Financial Shocks, Productivity, and Prices

Simone Lenzu[*] David A. Rivers[†] Joris Tielens[‡] Shi Hu[§]

September 2024

Abstract

We study the interconnection between the productivity and pricing effects of financial shocks. Combining administrative records on firm-level output prices and quantities with quasi-experimental variation in credit supply, we show that a tightening of credit conditions has a persistent, yet delayed, negative effect on firms' long-run physical productivity growth (TFPQ) but also induces firms to change their pricing policies. As a result, commonly used revenue-based productivity measures (TFPR)—which conflate the pricing and productivity effects—offer biased predictions regarding the consequences of financial shocks for firms' productivity growth, underestimating the long-run elasticity of physical productivity to credit supply by almost half. Moreover, we show that the pricing adjustments themselves also have productivity implications. Firms coping with a contraction of credit use low pricing as a source of internal financing, allowing them to avoid cutting expenditures on productivity-enhancing activities, thereby softening the impact of financial shocks on long-run productivity growth.

Keywords: Productivity, Pricing, Financial Constraints, Innovation.

JEL: D22, D24, E31, E44, G01.

[*]New York University, Stern School of Business, E-mail: slenzu@stern.nyu.edu; [†]University of Western Ontario, E-mail: drivers2@uwo.ca; [‡]KU Leuven and National Bank of Belgium, E-mail: Joris.Tielens@nbb.be. [§]New York University, E-mail: sh5012@nyu.edu. We thank Tobias Berg, Andrea Caggese, Robert Engle, Marcela Eslava, Ryan Kim, Filippo Mezzanotti, Holger Mueller, Cecilia Parlatore, Gordon Philipps, Nicola Pierri, Manju Puri, Adriano Rampini, Alexi Savov, Johannes Stroebel, Olivier Wang, and participants and discussants at NYU Stern, Columbia Workshop in New Empirical Finance, RCEA Growth, Innovation and Entrepreneurship Conference, IMF, European Summer Meeting of the Econometric Society, Eastern Finance Association, FINEST workshop, EBRD Finance and Productivity conference, International Industrial Organization Conference, Midwest Finance Association, Barcelona GSB Summer Forum, North American Meeting of the Econometric Society, European Association Industrial Organization meeting, and Society for Economic Dynamics for the useful comments and suggestions. Lenzu thanks the Center for Global Economy and Business at NYU Stern, and Rivers thanks the Social Sciences and Humanities Research Council of Canada for their generous financial support. We thank Jun Wong and Cangyuang Li for excellent research assistance. The views expressed in this paper are those of the authors and do not necessarily reflect the views of the National Bank of Belgium, the Eurosystem, or any other institution with which the authors are affiliated.

1 Introduction

Financial crises are frequently followed by persistent slowdowns in aggregate productivity growth (Cerra and Saxena, 2008; Reinhart and Rogoff, 2014; Hall, 2015). This has been recently documented for the U.S., Europe, and several developing countries in the wake of the Great Recession and subsequent sovereign debt crisis.[1] One explanation is that financial market conditions affect the ability of individual producers to sustain productivity growth (Midrigan and Xu, 2014; Cole, Greenwood, and Sanchez, 2016).

Despite the growing interest in this topic, studying micro-level productivity slowdowns and their drivers remains challenging. A key difficulty lies in their measurement: commonly used revenue productivity measures conflate output prices with physical productivity. Accordingly, observed productivity slowdowns could indicate an actual decline in physical productivity growth, declining output prices, or both.

In this paper, we construct a novel dataset that allows us to directly address this empirical challenge and systematically examine the separate physical productivity and output price responses to a contraction in credit supply, as well as their relationship. Our analysis demonstrates that accounting for the endogenous response of prices is crucial for measuring and understanding how firms respond to financial shocks and the associated implications for productivity growth.

We find that a sudden tightening of financial conditions causes a delayed, but persistent and economically significant reduction in firm-level physical productivity growth (TFPQ). Revenue-based measures of productivity (TFPR), however, provide biased estimates of the effects on physical productivity as they also capture a change in pricing policies.[2] In the immediate aftermath of the credit crunch, firms cut output prices and, as a result, TFPR estimates suggest a short-run slowdown of firm-level productivity growth, despite TFPQ being unaffected. In the medium-to-long run, the TFPR and TFPQ responses are correlated, however the former substantially understates the decline in the latter because firms more affected by the shock eventually raise prices.

Furthermore, we show that firms that are able to respond to the shock in the short run by lowering output prices experience a significantly lower contraction in productivity

[1] See, e.g., Jordà, Schularick, and Taylor (2013) and Queralto (2020).

[2] The TFPR-TFPQ terminology, now standard in the literature, was first introduced by the seminal contribution of Foster, Haltiwanger, and Syverson (2008). See Syverson (2011) for a discussion of the relationship between quantity- and revenue-based productivity measures.

growth in the long run. The reason is that financial shocks deprive firms of the liquidity needed to fund investments in innovation and human capital that sustain productivity growth over time. By using low prices as a source of internal finance, firms can generate liquidity from the product market, allowing them to relieve the pressure to reduce expenditures in productivity-enhancing investments.

The findings in this paper offer a novel perspective and new insights regarding the contribution of financial factors to firm-level productivity growth. For one, they suggest that the consequences of financial shocks are sizable but take time to materialize, although movements in prices convey the (mistaken) impression that they impair firm-level productivity immediately. For another, they reveal that the price adjustments themselves have direct implications for productivity growth, as firms can use pricing adjustments as a source of internal finance.

The paper proceeds as follows. In Section 2 we describe the data used in our empirical analysis and discuss issues related to the measurement of prices and productivity. We build a novel micro-level panel dataset that spans a decade of business and credit records for manufacturing firms in Belgium. Combining confidential administrative data from different sources, our dataset merges information on firm/product-specific output prices and quantities, a detailed account of firms' balance sheets and income statements, and comprehensive records of firm-bank credit relationships. The availability and granularity of our data enable us to build firm-level price indices that aggregate across the heterogeneous products of multi-product firms and allow us to compute firm-level technical efficiency measures.

Section 3 details the empirical design that allows us to identify firm-level credit supply shocks. The national business credit registry offers a detailed account of firms' overall access to bank finance, as well as disaggregated information on their credit suppliers and their individual positions with firms. By combining this information with the occurrence of an aggregate financial shock that differentially affected lending institutions in Belgium, we are able to isolate variation in firm-level credit driven by changes in credit supply, separately from changes in credit demand. Specifically, we use the burst of the 2010-2012 European sovereign debt crisis as a natural experiment to construct a set of firm-specific credit supply shifters, which allow us to identify the causal impact of credit supply shocks on firm-level productivity growth and pricing behavior.

Section 4 presents our main results on the separate effects of credit supply shocks on productivity and prices. Our estimates reveal that firms coping with a tightening of credit supply experience a significant contraction in TFPQ growth that materializes three years after the credit shock and persists over time. Specifically, we estimate that a one standard deviation difference in exposure to the credit shock translates into a reduction of long-run productivity growth by 6.1 percent, which implies a long-run elasticity of firm-level productivity to credit supply of 0.4. The persistent productivity slowdown helps rationalize the slow economic recovery after financial crises documented by previous studies (Queralto, 2020).

A rather different picture emerges when we examine estimates based on TFPR. The reason is that revenue-based productivity estimates capture not only changes in physical productivity, but also changes in firm output prices, which we show are also directly affected by the shock. In the short-run, the shock induces firms to reduce prices, with a one standard deviation difference in exposure to the shock leading to a 2 percent drop in prices, whereas TFPQ is unaffected. As a result, the TFPR estimates erroneously suggest that firms facing a financial shock experience an immediate slowdown of productivity growth. In the long-run, firms eventually increase prices in response to the shock, with a one standard deviation in exposure generating an increase in prices of up to 4 percent in the years following the shock. Consequently, while revenue and physical productivity growth do co-vary over longer horizons, TFPR estimates significantly understate (by about half) the true impact of a tightening of financial conditions on physical productivity growth.

After decoupling the productivity and price effects of financial shocks, in Section 5, provide evidence on the economic mechanisms underlying these responses. First, we show that the sudden tightening of credit supply conditions has an immediate, contractionary effect on expenditures on productivity-enhancing activities, such as investments in innovation and worker's human capital. Using variation in these expenditures driven by firms' heterogeneous exposure to the credit shock, we then show that the contraction in investments in intangibles leads to a persistent, yet delayed, reduction in firm-level productivity growth. Specifically, we estimate that a one percentage point reduction in the probability of undertaking any R&D investment translates into a decrease in long-run productivity growth of 1.4 percent. Similarly, a one percent reduction in R&D and training expenses leads to decreases of productivity of over

2 percent and 0.4 percent, respectively.

Next, we study the mechanisms underlying the price response. First, we document that the credit supply shock led firms to seek alternative, more expensive, sources of external funding, leading to an increase in borrowing costs. Second, as discussed above, the shock reduced long-run productivity growth for firms. Together, higher financing costs and lower production efficiency lead to an increase in operating costs, which explains why prices of producers more exposed to the credit crunch increase in the long-run, compared to less exposed producers.

A fundamentally different force explains the contraction of output prices in the immediate aftermath of the shock. The sudden tightening of credit supply conditions starves firms of liquidity and exposes them to the risk of financial distress. Since cutting costs or raising external finance from alternative sources takes time or might not be possible, firms use low pricing as a source of internal finance to counteract the reduction in external finance. A more aggressive pricing strategy, while sub-optimal in normal circumstances, allows firms to generate additional cash flows by selling off their inventories (Kim, 2020).

By decoupling the effects of financial shocks on firm productivity and pricing, our results not only enhance our understanding of the real and nominal effects of financial shocks, but also reveal an important inter-temporal relationship between them. In Section 5.3, we document a strong, negative correlation between a firm's short-term price response and long-run productivity growth. That is, firms that price more aggressively in reaction to the financial shock are the ones that experience a less pronounced long-run contraction in productivity growth. The explanation we propose is that liquidity is fungible, and firms that can leverage price reductions as a source of internal finance are able to avoid significant reductions in productivity-enhancing investments, thus softening the long-run impact on productivity.

To provide evidence for this hypothesis, we leverage cross-sectional variation in firm's latent ability to respond to the credit tightening by lowering prices. Previous work has shown that liquidity constrained firms shed inventories when hit by financial shocks (Gertler and Gilchrist, 1994; Kashyap, Lamont, and Stein, 1994) and that firms with larger inventory holdings are more likely to drop prices in the attempt to generate extra cash flows from the product market (Kim, 2020). Based on these insights—which find support in our data—we exploit heterogeneity in the (pre-shock) availability of firm-level inventories of both finished and unfinished goods as well as inventories of intermediate inputs used in production, scaled by firm's total assets. We show that, consistent with an inventory channel, inventory levels are highly predictive of the observed price response. We then document that producers that can more readily adjust their pricing policies reduce their expenditures on productivity-enhancing activities less than other producers, and as a result they experience lower reductions in productivity growth in the long run.

Relation to the literature. This paper contributes to the literature studying the relationship between finance and productivity growth, and more specifically the influence of financial market conditions on producers' technical efficiency.[3] Using aggregate data from advanced economies and emerging market economies, Queralto (2020) documents a persistent productivity drop following financial crises, suggesting that financial tightening acts as a drag on business productivity. Midrigan and Xu (2014) develop and calibrate a quantitative model highlighting the role played by financial frictions in determining firm-level TFP growth. More closely related to our study, Caggese (2019) and Manaresi and Pierri (2018) offer micro evidence on the negative relationship between financial frictions and firm-level revenue productivity growth.[4] To the best of our knowledge, our paper is the first to quantify the causal effects of financial shocks on firm-level productivity, disentangling changes in technical efficiency from simultaneous pricing effects.

Our paper also relates to a strand of studies documenting that revenue and physical productivity estimates may offer intrinsically different predictions in a variety of contexts. Foster, Haltiwanger, and Syverson (2008) explores the separate influence of physical productivity and demand on firm survival. Others emphasize the distinction between revenue and physical productivity when studying the implications of resource misallocation (Hsieh and Klenow, 2009; Haltiwanger, Kulick, and Syverson, 2018), foreign market participation (Katayama, Lu, and Tybout, 2009), trade liberalization (Eslava et al., 2013), learning-by-exporting (Garcia-Marin and Voigtländer, 2019), and firm dynamics (Eslava and Haltiwanger, 2020). We are the first to show that distinguishing between the two productivity measures is crucial to understanding the implications of financial

[3] See Levine (2005) for a review of the finance and growth literature.

[4] See also Levine and Warusawitharana (2021) and Duval, Hong, and Timmer (2020).

shocks on firm productivity. Moreover, the bifurcation between the short-run TFPR and TFPQ effects is the result of a novel mechanism that is not ascribable to the demand– and supply–side explanations documented thus far in the literature. In contrast, it is driven by firms' responses to a sudden tightening of credit market conditions, which leads them to fundamentally change their behavior in the product market.

Finally, our paper bridges the finance-and-productivity literature with the previously unconnected literature studying how financial factors influence producers' pricing policies.[5] Within this literature, our paper is closest to Kim (2020), which documents a reduction of firms' output prices in response to a credit supply shock, emphasizing the role played by inventory management.[6] By studying both prices and productivity together, our paper demonstrates that the use of low pricing as a way to raise liquidity from the product market has not only nominal implications (pricing behavior), but also important real effects, as it mediates the impact of financial shocks on long-run productivity growth.

2 Data and measurement

The central objective of our analysis is to understand the consequences of financial shocks on productivity and pricing dynamics, as well as their relationship. To this end, we construct a novel product-firm-bank-matched dataset that allows us to observe information on product-level prices and quantities of the individual goods produced by manufacturing firms in Belgium, as well as detailed accounts of their production choices, assets and liabilities structure, and access to credit markets. Overcoming the limitations of previous empirical studies interested in the finance-productivity nexus, the granularity of these data allows us to us to compute firm-level technical efficiency measures.

[5]Chevalier (1995a) and Chevalier and Scharfstein (1995; 1996) provide empirical evidence that a firm's financial condition affects its pricing strategy. Borenstein and Rose (1995), Busse (2002), and Phillips and Sertsios (2013) document a contraction of firm output prices in response to financial shocks. Gilchrist et al. (2017) studies the role played by firms' liquidity constraints in the determination of inflation dynamics during the Great Recession.

[6]See also Amihud and Mendelson (1983) for Hendel (1996) for treatments of optimal price and inventory policy under supply and demand uncertainties.

2.1 Data

Our dataset combines confidential information from four administrative sources—PRODCOM, firms' annual accounts, corporate credit register records, and individual bank balance sheets—which we briefly describe below. Additional details on the sources, data construction, and variable definitions are provided in Appendix A.

Product-level prices and quantities. We use the PRODCOM database to obtain detailed information on firms' real activity (value and quantity of production) for all manufacturing products for a large sample of firms. The PRODCOM survey, commissioned by Eurostat and administered in Belgium by the National Statistical Agency, is designed to cover at least 90% of production value within each NACE 4-digit manufacturing industry by surveying all firms operating in the country with (a) a minimum of 20 employees or (b) total revenue above 4.5 million euros (European Commission, 2014).[7] The surveyed firms are required to disclose product-specific revenues (in euros) and quantities (e.g., volume, kg, m^2, etc.) of all products sold on a monthly basis, disaggregated at the 8-digit product level (e.g., 15.93.11.93 for "Sparkling wine, alcohol by volume > 8.5%", 15.93.11.95 for "Sparkling wine, alcohol by volume ≤ 8.5%"). These data allow us to compute a firm-level price index, as well as a firm-level quantity-index used in the production function estimation.

Firm balance sheets and real investment activity. Data from the firms' annual accounts (AA) from the Belgian Central Balance sheet office provide us with detailed information on total firm revenues, production inputs (capital, labor, intermediate inputs), and the stock of inventories. These variables, combined with the price and quantity data from PRODCOM, allow us to estimate quantity-based production functions and recover firm-level technical efficiency. Moreover, the AA also contain information on firms' employment, capital investments, and investments in R&D and employee training. The latter are commonly regarded as productivity-enhancing expenses, which allow us to shed light on the channels through which credit tightening affects firms' productivity activity and productivity and how the ability to adjust prices can mediate these effects.

[7] The statistical classification of economic activities in the European Community, commonly referred to as NACE, is the standard industry classification system used in the European Union.

Table 1: Summary statistics

Panel a: Firm characteristics

	Mean	pc25	pc50	pc75	SD	N
Total Assets (Million Euros)	91.188	7.877	14.725	37.596	322.192	1024
Total Revenues (Million Euros)	70.855	11.075	21.221	53.509	161.796	1024
Employees	177	42	79	166	317	1024
Bank debt / Total Assets (Lev)	0.21	0.04	0.15	0.34	0.19	1024
Long-term debt / Long-term Liab.	0.80	0.70	1	1	0.35	1024
Inventories/Assets (Inv)	0.19	0.09	0.17	0.27	0.13	1024
Z-score	2.06	1.35	2.04	2.67	1.12	1024
(Credit Supply) Shock	0.14	0.11	0.15	0.17	0.05	1024

Panel b: Credit, productivity, and prices (growth rates)

	Short-term			Long-term		
	Mean	SD	N	Mean	SD	N
ΔCredit	-0.15	0.56	1024	-0.62	1.07	650
Δfc	0.00	0.12	700	0.00	0.16	386
$\Delta \ln TFPR$	0.03	0.11	1024	0.03	0.15	650
$\Delta \ln TFPQ$	0.04	0.16	1024	0.02	0.32	650
$\Delta \ln P$	0.01	0.14	1024	0.10	0.25	650

Panel c: Investment and employment variables (growth rates)

	Short-term			Long-term		
	Mean	SD	N	Mean	SD	N
Inv. rate R&D	0.10	0.35	775	2.97	9.11	484
Any R&D Expense	0.16	0.37	775	0.20	0.40	484
Training Expenses	0.48	1.70	701	1.29	2.77	459
Inv. rate M&E	0.12	0.17	1024	1.02	1.10	649
Employees	-0.02	0.13	1024	0.00	0.33	650

Notes: This table reports the summary statistics of the main variables used in the empirical analysis. Panel a presents descriptive statistics about the firms in our sample. All these variables are measured prior to the Greek bailout (end of fiscal year 2009). We also report the summary statistics of the credit supply shock (Shock, defined in section 3). When running the regression models, this variable is standardized to have a mean of zero and a standard deviation of one. Panels b and c focus on outcome variables. Panel b presents short-term growth rates (2009–2010) and long-term cumulative growth rates (2009–2016) for credit balances, financing costs, and measures of productivity and prices. Panel c reports short-term (2009-2010) and long-term (2009–2016) cumulative investments in R&D and machinery and equipment (M&E), cumulative growth rates of training costs, and cumulative employment growth rates. The variable "Any R&D Expense" is a dummy indicating whether the firm had any investment in R&D.

Credit data. A key feature of our data, used in the construction of the firm-specific credit supply shifters, is the ability to measure the amount of bank credit received by each firm from individual lenders. Unique firm identifiers allow us to merge our firm-product-level data with confidential firm-bank records from the Belgian Corporate Credit Registry (CCR). These data provide information on firms' credit relationships and monthly credit balances maintained with each financial institution operating under the supervision of the National Bank of Belgium (NBB).[8]

Bank balance sheets. As we explain in more detail in Section 3, the linchpin of our identification strategy is the burst of the European sovereign debt crisis—and subsequent contraction of bank credit—that followed the bailout of the Greek sovereign debt in 2010. We leverage information on firms' heterogeneous exposure to banks differentially impacted by the European sovereign crisis in order to isolate firm-specific variation in credit availability (i.e., movements in credit supply). To do so, we merge in bank balance sheet data from the NBB supervisory records, which provide us with quarterly accounting information on the balance sheets and income statements for each bank in the CCR. The key variable of interest is the bank-level stock of sovereign securities that experienced a significant loss in value after the burst of the European sovereign crisis.

Sample properties. We focus our analysis on an 11-year window centered around the Greek sovereign bailout (2006-2016), restricting our sample to firms with active lending relationships in the twelve months before the Greek bailout. Our final sample consists of $1,024$ firms and a total of $9,667$ firm-year observations between 2006 and 2016. As we discuss in Appendix A, we construct our sample starting from the PRODCOM database, focusing on firms whose main activity is within manufacturing, and merge in the data from the AA and the CCR. In this process, we drop observations with missing information on prices and on other variables used in the productivity estimation (inputs and outputs).[9]

[8] To harmonize the frequency of the CCR records with that of the AA variables, we sum each firm's monthly credit balances (authorized credit) across its lenders and compute firm-level yearly debt balances averaging across months of each fiscal year.

[9] In order to perform the production function estimation, we focus on industries (NACE Rev. 1.1 2-digit codes) with at least 50 firms and 200 firm-year observations. This leaves us with 16 industries, which covers over ninety percent of total manufacturing output in PRODCOM. Moreover, we require firms in our final sample to report information on firm-level inventories, which is mandatory only for larger firms filing complete AA. This filter, coupled with the PRODCOM inclusion criteria, implies that our sample is highly representative of the manufacturing sector but tends to under-sample smaller firms. A large body of research highlights how credit supply shocks tend to affect smaller firms more than larger firms (e.g.,

To minimize the impact of outliers, we trim the observations at the tails of the firm-level price growth distribution (top and bottom one percent) and winsorize variables measured in levels (growth rates) at the 1 percent (2.5 percent) level. Table 1 presents the summary statistics of the key variables used in the empirical analysis.

2.2 Productivity estimation

We estimate firm-level physical productivity (TFPQ) as the residual from a gross output production function:

$$\ln TFPQ_{jt} = q_{jt} - f(k_{jt}, l_{jt}, m_{jt}; \gamma), \tag{1}$$

where lowercase letters denote logs. The variable q_{jt} denotes firm-level output (quantity produced) produced by firm j in year t. The variables k_{jt}, l_{jt}, m_{jt} denote capital, labor, and intermediate inputs, respectively. $f(\cdot)$ is the (log) production function, and γ is a vector of structural parameters to be estimated. TFPQ captures a firm's capability to turn inputs into physical output. As explained in Foster, Haltiwanger, and Syverson (2008), it is the appropriate measure of a firm's technical efficiency, essentially reflecting its average per unit cost of production.

We measure the firm-level quantity index, Q_{jt}, by dividing firm-level revenues (net of any changes in inventory value of finished goods) by a firm-level price index.[10] We measure the firm-level price index, P_{jt}, by aggregating and concatenating price changes across products of multi-product firms. Specifically, we first compute a Törnqvist index, a standard measure used by statistical agencies, to measure the average yearly growth rate of prices across 8-digit products within a firm:

$$P_{jt}/P_{jt-1} = \prod_{p \in \mathcal{P}_{jt}} (P_{jpt}/P_{jpt-1})^{\bar{s}_{jpt}},$$

where $\mathcal{P}_{jt}$ represents the set of 8-digit products manufactured by firm j, P_{jpt} is the unit value of product p in $\mathcal{P}_{jt}$, and $\bar{s}_{jpt}$ is a Törnqvist weight computed as the average of the sales shares of product p in $\mathcal{P}_{jt}$ between t and $t-1$.[11] We then build our firm-level

Gertler and Gilchrist (1994) and Bottero et al. (2020)). As a result, our reduced-form results on the real effects of financial shocks on productivity and prices may represent a lower bound of the effects observed across the entire firm size distribution.

[10] To adjust our output measure for changes in inventories, we first adjust firm-level revenues by the change in firm-level inventory of final goods and then apply the firm-level price index to compute the adjusted quantity index.

[11] To ensure comparability of product-level prices across firms and over time, we define products as unique

price index (in levels), P_{jt}, by recursively concatenating the year-to-year Törnqvist index starting from a firm-specific base year: $P_{jt} = P_{jB} \prod_{\tau=B+1}^{t} P_{jt}/P_{jt-1}$. Following Eslava and Haltiwanger (2020), we construct the base price index, P_{jB}, as a geometric average of the prices of all products of firm j in the base year B scaled by the average price for that product. This allows us to capture cross-sectional differences in prices across firms, which are important for the purposes of the productivity estimation.

On the input side, we measure labor services, L_{jt}, and intermediate inputs, M_{jt}, using the wage bill and the expenses on materials and services used in production. To measure capital services, K_{jt}, we follow the perpetual inventory method using information on the flows of investments in fixed assets. We deflate labor, intermediate inputs, and capital by the corresponding industry-year price deflators.

We estimate the production function separately for each industry. The details of the estimation routine are provided in Appendix B.2 together with the corresponding elasticity estimates. Our approach is based on Gandhi, Navarro, and Rivers (2020) but augmented to allow for differences in market power in the product market (Blum et al., 2024) and to control for differences in output quality (De Loecker et al., 2016). This structural approach identifies the production function by addressing the simultaneity bias that arises from the correlation between input choices and unobserved productivity (Marschak and Andrews Jr., 1944), and it solves the identification problem that affects the estimates of the output elasticities of flexible inputs. Moreover, consistent with our empirical findings, we allow firm-level productivity to evolve according to a controlled Markov process in which firm investments in innovation (R&D and employee training) affect future productivity growth.[12] Since the European sovereign debt crisis (and preceding global financial crisis) may have generated frictions that caused firms to deviate from the unconstrained optimization, we perform the production function estimation using only data prior to 2008, and then apply the production function estimates to all years in order to compute productivity for the full sample.[13] We model the production

combinations of 8-digit PRODCOM product codes and units of quantity measurement (e.g., liters, kilograms, etc.). We then compute unit values for each product (i.e., prices) by dividing total value by total quantity for each firm-product-time observation.

[12] As in Ericson and Pakes (1995) and Doraszelski and Jaumandreu (2013), the distribution of firm productivity in period t depends on both past expenditures on innovation and past productivity realizations.

[13] As a robustness check, we replicate our analysis using a productivity index derived from index-function methods–which does not rely on estimating the production function–as well as compute production function estimates using the full sample period. As shown in Appendix D, these results are similar to

function $f(\cdot)$ flexibly, which allows us to compute firm-time varying elasticities without imposing restrictions on the elasticity of substitution between different inputs.

To underscore the importance of decoupling the effects of financial shocks on firms' productivity growth and pricing policies, we compute a common revenue productivity measures (TFPR), used in the literature as proxy for TFPQ when separate information on firm-level information on prices and quantities is not available. To construct this measure, we work under the standard assumptions in the literature and construct a productivity index that is the residual from production function estimation where a firm's total revenues (net of changes in inventories) deflated by an industry-level price index is used as a proxy for the firm's physical output:

$$\ln TFPR_{jt} = r_{jt} - f(k_{jt}, l_{jt}, m_{jt}; \tilde{\gamma}), \tag{2}$$

where we denote the vector of parameters that determine revenue elasticities by $\tilde{\gamma}$ to distinguish it from the vector of structural parameters that characterize the curvature of the quantity production function in equation (1).[14]

3 Empirical design

The credit balances observed in the CCR data result from a combination of factors, some of which are attributable to the supply of credit, while others relate to firms' financial needs, investment opportunities, and consequently, credit demand. Since the same events that alter supply-side conditions may also trigger demand-side adjustments, we face a classic identification challenge in estimating how firm-level outcomes are affected by credit availability. We overcome this challenge by exploiting quasi-experimental variation in the credit supply faced by individual producers. This variation is driven by their heterogeneous exposure to lenders holding different amounts of distressed sovereign securities in the wake of the 2010-2012 European sovereign debt crisis.

The key event in our study is the bailout request made by the Greek government in April 2010, which sparked tension in European sovereign markets and led to a reassessment of the risk profile of sovereign securities issued by peripheral European

our main results.

[14]As pointed out by Klette and Griliches (1996), under general conditions, revenue elasticities are biased proxies of the elasticities estimated from quantity production functions.

countries (Greece, Italy, Portugal, Spain, and Ireland, hereafter referred to as GIPSI).[15] The events in Greece triggered a sharp increase in the spread between the yield to maturity of GIPSI's bonds and German bonds, which were regarded as safe assets. The sudden change in the risk profile of these securities negatively impacted the balance sheets of financial intermediaries holding them, which, in turn, transmitted the shock to their borrowers in the form of credit tightening. This can be seen in the aggregate raw data, which reveals a divergence in credit supply after the Greek bailout between banks with high versus low exposure to distressed sovereigns (Figure A.1, Appendix C).

Belgian firms rely heavily on bank debt as their primary source of external finance. In our sample, only 1.35 percent of the firms are publicly listed and only 0.87 percent of them issue publicly traded bonds. The share of bank debt provided by banks reporting in the credit registry amounts, on average, to 21 percent of firms' total assets and debt vis-à-vis financial institutions represents, on average, 80 percent of firms' long-term liabilities (Table 1). Moreover, previous literature has shown that financial frictions prevent or limit a firm's ability to substitute toward alternative forms of external finance (Chodorow-Reich, 2014). Taken together, these observations suggest that a tightening of credit supply by a firm's legacy lender is likely to have significant effects on the firm's real activity.

3.1 Identification strategy

Following Bottero, Lenzu, and Mezzanotti (2020), we use the Greek bailout as a natural experiment to construct firm-specific credit supply shifters based on the presence and significance of firms' credit relationships with lenders differentially exposed to distressed sovereign securities. Specifically, we construct these shifters by measuring the weighted-average exposure of firm j's lenders to the sovereign shock:

$$\text{Shock}_j = \sum_{b \in \mathcal{B}_j} \omega_{jb} \cdot \text{GIPSI Sovereigns}_b,$$

where $\mathcal{B}_j$ represents the set of financial institutions lending to firm j in 2010:Q1, the quarter prior to the Greek bailout request, ω_{jb} is the share of firm j's credit received from bank b in the same quarter, and the variable "GIPSI Sovereigns$_b$" measures bank b's holdings of sovereign securities issued by GIPSI countries in 2010:Q1, scaled by bank b's

[15] See Appendix C and Lane (2012) for a description of the European sovereign crisis.

risk-weighted assets. By focusing on pre-bailout holdings we ensure that our measure is not affected by any endogenous portfolio adjustment that banks made in response to the sovereign crisis itself (Becker and Ivashina, 2018). At the onset of the sovereign crisis, the average firm in our sample was borrowing from a pool of banks that had invested a substantial portion of their risk-weighted assets (14 percent) in sovereign bonds issued by peripheral European countries. We also observe significant dispersion in firm exposure, as indicated by the standard deviation of Shock_j (4.6 percent).

Leveraging the heterogeneous exposure of individual firms to the sovereign crisis, we estimate empirical impulse-response functions for productivity and prices in response to the credit supply shock using local linear projections (Jordà, 2005). Specifically, we run a sequence of cross-sectional regressions over different time horizons, indexed by τ:

$$\Delta_\tau Y_j = \beta_\tau \cdot \text{Shock}_j + \Gamma^{'}_{K,\tau}\mathbf{K}_j + \Gamma^{'}_{X,\tau}\mathbf{X}_j + i_{ind,\tau} + i_{reg,\tau} + u_{j\tau}. \tag{3}$$

The left-hand-side variable $\Delta_\tau Y_j$ measures the cumulative growth rate of a firm-level outcome variable between the year prior to the crisis (2009) and the year $2009 + \tau$, where $\tau = \{1, ..., 7\}$. To facilitate the interpretation of the treatment effects, we de-mean and scale Shock_j by its standard deviation. Therefore, the coefficients of interest, β_τ, capture the effect of a one standard deviation difference in exposure to the credit shock on the $\tau-$year cumulative growth rate of Y_j.[16]

We follow Bottero, Lenzu, and Mezzanotti (2020) by including bank-level controls ($\mathbf{K}_j$), all of which are measured before the Greek bailout in order to account for the fact that a bank's level of sovereign holdings is correlated with other bank characteristics (e.g., capitalization and exposure to stability of funding) that might affect a bank's propensity to adjust credit supply following the burst of the sovereign crisis.[17]

We also account for firms' heterogeneous scope and strength of credit market interactions by controlling for the average length and number of lending relationships of the borrower ($\mathbf{X}_j$), measured before the burst the of the crisis.

We restrict the analysis to within-industry and within-region variation through the

[16] All of our baseline results are obtained from unweighted regressions. However, weighting observations by firm size (revenues at the end of 2009) leads to quantitatively very similar results.

[17] The bank-level controls in $\mathbf{K}_j$ include measures of lender size, funding structure, liquidity position, and lending portfolio quality. Similar to our measure of GIPSI sovereign exposure, each of these variables is constructed as a firm-level weighted average of the lender-specific variables, measured in the last quarter before the shock (2010:Q1), with weights based on the share of firm j's credit received from each bank (ω_{jb}). See Appendix A for further details on the sources and definitions of the control variables.

inclusion of detailed fixed effects, $i_{ind,\tau}$ and $i_{reg,\tau}$, to address the possibility that lenders with high sovereign holdings might specialize in industries or geographical regions experiencing a more severe contraction in economic activity (Paravisini et al., 2014).[18] This granular set of fixed effects ensures that the estimated productivity and price effects are not capturing firms' responses to a contraction in local, industry-level, or aggregate demand that might have resulted from the tensions in sovereign markets (Bocola, 2016). Moreover, by estimating the model in first-differences we control for any unobserved, time-invariant characteristics which might vary between more and less exposed firms.

Finally, we cluster standard errors at the main lender-level to account for the correlation of residuals across firms that share the same primary lender and are therefore exposed to similar treatment effects (Khwaja and Mian, 2008).[19]

3.2 Exposure to the sovereign shock and credit availability

We begin by demonstrating that the outbreak of the sovereign crisis impaired access to credit for firms borrowing from lenders highly exposed to distressed sovereigns. Figure 1, panel a, presents the dynamic effect of exposure to the sovereign shock on firms' cumulative growth rate of bank credit ($\Delta_\tau \text{Credit}_j$), estimated according to model (3). The full regression output is reported in Appendix C. A one standard deviation increase in lenders' exposure to GIPSI sovereigns corresponds to a (cumulative) reduction of about 18 percent of firms' total bank credit in the three years following the outbreak of the sovereign crisis.

The sovereign shock affected not only firms' access to external finance but also the cost of that finance. We do not have direct information on bank-specific lending rates. Therefore, we construct a proxy for firms' average financing costs using the ratio of financial charges to financial debt from the AA data ($\Delta_\tau fc_j$), and study how this measure of financing costs changes in the aftermath of the Greek bailout based on the firm's exposure to banks with varying holdings of distressed sovereigns.[20] While this

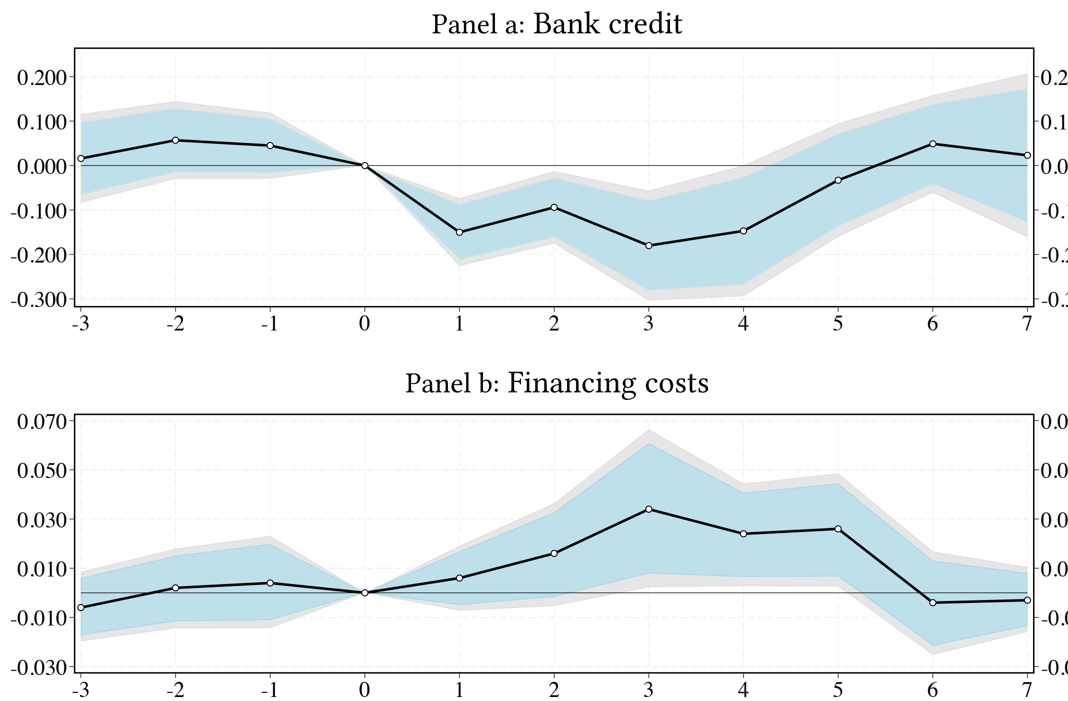
[18]Region fixed effects indicate in which of the three Belgian regions (the Flemish region, the Walloon region, and the Brussels-Capital region) the firm is headquartered. Industry fixed effects are measured using the industry code of the main product of the firm (measured in terms of production value in PRODCOM).

[19]As a robustness exercise, we also experimented with the Adao et al. (2019) procedure for computing standard errors and find that our results are robust, and if anything our clustered standard errors are more conservative.

[20]We measure average financing costs as $fc_{j,t} = \frac{\text{Financial Charges}_{j,t}}{\text{End of Year Financial Debt}_{j,t-1}}$ and compute their change

is admittedly a noisy measure of the interest rates paid by firms, it is still potentially informative about the direction and timing of the change in financing costs facing firms. Figure 1, panel b, shows that a one standard deviation increase in lenders' exposure to GIPSI sovereigns eventually leads to an increase in the average cost of finance by about 3 percent in the years following the outbreak of the European sovereign crisis. Taken together, the movement of the quantity and cost of finance in *opposite* directions is consistent with a tightening of credit supply conditions, as a contraction in credit demand would have led to a reduction of both quantity and prices.

Figure 1: Exposure to the sovereign shock and credit market outcomes



Notes: This figure explores the relationship between firms' exposure to the sovereign shock and the cumulative growth rate of bank credit (panel a) and change in financing costs (panel b) estimated using model (3). The sky blue shaded areas depict 90 percent confidence intervals and the gray shaded areas depict 95 percent confidence intervals based on the estimated clustered standard errors.

To interpret this credit contraction as the causal effect of shocks to credit supply, it must be the case that, absent the sovereign debt crisis, firms borrowing from banks with high GIPSI exposure would not have experienced a differential change in their credit supply relative to firms borrowing from banks with low exposure. Two pieces of evidence

relative to 2009 ($\Delta_\tau fc_j = fc_{j,2009+\tau} - fc_{j,2009}$).

lend support to this parallel trends assumption. First, Table A.1 in Appendix A shows that the sample of firms borrowing from more and less exposed lenders appears well-balanced on observable pre-shock characteristics, including size, bank leverage, productivity, and price level. Second, in direct support of the assumption, Figure 1 shows no differential trends in credit market outcomes between more and less affected firms prior to the sovereign shock.

In Appendix C, we present a series of additional empirical results and robustness tests. First, to provide further evidence that our results are driven by a sudden tightening of credit supply, rather than by demand-side factors, we leverage the availability of micro-data on individual firm-bank relationships and estimate a version of model (3) at the firm-bank relationship level, augmenting the regression model with firm-level fixed effects. This within-firm specification allows us to test whether banks with higher GIPSI holdings reduced their credit supply to the *same firm* relative to banks with lower GIPSI holdings, thereby controlling for unobservable changes in firm-specific factors, such as a contraction in credit demand or a worsening of firms' credit worthiness. The results indicate that more exposed banks indeed reduced lending relative to less exposed banks lending to the same firm. In addition, while the within-firm estimates are largely unaffected by whether we include firm-fixed effects, the R^2 of the regressions increase by a factor of seven to thirteen, depending on the time horizon, after inclusion of the fixed effects. In the spirit of Oster (2019), this observation suggests that while unobserved firm-specific factors (e.g., changes in credit demand) are important for explaining overall variation in bank lending to firms, this variation is not correlated with exposure to the sovereign shock.

Second, we study the impact of the shock on different types of credit: term loans and credit lines. Prior literature highlights how both products are used by firms to finance their production as well as their innovation activity (see, e.g., Hall and Lerner, 2010a and Manso, 2011). We find a significant contraction in the amounts borrowed across different credit types, suggesting that the credit tightening impacted various aspects of firms' financing.

Finally, as an additional validation exercise, we analyze the real effects of financial shocks on firms' input demands. Prior studies documented a contraction in investments and employment by firms experiencing a credit tightening.[21] Consistent with this

[21]See, e.g., Chodorow-Reich (2014) for employment, Cingano et al. (2016) for investments, and Bottero et al. (2020) for both employment and investments.

evidence, we also find that firms more exposed to the sovereign shock display a persistent contraction in the cumulative investment rate in machinery and equipment and a reduction of employment growth relative to less exposed ones.

4 Decoupling productivity and price effect of financial shocks

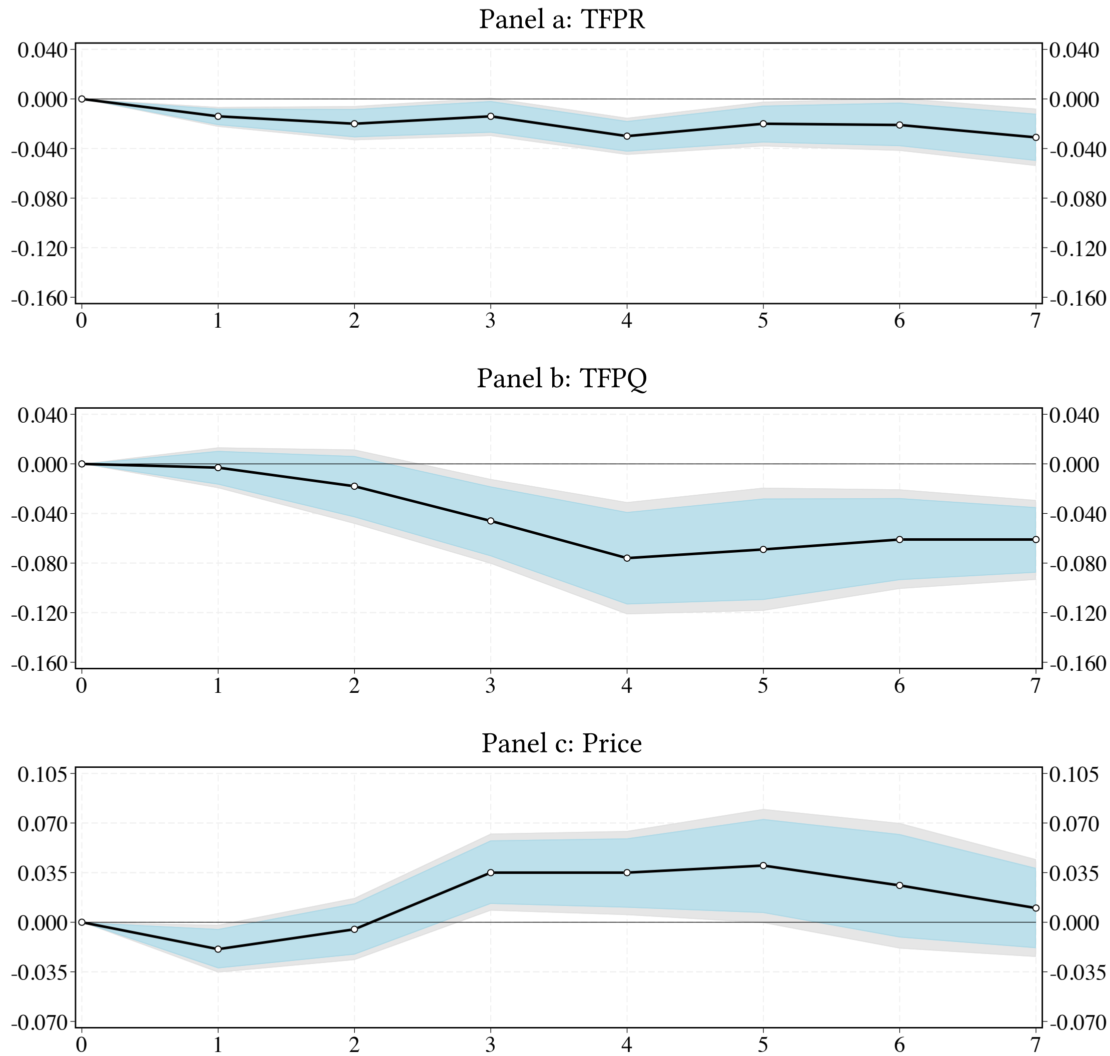
4.1 Productivity and pricing effects

Having established the pass-through of lenders' balance sheet shocks to firms' credit supply, we now turn to quantifying the separate effects of the credit tightening on firm-level productivity and prices. Figure 2 presents the estimated cumulative responses according to model (3). The full regression output is reported in Table A.6 in the Appendix.

Productivity effects. We begin by examining the response of revenue productivity (TFPR), a commonly used proxy for physical productivity in the literature when firm-level price data is unavailable. The estimates in panel a confirm that, in line with previous findings (see, e.g., Manaresi and Pierri, 2018), the exposure to a credit supply shock leads to a statistically and economically significant contraction of revenue productivity growth that materializes in the immediate aftermath of the shock and persists over time.

The TFPQ response, however, paints a substantially different picture regarding both the timing and magnitude of the impact of credit tightening on firms' productivity growth (panel b of Figure 2). First, in stark contrast to the TFPR estimates, credit supply shocks have *no impact* on firms' technical productivity growth in the short run.

Figure 2: Response of productivity and prices to negative credit supply shocks



Notes: This figure plots the coefficient estimates (solid lines) and associated confidence intervals capturing the effect of the credit supply shock on firm-level revenue productivity (TFPR), technical productivity (TFPQ), and prices. The sky blue shaded areas depict 90 percent confidence intervals and the gray shaded areas depict 95 percent confidence intervals based on the estimated clustered standard errors.

The estimated effect becomes economically sizable and statistically significant only three years after the shock. Second, revenue-based measures also offer a biased prediction regarding the long-run effects of the shock on physical productivity growth. While TFPR and TFPQ move in the same direction over the medium-to-long run, the estimated contraction in productivity growth is about twice as as large as that suggested by the revenue-based estimates. A one standard deviation exposure to the shock translates into a contraction of 6 percent in firms' physical productivity growth by the end of our sample period. Combined with the effects of the shock on firm-level credit growth, these estimates imply a long-run elasticity of firm-level physical productivity to credit supply of approximately 0.4, which is *twice as large* than the elasticity implied by the revenue-based estimates.

Pricing effects. The bifurcation between the revenue-based and quantity-based productivity growth effects is driven by a statistically significant and economically meaningful adjustment of firms' output prices in response to the tightening of financial conditions. A one standard deviation increase in exposure to the credit shock leads, on average, to an immediate reduction of about 2 percent in firms' output prices (panel c of Figure 2). The short-term reduction of output prices is consistent with empirical findings in previous works documenting how firms adjust their short-term pricing policies in response to a deterioration of financing conditions (Borenstein and Rose, 1995; Busse, 2002; Phillips and Sertsios, 2013; Kim, 2020). However, the price contraction is short-lived as firms that were more exposed to the credit shock eventually increase their prices relative to less exposed firms. A one standard deviation increase in exposure to the credit shock implies a cumulative increase in output prices of up to 4 percent in the five years following the shock, before reverting back to zero by the end of our sample period.

Taken together, the empirical evidence reveals that, in the short-run, estimates based on TFPR are substantially *upward biased*, whereas over longer horizons, they are substantially *downward biased*. Importantly, while short-run adjustments in revenue-based productivity solely pick up the movements in output prices, in the same way, the subsequent rebound of output prices explains why inference based on revenue-based measures substantially underestimates the long-run slowdown of physical productivity growth. Previous studies have emphasized how supply side shocks—such as productivity innovations—are passed-through to output prices, generating a muted, or even opposite, response of TFPR relative to TFPQ (Foster, Haltiwanger, and Syverson, 2008; Foster, Haltiwanger, and Syverson, 2016; Moreira, 2020). Our analysis indicates that similar forces can also explain long-run price dynamics (and thus the implied TFPR-TFPQ bifurcation) following episodes of financial market distress, emphasizing the important role played by the availability and cost of external finance for firms' production and pricing decisions.

4.2 Robustness analysis

In Appendix D, we present a series of robustness checks that validate the estimated productivity and pricing effects.

We first show that the estimated effects of financial shocks on productivity growth are robust to alternative ways of measuring productivity. We repeat the production function estimations assuming a less flexible, but more traditional, Cobb-Douglas functional form. Additionally, instead of estimating the production function parameters, we calibrate input elasticities based on the average revenue shares within each industry (index function approach). Finally, we developed an alternative production function estimation procedure that accounts for the possibility of working capital constraints distorting the first-order condition of intermediate inputs. In all cases, the estimates are comparable to the ones obtained by our baseline production function estimation approach.

As is typically the case, we cannot directly measure capacity utilization in the data. Therefore, our productivity estimates could be biased if firms adjust capacity utilization in response to the financial shock. To address this concern, we were able to merge supplementary survey data on firm-level capacity utilization for a subsample of firms in our sample. Examining the response of this variable to the shock, we find that the financial shock leads to a positive but economically and statistically small increase in capacity utilization. Since not accounting for an increase in capacity utilization would likely lead to an upward bias in the TFPQ estimates, these findings suggest that, if anything, we are underestimating the effects of the financial shock on firm-level TFPQ growth. That is, our baseline estimates might provide a lower bound of the effect of the shock on firm-level productivity.

We then assess the robustness of the pricing effects. As explained in Section 2.2, when constructing a firm-level price index, one needs to take a stance on how to aggregate the prices across the heterogeneous products produced by a firm. In our baseline specification, we use a conventional Törnqvist index. In Appendix D, we show that the estimated initial contraction, and subsequent rebound, of prices following the financial shock is also evident when one uses alternative price measures. Specifically, we demonstrate that our results remain robust when constructing the firm-level price index as the revenue-share weighted average of product-level prices or when using only the price of the firm's main product (the product with the highest revenue share).

As discussed above, in order to perform the production function estimation we constructed a firm-level measure of output produced, X_{jt}, adjusting firm-level revenues by the change in inventories. To do so, we deflated the total change in inventories (in euros) by our price index, which might be a source of bias if firms reduce the prices of different products depending on their product-specific inventory stock. To address this concern, we re-estimated our baseline regressions on the subsample of single-product firms, finding estimates that are quantitatively similar, though less precisely estimated due to the smaller sample size.

Another concern is related to possible survival bias. About one-third of the firms in our regression sample in 2009–2010 are not in the regression sample by the end of our sample period. In Appendix A we discuss how this appears to be driven largely by the sampling scheme adopted by PRODCOM and survey attrition, rather than by selection induced by the financial shock. As an additional robustness test against survival bias, we show that the productivity and price estimates remain when re-estimating our baseline regressions on the subsample of permanent firms.

5 Transmission mechanisms

Having decoupled the effects of financial shocks on firm's productivity growth and pricing policies, we now provide evidence regarding the economic mechanisms underlying both responses. We show that in the immediate aftermath of the financial shock firms take actions to counteract the liquidity shortage that arose due to the drop in external financing. We document that producers reduce output prices in an attempt to increase cash flows from the product market by liquidating their existing stock of final goods. At the same time, firms exposed to the shock reduce operating costs by cutting expenditures on investments in innovation, which explains the persistent, but delayed, negative impact on long-run productivity growth. This productivity slowdown, combined with the increase in financing costs, explains the long-run increase in prices, as increases in the cost of production are passed-through to customers.

5.1 Transmission of financial shocks to productivity growth

Innovation in production processes, human capital accumulation, and organizational changes are the engine of firms' productivity growth (Syverson, 2011).[22] The availability of external financing plays a central role in this process. Like any form of investment, innovation requires financing (Kerr and Nanda, 2015; Howell, 2017). Compared to other forms of investment, productivity-enhancing investments have delayed returns and tend to provide poor collateral to creditors (Shleifer and Vishny, 1992; Caggese, 2012). Therefore, they are among the first expenses cut by firms coping with a tightening of credit supply conditions (Almeida and Campello, 2007).

The data provide strong support in favor of the hypothesis that the transmission of financial shocks to firms' productivity growth operates through an innovation channel. We first show that firms reduced investments in innovation in response to the credit supply shock. We then provide evidence linking these reductions to sizable contractions in long-run productivity growth.

[22] Garcia-Macia (2017), Huber (2018), Anzoategui et al. (2019) highlight that reduced investments in intangible assets over time can lead to a slowdown of firms' productivity growth. Bloom et al. (2013) emphasizes the role of information technology investments and organizational capital in generating productivity increases at the firm level.

Table 2: Response of productivity-enhancing activities and TFPQ growth

Panel a: *Response of productivity-enhancing activity to the financial shock*

Dep. Var. ↓	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 7$
Inv. Rate R&D	-0.042***	-0.109***	-0.338***	-0.586***	-0.479	-0.267	-0.394
	(0.012)	(0.017)	(0.080)	(0.198)	(0.321)	(0.527)	(0.522)
Any R&D Expense	-0.044***	-0.020	-0.072***	-0.038	-0.039	-0.022	0.035
	(0.015)	(0.024)	(0.017)	(0.026)	(0.028)	(0.030)	(0.037)
Training Expenses	-0.225***	-0.033	0.138	0.119	0.128	0.231	0.333*
	(0.069)	(0.07)	(0.089)	(0.113)	(0.131)	(0.172)	(0.162)

Panel b: *Response of productivity to contraction in productivity-enhancing activity*

	Dep. Var: $\Delta_\tau \ln TFPQ$						
Endogenous Var. ↓	$\tau = 1$	$\tau = 2$	$\tau = 3$	$\tau = 4$	$\tau = 5$	$\tau = 6$	$\tau = 7$
Inv. Rate R&D	0.124	0.591	1.498***	2.243**	2.547**	1.589***	2.178**
	(0.223)	(0.401)	(0.535)	(0.979)	(1.188)	(0.594)	(0.968)
Any R&D Expense	0.108	0.548	1.324**	1.650**	1.790*	1.271**	1.438***
	(0.195)	(0.421)	(0.538)	(0.741)	(0.888)	(0.538)	(0.512)
Training Expenses	-0.039	0.034	0.317	0.643*	0.410**	0.373*	0.438*
	(0.045)	(0.080)	(0.251)	(0.359)	(0.191)	(0.223)	(0.231)

Notes: Panel a reports the estimates of the cumulative effect of the credit supply shock on investments in productivity-enhancing activities (R&D and employee training expenses) estimated using the model in (3). Panel b reports the 2SLS estimates capturing the effect of variation in R&D and training expenses in the aftermath of the credit supply shock, instrumented with the credit supply shock, on cumulative TFPQ growth over different horizons, estimated using the model in equation (4). Standard errors are clustered at the main-lender level and reported in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level.

Innovation response to financial shocks. We compute three indicators of firm expenditures on productivity-enhancing activities. First, for each year following the burst of the sovereign crisis, we compute the R&D investment rate (Inv. Rate R&D$_\tau$, which is the ratio of cumulative expenses on R&D up to year 2009+τ ($\tau = \{1, ..., 7\}$) scaled by the stock of intangible assets in 2009. Our second indicator is a dummy variable that identifies firms investing any positive amount in R&D in a given year (Any R&D Expense$_\tau$). This variable captures the extensive margin of innovation, accounting for the lumpy nature of R&D investments. Our third indicator recognizes that innovation spurs from R&D as long as a skilled and appropriately trained workforce is capable of integrating new technologies into the existing production processes (Hall and Lerner, 2010b). To capture this aspect, we gather information on employee training expenditures (Training Expenses$_\tau$). Specifically, we calculate cumulative average training expenditures per employee, scaled by expenditures per employee in 2009.[23]Table 2, panel a, shows that firms more exposed to the credit supply shock reduce investments in innovation and training more than less exposed counterparts. For several years after the outbreak of the sovereign crisis, firms borrowing from lenders more exposed to distressed sovereigns display a widening innovation gap. We estimate that, on average, a one standard deviation increase in lenders' exposure to the distressed securities translates into a drop of about 4 percent in the R&D investment rate after one year, and a reduction of up to 59 percent in the cumulative R&D investment rate four years later. The effect of the credit contraction is also evident if one looks at the extensive margin of R&D investments, with a reduction of over 4 percentage points in the probability of devoting any resources to R&D in the year after the shock. Investments in human capital are also affected. Comparing two firms with a one standard deviation difference in lenders' exposure to the shock, we observe that the more exposed firm cuts expenditures on training by about 20 percent more per employee. The effect on training is more transitory relative to the estimated effects on R&D.[24]

Impact of innovation expenditures on productivity growth. We take our analysis one step further and provide direct evidence connecting the availability of external financing, productivity-enhancing activities, and productivity growth. Mirroring model (3), we run a sequence of IV-linear projections at different horizons:

$$\Delta_\tau \ln TFPQ_j = \alpha_\tau \cdot \Delta_1 Z_j + \Gamma'_{K,\tau}\mathbf{K}_j + \Gamma'_{X,\tau}\mathbf{X}_j + i_{ind,\tau} + i_{reg,\tau} + u_{j\tau}. \tag{4}$$

The left-hand-side variable measures the *cumulative* growth rate of TFPQ between the year 2009 and year 2009 + τ, $\tau = \{1, ..., 7\}$. The (endogenous) regressors of interest, $\Delta_1 Z_j$, measure changes in investments in innovation from 2009 to 2010 (R&D and training

[23]Recall that Table A.5 shows that firms also decrease their investment in machinery and equipment in response to the shock. To the extent that these expenditures reflect firms upgrading to more productive vintages (as opposed to maintaining existing vintages), a reduction in these expenditures in response to the shock could also generate a reduction in TFPQ growth.

[24]These results are in line with those documented in recent papers (Manaresi and Pierri, 2018; Duval, Hong, and Timmer, 2020), suggesting that the contraction in credit supply reduces productivity growth because it forces firms to cut investments in productivity-enhancing activities. They are also consistent with Caggese (2019), which provides evidence linking financial frictions and productivity growth over a firm's life cycle through the impact that such frictions have on the ability to sustain more radical innovation.

expenditures), which we have just shown are affected by the contraction in credit supply. These changes in investments are instrumented with our credit supply shock (Shock_j) in order to isolate variation in expenditures that is driven by firms' differential exposure to the credit tightening. This estimation approach allows us to tease out the credit supply driven connection between two endogenous variables (productivity and investments), whose covariation could otherwise be determined by factors other than the availability of external financing.

Table 2, panel b, reports the estimates over different horizons. The innovation gap materializes into lower productivity growth, as evidenced by the positive estimated coefficients. The timing of the effect is as relevant as its direction. A contraction of productivity-enhancing investments, driven by the lack of financing possibilities, is not felt immediately but rather materializes into a productivity slowdown in the medium-long run. For example, we estimate that a one percent reduction in the R&D investment rate in 2010 translates into a reduction of productivity growth of over 2 percent six years later. Similarly, a reduction in training expenses per employee by one percent translates into 0.4 percent lower productivity growth six years later. These results offer direct evidence of the link between productivity growth and firms' decisions to innovate. More specifically, the delayed and persistent productivity response documented by our analysis helps rationalize the slow economic recovery observed after financial crisis.[25]

Other transmission mechanisms. We note that the connection between financial shocks and firm-level productivity dynamics could also operate through other channels besides the investment channel. While we do not directly test these alternative theories, our earlier results from Figure 2 offer insights regarding their empirical relevance. In light of the negative long-run response of productivity growth, we can rule out economic channels predicting that a tightening of external financing conditions might spur productivity growth because, for example, it forces firms to cut production slackness (Field, 2003) or be more selective in their investment projects (Jensen, 1986). The timing of the TFPQ response further narrows the set of channels that produce predictions consistent with the data. Specifically, our findings are inconsistent with the hypothesis that financial shocks affect firms' technical efficiency because they force firms to inefficiently use their

[25] See, among others, the evidence in Cerra and Saxena (2008), Jordà, Schularick, and Taylor (2013), Reinhart and Rogoff (2014), and Hall (2015).

resources, for example because a lack of working capital impedes certain input purchases, or because the shock shifts managers' attention towards seeking alternative sources of financing and away from maximizing efficiency. In fact, in both cases, one would expect to see an immediate productivity effect that gradually fades as firms regain access to credit markets, which is the opposite of what our TFPQ estimates indicate.

5.2 Transmission of financial shocks to pricing policies

We now examine the economic forces that lead firms to adjust their pricing behavior in response to tightening credit supply conditions and why these responses differ depending on the time horizon.

Long-run price adjustment. Consider first the long-run price dynamics. The estimates in Figure 2 indicate a gradual increase in output prices and subsequent mean reversion. One natural explanation for this is that the shock eventually led to an increase in production costs, and firms passed this through to consumers. The empirical analysis presented so far provides two pieces of evidence to support this idea.

First, as shown in Section 3, firms were eventually able to compensate for the contraction in credit, but only by relying on more expensive sources of financing. This finding is consistent with the ones in Barth III and Ramey (2001) and Christiano et al. (2015), whereby higher financing costs leads to a rise in the cost of working capital, which increases firms' production costs. Secondly, financial shocks set firms on a lower (long-run) productivity growth path. To the extent that firms pass through efficiency gains to consumers in the form of lower prices, ceteris paribus, firms more affected by the credit shock will price at a higher level relative to similar, less affected competitors.

Short-run price adjustment. In contrast to the long-run increase in prices, in the short-run we find that firms more affected by the credit crunch reduce their prices relative to less affected ones. We show that this adjustment can be explained by firms using low pricing as a source of internal finance in an effort to counterbalance the drop in external financing.

Recognizing the increased value of liquidity, firms have the option to liquidate assets or reduce operating costs in order to increase cash flows. As discussed in Section 3.2 and 5.1, we do find evidence that firms exposed to the credit crunch reduce investments in machinery and equipment, employment growth, and investments in intangibles. However, firms may be limited in their ability or willingness to leverage these options as these actions might impact current revenues by reducing output produced, or, as we have shown in Section 5.1, have severe long-term consequences for firm productivity and thus firm value. [26]

An alternative option is to raise liquidity from the product market, by selling their inventories at discounted prices. While this behavior would be sub-optimal in normal circumstances, selling off inventories can help firms generate additional cash flows when the financial shock makes liquidity is particularly valuable. As a first piece of evidence for this hypothesis, we show that producers that were more likely to be impacted by the credit crunch are those that display sharper adjustments of their pricing policies.

Table 3 shows how the short-term price response to the credit shock varies depending on the importance of the bank-credit shock for firms' financing. Column (1) shows that firms that entered the crisis with higher leverage (the ratio of bank debt to total assets at the end fiscal year 2009) reduced prices more aggressively when coping with the credit crunch. Column (2) shows that price reduction is increasing in the likelihood of financial distress (measured by the Z-score at the end of fiscal year 2009), as firms in precarious financial conditions are more affected by debt rollover risk.[27] In fact, the data indicates that the credit shock had practically no impact on the pricing behavior of firms that entered the sovereign crisis with strong balance sheets.

Next, we present empirical evidence linking pricing and inventory adjustments. As documented by Kim (2020), firms with higher levels of existing inventories should be better able to exploit low pricing as a form of liquidity management, as liquidating existing inventories does not involve incurring additional production costs.[28] We find strong support for this prediction in the data. First, in Columns (4) and (5) we show that borrowers more exposed to the credit supply shock did indeed liquidate some of the

[26]In principle, reducing wages is another potential option to cut operating costs without impacting a firm's production capacity. We find no evidence that firms reduce wages in response to the shock. Unlike other countries with more flexible labor markets (see e.g., Chan et al., 2023 in the context of Denmark), collective bargaining plays a dominant role in shaping employment compensations in Belgium, which prevents firms from unilaterally downard-adjusting wages (Alvarez et al., 2006).

[27]The Z-score (Altman, 1968) is a credit-strength test that gauges a company's likelihood of bankruptcy. A score below 1.8 indicates a likelihood of bankruptcy, while a score above 2.9 signals a very low likelihood of financial distress. See Appendix A for Z-score construction' details.

[28]Previous work has shown that liquidity constrained firms also shed inventories in response to demand and monetary policy shocks. See, e.g., Gertler and Gilchrist (1994) and Kashyap, Lamont, and Stein (1994).

inventories in the immediate aftermath of sovereign shock relative to less exposed firms. This response is primarily driven by firms that entered the crisis with larger inventory holdings.[29]

Importantly, firms that can count on larger inventory stocks to liquidate more likely use low pricing as a source of internal finance, cutting output prices more aggressively in response to the credit shock (column 6). A one-standard deviation increase in exposure to the shock leads to a relative contraction in output prices of over 2 percent for a firm with 27 cents worth of inventories per euro of assets (75th percentile), while a firm with 9 cents worth of inventories per euro of assets (25th percentile) reduced output prices by 1.5 percent. Underscoring the external validity of the analysis, we note that our estimates are consistent in both direction and magnitude with those reported in Kim (2020), estimated using consumer price data for a sample of US firms whose lenders were differentially exposed to the Lehman Brothers' default.

5.3 Linking the productivity and price effects of financial shocks

In this section we provide evidence showing that the price and productivity effects of financial shocks are in fact linked. We begin by documenting a statistical relationship between the causal effect of financial shocks on short-run pricing policies and long-run productivity growth. We compute the contribution of each firm to the average short-term price effect ($\hat{\beta}_1$) and the average long-run TFPQ growth effect ($\hat{\beta}_7$) presented in the impulse-responses in Figure 2 (panels b and c).[30] We then group firms into percentiles based on their contribution to the short-term pricing response. The binned scatter plot in Figure 3, panel a shows the average contribution to the long-term TFPQ response (y-axis) within each group of firms, sorted by to their contribution to the short-term pricing response (x-axis). We find a strong negative correlation between firms' short-term

[29]Our firm-level inventory measure includes finished goods, semi-finished goods, and raw materials, all measured at the end of 2009. In unreported results, we show that our results are driven by inventories of semi-finished goods and raw materials. This is consistent with the idea that many of the finished goods in inventory at the end of 2009 were sold by the time the credit supply shock arrives (April 2010), and that firms use inventories of semi-finished goods and raw materials (some of which have likely already been converted to final goods when the shock arrives) to respond to the shock.

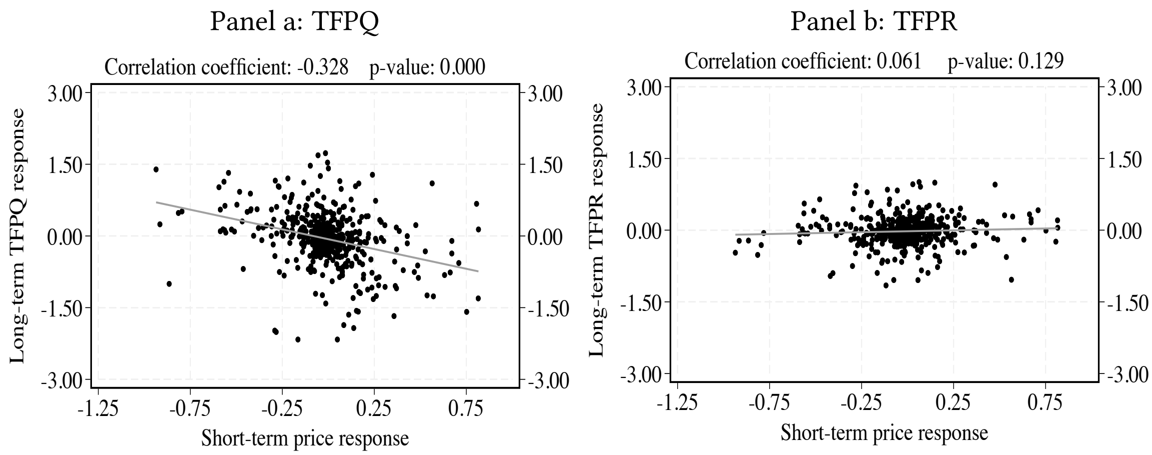
[30]The contribution of each firm to the average short- and long-run treatment effects ($\hat{\beta}_\tau$, $\tau = 1, 7$) of productivity and prices are obtained using the influence function method (Cook and Weisberg, 1982). We rescale the influence functions so that the average contribution across observations (firms) equals the estimated treatment effects at each horizon.

Table 3: Heterogeneous pricing effects

	Heterogeneous impact of the credit shock			*Inventory channel*						
	Short-run effect			Short-run effect			Short-run effect			Long-run effect
	$\Delta \ln P$			$\Delta \ln Inv$		$\Delta \ln P$	Inv. Rate R&D	Any R&D Expense	Training Expenses	$\Delta \ln TFPQ$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Shock	-0.013 (0.008)	-0.034*** (0.010)	-0.022*** (0.008)	-0.041*** (0.008)	-0.020* (0.012)	-0.012 (0.007)	-0.068*** (0.010)	-0.094*** (0.016)	-0.289*** (0.071)	-0.110*** (0.029)
Shock × Bank Leverage	-0.034*** (0.014)									
Shock × Z-score		0.007*** (0.003)								
Shock × Safe			0.016* (0.009)							
Shock × Firm Inv.					-0.114** (0.053)	-0.033* (0.017)	0.136** (0.054)	0.237*** (0.078)	0.312** (0.141)	0.238** (0.111)
Low inv. holdings					-0.030*** (0.009)	-0.015** (0.007)	-0.056*** (0.011)	-0.073*** (0.014)	-0.262*** (0.067)	-0.086*** (0.023)
High inv. holdings					-0.050*** (0.011)	-0.021*** (0.008)	-0.033* (0.016)	-0.032 (0.019)	-0.209*** (0.065)	-0.045** (0.017)
Low-High inv. holdings					0.020** (0.010)	0.006* (0.003)	-0.023** (0.009)	-0.041*** (0.013)	-0.053** (0.024)	-0.041** (0.020)
R-squared	0.069	0.063	0.063		0.069	0.063	0.048	0.104	0.034	0.106
Observations	1024	1024	1024		1024	1024	775	775	703	650

Notes: This table reports the estimates of the heterogeneous effect of the credit supply shock on short-term ($\tau = 1$) prices and expenditures in productivity-enhancing activities and long-term ($\tau = 7$) TFPQ growth, estimated using model (3). The baseline model is augmented to include an interaction between firm-level measures of exposure to the credit shock (Columns (1)–(3)) and firm-level inventory stocks (Columns (4)–(9)). The middle panel reports the estimated effects evaluated at different points of the distribution of firm-level inventory holdings (Low and High inventory holdings, corresponding to the 25th and 75th percentiles, respectively) and their difference. Clustered standard errors are reported in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level.

Figure 3: Linking the short-term price and long-term productivity response



Notes: These binned scatter plots show the correlation between firms' short-term price and long-term productivity response to the financial shock. In each plot, a dot represents the average contribution to the productivity response (y-axis) and the average contribution to the price response (x-axis) of observations that belong to a given percentile of the distribution of the price and productivity responses. The grey line is the best linear predictor of the long-run productivity effect given the short-term price effect.

price response and long-term productivity growth response, with a correlation coefficient of -0.328 (significant at the one percent level). This exercise reveals that firms that endogenously respond to the financial shock by pricing more aggressively are the ones that experience, in the long-run, a less pronounced contraction of physical productivity growth.

It is important to note that the revenue productivity estimates are unable to detect the inter-temporal relationship between the price and productivity responses to the financial shock, casting further doubt on inferences based on TFPR movements (panel b). Since firms affected by the shock eventually increase prices, the revenue productivity estimates suggest either no relationship or even a positive relationship between short-term price adjustments and long-run productivity.

We now provide evidence showing how the inventory and innovation channels interact to generate the link between short-term price and long-term productivity effects. Increasing cash flows through inventory sales and reducing innovation expenses are substitutable for the purpose of freeing up liquidity. Therefore, ceteris paribus, we expect that firms that are able to expand sales by selling inventories at discounted prices should be able to reduce their investments in innovation less, thereby mitigating the long-run impact of the credit shock on productivity growth.

To test this hypothesis, we examine how the effect of the shock on investments in productivity enhancing activities (R&D and training) varies with a firm's inventories. The results reported in columns (7)–(9) of Table 3 show that firms that can rely on a larger stock of inventories to liquidate are the ones that display a smaller contraction of both innovation expenses and investments in workers' human capital. Comparing firms at the 25th and 75th percentile of inventories, we find that firms that were better able to reduce prices in response to the shock (i.e., those with higher inventory levels) reduce their investments in innovation by between 20 and 60 percent less.

Finally, we show that firms' ability to reduce prices more, and therefore reduce investments in intangibles less, translates into significantly smaller contractions in long-run productivity. In column (10), we examine the heterogeneous response of long-term productivity growth as a function of firms' inventories. Consistent with the evidence provided by the non-parametric exercise in Figure 3, the estimates indicate that firms with greater ability to adjust prices in response to a financial shock systematically experience a lower contraction of long-run productivity growth in response to the shock. To put out estimates into perspective, firms that differ in their ability to respond by pricing more aggressively thanks to their possibility to tap into larger inventory stocks (25th vs 75th percentile of the distribution of inventories) experience a contraction in TFP growth that is almost 50 percent smaller.

6 Conclusions

This paper sheds new light on the nexus between financing frictions and firm-level productivity growth. Using detailed administrative records of firm-level output prices and quantities, combined with quasi-experimental variation in credit availability, we systematically explore the relationship between a tightening of financing conditions and firm productivity growth, emphasizing the crucial role of firm price adjustments in quantifying and understanding this relationship.

By disentangling the effects of pricing and productivity, we document that financial shocks have no immediate impact but instead cause a substantial, delayed, and persistent long-term effect on firm-level technical productivity growth. We show that this occurs because a tightening of external finance conditions leads to a contraction in investments in intangible assets, such as R&D and worker human capital, setting firms on a lower productivity growth path. Importantly, because firms adjust their pricing policies to cope with the shock, we also document that revenue-based productivity measures provide biased estimates and potentially misleading predictions regarding the implications of financial shocks on firm productivity in both the short and long term.

These findings have significant welfare implications. For once, they support the hypothesis that the slow economic recovery observed after episodes of financial market distress is driven, at least in part, by a slowdown in firm-level productivity growth, and highlight that this channel's impact on long-term growth is more pronounced and longer-lasting than previously understood. For another, the long-run increase in output prices, driven by the pass-through of financial costs and by the productivity slowdown, further exacerbates the impact of financial shocks on consumers.

This study also emphasizes that understanding and accounting for the endogenous price response to financial shocks extends beyond measurement considerations. Financial shocks jeopardize a firm's capacity to sustain productivity growth through investments in innovation and human capital. We are the first to document how the ability to generate additional cash flows via low pricing in the product market enables firms to mitigate this effect, thereby softening the long-term impact of the shock on productivity. This mechanism highlights that the nominal and real impacts of financial shocks are more interconnected than previously recognized. The connection between the behavior of firms in product markets and productivity growth is an active area of research. For example, research shows that product market conditions shape aggregate productivity through misallocation effects and firm selection (see, e.g., Restuccia and Rogerson, 2017 and Syverson, 2004). This paper offers new insights that further connect the two by showing that firms' actions in product markets can help mediate the effect of financial shocks on within-firm productivity growth.

Due to data availability, this study focuses on manufacturing firms. As economies transition toward service-oriented structures, it would be relevant to extend our analysis to the services sector. Due to its higher demand cyclicality and limited collateralizable assets, financial crises could result in even more severe financial frictions in services compared to manufacturing. In that case, we would expect to find even larger contractions in innovation and productivity growth than the ones presented in this paper. Moreover, while in this study we exploit data from Belgium, we believe our findings are more broadly applicable to other countries, particularly other EU countries that share a similar market institutions and dependence on external finance and to some extent to the UK and the US. We leave these as topics for future research.

References

Adao, R., M. Kolesár, and E. Morales (2019): "Shift-share designs: Theory and inference," *The Quarterly Journal of Economics*, 134, 1949–2010.

Almeida, H. and M. Campello (2007): "Financial Constraints, Asset Tangibility, and Corporate Investment," *The Review of Financial Studies*, 20, 1429–1460.

Altman, E. I. (1968): "Financial Ratios, Discriminant Analysis and the Prediction of Corporate Bankruptcy," *The Journal of Finance*, 23, 589–609.

Alvarez, L. J., E. Dhyne, M. Hoeberichts, C. Kwapil, H. Le Bihan, P. Lünnemann, F. Martins, R. Sabbatini, H. Stahl, P. Vermeulen, et al. (2006): "Sticky prices in the euro area: a summary of new micro-evidence," *Journal of the European Economic association*, 4, 575–584.

Amihud, Y. and H. Mendelson (1983): "Price smoothing and inventory," *The Review of Economic Studies*, 50, 87–98.

Amiti, M. and D. E. Weinstein (2011): "Exports and Financial Shocks," *The Quarterly Journal of Economics*, 126, 1841–1877.

Angelini, P., G. Grande, and F. Panetta (2014): "The Negative Feedback Loop Between Banks and Sovereigns," Bank of Italy Occasional Working Paper.

Anzoategui, D., D. Comin, M. Gertler, and J. Martinez (2019): "Endogenous Technology Adoption and R&D as Sources of Business Cycle Persistence," *American Economic Journal: Macroeconomics*, 11, 67–110.

Barth III, M. J. and V. A. Ramey (2001): "The cost channel of monetary transmission," *NBER Macroeconomics Annual*, 16, 199–240.

Becker, B. and V. Ivashina (2018): "Financial Repression in the European Sovereign Debt Crisis," *Review of Finance*, 22, 83–115.

Bernanke, B. S., C. S. Lown, and B. M. Friedman (1991): "The Credit Crunch," *Brookings Papers on Economic Activity*, 1991, 205–247.

Bloom, N., B. Eifert, A. Mahajan, D. McKenzie, and J. Roberts (2013): "Does Management Matter? Evidence from India," *Quarterly Journal of Economics*, 128, 1–51.

Blum, B. S., S. Claro, I. Horstmann, and D. A. Rivers (2024): "The ABCs of Firm Heterogeneity When Firms Sort into Markets: The Case of Exporters," *Journal of Political Economy*, 132, 1162–1208.

Bocola, L. (2016): "The Pass-Through of Sovereign Risk," *Journal of Political Economy*, 124, 879–926.

Borenstein, S. and N. L. Rose (1995): "Bankruptcy and Pricing Behavior in US Airline Markets," *American Economic Review*, 85, 397–402.

Bottero, M., S. Lenzu, and F. Mezzanotti (2020): "Sovereign Debt Exposure and the Bank Lending Channel: Impact on Credit Supply and the Real Economy," *Journal of International Economics*, 126.

Busse, M. (2002): "Firm Financial Condition and Airline Price Wars," *RAND Journal of Economics*, 33, 298–318.

Caggese, A. (2012): "Entrepreneurial Risk, Investment, and Innovation," *Journal of Financial Economics*, 106, 287–307.

——— (2019): "Financing Constraints, Radical versus Incremental Innovation, and Aggregate Productivity," *American Economic Journal: Macroeconomics*, 11, 275–309.

Cerra, V. and S. C. Saxena (2008): "Growth Dynamics: The Myth of Economic Recovery," *American Economic Review*, 98, 439–457.

Chan, M., S. Salgado, and M. Xu (2023): "Heterogeneous Passthrough from TFP to Wages," *Available at SSRN 3538503*.

Chevalier, J. A. (1995a): "Capital Structure and Product-Market Competition: Empirical Evidence from the Supermarket Industry," *American Economic Review*, 415–435.

Chevalier, J. A. and D. S. Scharfstein (1995): "Liquidity Constraints and the Cyclical Behavior of Markups," *American Economic Review*, 85, 390–396.

——— (1996): "Capital-Market Imperfections and Countercyclical Markups: Theory and Evidence," *American Economic Review*, 86, 703–725.

Chodorow-Reich, G. (2014): "The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008–9 Financial Crisis," *Quarterly Journal of Economics*, 129, 1–59.

Christiano, L. J., M. S. Eichenbaum, and M. Trabandt (2015): "Understanding the great recession," *American Economic Journal: Macroeconomics*, 7, 110–167.

Cingano, F., F. Manaresi, and E. Sette (2016): "Does Credit Crunch Investment Down? New Evidence on the Real Effects of the Bank-Lending Channel," *The Review of Financial Studies*, 29, 2737–2773.

Cole, H. L., J. Greenwood, and J. M. Sanchez (2016): "Why Doesn't Technology Flow from Rich to Poor Countries?" *Econometrica*, 84, 1477–1521.

Cook, R. D. and S. Weisberg (1982): *Residuals and Influence in Regression*, New York: Chapman and Hall.

De Loecker, J. and P. K. Goldberg (2014): "Firm Performance in a Global Market," *Annual Review of Economics*, 6, 201–227.

De Loecker, J., P. K. Goldberg, A. K. Khandelwal, and N. Pavcnik (2016): "Prices, Markups and Trade Reform," *Econometrica*, 84, 445–510.

Doraszelski, U. and J. Jaumandreu (2013): "R&D and Productivity: Estimating Endogenous Productivity," *Review of Economic Studies*, 80, 1338–1383.

Duval, R., G. H. Hong, and Y. Timmer (2020): "Financial Frictions and the Great Productivity Slowdown," *The Review of Financial Studies*, 33, 475–503.

Ericson, R. and A. Pakes (1995): "Markov-Perfect Industry Dynamics: A Framework for Empirical Work," *The Review of Economic Studies*, 62, 53–82.

Eslava, M. and J. Haltiwanger (2020): "The Life-Cycle Growth of Plants: The Role of Productivity, Demand and Distortions," Working Paper, NBER.

Eslava, M., J. Haltiwanger, A. Kugler, and M. Kugler (2013): "Trade and Market Selection: Evidence from Manufacturing Plants in Colombia," *Review of Economic Dynamics*, 16, 135–158.

European Commission (2014): "Quality Report on PRODCOM 2014 Annual Data," PRODCOM Working Group.

Field, A. J. (2003): "The Most Technologically Progressive Decade of the Century," *American Economic Review*, 93, 1399–1413.

Foster, L., J. Haltiwanger, and C. Syverson (2008): "Reallocation, Firm Turnover, and Efficiency: Selection on Productivity or Profitability?" *American Economic Review*, 98, 394–425.

——— (2016): "The Slow Growth of New Plants: Learning about Demand?" *Economica*, 83, 91–129.

Gandhi, A., S. Navarro, and D. A. Rivers (2020): "On the Identification of Gross Output Production Functions," *Journal of Political Economy*, 128, 2973–3016.

Garcia-Macia, D. (2017): *The Financing of Ideas and the Great Deviation*, International Monetary Fund.

Garcia-Marin, A. and N. Voigtländer (2019): "Exporting and Plant-level Efficiency Gains: It's in the Measure," *Journal of Political Economy*, 127, 1777–1825.

Gertler, M. and S. Gilchrist (1994): "Monetary Policy, Business Cycle, and the Behavior of Small Manufacturing Firms," *Quarterly Journal of Economics*, 109, 309–340.

Gilchrist, S., R. Schoenle, J. Sim, and E. Zakrajšek (2017): "Inflation Dynamics during the Financial Crisis," *American Economic Review*, 107, 785–823.

Hall, B. H. and J. Lerner (2010a): "The financing of R&D and innovation," in *Handbook of the Economics of Innovation*, Elsevier, vol. 1, 609–639.

——— (2010b): "The Financing of R&D and Innovation," in *Handbook of the Economics of Innovation*, Elsevier, vol. 1, 609–639.

Hall, R. E. (2015): "Quantifying the Lasting Harm to the US Economy from the Financial Crisis," *NBER Macroeconomics Annual*, 29, 71–128.

Haltiwanger, J., R. Kulick, and C. Syverson (2018): "Misallocation Measures: The Distortion That Ate the Residual," Working Paper, NBER.

Hendel, I. (1996): "Competition Under Financial Distress," *The Journal of Industrial Economics*, 44, 309–324.

Howell, S. T. (2017): "Financing Innovation: Evidence from R&D Grants," *American Economic Review*, 107, 1136–1164.

Hsieh, C.-T. and P. J. Klenow (2009): "Misallocation and Manufacturing TFP in China and India," *Quarterly Journal of Economics*, 124, 1403–1448.

Huber, K. (2018): "Disentangling the Effects of a Banking Crisis: Evidence from German Firms and Counties," *American Economic Review*, 108, 868–898.

Jensen, M. C. (1986): "Agency Costs of Free Cash Flow, Corporate Finance, and Takeovers," *American Economic Review*, 76, 323–329.

Jordà, Ò. (2005): "Estimation and Inference of Impulse Responses by Local Projections," *American Economic Review*, 95, 161–182.

Jordà, Ò., M. Schularick, and A. M. Taylor (2013): "When Credit Bites Back," *Journal of Money, Credit and Banking*, 45, 3–28.

Kashyap, A. K., O. A. Lamont, and J. C. Stein (1994): "Credit Conditions and the Cyclical Behavior of Inventories," *Quarterly Journal of Economics*, 109, 565–592.

Katayama, H., S. Lu, and J. R. Tybout (2009): "Firm-level Productivity Studies: Illusions and a Solution," *International Journal of Industrial Organization*, 27, 403–413.

Kerr, W. R. and R. Nanda (2015): "Financing Innovation," *Annual Review of Financial Economics*, 7, 445–462.

Khwaja, A. I. and A. Mian (2008): "Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market," *American Economic Review*, 98, 1413–1442.

Kim, R. (2020): "The Effect of the Credit Crunch on Output Price Dynamics: The Corporate Inventory and Liquidity Management Channel," *Quarterly Journal of Economics*, 136, 563–619.

Klette, T. J. and Z. Griliches (1996): "The Inconsistency of Common Scale Estimators when Output Prices are Unobserved and Endogenous," *Journal of Applied Econometrics*, 11, 343–361.

Lane, P. R. (2012): "The European Sovereign Debt Crisis," *The Journal of Economic Perspectives*, 26, 49–67.

Levine, O. and M. Warusawitharana (2021): "Finance and Productivity Growth: Firm-level Evidence," *Journal of Monetary Economics*, 117, 91–107.

Levine, R. (2005): "Finance and Growth: Theory and Evidence," *Handbook of Economic Growth*, 1, 865–934.

Manaresi, F. and N. Pierri (2018): "Credit Supply and Firm Productivity Growth," Working Paper, IMF.

Manso, G. (2011): "Motivating innovation," *The journal of finance*, 66, 1823–1860.

Marschak, J. and W. H. Andrews Jr. (1944): "Random Simultaneous Equations and the Theory of Production," *Econometrica*, 12, 143–205.

Midrigan, V. and D. Y. Xu (2014): "Finance and Misallocation: Evidence from Plant-Level Data," *American Economic Review*, 104, 422–458.

Moreira, S. (2020): "Firm Dynamics, Persistent Effects of Entry Conditions, and Business Cycles," Working Paper.

Oster, E. (2019): "Unobservable Selection and Coefficient Stability: Theory and Evidence," *Journal of Business & Economic Statistics*, 37, 187–204.

Paravisini, D., V. Rappoport, P. Schnabl, and D. Wolfenzon (2014): "Dissecting the Effect of Credit Supply on Trade: Evidence from Matched Credit-Export Data," *The Review of Economic Studies*, 82, 333–359.

Phillips, G. and G. Sertsios (2013): "How Do Firm Financial Conditions Affect Product Quality and Pricing?" *Management Science*, 59, 1764–1782.

Queralto, A. (2020): "A Model of Slow Recoveries from Financial Crises," *Journal of Monetary Economics*, 114, 1–25.

Reinhart, C. M. and K. S. Rogoff (2014): "Recovery from Financial Crises: Evidence from 100 Episodes," *American Economic Review*, 104, 50–55.

Restuccia, D. and R. Rogerson (2017): "The Causes and Costs of Misallocation," *Journal of Economic Perspectives*, 31, 151–174.

Shleifer, A. and R. W. Vishny (1992): "Liquidation Values and Debt Capacity: A Market Equilibrium Approach," *The Journal of Finance*, 47, 1343–1366.

Syverson, C. (2004): "Market Structure and Productivity: A Concrete Example," *Journal of Political Economy*, 112, 1181–1222.

——— (2011): "What Determines Productivity?" *Journal of Economic Literature*, 49, 326–365.

Financial Shocks, Productivity, and Prices

Simone Lenzu David A. Rivers Joris Tielens Shi Hu

Online Appendix

A Data Appendix

In this appendix, we provide additional details on the source and definition of the variables used in the empirical analysis.

Firm-level variables. We denote by $\text{Credit}_{j,t}$ the firm-level outstanding bank credit balance (sum of term loans, credit lines, credit backed by receivables) from the CCR, which is constructed by summing across all lenders b of firm j in year t ($b \in \mathcal{B}_{j,t}$) , $\text{Credit}_{j,t} = \sum_{b \in \mathcal{B}_{j,t}} \text{Credit}_{jb,t}$. As in Chodorow-Reich (2014), we measure the τ-year cumulative growth in total bank credit of each firm as $\Delta_\tau \text{Credit}_j = \frac{\text{Credit}_{j,2009+\tau} - \text{Credit}_{j,2009}}{0.5(\text{Credit}_{j,2009+\tau} + \text{Credit}_{j,2009})}$, where $\text{Credit}_{j,2009}$ measures the average outstanding bank credit of firm j in the year prior to the burst of the sovereign crisis, and $\text{Credit}_{j,2009+\tau}$ measures the average outstanding credit τ-years afterwards. We measure average financing costs incurred during a year using information on financial charges and outstanding principal of financial debt from the firms' income statements and balance sheets as reported in the AA: $fc_{j,t} = \frac{\text{Financial Charges}_{j,t}}{\text{End of Year Financial Debt}_{j,t-1}}$. We then compute the change in the average financing costs relative to 2009, $\Delta_\tau fc_j = fc_{j,2009+\tau} - fc_{j,2009}$.

From the AA, we gather the following set of firm-level variables from firms' balance sheets and income statements: firm size (natural logarithm of total assets), bank leverage (bank debt outstanding over total assets), and stock of inventories (sum of final goods and intermediate goods over total assets), all measured at the end of the fiscal year 2009. For each firm in our sample, we construct the Z-score at the end of fiscal year 2009 by adapting the Altman (1968) formula to private firms: Z-score = 3.107 × (EBIT / Total Assets) + 0.998 × (Sales / Total Assets) + 0.420 × (Capital / Total Liabilities) + 0.717 × (Working Capital / Total Assets) + 0.847 × (Retained Earnings / Total Assets).

Using the information reported in the AA, we compute three indicators of firm expenditures on productivity-enhancing activities, investments in machines and equipment, and growth rate of employment and average compensations. First, for each year following the burst of the sovereign crisis, we compute the R&D investment rate (Inv. Rate R&D$_\tau$), which is the ratio of cumulative expenses on R&D up to year 2009+τ scaled by the stock of intangible assets in 2009: Inv. Rate R&D$_\tau$ = $\sum_{t=1}^{\tau}$ R&D Expenditures$_{2009+t}$/Intangible Assets$_{2009}$. Our second indicator is a dummy variable that flags firms investing any positive amount in R&D in a given year (Any R&D Expense$_\tau$). This variable captures the extensive margin of innovation, accounting for the lumpy nature of R&D investments. Our third indicator recognizes that innovation spurs from R&D as long as a skilled and appropriately trained workforce is capable of integrating new technologies into the existing production processes. To capture this aspect, we gather information on employee training expenditures (Training Expenses$_\tau$). Specifically, we calculate cumulative average training expenditures per employee scaled by expenditures per employee in year 2009: Training Expenses$_\tau$ = $\left(\sum_{t=1}^{\tau} \text{Training Expenditures}_{2009+t}/\tau\right)$ /Training Expenditures$_{2009}$ − 1.

Similarly, we compute the cumulative growth rate in investments in machinery and equipment (M&E) as the ratio of cumulative expenses in machinery and equipments up to year 2009+τ scaled by the stock of these assets in 2009: Inv. Rate M&E$_\tau$ = $\sum_{t=1}^{\tau}$ M&E Expenditures$_{2009+t}$/Stock of M&E$_{2009}$. We then use information from the firm's social balance sheet—a subsection of the Annual Accounts—to compute the growth rate of total employees, part-time employees, and full-time employees ($\Delta_\tau X_j = \frac{X_{j,2009+\tau} - X_{j,2009}}{0.5(X_{j,2009+\tau} + X_{j,2009})}$. Finally, we compute the growth rate of Average Labor Compensations, defined as the ratio of total wage bill to total employment.

Section 2.2 and Appendix B.2 describe how we construct our measures of price and productivity growth.

Bank-level variables. We collect bank-level variables from confidential supervisory records of the National Bank of Belgium. The key variable of interest is banks' exposure to the sovereign crisis via their holdings of GIPSI sovereign securities—GIPSI Sovereigns$_b$ = GIPSI Sovereign Holdings$_b$/Risk-weighted Assets$_b$ in 2010:Q1—which is used to construct our firm-level credit supply shifter, as described in Section 3. We also gather information on a battery of bank-level characteristics which are included as controls in all econometric specifications. The set of bank-level variables includes bank size (natural logarithm of bank assets), variables capturing banks' funding structure (Tier 1 ratio, deposits over risk-weighted assets, net interbank liabilities scaled by risk-weighted assets), liquidity position (liquidity over risk-weighted assets), and quality of lending portfolio (non-performing loans over risk-weighted assets), measured before the shock (2010:Q1). Similar to our measure of GIPSI sovereign exposure, we aggregate these lender-specific variables to the firm-level by computing a weighted average across lenders using the share of firm j's credit received from each bank in the pre-shock period as weights.

Firm-bank-level variables. Exploiting the panel dimension of the CCR, we calculate length of the lending relationship (in quarters) between borrower j and bank b, $\text{Length of relationships}_{jb}$, measured as the number of consecutive quarters the relationship has been in place between 2006:Q1 and 2010:Q1. We subsequently aggregate across lenders and calculate the firm-level weighted average length of lending relationships as $\text{Length of relationships}_j = \sum_{b \in \mathcal{B}_j} \omega_{jb} \times \text{Length of relationships}_{jb}$, where ω_{jb} is the share of debt provided by each lender in 2010:Q1. We also compute the number of active lending relationships of each firm in the last quarter before the crisis ($\text{Number of relationships}_j$).

Sample construction. The construction of our dataset begins with the PRODCOM database, as it contains the crucial data on prices and quantities for our analysis. We start with a sample of 3,169 manufacturing firms operating in 16 manufacturing industries that report a "normal" legal and economic standing (i.e., no liquidation, no default, no ongoing mergers or acquisitions), strictly positive total assets and total PRODCOM revenues, and a well-defined location (region) in 2009 (the year prior to the burst of the sovereign crisis). We then exclude a small number firms for which we are unable to construct our Tornqvist price index (due to a lack of continuing products in consecutive years), resulting in a sample of 3,127 firms.

Next, we merge this sample with the annual accounts and VAT dataset, excluding observations with missing VAT revenues and missing information on production inputs (wage bill, intermediate input expenses, and stock of tangible capital needed to initialize the PIM method). We also exclude observations with missing information on inventories[31]. These variables are required to obtain the firm-level TFPQ estimate used in our empirical analysis and to demonstrate the role of the inventory channel in explaining

[31] Reporting this information is mandatory for firms filing a complete annual account form but is optional for firms filing simplified versions of the annual account.

Table A.1: Characteristics of high and low exposure firms

	Low exposure	High Exposure	Difference (1)-(2)
	(1)	(2)	(3)
Total Assets (Million Euros)	108.196	76.755	31.440
	(15.515)	(13.243)	(20.375)
Bank Leverage	0.217	0.198	0.019
	(0.009)	(0.009)	(0.013)
$\ln TFPR$	6.342	5.952	0.390
	(0.175)	(0.166)	(0.241)
$\ln TFPQ$	12.694	12.307	0.386
	(0.181)	(0.170)	(0.248)
$\ln P$	1.735	1.679	0.056
	(0.147)	(0.123)	(0.191)
Inventories	0.190	0.191	-0.002
	(0.006)	(0.006)	(0.008)
Z-score	2.019	2.096	-0.077
	(0.047)	(0.048)	(0.068)

Notes: This table compares firm characteristics, measured at the end of fiscal year 2009, across firms borrowing from banks with low holdings (below median) and high holdings (above median) of distressed sovereign bonds. Columns 1 and 2 report means and their standard errors (in parentheses). Column 3 reports the difference and standard errors (in parentheses) of a two-tailed test of equality of the means of the two groups. *** denotes that the mean difference is significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level.

firms' price dynamics. After imposing these filters, we are left with a sample of $1,265$ firms and $12,633$ firm-year observations in the time-frame $2006 - 2016$.

Finally, we merge this firm-level sample with the records of the combined CCR-Schema A dataset (after collapsing it at the firm level) to obtain information on firms' credit balances and to construct our firm-level credit supply shock. In doing so, we keep firm-year observations for which we observe a positive credit balance in 2009 in the CCR vis-à-vis at least one of the domestic banks filing the Schema A form. This process results in a final sample of $1,024$ firms and $9,667$ firm-year observations between 2009 and 2016. The total PRODCOM sales in 2009 for the firms in our final sample account for approximately 65 percent of total PRODCOM sales in 2009.

Comparison of firm characteristics and sample attrition. Table A.1 compares characteristics for the group of firms borrowing from banks with low GIPSI holdings (below the median of Shock_j) and the group of firms with high GISPI holdings (above the median of Shock_j), measured at the end of fiscal year 2009, before the burst of the sovereign debt crisis. Columns 1 and 2 report means and their standard errors (in parentheses). Column 3 reports the difference and standard errors (in parentheses) of a two-tailed test of equality of the means of the two groups. All firm variables are measured at the end of fiscal year 2009, the last quarter before the burst of the sovereign crisis.

We observe a significant degree of sample attrition between 2009 and 2016, with the number of firms dropping from 1024 to 652 between the beginning and the end of our sample. This appears to be driven largely by the sampling scheme adopted by PRODCOM and survey attrition in the response rate, rather than by selection induced by the financial shock. As we note in the paper, while the PRODCOM survey is designed to cover at least 90% of domestic production value within each NACE 4-digit manufacturing industry, firms might not be surveyed in all years. Since the construction of the price index requires the concatenation of the yearly price changes, a firm that is not surveyed in a given year permanently exits our regression sample, even if the firm later re-enters the sample. To confirm that attrition due to PRODCOM sampling scheme is the main driver of the attrition in our regression sample, we matched our sample to the Crossroads Bank for Enterprises (the Belgian census of enterprises) and found that 86% of the firms that exit at some point our regression sample still appear in the Crossroads as firms in "normal legal standing" in 2016 (the last year in our sample).

B Price measurement and productivity estimation

B.1 Price measurement

To build our baseline price measure (in levels), we follow a standard approach by concatenating yearly price changes starting from a base year. We first aggregate price changes across products of multi-product firms using a Törnqvist index, a standard measure used by statistical agencies, to measures the yearly growth rate of prices across 8-digit products within a firm:

$$P_{jt}/P_{jt-1} = \prod_{p \in \mathcal{P}_{jt}} (P_{jpt}/P_{jpt-1})^{\bar{s}_{pt}},$$

where $\mathcal{P}_{jt}$ represents the set of 8-digit products manufactured by firm j, P_{jpt} is the unit value of product p in $\mathcal{P}_{jt}$, and $\bar{s}_{jpt}$ is a Törnqvist weight computed as the average of the sale shares of product p in $\mathcal{P}_{jt}$ between t and $t-1$.[32] We then build our price firm-time price measure, P_{jt}, by recursively concatenating the year-to-year Törnqvist index starting from a firm-specific base year:

$$P_{jt} = P_{jB} \prod_{\tau=B+1}^{t} P_{jt}/P_{jt-1}.$$

The construction of firm-specific base year, P_{jB}, follows Eslava and Haltiwanger (2020):

$$P_{jB} = P_{base,B} \prod_{\mathcal{P}_{jB}} \left(\frac{P_{jpB}}{\bar{P}_{pB}}\right)^{s_{jpB}}, \quad \bar{P}_{pB} = \prod_{j} \left(P_{jpB}\right)^{\tilde{s}_{jpB}},$$

where B is the first year in which firm j is in the sample, $\mathcal{P}_{jB}$ is the set of products produced by firm j in year B, and $\bar{P}_{pB}$ is the geometric average of prices for product p in the base year, with weights $\tilde{s}_{jpB}$ denoting the revenue share of firm j in total revenues for product p in year B. $P_{base,B}$ is an overall base price such that:

$$P_{base,B} = \begin{cases} 1 & \text{if } B \text{ is the first year of the sample} \\ \prod \left(P_{jB-1}\right)^{\tilde{s}_{jB-1}} & \text{if } B > \text{first year of the sample} \end{cases}$$

We also construct two alternative price measures which deliver similarly robust results. The first is a simple revenue-share weighted-average of the product-level prices. The second avoids taking a stance on aggregation across different products, and uses just the change in the price of the firm's main product.

B.2 Productivity estimation

Our main production function estimation strategy follows the two-stage estimation routine in Gandhi, Navarro, and Rivers (2020) (GNR, henceforth), augmented to control for differences in output quality (De Loecker et al., 2016), and extended by allowing productivity to evolve according to a controlled Markov process in which firm investments in innovation (R&D and employee training) affect future productivity growth. We also modify the procedure to control for differences in market power in the product market following an approach similar to that used in Blum et al., 2024. We outline here the basic steps of the procedure, as well as our modifications, and refer the reader to GNR

[32] To ensure comparability of product-level prices across firms and within firms over time, we define products as unique combinations of 8-digit PRODCOM product codes and units of quantity measurement (e.g., liters, kilograms, etc.). We then compute unit values for each product (i.e., prices) by dividing total value by total quantity for each firm-product-time observation.

for additional details regarding the standard GNR approach.

B.2.1 Production Functions

We first discuss the quantity production function (in logs) that relates observed output measured in quantities to inputs:

$$q_{jt} = f(k_{jt}, l_{jt}, m_{jt}; \gamma) + \underbrace{\omega_{jt} + \epsilon_{jt}}_{\ln TFPQ_{jt}} \tag{A.1}$$

where k, l, m, are capital, labor, and intermediate inputs (materials, third-party services, and energy consumption) used by the firm to produce (log) quantities q. ω_{jt} is a persistent productivity shock that is observable by the firm when it makes production decisions, and unobserved by the econometrician. ϵ_{jt} represents non-persistent shocks that are not observable (or predictable) by firms before making their input decisions at t. Physical productivity, TFPQ, is defined as the sum of these two shocks and therefore can be formed as:

$$\ln TFPQ_{jt} = q_{jt} - f\left(k_{jt}, l_{jt}, m_{jt}; \gamma\right).$$

As discussed in the main text, from this measure we can construct a standard revenue productivity measure, computed as the residual of a revenue production function relating output, measured in revenues, to inputs:

$$r_{jt} = f(k_{jt}, l_{jt}, m_{jt}; \tilde{\gamma}) + \underbrace{\tilde{\omega}_{jt} + \tilde{\epsilon}_{jt}}_{\ln TFPR^R_{jt}}, \tag{A.2}$$

where we use $\tilde{\gamma}$, $\tilde{\omega}$, and $\tilde{\epsilon}$ to distinguish these objects from those of the quantity-based production function.

B.2.2 Estimation Routine

We assume that productivity evolves following a controlled first-order Markov process. Specifically, as in Ericson and Pakes (1995) and Doraszelski and Jaumandreu (2013), the distribution of productivity in period t is allowed to depend on past expenditures on innovation as well as past realizations of productivity:

$$P_\omega\left(\omega_{jt} \mid \mathcal{I}_{jt-1}\right) = P_\omega\left(\omega_{jt} \mid \omega_{jt-1}, \boldsymbol{Z}_{jt-1}, \boldsymbol{Z}_{jt-2}\right)$$

where $\mathcal{I}_{jt}$ denotes the firm's information set in period t and the vector $\boldsymbol{Z}_j$ includes firm j's investment rate in R&D, a dummy indicating any R&D expense, and training expenses per employee.[33] This implies that we can write $\omega_{jt} = h\left(\omega_{jt-1}, \boldsymbol{Z}_{jt-1}, \boldsymbol{Z}_{jt-2}\right) + \xi_{jt}$, where $h\left(\omega_{jt-1}, \boldsymbol{Z}_{jt-1}, \boldsymbol{Z}_{jt-2}\right) = \mathbb{E}\left[\omega_{jt} \mid \omega_{jt-1}, \boldsymbol{Z}_{jt-1}, \boldsymbol{Z}_{jt-2}\right]$ and ξ_{jt} is an unanticipated productivity "innovation" such that $\mathbb{E}\left[\xi_{jt} \mid \mathcal{I}_{jt-1}\right] = 0$. ϵ_{jt} is an unanticipated shock to output that is assumed to be independent of the firm's information set in period t. Capital and labor are assumed to pre-determined, i.e., k_{jt} and l_{jt} are assumed to be in the firm's information set in period t. Intermediate inputs as flexibly chosen in period t.

The estimation routine consists of two steps.

Step 1. The first step of the estimation strategy in GNR is based on a transformation of the firm's first-order condition for intermediate inputs, which relates observed input shares for intermediate inputs to the elasticity of output for intermediate inputs. The baseline specification in GNR assumes firms are perfectly competitive in the product market. In order to allow for imperfect competition, we modify the first stage of GNR following the approach in Blum et al., 2024. The first step of GNR shows that under perfect competition the output elasticity of intermediate inputs, $\frac{\partial}{\partial m_{jt}} f(k_{jt}, l_{jt}, m_{jt})$, can be recovered by regressing the shares of intermediate inputs on input levels:

$$s_{jt} = \ln\left(\frac{\partial}{\partial m_{jt}} f(k_{jt}, l_{jt}, m_{jt})\right) - \epsilon_{jt} \tag{A.3}$$

where $s_{jt} \equiv \frac{P_t^M M_{jt}}{R_{jt}}$ are the intermediate input shares, and P_t^M is the price of intermediates.[34]

Blum et al., 2024 show that under imperfect competition, this generalizes to

$$s_{jt} = \ln\left(\frac{\partial}{\partial m_{jt}} f(k_{jt}, l_{jt}, m_{jt})\right) - \ln \mu_{jt} - \epsilon_{jt}, \tag{A.4}$$

where μ_{jt} is the markup charged by firm i in period t. They further show that one can express the equilibrium markup as a function of the elasticity of demand at the optimum. Here we assume that the quantity demanded can be written as a function of price P_{jt},

[33] Since an important result in our paper is that firm investments in innovation drive productivity growth, we allow the process for productivity to depend on these investments for internal consistency. However, productivity estimates derived from assuming an exogenous Markov process, in which these investment do not enter, yield similar quantitative results.

[34] GNR also includes in equation (A.3) a constant term $\ln(\mathcal{E}) = \ln(E[e^{\epsilon_{jt}}])$. For simplicity and since Gandhi, Navarro, and Rivers (2020) notes that this term is close to zero in practice, we abstract away from this.

demand shifters z_{jt}, and a multiplicative demand shock χ_{jt}: $Q_{jt} = Q\left(P_{jt}, z_{jt}\right)\chi_{jt}$. As a result, the demand elasticity $\left(\frac{\partial Q_{jt}}{\partial P_{jt}}\frac{P_{jt}}{Q_{jt}}\right)$ will generically be a function of $\left(P_{jt}, z_{jt}\right)$. Thus equation (A.4) can be re-written as:

$$s_{jt} = \ln\left(\frac{\partial}{\partial m_{jt}} f(k_{jt}, l_{jt}, m_{jt})\right) - \ln\mu\left(P_{jt}, z_{jt}\right) - \epsilon_{jt}. \tag{A.5}$$

A regression of shares on $\left(k_{jt}, l_{jt}, m_{jt}, P_{jt}, z_{jt}\right)$ recovers ϵ_{jt} and a term combining the output elasticity of intermediate inputs and the markup.

Step 2. The second step of the estimation procedure recovers the production function and productivity. In contrast to the standard GNR procedure, since the output elasticity of intermediate inputs is no longer recovers in the first stage, it is recovered in the second stage, along with the rest of the production function.

Let x_{jt} denote the output of the firm (either in quantities or revenues); we have that $x_{jt} = f\left(k_{jt}, l_{jt}, m_{jt}\right) + \omega_{jt} + \epsilon_{jt}$. Define output net of the ex-post shock ϵ_{jt} as

$$\mathcal{X}_{jt} \equiv x_{jt} - \epsilon_{jt} = f\left(k_{jt}, l_{jt}, m_{jt}\right) + \omega_{jt} \tag{A.6}$$

where $\mathcal{X}_{jt}$ is an "observable term" that can be recovered from the estimates in Step 1.

Exploiting the Markovian property of ω_{jt}, equation (A.6) can be re-written as:

$$\mathcal{X}_{jt} = \underbrace{h\left(\mathcal{X}_{jt-1} - f\left(k_{jt-1}, l_{jt-1}, m_{jt-1}\right)\right)}_{h(\omega_{jt-1}, \boldsymbol{Z}_{jt-1}, \boldsymbol{Z}_{jt-2})} + f\left(k_{jt}, l_{jt}, m_{jt}\right) + \xi_{jt}.$$

We flexibly model $f\left(k_{jt}, l_{jt}, m_{jt}\right)$ and $h\left(\omega_{jt-1}, \boldsymbol{Z}_{jt-1}, \boldsymbol{Z}_{jt-2}\right)$ as:

$$f(k_{jt}, l_{jt}, m_{jt}) = \sum_{0<\tau_k+\tau_l+\tau_m\leq\tau} \gamma_{\tau_k,\tau_l,\tau_m} k_{jt}^{\tau_k} l_{jt}^{\tau_l} m_{jt}^{\tau_m} \tag{A.7}$$

$$h(\omega_{jt-1}) = \sum_{0<a\leq A} \psi_a \tilde{\omega}_{jt-1}^a + \sum_{0<b_1\leq B_1} \varphi_{b_1} \boldsymbol{Z}_{jt-1}^{b_1} + \sum_{0<b_2\leq B_2} \varphi_{b_2} \boldsymbol{Z}_{jt-2}^{b_2}. \tag{A.8}$$

Combining equations (A.7) and (A.8), we construct the following recursive estimation equation:

$$\begin{aligned}\mathcal{X}_{jt}(\psi,\gamma) \quad = \quad & f(k_{jt}, l_{jt}, m_{jt};\gamma) + \sum_{0<a\leq A} \psi_a\left(\mathcal{X}_{jt-1}(\psi,\gamma) - f(k_{jt-1}, l_{jt-1}, m_{jt-1};\gamma)\right)^a \\ & + \sum_{0<b_1\leq B_1} \varphi_{b_1} \boldsymbol{Z}_{jt-1}^{b_1} + \sum_{0<b_2\leq B_2} \varphi_{b_2} \boldsymbol{Z}_{jt-2}^{b_2} + \xi_{jt}\end{aligned} \tag{A.9}$$

and identify the vector of coefficients (ψ, φ, γ). Because $\left(k_{jt}, l_{jt}, \mathcal{X}_{jt-1}, \boldsymbol{Z}_{jt-1}, \boldsymbol{Z}_{jt-2}\right) \in$

$\mathcal{I}_{jt-1}$, and are thus orthogonal to ξ_{jt}, they can be used as instruments for themselves. However, since m_{jt} is chosen in period t it is correlated with ξ_{jt}. Therefore we use m_{jt-1} as an instrument and use the following moment conditions:

$$
\begin{aligned}
\mathbb{E}[\xi_{jt} \cdot k_{jt}^{\tau_k} l_{jt}^{\tau_l} m_{jt-1}^{\tau_m}] &= 0. \\
\mathbb{E}[\xi_{jt} \cdot \mathcal{X}_{jt-1}^{a}] &= 0. \\
\mathbb{E}[\xi_{jt} \cdot \boldsymbol{Z}_{jt-1}^{b_1}] &= 0. \\
\mathbb{E}[\xi_{jt} \cdot \boldsymbol{Z}_{jt-2}^{b_2}] &= 0.
\end{aligned}
$$

Controlling for Input Price Bias in Quantity Production Functions. The final component of our estimation procedure concerns the quantity-based specification. In a typical production function estimation, data on physical quantities of output and inputs are often not available and instead are measured as values (revenues for output and expenditures for inputs) that are deflated by common aggregate (often industry-level) deflators. Previous work has shown that this can lead to biased estimates of the production function and productivity (Katayama, Lu, and Tybout, 2009). Under some conditions, these biases cancel out (see De Loecker and Goldberg, 2014). However, when output is measured in quantities, the biases no longer cancel out. To deal with this, we follow the approach in De Loecker et al. (2016), which suggests using a control function of (output) prices and market shares to correct for the bias.

In practice, for the quantity-based production function estimation, we augment the production function with a control function in prices and market shares. That is, we replace $C(k_{jt}, l_{jt})$ with $C(k_{jt}, l_{jt}) + cf\left(P_{jt}, ms_{jt}\right)$ in equation (A.9):

$$
\begin{aligned}
\mathcal{X}_{jt}(\psi,\gamma) = \;& -C(k_{jt}, l_{jt};\gamma) - cf(P_{jt}, ms_{jt};\phi) \\
& + \textstyle\sum_{0<a\leq A} \psi_a \left(\mathcal{X}_{jt-1}(\psi,\gamma) + C(k_{jt-1}, l_{jt-1};\gamma) + cf(P_{jt-1}, ms_{jt-1};\phi)\right)^a \\
& + \textstyle\sum_{0<b_1\leq B_1} \varphi_{b_1} \boldsymbol{Z}_{jt-1}^{b_1} + \sum_{0<b_2\leq B_2} \varphi_{b_2} \boldsymbol{Z}_{jt-2}^{b_2} + \xi_{jt}
\end{aligned}
\tag{A.10}
$$

where we also approximate $cf(\cdot)$ with a sieve in price and market shares. Accordingly, we add moments interacting ξ_{jt} and the terms of the sieve approximation to estimate the parameters (ϕ) of the sieve for $cf(\cdot)$. The remaining steps of the estimation procedure are unchanged.

Table A.2: Production function estimates

	Output Elasticities					
	Quantity-Based			Revenue-Based		
Industry Code (NACE Rev. 1.1)	γ^l	γ^m	γ^k	$\tilde{\gamma}^l$	$\tilde{\gamma}^m$	$\tilde{\gamma}^k$
15	0.184	0.778	0.057	0.233	0.736	0.043
17	0.256	0.705	0.043	0.310	0.685	0.022
18	0.277	0.685	0.045	0.355	0.642	0.040
20	0.211	0.741	0.049	0.257	0.698	0.042
21	0.209	0.732	0.075	0.259	0.692	0.052
22	0.216	0.777	0.018	0.269	0.709	0.015
24	0.192	0.753	0.071	0.256	0.713	0.043
25	0.192	0.762	0.051	0.263	0.694	0.052
26	0.205	0.729	0.075	0.269	0.681	0.053
27	0.241	0.750	0.071	0.236	0.724	0.041
28	0.268	0.667	0.070	0.328	0.627	0.042
29	0.270	0.688	0.048	0.330	0.640	0.033
31	0.292	0.686	0.047	0.328	0.641	0.045
32	0.273	0.640	0.134	0.399	0.625	0.073
33	0.248	0.686	0.035	0.328	0.630	0.031
36	0.235	0.722	0.056	0.307	0.665	0.033

Notes: This table reports the within industry average production function elasticities estimated using the approach of Gandhi, Navarro, and Rivers (2020), as described above. The first three columns report the estimates obtained from a quantity production function estimation. The last three columns report the estimates obtained from a revenue production function estimation.

B.3 Estimation Results

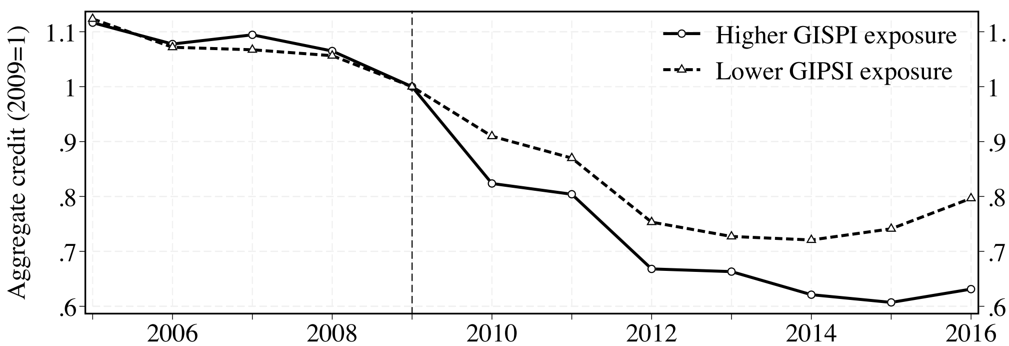
We perform the production function estimation separately for each 2-digit industry for both the quantity-based and revenue-based specifications (equations (A.1) and (A.2)). In Table A.2, we report the average elasticity estimates for each industry under our baseline specification with a non-parametric specification of the production technology $f(\cdot)$. For both the quantity and revenue versions, the elasticity estimates are sensible, highlighting roughly constant returns to scale, on average, across industries.

C The pass-through of the sovereign shock to credit supply

C.1 The burst of the European sovereign debt crisis.

After the parliamentary elections held in Greece in October 2009, the newly elected government acknowledged significant budget misreporting in previous years and a larger-than-expected fiscal deficit, which forced the Greek government to request, on April 23, 2010, an EU/IMF bailout package to cover its financial needs for the remainder of the year. In response to these events, international rating agencies downgraded Greece's sovereign debt rating to "junk bond" and the yields on Greek government bonds rose sharply, effectively barring the country's access to capital markets (Lane, 2012).

Figure A.1: Aggregate credit



Notes: This figure displays the time-series evolution of the aggregate credit supply provided by banks with above versus below median exposure to the sovereign crisis in the last quarter before the Greek bailout request (2010:Q1). Exposure to the sovereign crisis is based on residual holdings of GIPSI debt, as described in the main text. Both series are normalized by their 2009 level.

Shortly after the events in Greece, investors became concerned with the solvency and liquidity of the public debt issued by other peripheral European countries, starting with Ireland and Portugal, and soon after Spain and Italy (Angelini, Grande, and Panetta, 2014). The yield spread with Germany, which had been low and relatively stable for most Euro-zone countries since the introduction of the euro, significantly increased following news from Greece and the subsequent bailout at the end of the first quarter of 2010.

Investigating the channels of transmission of the financial shock to bank lending activity, Bottero, Lenzu, and Mezzanotti (2020) documents that the sovereign shock affected banks' lending because it unexpectedly increased the riskiness of bank assets, forcing financial intermediaries with low capital buffers to adjust the riskiness of their assets, and also impaired the ability to pledge these securities as collateral in interbank transactions, which is a crucial funding source for many banks.

The balance sheet shock had important credit supply implications. Figure A.1 plots the aggregate credit supplied to the firms in our dataset by financial intermediaries with above versus below median exposure to the sovereign crisis. It shows that, right after the burst of the crisis, the amount of credit provided by the two groups of banks started diverging relative to the pre-shock period.

Table A.3: Exposure to the sovereign shock and credit market outcomes

	Δ_τCredit (1)			$\Delta_\tau fc$ (2)		Δ_τTerm Loans (3)		Δ_τCredit Lines (4)	
	Pre		Post	Pre	Post	Pre	Post	Pre	Post
$\hat{\beta}_{-3}$	0.016	$\hat{\beta}_1$	-0.150***	-0.006	0.006	-0.018	-0.115**	-0.208*	-0.396***
	(0.047)		(0.036)	(0.007)	(0.006)	(0.052)	(0.053)	0.115	(0.111)
$\hat{\beta}_{-2}$	0.057	$\hat{\beta}_2$	-0.094**	0.002	0.016	-0.003	-0.181*	-0.452***	-0.439***
	(0.042)		(0.039)	(0.008)	(0.010)	(0.039)	(0.092)	0.101	(0.103)
$\hat{\beta}_{-1}$	0.045	$\hat{\beta}_3$	-0.18***	0.004	0.034**	-0.009	-0.172**	-0.31***	-0.426***
	(0.035)		(0.058)	(0.009)	(0.015)	(0.036)	(0.075)	0.098	(0.099)
		$\hat{\beta}_4$	-0.147*		0.024**		-0.169*		-0.384***
			(0.069)		(0.010)		(0.082)		(0.12)
		$\hat{\beta}_5$	-0.033		0.026**		-0.061		-0.451***
			(0.060)		(0.011)		(0.101)		-0.229
		$\hat{\beta}_6$	0.049		-0.004		-0.017		-0.229
			(0.052)		(0.010)		(0.097)		(0.147)
		$\hat{\beta}_7$	0.023		-0.003		-0.058		-0.189
			(0.086)		(0.006)		(0.114)		(0.163)

Notes: This table accompanies Figure 1. It reports the estimates of the effect of the credit supply shock on the cumulative growth rate of firm-level credit (column 1) and the change in the average financing costs (column 2) using model (3). Columns (3) and (4) study the individual effect of the credit shock on two categories of bank credit, term loans and revolving credit lines, respectively. All regressions include bank-level controls, firm-level controls, industry fixed effects, and region fixed effects. Clustered standard errors are reported in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level.

C.2 The effect of banks' sovereign holdings on firm-level credit supply.

Table A.3 reports the estimated cumulative effect of the bank balance sheet shock on the firm-level growth rate of bank credit (column 1) and the firm-level change in financing costs (column 2). Figure 1 in the paper graphs the coefficients and associated confidence intervals. In addition to the pre-trend check discussed in the main body of the paper, we perform a series of robustness analyses to test the validity of our identification strategy, which we discuss below.

Credit types. Columns (3) and (4) look at the components of bank credit, studying the individual effect of the shock on the growth rate of term loans and revolving credit lines. Both types of credit are used by firms to finance their production activity as well as their innovation expenses. We find that both types of credit are affected by the credit shock, although the effect of a one-standard deviation exposure to the shock is both larger (approximately 3 times as large) and more persistent for credit lines. To put these numbers in perspective, it is important to consider that, on average, a much larger amount of bank debt is held in term form of term loans (approximately 2.5 as larger, or 70 percent of firm's credit balance). Thus, the contribution contraction of the two types of credit on the total credit tightening is quantitatively similar, on average. Note also that the negative coefficients on credit lines in the pre-period indicate that, if anything, the firms most affected by the shock in the post-period where growing faster (higher credit demand/supply) before the credit crunch.

Within-firm estimator. We present additional analysis that supports the identification assumption that the drop in credit observed in the data is explained by a sudden tightening of credit *supply* rather than driven by demand-side factors. In particular, we address the potential concern that the coefficients are picking up a shift in firms' credit demand or a change in borrower's credit worthiness that takes place at the same time as the credit shock. To do so, we leverage the micro-data containing information on individual firm-bank relationships. Because the vast majority of the firms engage in multiple lending relationships at the same time, we can augment model (3) with firm fixed effects (i_j) and test whether banks with larger GIPSI holdings reduced their credit supply to the *same firm* relative to banks with lower holdings. By exploiting variation across lenders to the same firm, this within-firm estimator allows us to control for changes in unobservable firm-specific factors, such as a simultaneous contraction of credit demand or a worsening of firms' credit worthiness. Specifically, we estimate the following model at different horizons indexed by τ:

$$\Delta_\tau \text{Credit}_{jb} = \beta_\tau \cdot \text{GIPSI Sovereigns}_{jb} + \Gamma^{'}_{K,\tau}\mathbf{K}_{jb} + \Gamma^{'}_{X,\tau}\mathbf{X}_{jb} + i_{j,\tau} + u_{jb\tau}, \tag{A.11}$$

where now the left-hand side is the cumulative growth rate of credit to firm j that is provided by bank b specifically, $\Delta_\tau \text{Credit}_{jb}$, as opposed to the total credit summed across all banks.[35] In this case, the right-hand side variable of interest is the interaction between bank b's holdings of sovereign securities issued by GIPSI countries scaled by bank b's risk-weighted assets before the Greek bailout (GIPSI Sovereigns$_{jb}$). As we did in our main firm-level specification in Section 3 (model 3), we condition on a set of bank-level controls ($\mathbf{K}_{jb}$), which are now measured at the individual bank level, as well as two relationship-level controls ($\mathbf{X}_{jb}$)—the length of the lending relationship between firm j and lender b and the share of credit provided by lender b in firm j total credit—all measured before the the burst of the crisis. Finally, note that the industry and region fixed effects in model (3) are subsumed here by the firm fixed effects. As in our firm-level specification, we de-mean and scale the variable of interest (GIPSI Sovereigns$_{jb}$) by its standard deviation so that the coefficients β_τ in (A.11) capture the effect of a one standard deviation difference in the exposure to the credit shock on the $\tau-$year cumulative growth rate of credit to firm j from lender b.

Table A.4, column 1, presents the estimation results. The estimates show that among banks lending to the *same* firm, those that were more exposed to the shock (i.e., had larger holdings of GISPI sovereigns) decreased their lending to that firm relative to less exposed banks, providing strong evidence that the credit contraction was supply-driven. In column 2, we repeat the relationship-level regression, but omitting the firm fixed effects. Importantly, while the estimated coefficients are largely unaffected by whether we include firm-fixed effects, the R^2 of the regressions increase significantly (by about an order of magnitude) when fixed effects are included. In the spirit of Oster (2019), this observation demonstrates that while unobserved firm-specific factors (e.g., changes in credit demand) are important for explaining the overall variation in bank lending to firms, that variation is not correlated with exposure to the sovereign shock.

Table A.4 also highlights that the contraction in credit supply driven by the balance sheet shock is evident both along the intensive and extensive margins. For the extensive margin, we define the variable Cut$_{jb\tau}$ as an indicator variable for whether a lending

[35]In the construction of credit growth rates at the relationship level, we account for banks M&A by adopting the standard correction that identifies bank acquisitions over pairs of consecutive years and treats the acquired and acquiring bank as a single entity over that span (Bernanke, Lown, and Friedman, 1991).

Table A.4: Response of growth rate of credit to negative credit supply shocks: Within-firm estimation

	Total Growth ($\Delta_\tau \text{Credit}_{jb}$)		Extensive Margin ($\text{Cut}_{jb\tau}$)		Intensive Margin ($\Delta_\tau \ln\text{Credit}_{jb}$)	
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_1$	-0.259***	-0.284***	0.090***	0.087***	-0.100***	-0.115***
	(0.035)	(0.047)	(0.025)	(0.014)	(0.022)	(0.036)
	0.489	*0.083*	*0.456*	*0.056*	*0.449*	*0.050*
$\hat{\beta}_2$	-0.206***	-0.213***	0.054*	0.049	-0.180***	-0.168***
	(0.047)	(0.074)	(0.029)	(0.029)	(0.050)	(0.028)
	0.487	*0.049*	*0.491*	*0.055*	*0.473*	*0.034*
$\hat{\beta}_3$	-0.177*	-0.182**	-0.004	0.015	-0.411***	-0.319***
	(0.085)	(0.078)	(0.031)	(0.032)	(0.115)	(0.059)
	0.525	*0.024*	*0.487*	*0.044*	*0.555*	*0.037*
$\hat{\beta}_4$	-0.147*	-0.215***	-0.005	0.005	-0.507***	-0.566***
	(0.083)	(0.069)	(0.032)	(0.044)	(0.081)	(0.070)
	0.528	*0.030*	*0.544*	*0.049*	*0.554*	*0.045*
$\hat{\beta}_5$	-0.198*	-0.249***	0.017	0.046	-0.563***	-0.562***
	(0.105)	(0.069)	(0.046)	(0.045)	(0.149)	(0.095)
	0.529	*0.035*	*0.534*	*0.064*	*0.606*	*0.048*
$\hat{\beta}_6$	-0.264*	-0.310***	0.034	0.042	-0.657***	-0.749***
	(0.125)	(0.090)	(0.051)	(0.050)	(0.152)	(0.086)
	0.519	*0.040*	*0.530*	*0.077*	*0.576*	*0.050*
$\hat{\beta}_7$	-0.337***	-0.396***	0.036	0.075*	-0.794***	-0.943***
	(0.110)	(0.074)	(0.042)	(0.036)	(0.126)	(0.064)
	0.523	*0.051*	*0.524*	*0.080*	*0.564*	*0.071*
Firm FE	Y	N	Y	N	Y	N

Notes: This table reports estimates of the effect of the credit supply shock on credit growth at the firm-bank relationship-level using model (A.11). We report estimates for overall credit growth as well as the extensive and intensive margin separately, both with and without firm fixed effects. All regressions include bank-level controls and relationship-level controls. Standard errors are clustered at the lender-level and are reported in parentheses. R^2 are reported in italics. *** denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level.

relationship that existed between firm j and bank b before the sovereign crisis is still in place τ-years after the shock, with a 1 indicating the relationship is no longer in place. We also calculate the percentage change in credit balances between firm j and bank b for relationships that are in place both before the shock and τ-years after the shock ($\Delta_{\tau}\text{lnCredit}_{jb}$). Columns 3 and 4 show that banks more exposed to the shock are more likely to break existing lending relationships. Columns 5 and 6 show that banks also reduce their credit supply in surviving relationships. As was the case for the overall credit results, including firm fixed effects increases the R^2 but has little effect on the coefficients.

Finally, we note that the contraction in credit at the firm-bank level persists throughout out sample period, whereas the contraction in credit at the firm level (Figure 1 and Table A.3) was transitory. Together, these results suggest that over time firms were gradually able to compensate for the contraction in credit supply by their most exposed pre-shock lenders by establishing new lending relationships with other financial intermediaries.

Response of investments and employment. We analyze the real effects of financial shocks on firms' input demands. Prior studies have documented a contraction in investments and employment following a credit tightening.[36] Table A.5 shows that this is also the case in our setting. Column (1) shows the effect of the financial shock on the (cumulative) investment rate of machines and equipment and the growth rate of employment. Consistent with the presence of capital adjustment costs, we find a persistent contraction in investment, which compounds over time.

Column (2) shows the effect of the financial shock on the cumulative growth rate of employment. A one-standard deviation exposure to the shock leads to a contraction of 3.1 percent contraction in employment in the immediate aftermath of the shock, relative to a less exposed firm. Columns (3) and (4) further break down the employment response into two categories of workers: full-time and part-time employees. While both categories are impacted by the shock, we find that part-time workers employment experience a much larger (about 2.5 times larger, on impact) and persistent contraction relative to full-time workers. We interpret these results as consistent with the idea that full-time workers are 'more essential'—having longer tenure and more firm-specific human capital—and thus

[36]See, e.g., Chodorow-Reich (2014) for employment, Cingano et al. (2016) for investments, and Bottero et al. (2020) for both employment and investments.

Table A.5: Response of investment and employment

	Inv. Rate M&E (1)	Employees Total (2)	Employees Full-time (3)	Employees Part-time (4)	Average Labor Compensation (5)
$\hat{\beta}_1$	-0.026***	-0.031***	-0.028***	-0.067**	0.002
	(0.007)	(0.007)	(0.005)	(0.028)	(0.007)
$\hat{\beta}_2$	-0.041***	-0.017	-0.011	-0.124***	0.010
	(0.014)	(0.013)	(0.010)	(0.039)	(0.009)
$\hat{\beta}_3$	-0.058***	-0.013	-0.003	-0.057	0.011
	(0.015)	(0.012)	(0.011)	(0.042)	(0.006)
$\hat{\beta}_4$	-0.072***	-0.016	-0.007	-0.096*	0.019**
	(0.016)	(0.019)	(0.020)	(0.046)	(0.007)
$\hat{\beta}_5$	-0.048**	-0.011	-0.003	-0.146***	0.016**
	(0.022)	(0.012)	(0.012)	(0.054)	(0.007)
$\hat{\beta}_6$	-0.016	0.007	0.025	-0.085**	0.021**
	(0.032)	(0.015)	(0.016)	(0.039)	(0.008)
$\hat{\beta}_7$	-0.010	0.017	0.032*	-0.056	0.006
	(0.037)	(0.016)	(0.016)	(0.045)	(0.009)

Notes: This table reports the estimates of the effect of the credit supply shock on investments in machinery and equipment (M&E) and different measures of employment and labor compensation using the model in (7). In column 1, the dependent variable is the cumulative investment rate on machinery and equipment between the end of fiscal year 2009 and the end of fiscal year 2009 + τ ($\tau = \{1, ..., 7\}$), scaled by the book value these assets in 2009. In columns 2–4, the dependent variable is the growth rate of total employment, full-time workers' employment, and part-time workers' employment, respectively. In column 5, the department variable is the growth rate of the average compensations (measures as total wage bill over the total employment). All regressions include bank-level controls, firm-level controls, industry fixed effects, and region fixed effects. Clustered standard errors are reported in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level.

are less affected by the credit shock than part-time workers. Finally, in column (5), we find no evidence that firms reduced wages in response to the shock. If anything, we observe that the impact of the shock has a small positive effect on average labor compensations in the medium-to-long run. We speculate that this is driven by a change in the composition of the workforce, e.g., as firms lay off part-time workers, who tend to be paid lower wages and lower benefits.

D Effect of the Shock on Productivity and Prices

Table A.6 reports the estimated cumulative effect of credit supply shock on firm-level productivity and prices that are presented in the main body in Figure 2. Table A.7 presents a series of robustness checks to demonstrate the robustness of our estimates, which we discuss below.

D.1 Robustness analysis

Alternative productivity measures. In the baseline regressions reported in the paper, we estimate the measures of firm-level productivity as residuals from revenue or quantity production functions, modeling firms' production technologies using a Translog functional form (see Section 2.2 in the paper and Appendix B.2). We test the robustness of our results regarding the effect of financial shocks on productivity to alternative measures of productivity. In column (1) of Table A.7, we repeat the quantity production function estimation assuming a less flexible but more traditional Cobb-Douglas functional form. In columns (2), instead of estimating the production function parameters, we calibrate input elasticities to the average revenue shares within each industry (index function approach).

In the presence of borrowing constraints (e.g., if the firm faces some working-capital constraints) this first order condition for intermediate inputs might not hold with equality, possible leading to biased estimates in the the first-stage of our production function estimation procedure (equation (A.4)). As a first pass to address this concern, we performed the production function TFP prior 2008 (thus excluding both the great financial crisis and the sovereign debt crisis from the estimation sample). To further tackle this concern, we develop an alternative production function estimation procedure that allows for the possibility that working capital constraints might distort the first order condition of intermediate inputs. Specifically, in the spirit of the exercise in Manaresi and Pierri (2018), we augmented the first stage of the estimation to include a firm-specific credit supply shifter to capture wedges generate by financial frictions. The firm-specific credit supply shifter is a Bartik-style shifter computed following the method in Amiti and Weinstein (2011), which are designed to measure the impact of changes in bank credit supply on firms that is independent of the borrowers' characteristics and overall credit demand. We use these alternative estimates of γ_{jt} to construct an alternative productivity index and re-estimate our baseline regression. The results are presented in Column (3). Reassuringly, the estimated coefficients are essentially unchanged in terms of both point estimate and statistical precision.

Table A.6: Response of productivity and prices to negative credit supply shocks

	$\Delta_\tau \ln TFPR$	$\Delta_\tau \ln TFPQ$	$\Delta_\tau \ln P$
	(1)	(2)	(3)
$\hat{\beta}_1$	-0.014***	-0.003	-0.019**
	(0.004)	(0.008)	(0.008)
$\hat{\beta}_2$	-0.020***	-0.018	-0.005
	(0.007)	(0.014)	(0.010)
$\hat{\beta}_3$	-0.014*	-0.046***	0.035***
	(0.007)	(0.016)	(0.013)
$\hat{\beta}_4$	-0.030***	-0.076***	0.035**
	(0.007)	(0.021)	(0.014)
$\hat{\beta}_5$	-0.020**	-0.069***	0.040*
	(0.008)	(0.023)	(0.019)
$\hat{\beta}_6$	-0.021*	-0.061***	0.026
	(0.010)	(0.019)	(0.021)
$\hat{\beta}_7$	-0.031***	-0.061***	0.010
	(0.011)	(0.015)	(0.016)

Notes: This table reports the estimates of the effect of the credit supply shock on the cumulative growth rate of TFPR, TFPQ, and prices estimated using model (3). Clustered standard errors are reported in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level.

Capacity utilization. We study whether firms adjust their capacity utilization in response to the financial shock. To do so, we collect data on capacity utilization from a supplementary data source, the Business Survey administered by the National Bank of Belgium. The survey covers a subset of firms in our sample, asking them to report the percentage of their production capacity utilized.

After matching the Business Survey to our final regression sample, we are able to gather information on capacity utilization for 348 firms. In this sub-sample, firms operate (on average) with a seventy-two percent production capacity utilization before the crisis (year 2009). This figure increases to seventy-seven percent by the end of our sample (year 2016). We explore in this subsample how adjustments in capacity utilization might affect the interpretation of our baseline productivity results.

Table A.7: Response of productivity and prices to negative credit supply shocks: Robustness

	$\Delta_\tau \ln TFPQ$ Cobb-Douglas	$\Delta_\tau \ln TFPQ$ Index Func.	$\Delta_\tau \ln TFPQ$ Fin Frictions	Capacity Utilization	$\Delta_\tau \ln P$ Main	$\Delta_\tau \ln P$ Revenue	$\Delta_\tau \ln TFPQ$ Single Product	$\Delta_\tau \ln P$	$\Delta_\tau \ln TFPQ$ Balanced Sample	$\Delta_\tau \ln P$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\hat{\beta}_1$	0.002	0.003	-0.003	-0.01	-0.030***	-0.024**	0.001	-0.017	-0.005	-0.013
	(0.009)	(0.010)	(0.008)	(0.018)	(0.009)	(0.011)	(0.015)	(0.036)	(0.010)	(0.010)
$\hat{\beta}_2$	-0.014	-0.012	-0.018	-0.015	-0.018	0.008	-0.041	0.012	-0.008	-0.015
	(0.014)	(0.014)	(0.014)	(0.021)	(0.021)	(0.016)	(0.029)	(0.012)	(0.014)	(0.010)
$\hat{\beta}_3$	-0.049***	-0.050***	-0.046***	0.04	0.043*	0.050***	-0.051	0.027	-0.043***	0.016*
	(0.016)	(0.017)	(0.016)	(0.031)	(0.022)	(0.016)	(0.034)	(0.021)	(0.013)	(0.008)
$\hat{\beta}_4$	-0.074***	-0.075***	-0.076***	0.053	0.048*	0.047***	-0.057*	0.041	-0.050***	0.017
	(0.019)	(0.021)	(0.021)	(0.032)	(0.024)	(0.015)	(0.031)	(0.024)	(0.013)	(0.010)
$\hat{\beta}_5$	-0.059***	-0.057***	-0.066***	0.04	0.052***	0.039*	-0.052*	0.062	-0.058***	0.027
	(0.022	(0.022)	(0.023)	(0.030)	(0.022)	(0.021)	(0.031)	(0.039)	(0.015)	(0.016)
$\hat{\beta}_6$	-0.047***	-0.044***	-0.055***	0.065*	0.044	0.040	-0.066	0.044	-0.049***	0.019
	(0.018)	(0.017)	(0.018)	(0.034)	(0.032)	(0.026)	(0.037)	(0.044)	(0.015)	(0.018)
$\hat{\beta}_7$	-0.050***	-0.051***	-0.055***	0.053	0.054	0.054	-0.051*	0.021	-0.059***	0.009
	(0.015)	(0.014)	(0.016)	(0.03)	(0.045)	(0.047)	(0.026)	(0.025)	(0.015)	(0.016)

Notes: This table reports the estimates of the effect of the credit supply shock on alternative measures of the TFPQ and prices. All regressions are estimated using model (3). Clustered standard errors are reported in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level.

We estimate our local linear projection looking at the effect of the shock on the (cumulative) percentage change in capacity utilization. The results are reported in columns (4) of Table A.7. We find no significant effect on capacity utilization in the short-run: the coefficient estimates are economically small and, at best, marginally significant. In the medium-to-long run we find larger point estimates, although mostly insignificant. These results suggest that if anything the financial shock leads to a small increase in capacity utilization.

Alternative price measures. As explained in the paper, when constructing a firm-level price index, one needs to take a stance on how to aggregate the prices across the heterogeneous products produced by a firm. We did so building a conventional Törnqvist index, which computes a sales-weighted geometric average of the price changes across continuing products in firm's portfolios. Here we show that the estimated contraction and subsequent rebound of output prices following a negative credit supply shock is also evident when one uses alternative measures of firm-level prices. First, we construct a price index that averages across 8-digit product price levels, weighting them by their revenue-share. We then compute the price change in the firm-level price as the delta-log: $\Delta_\tau \ln P_j^{Rev} = \ln(\sum_{p\in\mathcal{P}_{j\tau}} s_{jp\tau}P_{jp\tau}) - \ln(\sum_{p\in\mathcal{P}_{j2009}} s_{jp2009}P_{jp2009})$. As before, P_{jpt} is the unit value of product p in $\mathcal{P}_{j\tau}$, and $s_{jp\tau}$ is a sale shares of product p in $\mathcal{P}_{j\tau}$. Second, we look at the change in the price of the main product of the firm (defined as the product with the highest revenue share), without taking a stance on aggregation across different products. The estimation results, reported in columns (5) and (6) of Table A.7, are largely in line with the estimates obtained using the Törnqvist price index.

Inventory adjustment and balanced sample. As discussed above, in order to perform the production function estimation we constructed a firm-level measure of output produced, X_{jt}, adjusting firm-level revenues by the change in inventories. To do so, we deflated the total change inventories (in euros) by our price index. To the extent that firms differentially reduce prices of different products depending on product-specific the inventory stock, this might generate biased results. To address this concern, we re-estimated our baseline regressions in the subsample of single-product firms (column (7) and (8) of Table A.7), finding estimates that are quantitatively similar, although less precisely estimated due to the smaller sample size.[37]

Another concern is related to possible survival bias. About one-third of the firms in our regression sample in 2009–2010 are not in the regression sample by the end of our sample period. In Appendix A we discuss how this appears to be driven almost entirely by the sampling scheme adopted by PRODCOM and survey attrition in the response rate, rather than be the results of selection induced by the financial shock. As an additional robustness test again survival bias, column (9) and (10) of Table A.7 show that the productivity and price estimates are essentially unchanged if we re-estimated our baseline regressions in the subsample of permanent firms.

[37] Approximately 40 percent (408 out of 1024) of the firms in our sample are classified as single-product firms, which is consistent with other studies showing the prevalence of multi-product firms in manufacturing.