channels. Section VI discusses the aggregate importance of our results. Finally, Section VII concludes the paper.

I. Data

Our analysis is based on several administrative and survey-based datasets collected by Statistics Denmark. The core data match the universe of bank loans between Danish banks and firms with banks' balance sheets on the one hand and manufacturing firms' output prices on the other hand.

A. *Loan Data*

Our data on lending is based on Danish banks' annual account-level reports of all loans and deposits to the Danish Tax Authority. We use the part of this dataset that covers firms (Statistics Denmark 2022e). All bank loan relationships are reported, including regular loans, syndicated loans, credit card debt, and accounts with variable utilization such as revolving loans or overdraft deposit accounts. The notable exception are mortgages, which in Denmark are provided outside the banking system by specialized mortgage institutions.[2] The primary variables reported by banks are the account balance as of December 31 and interest paid over the year. Loan accounts can be linked to banks as well as the borrowing firms through bank register numbers and unique identifiers of all firms taxed in Denmark.

We combine this dataset with bank balance sheet data provided by the Danish Financial Supervisory Authority ("Finanstilsynet," Finanstilsynet 2022) and the Monetary and Financial statistics of the Danish central bank (Danmarks Nationalbank 2022), which cover loans and deposits for each bank in total and by sector. The sum of loans in the microdata closely follows aggregate bank lending computed from balance sheets for the 2005–2010 period, as we show in Figure A.2 in the online Appendix.

In addition to outstanding loans and interest payments, the loan data also cover loan maturity and the contractual interest rate for some observations. However, these variables are not systematically reported by most banks. We therefore calculate an average interest rate for each firm i from end-of-year loan balances and total interest payments over a year:

$$i_{i,t} = \frac{Interest\_payments_{i,t}}{\frac{1}{2}\left(Loans_{i,t-1} + Loans_{i,t}\right)}. \tag{1}$$

For low- and medium-level interest rates, this average interest rate measure lines up well against the contractual rates when both are available in the data (Figure A.3 in the online Appendix). It fails to capture very high interest rates above 10 percent, which are typically associated with short-run loans—such as overdraft accounts and credit cards—that are often settled within a year and hence not adequately captured by the average end-of-year balance we use in our calculation. The coefficient from

[2] Danish mortgage institutions are highly regulated and fund themselves through bonds that exactly match the maturity of their mortgages. As a result, they did not experience a funding shortage comparable to the Danish banking sector.

a regression of average interest rate measure on the contractual interest rate is 0.91 for contractual interest rates below 10 percent and 0.41 overall.

B. *Price Data*

Producer Price Index Survey: We combine the loan data with two sources of price data. First, we use survey data underlying the PPI (Statistics Denmark 2022d). The Danish PPI microdata provide a very clear picture of the price developments of large Danish manufacturing firms, though with the disadvantage that they covers relatively few firms. They are based on a monthly survey in which firms report prices for a persistent selection of their product portfolios. On average, the data cover about 3,500 price quotes from about 500 firms. Products are classified using eight-digit Harmonized System codes. Firms also report whether goods are sold domestically or exported, and in our baseline results we only include domestic prices. The reported prices are transaction prices in Danish kroner and include temporary sales and discounts.[3] The survey is designed to allow adjustments for quality changes and product substitutions. The dataset is strongly balanced with very few gaps in price series. We perform quality adjustments and winsorize price changes at ± 1 log points in the monthly data.[4] We then transform the dataset to quarterly frequency by keeping the price in the first month of each quarter. The Danish PPI survey has been previously used in Dedola, Kristoffersen, and Züllig (2019), who provide important price-setting moments and show that the data are comparable to other European producer price datasets.

Export Unit Values: To complement the PPI data, we use export unit values calculated from data collected by Danish customs (Statistics Denmark 2022f). Compared to prices reported in the PPI data, unit values are a relatively noisy measure of prices. However, they are available for all goods exporters above a small annual export threshold. Customs data cover firms' export sales and quantities at the level of destination countries and eight-digit Combined Nomenclature (CN) product codes. The data are reported at a monthly frequency. We sum sales and quantities over countries and months and calculate yearly unit values for eight-digit CN categories c for each firm:

$$P_{i,c,t} = \frac{Sales_{i,c,t}}{Quantity_{i,c,t}}. \tag{2}$$

The CN classification is subject to frequent adjustment of product categories. We construct unit value indices for each firm and two-digit CN category based on consistent combinations of eight-digit CN categories. We describe this procedure in more detail in online Appendix A. These unit value indices are a noisy measure of

[3] When applying the sales filter of Nakamura and Steinsson (2008), we detect that a share of 0.31 percent of price observations and 3.5 percent of price decreases are sales. Therefore, sales are not a prominent feature in the Danish PPI data.

[4] This truncates 0.61 percent of price changes in the raw data.

underlying prices: there may be unobserved composition changes within reported eight-digit category sales, and firms may misclassify some products in some years. In Figure B.1 in the online Appendix, we benchmark changes in export unit value indices against changes in PPI prices in the same CN2 category for firms that appear in both datasets. Summing over the contemporaneous coefficient plus one lag and lead, the correlation between PPI prices and export unit values amounts to roughly 0.4.

C. *Other Data*

In addition to price and lending data, we use several other register datasets. First, we use the Danish firm register, which contains yearly data on industry codes, legal form, firm age, total wage bill, and employment for each firm active in Denmark (Statistics Denmark 2022c). Second, we use the Danish accounting statistics (Statistics Denmark 2022b) and sales from VAT data (Statistics Denmark 2022a). These annual datasets contain important balance sheet items such as sales, profits and total assets for most Danish firms. In addition, the accounting statistics contain more detailed balance sheet items such as inventories based on a survey taken by a large sample of Danish firms.

D. *Sample*

The starting point for our sample is the 2007 Danish firm register, which includes the population of Danish firms. It contains 7,281 active manufacturing firms, which account for about 24 percent of private sector employment. Based on practical considerations and our identification strategy, we impose several restrictions on this population—Table A.1 in the online Appendix illustrates the bite of each restriction. We exclude small firms with fewer than ten employees or less than 1,000,000 DKK (roughly 135,000 EUR) in sales, and condition on continuous activity between 2005 and 2010. Since our identification strategy requires an active lending relationship with at least one bank, we also exclude firms with bank loans of less than 1 percent of sales in 2007, or less than 100,000 DKK (about 13,500 EUR) in 2006 or 2007. This leaves us with 1,753 firms that represent 47 percent of manufacturing employment. Of these firms, we can link 213 firms to the PPI survey and 1,176 to export data. These matched firms account for 21 percent and 42 percent of total manufacturing employment.

In Table 1 we present a summary of firm characteristics in the matched PPI and export unit value datasets, compared to the full population of manufacturing firms that fulfill all sample restrictions. Firms in the PPI dataset are substantially larger and older than firms in the larger unit value dataset, who in turn are slightly larger and older than the population. Firms' reliance on bank loans, measured as percentages of sales or overall debt, is similar across the samples. Even though many firms have multiple banking connections, the share of a firm's primary bank in precrisis lending is high (around 0.85) and similar in all three groups, and even firms with multiple loan relationships should thus still be affected by a decrease in their primary bank's loan supply.

Table 1—2007 Sample Characteristics

	All firms		PPI match		Export unit values match	
	Mean	Median	Mean	Median	Mean	Median
Employment	98.4	38.0	367.8	154.0	131.6	53.0
Ann. employment growth 04–07 (percent)	6.5	3.0	2.7	0.8	5.5	2.6
Ann. employment growth 08–10 (percent)	−7.4	−7.1	−8.8	−7.9	−7.6	−7.1
Firm age (years)	20.7	18.7	30.2	26.8	22.6	20.3
Sales (mio DKK)	169.0	42.7	791.9	224.9	236.8	68.3
Ann. sales growth 04–07 (percent)	17.0	8.9	10.9	6.6	15.7	8.6
Ann. sales growth 08–10 (percent)	−1.9	−3.3	−3.0	−3.6	−1.8	−2.9
Profits (percent of sales)	5.4	5.5	5.2	4.5	5.2	5.2
Bank loans (percent of sales)	18.9	14.0	20.9	15.1	19.3	14.9
Bank loans (percent of debt)	47.7	44.0	47.7	42.5	47.7	43.6
Avg. interest rate (percent)	5.8	5.6	5.1	5.1	5.5	5.4
Bank connections (incl. deposits)	2.9	3.0	3.9	4.0	3.2	3.0
Bank connections (only loans)	2.3	2.0	3.0	3.0	2.6	2.0
Share of loans from prim. bank (percent)	88.3	99.2	82.3	91.6	86.1	98.0
Share of short-maturity loans (percent)	77.4	100.0	72.1	87.1	76.2	100.0
Equity share (percent)	29.1	28.4	35.8	34.7	31.2	31.0
Deposits (percent of sales)	2.4	0.3	2.4	0.5	2.4	0.4
Inventories (percent of sales)	14.3	12.5	17.5	15.5	16.6	14.8
Avg. ann. price change 04–07 (percent)			2.7	1.3	1.7	1.8
Avg. ann. price change 08–10 (percent)			1.8	0.8	2.2	1.4
Avg. demand elasticity			3.5	2.4	3.6	2.4
Observations	1,753		213		1,176	

Notes: Summary statistics for the population and the matched samples conditional on sampling restrictions. Unless stated otherwise, variables are measured in 2007. Growth rates of employment and sales are winsorized at the first and ninety-ninth percentile. The variables displayed in the last three rows are firm-level averages of good-level information. Demand elasticity denotes the estimated price elasticity of demand estimated for categories of goods by Broda and Weinstein (2006), which we will use in Section B.

II. Identification Strategy and Empirical Specification

Identification Strategy: Our identification strategy uses heterogeneous exposure of Danish banks to the 2007–2008 global financial crisis as a source of exogenous variation in the loan supply to borrower firms. During the early 2000s, aggregate lending of Danish banks grew rapidly (see Figure 1). Deposits were not growing at the same pace, and some banks relied heavily on funds borrowed in the interbank wholesale funding market to fund this expansion. When interbank lending markets froze up at the end of 2007, these "wholesale-funded" banks faced a severe funding shortage and were forced to cut their lending compared to "deposit-funded" banks. This pattern has been used in Jensen and Johannesen (2017) as a source of exogenous variation in the credit supply to Danish households. It is similar to identification strategies that use heterogeneity in US banks' exposure to the financial crisis as a source of variation in credit supply to US firms, such as Chodorow-Reich (2014).

We follow Jensen and Johannesen and divide Danish banks into a wholesale-funded and a deposit-funded group based on the ratio of loans on the asset side of their balance sheets to deposits on the liability side. We use the 2007 loans-weighted median loan-to-deposit ratio of 1.37 as a cutoff. Of the 145 banks in our sample in 2007, this puts 31 banks in the wholesale-funded and 131 banks in the deposit-funded group. Wholesale-funded banks include 2 out of the largest 5 and 9 out of the largest 15 banks

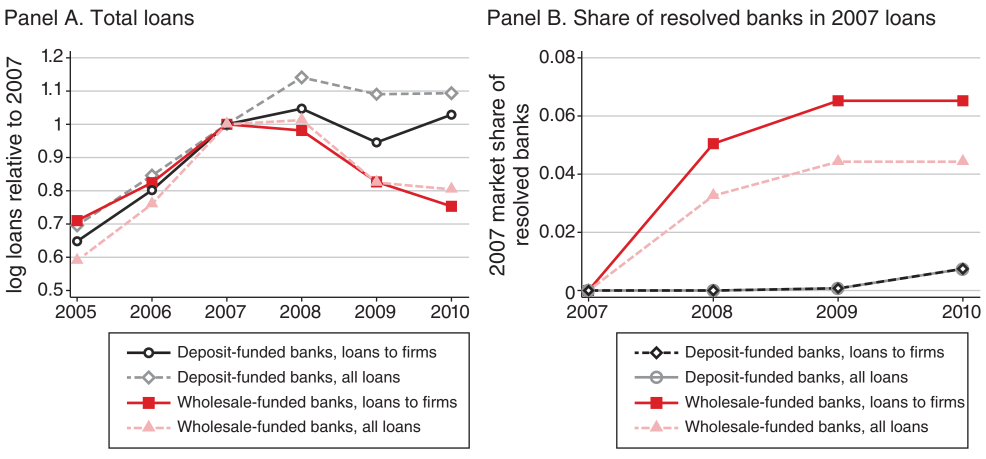

FIGURE 1. DEPOSIT-FUNDED BANKS VERSUS WHOLESALE-FUNDED BANKS

Notes: Panel A: Outstanding loans by banks with a 2007 loan-to-deposit ratio below (deposit-funded) and above (wholesale-funded) the loans weighted median, normalized to 2007. The solid lines sum loans to Danish nonfinancial firms, and the dashed lines sum all loans (including households). Panel B: 2007 market share of deposit- and wholesale-funded banks that are resolved by a given year. The solid lines sum over market shares in the market for loans to nonfinancial firms, the dashed lines in the overall loan market.

in terms of corporate lending. The difference in lending dynamics between the two groups is apparent in Figure 1, panel A. Aggregate lending of both groups grows rapidly in the years leading up to the crisis. Starting in 2008, lending of wholesale-funded banks drops relative to deposit-funded banks. This pattern is evident in total loans, as well as when only looking at loans to Danish nonfinancial firms. By the end of 2010, the gap in outstanding loans relative to 2007 amounts to about 25 percent. Moreover, as suggested by panel B, banks in the exposed group are substantially more likely to be resolved over the course of the crisis. By the end of 2010, banks from the exposed group accounting for about 10 percent of 2007 corporate loans have ceased to exist as independent entities or transferred substantial shares of their loan portfolio into bad banks. This includes the resolution of 4 out of the largest 15 banks. In contrast, bank failures in the deposit-funded group are rare, and the 2007 market share of resolved banks in that group amounts to roughly 1 percent.

Firm Exposure: Our main analysis proceeds at the firm level. To map bank-level to firm-level shocks, we define firms' exposure to loan supply shocks as the share of loans with wholesale-funded banks in their total 2007 bank lending relationships, i.e., the part of bank loans that is extended by high loan-to-deposit banks prior to the crisis.

$$(3) \qquad Exposure_i = \frac{\sum_{b \in B} Loans_{i,b,2007} \times Wholesale\text{-}funded_b}{\sum_{b \in B} Loans_{i,b,2007}}.$$

Firm exposure commonly takes values 0 or 1, but may take values in between as well since some firms have multiple banking relationships. Of the firms with price information, slightly below 35 percent have loans with wholesale-funded banks only, and slightly above 35 percent have loans with deposit-funded banks only. The distribution of exposure of the 30 percent remaining firms is roughly uniform. The full distribution of our exposure measure is shown in Figure A.4 in the online Appendix.

Several closely related papers, most importantly Chodorow-Reich (2014) and Kim (2021), use Bartik-style instruments that are constructed from the weighted average change in banks' lending or other measures of bank health during the financial crisis, where the weights reflect the importance of each bank in firms' precrisis loan relationships. We instead directly use variation in exposure (i.e what would be the weights in a Bartik instrument) as our source of identification. As discussed by Goldsmith-Pinkham, Sorkin, and Swift (2020) and Borusyak, Hull, and Jaravel (2022), the exclusion restrictions of Bartik-style instruments rely on either independence of the weights (i.e., our exposure measure) or a large number of independent aggregate (in this case bank-level) shocks. We think that the former assumption is appropriate in our case, and we prefer to directly use exposure as the basis for our estimation since mergers and absorption of parts of some banks' loan portfolio into bad banks make it difficult to construct loan supply measures for some of the most exposed banks after 2007.

Estimation: Our baseline estimates for price outcomes come from variants of the following dynamic difference-in-difference specification:

$$\text{(4)} \quad \log Price_{i,p,t} = \Lambda_{i,p} + \Gamma_{s(i),t} + \sum_{\substack{k=2005 \\ k\neq 2007}}^{2010} \mathbf{1}\{t = k\} \times \left(\beta_k Exposure_i + \gamma_k \mathbf{X}_i\right).$$

This specification estimates the dynamic effect of exposure of firm i on the price of product p relative to its value in the 2007 base period. We include observations from the 2005–2010 period and estimate one coefficient for each year except 2007. The price in the 2007 base period is absorbed in the firm-product fixed effect $\Lambda_{i,p}$.[5] We include sector-time fixed effects $\Gamma_{s(i),t}$ that ensure estimates are identified from variation of exposure within sectors. Moreover, in our main specification we control for dynamic effects of a number of constant 2007 firm characteristics $\mathbf{X}_i$ (see, e.g., Bentolila, Jansen, and Jiménez (2018) for a similar application). These controls include the average interest rate, the short-term loan share, and the deposit-to-sales and loans-to-sales ratios.

We estimate equation (4) using OLS for PPI prices and loan outcomes. For export unit values, we use an approach that puts less weight on more volatile series. Unit values are prices measured with error, and we expect the variance of this measurement error to vary between different series. For example, some firms may be more careful in correctly classifying their exports, while others may frequently misclassify products. Within-category composition changes—for example, between customers who are charged different prices—are another possible source of measurement

[5] For firm outcomes such as loans, the dependent variable is at the firm level, i.e., $Y_{i,t}$, and we include firm fixed effects instead.

error and likely affect lower-volume unit value series especially. Other contributions working with unit values, such as Broda and Weinstein (2006a) or Amiti, Itskhoki, and Konings (2019), weight unit value regressions by sales volume to deal with the resulting inefficiency. We implement an iterated FGLS estimator when using unit value outcomes: we estimate equation (4) using OLS first, calculate the variance of the regression residuals for each series, and then use the inverse variance as weight in the next iteration. We repeat this step until the weights converge. We present alternatives to this procedure in robustness checks discussed below—in general, alternative estimates are comparable in magnitude, but less precise than our FGLS estimates.

Our baseline results are reduced-form coefficients—i.e., we separately show that exposure has an effect on loan outcomes and prices by estimating equation (4) with credit and price outcomes. We also provide estimates of the elasticity of prices to the supply of loans. These estimates are obtained from instrumental variable estimates of the following specification:

$$\log Price_{i,p,post} \;=\; \Lambda_{i,p} + \Gamma_{s(i),post} + \beta \log Loans_{i,post} + \gamma \mathbf{X}_i. \tag{5}$$

We estimate this equation on samples including observations from the base period in 2007 and a post period that is either 2008, 2009, or 2010. We always use 2008 levels of loans for the post period; i.e., we estimate the dynamic response of prices over different horizons to the 2007–2008 drop in loans. We use $Exposure_i \times Post_t$ as an instrument for $\log Loans_{i,t}$. Since our instrument does not vary over time, we cannot clearly distinguish between time variation in the extent of the credit supply shock and a possibly delayed dynamic response to it.

Identification: A causal interpretation of our estimates requires exposure to other shocks that affect prices to be independent of firm exposure to wholesale-funded banks. This could be violated if there is sorting between banks and firms along observeable or unobserveable dimensions that correlate with exposure to the Great Recession through channels other than the supply of bank loans. Our dynamic diff-in-diff estimates show that there is no significant difference between exposed and nonexposed firms in the development of loan or price outcomes prior to 2007, and that firms behave very similarly during normal times.

Moreover, we show that exposed and nonexposed firms are very similar according to observable characteristics, and that the modest differences in firm characteristics do not impact firms' response to the credit supply shock. We show in Table A.2 in the online Appendix that there are no substantial differences in a large number of firm characteristics between firms who borrow from wholesale-funded or deposit-funded banks. There are some systematic differences between firms with partial exposure (i.e., exposure between 0.02 and 0.98) and firms with full ($>$ 0.98) or no exposure ($<$ 0.02). These differences arise since larger firms are more likely to have multiple banking relationships and hence do exhibit intermediate exposure. Therefore, we report Kolmogorov-Smirnov test statistics comparing the distribution of precrisis characteristics between firms with no or full exposure and, separately, firms with low (0.02–0.5) and high (0.5–0.98) partial exposure. With respect to most characteristics, the respective p-values do not indicate any systematic differences in firm characteristics by exposure.

The only consistently significant difference between exposed and nonexposed firms is the reported share of short-term loans in total loans—exposed firms seem to have longer-term loans than nonexposed firms. However, there are important inconsistencies in the maturity variable included in the loan data: except for the three largest banks, banks report all loans as short-term loans. It therefore seems likely that the distribution reflects differences in reporting standards between different banks rather than actual differences in loan maturity. We control for the dynamic impact of the short-term loan share in all our regressions. Since the terms of longer-term loans are more difficult to adjust, a lower short-term share of exposed firms would reduce the extent to which bank-level shocks are transmitted to exposed firms and reduce the power of our identification strategy, but not affect its validity.

In our baseline regressions, we control for three additional precrisis firm characteristics that exhibit modest differences of firms by exposure. The share of bank deposits in sales prior to 2008 is higher for firms with high partial exposure than those with low partial exposure, although the difference between firms with no and full exposure is small and of the opposite sign. Furthermore, we include the 2007 loans-to-sales ratio and the 2007 average interest rate as controls. The p-values for Kolmogorov-Smirnov tests of equality of distributions of these firm characteristics between firms with low and high exposure are 0.13 and 0.07, respectively. We show in numerous robustness checks that our results are not sensitive to the inclusion of additional controls. We estimate specifications that exclude all control variables, pick covariates from the firm characteristics in Table A.2 using the post-double selection LASSO[6] methodology proposed in Belloni, Chernozhukov, and Hansen (2014) and Belloni et al. (2017), or include all firm characteristics in Table A.2 as controls. Our results are not affected by any of these variations. Furthermore, all our regressions include sector-time fixed effects and coefficients are identified from within-sector variation, so modest differences in the distribution of firms over sectors (see Figure A.5 in the online Appendix) are not a concern for identification.

III. Cross-Sectional Variation in Loan Supply

Baseline "First Stage" Result: We first document the dynamic effect of firm exposure on loan outcomes. As expected from a negative credit supply shock, we find that firms' exposure has a large negative effect on outstanding loan balances and a large positive effect on the average interest rate paid on the remaining loans. We use the inverse hyperbolic sine (IHS) transform of loans as our baseline outcome to deal with zeros and right skewness of loan balances. Like with log outcomes, one can interpret estimates with IHS outcomes as approximate elasticities.[7] We find

[6] The PDSLASSO procedure includes controls if they predict firms exposure or if they predict the development of outcome variables over the 2005–2010 period.

[7] The inverse hyperbolic sine function (IHS) is defined as $\log\left(x + \sqrt{x^2+1}\right)$. For most of its range it is approximately equal to $IHS(X) \approx \log(2x)$, and regressions with IHS outcomes can be interpreted similarly to regressions with log outcomes. The advantage of the IHS is that it is defined at zero, with $IHS(0) = 0$.

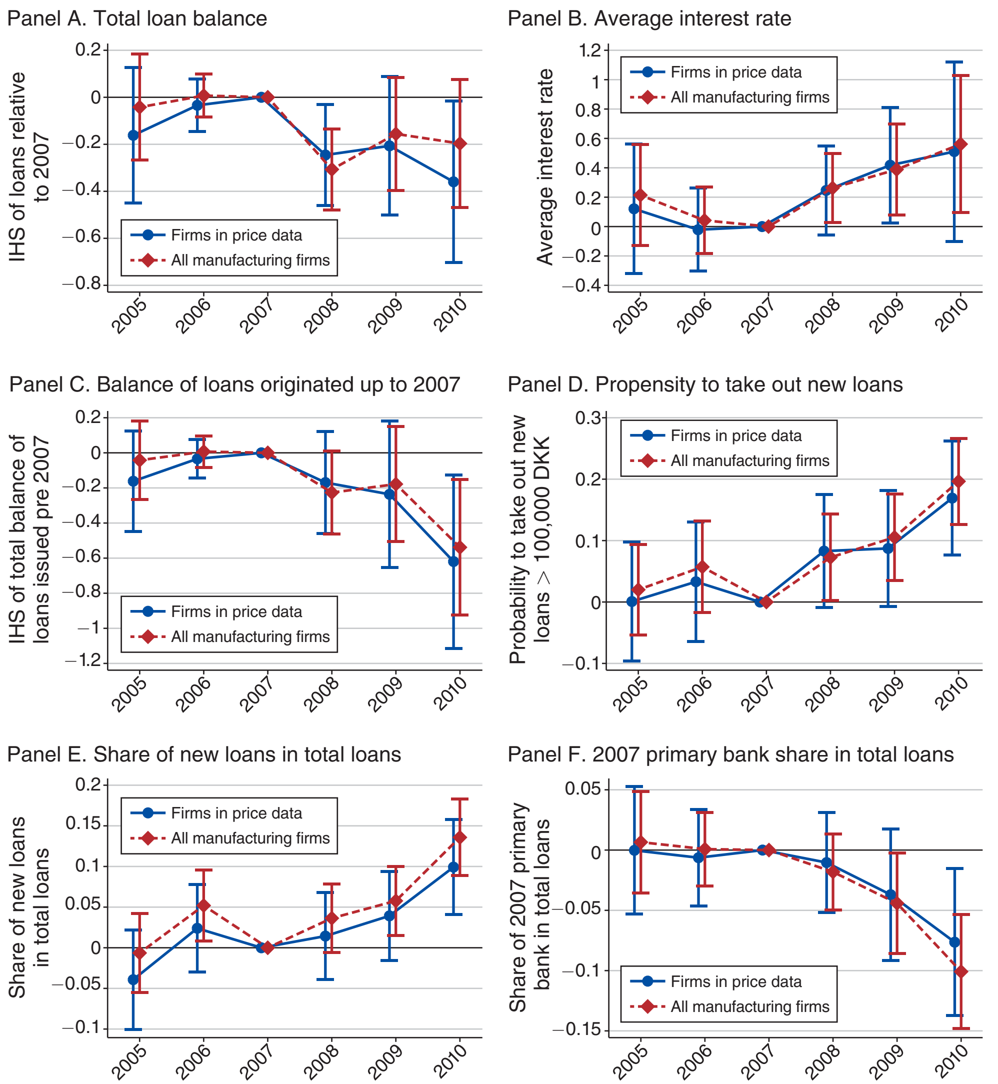

Figure 2. Effects of Firm Exposure on Loan Outcomes

Notes: The figures show estimates of the effect of exposure to wholesale-funded banks in 2007 on loan market outcomes 2005–2010. The dynamic difference-in-difference specification we estimate follows equation (4). The sample includes all firms that fulfill sampling criteria (diamonds) and the subset that can be matched to a price in either the PPI or unit value dataset (circles). The figures include 95 percent confidence intervals based on standard errors clustered at the firm level.

that full exposure decreases outstanding loans by about 20–30 percent (panel A of Figure 2). The effect on loan balances is similar regardless of whether we include firms for which we observe prices or all manufacturing firms. Panel B illustrates the effect for the average interest rate, which increases by 0.25–0.5 pp between 2008 and 2010. Given a 2007 mean interest rate of about 5.4 percent, this effect corresponds to a relative increase of 5–10 percent. For both loans and interest rates, we do not find significant differences by exposure prior to the onset of the financial crisis.

Mechanism: In the last four panels of Figure 2, we illustrate the mechanism behind the decrease in loan balances in more detail. Exposed firms repay loans that were originated up to 2007 faster than other firms (panel C), but are more likely to take out new loans after 2007 (panel D). These additional new loans amount to 5–10 percent of firms' total loans in 2009 and 2010 (panel E). New loans are often taken out from banks that are not firms' 2007 "primary bank" (i.e., the largest bank in terms of 2007 loan volume)—the share of exposed firms' 2007 primary bank in firms' loans decreases by 5 to 10 pp by the end of 2010 (panel E). These patterns are inconsistent with a demand-driven differential decrease in bank lending, thereby lending further support to our identification strategy.

Robustness: We present important robustness checks to these results in Tables A.3, A.4, and A.5 in the online Appendix. Table A.3 shows results for outcomes that deal with the issue of zeros and right-skewed loan growth rates in different ways. The results are robust to using either logarithms or growth rates of loan balances relative to 2007, winsorized at the fifth and ninetieth percentile, as outcome variables. All specifications show a strong drop in loan balances after 2007. In Table A.4 we present additional robustness checks for the loan volume results: the large drop in loan balances by the end of 2008 is robust to controling for sparser two-digit industry-year fixed effects or linear firm-level trends. The results do not substantially change if we add additional dynamic control variables or pick controls for the diff-in-diff using the PDSLASSO method. Analogous robustness checks for the interest rate results are provided in Table A.5.

IV. The Effect of Credit Supply Shocks on Prices

A. *Baseline Result*

We now turn to effects of firms' exposure on their output prices. We estimate reduced-form results using variants of equation (4) with log prices and unit values as dependent variables. For PPI prices, the data are in quarterly frequency and we estimate one coefficient for each half year (in figures) or year (in tables). For unit values indices, which are constructed at yearly frequency, we estimate one coefficient for each year.

Exposed firms increase their domestic PPI prices by roughly 3.5 percent relative to nonexposed firms in 2008. We find a peak effect of 5 percent in 2009 (Figure 3, panel A). Prior to the global financial crisis, prices of exposed and nonexposed firms evolve in parallel. Similarly, unit values of goods exported by exposed firms increase by 2 percent in 2008 relative to unexposed firms, and the difference peaks at 3.8 percent in 2009 (Figure 3, panel B). Prior to the global financial crisis, unit values of both groups of firms evolve roughly in parallel. Despite the differences in the underlying data source and firm samples, both unit values and prices exhibit very

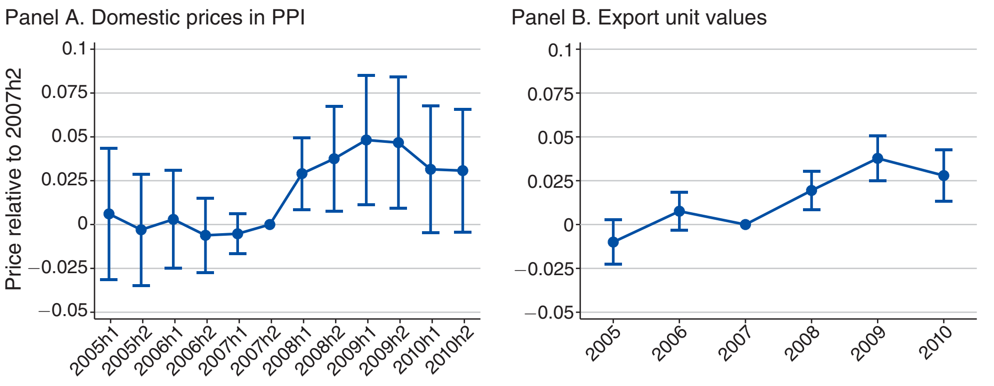

Figure 3. Effect of Firm Exposure on Prices

Notes: The figure shows estimates of the effect of exposure to wholesale-funded banks in 2007 on PPI prices and export unit values 2005–2010. The dynamic difference-in-difference specification we estimate follows equation (4), but with a dynamic effect estimated for each half year in the case of the quarterly PPI data. The figures include 95 percent confidence intervals based on standard errors clustered at the firm level.

similar dynamics. Estimated coefficients and standard errors for the years 2008–2010 are provided in the tables in online Appendix A.5.

Robustness of Reduced Form Results: We present results for different specifications and samples using PPI prices in Tables A.6 and A.7. In column (2) of Table A.6, we include linear firm trends, and in column (3) we add fixed effects for each year-two-digit CN product category combination (in addition to sector-time fixed effects). Neither substantially alters the estimated coefficients. In column (4) we omit all control variables, and in column (5) we choose control variables out of a larger set of 12 variables using the PDSLASSO methodology proposed in Belloni, Chernozhukov, and Hansen (2014) and Belloni et al. (2017). The controls selected by this procedure include our baseline controls used in all specifications, the logarithm of 2007 employment, the 2007 market share of a firm, and the profit-to-sales ratio in 2007. In column (6) we include all 12 variables that we allow the LASSO to pick from. In Table A.7 we vary the sample used to estimate the effects. In column (1) we restrict the sample to firms with exposure below 0.02 or above 0.98, and in column (2) we restrict the sample to firms with only one important lending relationship. The effects are very similar in this smaller sample. In columns (3) to (6) we relax the baseline sample restrictions. The results are not affected by including products that exit the PPI sample between 2007 and 2010.[8] In column (4) we include export prices reported in the PPI, which reduces the magnitude of the effects, in line with the lower estimates we find for export unit values more generally. In column (5) we include firms with low levels of loans. As one would expect, this reduces the size of the coefficients further. Finally, if we relax all three restrictions at the same

[8] Products may exit due to substitutions and firms due to panel rotation—we do not include firms that become inactive in the firm register.

time—i.e., we include all price series with an observation in 2007—the coefficients become substantially smaller but remain positive.

In Tables A.8 and A.9 we present analogous robustness checks for export unit values. Our main finding is robust to including trends and product category-time fixed effects, dropping controls, picking them via PDSLASSO, or including a larger set of control variables. Estimated effects are similar when we drop firms with partial exposure or only include firms with one major bank. They are robust to including products that enter between 2005 and 2007 or exit between 2008 and 2010, and to including firms with low loans. Like for domestic prices, estimated effects are smaller when we drop all sampling restrictions at the same time. Finally, in Figure A.6 we provide two robustness checks with respect to the FGLS estimation. First, the estimated peak effect in 2009 is significant and similar in magnitude if we estimate the effect using unweighted OLS but exclude the 20 percent most volatile unit value series instead. Moreover, we show that the iterated FGLS quickly converges to stable coefficients and standard errors and changes little after two iterations.

IV Estimates: Our results so far provide reduced form evidence that firms with loan relationships to wholesale-funded banks see their loan balances decrease and increase their prices relative to other firms. Here, we provide direct instrumental variable estimates for the loan supply elasticity of firms' prices. We estimate three elasticities based on specification (5) over the 2007 to 2008, 2007 to 2009, and 2007 to 2010 horizons. All three specifications use the 2007–2008 drop in lending as the endogenous variable and exposure to wholesale-funded banks interacted with a time dummy as an instrument. Panel B of Table 2 presents our baseline IV estimates. The elasticity estimates for PPI prices lie between -0.04 and -0.06. As expected, the IV estimates are less precise than the reduced form estimates, but significant at 5 percent or 10 percent confidence levels for the 2007 to 2008 and 2007 to 2009 horizons, respectively. The analogous estimates for the export unit values, using the variance of FGLS residuals from equation (4) as weights, are larger but insignificant.

The IV estimates are based on a somewhat weak first stage. The F-statistic indicates a significant first stage relationship in the PPI sample but an insignificant first stage relationship in the export unit value sample. Both are below common critical values for worst-case scenario upper bounds on the bias of the IV estimator under weak instruments provided by Montiel Olea and Pflueger (2013). We deal with this issue in several ways. First, weak instruments always bias IV estimates toward the OLS estimate, which we provide in panel A of Table 2. All OLS estimates are smaller (in absolute terms) than the baseline IV estimates, suggesting that our IV estimates are a lower bound. Second, we follow the recommendation of Andrews, Stock, and Sun (2019) and provide an Anderson-Rubin test of the null hypothesis that the loan supply elasticity of prices is zero. This test is fully robust to weak instruments and the null is rejected with high confidence, even in the unit value sample.[9]

Finally, we estimate a specification with an alternative first stage in panel C of Table 2 to address possible issues with the baseline IV. First, while we observe prices

[9]The Anderson-Rubin test is based on the reduced form regression, which is strongly significant for both the PPI and unit value sample.

Table 2—IV Estimates of Loan Supply Elasticity of Prices

	Domestic prices in PPI			Export unit values		
	2007–2008	2007–2009	2007–2010	2007–2008	2007–2009	2007–2010
Panel A. OLS						
log loans	−0.021	−0.013	−0.016	0.000	0.001	−0.001
	(0.006)	(0.007)	(0.006)	(0.002)	(0.003)	(0.003)
Observations	5,682	5,673	5,548	5,730	5,730	5,730
Firms	213	213	213	1,080	1,080	1,080
Panel B. Baseline IV–first stage regression at product level						
log loans	−0.054	−0.061	−0.041	−0.108	−0.207	−0.150
	(0.025)	(0.037)	(0.030)	(0.088)	(0.164)	(0.122)
1st stage F-stat.	4.522	4.522	4.595	1.727	1.727	1.727
(p-value)	(0.035)	(0.035)	(0.033)	(0.189)	(0.189)	(0.189)
Anderson-Rubin stat.	7.071	4.457	2.235	10.559	28.927	12.032
(p-value)	(0.008)	(0.036)	(0.136)	(0.001)	(0.000)	(0.001)
Observations	5,682	5,673	5,548	5,730	5,730	5,730
Firms	213	213	213	1,080	1,080	1,080
Panel C. Two-sample IV–first stage regression at firm level						
log loans	−0.184	−0.267	−0.170	−0.105	−0.205	−0.152
	(0.080)	(0.116)	(0.075)	(0.047)	(0.089)	(0.067)
1st stage F-stat.	5.448	5.448	5.448	5.448	5.448	5.448
(p-value)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Observations	5,730	5,721	5,593	5,762	5,762	5,762
Firms	213	213	213	1,089	1,089	1,089

Notes: Loan supply elasticity of prices estimated from equation (5). For each column, we include observations from the base period 2007 and a post period (any year from 2008 to 2010), instrumenting log loans with an exposure × post interaction. Observations in the export unit values are weighted with the inverse variance of reduced form OLS residuals for each series. Standard errors clustered at the firm level in parentheses.

at the product level, the first-stage regressions only involve firm-level variables. The first-stage regression for our baseline IV estimates therefore "stacks" firm-level outcomes for each product, and overweights firms with many products, which is likely inefficient. Second, the baseline IV estimates are based on first-stage regressions using the separate samples of firms in the PPI and the export unit values instead of using the full set of firms with price information like in Section III. To address these two problems, we use a two-sample IV approach following Angrist and Krueger (1992) and estimate the unweighted first-stage regression at the firm level on the full sample of manufacturing firms.[10] The second stage is the same product-level regression including only firms in the PPI or export unit value datasets, respectively. Panel C of Table 2 presents the results for this two-sample IV estimator. The two-sample first stage is stronger and by construction identical between the PPI and export unit value samples. The resulting IV estimates for the loan supply elasticity of prices are similar between the two samples, larger, and all significant. They vary between −0.11 and −0.27.

[10] We estimate cluster-robust standard errors following Pacini and Windmeijer (2016).

B. *Effect Heterogeneity*

Do the price-increasing effects vary for different types of products? The answer to this question can potentially reconcile our main result with Kim (2021). We focus on two product characteristics that are closely related to the demand response to price changes: the elasticity of a product's demand and the degree of strategic complementarity in prices between competitors in a product market. We estimate treatment effect heterogeneity by augmenting our baseline model with additional interaction terms:

$$(6)\qquad \log Price_{i,p,t} = \Lambda_{i,p} + \Gamma_{s(i),t} + \sum_{\substack{k=2005\\k\neq 2007}}^{2010} \mathbf{1}\{t = k\} \times \left[\beta_k Exposure_i + \eta_k \mathbf{X}_i + \gamma_k z_p + \delta_k \left(Exposure_i \times z_p\right)\right].$$

z_p is a standardized, time-invariant product characteristic such as the price elasticity of demand or the degree of strategic complementarity.

In Table 3 we present evidence that the effect of a negative credit supply shock on prices is smaller for products with more elastic demand. We use demand elasticities estimated in Broda and Weinstein (2006a) from data on US imports.[11] We use two levels of product definitions: one over 958 and a finer one over 13,972 product categories.[12] We standardize the measure by subtracting the median across goods in the data because the distribution is heavily right skewed and dividing by the standard deviation. In the domestic PPI, price increases of goods with a demand elasticity that is 1 standard deviation higher than the median are 1.2 to 4.4 percent lower in the medium run. In the export unit value data, the interaction coefficients are somewhat smaller, but they are significantly negative in both samples for at least one year during the recession. This result is consistent with the idea that increasing prices to raise liquidity is less attractive when customers are more likely to switch to other suppliers.

The second set of interactions included in Table 3 measure strategic complementarity at the good level, i.e., the average responsiveness of prices to relative price differences with competitors. We estimate this parameter from exchange-rate pass-through into domestic-currency export unit values prior to the Great Recession at the two-digit CN level following Amiti, Itskhoki, and Konings (2014). The details of the pass-through estimation are deferred to online Appendix C.3. As one would expect, we find that the increase in prices after a credit supply shock is lower for products with higher strategic complementarity.[13]

[11] The data are available online at Broda and Weinstein (2006b).

[12] Product substitutability increases with the level of disaggregation and thus the estimated level of the elasticity of demand is generally higher in the latter case. Details on the way we match both Broda and Weinstein elasticities to our data are provided in online Appendix C.1.

[13] The size of the interaction coefficient is such that only for goods with the highest strategic complementarities is the effect of a credit supply shock on prices potentially negative. The concept is invariably linked to the variability of markups, where the variability of markups increases with strategies complementarity. We present evidence below that supports the view that firms temporarily increase their markups when they cannot access external liquidity.

Table 3—Price Outcomes: Heterogeneity by Product Characteristics

	Domestic prices in PPI			Export unit values		
Interaction with:	Demand elasticity (1)	—, alternative definition (2)	Strategic complementarity (3)	Demand elasticity (4)	—, alternative definition (5)	Strategic complementarity (6)
2008	0.023	0.034	0.039	0.019	0.019	0.021
	(0.015)	(0.014)	(0.017)	(0.006)	(0.006)	(0.008)
2009	0.042	0.053	0.053	0.028	0.027	0.037
	(0.022)	(0.021)	(0.024)	(0.007)	(0.007)	(0.009)
2010	0.034	0.044	0.034	0.025	0.021	0.030
	(0.025)	(0.019)	(0.028)	(0.009)	(0.009)	(0.011)
2008 × Interaction	−0.024	−0.003	−0.017	0.002	0.002	−0.002
	(0.017)	(0.008)	(0.004)	(0.005)	(0.004)	(0.005)
2009 × Interaction	−0.044	−0.012	−0.016	−0.012	−0.009	−0.014
	(0.019)	(0.013)	(0.007)	(0.004)	(0.005)	(0.006)
2010 × Interaction	−0.043	−0.024	−0.007	−0.012	−0.005	−0.011
	(0.020)	(0.013)	(0.008)	(0.006)	(0.006)	(0.007)
Firm-product	Yes	Yes	Yes	Yes	Yes	Yes
Time-product	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,366	14,400	16,438	17,118	16,722	17,220
Firms	212	200	213	1,085	1,072	1,089
Interact.: Centered	2.40	3.40	0.00	2.39	3.97	0.00
Interact.: SD	2.74	7.27	0.16	2.48	9.74	0.16

Notes: Credit shock exposure interacted with product-level characteristics as in equation (6): (1) demand elasticities from Broda and Weinstein provided at four-digit SITC Rev. 3 level (see online Appendix C.1 for details); (2) demand elasticities from the same source at much finer ten-digit Harmonized System code disaggregation; (3) own estimates of strategic complementarities proxied by the exchange rate pass-through into domestic-currency prices (see online Appendix C.3). Interaction variables are normalized to unit variance, the first two centered around the sample median and the latter around zero (no strategic complementarities). A few observations are dropped because we do not have an estimate of the respective product-level characteristic. All models contain firm-product and time-product sector fixed effects. Standard errors clustered at the firm level in parentheses.

C. *Reconciling Results with Kim* (2021)

In contrast to our results, Kim (2021) finds that negative loan supply shocks lead to lower prices in a sample of producers of consumer packaged goods, mostly in food manufacturing. This effect is driven by a fire sale of firms' inventories. Such a fire sale should be a viable strategy to raise cash only for firms who face relatively elastic short-run demand. Indeed, both Kim (2021) and this paper find that the effect of a negative credit supply shock on prices is smaller (less positive) for products facing more elastic demand. In the lower half of Figure 4, we show cumulative densities of the coarser definition of Broda-Weinstein demand elasticities used above. The distribution in our sample (solid line) is similar to the distribution for Danish and European Union manufacturing firms overall, with a median between 2 and 3 for all three groups.[14] In contrast, the median demand elasticity for firms in sectors

[14] For the distribution in our sample, we merge demand elasticities to the products in our data, average over goods in a firm, and further aggregate using firms' sales as weights. For the reference distribution in total industrial output, we use Eurostat's Prodcom Annual Data of 2007 sales in Denmark and the European Union, respectively, at

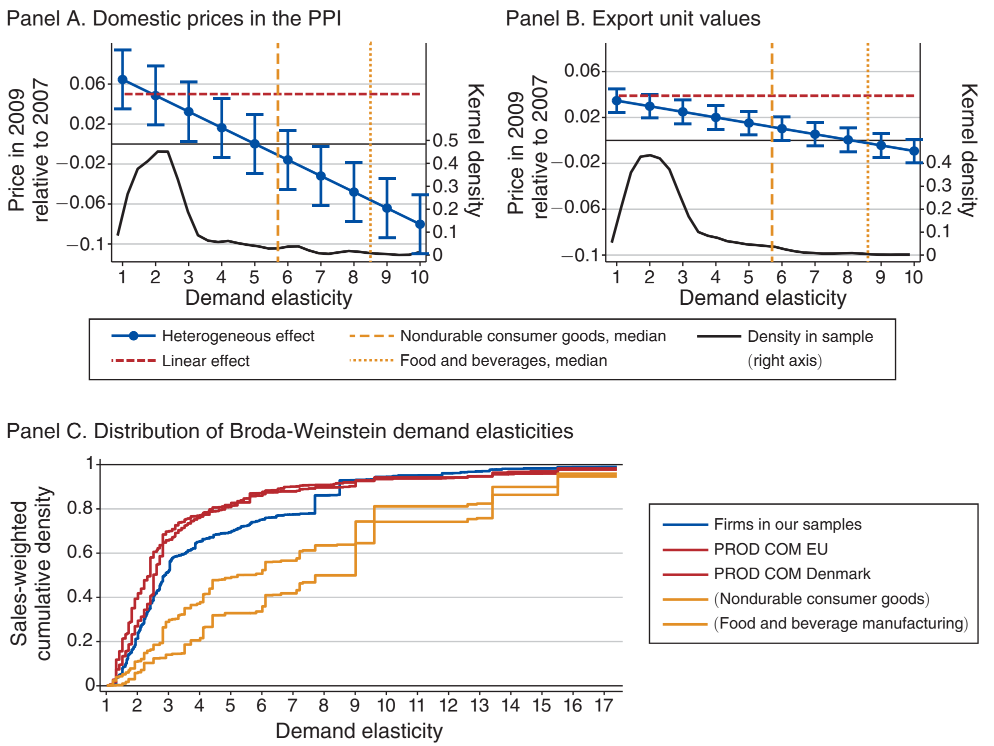

FIGURE 4. MARGINAL EFFECTS OF CREDIT SUPPLY SHOCK BY DEMAND ELASTICITY

Notes: Blue connected dots in panels A and B: effect of exposure to credit supply shock on prices in 2009 evaluated at different levels of demand elasticities (Broda and Weinstein 2006) (90 percent confidence intervals). Estimates based on equation (6). Red dashed line: linear estimate based on equation (4). The gray lines show kernel densities of product-level demand elasticities in the estimation sample. Vertical lines: sales-weighted median of elasticities among nondurable consumer goods and foods and beverages. Panel C shows cumulative densities of the firms in our sample compared to four benchmarks: red lines show distributions in total industrial production in European and Danish industrial production according to Eurostat (2021) Prodcom sales weights from 2007. Orange lines show distributions in the Prodcom data of two subsamples of products consisting only of consumer packaged goods. See online Appendix C.1 for our definition of the two subsamples.

that produce nondurable consumption goods lies at about 5.7, and for firms in manufacturing of food and beverages at about 8.6.[15] The latter two sectors are similar to the sample of Kim (2021), and one would expect that it is easier for these firms to sell off inventory by lowering their prices but more costly to generate internal liquidity by raising prices.

In the top panel of Figure 4, we relate the predicted effect for different demand elasticities from the linear interaction model described in Section B to the distribution

the eight-digit product code level. We first translate Prodcom product codes (the first four digits of which describe industries) to the six-digit Combined Nomenclature we have for products in the sample using Eurostat's correspondence tables, and then merge demand elasticities, which are defined by SITC code. This way, we match at least one demand elasticity for Prodcom product codes representing 93 percent of Danish industrial production (92 percent of nondurable consumer goods and 100 percent of food and beverages).

[15] We obtain these estimates by subsetting the Prodcom data to a basket of goods that is defined in online Appendix C.1. The full cumulative densities are shown in Figure 4, panel C.

of demand elasticities in our sample. The bulk of firms in our sample sell products with demand elasticities for which a negative credit supply shock increases prices. However, for the median demand elasticity in manufacturing of nondurable consumption goods and food and beverage manufacturing shown by the dashed vertical lines, our estimates imply a much lower or negative response of prices after a negative credit supply shock.[16] Our results are thus at least qualitatively consistent with the estimates in the narrower sample of Kim (2021). One implication of this result is that negative credit supply shocks may increase or decrease aggregate prices depending on the composition of market demand elasticities in an economy. Our estimates suggest a positive effect for the bulk of manufactured output, and we thus view a positive relationship as the relevant case for aggregate prices.

V. Working Capital and Liquidity Channels

The two channels consistent with an increase in prices after a negative loan supply shock are the liquidity channel of Gilchrist et al. (2017) and Chevalier and Scharfstein (1996) and the working capital channel (see, e.g., Christiano and Eichenbaum 1992; Bigio 2015; Christiano, Eichenbaum, and Trabandt 2015). The two channels differ in terms of their prediction for firms' price-cost markup. In the case of the liquidity channel, firms raise liquidity internally when external credit becomes more difficult to obtain by increasing short-run profits through higher prices, at the cost of a lower market share in the future. This suggests that measures of profitability should increase. In contrast, the working capital channel works through pass-through of higher marginal cost, and consequently the price-cost markup and profitability should decline or stay constant. We provide evidence for an important role of the liquidity channel.

Liquidity Channel: In Figure 5, panel A we show the response of two proxies for markups we can obtain in the data: the amount of sales per worker and the gross operating margin, defined as sales minus purchases and labor cost relative to sales. Both measures point to an increase in price-cost markups after the shock. The gross operating margin of exposed firms increases by 3 pp in 2009 relative to unexposed firms.[17] We also find that in the longer run, the market share of exposed manufacturing firms in total sector sales in the European Union declines.[18] The latter effect appears gradually and amounts to one-tenth of firms' 2007 market share by 2013 (Figure 5, panel B). The increase in measures of profitability in response to the loan

[16] The median demand elasticity reported in Kim (2021), based on Hottman, Redding, and Weinstein (2016), is 3.9—which would still be in the right tail of our data—but these demand elasticities are estimated at the UPC-firm level in Hottman, Redding, and Weinstein (2016) and hence not directly comparable to elasticities at the good-country level in Broda and Weinstein (2006a).

[17] Point estimates and standard errors are very similar if we also subtract interest expenses on bank loans when computing this margin. Additionally, Table A.10 shows that even accounting profits after interest, taxes, and depreciation, which include many (quasi-)fixed costs, increase by around 1.4 percent.

[18] The market shares are based on annual sales in the microdata and nominal market size in the annual EU Prodcom statistics.

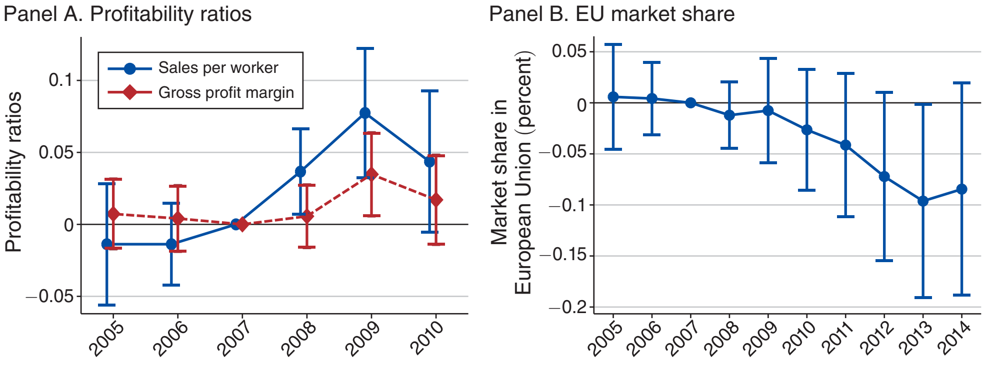

Figure 5. Effects on Profit and Market Shares

Notes: Firm-level outcomes by exposure estimated from equation (4). The gross profit margin is calculated as sales minus the wage bill and purchases relative to sales. Market shares are calculated as firms' sales divided by total European Union sales in the corresponding four-digit NACE sector. The source of the latter data are PRODCOM statistics. 95 percent confidence intervals based on standard errors clustered at the firm level.

supply shock and the longer-run decrease in market share is strong evidence for the liquidity channel.

The response of prices to the credit supply shock for firms with different levels of cash holdings lends further support to the importance of liquidity. In Table 4 we show price outcomes interacted with the ratio of firms' 2007 bank deposits to sales. At least in the unit value data with a broader set of firms, firms with more precrisis cash holdings respond less to the credit supply shock, which is consistent with a liquidity-generating motive for price increases.

We provide estimates of additional firm-level outcomes in Table A.10. Consistent with Chodorow-Reich (2014) and Züllig (2021), we find that cutting labor input cost is another important margin for exposed firms to improve their cash flow situation. We find no significant response of inventories, consistent with a limited importance of the inventory-fire-sale channel of Kim (2021) in our sample. Additionally, in online Appendix Figure A.7, we show that firms facing relatively elastic demand—i.e., firms that we find to increase prices less in response to a credit supply shock—have a stronger drop in their wage bill. This indicates that firms selling products with lower demand elasticities temporarily boost their operating cash flow by increasing prices, whereas others do so by decreasing labor cost. Overall, these findings are consistent with firms trading off short-run profit margin against long run market share as predicted by the liquidity channel if the demand they face allows them to do so.

Working Capital Channel: The increase in gross profit suggests an important role for the liquidity channel, but does not rule out that firms also pass through increased cost of financing. The working capital channel is based on the idea that firms need to prefinance input expenses prior to production. In a highly stylized model of production with working capital, marginal cost *MC* is a product of the

Table 4—Price Outcomes: Heterogeneity by Firm Balance Sheets

	Domestic prices in PPI		Export unit values	
Interaction with:	Liquidity	Working capital	Liquidity	Working capital
	(1)	(2)	(3)	(4)
2008	0.038	0.050	0.020	0.018
	(0.012)	(0.012)	(0.006)	(0.007)
2009	0.051	0.074	0.036	0.030
	(0.018)	(0.017)	(0.007)	(0.008)
2010	0.038	0.058	0.027	0.026
	(0.018)	(0.017)	(0.008)	(0.009)
2008 × Interaction	0.014	0.023	−0.017	−0.002
	(0.017)	(0.014)	(0.007)	(0.005)
2009 × Interaction	0.007	0.042	−0.022	0.019
	(0.035)	(0.023)	(0.009)	(0.005)
2010 × Interaction	0.029	0.047	−0.015	0.034
	(0.033)	(0.022)	(0.010)	(0.007)
Firm-product	Yes	Yes	Yes	Yes
Time-4d NACE	Yes	Yes	Yes	Yes
Observations	16,439	15,773	17,286	14,016
Firms	213	202	1,089	794
Interact.: Centered around	0.02	0.33	0.02	0.333
Interaction: SD	0.06	0.19	0.04	0.25

Notes: Credit shock exposure interacted with standardized firm-level balance sheet information from 2007 (equation (6)). Before standardization, balance sheet information is winsorized at the ninety-ninth percentile. Liquidity is measured by the deposit-to-sales ratio in 2007, whereas deposits are measured directly in the bank loan data. Working capital is defined as total inventories and accounts receivable net of accounts payable, following Barth and Ramey (2002), relative to sales in 2007. The latter is not available for all firms. All models contain firm-product and time-sector fixed effects. Standard errors clustered at the firm level in parentheses.

input unit cost C and the cost for prefinancing a share ψ of that cost at the interest rate r^w.

$$MC_t = \left[(1-\psi) + \psi(1+r_t^w)\right]C_t. \tag{7}$$

With a credit supply shock, r_t^w increases the marginal cost of production, potentially leading to cost-driven price increases.

We first provide evidence that firms with more working capital do increase their prices more after a credit supply shock. We measure working capital as the sum of inventories and accounts receivable net of payable relative to sales as in Barth and Ramey (2002). The idea is that a high level of current assets reflect a longer gap between cash inflows and outflows. Table 4 shows that firms with higher levels of working capital indeed increase prices more after a credit supply shock. However, the working capital share correlates with numerous other firm characteristics—as an example, by construction, working capital correlates with the level of inventories, which may reflect differences in the working-capital intensity of production, but also the history of a firm's demand shocks. We thus recommend caution with a causal interpretation of this coefficient.

Table 5—Working Capital Loans and Marginal Cost

Predicted marginal cost increase	Prefinance share	Lending rate increase	Marginal cost increase
Stylized Christiano, Eichenbaum, and Trabandt	0.56		0.08%
Working capital share, sample mean	1.27	0.14pp	0.17%
—, p10	0.55		0.08%
—, p90	1.99		0.27%
Estimated price increase between 2007 and 2010			
PPI prices			3.38%
Export unit values			2.79%

Notes: The first row provides an estimate of marginal cost changes if firms borrow 56 percent of their production expenses at the beginning of each quarter, as estimated by Christiano, Eichenbaum, and Trabandt, and the annualized (quarterly) interest rate to do so increases by 0.55 pp p.a. (0.14 pp at a quarterly rate). This is the largest response of the interest rate we estimate to the credit supply shock (see Figure 2, panel B), namely for 2010. The semielasticity we use is $\psi/\left[(1-\psi)+\psi(1+r_t^w)\right]$, where r_t^w is the quarter equivalent of the firm's 2007 effective interest rate, which is on average 1.43 percent. Rows 2–4 use the working capital ratio (inventories + accounts receivable − accounts payable divided by a fourth of annual sales) from our own microdata to proxy for the prefinance share. It can have values larger than 1 if production is financed more than a quarter in advance. The lower panel provides the estimated effects on prices for reference.

We also provide a back-of-the envelope calculation suggesting that higher cost of working capital can explain only a small part of price increases. With marginal cost given by equation (7), the semielasticity with respect to the lending rate is given by $\psi/\left[(1-\psi)+\psi(1+r_t^w)\right]$. This provides an upper bound for effects on prices under full pass-through. Our estimates suggest annual lending rates of exposed firms increase by up to 0.55 pp (see Figure 2, panel B), which corresponds to 0.14 pp in quarterly terms. We use this figure to calculate a rough estimate of the corresponding increase in marginal cost.

The calculation depends crucially on the share of expenses that are prefinanced. Christiano, Eichenbaum, and Trabandt (2015) estimate this share to be 0.56 of quarterly cost in the United States.[19] This implies an increase in marginal cost far below the estimated price increase, as shown in the first row of Table 5. In Danish data, the mean ratio of measured working capital to annual sales is 0.32, corresponding to prefinancing of 1.27 quarters of cash flow. At the tenth and ninetieth percentiles, working capital ratios imply prefinancing of about 0.5 and 2 quarters of cash flow. Using these values, we calculate a marginal cost increase of 0.17 percent for the average firm and 0.27 percent for firms at the ninetieth percentile. Such an increase would account for a minor share of the price increase we estimate. Consistent with the increase in profitability measures, we conclude that the major part of price increases is due to firms raising liquidity from higher profits, rather than cost pass-through.

[19] More concretely, they estimate the parameters of a DSGE model in which the final goods producer borrows to finance a fraction ψ of its expenditure on inputs and repays the loan at the end of the period with interest r_t^w. The model is estimated on precrisis quarterly data and matched to a set of impulse responses to three identified shocks from a VAR. We use their resulting posterior mean.

VI. Aggregate Implications

How important is the effect of loan supply on aggregate inflation during the Great Recession? We calculate a partial equilibrium counterfactual of PPI dynamics in a scenario in which wholesale-funded banks' loan supply follows the same path as deposit-funded banks'. This counterfactual can be easily obtained from our difference-in-difference estimates as follows:

$$\log \widehat{Price}_{i,p,t}\big(Exposure_i = 0\big) = \log Price_{i,p,t} - \sum_{\substack{k=2005 \\ k \neq 2007}}^{2010} \mathbf{1}\{t = k\} \times \big(\hat{\beta}_k Exposure_i\big). \tag{8}$$

We aggregate observed and predicted prices to a sales-weighted aggregate price index that follows the construction of the official PPI. For firms that do not fulfill our sample restrictions because of small lending relationships, we use observed prices in the counterfactual calculation. The solid line in Figure 6 plots the index of observed prices. It can deviate from the price index of manufacturing output published by Denmark Statistics (the dotted line) because we do not know the exact product- and firm-level weights for the aggregation, but it tracks the medium-run dynamics of prices rather well. The counterfactual index based on predicted values implied by our regression drops in 2008 and remains around 3 percent below the index based on observed prices in 2009, after which the difference becomes statistically insignificant.

How much of the "missing disinflation" can our estimates explain? In online Appendix D.1 we estimate a VAR model on time series of GDP and the PPI up to 2007. We then generate a benchmark prediction for manufacturing PPI dynamics over the Great Recession as a forecast of the price series conditional on the observed developments of real output. Given the 9 percent contraction of GDP, the VAR predicts a medium-run price level around 7 percent below the observed one.[20] Our counterfactual therefore closes around 40 percent of the medium-run gap between actual price developments and the conditional forecast.

Our counterfactual does not take into account any general equilibrium effects, but we think that such effects would likely further reduce prices. First, the counterfactual PPI assumes that prices of firms dropping out of the treatment effect estimation due to the restrictions made in Table A.1 are unaffected. Second, if firms compete for funds in the lending market, a higher loan supply from wholesale-funded banks would reduce congestion for funds from deposit-funded banks and, thereby, lower prices of their borrowers as well. Furthermore, in the presence of strategic complementarities, lower prices of exposed firms would likely also lead to lower prices of nonexposed competitors.

[20] The actual price index fell around 2 percent. A detailed description of aggregation to the PPI index and the computation of the counterfactual can be found in online Appendix D.1. The VAR-based estimate has relatively strong inertia, which is why we focus on the comparison of the medium-run price levels only.

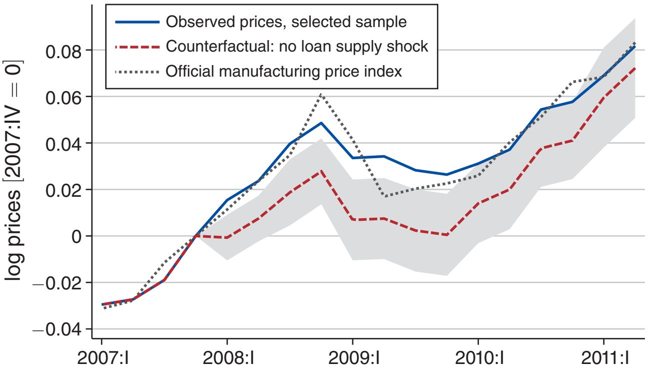

Figure 6. Counterfactual Path of Producer Prices without Differential Credit Supply Shock

Notes: The figure depicts a sales-weighted PPI index constructed from PPI microdata, compared to a counterfactual in which wholesale-funded banks' loan supply follows the same path as deposit-funded banks' loan supply. The counterfactual scenario is based on the coefficient estimates in Figure 3 (90 percent confidence intervals in gray).

VII. Conclusions

In this paper we estimate the effect of a large credit supply shock on firms' output prices. We find that firms with precrisis lending relationships to banks who are particularly exposed to the global financial crisis decrease their loan balances and pay higher interest rates during the recession. In turn, they raise their prices and increase their profits in the short run, but lose market share in the longer run. The estimated effects decline in the elasticity of demand for goods, but are positive for the vast majority of industrial output.

Our results are consistent with the idea that firms face a trade-off between short-run profits and longer-run market share, and that price increases are a viable and important strategy to raise cash in the short run when other sources of liquidity dry up. They support prior work that incorporates liquidity constraints and customer markets into macroeconomic models of price setting, such as Gilchrist et al. (2017), and suggest that credit supply can explain an important part of the "missing disinflation."

Our work also suggests that policies that affect banks' credit supply have a direct impact on inflation. In the aftermath of the global financial crisis, many regulators have introduced macroprudential policy tools such as countercyclical capital buffers that do exactly that. The interaction of such policies with central banks' traditional price stability targets have not been broadly discussed and analyzed in the literature, and we hope that our work contributes to inspiring further work in this direction.

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