Global Demand Spillovers: When the Central Bank Buys Corporate Bonds *

Kerry Siani†

February 11, 2021

Abstract

Using novel data and a difference-in-differences strategy, I find that central bank corporate bond purchases spillover to foreign jurisdictions through bond underwriting networks. The diff-in-diff exploits the European Central Bank's 2016 corporate sector purchase program. I compare U.S. firms connected to underwriters with more or less Eurozone clients. Firms connected with banks with more European clients had larger orderbooks and issued more at lower costs. Treated firms do not increase real investment, but rather increase equity payouts. I identify a novel channel through which demand shocks spread across borders: the bond underwriting network. These results matter for understanding the overall impact of corporate quantitative easing programs.

Keywords: Corporate bonds, unconventional monetary policy, securities underwriting

JEL codes: G24, G15, E52, E44

*I am grateful for the comments and suggestions of Simona Abis, Charles Calomiris, Kent Daniel, Olivier Darmouni, Xavier Giroud, Wei Jiang, Yiming Ma, Andrew MacKinlay (discussant), Harry Mamaysky, Lira Mota, Giorgia Piacentino, Tomasz Piskorski, Tano Santos, David Scharfstein, Sergio Schmukler (discussant), Suresh Sundaresan, Cristina Tessari, Kairong Xiao, Neng Wang, and seminar participants at Columbia Business School, the EFA Annual Meeting 2020, and NFA Annual Conference PhD Session 2020. Thank you to the Chazen Institute for helping fund this project. Thank you to Informa Global Markets for access to and help with the data.

†Columbia Business School. Email: kfy2101@columbia.edu

In recent years, central banks have adopted the unconventional monetary policy of buying corporate bonds. The European Central Bank (ECB) announced its 2016 corporate bond purchasing program (CSPP) to "further strengthen the pass-through of the Eurosystem's asset purchases to the financing conditions of the real economy."[1] In a break from convention, the U.S. Federal Reserve announced in March 2020 that it, too, would participate in large-scale corporate bond purchases. Meanwhile, the reliance of large corporations on financing from corporate bonds has increased significantly since the Great Recession. U.S. corporations issued over $1.3 trillion in bonds in 2018, representing 6 times the volume of equity issuance and a 27% increase since 2010.[2] Is there any economic spillover from the ECB's corporate quantitative easing program, and if so, how does this spread to U.S. firms? Are there any long term effects?

Evaluating spillover effects of monetary policy across economies that are already linked through trade and large global corporations is difficult because monetary measures often coincide with other significant macro events and fiscal policy that could impact the real economy. In this paper, I overcome this problem by tracing spillovers via bond underwriting networks. Specifically, I compare issuance outcomes and decisions at non-European firms that are more and less exposed to the ECB's corporate bond-buying program through their bank underwriting networks. The logic is as follows: U.S. firms that were more exposed to the ECB's bond buying program responded to the corresponding positive shifts in bond demand by issuing more bonds at lower yields, a response congruous to that of eligible firms in the Eurozone.[3]

[1] ECB Press Release, April 21, 2016: ECB announces details of the corporate sector purchase programme (CSPP) https://www.ecb.europa.eu/press/pr/date/2016/html/pr160421_1.en.html

[2] SIFMA 2019; see Appendix for more aggregate data on this market.

[3] See, for example, De Santis and Zaghini (2019), Zaghini (2019), Grosse-Rueschkamp et al. (2019), Todorov (2020), Rischen and Theissen (2017), Abidi and Miquel-Flores (2018).

To conduct the study, I construct a novel data set merging an industry data with detailed characteristics on bond issuance with Compustat. The combined dataset includes information on order book size, new issue concessions, and fulfillment time for 4,629 bonds issued by 833 issuers through 87 bank underwriters. I construct a firm-level exposure metric to the Euro-zone using firm-bank underwriting relationships and bank-level investor relationships. I find that, following the start of the ECB's bond buying program, firms that have relationships with banks that are more exposed to the Euro area receive more orders for their bonds, have lower costs of capital, and issue more bonds. I show that this is not related to firm fundamentals or geography. Moreover, I find that riskier firms and less frequent bond issuers in particular raised more bond capital in response to the positive credit supply shock. The magnitude of the impact on U.S. firms can be considered a lower bound for the overall spillover effect of ECB corporate bond purchases on the U.S., because the analysis differences out any overall impact that affects all firms similarly.

Next, I investigate the long-term consequences of the unconventional monetary policy on a firm's debt and real investment. I find evidence that firms that are more exposed to the ECB's bond buying program pay out more to equity holders in lieu of making real investments. Treated firms do not increase operational assets (using non-cash assets as a proxy) as much as they do following normal bond issuances. In addition, I find that firm leverage ratios remain elevated up to two years after bond issuance.

My empirical methodology relies on two key assumptions: (i) banking relationships with both firms and bond investors are persistent and (ii) the cross-sectional variation in US firms' bank exposure to Europe is orthogonal to the cross-sectional variation in US firms' operational exposure to Europe. I verify that these two assumptions are true in the data. Indeed, I find that (i) firms (banks) tend to choose banks (investors) with whom they have

existing relationships, and this tendency did not change in response to or in anticipation of the ECB's quantitative easing program, and (ii) firms that work with more European banks do not necessarily have more operational exposure to Europe. Hence, the channel that I identify has to do solely with bank networks, rather than direct effects of ECB policy on U.S. firms.

To clarify the identification logic, consider the example of Barclays and Wells Fargo, two large bank underwriters active in the U.S. corporate bond market. Barclays, as a bank that conducts a significant proportion of its business in Europe, has more institutional investor relationships in Europe relative to the rest of the world. Specifically, 75% of Barclays' institutional relationship network is in the Euro-zone. In contrast, 0% of Wells Fargo's institutional relationship network is in Europe. After the implementation of the ECB's bond buying program, prices of Euro-area securities are driven up and institutional investors seek out bonds of a similar risk profile in the U.S. market. Because bank-investor relationships are persistent, European investors participate in bonds underwritten by Barclays, but not by Wells Fargo. As a result, firms that work with Barclays receive a larger order book for their bonds and a lower cost of capital after the start of the ECB program. Moreover, they also issue more debt in the quarters following the program. My estimates suggest that moving from the 25th to the 75th percentile of exposure to the Euro-zone through the bank network reduces annual cost of capital by 3 basis points and increases a firm's issuance volume by nearly $85 million per quarter.

To check that the chain of causality is indeed from the ECB's bond buying program to firm issuance outcomes, I control for firm-specific demand by including firm fixed effects in my main specification.[4] Next, I control for potential secular changes in firm-specific real

[4]This is akin to the empirical strategies in Khwaja and Mian (2008), Schnabl (2012),Paravisini et al. (2015)

demand for bonds by running a within-firm-year regression. I want to ensure that my results reflect movement *along* the firm's demand curve for bond capital, rather than reflecting a simultaneous *shift outward* of the firm's demand curve due to changes in real investment opportunities that coincide with the ECB's policies. I find that my baseline results hold. This test supports the notion that the ECB's QE program impacted U.S. firms via the bank underwriting network, lowering their costs of capital and encouraging increased issuance.

Next, I discuss other potential mechanisms of monetary actions spilling over to U.S. firms, including the ECB's 2015 government and agency bond purchases, as well as the Federal Reserve's third round of quantitative easing (QE3). I provide evidence that the heterogeneous impacts on issuance volumes and costs of capital that I identify across U.S. firms is caused primarily by the ECB's corporate bond purchase program, rather than the government bond purchase programs that occurred around the same time. While firms may have been directly impacted by the Eurozone crisis or the concurrent fiscal measures, the heterogeneity of bond issuance outcomes across the cross section of U.S. firms can be attributed to the corporate bond buying program.

I run a series of robustness checks to ensure that results are not being driven by endogenous firm-bank choice. Firms working with banks that are more exposed to Europe may have more operational exposure to European banks. To deal with this potential confounder, I construct a measure of the extent of Euro-zone operations that each corporate issuer has by scraping the text in their company filings. I find that cross-sectional variation in the measure of operational exposure to Europe has no impact on firms' bond issuance decisions after the start of the ECB's QE program. Further, I find no evidence that the metric of operational exposure is correlated with a firm's exposure through its bank network. The robustness checks confirm the hypothesis that shocks can be transmitted through bank

underwriting networks, providing an additional channel for monetary policy transmission outside of traditional mechanisms.

Why might demand shocks transmit globally through bank underwriting networks? While it is well established that demand shocks transmit through bank networks via a balance sheet channel,[5] other mechanisms through which banks connect global markets are less clear. The observed heterogeneous treatment across firms in primary bond markets suggests partial market segmentation. In the corporate bond underwriting market, banks act as match makers between buyers and sellers. When underwriting bonds, banks incur costs in search for buyers of securities. Because banks and investors are repeat agents in this market, banks can reduce long-term search costs by maintaining relationships with the same investors. As such, demand shocks transmit through bank underwriting networks. I conduct a series of empirical tests that show results consistent with such a mechanism. Using a novel metric of time spent on issuance to proxy for search costs, I find evidence that banks do incur costs when searching beyond their typical network. This suggests that banks derive value from forming long-term relationships with investors.

My paper contributes to the literature on direct and indirect effects of unconventional monetary policy on the real economy. Many empirical studies identify direct effects of the ECB's corporate bond buying on firms. Broadly, they find that the CSPP lowered issuance costs for eligible firms (Pegoraro and Montagna (2019), Rischen and Theissen (2017)), encouraging them to raise more bond capital (De Santis and Zaghini (2019)). Moreover, the benefits of lower issuance costs spilled over to ineligible Euro-area firms (Abidi and Miquel-Flores (2018) and Zaghini (2019)). Eligible issuers use the funds to substitute away from bank loans (Grosse-Rueschkamp et al. (2019), Arce et al. (2018) and Galema and Lugo

[5]See, for example, Peek and Rosengren (1997), Kashyap and Stein (2000), Khwaja and Mian (2008), and Chodorow-Reich (2014b)

(2019)) or increase dividends rather than fund real investment (Todorov (2020)). Recent papers (Boyarchenko et al. (2020), Halling et al. (2020), Gilchrist et al. (2020), Flanagan and Purnanandam (2020)) analyze the announcement effects of the Federal Reserve's bond buying program, including higher issuance, lower yields and relaxed funding constraints. Moreover, Ma et al. (2020), Kargar et al. (2020) and Falato et al. (2020) document that the Fed's March 2020 announcements alleviated the significant stresses on bond fund outflows and secondary market liquidity. I add to this literature by focusing on issuance markets and studying the spillovers of the ECB's program to firms in the U.S.

My paper further relates to the literature on the role of banks in transmitting monetary policy. Many empirical studies (Kashyap and Stein (2000), Bernanke and Blinder (1992), Kashyap et al. (1993),Jiménez et al. (2012)) document the traditional bank lending channel of conventional monetary policy (Kashyap and Stein (1994)).[6] Additional transmission channels have been explored in recent studies of unconventional monetary policy tools. For example, Grosse-Rueschkamp et al. (2019) find evidence of a capital structure channel, where the ECB's bond-buying program makes bond issuance more attractive than bank borrowing for eligible firms, allowing banks to increase lending to other segments of the economy.[7] My study expands upon the ways in which unconventional monetary policy transmits through financial institutions.[8]

[6]See Nakamura and Steinsson (2018) for a discussion of identifying monetary policy shocks.

[7]Chakraborty et al. (2020) document an origination channel following the Federal Reserve's mortgage-backed securities (MBS) purchase program, where banks with greater exposure to the MBS purchases increased mortgage lending but decreased firm lending. Further transmission mechanisms of monetary policy are documented in Drechsler et al. (2017), Rodnyansky and Darmouni (2017), Darmouni et al. (2019); see Drechsler et al. (2018a) for a survey.

[8]My findings that firms more exposed to the bond buying program have lower costs of capital relative to unexposed firms is consistent with the framework in Drechsler et al. (2018a), which shows that by increasing overall risk taking, QE decreases the economy-wide aversion to risk. There is also evidence that banks inhibited the efficacy of the ECB's monetary policies by extending subsidized credit to distressed, less productive borrowers (Acharya et al. (2019)).Acharya et al. (2020) find evidence that the transmission mechanism may be impaired if bank balance sheets are weak. Moreover, the risk-taking channel of monetary policy (Adrian

My paper also contributes to the literature on the effect of credit supply frictions on firm financing decisions and outcomes. If financial markets are efficient, shocks to one region should not impact credit activity in other markets, holding investment opportunities constant. Many studies provide evidence to the contrary, suggesting that credit-supply frictions faced by banks can influence firm capital-raising decisions and outcomes.[9] A rich empirical literature uses natural experiments (Peek and Rosengren (1997), Khwaja and Mian (2008), Paravisini (2008), Schnabl (2012), Gilje et al. (2016), Chava and Purnanandam (2011)) and variations in bank relationships across firms (Ivashina and Scharfstein (2010), Lin and Paravisini (2013), Chodorow-Reich (2014b)) or bank balance sheets (Becker and Ivashina (2018)) to deal with potential endogenous shifts in firm demand for capital. Firms are also known to respond to supply frictions in capital markets (Saretto and Tookes (2013), Chakraborty and MacKinlay (2020), Massa et al. (2013), Faulkender and Petersen (2006), Almeida et al. (2012)). I add to this literature by exploiting the ECB's QE program as a natural experiment to identify how firms respond to positive shocks to credit supply. Moreover, my findings that firms respond to favorable credit conditions and payout to equity holders is consistent with the theoretical market timing framework of Bolton et al. (2013).

The outline of this paper is as follows. Section I discusses the institutional background on the bond primary market, introduces facts about bank relationships, and describes the ECB monetary policy program of interest. Section II describes the data used in this study, defines key variables, and gives summary statistics. Empirical strategy is outlined in Section III, and my empirical results are in Section IV. Section V has a brief discussion of results and

and Shin (2010), Drechsler et al. (2018b), and Borio and Zhu (2012)) suggests that expansionary monetary policy could encourage financial institutions to increase risk. However, Chodorow-Reich (2014a) finds unconventional monetary policy may also have helped to stabilize some financial institutions while others increased risk-taking.

[9] A related literature discusses how equity capital inflows affect equity issuance, using micro-data on firm-level issuance. See, for example, Calomiris et al. (2019).

various robustness checks. I discuss the underlying mechanism in Section VI, and I conclude and discuss next steps in Section VII.

I. Institutional Detail

A. Corporate Bond Market

The corporate bond market is an important source of capital for corporations. Corporate bond issuance is over eight times the volume of common equity issuance.[10] In 2018, 87% of that issuance was Investment Grade (rated above BBB-), and 81% of the issuance was fixed-rate (See Figure 1). Unlike equity issuance and bank lending, investment grade corporate bond issuance is subject to less information asymmetry.[11] In addition, corporate bond issuers tend to be larger, more transparent firms.[12] The median non-financial bond issuer in my sample (detailed in Section II) had $5.2 billion annual revenue and $12 billion in total assets as of 2010. By comparison, the median non-financial firm in Compustat had $31 million in annual revenue and $203 million in total assets in 2010.

Corporate bonds are often underpriced at issuance.[13] Cai et al. (2007) document an average of 47 basis points of underpricing for high-yield bond initial price offerings (IPOs), as measured by the initial excess return of a given bond relative to a benchmark. Consistent with the paper's finding that underpricing is higher for riskier bonds, in my sample of safer,

[10]SIFMA 2018: Common Stock issuance (including IPOs and follow-ons issued in the US) was $199.3 bn in 2017; Corporate Debt issuance (including public and private, IG and high yield bonds issued in the US) was $1.637 trillion

[11]See Myers and Majluf (1984): bond issuance is higher in the "pecking order" than equity issuance due to less information sensitivity

[12]See Diamond (1991): in the presence of moral hazard, borrowers start out by being monitored by banks, and graduate from bank loans to bond issuance once they have acquired better reputations

[13]Goldstein and Hotchkiss (2020) find evidence that while initial underpricing is small, prices continue to rise in the two weeks following issuance.

investment grade firms, underpricing per bond is on average 29 basis points by their metric.[14] Rationales behind underpricing typically focus on information asymmetries: for example, issuance signals bad firm performance (Myers and Majluf (1984)), informed investors are compensated for sharing valuation information with the underwriter (Benveniste and Spindt (1989)), uninformed investors are compensated for the expectation that they will overpay (Rock (1986)).

In a typical corporate bond deal, a firm hires a median of four banks to underwrite the planned bond before the intended issuance date. The bond underwriting market is relatively concentrated.[15] There are 87 active bank underwriters in my sample (i.e., banks that underwrote at least three deals before and after June 2016). The top five most active banks participate (Bank of America, Citi, JP Morgan, Morgan Stanley, and Goldman Sachs) in 89% of the issuer-days in my sample. As a result, the bank-level variation in my sample mostly comes from smaller banks that likely have more limited and distinct sets of investor relationships. For banks that underwrite bonds, bond underwriting is a material portion of their business. According to a trade report by SIFMA, between 19-29% of investment bank revenues arise from debt underwriting and related activities.[16]

The primary market for corporate bonds has a unique feature that provides exclusivity to underwriters for selling new bonds. That is, the only way in which institutional investors can purchase newly issued bonds when firms raise bond capital is through the underwriters chosen by the firm. In the investment grade market, the primary services that the bank

[14] In Cai et al. (2007), underpricing is measured as the return within a 7-day window following issuance, or $(P_{t+n} - P_t)/P_t$, relative to a comparable index of bonds in the same ratings and maturity. In translating the average 3.4 basis point annual new issue concession (see Table II) to the Cai et al. (2007) metric, I assume a 10-year bond issued with a coupon of 3.485% issued at par, which trades to a yield of 3.451% in the first day following issuance. I also assume zero return on the market index.

[15] See Manconi et al. (2018) for a discussion on underwriter competition and how it impacts bond pricing

[16] Source: SIFMA 2020, page 88: `https://www.sifma.org/wp-content/uploads/2020/09/US-Fact-Book-2020-SIFMA.pdf`

provides are: (1) searching for investors, collecting orders, setting pricing, and allocating the security and (2) ensuring post-issuance price stability.[17] Banks incur costs in the initial assessment of a firm's management, operations and creditworthiness. If a bank has already worked with a firm, the marginal cost of marketing and selling the firm's securities is lower. James (1992) calls the setup of a bank-firm relationship as investing in "durable transactor-specific assets", rationalizing persistent bank-firm linkages.[18] In the next section, I directly test the supposition of bank-firm relationships.

B. Bank relationships

The methodological approach of the paper relies on (1) persistent bank relationships with both investors and issuers, and (2) the constancy of these relationships throughout the European Central Bank's monetary policy program. Absent sticky relationships, U.S. firms would not have differential exposure to the ECB's monetary policy, and I would not be able to identify heterogeneous outcomes from the bond buying program. The empirical literature on corporate-bond bank-firm relationships shows evidence of persistent firm-bank underwriting relationships (Chakraborty and MacKinlay (2020), Yasuda (2005), Daetz et al. (2018)). The key to my identification strategy is that the stickiness does not decline following the CSPP program.

The empirical literature on investor-bank relationships in the bond primary markets is less developed.[19] While in equity underwriting, investor-bank relationships may be persis-

[17] Yasuda (2005) discusses another service that underwriting banks provide: insurance for unsold securities. However, this is more likely to be the case for non-investment grade issues.

[18] It is also well established in the literature that firm-bank relationships in the bank loan market are sticky; see Darmouni (2019) or Schwert (2018) for recent discussions. Theories behind firm-bank lender persistence include market participants trying to avoid adverse selection (Sharpe (1990)), costly information acquisition (Sufi (2007)), moral hazard (Holmstrom and Tirole (1997)), and costly monitoring (Williamson (1987)).

[19] See Hendershott et al. (2020) for evidence of persistent investor-bank relationships in secondary corpo-

tent due to information asymmetries (see Benveniste and Spindt (1989) and Cornelli and Goldreich (2001)), in investment grade markets, information asymmetries are likely less critical. Instead, bank-investor relationships may persist due to search frictions (see Duffie et al. (2007) and Henderson and Tookes (2012)) or profit-sharing with investors active in secondary market trading (see Nikolova et al. (2020) and Loughran and Ritter (2002)). Consistent with this literature, I first present evidence of persistence in bank-firm and bank-investor links. I then show that there is exclusivity among both sets of relationships.

First, I compare bank relationships prior to the start of the CSPP to those after the start of the CSPP. See Table I for the results. I find that firms typically choose bank underwriters with whom they have worked in the past. On average, 87% of banks that firms choose are from pre-existing relationships. Importantly, this is true in both the pre- and post-CSPP periods. Moreover, in only 1% of cases do firms choose entirely new banks to underwrite a bond. This suggests that bank-firm relationships are relatively persistent.

Bank-investor relationships are not fully disclosed. However, I can identify trades of insurance investors from the National Association of Insurance Commissioners (NAIC) regulatory filings, which constitute on average 11% of the buyers in the primary market for investment grade bonds in my sample period. I follow Nikolova et al. (2020) and identify primary market investors as those that buy a bond on the issuance date at the initial offer price and from one of the bond's initial underwriters. I find that, similar to bank-firm relationships, bank-investor relationships also tend to be sticky. On average, 90% of identified sales volume is attributed to investors with whom banks have existing relationships, and this is the same both pre- and post-CSPP. There is a very low proportion of days (3.5%) in which banks sell to a new set investors, and this does not change after the CSPP program

rate bond markets. The literature on investor-bank relationships is extensive in the equity IPO literature; see Binay et al. (2007) for a survey

begins.

The results suggest that in the two-year period before and after the implementation of the CSPP, bank-firm and bank-investor relationships are persistent. Firms tend to work with banks with whom they have existing relationships. Similarly, banks tend to work with investors with whom they have existing relationships.[20] Firms do not appear to be creating many more new links with banks following the CSPP, nor are banks creating many more new links with investors. This finding allows me to interpret the underlying bank-firm-investor network as a mechanism through which foreign demand shocks can heterogeneously impact U.S. firms.

Next, I show evidence that these relationships are, for the most part, exclusive. Absent some degree of exclusivity, banks could link with all firms and all investors, thus invalidating my identification strategy. I plot histograms of the number of banks with whom an agent has a relationship from 2000-2018. If there were no relationship exclusivity in firm-bank links, then I would expect to see the full mass of firms at the maximum number of banks. Similarly, if there were no exclusivity in investor-bank links, then I should see all investors linked with all banks. Instead, what I find in Figure 2 is that most firms hire fewer than 10 bank underwriters, and most investors buy from fewer than 10 banks. This suggests that there is some degree of exclusivity to bank relationships.

Given these two pieces of evidence on bank relationships, I posit that there is a persistent firm-bank-investor relationship network where firms have heterogeneous exposure to banks, and banks have heterogeneous exposure to investors. These two layers of heterogeneity are reasonably persistent. This allows me to trace demand shocks from the ECB's monetary

[20]Note that in order to extrapolate from the insurance buyers' data, I make the assumption that bank-investor relationships among insurance investors do not differ significantly from bank-investor relationships among other classes of investors.

policy through the network.

C. ECB Quantitative Easing Program

I focus on the European Central Bank's (ECB) implementation of the Corporate Sector Purchase Program (CSPP) between June 8, 2016 and December 19, 2018 as an extension of its expansionary monetary policy efforts. The CSPP was part of a broader ECB asset purchase program (APP) that was designed to stimulate the economy in an environment where key interest rates were hitting their lower bound. The APP included 4 programs introduced between 2014-2016: the Third Covered Bond Purchase Programme (CBPP3), the Asset-backed Securities Purchase Programme (ABSPP), the Public Sector Purchase Programme (PSPP), and the CSPP. The decision to add corporate bonds to the assets purchased was released on March 10, 2016, with the stated goal to "further strengthen the pass-through of the Eurosystem's asset purchases to the financing conditions of the real economy."[21]

To implement the CSPP, the ECB coordinated purchases that were carried out by six Eurosystem national central banks. Eligible bonds included Euro-denominated bonds issued by Euro-area non-bank corporations with remaining maturity of six months to 30 years. Because bonds purchased needed to be eligible as collateral for Eurosystem credit operations, only investment grade securities were included in the program. The ECB deliberately targeted a wide range of ratings, sectors, countries, and issuers in order to remain market-neutral. CSPP purchases were conducted proportionally to the market value of eligible bonds.

Following the start of the CSPP, yields for both eligible and non-eligible bonds in the Euro-area dropped.[22] For the set of investment grade bond issuers in my sample, I plot the

[21] ECB Economic Bulletin Issue No. 5/2016 (August 2016)

[22] See Grosse-Rueschkamp et al. (2019) or Todorov (2020) for a discussion of effects on Euro zone markets.

average weekly yield on new bonds in Figure 3. I find, consistent with the literature, that there is a significant drop in new issue yields for Euro-denominated corporate bonds, and a similarly timed but smaller drop in yields for USD-denominated corporate bonds.

In response, institutional investors in the Euro-area invested more in USD denominated corporate bonds.[23] In 2016, Euro-area investors holdings of USD non-financial corporate debt securities issued by U.S. firms increased by 17 percent, with the bulk of the increase occurring in the second quarter following the start of bond purchases. Meanwhile, total U.S. corporate bonds outstanding increased by only 5%. Euro-area holdings of U.S. debt securities continued to rise in 2017 at 15%, compared to an increase in U.S. corporate bonds outstanding of 4%.[24] Indeed, this is consistent with Becker and Ivashina (2015), who find that conditional on credit ratings, investors are biased toward higher yielding bonds. In this setting, the ECB's policy drove down the yields of Euro corporate bonds, thus making U.S. investment grade corporate bonds relatively more attractive.

The pattern of growth in aggregate Euro-area holdings of USD non-financial corporate bond securities, plotted in Figure 4, is consistent with this story. There is a spike in the growth of Euro-area holdings of U.S. non-financial corporate bonds that corresponds with the start of the ECB's purchases in June 2016, as Euro-area investors began acquiring U.S. corporate bonds in larger quantities.[25] The primary way in which European investors can

[23] In September 2016, the bond fund PIMCO published a report advising European investors to cross the Atlantic, stating "the U.S. corporate bond market still remains the most attractive credit market, even after adjusting for currency hedging costs". Source: Kiesel and Dragesic, 2016. "U.S. Corporates: Crossing the Atlantic to Find Value." PIMCO

[24] Overall foreign holdings of U.S. corporate bonds (including ABS and MBS) increased by 11% in 2016. Sources: ECB Securities Holdings Statistics Warehouse, SIFMA 2020 Capital Markets Factbook.

[25] Note that it is not necessary for European issuers to be directly comparable to U.S. bond issuers. Rather, the key is there is some level of substitutability for European investors between U.S. investment grade bonds and European investment grade bonds. Indeed, Carey and Nini (2007) find that lenders cross borders more readily than borrowers in the syndicated loan market. Moreover, Berg et al. (2017) find that systematic pricing differences between U.S. and European syndicated loans can be partially explained by participation of institutional investors.

access U.S. corporate bonds in primary markets is through their existing bank underwriting relationships. Thus, banks that already had existing relationships with Euro-zone investors would have access to this surge in demand for investment grade securities. U.S. firms that had relationships with these banks would benefit from greater demand for their new bonds.

II. Data

A key innovation in this paper is the use of a new industry dataset, collected by Informa Global Markets, that has bond-level issuance data starting in 2000. For this paper, I use detailed bond-level data on all US dollar corporate bond issuance that the data aggregator collected from surveying bank underwriters on a daily basis. New variables to the literature include the order book size, oversubscription and new issue discounts of each bond issuance. The dataset also provides the underwriters on each bond deal, descriptions on how the bond was received by the primary market, and bond characteristics (including ratings, tenor, size, coupon, initial yield, and price). I restrict the sample to September 2010 - June 2018. September 2010 is when the variables discussed are consistently collected for all bond issuances. I further restrict the sample to US dollar denominated corporate bonds issued in the investment grade market.

To obtain borrower firm characteristics, I merge the bond-level data with Compustat. Because there is no common firm identifier between my dataset and Compustat, I do a combination of fuzzy string merging and manual matches. Issuers that cannot be matched to Compustat are either private, foreign, or sovereign or supra-sovereign entities. For primary market buyer information, I use the National Association of Insurance Commissioners (NAIC) Schedule D regulatory filings. Following Nikolova et al. (2020), I identify all primary

market bond buying as purchases on the bond's issuance date, at the issuance price, and from one of the underwriters.

My identification strategy relies on the bank-level exposure to the European institutional investor market. To get to this information, I look at the 2010 10-K SEC filing (or international equivalent) for all 87 bank underwriters that underwrote at least three deals before and after June 2016 (the start of the corporate bond buying program). To get at the exposure of a bank's sales and trading business to the Euro-zone, I compute the ratio of "institutional securities revenues" that are earned in the Euro-zone, and call that "euro-exposure".

The new data provides several variables of interest. One outcome variable of interest is the oversubscription of a bond issue, measured as the ratio of that order book to the amount issued. I use this metric to test if firms exposed to the Eurozone received more orders for their bonds. A second outcome of interest used to test changes in cost of bond capital is the new issue concession, or the difference between the yield to maturity in the primary market and contemporaneous secondary market trading for securities of similar tenor, covenants and seniority that are issued by the same firm. This is analogous to the security's underpricing at issuance.[26] The secondary market for corporate bonds is typically not very liquid, making underpricing inherently difficult to measure. The data aggregator overcomes this hurdle by identifying similar securities to the newly issued bond and collecting both trader quotes and trades prior to the announcement of a new bond. A third new variable is the number of hours it takes to underwrite a bond. To uncover this metric for each bond, I write a code that scrapes the news headlines in Informa's website for both the announcement and pricing of every bond issuance, and compute the difference in hours.

[26]See Goldstein et al. (2019), Nagler and Ottonello (2019) for a discussion of underpricing in new bonds; Benveniste and Spindt (1989), Booth and Chua (1996), Ellul and Pagano (2006) on securities in general. See Ljungqvist (2007) for a survey on IPO underpricing. Note that in some markets, corporate bonds can be overpriced; see Ding et al. (2020) for a discussion.

In my sample, I include non-financial, non-sovereign issuers that I can match to Compustat. To focus purely on the effect of the demand shock through bank underwriting networks, I further exclude firms domiciled in the Euro-zone. I include only the bonds for which the primary variables of interest, oversubscription and new issue concessions, are reported by the data aggregator, which reduces my sample by 24%. The final sample for my baseline analysis consists of 4,629 bonds issued in 2,613 unique transactions by 833 non-European issuers. The median bond in my 2010-2018 sample is a 10-year bond, $500 million in size, rated BBB+/A-, with a coupon of 3.5%. It is underwritten in one day by 4 banks, is over 3x oversubscribed, and takes over 6 hours to price after announcement. The average bond has 3.4 basis points of underpricing, which for the median $500 million 10-year bond, represents an additional $1.7 million in interest cost on an undiscounted basis. See Table II for summary statistics.

III. Empirical Strategy

The purpose of the primary empirical analysis is to understand the causal effect of the ECB's bond buying program on U.S. firms. Thus, while there may be aggregate effects of ECB's QE program on the U.S. capital markets through secondary market trading and fund flows, I focus exclusively on the primary market for corporate bonds, where European investors can only participate via the bank underwriters hired by U.S. corporate issuers. This way, I can identify causality by exploiting the cross-sectional heterogeneity of (1) firms' underwriting relationships in bond issuance markets and of (2) underwriters' investor relationships. I argue that a firm's exposure to the Eurozone through this network of relationships is orthogonal to its demand for capital. Note that I difference out any aggregate effects, so

my results likely underestimate the total effect of the ECB's policies on U.S. firms.

I use the firms' exposure to the Eurozone through their bank underwriting networks as the continuous treatment variable for a series of difference-in-differences regressions. I then run two sets of diff-in-diff analyses: first, I condition on bond issuance and compare issuance outcomes; second, I run unconditional regressions that compare the extensive margin of borrowing across the full universe of firms that issue bonds in my sample. In the first set of analyses, the "control" group consists of firms that issue bonds in the sample period that have low exposure to the Eurozone via their bank underwriting networks, while the "treatment" group consists of bond issuers that have high exposure to the Eurozone via their bank underwriting networks. By conditioning on actual issuance, these regressions focus only on the subset of firms that have demonstrated demand for capital (similar to the logic in Becker and Ivashina (2014)), allowing me to identify effects of the shock to supply of capital. Outcome variables include oversubscription and new issue concessions. By subtracting out the secondary market yield, the new issue concession controls for aggregate credit market fluctuations and firm-specific shocks in credit risk and probability of default.

In the second set of analyses, my aim is to identify how firms respond to changes in observed issuance outcomes. To do so, I aggregate the data to the firm-quarter level, and include observations unconditional on issuance. For these anlyses, the "control" group consists of firms that are bond issuers at any point in the sample period that have low exposure to the Eurozone via their bank underwriting networks, while the "treatment" group consists of firms that are bond issuers in the sample period that have high exposure to the Eurozone via their bank underwriting networks.

Finally, I identify long-term effects of increased issuance resulting from the bond buying program. To do this, I first identify the set of U.S. firms who issue more as a result of greater

exposure to the ECB's CSPP by running a simple predictive model of bond issuance and computing realized residuals. I classify "treated" firm-quarters as firms that issue more than predicted by the firms' past issuance and characteristics, conditional on being more exposed to the CSPP. I run an event study analysis on various balance sheet characteristics and plot the differing patterns in post-issuance investment activity for firms issuing as a response to central bank bond-buying demand vs. firms issuing on their own accord.

I construct the treatment variable as follows. To identify the bank network channel, I need to quantify how exposed each firm is to the Eurozone via its bank underwriting relationships. First, I estimate how exposed each *bank underwriter* is to the Eurozone via its institutional investor relationships. In the ideal data scenario, I would observe all bank-investor relationships, and directly compute each underwriter's exposure to the Euro-zone by the proportion of existing investor relationships. I would then compute firm exposure as the weighted average of this proportion across all banks hired by the firm over a set time period.

Unfortunately, I do not observe the identities of European investors in the primary market. To overcome this empirical challenge, I construct a proxy for a bank's exposure to the Eurozone institutional investor base. From interviews with industry participants, I find that selling primary market securities to investors is heavily relationships based.[27] Thus, in order to sell to Eurozone investors in the primary market, underwriters typically must have some physical presence in the Eurozone. Specifically, underwriters must have an office in the Eurozone that conduct institutional securities business that would require connecting with local institutional investors. Following this notion, I construct a metric for how much of a physical presence each underwriter has in Institutional Securities (IS) business in the

[27] See also Nikolova et al. (2020)

Eurozone. Bank-level exposure is measured as follows:

$$Eurexp_u = \frac{euro\_ISrev_{2010}}{total\_ISrev_{2010}} \tag{1}$$

I collect the data for both numerator and denominator from 2010 bank company filings. The idea is to find out what proportion of a bank's investor relationships is located in the Euro-zone. The denominator is the bank's revenues from Institutional Securities: that is, any business associated with the trading or underwriting of securities.[28] The numerator is the bank's revenues from Institutional Securities made in the Eurozone. I exclude M&A advisory, commercial lending, mortgages, and other lines of business not directly related to building relationships with local institutional investors. I collect this metric for all banks that underwrite bonds at least three times pre- and post-QE from 10-Ks, 20-Fs, annual reports, or investor presentations. The average bank-level metric of exposure to the Eurozone is 18%, with a standard deviation of 27%. Bank-level exposure ranges from 0% to 97%, with a median of 4%.

To get to firm-level *Eurexp*, I compute a weighted average of the firm's bank relationships based on all firm-bank underwriting interactions between 2009-2011.[29] I include only underwriters listed as active lead banks, to ensure that lower-tiered underwriters that do not participate in the bond allocation process, such as passive underwriters and co-managers, are not taken into account. I first compute the average exposure across all underwriters for a given bond to compute a bond-specific Euro-exposure. Then, I compute the average

[28] Only 87% of banks in my sample report revenues segmented into geographies and business lines. In the absence of revenue numbers, I use assets for both the numerator and denominator, excluding assets associated with retail or mortgage lending (9%). In the absence of revenue *and* asset numbers, I use employee headcount (4%).

[29] Results reported are based on a firm's bank network as of 2009-2011 in order to capture the period prior to the start of the Euro-zone crisis. However, benchmark results for bond-level oversubscription and new issue concessions are robust to using underwriting relationships for 2010-2012, 2011-2013, and 2012-2014.

bond exposure across all bonds for a given firm to compute a firm-specific Euro-exposure. It is reasonable to average across all of the underwriters for a given bond because the bond allocation is split evenly across all underwriters. Across non-financial corporate issuers that are not located in the Eurozone, the average euro-exposure at the firm level is 22%, with a standard deviation of 11%. Firm-level exposure ranges from 0% to 79%. The inter-quartile range is 14%.

$$Eurexp_i = \frac{1}{N_{d,i}} \sum_{\forall d \in D_i} \frac{\sum_{\forall u \in U_{d,i}} Eurexp_u}{N_{u,d,i}} \tag{2}$$

Note that for the baseline analyses, the treatment variable is time-invariant across firms. Thus, the interpretation of $Eurexp_i$ is the firm-level exposure to the Eurozone via bank underwriting networks, holding the network fixed. This allows me to run analyses unconditional on issuance, since the exact makeup of each underwriting syndicate varies only slightly across bonds issued by the same firm. In the robustness checks, I rerun the first set of conditional regressions using a time-varying exposure metric that exploits the exact makeup of underwriting syndicates for each bond, which yields similar results to my baseline strategy.

A key identifying assumption in a difference-in-differences model is parallel pre-trends. To test this assumption, I compare the pre-trends of several key firm characteristics for firms in the highest tercile of exposure to the Eurozone vs. all other bond issuing firms from 2010-2015, and report the findings in Table III. I find no discernible difference in growth rates between the two groups for any of the characteristics. That is, I cannot reject the null hypothesis that the mean growth rates in total debt, revenue, size, and cash holdings of the two groups of firms are the same prior to the ECB's CSPP, suggesting the existence of parallel pre-trends. I also compare the levels of leverage, revenues (logged), assets (logged), and cash (logged). There is no meaningful difference between the two groups in leverage, suggesting

the riskiness of firms in the treatment and control groups is comparable. Moreover, while the high exposure group is on average slightly larger by balance sheet size, it is the difference in growth rates between the two groups that matters for the parallel trends assumption.

Another key assumption of my identification strategy is that the geographic distribution of a bank's investor network is not correlated with the geographic footprint of the same bank's corporate clients. In the next section, I run a series of robustness tests to ensure that a bank's exposure to the Eurozone is unrelated to the geographic footprint of that bank's corporate clients. These tests fulfill the necessary condition for identification: the treatment is orthogonal to unobserved characteristics that could correlate with issuance decisions.

IV. Results

In this section, I first present my results for the effect of the ECB's bond purchasing program on bond issuance outcomes for U.S. firms. Then, I describe several robustness checks. Overall, I find that the aggregate demand shock introduced by the ECB's QE program impacts US firms' capital raising decisions and outcomes heterogeneously based on their bank relationships. Conditional on issuing bonds, 'treated' firms receive more orders and have less underpricing per bond. In addition, 'treated' firms issue more bonds. Moreover, I find some evidence that treated firms payout more to equity holders in the years following the start of CSPP, while there is no discernible increase in real asset acquisition.

A. *More treated issuers receive more orders*

First, I find that treated firms had larger order books for their bonds following the start of the ECB program. Specifically, I run the following regression:

$$Y_{it} = \beta_{DID} Eurexp_i Post_t + \beta_1 Post_t + X'_{it}\gamma_i + \alpha_i + \alpha_{ind,post} + \alpha_q + \epsilon_{it} \quad (3)$$

Note that $Post_t$ is based on day-level variation. I include firm fixed effects (α_i), which absorbs the non-interacted firm-specific $Eurexp_i$, industry by post fixed effects ($\alpha_{ind,post}$), and quarter fixed effects (α_q) to absorb macro credit variation. Table IV show the first results from my main specification. The dependent variable is *Oversubscription*, a metric of the ratio of the order book size and the amount ultimately issued. The median oversubscriptiton ratio is 3.2, while the mean is 3.7 (see Table II). The statistically significant and positive coefficient on β_{DID} indicates that "treated" firms achieve bigger order books after the ECB's CSPP begins. In terms of economic magnitude, an increase from the 25th to 75th percentile of *Eurexp* at the firm level will increase the median orderbook of the median $500 million bond by $200 million (or increase the mean orderbook of the average $727 million bond by $250 million).

The richness of the dataset allows me to estimate within-firm effects, thus controlling for all time-invariant firm characteristics that could impact credit demand. Within my sample period, non-financial, non-Euro-zone corporate issuers issue on average 6 bonds. I also include quarter fixed effects to account for business cycle variations in credit supply. To absorb any significant sector-wide changes from the pre-2016 to post-2016 periods, I include industry x post fixed effects in all of my main specifications. To control for variations in investor behavior due to day-to-day changes in how busy the primary bond markets are, I

control for the total dollar amount issued in the corporate bond market on day t. To account for unobservable potential complexity of a bond issuance, I control for the number of banks underwriting the deal.

In Column (2), I add firm and deal controls. This absorbs key firm characteristics that can vary over time, such as size, revenue, and rating. It also ensures that any firm decisions to change tenor or size of the bond issuance do not bias my results. The addition of these controls increases my point estimate to $\hat{\beta}_{DID} = 2.697$. One potential source of bias in Column (1) is that an increase in the size of the bond mechanically decreases oversubscription. As I will discuss below, I find that treated firms issue more after the ECB shock, so this could have biased my coefficient downward. In specification (3), I add Leverage x Post and Size x Post controls, which absorbs any significant level changes in firm size or leverage.

B. More treated issuers have less underpricing

In Table V, I estimate the impact of the ECB QE program on the pricing of new securities. By focusing on the new issue concessions, a measure of the difference between primary and secondary market yields, I effectively control for underlying changes in firm-specific expected cash flow realizations or default probabilities. Moreover, because secondary market prices may also improve in response to bond buying, these estimates represent a lower bound for improvements in a firm's cost of capital resulting from the ECB policy.

The primary specification is the same as Table IV, as detailed in the previous section. For regressions (2)-(3), the $\hat{\beta}_{DID}$ is negative and statistically significant to the 1% level, indicating that the ECB QE program had a positive impact on "treated" firms. The magnitude of this effect is also economically significant: the estimate of $\hat{\beta}_{DID}$ for regression (3) can be interpreted as follows: if a firm moves from the 25th to the 75th percentile of the *Eurexp*

treatment variable, it will face a 3bp lower new issue concession. For the median 10-year bond of $500 million, that is over $1.2 million in additional present value coupon cost.

C. More treated firms issue more

Next, I focus on changes in the firm issuance decision as a result of treatment. Consistent with the literature on market timing (see, for example, Bolton et al. (2013), Baker and Wurgler (2002), Jenter et al. (2011)), firms more impacted by the ECB bond buying program should respond to the lower cost of bond capital by issuing more bonds. To test if this is the case, I aggregate the data up to the firm-quarter level so that I can incorporate both the intensive and extensive margin of borrowing from the bond market, and my regressions are no longer conditional on issuance. In this analysis, the "control" group consists of bond issuers with low exposure to the Eurozone via their bank underwriting networks, while the "treatment" group consists of bond issuers with high exposure to the Eurozone.

$$Y_{iq} = \beta_{DID} Eurexp_i Post_q + X'_{iq}\gamma + \alpha_i + \alpha_{ind,q} + \epsilon_{iq} \tag{4}$$

The outcome variable for regressions (1)-(2) is the amount issued in billions of USD at the firm-quarter level. In this difference-in-difference specification, "Post" refers to after Q1 2016. For all of the specifications, I find an economically significant uptick in issuance by treated firms after the ECB shock. Column (3) of Table VI includes firm fixed effects to isolate within-firm variation in issuance volume, industry-quarter fixed effects to absorb any industry specific shocks, and controls for firm revenue, size, leverage, and credit rating. I also account for any changes in firm size pre- and post-QE with total assets x post controls. I interpret the estimate $\hat{\beta}_{DID}$ as follows: if a firm moves from the 25th percentile to the

75th percentile of firm exposure, it increases its issuance volume by $85 million. This is an economically significant amount, given the corresponding change in annual cost of capital was 3 basis points.

Regression (3) is a linear probability model that includes all of the fixed effects of regression. The coefficient is positive and statistically significant at the 5% level, suggesting that treated firms have a higher probability of issuing. In regression (4), the dependent variable is the number of bonds issued. I find an economically meaningful and statistically significant positive coefficient on the number of bonds, suggesting that treated firms not only issue more in volume, but they also choose to issue more bonds.

D. Pre-trends analysis

It is possible that results are driven by pre-existing trends. For example, perhaps treated firms had already begun to issue more prior to the ECB's program, and the effect is unrelated to bank influence. To ensure that the findings are not driven by pre-existing trends, I run the Granger (1969) causality test:

$$Y_{iy} = \alpha_{ind,y} + \alpha_i + \sum_{\tau=0}^{6} \beta_{-\tau} Eurexp_i \times D_{y-\tau} + \sum_{\tau=1}^{3} \beta_{+\tau} Eurexp_i \times D_{y+\tau} + X'_{iy}\gamma + \epsilon_{iy} \qquad (5)$$

The outcome variable, Y_{iq}, is the amount issued by firm i in year y. $\alpha_{ind,y}$ are industry-quarter fixed effects to absorb time-varying industry shocks, α_i is a firm fixed effect to absorb between-firm time-invariant variation, $Treat_i$ is an indicator variable equal to one if the firm is in the top tercile of exposure to the Euro-zone and zero if the firm is in the bottom tercile, D_t is an indicator for each quarter (2015Q4 is the omitted time period), $Treat_i \times D_t$ is the interaction term between quarter dummies and the firm's exposure status, and X_{it} are

firm-level control variables including return on assets (net income divided by total assets) and size (log of total assets) to account for within-firm time-varying heterogeneity. Standard errors are clustered at the firm level to account for potential serial correlation across time.

Figure 5 plots the estimates of coefficients on the difference-in-differences term, β_t for each quarter, with the 95% confidence intervals bars. This parameter captures the difference in the respective outcome variable between firms that are most exposed to the ECB's policy and firms that have little exposure. Estimating coefficients on amount issued is challenging given the lumpiness of issuance data, but it is a reasonable first pass to understand potential pre-trends. Prior to the start of CSPP (the first dashed line), coefficients are not significantly different from zero, helping to rule out pre-trends. After the start of the ECB's program, treated firms have a higher yearly issuance volume than control firms, with a steadying increasing effect that continues into 2019, after the end of the bond buying program.

E. Effects of concurrent government QE programs

The CSPP coincided with many other programs in the Eurozone. Notably, in March 2015, the ECB began net purchases of large amounts of bonds issued by governments, agencies, and multilateral organizations under the Public Sector Purchase Programme (PSPP).[30] The first phase of the program began March 9, 2015 and ended December 19, 2018. Because the PSPP overlapped with the CSPP, it is possible that some of the impact on U.S. firms results from the government bond purchase program and not exclusively from the corporate bond purchase program. That is, as the ECB purchased government bonds, Euro-zone investors could also rebalance portfolios towards purchasing U.S. corporate bonds.

I can test this directly in the data by splitting my data and analyzing exclusively the

[30] See `https://www.ecb.europa.eu/mopo/implement/app/html/index.en.html` for more detail

period prior to the start of CSPP. Specifically, I run my baseline regression on a restricted sample period of Q1 2014 - Q1 2016, and change the "Post" dummy to March 9, 2015. By doing so, I am focusing exclusively on the heterogeneous impact of the government bond purchases on U.S. firms on the subset of bond issuance events starting four quarters prior to the start of PSPP up to four quarters following the start of PSPP. The results for changes in underpricing and oversubscription are in Table VII, and the results for amount issued by quarter are in Table VIII. I include the same controls and fixed effects as the main regressions to absorb potential confounders. I find that there is no significant difference in outcomes between firms with greater exposure to the Eurozone after the start of ECB government QE and firms with less exposure to the Eurozone. While coefficients for amount issued in Tablee VIII are positive, they are much smaller in magnitude and not statistically significant from zero. Thus, my results support the notion that the ECB's government bond purchases had a negligible effect on U.S. firms via the bank underwriting channel. Instead, heterogeneous impacts across U.S. firms from the ECB's policies can be attributed primarily to spillover effects of the corporate bond purchasing program.

In principle, other concurrent central bank programs could impact U.S. firm issuance. Importantly, the U.S. Federal Reserve was purchasing significant amounts of government bonds and mortgage-backed securities from September 2012 to October 2014 in the third wave of its quantitative easing program (QE3). These purchases likely had an impact on corporate bond markets in the U.S., and my baseline results may be picking up part of this effect. To test if this is the case, I restrict my sample to begin first quarter of 2015, after the end of the Federal Reserve's QE3 program. I find very similar results (see Table IA.1), suggesting that the increase in firm issuance I find in my data is not associated with the Fed's bond buying program.

F. What are the longer term effects of the program?

The ECB bond buying program lasted through December 2018. How did firms respond to this temporary decrease in their cost of accessing bond capital? Theory predicts that firms would respond to favorable external capital conditions by issuing more securities (Bolton et al. (2013)) and paying out to shareholders. In this section, I follow Darmouni and Siani (2020) and relate firm balance sheet characteristics up to two years following issuance during the bond buying program to explore how firms that issued more in response to the ECB policy used the proceeds relative to a control group of firms that either issued bonds pre-ECB policy or were not affected by the ECB. I find suggestive evidence that firms used the proceeds to pay down equity holders rather than increasing real investments.

Concretely, I first identify all firms that issued more than they normally would as a result of the ECB policy, and call these firms "treated". I then compare the balance sheet adjustments prior to and following bond issuance for treated and controls firms. To be defined as firms that issued more than they normally would have, I look at firms whose issuance from June 2016 - December 2018 is statistically greater than issuance June 2013 - December 2015, controlling for industry trends and firm characteristics. That is, I run a predictive model where issuance during CSPP is a function of firm issuance in 2013-2015 and a vector of 2015 characteristics including firm credit rating, size, return on assets, and leverage: $Y_{i,16Q2-18Q4} = f(Y_{i,2013-2015}, X_{i,2015})$. I then collect the residuals from the following regression.

$$Y_{i,16Q2-18Q4} = \sum_{m=2013}^{2015} \beta_m Y_{im} + X'_{i,2015}\gamma + \epsilon_i \tag{6}$$

Treated firm-quarters are the intersection of firms that (1) have realized residuals $\hat{\epsilon}_i$ above

the median in the cross section, (2) are issuing during the bond buying program, and (3) have exposure to the Eurozone through their bank underwriting network in the top tercile of firms. The control group includes any firm-quarters that are either in the bottom tercile of the Euro exposure metric, have $\hat{\epsilon}_i$ realizations below the median, or issued prior to the ECB's bond buying program. Note that this definition divides firm-quarters into control and treatment groups, allowing the same firm to be included in both groups. I then run the following event study regression separately for treatment and control groups:

$$Y_{iq} = \sum_{m \in [-4,8]} \beta_m B_{f,q+m} + X'_{iq}\gamma + \alpha_i + \alpha_{ind,q} + \epsilon_{iq} \tag{7}$$

The outcome variable, Y_{iq}, is the relevant quarterly balance sheet variable for firm i. $\alpha_{ind,q}$ are industry-quarter fixed effects to absorb time-varying industry shocks, α_i is a firm fixed effect to absorb between-firm time-invariant variation, $B_{f,q}$ is amount issued in quarter q by firm f (2015Q4 is the omitted time period), and I include controls for issuer credit rating and return on assets to account for time-varying within-firm heterogeneity. Standard errors are clustered at the firm level to account for potential serial correlation across time.

Figure 7 plots the estimate β_t for each quarter, with the 95% confidence interval bars marked around each point estimate. First, I find that firms that issued more as a result of the bond buying program continue to have higher leverage up to two years following issuance, similar to bond issuance in the control group. Secondly, whereas control firms pay down debt coming due in the coming year with new bond proceeds, issuers responding to the ECB's bond buying program did not have a significant decrease in current debt, suggesting that the ECB-encouraged bond issuance was not used to pay down debt coming due. Moreover, using non-cash assets as a proxy for operating assets, I find that treated firms invest less in real operations than control group issuance. Finally, firms that issued in response to the

bond buying program had a greater increase in net equity payouts than the control group in the quarters following bond issuance. This result suggests that corporate bond-buying programs, while able to spur issuance, are unlikely to lead to increases in real investment.[31]

V. Discussion and Robustness

The paper's empirical strategy identifies heterogeneous spillover effects of the ECB's CSPP across U.S. firms through the bond underwriting channel. The magnitudes recorded in the paper should be considered a lower bound for the aggregate spillover effects of central bank corporate bond purchases. There are many other avenues through which the U.S. real economy may be impacted by the ECB QE program in aggregate.

First, non-European investors may be exposed to the ECB bond purchases through their holdings of Euro corporate bonds, leading them to substitute towards comparable U.S. bonds (see Koijen et al. (2020)). This channel would impact both secondary and primary market bonds. This paper's contribution focuses on the bank underwriting network channel, which impacts exclusively primary markets that rely heavily on sticky investor-bank relationships. My analysis thus is more directly relevant for firm issuance decisions and outcomes, because the cost of capital faced by firms is determined in primary markets. A firm's exposure to the ECB through its bank underwriting network is unlikely to be correlated with the makeup of its U.S. investors' portfolios. Thus, the heterogeneous effect measured in the cross-section of U.S. firms in this paper can be considered a lower bound that would be additive to other potential impacts of the ECB's bond purchase program on U.S. firms.

Second, certain global firms that operate in both the U.S. and Europe may experience

[31] This is consistent with findings of Sharpe and Suarez (2020), who survey CFOs and find that investment is relatively insensitive to changes in interest rates.

real benefits from the ECB's stimulative response ot the Eurozone crisis, for example from improved access to trade credit (see Adelino et al. (2020)). Again, my results can be considered as a lower bound for the overall real effect of the ECB bond purchases on U.S. firms. My finding that firms exposed to the ECB's bond purchase program through bond underwriting networks did not have long-term increases in real investment further support the idea that the underwriting network channel is orthogonal to any cross sectional heterogeneity in real effects.

Finally, U.S. firms in my sample could also be directly impacted by CSPP by issuing bonds more cheaply directly in the Eurozone. While U.S. issuers would be ineligible to benefit directly from ECB purchases due to the European local eligibility requirement, many papers document the spillover effects within the Eurozone of the CSPP to ineligible borrowers (Zaghini (2019), Abidi and Miquel-Flores (2018), Arce et al. (2018)). For U.S. firms that issue both USD and Euro-denominated bonds, they may increase leverage through issuing directly into the Euro market, taking advantage of lower yields as Euro-zone investors rebalance their portfolios towards ineligible Euro-denominated bonds. If firms did indeed substitute away from USD borrowing into Euro borrowing,[32] I would expect to see an increase in the Euro-denominated bond issuance volume by U.S. firms. To check if this is the case, I consider the 119 U.S. firms in my sample that issue both Euro-denominated and USD-denominated bonds (including companies like 3M, Coca Cola, McDonald's, WalMart). I find that the proportion of bond issuance that these firms issued in Euros vs. USD is 18% in 2014, stays at 20% in 2015 and 2016, and drops to 17% in 2017. Thus, the direct effects of ECB corporate bond purchasing on the Euro-denominated issuance of U.S. firms does not appear to be economically significant.

[32]See Maggiori et al. (2020) for a discussion of large firms borrowing from foreigners

A. *Robustness: Ruling out changes in firm demand for capital*

Observed prices and quantities of debt are equilibrium outcomes. While the goal of the identification strategy is to isolate the impact of a shift in the supply of capital (demand for bonds), there is always the possibility of an unobserved latent portion of firm demand that is biasing the results. Put another way, I want to ensure that the results I observe (new bonds become more expensive and firms issue more bonds) reflects movement *along* the firm's demand curve for bonds, rather than reflecting a simultaneous *shift outward* of the firm's demand curve due to changes in real investment opportunities. While firm fixed effects in the baseline regression absorb cross sectional variation in time-invariant firm taste for bond capital, and industry-quarter fixed effects absorb industry-specific trends in demand for capital, bias could arise if firms change their demand for bond capital over time. In particular, if this change in firm demand for bond capital is correlated with the ECB's policy, then it would bias my results.

To overcome this problem, I compare the same firm's bond issuance from one set of banks relative to another set of banks in the same broader time period. By using a within-firm-time comparison, I aim to absorb firm-specific changes in demand for bond credit over time.[33] Thus, the difference in bond issuance outcomes can be directly attributed to shocks in the bank underwriting network.

Concretely, I compute firm-level $Eurexp_{it}$ for each firm again. In this iteration, I allow the exposure metric to vary across bonds within firm. The logic is the following: while firms typically have a set group of banks from which they choose each bond issuance's underwriters, each individual bond issuance may have a slightly different set of banks. Thus, a firm's exposure to the Eurozone will change from one bond to the next based on the set

[33] This is akin to the approach in Chakraborty et al. (2020).

of underwriters they choose. I use this time-varying exposure metric as the continuous treatment variable for my Diff-in-diff. I then absorb firm-year fixed effects. The identifying assumption is that a firm's demand for capital does not change significantly within one year. Thus, any changes in outcomes of bond issuance will stem entirely from the ECB's QE program heterogeneously impacting firms via different underwriters.

I estimate the following model for firm i issuing a bond on day t:

$$Y_{it} = \beta_{DID} Eurexp_{it} Post_t + \beta_1 Post_t + \beta_2 Eurexp_i + X'_{it}\gamma + \alpha_{i,year} + \alpha_{ind} + \epsilon_{it} \qquad (8)$$

Results are in Table IX. I find very similar results to my benchmark specification. That is, there is a statistically significant (at the 5% level) reduction in underpricing of bonds for firms when they are more exposed to the Eurozone via their underwriter, confirming the supposition that the results are from a shift in supply of capital rather than demand.

B. Robustness check: excluded industries

As a further test that the ECB's program indeed impacted firms, I check if U.S. industries excluded from the CSPP were impacted. Since the CSPP included only non-financial corporate issuers, I check if my baseline result holds for U.S. financial issuers. Investors substituting away from securities issued by Euro-area firms to those issued by U.S. firms are purchasers of non-financial corporate bonds, thus I should find that my baseline results do not hold for U.S. financial firms. To test this, I estimate my benchmark difference-in-differences model, with the outcome variables of oversubcription and underpricing, on the subset of non-Eurozone financial firms issuing in U.S. dollars.[34] My results are in Table

[34]For this exercise, I define a financial firm as one with a NAICS2 category of 52

X. I find there is no statistically or economically significant increase in oversubscription for financial issuers that are more exposed to the Eurozone area via their bank underwriting relationships. Moreover, there is no discernible increase in prices for these issuers. I interpret these results as further evidence that the ECB's bond buying program spilled over to the U.S. non-financial sector.

C. Robustness check: endogenous bank-firm relationships

A potential threat to identification is that the bank-firm relationship is endogenous. Firms do not select banks randomly. In the syndicated loan market, Chen and Song (2013) have found that firms and banks match by size, and Schwert (2018) find that bank-dependent firms match with well-capitalized lead arrangers.[35] It is quite plausible that banks that are more exposed to the Euro-area market match with firms that are also more exposed to the Euro-area. This would invalidate my identification strategy, because the ECB's QE program could then impact firms with Euro-area operations through a demand-side channel. For example, a U.S. firm with retail branches in the Euro-area may experience an increase in demand for its products sold in Europe following the QE program, and thus raise more debt.

To shut down this channel of firm-bank endogenous choice, I run two tests. First, I check how much the stock market returns of each firm i in my sample are correlated with the Euro-area stock market returns. The logic is the following: if firms are more exposed to the Euro-zone outside of their bank networks, then their stock returns should be more strongly correlated with Euro-market stock returns. My identification strategy relies on a firm's geographic exposure to Europe to be orthogonal to its bank-network exposure. Thus, in order for my identification strategy to hold, cross-sectional variation in co-movement with

[35] See Schwert (2018) for a recent survey of ways firms and banks match in the bank lending market

Euro-area stock market returns should not correlate with my bank-level measure of $Eurexp_i$.

Because the Euro-area stock markets is strongly correlated with the U.S. stock market index, I first run a regression of Euro-area stock market returns on the S&P index returns: $r_t^e = \beta r_t^{\$} + \epsilon_t$, and recover the residuals $\hat{\epsilon}_t$. I then find the correlation between the stock return of each of the firms in my sample with the stock market residuals over the period 2010-2016 (i.e., before the start of the CSPP): $corr_{i,t} = corr(\hat{\epsilon}_t, r_{i,t})$, and plot these against *Eurexp* in Figure 8. On the x-axis is how exposed firms are by my bank-relationship metric, from the least exposed to the most exposed, and the y-axis is the estimated $corr_{i,t}$. Each dot on the graph is one firm's correlation of stock returns with the Euro-market return residual. Firms with higher *Eurexp* do not appear to have stock returns more correlated with Euro-zone stock market returns. I consider this sufficient variation in correlation with the Euro-area market within each bucket of my bank-relationship Euro-exposure metric to make it unlikely that firms with higher *Eurexp* are systematically operating more in the Euro-zone in a way that would invalidate my identification strategy.

The second check of firm-bank endogenous choice uses financial reports to explore whether banks with more exposure to European investors also work with firms that are more exposed to the Euro-area economies. In order to quantify how exposed each firm is to the Euro-zone, I pull the 2010 10-K (or 20-F, for international firms) of each firm in my sample. I scrape the text of each file and count (1) the number of times the word "Euro", "euro", or "Europe" occurs in each text; and (2) the number of times each country in the Euro-zone is mentioned in each text, weighted by the GDP of the respective country to account for the relative importance of each country to the firm's overall exposure to the Euro-zone. I weight both metrics by the number of total words in each filing to avoid arbitrarily over-weighting longer documents. Again, I plot these metrics along an x-axis of 50 bins of *Eurexp* in Figure 9. I

find no systematic correlation between a firm having operations in Europe according to its company filings and the *Eurexp* I measure via its bank.

Next, I use the metrics constructed above to ensure that the firm's operational exposure to the Euro-zone does not drive my results. A reasonable concern is that the increase in amount issued is due to a shift in treated firms' operations. Firms that are exposed to Europe through their bank relationships may simply be exposed to Europe via their operations. To address this issue, I employ the core diff-in-diff specification using the two alternative, operations-based measures of firm-level exposure to the Euro-zone constructed above in place of my measure of *Eurexp* in the main specification. The diff-in-diff coefficients, reported in Table XI, are not statistically different from 0, and are even slightly negative. This suggests that the spillovers from the CSPP were channeled through the bank underwriting network, and not through firm operations.

VI. Mechanism

In this section, I explore potential mechanisms that drive the results. What is preventing every bank underwriter from selling to institutional investors in Europe in order to take advantage of these positive demand shocks? One potential mechanism is that primary markets are partially segmented by continent. That is, consistent with the well-established home bias of investors, firms with higher exposure to European institutional investors also would underwrite more bonds for European issuers. Under this hypothesis, banks with a higher proportion of European operations would have more personnel in the Eurozone covering both issuers and investors. When these banks work with corporate clients outside of the Eurozone, those clients get access to a greater share of institutional investors from Europe.

To test this hypothesis empirically, I plot a binscatter of bank-level exposure $Eurexp_u$ (on the x-axis) vs. the number of bonds underwritten for Eurozone corporate clients (on the y-axis). I absorb year fixed effects to control for macro trends and I control for the number of bonds underwritten by that bank-year to deal with potential bias from underwriter size. In Figure 10, I find that there is a close link between $Eurexp_u$ and the presence of underwriter u in the Eurozone corporate bond market. This suggests that underwriters that have strong institutional investor relationships in the Eurozone also have a larger proportion of their corporate issuer relationships in the Eurozone.

The partial market segmentation mechanism suggests that firms benefit from working with banks that have existing relationships with a broad network of investors. Firms that have strong brand name recognition or that issue frequently would likely benefit less from their bank's relationships, because they may have their own relationships with investors. To check if this is the case, I run the benchmark unconditional regression of issuance per quarter on the following subsets of firms: frequent and infrequent issuers, and highly rated (A and above) and lower rated (BBB+ and below) firms. See Table XII for the results. I find that the positive shock to supply of capital stemming though bank underwriting networks impacts infrequent issuers and riskier firms more than frequent issuers and safer firms. On the margin, the incremental increase in bond capital in response to bond purchases is more pronounced in riskier, less well-known firms. This suggests that firms that may be more financially constrained (as proxied by a lower credit rating) may be more susceptible to demand shocks from investors, and may thus benefit more from the spillover effects. Data limitations prevent further exploration into riskier, speculative-grade firms, but additional research on spillover effects for more financially constrained firms would be fruitful.

Another related mechanism is the following: banks may have a set of long-term investor

relationships, and could face search costs to find incremental investors, a common cost associated with dealers in the secondary market (see, for example, Duffie et al. (2005)). Since the search for incremental investors beyond the bank's usual investor base is costly, a bank has an incentive to sustain relationship capital with its existing investor relationships.

If it is the case that banks incur search costs to find incremental investors beyond their relationship investor base, I would expect (1) when a bank's existing institutional investor base puts in more orders for bonds, search costs are lower; and (2) when banks have more bonds to issue than usual, they incur greater costs to sell those bonds than on days with fewer bonds to underwrite. I use the time it takes to sell a bond, from announcement to pricing, as a proxy for search costs. To test hypothesis (1), I check if the cost of banks placing bonds for a given firm goes down with the ECB shock by running the following regression:

$$time_{uit} = \beta_{DID} Eurexp_u \times Post_t + \beta_1 \times Post_t + X'_{it}\gamma_1 + \alpha_{u,q} + \alpha_i + \epsilon_{uit} \tag{9}$$

See Table XIII for the results. I include bank-quarter fixed effects which control for time-varying bank characteristics and firm fixed effects to absorb cross-sectional variation in investor preferences across firms. This is economically significant, since the median time to issue a bond is 6.7 hours. Column (3) further controls for the total number of bonds that were issued on day t. The estimated coefficient in Column (3) indicates that moving from the 25th to the 75th percentile of bank exposure decreases the time it takes to price the bond by over 1 hour.

Next, I test hypothesis (2): when banks have more bonds to underwrite than usual, they incur greater costs to sell those bonds, controlling for bond characteristics. Indeed, I find evidence (in Table IA.3) consistent with this hypothesis. If there are more sellers than buyers on a given day, banks will take *more* time to place the bond. I interpret this finding to

mean that the relative mass of buyers and sellers for any given bank is reasonably persistent. Because I include bank-quarter fixed effects, I am holding fixed the size of the bank's investor base. The magnitude is not large: it takes an extra 8 minutes for every 10% increase in the bank's underwritten issuance on a given day; however, the coefficient statistically significant, suggesting that there is an increase in cost to the bank's placement of bonds in the primary market when there are more sellers than usual.

I interpret these results to support the hypothesis that banks incur search costs to find incremental investors beyond their typical investor base. This is consistent with a story in which banks prefer to maintain the same investor base. For the bond underwriting market as a whole, this suggests that local demand shocks can propagate through the bank-investor network to impact the issuing decisions of firms.

VII. Concluding Remarks

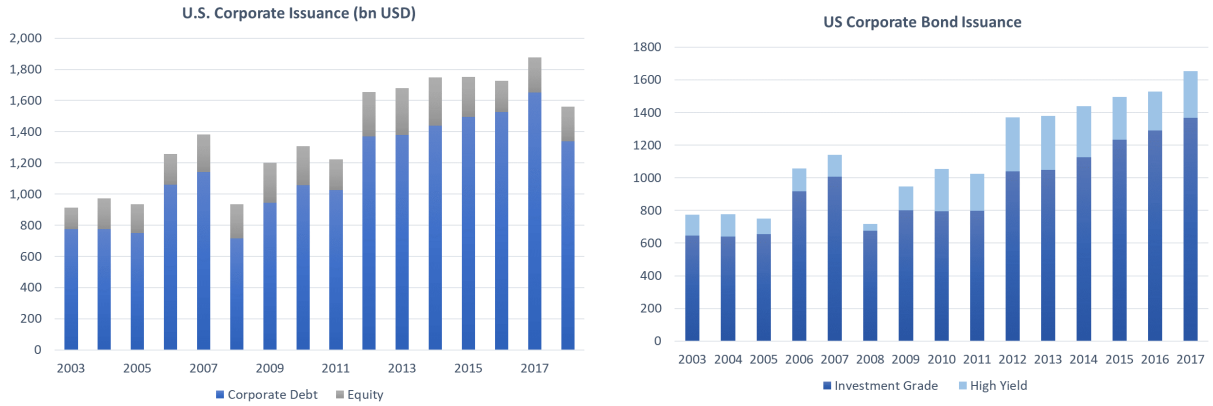
In this paper, I have identified how monetary policy-driven demand shocks propagated from the Eurozone to the U.S. corporate issuer market through the pre-existing network of firms, underwriters and investors. Using data that is novel to the literature on bond underwriting, I find that firms are differentially impacted by European demand shocks due to their bank relationships. Firms that are more "treated" have larger order books and achieve lower underpricing. Moreover, firms respond to the lower costs of capital caused by the ECB's bond buying program by issuing more bonds. Riskier firms and firms that issue less frequently end up increasing quarterly issuance by more than safer and more frequent issuers. There is evidence suggesting that treated firms pay out equity holders more than control firms in the quarters following the bond-buying program initiation, while there is

a smaller increase in real asset acquisition relative to normal bond issuance. I implement a number of robustness checks to ensure that the results do not arise from a demand-side channel or endogenous firm-bank selection.

The results raise important implications for unconventional monetary policy. Large, prolonged central bank purchases of corporate bonds can have spillover effects to other economies. As the U.S. and the Eurozone add corporate bond purchases to their unconventional monetary policy toolkit, unintended effects on other economies should be taken into consideration. In particular, the channel of portfolio rebalancing occurring through bank underwriting networks is important for primary markets, where firms raise bond capital. The ECB's bond purchase program caused a capital raising effect but no increase in real investment in U.S. firms. The magnitude of my results should be considered a lower bound for the overall spillover effects of these bond purchasing policies.

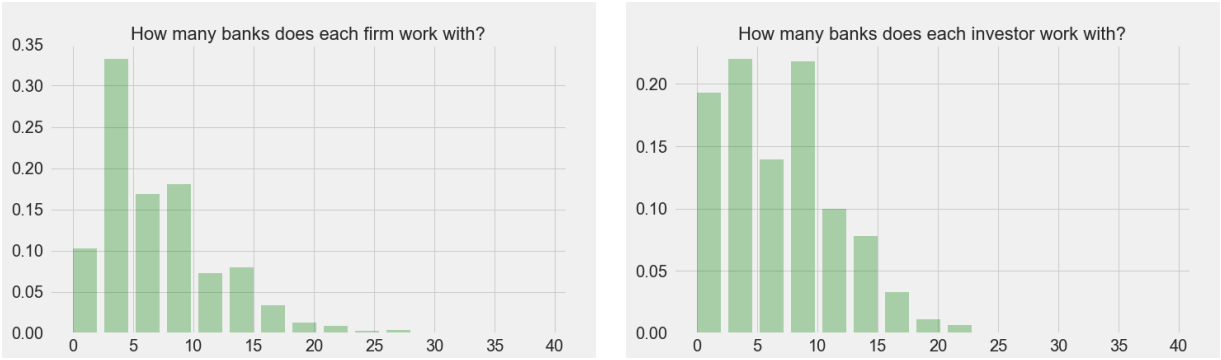
Figures and Tables

Figure 1. Total Corporate Issuance



Source: SIFMA 2018

Figure 2. Exclusivity of underwriting relationships



(a) Firm-bank underwriting relationships **(b)** Investor-bank underwriting relationships

Source: National Association of Insurance Commissioners

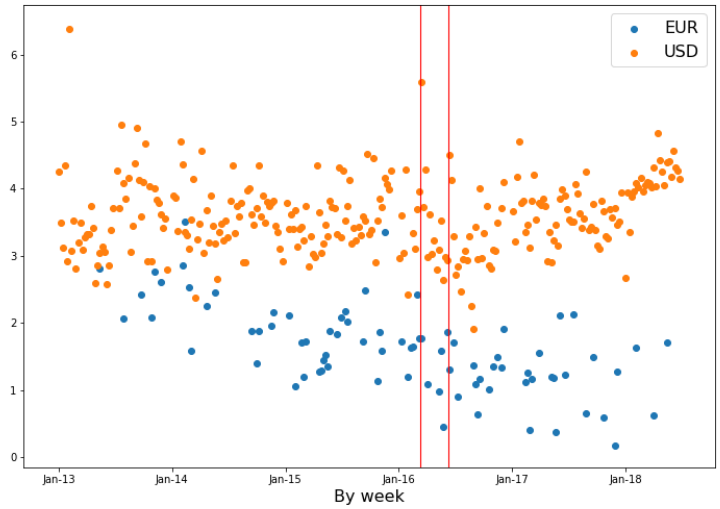


Figure 3. Offering yield on newly issued bonds

Notes: I compute the weekly averages of yields on newly issued bonds rated at least BB+ in Euros vs. in US Dollars. Vertical lines indicate the announcement (March 2016) and start (June 2016) of the ECB CSPP.
Source: Mergent FISD, IGM

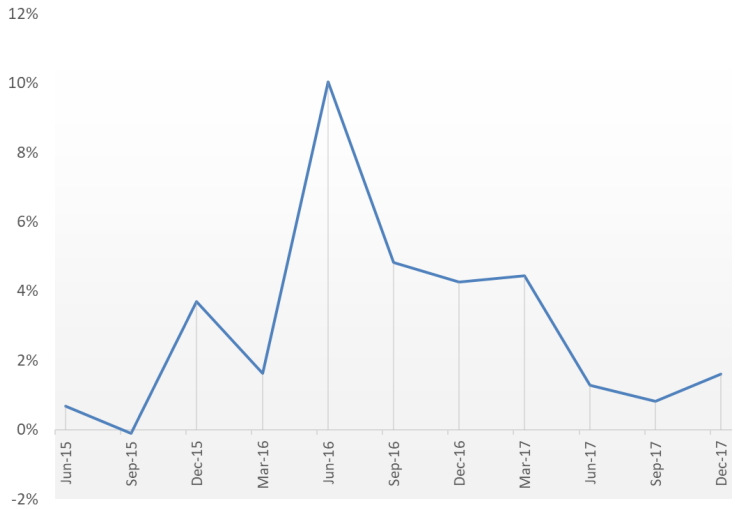


Figure 4. Growth in U.S. non-financial corporate debt securities held by Euro-area residents

Source: ECB Securities Holdings Statistics (SHS)

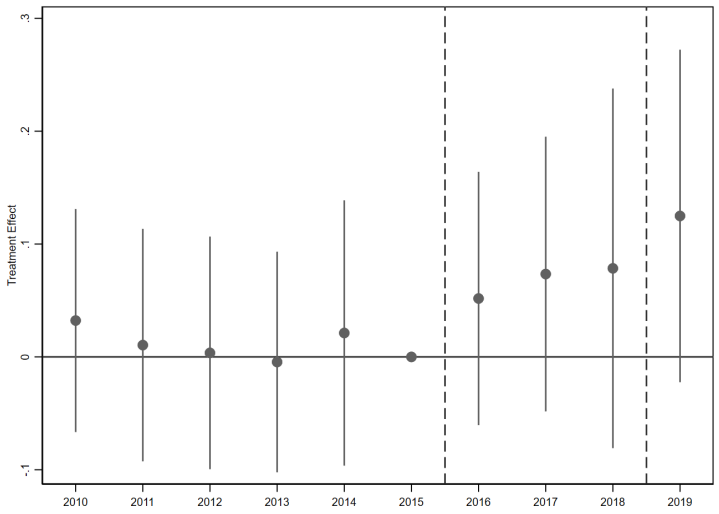
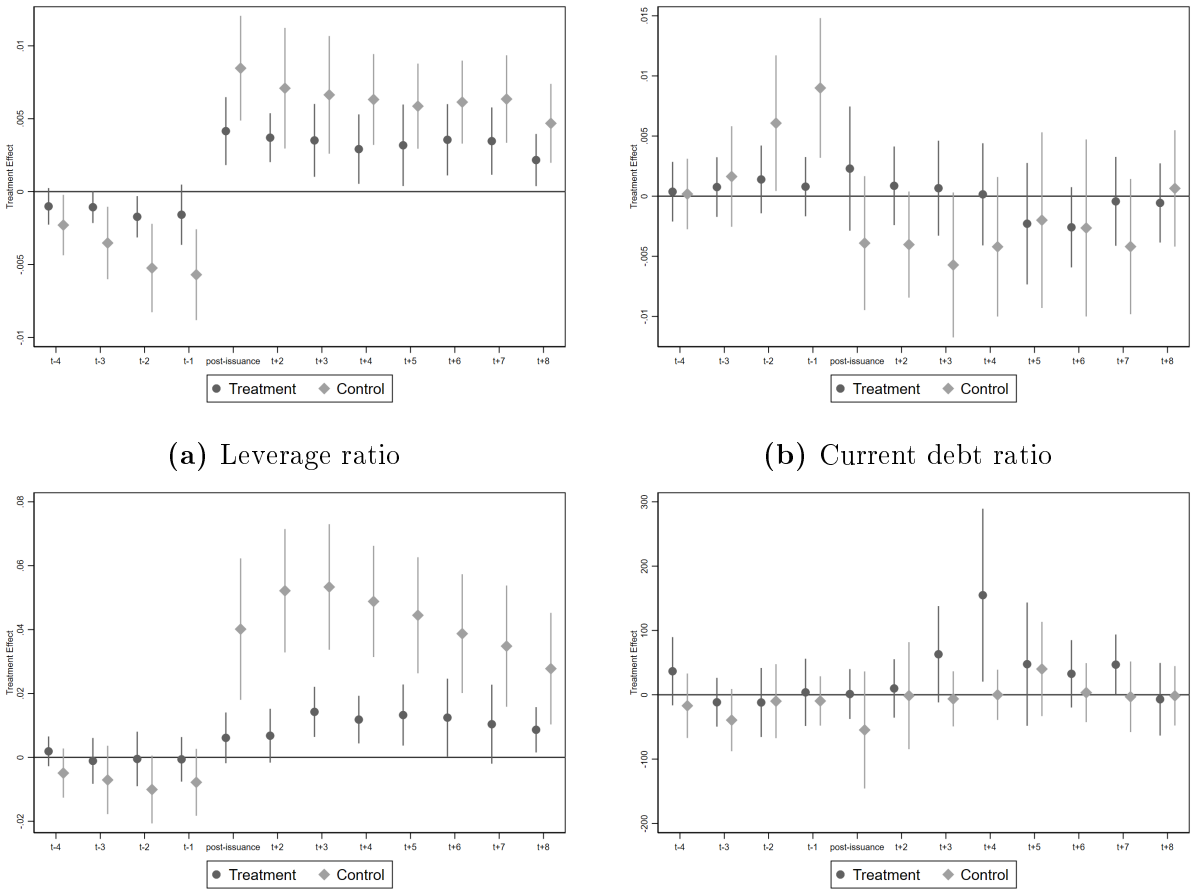


Figure 5. Yearly coefficient plot for amount issued

Notes: Excludes financial and Eurozone issuers, includes only firms reporting in US dollars. Vertical bands represent ±1.96 times the standard error of each point estimate, as per Autor (2003). Here, 2015 is omitted due to collinearity. Firm controls include issuer credit rating, log(total assets) and return on assets (net income divided by total assets). Includes firm and industry year fixed effects. Standard errors clustered by firm. Vertical dashed lines signify the rough start and end dates of the ECB's bond buying program.



(a) Leverage ratio

(b) Current debt ratio

(c) Operating assets

(d) Net equity payout

Figure 7. Coefficient plots on exposed vs. unexposed firms

Notes. Plots point estimates of coefficients corresponding to amount issued in each quarter. Observations include firm-quarters in which bond issuance occurred in the next 4 quarters or the prior 8 quarters. Vertical bands represent ± 1.96 times the standard error of each point estimate. "Treatment" includes firms that that (1) have realized residuals $\hat{\epsilon}_i$ above the median in the cross section, (2) are issuing during the bond buying program, and (3) have exposure to the Eurozone through their bank underwriting network in the top tercile of firms for the Euro exposure metric. "Control" are firm-quarters that are either in the bottom tercile of the Euro exposure metric, have $\hat{\epsilon}_i$ realizations below the median, or issued prior to the ECB's bond buying program. Outcome variables are (1) leverage ratio, as measured by total long term debt divided by total assets, (2) current debt ratio, as measured by debt due in one year divided by total long term debt outstanding, (3) log of non-cash assets (at-che), and (4) net equity payout (equity purchases minus equity issuance plus dividends). I include firm fixed effects, industry-quarter fixed effects, and control for firm quarterly profitability (ni/at) and credit rating.

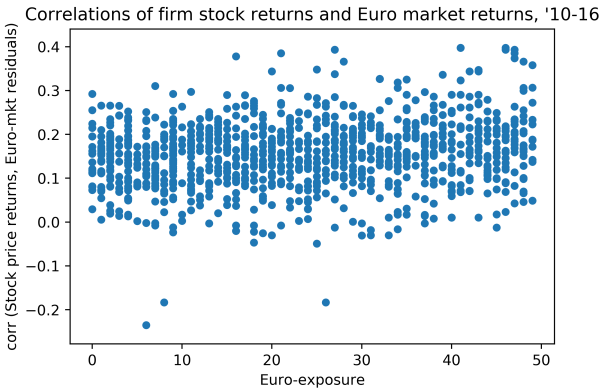


Figure 8. Correlations of firm stock returns and Euro market returns, 2010-2016

Notes. Y-axis is $corr_{i,t} = corr(\hat{\epsilon}_t, r_{i,t})$, where $\hat{\epsilon}_t$ is the residual from a regression of Euro stock market returns on US stock market returns: $r_t^e = \beta r_t^{\$} + \epsilon_t$. X-axis is the $Eurexp_u$ as defined in the text.

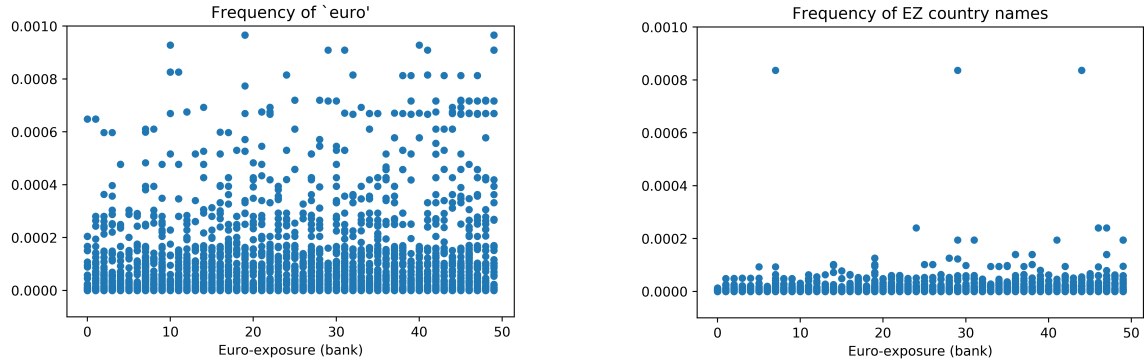


Figure 9. Frequency of Euro-zone words in SEC filing texts, 2010

Notes. Y-axis is (1) the number of times the word "Euro", "euro", or "Europe" occurs in each text, weighted by length of text; and (2) the number of times each country in the Euro-zone is mentioned in each text, weighted by the GDP of the respective country to account for the relative importance of each country to the firm's overall exposure to the Euro-zone, weighted by length of text. X-axis is the $Eurexp_u$ as defined in the text.

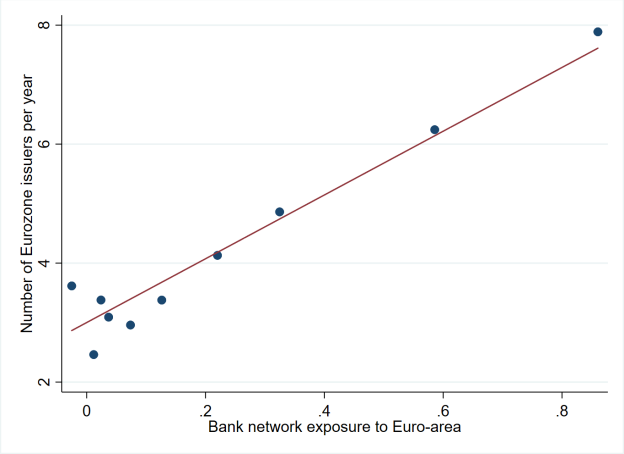


Figure 10. Correlations of $Eurexp_u$ and frequency of underwriting for Eurozone firms

Notes. Bin scatter for banks' $Eurexp_u$ metric vs. the frequency of underwriting for Eurozone corporate bonds issuances in the sample period September 2010-June 2018. I absorb year fixed effects and control for the number of total deals the bank underwrites in a given year.

Table I Bank relationships, pre- and post-CSPP

	Pre-CSPP	**Post-CSPP**	**p-value**
Firm-bank selection			
Average % of banks with existing firm relationships	86.9%	86.7%	0.781
% of deals with all new relationships	1.3%	1.4%	0.797
Bank-investor selection			
Average % of sale volume to existing relationships	90.3%	90.7%	0.687
Average % of investors with existing bank relationships	84.3%	85.4%	0.326
% of deals with all new relationships	3.6%	3.5%	0.899

Notes. I compare the two years prior to the start of ECB's CSPP to the two years after the program: "Pre-CSPP" is January 1, 2014 - December 31, 2015, while "Post-CSPP" is June 8, 2016 - June 8, 2018. To compute the *Average % of banks with existing firm relationships*, I count for each bond issuance the number of banks that each issuer chooses for which there is a pre-existing relationship, conditional on the firm having issued at least 3 times since 2000. The *% of deals with all new relationships* is the percent of bond issuances in the sample period that have zero pre-existing bank-firm relationships. *Average % of sale volume to existing relationships* is the proportion of identified primary market sales sold to existing relationships, averaged across each bond. To compute *Average % of investors with existing bank relationship*, I count for each day the number of investors that each bank chooses for which there is a pre-existing relationship, conditional on the bank having underwritten at least 3 times since 2000. The *% of deals with all new relationships* is the percent of bond issuances in the sample period that have zero pre-existing bank-investor relationships. P-values are computed using a two-sided t-test on the null hypothesis that the means for pre- and post-CSPP are the same. Because p-values are well greater than 10%, I cannot reject the null hypothesis that the means of bank-firm and bank-investor relationship persistence are the same before and during CSPP.

Table II Sample Summary Statistics

	Mean	Std Dev	10%	50%	90%
Bond characteristics					
Amount per bond (MM)	726.9	628.9	300.0	500.0	1,250.0
Credit spread (bps)	132.9	74.4	57.0	117.5	227.5
Tenor (years)	12.3	10.2	3.0	10.0	30.0
Coupon	3.485%	1.169%	1.900%	3.500%	4.900%
New Issue Concession	3.4	13.2	-9.0	2.5	16.0
Oversubscription	3.7	2.0	1.8	3.2	6.2
Credit Rating	15.7	2.1	13.0	15.5	18.5
Time (hours)	7.1	3.5	5.2	6.7	8.6
Issuer characteristics					
Amount raised per year (MM)	3,265.9	5,008.1	400.0	1,500.0	7,650.0
Number of issues per firm	6.4	4.3	2.0	6.0	13.0
Number of bonds per firm	14.4	12.8	3.0	11.0	33.0
Number of bonds per issuance	2.6	1.8	1.0	2.0	5.0
Number underwriters per bond	4.2	2.3	2.0	4.0	7.0
Firm Leverage (LT debt / Assets)	31%	14%	15%	29%	49%
Cash to Assets	10%	13%	0%	5%	23%

Source: Informa Global Markets, Compustat. See Table IA.4 for ratings information

Table III Parallel pre-trends: Treated vs. Control firms

	Exp_high=0:Mean	Exp_high=0:SD	Exp_high=1:Mean	Exp_high=1:SD	StandardDiff
Growth(Debt)	.022783	.17795	.0201434	.3037339	.010604
Growth(Revenue)	.005914	.2376398	-.0002481	.1946043	.0283722
Growth(Assets)	.0156701	.1003874	.0118045	.070981	.0444638
Growth(Cash)	-.0041961	.7293638	.0015124	.7130185	-.0079148
Leverage	.2534093	.1606761	.264257	.1422444	-.0714885
Revenues	7.728998	1.877097	8.252946	2.060455	-.2658406
Size	10.28285	2.108609	10.89028	2.29947	-.2753395
Cash	7.07548	2.870106	7.508742	3.225616	-.1419117

Notes: I compare growth in various firm characteristics of the bottom and top quartile of firms sorted by *Eurexp*. Growth in characteristic X_t is measured as $log(X_t) - log(X_{t-1})$. The mean and standard deviations are computed on firm-quarter data from 2010. Firms that issue bonds at any point in the sample are included, and firm-quarters are included unconditional on issuance in that quarter. The standardized difference is computed: $diff = \frac{\mu_1 - \mu_2}{\sqrt{\frac{\sigma_1^2 + \sigma_2^2}{2}}}$, and has the interpretation of the difference in means of the two groups in units of standard deviation, as per Austin (2009). A standardized difference of less than 10% is considered indicative that there is lack of meaningful correlation between the group definition and covariate, though there is not a consensus on the appropriate threshold (see Austin et al. (2007) for a discussion) By definition, this test statistic is not impacted by sample size. *Source*: Compustat

Table IV Increase in interest for treated firms' bonds

	(1) Oversubscription	(2) Oversubscription	(3) Oversubscription
Eurexp_i x Post_t	2.456**	2.697**	2.908**
	(1.194)	(1.221)	(1.239)
Size of bond (USD mm)		-0.193***	-0.196***
		(0.0561)	(0.0570)
Tenor of bond (years)		0.0346	0.0317
		(0.0308)	(0.0309)
Rating of bond		-0.537***	-0.493**
		(0.193)	(0.201)
Revenue that quarter		0.533	1.001
		(0.415)	(0.678)
Number of lead underwriters		-0.0875	-0.0806
		(0.0828)	(0.0817)
Other bonds issued same day		-0.0189	-0.0205
		(0.0482)	(0.0478)
Post_t×Size			-1.131
			(1.041)
Post_t×Lev			-0.121
			(0.198)
Firm FE	✓	✓	✓
Ind x Post FE	✓	✓	✓
Quarter FE	✓	✓	✓
Observations	3037	3037	3037
R-squared	0.404	0.412	0.415

Notes: Includes non-Eurozone USD non-financial corporate issuance, September 2010-June 2018. Post is after June 8, 2016. Estimation is via OLS. Controls are normalized to variance 1 and include issuer credit rating, tenor, amount issued, firm revenue, the dollar amount issued on day t other than firm i, and the number of underwriting banks. Size refers to total assets, leverage refers to the ratio of total long-term debt to total assets. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table V Decrease in underpricing for treated firms' bonds

	(1) Underpricing (bps)	(2) Underpricing (bps)	(3) Underpricing (bps)
Eurexp_i x Post_t	-20.62**	-21.88***	-20.95***
	(8.267)	(7.635)	(7.364)
Size of bond (USD mm)		2.349***	2.270***
		(0.271)	(0.254)
Tenor of bond (years)		0.745***	0.760***
		(0.163)	(0.164)
Rating of bond		1.068	1.632
		(1.259)	(1.322)
Revenue that quarter		3.471**	1.976***
		(1.362)	(0.697)
Number of lead underwriters		-0.0151	-0.0489
		(0.440)	(0.438)
Other bonds issued same day		0.0867	0.115
		(0.288)	(0.290)
Post_t×Size			3.256***
			(0.915)
Post_t×Lev			1.681
			(1.237)
Firm FE	✓	✓	✓
Ind x Post FE	✓	✓	✓
Quarter FE	✓	✓	✓
Observations	3037	3037	3037
R-squared	0.501	0.522	0.526

Notes: Includes non-Eurozone USD non-financial corporate issuance, September 2010-June 2018. Post is after June 8, 2016. Estimation is via OLS. Controls are normalized to variance 1 and include issuer credit rating, tenor, amount issued, firm revenue, the dollar amount issued on day t other than firm i, and the number of underwriting banks. Size refers to total assets, leverage refers to the ratio of total long-term debt to total assets. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table VI Increase in issuance at firm level

	(1) Amount (USD Bn)	(2) Amount (USD Bn)	(3) Pr(Issue)	(4) # Bonds
Eurexp_i x Post_t	0.742***	0.618**	0.255**	0.686***
	(0.247)	(0.245)	(0.121)	(0.262)
Post_t×Log(Total Assets)		0.438***	0.0445	0.382***
		(0.113)	(0.0309)	(0.0954)
Rating of bond		-0.990***	-0.595***	-1.173***
		(0.147)	(0.0327)	(0.0725)
Revenue		0.0495	-0.0372	-0.00432
		(0.0386)	(0.0453)	(0.0420)
Size		-0.0912	0.127***	0.0698
		(0.0622)	(0.0444)	(0.0674)
Leverage		2.158***	0.710***	2.161***
		(0.368)	(0.127)	(0.352)
Firm FE	✓	✓	✓	✓
Ind x Qtr FE	✓	✓	✓	✓
Observations	8615	8615	8615	8615
R-squared	0.204	0.286	0.365	0.360

Notes: Includes non-Eurozone USD corporate issuance, by firm-quarter. Post is after Q1 2016. Controls for firm revenue, total assets, credit rating. Controls are normalized to variance 1. Size refers to total assets, leverage refers to the ratio of total long-term debt to total assets. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table VII Impact of Government QE: Underpricing and Oversubscription

	(1) Underpricing (bps)	(2) Oversubscription
Eurexp_i x Post_QE2015	-4.273	-2.060
	(14.70)	(2.280)
Size of bond (USD mm)	0.171	-0.238***
	(0.439)	(0.0870)
Tenor of bond (years)	1.344***	-0.0358
	(0.248)	(0.0674)
Rating of bond	-2.313	-0.939
	(6.681)	(0.623)
Revenue that quarter	-301.1***	7.489
	(81.16)	(11.36)
Number of lead underwriters	-0.791	-0.0215
	(0.865)	(0.253)
Other bonds issued same day	-0.0248	0.0674
	(0.612)	(0.0975)
post_2015×Size	262.6***	-7.249
	(74.74)	(10.40)
post_2015×Lev	3.843	0.476
	(2.409)	(0.414)
Firm FE	✓	✓
Ind x Post_QE2015 FE	✓	✓
Quarter FE	✓	✓
Observations	942	942
R-squared	0.678	0.502

Notes: Includes non-Eurozone USD corporate issuance, by firm-quarter. Sample includes all bonds issued between January 2014 and February 2016. Post is after March 9, 2015. Controls for firm revenue, total assets, credit rating. Controls are normalized to variance 1. Size refers to total assets, leverage refers to the ratio of total long-term debt to total assets. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table VIII Impact of Government QE: Volume Issued

	(1) Amount (USD Bn)	(2) Amount (USD Bn)	(3) Pr(Issue)	(4) # Bonds
Eurexp_i x Post_QE2015	0.417	0.418	0.0667	0.503
	(0.395)	(0.339)	(0.140)	(0.348)
post_2015×Log(Total Assets)		0.405	0.100	0.518
		(0.471)	(0.0763)	(0.320)
Rating of bond		-1.652***	-0.759***	-1.875***
		(0.286)	(0.0582)	(0.160)
Revenue		0.147	0.0163	0.125
		(0.102)	(0.0908)	(0.126)
Size		-0.149	0.115	-0.0113
		(0.331)	(0.140)	(0.294)
Leverage		3.434***	1.149***	3.840***
		(1.038)	(0.230)	(0.762)
Firm FE	✓	✓	✓	✓
Ind x Qtr FE	✓	✓	✓	✓
Observations	2909	2909	2909	2909
R-squared	0.232	0.381	0.384	0.427

Notes: Includes non-Eurozone USD corporate issuance, by firm-quarter. Sample includes all bonds issued between January 2014 and February 2016. Post is after Q1 2015. Controls for firm revenue, total assets, credit rating. Controls are normalized to variance 1. Size refers to total assets, leverage refers to the ratio of total long-term debt to total assets. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table IX Ruling out firm demand explanations

	(1) Underpricing (bps)	(2) Underpricing (bps)	(3) Underpricing (bps)
Post_t×Eurexp_b	-14.72**	-13.86**	-14.31**
	(6.470)	(6.826)	(6.906)
Post_t	-0.471	-1.021	-0.729
	(2.012)	(1.947)	(2.048)
Eurexp_b	5.425	5.453	5.535
	(5.071)	(5.074)	(5.127)
Other bonds issued same day		0.334	0.342
		(0.468)	(0.467)
Size of bond (USD mm)		0.626	0.631
		(0.420)	(0.422)
Tenor of bond (years)		0.892***	0.893***
		(0.146)	(0.146)
Rating of bond		0.746	0.682
		(3.116)	(3.113)
Number of lead underwriters		0.0980	0.101
		(0.697)	(0.692)
Revenue that quarter		1.708**	2.018**
		(0.775)	(0.970)
Post_t×Size			0.283
			(0.319)
Post_t×Lev			-0.753
			(1.575)
Firm x Year FE	✓	✓	✓
Industry FE	✓	✓	✓
Observations	3104	3104	3104
R-squared	0.830	0.835	0.835

Notes: Includes non-Eurozone USD non-financial corporate issuance, September 2010-June 2018. Post is after June 8, 2016. Estimation is via OLS. Controls are normalized to variance 1 and include rating, tenor, amount, amount issued by other firms on the same day, revenue, and number of underwriting banks. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table X Financial issuers do not benefit from ECB program

	(1) Oversubscription	(2) Oversubscription	(3) Underpricing (bps)	(4) Underpricing (bps)
Eurexp_i x Post_t	-0.408	-1.280	-5.579	2.243
	(1.816)	(2.282)	(10.37)	(9.390)
Size of bond (USD mm)		-0.0791		0.565**
		(0.0969)		(0.268)
Tenor of bond (years)		-0.0235		1.614***
		(0.131)		(0.460)
Rating of bond		-0.573**		0.756
		(0.281)		(1.920)
Revenue that quarter		-3.117***		-18.27***
		(0.444)		(3.173)
Number of lead underwriters		0.0935		1.662**
		(0.0844)		(0.658)
Other bonds issued same day		-0.105**		-0.681**
		(0.0521)		(0.302)
Post_t×Size		0.161*		-0.470
		(0.0932)		(1.383)
Post_t×Lev		0.467*		-1.671
		(0.279)		(1.188)
Firm FE	✓	✓	✓	✓
Ind x Post FE	✓		✓	
Quarter FE	✓	✓	✓	✓
Observations	1118	1118	1118	1118
R-squared	0.375	0.391	0.415	0.453

Notes: Includes non-Eurozone USD financial firm issuance, September 2010-June 2018. Financial firms are defined as those with NAIC2 = 52. Post is after June 8, 2016. Estimation is via OLS. Controls are normalized to variance 1 and include rating, tenor, amount, amount issued by other firms on the same day, revenue, and number of underwriting banks. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table XI Increase in issuance at firm level on operational exposure to Euro-zone

	(1) Amount (USD Bn)	(2) Pr(Issue)	(3) # Bonds	(4) Amount (USD Bn)	(5) Pr(Issue)	(6) # Bonds
Num Words Euro x Post_t	-107.9	-34.91	-258.5			
	(306.0)	(99.35)	(240.4)			
Wtd Country x Post_t				-1377.2	-556.1	-2070.1
				(2301.0)	(905.2)	(2087.9)
Post_t×Log(Total Assets)	-0.164	0.0297	0.240*	-0.164	0.0299	0.238*
	(0.219)	(0.0499)	(0.127)	(0.217)	(0.0500)	(0.127)
Controls	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Ind x Qtr FE	✓	✓	✓	✓	✓	✓
Observations	7457	7457	7457	7457	7457	7457
R-squared	0.309	0.366	0.364	0.309	0.366	0.364

Notes: Includes non-Eurozone USD corporate issuance, by firm-quarter. Post is after Q1 2016. Controls, normalized to variance 1, include credit rating, firm revenue, total assets, and leverage. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table XII Heterogeneous increase in issuance at firm level

	(1) Frequent Issuers	(2) Infrequent Issuers	(3) A-rated and above	(4) BBB rated
Eurexp_i x Post_t	0.395	0.774***	0.201	0.785**
	(0.989)	(0.251)	(0.355)	(0.318)
Post_t×Log(Total Assets)	0.915**	0.357***	0.485*	0.425***
	(0.359)	(0.0847)	(0.276)	(0.108)
Controls	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Ind x Qtr FE	✓	✓	✓	✓
Observations	2068	6465	3517	4928
R-squared	0.309	0.308	0.330	0.486

Notes: Includes non-Eurozone USD corporate issuance, by firm-quarter. Frequent issuers are those that issued more than 13 bonds in the sample period; infrequent issuers issued 13 or fewer. Post is after Q1 2016. Controls for firm revenue, total assets, credit rating. Controls are normalized to variance 1. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table XIII More buyers, less time spent on placement

	(1) Time (hours: ann. to price)	(2) Time (hours: ann. to price)	(3) Time (hours: ann. to price)
Eurexp_b x Post_t	-3.743**	-3.721**	-3.603**
	(1.592)	(1.602)	(1.562)
Post	0.0934	-0.412	-0.141
	(0.327)	(0.329)	(0.335)
Number of bonds			0.0531***
			(0.00599)
Controls		✓	✓
Bank x Qtr FE	✓	✓	✓
Firm FE	✓	✓	✓
Observations	10496	10496	10496
R-squared	0.813	0.820	0.823

Notes: Dataset includes one observation per deal for each bank. I exclude financial issuance. Controls include amount issued, tenor, firm revenue, firm size, and leverage. Standard errors are clustered at the bank level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Internet Appendix

Appendix A. Further Robustness Checks

One potential source of endogeneity is the following: some firms anticipate the increase in demand for more European-exposed banks. These firms switch to work with more European banks. Firms that switch banks have some characteristic, unobserved by the econometrician, that is correlated with demand shocks following the ECB QE program. To check if switching is driving my results, I conduct the following test. I exclude any firm that has switched banks following the start of CSPP. That is, any firms that issue bonds with a bank that it has not previously worked with in my sample (2000-2016) are excluded from the analysis. I run my primary Diff-in-diff specification on the subset of firms that work exclusively with banks with which they had prior relationships after June 2016. That is, firms that select *any* new banks after June 8, 2016 are excluded from the analysis. The results are in Appendix Table (IA.2). The coefficients do not change in economic or statistical significance. Even narrowing down to bond issuances where the firm exhibits zero switching behavior, the effect still holds. Thus, firms switching banks cannot be driving my result.

Appendix B. Additional Tables

Table IA.1 Increase in issuance at firm level, 2015-2018

	(1) Amount (USD Bn)	(2) Pr(Issue)	(3) # Bonds
Eurexp_i x Post_t	0.692**	0.299**	0.742**
	(0.275)	(0.147)	(0.316)
Post_t×Log(Total Assets)	0.891***	0.244***	1.014***
	(0.252)	(0.0832)	(0.249)
Rating of bond	-1.327***	-0.575***	-1.420***
	(0.202)	(0.0546)	(0.113)
Revenue	0.00468	-0.127	-0.110
	(0.0762)	(0.103)	(0.129)
Size	-0.342	0.121	-0.0757
	(0.244)	(0.113)	(0.295)
Leverage	3.909***	1.349***	4.081***
	(0.916)	(0.254)	(0.782)
Firm FE	✓	✓	✓
Ind x Qtr FE	✓	✓	✓
Observations	3270	3270	3270
R-squared	0.435	0.396	0.453

Notes: Includes non-Eurozone USD corporate issuance, by firm-quarter. Sample includes all issuance from January 2015 - June 2018. Post is after Q1 2016. Controls for firm revenue, total assets, credit rating. Controls are normalized to variance 1. Size refers to total assets, leverage refers to the ratio of total long-term debt to total assets. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table IA.2 Main specification, excluding switching firms

	(1) Oversubscription	(2) Oversubscription	(3) Underpricing (bps)	(4) Underpricing (bps)
Eurexp_i x Post_t	3.250**	4.477***	-20.62**	-19.48**
	(1.481)	(1.492)	(8.267)	(9.682)
Post_t×Size		-1.210		2.873***
		(0.752)		(0.958)
Post_t×Lev		-0.249		1.670
		(0.215)		(1.626)
Controls		✓		✓
Firm FE	✓	✓	✓	✓
Ind x Post FE	✓		✓	
Quarter FE	✓	✓	✓	✓
Observations	2826	2620	3037	2620
R-squared	0.418	0.450	0.501	0.544

Notes: Includes non-Eurozone USD non-financial corporate issuance, September 2010-June 2018. Post is after June 8, 2016. Estimation is via OLS. Excludes firms that select any new bank underwriter after June 8, 2016. Controls are normalized to variance 1 and include issuer credit rating, tenor, amount issued, firm revenue, the dollar amount issued on day t other than firm i, and the number of underwriting banks. Size refers to total assets, leverage refers to the ratio of total long-term debt to total assets. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table IA.3 More sellers, more time spent on placement

	(1) Time (hours: ann. to price)	(2) Time (hours: ann. to price)
Log(Amount underwritten by the bank)	0.236***	0.130***
	(0.0267)	(0.0239)
Log(Amount issued in the market on that day)	0.176***	0.147***
	(0.0476)	(0.0451)
Controls		✓
Bank x Qtr FE	✓	✓
Firm FE	✓	✓
Observations	10479	10479
R-squared	0.816	0.821

Notes: Dataset includes one observation per deal for each bank. I exclude financial issuance. Standard errors are clustered at the bank level. Controls include amount issued, tenor, firm revenue, firm size, leverage, the log of the total amount of bonds issued on that day, and the log of the total amount underwritten by that bank on that day. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table IA.4 Credit Rating Legend

Moody's	**S&P**	**Fitch**	**Numerical**
Aaa	AAA	AAA	22
Aa1	AA+	AA+	21
Aa2	AA	AA	20
Aa3	AA-	AA-	19
A1	A+	A+	18
A2	A	A	17
A3	A-	A-	16
Baa1	BBB+	BBB+	15
Baa2	BBB	BBB	14
Baa3	BBB-	BBB-	13

References

Nordine Abidi and Ixart Miquel-Flores. Who benefits from the corporate qe? a regression discontinuity design approach. *A Regression Discontinuity Design Approach (March 16, 2018)*, 2018.

Viral V Acharya, Tim Eisert, Christian Eufinger, and Christian Hirsch. Whatever it takes: The real effects of unconventional monetary policy. *The Review of Financial Studies*, 32(9):3366–3411, 2019.

Viral V Acharya, Björn Imbierowicz, Sascha Steffen, and Daniel Teichmann. Does the lack of financial stability impair the transmission of monetary policy? *Journal of Financial Economics*, 2020.

Manuel Adelino, Miguel A Ferreira, Mariassunta Giannetti, and Pedro Pires. Trade credit and the transmission of unconventional monetary policy. Technical report, National Bureau of Economic Research, 2020.

Tobias Adrian and Hyun Song Shin. Financial intermediaries and monetary economics. In *Handbook of monetary economics*, volume 3, pages 601–650. Elsevier, 2010.

Heitor Almeida, Murillo Campello, Bruno Laranjeira, Scott Weisbenner, et al. Corporate debt maturity and the real effects of the 2007 credit crisis. *Critical Finance Review*, 1(1):3–58, 2012.

Óscar Arce, Ricardo Gimeno, and Sergio Mayordomo. The effects of the eurosystemâĂŹs corporate sector purchase programme on spanish companies. *Banco de Espana Article*, 2:18, 2018.

Peter C Austin. Using the standardized difference to compare the prevalence of a binary variable between two groups in observational research. *Communications in statistics-simulation and computation*, 38(6):1228–1234, 2009.

Peter C Austin, Paul Grootendorst, and Geoffrey M Anderson. A comparison of the ability of different propensity score models to balance measured variables between treated and untreated subjects: a monte carlo study. *Statistics in medicine*, 26(4):734–753, 2007.

Malcolm Baker and Jeffrey Wurgler. Market timing and capital structure. *The journal of finance*, 57(1):1–32, 2002.

Bo Becker and Victoria Ivashina. Cyclicality of credit supply: Firm level evidence. *Journal of Monetary Economics*, 62:76–93, 2014.

Bo Becker and Victoria Ivashina. Reaching for yield in the bond market. *The Journal of Finance*, 70(5):1863–1902, 2015.

Bo Becker and Victoria Ivashina. Financial repression in the european sovereign debt crisis. *Review of Finance*, 22(1):83–115, 2018.

Lawrence M Benveniste and Paul A Spindt. How investment bankers determine the offer price and allocation of new issues. *Journal of financial Economics*, 24(2):343–361, 1989.

Tobias Berg, Anthony Saunders, Sascha Steffen, and Daniel Streitz. Mind the gap: The difference between us and european loan rates. *The Review of Financial Studies*, 30(3):948–987, 2017.

Ben Bernanke and A Blinder. l992. the federal funds rate and the channels of monetary transmission. *American Economic Review*, 82(4):90–2, 1992.

Murat M Binay, Vladimir A Gatchev, and Christo A Pirinsky. The role of underwriter-investor relationships in the ipo process. *Journal of Financial and Quantitative Analysis*, pages 785–809, 2007.

Patrick Bolton, Hui Chen, and Neng Wang. Market timing, investment, and risk management. *Journal of Financial Economics*, 109(1):40–62, 2013.

James R Booth and Lena Chua. Ownership dispersion, costly information, and ipo underpricing. *Journal of financial economics*, 41(2):291–310, 1996.

Claudio Borio and Haibin Zhu. Capital regulation, risk-taking and monetary policy: a missing link in the transmission mechanism? *Journal of Financial stability*, 8(4):236–251, 2012.

Nina Boyarchenko, Anna T Kovner, and Or Shachar. It's what you say and what you buy: A holistic evaluation of the corporate credit facilities. 2020.

Nianyun Cai, Jean Helwege, and Arthur Warga. Underpricing in the corporate bond market. *The Review of Financial Studies*, 20(6):2021–2046, 2007.

Charles W Calomiris, Mauricio Larrain, and Sergio L Schmukler. Capital inflows, equity issuance activity, and corporate investment. *Journal of Financial Intermediation*, page 100845, 2019.

Mark Carey and Greg Nini. Is the corporate loan market globally integrated? a pricing puzzle. *The Journal of Finance*, 62(6):2969–3007, 2007.

Indraneel Chakraborty and Andrew MacKinlay. The effects of financing frictions in investment-grade debt markets. *University of Miami Business School Research Paper*, (18-11), 2020.

Indraneel Chakraborty, Itay Goldstein, and Andrew MacKinlay. Monetary stimulus and bank lending. *Journal of Financial Economics*, 136(1):189–218, 2020.

Sudheer Chava and Amiyatosh Purnanandam. The effect of banking crisis on bank-dependent borrowers. *Journal of financial economics*, 99(1):116–135, 2011.

Jiawei Chen and Kejun Song. Two-sided matching in the loan market. *International Journal of Industrial Organization*, 31(2):145–152, 2013.

Gabriel Chodorow-Reich. Effects of unconventional monetary policy on financial institutions. Technical report, National Bureau of Economic Research, 2014a.

Gabriel Chodorow-Reich. The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis. *The Quarterly Journal of Economics*, 129(1):1–59, 2014b.

Francesca Cornelli and David Goldreich. Bookbuilding and strategic allocation. *The Journal of Finance*, 56(6):2337–2369, 2001.

Stine Louise Daetz, Jens Dick-Nielsen, and Mads Stenbo Nielsen. The value of bond underwriter relationships. 2018.

Olivier Darmouni. Informational frictions and the credit crunch. *The Journal of Finance*, 2019.

Olivier Darmouni and Kerry Siani. Crowding out bank loans: Liquidity-driven bond issuance. *Available at SSRN 3693282*, 2020.

Olivier Darmouni, Oliver Giesecke, and Alexander Rodnyansky. The bond lending channel of monetary policy. *Available at SSRN 3419235*, 2019.

Roberto A De Santis and Andrea Zaghini. Unconventional monetary policy and corporate bond issuance. 2019.

Douglas W Diamond. Monitoring and reputation: The choice between bank loans and directly placed debt. *Journal of political Economy*, 99(4):689–721, 1991.

Yi Ding, Wei Xiong, and Jinfan Zhang. Overpricing in chinaâĂŹs corporate bond market. Technical report, National Bureau of Economic Research, 2020.

Itamar Drechsler, Alexi Savov, and Philipp Schnabl. The deposits channel of monetary policy. *The Quarterly Journal of Economics*, 132(4):1819–1876, 2017.

Itamar Drechsler, Alexi Savov, and Philipp Schnabl. Liquidity, risk premia, and the financial transmission of monetary policy. *Annual Review of Financial Economics*, 10:309–328, 2018a.

Itamar Drechsler, Alexi Savov, and Philipp Schnabl. A model of monetary policy and risk premia. *The Journal of Finance*, 73(1):317–373, 2018b.

Darrell Duffie, Nicolae Gârleanu, and Lasse Heje Pedersen. Over-the-counter markets. *Econometrica*, 73(6):1815–1847, 2005.

Darrell Duffie, Nicolae Gârleanu, and Lasse Heje Pedersen. Valuation in over-the-counter markets. *The Review of Financial Studies*, 20(6):1865–1900, 2007.

Andrew Ellul and Marco Pagano. Ipo underpricing and after-market liquidity. *The Review of Financial Studies*, 19(2):381–421, 2006.

Antonio Falato, Itay Goldstein, and Ali Hortaçsu. Financial fragility in the covid-19 crisis: The case of investment funds in corporate bond markets. Technical report, National Bureau of Economic Research, 2020.

Michael Faulkender and Mitchell A Petersen. Does the source of capital affect capital structure? *The Review of Financial Studies*, 19(1):45–79, 2006.

Thomas Flanagan and Amiyatosh Purnanandam. Corporate bond purchases after covid-19: Who did the fed buy and how did the markets respond? *Available at SSRN 3668342*, 2020.

Rients Galema and Stefano Lugo. When central banks buy corporate bonds: Target selection and impact of the european corporate sector purchase program. 2019.

Simon Gilchrist, Bin Wei, Vivian Z Yue, and Egon Zakrajšek. The fed takes on corporate credit risk: An analysis of the efficacy of the smccf. Technical report, National Bureau of Economic Research, 2020.

Erik P Gilje, Elena Loutskina, and Philip E Strahan. Exporting liquidity: Branch banking and financial integration. *The Journal of Finance*, 71(3):1159–1184, 2016.

Michael A Goldstein and Edith S Hotchkiss. Providing liquidity in an illiquid market: Dealer behavior in us corporate bonds. *Journal of Financial Economics*, 135(1):16–40, 2020.

Michael A Goldstein, Edith S Hotchkiss, and David J Pedersen. Secondary market liquidity and primary market pricing of corporate bonds. *Journal of Risk and Financial Management*, 12(2): 86, 2019.

Clive WJ Granger. Investigating causal relations by econometric models and cross-spectral methods. *Econometrica: journal of the Econometric Society*, pages 424–438, 1969.

Benjamin Grosse-Rueschkamp, Sascha Steffen, and Daniel Streitz. A capital structure channel of monetary policy. *Journal of Financial Economics*, 133(2):357–378, 2019.

Michael Halling, Jin Yu, and Josef Zechner. How did covid-19 affect firmsâĂŹ access to public capital markets? *The Review of Corporate Finance Studies*, 2020.

Terrence Hendershott, Dan Li, Dmitry Livdan, and Norman Schürhoff. Relationship trading in over-the-counter markets. *The Journal of Finance*, 75(2):683–734, 2020.

Brian J Henderson and Heather Tookes. Do investment banks' relationships with investors impact pricing? the case of convertible bond issues. *Management Science*, 58(12):2272–2291, 2012.

Bengt Holmstrom and Jean Tirole. Financial intermediation, loanable funds, and the real sector. *the Quarterly Journal of economics*, 112(3):663–691, 1997.

Victoria Ivashina and David Scharfstein. Bank lending during the financial crisis of 2008. *Journal of Financial economics*, 97(3):319–338, 2010.

Christopher James. Relationship-specific assets and the pricing of underwriter services. *The Journal of Finance*, 47(5):1865–1885, 1992.

Dirk Jenter, Katharina Lewellen, and Jerold B Warner. Security issue timing: What do managers know, and when do they know it? *The Journal of Finance*, 66(2):413–443, 2011.

Gabriel Jiménez, Steven Ongena, José-Luis Peydró, and Jesús Saurina. Credit supply and monetary policy: Identifying the bank balance-sheet channel with loan applications. *American Economic Review*, 102(5):2301–26, 2012.

Mahyar Kargar, Benjamin T Lester, David Lindsay, Shuo Liu, and Pierre-Oliver Weill. Corporate bond liquidity during the covid-19 crisis. 2020.

Anil Kashyap, Jeremy Stein, and David Wilcox. The monetary transmission mechanism: Evidence from the composition of external finance. *American Economic Review*, 83(1):78–98, 1993.

Anil K Kashyap and Jeremy C Stein. Monetary policy and bank lending. In *Monetary policy*, pages 221–261. The University of Chicago Press, 1994.

Anil K Kashyap and Jeremy C Stein. What do a million observations on banks say about the transmission of monetary policy? *American Economic Review*, 90(3):407–428, 2000.

Asim Ijaz Khwaja and Atif Mian. Tracing the impact of bank liquidity shocks: Evidence from an emerging market. *American Economic Review*, 98(4):1413–42, 2008.

Ralph SJ Koijen, François Koulischer, Benoît Nguyen, and Motohiro Yogo. Inspecting the mechanism of quantitative easing in the euro area. *Journal of Financial Economics*, 2020.

Huidan Lin and Daniel Paravisini. The effect of financing constraints on risk. *Review of finance*, 17 (1):229–259, 2013.

Alexander Ljungqvist. Ipo underpricing. In *Handbook of Empirical Corporate Finance*, pages 375–422. Elsevier, 2007.

Tim Loughran and Jay R Ritter. Why donâĂŹt issuers get upset about leaving money on the table in ipos? *The Review of Financial Studies*, 15(2):413–444, 2002.

Yiming Ma, Kairong Xiao, and Yao Zeng. Mutual fund liquidity transformation and reverse flight to liquidity. *Available at SSRN 3640861*, 2020.

Matteo Maggiori, Brent Neiman, and Jesse Schreger. International currencies and capital allocation. *Journal of Political Economy*, 128(6):2019–2066, 2020.

Alberto Manconi, Ekaterina Neretina, and Luc Renneboog. Underwriter competition and bargaining power in the corporate bond market. *European Corporate Governance Institute (ECGI)-Finance Working Paper*, (548), 2018.

Massimo Massa, Ayako Yasuda, and Lei Zhang. Supply uncertainty of the bond investor base and the leverage of the firm. *Journal of Financial Economics*, 110(1):185–214, 2013.

Stewart C Myers and Nicholas S Majluf. Corporate financing and investment decisions when firms have information that investors do not have. *Journal of Financial Economics*, 13(2):187–221, 1984.

Florian Nagler and Giorgio Ottonello. Inventory capacity and corporate bond offerings. *BAFFI CAREFIN Centre Research Paper*, (2017-48), 2019.

Emi Nakamura and Jon Steinsson. Identification in macroeconomics. *Journal of Economic Perspectives*, 32(3):59–86, 2018.

Stanislava Nikolova, Liying Wang, and Juan Julie Wu. Institutional allocations in the primary market for corporate bonds. *Journal of Financial Economics*, 2020.

Daniel Paravisini. Local bank financial constraints and firm access to external finance. *The Journal of Finance*, 63(5):2161–2193, 2008.

Daniel Paravisini, Veronica Rappoport, Philipp Schnabl, and Daniel Wolfenzon. Dissecting the effect of credit supply on trade: Evidence from matched credit-export data. *The Review of Economic Studies*, 82(1):333–359, 2015.

Joe Peek and Eric S Rosengren. The international transmission of financial shocks: The case of japan. *The American Economic Review*, pages 495–505, 1997.

Stefano Pegoraro and Mattia Montagna. The transmission channels of quantitative easing: Evidence from the cross-section of corporate bond prices and issuance. 2019.

Tobias Rischen and Erik Theissen. Underpricing in the european corporate bond market. *SSRN Working Paper Series*, 2017.

Kevin Rock. Why new issues are underpriced. *Journal of financial economics*, 15(1-2):187–212, 1986.

Alexander Rodnyansky and Olivier M Darmouni. The effects of quantitative easing on bank lending behavior. *The Review of Financial Studies*, 30(11):3858–3887, 2017.

Alessio Saretto and Heather Tookes. Corporate leverage, debt maturity and credit default swaps: The role of credit supply. *Review of Financial Studies*, 26(5):1190–1247, 2013.

Philipp Schnabl. The international transmission of bank liquidity shocks: Evidence from an emerging market. *The Journal of Finance*, 67(3):897–932, 2012.

Michael Schwert. Bank capital and lending relationships. *The Journal of Finance*, 73(2):787–830, 2018.

Steven A Sharpe. Asymmetric information, bank lending, and implicit contracts: A stylized model of customer relationships. *The journal of finance*, 45(4):1069–1087, 1990.

Steven A Sharpe and Gustavo A Suarez. Why isn't business investment more sensitive to interest rates? evidence from surveys. *Management Science*, 2020.

Amir Sufi. Information asymmetry and financing arrangements: Evidence from syndicated loans. *The Journal of Finance*, 62(2):629–668, 2007.

Karamfil Todorov. Quantify the quantitative easing: Impact on bonds and corporate debt issuance. *Journal of Financial Economics*, 135(2):340–358, 2020.

Stephen D Williamson. Costly monitoring, loan contracts, and equilibrium credit rationing. *The Quarterly Journal of Economics*, 102(1):135–145, 1987.

Ayako Yasuda. Do bank relationships affect the firm's underwriter choice in the corporate-bond underwriting market? *The Journal of Finance*, 60(3):1259–1292, 2005.

Andrea Zaghini. The cspp at work: yield heterogeneity and the portfolio rebalancing channel. *Journal of Corporate Finance*, 56:282–297, 2019.