Essays on Finance and Economic Policy

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Abstract

This dissertation presents three empirical studies on how financial frictions shape the transmission and effects of economic policy. The dissertation investigates three settings: financial regulation, corporate tax policy, and monetary policy. The first study reveals how a particular type of banking regulation leads to distorted lending behavior when banks are subject to informational and agency frictions. The second study shows how financial frictions lower the responsiveness of corporate investment to a large-scale investment tax credit. The third study provides insights into when the transmission of unconventional monetary policy ('quantitative easing') through the banking system fails.

Contents

List of Tables

List of Figures

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Introduction

A growing body of research shows that financial frictions affect the behavior of firms, households, and financial intermediaries. While we have evidence of the presence of such frictions, less is known about how they affect the transmission and effects of economic policy. As this dissertation shows, policy that does not take into account how frictions shape the behavior of economic agents runs the risk of producing unintended consequences or losing some of its effectiveness.

This dissertation uses novel micro-level financial data to study how financial frictions matter for economic policy in three settings: financial regulation, corporate tax policy, and monetary policy. The first chapter shows how financial frictions lead banks to misallocate credit in response to higher regulatory capital ratios, which adversely affects aggregate productivity. The second chapter shows that financial frictions affect how firms respond to an investment tax credit designed to stimulate investment. The third chapter studies unconventional monetary policy, which partly relies on the presence of financial frictions in the banking sector to be effective. This final chapter shows that concurrent financial regulation can undo part of this transmission mechanism even when financial frictions are present. All three chapters rely on proprietary administrative data provided by the Bank of Portugal. The data provides information on the financial statements of both banks and firms, as well as the credit relationships that link them.

The first chapter provides evidence that banks hit with a negative shock to capital adequacy inefficiently reallocate credit and that this credit reallocation contributes to the misallocation of capital. A regulatory intervention by the European Banking Authority in

2011 provides a natural experiment that unexpectedly increased capital requirements for a subset of banks. Using administrative data from the Bank of Portugal, the chapter shows that exposed banks cut back on credit for all but a subset of financially distressed firms for which banks had been underreporting incurred loan losses. The credit allocation to these underreported firms is consistent with two perverse lending incentives. First, banks roll over loans to distressed firms with underreported losses to avoid realizing a large loss in case of firm insolvency. Second, undercapitalized banks gamble for the resurrection of distressed borrowers and underreport losses to avoid regulatory scrutiny of risky loans. The chapter develops a method to back out the underreporting of loan losses using detailed loan-level data and then shows that the credit reallocation affects firm-level investment and employment. A partial equilibrium estimate suggests that the credit reallocation accounts for close to 20% of the decline in productivity in this period.

The second chapter studies how debt frictions and demand affect corporate investment using administrative data from a large temporary investment tax credit in Portugal. Exogenous variation in demand for exporting firms comes from product-destination-level changes in foreign demand. Debt frictions are proxied by an index of different debt-earnings ratios. Debt has a strong, non-linear effect on the likelihood that a firm invests in response to the tax credit. Firms in the best two quartiles of the debt-earnings index have roughly equal predicted take-up probabilities. For firms in the third quartile predicted take-up drops by 50% while firms in the worst debt-earnings quartile have a predicted take-up rate close to zero. There are also important interactions between debt and demand. For firms in the best debt-earnings quartile, demand has a highly significant positive effect with a 10% increase in predicted demand leading to a 9 p.p. higher take-up probability. In contrast, for firms in the worst debt-earnings quartile, demand ceases to have a significant impact on take-up. OLS attenuates the effects of demand by a factor of three. These results highlight the limit of panel regressions that only allow for a linear effect of debt on investment, do not instrument for demand, and do not allow for an interaction between demand and debt.

The final chapter studies the transmission channels of the European Central Bank's

(ECB) asset purchase programs via the banking sector using proprietary data from the Bank of Portugal. Banks that hold larger amounts of assets eligible for ECB purchase prior to announcement of the programs realize trading profits from selling these assets following the announcement. Banks use most of these gains to increase their cash holdings. There is a moderate positive effect on loan approval rates for new corporate borrowers, which is stronger for riskier borrowers. However, banks do not offer significantly lower interest rates or provide additional loans to existing customers. The chapter investigates an additional origination channel. The ECB's purchase of asset-backed securities (ABS) and covered bonds does not lead banks with pre-existing issuance technology to originate more loans. The results suggest that the pass-through of asset purchase programs to lending conditions may occur through channels other than bank balance sheets.

Chapter 1

Chapter 1: When Losses Turn Into Loans

1.1 Introduction

Financial crises often leave behind a weakened banking sector. A weak banking sector can stifle the post-crisis recovery when banks become impaired in their ability to channel resources to the most productive firms in the economy. The Japanese banking system following the crash in the 1990s is often cited as an example of this phenomenon as Japanese banks are thought to have continued lending to nearly-insolvent 'zombie' firms, crowding out lending to more productive firms. With Europe following the Japanese pattern of a prolonged economic slump, the question of whether weak banks impede economic recovery arises with new urgency.[1]

Existing research has not been able to establish a credible causal chain from a weak banking sector to adverse effects on productivity and growth. One strand of literature has provided evidence for an empirical link between weak banks, measured by the size of their regulatory capital cushion, and lending to failing ('zombie') firms but not established

[1]See for example Hoshi and Kashyap (2015) on the parallels between Japan and Europe.

causality.[2] A separate strand of literature has suggested that propping up failing firms can have real economic costs by crowding out healthy firms but not investigated how 'zombie' lending can directly drive the misallocation of resources in the economy.[3] However, in order to fully assess the effect of a weak banking sector, it is crucial to trace the entire causal chain from the distorted lending incentives of undercapitalized banks to resource misallocation and aggregate productivity.

In this paper, we show that a weak banking sector has contributed to a slowdown in productivity in the aftermath of the European sovereign debt crisis. We exploit an intervention by the European Banking Authority in 2011, which caused a subset of banks to be below the regulatory capital standards. We show that affected banks respond to their diminished capital adequacy by distorting their reporting and lending choices at the micro-level. We then show how these distorted incentives at the micro-level drive the misallocation of resources across firms and aggregate up to large negative effects on productivity at the macro-level. Our results have important implications for regulatory policy. First, ensuring in pre-crisis times that banks are sufficiently capitalized, even after experiencing large losses in a financial crisis, eliminates the distorted lending incentives that arise when banks are undercapitalized. It also eliminates difficult post-crisis policy decisions on how to recapitalize banks. Second, our results point to the importance of eliminating some bank discretion in how to reach higher capital ratios since in the presence of discretion, banks will not only cut back on lending but also distort the allocation of lending.

We establish the first link in the causal chain by exploiting quasi-experimental variation in banks' capital requirements. The European Banking Authority (EBA) in 2011 unexpectedly announced that a subset of Portuguese banks had to meet substantially higher capital ratios by mid-2012. Our exposure definition exploits both eligibility, which was based on a size cut-off, and the severity of the capital shortfall, which was determined by prior sovereign

[2]See Peek and Rosengren (2005), Schivardi *et al.* (2017) and Acharya *et al.* (2017).

[3]See Caballero *et al.* (2008), Moreno-Serra *et al.* (2016).

bond holdings.[4] Banks correctly anticipated that as long as they made a credible attempt to comply with the EBA requirements, the Portuguese government would step in at the compliance deadline to make up any remaining capital shortfall.[5] All exposed banks received a capital injection at the EBA deadline, which allowed them to comply with the EBA requirements.

We complement the quasi-experimental variation in banks' capital requirements with a method to detect the underreporting of loan losses at the firm-bank level. In the period we study, banks are required to deduct a fraction of a loan as a loss when a corporate borrower falls behind on loan repayments. In Portugal, the size of this mandatory deduction is tied to the time the firm has been behind on repayment. Banks can hence reduce loan losses by underreporting the time a firm has been behind on repayment. We develop an algorithm to measure loss underreporting in monthly bank reports on the same firm.[6] Because the regulatory deduction schedule features several discrete jumps, the incentive to underreport is largest just below such a jump ('bunching'). We conduct several validation tests to show that underreporting responds to these jumps in the regulatory schedule, thus confirming that banks strategically report to minimize losses.[7]

Our main result, which establishes the first link in the causal chain, is that exposed banks respond to higher capital requirements not only by cutting back on lending but also by reallocating credit to a subgroup of distressed firms whose loan losses banks had been underreporting prior to the EBA announcement. These results are estimated in a

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[4]Defining exposure only based on eligibility would imply that we compare big and small banks. In addition, this approach would reduce statistical power since not all eligible banks were affected by the EBA exercise. We confirm that both groups of banks, based on our exposure definition, are balanced on observables (though some moderate size imbalance remains) and that sovereign bond holdings do not follow differential trends prior to the EBA announcement, which could be correlated with differential trends in credit supply.

[5]The IMF and the European Commission had provided a bailout the Portuguese government in early 2011. Part of the bailout money was earmarked for the recapitalization of banks.

[6]Since banks do not provide loan identifiers, we cannot track how long each loan has been overdue in the data. We hence have to approximate this exercise with a more involved approach.

[7]Unlike existing work (Diamond and Persson (2016), Dee *et al.* (2017), Best and Kleven (2016)), we do not identify bunching based on a cross-sectional distribution over a continuous variable (such as house prices or test scores) but directly calculate bunching from repeated observations of the same firm-bank pair.

difference-in-difference design, in which we compare changes in credit from exposed and non-exposed banks to the same firm. In contrast, exposed banks do not increase credit to distressed firms that are not underreported. We also show that the credit reallocation is unlikely to be driven by firm-level shocks increasing credit demand from distressed, underreported firms. Exposed banks change their credit allocation only in the period between the EBA announcement and the EBA deadline. This implies that firm-level shocks would have to match the exact timing of the regulatory intervention to be able to account for our results. Moreover, given that we control for firm-level changes in credit, firm-level shocks would have to drive up credit demand at exposed banks but not at non-exposed banks. Furthermore, we show that underreported firms borrowing from exposed and non-exposed banks do not have diverging pre-trends in credit or liquidity, that observable measures of firm quality are not correlated with the borrowing share from exposed banks, and that the results are robust to controlling for relationship characteristics such as whether the bank is the main lender.

A natural explanation for these results is that the EBA intervention heightened distorted lending incentives for exposed banks. The first lending incentive is driven by exposed banks attempting to delay the recognition of losses. We show that banks had been underreporting loan losses with the onset of the European sovereign debt crisis in 2010. While this underreporting allows banks to boost reported capital and to avoid costly equity issuance, it also locks banks into a vicious cycle with financially distressed firms whose losses have not yet been fully accounted for on banks' financial statements. Cutting lending to a distressed firm runs the risk of pushing that firm into insolvency, which would force the bank to recognize previously underreported losses. The capital requirements imposed by the EBA gave exposed banks an additional reason to avoid capital-reducing losses and to roll over loans to underreported firms. Consistent with this incentive to delay the recognition of losses, we find that exposed banks sharply increased the amount of loss underreporting for the duration of the EBA intervention.

The second lending incentive arises as exposed banks increase their exposure to high-volatility firms in anticipation of the government bailout. At the same time, these loans were more likely to be underreported since this would reduce the likelihood of a potential monitoring of these loans. Consistent with this motive, we find that among firms with loan losses, underreported firms have higher levels of volatility, as measured by sales volatility and predicted default probabilities.

We then establish the second link in the causal chain and show that the credit shock induced by the EBA intervention had real effects on investment and employment. Estimating the size of the credit effect at the firm-level allows us to confirm that firms do not undo the firm-bank level credit shocks by substituting among different lenders. We then estimate the effect of the credit shock on employment and investment by instrumenting for the firm-level credit change with the firm-level pre-shock borrowing share from exposed banks. The credit shock, which is positive for underreported firms and negative for all other firms, has a large and significant pass-through into employment and investment. A one euro change in credit supply leads firms to adjust their labor spending by 16 cents and their investment spending by 40 cents. In addition, we find that the credit shock significantly decreases the likelihood of underreported firms exiting, while increasing the likelihood of exit for all other firms. Because the firm-level credit shocks are large, the effects on investment and employment are sizable. A partial equilibrium calculation implies that firms borrowing entirely from exposed banks decrease employment and investment by 9% and 6%, respectively. The equivalent calculations for underreported firms implies a relative increase in employment and investment by 8% and 6%, respectively.

We complete the causal chain by translating the firm-level effects into productivity losses. Following Petrin and Levinsohn (2012), we decompose total productivity growth into firm-level growth rates of technical efficiency and a term that captures how efficiently production inputs are allocated across firms in the economy. This decomposition allows us to map our cross-sectional firm-level regression results into aggregate productivity growth. Based on these partial equilibrium estimates, the EBA intervention accounts for over 50% of the decline in aggregate productivity in 2012. This is driven by the fact that the credit reallocation implies that capital is reallocated to underreported firms with low factor returns and that the EBA-induced credit crunch reduces factor use by firms where those factors would have generated a high return. A simulation exercise suggests that keeping the level of credit unchanged but maintaining the credit reallocation to underreported firms accounts for close to 20% of the productivity decline in 2012. This result suggests that the credit reallocation matters for productivity losses above and beyond the effect of the credit crunch. We also show that there are additional productivity losses from negative spillover effects that underreported firms have on firms not exposed to EBA banks in the same industry.

Our work is closely related to the literature on 'zombie' lending by undercapitalized banks (see Sekine *et al.* (2003) for a survey on Japan). Relative to this literature, we overcome key identification challenges and trace the negative effects of distorted lending on aggregate productivity. Existing research shows that banks close to the regulatory capital constraint tend to give more loans to poorly performing firms, defined by some observable metric, than banks far away from the regulatory constraint (Peek and Rosengren (2005), Albertazzi and Marchetti (2010), Acharya *et al.* (2017), and Schivardi *et al.* (2017)). Interpreting these results as a causal relationship between bank capital and 'zombie' lending is problematic since distance from the constraint may be correlated with unobserved bank quality. We address this problem by relying on quasi-experimental variation in capital adequacy.

We address two additional empirical challenges present in the existing literature. First, by focusing on a temporary change in regulatory policy and on firms that have lending relationships with both exposed and non-exposed banks, we mitigate concerns in the existing literature that observed changes in credit allocation may pick up (efficient) lending to distressed firms with good long-run fundamentals. Second, we are able to estimate the extent of 'zombie' lending more precisely by relying on our underreporting measure instead of measures of firm performance. We show that distorted lending is present only for the subset of poorly performing firms whose loan losses had been underreported by the bank. Estimating the change in credit across all poorly performing firms would hence underestimate the extent of 'zombie' lending.

Our work ties in the 'zombie' lending literature with research on the real effects of this phenomenon. So far, there has been no conclusive evidence on the how costly distorted lending is for the economy. Existing research provides evidence that the continued existence of 'zombie' firms can have negative spillovers on healthy firms in the same industry (Caballero *et al.* (2008), Moreno-Serra *et al.* (2016), and Acharya *et al.* (2017)). Schivardi *et al.* (2017) however find no such effects in Italy. We take a much more direct approach and show how credit distortions drive the misallocation of resources, which in turn lowers aggregate productivity. In addition, we confirm the existence of negative industry-level spillovers using a quasi-experimental version of the specification in Schivardi *et al.* (2017).

We build on a large literature documenting the existence of frictions that distort the behavior of financial institutions. The first mechanism, which we call delayed loss recognition, is related to a growing research agenda on how banks manage financial reporting to improve performance when performance metrics depend on reported figures (Acharya and Ryan (2016), Falato and Scharfstein (2016)). The lending behavior we document is similar to gains trading which involves financial institutions selling assets with high unrealized gains while retaining assets with unrealized losses to boost regulatory capital (Ellul *et al.* (2015), Milbradt (2012)). The second mechanism, gambling for resurrection of distressed borrowers, is related to a large literature on "risk shifting" or "asset substitution" by financial institutions (Jensen and Meckling (1976), Biais and Casamatta (1999)). In the context of Europe, several papers have documented that undercapitalized banks increase their exposure to risky sovereign debt (Acharya and Steffen (2015), Drechsler *et al.* (2016), Crosignani (2017)) and risky borrowers (Bonaccorsi and Kashyap (2017)).

Our identification strategy follows a growing literature that uses shocks to bank health to study effects on credit (Chodorow-Reich (2014), Khwaja and Mian (2008)). In particular, we contribute to a literature that highlights potential unintended consequences of banking regulation (Behn *et al.* (2016), Koijen and Yogo (2015)). Gropp *et al.* (2017) exploit the same regulatory intervention by the European Banking Authority to show that banks adjust to higher regulatory capital requirements by cutting back on assets (including loans) rather than raising equity. While we confirm the finding that banks reduce credit supply in response to higher minimum capital ratios, our primary contribution lies in documenting reallocation effects arising from distorted lending incentives.

Finally, we connect a broad literature on banking frictions with a literature on misallocation, which argues that the misallocation of production factors is a key cause of low productivity and slow economic growth (Restuccia and Rogerson (2008), Hsieh and Klenow (2009)). A growing number of papers have argued that the presence of financial frictions at the firm-level is a driver of misallocation (Gopinath *et al.* (2017), Moll (2014), and Midrigan and Xu (2014)). We show that bank-level friction can lead to differential tightening of firm-level financial frictions, thus providing a direct channel through which banks might contribute to the misallocation of capital.

The remainder of the paper is organized as follows. Section 2 describes our method for measuring loss underreporting. Section 3 describes the natural experiment, the data and our results. Section 4 quantifies the effects on aggregate productivity. Section 5 concludes.

1.2 Loss Underreporting: A Tool to Measure Distorted Lending Incentives

This section explains why underreporting is correlated with distorted lending incentives and provides supporting empirical evidence. We provide background on the regulatory environment that governs the reporting of loan losses in Portugal and describe our methodology for measuring underreporting of loan losses. Finally, we provide evidence that our method produces reliable results by showing that underreporting responds to incentives present in the regulatory rules.

1.2.1 Two Sources of Distorted Lending Incentives

We argue that the underreporting of loan losses is correlated with two types of distorted lending incentives: the delayed recognition of losses and gambling for the resurrection of distressed borrowers.

Existing research has argued that bank shareholders often resist raising new capital (Myers and Majluf (1984), Admati *et al.* (2017)) and prefer to find other ways to improve regulatory capital ratios, in particular when the bank is already undercapitalized. Since reported losses deplete a bank's regulatory capital, banks can protect capital by delaying the recognition of loan losses. This mechanism is consistent with a growing body of research that shows how banks manage financial reporting to improve performance when performance metrics are based on reported numbers (Acharya and Ryan (2016), Falato and Scharfstein (2016)). Since loans constitute the largest asset class for Portuguese banks, underreporting loan losses is an effective tool to boost regulatory capital.

The underreporting of loan losses locks undercapitalized banks in a vicious cycle with financially distressed firms whose losses have not yet been fully accounted for on banks' financial statements. If a bank cuts lending to such a firm, it runs the risk of pushing the firm into insolvency and having to recognize the entire underreported loss. In contrast, if the bank rolls over a loan, it avoids the risk of having to mark down the inflated value of the loan. This lending behavior is similar to gains trading where financial institutions sell assets with high unrealized gains while retaining assets with unrealized losses to boost regulatory capital (Ellul *et al.* (2015), Milbradt (2012)). The incentive to avoid recognizing a loss to boost regulatory capital leads to the distorted incentive to lend to distressed firms even when such loans have negative net-present value (NPV). Delayed loss recognition predicts that banks underreport loans that have large uncovered losses in the event of firm insolvency, for example loans with little collateral. The reason is twofold. First, un-collateralized loans have a more front loaded regulatory deduction schedule making underreporting more valuable relative to collateralized loans. Second, to the extent that banks anticipate having to roll over loans to underreported firms, banks anticipate that rolling over loans to firms whose loans are backed by collateral, which can be sold in the case of insolvency, is less valuable than rolling over loans where the bank would have to bear the full loss in case of insolvency.

The second type of distorted lending incentives arises when undercapitalized banks gamble for the resurrection of their distressed borrowers. If a bank is sufficiently undercapitalized that it will default in some states of the world, bank shareholders start to like gambles. Lending to distressed firms constitutes a gamble if the states of the world in which those distressed firms go under are also the states of the world in which the bank itself goes under. In that case, limited liability protects bank shareholders from losses in these states. Bank shareholders hence only care about states in which distressed firms recover, which are likely to coincide with the bank remaining solvent. Such “risk shifting" or “asset substitution" behavior leads banks to invest in negative NPV projects when these projects have sufficient variance to present a valuable out of the money call option to bank shareholders (Jensen and Meckling (1976)).[8] Banks that gamble for the resurrection of distressed firms also have an incentive to reduced reported loan losses on these firms to avoid a potential monitoring of these loans by financial markets or the European regulator.

1.2.2 Loan Loss Reporting in Portugal

We exploit the regulatory framework on loan impairment losses in Portugal to measure the underreporting of loans losses. An impairment loss is an expense that a bank deducts from its income to reflect that a loan may not get repaid in full. Impairment losses reduce banks' regulatory capital by reducing retained earnings. On the balance sheet, impairment losses mark down the value of an asset.

Accounting rules and regulatory policy determine the minimum size of impairment losses. In the period we study, banks in Portugal are subject to two types of such regulation: international accounting rules (IAS39) and Portuguese rules (notice 3/95).[9] The latter ties

[8] This theory has recently received attention in the context of the European sovereign debt crisis (Acharya and Steffen (2015), **?**).

[9] Notice 3/95 was in place prior to the introduction of IAS39 but continued to matter for bank behavior for two reasons. First, banks have to follow notice 3/95 for their financial reporting on an individual basis. Second, notice 3/95 becomes effectively binding for many banks on a consolidated basis as well. Banks subject to the standardized approach under the Basel regulatory framework have to deduct the difference between impairment losses under 3/95 and IAS39 from their regulatory capital, making 3/95 the effectively binding regulation. The remaining banks are not subject to 3/95 banks on a consolidated basis. However, even those banks avoid a high disparity between impairments under IAS39 and notice 3/95. Moreover, impairment losses deducted at the individual basis will still negatively affect regulatory capital on the consolidated basis through

the size of the impairment to the number of months a loan has been overdue, as well as the type of collateral (see Figure 1).[10] The Portuguese rules imply that banks can reduce reported losses by managing the reported time a loan has been overdue.

We exploit the detailed reporting of overdue loans by banks in order to measure loss underreporting. Banks are required to report the length a loan has been overdue, as well as the type of collateral, to the Central Credit Register (*Central de Responsabilidades de Credito*) at a monthly frequency.[11] Banks report the time overdue in discrete intervals, or buckets, which correspond to the regulatory buckets shown in Figure 1.

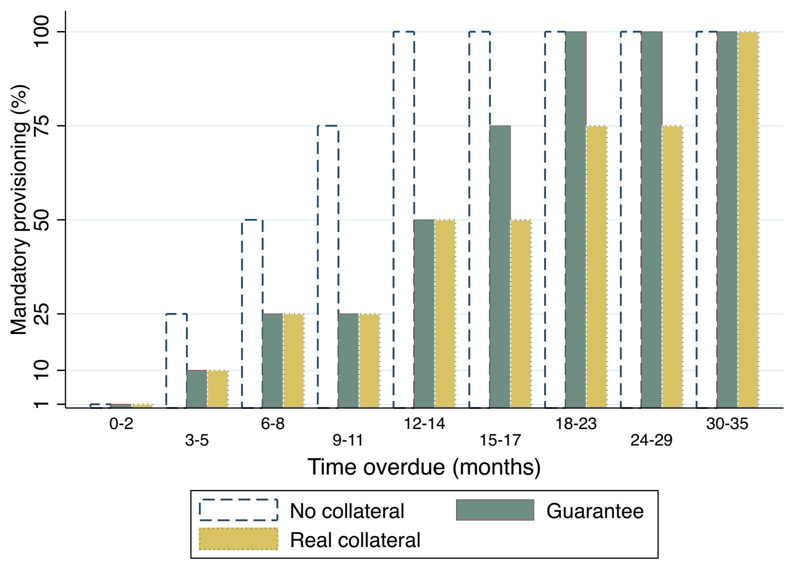


Figure 1: *Regulatory Rules on Loss Deductions*

Notes. The graph shows the regulatory rules that govern mandatory minimum deductions for loan losses based on the number of months a loan has been overdue, and the type of collateral.

We focus on firm-finance loans granted to non-financial firms. Firm-finance loans tend to

retained earnings. The Portuguese regulator refers to impairment losses under 3/95 as loan loss provisions but we use the term impairment losses for ease of exposition.

[10]We use the term overdue as a synonym for a borrower being behind on payments. A loan is non-performing if the borrower is more than 90 days behind on repayments. A borrower can have overdue loans which are not yet non-performing if the payments are overdue for less than 90 days. We use default and overdue as synonyms.

[11]Banks start reporting this variable in 2009.

have longer maturities than some other credit products, such as credit cards, and therefore are better suited for detecting loss underreporting which requires us to track a lending relationship over time. Firm-finance loans constitute the main loan product for firms and capture about 36% of the banks' corporate loan portfolio. However, since the vast majority of firms have at least one firm-finance loan with each of their lenders, we capture almost the entire population of bank-dependent firms in Portugal. In Appendix A we present descriptive statistics on the loans that we use to measure the underreporting of loan losses. 73% of loans are collateralized and 67% have an origination maturity above a year.

1.2.3 A Method to Detect Underreporting of Loan Losses

Our aim is to measure to what extent banks underreport loan losses by managing the reported time a loan has been overdue. Unfortunately, we cannot simply compare reported time overdue to the actual time overdue in the data since banks do not provide identifiers to track loans over time. Instead, we develop an algorithm to measure the extent of underreporting in each reporting bucket for all firm-bank pairs at a monthly frequency.

Algorithm We now illustrate the basic version of the algorithm. We denote the observed loan balance reported in overdue bucket k in month t by $B_{ib}(t;k)$ where i denotes the firm and b the bank. We drop the firm-bank subscripts in the discussion that follows. There are 14 reporting buckets which correspond to the overdue buckets in the regulatory schedule: $k \in \{\{0\},\{1\},\{2\},\{3,4,5\},\ldots,\{30,\ldots,35\}\}$.

The goal of the algorithm is to measure excess mass, a term we borrow from the bunching literature.[12] We define excess mass in an overdue bucket k in month t, $E(t;k)$, as the lending balance that is reported in a bucket k that exceeds the lending balance we would have

[12]Our set-up differs from the standard bunching setting where the researcher observes a continuous variable, such as house prices or test scores. In those settings, bunching can be measured based on excess mass in the observed cross-sectional distribution at points of particular importance, such as test score cut-offs (see Diamond and Persson (2016), Dee *et al.* (2017) or Best and Kleven (2016)). In our set-up, we instead calculate excess mass from repeated observations of the same firm-bank unit and detect discrepancies in observed reporting for the same firm-bank pair over time. In contrast to the standard setting, we also have to address the challenge that reported time is not continuous but discretized.

Table 1: *Example of Loss Underreporting*

	<30 days	Overdue 1 month	2 months	Performing credit	Excess mass
2012m1	EUR 50			EUR 450	0
2012m2		EUR 50		EUR 450	0
2012m3		**EUR 50**		EUR 450	50
2012m4			EUR 50	EUR 450	0

Notes. The table shows a stylized example of the loan data collapsed to the monthly firm-bank level. We show lending volumes of a hypothetical firm-bank pair. We show the first three reporting categories of how long a loan has been overdue. Performing credit denotes the balance of loans in the firm-bank pair which are not (yet) overdue. Panel A shows an example where the bank does not update the reported time overdue in March, which is registered as excess mass by the algorithm (mechanism 1). The excess mass column shows the excess mass as calculated by the formula given in the text. The last rows in each example illustrate that the algorithm is “memory-less": As long as reporting is consistent relative to the previous month, the algorithm does not register excess mass.

expected to observe in bucket k based on the amount observed at $t-1$. For the first three reporting categories, which consist of a single month, excess mass is defined as

$$E(t;k) = B(t;k) - B(t-1;k-1). \tag{1.1}$$

Intuitively, the loan balance we observe in bucket k at t must be the loan balance that has moved up from the preceding bucket in the previous period. We define excess mass as the deviation from this identity. For reporting buckets that consist of several months, we have to adjust this simple formula and introduce an auxiliary step, which is described in Appendix A.

Table 1 provides a stylized example of the loan data, a monthly firm-bank panel, with the overdue loan balance reported separately for each bucket. The third row of Table 1 shows an example where the bank does not update the reported time overdue, thereby underreporting how the long has been overdue. Banks use three mechanisms to adjust the reported time overdue: (a) they don’t update the reported time (as shown in Table 1), (b) they combine new overdue loan installments with the existing overdue loan balance and report a (lower) average time overdue[13], and (c) they grant new performing credit in exchange for the repayment of the longest overdue portion of the loan. In Appendix A, we provide stylized examples of the second and third type of behavior and show that most underreporting is driven by these two types of behavior.[14]

The algorithm is Markovian and only records inconsistencies relative to $t-1$. That is, it does not keep a tally of how far the reporting has fallen behind the 'true' time overdue. In Appendix A, we provide evidence from a subset of single-loan relationships, where we can track the 'true' time overdue and show that the gap between reported and true time widens over time. This suggests that the algorithm returns a lower bound of the underreporting of loan losses.

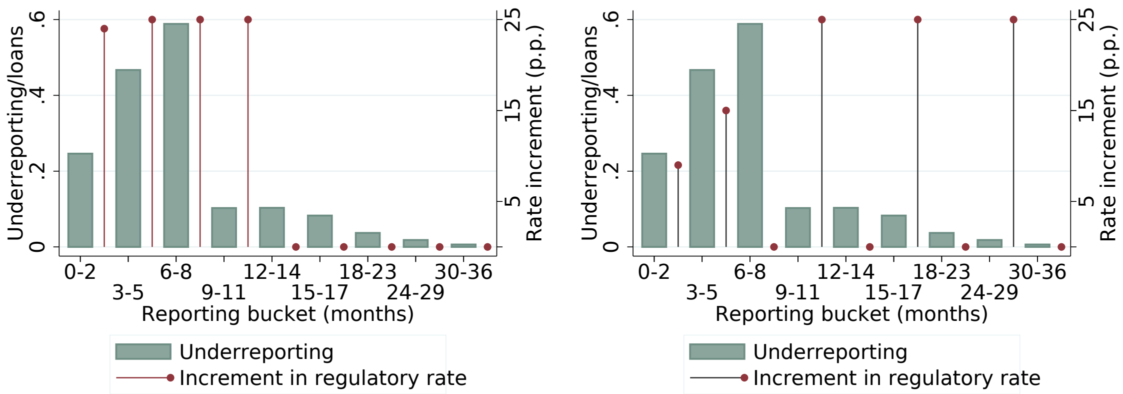
For ease of exposition, the version of the algorithm outlined here does not take into account flows in the data. Flows consist of additional loan installments falling overdue, loan repayments, or loan restructuring and write-offs. In Appendix A, we describe the full version which incorporates inflows and outflows in the data. Appendix A also describes extensive robustness checks.[15] We run the full version of the algorithm on the set of non-performing corporate firm-finance lending relationships in 2009-2016.

Validity Checks Given that the regulatory deduction schedule features several discrete jumps, we would expect banks to do most of their reporting management in reporting buckets just before a jump ('bunching'). We test whether underreporting in fact occurs in buckets just before a jump. Such responsiveness of bank behavior at the micro-level is

[13]According to the regulatory rules, banks should combine new overdue loan installments with the existing overdue balance but report everything at the longest time overdue, not at the average.

[14]There are two actions that banks can take to reduce reported loan losses that are not captured by the algorithm. First, banks can swap out all overdue credit for performing credit. This action will not be captured by the algorithm since there is no more overdue lending reported. Second, banks could prevent a firm from falling overdue in the first place by granting loans that allow the firm to stay current on loan repayments.

[15]We show that we can bound the effect of flows by calculating excess mass for the set of most restrictive and most permissive assumptions respectively. We show that the bounds are narrow since credit flows are quantitatively small relative to credit stocks.



(a) *Bunching Test* **(b)** *Placebo test*

Figure 2: *Underreported Losses by Reporting Category*

Notes. The graphs show the amount of loss underreporting scaled by the overdue loan balance by reporting bucket. We show averages across all firm-bank pairs for loans without collateral. The vertical lines denote increments in the regulatory impairment deduction rate from one reporting category to the next for loans without collateral. A dot at zero means that the rate remains constant between two buckets. The right panel show the rate increments for loans with collateral and illustrates the logic of the Placebo test described in detail in section 2 and the results of which are reported in table 20

evidence that our measure is indeed picking up strategic behavior.[16]

Figure 2a illustrates the intuition of our first validity test. We plot the distribution of underreported losses across reporting categories for all firm-bank pairs. We pick loans that have no collateral as an example. Figure 2a provides suggestive evidence that the amount of underreporting responds to the increments in the regulatory deduction rate, which we plot as vertical lines. We can formally test this responsiveness by regressing the amount of underreporting in a reporting category on the size of the rate increment in the next higher category. We run this regression separately for each type of collateral since the regulatory rules differ by collateral type. We describe the regression specification in detail in Appendix A.

The regression confirms that, for each type of collateral, the amount of underreporting is statistically significantly higher when there is an increase in the regulatory rate in the next higher bucket relative to buckets where the regulatory rate stays constant (see Appendix A).

[16]The algorithm does not restrict excess mass to be zero even when there is no increase in the regulatory rate in the next higher reporting bucket.

Moreover, we find that underreporting is higher if the increment in regulatory deduction rate is higher, suggesting that underreporting responds not only the location of the jumps in the regulatory rate but also to the size of the increment.[17]

Figure 2b shows a natural placebo test. If we regress underreported losses on the regulatory increments of another collateral type, we should not find positive and significant coefficients in categories where only the other collateral type features an a jump in the deduction rate. Appendix A provides evidence that we find negative coefficients for all three collateral types, suggesting that there is significantly *less* underreporting when only other collateral types feature an increase in the regulatory rate.

In Appendix A, we provide an additional validity check which is based on the sample of single-loan relationships, where we can directly trace the time a loan has been overdue. As expected, we find that underreporting is most pronounced in the months when the regulatory rates increases.

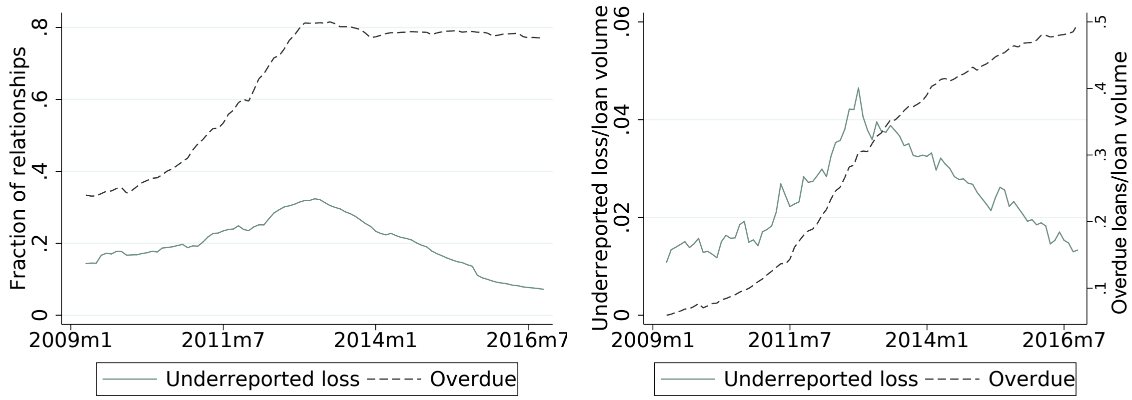
Underreporting as a Tool to Measure Distorted Lending Incentives

Underreporting of loan losses is a powerful tool to identify lending driven by distorted incentives. Banks only underreport about half of firms with overdue loans and this underreporting is very persistent, giving us meaningful variation among firms with overdue loans (see Appendix A for more details).[18] By relying on our measure of underreported losses, we overcome the challenge that distorted lending incentives do not necessarily apply to all firms that exhibit observable signs of financial distress or poor performance. Looking at the average effect for all poorly performing firms, as done in the existing literature, would

[17]There is one exception where this monotonicity fails: the largest increment for loans with either real collateral or borrower guarantees, which does not feature more underreporting relative to the second-largest increment. This non-monotonicity arises because loans in the reporting category just below the second-largest jump have to be declared non-performing, which has additional negative effects beyond increasing the impairment loss. Non-performing loan ratios are a closely watched indicator of bank health by both the regulator and financial markets giving banks a reason to concentrate their underreporting in lower reporting buckets.

[18]A variance decomposition confirms that most variation in underreporting is driven by within-firm rather than by between-firm variation. To obtain this decomposition, we regress the amount of underreporting on a firm, bank, time and relationship fixed effect.

underestimate the true extent of 'zombie' lending



(a) *Fraction of Relationships*

(b) *Share of Loan Volume*

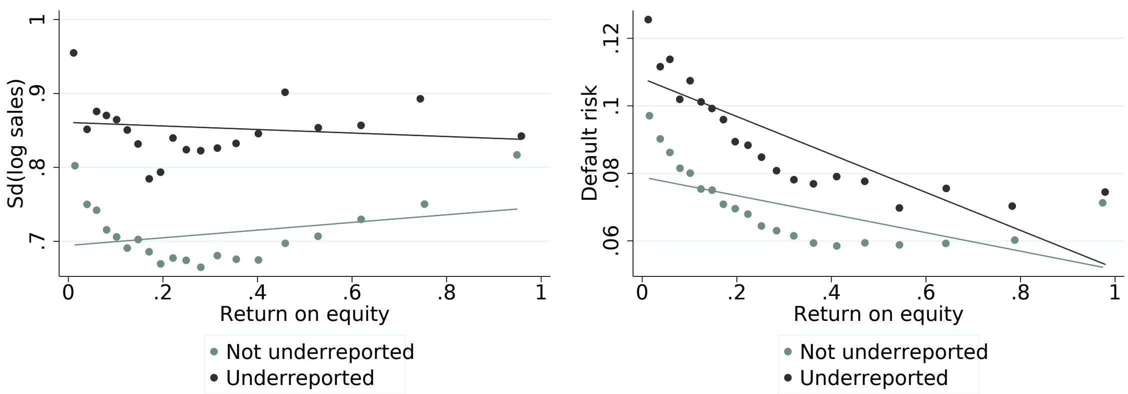
Figure 3: *Prevalence of Loss Underreporting*

Notes. Panel a shows the fraction of firm finance lending relationships that have a some overdue loans and the fraction of relationships that are subject to loss underreporting as measured by the our algorithm. Panel b shows the overdue balance scaled by total loan volume (RHS), and the amount of underreported losses scaled by total loan volume (LHS).

The predictions of the two sources of distorted lending incentives are borne out in the data. Analysis in Appendix B shows that, among firms with overdue loans, underreported firms have statistically significant lower collateral values, hold more assets and a higher share of social security and other debt obligations to the government, which take seniority over any bank debt in Portugal. This is in line with the prediction that delaying losses is most important in lending relationships that have large uncovered losses in the case of firm insolvency.

In line with the risk-shifting mechanism, we find that underreported firms display higher levels of risk for all levels of profitability relative to firms that have overdue loans but are not underreported. Panels a and b of Figure 4 plot firm-level risk measures (sales volatility and predicted default risk based on firm observables) against firm-level return on equity, residualized on year, industry, firm age, district and size.

We now address two potential shortcomings of using underreporting to identify distorted lending incentives. First, our measure of loss underreporting only applies to firms that



(a) *Volatility of Sales* **(b)** *Predicted Default Risk*

Figure 4: *Correlation of Underreporting with Firm-Level Risk*

Notes. The graphs show a residualized binned scatter plot of firm-level risk measures against the return on equity. The left panel uses the standard deviation of firm-level sales across 2005-2015. The right panel uses default risk based on the credit risk prediction model of Antunes *et al.* (2016) The sample only includes firms with overdue loans. We compare firms that are underreported to firms that are not underreported. The correlations are residualized on firm age, year, district, industry and firm size.

already have some overdue loans. However, in the time period we study a large number of firms fell behind on payments (see Appendix B), implying that we capture a large fraction of lending in the economy. Moreover, distorted lending incentives are most likely to arise for firms that are already close to financial distress and likely to have already defaulted on a loan payment.

Another potential challenge is that underreporting may be correlated with unobserved firm-quality differences and banks may exploit soft information to underreport firms where continued lending is positive net present value. This would imply that underreporting does not capture banks inefficiently lending to failing firms but banks efficiently lending to firms likely to recover. While our empirical specification, outlined in the next section, relies on comparing changes in credit to the same (underreported) firm, it is still helpful to address this point more generally.

First, underreported firms show signs of severe financial distress. These firms are highly levered, have little cash, and exhibit low profitability and sales growth. Based on these observables, underreported firms do not look like firms that are likely to recover soon. We provide additional evidence in the next section that these signs of financial distress do not appear to be driven by temporary negative shocks, at least in the period we study. We also show there is no evidence that underreported firms have significantly better fundamentals than their non-underreported peers (see Appendix B).

Second, we compare long-run outcomes for underreported and non-underreported firms. In Figure 5, we plot the path of exit, sales, return on assets and the fraction of loans overdue from the year in which the firm first has overdue loans. The variables are residualized on year$\times$industry and firm size fixed effects. Underreported firms perform worse over the long-run than non-underreported firms (which have overdue loans). While ex-post outcomes are not the same as banks' ex-ante expectations, it is unlikely that banks would consistently overpredict the long-run outcomes of firm that they choose to underreport.

1.3 The Cost of Undercapitalized Banks: A Natural Experiment

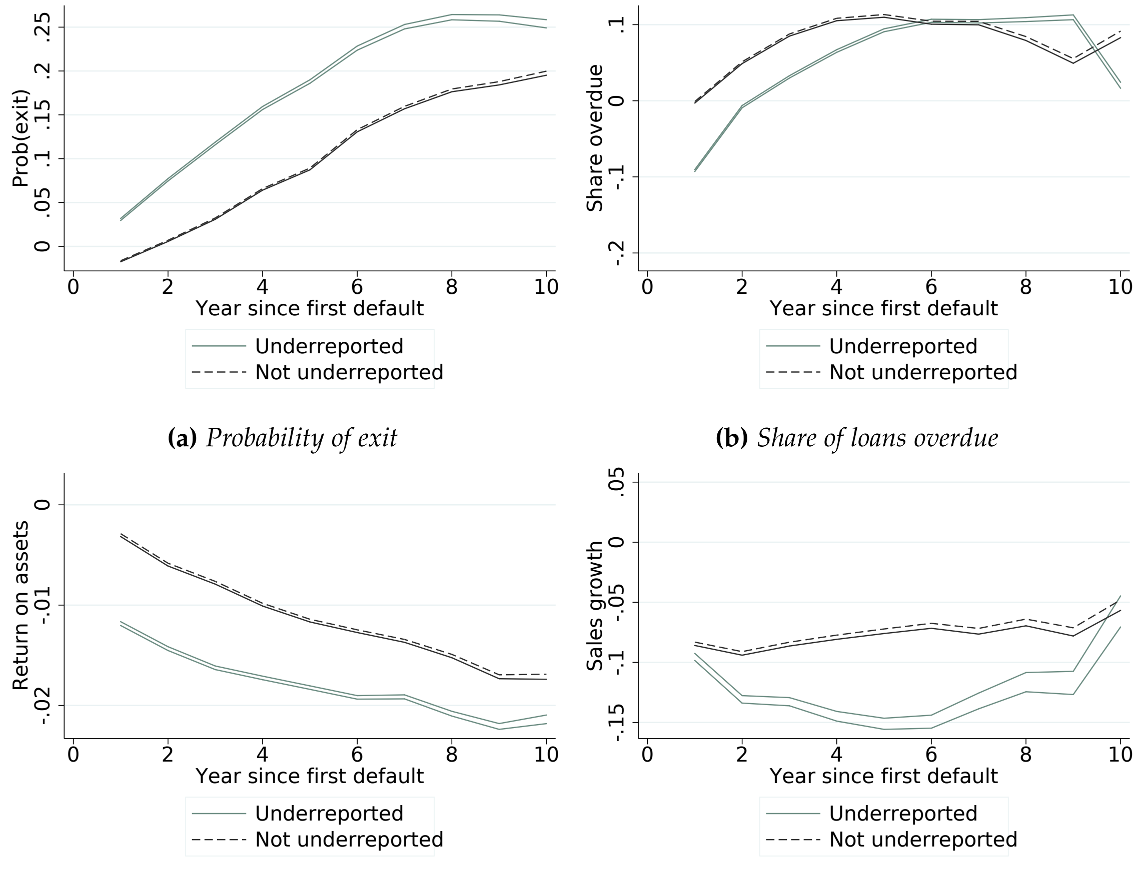
This section first describes the regulatory intervention by the European Banking Authority which we exploit for identification. We briefly describe our data and then present our main results.

1.3.1 The 2011 EBA Special Capital Enhancement Exercise

In October 2011, the European Banking Authority (EBA)[19] announced a Special Capital Enhancement Exercise to force banks with large, or overvalued, sovereign debt exposures to improve their capital ratios by June 2012. The EBA intervention applied to the largest banks in each country based on a cut-off determined by the EBA.[20] The EBA exercise, at least in its full scope, was plausibly unexpected given that banks had already undergone a round of EBA stress tests in June 2011. The Financial Times on October 11, 2011 reports that the EBA

[19] The EBA is an EU agency tasked with harmonizing banking supervision in the EU.

[20] Banks covered by the EBA exercise had to jointly hold at least 50% of the national banking sector as of the end of 2010 (EBA 2011).



(a) *Probability of exit* **(b)** *Share of loans overdue*

(c) *Return on assets* **(d)** *Sales growth*

Figure 5: *Long-Run Trends: Underreported vs Non-Underreported Firms*

Notes. The graphs show the average evolution of firm-level measures over time. We plot the 95 confidence intervals of the residualized mean for each group. The variables are residualized on year×industry fixed effects and firm size. The x-axis are years following the first time we observe an overdue loan in the data (for a given firm). The upwards trend in sales is likely due to a survivorship bias since firms that exit drop out of the sample.

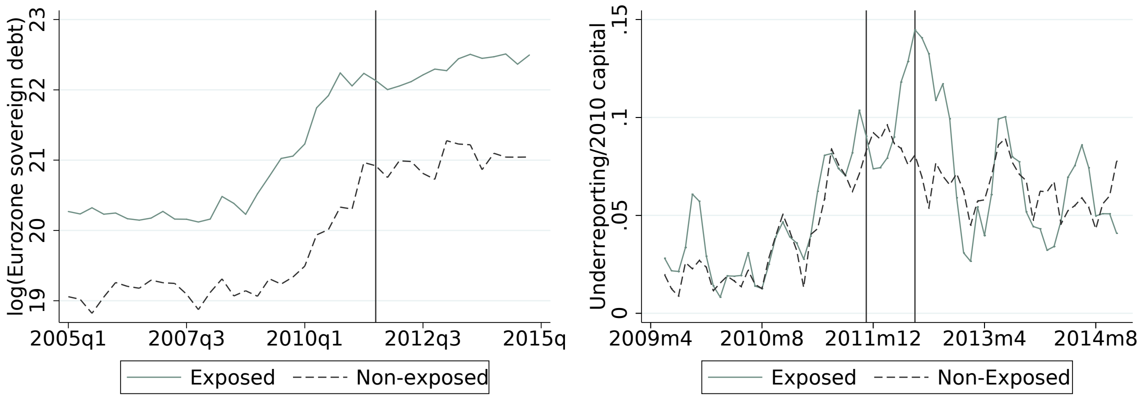
requirements were "well beyond the current expectations of banks and analysts".[21] The intervention led to a large capital shortfall for most eligible Portuguese banking groups since their Eurozone sovereign debt holdings were substantial and often valued above market prices in their balance sheets.[22]

[21]See Financial Times Article "Europe's banks face 9% capital rule" by Patrick Jenkins, Ralph Atkins, and Peter Spiegel. October 11 2011.

[22]In Portugal four banking groups (containing 7 banks) were subject to the Capital Exercise. Banks had to achieve a minimum Core Tier 1 ratio of 9% including an additional 'sovereign buffer', which reflected capital needs due to sovereign debt holdings.

We define a bank as exposed to the EBA intervention if it belongs to a banking group that was both subject to the intervention and had a large capital shortfall. We exploit variation in eligibility and variation in the EBA capital shortfall. The shortfall was driven by both quantity and valuation of banks' sovereign bond holdings. We use the variation in the shortfall to address the size imbalance that stems from the EBA targeting only the largest banks. Our control group hence consists of banks that were subject to the EBA intervention but had a below median sovereign debt holding in the group of large banks and hence a small capital shortfall under the EBA intervention. We also include in the control group any commercial bank operating in Portugal not subject to the EBA intervention. We exclude any bank whose foreign parent was subject to the EBA intervention in another European country. Using variation in pre-announcement sovereign debt holdings is valid as long these holdings are not correlated with other bank-level trends that affect credit allocation. Figure 6a shows that sovereign bond holdings followed parallel trends among the two groups prior to the EBA announcement providing evidence consistent with this assumption. Table 2 shows that both groups of banks are balanced on observables prior to the shock with some remaining size imbalance, which is to be expected given the selection criteria of the EBA intervention.

The EBA intervention temporarily heightened two sources of distorted incentives for exposed banks. First, exposed banks wanted to comply with the higher capital ratios but do so without raising costly new capital. Hence exposed banks had an incentive to boost reported capital by increasing the intensity of their loan loss underreporting and simultaneously rolling over loans to underreported firms. Figure 6b shows that underreporting at exposed and non-exposed banks follows the same increasing trend with the onset of the crisis but shoots up for exposed banks with the announcement of the EBA intervention. This increase lasts until the EBA deadline, at which point exposed banks roll back the additional underreporting. In addition, banks also had an incentive to continue lending to firms with underreported losses in order to avoid realizing a large loss in case of firm insolvency.



(a) *Evolution of Sovereign Debt Holdings*

(b) *Evolution of Underreported Losses*

Figure 6: *Comparing Behavior of Exposed and Non-Exposed Banks*

Notes. Panel a shows the aggregate log Eurozone sovereign debt holdings of banks exposed and not exposed to the EBA Special Capital Enhancement exercise. The vertical line denotes the announcement of the EBA exercise. Panel b shows the evolution of aggregate underreported losses for the two groups of banks. Underreported losses are scaled by 2010 bank capital. The first vertical line denotes the announcement of the EBA intervention. The second vertical line denotes the EBA compliance deadline.

The second source of distorted incentives arose due to the prospect of a government bailout. Affected banks anticipated that as long as they made a credible attempt to comply with the EBA requirements, the Portuguese government would step in to make up any remaining capital shortfall at the compliance deadline.[23] These expectations were validated when in June 2012, at the EBA compliance deadline, the Portuguese government provided EUR 6 bn of capital in the form of convertible contingent bonds to all exposed banks. The anticipated bailout gave bank shareholders the incentive to gamble for the resurrection of distressed borrowers. The bailout was effectively a government guarantee to cover any loss in June 2012. From the shareholders perspective, distressed firms would either recover allowing them to satisfy the constraint without the government's help, or they would fail but the resulting losses would be borne by the government.

[23]In May 2011, the Portuguese government had received a financial assistance package from the IMF and European Financial Stability Facility, which explicitly earmarked EUR 12 bn to recapitalize Portuguese banks. A press release by the Portuguese central bank in 2011 reads: "This means that there is sufficient public provision of equity available to recapitalise banks in the event that marked-based solutions do not materialise as would be desirable." www.bportugal.pt/sites/default/files/anexos/documentos-relacionados/combp20111208_0.pdf

1.3.2 Data

We use proprietary administrative data from the Portuguese central bank. We combine quarterly bank balance sheet data with information from the EBA website to determine which banks were eligible for the exercise either directly, or through a foreign parent, and to obtain the capital shortfall due to the EBA intervention. We merge the bank information with the credit register data (*Central de Responsabilidades de Credito*), a loan level database, which covers the universe of lending relationships that exceed EUR 50. We collapse the loan data to the quarterly firm-bank level. We then merge this information with balance sheet and other financial variables for non-financial firms. The data comes from the Simplified Corporate Information (*Informacao Empresarial Simplificada*), an annual, mandatory firm census.

We work with three final datasets. First, a quarterly dataset of loan balances at the firm-bank level from 2009-2015 spanning 45 banks, 144,050 non-financial firms, and 380,286 lending relationships. The dataset covers over 90% of loans made in Portugal. Second, we collapse the firm-bank data to a quarterly firm-level dataset covering the same time period and number of firms. Third, we use the annual firm-level information from 2009-2015. We drop firms with fewer than 2 employees or missing information (or negative values) on assets or employees in 2008-2011. The firms in our resulting sample cover 81% of sales and 73% of assets in Portugal. We winsorize all outcome variables at the 1% level separately for each 2-digit industry.

1.3.3 Results

Banks subject to the EBA intervention cut credit for all but the subset of financially distressed firms whose loan losses they had been underreporting prior to the EBA intervention. This credit reallocation is present both at the firm-bank level, controlling for the total change in firm-level credit, and at the firm-level. We show that there is a substantial pass-through of the credit shock into employment and investment spending.

Table 2: *Descriptive Statistics: Firms and Banks*

Firms		Banks			
	Baseline		Not exposed	Exposed	Dif
Assets (m)	1.62	Assets (100 bn)	0.42	0.98	0.56***
	(6.05)		(0.32)	(0.34)	(0.21)
Employees	13.46	Sovereign bonds	0.04	0.06	0.02
	(114.14)		(0.04)	(0.02)	(0.01)
Total credit (m)	0.52	Loans	0.46	0.49	0.03
	(4.86)		(0.14)	(0.11)	(0.06)
Share NPLs	0.07	NPLs	0.02	0.02	0.00
	(0.22)		(0.02)	(0.01)	(0.00)
Return on assets	0.03	Return on assets	0.00	0.00	0.00
	(0.07)		(0.02)	(0.00)	(0.00)
Sales growth	0.13	Deposits	0.33	0.40	0.07
	(0.48)		(0.17)	(0.13)	(0.07)
Leverage	0.28	Capital ratio	0.10	0.14	0.04
	(0.73)		(0.14)	(0.02)	(0.03)
Current ratio	2.43	Liquid assets	0.01	0.01	0.00
	(4.29)		(0.01)	(0.00)	(0.00)
Cash/assets	0.13	Central bank funding	0.12	0.09	-0.03
	(0.17)		(0.11)	(0.06)	(0.04)
Fixed assets/assets	0.47	Interbank market	0.22	0.13	-0.09
	(0.29)		(0.20)	(0.11)	(0.06)
N	144,050		38	7	

Notes. The table shows descriptive statistics for firms and banks in our sample. All variables are measured at the end of 2010. We only include firms in our sample (firms that report consistently to the annual firm census in our sample period in 2008-2011). All bank variables with exception of assets are scaled by total assets. Exposed refers to banks that are exposed to the EBA intervention. Dif refers to the difference in means for exposed and non-exposed banks. ** indicate significance at the 0.05 level.

Credit Effects at the Firm-Bank Level

We run the following difference-in-differences specification at the firm-bank level

$$
\begin{aligned}
g_{ibt}^{\text{credit}} = & \sum_{\tau=-2}^{5} \beta_{\tau}^{treat} \left(\text{period}_{\tau} \times \text{exposed}_{b}\right) + \sum_{\tau=-2}^{5} \beta_{\tau}^{period} \tau \left(\text{period} \times \text{underreported}_{ib}\right) \\
& + \sum_{\tau=-2}^{5} \beta_{\tau}^{treatgroup} \left(\text{period}_{\tau} \times \text{underreported}_{ib} \times \text{exposed}_{b}\right) + \theta_{it} + \varphi_{b} \qquad (1.2) \\
& + \beta_{1}^{base} \left(\text{underreported}_{ib} \times \text{exposed}_{b}\right) + \beta_{2}^{base} \text{underreported}_{ib} + \alpha_{2} X_{ibt} + \epsilon_{ibt}
\end{aligned}
$$

where i, b and t index firms, banks and quarters respectively.[24] The main explanatory variables are exposed_b, a dummy variable that is 1 for banks exposed to the EBA intervention and underreport_{ib}, a dummy that is 1 if the lending relationship has underreported loan losses in the four quarters prior to the announcement of the shock. This dummy is based on our measure of underreporting.

period_τ is a dummy that indexes periods of three quarters. The periods of interest are the EBA shock (2011Q4-2012Q2) and the period following the EBA deadline (and bank bailout) (2012Q3-2013Q1). We also include two pre-period dummies and one post-bailout period dummy, all of which are of equal length.[25]

φ_b is a bank fixed effect and X_{ibt} are relationship level controls.[26] Standard errors are two-way clustered at the firm and bank level.[27] We follow the literature and estimate the effect on changes rather than (log) levels. The growth rate of credit is our dependent variable:

---

[24] We condition on relationships that are present throughout the entire period of interest. In a separate specification, we investigate the effect on the probability that a lending relationship is cut.

[25] The two pre-periods allow us to test for pre-trends in credit allocation, while the inclusion of the post-bailout period allows us to study the evolution of credit following the EBA deadline. The sample period includes 2009Q1-2014Q4 which allows us to estimate each β_τ. This implies that the quarters not contained in any of the period dummies are the omitted base group. A standard difference-in-differences would omit the t-2 and t-1 terms and include only a single post coefficient which would summarize the average treatment effect in the post period.

[26] The relationship controls are the lending share of the bank, the length of the relationship, a dummy if the bank is the main lender, the share of the firm in the bank's loan portfolio

[27] We also run a version with standard errors only clustered at the bank-level.

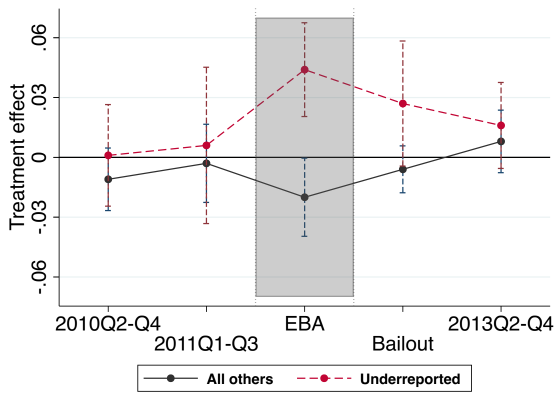
$y_{ibt} = \text{credit}_t/\text{credit}_{t-1} - 1$. The growth rate allows us to decompose the total change in credit into the portion coming from overdue credit and the portion coming from performing credit (credit that is not overdue).

The firm×quarter fixed effects, θ_{it}, control for the firm-level changes in credit growth. This implies that we compare changes in the share of credit coming from exposed and non-exposed bank to the same firm (Khwaja and Mian (2008)). This estimator requires firms to have multiple lending relationships, which is true for 56% of firms in our sample. We also run a model with separate firm and quarter fixed effects which then also includes firms that only have a single lending relationship.

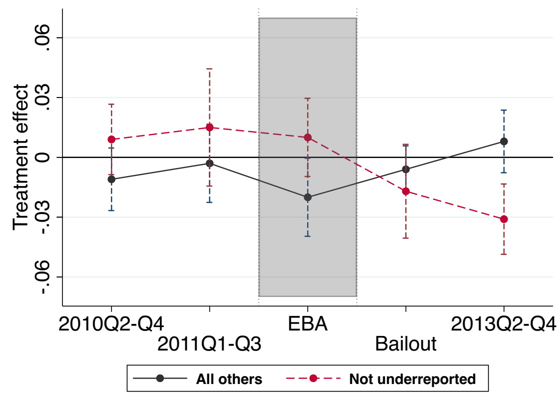
The coefficients of interest are $\beta_{\tau}^{treatgroup}$ on the triple interaction, which estimate the treatment effects for the subset of underreported firms. Our hypothesis is that the EBA intervention increased distorted lending incentives for exposed banks and we expect this coefficient to be positive during the EBA intervention. Given that the differential incentives disappear with the government bailout, we expect $\beta_{\tau}^{treatgroup}$ to either turn to zero (or negative) following the EBA deadline.

We also estimate the baseline treatment effects for all other firms, β_{τ}^{treat} for two reasons. First, the existing literature suggests that a tightening of capital requirements can lead banks to shed assets and decrease credit supply (Admati *et al.* (2017), Gropp *et al.* (2017)). We want to test whether the effect is present in this setting. Second, the total treatment effect for the subset of interest, firms with underreported losses, is $\beta_{\tau}^{treat} + \beta_{\tau}^{treatgroup}$. We need to estimate the baseline treatment effect in order to calculate the full treatment effect on the subset of underreported firms.

Results Figure 7a shows our main credit results (see also Appendix B for corresponding point estimates). Following the announcement of the EBA intervention, exposed banks increase credit supply to firms in financial distress that are subject to prior loss underreporting. The coefficient on the triple interaction of $\text{period}_{\tau} \times \text{underreported}_{ib} \times \text{exposed}_{b}$ in equation 1.2 is positive and strongly significant during the EBA intervention. This positive treatment effect for underreported distressed firms contrasts with the credit crunch for all other lending relationships at exposed banks. The coefficient on $\text{EBA}_t \times \text{exposed}_t$ in equation 1.2 is negative and statistically significant (Figure 7a and columns 2 and 3 of Table 3). The magnitude of the shock is large. The baseline treatment effect of borrowing from exposed banks is a 2 percentage point (p.p.) drop in quarterly credit growth between the announcement and deadline of the EBA intervention. In contrast, the treatment effect for underreported, distressed firms is an increase in credit growth at exposed banks of just over 2 p.p.[28] These changes are equivalent to 4% of a standard deviation of credit growth.



(a) *Underreported firms*



(b) *Non-underreported firms*

Figure 7: *Firm-Bank Credit Results*

Notes. The graphs shows results of the firm-bank level credit regression in specification 1.2, which includes firm×time and bank fixed effects as well as firm-bank-level controls. The dependent variable is the quarterly credit growth.. We plot the coefficients on the two interactions $\text{period}_\tau \times \text{exposed}_b$ and $\text{period}_\tau \times \text{underreported}_{ib} \times \text{exposed}_b$, which are the respective treatment effects for the baseline group of firms and the group of firms subject to loss underreporting. In panel b, we plot the triple interaction $\text{period}_\tau \times \text{not underreported}_{ib} \times \text{exposed}_b$, which are relationships with loan losses but which are not underreported. Standard errors are clustered at the firm and bank level. The shaded area marks the period of the EBA intervention. See Appendix B for point estimates. N = 1,981,219.

If loss underreporting correctly identifies firms for which distorted lending incentives drive additional lending, we should find that exposed banks do not increase credit supply to firms that are distressed but are not subject to underreporting. In Figure 7b, we show results from running specification 1.2 but replacing the triple interaction with the subgroup of firms that have overdue loans but are not subject to underreporting prior to the shock.

[28]The total treatment effect adds the baseline treatment effect and the treatment effect for the subgroup of underreported firms.

We find no evidence of differential treatment effects for these relationships at the intensive margin and a small positive treatment effect at the extensive margin.[29]

The results suggest that effects are driven by changes in bank credit supply in response to the EBA intervention. There is no evidence of differential credit allocation at exposed banks in the two periods prior to the shock, lending credibility to our parallel trends assumption. The lack of pre-trends applies to both the baseline group of firms and to the subgroup of underreported, distressed firms. Second, the preferential credit treatment for underreported, distressed firms only occurs during the period of the EBA shock when exposed banks face heightened distorted lending incentives. Similarly, the credit crunch only occurs in the period of the EBA shock when banks attempt to comply with tighter capital requirements. We provide a series of further robustness checks in Appendix B.[30]

While the differential treatment effect in growth rates disappears with the EBA deadline, the effect is persistent in levels. That is, we do not find evidence of *negative* treatment effects for underreported, distressed firms in the periods after the EBA shock. This suggests that banks do not withdraw the additional credit granted during the EBA shock following the EBA deadline.

The effect on changes in total credit is almost entirely driven by performing credit (column 4 of Table 3). If underreported, distressed firms were simply converting more of their performing loan balances into overdue loans, we would expect no change in total credit, a reduction in performing credit, and an increase in overdue credit. Instead, we find an increase in total credit, an increase in performing credit, and a (statistically insignificant) reduction in overdue credit.

There are similar patterns when looking at the probability that a bank grants a new loan. We construct a dummy that is one if there is a new loan in a firm-bank relationship.[31]

[29]See also Appendix B

[30]We show that the estimated treatment effects are robust to the inclusion of firm-level controls averaged over the pre-period and interacted with period dummies. We also show that the estimated treatment effects are robust to differential clustering of standard errors, exclusion of relationship controls, and a weighted least squares specification.

[31]Our definition of a new loan requires that the total number of loans in a firm-bank relationships increases

Table 3: *Regression Results Firm-Bank Level: Intensive Margin*

Growth rate of credit	(1)	(2)	(3)	(4)	(5)	(6)
		Total credit		Performing	Non-perf	New loan
$\text{Pre1}_t \times \text{exposed}_b$		-0.011	-0.010	-0.011	-0.000	-0.024
		[0.008]	[0.010]	[0.008]	[0.002]	[0.015]
$\text{Pre2}_t \times \text{exposed}_b$		-0.003	-0.005	-0.003	-0.000	-0.022
		[0.010]	[0.010]	[0.010]	[0.002]	[0.014]
$\text{EBA}_t \times \text{exposed}_b$		-0.020**	-0.022*	-0.022**	0.001	-0.039***
		[0.010]	[0.013]	[0.009]	[0.002]	[0.014]
$\text{Bailout}_t \times \text{exposed}_b$		-0.006	-0.008	-0.004	-0.002	-0.013
		[0.006]	[0.008]	[0.007]	[0.003]	[0.011]
$\text{Post bailout}_t \times \text{exposed}_b$		0.008	0.006	0.009	-0.002	0.004
		[0.008]	[0.012]	[0.009]	[0.002]	[0.011]
$\text{Pre1}_t \times \text{exposed}_b \times \text{underreported}_{ib}$	0.008	0.001	0.018	0.007	-0.006	0.009
	[0.013]	[0.013]	[0.012]	[0.013]	[0.007]	[0.008]
$\text{Pre2}_t \times \text{exposed}_b \times \text{underreported}_{ib}$	0.008	0.006	0.021	0.010	-0.004	0.016
	[0.023]	[0.023]	[0.025]	[0.020]	[0.007]	[0.023]
$\text{EBA}_t \times \text{exposed}_b \times \text{underreported}_{ib}$	0.041***	0.044***	0.050***	0.038**	0.005	0.069***
	[0.013]	[0.012]	[0.019]	[0.015]	[0.009]	[0.022]
$\text{Bailout}_t \times \text{exposed}_b \times \text{underreported}_{ib}$	0.019	0.027*	0.027	0.035**	-0.008	0.042**
	[0.019]	[0.016]	[0.019]	[0.015]	[0.011]	[0.017]
$\text{Post bailout}_t \times \text{exposed}_b \times \text{underreported}_{ib}$	0.005	0.016	0.023*	0.022	-0.007	0.030**
	[0.014]	[0.011]	[0.013]	[0.014]	[0.010]	[0.012]
Bank*quarter FE	Y	N	N	N	N	N
Firm*quarter FE	Y	Y	N	Y	Y	Y
Firm, quarter FE	N	N	Y	N	N	N
N	1,981,219	1,981,219	1,981,219	1,981,219	1,981,219	1,981,219
R2	0.381	0.379	0.057	0.383	0.405	0.413
Banks	45	45	45	45	45	45

Notes. The table shows credit regressions results at the firm-bank level. The dependent variable is the quarterly growth rate in total credit for a given firm-bank pair in columns (1)-(5). Columns (4) and (5) decompose total credit growth into performing and non-performing credit. These growth rates are defined as the quarterly change are scaled by lagged total credit. For example, the growth rate in performing credit is defined as $\Delta c_{it}^{perf}/c_{i,t-1}^{all}$. Column 6 presents results from a linear probability model where the dependent variable is a dummy that is 1 if the number of loans in a firm-bank pair increases (conditional on an increase in loan volume). The explanatory variable exposed is a dummy that is 1 for banks exposed to the EBA shock. The sample period is 2009q1-2014q4. Pre 1 and 2, EBA, bailout and post-bailout are dummies that identify the following time periods: The EBA shock (2011q4-2012q2), the bailout period (2012q-2012q4), two pre-periods and one post-bailout period all of equal length. Underreported is a firm-bank dummy that identifies relationships subject to loss underreporting in the four quarters prior to the EBA shock. All regressions include bank fixed effects and firm-bank controls (see text for details). Standard errors in parentheses and are two-way clustered by bank and firm. Additional interaction effects are omitted. See equations in section 3 for details on full set of interaction effects included. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Column 6 of Table 3 shows that we find a large significant increase in the probability that a new loan is granted to a underreported, distressed firms at exposed banks in the period of the EBA shock. In contrast, the probability declines for all other firms at exposed banks.

The differential credit behavior is also visible at the extensive margin. The probability that an exposed bank cuts a relationship increases by almost 6 percentage points during the EBA shock (Table 4).[32] In contrast, the probability falls for underreported, distressed firms.[33]

Credit Effects at the Firm-Level

To detect whether firms undo effects at the firm-bank level by adjusting their credit coming from non-exposed banks, we analyze changes in credit allocation at the firm-level. We run the following fully dynamic differences-in-differences specification[34]

$$\begin{aligned}\Delta \log \text{credit}_{it} = &\sum_{t=-5}^{10} \delta_t^{treatgroup} \left(\text{quarter}_t \times \text{treatment}_i \times \text{underreported}_i\right) \\ &+ \sum_{t=-5}^{10} \delta_t^{treatment} \left(\text{quarter}_t \times \text{treatment}_i\right) + \text{controls} + \alpha_1 X_{it} + \theta_i + \varepsilon_{it}\end{aligned} \tag{1.3}$$

and that the total loan balance in the firm-bank relationships increases. While the credit register data does not allow us to track individual loans, banks report each individual lending operation to a given firm allowing us to count the number of loans each period. Since existing loans can be split into several loans due to, for example, a restructuring operation we also impose the second condition on the total loan balance.

[32]Our indicator is a dummy that turns one in the month the performing credit balance drops to zero. We focus on the performing credit stock since banks often report relationships that only have non-performing credit to the credit register for a very long time even when the credit is fully written off. The reason is that banks wait for the conclusion of the official insolvency process to stop reporting the debt to the credit register. Given very lengthy bankruptcy procedures in Portugal, this implies that non-performing loan stocks can be reported in the credit register for years even though there no longer exists a meaningful credit relationship.

[33]We cannot estimate pre-trends in this specification since we condition on the sample of relationships with positive loan balances in the pre-period. Since we estimate the cumulative effect of existing a lending relationship, the dummy for exit remains 1 following the quarter of exit and contributes to the estimated probability in all subsequent quarter, the changes in the coefficients are informative about the additional exit. This implies that as in intensive margin, the effects predominantly take place during the EBA shock.

[34]See for example Jäger (2016) and Jaravel *et al.* (2015)).

Table 4: *Regression Results Firm-bank Level: Extensive Margin*

Pr(relationship cut)	(1)	(2)	(3)
$EBA_t \times exposed_b$	0.057***	0.056***	0.058***
	[0.011]	[0.011]	[0.012]
$Bailout_t \times exposed_b$	0.041***	0.042***	0.043***
	[0.009]	[0.008]	[0.008]
Post $bailout_t \times exposed_b$	0.029***	0.030***	0.029***
	[0.010]	[0.009]	[0.009]
$EBA_t \times exposed_b \times underreported_{ib}$	-0.217***	-0.202***	-0.219***
	[0.034]	[0.027]	[0.057]
$Bailout_t \times exposed_b \times underreported_{ib}$	-0.106***	-0.090***	-0.105**
	[0.033]	[0.030]	[0.047]
Post $bailout_t \times exposed_b \times underreported_{ib}$	-0.053***	-0.041***	-0.050**
	[0.018]	[0.015]	[0.024]
Firm FE	Y	N	Y
Firm controls	N	Y	N
N	2,973,566	2,538,082	2,973,566
R2	0.706	0.137	0.706
Banks	46	45	46

Notes. The table shows credit regressions results at the firm-bank level for the extensive margin (linear probability model). The dependent variable is a dummy that turns one when the relationship is cut, defined by the performing loan balance dropping to zero. The explanatory variable exposed is a dummy that is 1 for banks exposed to the EBA shock. Pre period 1 and 2, EBA, bailout and post-bailout are dummies that identify the following time periods: The EBA shock (2011q4-2012q2), the bailout period (2012q-2012q4), and one post-bailout period all of equal length. We cannot estimate pre-trends in this regression since we condition on a sample of relationships that have positive loan balances in the pre-periods. underreported is a dummy that identifies relationships subject to underreported losses in the four quarters prior to the EBA shock. All regressions include bank and quarter fixed effects. Column 1 and 3 contain firm fixed effects. Column 2 includes industry×quarter fixed effects and firm-level sales growth and leverage interacted with the time period to allow for flexible time trends. Standard errors in parentheses and are two-way clustered by bank and firm. Additional interaction effects are omitted. See equation 1.2 in section 3 for details on full set of interaction effects included. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

where treatment_i is the firm-level borrowing share from exposed banks prior to the shock.[35] We standardize this variable to be able to interpret coefficients as the percentage change in credit in response to a standard deviation increase in the borrowing share from exposed banks.[36] underreported_i is a dummy that captures firms with underreported losses prior to the announcement of the shock. Standard errors are clustered at the firm-level.

In contrast to the firm-bank level specification, we can no longer control for the firm-level change in total credit, which captures changes in credit demand. We therefore include a range of firm-level controls interacted with quarter dummies to allow for flexible differences in time trends across firms. These controls include 2-digit industry and several firm characteristics averaged over 2008-2010 (sales growth, capital/assets, interest paid/ebitda and the current ratio). The inclusion of controls accounts for potential long-term trends at the firm-level that could affect credit demand.

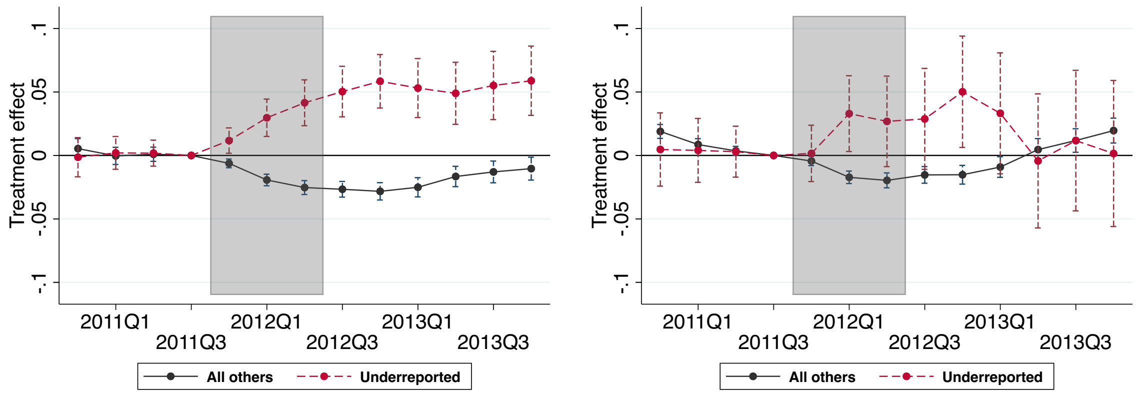
Results Figure 8a shows our main credit results at the firm-level. Following the announcement of the EBA intervention, underreported firms with a larger borrowing share from exposed banks experience a faster growth in credit than underreported firms who are less reliant on exposed banks. A the same time, credit declines for all other firms with a larger borrowing share from exposed banks. Both effects shift back to zero following the bank bailout at the EBA deadline. We hence confirm that the credit reallocation at the firm-bank level is also present at the firm-level, suggesting that firms cannot undo the effects at the firm-bank level.

Unlike in the firm-bank results, the positive treatment effect for underreported firms does not immediately revert after the bank bailout at the EBA deadline. This persistent effect on total credit is driven by an increase in overdue credit which begins after the EBA deadline (see additional results in Appendix B). This result suggests that banks can stave off

[35] Following (Chodorow-Reich (2014)) this is defined as $\text{treat}_i = \frac{\sum_{b=1}^{B^{exp}} L_{ib,pre}}{\sum_{b=1}^{B^{all}} L_{ib,pre}}$ where $L_{ib,pre}$ denotes the stock of total credit of firm i at bank b in 2010. B^{exp} is the set of exposed banks, while B^{all} is the set of exposed and non-exposed banks.

[36] Figure 28 in Appendix B shows that we have variation in treatment intensity.

additional default for underreported firms in the short-run but eventually their default rates catch up. This result, together with the absence of pre-trends at the firm-level, provides further support for the argument that the credit reallocation is not driven by underlying differences in firm-level quality or liquidity trends. The increase in credit during the EBA intervention is again driven by performing credit as shown in Figure 8b.



(a) *Total Credit* **(b)** *Performing Credit*

Figure 8: *Firm-Level Credit Treatment Effects*

Notes. The graphs show results of the firm-level credit regression in specification 1.3. The dependent variable is the quarterly log of total credit for a given firm in panel a and the quarterly log of performing credit in panel b. We plot the coefficients on the two interactions quarter$_t$ × treatment$_i$ and quarter$_t$ × treatment$_i$ × underreported$_i$, which are the treatment effects for the baseline group of firms, and the group of firms subject to loss underreporting. The shaded area marks the period of the EBA intervention. The specification includes the full set of interactions, industry×quarter and firm fixed effects, as well as firm-level controls interacted with quarter. All coefficients should be interpreted as changes in the dependent variable relative to the (normalized) base quarter 2011Q3. Standard errors are clustered at the firm-level. N= 1,346,771.

The economic significance of the credit reallocation is large. For underreported, distressed firms, the total treatment effect of borrowing exclusively from exposed banks versus borrowing exclusively from non-exposed banks is equal to a 16% increase in total credit relative to the base quarter (2011Q3).[37] For all other firms, the total treatment effect is a decline in credit of 14% relative to the base quarter.

[37] This is the cumulative effect over the combined EBA and bailout period, which runs from 2011Q3 to 2013Q1. A standard deviation in the borrowing share in our sample is the equivalent of borrowing entirely from exposed and borrowing entirely from non-exposed. For underreported firms, this is the total treatment effect $\beta_\tau^{treat} + \beta_\tau^{treatgroup}$ in equation 1.3, or in other words, we add the two coefficients displayed in Figure 8a.

Effects into Employment and Investment

We use an instrumental variable design to estimate the pass-through of the credit shocks into employment and investment at the annual firm-level.

$$y_{is} = \gamma \Delta \log \text{credit}_{is} + \text{controls} + u_{is} \tag{1.4}$$

where i and s index firms and industries, respectively.

We instrument for $\Delta \log \text{credit}_{is}$ with the firm-level borrowing share from banks exposed to the EBA shock. We include the same controls as in the firm-level credit specification, equation 1.3. To address concerns that treated firms may have been on different long-term trends, we include a lag of the dependent variable.

The dependent variable is either the symmetric growth rate of employees, wages and fixed assets, or investment spending scaled by lagged fixed assets. The symmetric growth rate is a second-order approximation of the log difference growth rate around zero (Davis *et al.* (1996), Chodorow-Reich (2014)). This growth rate is attractive since it takes into account observations that turn to zero and is bounded between -2 and 2.[38] Because this employment effect combines extensive and intensive margin changes, we run a separate specification isolating the intensive margin effects. Growth rates are calculated between 2011 and 2012 since we expect real outcomes to be affected in 2012 as this is when most of the EBA intervention occurs.

Results We estimate that the credit shock has a 40% pass-through into investment[39] and a 11% pass-through into employment (see Table 5). If we allow for the effect of exit, the pass-through into employment jumps to 60%. The first-stage F-statistics are close to 200,

[38]The formula is

$$g^{y}_{i,s} = \frac{y_t - y_{t-1}}{0.5(y_t + y_{t-1})}$$

[39]While the firm census asks for CAPEX, in reality only large firms provide CAPEX numbers. As a result our instrument loses power because we have a much smaller sample and credit shocks tend to be less important for the largest firms. We instead resort to the growth rate in fixed assets to measure investment. Table 5 reports results for using CAPEX scaled by lagged fixed assets and shows that we obtain similar results despite a weak instrument problem (F-statistic of 3).

comfortably above the Stock and Yogo (2005) criterion for 5% maximal bias.

Table 5: *Pass-Through Into Employment and Investment*

Growth rate	(1)	(2)	(3)	(4)	(5)	(6)
		Employees		Wages	CAPEX	Fixed assets
	OLS	Ext + int	Intensive			
Δ log credit$_f$	0.082***	0.596***	0.109***	0.160***	0.391**	0.353***
	[0.004]	[0.084]	[0.025]	[0.033]	[0.138]	[0.109]
Lag		-0.041	-0.011	0.152***		0.129***
		[0.035]	[0.010]	[0.018]		[0.034]
Controls	N	Y	Y	Y	N	Y
Industry, size FE	Y	Y	Y	Y	N	Y
N	156,784	156,784	119,563	119,563	13,431	119,563
First-stage F statistic		200	176	176	3	176

Notes. The table shows IV regression results at the annual firm-level for 2012. The dependent variable in columns 1-2 is the symmetric growth rate of employment, which is a second order approximation to the log difference growth rate and incorporates observations than turn to 0 (firm exit). In the remaining columns, we condition on the sample of firms that do not exit (intensive margin) and use the log difference growth rate. Column 5 estimates the effect on CAPEX scaled by lagged fixed assets. Given that only larger firms report CAPEX, this result should be treated with caution (weak instrument). With the exception of column 1, we instrument for the log change in credit using the (normalized) firm-level borrowing share from banks exposed to the EBA intervention. Controls consist of firm-size and 2-digit industry FE, as well as firm-level log total assets, interest/ebitda, capital/assets, currrent ratio, cash/assets and sales growth all averaged over 2008-2010. Lag refers to the lag of the dependent variable. Standard errors are clustered by industry. Standard errors in parentheses. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

The real effects of the EBA intervention persist into 2013 but dissipate in 2014. However, it is difficult to precisely estimate the long-run pass-through since the credit shock is short-lived and hence the instrument loses power after 2012. For additional robustness, we show that results are similar when dropping firm-level controls and the lagged dependent variable (see Appendix B). We also conduct placebo exercises running the same specification in the years prior to the shock and find no significant effects (see Table 25 in Appendix B).

A partial equilibrium back-of-the-envelope calculation that combines the firm-level credit estimates with the pass-through coefficients is suggestive of the magnitude of the real effects. In 2012, underreported firms borrowing entirely from exposed banks increased employment and investment by 8% and 6% ,respectively, relative to underreported firms borrowing entirely for non-exposed banks.[40] For all other firms, the equivalent calculation implies a

[40] A standard deviation in the borrowing share in our sample is the equivalent of borrowing entirely from

decline in employment and investment of 9% and 6%, respectively.

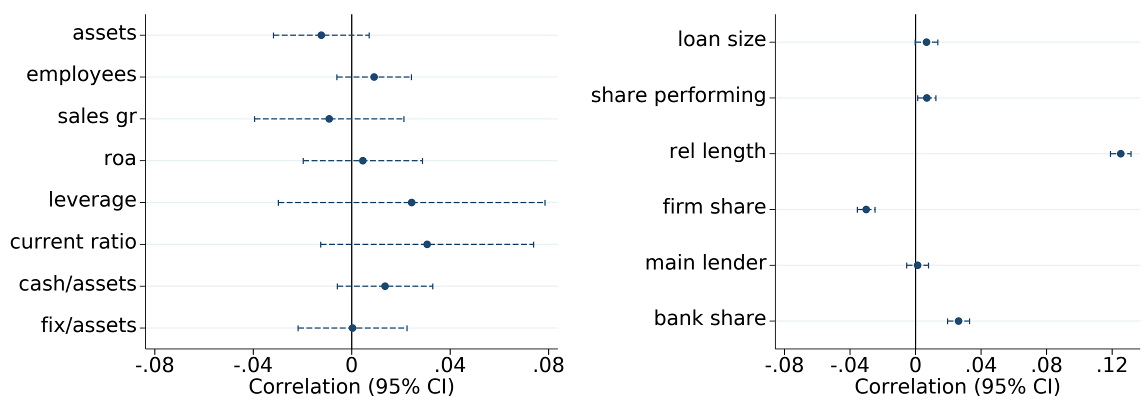
Potential Threats to Identification

The validity of our results rests on the assumption that the credit reallocation to underreported firms by exposed banks is not driven by credit demand. For this assumption to be violated in the context of our triple-difference design, banks have to underreport firms with better long-run fundamentals, those firms have to experience temporary financial distress driving up their credit needs coinciding exactly with the duration of the EBA intervention, and the nature of lending relationships has to be such that only exposed banks are in a position to respond to these additional credit needs. To address this possibility, we first provide evidence that observable characteristics of underreported firms are not systematically correlated with how much they borrow from exposed banks prior to the EBA intervention (see Figure 9a). Turning to the firm-bank level, Figure 9b shows that EBA banks are no more likely to be the main lender, to grant a different level of credit, or to have a different share of performing credit. EBA banks seem to have slightly longer lending relationships and firms on average account for a larger share in the EBA banks' loan portfolio. These differences are likely to reflect that exposed banks on average are larger and have been present in Portugal for longer. To account for these differences, we control for relationship characteristics in all firm-bank level specifications.

Second, we investigate the potential presence of differential financial shocks driving outcomes. Given that we absorb any firm-level changes by firm$\times$time fixed effects in our main specification, differential shocks to credit demand provide a potential challenge only for our firm-level regressions. At the firm-level, the main difficulty for confounding firm-level financial shocks to explain the results stems from the fact that the EBA intervention is temporary. For concurrent liquidity shocks to explain the results, we would need that firms borrowing from exposed banks experience a negative liquidity shock, leading to

exposed and borrowing entirely from non-exposed. We can multiply the firm-level coefficient from the first-stage credit regression with the pass-through coefficient (0.14*0.353 for investment and 0.14*0.596 for employment.

a positive credit demand shock, at the time of the EBA intervention and that this shock dissipates with the onset of the EBA deadline. Nonetheless, we provide evidence against different liquidity trends prior to the shock by estimating a dynamic firm-level difference-in-differences regression at the annual level with liquidity ratios as the dependent variable. Figures 30a - 30b in Appendix B show that there are no pre-trends in either the current ratio or the cash/assets ratio for these firms. Figure 30c in Appendix B plots firm×quarter fixed effects from a regression that decomposes credit change of firms with multiple lending relationships into a bank and a firm-time component. The firm×quarter fixed effects can be interpreted as a measure of firm-level credit demand. There is no evidence of differential trends in credit demand prior to the EBA intervention.



(a) *Firm-level* **(b)** *Relationship-level*

Figure 9: *Correlations with Borrowing Share from Exposed Banks*

Notes. Panel a shows the correlation of normalized firm-level observables with the (normalized) firm-level treatment variable for the subset of firms subject to loss underreporting. Treatment is the borrowing share from banks exposed to the EBA intervention. The correlations are conditional on 2-digit industry fixed effects and firm size buckets. All variables are averaged over 2008-2010. The right panel shows the correlation of normalized relationship-level variables with a bank exposure dummy for the subset of relationships subject to loss underreporting. share performing refers to the share of total credit that is not in default. rel length refers to relationship length. firm share refers to the share of the firm's loan balance in the bank's loan portfolio. main lender is a dummy if the bank is the firm's largest lender. bank share refers to the share of the bank in the firm's loan portfolio.

One remaining potential issue is that underreported firms may be aware of their special status and also aware of the EBA shock affecting their lender. Firms could use the shock to extract additional credit from the bank by threatening immediate default on outstanding payments, which would impose a loss on the bank at a time when bank capital is scarce. According on anecdotal evidence, firms are passive actors in banks' reporting management and likely unaware of whether or not they are underreported. However, even if this mechanism were in operation, it would be consistent with the distorted lending channel that this paper documents.

1.4 Measuring the Effect on Misallocation and Productivity

In this section, we quantify the effects of changes in credit allocation due to the EBA intervention on aggregate productivity growth. We first outline the theoretical framework that allows us to decompose aggregate productivity into firm-level changes in inputs and TFP. This exercise follows the popular approach of inferring the presence of distortions, which give rise to factor misallocation, by measuring wedges in firms' first-order conditions (Restuccia and Rogerson (2008), Hsieh and Klenow (2009)). Estimating, and then plugging in, the pass-through of firm-level credit shocks into firm-level input use and TFP allows us to quantify the (partial equilibrium) effect of the EBA intervention on aggregate productivity. Finally, we address concerns about the measurement of wedges by using our quasi-experimental set-up to show that firm-level wedges respond to firm-level credit shocks, providing evidence that wedges are at least partially due to financial frictions.

1.4.1 Decomposing Productivity Growth

We use a (partial equilibrium) decomposition of productivity growth due to Petrin and Levinsohn (2012), which allows us to aggregate firm-level changes that arise as a result of the EBA intervention. This productivity decomposition is based on an economy with N firms, each of which produces a single good with a production technology $Q_i(A_i, X_i)$, where A_i and X_i denote firm-level TFP and inputs. Production uses two primary inputs, capital and labor, and two intermediate inputs, materials and services. Together these make up the input vector X_i.[41]

The portion of firm i's output which is not used as an intermediate input at other firms goes into final demand Y_i:

$$Y_i = Q_i - \sum_{x \in M,S} \text{input}_{xi}. \tag{1.5}$$

where M and S index materials and services.

Aggregate productivity growth (APG) is defined as a revenue-based Solow residual: the difference between the change in the value of final output and the change in the costs of primary inputs (all deflated).

$$APG \equiv \sum_i P_i dY_i - \sum_i \sum_{x \in K,L} W_{xi} d \text{ input}_{xi} \tag{1.6}$$

where W_{xi} denote the price of input x for firm i and K and L index capital and labor.[42]

By totally differentiating output, aggregate productivity growth can be decomposed into the change in firm-level physical productivity, or TFP, A_i and the reallocation of inputs across firms.

$$APG = \underbrace{\sum_{i=1}^{N} D_i d \log A_i}_{\text{Technical efficiency}} + \underbrace{\sum_{i=1}^{N} D_i \sum_{x \in K,L,M,S} (\epsilon_{xi} - s_{xi})\, d \log \text{input}_{xi}}_{\text{Reallocation of inputs}} \tag{1.7}$$

where $D_i = \frac{P_i Q_i}{\sum_i VA_i}$ is a Domar weight[43], $s_{xi} = \frac{W_{xi}\text{input}_{xi}}{P_i Q_i}$ is the revenue share of input x, and ϵ_{xi} is the output elasticity with respect to to input x.

In the absence of any frictions and distortions, firm profit maximization implies that the revenue share of an input equals the output elasticity ($\epsilon_{xi} = s_{xi}$). In this frictionless

[41] The choice of services as an intermediate is somewhat unorthodox but the Portuguese firm data does not provide information on electricity use, which is frequently used as an intermediate input alongside materials. However, the Portuguese firm data provides high quality information on services used in the production process.

[42] This expression is in terms of final demand, which already incorporates the effect of changes in intermediate inputs.

[43] Domar weights scale firm-level revenue ($P_i Q_i$) by total value added (VA_i). The Domar weights hence sum to more than 1.

benchmark, all firms equate marginal products and the reallocation term would be zero. In other words, the Solow residual equals aggregate TFP. Hence there would be no productivity gains from reallocating an input across firms because an input earns the same marginal product at each firm. However, in practice many real-world features lead to input wedges (Hsieh and Klenow (2009)). To the extent that wedges are driven by distortions such as financial constraints, taxes, monopoly power or other types of market failures, reallocating inputs to firms with high wedges increases aggregate productivity. In turn, anything that leads inputs the be allocated *away* from high wedge firms and towards low wedge firms reduces productivity and therefore output.

We can take the decomposition in equation 1.7 to the data using the following approximation[44]

$$APG_t \approx \sum_i \bar{D}_{it}(\Delta \log A_{it}) + \sum_i \bar{D}_{it} \sum_x (\epsilon_{xi} - \bar{s}_{xit})(\Delta \log \text{ input}_{xit}) \tag{1.8}$$

where a bar denotes the average across years t and $t-1$. Appendix C provides details on how we map this expression to firm-level data based on estimating production function parameters and firm-level TFP. Our preferred method estimates production function parameters separately for each 3-digit industry using cost shares.

We show that Portugal, like other Eurozone periphery countries, experienced negative productivity growth in the years leading up to the sovereign debt crisis. These estimates incorporate the services sector, which represents about 75% of employment and value added in Portugal (see Dias *et al.* (2016b) and Dias *et al.* (2016a) on the importance of accounting for services in aggregate productivity). Table 6 shows that this negative productivity growth was driven by an increase in the misallocation of inputs across firms, in particular of capital.[45] We thus confirm the finding of Gopinath *et al.* (2017) who document that the slow manufacturing productivity growth in Southern Europe in the 2000s was predominantly

[44]Equation 1.7 describes aggregate productivity growth in continuous time. We can use Tornquist-Divisia approximations to estimate this expression using discrete-time data.

[45]This result is robust to measuring capital both as the deflated value of fixed assets and using a perpetual inventory method to construct the real capital stock. See Appendix C for more details.

Table 6: *Aggregate Productivity Growth (APG) Decomposition*

	(1)	(2)	(3)	(4)	(5)
Year	APG	Technical efficiency	Labor	Capital	Intermediates
2007	-5.81	-0.39	0.60	-9.63	3.61
2008	-4.34	9.01	0.70	-11.60	-2.45
2009	-8.39	9.15	1.19	-19.60	0.87
2010	-1.38	7.14	1.30	-10.30	0.48
2011	-9.95	-2.60	1.50	-11.00	2.15
2012	-8.10	-4.90	2.50	-8.60	2.90
2013	-6.99	-8.18	1.80	-3.10	2.49
2014	10.31	18.32	0.52	-2.26	-6.27
Mean	-4.33	3.44	1.26	-9.51	0.47
Sd	6.49	8.90	0.68	5.39	3.31

Notes. The table shows average annual percentage growth rates. Column 1 is aggregate productivity growth. Columns 2-5 decompose the number in column 1 into the contribution of technical efficiency growth and reallocation of primary and intermediate inputs. Each column approximates a continuous-time measure of growth using discrete-time data. Output elasticities are computed using industry-level cost shares. Technical efficiency is a production function residual.

driven by a growing misallocation of capital.

1.4.2 The Effect of the EBA Intervention on Aggregate Productivity

We use the productivity decomposition in equation 1.7 to quantify how much of the decline in aggregate productivity growth can be explained by the EBA intervention. In this partial equilibrium exercise, we will assume that the firm-level wedges and Domar weight remain constant and estimate how firm-level TFP and input use change due to the EBA intervention. This quantification exercise can also be interpreted as the productivity losses that could have been avoided in a hypothetical world where all firms had borrowed from non-EBA banks (assuming those banks would have left their behavior unchanged).

The productivity decomposition in equation 1.8 shows that the EBA intervention can affect productivity growth in two ways. First, credit shocks could directly impact firm-level TFP. Second, credit shocks can lead inputs to be reallocated across firms. When undercapitalized banks reallocate credit from non-distressed firms to distressed, underreported firms, they prevent capital held by underreported firms from being reallocated to firms where this capital would have potentially earned higher returns. At the same time, credit taken up by underreported firms shrinks the available credit supply for firms with potentially high factor returns forcing them to shed inputs.[46]

The decomposition in equation 1.8 allows us to estimate the impact of the EBA intervention on both firm-level TFP and input use, and then map the predicted changes into productivity growth. To obtain these predicted changes, we combine the estimate of the size of the credit shock with the estimated pass-through of the credit shock into input use and TFP. For example, the change in labor due to the EBA intervention for a firm with a pre-shock borrowing share from exposed banks equal to treatment_i is

$$\Delta\widehat{\log L_i} = \hat{\gamma}^L \times \underbrace{\left(\hat{\delta}^{treat}\text{treatment}_i\right)}_{\Delta\,\widehat{\log\text{credit}_i}} \tag{1.9}$$

where $\hat{\delta}^{treat}$ is the estimated treatment effect in the firm-level credit regression, specification 1.3,[47] and $\hat{\gamma}^L$ is the estimated pass-through coefficient into employment growth based on specification 1.4 in section 3.[48]

While we find large and significant pass-through into all four (deflated) inputs, we find no significant pass-through into firm-level TFP (see Table 7). Therefore, we treat the effect of the EBA intervention on TFP as zero and focus on the effect on factor misallocation. This result highlights the limitation of using firm-level TFP residuals measures to learn about changes in aggregate productivity.[49]

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[46]This channel is consistent with a growing body of research that points to firm-level financial frictions as a driver of factor misallocation (Gopinath *et al.* (2017), Moll (2014), and Midrigan and Xu (2014)). We provide evidence that these firm-level financial constraints can in turn be caused by frictions at the bank-level.

[47]We estimate a non-dynamic version of 1.3 (not reported) to obtain point estimates on the cumulative change in credit during the EBA and bailout periods.

[48]For the subset of underreported firms, the size of the credit shock is given by the total treatment effect $(\hat{\delta}^{treat} + \hat{\delta}^{underreport})\text{treatment}_i$. We re-estimate the pass-through for deflated values of capital since the productivity decomposition in equation 1.8 is specified in deflated values. In addition, we estimate the pass-through into firm-level TFP and deflated intermediate inputs.

[49]TFP residuals are not generally informative about firm-level wedges since there is no inherent reason to expect firm-level TFP residuals and distortions to be correlated (Restuccia and Rogerson (2008), Hsieh and

Table 7: *Pass-Through Into Input Use and TFP*

Panel a	(1)	(2)	(3)	(4)	(5)
	TFP	Labor	Capital	Materials	Services
Δ log credit$_f$	-0.081	0.596***	0.704***	0.636***	0.636***
	[0.055]	[0.012]	[0.015]	[0.096]	[0.012]
Lag	-0.326***	-0.178***	0.170***	-0.350***	0.144***
	[0.023]	[0.003]	[0.012]	[0.005]	[0.015]
Controls	Y	Y	Y	Y	Y
Industry, size FE	Y	Y	Y	Y	Y
N	119,563	119,563	119,563	119,563	119,563
First-stage F statistic	195	195	195	195	195

Panel b	(1)	(2)	(3)	(4)
	Labor gap	Capital gap	Materials gap	Services gap
Δ log credit$_f$	-0.120***	-0.178**	-0.075	0.020
	[0.017]	[0.022]	[0.072]	[0.053]
Controls	Y	Y	Y	Y
Industry, size FE	Y	Y	Y	Y
N	102,495	102,495	102,495	102,495
First-stage F statistic	193	193	193	193

Notes. The table shows IV regression results at the annual firm-level. In panel a, the dependent variables are symmetric growth rates, which are second order approximation to the log difference growth rate. All variables are deflated according to procedure described in Appendix C. Capital refers to the real capital stock computed using the perpetual inventory method. TFP, or technical efficiency, is a production function residual. Labor refers to the number of employees. In panel b, dependent variables are firm-level gaps between output elasticities and revenue shares. We use the log change of the absolute value of the gap (to allow for negative gaps). We instrument for the log change in credit using the (normalized) firm-level borrowing share from banks exposed to the EBA shock prior to the shock. Controls consist of firm-size and 2-digit industry FE, as well as firm-level log total assets, interest/ebitda, capital/assets, current ratio, cash/assets and sales growth all averaged over 2008-2010. Lag refers to the lag of the dependent variable. Standard errors are clustered by industry. Standard errors in parentheses. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 8 shows that EBA intervention can account for over 50% of the decline in productivity growth in 2012. The results for capital are even starker: Our partial equilibrium estimates suggest that over 70% of the increase in capital misallocation can be explained by the EBA intervention. While the EBA intervention reduced allocative efficiency of capital, it positively contributed to the allocative efficiency of labor and intermediates. This potentially reflects the fact that the credit crunch corrects some of the pre-crisis over-expansion of firms with low labor or intermediate input productivity. .

Table 8: *Aggregate Productivity Growth (APG): Counterfactuals*

	(1)	(2)	(3)	(4)	(5)
	APG	Technical efficiency	Labor	Capital	Intermediates
Actual	-8.10	-4.90	2.50	-8.60	2.90
Decomposition (partial equilibrium)					
Contribution of EBA intervention	-4.58	0.00	1.03	-6.00	0.39
Contribution of credit reallocation in response to EBA (simulation)					
Minimum	-0.67	0.00	0.29	-1.30	0.34
Mean	-1.40	0.00	0.21	-1.70	0.09
Maximum	-2.04	0.00	-0.13	-2.00	-0.17

Notes. The table shows results from a partial equilibrium decomposition of aggregate productivity growth (APG). Contribution of EBA combines the effect of the credit crunch and the credit reallocation. Contribution of credit reallocation isolates the effect of the credit reallocation (keeping the level of credit constant). The simulation is described in the text. Capital is computed the perpetual inventory method described in the Appendix C. Technical efficiency refers to firm-level production function residual. All numbers are average annual percentage growth rates. Each column approximates a continuous-time measure of growth using discrete-time data. Output elasticities and technical efficiency are computed industry-level cost shares.

This counterfactual is partial equilibrium in nature and may over- or understate the true effects on productivity, depending on the sign of the general equilibrium effects of the EBA intervention. For example, it is possible that the negative credit shock for firms borrowing from exposed banks would have led their competitors borrowing from non-exposed banks to increase their input use. If competitors have high marginal products, then this effect

Klenow (2017), Nishida *et al.* (2017)).

may have moderated the negative effect on productivity.[50] In contrast, negative spillover effects, evidence of which we document in Appendix B, would suggest that this exercise understates the true effect on productivity.

Disentangling Credit Crunch and Credit Reallocation

Until now, we have lumped together the effect of the credit crunch and the credit *reallocation* to underreported firms. We now ask how much of the 2012 productivity decline can be explained by the reallocation component.[51] We proceed in two steps. First, we isolate the effect of the credit crunch by keeping the level of the credit crunch constant but changing the incidence of the credit shock. We assume that underreported, distressed firms receive the baseline credit crunch treatment and simulate assigning their positive treatment effect instead to a randomly chosen subset of non-distressed firms. We run this simulation 10,000 times (for 10,000 different subsets of firms) holding the size of the subset fixed at the number of underreported, distressed firms. Second, we subtract this simulated 'credit crunch only' effect from the overall contribution of the EBA intervention to isolate the effect of the credit reallocation.

Table 8 shows the credit reallocation induced by the EBA shock accounts for close to 20% of the total productivity decline in 2012. The reallocation component has an unambiguously negative effect on capital misallocation. The reallocation component also appears to have small positive effects on the allocative efficiency of labor and intermediate inputs. This suggests that some the underreported, distressed firms have a higher marginal return on labor and intermediate input use than some non-distressed firms.

[50] For example, Rotemberg (2017) shows that ignoring such competition spillovers can lead to an overestimate of the effects of a policy intervention on aggregate productivity.

[51] The credit reallocation to underreported firms amplifies the credit crunch for all other firms by shrinking the credit supply available to non-underreported firms. Hence part of the credit crunch effect on productivity should be attributed to the distorted lending incentives driving the credit reallocation. The previous exercise therefore constitutes an upper bound, which assumes that the entire credit crunch is driven by the reallocation.

1.4.3 Do Firm-Level Gaps Capture Distortions?

A key assumption in the productivity decomposition is that firm-level input wedges capture firm-level distortions or frictions. A growing literature has argued that misallocation measures based on firm-level wedges may simply be the result of adjustments costs, time-varying mark-ups or volatility in productivity shocks. These forces imply that static first-order conditions, the deviation from which we pick up as wedges, are not the right benchmark for efficiency (Asker *et al.* (2014), Restuccia and Rogerson (2017)). However, we show that firm-level wedges respond the credit shocks induced by the EBA intervention, providing evidence that the wedges, at least partially, capture financial frictions at the firm-level.

We rely on our firm-level IV specification given by equation 1.4 to estimate the effect on firm-level wedges. The dependent variables are now log changes in the absolute value of firm-level wedges between estimated output elasticities and (nominal) revenue shares of labor, capital and intermediate inputs (materials and services). In practice, the revenue shares will drive the results since output elasticities are estimated at the 3-digit level and will be absorbed by industry fixed effects.[52]

We find significant effects on firm-level labor and capital wedges of about 12-17% (see panel b of see Table 7). We find no statistically significant effects on pass-through into wedges of intermediate inputs (materials and services). This is in line with Petrin and Sivadasan (2013), who find that intermediate inputs in Chile are subject to fewer distortions and generally feature lower wedges in the data than primary inputs such as labor and capital.

The validity of these estimates relies on the assumption that there are no other concurrent shocks, which are correlated with the firm-level borrowing share from EBA banks, that could drive the changes in wedges. We address one popular potential alternative determinant of wedges: time-varying volatility of productivity shocks (Asker *et al.* (2014)). The firm-level

[52] Revenue shares are the key ingredient to firm-level wedges in a wide range of misallocation frameworks such as Hsieh and Klenow (2009).

borrowing share from EBA banks is not correlated with firm-level sales or productivity volatility nor with sales cyclicality.[53] In addition, we confirm that the results are robust to controlling for firm level sales and productivity volatility in Appendix B.

1.4.4 Indirect Channel: Industry Spillovers

Firms that are not directly affected by the EBA intervention can still be indirectly affected by the presence of underreported, distressed firms in the same industry. For example, Caballero *et al.* (2008) provide evidence from Japan that a higher share of near-insolvent firms ('zombies') reduces the profits for healthy firms in the same industry, which discourages entry and investment of healthy firms. Such congestion effects act like a tax on healthy firms causing them to hire less labor and capital than they would have done in the absence of the zombie firms.[54]

We quantify the productivity losses from this channel by regressing input use and TFP in the sample of firms that borrow exclusively from *non-exposed* banks and are *not* underreported on the share of underreported firms in their industry.

$$\Delta \log \text{input}_{is} = \varphi \text{share}_s + \text{controls} + v_{is} \tag{1.10}$$

where i and s denote firm and industry. share_s is the share of underreported firms in a 3-digit industry based on total assets held by these firms, which fluctuates between 0% and 18% in our data. Controls include firm-level characteristics in the pre-period.

This regression is problematic because the share of underreported firms may be correlated with unobserved industry-level shocks driving the performance of non-distressed and non-exposed firms. To overcome this problem, we instrument for the share of underreported

[53] For productivity volatility, we follow Asker *et al.* (2014) and compute $sd(\log(A)_{it} - \log A_{i,t-1})$ where $\log(A)_{it}$ are the revenue-based production function residuals, which we have been referring to as TFP. The correlations are -0.012 for firm-level cyclicality. (measured as correlation of firm-level log sales with industry-level log sales), -0.0098 for firm-level productivity volatility and -0.0221 for firm-level volatility of log sales.

[54] There is also evidence for such a negative spillover channel in Europe (Moreno-Serra *et al.* (2016) and Acharya *et al.* (2017)).

firms using the average industry exposure to the EBA shock.[55] This instrument exploits that industries more exposed to the EBA intervention will have a larger share of underreported firms in 2012, as the the heightened distorted lending incentives will lead underreported firms borrowing from EBA banks to expand.[56]

Table 9 shows that we find significant and large, negative spillover effects on sales, capital, labor and services by firms that borrow only from non-EBA banks. A standard deviation increase in the share of underreported firms implies 10% percent lower sales growth for firms not directly affected by the EBA shock through their lender. We find no spillover effects on the use of materials or firm-level TFP. In Appendix B, we show that these results are robust to using less or more fine-grained industry definitions.

The validity of the spillover estimates relies on an exclusion restriction that the average industry exposure to the EBA shock is only correlated with the outcomes of non-EBA firms through the share of underreported firms in their industry. This could be potentially violated if the EBA-induced credit crunch spurs the expansion of competitors borrowing from non-exposed banks in the same industry. However, such competition effects would bias us against finding *negative* spillovers.

We map these spillovers into productivity by again assuming that all firms had borrowed from non-exposed banks. Based on the first stage regression (not reported) we obtain counterfactual industry shares, which we can map into counterfactual input use by firms not affected by the EBA shock through their lender. The aggregate productivity losses are small and can only account for about one percentage point of the total decline in productivity.

[55] A common fix to this problem, replacing the level share with the change in the share, only identifies a relative effect rather than the level effect we are interested in (see Schivardi *et al.* (2017) for details on this critique).

[56] By focusing on firms that borrow from non-EBA banks, we ensure that the direct effect of the EBA shock on non-underreported firms (which is negative and potentially correlated with the instrument) does not confound our estimates. Schivardi *et al.* (2017) estimate spillovers using the share of lending from banks close to the capital constraint in an industry-region unit. However, they cannot control for the decline in credit supply to healthy firms at low capital banks, which we document in this paper. Hence their estimated spillover effects will combine the negative credit supply effect for healthy firms, which we treat as part of the direct channel, and the congestion spillover, which is the focus of this subsection. We also improve on their identification strategy by using an exogenous source of bank capital adequacy.

Table 9: *Regression Results: Spillovers*

	(1)	(2)	(3)	(4)	(5)	(6)
	Sales	Capital	Labor	Materials	Services	TFP
Industry share of	-0.107***	-0.082***	-0.044***	-0.029	-0.073***	0.013
underreported firms	[0.017]	[0.019]	[0.006]	[0.025]	[0.013]	[0.010]
N	43,273	43,273	43,273	43,273	43,273	43,273
First-stage	3522	3523	3524	3525	3526	3527
	(1)	(2)		(1)	(2)	
Tradable	-0.109***	-0.080***	High collateral	-0.077***	-0.073***	
	[0.017]	[0.018]		[0.015]	[0.018]	
Non-tradable	-0.121***	-0.066***	Low collateral	-0.069***	-0.131***	
	[0.015]	[0.016]		[0.024]	[0.029]	
N	43,273	43,273		34,346	34,347	
First-stage	2626	2627		1831	1832	

Notes. The table shows IV regression results at the firm-level for 2012. Share underreported refers to the asset-weighted share of distressed, underreported firms in a 3-digit industry. We instrument for this variable using the average firm-level borrowing share from EBA banks. We standardize the share such that the coefficients should be interpreted as the effect of increasing the industry-share of underreported firms by a standard deviation. The dependent variables are all in log changes and deflated. TFP is referred to as technical efficiency in the text and is a production function residual computed. Controls consist of firm-size bucket FE as well as firm-level log total assets, interest/ebitda, capital/assets, currrent ratio, cash/assets and sales growth all averaged over 2008-2010. Robust standard errors in parentheses. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

The reason for the small effects is that the median industry-level share of underreported firms is small, limiting the size of the negative spillovers.[57]

1.5 Conclusion

This paper provides evidence from Portugal that a weak banking sector has contributed to low productivity growth in the aftermath of the European sovereign debt crisis. The richness of our data allows us to establish a credible causal chain from a weak banking sector to adverse effects on productivity and growth. Our identification strategy relies both on a natural experiment that induces exogenous variation in banks' capital adequacy and the ability to identify where banks are underreporting incurred loan losses. We argue that the incentive to underreport loan losses is correlated with two distorted lending incentives: delayed loss recognition and increasing exposure to high-volatility, distressed borrowers. Our measure of loss underreporting provides us with a powerful tool to detect and quantify distorted lending. We show that the credit reallocation affects firm-level investment and employment and quantify productivity losses from the resulting misallocation of labor and capital.

While we exploit a relatively short-lived regulatory intervention to cleanly identify the costs of undercapitalized banks, we provide evidence that the problem persists beyond the regulatory intervention we study. The underreporting of loan losses is pervasive both in the lead-up to the regulatory intervention and in the years after the intervention. This reflects that Portuguese banks faced persistent capital pressures in the aftermath of the sovereign debt crisis.

Our paper contributes to the important policy debate on how to regulate bank capital. The results in this paper suggest that European economies, at least in the periphery, may have benefited from higher pre-crisis levels of capital which would have better insulated European banks from losses during the sovereign debt crisis and avoided distorted lending

[57] The median industry level share of underreported firms in terms of assets is about 1%. Maximum exposure is 16%.

incentives arising at undercapitalized banks. Moreover, our results highlight that simply raising capital ratios may heighten perverse lending incentives so long as banks are not forced to raise equity at the same time.

Chapter 2

Chapter 2: Debt or Demand?

2.1 Introduction

Corporate investment in the periphery of the Eurozone has suffered a prolonged decline since 2008 which further intensified in the aftermath of the European sovereign debt crisis. Two competing explanations of this decline have been put forward: the lack of product demand and the large stock of corporate debt. The first explanation holds that a negative demand outlook limits firm investment. The second explanation argues that a large debt stock inhibits the ability of firms to finance investment as interest payments reduce available cash flow, tightens collateral constraints impeding access to external financing, and reduces incentives to invest as debt rather than equity holders pocket the proceeds from investing.

This paper contributes two insights to this debate. Studying a large, temporary investment tax credit in Portugal, we first show that the relationship between a firm's debt burden and the likelihood that a firm invests in response to the tax credit is negative but non-linear. Below a certain 'kink point', the negative relationship between debt and the likelihood that a firm uses the tax credit is moderate. However, once a firm crosses over this kink point of indebtedness, a larger debt burden is associated with a sharp decrease in the likelihood that a firm invests in response to the tax credit. Second, the effect of demand is mediated by the size of a firm's debt burden. We find that there is a positive relationship between demand for a firm's product and the likelihood that the firm invests in response to the tax credit. However, demand ceases to have a significant effect on tax credit take-up for the tail of highly indebted firms.

Our results are consistent with theories of debt overhang, which predict that beyond a certain level of leverage firm owners will forgo profitable investment projects because the proceeds from these projects accrue to the firm's debt holders rather than to owners (Myers (1977)). This characterization of firm control rights applies to the vast majority of Portuguese firms, which are privately owned, are frequently managed by those owners, and finance investment with bank debt. Our results hence suggest that the distribution of debt across firms, rather than the absolute stock of debt, matters for correctly quantifying the adverse consequences of high corporate sector debt. Moreover, our results provide a potential explanation why a pick-up in demand conditions will have limited success in speeding up the post-crisis recovery in the absence of a concurrent reduction in corporate debt.

We study the 2014 extraordinary investment tax credit in Portugal (*Credito Fiscal Extraordinario ao Investimento* - CFEI). Since this large and temporary tax credit gave firms a strong incentive to invest in the second half of 2013, it provides an ideal setting to better understand the factors that hold back corporate investment. Firms received a 20% tax credit on all investment undertaken between June and December 2013. The benefit was large, being equivalent to a reduction of the effective corporate income tax rate from 25% to 7.5%. The investment tax credit was universal, applying to all firms, sectors, and types of investment (including replacement investment). It was granted automatically based on 2013 tax returns. Firms were allowed to defer the tax benefit for up five years in cases when the tax credit exceeded the 2013 tax bill.

Our main empirical specification is an instrumental linear probability model that predicts each firm's take-up of the investment tax credit. We obtain firm-level data on the take-up of the tax credit from the Portuguese Tax Authority. Our main explanatory variables are the log change in firm-level sales, which we instrument with a measure of foreign demand constructed from customs data, and a measure of a firm's debt burden. For the latter, we construct an index that combines three commonly used debt-earning ratios. To allow for a non-linear effect of debt, we divide firms into four quartiles based on our debt-earnings index. We include interaction terms between the debt quartiles and the demand measure to study the interaction between debt and demand. We include a wide range of controls that potentially affect firms' investment decisions: estimated default risk, indicators for solvency and negative equity, past investment spending, productivity, liquidity measures, and industry/region/main lender fixed effects.

We use customs data to construct a demand instrument for exporting firms following Berman *et al.* (2015). This measure exploits exogenous variation in demand from product-destination level changes in foreign demand. We first calculate the share of each product-destination pair in a firm's export portfolio prior to the tax credit. We then calculate how much an export destination imported of a given good from countries other than Portugal in the year of the tax credit based on global import data. We combine the (time-invariant) firm-level shares with the change in foreign demand to obtain a firm-level measure of the change in product demand. We use this instrument to predict the log change firm-level sales in the sample of exporters. As a robustness test, we use an additional instrument which is based on the fraction of Portuguese firms in an industry that report low product demand as a major investment limitation in a bi-annual survey.

We combine this variation in demand with a measure of a firm's debt burden. Given that exogenous variation in debt is notoriously hard to come by, we proxy indebtedness by an index of three commonly used debt-earnings ratios. The ratios are debt/EBITDA, interest payments/EBITDA, and debt/cash flow. Debt-to-earnings ratios capture the ability of a firm to service its debt burden and capture two types of debt frictions that can generate under-investment: debt overhang and net worth based borrowing constraints. Unlike debt overhang, which reduces the attractiveness of new investment to firm owners, net worth based financial constraints limit the ability of highly indebted firms to finance investment they would like to undertake. A high debt-earnings ratios identifies firms for which financial constraints are likely to bind. The cash flow of highly indebted firms is absorbed by debt payments leaving little free cash flow for investment. However, their low net worth means that they cannot easily finance investment with debt as banks are reluctant to lend to low net worth firms.

We present the following two main results. First, there is a kink in the relationship between indebtedness and the take-up of the tax credit. Firms in the lower two quartiles of the debt-earnings index have roughly equal predicted take-up probabilities. In contrast, predicted take-up drops by 50% for firms in the third quartile while firms in the worst debt-earnings quartile have a predicted take-up rate close to zero. Second, the effect of demand is mediated by a firm's debt burden. For firms in the lowest debt-earnings quartile, demand has a highly significant positive effect with a 10% increase in sales (instrumented by foreign demand) leading to a 9 p.p. higher take-up probability. In contrast, demand ceases to have a significant impact on take-up for firms in the worst debt-earnings quartile. OLS attenuates the effects of demand by a factor of three, highlighting the importance of using exogenous variation in demand. We find similar results when predicting the amount of invested conditional on a firm using the tax credit. Because the sample shrinks significantly in this intensive margin specification, we obtain less precise estimates.

This paper relates to a large literature on the determinants of corporate investment. Our findings are consistent with theories of debt overhang. The basic insight that a large debt burden inhibits investment when firm owners have control rights goes back to Myers (1977). Lamont (1995) shows that debt overhang is most likely to bite in adverse macroeconomic conditions. Empirical corporate finance research has provided evidence for a debt-overhang driven negative relationship between debt and investment (Hennessy (2004), Giroud *et al.* (2012)). We provide evidence that the relationship between debt and investment is not only negative but non-linear, consistent with the predictions of debt overhang models.

Our findings are also consistent with a growing literature on financial constraints limiting corporate investment. Financial constraints also generate a negative relationship between debt and investment. If firms face net worth based borrowing constraints, any negative shock to the value of their collateral limits the ability of firms to borrow (Bernanke (1983), Bernanke *et al.* (1996), Moore and Kiyotaki (1997)). Given that Portuguese firms are heavily bank-dependent for their external financing needs, financial constraints are likely relevant for many Portuguese firms. We leave the challenge of disentangling debt overhang from the effect of financial constraints for future work.

We contribute to the debate on the determinants of sluggish investment dynamics in the aftermath the 2008 financial crisis. Kahle and Stulz (2013) argue that demand factors are more important than firm-level balance sheet constraints. Similarly, Bloom (2009) and Bloom *et al.* (2014) highlight the importance of uncertainty, in particular an uncertain demand outlook, in dampening firm investment.[1] In contrast, Giroud and Mueller (2017) provide evidence that firm balance sheets can account for a significant fraction of job losses in the US post-2008. Kalemli-Ozcan *et al.* (2015) use pan-European firm-level data to show that higher debt levels are associated with lower investment post-crisis. Mian and Sufi (2011) and Mian and Sufi (2014) argue that household debt overhang has been an important factor dragging down household consumption in the US post-2008.

We provide evidence that these different explanations are not mutually exclusive but interact in important ways. An improved demand outlook may have little effect if a large debt burden constrains firms from responding. For example, Arellano *et al.* (2016) show that a higher level of uncertainty affects highly indebted firms more as their ability to insure against the risk is more limited. More generally, our results highlight the limitations of investment panel regressions in which a measure of indebtedness enters linearly, which fail to instrument for demand and that omit an interaction of debt and demand. Any of these omissions would lead to an underestimate of the negative effect of debt on investment spending

This paper is organized as follows: Section 1 provides details on the investment tax credit. Section 2 discusses the data sources and presents our empirical strategy. Section 3

[1]We abstract from the role of uncertainty in our analysis due to the difficulty of constructing reliable measures of firm-level idiosyncratic risk.

provides results and section 4 concludes.

2.2 Background: 2013 Investment Tax Credit

In this section we provide details on the 2014 extraordinary investment tax credit (*Crédito Fiscal Extraordinário ao Investimento* - CFEI) in Portugal. The CFEI tax credit was announced by the Portuguese government in May 2013 with the aim to stimulate corporate investment that had been slow to recover following the 2011 financial crisis in Portugal. The tax credit provided firms with an automatic 20% tax rebate on all investment spending undertaken between June and December 2013. The tax credit was designed to be as general as possible in order to avoid falling under EU state aid restrictions, which apply to other fiscal incentives in Portugal. These rules restrict state aid to particular disadvantaged regions and industries. In contrast, the CFEI applied to all sectors, regions, and type of investment expenditures. Importantly, it applied not only to new investment projects but also to existing investment projects and replacement investment.

The tax credit was large. Firms received a tax credit worth 20% of investment expenditures between the 1st of June and the 31st of December 2013, limited to 70% of the total tax bill and with a maximum eligible investment expenditure of EUR 20M per firm. The benefit was equivalent to a reduction of the effective corporate income tax rate from 25% to approximately 7.5%. The tax credit could be deferred for up to five years when a firm's 2013 corporate income tax bill was too low to take full advantage of the CFEI benefit. The data show that most of the tax credit was disbursed in 2013, with 2014 deferred credits being less than a third of the 2013 tax credits claimed. The gross tax credits paid out under CFEI amounted to EUR 233 M, 2.37% of total capital expenditure (CAPEX) spending in 2013.

Take-up, measured as the fraction of firms receiving a tax credit, was very low with only about 4% of eligible firms claiming the tax credit. Manufacturing firms dominated the use of the investment tax credit but services, which usually do not qualify for any type of investment tax benefits, also made use of the tax credit. Take-up differs substantially across firm size: 24.74% of large and 23.81% medium firms used the tax credit, while 8% and less than 1% of small and micro firms used the tax credit.[2]

Aggregate investment increased in the year of the tax credit. According to firm balance sheet data, total CAPEX in 2013 increased by 6.15% relative to 2012. Similarly, national accounts data from INE (National Statistics Office) registered 8.11% growth in gross fixed capital formation (GFCF) in 2013 after four consecutive years of negative investment growth.[3] While some of this increase may be due to the investment tax credit, estimating the causal effect of the investment tax credit on aggregate investment is beyond the scope of this paper.

The tax credit imposed no additional administrative burden on firms. The tax credit was automatically applied based on the investment spending indicated on a firm's tax return. The tax credit was widely advertised following the announcement in May 2013. It is hence unlikely that either lack of knowledge or administrative costs would have prevented firms from making use of the investment tax credit. While the credit only applied to investment conducted between June and December, some flexibility was allowed for firms who had already begun implementing investment projects in the first semester of 2013.

In comparison to the existing investment tax benefit programs, CFEI was larger in gross payments and applied to a much wider range of firms and industries. These differences reflect that the existing investment tax benefits apply only to investment in structurally disadvantaged regions, such as Northern Portugal, or to investment in specific industries such as agriculture.[4] Figure 32 in the Appendix shows that CFEI, in aggregate terms, paid out more than the three main alternative investment tax credits combined. Moreover, the number of firms benefiting from the CFEI was 6-times higher than the number of firms

[2]Figure 34 in the Appendix shows the use of tax credits in 2013 disaggregated by industry and size.

[3]Note that the concepts of capital expenditure and GFCF are differ slightly in what they include as investment spending, which yields the difference in numbers. Additionally, many firms with positive investment do not report the cash flow statement where our measure of capex is located, so the capex measure covers a smaller pool of firms

[4]There are a variety of investment tax benefits, of which three make up the majority of the claims. One is a contractual program with a 10 year period for big investment projects in strategic economic sectors subject to a formal approval process. The other two programs consist of automatic tax deductions either under the investment fiscal support regime designed for specific industries and regions on the one hand or research and development projects on the other hand. All three programs have similar eligibility criteria, falling under the European Union (EU) state aid regulation which restricts aid to certain industries (such as agriculture), regions (preferences for poorer areas), and types of investment.

using an existing tax credit. Table 29 in the Appendix shows that there is little overlap in use suggesting that the CFEI was successful in targeting firms that were not already benefiting from an existing program.

2.3 Data and Empirical Strategy

In this section we describe our data and empirical strategy. We combine administrative tax data and firm census data to estimate the effect of debt frictions and demand on (a) take-up rates of the investment tax credit, and (b) the amount invested. We instrument for product demand using variation in foreign demand for exporting firms following Berman *et al.* (2015).

2.3.1 Data

We collect data on firm-level tax credit use provided online by the Portuguese Tax Authority. The data cover tax benefits received in 2013 and 2014. Given that firms could defer the benefit for up to five years if their tax bill was low, it is possible that we do not capture every single firm that used the tax credit. However, the data show that the 2014 benefits were already only a small fraction of benefits disbursed in 2013. Hence it is likely that most firms at least received some of the benefit in either 2013 or 2014 allowing us to accurately estimate the effect on take-up rates.

We combine the tax data with proprietary data from the Bank of Portugal. We obtain information on the universe of corporate lending relationships from the credit register (*Central de Responsabilidades de Credito* or CRC) which contains monthly information on every loan in Portugal that exceeds EUR 50. We merge the credit register with balance sheet and other financial variables for non-financial firms. The data comes from the Simplified Corporate Information (*Informacao Empresarial Simplificada* or IES), an annual, mandatory firm census.

Our final dataset covers 286,775 non-financial Portuguese firms in an unbalanced panel from 2010 to 2014, with a total of 986,141 firm-years. We only include firms that have data in both the credit register and firm census for our sample period. We drop observations with any of the following characteristics: negative assets or liabilities, missing sales, age greater than 250, and missing industry classification. All variables are scaled by total assets or liabilities and winsorized at the 2% level. For most of our analysis we restrict the period to 2012, for which we have observations for 199,767 firms holding around 365 billion euros in assets. In many specifications, we focus on the sample of 8,874 exporting firms, defined as any firm with positive export value. In 2012, 3,629 firms were exporters, approximately 1.8% of the 2012 sample.

Table 30 in the Appendix provides descriptive statistics for our sample of firms by take-up of the investment tax credit. We find that firms using the investment tax credit tend to be larger (in terms of total assets and number of employees) and older than the average firm. They have more trade debt and less bank and debt owned to government institutions (e.g. social security debt)[5] and perform better in terms of cash flow and equity. Nonetheless, they pay a comparable amount of income tax as a fraction of total assets and earn an equivalent amount of sales.

Firms that accessed the investment tax credit on average have lower credit risk. None of the firms using CFEI were insolvent in 2012 nor did they suffer a default episode that year.[6] Firms that use the tax credit have slightly more outstanding performing credit but little to no overdue credit. Both groups of firms have on average 20% sales growth from 2011 to 2012. The change in fixed assets, calculated net of depreciation, is less volatile in the group of firms that take up the tax credit. Capital expenditure is also fairly similar across groups, making up $4-5\%$ of total assets. We also find that firms that use the investment tax credit have slightly lower TFP.[7]

[5]This debt predominantly takes the form of unpaid social security or income tax obligations. This debt is senior to all other debt obligations and cannot take a haircut.

[6]A firm is insolvent if it has an open process in bankruptcy court or has been liquidated. Default is defined as any incident of at least three months of overdue credit, where the overdue credit accounts for at least 5% of the loan volume (with a minimum of 50 Euros) with that lender. We follow the methodology described in Antunes *et al.* (2016) to estimate default risk.

[7]We estimate TFP following the Olley and Pakes (1996) three step regression procedure which allows for

2.3.2 Measuring Firm-level Debt Burden

We measure debt frictions by a firm's capacity to serve its debt burden. We prefer debt-earnings measure over leverage ratios such as debt/equity or debt/assets. The large variation in the structure of liabilities and assets across firm size and industry makes such measures less comparable than debt-earnings ratios. Similarly, simple definition of negative equity that are frequently employed in household research are not valid for firms with complex balance sheets. Moreover, negative equity is usually defined in market values while the regulatory balance sheet information only provides book values.

Instead, we combine information from three commonly used debt-earnings ratios into a single index. Debt-to-earnings ratios capture two types of debt frictions that generate under-investment: debt overhang and net worth based borrowing constraints. Debt overhang describes a situation in which the firm's managers forgo positive net-present value (NPV) investment projects because the surplus from these projects accrues to a firm's debt holders rather than its equity holders who have control rights (Myers (1977)). Debt overhang is highly relevant in Portugal where the vast majority of firms are privately owned, often run by their owner, and heavily reliant on bank debt.

The second debt friction are financial constraints that limit firms' ability to raise sufficient external financing to finance positive NPV projects when firms are highly indebted. Such net worth based financial constraints are now a standard ingredient in macro-finance models. Unlike debt overhang, which reduces the attractiveness of new investment to firm owners, net worth based financial constraints limit the ability of highly indebted firms to finance investment they would like to undertake. Debt-earnings ratios capture when such financial constraints are likely to bind. The cash flow of highly indebtedness firms is absorbed by debt payments leaving little free cash flow for investment. However, their low net worth means that they cannot easily finance investment out of debt as banks are reluctant to lend to low net worth firms.

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endogeneity of some of the inputs, selection (due to firms exiting the market), and long-lasting unobserved differences across firms.

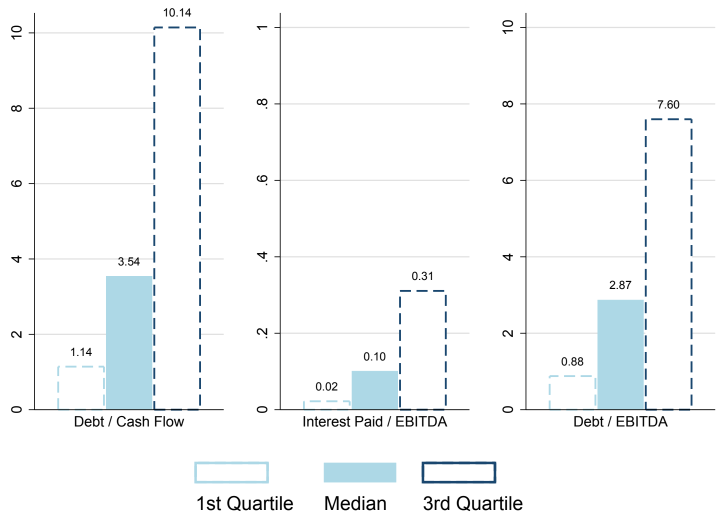


Figure 10: *Distribution of Debt-Earnings Ratios*

Notes. Figure 10 shows the quartile cut-offs for the the debt-earnings ratios used to construct our debt index. All numbers are for Portuguese non-financial firms in 2012. For ease of exposition, we show debt/cash flow even though we use cash flow/debt in the construction of the index. The values are shown in absolute value due to the discontinuity caused by negative EBITDA.

Our debt-earnings index is constructed as follows. We rank firms from least to most indebted, allowing for ties, according to each of the following three ratios: debt/EBITDA, cash flow/debt, interest paid/EBITDA. We then divide firms into quartiles according to their ranks. We assign each of the quartiles a point value (1st quartile=1, 2nd quartile=2, etc). Finally we rank the firms by their combined points and again divide them into quartiles. Firms in the first quartile of our debt overhang proxy have a small debt burden, while firms in the fourth quartile have a severe debt burden. Figure 10 shows the quartile cut-offs for the debt-earnings ratios. Table 31 in the Appendix shows the number of firms in each quartile by use of the investment tax credit.

Quartiles are attractive for three reasons. First, the quartiles allow the impact of debt frictions to increase in a non-linear fashion with the severity of the friction. Second, this approach also allows us to deal with the possible discontinuity that arises from some firms reporting negative EBITDA. Negative EBITDA values make it difficult to interpret a continuous debt/EBITDA measure. In this case negative EBITDA firms would be ranked at the bottom even though they will face a more severe squeeze from their debt burden than firms with positive EBITDA and the same amount of debt. To address this issue, we shift firms with negative EBITDA (but positive debt) to the right of the distribution, above firms with positive debt and positive EBITDA. We chose this approach over simply dropping the negative EBITDA observations because these firms represent a third of our sample. Third, we prefer the relative ranking of firms given that the choice of an absolute threshold is somewhat arbitrary. For example, a debt/EBIDTA level of five, which is considered a red flag by credit analysts, corresponds to the 65th percentile in our sample in 2012 and hence would include a larger number of firms in the highest debt group.

2.3.3 Measuring Product Demand

We use an instrumental variable approach to isolate plausibly exogenous variation in firm sales. Our main instrument is the change in foreign demand for exporting firms. As a robustness test, we also rely on investment survey data that directly asks for the role of product demand in limiting investment.

Exporter Demand We construct an instrument for foreign demand based on non-Portuguese imports to Portuguese export destinations from product-level customs data. We proceed in two steps to construct instruments for foreign and domestic demand following Berman *et al.* (2015). We first calculate time-invariant weights for each export product-destination pair in a firm's export portfolio over the years 2005-2011 based on detailed customs data. We then use global import data to calculate how much an export destination imported of a given good from countries other than Portugal. This value is our measure of the export destination's demand for a given product. Hence all time-variation in the demand instrument comes from changes in the country-level imports, not the firm-level weights.

We can then construct our firm-level instrument for foreign demand as follows:

$$\text{Foreign Demand}_{it} = \sum_{jp} \omega_{ijp} M_{jp,t} \tag{2.1}$$

where ω_{ijp} is the average share of each product p (defined at the HS-6 level) and destination j in firm i's exports over the period (time invariant weights) and $M_{jp,t}$ is the total value of imports for product p and destination j (not including Portugal) in year t.

We can also use a similar approach to construct an instrument for domestic demand.

$$\text{Domestic Demand}_{it} = \sum_{p} \omega_{ip} M_{p,t}^{PT} \tag{2.2}$$

where ω_{ip} is the average share of each product p in firm i's exports over the period and $M_{p,t}^{PT}$ is the total values of Portuguese imports for product p in year t.

Our baseline version focuses on the firm's core product, which is the HS-4 product with the highest average export value over the period. As a robustness check, we consider all products of a given firm. For the core product measure, ω_{ij}^{core} are the weights of destination j in firm i's core product exports. That is,

$$\text{Core Foreign Demand}_{it} = \sum_{j} \omega_{ij}^{core} M_{j,t}^{core}$$

The equivalent measure for domestic demand is simply the aggregate imports of the core product for each firm.

To construct firm-level weights, we use data from the Portuguese customs agency (*Comercio Internacional*) from 2005-2013. The data provides the export value (price times quantity) of each product (HS6-level) to each destinations at the annual level. The data provides tax identifiers for each firm and hence allows us to match the customs data to the firm-level census and credit register. We can match 28,329 across datasets. We match the firm-level Portuguese customs data to BACI, an International Trade Database, to construct the changing import values for each product. BACI provides data on import values disaggregated by country and product (HS6-level). Figure 36 in the Appendix shows the a time series of the demand measures compared with actual export and sales values. While the scales are very different, the aggregated constructed measures largely follow the aggregate trends. In 2013, around a quarter of firms received a negative foreign demand shock (defined as a greater than 5% decrease in our demand measures) while around twelve percent had negative domestic demand growth.

Industry Level Survey Responses We use micro data from a biannual investment outlook survey conducted by the Portuguese National Statistics Office to construct a measure of whether firms perceive poor sales to be a major investment limitation.

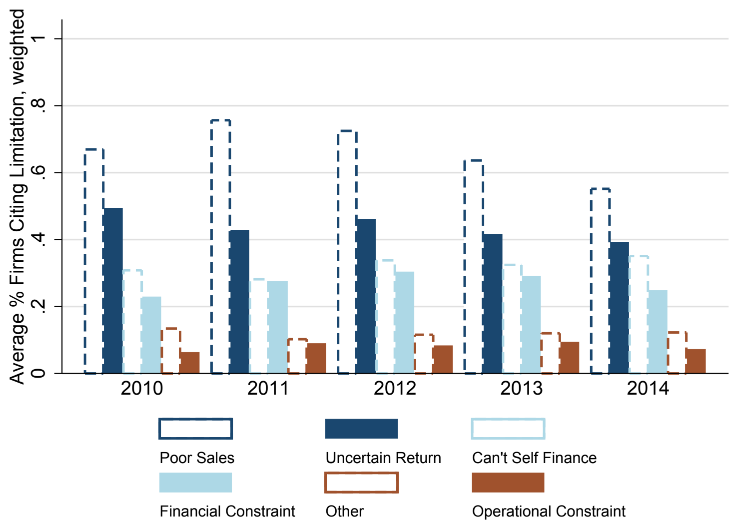


Figure 11: *Survey Responses: Limitations to Investment*

Notes. Figure 11 shows the fraction e of firms citing each of the main investment limitations across years. Averages are weighted by sample weights described in the text. For ease of exposition, some limitations are combined. "High interest rate", "poor access to bank credit", and "poor access to capital markets" are all considered financial constraints. "Low production capacity" and "can't find qualified personnel" are considered operational constraints. The remaining limitations are left as presented in the survey.
Source: Portuguese firm census, own calculations

The survey has a sample of approximately 3,500 firms each year. The survey samples from firms with more than 4 workers and annual sales of at least 125,000 Euros according to two stratification variables: industry group and number of workers.[8] Firms with more than 200 workers are exhaustively sampled. Because firms with more than 200 workers are very different from the firms with less than 200 workers, which make the bulk of firms in Portugal, we run our results separately for the two groups.

We focus on the following set of questions about limitations to investment: (1) "was

[8]The survey follows a methodology that is uniform across participating European Union countries. Specifically, the National Statistics Office classifies firms by industry at the 2-digit level except for manufacturing firms where they use 3 digit industry codes. They exclude agriculture and some professional service firms. Additionally, they classify firms into four employment groups: firms with less than 50 employees, with between 50-249 employees, between 250 and 499, and more than 500 employees.

investment limited by some factor"; (2) "which factors presented limitations" (selected from a list); (3) "which factor was the most severe limitation". Firms answer these questions twice a year, in April and October, for different reference periods: the current year, the previous year (in April), or the next year (in October). We use the most recent firm response available. Figure 11 summarizes firm responses to the list of limitations. Poor sales outlook or low demand is always the most cited limitation, with uncertain returns and financing constraints the next most cited.

For each group, we collapse the firm-level survey data to the 2-digit industry level and take the average response in each industry as our measure for the impact of low demand on investment. That is, for each industry-year we calculate

$$\text{Low Demand}_{nt} = \frac{\sum_{nt} \mathbb{1}_{it}(\text{low demand})}{N_{nt}} \tag{2.3}$$

where $\mathbb{1}(\text{low demand})$ is an indicator of whether a firm cited low product demand as a limitation and N_{nt} is the number of sampled firms in that industry. To correct for sampling bias, we calculate weights, θ_{nt}, equal to the percentage of firms in each stratum (as defined by the 2-digit industry codes and employment groups) in the population relative to the survey sample. We define the population as the group of firms reporting to firm census. That is,

$$\theta_{nt} = \frac{n_{st}/N_t}{s_{st}/S_t} \tag{2.4}$$

where n_{st} and s_{st} are the number of firms in each stratum in the population and survey, respectively. N_t is the total number of firms observed in IES each year while S_t is the total number of firms surveyed each year.

Table 33 in the Appendix provides descriptive statistics on the resulting measure. We have 64 industries with an average of 80 firms. On average 73% firms in an industry cite poor sales as a major limitation to investment, with twenty industries having all sampled firms cite poor demand outlook. The lowest proportion occurs in sewage (12 firms sampled) and veterinarians (3 firms sampled) with no firms citing poor sales as a major outlook. In Figure 12 we show that this industry measure is highly correlated with sales growth. The impact of these limitations also varies substantially with size: 70.8% of micro firms cite poor sales as a limitation compared with 57.9% of large firms.

2.3.4 Empirical Strategy

We use the following instrumental variable linear probability model model to estimate the effect of debt frictions and demand on take-up of the 2013 investment tax credit.

$$\begin{aligned} Pr(\text{take up})_i = &\beta^{\text{demand}} \log \hat{S}_i + \sum_{j=1}^{3} \beta_j^{\text{debt}} \text{quartile}_i^j + \\ &\sum_{j=1}^{3} \beta_j^{\text{interact}} \text{quartile}_i^j \times \log \hat{S}_i + \gamma X_i + u_i \end{aligned} \tag{2.5}$$

where i indexes firms and j debt quartiles.

The dependent variable is a dummy that is one if the firm has received a tax credit in either 2013 or 2014. The main explanatory variables are demand and debt burden. $\hat{S}_i$ is the predicted firm sales based on the following first stage:

$$\log \hat{\mathrm{S}}_i = \hat{\alpha}^z Z_i + \sum_{j=1}^{3} \hat{\alpha}_j^{\text{debt}} \text{quartile}_i^j + \hat{\theta} X_i \tag{2.6}$$

where Z_i denotes the instrument (foreign demand or industry share citing low demand). We include the same controls as in the second-stage regression.

We allow for a non-linear effect of debt-earnings ratios by including the upper three quartiles of the combined debt-earnings index (the first quartile is the omitted category). Firms in the fourth quartile have the worst debt-earnings ratios and face the highest debt burden. We also include an interaction term between the quartiles and predicted firm sales. This allows the effect of demand to depend non-linearly on the debt-earnings ratios.

Controls, X_i, include 2012 levels and growth rates so that $X_{ijdb} = \{X_{ijdb,t-1}, \Delta X_{ijdb,t-1}\}$. We control for credit default risk, computed following the methodology in Antunes *et al.* (2016), indicators for insolvency, past default, and negative equity. We also control for past investment trends by including fixed assets growth, TFP, and cash levels. Finally, we include a dummy variable indicating whether a firm paid income tax in 2012 and the level of other investment tax benefits received. Where possible we include firm size category, industry, district, and banking group fixed effects. The banking group fixed effects are defined as the banking group of the main lender of each firm (the bank with the largest share of the firm's loan balance).[9] We include district fixed effects to absorb geographic variation in investment. The firm size classification follows EU criteria which are based on total assets, number of employees, and the amount of sales.[10]

The identification assumption is that our instrument is correlated with firm-level sales only through only its effect on demand for the firm's product.

Intensive Margin We also estimate the effect of demand and debt frictions on the amount of invested conditional on a firm using the tax credit. The instrumental variable specification is as follows:

$$\begin{aligned}\log \text{CAPEX}_i =& \beta^{\text{demand}} \log \hat{S}_i + \sum_{j=1}^{3} \beta_j^{\text{debt}} \text{quartile}_i^j + \\ & \sum_{j=1}^{3} \beta_j^{\text{interact}} \text{quartile}_i^j \times \log \hat{S}_i + \gamma X_i + \epsilon_i\end{aligned} \tag{2.7}$$

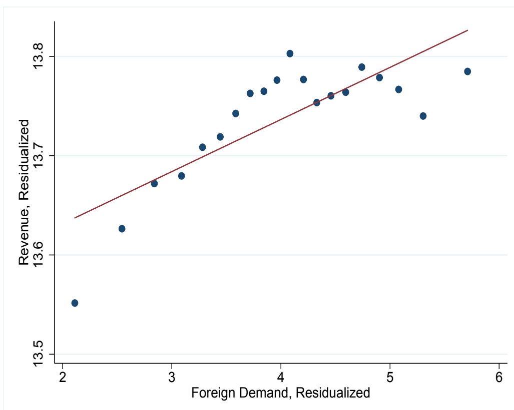
where i and j index firms and debt quartiles. The dependent variable is log investment spending (CAPEX) and we condition on the sample of firms using the extraordinary tax credit. The explanatory variables as in specification 2.6.

[9]We use group level fixed effects rather than bank level fixed effects in order to reduce the number of categories. Our dataset covers 17 banking groups and almost 100 main lenders, so while we may lose some heterogeneity in using the bank groups, the bank level main lender leaves the regression with much fewer observations.

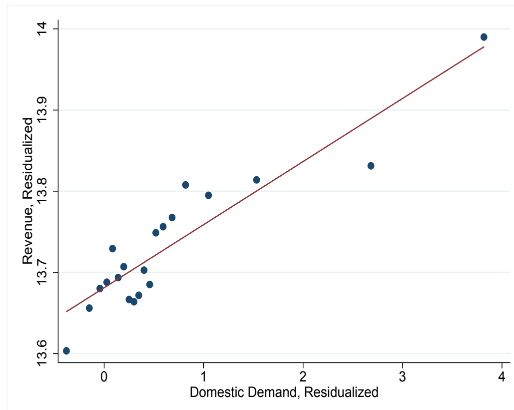
[10]Specifically: micro firms have less than 10 employees and total assets or sales below 2M Euros; small firms have less than 50 employees and total assets or sales below 10M Euros; medium firms have less than 250 employees and total assets below 43M or sales below 50M; and large firms have more than 250 employees or total assets above 43M and sales above 50M.

2.4 Results

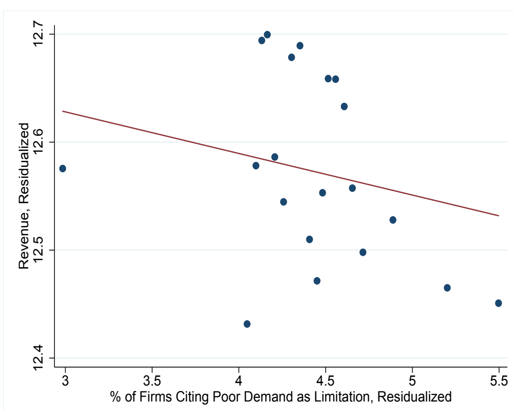
We find large and significant effects of both product demand and debt burden on the take-up of the investment credit as well as the amount invested. The debt-earnings index has a non-linear effect on investment, with the negative effect being concentrated in the most indebted quartile. The effect of demand is mediated by the effect of debt, with the positive effect disappearing for the most indebted quartile.



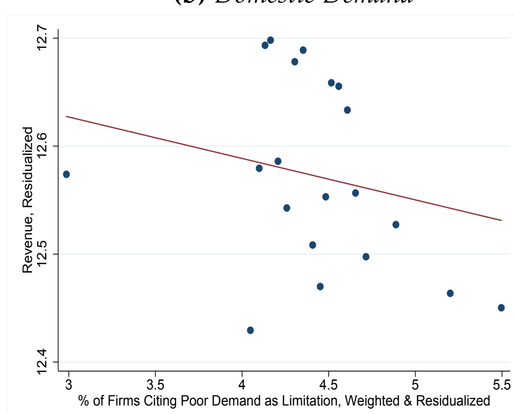
(a) *Foreign Demand*



(b) *Domestic Demand*



(c) *Low Demand, All Firms*



(d) *Low Demand, Firms <200 Workers*

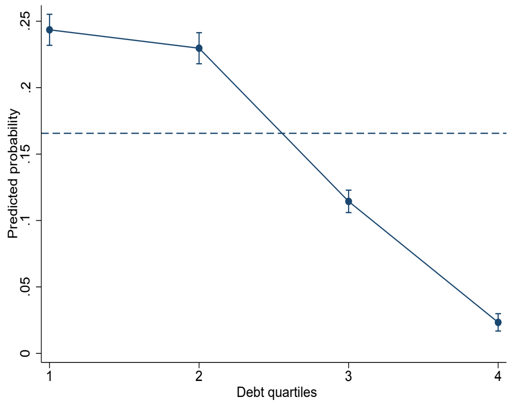
Figure 12: *First Stage for Demand Instruments*

Notes. The figure shows a binned scatterplot of log(sales) and our demand instruments in 2012, after residualizing the controls described in 2.3.4. Instruments are normalized to have unit variance. Panel (a) and (b) show the trade instruments described in equations 2.1 and 2.2.

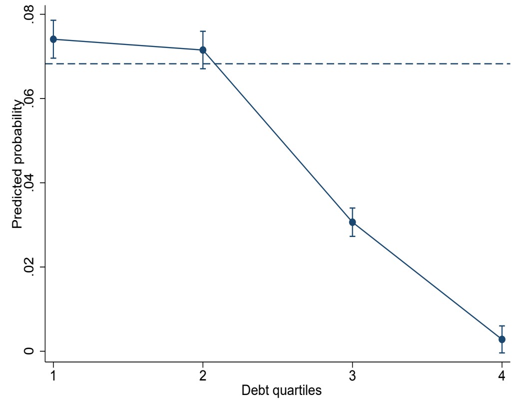
2.4.1 First stage regression

The first-stage regression suggests that the foreign demand instrument is strongly correlated with firm sales. Table 34 presents the correlation of each the instruments with log sales in the cross-section of firms in 2012.[11] A one standard deviation increase in export demand leads to an 6% increase in sales. The survey-based instrument, which we use for robustness, enters with the opposite sign since we are measuring the fraction of firms in an industry that report being constrained by low demand. A standard deviation increase in the fraction of firms that cite demand as a major limitation to investment is associated with a decrease in sales of 2%. Figure 12 depicts the first stage correlations across all years (2010-2012), after residualizing on controls and year fixed effects. The instruments, along with the included controls, explain around 55-60% of variation in log sales. The F-statistics shown in Table 8 are between 57.1 and 74.2 is significantly above the Stock and Yogo (2005) criterion for 5% maximal relative bias.

[11] The instruments have been normalized to have unit variance and we include all controls from the second stage.



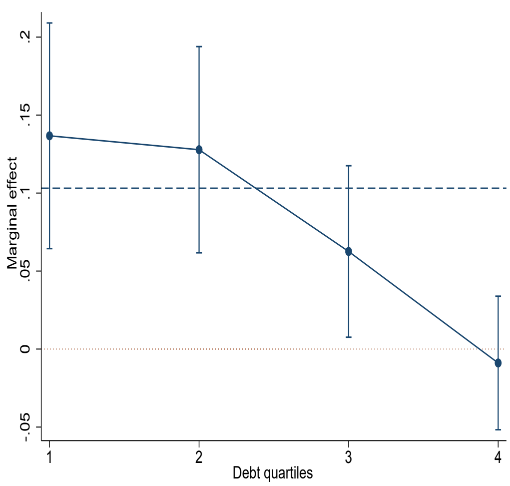
(a) *Exporting Firms*



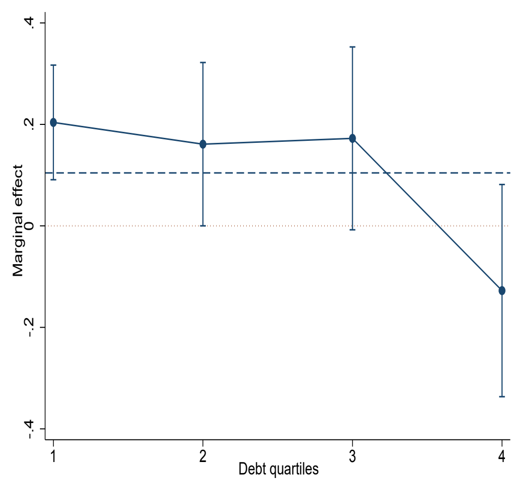
(b) *All Firms*

Figure 13: *Probability of Take-Up, By Debt-Earnings Quartile*

Notes. The figure shows the predicted probability of tax credit take-up for the four debt quartiles. Panel (a) shows all exporting firms using the foreign demand measure as an instrument for demand and panel (b) shows results for all firms using the survey-based demand instrument. The dotted lines show the average predicted probabilities, which are 16.57% and 6.83% respectively.



(a) *Exporting Firms*



(b) *All Firms*

Figure 14: *Marginal Effect of Demand on Take Up, By Debt-Earning Quartile*

Notes. The figure shows the marginal effect of the (instrumented) log sales by debt-earnings quartile. The blue dotted line shows the average marginal effect while the dashed line is at zero. Panel (a) uses the foreign demand instrument in the sample of exporting firms. Panel (b) shows results for the survey-based instrument in the full sample.

2.4.2 Extensive Margin

We find that both product demand and debt burden are significant predictors of tax credit take-up. We find that predicted take-up drops across the debt quartiles. Firms in the lowest debt quartile have average take-up rates of around 20% while firms in the highest debt quartile have average take-up rates of close to 0 (see Figure 13). Importantly, this effect is non-linear. We also find that the effect of the debt-earnings ratios is attenuated in the OLS specification, in which we do not instrument for demand, by a factor of 3.

Demand positively affects take-up. A 10% increase in sales is associated with an 8 percentage points (pp) higher take-up across all firms. However, the impact of demand is mediated by the debt burden as shown in Figure 14. The marginal effect of an increase in sales declines in importance in the higher debt-quartiles. In the highest debt-earnings quartile, demand does not have a statistically significant impact.

Results from measuring demand based on the investment survey in the full sample of firms confirm the non-linear effects found in the sample of exporters. However the effects are smaller in magnitude due to the inclusion of smaller firms which have lower take-up rates than larger firms.

2.4.3 Intensive Margin

The intensive margin broadly confirms the extensive margin results (see Table 11). The intensive margin results are estimated on a much smaller sample since we condition on firms that use the investment tax credit, which provides some challenges. As very few firms in the fourth quartile of the debt-earnings index use the tax credit, we combine the third and fourth quartile for the intensive margin analysis. Moreover, our demand instruments suffer from a weak instrument problem in the smaller sample making inference challenging.

We again find evidence of a non-linear effect of the debt-earnings index on investment. Moving from the lowest to the highest debt group implies a 17% decline in investment. We find that, as in the extensive margin, demand has a substantial and statistically significant impact on the amount invested. A 1% increase in predicted log sales leads to a 1.4% increase

Table 10: *Regression Results: Take-up of Tax Credit*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	Ln(Revenue) instrumented using					
		Trade Measure	Trade Measure	Survey Limitations	Survey Limitations	All	Trade Measure
Sample	All firms	All firms	<200 employees	All firms	<200 employees	All firms	Positive EBITDA
Ln(sales)	2.897***	8.880***	10.008***	10.438***	10.310***	7.870***	10.747***
	[0.086]	[2.751]	[2.642]	[2.857]	[2.879]	[2.380]	[3.175]
2nd Debt Quartile	-0.804***	-1.197	-1.289	0.067	-0.074	-1.065	-0.938
	[0.280]	[0.922]	[0.939]	[3.243]	[3.133]	[1.085]	[0.891]
3rd Debt Quartile	-3.896***	-7.045***	-6.987***	-0.842	-0.854	-4.909***	-5.978***
	[0.243]	[0.863]	[0.868]	[1.614]	[1.556]	[0.935]	[0.966]
4th Debt Quartile	-3.947***	-12.359***	-12.140***	-12.639**	-12.645**	-8.465***	-9.823
	[0.226]	[1.182]	[1.193]	[6.262]	[5.995]	[1.178]	[9.372]
First stage F-statistic	-	57.13	56.49	74.22	74.03	12.12	56.25
Observations	82,173	15,152	14,738	74,701	74,112	14,853	12,584

Notes. Table 10 shows the marginal effects of demand and debt measures on the probability of a firm using the CFEI tax credit. The dependent variable is a dummy indicating whether or not a firm took up the tax credit in 2013 or 2014. Bank group, industry, firm size and district fixed effects are included in all regressions, as well as firm controls for solvency and past investment trends. Robust standard errors are in brackets. In the first column we show results from an OLS regression, which does not instrument for demand. The following columns show marginal effects for results instrumenting for log sales. Instruments normalized to have unit variance. Trade measure refers to the foreign demand based instrument described in the text. Survey Limitations refers to the share of firms that report demand as a major limitation to investment. Quartile variables are dummies indicating a firm is in the 2nd, 3rd, or 4th quartile of our debt-earning index. Effects should be read as the discrete change with respect to the base category, firms in the first quartile (that is, the least-burdened firms). The final column shows results for a sample of firms with strictly positive EBITDA.* $(p < 0.05)$, ** $(p < 0.01)$, *** $(p < 0.001)$

in investment spending. This effect is again attenuated in the OLS specification. There is some evidence that debt and demand interact in the OLS specification. However the IV estimates are too imprecisely estimated to be able to reject the null of a constant effect across the debt quartiles.

2.4.4 Discussion

Our results imply that two of the main explanations for the tepid investment post-crisis - a large private debt burden and low product demand - play an important role in the investment decision of firms. A back-of-the-envelope calculation based on our baseline estimates suggests that if all firms had the same level of demand as the top ten firms in 2012 and 2013, average take-up would have been 11 percentage points higher. However, the impact of debt and demand are not independent. Even a strong pick-up in demand may not

Table 11: *Regression Results: Amount Invested*

	(1)	(3)	(4)	(5)	(6)	(7)
	OLS	Ln(Revenue) instrumented using				
		Trade Measure	Trade Measure	Survey Limitations	Survey Limitations	All
Sample	All Firms	All Firms	<200 employees	All Firms	<200 employees	All Firms
Ln(sales)	0.891***	1.376**	1.179*	-1.480	-1.652	1.326*
	[0.037]	[0.680]	[0.642]	[3.724]	[4.083]	[0.691]
2nd Debt Quartile	0.033	0.043	0.065	-0.034	-0.029	0.034
	[0.052]	[0.080]	[0.084]	[0.127]	[0.129]	[0.113]
3rd & 4th Debt Quartile	-0.077	-0.227*	-0.176	0.591	0.595	-0.070
	[0.073]	[0.116]	[0.120]	[0.881]	[0.915]	[0.161]
First stage F-statistic		10.94	11.53	3.42	3.72	6.12
R-squared	0.537					
Observations	5,325	2,492	2,325	4,850	4,641	2,439

Notes. Table 11 shows the results for the intensive margin regression. The dependent variable is log(capex), with investment spending data supplemented by the tax credit amount if a firm did not report investment spending to the firm census. The regressions are run on the sample of firms who use CFEI tax credit. The third and fourth debt quartiles are combined into a single category here, as very few firms (55) in the fourth quartile use the tax credit. Bank group, industry, firm size and district fixed effects are included in all regressions, as well as firm controls for solvency and past investment trends. Robust standard errors are in brackets.* ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)

affect investment when firms are saddled with a high debt-burden, which is the care for a growing number of Portuguese firms. Moving all firms into the lowest (best) quartile of the debt-earnings index, would have led to an average take-up of 17% instead of 4%. Most of this jump is driven by the fact that moving firms out of the third and fourth debt quartiles has a large effect on take-up rates, reflecting the non-linear impact of debt. These results suggest that the distribution of indebtedness, rather than the absolute stock of debt, matters for correctly quantifying the adverse consequence of high corporate sector debt.

2.5 Conclusion

This paper provides evidence that the large stock of corporate debt plays an important role in explaining subdued investment in Europe since the 2008 financial crisis. We exploit a temporary investment tax credit in Portugal in 2013, which generated large incentives to invest in the second semester of 2013, to show that debt played two important roles in reducing firms' take-up of the tax credit. First, we find that the effect of a firm's debt burden is non-linear with the negative effect being most pronounced after a ‘kink’ point. As firms cross over this kink point, the negative effect of debt on corporate investment is shoots up. Second, the positive effect of demand on investment disappears when the firm has a high debt burden. This implies that the positive effects of an incipient recovery are limited as long as firms remain highly indebted. Both of these results are consistent with theories of debt overhang which predict that under-investment will kick in strongly once a firm crosses a critical debt point. Calibrating a debt overhang model based on our results is a fruitful avenue for future research. Our results also highlight the need for policies that directly address firms’ excessive debt burden. For example, policies to improve the ability of firms to restructure existing debt can potentially have powerful positive effects on investment.

Chapter 3

Chapter 3: What Did 1 Trillion Euros Buy Us?

3.1 Introduction

In 2015, the European Central Bank (ECB) embarked on a quantitative easing (QE) program with the aim of reviving economic growth in the Eurozone. One of the main promised effects was that QE would spur banks to ease funding conditions for European companies and households, which heavily rely on bank credit. Given that the ECB's QE program was significantly larger in scope - relative to the size of the economy - than its counterpart in the US and given the importance of bank financing in Europe, QE promised potentially large effects on the real economy.[1] Nevertheless, there is little empirical evidence on how successful QE has been in stimulating the economy in Europe.

In this paper, we trace the transmission of quantitative easing through the banking sector using comprehensive micro-level data from the Bank of Portugal. We find that

[1]The Expanded Asset Purchase Program (EAPP), which was announced on January 22, 2015, significantly increased the scope of the ECB's purchases of asset-backed securities (ABSPP) and covered bonds (CBPP3), and extended purchases to sovereign bonds (PSPP). Bloomberg calculates that the ECB assets are 39% of GDP while assets of the US Federal Reserve only stand 23% of GDP. Source: https://www.bloomberg.com/news/articles/2017-08-24/central-bank-balance-sheets-are-headed-for-a-great-divergence

quantitative easing has little effect on lending conditions. There is no evidence that banks which benefited more from QE, either due to a valuation gain on their assets or improved conditions to originate securities, pass on these gains to their customers in the form of higher lending quantities or meaningfully lower interest rates. We only find a moderate positive effect on approval rates for new corporate clients. A likely explanation is that uncertainty about new liquidity regulations, introduced around the same time as QE, induced banks to use the proceeds from QE to shore up their liquidity buffers instead of easing funding conditions. Moreover, improved conditions to originate securities such as asset-backed securities (ABS) due to QE may not have been enough to overcome demanding regulatory requirements on the issuance of such securities.

Our identification strategy relies on exploiting cross-sectional heterogeneity across banks to study two transmission channels of QE. Using a difference-in-difference design, we first compare the lending behavior of banks that held relatively more, or relatively fewer, securities that were eligible for purchase by the ECB. To study whether banks responded by originating more asset-backed securities or covered bonds, both of which were purchased by the ECB as part of QE, we compare banks that had originated such securities prior to 2008 versus those that did not originate such securities prior to 2008. The rationale is that banks which originated such securities prior to the collapse of the securitization market following the Lehman failure possess the technology, or capacity to structure ABS and covered bonds, and hence could more easily take advantage of improved origination conditions.

Our empirical strategy addresses two key identification concerns. First, we employ borrower$\times$month fixed effects in order to isolate movements in credit supply from movements in credit demand. Where possible, we add lending relationship-level controls to account for differences not absorbed by the borrower$\times$month fixed effects. Second, our exposure measure only uses information prior to the QE announcement in order to avoid picking up banks' endogenous responses to QE. We confirm that banks in our treatment and control groups show no significant differences in key characteristics such as size, profitability, leverage, and capitalization and exhibit similar lending trends prior to the announcement of

QE.

We investigate two channels through which QE may affect lending conditions: the balance sheet channel and the origination channel. The balance sheet channel works through the valuation gain for banks that hold QE-eligible securities on their balance sheet. The portfolio re-balancing induced by the ECB purchases increases the price of QE-eligible securities.[2] These price increases lead to a valuation gain for banks that hold these securities on their balance sheet. This valuation gain increases the market value of banks' net worth, relaxes banks' funding constraints, and potentially allow banks to pass on the gains to their borrowers (Bernanke and Gertler (1989) and Gertler and Kiyotaki (2011)). We use detailed security-level bank balance sheet data to compute the degree of bank exposure to QE. We define exposure to QE based on the holdings of QE-eligible securities in the month prior to the first QE-related announcement. We divide banks into credit-weighted quartiles based on QE-eligible holdings scaled by total assets and define banks in the highest and lowest quartiles as our respective treatment and control groups.

The second QE transmission channel is the origination channel. The origination channel refers to the improved ability of banks to originate ABS and covered bonds due to QE increasing market liquidity and driving up securities prices. In principle, the improved ability to securitize loans encourages banks to improve access to finance since it allows banks to shift some lending risk off their balance sheets. On the liability side, the ability to originate covered bonds should improve access to stable long-term funding for financial institutions.[3] We define an institution as exposed if it had an ABS or covered bond outstanding prior to the Lehman bankruptcy in 2008. The rationale for this measure is that only a subset

[2]We confirm that there is a positive price impact of QE in Portugal using an event-study regression based on the QE announcement dates. We find that the announcement of QE operations is related to drops in yields between 16 and 93 basis points for eligible Portuguese securities. See Appendix A.

[3]Covered bonds, which are bonds backed by a pool of mostly high-grade mortgages or loans to the public sector, are an important source of long-term funding for financial institutions. They are considered as relatively safe by investors since investors have a preferential claim on the assets in the cover pool in the event of default. The issuance of covered bonds decreases banks' reliance on short-term money market funds. This reduces the maturity mismatch between banks' assets and liabilities and hence improves banks' ability to take on long-maturity assets (i.e. loans).

of financial institutions operating in Portugal have the technology to issue covered bonds and/or ABS.

We find that while banks benefit from valuation gains and improved origination conditions due to QE, these gains largely do not lead to improved funding conditions for borrowers. Banks with larger prior holdings of QE-eligible assets sell eligible assets upon announcement and book sizable trading profits.[4] Banks also originate covered bonds and ABS, some of which are eligible for purchase by the ECB.[5] However, there is no differential increase in loan volumes for existing corporate borrowers nor a meaningful drop in interest rates charged for new corporate loans following the QE announcement. This result holds for both bank-level exposure definitions. We find some positive transmission of the balance sheet channel to the approval rates of new corporate borrowers, in particular borrowers with higher credit risk. These results are robust to including bank, firm-level, relationship, and, where possible, loan-level controls and different saturations with fixed effects.

The lack of transmission of QE through the banking sector may be due to counteracting regulatory pressures faced by banks. In January 2015, concurrent with the announcement of QE, the EU Commission passed additional regulatory measures that forced banks to hold more liquid assets beginning in October 2015.[6] In line with banks shoring up their liquidity buffers, we find that banks which experience a valuation gain due to QE, increase their holdings of liquid assets.[7] Similarly, origination of ABS and covered bonds may have been hindered by regulatory pressures. Following 2008, regulatory requirements for ABS were

[4]Using transaction-level data from the Bank of Portugal, we find that most QE transactions were with foreign investment banks. Given that Portuguese banks booked trading profits immediately following announcement, it is likely that Portuguese banks sold their eligible assets to foreign investment banks following the announcement date. The investment banks then held the assets until the ECB started their purchases a few weeks following the announcement.

[5]As of June 2015, Portuguese banks issued a total of 12.7 EUR billion in covered bonds and ABS following the announcement of CBPP3 and ABSPP on September 4, 2014. Of these, 4.4 EUR billion were eligible for QE (see Table 38 in Appendix B for a list of issuances).

[6]Banks had to satisfy a minimum liquidity coverage ratio of 60% beginning in October 2015. This regulatory announcement was part of wider phase-in of Basel III liquidity requirements.

[7]Butt *et al.* (2015) look at UK banks that receive more deposits to conduct QE transactions but find no evidence of an effect on lending. They attribute this to the 'flightiness' of the deposits.

tightened and banks have to hold large amounts of regulatory capital even for highly rated ABS. Moreover, local regulators reserve the right to refuse the recognition of a transfer of risk as past of an ABS issuance.

Another possibility is that our identification strategy does not identify the channels through which QE affects the real economy in the Eurozone. Our identification strategy only captures transmission channels that have differential effects on financial institutions since we exploit cross-sectional heterogeneity across banks. If QE only works through economy-wide transmission channels that equally affect all banks, our empirical strategy will not be able to identify such effects. For example, QE may work mostly through signaling a monetary policy easing which reduces interest rates and depreciates the exchange rate.[8] Another possibility is that the importance of unconventional monetary policy is highest when banks are capital constrained. There is existing evidence that the least capitalized banks are most responsive to unconventional monetary policy (Drechsler *et al.* (2016)). Crosignani (2017), Acharya *et al.* (2017)). We study a later period when European banks were generally less capital constrained and we do not explicitly compare along the degree of capitalization.

Related Literature Our research contributes to the literature on the bank lending channel of monetary policy (Kashyap and Stein (1994)), specifically to a growing body of literature studying the transmission of unconventional monetary policy to the real economy. Several papers evaluate the lending effects of QE programs by the US Federal Reserve, exploiting differential bank holdings of mortgage-backed securities, similar to our cross-sectional identification strategy. Rodnyansky and Darmouni (2017), Di Maggio *et al.* (2016), Chakraborty *et al.* (2017) and Luck and Zimmermann (2017) find positive effects of QE on lending in the mortgage market, especially for QE3, which is the most similar to the ECB's QE program.[9]

[8]There are general downward trends in loan-level interest across banks, which may point in this direction. However, these trends generally began prior to the QE announcement suggesting that this may due to forces other than QE.

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

Chakraborty *et al.* (2017) find that the increase in credit supply in the mortgage market crowds out commercial and industrial (C&I) lending. Luck and Zimmermann (2017) in contrast find positive effects on C&I lending using more comprehensive loan data. Similar to Chakraborty *et al.* (2017), we investigate not only the balance sheet channel but also the origination channel of QE. However, unlike Chakraborty *et al.* (2017), we do not find evidence of a transmission to lending conditions via the origination channel. Beyond the US, Morais *et al.* (2015) study the international transmission of quantitative easing through foreign banks.

Our results suggest that the ECB's QE may have been less effective than US QE programs at stimulating lending. A potential explanation is the absence of a concurrent tightening of liquidity requirements in the US. Another potential explanation is that the ECB purchases are heavily concentrated on sovereign bonds relative to ABS/covered bonds. Cross-sectional evidence from the US suggests that purchases of government debt (QE2) appear to have had no effects on lending conditions, while purchases of US mortgage-backed securities (QE1, QE3) had a positive lending effects (Rodnyansky and Darmouni (2017), Luck and Zimmermann (2017)).

We also contribute to a growing literature studying unconventional policy measures in Europe. Similar to our research design, Acharya *et al.* (2017) exploit variation in banks' sovereign bond holdings to study the effect of the announcement of the ECB's Outright Monetary Transactions (OMT) on lending conditions. However, as in Rodnyansky and Darmouni (2017), their analysis is limited to the syndicated loan market, which represents a very special segment of lending markets and does not speak to the effect of unconventional monetary policy on small and medium-sized enterprises. Accornero *et al.* (2017) study the ECB liquidity injections (LTRO) in 2011-2012 and find some evidence in Italy that these interventions stabilized credit supply by banks who suffered a withdrawal of liquid funding sources.

We confirm the finding that QE is successful in moving bond yields, at least in the short-run, using an event study design. Gagnon *et al.* (2011) and Krishnamurthy and

Vissing-Jorgensen (2011) examine the announcement effects of long-term asset purchases by the US Federal Reserve. Similarly, Krishnamurthy *et al.* (2013) use an event study to evaluate the two ECB unconventional policies prior to QE that involve government bond purchases.[10] Falagiarda and Reitz (2015) also look at the effects of ECB unconventional policy announcements on sovereign spreads. Beirne *et al.* (2011) estimate the effect of the ECB's first two covered bond purchase programs on covered bond spreads and, most recently, De Santis (2016) and Koijen *et al.* (2016) estimate the effect of the ECB's QE program on asset prices.

Our paper also contributes to the literature on the relationship between monetary policy and bank risk-taking. We find that the reach-for-yield behavior that characterizes an easing of conventional monetary policy (Jimenez *et al.* (2014)) potentially also applies to unconventional monetary policy. Kurtzman *et al.* (2017) find evidence in the US that the Federal Reserve's QE programs relaxed lending standards. This is in line with evidence documenting how undercapitalized European banks increase their exposure to sovereign bonds (e.g. Drechsler *et al.* (2016)).

3.2 Background and Data

In this section, I first provide background on the ECB's asset purchase programs. I then describe our data and describe the two transmission channels through the banking sector I study in this paper.

3.2.1 Background on QE in Europe

The Expanded Asset Purchase Program (EAPP) is the first quantitative easing program undertaken by the European Central Bank. The ECB's QE program, which was announced on January 20, 2015, significantly increased the scope of the ECB's purchases of asset-backed securities (ABSPP) and covered bonds (CBPP3), and extended purchases to sovereign bonds

[10] The Securities Purchase Program (SMP) and the Outright Monetary Transactions (OMT) program.

(PSPP).

The ECB began its first smaller-scale asset purchase program in 2009. The covered bond purchase program CBPP1 was in operation between July 2009 and June 2010. This program was succeeded by a second covered bond purchase program, CBPP2, in 2011. However, the purchase volumes with EUR 60 billion and EUR 16.4 billion respectively were small compared to the EUR 143 billion of covered bonds purchased under the ECB's QE program, as of January 2016.[11] The first sovereign bond purchase program, the Securities Market Program (SMP), was announced in 2010 and targeted sovereign bonds of distressed Eurozone members. The SMP was superseded in 2012 by the Outright Monetary Transactions (OMT) program which allows the ECB to buy government debt of countries that are part of an official financial assistance program. To date, the OMT has not been used.

In September 2014, the ECB announced its third covered bond purchase program CBPP3 and added a purchase program for asset-backed securities (ABSPP). With the announcement of the ECB's QE program in January 2015, the ECB significantly extended the scope of CPPP3 and ABSPP and complemented the two programs with a public sector bond purchase program (PSPP). In March 2016, the monthly purchase volume was expanded to EUR 80 bn.[12] Figure 15 shows the total purchases under the ECB's QE program. The bulk of the EUR 60 bn monthly purchase volume in 2015 is concentrated in sovereign bonds (PSPP). ABSPP is the smallest of the three purchase programs reflecting the lack of large, liquid ABS markets in Europe. ABS, covered bonds and sovereign bonds are eligible for purchase by the ECB as long as they are denominated in EUR, issued by a financial institution (or sovereign) that is resident in the Eurozone, and eligible for collateral at the ECB. As in previous programs, the purchases, which are mostly conducted by national central banks, can take place both in primary and secondary markets with purchases being concentrated in secondary markets for all three programs.

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[11]See `https://www.ecb.europa.eu/mopo/implement/omt/html/index.en.html` for updated numbers. Accessed 07.01.2016

[12]See Table 36 in Appendix B for exact announcement and implementation dates.

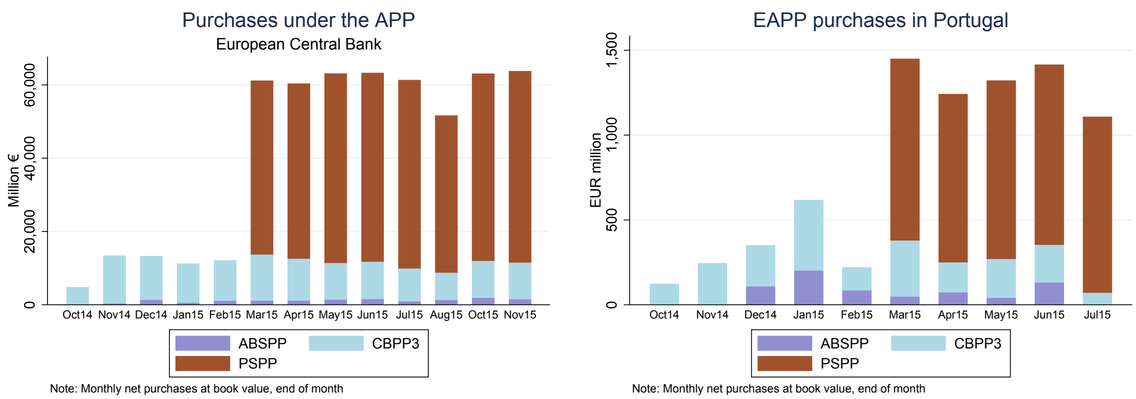


Figure 15: *Purchases Through the European QE Program*

Notes. Panel a displays the volume of asset purchases conducted from October 2014 to November 2015 for all members of the Euro Area. Panel b displays the volume of asset purchases in Portugal under EAPP from October 2014 to July 2015.

Portugal provides an excellent laboratory to study the effect of QE on lending conditions since purchases were large relative to market size and low liquidity markets are likely to register a larger price impact than larger and more liquid markets. I provide evidence of the yield impact in Appendix A. Figure 15 shows total purchases in Portugal. The allocation of purchases to Portugal follows the 2.5% capital key at the ECB.

3.2.2 Data

Credit Register and Firm Data

I use the loan level Central Credit Register maintained by the Banco de Portugal. Our sample period throughout the analysis is January 2013 to August 2015. With a loan threshold of EUR 50, the Credit Register contains almost the universe of outstanding corporate loans. The Credit Register also contains codes that identify whether a loan is securitized, or whether the loan is part of the cover pool of a covered bond. This allows us to identify the loans that banks use to issue an ABS or a covered bond.

I also draw on a database on credit report downloads. Banks can request information on a potential borrower from the Central Credit Register. While the cost of this request is zero, consultations of the Central Credit Register are usually conducted for a serious inquiry of a potential new corporate client. This allows us to construct a measure of the likelihood that a credit report download results in a new lending relationship. This likelihood provides a reliable measure of loan originations in the economy.

I obtain data on firm interest rates from a database on new lending operations that contains information on the amount, maturity, interest rate, and collateral of new loans. This new operations data was made a mandatory reporting requirement for the eight largest Portuguese commercial banks in June 2012. In January 2015, the reporting scope was extended to all banks. The banks that start reporting after January 2015 however represent on average only 13.9% of new lending operations.

Finally, I match each firm loan to information from the firm's financial statements from the Portuguese Firm Register (Informação Empresarial Simplificada) which allows us to calculate a time-varying credit risk measure.

Bank Data

I draw on two data sources to compute banks' exposure to the ECB's asset purchases. First, I use supervisory data for balance sheet and profit and loss information for financial institutions operating in Portugal. Our sample of financial institutions contains credit granting financial institutions that both report to the Central Credit Register and to the Portuguese financial supervisor. This includes foreign bank subsidiaries operating in Portugal. I have a total of 63 institutions. Second, for about half of this sample of banks I have ISIN-level security holdings from the SIET database (*Estatìsticas de Emissões de Títulos*). The banks that report in SIET represent 95.8% of the total credit volume in the economy. Second, I use transaction-level data on all QE purchases conducted by the Bank of Portugal on behalf of the European Central Bank. This data includes the date, volume, and counterparty of each QE transaction in Portugal.

3.2.3 Transmission Channels Through the Banking Sector

Our identification strategy relies on the differential impact of QE on financial institutions and can hence only capture channels that have the potential for a differential effect on banks' lending behavior. There are two such mechanisms: the balance sheet channel and the origination channel. Our identification strategy cannot capture economy-wide transmission channels, such as the central bank signaling more accommodative monetary policy which potentially lowers interest rates and depreciates the exchange rate.

Balance Sheet Channel

I confirm that the portfolio re-balancing induced by the ECB's asset purchases increases the price of QE-eligible securities. I use an event-study regression based on the QE announcement dates (see Appendix A). I find that the announcement of QE leads to a drop in yields between 16 and 93 basis points for eligible Portuguese securities. Figure 39 in Appendix A illustrates the evolution of yield spreads of Portuguese government bonds, covered bonds, and ABS during the announcement and implementation of QE.[13]

These price increases lead to a valuation gain for the banks that hold QE-eligible securities on their balance sheet. This valuation gain increases the market value of banks' net worth, relaxes banks' funding constraints and potentially allows banks to increase their lending (Bernanke and Gertler (1989) and Gertler and Kiyotaki (2011)). The view of QE as stealth recapitalization of the banking sector has also featured in theoretical work by Brunnermeier and Sannikov (2016). If banks choose to sell securities on their balance sheet, they will realize this valuation gain as an increase in their trading income. The additional profit can add to bank net worth via retained earnings, or to a bank's liquidity position if it is held in liquid assets. I provide more details on the valuation gains of banks in section 3.

[13]Unfortunately, it is not possible to identify the yield impact over the course of the purchases which means it is not possible to calculate the total realized valuation gain for each financial institution.

Origination Channel

The origination channel refers to the increased incentives to originate ABS and covered bonds given improved issuance conditions such as higher market liquidity and securities prices. Covered bonds, which are bonds backed by a pool of mostly high-grade mortgages or loans to the public sector, are an important source of long-term funding for financial institutions in Europe. They are considered as relatively safe by investors since investors have a preferential claim on the assets in the cover pool in the event of default. The issuance of covered bonds decreases banks' reliance on short-term money market funds. This reduces the maturity mismatch between banks' assets and liabilities and hence improves banks' ability to take on long-maturity assets (i.e. loans). On the asset side, the ability to securitize lending also encourages banks to improve access to finance since it allows banks to shift some of the lending risk off their balance sheets. As of June 2015, banks issued a total of 12.7 EUR billion in covered bonds and ABS following the announcement of CBPP3 and ABSPP on September 4, 2014. Of these, 4.4 EUR billion were eligible for purchase by the ECB.

3.3 Effect on Credit Supply

In this section, we investigate the effect of the ECB's QE program on credit supply through the valuation and origination channels. We first define the measure of bank exposure to QE for each of the two channels, then outline our empirical specification and present results. We discuss regulatory policy changes that coincided with QE that may account for our finding of a very limited pass-through of the ECB's QE program to lending conditions

3.3.1 Exposure Definition

Balance Sheet Channel We define exposure through the balance sheet channel based on banks' holdings of QE-eligible assets in the month prior to the first QE-related announcement (September 2014). We divide banks into credit-weighted quartiles based on QE-eligible holdings scaled by total assets and define banks in the highest and lowest quartiles as our respective treatment and control groups. The credit-weighting ensures that each quartile represents roughly 25% of total credit in the economy. Our sample consists of 40 non-exposed banks and 16 exposed banks. For almost half our bank sample, for which we have security-level balance sheet data, we construct a direct measure of balance sheet exposure using holdings of QE-eligible assets.[14] The main criteria is that the security be of the correct asset class and eligible for collateral at the ECB. We obtain the list of marketable securities that are eligible for collateral from the ECB website at the monthly frequency.[15] We then check that the securities listed at the ECB also fulfill the additional criteria for QE eligibility, namely that the security be denominated in Euro and is issued in the Eurozone.

For the remaining banks in our sample, which represent less than 5% of total credit in the economy, we use holdings of Eurozone sovereign bonds from their monthly balance sheet information as a proxy for total QE-eligible security holdings. Holdings of covered bonds and ABS are only 26% of total QE-eligible holdings for the banks where the information is available. However, these non-sovereign bond holdings are driven almost entirely by banks holding covered bonds that they themselves issued. Given that banks that issue covered bonds (and ABS) report in the security-level balance sheet database, sovereign bond holdings are good proxy for overall QE exposure for the remaining banks.

The valuation gain only applies to securities held in the trading book of the bank's balance sheet. Securities in the trading book, in contrast with securities held in the banking book, are mark to market and as a result their value fluctuates with QE-induced changes in the market price. Moreover, securities in the trading book, as the name suggests, are held for the purpose of trading while securities held in the banking book are intended to be held to maturity. This suggests that realized gains are based on the sale of securities in the

[14] In order to determine eligibility, we apply the definition set out by the ECB in decicions 2014/45 (including annex 1 and annex 2) and 2015/774. The relevant documents can be found at: PSPP `https://www.ecb.europa.eu/ecb/legal/pdf/oj_jol_2015_121_r_0007_en_txt.pdf`. ABSPP `https://www.ecb.europa.eu/press/pr/date/2014/html/pr141002_1_Annex_1.pdf?c4144e9908c29df066a053246f81d1ff`. CBPP `https://www.ecb.europa.eu/press/pr/date/2014/html/pr141002_1_Annex_2.pdf?0ba2a520b8a2b7ad8ff6bfb99333ba2`

[15] The list is available at `https://www.ecb.europa.eu/paym/coll/assets/html/index.en.html`

trading book. We nevertheless base our exposure measure on the sum of banking book and trading book exposures since our data does not allow us to precisely determine the fraction held in the trading book. Based on data from the Portuguese Banking Association and annual reports of the major Portuguese banks, only few banks have any positive banking book securities holdings. Based on publicly available data, on average 83% of the security portfolio is held in the trading book (see Table 12) Lastly, banks face no restrictions on the transfer of securities from the banking to the trading book in order to realize valuation gains. Based on its publicly available annual reports Banco Comercial Portugues for example transferred about 75% of its banking book to the trading book at the beginning of 2015.

The cut-off date for the exposure definition is August 2014, a month before the announcement of ABSPP and CBPP3 and four months before the announcement of QE. We run a news search for articles related to the asset purchase programs and do not find evidence of coverage prior to the end of August 2014.[16] Moreover, we find evidence of a significant announcement effect on yields both in September 2014 (for ABSPP and CBPP3) and in January 2015 (for PSPP) which is further evidence that the purchase programs were not anticipated prior to announcement (see Appendix A). Table 37 in Appendix B shows that most banks get assigned the same quartile regardless of the exact month we choose in the period prior to the announcement of CBPP3 and ABSPP. This is particulary true of the non-exposed banks suggesting that these banks are characterized by a different business model that features a smaller securities portfolio. In Figure 16, we show that the average eligible security shares are stable in the months leading up to QE in all four quartiles.

Figure 17 shows the distribution of QE holdings as a share of total assets in August 2014. The histogram shows considerable dispersion with a minimum of zero and a maximum of over 20% of assets. Table 12 shows that the average shares of eligible assets are 11% in the exposed group (or highest quartile) and 4% in the non-exposed group (or lowest quartile).

[16] The first mention is in the FT on the 25th of August in a blog by Gavin Davies entitled "Draghi steals the show at Jackson Hole".

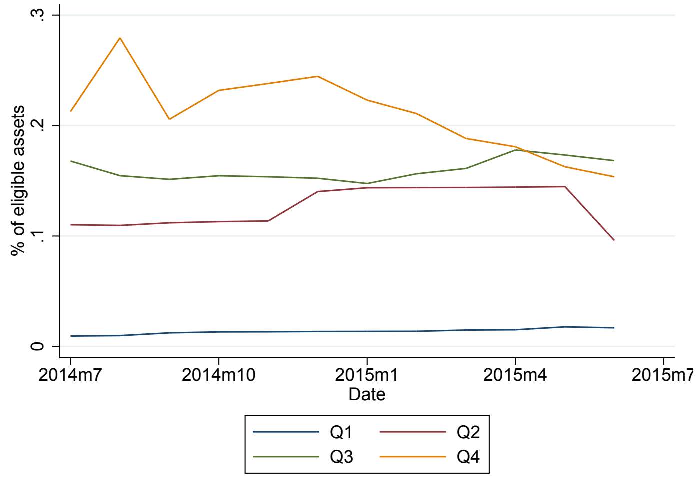


Figure 16: *Evolution of QE-Eligible Securities Holdings Over Time*

Notes. The figure displays the average share of eligible QE securities of total assets in each quartile over time. Quartiles are defined in August 2014.

Balance Table 12 provides balance tests for the two groups of banks. Overall, the balance checks shows that the treatment and control groups are very similar — with the exception of their holdings of QE-eligible securities. This lends credibility to our identifying assumption that the only factor driving the differential behavior of these two groups is their differential exposure to QE. There are no significant differences between exposed and non-exposed groups in size, profitability and leverage. Exposed banks are on average less capitalized but this difference is not statistically significant. By construction, there are large and significant differences in QE-eligible securities holdings. The large and significant difference in overall securities holdings reflects the fact that QE-eligible securities represent a large share of the overall securities portfolio for exposed banks. Exposed banks rely more on household deposits while non-exposed banks have more firm deposits as a percentage of assets. Non-exposed banks tap slightly more into the liquidity provision by the ECB as part of the TLTRO and LTRO programs. We therefore control for LTRO take-up in all regressions. Profitability is very low for both groups reflecting low profitability of Portuguese banks (and the financial sector) more generally. For both groups, credit to household and firms represents an important balance sheet item.

Table 12: *Descriptive Statistics for Exposed and Non-Exposed Banks*

	Exposed	Non-exposed	Normalized difference
Size (EUR M)	56,031	42,021	0.45*
Tier 1 Ratio (units)	10.48	14.41	-0.65
Leverage (units)	87.67	82.76	0.74
Net Income (% of Assets)	-0.38	0.06	-0.68
Credit to Households (% of Assets)	25.86	23.97	0.14*
Credit to Firms (% of Assets)	18.75	26.12	-0.66**
Securities (% of Assets)	28.13	14.43	1.59***
Eligible Securities (% of Assets)	11.07	4.21	1.62***
Securities in trading book (% of Assets)	0.86	0.97	-0.81
Share of ABS/covered bonds (% of Eligible Assets)	0.52	0.62	-0.38
LTRO/TRLTRO (% of Assets)	3.60	4.80	-0.17**
Households' Deposits (% of Assets)	20.28	9.15	1.60***
Firms' Deposits (% of Assets)	18.75	26.12	-0.66**
Interest rate (%)	9.93	7.91	0.73
Acceptance rate (% of consultations)	1.17	1.00	0.21
N	16	40	

Notes. The table shows means and standardized mean differences for exposed and non-exposed banks in August 2014. Exposed and control groups are defined as the highest and lowest (credit-weighted) quartiles of the pre-QE holdings of eligible securities as a share of assets. Share of ABS/covered bonds refers to share of those assets relative to all QE-eligible securities on the bank's balance sheet. The significance stars refer to pairwise t-tests on the mean difference. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

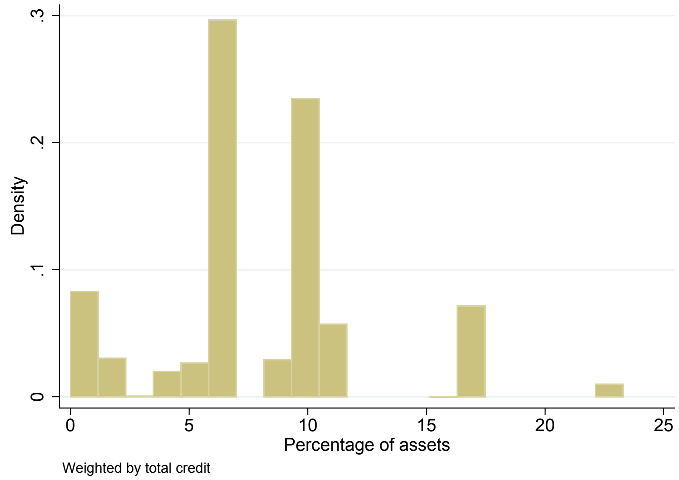


Figure 17: *Histogram of Balance Sheet Exposure*

Notes. The figure displays the share of eligible QE securities as a fraction of total assets. The distribution is weighted by total bank credit volume. Securities are measured in book values.

Empirical Specification

We employ a difference-in-differences (DiD) strategy to identify the effect of QE on lending conditions. We look at the following three main outcomes: (1) The effect on the lending supply to existing customers (intensive margin), (2) the effect on the lending supply to new customers (extensive margin), and (3) the effect on interest rates on new corporate loans.[17]

Intensive Margin Our baseline specification is

$$\log \text{credit}_{ibt} = \sum_{k=-11,5} \beta_k \left(\text{exposed}_b \times \text{month}_k\right) + \mathbf{X}_{ibt}\gamma + \mu_b + \theta_{it} + \epsilon_{ibt}, \tag{3.1}$$

where the dependent variable is the log credit volume at firm i and bank b in month t.[18] The set of leads and lags around the announcement date is interacted with the bank

[17] We are currently in the process of extending the results to the universe of household loans in Portugal.

[18] Loan volume includes regular, potential, overdue and renegotiated credit. We exclude written off credit since this is credit that the bank no longer expects to recover. The reason for combining the remaining categories is that classifications of regular credit into overdue or renegotiated credit can induce movements of regular credit that are unrelated to movements in the total credit volume of the firm. Similarly, the drawing down of credit lines leads to a reduction in potential credit and an increase of regular credit. Such movements do not reflect the granting of additional credit but merely the reallocation of existing resources. Abstracting from these

exposure dummy $\exp_b$, defined as belonging to the highest quartile of the credit-weighted QE-eligible securities to total assets distribution. The set of β_k coefficients capture the differential evolution in lending between exposed and non-exposed banks before and after the announcement. The sample ranges from December 2013 to August 2015. We normalize August 2014, the month prior to the first QE-related announcement, to zero.

Our baseline specification includes bank fixed effects to absorb any time-invariant heterogeneity across banks. In addition, we include measures of banks' (time-varying) funding conditions. Since the ECB continued its liquidity supply to banks via the LTRO and TLRO operations concurrently with the QE purchases, we include the bank-level LTRO and TLTRO share as a control variable. We cluster standard errors at the bank level.

We include firm$\times$month fixed effects that absorb any *time-varying* changes in credit demand at the firm-level in order to address concerns that changes in observed credit growth are driven by firm-level credit demand rather than credit supply.[19] As long as lending trends at both groups of banks would have evolved in the same way absent the ECB's QE program, the β_k coefficients identify the causal effect of QE on lending volumes.

We also include the following firm-bank level controls to account for changes in the type of lending relationship over time: The default status of the firm[20], the length of the lending relationship and flags for restructured credit, written-off credit and the existence of a credit line for a firm-bank pair.

Extensive Margin For the extensive margin regressions, the outcome variable is the probability of a loan application being approved. We run the following linear probability specification

movements and only focusing on regular credit could lead to misleading results. Any increase in the credit volume will be due to an increase in either in regular credit or in potential credit, which is the effect we want to capture.

[19] An important feature of the data is that non-financial firms have multiple lending relationships. On average 54% of the firms in our sample have multiple banking relationship with the average number of lenders being 1.9 (median of 2).

[20] We define a loan as being in default when the loan has been overdue for more than three consecutive months.

$$\text{loan approved}_{ibt} = \sum_{k=-11,5} \beta^a_k \left(\text{exposed}_b \times \text{month}_k\right) + \mathbf{X}_{ibt}\gamma \\ + \mu_b + \theta_i + \varphi_t + \epsilon_{ibt}. \tag{3.2}$$

Given the nature of the credit report data, we can only include firm, bank and time fixed effects as well as time since application and the share of TLTRO/LTRO as additional controls. Firm×time fixed effects would require a firm repeatedly making loan applications to different banks in the same month, which is an excessively strong requirement of the data.

In order to define the probability of a successful loan application, we draw on a database that records all credit report downloads a bank makes regarding potential clients. The dependent variable equals 1 if the credit report inquiry made in month t by bank b about firm i is successful and we see a credit relationship between t and $t + 12$ and equals 0 otherwise. Figure 40 in Appendix B shows that virtually all successful loan applications get their approval within the first twelve months, with the majority getting approval within three months.

Interest Rates Our final outcome are interest rates on new corporate loans:

$$r_{jibt} = \sum_{k=-11,5} \beta^r_k \left(\text{exposed}_b \times \text{month}_k\right) + \mathbf{W}_{jibt}\gamma^r + \mathbf{X}_{ibt}\gamma + \mu_b + \theta_{it} + \epsilon_{ijbt}. \tag{3.3}$$

We include a wide range of loan-level characteristics — the size of the loan, a dummy for collateral, maturity and type of loan product. In addition, we control for the same firm-bank level variables as in the intensive margin specification. We use the same fixed effect specifications as in the intensive margin case.

3.3.2 Results

Quantities The results suggest that the ECB's QE program had little to no effect on lending quantities to existing corporate clients through the valuation channel. Banks exposed to QE via large prior holdings of eligible assets display no differential trends in their credit supply to existing customers following the two QE announcement dates we study (see Figure 18). Loan quantities display a general downward trend over this period, potentially reflecting the private sector deleveraging in Portugal during this period. The dynamic specification suggests no differential pre-trends in lending volumes for exposed and non-exposed banks lending credibility to our DiD design. In Appendix B Table 39, we estimate the non-dynamic version of the DiD specification in Figure 18 to confirm that there is no statistically significant change in credit supply following the QE announcement.

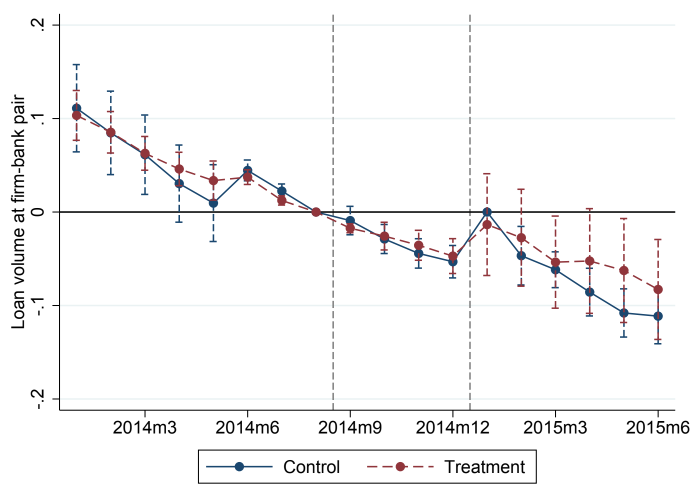


Figure 18: *Dynamic DiD: Intensive Margin*

Notes. The figure displays results from the intensive margin difference-in-difference specification 3.1 for the balance sheet sheet channel. The dependent variable is the log loan volume in a given firm-bank pair. The specification includes dummies for each month as well as monthly dummies interacted with the bank-level exposure indicator. A bank is defined as exposed if it belongs to the highest credit-weighted quartile of QE-eligible securities as a share of assets in August 2014. The regression includes firm$\times$month fixed effects as well as relationship and bank-level controls. The two vertical lines indicate the two QE-related announcement dates. Standard errors are clustered at the bank-level.

Interest rates For interest rates, there is also little evidence that QE had a measurable impact through the valuation channel. Figure 19 shows that there is no differential effect following the first announcement date and only a small and very short-lived drop in interest rates charged on new corporate loans at exposed banks following the second announcement. In Appendix B Table 40, we estimate that the average drop in interest rate in the six months following the January 2015 QE announcement is only about 40 basis points. This effect is not statistically significant across a wide range of specification.

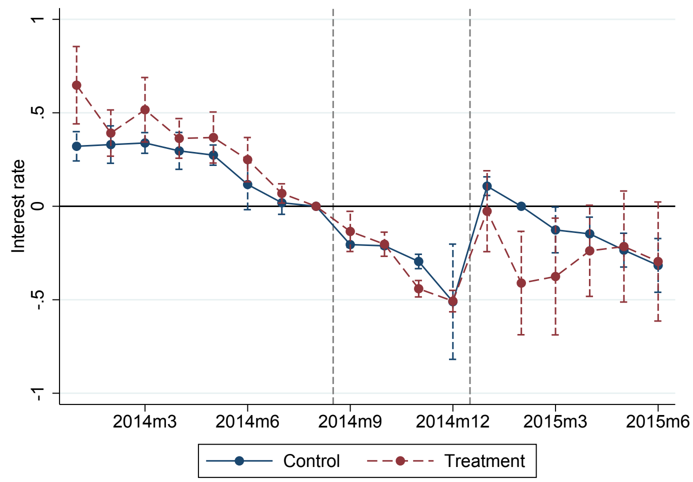


Figure 19: *Dynamic DiD: Interest Rates*

Notes. The figure displays results from the intensive margin difference-in-difference specification 3.3 for the balance sheet channel. The dependent variable is the interest rate on a new loan. The coefficients represent basis points. The specification includes dummies for each month as well as monthly dummies interacted with the bank-level exposure indicator. A bank is defined as exposed if it belongs to the highest credit-weighted quartile of QE-eligible securities as a share of assets in August 2014. The regression includes firm×month fixed effects as well as loan-level, firm-bank level and bank-level controls. The two vertical lines indicate the two QE-related announcement dates. Standard errors are clustered at the bank-level.

New Customers Loan approval rates to new clients exhibit differential trends at exposed and non-exposed banks following the announcement of the ECB's QE program. Figure 20 shows a positive evolution in loan probabilities for both groups of banks that begins following the announcement of the covered bond and ABS purchase programs in September 2014. The treatment group shows strongly significant and persistently higher loan approval rates following QE. There are no statistically significant differences in loan approval probabilities between the two groups in the months prior to announcement lending credibility to our identification assumption of parallel trends prior to the QE announcements. These results are estimated in a specification that includes firm fixed effects but not firm×month fixed effects since there are not sufficiently many firms that apply to exposed and non-exposed banks in the same month. It is hence possible that some of the effects may be driven by an increase in credit demand at firms that coincides with the announcement dates. We do not find evidence of any such confounding demand shocks when comparing results with and without firm$\times$month fixed effects in the specification for existing clients, but it is possible that credit demand patterns differ among existing and new customers of the bank.

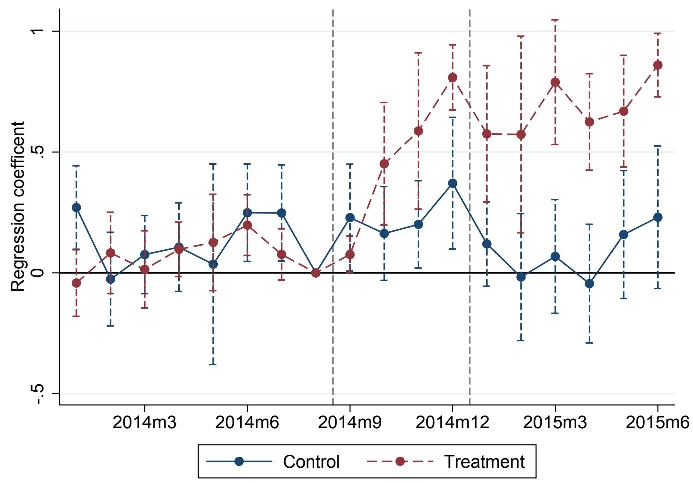


Figure 20: *Dynamic DiD: Extensive Margin*

Notes. The figure displays results from the extensive margin specification 3.2 for the balance sheet sheet channel. The dependent variable is a dummy whether or not a bank approved a firm's loan application in a given month. The specification includes monthly dummies as well as monthly dummies interacted with a bank exposure indicator. Exposure is defined as belonging to the highest credit-weighted quartile of QE-eligible securities as a share of assets in August 2014 (balance sheet channel). The regression includes firm fixed effects, controls for the length of the approval process, as well as bank-level controls. The two vertical lines indicate the two QE-related announcement dates. Standard errors are clustered at the bank-level.

We also run a non-dynamic version of specification 3.2 to estimate the cumulative pass-through into loan approval rates. The probability of a loan approval is 20 p.p. higher at exposed banks following the QE announcement which is both economically and statistically highly significant (see first column of Table 13). When controlling for a measure of credit risk, we find that the effect drops in statistical signifiance and a high credit risk measure, that is a high risk measure, reduces the likelihood of credit approval by 7 p.p. as expected. We use predicted default risk based on the credit risk prediction model of Antunes *et al.* (2016) that uses firm observables. In column (3) however, we interact the credit risk measure with the Exp $\times$ Post coefficient and find evidence consistent with a risk-shifting effect: A one unit increase in the credit risk measure of the applicant (relative to the mean) leads to a 6 p.p. increase in the likelihood of approval at an exposed bank relative to a non-exposed bank in the post-QE period. Given an average monthly loan approval rate of 2.5%, this magnitude is economically very significant. Comparing firms that obtain credit relationships at exposed banks before and after QE, firms that obtain credit at exposed banks after QE are younger, more levered again have a higher share of missing information but are similar on all other dimensions (see Table 14). This evidence suggests that QE reinforced a shift toward 'riskier' firms based on observable financial characteristics.

Table 13: *Balance Sheet Channel: Extensive Margin*

	1	2	3	4
Exp × Post	0.008***	0.205***	0.002*	0.000
	(0.002)	(0.024)	(0.001)	(0.001)
Risk measure			-0.068***	-0.076***
			(0.016)	(0.017)
Exp × Post ×				0.060***
risk measure				(0.021)
Observations	2,684,082	452,126	1,886,822	1,886,822
Bank FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Borrower FE	No	Yes	No	No
Borrower×Post FE	No	No	No	No
Borrower×Date FE	No	No	No	No

Notes. Results from difference-in-differences regression. The dependent variable is the likelihood a corporate loan is approved. Exposed and control groups are defined as the highest and lowest (credit-weighted) quartiles of the pre-QE holdings of eligible securities as a share of assets. The post period begins in January 2015, the month of the ECB's official QE announcement, and end ins June 2015. Standard errors are clustered at the bank-level. The risk measure refers to predicted default risk based on the credit risk prediction model of Antunes *et al.* (2016) that uses firm observables. Control variables (see text) are omitted to conserve space. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

3.3.3 Origination Channel

Exposure Definition

We define an institution as exposed via the origination channel if it had an ABS or covered bond outstanding prior to the Lehman bankruptcy in 2008. The rationale for this measure is

Table 14: *Descriptive Statistics for Firms at Exposed Banks Before and After QE*

	Exposed after QE	Exposed pre QE	Normalized difference
Firm age (years)	12.255	14.784	-0.213
Turnover (EUR m)	1.7	1.7	-0.004
Revenue growth (%)	0.274	0.177	0.150
ROA (%)	-0.014	-0.025	0.052
Intangible assets (%)	0.050	0.043	0.042
Share missings (%)	0.179	0.143	0.299
Pr. default (%)	0.032	0.027	0.152
Leverage (%)	0.605	0.587	0.070
Overdue (%)	0.052	0.061	-0.039
N	6,408	6,735	.

Notes. The table shows average firm characteristics of firms that form new lending relationships with exposed banks before and after the QE announcement as well as the normalized difference between means.

that only a subset of financial institutions operating in Portugal have the technology to issue covered bonds and/or ABS. For example, a credit institution that has issued a covered bond has the ongoing obligation to maintain sufficient assets in the cover pool, to monitor the assets' credit quality, to maintain the correct amount of over-collateralization and to keep up-to-date valuations of the loans' underlying collateral. Moreover, the credit institution needs access to the technology to structure and place the security. We choose the 2008 cut-off because the Lehman bankruptcy led to a widespread collapse of the securitized asset market in Europe and many banks stopped issuing ABS and covered bonds post-2008. Hence issuance post-2008 is not a good proxy of whether a credit institutions possesses the issuance technology. Only 12 (out of 63) institutions in our sample have outstanding ABS or covered bonds before 2008. Of these, 7 institutions issue eligible ABS and/or covered bonds following the announcement of CBPP3 and ABSPP in September 2014. One institution which did not have covered bonds outstanding prior to 2008 issues an eligible covered bond in September 2015. These banks are spread across the balance sheet exposure quartiles: 3 banks are non-exposed according to the balance sheet measure, 4 banks are exposed according to the balance sheet measure and the remaining banks are neither exposed or non-exposed according to the balance sheet measure. Unlike the exposure measure based on securities holdings where we achieve good balance in bank characteristics, the origination exposure mostly captures larger banks.

Specification and Results

We re-run a non-dynamic version of the DiD specifications described for the balance sheet channel. Tables 16, 15 and 17 show that there are no statistically significant effects neither at the extensive and intensive margins of loan volumes, nor on loan-level interest rates. In the extensive margin specification, the interaction of exposure and a measure of firm predicted default risk is significant. However, we cannot rule out that this effect stems from confounding differences in bank characteristics. Unlike with our baseline definition of exposure based on securities holdings where we achieve good balance in bank characteristics, the origination exposure mostly captures large banks. Since firms tend to switch from smaller to larger banks as they grow and funding needs change, some of this effect may be driven by effects independent of QE.

Table 15: *Origination Channel: Intensive Margin*

	1	2	3	4	5
Exp $\times$ Post	-0.021	0.054	0.008	-0.005	-0.019
	(0.053)	(0.055)	(0.053)	(0.048)	(0.073)
Observations	11402946	11402946	11399334	11399334	11399334
Bank FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No	No
Borrower FE	No	Yes	Yes	No	No
Borrower$\times$Post FE	No	No	No	Yes	No
Borrwer$\times$Date FE	No	No	No	No	Yes
Controls	No	No	Yes	Yes	Yes

Notes Results from difference-in-differences regression. The dependent variable is the log loan volume. Exposed banks are defined as those that issued ABS or covered bonds prior to 2008. The post period begins in January 2015, the month of the ECB's official QE announcement, and end ins June 2015. Standard errors are clustered at the bank-level. Control variables (see text) are omitted to conserve space. * p<0.1, ** p<0.05, *** p<0.01.

We also look at the effect on loans that are eligible for securitization or inclusion in a covered bond. We identify these loans using loan-level information in the Central Credit

Table 16: *Origination Channel: Interest Rates*

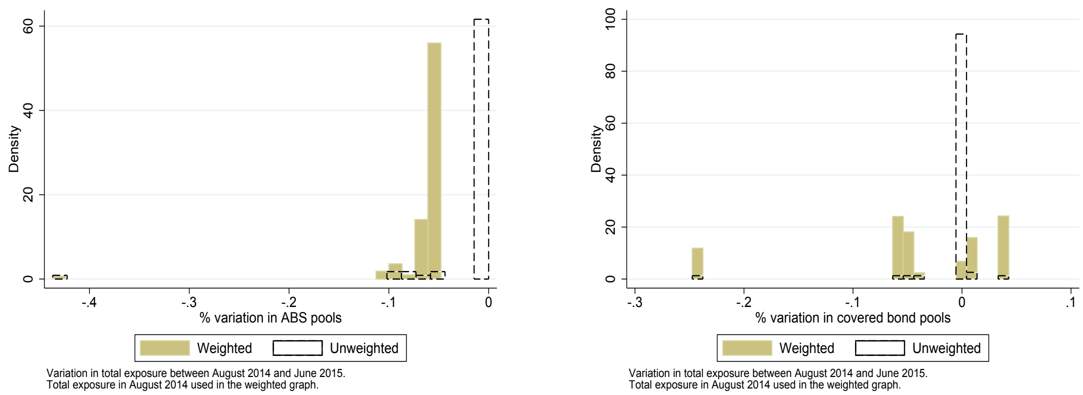
	1	2	3	4	5
Exp × Post	0.076	0.080	0.207	0.292	0.416
	(0.221)	(0.199)	(0.134)	(0.212)	(0.256)
Observations	1,156,719	1,156,719	571,687	571,687	571,687
Bank FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No	No
Borrower FE	No	Yes	Yes	No	No
Borrower*Post FE	No	No	No	Yes	No
Borrower*Date FE	No	No	No	No	Yes
Controls	No	No	Yes	Yes	Yes

Notes Results from difference-in-differences regression. The dependent variable is the loan-level interest rate (measured on a scale of 0-100). Exposed banks are defined as those that issued ABS or covered bonds prior to 2008. The post period begins in January 2015, the month of the ECB's official QE announcement, and end ins June 2015. Standard errors are clustered at the bank-level. Control variables (see text) are omitted to conserve space. * p<0.1, ** p<0.05, *** p<0.01.

Table 17: *Origination Channel: Extensive Margin*

	1	2	3
Exp × Post	0.003	0.000	-0.001
	(0.002)	(0.001)	(0.001)
Risk measure		-0.042***	-0.049***
		(0.010)	(0.010)
Exp × Post ×			0.046***
risk measure			(0.009)
Observations	5,730,278	4,073,398	4,073,398
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Borrower FE	No	No	No
Borrower×Post FE	No	No	No
Borrower×Date FE	No	No	No

Notes. Results from difference-in-differences regression. The dependent variable is the likelihood a corporate loan is approved. Exposed banks are defined as those that issued ABS or covered bonds prior to 2008. The post period begins in January 2015, the month of the ECB's official QE announcement, and end ins June 2015. The risk measure refers to predicted default risk based on the credit risk prediction model of Antunes *et al.* (2016) that uses firm observables. Standard errors are clustered at the bank-level. Control variables (see text) are omitted to conserve space. * p<0.1, ** p<0.05, *** p<0.01.



(a) *ABS*

(b) *Covered Bonds*

Figure 21: *Variation in Loans Included in Cover Bond Pool or ABS*

Notes. This figure shows bank-level variation in the change in the value of loans that are either included in an ABS (panel a), or a covered bond pool (panel b).

Register where banks are required to flag securitized loans (both those off and on balance sheet) as well as cover pool loans. Table 38 in Appendix B shows that there were indeed issuances of new covered bonds and ABS following the announcement of ABSPP and CBPP3 in September 2014. In Figure 21 however ,we show that this has not translated into an increase in the volume of securitized loans or the volume of loans in cover bond pools.

3.4 Discussion

A potential explanation for the limited effects of QE on lending conditions stems from regulatory pressures that counteracted the effects of QE. Liquidity requirements phased in around the same time may have counteracted the balance sheet channel, while the introduction of more demanding capital charges on securitized instruments may have counteracted incentives to originate and securitize loans.

3.4.1 Liquidity Regulation

The introduction of two new regulatory liquidity requirements, together with some uncertainty about which assets would count as sufficiently liquid to be included in the liquidity buffers, may account for why banks did not pass on more of their valuation gains to their borrowers.

Concurrent with the announcement of the QE programs, the EU Commission published information on the implementation of new liquidity requirements required by the Basel Committee on Banking Supervision.[21] From October 2015, banks had to comply with 60% of the liquidity coverage requirement that would be binding from January 2018 onwards. The liquidity coverage requirement (LCR) is designed to ensure that banks hold sufficiently liquid assets to serve their liabilities in a stress period without having to rely on central bank liquidity or government assistance. The LCR is calculated as the ratio of liquid assets to net liquidity outflows over a 30 day stress period. Net liquidity outflows are calculated by deducting liquidity inflows from liquidity outflows. The resulting minimum ratio is phased in starting at 60% and reaching 100% when fully implemented.

In addition to the LCR, the Basel Committee on Banking Supervision finalized its standard for an additional liquidity requirement, the net stable funding ratio (NSFR) in October 2014. The NFSR would become binding in January 2018. The NSFR requires that banks maintain a stable funding profile in relation to the composition of their assets and off-balance-sheet activities over the period of one year.

Consistent with this channel, we find that banks with higher holdings of QE-eligible assets realize profits from selling securities but use a large portion of these gains to increase their holdings of liquid assets. We run a dynamic difference-in-difference specification at the bank-level to investigate changes in the balance sheet of exposed banks. Figure 22b shows that on average exposed banks realize a 2-5 percentage point higher return on equity

[21] In December 2010, the Basel Committee announced the introduction of a liquidity coverage ratio and a net stable funding ratio from 2015 and 2018, respectively. These requirements were established as regulatory requirement by EU Regulation No 575/2013 and Directive 2013/36/EU (CRR/CRD IV) in June 2013. The delegated act published in January 2015 specifies the regulation set out in 575/2013.

from trading securities (we scale trading profits by bank equity in 2014Q1). Banks start realizing trading profits following the 2014 announcement of the ABS and covered bond purchase programs and continue realizing higher trading profits following the 2015 QE announcement. However, while the estimated effects following January 2015 are large, the latter effects are only noisily estimated

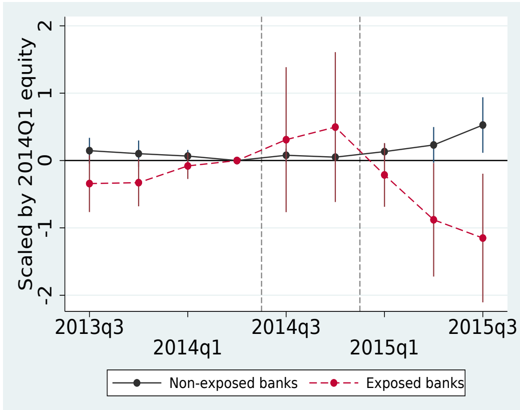
The increase in trading profits coincides with exposed banks reducing the size of their security portfolio. Figure 22a shows that there is a significant drop in the assets-for-sale portfolio of exposed banks following the QE announcement. The reduction in assets holdings occurs later than the realization of trading profits due to two reasons. First, the stock measure takes longer to reflect the effect of banks selling off QE-eligible securities. Second, since the asset-for-sale portfolio is mark-to-market, the QE-induced price increases lead to an increase in the valuation of the assets that are still held by the bank.

Finally, Figure 23 shows that there is a concurrent increase in liquid assets at exposed banks suggesting that some of the proceeds from realizing QE valuation gains are used to stock up liquid assets. Liquid assets consist of cash and highly liquid assets, as defined by the accounting practices of the Portuguese banking supervisor.

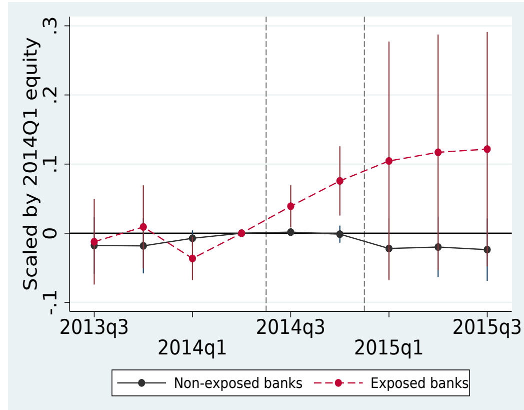
3.4.2 Challenges to ABS and Covered Bond Markets

Regulatory requirements may also explain why banks did not increase the origination of loans that would be eligible for inclusion in a covered bond or an asset-backed security. For ABS, three regulatory changes made the origination of ABS relatively unattractive, counteracting the positive incentives due to QE.

First, new capital charges on ABS on banks and insurance companies reduced the attractiveness of holding and originating securitized instruments. In December 2014, the Basel Committee published revised securitization guidelines, which would form part of the Basel III capital requirements. These guidelines included a sharp increase in the capital charges on ABS in order to address concerns of moral hazard in the securitization market which had become evident in the wake of the 2008 financial crisis. At the same time, the



(a) *Assets for Sale*



(b) *Securities Trading Income*

Figure 22: *Bank-Level Event Studies*

Notes. This figure shows bank-level difference-in-difference regression results. Exposure is defined as belonging to the highest credit-weighted quartile of QE-eligible securities as a share of assets in August 2014 (balance sheet channel). The dependent variables are the size of the asset for sale portfolio and reported income from securities trading. Both variables are scaled by 2014Q1 book equity. The regression includes bank fixed effects and time-varying bank-level controls. Standard errors are clustered by bank.

introduction of Solvency II placed significant capital charges on ABS held by insurance companies in Europe. The regulation was phased in 2015, the same year QE was announced and executed in Europe.[22].

Second, additional regulations limited the ability of banks to remove the credit risk from their portfolio limiting the usefulness of originating an ABS. Banks faced requirements to retain a portion of the risk attached to their ABS. In addition, local regulators in Europe reserved the right to recognize the transfer of risk as part of an ABS issuance when "the reduction in risk-weighted exposure amounts achieved by the securitization transaction is not justified by a commensurate transfer of credit risk to third parties".[23]

Third, ABS for the most part could not be included in the new regulatory liquidity

[22]Solvency II directive has been passed by European Parliament in November 2009. The Delegated Regulation in October 2014 contained implementation guidelines. The capital charges for insurance companies were somewhat eased by an amendment in September 2015 (Delegated Regulation (EU) 2015/35)

[23]According to guidelines published by the European Banking Authority in July 2014. See EBA guidelines p.1 at `https://www.eba.europa.eu/documents/10180/749215/EBA-GL-2014-05+Guidelines+on+Significant+Risk+Transfer.pdf`

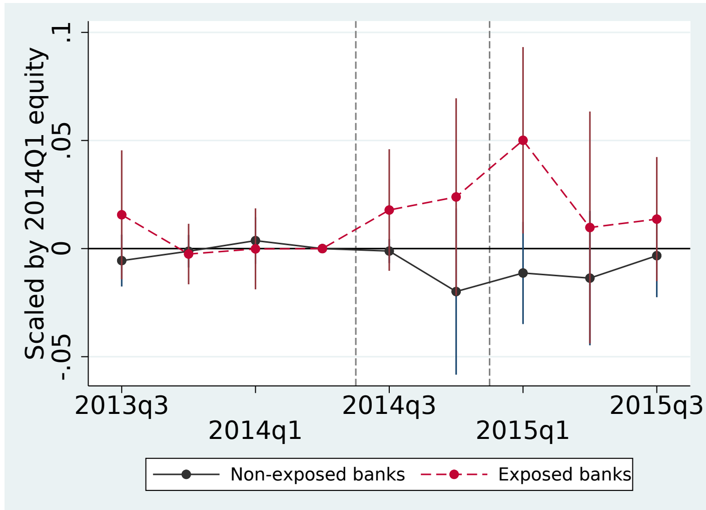


Figure 23: *Bank-Level Event Studies: Liquid Assets*

Notes. This figure shows bank-level difference-in-difference regression results. Exposure is defined as belonging to the highest credit-weighted quartile of QE-eligible securities as a share of assets in August 2014 (balance sheet channel). The dependent variable is liquid assets scaled by 2014Q1 book equity. The regression includes bank fixed effects and time-varying bank-level controls. Standard errors are clustered by bank.

buffers that banks needed to comply with, further limiting the usefulness to banks. Only senior tranches of very specific RMBS are counted as a so-called high-quality liquid assets.[24]

Covered bonds may have been subdued for slightly different reasons. First, spreads on unsecured bonds, an alternative funding source to covered bonds, had been declining even prior to the announcement of QE. This trend reduced the relative attractiveness of covered bond which have higher maintenance costs due to the existence of the collateral pool. Moreover, the ongoing liquidity provision by the European Central Bank implied that banks had access to cheap funding sources throughout the QE period. At the same time, covered bonds were also not eligible to be included in the new liquidity requirements.

[24]Only Residential mortgage-backed securities rated ECAI 1 with a minimum issue size EUR 100 million (or the local currency equivalent), and a maximum time to maturity of 5 years are eligible. In addition, additional requirements have to be satisfied, e.g. they have to be exclusively first-lien residential mortgages.

3.5 Conclusion

We study the effect of the ECB's quantitative easing on lending conditions using detailed data on the universe of corporate bank lending relationships from Portugal. We find that QE had little effect on banks' lending conditions. While banks benefited from QE through valuation gains and improved issuance conditions for asset-backed securities and covered bonds, these effects were largely not passed on the banks' borrowers in the form of lower interest rates or higher lending quantities. Some evidence points to the role of regulatory pressures working in the opposite directions. In particular, new liquidity requirements may have led banks to realize valuation gains to build up their liquidity buffers. At the same time, QE may not have been sufficient to revive the ABS and covered bond markets in the presence of tighter regulation on capital charges for ABS.

These findings are important for informing unconventional monetary policy, which has become an increasingly important tool in a zero-lower bound environment. Our findings suggest that the transmission of unconventional monetary policy through the banking sector is limited, even when the central bank's asset purchase volume is large and the economy is heavily bank-dependent. Further investigating the role of financial regulation in shaping the transmission of monetary policy is a fruitful avenue for future research.

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Chapter 1: Appendix A - A Method to Detect the Underreporting of Loan Losses

Notation

We denote the observed loan balance reported in overdue bucket k in month t by $B_{ib}(t;k)$ where i denotes the firm and b the bank. We will drop the firm-bank subscripts in the discussion that follows. There are 14 reporting buckets of overdue which correspond to the overdue buckets in the regulatory schedule

$$k \in \{\{0\}, \{1\}, \{2\}., \{3,4,5\}, \ldots, \{30, \ldots, 35\}\}.$$

where 0 refers to loans overdue less than 30 days. We denote the set of available reporting buckets by K. The first three buckets are monthly, thereafter we observe three-month buckets and thereafter 6-month buckets.

We also define a series of unobserved buckets c, which are defined at the monthly frequency

$$c \in \{\{0\}, \{1\}, \{2\}., \ldots, \{35\}\}.$$

We also define an unobserved amount of lending $C(t;\{c\})$, which is the the loan balance in each of the unobserved monthly buckets. These underlying unobserved loan balances have to add up the observed distribution: $B(t;k) = \sum_{c \in k} C(t;c)$. We will exploit the fact that we can observe the first three monthly buckets in the data, that is, we can observe $C(t;c)$ for $c \in \{0,1,2\}$.

We first assume that there are no inflows or outflows, with the exception of entry mass $\text{IN}(t;0) = C_j(t;0)$ that enters the system in the lowest reporting bucket. We relax this assumption in the following section. In the absence of any inflows and outflows, it must hold that

$$C(t;c) = C(t-1;c-1).$$

Intuitively, the loan balance we observe in bucket c at t must be the loan balance that has moved up from the preceding bucket in the previous period. We define excess mass as the deviation from this identity:

$$E(t;b) = C(t;c) - C(t-1;c-1). \tag{3.4}$$

We also assume that excess mass occurs only at the upper edge of a bucket. That is, there is no incentive to delay moving up a reporting bucket before a loan reaches the highest 'sub-bucket'. Formally, these assumptions are:

1. $C(t;c) = C(t-1;c-1),$ for all c with
   $\min\{k\} < c < \max\{k\},$ for k with $c \in k$.
2. $C(t;c+1) + C(t;c) = C(t-1;c) + C(t-1;c-1),$ for all c with
   $c = \max\{k\},$ for k with $c \in k$.

Baseline Algorithm

The goal of the algorithm is to compute the amount of excess mass in each reported overdue bucket k in each month t for each lending relationship.

We define the auxiliary concept of cumulative excess mass as

$$\bar{E}(t;k) = \sum_{j=1}^{s} E(t-j;k). \tag{3.5}$$

Cumulative excess mass is the excess mass accumulated in a bucket k over the past s months where s denotes the length of the bucket (e.g. three months).

We proceed in two steps: We first calculate cumulative excess mass $\bar{E}(t;k)$ from the observed mass $B(t;k)$, and then recursively calculate excess mass $E(t;k)$ from the cumulative excess mass.

The algorithm consists of the following steps:

1. Set $E(-1;k) = 0 = E(0;k)$ for all $k \in K$.

2. For all $t = 1, \ldots, T$

   (a) $E(t; \{0,1,2\}) = B(t; \{2\}) - B(t; \{1\})$.

   (b) $\bar{E}(t; \{0,1,2\}) = \sum_{\tau=t-2}^{t} E(\tau; \{0,1,2\})$.

   (c) For all $k = 4, \ldots, 8$

      i. Cumulative excess mass

$$\bar{E}(t;k) = B(t;k) - B(t-3;k-1) + \bar{E}(t;k-1).$$

      ii. Excess mass

$$E(t;k) = \bar{E}(t;k) - E(t-2;k) - E(t-1;k).$$

   (d) For $k = 9$

      i.

$$\begin{aligned}\bar{E}(t,k) = {} & B(t;k) - B(t-6;k-1) - B(t-6;k-2) \\ & + \bar{E}(t,k-1) + \bar{E}(t-3,k-1) + \bar{E}(t-3,k-2).\end{aligned}$$

   (e) For $k = 10, \ldots, K$

      i.

$$\bar{E}(t,b) = B(t;k) - B(t-6;k-1) + \bar{E}(t,k-1).$$

      ii.

$$E(t;k) = \bar{E}(t;k) - \sum_{\tau=1}^{5} \bar{E}(\tau;k).$$

We initialize the level of excess mass at zero in the month when our data is first available (January 2009) (step 1). For the first three buckets, we observe each month reported separately and hence directly use the baseline formula to calculate excess mass for the first bucket (step 2a). We here assume that excess mass will occur at the threshold between bucket $\{2\}$ and $\{3\}$ since the deduction rate is constant across the first three buckets. In step 2b, we obtain cumulative excess mass for the first combined three-month bucket at t

by adding the excess mass in the first combined three-month bucket across the past three months. For the following buckets, we have to take into account that reporting is done in buckets that stretch over three or six months respectively. We first compute cumulative excess mass in step i. by exploiting that the amount we observe in bucket k at time t is the sum of the amount that has been moved from the preceeding bucket $k-1$ over the course of the last three months minus the cumulative excess mass in the preceeding bucket that was not transferred over the last three months, plus the cumulative excess mass that has stayed behind in bucket k over the past three months. Once we have calculated excess mass in step i., we can then recursively compute excess mass in step ii. We repeat similar steps for the six-month buckets.

Algorithm with Flows

We now explain how to adjust the baseline algorithm for flows. We allow for the observed lending at t to be affected by time t inflows and outflows. The observed lending stock evolves as follows

$$\text{stock}_t = \text{stock}_{t-1} + \text{net inflow}_t$$

. We can further decompose net inflows into the following components

$$\text{net inflow}_t = \text{entry}_t + \text{installments}_t - \text{written-off}_t - \text{restructured}_t + \text{residual}_t$$

. Inflows, other than the initial entry inflow, consist of installments that fall overdue. Inflows into buckets higher than the initial $k = 0$ will lead us to *overestimate* excess mass since these flows add to the observed mass at t. Since installments tend to be of fixed size and occur at regular intervals, we classify an increase in the overdue loan balance that corresponds to an exact decrease in the balance of performing credit and that occurs at least twice as an installment.

Outflows in contrast will lead us to *underestimate* excess mass since we subtract too

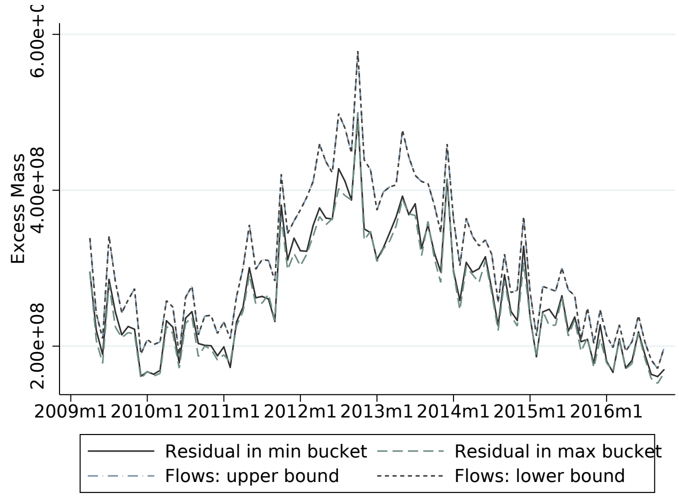


Figure 24: *Robustness of Excess Mass to Assumptions*

Notes. The graph shows the aggregate amount of excess mass when varying different assumptions. The first two lines show the results when I allocate residual flows to the lowest (highest) reporting bucket. The remaining lines show the effect of choosing the bounds on flows such that they have the minimum (maximum) impact on excess mass.

much past mass. In the extreme case, this will lead us to obtain negative excess mass. Outflows happen for three reasons: repayment, restructuring and write-offs. If a bank restructures or write offs an overdue loan, it reduces the overdue balance and increases the restructured/write-off balance which are separate entries in our data. We can therefore measure outflows into these two categories by a reduction in the overdue balance in a given bucket that is less or equal to the change in restructured/written-off balance in the same month. We cannot directly measure repayments of overdue loans which will instead be recorded as a (negative) residual. We distribute the residual across buckets by assigning the residual to the buckets with non-zero overdue balances in line with the share of lending reported in that bucket.

Since this distribution of residual flows to buckets is somewhat arbitrary, we conduct robustness checks to see how much the results change when shifting the residual flows to the lowest (highest) buckets. Since residual flows are small, the results are not affected by this assumption (see figure 24).

The basic formula adjusted for flows is

$$E(t;k) = [C(t;c) - \mathrm{IN}(t;c)] - [C(t-1;c-1) - \mathrm{OUT}(t;c-1)] \,. \tag{3.6}$$

We subtract inflows out of bucket c since these flows contribute to *observed* mass but do not contribute to *excess* mass. We add outflows from the preceding bucket since we do not expect these outflows to have moved up into the next reporting bucket. If we observed only monthly buckets, then we could again apply the simple formula to all buckets. However, for the three-month and six-month buckets, we again need to resort to the auxilary concept of cumulative excess mass.

The formula for cumulative excess mass adjusted for flows is as follows:

$$\begin{aligned}\bar{E}(t,k) &= B(t;k) - B(t-3;k-1) + \bar{E}(t,k-1) \\ &\quad + \hat{\mathrm{OUT}}(t;k-1) - \tilde{\mathrm{IN}}(t;k-1) - \hat{\mathrm{IN}}(t;k) + \hat{\mathrm{OUT}}(t;k).\end{aligned}$$

The flow adjustments consists of the following components. We denote individual monthly buckets within each three-month bucket as $k\{1\}, k\{2\}, k\{3\}$. Hence $k\{2\}$ refers to the middle bucket within the three month bucket k.

1. $\hat{\mathrm{OUT}}(t;k-1)$: For outflows, we want to subtract all outflows out of the preceding bucket over the past three months, which we would not have expected to have turned up in the current bucket. Specifically, these are the outflows from the 'boundary' bucket $\{3\}$ that we would not expect to move across into the next bucket:

$$\begin{aligned}\hat{\mathrm{OUT}}(t;k-1) &= \mathrm{OUT}(t,(k-1)\{3\}) \\ &\quad + \mathrm{OUT}(t-1,(k-1)\{3\}) + \mathrm{OUT}(t-2;(k-1)\{3\}).\end{aligned}$$

2. $\tilde{\mathrm{IN}}(t;k-1)$: There are inflows into the previous bucket $k-1$, some of which we expect

to have moved by time t and we need to add:

$$\tilde{\text{IN}}(t;k-1) = \underbrace{\text{IN}(t-3;(k-1)\{1\})) + \text{IN}(t-3;(k-1)\{2\})) + \text{IN}(t-3;(k-1)\{3\}))}_{\text{already incorporated in B(t-3,k-1)}}$$

$$+ \text{IN}(t-2;(k-1)\{2\}) + \text{IN}(t-2;(k-1)\{3\})) + \text{IN}(t-1;(k-1)\{3\})$$

$$= \text{IN}(t-2;(k-1)\{2\}) + \text{IN}(t-2;(k-1)\{3\})) + \text{IN}(t-1;(k-1)\{3\}).$$

3. $\hat{\text{IN}}(t;k)$: There are inflows into the current bucket k which we do not expect to have moved up to the next reporting bucket so they need to be added. Note that outflows only affect how much moves on to the *next* bucket but not how much sticks around:

$$\hat{\text{IN}}(t;k) = \underbrace{\text{IN}(t;k\{1\})) + \text{IN}(t;k\{2\})) + \text{IN}(t;k\{3\}))}_{\text{IN}(t;k)}$$

$$+ \text{IN}(t-1;k\{1\}) + \text{IN}(t-1;k\{2\})) + \text{IN}(t-2;k\{1\}).$$

4. $\hat{\text{OUT}}(t;k)$: Some of the mass that has moved up into the current bucket over the course of the past three months may have left the current bucket in the form of outflows, which we need to subtract. We only want to correct for the part that came in and then left again. So effectively we subtract the outflows from the bucket k from the inflow into bucket k. We cannot precisely tell which outflows exactly correspond to the inflows hence we just consider the total outflows. The earliest such outflow can occur at $t-2$. Outflows at time t does not affect the measure of excess mass:

$$\hat{\text{OUT}}(t;k) = \text{OUT}(t-1,k)\text{OUT}(t-2;k).$$

We cannot measure the flows in and out of unobservable sub-buckets, which we denoted by $k\{1\}, k\{2\}, k\{3\}$. Hence we have to approximate the flows that we defined above by making an assumption how the total flow is distributed across the months that comprise a given bucket. We can however specify the bounds for each flow component and have an exact measure for the last. The bounds are as follows:

1. $0 \leq \hat{\text{OUT}}(t;k-1) \leq \sum_{j=0}^{3} \text{OUT}(t-j;k-1)$

2. $0 \leq \tilde{\mathrm{IN}}(t;k-1) \leq \mathrm{IN}(t-1;k) + \mathrm{IN}(t-2;k)$

3. $\mathrm{IN}(t;k) \leq \hat{\mathrm{IN}}(t;k) \leq \mathrm{IN}(t;k) + \mathrm{IN}(t-1;k) + \mathrm{IN}(t-2;k)$

4. $\hat{\mathrm{OUT}}(t;k) = \mathrm{OUT}(t-1,k) + \mathrm{OUT}(t-2;k)$

Table 18: *Effects of Assumptions on Flows*

Total mass estimate	$\hat{\mathrm{OUT}}(t;k-1)$	$\tilde{\mathrm{IN}}(t;k)$	$\hat{\mathrm{IN}}(t;k-1)$
Effect on excess mass	+	−	−
Max	Upper	Lower	Lower
Baseline	Upper	Upper	Upper
Min	Lower	Upper	Upper

Table 18 shows the combination of assumptions that generate the largest (and smallest) excess mass. For our baseline results, we choose the upper bounds for all flows which is a middle ground between combinations that yield that largest and smallest results respectively. In Figure 24, we present results using the maximum and minimum combinations respectively. Figure 25 shows that we get similar results when ignoring flows and simply using the formulas that only consider stocks. The reason is both that flows are small relative to stocks and that in many instances inflows and outflows cancel out.

The formulas above applied to three-month reporting buckets. We now present formulas for the six-months buckets. For the first 6-month bucket

1. $\hat{\mathrm{OUT}}(t;k-1) = \sum_{j=0}^{6} \mathrm{OUT}(t-j;k-1)$

2. $\hat{\mathrm{IN}}(t;k-1) = \sum_{j=1}^{5} \mathrm{IN}(t-j;k-1)$

3. $\hat{\mathrm{OUT}}(t;k-2) = \sum_{j=3}^{3} \mathrm{OUT}(t-j;k-2)$

4. $\hat{\mathrm{IN}}(t;k-2) = \sum_{j=4}^{2} \mathrm{IN}(t-j;k-2)$

5. $\hat{\mathrm{OUT}}(t;k) = \sum_{j=1}^{5} \mathrm{OUT}(t-j;k)$

6. $\hat{\mathrm{IN}}(t;k) = \sum_{j=0}^{6} \mathrm{IN}(t-j;k)$

And for the following two six-month buckets:

1. $\hat{\text{OUT}}(t;k-1) = \sum_{j=0}^{6} \text{OUT}(t-j;k-1)$

2. $\hat{\text{IN}}(t;k-1) = \sum_{j=1}^{5} \text{IN}(t-j;k-1)$

3. $\hat{\text{OUT}}(t;k) = \sum_{j=1}^{5} \text{OUT}(t-j;k)$

4. $\hat{\text{IN}}(t;k) = \sum_{j=0}^{6} \text{IN}(t-j;k)$

Additional restrictions

We impose the following additional restrictions:

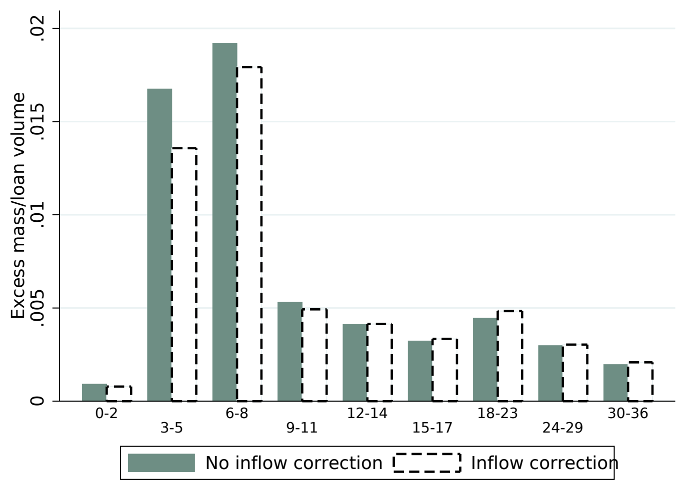


Figure 25: *Underreported Losses by Reporting Buckets*

Notes. The graph shows the distribution of excess mass (or underreporting) across reporting buckets. I scale the amount of excess mass by the total loan balance of that firm-bank pair. I compare the results of the algorithm with and without incorporating the effects of flows (repayments, new installments falling overdue, debt write-offs or restructuring) in the data.

1. We impose that excess mass can never exceed observed mass in bucket.[25]

$$\bar{E}(t;k) = \max\left(\bar{E}(t;k); B(t;k)\right).$$

2. We also impose that excess mass must be weakly positive since negative excess mass is just a mismeasured outflow: $B(t;k) \leq 0$.

3. We adjust for the common practice of banks to move overdue loans off their balance sheet in December to boost end-of-year statements, and putting the overdue balance back on in January. This leads to spurious fluctuation in our measure of excess mass.

Examples We now provide stylized examples to illustrate the three mechanisms banks use to adjust the reported time overdue. In Table 19, we replicate the structure of our data. A monthly firm-bank credit panel with the overdue loan balance reported separately for each bucket. The first mechanism consists of simply not updating the reported time overdue (panel a of Table 19). The second mechanism involves banks combining different overdue loan installments. According to the regulatory rules, banks should report (and deduct losses) for new overdue installments at the rate of the longest overdue portion. However, a common practice shown in panel b of Table 19 is to combine a new installment with the existing overdue balance and to report a (lower) averaged time instead. The third mechanism is granting new performing credit in exchange for the repayment of an overdue portion of the loan. In this case, banks treat the 'repaid' portion as the portion that had been overdue the longest. The time overdue reported on the remaining overdue balance can hence stay

[25]There are few cases where this restriction matters, namely:

(a) The algorithm subtracts from measure of current excess mass a measure of mass we do not expect to have moved up. Given the backwards looking nature of the cumulative mass, we may get cases where we compute a positive mass even though there is no mass in that bucket. In those cases, we replace the measure of mass we do not expect to have moved up with zero

(b) A related case where we compute positive excess mass due to positive excess mass in the previous bucket even though there is not mass

(c) We also impose that excess mass must be weakly positive since negative excess mass are just mismeasured outflows:

.

constant. The bank has not provided any net new liquidity but simply changed labels (see panel b of Table 19). Figure 26 in this appendix shows that most underreporting is driven by the second and third mechanism.[26]

Validity Checks

First validity test The first validity test regresses excess mass, the amount of underreporting, at firm i, bank b, month t, collateral type c, and reporting bucket k on a set of dummies that capture the increments in the mandatory deduction rate between reporting bucket k and $k+1$:

$$\frac{\text{excess mass}_{ibkct}}{\text{overdue loans}_{ibkct}} = \sum_{j=1}^{5} \beta_j \Delta \text{deduction rate}_j + \varphi_b + \theta_i + \mu_t + \epsilon_{ibkct}. \tag{3.7}$$

where i, b, c, k and t index firms, banks, collateral type, reporting category and month. We include firm-bank fixed effects and hence only use variation within a given lending relationship. We cluster standard errors at the firm-bank level. j indexes the possible increments in the regulatory rate, ranging from 0 to 25 percentage points (p.p.).

The coefficients β_j measure the additional amount of excess mass that occurs in buckets when the change in the regulatory deduction rate from k and $k+1$ is equal to Δrate_j, relative to buckets where the regulatory rate stays constant. If banks act strategically, we would expect all coefficients to be positive and statistically significant, and larger rate increments to have larger coefficients. We only consider relationships that have a single type of collateral to avoid confounding the estimate by including relationships with several types of collateral since the regulatory rules differ by collateral type. We estimate the specification separately for each type of collateral. Results are presented in Table 20 and discussed in section 2.

[26] Strategic reporting entails the risk of an audit by the financial regulator. The Portuguese supervisor does not inspect the granular loan-level data we exploit and hence does not typically check for reporting inconsistencies at the relationship level. However, the supervisor does reserve the right to send inspection teams to conduct spot checks on a bank's books. Hence banks are likely aware of the possibility that they may have to justify their reporting choices following an audit.

Table 19: *Examples of Loss Underreporting*

Panel A: Example 1

	<30 days	**Overdue 1 month**	**2 months**	**Performing credit**	**Excess mass**
2012m1	EUR 50			EUR 450	0
2012m2		EUR 50		EUR 450	0
2012m3		**EUR 50**		EUR 450	50
2012m4			EUR 50	EUR 450	0

Panel B: Example 2

	<30 days	**Overdue 1 month**	**2 months**	**Performing credit**	**Excess mass**
2012m1	EUR 50			EUR 450	0
2012m2		EUR 50		EUR 450	0
2012m3	EUR 30 →	**EUR 80**	← EUR 50	EUR 470	80
2012m4		**EUR 30**		EUR 420	30
2012m5			EUR 30	EUR 420	0

Notes. The table shows stylized examples of the loan data collapsed to the monthly firm-bank level. I show lending volumes of a hypothetical firm-bank pair. I show the first three reporting categories of how long a loan has been overdue. Performing credit denotes the loan balance which is not (yet) overdue. Panel A shows an example where the bank does not update the reported time overdue in March, which is registered as excess mass by the algorithm (mechanism 1). Panel B shows the other two mechanisms: In March, a new portion of EUR 30 falls overdue (reducing performing credit by EUR 30). According to the rules, the bank should report the total in the category of the longest overdue portion (2 months). Instead the bank reports the total at the averaged time overdue (1 month). The algorithm registers an excess of EUR 80. In March, the bank also grants EUR 50 of new performing credit, which means that the performing balance is EUR 450 - 30 + 50 = 470. In April, the firm uses the new credit to pay back EUR 50 of the overdue balance. The bank treats the repaid portion as the longest overdue and reports the EUR 30 in the same overdue category as in March. The last rows in each example illustrate that the algorithm is "memory-less": As long as reporting is consistent relative to the previous month, the algorithm does not register excess mass.

Second validity check Since we can directly trace the time a loan has been overdue in the subset of relationships with only a single loan, we can plot the average amount of underreporting against the actual overdue duration based on the data.[27] We expect underreporting to be most pronounced in the month after the regulatory deduction rate

[27]This exercise resembles the more traditional bunching graphs, which plot the cross-sectional distribution to provide a visual test for the presence of excess mass at the points where bunching is expected to occur.

Table 20: *Algorithm Validity Check:*
Bunching at Points of Rate Increases

Panel a: Bunching test			
	(1)	(2)	(3)
Excess mass/loans	No collateral	Guarantee	Real collateral
Increase in deduction rate in next higher reporting bucket			
9 p.p.		0.244***	0.110***
		[0.002]	[0.005]
15 p.p.		0.451***	0.349***
		[0.002]	[0.004]
24 p.p.	0.178***		
	[0.005]		
25 p.p.	0.324***	0.098***	0.014***
	[0.005]	[0.001]	[0.002]
N	363,132	1,253,589	232,659
R2	0.581	0.450	0.464

Panel b: Placebo test			
	(1)	(2)	(3)
Excess mass/loans	No collateral	Guarantee	Real collateral
Increase in deduction rate in next higher reporting bucket			
25 p.p.	-0.024***	-0.004***	-0.015***
	[0.001]	[0.001]	[0.001]
N	363,132	1,253,589	232,659
R2	0.199	0.213	0.224

Notes. The table shows regression results for the first validity test of the loss underreporting algorithm. The dependent variable is the amount of excess mass (or underreporting) scaled by the total overdue loan balance in a given reporting bucket for a firm-bank relationship (see table 1 for a visual depiction). The explanatory variables are a series of dummies that capture how much the regulatory deduction rate increases from the current reporting bucket to the next higher reporting bucket (e.g. deduction rate of 1% vs. 10% = increase of 9 p.p.). This difference measures the intensity of the incentive to underreport. The sample is split by collateral type since the regulatory rules differ by collateral type. Each column corresponds to the results of a regression in the sample of firm-bank pairs that only have that type of collateral. The omitted baseline category is 0 (no rate increase). Hence the coefficients capture how much more excess mass (or underreporting) occurs in reporting buckets where there is an increase in the regulatory rate in the next higher bucket. Regressions include firm$\times$bank fixed effects. The placebo test regresses underreporting on buckets where there is a rate increase for the *other* collateral type but not for the given collateral type. Standard errors are clustered by firm-bank pair. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level increases as this implies that banks continues to deduct at the lower rate associated with the previous reporting bucket. For example, the regulatory rate increases when switching from reporting that the loan has been overdue 5 months to reporting that is has been overdue 6 months. Hence the incentive to underreport is highest when the actual overdue duration has reached 6 months. By reporting that the 6-month overdue loan continues to have been overdue only 5 months, the bank avoids the jump up in impairment losses associated with reporting 6 months. As in the first exercise we select the loans that have only a single type of collateral since the regulatory schedule differs by collateral type. Figure 27 provides visual evidence of 'bunching'. In other words, the figures show spikes in the amount of underreporting just after an increase in the regulatory rate as we would expect. Moreover, the spikes occur in different places for different collateral types in line with differences in the regulatory rules.

We formally confirm the existence of bunching by regressing the amount of excess mass in month t on a categorical variable that captures the same set of increments in the regulatory deduction rate as above. Table 21 confirms that an increase in the regulatory rate strongly correlates with an increase in the scaled amount of loss underreporting, or excess mass. For example, an increase in the rate by 24 percentage points leads to an 11 percentage point increase in the loan balance that is subject to loss underreporting (relative to the time periods without an increase in the regulatory rate). The effect is non-monotonic with larger increases for the 3-5 months reporting category, which corresponds to $\Delta\text{rate}_{t-1} = 9$ for collateralized loans and $\Delta\text{rate}_{t-1} = 24$ for non-collateralized loans. This non-monotonicity is due to the pressures to avoid classifying loans as non-performing explained in chapter 1 section 2.

Table 21: *Loss Underreporting: Bunching in Sample of Single-Loan Relationships*

Excess mass/loan balance	(1)	(2)	(3)
Increase in regulatory rate between $t-1$ and t			
9 p.p.	0.062***	0.063***	0.061***
	[0.008]	[0.008]	[0.008]
15 p.p.	0.032***	0.033***	0.032***
	[0.007]	[0.007]	[0.007]
24 p.p.	0.111***	0.109***	0.115***
	[0.018]	[0.018]	[0.021]
25 p.p.	0.026***	0.025***	0.030***
	[0.004]	[0.004]	[0.004]
Bank, firm FE	Y	Y	N
Controls	N	Y	N
N	601,502	601,502	603,252
R2	0.118	0.118	0.019

Notes. The table shows regression results for the second validity test of the loss underreporting algorithm. The dependent variable is the amount of excess mass scaled by the total loan balance in a given reporting bucket for a firm-bank relationship (see table 1 for a visual depiction). The explanatory variables are a series of dummies that capture how much the regulatory deduction rate increases from the reporting bucket in month t-1 to the reporting bucket at t. t refers to the constructed time overdue (counting in the data how long a loan has been overdue). The increase in the regulatory rate measures the intensity of the incentive to underreport. The sample only includes relationships with a single loan for which we can construct the time overdue. The omitted baseline category is 0 (no rate increase). Hence the coefficients capture how much more excess mass (or underreporting) occurs in months where there is an increase in the rate in the following month. Controls are the type of collateral. Standard errors are clustered by firm-bank pair. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

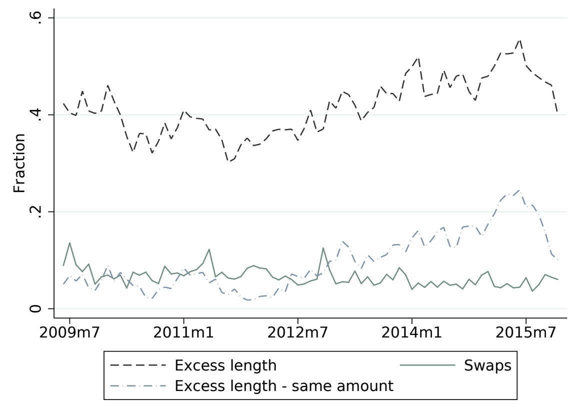
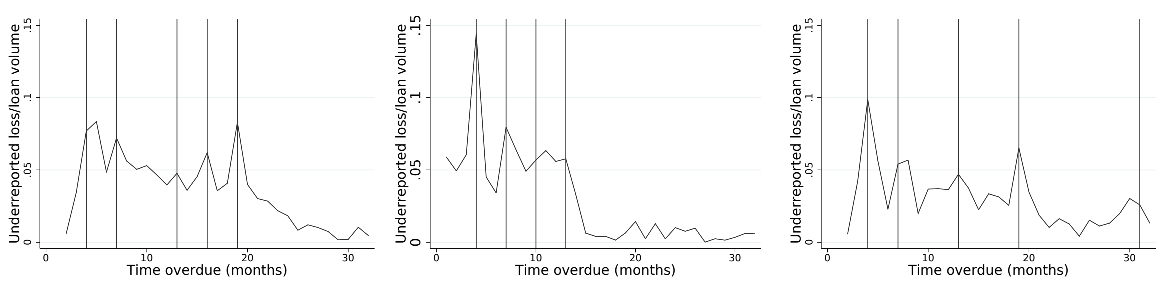


Figure 26: *Decomposition of Underreported Losses by Mechanism*

Notes. The graph shows the decomposition of underreported losses by the mechanisms discussed in section 2. Excess length refers to spells of overdue reporting in a bucket that exceed the permissible length (e.g. loan reported to be overdue 3-5 months for 4 months in a row.). Excess length - same amount refers to spells that exceed the permissible length where the loan balance does not change. Swaps refer to cases where there is a decrease in the overdue balance equal to an increase in the performing loan balance. This captures the last mechanism where banks grant new credit in exchange for the firm repaying the longest overdue credit portion. All numbers are scaled by the total amount of excess mass.



(a) *Loans with Guarantee* **(b)** *Loans without Collateral* **(c)** *Loans with Real Collateral*

Figure 27: *Algorithm Validity Test for Single-loan Relationships*

Notes. The graph plots the average amount of underreporting against the actual time a loan has been overdue. I only consider single-loan relationships where I can track the actual time overdue (the number of months the bank has reported any positive overdue loan balance). The vertical lines denote the points where I would expect most underreporting to occur (increase in the regulatory deduction rate). These points differ according type of collateral. I only consider loans with a single type of collateral.

Chapter 1: Appendix B - Additional Tables and Figures

Table 22: *Additional Descriptive Statistics*

Firm finance loans		Underereported firms		
	Average		Average	Difference
Loan amount	269,729	Total assets (m)	1.09	0.516***
	($2x10^6$)		(5.165)	[0.041]
Fraction overdue	0.50	Debt/assets	0.205	0.097***
	(0.42)		(0.41)	[0.004]
Fraction collateralized	0.73	EBIT/sales	0.13	0.009***
	(0.44)		0.16	[0.002]
Fraction w/ guarantee	0.79	Debt/EBITDA	-0.300	-0.771***
	(0.41)		(12.911)	[0.066]
Fraction w/ real collateral	0.32	EBITDA/assets	-0.052	0.011***
	(0.47)		(0.292)	[0.002]
Maturity < 1yr	0.23	Sales growth	-0.030	-0.018***
	(0.42)		(0.709)	[0.003]
Resid maturity < 1yr	0.48	Cash/assets	0.053	-0.019***
	(0.50)		(0.151)	[0.001]
		Debt to government/assets	0.089	0.051***
			(0.135)	[0.002]
		Collateral ratio	0.02	-0.039***
			0.17	[0.002]
N	1,332,435		18,314	

Notes. The left panel shows descriptive statistics at the loan-level for firm finance loans that have an overdue loan balance at some point over their lifetime. This is the sample of loans on which we run the algorithm to detect the underreporting of loan losses. The first column of the right panel shows averages for firms that are subject to loss underreporting in a given year. The second column of the right panel shows differences in means relative to firms that have overdue loans but are not underreported. The collateral ratio combines the extensive margin (has any collateral) and the intensive margin (value of collateral). Standard errors in parentheses. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 23: *Treatment Effect for Firms with Correctly Reported Losses*

	(1)	(2)	(3)	(4)
	Intensive		Extensive	
$\text{Pre1}_t \times \text{exposed}_b \times \text{fully reported}_{ib}$	0.009	0.003		
	[0.009]	[0.010]		
$\text{Pre2}_t \times \text{exposed}_b \times \text{fully reported}_{ib}$	0.015	0.015		
	[0.015]	[0.016]		
$\text{EBA}_t \times \text{exposed}_b \times \text{fully reported}_{ib}$	0.010	0.015	-0.078***	-0.086***
	[0.008]	[0.018]	[0.004]	[0.009]
$\text{Bailout}_t \times \text{exposed}_b \times \text{fully reported}_{ib}$	-0.017	-0.012	-0.020***	-0.018***
	[0.012]	[0.013]	[0.004]	[0.005]
$\text{Post bailout}_t \times \text{exposed}_b \times \text{fully reported}_{ib}$	-0.031***	-0.011	-0.006***	0.002
	[0.009]	[0.007]	[0.003]	[0.003]
Firm×quarter FE	Y	N	N	N
Firm, quarter FE	N	Y	Y	N
N	1,981,219	1,981,219	2,980,249	2,538,082
R2	0.379	0.058	0.351	0.301
Banks	45	45	46	45

Notes. The table shows additional credit regressions results at the firm-bank level for the intensive and extensive margin. The specification is as in equation 1.2 but we replace the interaction effects with the group of firms that have overdue loans but whose losses are not underreported. Additional interaction effects are omitted. See equations in section 3 for details on full set of interaction effects included. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 24: *Regression Results Firm-Bank Level: Robustness Checks*

Growth rate of total credit	(1)	(2)	(3)	(4)	(5)
			OLS		WLS
Pre1$_t$×exposed$_b$	-0.009	-0.011	-0.009	-0.016*	-0.026**
	[0.008]	[0.010]	[0.011]	[0.009]	[0.011]
Pre2$_t$×exposed$_b$	-0.004	-0.003	-0.002	-0.018	-0.023**
	[0.010]	[0.013]	[0.010]	[0.011]	[0.011]
EBA$_t$×exposed$_b$	-0.022**	-0.020*	-0.020	-0.029***	-0.038***
	[0.010]	[0.012]	[0.013]	[0.011]	[0.010]
Bailout$_t$×exposed$_b$	-0.009	-0.006	-0.005	-0.015*	-0.012*
	[0.006]	[0.008]	[0.008]	[0.008]	[0.007]
Post bailout$_t$×exposed$_b$	0.006	0.008	0.008	0.002	0.006
	[0.007]	[0.010]	[0.012]	[0.009]	[0.008]
Pre1$_t$×exposed$_b$×underreported$_{ib}$	0.002	0.001	0.012	0.013	-0.003
	[0.013]	[0.014]	[0.010]	[0.008]	[0.012]
Pre2$_t$×exposed$_b$×underreported$_{ib}$	0.006	0.006	0.024	0.012	0.005
	[0.023]	[0.028]	[0.023]	[0.016]	[0.019]
EBA$_t$×exposed$_b$×underreported$_{ib}$	0.043***	0.044***	0.051***	0.051***	0.073***
	[0.013]	[0.014]	[0.017]	[0.015]	[0.014]
Bailout$_t$×exposed$_b$×underreported$_{ib}$	0.027	0.027	0.026	0.034***	0.040***
	[0.017]	[0.019]	[0.021]	[0.010]	[0.013]
Post bailout$_t$×exposed$_b$×underreported$_{ib}$	0.016	0.016	0.021	0.014	0.029***
	[0.012]	[0.013]	[0.014]	[0.010]	[0.009]
Firm×quarter FE	Y	Y	N	N	Y
Firm, quarter FE	N	N	Y	Y	N
Relationship controls	N	Y	Y	Y	Y
Firm-level controls	N	N	Y	N	N
N	1,981,219	1,981,219	1,859,321	5,244,714	1,981,219
R2	0.378	0.379	0.057	0.069	0.417
Banks	45	45	45	45	45

Notes. The table shows additional credit regressions results at the firm-bank level for the intensive margin. The dependent variable is the quarterly growth rate in total credit for a given firm-bank pair. The explanatory variable exposed is a dummy that is 1 for banks exposed to the EBA shock. Sample period is 2009q1-2014q4. Pre 1 and 2, EBA, bailout and post-bailout are dummies that identify the following time periods: The EBA shock (2011q4-2012q2), the bailout period (2012q-2012q4), two pre-periods and one post-bailout period all of equal length. underreported is a dummy that identifies relationships subject to underreported losses in the four quarters prior to the EBA shock. All regressions include bank fixed effects. Standard errors in parentheses and are two-way clustered by bank and firm with the exception of column (2) which is clustered by bank-level. Column (4) does not restrict the sample to firms with multiple lending relationships. Column (5) weights by the square root of the loan balance. Square root weighting is attractive because it does not give as much weight to the tail of very large firms as level weighting. Additional interaction effects are omitted. See equations in section 3 for details on the full set of interaction effects included. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 25: *Pass-Through Into Employment and Investment: Persistence and Placebo Tests*

	(1)	(2)	(3)	(4)	(5)
Growth rate			Employees		
	2013	2014	2011	2009	2008
Δ log credit$_f$	-0.555**	3.326	-0.028	-0.653	0.102
	[0.218]	[6.853]	[0.045]	[0.978]	[0.074]
Controls	Y	Y	Y	Y	Y
Industry, size FE	Y	Y	Y	Y	Y
First stage F-statistic	8.8	0.277	116.7	1.5	6
N	105,170	93,729	126,595	126,595	124,478

Notes. The table shows regression results at the annual firm-level for different years. The dependent variable is the symmetric growth rate of employment, which is a second order approximation to the log difference growth rate and incorporates observations that turn to 0 (firm exit). We instrument for the log change in credit using the (normalized) firm-level borrowing share from banks exposed to the EBA shock prior to the shock. Relative to table 5 we only vary the year of the dependent and independent variables. Controls consist of firm-size and 2-digit industry FE, as well as firm-level log total assets, interest/ebitda, capital/assets, currrent ratio, cash/assets and sales growth all averaged over 2008-2010. Lag refers to the lag of the dependent variable. Standard errors are clustered by industry. Standard errors in parentheses. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 26: *Pass-Through Into Employment and Investment*

Panel a	(1)	(2)	(3)	(4)	(5)
	Labor	Capital	Materials	Services	TFP
Δ log credit$_f$	0.097**	0.692***	0.138**	0.284**	-0.081
	[0.018]	[0.105]	[0.024]	[0.053]	[0.055]
Lag	-0.062**	0.046***	-0.317***	0.028**	-0.326***
	[0.012]	[0.005]	[0.005]	[0.008]	[0.023]
Controls	Y	Y	Y	Y	Y
Industry, size FE	Y	Y	Y	Y	Y
N	78,288	78,288	78,288	78,288	78,288
First-stage F statistic	195.8	195.8	195.8	195.8	195.8

Panel b	(1)	(2)	(3)	(4)
	Labor gap	Capital gap	Materials gap	Services gap
Δ log credit$_f$	-0.100**	-0.195**	-0.020	0.012
	[0.020]	[0.033]	[0.062]	[0.033]
Controls	Y	Y	Y	Y
Industry, size FE	Y	Y	Y	Y
N	102,495	102,495	102,495	102,495
First-stage F statistic	193	193	193	193

Notes. The table shows IV regression results at the annual firm-level in 2012. All the dependent variables are in log differences. All variables are deflated according to procedure described in online appendix. In panel b, dependent variables are firm-level gaps between output elasticities and revenue shares, computed based on cost shares. We use the log change of the absolute value of the gap (to allow for negative gaps). We instrument for the log change in credit using the (normalized) firm-level borrowing share from banks exposed to the EBA shock prior to the shock. Capital refers to the real capital stock computed using the perpetual inventory method. TFP, or technical efficiency, is a production function residual. Controls consist of firm-size and 2-digit industry FE, as well as firm-level log total assets, interest/ebitda, capital/assets, current ratio, cash/assets and sales growth all averaged over 2008-2010. Lag refers to the lag of the dependent variable. In panel b, we control for firm-level sales cyclicality and productivity volatility. Standard errors are clustered by industry. Standard errors in parentheses. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 27: *Robustness: Spillovers*

	(1)	(2)	(3)	(4)	(5)	(6)
	2-digit		4-digit		1-digit-district	
	Sales	Capital	Sales	Capital	Sales	Capital
Industry share of	-0.093***	-0.104***	-0.139***	-0.085***	-0.122***	-0.117**
underreported firms	[0.018]	[0.019]	[0.020]	[0.023]	[0.047]	[0.046]
N	43,273	43,273	43,273	43,273	43,273	43,273
First-stage	3381	3381	2531	2531	549.8	549.8

Notes. The table shows IV regression results at the firm-level for 2012. Share underreported refers to the asset-weighted share of distressed, underreported firms. We vary the definition of industry. The last column shows results for creating combined district-1-digit industry categories. We instrument for this variable using the average firm-level borrowing share from EBA banks. We standardize the share such that the coefficients should be interpreted as the effect of increasing the industry-share of underreported firms by a standard deviation. The dependent variables are all in log changes and deflated. Capital refers to the real stock of capital computed using the perpetual inventory method described in Appendix C. Controls consist of firm-size bucket FE as well as firm-level log total assets, interest/ebitda, capital/assets, current ratio, cash/assets and sales growth all averaged over 2008-2010. Robust standard errors in parentheses. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

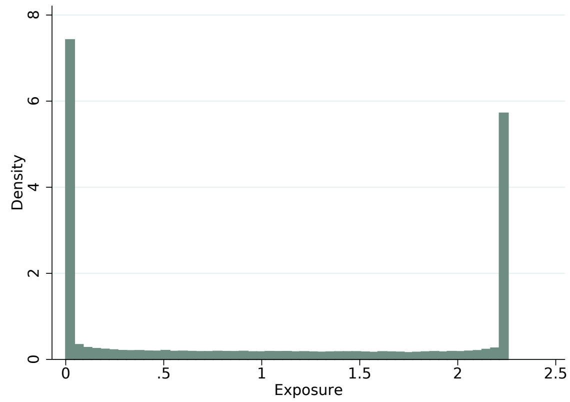
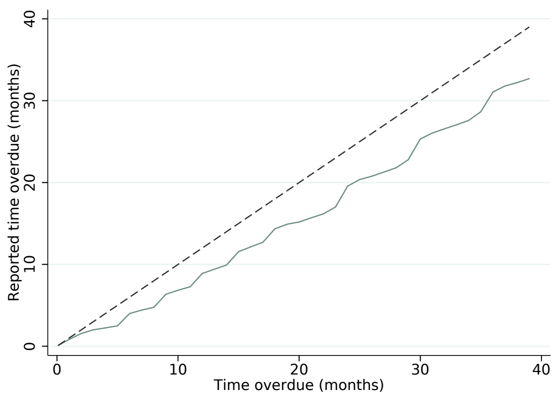
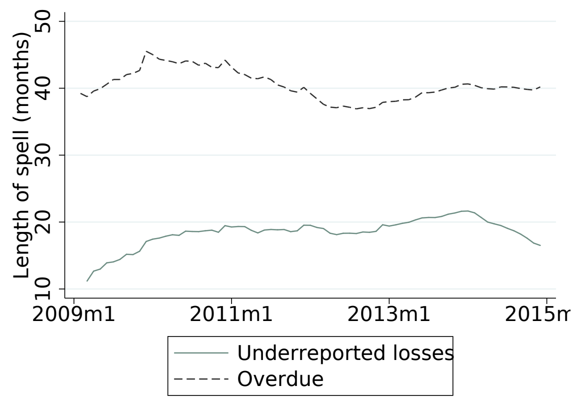


Figure 28: *Histogram of Firm-Level Exposure to EBA Shock*

Notes. The graph shows a histogram of the continuous exposure measure for firms with underreported losses. The exposure measure is the share of credit coming from banks exposed to the EBA shock in 2010. We standardize the measure to have unit variance. The peaks at 0 and 2.25 represent firms borrowing exclusively from exposed and non-exposed banks.



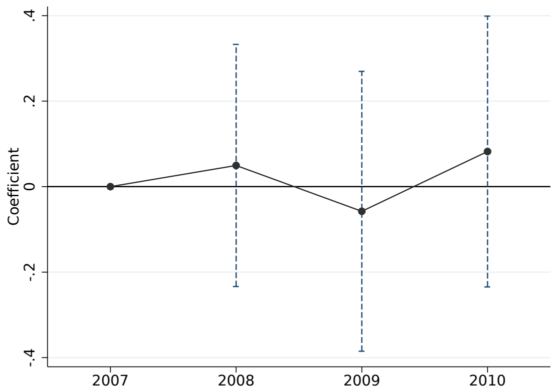
(a) *Evidence of Reporting Management from Single-Loan Relationships*



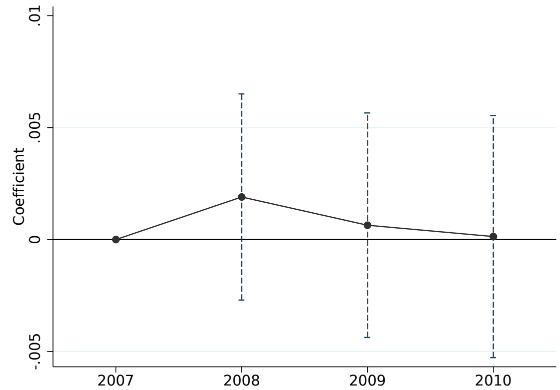
(b) *Persistence of Unrecognized Losses*

Figure 29: *Additional Results on Underreporting of Loan Losses*

Notes. Panel a compares the average constructed time overdue (x-axis) with the reported time overdue (y-axis) for the sample of relationships with a single loan. In this sample, we can track how long a loan has been overdue by simply counting the number of months the loan has been reported as overdue in the data. Panel b shows the average length of spells of overdue (or non-performing) and unrecognized losses at the firm-bank level. A spell is measured as a period of time in which a firm-bank relationship features either some overdue loan balance, and an unrecognized loss respectively. We allow reporting gaps of up to three months.



(a) *Current Ratio*

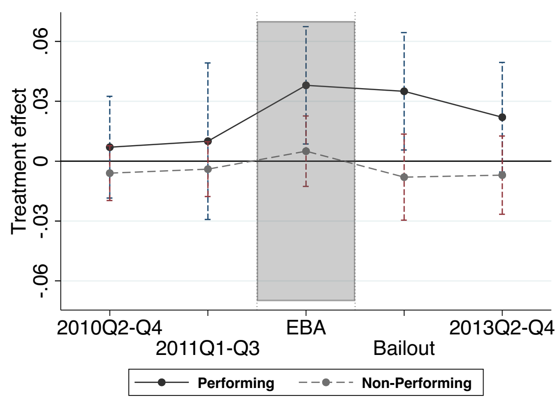


(b) *Cash/Assets*

(c) *Firm×time fixed effects*

Figure 30: *Liquidity and Credit Pre-Trends*

Notes. Panels a and b show results from a dynamic differences-in-differences specification where we interact the firm-level borrowing share from banks exposed to the EBA shock with year dummies for the period prior to the EBA shock. We run the regression in the subset of firms subject to loss underreporting. The two panels show two different liquidity measures. Standard errors are clustered at the firm-level. Panel c shows fixed effects from a regression that decomposes quarterly firm-bank credit growth into a bank and firm×quarter component. The firm×time fixed effects can be interpreted as a measure of firm-level credit demand. No NPL refers to firms without overdue loans.



(a) *Decomposition of Credit*

Figure 31: *Additional Results: Firm-Level Regression*

Notes. The graphs show regression results at the quarterly firm-level. The dependent variables are the quarterly log of performing and non-performing credit, respectively. We plot the coefficients on the interaction treatment$_i\times$quarter$_t\times$underreported$_i$, which are the treatment effects for the group of firms subject to loss underreporting. The vertical lines denote the EBA announcement and compliance deadline. The specification, equation 1.3, includes the full set of interactions, industry$\times$quarter and firm fixed effects, as well as firm-level controls interacted with quarter. All coefficients should be interpreted as changes in the dependent variable relative to the (normalized) base quarter 2011Q3. Standard errors are clustered at the firm-level. N= 1,346,771.

Chapter 1: Appendix C - Estimating Production Functions

In order to compute the aggregate productivity decomposition in chapter 1 section 4, we need to estimate firm-level technical efficiency as well as output elasticities. We use two approaches to obtain output elasticities. First, we compute 3-digit industry-level cost shares following Nishida *et al.* (2017) and Bollard *et al.* (2013). Second, we estimate the following Cobb-Douglas revenue production function at the annual firm level:

$$q_{it} = \beta_l l_{it} + \beta_k k_{it} + \beta_m m_{it} + \beta_s s_{it} + \epsilon_{it}. \tag{3.8}$$

where i indexes firms and t years. q_{it} is the log of real output, l_{it} is the log of the number of employees, m_{it} is the log of real intermediate materials, and s_{it} is the log of real services used by firm i in year t. We estimate the production separately for each 2-digit industry level, and for each 3-digit level for manufacturing firms. We winsorize all variables at the 1% level prior to taking logs.

We obtain real output by deflating firm revenue by a 2-digit industry price index, which we obtain from the Portuguese statistics office (three digit for certain manufacturing industries). For non-manufacturing industries for which no price index is available, we use alternative deflators at the 2-digit level depending on the type of industry (agricultural price deflator, consumer price index, or services price index from Eurostat). We obtain the real value of intermediate materials by deflating the cost of materials by a material input deflator from Eurostat, and proceed similarly for services. We adjust materials for the change in inventories.

We measure capital in two ways. We either use the deflated book value of fixed assets or the perpetual inventory method. The latter is computed as follows. We deflate the stock of fixed assets in 2006 (or the earliest available firm-level observation) by the 2006 capital goods deflator. We then compute the firm-level change in real fixed assets by adjusting lagged real fixed assets by the firm-level depreciation rate and adding firm-level investment spending according to the following formula:

$$k_{it} = (1 - \delta_{it})k_{t-1} + \left(\frac{I_{it}}{def_t}\right)$$

From 2009 onwards, we use CAPEX reported in the cash-flow statement when available (which is expenditure on tangible and intangible investment). Before 2009, or when CAPEX is not reported, we simply use the change in the book value of fixed assets. We deflate investment spending by the capital goods deflator.

We calculate firm-level log technical efficiency based on the gross output function as

$$\log A_{it} = q_{it} - \left(\hat{\beta}_l l_{it} + \hat{\beta}_k k_{it} + \hat{\beta}_m m_{it} + \hat{\beta}_s s_{it}\right) \tag{3.9}$$

where we either use the coefficients based on cost shares, or our estimated coefficients.

Our baseline estimates follow Wooldridge (2009). For robustness, we run two further production functions estimations. We estimate the same specification but with firm×period fixed effects, where the periods are 2005-2008, 2009-2012, 2013-2015. We also employ a translog specification, where we relax the Cobb-Douglas restrictions that the elasticities of ouptut are constant and the elasticity of substitution between inputs is one. The translog specification is given by

$$q_{it} = \sum_j \beta_j X_{it}^j + \beta_{jj} X_{it}^{j^2} + \sum j \neq k \sum_k \beta_{jk} X_{it}^j X_{it}^k + \epsilon_{it}. \tag{3.10}$$

In Table 28, we provide the average estimated elasticities for all three methods. We drop all observations where the coefficients are negative, zero or missing. Our estimates appear reasonable as the average sum of elasticities is close to 1 suggesting constant returns to scale.

Table 28: *Production Function Coefficient Estimates*

	Cost shares		Fixed assets			Inventory method	
		Wooldridge	Translog	OLS	Wooldridge	Translog	OLS
Sum	1.16	0.93	1.10	1.05	1.13	1.10	1.10
	(0.33)	(0.53)	(0.66)	(0.33)	(0.48)	(0.57)	(0.25)
Materials	0.33	0.32	0.35	0.29	0.32	0.34	0.28
	0.26	(0.19)	(0.35)	(0.11)	(0.20)	(0.34)	(0.12)
Services	0.32	0.59	0.53	0.46	0.69	0.52	0.45
	0.2	(0.25)	(0.24)	(0.11)	(0.38)	(0.25)	(0.12)
Employees	0.24	0.31	0.38	0.38	0.31	0.37	0.37
	(0.17)	(0.19)	(0.30)	(0.13)	(0.18)	(0.23)	(0.12)
Capital	0.28	0.02	0.04	0.02	0.04	0.05	0.04
	(0.27)	(0.02)	(0.07)	(0.02)	(0.03)	(0.06)	(0.02)
N	785	2590	2590	2590	2590	2590	2590

Notes. The table shows production function coefficients estimates. The first column shows coefficients based on 3-digit industry cost shares. The remaining columns are based on a gross output (revenue deflated by industry deflators) Cobb-Douglas production function specifications. We show averages across industry-level coefficients and standard errors in parentheses. Wooldridge refers to the Wooldridge (2009) methodology. OLS and translog specification refer to a OLS version adding fixed effects and a translog specification (following Petrin and Sivadasan (2013)). Fixed assets refers to the deflated book value of fixed assets to measure capital while the inventory method uses the perpetual inventory method to compute the real capital stock (see text for details).

Chapter 2: Appendix A - Additional Tables and Figures

Table 29: *Tax Credit Use in 2013*

		2013 Extraordinary Tax Credit					
		All Firms			Exporters		
		Didn't Receive	Received	Total	Didn't Receive	Received	Total
Standard Tax Benefit Programs	Didn't Receive	180,914	7,196	188,110	18,433	2,554	20,987
		95.51	*3.80*	*99.31*	*84.64*	*11.73*	*96.37*
	Received	690	612	1,302	352	439	791
		0.36	*0.32*	*0.69*	*1.62*	*2.02*	*3.63*
	Total	181,604	7,808	189,412	18,785	2,993	21,778
		95.88	*4.12*	*100*	*86.26*	*13.74*	*100*

Notes. Table 29 shows the overlap between use of regular investment tax credits and CFEI in 2013, both in terms of the number of firms and the percentage of firms in 2013. The second half of the table shows the same statistics for exporting firms (defined as any firm with positive exports in 2013). There are 189,412 firms in the sample in 2013, of which 11.5% are exporters. 4.12% of firms overall and 13.74% of exporters used CFEI.

Table 30: *Firm Characteristics by Use of Tax Credit*

	No Tax Credit	Tax Credit	All	T-statistic
Credit Risk	0.05	0.02	0.05	128.74***
	0.05	*0.03*	*0.05*	
Bankruptcy indicator	0.05	0.00	0.04	103.13***
	0.21	*0.05*	*0.20*	
Default indicator	0.06	0.00	0.06	116.48***
	0.24	*0.06*	*0.24*	
Negative Capital indicator	0.24	0.02	0.23	186.26***
	0.43	*0.15*	*0.42*	
Revenue growth	0.19	0.20	0.19	-0.28
	12.85	*3.43*	*12.62*	
Growth in fixed assets	42.13	0.66	40.35	3.10***
	7462.49	*9.08*	*7300.80*	
Cash / Assets	0.23	0.21	0.23	14.91***
	0.27	*0.22*	*0.27*	
Log(TFP)	0.77	0.70	0.77	25.48***
	0.49	*0.33*	*0.48*	
Income Tax / Assets	0.01	0.02	0.01	-26.61***
	0.02	*0.02*	*0.02*	
Age	12.90	17.32	13.05	-45.31***
	12.01	*14.11*	*12.12*	
Log(Assets)	12.19	14.34	12.26	-193.78***
	1.62	*1.60*	*1.67*	
Employees	9.31	56.96	10.95	-22.76***
	91.70	*306.40*	*106.83*	
Trade Debt / Assets	0.37	0.43	0.37	-39.92***
	0.29	*0.24*	*0.29*	
Bank Debt / Assets	0.26	0.22	0.26	32.76***
	0.26	*0.18*	*0.26*	
Public Debt / Assets	0.12	0.08	0.11	58.42***
	0.18	*0.09*	*0.18*	
Cash Flow / Assets	-0.05	0.10	-0.04	-161.96***
	0.44	*0.10*	*0.43*	
Capital / Assets	0.03	0.38	0.04	-159.25***
	1.14	*0.24*	*1.12*	
Revenue / Assets	1.34	1.40	1.34	-8.05***
	1.49	*1.14*	*1.48*	

Notes. Table 30 shows means and standard deviations for 2010-2012 values of selected firm characteristics by use of the CFEI tax credit. The last column shows t-test for difference in means. Credit risk is defined as the probability of default following the methodology described in Antunes *et al.* (2016). Default is defined as any spell of at least three months of overdue credit, where the overdue credit accounts for at least 5% of the loan volume (with a minimum of 50 euros). A firm is defined as bankrupt if it has an open process in bankruptcy court or has been liquidated. The change in fixed assets is calculated net of depreciation. TFP is estimated following the Olley and Pakes (1996) three step regression procedure which allows for endogeneity of some of the inputs, selection (due to firm exit), and long-lasting unobserved differences across firms.

Table 31: *Tax Credit Use by Debt Quartile*

	No Tax Credit	Tax Credit	All
First Quartile	53,849	3,283	57,132
	29.91%	*1.82%*	*1.82%*
Second Quartile	30,816	2,384	33,200
	17.12%	*1.32%*	*18.44%*
Third Quartile	46,377	1,335	47,712
	25.76%	*0.74%*	*26.50%*
Fourth Quartile	41,796	180	41,976
	23.22%	*0.10%*	*23.32%*
Total	172,838	7,182	180,020
	96.01%	*3.99%*	*100.00%*

Notes. Table 31 shows the number of firms in each quartile of the debt-earnings index, whose construction is described in 2.3.2, by use of the CFEI tax credit. In italics is the frequency of each group in the total firms in 2012.

Table 32: *Firm Characteristics by Debt-Earnings Quartile*

	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile	Overall
ROA	11.12%	7.58%	-4.76%	-32.11%	-3.64%
	77.13	*46.74*	*63.50*	*106.37*	*81.75*
TFP	2.76	2.52	2.38	2.05	2.45
	2.98	*14.64*	*3.04*	*3.33*	*7.21*
Labor Productivity	110408.73	110825.81	110531.77	68948.51	99864.35
	1221027.42	*1789435.92*	*685618.06*	*1282192.42*	*1312482.43*
Credit Risk	2.43%	3.66%	6.11%	9.07%	5.03%
	2.27	*2.93*	*4.25*	*5.86*	*4.63*

Notes. Table 32 shows means and standard deviations for selected firm characteristics by debt-earnings quartile (2010-2012). ROA is calculated as EBITDA over assets and labor productivity is revenue divided by the number of employees. Credit risk is defined as the probability of default following the methodology described in Antunes *et al.* (2016). TFP is estimated following the Olley and Pakes (1996) three step regression procedure which allows for endogeneity of some of the inputs, selection (due to firm exit), and long-lasting unobserved differences across firms.

Table 33: *Descriptive Statistics of Investment Survey, Selected Industries*

	Sampled Firms all years	Sampled Firms, 2012	Average Workers	Average Sales (M Euros)	Mean Investment (M Euros)	% Demand any year	% Demand 2012
Sale & repair of motor vehicles	120	39	183.55	195.95	0.83	83.87%	95.65%
Food & beverage	146	52	496	164.63	0.70	88.06%	92.59%
Mining & quarrying	46	16	56.15	82.11	0.10	92.00%	90.91%
Construction	202	58	177.45	125.83	0.33	82.24%	88.57%
Publishing	84	28	159	300.99	0.26	75.00%	85.71%
Electricity & Gas	9	3	433.67	90.12	34.35	11.11%	33.33%
Water supply	74	27	317.30	300.44	42.04	20.69%	25.00%
Mining of metal ores	77	27	189.16	80.80	3.52	11.90%	18.75%
Sewage	39	14	83.29	93.55	9.39	4.00%	0.00%
Veterinarians	9	3	16.17	238.56	0.05	0.00%	0.00%

Notes. Table 33 shows descriptive statistics from the investment survey for the five industries most affected by low demand in 2012 and the five industries least affected. Industry is defined at the two-digit level and industries with fewer than three firms surveyed are dropped from the sample. Averages were calculated as a simple average across surveyed firms in the period 2010-2012. The last two columns show the fraction of firms that mention demand as a limitation in any year and in 2012 respectively.

Table 34: *First Stage Results*

Foreign Demand	6.335***	6.420***			7.108***	7.114***
	[0.664]	[0.673]			[0.836]	[0.837]
Domestic Demand	8.712***	9.243***			10.327***	10.353***
	[0.547]	[0.574]			[0.697]	[0.698]
% Firms Citing Poor Sales			1.971***	1.985***	5.048***	5.142***
			[0.559]	[0.560]	[1.379]	[1.390]
2nd Debt Quartile	-4.181***	-4.347***	1.29	1.284	-5.767***	-5.803***
	[1.611]	[1.653]	[1.075]	[1.075]	[2.175]	[2.178]
3rd Debt Quartile	-8.394***	-9.075***	2.770**	2.763**	-8.506***	-8.566***
	[1.774]	[1.826]	[1.099]	[1.100]	[2.303]	[2.307]
4th Debt Quartile	-28.123***	-29.473***	-4.758***	-4.763***	-25.350***	-25.388***
	[2.700]	[2.774]	[1.348]	[1.348]	[3.491]	[3.497]
R-squared	0.621	0.611	0.549	0.549	0.571	0.570
Observations	15,152	14,738	74,701	74,112	14,853	14,464
Sample	All Firms	<200 employees	All Firms	<200 employees	All Firms	<200 employees

Notes. Table 34 shows results from an OLS regression where the dependent variable is ln(revenue), winsorized at the fifth percentile. The demand instruments have been normalized to have unit variance. Columns (1), (3), and (5) show results from the sample of all firms while the remaining columns restrict the sample to firms with fewer than 200 employees. All instruments were winsorized at the fifth percentile and normalized to have unit variance. Foreign Demand is a sum of imports weighted by the intensity with which a given firms trades products to each destination. Domestic demand is a corollary, as the sum of Portuguese imports weighted by the intensity with which each firm trades those products.

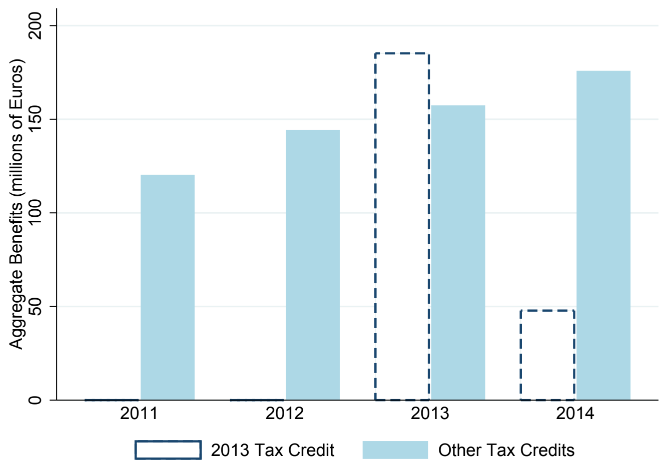


Figure 32: *Aggregate Use of Investment Tax Credits*

Notes. The figure compares the gross benefits received by firms annually from the three main investment tax benefits programs provided by the Portuguese government and the 2013 CFEI tax credit. The 2013 CFEI payout was larger than the three alternative programs combined. CFEI tax claims can be deferred until 2017 if firms did not pay sufficient income tax in 2013 to reap the entire benefit of their investment in the second semester of 2013. Credit claimed in 2014 due to deferred benefits totals about a third of the total payout in 2014.

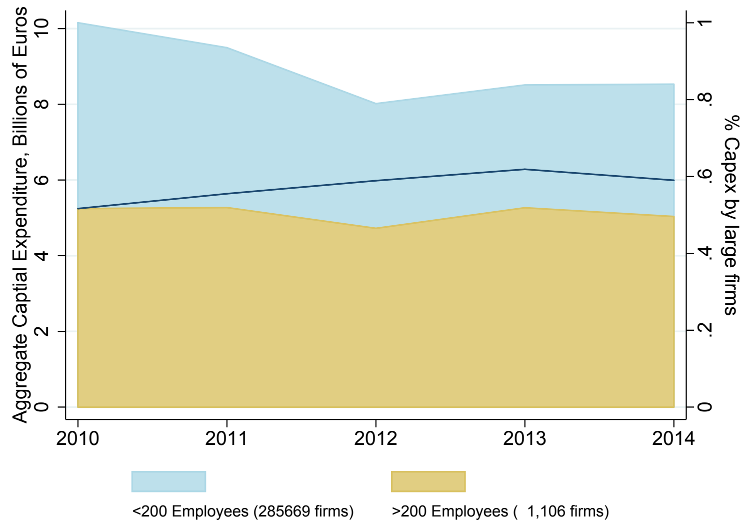
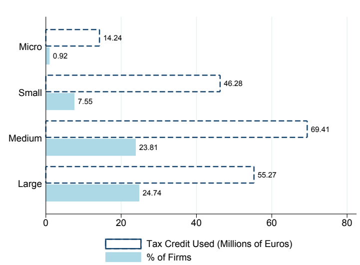
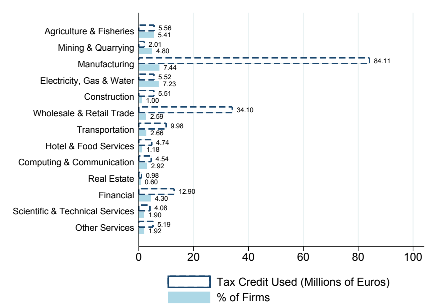


Figure 33: *Capital Expenditure, by Number of Employees*

Notes. The figure shows, on the left hand scale, the total capital expenditure reported to Portuguese firm census each year as an approximation of aggregate investment. As many micro and small firms are not required to report CAPEX, this is the lower bound of investment expenditure. The light blue area on top shows how much investment spending was by firms with fewer than 200 employees (99.6% of the sample of firms). The sand area below shows investment spending by firms with more than 200 employees. To get a sense of the changing composition of investing firms, the navy line running across the graph, on the right hand scale, shows the percent of capital expenditure by large firms (with more than 200 employees), around 60% in 2013.



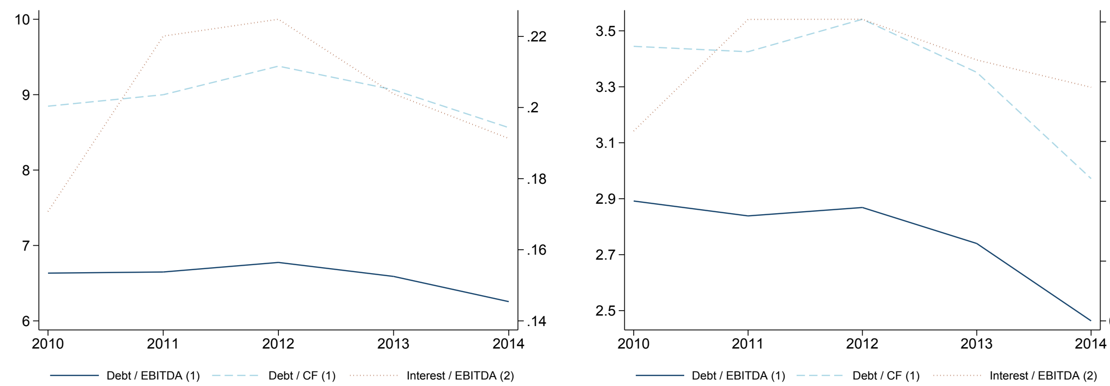
(a) *By EU Size Categories*



(b) *By Activity*

Figure 34: *Use of Investment Tax Credit by Size and Industry*

Notes. Figure 34 compares the size of the tax credit in millions of Euros (navy blue bar) and the percent of firms in each category (light blue bar) who used the 2013 extraordinary tax credit, by firm size and industry. Manufacturing and retail firms dominate use of the tax credit in terms of volume, while manufacturing and utilities have the largest percentage of firms using CFEI. Size categories are based on number of workers, sales, and total assets.

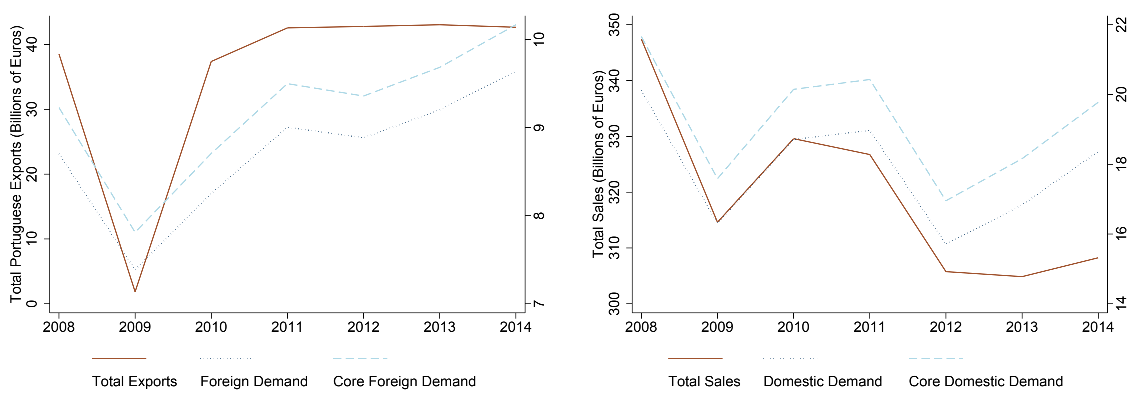


(a) *Mean*

(b) *Median*

Figure 35: *Debt-Earnings Ratios Over Time*

Notes. Figure 35 shows the mean and median value of the three debt-earnings ratios used for the debt quartiles over time. The values are shown in absolute value due to the discontinuity caused by negative EBITDA and are winsorized at the 95th percentile.

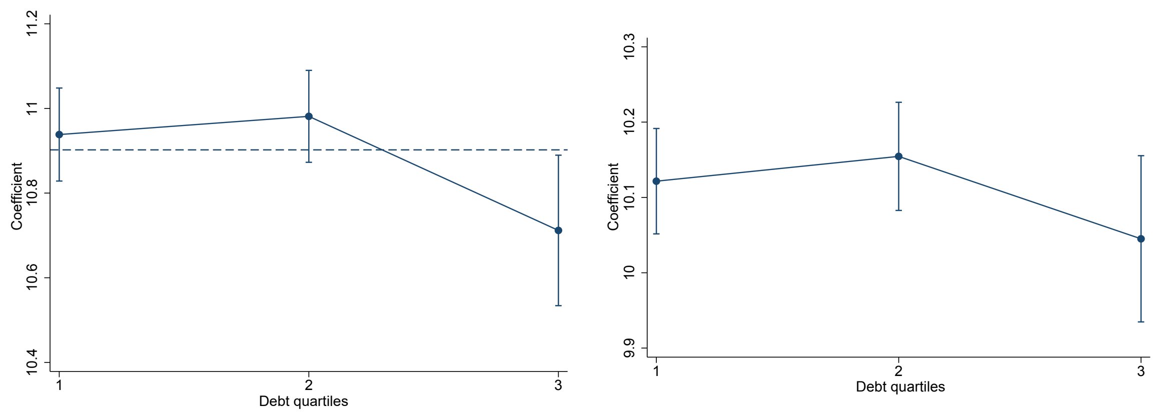


(a) *Exports*

(b) *Sales*

Figure 36: *Aggregate Sales and Demand Proxies*

Notes. The figure compares aggregate sales and exports (solid line) with our demand measures, whose construction is described in 2.3.3. We find that in both cases the aggregated measures (in the dotted and dashed lines) largely follow the trend of aggregate exports and turnover for foreign demand and domestic demand respectively.

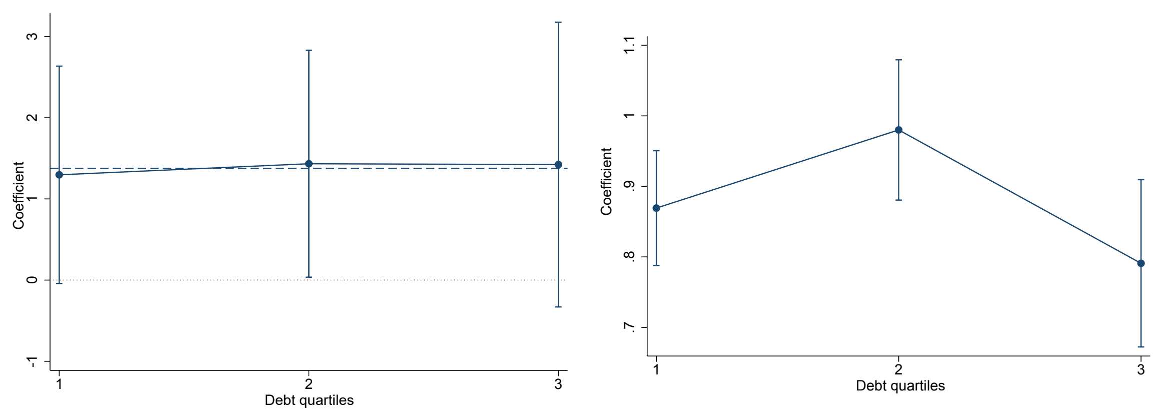


(a) *Trade Instrument*

(b) *OLS*

Figure 37: *Predicted Investment By Debt-Earning Quartile (Intensive Margin)*

Notes. The figure shows the predicted amount invested across the three debt quartile in the sample of firms that use the tax credit. We combine the third and fourth quartile due to the limited number of firms that use the investment tax credit in the higher debt quartiles. In panel (a) we show the sample of exporting firms using the foreign demand instrument. Panel (b) shows the results for OLS. The dotted line shows the average predicted investment.



(a) *Trade Instrument*

(b) *OLS*

Figure 38: *Impact of Log Sales By Debt-Earning Quartile*

Notes. The figure shows the impact of a 10% increase in predicted revenue on investment in the sample of firms that use the tax credit. We combine the third and fourth quartile due to the limited number of firms that use the investment tax credit in the higher debt quartiles. In panel (a) we show the sample of exporting firms using the foreign demand instrument. Panel (b) shows the results for OLS. The dotted line shows the average predicted investment, while the dashed line is zero.

Chapter 3: Appendix A - Yield Impact

This appendix provides details on the event study regression that we run to estimate the yield impact of the QE announcements. We regress a panel of daily yields on announcement day dummies as well as a set of control variables. Equation 3.11 shows the regression specification.

$$y_{it} = \alpha_0 + \alpha_1 \Delta q_{it} + \beta_1 D_{\text{announce}} + \beta_2 D_{\text{day post announce}} + \mathbf{x}_{it}\mathbf{fl} + \epsilon_{it} \tag{3.11}$$

where y_{it} denotes the yield of security i on day t, q_{it} the QE purchase volume.

Our dependent variables are the yields of government bonds that enter the Portuguese sovereign bond index GTPTE10Y Govt, the yields of covered bonds that enter the Portuguese covered bond index provided by iBoxx and all yields of outstanding ABS issued by Portuguese banks.[28] The controls include the risk free-rate, which we approximate by the 5-year Euro swap rate, the daily QE purchase volume and spreads of comparable securities outside the Eurozone area. For the sovereign bond yield regression, we use the Polish 10-year sovereign bond index denominated in Euro as a comparable index. We also include the yield of a Greek sovereign bond index which controls for Portuguese sovereign yield fluctuations driven by spillovers from the Greek crisis. For the covered bond regression we use the UK Euro-denominated covered bond index provided by iBoxx as a control for movements in covered bond markets unrelated to QE. For the ABS regression, we construct a comparable index from UK ABS denominated in Euro.

The results in Table 35 show that there is a large and highly significant negative impact on all three yields on the day of the announcement. For sovereign bonds and covered bonds, the effect persists on the day after the announcement. These estimates identify the causal effect of the announcement on yields as long as (a) the announcement is not expected and (b) capital is sufficiently fast-moving to affect yields within a day, and (c) there are not other concurrent shocks moving yields. In all specifications, dummies for the one and two

[28] The sovereign bond and covered bond indexes are available on Bloomberg. There is no ABS index available and we hence consider all outstanding ABS.

days ahead of the announcement are insignificant (results not reported) suggesting that announcement was not expected immediately prior to the announcement day.[29] The fact that we find a significant impact on the day after the announcement suggests that the yield impact may have been slow-moving.

The actual purchase quantities have no measurable effect on yields. However, this could be due to lack of daily variation in purchase amounts. Moreover, this specification does not identify the causal effect since we cannot rule out reverse causality (central bank purchases respond to yield movements) or omitted variables driving both purchase amounts and yields.

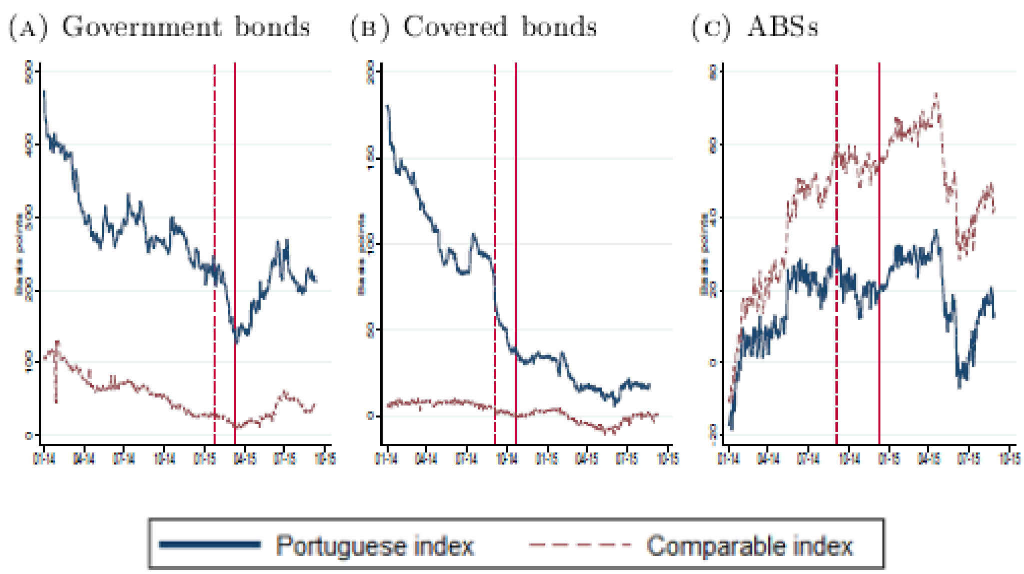


Figure 39: *Yield Spreads of Portuguese Assset Indices*

Notes. The blue (solid) line represents the spread over the risk-free rate of the Portuguese index. The red (dashed) line represents the spread of a comparable index that is not affected by QE. We use the generic euro-denominated 10-year government bond yields of Portugal and Poland in the first graph. In the second graph we compare the Portuguese and the British euro-denominated covered bond indices from iBoxx. We create euro-denominated ABS indices for Portugal and the UK using QE-eligible securities. The vertical lines represent the announcement and implementation dates of QE for each asset class.

[29]Of course, this does not rule out that QE expectations were formed at some point prior to 1-2 days ahead of the announcement and the effect was already priced in at that time. This would lead us to underestimate the true announcement effect.

Table 35: *Event Study Regression*

	(1) PSPP	(2) PSPP	(3) CBPP3	(4) ABSPP
Announcement	-17.20***	-13.55***	-7.097***	-0.928**
	(1.988)	(2.054)	(0.522)	(0.186)
Day After	-13.47***	-8.543***	-9.045***	0.472
Announcement	(1.587)	(1.714)	(2.277)	(0.637)
Δ risk-free	0.356***	0.637***	0.452***	0.112**
	(0.0281)	(0.0471)	(0.137)	(0.0282)
Δ PSPP	0.00790**	-0.00116		
	(0.00273)	(0.00289)		
Δ spread	0.0803***	0.0582***		
comparable PSPP	(0.0176)	(0.0133)		
Δ Greek		0.0660***		
control		(0.00329)		
Δ CBPP3			0.00236	
			(0.00270)	
Δ spread			0.548***	
comparable CBPP3			(0.131)	
Δ ABSPP				-0.000241
				(0.000418)
Δ spread				0.130**
comparable ABSPP				(0.0375)
N	3,528	3,528	5,292	3,087
Nr of securities in index	8	8	12	4

Notes. The table shows results from an event study regression. Our dependent variables are the yields of government bonds that enter the Portuguese sovereign bond index GTPTE10Y Govt, the yields of covered bonds that enter the Portuguese covered bond index provided by iBoxx and all yields of outstanding ABS issued by Portuguese banks. Controls include the risk free-rate, which we approximate by the 5-year Euro swap rate, the daily QE purchase volume and spreads of comparable securities outside the Eurozone area. For the sovereign bond yield regression, we use the Polish 10-year sovereign bond index denominated in Euro as a comparable index. We also include the yield of a Greek sovereign bond index which controls for Portuguese sovereign yield fluctuations driven by spillovers from the Greek crisis. For the covered bond regression we use the UK Euro-denominated covered bond index provided by iBoxx as a control for movements in covered bond markets unrelated to QE. For the ABS regression, we construct a comparable index from UK ABS denominated in Euro. The dependent variables are spreads on securities purchased by the ECB. Announcement refers to the day when the respective program is announced.

Chapter 3: Appendix B - Additional Tables and Figures

Table 36: *QE Announcement and Implementation Dates*

Program	**Announcement**	**Implementation (Portugal)**
CBPP3	Sep. 4 2014	Oct. 20 2014
ABSPP	Sep. 4 2014	Nov. 21 2014
PSPP	Jan. 22 2015	Mar. 9 2015

Notes. The table shows the announcement and implementation dates of different QE programs in Portugal.

Table 37: *Transition Matrix for Exposure*

Max quartile	**1**	**2**	**3**	**4**
Min quartile = **1**	35	2	2	8
Min quartile = **2**	0	0	1	7
Min quartile = **3**	0	0	0	0
Min quartile = **4**	0	0	0	8

Notes. The table shows the transition matrix for banks moving across quartiles between January and August 2014. Quartiles are defined by QE-eligible asset holdings scaled by total assets. Quartiles are credit-weighted. Quartiles are used to define bank exposure to QE via the balance sheet channel.

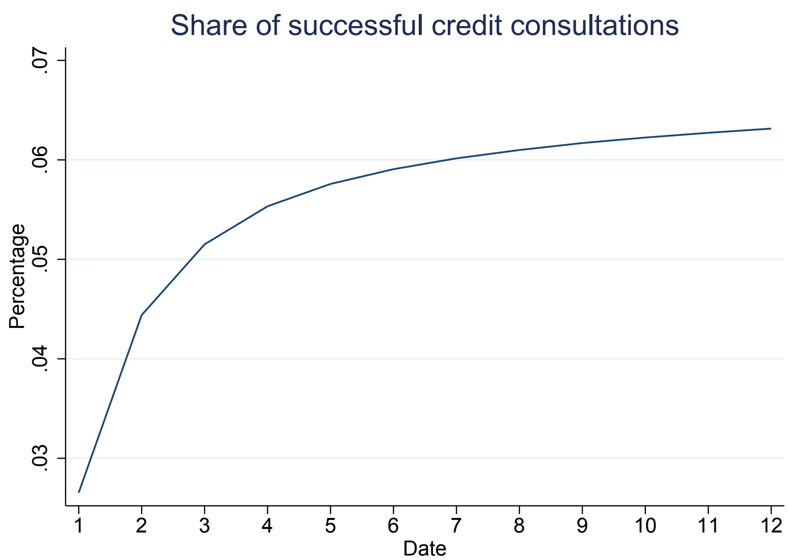


Figure 40: *Approved Loan Consultations*

Notes. The figure displays the percentage of loan consultations that get approved t months after the consultation is made.

Table 38: *Origination of ABS and Covered Bonds Post-QE*

Issuer/originator	Type	Date of issuance	Amount issued	Eligible (8 Jan 2016)
Banif	ABS	30-09-2014	465	Yes
Banif	ABS	30-09-2014	186	No
Banif	ABS	30-09-2014	180	No
Banif	ABS	30-09-2014	55	No
Banif	ABS	30-09-2014	41	No
Banif	Covered bond	27-10-2014	50	Yes
Banco Popular	Covered bond	30-12-2014	290	Yes
CGD	Covered bond	27-01-2015	1,000	Yes
Santander Totta	Covered bond	04-03-2015	750	Yes
BPI	Covered bond	30-03-2015	1,250	Yes
Montepio	ABS	03-06-2015	546	Yes
Montepio	ABS	03-06-2015	399	No
Montepio	ABS	03-06-2015	87	No
Montepio	ABS	03-06-2015	76	No
Montepio	ABS	03-06-2015	16	No
Banif	ABS	07-06-2015	440	Yes
Banif	ABS	07-06-2015	173	No
Banif	ABS	07-06-2015	164	No
Banif	ABS	07-06-2015	36	No
Banif	ABS	07-06-2015	33	No
Banco Popular	Covered bond	30-06-2015	225	Yes
Credibom	ABS	21-07-2015	500	Yes
Credibom	ABS	21-07-2015	146	No
Banco Popular	Covered bond	28-09-2015	300	Yes
Novo Banco	Covered bond	07-10-2015	1,000	Yes
Novo Banco	Covered bond	07-10-2015	1,000	Yes
Novo Banco	Covered bond	07-10-2015	1,000	Yes
Novo Banco	Covered bond	07-10-2015	700	Yes
BPI	Covered bond	07-10-2015	200	Yes
BPI	Covered bond	07-10-2015	100	Yes
Santander Totta	Covered bond	27-10-2015	750	Yes
Montepio	Covered bond	09-12-2015	500	Yes

Notes. The table shows new issuances of ABS or covered bond by bank.

Table 39: *Balance Sheet Channel: Intensive Margin*

	1	2	3	4	5
Exp × Post	0.048	0.100*	0.045	0.039	0.027
	(0.041)	(0.051)	(0.048)	(0.041)	(0.074)
Observations	6859464	6859464	6856570	6856570	6856570
Bank FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No	No
Borrower FE	No	Yes	Yes	No	No
Borrower×Post FE	No	No	No	Yes	No
Borrower×Date FE	No	No	No	No	Yes
Controls	No	No	Yes	Yes	Yes

Notes. Results from difference-in-differences regression. The dependent variable is the logarithm of firm-bank loan volume. Exposed and control groups are defined as the highest and lowest (credit-weighted) quartiles of the pre-QE holdings of eligible securities as a share of assets. The post period begins in January 2015, the month of the ECB's official QE announcement, and end ins June 2015. Coefficients should be interpreted as basis points. Some control variables (see text) are omitted to conserve space. Standard errors are clustered at bank-level.

Table 40: *Balance Sheet Channel: Interest Rates*

	1	2	3	4	5
Exp × Post	-0.415	-0.135	-0.240	-0.398	-0.338
	(0.259)	(0.207)	(0.385)	(0.349)	(0.411)
Observations	699,504	699,504	346,602	346,602	346,602
Bank FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No	No
Borrower FE	No	Yes	Yes	No	No
Borrower×Post FE	No	No	No	Yes	No
Borrower×Date FE	No	No	No	No	Yes
Controls	No	No	Yes	Yes	Yes

Notes. Results from difference-in-differences regression. The dependent variable is the loan-level interest rate on a new corporate loan (measured on a scale of 0-100). Exposed and control groups are defined as the highest and lowest (credit-weighted) quartiles of the pre-QE holdings of eligible securities as a share of assets. The post period begins in January 2015, the month of the ECB's official QE announcement, and end ins June 2015. Some control variables (see text) are omitted to conserve space. Standard errors are clustered at bank-level.