The Real Effects of Bank Distress:

Evidence from Bank Bailouts in Germany

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This version: December 31, 2017

Abstract

How does bank distress impact their customers' probability of default? We address this question by looking at a unique sample of German firms from 2000 to 2012. We follow their firm-bank relationships through times of crises and distress. We find that a bank bailout leads to a bank-induced increase in the firms' probability of default. This effect mainly stems from bailouts during the 2008-09 recession. We further find that the direction and magnitude of the effect depends on firm quality and the relationship orientation of banks.

Keywords: bank distress, bank risk channel, relationship banking, firm probability of default, financial crisis, evergreening

JEL Classification: G01, G21, G24, G33

We thank Allen Berger, Michiel van Leuvensteijn, Xinming Li, Georg Licht, Oliver Rehbein, Klaus Schaeck, Otto Toivanen, Steven Vanhaverbeke, seminar participants at KU Leuven, University of Surrey and ZEW, as well as conference participants at the Portsmouth-Fordham Conference on Banking & Finance, the EBA Research Workshop (London), the DNB-EBC-CEPR Conference (Amsterdam), the Midwest Finance Association Conference (Chicago), and the Financial Management Association Conference (Boston) for useful comments. The views and opinions expressed in this article are those of the authors and do not necessarily reflect the views of the Deutsche Bundesbank.

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1 Introduction

The global recession of 2008-09 has shown that banks trigger and amplify shocks to the real economy. This paper studies how firms' default risk is affected when their banks get into distress. We examine whether the generated effects are different when bank distress happens in normal times or when a systemic crisis hits the banking sector. Besides, we analyze whether the banks' relationship orientation has different treatment effects on firms. We furthermore investigate whether relationship-oriented banks that are in distress generate differential impacts depending upon whether bank distress is idiosyncratic in nature versus more systemic. We use detailed bank-firm level micro-data from Germany, a bank-based economy, to study how bank distress impacts on firms' default probabilities and credit availability.

Banks play an important role in providing credit and liquidity to the economy (Krahnen and Schmidt, 2004). Shocks to bank liquidity or impairments of their balance sheet translate into the real economy if firms cannot easily turn to alternative financing sources. We investigate how bank distress impacts a firm's probability of default (PD) and recommended maximum loan amount, as perceived by an independent credit rating agency. We also analyze how firm sales are affected if a bank gets into distress. We examine how bank distress transmits to firms with different default probabilities, and whether the relationship orientation of banks mitigates the potential negative impacts on firms. Finally, we investigate whether the impacts depend on whether a bank distress event is idiosyncratic in nature or happens in times of a systemic banking crisis.

We apply recent methods used in the literature on the transmission of shocks to identify a "*bank risk channel*". Banks affect firm risk through several factors, such as whether credit is granted or not, the loan amount, other loan conditions or the general quality and extent of services provided. We classify supply related factors affecting firm risk as the *bank risk channel*. We also control for what we call the "*firm risk channel*" which captures demand related factors affecting firm risk such as a firm's industry, general economic conditions, the institutional environment the firm faces as well as a firm's idiosyncratic risk. To separate the bank-risk channel and the firm-risk channels, we apply the methods employed to disentangle supply and demand for loans (e.g. Khwaja and Mian, 2008; Morais, Peydró, and Ruiz 2016; or Degryse et al. 2016) to a setting of risk transmission in bank-firm relationships. In this way, we study real effects of bank distress.

We also study whether the *bank risk channel* following bank distress differs depending upon whether bank distress is idiosyncratic or systemic in nature. In particular, we investigate whether the 2008-2010 banking crisis had different effects that go beyond the usual adjustments when banks are distressed. In times of financial crises, banks may find it necessary (or be mandated by the regulator) to change their lending policy and make their loan decisions less opaque. This change might go beyond adjustments in loan characteristics such as interest rates and collateral requirements but constitute a structural change in the bank's lending policy.

We investigate whether distressed banks adjust the riskiness of their loan portfolio and whether bank distress has impacts on firms' PD. Specifically, we ask how distressed banks deal with the risk composition of their loan portfolios. Banks may change their lending practices and put even low to medium risk firms subject to tighter and more variable loan conditions. This may lead to an increase in perceived firm riskiness even for firms that have a viable financial condition. In contrast, banks in distress may loosen their credit standards, provide soft loan terms, and in this way "evergreen" the more risky borrowers in a bet to reduce potential losses on them (Peek and Rosengren, 1997) or comply with local political guidelines (Gropp et al., 2010). If "evergreening" is in place, we expect PDs to decrease due to the application of more generous loan policies. Because the impact of banks' strategies might differ from normal times compared to when a systemic crisis is in place (Degryse et al., 2013; Ivashina and Scharfstein, 2010), we differentiate between normal times and times of crisis in the analysis.

We combine several unique datasets to tackle these questions. First, we employ the Mannheim Enterprise Panel[1] (MUP) which covers for almost any German non-financial entity an individual credit rating, its bank-firm relationships[2] and other firm-specific information between 1999 and 2013. Second, we combine the information on the bank names with regulatory and bank balance sheet data from Deutsche Bundesbank in order to identify banks in distress. Third, we obtain information from MUP such as banks' regional or industry-

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[1] The Mannheim Enterprise Panel (*Mannheimer Unternehmenspanel* – MUP) of the Centre for European Economic Research (ZEW) is the most comprehensive micro database of companies in Germany outside the official business register (which is not accessible to the public). The MUP is based on the firm data pool of Creditreform e.V., which is the largest credit rating agency in Germany.

[2] We know up to six bank relationships for firms. The first bank is declared by Creditreform as the firm's main bank or "Hausbank".

specific market and portfolio shares, default rates in corporate banking or relationship orientation measures.

The literature on financial intermediation has put a lot of emphasis on the link between firms and banks when firms are in financial distress. A prominent question of interest is whether especially relationship-oriented banks help in smoothing out credit constraints that firms face (e.g. Berger and Udell, 1995; Berger and Udell, 2002). Bolton et al. (2016) build a model where relationship banks compete with transaction banks and conclude that whilst relationship banks charge higher rates in normal times, they are able to supply continued lending at more favorable terms in times of crisis. Firms that depend more on the business cycle therefore prefer to engage with relationship banks. An assessment of Italian loan-level data confirms these predictions. Beck et al. (2016) study the role of banks' business models on firms' credit constraints in normal and crisis times. They find that firms with more relationship oriented banks in their vicinity have a lower probability of experiencing credit constraints during economic downturns.

In studies that analyze credit supply shocks, the above arguments usually are referred to as the so called *bank lending channel* (e.g. Gambacorta, 2005; Kishan and Opiela, 2000, Kwaja and Mian, 2008; Nilsen, 2002). Though we do not analyze the supply and demand for loans, we also want to make sure to differentiate between firm-related and bank-related changes in the PD. In Khwaja and Mian (2008), firm-related changes in demand are termed *firm borrowing channel*. In our environment, the term *firm risk channel* is the more appropriate, which we distinguish from a *bank risk channel.* Specifically, we apply a clustering method similar to Degryse et al. (2016). In this way we introduce firm-year-fixed effects in the sense of Kwaja and Mian (2008) even when we observe single bank relationship customers and the outcome variable is on the firm-year level. In the environment of PDs, this will enable to cancel out yearly industry, regional, age and firm size effects on PDs that arise in the economy.

Our work mostly builds up on the stream of literature dealing with the transmission of shocks from the financial industry into the real economy (e.g. Peek and Rosengren, 1997; Kishan and Opiela, 2000; Nilsen, 2002; Gambacorta, 2005; Khwaja and Mian, 2008; Amiti and Weinstein, 2009; Loutskina and Strahan, 2009, Santos, 2010; Puri et al., 2011; Jiménez et al., 2012; De Haas and Van Horen, 2012a and 2012b, Chodorow-Reich, 2014). A second stream of literature relevant for this work is the literature on relationship banking and financial intermediation between firms and banks over the business cycle (e.g. Holmstrom and Tirole, 1997; Ivashina and Scharfstein, 2010; Bolton et al., 2013; Degryse et al., 2013; Beck et al., 2016). Our paper contributes to these two strands of literature by studying a unique indicator of real effects, i.e. the firms' probability of default, and identifying the role of banks' business models in this transmission.

Our paper generally contributes to the wide literature on information asymmetries between firms and their financial intermediaries on the one hand and the market on the other hand (Stiglitz and Weiss, 1981; Sharpe, 1990; Rajan, 1992; Petersen and Rajan, 1994; Berger and Udell, 1995; Boot and Thakor, 2000; Agarwal and Hauswald. 2010).

The remainder of this article is organized as follows: Section 2 presents relevant strands of the literature, sketches the banking and corporate environment in Germany and introduces the applied data sources and the empirical methodology used to address the research questions. In Section 3 results are shown and discussed. Section 4 concludes.

2 Data and Empirical Methodology

2.1 Data

2.1.1 Firm and bank level data

For the *firm and bank level data*, we use the *Mannheim Enterprise Panel (MUP)*, a panel dataset generated by Centre for European Economic Research (ZEW). It contains the complete data pool of Creditreform e.V. (on a half-yearly basis), the largest credit rating agency in Germany. The MUP is the most comprehensive micro database of companies in Germany next to the official Business Register of the Federal Statistical Office (which is not accessible to the public). Comparisons of MUP with the Business Register reveal that the coverage of MUP nearly represents the universe of firms in Germany. It therefore provides a representative picture of the corporate landscape in Germany. For detailed information about data collection, processing and definitions see Bersch et al. (2014).

The MUP contains a large number of firm characteristics. It includes firm size (annual sales, number of employed persons), industry (five-digit industry sector code according to NACE rev. 2), legal form, date of foundation and of closure, the company's complete address, shareholder structure and personal details about the involved persons. More importantly for our analysis, the data also includes Creditreform's credit rating score and information on the firms' banking relationships. The credit rating score is an index ranging from 100 to 600, showing the firm's credit rating for each panel year. The credit rating is translated into probabilities of default using a definition provided by Creditreform. The credit score has already been used in a number of recent papers (Hoewer, 2009; Brown et al., 2012; Cremers and Schliessler, 2014). The dataset includes up to six banking relationships of a company. The first relationship is denoted as the main bank (‘Hausbank‘), i.e. the bank used for day-to-day transactions, credit lines and which is most likely the firm’s main lender. Our analysis relies on the firm’s main bank relationship as it constitutes the prominent external financier for the firm.

Interestingly, the data from Creditreform also contains the identity of the bank’s branch that the company employs. The bank branches themselves are linked to the overall bank by the unique German bank identifier BLZ. Using this link, ZEW constructs a panel of all banks operating in Germany. By aggregating information on all firms connected to a particular bank, we are able to infer bank’s market shares or portfolio shares by region or industry. Moreover, we are able to derive rates of firm failures by bank that go beyond information provided in banks’ balance sheets.[3] The ZEW Bankpanel therefore gives a clear picture of the structure of the corporate banking sector in Germany.

2.1.2 Data on bank distress

Our second dataset concerns information on bank distress. We employ three sources. First, the German banking system contains three banking pillars (i.e. commercial banks, savings bank sector, and cooperative bank sector). Each banking pillar has a voluntary financed insurance fund operated by the respective bankers association that may provide ‘capital support’ when a bank within the pillar is in distress. While supervisors (i.e. BaFin and Bundesbank) may be consulted during the process, the final decision on granting capital support rests on the respective insurance schemes. The respective insurance scheme and the member bank sign a contract which includes the specific shortcomings of the troubled bank that need to be addressed and plans on how to resolve the distress. The insurance scheme usually gains far-reaching control rights if the member bank becomes distressed, in general going along with restructuring and deleveraging orders.[4] If capital support measures are still considered insufficient (maybe if the distressed bank has reached a stage in which recovery is no longer possible) bankers associations have the power to order restructuring mergers (also called “distressed mergers”) in the course of the resolution process.

[3] The individual relationship entering a bank’s portfolio may be weighted by its rank (main bank or not) as well as its PD or its number of employees.

[4] Bian et al. (2016), for example, find for German savings banks restructuring activities to be significantly higher in a bailout by the bankers association than in a bailout by politicians.

Second, at the end of 2008, as response to the financial and economic crisis, the Financial Market Stabilization Fund ("*Sonderfonds Finanzmarktstabilisierung*", SoFFin) was founded which complements the described voluntary measures by the banking industry. Even though SoFFin support has been only granted to a small number of major German banks these government bailout measures have been large in volume and may have thus significantly impacted the banking sector and caused competitive distortions (see Kick and Koetter, 2016). Third, in addition to the described measures, also supervisors can intervene. If BaFin and Bundesbank deem these measures inadequate or insufficient, they can also intervene according to the German Banking Act ("Kreditwesengesetz"). This includes severe interventions like moratoria or finally revoking the bank's charter.

The bankers associations' and the supervisors' decisions are not independent of each other, with various decision makers (BaFin, Bundesbank, bankers associations and the boards of the insurance schemes) involved. Even though the bailout process appears to be opaque, the interventions of the different stakeholders complement each other and constitute a kind of well-functioning "private-public partnership". For a detailed description of the protection schemes in the German banking sector see also Kick et al. (2016).

We apply the definitions of bank distress of Kick and Prieto (2013) who investigate the competition-stability nexus in the German banking system. They employ several definitions, among them distressed mergers (which are closest to outright bank defaults), capital support (capital injections and guarantees) by the banks' respective banking pillars.[5] Since outright default is a very rare event in Germany, we concentrate on capital injections. We use the initial capital injection for the bank such that it really constitutes a unique event for the bank.

2.2 Empirical Methodology

Our firm-level dataset contains information on the individual bank-firm relationship over the period 2000 to 2012. We focus on the main bank relationships. To investigate the treatment of "bank distress" on firms' outcomes (in particular their probability of default), only a selected sample of firms will be employed. The reason is that not all banks (and in turn their firms) are equally likely to receive the treatment.

We use *nearest neighbor matching of banks* in order to find an appropriate control group of banks which would have had a similar likelihood of receiving the treatment, but which have

[5] Kick and Prieto (2013) have a broader focus and deal also with other indicators of bank risk. In particular, they employ also continuous measures such as banks' Non-Performing Loans (NPL) ratios and Z-scores.

not received capital injections. Our method has to be distinguished from a standard matching approach, where the matching both serves to alleviate the bias of selection into treatment and to construct an adequate control group. In our setting, the problem of selection into treatment plays a subordinate role as the state of distress in banks can be assumed to be exogenous to an individual firm outcome. While one could argue that distress of large customers may trigger default in banks, the median firm in our sample has 6 employees. We further drop firms with more than 10,000 employees from the analysis. The matching rather serves as a device to obtain an appropriate control group of banks that can be traced over the same time span and has a similar likelihood of receiving the treatment. Therefore, we conduct the matching on the bank level and only later enrich the sample of nearest neighbors with firm data.

We match the treated banks (i.e. banks with a capital injection) with control banks at period t-1, i.e. one year before the initial capital support measure is conducted. We match with control banks that are non-treated neither in that year nor in any of the three subsequent years after the treatment (including the treatment year). The matching yields at least one control bank for every treated bank (initial capital support). In order to obtain more observations for the firm-level analysis in the second step, we allow for up to three nearest neighbors. We trace the neighbors throughout the sample time span and link them to the firms having firm-bank relationships to these banks.

A challenging feature of the German Banking Market is the occurrence of numerous bank mergers in almost any banking segment. The number of banks has decreased from 4,300 banks in 1990 to 2,700 in 2000, and 2,000 banks in 2010. Mergers are often a means to restructure a bank and prevent it from defaulting. Therefore, an initial capital support occurs more frequently before a merger compared to the situation where no merger takes places.. From an econometric point of view, mergers are difficult to deal with for two major reasons. First, they are a second treatment which is not independent from the first treatment. Second, the merger substantially impairs the conduction of a control group study because the bank before the merger will be different from the one afterwards.

There are two ways to handle these problems in the analysis. One way is to introduce a differentiated analysis by type of treatment, i.e. whether only treatment 1 (capital support) happens or treatment 1 is accompanied or followed by treatment 2 (the merger). The latter case will then be a different treatment effect that is estimated. Another way is to only look at treatment 1 and condition on a sufficient (e.g. 3 years) time span before treatment 2 happens. We would then only look at a maximum -3 to +3 years window (including the treatment year) before and after treatment 1. Such a methodology yields a valid estimation framework for a control group setting, since the treated bank is still structurally the same. As a matter of fact it has to be stated that this choice also limits the scope of our analysis because we cannot analyze cases where both treatment 1 and 2 occur.

We apply method 2 in our analysis. The sample of treated banks is therefore restricted to banks existing at least 3 years before and 3 years after the treatment as the same unit. As we want to follow firms in a window -3 to +3, treatments before 2003 are not taken into account, so are treatments taking place after 2010.

2.2.1 Nearest-Neighbor Matching

There is considerable heterogeneity between the treated banks stemming from the size of the capital injection (i.e. the intensity of the treatment). In order to reduce the heterogeneity within the treatment group, we split treated banks into two groups: one where banks encounter a large treatment (above median capital injection to equity ratio) and one where banks experience a weaker treatment (below median capital injection to equity ratio). Differences in the magnitude of treatment may require different control groups. We therefore estimate two models to obtain the propensity score and afterwards unite the two sets of treated and control banks to a joint sample. The split of the treatment group also ensures that we have more homogenous treatment groups and enables later distinguishing upon the size of the treatment. In order to find the nearest neighbors, we use observables in the year just before the treatment. Apart from a variety of observable characteristics of banks, we postulate the following fixed matching criteria:

1. Treatment and control observation are in the same year.
2. Treatment and control bank are localized in the same Bundesland (i.e.region).
3. At the year of evaluation, both have at least 3 years of observations before and after the matched point in time.
4. Treatment and control bank are of the same type (commercial bank, savings bank, cooperative bank).

The first and second restrictions guarantee that treatment and control bank face the same (regional) macroeconomic conditions. The third restriction leaves us with those banks that can be traced over a sufficient time span. Condition four accounts for the fact that most of the capital injections stem from bank deposit insurance schemes which are organized separately (“three pillars”). Condition 2 also helps to comply with supervision based on the level of the respective Bundesland.

The matching equation itself includes a variety of variables that are summarized in Bank balance sheet and bank income statement information comes from Deutsche Bundesbank Bank Supervisory Data. Aggregated Bank Customer information stems from the MUP. Table 2 shows the output of the matching regression where the dependent variable *affected bank* takes the value of 1 if a bank receives an initial capital injection in period t +1. Our results are in line with the literature. Size plays a prominent role as well as the amount of loans the bank has in place. As expected, the NPL ratio exerts a positive effect on the probability of receiving a capital injection. The reserves ratio is negatively associated with the likelihood of getting a capital injection while hidden liabilities[6] are positively associated. In general, effects are more pronounced for severe treatments.

The share of single relationship customers is negatively associated with receiving a capital injection. This is probably the case because the more intensely a bank is involved in customer relationships, the less involved it is in trading and investment banking activities and the less exposed it is to heavy write-offs or liquidity shocks. On the other hand, the share of customers within a 50km distance to the headquarters implies a regional concentration of customers. The bank is therefore less hedged against intra-regional shocks. In line with expectations, the variable is positively significant for severe treatments.

The matching regression yields a propensity score to receive an initial capital injection from banks' depository scheme in period t+1 given the characteristics of period t. The propensity score is scaled by bank type, the region of the headquarters as well as the year of observation such that we compare banks with the same business model and within the same macroeconomic environment. With the resulting scaled propensity score, we perform nearest neighbor matching.

Table 3 shows information on the propensity score matching by year of treatment. We obtain a sample of 76 banks, of which 23 banks are treated and 53 are untreated. For each of the 23 treated banks we have at least one and up to three control banks. The number of distress events varies considerably across years. Most events happen in the years 2003 to 2005. In 2007, one year before the global financial crisis, only 1 treatment can be observed, while the number increases again for the crisis years.

By comparing characteristics of treated and control banks we receive a picture of how relevant the treatment is. Figure 1 shows median bank covariates before and after the

[6] The liabilities are hidden for the public, but the supervisor knows them.

treatment for both treatment and control banks. Sample banks are on average small, with total assets reaching only 500 million Euros at the median. Treated and control banks show similar trends before the treatment period while after the treatment period, total assets increase only at control banks. Treated banks have to pay back the capital injection and may be under pressure to shrink balance sheets and build reserves in order to fulfil minimum capital requirements. However, the number of customers does not decrease for treated banks after the capital support which indicates that banks on average do not try to get rid of customers.

The second row of **Figure 1** shows the developments in the NPL ratio (obtained from Bundesbank Supervisory Data) and in the share of distressed customers (which stems from MUP-data). The two measures are highly related: every distressed customer will represent a non-performing loan but not necessarily vice-versa. Correspondingly, NPL ratios are naturally higher than customer default rates.

Before the treatment period, ratios of distressed customers rise for both treatment and control banks and develop nearly identically which may reflect generally worsening macroeconomic conditions. In the treatment period and afterwards, the ratios of distressed customers are higher at affected banks. However, the ratio of distressed customers seems to increase less than the NPL ratio and eventually returns to the same level as for control banks. The absence of higher rates in payment default may be interpreted as a tentative sign for banks' tendency to reduce balance sheet losses and evergreen customers.

Measures of banks' riskiness and return show a similar picture. The third row contains average growth in RWA (risk-weighted assets) on the left and ROE (returns on equity) on the right. For both measures, there is a strong downward trend for treated banks (approaching -7% in RWA-Growth and 0% ROE). Both figures, however, remain relatively stable at control banks. Overall, these measures point to difficult conditions at treated banks. They may therefore be under pressure to build up reserves and increase equity ratios. An improvement in capitalization can, indeed, be observed for treated banks (see bottom row in **Figure 1**). However, reserve ratios of treated banks remain substantially lower compared to control banks, possibly because banks first need to restore capital before being able to build up reserves.

To conclude, the graphs show that bank characteristics of treated and control banks evolve similarly in terms of trends and levels before and also, for non-performance related variables, after the treatment occurs. Performance-related measures indicate difficult conditions at distressed banks which should have significant effects on their customer portfolio.

2.2.2 Estimating Firm Outcomes using the Matched Bank Sample

After conducting nearest-neighbor matching, we obtain 74 banks consisting of 23 treated and 51 control banks. A bank may serve as a control bank more than once within the sample. We connect banks to firms through the firm's main bank relationship. As outlined in section 2.1, the main bank is the firm's most important external financier and our analysis therefore relies on this relationship.

We obtain about 267,000 observations stemming from about 50,000 individual firms. Table 4 shows the size of the compound sample by year of observation and year of treatment. Some firms may occur multiple times within the sample because two different treated banks may have the same control bank. We introduce the variable neighbor as an identifier which captures every matched set of bank neighbors. The dataset is therefore uniquely defined on the firm-bank-neighbor-year level. Firms in the sample are on average young (about 21 years) and small (about 7 employees and 2 to 2.5 million Euro in sales). Table 5 shows further firm characteristics comparing firms at treated and non-treated banks in the year before the treatment.

In order to capture the bank-induced effects (i.e. supply effects), we would ideally include firm fixed effects to control for firm-specific demand (e.g. Khwaja and Mian 2008)). In our setting this is impossible as we focus on the firm's main bank relationship. We therefore follow recent literature and replace the firm fixed effects by a grouping of firm observations where firms in one group face the same legal, macroeconomic, spatial and industrial environment (e.g. Degryse et al. 2016, Morais et al. 2016). These papers show that controlling for firm demand in this way hardly affects the estimated supply effects. The grouping we apply is on the level of *industry, size class, legal form, single-relationship (yes, no), age class, region* and *year* (see the Appendix for a detailed overview of the respective underlying classifications).

We further control for potential differences related to the organization of the credit rating agency. Creditreform is organized in 130 divisions across Germany. Each division is identified as part of the firm ID. We control for a combination of division and year because risk assessment may slightly differ across divisions. Furthermore, the rating methodology undergoes some regular revisions which might be implemented at different points in time by each division. Therefore we include division-year fixed effects.

2.2.3 Defining our Model

To sum up, we apply a nearest-neighbor matching approach for banks and we use group fixed effects for firms. We assume our treatment (i.e. capital injection to bank) to be exogenous to an individual firm's performance. First, the firms in our sample are on average small (90% of the sample firms have less than 50 employees). It is therefore unlikely that a single firm triggers a bank's capital injections. We also control for regional demand shocks both by the group fixed effects approach as well as the matching of banks which settles the estimation framework to the same macroeconomic environment. Second, banks are silent on the possibility of capital injections up to the moment they are indispensable. Given that we apply matching on bank performance covariates right before the treatment occurs, the treatment should not be foreseeable for customer-firms ex-ante. Therefore, we do not need to include any other firm or bank related characteristics for identification of the treatment effect. Robustness checks in Section 3.3 show that our results remain unaffected by the inclusion of a variety of firm and bank covariates.

The methodology we implement is a combination of a conditional difference in difference approach and a fixed effects approach. We want to estimate the impact of bank distress on firm outcomes, in particular firm PD (probability of default of firm i over one year evaluated by Creditreform). Like in any difference in difference setup, we need (in addition to an intercept on the right-hand side), i) the treatment dummy (*affected bank),* ii) the indicator for after-treatment periods (*post)* and iii) the interaction of both in order to represent our four states of the world. This interaction term shows the treatment effect, i.e. in our case how, for example, the PD of firms connected to banks in distress evolves compared to the average PD of firms connected to banks not in distress. Our final model therefore is specified as:

$$firm\ outcome_{i,t} = \beta_0 + \beta_{post} * post_{ik,t} + \beta_{affected} * affected_{ik,t} \qquad (1)$$

$$+\beta_{ATET} * affected_{ik,t} * post_{ik,t} + \rho_{gk,t} + \varepsilon_{igk,t}$$

i: firm, k: bank, g: group, t: time

Firm outcome may be, for example, firm PD or sales. Note that $\rho_{gk,t}$ is a group fixed-effect consisting of: industry, size class, age class, region, Creditreform division, matched banks, year.

Note that we drop the i, k and t subscripts for the components of $\rho_{gk,t}$ as they always refer to a specific combination of i,k and t. Further remark that $post_{ik,t}$ takes the value of 1 if firm i has relationship with bank k in period t and period t is after the treatment year (or the treatment year). The indicator $affected_{ik,t}$, takes the value of 1 if firm i has relationship with bank k in period t and bank k is a treated bank. Analogous holds for the interaction of both.

The group effect $\rho_{gk,t}$ serves to absorb demand side and business cycle effects associated to each group of firms that may influence firms' outcomes. The Creditreform division takes account for heterogeneous risk assessment methodologies across different Creditreform divisions and/or time. Finally, the indicator for the set of matched banks leaves us with an estimator of the treatment effect within the matched bank neighbor(s) stemming from the bank-level propensity score matching.

2.2.4 Estimating our Model

In order to estimate our model we choose a population-average GLM-estimator, also referred to as a generalized estimating equation (GEE). The GEE framework is often used in settings where the covariance structure of residuals is unknown. As GEE estimators are population-average models, they focus on the average effect over an unspecified population of individuals. They are frequently used to estimate average responses in clustered samples. Our setting with 130 different clubs evaluating the PD of firms seems to be exactly of such a kind. We do not know the covariance structure within the clusters but are still able to receive consistent estimates even if the covariance structure is misspecified. The estimator is similar to a random-effects Tobit regression with a Gaussian random-effect (Robustness Checks in Section 3.3 show that our results are confirmed using OLS, RE or Tobit regressions).

Other than in a genuine fixed- or random-effects setting, we do not take our firm identifier as panel and neither year as our time variable. Instead, a group identifier is our panel variable. Note that the timing of the observation, year, is part of the panel variable. The theoretical "time" variable is constituted by the individual firm-year observations that are part of group g in year t. We bundle the group identifier in a "fixed effect" $\widehat{\rho_{gk,t}}$ We assume exchangeable correlation structure of residuals within each group. This structure is a reasonable assumption since groups are narrowly defined and especially are constituted within each division unit.

Our final dataset consists of about 267,000 observations which represent about 50,000 individual firms, each over a period of up to 6 years. We follow firms in our matched sample 3 years before and 3 periods after the treatment (including the treatment year). There are a couple of reasons to do so. First, we choose a short period of time after treatment in order to capture the direct impact of the treatment and to make sure that our measurement is less likely to be contaminated by other influences. Second, there are substantial dynamics in firms' outcomes, at least in their yearly *PD.* Hence, the longer the time window the more of these yearly movements will overlay each other and keep us from getting a valid estimate of the treatment effect.

3 Empirical Results

This section presents results for our conditional difference in difference estimations of bank risk on firm outcomes, in particular their PD. Robustness checks are presented in Section 3.3 where we verify our results for the inclusion of other covariates and the choice of different regression techniques.

As a starting point, we apply the conditional difference-in-difference analysis on all firms and banks in our sample in order to identify a general bank-risk induced effect on a firm's PD (or another firm outcome variable see Section 3.1). In Section 3.2, we apply our model in (1) to different subsets of banks and firms that may yield insights into the heterogeneity of the treatment effect. We investigate whether the bank-risk induced effect depends on firm risk classes, the bank's business model (relationship versus transaction bank), firm industry, age and size. Moreover, we also examine whether the bank-risk induced effect on firm PD differs between crisis years and normal times. We are able to investigate these issues because of the grouping of observations instead of using genuine fixed-effects which still leaves us with some firm-level variation on the right hand side within each year.

3.1 Baseline Results

Table 6 shows the baseline GLM estimations on the full sample of firms and banks from 2000 to 2012. Specifications (A1) and (A2) show the results that serve to answer our first research question, i.e. whether there exists a bank-induced risk transmission effect from bank distress to customer firms. The coefficients are to be interpreted in percent. We find that the PD of customers at distressed banks raised on average by 12% after the treatment occurred than that of customers at control banks. With an average *PD* of about 10%, this means that the average probability of default of treated customers increased to about 11.2% which is a substantial increase.

The strong results are mainly driven by customers entering the worst rating classes (80% PD+) which is obvious when looking at specification (A2) that excludes customers who default within the sample period. However, also for non-defaulting customers, PD increases by 6.9% at treated banks. The importance of defaulting customers is confirmed by specification (A5) that estimates the probability of actual default using a FE-Probit regression framework. Customers at treated bank have a 6.8% higher probability of actually defaulting after the treatment which coincides with the results found in specification (A2).

Specifications (A3) and (A4) show results when using another dependent variable as an indicator: the variable *MAXLOAN*. Creditreform adds a maximum loan recommendation to most firms that are evaluated by them. So *MAXLOAN* serves as a benchmark to trade creditors on how much credit could be granted to the firm. The impacts on MAXLOAN provide us with another indicator of real effects for firms. The regression coefficients in (A3) and (A4) show that maximum loan recommendations go down on average by about 900 Euros, or about 8% in relative terms, when looking at the log values. Given that most firms in the sample are small firms, this constitutes a severe slump in their scope of operation. Finally, specification (A6) shows the impact of bank distress on firm sales. We find that bank distress leads to a decrease in firm sales by about 4%.

We visualize these effects by plotting the outcome variables for treated and untreated banks around the treatment year. In order to do that, we first estimated the models and then removed the fixed-components $\widehat{\rho_{gk,t}}$ in (1) from the outcome variables. The resulting adjusted values for *PD* and *MAXLOAN* are shown in Figure 2. We observe parallel trends for both *PD* and *MAXLOAN* for the three years before the treatment and afterwards a visible increase in *PD* and a decrease in *MAXLOAN*. Interestingly, we see differences in levels before the treatment for both variables, i.e. treated banks have on average better customers before the treatment than control banks. After the treatment occurs, the average *PD* of customers at treated banks approaches the level of control bank customers.

This observation may first seem surprising, as banks that go into distress may be expected to have lent also to on average worse firms. On the other hand, there are good reasons to believe that a bank’s turmoil does not originate in the domestic corporate sector but rather in other areas of their business such as real estate or their business abroad, especially in the crisis years. The observation actually fits to our basic assumption that credit rating agencies take firms' funding situation at their main bank into account and adjust credit ratings if lending conditions, collateral requirements and services quality at firms' main banks change. Credit rating agencies will somehow find out if banks provide excess funding to firms of a certain efficiency level, assigning better credit ratings as long as banks carry on supplying firms with loans and in particular current accounts. Furthermore, if banks running into distress had the strategy to keep inefficient contracts on their balance sheets, fewer firms were actually defaulting before (compare specification A5) and this also will be expressed in better average credit ratings.

For a more detailed picture of the effects, we now turn to an analysis of different macroeconomic conditions. Specifically, we want to answer the question whether distress events that happen during a systemic crisis have different impact on firms than distress events outside a systemic crisis. Furthermore, we shed light on the question whether relationship and transaction banks behave differently and investigate whether borrowers are differentially affected depending on their risk class as measured by the PD.

3.2 Relationship Banking, the Crisis, and Evergreening

In this section, we apply our model (1) to subsets of firms, stratifying the sample on the level of risk classes, bank characteristics and treatment years. We define crisis treatments to be treatments occurring in the peak of the financial crisis 2008 and 2009 and all other treatment years as non-crisis years. We employ indicators of a bank's relationship orientation from Bersch (2016). They are defined according to the composition of the customer portfolio of a particular bank along the arrays a) share of single relationship customers, b) share of main bank customers and c) customers within a 50km distance around headquarters. These measures were already included in the matching equation presented in Table 2. The share of single relationship customers is constructed as:

$$single\ share_{kt} = \frac{\sum_i I(bank_{it}=k) * I(\ singlerel_{it}==1)}{\sum_i I(bank_{it}=k)} \tag{2}$$

I.e. (2) calculates the sum of all customer firms of bank k who only have relationship with bank k over all customers of bank k, including multiple-relationship firms. This variable is an indicator of the average importance of bank *K* to its customers and thereby serves as a proxy of how much asymmetric information bank k on average holds on customer firms towards the market. Analogously, the share of main customers of bank k takes the sum of all customers of bank k, who have their main bank with bank k over all customers of bank k including multiple-relationship customers:

$$main\ bank\ share_{kt} = \frac{\sum_i I(bank_{1,it}=k)}{\sum_{r=1}^{6}\sum_i I(bank_{r,it}=k)} \tag{3}$$

This indicator measure defines the average role bank *k* has to its customers even if customers have multiple relationships. In other words, it gives us the average value bank k assigns to its customer portfolio. The third measure of relationship orientation considers the geographical distribution of borrowers and is motivated by the results on the role of distance in relationship lending. Shorter distances may provide the bank with more information and allow to perform relationship banking (e.g., Petersen and Rajan, 2002; Degryse and Ongena, 2005; Agarwal and Hauswald, 2010). It is defined as the share of customers located within 50km around the headquarters of bank k and indicates bank k's regional focus:

$$share\ 50km_{\mathrm{kt}} = \frac{\sum_{r=1}^{6}\sum_i I(\mathrm{bank}_{r,it}=k)*I(distance_{ik,t}\leq 50km)}{\sum_{r=1}^{6}\sum_i \mathrm{I}(\mathrm{bank}_{r,it}=k)} \tag{4}$$

Based on these three measures we construct a dummy variable *relationship bank* that indicates whether some bank k exceeds the 75 percentile among all banks in a year t in at least one of the measures.

We analyze the role of relationship banking in order to investigate how close customers are affected when banks go into distress. The question of whether close bank-firm relationship shield customers against crises has been subject to a variety of studies in the field of financial intermediation (e.g. Peek and Rosengren, 1997; Ivashina and Scharfstein, 2010).

In the following section we first start out with the question of whether treatments occurring during the crisis years have differential effects than those in non-crisis years. Then we examine whether the banks' relationship orientation has different treatment effects on firms. Finally, we extend this analysis to the joint investigation of crisis and relationship bank effects.

3.2.1 Bank Distress in the Crisis

Table 7 shows the same specifications as in Table 6 but now making a distinction in the timing of the treatment. We observe that the effects in Table 6 are driven by those treatments occurring in the crisis years 2008 and 2009. The effects in crisis years are much stronger. Borrowers at distressed banks face an increase in *PD* of about 23% after treatment (B1a). When only looking at non-defaulting firms, the treatment effect equals 13% (B2a). With respect to the maximum loan recommendation MAXLOAN the treatment effect is equal to -10% (B4a). While the regression employing *MAXLOAN* loses significance (possibly due to non-linearities), it shows, however, a stronger negative coefficient.

For non-crisis years, none of the coefficients is significant; however, they remain qualitatively in line with the overall results. Hence, bank distress does not seem to have a per se adverse effect on borrowers but it does if distress happens in the course of a severe financial crisis.

We have shown that macroeconomic environments influence the pass-through of risks into the real sector, identifying a *bank-induced risk channel* from banks to their corporate customers.

3.2.2 Relationship versus Transaction Banks

We now study whether a bank's business model influences the previously reported bank-induced risk effects. In particular, we investigate whether a relationship or transactional orientation has different impacts on firm outcomes. Relationship banks may provide liquidity insurance for customers (e.g. Berger and Udell, 1995, Bolton et al. 2016), i.e. they charge on average higher rates but on the other hand keep providing liquidity even if firms are temporarily under pressure. Relationship banks in distress may be less able to fulfill this job. However, observably bad risks could also be kept alive, i.e. "evergreened".

In Table 8, we examine how bank distress impacts firm sales depending on the fact if the main bank is a relationship bank or a transaction bank. While distress at a relationship bank leads to significant increase in firm sales, firm sales goes down if a firm uses a transaction bank as main bank and this main bank gets into distress. This finding suggests that relationship banks and transaction banks behave quite differently when getting into distress. Relationship banks shield their customers while transaction banks pass on their risk.

We now look more in detail how the impact of bank distress interferes with the bank business model and the customer risk classes. We use quantile regressions (QR) where *PD* is the dependent variable. Note that we now use the subset of firms who do not default within the sample in order to distinguish impacts upon the assigned *PD* and impacts on actual default. The latter will be analyzed in a further step. The application of quantile regression techniques is not straight-forward in the context of fixed effects because standard software packages do not provide an a priori solution to such a regression set-up. We rely on a method introduced in Canay (2011) that tackles the problem in a two-stage regression framework. In the first step, we estimate a fixed-effects model with all non-time-constant regressors on the right-hand-side (which equals the regression setup from (1) in a DiD-framework) and then subtract the fixed part $\widehat{\rho_{gk,t}}$ from the outcome variable y of interest. In the second step, we estimate one equation for every quantile of this new variable y* with bootstrapped standard errors from 250 replications. In our setup, the adjusted outcome variable y* is exactly what we used to generate the graphs in Figure 2.

Figure 3 shows QR-plots using the dependent variable *PD* in all of the graphs. Note that the effects here are to be interpreted as percentage points as they now come from a FE-OLS-regression. Figure 3a) shows the QR-plot only for transaction banks (i.e. banks who do not exceed the 75th percentile in any of the relationship variables introduced above) whereas 3b) shows only relationship banks. As it is best practice with quantile regressions, we drop the lower and upper quantiles because effects are often unstable there. 3c) compares the quantile effects for relationship and transaction banks, but now showing percentage effects. They are calculated by dividing the p.p. effects from the regression by the respective constant term in that quantile.

While at both types of banks, median risk borrowers are equally affected, differences between relationship and transaction banks can be observed for low and high risk customers. At transaction banks, high risk customers are affected strongly and face a significant increase in PD. However, they are untouched at relationship banks. On the contrary, low risk customers (i.e. below median quantiles) do not experience effects at transaction banks but are quite strongly affected at relationship banks. The p.p. effect equals around 0.7, i.e. they experience an increase in PD.

This is possibly the most direct evidence that relationship banks may leave the worst customers untouched in order to reduce the risk of an actual default of those, a phenomenon often termed “Evergreening”. The resources that relationship banks keep at inefficient firms will be badly missed at more efficient firms, which may explain the strong effects for the good quantiles of the distribution at relationship banks. What is more, the relatively decent p.p. effects can be misleading when looking at the actual increase in default probability they represent in 3c), peaking at almost 15% increase in *PD* for the 0.2 quantile. Again be aware that for transaction banks, we find non-significant near-zero effects in this quantile.

3.2.3 Relationship Banking in the Crisis

In the last section we have shown that relationship and transaction banks in fact behave opposite in times of distress. We now turn to the question whether the role of relationship banks is different in crisis times. In order to do that, we apply the same methodology as before by employing QR techniques to address banks' behavior towards different risk-classes but now only look at relationship banks and distinguish their behavior in the crisis years and non-crisis years. It should be noted that there are limitations as we do not observe the same bank in distress once in crisis years and once in none-crisis years.

Figure 4 shows the resulting QR-plots for the subset of relationship banks distinguished by treatment occurring within and those outside the crisis years. While the effects that we concluded for the below median quantiles still seem to be in place in crisis years, Evergreening of inefficient firms is only found for treatments in non-crisis years. Note in particular that we find even negative effects for non-crisis treatments in the upper quantiles of the risk-distribution. The evidence for crisis years is compelling: relationship banks in the crisis show nearly the same pattern of treatment effects than do transaction banks in Figure 3. We take this as evidence that the merits of relationship banking that are still in place for treatments in normal times are absent when a systematic crisis hits the economy. The logical explanation would be that distressed banks in the crisis are unable to shield inefficient firms from the shock and also cut down liquidity provision to them.

3.3 Robustness of our Results

We carry out various robustness checks. First, our results are robust to different estimators applied to the data. Table 9 shows the regression framework from specification A2 now using different estimators. Note that the coefficients shown in specifications C1 to C8 have to be interpreted as p.p. effects. We see that effects remain qualitatively similar no matter which estimator is used. However, OLS and firm-fixed effects models (C1 to C4) show an underestimation of the effect. This finding is likely due to both the demand side (firms' order situation, idiosyncratic and market risk) and Creditreform division effects (differences in risk-assessment and application of new methodologies by rating agencies) that we aim to exclude by applying our grouping in equation (1). Moreover, column C7 and C8 take into account that the dependent variable is bounded between 0 and 1, which calls for a truncated regression.

Second, Table 10 gives evidence on whether the inclusion of bank and firm covariates into the regression changes the coefficient estimates on *PD*. Again, the baseline specification A2 builds the basis for this table, i.e. specification A2 equals specification D1, again this time with a logit link. Moving more to the right of the table, we include more and more covariates into the regression. In a well-specified conditional DiD-setup, coefficients ought to remain stable when including covariates from the matching equation. While firm characteristics are not part of the matching equation, they enter through the grouping applied in equation (1) and given little time variation in firm covariates, including these covariates should also not change our coefficients on the treatment effect. Table 10 shows this to be the case for the bank-covariates employed in the matching equation (compare Table 1 for an overview) and the firm characteristics entering into the group-fixed effect.

Third, in Table 9, we examine whether the impact of bank distress on firm PD is different for different subsamples. We find that our results are robust to firm location and restrictions on the macroeconomic environment. Results differ to some extent depending on the bank type. The treatment effect is, for example, stronger if cooperative banks are excluded. Moreover, the treatment effect also depends on firm age. With the exception of very young firms, we find that the treatment effect goes down with firm age, i.e. younger firms are more strongly affected when their main bank gets into distress than older ones.

4 Concluding Remarks

Banks are important origins of shocks to the economy. We investigate whether bank bailouts lead to bank-induced changes in their customers' probability of default, maximum loan recommendations (both determined by an external credit agency, and not self-reported by banks) and sales. Our empirical analysis of bank bailouts in Germany over the period 2000-2012 shows that a bank bailout following bank distress leads to a bank-induced increase in the probability of default, and a lowering of the maximum loan recommendations and of sales. We find that these effects are mainly driven by bank bailouts occurring during the global recession.

Relationship and transaction banks that are bailed out generate very different bank-induced risk effects. While transaction banks lead to an increase in the probability of defaults for firms with above median riskiness, relationship banks seem to shield high risk firms from increases in probability of default. However, they lead to a somewhat higher probability of default for higher quality firms. This suggests that distressed relationship banks are perceived to evergreen their lower quality customers and are less able to perform relationship lending for higher quality firms.

We furthermore find that the bank-induced risk effects are more pronounced during the 2008/2009 financial crisis. In that environment, also the lower quality customers of relationship banks see their probability of default increasing.

From a policy perspective, the limited bank-induced impacts following a bank bailout in non-crisis times may please policy makers who are concerned of job losses and regional economic downturns. At the same time, it may prevent such distressed banks to clean their balance sheets and prevent resources to be allocated to more efficient uses, eventually with beneficial long run effects for the local economy.

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6 Appendix

Table 1:
Variables employed in the matching equation

Dependent	Affected Bank	Bank receives capital injection in treatment year t+1
Bank Balance Sheet Information	Total Assets	Log of GDP deflated total assets
	Total Loans	Log of GDP deflated total loans
	NPL Ratio	Non-performing loans over total assets (in %)
	RWA Growth	Risk-weighted assets growth (in %)
	Reserves Ratio	Bank reserves (according to section 340 f/g of the German Commercial Code) to total assets (in %)
	Hidden Liabilities	Dummy variable that takes on one for banks with avoided write-offs on its balance sheet
	Reserve Reduction	Dummy variable that takes on one if bank reserves are reduced
	Share of Customer Loans	Customer loans over total assets (in %)
Other Bank-specific Information	HHI	Hirschman-Herfindahl-Index (based on 14 business sectors)
	ROE	Return on equity (in %)
Aggregate Bank Customer Information	Bank Customers	Log of number of bank customers
	Share of Customers in Distress	Number of distressed customers over total number of customers
	Share of Single Relationship Customers	Number of customers with a single relationship over total number of customers
	Share of Main Bank Customers	Number of customers that use the affected bank as main bank to total number of customers
	Share of Regional Customers	Customers within a range of 50km to total number of customers

Table 2:
Matching Regression

Method	Logit	
Controls	Bank Type Dummies, Year Dummies, Headquarters in E/W Germany	
	Below Median Regression	*Above Median Regression*
Observations	9,926	9,778
Pseudo R-squared	0.143	0.308
Dependent Variable	Bank Receives Initial Capital Injection (CI) in Period t+1	
Total Assets	3.062**	3.690***
	(1.479)	(1.365)
Number of Bank Customers	0.188	-0.0198
	(0.342)	(0.212)
Total Loans	-2.543*	-3.314**
	(1.342)	(1.297)
RWA Growth	-0.0164	0.0139
	(0.0289)	(0.0140)
Share of Customer Loans	0.0114	0.0146
	(0.0184)	(0.0157)
NPL-RATIO	0.0576*	0.0541***
	(0.0325)	(0.0194)
Reserves Ratio	-0.848**	-1.912***
	(0.334)	(0.489)
Hidden Liabilities	-0.462	1.249**
	(0.695)	(0.487)
Reserve Reduction	0.431	0.341
	(0.638)	(0.531)
Equity Ratio	-0.174	-0.0690
	(0.192)	(0.118)
HHI	-0.0319	0.0271*
	(0.0335)	(0.0155)
ROE	-0.00712	-0.00518
	(0.00870)	(0.00448)
Share of Customers in Distress	-46.29**	1.754
	(20.97)	(3.997)
Share of Single Relationship Customers	-1.283	-3.391*
	(2.341)	(1.927)
Share of Regional Customers	-0.0437	5.972**
	(2.313)	(2.418)
Share of Main Bank Customers	2.527	0.816
	(1.777)	(1.778)
Constant term	-15.03**	-17.24***
	(7.620)	(6.641)

The table shows the logit regression used to calculate the propensity score for the matching. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Variables are explained in Table 1.

Table 3:
Number of treated banks and control banks

Treatment Year	Treated Banks	Control Banks	Total
2003	5	10	15
2004	3	7	10
2005	4	11	15
2006	1	2	3
2007	3	7	10
2008	4	8	12
2009	2	6	8
2010	1	2	3
Total	23	53	76

For each treated bank up to 3 control banks are selected. Each bank is observed for a total of 6 years around the treatment year. The full sample period goes from 2000 to 2012.

Figure 1

Median Bank-Characteristics of Treated Banks (solid) and Control Banks (dashed) Before and After Treatment

The timeline refers to years before and after matching. Matches are obtained using nearest-neighbor matching on bank covariates in period t-1. The set of control banks may be constituted by the three nearest neighbors of bank k.

Table 4:
Firm-Observations by Year of Observation (left) and Year of Treatment (top)

Year of Observation	Treatment Year 2003	2004	2005	2006	2007	2008	2009	2010	Total
2000	10,144								10,144
2001	10,368	5,450							15,818
2002	10,514	5,330	5,166						21,010
2003	**10,972**	5,314	5,497	2,748					24,531
2004	11,631	**5,491**	5,604	2,808	1,652				27,186
2005	11,735	5,453	**5,258**	2,737	1,707	2,850			29,740
2006		5,348	5,344	**3,035**	1,833	3,066	12,114		30,740
2007			5,360	3,031	**1,941**	3,373	12,260	1,299	27,264
2008				3,045	2,145	**3,739**	12,487	1,604	23,020
2009					2,281	4,105	**12,536**	1,895	20,817
2010						4,426	12,534	**2,147**	19,107
2011							12,528	2,446	14,974
2012								2,844	2,844
Total	65,364	32,386	32,229	17,404	11,559	21,559	74,459	12,235	267,195

Firms may occur multiple times because two treated banks may have the same control bank. The dataset is uniquely defined on the firm-bank-neighbor-year-level.

Table 5:
Comparison of Firms of Treated and Control banks

Variable	Number of Observations		Mean (year before treatment)		Mean Difference
	Treated	Control	Treated	Control	
Number of Bank Relationships	7538	36219	1.35	1.28	0.0659***
Main Bank Switch (Y/N)	7538	36219	0.01	0.01	0.0014
Main Bank Drop (Y/N)	7538	36219	0.01	0.01	0.0012
Payment Status	7538	36219	26.36	26.81	-0.4455***
PD	7538	36219	0.1	0.11	-0.0094***
Number of Employees	5299	25519	7.94	6.93	1.0084**
Sales (in 1,000)	5366	25809	2648.55	2172.76	511.79***
Entrepreneur (Y/N)	7538	36219	0.84	0.85	-0.0071
Number of Managers	7538	36219	1.08	1.08	0.0058
Financier (Y/N)	7538	36219	0.19	0.19	-0.0004
Max. Recomm. Loan (in 1,000)	6610	31216	15.24	11.79	3.4475***
Distance Firm Bank (in km)	7369	35259	8.7	9.9	-1.198***
Age	7538	36219	20.71	24.26	-3.5446***
Single Relationship (Y/N)	7538	36219	0.72	0.78	-0.0544***

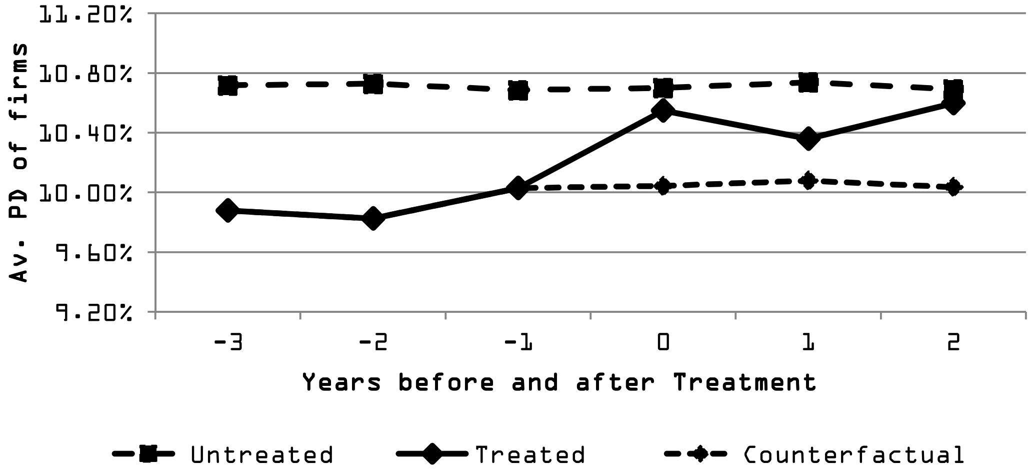
Significance levels*** p<0.01, ** p<0.05, * p<0.1

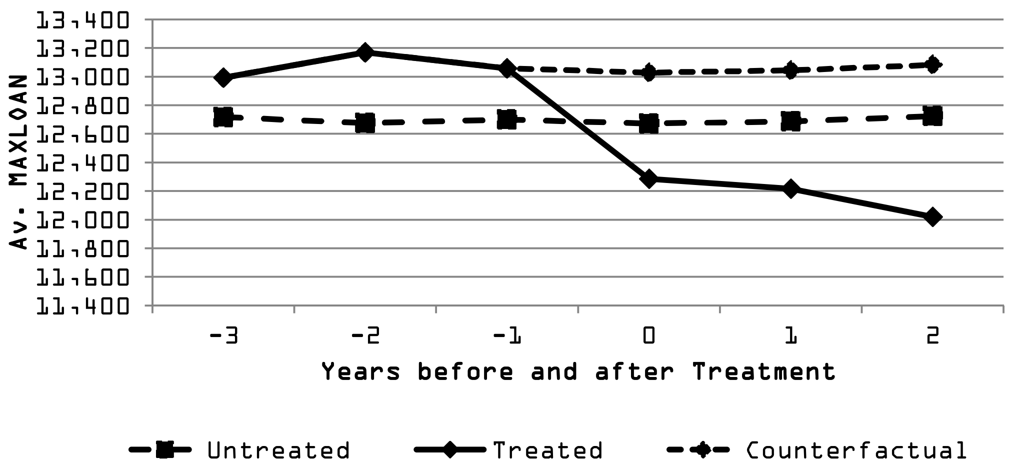
Table 6:
Impact of Bank Distress on Firm Distress

Specification	A1	A2	A3	A4	A5	A6
Estimator	GLM logit link	GLM logit link	OLS FE	OLS FE	FE Probit	OLS FE
Dependent Variable	PD	PD	MAXLOAN	LOG MAXLOAN	DEFAULT	LN SALES
Sample	all	no defaultees	all	all	all	all
Time	All Years	All Years	All Years	All Years	All Years	All Years
Treatment Effect	0.120***	0.0694***	-905.0**	-0.0794***	0.0675**	-0.0368*
Observations	267,195	228,708	214,833	214,833	197,692	187,280
Number of groups	54,407	53,332	51,443	51,443	-	43,450

Conditional Difference-in-Difference-Estimates on the Firm-Bank-Neighbor-Year-Level. (Robust) standard errors in parentheses Specifications A1 and A2 show GLM-estimates on firms' individual *PD*, specification A3 and A4 introduce the new variable *MAXLOAN* in two FE-OLS estimations. Specification A5 shows FE-Probit results on actual default of firms. Specification A6 show the impact on firm sales. All specifications except specification A5 use robust standard errors, *** p<0.01, ** p<0.05, * p<0.1

Figure 2
Average Adjusted Outcome Values from Regression Specifications A1 (top) and A3 (bottom).





The counterfactual situation is calculated by applying the trends from control observations to treated observations after the time of the treatment.

Table 7:
Impact of Bank Distress on Firm Distress: Crisis versus normal times.

Panel a

Specification	B1a	B2a	B3a	B4a	B5a
Estimator	GLM logit link	GLM logit link	OLS FE	OLS FE	FE Probit
Dep. Variable	PD	PD	MAXLOAN	Log MAXLOAN	DEFAULT
Sample	all	no defaulters	all	all	all
Time	Crisis	Crisis	Crisis	Crisis	Crisis
Treatment Effect	0.231***	0.132***	-1,323	-0.102***	0.141***
	(0.0652)	(0.0309)	(908.1)	(0.0206)	(0.0457)
Observations	108,253	96,770	92,702	92,702	80,039
Number of groups	23,106	22,812	22,605	22,605	16,604

Panel b

Specification	B1b	B2b	B3b	B4b	B5b
Estimator	GLM logit link	GLM logit link	OLS FE	OLS FE	FE Probit
Dep. Variable	PD	PD	MAXLOAN	Log MAXLOAN	DEFAULT
Sample	all	no defaulters	all	all	all
Time	No Crisis	No Crisis	No Crisis	No Crisis	No Crisis
Treatment Effect	0.0528	0.00916	-360.6	-0.0459	0.0199
	(0.0424)	(0.0283)	(337.0)	(0.0317)	(0.0327)
Observations	158,942	131,938	122,131	122,131	117,653
Number of groups	31,301	30,520	28,838	28,838	24,407

Conditional Difference-in-Difference-Estimates on the firm-bank-neighbor-year-level depending on the year of treatment (within crisis or not). Specifications are the same as in Table 6. (Robust) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 8: Impact of Bank Distress on Firm Sales: Relationship versus Transaction Banks

Specification	E7	E8
Estimator	OLS FE	OLS FE
Dependent Variable	LN SALES	LN SALES
Sample Restriction Firms	All	All
Sample Restriction Banks	Relationship-Oriented Banks	Transaction Banks
Time	All Years	All Years
Treatment Effect	0.157**	-0.0335
Observations	28,719	155,872
Number of Groups	6,668	37,462
R-squared	0.637	0.624

Conditional Difference-in-Difference-Estimates on the firm-bank-neighbor-year-level depending on the business model of the main bank (relationship bank versus transaction bank). (Robust) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Figure 3

Results of Quantile Regressions (using *PD* as a dependent variable and distinguishing upon relationship and transaction banks).

We apply a method for fixed-effects in quantile regressions introduced in Canay (2011). Standard errors are bootstrapped with 250 replications. Plots a) and b) are p.p. effects and show 5%-confidence intervals. Plot c) shows the p.p. effect in relation to the respective constant in quantile q, i.e. the percentage effect. White boxes/prisms show insignificant areas at the 5% level.

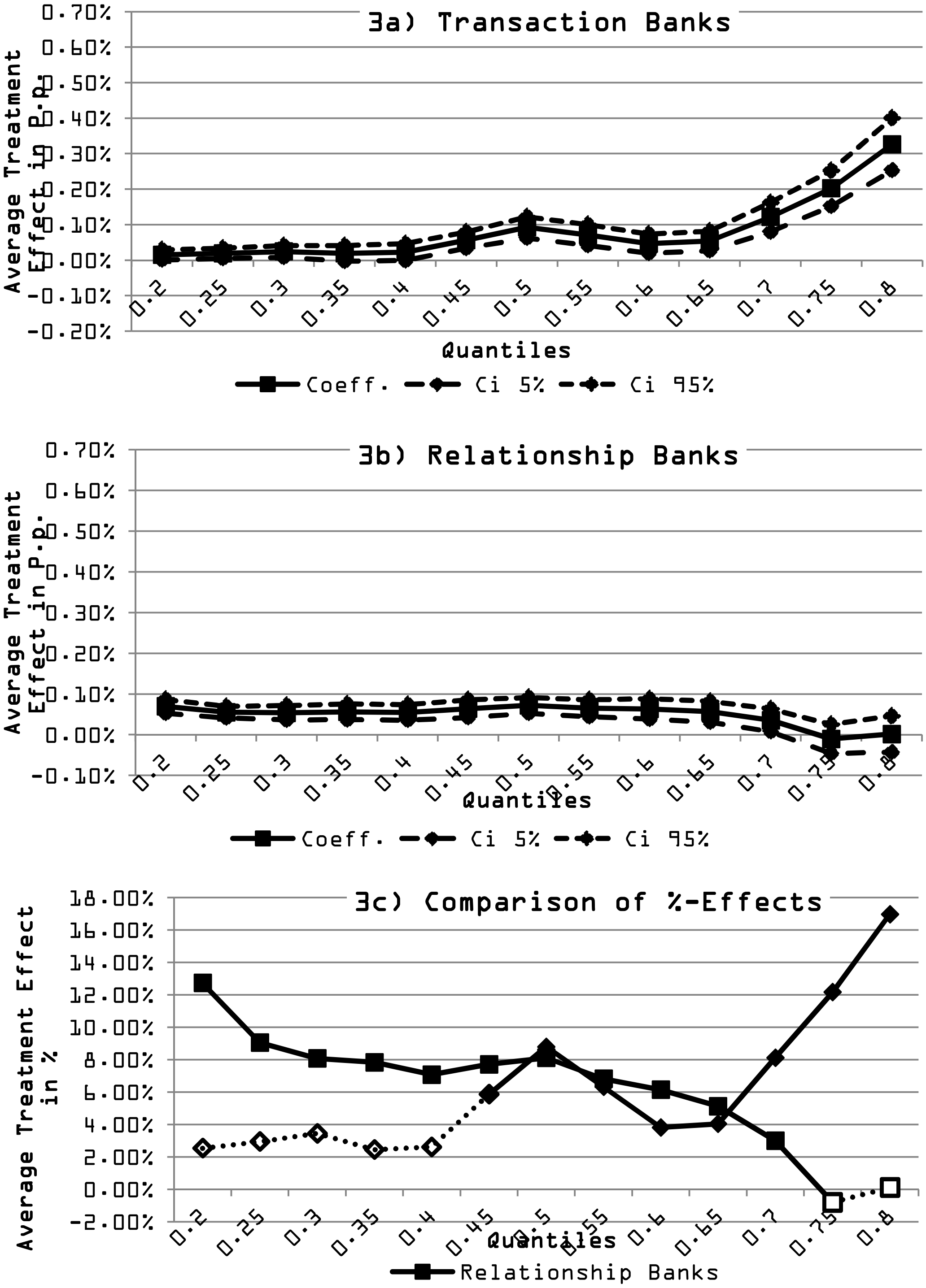


Figure 4

QR-plots using *PD* as a dependent variable and distinguishing relationship banks running into distress within and outside the crisis years. Plots 4a) and 4b) are p.p. effects and show 5%-confidence intervals. Plot 4c) shows the p.p. effect in relation to the respective constant in quantile q, i.e. the percentage effect. White boxes/prisms in 4c) show insignificant areas at the 5% level.

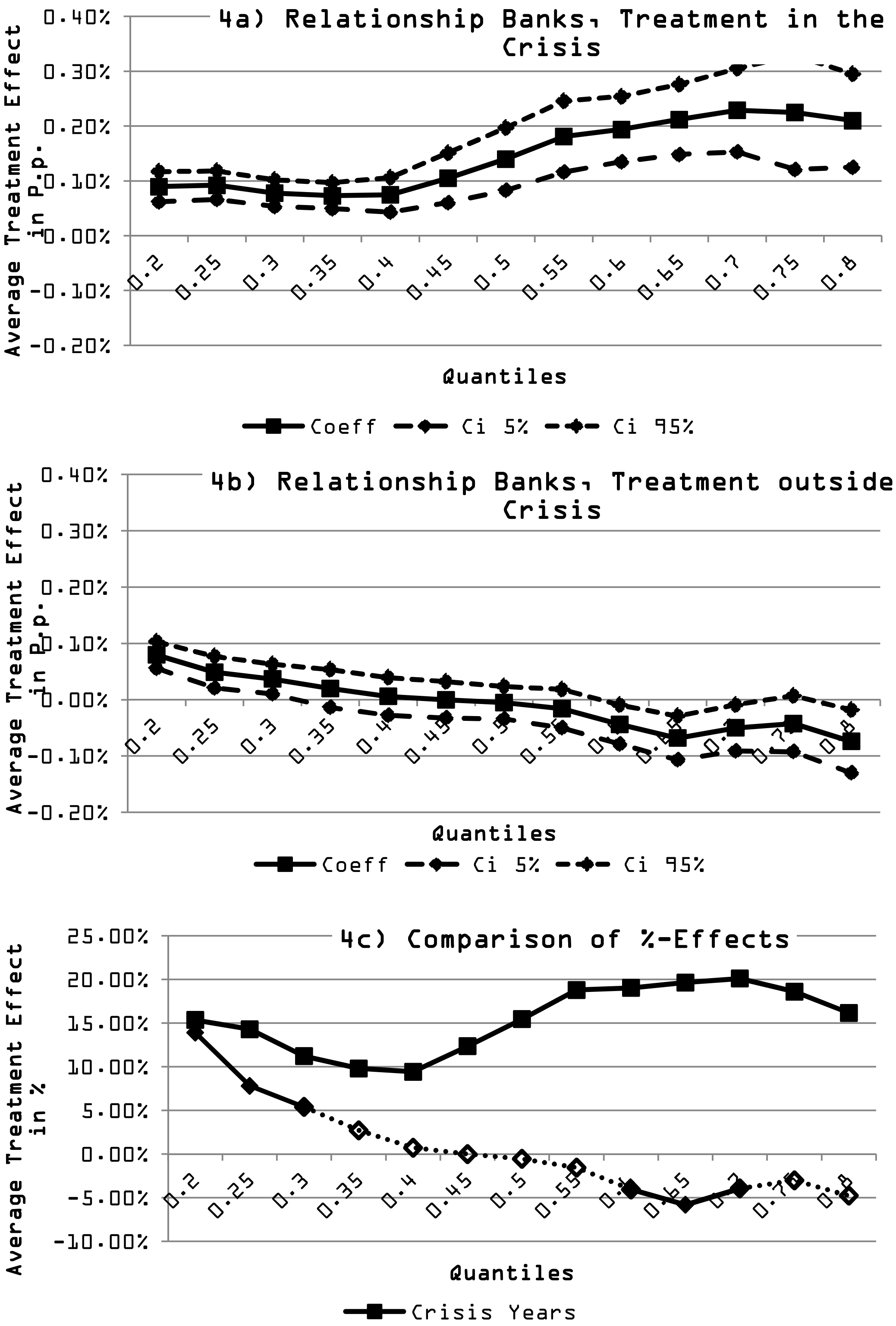


Table 9:
Robustness Check for the application of different estimators on the variable *PD*.

Specification	Estimator	Number of Observations	Number of Groups	Treatment Effect
C1	OLS	228,708		0.000548**
C2	OLS robust	228,708		0.000548**
C3	Genuine FE	228,708	56,157	0.000695***
C4	Genuine RE	228,708	56,157	0.000690***
C5	Group FE	228,708	50,349	0.00198***
C6	Group RE	228,708	50,349	0.00101***
C7	Tobit robust	228,708	50,349	0.000914***
C8	GEE robust	228,708	50,349	0.00110***

C1 and C2 show basic OLS estimations, C3 and C4 FE-estimates on the firm-level, C5 and C6 FE and RE estimates on the group-level, C7 is a random effects Tobit estimation with 0 lower and 1 upper bound. Finally, C8 is the GEE estimator applied in our main regressions, however, this time with an identity-link, i.e. it gives the p.p. effect for reasons of comparison to the other models. All models are estimated without firms who default within the sample duration.

Table 10:

Robustness Check for the inclusion of bank and firm covariates.

All models are estimated without firms who default within the sample duration. Baseline specification is specification A2 from Table 6 using all non-defaulting firms and a logit link function.

		Control Variables									
Speci-fication	Observations	Firm Sales	Firm Employees	Banktype Dummies	NPL Ratio	RWA Growth, Reserves, Hidden Liabilities, EQ Ratio, HHI Sec14, ROE	Share of Distressed/ Single Relationship/ Within 50km/ Main Bank Customers	Total Assets	Number of Customers	Total Loans	Treatment Effect (%)
D1	228,708										0.0694***
D2	168,728		X								0.0554***
D3	145,734	X	X								0.0500**
D4	145,629	X	X		X						0.0682***
D5	143,130	X	X		X	X	X				0.0556**
D6	143,130	X	X	X	X	X	X	X			0.0683**
D7	143,130	X	X	X	X	X	X		X		0.0630**
D8	143,130	X	X	X	X	X	X			X	0.0670**
D9	143,130	X	X	X	X	X	X	X	X		0.0731**
D10	143,130	X	X	X	X	X	X	X		X	0.0532**

Table 9: Robustness Checks: Various sample restrictions

Specification	F1	F2	F3	F4	F5	F6	F7	F8	F9	F10	F11
Estimator						GLM logit link					
Dependent Variable						PD					
Sample Restriction	No firms in east Germany	Two Year Window	No Regions with 2 subse-quent years of neg. GDP growth	No Private Banks	No Public Banks	No Coop. Banks	Only Public Banks	Only Firms not older than 5	Only Firms not older than 10	Only Firms not older than 20	Only Firms older than 50
Time						All Years					
Treatment Effect	0.0909**	0.103**	0.130***	0.0890**	0.0975**	0.186***	0.146**	-0.00441	0.126**	0.0925**	0.0388
Observations	213,953	178,205	181,446	231,393	144,986	158,011	122,209	40,627	96,139	184,716	16,972
Number of groups	41,004	36,408	45,900	42,342	36,229	30,243	18,178	11,695	25,276	40,334	7,839

Table A 1:
Industry Definition and Distribution according to NACE Classification

No.	Industry Sector Groups	Observations	Percent	Industry sector classification (NACE rev. 2)
1	Cutting-edge technology manufacturing	1,091	0.41	20.2, 21, 24.46, 25.4, 26.11, 26.2, 26.3, 26.4, 26.51, 26.6, 26.7, 30.3, 30.4
2	High-technology manufacturing	3,799	1.42	20.13, 20.14, 20.16, 20.42, 20.51, 20.53, 20.59, 22.11, 23.19, 23.44, 26.12, 27.11, 27.12, 27.2, 27.31, 27.33, 27.4, 27.9, 28.11, 28.12, 28.13, 28.15, 28.23, 28.24, 28.29, 28.3, 28.41, 28.49, 28.92, 28.93, 28.94, 28.99, 29.1, 29.31, 29.32, 30.2, 33.2
3	Non-high-tech manufacturing	21,364	8.00	10-33 (excl. sectors 1 and 2)
4	Technology-intensive services	11,376	4.26	61.1-61.3, 62, 63.1, 71.1, 71.2, 72.1
5	Non-technical consulting services	9,636	3.61	69, 70.2, 72.2, 73
6	Other business-oriented services	14,609	5.47	61-63, 69-72, 77.1, 77.3, 77.4, 78, 80, 81 (ex 70.1, 74.2)
7	Consumer-oriented services	56,498	21.14	55-56, 58-60, 68, 74.2, 75, 77.2, 79, 85.5-85.6, 86-88, 90-93, 95-96
8	Energy/Mining/Disposal	2,362	0.88	5-9, 35-39
9	Construction	46,787	17.51	41-43
10	Trade	72,674	27.2	49-52
11/12	Traffic/Mailing	11,168	4.18	49-53
13	Banks/ Insurances/ Financial Services	Excluded from Firm Sample		64 (excl. .64.2), 65, 66,67
14	Holdings	6,801	2.55	70.1, 64.2
0	Other (e.g. Forestry/ Agriculture)	8,354	3.13	< 10
	Total	267,195	100	

Source: Own classification, NIW/ISI/ZEW Listen 2012 (Gehrke et al., 2013)

6.1.2 Legal Forms

Table A 2:
Legal Forms in the Main Regression Sample

No.	Industry Sector Groups	Observations	Percent
1	Liberal Profession	10,116	3.79
2	Commercial Operation ("Gewerbebetrieb")	139,848	52.34
3	BGB-Company ("BGB Gesellschaft")	10,188	3.81
4	Partnership ("Arbeitsgemeinschaft")	19	0.01
5	One-Man Business ("Einzelfirma)	11,148	4.17
6	General Partnership ("OHG")	523	0.2
7	Limited Partnership ("KG")	842	0.32
8	limited partnership with a limited liability company as general partner ("GmbH & Co. KG")	5,926	2.22
9	Limited Liability Company ("GmbH")	85,289	31.92
10	Corporation ("AG")	40	0.01
	Registered Co-Operative ("eG")	1,520	0.57
11	Registered Association ("eV")	1,736	0.65
	Total	267,195	100