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Banks' credit losses and lending dynamics

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Non-technical summary

Research Question

How do banks react to a capital shortage? While there is broad agreement that banks reduce their lending after a reduction in capital, assessments of the strength of this response vary greatly. Assuming, for example, constant leverage, banks with a capital ratio of 10% reduce their lending by 10 euro for each euro lost. Empirical research has found significantly weaker, but nevertheless very different, reductions of between 0.5 and 5 euro (for each euro of capital lost).

Contribution

We examine the reaction of all German banks to large credit losses. This also captures the impact of capital shocks as losses indirectly reduce capital.

In order to clearly separate cause from effect, the shocks from losses must come as a surprise. Our novel approach is to select the worst credit losses in a single industry from the individual history of each bank and then examine lending after these severe events for deviations from the base case.

Another contribution is our method for modeling loan demand. To this end, we construct a "twin" for each bank from the lending operations of other banks and include this bank in the estimate as a tailored competitor.

Results

After a substantial loss, banks reduce their lending by an average of 1.32 euro for each euro lost. This figure is more in the lower range of previous empirical studies, and therefore contradicts the assumption of constant leverage likewise. Weakly capitalized banks also reduce their lending, and some estimates suggest that weak capital reinforces the lending effect of a large loss during a financial crisis.

Nichttechnische Zusammenfassung

Fragestellung

Wie reagieren Banken auf Kapitalknappheit? Weitgehend einig ist man sich darüber, dass Banken nach einem Kapitalrückgang ihre Kreditvergabe vermindern. Die Stärke der Reaktion wird jedoch sehr verschieden eingeschätzt. Unterstellt man beispielsweise eine konstante Verschuldungsquote, so reduzieren Banken mit einer Eigenkapitalquote von 10% nach einem Kapitalrückgang von einem Euro ihre Kreditvergabe um 10 Euro. Die empirische Forschung hat deutlich schwächere, aber gleichwohl sehr unterschiedliche Reduktionen zwischen einem halben und 5 Euro gefunden (für jeden Euro an fehlendem Eigenkapital).

Beitrag

Wir untersuchen die Reaktion der Kreditvergabe aller deutscher Banken auf große Kreditverluste. Damit erfassen wir auch den Einfluss von Kapitalrückgängen, weil Verluste mittelbar das Eigenkapital schmälern.

Für eine saubere Trennung von Ursache und Wirkung müssen die Schocks aus Verlusten überraschend sein. Unser neuer Ansatz besteht darin, in der Historie jeder Bank die schlimmsten Kreditverluste in einer einzelnen Branche zu selektieren und anschließend die Kreditvergabe nach diesen einschneidenden Ereignissen auf Abweichungen vom Normalfall zu untersuchen.

Ein weiterer Beitrag ist unsere Methode für die Modellierung der Kreditnachfrage. Dazu konstruieren wir für jede Bank aus den Kreditgeschäften anderer Banken einen "Zwilling", der als maßgeschneiderter Wettbewerber in die Schätzung eingeht.

Ergebnisse

Nach einem schweren Verlust reduzieren Banken ihre Kreditvergabe um durchschnittlich 1,32 Euro je verlorenem Euro. Dieser Wert liegt eher im unteren Bereich früherer empirischer Untersuchungen und steht damit ebenso wie diese im Widerspruch zur Annahme einer konstanten Verschuldungsquote. Schwach kapitalisierte Banken verringern ebenfalls ihre Kreditvergabe, und gemäß einigen Schätzungen verstärkt eine schwache Kapitaldecke die Wirkung eines großen Verlustes auf die Kreditvergabe während einer Finanzkrise.

DEUTSCHE BUNDESBANK DISCUSSION PAPER NO 36/2021

Banks' Credit Losses and Lending Dynamics*

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Abstract

Using detailed data of all German banks, we find that banks which have suffered heavy credit losses reduce their corporate lending business by 1.32 euro for each euro lost; with 95% confidence, the effect is between 0.85 and 1.80 euros. This sensitivity is in line with (quite heterogeneous) results of earlier studies but significantly lower than those arising from the assumption of constant leverage. Weakly capitalized banks grant fewer new loans than other banks. We control for credit demand using a new method, the construction of tailored hypothetical bank competitors.

Keywords: Credit losses, Bank lending

JEL classification: G 21

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1 Introduction

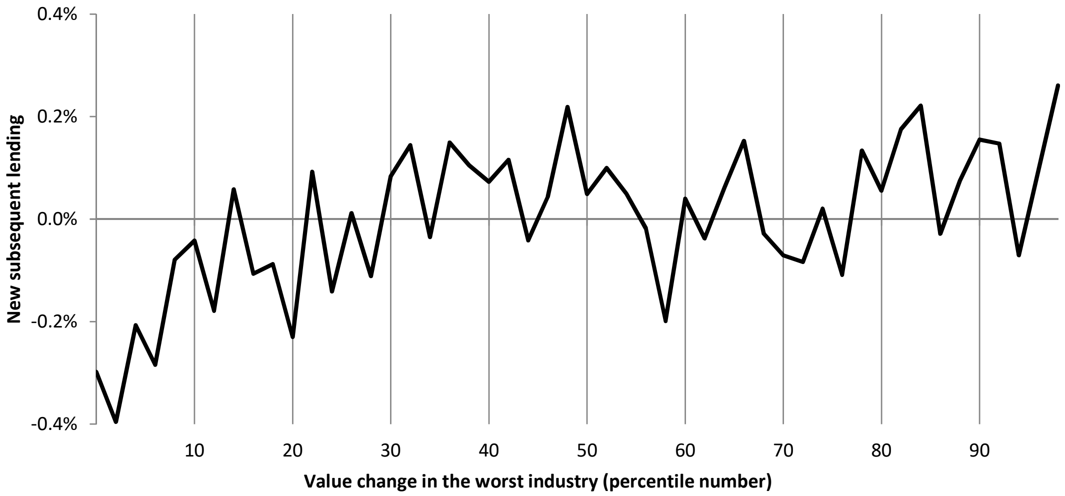
Providing the real economy with credit is a core function of the banking sector. An impaired ability or willingness of banks to extend credit may do harm to the real economy as economically viable projects may not be funded.

Using detailed data of German banks' credit portfolios, we estimate how a bank adjusts its domestic corporate lending after a shock in the form of a heavy credit loss. We are mainly interested in the identification and even more the quantification of the effect rather than deeper reasons for its existence as our study is mainly motivated by stress tests for which the investigated effect is an important parameter.

In our main analysis, we contrast the severity of a bank's credit loss in a single industry with new lending in the other industries the bank lends to. To give a first impression of the effect, Figure 1 plots new lending business against loss severity. The x-axis is given by percentile numbers of losses in a bank's worst industry (defined as the one with the largest loss in a given quarter), starting with the biggest losses on the left.[1] The black line displays average new lending to the other industries over the four subsequent quarters. The ten percent biggest losses, placed on the left of the scale, seem to lead to a reduction in new lending, relative to the 90 percent smaller losses on the right where lending appears to randomly oscillate around a constant.

Controlling for alternative sources of this drop in lending, our main estimate confirms the visual impression by assigning the dummy for the 10% largest losses a coefficient of −0.19. Expressed as a linear effect, each euro lost in a severe credit event lets the bank reduce its lending by 1.32 euros, as the point estimate, or between 0.85 and 1.80 euros as the 95% confidence interval. The effect is moderate compared to values found in the literature (Section 2) and much weaker than the one implied by the assumption of constant leverage.

Figure 1: New lending business vs credit losses



For each bank, industry, and quarter, we calculate valuation changes relative to total assets; negative values are losses. The industry with the lowest value change (biggest loss) defines the worst loss and industry for each bank/time observation. All such values are grouped in percentiles (each covering 2%) of bank specific samples, defining the x-axis (2–100), with worst values (biggest losses) on the left. Values on the y-axis are based on new domestic corporate lending after the four subsequent quarters, excluding the worst industry, normalized by total assets. Plotted values are averages, taken over the subsamples defined by the loss percentiles, subject to a uniform centering.

[1] Percentile numbers are calculated for each bank individually; each of the 50 data points covers 2%.

Our primary data from the Bundesbank's borrower statistics (Kreditnehmerstatistik) covers the domestic corporate lending of all German banks over 60 quarters, broken down into 23 industries and, further, into 3 maturity bands. At this level of aggregation we also know credit losses under a definition that is mainly driven by write-downs (and write-ups). This focus on concrete losses of individual loans helps us because our identification works the better the more severe and the more idiosyncratic the credit events are. The focus on write-downs is one of a number of reasons why we prefer the borrower statistics over the borrower specific German credit register; see Section 4 for the details.

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 could hardly be predicted. The basic idea is that we only look at banks that have recently suffered a really substantial credit loss in a single industry. We argue that such losses are exogenous for the most part because no bank expects an immediate credit loss of, say, 30% of its loan exposure to an industry.

To be clear: Bank managers do expect that the bank will suffer a big loss sooner or later. However, they have no idea when this will be, even if they have chosen a particularly risky (or safe) strategy. Akin to the Latin motto *mors certa, hora incerta*[2], it is not the possibility of a heavy loss that is key to our identification strategy, but its point in time. We are also confident that heavy losses as we define them are noticeable events to banks as they differ greatly from normal losses in our data.[3]

We take a number of measures to ensure that the shocks selected really have come as a surprise. First, we define losses over the shortest horizon possible in our data. Second, we use a dummy variable for the occurrence of an extreme loss rather than the loss extent, which dampens the potential influence that banks may have on the size of losses. Third, for each bank and quarter we select the biggest euro loss from 23 industries, which creates a sample of losses with boosted severity, compared to losses in a fixed industry. Fourth, and most important, we implement the motto *mors certa* by construction, as extreme losses are defined to be the worst 10% of losses from the *individual* history of each bank such that a self-selection into a high or low frequency of extreme losses is a priori impossible.

While many empirical studies make use of a common single shock (natural disasters, unforeseen political shocks etc.), our concept creates shocks in the whole observation period which, on the one hand, raises general endogeneity concerns since shocks could in principle impact shocks that occur later. We therefore run an extensive set of robustness tests regarding the construction of a big loss. On the other hand, shocks scattered over the whole period appear under varying macroeconomic conditions, which makes them more representative than a simultaneous shock whose representativeness can only arise from the cross-section of subjects or economies included. In that sense, the varying economic conditions under which our shocks occur represent a first implicit robustness test that our estimates have to pass.

To address the typical problem that credit supply and demand are not separately observed, we make the key assumption that it is sufficient to control for demand at an aggregation level defined by the combinations of 23 industries and 401 German counties (we call such combinations *segments*). While our data would not allow us to drill further down anyway, the results of Degryse, De Jonghe, Jakovljević, Mulier, and Schepens (2019) suggest that these industry/county

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

[3] The loss distribution has a fat tail; see Section 4.2.

segments are disaggregate enough to absorb most of the variation that could be absorbed by borrower/time fixed effects.[4]

Instead of relying on fixed effects exclusively we take a new approach and include the new lending of a bespoke hypothetical competitor in the main estimate. The credit exposures of this *benchmark bank* are distributed over industry/region segments in the same way as the bank under consideration.

If, for instance, a locally active bank has made 1/3 of its total lending to farmers (constituting an industry) in the county Vechta and 2/3 to food producers (another industry) in the county Cloppenburg, it is benchmarked by all other banks' loan exposures to exactly those two industry/county segments at the same $(1/3, 2/3)$ proportion. In this example, "all other banks" can actually mean very few banks, depending on who else is lending in Vechta and Cloppenburg to farmers and food producers. If, by contrast, the bank under consideration is active in many industries throughout Germany, it is benchmarked by an equally composed and hence nationally active well-diversified hypothetical competitor.

This design optimally absorbs homogeneous demand shocks, that is, the absorption would be perfect if all firms in an industry/region segment asked the banks that are lending to this segment for the same proportional extension (or reduction) of credit.

Following the current state of the art (Khwaja and Mian, 2008), we would include, according to our key assumption, industry/region/time fixed effects in an estimation of bank/industry/region/time observations of new lending. While this disaggregate observation level has some technical disadvantages, we would not really benefit from the disaggregation as we are ultimately interested in the bank perspective when it comes to the question of by how much banks cut their lending after a heavy loss. The benchmark bank as a demand control allows us to reconcile a number of otherwise incompatible features:

- Estimation at bank level, at which we can integrate the intensive and extensive margin of lending and avoid the notorious noise accompanying relative changes in disaggregate lending positions[5];
- Demand control at sub-portfolio level (of industry/region segments);
- Selection of the – random – worst industry in order to boost the severity of losses.

We also have to take into account that some banks are well diversified over industries and regions whereas most of them lend only locally.[6] Counting how many industry/county segments are covered (lent to) by a bank in a typical quarter, only 1% of the banks (23 in number) are responsible for one fourth of all such bank–industry/region relationships. In a regression with industry/region/time fixed effects, these 23 banks would be treated as if they were responsible for one fourth of the portfolio decisions (around 350) and hence be taken unduly important in a study of bank behavior. Weighting observations might put this right but weighting is, in the end, quite similar to what we do.

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

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

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

[6]The local concentration is partly a consequence of the "regional principle" followed by German savings banks and cooperative banks. Each cooperative bank is bound to a certain region (which can even be a single county) and must not "poach" in the region of other cooperative banks. Exceptions do exist but represent a small amount of lending only. A similar principle holds for savings banks.

credit loss. Some estimates (but not all) suggest that weak capital reinforces the lending effect of a big loss during a financial crisis.

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 weak, if any, evidence of such a dampening effect.

Third, non-profit organizations and retail customers do not seem to be considered for loan reductions after big losses. Surprisingly, we also find no effect on securities holdings, despite their better liquidity, which points to an isolated management of credit losses within the banking book. By contrast, low capital does trigger securities sales.A new method for demand control and a non-standard shock deserve thorough tests. The first test regards the hierarchy of regions (county or higher) within which demand is assumed to be homogeneous; this determines the construction of the benchmark bank. Contrary to a fixed-effects setting where elementary FEs absorb aggregate FEs, it is possible and beneficial to use two benchmark banks simultaneously, one for local demand factors (with matching exposures to industry/county segments) and one for supra-regional factors (with matching exposures to industries but no regard to the location of borrowers).

We then test our shock concept extensively for potential endogeneity issues: we vary the definition of the big-loss dummy, construct a matching control sample using a mix of exact and propensity score matching, remove remainders of systematic factors and autocorrelation in default risk, and vary the severity of losses by variation of the tail probability. None of these and some further tests put the main results in question, except that the mentioned reinforcement effect between big losses and low capital during crisis times gets lost.

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 the 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.

There are further measures used in the literature as 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 credit losses incurred by an individual bank in a single industry. As explained above, we assume that the heaviest of such credit losses are exogenous events. We estimate by how much a bank that has suffered such a substantial loss in a certain industry expands or contracts its credit exposure to the *other* industries afterwards.

We exclude the industry with the most substantial loss for three reasons. First, large further write-downs (but also write-ups) in this industry can be expected, for instance as a result of

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

Study / Assumption	Reduction	By banks with ...	Sample
Constant leverage	10.00 euro	—	—
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%.

an intensified scrutiny of problem loans, the revaluation of collateral, or shocks to the liquidation value. We are hesitant to interpret the corresponding exposure changes as actual lending decisions. Second, banks may wish to keep the industry composition of their credit portfolio constant. Big losses in an industry would then be followed by increased lending, particularly to that industry. And third, the split between the problematic industry and the rest of the portfolio tempers the effect of systematic credit risk factors as 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 (2002Q4–2017Q4), index i for a bank (1,774 in raw data), j for an industry (23), and k for a maturity bracket (3). Our data contains the loan exposures $ex^k_{t,i,j}$ to each bank/industry/maturity cell and corresponding value changes $c^k_{t,i,j}$, which are changes in the valuation of the exposure between $t-1$ and t, based on the positions in $t-1$. A write-down is reflected in negative values of $c^k_{t,i,j}$ (or negative contributions to it, if multiple revaluations overlay).

We make use of the maturity information in the calculation of a control variable, the amount of maturing (or expiring) loans (see Section 3.2); the key variables $ex_{t,i,j} \equiv \sum_k ex^k_{t,i,j}$ and $c_{t,i,j} \equiv \sum_k c^k_{t,i,j}$ are given at bank/industry level.

Net new lending business $n_{t,i,j}$ over a horizon of T quarters is of key interest in our analysis. The horizon is one year throughout ($T = 4$), aside from one robustness test, and therefore skipped in the notation. In the base case, new lending business is defined as the simple exposure difference

$$n_{t,i,j} \quad \equiv \quad \frac{ex_{t+4,i,j} - ex_{t,i,j}}{TA_{t,i}}, \tag{1}$$

which is normalized by total assets $TA_{t,i}$. This measure of new business includes value changes, which could also be subtracted from the exposure difference. While doing so makes sense if the bank management's mere action is to be isolated, we prefer to include value changes as the result is the micro-counterpart to the ultimate loan growth in the whole economy; the alternative definition is subject to a robustness test in Section 5.3.

Losses in an industry and, among them, the severe ones, are identified as follows. For each bank and quarter, we select the industry with the worst value change:

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

Observations with $\min_j\,(c_{t,i,j}) \geq 0$ are excluded because almost all of them contain multiple industries with zeros. Each of the industries would be a candidate "worst" industry such that we could not sensibly define the remaining industries. Our focus is on the most negative values (that is, the biggest losses) anyway.

In the estimates, we investigate the relationship between the worst value change:

$$c_{t,i}^{\text{bad}} \equiv \frac{c_{t,i,\text{bad}(t,i)}}{TA_{t,i}} \tag{3}$$

(now as a proportion of total assets) and subsequent new lending business in the remaining portfolio:

$$n_{t,i}^{\neg\text{b}} \equiv n_{t,i,[\neg\text{bad}(t,i)]} \equiv \sum_{j\neq\text{bad}(t,i)} n_{t,i,j}. \tag{4}$$

We use brackets $[\ldots]$ as a symbol for aggregation: $[j]$ means aggregation over all possible values of j, whereas $[\neg j]$ means that a certain index value, such as bad (t,i), is excluded from aggregation. Of course, $c_{t,i}^{\text{bad}}$ and $n_{t,i}^{\neg\text{b}}$ are bound to the existence of bad (t,i).

3.1 Controlling for demand

Following the general consensus in the literature, an analysis like ours crucially depends on a proper control for credit demand and systematic credit risk factors. This view will turn out to apply to our data as well, but our approach is new, to our knowledge. We construct a bespoke hypothetical competitor, the benchmark bank, of each individual bank from all other banks in such a way that its exposure to each of the 23 industries is distributed over Germany's 401 counties in almost exactly the same way as the bank under consideration. That is, we control for demand at the level of 9,223 industry/county segments in a way that reflects portfolio weights and corrects for bank size.

As borrowers are not bound to their county when asking for credit, we vary the notion of region between different levels of aggregation, from 401 counties via 38 districts and 16 states to the maximum aggregate of the whole country. It will turn out that our model benefits from the simultaneous presence of hypothetical competitors at different regional levels, thus covering demand within and across counties.

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. First, we define exposure weights of bank i

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

of industry/region cells, relative to total assets $TA_{t,i}$. The task is to rescale the exposures of the bank's competitors such that the resulting weights replicate those of bank i. After aggregation over all banks except bank i but prior to rescaling, portfolio weights at industry/region level are

$$w_{t,[\neg i],j,r} = \frac{ex_{t,[\neg i],j,r}}{TA_{t,[\neg i]}} = \frac{\sum_{k\neq i} ex_{t,k,j,r}}{\sum_{k\neq i}\sum_{j,r} TA_{t,k}}$$

(as introduced above, the operator $[\neg i]$ stands for summation over all banks, except bank i). Hence, the rescaling factor which transforms the weights of the bank aggregate into those of bank i should ideally be:

$$\nu_{t,i,j,r} \quad \equiv \quad \frac{w_{t,i,j,r}}{w_{t,[\neg i],j,r}}. \tag{5}$$

Remark. This rescaling factor has a surprising mathematical interpretation. It performs a *measure transform* of the exposure distribution $\left(w_{t,[\neg i],j,r}\right)_{j,r}$ to $\left(w_{t,i,j,r}\right)_{j,r}$. In other words, the rescaling factor is the 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 (5) is zero, which happens if no competitor is found for bank i in this industry/region cell at that time. Luckily, the problem only applies to 1.5% of all cells with positive numerators (and only at county level), 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 lifting all rescaling factors proportionally to make them sum up to one again:

$$\nu^*_{t,i,j,r} \quad \equiv \quad \left(\sum_{k,l} w_{t,i,k,l} \mathrm{I}\left(w_{t,[-i],k,l} > 0\right)\right)^{-1} \mathrm{I}\left(w_{t,[-i],j,r} > 0\right) \nu_{t,i,j,r} \,,$$

where $\mathrm{I}(\ldots)$ is an indicator function. This adjustment is equivalent to assigning average values to missing cells. It turns out that the actual error in the portfolio composition is 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 potential source of error but the German bond market and lending from German insurance companies are relatively small.[9]

To calculate the new lending business of the benchmark bank, we determine the region-specific aggregate new business (1) of all banks (but bank i), that is,

$$n_{t,[\neg i],j,r} \quad \equiv \quad \frac{ex_{t+4,[\neg i],j,r} - ex_{t,[\neg i],j,r}}{TA_{t,[\neg i]}} \,, \tag{6}$$

and rescale it to bring it in line with the portfolio weights of bank i:

$$n^{\mathrm{bm}}_{t,i,j,r} \quad \equiv \quad \nu^*_{t,i,j,r}\, n_{t,[\neg i],j,r}. \tag{7}$$

Region-specific figures are no longer needed. We aggregate new business over regions and also over all industries, except the "bad" industry of bank i (symbolized by superscript ¬b):

$$n^{reg,\neg \mathrm{b}}_{t,i} \quad \equiv \quad n^{\mathrm{bm}}_{t,i,[\neg \mathrm{bad}(t,i)],[r]}, \quad reg \in \{\mathrm{cty}, \mathrm{dist}, \mathrm{state}, \mathrm{DE}\}\,. \tag{8}$$

This is the new lending business of the benchmark bank. It is independent of individual regions

[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 22×401 cells, only 1.5% cannot be matched properly. To measure the deviation, we choose (for a single quarter) all cells 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.

[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).

but, of course, still characterized by the region level reg.

Let us resume the discussion of the pros and cons of our approach for a moment. Standard fixed effects would conflict with the selection of a bank's worst industry. Normally, industries are a simple dimension that observations can be divided into, whereas the worst industry is random. If we included industry/region/time fixed effects, we would typically fail to find another bank with the same worst industry at the same time for the same region and hence lose the majority of observations. The design of the benchmark bank does not suffer from this problem.

Our approach may appear similar to the "synthetic control" introduced by Abadie, Diamond, and Hainmueller (2010) in the context of cigarette consumption and applied in a banking context by Dasgupta and Mason (2020), in that hypothetical observations are constructed as weighted averages from other observations. 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 benchmark bank's purpose is rescaling the actual competitors' lending business to the profile of the bank under consideration regardless of the competitors' business models, 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. Furthermore, the benchmark bank is also technically different.[10]

While the rescaling mechanism aligns the aggregate portfolio composition of competitors to the portfolio of bank i, it does not alter their relative market shares within each industry/region cell. This invariance is important for the ability of $n_{t,i}^{reg,\neg \mathrm{b}}$ to absorb demand shocks, or better for the question of which component of demand shocks can be 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 cell 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, a toy "model" for changes in loans to industry j in region r, here in euros,

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

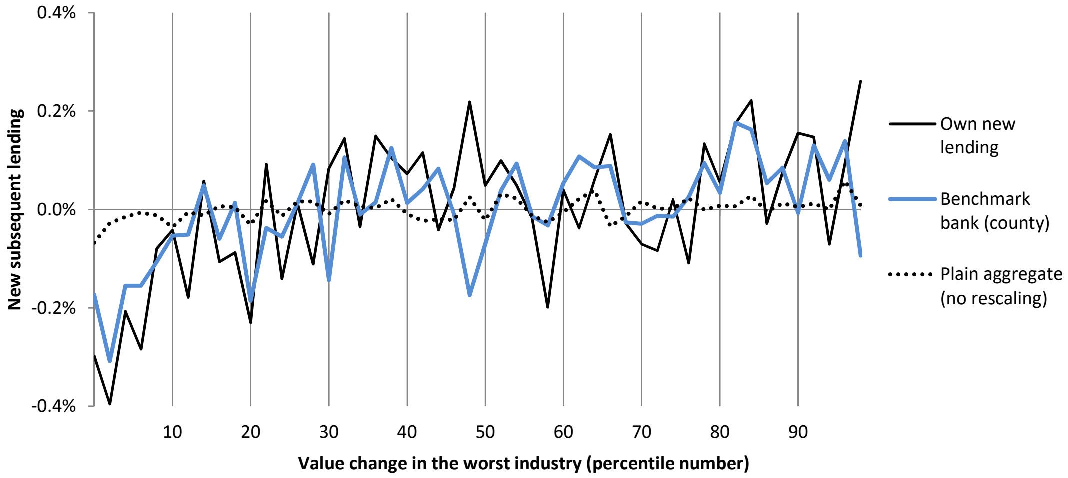
would capture this component by the factor $\gamma_{j,r}$.[11] As a control variable, the new lending $n_i^{reg,\neg \mathrm{b}}$ of the benchmark bank is, in a sense, perfect for absorbing all these factors $\gamma_{j,r}$ with proportional weights as their loadings are the same in a factor representation of $n_i^{reg,\neg \mathrm{b}}$ and $n_i^{\neg \mathrm{b}}$. 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 benchmark banks. If demand and/or systematic credit risk factors matter, $n_{t,i}^{\mathrm{bm},\neg \mathrm{b}}$ should

[10] The dependent variable of the synthetic control is a weighted average of the dependent variables of other bank. Such a representation is not possible for the benchmark bank.

[11] Assume that the whole lending in a certain industry/region cell 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, γ would be equal to $-\frac{1}{3}$and the lending of three banks would drop by 1, 2 and 3 million euro, respectively.

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 levels of $c_{t,i}^{\text{bad}}$. "Own lending" (black solid line) is $n_{t,i}^{\neg\text{b}}$ from (4), the new business of bank i, exclusive of the "bad" industry where the biggest loss has been made. "Benchmark bank" (blue solid line) is $n_{t,i}^{\text{cty},\neg\text{b}}$, the corresponding new business of the benchmark bank obtained from rescaling at industry/county level. "Plain aggregate" (dotted line) is $n_{t,i}^{\text{plain},\neg\text{b}}$ from (10), the benchmark new business without rescaling. The variables are centered and normalized by total assets.

be sensitive to the severity of $c_{t,i}^{\text{bad}}$. To benchmark the benchmark, we also calculate the new lending business of all banks (except i) *without* rescaling by an aggregation of $n_{t,[\neg i],j,r}$ from (6) over sectors and regions:

$$n_{t,i}^{\text{plain},\neg\text{b}} \equiv n_{t,[\neg i],[\neg\text{bad}(t,i)],[r]}. \tag{10}$$

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

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 $n_{t,i}^{\text{cty},\neg\text{b}}$ (blue solid line) and $n_{t,i}^{\neg\text{b}}$ (black solid line) indicates that demand matters; not controlling for it would give the wrong impression of the supply side of lending. The weaker sensitivity of $n_{t,i}^{\text{plain},\neg\text{b}}$ (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 Other control variables

Our data allow us to calculate the approximate *share of maturing loans*, which 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.

Moreover, the variable may influence the extent of loan cuts after a severe 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

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

loans potentially helps to explain lending dynamics. We therefore interact $ml^{\neg b}_{t,i}$ with our key regressor, the dummy variable for severe losses, defined below in (12).

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

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

which is then aggregated over four periods and sectors, consistently with the construction of new business, and normalized by portfolio size:

$$ml^{\neg b}_{t,i} \equiv \min\left(1, \frac{\sum_{s=0}^{3} ml_{t+s,i,[\neg \text{bad}(t,i)]}}{ex_{t,i,[\neg \text{bad}(t,i)]}}\right). \tag{11}$$

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

3.3 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 $c^{\text{bad}}_{t,i}$ in the history of bank i is of key interest:

$$bigL_{t,i} \equiv \text{I}\left(c^{\text{bad}}_{t,i} < \text{Qtl}_{10\%}\left(c^{\text{bad}}_{\cdot,i}\right)\right), \tag{12}$$

where $\text{I}(\ldots)$ 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 and quarter specific) 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.

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:

$$\begin{aligned} n_{t,i}^{\neg b} &= \beta_1\, bigL_{t,i} + \beta_2\, lowC_{t-4,i} + \beta_3\, bigL_{t,i} \times lowC_{t-4,it,i} \\ &\quad + \beta_4\, n_{t-4,i}^{\neg b} + \beta_5\, ml_{t,i}^{\neg b} + \beta_6\, n_{t,i}^{\mathrm{cty},\neg b} + \beta_7\, n_{t-4,i}^{\mathrm{cty},\neg b} + \beta_8\, n_{t,i}^{\mathrm{DE},\neg b} + \beta_9\, n_{t-4,i}^{\mathrm{DE},\neg b} \\ &\quad + \alpha_i^{\mathrm{bk}} + \alpha_t^{\mathrm{qrt}} + \alpha_{\mathrm{bad}(t,i)}^{\mathrm{ind}} + \varepsilon_{t,i}\,, \end{aligned} \tag{13}$$

in which we include lagged (own) new lending, the share of maturing loans, and contemporaneous and lagged new lending of benchmark banks.

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 $n^{\mathrm{cty},\neg b}$ at county level would be the perfect control variable. It is clearly imperfect for different reasons.

First, the bigger 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 $n^{\mathrm{cty},\neg b}$.

While these arguments call for control at a higher level of regional aggregation such as $n^{\mathrm{DE},\neg b}$, 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 is actually made in the first robustness test of Section 5.3 and targeted at a balance between power and parsimony.

We further include fixed effects in three dimensions. Bank fixed-effects α_i^{bk} target at capturing business models, the general fortune of banks in gaining market shares, and those static components of bank risk profiles that are not yet neutralized by the bank specific definition of $bigL$. Quarterly time fixed effects α_t^{qrt} capture the general lending development in the observed period and, finally, fixed effects $\alpha_{\mathrm{bad}(t,i)}^{\mathrm{ind}}$ 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, Gündüz, and Raupach (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, broken down into 23 industries (Table 15), and three brackets of maturity at grant (0–1y, 1–5y, >5y), yielding $69 = 23 \times 3$ subportfolios. The information on loan terms turns out to be a significant determinant of new lending.

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

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

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 loss concept are not the only reasons why we prefer the latter. The credit register also has a reporting threshold of 1 million euro. The 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.

We use the credit register only as a proxy for the regional distribution of exposures when constructing 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 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 potential source of error but the German bond market and lending from German insurance companies are relatively small.[14]

We use quarterly data from 2002Q4, the first time when valuation changes were reported, to 2017Q4. 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 not to matter much.

A mild outlier treatment is applied: we remove the first and 99th percentile of the new-business variable $n_{t,i}^{\neg b}$. Furthermore, we remove banks with a total exposure of less than 10 million euro. We limit losses (at the most disaggregate level) 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.

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 14.7% (Table 15, column

[14] 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).

"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*.[15] This absence of a visible relationship is consistent with our belief that the sources of extreme losses are mostly idiosyncratic.

4.2 Surprises in credit losses

We restrict ourselves to domestic 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[16] that is uniquely determined by the 75% zeros. Then, the 25% non-zero losses consist to 86% of single-default events.[17]

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 such that they lack the surprise aspect that is essential to our identification strategy.[18]

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. Two arguments support that they do differ. First, Table 2 documents the value change $c_{t,i}^{\text{bad}}$ (the negative of a loss rate) as defined in (3) to be extremely leptokurtic. Second, compared to the average loss in the worst industry, which is $E\left(c_{t,i}^{\text{bad}}\right) = -0.04\%$ of total assets, the average *big* loss $E\left(c_{t,i}^{\text{bad}} \,\middle|\, bigL_{t,i} = 1\right) = -0.18\%$ is three to four times larger. In addition, a loss of barely 0.2 of total assets sounds negligible. However, the quarterly profit before taxes (in the period 2003 to 2017) was only 0.055 on average, such that a big quarterly loss in our data is more than three times as large as a bank's average profit in the same period.

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.

[15] A little regression of the 23 industry-specific averages of *bigL*, as presented under "Extreme losses" in Table 15 on the other three variables gives no significant result.

[16] 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 15.

[17] 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 $\Pr(N = 1 \mid N > 0) = 0.216/0.25 = 0.862$.

[18] 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

	$c_{t,i}^{\text{bad}}$	$n_{t,i}^{\neg\text{b}}$	$n_{t,i}^{\text{cty},\neg\text{b}}$	$n_{t,i}^{\text{DE},\neg\text{b}}$	$ml_{t,i}^{\neg\text{b}}$
Mean	–0.04%	0.60%	0.22%	0.06%	2.34%
Std	0.09%	1.76%	1.65%	0.64%	1.93%
Q25	–0.05%	–0.31%	–0.37%	–0.16%	1.47%
Median	–0.01%	0.39%	0.19%	0.08%	2.03%
Q75	0.00%	1.26%	0.81%	0.34%	2.73%
Skewness	–11.8	1.6	–1.8	–3.0	7.6
Kurtosis	411.4	10.1	57.4	77.0	108.1
N	25964	25964	25964	25964	25964

All variables are normalized by the bank's total assets. $c_{t,i}^{\text{bad}}$ is the value change in the worst industry, according to (3). New lending of the bank under consideration through four quarters is given by $n_{t,i}^{\neg\text{b}}$ while $n_{t,i}^{\text{cty},\neg\text{b}}$ and $n^{\text{DE},\neg\text{b}}$ are the same for the benchmark banks at county and national level. $ml_{t,i}^{\neg\text{b}}$, defined in (11), is the approximate share of loans maturing through the next four quarters. Estimates are based on the sample used in the base case estimate of Table 3, column 1. The first and 99th percentile of $n_{t,i}^{\neg\text{b}}$ have been removed prior to the estimate.

4.3 Summary statistics

In Table 15 in Appendix G, we report the composition of the aggregate credit portfolio and corresponding losses. Descriptive statistics of variables directly or indirectly used in the regression (13) are presented in Table 2.

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 banks in our sample.

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 split credit exposures into the region-specific variables $ex_{t,i,j,r}$ used in Section 3.1.

5 Results

5.1 Baseline results

Table 3 presents the result of our base case Equation (13) and of some alternative specifications. We draw the following conclusions:

Table 3: Impact of big losses on new lending business

Dependent variable: New lending $n^{\neg b}_{t,i}$ (4 quarters)	(1) Base case	(2)	(3)	(4)	(5)
Big loss $bigL_{t,i}$	–0.190***	–0.198***		–0.202***	–0.186***
	(0.0381)	(0.0406)		(0.0366)	(0.0382)
Low capital $lowC_{t-4,i}$	–0.201***	–0.207***	–0.218***		–0.185***
	(0.0545)	(0.0543)	(0.0509)		(0.0543)
Interaction $bigL_{t,i} \times lowC_{t-4,i}$	–0.0854	–0.0501			–0.0814
	(0.116)	(0.118)			(0.117)
New lending, lag 4 $n^{\neg b}_{t-4,i}$	0.0247***	0.0250***	0.0250***	0.0252***	0.0279***
	(0.00608)	(0.00614)	(0.00607)	(0.00608)	(0.00602)
Benchm. (county) $n^{\text{cty},\neg b}_{t,i}$	0.0317***	0.0317***	0.0317***	0.0320***	
	(0.0112)	(0.0113)	(0.0112)	(0.0112)	
—, lag 4	0.00133	0.00128	0.00127	0.00114	
	(0.00907)	(0.00909)	(0.00908)	(0.00906)	
Benchm. (DE) $n^{\text{DE},\neg b}_{t,i}$	0.104**	0.104**	0.101**	0.102**	
	(0.0481)	(0.0479)	(0.0481)	(0.0482)	
—, lag 4	0.110***	0.108***	0.110***	0.106***	
	(0.0376)	(0.0377)	(0.0375)	(0.0375)	
Maturing loans $ml^{\neg b}_{t,i}$	–0.150***	–0.147***	–0.150***	–0.152***	–0.152***
	(0.0378)	(0.0382)	(0.0378)	(0.0379)	(0.0380)
$ml^{\neg b,\text{ centered}}_{t,i} \times bigL_{t,i}$		–0.0402			
		(0.0373)			
Fixed effects	bank, time, worst industry (bad (t,i))				
Observations	25964	25964	25964	25964	25964
Adj. R^2	0.2529	0.2530	0.2518	0.2522	0.2500
Adj. R^2 (within)	0.0135	0.0136	0.0120	0.0125	0.00962

This table shows how a big credit loss in a single industry changes new corporate lending to other industries (see Equation 13). All variables in percent, except dummies. Period: 2002Q4–2017Q4. 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). The dummy $bigL_{t,i} \times lowC_{t-4,i}$ is $bigL \times lowC$. Benchmark new lending $n^{\text{cty},\neg b}_{t,i}$, 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 $n^{\text{DE},\neg b}_{t}$ resembles industry weights but neglects regional weights. The approximate share of maturing loans $ml^{\neg b}_{t,i}$ is defined in (11). It has been centered for the interaction with $bigL$. The industry with a bank's largest loss in quarter t is denoted by bad (t,i). Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

First, substantial credit losses, that is the worst 10% of quarterly credit losses in a single industry, lead to a significant reduction in new lending. If a loss belongs to these 10%, the bank reduces the lending to other industries (compared to a bank without such a loss) by 0.19% of total assets. Equation (25) in Appendix D allows us to transform the dummy coefficient into a euro sensitivity: one euro of substantial credit losses leads to a reduction of 1.32 euro in new lending; the 95% confidence interval for this estimate is $[0.85, 1.79]$.

Second, controlling for credit demand is crucial. The corresponding variable is highly significant; its inclusion reduces the coefficients for the credit losses as can be seen by comparing the fourth and fifth columns in Table 3.

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

Fourth, bank capital matters. We see that banks with low capital provide significantly less credit than banks with a higher capital ratio. However, in this specification the interaction term of *lowC* and *bigL* is insignificant; only in times of crises we observe that thinly capitalized banks 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 as 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 (as long as crisis times do not prevent it) 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.

Fifth, the share of maturing loans $ml_{t,i}^{\neg b}$ helps to explain lending dynamics (and will do so throughout all specifications; this variable is responsible for 43% of the R^2 in the base case). The negative coefficient of -0.15 says that most (85%) but not all maturing loans are replaced by new credit. The interaction of $ml_{t,i}^{\neg b}$ and $bigL_{t,i}$ in column 2 is insignificant, that is, our conjecture that bank manager would particularly resort to maturing loans when they reduce lending is not confirmed. As the interaction is also insignificant in various other (unreported) specifications, we omit it in further estimates.

Sixth, comparing the R^2 in columns (1) and (3) of Table 3, including the dummy *bigL* for extreme losses (and the insignificant term *interact*) increases the explanatory power by only 0.15 percentage points, which is a small gain at first glance. But a driver of seemingly low power at bank level can have greater aggregate effects:

Supposing a simplified model outlined in Appendix C, the gain in R^2 at bank level by 0.15 percentage points would lead to an R^2 gain of 10.8% if the same estimate were executed at national level (which we cannot do in practice as it conflicts with our identification approach). This argument is in line with the observations of Berrospide and Edge (2010), who find modest effects of bank capital on lending when analyzed at bank level, and larger effects on the corresponding national aggregates.

The issue may become relevant when microprudential results such as ours are transferred to macroprudential considerations, especially in stress tests. Then, a common shock hits the banking system, and an effect that otherwise appears to get lost in idiosyncratic variation may have considerable aggregate consequences.

Have crisis times been different?

As our observation period from 2002 to 2017 includes the global financial crisis and the sovereign debt crisis, the question is warranted as to what degree our results are driven by either of them. Banks might have reacted quite differently to a big credit loss in crisis times. Being times of 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, the important year – 2007 – is not at our disposal because capital figures in that year are considered unreliable as a result of the transition from Basel I to Basel II in Germany. 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 and just call it "the crisis". 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.

Indeed, Table 4 shows that the crisis period is special. 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, to an insignificant level in the crisis period. By contrast, and quite intuitively, the level of capital matters a lot more than in normal times.

While banks do not seem to directly react to a big loss in the crisis period, the coefficient -0.427 of the interaction term in column 2 suggests that they do react when big losses combine with low capital (however, this finding will not survive a robustness test executed in Section 5.3 where we change the definition of *bigL*).

Due to the limited data amount in the crisis period and a general significance drop of various coefficients in column 2, we check another specification that interacts key regressors with a crisis dummy. In column 4 we complement the base case model (13) by the terms $Cris_t \times bigL_{t,i}$, $Cris_t \times lowC_{t-4,i}$, and $Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$; the crisis dummy alone would be absorbed by the time FEs. The primary effect of big losses, captured by β_1, is then larger than in the base case. If the effect were really lost in the crisis, as column 2 suggests, we should find a significantly positive coefficient of $Cris_t \times bigL_{t,i}$. Its actual insignificance (and the fact that column 4 relies on more data) lets us stay with the view that big losses affect the lending also in crisis times.

Most interestingly, the triple interaction $Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$ has a strong negative lending effect that is fully consistent with the one found for $bigL_{t,i} \times lowC_{t-4,i}$ in the sample split. As such an effect is of key interest for stress test modelers, we will run a number of the robustness tests with and without crisis interactions.

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 severe loss. From the positive coefficients of benchmark new business in the base case regression, we have concluded that substitution, if present, does not dominate the effect of demand.

We refine this observation in Table 5 by additional terms, which interact *bigL*, the indicator of big losses, with the new lending of benchmark banks (centered[19], at county level and nationwide). If competitors of a bank that has incurred a big loss substitute its lending cut, 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.

The negatively significant coefficient at county level suggests substitution to exist whereas the coefficient of the interaction term with the nationwide benchmark is even positive and weakly significant (columns 2 and 3). This difference is consistent with the fact that the county-level benchmark bank of a locally active bank has the same local focus. Such a bank (better: its constituents) has a higher chance to be asked to jump in for the local bank that denies credit, rather than the nationwide benchmark bank constructed from banks sitting anywhere.

The weakly significant positive second coefficient points to common background factors not yet controlled for. Indeed, both interaction terms become insignificant if systematic credit risk factors are removed from c^{bad} before *bigL* is sampled.[20] Altogether, we find rather weak (if any) evidence of a substitution effect.

[19]Subtracting the mean allows for a direct interpretation of the coefficient's sign.
[20]Unreported results; see formula (14) for how we remove the systematic component.

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

Dependent variable: New lending $n^{\neg b}_{t,i}$ (4 quarters)	(1) Base case	(2) Crisis	(3) Normal	(4) Interaction
Big loss $bigL_{t,i}$	–0.190***	–0.0867	–0.213***	–0.202***
	(0.0381)	(0.0640)	(0.0480)	(0.0473)
Low capital $lowC_{t-4,i}$	–0.201***	–0.316***	–0.170**	–0.174***
	(0.0545)	(0.115)	(0.0670)	(0.0614)
Interaction $bigL_{t,i} \times lowC_{t-4,i}$	–0.0854	–0.427**	0.185	0.0980
	(0.116)	(0.210)	(0.142)	(0.138)
New lending, lag 4 $n^{\neg b}_{t-4,i}$	0.0247***	–0.0338***	0.0118*	0.0246***
	(0.00608)	(0.0124)	(0.00712)	(0.00608)
Benchm. (county) $n^{cty,\neg b}_{t,i}$	0.0317***	0.0425**	0.0155	0.0318***
	(0.0112)	(0.0205)	(0.0139)	(0.0112)
—, lag 4	0.00133	–0.0310*	0.0115	0.00138
	(0.00907)	(0.0166)	(0.0107)	(0.00908)
Benchm. (DE) $n^{DE,\neg b}_{t,i}$	0.104**	0.212**	0.0134	0.101**
	(0.0481)	(0.0835)	(0.0612)	(0.0481)
—, lag 4	0.110***	0.184**	0.0993**	0.109***
	(0.0376)	(0.0766)	(0.0479)	(0.0376)
Maturing loans $ml^{\neg b}_{t,i}$	–0.150***	–0.445***	–0.105**	–0.150***
	(0.0378)	(0.0599)	(0.0425)	(0.0378)
$Cris_t \times bigL_{t,i}$				0.0417
				(0.0811)
$Cris_t \times lowC_{t-4,i}$				–0.0861
				(0.0936)
$Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$				–0.600**
				(0.256)
Fixed effects	bank, time, worst industry (bad (t,i))			
Observations	25964	8852	17079	25964
Adj. R^2	0.2529	0.4173	0.2639	0.2533
Adj. R^2 (within)	0.0135	0.0430	0.00698	0.0139

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: 2002Q4–2017Q4. The year 2007 is excluded for data reasons. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

Impact on other loan sectors

While three sectors have been excluded from the main analysis for different reasons, they are nevertheless worth a look as dependent variables. We only sketch the results here and present details in Appendix E and Table 14.

We first look at lending to the "worst" industry bad (i,t). Despite endogeneity issues and other reasons to exclude it from the main analysis[21], measuring the lending effect on this industry may nevertheless help to calibrate stress tests. Running a regression with controls closely corresponding to the base case[22], we find no lending effect of a big loss at all. Idiosyncratic shocks seem to contribute the biggest part to the lending dynamics in a single industry.

In Section 4.2 we argued that loans to non-profit organizations (NPOs) and retail loans are unlikely to create sufficiently surprising shocks. Banks could, however, also resort to these assets to downsize their lending business. Big losses seem to have no effect on lending to NPOs; only low capital puts pressure on it, and only in crisis times. Neither we see an effect of *bigL* in the

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

[22] We include a further lag term (two years) of new lending. It is needed to capture a negative bouncing effect of temporary shocks to loan levels.

Table 5: Is decreased lending substituted by competitors?

Dependent variable: New lending $n_{t,i}^{\neg b}$ (4 quarters)	(1) Base case	(2)	(3)
Big loss $bigL_{t,i}$	–0.190***	–0.184***	–0.187***
	(0.0381)	(0.0383)	(0.0498)
Low capital $lowC_{t-4,i}$	–0.201***	–0.201***	–0.173***
	(0.0545)	(0.0544)	(0.0613)
Interaction $bigL_{t,i} \times lowC_{t-4,i}$	–0.0854	–0.0942	0.0808
	(0.116)	(0.116)	(0.138)
$bigL_{t,i} \times n_t^{\text{cty,centered},\neg b}$		–0.0692**	–0.0677**
		(0.0322)	(0.0323)
$bigL_{t,i} \times n_t^{\text{DE,centered},\neg b}$		0.163*	0.164*
		(0.0875)	(0.0915)
$Cris_t \times bigL_{t,i}$			0.0144
			(0.0871)
$Cris_t \times lowC_{t-4,i}$			–0.0872
			(0.0936)
$Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$			–0.574**
			(0.256)
...	...	...	...
Observations	25964	25964	25964
Adj. R^2	0.2529	0.2533	0.2537
Adj. R^2 (within)	0.0135	0.0140	

All variables, fixed effects, and period as in Table 4, except $bigL_{t,i} \times n_t^{\text{cty,centered},\neg b}$ and $bigL_{t,i} \times n_t^{\text{DE,centered},\neg b}$ 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.

retail sector. Low capital, by contrast, induces banks to divest retail loans, albeit at a very small scale relative to their big mass of around 11% of total assets. We expected the opposite since relatively low amounts of required capital are attributed to retail loans such that a shift from corporate to retail loans would normally save regulatory capital.

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 6, we estimate the impact of big losses ($bigL = 1$) on the change in securities positions. The general impression is that big losses have no impact on securities holdings.

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.

Corporate and bank bonds should normally be treated like the corresponding loans because bonds and loans of the same firm require the same amount of regulatory capital, by and large. However, we find only a weak effect of $lowC$ on corporate loans and no effect of $bigL$. We conclude 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 reveal much regulatory capital if they mainly consist of highly rated senior tranches.

The results concerning government bonds (domestic or foreign) are somewhat puzzling. We

Table 6: Impact on securities holdings: stocks, certificates, and bonds

Dep.: Δposition/TA (%) Securities type:	(1) Stocks and certificates	(2) Bank bonds	(3) Corporate bonds	(4) Government bonds (DE)	(5) Government bonds (foreign)
$bigL_{t,i}$	–0.0186	0.0231	–0.0134	0.0249	–0.00443
	(0.0224)	(0.0595)	(0.0177)	(0.0184)	(0.00874)
$lowC_{t-4,i}$	–0.0402	0.0997	–0.0474**	–0.0966***	–0.0521***
	(0.0305)	(0.0684)	(0.0229)	(0.0224)	(0.0112)
$bigL_{t,i} \times lowC_{t-4,i}$	–0.0685	0.235	0.0211	–0.0225	0.0304
	(0.0677)	(0.165)	(0.0507)	(0.0487)	(0.0245)
Δposit./TA (%), lag 4	–0.0201**	–0.196***	–0.0565***	–0.0924***	–0.123***
	(0.0102)	(0.00802)	(0.00999)	(0.0106)	(0.0118)
posit./TA (%), lag 4	–0.0861***	–0.241***	–0.120***	–0.191***	–0.172***
	(0.00555)	(0.00593)	(0.00651)	(0.00922)	(0.00959)
Fixed effects	bank, time, worst industry (bad (t,i))				
Observations	22517	22628	22662	22462	22501
Adj. R^2	0.2150	0.2312	0.1843	0.1909	0.1750
Adj. R^2 (within)	0.0344	0.131	0.0397	0.0908	0.0730

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: 2002Q4–2017Q4. 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.

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 6 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 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 6 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 loans to other sectors or on its securities holdings. This result is in contrast to the findings of De Jonghe et al. (2020) who find that changes in a bank's capital requirements impact nearly all balance positions.

5.3 Robustness tests

As we apply a novel method to control for loan demand and also use a non-standard type of shock to the banks, 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 portfolio replication by 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 moments; (3) eliminating systematic components in the credit losses; (4) correcting for autocorrelation in the credit losses caused by seasonal effects; and (5) varying the severity of losses and low capital ratios 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 (net of value changes), and the measure of capital. The tests are summarized in Section 6.

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

For reasons outlined in Section 3.3, the 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"); 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[23] the same. Finally, we also include the unweighted aggregate, which does not even account for industry weights.

Table 7 shows the results. Most importantly, the choice of the benchmark bank leaves the impact of the main regressors virtually unchanged, and the interaction term is not significant anywhere.

In column 5, which contains the new business of all four types of benchmark banks, significant coefficients are somewhat scattered over the aggregation levels, with the highest power at state level. This rather unclear pattern may also be influenced by comparably high correlations between variables at neighbored aggregation levels, with a maximum corr $\left(n^{\text{state},\neg\text{b}}, n^{\text{DE},\neg\text{b}}\right) = 0.78$.[24] We state a considerable influence of both local and systematic factors on demand; note, however, that local factors are not idiosyncratic to banks but systematic insofar as they concern *different* banks in the same region, by construction.

If only one $n^{reg,\neg\text{b}}$ is included (columns 1–4), we see that its coefficient gets larger with a rising aggregation up to the state level, as well as the within-R^2. This effect is consistent with a variation in the presence (if not prevalence) of idiosyncratic drivers in local lending. The higher the level of aggregation in benchmark new lending, the smaller the disturbances that bias the estimation coefficients downwards.[25] 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 simple nationwide aggregate new lending $n_{t,i}^{\text{agg},\neg\text{b}}$ in column 7 is assigned borderline significance, the opposite sign, and the least explanatory power of all control variables for loan demand.

[23] 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.

[24] See Table 16 in Appendix G. The correlation between the (ultimately chosen) county and nationwide variable is 0.28 (0.41 between their lags) and hence moderate.

[25] For illustration, think of a model $Y_i = \alpha + \beta X_i + \varepsilon_i$ with an explanatory variable that suffers independent shocks which cannot be split off from the observed variable: $X_i = Z_i + \eta_i$. Here, η stands for idiosyncratic factors that drive individual local lending. The higher the variance of η, the lower the asymptotic regression coefficient $\beta = \text{cov}(Y, X)/\text{var}(X) = \text{cov}(Y, Z)/(\text{var}(Z) + \text{var}(\eta))$.

Table 7: Benchmark banks: Varying the granularity of regions

Dependent variable: New lending $n_{t,i}^{\neg b}$	(1)	(2)	(3)	(4)	(5)	(6) Base case	(7) No wgts.
$bigL_{t,i}$ (dummy)	–0.187***	–0.186***	–0.189***	–0.190***	–0.190***	–0.190***	–0.186***
	(0.0382)	(0.0382)	(0.0381)	(0.0381)	(0.0381)	(0.0381)	(0.0382)
$lowC_{t-4,i}$ (dummy)	–0.186***	–0.197***	–0.200***	–0.202***	–0.203***	–0.201***	–0.185***
	(0.0543)	(0.0542)	(0.0543)	(0.0544)	(0.0545)	(0.0545)	(0.0543)
$bigL_{t,i} \times lowC_{t-4,i}$	–0.0822	–0.0818	–0.0815	–0.0847	–0.0809	–0.0854	–0.0801
	(0.117)	(0.116)	(0.116)	(0.116)	(0.116)	(0.116)	(0.117)
$n_{t-4,i}^{\neg b}$ (lag 4)	0.0276***	0.0266***	0.0258***	0.0246***	0.0249***	0.0247***	0.0280***
	(0.00598)	(0.00611)	(0.00611)	(0.00611)	(0.00609)	(0.00608)	(0.00602)
$ml_{t,i}^{\neg b}$ maturing	–0.153***	–0.153***	–0.152***	–0.148***	–0.151***	–0.150***	–0.152***
	(0.0372)	(0.0384)	(0.0381)	(0.0386)	(0.0378)	(0.0378)	(0.0380)
$n_t^{\text{cty},\neg b}$ (county)	0.0374***				0.0231*	0.0317***	
	(0.0109)				(0.0121)	(0.0112)	
—, lag 4	0.0134				–0.00007	0.00133	
	(0.00875)				(0.00958)	(0.00907)	
$n_t^{\text{dist},\neg b}$ (district)		0.0875***			–0.00256		
		(0.0244)			(0.0332)		
—, lag 4		0.0457**			0.00031		
		(0.0182)			(0.0252)		
$n_t^{\text{state},\neg b}$ (state)			0.136***		0.109**		
			(0.0330)		(0.0439)		
—, lag 4			0.0558**		0.0176		
			(0.0238)		(0.0345)		
$n_t^{\text{DE},\neg b}$ (DE)				0.122**	0.0160	0.104**	
				(0.0482)	(0.0559)	(0.0481)	
—, lag 4				0.108***	0.0872**	0.110***	
				(0.0371)	(0.0434)	(0.0376)	
$n_t^{\text{plain},\neg b}$ (no wgts.)							–0.269*
							(0.150)
—, lag 4							0.102
							(0.173)
Fixed effects	bank, time, worst industry (bad (t,i))						
Observations	25964	25964	25964	25964	25964	25964	25964
Adj. R^2	0.2511	0.2519	0.2531	0.2523	0.2536	0.2529	0.2500
Adj. R^2 (within)	0.0110	0.0121	0.0137	0.0126	0.0144	0.0135	0.00967

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 cell 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 unweighted German aggregate of new lending (excluding the "worst" industry of bank i) as control variable; see (10). The industry with a bank's largest loss in quarter t is denoted by bad (t,i). Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

Provided that differences in 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. We have not tested the alternative order, that is, accounting for regions alone first and then for the industry composition.

The ultimate specification (column 6) includes the benchmark banks at county and national level and thus the two ends of the aggregation scale. This choice is not literally suggested by column 5 but made in respect of the lowest correlation among these regressors and of model variants (used below) under which estimates like column 5 would favor the national over the state level.

Varying the definition 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 $\left\{c_{t,i}^{\text{bad}} : t = 1, \ldots, T\right\}$ 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. To exclude them, we replace the bank-specific loss samples with quarter-specific samples such that $bigL_{t,i}$ takes value 1 if $c_{t,i}^{\text{bad}}$ 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).

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.

Table 8 compares the definitions, either with base case controls (columns 1–3) or interacted with the crisis dummy (4–6). As expected, big losses from the pooled sample create the biggest lending cuts, closely followed by the base case estimate. The other coefficients are mainly unaffected, except the triple interaction of big losses, low capital, and the crisis period, which becomes insignificant for the quarterly defined version of *bigL*. Including a number of unreported further tests[26] we can neither confirm nor deny the plausible hypothesis that banks would contract their lending even further when big losses combine with low capital in generally stressful times. Some signals point to such a behavior but they are not robust.

While the reaction to quarterly selected big losses is one fourth weaker than the others, we deem its significance and 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.

Matching control samples

While our definition of *bigL* prevents banks from being "over-treated" by big losses throughout the observation period, a *temporary* selection into a high probability of big losses is still possible and could be endogenous. To test for such effects, we construct balanced samples using a combination of exact matching for quarters and propensity score matching (PSM) for other characteristics.

In a first step, we use the whole sample to estimate an overarching propensity score that predicts the probability to be "treated" ($bigL = 1$). In a second step, we go through each quarter and assign every observation of a "treated" bank its so-called nearest neighbor, that is, the bank with the closest score (treatment probability) among all untreated observations in the same (!) quarter. Since the definition of *bigL* declares exactly 10% of banks to be "treated", we are left with 20% of the initial sample size if only a single nearest neighbor is included. Alternatively, we allow for three and five nearest neighbors to be included to achieve a balance between matching quality and coverage. Details of the matching procedure are outlined in Appendix F.

[26] We have also varied the definition of *bigL* in most of the tests of this section. The triple interaction loses significance in a couple of them.

Table 8: Testing variants of the dummy $bigL$ for big losses

Dependent: $n_{t,i}^{\neg b}$	(1)	(2)	(3)	(4)	(5)	(6)
Benchmark bank:	Base case variables			With crisis effects		
Samples for $bigL_{t,i}$:	By bank	Quarterly	Pooled	By bank	Quarterly	Pooled
Big loss $bigL_{t,i}$	–0.190***	–0.176***	–0.237***	–0.202***	–0.156***	–0.237***
	(0.0381)	(0.0424)	(0.0452)	(0.0473)	(0.0516)	(0.0547)
Low capital $lowC_{t-4,i}$	–0.201***	–0.238***	–0.250***	–0.174***	–0.160***	–0.224***
	(0.0545)	(0.0493)	(0.0507)	(0.0614)	(0.0547)	(0.0568)
$bigL_{t,i} \times lowC_{t-4,i}$	–0.0854	–0.152	–0.0371	0.0980	–0.0421	0.126
	(0.116)	(0.120)	(0.134)	(0.138)	(0.144)	(0.159)
New lending, lag 4 $n_{t-4,i}^{\neg b}$	0.0247***	0.0238***	0.0253***	0.0246***	0.0239***	0.0254***
	(0.00608)	(0.00572)	(0.00602)	(0.00608)	(0.00573)	(0.00602)
Benchm. (county) $n_{t,i}^{cty,\neg b}$	0.0317***	0.0179*	0.0152	0.0318***	0.0180*	0.0153
	(0.0112)	(0.0107)	(0.0109)	(0.0112)	(0.0107)	(0.0109)
—, lag 4	0.00133	0.00993	–0.0106	0.00138	0.0101	–0.0104
	(0.00907)	(0.00808)	(0.00882)	(0.00908)	(0.00809)	(0.00883)
Benchm. (DE) $n_{t,i}^{DE,\neg b}$	0.104**	0.167***	0.230***	0.101**	0.163***	0.229***
	(0.0481)	(0.0495)	(0.0470)	(0.0481)	(0.0496)	(0.0471)
—, lag 4	0.110***	0.0910***	0.101***	0.109***	0.0916***	0.0991***
	(0.0376)	(0.0339)	(0.0355)	(0.0376)	(0.0341)	(0.0356)
Maturing loans $ml_{t,i}^{\neg b}$	–0.150***	–0.163***	–0.166***	–0.150***	–0.162***	–0.167***
	(0.0378)	(0.0380)	(0.0390)	(0.0378)	(0.0380)	(0.0391)
$Cris_t \times bigL_{t,i}$				0.0417	–0.0588	0.00104
				(0.0811)	(0.0831)	(0.0883)
$Cris_t \times lowC_{t-4,i}$				–0.0861	–0.232***	–0.0799
				(0.0936)	(0.0850)	(0.0830)
$Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$				–0.600**	–0.369	–0.541*
				(0.256)	(0.248)	(0.281)
Fixed effects	bank, time, worst industry (bad (t,i))					
Observations	25964	28155	28161	25964	28155	28161
Adj. R^2	0.2529	0.2570	0.2446	0.2533	0.2576	0.2449
Adj. R^2 (within)	0.0135	0.0146	0.0165	0.0139	0.0155	0.0169

All variables as in Table 4, except $bigL$ and the interaction with $lowC$ and crisis dummy. 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 (columns 1 and 4), quarterly samples across banks (columns 2 and 5), and the pooled sample (columns 3 and 6). Total period: 2002Q4–2017Q4. The year 2007 is excluded for data reasons. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

Table 9 presents results from the base case (column 1) and the matching samples that include 1, 3, or 5 nearest neighbors ($bigL = 0$) of each observation with $bigL = 1$. Columns 2–4 show that a reduction to one matching observation reduces the main coefficient both in size and significance but still preserves the results of the base case. The more neighbors we include the closer we get to the baseline effect. Basically the same is found if crisis dummies are included (columns 5–8), with the exception that the role of low capital is less clear than before. Altogether, we consider this robustness test particularly tough and, consequently, its outcome to be the most important confirmation of the main result.

Removing 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

Table 9: Using a balanced sample based on propensity score matching

Dependent: $n_{t,i}^{\neg b}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	Full		PSM		Full		PSM	
Nearest neighbors:		1	3	5		1	3	5
$bigL_{t,i}$	–0.19***	–0.11**	–0.15***	–0.17***	–0.20***	–0.12*	–0.15***	–0.16***
	(0.038)	(0.054)	(0.043)	(0.041)	(0.047)	(0.069)	(0.05)	(0.05)
$lowC_{t-4,i}$	–0.20***	–0.28*	–0.223**	–0.23***	–0.17***	–0.198	–0.177	–0.19*
	(0.054)	(0.144)	(0.104)	(0.085)	(0.06)	(0.175)	(0.12)	(0.10)
$bigL_{t,i} \times lowC_{t-4,i}$	–0.085	–0.060	–0.115	–0.072	0.098	0.0432	0.061	0.125
	(0.116)	(0.18)	(0.140)	(0.129)	(0.138)	(0.215)	(0.171)	(0.16)
$Cris_t \times bigL_{t,i}$					0.042	0.002	0.010	–0.002
					(0.081)	(0.117)	(0.10)	(0.09)
$Cris_t \times lowC_{t-4,i}$					–0.086	–0.205	–0.147	–0.116
					(0.09)	(0.29)	(0.19)	(0.156)
$Cris_t \times bigL_{t,i} \ldots$					–0.60**	–0.36	–0.59*	–0.65**
$\times lowC_{t-4,i}$					(0.256)	(0.400)	(0.31)	(0.29)
...	...	...	...	...	...	...	...	...
Observations	25964	5399	8940	11343	25964	5399	8940	11343
Adj. R^2	0.253	0.251	0.252	0.255	0.253	0.252	0.25	0.257
Adj. R^2 (within)	0.0135	0.027	0.020	0.021	0.014	0.028	0.020	0.023

All variables as in Table 4 (base case in column 1 and interaction with crisis dummy in column 5). In the other columns, the control sample ($bigL = 0$) matches time perfectly and other variables based on a propensity score: First, the score is estimated from the pooled sample. Then, each observation of a "treated" bank in a quarter is assigned those 1, 3, or 5 observation(s) of "untreated" banks in the *same* quarter whose propensity scores are closest to that of the "treated" bank. Control variables left out 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 bad (t, i) where the quarter's largest loss has occurred. Total period: 2002Q4–2017Q4. 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.

of big losses of other banks' loans to the same industry. Those banks that actually suffer a big loss such that *bigL* is 1 tend to reduce their lending to the other industries just like the bank of interest, and so will the benchmark bank, as a linear aggregate 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[27]:

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

[27] 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.

This idiosyncratic component replaces the original value change at the instances where $c^{\text{bad}}_{t,i}$ and $bigL_{t,i}$ are constructed. The procedure decouples big losses from systematic factors, and the variation of new lending coming from the latter is turned into noise.

Using this idiosyncratic version of $bigL$ reduces the main coefficient by 20% (column 2 of Table 17 in Appendix G), which seems to confirm once again that big losses are predominantly idiosyncratic by nature but also points to a role for systematic factors. If we have not missed important further mechanisms that might bias our base case estimate, 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.

Removing autocorrelation in losses

Autocorrelation in the time series of c^{bad} 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 c^{bad} 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

$$c^{\text{bad}}_{t,i} = \alpha_i + \beta_1 c^{\text{bad}}_{t-4,i} + \varepsilon_{t,i} \,.$$

Comparing columns 1 and 3 of Table 17, we observe a minor reduction of the main coefficient from 1.90 to 1.71 but avoid to interpret the sign of this change as 3,400 observations are lost when we calculate the residuals.

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 10, we find the results to be robust against a variation in the loss tail probability between 4% and 40%. That the coefficient of $bigL$ reaches its maximum somewhere in the vicinity of 8% roughly corresponds with the shape of $n^{-\text{b}}_{t,i}$ 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 $lowC$ is larger, the deeper we go into the lower distribution tail of the capital ratio. Confusingly, the interaction term turns significant for a tail probability of 40% (with an unexpected positive sign). However, we are hesitant to call this event a “tail” of the distribution.

Time horizon

The next test concerns the horizon over which the bank is measured to adapt its lending business. Column 1 of Table 11 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

Table 10: Varying the tail probability of losses and capital ratios

Dependent: $n^{\neg b}_{t,i}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tail probability (%):	2	4	6	8	10	15	20	30	40
Panel A: Varying loss tail probability									
$bigL_{t,i}$	–0.0100	–0.171**	–0.209***	–0.204***	–0.190***	–0.173***	–0.159***	–0.141***	–0.0969**
	(0.138)	(0.0668)	(0.0503)	(0.0432)	(0.0381)	(0.0326)	(0.0307)	(0.0321)	(0.0389)
$lowC_{t-4,i}$	–0.246***	–0.250***	–0.210***	–0.209***	–0.201***	–0.270***	–0.296***	–0.229**	–0.176
	(0.0454)	(0.0478)	(0.0502)	(0.0523)	(0.0545)	(0.0603)	(0.0697)	(0.0939)	(0.133)
$bigL_{t,i} \times lowC_{t-4,i}$	–0.427	–0.0387	0.000478	–0.0357	–0.0854	0.100	0.107	–0.0127	–0.0303
	(0.315)	(0.172)	(0.151)	(0.124)	(0.116)	(0.101)	(0.0954)	(0.104)	(0.138)
...	...	...	...	...	...	...	...	...	...
Observations	33213	31154	29310	27585	25964	22441	19998	17553	18048
Adj. R^2	0.2494	0.2496	0.2514	0.2547	0.2529	0.2587	0.2505	0.2412	0.2344
Adj. R^2 (within)	0.0139	0.0135	0.0130	0.0139	0.0135	0.0165	0.0157	0.0187	0.0158
Panel B: Varying tail probability of capital ratio									
$bigL_{t,i}$	–0.199***	–0.197***	–0.188***	–0.190***	–0.190***	–0.191***	–0.184***	–0.211***	–0.275***
	(0.0367)	(0.0371)	(0.0374)	(0.0378)	(0.0381)	(0.0389)	(0.0402)	(0.0413)	(0.0438)
$lowC_{t-4,i}$	–0.222*	–0.231***	–0.112	–0.189***	–0.201***	–0.140***	–0.0575	–0.0712**	–0.117***
	(0.130)	(0.0869)	(0.0712)	(0.0618)	(0.0545)	(0.0445)	(0.0393)	(0.0341)	(0.0324)
$bigL_{t,i} \times lowC_{t-4,i}$	–0.0703	–0.0950	–0.186	–0.115	–0.0854	–0.0567	–0.0781	0.0315	0.172**
	(0.276)	(0.186)	(0.154)	(0.129)	(0.116)	(0.0979)	(0.0840)	(0.0761)	(0.0702)
...	...	...	...	...	...	...	...	...	...
Observations	25964	25964	25964	25964	25964	25964	25964	25964	25964
Adj. R^2	0.2524	0.2526	0.2524	0.2528	0.2529	0.2526	0.2523	0.2523	0.2527
Adj. R^2 (within)	0.0127	0.0131	0.0128	0.0133	0.0135	0.0131	0.0127	0.0126	0.0131

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 bad (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 11: Different time horizons for new lending business

Dependent: $n^{\neg b}_{t,i}$	(1)	(2)	(3)	(4)	(5)	(6)
Benchmark bank:	Base case variables			With crisis effects		
Horizon h in quarters:	1	4	8	1	4	8
Big loss $bigL_{t,i}$	–0.0369**	–0.190***	–0.298***	–0.0329*	–0.202***	–0.394***
	(0.0154)	(0.0381)	(0.0653)	(0.0185)	(0.0473)	(0.0862)
Low capital $lowC_{t-4,i}$	–0.0372*	–0.201***	–0.483***	–0.0379	–0.174***	–0.382***
	(0.0221)	(0.0545)	(0.0889)	(0.0250)	(0.0614)	(0.104)
$bigL_{t,i} \times lowC_{t-4,i}$	–0.0410	–0.0854	–0.0579	–0.0388	0.0980	0.0326
	(0.0558)	(0.116)	(0.196)	(0.0672)	(0.138)	(0.257)
New lending, lag 4 $n^{\neg b}_{t-4,i}$	–0.00670	0.0247***	0.0206**	–0.00671	0.0246***	0.0210**
	(0.00611)	(0.00608)	(0.00838)	(0.00611)	(0.00608)	(0.00839)
Benchm. (county) $n^{\text{cty},\neg b}_{t,i}$	–0.0779***	–0.150***	–0.371***	–0.0779***	–0.150***	–0.371***
	(0.0129)	(0.0378)	(0.0644)	(0.0129)	(0.0378)	(0.0645)
—, lag 4	0.0125	0.0317***	0.0357**	0.0125	0.0318***	0.0361**
	(0.00767)	(0.0112)	(0.0180)	(0.00767)	(0.0112)	(0.0181)
Benchm. (DE) $n^{\text{DE},\neg b}_{t,i}$	0.00593	0.00133	0.0156	0.00594	0.00138	0.0161
	(0.00827)	(0.00907)	(0.0126)	(0.00827)	(0.00908)	(0.0126)
—, lag 4	0.234***	0.104**	0.472***	0.234***	0.101**	0.470***
	(0.0408)	(0.0481)	(0.0572)	(0.0408)	(0.0481)	(0.0573)
Maturing loans $ml^{\neg b}_{t,i}$	0.0490	0.110***	0.0429	0.0490	0.109***	0.0440
	(0.0364)	(0.0376)	(0.0435)	(0.0364)	(0.0376)	(0.0436)
$Cris_t \times bigL_{t,i}$				–0.0124	0.0417	0.247*
				(0.0332)	(0.0811)	(0.132)
$Cris_t \times lowC_{t-4,i}$				0.00209	–0.0861	–0.259*
				(0.0415)	(0.0936)	(0.144)
$Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$				–0.00717	–0.600**	–0.288
				(0.122)	(0.256)	(0.401)
Fixed effects	bank, time, worst industry (bad (t, i))					
Observations	28599	25964	21361	28599	25964	21361
Adj. R^2	0.1114	0.2529	0.3764	0.1114	0.2533	0.3766
Adj. R^2 (within)	0.0123	0.0135	0.0327	0.0122	0.0139	

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 $h = 1, 4, 8$ quarters. Column 2 coincides with column 1 of Table 3. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

3), the quantitative lending effect of *bigL* rises by 60–100%, which documents that reactions are not 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. Including the crisis dummy and interaction terms (columns 4–6) does not change the picture; we note, however, that the triple interaction of crisis, big loss, and low capital is not significant for new lending over a horizon of one or eight quarters.

Varying fixed effects

In Table 12, 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 12: Varying fixed effects

Dependent: $n^{\neg b}_{t,i}$	(1) Base case	(2)	(3)	(4)	(5)	(6)	(7)
$bigL_{t,i}$	–0.19***	–0.23***	–0.20***	–0.19***	–0.20***	–0.22***	–0.19***
	(0.038)	(0.0367)	(0.039)	(0.038)	(0.038)	(0.037)	(0.039)
$lowC_{t-4,i}$	–0.201***	–0.203***	–0.0682	–0.062	–0.204***	–0.200***	–0.0637
	(0.0545)	(0.054)	(0.044)	(0.044)	(0.054)	(0.0543)	(0.044)
$bigL_{t,i} \times lowC_{t-4,i}$	–0.0854	–0.0873	–0.166	–0.167	–0.0876	–0.0853	–0.160
	(0.116)	(0.116)	(0.118)	(0.119)	(0.116)	(0.116)	(0.118)
...	...	...	...	...	...	...	...
Bank FEs	yes	yes			yes	yes	
Time FEs	yes		yes		yes		yes
FEs of worst indu.	yes			yes		yes	yes
Observations	25964	25964	25964	25964	25964	25964	25964
Adj. R^2	0.2529	0.246	0.093	0.089	0.251	0.247	0.095
Adj. R^2 (within)	0.0135	0.0394	0.0662	0.0865	0.0134	0.0398	0.0661

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: 2002Q4–2017Q4. The year 2007 is excluded for data reasons. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

An alternative definition of new lending business

Next, we test an alternative to the definition of new business, which excludes value changes, as opposed to the formula (1) used so far:

$$n^{\text{alt}}_{t,i,j} \equiv \frac{ex_{t+4,i,j} - ex_{t,i,j} - \sum_{k=1}^{4} c_{t+k,i,j}}{TA_{t,i}}. \tag{15}$$

This definition puts weight on the pure action of bank managers, that is the pure loan contracting minus expiring loans. Column 4 of Table 17 in Appendix G shows that the main regression is quite insensitive to this modification: *bigL* has a 30% stronger effect under the alternative definition, while the R^2 is nearly the same.

Varying the measure of capital

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 13 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 use quarterly samples 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 simply 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 is 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*

Table 13: Varying measures of capital

Dependent: $n_{t,i}^{\neg b}$ (%)	(1)	(2)	(3)	(4)	(5)	(6)
Measure of capital	*lowC*	*CapRatio*	*CapBuffer*	*DtT*	*LowDtT*	*PoT*
Variable type	Dummy	Contin.	Contin.	Contin.	Dummy	Contin.
$bigL_{t,i}$	–0.190***	–0.197***	–0.204***	–0.238***	–0.192***	–0.196***
	(0.0381)	(0.0371)	(0.0690)	(0.0753)	(0.0395)	(0.0392)
Capital measure (lag 4)	–0.201***	0.0370***	0.0369***	0.0762***	0.00293	0.458
	(0.0545)	(0.00820)	(0.00813)	(0.0104)	(0.0653)	(0.494)
Interaction with *bigL*	–0.0854	–0.000116	0.00113	0.00948	–0.0960	–0.548
	(0.116)	(0.0122)	(0.0105)	(0.0135)	(0.112)	(0.610)
...	...	...	...	...	...	...
Observations	25964	25964	25964	24203	24203	24203
R^2	0.2529	0.2540	0.2540	0.2472	0.2450	0.2450
R^2_{within}	0.0135	0.0149	0.0149	0.0156	0.0127	0.0128

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 bad (t, i) where a quarter's largest loss has occurred. Standard errors in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level.

trouble[28] (DtT) used in column 4 brings buffer and risk to one scale by dividing the buffer through its standard deviation:

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

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

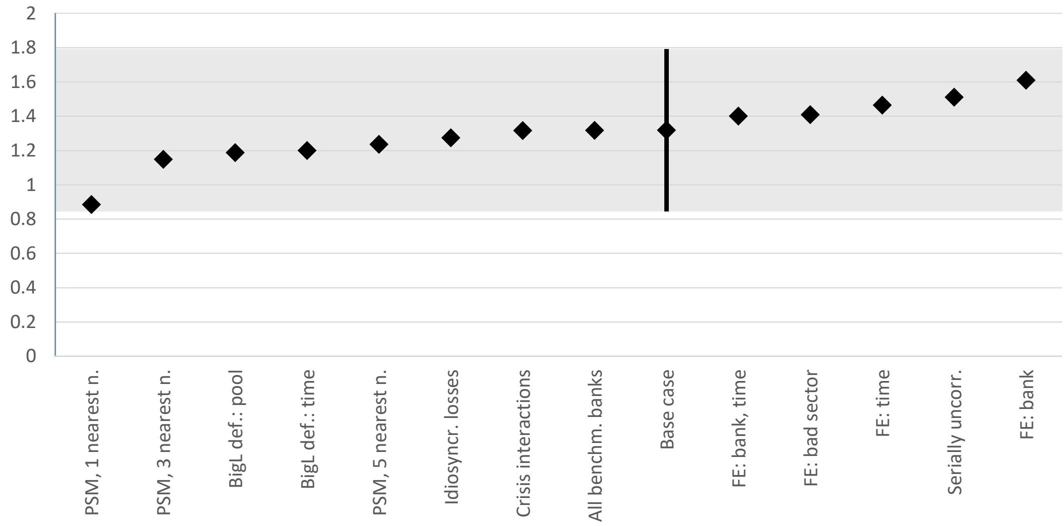
In column 5, we use the dummy for the smallest 10% of DtT values to test for a nonlinearity in the link between *DtT* and lending cuts. It is possible that banks start to take serious action only if the risk that capital falls under the regulatory minimum is really high.

In column 6, we refine this idea by the *probability of trouble* (PoT), defined as $\Phi(-DtT_{t,i})$, where Φ 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.

Table 13 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.

[28] 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.

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 bad (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") or not conditioned at all ("pool"). "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.

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 (1.32 euro less new lending for each euro lost in a substantial credit event), which we do now for various specifications tested in the last section.

Plotting these linear effects in Figure 3, we find all point estimates to be covered by the 95% confidence interval $[0.85, 1.79]$ spanned by the baseline estimate. We conclude that model details do not seem to 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. The few instances of a significantly negative sensitivity to their interaction – when big losses combine with low capital in crisis times – are not robust. As model uncertainty spans a slightly narrower range of plausible values than the estimation error, 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 0.85 and 1.80 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 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.

At bank level, the explanatory power of our measure of losses is quite small. At national level, however, the explanatory power of an aggregate estimation may become much higher.

We find only little evidence that other banks step in to make up for the lower credit supply of those banks that have suffered a large credit loss. In addition, big losses do not seem to trigger changes in other asset positions such as retail loans and various securities.

Finally, our new method to control for demand using benchmark banks successfully avoids the noise inherent in estimates of disaggregate relative changes while allowing for a lower aggregation level in the demand for control. Including two benchmark banks, one fitting locally and one nationally, we are able 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 bank and firm characteristics, the stance of monetary and macroprudential policy and market conditions and their relation to credit growth.

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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.

Denote by $ex^{\mathrm{BS}}_{t,i,j}$ the exposure of bank i to industry j at time t as reported in the borrower statistics (BS). This is an aggregate over the three maturity bands. Furthermore, denote by $ex^{\mathrm{CR}}_{t,i,j,r}$ the domestic part of the exposure of bank i to industry[29] 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 cell.

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.[30] 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:

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

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

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

[30] 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 the sector affiliation of a borrower 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 cell (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:

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

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 would like the current exposures to be increased. If loan demand is transformed into supply by a similar mechanism, for instance

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

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

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

and for all other banks:

$$\begin{aligned} n_{[\neg i],j,r} &= \frac{N_{[\neg i],j,r}}{TA_{[\neg i]}} = \frac{\omega_{j,r}\eta_{j,r}\, ex_{[\neg i],j,r}}{TA_{[\neg i]}} + \frac{\sum_{k\neq i}\omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_{[\neg i]}} \\ &= \omega_{j,r}\eta_{j,r} w_{t,[\neg i],j,r} + \frac{\sum_{k\neq i}\omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_{[\neg i]}}. \end{aligned}$$

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 $\nu_{i,j,r}$ [31] according to (7):

$$\begin{aligned} n^{\text{bm}}_{i,j,r} &= \nu_{i,j,r}\, n_{[\neg i],j,r} \\ &= \omega_{j,r}\eta_{j,r}\left(\nu_{i,j,r}\, w_{[\neg i],j,r}\right) + \nu_{i,j,r}\frac{\sum_{k\neq i}\omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_{[\neg i]}}. \\ &= \omega_{j,r}\eta_{j,r} w_{i,j,r} + \nu_{i,j,r}\frac{\sum_{k\neq i}\omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_{[\neg i]}}. \end{aligned} \tag{20}$$

$$\begin{aligned} n^{\text{bm}}_{i,j,r} &= \nu_{i,j,r}\, n_{[\neg i],j,r} \\ &= \omega_{j,r}\eta_{j,r}\left(\nu_{i,j,r}\, w_{[\neg i],j,r}\right) + \nu_{i,j,r}\frac{\sum_{k\neq i}\omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_{[\neg i]}}. \\ &= \omega_{j,r}\eta_{j,r} w_{i,j,r} + \nu_{i,j,r}\frac{\sum_{k\neq i}\omega_{j,r}\varepsilon_{k,j,r}\xi_{k,j,r}}{TA_{[\neg i]}}. \end{aligned}$$

Comparing (19) and (20), we see that the common proportional factor $\omega_{j,r}\eta_{j,r}$ of the demand shocks in cell (j, r) has the same weight in the new lending of bank i and its benchmark bank.

[31] Here we ignore the slight deviation of $\nu^*_{t,i,j,r}$ from $\nu_{t,i,j,r}$.

The aggregation over sectors and regions, which results in new lending at portfolio level, also preserves this congruence:

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

The bracketed terms are the same. Hence, $n_i^{reg,\neg \mathrm{b}}$ is able to absorb these proportional components of the demand shocks particularly well.

C Explanatory power

A simplified version of Equation (13) is:

$$
n_i = \gamma + \beta \, \mathrm{I}\left(c_i < \delta\right) + \varepsilon_i \tag{21}
$$

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

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

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):

$$
n = \gamma + \beta \, \mathrm{I}\left(c < \delta\right) + \varepsilon \tag{22}
$$

with

$$
\varepsilon = \sum_{i=1}^{N} m_i \varepsilon_i,
$$

where $m_i \equiv ex_i/ex$ is the market share of bank i concerning the credit volume. This holds because we can rewrite (21) 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 ε_i to be uncorrelated in the cross-section and obtain:

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

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 (22) would be

$$R^2 = \frac{R^2_{\mathrm{bl}}}{R^2_{\mathrm{bl}} + \mathit{HHI}\, \left(1 - R^2_{\mathrm{bl}}\right)}. \tag{23}$$

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

D Transforming the effect of *bigL* into an effect of losses

We want to transform the effect of the key dummy *bigL* back 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: $\mathit{bigL}_{t,i} = \mathrm{I}\left(c^{\mathrm{bad}}_{t,i} < \delta_i\right)$.[32] In the baseline regression (13), *bigL* occurs at two places:

$$n^{\neg \mathrm{b}}_{t,i} = \beta_1\, \mathit{bigL}_{t,i} + \beta_3\, \mathit{bigL}_{t,i} \times \mathit{lowC}_{t-4,i} + \ldots \tag{24}$$

The coefficient β_1 can be seen as the (additional) change in new lending in case $c^{\mathrm{bad}}_{i,t} < \delta_i$, compared to the complement $c^{\mathrm{bad}}_{i,t} \geq \delta_i$. The fact that *bigL* is insensitive to the variation of c^{bad} within each of these cases suggests to relate the coefficients to the following measure of variation:

$$\Delta \;\equiv\; E\left(c^{\mathrm{bad}}_{i,t} \;\middle|\; c^{\mathrm{bad}}_{i,t} < \delta_i\right) - E\left(c^{\mathrm{bad}}_{i,t} \;\middle|\; c^{\mathrm{bad}}_{i,t} \geq \delta_i\right).$$

So, as for the euro effect of c^{bad} captured by β_1 alone we would divide it by Δ. The fraction β_1/Δ is an effect "in euros of euros" because $n^{\neg \mathrm{b}}_{t,i}$ and $c^{\mathrm{bad}}_{t,i}$ are both normalized by total assets, which cancels out. For β_3 and the interaction term we have to take into account that $\mathit{bigL}_{t,i}$ has an effect only if $\mathit{lowC}_{t-4,i}$ equals one. The total linearized effect transmitted by both regression terms is then:

$$\widetilde{\beta} \equiv \frac{1}{\Delta}\left(\beta_1 + \beta_3\, E\left(\mathit{lowC}_{t-4,i} \;\middle|\; c^{\mathrm{bad}}_{i,t} < \delta_i\right)\right), \tag{25}$$

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 Σ; the variation of other components of $\widetilde{\beta}$ is neglected. With $H \equiv \left[1/\Delta, E\left(\mathit{lowC}_{t-4,i} \;\middle|\; c^{\mathrm{bad}}_{i,t} < \delta_i\right)/\Delta\right]$, 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 $\mathit{bigL}_{t,i} \times \mathit{Cris}_t$ and the triple interaction $\mathit{bigL}_{t,i} \times \mathit{lowC}_{t,i} \times \mathit{Cris}_t$ in the calculation of the total effect.

[32] The cutoff point δ_i is the 10% quantile of the bank specific sample $\left\{c^{\mathrm{bad}}_{t,i}\right\}_{t=1,\ldots,T}$.

Table 14: 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	
$bigL_{t,i}$	–0.0136	–0.0188	–0.0109	–0.0474	0.00001	–0.0004
	(0.0108)	(0.0147)	(0.0325)	(0.0373)	(0.002)	(0.002)
$lowC_{t-4,i}$	–0.0279*	–0.0407**	–0.0352	0.0266	–0.006***	–0.005***
	(0.0153)	(0.0171)	(0.0418)	(0.0468)	(0.00175)	(0.0021)
$bigL_{t,i} \times lowC_{t-4,i}$	–0.0827**	–0.0515	–0.0784	–0.104	0.00258	0.00308
	(0.0371)	(0.0476)	(0.0957)	(0.114)	(0.00388)	(0.005)
$Cris_t \times bigL_{t,i}$		0.0136		0.111		0.00132
		(0.0213)		(0.0739)		(0.003)
$Cris_t \times lowC_{t-4,i}$		0.0390		–0.178***		–0.002
		(0.0277)		(0.0681)		(0.003)
$Cris_t \times bigL_{t,i} \times lowC_{t-4,i}$		–0.0869		0.0502		–0.00196
		(0.0779)		(0.215)		(0.008)
...	...	...	...	...	...	...
Observations	15698	15698	25491	25491	25480	25480
Adj. R^2	0.0978	0.0979	0.2269	0.2272	0.0830	0.0829
Adj. R^2 (within)	0.0294	0.0295	0.0135	0.0138	0.0142	0.0141

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 the dependent variable's respective sector, which is bad (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: 2002Q4–2017Q4. 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.

E The effect of *bigL* on further loan sectors

In this appendix we present detailed results on the impact of *bigL* and *lowC* on lending to the troubled sector bad (t, i) and the two non-corporate sectors contained in our dataset, non-profit organizations (NPOs) and retail.

In columns 1 and 2 of Table 14 we run a regression of the exposure change in industry bad (t, i) with controls closely corresponding to the base case, except for a further lag term (two years) of new lending. It is needed to capture a negative bouncing effect of temporary shocks to loan levels. We find no effect of a big loss on lending to the troubled industry. Sensitivities are weak and partly at a questionable significance level.

Estimates for NPOs and the retail sector (columns 3–6) differ from the base case in that lending variables cover one of the non-corporate sectors only. Taking j to be either the NPO or retail sector, we estimate:

$$\begin{aligned} n_{t,i,j} \quad = \quad & \beta_1\, bigL_{t,i} + \beta_2\, lowC_{t-4,i} + \beta_3\, bigL_{t,i} \times lowC_{t-4,it,i} + \beta_4\, n_{t-4,i,j} + \beta_5\, ml_{t,i,j} \\ & + \beta_6\, n^{\text{cty}}_{t,i,j} + \beta_7\, n^{\text{cty}}_{t-4,i,j} + \beta_8\, n^{\text{DE}}_{t,i,j} + \beta_9\, n^{\text{DE}}_{t-4,i,j} + \alpha^{\text{bk}}_i + \alpha^{\text{qrt}}_t + \alpha^{\text{ind}}_{\text{bad}(t,i)} + \varepsilon_{t,i,j}\,. \end{aligned}$$

As the lending variables are normalized by total assets as before, β_1 informs us about potential cuts relative to the bank's overall exposure. It is therefore directly comparable to β_1 in (13) and includes the extensive margin but is not necessarily informative about the relative change within a sector.

F Construction of a matching control sample

The procedure generates the matching control sample used in the estimates for Table 9. It consists of two steps.

First, we try to predict the "treatment" *bigL* in a probit model. The variables suspected of at least some predictive power are the bank's log total assets (lagged by four quarters), the share of maturing loans, and the regulatory Tier-1 capital ratio (lag 4), quarterly centered by the median across banks. We furthermore include dummies for seven bank types[33] as a proxy for the business model and dummies for the industry in which the bank's biggest loss has occurred.[34] In this prediction exercise, carried out for the pooled sample, the maturing loans, capital, and ten of the industry dummies are significant at 5% or better but neither of the bank types, which is not surprising as the base case *bigL* is inevitably of equal frequency in each bank's history. With a pseudo R^2 of 0.2%, the model has weak power only, which is good because the "treatment" is more likely to be idiosyncratic by nature, the worse it is to predict.

Second, we go through all quarters t and select, for every bank i with $bigL_{t,i} = 1$, either one, three, or five other bank(s) with $bigL_{t,i'} = 0$ whose propensity score (the predicted treatment probability) is closest to that of bank i in this quarter. These contemporaneous "untreated" nearest neighbors constitute the control sample. It balances differences in the treatment frequency between different quarters perfectly, while other variables, as post-matching tests indicate, are balanced very well, with the exception of one bank type: no balance can be achieved for the small group of Landesbanken.

Using different samples for steps 1 (pooled score) and 2 (quarterly matching) avoids disadvantages of either approach. On the one hand, quarterly estimates of the propensity score are no option because they have proven to be too unstable. On the other hand, selecting nearest neighbors from a pooled sample would raise endogeneity issues[35].

G Supplementary tables

[33] The types are commercial and universal banks, mortgage banks, cooperative banks, savings banks, Landesbanken, building associations, and special banks.

[34] $D_{t,i,j} \equiv \mathrm{I}\left(\mathrm{bad}\left(t,i\right) = j\right)$; see footnote 13 on page 12.

[35] 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. The pooled approach would also have to account for the varying treatment frequency through time but would probably not end up with a perfect matching of quarters, unlike our approach.

Table 15: 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 (12), 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 16: Correlations of key variables

Variable	$n_{t,i}^{\neg b}$	~, L4	$n_{t,i}^{cty,\neg b}$	~, L4	$n_{t,i}^{distr,\neg b}$	~, L4	$n_{t,i}^{state,\neg b}$	~, L4	$n_{t,i}^{DE,\neg b}$	~, L4	$ml_{t,i}^{\neg b}$
$n_{t,i}^{\neg b}$	1										
~, L4	0.215	1									
$n_{t,i}^{cty,\neg b}$	0.116	0.090	1								
~, L4	0.107	0.145	0.138	1							
$n_{t,i}^{distr,\neg b}$	0.153	0.103	0.450	0.229	1						
~, L4	0.141	0.206	0.187	0.539	0.295	1					
$n_{t,i}^{state,\neg b}$	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			
$n_{t,i}^{DE,\neg b}$	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	
$ml_{t,i}^{\neg b}$	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 7, column 5. While $n_{t,i}^{\neg b}$is the new lending of bank i, the other $n_{t.i}^{reg,\neg b}$ are lending variables of benchmark banks that replicate the portfolio composition ob bank i at some level in the hierarchy of regions, from cty (county) via distr (district) and state to DE (whole Germany). L4 denotes a lag of 4 quarters.

Table 17: Further robustness tests

Dependent: $n_{t,i}^{\neg b}$ Setup:	(1) Base case	(2) Idiosyncratic losses	(3) Loss residuals	(4) Alternative new lending
Big loss $bigL_{t,i}$	–0.190***	–0.153***	–0.173***	–0.247***
	(0.0381)	(0.0328)	(0.0346)	(0.0332)
Low capital $lowC_{t-4,i}$	–0.201***	–0.258***	–0.221***	–0.193***
	(0.0545)	(0.0504)	(0.0569)	(0.0471)
Interaction $bigL_{t,i} \times lowC_{t-4,i}$	–0.0854	–0.153	–0.0303	0.0342
	(0.116)	(0.104)	(0.122)	(0.103)
New lending, lag 4 $n_{t-4,i}^{\neg b}$	0.0247***	0.0355***	0.0279***	0.0215***
	(0.00608)	(0.00526)	(0.00638)	(0.00504)
Benchm. (county) $n_{t,i}^{cty,\neg b}$	0.0317***	0.0222**	0.0270**	0.0338***
	(0.0112)	(0.00974)	(0.0114)	(0.00930)
—, lag 4	0.00133	–0.0104	0.00196	–0.00574
	(0.00907)	(0.00792)	(0.00973)	(0.00783)
Benchm. (DE) $n_{t,i}^{DE,\neg b}$	0.104**	0.212***	0.109**	0.0799*
	(0.0481)	(0.0396)	(0.0513)	(0.0422)
—, lag 4	0.110***	0.0974***	0.101**	0.0938***
	(0.0376)	(0.0340)	(0.0405)	(0.0332)
Maturing loans $ml_{t,i}^{\neg b}$	–0.150***	–0.137***	–0.154***	–0.127***
	(0.0378)	(0.0269)	(0.0386)	(0.0323)
Fixed effects	bank, time, worst industry (bad (t,i))			
Observations	25964	37020	22609	25732
Adj. R^2	0.2529	0.2622	0.2632	0.2438
Adj. R^2 (within)	0.0135	0.0170	0.0141	0.0138

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 $c_{t,i}^{bad} = \alpha_i + \beta_1 c_{t-4,i}^{bad} + \varepsilon_{t,i}$, which includes the only lag that has turned out significant in a more extensive regression on multiple lags of c^{bad}. We return to the original *bigL* in column 4, whereas new lending business (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.