EUROPEAN CENTRAL BANK

EUROSYSTEM

Working Paper Series

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When losses turn into loans: the cost of undercapitalized banks

ECB - Lamfalussy
Fellowship Programme

No 2228 / January 2019

ECB Lamfalussy Fellowship Programme

This paper has been produced under the ECB Lamfalussy Fellowship programme. This programme was launched in 2003 in the context of the ECB-CFS Research Network on “Capital Markets and Financial Integration in Europe”. It aims at stimulating high-quality research on the structure, integration and performance of the European financial system.

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Abstract

We provide evidence that a weak banking sector has contributed to low productivity growth following the European sovereign debt crisis. An unexpected increase in capital requirements for a subset of Portuguese banks in 2011 provides a natural experiment to study the effects of reduced bank capital adequacy on productivity. Affected banks respond not only by cutting back on lending but also by reallocating credit to firms in financial distress with prior underreported loan loss provisioning. We develop a method to detect when banks delay loss reporting using detailed loan-level data. We then show that the credit reallocation leads to a reallocation of production factors across firms. A partial equilibrium exercise suggests that the resulting increase in factor misallocation accounts for 20% of the decline in productivity in Portugal in 2012.

JEL classification: G21; G38; E51; D24; O47

Keywords: bank capital, productivity, misallocation, banking regulation, non-performing loans

Non-technical summary

Many European banks were left with little regulatory capital following the 2008 global financial crisis and the subsequent sovereign debt crisis in Europe due to the large losses they had suffered. Banks with little regulatory capital can pose a threat to the economic recovery when they begin to channel credit to the wrong firms in the economy. Japan in the 1990s is often cited as an example of weak banks continuing to lend to nearly-insolvent 'zombie' firms, crowding out lending to more productive firms. With Europe following the Japanese pattern of a sluggish economic recovery, this paper addresses the key question of whether weak banks impede economic recovery and growth.

We provide evidence that a weak banking sector has contributed to a slow recovery in Europe through its negative effects on productivity growth. To establish this result, we exploit a regulatory intervention by the European Banking Authority in 2011 which forced a subset of European banks to comply with more demanding capital requirements. Using administrative data from Portugal, we show that affected banks respond to the new requirements by reducing both the overall amount of lending but, importantly, also by channeling more credit to firms in financial distress whose loan losses banks had been underreporting. We develop a method that allows us to detect where banks engage in such underreporting of losses on non-performing loans. Two potential mechanisms may explain our results: First, lending to firms with underreported losses is a way of delaying the recognition of additional losses that would further eat into the bank's regulatory capital. Cutting lending to an underreported firm runs the risk of pushing that firm into insolvency, which would force the bank to recognize previously underreported losses. Second, an expected bailout by the Portuguese government may have given banks an incentive to gamble for the survival of distressed firms. Our data, which covers the balance sheets of all Portuguese firms and banks as well as their lending relationships, also allow us to study how the changes in credit allocation affect how efficiently capital, labor and other production inputs are allocated across firms. Using a theoretical framework that incorporates our empirical results, we show that this type of credit misallocation leads to a misallocation of production inputs which translates into substantially lower aggregate productivity growth.

Our results highlight the importance of ensuring that the financial sector builds up sufficient capital buffers in pre-crisis times to withstand losses during a crisis. The results also suggest that specifying capital requirements in terms of levels rather than in terms of the ratio of capital to (risk-weighted) assets may avoid some of the distorted behavior described in this paper. Finally, our results also point to the need to carefully monitor not only the overall amount of credit supplied by banks but also who this credit is flowing to. Even with the level of credit supply unchanged, a deterioration in the efficiency of credit allocation can have sizable negative effects on aggregate productivity. This is especially true in highly bank-dependent economies.

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. While much research in recent years has focused on frictions in the banking system limiting the overall supply of credit to the economy, little attention has been paid to how these frictions affect the composition of credit supply. At the same time, a growing body of evidence has highlighted the link between factor misallocation and slow productivity growth but not linked the increase in factor misallocation to an increase in credit misallocation induced by frictions in the banking system.

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. To establish this causal chain, 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 lending choices at the micro-level driving a misallocation of production factors across firms which aggregate up to a negative effect on productivity at the macro level.

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 European banks had to meet certain capital ratios by mid-2012, which substantially affected a subset of Portuguese banks. Our exposure definition exploits both eligibility, which was based on a bank size cut-off, and the severity of the capital shortfall, which was determined by prior sovereign bond holdings.[2]

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

[2]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

As long as banks 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. 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 delaying of loan losses. Following European regulation, the Portuguese central bank since 2005 required all banks under its supervision to report loan impairments in consolidated statements according to the incurred loss model prescribed by the international accounting standards (IAS).[3] When the sovereign debt crisis hit the Portuguese economy in 2011/2012, banks faced a drastic deterioration of loan quality. Under the IMF-EU assistance program and aware of this likely development, the authorities designed and implemented a suite of supervisory actions aimed at assessing the impairment amounts recorded by the eight largest banking groups.[4] Overall, almost 4 billion euro of underreported impairment losses were detected and had to be accounted for by banks as a result of these inspections. Throughout this period, and until 2015, the Portuguese central bank kept in place a rule that establishes provisions for non-performing loans reported in banks' individual statements.[5] This rule ties the size of provisions, which are relevant for tax purposes, to the time a loan has been behind on repayment. Importantly, the supervisory rules required banks to deduct from own funds the difference between the sum of provisions computed on an individual basis and impairments from the consolidated accounts. Thus, any bank deliberately underestimating impairments reported under IAS39 in its consolidated statements would not want to

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.

[3]On the balance sheet, impairment losses mark down the value of the asset and reduce banks' capital.

[4]For more information, see "Special inspections program results": https://www.bportugal.pt/sites/default/files/anexoscombp20111216_en.pdf; "On-site inspections programme on exposure to the construction and real estate": https://www.bportugal.pt/sites/default/files/anexoscombp20121203_en.pdf, "Credit portfolio impairment review exercise confirms the resilience and robustness of the national banking system regarding regulatory own funds": https://www.bportugal.pt/en/comunicado/credit-portfolio-impairment-review-exercise-confirms-resilience-and-robustness-national and "Results of the business plan analysis carried out on the banking system's main clients (ETRICC 2)":(https://www.bportugal.pt/en/comunicado/results-business-plan-analysis-carried-out-banking-systems-main-clients-etricc-2.

[5]Loan loss provisions are a standard accounting adjustment made to a bank's loan loss reserves included in the financial statements of banks.

show a high disparity relative to the provisioning for non-performing loans. Therefore, we conjecture that banks underestimating the recognition of impairment losses under IAS 39 have incentives to also underreport overdue loans, thereby delaying the record of provisions in individual statements.

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 loss provisions banks had been underreporting prior to the EBA announcement.[6] In contrast, exposed banks do not increase credit to distressed firms that are not underreported. These results are estimated in a difference-in-difference design, in which we compare changes in credit from exposed and non-exposed banks to the same firm. We show that this credit reallocation is unlikely to be driven by increased credit demand from underreported firms. Exposed banks change their credit allocation only in the period between the EBA announcement and the EBA deadline. Firm-level shocks driving up credit demand would hence have to match the exact timing of the regulatory intervention to be able to account for our results. Moreover, given that we compare changes in lending to the same firm, firm-level shocks would have to drive up credit demand at exposed but not at non-exposed banks. To lend further credibility to our results, 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 our results are robust to controlling for relationship characteristics such as whether the bank is the main lender.

A natural explanation for the observed changes in credit composition is that the EBA intervention heightens distorted lending incentives for exposed banks. The first lending incentive is driven by exposed banks attempting to delay the recognition of loan loss provisions and presumably also of impairment losses with implications for capital. We show that banks had been underreporting overdue loans with the onset of the European sovereign debt crisis in 2010. This underreporting also locks banks into a vicious cycle with financially distressed firms whose loan loss provisioning have not yet been fully accounted for on banks' financial statements. Cutting lending to an underreported firm

[6] Underreporting in this context is defined as delay in recording non-performing loans, which implies delay in recording loan loss provisions under Notice 5/95 and should not be interpreted as misreporting of losses.

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 give exposed banks an additional reason to avoid capital-reducing losses and to roll over loans to underreported firms. Consistent with this incentive to delay losses, we find that exposed banks sharply increase the amount of underreporting for the duration of the EBA intervention. The second lending incentive arises as exposed banks gamble for the resurrection of distressed borrowers in anticipation of the government bailout.

We establish the second link in the causal chain by showing how the changes in credit composition affect the firm-level use of production factors. We first run a firm-level version of our firm-bank specification to confirm that firms do not undo the firm-bank level credit shocks by substituting among different lenders. In the next step, we estimate the effect of the credit shock on factor use by instrumenting for the firm-level credit shock with the firm-level pre-intervention 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 effect on the use of labor, capital, and intermediate inputs. A one euro change in credit supply leads firms to adjust their labor spending by 16 cents, their investment spending by 40 cents, and their spending on materials and services, which capture intermediate inputs, by 14 cents and 29 cents respectively. In addition to these intensive margin effects, we find that the credit shock significantly decreases the likelihood of underreported firms exiting, while increasing the likelihood of exit for all other firms.

In the final step of the causal chain, we show that the changes in firms' factor use matter for aggregate productivity. Following Petrin and Levinsohn (2012), we decompose total productivity growth into firm-level growth rates of TFP and a term that captures how efficiently production factors 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 causes capital to be 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 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 in the same industry that do not borrow from EBA banks.

Our work is related to a growing body of literature that documents how frictions in the banking system limit the supply of credit to firms using quasi-experimental variation in bank health (Klein et al. (2002), Khwaja and Mian (2008), Chodorow-Reich (2014), Amiti and Weinstein (2018)). In particular, our paper is related to papers using variation in regulatory rules to study effects on bank behavior (Koijen and Yogo (2015), Gropp et al. (2017)). While we confirm the finding that banks reduce credit supply in response to changes in (regulatory) frictions, our primary contribution lies in documenting the effects on credit composition arising from distorted lending incentives and the resulting effects on aggregate productivity.

Our paper is also related to an earlier literature on 'zombie' lending in Japan, which has received renewed interest following Europe's experience since 2008 (see Sekine et al. (2003) for a survey on Japan). One strand of this 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 causality (Peek and Rosengren (2005), Schivardi et al. (2017), Albertazzi and Marchetti (2010), and Acharya et al. (2017)). Beyond introducing quasi-experimental variation in bank capital adequacy to establish causality, we more precisely estimate the extent of 'zombie' lending by relying on our underreporting measure instead of measures of poor firm performance. We show that distorted lending is present only for the subset of poorly performing firms whose overdue credit had been underreported by the bank. This implies that estimating the change in credit across all poorly performing firms would underestimate the extent of 'zombie' lending. An additional advantage of our approach is that we show how banks' underreporting of risk, documented in other contexts for example by Behn et al. (2016) and Begley et al. (2017), changes who banks allocate credit to.

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 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), McGowan 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).

Conceptually, we build on a large literature studying how frictions 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 behavior consistent with risk-shifting by undercapitalized banks (Acharya and Steffen (2015), Drechsler et al. (2016), Crosignani (2017), Bonaccorsi and Kashyap (2017)).

Finally, we contribute to the literature on misallocation by tracing the causal impact of a policy change on misallocation and aggregate productivity. The misallocation of production factors has been proposed as a key cause of low productivity and slow economic growth (Restuccia and Rogerson (2008), Hsieh and Klenow (2009)). A growing number of papers have suggested that firm-level financial frictions are an important driver of misallocation (Gopinath et al. (2017), Moll (2014), and Midrigan and Xu (2014)). However, there has been a lack of quasi-experimental studies providing evidence of such a causal channel. We fill this gap by showing that bank-level frictions affect financing conditions for firms, which in turn drive the misallocation of production factors. We hence provide evidence of direct channel through which banks contribute to the misallocation of factor inputs.

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.

2 Loss Underreporting: A Tool to Measure Distorted Lending Incentives

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

2.1 Loan Loss Reporting in Portugal

We make use of the rules that regulate the reporting of loan loss provisioning and loan impairment losses in Portugal to construct our measure of underreporting. Since 2005, the Portuguese central bank required all banks under its supervision to report loan impairments in consolidated statements according to the incurred loss model prescribed by the international accounting standards (IAS). Under the IMF-EU assistance program that was implemented in the wake of the sovereign crisis of 2011, the authorities addressed the underlying deterioration of credit quality with, among other measures, a suite of supervisory actions aimed at assessing impairments recorded by the main Portuguese banking groups, which revealed the true dimension of the problem. Throughout this period, and until 2015 the Portuguese central bank kept in place a rule that establishes provisions for non-performing loans reported in banks' individual statements (Notice 3/95). Provisions reported in individual statements are relevant for tax purposes. Moreover, the supervisory rules require banks to deduct from own funds the difference between the sum of provisions computed on an individual basis and impairments from the consolidated accounts. Thus, any bank deliberately underestimating impairments reported under IAS39 in its consolidated statements would not want to show a high disparity relative to provisioning for non-performing loans. Therefore, we conjecture that banks underestimating the recognition of impairment losses under IAS 39 have incentives to also underreport overdue loans, thereby delaying the record of provisions in individual statements.

Notice 3/95 ties the size of loan loss provisions to the time a loan has been behind on repayment. We exploit the detailed reporting of overdue loans by banks 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.[7] Banks report the time overdue in discrete intervals, or buckets, which correspond to the regulatory buckets in Notice 3/95 shown in Figure 1.

We focus on firm-finance loans granted to non-financial firms. Firm-finance loans tend to have longer maturities than some other credit products, such as credit cards, and therefore are better suited for detecting overdue credit 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. As 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. Table 11 in Appendix B presents 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.

2.2 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

[7]Banks start reporting this variable in 2009.

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.[8] 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 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

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

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. Banks use three mechanisms to adjust the reported time overdue: (a) they do not update the reported time, (b) they combine new overdue loan installments with the existing overdue loan balance and report a (lower) average time overdue,[9] and (c) they grant new performing credit in exchange for the repayment of the longest overdue portion of the loan. In Appendix A, we show that most underreporting is driven by the latter two types of behavior.[10] A potential concern is that the first two patterns may simply reflect cases in which, each month, the firm repays an overdue installment but a new one falls overdue. However,

[8] 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.

[9] 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.

[10] 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.

in such cases we should observe a reduction in the performing credit balance. Yet for 70% of observations that feature underreporting, the performing credit balance remains unchanged. Moreover, we now present several validity checks that suggest that underreporting responds to the incentives inherent in the regulatory rules, which is inconsistent with underreporting being driven by normal accounting practices.

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. 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.[11] 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 evidence that our measure is indeed picking up strategic behavior.[12]

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

[11] 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.

[12] 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.

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 Table 9 in Appendix A). 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.[13]

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. Table 9 in Appendix A shows 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.

2.3 Underreporting as a Tool to Measure Distorted Lending Incentives

Underreporting of loan losses is a powerful tool to identify lending driven by distorted incentives. We argue that the underreporting of loan losses is correlated with two types of distorted lending incentives: the delayed recognition of losses and risk-shifting.

The incentive to delay losses arises since reported losses reduce the bank's regulatory capital position. 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 their regulatory capital position. One such way is to delay the reporting

[13] 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.

of losses by rolling over loans to previously underreported firms, even if such loans have negative net present value (NPV). If a bank cuts lending to an underreported firm, it runs the risk of pushing the firm into insolvency and having to recognize the entire unreported 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)).

In line with this mechanism, we find that banks delay losses in relationships that have large uncovered losses in the case of firm insolvency: 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 (see Table 11 in Appendix B).[14]

The second type of distorted lending incentives arises due to risk-shifting. 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 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)).[15] Banks simultaneously have an incentive to reduce reported loan losses on these firms to avoid a potential monitoring of these loans by financial markets or the financial regulator.

In line with this second 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

[14] Un-collateralized loans have a more front loaded regulatory deduction schedule making underreporting more valuable relative to collateralized loans. In addition, to the extent that banks anticipate having to roll over loans to underreported firms, 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.

[15] This theory has recently received attention in the context of the European sovereign debt crisis (Acharya and Steffen (2015), Crosignani (2017)).

on equity, residualized on year, industry, firm age, district and size.

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 Figure 3).[16] 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. This implies that estimating the average effect for all poorly performing firms, as done in the existing literature, would underestimate the true extent of 'zombie' lending

Potential shortcomings 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 already have some overdue loans. It does not capture cases where a bank prevents a firm from falling overdue by granting loans that allow a firm to stay current on loan repayment. However, in the time period we study, a large number of firms have overdue payments in the data (see Figure 3), implying that we capture a large fraction of lending in the economy.

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 has 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 Table 11 in Appendix B).

[16] 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. The average duration of a spell of underreporting is 20 months.

Second, we compare long-run outcomes for underreported and non-underreported firms. In Figure 13 in Appendix B, 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×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.

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.

3.1 The 2011 EBA Special Capital Enhancement Exercise

In October 2011, the European Banking Authority (EBA)[17] 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.[18] 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 requirements were "well beyond the current expectations of banks and analysts".[19] The intervention led to a large capital shortfall for most eligible Portuguese banks since their Eurozone sovereign debt holdings were substantial and often valued above market prices in their balance sheets.[20]

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

[18]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).

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

[20]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 below median sovereign debt holdings in the group of large banks (and therefore 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.

Our identification strategy rests on the assumption that there are no unobserved differences between the two groups of banks that could drive the observed credit allocation during the EBA intervention. Table 2 shows that the groups are balanced on observables prior to the EBA intervention — though some imbalance on size remains given the selection criteria of the EBA intervention. In particular, we show that both groups of banks made similar use of the ECB's long-term refinancing operations (LTRO), which were also introduced in late 2011. To further address potential confounding effects from the LTRO operations, we show that our results are robust to controlling for the loans that banks obtained under the LTRO program. Another potential concern is that Eurozone sovereign debt holdings may be correlated with unobserved differences across eligible banks. Figure 6a shows that there are no systematic differences in the Eurozone sovereign debt holdings of *eligible* banks both before and during the EBA intervention, suggesting that differences in debt holdings are unlikely to reflect short-run shocks that could also affect credit allocation.[21] Figure 6b shows that while there was considerable stress in sovereign debt markets during this time, the peak in Portuguese sovereign debt spreads does not match the timing of the EBA intervention. This suggests that events in sovereign debt markets are unlikely to account for our results.

The EBA intervention temporarily heightened two sources of distorted incentives for exposed banks. First, exposed banks wanted to comply with the higher capital ratios

[21] The increase in sovereign debt holdings in both groups at the end of 2011 may be driven by the fact that all large banks purchased sovereign debt as collateral to access the LTRO program (Crosignani et al. (2018)).

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.[22] Figure 5 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 to increasing their underreporting, 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.

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.

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

[22] It is important to note that the EBA requirements applied at an consolidated level while impairment losses under Notice 3/95 applied at an individual level. However, as explained in section 2, banks likely avoided noticeable discrepancies in the loan loss reporting between consolidated and individual statements.

[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`

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.

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.

3.3.1 Credit Effects at the Firm-Bank Level

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

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

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 intervention. This dummy is based on our measure of underreporting described in section 2.

period$_\tau$ is a dummy that indexes periods of three quarters. The periods of interest are the EBA intervention (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: $g_{ibt}^{\text{credit}} = \text{credit}_{ibt}/\text{credit}_{ib,t-1} - 1$. 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.[28] This decomposition is important to rule out that observed changes in total credit are driven solely by some firms paying down overdue credit and underreported firms accumulating more overdue credit.

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

[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.

[28]Results, available upon request, show that results are similar when using the log changes.

intervention increased distorted lending incentives for exposed banks. We therefore 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 Table 12 in 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 2 is positive and strongly significant during the EBA intervention. This positive treatment effect for underreported distressed firms contrasts with the reduction in credit supply for all other lending relationships at exposed banks. The coefficient on $\text{EBA}_t \times \text{exposed}_b$ in equation 2 is negative and statistically significant (Figure 7a and columns 2 and 3 of Table 12).

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 firms is an increase in credit growth at exposed banks of just over 2 p.p.[29] These changes are equivalent to 4% of a standard deviation of credit growth.

If loss underreporting correctly identifies firms which benefit from additional lending due to banks' distorted incentives, 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 2 but replacing the triple interaction with the subgroup of firms that have overdue loans but are not subject to underreporting

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

prior to the intervention. 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.

The results suggest that the 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 intervention, 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 firms. Second, the preferential credit treatment for underreported firms only occurs during the period of the EBA intervention when exposed banks face heightened distorted lending incentives. Similarly, the credit crunch only occurs in the period of the EBA intervention when banks attempt to comply with tighter capital requirements. 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 firms in the periods after the EBA intervention. This suggests that banks do not withdraw the additional credit granted during the EBA intervention following the EBA deadline. We provide a series of further robustness checks in Table 13 in Appendix B.[30]

The results suggest that banks actively change their lending behavior during the EBA intervention. The change in total credit is almost entirely driven by performing credit (column 4 of Table 12 in Appendix B). If underreported 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. Moreover, we find 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] Column 6 of Table 12 in Appendix

[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, excluding relationship controls, and including the LTRO take-up.

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

B shows that we find a large significant increase in the probability that a new loan is granted to a underreported firms at exposed banks in the period of the EBA intervention. 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 intervention (Table 14 in Appendix B).[32] In contrast, the probability falls for underreported firms.[33]

3.3.2 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 dynamic differences-in-differences specification[34]

$$\begin{aligned}\Delta \log \text{credit}_{it} = &\sum_{t=-5}^{10} \delta_t^{treatgroup} \left(\text{quarter}_t \times \text{treatment}_i \times \text{underreported}_i\right) \\ &+ \sum_{t=-5}^{10} \delta_t^{treatment} \left(\text{quarter}_t \times \text{treatment}_i\right) + \text{controls} + \alpha_1 X_{it} + \theta_i + \varepsilon_{it}\end{aligned} \tag{3}$$

where treatment_i is the firm-level borrowing share from exposed banks prior to the intervention.[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. underreported_i is a dummy that captures firms with underreported

[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 intervention.

[34] For papers using the same diff-in-diff specification see for example Jäger (2016) and Jaravel et al. (2015).

[35] Following Chodorow-Reich (2014), this variable 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.

losses prior to the announcement of the intervention. 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 Figure 15 in Appendix B). This result suggests that banks can stave off additional default for underreported firms in the short-run but not in the medium to long-run. 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.

The economic significance of the credit reallocation is large. For underreported 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).[36] For all other firms, the total treatment effect is a decline in

[36]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 moving

credit of 14% relative to the base quarter.

3.3.3 Effects into Employment and Investment

We use an instrumental variable design to estimate the pass-through of the credit shocks into firm-level employment and investment in 2012.

$$y_{is} = \gamma \Delta \log \text{credit}_{is} + \text{controls} + u_{is} \tag{4}$$

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 intervention. We include the same controls as in the firm-level credit specification, equation 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.[37] 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[38] and a 11% pass-through into employment (see Table 3). If we allow for the effect of exit,

from borrowing entirely from exposed to borrowing entirely from non-exposed. For underreported firms, this is the total treatment effect $\beta_\tau^{treat} + \beta_\tau^{treatgroup}$ in equation 3, or in other words, we add the two coefficients displayed in Figure 8a.

[37]The formula is

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

[38]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 3 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).

the pass-through into employment jumps to 60%. The first-stage F-statistics are close to 200, comfortably above the Stock and Yogo (2005) criterion for 5% maximal bias.

The real effects of the EBA intervention persist into 2013 but dissipate in 2014 (see Table 15 in Appendix B). However, it is difficult to precisely estimate the long-run pass-through since the credit intervention is short-lived and hence the instrument loses power after 2012. We also conduct placebo exercises running the same specification in the years prior to the intervention and find no significant effects (see Table 15 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.[39] For all other firms, the equivalent calculation implies a decline in employment and investment of 9% and 6%, respectively.

3.3.4 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 an increase in credit demand by underreported firms. 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

[39] A standard deviation in the borrowing share in our sample is the equivalent to moving from borrowing entirely from exposed to 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).

for a larger share in the EBA banks' loan portfolio. These differences likely 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 observed 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 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 with liquidity ratios as the dependent variable. Figures 14a - 14b in Appendix B show that there are no pre-trends in either the current ratio or the cash/assets ratio for these firms.

One remaining potential issue is that underreported firms may be aware of their special status and also aware of the EBA intervention affecting their lender. Firms could use the intervention 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.

4 Measuring the Effect on Misallocation and Productivity

In this section, we quantify the effects of changes in credit allocation 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)). We use 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.

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.[40]

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

$$Y_i = Q_i - \sum_{\mathrm{x} \in M,S} \mathrm{input}_{xi}. \tag{5}$$

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):

$$APG \equiv \sum_i P_i dY_i - \sum_i \sum_{\mathrm{x} \in K,L} W_{xi} d \ \mathrm{input}_{xi} \tag{6}$$

where W_{xi} denotes the price of input x for firm i and K and L index capital and labor.[41]

[40]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.

[41]This expression is in terms of final demand, which already incorporates the effect of changes in

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

$$APG = \underbrace{\sum_{i=1}^{N} D_i d \log A_i}_{\text{TFP}} + \underbrace{\sum_{i=1}^{N} D_i \sum_{x \in K,L,M,S} (\epsilon_{xi} - s_{xi})\, d \log \text{input}_{xi}}_{\text{Reallocation of inputs}} \tag{7}$$

where $D_i = \frac{P_i Q_i}{\sum_i VA_i}$ is a Domar weight[42], $s_{xi} = \frac{W_{xi}\text{input}_{xi}}{P_i Q_i}$ 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 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 7 to the data using the following approximation[43]

$$APG_t \approx \sum_i \bar{D}_{it}(\Delta \log A_{it}) + \sum_i \bar{D}_{it} \sum_x (\epsilon_{xi} - \bar{s}_{xit})(\Delta \log \ \text{input}_{xit}) \tag{8}$$

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

intermediate inputs.

[42] 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.

[43] Equation 7 describes aggregate productivity growth in continuous time. We can use Tornquist-Divisia approximations to estimate this expression using discrete-time data.

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. (2016a) and Dias et al. (2016b) on the importance of accounting for services in aggregate productivity). Table 4 shows that this negative productivity growth was driven by an increase in the misallocation of inputs across firms, in particular of capital.[44] 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 driven by a growing misallocation of capital.

4.2 The Effect of the EBA Intervention on Aggregate Productivity

We use the productivity decomposition in equation 7 to quantify how much of the decline in aggregate productivity growth can be explained by the EBA intervention. The productivity decomposition 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.[45]

The decomposition in equation 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. 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

[44] 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.

[45] 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.

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). To obtain the 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

$$\Delta\widehat{\log L_i} = \hat{\gamma}^L \times \underbrace{\left(\hat{\delta}^{treat}\text{treatment}_i\right)}_{\Delta\,\widehat{\log\text{credit}_i}} \tag{9}$$

where $\hat{\delta}^{treat}$ is the estimated treatment effect in the firm-level credit regression, specification 3,[46] and $\hat{\gamma}^L$ is the estimated pass-through coefficient into employment growth based on specification 4 in section 3.[47]

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 5). 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.[48]

Table 6 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.

This counterfactual is partial equilibrium in nature and may over- or understate the

[46] We estimate a non-dynamic version of 3 (not reported) to obtain point estimates on the cumulative change in credit during the EBA and bailout periods.

[47] 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 8 is specified in deflated values. In addition, we estimate the pass-through into firm-level TFP and deflated intermediate inputs.

[48] 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 Klenow (2017), Nishida et al. (2017)).

true effects on productivity, depending on the sign of the general equilibrium effects. For example, it is possible that the negative credit shock for firms borrowing from exposed banks led their competitors borrowing from non-exposed banks to increase their input use. If competitors have high marginal products, then this effect may have moderated the negative effect on productivity.[49] In contrast, negative spillover effects, evidence of which we document in the next subsection, would suggest that this exercise understates the true effect on productivity.

4.2.1 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.[50] 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 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 6 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.

[49]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.

[50]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.

4.3 Do Firm-Level Wedges 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 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.[51]

We find significant effects of the firm-level credit shocks on firm-level labor and capital wedges of about 12-17% (see panel b of see Table 5). 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 borrowing share from EBA banks is not correlated with firm-level sales or productivity volatility nor with sales cyclicality.[52] In addition, we confirm that the results

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

[52] 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

are robust to controlling for firm level sales and productivity volatility in Appendix B.

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.[53]

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:

$$\Delta \log \text{input}_{is} = \varphi \text{share}_s + \text{controls} + v_{is} \tag{10}$$

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 firms using the average industry exposure to the EBA shock.[54] 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.[55]

of log sales.

[53]There is also evidence for such a negative spillover channel in Europe (McGowan et al. (2016) and Acharya et al. (2017)).

[54]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).

[55]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

Table 7 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. Table 7 also shows 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. 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.[56]

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.

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

5 Conclusion

This paper studies how a weak banking sector affects productivity and growth in the aftermath of a financial crisis. Our contribution is to establish a credible causal chain from a weak banking sector to adverse effects on productivity and growth. To establish this causal chain, we rely 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. The richness of our data allows us to trace how the heightened credit misallocation translates into heightened misallocation of factor inputs which in turn drags down aggregate productivity growth. While we exploit a relatively short-lived regulatory intervention to cleanly identify the costs of a weak banking sector, the causal mechanism we identify in this paper is likely to apply beyond the period we study. We show that the underreporting of loan losses is pervasive both in the lead-up to the regulatory intervention we study and in the years following the intervention.

Our results highlight the importance of understanding the effect of supply side frictions in credit markets on the composition of credit, especially in economies where banks play a key role in allocating resources to firms. We show that in such bank-dependent economies the (mis)allocation of credit feeds through to the (mis)allocation of production factors. We show that, at least in partial equilibrium, this channel can have a significant impact on aggregate productivity growth. Further quantifying the impact of this causal channel on aggregate productivity is a fruitful avenue for future research.

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Tables

Table 1: Examples of Loss Underreporting

Panel A: Example 1

EUR m	**<30 days**	**Overdue 1 month**	**2 months**	**Performing credit**	**Excess mass**
2012m1	5			45	0
2012m2		5		45	0
2012m3		**5**		45	5
2012m4			5	45	0

Panel B: Example 2

	<30 days	**Overdue 1 month**	**2 months**	**Performing credit**	**Excess mass**
2012m1	5			45	0
2012m2		5		45	0
2012m3	5 →	**10**	← 5	44	10
2012m4		**6**		40	6
2012m5			6	40	0

Notes. The table shows stylized examples 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 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 5 m fall overdue (reducing performing credit by that amount). 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 10 m. In March, the bank also grants EUR 4 m of new performing credit, which means that the performing balance is EUR 45 - 5 + 4 = 44 m. In April, the firm uses the new credit to pay back EUR 4 m of the overdue balance. The bank treats the repaid portion as the longest overdue and reports the EUR 6 m 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.

Table 2: Descriptive Statistics: Firms and Banks

Firms		Banks	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	LTRO	0.08	0.08	0.00
	(0.17)		(0.06)	(0.03)	(0.30)
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.

Table 3: Pass-Through Into Employment and Investment

Growth rate	(1)	(2)	(3)	(4)	(5)	(6)
		Employees		Wages	CAPEX	Fixed assets
	Extensive + intensive					
	OLS	IV	Intensive			
Δ log credit$_i$	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. No significance stars are shown.

Table 4: Aggregate Productivity Growth (APG) Decomposition

	(1)	(2)	(3)	(4)	(5)
	APG	TFP	Labor	Capital	Intermediates
Year					
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 TFP 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. TFP is a production function residual.

Table 5: Pass-Through Into Input Use and TFP

Panel a	(1)	(2)	(3)	(4)	(5)
	TFP	Labor	Capital	Materials	Services
Δ log credit$_i$	-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: Wedges	(1)	(2)	(3)	(4)
	Labor	Capital	Materials	Services
Δ log credit$_i$	-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

Panel c: Wedges	(1)	(2)	(3)	(4)
	Labor	Capital	Materials	Services
Δ log credit$_i$	-0.100	-0.195	-0.020	0.012
	[0.020]	[0.033]	[0.062]	[0.033]
Controls	Y	Y	Y	Y
Volatility control	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 is a production function residual. Labor refers to the number of employees. In panel b and c, dependent variables are firm-level wedges between output elasticities and revenue shares. We use the log change of the absolute value of the wedge (to allow for negative wedges). 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. In panel c, we additionally control for firm-level sales cyclicality and productivity volatility. Standard errors are clustered by industry. Standard errors in parentheses. No significance stars are shown.

Table 6: Aggregate Productivity Growth (APG): Counterfactuals

	(1)	(2)	(3)	(4)	(5)
	APG	TFP	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 using the perpetual inventory method described in the Appendix C. TFP 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 TFP are computed industry-level cost shares.

Table 7: Spillovers from Underreported Firms

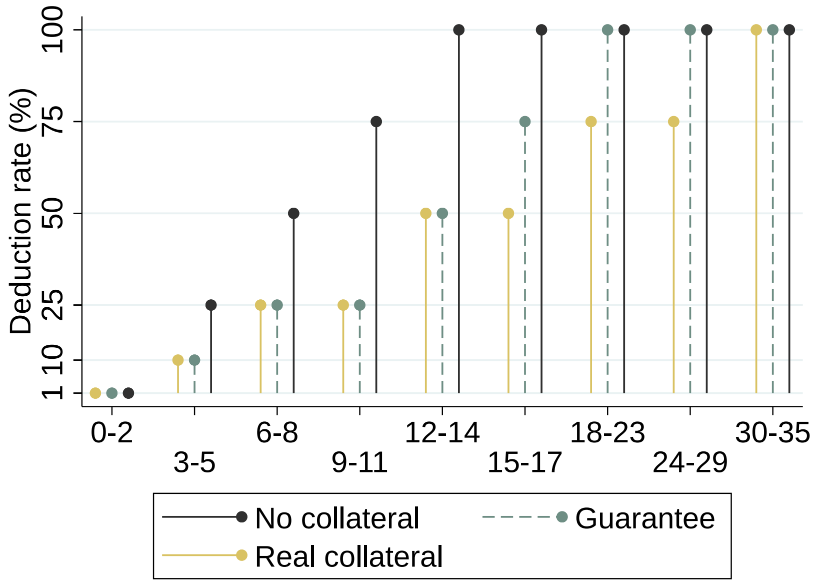
	(1)	(2)	(3)	(4)	(5)	(6)
	Sales	Capital	Labor	Materials	Services	TFP
Industry share of underreported firms	-0.107	-0.082	-0.044	-0.029	-0.073	0.013
	[0.017]	[0.019]	[0.006]	[0.025]	[0.013]	[0.010]
N	43,273	43,273	43,273	43,273	43,273	43,273
First-stage	3522	3523	3524	3525	3526	3527

	(1)	(2)	(3)	(4)	(5)	(6)
	2-digit		4-digit		1-digit-district	
	Sales	Capital	Sales	Capital	Sales	Capital
Industry share of underreported firms	-0.093	-0.104	-0.139	-0.085	-0.122	-0.117
	[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	550	550

Notes. The tables show 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 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. Panel b shows results when varying the granularity of the industry definition. Robust standard errors in parentheses. No significance stars are shown.

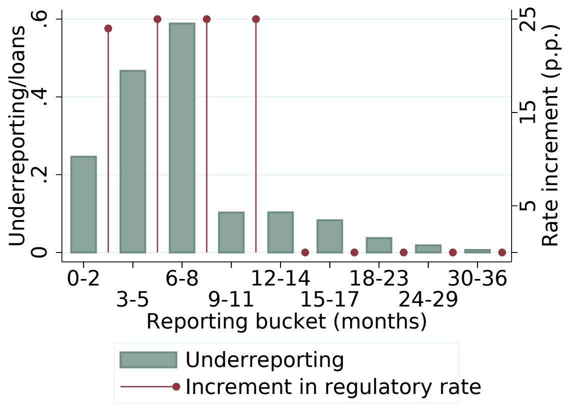
Figures

Figure 1: Regulatory Rules on Loan Losses

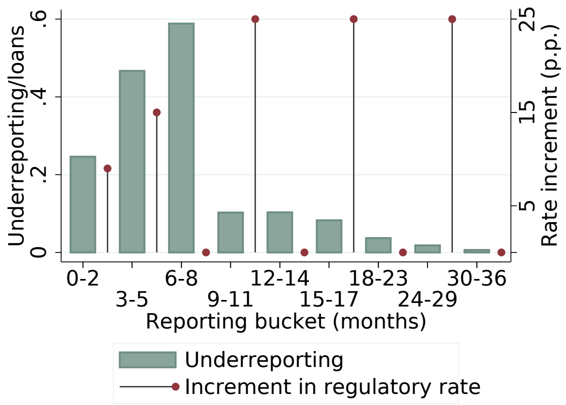


Notes. The graph shows the regulatory rules according to Notice 3/95 that govern mandatory minimum deductions for loan losses based on the number of months a loan has been overdue and the type of collateral.

Figure 2: Underreported Losses by Reporting Category



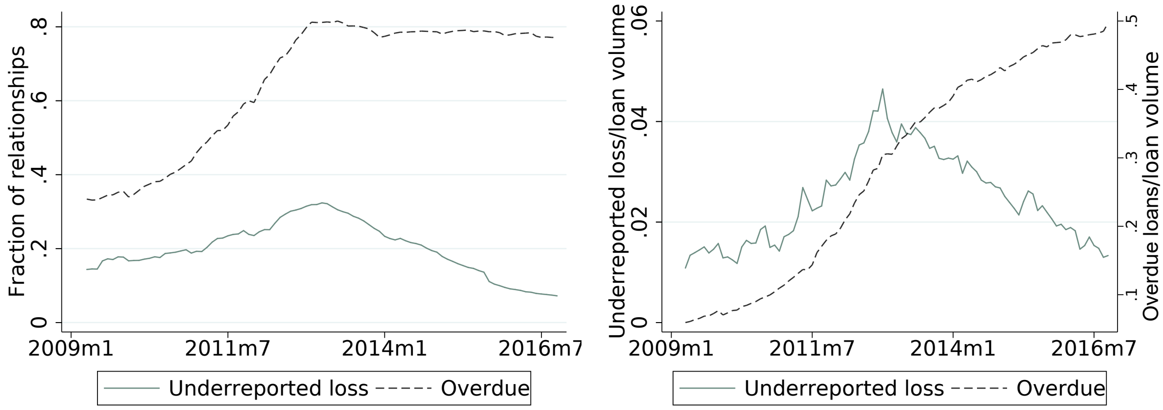
(a) Bunching Test



(b) Placebo test

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 (see Figure 1). 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 9

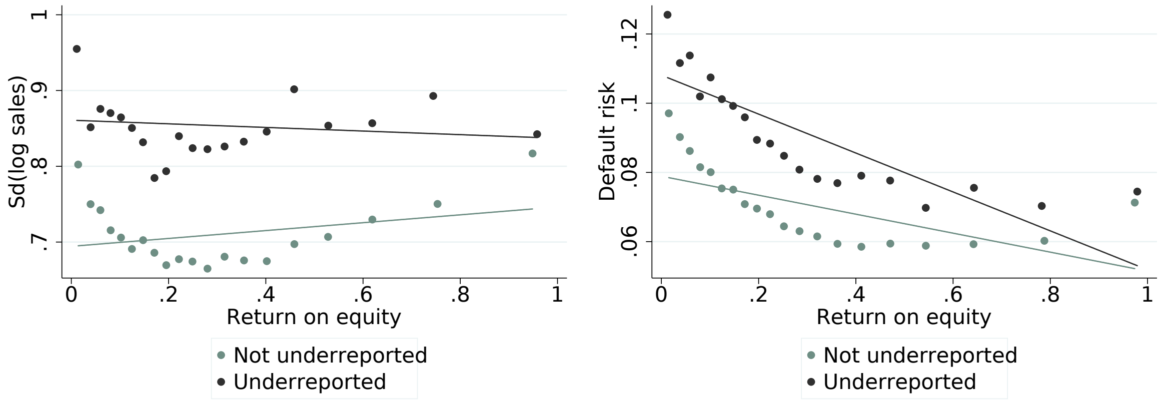
Figure 3: Prevalence of Loss Underreporting



(a) Fraction of Relationships (b) Share of Loan Volume

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).

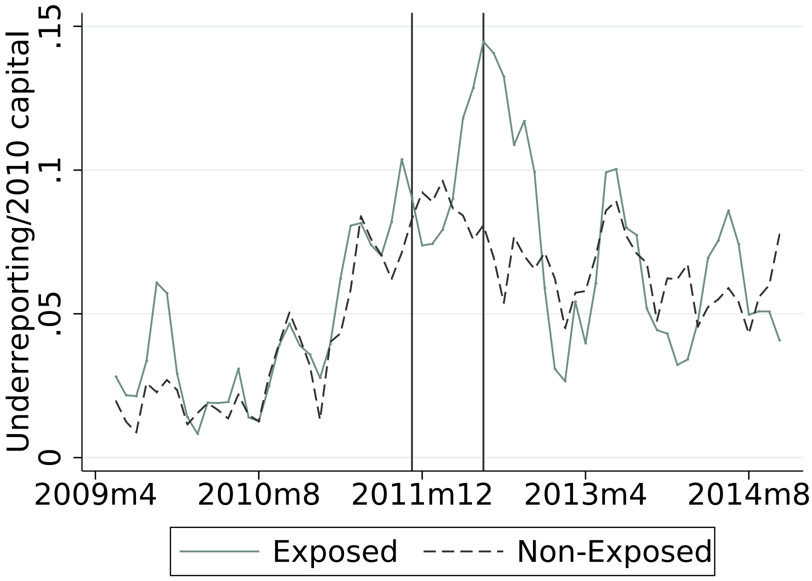
Figure 4: Correlation of Underreporting with Firm-Level Risk



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

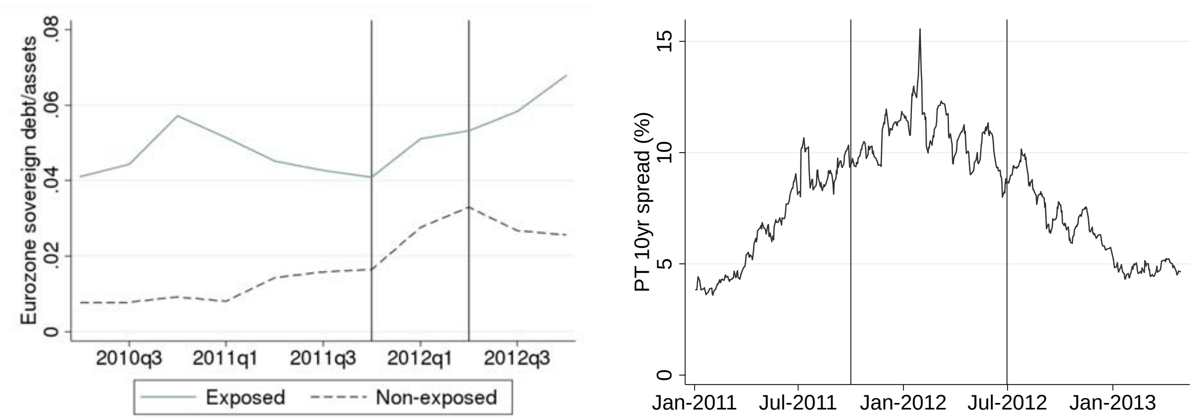
Notes. The graphs show a residualized binned scatter plot of firm-level risk measures against the return on equity. The left panel uses the standard deviation of firm-level sales across 2005-2015. The right panel uses default risk based on the credit risk prediction model of Antunes et al. (2016). The 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.

Figure 5: Underreporting of Loan Losses by Exposed and Non-Exposed Banks



Notes. The figure shows the evolution of aggregate underreported losses for exposed and non-exposed 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.

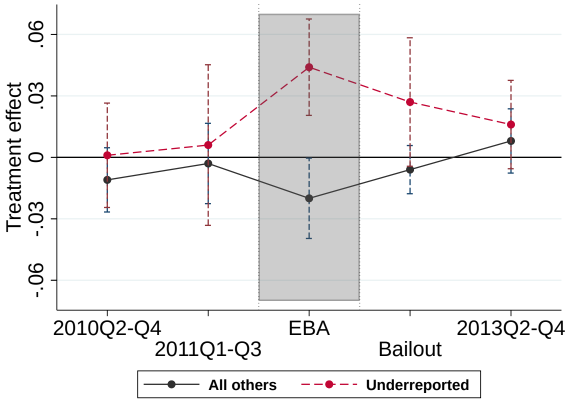
Figure 6: Potential Identification Threats from Sovereign Debt



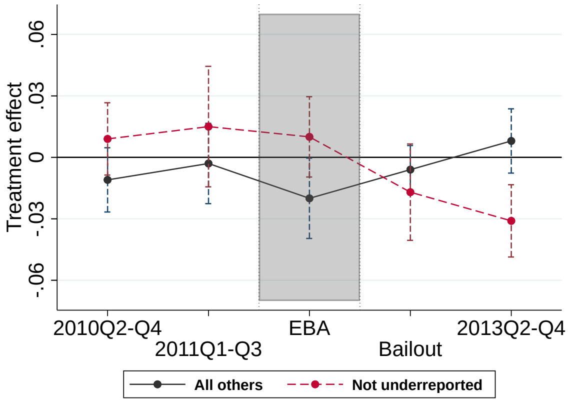
(a) Evolution of Sovereign Debt Holdings (b) Evolution of Underreported Losses

Notes. Panel a shows the average share of Eurozone sovereign debt of EBA *eligible* banks exposed and not exposed to the EBA Special Capital Enhancement exercise. Panel b shows the evolution of spreads on Portuguese sovereign debt (10-year bond relative to German 10-yr bond). Vertical lines denote the EBA regulatory intervention.

Figure 7: Firm-bank Credit Results



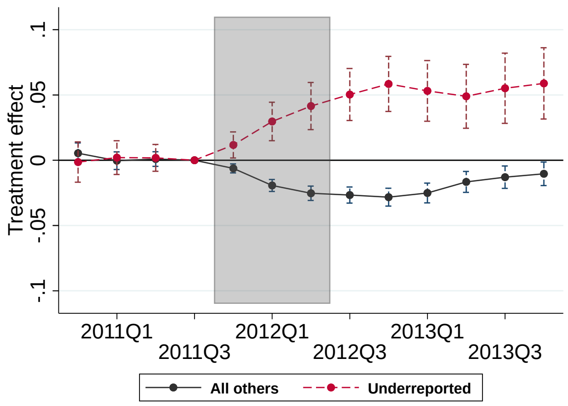
(a) Underreported firms



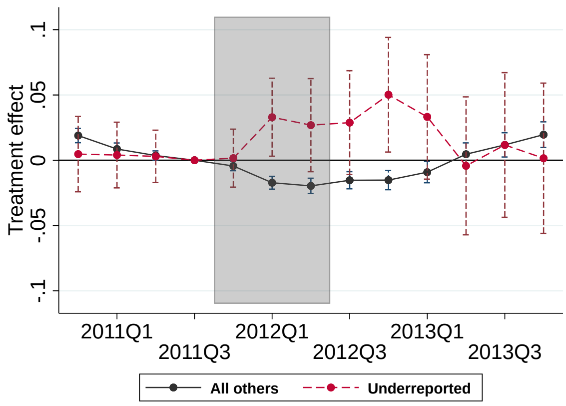
(b) Non-underreported firms

Notes. The graphs shows results of the firm-bank level credit regression in specification 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 period$_\tau$ × exposed$_b$ and period$_\tau$ × underreported$_{ib}$ × 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 period$_\tau$ × not underreported$_{ib}$ × 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 Table 12 in Appendix B for point estimates. N = 1,981,219.

Figure 8: Firm-level Credit Treatment Effects



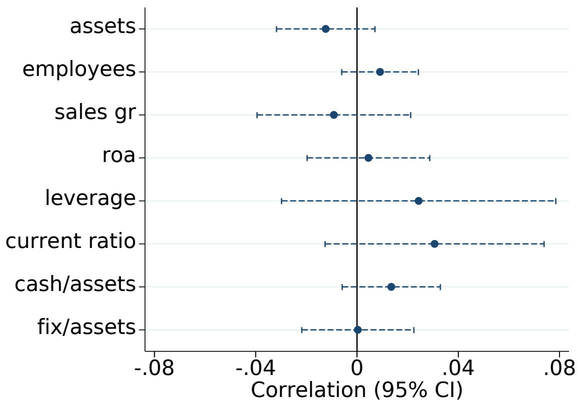
(a) Total Credit



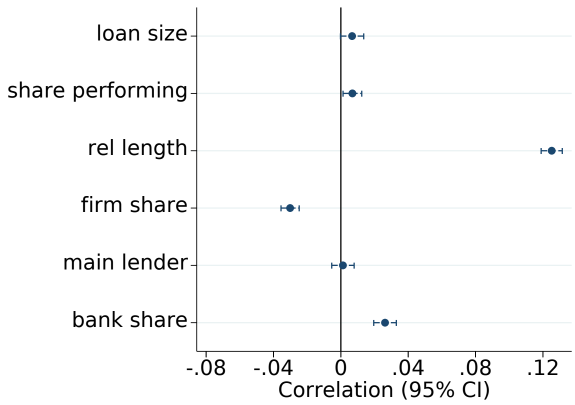
(b) Performing Credit

Notes. The graphs show results of the firm-level credit regression in specification 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.

Figure 9: Correlations with Borrowing Share from Exposed Banks



(a) Firm-level



(b) Relationship-level

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.

For Online Publication: Appendix A1: 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 $\mathrm{IN}(t;0) = C_j(t;0)$ 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:

$$E(t;b) = C(t;c) - C(t-1;c-1). \tag{11}$$

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

$$\bar{E}(t;k) = \sum_{j=1}^{s} E(t-j;k). \tag{12}$$

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 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 11a in Appendix B).

The basic formula adjusted for flows is

$$E(t;k) = [C(t;c) - \text{IN}(t;c)] - [C(t-1;c-1) - \text{OUT}(t;c-1)]\,. \tag{13}$$

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 auxiliary 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:

$$\begin{aligned}\tilde{\mathrm{IN}}(t;k-1) &= \underbrace{\mathrm{IN}(t-3;(k-1)\{1\})) + \mathrm{IN}(t-3;(k-1)\{2\})) + \mathrm{IN}(t-3;(k-1)\{3\}))}_{\text{already incorporated in B(t-3,k-1)}} \\ &\quad + \mathrm{IN}(t-2;(k-1)\{2\}) + \mathrm{IN}(t-2;(k-1)\{3\})) + \mathrm{IN}(t-1;(k-1)\{3\}) \\ &= \mathrm{IN}(t-2;(k-1)\{2\}) + \mathrm{IN}(t-2;(k-1)\{3\})) + \mathrm{IN}(t-1;(k-1)\{3\}).\end{aligned}$$

3. $\hat{\mathrm{IN}}(t;k)$: 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:

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

4. $\hat{\mathrm{OUT}}(t;k)$: 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 $t-1$. Outflows at time t do not affect the measure of excess mass: $\hat{\mathrm{OUT}}(t;k) = \mathrm{OUT}(t-1,k) + \mathrm{OUT}(t-2;k)$.

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. $0 \leq \tilde{\text{IN}}(t; k-1) \leq \text{IN}(t-1; k) + \text{IN}(t-2; k)$
3. $\text{IN}(t; k) \leq \hat{\text{IN}}(t; k) \leq \text{IN}(t; k) + \text{IN}(t-1; k) + \text{IN}(t-2; k)$
4. $\hat{\text{OUT}}(t; k) = \text{OUT}(t-1, k) + \text{OUT}(t-2; k)$

Table 8: Effects of Assumptions on Flows

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

Table 8 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 11a in Appendix B, we present results using the maximum and minimum combinations respectively. Figure 11b in Appendix B shows that the 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. The six-month buckets have analogous formula.

Additional restrictions

We impose the following additional restrictions:

1. We impose that excess mass can never exceed observed mass in bucket.
2. We also impose that excess mass must be weakly positive since negative excess mass is just a mis-measured 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.

Appendix A2: Validity Checks for Algorithm

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 $k+1$:

$$\frac{\text{excess mass}_{ibkct}}{\text{overdue loans}_{ibkct}} = \sum_{j=1}^{5} \beta_j \Delta \text{deduction rate}_j + \varphi_b + \theta_i + \mu_t + \epsilon_{ibkct}. \tag{14}$$

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 9 and discussed in section 2.

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.[57] We expect underreporting to be most pronounced when the regulatory deduction rate increases as this implies that banks continue 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 10 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 10 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

[57]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.

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 section 2.

Table 9: 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. No significance stars are shown.

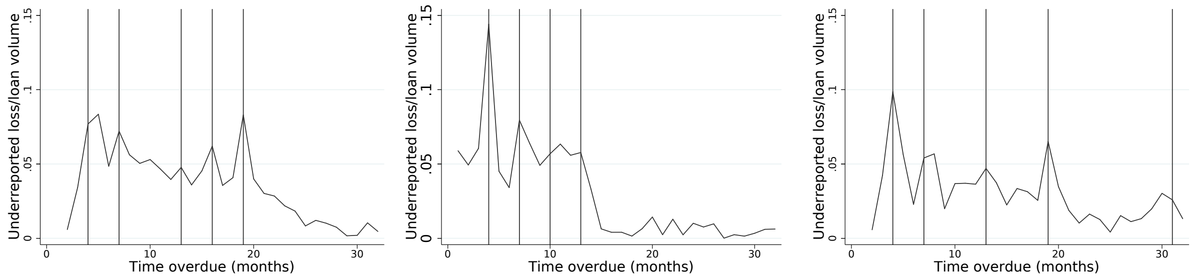
For Online Publication: Appendix B: Additional Tables and Figures

Table 10: 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. No significance stars are shown.

Figure 10: Algorithm Validity Test for Single-loan Relationships



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

Notes. The graph plots the average amount of underreporting against the actual time a loan has been overdue. We only consider single-loan relationships where we 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 we would expect most underreporting to occur (increase in the regulatory deduction rate). These points differ according type of collateral. We only consider loans with a single type of collateral.

Table 11: Additional Descriptive Statistics

Firm finance loans		Underreported firms		
	Average		Average	Difference
Loan amount	269,729	Assets (m)	1.09	0.516
	($2x10^6$)		(5.165)	[0.041]
Fraction overdue	0.50	Leverage	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 descriptive statistics 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.

Table 12: 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]
Post $\text{bailout}_t \times \text{exposed}_b$		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]
Post $\text{bailout}_t \times \text{exposed}_b \times \text{underreported}_{ib}$	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 changes scaled by lagged total credit. For example, the growth rate in performing credit is defined as $\Delta c^{perf}_{ibt}/c^{all}_{ib,t-1}$. 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 intervention (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. Significance stars are not shown.

Table 13: Regression Results Firm-Bank Level: Robustness Checks

Growth rate of total credit	(1)	(2)	(3)	(4)
$Pre1_t \times exposed_b$	-0.009	-0.009	-0.016	-0.011
	[0.008]	[0.011]	[0.009]	[0.009]
$Pre2_t \times exposed_b$	-0.004	-0.002	-0.018	-0.004
	[0.010]	[0.010]	[0.011]	[0.011]
$EBA_t \times exposed_b$	-0.022	-0.020	-0.029	-0.022
	[0.010]	[0.013]	[0.011]	[0.010]
$Bailout_t \times exposed_b$	-0.009	-0.005	-0.015	-0.009
	[0.006]	[0.006]	[0.008]	[0.006]
Post $bailout_t \times exposed_b$	0.006	0.008	0.002	0.008
	[0.007]	[0.012]	[0.009]	[0.008]
$Pre1_t \times exposed_b \times underreported_{ib}$	0.002	0.012	0.013	0.001
	[0.013]	[0.010]	[0.008]	[0.013]
$Pre2_t \times exposed_b \times underreported_{ib}$	0.006	0.024	0.012	0.006
	[0.023]	[0.023]	[0.016]	[0.023]
$EBA_t \times exposed_b \times underreported_{ib}$	0.043	0.051	0.051	0.044
	[0.013]	[0.017]	[0.015]	[0.012]
$Bailout_t \times exposed_b \times underreported_{ib}$	0.027	0.026	0.034	0.027
	[0.017]	[0.021]	[0.010]	[0.016]
Post $bailout_t \times exposed_b \times underreported_{ib}$	0.016	0.021	0.014	0.011
	[0.012]	[0.014]	[0.010]	[0.009]
Firm×quarter FE	Y	N	N	Y
Firm, quarter FE	N	Y	Y	N
Relationship controls	N	Y	Y	Y
Firm-level controls	N	Y	N	N
N	1,981,219	1,859,321	5,244,714	1,981,219
R2	0.378	0.057	0.069	0.417
Banks	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. See Table 12 for additional details. Relative to column 2 of Table 12, column 1 omits our baseline controls, column 2 adds additional firm-level controls (ebitda/assets, leverage, sales growth - all interacted with the period dummies), column 3 clusters standard errors at the bank-level, and column 4 adds control for the bank-level use of the LTRO program. No significance stars are shown.

Table 14: Regression Results Firm-bank Level:
Extensive Margin

Pr(relationship cut)	(1)	(2)	(3)
$\text{EBA}_t \times \text{exposed}_b$	0.057	0.056	0.058
	[0.011]	[0.011]	[0.012]
$\text{Bailout}_t \times \text{exposed}_b$	0.041	0.042	0.043
	[0.009]	[0.008]	[0.008]
Post $\text{bailout}_t \times \text{exposed}_b$	0.029	0.030	0.029
	[0.010]	[0.009]	[0.009]
$\text{EBA}_t \times \text{exposed}_b \times \text{underreported}_{ib}$	-0.217	-0.202	-0.219
	[0.034]	[0.027]	[0.057]
$\text{Bailout}_t \times \text{exposed}_b \times \text{underreported}_{ib}$	-0.106	-0.090	-0.105
	[0.033]	[0.030]	[0.047]
Post $\text{bailout}_t \times \text{exposed}_b \times \text{underreported}_{ib}$	-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

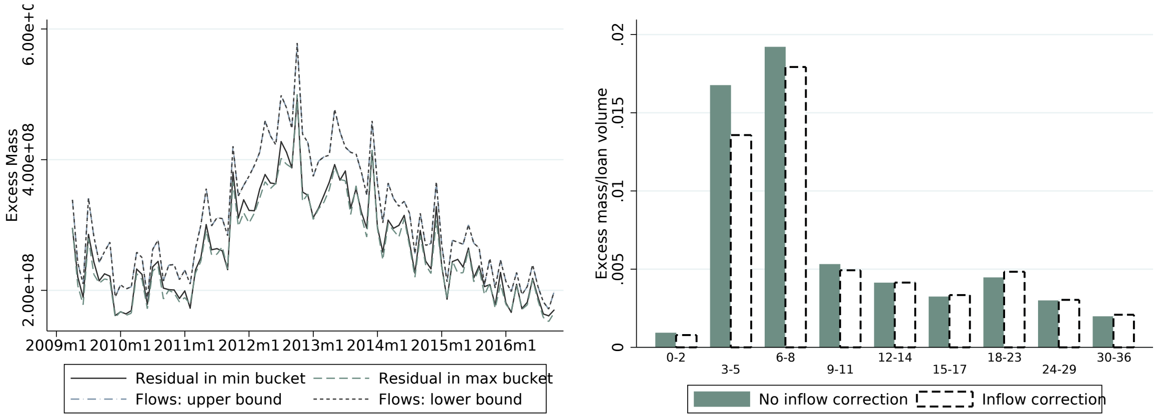
Notes. The table shows credit regressions results at the firm-bank level for the extensive margin (linear probability model). The dependent variable is a dummy that turns one when the relationship is cut, defined by the performing loan balance dropping to zero. The explanatory variable exposed is a dummy 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 2 in section 3 for details on full set of interaction effects included. No significance stars are shown.

Table 15: Employment and Investment Results: Persistence and Placebo Tests

	(1)	(2)	(3)	(4)	(5)
Growth rate			Employees		
	2013	2014	2011	2009	2008
Δ log credit$_i$	-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 IV 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 3, 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. No significance stars are shown.

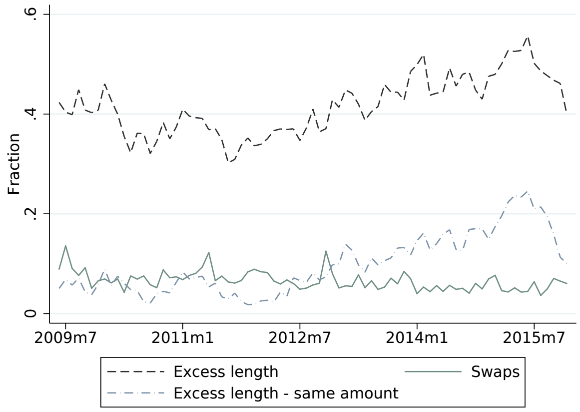
Figure 11: Robustness Checks on Algorithm



(a) Robustness of Algorithm (b) Underreporting by Reporting Buckets

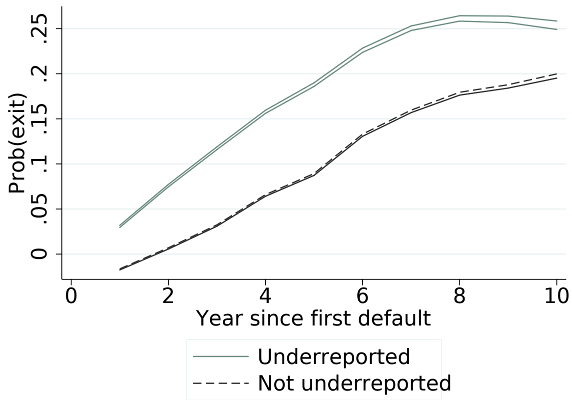
Notes. Panel a shows the aggregate amount of excess mass when varying different assumptions. The first two lines show the results when we allocate residual flows to the lowest (highest) reporting bucket. The remaining lines show the effect of choosing the bounds on flows such that they have the minimum (maximum) impact on excess mass. Panel b h shows the distribution of excess mass (or underreporting) across reporting buckets. We scale the amount of excess mass by the total loan balance of that firm-bank pair. We 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.

Figure 12: Decomposition of Underreported Losses by Mechanism

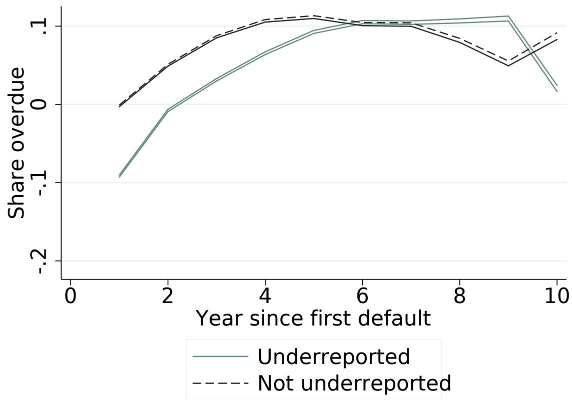


Notes. The graph shows the decomposition of underreported losses by the mechanisms discussed in section 2. Excess length refers to spells of overdue reporting in a bucket that exceed the permissible length (e.g. loan reported to be overdue 3-5 months for 4 months in a row.). Excess length - same amount 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.

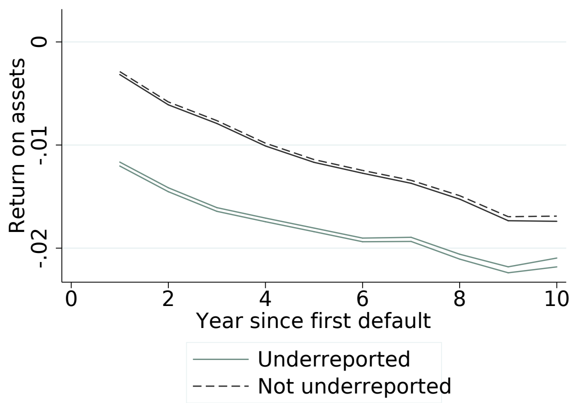
Figure 13: Long-run Trends: Underreported vs Non-underreported Firms



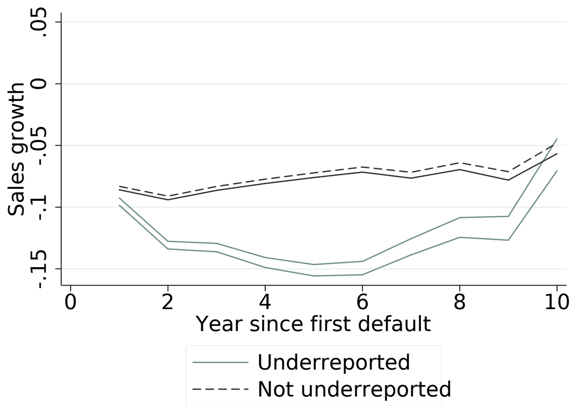
(a) Probability of exit



(b) Share of loans overdue



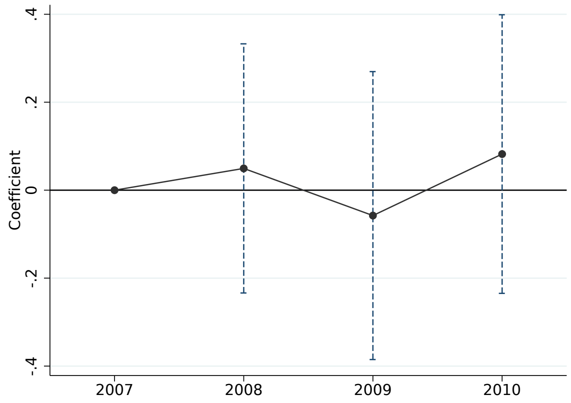
(c) Return on assets



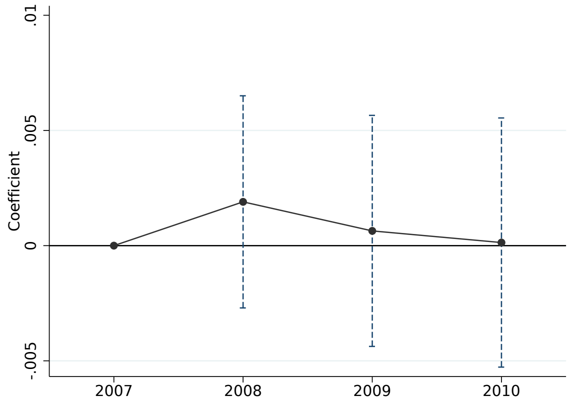
(d) Sales growth

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.

Figure 14: Liquidity and Credit Pre-trends



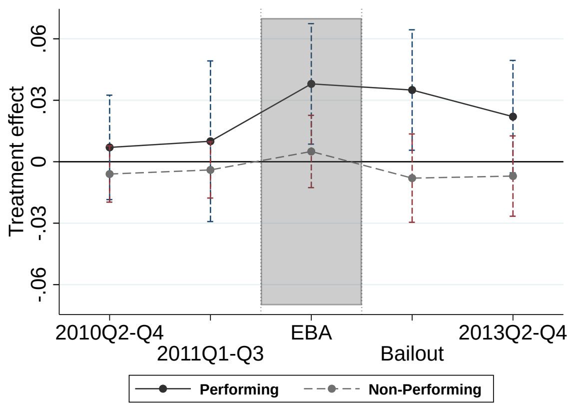
(a) Current Ratio



(b) Cash/Assets

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 under-reporting. The two panels show two different liquidity measures. Standard errors are clustered at the firm-level.

Figure 15: 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$_i$ × quarter$_t$ × 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 3, 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.

Appendix C: Estimating Production Functions

In order to compute the aggregate productivity decomposition in 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:

$$q_{it} = \beta_l l_{it} + \beta_k k_{it} + \beta_m m_{it} + \beta_s s_{it} + \epsilon_{it}. \tag{15}$$

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 TFP based on the gross output function as

$$\log A_{it} = q_{it} - \left(\hat{\beta}_l l_{it} + \hat{\beta}_k k_{it} + \hat{\beta}_m m_{it} + \hat{\beta}_s s_{it}\right) \tag{16}$$

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

$$q_{it} = \sum_j \beta_j X_{it}^j + \beta_{jj} X_{it}^{j^2} + \sum j \neq k \sum_k \beta_{jk} X_{it}^j X_{it}^k + \epsilon_{it}. \tag{17}$$

In Table 16 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 16: Production Function Coefficient Estimates

		Fixed assets			Inventory method		
	Cost shares	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).

Acknowledgements

We thank Edoardo Acabbi, Juliane Begenau, Gabriel Chodorow-Reich, John Coglianese, Karen Dynan, Andrew Garin, Gita Gopinath, Emmanuel Farhi, Jeff Frieden, Xavier Gabaix, Sam Hanson, Eben Lazarus, Matteo Maggiori, Pascal Noel, Marcus Opp, Martin Rotemberg, Jesse Schreger, Jann Spiess, Jeremy Stein, and Adi Sunderam, for helpful comments. The authors also thank Manuel Adelino, Nuno Alves, Antonio Antunes, Diana Bonfim, Isabel Correia, Sudipto Karmakar, Gil Nogueira, Maximiano Pinheiro, and seminar participants at the Banco de Portugal. The contribution by Laura Blattner to this paper has been prepared under the Lamfalussy Fellowship Program sponsored by the European Central Bank. Any views expressed are only those of the authors and do not necessarily represent the views of the ECB, the Banco de Portugal or the Eurosystem.

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PDF ISBN 978-92-899-3490-9 ISSN 1725-2806 doi:10.2866/827615 QB-AR-19-009-EN-N