20
WORKING
PAPERS 2021

NOT ALL SHOCKS ARE CREATED EQUAL: ASSESSING HETEROGENEITY IN THE BANK LENDING CHANNEL

Laura Blattner | Luísa Farinha
Gil Nogueira

BANCO DE PORTUGAL
EUROSYSTEM

20

WORKING PAPERS 2021

NOT ALL SHOCKS ARE CREATED EQUAL: ASSESSING HETEROGENEITY IN THE BANK LENDING CHANNEL

Laura Blattner | Luísa Farinha
Gil Nogueira

DECEMBER 2021
The analyses, opinions and findings of these papers represent the views of the authors, they are not necessarily those of the Banco de Portugal or the Eurosystem

Please address correspondence to
Banco de Portugal, Economics and Research Department
Av. Almirante Reis, 71, 1150-012 Lisboa, Portugal
Tel.: +351 213 130 000, email: estudos@bportugal.pt

BANCO DE PORTUGAL
EUROSYSTEM

Lisboa, 2021 • www.bportugal.pt

Working Papers | Lisboa 2021 • Banco de Portugal Av. Almirante Reis, 71 | 1150-012 Lisboa • www.bportugal.pt •
Edition Economics and Research Department • ISBN (online) 978-989-678-803-2 • ISSN (online) 2182-0422

Not All Shocks Are Created Equal: Assessing Heterogeneity in the Bank Lending Channel

Laura Blattner
Stanford University

Luísa Farinha
Banco de Portugal

Gil Nogueira
Banco de Portugal

December 2021

Abstract
We provide evidence that the strength of the bank lending channel varies considerably across three major positive events in the European sovereign debt crisis – the Greek debt restructuring (PSI), outright monetary transactions (OMT), and quantitative easing (QE). We study how lending responds to each event combining credit registry data with security-level bank balance sheet data from Portugal, a country that was directly exposed to all three events. Even though the price of sovereign debt increased by substantially more after the PSI and OMT announcements, only QE had statistically and economically significant effects on lending to firms and households. We find that banks only *realized* trading gains after QE but not the other two events. These results suggest that banks' incentives to sell bonds are an important determinant of the transmission of sovereign debt interventions to the real economy.

JEL: E52, E58, G18, G21

Keywords: asset purchases, bank lending channel, OMT, PSI, QE.

Acknowledgements: We thank Juliane Begenau, Diana Bonfim, Matteo Maggiori, Steven Ongena, Francisca Rebelo, Anthony Saunders, Philipp Schnabl, Amit Seru for comments and discussions. We also thank seminar participants at New York University and Stanford University. Any errors or omissions are the responsibility of the authors. The contribution by Gil Nogueira to this paper has been prepared under the Lamfalussy Fellowship Program sponsored by the European Central Bank. Any views expressed are only those of the authors and do not necessarily represent the views of the ECB, the Banco de Portugal or the Eurosystem.

E-mail: lblattner@stanford.edu; lfarinha@bportugal.pt; anogueira@bportugal.pt

1. Introduction

Interventions in sovereign debt markets have been a key tool for macroeconomic stabilization since the onset of the European sovereign debt crisis. Prominent examples include the ECB's Outright Monetary Transaction (OMT) and Quantitative Easing (QE) programs. A central goal of these interventions is to recapitalize the banking sector by appreciating the value of bonds held by the banking sector (Brunnermeier and Sannikov 2014). Existing empirical evidence has found mixed evidence of the ability of this "stealth recapitalization" of the banking sector to stimulate credit supply and transmit to the real economy (Acharya *et al.* (2019); Ferrando *et al.* (2019)). Despite a growing literature studying various sovereign debt market interventions in isolation, we lack an understanding when these interventions succeed at stimulating the real economy by recapitalizing the banking sector.

This paper addresses this gap by studying the effect of three key sovereign debt interventions in Europe for an identical set of Portuguese banks using novel security-level bank balance sheet data. We provide two key results: First, credit supply is unrelated to the initial yield impact of the sovereign debt intervention – and, by extension, unrelated to banks' total windfall gain from the price appreciation. The announcement of the OMT program and the Greek debt restructuring with private sector involvement (PSI) had large effects on Portuguese sovereign debt yields (around 300-500 basis points) but did not lead to significant effects on credit supply. In contrast, the announcement of the ECB's QE program had a much smaller impact on yields (around 80 basis points) but led to economically and statistically significant effects on credit supply and firm outcomes. Second, we show that banks only realized gains by reducing their sovereign debt exposure in response to QE but not the other two events. These results suggest that banks' incentives to sell assets are an important determinant of the transmission of sovereign debt interventions to the real economy.

Portugal provides a unique laboratory to study the effect of sovereign debt interventions. Portugal was exposed directly to all three sovereign debt events and offers unique data to study the response of the banking sector. The debt restructuring with private sector involvement (PSI) avoided a disorderly Greek default (Zettelmeyer *et al.* (2013)). Positive news from Greece signaled lower risk of contagion and reduced government bond yields in other European periphery countries such as Portugal (Mink and De Haan (2013)). We estimate that PSI reduced yields on 10-year Portuguese sovereign bonds by about 350 basis points (bp). OMT directly affected Portuguese government bonds as Ireland and Portugal were the only countries that were eligible for OMT purchases because they received received financial sovereign support from the eurozone's bailout funds EFSF/ESM but did not default on their debt. The yields of Portuguese bonds dropped significantly because of the OMT announcement: Krishnamurthy *et al.* (2018) estimate about a 500 bp reduction in the 10-year yields of Portuguese sovereign debt in response to OMT. QE, officially known as the Asset Purchase Program, also had a positive effect on the price of sovereign debt securities from Portugal and other European countries. Koijen *et al.* (2021) estimate a 80 bp reduction in Portuguese yields. In contrast to PSI and OMT, the ECB actually purchased government bonds under the QE program.

We leverage several administrative datasets at the Bank of Portugal. First, we use ISIN-level data with monthly information on the price, quantity, and book value of each security held by each Portuguese bank. This detailed data allows us to compute both the overall valuation effect due to an increase in bond prices triggered by each event as well the extent to which banks realize these gains by selling securities that have gone up in value. All results are estimated in a sample of 44 banks that are active throughout the entire time period. We combine the ISIN-level data with bank financial statements. Second, we use credit registry data to measure the effect on lending to both non-financial firms and households. With a reporting threshold of EUR50, the credit registry allows us to capture the near-universe of lending relationships in Portugal. Finally, we use data on firm financial statements to study effects on firm-level outcomes.

Our identification strategy compares banks with different amounts of pre-event sovereign debt holdings as is standard in the literature. We define bank-level exposure to sovereign debt as the fraction of European sovereign debt securities holdings to total assets. We standardize this measure such that all regression results can be interpreted as the effect of a one standard deviation difference in exposure to sovereign debt. Our difference-in-difference strategy relies on the assumptions that there were no differential pre-trends associated with bank exposure to sovereign debt and no concurrent shocks that coincided with the events of interest that were also correlated with bank exposure.

We first show that the three sovereign debt events differ significantly in their effect on bank net worth. We estimate all results in a bank-level specification that compares banks with different amounts of pre-event sovereign debt holdings. In line with their impact on sovereign debt yields, PSI and OMT lead an increase in net worth of 18% and 6% respectively, while QE leads to a smaller increase of 3%.[1] However, we show that more exposed banks do not reduce their sovereign debt holdings in response to either the PSI and OMT announcements. Consistent with this finding, we find that neither PSI nor OMT lead to significantly higher *realized* trading gains for more exposed banks. In contrast, QE, despite its smaller sovereign yield impact, has statistically and economically meaningful effects on both sovereign debt holdings and realized trading gains. A standard deviation increase in pre-event sovereign debt exposure is associated with a 2% reduction in sovereign debt holdings and a 2% increase in realized trading gains in the period following the QE announcement.

1. This estimates compare two banks who are a standard deviation apart in terms of their holdings of sovereign debt as a share of total assets.

We then show that the three events also differ significantly in their effect on lending to both firms and households. Our results are estimated in a difference-in-difference specification at the borrower-bank level, using the standard identification strategy from Khwaja and Mian (2008). We find that a standard deviation increase in pre-event sovereign debt holdings is associated with a 34% increase in lending to non-financial firms and a 11% increase in lending to households following the announcement of QE. In contrast, we cannot reject the null hypothesis that both PSI and OMT had no effect on lending to either firms or households. Whether overall lending supply increases at the borrower-level depends on the degree of substitution from more to less exposed lenders. Following Jiménez *et al.* (2020), we estimate the effect of exposure to sovereign debt on total lending at the borrower level, correcting for the 'bias' that arises due to demand factors. Following QE, a standard deviation increase in exposure to sovereign debt increases firm lending by 16% and household lending by 6%. These results suggest that, while there is some substitution by less exposed lenders, borrowers increase their total credit exposure following QE. We find similar patterns when studying the extensive margin effects on both the likelihood that an existing lending relationship is terminated as well as on the likelihood that a new loan application is approved (using a similar methodology to Jiménez *et al.* (2012)). We do not find evidence that any of the events leads to an increase in lending to ex-ante riskier borrowers with the exception of QE where we observe an increase in loan approval rates for ex-ante riskier borrowers. For households, we find that the QE-driven increase in credit mostly takes the form of increased auto lending, which might be suggestive of an increase in durable spending. For firms, in contrast, we find a small uptick in sales for newly approved borrowers following QE but no measurable effect on investment after QE. In line with our null results for lending, we find no effects on firm outcomes following either PSI or OMT.

Our results have important implications for macroeconomic general equilibrium models that include a financial accelerator mechanism. In these types of models, sovereign debt market interventions always relax the banks' collateral constraint by increasing bank net worth.[2] If the collateral constraint is binding – as is typically the case in workhorse models such Gertler and Kiyotaki (2010) and Gertler and Karadi (2011) – then relaxing the constraint should immediately increase banks' ability to lend. However, we show that this model prediction does not hold in the data and the transmission to lending instead depends on whether banks sell assets and *realize* any hypothetical trading gains. Our findings imply that forces that determine banks' incentive to sell assets might play an important role in reconciling theoretical models with the data.

2. These models typically do not distinguish between market and book value of equity and specify the collateral constraint in terms of the market value of bank equity. Therefore, any increase in the market value of assets held by the bank increases the market value of equity and relaxes the constraint.

Existing literature sheds some light on what might drive banks' incentive to reduce sovereign debt holdings and increase lending to firms and households. Our results strongly support portfolio re-balancing as the primary mechanism for the effective pass-through of unconventional monetary policy. Vayanos and Vila (2021) argue that external demand shocks (e.g., asset purchases) create positive price pressure on specific assets. Arbitrageurs re-balance their portfolio by investing in other assets, spreading the effect of external demand shocks to other asset classes. In the setting we study, banks engage in such trading by selling government bonds and investing in corporate and household loans.

Our results are also consistent with the previous literature on the muted effect of earlier interventions on lending. If banks are near the regulatory capital requirements, re-investing trading gains from zero or low risk-weight bonds into high risk-weight loans might not be feasible without also raising equity (see also Acharya and Steffen (2015) for this argument). Acharya *et al.* (2019) make this argument to explain why the OMT announcement did not lead to significant lending increases in the syndicated loan market. We show that average capital ratios were particularly low at the onset of PSI and OMT, providing a possible explanation for why banks had a limited incentive to realize gains at these earlier sovereign debt interventions. Second, Becker and Ivashina (2017) and Ongena *et al.* (2019) argue that government pressure plays a role in banks increasing their exposure to domestic sovereign debt. In the absence of external demand for sovereign debt, government suasion might have played a role in disincentivizing banks to offload some of their sovereign debt holdings. Finally, the need for sovereign debt as collateral might have been particularly strong when PSI and OMT were introduced. Crosignani *et al.* (2020) argue that Portuguese banks bought sovereign debt in order to be able to tap the ECB's LTRO operations that required high-quality collateral. High reliance on ECB liquidity could also explain low incentives to reduce sovereign debt holdings.

Literature. We contribute to a large empirical literature exploiting quasi-natural experiments to measure the effect of bank health on lending (see Kashyap and Stein (1994), Holmstrom and Tirole (1997a)), for the original definition of this concept and Blattner (2021) for a meta-database and summary of the empirical literature). In contrast to the existing empirical literature, we study multiple events in the same sample of banks to understand when sovereign debt interventions succeed at stimulating the real economy via the banking sector. We also exploit security-level bank balance sheet data to show that banks' trading behavior in response to sovereign debt events is a key determinant in whether "stealth recapitalization" successfully transmits to the real economy.

Existing research most closely related includes Acharya *et al.* (2019), who study the effect of the OMT announcement in the European syndicated loan market, and – similar to our findings – find muted effects on lending. Unlike Acharya *et al.* (2019), we do not find that OMT led to significant increases in zombie-lending in Portugal which could be due to the fact that banks concentrate zombie-lending on bigger firms (present in the syndicated loan data) while our data includes many

SMEs. Ferrando *et al.* (2019) also estimate the effect of the OMT announcement on European SMEs across several countries and find small positive effects when looking at both the likelihood of loan denials and price rationing. Other related papers include Bottero *et al.* (2019) who study the corporate credit supply effects of the onset of the sovereign debt crisis in Italy. In contrast, we explicitly focus on events that had a positive price impact in sovereign debt markets.

We also contribute to the extensive literature that focuses explicitly on the transmission and effects of unconventional monetary policy. In the US, Rodnyansky and Darmouni (2017), Di Maggio *et al.* (2019), Chakraborty *et al.* (2020) and Luck and Zimmermann (2020) exploit the differential bank holdings of mortgage-backed securities—similar to our cross-sectional identification strategy—find positive the effects of QE on lending in the mortgage market, especially for QE3, the most similar to the ECB's QE program.[3] Similar to our findings, Luck and Zimmermann (2020) find positive effects on both commercial and industrial (C&I) and household lending.[4] Beyond the US, Morais *et al.* (2019) study the international transmission of quantitative easing through foreign banks. In Europe, the literature on the real effects of QE has focused on the real effects of corporate-sector purchase programs (e.g. Grosse-Rueschkamp *et al.* (2019), see also Dell'Ariccia *et al.* (2018) for a survey).

Our results also have implications for a related theoretical literature in macro-finance. First, we analyze empirically the effect of policies that potentially affect lending through changes in the value of assets held by banks (Brunnermeier and Sannikov (2016)), or through incentives to re-balance bank portfolios with assets that have similar characteristics (Goldstein *et al.* (2018)). Our results also have important implication for a large theoretical literature that embeds financial intermediaries into general equilibrium macroeconomic models. Key papers in the literature include Gertler and Kiyotaki (2010), Gertler and Karadi (2011), Holmstrom and Tirole (1997b), Holmström and Tirole (1998), Bernanke *et al.* (1999), Brunnermeier and Sannikov (2014), and He and Krishnamurthy (2013)).[5] Our empirical results present a key empirical challenge for these models as the mechanical link between (market value) net worth appreciation and credit supply suggested by these models is not present in the data. Our findings on the importance of banks *realizing* trading gains suggest that incentives to hold and sell sovereign debt deserve more careful consideration in theoretical models.

3. The Federal Reserve's QE3 and the ECB EAPP were both open-ended commitments to purchase a certain monthly volume of securities until economic conditions improved. In earlier work, Stroebel and Taylor (2012) analyze the effect of the Federal Reserve's mortgage-backed securities purchase program on mortgage spreads.

4. In earlier work, Chakraborty *et al.* (2020) found that the increase in credit supply in the US mortgage market crowds out C&I lending.

5. Other papers in this literature include Gromb and Vayanos (2002), Brunnermeier and Pedersen (2009), Adrian and Shin (2014), Adrian and Shin (2010) Jermann and Quadrini (2012), and Nuño and Thomas (2017).

The remainder of this paper is organized as follows. Section 2 describes the data and the sovereign debt events we exploit. Section 3 describes the empirical strategy. Section 4 provides results. Section 5 concludes.

2. Background and Data

In this section, we provide background on the three episodes in European sovereign debt markets that we study in this paper. We also describe our data and provide summary statistics.

2.1. Background on Events in European Sovereign Debt Markets

We study three key events that induced large changes in sovereign debt prices: the Greek government PSI program in February 2012, the OMT announcement in July 2012, and finally the announcement of the ECB's QE program in January 2015.

PSI. On April 23 2010, the Greek government officially requested a bailout package from the EU and IMF, triggering a series of events that culminated in the European sovereign debt crisis (Lane, 2012). In 2012, there were two major interventions in the European securities market as a direct response to the sovereign debt crisis. In late February, the Greek government announced the *private sector involvement* (PSI), a program to restructure sovereign debt held by private investors. Under this agreement, bond holders accepted a haircut of 53.5% in exchange for new Greek government debt securities. On 9 March 2012, the International Swaps and Derivatives Association (ISDA) declared that Greece's restructuring represented a default, implying that credit default swaps would be triggered. The Greek government introduced a retroactive collective action clause to enforce private sector participation. PSI is widely seen as a positive system-wide shock as the restructuring prevented a potential disorderly default (e.g., Zettelmeyer *et al.* (2013)). Portuguese yields peaked in the month before PSI was announced and declined steadily in the subsequent months (Figure 1). In Appendix B, we estimate that PSI reduced Portuguese government bond yields by 160-420 basis points.

OMT. In July 2012, the then-president of the ECB, Mario Draghi, gave a speech in London that included the famous remark that "within our mandate, the ECB is ready to do whatever it takes to preserve the euro. And believe me, it will be enough." This speech was succeeded a week later by the *Outright Monetary Transactions* (OMT) program, in which the ECB pledged to buy government bonds from countries that received financial sovereign support from the eurozone's bailout funds EFSF/ESM. This program was subject to conditionality: countries could not default on their debt and had to actively request intervention from the ECB. Even though the ECB never made use the OMT program, the OMT was perceived as successful in bringing down bond yields (Krishnamurthy *et al.* (2018)).

QE. In January 2015, the ECB announced the *Public Sector Purchase Program* (PSPP), the first major government bond quantitative easing program in Europe. The ECB pledged to buy government bonds in the secondary market every month. The objective of these purchases was to decrease yields at the long end and transmit monetary policy to final borrowers through an easing of credit conditions. PSPP distinguished itself from OMT for two reasons. First, it increased prices of government bonds not only from the periphery but also from countries in the core of the Euro Area (Koijen *et al.* (2021)). Second, it resulted in actual purchases of assets from the central bank.

2.2. Portugal as a Laboratory

Portugal provides an excellent laboratory to study key events in sovereign debt markets that affected European banks in the aftermath of the sovereign debt crisis.

At the onset of the crisis, the balance sheets of Portuguese banks were directly exposed to securities from periphery countries (Greece, Ireland, Italy, Portugal, and Spain). Throughout the crisis period, banks increased their exposure to Portuguese securities significantly. Figure 2 depicts the evolution of sovereign debt held by Portuguse banks. In January 2010, sovereign debt from Portugal represented about 2% of the assets held by banks in the sample. At the end of 2016, Portuguese debt securities represented almost 6% of the assets held by these banks. The exposure to sovereign debt securities from other periphery countries was about 1% in the beginning of 2011 and increased to approximately 3% at the end of 2016.

Portugal was also directly exposed to each of the major programs that affected bond prices. Portugal and Ireland were the only countries that were eligible for OMT purchases, which required participants to have received financial sovereign support from the eurozone's bailout funds EFSF/ESM and to have no past default. Among periphery countries, Portugal was also the most exposed to sovereign debt events in Greece (Mink and De Haan (2013)). Finally, the ECB actively bought Portuguese government bonds from Portuguese banks through the implementation of the QE program (Koijen *et al.* (2021)).

2.3. Data

We use data from the Bank of Portugal for the universe of banks that operate continuously in Portugal between April 2010 and January 2016. Table A.4 provides a comprehensive list of data sources and Appendix C describes how we process these data sets.

2.3.1. Credit Register. We obtain monthly lending data for all banks in Portugal from the Portuguese credit register maintained by the Bank of Portugal. The credit register has two distinctive advantages. First, there is nearly universal coverage because banks must report all exposures above EUR 50. Second, it covers both households and non-financial firms. Household loans include credit card borrowing, auto loans, personal loans, and mortgage loans. An important feature of the data is that a substantial fraction of non-financial firms and households have multiple lending relationships. This allows us to implement a borrower fixed effect strategy following Khwaja and Mian (2008).

2.3.2. Credit Registry Inquiries. We also draw on the database of bank inquiries to the credit registry maintained by the Bank of Portugal. Banks can request information on a potential borrower from the credit register. Inquiries are usually requested only for a loan application of a potential new corporate or household borrower by lenders that have no pre-existing credit relationship. We match the inquiry data to the credit registry to compute successful and unsuccessful loan applications following Jiménez *et al.* (2012). Successful applications are credit inquiries which are followed by a new lending relationship with the inquiring bank in the subsequent 6 months.

The household credit register contains a large number of observations, which makes model estimation computationally burdensome. We draw a 10% sample of the household credit register (including inquiries) to make computations easier.

2.3.3. Bank Data. We use the following types of bank data from the Bank of Portugal. We retrieve bank balance sheet variables from BBS (Bank Balance Sheet Database), which has monthly financial statements for all banks. We augment this dataset with quarterly regulatory ratios from an additional dataset at the Bank of Portugal which is used for prudential capital regulation. At the ISIN level, we use SIET (Estatísticas de Emissões de Títulos), a dataset that contains the list of all securities (with ISIN) held by each bank at the monthly frequency. We also use an *Asset Purchase Program* (APP) purchases dataset. This file contains the list of transactions of Portuguese assets purchased by the ECB under the APP and includes the identification of counterparties. This dataset is provided by the Bank of Portugal.

We define bank exposure to sovereign securities using ISIN-level data from SIET. We obtain a list of marketable securities that are eligible for collateral at the monthly frequency from the ECB website.[6] Securities from SIET count towards our exposure to sovereign debt measure if they satisfy three conditions: 1) they appear at least once in the list of marketable securities from the ECB; 2) they are issued by the central government and; 3) they are denominated in Euro. For each bank, we compute the fraction of exposed assets to total assets:$\text{exposure}_b = \frac{\text{exposed assets}_b}{\text{total assets}_b}$. We standardize this variable to obtain a comparable exposure measure across events. Exposure is defined in the month prior to the event date.

A key advantage of the SIET dataset is that we can calculate gains on sovereign debt security holdings using information on the underlying securities in a bank's

6. See website `https://www.ecb.europa.eu/paym/coll/assets/html/index.en.html` Accessed 01/05/2021.

portfolio. Total monthly gains from holding a security are derived by multiplying the monthly change in price by the outstanding position in that security at the beginning of the month. Realized gains are cumulative gains that banks monetize when they close their position in a certain security over the course of a month. We aggregate across the security-level changes in total gains to obtain bank-level changes, which we denote by Δvalue_{bt}. Note the we do not observe intra-month trading but only the changes in net positions at the end of the month. This data limitation implies that we might miss some realized gains if the bank makes profitable intra-month trades that do not lead to net changes in the holdings at the end of the month.

To obtain the total change in bank net worth that results from interventions in the European sovereign debt market, we use data both on bank-level book capital (from the bank balance sheet database) and the bank-level valuation changes we compute from SIET. We then compute the total net worth change as follows

$$\Delta \log \text{net worth}_{bt} = \log(1 + \text{capital}_{b,t-1} + \Delta\text{value}_{bt}) - \log(1 + \text{capital}_{bt}), \quad (1)$$

where $\text{capital}_{b,t-1}$ is the book value of capital plus reserves of bank b in the last period before the event and Δvalue_{bt} is the change in value or capital gains for securities in SIET. We set $\Delta \log \text{net worth}_{bt} = -1$ if $-\Delta\text{value}_{bt} > \text{capital}_{bt}$. We winsorize monthly price changes from SIET at the 1% level.

2.3.4. Firm Data. We use annual firm financial statements from the Central Balance Sheet Database (CB), a repository of financial statements maintained by Banco de Portugal that covers the universe of non-financial corporations. We retrieve CAPEX/Sales and interest rates[7] from this dataset and winsorize these variables at the 1% level.

2.4. Descriptive Statistics

Table 1 shows descriptive statistics for the variables used in the analysis for each of the 44 banks across the three events. In Columns (1) and (2) of Table 2a, we show descriptive statistics weighted by the number of borrowers in the sample (including borrowers with single relationships). In Columns (3) and (4), we show unweighted statistics. In Tables 2a and 2b we provide descriptive statistics for firm and household borrowers. We show statistics for households with multiple credit relationships, as we use borrower fixed effects to control for demand factors throughout our analysis (see Khwaja and Mian (2008)). In Table A.1, we provide descriptive statistics separately for each event.

7. We define CAPEX as $\text{CAPEX}_t = \text{Fixed Assets}_t - \text{Fixed Assets}_{t-1} + \text{Depreciation}_t$. We replace CAPEX_t by 0 whenever it is smaller than 0. We define interest rates as $\frac{\text{Interest paid}_t}{\frac{\text{Loans}_t + \text{Loans}_{t-1}}{2}}$.

3. Empirical Specification

We estimate two types of specification. First, we trace the effect of each sovereign debt event on bank-level net worth in a bank-level difference-in-difference specification. Second, we estimate the effect on various lending outcomes in a bank-borrower-level difference-in-difference specification.

Bank-level specification. To trace bank-level net worth effects, we estimate the following bank-level difference-in-difference specification for each of the three sovereign debt events:

$$\Delta y_b = \beta_1 \text{exposure}_b + \gamma X_b + \varepsilon_b, \tag{2}$$

where Δy_b is the dependent variable of interest.

The first dependent variable measures the change in exposed assets after each sovereign debt event as a percentage of bank assets in the month before the event: $\frac{\Delta\text{exposed assets}_{bt}}{\text{total assets}_{b,t-1}}$. To obtain changes in quantity (as opposed to picking up valuation effects), we compute changes in holdings by multiplying changes in quantity by the price in the month prior to the event.

The second dependent variable measures the effect on bank net worth. We first measure the effect on net worth due to valuation effects that stem from the increase in sovereign debt prices as a result of the sovereign debt event. As discussed in Section 2.3, we measure both total valuation gains due to the appreciated bond prices as well as realized gains that occur when banks liquidate part of their bond holdings.

We also include a number of bank controls X_i to absorb any differences in size (total assets), capitalization (capital ratio), non-performing loans, or loan acceptance rates. The key identification assumption, besides a standard parallel trends assumption, is that the degree of a bank's exposure to sovereign debt is not correlated with any other contemporaneous bank-level shocks.

One potential confounding factor are the ECB's long-term refinancing operations (LTRO) that provided cheap liquidity to the European banking system (e.g., Carpinelli and Crosignani (2021)). To address this concern, we include bank-level LTRO take-up in our controls X_b.

We weight bank-level regressions by the number of borrowers in the sample to reflect a bank's relative economic impact. We provide unweighted results in Table A.5. Weighting by bank size makes our bank-level estimates more comparable to the borrower-bank-level specification which effectively weight by bank size as larger banks – with more lending relationships – contribute more observations to the dataset.

We also estimate a bank-level dynamic differences-in-differences specification:

$$y_{bt} = \sum_{k=-4}^{4} \beta_k (\text{exposure}_b \times \mathbb{1}_{t=k}) + \alpha_b + \alpha_t + \varepsilon_{bt} \tag{3}$$

where y_{bt} is the outcome of interest in period t, α_b and α_t are bank and time fixed effects, and $\mathbb{1}_{t=k}$ is an indicator variable that is equal to 1 in period k.

Borrower-bank-level specification. At the borrower(i)-bank(b) level, we estimate the following dynamic difference-in-difference specification for each event:

$$y_{ibt} = \sum_{k=-10}^{12} \beta_k(\text{exposure}_b \times \mathbb{1}_{t=k}) + \delta t + \alpha_b + \varphi X_{bi} + \alpha_{it} + \varepsilon_{ibt} \quad (4)$$

where y_{ibt} is the outcome of interest. α_{it} is a borrower-time fixed effect (Khwaja and Mian (2008)) that absorbs any borrower-level changes in credit growth, allowing us to identify the effect of changes in the share of lending supplied by more versus less exposed banks. We include borrower-bank level controls X_{bi}, including the number of years the relationship has existed to address worries that longer relationship might receive a differential treatment from recently established lending relationships. We cluster standard errors at the bank level, which is our level of treatment (Bertrand *et al.* (2004)).

We find that lending responses to all three events exhibit pre-trends in the dynamic specification (see Figure A.1 in the Appendix). However, these pre-trends are well approximated by a linear trend. Our main dynamic specification therefore includes a linear trend δt (for similar approaches see for example Bhuller *et al.* (2013), Dobkin *et al.* (2018); Goodman-Bacon (2018a); Goodman-Bacon (2018b). These pre-trends likely reflect secular trends affecting exposed and non-exposed banks differentially. For example, exposed banks might be more exposed to business cycle dynamics in this period leading to differential secular trends in lending.

The key coefficients of interest, β_k, show the change in lending relative to any pre-existing linear trend. The key identifying assumption is that, conditional on the included controls, the timing of the sovereign debt event is uncorrelated with deviations of the lending outcome from a linear time trend.

We also estimate a standard non-dynamic difference-in-difference specification (following for example Khwaja and Mian (2008))

$$\Delta y_{ib} = \beta_1 \text{exposure}_b + \alpha_i + \varphi X_{bi} + \varepsilon_{ib} \quad (5)$$

where Δy_{ib} is the change in the variable of interest. α_i is a firm fixed-effect and X_{bi} are firm-bank level controls as before. This specification allows us to obtain single estimate that is more easily comparable across events.

In addition to the lending volume in existing lending relationships (intensive margin), we also estimate effect of sovereign debt exposure on the likelihood that an existing relationships ends and on new loan approval rates using credit registry inquiries (as in Jiménez *et al.* (2012)). The latter two outcomes are estimated using a linear probability model that can accommodate the large number of fixed effects.

Borrower-level specification. We follow Jiménez *et al.* (2020) and construct a borrower-level lending effect that correct for the bias induced by non-random sorting of borrowers and lenders. We obtain this coefficient by combining the

OLS coefficient which omits the firm fixed-effect $\hat{\beta}_{OLS}$ and the within firm-bank estimator$\hat{\beta}_{FE}$ from equation 5:

$$\hat{\overline{\beta}} = \overline{\beta}_{OLS} - (\hat{\beta}_{OLS} - \hat{\beta}_{FE}) \times \frac{Var(\delta_i)}{Var(\overline{\delta}_j)}, \tag{6}$$

where $Var(\delta_i)$ is the variance of the borrower-bank level standardized exposure to sovereign debt and $Var(\overline{\delta}_j)$ is the variance of δ_i, averaged at the borrower level.

$\hat{\overline{\beta}}$ can be interpreted as the effect of bank exposure to sovereign debt on borrower-level lending, adjusted for "demand factors". This definition is contingent on the assumption that the within-firm strategy does successfully capture any borrower-level demand shocks that are concurrent with the sovereign debt events. If borrower demand is relationship-specific, for example, because different lenders provide different types of loans, the above correction would not be sufficient to adjust for demand effects.

Firm-level specification. Finally, we estimate the effect of the sovereign debt events on firm-level outcomes. Since we can no longer include the firm$\times$time fixed effect, we control for time-varying factors that may affect firm demand by interacting firm controls with a linear time trend. We estimate the following equation:

$$y_{it} = \alpha_i + \alpha_t + \sum_m \lambda_m[(t+6) \times \mathbb{1}_{i=m}] + \beta(\text{exposure}_b \times \mathbb{1}_{t\geq 0}) + \varepsilon_{it} \tag{7}$$

Where y_{it} is a firm-level outcome t years after the event, α_i and α_t are firm and year fixed effects, $\mathbb{1}_{i=m}$ is an indicator variable equal to 1 for firm i and 0 otherwise, and $\mathbb{1}_{t\geq 0}$ is an indicator variable equal to 1 for observations in the year of the event or subsequent years.

We first analyze lending outcomes at the firm-level by aggregating loans in the credit register at the firm level. We then estimate the effect on sales and capital expenditure (CAPEX).

We measure the effect of exposure to government debt securities on two group of firms. First, we analyze firms that had pre-existing credit relationships. Our definition of firm-level exposure for existing credit relationships is similar to the one used by Jasova *et al.* (2021). We define firm-level exposure$_i$ as

$$\text{exposure}_i = \frac{\sum_b(\text{exposure}_b \times \text{credit}_{bi})}{\sum_b(\text{credit}_{bi})} \tag{8}$$

where exposure$_b$ is the bank-level exposure variable used in the rest of the analysis, and credit$_{bi}$ is the outstanding amount of the firm-bank credit relationship.

When estimating the effect of obtaining a new lending relationship, we define firm exposure as

$$\text{exposure}_i = \frac{\sum_b(\text{exposure}_b)}{B_i} \tag{9}$$

where B_i is the number of loan applications between the month of the sovereign debt event and one year after the event.

4. Results

This section presents our main results. First, we discuss the effect of exposure to sovereign debt securities on bank balance sheets for PSI, OMT, and QE. Second, we discuss the transmission of bank balance sheet events to firms and households.

4.1. Bank-level Results

Table 2 shows that exposed bank change their sovereign debt holdings only in response to the QE announcement but not in response to PSI or OMT. While we cannot reject the null hypothesis of no effect on sovereign debt holdings, we find a strong reduction in sovereign debt exposure following QE. A one standard deviation increase in pre-QE sovereign debt holdings leads to 2 percentage point reduction in banks' sovereign debt holdings (as a share of assets) following the announcement of QE. Given an average holding of about 5 percent (see Table A.3), this represents approximately a 40% average reduction in banks' sovereign debt holdings. While Table 2 reports results for a one-year horizon after each event (strictly speaking, a 13-month horizon since it includes the month of the event and the subsequent twelve months), Table A.5, repeats this analysis at shorter horizons – *month* and *quarter*. Note that the *month* horizon includes the month of the announcement and the subsequent month. The *quarter* horizon includes the month of the announcement and the subsequent three months.

Figure 3 presents results from the dynamic difference-in-difference specification between the first quarter of 2014 and the last quarter of 2015. We use the cumulative variation in debt holdings as the dependent variable. Consistent with QE having an effect on sovereign debt holdings, exposed banks started reducing their sovereign debt holdings more in the quarter when QE was announced. Micro-data from transactions conducted by the Bank of Portugal under the ECB's program also confirm that Portuguese banks were actively selling sovereign debt in response to QE. Figure 6 depicts the percentage of Portuguese securities that were sold to the Bank of Portugal by Portuguese commercial banks in the sample. The figure shows that these institutions were the counterparties in over 20% of the purchases of Portuguese securities.

We now turn to the effect on bank net worth. Table 3 shows that while PSI and OMT led to valuation gains, banks were realizing these gains only in response to QE. One additional standard deviation in sovereign debt exposure led to an economically and statistically significant valuation gain of 18% following the announcement of PSI. Effects are smaller and statistically insignificant for OMT and QE (6% and 3% respectively). Turning to realized gains in Columns 4-6 of Table 3, QE is the only event that leads to economically and statistically significant realized gains. A one standard deviation increase in exposure to sovereign debt increases realized gains by 2% after the announcement of QE. These results are consistent with the results from Table 3 that show that only QE leads to a significant reduction in banks' sovereign debt positions. Tables A.6 and A.7 show that our results are robust to using shorter time horizons and no regression weights.

4.2. Borrower-bank-level Results

We present our main results on the lending effects at the intensive margin in Figure 4 (dynamic specification) and Table 4 (pooled specification). Results for PSI, OMT, and QE differ significantly. Exposure to sovereign debt has a strong positive effect on lending for QE, but no detectable effect for PSI and OMT. Comparing two banks who are a standard deviation apart in terms of exposure to affected securities, average lending to firms increases by 34% after the QE announcement, by 11% for PSI, and by 6% for OMT (Columns 1 to 3 of Table 4). However, we cannot reject the null that the effect of the PSI and OMT interventions is zero at conventional levels of significance. For households, we find similar patterns with lending increasing by 11%, 4%, and 1% for QE, PSI and OMT, respectively (Columns 3 to 6 of Table 4). Tables A.8 and A.11, show that our results are robust to varying the time horizon and including only lending relationships present in at least 2 of the three events. Figure 4 also shows the absence of pre-trends in all three specifications conditional on the included linear time trend that absorbs secular trends in lending at exposed banks.

Turning next to the extensive margin, Figure 5 (dynamic specification) and Table 5 (pooled specification) show that rates of relationship termination follow similar patterns. Exit rates for firms decrease by 4%, 1% and 0.5% for QE, PSI, and OMT respectively (Columns 1 to 3 of Table 5). Again, we cannot reject the null that the effect of the PSI and OMT interventions is zero at conventional levels of significance. For households, we get a decrease in the exit rates by 2%, 0.3% and 0.0% (Columns 3 to 6 of Table 5), with the latter two estimates not statistically significantly different from zero. As in the intensive margin case, Figure 5 shows the absence of pre-trends for all three events (again conditional on a linear time trend). In Table A.12, we repeat the analysis for shorter time horizons. We also repeat the analysis for relationship terminations without including borrower fixed effects from Equation 5. In the specification without fixed effects, we include the following firm-level controls interacted with time trends: log firm assets, log total workers, EBITDA/assets, equity ratio and an indicator variable that is equal to 1 if the firm has loans that are more than 90 days overdue . We report coefficients in Table A.14. Results are directionally similar but statistically weaker.

We now turn from the effects on firms and households that have an existing lending relationship to the formation of new lending relationships. We find a similar pattern of statistically and economically large effects for QE, with moderate to no effects for PSI and OMT (see Table 6). The acceptance rates for firms increases by 4% for QE and by 2% for PSI. We observe no significant effect for OMT.

Households do not experience statistically significant changes in acceptance rates in response to any of the three events. In Table A.16, we repeat the analysis for different time horizons. We also present results without the borrower fixed effects in Table A.19. Results are again directionally similar but statistically weaker.

Effects on borrower composition. We analyze the relationship between exposure to government debt securities and borrower composition in Table 7. In particular, we ask whether lending results are more pronounced for ex-ante riskier borrowers as defined by a proprietary default risk prediction model developed by the Bank of Portugal (Antunes *et al.* (2016)). This measure of riskiness is only available for firms but not households. The model is based on firm observables in balance sheet and income statement data. For this exercise, we further restrict the sample to firms for which default predictions are available. In Columns 1 to 3, the dependent variable is the 1-year change in the logarithm of lending. In Columns 4 to 6, the dependent variable is an indicator equal to 1 for a successful loan application. We find some evidence that QE led to an increase in lending to borrowers with higher predicted default risk at the extensive margin. We do not find economically meaningful effects of changes in the composition of borrowers in response to the other two events.

4.3. Borrower-level Results

Table 8 shows lending results at the borrower-level. We show each component in equation 6 that we use to estimate the effect of exposure to sovereign debt on lending at the borrower-level. Column 1 in Table 8 depicts the "bias-corrected" estimates. QE has a stronger effect on total lending to firms and households than PSI and OMT. For example, at the intensive margin, firms increase borrowing by 16% following QE but only by 6-7% following PSI and OMT. The QE effect is considerably smaller than the coefficient in the borrower-bank specification (34%), implying that there is substantial substitution of credit from less exposed lenders. We observe similar results for households at the intensive margin. QE leads to an increase in lending by 6% to households at the intensive margin (vs. 11% in the borrower-bank specification). The PSI and OMT coefficient ar around 1%.

Turning to the extensive margin results, we find small coefficients for households, consistent with the null results in Table 6. For firms, the extensive margin effect on lending is around 5%, which is close to the coefficient in the borrower-bank specification (4%). This finding suggests that there is little substitution towards less exposed lenders at the extensive margin.

4.4. Household-level Results By Product

We now turn to additional household-level results by lending product. Since we do not have data on household outcomes other than the credit registry data, we provide estimates of effects on household-level borrowing across the following lending products: credit card borrowing, mortgages, consumer loans, auto loans and overdrafts on checking accounts. In Table 9, we provide estimates from a household-level difference-in-difference specification as well as the "bias-corrected" coefficient based on Equation 6. The extensive margin results (bottom panel) confirm our earlier finding that there are no measurable effects on loan acceptance rates across any type of household loan product following the three sovereign debt events. At the intensive margin (top panel), there is some evidence that the increase in consumer lending following the QE announcement in the borrower-bank specification (Table 4) is driven by auto loans. Auto loans are frequently used as a measure of durable consumption, suggesting a moderate positive consumption impact of QE. QE is also associated with a decrease in mortgage lending. However, the drop in magnitude when computing the bias-corrected coefficient suggests that some of this drop might in fact be driven by an (unrelated) reduction in mortgage demand by borrowers at exposed banks.

We find no positive lending effects following OMT, with the exception of a small increase in mortgages. This finding is in line with a small, positive (but statistically insignificant) effect on household lending in Table 4. We also find some positive effects on auto, credit cards, and overdrafts in response to PSI. These findings are also in line with our baseline results in Table 4, where we found positive – though very noisy – effects. Another interpretation of the PSI effects is that these across-the-board positive effects on consumer lending reflect differential credit needs at the onset of the European sovereign debt crisis. In particular, overdrafts might reflect an unexpected increase in liquidity needs more than an expansion in debt-financed consumption. The "bias-corrected" coefficients are quite close in magnitude to the OLS coefficients, suggesting that the positive effects are not simply driven by the selection of borrowers with high credit demand towards high-exposure banks. However, this bias-correction depends on (i) the borrower-bank specification successfully controlling for demand effects and (ii) demand effects being equal in the sample of single and multiple relationship households. If one of these assumptions is violated, the bias-correction would not successfully remove differential demand effects.

4.5. Additional Firm-level Results

We now turn to additional firm-level results obtained using firm-level financial statements. In Table 10, we estimate Equation 7 using three firm-level outcomes as the dependent variable: (imputed) interest rates, sales, and CAPEX. The left panel depicts coefficients for firms with existing credit relationships (intensive margin) while the right panel depicts coefficients for firms that apply for new loans in the year after each event (extensive margin). We do not find a statistically or economically meaningful relationship between exposure and interest rates. Note that we have fewer observations in the interest rate regressions than in the lending regressions. This difference is caused by two factors. First, some firms may have no loans. In these cases it is impossible to impute an interest rate. Second, we use a panel of firm financial statements to obtain interest rates and firms may not report financial statements in some years, making it again impossible to impute an interest rate.

In panels b and c of Table 10, we measure the effect on log sales and on the ratio of capital expenditure (CAPEX) to pre-period sales.[8] Overall, there does not appear to be strong pass-through of credit conditions into sales or investment for any of the three events. For OMT and PSI, this finding is consistent with our null-result on lending. It is more surprising to find no meaningful effects following the announcement of QE. One potential explanation is that firm-level credit constraints had already eased when QE was announced and firms had already been able to fund all positive-NPV investment projects available to them. This finding is consistent with Bidder *et al.* (2021) who find no firm-level pass-through for bank-level shocks in non-recessionary time periods.

5. Conclusion

Central banks around the world rely on asset purchase programs as part of their policy kit to stimulate the real economy. The bank lending channel, whereby an increase in bank net worth relaxes bank collateral constraints and increases credit supply, has been proposed as a key transmission mechanism for central bank asset purchases. A similar logic of 'stealth recapitalization' applies to other events that lead to price appreciations of bonds frequently held by the banking sector.

We provide evidence that not all shocks are created equal – the strength of this bank lending channel varies considerably across three major events in the European sovereign debt crisis – the Greek debt restructuring with private sector involvement (PSI), outright monetary transactions (OMT), and quantitative easing (QE). We study both how banks' security trading and lending behavior responds to these three events. We show that while sovereign bond yields drop by significantly more after PSI and OMT than after the QE announcement, only QE has statistically and economically meaningful effects on bank lending to firms and households. We trace these differences to the fact the banks only sell assets and realize trading gains after the onset of QE but none of the other two events.

The increased incentive to realize gains is consistent with portfolio rebalancing having a prime role in the transmission of unconventional monetary policy. Acting as arbitrageurs, banks accommodate external demand shocks by reallocating their portfolio and investing in other assets. Our results corroborate the previous literature showing a muted transmission of trading gains to credit markets. Several hypotheses have been brought forward to explain these findings: (i) banks were weakly capitalized, which limited the substitution to high risk-weight loans, (ii) governments exerted suasion on the banking sector to hold domestic sovereign

8. The average CAPEX/sales ratio is 11.1% for firms in the intensive margin sample and 9.4% for firms in the extensive margin sample.

debt, and (iii) banks were incentivized to hold sovereign debt as collateral to tap central bank liquidity. Further investigating which of these hypotheses played the most important role is a fruitful direction for future research.

References

Acharya, Viral V, Tim Eisert, Christian Eufinger, and Christian Hirsch (2019). "Whatever It Takes: The Real Effects of Unconventional Monetary Policy." *The Review of Financial Studies*, 32(9), 3366–3411.

Acharya, Viral V and Sascha Steffen (2015). "The Greatest Carry Trade Ever? Understanding Eurozone Bank Risks." *Journal of Financial Economics*, 115(2), 215–236.

Adrian, Tobias and Hyun Song Shin (2010). "Liquidity and leverage." *Journal of Financial Intermediation*, 19(3), 418–437.

Adrian, Tobias and Hyun Song Shin (2014). "Procyclical Leverage and Value-at-Risk." *Review of Financial Studies*, 27(2), 373–403. Publisher: Society for Financial Studies.

Antunes, António, Homero Gonçalves, Pedro Prego, *et al.* (2016). "Firm default probabilities revisited." *Economic Bulletin and Financial Stability Report Articles*.

Becker, Bo and Victoria Ivashina (2017). "Financial Repression in the European Sovereign Debt Crisis*." *Review of Finance*, 22(1), 83–115.

Bernanke, Ben S., Mark Gertler, and Simon Gilchrist (1999). "Chapter 21 The financial accelerator in a quantitative business cycle framework." In *Handbook of Macroeconomics*, vol. 1, pp. 1341–1393. Elsevier.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan (2004). "How much should we trust differences-in-differences estimates?" *The Quarterly Journal of Economics*, 119(1), 249–275.

Bhuller, Manudeep, Tarjei Havnes, Edwin Leuven, and Magne Mogstad (2013). "Broadband Internet: An Information Superhighway to Sex Crime?" *Review of Economic Studies*, 80(4), 1237–1266.

Bidder, Rhys M., John R. Krainer, and Adam Hale Shapiro (2021). "De-leveraging or de-risking? How banks cope with loss." *Review of Economic Dynamics*, 39, 100–127.

Blattner, Laura (2021). "How Important is the Bank Lending Channel? Evidence from a Meta-Study."

Bottero, Margherita, Simone Lenzu, and Filippo Mezzanotti (2019). "Sovereign Debt Exposure and Bank Lending Channel: Impact on Credit Supply and the Real Economy." *Journal of International Economics*.

Brunnermeier, Markus K. and Lasse Heje Pedersen (2009). "Market Liquidity and Funding Liquidity." *The Review of Financial Studies*, 22(6), 2201–2238.

Brunnermeier, Markus K. and Yuliy Sannikov (2014). "A Macroeconomic Model with a Financial Sector." *American Economic Review*, 104(2), 379–421.

Brunnermeier, Markus K and Yuliy Sannikov (2016). "The I theory of money." Tech. rep., National Bureau of Economic Research.

Carpinelli, Luisa and Matteo Crosignani (2021). "The Design and Transmission of Central Bank Liquidity Provisions." *Journal of Financial Economics*, 141, 27–47.

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

Crosignani, Matteo, Miguel Faria-e Castro, and Luis Fonseca (2020). "The (Unintended?) Consequences of the Largest Liquidity Injection Ever." *Journal of Monetary Economics*, pp. 97–112.

Dell'Ariccia, Giovanni, Pau Rabanal, and Damiano Sandri (2018). "Unconventional Monetary Policies in the Euro Area, Japan, and the United Kingdom." *Journal of Economic Perspectives*, 32(4), 147–72.

Di Maggio, Marco, Amir Kermani, and Christopher J Palmer (2019). "How Quantitative Easing Works: Evidence on the Refinancing Channel." *The Review of Economic Studies*, 87(3), 1498–1528.

Dobkin, Carlos, Amy Finkelstein, Raymond Kluender, and Matthew J. Notowidigdo (2018). "The Economic Consequences of Hospital Admissions." *American Economic Review*, 108(2), 308–52.

Ferrando, Annalisa, Alexander Popov, and Gregory F. Udell (2019). "Do SMEs Benefit from Unconventional Monetary Policy and How? Microevidence from the Eurozone." *Journal of Money, Credit and Banking*, 51(4), 895–928.

Gertler, Mark and Peter Karadi (2011). "A model of unconventional monetary policy." *Journal of Monetary Economics*, 58(1), 17–34.

Gertler, Mark and Nobuhiro Kiyotaki (2010). "Chapter 11 - Financial Intermediation and Credit Policy in Business Cycle Analysis." In *Handbook of Monetary Economics*, vol. 3, edited by Benjamin M. Friedman and Michael Woodford, pp. 547–599. Elsevier.

Goldstein, Itay, Jonathan Witmer, and Jing Yang (2018). "Following the money: Evidence for the portfolio balance channel of quantitative easing." Tech. rep., Bank of Canada.

Goodman-Bacon, Andrew (2018a). "Difference-in-Differences with Variation in Treatment Timing." Working Paper 25018, National Bureau of Economic Research.

Goodman-Bacon, Andrew (2018b). "Public Insurance and Mortality: Evidence from Medicaid Implementation." *Journal of Political Economy*, 126(1), 216–262.

Gromb, Denis and Dimitri Vayanos (2002). "Equilibrium and welfare in markets with financially constrained arbitrageurs." *Journal of Financial Economics*, 66(2), 361–407.

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

He, Zhiguo and Arvind Krishnamurthy (2013). "Intermediary Asset Pricing." *American Economic Review*, 103(2), 732–770.

Holmström, Bengt and Jean Tirole (1998). "Private and Public Supply of Liquidity." *Journal of Political Economy*, 106(1), 1–40.

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

Holmstrom, Bengt and Jean Tirole (1997b). "Financial Intermediation, Loanable Funds, and the Real Sector." *The Quarterly Journal of Economics*, 112(3), 663–691.

Jasova, Martina, Caterina Mendicino, and Dominik Supera (2021). "Policy uncertainty, lender of last resort and the real economy." *Journal of Monetary Economics*, 118, 381–398.

Jermann, Urban and Vincenzo Quadrini (2012). "Macroeconomic Effects of Financial Shocks." *American Economic Review*, 102(1), 238–271.

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

Jiménez, Gabriel, Atif Mian, José-Luis Peydró, and Jesús Saurina (2020). "The real effects of the bank lending channel." *Journal of Monetary Economics*, 115, 162–179.

Kashyap, A and J Stein (1994). "Monetary policy and bank lending."

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

Koijen, Ralph SJ, François Koulischer, Benoît Nguyen, and Motohiro Yogo (2021). "Inspecting the mechanism of quantitative easing in the euro area." *Journal of Financial Economics*, 140(1), 1–20.

Krishnamurthy, Arvind, Stefan Nagel, and Annette Vissing-Jorgensen (2018). "ECB policies involving government bond purchases: Impact and channels." *Review of Finance*, 22(1), 1–44.

Luck, Stephan and Tom Zimmermann (2020). "Employment effects of unconventional monetary policy: Evidence from QE." *Journal of Financial Economics*, 135(3), 678–703.

Mink, Mark and Jakob De Haan (2013). "Contagion during the Greek sovereign debt crisis." *Journal of International Money and Finance*, 34, 102–113.

Morais, BERNARDO, JOSÉ-LUIS PEYDRÓ, JESSICA ROLDÁN-PEÑA, and CLAUDIA RUIZ-ORTEGA (2019). "The International Bank Lending Channel of Monetary Policy Rates and QE: Credit Supply, Reach-for-Yield, and Real Effects." *The Journal of Finance*, 74(1), 55–90.

Nuño, Galo and Carlos Thomas (2017). "Bank Leverage Cycles." *American Economic Journal: Macroeconomics*, 9(2), 32–72.

Ongena, Steven, Alexander Popov, and Neeltje Van Horen (2019). "The Invisible Hand of the Government: Moral Suasion during the European Sovereign Debt Crisis." *American Economic Journal: Macroeconomics*, 11(4), 346–79.

Rodnyansky, Alexander and Olivier Darmouni (2017). "The Effects of Quantitative Easing." *Review of Financial Studies*, 30(11), 3858–3887.

Stroebel, Johannes and John B Taylor (2012). "Estimated impact of the Federal Reserve's mortgage-backed securities purchase program." *international Journal of central Banking*, 8(2).

Vayanos, Dimitri and Jean-Luc Vila (2021). "A Preferred-Habitat Model of the Term Structure of Interest Rates." *Econometrica*, 89(1), 77–112.

Zettelmeyer, Jeromin, Christoph Trebesch, and Mitu Gulati (2013). "The Greek debt restructuring: an autopsy." *Economic Policy*, 28(75), 513–563.

Tables

(a) Bank Descriptive Statistics

	Weighted		Unweighted	
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)
% exposed assets	4.25	3.92	2.29	6.04
Total assets (€Bi)	51.66	45.30	10.68	26.38
Capital ratio	14.47	12.52	19.49	32.52
% arrears (firms)	10.49	8.89	8.98	11.31
% arrears (households)	5.61	7.11	6.83	12.38
% accepted firm applications	12.86	11.45	11.99	19.57
% accepted household app.	21.89	15.94	11.82	17.50
% LTRO	0.45	0.75	0.15	1.60
Observations	132		132	

(b) Borrower Descriptive Statistics

	Firms		Households	
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)
Δ log loans (1 month)	-0.28	1.44	-0.32	1.25
Δ log loans (3 months)	-0.54	1.98	-0.57	1.69
Δ log loans (12 months)	-1.51	3.24	-1.33	2.58
Exit rate (1 month)	3.11	17.35	4.96	21.71
Exit rate (3 months)	5.77	23.32	8.63	28.07
Exit rate (12 months)	15.24	35.94	18.97	39.21
Relationship length (years)	4.56	2.83	3.1	1.62
Observations	684,426		679,299	

Table 1. Descriptive Statistics

Notes: The table shows descriptive statistics for banks, firm-bank and household-bank pairs used in the analysis. In panel a, we include an observation for each bank prior to each of the three events. In Columns (1) and (2) of panel a, statistics are weighted by the total number of borrowers (firms and households). In Columns (3) and (4) of panel b, we use sample weights. Relationship length for households is censored to the left because the household credit register only starts in 2009. We use the month before each event as the reference date: January 2012 for PSI, June 2012 for OMT, and December 2014 for QE.

	$\frac{\Delta \text{ Exposed securities}}{\text{Total assets}}$		
	PSI (1)	OMT (2)	QE (3)
Exposure	0.001 (0.002)	-0.003 (0.003)	-0.019** (0.009)
Bank controls	Yes	Yes	Yes
Observations	44	44	44
R-squared	0.209	0.296	0.616

Table 2. Regression Results: Security Holdings

Notes: The table reports estimates of coefficient β_1 from Equation 2. The dependent variable $\frac{\Delta \text{ Exposed securities}}{\text{Total assets}}$ is the change in the ratio of exposed securities over total assets one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total bank assets in the last month before the event. We use bank-level characteristics from Table A.3 as additional controls. Observations are weighted by the number of bank customers (firms and households) in the sample. Robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	Δ log net worth					
	Price appreciation			Capital gains		
	PSI (1)	OMT (2)	QE (3)	PSI (4)	OMT (5)	QE (6)
Exposure	0.182** (0.071)	0.062 (0.049)	0.027 (0.025)	0.001 (0.073)	-0.012 (0.053)	0.024*** (0.006)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44	44	44	44	44	44
R-squared	0.442	0.501	0.510	0.330	0.424	0.618

Table 3. Regression Results: Bank-level Net Worth Gains

Notes: The table reports estimates of coefficient β_1 from Equation 2. The dependent variable Δ log net worth is the change in log bank net worth induced by price appreciation (Columns 1 to 3) and capital gains (Columns 4 to 6) one year after the event. We obtain gains from price appreciation by multiplying monthly price variation by outstanding security holdings. Realized gains are cumulative gains that banks monetize when they close their position in a certain security. We compute the effect of price variation and capital gains on bank net worth using Equation 1. The independent variable Exposure is the standardized ratio of exposed securities to total bank assets in the last month before the event. We use bank-level characteristics from Table A.3 as additional controls. Observations are weighted by the number of borrowers (firms and households) in the sample. Robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	Δ log outstanding loans					
	Firms			Households		
	PSI (1)	OMT (2)	QE (3)	PSI (4)	OMT (5)	QE (6)
Exposure	0.109 (0.083)	0.064 (0.090)	0.336*** (0.084)	0.042 (0.036)	0.013 (0.035)	0.110*** (0.018)
Observations	241,311	233,402	209,713	242,823	231,840	204,636
R-squared	0.433	0.434	0.426	0.461	0.468	0.469

Table 4. Regression Results: Outstanding Loans

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Δ log outstanding loans is the change in one plus log outstanding loans one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. Standard errors are clustered at the bank-level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	Relationship ends = 1					
	Firms			Households		
	PSI (1)	OMT (2)	QE (3)	PSI (4)	OMT (5)	QE (6)
Exposure	-0.010	-0.005	-0.039***	-0.003	0.0004	-0.019***
	(0.008)	(0.009)	(0.008)	(0.006)	(0.005)	(0.002)
Observations	241,311	233,402	209,713	242,823	231,840	204,636
R-squared	0.427	0.43	0.43	0.4667	0.4741	0.477

Table 5. Regression Results: Termination of Credit Relationships

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Relationship ends = 1 is an indicator variable that is equal to 1 if the borrower does not have a credit relationship with the bank 1 year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. Standard errors are clustered at the bank-level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	Accepted application = 1					
	Firms			Households		
	PSI (1)	OMT (2)	QE (3)	PSI (4)	OMT (5)	QE (6)
Exposure	0.022**	-0.001	0.036***	-0.002	-0.011	0.009
	(0.011)	(0.013)	(0.009)	(0.006)	(0.011)	(0.014)
Observations	14,672	16,279	19,859	13,051	13,477	16,538
R-squared	0.450	0.459	0.456	0.456	0.454	0.461

Table 6. Regression Results: Loan Applications

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Accepted application = 1 is an indicator variable that is equal to 1 if the loan application is accepted. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. The sample consists of loan applications between the 4th and the 12th month after the month of the event. We control for bank characteristics with variables from Table 1. Standard errors are clustered at the bank-level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	Δ log outstanding loans			Accepted application = 1		
	PSI (1)	OMT (2)	QE (3)	PSI (4)	OMT (5)	QE (6)
Exposure	0.176**	0.113	0.249***	0.018**	-0.007	0.029**
	(0.084)	(0.099)	(0.059)	(0.008)	(0.011)	(0.011)
Exposure×P(default)	-0.005*	-0.005**	0.001	0.001	0.001	0.001**
	(0.002)	(0.003)	(0.006)	(0.001)	(0.001)	(0.001)
Average P(default)	6.465	6.371	5.315	4.153	3.990	4.323
Observations	203,285	197,405	164,066	14,047	15,676	18,000
R-squared	0.436	0.438	0.437	0.450	0.460	0.451

Table 7. Regression Results: By Borrower Composition

Notes: The table shows coefficients from estimating Equation 5. In Columns 1 to 3 the dependent variable Δ log outstanding loans is the change in log one plus outstanding loans one year after the event. In Columns 4 to 6 the dependent variable Accepted application = 1 is an indicator equal to 1 for firms with accepted loan applications 4-12 months after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. The independent variable Exposure × P(default) is the interaction between Exposure and the probability of default provided by Antunes *et al.* (2016). We control for bank characteristics with variables from Table 1. Standard errors are clustered at the bank-level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	$\hat{\hat{\beta}}$ (1)	$\hat{\beta}_{FE}$ (2)	$\hat{\beta}_{OLS}$ (3)	$\overline{\beta}_{OLS}$ (4)	$Var(\delta_i)$ (5)	$Var(\overline{\delta}_j)$ (6)
Firms, intensive margin						
PSI	0.069	0.109	0.094	0.044***	1.025	0.624
		(0.083)	(0.089)	(0.009)		
OMT	0.062	0.064	0.050	0.038***	1.017	0.584
		(0.090)	(0.098)	(0.010)		
QE	0.162	0.336***	0.325***	0.146***	0.978	0.654
		(0.084)	(0.079)	(0.010)		
Households, intensive margin						
PSI	0.015	0.042	0.032	0.001	1.029	0.736
		(0.036)	(0.044)	(0.006)		
OMT	0.014	0.013	-0.008	-0.016**	1.030	0.714
		(0.035)	(0.042)	(0.007)		
QE	0.063	0.110***	0.091***	0.037***	1.025	0.740
		(0.018)	(0.021)	(0.007)		
Firms, extensive margin						
PSI	0.032	0.023**	0.018**	0.022**	1.331	0.666
		(0.011)	(0.009)	(0.009)		
OMT	0.000	-0.001	0.003	0.008	1.157	0.568
		(0.013)	(0.014)	(0.010)		
QE	0.048	0.036***	0.030***	0.037***	1.406	0.737
		(0.009)	(0.008)	(0.009)		
Households, extensive margin						
PSI	-0.036	-0.002	-0.013	-0.051***	1.029	0.736
		(0.006)	(0.009)	(0.013)		
OMT	-0.032	-0.011	-0.016	-0.039***	1.030	0.714
		(0.011)	(0.012)	(0.014)		
QE	0.012	0.010	0.008	0.009	1.025	0.740
		(0.014)	(0.015)	(0.015)		

Table 8. Regression Results: Borrower-level Lending Effects

Notes: The table shows coefficients from Equation 6. Column 1 depicts estimates for the bias-corrected effect of exposure to sovereign debt on Δ log outstanding loans (change in one plus outstanding loans one year after each event). Column 2 depicts estimates for Equation 5, including borrower fixed effects. Column 2 depicts estimates for Equation 5, excluding borrower fixed effects. Column 3 depicts estimates for Equation 5, including borrower fixed effects. Column 4 depicts estimates for the effect of exposure to sovereign debt on Δ log outstanding loans, not correcting for demand-driven bias. Column 5 depicts the variance of the standardized exposure to sovereign debt measured at the bank-borrower level. Column 6 depicts the variance of the average standardized exposure to sovereign debt measured at the borrower level. We weight each observation by total outstanding credit in the month before each of the events when computing borrower-level averages. Standard errors in parentheses are clustered at the bank-level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

		Overdrafts (1)	Credit card (2)	Mortgages (3)	Consumer (4)	Auto (5)
Intensive margin						
PSI						
	$\overline{\beta}_{OLS}$	0.026***	0.032***	-0.009*	0.030**	0.021**
		(0.009)	(0.010)	(0.005)	(0.012)	(0.009)
	$\hat{\overline{\beta}}$	0.034	0.022	-0.005	0.045	0.029
OMT						
	$\overline{\beta}_{OLS}$	-0.017	0.010	0.015**	0.009	0.011
		(0.010)	(0.011)	(0.006)	(0.013)	(0.010)
	$\hat{\overline{\beta}}$	-0.005	0.014	0.022	0.025	0.015
QE						
	$\overline{\beta}_{OLS}$	-0.010	0.013	-0.035***	-0.014	0.042***
		(0.011)	(0.012)	(0.008)	(0.014)	(0.011)
	$\hat{\overline{\beta}}$	0.007	0.019	-0.029	-0.007	0.042
Extensive margin						
PSI						
	$\overline{\beta}_{OLS}$	0.004	-0.012*	-0.005	-0.007	-0.039***
		(0.004)	(0.007)	(0.004)	(0.007)	(0.009)
	$\hat{\overline{\beta}}$	0.004	-0.011	-0.004	-0.001	-0.032
OMT						
	$\overline{\beta}_{OLS}$	-0.001	0.012	-0.004	-0.001	-0.052***
		(0.003)	(0.009)	(0.005)	(0.008)	(0.009)
	$\hat{\overline{\beta}}$	-0.001	0.009	-0.001	-0.001	-0.043
QE						
	$\overline{\beta}_{OLS}$	0.002	-0.012**	-0.002	0.000	-0.001
		(0.003)	(0.005)	(0.004)	(0.006)	(0.006)
	$\hat{\overline{\beta}}$	0.003	-0.008	-0.002	0.000	-0.004

Table 9. Regression results: Household lending, By Product

Notes: The table shows coefficients from Equation 6 for five major credit product categories used by households. $\overline{\beta}_{OLS}$ is the effect of exposure to sovereign debt on lending, excluding household fixed effect, which is potentially biased by demand effects. $\hat{\overline{\beta}}$ is the bias-corrected effect of exposure to sovereign debt on Δ log outstanding loans for each product. The dependent variable in the intensive margin panel is Δ log outstanding loans for each credit product (change in one plus outstanding loans one year after each event). The dependent variable in the extensive margin is a dummy if a new lending relationship of that product type is created. We weight each observation by total outstanding credit in the month before each of the events when computing borrower-level averages. Standard errors in parentheses are clustered at the bank-level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) Interest rates

	Intensive margin			Extensive margin		
	PSI	OMT	QE	PSI	OMT	QE
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.010	0.035	-0.034	0.055*	-0.060*	-0.013
	(0.022)	(0.022)	(0.026)	(0.029)	(0.032)	(0.030)
Observations	923,186	901,385	665,307	498,954	490,769	437,256
R-squared	0.734	0.735	0.784	0.728	0.728	0.786

(b) Log sales

	Intensive margin			Extensive margin		
	PSI	OMT	QE	PSI	OMT	QE
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.003	0.014*	-0.006	0.003	-0.004	0.030***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)
Observations	1,072,841	1,046,565	933,810	617,178	607,964	771,650
R-squared	0.866	0.867	0.897	0.888	0.893	0.905

(c) CAPEX/Sales

	Intensive margin			Extensive margin		
	PSI	OMT	QE	PSI	OMT	QE
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	0.003**	0.003**	0.000	-0.004**	-0.002	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	980,886	958,210	846,578	566,540	558,472	694,131
R-squared	0.628	0.634	0.638	0.599	0.597	0.684

Table 10. Regression Results: Firm Outcomes

Notes: The table shows coefficients from estimating Equation 7. In Table 11a, the dependent variable is the average interest rate. In Table 11b, the dependent variable is the logarithm of one plus sales. In Table 11c, the dependent variable is the ratio of CAPEX/Sales. Columns 1 to 3 depict estimates for firms with existing credit relationships in the month before each event. Columns 4 to 6 depict estimates for firms that apply to new credit relationships in the year after each event. Standard errors are clustered at the firm-level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Figures

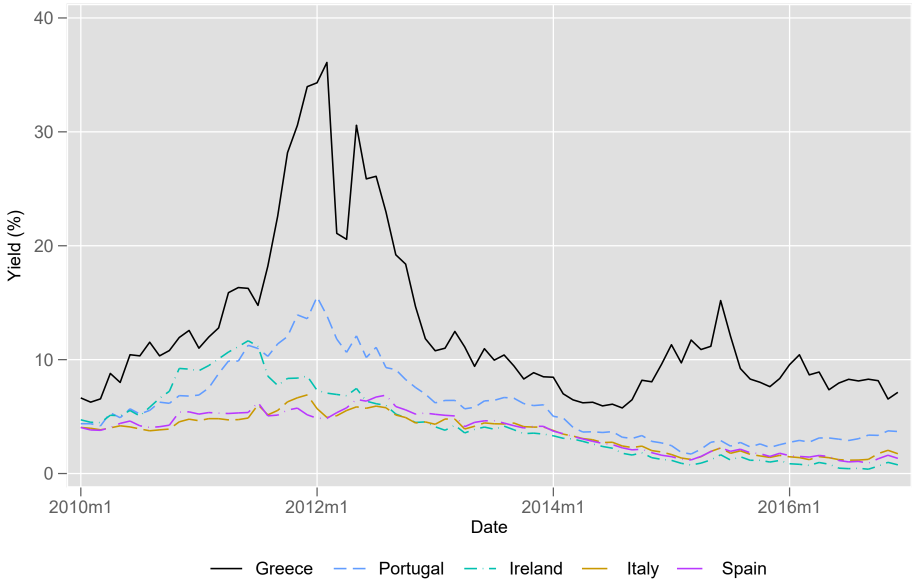


Figure 1: Government Bond Yields of Periphery Countries

Notes: The table shows 10-year government bond yields for Greece, Portugal, Ireland, Italy and Spain. Source: Bloomberg.

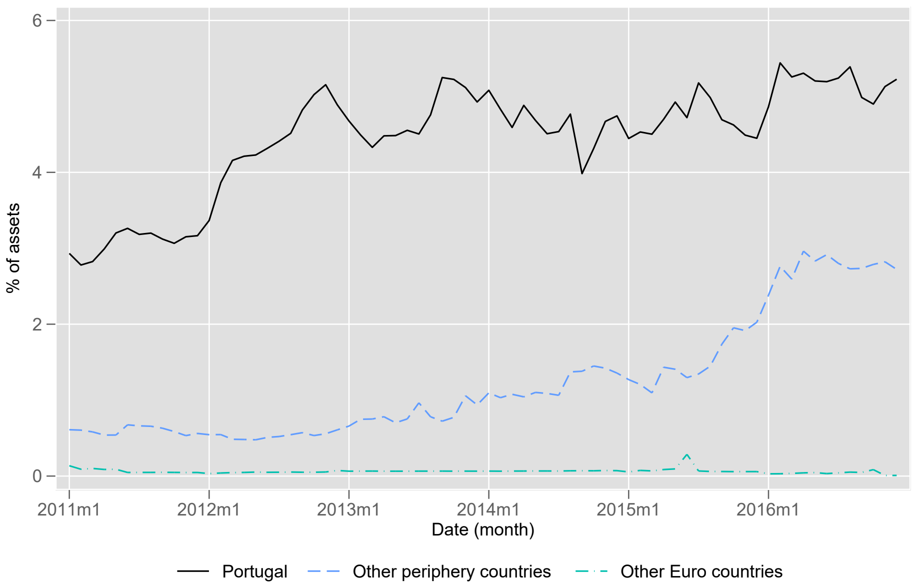


Figure 2: Exposure to Sovereign Debt

Notes: The figure shows the book value of sovereign debt securities held by banks in the sample as a percentage of total assets. We divide securities by issuer country. *Portugal* includes securities issued by the Portuguese government. *Other periphery countries* includes securities issued by Greece, Spain, Italy and Ireland. *Other Euro Area countries* includes securities issued by the remaining countries from the Euro Area.

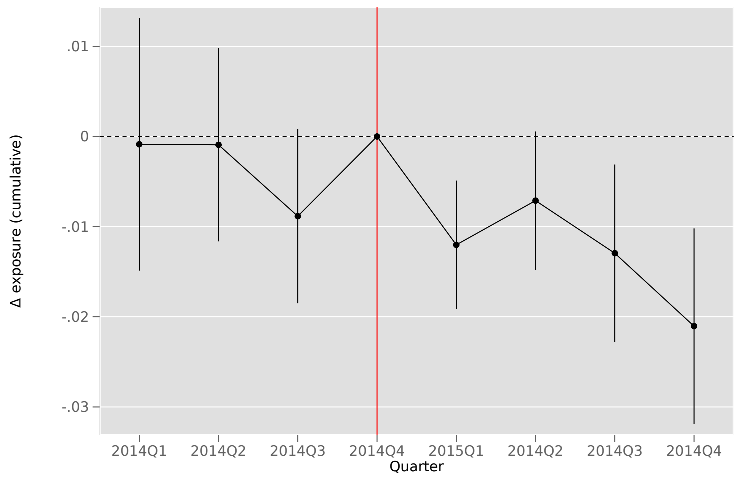
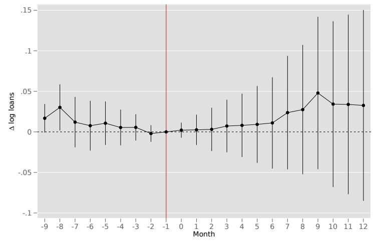
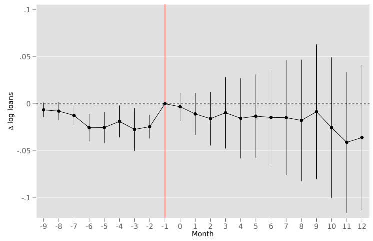


Figure 3: Dynamic Variation of Exposed Security Holdings

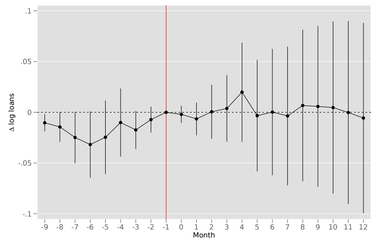
Notes: The figure reports coefficients and 95% confidence intervals from equation 3. The dependent variable is the cumulative variation in exposed security holdings as a percentage of total assets in the month before the QE announcement. The independent variable is the standardized ratio of exposed securities to total assets, measured in the last month before the event. Heteroskedasticity robust standard errors are used to calculate confidence intervals.



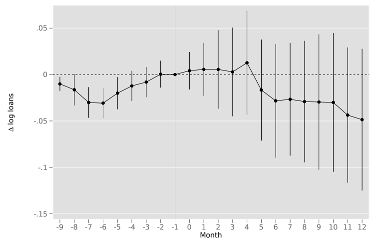
(a) PSI: Firms



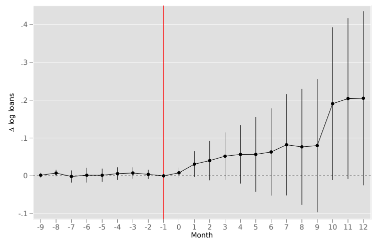
(b) PSI: Households



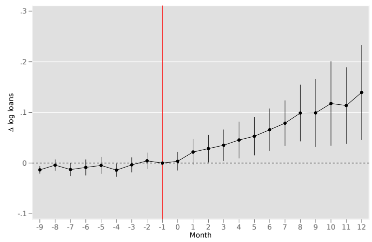
(c) OMT: Firms



(d) OMT: Households



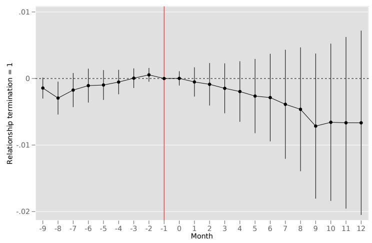
(e) QE: Firms



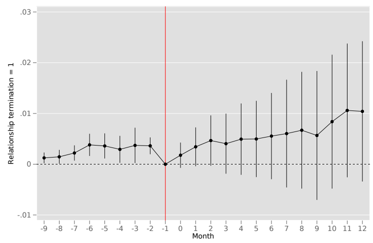
(f) QE: Households

Note: Each panel of the figure shows the results from a dynamic difference-in-difference regression at the borrower-bank level around a sovereign debt event. The dependent variable is log(1+loan volume). We show coefficients on the Exposure variable, which is the standardized ratio of exposed securities to total assets, measured in the last month before the event. The left panels show results for non-financial borrowers while the right panels show results for households. The regression include bank and firm-time fixed effects as well as linear time trend. Standard errors are double clustered at the firm and at the bank level.

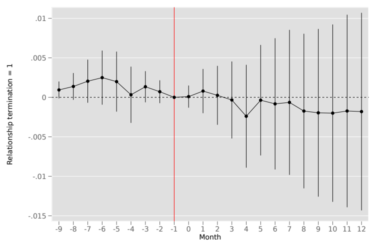
Figure 4: Borrower-Bank Regression Results: Intensive Margin



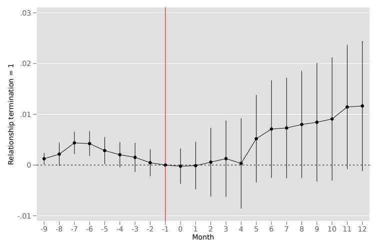
(a) PSI: Firms



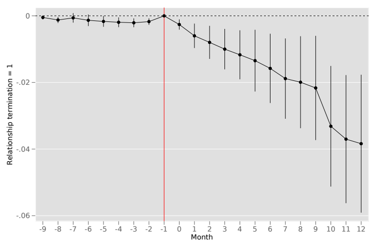
(b) PSI: Households



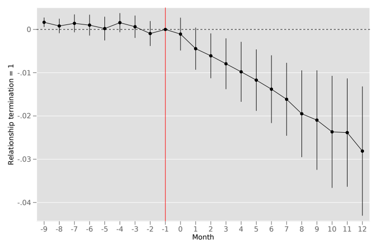
(c) OMT: Firms



(d) OMT: Households



(e) QE: Firms



(f) QE: Households

Note: Each panel of the figure shows the results from a dynamic difference-in-difference regression at the borrower-bank level around a sovereign debt event. The dependent variable is a dummy if a lending relationship is terminated. We show coefficients on the Exposure variable, which is the standardized ratio of exposed securities to total assets, measured in the last month before the event. The left panels show results for non-financial borrowers while the right panels show results for households. The regression include bank and firm-time fixed effects. The regression specification includes a linear time trend. Standard errors are double clustered at the bank level and at the firm level.

Figure 5: Borrower-Bank Regression Results: Extensive Margin

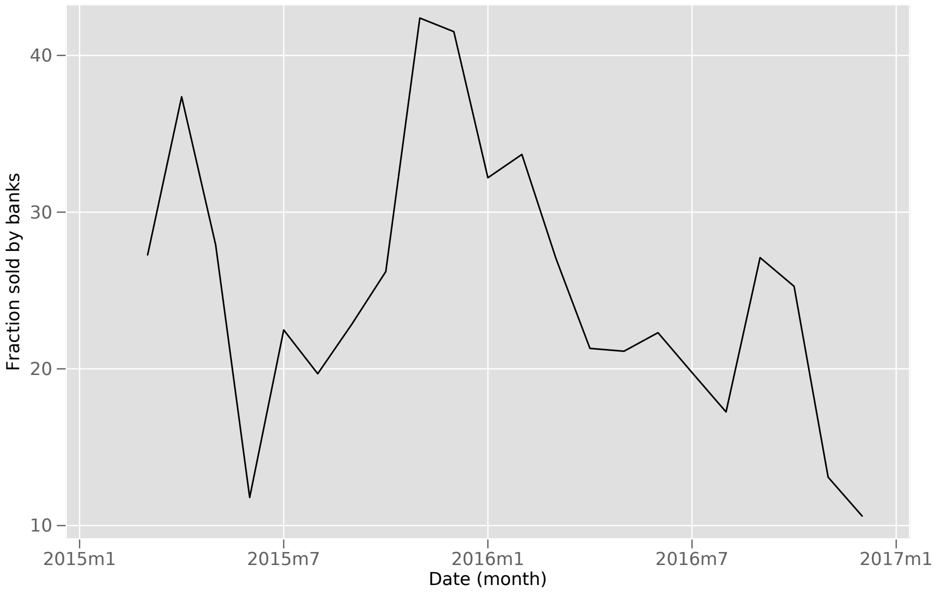


Figure 6: Fraction of Portuguese Assets Sold to the ECB by Banks in the Sample

Notes: The figure shows the percentage of ECB Portuguese government debt security purchases in which the counterparty was a Portuguese bank in our sample. Percentages are weighted by security market value.

Appendix A: Tables and Figures

(a) Bank Descriptive Statistics

	Weighted		Unweighted	
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)
% exposed assets	3.50	3.34	1.78	4.03
Total assets (EUR M)	54,972.23	48,194.50	11,319.20	28,330.86
Capital ratio	12.22	12.47	20.98	30.87
% arrears (firms)	8.02	6.83	7.09	8.66
% arrears (households)	5.77	7.13	6.31	11.63
% accepted applications (firms)	13.11	11.78	14.68	21.40
% accepted applications (households)	20.60	13.69	10.75	14.47
% LTRO	0.31	0.51	0.23	1.09
Observations	44		44	

(b) Borrower Descriptive Statistics

	Firms		Households	
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)
Δ log loans (Month)	-0.29	1.44	-0.33	1.28
Δ log loans (Quarter)	-0.55	1.97	-0.58	1.71
Δ log loans (Year)	-1.55	3.21	-1.36	2.58
Exit rate (Month)	3.19	17.58	5.04	21.88
Exit rate (Quarter)	5.87	23.51	8.76	28.27
Exit rate (Year)	15.46	36.16	19.36	39.51
Relationship length (years)	4.08	2.37	2.45	0.92
Observations	241,311		241,311	

Table A.1. Descriptive Statistics by Event (PSI)

Notes: The table shows descriptive statistics for banks, firm-bank and household-bank pairs used in the analysis. In Columns (1) and (2) of Table A.2a, statistics are weighted by the total number of customers (firms and households) in the analysis. In Columns (3) and (4) of Table A.2a, we use sample weights. Relationship length for households is censored to the left because the household credit register only starts in 2009. We use the month before each event as the reference date: January 2012 for PSI, June 2012 for OMT, and December 2014 for QE.

(a) Bank Descriptive Statistics

	Weighted		Unweighted	
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)
% exposed assets	4.26	4.05	2.27	5.48
Total assets (EUR M)	55,261.49	48,491.42	11,339.60	28,488.66
Capital ratio	13.44	13.29	21.77	27.12
% arrears (firms)	9.96	8.49	8.30	9.97
% arrears (households)	5.43	7.07	6.70	13.06
% accepted applications (firms)	13.10	12.15	11.98	19.93
% accepted applications (households)	24.37	17.85	15.26	21.82
% LTRO	0.37	0.86	0.14	0.55
Observations	44		44	

(b) Borrower Descriptive Statistics

	Firms		Households	
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)
Δ log loans (Month)	-0.3	1.44	-0.32	1.23
Δ log loans (Quarter)	-0.57	1.98	-0.57	1.68
Δ log loans (Year)	-1.5	3.2	-1.33	2.56
Exit rate (Month)	3.22	17.64	5.02	21.83
Exit rate (Quarter)	5.99	23.73	8.78	28.31
Exit rate (Year)	15.12	35.82	19.11	39.31
Relationship length (years)	4.32	2.49	2.76	1.06
Observations	233,402		233,402	

Table A.2. Descriptive Statistics by Event (OMT)

Notes: The table shows descriptive statistics for banks, firm-bank and household-bank pairs used in the analysis. In Columns (1) and (2) of Table A.3a, statistics are weighted by the total number of customers (firms and households) in the analysis. In Columns (3) and (4) of Table A.3a, we use sample weights. Relationship length for households is censored to the left because the household credit register only starts in 2009. We use the month before each event as the reference date: January 2012 for PSI, June 2012 for OMT, and December 2014 for QE.

(a) Bank Descriptive Statistics

	Weighted		Unweighted	
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)
% exposed assets	5.06	4.20	2.81	8.02
Total assets (EUR M)	44,168.39	36,783.05	9,376.42	22,449.17
Capital ratio	18.04	16.98	15.72	38.86
% arrears (firms)	13.76	10.22	11.55	14.29
% arrears (households)	5.64	7.13	7.47	12.66
% accepted applications (firms)	12.35	10.23	9.32	17.24
% accepted applications (households)	20.66	15.77	9.44	15.11
% LTRO	0.69	0.79	0.07	2.51
Observations	44		44	

(b) Borrower Descriptive Statistics

	Firms		Households	
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)
Δ log loans (Month)	-0.25	1.44	-0.32	1.23
Δ log loans (Quarter)	-0.48	2	-0.55	1.66
Δ log loans (Year)	-1.45	3.33	-1.28	2.61
Exit rate (Month)	2.89	16.74	4.79	21.36
Exit rate (Quarter)	5.41	22.62	8.29	27.57
Exit rate (Year)	15.12	35.83	18.35	38.71
Relationship length (years)	5.37	3.42	4.25	2.11
Observations	233,402		233,402	

Table A.3. Descriptive Statistics by Event (QE)

Notes: The table shows descriptive statistics for banks, firm-bank and household-bank pairs used in the analysis. In Columns (1) and (2) of Table A.4a, statistics are weighted by the total number of customers (firms and households) in the analysis. In Columns (3) and (4) of Table A.4a, we use sample weights. Relationship length for households is censored to the left because the household credit register only starts in 2009. We use the month before each event as the reference date: January 2012 for PSI, June 2012 for OMT, and December 2014 for QE.

Dataset name	Description	Unit of information	Frequency
Bank Balance Sheet (BBS)	Balance sheet data for monetary and financial instititions	Non-consolidated financial institution	Monthly
Central Balance Sheet (CB)	Financial statement data for non-financial corporations	Firm	Annual
Central Credit Responsibility (CRC_EXP)	Credit register	Borrower-lender credit exposure	Monthly
Central Credit Responsibility (CRC_EXP), inquiries	Credit register inquiries	Borrower-lender credit inquiry	Real time
Eligible assets historical snapshot	List of eligible assets for monetary policy operations at the ECB	Security	Monthly
Estatísticas de Emissões de Títulos (SIET)	Securities register	Security-investor	Monthly
Monetary policy operations (MP)	List of ECB monetary policy operations	Operation-counterpart	Daily
Quantitative Easing operations (APP)	List of ECB QE purchases	Operation-counterpart	Daily
Sistema Interno de Avaliação de Crédito (SIAC)	Proprietary credit rating system from Banco de Portugal	Firm	Annual

Table A.4. List of Data Sets

Notes: This table lists the datasets used in the paper.

(a) Weighted

	$\frac{\Delta\text{ Exposed securities}}{\text{Total assets}}$								
	PSI			OMT			QE		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)	Month (7)	Quarter (8)	Year (9)
Exposure	0.001	0.002*	0.001	0.001	-0.0004	-0.003	-0.007*	-0.011***	-0.019**
	(0.001)	(0.001)	(0.002)	(0.0004)	(0.002)	(0.003)	(0.004)	(0.003)	(0.009)
Observations	44	44	44	44	44	44	44	44	44
R-squared	0.359	0.392	0.209	0.267	0.216	0.296	0.411	0.595	0.616

(b) Unweighted

	$\frac{\Delta\text{ Exposed securities}}{\text{Total assets}}$								
	PSI			OMT			QE		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)	Month (7)	Quarter (8)	Year (9)
Exposure	0.003***	0.003***	-0.010*	0.001	-0.009***	-0.013***	-0.029***	-0.034***	-0.020***
	(0.001)	(0.001)	(0.006)	(0.0004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.001)
Observations	44	44	44	44	44	44	44	44	44
R-squared	0.515	0.589	0.457	0.433	0.700	0.615	0.891	0.917	0.877

Table A.5. Robustness: Bank-level Exposed Security Holdings

Notes: The table reports estimates of coefficient β_1 from Equation 2. The dependent variable $\frac{\Delta\text{ Exposed securities}}{\text{Total assets}}$ is the change in the ratio of exposed securities over total assets one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total bank assets in the last month before the event. We use bank-level characteristics from Table A.3 as additional controls. In Table A.6a, observations are weighted by the number of bank customers (firms and households) in the sample. In Table A.6b, we use unit sample weights. Robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) Weighted

	Δ log net worth								
	PSI			OMT			QE		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)	Month (7)	Quarter (8)	Year (9)
Exposure	0.0866**	0.110**	0.182**	0.0779*	-0.00448	0.0615	0.00472***	0.0332**	0.0267
	(0.0400)	(0.0461)	(0.0705)	(0.0449)	(0.0329)	(0.0487)	(0.00134)	(0.0134)	(0.0247)
Observations	44	44	44	44	44	44	44	44	44
R-squared	0.378	0.410	0.442	0.472	0.485	0.501	0.140	0.486	0.510

(b) Unweighted

	Δ log net worth								
	PSI			OMT			QE		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)	Month (7)	Quarter (8)	Year (9)
Exposure	0.060*	0.094**	0.191***	0.008	0.028	0.082***	0.032*	0.038**	-0.032*
	(0.031)	(0.040)	(0.063)	(0.018)	(0.019)	(0.020)	(0.018)	(0.018)	(0.019)
Observations	44	44	44	44	44	44	44	44	44
R-squared	0.197	0.257	0.397	0.157	0.196	0.329	0.128	0.130	0.140

Table A.6. Robustness: Price Appreciation

Notes: The table reports estimates of coefficient β_1 from Equation 2. The dependent variable Δ log net worth is the change in log bank net worth induced by price appreciation one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total bank assets in the last month before the event. We use bank-level characteristics from Table A.3 as additional controls. In Table A.7a, observations are weighted by the number of bank customers (firms and households) in the sample. In Table A.7b, we use unit sample weights. Robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) Weighted

	Δ log net worth								
	PSI			OMT			QE		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)	Month (7)	Quarter (8)	Year (9)
Exposure	-0.004	0.002	0.001	0.002**	-0.077	-0.013	0.006*	0.014**	0.024***
	(0.062)	(0.061)	(0.072)	(0.001)	(0.047)	(0.053)	(0.003)	(0.005)	(0.006)
Observations	44	44	44	44	44	44	44	44	44
R-squared	0.298	0.333	0.330	0.150	0.459	0.424	0.275	0.528	0.618

(b) Unweighted

	Δ log net worth								
	PSI			OMT			QE		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)	Month (7)	Quarter (8)	Year (9)
Exposure	-0.016	-0.014	0.029	0.0004*	-0.012	0.013	0.028	0.042**	0.037**
	(0.024)	(0.024)	(0.027)	(0.0002)	(0.015)	(0.019)	(0.018)	(0.018)	(0.018)
Observations	44	44	44	44	44	44	44	44	44
R-squared	0.124	0.134	0.150	0.083	0.051	0.135	0.053	0.138	0.136

Table A.7. Robustness: Capital Gains

Notes: The table reports estimates of coefficient β_1 from Equation 2. The dependent variable Δ log net worth is the change in log bank net worth induced by capital gains one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total bank assets in the last month before the event. We use bank-level characteristics from Table A.3 as additional controls. In Table A.8a, observations are weighted by the number of bank customers (firms and households) in the sample. In Table A.8b, we use unit sample weights. Robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) PSI

	Δ log outstanding loans					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.014	0.031	0.109	0.001	0.014	0.042
	(0.013)	(0.024)	(0.083)	(0.013)	(0.021)	(0.036)
Observations	241,311	241,311	241,311	242,823	242,823	242,823
R-squared	0.401	0.411	0.433	0.435	0.441	0.461

(b) OMT

	Δ log outstanding loans					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.004	0.025	0.064	0.015	0.022	0.013
	(0.017)	(0.034)	(0.090)	(0.013)	(0.020)	(0.036)
Observations	233,402	233,402	233,402	231,840	231,840	231,840
R-squared	0.404	0.411	0.434	0.453	0.459	0.468

Table A.8. Robustness: Outstanding Loans (1/2)

(a) QE

	Δ log outstanding loans					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.051***	0.092***	0.336***	0.017*	0.026*	0.110***
	(0.010)	(0.015)	(0.084)	(0.009)	(0.014)	(0.018)
Observations	209,713	209,713	209,713	204,636	204,636	204,636
R-squared	0.404	0.415	0.426	0.456	0.467	0.469

Table A.9. Robustness: Outstanding Loans (2/2)

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Δ log outstanding loans is the change in one plus log outstanding loans one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. Errors are clustered at bank level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) PSI

	Δ log outstanding loans					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.0089** (0.003)	0.015*** (0.004)	0.034** (0.013)	-0.005 (0.005)	-0.002 (0.004)	-0.021 (0.013)
Observations	130,351	130,351	130,351	108,939	108,939	108,939
R-squared	0.395	0.405	0.405	0.457	0.458	0.47

(b) OMT

	Δ log outstanding loans					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.005 (0.003)	0.017*** (0.005)	0.019 (0.015)	-0.007 (0.008)	-0.016 (0.011)	-0.030*** (0.010)
Observations	130,351	130,351	130,351	108,939	108,939	108,939
R-squared	0.411	0.408	0.41	0.476	0.479	0.469

Table A.11. Robustness: Outstanding Loans (Closed Sample) (1/2)

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Δ log outstanding loans is the change in one plus log outstanding loans one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. We restrict the sample to borrowers who have credit relationships with banks in the three events. Errors are clustered at bank level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) QE

	Δ log outstanding loans					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.075***	0.129***	0.518***	0.023***	0.039***	0.182***
	(0.018)	(0.023)	(0.129)	(0.007)	(0.013)	(0.030)
Observations	130,351	130,351	130,351	108,939	108,939	108,939
R-squared	0.433	0.443	0.455	0.471	0.478	0.484

Table A.11. Robustness: Outstanding Loans (Closed Sample) (2/2)

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Δ log outstanding loans is the change in one plus log outstanding loans one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. We restrict the sample to borrowers who have credit relationships with banks in the three events. Errors are clustered at bank level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) PSI

	Relationship ends = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	-0.001	-0.003	-0.010	0.001	-0.0002	-0.003
	(0.001)	(0.002)	(0.008)	(0.002)	(0.003)	(0.006)
Observations	241,311	241,311	241,311	242,823	242,823	242,823
R-squared	0.405	0.409	0.427	0.4413	0.4442	0.4667

(b) OMT

	Relationship ends = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.0003	-0.001	-0.005	-0.002	-0.002	0.0004
	(0.002)	(0.004)	(0.009)	(0.002)	(0.003)	(0.005)
Observations	233,402	233,402	233,402	231,840	231,840	231,840
R-squared	0.407	0.413	0.43	0.4523	0.4615	0.4741

Table A.12. Robustness: Termination of Credit Relationships (1/2)

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Relationship ends = 1 is an indicator variable that is equal to 1 if the borrower does not have a credit relationship with the bank one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. Standard errors are clustered at the bank-level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) QE

	Relationship ends = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	-0.006***	-0.010***	-0.039***	-0.003	-0.005**	-0.019***
	(0.001)	(0.002)	(0.008)	(0.002)	(0.002)	(0.002)
Observations	209,713	209,713	209,713	204,636	204,636	204,636
R-squared	0.408	0.418	0.43	0.4569	0.4656	0.477

Table A.13. Robustness: Termination of Credit Relationships (2/2)

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Relationship ends = 1 is an indicator variable that is equal to 1 if the borrower does not have a credit relationship with the bank one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. Standard errors are clustered at the bank-level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) PSI

	Relationship ends = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	-0.002	-0.003	-0.012	0.003	0.003	0.001
	(0.001)	(0.002)	(0.007)	(0.002)	(0.004)	(0.008)
Observations	315,187	315,187	315,187	438,359	438,359	438,359
R-squared	0.011	0.012	0.017	0.02	0.035	0.072

(b) OMT

	Relationship ends = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.0001	-0.001	-0.006	-0.0002	0.0008	0.004
	(0.002)	(0.004)	(0.010)	(0.003)	(0.004)	(0.005)
Observations	307,550	307,550	307,550	427,904	427,904	427,904
R-squared	0.011	0.012	0.016	0.026	0.045	0.079

Table A.14. Robustness: Relationship Terminations Without Borrower FE (1/2)

Notes: The table shows coefficients from estimating Equation 5, excluding the borrower fixed effect θ_j. The dependent variable Relationship ends = 1 is an indicator variable that is equal to 1 if the borrower does not have a credit relationship with the bank one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. For firms (Columns 1 to 3), we use 1 plus the logarithm of firm assets and firm workers, EBITDA/Assets, equity ratio, and an indicator variable that is equal to 1 if the firm has 90-day overdue loans as additional control variables. We replace values by 0 whenever balance sheet data is missing. Errors are clustered at bank level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) QE

	Relationship ends = 1					
	Firms			Households		
	Month	Quarter	Year	Month	Quarter	Year
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.007***	-0.013***	-0.030***	-0.003	-0.005**	-0.017***
	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.003)
Observations	266,990	266,990	266,990	401,652	401,652	401,652
R-squared	0.007	0.009	0.021	0.021	0.036	0.062

Table A.15. Robustness: Relationship Terminations Without Borrower FE (2/2)

Notes: The table shows coefficients from estimating Equation 5, excluding the borrower fixed effect θ_j. The dependent variable Relationship ends = 1 is an indicator variable that is equal to 1 if the borrower does not have a credit relationship with the bank one month, one quarter, and one year after the event. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. For firms (Columns 1 to 3), we use 1 plus the logarithm of firm assets and firm workers, EBITDA/Assets, equity ratio, and an indicator variable that is equal to 1 if the firm has 90-day overdue loans as additional control variables. We replace values by 0 whenever balance sheet data is missing. Errors are clustered at bank level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) PSI

	Accepted application = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.043***	0.022	0.022**	-0.021	-0.007	-0.002
	(0.015)	(0.015)	(0.011)	(0.014)	(0.018)	(0.006)
Observations	4,470	3,573	14,672	3,258	3,116	13,051
R-squared	0.432	0.449	0.450	0.445	0.457	0.456

(b) OMT

	Accepted application = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.011	-0.004	-0.001	-0.024**	-0.005	-0.011
	(0.021)	(0.014)	(0.013)	(0.011)	(0.012)	(0.011)
Observations	2,589	3,420	16,279	2,908	3,096	13,477
R-squared	0.449	0.453	0.459	0.432	0.471	0.454

Table A.16. Robustness: Loan Applications (1/2)

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Accepted application = 1 is an indicator variable that is equal to 1 if the loan application is accepted. *Month*, *Quarter*, and *Year* report coefficients for applications 0-1, 2-3, and 4-12 months after the event, respectively. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. Errors are clustered at bank level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) QE

	Accepted application = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.037***	0.024***	0.036***	0.012	-0.006	0.009
	(0.008)	(0.008)	(0.009)	(0.018)	(0.014)	(0.014)
Observations	6,549	5,653	19,859	3,352	3,742	16,538
R-squared	0.492	0.495	0.456	0.454	0.451	0.461

Table A.17. Robustness: Loan Applications (2/2)

Notes: The table shows coefficients from estimating Equation 5. The dependent variable Accepted application = 1 is an indicator variable that is equal to 1 if the loan application is accepted. *Month*, *Quarter*, and *Year* report coefficients for applications 0-1, 2-3, and 4-12 months after the event, respectively. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. Errors are clustered at bank level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) PSI

	Accepted application = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.027***	0.018*	0.017*	-0.012	0.001	-0.010
	(0.009)	(0.010)	(0.009)	(0.025)	(0.018)	(0.024)
Observations	30,336	27,598	110,587	26,058	25,033	103,445
R-squared	0.047	0.053	0.042	0.081	0.091	0.125

(b) OMT

	Accepted application = 1					
	Firms			Households		
	Month (1)	Quarter (2)	Year (3)	Month (4)	Quarter (5)	Year (6)
Exposure	0.003	0.001	0.000	-0.046	-0.041	-0.027
	(0.016)	(0.009)	(0.012)	(0.033)	(0.034)	(0.022)
Observations	21,426	25,225	119,448	22,363	23,860	106,456
R-squared	0.049	0.041	0.033	0.119	0.147	0.131

Table A.18. Robustness: Loan Applications Without Borrower FE (1/2)

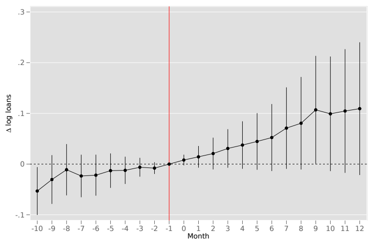
Notes: The table shows coefficients from estimating Equation 5, excluding the borrower fixed effect θ_j. The dependent variable Accepted application = 1 is an indicator variable that is equal to 1 if the loan application is accepted. *Month*, *Quarter*, and *Year* report coefficients for applications 0-1, 2-3, and 4-12 months after the event, respectively. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. For firms (Columns 1 to 3), we use 1 plus the logarithm of firm assets and firm workers, EBITDA/Assets, equity ratio, and an indicator variable that is equal to 1 if the firm has 90-day overdue loans as additional control variables. We replace values by 0 whenever balance sheet data is missing. Errors are clustered at bank level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

(a) QE

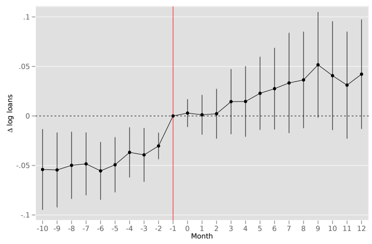
	Accepted application = 1					
	Firms			Households		
	Month	Quarter	Year	Month	Quarter	Year
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	0.015	0.009	0.015	-0.024	-0.021	-0.010
	(0.013)	(0.012)	(0.012)	(0.026)	(0.022)	(0.024)
Observations	44,770	37,950	138,439	23,474	24,879	109,917
R-squared	0.036	0.030	0.042	0.105	0.076	0.086

Table A.19. Robustness: Loan Applications Without Borrower FE (2/2)

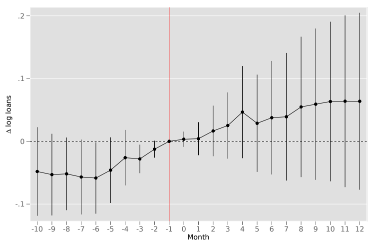
Notes: The table shows coefficients from estimating Equation 5, excluding the borrower fixed effect θ_j. The dependent variable Accepted application = 1 is an indicator variable that is equal to 1 if the loan application is accepted. *Month*, *Quarter*, and *Year* report coefficients for applications 0-1, 2-3, and 4-12 months after the event, respectively. The independent variable Exposure is the standardized ratio of exposed securities to total assets, measured in the last month before the event. We control for bank characteristics with variables from Table 1. For firms (Columns 1 to 3), we use 1 plus the logarithm of firm assets and firm workers, EBITDA/Assets, equity ratio, and an indicator variable that is equal to 1 if the firm has 90-day overdue loans as additional control variables. We replace values by 0 whenever balance sheet data is missing. Errors are clustered at bank level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.



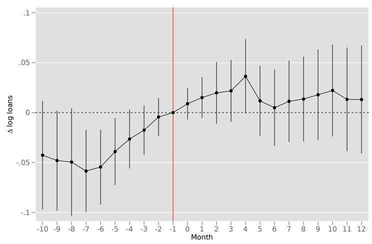
(a) PSI: Firms



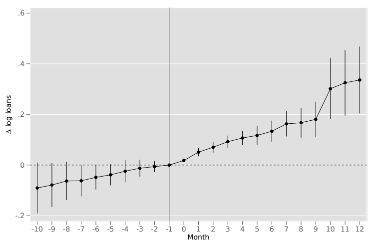
(b) PSI: Households



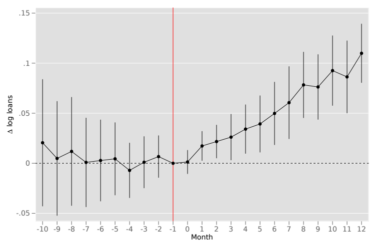
(c) OMT: Firms



(d) OMT: Households



(e) QE: Firms



(f) QE: Households

Note: Each panel of the figure shows the results from a dynamic difference-in-difference regression at the borrower-bank level around a sovereign debt event. The dependent variable is log(1+loan volume). We show coefficients on the Exposure variable, which is the standardized ratio of exposed securities to total assets, measured in the last month before the event. The left panels show results for non-financial borrowers while the right panels show results for households. The regression include bank and firm-time fixed effects. The regression specification for PSI includes a linear time trend. double clustered at the bank level and at the firm level

Figure A.1: Borrower-Bank Regression Results: Intensive Margin

Appendix B: Effect of PSI on Portuguese bond yields

In this section we measure the effect of the Greek debt restructuring (PSI) on Portuguese government bond yields. We follow a methodology that is similar to the one used by Krishnamurthy *et al.* (2018) to measure the impact of OMT on yields of government bonds in periphery Euro Area countries.

There was a series of events that signalled an increase in the probability of a successful debt restructuring between February and April 2012. We obtain a list of these events using Factiva. We search for all news between February 2012 and April 2012 that include the terms "Greece" and "PSI". We filter the results to include only news from Reuters. Using this procedure, in Table B.1 we obtain a list of news articles that indicate an increase in the probability of a successful Greek debt restructuring.

Date	Title	Summary
4/25/2012	Greece says final participation rate in bond swap is 96.9 pct	Greek bond swap completed with 96.9% participation rate
3/9/2012	Gilts dip as Greek deal confirmed, eye UK data	A sufficient number of private creditors agreed on PSI conditions for a successful deal (86% participation rate)
3/8/2012	EURO GOVT-Italian, Spanish yields fall on Greek hopes	Yields of periphery countries went down with increased optimism of a successful Greek debt swap
2/21/2012	ANNOUNCEMENT-Greece launches debt swap plan (PSI)	Greece announced the final terms of the debt swap program
2/20/2012	Deal near to lower Greek debt-Greek finmin source	European countries appeared to agree on credit conditions to finance the Greek debt swap
2/13/2012	Greek bond swap seen wrapped up in March- govt	The Greek government announced that the Greek debt swap would be completed in March 2012
2/7/2012	EURO GOVT-Bunds slide as markets anticipate Greek deal	Yields of periphery countries went down with positive signs of a Greek debt swap deal
2/5/2012	France says Greek PSI talks going "relatively well"	French finance minister said talks on the Greek debt swap were moving relatively well

Table B.1. List of PSI News

Notes: The table lists news between February and April 2012 that were associated with an increase in the probability of a successful Greek debt restructuring. We obtain this list using the procedure described in Appendix B

In Table B.2 we estimate the following equation:

$$\Delta yield_t = \beta \mathbb{1}_t^{PSI} + \varepsilon_t \tag{B.1}$$

where $\Delta yield_t$ is the change in yield between day $t-1$'s close and day t's close, and $\mathbb{1}_t^{PSI}$ is an indicator equal to 1 on the day of the news reported in Table B.1 and on the subsequent working day. We retrieve yields for the 2-year, 5-year, and 10-year reference Portuguese government bonds from MarketWatch.

		Δ yield (bps)	
Variable	(1)	(2)	(3)
PSI event = 1	-34.625***	-19.267**	-13.108**
	(11.163)	(8.001)	(5.384)
Cumulative effect (bps)	-415.500	-231.204	-157.296
N	269	269	269
R-squared	0.026	0.009	0.014

Table B.2. Regression: Effect of PSI News on Yield Changes

Notes: The table shows coefficients from estimating Equation B.1 using yields of the reference 2-year, 5-year, and 10-year Portuguese government bonds. We retrieve yields measured at each day's close from MarketWatch for all days with available data in 2012. *Cumulative effect (bps)* measures the sum of daily yield changes for all days in which the indicator variable associated with the PSI event is equal to 1. We display heteroskedasticity-robust standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

On average, Portuguese bond yields fall on days with positive news about the Greek debt restructuring. We compute the cumulative change in yields on days with positive news about PSI. Yields fall by about 420 bps at the 2-year maturity, 230 bps at the 5-year maturity, and 160 bps at the 10-year maturity. These yield drops are larger than the ones reported by Krishnamurthy *et al.* (2018) for the OMT event (74 bps for the 2-year bond, 152 for the 5-year bond, 118 bps for the 10-year bond).

Appendix C: Data processing

The credit register identifies banks with bank codes attributed by Banco de Portugal. We tabulate the number of credit relationships held by each bank to verify if there are breaks in the number of reported credit relationships. Whenever a large number of credit relationships move from one bank code to another, we assume that both bank codes belong to the same bank. However, when credit relationships move to another bank code that already has a large number of credit relationships, we assume that the first bank was acquired and drop it. We also drop credit relationships when there is a sudden increase or decrease in the number of credit relationships for a given bank code and we cannot track these relationships in a new bank code.

We analyze reporting gaps by aggregating credit exposures using the following variables: bank code, responsibility level (e.g., whether it is a joint credit exposure), maturity, and product. We verify whether there are 1 to 2 month reporting gaps for these exposures. We replace the missing exposure by the exposure in the first month after the reporting gap in such cases.

In the securities register, we aggregate exposures using ISINs and bank codes. We fill one month exposure gaps with the average of the exposures in the surrounding months.

Some banks report at the individual level but belong to a consolidated bank. We aggregate data at the consolidated bank in such cases. We assume that absolute term changes of monthly bank variables expressed as stocks (e.g., total assets) greater than 10 p.p. and reporting gaps for banks that keep reporting in at least one dataset are driven by changes in reporting standards. We remove the effect of sudden changes and fill reporting gaps with the last observed value in such cases.

We define the bank sample as the group of institutions that report non-zero total assets in the Bank Balance Sheet (BBS) database for all periods between April 2010 and January 2016.

Working Papers

2018

1|18 Calibration and the estimation of macroeconomic models
Nikolay Iskrev

2|18 Are asset price data informative about news shocks? A DSGE perspective
Nikolay Iskrev

3|18 Sub-optimality of the friedman rule with distorting taxes
Bernardino Adão | André C. Silva

4|18 The effect of firm cash holdings on monetary policy
Bernardino Adão | André C. Silva

5|18 The returns to schooling unveiled
Ana Rute Cardoso | Paulo Guimarães | Pedro Portugal | Hugo Reis

6|18 Real effects of financial distress: the role of heterogeneity
Francisco Buera | Sudipto Karmakar

7|18 Did recent reforms facilitate EU labour market adjustment? Firm level evidence
Mario Izquierdo | Theodora Kosma | Ana Lamo | Fernando Martins | Simon Savsek

8|18 Flexible wage components as a source of wage adaptability to shocks: evidence from European firms, 2010–2013
Jan Babecký | Clémence Berson | Ludmila Fadejeva | Ana Lamo | Petra Marotzke | Fernando Martins | Pawel Strzelecki

9|18 The effects of official and unofficial information on tax compliance
Filomena Garcia | Luca David Opromolla Andrea Vezulli | Rafael Marques

10|18 International trade in services: evidence for portuguese firms
João Amador | Sónia Cabral | Birgitte Ringstad

11|18 Fear the walking dead: zombie firms, spillovers and exit barriers
Ana Fontoura Gouveia | Christian Osterhold

12|18 Collateral Damage? Labour Market Effects of Competing with China – at Home and Abroad
Sónia Cabral | Pedro S. Martins | João Pereira dos Santos | Mariana Tavares

13|18 An integrated financial amplifier: The role of defaulted loans and occasionally binding constraints in output fluctuations
Paulo Júlio | José R. Maria

14|18 Structural Changes in the Duration of Bull Markets and Business Cycle Dynamics
João Cruz | João Nicolau | Paulo M.M. Rodrigues

15|18 Cross-border spillovers of monetary policy: what changes during a financial crisis?
Luciana Barbosa | Diana Bonfim | Sónia Costa | Mary Everett

16|18 When losses turn into loans: the cost of undercapitalized banks
Laura Blattner | Luísa Farinha | Francisca Rebelo

17|18 Testing the fractionally integrated hypothesis using M estimation: With an application to stock market volatility
Matei Demetrescu | Paulo M. M. Rodrigues | Antonio Rubia

18|18 Every cloud has a silver lining: Micro-level evidence on the cleansing effects of the Portuguese financial crisis
Daniel A. Dias | Carlos Robalo Marques

19|18 To ask or not to ask? Collateral versus screening in lending relationships
Hans Degryse | Artashes Karapetyan | Sudipto Karmakar

20|18 Thirty years of economic growth in Africa
João Amador | António R. dos Santos

21|18 CEO performance in severe crises: the role of newcomers
Sharmin Sazedj | João Amador | José Tavares

22|18 A general equilibrium theory of occupational choice under optimistic beliefs about entrepreneurial ability
Michele Dell'Era | Luca David Opromolla | Luís Santos-Pinto

23|18 Exploring the implications of different loan-to-value macroprudential policy designs
Rita Basto | Sandra Gomes | Diana Lima

24|18 Bank shocks and firm performance: new evidence from the sovereign debt crisis
Luísa Farinha | Marina-Eliza Spaliara | Serafem Tsoukas

25|18 Bank credit allocation and productivity: stylised facts for Portugal
Nuno Azevedo | Márcio Mateus | Álvaro Pina

26|18 Does domestic demand matter for firms' exports?
Paulo Soares Esteves | Miguel Portela | António Rua

27|18 Credit Subsidies
Isabel Correia | Fiorella De Fiore | Pedro Teles | Oreste Tristani

2019

1|19 The transmission of unconventional monetary policy to bank credit supply: evidence from the TLTRO
António Afonso | Joana Sousa-Leite

2|19 How responsive are wages to demand within the firm? Evidence from idiosyncratic export demand shocks
Andrew Garin | Filipe Silvério

3|19 Vocational high school graduate wage gap: the role of cognitive skills and firms
Joop Hartog | Pedro Raposo | Hugo Reis

4|19 What is the Impact of Increased Business Competition?
Sónia Félix | Chiara Maggi

5|19 Modelling the Demand for Euro Banknotes
António Rua

6|19 Testing for Episodic Predictability in Stock Returns
Matei Demetrescu | Iliyan Georgiev
Paulo M. M. Rodrigues | A. M. Robert Taylor

7|19 The new ESCB methodology for the calculation of cyclically adjusted budget balances: an application to the Portuguese case
Cláudia Braz | Maria Manuel Campos
Sharmin Sazedj

2020

1|20 On-site inspecting zombie lending
Diana Bonfim | Geraldo Cerqueiro | Hans Degryse | Steven Ongena

2|20 Labor earnings dynamics in a developing economy with a large informal sector
Diego B. P. Gomes | Felipe S. Iachan | Cezar Santos

3|20 Endogenous growth and monetary policy: how do interest-rate feedback rules shape nominal and real transitional dynamics?
Pedro Mazeda Gil | Gustavo Iglésias

4|20 Types of International Traders and the Network of Capital Participations
João Amador | Sónia Cabral | Birgitte Ringstad

5|20 Forecasting tourism with targeted predictors in a data-rich environment
Nuno Lourenço | Carlos Melo Gouveia | António Rua

6|20 The expected time to cross a threshold and its determinants: A simple and flexible framework
Gabriel Zsurkis | João Nicolau | Paulo M. M. Rodrigues

7|20 A non-hierarchical dynamic factor model for three-way data
Francisco Dias | Maximiano Pinheiro | António Rua

8|20 Measuring wage inequality under right censoring
João Nicolau | Pedro Raposo | Paulo M. M. Rodrigues

9|20 Intergenerational wealth inequality: the role of demographics
António Antunes | Valerio Ercolani

10|20 Banks' complexity and risk: agency problems and diversification benefits
Diana Bonfim | Sónia Felix

11|20 The importance of deposit insurance credibility
Diana Bonfim | João A. C. Santos

12|20 Dream jobs
Giordano Mion | Luca David Opromolla | Gianmarco I.P. Ottaviano

13|20 The DEI: tracking economic activity daily during the lockdown
Nuno Lourenço | António Rua

14|20 An economic model of the Covid-19 pandemic with young and old agents: Behavior, testing and policies
Luiz Brotherhood | Philipp Kircher | Cezar Santos | Michèle Tertilt

15|20 Slums and Pandemics
Luiz Brotherhood | Tiago Cavalcanti | Daniel Da Mata | Cezar Santos

16|20 Assessing the Scoreboard of the EU Macroeconomic Imbalances Procedure: (Machine) Learning from Decisions
Tiago Alves | João Amador | Francisco Gonçalves

17|20 Climate Change Mitigation Policies: Aggregate and Distributional Effects
Tiago Cavalcanti | Zeina Hasna | Cezar Santos

18|20 Heterogeneous response of consumers to income shocks throughout a financial assistance program
Nuno Alves | Fátima Cardoso | Manuel Coutinho Pereira

19|20 To change or not to change: the impact of the law on mortgage origination
Ana Isabel Sá

2021

1|21 Optimal Social Insurance: Insights from a Continuous-Time Stochastic Setup
João Amador | Pedro G. Rodrigues

2|21 Multivariate Fractional Integration Tests allowing for Conditional Heteroskedasticity withan Application to Return Volatility and Trading
Marina Balboa | Paulo M. M. Rodrigues Antonio Rubia | A. M. Robert Taylor

3|21 The Role of Macroprudential Policy in Times of Trouble
Jagjit S. Chadha | Germana Corrado | Luisa Corrado | Ivan De Lorenzo Buratta

4|21 Extensions to IVX Methodsnof Inference for Return Predictability
Matei Demetrescu | Iliyan Georgiev | Paulo M. M. Rodrigues | A.M. Robert Taylor

5|21 Spectral decomposition of the information about latent variables in dynamic macroeconomic models
Nikolay Iskrev

6|21 Institutional Arrangements and Inflation Bias: A Dynamic Heterogeneous Panel Approach
Vasco Gabriel | Ioannis Lazopoulos | Diana Lima

7|21 Assessment of the effectiveness of the macroprudential measures implemented in the context of the Covid-19 pandemic
Lucas Avezum | Vítor Oliveiral | Diogo Serra

8|21 Risk shocks, due loans, and policy options: When less is more!
Paulo Júlio | José R. Maria | Sílvia Santos

9|21 Sovereign-Bank Diabolic Loop: The Government Procurement Channel!
Diana Bonfim | Miguel A. Ferreira | Francisco Queiró | Sujiao Zhao

10|21 Assessing the effectiveness of the Portuguese borrower-based measure in the Covid-19 context
Katja Neugebauer | Vítor Oliveira | Ângelo Ramos

11|21 Scrapping, Renewable Technology Adoption, and Growth
Bernardino Adão | Borghan Narajabad | Ted Temzelides

12|21 The Persistence of Wages
Anabela Carneiro | Pedro Portugal | Pedro Raposo | Paulo M.M. Rodrigues

13|21 Serial Entrepreneurs, the Macroeconomy and top income inequality
Sónia Félix | Sudipto Karmakar | Petr Sedláček

14|21 COVID-19, Lockdowns and International Trade: Evidence from Firm-Level Data
João Amador | Carlos Melo Gouveia | Ana Catarina Pimenta

15|21 The sensitivity of SME's investment and employment to the cost of debt financing
Diana Bonfim | Cláudia Custódio | Clara Raposo

16|21 The impact of a macroprudential borrower based measure on households' leverage and housing choices
Daniel Abreu | Sónia Félix | Vítor Oliveira | Fátima Silva

17|21 Permanent and temporary monetary policy shocks and the dynamics of exchange rates
Alexandre Carvalho | João Valle e Azevedo | Pedro Pires Ribeiro

18|21 On the Cleansing Effect of Recessions and Government Policy: Evidence from Covid-19
Nicholas Kozeniauskas | Pedro Moreira | Cezar Santos