The Review of Financial Studies

Trade Credit and the Transmission of Unconventional Monetary Policy

Manuel Adelino
Duke University, CEPR, and NBER, USA

Miguel A. Ferreira
Nova School of Business and Economics, CEPR, and ECGI, Portugal

Mariassunta Giannetti
Stockholm School of Economics, CEPR, and ECGI, Sweden

Pedro Pires
Nova School of Business and Economics, Portugal

We show that production networks are important for the transmission of unconventional monetary policy. Firms with bonds eligible for purchase under the European Central Bank's Corporate Sector Purchase Program act as financial intermediaries by extending additional trade credit to their customers. The increase in trade credit is pronounced from core countries to periphery countries and for financially constrained customers. Customers then increase investment and employment in response to the increased trade financing, whereas suppliers expand their customer base, contributing to upstream industry concentration. Our findings suggest that trade credit redistributes the effects of monetary policy across regions and firms. (*JEL* E50, G30)

Received May 6, 2021; editorial decision April 24, 2022 by Editor Itay Goldstein. Authors have furnished an Internet Appendix, which is available on the Oxford University Press Web site next to the link to the final published paper online.

We thank Itay Goldstein (the editor), two anonymous referees, Fabio Feriozzi, Luc Laeven, Ali Ozdagli, Mitchell Petersen, Julien Sauvagnat, Rodney Ramcharan, and Philipp Schnabl; conference participants at the ECB-RFS Macro-Finance Conference, the European Finance Association Annual Meeting, the Financial Intermediation Research Society, the NBER Summer Institute Corporate Finance Meeting, the Stanford Institute for Theoretical Economics (SITE) Financial Regulation Conference, the Western Finance Association, the 17th Annual Conference on Corporate Finance at Washington University in St. Louis, and the Norges Bank Workshop on Frontier Research in Banking; and seminar participants at the Bank of Italy, BI Norwegian Business School, Carnegie Mellon University, Deutsche Bundesbank, Emory University/Federal Reserve Bank of Atlanta, Georgia Tech, Imperial College London, London Business School, Michigan State University, Oxford University, Singapore Management University, University of Florida, University of Illinois at Chicago, University of Illinois at Urbana–Champaign, University of Mannheim, and University of Maryland for helpful comments. Ferreira holds the BPI | Fundação "la Caixa" Chair in Responsible Finance, is an independent board member at BPI Asset Management, and acknowledges financial support from the Fundação para a Ciência e Tecnologia (FCT). Giannetti acknowledges financial support from the Bank of Sweden Tercentenary Foundation and the Jan Wallander and Tom Hedelius Foundation. Supplementary data can be found on *The Review of Financial Studies* web site. Send correspondence to Miguel Ferreira, miguel.ferreira@novasbe.pt.

The Review of Financial Studies 00 (2022) 1–39

https://doi.org/10.1093/rfs/hhac040 Advance Access publication June 25, 2022

Conventional monetary policy affects access to external finance through its effects on bank lending and consequently affects primarily small and bank-dependent firms (Gertler and Hubbard 1988; Gertler and Gilchrist 1994; Holmstrom and Tirole 1997). Large firms with access to public debt markets are less sensitive to the monetary policy stance.

Unconventional monetary policy involves directly purchasing assets in public debt markets in order to lower interest rates and stimulate the economy. Central banks purchase short-term and long-term Treasury bonds and mortgage-backed securities, which can benefit firms indirectly (Foley-Fisher, Ramcharan, and Yu 2016), but more recently also target nonfinancial firms directly through corporate bond purchases. One such program is the European Central Bank's (ECB) Corporate Sector Purchase Program (CSPP), which involves the purchase of investment-grade corporate bonds. These large-scale asset purchases affect bond yields and issuance volumes (Abidi and Miquel-Flores 2018; Galema and Lugo 2019; Zaghini 2019; Todorov 2020) and benefit primarily large firms with access to bond markets. Small firms can benefit indirectly from these purchases as large firms' demand for bank loans decreases and banks expand credit supply to small firms (Grosse-Rueschkamp, Steffen, and Streitz 2019; Arce, Mayordomo, and Gimeno 2020).

This paper proposes and tests a new channel through which the benefits of unconventional monetary policy can be redistributed to firms without access to bond markets: the *trade credit channel*, which operates independently from any effects of monetary policy on bank lending. Using the ECB's CSPP as a laboratory, we hypothesize that firms with bonds eligible under the CSPP act as financial intermediaries by providing trade credit to their customers as eligible firms experience a decrease in the cost of capital. The ability to issue bonds at a relatively lower yield leads these firms to ration fewer marginal customers from trade credit. We examine whether this channel mitigates or exacerbates asymmetries in the transmission of monetary policy by studying which customers are supported by suppliers with CSPP-eligible bonds and the resultant real effects. We also explore how the CSPP affects eligible firms' power to acquire and retain customers and thus their competitive position in product markets.

The CSPP was announced in March 2016 and implemented starting in June 2016 and it was designed with a strict eligibility criteria in place, as only investment-grade bonds issued by nonfinancial firms from the euro area can be purchased by the ECB. The design of the CSPP allows us to implement a difference-in-differences analysis to address concerns that concurrent shocks to the implementation of the CSPP can affect eligible firms and their customers.

Cash flows from the CSPP are disproportionately injected into the core of the euro area, including France, Germany, and the Benelux Economic Union (Bayoumi and Eichengreen 1993), where most firms with eligible bonds are located. In fact, firms in core countries are able to issue more bonds thanks to more developed capital markets and stronger legal institutions

(Becker and Josephson 2016). However, if targeted (large and financially unconstrained) firms pass on the additional liquidity to their customers through trade credit, unconventional monetary policy tools can also reach small and financially constrained firms through production networks. In addition, while firms located in core countries may experience a larger direct effect from the CSPP through bond issuance and lower cost of capital, whether the increase in liquidity spills over to other regions depends on the geography of production networks.

We investigate these questions using new data containing information on firm-level customer-supplier networks. We compare the amount of trade credit extended by eligible firms and noneligible firms, before and after the CSPP announcement. We find that eligible firms increase the amount of trade credit they provide to customers (i.e., accounts receivable) more than noneligible firms following the CSPP. As we would expect, at the same time, the accounts payable of customers of eligible firms increase after the CSPP.

We find no statistically significant differences in trade credit between eligible firms, or their customers, and control firms before the start of the CSPP. In addition, we find a discrete increase in eligible firms' accounts receivable around the investment-grade rating cutoff, the most relevant criterion for eligibility under the CSPP. This suggests that changes in trade credit are not triggered by demand shocks or other contemporaneous shocks such as, for example, worsening credit conditions that could have simultaneously increased firms' reliance on trade credit and led the ECB to start the CSPP. We provide evidence that the effects of the CSPP are instead driven by the decrease in the cost of external funding triggered by the ECB's asset purchases. Indeed, we find that eligible firms that provide more trade credit issue bonds after the start of the CSPP.

One important feature of our setting is that eligible firms are, by the nature of the program, larger than noneligible firms, on average. To account for the possibility of preexisting differences between treated and control firms, we repeat our tests using matched samples. We also sort firms into size deciles and include size decile dummies interacted with year dummies in the regressions. We find similar estimates in these more stringent specifications, which indicates that asymmetric shocks affecting firms of different sizes are unlikely to drive our findings.

The effects of the trade credit channel of monetary policy are heterogeneous across regions and firms. Firms targeted by the CSPP extend trade credit to customers that are more financially constrained. In particular, we find that smaller firms, non-investment-grade firms, unrated firms, firms with higher leverage and firms with lower tangible assets to pledge as collateral receive more trade credit.

The CSPP also produces redistributive effects across regions. While the ECB aimed to purchase investment-grade bonds of firms in any euro area country, firms in core countries were able to issue more bonds after the

announcement of the program than firms in periphery countries. Arguably as a consequence of the asymmetric change in financial conditions, we only detect an increase in accounts receivable for suppliers located in the core and no significant effects in the periphery. We find the opposite when we consider the *customers* of eligible firms. Customers located in core countries show small and insignificant increases in accounts payable, while customers in periphery countries show a significant increase in accounts payable. We provide direct evidence that links between suppliers in the core and customers in the periphery drive the effects. These results suggest that trade credit helped to relax financial constraints in periphery countries where banks were more affected by the 2010–2011 European sovereign debt crisis. Monetary policy transmission through production networks mitigates the asymmetric effects that arise from the regional distribution of eligible firms and their ability to issue investment-grade bonds, which benefits core countries to a larger extent.

Finally, we show that the trade credit channel produces real effects. As a result of the increase in accounts payable, the customers of eligible suppliers increase long-term investment in fixed assets and human capital. They also increase investment in working capital by extending more trade finance to their own customers and increasing inventories. At the same time, eligible firms improve their position in product markets. Eligible suppliers acquire new customers and are more likely to maintain existing business relationships, thus increasing their market share as a result of the CSPP.

Our findings highlight mechanisms of tantamount importance in light of the expanded direct assets purchases announced by the Federal Reserve Board and the ECB in March 2020. Notably, both the unlimited quantitative easing program of the Federal Reserve Board and the Pandemic Emergency Purchase Program (PEPP) of the ECB involve direct interventions in corporate bond markets. We highlight a new channel through which quantitative easing affects the real economy and complements the stimulus arising from the effects of asset purchases on banks' balance sheets and lending. Trade credit can transmit the stimulus of unconventional monetary policy interventions to firms that are not directly targeted by the policy. However, monetary policy interventions may promote concentration in upstream industries with long-run consequences on industrial structure.

Our paper contributes to the literature on the effects of large-scale asset purchases on bank lending and real economic activity (Rodnyansky and Darmouni 2017; Acharya et al. 2019; Grosse-Rueschkamp, Steffen, and Streitz 2019; Chakraborty, Goldstein, and MacKinlay 2020; Di Maggio, Kermani, and Palmer 2020).

We also contribute to the literature on trade credit. Previous research provides mixed evidence on whether trade credit attenuates or amplifies the transmission of conventional monetary policy to the real economy. While Gertler and Gilchrist (1993) find no substitution between bank loans and trade credit following a tightening of monetary policy, Nilsen (2002) provides evidence

that both small and large firms increase trade credit following monetary contractions. Other studies explore whether trade credit can provide an alternative source of liquidity that mitigates the effects of bank liquidity shocks. Love, Preve, and Sarria-Allende (2007) find that trade credit collapsed in the aftermath of the 1997 Asian crisis, while Garcia-Appendini and Montoriol-Garriga (2013) find that cash-rich suppliers extended more trade credit during the 2007–2009 global financial crisis. Restrepo, Cardona Sosa, and Strahan (2019) show that firms rely less on short-term loans and more on cash and trade credit for liquidity management following an exogenous increase in the relative cost of short-term bank credit in Colombia in 2011.

Last, we contribute to the growing literature on the importance of production networks in the transmission of economic shocks. A number of papers explore how negative shocks are transmitted through the supply chain and show that upstream negative liquidity shocks are amplified and transmitted to customers (Boissay and Gropp 2013; Jacobson and von Schedvin 2015; Barrot and Sauvagnat 2016). A few recent papers consider the role of bank liquidity shocks in supply chains (Costello 2020; Huremovic et al. 2020; Alfaro, García-Santana, and Moral-Benito 2021) and how banking structure is related to the propagation of shocks along the supply chain (Giannetti and Saidi 2019). Using the stock market reaction to monetary policy shocks, Ozdagli and Weber (2019) show that input-output linkages are an important transmission mechanism of macroeconomic shocks through higher-order demand effects.

To the best of our knowledge, we are the first to study the trade credit channel of unconventional monetary policy, which is independent of the bank lending channel. By exploiting exogenous variation in suppliers' ability to access external debt financing, we show that trade credit enhances the transmission of quantitative easing interventions to the real economy. We also contribute to a nascent strand of the literature that explores regional heterogeneity in the transmission of monetary policy (Beraja et al. 2018) by studying how the effects of large-scale asset purchases are redistributed across regions through production networks.

1. Data and Methodology

This section describes the data, variables, and the empirical methodology.

1.1 Sample

Our initial sample consists of a panel of publicly listed and privately held firms in the period 2013–2017 drawn from the Bureau Van Dijk's Orbis database, which contains financial statements for companies worldwide.[1] We restrict the sample to firms based in the 19 member states that are part of the Economic

[1] Since the overwhelming majority of companies in Orbis report unconsolidated accounts, we only include data from unconsolidated financial statements in our sample to avoid double counting the financial reports.

Table 1
Summary statistics

	Mean	Median	SD	Minimum	Maximum	Number of observations
Eligible	0.0003	0.0000	0.0177	0.0000	1.0000	2,248,514
Has eligible supplier	0.0007	0.0000	0.0257	0.0000	1.0000	2,248,514
Eligible suppliers share	0.0002	0.0000	0.0125	0.0000	1.0000	2,248,514
Accounts receivable	0.3012	0.1849	0.5574	0.0000	4.6879	2,248,514
Accounts payable	0.2284	0.1134	0.5555	0.0000	4.9558	2,248,514
Assets (€million)	30.1234	3.2753	703.8566	0.0000	198,929	2,248,514
Sales (€million)	22.5520	3.1251	367.4128	0.0000	107,970	2,248,514
Cash	0.1130	0.0480	0.1536	0.0000	0.8158	2,248,514
PPE	0.2358	0.1303	0.2600	0.0000	0.9767	2,248,514
Net margin	−0.0528	0.0157	0.7375	−7.0694	1.7098	2,248,514
Liabilities	0.6402	0.6690	0.2958	0.0035	1.8202	2,248,514

This table shows mean, median, standard deviation, minimum, maximum, and the number of observations for each variable. Table A.1 in the appendix defines the variables. The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. Variables are winsorized at the top and bottom 1%.

and Monetary Union of the European Union (euro area). We exclude from our sample firms that are classified as small companies by Orbis.[2] We also exclude financial firms (SIC codes 6000–6999) and public administration entities (SIC codes 9000–9999). Finally, we require nonmissing data on the main firm outcomes: the ratio of accounts receivable to sales (*Accounts receivable*), and the ratio of accounts payable to sales (*Accounts payable*).[3] The control variables include total assets (*Assets*), the ratio of cash to total assets (*Cash*), the ratio of property, plant, and equipment to total assets (*PPE*), the ratio of net income to sales (*Net margin*), and the ratio of total liabilities to total assets (*Liabilities*).

We also consider additional firm outcomes for investment in short-term assets, investment in assets and capital expenditures, employment, and financing decisions. The investment variables include the change in accounts receivable over lagged total assets (Δ*Accounts Receivable*), the change in inventories over lagged total assets (Δ*Inventories*), the change in total assets over lagged total assets (*Asset growth*), the change in tangible assets plus depreciation over lagged total assets (*CAPEX*), and the change in the number of employees over lagged number of employees (*Labor growth*). The financing variables include the change in accounts payable (Δ*Accounts payable*), the change in financial debt (Δ*Total debt*), the change in long-term debt (Δ*Long-term debt*), the change in short-term debt (Δ*Short-term debt*), and the change in cash and equivalents (Δ*Cash*); all variables are scaled by lagged total assets.

Table 1 reports summary statistics for the variables used in our analysis. The final sample consists of 510,298 unique firms for a total of 2,248,514 firm-year observations. Panel A of Table IA.1 in the Internet Appendix lists the number

[2] Companies in Orbis are considered to be small when they have less than €1 million in operating revenue, less than €2 million in total assets, and fewer than 15 employees.

[3] We scale accounts payable by sales rather than cost of goods sold because the latter is not available in Orbis.

of observations and firms by country. Table A.1 in the appendix defines the variables.

1.2 CSPP and eligible firms

The ECB started a series of direct asset purchases programs to ease monetary conditions in the euro area, lower long-term interest rates and ultimately increase long-term inflation expectations and achieve the inflation target in 2012 (Rostagno et al. 2021). Initially, the outright monetary transmission program and the asset purchase program were limited to asset-backed securities and sovereign bonds. To further ease credit conditions in the euro area, on March 10, 2016, the ECB announced the CSPP, which implied an expansion of its asset purchase program to include investment-grade corporate bonds, as a tool to strengthen its accommodative monetary policy stance and to improve firms' financing conditions.[4]

We start from the list of marketable bonds accepted as collateral for Eurosystem credit operations that was published by the ECB the day before the CSPP announcement on March 9, 2016. From this list, consistent with the CSPP eligibility criteria, we retain euro-denominated securities (denominations EUR, DEM, FRF) classified as bonds (type AT01) or medium term notes (type AT02) issued by corporations (issuer group IG3) and financial corporations other than credit institutions (issuer group IG9) resident in a country member of the euro area.[5]

To assign each bond to a unique firm, we first consider the issuer name of each bond in the list of bonds accepted as collateral by the ECB. We also consider the name of corporations and financial corporations other than credit institutions (guarantor groups GG3 and GG9) that guarantee eligible bonds provided that the guarantors are resident in a country member of the euro area. In fact, several bonds are issued by financial subsidiaries and are guaranteed by the parent organization (e.g., bonds issued by "Volkswagen Intl Finance N.V." and by "Iberdrola Finanzas S.A.U." are guaranteed by "Volkswagen AG" and by "Iberdrola S.A.," respectively). Next, we apply a fuzzy-string matching technique to identify in Orbis the eligible firms (i.e., firms with bonds eligible for purchase under the CSPP before the announcement). As a result, our sample includes 151 unique nonfinancial eligible firms for which we have data on the variables of interest. Panel A of Table IA.1 in the Internet Appendix reports the number of eligible firms by country. Panel B lists the frequency of firms with different ratings, distinguishing between eligible and noneligible firms.

Figure 1 shows that the percentage that each country represents of the gross domestic product (GDP) in the euro area as of 2015 (panel A) and of the total

[4] The bond issue has to have an investment-grade rating from at least one of the four rating agencies: S&P, Moody's, Fitch Ratings, or DBRS.

[5] Data are available at https://www.ecb.europa.eu/paym/coll/assets/html/list-MID.en.html. The issuer should not be a credit institution or have a parent undertaking. Other criteria include a remaining maturity of at least 6 months and fewer than 31 years, and a yield to maturity equal to or above the ECB's deposit facility rate.

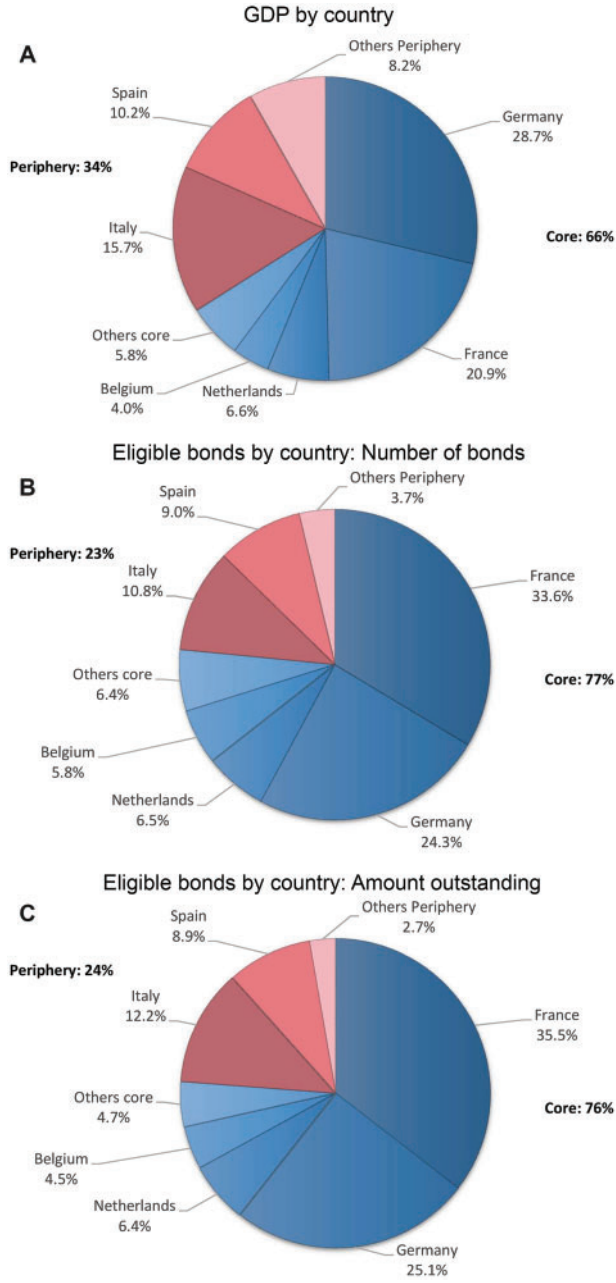

Figure 1
Eligible corporate bonds under the CSPP by country
Panel A reports the percentage that each country represented of the GDP in the euro area as of 2015. Panels B and C show the percentage that each country (i.e., country of risk) represented of the ECB's CSPP in terms of the number of eligible bonds and total amount outstanding in the euro area as of March 2016. The sample consists of bond issues of nonfinancial firms based in the euro area.

number of eligible bonds (panel B) and amount outstanding (panel C) in the euro area just before the start of CSPP (as of March 2016). We find that about 77% of the universe of eligible bonds under the CSPP are issued by firms in core countries (Austria, Belgium, Finland, France, Germany, Luxembourg, and Netherlands), while countries in the core represent only about 66% of the GDP of the euro area.[6]

Previous literature has shown that the CSPP fostered the ability of eligible firms to tap public debt markets stimulating new issuance of bonds and reducing the bond yields of eligible firms (Abidi and Miquel-Flores 2018; Galema and Lugo 2019; Grosse-Rueschkamp, Steffen, and Streitz 2019; Zaghini 2019; Arce, Mayordomo, and Gimeno 2020; Todorov 2020).

The geographic distribution of eligible firms implies that the consequences of the CSPP in terms of firms' ability to issue investment-grade bonds and benefit from lower yields are asymmetric across regions. Panel A of Figure 2 shows the issuance of investment-grade euro-denominated bonds before and after the CSPP. In core countries, the new issuance of investment-grade euro-denominated bonds increased by €51 billion in 2016 (from €76 billion to €127 billion). New issuance of investment-grade euro-denominated bonds in periphery countries increased by €11 billion (from €18 billion to €29 billion). The figure also shows that the increase in issuance for investment-grade bonds continues through 2017.

While differences in bond issuance also could be explained by differences in firm-specific credit demand, panel B of Figure 2 shows that the new issuance of non-investment-grade euro-denominated bonds (that were not targeted by the CSPP) was almost unchanged from 2015 to 2016 in both core and periphery countries. Given that before the CSPP investment-grade bonds and non-investment-grade bonds show similar behavior, differences in the demand for credit are unlikely to explain the differences in bond issuance.

1.3 Customers of eligible firms

We link each eligible firm (i.e., supplier) to all disclosed customers reported in the FactSet Revere Supply Chain Relationship database. FactSet Revere collects customer and supplier relationship information from primary public sources, such as SEC 10-K annual filings, investor presentations, and press releases. If we consider customer and supplier relationships, FactSet Revere include over 25,000 companies worldwide, which are the source of the supply chain relationship data, and over 105,000 target companies, which are disclosed by source companies.

[6] The composition of assets in the overall asset purchase program is at the discretion of the ECB. The ECB's corporate bond purchases reflect the universe of outstanding corporate bonds, while sovereign bond purchases follow strict rules and have to be proportional to the capital key (i.e., euro area national central banks' individual shares in the ECB's capital). Figure IA.1 in the Internet Appendix shows that core countries represent 73% of the ECB's corporate bond holdings under the CSPP.

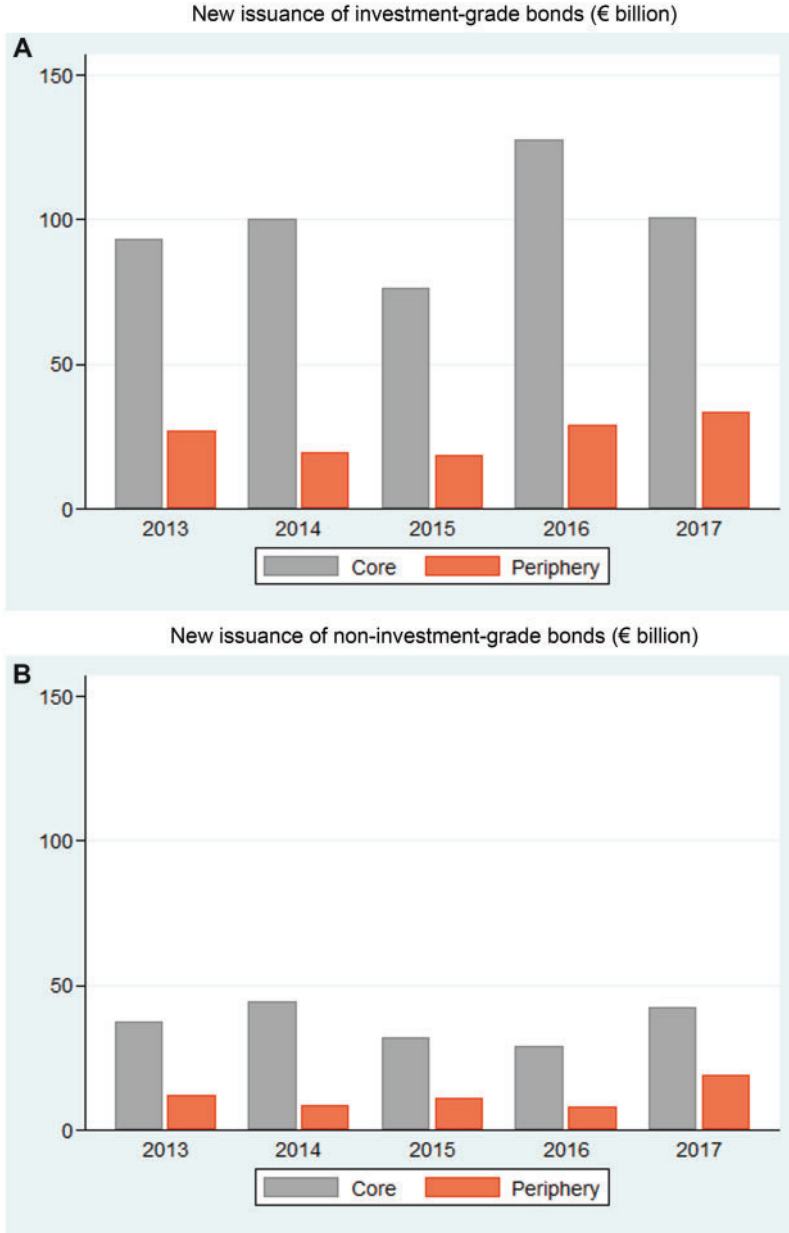

Figure 2
New issuance of corporate bonds around the CSPP: Core versus periphery countries
This figure shows the amount of bonds issued (€billion) by core and periphery euro area firms in the euro-denominated corporate bond market. Bond issuance data are obtained from SDC New Issues and includes bond issues of nonfinancial firms based in the euro area in the 2013–2017 period. Panel A shows new issuance of investment-grade bonds. Panel B shows new issuance of non-investment-grade bonds.

Using FactSet Revere, we can track the effects on suppliers and their customers. We identify customers of eligible firms using direct and reverse relationships. A direct relationship is disclosed by the company that lists the target company as a material customer, and a reverse relationship is disclosed by another company listing the source company as a material supplier. As a result, our data include a comprehensive network of supply chain interconnections.

To better understand our data, consider "Deutsche Telekom AG" as an example. Using direct relationships, the company discloses 24 active material customers as of 2015. This list includes public entities, such as "Government of Germany," "Government of Switzerland," and "European Commission," and corporations, such as "Deutsche Post AG," "Daimler AG," "ABB Ltd.," and "Netflix, Inc." Using reverse relationships, "Deutsche Telekom AG" is disclosed as a material supplier by an additional 11 unique companies. Customers that reported the firm as a supplier include "Freenet AG," "Drillisch AG," and "KION Group AG," among other corporations. In total, considering both direct and reverse relationships, "Deutsche Telekom AG" has a total of 35 unique customers identified using FactSet Revere.

We begin by filtering the FactSet Revere data only selecting suppliers that are CSPP-eligible firms. On average, eligible firms report 7.6 customers domiciled in the euro area. We then match the list of customers of eligible firms in FactSet Revere to Orbis using ISIN identifiers, when available, and a fuzzy-string matching algorithm using names for the remaining firms. The final sample includes 318 unique nonfinancial customers of eligible firms, domiciled in the euro area, after matching with Orbis to obtain firm fundamentals.

Panel A of Table IA.1 in the Internet Appendix reports the number of customers of eligible firms by country. On average, customers that we identify through direct and reverse relationships (i.e., from the reporting of customers or suppliers) have similar size. While the customers of eligible firms in our sample are, on average, smaller than their suppliers, our sample includes relatively large customers of eligible firms, which are less likely to be financially constrained than the typical firm in the sample.[7] Table IA.2 in the Internet Appendix reports on the interconnections between suppliers and their customers by country pair.[8]

1.4 Empirical methodology

Our hypothesis is that eligible firms experience a decrease in cost of capital due to the CSPP and find it optimal to use the liquidity that they obtain through bond issuance to extend trade credit to marginal customers to which they were previously rationing trade credit. We thus view trade credit usage as driven by supply, consistent with growing evidence that customers can use the available

[7] On average, eligible firms have €20.4 billion in assets and their customers have €10.6 billion, whereas the average firm in the sample is much smaller with €30.1 million in assets.

[8] We consider all active relationships as of 2014 or 2015 (the year before the introduction of the CSPP).

slack in payment terms at little or no cost (Giannetti, Burkart, and Ellingsen 2011; Klapper, Laeven, and Rajan 2012; Murfin and Njoroge 2015; Barrot 2016; Breza and Liberman 2017; Giannetti, Serrano-Velarde, and Tarantino 2021).

To identify whether differences in trade credit provision arise from the access to capital markets rather than demand shocks, our empirical methodology exploits CSPP-eligible firms and the timing of the policy. We perform difference-in-differences estimations by comparing changes in the outcome variables between treatment and control groups around the CSPP announcement (the treatment). Specifically, we estimate the regression:

$$Y_{i,t} = \beta_0 + \beta_1 Treated_i \times Post_t + \beta_2 \times X_{i,t-1} + \eta_i + \eta_{j,t} + \eta_{c,t} + \varepsilon_{i,t} \quad (1)$$

where the main outcome variables ($Y_{i,t}$) are the ratio of accounts receivable to sales (*Accounts receivable*) and the ratio of accounts payable to sales (*Accounts Payable*). The treatment group variable (*Treated*) is alternatively: (1) *Eligible*, a dummy variable that takes the value of one if a firm has bonds eligible for purchase under the CSPP before the CSPP announcement, and zero otherwise; (2) *Has eligible supplier*, a dummy variable that takes the value of one if a firm is a customer of a firm with eligible bonds, and zero otherwise; and (3) *Eligible suppliers share*, the firm's share of eligible suppliers relative to the firm's total number of suppliers.

Post is a dummy variable that takes the value of one in 2016, the year the CSPP is announced and implemented and thereafter, and zero otherwise. $X_{i,t-1}$ is a set of firm-specific control variables that includes *log*(*Assets*), *Cash*, *PPE*, *Net margin*, and *Liabilities*. All control variables are lagged by 1 year. The regressions include firm fixed effects η_i, industry-year fixed effects $\eta_{j,t}$ (using the Fama-French 10-industry classification), and country-year fixed effects $\eta_{c,t}$. Standard errors are clustered at the firm level to correct for heteroscedasticity and within-firm residual correlation.

A positive and significant estimate of the coefficient β_1 indicates that eligible firms extend more trade credit (*Accounts receivable*) or that customers of eligible firms receive more trade credit (*Accounts payable*) due to the CSPP, as long as demand and other shocks evolve similarly for treated and control firms. To validate this assumption, as is customary in the application of difference-in-differences methods, we evaluate whether there are any preexisting differential trends in the use of trade credit for eligible firms and their customers before the CSPP. This helps to address concerns that contemporaneous shocks might lead to a positive β_1 coefficient. This could include differential demand shocks to treated and control firms, or worsening credit conditions that might lead firms with better access to external finance to extend more trade credit during the implementation of the CSPP by the ECB. In both of these alternative hypotheses, we would expect to observe a gradual increase in the provision of trade credit before the start of the policy. The absence of preexisting trends

suggests that the CSPP, and not differences in demand or in the ability to provide trade credit, lead eligible suppliers to increase trade credit provision.

To further validate our identifying assumption, we test whether the provision of trade credit increases around the investment-grade cutoff (i.e., the eligibility criteria) for eligible firms relative to noneligible firms after the CSPP announcement. As with the pre-trends tests, this helps to address the concern of whether firms of different size or with different access to external finance may have increased the provision of trade credit for reasons unrelated to the CSPP.

2. Effects of Unconventional Monetary Policy

In this section, we first examine whether eligible firms' enhanced ability to tap debt markets, following the purchases of bonds by the ECB, is transmitted through trade credit in production networks. We then check the robustness of our results using different empirical approaches.

2.1 Accounts receivable

The CSPP decreased the cost of capital of eligible firms by decreasing bond yields and fostering bond issuance. This, in turn, allowed CSPP-eligible firms to extend more trade credit to marginal customers that were previously rationed.

To test our main hypothesis, we examine whether CSPP-eligible firms experienced an increase in the ratio of accounts receivable to sales (*Accounts receivable*) following the CSPP. Table 2 shows the results. The estimates in column 1 show that eligible firms (treatment group) experience a significant increase in the ratio of accounts receivable to sales of about 10 percentage points relative to noneligible firms (control group) after the announcement of the CSPP. The effect is economically significant as a 10-percentage-point increase in accounts receivable indicates that firms in the treatment group increase days receivable, relative to those in the control group, by about 36 days ($36 = 0.1 \times 360$), which is about one third of the sample average. The results remain robust when we add firm-specific controls (column 3) and country-year fixed effects (column 5) to the specification in column 1, which already includes firm fixed effects and industry-year fixed effects.

Columns 2, 4, and 6 show the evolution of the differences in accounts receivable between the treatment and control groups in the years before and after the CSPP (based on estimates in which *Eligible* is interacted with dummy variables for each event year). Figure IA.2 in the Internet Appendix shows the evolution of the differential effect in accounts receivable as estimated in column 2. We do not find statistically significant differences between treatment and control groups in the pretreatment period. While the increase in accounts receivable becomes statistically significant only after the start of the CSPP, we find a positive but insignificant increase in eligible firms' accounts receivable already in 2015. A possible explanation is that the Public Sector Purchase

Table 2
Effect of CSPP on accounts receivable of eligible firms

	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post*	0.103***		0.102***		0.089***	
	(0.032)		(0.032)		(0.032)	
Eligible × *2014*		−0.007		−0.007		−0.013
		(0.022)		(0.022)		(0.022)
Eligible × *2015*		0.046		0.046		0.042
		(0.043)		(0.043)		(0.043)
Eligible × *2016*		0.077**		0.077**		0.062*
		(0.038)		(0.038)		(0.038)
Eligible × *2017*		0.156**		0.156**		0.139**
		(0.062)		(0.062)		(0.062)
log(Assets)			−0.013***	−0.013***	−0.014***	−0.014***
			(0.002)	(0.002)	(0.002)	(0.002)
Cash			−0.050***	−0.050***	−0.045***	−0.045***
			(0.005)	(0.005)	(0.005)	(0.005)
PPE			−0.086***	−0.086***	−0.087***	−0.087***
			(0.007)	(0.007)	(0.007)	(0.007)
Net margin			−0.006***	−0.006***	−0.006***	−0.006***
			(0.002)	(0.002)	(0.002)	(0.002)
Liabilities			−0.010**	−0.010**	−0.007	−0.007
			(0.005)	(0.005)	(0.005)	(0.005)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	No	No	No	No	Yes	Yes
Number of observations	2,248,514	2,248,514	2,248,514	2,248,514	2,248,514	2,248,514
R-squared	.74	.74	.74	.74	.74	.74

This table presents difference-in-differences estimates of firm-level panel regressions of the ratio of accounts receivable to sales (*Accounts receivable*). *Eligible* is a dummy variable that takes the value of one if a firm had bonds eligible for purchase under the CSPP before the CSPP announcement date (March 2016), and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

Program (PSPP) implemented by the ECB in 2015 involved purchasing large amounts of government bonds and may have spilled over to investment-grade corporate bonds, which are often viewed as close substitutes (Krishnamurthy and Vissing-Jorgensen 2013). The trade credit provision by eligible firms accelerates after the implementation of the CSPP in 2016 and further increases in 2017, presumably after firms had time to issue bonds and then renegotiate trade credit terms with their customers.[9] Consistent with these facts, we find that the interaction term *Eligible* × *2015* coefficient is not statistically different from the *Eligible* × *2016* coefficient (with a *p*-value between .5 and .6), but it is strongly statistically different from the *Eligible* × *2017* coefficient (with a *p*-value between .04 and .08). Similarly, the sum of the interaction term coefficients for years 2014 and 2015 are statistically different from the sum of the interaction term coefficients for years 2016 and 2017 (with a *p*-value below .01 in all columns). Acceleration after the start of the CSPP makes it unlikely

[9] This is also consistent with evidence that the yields of eligible bonds decreased not only upon the announcement of the CSPP but also in subsequent months as the ECB actually implemented the purchases. Thus, we would expect to find stronger effects in 2017 (see Kanda, Pinto, and Silva 2021).

Table 3
Effect of CSPP on accounts receivable of eligible firms: Additional tests

A. Estimates excluding top-rated eligible firms (AAA to A)

	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post*	0.117***		0.115***		0.104***	
	(0.039)		(0.039)		(0.039)	
Eligible × *2014*		−0.007		−0.007		−0.012
		(0.026)		(0.026)		(0.026)
Eligible × *2015*		0.011		0.011		0.007
		(0.033)		(0.033)		(0.033)
Eligible × *2016*		0.091**		0.089*		0.076*
		(0.046)		(0.046)		(0.046)
Eligible × *2017*		0.147**		0.145**		0.130*
		(0.067)		(0.067)		(0.067)
Controls	No	No	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	No	No	No	No	Yes	Yes
Number of observations	2,248,385	2,248,385	2,248,385	2,248,385	2,248,385	2,248,385
R-squared	.74	.74	.74	.74	.74	.74

B. Difference-in-differences with a bandwidth around the investment-grade cutoff

	BBB+ to BB-			A+ to B−		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post*	0.273***	0.282***	0.230**	0.187***	0.185***	0.114*
	(0.104)	(0.104)	(0.090)	(0.066)	(0.066)	(0.059)
Controls	No	Yes	Yes	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	No	No	Yes	No	No	Yes
Number of observations	663	663	648	1,094	1,094	1,076
R-squared	.55	.56	.49	.57	.58	.57

(*Continued*)

that unobserved demand shocks could drive our main results. We perform a variety of additional tests to support this conclusion.

First, panel A of Table 3 provides evidence supporting the spillover effects of the PSPP. If the PSPP drives the positive coefficient in 2015, we should observe a lessening of the spillover when we exclude firms rated AAA to A (i.e., the most highly rated firms) from the group of eligible firms in the estimation sample. This test has the advantage that the treated group includes the subset of eligible firms most affected by the ECB purchases during the implementation of the CSPP, that is, those rated A− to BBB− (Grosse-Rueschkamp, Steffen, and Streitz 2019). Consistent with our conjecture, we find that the magnitude of the coefficient for eligible firms in 2015 is reduced to 0.007 − 0.012, or about a quarter of the magnitude of 0.042 − 0.046 when we include all eligible firms in Table 2.

Second, panel B of Table 3 exploits the discontinuity around the investment-grade cutoff. The sample in columns 1–3 includes only BBB+ rated through BB− rated bonds, that is, bonds just above and below the investment-grade cutoff. The estimated coefficient for the *Eligible* dummy variable between 0.23 to 0.28 is nearly three times as large as the one in the full sample in Table 2. The

Table 3
Continued

C. Eligible bond issuers versus eligible nonissuers

	(1)	(2)	(3)
Eligible issuer × *Post*	0.133***	0.133***	0.120***
	(0.044)	(0.044)	(0.044)
Eligible nonissuer × *Post*	0.031	0.029	0.017
	(0.027)	(0.027)	(0.027)
Controls	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes
Country-year fixed effects	No	No	Yes
Number of observations	2,248,514	2,248,514	2,248,514
R-squared	.74	.74	.74

This table presents difference-in-differences estimates of firm-level panel regressions of the ratio of accounts receivable to sales (*Accounts receivable*). *Eligible* is a dummy variable that takes the value of one if a firm had bonds eligible for purchase under the CSPP before the CSPP announcement date (March 2016), and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. Panel A presents the results for eligible firms excluding top-rated (AAA to A) eligible firms. Panel B presents the results restricting the sample to a bandwidth around the investment-grade cutoff, either BBB+ to BB- in columns 1–3 or A+ to B− in columns 4–6. Panel C presents the results for eligible firms that have issued bonds and eligible firms that have not issued bonds after the CSPP announcement date. *Eligible issuer* is a dummy variable that takes the value of one if an eligible firm has issued bonds after the CSPP announcement date (i.e., in the period from March 2016 to December 2017), and zero otherwise. *Eligible nonissuer* is a dummy variable that takes the value of one if an eligible firm has not issued bonds after the CSPP announcement date, and zero otherwise. Regressions include the same control variables as those in Table 2 (coefficients not shown). The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

larger effect is consistent with the evidence that firms just above the investment-grade cutoff experienced larger relative decreases in the cost of capital and issued more bonds than firms with higher ratings (Grosse-Rueschkamp, Steffen, and Streitz 2019). The larger estimated coefficient also suggests that unobserved heterogeneity for firms further away from the investment-grade cutoff tends to bias our estimates downward.

This interpretation is confirmed in columns 4–6, in which we use a wider bandwidth around the investment-grade threshold to A+ rated through B− rated bonds. When we increase the heterogeneity of treated and control firms, the estimates for the coefficient of the *Eligible* dummy variable are lower than in columns 1–3 in Table 2, but still significantly larger than our baseline specifications in columns 1–3 in which all eligible firms are included in the treated group.

Third, we test the hypothesis that eligible firms that have issued bonds after the start of the CSPP, as opposed to eligible firms that did not issue bonds during this period, are responsible for the increase in accounts receivable. To perform this analysis, we decompose the *Eligible* dummy variable into two variables: the *Eligible issuer* dummy variable that takes the value of one if an eligible firm has issued bonds after the CSPP announcement date (i.e., between March 2016 and December 2017), and zero otherwise; and the *Eligible nonissuer* dummy variable that takes the value of one if an eligible firm has not issued bonds after the CSPP announcement date, and zero otherwise. Following the

start of the CSPP, 70% of the eligible firms issue bonds. Consistent with our interpretation of the evidence, panel C of Table 3 shows that the differential increase in accounts receivable is driven by eligible firms that have issued bonds after the start of the CSPP.

Fourth, we perform a placebo test by estimating the change in the ratio of accounts receivable to sales of U.S. investment-grade firms (the main criterion used by the ECB to determine eligible bonds), after the introduction of the CSPP.[10] In this test, we use Compustat data, which only contains publicly listed firms, as Orbis offers a limited coverage of U.S. firms. Table IA.3 in the Internet Appendix reports the estimates. We find no evidence that U.S. investment-grade firms experience an increase in accounts receivable relative to control firms after the CSPP.

Finally, we explore to what extent the effects of the CSPP may spill over beyond eligible firms. Noneligible bonds may be affected by central banks' bond purchases as investors rebalance their portfolios toward these bonds (and thus decreasing their yields). If the CSPP produces spillovers to noneligible bonds, our estimates are lower bounds for the actual effect of the CSPP. To check for this possibility, we estimate the accounts receivables regressions in Table 2, including the interaction of a dummy variable for firms whose bonds are not eligible under the CSPP (*Noneligible*) with the *Post* dummy variable. Table IA.4 in the Internet Appendix shows that the coefficient for the interaction term *Noneligible* × *Post* is not statistically significant and the coefficient for the interaction term *Eligible* × *Post* is similar to that in Table 2. We conclude that firms with noneligible bonds do not seem to extend more trade credit to their customers after the CSPP and are unlikely to attenuate our estimates of the trade credit channel of monetary policy.

2.2 Accounts payable

We also examine the effect of the CSPP on firms in the downstream network of eligible firms. We test whether customers of eligible firms are indeed the recipients of the increase in trade financing by eligible suppliers following the CSPP. The dependent variable is the ratio of accounts payable to sales (*Accounts payable*).

Table 4 shows the results. In panel A, the explanatory variable of interest is the interaction of the treatment group dummy variable *Has eligible supplier* with the *Post* dummy variable. The interaction term *Has eligible supplier* × *Post* measures the differential effect on accounts payable between firms with eligible suppliers and those without eligible suppliers following the CSPP.

In column 1, we find that customers of eligible firms (treatment group) experience an increase in the ratio of accounts payable to sales of 4.8 percentage

[10] We do not use firms domiciled in countries of the European Union that are not members of the euro area as a placebo because some central banks enacted policies similar to those of the ECB during this period.

Table 4
Effect of CSPP on accounts payable of eligible firms' customers

A. Eligible supplier dummy variable

	(1)	(2)	(3)	(4)	(5)	(6)
Has eligible supplier × Post	0.048***		0.045***		0.032*	
	(0.017)		(0.017)		(0.017)	
Has eligible supplier × 2014		0.030		0.028		0.025
		(0.023)		(0.023)		(0.023)
Has eligible supplier × 2015		0.039		0.038		0.039
		(0.027)		(0.027)		(0.027)
Has eligible supplier × 2016		0.034**		0.031*		0.018
		(0.016)		(0.017)		(0.017)
Has eligible supplier × 2017		0.110***		0.107***		0.092***
		(0.034)		(0.034)		(0.034)
log(Assets)			−0.045***	−0.045***	−0.046***	−0.046***
			(0.002)	(0.002)	(0.002)	(0.002)
Cash			0.022***	0.022***	0.026***	0.026***
			(0.005)	(0.005)	(0.005)	(0.005)
PPE			−0.048***	−0.048***	−0.047***	−0.047***
			(0.008)	(0.008)	(0.008)	(0.008)
Net margin			−0.019***	−0.019***	−0.019***	−0.019***
			(0.002)	(0.002)	(0.002)	(0.002)
Liabilities			0.066***	0.066***	0.069***	0.069***
			(0.006)	(0.006)	(0.006)	(0.006)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	No	No	No	No	Yes	Yes
Number of observations	2,248,514	2,248,514	2,248,514	2,248,514	2,248,514	2,248,514
R-squared	.71	.71	.71	.71	.71	.71

(*Continued*)

points relative to customers without a business relationship with eligible firms (control group). The effect is statistically and economically significant as customers of eligible firms relative to the control group benefit from an extension in payment terms of about 17 days ($17 = 0.048 \times 360$), which is about 20% of the sample mean. The results remain robust when we add firm-specific controls (column 3) and country-year fixed effects (column 5) to the specification in column 1, which already includes firm fixed effects and industry-year fixed effect. Importantly, columns 2, 4, and 6 show that treatment and control groups follow parallel trends in the pretreatment period and that the increase in accounts payable is particularly pronounced in 2017. Taken together with the evidence in Tables 2 and 3 for suppliers' provision of trade credit, the evidence on the timing of the increase in accounts payable is consistent with a causal effects of the CSPP. Similarly to the results for *Accounts Receivable*, the coefficient for the interaction term *Eligible* × *2015* is not statistically different from the coefficient for *Eligible* × *2016* (with a *p*-value between .4 and .8), but it is significantly different from the coefficient for *Eligible* × *2017* (with a *p*-value below .01 in all columns). Panel A of Figure IA.3 in the Internet Appendix shows the evolution of the differential effect in accounts payable as estimated in column 2. This confirms that the increase in trade credit usage by customers of eligible firms is unlikely to be driven by demand shocks concomitant to the CSPP.

Table 4
Continued

B. Share of eligible suppliers

	(1)	(2)	(3)	(4)	(5)	(6)
Eligible suppliers share × Post	0.069**		0.067**		0.051*	
	(0.030)		(0.029)		(0.029)	
Eligible suppliers share × 2014		0.051		0.043		0.040
		(0.045)		(0.045)		(0.045)
Eligible suppliers share × 2015		0.039		0.035		0.036
		(0.041)		(0.040)		(0.040)
Eligible suppliers share × 2016		0.050**		0.042*		0.027
		(0.025)		(0.025)		(0.025)
Eligible suppliers share × 2017		0.159***		0.155**		0.138**
		(0.062)		(0.060)		(0.060)
log(Assets)			−0.045***	−0.045***	−0.046***	−0.046***
			(0.002)	(0.002)	(0.002)	(0.002)
Cash			0.022***	0.022***	0.026***	0.026***
			(0.005)	(0.005)	(0.005)	(0.005)
PPE			−0.048***	−0.048***	−0.047***	−0.047***
			(0.008)	(0.008)	(0.008)	(0.008)
Net margin			−0.019***	−0.019***	−0.019***	−0.019***
			(0.002)	(0.002)	(0.002)	(0.002)
Liabilities			0.066***	0.066***	0.069***	0.069***
			(0.006)	(0.006)	(0.006)	(0.006)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	No	No	No	No	Yes	Yes
Number of observations	2,248,514	2,248,514	2,248,514	2,248,514	2,248,514	2,248,514
R-squared	.71	.71	.71	.71	.71	.71

This table presents difference-in-differences estimates of firm-level panel regressions of the ratio of accounts payable to sales (*Accounts payable*). *Has eligible supplier* is a dummy that takes the value of one if a firm had a supplier with CSPP-eligible bonds, and zero otherwise. *Eligible suppliers share* is the number of eligible suppliers divided by the total number of suppliers. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

We also examine whether customers with a larger share of eligible suppliers (measured as a the number of eligible suppliers divided by the total number of suppliers) benefited more from the increase in trade credit by eligible firms. Panel B of Table 4 shows the results. The explanatory variable of interest is the interaction of the treatment variable *Eligible Suppliers Share* with the *Post* dummy variable.

In column 1, we find that the *Eligible suppliers share* × *Post* coefficient is positive and statistically significant. A one-standard-deviation increase in the share of eligible suppliers (0.362 in the sample of customers of eligible suppliers) leads to an increase of 2.5 percentage points ($=0.362 \times 0.069$) in accounts payable of customers of eligible suppliers (i.e., a delay in payment terms of about 9 days), which corresponds to 11% of the sample mean. The results are robust across specifications in columns 3 and 5. In addition, columns 2, 4, and 6 show no evidence of significant preexisting differential trends between treatment and control groups indicating that the two groups of firms are unlikely to experience asymmetric demand shocks. Panel B of Figure IA.3

in the Internet Appendix shows the evolution of the effect in accounts payable, which confirms the inexistence of preexisting differential trends.

We also take into account the 82 customers of eligible firms whose bonds are eligible for purchase under the CSPP. To correct for the overlap between eligible firms and customers of eligible firms whose bonds are eligible for purchase under the CSPP, we estimate regressions in which the dependent variable is the ratio of accounts payable to sales (*Accounts Payable*) including both the *Has eligible supplier* × *Post* and *Eligible* × *Post* interaction terms. Table IA.5 of the Internet Appendix shows that the estimates of the *Has eligible supplier* × *Post* coefficient are similar to those in Table 4.

It is also useful to ask whether following the announcement of the CSPP, eligible firms accelerate payments to their own suppliers, as there also could be positive spillovers in upstream industries (Barrot and Nanda 2020). Table IA.5 shows that eligible firms neither accelerate nor delay payments to their suppliers as the coefficient for *Eligible* × *Post* is insignificant. This finding is consistent with the results of several papers showing that large customers receive plenty of trade credit from their smaller suppliers (e.g., Murfin and Njoroge 2015; Giannetti, Serrano-Velarde, and Tarantino 2021). It should not come at a surprise in our context because suppliers may be able to acquire new customers and expand their market share by extending trade credit (as we will show in Section 4.3), but eligible firms receive no obvious benefits for accelerating payments to smaller suppliers.

2.3 Robustness

A possible concern with our baseline results is that our firm-year panel regressions might be insufficient to cope with the heterogeneity of firms in the sample and the differential effects we estimate may capture asymmetric demand shocks rather than an increase in the supply of trade credit. To further establish the validity of our results, we employ several additional empirical approaches: (1) regressions with size-decile-by-year fixed effects; (2) regressions using a matched sample; (3) regressions excluding firms with less than €10 million in assets from the sample; (4) regressions with country-industry-year fixed effects; (5) regressions with industry-year fixed effects using two-digit SIC codes; (6) regressions with standard errors clustered at the industry-year level; and (7) regressions using the logarithm of the levels of trade finance.

First, we estimate specifications with firm size-decile-by-year fixed effects. We sort firms into size deciles each year based on total assets and interact each decile with yearly dummies. The size-by-year fixed effects control for time-varying demand shocks that could have asymmetric effects on firms of different sizes and contaminate our estimates of the trade credit channel.

Columns 1 and 3 of Table 5, panel A, show a statistically significant increase of about 10 percentage points in the accounts receivable of eligible firms

Table 5
Size decile-by-year fixed effects

A. Accounts receivable

	(1)	(2)	(3)	(4)
Eligible × *Post*	0.095***		0.094***	
	(0.032)		(0.032)	
Eligible × *2014*		−0.015		−0.013
		(0.022)		(0.022)
Eligible × *2015*		0.042		0.045
		(0.043)		(0.043)
Eligible × *2016*		0.066*		0.066*
		(0.038)		(0.038)
Eligible × *2017*		0.144**		0.144**
		(0.062)		(0.062)
Controls	No	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Size decile-by-year fixed effects	Yes	Yes	Yes	Yes
Number of observations	2,248,512	2,248,512	2,248,512	2,248,512
R-squared	.74	.74	.74	.74
B. Accounts payable				
Has eligible supplier × *Post*	0.028*		0.028*	
	(0.017)		(0.017)	
Has eligible supplier × *2014*		0.019		0.019
		(0.023)		(0.023)
Has eligible supplier × *2015*		0.027		0.030
		(0.027)		(0.027)
Has eligible supplier × *2016*		0.009		0.011
		(0.016)		(0.017)
Has eligible supplier × *2017*		0.082**		0.082**
		(0.034)		(0.034)
Controls	No	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Size decile-by-year fixed effects	Yes	Yes	Yes	Yes
Number of observations	2,248,512	2,248,512	2,248,512	2,248,512
R-squared	.71	.71	.71	.71

This table presents difference-in-differences estimates of firm-level panel regressions of the ratio of accounts receivable to sales (*Accounts receivable*) and the ratio of accounts payable to sales (*Accounts payable*). *Eligible* is a dummy variable that takes the value of one if a firm had bonds eligible for purchase under the CSPP before the CSPP announcement date (March 2016), and zero otherwise. *Has eligible supplier* is a dummy that takes the value of one if a firm had a supplier with CSPP-eligible bonds, and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. Firms are sorted into size deciles each year where size is defined as total assets in each year. Regressions include the same control variables as those in Table 2 (coefficients not shown). The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

relative to noneligible firms. Similarly, columns 1 and 3, panel B, we find a corresponding increase in the accounts payable of customers of eligible firms of about 3 percentage points relative to firms without a link to the upstream network of eligible firms. When we estimate the dynamic effects of the CSPP in columns 2 and 4 of panels A and B, we again find statistically significant effects for both accounts receivable of eligible firms and for accounts payable of their customers in the post-CSPP period. Overall, these findings mitigate

the concern that asymmetric demand shocks to firms of different size drive our findings.[11]

Second, we perform the difference-in-differences estimation around the CSPP announcement using a matched sample. We consider both the effect of the CSPP on eligible firms and the effect of the CSPP on customers of eligible firms. We select control firms that best match each firm in the treatment group using propensity score matching with replacement (the nearest neighbor) on multiple covariates in the 2 years preceding the event: *log*(*Assets*), *Firm age*, *Net margin*, and *Sales growth*. Each treated firm is matched to a control firm domiciled in the same region (i.e., either core or periphery) of the euro area. Table IA.7 in the Internet Appendix shows the estimates of the logistic regression used to apply the first step of the propensity score matching procedure. Firm size is the most important covariate in the matching procedure.

Panel A of Table 6 reports the tests of equality of pretreatment means and medians between the treatment and control groups. We cannot reject the hypothesis of equal means or medians between treatment and control groups in the sample of eligible firms. There are a few remaining differences for customers of eligible firms, but firms are well matched on size, age, and performance.

Panel B of Table 6 presents the results of difference-in-differences estimators using the matched sample. The estimates are qualitatively and quantitatively similar to our baseline specifications. Columns 1 and 2 show a 10 to 12 percentage points increase in accounts receivable of eligible firms (treated group) after the CSPP relative to noneligible firms (control group). The economic magnitude of the effect on accounts receivable is similar to that in Table 2, but the effect is more imprecisely estimated in the matched sample. In columns 3 and 4, we find a positive and statistically significant increase in the accounts payable of customers of eligible firms. The effect is also economically significant as the accounts payable of customers of eligible firms increase by 6 to 7 percentage points relative to the control group after the CSPP. The economic magnitude of the effect on accounts payable is larger in the matched sample than that in Table 4. We check the robustness of the matching estimator using a more parsimonious model. Table IA.8 in the Internet Appendix shows the difference-in-differences estimates using only on *log*(*Assets*), *Firm age*, as well as region (core or periphery) as covariates. The results are similar to those in Table 6.

Third, we assess the sensitivity of our baseline results to the exclusion of small firms from the sample. Specifically, we estimate our baseline specifications excluding firms with less than €10 million in assets as of 2015 (the year before the announcement of the CSPP) from the sample. The results in columns 1–3 of Table IA.9 in the Internet Appendix show that the accounts receivable of eligible firms increase by about 10 percentage points relative to noneligible firms in the

[11] The robustness of the estimates in Table 5 is confirmed in Table IA.6 in the Internet Appendix in which we include the interactions of firm size (*Assets*) in 2015 with the *Post* dummy variable or with yearly dummies in the regressions, instead of including firm size decile-by-year fixed effects.

Table 6
Propensity score matching

A. Summary statistics (pretreatment)

	Eligible firms							
	Mean				Median			
	Non treated	Treated	Control	t-test (p-value)	Non treated	Treated	Control	Pearson χ^2 (p-value)
Assets (€million)	24.4	19,890.8	17,066.8	0.808	3.4	9,218.5	9,163.6	0.903
Firm age	21.7	39.2	37.7	0.638	19.0	30.5	33.0	0.807
Net margin	−0.051	0.256	0.186	0.661	0.018	0.181	0.058	0.000
Sales growth	0.177	0.237	0.186	0.676	0.036	0.029	0.027	0.714
	Customers of eligible firms							
	Mean				Median			
	Non treated	Treated	Control	t-test (p-value)	Non treated	Treated	Control	Pearson χ^2 (p-value)
Assets (€million)	23.8	10,195.2	8,952.2	0.969	3.4	1,920.3	1,906.4	1.000
Firm age	21.7	34.4	29.7	0.018	19.0	26.0	23.0	0.242
Net margin	−0.051	0.062	0.046	0.904	0.018	0.078	0.049	0.242
Sales growth	0.177	0.155	0.231	0.324	0.036	0.047	0.023	0.181

B. Difference-in-differences estimates

	Accounts receivable		*Accounts payable*	
	(1)	(2)	(3)	(4)
Eligible × *Post*	0.103	0.129*		
	(0.069)	(0.075)		
Has eligible supplier × *Post*			0.069**	0.059**
			(0.030)	(0.027)
Controls	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Number of observations	1,309	1,272	2,699	2,623
R-squared	.72	.75	.66	.72

This table presents difference-in-differences estimates of firm-level panel regressions of the ratio of accounts receivable to sales (*Accounts receivable*) and accounts payable to sales (*Accounts payable*). Panel A shows pretreatment (CSPP announcement) means and medians of nontreated, treated, and control groups and tests of the difference in mean and median between treated and control groups. Treated firms consist of either firms with eligible bonds (eligible firms) or firms with eligible suppliers (customers of eligible firms). The samples includes only treated firms with nonmissing information in Orbis on the year before the treatment. Nontreated firms are all other firms. Control firms are firms that best match treated firms (nearest neighbor) using propensity score matching with replacement on *log*(*Assets*), *Firm age*, *Net margin*, *Sales growth*, and region (exact match on core or periphery countries). Panel B shows estimates of the difference-in-differences regressions. *Eligible* is a dummy variable that takes the value of one if a firm had bonds eligible for purchase under the CSPP before the CSPP announcement date (March 2016), and zero otherwise. *Has eligible supplier* is a dummy that takes the value of one if a firm had a supplier with CSPP-eligible bonds, and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. Regressions include the same control variables as those in Table 2 (coefficients not shown). The sample consists of a matched sample based on Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

post-CSPP period. The results in columns 4–6 show that the accounts payable of customers of eligible firms increase by about 4 percentage points relative to the control group in the post-CSPP period in this sample. The estimates are similar to those reported in Tables 2 and 4 and confirm that our baseline results are not significantly affected by firm size heterogeneity between treatment and control

groups. We also check the sensitivity of our results using a sample excluding firms domiciled in Germany. We do so because Germany is underrepresented in the Orbis database (see panel A of Table IA.1 in the Internet Appendix). Table IA.10 in the Internet Appendix shows that our results are qualitatively unchanged when we exclude German firms from the sample.

Fourth, we estimate specifications with country-industry-year fixed effects to account for the possibility that our baseline results are driven by time-varying demand shocks across different country-industry pairs. Table IA.11 in the Internet Appendix shows that the accounts receivable of eligible firms experience a statistically significant increase of about 9 percentage points relative to control firms after the CSPP, and the accounts payable of customers of eligible firms experience a statistically significant increase of 3–4 percentage points relative to the control group.

Fifth, we estimate specifications with industry-year fixed effects using two-digit SIC codes instead of the Fama-French 10-industry classification. While the two-digit SIC codes capture better industry heterogeneity, there may be few firms in a given industry in each country. Table IA.12 in the Internet Appendix shows that our estimates are similar to those in Tables 2 and 4 when we use industry dummies based on two-digit SIC codes.

Sixth, we estimate specifications with alternative ways of clustering the standard errors. Table IA.13 in the Internet Appendix shows that our baseline results are robust when we use standard errors adjusted for clustering at the industry-year level instead of at the firm level.

Finally, we estimate our baseline specifications using either the logarithm of accounts receivable or the logarithm of accounts payable as dependent variables. Table IA.14 in the Internet Appendix shows that our estimates are qualitatively unaffected when we use this approach.

3. Heterogeneous Effects

In this section, we investigate how different customer characteristics, such as financial constraints, ability to access public markets, growth opportunities, and tangibility, contribute to monetary policy transmission through trade finance. We also examine whether the effects of the trade credit channel of monetary policy are heterogeneous across regions within the euro area.

3.1 Customer characteristics

To better understand the distributional consequences of the trade credit channel of monetary policy, we explore *which* customers receive more trade credit. We first consider whether the shock of monetary policy on the ability of eligible suppliers to tap bond markets is redistributed to more financially constrained firms. We consider several proxies for firm-specific financial constraints and partition the sample by the median of these proxies. Table 7 presents the estimates of our baseline accounts payable regression (in column 3 of Table 4)

Table 7
Effect of CSPP on accounts payable of eligible firms' customers: The role of financial constraints

Panel A

	Investment-grade (1)	*Non-investment-grade* (2)	*Rated* (3)	*Unrated* (4)	*Mature firms* (5)	*Young firms* (6)	*Low liabilities* (7)	*High liabilities* (8)	*High PPE* (9)	*Low Low PPE* (10)
Has eligible supplier × Post	−0.049	0.041**	−0.055	0.047**	0.018	0.080*	0.023	0.082***	0.022**	0.039*
	(0.048)	(0.021)	(0.038)	(0.022)	(0.015)	(0.042)	(0.020)	(0.031)	(0.010)	(0.023)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	697	2,247,817	1,169	2,247,345	1,153,984	1,086,350	1,124,258	1,124,256	1,124,256	1,124,258
R-squared	.77	.71	.68	.71	.70	.73	.71	.72	.72	.72
Difference (*p*-value)	.07		.02		.17		.11		.51	

Panel B

	High sales (1)	*Low sales* (2)	*Low sales growth* (3)	*High sales growth* (4)	*Low asset growth* (5)	*High asset growth* (6)	*High EBITDA* (7)	*Low EBITDA* (8)	*Public firms* (9)	*Private firms* (10)
Has eligible supplier × Post	0.019	0.191	0.027	0.044**	0.014	0.052**	0.014**	0.066***	0.010	0.073**
	(0.015)	(0.371)	(0.032)	(0.019)	(0.029)	(0.026)	(0.006)	(0.023)	(0.023)	(0.035)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,124,256	1,124,258	1,081,305	1,081,303	1,090,815	1,090,812	1,082,737	1,082,737	10,051	2,238,463
R-squared	.68	.72	.76	.75	.75	.73	.71	.72	.69	.71
Difference (*p*-value)	.75		.63		.47		.06		.13	

This table presents difference-in-differences estimates of firm-level panel regressions of the ratio of accounts payable to sales. *Has eligible supplier* is a dummy that takes the value of one if a firm had a supplier with CSPP-eligible bonds, and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. Regressions include the same control variables as those in Table 2 (coefficients not shown). The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Columns 1 and 2, panel A, present the results for the group of firms with investment-grade rating and the group of firms either with a speculative-grade rating or without a credit rating. Columns 3 and 4, panel A, present the results for the group of firms with a credit rating and the group of firms without a credit rating. In columns 5 and 6, panel A, the mature and young firms' groups consist of those firms that are above and below the median of the distribution of firm's age since the year of incorporation. In columns 7 and 8, panel A, the low and high liabilities groups consist of those firms that are below or above the median of the distribution of the ratio of liabilities to assets. In columns 9 and 10, panel A, the low and high PPE groups consist of those firms that are below or above the median of the distribution of the ratio of PPE to assets. In columns 1 and 2, panel B, the low and high sales groups consist of those firms that are below or above the median of the distribution of sales. In columns 3 and 4, panel B, the low and high sales growth groups consist of those firms that are below or above the median of the distribution of sales growth. In columns 5 and 6, panel B, the low and high asset growth groups consist of those firms that are below or above the median of the distribution of asset growth. In columns 7 and 8, panel B, the low and high EBITDA groups consist of those firms that are below or above the median of the distribution of EBITDA-to-assets ratio. Columns 9 and 10, panel B, present the results for the group of publicly listed firms and the group of privately held firms. The difference is the *p*-value of the F-test for the difference on the *Has eligible supplier × Post* coefficient between groups. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

for the subsamples of constrained and unconstrained firms (i.e., customers of eligible firms).

Columns 1 and 2 in panel A show the estimates separately for the group of investment-grade firms (i.e., long-term issuer credit rating of at least BBB− before the CSPP) versus the group of non-investment-grade firms, which does not benefit directly from the CSPP. Columns 3 and 4 show the estimates for the group of rated firms (i.e., firms with long-term issuer credit rating before the CSPP) versus unrated firms. We find that only non-investment-grade and unrated firms with eligible suppliers experience a statistically significant increase in accounts payable in the post-CSPP period. These results suggest that customers of eligible firms that are not able to tap public debt markets benefit from the increase in trade credit offered by eligible firms.

In columns 5 and 6, we distinguish between mature firms and young firms (based on the median of firm age), as the latter are likely to face more financial frictions and have more growth opportunities. Even though the difference is not statistically significant, the point estimate for the increase in payables is larger for young firms than mature firms.

Columns 7 and 8 show that only customers of eligible firms with a high ratio of liabilities to assets (i.e., above the median of the ratio) experience a positive and statistically significant increase in accounts payable relative to control firms in the post-CSPP period. These results suggest that firms with higher external financial dependence benefit more from the increase in trade credit offered by eligible firms.

In columns 9 and 10, we find a positive and statistically significant increase in accounts payable for customers of eligible firms with both a high and low PPE-to-assets ratio (based on the median of the ratio) in the post-CSPP period. While we cannot statistically distinguish the two coefficients, the magnitude of the coefficient of the *Has eligible supplier* × *Post* variable in the group of low PPE firms is almost the double than that in the group of high PPE firms. These results suggest that customers of eligible firms that have less tangible assets to pledge as collateral are more likely to benefit from the increase in trade credit offered by eligible firms after the CSPP.

Panel B of Table 7, column 1, shows statistically insignificant increases in accounts payable of large firms with eligible suppliers (i.e., firms with above-median sales). The magnitude of the coefficient for the *Has eligible supplier* × *Post* variable in the group of small firms (column 2) is much larger in magnitude than that in the sample of large firms, but it is imprecisely estimated.

Columns 3–6 show that only customers of eligible firms with high rates of sales growth and asset growth experience a positive and statistically significant increase in accounts payable in the post-CSPP period. These results suggest that firms with greater growth opportunities and more external financing needs benefit more from the increase in trade credit offered by eligible suppliers. In addition, columns 7 and 8 show that customers of eligible firms with lower cash flows from operations (firms with below-median EBITDA-to-assets ratio) have

a significantly higher increase in accounts payable than customers with higher ability to generate internal financing.

Finally, column 9 shows no statistically significant increase in the ratio of accounts payable to sales for customers of eligible firms that are publicly listed. Customers with better access to public markets are less likely to be financially constrained and rely less on trade finance. Column 10 shows that customers of eligible firms that are private experience a statistically significant increase in the ratio of accounts payable to sales. The results suggest that firms without the ability to tap public markets obtain more trade credit from eligible suppliers.

Overall, our results suggest that the trade credit channel of monetary policy helps to redistribute the benefits of unconventional monetary policy interventions to financially constrained firms in the downstream network of eligible firms.

3.2 Regional effects

In this section, we examine whether the CSPP produces heterogeneous effects across regions. Specifically, we study how the CSPP affects firms domiciled in countries of either the core (Austria, Belgium, Finland, France, Germany, Luxembourg, and Netherlands) or the periphery of the euro area. This is important as countries in the periphery were more negatively affected by the 2011–2012 European sovereign debt crisis and firms in these countries were more likely to face financial constraints. In fact, firms in periphery countries are more likely to be affected because of the repercussions of the sovereign debt crisis on the banking system and less developed financial markets. These tests allow us to evaluate whether the benefits of the CSPP are redistributed from firms domiciled in core countries to firms domiciled in periphery countries. Specifically, we investigate whether the customers of eligible firms in periphery countries are able to fill their financing gap by delaying the payment of goods and services purchased from eligible firms, especially from those located in core countries.

Panel A of Table 8 shows the estimates of our baseline accounts receivable and payable regressions separately for the group of firms in core countries and periphery countries of the euro area. We find that eligible firms from core countries significantly increase accounts receivable relative to noneligible firms following the CSPP, while eligible firms in the periphery show no effect. In contrast, customers from the periphery experience a statistically significant increase in accounts payable, whereas customers in the core experience no effect. This suggests that customers of eligible firms, facing deteriorating credit market conditions in the periphery, were able to fill their financing gap using trade credit as the CSPP decreased the cost of extending trade credit for eligible suppliers.

Panel B of Table 8 provides a more direct test for the regional propagation of monetary policy through the trade credit channel by decomposing the *Has eligible supplier* dummy variable into two variables: the *Has core eligible*

Table 8
Core versus periphery countries

A. Core versus periphery countries

	Core countries		Periphery countries	
	Accounts receivable (1)	*Accounts payable* (2)	*Accounts receivable* (3)	*Accounts payable* (4)
Eligible × *Post*	0.126***		0.018	
	(0.045)		(0.025)	
Has eligible supplier × *Post*		0.017		0.072**
		(0.019)		(0.032)
Controls	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Number of observations	650,691	650,691	1,597,823	1,597,823
R-squared	.77	.71	.73	.71

B. Core versus periphery countries eligible suppliers

	Euro area countries	Core countries	Periphery countries
	Accounts payable		
	(1)	(2)	(3)
Has core eligible supplier × *Post*	0.044**	0.018	0.077
	(0.020)	(0.020)	(0.051)
Has periphery eligible supplier × *Post*	0.024*	0.003	0.034*
	(0.013)	(0.023)	(0.018)
Controls	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes
Number of observations	2,248,514	650,691	1,597,823
R-squared	.71	.71	.71

This table presents difference-in-differences estimates of firm-level panel regressions of the ratio of accounts receivable to sales and the ratio of accounts payable to sales. *Eligible* is a dummy variable that takes the value of one if a firm had bonds eligible for purchase under the CSPP before the CSPP announcement date (March 2016), and zero otherwise. *Has eligible supplier* is a dummy that takes the value of one if a firm had a supplier with CSPP-eligible bonds, and zero otherwise. *Has core eligible supplier* is a dummy variable that takes the value of one if a firm is a customer of eligible firms with headquarters in core countries, and zero otherwise. *Has periphery eligible supplier* is a dummy variable that takes the value of one if a firm is a customer of eligible firms with headquarters in periphery countries, and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. Regressions include the same control variables as those in Table 2 (coefficients not shown). The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

supplier dummy variable that takes the value of one if a firm has an eligible supplier domiciled in a core country, and zero otherwise; and the *Has periphery eligible supplier* dummy variable that takes the value of one if a firm has an eligible supplier domiciled in a periphery country, and zero otherwise.

In column 1, we find that customers of eligible suppliers located in core countries increase their accounts payable to a larger extent than customers of eligible suppliers located in periphery countries. Column 2 shows that these effects are not driven by customers in core countries, as they experience changes in accounts payable that are not statistically distinguishable from those of

control firms, regardless of where their suppliers are located. In column 3, we find that customers located in periphery countries had more access to trade credit during the post-CSPP period. The coefficient for the *Has core eligible supplier* dummy variable is more than twice in magnitude than that of the *Has periphery eligible supplier* dummy variable, even though it is imprecisely estimated. In Table IA.15 in the Internet Appendix, we show the estimates when we limit the periphery countries to Italy and Spain, the two largest countries in the periphery, for which we potentially have a better control sample. We find a positive and statistically significant coefficient (at the 10% level) for the *Has core eligible supplier* dummy variable, while the coefficient for the *Has periphery eligible supplier* dummy variable is insignificant. Overall, our results suggest that suppliers from core countries extend more trade credit following the CSPP to firms located in periphery countries that are part of their production network.

4. Real Effects

In this section, we first investigate the impact of the CSPP on the investment and financial policies of eligible firms. We then investigate whether the trade credit channel of monetary policy produce real effects on customers of CSPP-eligible firms. Finally, we examine whether the CSPP affects the competitive position of eligible suppliers in product markets.

4.1 Effects on eligible firms

In this section, we test whether the CSPP affects the investment and financing decisions of eligible firms. This also allows us to compare the economic relevance of the trade channel to other channels through which unconventional monetary policy affects the real economy.

Panel A of Table 9 shows that eligible firms experience a significant increase in total assets following the CSPP. Column 1 shows that eligible firms experience a statistically significant increase of 2.3 percentage points in *Asset growth* relative to the control group. Next, we decompose the increase in total assets into *CAPEX*, Δ*Inventories*, and Δ*Accounts receivable* (all variables scaled by lagged total assets). We find that eligible firms experience an increase of 0.2 percentage points in *CAPEX* (column 2), an increase of 0.5 percentage points in inventories (column 3), and an increase of 0.9 percentage points in accounts receivable (column 4) relative to control firms. In addition, column 5 shows a 1.8 percentage points increase in the growth rate of employment (*Labor growth*) for eligible firms relative to control firms. While the increases in *CAPEX*, Δ*Inventories*, and *Labor growth* are not statistically significant, the increase in Δ*Accounts receivable* is significant and consistent with our findings on the ratio of accounts receivable to sales in Table 2.

Panel B of Table 9 shows the effect on the external and internal financing of eligible firms (all variables scaled by lagged total assets). Column 1 shows

Table 9
Real effects of CSPP: Investment and financing of eligible firms

A. Investment in fixed assets, working capital, and human capital

	Asset growth (1)	*CAPEX* (2)	*ΔInventories* (3)	*ΔAccounts receivable* (4)	*Labor growth* (5)
Eligible × *Post*	0.023*	0.002	0.005	0.009**	0.018
	(0.014)	(0.005)	(0.004)	(0.004)	(0.024)
Controls	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	2,248,512	2,116,731	2,246,443	2,247,599	1,779,908
R-squared	.55	.48	.32	.29	.28

B. External and internal financing

	ΔTotal debt (1)	*ΔLong-term debt* (2)	*ΔShort-term debt* (3)	*ΔAccounts payable* (4)	*ΔCash* (5)
Eligible × *Post*	0.016	0.009	0.005	0.008**	0.003
	(0.011)	(0.011)	(0.006)	(0.003)	(0.005)
Controls	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	2,081,233	2,090,098	2,236,327	2,238,452	2,231,250
R-squared	.26	.22	.18	.27	.43

This table presents difference-in-differences estimates of firm-level panel regressions of eligible firm outcomes. Panel A shows regressions for asset growth, CAPEX (scaled by lagged assets), change in inventories (scaled by lagged assets), change in receivable (scaled by lagged assets), and labor growth. Panel B shows regressions for change in total debt, change in long-term debt, change in short-term debt, change in accounts payable, and change in cash (all variables scaled by lagged assets). *Eligible* is a dummy variable that takes the value of one if a firm had bonds eligible for purchase under the CSPP before the CSPP announcement date (March 2016), and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. Regressions include the same control variables as those in Table 2 (coefficients not shown). The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

an increase of 1.6 percentage points in external finance (Δ*Total debt*) in the post-CSPP period, which can be decomposed into increases of 0.9 percentage points (column 2) and 0.5 percentage points (column 3) in long-term debt and short-term debt, respectively. Column 4 shows that eligible firms experience a statistically significant increase of 0.8 percentage points in Δ*Accounts payable* relative to control firms, which is consistent with our conjecture that any firm, independently from its liquidity position, finds it optimal to accept trade credit. Column 5 shows no effect on cash holdings (Δ*Cash*). While the positive effects of the CSPP on financial debt of eligible firms are imprecisely estimated in panel B of Table 9, Table IA.16 in the Internet Appendix shows a statistically significant increase in net debt issuance for eligible firms located in core countries.[12]

[12] Consistent with previous literature, Figure IA.4 in the Internet Appendix shows that eligible firms experience a statistically significant increase in net bond issuance relative to noneligible firms after the announcement of the CSPP. The results are determined using a sample of publicly listed firms drawn from Capital IQ/Compustat

In sum, eligible firms increase total assets, which is mainly explained by the increase in trade credit to customers (accounts receivable) as a result of the CSPP. Our results suggest that for each additional euro of net debt issuance, eligible firms pass through 56 cents (=0.009/0.016 using the estimates in column 4 of panel A and column 1 of panel B) in trade credit to customers.

4.2 Effects on customers of eligible firms

Our results show that firms in the downstream network receive more trade credit from eligible firms with easier access to the bond market thanks to the unconventional monetary policy. In this section, we test whether the customers of eligible firms can take advantage of potential investment opportunities due to the increase in the supply of trade credit. We also explore how the trade credit channel of monetary policy affects the financial policy of customers of eligible firms.

Panel A of Table 10 shows that the customers of eligible firms experience a statistically significant increase in total assets following the CSPP arguably due to the increased ability to rely on trade credit. Column 1 shows that firms in the treatment group experience a statistically significant (at the 10% level) increase of 2.5 percentage points in *Asset growth* relative to the control group. Next, we decompose this increase in asset growth into *CAPEX*, Δ*Inventories*, and Δ*Accounts receivable* (all variables scaled by lagged total assets). We find that customers of eligible firms experience an increase of 0.5 percentage points in *CAPEX* (column 2), an increase of 0.4 percentage points in inventories (column 3), and an increase of 1.1 percentage points in accounts receivable (column 4) relative to control firms. In addition, column 5 shows a 3.4 percentage points increase in the growth rate of employment (*Labor growth*) for treated firms relative to control firms.

Our estimates show that the trade credit channel of monetary policy is quantitatively similar to the effect of the credit reallocation from eligible firms to noneligible firms following the CSPP. In a sample of firms that access the syndicated loan market, Grosse-Rueschkamp, Steffen, and Streitz (2019) estimate that noneligible firms that borrow from banks with a high share of eligible borrowers experience an increase in asset growth rates of about 6.5 percentage points and an increase in the CAPEX to fixed assets ratio of about 3.8 percentage points (i.e., 64% of the mean at 5.9%) following the CSPP. Arce, Mayordomo, and Gimeno (2020) find that noneligible firms in Spain experience an increase in the CAPEX-to-assets ratio of about 20% of the mean due to the bank credit reallocation following the CSPP.

We find that customers of eligible firms experience a differential increase of 2.5 percentage points in asset growth rates, 0.5 percentage points in the CAPEX-to-assets ratio (i.e., 20% of the pre-CSPP mean ratio at 2.6%), and

Global. This effect is concentrated in core countries. In contrast, we find no differential effect on bank debt once the CSPP went into effect.

Table 10
Real effects of CSPP: Investment and financing of eligible firms' customers

A. Investment in fixed assets, working capital, and human capital

	Asset growth (1)	*CAPEX* (2)	*ΔInventories* (3)	*ΔAccounts receivable* (4)	*Labor growth* (5)
Has eligible supplier × Post	0.025*	0.005*	0.004**	0.011**	0.034*
	(0.015)	(0.003)	(0.002)	(0.005)	(0.020)
Controls	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	2,248,512	2,116,731	2,246,443	2,247,599	1,779,908
R-squared	.55	.48	.32	.29	.28

B. External and internal financing

	ΔTotal debt (1)	*ΔLong-term debt* (2)	*ΔShort-term debt* (3)	*ΔAccounts payable* (4)	*ΔCash* (5)
Has eligible supplier × Post	0.008	0.005	0.004	0.009***	−0.007
	(0.007)	(0.007)	(0.004)	(0.003)	(0.004)
Controls	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	2,081,233	2,090,098	2,236,327	2,238,452	2,231,250
R-squared	.26	.22	.18	.27	.43

This table presents difference-in-differences estimates of firm-level panel regressions of eligible firms' customers outcomes. Panel A shows regressions for asset growth, CAPEX (scaled by lagged assets), change in inventories (scaled by lagged assets), change in receivable (scaled by lagged assets), and labor growth. Panel B shows regressions for change in total debt, change in long-term debt, change in short-term debt, change in accounts payable, and change in cash (all variables scaled by lagged assets). *Has eligible supplier* is a dummy that takes the value of one if a firm had a supplier with CSPP-eligible bonds, and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. Regressions include the same control variables as those in Table 2 (coefficients not shown). The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

3.4 percentage points in the growth rate of employment. Our estimates are of the same order of magnitude as those in Arce, Mayordomo, and Gimeno (2020) but economically smaller than those in Grosse-Rueschkamp, Steffen, and Streitz (2019). While our eurozone sample of private firms is similar to the Spanish sample of firms in Arce, Mayordomo, and Gimeno (2020), the sample in Grosse-Rueschkamp, Steffen, and Streitz (2019) consists of larger firms with access to the syndicated loan market.

We also view our estimated effect of the trade credit channel as economically meaningful in comparison to the effects of other unconventional monetary policy tools. For instance, Acharya et al. (2019) estimate that the ECB's purchases of sovereign bonds in the secondary market under the 2012 Outright Monetary Transaction program had no effect on investment as firms that benefitted from an increase in the supply of credit from the most affected banks just built up cash holdings.

We also investigate how customers of eligible firms finance their asset growth. Panel B of Table 10 shows the effect on external and internal financing (all

variables scaled by lagged total assets). We find that an increase in external finance (Δ*Total debt*) in column 1 but statistically insignificant, including long-term debt in column 2 and short-term debt in column 3. Column 4 shows that customers of eligible firms experience a statistically significant increase of 0.9 percentage points in Δ*Accounts payable* relative to control firms in the post-CSPP period, which is consistent with our findings on the ratio of accounts payable to sales in Table 4. This increase in accounts payable represents more than one third of the capital needs to fund the asset growth of treated firms. Finally, the differential effect on cash holdings (Δ*Cash*) in column 5 is negative but statistically insignificant.

In sum, the customers of eligible firms increase investment in fixed assets, human capital, and working capital as a result of the CSPP suggesting that the propagation of monetary policy shocks through trade credit helps to relax financial constraints in the downstream network of eligible firms. These real effects seem to be ascribed to suppliers' trade finance, independently from the effect of a potential improvement in the access to bank credit.

4.3 Effects on product market competition

This section sheds light on the benefits to suppliers in product markets from extending trade credit. We test the hypothesis that eligible suppliers extend more trade credit following the decrease in their cost of capital due to the CSPP in order to strengthen their position in product markets. Specifically, we test whether CSPP-eligible firms are more likely to retain existing customers and establish new business relationships than noneligible comparable firms in the post-CSPP period. We consider the number of customer relationships that a firm is able to maintain and the number of new relationships that are initiated as dependent variables. The sample consists of FactSet Revere Supply Chain Relationship nonfinancial firms based in the euro area in the 2013–2017 period. The regressions include firm fixed effects and industry-year fixed effects.

Panel A of Table 11 reports the results. In column 1, we find that eligible firms are able to maintain a greater number of customer relationships relative to noneligible firms after the CSPP. The results remain robust when we add firm-specific controls (column 2) and country-year fixed effects (column 3) to the specification in column 1.

In column 4, we find that eligible firms experience a statistically significant increase of 3.3 in new customer relationships relative to noneligible firms in the post-CSPP period. The effect is economically significant as the sample average of new customer relationships is 8.3 in the group of eligible firms. The results remain robust in columns 5 and 6 when we add firm-specific controls and country-year fixed effects to the specification in column 4.

We also analyze product market outcomes separately for the group of eligible firms from core countries and the group of eligible firms from periphery countries; the latter group did not increase the provision of trade credit after the start of the CSPP, as shown in Table 8. Panel B reports the results for

Table 11
Effect of CSPP on customer relationships maintained and new relationships initiated of eligible firms

	Number of customers kept			Number of new customers		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Euro area countries						
Eligible × *Post*	9.514***	8.223***	7.624***	3.281***	2.767**	2.715**
	(1.935)	(2.073)	(2.043)	(1.234)	(1.231)	(1.219)
Controls	No	Yes	Yes	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	No	No	Yes	No	No	Yes
Number of observations	9,434	6,045	6,037	9,434	6,045	6,037
R-squared	.84	.84	.84	.58	.57	.57
B. Core countries						
Eligible × *Post*	11.700***	9.968***	9.729***	4.247***	3.429**	3.612**
	(2.334)	(2.510)	(2.481)	(1.482)	(1.501)	(1.471)
Controls	No	Yes	Yes	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	No	No	Yes	No	No	Yes
Number of observations	6,558	3,883	3,883	6,558	3,883	3,883
R-squared	.84	.84	.84	.59	.58	.58
C. Periphery countries						
Eligible × *Post*	0.624	−0.359	−0.580	−0.419	0.275	−0.648
	(1.318)	(1.445)	(1.548)	(1.152)	(1.058)	(1.248)
Controls	No	Yes	Yes	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	No	No	Yes	No	No	Yes
Number of observations	2,876	2,162	2,154	2,876	2,162	2,154
R-squared	.85	.85	.85	.51	.50	.52

This table presents difference-in-differences estimates of firm-level panel regressions of the number of customer relationships maintained and number of new customer relationships initiated. The dependent variable in columns 1–3 is the number of customer relationships maintained by a supplier relative to the existing relationships in the previous year. The dependent variable in columns 4–6 is the number of new customer relationships initiated by a supplier relative to the existing relationships in the previous year. *Eligible* is a dummy variable that takes the value of one if a firm had bonds eligible for purchase under the CSPP before the CSPP announcement date (March 2016), and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. Regressions include the same control variables as those in Table 2 (coefficients not shown). The sample consists of FactSet Revere Supply Chain Relationship nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

suppliers in core countries. We find that eligible firms from core countries experience a significant differential increase in both the number of customer relationships maintained and the number of new customer relationships initiated after the CSPP. Panel C shows that eligible firms from periphery countries do not experience similar differential increases.

Table IA.17 in the Internet Appendix shows that the results are robust when we use the logarithm of one plus either the number of customer relationships kept or the number of new customer relationships initiated as the dependent variables. The differential effect on the number of customers kept is statistically and economically significant at 15% to 26%, while the effect is 20% to 24% on the number of new customers. Table IA.18 in the Internet Appendix shows that

Table 12
Effect of CSPP on sales market share of eligible firms

	Euro area countries		Core countries		Periphery countries	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post*	0.373*	0.374*	0.499*	0.499*	0.108	0.109
	(0.226)	(0.226)	(0.302)	(0.302)	(0.297)	(0.297)
Controls	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	2,248,514	2,248,514	650,691	650,691	1,597,823	1,597,823
R-squared	.98	.98	.98	.98	.97	.97

This table presents difference-in-differences estimates of firm-level panel regressions of sales market share at the four-digit SIC level. *Eligible* is a dummy variable that takes the value of one if a firm had bonds eligible for purchase under the CSPP before the CSPP announcement date (March 2016), and zero otherwise. *Post* is a dummy variable that takes the value of one in the years 2016 and 2017, and zero otherwise. Regressions include the same control variables as those in Table 2 (coefficients not shown). The sample consists of Bureau Van Dijk's Orbis nonfinancial firms based in the euro area in the 2013–2017 period. All explanatory variables are lagged by 1 year. Table A.1 in the appendix defines the variables. Robust standard errors adjusted for firm-level clustering are reported in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

the results in Table 11 are robust when we limit the analysis only to eligible firms and their euro area competitors as identified by FactSet Revere as a control group. The point estimates are both qualitatively and quantitatively similar.

We complement the results at the extensive margin in Table 11 with results at the intensive margin. We provide additional evidence that eligible firms strengthen their competitive position after the CSPP using the sales market share at the four-digit SIC level as a product market outcome variable. Table 12 presents the estimates. In columns 1 and 2, we find that eligible firms increase their market share by 0.37 percentage points relative to noneligible firms after the CSPP. Columns 3 and 4 show that eligible firms from core countries experience a statistically significant differential increase in market share of 0.50 percentage points, whereas columns 5 and 6 show that those from periphery countries do not experience a similar increase.

Overall, our findings suggest that unconventional monetary policies interventions that reduce the cost of capital of large firms with access to capital markets can affect firms' competitive positions in product markets and contribute to concentration in upstream markets.

5. Conclusion

In current economic conditions with interest rates near the zero lower bound in most developed economies, monetary authorities have resorted to unconventional monetary policy interventions to achieve their inflation targets. Large-scale asset purchases reducing the cost of public debt favor large firms that have easier access to public debt markets. Concerns about the asymmetric effects of these unconventional monetary policies are particularly relevant in economic areas, such as the euro area, with differences in creditor protections and bond market development across regions.

We show that trade credit in production networks can mitigate the asymmetric effects of central banks' asset purchases programs. Firms with bonds eligible for purchase under the ECB's CSPP (i.e., investment-grade bonds) expand the provision of trade credit to their customers. The expansion in trade credit benefits especially financially constrained customers and customers located in periphery countries where the European sovereign debt crisis was more severe and led to bank credit tightening.

We also provide evidence of real effects and product market effects of unconventional monetary policies. As a result of the increase in trade finance, firms with suppliers whose bonds are eligible under the CSPP increase employment and investment and provide more trade credit to their own customers. Trade credit thus plays an important role in the transmission of monetary policy. While economic and financial integration through production networks can facilitate the transmission of monetary policy to economically depressed regions and to firms with limited access to financial markets, we also show that eligible firms are able to expand their customer base due to their ability to extend more trade finance to their customers. This suggests that unconventional monetary policy can increase concentration in upstream industries with potentially long-lasting consequences on the spatial distribution of economic activity.

Our paper has important implications for the understanding of the transmission mechanisms of unconventional monetary policy interventions, a topic of tantamount importance as large-scale asset purchases have been considerably expanded worldwide in light of the looming recession due to the COVID-19 pandemic.

Appendix

Table A.1
Variable definitions

Variable	Definition
Eligible	Dummy variable that equals one if a firm had bonds eligible for purchase under the CSPP before the CSPP announcement, and zero otherwise
Eligible issuer	Dummy variable that takes the value of one if an eligible firm has issued bonds after the CSPP announcement date, and zero otherwise
Eligible nonissuer	Dummy variable that takes the value of one if an eligible firm has not issued bonds after the CSPP announcement date, and zero otherwise
Has eligible supplier	Dummy variable that equals one if a firm is reported as a customer of eligible firms in FactSet Revere Supply Chain Relationships database, and zero otherwise
Eligible suppliers share	Number of eligible suppliers divided by total number of suppliers
Has core eligible supplier	Dummy variable that equals one if a firm is a customer of an eligible firm with headquarters in core countries, and zero otherwise
Has periphery eligible supplier	Dummy variable that equals one if a firm is a customer of an eligible firm with headquarters in periphery countries, and zero otherwise
Accounts receivable	Accounts receivable (Orbis item *DEBTORS*) divided by operating revenue (Orbis item *OPRE*)
Accounts payable	Accounts payable (Orbis item *CREDITORS*) divided by operating revenue (Orbis item *OPRE*)
Assets	Total assets (Orbis item *TOAS*)
Sales	Operating revenue (Orbis item *OPRE*)
Cash	Cash and cash equivalent (Orbis item *CASH*) divided by total assets (Orbis item *TOAS*)
PPE	Tangible fixed assets (Orbis item *TFAS*) divided by total assets (Orbis item *TOAS*)
Net margin	Net income (Orbis item *PL*) divided by operating revenue (Orbis item *OPRE*)
Liabilities	Current liabilities (Orbis item *CULI*) plus noncurrent liabilities (Orbis item *NCLI*) divided by total assets (Orbis item *TOAS*)
Asset growth	Change in total assets (Orbis item *TOAS*) divided by the previous year total assets
CAPEX	Change in tangible fixed assets (Orbis item *TFAS*) plus depreciation and amortization (Orbis item *DEPR*) divided by the previous year total assets (Orbis item *TOAS*)
Labor growth	Change in number of employees (Orbis item *EMPL*) divided by the previous year number of employees
Δ*Inventories*	Change in inventories (Orbis item *STOK*) divided by the previous year total assets (Orbis item *TOAS*)
Δ*Accounts receivable*	Change in accounts receivable (Orbis item *DEBTORS*) divided by the previous year total assets (Orbis item *TOAS*)
Sales growth	Change in operating revenue (Orbis item *OPRE*) divided by the previous year operating revenue
EBITDA	EBITDA (Orbis item *EBTA*) divided by the previous year total assets (Orbis item *TOAS*)
Firm age	Number of years since the year of incorporation (*DATEINC_YEAR*)
Δ*Accounts payable*	Change in accounts payable (Orbis item *CREDITORS*) divided by the previous year total assets (Orbis item *TOAS*)
Δ*Total debt*	Change in financial debt (Orbis item *LTDB* plus Orbis item *LOAN*) divided by the previous year total assets (Orbis item *TOAS*)
Δ*Long-term debt*	Change in long-term debt (Orbis item *LTDB*) divided by the previous year total assets (Orbis item *TOAS*)
Δ*Short-term debt*	Change in current loans (Orbis item *LOAN*) divided by the previous year total assets (Orbis item *TOAS*)
Δ*Cash*	Change in cash and cash equivalents (Orbis item *CASH*) divided by the previous year total assets (Orbis item *TOAS*)

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