When Losses Turn Into Loans: The Cost of Undercapitalized Banks

Job Market Paper

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Abstract

We provide evidence that a weak banking sector has contributed to low productivity growth in the aftermath of 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. Using detailed administrative data from the Bank of Portugal, we show that affected banks respond not only by cutting back on lending but also by increasing their underreporting of loan losses, which inflates reported capital, and by reallocating credit to firms in financial distress with prior underreported losses. To establish these results, we develop a method to detect the underreporting of losses using detailed loan-level data. We argue that this credit reallocation is consistent with distorted lending incentives arising either from the attempt to avoid the recognition of underreported losses, or from gambling on risky firms in response to an expected government bailout. We then show that the credit reallocation affects firm-level investment and employment. Finally, we translate the firm-level changes into aggregate productivity. This partial equilibrium exercise suggests that the credit reallocation driven by the regulatory intervention accounts for 20% of the decline in productivity in Portugal in 2012.

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1 Introduction

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

Existing research has not been able to establish a credible causal chain from a weak banking sector to adverse effects on productivity and growth. One strand of literature has provided evidence for an empirical link between weak banks, measured by the size of their regulatory capital cushion, and lending to failing ('zombie') firms but not established causality.[2] A separate strand of literature has suggested that propping up failing firms can have real economic costs by crowding out healthy firms but not investigated how 'zombie' lending can directly drive the misallocation of resources in the economy.[3] However, in order to fully assess the effect of a weak banking sector, it is crucial to trace the entire causal chain from the distorted lending incentives of undercapitalized banks to resource misallocation and aggregate productivity.

In this paper, we show that a weak banking sector has contributed to a slowdown in productivity in the aftermath of the European sovereign debt crisis. We exploit an intervention by the European Banking Authority in 2011, which caused a subset of banks to be below the regulatory capital standards. We show that affected banks respond to their diminished capital adequacy by distorting their reporting and lending choices at the micro-level. We then show how these distorted incentives at the micro-level drive the misallocation of resources across firms and aggregate up to large negative effects on productivity at the macro-level. Our results have important implications for the policy debate on bank recapitalization. First, we document that banks respond to higher capital ratios not only by cutting back on lending but also by distorting their reporting and lending choices — two unintended consequences which had not previously been known. Second, we provide an estimate of the economic costs of not forcing banks to raise new equity capital after a crisis. Third, we quantify by how much weak banks overstate their financial health by underreporting losses.

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

[1] See for example Hoshi and Kashyap (2015) on the parallels between Japan and Europe.
[2] See Peek and Rosengren (2005), Schivardi et al. (2017) and Acharya et al. (2017)
[3] Caballero et al. (2008), Moreno-Serra et al. (2016)

was based on a size cut-off, and the severity of the capital shortfall, which was determined by prior sovereign bond holdings.[4] Banks correctly anticipated that as long as they made a credible attempt to comply with the EBA requirements, the Portuguese government would step in at the compliance deadline to make up any remaining capital shortfall.[5] All exposed banks received a capital injection at the EBA deadline, which allowed them to comply with the EBA requirements.

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

Our main result, which establishes the first link in the causal chain, is that exposed banks respond to higher capital requirements not only by cutting back on lending but also by reallocating credit to a subgroup of distressed firms whose loan losses banks had been underreporting prior to the EBA announcement. These results are estimated in a difference-in-difference design, in which we compare changes in credit from exposed and non-exposed banks to the same firm. In contrast, exposed banks do not increase credit to distressed firms that are not underreported. We also show that the credit reallocation is unlikely to be driven by firm-level shocks increasing credit demand from distressed, underreported firms. Exposed banks change their credit allocation only in the period between the EBA announcement and the EBA deadline. This implies that firm-level shocks would have to match the exact timing of the regulatory intervention to be able to account for our results. Moreover, given that we control for firm-level changes in credit, firm-level shocks would have to drive up credit demand at exposed

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

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

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

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

banks but not at non-exposed banks. Furthermore, we show that underreported firms borrowing from exposed and non-exposed banks do not have diverging pre-trends in credit or liquidity, that observable measures of firm quality are not correlated with the borrowing share from exposed banks, and that the results are robust to controlling for relationship characteristics such as whether the bank is the main lender.

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

The second distorted lending incentive arises as exposed banks gamble on risky, distressed firms in anticipation of the government bailout. The prospect of the government covering loan losses increased incentives to lend to risky, distressed firms despite the fact that these firms were likely to fail. At the same time, banks underreported losses on these risky firms because underreporting allowed banks to avoid regulatory scrutiny of risky loans. Consistent with the gambling motive, we find that among firms with loan losses, underreported firms have higher levels of riskiness, as measured by sales volatility and predicted default risk.

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

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

The causal chain connecting weak banks and the misallocation of resources applies beyond the period of the EBA intervention. A novel dataset on new lending operations, which is only available after the period of the EBA intervention, allows us to identify new loans that receive a subsidized interest rate via a matching procedure. We show that banks are more likely to grant new subsidized loans to distressed, underreported firms when they have a high shadow cost of capital. To establish this result, we use within-bank variation in the intensity of underreporting as a proxy for the bank's shadow cost of capital.[8] The estimated magnitude is broadly consistent with the quasi-experimental results and aligns with prior research that shows that loan subsidies are a popular tool to keep failing borrowers afloat (Caballero et al. (2008)).

Beyond its negative effects on productivity, a weak banking sector can also have adverse effects on financial stability. The underrreporting of loan losses leads banks to overstate their regulatory capital, making it difficult for the financial regulator to assess the true health of the banking sector. We show that the attempt of the EBA to make banks safer had the adverse consequence of leading banks to substantially increase their underreporting of loan losses in order to protect regulatory capital. We calculate that without this loss underreporting, banks would have had to raise additional funds amounting to between 4% and 20% of their regulatory capital.[9] We show that loss

[8]We obtain similar but noisier results using within-bank variation in the proximity to the regulatory capital constraint

[9]The range is due the fact that we have to make an assumption about how to calculate the actual time that an underreported loan has been overdue. The most conservative assumption yields 4%, while

underreporting remains elevated after the EBA intervention and hence continues to reduce capital adequacy in the post-EBA period. To the extent that European governments provide implicit bailout guarantees to their banks, these unrecognized losses constitute a hidden fiscal liability for their sovereigns.

Our work is closely related to the literature on 'zombie' lending (see Sekine et al. (2003) for a survey on Japan). Existing research has documented that banks close to the capital constraint tend to give more loans to poorly performing firms, defined by some observable metric, than banks far away from the capital constraint (Peek and Rosengren (2005), Acharya et al. (2017), and Schivardi et al. (2017)). Interpreting these results as evidence of distorted lending incentives is problematic for two reasons. First, distance from the capital constraint may be correlated with unobserved bank quality. We address this problem by relying on quasi-experimental variation in capital adequacy. Second, the results may pick up (efficient) lending to temporarily distressed firms with good fundamentals, or, by failing to exclude poorly performing firms that are not subject to distorted lending incentives, underestimate the true effect. We argue that our measure of loss underreporting helps to address these two challenges because it gives us meaningful variation among distressed firms[10] and we provide evidence that underreporting is not correlated with better long-run fundamentals.

Our work ties in the 'zombie' lending literature with research on the real effects of this phenomenon. Caballero et al. (2008) provide a model and related evidence that the continued existence of 'zombie' firms can have negative spillovers on healthy firms in the same industry. Moreno-Serra et al. (2016) and Acharya et al. (2017) find similar effects in Europe by replicating their research design. Schivardi et al. (2017) however find no such effects in Italy using a slightly amended empirical specification. We both directly map our credit results into firm-level outcomes and confirm the existence of negative industry-level spillovers using a quasi-experimental version of the specification in Schivardi et al. (2017).

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

assuming that these loans have been overdue long enough to have been written off completely yields 20%.

[10]Banks underreport about half of the firms with overdue loans.

Casamatta (1999), Acharya and Steffen (2015), Crosignani (2015)).

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

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

The remainder of the paper is organized as follows. Section 2 describes our method for measuring loss underreporting. Section 3 describes the natural experiment, the data and our results. Section 4 quantifies the effects on aggregate productivity. Section 5 provides evidence on loan subsidies. Section 6 quantifies the effect on capital adequacy. Section 7 concludes.

2 Loss Underreporting: A Tool to Measure Distorted Lending Incentives

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

2.1 Two Sources of Distorted Lending Incentives

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

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

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

The second type of distorted lending incentives arises when undercapitalized banks gamble for the resurrection of their distressed borrowers. If a bank is sufficiently undercapitalized that it will default in some states of the world, bank shareholders start to like gambles. Lending to distressed firms constitutes a gamble if the states of the world in which those distressed firms go under are also the states of the world in which the bank itself goes under. In that case, limited liability protects bank shareholders from losses in these states. Bank shareholders hence only care about states in which distressed firms recover, which are likely to coincide with the bank remaining solvent. Such "risk shifting" or "asset substitution" behavior leads banks to invest in negative NPV projects when these projects have sufficient variance to present a valuable out of the money call option to bank shareholders (Jensen and Meckling (1976)).[11]

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

Banks that gamble for the resurrection of distressed firms also have an incentive to underreport loan losses on these firms. The Bank of Portugal, which regulated Portuguese banks at the time, was concerned about risky lending to failing firms in the wake of the 2010-2011 sovereign debt crisis. For example, the Governor of the Bank of Portugal said in a speech: *"The composition of loans granted to non-financial corporations raises concerns."* And later: *"It is crucial that, as deleveraging proceeds, banks continue to provide an adequate level of financing to the most productive [...] firms in the economy"*.[12] The main tool used by the regulator to detect risky lending are reported losses. For example, the Bank of Portugal implemented programs in 2011 and 2012 that inspected a snapshot of a random sample of bank loans. These inspections would have been able to flag cases in which banks provided credit to a firm that had already defaulted on a large part of its loan balance.[13] Hence banks had an incentive to reduce reported losses when engaging in risky lending to avoid regulatory scrutiny. Gambling for resurrection predicts that underreported firms should be riskier relative to firms that are not underreported (but also have overdue loans).

2.2 Loss Reporting in Portugal

We exploit the regulatory framework on loan impairment losses in Portugal to measure the underreporting of loans losses. Once a firm falls behind on loan repayments, the bank has to deduct a fraction of the book value of the loan as an impairment loss.[14] These deductions are governed by rules set by the financial regulator. Banks deduct impairment losses as an expense from their income. As a result, impairment losses reduce banks' regulatory capital by reducing retained earnings. On the balance sheet, impairment losses mark down the value of the asset.

In Portugal, the minimum impairment loss a bank has to deduct depends on the number of months a loan has been overdue (behind on payments) as well as the type of collateral (see Figure 1).[15] The regulator imposes this mandatory deduction schedule to

[12]Source. `http://www.bis.org/review/r140805d.pdf` `https://www.bis.org/review/r120215c.pdf`

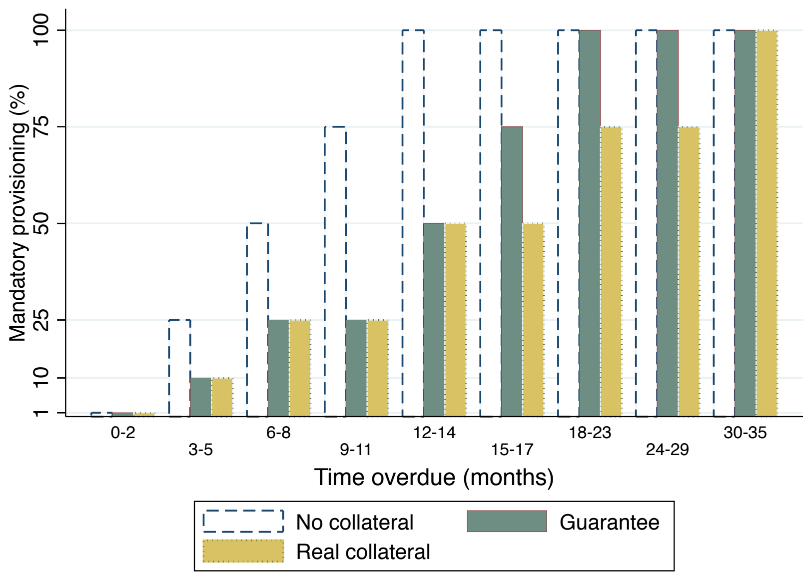
[13]While banks were only required to report new lending operations from mid-2012 onwards (partly driven by an attempt of the regulator to monitor the quality of loans), auditors and regulators would have been able to infer a new loan by comparing residual and origination maturity in their snapshot of loans. They would not have been able to infer any underreporting for which they would have needed the panel data we exploit in this paper.

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

[15]These rules are set out in Banco de Portugal instruction 3/95. 3/95 was in place before the international accounting standards IAS39, which also contain rules on impairment losses, became binding in Portugal. The Portuguese regulator refers to impairment losses under 3/95 as loan loss provisions but we use the term impairment losses for ease of exposition. Since the Portuguese financial regulator preferred the more stringent existing framework, banks have to deduct the difference between impairment losses under 3/95 and IAS39 from their regulatory capital. This means that 3/95 is the effectively binding

force banks to recognize that a full repayment of the loan is less and less likely the longer a loan has been overdue and to reflect the resulting loss on their financial statements.[16]

Figure 1: Regulatory Rules on Loss Deductions



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

This regulatory setting opens up the possibility of reducing reported losses by managing the reported time a loan has been overdue. 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.[17] Banks report the time overdue in discrete intervals, or buckets, which correspond to the regulatory buckets shown in Figure 1. We exploit this detailed reporting in order to measure loss underreporting.

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 loss underreporting which requires us to track a lending relationship over time. Firm-finance loans constitute the main loan product for firms and capture about 36% of the banks' corporate loan portfolio. However, since

regulation with regards to impairment losses in Portugal. 3/95 applies on an individual basis to all banks (regardless of whether they follow the standard approach or the internal-rating based approach under the Basel regulatory framework). On a consolidated basis, banks do not have to comply with 3/95 but impairment losses deducted at the individual basis will still negatively affect regulatory capital on the consolidated basis through retained earnings.

[16] In principle, a bank should deduct the full difference between the carrying amount of the asset (shown on the balance sheet) and the expected discounted cash flow (at the origination interest rate) as an impairment loss. However, impairment losses above the regulatory minimum are not tax-deductible in Portugal and hence Portuguese banks generally do not exceed the minimum deduction required (Dermine and de Carvalho (2008)).

[17] Banks start reporting this variable in 2009.

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 5 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 origination maturity above a year.

2.3 A Method to Detect Underreporting of Loan Losses

Our aim is to measure to what extent banks underreport loan losses by managing the reported time a loan has been overdue. Unfortunately, we cannot simply compare reported time overdue to the actual time overdue in the data since banks do not provide identifiers to track loans over time. Instead, we develop an algorithm to measure the extent of underreporting in each reporting bucket for all firm-bank pairs at a monthly frequency.[18]

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

Table 1: Example of Loss Underreporting

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

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

[18]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.

The goal of the algorithm is to measure excess mass, a term we borrow from the bunching literature (Diamond and Persson (2016), Dee et al. (2017) or Best and Kleven (2016)). 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. The third row of Table 1 shows an example where the bank does not update the reported time overdue, thereby underreporting how the long has been overdue. Banks use three mechanisms to adjust the reported time overdue: (a) they don't update the reported time (as shown in Table 1), (b) they combine new overdue loan installments with the existing overdue loan balance and report a (lower) average time overdue[19], and (c) they grant new performing credit in exchange for the repayment of the longest overdue portion of the loan. In Appendix A, we provide stylized examples of the second and third type of behavior and show that most underreporting is driven by these two types of behavior.

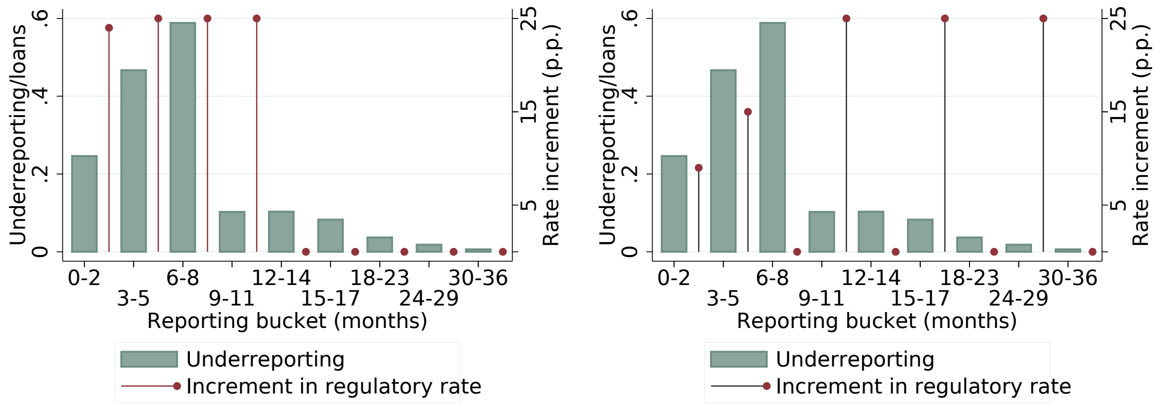
The algorithm is Markovian and only records inconsistencies relative to $t - 1$. That is, it does not keep a tally of how far the reporting has fallen behind the 'true' time overdue. In Appendix A, we provide evidence from a subset of single-loan relationships, where we can track the 'true' time overdue and show that the gap between reported and true time widens over time (Figure 6a in Appendix B). 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.[20] We run the full version of the algorithm on the set of non-performing corporate firm-finance lending relationships in 2009-2016.

[19]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.

[20]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.

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. 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 3

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.

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.[21]

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

The regression confirms that, for each type of collateral, the amount of underreporting

[21]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.

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 3 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.[22]

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 3 in Appendix B 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.

2.3.1 Underreporting as a Tool to Measure Distorted Lending Incentives

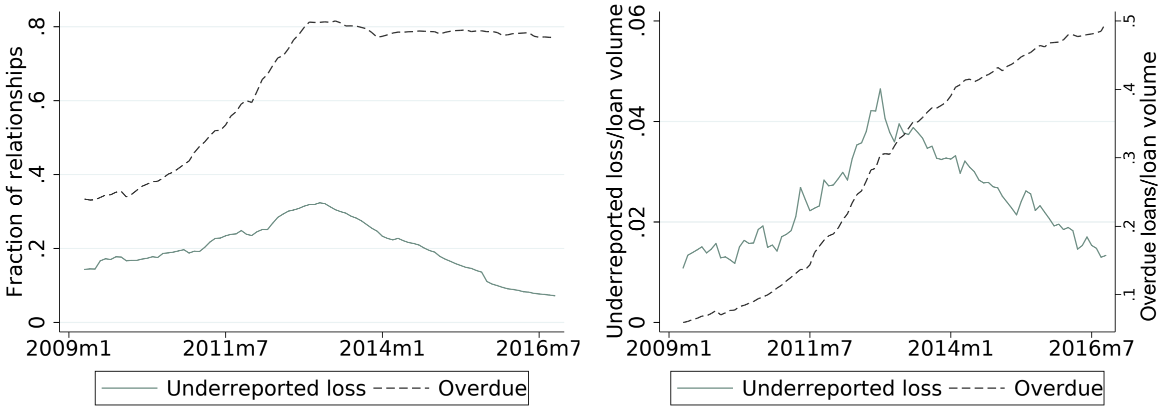
Underreporting of loan losses is a powerful tool to identify lending driven by distorted incentives. Banks only underreport about half of firms with overdue loans and this underreporting is very persistent, giving us meaningful variation among firms with overdue loans (see Figure 3 and Figure 6b in Appendix B).[23] 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 avoids the attenuation bias in measures of 'zombie' lending in the existing literature.

distorted that the predictions of the two sources of distorted lending incentives are borne out in the data. Table 5 in Appendix B shows that, among firms with overdue loans, underreported firms have statistically significant lower collateral values, hold more assets and a higher share of social security and other debt obligations to the government, which take seniority over any bank debt in Portugal. This is in line with the prediction that delaying losses is most important in lending relationships that have large uncovered losses in the case of firm insolvency.

[22]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.

[23]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.

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).

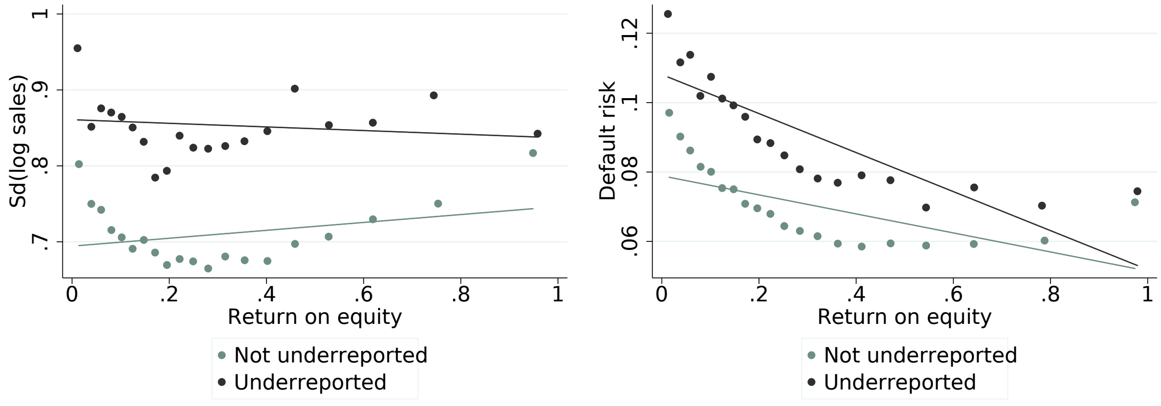
In line with the risk-shifting mechanism, we find that underreported firms display higher levels of risk for all levels of profitability relative to firms that have overdue loans but are not underreported. Panels a and b of Figure 4 plot firm-level risk measures (sales volatility and predicted default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

We now address two potential shortcomings of using underreporting to identify distorted lending incentives. First, our measure of loss underreporting only applies to firms that already have some overdue loans. However, in the time period we study a large number of firms fell behind on payments (see Figure 3 in Appendix B), implying that we capture a large fraction of lending in the economy. Moreover, distorted lending incentives are most likely to arise for firms that are already close to financial distress and likely to have already defaulted on a loan payment.

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

First, underreported firms show signs of severe financial distress. These firms are highly levered, have little cash, and exhibit low profitability and sales growth. Based on these observables, underreported firms do not look like firms that are likely to recover soon. We provide additional evidence in the next section that these signs of financial

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.

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 5 in Appendix B).

Second, we compare long-run outcomes for underreported and non-underreported firms. In Figure 7 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. While we expect some difference given the different risk profiles, we show that the trends are largely similar for the group of firms whose losses are underreported and those that are never underreported (but 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)[24] 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.[25] The EBA announcement was plausibly unexpected given that banks had already undergone a round of EBA stress tests in June 2011. The Financial Times in 2011 reports that the EBA requirements were "well beyond the current expectations of banks and analysts". The intervention led to a large capital shortfall for most eligible Portuguese banking groups since their Eurozone sovereign debt holdings were substantial and often valued above market prices in their balance sheets.[26]

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. 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.[27] As a result, the EBA announcement sparked expectations that banks would tap into the bailout fund to comply with the requirements. For example, Bloomberg reported: "A possible outcome is Portuguese banks have to tap the bailout fund, said Andre Rodrigues, an analyst at Caixa Banco de Investimento SA in Lisbon." 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. All banks with large capital shortfalls due to the EBA exercise received a government bailout in June 2012, which allowed them to comply with the EBA requirements.

We define a bank as exposed to the EBA intervention if it belongs to a banking group that was both subject to the intervention and had a large capital shortfall. We exploit variation in eligibility and variation in the EBA capital shortfall. The shortfall was driven by both quantity and valuation of banks' sovereign bond holdings. We use the variation in the shortfall to address the size imbalance that stems from the EBA targeting only the largest banks. Our control group hence consists of banks that were subject to the EBA intervention but had a small capital shortfall and also any commercial bank operating in Portugal not subject to the EBA intervention. We exclude any bank whose

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

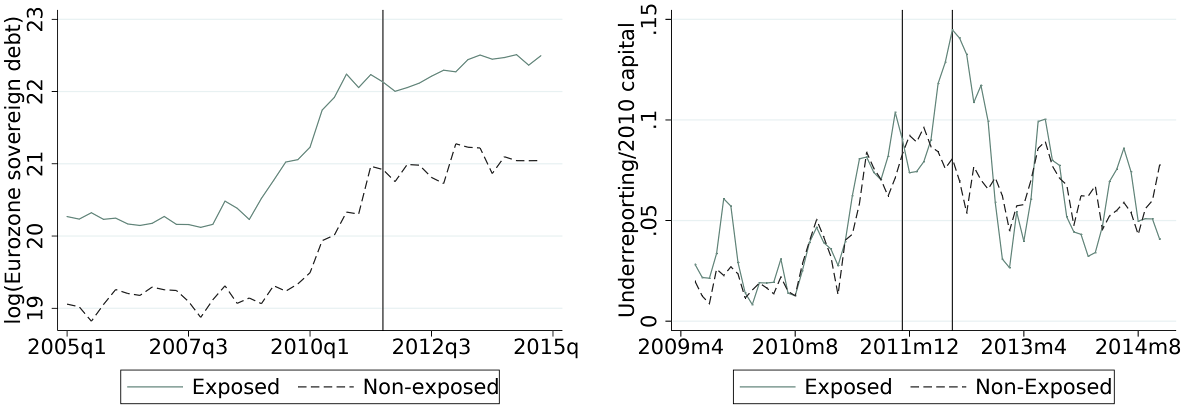
[25] Banks covered by the EBA exercise had to jointly hold at least 50% of the national banking sector as of the end of 2010 (EBA 2011).

[26] 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.

[27] The European Commission report on the Assistance Programme says: "The financing needs for the Programme's financial sector strategy primarily consist of a contingency provision to support stronger banking sector capitalization" (Commission 2011). see p. 16 of `http://ec.europa.eu/economy_finance/publications/occasional_paper/2011/pdf/ocp79_en.pdf`

foreign parent was subject to the EBA intervention in another European country. Using variation in pre-announcement sovereign debt holdings is valid as long these holdings are not correlated with other bank-level trends that affect credit allocation. Figure 5a shows that sovereign bond holdings followed parallel trends among the two groups prior to the EBA announcement providing evidence consistent with this assumption. Table 2 shows that both groups of banks are balanced on observables prior to the shock with some remaining size imbalance, which is to be expected given the selection criteria of the EBA intervention.

Figure 5: Comparing Behavior of Exposed and Non-exposed Banks



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

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

The EBA intervention temporarily heightened two sources of distorted incentives for exposed banks. First, exposed banks wanted to comply with the higher capital ratios but do so without raising costly new capital. Hence exposed banks had an incentive to boost reported capital by increasing the intensity of their loan loss underreporting and simultaneously rolling over loans to underreported firms. Figure 5b 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. Banks had an incentive to complement this underreporting with rolling over loans to firms with underreported losses. Doing so allowed banks 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. 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 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

Table 2: Descriptive Statistics: Firms and Banks

Firms	Baseline	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	Central bank funding	0.12	0.09	-0.03
	(0.17)		(0.11)	(0.06)	(0.04)
Fixed assets/assets	0.47	Interbank market	0.22	0.13	-0.09
	(0.29)		(0.20)	(0.11)	(0.06)
N	144,050		38	7	

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

$$\begin{aligned} g_{ibt}^{\text{credit}} &= \sum_{\tau=-2}^{5} \beta_\tau^{treat} \left(\text{period}_\tau \times \text{exposed}_b\right) + \sum_{\tau=-2}^{5} \beta_\tau^{period} \tau \left(\text{period} \times \text{underreported}_{ib}\right) \\ &+ \sum_{\tau=-2}^{5} \beta_\tau^{treatgroup} \left(\text{period}_\tau \times \text{underreported}_{ib} \times \text{exposed}_b\right) + \theta_{it} + \varphi_b \qquad (2) \\ &+ \beta_1^{base} \left(\text{underreported}_{ib} \times \text{exposed}_b\right) + \beta_2^{base} \text{underreported}_{ib} + \alpha_2 X_{ibt} + \epsilon_{ibt} \end{aligned}$$

where i, b and t index firms, banks and quarters respectively.[28] The main explanatory variables are exposed_b, a dummy variable that is 1 for banks exposed to the EBA intervention and underreport_{ib}, a dummy that is 1 if the lending relationship has underreported loan losses in the four quarters prior to the announcement of the shock. This dummy is based on our measure of underreporting.

period_τ is a dummy that indexes periods of three quarters. The periods of interest are

[28] 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.

the EBA shock (2011Q4-2012Q2) and the period following the EBA deadline (and bank bailout) (2012Q3-2013Q1). We also include two pre-period dummies and one post-bailout period dummy, all of which are of equal length.[29]

φ_b is a bank fixed effect and X_{ibt} are relationship level controls.[30] Standard errors are two-way clustered at the firm and bank level.[31] We follow the literature and estimate the effect on changes rather than (log) levels. The growth rate of credit is our dependent variable: $\mathrm{y}_{ibt} = \mathrm{credit}_t/\mathrm{credit}_{t-1} - 1$. The growth rate allows us to decompose the total change in credit into the portion coming from overdue credit and the portion coming from performing credit (credit that is not overdue).

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

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

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

Results Figure 6a shows our main credit results (see also Table 7 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 $\mathrm{period}_\tau \times \mathrm{underreported}_{ib} \times$

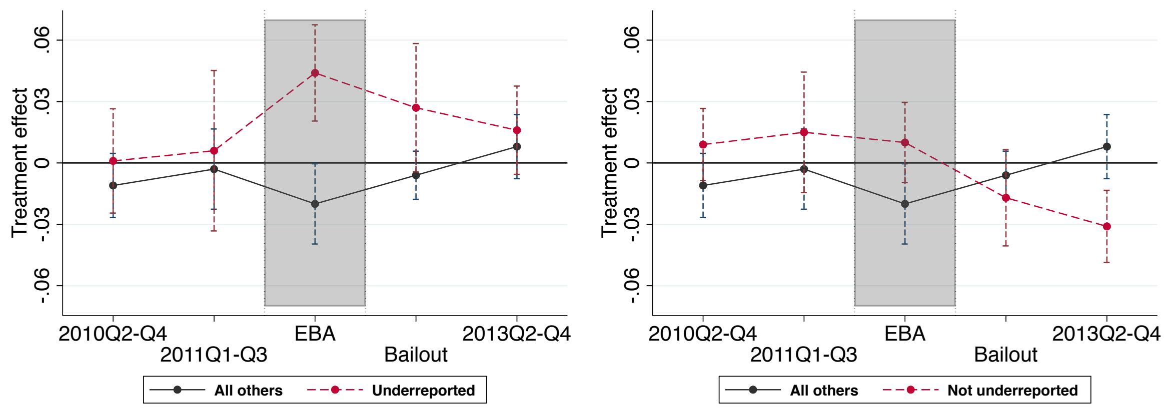
[29]The two pre-periods allow us to test for pre-trends in credit allocation, while the inclusion of the post-bailout period allows us to study the evolution of credit following the EBA deadline. The sample period includes 2009Q1-2014Q4 which allows us to estimate each $\beta_\mathcal{T}$. This implies 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.

[30]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

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

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 credit crunch for all other lending relationships at exposed banks. The coefficient on EBA$_t$ $\times$ exposed$_t$ in equation 2 is negative and statistically significant (Figure 6a and columns 2 and 3 of Table 7). The magnitude of the shock is large. The baseline treatment effect of borrowing from exposed banks is a 2 percentage point (p.p.) drop in quarterly credit growth between the announcement and deadline of the EBA intervention. In contrast, the treatment effect for underreported, distressed firms is an increase in credit growth at exposed banks of just over 2 p.p.[32] These changes are equivalent to 4% of a standard deviation of credit growth.

Figure 6: 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$\times$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$_\tau$ $\times$ exposed$_b$ and period$_\tau$ $\times$ underreported$_{ib}$ $\times$ 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$_\tau$ $\times$ not underreported$_{ib}$ $\times$ 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 7 in Appendix B for point estimates. N = 1,981,219.

If loss underreporting correctly identifies firms for which distorted lending incentives drive additional lending, we should find that exposed banks do not increase credit supply to firms that are distressed but are not subject to underreporting. In Figure 6b, 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 prior to the shock. 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.[33]

The results suggest that effects are driven by changes in bank credit supply in response to the EBA intervention. There is no evidence of differential credit allocation at exposed

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

[33]See also Table 6 in Appendix B

banks in the two periods prior to the shock, lending credibility to our parallel trends assumption. The lack of pre-trends applies to both the baseline group of firms and to the subgroup of underreported, distressed firms. Second, the preferential credit treatment for underreported, distressed firms only occurs during the period of the EBA shock when exposed banks face heightened distorted lending incentives. Similarly, the credit crunch only occurs in the period of the EBA shock when banks attempt to comply with tighter capital requirements. We provide a series of further robustness checks in Table 8 in Appendix B.[34]

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

The effect on changes in total credit is almost entirely driven by performing credit (column 4 of Table 7 in Appendix B). If underreported, distressed firms were simply converting more of their performing loan balances into overdue loans, we would expect no change in total credit, a reduction in performing credit, and an increase in overdue credit. Instead, we find an increase in total credit, an increase in performing credit, and a (statistically insignificant) reduction in overdue credit.

There are similar patterns when looking at the probability that a bank grants a new loan. We construct a dummy that is one if there is a new loan in a firm-bank relationship.[35] Column 6 of Table 7 in Appendix B shows that we find a large significant increase in the probability that a new loan is granted to a underreported, distressed firms at exposed banks in the period of the EBA shock. In contrast, the probability declines for all other firms at exposed banks.

The differential credit behavior is also visible at the extensive margin. The probability that an exposed bank cuts a relationship increases by almost 6 percentage points during the EBA shock (Table 9 in Appendix B).[36] In contrast, the probability falls for

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

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

[36]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.

underreported, distressed firms.[37]

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 fully dynamic differences-in-differences specification[38]

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

where treatment_i is the firm-level borrowing share from exposed banks prior to the shock.[39] 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.[40] underreported_i is a dummy that captures firms with underreported losses prior to the announcement of the shock. Standard errors are clustered at the firm-level.

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

Results Figure 7a 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

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

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

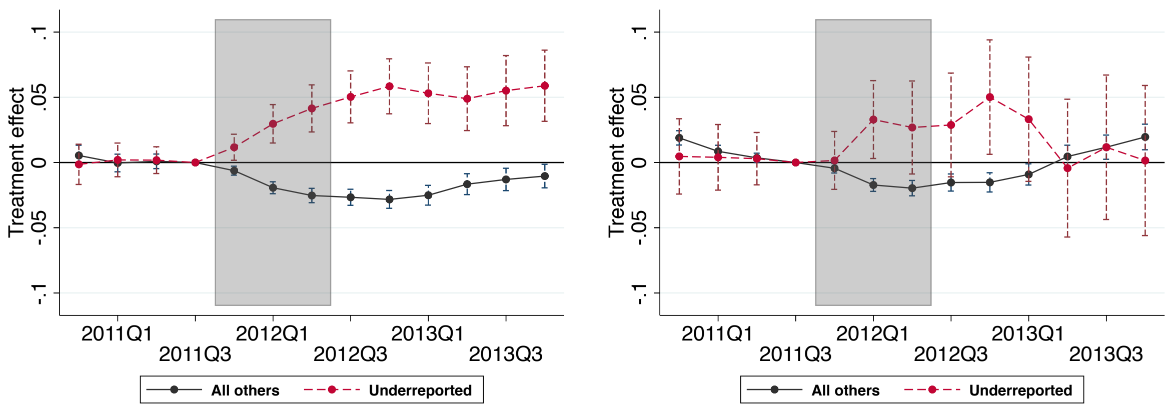
[39]Following (Chodorow-Reich (2014)) this is defined as $\text{treat}_i = \frac{\sum_{b=1}^{B^{exp}} L_{ib,pre}}{\sum_{b=1}^{B^{all}} L_{ib,pre}}$ where $L_{ib,pre}$ denotes the stock of total credit of firm i at bank b in 2010. B^{exp} is the set of exposed banks, while B^{all} is the set of exposed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

[40]Figure 5 in Appendix B shows that we have variation in treatment intensity.

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 9a in Appendix B). This result suggests that banks can stave off additional default for underreported firms in the short-run but eventually their default rates catch up. This result, together with the absence of pre-trends at the firm-level, provides further support for the argument that the credit reallocation is not driven by underlying differences in firm-level quality or liquidity trends. The increase in credit during the EBA intervention is again driven by performing credit as shown in Figure 7b.

Figure 7: 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$_t$ $\times$ treatment$_i$ and quarter$_t$ $\times$ treatment$_i$ $\times$ 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$\times$quarter and firm fixed effects, as well as firm-level controls interacted with quarter. All coefficients should be interpreted as changes in the dependent variable relative to the (normalized) base quarter 2011Q3. Standard errors are clustered at the firm-level. N= 1,346,771.

The economic significance of the credit reallocation is large. For underreported, distressed firms, the total treatment effect of borrowing exclusively from exposed banks versus borrowing exclusively from non-exposed banks is equal to a 16% increase in total credit relative to the base quarter (2011Q3).[41] For all other firms, the total treatment effect is a decline in credit of 14% relative to the base quarter.

[41] This is the cumulative effect over the combined EBA and bailout period, which runs from 2011Q3 to 2013Q1. A standard deviation in the borrowing share in our sample is the equivalent of borrowing entirely from exposed and borrowing entirely from non-exposed. For underreported firms, this is the

3.3.3 Effects into Employment and Investment

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

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

where i and s index firms and industries, respectively.

We instrument for $\Delta \log \text{credit}_{is}$ with the firm-level borrowing share from banks exposed to the EBA shock. We include the same controls as in the firm-level credit specification, equation 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.[42] 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[43] and a 11% pass-through into employment (see Table 3). If we allow for the effect of exit, the pass-through into employment jumps to 60%. The first-stage F-statistics are close to 200, 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 10 in Appendix B). However, it is difficult to precisely estimate the long-run pass-through since the credit shock is short-lived and hence the instrument loses power after 2012. For additional robustness, we show that results are similar when dropping firm-level controls and the lagged dependent variable (see Table **??** in Appendix B). We also conduct placebo exercises running the same specification in the years prior to the shock

total treatment effect $\beta_{\tau}^{treat} + \beta_{\tau}^{treatgroup}$ in equation 3, or in other words, we add the two coefficients displayed in Figure 7a.

[42] The formula is

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

[43] 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).

Table 3: Pass-Through Into Employment and Investment

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

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

and find no significant effects (see Table 10 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.[44] 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 credit demand. For this assumption to be violated in the context of our triple-difference design, banks have to underreport firms with better long-run fundamentals, those firms have to experience temporary financial distress driving up their credit needs coinciding exactly with the duration of the EBA intervention, and the nature of lending relationships has to be such that only exposed banks are in a position to respond to these additional credit needs. To address this possibility, we first provide evidence that observable characteristics of underreported firms are not

[44] A standard deviation in the borrowing share in our sample is the equivalent of borrowing entirely from exposed and borrowing entirely from non-exposed. We can multiply the firm-level coefficient from the first-stage credit regression with the pass-through coefficient (0.14*0.353 for investment and 0.14*0.596 for employment.

systematically correlated with how much they borrow from exposed banks prior to the EBA intervention (see Figure 8a). Turning to the firm-bank level, Figure 8b shows that EBA banks are no more likely to be the main lender, to grant a different level of credit, or to have a different share of performing credit. EBA banks seem to have slightly longer lending relationships and firms on average account for a larger share in the EBA banks' loan portfolio. These differences are likely to reflect that exposed banks on average are larger and have been present in Portugal for longer. To account for these differences, we control for relationship characteristics in all firm-bank level specifications.

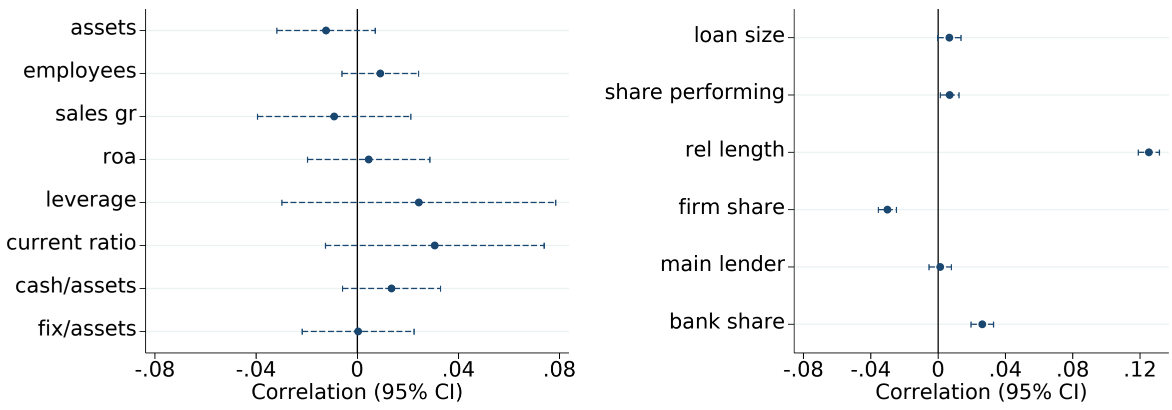
Second, we investigate the potential presence of differential financial shocks driving outcomes. Given that we absorb any firm-level changes by firm×time fixed effects in our main specification, differential shocks to credit demand provide a potential challenge only for our firm-level regressions. At 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 at the annual level with liquidity ratios as the dependent variable. Figures 8a - 8b in Appendix B show that there are no pre-trends in either the current ratio or the cash/assets ratio for these firms. Figure 8c in Appendix B plots firm×quarter fixed effects from a regression that decomposes credit change of firms with multiple lending relationships into a bank and a firm-time component. The firm×quarter fixed effects can be interpreted as a measure of firm-level credit demand. There is no evidence of differential trends in credit demand prior to the EBA intervention.

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

4 Measuring the Effect on Misallocation and Productivity

We show that the EBA intervention can account for over 50% of the decline in productivity in 2012. 40% of this effect is driven by the reallocation of credit to distressed,

Figure 8: 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.

underreported firms. To establish this result, we follow the popular approach of inferring the presence of distortions, which give rise to factor misallocation, by measuring wedges, or gaps, in firms' first-order conditions (Restuccia and Rogerson (2008), Hsieh and Klenow (2009)). 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 negative effect on productivity is driven by the heightened misallocation of capital across firms, which occurs through three channels. First, the EBA shock causes credit, and capital, to be reallocated away from firms where an additional unit of capital would earn a high return, and instead towards distressed, underreported firms where an additional unit of capital earns a low return. Second, firm-level distortions, measured by marginal product gaps, increase in response to the credit shock. Third, there are negative spillovers from distressed, underreported firm to healthy firms in the same industry.

We find mixed effects for labor misallocation. Keeping the firm-level labor distortions constant at the pre-shock level, we find that the allocative efficiency of labor improves. This potentially reflects the fact that the credit crunch corrects some of the pre-crisis over-expansion of firms with low labor productivity. However, we also find that, like capital distortions, firm-level labor distortions grow in response to the credit shock, suggesting that the allocative efficiency of labor declines.[45] The EBA intervention has a small

[45]We find small positive effects on the allocative efficiency of intermediate inputs and no evidence that

positive effect on the allocative efficiency of intermediate inputs. We find no evidence that the credit shock induced by the EBA intervention affects firm-level TFP.[46]

4.1 Decomposing Productivity Growth

We compute the contribution of the EBA intervention to aggregate productivity growth using a decomposition due to Petrin and Levinsohn (2012). 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 technical efficiency 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.[47]

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

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

where M and S index materials and services.

We define aggregate productivity growth (APG) as the difference between the change in the value of final output and the change in the costs of primary inputs (all deflated).

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

where W_{xi} denote the price of input x for firm i and K and L index capital and labor.[48]

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

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

intermediate input gaps respond to the credit shocks. This is line with existing work that finds that distortions matter more for primary than intermediate inputs

[46] Firm-level TFP, which we call technical efficiency below, is the efficiency with which firms combine inputs to generate output

[47] 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.

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

where $D_i = \frac{P_i Q_i}{\sum_i VA_i}$ is a Domar weight[49], $s_{xi} = \frac{W_{xi}\text{input}_{xi}}{P_i Q_i}$ 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. 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 gaps, sometimes to referred to as wedges (see for example Hsieh and Klenow (2009)). To the extent that gaps are driven by distortions such as financial constraints, taxes, or market failures, reallocating inputs to firms with high gaps increases aggregate productivity. In turn, anything that leads inputs the be allocated *away* from high gap firms and towards low gap firms reduces productivity and therefore output.

We can take the decomposition in equation 7 to the data using the following approximation[50]

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

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 technical efficiency. Our preferred method estimates production function parameters separately for each 3-digit industry using cost shares.

We show that Portugal, like other Eurozone periphery countries, experienced negative productivity growth in the years leading up to the sovereign debt crisis. These estimates incorporate the services sector, which represents about 75% of employment and value added in Portugal (see Dias et al. (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 increasing misallocation of inputs across firms, in particular the misallocation of capital.[51] 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. Our results are also consistent with the argument that capital inflows into Portugal following the introduction of the euro were largely going into low productivity sectors (Reis (2013)).[52] Table 4 shows that productivity growth took a sharp hit with the onset

[49]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.

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

[51]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.

[52]Unlike the Hsieh and Klenow (2009) framework which focuses on within-industry factor dispersion,

Table 4: Aggregate Productivity Growth (APG) Decomposition

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

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

of the sovereign debt crisis in 2011. We are interested in quantifying the contribution of the EBA intervention to the pronounced decline in productivity growth in 2012.

4.2 Direct Channel

The productivity decomposition in equation 8 tells us that the EBA intervention can affect productivity growth in two ways. First, credit shocks could directly impact firm-level technical efficiency. 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.[53]

The appeal of the decomposition in equation 8 is that we can estimate the impact of the EBA intervention on both firm-level technical efficiency and input use, and then map the predicted changes directly into productivity growth. To obtain these predicted changes, we combine the estimate of the size of the credit shock with estimates of the pass-through of the credit shock into input use and technical efficiency. 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

the Petrin and Levinsohn decomposition also incorporates between-sector misallocation.

[53] 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.

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

where $\hat{\delta}^{treat}$ is the estimated treatment effect in the firm-level credit regression, specification 3,[54] and $\hat{\gamma}^L$ is the estimated pass-through coefficient into employment growth based on specification 4 in section 3.[55] 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 technical efficiency and deflated intermediate inputs.

While we find large and significant pass-through into all four (deflated) inputs, we find no significant pass-through into firm-level technical efficiency (see Table 5). Therefore, we treat the effect of the EBA intervention on technical efficiency as zero and focus on the effect on factor misallocation. This result highlights that simply measuring the effect of credit shocks on firm-level TFP residuals would not capture the the effect of the EBA intervention on aggregate productivity.[56]

We then compute input changes due to the EBA intervention based on equation 9 and map these changes into the productivity decomposition in equation 8.[57] The result tells us how much aggregate productivity growth, in partial equilibrium, can be explained by the EBA intervention. In other words, it tells us the productivity losses we could have avoided in a hypothetical world where all firms had borrowed from non-EBA banks (assuming those banks would have left their behavior unchanged). 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 suggests that the credit crunch led some firms, which had expanded excessively prior to the crisis, to cut back on labor and intermediate inputs.

This counterfactual is partial equilibrium in nature and may over- or understate the true effects on productivity, depending on the sign of the general equilibrium effects of the EBA intervention. For example, it is possible that the negative credit shock

[54] 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.

[55] For the subset of underreported firms, the size of the credit shock is given by the total treatment effect $(\hat{\delta}^{treat} + \hat{\delta}^{underreport})\text{treatment}_i$.

[56] TFP residuals are not generally informative about firm-level gaps 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)).

[57] We assume revenue shares and Domar weights to be constant in this exercise. We investigate potential changes in revenue shares due to the EBA intervention in the next subsection.

Table 5: Pass-Through Into Input Use and TFP

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

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

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

for firms borrowing from exposed banks would have led their competitors borrowing from non-exposed banks to increase their input use. If competitors have high marginal products, then this effect may have moderated the negative effect on productivity. 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.

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.[58] We proceed in two steps. First, we

[58]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.

Table 6: Aggregate Productivity Growth (APG): Counterfactuals

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

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

isolate the effect of the credit crunch by keeping the level of the credit crunch constant but changing the incidence of the credit shock. We assume that underreported, distressed firms receive the baseline credit crunch treatment and simulate assigning their positive treatment effect instead to a randomly chosen subset of non-distressed firms. We run this simulation 10,000 times (for 10,000 different subsets) 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. This suggests that some the underreported, distressed firms have higher marginal return on labor and intermediate use than some non-distressed firms.

The previous exercise therefore constitutes an upper bound, which assumes that the entire credit crunch is driven by the reallocation.

4.3 Changes in Firm-Level Input Distortions

Up until now, we have assumed that firm-level input gaps in equation 8, our measure of firm-level distortions, stay constant. In other words, we have kept firm-level input distortions constant and focused on whether underreported firms that benefit from credit reallocation have smaller or larger distortions than those whose credit supply, and input use, shrink as a result of the EBA intervention. However, a credit shock may not just change the use of an input but also affect the input's revenue share, which is a key ingredient in our measure of distortions. A significant pass-through of the EBA shock into firm-level gaps is both additional evidence that the credit distortions drive the misallocation of inputs and suggests that the contribution of the EBA intervention to the decline in productivity, which are based on constant revenue shares, are a lower bound estimate.

We again rely on our firm-level IV specification given by equation 4 to estimate the pass-through into firm-level gaps. The dependent variables are now log changes in the absolute value of firm-level gaps 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.[59]

We find significant pass-through into firm-level labor and capital gaps of about 12-17% (see panel b of see Table 5). This is smaller than the pass-through into input use. We find no statistically significant effects on the gaps 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 gaps 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 up gaps.[60] A growing literature has argued that misallocation measures based on firm-level gaps 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 gaps, are not the right benchmark for efficiency (Asker et al. (2014), Restuccia and Rogerson (2017)). This poses a threat to identification if any of these forces are correlated with the firm-level borrowing share from EBA banks. However, we do not find evidence that the firm-level borrowing share from EBA banks is correlated with firm-level sales or productivity volatility nor with sales cyclicality.[61] Nonetheless, we show that the results are robust to controlling for firm

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

[60]This is simply a variant of the exclusion restriction maintained throughout the paper.

[61]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 technical efficiency. The correlations are -0.012 for firm-level cyclicality. (measured as correlation of firm-level log sales with industry-level log sales), -0.0098 for firm-level productivity volatility and -0.0221 for firm-level volatility of log sales.

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

We quantify the productivity losses from this channel by regressing input use and technical efficiency 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 pre-period characteristics.

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.[62] 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.[63]

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 technical efficiency. In Appendix

[62] 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).

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

B, we show that these results are robust to using less or more fine-grained industry definitions.

Table 7: Regression Results: Spillovers

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

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

The 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.[64]

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

4.4.1 Aggregate Demand and Fire Sale Externalities

Given that Portugal experienced a recession in 2012, it is possible that the reallocation of credit to underreported firms, which prevented them from exiting, may have had positive spillover effects that partly offset the negative spillovers highlighted so far. For example, aggregate demand externalities or fire sale externalities both suggest reasons why avoiding the sudden exit of a large number of underreported firms would have had positive effects on firms in the same industry.[65] To get a sense of the potential magnitude of these effects, we re-run the spillover regression with two sample splits.

First, we split the sample based on tradable and non-tradable industries. The effect of local demand conditions, and hence aggregate demand externalities, should matter more for non-tradable industries (Mian and Sufi (2014)). If we estimate significantly smaller spillover effects in non-tradable industries, this would be evidence of moderating positive spillovers due to aggregate demand externalities. Following Amador and Soares (2012), we define an industry as tradable if it has an average, asset-weighted, export-share above 15%.[66] We find that capital spillovers are lower in non-tradable industries but the difference is only significant at the 10% level (see table 7). Moreover, we obtain the opposite result for sales.

Second, we split the sample on the degree of collateralization. We expect industries with a high share of real collateral to experience more severe fire sales. We compute the ratio of real collateral value to total loan balance based on credit register data and split industries into high and low collateral industries at the median.[67] Table 7 shows that capital spillovers are significantly larger in low collateral industries consistent with a moderating effect from avoiding collateral fire sales. However, we find the opposite effect for sales. Based on these results, there is no clear evidence that either aggregate demand or fire sale externalities play a large role in mitigating negative spillovers.

5 Subsidized Lending and Credit Misallocation

We now provide evidence that the costs of undercapitalized banks persist beyond the EBA regulatory intervention. We exploit a novel dataset on new lending operations available after the EBA intervention which allows us to identify new loans that receive a subsidized interest rate via a matching procedure. We provide evidence that banks with a high shadow cost of capital are more likely to grant new subsidized loans to underreported, distressed firms.

[65] Aggregate demand externalities arise when individual firms do not internalize the impact of their decisions on aggregate spending and income in the presence of a zero lower bound on (nominal) interest rates and nominal rigidities (Farhi and Werning (2016)). Fire sale externalities arise when banks are suddenly trying to foreclose on loans at the same time and do not internalize the negative effect on prices by selling collateral at the same time.

[66] We work at the 3-digit level and drop industries with fewer than seven firms.

[67] This is an imperfect test since there may be spillover from firesales of collateral in other industries.

5.1 Identifying Subsidized Loans

We rely on a dataset recording all new lending operations to new and existing clients at major Portuguese commercial banks that has been collected by the Portuguese central bank since mid-2012. Our sample consists of over 1.5 million new lending operations from June 2012 to May 2015.[68]

Following existing work (Caballero et al. (2008), Moreno-Serra et al. (2016) and Acharya et al. (2017)), we classify a firm as subsidized if it pays a lower interest rate than an investment grade firm. However, unlike the existing literature that defines subsidies based on an imputed interest rate at the annual firm-level, we can identify subsidies at the loan-level. In particular, we match each lending operation to a non-investment grade firm to three loans granted to an investment grade firm.[69] We match (with replacement) on loan amount, maturity, and the length the interest rate is fixed. In addition, we force an exact match on the firm size category, date of origination (quarterly level), lender and two-digit industry. A new lending operation to a non-investment grade firm is considered subsidized if it has an interest rate lower than the average interest rate on the three matched loans to investment grade firms. Selecting multiple matches ensures that we do not spuriously pick up on loans with particularly high interest rates as matches. Using investment grade firms as our benchmark is a conservative lower bound since investment grade firms tend to pay the lowest interest rates in the economy (controlling for loan characteristics).

For 87% of operations, we find three close matches.[70] We confirm that we do not pick up loans with lower credit volume, less collateral, shorter maturities, or floating rates, all of which we would expect to lower interest rates.[71] We provide additional descriptive statistics in Appendix B.

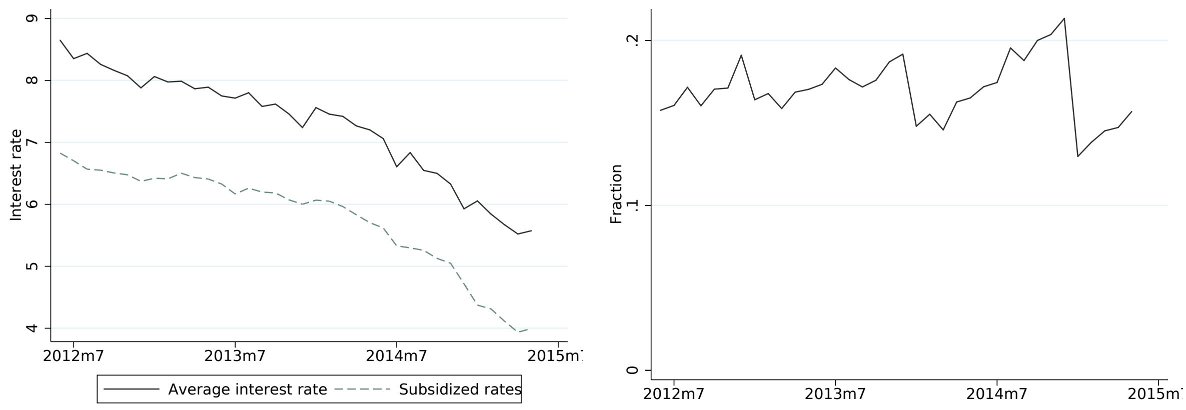
[68] We drop all operations with zero maturity since these reflect account overdrafts rather than genuine new loans. We also drop observations with any missing information. The data quality is very high so that only very few observations are dropped due to missing information.

[69] We define an investment grade firm based on the credit rating model for non-financial firms by Antunes et al. (2016). The default risk cut-offs for investment grade are designed to match those of rating agencies. We have to rely on this constructed rating model since most Portuguese firms do not issue bonds and hence have no market rating.

[70] We drop about 2% of the roughly 4 million matches that have a squared Mahalanobis distance above 10. The remaining matches have an average Mahalanobis distance of 0.2 with a standard deviation of 0.97. The median is very small: $2.4x10^{-4}$. This suggests that our average match quality is very high.

[71] In Table 11 in Appendix B we provide the p-values from one-sided t-tests in the direction that we would expect to lower interest rates. We can reject differences between subsidized loans and their matches on all these characteristics.

Figure 9: Subsidized New Lending Operations



(a) Interest Rates (b) Fraction Subsidized

Notes. The left panel shows the average interest rate on new lending operations by major Portuguese commercial banks. The dashed line shows the average interest rate on subsidized lending operations (in percent). The right panel shows the fraction of new lending operations that we identify as subsidized. The seasonal patterns is likely due to bank's attempt to 'clean up' balance sheets and reduce their non-performing loan ratios at the end of the year.

5.2 Loan Subsidies as Evidence of Perverse Lending Incentives

To investigate whether undercapitalized banks are more likely to grant subsidized loans, we run the following panel regression at the monthly firm-bank level.

$$Pr(\text{new loan})_{ibt} = \beta \left(\text{lowcap}_{b,t-3} \times \text{underreported}_{ib,t-3}\right) + \text{controls} + \varphi_b + \theta_{it} + \nu_{ibt} \quad (11)$$

The dependent variable is a dummy if a firm receives a new subsidized loan. The main explanatory variable is a lagged indicator if a bank has a high shadow cost of capital, which implies that the bank is undercapitalized or close to being undercapitalized. To measure this shadow cost, we divide banks into quartiles based on the intensity of their loss underreporting in a given quarter.[72] We compare the top quartile, who underreport most intensely, with the bottom quartile. In section 3, we provide evidence that loss underreporting increases when the bank's shadow cost of capital increases as the bank is trying to inflate the value of its capital. This implies that the intensity of underreporting is good indicator for a bank's shadow cost of capital. This bank-level indicator is lagged by three months in order to mitigate concerns of reverse causality or spurious contemporaneous correlation between the degree of underreporting and credit allocation.

The regression includes bank and firm×month fixed effects. Our estimates are hence identified off periods of high shadow cost versus low shadow cost for the same bank, and off firms that receive a new lending relationship from two different banks in the same month. We also include an interaction between the bank-level measure of the shadow

[72]To account for the fact that loss underreporting is likely to rise with a larger stock of non-performing loan, we scale by the total stock of overdue loans.

cost of capital and a lagged firm-bank measure of loss underreporting. The rationale is that underreported firms are likely to benefit from distorted lending incentives of low capital banks.

Unlike in our EBA set-up, we no longer have exogenous variation in the capital adequacy of banks. We have to assume that the regression error term is uncorrelated with the lagged indicator of the bank's shadow cost of capital. This identification assumption would be violated if firms that are likely to receive a subsidy increase their demand for new loans during a time when the bank has a high shadow cost of capital. We include bank-level controls to absorb some of the time-varying bank-level drivers of credit allocation, namely the liquidity ratio, deposit ratio, interbank ratio, log total assets, and risk-weighted assets. We also include the same relationship level controls as in section 3 to account for potential differences in relationships that are not absorbed by the firm×month fixed effects.[73] Standard errors are either two-way clustered at the firm and bank level or clustered at the bank-level.

Results The results in Table 8 show that banks are more likely to grant a subsidized loan when they have a high shadow cost of capital. Moreover, the predominant beneficiaries are underreported firms in financial distress. Similar to our finding in the EBA context, firms in distress that are not underreported are not more likely to obtain a subsidized loan. These results are robust to excluding controls and including separate firm and month fixed effects instead of firm×month fixed effects. The baseline effect is sensitive to the inclusion of a bank fixed effect although the effect for underreported firms is not.[74] We also show that we obtain similar results when we use an indicator if the bank was below the median Core Tier 1 capital ratio in the previous quarter.

6 Loss Underreporting and Capital Adequacy

We quantify by how much banks overstate their regulatory capital due to the underreporting of loan losses. Without loss underreporting, banks affected by the EBA intervention would have had to raise additional funds equal to between 4% and 20% of their regulatory capital, depending on the assumptions we make in the calculation. In the post-EBA period, almost 20% of banks in the sample, including several large ones, would at some point have violated their capital constraint when fully accounting for all loan losses on their balance sheets. These results suggest that underreporting presents a financial stability risk because banks' regulatory capital must cover not only unexpected loan losses but also the (expected) losses that banks have not yet recognized.

[73]The controls are the portfolio share of the firm at the bank, the lending share of the bank, the length of the lending relationship and whether the bank is the firm's main lender.

[74]We have a smaller sample of banks compared to section 3 since not all Portuguese banks are required to report their new lending operations.

Table 8: Regression Results: Subsidized Loans

	(1)	(2)	(3)	(4)
	Measure bank shadow cost by			
Pr(subsidized loan)	Intensity of loss underreporting		Proximity to capital constraint	
High shadow cost of capital$_b$	0.008	0.007	-0.003	-0.003
	[0.005]	[0.006]	[0.003]	[0.003]
Underreported$_{ib}$	-0.032***	-0.025***	-0.018***	-0.014***
	[0.006]	[0.003]	[0.006]	[0.004]
High shadow cost$_b$×underreported$_{ib}$	0.043***	0.030***	0.018**	0.013***
	[0.012]	[0.008]	[0.007]	[0.004]
Bank share	0.053***	0.055***	0.048***	0.047***
	[0.006]	[0.008]	[0.005]	[0.006]
Firm share	-0.000	-0.000*	-0.147	-0.056
	[0.000]	[0.000]	[0.149]	[0.102]
Main lender	0.007*	0.009**	0.009***	0.009***
	[0.004]	[0.004]	[0.003]	[0.002]
Bank FE	Y	Y	Y	Y
Firm×month FE	Y	N	Y	N
Firm, month FE	N	Y	N	Y
Bank controls	Y	Y	Y	Y
N	392,367	392,367	1,379,642	1,379,642
R2	0.474	0.192	0.422	0.166
Banks	29	29	31	31

Notes. Regression results are based on monthly data on new lending operations from June 2012 to May 2015 for major Portuguese commercial banks. The dependent variable is a dummy if the bank grants a subsidized new loan. Subsidies are identified according to the matching procedure discussed in section 5. We measure the shadow cost of capital in two ways. Columns (1)-(2) show results when comparing banks in the highest quartile of loss underreporting to banks in the lowest quartile of underreporting. Columns (2)-(4) show results when comparing banks above and below the median core tier 1 capital ratio. Both indicators are lagged by a quarter. Underreported identifies relationships subject to loss reporting in the previous quarter. Bank share refers to the lending share of the bank. Firm share is the weight of the firm in the bank's loan portfolio. Main lender is a dummy if the bank is the firm's main lender. Standard errors are two-way clustered by firm and bank.*, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

We compute the additional capital needs in the absence of loss underreporting by calculating the loss if banks had applied a higher deduction rate on the loans where we detect underreporting. This loss is given by

$$\text{loss}(t;k) = \sum_{q}^{3} \omega_k^q (\text{rate}_{k+j}^q - \text{rate}_k^q) E(t;k), \tag{12}$$

where t indexes months, q indexes one of three collateral types, k indexes the reporting buckets described in section 2, and j indexes the assumption we make about the correct regulatory rate. $E(t;k)$ is the excess mass returned by the algorithm for a given firm-bank pair in month t and reporting bucket k. rate_k refers to the regulatory deduction rate for bucket k and collateral type q. ω_k^q is the fraction of lending in bucket k that has collateral type q. For example, suppose a firm has an overdue lending balance of EUR 100 in the third reporting bucket, all of which is backed by real collateral. The bank has to apply a deduction rate of 1% to this overdue loan balance. The next higher bucket $k+1$ has a deduction rate of 10% if the loan remains collateralized. If we detect that the EUR 100 are underreported and, assuming that the bank should have deducted at the rate of the next higher reporting bucket, then this implies an additional capital shortfall equal to $(0.10 - 0.01) \times 100 =$ EUR 9.[75]

For each bank, we aggregate the shortfalls across reporting buckets and corporate borrowers. We then deduct the resulting shortfall from the bank's regulatory capital to obtain a counterfactual capital ratio. This exercise is conservative since we cannot track the absolute discrepancy between the reported and 'actual' time overdue. We therefore trace out the potential range of shortfalls by varying j, our assumption on the correct deduction rate. The lower bound assumes that the bank should have deducted impairment losses at the next higher rate ($j = 1$). As an upper bound we calculate the impairment losses which would have resulted from fully writing off the loan ($j = j^{max}$ and $\text{rate}_{j^{max}} = 1$).

Up to now our calculations only capture underreported losses in the corporate loan portfolio. We now extrapolate to household loans.[76] We calculate the underreported impairment losses as a fraction of the total corporate loan portfolio and apply this impairment fraction to household loans. Since the same regulatory rules governing impairment losses apply to household loans, banks are likely to use the same type of loss underreporting for household loans.[77]

In Table 9, we calculate the additional impairment losses had banks not engaged in underreporting during the EBA intervention. The average bank-level shortfall ranges from 2% to 10% of the pre-intervention Core Tier 1 capital when considering only corporate loans. Extrapolating to household loans increases the average shortfall to 6%-20% (where 20% assumes that the bank should have fully written off all lending subject to underreporting). Given that the government stepped in to bail out the banks affected by the EBA intervention, our calculations imply an additional government liability of 0.3 - 4 bn EUR if banks had fully recognized these losses. Compared to 6 bn EUR which the Portuguese government injected into banks at the EBA deadline, this would have meant a potentially large increase in the bank bailout.

We conduct a similar counterfactual exercise on capital shortfall in the period following the EBA intervention. In this exercise, we also take into account unrecognized losses arising from the credit subsidies we document in section 6. We assume that a firm receiv-

[75] In this exercise, we do not consider a potential impact on the denominator (risk-weighted assets). Most corporate loans already carry a risk-weight of a 100 and remain at this risk weight as long as they are adequately provisioned.

[76] Household loans include consumer loans and household mortgages.

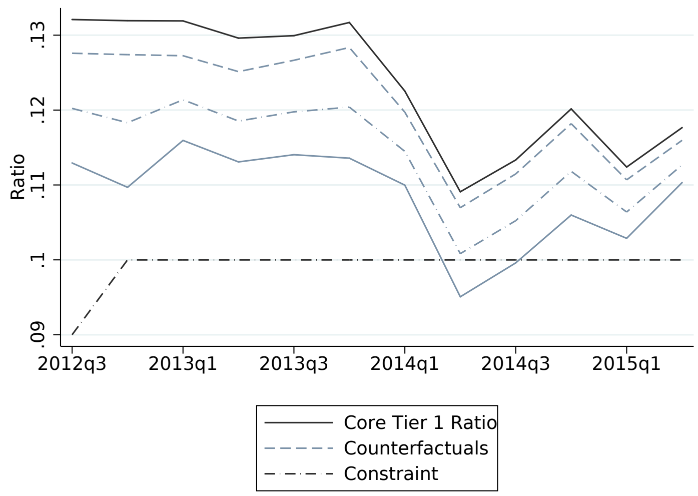
[77] In contrast to corporate loans, impairment losses on household mortgages in Portugal are not tax deductible, giving banks an ever stronger incentive to reduce impairment losses on household loans.

Table 9: Capital Shortfall Due to Underreported Losses during EBA Intervention

Average across exposed banks	EUR (m)	% of capital
Core Tier 1 capital	13,500	
Risk-weighted assets	140,000	
Shortfall, lower bound	274	2%
Shortfall, upper bound	1410	10%
Adding household loans, lower bound	755	6 %
Adding household loans, upper bound	3890	20%

Notes. The table displays averages for the group of banks exposed to the EBA intervention. All numbers are in EUR millions. Shortfall refers to the losses not accounted for in banks' corporate loan portfolios due to underreporting. The lower and upper bounds are obtained by making different assumptions on the correct deduction rate when calculating the underreported losses (see text). In the last two rows, we extrapolate to underreported losses in the household loan portfolio of exposed banks. Core Tier 1 capital and risk-weighted assets are in 2011Q3 numbers while shortfalls are calculated across the EBA intervention period 2011Q4-2012Q2.

Figure 10: Counterfactual Capital Ratios Without Loss Underreporting



Notes. The graph shows the average Core Tier 1 capital ratios of Portuguese banks and compares this to the counterfactual capital ratios if banks had fully accounted for all underreported losses. The first dashed line is the counterfactual based on only calculating the amount of underreported impairment losses in the corporate loan portfolio. This number also incorporates the extrapolation to household loans described in section 6. The bottom two lines add losses from the subsidized loans we document in section 5 assuming distressed firms would have defaulted in the absence of the subsidies. The bottom line assume full default while the second lowest line assumes partial default. The horizontal dashed line indicates the minimum regulatory capital ratio.

ing a subsidized loan would have (partially) defaulted in the absence of the subsidy.[78] In Figure 10, we translate these shortfalls into counterfactual capital ratios and plot actual

[78]We consider the case where the firm would have defaulted on a typical fraction of the loan balance, which is 28% in our sample, as well as the more extreme case where the firm would have defaulted on the entire outstanding loan balance.

capital ratios against the various counterfactuals in the post-EBA period.[79] The graph shows that when we incorporate shortfalls from subsidized loan operations, the counterfactual capital ratios quickly approach the regulatory constraints. We find that up to 8 banks out of our sample of 43 banks would have violated their regulatory Core Tier 1 constraint at some point in 2012-2015. On average the violation would have occurred for at least two quarters. Given that the banks that would have incurred a violation hold a significant share of the loan stock in the economy, the reduction in capital adequacy due to unrecognized losses has potentially systemic implications.

7 Conclusion

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

While we exploit a relatively short-lived regulatory intervention to cleanly identify the costs of undercapitalized banks, we provide evidence that the problem persists beyond the regulatory intervention we study. The underreporting of loan losses is pervasive both in the lead-up to the regulatory intervention and in the years after the intervention. This reflects that Portuguese banks faced persistent capital pressures in the aftermath of the sovereign debt crisis. In line with these capital pressures continuing to induce perverse lending incentives, we show that banks are more likely to provide new loans at subsidized interest rates to firms with underreported loan losses when they face higher capital pressures.

Our paper contributes to the important policy debate on whether banks should be forced to recapitalize quickly following a crisis and what tools are best suited to achieve this goal. The results in this paper suggest that European economies, at least in the periphery, may have benefited from a swift recapitalization program such as TARP in the US. Moreover, our results highlight that simply raising capital ratios may heighten perverse lending incentives so long as banks are not forced to raise equity at the same

[79] We choose the intermediate level $j = 2$ and use the extrapolated versions, which incorporate the effect from the household loan portfolio.

time. In addition, we show that the underreporting of losses overstates reported capital adequacy, making it harder to assess the true resilience of the banking sector. Weighing the benefits of a swift recapitalization documented in this paper against potential costs is a fruitful avenue for future research.

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Appendix A: A Method To Detect the Underreporting of Loan Losses

Notation

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

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

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

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

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

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

We first assume that there are no inflows or outflows, with the exception of entry mass $\mathrm{IN}(t;0) = C_j(t;0)$ 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:

$$E(t;b) = C(t;c) - C(t-1;c-1). \tag{13}$$

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 auxillary concept of cumulative excess mass as

$$\bar{E}(t;k) = \sum_{j=1}^{s} E(t-j;k). \tag{14}$$

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 1).

The basic formula adjusted for flows is

$$E(t;b) = [C(t;c) - \mathrm{IN}(t;c)] - [C(t-1;c-1) - \mathrm{OUT}(t;c-1)]\,. \tag{15}$$

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

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

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

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:

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

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:

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

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:

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

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 $t-2$. Outflows at time t does not affect the measure of excess mass:

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

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

1. $0 \leq \hat{\text{OUT}}(t;k-1) \leq \sum_{j=0}^{3} \text{OUT}(t-j;k-1)$
2. $0 \leq \tilde{\text{IN}}(t;k-1) \leq \text{IN}(t-1;k) + \text{IN}(t-2;k)$
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 1: 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 1 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 1, we present results using the maximum and minimum combinations respectively. Figure 3 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. We now present formulas for the six-months buckets. For the first 6-month bucket

1. $\hat{\text{OUT}}(t;k-1) = \sum_{j=0}^{6} \text{OUT}(t-j;k-1)$
2. $\hat{\text{IN}}(t;k-1) = \sum_{j=1}^{5} \text{IN}(t-j;k-1)$
3. $\hat{\text{OUT}}(t;k-2) = \sum_{j=3}^{3} \text{OUT}(t-j;k-2)$
4. $\hat{\text{IN}}(t;k-2) = \sum_{j=4}^{2} \text{IN}(t-j;k-2)$
5. $\hat{\text{OUT}}(t;k) = \sum_{j=1}^{5} \text{OUT}(t-j;k)$
6. $\hat{\text{IN}}(t;k) = \sum_{j=0}^{6} \text{IN}(t-j;k)$

And for the following two six-month buckets:

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

2. $\hat{\text{IN}}(t;k-1) = \sum_{j=1}^{5} \text{IN}(t-j;k-1)$
3. $\hat{\text{OUT}}(t;k) = \sum_{j=1}^{5} \text{OUT}(t-j;k)$
4. $\hat{\text{IN}}(t;k) = \sum_{j=0}^{6} \text{IN}(t-j;k)$

Additional restrictions

We impose the following additional restrictions:

1. We impose that excess mass can never exceed observed mass in bucket.[80]

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

2. We also impose that excess mass must be weakly positive since negative excess mass is just a mismeasured outflow: $B(t;k) \leq 0$.
3. We adjust for the common practice of banks to move overdue loans off their balance sheet in December to boost end-of-year statements, and putting the overdue balance back on in January. This leads to spurious fluctuation in our measure of excess mass.

Examples We now provide stylized examples to illustrate the three mechanisms banks use to adjust the reported time overdue. In Table 2, we replicate the structure of our data. A monthly firm-bank credit panel with the overdue loan balance reported separately for each bucket. The first mechanism consists of simply not updating the reported time overdue (panel a of Table 2). The second mechanism involves banks combining different overdue loan installments. According to the regulatory rules, banks should report (and deduct losses) for new overdue installments at the rate of the longest overdue portion. However, a common practice shown in panel b of Table 2 is to combine a new installment with the existing overdue balance and to report a (lower) averaged time instead. The third mechanism is granting new performing credit in exchange for the repayment of an overdue portion of the loan. In this case, banks treat the 'repaid' portion as the portion that had been overdue the longest. The time overdue reported on the remaining overdue balance can hence stay constant. The bank has not provided any net new liquidity but simply changed labels (see panel b of Table 2). Figure 2 in this appendix shows that most underreporting is driven by the second and third mechanism.[81]

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

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

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

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

.

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

Validity Checks

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

$$\frac{\text{excess mass}_{ibkct}}{\text{overdue loans}_{ibkct}} = \sum_{j=1}^{5} \beta_j \Delta\text{deduction rate}_j + \varphi_b + \theta_i + \mu_t + \epsilon_{ibkct}. \tag{16}$$

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 3 and discussed in section 2.

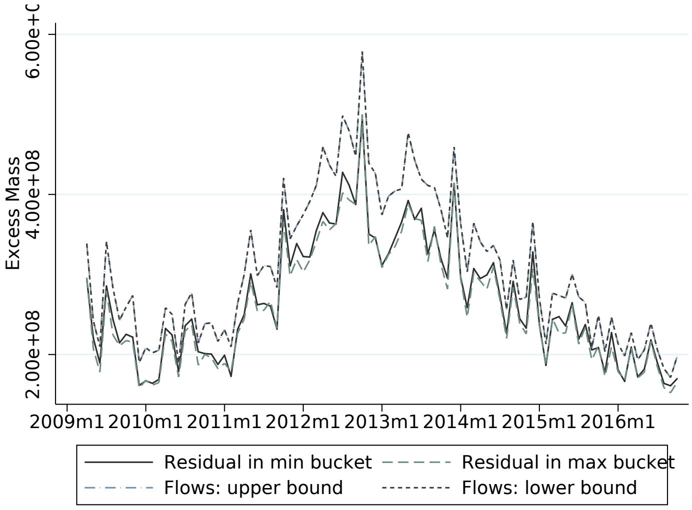
Second validity check Since we can directly trace the time a loan has been overdue in the subset of relationships with only a single loan, we can plot the average amount of underreporting against the actual overdue duration based on the data.[82] We expect underreporting to be most pronounced in the month after the regulatory deduction rate increases as this implies that banks continues to deduct at the lower rate associated with the previous reporting bucket. For example, the regulatory rate increases when switching from reporting that the loan has been overdue 5 months to reporting that is has been overdue 6 months. Hence the incentive to underreport is highest when the actual overdue duration has reached 6 months. By reporting that the 6-month overdue loan continues to have been overdue only 5 months, the bank avoids the jump up in impairment losses associated with reporting 6 months. As in the first exercise we select the loans that have only a single type of collateral since the regulatory schedule differs by collateral type. Figure 4 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 4 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

[82]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.

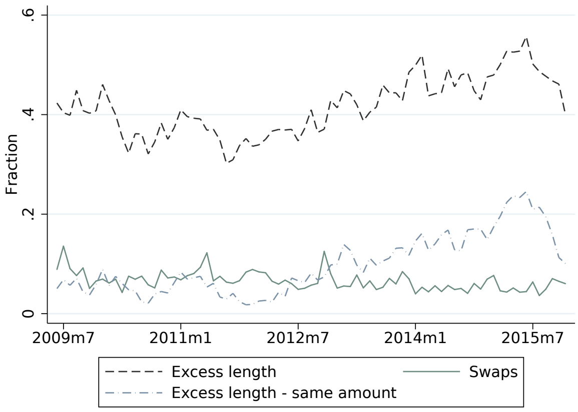
11 percentage point increase in the loan balance that is subject to loss underreporting (relative to the time periods without an increase in the regulatory rate). The effect is non-monotonic with larger increases for the 3-5 months reporting category, which corresponds to $\Delta\text{rate}_{t-1} = 9$ 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.

Figure 1: Robustness of Excess Mass to Assumptions



Notes. The graph shows the aggregate amount of excess mass when varying different assumptions. The first two lines show the results when 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.

Figure 2: 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.

Table 2: Examples of Loss Underreporting

Panel A: Example 1

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

Panel B: Example 2

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

Notes. The table shows stylized examples of the loan data collapsed to the monthly firm-bank level. 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 30 falls overdue (reducing performing credit by EUR 30). According to the rules, the bank should report the total in the category of the longest overdue portion (2 months). Instead the bank reports the total at the averaged time overdue (1 month). The algorithm registers an excess of EUR 50. In March, the bank also grants EUR 50 of new performing credit, which means that the performing balance is EUR 450 - 30 + 50 = 470. In April, the firm uses the new credit to pay back EUR 50 of the overdue balance. The bank treats the repaid portion as the longest overdue and reports the EUR 30 in the same overdue category as in March. The last rows in each example illustrate that the algorithm is "memory-less": As long as reporting is consistent relative to the previous month, the algorithm does not register excess mass.

Table 3: 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

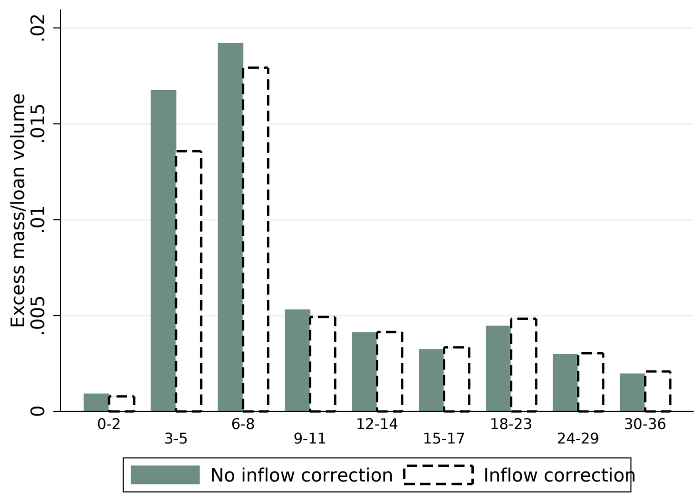
Notes. The table shows regression results for the first validity test of the loss underreporting algorithm. The dependent variable is the amount of excess mass (or underreporting) scaled by the total overdue loan balance in a given reporting bucket for a firm-bank relationship (see table 1 for a visual 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×bank fixed effects. The placebo test regresses underreporting on buckets where there is a rate increase for the *other* collateral type but not for the given collateral type. Standard errors are clustered by firm-bank pair. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level

Table 4: Loss Underreporting:
Bunching in Sample of Single-Loan Relationships

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

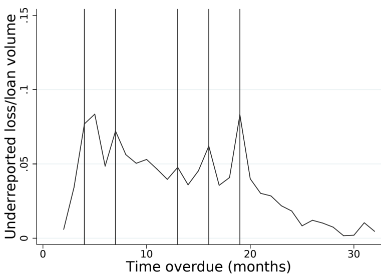
Notes. The table shows regression results for the second validity test of the loss underreporting algorithm. The dependent variable is the amount of excess mass scaled by the total loan balance in a given reporting bucket for a firm-bank relationship (see table 1 for a visual depiction). The explanatory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

Figure 3: Underreported Losses by Reporting Buckets

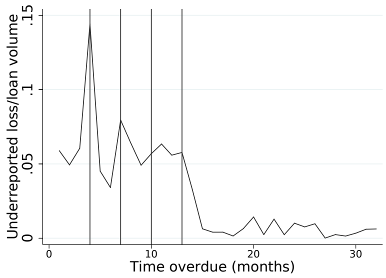


Notes. The graph shows the distribution of excess mass (or underreporting) across reporting buckets. We scale the amount of excess mass by the total loan balance of that firm-bank pair. We compare the results of the algorithm with and without incorporating the effects of flows (repayments, new installments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

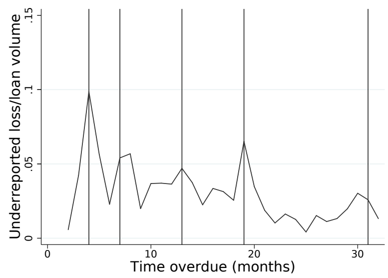
Figure 4: Algorithm Validity Test for Single-loan Relationships



(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.

Appendix B: Additional Results

Table 5: Additional Descriptive Statistics

Firm finance loans		Undereported firms		
	Average		Average	Difference
Loan amount	269,729	Total assets (m)	1.09	0.516***
	($2\text{x}10^6$)		(5.165)	[0.041]
Fraction overdue	0.50	Debt/assets	0.205	0.097***
	(0.42)		(0.41)	[0.004]
Fraction collateralized	0.73	EBIT/sales	0.13	0.009***
	(0.44)		0.16	[0.002]
Fraction w/ guarantee	0.79	Debt/EBITDA	-0.300	-0.771***
	(0.41)		(12.911)	[0.066]
Fraction w/ real collateral	0.32	EBITDA/assets	-0.052	0.011***
	(0.47)		(0.292)	[0.002]
Maturity < 1yr	0.23	Sales growth	-0.030	-0.018***
	(0.42)		(0.709)	[0.003]
Resid maturity < 1yr	0.48	Cash/assets	0.053	-0.019***
	(0.50)		(0.151)	[0.001]
		Debt to government/assets	0.089	0.051***
			(0.135)	[0.002]
		Collateral ratio	0.02	-0.039***
			0.17	[0.002]
N	1,332,435		18,314	

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

Table 6: Treatment Effect for Firms with Correctly Reported Losses

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

Notes. The table shows additional credit regressions results at the firm-bank level for the intensive and extensive margin. The specification is as in equation 2 but we replace the interaction effects with the group of firms that have overdue loans but whose losses are not underreported. Additional interaction effects are omitted. See equations in section 3 for details on full set of interaction effects included. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 7: 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 change are scaled by lagged total credit. For example, the growth rate in performing credit is defined as $\Delta c_{it}^{perf}/c_{i,t-1}^{all}$. Column 6 presents results from a linear probability model where the dependent variable is a dummy that is 1 if the number of loans in a firm-bank pair increases (conditional on an increase in loan volume). The explanatory variable exposed is a dummy that is 1 for banks exposed to the EBA shock. The sample period is 2009q1-2014q4. Pre 1 and 2, EBA, bailout and post-bailout are dummies that identify the following time periods: The EBA shock (2011q4-2012q2), the bailout period (2012q-2012q4), two pre-periods and one post-bailout period all of equal length. Underreported is a firm-bank dummy that identifies relationships subject to loss underreporting in the four quarters prior to the EBA shock. All regressions include bank fixed effects and firm-bank controls (see text for details). Standard errors in parentheses and are two-way clustered by bank and firm. Additional interaction effects are omitted. See equations in section 3 for details on full set of interaction effects included. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 8: Regression Results Firm-Bank Level: Robustness Checks

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

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

Table 9: 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. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 10: Pass-Through Into Employment and Investment: Persistence and Placebo Tests

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

Notes. The table shows regression results at the annual firm-level for different years. The dependent variable is the symmetric growth rate of employment, which is a second order approximation to the log difference growth rate and incorporates observations that turn to 0 (firm exit). We instrument for the log change in credit using the (normalized) firm-level borrowing share from banks exposed to the EBA shock prior to the shock. Relative to table 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. Standard errors in parentheses. *, **, and *** indicate significance at the 0.1, 0.05 and 0.01 level.

Table 11: Descriptive Statistics: New Lending Operations

	Non-subsidized firms	Subsidized firms	Matches	p-value
Interest rate	7.68	5.70	5.57	
	(3.55)	(2.37)	(2.61)	
Amount (m)	0.09	0.05	0.05	0.99
	(2.51)	(0.19)	(0.21)	
Maturity (days)	377.30	313.45	280.36	1
	(876.80)	(720.03)	(596.54)	
Collateralized	0.43	0.37	0.28	1
	(0.49)	(0.48)	(0.45)	
Days rate fixed	114.24	91.85	97.36	1
	(245.87)	(78.26)	(122.91)	
Total loans (m)	0.38	0.28	0.14	
	(4.12)	(1.97)	(0.48)	
Rel length (months)	76.76	79.55	85.24	
	(36.31)	(36.08)	(34.01)	
Existing borrower	0.85	0.98	0.83	
	(0.36)	(0.15)	(0.37)	
Main lender	0.51	0.51	0.54	
	(0.50)	(0.50)	(0.50)	
Subsidy (b.p.)		302.67		
		(153.63)		
N	1,291,224	276,416	60,336	

Notes. The table show descriptive statistics for new lending operations from June 2012 -May 2015. The first column shows all operations that are neither subsidized nor used as a matched control. The second column shows lending operations that we identify as subsidized according to the matching procedure described in text. The third column shows lending operations to investment-grade firms that serve as matches. The last column shows p-values on one sided t-tests between subsidized and matched controls in the direction that we would expect to lower interest rates (lower amount, lower maturity, not collteralized, fewer days fixed rate). Total loans refers to the total amount of new loans in a given firm-bank-month triple, while amount refers to the size of an individual loan. Subsidy (measured in basis points) is the average difference in interest rates on subsidized loans and loans to firms in the worst rating category according to a credit rating model for non-financial firms by Antunes et al. (2016).

Table 12: Pass-Through Into Employment and Investment

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

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

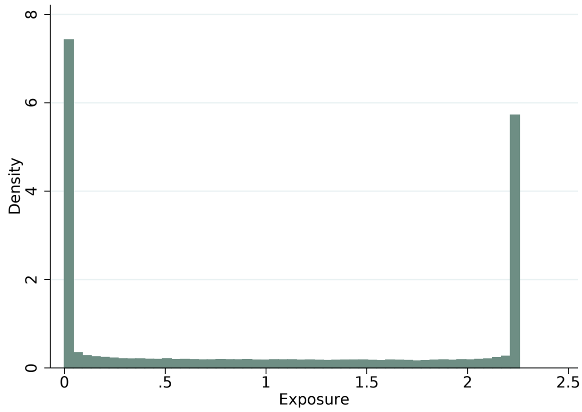
Notes. The table shows IV regression results at the annual firm-level in 2012. All the dependent variables are in log differences. All variables are deflated according to procedure described in online appendix. In panel b, dependent variables are firm-level gaps between output elasticities and revenue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

Table 13: Robustness: Spillovers

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

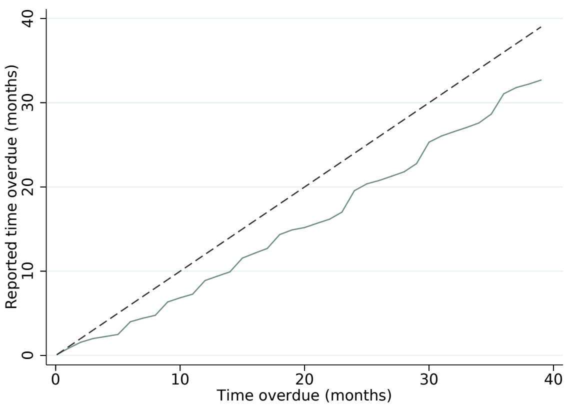
Notes. The table shows IV regression results at the firm-level for 2012. Share underreported refers to the asset-weighted share of distressed, underreported firms. We vary the definition of industry. The last column shows results for creating combined district-1-digit industry categories. We instrument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

Figure 5: Histogram of Firm-Level Exposure to EBA Shock

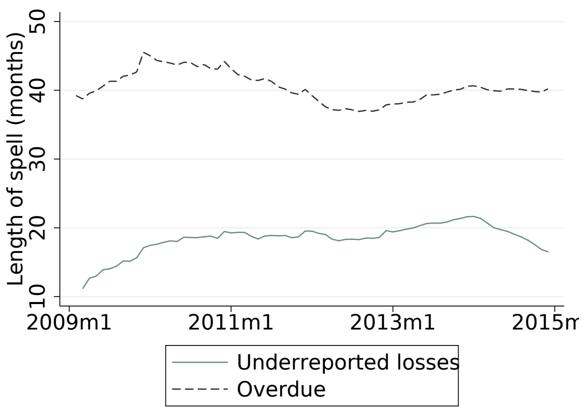


Notes. The graph shows a histogram of the continuous exposure measure for firms with underreported losses. The exposure measure is the share of credit coming from banks exposed to the EBA shock in 2010. We standardize the measure to have unit variance. The peaks at 0 and 2.25 represent firms borrowing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

Figure 6: Additional Results on Underreporting of Loan Losses



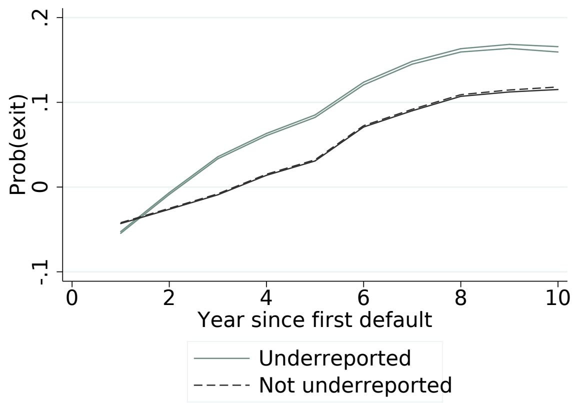
(a) Evidence of Reporting Management from Single-Loan Relationships



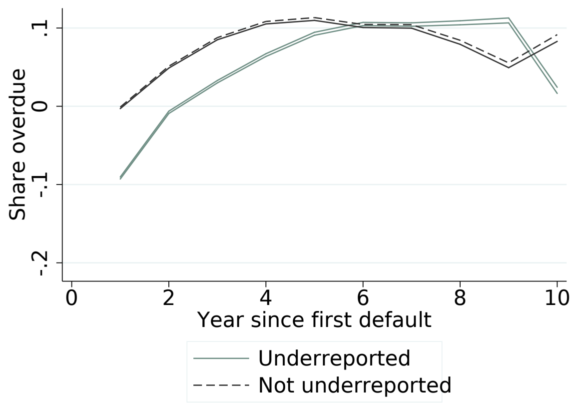
(b) Persistence of Unrecognized Losses

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

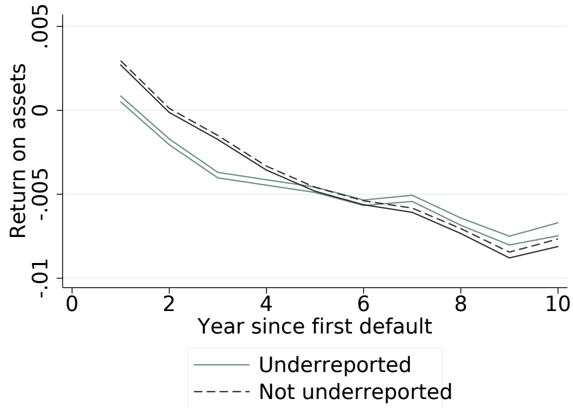
Figure 7: Long-run Trends: Underreported vs Non-underreported Firms



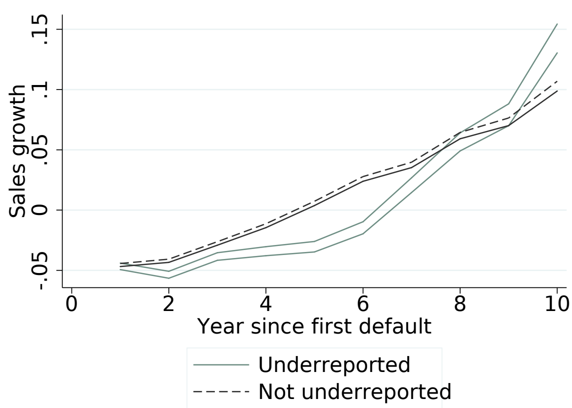
(a) Probability of exit



(b) Share of loans overdue



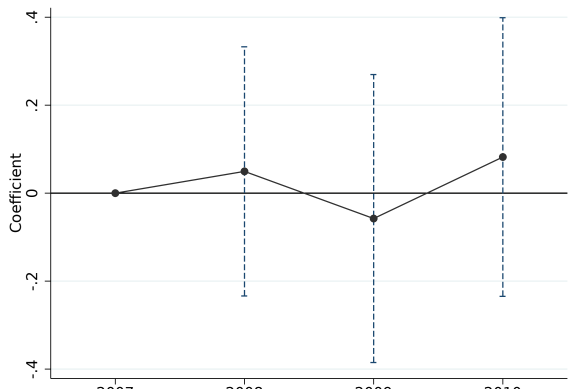
(c) Return on assets



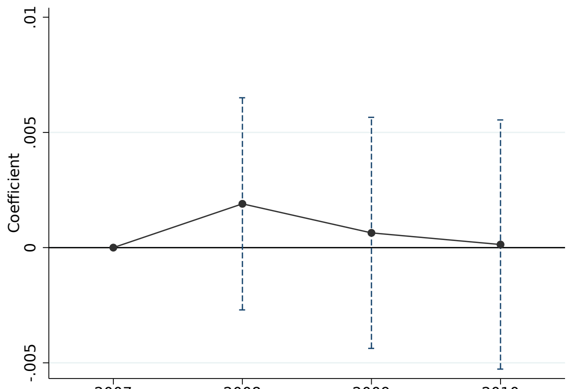
(d) Sales growth

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 8: Liquidity and Credit Pre-trends



(a) Current Ratio

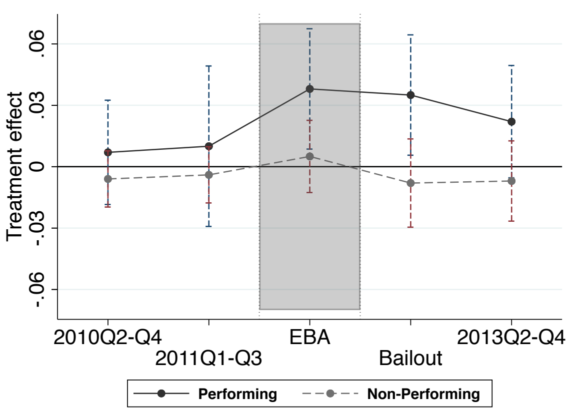


(b) Cash/Assets

(c) Firm×time fixed effects

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

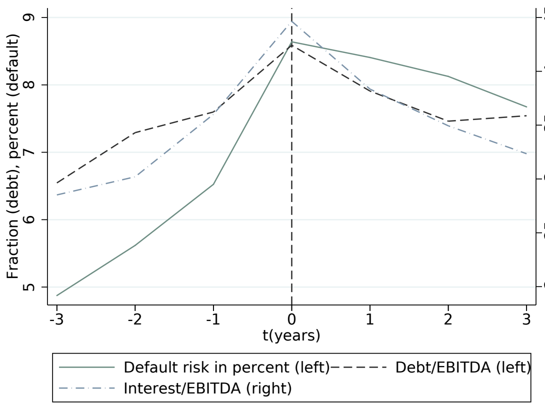
Figure 9: Additional Results: Firm-level Regression



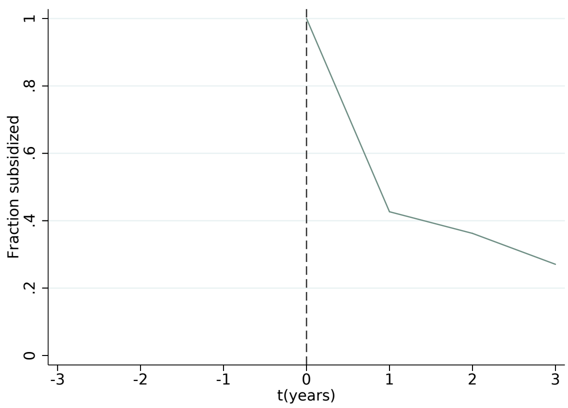
(a) Decomposition of Credit

Notes. The graphs show regression results at the quarterly firm-level. The dependent variables are the quarterly log of performing and non-performing credit, respectively. We plot the coefficients on the interaction treatment$_i \times$quarter$_t \times$underreported$_i$, which are the treatment effects for the group of firms subject to loss underreporting. The vertical lines denote the EBA announcement and compliance deadline. The specification, equation 3, includes the full set of interactions, industry$\times$quarter and firm fixed effects, as well as firm-level controls interacted with quarter. All coefficients should be interpreted as changes in the dependent variable relative to the (normalized) base quarter 2011Q3. Standard errors are clustered at the firm-level. N= 1,346,771.

Figure 10: First-Time Loan Subsidies



(a) Evolution of Firm-Characteristics Around First Subsidy



(b) Persistence of Loan Subsidies

Notes. Panel a shows the average evolution of firm-level financial health indicators in the years around the year a firm first receives a subsidized loan ($t = 0$). Panel b shows the fraction of subsidized new loans a firm receives following the year a firm first receives a subsidized loan ($t = 0$).

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:

$$q_{it} = \beta_l l_{it} + \beta_k k_{it} + \beta_m m_{it} + \beta_s s_{it} + \epsilon_{it}. \tag{17}$$

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

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

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

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

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

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

$$\log A_{it} = q_{it} - \left(\hat{\beta}_l l_{it} + \hat{\beta}_k k_{it} + \hat{\beta}_m m_{it} + \hat{\beta}_s s_{it}\right) \tag{18}$$

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

$$q_{it} = \sum_j \beta_j X_{it}^j + \beta_{jj} X_{it}^{j^2} + \sum j \neq k \sum_k \beta_{jk} X_{it}^j X_{it}^k + \epsilon_{it}. \tag{19}$$

In table 14 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 14: 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).