# Banks' Credit Losses and Lending Dynamics*

Peter Raupach† and Christoph Memmel‡
Deutsche Bundesbank

January 15, 2023

**Abstract**

We investigate how a bank adjusts its lending when it has suffered one of the heaviest credit losses in its history. Showing that such a shock is mostly exogenous, we estimate from granular data of all German banks that the heaviest losses induce banks to reduce their corporate lending by 1.79 euro for each euro lost. This sensitivity is in line with (quite heterogeneous) results of earlier studies but significantly lower than the sensitivity under a constant-leverage regime. To control for credit demand we construct a synthetic competitor of each bank. This new method reconciles estimation at bank level with demand clustering at the level of subportfolios.

**Keywords:** Credit losses, Bank lending

**JEL classification:** G 21

*The authors thank Christian Bittner, Ben Craig, Olivier De Jonghe, Co-Pierre Georg, Rainer Haselmann, Björn Imbierowicz, Jean-Stéphane Mésonnier, Frieder Mokinski, Steven Ongena, Esteban Prieto, Edgar Vogel and the participants of the Bundesbank's Seminar (Frankfurt, 2019) and of the 3rd Annual Workshop of ESCB Research Cluster3 (Madrid, 2019) for their helpful comments. The views expressed in this paper are those of the authors and do not necessarily reflect the views of the Deutsche Bundesbank or the Eurosystem. Source file of this version: Lending_20230115.lyx

†Email: peter.raupach@bundesbank.de; tel: +49 (0)69 9566 8536; Deutsche Bundesbank, Wilhelm-Epstein-Strasse 14, 60431 Frankfurt.

‡Email: christoph.memmel@bundesbank.de; tel: +49 (0)69 9566 4421; Deutsche Bundesbank, Wilhelm-Epstein-Strasse 14, 60431 Frankfurt.

# 1 Introduction

It is important to know which factors might impair corporate lending as this function of the banking sector is critical to the economy. Capital regulation is probably the factor that is most intensively discussed by the banking industry, policy makers, and academics. Positions range from the viewpoint that capital ratios of 30% and more would have hardly any detrimental effects (Admati, DeMarzo, Hellwig, and Pfleiderer, 2010) to claims put forward by industry representatives that every additional euro of required capital forces banks to cut their lending by a multiple, for example by 10 euros for every euro capital if their target capital ratio is 10%.

While the empirical literature agrees that shocks to capital do have an effect on lending at least in the short run, there is much less agreement on the size of the effect; see the next session for an overview. But size matters, especially for macroprudential stress tests, because the lending reduction after a credit shock is key to feedback effects between banks and the real economy.

Our empirical contribution to this literature, initiated by efforts to calibrate such stress tests to the German credit sector, is to estimate the lending effect for all German banks. We find a moderate sensitivity of corporate lending – far below the scenarios suggested by the industry – and a mixed pattern of effects on other loan sectors and security positions.

We furthermore make two methodological contributions. First, we exploit a simple new type of shock where we differentiate between the largest credit losses in a bank's individual history and other, smaller losses. While a dummy for these big losses is not completely free from a bank's risk-taking ex ante, it turns out to be virtually unpredictable for a bank and passes a large set of endogeneity tests as well. The shock does not seem to bear essential disadvantages compared to the "earthquake-type" shock often sought after, and has the advantage that it is evenly scattered in the long- and cross-section and thus captures a greater variety of economic conditions.

The second methodological contribution is a new approach to the control for demand. We construct for each bank a tailored synthetic competitor whose new lending is included as a control variable. In this way we can combine estimations at bank level, which both diminishes noise and avoids an over-representation of highly diversified banks, with controlling for demand at a more granular level. The approach differs from the "synthetic control" method known from the literature.

The relationship between a shock to a bank and its lending to the real economy is difficult to establish, mainly for three reasons: (i) the endogeneity of bank capital, (ii) the problems of disentangling credit supply and demand, and (iii) the presence of other institutions that might step in for the affected bank.

To overcome the endogeneity problem of bank capital, we select events that directly affect capital but are hard to predict. The basic "treatment" concept is the realization of an exceptional credit loss caused by a very small number of borrowers. As our data source is broken down into 23 industry subportfolios (and not further), we use losses in a single industry. To improve the focus on relevant events, we first pre-select the largest of the 23 industry-specific losses reported for each bank and quarter.

Then, we consider each bank's individual history of such pre-selected losses in up to 72 quarters, from which the largest ten percent are defined to be *big losses*; a corresponding dummy is the treatment. Note that we use *big loss* as a technical term. It does not directly denote a loss but the event that a loss belongs to the upper decile of a certain sample. Selecting losses from a bank-individual sample is an essential feature of our approach – it precludes the obvious endogeneity concern that banks make strategic risk taking decisions. Given our definition, the long-term level of credit risk has a priori no impact on the frequency of big losses.

Our definition of a big loss creates exogenous events for the most part. Intuitively, a bank manager does not expect a credit loss of, say, 30% of its loan exposure in an industry to occur *in the next quarter*. If she did, she would do her utmost to avoid that loss. Even though bank managers are clearly aware that they will make a large loss sooner or later, they would fail to predict whether the next quarter's loss would be among the 10% worst losses in the bank's history during the observation period. That is, it is not the possibility of a big loss that is key to our identification strategy, but its timing.

In brief, the main reasons for exogeneity are (i) the mostly idiosyncratic nature of firm defaults, (ii) the low degree of diversification in the industry subportfolios, (iii) the treatment's invariance to the long-term level of credit risk, and (iv) its very low sensitivity to temporary shifts in credit risk. Below we detail these reasons together with the tests performed.

Further design features of a big loss strengthen its surprising nature. First, we take the shortest loss horizon possible, a quarter. Second, we regress on a dummy for big losses rather than on the loss extent, which dampens the potential influence that bank managers may have on the loss size. Third and, to our knowledge, new to the banking literature, the pre-selection of losses from 23 industries boosts the size of losses in the sample, which results in noticeable events when it comes to the worst ten percent. These losses are three times as large as the average quarterly profit and differ greatly from the other losses in the sample, due to a fat distribution tail[1].

To motivate our shock design further, Figure 1 provides a first impression of how losses affect lending and shows why we focus on the worst 10% of losses. The starting point is an unbalanced bank/quarter panel in which each data point represents the worst loss made in a single industry. To create the graph, we keep every bank fixed, sort its history of up to 72 such losses by size, and assign each loss its percentile rank between 0 and 100. This rank defines the x-axis, and a rank above 90 indicates a *big loss*. Values on the y-axis are based on the bank's new domestic corporate lending in the four quarters following a loss, excluding the industry from which the loss originated, normalized by total assets. This gives a pair of a loss percentile rank and new lending for each bank and quarter. To maintain data confidentiality, we do not show a scatterplot but a moving average using kernel smoothing.

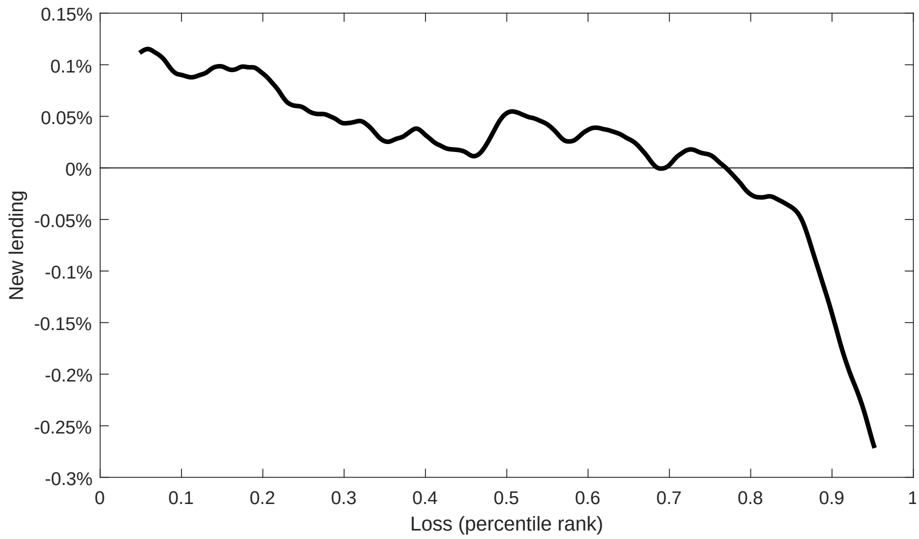
The key message is that the ten percent biggest losses that lie to the right of the 90 are special. They seem to trigger a significant drop in new lending compared with the smaller losses on the left.

Our estimates with new lending as the dependent variable and the *big-loss* dummy as the key regressor confirm the visual impression. Expressed as a linear effect, each euro lost in a big loss lets the bank reduce its lending by slightly less than 2 euros. This effect is moderate compared to values found in the literature (Section 2) and much weaker than the effect resulting from the assumption of constant leverage.

Turning to the problem of disentangling credit supply and demand, we share with many authors (see Section 2) the problem that data are limited to loans granted and lack more direct demand signals such as loan applications. In a broader sense, we compensate for this lack in the usual way by assuming that loan demand is homogeneous across a group of similar borrowers. Demand can then be filtered out by comparing the lending of different banks to the same group. Since the homogeneity assumption is weaker the smaller the group, the current state of the art (Khwaja and Mian, 2008) recommends choosing the most granular data – on individual borrowers, if available – and estimating the effect of interest at this most granular observation

[1] See Section 4.2.

Figure 1: New lending vs credit losses




For each bank and quarter, we select the largest loss in a single industry, normalized by total bank assets. This gives up to 72 quarterly losses by bank. Keeping a bank fixed, we sort its losses by size and assign each loss its percentile rank (between 0 and 100). This rank defines the x-axis. Values on the y-axis are based on new domestic corporate lending in the four quarters following a loss, excluding the industry from which the loss originated, normalized by total assets. Plotted values are moving averages, using a kernel smoother that includes 6,000 observations. A scatterplot of all observations (pairs of loss percentile rank and new lending) would conflict with data confidentiality.

level, with a demand-absorbing fixed effect included for each borrowing observation.

We also control for demand at the maximum granularity level possible with our data, which is the lending to 9,223 *local industries*, each of them being a selection from 23 industries and 401 counties. If we were to apply the standard method to this data, we would choose bank/industry/county/time specific observations of new lending as the dependent variable and include industry/county/time fixed effects to absorb credit demand.

However, this disaggregate estimation is inconsistent with the idea that a corporate loan portfolio is managed as a whole. If we want to learn about portfolio adjustments, it matters whether we make estimates at the level of portfolios or small parts of them, given that a few banks are well diversified across industries and regions whereas most of them lend only to a few industries in their region. Taking bank/industry/county/time observations, as in the standard setup, only 1% of the banks (23 in number) would make up 25% of these granular observations and hence be regarded as if these few banks had made 25% of the decisions on portfolio adjustments. We avoid such an over-representation of banks with particularly diversified portfolios by estimates at bank level; nevertheless we control for demand at the granular level of local industries.

To this end, we take a new approach and construct a bespoke competitor for each bank. This *benchmark bank* has the same portfolio weight in each local industry as the bank under consideration. The main estimation is performed at bank level and includes the new lending of the benchmark bank as a control variable. Estimating at bank level also has the positive side effect that we can integrate the intensive and extensive margin of lending and avoid the notorious noise associated with relative changes.[2] The benchmark bank also facilitates a direct estimate of whether competitors jump in for banks who reduce their lending after a large loss.

[2]The dependent variable in the standard approach is a relative change in lending; very small initial positions can turn into huge values even if the exposure is completely insignificant.

As demand is driven by both local and non-local factors, it is beneficial to include the lending of two benchmark banks as controls, one that the replicates portfolio weights of local industries at the county level (for local factors) and one that replicates industry weights but ignores the location of borrowers (for nationwide factors).

Our method might be reminiscent of the "synthetic control" method[3] but actually differs in purpose and design. A synthetic control is a fictitious observation constructed from untreated observations that aims to match selected characteristics between treated and untreated observations. Our method does not create or modify observations, but defines a control variable that allows us to reconcile two otherwise incompatible features: bank-level estimation and a granular-level control for demand.

We combine two detailed datasets on the lending of all German banks, Bundesbank's borrower statistics and the credit register. Either of them has its pros and cons. Using the register we could even control for demand at borrower level but abstain from this option since it would imply a strong selection bias towards firms that are just not representative for the average bank in our sample.

By contrast, the borrower statistic is consistent with the balance sheet. It splits industry exposures further up into maturity brackets which provide us with a powerful control for loans that are about to expire. Losses in the borrower statistic are basically limited to write-downs and hence to material credit events, which is exactly what we want our shocks to be. Because of this feature and the maturity information we prefer the borrower statistic as our main data source. Since its industry breakdown would be too coarse for a proper control for demand, we match it with firm locations from the credit register to obtain an approximate regional exposure distribution over counties for each industry subportfolio. Degryse, De Jonghe, Jakovljević, Mulier, and Schepens (2019) show that such a breakdown into local industries is granular enough to absorb most of the variation that could be absorbed by borrower/time FEs.[4]

We complement our baseline result by a number of further observations. First, a weak capital basis (defined as the bottom decile of capital ratios) leads to a similar lending reduction as a big credit loss. However, weak capital does not seem to reinforce the lending effect of a big loss.

Second, we test whether a big loss triggers loan extensions by the benchmark bank, which may give a hint as to whether competitors would step in for the bank and thereby dampen the impact of the primary lending cut on firms. We find no such dampening effect.

Third, also retail loans are reduced after big losses. Surprisingly, we find no effect on securities holdings, despite their better liquidity, which points to an isolated management of credit losses within the banking book. Evidence is mixed for the effect of low capital on securities, which does not seem to trigger sales of stocks or bank bonds but surprisingly does so for government bonds.

We test the shock concept extensively, starting with modified definitions of a big losses. In a first test, the largest 10% of losses are selected from quarterly (rather than bank-individual) samples, which eliminates long-sectional variation in the frequency of big losses (but creates differences between banks).

In a second test, we combine bank- and time-dependent thresholds to a selection mechanism for big losses that stabilizes their frequency in the long- and cross-section, which makes the treatment invariant both to a bank's static risk choice and to a temporary nationwide shift in credit risk. The latter neutralizes the impact of a systematic credit risk factor common to all German corporates. Independently of this test we know from earlier research on the

[3] Abadie, Diamond, and Hainmueller (2010); Dasgupta and Mason (2020)

[4] Degryse et al. (2019) use data from Belgium, which is characterized by SMEs the same as Germany. See column 3 of their Table 2 where the industry-location-time fixed effects correspond to our key assumption.

same data that systematic factors play only a little role in the variation of losses at bank level (Memmel, Gündüz, and Raupach, 2015). Single-industry losses as in the present paper are even less systematic and, based on indirect evidence, typically caused by a single borrower.

In further tests, we orthogonalize losses by subtraction of systematic factors, correct for autocorrelation, and vary the severity of losses by variation of the tail probability. None of these and further tests and several combinations thereof put the results in question.

Returning to the baseline specification with its invariance to static choices of credit portfolio risk, there remains an endogeneity problem to discuss: the possibility that a bank chooses credit risk dynamically and that this temporary choice has an impact on the occurrence of big losses. If that impact is large enough, we might misinterpret a possible link between a temporary risk choice and subsequent lending as driven by the big loss. We claim that this impact may exist but is not material. Thus, the aforementioned possible link between risk and lending is virtually unrelated to our shock and merely noise in the estimate. Two arguments support our claim:

First, we contrast the big-loss sample with a matching control sample based on a propensity score for treatment. The idea is that the score predictors could also be correlated with a dynamic risk choice. For example, a temporary reduction in lending standards may stimulate additional lending to marginal borrowers with high default risk, which later turns into frequent and high losses. If this relationship is strong enough, earlier changes in lending can potentially predict large losses. As the matching procedure eliminates the impact of the score predictors on the treatment, it consequently eliminates the impact of the risk choice to the extent of its co-movement with the predictors. Estimates with the matching sample lead to similar results as the baseline estimate.

Second, there are limits to the speed at which credit portfolio risk can be changed by bank managers. The main option for this is a change in lending standards which, however, affects loan quality only at grant. Such a change remains limited to the loan turnover, which is only 14% per year in our sample. That is, it takes a few years of decidedly low standards to have an effect on losses. However, the longer such a high-risk period lasts, the more the effect on big losses is neutralized by their constant fraction of 10% in the observation period.

As we cannot observe the risk choices of bank managers, we use Monte-Carlo simulations to document that credit risk cannot pile up fast enough to have a sufficient impact on the occurrence of big losses. In the simulations, a realistic revolving loan portfolio is subject to a data-consistent exogenous random turnover of loans. In the course of time, lending standards randomly switch between two very different regimes. Every maturing loan is replaced by a loan with an annual default probability of either 1% or 10%, depending on the current regime, such that the portfolio either drifts towards reasonably safe assets or junk. The dynamic portfolio composition feeds into a realistic credit portfolio model that generates single-industry losses and big-loss events just as in the empirical setup.

The simulated data generating process can be used to estimate how well a bank manager could forecast a future big loss based on her knowledge of current and past lending standards. This forecast exercise basically fails since various regressions never reach an $R^2$ in excess of 1.5% even under conditions favorable for predictive power. If it is virtually impossible to predict big losses from lending standards, then a potential link between the latter and subsequent new lending would not bias a regression of new lending on big losses.

Altogether, a large number of tests confirm the main result. The simple treatment concept used in this paper – contrasting exceptional losses in a narrow sector with the other, "normal" losses – combines sufficient exogeneity with dispersion across banks and time. As every bank is equally hit by shocks regardless of its risk profile, while bank managers fail to predict when that will happen, the concept is reminiscent of the Latin motto *mors certa, hora incerta*[5]

The paper is structured as follows. In Section 2, we give a brief overview of the literature. Section 3 describes the empirical model, and the data used is explained in Section 4. In Section 5, we present the empirical baseline results, extensions, and the outcome of robustness tests. Section 6 summarizes and concludes.

## 2 Literature

The question of bank capital and lending has often been investigated; see, for instance, Kim and Sohn (2017) for an overview. There is much empirical evidence that banks experiencing binding capital constraints reduce their lending (see, for instance, Acharya, Eisert, Eufinger, and Hirsch (2018), Gropp, Mosk, Ongena, and Wix (2018), Tölö and Miettinen (2018), and Popov and Van Horen (2014)). The relationship is often found to be non-linear and influenced by bank characteristics: According to Brei, Gambacorta, and von Peter (2013) and Carlson, Shan, and Warusawitharana (2013), a bank's capital endowment is crucial for the strength of the relationship between capital and lending; Kim and Sohn (2017) and Ivashina and Scharfstein (2010) stress the impact of banks' liquidity.

Many researchers study cross-border lending, for instance Peek and Rosengren (1997), Aiyar, Calomiris, Hooley, Korniyenko, and Wieladek (2014), and De Haas and van Horen (2013). Apart from documenting the international spillover of financial shocks, this approach helps to separate credit supply and demand. We also look at spillovers; however, across industries rather than countries.

While an effect between capital losses and lending is generally evident, the size of the effect is less clear. But its size matters, especially in the context of stress tests, as the lending reduction after a credit shock is a central link between the financial sector and the real economy, and hence key to the modeling of feedback effects between them. Table 1 documents that estimates of the lending reduction caused by a capital gap (measured in euro reduced per euro of the gap) varies a lot across empirical studies. These estimates provide the context for our results.

The capital cushion of a bank, that is the capital in excess of the regulatory minimum, is exposed to different kinds of shocks, which correspond to different measures used by researchers to quantify these shocks. Typical measures are: (i) changes in a bank's capital ratio, (ii) the deviation of the capital ratio from a target level, (iii) changes in a bank's capital requirements, and (iv) losses that have an impact on bank capital.

All four measures are used in the literature: while Hancock and Wilcox (1994) make use of changes in the capital ratio, Berrospide and Edge (2010) look at the deviation of the actual capital ratio from an estimated target ratio. Changes in capital requirements (or their announcement) have the methodological advantage that they can be considered as exogenous (see, for instance, Gropp et al. (2018)); in addition, these studies are not affected by the problem of a possible substitution of credit supply (as all banks are similarly concerned by changes in capital requirements). However, there is little variation in the cross-section of banks, with a few exceptions such as Aiyar et al. (2014), Aiyar, Calomiris, and Wieladek (2016), Imbierowicz, Kragh, and Rangvid (2018), and De Jonghe, Dewachter, and Ongena (2020). These authors make use of the time variation in minimum capital requirements in the UK, Denmark, and Belgium where bank supervisors actively exert their discretion to prescribe bank individual capital surcharges. Furthermore, there is often a wedge between announcements of regulatory reforms (or details thereof) and their implementation.

[5]Death is certain, its hour is not.

Table 1: Effect of a capital gap of 1 euro on lending

| Study / Assumption | Reduction | By banks experiencing ... | Sample |
|---|---|---|---|
| Constant leverage | 10.00 euro | — | — |
| Aiyar et al. (2014) | 5.50 euro | Capital shocks | Foreign subsidiaries of UK banks, 1999–2006 |
| Hancock and Wilcox (1994) | 4.63 euro | Low capital ratio | US banks, 1991 |
| Berrospide and Edge (2010) | 1.86 euro | — | US banks, 1992–2008 |
| Hancock and Wilcox (1993) | 1.37 euro | Large loan losses | US banks, 1990 |
| Gambacorta and Shin (2018) | 0.36 euro | — | Int. banks, 1995–2012 |

This table shows the reduction in a bank's lending ("Lending red."; horizon: one year) as a consequence of a capital gap of 1 euro. "Constant leverage": a target capital ratio of 10% is assumed. Concerning the study Gambacorta and Shin (2018): own calculations under the assumption of a loan-to-asset ratio of 60%.

There are further measures used in the literature since shocks affecting credit supply may not only result from changes in capital but also from funding shocks in general such as the collapse of interbank funding after the Lehman crash (De Jonghe, Dewachter, Mulier, Ongena, and Schepens, 2020).

As we deal with losses in the credit portfolio rather than capital gaps, we provide only indirect evidence for a reader who is primarily interested in the role of capital. How indirect it is depends on the attitude towards the assumption that a one-euro credit loss reduces bank capital by one euro and that the bank's capital ratio has been at its target level prior to the credit event.

Other authors focus on the separation of credit demand and supply. One approach compares the loan granting of banks affected by a shock with the outcome of non-affected banks (Peek and Rosengren, 1997), which is also our approach. Another approach is the separate observation of loan demand (for instance by loan applications) and realized loans (Jiménez, Ongena, Peydró, and Saurina, 2012; Puri, Rocholl, and Steffen, 2011; Jiménez, Ongena, and Peydró, 2014). This approach is highly preferable but mostly lacks the data necessary, as in our case.

Altogether, there is substantiated empirical evidence that a gap in a bank's capital endowment, a significant loss, or a capital ratio below the target lead to a reduction in new lending. However, the estimates largely disagree on the size of this effect, ranging from a reduction of less than half a euro to ten euro for each euro of capital lost.

## 3 Empirical modeling

Our data allows us to identify a credit loss incurred by a bank in a single industry. As explained, we consider belonging to the biggest of such credit losses to be exogenous. We estimate by how much a bank that has suffered such a big loss in an industry expands or contracts its credit exposure to the *other* industries afterwards.

We exclude the industry of this loss from the measurement of new lending for three reasons. First, a big loss is likely to be followed by large further write-downs (but also write-ups) in the same industry, for instance as a result of an intensified scrutiny of problem loans, the revaluation of collateral, or shocks to the liquidation value. We are hesitant to interpret the resulting exposure changes as actual lending decisions. Second, banks may wish to keep the industry composition of their credit portfolio constant such that they would seek to replace a lost exposure by loans to the same industry. And third, the split between the problematic industry and the rest of the portfolio tempers the effect of systematic credit risk factors since inter-sector spillover effects are typically lower than intra-sector effects (Chernih, Henrard, and Vanduffel, 2010). The impact of a systematic component common to different industries is nevertheless subject to a robustness test in Section 5.3.

Throughout this paper, $t$ stands for a quarter (2002Q4–2020Q4), index $i$ for a bank (1,774 in raw data), and $j$ for an industry (23). The data is actually further broken down into three maturity brackets that we only use in the calculation of a control variable, the amount of maturing (or expiring) loans.[6]

Our data contains the loan exposure $ex_{t,i,j}$ of bank $i$ to industry $j$ (in euros) and the corresponding value change $c_{t,i,j}$ (euros), which is the change in the valuation of the exposure between $t-1$ and $t$, based on the position in $t-1$. A negative value of $c_{t,i,j}$ thus means a loss.

In order to focus on relevant events, we select for each bank and quarter the largest loss in a single industry, normalized by total assets:

$$L_{t,i} \equiv -\frac{1}{TA_{t,i}} \min_j \left(c_{t,i,j}\right) \quad \text{if } \min_j \left(c_{t,i,j}\right) < 0. \tag{1}$$

The relevant "worst" industry is denoted as:

$$\text{wst}\left(t,i\right) \equiv \text{argmin}_j \left(c_{t,i,j}\right) \quad \text{if } \min_j \left(c_{t,i,j}\right) < 0. \tag{2}$$

Observations wit non-negative minima are excluded since they mostly originate from multiple zeros and hence from multiple candidates for $\text{wst}\left(t,i\right)$. This ambiguity makes it difficult to define "lending to the other industries $j \neq \text{wst}\left(t,i\right)$" in a sensible way, and attempts to do so appear less clean than deleting these observations.

We estimate the relationship between the biggest losses and subsequent new lending. The latter is simply the one-year change in corporate loan exposures except the worst industry, normalized by total assets $TA_{t,i}$:

$$n_{t,i} \equiv \frac{1}{TA_{t,i}} \sum_{j \neq \text{wst}(t,i)} \left(ex_{t+4,i,j} - ex_{t,i,j}\right). \tag{3}$$

A simple exposure difference includes changes in the valuation of loans, which we prefer to include since the result is the micro-counterpart to the ultimate growth of corporate loans in the whole economy. Value changes could also be subtracted from the exposure difference, which puts more focus on the mere action of bank management; this variant is subject to a robustness test in Section 5.3.

### 3.1 Controlling for demand

Our analysis crucially depends on a proper control for credit demand and systematic credit risk factors. We follow the literature, in particular Peek and Rosengren (1997), insofar as we suppose homogeneous credit demand in each of the most granular data segments available (in our case, firms from the same *local industry* that is based in the same county) and contrast a bank's new lending to every such segment with the lending of other banks to the same segment.

For reasons outlined in the introduction we prefer an estimation at bank level, which needs to be reconciled with the heterogeneity of demand in every observation. We therefore contrast each bank, which is hit by different demand shocks, with a constructed bespoke competitor that is equally hit by the same shocks, provided our key assumption holds that credit demand is

[6] See Section 3.2 and Appendix C.

homogeneous in each of the $23 \times 401$ local industries of our data. To this end, this competitor must replicate the bank's distribution of credit exposures over the local industries, which we achieve by scaling the total exposure of all other banks to each local industry up or down. This rescaling keeps the percentage change in lending to each local industry constant, that is, if the other banks' aggregate lending to a local industry has doubled, the constructed competitor doubles it as well. Yet it doubles another, rescaled exposure whose weight in the portfolio fits with that of the bank under consideration.

We call this competitor *benchmark bank* and include its new lending in the estimate as a control variable. We thus control for demand at the level of 9,223 local industries. As, however, borrowers are not bound to their county when asking for credit, we do not rely on counties exclusively but vary the size of regions, from 401 counties via 38 districts and 16 states to the maximum aggregate of the whole country.

To understand the concept, it is sufficient to start with $ex_{t,i,j,r}$, the exposure of bank $i$ to industry $j$ in region $r$ (a county, a district, state, or the single country) at time $t$. How we construct this figure is described in Section 4.4 and Appendix A. For every bank $i$, we then calculate the exposure weight of each industry/region segment $(j, r)$ relative to total assets:

$$w_{t,i,j,r} \equiv \frac{ex_{t,i,j,r}}{TA_{t,i}}. \tag{4}$$

Similarly, the aggregate exposure weight of all other banks is given by:

$$w_{t,\neg i,j,r} \quad \equiv \quad \frac{\sum_{k \neq i} ex_{t,k,j,r}}{\sum_{k \neq i} TA_{t,k}}, \tag{5}$$

where the subscript $\neg i$ symbolizes "all except $i$". The benchmark bank is constructed by scaling each of these weights up or down to fit with its counterpart from bank $i$, which obviously requires the scaling factor to be:

$$\nu_{t,i,j,r} \quad \equiv \quad \frac{w_{t,i,j,r}}{w_{t,\neg i,j,r}}. \tag{6}$$

*Remark.* This rescaling factor has a surprising mathematical interpretation. It performs a *measure transform* of the exposure distribution $(w_{t,\neg i,j,r})_{j,r}$ to $(w_{t,i,j,r})_{j,r}$. In other words, the rescaling factor is the discrete density (or Radon-Nikodym derivative) of the latter relative to the former.[7]

Rescaling by $\nu_{t,i,j,r}$ cannot work perfectly if the denominator in (6) is zero, which happens if no competitor is found for bank $i$ in this industry/region segment at that time. Luckily, the problem only applies to 1.5% of all segments with positive numerators, which we consider tolerable for the purpose of controlling for demand. We simply leave the weights in the benchmark portfolio as zero where the denominator is zero and correct for the lost exposure by a proportional shift of all factors, which is equivalent to assigning average values to missing segments. The ultimate error in the portfolio composition is actually much lower than 1.5%, on average.[8]

We construct the hypothetical competitor only from banks. Ignoring the bond market and other financial intermediaries, such as insurance companies, as a funding alternative is a poten-

[7] Shiryaev (1995) gives an excellent introduction into measure transforms on discrete probability spaces.

[8] Each portfolio in our main estimate covers 22 of 23 industries and 401 regions. Of these $22 \times 401$ segments, only 1.5% cannot be matched properly. To measure the deviation, we choose (for a single quarter) all segments with a positive original weight $w_{t,i,k,l}$ and define bank-specific samples of the deviations $\nu^*_{t,i,j,r} w_{t,[-i],k,l} - w_{t,i,k,l}$, of which we calculate standard deviations as a bank-specific error measure. These 1,774 standard deviations have a maximum of 5% and a mean of 0.03%. Matching at higher regional aggregation level is perfect.

tial source of error. However, the German bond market and lending from German insurance companies are relatively small.[9]

To determine the new lending of the benchmark bank, we aggregate the change in lending to every industry/region segment over banks and rescale it by the factor used in the portfolio replication:

$$nbm_{t,i,j,r} \quad \equiv \quad \nu_{t,i,j,r} \times \frac{\sum_{k \neq i} \left( ex_{t+4,k,j,r} - ex_{t,k,j,r} \right)}{\sum_{k \neq i} TA_{t,k}}. \tag{7}$$

As region-specific figures are no longer needed, we aggregate over regions and, to make it comparable to $n_{t,i}$ from (3), also over industries, except the "worst" industry in which bank $i$ has suffered the largest loss of the quarter:

$$nbm^{reg}_{t,i} \quad \equiv \quad \sum_{j \neq \mathrm{wst}(t,i)} \sum_{r} nbm_{t,i,j,r}, \quad reg \in \{\mathrm{cty}, \mathrm{dist}, \mathrm{state}, \mathrm{DE}\}\,. \tag{8}$$

This new lending of the benchmark bank forms a control variable in the estimates. It is independent of individual regions but, of course, still characterized by the region level $reg$ at which portfolio compositions have to match.

Our approach may appear similar to the "synthetic control" introduced by Abadie et al. (2010) in the context of cigarette consumption and applied in a banking context by Dasgupta and Mason (2020), in that hypothetical variables are constructed as weighted averages from other variables. The synthetic control approach is different in purpose and construction, however. To stay in the banking context, Dasgupta and Mason assign each member of a treated group of banks an untreated (!) counterpart constructed from the total sample of untreated banks. Thus, the purpose is matching selected characteristics between treated and untreated banks, similar to the purpose of propensity score matching (we perform such a matching exercise in Section 5.3).

By contrast, the purpose of the benchmark bank is rescaling the actual competitors' lending business to the profile of the bank under consideration, regardless of the competitors' business models, their size or any other similarity criterion regarded in the synthetic control approach. Importantly, treated banks belong to the constituents of the benchmark bank as well (because they compete with the bank), and the benchmark bank's new lending is a control variable rather than the dependent variable in another observation. The benchmark bank is also technically different.[10]

While the rescaling mechanism aligns the aggregate portfolio composition of competing banks to the portfolio of bank $i$, it does not alter the relative market shares of these banks within each industry/region segment. This invariance is important for the ability of $nbm^{reg}_{t,i}$ to absorb demand shocks, or better for the question of which component of demand shocks is properly absorbed by the variable:

Let us show in more detail which type of shocks the benchmark bank captures particularly well. Demand shocks from a certain industry/region segment to individual banks are likely to include a common factor. As well, they should reflect existing bank-borrower relationships to some degree, which suggests the existence of a joint component of these shocks that is proportional to current credit exposures. If this component is still present in the ultimate new lending,

[9] In 2010, German banks were lending 1,317 billion euro to German corporates and the self-employed; German non-financials had 251 billion euro in bonds outstanding; insurers were lending 23 billion euro to corporates. Sources: Deutsche Bundesbank (2012, Sect. IV), Deutsche Bundesbank (2014, Sect. VII), Deutsche Bundesbank (2020, Sect. II).

[10] The dependent variable of the synthetic control is a weighted average of the dependent variable of other banks. Such a representation is not possible for the benchmark bank, which is composed at a more granular level.

a toy "model" for changes in loans to industry $j$ in region $r$, here in euros,

$$N_{i,j,r} \quad = \quad \gamma_{j,r} ex_{i,j,r} + \text{noise} \qquad (t \text{ omitted}), \tag{9}$$

would capture this component by the factor $\gamma_{j,r}$.[11] As a control variable, the new lending $nbm_i^{reg}$ of the benchmark bank is, in a sense, perfect for absorbing all these factors $\gamma_{j,r}$ with proportional weights as their loadings are the same in a factor representation of $nbm_i^{reg}$ and $n_i$. By contrast, the simple aggregation of competitors' new lending without rescaling leads to different factor loadings. Detailed arguments are given in Appendix B.

To get a feeling for the usefulness of this whole machinery, we repeat the exercise of Figure 1 for the county-level benchmark bank. If demand and/or systematic credit risk factors matter, $nbm_{t,i}^{\text{cty}}$ should be sensitive to the severity of $L_{t,i}$. To benchmark the benchmark, we also calculate the plain aggregate new lending of all banks (except $i$) *without* any rescaling:

$$nbm_{t,i}^{\text{plain}} \equiv \frac{\sum_{j \neq \text{wst}(t,i)} \sum_r \sum_{k \neq i} \left( ex_{t+4,k,j,r} - ex_{t,k,j,r} \right)}{\sum_{k \neq i} TA_{t,k}}. \tag{10}$$

This alternative benchmark variable is a function of $t$ and wst $(t, i)$ but basically invariant to the portfolio weights of bank $i$.[12] If these weights are relevant, $nbm_{t,i}^{\text{cty}}$ should be more sensitive to $L_{t,i}$ than the plain aggregate new lending $nbm_{t,i}^{\text{plain}}$.

Figure 2 displays the new lending business shown in Figure 1, the benchmark at county level, and the unweighted aggregate. The quite impressive similarity of $nbm_{t,i}^{\text{cty}}$ (blue solid line) and $n_{t,i}$ (black solid line) indicates that demand matters; not controlling for it would give the wrong impression of the supply side of lending. The weak (if existent) sensitivity of $nbm_{t,i}^{\text{plain}}$ (dotted line) suggests that the rescaling mechanism, targeted at a good fit of local bank business and industry composition, captures a significant dimension of demand.

### 3.2 Estimation

We want to know how a bank's new lending reacts to heavy credit losses and capital. The dummy variable for the 10% of biggest losses of all $L_{t,i}$ in the history of bank $i$ is of key interest:

$$bigL_{t,i} \equiv \mathrm{I}\left(L_{t,i} > \mathrm{Qtl}_{90\%}\left(L_{\cdot,i}\right)\right), \tag{11}$$

where $\mathrm{I}(\ldots)$ is an indicator function and dots stand for sampled indices; in this case, it is time. Other samples from which the biggest losses can be selected (pooled, quarter specific, and using a more sophisticated approach) are subject to robustness tests. To keep the effect of a big loss as free as possible from those of subsequent small losses, we delete such observations within the following 3 quarters:

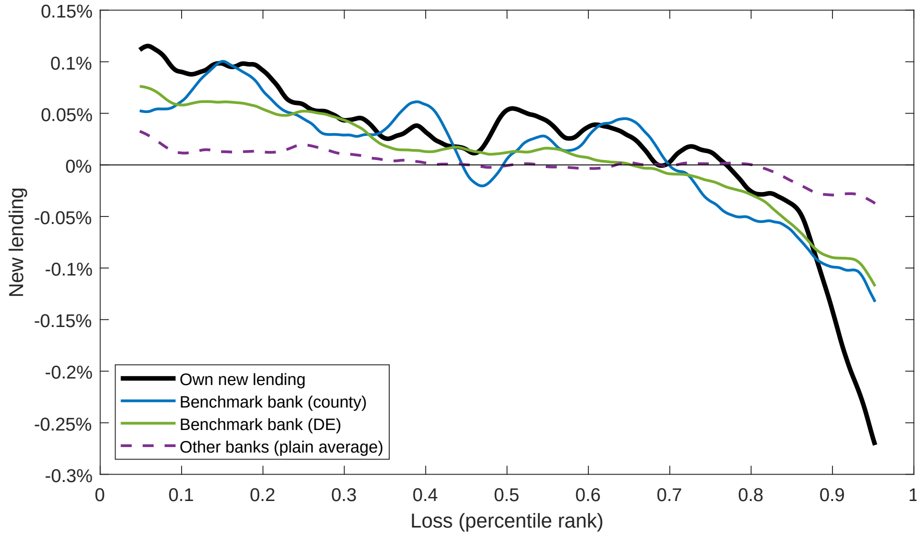
$$\text{Delete obs. } (t+s, i) \text{ if } bigL_{t,i} = 1 \text{ and } bigL_{t+s,i} = 0, \qquad s = 1, 2, 3.$$

Otherwise, the time span over which we measure new lending could include quarters in which both big and small losses take effect simultaneously.

[11] Assume that the whole lending in a certain industry/region segment falls from 18 million euro to 12 million euro as a consequence of a negative demand shock. Suppose further that three banks have had an exposure of 3, 6, and 9 million euro, respectively. In this case, and assuming there is no noise, $\gamma$ would be equal to $-\frac{1}{3}$and the lending of the banks would drop by 1, 2 and 3 million euro, respectively.

[12] Excluding a single bank from the German aggregate of bank loans has negligible impact on the outcome.

Figure 2: New lending business vs credit losses; controlling for demand




Values on the x-axis are the same as in Figure 1, representing percentile ranks of $L_{t,i}$. "Own lending" (black solid line) is $n_{t,i}$ from (3) as shown in Figure 1, the new corporate lending of bank $i$, exclusive of the "worst" industry where the biggest loss has been made. "Benchmark bank (county)" (blue solid line) is $nbm^{\text{cty}}_{t,i}$, the corresponding new lending of the benchmark bank obtained from rescaling at industry/county level. "Benchmark bank (county)" (green solid line) is $nbm^{\text{DE}}_{t,i}$, the new lending of the benchmark bank that replicates the industry composition but ignores the location of borrowers. "Other banks (plain average)" (violet dotted line) is $nbm^{\text{plain}}_{t,i}$ from (10), the benchmark new lending without rescaling. Plotted values are moving averages, using a kernel smoother that includes 10,000 observations.

---

The capital related counterpart of $bigL$ is defined as a dummy for *low capital*:

$$lowC_{t,i} \equiv \text{I}\left(Cap^{\text{Tier 1}}_{t,i} < \text{Qtl}_{10\%}\left(Cap^{\text{Tier 1}}_{t,\cdot}\right)\right),$$

where $Cap^{\text{Tier 1}}$ is the Tier-1 capital ratio based on risk-weighted assets. Importantly, $lowC_{t,i}$ is determined quarter by quarter, unlike $bigL_{t,i}$. We prefer to look at a bank's capitalization relative to its peers at the same point in time as $Cap^{\text{Tier 1}}$ strictly goes up in the period under investigation. We lag $lowC$ by four quarters to avoid the mechanical effect of a severe loss on capital. The dummy variable $bigL_{t,i} \times lowC_{t-4,i}$ is the logical AND of $lowC_{t-4,i}$ and $bigL_{t,i}$.

In the base case, we estimate new lending business over four quarters:

$$\begin{aligned} n_{t,i} \;\; = \;\; & \beta_1\, bigL_{t,i} + \beta_2\, lowC_{t-4,i} + \beta_3\, bigL_{t,i} \times lowC_{t-4,i} \\ & + \beta_4\, n_{t-4,i} + \beta_5\, ml_{t,i} + \beta_6\, nbm^{\text{cty}}_{t,i} + \beta_7\, nbm^{\text{cty}}_{t-4,i} + \beta_8\, nbm^{\text{DE}}_{t,i} + \beta_9\, nbm^{\text{DE}}_{t-4,i} \\ & + \alpha^{\text{bk}}_i + \alpha^{\text{qrt}}_t + \alpha^{\text{ind}}_{\text{wst}(t,i)} + \varepsilon_{t,i}\,, \end{aligned} \tag{12}$$

in which we include the bank's lagged new lending, the share of maturing loans $ml_{t,i}$ (see below), and the new lending of benchmark banks plus their 4-quarter lags.

We choose to include benchmark new lending both at county and national level, as either of the variables contributes to the estimate in its own way. If all borrowers were locally active and bound to credit from banks present in their county, the benchmark new lending $nbm^{\text{cty}}$ at county level would be the perfect control variable. It is clearly imperfect for different reasons.

First, the larger a borrower or the more widespread its business, the easier it is to approach another bank situated elsewhere if the current lender suddenly stops lending. Second, credit demand can be driven by systematic factors that affect larger regions commonly. Third, the business of a bank's local competitors is driven by idiosyncratic factors to a larger extent than the business of a higher aggregate of competitors, which may impair the statistical power of the locally adapted $nbm^{\text{cty}}$.

While these arguments call for control at a higher level of regional aggregation such as $nbm^{\text{DE}}$, the locally fitting benchmark bank nevertheless plays its own role as it captures local demand better than the others. Our decision to include the two ends of the aggregation scale and no intermediate levels (district and state) balances power and parsimony and relies on the first robustness test of Section 5.3.

As every industry exposure in our data is further broken down into three brackets of maturity at grant, we can approximately determine how much of the loan exposure should expire in the measurement period of $n_{t,i}$. The resulting *share of maturing loans*, $ml_{t,i}$, defined in Appendix C, is a natural lending driver simply because many loans are not rolled over when they expire, especially in project finance. Credit financing with limited lifetime creates a general bouncing in credit exposures that can partly be captured by lagged exposures (which are also included in our estimates). However, the share of maturing loans is clearly a more direct predictor.

The share of maturing loans may also influence the extent of loan cuts after a big loss since there is trivially no better time for getting rid of a loan than the day of its expiry. By contrast, loan reduction before maturity requires action, such as loan sales, and involves transaction and administrative costs. Since a bank manager who intends to downsize a loan portfolio is likely to resort to maturing loans as the presumably cheapest alternative, the available amount of such loans potentially helps to explain lending dynamics. We therefore interact $ml_{t,i}$ with our key regressor $bigL_{t,i}$.

We further include fixed effects in three dimensions. Bank fixed-effects $\alpha_i^{\text{bk}}$ target at capturing business models, the general fortune of banks in gaining market shares, and those static components of bank risk profiles that might not yet be neutralized by the bank specific definition of $bigL$. Quarterly time fixed effects $\alpha_t^{\text{qrt}}$ capture the general lending development in the observed period and, finally, fixed effects $\alpha_{\text{wst}(t,i)}^{\text{ind}}$ for the "worst" industry that recorded the loss[13] capture differences in the spillover of problems in an industry to credit demand in other industries; a reasonable part of these differences, however, should already be captured by the lending of benchmark banks.

# 4 Data

## 4.1 General aspects

We take a bank's domestic corporate credit portfolio and the corresponding losses from the Bundesbank's borrower statistics; Memmel et al. (2015) and the documentation Deutsche Bundesbank (2009) describe the data set in detail. It is consistent with the balance sheet and gives – at bank level and at quarterly frequency – the domestic corporate credit portfolio of 2378 banks, broken down into 23 industries (Table 17), and three brackets of maturity at grant (0–1y, 1–5y, >5y).

The data includes the change in value due to changes in a borrower's creditworthiness in the same breakdown. As these changes must be essential enough to become effective in the balance sheet, they include write-downs and write-ups but exclude rating transitions between non-default grades. This narrow scope fits our needs well because a write-down is a strong signal that something serious must have happened to a loan.

Although the German credit register (Millionenkredit-Register) would even provide us with bank-borrower information, the maturity breakdown of the borrower statistics and its stricter

[13] These industry dummies are formally defined as $D_{t,i,j} \equiv \mathrm{I}\left(\text{wst}\left(t,i\right)=j\right)$; see (2).

loss concept are not the only reasons why we prefer the latter. The credit register also has a reporting threshold of 1 million euro. Loans falling under this threshold do not matter much for the biggest banks, but matter a lot for the majority of banks in our sample. Their portfolio compositions would suffer from heavy biases if we restricted the analysis to loans covered by the credit register.

The register's advantage that it allows for an extremely granular control for demand à la Khwaja and Mian (2008) is maybe not as large as it may seem: Using the Belgian credit register, Degryse et al. (2019) show that most corporate borrowers in their data have – just as in Germany – one lending relationship only (such that they drop out of estimates with borrower/time FEs) and that having them included in the estimates makes a big difference. Granular FEs are clearly the method of choice if all weight is put on a clean identification but the potential bias involved becomes less acceptable if more weight is put on a correct quantification, as in our paper.

The credit register is only used as a proxy for the regional distribution of exposures in the construction of benchmark banks, for lack of regional information in the borrower statistics. We would, however, be hesitant to use this proxy for an assignment of the core variable – an individual bank's new lending – to regions, which would be the prerequisite for a standard FE control for demand at industry/region level.

We use quarterly data from 2002Q4, the first time when valuation changes were reported, to 2020Q4. Unfortunately, capital figures for the whole of 2007 are not at our disposal, which precludes a thorough analysis of the effects of the global financial crisis. The data gap is not caused by the crisis but by inconsistencies involved with the transition from Basel I to II.

New lending is simply defined as the change in the stock of outstanding loans from one period to the next, consistently with most related studies (for instance Hancock and Wilcox (1993), Berrospide and Edge (2010), and Gambacorta and Shin (2018)). We also try the alternative definition (15), which corrects for exposure changes due to revaluations. While the possibility to do this is a nice feature of our data, it turns out that it does not matter much.

We apply a mild outlier treatment by winsorizing the first and 99th percentile of the new-lending variable $n_{t,i}$. Losses (at industry level) are limited to the exposure reported for the previous quarter, which has an effect in 0.07% of the observations. Although not necessarily being data errors, these cases would make trouble in the form of more than total losses or losses arising from zero exposures. Furthermore, we remove banks with a total exposure of less than 10 million euro (leaving 1,730 banks over) and banks that have fewer than 20 observations of the loss $L_{t,i}$ under definition (1). The main estimate contains 1,193 banks.

If credit exposures and default probabilities were homogeneous across industries, the extreme credit events (those where *bigL* equals 1) would be equally spread over industries as well. As actual losses and exposures are heterogeneous across industries, the frequencies of extreme events are different in fact; however, in a moderate band between 4.6% and 14.9% (Table 17, column "Extreme losses"). Surprisingly, we cannot identify any pattern in the relationship between the occurrence of an extreme loss on the one side and, on the other side, an industry's portfolio share, its average loss rate, and the frequency of being the "worst" industry (cf. (2)) even though each of the latter should be a driver of *bigL*.[14] This absence of a visible relationship is consistent with our belief that the sources of extreme losses are mostly idiosyncratic.

[14] A regression of the 23 industry-specific averages of *bigL*, as presented under "Extreme losses" in Table 17 on the other three variables gives no significant result.

### 4.2 Surprises in credit losses

We restrict ourselves to corporate loans, leaving out the three private household sectors included in the borrower statistics, and the sector of non-profit organizations. We do so in order to strengthen the exogeneity of events. It is more a surprise to a bank if a single corporate loan has to be written off, compared to ten retail loans perishing. That is, the loss distribution of a few large loans tends to be more extreme in the tail than the loss distribution of a more granular portfolio of retail loans. Restricting ourselves to corporate loans, we argue that most of the non-zero losses observed in the corporate sectors originate from single defaults:

In our sample, 75% of the valuation changes in an industry are zero, on average, which gives us an idea of how often a single default accounts for the whole loss in an industry portfolio. Under the simplifying assumption that all loans default independently at a uniform constant intensity, the number of defaults in a portfolio follows a Poisson distribution[15] that is uniquely determined by the 75% zeros. Then, the 25% non-zero losses consist to 86% of single-default events.[16]

In a granular retail portfolio, by contrast, losses at portfolio level are much more frequent, more stable in size, and to a lesser degree driven by idiosyncratic factors. As a result, they lack the surprise aspect that is essential to our identification strategy.[17]

Idiosyncrasy alone is not sufficient to make the strategy work. We could not argue that banks are surprised by the credit events we focus on if the biggest losses in the sample did not really differ from normal losses. Three arguments support that they do differ and matter. First, Table 2 documents the loss rate $L_{t,i}$ as defined in (1) to be extremely leptokurtic. Second, compared to the average loss in the worst industry, which is $E\left(L_{t,i}\right) = 0.04\%$ of total assets, the average *big* loss $E\left(L_{t,i} \;\middle|\; bigL_{t,i} = 1\right) = 0.16\%$ is four times larger. Third, although a loss of 0.16% of total assets might seem negligible at first glance, that figure appears in another light if contrasted with profits. In our sample, the quarterly profit before taxes is only 0.055% on average, such that a big quarterly loss is three times as large as the average profit.

What is more, we look at losses over the shortest possible horizon of one quarter. If we chose a year, the bank could possibly react to a loss endogenously already in the period used for measuring whether it is a big loss or not. This choice would potentially blend the shock with endogenous, unsurprising elements.

In the robustness tests of Section 5.3 we will return to the surprising nature of big losses when we test whether dynamic credit risk taking could turn them into predictable events.

### 4.3 Summary statistics

In Table 17 in Appendix H, we report the composition of the aggregate credit portfolio and corresponding losses. Descriptive statistics are presented in Table 2, and correlations are given in Table 18 in Appendix H. The extreme kurtosis of $L_{t,i}$ supports our decision to transform the variable $L_{t,i}$ into a dummy variable. The *bigL*-conditional means at the bottom of the table show that loan growth significantly differs whether the bank has suffered a big loss:

$$E\left(n_{t,i} \;\middle|\; bigL_{t,i} = 0\right) = 0.63\% \quad \text{vs.} \quad E\left(n_{t,i} \;\middle|\; bigL_{t,i} = 1\right) = 0.29\%\,.$$

[15] The assumption of independence is not as far-fetched as it may seem: Memmel et al. (2015) find that more than 90% of the variation in a bank's loss rate is bank specific and less than 10% is due to systematic factors. The distribution is *exactly* Poisson only if a loan can default multiple times within a quarter, which does not make a difference for the low default probabilities documented in Table 17.

[16] Taking $N$, the number of loan defaults in a portfolio, to be Poisson distributed, the given probability $\Pr(N = 0) = 0.75$ implies $\Pr(N = 1) = 0.216$ and this, in turn $\Pr\left(N = 1 \mid N > 0\right) = 0.216/0.25 = 0.862$.

[17] Furthermore, we leave out non-profit organizations because their behavior (as not profit-maximizing) may be quite heterogeneous and different from that of corporates.

Table 2: Descriptive statistics of key variables

| | Loss | New lending | | | Capital | Maturing loans |
|---|---|---|---|---|---|---|
| | $L_{t,i}$ | $n_{t,i}$ | $nbm_{t,i}^{\text{cty}}$ | $nbm_{t,i}^{\text{DE}}$ | $Cap_{t,i}^{\text{Tier 1}}$ | $ml_{t,i}$ |
| Mean | 0.04% | 0.58% | 0.47% | 0.24% | 13.0% | 2.30% |
| Std | 0.08% | 1.69% | 1.35% | 0.52% | 4.61% | 2.24% |
| Q25 | 0.00% | –0.24% | –0.19% | –0.06% | 9.68% | 1.40% |
| Median | 0.01% | 0.49% | 0.35% | 0.20% | 12.6% | 1.91% |
| Q75 | 0.04% | 1.38% | 1.06% | 0.51% | 15.3% | 2.56% |
| Skewness | 14.96 | 0.16 | 0.41 | 0.28 | 2.20 | 9.45 |
| Kurtosis | 617.99 | 6.00 | 5.73 | 4.45 | 21.4 | 133 |
| Mean $\left(bigL_{t,i} = 0\right)$ | 0.02% | 0.63% | 0.49% | 0.26% | 13.3% | 2.27% |
| Mean $\left(bigL_{t,i} = 1\right)$ | 0.16%*** | 0.29%*** | 0.30%*** | 0.06%*** | 10.8% | 2.46% |
| Observations | 24041 | 24041 | 24041 | 24041 | 24041 | 24041 |

All variables except the capital ratio are normalized by the bank's total assets. $L_{t,i}$ is a bank's largest loss in one of 23 industry subportfolios, according to (1). New lending of the bank through four quarters is given by $n_{t,i}$ while $nbm_{t,i}^{\text{cty}}$ and $nbm_{t,i}^{\text{DE}}$ are the new lending of the benchmark banks at county and national level. $Cap_{t,i}^{\text{Tier 1}}$ is the Tier-1 capital ratio based on risk-weighted assets. $ml_{t,i}$, defined in (21), is the approximate loan volume maturing through the next four quarters, divided by total assets. Estimates are based on the sample used in the base case estimate of Table 3, column 1. The first and 99th percentile of $n_{t,i}$ have been winsorized. *** means that the difference in the conditional means is significant at the 1% level.

However, the corresponding differences in benchmark new lending at county and national level suggest a role for loan demand in that effect.

### 4.4 Regional distribution of exposures

The borrower statistics ("Kreditnehmerstatistik") do not contain information on the regions (in our case, counties) lent to. In order to be able to control for demand at a granular level of regions, we complement this data set with the German credit register ("Millionenkredit-Register").

Even though the detailed information on individual borrowers in the credit register lends itself to many analyses, it is biased due to a reporting threshold of 1 million euro, which does not matter much for the biggest banks, but matters a lot for the majority of German banks.

We could even construct a good set of shocks from the credit register as it includes the large borrowers that tend to cause big losses (cf. Section 4.2), but the reaction of small and medium-size banks in their lending would be fairly misrepresented if it were only calculated from loans in excess of 1 million euro.

Moreover, the lending relationship with a big borrower is presumably particularly valuable to the bank, which may motivate it to protect this relationship at the cost of relationships with smaller borrowers that would then face more drastic reductions.

That is why we are hesitant to construct our main dependent variable from the credit register; we only use it to obtain a proxy for the regional distribution of credit exposures when we construct the control variable for demand. Appendix A gives the details of how we divide credit exposures into regions.

# 5 Results

## 5.1 Baseline results

Table 3 presents the result of our base case Equation (12) and of some alternative specifications. We draw the following conclusions:

Table 3: Impact of big losses on new lending business

| Dependent variable: New lending $n_{t,i}$ (4 quarters) | (1) Base case | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Big loss $bigL_{t,i}$ | –0.255*** | –0.260*** | | –0.254*** | –0.268*** |
| | (0.0359) | (0.0366) | | (0.0350) | (0.0365) |
| Low capital $lowC_{t-4,i}$ | –0.240*** | –0.248*** | –0.240*** | | –0.231*** |
| | (0.0551) | (0.055) | (0.0513) | | (0.0567) |
| Interaction $bigL_{t,i} \times lowC_{t-4,i}$ | 0.0421 | 0.0986 | | | 0.00966 |
| | (0.115) | (0.116) | | | (0.119) |
| New lending, lag 4 $n_{t-4,i}$ | 0.0445*** | 0.0452*** | 0.0460*** | 0.0459*** | 0.0803*** |
| | (0.0105) | (0.0105) | (0.0106) | (0.0105) | (0.0104) |
| Benchm. (county) $nbm^{\text{cty}}_{t,i}$ | 0.0713*** | 0.0711*** | 0.0715*** | 0.0722*** | |
| | (0.0114) | (0.0115) | (0.0115) | (0.0115) | |
| —, lag 4 | –0.00249 | –0.00255 | –0.00334 | –0.00263 | |
| | (0.0109) | (0.0109) | (0.0109) | (0.0109) | |
| Benchm. (DE) $nbm^{\text{DE}}_{t,i}$ | 0.790*** | 0.782*** | 0.791*** | 0.789*** | |
| | (0.0512) | (0.0509) | (0.0513) | (0.0512) | |
| —, lag 4 | 0.0854* | 0.0843* | 0.0873* | 0.0808* | |
| | (0.0464) | –0.0463 | (0.0465) | (0.0464) | |
| Maturing loans $ml_{t,i}$ | –0.118*** | –0.114*** | –0.118*** | –0.120*** | –0.120*** |
| | (0.0346) | –0.0347 | (0.0346) | (0.0344) | (0.0350) |
| —, lag 4 | –0.136*** | –0.136*** | –0.139*** | –0.137*** | |
| | (0.0282) | (0.0283) | (0.0284) | (0.0283) | |
| $ml^{\neg\text{b, centered}}_{t,i} \times bigL_{t,i}$ | | –0.0587* | | | |
| | | (0.0330) | | | |
| Fixed effects | bank, time, worst industry | | | | |
| Observations | 24041 | 24041 | 24041 | 24041 | 24041 |
| Adj. $R^2$ | 0.2356 | 0.236 | 0.2338 | 0.2347 | 0.2029 |
| Adj. $R^2$ (within) | 0.0543 | 0.0548 | 0.0519 | 0.0531 | 0.0137 |

This table shows how a big credit loss in a single industry changes new corporate lending to other industries (see Equation 12). All variables in percent, except dummies. Period: 2002Q4–2020Q4. The year 2007 is excluded for data reasons. All losses in the sample have been pre-selected as the worst loss in a single industry for each time and bank. The biggest 10% of such credit losses, taken from the individual history of each bank, are marked by the dummy variable $bigL$. The dummy $lowC$ takes value 1 if a bank's capital ratio is in the first decile of all banks' Tier-1 capital ratios in the respective quarter (lag 4). Benchmark new lending $nbm^{\text{cty}}_{t,i}$, defined in (8), is the new lending of a hypothetical competitor of bank $i$, constructed from all other banks such that it resembles the bank's portfolio weight in each industry in each county; the nationwide counterpart $nbm^{\text{DE}}_t$ resembles industry weights but neglects regional weights. The approximate share of maturing loans $ml_{t,i}$ is defined in (21). It has been centered for the interaction with $bigL$. The industry with a bank's largest loss in quarter $t$ is denoted by $\text{wst}(t,i)$. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

First, big credit losses, that is the worst 10% of quarterly credit losses in a single industry, lead to a significant reduction in new lending. A bank that has suffered such a big loss reduces the lending to other industries (compared to a bank without such a loss) by 0.26% of total assets. Transforming the dummy coefficient into a euro sensitivity (Appendix E), we find that one euro of substantial credit losses leads to a reduction of 1.79 euro in new lending; the 95% confidence interval for this estimate is $[1.30, 2.28]$.

Second, a potential substitution effect for credit supply through other institutions is dominated by the demand effect, provided it exists. Otherwise, $nbm_t^{\text{cty}}$ and $nbm_t^{\text{DE}}$ should have negative coefficients. We have a closer look at this question in Section 5.2.

Third, bank capital matters. We see that banks with low capital provide significantly less credit than banks with a higher capital ratio. However, the interaction term of $lowC$ and $bigL$ is insignificant; not even in times of crises we find thinly capitalized banks to react more strongly to heavy losses (see below). This result conflicts with common wisdom insofar as a thinly capitalized bank should always suffer more from a heavy loss than its well capitalized counterpart since a bigger part of the capital buffer is lost, ceteris paribus.

To offer an explanation, capital shortfalls (or the perception of them) might be dealt with mainly on the liability side by corporate action such as retaining earnings, issuing new capital, or debt-equity swaps. This argument is supported by Memmel and Raupach (2010) who find around 80% of the adjustment of capital ratios to take place on the liability side. Similarly, Kok and Schepens (2013) find that banks whose current capital ratios are below a target level try to increase their capital rather than change the asset composition.

Fourth, controlling for credit demand is crucial. The new lending of both the counties-based and the national benchmark bank is highly significant; their inclusion reduces the coefficients for the credit losses as can be seen by comparing columns 1 and 5 in Table 3.

Fifth, the share of maturing loans $ml_{t,i}$ helps to explain lending dynamics (and will do so throughout all specifications; this variable is responsible for 20% of the baseline within-$R^2$). The negative coefficient of $-0.12$ says that most (88%) but not all maturing loans are replaced by new credit. The interaction of $ml_{t,i}$ and $bigL_{t,i}$ in column 2 is only significant at 10%, that is, we find only weak support for our conjecture that bank managers would particularly resort to maturing loans when they reduce lending. As the interaction is insignificant in various other (unreported) specifications, we omit it in the sequel.

Sixth, big losses have low explanatory power for lending, at first glance, as the inclusion of $bigL_{t,i}$ and $bigL_{t,i} \times lowC_{t-4,i}$ increases the within-$R^2$ by only 0.24 percentage points (Table 3, column 1 vs. 3). But a driver of seemingly low power at bank level can have greater aggregate effects, which may become relevant when microprudential results are transferred to macroprudential considerations. In stress tests, a common shock hits the banking system, and an effect that otherwise appears to get lost in idiosyncratic variation may have considerable aggregate consequences.

To illustrate the difference, suppose banks react to big losses as in our baseline estimate whereas the individual shocks $bigL_i$ are replaced by a common (stress test) shock $bigL$. According to a simplified model outlined in Appendix D, the 0.24 percentage points of the $R^2$ attributed to $bigL$ in the bank-level estimate correspond to 16.3 percentage points attributed to the common $bigL$ in an estimate at national level.

#### Have crisis times been different?

As our observation period from 2002 to 2020 includes the global financial crisis and the sovereign debt crisis, the question is warranted whether banks have reacted differently to a big credit loss in these times. Being characterized by distressed capital, these periods are also an opportunity to take a closer look at the interplay of capital and big losses which does not seem to matter in the results shown so far.

As mentioned in Section 4.1, the important year 2007 is not at our disposal. We therefore lump the remaining part of the first crisis together with the second and define a single crisis period lasting from 2008Q1 to 2012Q4. Both crises are fairly different in nature but have in common that bank capital was under distress and fears were great. Even though big losses in a single industry were certainly not perceived as the biggest risks, they were particularly inconvenient when occurring during that time.

Table 4 shows that the crisis period is special to some degree. If we use a sample split (columns 2 and 3), the coefficient of *bigL* increases in size if the crisis is excluded, whereas it falls, against intuition, in the crisis period. By contrast, and quite intuitively, the level of capital matters a lot more in the crisis.

Having not found a particular effect when big losses combine with low capital for the whole period, the sample split does not uncover any such effect limited to crisis or normal times. Neither we find a significant interaction term in one of the robustness tests executed in Section 5.3 where we change the definition of *bigL*.

Column 4 presents another specification that runs over the whole period but, instead, interacts a crisis dummy with the key regressors *bigL* and *lowC* and combinations thereof. While the primary effect of big losses appears to be larger than in the base case, the most interesting triple interaction $Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$ remains insignificant and so replicates the non-finding of the sample split.

## 5.2 Further results

### Do other banks step in?

Next, we have a deeper look into the potential substitution of credit supply by the competitors of a bank that has suffered a big loss. The positive coefficients of benchmark new lending in the base case regression suggest that a substitution, if present, does not dominate the effect of demand.

We refine this observation in Table 5 by additional terms, which interact *bigL* with the centered[18] new lending of benchmark banks. If competitors substitute the lending cut of a bank that has incurred a big loss, they lend more relative to what they would lend as a pure reaction to credit demand. Substitution should therefore entail negative coefficients in the gray rows of Table 5. As they lack significance, we find no evidence of a substitution effect.

### Impact on other loan sectors

While three sectors have been excluded from the main analysis for different reasons, they are nevertheless interesting dependent variables. In column 1 and 2 of Table 6 we first look at lending to the troubled industry in which the largest loss $L_{t,i}$ has been suffered. Despite endogeneity issues and other reasons to exclude it from the main analysis[19], measuring the lending effect on this industry may nevertheless help to calibrate stress tests. In a regression similar to the base case[20], we find a small lending effect of a big loss in the same industry when considered relative to total assets, our standard normalizing variable. Relative to the industry's own exposure, the effect is at the same order of magnitude as in the base case.[21]

[18] Subtracting the mean allows for a direct interpretation of the coefficient's sign.

[19] See the introduction and the beginning of Section 3.

[20] The dependent variable refers to a single sector only. They are normalized by total assets as before such that coefficients inform us about potential cuts relative to the bank's overall exposure. They include the (borrower-) extensive margin but are not necessarily informative about the relative change within a sector. We include a further lag term (two years) of new lending to capture a negative bouncing effect of temporary shocks to loan levels.

[21] Normalizing the dependent variable by total assets, the estimated coefficient is smaller by factor 10 but refers to a single factor. This means a lot more relative to the sector's exposure and would be interesting per se, but normalization by sector exposures has proven to introduce too much noise. Furthermore, a uniform normalizing variable has the advantage that the effects become comparable from a bank/portfolio perspective, which is particularly suitable for stress tests.

Table 4: Impact of big losses in crisis and normal times

| Dependent variable: New lending $n_{t,i}$ (4 quarters) | (1) Base case | (2) Crisis | (3) Normal | (4) Interaction |
|---|---|---|---|---|
| Big loss $bigL_{t,i}$ | –0.255*** | –0.133** | –0.313*** | –0.302*** |
| | (0.0359) | (0.0566) | (0.0487) | (0.0472) |
| Low capital $lowC_{t-4,i}$ | –0.240*** | –0.410*** | –0.219*** | –0.260*** |
| | (0.0551) | (0.102) | (0.0704) | (0.0646) |
| Interaction $bigL_{t,i} \times lowC_{t-4,i}$ | 0.0421 | 0.0207 | 0.128 | 0.127 |
| | (0.115) | (0.180) | (0.158) | (0.153) |
| New lending, lag 4 $n_{t-4,i}$ | 0.0445*** | –0.138*** | 0.0162 | 0.0443*** |
| | (0.0105) | (0.0186) | (0.0125) | (0.0106) |
| Benchm. (county) $nbm^{\text{cty}}_{t,i}$ | 0.0713*** | 0.111*** | 0.0623*** | 0.0713*** |
| | 0.0114) | (0.0217) | (0.0137) | (0.0114) |
| —, lag 4 | –0.00249 | 0.0285 | 0.00487 | –0.00254 |
| | (0.0109) | (0.0198) | (0.0132) | (0.0109) |
| Benchm. (DE) $nbm^{\text{DE}}_{t,i}$ | 0.790*** | 0.483*** | 0.745*** | 0.791*** |
| | (0.0512) | (0.0837) | (0.0668) | (0.0512) |
| —, lag 4 | 0.0854* | –0.0845 | 0.167*** | 0.0855* |
| | (0.0464) | (0.0821) | (0.0606) | (0.0464) |
| Maturing loans $ml_{t,i}$ | –0.118*** | –0.444*** | –0.0668* | –0.118*** |
| | (0.0346 | (0.0721) | (0.0342) | (0.0346) |
| —, lag 4 | –0.136*** | –0.0821 | –0.164*** | –0.136*** |
| | (0.0282) | (0.0538) | (0.0296) | (0.0282) |
| $Cris_t \times bigL_{t,i}$ | | | | 0.120* |
| | | | | (0.0725) |
| $Cris_t \times lowC_{t-4,i}$ | | | | 0.0674 |
| | | | | (0.1020 |
| $Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$ | | | | –0.240 |
| | | | | (0.2330) |
| Fixed effects | bank, time, worst industry | | | |
| Observations | 24041 | 6975 | 17007 | 24041 |
| Adj. $R^2$ | 0.2356 | 0.4296 | 0.2391 | 0.2357 |
| Adj. $R^2$ (within) | 0.0543 | 0.0776 | 0.0437 | 0.0543 |

All variables as in Table 3, except in column 4, which includes interaction terms with a crisis dummy for the period 2008Q1–2012Q4. This is also the estimation period for column 2. Total period: 2002Q4–2020Q4. The year 2007 is excluded for data reasons. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

Big losses seem to have no effect on lending to non-profit organizations (NPOs), by contrast to the retail sector where we see a sensitivity half as large as for the corporate sectors in the base case. As retail loans make up around 11% of total assets and hence much less than the total of corporate loans, relative cuts in the retail sector are actually substantial.

Retail loans are basically the only sector in which the crisis period seems to have made a real difference. In column 6, the interaction term $Cris_t \times bigL_{t,i}$ offsets the effect of $bigL_{t,i}$ almost perfectly, suggesting that banks resort to cuts in retail lending after big losses in normal times quite heavily – to the same euro extent as in the corporate sector – but stop to do so in crisis times. Capital cannot explain this relationship well, neither empirically (the coefficient of $lowC$ is tiny and insignificant) nor theoretically, as retail exposures do not bind much regulatory capital.

Table 5: Is decreased lending substituted by competitors?

| Dependent variable: New lending $n_{t,i}$ (4 quarters) | (1) Base case | (2) | (3) |
|---|---|---|---|
| Big loss $bigL_{t,i}$ | –0.255*** | –0.247*** | –0.293*** |
| | (0.0359) | (0.0368) | (0.0483) |
| Low capital $lowC_{t-4,i}$ | –0.240*** | –0.239*** | –0.258*** |
| | (0.0551) | (0.0550) | (0.0646) |
| Interaction $bigL_{t,i} \times lowC_{t-4,i}$ | 0.0421 | 0.0378 | 0.120 |
| | (0.115) | (0.115) | (0.153) |
| $bigL_{t,i} \times nbm_t^{\text{cty,centered}}$ | | –0.00646 | –0.00851 |
| | | (0.0317) | (0.0317) |
| $bigL_{t,i} \times nbm_t^{\text{DE,centered}}$ | | 0.107 | 0.106 |
| | | (0.0930) | (0.0931) |
| $Cris_t \times bigL_{t,i}$ | | | 0.119* |
| | | | (0.0726) |
| $Cris_t \times lowC_{t-4,i}$ | | | 0.0667 |
| | | | (0.102) |
| $Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$ | | | –0.233 |
| | | | (0.232) |
| ... | ... | ... | ... |
| Observations | 24041 | 24041 | 24041 |
| Adj. $R^2$ | 0.2356 | 0.2357 | 0.2357 |
| Adj. $R^2$ (within) | 0.0543 | 0.0543 | 0.0543 |

All variables, fixed effects, and period as in Table 4, except $bigL_{t,i} \times nbm_t^{\text{cty,centered}}$ and $bigL_{t,i} \times nbm_t^{\text{DE,centered}}$ where the indicator of big losses is interacted with the (centered) new lending of benchmark banks. Benchmark banks differ in the aggregation level (or size) of regions at which they are fit to individual banks. Column 1 is the base case of Table 3. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

### Impact on securities holdings

Our data breaks securities holdings of banks down into i) stocks, ii) corporate bonds, iii) bank bonds, and iv) government bonds, where the latter are subdivided into domestic and foreign bonds. In Table 7, we estimate the impact of big losses ($bigL = 1$) on the change in securities positions.

Regarding stocks, we expect that a bank with low capital reduces these positions because their sale releases much more regulatory capital than selling the same amount of, say, bonds with a reasonable rating. However, we do not find any impact of big losses or low capital.

A corporate or bank bond should normally be treated in the same way as a loan to the same firm because both require the same amount of regulatory capital, by and large. We only find an effect of $lowC$ on corporate bonds and basically no effect of $bigL$. Our estimate suggests that corporate and bank bonds are not an important means to steer corporate credit risk, which is surprising as bonds are easier to divest than loans. The finding is less surprising for bank bonds since the definition of this asset class includes covered bonds – selling them would not release much regulatory capital if they mainly consist of highly rated senior tranches.

The results concerning government bonds (domestic or foreign) are somewhat puzzling. We would expect banks with low capital or a big loss to resort to government bonds and increase these positions as they require zero regulatory capital. Instead, we observe the opposite effect in Table 7 for capital. This pattern is qualitatively in line with the idea that government bonds are generally an unattractive investment but are held as an insurance against illiquidity if corporate loans are funded by short-term liabilities. If a reduction in the loan exposure is paralleled by a

Table 6: Lending to the worst industry and non-corporate sectors

| Dependent: $n_{t,j}$ | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Sector: | Worst industry | | Non-profit organizations | | Retail | |
| $bigL_{t,i}$ | –0.0253** | –0.0292** | –0.000805 | –0.000721 | –0.111*** | –0.195*** |
| | (0.0103) | (0.0129) | (0.00186) | (0.00245) | (0.0358) | (0.0450) |
| $lowC_{t-4,i}$ | –0.0217 | –0.0346** | –0.00539** | –0.00395 | 0.00720 | 0.00973 |
| | (0.0145) | (0.0162) | (0.00220) | (0.00253) | (0.0455) | (0.0510) |
| $bigL_{t,i} \times lowC_{t-4,i}$ | –0.0173 | –0.00760 | 0.00552 | 0.00310 | –0.0254 | –0.0165 |
| | (0.0372) | (0.0477) | (0.00456) | (0.00610) | (0.110) | (0.140) |
| $Cris_t \times bigL_{t,i}$ | | 0.0126 | | –0.000300 | | 0.217*** |
| | | (0.0207) | | (0.00377) | | (0.0737) |
| $Cris_t \times lowC_{t-4,i}$ | | 0.0530* | | –0.00493 | | –0.00624 |
| | | (0.0287) | | (0.00450) | | (0.0803) |
| $Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$ | | –0.0387 | | 0.00738 | | –0.00531 |
| | | (0.0771) | | (0.00958) | | (0.225) |
| ... | ... | ... | ... | ... | ... | ... |
| Observations | 18524 | 18524 | 23472 | 23472 | 23472 | 23472 |
| Adj. $R^2$ | 0.0955 | 0.0956 | 0.0802 | 0.0802 | 0.2473 | 0.2477 |
| Adj. $R^2$ (within) | 0.0185 | 0.0186 | 0.0340 | 0.0339 | 0.0205 | 0.0209 |

The dependent variable is new lending either in the worst industry where the loss occurred (columns 1 and 2) or in one of the non-corporate sectors, normalized by total assets. All variables in percent, except dummies. The key variables *bigL* and *lowC* meet the base case definition; big losses do not include the non-corporate sector. Control variables are defined analogously to the base case but are restricted to the dependent variable's respective sector, which is wst $(t, i)$ in columns 1 and 2 (see (2)). The estimate for the worst industry also includes new lending lagged by 8 quarters. Fixed effects are the same as in the base case (bank, time, worst industry). Total period: 2002Q4–2020Q4. Crisis period: 2008Q1–2012Q4. The year 2007 is excluded for data reasons. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

reduction in short-term funding, the bank could respond to the reduced need for liquid assets by selling government bonds. The reduction in government bonds shown in Table 7 would then be directly caused by the reductions found in the main table – or by the loss itself, which is also an exposure reduction. However, the effect is a bit large for this explanation, and the puzzle remains.

In general, we find no evidence that big losses in a bank's corporate credit portfolio have an impact on its securities holdings, which may point to an isolated management of credit losses within the banking book. This finding is in contrast to the results of De Jonghe et al. (2020) who find that changes in a bank's capital requirements impact nearly all balance sheet items.

We also notice substantial negative autocorrelation in all five asset classes, which could mean that banks try to keep each of these positions near a target inventory. As securities positions can be quickly adapted, it is possible that they are used as expected (for instance, stock sales for capital relief after a big loan loss), but too quickly and for a too short period of time to become visible in our estimates at quarterly frequency.

## 5.3 Robustness tests

As we use a non-standard type of shock to the banks and also apply a novel method to control for loan demand, we perform an extensive set of robustness tests. The first test investigates the optimal size of regions (county, district,...) within which benchmark banks must replicate the portfolio composition of the bank under consideration. As well, we measure how much the inclusion of benchmark banks contributes to the model's explanatory power.

We then test for potential endogeneity issues of the treatment ($bigL = 1$) using a whole group of modifications: (1) changing the samples used in the definition of *bigL*; (2) constructing control samples with matching propensity scores; (3) eliminating potential systematic components in

Table 7: Impact on securities holdings: stocks, certificates, and bonds

| Dep.: Δposition/TA (%)<br>Securities type: | (1)<br>Stocks and certificates | (2)<br>Bank bonds | (3)<br>Corporate bonds | (4)<br>Government bonds (DE) | (5)<br>Government bonds (foreign) |
|---|---|---|---|---|---|
| $bigL_{t,i}$ | –0.0288 | 0.0340 | –0.0106 | 0.0110 | –0.0172* |
| | (0.0238) | (0.0547) | (0.0168) | (0.0186) | (0.00923) |
| $lowC_{t-4,i}$ | –0.0455 | 0.0143 | –0.0636*** | –0.103*** | –0.0399*** |
| | (0.0322) | (0.0621) | (0.0213) | (0.0221) | (0.0124) |
| $bigL_{t,i} \times lowC_{t-4,i}$ | –0.0573 | 0.00691 | 0.0389 | –0.0537 | 0.0453* |
| | (0.0722) | (0.141) | (0.0474) | (0.0522) | (0.0261) |
| Δposit./TA (%), lag 4 | 0.0137 | –0.163*** | –0.0148 | –0.0862*** | –0.111*** |
| | (0.00968) | (0.00765) | (0.00963) | (0.0109) | (0.0111) |
| posit./TA (%), lag 4 | –0.110*** | –0.227*** | –0.114*** | –0.227*** | –0.189*** |
| | (0.00486) | (0.00498) | (0.00609) | (0.00733) | (0.00724) |
| Fixed effects | bank, time, worst industry | | | | |
| Observations | 27361 | 27361 | 27361 | 27361 | 27361 |
| Adj. $R^2$ | 0.2001 | 0.2294 | 0.1813 | 0.2203 | 0.1777 |
| Adj. $R^2$ (within) | 0.0699 | 0.132 | 0.0565 | 0.149 | 0.116 |

The dependent variable is the change in a securities position, normalized by total assets. All variables in percent, except dummies. The key variables *bigL* and *lowC* meet the base case definition. Control variables are the dependent variable lagged by four quarters and the level of the position, both normalized by total assets. Bank bonds include covered bonds (Pfandbriefe). Total period: 2002Q4–2020Q4. The year 2007 is excluded for data reasons. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

the credit losses; (4) correcting for autocorrelation in the credit losses caused by seasonal effects; and (5) varying the severity of losses and states of low capital by variation of the tail probability.

Further tests capture individual aspects, which are: the impact of the time horizon over which new lending is measured, the included fixed effects, an alternative definition of new lending, which corrects for changes in loan valuation, and the measure of capital. We summarize the tests in Section 6.

### The breakdown of regions vs. demand control and the use of portfolio rescaling

For reasons outlined in Section 3.2, benchmark banks at different regional levels may capture different features of bank competition and credit demand. In order to test their relative performance – and whether it makes sense to combine them – we construct four types of benchmark banks that differ in the size of regions across which a benchmark bank must replicate the industry/region portfolio composition of the bank under consideration; the industry breakdown is unchanged.

The portfolio composition of the most disaggregate benchmark bank fits at the level of 401 counties (in pairs with 23 industries, as in all cases). The next type fits at the level of 38 administrative districts ("Regierungsbezirke"), a political sub-structure of the 16 German states ("Bundesländer"); these states define the next-higher aggregation level. We also construct a nationwide benchmark bank (marked by "DE" for "Deutschland") with portfolio weights that fit with regard to industries, whereas any regional information is ignored. If the portfolios of two banks have equal industry weights, their nationwide benchmark bank is nearly[22] the same. Finally, we also include the plain national aggregate (10), which does not even account for industry weights.

Table 8 shows the results. Most importantly, the choice of the benchmark bank leaves the impact of the main regressors nearly unchanged, and the interaction term is not significant

[22]There are differences because a bank is excluded from the construction of its benchmark bank. However, no German bank is dominant enough to make a relevant difference in that respect.

anywhere.

In column 5, which contains the new business of all four types of benchmark banks, the coefficients at all aggregation levels are significant, with the largest coefficient at state level.

If only one benchmark bank is included (columns 1–4), we see that the coefficient and the within-$R^2$ increase with the size of regions. This effect is consistent with a decreasing presence of idiosyncratic drivers in local lending. The higher the level of regional aggregation, the smaller the disturbances that bias the estimation coefficients downwards.[23] The most disaggregate level nevertheless adds more than just noise to the model; otherwise, new lending at county level should turn insignificant in the full specification of column 5.

In light of Figure 2, it is not surprising that the plain nationwide aggregate new lending $n_{t,i}^{\text{agg},\neg\text{b}}$ in column 7 is insignificant and has the opposite sign.

Provided that differences in the within-$R^2$ are a proper success criterion for alternative specifications, we may draw the conclusion that adapting the industry composition of the benchmark bank matters a lot, whereas the regional distribution does not create much power on top.

The ultimate baseline specification of column 6 includes the benchmark banks at county and national level and thus the two ends of the aggregation scale. This choice is made in respect of general model parsimony and the lowest correlation among these regressors (Table 18).

**Alternative definitions of a big loss**

Our base case specification defines a big loss to be one of the worst 10% of losses from the individual history $\{L_{t,i} : t = 1, \ldots, T\}$ of each bank. We prefer this choice because it excludes the self-selection of banks into suffering particularly many (or few) losses to a large extent, at least to the extent that this selection is static. Whatever a bank does, it is guaranteed to experience the same proportion of big losses as every other bank.

This approach does not exclude trends, however, unlike a selection from quarter-specific samples such that $bigL_{t,i}$ takes value 1 if $L_{t,i}$ belongs to the worst 10% across all banks in that quarter. This improvement comes at the price of some banks having many big losses while others do not have any in their history (keep in mind, however, that our estimates include bank fixed effects).

The approach also eliminates substantial seasonality in big losses[24] which is most likely due to seasonal updates of credit risk information, for instance through the annual reports that corporate borrowers have to submit. As we do not see endogeneity issues with this seasonality, we accept it for the base case in favor of explanatory power, simply because losses in Q1 are indeed larger than the others, on average and in the extremes.

Both definitions of *bigL* have in common that they lose power as either some big losses in particularly risky bank portfolios are ignored, or losses from a particularly bad quarter. This motivates a third definition that simply selects losses from the pooled sample. Focusing on the most severe losses possible, we should observe the strongest lending reaction.

All three definitions have in common that the frequency of big losses substantially varies in one dimension (in time for the base case, in the cross-section for the quarterly definition) or even in both dimensions (for the pooled definition). It is, however, possible to take a more balanced approach and achieve quite stable frequencies both in the cross-section and time. To this end,

[23] For illustration, think of a model $Y_i = \alpha + \beta Z_i + \varepsilon_i$. The explanatory variable $Z_i$ is, however, only indirectly observable through $X_i = Z_i + \eta_i$ with an independent shock $\eta_i$. Here, $\eta_i$ stands for idiosyncratic factors that drive individual local lending. An estimation must then rely on the model $Y_i = \alpha' + \beta' X_i + \varepsilon'_i$. The higher the variance of $\eta$, the lower becomes the asymptotic regression coefficient $\beta' = \text{cov}(Y, X) / \text{var}(X) = \text{cov}(Y, Z) / (\text{var}(Z) + \text{var}(\eta))$.

[24] The base case big loss has the highest frequency in Q1 (24%), followed by Q4 (10.4%), Q2 (5.3%), and Q3 (4.9%).

Table 8: Benchmark banks: Varying the granularity of regions

| Dependent variable: New lending $n_{t,i}$ | (1) | (2) | (3) | (4) | (5) | (6) Base case | (7) Plain agg. |
|---|---|---|---|---|---|---|---|
| $bigL_{t,i}$ (dummy) | –0.260*** | –0.254*** | –0.250*** | –0.257*** | –0.251*** | –0.255*** | –0.261*** |
| | (0.0364) | (0.0359) | (0.0359) | (0.0360) | (0.0357) | (0.0359) | (0.0365) |
| $lowC_{t-4,i}$ (dummy) | –0.218*** | –0.241*** | –0.244*** | –0.248*** | –0.244*** | –0.240*** | –0.227*** |
| | (0.0559) | (0.0555) | (0.0552) | (0.0553) | (0.0549) | (0.0551) | (0.0564) |
| $bigL_{t,i} \times lowC_{t-4,i}$ | 0.0232 | 0.0408 | 0.0397 | 0.0459 | 0.0458 | 0.0421 | 0.0244 |
| | (0.119) | (0.117) | (0.117) | (0.116) | (0.115) | (0.115) | (0.120) |
| $n_{t-4,i}$ (lag 4) | 0.0654*** | 0.0531*** | 0.0512*** | 0.0456*** | 0.0419*** | 0.0445*** | 0.0733*** |
| | (0.0104) | (0.0105) | (0.0105) | (0.0106) | (0.0106) | (0.0105) | (0.0105) |
| $ml_{t,i}$ maturing | –0.104*** | –0.105*** | –0.106*** | –0.112*** | –0.117*** | –0.118*** | –0.0921*** |
| | (0.0339) | (0.0344) | (0.0343) | (0.0348) | (0.0344) | (0.0346) | (0.0344) |
| —, lag 4 | –0.118*** | –0.132*** | –0.127*** | –0.135*** | –0.138*** | –0.136*** | –0.110*** |
| | (0.0272) | (0.0282) | (0.0281) | (0.0284) | (0.0285) | (0.0282) | (0.0275) |
| $nbm_t^{\text{cty}}$ (county) | 0.124*** | | | | 0.0405*** | 0.0713*** | |
| | (0.0118) | | | | (0.0117) | (0.0114) | |
| —, lag 4 | 0.0307*** | | | | –0.00926 | –0.00249 | |
| | (0.0106) | | | | (0.0114) | (0.0109) | |
| $nbm_t^{\text{dist}}$ (district) | | 0.360*** | | | 0.121*** | | |
| | | (0.0227) | | | (0.0293) | | |
| —, lag 4 | | 0.0899*** | | | –0.0110 | | |
| | | (0.0209) | | | (0.0294) | | |
| $nbm_t^{\text{state}}$ (state) | | | 0.443*** | | 0.143*** | | |
| | | | (0.0271) | | (0.0363) | | |
| —, lag 4 | | | 0.140*** | | 0.0803** | | |
| | | | (0.0250) | | (0.0374) | | |
| $nbm_t^{\text{DE}}$ (DE) | | | | 0.854*** | 0.562*** | 0.790*** | |
| | | | | (0.0512) | (0.0586) | (0.0512) | |
| —, lag 4 | | | | 0.0868* | 0.0216 | 0.0854* | |
| | | | | (0.0451) | (0.0525) | (0.0464) | |
| $nbm_t^{\text{plain}}$ (no wgts.) | | | | | | | –0.250 |
| | | | | | | | (0.214) |
| —, lag 4 | | | | | | | –0.198 |
| | | | | | | | (0.162) |
| Fixed effects | bank, time, worst industry | | | | | | |
| Observations | 24041 | 24041 | 24041 | 24041 | 24041 | 24041 | 24041 |
| Adj. $R^2$ | 0.2136 | 0.2267 | 0.2300 | 0.2332 | 0.2406 | 0.2356 | 0.2053 |
| Adj. $R^2$ (within) | 0.0270 | 0.0432 | 0.0473 | 0.0512 | 0.0604 | 0.0543 | 0.0167 |

All variables and observation period as in Table 3, except the definition of benchmark banks. They differ in the aggregation level (or size) of regions at which they are fit to individual banks. That is, the benchmark bank has the same portfolio weight in each industry/region segment as the individual (benchmarked) bank. A region can be a county, an administrative district, a state, or Germany (DE). Column 6 coincides with column 1 of Table 3. Column 7 uses the plain national aggregate of new lending (excluding bank $i$ and its "worst" industry) as control variable; see (10). Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

we take the logarithm of $L_{t,i}$ (which has a much less skewed distribution than $L_{t,i}$ and is not bounded by 0) and set

$$bigL_{t,i} \equiv \mathrm{I}\left(\log L_{t,i} > \mu_i + \nu_t\right). \tag{13}$$

The thresholds $\mu_i$ and $\nu_t$ are subject to calibration towards a low variation of both $E\left(bigL_{t,i} \mid i\right)$ and $E\left(bigL_{t,i} \mid t\right)$ under the restriction $E\left(bigL_{t,i}\right) = 10\%$. Our calibration algorithm[25] (see Appendix F) pushes the standard deviation of $E\left(bigL_{t,i} \mid t\right)$ down from 6.6% (base case) to 0.3% and that of $E\left(bigL_{t,i} \mid i\right)$ from 10.2% (quarterly definition) to 1.4%. If big losses that are such equally distributed in the long- and cross-section replicate our base case findings, trends or strategic self-selection are probably not a concern.

Table 9 compares the main estimate under the four alternative definitions of *bigL*. Surprisingly, big losses from the pooled sample (column 3) do not trigger the biggest lending cuts but the base case estimate. The quarterly and balanced definition show the weakest effect, presumably due to the dilution of the bigger losses in Q1 with smaller losses in other quarters.

Other coefficients are mainly unaffected. Including a number of unreported further tests[26] that include, as in Table 4, the interaction of the crisis period with big losses and low capital, we cannot confirm the nevertheless plausible hypothesis that banks would contract their lending even further when big losses combine with low capital in generally stressful times.

While the reaction to quarterly selected or balanced big losses is one fourth to one third weaker than in the base case, we deem their significance and the consistent sign an important counter-argument to the suspicion that a simple coincidence of bad quarters (in terms of losses) could have driven our main estimate; general recessionary lending cuts are absorbed by time fixed effects anyway. Two further tests (executed below: removing systematic loss components and including time FEs) do not substantiate this suspicion either.

**Dynamic credit risk taking – Matching control samples**

While the constant 10% share of *bigL* in every bank's sample of losses prevents an over-treatment for the whole observation period, we have already discussed the possibility that bank managers take high default risks *temporarily* which then might create an endogenous link between big losses and subsequent lending. To the extent that such a (mostly unobserved) temporary risk choice is reflected in observable variables, we test for its potential link with big losses by constructing balanced samples from a propensity score matching (PSM) procedure.

A propensity score predicts the probability to be treated ($bigL = 1$) under a probit model from lagged variables. We include the bank's new lending (5Q and 8Q lags) since a lowering of lending standards might correlate with loan expansion; the new lending of benchmark banks (4Q) if such a standard lowering has a systematic component; the share of maturing loans as a proxy for the leeway to adjust credit portfolio risk (4Q), log total assets (4Q), regulatory T1-capital (4Q, centered at quarterly medians), which can have effects in either direction[27]; and FEs for the industry that recorded the loss, which capture industry specific portfolio shares and credit risk levels.[28]

[25]The approach is similar to a panel quantile regression with bank and quarter fixed effects as the only regressors. As Stata's quantile regression routines at hand do not really bring the variation down, we use our own algorithm.

[26]We have also varied the definition of *bigL* in most of the other tests of this section.

[27]A capital ratio below the optimum normally reduces a bank's risk appetite and triggers action to restore the optimum ratio. Very scarce capital could, however, tempt a gambling for resurrection.

[28]Since we perform further (unreported) tests in which we combine PSM with the alternative definitions of *bigL* from the last test, we include further dummies for seven bank types (commercial and universal banks, mortgage banks, cooperative banks, savings banks, Landesbanken, building associations, and special banks) as a proxy for

Table 9: Testing variants of the dummy $bigL$ for big losses

| Dependent: $n_{t,i}$ | (1) (base case) | (2) | (3) | (4) |
|---|---|---|---|---|
| Samples for $bigL_{t,i}$: | By bank | Quarterly | Pooled | Balanced |
| Big loss $bigL_{t,i}$ | –0.255*** | –0.174*** | –0.232*** | –0.167*** |
| | (0.0359) | (0.0408) | (0.0436) | (0.0316) |
| Low capital $lowC_{t-4,i}$ | –0.240*** | –0.168*** | –0.196*** | –0.184*** |
| | (0.0551) | (0.0531) | (0.0503) | (0.0557) |
| $bigL_{t,i} \times lowC_{t-4,i}$ | 0.0421 | –0.141 | –0.0969 | –0.177 |
| | (0.115) | (0.122) | (0.132) | (0.122) |
| New lending, lag 4 $n_{t-4,i}$ | 0.0445*** | 0.0521*** | 0.0388*** | 0.0589*** |
| | (0.0105) | (0.0105) | (0.0104) | (0.0107) |
| Benchm. (county) $nbm^{\mathrm{cty}}_{t,i}$ | 0.0713*** | 0.0671*** | 0.0752*** | 0.0590*** |
| | (0.0114) | (0.0107) | (0.0106) | (0.0114) |
| —, lag 4 | –0.00249 | –0.00519 | –0.00965 | 0.00134 |
| | (0.0109) | (0.0100) | (0.0101) | (0.0109) |
| Benchm. (DE) $nbm^{\mathrm{DE}}_{t,i}$ | 0.790*** | 0.769*** | 0.813*** | 0.805*** |
| | (0.0512) | (0.0483) | (0.0471) | (0.0525) |
| —, lag 4 | 0.0854* | 0.0603 | 0.0760* | 0.0525 |
| | (0.0464) | (0.0434) | (0.0426) | (0.0461) |
| Maturing loans $ml_{t,i}$ | –0.118*** | –0.137*** | –0.135*** | –0.122*** |
| | (0.0346) | (0.0420) | (0.0395) | (0.0347) |
| —, lag 4 | –0.136*** | –0.134*** | –0.122*** | –0.135*** |
| | (0.0282) | (0.0280) | (0.0268) | (0.0275) |
| Fixed effects | bank, time, worst industry | | | |
| Observations | 24041 | 25165 | 25931 | 23206 |
| Adj. $R^2$ | 0.2356 | 0.2397 | 0.2395 | 0.2402 |
| Adj. $R^2$ (within) | 0.0543 | 0.0519 | 0.0546 | 0.0550 |

All variables as in Table 4, except $bigL$ and the interaction with $lowC$. The definition of the loss tail dummy $bigL$ varies in the samples; the biggest 10% of losses are taken from: the history of individual banks (column 1), quarterly samples across banks (column 2), and the pooled sample (column 3). In column 4, a loss is defined to be big if it exceeds a threshold $\exp(\mu_i + \nu_t)$. The components $\mu_i$ and $\nu_t$ are optimized towards equal frequencies of big losses both in the long- and cross-section. Total period: 2002Q4–2020Q4. The year 2007 is excluded for data reasons. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

The subsequent matching algorithm assigns every observation of a treated bank its nearest neighbor, that is, the bank with the nearest score (treatment probability) among all untreated observations. The matching is performed quarter by quarter to achieve a perfect match in the time dimension.[29]

We estimate the score in two versions, one quarterly, which yields a very good match of all variables but creates some variation in the probit coefficients, and the other from the pooled sample across all quarters, which gives a more precise probit estimate but leads to an imperfect balance of moments between treatment and control sample. Since 10% of the observations are treated by design, selecting a single nearest neighbor leaves us with only 20% of the initial sample. We therefore test alternative selections of three and five nearest neighbors to achieve a balance between matching quality and coverage.

Table 10 contrasts the base case (column 1) with estimation results when the score estimation and number of nearest neighbors vary. If an untreated observation has been multiply selected as

the business model. These bank FEs only make sense if the treatment share varies in the cross section, which is not the case in the baseline specification.

[29] Selecting nearest neighbors from a pooled sample would raise endogeneity issues. For instance, a matching pair could consist of close competitors observed at times with an offset of say, one or two years. The decisions of the bank observed earlier could have causal impact on the other bank observed later.

Table 10: Propensity score matching

| Dependent: $n_{t,i}$ | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Sample, matching: | Full | PSM, quarterly scores | | | PSM, joint score | | |
| # nearest neighbors: | | 1 | 3 | 5 | 1 | 3 | 5 |
| $bigL_{t,i}$ | –0.255*** | –0.142** | –0.189*** | –0.172*** | –0.167** | –0.144*** | –0.155*** |
| | (0.0359) | (0.0697) | (0.0560) | (0.0501) | (0.0738) | (0.0524) | (0.0499) |
| $lowC_{t-4,i}$ | –0.240*** | –0.390* | –0.524*** | –0.495*** | –0.178 | –0.165 | –0.263*** |
| | (0.0551) | (0.231) | (0.132) | (0.107) | (0.195) | (0.121) | (0.0912) |
| $bigL_{t,i} \times lowC_{t-4,i}$ | 0.0421 | 0.448* | 0.570*** | 0.384* | 0.441* | 0.246 | 0.298* |
| | (0.115) | (0.268) | (0.202) | (0.202) | (0.263) | (0.194) | (0.177) |
| ... | | | | | | | |
| Observations | 24041 | 2384 | 5079 | 7720 | 2605 | 5562 | 8431 |
| Adj. $R^2$ | 0.2356 | 0.3272 | 0.3819 | 0.4034 | 0.2816 | 0.3190 | 0.3499 |
| Adj. $R^2$ (within) | 0.0543 | 0.0603 | 0.0508 | 0.0386 | 0.0428 | 0.0429 | 0.0529 |

All variables as in Table 3. Every treated observation ($bigL = 1$) is assigned 1, 3, or 5 untreated observation(s) from the same quarter, based on the propensity score being most similar to that of the treated observation. The score predicts the probability of a big loss from lagged covariates: new lending (5Q and 8Q lag); new lending of benchmark banks (4Q); maturing loans (4Q), log TA (4Q), regulatory T1-capital (4Q, centered at quarterly medians); FEs for bank type and the industry that recorded the loss. In columns 2–4, propensity score estimation and matching is done for each quarter separately. The samples in columns 5–7 are also subject to quarterly matching but based on a joint propensity score estimated from all quarters at once. An untreated observation multiply selected as closest neighbor is given an according weight in the panel regression. Control variables left out in the table are: lagged new lending (4 quarters), new lending of county specific and nationwide benchmark bank including their lags, share of maturing loans, furthermore FEs for bank, time, and the industry wst $(t, i)$ where the quarter's largest loss has occurred. Total period: 2002Q4–2020Q4. The year 2007 is excluded for data reasons. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

closest neighbor in PSM, we give it an according weight in the panel regression. PSM reduces the main coefficient in size but still preserves the baseline result. The same holds when we apply PSM to the other definitions of *bigL* (quarterly, pooled, balanced). Altogether, we consider this robustness test particularly tough and, consequently, its outcome to be the most important confirmation of the main result.

### Dynamic lending standards

The last test controls for temporary credit risk taking only to the extent of its co-movement with observable variables. We now care about the other, unobserved part by means of a Monte-Carlo simulation.

Our main argument against a substantial effect of temporarily low lending standards is the relative inert portfolio composition as a consequence of a limited loan turnover. We see in our data that German corporate loans have a fairly long time to maturity such that there is not very often an opportunity to apply bad lending standards.

We simulate a data consistent exogenous random turnover of loans on which the manager has no impact. If, however, a loan matures, the manager has an extreme leeway materializing in a choice for the new loan's PD of either 1% or 10%, which is indeed a high default risk. In the simulated course of time, the portfolio composition drifts between a better or worse portfolio and generates losses from which we derive big-loss events just as in our empirical setup.

The simulated data generating process can be used to estimate how well a bank manager could predict a future big loss based on her knowledge of current and past lending standards and, in addition, of the current expected portfolio loss. We describe the model in detail in Appendix G; Table 16 reports the $R^2$ from attempts to regress the simulated *bigL* on lending standards and the dynamic expected portfolio loss under varying conditions.

As the $R^2$ does not exceed 1.5% in any of the configurations, we conclude that a bank manager has basically no chance to predict a big loss from her temporary risk choices, not to mention a static choice which can have no effect from the outset. Consequently, a big loss comes as a surprise to the manager to the largest part, and any endogenous potential link between temporary risk taking and later lending decisions is unable to bias the key sensitivity, that is, the coefficient of *bigL*.

**Systematic credit risk factors**

Even though we are confident that the big losses used in our regression are mainly idiosyncratic (not least because they are made in a weakly diversified subportfolio, as outlined in Section 4.2), of course they also have systematic components. We have to test the impact of these components for two reasons.

First, losses in the same industry incurred by different banks may be linked by (intra-sector) risk factors. The big loss in an industry subportfolio of one bank can then increase the probability of big losses of other banks' loans to the same industry. Those banks that actually suffer a big loss tend to reduce their lending to the other industries just like the bank of interest, and so will the benchmark bank, as a function of the other banks. There is consequently a closer link between new lending of a bank and its benchmark bank than the one established by credit demand only. This link, in turn, may bias the coefficient of *bigL* downwards because the benchmark bank's new lending captures more of the variation than the part actually attributable to common credit demand.

Second, systematic credit risk factors common to different industries may create a positive correlation between simultaneous large losses. As our empirical strategy binds us to the largest loss in a single industry, we might miss to account for the effect of the, say, second largest and assign its lending impact to the largest one. Supposing a strong correlation between single-industry losses, this "misallocation" of the effect may lead to an overestimate.

In order to test the potential impact of systematic credit risk factors on our results, we repeat the estimates using the idiosyncratic component of a loss. To this end, we subtract the nationwide average of value changes in a single industry from the individual value change, which removes both intra- and inter-sector factors reasonably well[30]:

$$c_{t,i,j}^{\text{idio}} \equiv c_{t,i,j} - TA_{t,i} \frac{\sum_k c_{t,k,j}}{\sum_k TA_{t,k}}. \tag{14}$$

This idiosyncratic component replaces the original value change in (1), the definition of $L_{t,i}$.

Selecting big losses from this idiosyncratic loss version reduces the main coefficient by 20% (column 2 of Table 19 in Appendix H), which points to a role for systematic factors but also seems to confirm once again that big losses are predominantly idiosyncratic by nature. Provided that our baseline estimate captures all relevant factors, the potential downward bias created by inter-sector factors seems to be dominated by the mentioned upward bias caused by intra-sector factors.

The figures are generally in line with Memmel et al. (2015) who find systematic factors to explain around 8% of the variation of credit losses in the same data as used by us.

[30] Memmel et al. (2015) use the same data as in this paper to regress credit portfolio losses on nationwide loss averages. The estimated coefficients of the latter are between 0.7 and 1.2 such that residuals from these estimates are quite similar to the modified losses used here, which correspond to residuals obtained from an estimate with coefficient 1.

### Autocorrelation in losses

Autocorrelation in the time series of $L_{t,i}$ should not play a major role as it would otherwise jeopardize our identification strategy. There is, however, substantial seasonality in the losses because many banks tend to revise their loans more intensively before the annual statement. When regressing $L_{t,i}$ on its lags (from 1 to 8 quarters, including the same FEs as in the main estimate), we consistently find a significantly positive coefficient for the fourth (one-year) lag but none for the lags 1–3.

We are not concerned about this autocorrelation because it probably does not mean more than the ability to predict the time of the next annual statement from the data. Nevertheless we test its impact by replacing the original value changes by residuals of the regression

$$\log L_{t,i} = \alpha_i + \beta_1 \log L_{t-4,i} + \varepsilon_{t,i}\,.$$

Comparing columns 1 and 3 of Table 19, the main coefficient changes from –0.255 to –0.145. The sensitivity reduction is not surprising because the residuals are basically free from seasonality and so dilute the prominent role of the first quarter, similar to the selection of big losses from quarterly samples.

### Severity of losses and low capital endowment

First, we test the sensitivity to the severity of big losses by varying the probability of the loss tail. The lower it is, the bigger the losses and hence the potential effect, albeit at the cost of events included. In Panel A of Table 11, we find the results to be robust against a variation in the loss tail probability between 2% and 40%. That the coefficient of determination $R^2$ reaches its maximum somewhere in the vicinity of 15% roughly corresponds with the shape of $n_{t,i}$ in Figure 1. Similar to the preceding tests, neither zooming into the tail (columns 1–4) nor out of it (columns 6–9) has an effect on the interaction term. It remains insignificant.

Varying the tail probability of the capital ratio (Panel B) has an effect that confirms our expectations pretty exactly since the coefficient of *lowC* tends to be larger, the deeper we go into the lower distribution tail of the capital ratio.

### Time horizon

The next test concerns the horizon over which the bank is measured to adapt its lending business. Column 1 of Table 12 suggests that one quarter includes only a disproportionately small part of the reaction to a severe loss. If the horizon is extended from four to eight quarters (column 3), the quantitative lending effect of *bigL* rises by 20%, which documents that reactions are not fully completed after one year. The impact of low capital shows the same pattern, while the interaction term remains insignificant, as in almost all preceding estimates.

### Fixed effects

In Table 13, we check whether our main result is sensitive to the introduction of certain fixed effects; they should not absorb too much of the explanatory power of *bigL*.

Altogether, the impact of big losses is quite stable. From columns 3, 4, and 7, where bank FEs are omitted, we conclude that bank FEs are essential to capture the role of low capital correctly.

Table 11: Varying the tail probability of losses and capital ratios

| Dependent: $n_{t,i}$ Tail probability (%): | (1) 2 | (2) 4 | (3) 6 | (4) 8 | (5) 10 | (6) 15 | (7) 20 | (8) 30 | (9) 40 |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Varying loss tail probability* | | | | | | | | | |
| $bigL_{t,i}$ | –0.258** | –0.286*** | –0.263*** | –0.266*** | –0.255*** | –0.176*** | –0.152*** | –0.126*** | –0.134*** |
| | (0.101) | (0.0618) | (0.0471) | (0.0402) | (0.0359) | (0.0313) | (0.0301) | (0.0337) | (0.0449) |
| $lowC_{t-4,i}$ | –0.248*** | –0.221*** | –0.206*** | –0.185*** | –0.240*** | –0.202*** | –0.249*** | –0.142 | –0.0925 |
| | (0.0457) | (0.0476) | (0.0497) | (0.0523) | (0.0551) | (0.0627) | (0.0734) | (0.100) | (0.141) |
| $bigL_{t,i} \times lowC_{t-4,i}$ | 0.119 | 0.0415 | 0.0818 | –0.00214 | 0.0421 | –0.0487 | 0.00180 | –0.172 | –0.197 |
| | (0.299) | (0.215) | (0.151) | (0.131) | (0.115) | (0.105) | (0.0997) | (0.111) | (0.146) |
| ... | ... | ... | ... | ... | ... | ... | ... | ... | ... |
| Observations | 30318 | 28743 | 27005 | 25555 | 24041 | 20876 | 18479 | 16088 | 16649 |
| Adj. $R^2$ | 0.2272 | 0.2315 | 0.2338 | 0.2369 | 0.2356 | 0.2423 | 0.2471 | 0.2373 | 0.2190 |
| Adj. $R^2$ (within) | 0.0487 | 0.0516 | 0.0543 | 0.0550 | 0.0543 | 0.0543 | 0.0580 | 0.0516 | 0.0518 |
| *Panel B: Varying tail probability of capital ratio* | | | | | | | | | |
| $bigL_{t,i}$ | –0.243*** | –0.246*** | –0.258*** | –0.260*** | –0.255*** | –0.260*** | –0.241*** | –0.246*** | –0.263*** |
| | (0.0351) | (0.0352) | (0.0353) | (0.0355) | (0.0359) | (0.0365) | (0.0371) | (0.0397) | (0.0426) |
| $lowC_{t-4,i}$ | –0.0383 | –0.304*** | –0.328*** | –0.272*** | –0.240*** | –0.284*** | –0.237*** | –0.236*** | –0.226*** |
| | (0.132) | (0.0885) | (0.0733) | (0.0619) | (0.0551) | (0.0438) | (0.0391) | (0.0343) | (0.0327) |
| $bigL_{t,i} \times lowC_{t-4,i}$ | –0.404 | –0.0837 | 0.121 | 0.112 | 0.0421 | 0.0578 | –0.0514 | –0.00791 | 0.0323 |
| | (0.254) | (0.188) | (0.158) | (0.133) | (0.115) | (0.0947) | (0.0840) | (0.0702) | (0.0648) |
| ... | ... | ... | ... | ... | ... | ... | ... | ... | ... |
| Observations | 24041 | 24041 | 24041 | 24041 | 24041 | 24041 | 24041 | 24041 | 24041 |
| Adj. $R^2$ | 0.2348 | 0.2355 | 0.2358 | 0.2357 | 0.2356 | 0.2365 | 0.2364 | 0.2367 | 0.2365 |
| Adj. $R^2$ (within) | 0.0533 | 0.0541 | 0.0544 | 0.0543 | 0.0543 | 0.0554 | 0.0552 | 0.0555 | 0.0554 |

All variables and period as in Table 3, with the following exceptions: In Panel A, $bigL_{t,i}$ is based on a varying probability of the loss tail (row 2, in %). In Panel B, the dummy $lowC_{t-4,i}$ is based on a varying tail probability of the Tier-1 capital ratio for quarterly samples. All estimates include standard controls, which are: lagged new lending (4 quarters), new lending of county specific and nationwide benchmark bank including their lags, share of maturing loans, furthermore FEs for banks, time, and the industry wst $(t, i)$ where a quarter's largest loss has occurred. Column 5 is identical to the base case in Table 3, column 1. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

Table 12: Different time horizons for new lending business

| Dependent: $n_{t,i}$ <br> Horizon $h$ in quarters: | (1) <br> 1 | (2) <br> 4 | (3) <br> 8 |
|---|---|---|---|
| Big loss $bigL_{t,i}$ | –0.0588*** | –0.255*** | –0.306*** |
| | (0.0165) | (0.0359) | (0.0829) |
| Low capital $lowC_{t-4,i}$ | –0.0489* | –0.240*** | –0.387*** |
| | (0.0251) | (0.0551) | (0.120) |
| $bigL_{t,i} \times lowC_{t-4,i}$ | 0.00298 | 0.0421 | –0.419 |
| | (0.0552) | (0.115) | (0.342) |
| New lending, lag $h$ $n_{t-h,i}$ | –0.0117 | 0.0445*** | –0.0627*** |
| | (0.0113) | (0.0105) | (0.0152) |
| Maturing loans $ml_{t,i}$ | –0.0503*** | –0.118*** | –0.363*** |
| | (0.0181) | (0.0346) | (0.0856) |
| —, lag 4 | –0.0199 | –0.136*** | –0.220*** |
| | (0.0172) | (0.0282) | (0.0572) |
| Benchm. (county) $nbm_{t,i}^{\text{cty}}$ | 0.0339*** | 0.0713*** | 0.113*** |
| | (0.00988) | (0.0114) | (0.0175) |
| —, lag $h$ | 0.00641 | –0.00249 | –0.0276 |
| | (0.00910) | (0.0109) | (0.0182) |
| Benchm. (DE) $nbm_{t,i}^{\text{DE}}$ | 0.313*** | 0.790*** | 1.202*** |
| | (0.0511) | (0.0512) | (0.0589) |
| —, lag $h$ | –0.00572 | 0.0854* | 0.309*** |
| | (0.0501) | (0.0464) | (0.0583) |
| Fixed effects | bank, time, worst industry | | |
| Observations | 24961 | 24041 | 16453 |
| Adj. $R^2$ | 0.1122 | 0.2356 | 0.3830 |
| Adj. $R^2$ (within) | 0.0124 | 0.0543 | 0.0997 |

All variables and period as in Table 4, except that figures for new lending and the share of maturing loans are calculated over a varying horizon of $h = 1, 4, 8$ quarters. Column 2 is the base case. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

Table 13: Varying fixed effects

| Dependent: $n_{t,i}$ | (1) <br> Base case | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| $bigL_{t,i}$ | –0.255*** | –0.184*** | –0.232*** | –0.155*** | –0.268*** | –0.169*** | –0.220*** |
| | (0.0359) | (0.0342) | (0.0351) | (0.0338) | (0.0358) | (0.0344) | (0.0354) |
| $lowC_{t-4,i}$ | –0.240*** | –0.230*** | –0.0887* | –0.0790* | –0.241*** | –0.230*** | –0.0898** |
| | (0.0551) | (0.0562) | (0.0453) | (0.0459) | (0.0551) | (0.0561) | (0.0453) |
| $bigL_{t,i} \times lowC_{t-4,i}$ | 0.0421 | 0.0218 | –0.0312 | –0.0379 | 0.0438 | 0.0192 | –0.0294 |
| | (0.115) | (0.118) | (0.114) | (0.118) | (0.115) | (0.118) | (0.114) |
| ... | ... | ... | ... | ... | ... | ... | ... |
| Bank FEs | yes | yes | | | yes | yes | |
| Time FEs | yes | | yes | | yes | | yes |
| FEs of worst indu. | yes | | | yes | | yes | yes |
| Observations | 24041 | 24041 | 24041 | 24041 | 24041 | 24041 | 24041 |
| Adj. $R^2$ | 0.2356 | 0.2030 | 0.1382 | 0.1071 | 0.2347 | 0.2045 | 0.1392 |
| Adj. $R^2$ (within) | 0.0543 | 0.0553 | 0.108 | 0.105 | 0.0556 | 0.0545 | 0.107 |

All variables as in Table 3 (base case), except fixed effects. All estimates include standard controls, which are: lagged new lending (4 quarters), new lending of county specific and nationwide benchmark bank including their lags, share of maturing loans. Total period: 2002Q4–2020Q4. The year 2007 is excluded for data reasons. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

### The definition of new lending

Next, we test an alternative to the definition of new business, which excludes value changes, as opposed to the formula (3) used so far:

$$n_{t,i}^{\mathrm{alt}} \equiv n_{t,i} - \frac{1}{TA_{t,i}} \sum_{k=1}^{4} \sum_{j \neq \mathrm{wst}(t,i)} c_{t+k,i,j}. \tag{15}$$

This definition puts weight on the pure action of bank managers, that is the pure loan contracting minus expiring loans. Column 4 of Table 19 in Appendix H shows that the main regression is almost insensitive to this modification.

### Capital measures

To check whether the key result is sensitive to the measure of capital, we replace the dummy *lowC* with different transformations of regulatory Tier-1 capital. Column 1 of Table 14 recaps the base case, in which we had constructed quarter-specific samples of regulatory Tier-1 ratios (T1 capital to risk-weighted assets) and used the dummy for the lowest 10% of ratios (in each quarter) as regressor.

In column 2, we go one step back and make direct use of Tier-1 ratios, from which we subtract the quarter-specific median ratio in order to remove the general upward trend of capital (which motivated us to rely on quarterly samples of capital in the base case).

In column 3, we abandon the de-trending and account for one of the reasons for the upward trend in capital instead, which is the stepwise increase in the regulatory minimum Tier-1 ratio from 4.5% to 6% within the observation period. *CapBuffer* is the difference between the actual and the minimum Tier-1 ratio. The closer it gets to zero, the higher the risk that the bank is placed into supervisory conservatorship, which suggests this buffer as a fairly natural measure for the pressure to deleverage.

The buffer ignores the fact that banks take different levels of asset risk. The *distance to trouble*[31] (DtT) used in column 4 brings buffer and risk to one scale by dividing the buffer through its standard deviation:

$$DtT_{t,i} \quad \equiv \quad \frac{CapBuffer_{t,i}}{\mathrm{std}\left(\Delta CapBuffer_{\cdot,i}\right)}.$$

The standard deviation is static and estimated from all quarterly differences for banks with a minimum of 50 observations.

In column 5, we use the dummy for the smallest 10% of DtT values to test for a nonlinearity in the link between $DtT$ and lending cuts. It is possible that banks start to take serious action only if the risk that capital falls under the regulatory minimum is really high.

In column 6, we refine this concept by the *probability of trouble* (PoT), defined as $\Phi\left(-DtT_{t,i}\right)$, where $\Phi$ is the standard normal cdf. The PoT is the probability that the bank falls short of minimum capital in the next quarter under the assumption that $\Delta CapBuffer$ is normally distributed with mean zero. Memmel and Raupach (2010) identified the long-term average of this probability as a key parameter in the dynamics of bank capital ratios. Note that a low DtT corresponds to a high PoT such that we expect opposite signs for the capital measure in columns 4 and 6.

[31] The name draws an analogy to the closely related *distance to default* which, however, measures the distance to *zero* capital rather than to the regulatory minimum. *Trouble* stands for supervisory conservatorship.

Table 14: Varying measures of capital

| Dependent: $n_{t,i}$ (%) | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Measure of capital | *lowC* | *CapRatio* | *CapBuffer* | *DtT* | *LowDtT* | *PoT* |
| Variable type | Dummy | Contin. | Contin. | Contin. | Dummy | Contin. |
| *bigL*, | –0.255*** | –0.248*** | –0.251*** | –0.228*** | –0.225*** | –0.254*** |
| | (0.0359) | (0.0349) | (0.0361) | (0.0362) | (0.0370) | (0.0357) |
| Capital measure (lag 4) | –0.240*** | 0.0556*** | 0.0553*** | 0.112*** | –0.0442 | 0.219 |
| | (0.0551) | (0.00591) | (0.00588) | (0.0100) | (0.0705) | (0.696) |
| Interaction with *bigL* | 0.0421 | –0.0100 | –0.00550 | 0.0213 | –0.286** | –2.226** |
| | (0.115) | (0.0114) | (0.00945) | (0.0136) | (0.117) | (0.912) |
| ... | ... | ... | ... | ... | ... | ... |
| Observations | 24041 | 24041 | 24041 | 23076 | 23076 | 23076 |
| $R^2$ | 0.2356 | 0.2391 | 0.2391 | 0.2395 | 0.2342 | 0.2342 |
| $R^2_{\text{within}}$ | 0.0543 | 0.0585 | 0.0585 | 0.0600 | 0.0534 | 0.0534 |

All variables and period as in the base case (Table 3 column 1), except the capital measure and the interaction term. Column 1 is the base case. *CapRatio* is the regulatory Tier-1 ratio, net of quarter-specific median values. *CapBuffer* is the Tier-1 ratio minus the regulatory minimum ratio legally effective at the time. *DtT* (for "distance to trouble") is *CapBuffer* divided by the standard deviation of quarterly changes in *CapRatio*. *LowDtT* is a dummy for the 10% lowest *DtT* realizations in the pooled sample. *PoT* is the probability that *CapBuffer* falls below zero in the next quarter, assuming a $N(0, \sigma)$ distribution of changes. All estimates include standard controls, which are: lagged new lending (4 quarters), new lending of county specific and nationwide benchmark bank including their lags, share of maturing loans, furthermore FEs for banks, time, and the industry wst $(t, i)$ where a quarter's largest loss has occurred. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

Table 14 shows that the sensitivity of new lending to big losses is basically independent of the capital measure. Sensitivities to capital have consistent signs, and neither of the refinements brings about a significant interaction term, except columns (5) and (6) where the interaction becomes significant while that of the capital measure gets insignificant. This finding is hard to interpret since *LowDtD* and *PoT* are transforms of *DtD* which follows the dominant pattern that capital matters for lending but not the interaction with *bigL*.

## 6 Summary and conclusion

Let us first compare the base case and various robustness tests in one graph. In Section 5.1 we have already transformed the baseline coefficients of *bigL* into a linear effect of 1.79 euro less new lending for each euro lost in a substantial credit event. We do this now for various specifications tested in the last section and plot the linear effects in Figure 3.

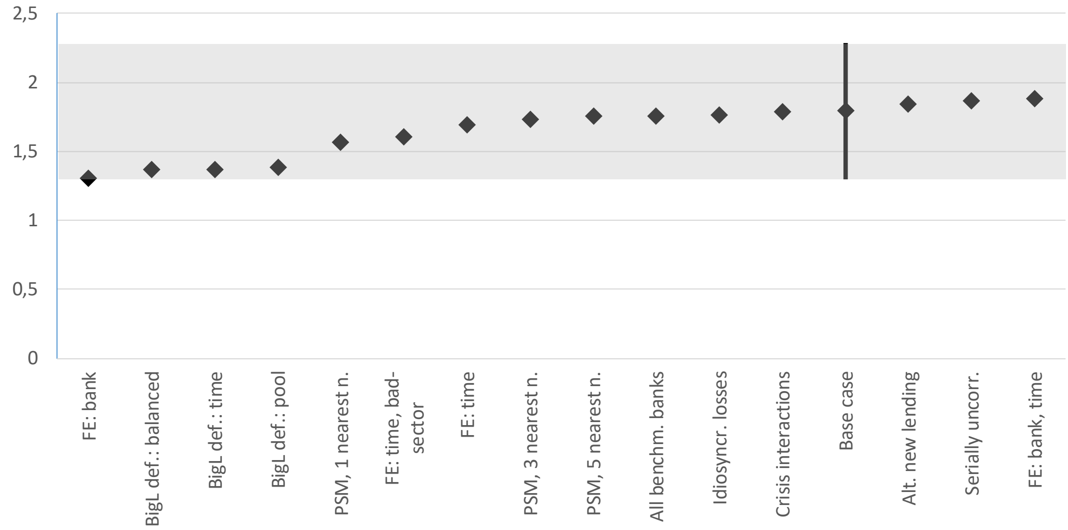
All point estimates are covered by the 95% confidence interval $[1.30, 2.29]$ spanned by the baseline estimate. It seems that model details do not create more uncertainty about the size of the effect than the data driven estimation error. It is fair to say that the key sensitivities of new corporate lending – to big losses and to a low level of regulatory capital – are robust. By contrast, we do not find evidence that their interaction – when big losses combine with low capital – plays a role.

As model uncertainty spans a slightly narrower range of plausible values than the estimation error in the base case, our key conclusion refers to the latter:

*A bank reacts to each euro lost in a severe credit event by a lending reduction that most likely ranges between 1.30 and 2.29 euros.*

This reduction is moderate, compared to values found in the literature (Table 1), but decidedly below the effect derived under a constant-leverage assumption: If banks were using

Figure 3: Linearized lending reduction across different specifications




This figure shows how the linearized lending reduction (in euros) after a loss of one euro depends on the specification. Diamonds are point estimates of the linearized effect. The shaded area corresponds to the 95% confidence interval of the estimated lending reduction in the base case, as spanned by the vertical bar. Specifications: "FE" stands for "fixed effects" included (the base case is FEs for each bank, each quarter and each industry wst $(t, i)$ where the loss occurred). "PSM" stands for "propensity score matching" with 1, 3, or 5 nearest neighbors included in the control sample, "BigL def." for the definition of big losses, that is, whether they are defined by bank (base case), by each point in time ("time"), not conditioned at all ("pool"), or taking the "balanced" approach introduced in (13). "Serially uncorr." means that the losses have undergone a serial orthogonalization before *bigL* is sampled. "Idiosyncr. losses" means that nationwide systematic credit risk factors have been removed. "Crisis interactions" stands for the inclusion of a dummy for the period 2008–2012 and corresponding interaction terms. "Alt. new lending" defines new lending as position changes net of value changes; see (15).

corporate loans as the only means to keep their capital ratios strictly constant at, say, 10%, they would reduce lending by 10 euro for every euro lost.

We find no evidence that other banks step in to make up for the lower credit supply of those banks that have suffered a large credit loss. Big losses trigger remarkable changes in retail exposures but hardly any in securities positions despite their better liquidity. An isolated management of securities and loan portfolios can possibly explain this independence.

Finally, our new method to construct bespoke competitors for each bank reconciles a granular control for demand with estimates at bank level. This level avoids the noise inherent in estimates of disaggregate relative changes in lending and takes the right perspective in an analysis of bank wide portfolio adjustments. It is beneficial to include more than one such competitor to capture both local and nationwide factors of credit demand.

In the paper, we mostly deal with the questions of credit growth, but not much with the question of the causes. Future research could investigate how bank and firm characteristics, the stance of monetary and macroprudential policy and market conditions affect credit growth.

# References


Abadie, A., A. Diamond, and J. Hainmueller (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association 105(490)*, 493–505.

Acharya, V. V., T. Eisert, C. Eufinger, and C. Hirsch (2018). Real effects of the sovereign debt crisis in Europe: Evidence from syndicated loans. *The Review of Financial Studies 31*(8), 2855–2896.

Admati, A. R., P. M. DeMarzo, M. Hellwig, and P. Pfleiderer (2010). Fallacies, irrelevant facts, and myths in the discussion of capital regulation: Why bank equity is not expensive. Technical report.

Aiyar, S., C. W. Calomiris, J. Hooley, Y. Korniyenko, and T. Wieladek (2014). The international transmission of bank capital requirements: Evidence from the UK. *Journal of Financial Economics 113*(3), 368–382.

Aiyar, S., C. W. Calomiris, and T. Wieladek (2016). How does credit supply respond to monetary policy and bank minimum capital requirements? *European Economic Review 82*, 142–165.

Berrospide, J. M. and R. M. Edge (2010). The effects of bank capital on lending: What do we know, and what does it mean? *International Journal of Central Banking 6(4)*, 5–54.

Brei, M., L. Gambacorta, and G. von Peter (2013). Rescue packages and bank lending. *Journal of Banking & Finance 37*(2), 490–505.

Carlson, M., H. Shan, and M. Warusawitharana (2013). Capital ratios and bank lending: A matched bank approach. *Journal of Financial Intermediation 22*(4), 663–687.

Chernih, A., L. Henrard, and S. Vanduffel (2010). Reconciling credit correlations. *Journal of Risk Model Validation 4*(2).

Dasgupta, K. and B. J. Mason (2020). The effect of interest rate caps on bankruptcy: Synthetic control evidence from recent payday lending bans. *Journal of Banking and Finance 119*, 105917.

Davydenko, S. A. and J. R. Franks (2008). Do bankruptcy codes matter? A study of defaults in France, Germany, and the UK. *The Journal of Finance 63*(2), 565–608.

De Haas, R. and N. van Horen (2013). Running for the exit? international bank lending during a financial crisis. *The Review of Financial Studies 26*(1), 244–285.

De Jonghe, O., H. Dewachter, K. Mulier, S. Ongena, and G. Schepens (2020). Some borrowers are more equal than others: Bank funding shocks and credit reallocation. *Review of Finance 24*(1), 1–43.

De Jonghe, O., H. Dewachter, and S. Ongena (2020). Bank capital (requirements) and credit supply: Evidence from Pillar 2 decisions. *Corporate Finance 60*, 101518.

Degryse, H., O. De Jonghe, S. Jakovljević, K. Mulier, and G. Schepens (2019). Identifying credit supply shocks with bank-firm data: Methods and applications. *Journal of Financial Intermediation 40*, 100813.

Deutsche Bundesbank (2009). Guidelines on the borrowers statistics. January 2009.

Deutsche Bundesbank (2012). Monthly Report. 12/2012.

Deutsche Bundesbank (2014). Monthly Report. 03/2014.

Deutsche Bundesbank (2020). Capital market statistics. 02/2020.

Gambacorta, L. and H. S. Shin (2018). Why bank capital matters for monetary policy. *Journal of Financial Intermediation 35*, 17–29.

Gropp, R., T. Mosk, S. Ongena, and C. Wix (2018). Banks response to higher capital requirements: Evidence from a quasi-natural experiment. *The Review of Financial Studies 32*(1), 266–299.

Hancock, D. and J. A. Wilcox (1993). Has there been a Capital Crunch in banking? The effects on bank lending of real estate market conditions and bank capital shortfalls. *Journal of Housing Economics 3(1)*, 31–50.

Hancock, D. and J. A. Wilcox (1994). Bank capital and credit crunch: The roles of risk-weighted and unweighted capital regulations. *Journal of the American Real Estate and Urban Economics Association 22(1)*, 59–94.

Imbierowicz, B., J. Kragh, and J. Rangvid (2018). Time-varying capital requirements and disclosure rules: Effects on capitalization and lending decisions. *Journal of Money, Credit and Banking 50*(4), 573–602.

Ivashina, V. and D. Scharfstein (2010). Bank lending during the financial crisis of 2008. *Journal of Financial economics 97*(3), 319–338.

Jiménez, G., S. Ongena, and J.-L. Peydró (2014). Hazardous time for monetary policy: What do twenty-three million bank loans say about the effects of monetary policy on credit risk-taking? *Econometrica 82*(2), 463–505.

Jiménez, G., S. Ongena, J.-L. Peydró, and J. Saurina (2012). Credit supply and monetary policy: Identifying the bank balance-sheet channel with loan applications. *American Economic Review 102*(5), 2301–26.

Khwaja, A. I. and A. Mian (2008). Tracing the impact of bank liquidity shocks: Evidence from an emerging market. *American Economic Review 98*(4), 1413–1442.

Kim, D. and W. Sohn (2017). The effect of bank capital on lending: Does liquidity matter? *Journal of Banking and Finance 77*, 95–107.

Kok, C. and G. Schepens (2013). Bank reactions after capital shortfalls. Working Paper Research, National Bank of Belgium.

Memmel, C., Y. Gündüz, and P. Raupach (2015). The common drivers of default risk. *Journal of Financial Stability 16*, 232–247.

Memmel, C. and P. Raupach (2010). How do banks adjust their capital ratios? *Journal of Financial Intermediation 19*, 509–528.

Peek, J. and E. S. Rosengren (1997). The international transmission of financial shocks: The case of Japan. *The American Economic Review*, 495–505.

Popov, A. and N. Van Horen (2014). Exporting sovereign stress: Evidence from syndicated bank lending during the euro area sovereign debt crisis. *Review of Finance 19*(5), 1825–1866.

Puri, M., J. Rocholl, and S. Steffen (2011). Global retail lending in the aftermath of the us financial crisis: Distinguishing between supply and demand effects. *Journal of Financial Economics 100*(3), 556–578.

Shiryaev, A. N. (1995). *Probability (2nd ed.).* Springer-Verlag New York, Inc.

Tölö, E. and P. Miettinen (2018). How do shocks to bank capital affect lending and growth? *Bank of Finland Research Discussion Paper* (25).

# Appendix

## A Regional distribution of exposures

This appendix supplements Section 4.4 with details of how we calculate the regional exposures $ex_{t,i,j,r}$ used in Section 3.1.

We denote the exposure of bank $i$ to industry $j$ at time $t$ as reported in the borrower statistics (BS) by $ex^{\mathrm{BS}}_{t,i,j}$. This is an aggregate over the three maturity bands. Furthermore, $ex^{\mathrm{CR}}_{t,i,j,r}$ is the domestic part of the exposure of bank $i$ to industry[32] $j$ in region $r$ (a county) at time $t$, obtained from the German credit register (CR). This number is an aggregate over individual borrowers grouped into the same industry/region segment.

Summing CR exposures over regions, the typical relationship between CR and BS exposures, especially for small banks and banks more active in retail and SME lending, is:

$$\sum_r ex^{\mathrm{CR}}_{t,i,j,r} \quad < \quad ex^{\mathrm{BS}}_{t,i,j}. \tag{16}$$

The converse can happen as well because the definition of credit is more general in the credit register.[33] As we want to keep the figures as close to the BS as possible (because we get loss data from it), we use credit register data only to approximate the regional distribution of credit. To this end, we first downsize CR exposures, if necessary, to the amount explained by BS data:

$$ex^{\mathrm{CR}*}_{t,i,j,r} \quad \equiv \quad \min\left(1, \frac{ex^{\mathrm{BS}}_{t,i,j}}{\sum_r ex^{\mathrm{CR}}_{t,i,j,r}}\right) ex^{\mathrm{CR}}_{t,i,j,r}. \tag{17}$$

This modification has no effect under the typical condition (16). What is left after subtracting credit register exposures is called "pure BS" exposure:

$$ex^{\mathrm{BS,pure}}_{t,i,j} \quad \equiv \quad ex^{\mathrm{BS}}_{t,i,j} - \sum_r ex^{\mathrm{CR}*}_{t,i,j,r}.$$

This is the part of the exposure about which we have no regional information. We assume these loans to completely originate from the region of the bank's head office (given by the function $\mathrm{seat}\,(i)$), which makes more sense the smaller the bank.

The final exposure of bank $i$ in industry $j$ in region $r$ at time $t$ used in Section 3.1 is consequently:

$$ex_{t,i,j,r} \quad \equiv \quad ex^{\mathrm{CR}*}_{t,i,j,r} + \mathrm{I}\,(r = \mathrm{seat}\,(i))\, ex^{\mathrm{BS,pure}}_{t,i,j}, \tag{18}$$

where $\mathrm{I}\,(\ldots)$ is an indicator function. The breakdown guarantees that the initial BS exposure is preserved: $\sum_r ex_{t,i,j,r} = ex^{\mathrm{BS}}_{t,i,j}$.

[32] Sectors in the BS are an aggregation of the credit register's sectors. We work with the former throughout the paper.

[33] For instance, the CR counts bonds held by a bank as credit to the bond issuer; similar for CDS protection sold. Neither of the two is reflected in the BS, which is strictly held consistent with banks' balance sheets (bonds are a separate balance sheet position; CDSs are off-balance sheet). CR exposures can also exceed BS exposures if a bank reports a borrower's sector affiliation inconsistently in the BS and CR.

# B Disentangling the shock absorption capacity of the benchmark bank

In this appendix, we argue why the rescaling mechanism is particularly suited for filtering out those parts of demand shocks that are proportional to existing credit exposures. Let us zoom into a certain industry/region segment $(j, r)$ and consider the demand shocks to individual banks. A joint proportional component appears quite natural in the presence of medium- or long-term lending relationships, and the shock might be captured well by the following model:

$$N_{i,j,r}^{\text{demand}} \quad = \quad \eta_{j,r}\, ex_{i,j,r} + \varepsilon_{i,j,r} \qquad \text{(in euros, } t \text{ omitted).}$$

Let us ignore the noise part (and how it should ideally be set up) and focus on $\eta_{j,r}$, the common factor by which loan applicants wish the current exposures to be increased. If loan demand is transformed into supply by a similar mechanism, for instance

$$N_{i,j,r} \quad = \quad \omega_{j,r} N_{i,j,r}^{\text{demand}} + \xi_{i,j,r} = \omega_{j,r}\eta_{j,r}\, ex_{i,j,r} + [\omega_{j,r}\varepsilon_{i,j,r} + \xi_{i,j,r}] \qquad \text{(in euro),}$$

(in which $\omega_{j,r}$ has a positive expectation), the new lending of bank $i$ amounts to:

$$n_{i,j,r} = \frac{N_{i,j,r}}{TA_i} = \omega_{j,r}\eta_{j,r} w_{i,j,r} + \frac{\omega_{j,r}\varepsilon_{i,j,r}\xi_{i,j,r}}{TA_i} \tag{19}$$

with $w_{i,j,r}$ from (4). The new lending of all other banks is, using $w_{t,\neg i,j,r}$ from (5) and the shortcut $TA_{\neg i} \equiv \sum_{k \neq i} TA_k$:

$$n_{\neg i,j,r} = \frac{\sum_{k \neq i} N_{k,j,r}}{TA_{\neg i}} = \omega_{j,r}\eta_{j,r} w_{t,\neg i,j,r} + \frac{\sum_{k \neq i} \omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_{\neg i}}.$$

The product $\omega_{j,r}\eta_{j,r}$ corresponds to $\gamma_{j,r}$ from (9) in the main text. The benchmark new lending is obtained through rescaling this aggregate new lending by $\nu_{i,j,r}$ according to (7):

$$\begin{aligned} nbm_{i,j,r} \quad &= \quad \nu_{i,j,r}\, n_{\neg i,j,r} \\ &= \quad \omega_{j,r}\eta_{j,r} \left(\nu_{i,j,r}\, w_{\neg i,j,r}\right) + \nu_{i,j,r} \frac{\sum_{k \neq i} \omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_{\neg i}}. \\ &= \quad \omega_{j,r}\eta_{j,r} w_{i,j,r} + \nu_{i,j,r} \frac{\sum_{k \neq i} \omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_{\neg i}}. \end{aligned} \tag{20}$$

Comparing (19) and (20), we see that the common proportional factor $\omega_{j,r}\eta_{j,r}$ of the demand shocks in segment $(j, r)$ has the same weight in the new lending of bank $i$ and that of its benchmark bank. The aggregation over sectors and regions, which results in new lending at portfolio level, preserves this congruence:

$$\begin{aligned} n_i \quad &= \quad \left[\sum_r \sum_{j \neq \text{wst}(i)} \omega_{j,r}\eta_{j,r} w_{i,j,r}\right] + \sum_r \sum_{j \neq \text{wst}(i)} \nu_{i,j,r} \frac{\sum_{k \neq i} \omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_i} \\ nbm_i^{reg} \quad &= \quad \left[\sum_r \sum_{j \neq \text{wst}(i)} \omega_{j,r}\eta_{j,r} w_{i,j,r}\right] + \sum_r \sum_{j \neq \text{wst}(i)} \frac{\omega_{j,r}\varepsilon_{i,j,r}\xi_{i,j,r}}{TA_{\neg i}}. \end{aligned}$$

As the bracketed terms are the same, $nbm_i^{reg}$ is able to absorb the proportional component of demand shocks particularly well.

# C Loan turnover

We calculate the approximate *share of maturing loans* from the three bands of maturity that each industry exposure in the borrower statistics is split into; maturity is meant to be *at grant*, such that a loan remains in its category throughout.

Each maturity band is assigned the average share of loans that mature within the following quarter under the assumption of a constant stream of loans with a uniform maturity equal to the interval's midpoint. We therefore assign a maturing rate of 1/2 (per quarter) to the 0–1y band (as we assume each of its loans has a maturity of 2 quarters), 1/12 to the 1–5y band (maturity 12 quarters), and 1/28 to the >5y band (maturity 28 quarters).

Taking $\left(ex_{t,i,j}^{(k)}\right)_{k=1,2,3}$ to be the maturity-specific exposures in an industry, we calculate the quarterly euro amount of maturing loans,

$$ml_{t,i,j} \quad \equiv \quad \frac{1}{2}ex_{t,i,j}^{(1)} + \frac{1}{12}ex_{t,i,j}^{(2)} + \frac{1}{28}ex_{t,i,j}^{(3)},$$

which is then aggregated over four periods and sectors, consistently with the definition of new lending, and normalized by total assets:

$$ml_{t,i} \quad \equiv \quad \frac{1}{TA_{t.i}} \sum_{j \neq \text{wst}(t,i)} \min\left(ex_{t,i,j}, \sum_{s=0}^{3} ml_{t+s,i,j}\right). \tag{21}$$

The minimum operator is necessary because the amount of maturing loans can actually exceed the exposure, for instance if all loans belong to the first maturity band; they would be completely replaced twice a year.

# D Explanatory power

A simplified version of Equation (12) is:

$$n_i = \gamma + \beta\,\mathrm{I}\,(c_i < \delta) + \varepsilon_i \tag{22}$$

with $\text{var}(\varepsilon_i) = \sigma_{\text{bl}}^2$ ("bl" for *bank level*) and $\mathrm{I}\,(c_i < \delta) = bigL_i$ with $\Pr(c_i < \delta) = \alpha$. In the baseline regression, we set $\alpha = 10\%$. At the bank level, the coefficient of determination $R_{\text{bl}}^2$ is

$$R_{\text{bl}}^2 = \frac{\beta^2 \text{var}\left(\mathrm{I}\,(c_i < \delta)\right)}{\beta^2 \text{var}\left(\mathrm{I}\,(c_i < \delta)\right) + \sigma_{\text{bl}}^2}.$$

Aggregating the new lending of all banks and assuming $c_i$ to be perfectly correlated leads to the following relationship (variables without the index $i$):

$$n = \gamma + \beta\,\mathrm{I}\,(c < \delta) + \varepsilon \tag{23}$$

with $\varepsilon \equiv \sum_{i=1}^{N} m_i \varepsilon_i$, where $m_i \equiv ex_i/ex$ is the market share of bank $i$ concerning the credit volume. This holds because we can rewrite (22) as follows:

$$
\begin{aligned}
n &= \sum_{i=1}^{N} \frac{ex_i}{ex} n_i = \sum_{i=1}^{N} \frac{ex_i}{ex} \left(\gamma + \beta \, \mathrm{I}\left(c_i < \delta\right) + \varepsilon_i\right) \\
&= \gamma + \beta \, \mathrm{I}\left(c < \delta\right) + \sum_{i=1}^{N} m_i \varepsilon_i
\end{aligned}
$$

Under the assumption that $c_i$ is perfectly correlated in the cross-section of banks, we obtain:

$$
\mathrm{var}\left(\mathrm{I}\left(c < \delta\right)\right) = \mathrm{var}\left(\mathrm{I}\left(c_i < \delta\right)\right).
$$

By contrast, we assume the bank-individual effect $\varepsilon_i$ to be uncorrelated in the cross-section and obtain:

$$
\mathrm{var}(\varepsilon) = \mathit{HHI} \, \sigma_{\mathrm{bl}}^2
$$

where $\mathit{HHI} = \sum_{i=1}^{N} m_i^2$ is the Hirschman-Herfindahl index of the banks' market shares. Accordingly, the $R^2$ of Equation (23) would be

$$
R^2 = \frac{R_{\mathrm{bl}}^2}{R_{\mathrm{bl}}^2 + \mathit{HHI} \, \left(1 - R_{\mathrm{bl}}^2\right)}. \tag{24}
$$

Hence, the smaller $\mathit{HHI}$ becomes, that is, the less concentrated the banking system is, the closer $R^2$ gets to 1.

# E Transforming the effect of *bigL* into an effect of losses

To compare our estimation results with values in the literature we transform the effect of the key dummy *bigL* into a linear effect (in euros) of the credit loss itself (in euros). Such a transform is justified as the loss is the dummy's only determinant: $bigL_{t,i} = \mathrm{I}\left(L_{t,i} < \delta_i\right)$.[34] In the baseline regression (12), *bigL* occurs at two places:

$$
n_{t,i} = \beta_1 \, bigL_{t,i} + \ldots + \beta_3 \, bigL_{t,i} \times lowC_{t-4,i} + \ldots \tag{25}
$$

The coefficient $\beta_1$ can be seen as the (additional) change in new lending in case $L_{i,t} < \delta_i$, compared to the complement $L_{i,t} \geq \delta_i$. The fact that *bigL* is insensitive to the variation of $L$ within either of these cases suggests to relate the coefficients to the following measure of variation:

$$
\Delta \equiv E\left(L_{i,t} \mid L_{i,t} < \delta_i\right) - E\left(L_{i,t} \mid L_{i,t} \geq \delta_i\right).
$$

So, as for the euro effect of $L$ captured by $\beta_1$ alone we would divide it by $\Delta$. The fraction $\beta_1/\Delta$ is an effect "in euros of euros" because $n_{t,i}$ and $L_{t,i}$ are both normalized by total assets, which cancels out. For $\beta_3$ and the interaction term we have to take into account that $bigL_{t,i}$ has an effect only if $lowC_{t-4,i}$ equals one. The total linearized effect transmitted by both regression terms is then:

$$
\widetilde{\beta} \equiv \frac{1}{\Delta} \left(\beta_1 + \beta_3 \, E\left(lowC_{t-4,i} \mid L_{i,t} < \delta_i\right)\right), \tag{26}
$$

[34] The cutoff point $\delta_i$ is the 10% quantile of the bank specific sample $\{L_{t,i}\}_{t=1,\ldots,T}$.

which can be interpreted as the euro sensitivity of new lending to each euro lost in the "bad" industry.

We derive a confidence interval for $\widetilde{\beta}$ under the assumption that the estimator $[\beta_1, \beta_3]^\top$is asymptotically bivariate normal with covariance matrix $\Sigma$; the variation of other components of $\widetilde{\beta}$ is neglected. With $H \equiv [1/\Delta, E\left(lowC_{t-4,i} \mid L_{i,t} < \delta_i\right)/\Delta]$, the 95% confidence interval is then given by:

$$\widetilde{\beta} \pm 1.96\sqrt{H\Sigma H^\top}.$$

For the extended model that interacts losses and capital with the crisis period we also include $bigL_{t,i} \times Cris_t$ and the triple interaction $bigL_{t,i} \times lowC_{t,i} \times Cris_t$ in the calculation of the total effect.

# F Balancing long- and cross-sectional variation in the frequency of big losses

We define

$$X_{t,i} \equiv \log\left(L_{t,i}\right) \qquad \text{and} \qquad bigL_{t,i} \equiv \mathrm{I}\left(X_{t,i} > \mu_i + \nu_t\right).$$

We set the constraint $E\left(bigL_{t,i}\right) = 10\%$ and calibrate $\mu_i$ and $\nu_t$ such that $E\left(bigL_{t,i} \mid i\right)$ and $E\left(bigL_{t,i} \mid t\right)$ vary as little as possible, which is measured by their standard deviations. We do not perform an exact optimization but employ the following ad-hoc algorithm that terminates at satisfactory values:

1. Start with $\nu_t \equiv 0$ for all $t$. As we have an unbalanced sample, let $\mathscr{G}_i$ be the set of all $t$ for which $X_{t,i}$ is defined and, vice versa, be $\mathscr{H}_t$ the set of all $i$ with a defined $X_{t,i}$.

2. Set
$$\mu_i \equiv \mathrm{Qtl}_{90\%}\left(\{X_{t,i} - \nu_t\}_{t \in \mathscr{G}_i}\right).$$
Then, the inequality $X_{t,i} - \mu_i - \nu_t > 0$ should hold for $\approx 10\%$ of observations in each $\mathscr{G}_i$. There are small-sample deviations from 10% since we have a maximum of 72 quarters.

3. Set
$$\nu_t := \mathrm{Qtl}_{90\%}\left(\{X_{t,i} - \mu_i\}_{i \in \mathscr{H}_t}\right).$$
This makes $X_{t,i} - \mu_i - \nu_t > 0$ hold for pretty exactly 10% of the observations in each $\mathscr{H}_t$, due to the large cross section of more than 1,000 banks.

4. Stop if $\mathrm{std}\left(E\left(bigL_{t,i} \mid i\right)\right)$ and $\mathrm{std}\left(E\left(bigL_{t,i} \mid t\right)\right)$ have not changed since their last measurement after step 3. Otherwise, go to step 2.

To give an idea how well this algorithm works, Table 15 compares how the frequency of big losses varies in the long- and cross-section under the four alternative definitions of *bigL*. Unsurprisingly, the bank specific sampling approach performs best in the cross section and poorly in the long section, and the quarterly sampling in the opposite order; pooled sampling combines the disadvantages of both (but focuses better on the largest losses, which is not reflected here). The balanced approach, however, is almost as good as the best in either dimension.

# G Simulating the impact of dynamic lending standards

The simulation consists of the following building blocks: a model for the portfolio composition in terms of loan size and maturity; a model for the dynamics of lending standards; a credit

Table 15: Variation in the frequency of big losses in the long- and cross-section

| Definition of $bigL$: | $E\left(bigL_{t,i}\right)$ | $\text{std}\left(E\left(bigL_{t,i} \mid i\right)\right)$ | $\text{std}\left(E\left(bigL_{t,i} \mid t\right)\right)$ |
|---|---|---|---|
| Bank specific (base case) | 10.25% | 1.0% | 6.6% |
| Quarterly | 9.94% | 10.2% | 0.0% |
| Pooled | 10.00% | 9.5% | 6.3% |
| Balanced | 10.00% | 1.4% | 0.3% |

portfolio model which generates losses; the calculation of big-loss events; and the evaluation

**The revolving portfolio**

We simulate 500 consecutive observation periods of 72 quarters, as in our data. The composition of loan sizes is static: in each of 23 identical industry subportfolios the bank holds 50 loans whose size has a constant decay factor of 0.8 when sorted in descending order. This factor is calibrated to the upper tail of a loan-size distribution obtained from individual loan data. In formal terms, the bank has $N = 23 \times 50$ loans numbered $(j, k)$ with loan volume $size_{j,k} = 0.8^k$.

At grant, loans have random maturities, drawn independently of each other and of loan size from a uniform distribution between 1 and 32 quarters. The distribution is consistent with the breakdown of our data in three maturity brackets (see Appendix C); the upper bound of 32 quarters is a rather low value since the bracket for the longest maturities covers all values *in excess* of 20 quarters and has no upper limit; see Appendix C. As a result, each of the 50 loans in an industry is a fixed-size investment in a consecutive stream of loans with random maturities. Each time a loan is replaced, the bank manager has a chance to choose the new loan's default risk according to the lending standards at the time.

**Dynamic lending standards**

Lending standards follow a stationary two-state Markov process. A state of the standards is not explicitly modeled but proxied by a default probability preferred for every new loan made at a point in time. So, formally, lending standards follow a quarterly Markov process $PD_t$ that takes values in $\{\text{lo} = 0.01, \text{hi} = 0.1\}$ which are understood as annual default probabilities. The process $PD_t$ affects the portfolio risk via the simple mechanism that every loan that matures at $t$ is replaced by a loan with an annual default probability equal to $PD_t$.

The transition matrix is implicitly parameterized by the stationary state probability $\mathbf{P}\left(PD_t = \text{hi}\right)$ fixed at 20% and the mean sojourn time in the high-risk state. We vary the mean sojourn time as it is a measure for the time the bank manager typically spends filling the portfolio with high-risk loans. If $PD_t$ changes too often, the portfolio quality changes only gradually, whereas a high-risk period of length comparable to the observation window may have no effect on big losses either even though the portfolio quality changes dramatically, simply because the treatment frequency for the whole observation period remains constant at 10%.

**Credit portfolio model**

Loan defaults are triggered by linked latent factors as in the IRB model of Basel II. The bank has $N = 23 \times 50$ loans numbered $(j, k)$ with the current annual default probabilities $PD_{t,j,k} \in \{0.01, 0.1\}$ received at grant. Default derives from latent normal "asset returns" coupled by a single systematic factor:

$$X_{t,j,k} = \sqrt{\rho} Y_t + \sqrt{1-\rho} Z_{t,j,k}$$

where $[Y_t, Z_{t,1,1}, \ldots, Z_{t,23,50}]$ are independent standard normal. Loan $(j,k)$ defaults if the following dummy equals one:

$$\zeta_{t,j,k} = \mathrm{I}\left(X_{t,j,k} \leq \Phi^{-1}\left(PD'_{t,j,k}\right)\right)$$

where $\Phi^{-1}$ is the standard normal quantile function and $PD'_{t,j,k} \equiv 1-(1-PD_{t,j,k})^{1/4}$ the quarterly (rather than annual) default probability. The subportfolio loss in industry $j$ is

$$L_{t,j} = \sum_{k=1}^{50} \zeta_{t,j,k} size_{j,k} LGD_{t,j,k}$$

with i.i.d. $LGD_{t,j,k} \sim \mathrm{beta}(a,b)$ with parameters calibrated to a mean of 0.39 and standard deviation 0.34 (Davydenko and Franks, 2008).

### Evaluation

We first calculate big losses as in the empirical setup. The worst loss in a quarter is, according to (1), given by $L_t \equiv \max_j L_{t,j}$ of which we have simulated $500 \times 72$ consecutive observations. [35] In each of the 500 observation periods we select the 10% largest values of $L_t$ to be big losses as in (11), which results in a time series $bigL_t$ of 36,000 quarters.

We investigate how well big losses can be predicted from the knowledge of lending standards. To this end, we regress $bigL_{t+1}$ on a number of variables the bank manager can know at time $t$. These are the loan standards $PD_{t-L}$ for the lags 0, 1, 2, 4, 8, 12, 16, 20 and the expected quarterly portfolio loss given by

$$EL_t \equiv E(LGD) \sum_{j,k} size_{j,k} PD'_{t,j,k}. \tag{27}$$

Table 16 shows the predictive power achieved in these regressions, measured by the regression $R^2$. We test $PD_t$ without lags (column 1), with all lags as listed above (column 2), the expected loss (column 3) and all together (column 4). We test different values for the mean sojourn time in the high-risk period as this is a crucial for the time given to risk-seeking managers to accumulate high default risk in the portfolio. Further parameter variations such as for the asset correlation $\rho$, the stationary state probability $\mathbf{P}(PD_t = 0.1)$, and the number of loans have only a minor effect.

# H Supplementary tables

[35] We let the simulation start two observation periods earlier to achieve a stationary distribution of the the portfolio composition.

Table 16: Regressing simulated big losses on lending standards – Predictive power

| Dependent: $bigL_{t+1}$<br>Regressors included: | (1)<br>$PD_t$ | (2)<br>$PD_t$ and lags | (3)<br>$EL_t$ | (4)<br>$EL_t$, $PD_t$ and lags |
|---|---|---|---|---|
| Mean sojourn time in high-risk state (quarters): | | | | |
| 2 | 0.0% | 0.4% | 0.2% | 0.4% |
| 4 | 0.3% | 1.0% | 0.9% | 1.0% |
| 8 | 0.3% | 1.2% | 1.2% | 1.2% |
| 12 | 0.4% | 1.1% | 1.2% | 1.2% |
| 20 | 0.3% | 0.7% | 0.8% | 0.8% |
| 40 | 0.4% | 0.5% | 0.5% | 0.5% |
| 60 | 0.2% | 0.2% | 0.3% | 0.3% |

Table entries are $R^2$ values of various regressions. Dependent variable: $bigL_{t+1}$ obtained from a simulation over 36,000 quarters as described in Appendix G. Regressors are the "lending standard" at time $t$, which is the annual default probability of each new loan granted at $t$ equal to $PD_t \in \{0.01, 0.1\}$; furthermore its lags over 0, 1, 2, 4, 8, 12, 16, and 20 quarters; and the expected portfolio loss $EL_t$ as given in (27). The mean sojourn time (in quarters) for the high-risk state parameterized the transition law for the two-state Markov process $PD_t$ in combination with the stationary state probability $\mathbf{P}\,(PD_t = 0.1) = 0.2$.

Table 17: Lending and losses by industry

| No. | Industry, code | Lending | Losses (p.a.) | Worst industry | Extreme losses |
|---|---|---|---|---|---|
| 1 | Agriculture, forestry, fishing and aquaculture (110) | 2.79 | 0.59 | 4.08 | 8.56 |
| 2 | Electricity, gas and water supply; refuse disposal, mining and quarrying (120) | 7.99 | 0.34 | 1.49 | 14.70 |
| 3 | Chemical industry, manufacture of coke and refined petroleum products (131) | 4.13 | 1.76 | 0.29 | 14.93 |
| 4 | Manufacture of rubber and plastic products (132) | 11.55 | 1.18 | 0.93 | 10.62 |
| 5 | Manufacture of other non-metallic mineral products (133) | 5.83 | 1.84 | 0.69 | 9.06 |
| 6 | Manufacture of basic metals and fabricated metal products (134) | 12.62 | 0.19 | 4.32 | 12.63 |
| 7 | Manufacture of machinery and equipment; manufacture of transport (135) | 1.17 | 0.85 | 3.48 | 13.90 |
| 8 | Manufacture of computer, electronic and optical products (136) | 0.80 | 1.51 | 1.57 | 10.91 |
| 9 | Manufacture of wood, pulp, paper, furniture, printing... (137) | 0.58 | 1.68 | 4.49 | 9.20 |
| 10 | Textiles, apparel and leather goods (138) | 2.40 | 1.60 | 0.92 | 10.44 |
| 11 | Manufacture of food products and beverages; manufacture of tobacco products (139) | 3.43 | 1.72 | 2.53 | 9.48 |
| 12 | Construction (140) | 1.37 | 1.84 | 11.7 | 7.91 |
| 13 | Wholesale and retail trade; repair of motor vehicles and motorcycles (150) | 2.12 | 1.99 | 20.83 | 11.73 |
| 14 | Transportation and storage; post and telecommunications (160) | 0.44 | 2.53 | 4.36 | 7.41 |
| 15 | Financial intermediation (excluding MFIs) and insurance companies (170) | 1.71 | 1.18 | 1.58 | 4.61 |
| 16 | Housing enterprises (181) | 6.83 | 1.04 | 4.32 | 12.70 |
| 17 | Holding companies (182) | 4.39 | 1.04 | 0.88 | 12.20 |
| 18 | Other real estate activities (183) | 10.16 | 1.14 | 6.73 | 13.12 |
| 19 | Hotels and restaurants (184) | 1.43 | 2.05 | 6.15 | 5.78 |
| 20 | Information and communication; research and development; membership (185) | 6.33 | 1.46 | 8.01 | 7.46 |
| 21 | Health and social work (enterprises and self-employment) (186) | 5.53 | 0.61 | 2.91 | 8.45 |
| 22 | Rental and leasing activities (187) | 2.11 | 1.10 | 0.79 | 8.63 |
| 23 | Other service activities (188) | 4.30 | 1.46 | 6.94 | 7.37 |

All figures in percent. Column "Lending" shows the composition of the aggregate domestic corporate credit portfolio of all German banks as reflected by the Bundesbank's borrower statistics; column "Losses p.a." shows annual loss rates for each industry sector. For each bank and quarter, the "worst industry" is defined as the one with the biggest subportfolio loss, in euro relative to total assets. Column "Worst industry" shows how often an industry has been the "worst". Column "Extreme losses" shows industry-specific averages of the dummy *bigL* as defined in (11), that is the frequency at which an industry has been responsible for the biggest 10% of industry-specific losses in a bank's history (among losses made in the "worst" industry). Industry sectors and codes are defined in the Bundesbank's borrower statistics (Deutsche Bundesbank, 2009).

Table 18: Correlations of key variables

| Variable | $n_{t,i}$ | ~, L4 | $nbm^{\text{cty}}_{t,i}$ | ~, L4 | $nbm^{\text{distr}}_{t,i}$ | ~, L4 | $nbm^{\text{state}}_{t,i}$ | ~, L4 | $nbm^{\text{DE}}_{t,i}$ | ~, L4 | $ml_{t,i}$ |
|---|---|---|---|---|---|---|---|---|---|---|---|
| $n_{t,i}$ | 1 | | | | | | | | | | |
| ~, L4 | 0.215 | 1 | | | | | | | | | |
| $nbm^{\text{cty}}_{t,i}$ | 0.116 | 0.090 | 1 | | | | | | | | |
| ~, L4 | 0.107 | 0.145 | 0.138 | 1 | | | | | | | |
| $nbm^{\text{distr}}_{t,i}$ | 0.153 | 0.103 | 0.450 | 0.229 | 1 | | | | | | |
| ~, L4 | 0.141 | 0.206 | 0.187 | 0.539 | 0.295 | 1 | | | | | |
| $nbm^{\text{state}}_{t,i}$ | 0.173 | 0.133 | 0.381 | 0.242 | 0.744 | 0.352 | 1 | | | | |
| ~, L4 | 0.140 | 0.201 | 0.176 | 0.485 | 0.310 | 0.786 | 0.345 | 1 | | | |
| $nbm^{\text{DE}}_{t,i}$ | 0.168 | 0.153 | 0.284 | 0.267 | 0.550 | 0.341 | 0.744 | 0.344 | 1 | | |
| ~, L4 | 0.139 | 0.222 | 0.137 | 0.419 | 0.287 | 0.639 | 0.342 | 0.779 | 0.353 | 1 | |
| $ml_{t,i}$ | 0.117 | 0.143 | –0.022 | –0.061 | –0.041 | –0.083 | –0.068 | –0.123 | –0.083 | –0.173 | 1 |

Correlation of variables as used in Table 8, column 5. While $n_{t,i}$is the new lending of bank $i$, the other $nbm^{reg}_{t.i}$ are lending variables of benchmark banks that replicate the portfolio composition ob bank $i$ at some level in the hierarchy of regions, from counties (cty) via districts (distr) and states to the whole nation (DE). L4 denotes a lag of 4 quarters.

Table 19: Further robustness tests

| Dependent: $n_{t,i}$<br>Setup: | (1)<br>(base case)<br>By bank | (2)<br>Idiosyncratic<br>losses | (3)<br>Loss<br>residuals | (4)<br>Alternative<br>new lending |
|---|---|---|---|---|
| Big loss $bigL_{t,i}$ | –0.255*** | –0.203*** | –0.145*** | –0.264*** |
| | (0.0359) | (0.0324) | (0.0354) | (0.0356) |
| Low capital $lowC_{t-4,i}$ | –0.240*** | –0.278*** | –0.266*** | –0.237*** |
| | (0.0551) | (0.0513) | (0.0595) | (0.0548) |
| Interaction $bigL_{t,i} \times lowC_{t-4,i}$ | 0.0421 | –0.0641 | 0.157 | 0.0576 |
| | (0.115) | (0.105) | (0.129) | (0.116) |
| New lending, lag 4 $n_{t-4,i}$ | 0.0445*** | 0.0118 | 0.0413*** | 0.0439*** |
| | (0.0105) | (0.00818) | (0.0113) | (0.0106) |
| Benchm. (county) $nbm^{\text{cty}}_{t,i}$ | 0.0713*** | 0.146*** | 0.0790*** | 0.0703*** |
| | (0.0114) | (0.0118) | (0.0124) | (0.0114) |
| —, lag 4 | –0.00249 | –0.0113 | –0.00455 | –0.00322 |
| | (0.0109) | (0.00994) | (0.0118) | (0.0109) |
| Benchm. (DE) $nbm^{\text{DE}}_{t,i}$ | 0.790*** | 0.963*** | 0.751*** | 0.835*** |
| | (0.0512) | (0.0475) | (0.0589) | (0.0526) |
| —, lag 4 | 0.0854* | 0.169*** | 0.167*** | 0.0825* |
| | (0.0464) | (0.0425) | (0.0525) | (0.0477) |
| Maturing loans $ml_{t,i}$ | –0.118*** | –0.113*** | –0.142*** | –0.115*** |
| | (0.0346) | (0.0271) | (0.0408) | (0.0340) |
| —, lag 4 | –0.136*** | –0.0934*** | –0.139*** | –0.136*** |
| | (0.0282) | (0.0257) | (0.0322) | (0.0272) |
| Fixed effects | bank, time, worst industry | | | |
| Observations | 24041 | 37060 | 20560 | 24041 |
| Adj. $R^2$ | 0.2356 | 0.2381 | 0.2360 | 0.2390 |
| Adj. $R^2$ (within) | 0.0543 | 0.0774 | 0.0539 | 0.0561 |

Column 1 is the base case from Table 3. In columns 2–3, the variables and observation period are the same as in the base case, except *bigL* and its interaction with *lowC*. In column 2, the losses (which *bigL* is based on) have been adjusted for systematic components by subtracting nationwide weighted averages of losses in the same industry. In column 3, autocorrelated components have been removed from losses. Original losses are replaced by the residuals of a linear regression $\log L_{t,i} = \alpha_i + \beta_1 \log L_{t-4,i} + \varepsilon_{t,i}$, which includes the only lag that has turned out significant in a more extensive regression on multiple lags of $\log L_{t,i}$. We return to the original *bigL* in column 4, whereas new lending (also for benchmark banks) is based on definition (15) that subtracts valuation changes from the change in exposures. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.