Quantitative Easing, Bank Lending, and Regulation*

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Abstract

We study how the European Central Bank's quantitative easing (QE) program announced in January 2015 affected lending by Italian banks, and how the transmission channel is impacted by the regulatory settings that determine banks' valuation of their security holdings. We find that QE affects bank lending only through banks' holding of QE-eligible securities that are marked-to-market, and no effects on lending arise through banks' holdings of the same securities that are valued at historical cost. Because in 2015 only securities held in the trading book had to be marked-to-market, and because trading book holdings are small, we find a very limited impact of QE on bank lending. In addition, we find evidence of substitution. Branches of non-QE-exposed banks contracted lending in areas where QE-exposed banks dominate, reducing further the effectiveness of the policy. We also exploit a revision of the regulatory framework in 2018 requiring banks to mark-to-market a larger fraction of their securities, showing that a restart of QE in 2019 – after a pause – generated much bigger effects on bank lending.

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1. Introduction

This paper studies how accounting rules, through their interaction with capital regulations, affect the transmission of central bank quantitative easing (QE) programs on to bank lending. We study these issues using Italian credit registry data around the 2015 ECB's Public Sector Purchase Program (PSPP)—the ECB's largest QE intervention, with planned monthly purchases of €50 billion in Euro area sovereign debt.[1] Theoretical arguments predict that because central bank asset purchases can increase the price of the assets targeted for purchase, QE can improve the net worth of those banks with large balance sheet exposure to the targeted assets, thereby augmenting exposed banks' lending capacity during periods of financial distress—the recapitalization channel. Another important channel emphasized in the theoretical literature centers on liquidity.[2] By reducing the liquidity premium on assets included in the purchase program, QE gives banks the option to liquidate their holdings of these assets with little adverse price impact in order to meet loan demand.

However, whether QE actually augments banks' lending capacity through these theoretical mechanisms can depend on the accounting system used in capital regulation. Capital regulation typically uses mark-to-market accounting (MMA) to value assets that are available for sale (AFS), and historical cost accounting (HCA) to value assets that are classified as hold-to-maturity (see Figure 1). The choice of accounting system during periods of financial distress is complex. MMA can provide investors and regulators with timely information about a bank's risk profile. But when

[1] See for example: https://www.wsj.com/articles/suddenly-qe-becomes-flavor-of-the-month-for-the-eurozone-1429129592. At the time of the announcement, the ECB noted that: "the sizable purchase volume of the PSPP will contribute to achieving the underlying monetary policy objective of inducing financial intermediaries to increase their provision of liquidity to the interbank market and credit to the euro area economy." EUR-Lex, "Decision (EU) 2015/774 of the European Central Bank of 4 March 2015 on a secondary markets public sector asset purchase programme (ECB/2015/10)," Official Journal of the European Union, May 14, 2015, https://eur-lex.europa.eu/legal-content/EN/TXT/?uri=CELEX:32015D0010.

[2] (Gertler and Kiyotaki 2011, Brunnermeier and Sannikov 2014) elaborate on this recapitalization channel, while (Holmstrom and Tirole 1997, Gertler and Kiyotaki 2015, Drechsler, Schnabl et al. 2018) develop arguments centered around the liquidity channel.

asset prices are temporarily dislocated, MMA can also erode a bank's capital base and impair its lending capacity, leading to credit crunches and asset fire sales.[3]

Thus, during periods of financial distress, regulators sometimes shift towards historical cost accounting (HCA) in order to preserve the solvency of financial institutions and maintain the supply of credit. In our setting, because of European banking systems' large exposure to European sovereign debt, the EU revised its capital regulations in order to value a significant fraction of the sovereign assets held on banks' balance sheets at historical cost in 2010—when European sovereign debt markets were illiquid and volatile;[4] Italian banks adopted these regulations in 2010 as well.[5] That is, the revised regulations allowed Italian banks to record at historical cost even those sovereign assets ostensibly classified as available for sale (AFS) in the banking book and previously valued at market prices.

While this regulatory shift to HCA may have insulated much of banks' balance sheets from short-term fluctuations in sovereign assets, it can also limit the pass-through of QE programs like the subsequent 2015 PSPP onto loan supply. That is, the new regulations now valued all sovereign assets in the banking book at historical cost. Thus, banks could not record an increase in their regulatory capital from any PSPP-induced rise in the value of these AFS banking book assets, and increase lending: HCA can limit the recapitalization channel of QE. In the case of the liquidity channel, if a bank had unrealized losses on eligible AFS securities, it would still have no incentive to sell them in order to raise cash and make new loans – if the bank did sell them in order to make new loans, the realized losses would now have to be recorded against regulatory capital, making it more attractive in many cases for the bank to retain these securities and avoid realized losses.

Therefore, this interaction between accounting systems and capital regulations suggests that direct exposure to the PSPP is likely to emerge mainly from sovereign assets held in the trading

[3] See (Allen and Carletti 2008, Plantin, Sapra et al. 2008) for models that discuss the potential costs of MMA in bank capital regulation, while (Ellul, Jotikasthira et al. 2015) provide evidence on some of the potential drawbacks of HC accounting in the context of the US insurance industry.

[4] EUR-Lex, "Regulation (EU) No 575/2013 of the European Parliament and of the Council of 26 June 2013 on prudential requirements for credit institutions and investment firms and amending Regulation (EU) No 648/2012," Official Journal of the European Union, June 27, 2013, https://eur-lex.europa.eu/legal-content/EN/TXT/?uri=CELEX%3A32013R0575 (page 508, Article 467).

[5] See https://www.bancaditalia.it/pubblicazioni/bollettino-vigilanza/2010-05/20100518_II5.pdf

book—where regulations still allowed mark-to-market accounting (MMA). This segmentation in balance sheet exposure across the banking system means that only some banks might increase lending on account of the PSPP. Moreover, because of substitution, the overall PSPP loan supply response in the economy can be small or zero. That is, exposed banks might expand lending and potentially gain market share from the non-exposed banks if both types of banks compete in the same local markets (Allen and Gale 2004). Credible tests of the role of accounting systems in shaping the pass-through of QE onto bank lending require both detailed supervisory data on where banks actually assign targeted securities on their balance sheet, as well as high frequency credit registry-level data to address latent demand and possible contamination from other ongoing central bank policy interventions.

We use such data within a monthly difference-in-difference firm-bank level research design that absorb demand through firm by year-month fixed effects. Using an exposure measure that ignores the variation in the use HCA and MMA across the balance sheet—constructed simply as all sovereign assets eligible to be purchased by the ECB to total assets—we find no evidence that the PSPP increased lending at more exposed banks in the months immediately afterwards relative to otherwise.[6] However, when we identify exposure separately based on which sovereign assets are valued at HCA versus MMA for the purposes of capital regulation, the evidence suggests that banks exposed to the PSPP through their holdings of sovereign assets in the trading book—and thus subject to MMA—increased lending at the intensive margin almost immediately after the PSPP relative to otherwise.

However, because relatively few banks had significant exposure via their trading book, the magnitude of the intensive margin lending response is relatively small at about €145 million—this

[6] The eligibility criteria for an asset's purchase under the PSPP is in the January 22, 2015 press release https://www.ecb.europa.eu/press/pr/date/2015/html/pr150122_1.en.html. With respect to supranational institutions, the technical annex to the January 2015 statement stated that eligible securities included those issued by "certain agencies established in the euro area or certain international or supranational institutions located in the euro area." Because the exact criteria for agencies or international institutions were not fully clarified at the time, we consider these eligibility criteria to have been met if an international or supranational institution has its main headquarters in a euro area country. Additional eligibility criteria included the following: the security had to be euro denominated; it had to fulfill the collateral eligibility criteria for the ECB; and finally, the security's credit rating had to be sufficiently high: Eligible securities must have a credit rating of at least CQS3 (i.e., Credit Quality Step 3). This corresponds to at least BBB- for S&P's and Fitch, and Baa3 for Moody's.

despite some €50 billion a month in asset purchases. Moreover, much of the lending response among exposed banks was concentrated among those with relatively illiquid balance sheets. Note that these results are stable across myriad of robustness checks, including one that uses 23 million firm-bank observations. Also, if these results on loan quantities reflect latent demand, then the effects of the PSPP on interest rates should be non-negative. Using new data on interest rates, we document that exposed bank increased lending by offering lower interest rates to firms. Controlling for posted collateral and non-parametrically for firm-level demand, the average interest rate on loans decline by about 36 basis points among exposed banks in the months immediately following the PSPP relative to otherwise.

Rather than reflecting the interaction of accounting rules and capital regulations on the transmission of QE, the relatively narrow and small effect of the PSPP might reflect endogenous matching between a subset of banks and firms. To address this concern, we capitalize on the pause and resumption of the PSPP, and a reversion in capital regulation towards MMA accounting for AFS sovereign assets in the banking book. Specifically, with increased stability in debt markets, in 2018 the EU reverted to requiring MMA accounting for these assets. In addition, the ECB suspended its asset purchase program at the end of December 2018 and, then, announced on September 12, 2019 a resumption on November 1 of that year. Strikingly, exposure calculated as the sum of QE-eligible securities classified as AFS and those hold in the trading book—now all subject to MMA—is associated with a much bigger increase in lending at the intensive margin in the months immediately the PSPP's resumption. Note that the same exact exposure design in 2015, when AFS assets were valued at HC, suggest no lending response to the PSPP.

We also use the credit registry data to study the impact of the PSPP at the extensive margin—whether PSPP exposed banks were more likely to form new credit relationships. We again use firm-by-year-month fixed effects to address endogenous matching. We find that the probability that a credit application is successful increases by 4.6 percentage points in the months after the PSPP announcement among the PSPP-exposed banks relative to otherwise. However, in the aggregate, we find that this expansion in credit supply among PSPP-exposed banks occurs at the expense of non-exposed local competitors.

In particular, because banking competition in Italy occurs locally, we aggregate the data up to the province-branch level. We find that non-exposed banks decrease lending in the months after the PSPP when they face competition from PSPP-exposed banks. For example, in a province where PSPP- exposed banks account for loan market share at the 75th percentile, lending growth of non-exposed banks declined by about 0.3 percentage points. Existing credit relationships can be sticky, and much of this substitution occurs at the extensive margin. To wit, when the PSPP was announced, branches of non-exposed banks contracted lending to new customers in provinces in which their direct competitors' lending capacity was augmented by the PSPP; there is no such decline in lending to existing customers.

Taken together, the evidence in this paper suggests that the 2015 PSPP likely led to very small increases in lending at exposed banks at both the intensive and extensive margins, especially among the more illiquid subsample of banks. In part, the relatively small economic magnitudes reflect the fact that much of the sovereign asset exposure of Italian banks was in the banking book, where the regulatory framework limited the pass-through of the PSPP. Moreover, because of substitution from non-PSPP-exposed to PSPP-exposed banks, the program itself may have had little aggregate effect on expanding the supply of bank credit. An implication of these results is that asset purchase programs that target a broader segment of the banking system might lead to larger lending responses in the aggregate. That is, accounting systems and capital regulations can interact to shape the transmission of these policies. These results can inform theoretical models that study the tradeoffs between HCA and MMA and their potential impact on central bank policies that seek to remediate periods of financial distress.

We build on a large literature and Section 2 places our contribution in context. Section 3 describes the data, while Section 4 presents the main results. Section 5 examines substitution at the province and firm-level Section 6 concludes.

2. Related literature and contribution

To our knowledge, ours is the first paper to study how accounting rules, through their interaction with capital regulations, can affect the transmission of QE on to bank lending using credit-registry data. We however build on the growing literature on QE, the broader literature on the bank lending channel of monetary policy, and research on HCA versus MMA for financial institutions. In the case of the PSPP, previous studies have focused on the asset price responses to the program's announcement. Notably, the evidence in (Andrade, Breckenfelder et al. 2016) and (Altavilla, Carboni et al. 2015) suggest that the PSPP's announcement led to a drop in European sovereign yields, and an increase in asset prices, including the stock price of banks most exposed to the PSPP. Extrapolating from this asset price response, the model based results in (Andrade, Breckenfelder et al. 2016) suggest that the PSPP led to an increase in output in part through the bank lending channel. However, the actual transmission of these policies onto bank loan supply, and the overall efficacy of QE remains an open question—see the survey in (Jancokova, Kempf et al. 2020). Our evidence suggests that at least in the Italian context, the effects of the PSPP might have been muted.

The evidence in (Peydró, Polo et al. 2021) observe that the transmission of monetary policy onto bank lending can be impaired if banks hoard liquidity or engage in securities trading instead of lending—see also (Abbassi, Iyer et al. 2016) in the case of German data. And using similar Italian micro data to us, (Peydró, Polo et al. 2021) observe that banks more exposed to the PSPP may have engaged in more securities trading. Those authors do not directly measure the effects of the PSPP onto loan supply itself, and their evidence remains suggestive that the PSPP might have had only limited effects on credit supply. In addition there are other channels that can also dampen the transmission of monetary policy onto bank lending: because of credit substitution and agglomeration effects, when only some banks are exposed to the PSPP, the size of the aggregate effect of any credit expansion could be small (Allen and Gale 2004, Huber 2018).

For example, if PSPP exposed banks crowd out or become substitutes for non-exposed banks, then the net aggregate lending effect of the program could be zero. Conversely, if information frictions and other impediments to switching lenders are large, then PSPP-exposed banks that benefit from the augmented lending capacity might be unable to attract new clients (Petersen and Rajan 1995). This heterogeneity in bank-PSPP exposure can further mute the overall aggregate effect on lending when there are agglomeration effects in firm credit demand, and only a small

subset of firms in the supply network obtain credit (Jones 2011). Also, in economies such as Italy, where structural frictions might weigh on firm credit demand, the effects of the PSPP on credit usage could be second order.[7]

Our analysis of the bank-lending channel of QE is also closely related to work based on US data. (Foley-Fisher, Ramcharan et al. 2016, Darmouni and Rodnyansky 2017) and (Luck and Zimmermann 2018) for example find that some QE programs led to an expansion in bank lending to firms with positive real consequences. However, this result remains subject to debate and appears sensitive to the choice of firms used in the sample and the definition of the QE exposure counterfactual. For example, instead of using time dummies to denote QE, (Chakraborty, Goldstein et al. 2020) use quarterly Fed asset purchases and a sample of larger firms in the US. They find that QE exposed banks substituted away from lending to firms and towards mortgage lending to households, which in turn actually decreased firm investment.[8]

Our micro credit registry data help us to make progress relative to studies that use US data. In particular, because we observe the universe of Italian firms, our results are less likely to be sensitive to the choice of firms in any given sample. The credit registry data also identify firm credit applications, allowing us to study the effects of the PSPP at the extensive margin, and construct tests of substitution. For example, we can measure whether PSPP exposed banks are more likely to accept credit applications from firms that previously applied for credit from non-PSPP exposed banks. Unfortunately, we aware of no such data for the US. Moreover, with supervisory data on asset holdings across the balance sheet, we can make progress in understanding the effects of heterogeneity in the application of MMA and HCA in asset valuation.

The relatively high frequency of the data—month-level credit decisions—also help us to be clear on the counterfactual and exclude alternative interpretations stemming from other economic news or other ongoing monetary or fiscal policy interventions. Our approach builds on the broader literature examining the bank lending channel using micro-economic data. (Jiménez, Ongena et al. 2012) is a particularly important antecedent, as they use Spanish credit registry data to examine the effects of monetary policy at the extensive margin—see also (Jiménez, Ongena et al. 2014, Jiménez, Mian et al. 2020). Other classic studies in this literature include (Kashyap and Stein 2000)

[7] See for example https://www.ecb.europa.eu/press/pressconf/2015/html/is150122.en.html for a discussion of the relative importance of structural reforms versus quantitative easing in supporting domestic demand in the EU.

[8] There is also a sizeable literature on the transmission of QE through the mortgage and housing channel. See for example (Di Maggio, Kermani et al. 2017, Beraja, Fuster et al. 2019, Palmer, Kermani et al. 2020, Ramcharan 2020)

and (Peek and Rosengren 2000), while recent work by (Drechsler, Drechsel et al. 2016) focus on how other ECB policies impact bank lending.

3. Data

As the previous discussion makes clear, because the PSPP targeted specific securities, the impact of the program on bank lending would be expected to vary depending on a bank's holdings of securities eligible for purchase under the PSPP ("eligible securities"). Measuring this cross-sectional variation in bank exposure to the PSPP and identifying any supply effects from firm demand and other latent factors require detailed micro-data. This subsection describes the data in detail. We draw on four sources of data collected by the Bank of Italy.

First, we use the Italian credit register, which contains information for each firm-bank lending relationship. The credit register is a monthly panel dataset at the firm-bank-time level. That is, each entry represents the amount of lending made by a particular bank to a particular nonfinancial firm. All loans above €30,000 are included in the register, thereby making the coverage near-universal. Second, we use Taxia, a quarterly dataset with information on each term loan above €75,000 originated by a subset of Italian banks, and in particular, loan amount, interest rate, and firm identifier – so that we can link it with the credit register. Third, we use the initial information service (IIS) dataset, which records all instances in which a bank accesses the credit history of a firm—typically, when a firm applies for new credit from a bank it was not previously borrowing from. Fourth, we use the Bank of Italy credit and financial institutions' supervisory reports to obtain banks' balance sheet data. A key element of these data is represented by banks' security holdings at the ISIN level, which we use to construct each banks' measure of exposure to the QE policy announcement.

In most of our analysis, we work with monthly data over a 12-month window around the QE-announcement date (i.e., around January 22, 2015), and we extend the sample period for some robustness analyses. We focus on banking groups (hereinafter referred to as "banks") for which the bank holding company is a joint stock company. That is, we exclude mutual and cooperative

banks because of the different regulations to which they are subject. We also drop foreign banks, leaving our final sample at 95 banks. Our focus on banking groups (as opposed to single banks) is mainly motivated by the fact that key regulations such as capital requirements are checked by regulators at the group level.[9] Our approach is also consistent with recent papers that use similar data, such as Bottero, Minoiu et al. (2020), which use the same criteria to select banks in their sample.

In Table 1, we provide some key summary statistics. As of December 2014 (i.e., the month before the PSPP announcement), our sample from the credit register includes 216,718 firms that borrow from at least two banks. Focusing on this subset of firms allows us to identify changes in lending supply, as in Khwaja and Mian (2008). Panel A of Table 1 presents the distribution of the number of lending relationships, and Panel B presents some summary statistics about firm-bank lending relationships. Firms with at least two banking relationships borrow — per bank — slightly more than the universe of firms (€625,108 versus €414,973, on average) but have a similar mean and median of the log change in the amount borrowed. Panel C includes some key summary statistics about the IIS dataset (i.e., loan applications). In our sample window, banks access a new firm's credit history 662,904 times, of which 440,892 refers to unique firms. We note that 13,239 firms have at least two credit history checks in a given month, a feature that allows us to use the Khwaja and Mian (2008) identification approach in regard to the creation of new lending relationships.

Table 2 also contains some additional summary statistics about the banks in our sample. We report some statistics related to ECB policies other than the PSPP because later on in Section 0 we conduct several robustness checks related to such interventions. These summary statistics include: the maximum amount that banks can borrow under the targeted long-term refinancing operations (TLTRO) announced in June 2014; the net interbank position as of March 2014, which we relate in Section 0 to negative interests rates as in Bottero, Minoiu et al. (2020); a securitization

[9] Because of the regulatory approach, banks within the group do not need to meet capital requirements individually. This implies, for instance, that a credit expansion can be carried out only by some banks that are part of the group without the need to observe within-group borrowing and lending, which would cancel out anyway when regulators check group-level capital ratios.

dummy, which is equal to one if a bank has securitized any assets between August 2012 and August 2014; holdings of covered bonds and asset-backed securities (ABS); and the log change in borrowing from the ECB and deposits held at the ECB.

4. Regulation and the impact of QE on credit supply

4.1 Intensive margin

This section examines the impact of the PSPP on bank loan supply at the intensive margin. The research design combines the cross-sectional variation in banks' potential exposure to the PSPP with high frequency (monthly) data on lending. To measure banks' exposure to the PSPP, we first follow the literature and begin with the broadest definition: The ratio of a bank's securities that are eligible to be purchased under the PSPP to total bank assets (Darmouni and Rodnyansky 2017, Luck and Zimmermann 2018). Note that this broad exposure measure, defined as all eligible assets to total assets, ignores any accounting heterogeneity in the valuation of sovereign assets: It does not differentiate between sovereign assets assigned to the banking or trading book and the permitted accounting system used to value these assets. Also, this broad measure of exposure is highly persistent, and we use the ratio as of December 2014—just before the PSPP was announced—the main results are robust to exposure measures derived earlier as well.[10]

We next combine this bank-level variation in PSPP exposure with monthly lending data to study the lending response of PSPP-exposed banks before and after the PSPP announcement relative to banks not directly exposed to the PSPP. Monthly data are particularly helpful in this empirical setting. When the PSPP was announced, in early 2015, the ECB had several ongoing

[10] The PSPP announcement was a discrete event, but Mario Draghi's Jackson Hole speech in August 2014 foreshadowed the January 2015 ECB's asset purchase announcement, and anticipatory bias could affect our research design, as banks might have purchased eligible securities ahead of the announcement. Table IA1 in the online appendix suggests that this was not the case. In any month from June through December 2014, the probability that a bank previously not in the top quartile of exposure (eligible assets/assets) enters the top quartile is about 3.7 percent: In the runup to the announcement, most banks did not adjust their holdings of these assets in any great quantity.

policies that could also affect the banking sector, such as negative interest rates and the targeted longer-term refinancing operations (TLTRO), which first began in June 2014 (Andreeva and García-Posada 2021). Monthly data allow us to control directly for these and other ECB policies, as well as the broader economic conditions that both induced the ECB's policy responses and affected bank-firm credit decisions. Monthly data are an important advantage compared to studies that use lower frequency data, such as annual and even quarterly data.

Intuitively, with firm-by-month fixed effects and other non-parametric controls that absorb loan demand at the firm level, the difference-in-difference research design can identify whether the PSPP elicited a bank lending supply response to firms with multiple ongoing credit relationships. Note further that the Italian credit registry contains the near-universe of these bank-firm credit relationships. This allows us to measure more completely the effects of QE on business lending, rather than infer a treatment effect based on a selected sample of larger firms, usually from DealScan or through regulatory data in the US.

To be clear about the research design at the intensive margin, the dependent variable is the growth rate in lending, that is, the change in the log of disbursed loans $\Delta\log(L_{b,f,t})$ from bank *b* to firm *f* at time *t*, in comparison to *t*-1:

$$\Delta \log L_{b,f,t} = \sum_{\tau \neq 2014m12} \beta_\tau \times I_\tau \times QE_b + \sum_{\tau \neq 2014m12} \gamma_\tau \times I_\tau \times Y_b + \delta Z_{b,t} + \psi_b + \psi_{f,t} + \varepsilon_{b,f,t}. \quad (1)$$

The variable QE_b is the continuous ratio of eligible securities for purchase under the PSPP to bank assets in December 2014. We interact this variable with a set of time dummies I_τ, one for each month, dropping the one that corresponds to the pre-announcement month. Thus, our coefficients of interest are β_τ for $\tau \geq 2015m1$. These coefficients capture the effects of the PSPP announcement on bank lending supply. In addition, the coefficients β_τ for $\tau < 2015m1$ allow us to check for any possible pre-trends in lending growth. The terms ψ_b and $\psi_{f,t}$ are bank- and firm-by-time fixed effects, respectively. Firm-by-time fixed effects $\psi_{f,t}$ allow us to control for demand factors, as in Khwaja and Mian (2008).

Even with this suite of fixed effects, other factors that correlate with a bank's exposure to the PSPP could also affect equilibrium lending growth right at the time of the PSPP's announcement. To exclude alternative interpretations then, the benchmark specification includes two controls. First, the variable Y_b denotes bank size using the log of total assets. Second, because bank borrowing from the ECB has been shown to independently affect bank lending and trading behavior, we include the log change in the dollar value that bank *b* borrows from the ECB at time *t*, denoted by $Z_{b,t}$. Section 4 conducts a battery of additional controls, including several robustness checks related to other ECB policies (Drechsler, Drechsel et al. 2016), stress tests, and governance rules.

Column 1 of Table 4 shows the results using the broad measure of exposure to the PSPP. We find no evidence that broad exposure to the PSPP is associated with an increase in lending after the announcement. The point estimates are small, sometimes negative, and imprecisely estimated. In the four months after the PSPP announcement, none of the coefficient is statistically different from the July-November 2014 average.

Next, we use bank supervisory data to create measures of exposure that reflect the heterogeneity in the regulatory framework that banks were subject to. As a first step, we column 2 uses an exposure measure that includes only eligible securities that banks are allowed to sell (i.e., it excludes securities that are classified as "held to maturity" which are subject to very strict regulatory restrictions in relation to banks' ability to sell them). These securities include those classified as AFS in the banking book and those in the trading book. There is again no evidence that exposure to the PSPP via this measure significantly affects lending.

To gauge the salience of mark to market (MTM) versus historical cost (HC) accounting in shaping the impact of the PSPP on lending, column 3 uses an exposure measure that includes only eligible assets in the trading book. Under the post-2010 regulations, these are the only eligible assets are valued at MTM at the time of the 2015 PSPP announcement. The results are drastically different in comparison to the previous two columns. Column 3 shows that banks exposed to the PSPP via their trading book increased lending at the intensive margin almost immediately upon

the PSPP's announcement. This measure of exposure also shows no trend difference in lending growth between exposed and non-exposed banks in the period before the PSPP announcement (i.e., no pre-trends). As a further exercise, column 4 includes a control for non-eligible securities that must be marked-to-market to control for possible spillovers on the price and liquidity of such securities—the results are unchanged

When restricting attention to eligible securities in the trading book, only 36 out of 95 banks in the sample have strictly positive holdings, and the distribution is very skewed. To deal with possible noise and outlier effects, we create a PSPP trading book exposure dummy that equals one if a bank is in the top 15% of the exposure distribution, which corresponds approximately to the median of the conditional distribution of banks with positive holdings of PSPP-eligible securities.

Column 5 of Table 4 reports the point estimates using this dichotomous measure of exposure and the baseline controls. The point estimates show that loan growth increases by about 1.8 percentage points more than otherwise in January 2015 among exposed banks. We again find no evidence of any significant difference in loan growth across banks in the period before the announcement, nor do we observe any increase in loan growth at the intensive margin beyond January. We emphasize, however, that an increase in the growth rate of lending in January – followed by no significant changes after – corresponds to a permanent increase in the level of lending by exposed banks.

The estimates imply an increase in loan supply of about €145 million. This very small intensive margin effect likely reflects the fact that only a small number of banks had a substantial exposure to the PSPP through their holdings of sovereign securities in the trading book. We note that this result arises from the fact that only very few banks are substantially exposed, even if for the few that are exposed, the effect on loan supply is economically significant – 1.8 percentage points estimated, as shown in column 5.

To assuage the concerns that the small effect of the PSPP might be an artifact of our empirical specification or research design – rather than because of the regulatory treatment of sovereign assets – we provide additional robustness checks (available on request). There, we show that the results are unchanged when (i) we expand the time window to include the period from July

2012 to June 2016, that includes about 23 million observations in all, (ii) we account for possible anticipation effects by using banks' exposure to the PSPP as of June 2014 (i.e., seven months before the announcement), and (iii) we consider an alternative exposure dummy by separating banks into highly-, mid-, and low-exposed ones.

To further document the importance of MTM accounting in the transmission of QE, the final column of Table 4 exploits the pause and resumption of the PSPP and a change in the regulatory framework related to the accounting rules used by banks. Specifically, with increased stability in debt markets, in 2018 the EU reverted to requiring MTM accounting for sovereign assets classified as AFS in the banking book. Also, the ECB suspended its asset purchase program at the end of December 2018 and then announced, on September 12, 2019, that the program would restart as of November 1 of that year. Crucially, in 2019, the new regulations prescribed that banks had to use marked-to-market accounting not only for the securities in the trading book but also for all those classified as available-for-sale, including sovereign assets. Therefore, if mark-to-market accounting mediates the transmission of the policy on lending, we should expect that exposure based on sovereign assets held in the trading book and in the available-for-sale portion of the banking book should now be associated with an increase in lending after the resumption of the PSPP in September 2019.

This is indeed what we find. Using the baseline specification, we focus on the September 2019 announcement. We calculate banks' exposure as of August 2019 by including all PSPP-eligible securities in the trading book and available-for-sale category, as a fraction of total assets—an identical exercise to that in column 2. We exclude hold-to-maturity securities because they remain valued at historical cost. From column 6 in Table 4, the PSPP announcement is now associated with a significant increase in lending among banks more exposed to the PSPP via both the trading book and AFS securities in the banking book—all mark to market eligible securities.

The average banks' exposure as a fraction of total assets is of course now much higher than that based solely on the trading book, and the intensive-margin increase in lending in September 2019 is also correspondingly much higher. Column 6 also shows a positive effect in December 2019. This might be related to the actual restart of the purchases – the announcement was in

September 2019 but the purchases begun in November 2019 – and the first press conference of the new ECB president, Christine Lagarde, which was held in early December 2019, likely signaled the ECB's intention to continue with a fairly accommodative monetary policy stance.[11]

We have also conducted a battery of additional robustness tests (available on request) related to the 2019 announcement. First, we show that with a broad exposure measure that include all eligible securities in the banking and trading book (i.e., even those that are valued at historical cost), there is again no effect on bank lending, similar to what shown in column 1 of Table 4 for the 2015 announcement. This further confirms the role of historical versus mark-to-market accounting in the transmission of QE programs onto bank lending. Second, we add controls for other ECB policies. Third, we control for general equilibrium effects that might affect non-eligible securities and for anticipation effects. The results are very robust to these analyses and in some cases, the magnitude and significance are even stronger.

4.2 Mechanism and interest rates

To address more directly identification concerns, we now use economic theory and additional data to further validate our results. Notably, a determined skeptic might still argue that the effect of the PSPP that arises only through the trading book might stem from differences in latent demand across firms matched to exposed banks or other confounding factors. We address this concerns in two ways.

As a first step, we examine the mechanism by which the PSPP announcement led banks to expand bank lending. As we noted earlier, economic theory suggests that asset purchase programs can affect bank lending supply through two main channels: an improvement in the liquidity of the eligible securities, and or a recapitalization driven by higher market price of such securities. This exercise not only shows which mechanism is at work for the 2015 PSPP announcement but also provides validation for our results in light of these theoretical predictions.

[11] See e.g. the Wall Street Journal article "*Markets Welcome Christine Lagarde's First ECB Policy Meeting,*" December 12, 2019.

To conserve on the number of parameters estimated in these specifications, we condense the difference-in-difference design of our baseline specification (i.e., Column 5 of Table 4): Because most of the response to the PSPP was relatively immediate, we create an indicator variable that equals 1 in January 2015 and 0 otherwise. We then interact this 2015m1 indicator with our baseline PSPP exposure measure. Column 1 of Table 5 shows that the estimated impact of the PSPP on lending growth is invariant to this modification of the baseline difference-in-difference design.

Economic arguments observed that if illiquidity restricted loan supply, then by making it less costly for banks to rebalance their portfolio away from sovereign bonds in the trading book to meet loan demand, the PSPP should have the smallest effect among banks that already have plentiful cash and other liquid assets. To wit, these banks would already have sufficient liquidity to meet loan demand, and their lending response to the PSPP should be more muted relative to the more illiquid banks. Column 2 of Table 5 uses a triple interaction term design. It interacts $2015m1 * QE_b$ with an indicator variable that equals 1 if a bank is the top quartile of liquidity holdings – defined as cash plus central bank reserves over assets – as of December 2014.

This triple interaction term is negative and economically and statistically significant. It suggests that the lending response among these more liquid banks to the PSPP is about 66 percent smaller relative to other banks in the sample. As an alternative, column 3 derives the liquidity indicator variable based on the distribution of the deposits to loans ratio in December 2014, and the implied point estimate is almost identical to that in column 2. Columns 2 and 3 suggest that the PSPP increased lending at the intensive margin largely by relieving illiquidity at Italian banks most exposed to the ECB's asset purchases.[12]

Column 4 addresses the capital channel. This channel observes that if banks' distance from regulatory insolvency constrains their lending, central bank asset purchases that inflate the price

[12] Illiquid banks can increase their lending by selling previously illiquid sovereign assets. But the increase in the value of the option to sell these previously illiquid assets can also increase lending among illiquid banks even if these banks do not exercise the option in the short run. In Appendix B we thus investigate whether the PSPP affected the trading activity of banks. The evidence suggests that exposed banks do not sell their eligible securities more than non-exposed banks. That is, the PSPP likely increased the option value of selling eligible securities by limiting the price impact of such sales, but in the short run, it led to no increase in trading activity.

of government debt and improve the net worth of exposed banks should have the biggest effect on lending among exposed banks closest to regulatory insolvency. To gauge the salience of this mechanism, column 6 uses a triple interaction term, interacting $2015m1 * QE_b$ with an indicator variable that equals 1 if a bank's tier 1 capital ratio is in the top quartile of the distribution. This triple interaction term is negative though not significant; the sum of both interaction terms, reported at the bottom of Table 5, is also not significant and implies that the effect of the PSPP on lending growth among the most well-capitalized banks is about 10% less.[13]

We examine next the impact of the PSPP on interest rates. This can help address any remaining identification concerns about the effect of the PSPP on loan quantities: If the PSPP causally expanded credit supply, then the price of credit—the interest rate on loans—should decline. In contrast, if our results reflect a coincidental increase in latent credit demand, then interest rates should be non-decreasing in exposure to the PSPP after the program's implementation.

To test this prediction, we use new data on the interest rate on loan originations. These data are available quarterly for a subset of 37 of the 95 banks in our sample and only for term loans above €75,000. Despite these limitations, we show that our main extensive margin result presented in Table 4 holds in this subsample (results are available on request).

Using a modified version of the baseline specification, the dependent variable is the interest rate $i_{b,f,t}$ on a loan originated in quarter *t* by bank *b* and extended to firm *f*.[14] Column 5 of Table 5 presents the results. We use firm-by-time fixed effects to control for time-varying firm characteristics, and we find that more exposed banks reduce the interest rate in comparison to less exposed ones.[15] The result is statistically significant, and the reduction is 0.36 percentage points,

[13] The results are robust to using tier 1 common equity to risk-weighted assets or total capital to risk-weighted assets.
[14] The interest rate for newly originated term loans that we use is an APR that accounts for origination fees which is referred as *tasso annuo effettivo globale* (*taeg*) as defined by EU Council Directive 87/102/EEC.
[15] We note that the large majority of the new term loans in our dataset are on the intensive margin, that is, are extended by banks to firms that were already pre-existing customers.

so the magnitude is economically important too. Column 6 repeats the analysis controlling for the loan amount and the collateralization, showing that the results are unchanged.[16]

Taken together, these results suggest that the PSPP announcement generated a shift of the loan supply curve – consistent with an increase in quantities and a reduction in prices – and that the improved liquidity on secondary market of sovereign securities was the main mechanism that led exposed banks to increase lending after the 2015 announcement.

4.3 Extensive margin

In addition to increasing loan supply to existing customers, exposed banks might also form new credit relationships. This subsection uses data on loan applications to understand the impact of the PSPP on bank credit supply at the extensive margin. In Italy, when a firm applies for a new loan from a bank, Italian banks use the Bank of Italy's credit registry to learn about the firm's credit history. When the credit registry is accessed, the request is recorded in the initial information service (IIS) dataset and helps us measure loan demand at the firm level. We can then combine the IIS dataset with the credit register data to determine whether loan demand is met at the extensive margin (Jiménez, Ongena et al. 2012).

All loan applications are classified as either successful (i.e., a new loan was disbursed over the next three months in response to the application) or unsuccessful (i.e., a lending relationship did not begin). Note that this dataset does not include new loans to existing customers—those are classified as intensive margin responses—as applications for such loans are typically not recorded in the IIS dataset. That is, successful loan applications are cases where a bank lends to a particular borrower for the first time, forming a new credit relationship.

To understand the extensive margin, we modify the basic difference-in-difference research design along two dimensions. First, we use a simple linear probability model, and the dependent variable equals 1 if a firm's loan application to a bank in a specific month is successful, thereby

[16] We compute the collateralization as the dollar value of the collateral of all loans extended by bank b to firm f at the end of quarter t, as a ratio of total lending by bank b to firm f at the end of the same quarter. For a small fraction of observations this information is not available, so we use the same collateralization calculated in quarter $t+1$ or quarter $t-1$.

resulting in a new loan over the next three months, and 0 if the loan application is rejected. Second, because we have much fewer observations than at the intensive margin, we condense the research design with a post indicator variable that equals 1 in the months after the PSPP and 0 otherwise. We continue to use our baseline bank controls, along with bank fixed effects and year-month fixed effects. The coefficient on the post exposure interaction term measures whether 2015 PSPP-exposed banks, based on the baseline dummy indicator for above median trading book exposure, are more likely to accept loan applications relative to before the PSPP's announcement, as well as relative to banks not exposed to the PSPP.

Given the widespread publicity surrounding the PSPP, lower-quality firms could disproportionately apply to PSPP-exposed banks. This compositional shift in borrower quality would in turn lead to more rejections, making it hard to identify whether the PSPP affected loan supply at the extensive margin. To this end, column 1 of Table 6 uses firm fixed effects to absorb time-invariant firm characteristics that might shape firm credit risk. The results show that PSPP-exposed banks are more likely to grant credit after the PSPP. The point estimate suggests that the probability that an application is successful increases by 4.6 percentage points in the months after the PSPP announcement among the PSPP-exposed banks relative to otherwise.

Column 2 of Table 6 addresses the concern that time-varying economic shocks to firms can still bias these results. In particular, column 2 uses the most stringent firm-by-time fixed effects. This specification restricts the sample to only those firms that applied for a loan from two different banks within a calendar month and thus absorbs time-varying firm characteristics at a monthly frequency. The sample size shrinks dramatically, but even in this most demanding specification, we find evidence that exposure to the PSPP among banks in the months after the PSPP increases the probability that a firm's credit application is successful by 8.3 percentage points.

The last three columns of Table 6 investigate the role of liquidity and capital in mediating the effect of the PSPP on bank lending. Similar to the intensive margin results of Section 4.2, liquidity - defined as cash plus reserves over total assets or deposits over loans – appears to be a

strong driver of the extensive margin results, but the role of capital is more important than at the intensive margin.

5. The aggregate effects of the PSPP: firm and province-level evidence

The evidence thus far suggests that heterogeneity in the accounting treatment of sovereign assets on banks' balance sheets segmented exposure to the PSPP. Because of this segmentation, the PSPP augmented the lending capacity of only a narrow set of banks—those that recorded sovereign assets in their trading book, where assets are marked to market. Although the cost of credit declined for those firms that obtained credit, the overall size of the lending response appears small relative to the scale of the PSPP itself. But even this lending response might overstate the impact of the PSPP on quantities. If firms substituted borrowing from higher cost non-exposed banks towards cheaper PSPP-exposed banks, the net firm-level effect of the PSPP on loan supply could be small. Similarly, if PSPP-exposed banks form new credit relationships at the expense of non-exposed banks, the overall effect of the PSPP on loan supply in a local market might be non-existent. This section addresses these issues.

5.1 Firm-level evidence

This subsection studies credit substitution at the firm level. Building on the approach in (Jiménez, Mian et al. 2020), we aggregate the data up to the firm-level and examine whether firms that are previously more dependent on PSPP-exposed banks obtain an increase in net credit growth after the PSPP relative to otherwise. Consider a firm previously borrowing from both PSPP-exposed and non-PSPP exposed banks. If there is no substitution, then any increase in borrowing from the PSPP exposed bank after the PSPP should translate into positive loan growth at the firm-level. And the more dependent the firm is on PSPP-exposed banks, then the larger the increase in loan growth after the PSPP. In contrast, if there is perfect substitution, so that increased borrowing

from PSPP-exposed banks is fully offset by a decline in borrowing from non-exposed banks, then the PSPP should have no effect on loan growth at the firm-level.

To implement the approach of Jimenez et al. (2020), we focus on firms' borrowing as of December 2014 and January 2015, and we consider only bank-firm lending relationships that existed in both months. This is equivalent to restricting attention to the sample of our baseline intensive margin regression for these months. For each firm *f*, we aggregate lending at the firm level, and we compute the average exposure to PSPP as the average of the exposure of the banks that lend to *f* weighted by their loans as of December 2014, denoted by QE_f. As in Jimenez et al. (2020), our focus is on the specification

$$\Delta \log L_f = \beta^{firm} \times QE_f + \psi_f + \varepsilon_f$$

where ψ_f proxies for firm demand. Of course, unlike the firm-bank level data, we cannot use firm-by-time fixed effects to absorb latent demand, and OLS estimates of β^{firm} can be biased if firm demand ψ_f and firm exposure QE_f are correlated.

Table 7 shows the results of implementing the approach of Jimenez et al. (2020). Column 1 repeats our baseline loan-level regression using only the growth rate of lending from bank *b* to firm *f* between December 2014 and January 2015 and using firm fixed effects as in Khwaja and Mian (2008). The estimated coefficient is significant, and the magnitude is similar to that of our baseline regression in column 5 of Table 4. Column 2 estimates the same loan-level regression but using OLS, that is, omitting firm fixed effects. Finally, Column 3 estimates the firm-level regression with OLS. Using the formula in Jimenez et al. (2020) to recover the impact of the PSPP on bank lending at the firm level, we obtain $\beta^{firm} = 1.77$, which is almost identical to the loan-level coefficient obtained in the fixed-effect regression in column 1, and also very close to the results of our baseline regression in column 5 of Table 4.

This evidence suggests that firms with existing links to more exposed banks experienced an overall increase lending at the intensive margin. The magnitude of the results suggests that exposed banks neither crowd out nor enhanced the loan supply of their less exposed competitors toward firms with multiple established lending relationships. However, the firm-level evidence

only provides partial information about the aggregate consequences of the PSPP. If PSPP-exposed banks form new credit relationships at the expense of their non-PSPP exposed competitors in the local market, then the net aggregate effect of the PSPP could be limited. We take on this question next.

5.2 *Bank-by-province evidence*

To understand how the PSPP affected competition and aggregate loan supply in local banking markets, we use provinces as the relevant geographic area – the vast majority of Italian firms form credit relationships with bank branches co-located within the province.[17] There were 110 provinces in Italy during our time window.[18] Consider a bank that has branches in two provinces, A and B. Suppose further that the market share of PSPP-exposed banks in province A is higher than in province B. This means that for the same bank, its branches in province A will face more competition from PSPP-exposed banks relative to its branches in province B. If substitution features in the data, then loan growth at branches in province A should become slower relative to province B on account of the greater competition in province A when the ECB announces the PSPP. Note that because the unit of analysis is at the branch level, this research design can hold constant time-varying bank-level observables.

More precisely, let C_p denote the market share of PSPP-exposed banks in province p. We define the market share as the ratio of the sum of loans to firms made by PSPP-exposed banks in province p, as a fraction of total loans to firms in the province in that month:

[17] Regions or municipalities are alternative administrative areas, but we argue that using provinces produces more accurate results. In terms of regions, there are only 20 of them, and many banks operate only in a subset of such regions. As such, it might be difficult to get precise estimate at this level. Municipalities are very small in Italy. In 2013, according to Istat (Italian Statistical Institute), the average area of a municipality is 37.3 km^2, and the median is 21.9 km^2. If each municipality is approximately a circle, that means that the average radius of Italian municipalities is about 3.45 km, and the median is 2.64 km. In addition, looking at big municipalities does not change the above considerations. There are only 67 municipalities with an area greater than 250 km^2 (radius > 8.9 km); these large municipalities have, in total, 5.9 million inhabitants, about 9.9% of the population. Of these, 2.6 million are in Rome, by far the biggest city in Italy by both size and population. Source: Istat, "La superficie dei comuni, delle province e delle regioni italiane," *Statistiche Report*, February 19, 2013, https://www.istat.it/it/files/2015/04/Superfici-delle-unit%C3%A0-amministrative-Testo-integrale.pdf.

[18] The number of provinces has slightly reduced as of the writing of this paper because of some administrative changes.

$$C_p = \frac{\sum_{b(p)}\left[L_{b(p),\,2014m12} \times QE_b\right]}{\sum_{b(p)} L_{b(p),\,2014m12}},$$

where $L_{b(p),\,2014m12}$ denotes the total amount of loans extended as of December 2014 by the branches of bank b that operate in province p, and QE_b is as before, our dummy that equals 1 for banks with high exposure to the PSPP, as described in the previous section.

Then our province-level estimating equation is

$$\Delta \log \mathrm{L}_{\mathrm{b,p,t}} = \sum_{\tau \neq 2014\mathrm{m12}} \eta_\tau \times I_\tau \times C_p + \psi_{\mathrm{b,t}} + \psi_{\mathrm{p}} + \varepsilon_{b,p,t}.$$

The dependent variable is monthly loan growth rate by the branches of bank b in province p in month t. The coefficient of interest is η_τ, which is interacted with monthly dummies around the PSPP window, I_τ, and the baseline PSPP exposure dummy, QE_b. The sequence of coefficients η_τ measures whether loan growth at branches in the province differs in the months before and after the PSPP, depending on the market share of PSPP-exposed banks in the province. If there is substitution, then η_τ should be negative immediately after the PSPP is announced, as PSPP-exposed banks displace lending of their competitors. To account for the way substitution might impact differently exposed and non-exposed banks, we estimate the regression separately for the two sets of banks.

Using the suite of bank-by-time and province fixed effects, our key identification assumption is that shocks to loan growth at the branch-province level do not vary around the announcement window with the province-level PSPP market share variable and by whether a bank itself is exposed to the PSPP. Put differently, these estimates of substitution are unbiased even if there are province-specific variations in the lending supply component of a given bank, as long as such variations are not correlated with the market share of PSPP-exposed banks C_p. Standard errors are clustered at the bank and province level.

Column 1-2 of Table 8 estimate the regression separately for exposed and non-exposed, using all the loans that any given bank extends in province p at time t – both at the intensive and extensive margin. The coefficient on the PSPP market share, C_p, is never significant for exposed

banks (column 1), but it is significant for non-exposed banks in January 2015 (column 2). That is, lending by a given exposed bank is not affected by having more or fewer direct competitors that are also exposed. But lending by a non-exposed bank is negatively affected right after the PSPP announcement. In a province where PSPP-exposed banks account for loan market share at the 75th percentile (conditional on $C_p > 0$), lending growth of non-exposed banks declined by about 0.3 percentage points.

To better understand how a non-exposed bank is affected by exposed competitors, columns 3 and 4 analyze separately lending of non-exposed banks at the intensive and extensive margin, respectively. That is, column 3 restricts the sample to loans that branches of non-exposed bank *b* extends in province *p* at time *t* to a pre-existing customer, and column 4 restricts the sample to loans to new customers.[19] We find no effect at the intensive margin (column 3), but a strong and significant effect at the extensive margin in January 2015 and March 2015 (column 4).

That is, when the PSPP was announced, branches of non-exposed banks contracted lending to new customers in provinces in which their direct competitors are highly exposed, but we observe no effect on lending to existing customers. The significant effect in March 2015 is not surprising if one considers the fact that it often takes time for an application to be evaluated and – even if approved – for the loan to be disbursed. Indeed, in Section 4.3, we consider an application to be approved if a new loan is disbursed up to three months after the application, following Jiménez, Ongena et al. (2012). To reduce the possible noise associated with the lag in loan disbursement, column 5 repeats the analysis of column 4 by interacting the province-level market share of exposed banks with quarterly dummies, rather than monthly dummies.[20] The results are confirmed, and we find that in a province where PSPP-exposed banks account for loan market share at the 75th percentile (conditional on $C_p > 0$), lending growth at the extensive margin declined by more than five percentage points. Finally, column 6 repeats the analysis of Column 5 by looking at loans

[19] We define new customers to be firms that were not borrowing from bank *b* in the previous three months, consistently with the definition used in Section 4.3 when using application data and studying the extensive margin.
[20] If we use quarterly dummies to repeat the exercise of column 3 (i.e., intensive margin loans of non-exposed banks) we still find no significant effect after the announcement and no pre-trends.

to new customers, but this time, focusing on exposed banks. We find no effect at the extensive margin, in line with the results of column 1.[21]

Taken together, the results suggest that a substitution effect took place, with new lending shifting in part from non-exposed to exposed banks. Nonetheless, it is certainly possible that firms that would have otherwise borrowed from non-exposed banks benefit from the switch – either through lower interest rates or because e.g. their loan application is approved faster. Our data does not allow us to provide a clear answer to this question. When restricted attention to interest rate data referring to new lending relationship, our sample shrinks dramatically, and we cannot make inference about the causal effect of the PSPP on interest rates.[22]

6. Additional robustness checks

We now provide additional robustness checks to rule out the possibility that the effects of the QE announcement on highly exposed banks could be due to other factors. Overall, this section confirms the results of the benchmark analysis, and the additional controls we run have little or no effects on the estimated key coefficients. We consider three sets of additional controls. First, we explore in more detail the possibility that the results might be driven by other ECB policies. Second, we account for the fact that the largest Italian banks were subjected to stress tests during 2014, with the results made public in October 2014.[23] Finally, we consider the fact that some banks are under special governance rules. In particular, their top management is appointed by the Bank of Italy, an event that typically happens in response to deteriorating balance sheet situations, possible fraud, or both.[24]

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[21] We have also studied intensive margin loans of exposed banks, and found no effects in any of the months after the announcement and no pre-trends.

[22] Our sample allows us to estimate the effect of exposure on interest rate, as in Section XX, but only without firm fixed effect. We find no effect of exposure on rates, but the results could be biased by effects associated with borrower risk.

[23] Bank of Italy, "Risultati dell'esercizio di 'valutazione approfondita' (Comprehensive Assessment)," press release, October 26, 2014, https://www.bancaditalia.it/media/comunicati/documenti/2014-02/cs_261014.pdf.

[24] We also run some robustness checks that account for banks' mergers and acquisitions (M&As), and we find that the results are essentially unchanged. In particular, we account for M&As in two ways. First, we include an M&A dummy interacted with time dummies, with the M&A dummy equal to one for groups in which at least one bank of the group

In column 1 of Table 9, we consider the targeted long-term refinancing operations (TLTRO), an ECB policy announced in June 2014 aimed at improving bank lending to the euro area non-financial private sector. Banks were entitled to an initial TLTRO borrowing of up to 7% of the total amount of their loans to the euro area non-financial private sector outstanding on April 30, 2014, excluding loans to households for home purchases. For each bank, we compute this limit (as a share of total assets) and add it to the vector of control $\boldsymbol{Y_b}$ of the baseline regression, so that the limit interacts with the set of monthly time dummies.

In columns 2 and 3 of Table 9, we explore the possible effects of the ECB's negative interest rate policy. On June 5, 2014, the ECB announced a reduction in the deposit rate that brought its level below zero. To consider the possible effects of the policy, column 2 includes a control $D_{b,t}$ to the regression, where $D_{b,t}$ is the log change in deposits held by bank b at the ECB, similar to what we do for bank b's borrowing from the ECB in the baseline regression. Column 3 takes a different approach. There, we follow Bottero, Minoiu et al. (2020) and approximate the exposure to the negative interest rate policy as the net interbank position, computed as interbank loans minus deposits with a maturity of up to one week, normalized by total assets. As in Bottero, Minoiu et al. (2020), we compute the exposure before the June 2014 announcement — the latest data on interbank loans and deposits before the announcement are available as of March 31, 2014.

In columns 4 and 5, we account for the asset purchase programs announced before the PSPP. In particular, in September 2014, the ECB announced that it would purchase covered bonds and asset-backed securities (ABS). This announcement could have affected banks not only because of their direct holdings of covered bonds and ABS, similar to the PSPP, but also because banks can issue such assets. To deal with this possibility, we construct two measures of exposures to the September 2014 announcement. First, we construct a dummy equal to one for banks that have originated such assets between August 2012 and August 2014. Second, we compute banks' holdings of covered bonds and ABS in the trading book as a fraction of total assets, as of August

has been the target of M&A activities in 2014 or 2015. Second, for each banking group b, we assign a loan made at time t to the banking group b as long as the loan was extended by a bank that was part of the group in December 2014, even if $t \neq 2014m12$.

2014. Similar to public sector securities, several banks have no holdings of such securities. Hence, we define an exposure dummy equal to one if a bank's holdings are above a certain cutoff. We choose the cutoff to be the median of the banks' holdings distribution conditional on strictly positive holdings. We include the two exposure measures separately in columns 4 and 5.

In columns 6 and 7 of Table 9, we consider the stress tests performed by the ECB. More precisely, the ECB conducted a so-called *comprehensive assessment* that included not only stress tests but also an asset quality review. Among the 95 banks in our sample, 7 banks were subject to the assessment, and 2 failed the test. The results were announced in October 2014. In column 6, we add a dummy equal to one for the two banks that failed the test, which is included in $\boldsymbol{Y_b}$ so that it interacts with the time dummies. In column 7, we include the same dummy as in column 6 and an additional dummy equal to one for banks that were subject to the comprehensive assessment but passed the test, which we also include in $\boldsymbol{Y_b}$.

Finally, in column 8 of Table 9, we consider the fact that some banks are subject to special governance rules (i.e., *amministrazione controllata*) and that the top management personnel at those banks are appointed by the Bank of Italy. Out of the 95 banks in our sample, 4 were subject to these rules. In the estimation, we include a dummy equal to one for such banks, which we include in $\boldsymbol{Y_b}$ so it interacts with the set of monthly time dummies.

7. Conclusion

This paper has used Italian data to study the effects of QE on bank lending. We find that banks exposed to the ECB's Public Sector Purchase Program significantly increased lending at both the intensive and extensive margins, relative to less exposed banks. The impact was stronger among the more illiquid banks in the sample, suggesting that these asset purchases helped relieve illiquidity constraints at exposed banks. Crucially, we find that the overall effect on lending was very small. We link this result to the regulatory framework — and in particular, accounting rules set by banking supervisors — which affected the transmission of the program onto banks' lending.

Sovereign assets are mark to market when held in the trading book. And banks with greater trading book exposure increased lending relatively more once the program was announced – but only very few banks have a substantial amount of eligible securities in the trading book. In contrast, regulators allowed banks to value assets in the banking book at historical cost, and some of these assets were not intended for sale. We find that banks' exposure to the PSPP through the assets in their banking book – which account for the largest majority of banks' holdings of PSPP-eligible securities in Italy – engendered no lending response.

We also find that after the PSPP, branches of non-exposed PSPP banks cut back lending in those areas where PSPP-exposed banks dominate the local market, especially at the extensive margin. This further reduces the overall effect of the PSPP. We leave it to future research to better understand the real effects of this reallocation in the supply of credit across banks.

Figures and Tables

Table 1: Summary statistics, firms

Panel A

Number of lending relationships (December 2014)	Freq.	Percent
2	141,323	65.21
3	44,983	20.76
4	17,089	7.89
5	7,330	3.38
≥ 6	5,993	2.77
Total	216,718	100

Panel B

Firm-bank lending relationships	N	Mean	1st quartile	Median	3rd quartile	Std. dev.
All firms						
Amount borrowed, EUR (as of December 2014)	1,216,779	414,973	36,370	79,218	211,910	5,444,324
Log change amount borrowed	13,186,871	0.17	-2.04	0.00	0.61	46.33
Firms with more than one bank relationship						
Amount borrowed, EUR (as of December 2014)	563,922	625,108	43,978	115,779	346,191	7,587,313
Log change amount borrowed	61,171,28	0.19	-2.97	0.00	1.24	51.16

Panel C

Initial information service (IIS)	
Number of requests, July 2014 to June 2015	662,904
Number of unique firms	440,892
Number of firms that submit at least 2 applications in a month, July 2014 to June 2015	13,239

Panel A displays the distribution of the number of lending relationships for the firms with outstanding loans reported in Italian credit register data in December 2014. Panel B displays the distribution of the amount borrowed (in December 2014) and the log change in the amount borrowed (for each month in our sample, that is, July 2014 – June 2015) for all firms and for firms with more than one lending relationship. Panel C displays some key summary statistics about loan applications from the initial information service (IIS). We drop IIS requests made by bank *b* about firm *f* if *b* was already lending to *f* in the previous three months.

Table 2: Summary statistics, banks

	N	Mean	1st quartile	Median	3rd quartile	Std Dev
Eligible assets in trading book, % total assets	95	0.45	0.00	0.00	0.00	1.63
Eligible assets in trading book, % of trading book	95	9.42	0.00	0.00	1.70	32.68
Log of total assets	95	21.38	19.41	21.45	22.82	2.20
Business loans, % of total assets	95	25.07	10.85	26.66	37.21	15.51
Household loans, % of total assets	95	8.54	2.13	7.03	11.43	10.25
Securities, % of total assets	95	27.31	16.55	25.97	35.23	16.71
Non-eligible securities in trading book, % of tot. assets	95	4.12	0.00	0.19	6.30	6.46
Cash, % of total assets	95	0.36	0.01	0.27	0.51	0.49
Deposits over loans, %	95	88.61	44.53	70.21	118.27	80.48
Total capital, % of risk-weighted assets	95	22.65	12.17	15.90	26.00	24.13
CET1 capital, % of risk-weighted assets	95	21.51	10.49	13.78	26.00	24.55
Non-performing loans, % of total loans	95	4.21	0.78	2.70	6.28	4.64
TLTRO ceiling, % of total assets	95	2.66	1.28	2.56	3.21	3.60
Net interbank position, % of total assets	95	-0.35	-3.73	0.00	2.97	12.32
Securitization dummy	95	0.28	0.00	0.00	1.00	0.45
Covered bonds and ABS, trading book, % of tot. assets	95	1.44	0.00	0.00	0.40	3.36
Log change of borrowing from the ECB, %	1129	-12.17	0.00	0.00	0.00	270.17
Log change of deposits at the ECB, %	1129	0.59	-11.82	0.00	15.68	150.96

The table displays the key summary statistics for the banks in our sample. Cash includes bills and coins. TLTRO ceiling is the maximum amount that a bank can borrow from the ECB using the TLTRO program. The securitization dummy is equal to one for banks that have securitized assets between August 2012 and August 2014, and zero otherwise. The TLTRO ceiling is computed as of April 2014. The net interbank position is computed as of March 2014. Covered bonds and ABS in the trading book are computed as of August 2014. The log change in borrowing from and depositing at the ECB is computed month by month for our sample window, that is, July 2014 to June 2015. All other statistics are computed as of December 2014.

Table 3: Conditional correlation between banks' exposure dummy and banks' characteristics

Exposure dummy	Coef.	S.E.	P-value
Log of total assets	-0.030	[0.030]	0.320
Business loans, % of total assets	-0.002	[0.003]	0.477
Household loans, % of total assets	-0.002	[0.004]	0.617
Securities, % of total assets	0.002	[0.003]	0.490
Cash, % of total assets	0.098	[0.085]	0.255
Deposits over loans, %	-0.001	[0.001]	0.247
Total capital, % of risk-weighted assets	-0.004	[0.024]	0.853
CET1 capital, % of risk-weighted assets	0.002	[0.024]	0.946
Non-performing loans, % of total assets	-0.004	[0.010]	0.660
TLTRO ceiling, % of total assets	0.005	[0.013]	0.724
Net interbank position, % of total assets	0.001	[0.003]	0.766
Securitization dummy	-0.120	[0.129]	0.354
Covered bonds and ABS, trading book, % of total assets	-0.001	[0.015]	0.932
Observations	95		
R-squared	0.093		

The table displays the result of regressing the exposure dummy QE_b on banks' characteristics. Cash includes bills and coins. TLTRO ceiling is the maximum amount that a bank can borrow from the ECB using the TLTRO program. The securitization dummy is equal to one for banks that have securitized assets between August 2012 and August 2014, and zero otherwise. The TLTRO ceiling is computed as of April 2014. The net interbank position is computed as of March 2014. Covered bonds and ABS in the trading book are computed as of August 2014. All other statistics are computed as of December 2014.

Table 4: Intensive margin

	(1) Broad exposure	(2) AFS + trading book	(3) Trading book	(4) Trading book	(5) Trading book dummy (baseline)		(6) 2019: AFS + trading book
[2014m7]× QE_b	-0.048	-0.034	0.080	0.066	0.367	[2019m3]× QE_b	0.021
	[0.040]	[0.041]	[0.132]	[0.125]	[0.630]		[0.081]
[2014m8] × QE_b	-0.152***	-0.155**	0.188	0.204	0.452	[2019m4] × QE_b	0.176
	[0.054]	[0.066]	[0.171]	[0.166]	[0.611]		[0.161]
[2014m9] × QE_b	-0.003	0.022	0.277	0.238	0.834	[2019m5] × QE_b	0.099
	[0.055]	[0.052]	[0.234]	[0.228]	[0.580]		[0.065]
[2014m10] × QE_b	-0.085	-0.057	-0.099	-0.106	-0.889	[2019m6] × QE_b	0.051
	[0.053]	[0.059]	[0.234]	[0.229]	[0.909]		[0.114]
[2014m11] × QE_b	-0.006	-0.026	0.238	0.223	-0.342	[2019m7] × QE_b	0.071
	[0.062]	[0.072]	[0.274]	[0.262]	[0.915]		[0.137]
[2015m1] × QE_b	-0.076	-0.016	0.443**	0.416**	1.784**	[2019m9] × QE_b	0.248**
	[0.053]	[0.054]	[0.174]	[0.160]	[0.725]		[0.108]
[2015m2] × QE_b	-0.075*	-0.080*	0.144	0.142	0.040	[2019m10] × QE_b	-0.185
	[0.038]	[0.045]	[0.180]	[0.171]	[0.682]		[0.224]
[2015m3] × QE_b	0.014	-0.014	0.068	0.065	0.402	[2019m11] × QE_b	0.097
	[0.055]	[0.052]	[0.160]	[0.164]	[0.553]		[0.105]
[2015m4] × QE_b	-0.080*	-0.061	0.090	0.083	0.044	[2019m12] × QE_b	0.350***
	[0.043]	[0.047]	[0.164]	[0.160]	[0.558]		[0.095]
[2015m5] × QE_b	-0.114**	-0.130**	0.048	0.032	-0.532	[2020m1] × QE_b	0.107
	[0.050]	[0.057]	[0.183]	[0.174]	[0.734]		[0.142]
[2015m6] × QE_b	0.012	-0.003	0.065	0.056	0.238	[2020m2] × QE_b	0.020
	[0.054]	[0.049]	[0.126]	[0.118]	[0.502]		[0.048]
Observations	5,867,308	5,867,308	5,867,308	5,867,308	5,867,308		8,346,934
R-squared	0.394	0.394	0.394	0.394	0.394		0.369
Bank FEs	Yes	Yes	Yes	Yes	Yes		Yes
Firm-time FEs	Yes	Yes	Yes	Yes	Yes		Yes
Size	Yes	Yes	Yes	Yes	Yes		Yes
ECB lending	Yes	No	Yes	Yes	Yes		Yes
Non-eligible securities	No	No	No	Yes	No		No

The dependent variable is the change in the log of disbursed loans from bank *b* to firm *f* at time *t*. Column 1-5 refer to the January 2015 PSPP announcement, and column 6 refers to the September 2019 announcement. Column 1 uses the broad measure of exposure defined as all eligible securities over total assets. Columns 2 and 6 use the measure of exposure defined as eligible securities classified as available for sale or in the trading book over total assets. Columns 3 and 4 use the baseline measure of exposure defined as eligible securities in the trading book over total assets. Column 5 uses a dummy equal to one if the trading book exposure is in the top 15% of the distribution. Standard errors are clustered at the bank level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Liquidity, capital, and interest rates (intensive margin)

	(1) 2015m1 only	(2) Liquidity	(3) Liquidity	(4) Capital	(5) Interest rates	(6) Interest rates
[2015m1] × QE_b	1.733***	2.078***	1.775***	1.749***		
	[0.555]	[0.765]	[0.622]	[0.586]		
[2015m1] × QE_b × [Liquidity/assets]		-1.392*				
		[0.828]				
[2015m1] × QE_b × [Deposits/loans]			-1.158			
			[1.870]			
[2015m1] × QE_b × [Tier1 ratio]				-0.187		
				[2.494]		
[2014q3] × QE_b					0.223	0.232
[2015q1] × QE_b					-0.364*	-0.354*
[2015q2] × QE_b					-0.015	-0.010
[2015m1] × (QE_b + QE_b × [liquidity or capital])		0.686*	0.617	1.562		
Observations	5,867,308	5,867,308	5,867,308	5,867,308	80,505	79,983
R-squared	0.394	0.394	0.394	0.394	0.739	0.739
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm-time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Size	Yes	Yes	Yes	Yes	Yes	Yes
ECB lending	Yes	Yes	Yes	Yes	Yes	Yes
Loan amount	No	No	No	No	No	Yes
Collateral	No	No	No	No	No	Yes

The dependent variable is the change in the log of disbursed loans from bank *b* to firm *f* at time *t* (in columns 1-4) and the interest rate charged by bank *b* to firm *f* at time *t* (in columns 5-6). The variable QE_b is a dummy equal to one if the trading book exposure is in the top 15% of the distribution. Standard errors are clustered at the bank level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Extensive margin

	(1) Firm FEs	(2) Firm-by-time FEs	(3) Liquidity	(4) Liquidity	(5) Capital
[Post] × QE_b	0.046***	0.084*	0.157***	0.107**	0.102***
	[0.017]	[0.043]	[0.042]	[0.043]	[0.038]
[Post] × QE_b × [liquidity/assets]			-0.172**		
			[0.066]		
[Post] × QE_b × [deposits/loans]				-0.228*	
				[0.117]	
[Post] × QE_b × [Tier1 capital ratio]					-0.084
					[0.111]
[Post] × (QE_b + QE_b × [liquidity or capital])			-0.015	-0.121	0.018
			[XXX]	[XXX]	[XXX]
Observations	359,026	27,219	27,219	27,219	27,219
R-squared	0.705	0.585	0.585	0.585	0.585
Bank FEs	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	No	No	No	No
Firm FEs	Yes	No	No	No	No
Firm-time FEs	No	Yes	Yes	Yes	Yes
Size	Yes	Yes	Yes	Yes	Yes
ECB lending	Yes	Yes	Yes	Yes	Yes

The dependent variable is a dummy equal to one if a loan application by firm *f* to bank *b* at time *t* is granted between *t* and *t*+3 (i.e., we observe a new credit relationship in the credit register) and the lending relationship lasts at least three months, and zero otherwise. The variables [Tier 1 capital ratio], [liquidity/assets] and [deposits/loans] are dummies equal to one if the bank is in the top quartile of the distribution of the respective variable, where "liquidity" is defined the sum of cash and reserves. Standard errors are clustered at the bank level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Firm-level analysis

	(1) Loan-level, fixed effects	(2) Loan-level, OLS	(3) Firm level, OLS
QE_b	1.691***	1.263***	
	[0.418]	[0.303]	
QE_f			0.969**
			[0.424]
Observations	489,388	489,388	190,110
R2	0.395	0.0001	0.001
Firm FEs	Yes	No	No
Size	Yes	Yes	Yes
ECB borrowing	Yes	Yes	Yes

Table 8: Bank-by-province evidence

	(1)	(2)	(3)	(4)		(5)	(5)
	All loans	All loans	Intensive margin loans	Extensive margin loans		Extensive margin loans	Extensive margin loans
	Exposed banks	Non-exposed banks	Non-exposed banks	Non-exposed banks		Non-exposed banks	Exposed banks
[2014m7]×Cp	0.017	-0.002	0.024	-0.588			
	[0.065]	[0.035]	[0.036]	[1.216]			
[2014m8]×Cp	0.004	-0.018	0.007	0.871	[2014q3]×Cp	0.505	1.118
	[0.040]	[0.030]	[0.032]	[0.913]		[0.181]	[0.853]
[2014m9]×Cp	0.000	-0.004	0.018	-2.506			
	[0.035]	[0.031]	[0.034]	[1.608]			
[2014m10]×Cp	-0.008	-0.041	-0.007	-1.617			
	[0.040]	[0.048]	[0.051]	[2.050]			
[2014m11]×Cp	0.013	0.004	0.031	-0.725			
	[0.036]	[0.051]	[0.055]	[0.887]			
[2015m1]×Cp	-0.003	-0.054*	-0.024	-2.553*			
	[0.050]	[0.032]	[0.031]	[1.265]			
[2015m2]×Cp	0.037	-0.024	0.001	-0.544	[2015q1]×Cp	-0.943**	-0.047
	[0.065]	[0.037]	[0.043]	[1.190]		[0.359]	[1.259]
[2015m3]×Cp	0.062	0.002	0.024	-1.952***			
	[0.053]	[0.023]	[0.026]	[0.690]			
[2015m4]×Cp	0.026	-0.003	0.026	-1.717			
	[0.038]	[0.040]	[0.040]	[1.434]			
[2015m5]×Cp	-0.002	-0.001	0.025	-0.758	[2015q2]×Cp	-0.089	0.365
	[0.049]	[0.032]	[0.039]	[0.564]		[0.130]	[0.944]
[2015m6]×Cp	0.032	-0.064***	-0.041	-0.051			
	[0.040]	[0.017]	[0.025]	[1.500]			
Observations	822	14,313	14,304	9,066		9,066	301
R-squared	0.425	0.145	0.133	0.135		0.134	0.309
Bank-month FEs	Yes	Yes	Yes	Yes		Yes	Yes
Province FEs	Yes	Yes	Yes	Yes		Yes	Yes

Table 9: Robustness (other ECB policies, stress tests, and special governance rules)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[2014m7]× QE_b	0.534	0.210	0.526	0.350	0.308	0.457	0.366	0.416
	[0.593]	[0.615]	[0.676]	[0.626]	[0.656]	[0.598]	[0.583]	[0.545]
[2014m8] × QE_b	0.962**	0.676	0.675	0.565	0.430	0.570	0.654	0.537
	[0.480]	[0.616]	[0.640]	[0.551]	[0.679]	[0.597]	[0.546]	[0.590]
[2014m9] × QE_b	1.313**	0.911	0.868	0.819	0.986	1.029*	1.299**	0.769
	[0.550]	[0.561]	[0.611]	[0.538]	[0.634]	[0.560]	[0.524]	[0.643]
[2014m10] × QE_b	-0.331	-0.814	-0.833	-0.615	-0.847	-0.716	-0.647	-0.819
	[0.813]	[0.938]	[0.954]	[0.729]	[0.848]	[0.871]	[0.961]	[0.916]
[2014m11] × QE_b	-0.017	-0.628	-0.450	0.053	-0.433	-0.135	-0.464	-0.259
	[0.951]	[0.971]	[0.927]	[0.731]	[1.005]	[0.898]	[1.107]	[0.834]
[2015m1] × QE_b	2.264***	1.806**	1.999**	1.772**	1.734**	1.942***	2.106***	1.747**
	[0.668]	[0.814]	[0.799]	[0.723]	[0.760]	[0.677]	[0.683]	[0.700]
[2015m2] × QE_b	0.434	0.038	0.198	0.129	0.054	0.166	0.411	0.064
	[0.602]	[0.670]	[0.717]	[0.624]	[0.682]	[0.651]	[0.647]	[0.636]
[2015m3] × QE_b	0.609	0.279	0.567	0.321	0.482	0.518	0.673	0.375
	[0.580]	[0.536]	[0.583]	[0.538]	[0.604]	[0.550]	[0.608]	[0.553]
[2015m4] × QE_b	0.422	-0.090	0.185	0.099	0.091	0.168	0.341	0.074
	[0.524]	[0.514]	[0.606]	[0.527]	[0.537]	[0.524]	[0.518]	[0.526]
[2015m5] × QE_b	-0.129	-0.480	-0.397	-0.271	-0.639	-0.399	-0.373	-0.441
	[0.660]	[0.755]	[0.771]	[0.621]	[0.793]	[0.715]	[0.752]	[0.609]
[2015m6] × QE_b	0.381	0.165	0.354	0.165	0.354	0.344	0.448	0.150
	[0.522]	[0.553]	[0.517]	[0.472]	[0.530]	[0.492]	[0.469]	[0.502]
TLTRO	Yes	No	No	No	No	No	No	No
Deposits at the ECB	No	Yes	No	No	No	No	No	No
Net interbank posit.	No	No	Yes	No	No	No	No	No
Securitization	No	No	No	Yes	No	No	No	No
Covered bonds, ABS	No	No	No	No	Yes	No	No	No
Failed stress tests	No	No	No	No	No	Yes	Yes	No
Subject to stress tests	No	No	No	No	No	No	Yes	No
Governance rules	No	No	No	No	No	No	No	Yes
Observations	5,867,308	5,867,308	5,867,308	5,867,308	5,867,308	5,867,308	5,867,308	5,867,308
R-squared	0.394	0.394	0.394	0.394	0.394	0.394	0.394	0.394

The dependent variable is the change in the log of disbursed loans from bank *b* to firm *f* at time *t*. All columns include the controls of the baseline extensive margin regression (i.e., log size, non-eligible securities in the trading book as a fraction of assets, and log change in borrowing from the ECB). Standard errors are clustered at the bank level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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A. Appendix: QE announcement

This appendix provides some evidence suggesting that the QE policy announced on January 22, 2015, was likely not anticipated as of December 2014. We also find evidence of some anticipation in the one or two weeks leading to the announcement, but in any case, well within the month of January 2015. We emphasize that this one- or two-week anticipation period is not problematic for our empirical analysis. Indeed, the policy news would still be well within the month of January, which would not require altering our design in which we use December 2014 as the pre-announcement month. In fact, an anticipation by one or two weeks could well be responsible for the large impulse in lending that we observe in January 2015.

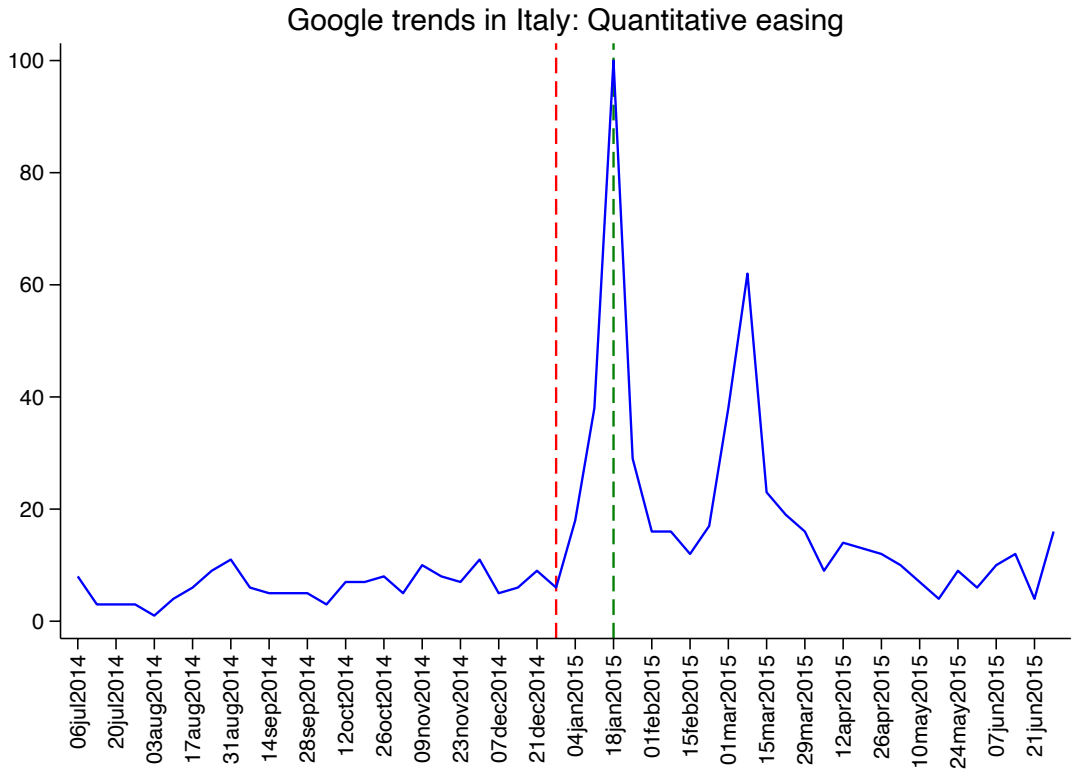
To study this issue, we use newspaper articles and Google Trends data for QE-related searches in Italy. These two approaches point to the same evidence — which is reassuring. That is, they both suggest a slight anticipation of the announcement by one or two weeks, but they show no clear expectations of the announcement as of December 2014.

With respect to newspaper articles that focus on financial issues (i.e., articles from *The Wall Street Journal*, *The Economist*, and *Financial Times*), only a few of them mention quantitative easing in December 2014, and they do so either by discussing this policy broadly, not only for Europe but also for the US and Japan, or by mentioning it as a possible policy in a general year-ahead type of discussion. One such article is in *The Wall Street Journal*, two in *The Economist*, and one in the *Financial Times*.

Starting in January 2015 and up to the week before the announcement, a few articles mention the possibility of QE in Europe. A clear sign that the PSPP policy is expected is evident only a couple of days before the actual announcement: "ECB Executive Board's QE Proposal Calls for Roughly EUR 50 Billion in Bond Buys per Month" (*The Wall Street Journal*, January 21); "Four Reasons Why QE Will Be Different in the Eurozone" (*Financial Times*, January 20).

We then look at Google Trends data for searches of QE and related terms in Italy during our sample window, as depicted in Figure A1. The graph clearly shows an increase in searches starting in the week of January 4–10, 2015, which then peaks in the week of January 18–24 (i.e., the week of the announcement). Importantly, we observe no significant increase in search activity in the months leading up to the announcement, in line with what we find about the newspaper articles.

Figure A1: Google searches of QE and related terms in Italy



The figure is based on the web searches for the topic "Quantitative easing" on Google for the period June 2014 to June 2015 in Italy, retrieved from the Google Trends website (https://trends.google.com/trends/). The vertical axis is normalized to 100 for the week with the highest number of searches. The two dotted lines refer to the weeks of December 28 – January 3 and January 18 – 24, respectively.

B. Trading activity of exposed banks

This appendix provides evidence that exposed banks did not change their holdings of PSPP-eligible securities after the January 2015 announcement, in comparison to non-exposed banks. This result is consistent with the results presented in the main part of the paper, that is, that the PSPP mainly affected the most illiquid banks. Thanks to the PSPP, the most illiquid banks have the option to liquidate their holdings of eligible securities in response to liquidity shocks, with likely lower adverse price impacts. But such liquidity shocks need not happen right after the PSPP announcement—in fact, they might not materialize at all. In other words, the PSPP increases banks' option value of liquidating their holdings of sovereign securities in the trading book, but such option might not be used in our sample window or even in the following months, if the underlying liquidity shocks do not hit the Italian banks.

We use an empirical specification similar to Peydro et al. (2020), in which the unit of observation is the trading activity of security *s* of bank *b* at time *t*. A security is defined at the most disaggregate level, that is, by its ISIN. We control for ISIN-by-time fixed effects to absorb trading activity that might be related to changes in the supply of a given security or to other factors that affect all banks equally.

We define the trading of security *s* by bank *b* at time *t* to account for adjustments along the intensive and extensive margins:

$$Trading_{s,b,t} = \frac{Quantity_{s,b,t} - Quantity_{s,b,t-1}}{0.5 * (Quantity_{s,b,t} + Quantity_{s,b,t-1})}.$$

That is, this variable takes a value between -200 and +200, with the extreme values representing the sale of all securities *s* held by the bank or the purchase of a security that was not previously held in the balance sheet, respectively.

We then estimate the following equation, which builds on our baseline specification:

$$Trading_{s,b,t} = \sum_{\tau \neq 2014m12} \beta_\tau \times I_\tau \times QE_b + \sum_{\tau \neq 2014m12} \gamma_\tau \times I_\tau \times \boldsymbol{Y_b} + \delta Z_{b,t} + \psi_b + \psi_{s,t} + \varepsilon_{s,b,t}$$

Similar to our main specification, the variable QE_b is the exposure dummy, and $\boldsymbol{Y_b}$ and $\mathrm{Z_{b,t}}$ include size, non-eligible securities in the trading book, and the log change in borrowing from the ECB. We also include security-by-time fixed effects, $\psi_{s,t}$, as discussed above, and bank fixed effects.

Table B1 reports the result. Column 1 estimates the regression using eligible securities in the trading book, that is, the securities that are used to construct our measure of exposure. The results show that exposed banks do not significantly change their holdings of eligible securities in comparison to non-exposed banks. For completeness, column 2 repeats the same exercise using eligible securities in the available-for-sale portion of the banking book. Because regulation makes it very difficult for banks to sell securities in the hold-to-maturity portion of the banking book, we do not include them in the regression. Column 2 shows that there are no significant changes even when we look at available-for-sale eligible securities.

Overall, the results are consistent with the effects of the PSPP through the liquidity value of the eligible securities held by banks. The PSPP likely increased the option value of selling the securities in the case of a liquidity shock, but we do not observe such shocks materializing in our sample.

Table B1: Trading activity

	(1)	(2)
[2014m7]× QE_b	0.611	0.036
	[0.440]	[0.058]
[2014m8] × QE_b	0.514	0.054
	[0.560]	[0.050]
[2014m9] × QE_b	-0.572	0.062
	[0.613]	[0.060]
[2014m10] × QE_b	-0.128	-0.035
	[0.512]	[0.076]
[2014m11] × QE_b	-0.213	0.010
	[0.535]	[0.063]
[2015m1] × QE_b	-0.014	0.029
	[0.710]	[0.069]
[2015m2] × QE_b	0.350	-0.007
	[0.604]	[0.103]
[2015m3] × QE_b	-0.766	0.040
	[0.668]	[0.070]
[2015m4] × QE_b	0.087	0.065
	[0.694]	[0.066]
[2015m5] × QE_b	0.434	0.061
	[0.426]	[0.059]
[2015m6] × QE_b	0.267	0.044
	[0.549]	[0.051]
Observations	3,304	3,343
R-squared	0.356	0.173
Security-time FEs	Yes	Yes
Bank fixed effects	Yes	Yes
Size	Yes	Yes
Non-eligible securities	Yes	Yes
EBC lending	Yes	Yes

The dependent variable is $Trading_{s,b,t}$. Column 1 uses QE-eligible securities in the trading book, and column 2 uses QE-eligible securities in the available-for-sale portion of the banking book. Standard errors are clustered at the bank level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C. Spillovers on lending supply of PSPP-exposed banks

The analysis at the bank-province level has established that a branch of a bank that faces PSPP-exposed competitors experiences a decline in lending growth if the bank is not exposed, but no decline if the bank is itself exposed. In this appendix, we perturb the baseline intensive margin result, building on the approach of Berg, Reisinger, et al. (2020) to further show that the lending supply of PSPP-exposed banks is not affected by the competition of other exposed banks.

Berg, Reisinger, et al. (2020) suggest that the outcome on treated banks — or firms more generally — might depend not only on whether a bank is treated but also on the fraction of treated banks among direct competitors. We adapt their econometric spillover model to our setting à la Khwaja and Mian (2008), so that we estimate

$$\Delta \log L_{b,f,t} = \beta[\text{Jan2015}]QE_b + \beta^T[\text{Jan2015}]QE_bC_p + \beta^C[\text{Jan2015}](1 - QE_b)C_p + \gamma[\text{Jan2015}]\boldsymbol{Y_b} + \delta Z_{b,t} + \psi_b + \psi_{f,t} + \varepsilon_{b,f,t},$$

where QE_b is the exposure dummy and C_p denotes the competition from QE-exposed banks in province *p* in which bank *b* is located, as previously defined.

Column 1 of Table C.1 provides the results for the main coefficient of interest (i.e., β). The results show that exposed banks increase their lending supply in response to the QE announcement, and the magnitude is virtually the same as in the baseline specification. One possible issue with our specification is that a small number of firms borrow from bank branches located in a different province. To deal with that, column 2 repeats the analysis by defining the exposure dummy C_p based on the province of the firm, as opposed to the province of the bank as in column 1.[25] Again, the results are essentially unchanged.

[25] In this second case, the firm-time fixed effect implies that we cannot separately identify the coefficients β^T and β^C but only their sum because there is no within-firm variation in the regressors QE_bC_p and $(1 - QE_b)C_p$. However, our coefficient of interest is β, which is not affected by this issue.

In our second test, we construct an indirect measure of exposure. In particular, for bank b, we compute the share τ_b of loans that are given to firms who have at least another lending relationship and such a relationship is with an exposed bank. We do so because there might be spillovers that differentially affect banks in a given province. We estimate a similar model as before, where we replace the competition index $\mathrm{C_p}$ with τ_b:

$$\Delta \log \mathrm{L_{b,f,t}} = \beta[\mathrm{Jan2015}]\mathrm{QE_b} + \beta^{\mathrm{T}}[\mathrm{Jan2015}]\mathrm{QE_b}\tau_\mathrm{b} + \beta^{\mathrm{C}}[\mathrm{Jan2015}](1-\mathrm{QE_b})\tau_\mathrm{b} + \gamma[\mathrm{Jan\ 2015}]\,\boldsymbol{Y_b} + \delta \mathrm{Z_{b,t}} + \psi_\mathrm{b} + \psi_\mathrm{f,t} + \varepsilon_\mathrm{b,f,t}$$

Column 3 of Table C.1 provides the results for the coefficient of interest β, which shows again that the result of the baseline regression is virtually unchanged.

Table C1: Spillovers on lending supply of QE-exposed banks

	(1)	(2)	(3)
[2015m1] × QE_b	2.720***	2.556***	1.719**
	[0.502]	[0.433]	[0.761]
Observations	5,867,308	5,860,335	5,867,308
R-squared	0.394	0.394	0.394
Bank FEs	Yes	Yes	Yes
Firm-time FEs	Yes	Yes	Yes
Size	Yes	Yes	Yes
Non-eligible securities	Yes	Yes	Yes
ECB lending	Yes	Yes	Yes

The dependent variable is the change in the log of disbursed loans from bank *b* to firm *f* at time *t*. In columns 1 and 2, we include the controls QE_bC_p and $(1-QE_b)C_p$ interacted with the January 2015 dummy. In column 1, p is the province of the bank, whereas in column 2, p is the province of the firm. Column 3 includes the controls $QE_b\tau_\mathrm{b}$ and $(1-QE_b)\tau_\mathrm{b}$, where τ_b is the share of loans of bank b that are given to firms who also have a lending relationship with at least one exposed bank. Standard errors are clustered at the bank level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.