London School of Economics and Political Science

Finance and the Real Economy

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To my mother and the memory of my father

Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

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Statement of conjoint work

I confirm that Chapter 2 was jointly co-authored with James Cloyne, Ethan Ilzetzki, and Henrik Kleven and I contributed 25% of this work.

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Abstract

This thesis studies the interaction between the financial sector and the real economy. Chapter 1 analyzes how lending cuts by banks affect firms. I identify an exogenous lending cut by a large German bank and examine the growth of firms and counties dependent on this bank. Firms directly exposed to reduced bank lending grew more slowly. On average, firms suffered when many other firms in their county experienced decreased bank lending, because of lower aggregate demand and agglomeration spillovers. The effects of the lending cut persisted after lending had resumed. Innovation and productivity fell, consistent with the persistent effects.

Chapter 2 investigates the effect of house prices on household borrowing using administrative mortgage data from the UK. The chapter develops an empirical approach that exploits individual house price variation coming from the timing of refinancing events around the Great Recession. There is a clear and robust effect of house prices on borrowing. The effect can largely be explained by households using the value of their house as collateral.

Chapter 3 focuses on financial institutions. How changes in bank size affect the real economy is an important question in the design of financial regulation. This chapter studies a natural experiment from postwar West Germany. Reforms by the Allied occupiers led to increases in the size of a number of banks. I estimate the effect of increased bank size on the growth of firms. The results suggest that firms did not benefit when their banks became larger. The findings are inconsistent with theories that argue the real economy benefits from increases in bank size. There is evidence that big banks are worse at processing soft information and take more risks. Big banks receive more mentions in the media, which could be an incentive for banks to become big.

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Chapter 1

Disentangling the Effects of a Banking Crisis: Evidence from German Firms and Counties

1.1 Introduction

The Great Recession followed a common pattern in many developed economies. There was a systemic banking crisis in the years 2008/09, during which bank lending fell. Subsequently, there were two years of negative output growth and a slow recovery, during which output failed to return to its pre-crisis trend. This persistence is unusual in the postwar history of developed economies (Friedman 1993). Is there a causal link between the reduction in bank lending and this growth pattern? Do bank lending cuts lead to deep and persistent recessions?

Motivated by these questions, this paper delivers causal evidence on the effects of bank lending on the real economy. I analyze a lending cut by Commerzbank, a large German bank. During the financial crisis, Commerzbank suffered significant losses on its international trading book. These losses were unrelated to its domestic loan portfolio, but forced it to reduce its loan supply to German borrowers. I study the effects of the lending cut using variation across German counties and firms in their dependence on Commerzbank.[1] The analysis produces two main findings. First, the lending cut did not only reduce the growth of firms that directly relied on Commerzbank's loan supply. There were also significant indirect effects on firms with undisturbed loan supply, through reductions in local aggregate demand and agglomeration spillovers. The second main finding is that the lending cut had persistent effects. Output and employment remained low even after lending had normalized.

By focusing on an imported lending cut, I address the key identification challenge

[1]Commerzbank refers to all branches that were part of the Commerzbank network in 2009, including Dresdner Bank.

that plagues the literature on financial frictions: the reverse causality between the health of the banking sector and economic growth. Unlike most developed economies, Germany experienced no house price boom or decline, no endogenous banking panic, relatively little uncertainty, and no sovereign debt crisis before or during the Great Recession. Therefore, the lending cut by Commerzbank provides a suitable natural experiment to disentangle the causal effects of bank lending. To verify my empirical strategy, I show that firms with a pre-crisis relationship to Commerzbank held less bank debt after the lending cut. In a survey, these firms reported restrictive bank loan supply in 2009 and 2010, but not in any year before or after Commerzbank's lending cut. An important contribution by Peek and Rosengren (2000) similarly uses an imported lending cut to isolate an exogenous loan supply shock.

A second identification challenge arises from the possibility that unobserved shocks affected counties dependent on Commerzbank at the same time as Commerzbank's lending cut. To address this possibility, I construct an instrumental variable (IV) for county Commerzbank dependence. The instrument is based on the enforced breakup of Commerzbank by the Allies after World War II, which led Commerzbank to set up three separate, temporary head offices, in Düsseldorf, Frankfurt, and Hamburg. The data show that Commerzbank expanded its branch network around its temporary head offices while it was broken up. The association between distance to these cities and Commerzbank dependence has survived until today. I can thus use a county's distance to the closest postwar head office as an instrument for Commerzbank dependence before the lending cut.

The first set of results shows that the lending cut had real effects on firms. Following the lending cut, firms dependent on Commerzbank reduced their capital stock and employment, relative to similar firms located in the same county, but with no pre-crisis Commerzbank relationship. Employment at a firm fully dependent on Commerzbank was on average 5.3 percent lower than at a firm with no Commerzbank relationship. I call these firm-level responses the *direct* effects of the lending cut, because they were driven by firms' immediate financial connections to Commerzbank. They are a partial equilibrium response, keeping constant other aggregate factors that affected firms independently of their banking relationships. The findings on the direct effects confirm the results of Almeida, Campello, Laranjeira, and Weisbenner (2012) and Chodorow-Reich (2014a).[2] I estimate effects of similar magnitude to the existing literature, which suggests that Commerzbank's lending cut has external relevance to the United States and other countries.

An important question is whether banking shocks affect growth at higher levels of

[2] Gan (2007); Khwaja and Mian (2008); Amiti and Weinstein (2011a); Schnabl (2012); Paravisini, Rappoport, Schnabl, and Wolfenzon (2015); Garicano and Steinwender (2016); Cingano, Manaresi, and Sette (2016a); Bentolila, Jansen, Jiménez, and Ruano (forthcoming) present further evidence.

economic aggregation. I test the effect on counties. I construct a measure of county Commerzbank dependence based on the average exposure to Commerzbank of firms in the county. The results show that GDP and employment in counties dependent on Commerzbank fell after the lending cut. A standard deviation increase in Commerzbank dependence lowered county employment after the lending cut by an average of 0.8 percent in the ordinary least squares (OLS specification and 1.3 percent in the IV specification. The IV point estimates, based on the distance instrument, imply larger effects than the OLS estimates, but are not statistically different. This suggest that unobserved, negative shocks cannot explain the OLS results. By conditioning on the linear distance to each of the postwar head offices in all IV specifications, I control for spurious correlations between growth after the lending cut and factors associated with proximity to one of the cities. This means the identification is solely driven by the distance to the closest postwar Commerzbank head office, rather than the distance to one particular city.

Having established there are real effects on firms and counties, I discuss two aspects of the results in more detail: indirect effects and persistence. The first aspect relates to the difference in magnitude between the firm and county effects. Two types of firm-level effects determine the response of county aggregates. The first are the *direct*, partial equilibrium effects. In addition, there are *indirect* effects of the lending cut. These impact firms independently of their direct financial connections to Commerzbank. They arise when the aggregate economic environment of a county responds to the lending cut. For example, if directly affected firms reduce employment, the consumption of households falls, lowering aggregate demand in the county. Furthermore, a fall in the innovation activities of directly affected firms reduces agglomeration spillovers to neighboring firms.

I investigate whether significant indirect effects of the lending cut affected the county response. Specifically, I estimate the effect on firms of increasing the Commerzbank dependence of other firms in the county, while keeping constant the firms' direct exposure to Commerzbank. The results show negative and sizable indirect effects on producers of non-tradables and firms with high innovation activities. The data reject the hypothesis that in a county fully dependent on Commerzbank these indirect effects were smaller than the direct effect on a firm that borrowed only from Commerzbank. There is no evidence for an indirect effect on tradables producers with low innovation activities. This pattern of heterogeneity suggests that reduced county aggregate demand and lower agglomeration spillovers in high-innovation industries generated the indirect effects. Migration and household debt were not affected, so they cannot explain the indirect effects.

The second aspect I discuss is that the effects on both firms and counties were persistent. The causal effects resemble the growth pattern of developed economies during and after the Great Recession. During the years of the lending cut, growth was significantly lower. In the subsequent two years, affected firms and counties remained on a lower, roughly parallel trend, without any sign of convergence to the level of unaffected firms and counties. This implies that a temporary bank lending cut can persistently keep output and employment low even after bank loan supply has normalized. The dynamics of the estimated effects suggest that the bank lending cuts during the financial crisis of 2008/09 may have contributed to the sluggish recovery from the Great Recession, even though the banking sector had stabilized by 2010 (Hall 2010).

Persistent effects are not generally a response to shocks. For example, I show that firms and counties exposed to lower export demand during the Great Recession recovered to the level of unaffected firms and counties in under two years. Neoclassical growth theory similarly implies that once credit markets have stabilized, the economy should converge back to its pre-crisis trend (Fernald and Jones 2014). A decrease in innovation and productivity, however, could explain the persistent effects. Indeed, firms reduced innovation activities, proxied by patenting, when they were directly affected by Commerzbank's lending cut. A back-of-the envelope growth accounting exercise suggests that county total factor productivity fell, implying that productivity losses may have played a role in generating the persistence.

Influential contributions by Bernanke (1983) and Bernanke and Blinder (1992) argue that banking shocks affect the real economy. A number of more recent empirical studies document that banking crises have been correlated with deep and persistent recessions (Cerra and Saxena 2008; Reinhart and Rogoff 2009; Schularick and Taylor 2012; Giesecke, Longstaff, Schaefer, and Strebulaev 2014; Krishnamurthy and Muir 2017). But there is ambiguous causal evidence on the effects at levels of aggregation higher than the firm-level. Peek and Rosengren (2000), Calomiris and Mason (2003), Ashcraft (2005), Benmelech, Bergman, and Seru (2011), and Mondragon (2015) find that banking shocks in the United States strongly reduce local economic activity. On the other hand, Driscoll (2004), Ashcraft (2006), and Greenstone, Mas, and Nguyen (2014) report no or only small effects. Mian and Sufi (2014b) argue that business financing was not an important problem in the United States during the Great Recession. In contrast, Christiano, Eichenbaum, and Trabandt (2015) and Beraja, Hurst, and Ospina (2015) calibrate models that show supply-side shocks, such as financial frictions, best account for the growth pattern. In the German setting, Dwenger, Fossen, and Simmler (2015), Hochfellner, Montes, Schmalz, and Sosyura (2015), and Popov and Rocholl (2015) argue that banking shocks have real effects.

Ashcraft (2005) speculates that a reason for the different findings may be that small, regional differences in exposure to bank shocks are not informative about the consequences of a large, systemic lending cut. An advantage of studying Commerzbank's lending cut is that the variation across counties in exposure to Commerzbank is large and uncorrelated with other contemporaneous shocks. In line with Romer and Romer (2017), the results show that going beyond binary measures of financial distress helps to identify the real effects of financial shocks.

I contribute to the literature by clearly differentiating between the contemporaneous effects of a lending cut and the effects after lending has stabilized. I present evidence that productivity is affected. Furthermore, the existing literature has had to rely on strong assumptions about the indirect effects. The findings of large indirect effects are of interest to researchers studying the aggregate implications of a range of shocks, not just banking crises. It is a general problem in empirical work that well-identified, partial equilibrium effects may not be informative about the aggregate implications of a given shock (Acemoglu 2010). While the effects I estimate do not easily aggregate into national effects (Nakamura and Steinsson 2014; Beraja, Hurst, and Ospina 2015; Chodorow-Reich 2017), the combination of firm and county data is sufficient to establish the two main findings of indirect county-level effects and persistence.

This paper also adds to the literature on the importance of a single firm, in this case a bank, in shaping macroeconomic outcomes. Models by Gabaix (2011) and Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi (2012) illustrate how idiosyncratic firm-level shocks may translate into large aggregate fluctuations. I show empirically that lending by a single financial institution can persistently affect regional output and employment, consistent with Amiti and Weinstein (forthcoming).

The paper proceeds in the following section by explaining the identification strategy and the institutional background. I describe the data in Section 1.3, including a new dataset on the relationship banks of German firms. Section 1.4 verifies my identification strategy, by showing that firms dependent on Commerzbank reported restricted loan supply and held less bank debt after Commerzbank's lending cut. Section 1.5 reports the firm-level results on the direct effect and Section 1.6 performs the county analysis. Section 1.7 discusses the evidence for the indirect effects and the persistent losses. Section 1.8 concludes.

1.2 Identification and Institutional Background

1.2.1 Identification Strategy

This paper aims to estimate the causal effects of exposure to a bank lending cut. There are two well-known identification challenges. The first is reverse causality. A negative, exogenous shock to firms harms their lenders, for example because some firms default on loans. Therefore, banks may experience financial distress and cut lending because of the performance of their borrowers. The second identification challenge is that an omitted variable may simultaneously affect both the outcome and bank loan supply. For example, an expected reduction in regional growth would induce local firms to reduce employment and banks to cut lending to that region. Both these endogeneity concerns would lead to spurious correlations between lending cut exposure and firm growth, even if the true causal effect of a lending cut was zero.

I overcome the identification challenges by using the Commerzbank dependence of German firms and counties as proxy for their exposure to Commerzbank's lending cut. Frictions on credit markets mean that firms depend on the loan supply of their relationship banks (Sharpe 1990a). Firms and counties, for which Commerzbank was an important relationship bank, were therefore more exposed to the lending cut.

A lending cut can affect firms through multiple channels. It can reduce access to bank loans, affect the interest rate on loans and deposits, reduce the length of loans, and increase uncertainty regarding future credit access. Using just one of these variables as regressor would overestimate the effect of this particular variable. Identifying the causal impact of each channel would require one separate instrument per channel (Chodorow-Reich 2014a). I do not pursue such approaches here. Instead, I estimate the reduced-form impact, where Commerzbank dependence serves as proxy for exposure to a lending cut. This strategy overcomes the problem of reverse causality because Commerzbank's lending cut was exogenous to the performance of its German loan portfolio, as shown in the next Section 1.2.2. To address possible bias due to omitted unobservable variables at the regional level, I propose an instrument for county Commerzbank dependence in the subsequent Section 1.2.3.

1.2.2 The Origin of Commerzbank's Lending Cut

This section argues that Commerzbank's lending cut during the financial crisis of 2008/09 was an exogenous shock to its German borrowers. Commerzbank was responsible for around 9 percent of total bank lending to German non-financial customers in 2006. Its lending stock developed in parallel to that of the other banks until 2007, as shown in Figure 5.1. In 2008 and 2009, lending by Commerzbank fell sharply. Subsequently, it returned to a parallel trend relative to its peer group of other commercial banks.[3]

Why did lending decrease? Commerzbank is a universal bank, which means it earns both interest income from lending and non-interest income from trading and investing in international financial markets. During the financial crisis, Commerzbank suffered significant losses and write-downs on its trading portfolio. The trading losses led to a fall in Commerzbank's equity capital in every year between 2007 and 2009, decreasing it by 68 percent during this period. Commerzbank responded by cutting its

[3] There are three types of banks in Germany: commercial banks, cooperative credit unions, and public banks (Landesbanken and savings banks). The cooperatives and public banks have a political and social mandate to upkeep lending, unlike the commercial banks. 6.5 and 6.6 explain why trading losses at other German banks did not have real economic consequences, discussing papers by Dwenger, Fossen, and Simmler (2015) and Popov and Rocholl (2015).

loan supply to the German economy for two reasons. First, the Basel II regulations require a bank to hold at least 4 percent of its risk-weighted assets in equity. When equity falls, banks have to reduce assets (and start raising new equity). Second, the equity losses raised Commerzbank's cost of external funds, so it needed to lower risk exposure to be able to access funding markets.

The changes in Commerzbank's equity capital were entirely driven by write-downs on financial instruments and profits, as shown in the left panel of Figure 5.2. Write-downs on financial instruments included, for example, changes in the valuation of derivatives the bank held, and were unconnected to the firm and household loan portfolio. The change in profits was also unrelated to firms and households. The right panel of Figure 5.2 illustrates that trading and investment income was entirely responsible for the negative profits. Interest income, on the other hand, which includes what Commerzbank earns from lending to firms and households, remained on an upward trend up to 2009.

The trading losses were due to Commerzbank's investments in asset-backed securities related to the United States subprime mortgage market and its exposure to the insolvencies of Lehman Brothers and the large Icelandic banks. In 2008, Commerzbank had wrongly forecast the duration of the financial crisis and the likelihood of institutional failures. Commerzbank head Martin Blessing admitted that his bank had reduced its exposure to asset-backed securities too late and had believed that the United States government would not let Lehman Brothers fail. In comparison, Deutsche Bank avoided damage by hedging against a persistent drop in the United States housing market early on. Overall, the evidence shows that reverse causality is not a concern when I analyze the effects of Commerzbank's lending cut.

A more detailed analysis of Commerzbank's trading and loan portfolios is in 6.2. This analysis draws on 110 financial analyst research reports and a number of bank financial statements. The reports confirm that Commerzbank's loan portfolio was not riskier than other German banks'. In fact, the reports interpret Commerzbank's stable relationships to German firms as a source of strength. Its loan and trading divisions operated fairly independently, with no cross-divisional hedging relationship. While Commerzbank's international trading portfolio suffered losses, German bond markets remained stable and did not affect the health of Commerzbank and other German banks. Commerzbank's 2009 acquisition of Dresdner Bank was agreed before both banks suffered the severe trading losses. Both banks followed a similar trading strategy and contributed approximately evenly to the trading losses of the joint institution. Hence, the estimated effects of the lending cut are not different for customers of the old Dresdner Bank. The analyst reports agree that Commerzbank had stabilized by 2011. It had refocused its operations on lending to German customers and had repaid the majority of the government support extended during the crisis.

1.2.3 An Instrument for County Commerzbank Dependence

The second identification concern is that unobserved shocks affected counties dependent on Commerzbank at the same time as the lending cut. To investigate this possibility, I propose an instrument for county Commerzbank dependence. The instrument isolates the effect of Commerzbank dependence from other unobservable determinants of county growth. It is the county's distance to the closest of three temporary, post-World War II head offices of Commerzbank. After World War II, the Americans were convinced that the Nazi government's ability to wage war effectively stemmed from the Third Reich's economic centralization. From 1948 to 1957, they forced three large German banks to break up into separate entities in mandated banking zones. During this period, Commerzbank and (and its 2009 acquisition Dresdner Bank) had three separate head offices in Düsseldorf, Frankfurt, and Hamburg.

These cities were chosen due to a combination of historic accident and power struggles among the Allies, rather than the bank's business considerations. In the first banking zone, North-Rhine Westphalia, the British declared Düsseldorf as the state capital, because it was the only city with a large building that had survived the war (Düwell 2006). The banks followed the political power and settled there. In the second, Northern zone, the British ordered the surviving and non-imprisoned bank board members to set up a central head office in Hamburg. Frankfurt was chosen as head office for the Southern zone because the Americans had placed the new central bank there. At the time, Frankfurt was far from its current role as Germany's financial center, but it was chosen for its central location (Horstmann 1991).

The literature has established that banks prefer to form relationships with geographically close customers (Guiso, Sapienza, and Zingales 2004; Degryse and Ongena 2005a). Indeed, in the years after the breakup, Commerzbank was significantly more likely to establish a new branch in counties close to its temporary head offices, as shown in Appendix Table 6.1. The association between county Commerzbank dependence and distance to a postwar head office has survived until today, allowing me to construct a distance instrument based on how far a county is located from the postwar head offices. This distance instrument is calculated as the minimum of the linear (geodesic) distances to Düsseldorf, Frankfurt, and Hamburg. None of the three linear distances is perfectly correlated with the distance instrument. That means I can control for each of the linear distances to Düsseldorf, Frankfurt, and Hamburg in the IV specifications. In addition, I control for the linear distances to Berlin and Dresden, because historic, pre-war head offices of Commerzbank were located there.

Controlling for the linear distances is a crucial aspect of my IV strategy. It addresses the concern that the instrument may simply pick up spurious factors that are correlated with proximity to one of the postwar head offices. For example, professional services (such as legal, accounting, consulting, and advertising firms) experience cyclical demand fluctuations and are clustered around Düsseldorf. One may worry that the demand shock to this industry during the Great Recession, rather than Commerzbank's lending cut, drives the results. By controlling for the linear distance to Düsseldorf, I statistically remove the correlation between industry concentration around Düsseldorf and growth after the lending cut. The identification is solely driven by the distance to the closest postwar Commerzbank head office, rather than the factors associated with proximity to one of the cities.

1.3 Data

This paper uses five datasets: a firm panel, a firm employment cross-section, a firm survey, a county panel, and a household panel. The firm panel is based on balance sheet data from the database Dafne by Bureau van Dijk. It contains firms with non-missing data from 2007 to 2012 for the following variables: employment, wage bill, bank loans, value added, production capital (fixed tangible assets), and capital depreciation. Dafne reports the firms' industry, foundation year, the export share (fraction of exports out of total revenue), and the import share (fraction of imports out of total costs). From the database Orbis, I match information on the firms' patents. To construct the firm employment cross-section, I extract data from Dafne for all firms, for which I can calculate the employment change from 2008 to 2012.

The firm survey is the Business Expectations Panel of the ifo Institute. The sample includes all firms that responded to the following two questions in 2006 and 2009: "How do you evaluate the current willingness of banks to grant loans to businesses: cooperative, normal, or restrictive?" and "Are your business activities constrained by low demand or too few orders: yes or no?"

I obtain proprietary data from the year 2006 on the names of the relationship banks (Hausbanken) of 112,344 German firms, recorded by the credit rating agency Creditreform. The agency collects information on the relationship banks from firm surveys and financial statements. In all three firm datasets, I link firms to their banks in 2006 using a unique firm identifier (Crefonummer). The pre-crisis timing avoids endogeneity from weak banks getting matched with weak firms during the Great Recession. I drop firms in the financial and public sectors. This leaves 2,011 matched firms in the panel, 48,101 in the employment cross-section, and 1,032 in the survey. I construct a variable to measure a firm's dependence on Commerzbank in 2006, called $CB\, dep_{fc}$ for firm f in county c. It equals the fraction of the firm's relationship banks that were

Commerzbank branches out of the firm's total number of relationship banks:

$$CB\ dep_{fc} = \frac{number\ of\ relationship\ banks\ that\ are\ Commerzbank\ branches_{fc}}{total\ number\ of\ relationship\ banks_{fc}}. \tag{1.1}$$

I additionally construct a county panel dataset from 2000 to 2012. It contains data on GDP, employment, and migration from the German Statistical Federal Office. A variable called county Commerzbank dependence ($\overline{CB\ dep_c}$ for county c) measures the average value of firm Commerzbank dependence for firms with their head office in the county, using all 112,344 firms in the dataset of relationship banks. For each firm, I additionally construct a variable $\overline{CB\ dep_{fc}}$ that measures the average Commerzbank dependence of all the other firms in the county, from the point of view of an individual firm (leave-out mean). I calculate the distance measures for the IV specifications using the average geodesic distance between firms in the county and the location of the former Commerzbank head offices.

The household panel I analyze is the nationally representative German Socio-Economic Panel (GSOEP). In 2002, 2007, and 2012 individuals reported the value of their outstanding debt. Every year they also reported a binary variable for whether they had any outstanding debt.

In some specifications in the paper, the outcome variable is the symmetric growth rate, a second-order approximation to the ln growth rate. This measure is bounded in the interval [-2,2]. It has become standard in the establishment-level literature because it naturally accommodates zeros in the outcome variable, for example due to zero household debt or firm exit (Davis, Haltiwanger, and Schuh 1998a).[4]

Table 5.1 summarizes the firm panel. Firms have an average of 3 relationship banks. German firms traditionally form close and durable ties to their relationship banks. Dwenger, Fossen, and Simmler (2015) report that only 1.7 percent of firms find a new relationship bank per year. There is no information in my data on what services exactly a firm receives from a particular bank. In a separate survey, Elsas (2005a) finds that relationship banks mostly finance bank loans, both long- and short-term, and provide payment transactions. A histogram of firm Commerzbank dependence is in the left panel of Figure 5.3. Just under half of firms have a Commerzbank branch among their relationship banks. The average value of firm Commerzbank dependence is 0.16.

To test whether firms borrowing from Commerzbank differ from other firms, I regress firm Commerzbank dependence on observables from the year 2006 using the firm panel. There is no evidence for an economically significant correlation between

[4]The formal definition of the symmetric growth of y between t-1 and t is: $g^y = 2 \cdot \frac{(y_t - y_{t-1})}{(y_t + y_{t-1})}$. The firm panel contains some insolvencies, but no cases of zero employment, because the German insolvency process takes long. The employment cross-section contains some cases of zero employment in 2012, because it includes more small firms, which have faster insolvency processes.

Commerzbank dependence and any of the firm characteristics, controlling for county and industry. An analysis of firm summary statistics by bins of Commerzbank dependence is in 6.1.

In general, my firm datasets underweight small firms and the service sector relative to the population. In the population, 98 percent of firms have under fifty employees and 60 percent are in the service sector (as defined by the Statistical Federal Office). In the employment cross-section, 72 percent of firms have fewer than 50 employees and 53 percent are in the service sector. The selection into the firm panel requires that Dafne reports balance sheet variables for every year. This leaves, on average, larger firms (15 percent under 50 employees) and fewer in the service sector (48 percent) in the firm panel. Importantly, the results in the two datasets turn out to be similar and there is no heterogeneity in the effects by firm size or sector.

County summary statistics are in Table 5.2. The mean population of a county in 2000 was 203,280 and mean county Commerzbank dependence is 0.12. There is significant variation in county Commerzbank dependence, as shown in the right panel of Figure 5.3 and in the map in Appendix Figure 6.1.

1.4 The Effect of the Lending Cut on Bank Debt

This section contains the first step of the empirical analysis. It verifies my empirical strategy by showing that Commerzbank's lending cut reduced the bank loan supply of firms. Hence, Commerzbank dependence is a valid proxy for firms' exposure to a lending cut. I find no effect on household debt and explain why.

1.4.1 Firm Survey Evidence on Commerzbank's Lending Cut

I examine whether firms dependent on Commerzbank perceived their banks to lend more restrictively. The results are in Table 5.3. The outcome variable is the answer to the question: "How do you evaluate the current willingness of banks to grant loans to businesses: cooperative, normal, or restrictive?" All the specifications control for firm industry, federal state, size, and age. A lagged dependent variable from 2006 accounts for pre-existing, time-invariant differences in bank loan supply.

The coefficient on firm Commerzbank dependence in column (3) has the interpretation that in 2009 a firm fully dependent on Commerzbank perceived its banks to be 0.47 standard deviations less willing to grant loans, compared to a firm with no Commerzbank relationship. The estimate is statistically significant at the 1 percent level. The effect remained significant in 2010, as Commerzbank continued its lending cut. There was no association between Commerzbank dependence and perceived bank loan supply in 2007 and 2008, indicating the absence of a pre-trend. Commerzbank repaid most of the government equity in 2011 and refocused its operations on the core business of lending. Accordingly, the negative effect of Commerzbank dependence disappeared in 2011 and turned positive in 2012. This is in line with Figure 5.1, which shows Commerzbank's lending stock returning to the same trend as the other commercial banks from 2011 onward. The lending cut only led to temporary credit constraints.

There was no difference in the perceived level of demand between firms dependent on Commerzbank and other firms in any year (6.3). This shows worse demand shocks cannot explain the reduction in loan supply.

1.4.2 The Effect of Commerzbank's Lending Cut on Firms' Bank Debt

Having established that firms dependent on Commerzbank reported reduced loan supply, I test whether the lending cut actually reduced bank debt. The outcome is the natural logarithm of firm bank loans. I run specifications using the firm panel dataset, including year and firm fixed effects. Table 5.4 presents the results. The regressor of interest is firm Commerzbank dependence interacted with d, a dummy for the years following the lending cut, 2009 to 2012.

The point estimate in column (1) indicates that firms dependent on Commerzbank held less bank debt after the lending cut, but the effect is imprecisely estimated. Column (2) controls for firm county, age, and size, while column (3) additionally conditions on industry and the export and import shares. These control variables improve the precision of the estimates. The coefficient in column (3) is statistically different from zero at the 1 percent level. It implies that a firm fully dependent on Commerzbank held 20.5 percent less bank debt in the years following the lending cut. This is similar to the decline in Commerzbank's aggregate lending stock by 17 percent during that period, compared to the other German banks (Figure 5.1).[5]

These results imply that Commerzbank dependence is a valid proxy for exposure to Commerzbank's lending cut. Firms dependent on Commerzbank were unable to substitute other lenders for Commerzbank. This was the case even though all firms were located in regions where other healthy lenders operated, as county Commerzbank dependence ranged from 1 to 31 percent. The results therefore suggest an important role for credit market frictions even in the presence of alternative healthy lenders.

1.4.3 The Effect of Commerzbank's Lending Cut on Household Debt

I investigate whether Commerzbank's lending cut also affected households' access to bank loans. 32 percent of Commerzbank's interest income in 2006 stemmed from

[5] There was no heterogeneity in the size of the lending cut by characteristics such as firm productivity, firm size, county Commerzbank dependence, or county economic growth (Appendix Figure 6.2). This suggests that Commerzbank did not cut lending disproportionately to firms with weaker growth prospects. Heterogeneity in the lending cut would not affect my identification strategy, since I use predetermined Commerzbank dependence as proxy for lending cut exposure.

households. Table 5.5 analyzes the household panel GSOEP. The outcome in the first three columns is the symmetric growth rate of debt. The effect of county Commerzbank dependence is small and statistically insignificant in all specifications. The estimate in column (2) controls for county characteristics and predetermined individual debt holdings. It implies that households in a county entirely dependent on Commerzbank experienced an increase in their growth rate of debt between 2007 and 2012 by 0.7 percentage points. Adding individual control variables in column (3) raises the coefficient, but it remains insignificant. The outcomes in columns (4) to (8) are dummies for whether an individual has any outstanding debt in the given year. There is no significant effect of county Commerzbank dependence in any year between 2008 and 2012.

These results can be explained by features of the German financial system that facilitate bank-switching for households. For example, the government-owned development bank KfW co-finances nationally standardized mortgage contracts in cooperation with private and public banks. This is important because mortgage debt comprised 91 percent of German household debt. Households can apply for these mortgages through any bank, regardless of whether they have a pre-existing relationship bank or not. KfW raised its mortgage commitments to households by 26.5 percent during the crisis. Aggregate lending to private customers by commercial banks actually rose slightly between 2007 and 2010, which suggests that other commercial banks were able to compensate households for Commerzbank's lending cut. In contrast, aggregate lending to corporate borrowers by commercial banks fell, which implies firms were not able to turn to other lenders. Consistent with these findings, a recent paper by Jensen and Johannesen (2017) shows that when bank-switching costs are low, there is no effect of lending cuts by individual banks on household debt.

1.5 The Direct Effect on Firms

Having established that Commerzbank dependence is a valid proxy for firm exposure to Commerzbank's lending cut, I proceed to estimating the real effects of the lending cut on firms. This section focuses on the *direct* effect, which is driven by firm's immediate financial connections to banks that cut lending. The effect operates independently of the economic environment the firm faces. That means it is a partial equilibrium response, identified by comparing two similar firms affected by the same aggregate shocks. The direct effect has been the focus of the firm-level literature, for example Almeida, Campello, Laranjeira, and Weisbenner (2012) and Chodorow-Reich (2014a).

1.5.1 Firm Specification

I use the firm panel to estimate equation 1.2, for firm f in county c at time t. β is the direct effect. d_t^{post} is a dummy for the years following the lending cut, 2009 to 2012:

$$y_{fct} = \zeta + \beta\, CB\, dep_{fc} * d_t^{post} + \kappa_c * d_t^{post} + \Gamma' X_{fc} * d_t^{post} + \gamma_{fc} + \lambda_t + \varepsilon_{fct}. \quad (1.2)$$

The specification includes county fixed effects interacted with the post-lending cut dummy, $\kappa_c * d_t^{post}$. This is an important step in isolating the direct effect. It keeps constant any county-specific shocks associated with the Commerzbank dependence of other firms in the county. Firm fixed effects γ_{fc} account for time-invariant, firm-specific differences in the outcome. Year fixed effects λ_t control for changes in the outcome that are common to all firms in a year, for example due to macroeconomic fluctuations. X_{fc} is a vector of further control variables, listed in Table 5.6. The standard errors are two-way clustered at the level of the county and the industry.

The identifying assumption in this section is that there were no unobservable shocks within counties correlated with firm Commerzbank dependence. The evidence supports this assumption. Figure 5.4 shows that firms with and without a relationship to Commerzbank followed parallel employment trends before the lending cut. The firm panel shows no strong correlation between Commerzbank dependence and firm observables in 2006 (6.1). There was no effect of Commerzbank dependence on perceived product demand in any year before the lending cut, and an effect on perceived credit constraints only during the lending cut (6.3).

1.5.2 Firm Results

Table 5.6 reports the main result of this section in column (3). The point estimate implies that, following the lending cut, employment at a firm fully dependent on Commerzbank was on average 5.3 percent lower than at a firm with no Commerzbank relationship. The modest impact of the control variables across the first three columns of Table 5.6 strengthens the argument that Commerzbank dependence was not significantly correlated with other determinants of firm growth. The existing literature estimates direct effects of a similar magnitude, suggesting that Commerzbank's lending cut has external relevance. For instance, Chodorow-Reich (2014a) for the United States and Bentolila, Jansen, Jiménez, and Ruano (forthcoming) for Spain find that firms connected to distressed banks reduced employment growth by 4 to 5 percentage points.

The remaining results in Table 5.6 support the view that reduced bank loan supply was responsible for the effect of Commerzbank dependence, rather than unobserved shocks hitting all firms dependent on Commerzbank. Column (4) reports no statistically significant effect on firms with a low share of bank loans out of total debt. The effect on bank-dependent firms is strong. Column (5) shows there is no effect on firms with Commerzbank dependence greater than 0 and up to one-quarter. These firms had a relatively large number of other relationship banks that could step in after Commerzbank cut lending. The effect is strongest for firms with Commerzbank dependence over one-half, which had few alternative options to access bank loans.[6]

Table 5.7 analyzes other outcomes and thereby sheds light on how firms adjust to a lending cut. The capital stock decreased by an average of 13 percent. Therefore, the capital-labor ratio fell, which suggests firms use bank loans primarily to finance capital investment. Firms dependent on Commerzbank were capital-constrained, which increased their average product of capital, measured as value added per capital in column (3). On the contrary, the lending cut did not affect the average product of labor and the average wage, relative to other firms in the same county, as shown in columns (4) and (5) respectively. This is consistent with a competitive county labor market. Column (6) reports no effect on the interest rate, in line with evidence from the United States credit card market (Ausubel 1991).

1.6 The Effect on Counties

The previous section established that there were significant direct effects of the lending cut on firms. In this section, I test whether the lending cut also had effects at a higher level of aggregation, on counties.

1.6.1 County Specification

I estimate equation 1.3 for county c at time t:

$$y_{ct} = \zeta + \rho \, \overline{CB\,dep_c} * d_t^{post} + \Gamma' X_c * d_t^{post} + \gamma_c + \lambda_t + \varepsilon_{ct}. \tag{1.3}$$

The coefficient on $\overline{CB\,dep_c} * d_t^{post}$, scaled by 100, measures the average percentage change in the outcome following the lending cut in a county fully dependent on Commerzbank. γ_c is a county fixed effect and λ_t a year fixed effect. X_c is a vector of time-invariant control variables, described in the notes of Table 5.5. The standard errors are clustered at the level of 42 quantiles of the county's industrial production share (GDP share of mining, manufacturing, utilities, recycling, construction). This is

[6] In unreported results, I find no heterogeneity in the effect on capital-intensive industries (consistent with Paravisini, Rappoport, Schnabl, and Wolfenzon (2015)), on large firms (consistent with Bentolila, Jansen, Jiménez, and Ruano (forthcoming)), on firms in counties with relatively high county Commerzbank dependence, or on firms dependent on Dresdner Bank before the 2009 acquisition. 6.4 shows firms dependent on Commerzbank did not suffer higher losses on the value of their financial assets during the financial crisis.

a more general method than clustering at the level of the county. It allows for arbitrary correlations of the errors across counties of similar industrial structure.

1.6.2 County OLS Results

The left panel of Figure 5.5 plots the growth rate of county GDP from 2007 to 2012 against Commerzbank dependence. The line of best fit shows a statistically significant negative relationship, suggesting that the lending cut lowered GDP growth.

Table 5.8 reports the results of the corresponding OLS specifications. The key result of this section is in column (2). The point estimate implies that a standard deviation increase in Commerzbank dependence (6 percentage points) lowered county GDP by an average of 1 percent after Commerzbank's lending cut. This specification controls for the two main identification concerns. The first concern is that idiosyncratic shocks to certain industries and exposure to the trade collapse during the Great Recession may be correlated with Commerzbank dependence. I control for the share of 17 industries among the county's firms in 2006 as well as the average export and import shares of firms in the county. The second main concern is that some regions fared worse because they were in the former GDR or because their Landesbank suffered losses in the financial crisis (Puri, Rocholl, and Steffen 2011). I add dummies for counties in these regions to the specification. Column (3) tests the robustness of the result further, by controlling for population density, ln population, ln GDP per capita, and household leverage. The coefficient remains stable, suggesting that the results are not driven by pre-existing differences in county characteristics.

The specification in column (4) estimates that a standard deviation increase in Commerzbank dependence lowered county employment by an average of 0.83 percent, conditional on the main controls.[7] Following Blanchard and Katz (1992), I investigate whether the effects can be explained by migration across counties in column (5). The outcome is county net migration divided by 2006 employment. The coefficient is insignificant and small, implying there was no migratory response. Mertens and Haas (2013) similarly report no association between county unemployment rates and migration in Germany.

1.6.3 County IV Results

I use the distance instrument to test whether there is any evidence for bias in the OLS estimates. The right panel of Figure 5.5 plots the growth rate of GDP from 2007 to 2012 against the distance instrument. There is a negative and statistically significant

[7] Burda and Hunt (2011) show that the German government's well-known short-time work scheme did not have a strong effect on the labor market. Firms could only claim subsidies for a maximum of 2 years. The level of short-time workers was back down to its pre-crisis value in 2011, suggesting if anything only a transitory impact (Fujita and Gartner 2014).

reduced-form relationship. Figure 5.6 confirms that the growth rate of GDP was lower only during the years of Commerzbank's lending cut. In the figures and in all IV specifications, I add five separate linear distance control variables, measuring the distances to five former head offices in Düsseldorf, Frankfurt, Hamburg, Berlin, and Dresden. This ensures that the effect is identified only through the distance to the closest of Commerzbank's postwar head offices. I also include a dummy for the former GDR to account for the postwar breakup of Germany.

Table 5.9 reports the regression results. Columns (1) and (2) show a strong first-stage relationship between the distance instrument and Commerzbank dependence. The IV second-stage coefficients in columns (3) to (7) report negative and significant effects on county GDP and employment and no effect on migration, consistent with the OLS results. Adding the list of control variables hardly affects the point estimates, strengthening the argument that the distance instrument is exogenous to county growth.[8]

In general, the IV point estimates imply larger effects than the OLS estimates. The coefficient in column (4) implies a GDP loss of 2.2 percent from a standard deviation increase in Commerzbank dependence, conditional on the main controls. There could be a number of reasons for the difference. First, county Commerzbank dependence may be measured with error, since it is based on the Creditreform sample of firms, which covers roughly half of total employment in Germany. Measurement error would attenuate the OLS, but not the IV estimates. Second, there is some evidence that Commerzbank's expansion across German counties was driven by economic considerations. For example, Klein (1993) describes that Commerzbank followed a unique branch expansion strategy in the former GDR after German reunification in 1990. The other German banks simply took over the pre-existing branch networks of the former GDR state banks, while Commerzbank built up its own. Commerzbank may have selectively expanded into counties that are less affected in recessions. In unreported results, I find no general association between county Commerzbank dependence and the average annual growth rate between 2000 and 2009. Only in the sole recessionary year 2003, counties dependent on Commerzbank grew faster. If this indicates a systematic positive correlation between county Commerzbank dependence and growth in recessions, OLS estimators of the effect of Commerzbank's lending cut on county growth would be biased upwards.

[8] Appendix Table 6.3 reports that the linear distances to postwar Commerzbank head offices or other major cities are uncorrelated with growth after the lending cut, conditional on the distance instrument. Appendix Table 6.4 shows that controlling for the linear distances removes the correlation between the instrument and a number of county characteristics. I confirm the effects of Commerzbank's lending cut using a county-level proxy for the change in bank loans in 6.7. An unreported placebo experiment for Deutsche Bank, using the distance to the closest postwar Deutsche Bank head office as instrument, finds no effect of Deutsche Bank dependence on county growth. Hence, there is no generic effect from dependence on large banks.

It is important to recognize, however, that the OLS and IV coefficients are not statistically different. This suggests the difference between the point estimates could also be driven by estimation error. The most important insight from this section is that the IV analysis confirms the negative effect of Commerzbank's lending cut on county growth.

1.7 Discussion of the Results

With the firm and county estimates in hand, I turn to discussing two aspects of how the lending cut affected firms and counties. First, I examine how the direct, firm-level effects translated into county outcomes. Specifically, I test whether there is evidence for an *indirect* effect on all firms in counties with high county Commerzbank dependence, independent of the firms' individual banking relationships. Second, I show that the temporary lending cut had *persistent* effects on firms and counties.

1.7.1 The Indirect Effect

The response of county aggregates depends on two types of firm-level effects. The first are the direct effects on firms borrowing from Commerzbank. In addition, there may also be indirect effects on all firms in a county. Such indirect effects arise through changes in the county's aggregate economic conditions due to the direct responses of firms borrowing from Commerzbank. This section explores whether indirect effects played a role in shaping the effect of the lending cut on counties.

I use the employment cross-section dataset to estimate equation 1.4. The larger sample size of 48,101 firms enables me to estimate the direct effect β and the indirect effect σ in the same specification. The outcome is the symmetric growth rate of firm employment between 2008 and 2012:

$$employment\ growth_{fc} = \zeta + \beta\, CB\ dep_{fc} + \sigma\, \overline{CB\ dep_{fc}} + \Gamma' X_{fc} + \xi_{fc}. \tag{1.4}$$

Table 5.10 presents the results. The main object of interest in this section is the indirect effect, that is the coefficient on the average Commerzbank dependence of other firms in the county. I include firm control variables in column (1). The point estimate is negative and statistically significant at the 5 percent level. Adding the county controls in column (2) hardly affects the estimate. To illustrate the size of the indirect effect implied by the point estimates, consider a firm fully dependent on Commerzbank, operating in a county where no other firm had Commerzbank among their relationship banks. This firm reduced employment growth between 2008 and 2012 by 3.6 percentage points, the direct effect.[9] If the same firm had operated in a county where

[9]This point estimate of the direct effect is slightly smaller than in Table 5.6, because I use a different

the Commerzbank dependence of the other firms had been one standard deviation (6 percentage points) greater, employment growth would have fallen by 4.6 percentage points. In this latter county, firms with no direct relationship to Commerzbank would have reduced employment growth by 1 percentage point, solely due to the indirect effect.

Table 5.11 gives an overview of the county employment change implied by the different estimates in the paper. The estimate in row 1, based solely on the direct effect, underestimates the county employment loss, because it ignores the indirect effect. The average county Commerzbank dependence is 0.12, so the direct effects harm only a relatively small fraction of firms. It is the indirect effect that amplifies the effects of the lending cut throughout the county economy. The estimates of the sum of direct and indirect effects are larger than the estimate in row 1, whether I use the county data (rows 2 and 3) or the firm data (row 4). The IV estimate based on the county dataset is close to the OLS estimate based on the firm employment cross-section dataset, supporting the view that there is no significant bias in the OLS estimates.

I turn to investigating which economic mechanisms underlie the indirect effect, by testing two theoretical channels. The first argues that the direct effects reduced local agglomeration spillovers. These can exist in the form of knowledge spillovers, transport costs of inputs and outputs, or the quality of the local labor market (Ellison, Glaeser, and Kerr 2010; Greenstone, Hornbeck, and Moretti 2010; Bloom, Schankerman, and Van Reenen 2013). There is evidence that high-innovation industries are particularly dependent on such spillovers (Jaffe, Trajtenberg, and Henderson 1993; Audretsch and Feldman 1996; Henderson 2003). This leads to the hypothesis that the indirect effect should increase with the innovation intensity of an industry. I classify industries with R&D spending in excess of 2.5 percent of revenue (the OECD cut-off) as high innovators, using data on German industries from Gehrke, Frietsch, Neuhäusler, Rammer, and Leidmann (2010). For low-innovation industries, I rely on Gehrke, Frietsch, Neuhäusler, Rammer, and Leidmann (2013), who identify a group of industries with the lowest score on all innovation indicators in the Mannheim Innovation Panel. The lists of high- and low-innovation industries are in Appendix Tables 6.5 and 6.6.

The second theoretical channel argues that household consumption fell due to employment losses at firms dependent on Commerzbank, reducing aggregate demand in the county. Producers of non-tradables rely strongly on local demand. Producers of tradables, on the other hand, mainly depend on national and global demand. Following the methodology of Mian and Sufi (2014b), I classify an industry as tradable if the sum of its exports is at least USD 10,000 per worker or USD 500 million in total (using industry data from the United States). The retail and restaurant sector are clas-

outcome, the symmetric growth rate. Using the ln difference as outcome renders the point estimates almost identical.

sified as non-tradable. In addition, firms with a Herfindahl index in the top quartile produce tradables and firms in the bottom quartile produce non-tradables. This uses the fact that non-tradable industries are highly dispersed, because they need to produce locally in the markets they serve, while tradable industries tend to be concentrated. If industries remain unclassified, I call them producers of part-tradables.

The interaction of innovation and tradability leaves me with seven industry types.[10] I estimate a separate indirect effect for each industry type, by interacting the variable $\overline{CB\ dep_{fc}}$ in equation 1.4 with a full set of industry type dummies. The specification controls for the direct effect, by including the variable $CB\ dep_{fc}$. In addition to the full set of firm and county control variables, the specification also includes fixed effects for the categories of tradability and innovation, to ensure that the coefficients are not biased by common shocks to firms in these categories.

Figure 5.7 plots estimates of the indirect effect by industry type. There is a statistically significant indirect effect for high-innovation producers of tradables and producers of non-tradables.[11] The effect on high-innovation firms is consistent with agglomeration spillovers particular to these industries. In unreported results, I find that the Commerzbank dependence of other high-innovation firms in the county drives the indirect effect on high-innovation firms. There is no significant indirect effect from the Commerzbank dependence of low- and medium-innovation firms. Furthermore, the indirect effect is larger in counties with a high, above-median density of high-innovation firms. This suggests agglomeration spillovers are more important in innovation clusters.

The significant indirect effect on producers of non-tradables is consistent with the second theory on demand. After directly affected firms in their county reduced employment, producers of non-tradables experienced the largest reduction in demand relative to the other industry types and cut employment.[12] Moretti (2010) studies the local employment multiplier in the US, finding that for each additional job in the tradable sector, 1.6 jobs are created in the non-tradable sector. The corresponding figure in my setting is 1.7.[13] Hence, my estimate of the local demand channel is close to Moretti

[10] The industry shares in my sample are: producers of tradables with low innovation activities: 2 percent; tradables, medium: 29; tradables, high: 8; part-tradables, low: 11; part-tradables, medium: 25; non-tradables, low: 5; non-tradables, medium: 20. Few firms are high-innovation part-tradables and non-tradables producers, so I add them to the medium-innovation industry types.

[11] I find no significant heterogeneity by industry type in the direct effect, so this cannot explain the results. In a robustness check, I find similar results when I do not follow the Mian and Sufi (2014b) methodology, but instead classify firms with a strictly positive export share as tradable producers.

[12] Changes in household debt cannot explain the non-tradable indirect effect. Di Maggio and Kermani (2017) estimate an elasticity of non-tradable employment with respect to household debt of 0.2. Using their estimate, the lower bound of the 90 percent confidence interval of the household debt effect from column (1) of Table 5.5 can only explain 15 percent of the indirect effect on non-tradable, low-innovation firms' employment.

[13] To get this figure, I first calculate the effect of the lending cut on tradable employment in a county, in which the tradable sector is fully dependent on Commerzbank. The direct effect leads to an employment

(2010).

The two theories predict no indirect effect on producers of tradables with low innovation activities. Indeed, the coefficient on these firms in Figure 5.7 is positive and statistically insignificant. In an unreported test, I also find no indirect effect for low- and medium-innovation tradables producers located in an industrial cluster, unlike for high-innovation firms. Furthermore, I find no heterogeneity in the direct effect by county Commerzbank dependence. This implies that potential increases in the difficulty of finding new lenders cannot explain the indirect effect.

1.7.2 The Persistence of the Effects

Firms dependent on Commerzbank reported restrictive bank loan supply in 2009 and 2010, but not in any year before or after (Section 1.4.1). Figure 5.4 shows that employment at firms with Commerzbank among their relationship banks developed in parallel to other firms before the lending cut. In 2009 and 2010, firms dependent on Commerzbank grew more slowly. Afterwards they remained on a lower, parallel trend for two years. Figure 5.6 illustrates the same pattern for counties. Counties close to the postwar head offices, with greater Commerzbank dependence, grew more slowly during the years of the lending cut and did not recover afterwards.

Such persistent losses do not occur in response to all economic shocks. For example, firms and counties exposed to the drop in export demand during the Great Recession converged to the level of unaffected firms and counties in under two years, as shown in 6.8. A standard neoclassical production function implies that temporary shocks to the capital stock do not lead to persistent output losses. But there is no such mechanism that facilitates convergence after productivity losses. I investigate whether there is evidence that the lending cut lowered innovation and productivity.

Table 5.12 examines the effect of the lending cut on firms' innovation activities, proxied by patents. The outcome in column (1) is the symmetric growth rate of the number of patents between the periods before (2005-08) and after Commerzbank's lending cut (2009-12). If a firm produced no patents in either period, the growth rate is set to zero. If a firm produced at least one patent from 1990 to 2004, I call it a patenting firm. The effect on these patenting firms is large. The growth rate of the number of patents was approximately 55 percentage points lower at patenting firms entirely de-

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loss of 3.5 percent for all tradable producers (estimated in the regression for Figure 5.7). In addition, 21 percent of tradable producers are high-innovators, so they also suffer the indirect effect of 39.9 percent. Overall, tradable employment declines by approximately 3.5 + 0.21*39.9 = 11.9 percent. The indirect effect on the average non-tradable firm is 25.9 percent. 23 percent of firms produce tradables. Therefore, the indirect effect reduces non-tradable employment by 0.23*25.9 = 6 percent. Multiplying the elasticity of non-tradable to tradable employment by 3.33, the ratio of non-tradable jobs to tradable jobs, gives the figure of 1.7. Further evidence on the local demand channel can be found in Bernstein, Colonnelli, Giroud, and Iverson (2017), Charles, Hurst, and Notowidigdo (forthcoming), and Giroud and Mueller (2017).

pendent on Commerzbank. There is no effect on non-patenting firms. It is possible that many non-patenting firms are structurally unsuited to ever issue patents, independent of credit supply, or that in a period of low global growth, few firms choose to commence patenting. Negative binomial count models in columns (2) and (3) confirm that after the lending cut, patenting firms dependent on Commerzbank issued significantly fewer patents. There was no significant difference before the lending cut.[14]

A growth accounting exercise can inform an estimate of productivity changes at the county level. Conventional measures of TFP overestimate productivity losses during recessions, because they do not account for decreases in the utilization of existing labor and capital (Basu, Fernald, and Kimball 2006). Since the lending cut had no effect on county growth in 2011 and 2012, I alleviate this problem by focusing on changes from 2008 to 2012. An IV specification estimates that a standard deviation increase in Commerzbank dependence lowered output per worker by 1.8 percent from 2008 to 2012. There are no data on county capital. I rely on the firm panel to estimate that the capital-labor ratio at firms fully dependent on Commerzbank fell by 14.8 percent. Under the assumption that for all the other firms the capital-labor ratio grew at an identical rate, growth accounting implies that a standard deviation increase in Commerzbank dependence reduced county TFP by 1.4 percent from 2008 to 2012. Fernald (2014) provides data on utilization-adjusted capital and labor inputs for the United States. I construct an adjustment factor to inflate my estimates of the changes in capital and labor. This factor is based on the average ratio of utilization-adjusted to unadjusted input changes, measured two years after the last three NBER recessions in Fernald's data. Incorporating this adjustment slightly lowers the estimated TFP shortfall to 1.3 percent. This point estimate needs to be treated with caution, since it relies on strong assumptions about the loss in capital and the utilization adjustment.[15] Overall, however, the firm and county data paint a consistent picture. The results suggest innovation and productivity fell after the lending cut, which could explain the persistent losses.

1.8 Conclusion

This paper presents new evidence on the causal effects of bank lending on economic activity. It analyzes a lending cut by Commerzbank, a large German bank. The lending cut was not caused by domestic factors, but it was imported to Germany through Commerzbank's trading losses on international financial markets during the financial crisis of 2008/09. The results show that the lending cut lowered the output and employment

[14]The average patenting process takes around two years. In unreported results, I find the effect on patents is entirely driven by the years after 2011, with no significant difference for the years before.

[15]I carry out two robustness checks. First, the estimate of TFP growth remains negative when I use adjustment factors larger than any value observed two years after a recession in Fernald's data. Second, to explain the output loss while keeping TFP constant, capital would have had to fall by 5.6 percent. This equals 1.9 times the output loss, which is implausibly large given historic movements.

of firms and counties dependent on Commerzbank. Employment at a firm fully dependent on Commerzbank fell by 5.3 percent, while a standard deviation increase in county Commerzbank dependence reduced county employment by 0.8 percent.

Two key findings stand out. First, there were indirect effects of the lending cut that affected firms independently of their immediate bank loan supply. The results suggest that these indirect effects operated through lower aggregate demand and reduced agglomeration spillovers among high-innovation firms. Second, a bank lending cut causes an extended hangover. Both firms and counties dependent on Commerzbank experienced lower growth rates during the years of the lending cut. Thereafter, they returned to the growth rates of unaffected firms and counties, but did not converge to the unaffected levels. This pattern resembles the growth experience of the United States and other developed economies following the financial crisis of 2008/09.

The findings in this paper contribute to the academic discussion about the Great Recession and its aftermath. Reifschneider, Wascher, and Wilcox (2015) and Anzoategui, Comin, Gertler, and Martinez (2017) interpret the productivity slowdown following the Great Recession as an endogenous response to weak aggregate demand. This paper's finding of an indirect demand effect suggests that bank lending cuts during the financial crisis can partially account for the aggregate demand shortfall. In addition, the evidence in this paper shows a direct, causal link from bank lending cuts to lower innovation and productivity. Since economies are unable to make up productivity shortfalls in only a few years, recoveries from banking crises are slow. This pattern can be seen in the slow recovery from the Great Recession and the lengthy recessions associated with banking crises in the cross-country literature.

Chapter 2

The Effect of House Prices on Household Borrowing: A New Approach

2.1 Introduction

It is a well-known fact that house prices are strongly correlated with household borrowing and consumption over the business cycle. These comovements have existed for a long time and were especially strong around the Great Recession. We illustrate this in Figure 6.3, which shows the evolution of house price growth, consumption growth, and mortgage debt growth in the US and UK over the last four decades. Motivated by such macro patterns, a leading narrative about the Great Recession argues that house price swings drive borrowing and consumption (e.g. Mian and Sufi 2011, 2014a; Mian, Rao, and Sufi 2013; Kaplan, Mitman, and Violante 2015). In this paper we revisit this question using a new approach, providing evidence both on the effect of house prices on borrowing and on the underlying mechanisms driving the effect.[1]

This is an area where causal identification is particularly difficult, because house price variation is endogenous and compelling quasi-experiments are difficult to find. The time series evidence in Figure 6.3 does not have a causal interpretation, a point emphasized by Campbell and Cocco (2007) and Attanasio, Blow, Hamilton, and Leicester (2009); Attanasio, Leicester, and Wakefield (2011). Much of the recent liter-

[1] I confirm that Chapter 2 was jointly co-authored with James Cloyne, Ethan Ilzetzki, and Henrik Kleven and I contributed 25% of this work. This research was carried out as part of the Bank of England's One Bank Research Agenda. It uses Financial Conduct Authority (FCA) Product Sales Data that have been provided to the Bank of England under a data-sharing agreement. The FCA Product Sales Data include regulated mortgage contracts only, and therefore exclude other regulated home finance products such as home purchase plans and home reversions, and unregulated products such as second charge lending and buy-to-let mortgages. The views expressed are those of the authors and do not necessarily reflect the views of the Bank of England, the Monetary Policy Committee, the Financial Policy Committee or the Prudential Regulatory Authority.

ature instead uses variation in house price growth across geographical areas, which raises concerns about confounding regional shocks (such as shocks to local income expectations) that drive both house prices and the outcome of interest. This requires the use of an instrument for regional house price growth, but fully conclusive instruments are difficult to find.[2]

Motivated by these challenges, we consider a different setting and a different approach to study the effect of house prices on borrowing. We examine the borrowing decisions of home refinancers using administrative data on the universe of mortgage contracts in the UK from 2005-2015. Our data and setting offer three main advantages. First, the dataset has information on *individual* house prices from mortgage appraisals by lenders. We present evidence showing that, in the UK, mortgage appraisals provide unbiased measures of actual house prices. Second, the data has a *panel dimension* as many homeowners refinance several times during the 10-year window we consider. This results from the fact that refinancing is a frequent phenomenon in the UK, because long-term fixed interest mortgages are not available (see Best, Cloyne, Ilzetzki, and Kleven 2015). The panel dimension of the data allows us to control for a rich set of fixed effects that deal with the standard confounders discussed in the literature. For example, confounding regional shocks will not be a threat to identification here as we control for county-by-time fixed effects.

Third and finally, the *institutional setting* helps with identification. Most mortgage products in the UK come with a relatively low interest rate for a short time period, typically 2-5 years, followed by a much higher reset rate. This creates a strong incentive to refinance around the onset of the reset rate, and we show that most homeowners do in fact refinance around this time. This implies that the timing of refinance is determined by past contract choices, namely the duration of the initial low interest rate in the last contract.[3] These mortgage institutions combined with the large house price swings over the period we consider create a potential quasi-experiment. Refinancers face very different house price shocks depending on whether they refinance before, during, or after the housing crisis, and this timing is determined largely by a mortgage contract choice made in the past. Loosely worded, we use the Great Recession interacted with pre-determined, idiosyncratic contract choices as a quasi-experiment for house prices.

We present three main sets of results. The first set of results concerns the impact of house prices on homeowner borrowing. While such borrowing effects are interesting

[2]Much recent work instruments regional house price growth using a topography-based measure of housing supply elasticities, namely proximity to mountains and oceans that restrict supply (as constructed by Saiz 2010). The idea is that regional housing markets are exposed differently to demand shocks because of their topography. A debate about this instrument highlights potential issues with the exclusion restriction and defiers (see e.g., Davidoff 2013, forthcoming).

[3]This quasi-exogeneity of refinancing stands in contrast to the US setting where the decision to refinance is endogenous to factors such as income shocks, liquidity needs, and the market interest rate (see Hurst and Stafford 2004).

in their own right (e.g. Mian and Sufi 2011), they are also indicative of the potential consumption effects of house prices and they relate to the same underlying mechanisms as consumption. We find clear evidence that house price appreciation induces homeowners to increase borrowing by extracting equity from their home, but the magnitude of the response is smaller than suggested by recent US estimates. The elasticity of borrowing with respect to house prices lies between 0.2-0.3 and is robust across a range of specifications.[4] In our preferred specification, the elasticity is identified from within-individual variation in house price growth. This variation comes from homeowners who refinance at least twice and experience different house price shocks due to how their (quasi-exogenous) refinance timing interacts with the housing cycle. Unlike previous studies, our results are based on non-parametric, graphical analyses in which we do not impose any *a priori* assumptions on functional form. A finding from this approach is that the borrowing elasticity is constant across the distribution of house price changes.[5]

The second set of results concerns patterns of heterogeneity and mechanisms. The two main reasons why house prices may affect borrowing are wealth effects and collateral effects (see e.g., Sinai and Souleles 2005; Berger, Guerrieri, Lorenzoni, and Vavra 2015).[6] All else equal, the wealth effect should be larger for older homeowners who have short horizons and are therefore in a position to cash in on their housing wealth, while the collateral effect should be larger for more leveraged homeowners. The existing literature has tried to distinguish between different mechanisms by studying such patterns of heterogeneity (Campbell and Cocco 2007; Attanasio, Blow, Hamilton, and Leicester 2009; Attanasio, Leicester, and Wakefield 2011). A challenge for such exercises, however, is that different dimensions of heterogeneity are highly correlated. For example, older homeowners have shorter horizons and more asset risk, but are also less levered, and so it is not clear if the age profile is picking up wealth or collateral effects. We resolve this issue through a multivariate and non-parametric analysis of heterogeneity in the elasticity of borrowing with respect to house prices. We consider four dimensions simultaneously: loan-to-value (LTV), age, income, and income growth. Our approach shows how the borrowing elasticity varies across bins of a given

[4]These elasticity estimates are roughly half the size of the US estimates provided by Mian and Sufi (2011).

[5]The finding of an isoelastic relationship motivates our focus on log-log specifications through most of the paper, because the log-log coefficient is a direct estimate of the elasticity (in robustness checks, we also report estimates of the marginal propensity to borrow). Reporting the elasticity also eases comparisons to the part of the literature that estimates the elasticity of total borrowing, as opposed to only mortgage borrowing, because there is no mechanical reason why these elasticities should differ. A possible economic reason for the elasticities to differ is that mortgage debt is generally cheaper than other forms of consumer debt, in which case households may shift debt onto their mortgage following a house price increase. Such shifting would lead our elasticity of mortgage borrowing to *overestimate* the elasticity of total borrowing.

[6]A third possible reason is the presence of substitution effects on housing consumption, but this channel is shut down here as we consider refinancers who stay in their existing houses.

dimension, while simultaneously allowing for differences in the elasticity across bins of the other three dimensions. The striking finding from this analysis is that there is essentially no heterogeneity in any dimension except one — loan-to-value — but this dimension is strong. More levered households are more responsive to house prices, with borrowing elasticities around 0.7 at loan-to-value ratios above 85%. By contrast, the age profile is completely flat after controlling non-parametrically for the other dimensions.

The strong relationship between borrowing elasticities and LTV is consistent with evidence on subprime borrowing in the US (Mian and Sufi, 2009, 2011), and it is strongly suggestive of collateral effects. The UK mortgage market offers an interesting way to investigate the collateral channel more deeply, arising from the presence of *observable* credit constraints that depend on collateral. Specifically, as described and analyzed by Best, Cloyne, Ilzetzki, and Kleven (2015), the mortgage interest rate features discrete jumps (notches) at critical LTV thresholds. These notches can be interpreted as soft credit constraints, around which increases in LTV (reductions in collateral) sharply increase the cost of borrowing.[7] In such a setting it is natural that house price growth leads to larger borrowing by moving homeowners to lower notches and reducing their interest rate, a form of collateral effect.

This brings us to the third set of results, which shows that the borrowing elasticity depends critically on whether the underlying price variation relaxes credit constraints (by pulling homeowners down to lower notches), reinforces credit constraints (by pushing homeowners up to higher notches), or leaves credit constraints unchanged. This analysis shows that the elasticity is much higher (around 0.5) among homeowners whose collateral constraint is relaxed by house price growth, and that the elasticity is zero among those whose collateral constraint is reinforced. Furthermore, we present an analysis of the dynamic interaction between house price growth and bunching responses to interest notches, which is consistent with the collateral channel. We show that, when house price growth pulls homeowners below interest notches and relaxes their credit constraint, they respond by extracting equity until their LTV hits an interest notch. These findings provide direct evidence on the important role of collateral-based credit constraints in a large and representative population of homeowners.

Seminal contributions by Mian and Sufi (2011) and Mian, Rao, and Sufi (2013) have shaped the debate about the effect of house prices on household debt and consumption. Their important findings suggest that house price booms and busts were key determinants of US economic growth before and during the Great Recession. To identify the causal effects of house prices, Mian and Sufi (2011) and Mian, Rao, and Sufi (2013) rely on relatively strong assumptions about the correlation between the de-

[7]Conceptually, a hard credit constraint (often assumed in models) can be interpreted as the special case of a prohibitively large interest rate notch at a collateral threshold.

terminants of household debt and the topography of US regions. Stroebel and Vavra (2014) and Kaplan, Mitman, and Violante (2015) use similar assumptions to study the mechanisms through which house prices affect real outcomes. Influential papers by Muellbauer and Murphy (1990), Carroll, Otsuka, and Slacalek (2011), and Case, Quigley, and Shiller (2013) rely on time-series or cross-state variation in house prices, which makes causal interpretation difficult. The identification challenges are exemplified by Hurst and Stafford (2004) who show that the timing of refinancing is endogenous to household liquidity shocks, and by Attanasio, Leicester, and Wakefield (2011) who argue that macroeconomic shocks and expectations explain the correlation between house prices and household borrowing.

Our contribution takes the literature beyond these concerns about confounders and the validity of topography-based instruments for house prices. We identify the causal effects of house prices using a new quasi-experimental approach that allow us to control non-parametrically for the key confounders discussed in the literature. Making no *a priori* assumptions about the functional form between house prices and borrowing, we show that the relationship is roughly isoelastic. Exploiting that our data spans the period before and after the Great Recession, we provide some of the first evidence on how the borrowing elasticity varies over the economic business cycle.[8] Furthermore, our non-parametric and multivariate heterogeneity analysis is new to the literature and informs an unresolved debate. Previous studies have found a negative age profile of wealth effects, which is inconsistent with standard life-cycle models (Attanasio and Weber 1994; Attanasio, Blow, Hamilton, and Leicester 2009; Attanasio, Leicester, and Wakefield 2011; Mian and Sufi 2011; Berger, Guerrieri, Lorenzoni, and Vavra 2015; Bhutta and Keys 2016). We show that the negative age profile reflects the confounding effects of collateral and that the true age profile is flat.

An additional contribution of our paper is to shed new light on the role of collateral constraints in driving the effect of house prices on borrowing. The closest study to this aspect of our paper is by DeFusco (2016). He uses a compelling natural experiment in Montgomery County, Maryland to show that relaxations of collateral constraints increased borrowing. Our paper uses a larger and representative sample (the full population of UK homeowners), studies the effects of both relaxing and tightening collateral constraints, introduces the bunching methodology as a test of collateral constraints, and examines not only the effects of a collateral shock, but generally how households respond to a house price shock, including tests for the wealth channel. Two recent papers find that LTV-dependent borrowing constraints affect households in response to other shocks, such as debt reductions (Ganong and Noel 2017) and interest rate changes (Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru, and Yao forthcoming).

[8]See also Guren, McKay, Nakamura, and Steinsson (2017) for an analysis of time variation in housing wealth effects.

Given that much of the recent literature focuses on the US, it is natural to ask if our results are transportable to the US setting. Two points are worth highlighting. First of all, our empirical design — relying on within-individual variation — identifies micro elasticities rather than macro elasticities. This implies that the various reasons why macro elasticities can vary across economies (such as the underlying source of the house price shock as highlighted by Kaplan, Mitman, and Violante 2015) are not relevant for assessing external validity in this case. Second, while micro elasticities may clearly vary across different markets (say because of differences in the strength of credit constraints), we present one piece of evidence in support of external validity: When we use an empirical specification without fixed effects (similar to previous work) and consider the pre-recession period, we obtain borrowing elasticities of a similar magnitude to those found by Mian and Sufi (2011) for the US between 2002-06.

The paper is organized as follows. Section 2.2 describes the institutional setting and data, section 2.3 analyzes the sources of house price variation used for identification, section 2.4 presents results on the effect of house prices on borrowing, section 2.5 presents results on heterogeneity and mechanisms, and section 2.6 concludes.

2.2 Institutional Setting and Data

2.2.1 UK Mortgage Market

The UK mortgage market has several institutional features that make it an excellent laboratory for investigating the relationship between house prices and homeowner borrowing. In contrast to the US mortgage market, long-term fixed-rate mortgages are unavailable in the UK. Almost all mortgage products feature a relatively low interest rate for an initial period followed by a penalizing reset rate.[9] The initial rate typically has a duration of 2-5 years and this rate may be either fixed or floating. The reset rate lasts for the remainder of the mortgage's duration and is always floating. The reset rate is penalizing in the sense that the same bank almost always offers an identical mortgage product with a much lower rate. For example, at current rates a refinancer could lower her interest payments by more than 200 basis points (without altering the amortization schedule or other features of the mortgage) by refinancing to avoid the penalizing rate.

In addition to the penalizing reset after the end of the initial low-interest period, most mortgage contracts feature large early repayment charges, typically 5 or 10 percent of the outstanding loan. These charges make it very costly to refinance or adjust borrowing before the end of the initial period.

The combination of penalizing reset rates and heavy early repayment charges implies that households have strong incentives to refinance right around the end of the

[9]More than 90% of mortgage products feature such reset rate structures (see e.g., MoneyFacts.co.uk).

initial duration. To confirm that households act on these incentives, Figure 5.8 shows the distribution of time between mortgages among refinancers in our data. The distribution features large spikes in refinancing activity around 2, 3, and 5 years after the previous mortgage, consistent with the fact that these are the most common durations on offer. The lightly shaded bars indicate the fraction of households in each month that refinance around the end date of their initial low-interest duration (within a window of 2 months before and 6 months after the end date). The figure demonstrates that the vast majority of households refinance around the time that the initial duration ends.[10]

This institutional setting has the following key advantages for our empirical approach. First, the fact that refinancing occurs around predetermined dates makes the time of refinance potentially orthogonal to individual circumstances. This contrasts with the US setting where the decision to refinance or take out home equity loans is likely to be correlated with unusual consumption and borrowing needs (see Hurst and Stafford 2004). Second, the fact that refinance events are frequent allows us to observe the same homeowner refinancing several times, facilitating the use of panel data methods. Third, the frequency of refinancing also implies that the market for home equity loans is minimal in the UK. As households are only a few years away from refinancing at any given time, home-equity based borrowing is done almost exclusively through equity extraction at the time of refinancing. Finally, it is worth highlighting that mortgage debt comprises nearly 90% of all household debt in the UK. Thus studying borrowing responses in the mortgage market gives a nearly complete view of household borrowing behavior.

When households refinance, the lender appraises the house value and this appraisal determines home equity. The household's decision about equity extraction then determines the new debt level, the loan-to-value (LTV) ratio, and the interest rate. The interest rate charged on UK mortgages follows a step function with discrete jumps (notches) at certain LTV thresholds. The most common interest rate notches occur at LTVs of 60%, 70%, 75%, 80%, and 85%. Figure 6.4 in the appendix shows the average interest rate schedule as a function of LTV across all mortgage products (see Best, Cloyne, Ilzetzki, and Kleven 2015 for details).[11] The overall level of the interest rate schedule depends on a number of mortgage contract characteristics (including the duration of the initial interest rate), but all contracts feature notches at critical LTV thresholds. These interest notches introduce a form of 'soft' credit constraints that de-

[10]How do borrowers choose their mortgage's initial duration? The main determinants in this choice are interest rates and expectations thereof. For example, a two-year initial duration will offer a lower interest rate than a five-year initial duration, but the five-year product hedges against interest rate increases in the remaining three years. The choice between the two will be determined by, among other things, risk preferences. Our empirical approach will be able to deal with unobserved heterogeneity in preferences for low-interest durations.

[11]Best, Cloyne, Ilzetzki, and Kleven (2015) provide a bunching analysis of borrowing responses to these interest rate notches.

pend on collateral values: borrowing costs jump sharply as the LTV ratio exceeds — and the collateral therefore falls below — the critical thresholds.[12] House price growth reduces a homeowners's LTV ratio, allowing her to borrow at a lower interest rate if it pulls her across interest notches. We will utilize this institutional feature to devise a test for the collateral channel.

2.2.2 House Price Measurement

We measure house prices based on lenders' house value appraisals. There are a number of useful reasons for this. First, these appraisals provide us with house price information at the individual level. Second, appraisals take place at every refinance event, providing us with several observations of house prices for each house-homeowner pair. Third, the appraisal provides the exact house price measure used by the lender to determine collateral, the LTV ratio and the interest rate. Hence, for capturing the collateral effect of house prices, there is no measurement error in the price measure we use.

Nevertheless, a potential concern with our house price measure is the presence of appraisal bias. A literature has shown that mortgage appraisals feature systematic upward bias in the US (e.g. Ben-David 2011; Agarwal, Ben-David, and Yao 2015; Agarwal, Ambrose, and Yao 2016), which may reduce the suitability of appraisals for capturing the true wealth effect of house prices in that setting. However, such appraisal bias does not seem to be a problem in the UK, as we demonstrate in two ways. First, while we do not observe actual market prices for refinanced properties, we do observe market prices (along with appraisals) when properties are purchased and the first mortgage is originated. Hence, Figure 5.9 shows a histogram of the difference between the purchase price and the appraisal for transacted properties. The difference is zero for the vast majority of transactions, showing that appraisals line up with the actual price for newly purchased homes.

However, appraisal bias may be more acute for refinances than for first mortgages, as there is no purchase price to anchor the appraisal for refinances. This motivates our second test in which we compare actual purchase prices (for transacted properties) with appraised prices (for refinanced properties) over time. The results are shown in Figure 5.10. Panel A plots the raw time series of actual and appraised prices. Taken at face value, this panel suggests that there *is* bias: appraised prices are slightly higher than purchase prices on average, and the appraised prices are too smooth during the financial crisis. But such a comparison does not account for the fact that the composition of properties in the two series is different, and that the composition of each series changes over time. To be able to accurately compare the two series and their changes over time, Panel B presents regression-adjusted price series in which we control non-

[12] Alongside these notches, there is also a hard collateral constraint as only a handful of mortgage products are currently available at LTVs exceeding 90%.

parametrically for two observables: the age of the homeowner and the postcode of the property. Specifically, we run the following regression separately for the price and the appraisal series:

$$P_i = \sum_t \beta_t \cdot \mathbf{I}[quarter_i \in t] + \sum_k \gamma_k \cdot \mathbf{I}[age_i \in k] + \sum_p \lambda_p \cdot \mathbf{I}[postcode_i \in p] + \nu_i, \quad (2.1)$$

where the first term includes a full set of quarter dummies, the second term includes dummies for twenty quantiles of the age distribution, and the third term includes dummies for twenty quantiles of the postcode-level distribution of house prices. Specifically, the last term is based on the average house price of each 6-digit postcode, and it includes dummies for the postcode's quantile position in the distribution of postcode-level prices. This term controls for the fact that the quality of neighborhoods that feature high or low activity differs across the two series and changes over time.

The plotted values in Panel B are the coefficients on the quarter dummies from equation (2.1), adding a constant equal to the effect of the average age and the average postcode (in each series separately). We see that, with non-parametric controls only for age and neighborhood, the two series track each other closely throughout the period and the recession is now clearly visible in the appraisal series. In other words, the differences in Panel A were due to differences in sample composition rather than real appraisal bias. We therefore conclude that appraisals are a good reflection of true property prices in the UK market.[13]

2.2.3 Data

The data come from a novel and comprehensive regulatory dataset containing the universe of mortgage product sales. These data are collected by the UK's Financial Conduct Authority (FCA) and available to restricted members of staff at the FCA and the Bank of England. This Product Sales Database (PSD) has information on all completed household mortgage product originations from April 2005, but does not include commercial or buy-to-let mortgages.[14]

Regulated lenders are required to submit quarterly information on all mortgage originations. The data include a range of information about the mortgage such as the loan size, the date the mortgage became active, the house price appraisal, the interest rate charged during the introductory period, whether the interest rate is fixed or variable, the end date of the initial duration (the time at which the higher reset rate starts

[13]Further evidence against consequential appraisal bias is that the equity extraction elasticity remains stable when controlling for fixed effects for month, household, and county x year, as well as a number of time-varying household characteristics (results in Section 2.4.2). Typical sources of appraisal bias are that certain households or banks tend to demand biased appraisals or that region- or household-specific income shocks lead to biased appraisals. The control variables account for all these possibilities.

[14]See https://www.fca.org.uk/firms/product-sales-data for officially published high level data.

applying), whether mortgage payments include amortization, and the mortgage term over which the full loan will be repaid. The data also include a number of borrower characteristics such as age, gross income, and whether the income is solely or jointly earned.[15]

Another useful feature of the PSD is that it contains information on whether the household is a refinancer. Using information about the characteristics of the property and the borrower, refinancing households can be matched over time to construct a panel. As noted above, since refinancing is a regular occurrence in the UK mortgage market, this provides us with multiple observations for the same household over the 11 years of the sample. Using our new panel, we can compute a range of useful household-level statistics including house price growth, mortgage debt growth, amortization, and equity extraction/injection.

Overall, the PSD contains around 14 million mortgage observations. Around half of these observations are mortgages for new house purchases, while the other half are refinancing events. Since we need to calculate the house price change and equity extracted for our analysis, we can only use refinancing observations where we observe a previous mortgage event (either the house purchase or a previous refinancing event) by the same household for the same property. Our sample is therefore a subset of the refinancers in the PSD.

Table 5.13 summarizes the data. Panel A compares descriptive statistics for home buyers (column 1), all refinancers (column 2), and refinancers in our estimation sample (column 3). There are no significant differences between the three groups in the share of couples, income, income growth, interest rate, and house price. Some differences between buyers and refinancers are to be expected. For example, buyers tend to be younger and have higher LTV ratios.

Panel B of Table 5.13 reports statistics for the 1.38 million observations in our estimation sample, split into three subsamples. As discussed above, practically all mortgages in the UK have an initial duration with a favorable interest rate, after which a higher reset rate kicks in. This gives a strong incentive for refinancing around the onset of the reset rate. The subsample in column 1 of panel B includes the 0.48 million observations where we know refinancing took place "on-time" (defined as between 2 months before and 6 months after the reset rate onset), while column 2 includes the 0.28 million observations where we know refinancing took place "off-time". For a large part of the sample, 0.61 million observations, we do not observe when the reset rate kicks in, because lenders were not always required to report this statistic to the Financial Conduct Authority. We summarize these observations in column 3. There are no significant differences across the three groups in any of the observables.

[15]Full details of the dataset can be found on the FCA's PSD website.

2.3 House Price Variation

There is large house price variation in the data. Figure 5.11 shows the distribution of house price growth between refinance events for homeowners in our estimation sample. To measure individual house price growth, the sample conditions on observing homeowners at least twice. The first price observation for each homeowner may come either from the first mortgage in the house or a refinance, while subsequent price observations always come from refinances. The distribution shows that house price growth lies between -30% and +60% across refinance events, giving us lots of variation to work with. We note that there is some round-number bunching at zero price growth, suggesting that some lenders set the new house price equal to the old house price whenever the two are very close (see Kleven 2016 for a discussion of round-number bunching).

While there is large house price variation in the data, the challenge is that much of it may be endogenous to demand factors that impact our outcome of interest. Our approach starts by controlling for obvious confounders by absorbing a rich set of fixed effects. Individual fixed effects control for time-invariant individual preferences for borrowing, month fixed effects control for time-varying macro factors that affect borrowing, while county-by-year fixed effects control for local, time-varying shocks to borrowing demand. Specifically, 'counties' are defined as local planning authorities (or councils), of which there are more than 400 in the UK and 32 in London alone.

Figure 5.12 shows the distribution of residual house price growth, after absorbing the fixed effects described above. Allowing for individual fixed effects on house price growth gives an R-squared of one among households with just two mortgage observations (one price growth observation), so the figure considers the sample of homeowners observed at least three times. Panel A shows the raw distribution of house price growth in this subsample as a benchmark (it looks similar to the raw distribution in the previous figure), while Panel B shows the residualized distribution. Importantly, there is large remaining house price variation even after controlling for fixed effects, between -20% and +20% across refinance events.

What drives this residual variation? In general there can be two sources of remaining variation. The first is that different properties experience different price growth *within counties*, so that county-by-year fixed effects do not fully absorb the housing cycle. This arises because of variation across neighborhoods within counties, variation across property types within neighborhoods, or completely idiosyncratic variation driven by features of the specific house. On the latter, note that the value of a specific house may increase due to home improvements undertaken by the owner, which would not be real house price appreciation. However, the data include an indicator for home improvement activity, which allow us to deal with this potential issue. Moreover, as described below, we consider IV-specifications that are unlikely to be affected by home improvements.

The second source of variation is idiosyncratic variation in the timing of refinance events relative to the price cycle. As described above, homeowners have a strong incentive to refinance around the onset of the reset rate, typically after 2, 3 or 5 years, as these are the most common products in the market. Hence, the timing of refinance is determined to a large extent by a duration choice made several years in advance, creating arguably quasi-exogenous variation. Figure 5.13 illustrates conceptually how this works. It compares two homeowners who start out at the same time (time 0), live in houses with the same price cycle (the solid blue line), but have different preferences over low-interest rate durations. One homeowner prefers 2-year fixed interest rate loans, while the other prefers 3-year fixed interest loans. Of course, this difference in duration preferences will be related to, for example, risk preferences that may themselves impact on borrowing behavior, but such time-invariant preference heterogeneity is absorbed by the individual fixed effect. What creates variation here is the *interaction* of idiosyncratic duration preferences with the housing cycle: The 2-year person refinances three times over a 6-year period, facing either positive or negative price shocks at each event, whereas the 3-year person refinances only two times facing a zero price shock each time. Our empirical strategy exploits this kind of within-person variation in price growth.

In Figure 5.14 we illustrate this point using the actual data. The figure plots average house price growth for homeowners who refinance at different times (in January of different years) by bins of the duration of their last mortgage. The two panels show the same graphs, but highlight two different homeowners who experience very different within-person price patterns due to past duration choices. The homeowner in Panel A refinances in January 2010 coming out of a 2-year mortgage chosen in 2008, and refinances again in January 2013 coming out of a 3-year mortgage chosen in 2010. This homeowner experiences a substantial negative shock the first time around, and a substantial positive shock the second time around. The homeowner in Panel B also refinances in January 2010 and January 2013, with the only difference being that in 2010 she was coming out of a 5-year mortgage chosen in 2005. As a result, this homeowner faces similar positive price growth in both refinance events. The empirical approach we propose uses this kind of within-person variation for identification: i.e., we use the change over time for Person A (who goes from negative to positive price growth) relative to the change over time for Person B (who goes from positive to positive price growth). This is a form of *triple-differences* strategy as we are comparing within-person changes in price *growth*.

The exogeneity of this duration-driven variation in house price growth requires that homeowners are not choosing durations in anticipation of future house price growth and future borrowing needs. For example, if homeowners were choosing 2-year mortgages (rather than 3-year mortgages) in late 2005 — anticipating that this would put them at the peak of the boom (rather than at the bottom of the bust) — to be able to extract more equity for consumption goods in late 2007, then our estimates would not be causally identified. A sufficient condition for ruling out such hyper-rational and forward-looking behavior is that homeowners are not able to forecast house prices with much precision. This assumption seems particularly persuasive around the time of the Great Recession, and it is consistent with a growing consensus that homeowners tend to have biased beliefs about future house prices (e.g., Case and Shiller 1989; Shiller 2007; Case, Shiller, Thompson, Laibson, and Willen 2012; Kaplan, Mitman, and Violante 2015). However, we do not necessarily need bias or irrationality for our strategy to work; a sufficient amount of house price uncertainty will do.

Another way of gauging the exogeneity of duration-driven house price growth is to check if duration choices, besides predicting future house price appreciation, predict other things of relevance to borrowing. Hence, Figure 6.5 in the appendix shows how much of the residual price variation (Panel A) and residual income variation (Panel B) can be explained by past duration choices, having absorbed all the other fixed effects. The figure shows that, while duration choices are strong predictors of future price growth, they do not predict future income. This lends further support to our strategy.

We present results from two types of strategies. We first consider OLS fixed effects regressions, which use all of the residual variation for identification. This includes idiosyncratic variation in price growth across properties within counties, and it includes idiosyncratic variation in the timing of refinance events. As discussed earlier, a concern with the first source of variation is that it may be partly driven by home improvements. Hence, we also consider IV-regressions in which we construct instruments based on past duration choices (which determine refinance timing). These results should not be affected by home improvements. Reassuringly, our OLS fixed effects and IV results turn out to be quite similar.

2.4 Do House Prices Affect Borrowing?

2.4.1 Baseline Specification

To establish a baseline, we start from a specification that is similar in spirit to specifications used in existing work. Specifically, we consider the following specification

$$\Delta \log D_{it} = \sum_{j} \beta_j \cdot \mathbf{I}\left[\Delta \log P_{it} \in j\right] + \nu_{it}, \tag{2.2}$$

where D_{it} and P_{it} denote mortgage debt and house prices, respectively, for individual i at time t. While we primarily consider log-specifications, we will also explore level-specifications and show that those yield the same qualitative results.[16] We allow for different bins of house price growth to have different effects on borrowing, as we do not (yet) want to commit to a specific functional form.

Equation (2.2) corresponds to the specification used by Mian and Sufi (2011), except for three differences: (i) we rely on individual rather than regional house price variation, (ii) we allow for a non-parametric specification without *a priori* functional form restrictions, and (iii) we do not instrument house prices using topography-based housing supply elasticities.[17]

Panel A of Figure 5.15 shows the results from this specification. It plots the log-change in mortgage debt against bins of the log-change in house prices. Three insights are worth highlighting. First, overall there is a clear positive relationship between house price growth and debt growth. Debt growth changes from 0% to 15% as house price growth changes from -10% to + 40%. Second, there is a strong asymmetry between negative and positive price shocks: Homeowners increase debt when their house becomes more valuable, but they do not reduce debt when their house becomes less valuable. A possible explanation for this phenomenon is the presence of liquidity constraints that prevent homeowners from injecting equity when negative house price shocks push up their LTV ratios. Third, the average elasticity of borrowing across the full range of house price growth — obtained from a log-linear specification — equals 0.3. However, this elasticity masks the heterogeneity between the negative and positive ranges, with an elasticity of 0 in negative ranges and an elasticity of 0.4 in positive ranges.

Panel B of Figure 5.15 investigates cyclical variation in the elasticity of borrowing. Again, we consider the elasticity obtained from a log-linear specification, splitting the sample into different years. The graph shows that the borrowing elasticity is strongly pro-cyclical, with the largest elasticities in the run-up to the recession and the smallest elasticities in the middle of the recession. This elasticity cycle is consistent with the asymmetry between negative and positive shocks shown in Panel A, but this is far from the whole story: When we condition the sample on positive price growth, or add a fixed effect for negative price growth in the full sample, the cyclicality largely survives. In general, there can be two possible explanations for the elasticity cycle. The first possibility is that the true elasticity is cyclical, because the underlying mechanisms

[16]The coefficient obtained from a log-specification represents a borrowing elasticity, whereas the coefficient obtained from a level-specification represents a marginal propensity to borrow (which can be translated into an average borrowing elasticity in the population in order to compare with the log-specification).

[17]Hilber and Vermeulen (2016) constructs a topography-based housing supply elasticity index for England (a la Saiz 2010), but not for the rest of the UK (Northern Ireland, Scotland and Wales). However, besides the potential issues with the exclusion restriction of such instruments (as discussed in the introduction), Hilber and Vermeulen (2016) show that the instrument does not have a strong first stage in the English setting: Topography does not predict house price variation in this country.

driving the effect change over the business cycle. The second possibility is that the true elasticity is not cyclical, but rather that a cyclical omitted variable (e.g. expectations) is creating a spurious cyclical estimate. As we move to better identified specifications below, we will be able to distinguish between these two hypotheses.

The outcome considered above equals total debt growth between the current and the last refinance event, i.e. $\Delta \log D_{it} = \log D_{it} - \log D_{it-1}$. This outcome captures both the equity extraction decision made by the homeowner at time t and the amortization between times $t-1$ and t. Because the amortization schedule was chosen as part of the last mortgage contract, it cannot respond to house price appreciation between $t-1$ and t. If there is any spurious correlation between amortization schedules and house price variation, this will lead to bias. Fortunately, we have sufficiently detailed information about mortgage contracts to precisely assess amortization for each homeowner. Hence, we now turn to an improved outcome variable that captures only the active equity extraction decision made at time t. This outcome equals $\log D_{it} - \log D_{it}^{P}$, where D_{it}^{P} denotes the pre-determined debt at time t based on past debt choices and amortization.[18]

The results for this outcome are shown in Figure 5.16, which is constructed exactly as is the previous figure. The results are qualitatively similar: there is a clear positive relationship between house price growth and borrowing overall, there is a strong asymmetry between positive and negative house price growth, and the borrowing elasticity is cyclical. The average elasticity of borrowing is a bit smaller here (0.23), but this reflects that the slope is now negative (as opposed to zero before) in the range of falling house prices. Within the range of increasing house prices, the elasticity is about 0.4, corresponding to the finding in the previous figure. In the analysis that follows we consider equity extraction as our outcome.

2.4.2 Fixed Effects Specification

Taking advantage of the fact that we observe multiple refinance events for each individual, we augment the baseline specification with fixed effects. That is, we specify

$$\log D_{ict} - \log D_{ict}^{P} = \sum_{j} \beta_j \cdot \mathbf{I}\left[\Delta \log P_{ict} \in j\right] + \alpha_i + \gamma_t + \delta_{ct} + \nu_{ict}, \tag{2.3}$$

where the index c denotes county (local planning authority as described above), α_i is an individual fixed effect, γ_t is a time fixed effect (at the monthly level), and δ_{ct} is a county-by-time fixed effect (at the yearly level).[19] The county-by-time fixed effect absorbs regional, time-varying factors (such as local shocks to income expectations), thus dealing directly with the main confounder discussed in the previous literature. By

[18]That is, we have $D_{it}^{P} = D_{it-1} +$ amortization between $t-1$ and t.

[19]There is some abuse of notation here as we use t to describe time in both months and years.

allowing for individual fixed effects in a first-differenced equation, this has the form of a triple-differences specification relying on within-individual variation in price growth.

The results are shown in Figure 5.17, distinguishing between a specification with individual and time fixed effects only (Panel A) and a specification that adds county-by-time fixed effects (Panel B). The following insights emerge. First, the two different fixed-effects specifications yield almost identical results. Once we have controlled for individual and time fixed effects, adding county-time fixed effects have no noticeable effect. Second, the relationship between equity extraction and house price growth is now monotonically increasing and almost perfectly linear in logs. There is no longer any asymmetry between negative and positive shocks. Third, the borrowing elasticity is now smaller, about 0.2.

The results in Figure 5.17 could be biased by individual, time-varying effects that are correlated with house price growth and impact on borrowing behavior. To investigate this threat to identification, we can exploit that the data include information on a number of individual, time-varying variables that are relevant for debt demand. Hence we consider a specification with such individual controls:

$$\log D_{ict} - \log D_{ict}^{P} = \sum_{j} \beta_j \cdot \mathbf{I}\left[\Delta \log P_{ict} \in j\right] + \alpha_i + \gamma_t + \delta_{ct} + X_{it}\theta + \nu_{ict}, \tag{2.4}$$

where X_{it} includes the income level, the income growth, the last mortgage interest rate, the age of the borrower, a dummy for couples, and dummies for a range of self-reported reasons for the current and the last refinance (including home improvement as one possible reason).

The results are shown in Figure 5.18. Panel A of the figure shows that the results are completely stable when moving to this richer specification: the average elasticity is the same and in fact the entire functional form is the same. Panel B of the figure returns to the question of cyclicality in the elasticity. The elasticities reported in this figure are based on a log-linear version of equation (2.4), interacting the house price growth variable by year dummies. We see that the richer specification has eliminated some, but not all of the elasticity cycle shown earlier. Hence we conclude that the strong cycle observed for the baseline specification was partly a result of bias and partly a real phenomenon.

As discussed in sections 2.2 and 2.3, our empirical strategy is based on the idea that the timing of refinance is quasi-exogenous in the UK. The argument was that homeowners tend to refinance around the onset of the reset rate, the timing of which is determined by a duration choice made in the last refinance event. We showed in section 2.2.1 that a majority of homeowners do indeed refinance around the onset of the reset rate, but we also saw that some homeowners refinance at other times, typically ‘too late’. There are a variety of reasons why some homeowners might refinance late

— including inattention and financial distress — but whatever the reason, it raises the concern that such homeowners endogenously tailor the timing of refinance to house price movements. If this is so, our estimates based on the full sample of refinancers — including both on-time and off-time refinancers — may be subject to selection bias.

To investigate this selection issue, Table 5.14 presents estimates of borrowing elasticities across samples that vary by refinance timing: the full sample in Panel A (summarizing the results already presented), the sample of on-time refinancers in Panel B, the sample of off-time refinancers in Panel C, and the sample of refinancers with missing duration information in Panel D. As mentioned earlier, even though almost all mortgage contracts in the UK (including variable-rate loans) come with a penalizing reset rate after a certain duration, we do not observe this duration for all homeowners as it was not always mandatory for lenders to provide it.[20] Overall, the table shows that elasticity estimates are very robust: across all four samples and fixed-effects specifications (columns 2-4), the elasticity varies between 0.17 and 0.27. It is interesting, however, that the elasticity is consistently higher in the off-time sample, consistent with a small selection bias. Hence our preferred estimates are those based on the on-time sample, featuring borrowing elasticities that are slightly smaller that those reported above.

Finally, we present two additional robustness checks on the fixed effects specification. The first check investigates the issue of home improvements. While working with individual house price information has several advantages, they do introduce a problem not present in regional-level data: the house price variation may be driven partly by idiosyncratic home improvements, which are endogenous and may not represent true increases in household net worth.[21] As a first check we use self-reported information on the reason for refinancing, including home improvements as one of the reasons, that is available for part of the sample. Hence, for homeowners who reported home improvement in their last refinance, we know that house price growth in the current refinance is likely to be driven partly by home improvements. Hence, Table 6.20 shows elasticity estimates in three subsamples: homeowners whose last refinance was for home improvements (Panel A), homeowners whose last refinance was not for home improvements (Panel B), and homeowners for whom the reason for the last refinance is unknown. The table shows that, for all the fixed effects specifications, the estimated elasticity is quite stable across samples. Specifically, among those who report no home improvement, we find elasticities that are similar to the elasticities for the full sample

[20] To be clear, we always observe the *actual* time between refinance events, it is only the duration of the low-interest rate period defined in the mortgage contract that we do not always observe. In the sample of homeowners with missing duration information, the actual time between refinance events features strong bunching at 2, 3 and 5 years, showing that these households do in fact have a fixed low-interest duration.

[21] In particular, home improvements do not increase household net worth unless they increase the house price by more than the amount invested in the house.

discussed above. This alleviates any major concerns about home improvements, but we acknowledge that our measure of home improvements is imperfect. Hence, the next section goes further by presenting IV-estimates that cannot be plausibly affected by home improvements.

The second check investigates alternative specifications. Starting from equation (2.3), Figure 6.6 shows how the results are affected by moving from a log-specification to a level-specification (Panel A) and by moving from house prices to housing net worth as the explanatory variable (Panel B). In each panel we continue to allow for different bins of the explanatory variable to have different effects on borrowing. While the results are qualitatively unaffected by these changes, the alternative specifications are useful for obtaining different types of parameters. Panel A yields an estimate of the marginal propensity to borrow (equal to 0.11) as opposed to the elasticity parameter discussed so far. Panel B yields an estimate of the elasticity with respect to housing net worth — defined as house price minus baseline mortgage debt — as opposed to the elasticity with respect to house prices.[22] The fact that the elasticity with respect to housing net worth is considerably smaller is a mechanical rather than substantive result: because housing net worth is only a fraction of the house price, any given log-change in house prices translates into a much larger log-change in housing net worth. This makes the elasticity with respect to housing net worth mechanically smaller.

2.4.3 IV Specification

Our fixed effects specification relies on two sources of residual variation: (i) idiosyncratic variation in price growth across houses within counties, (ii) idiosyncratic variation in the timing of refinance events across homeowners. The first source of variation could be endogenous due to for example home improvements (as discussed above) or endogenous selection into neighborhoods. Hence, in this section we consider an IV-strategy that relies solely on variation in the (pre-determined) timing of refinance events.

We do not want to rely on cross-sectional variation in duration choices, because these are insurance choices that reflect risk preferences and therefore may affect borrowing directly. As discussed above (see Figure 5.14), the most compelling source of variation is the *interaction* between the duration choice in the last mortgage (say 2-year vs 3-year fixed interest rate) and the time of the current refinance event (say 2010 vs 2011). Hence we construct instruments based on the interaction between dummies for past duration choices and dummies for the time of refinance, within different regions.

[22] We specify housing net worth as the house price minus *baseline* debt (as opposed to current debt) in order to avoid a clear endogeneity problem. This implies that the variation in housing net worth comes from the variation in house prices, and so the two elasticities are identified from the same source of variation.

The first stage of the IV is specified as follows

$$\Delta \log P_{ict} = \rho \cdot \text{last duration}_{it} \otimes \text{year}_t \otimes \text{region}_i + \alpha_i + \gamma_t + \delta_{ct} + X_{it}\eta + \mu_{ict}, \qquad (2.5)$$

where $\otimes$ denotes the outer product, so that the instrumental variables (last duration$_{it} \otimes$ year$_t \otimes$ region$_i$) include every possible interaction between last duration dummies, year of refinance dummies, and regional dummies. It is for computational reasons that the instruments are based on year dummies (rather than month dummies) and region dummies (rather than the more disaggregated county dummies). We include fixed effects for household, month and county-by-year, and we also allow for individual, time-varying controls X_{it} including duration dummies on their own. This specification implies we are identifying off of the interaction between last duration and time, taking out the average effects of duration and time separately.

The second stage of the IV is similar to the fixed effect specifications considered earlier, i.e.

$$\log D_{ict} - \log D^P_{ict} = \beta \cdot \Delta \widehat{\log P}_{ict} + \alpha_i + \gamma_t + \delta_{ct} + X_{it}\theta + \nu_{ict}, \qquad (2.6)$$

where $\Delta \widehat{\log P}_{ict}$ is the predicted house price growth from the first-stage specification (2.5).

The results are shown in Table 5.15. The table shows the estimated elasticities of equity extraction with respect to house price across five IV specifications. The richest specification in column (5) corresponds to the specification shown in equations (2.5)-(2.6). There is a non-trivial difference in the estimates between the basic specifications without household and month fixed effects (columns (1)-(2)) and the richer specification with those fixed effects (columns (3)-(5)). But across the richer specifications, the IV elasticity estimates are very stable (around 0.28-0.29) and slightly higher than the OLS estimates shown earlier. The fact that the IV estimates are higher is consistent with a (small) bias from home improvements in the OLS estimates: house price appreciation due to home improvements does not represent real appreciation and would therefore tend to attenuate the OLS estimates. These differences notwithstanding, the IV table confirms the overall qualitative results presented so far: There is a clear positive effect of house prices on borrowing, but the effects are smaller than recent estimates have suggested.

2.5 Why Do House Prices Affect Borrowing?

Having established a causal relationship between house prices and household borrowing, we now investigate the reasons for this relationship. Berger, Guerrieri, Lorenzoni, and Vavra (2015) provide a theoretical foundation for the various mechanisms that may be at play. Here we focus on the two main mechanisms discussed in the literature.

First, higher house prices increase homeowners' nominal housing wealth, so that borrowing responses may reflect the marginal propensity to consume out of wealth (Campbell and Cocco 2007; Case, Quigley, and Shiller 2013). However, it is not obvious that such changes in nominal wealth translate into real wealth, as highlighted by Sinai and Souleles (2005). They argue that homeownership provides a hedge against future housing expenditures for households with long expected tenures in their existing homes. This implies that house prices have negligible effects on lifetime net worth and should not affect borrowing. If wealth effects are operational they must therefore rely on expected changes in real housing consumption over the lifecycle. For example, old homeowners may expect to downsize or exit the housing market in the near future, in which case house price growth tends to increase net wealth. Young homeowners, on the other hand, have constant or increasing housing needs over the foreseeable future, so that the nominal wealth effect of house price growth will be offset by increases in future housing expenditures. This suggests larger wealth effects for old homeowners than for young homeowners. Hence, a number of existing papers assess the importance of wealth effects by studying heterogeneity with respect to age, but with conflicting results (Attanasio and Weber 1994; Campbell and Cocco 2007; Attanasio, Blow, Hamilton, and Leicester 2009; Mian and Sufi 2011).

Second, housing wealth is the largest form of household collateral. An increase in nominal housing wealth may therefore relax borrowing constraints, which tend to be proportional to collateral values. The collateral channel has been studied theoretically in the macro housing literature (e.g., Aoki, Proudman, and Vlieghe 2004; Iacoviello 2005), and it has been argued to be empirically important for household borrowing in a number of studies (e.g., Lustig and Nieuwerburgh 2005; Mian and Sufi 2011; DeFusco 2016).[23] The collateral channel implies heterogeneity across leverage ratios: Households with higher leverage are more collateral constrained, and house price appreciation is therefore more likely to relax collateral constraints for such households.

In the next section, we take a first step towards disentangling wealth and collateral effects based on a heterogeneity analysis that uses the power and granularity of our administrative data. We analyze heterogeneity in the borrowing elasticity along the main dimensions predicted to determine household borrowing responses, including age and leverage. This analysis suggests that the collateral channel plays a crucial role. We then explore the collateral channel more closely in the following section, proposing a new method to assess its empirical importance.

[23]The collateral channel has also been shown to be important for business investments and employment (Chaney, Sraer, and Thesmar 2012; Adelino, Schoar, and Severino 2015).

2.5.1 Heterogeneity Analysis

We investigate how borrowing elasticities vary along four dimensions of heterogeneity: loan-to-value (LTV), age, income level, and income growth. We consider two types of specifications. Univariate specifications investigate heterogeneity in each dimension separately, while multivariate specifications allow for heterogeneity in all four dimensions simultaneously. Many dimensions of heterogeneity are highly correlated, making it difficult to interpret results from univariate heterogeneity analyses. Our multivariate specifications allow us to disentangle which dimensions truly drive heterogeneity in responsiveness, and which dimensions only appear to do so by being correlated with other relevant dimensions. We estimate specifications of the type

$$\log D_{it} - \log D_{it}^{P} = \sum_{k}\sum_{j} \beta_j^k \cdot \mathbf{I}\left[X_{it}^k \in j\right] \cdot \Delta \log P_{it} + \sum_{k}\sum_{j} \lambda_j^k \cdot \mathbf{I}\left[X_{it}^k \in j\right] + \nu_{it}, \quad (2.7)$$

where $\mathbf{I}\left[X_{it}^k \in j\right]$ is a dummy equal to one when variable k (LTV, age, income, or income growth) falls in bin j. By allowing for a large set of bin dummies in each dimension (7 LTV bins, 9 age bins, 7 income bins, and 7 income growth bins), and by allowing for these dummies to affect both the slope and the intercept, our analysis is very non-parametric. Hence, the heterogeneity patterns we uncover will not be driven by overly restrictive functional form assumptions. We do assume that the effect of prices on borrowing *within* dimension k and bin j is log-linear, but this assumption is a good approximation as we show below. To increase precision, specification (2.7) does not include the household and time fixed effects considered in the previous section. It is possible to consider such an extension and the heterogeneity results turn out to be very similar, but standard errors increase substantially in fixed effects specifications with heterogeneity.

In Figure 5.19 we investigate heterogeneity with respect to age and LTV, which are the two main proxies for wealth and collateral effects as discussed above. The top panels show heterogeneity by pre-determined LTV, defined as the LTV ratio absent any equity extraction/injection and absent any house price growth between the current and last refinance event. This LTV is determined by the last choice of mortgage debt and amortization along with the last house price. The graphs show a strong monotonic relationship between the borrowing elasticity and LTV. This holds both when studying this dimension of heterogeneity on its own (Panel A) and when controlling for the other dimensions of heterogeneity (Panel B). In fact, going from the univariate to the multivariate specification hardly affects the relationship, although it increases standard errors somewhat. Hence, homeowners with low levels of collateral borrow much more against house price increases than do those with high levels of collateral. The strong degree of LTV heterogeneity is not driven by the log-linearity assumption made in equation (2.7), which we show in a fully non-parametric specification in appendix Figure 6.7.[24]

The bottom panels of Figure 5.19 investigate the effect of age. These panels show heterogeneity in the borrowing elasticity across 5-year bins between the ages of 20 and 60. Panel C presents results without controls for the other dimensions of heterogeneity. The figure shows the opposite pattern of wealth effects than what is suggested by standard lifecycle theory: young households are more responsive to house prices than old households. A similar pattern of heterogeneity was found by Attanasio, Blow, Hamilton, and Leicester (2009) using UK survey data and structural methods. They suggest that this puzzling pattern might arise because the young tend to be more leveraged than the old, so that collateral effects confound wealth effects (see Berger, Guerrieri, Lorenzoni, and Vavra 2015 for a similar argument). Panel D investigates and confirms this hypothesis. It shows that, once we control for LTV (as well as income and income growth), the age profile of borrowing elasticities is completely flat.

For completeness, Figure 5.20 displays heterogeneity across income levels (top panels) and income growth (bottom panels). Income is measured at the time of the last refinancing event, while income growth is measured as the log-change since the last refinancing event. We use dummies representing seven quantiles of the distribution of each of these variables. Once again, we consider the univariate specification on the left and the multivariate specification on the right. These graphs do not show any noticeable patterns of heterogeneity: they are quite flat across both income levels and income growth in both the univariate and multivariate cases.

How should we interpret these heterogeneity patterns? The fact that leverage is such a strong predictor of borrowing elasticities, even after controlling non-parametrically for other correlated factors, while at the same time the other factors have no predictive power clearly points to the collateral channel as being central. A few qualifications to this interpretation are worth mentioning. First, wealth effects may not be the only force driving heterogeneity across age (even conditional on the other controls), and so the flat age profile does not rule out wealth effects. Second, wealth effects may themselves lead to heterogeneity across LTV ratios, even absent a collateral channel. This issue is particularly pronounced in the log-log specification (2.7). A one percent increase in the house price represents a five percent increase in

[24] Figure 6.7 presents non-parametric estimates allowing for a large set of bin dummies for house price growth (as in the previous section) within three separate LTV categories. The three samples correspond to low-leverage homeowners (LTV below 60%), intermediate-leverage homeowners (LTV between 60-80%), and high-leverage homeowners (LTV above 80%). Two insights are worth highlighting. First, the *level* of equity extraction decreases with leverage as one might expect: highly leveraged households have a larger stock of existing debt, are more constrained in their borrowing capacity, and should be on an amortization path over their lifecycle. Second, the *slope* of equity extraction increases with leverage, consistent with our previous findings on elasticity heterogeneity. That is, homeowners with high leverage (low collateral) extract less equity, but are more inclined to increase equity extraction when house prices go up.

housing net worth for a homeowner at 80% LTV, but only a two percent increase for a homeowner at 50% LTV. Mechanically, there are heterogeneous wealth changes depending on LTV. As a robustness check, we have therefore tried a level specification as well, finding very similar qualitative results. This strengthens the conclusion that the collateral channel is crucial. Third and finally, leverage may be correlated with unobserved individual characteristics that affect borrowing behavior. A candidate would be self-control problems. As Mian and Sufi (2011) note, it is likely that households with greater self-control problems will be observed as credit constrained. However, when augmenting equation (2.7) with individual fixed effects (which should pick up self-control problems), we find that our heterogeneity results are qualitatively unchanged (albeit with larger standard errors). Controlling for fixed effects ensures that the variation in leverage isn't confounded with individual characteristics, such as self-control.

To conclude, the heterogeneity results are strongly suggestive of the collateral channel, but perhaps not fully conclusive to a skeptic. In the next section, we propose a more sophisticated test — one that exploits discrete changes in the tightness of collateral constraints around interest notches — providing our final piece of evidence in favor of the collateral channel.

2.5.2 Collateral Channel: A Test Using Interest Notches

The UK setting offers a novel way of investigating the collateral channel arising from the presence of observable credit constraints that depend on collateral. As described in section 2.2.1, the mortgage interest rate schedule features discrete jumps (notches) at critical LTV thresholds. There are notches at LTV ratios of 50%, 60%, 70%, 75%, 80%, 85%, and 90%.[25] These notches introduce 'soft' credit constraints: as the LTV ratio surpasses (and housing collateral therefore falls below) one of the critical thresholds, the cost of borrowing increases sharply.[26] The direct incentive created by these interest notches is for homeowners to choose LTV ratios below one of the thresholds, thus creating bunching in the LTV distribution. Such bunching represents borrowing responses to the interest rate — rather than responses to the house price — and was studied in detail by Best, Cloyne, Ilzetzki, and Kleven (2015). Here we consider whether house price movements interact with bunching responses to interest notches in a way that is consistent with the collateral channel. The basic idea is that house price growth, by increasing collateral, moves homeowners below interest notches and induces borrowing responses due to reduced costs of borrowing.

[25] Figure 6.4 illustrates most of these notches.

[26] The difference between such soft borrowing constraints and the hard borrowing constraints often assumed in theoretical models can be interpreted in terms of the size of the notch: a hard borrowing constraint is one where the borrowing cost jumps prohibitively at a threshold. In fact, the 90% LTV notch serves as a hard borrowing constraint for most homeowners in our data, because very few lenders have offered mortgage products above this level since the global financial crisis.

We start by presenting a simple test of heterogeneity, similar in spirit to the preceding analyses, before turning to a more sophisticated and conclusive analysis. Specifically, Figure 5.21 investigates if the equity extraction elasticity depends on whether the underlying price variation moves homeowners across notches and in which direction. We define the collateral constraint as being relaxed (reinforced) when house price variation moves the homeowner at least one notch down (up) and thus reduces (increases) the mortgage interest rate. Otherwise, the collateral constraint is defined as "unchanged."[27] Panel A of the figure considers a baseline specification without any other controls. This is a specification like (2.7) in which house price growth is interacted with dummies for the three notch scenarios (relaxed/reinforced/unchanged), but without simultaneously controlling for other dimensions of heterogeneity. This analysis shows that the elasticity is the highest (close to 0.5) when the collateral constraint is relaxed, and that the elasticity is the lowest (close to zero) when the collateral constraint is reinforced.[28] The fact that the elasticity is essentially zero when the collateral constraint is reinforced may be due to collateral constraints interacting with liquidity constraints, making it hard for homeowners to inject cash when house price growth increases their cost of borrowing. As a robustness check, Panel B introduces household and month fixed effects in the specification. This graph confirms the qualitative relationship between the borrowing elasticity and changes in collateral constraints, although the effect is smaller than in the baseline specification without fixed effects. The asymmetric response to relaxing and tightening borrowing constraints is suggestive of the importance of the collateral channel.

To provide more conclusive evidence of the collateral channel, Figure 5.22 analyzes the dynamic interaction between house price growth and bunching responses to interest rate notches. This figure focuses on the sample of households who are pulled down to a lower notch by house price growth, i.e. households whose collateral constraint is relaxed. For this analysis, it is useful to formally define three different LTV concepts. First, we define the pre-determined LTV = D_{it}^P/P_{it-1} as the homeowner's LTV at time t given past mortgage choices (the debt level and amortization schedule chosen at time $t-1$) and the old house price. Second, we define the passive LTV = D_{it}^P/P_{it} as the homeowner's LTV at time t given past mortgage choices and the new house price. This is the LTV that would apply if the homeowner simply rolled over her debt at time t, i.e. if she were "passive." Third, there is the actual chosen LTV = D_{it}/P_{it} that includes any equity extraction or injection at time t. By this terminology, the sample in the figure includes borrowers for whom the passive LTV is at least one notch down from their pre-determined LTV.

[27]However, this terminology should not be taken literally: house price appreciation may relax credit constraints even if it does not move homeowners to a lower interest rate notch.

[28]While the figure pools all years 2005-15, we have checked that the patterns are roughly the same inside and outside the recession years.

The figure shows two panels in which we compare the density distributions of the three LTV measures defined above. The x-axis in each panel represents the distance between a given LTV measure (pre-LTV, passive LTV, or chosen LTV) and the next-notch-up from the passive LTV. Panel A illustrates the implications of house price growth by comparing the distributions of pre-LTV and passive LTV. Two implications are worth highlighting. First, house price growth moves all borrowers from the positive range (in terms of their pre-LTV) to the negative range (in terms of their passive LTV). This follows from the fact that we are restricting the sample to households who are pulled down by at least one notch. Second, house price growth eliminates all bunching at interest notches: there is bunching at every notch in the pre-LTV distribution, but no bunching in the passive LTV distribution.[29]

How do borrowers respond to the relaxed collateral constraints? Panel B illustrates the implications of equity extraction behavior by comparing the distributions of the passive LTV and the final chosen LTV. Strikingly, equity extraction behavior largely recreates the qualitative pattern that existed before house price growth. We see a dramatic right-shift of the LTV distribution, moving borrowers back to around zero or into the positive range, and recreating bunching at notches. In other words, when house price growth pulls households below one or more notches (Panel A), most of them extract equity back to the next notch above (at zero) or a higher notch (in the positive range). Hence, this figure shows how house price growth interacts with bunching responses to interest notches in a way that is consistent with a collateral mechanism.[30,31]

To conclude, the multivariate heterogeneity analysis presented in the previous section combined with the notches analysis presented here provides quite compelling evidence of the importance of the collateral channel. Most previous work on the effect

[29]The fact that the passive LTV distribution primarily falls in the bins $(-5,0)$ and $(-10,-5)$, with a discrete drop between the two, is not a bunching response. It follows mechanically from the x-axis normalization and the fact that most homeowners are no longer 5 or 10 percentage points away from a notch. Furthermore, notice that bunching in the pre-LTV distribution is attenuated compared to the actual amount of bunching in the last refinance event due to amortization between the last and current refinance events.

[30]To be clear, what is new in Figure 5.22 compared to the more standard bunching analysis in Best, Cloyne, Ilzetzki, and Kleven (2015) is the illustration of a dynamic interaction between house price growth and bunching responses.

[31]It is also conceivable that some refinancers are targeting their previous monthly mortgage payment rather than borrowing up to a soft collateral constraint. They might do so because of liquidity constraints or behavioral factors (see Di Maggio, Kermani, and Ramcharan 2014 for an analysis of the mortgage payment channel). To explore such effects, Figure 6.8 shows the distribution of changes in monthly mortgage payments between the last and the current mortgage among homeowners who are pulled down to a lower notch by house price growth (i.e., the same sample as in Figure 5.22). In this sample, monthly payments are always reduced by house price growth as it pulls them below interest notches. But the total net change in the payment depends on other factors such as changes over time in interest rate levels and the amount of equity they choose to extract. If homeowners extract equity to target an unchanged monthly mortgage payment, then we would see excess bunching at zero in Figure 6.8. There is arguably a small spike at zero, but overall the distribution is quite smooth. This shows that, in this setting, homeowners do not primarily target an unchanged mortgage payment when choosing equity extraction (while they do target collateral notches as shown above).

of house prices on household borrowing was only able to estimate the total effect, because the empirical analyses were based on house price variation (regional or aggregate variation) that affect both wealth and collateral.

2.6 Conclusion

The global financial crisis of 2007-8 has reignited a debate on the role of house prices in driving household debt. A first generation of papers following the crisis studied this question using regional data in the US and found strong borrowing responses. This paper takes a different methodological tack to this question. Rich administrative mortgage data and novel features of the UK mortgage market allow us to construct a large panel of refinancers and study the relationship between borrowing and house prices at the household level. Exploring a far more granular source of house-price variation that relies on idiosyncratic rather than regional variation in house prices, we find household borrowing responses that are considerably smaller than those found for the US using regional data. Specifically, we find that a 10% percent increase in individual house price increases borrowing by 2%. Importantly, when we do not make full use of the panel structure and consider the pre-crisis period, we obtain similar elasticities to those found for the US.

Our rich dataset also allows us to explore why borrowing responds to house prices. The striking finding from this analysis is that there is essentially no heterogeneity in any dimension except one — loan-to-value — but that this dimension is very strong. In particular, the elasticity is strongly increasing in LTV ratios, even after controlling non-parametrically for factors such as age, income and income growth. This heterogeneity analysis together with a test using 'soft' credit constraints (interest rate notches based on collateral) strongly suggests that the housing collateral channel is the main driver of the elasticities we find.

The magnitude of these responses, and the importance of collateral constraints, has important implications for understanding household behavior in both micro- and macro-economics. A growing literature on macro and housing relies on collateral constraints to obtain realistic macro responses to boom-bust cycles in the housing market. Our findings affirm such theoretical approaches and provide microeconometric estimates that could help discipline future research in this area.

Chapter 3

Are Bigger Banks Better? Firm-Level Evidence from Germany

3.1 Introduction

Does the real economy benefit from having big banks? Will size-dependent banking regulation harm economic growth? These questions are at the forefront of the debate about financial regulation following the financial crisis 2008/09. The market share of the 10 biggest banks in the United States has risen from around 25 percent in 1990 to over 60 percent today (McCord and Prescott 2014). Since the failure of a big bank can destabilize the entire financial system, regulation to stop banks from getting bigger is being debated and implemented (Stern and Feldman 2004). Prominent policy proposals include direct caps on bank size and higher capital requirements for big institutions. Policymakers disagree about whether such size-dependent regulation, by limiting increases in bank size, could reduce the potential for efficiency gains in the banking system, restrict credit supply, and harm real economic growth (Haldane 2010; Stein 2013; Johnson 2016; Minneapolis Fed 2016).

Since exogenous variation in bank size is difficult to find, the academic literature has struggled to analyze the causal effects of increases in bank size (Bernanke 2016). The key contribution of this paper is to estimate the causal impact of bank size on the growth of firms in the real economy. I study a natural experiment from postwar Germany. Two reforms by the Allied occupiers permitted a number of institutions to consolidate from state-level banks into national banks. The reforms were not caused by the performance of the banks or the firms they were lending to. Hence, the reforms led to exogenous increases in the size of the relationship banks of a number of firms. A newly digitized dataset on German firms and their relationship banks enables me to compare the growth of firms with a relationship bank treated by the reforms to firms borrowing from other banks. The main results show that firms did not grow faster when their banks became larger. Additional analyses reveal the size increase did not improve banks' cost efficiency, but it negatively affected their opaque (small, young, low-collateral) customers, increased bank risk-taking, and raised the media presence of the consolidating banks.

Economic theory suggests that big banks may be more efficient, because they are more diversified (Diamond 1984; Boyd and Prescott 1986; Williamson 1986), can use internal capital markets (Stein 1997; Scharfstein and Stein 2000), and rely on a large capital base to fund loans and spread fixed costs. On the other hand, large organizations may be complex to manage (Williamson 1967; Krasa and Villamil 1992a,b; Cerasi and Daltung 2000) and worse at processing soft information, which may hurt small and opaque borrowers (Stein 2002; Berger and Udell 2002; Brickley, Linck, and Smith 2003). They may also take more risk, due to implicit "too-big-to-fail" subsidies by governments (Freixas 1999; Dávila and Walther 2017) and more severe agency problems (Rajan 2005). The net impact of increases in bank size on the real economy is an empirical question.[1]

The empirical challenge in estimating the causal effects of bank size is that banks do not become big randomly. One cause of differences in bank size is underlying heterogeneity in bank efficiency, for example due to the quality of bank managers. More efficient banks will capture a larger part of the market and hence become bigger than other banks. A second reason is that firms experience random growth shocks. These firms will demand more loans from their banks and leave more deposits, increasing the size of their banks. Third, banks may strategically consolidate with other banks, for example because they expect increases in the loan demand of the other banks. Such expectations are usually unobservable in the data, making it difficult to isolate the effects of size from the strategic factors that drove the consolidation. These reasons imply that, even in the absence of a causal effect of bank size, one would observe a positive correlation between bank size and bank efficiency, and between the growth of banks and the firms they lend to.

Two features of the postwar German banking system combined provide a natural experiment that overcomes the empirical challenge. The first feature is the reliance of German firms on relationship banking. Due to asymmetric information, firm-bank relationships were sticky, so that shocks to specific banks affected the relationship customers of the shocked banks more strongly. The second feature is the banking policy of the Allied occupiers in postwar Germany. The Allies wanted to punish three national banks (Commerzbank, Deutsche Bank, and Dresdner Bank) for their cooperation with the Nazis and to break their political power. In 1947 and 1948, the Allies broke up the treated banks into 30 independent state-level organizations, prohibiting the new banks from branching outside state borders. A first reform in 1952 permitted the state-level banks to consolidate with other state-level banks within three banking zones. Instead

[1]Section 3.3 reviews the theoretical advantages and disadvantages of big banks in more detail.

of 30 state-level banks, there were now 9 treated institutions, one for each former national bank in each banking zone. A second reform in 1957, after Germany became a sovereign nation, lifted the restrictions entirely and led to the reconsolidation of the treated banks into three national banks.[2]

Improvements in the attitude of the Allies towards Germany, mainly due to the emergence of the Cold War, were responsible for the implementation and timing of these reforms. Hence, they were unrelated to the counterfactual growth of the banks and their customers. Because of the reforms, firms with a treated relationship bank experienced exogenous increases in the size of their banks in 1952 and 1957.[3] Importantly, the reforms did not directly affect the range of products offered by the banks, the branch managers, staff, the number of bank branches, or other non-size determinants of bank efficiency. The reforms also did not change credit market competition, since the number of banks operating in each local banking market remained the same. This allows identification of the causal effects of bank size, keeping constant competition and other spurious confounders correlated with bank size.

Policymakers today often consider a bank systemically important if its assets exceed 1 percent of GDP. During the breakup, all of the state-level treated banks were below this threshold, relative to German GDP at the time. After they had reconsolidated in 1957, the assets of each treated bank exceeded 1 percent of GDP. Hence, the repeal of the Allied legislation transformed the treated banks from 30 relatively small, regional lenders into 3 banks of systemic importance. This makes this historic episode a relevant experiment for today's policy considerations. German banks at the time focused on the traditional activities of lending, deposit-taking, payment services, and security underwriting. These activities remain the focus of the vast majority of today's banks and still represent a key link between banks and the real economy.

The main analysis examines whether the increases in bank size, induced by the Allied banking reforms, affected the growth of firms. The firm-level identification strategy compares the growth of firms with a treated relationship bank to firms that borrowed from other, untreated banks. The implementation of the identification strategy requires information on the relationship banks and the growth of firms in postwar Germany. Historic volumes by the commercial information provider Hoppenstedt provide such information. Due to the poor print quality of the paper volumes, the data needed to be hand-digitized. The resulting new dataset includes the names of the relationship banks of around 5,900 firms, the growth of balance sheet variables for around 400 firms, and employment growth for around 2,300 firms.

[2]The US postwar occupiers of Japan also restructured the corporate financing system and broke up some of the largest companies. Unlike in Germany, however, the Japanese banks were not split up (Hoshi and Kashyap 2004).

[3]I focus on the 1952 and 1957 reforms and do not analyze the impact of the 1947/48 breakup, because no data exist for the immediate postwar period.

The main results provide little support for the argument that firms benefit from having big banks. The growth rates of bank debt, employment, and revenue per worker were not higher for firms with a treated relationship bank. Firms more likely to benefit from improvements in the efficiency of their banks, such as firms with a high bank debt-to-assets ratio or exporters, did not grow faster either. The treated banks did not form more new banking relationships than other banks and their new relationship customers did not grow faster than comparable firms. I separately examine a subsample of firms that are small, young, or in industries with a low share of easily collateralizable assets. These firms are "opaque", because when they apply for loans they rely on their banks to process hard-to-verify, soft information, for example to issue character loans. Opaque firms substituted bank debt with other sources of financing after their relationship banks grew in size, indicating a relative increase in their cost of bank debt. Firms with little access to alternative funding suffered a decrease in employment growth. The results on opaque firms are consistent with theories that argue big banks are worse at processing soft information.

The second set of results uses data on banks. Before the 1952 reform, total lending by all the treated, state-level banks grew in parallel to other untreated banks. After the reforms, however, lending growth was slightly lower. These findings are consistent with the firm-level results, indicating the reforms did not raise loan supply. Common measures of banks' cost efficiency include the ratios of non-interest expenses over total assets and employee compensation over total assets. If big banks are more efficient because they can spread fixed costs over a larger base, increases in size should lower these ratios. Compared to a set of similar, untreated banks, however, the ratios of the treated banks (aggregated to the level of their former national banking group) improved slightly less after the reforms. These findings are inconsistent with theories that emphasize the cost efficiency of big banks.

An additional bank-level analysis examines the number of times the treated banks and their executives were mentioned in the media. Their media mentions strongly increased after the reforms. Reporting about the reforms cannot explain the effect. The findings imply that the total number of media mentions of many, small banks is lower than the media mentions of one big bank, even when the aggregated size of the small banks is identical to the size of the big bank. An empirical literature shows that media presence affects consumer choices, political opinions, and voting (Enikolopov and Petrova 2015; Bursztyn and Cantoni 2016). Media presence may also be correlated with influence on politicians and regulators (Zingales 2017). Hence, the finding of a causal effect of bank size on media presence could account for the desire of managers to build big corporate empires, even when big firms are not more economically efficient (Jensen 1986; Stein 2003).

The third set of results examines the new banking relationships formed by firms.

Opaque firms were less likely to establish new relationships with the treated banks after the reforms, consistent with the reduced ability of big banks to process soft information. To test risk-taking, I use three measures of firm risk: the ratio of stock capital to assets, the volatility of employment growth before the reforms, and the volatility of revenue growth. Along all three dimensions, I find evidence that risky firms were more likely to establish new relationships with the treated banks after the reforms, relative to the untreated banks. Overall, the fraction of opaque firms among the relationship customers of the treated banks fell and the fraction of risky firms increased. The findings on risky firms are consistent with theories linking big banks to increased risk-taking, due to either moral hazard or bank-internal agency problems.

The final step of the empirical analysis examines the effects at a higher level of economic aggregation, on municipalities. The municipality-level results capture not only the effect of the reforms on the growth of firms. Other potential channels include local general equilibrium effects or the effects on households. The results show that municipalities with a treated bank branch experienced lower employment growth after the reforms. Similarly, municipalities with a larger share of firms with treated relationship banks grew more slowly. The negative effect on municipalities is consistent with the firm-level and bank-level analyses, since there is no evidence that any firm gained from the increases in bank size, while opaque firms grew more slowly, and overall lending by the treated banks declined. The municipality-level results are based on a small sample of around 80 municipalities, so caution is warranted in interpreting these results. Nonetheless, the results support the conclusion that there is no evidence of a beneficial effect of the reforms on employment growth.

Size-dependent banking regulation limits the growth of banks by imposing size caps or higher capital requirements on big banks. The opponents of such regulation often appeal to the real economic benefits of increases in bank size, for example by arguing that bigger banks offer higher credit supply to firms. The results of this paper suggest that the real economy did not benefit when bank assets grew beyond 1 percent of GDP. Hence, there is no evidence that the introduction of size-dependent banking regulation for banks of this size would forego significant economic benefits. There is empirical support for the theories that motivate size-dependent regulation, such as the reduced ability to process soft information and the higher risk-taking of big banks. Overall, the findings of this paper throw into question the empirical relevance of the standard arguments against size-dependent regulation.

This paper proceeds in the following section by describing institutional details about relationship banking and the postwar banking reforms. Section 3.3 reviews the theoretical channels of bank size, presents a simple model of how bank size can affect firm growth, and explains the identification strategy. Section 3.4 describes the data. The main results on the growth of firms are in Section 3.5. Section 3.6 presents the results based on bank data, Section 3.7 analyses new banking relationships, and Section 3.8 studies the effect on municipal employment growth. Section 3.9 concludes.

Related Literature A number of recent cross-sectional papers argue that big banks face increasing returns or are more efficient (Feng and Serletis 2010; Wheelock and Wilson 2012; Hughes and Mester 2013; Davies and Tracey 2014; Kovner, Vickery, and Zhou 2014; Biswas, Gómez, and Zhai 2017). In general, however, the cross-sectional evidence is mixed (Berger and Mester 1997; Berger, Demsetz, and Strahan 1999). The possibility of reverse causality, that is banks becoming bigger because they first experienced improvements in their efficiency, makes a causal interpretation of the cross-sectional data difficult. The evidence based on banking consolidations (mergers and acquisitions) is similarly ambiguous (Rhoades 1998; Berger, Demsetz, and Strahan 1999; Calomiris 1999; Focarelli and Panetta 2003). A challenge for this literature is that consolidations are not random. For instance, Focarelli, Panetta, and Salleo (2002) find that consolidating banks and the quality of their loan portfolios differ systematically from other banks. Calomiris and Karceski (2000) argue that this makes it difficult to find appropriate control groups and causally interpret bank performance after consolidations.

Another related literature has established that the relaxation of branching regulations in the US influenced real outcomes on many dimensions.[4] There are many potential channels, including increased competition in credit and deposit markets, the reallocation of lending across banks and states, changes in the incentives of bank managers, and increases in average bank size (Jayaratne and Strahan 1998; Stiroh and Strahan 2003; Berger, Demirguc-Kunt, Levine, and Haubrich 2004; Evanoff and Ors 2008). Hence, the deregulation literature cannot inform a clean estimate of the causal effects of bank size.

This paper contributes to the literature by identifying a shock to the size of banks that is exogenous to both the banks and their customers. This allows credible identification of the causal effects of bank size. I study how bank size affects the growth of firms, bank efficiency, and municipal employment growth, outcomes relevant to today's policy discussions about the regulation of big banks. The results about bank risk-taking and media mentions provide new causal evidence about the behavior of big organizations.

The findings on opaque firms contribute to an existing literature on how big banks interact with small firms. Berger, Kashyap, and Scalise (1995) show that, in the cross-

[4]It raised the performance incentives and pay of bank managers (Hubbard and Palia 1995), state income and output (Jayaratne and Strahan 1996), entrepreneurship (Black and Strahan 2002; Cetorelli and Strahan 2006; Kerr and Nanda 2009), and house price co-movements across states (Landier, Sraer, and Thesmar 2017). It lowered growth volatility (Morgan, Rime, and Strahan 2004), income volatility (Demyanyk, Ostergaard, and Sørensen 2007), and income inequality (Beck, Levine, and Levkov 2010).

section, big banks lend proportionally less to small firms. The evidence from consolidations is mixed, which may be explained by the non-randomness of consolidations (Berger, Saunders, Scalise, and Udell 1998; Peek and Rosengren 1998; Strahan and Weston 1998; Berger, Klapper, and Udell 2001; Sapienza 2002; Jagtiani, Kotliar, and Maingi 2016). Cole, Goldberg, and White (2004) analyze a firm survey and report that big banks are less likely to use soft information. Berger, Miller, Petersen, Rajan, and Stein (2005) find that firms located in markets with larger banks rely more on trade credit, which indicates credit constraints. My identification strategy uses exogenous variation in the size of the same bank serving the same firm. This strategy overcomes concerns that the non-randomness of consolidations or underlying, cross-sectional differences across regions, firms, and banks bias the estimated effects. An additional innovation relative to the small-firm literature is that I focus more broadly on opaque firms, rather than just small firms, and estimate real effects on employment, rather than just lending.

A number of papers study how banks acquire and use information about their borrowers. These papers do not speak directly to the question of bank size, but their findings support the view that the decreased use of soft information in large organizations can explain the negative effects of bank size on opaque firms. Liberti and Mian (2009) and Canales and Nanda (2012) find that banks with large hierarchies rely less on soft information, while Cerqueiro, Degryse, and Ongena (2011) report that the loan terms for opaque borrowers depend on the discretion of the loan officer. Skrastins and Vig (2018) show that the introduction of additional hierarchical layers in Indian bank branches reduced total lending and the performance of loans. Consistent with all these results, Qian, Strahan, and Yang (2015) report that loan officers' incentives and internal communication costs strongly affect the quality of information produced by banks.

Other papers investigate specific channels, through which bank size can affect efficiency. Houston, James, and Marcus (1997), Gilje, Loutskina, and Strahan (2016), and Cortés and Strahan (2017) show that banks use internal capital markets in response to shocks. Geographic diversification raises bank-internal agency problems (Goetz, Laeven, and Levine 2013), reduces bank risk (Goetz, Laeven, and Levine 2016), and lowers funding costs (Levine, Lin, and Xie 2016). Unlike these papers, I do not focus on the effects of internal capital markets or diversification. Instead, I estimate the causal effects of bank size, which may be partially driven by these channels.

3.2 Institutional Details

This paper's methodology relies on two institutional features of the postwar German banking system: relationship banking and the Allied banking reforms. In combination, these two features give rise to a natural experiment: Firms with a treated relationship bank were exposed to an exogenous increases in the size of their banks. This section describes the two features.

3.2.1 Relationship Banking

Economic history (Jeidels 1905; Calomiris 1995), case studies (summarized in Guinnane 2002), and recent evidence (Harhoff and Körting 1998; Elsas and Krahnen 1998; Elsas 2005a) suggest that relationship banking has played an important role in German corporate finance from the start of the 19th century until today. Firms of all sizes formed close and durable business ties to their banks, which reduced asymmetric information and improved banks' monitoring capabilities (Sharpe 1990a; Boot 2000).[5] The literature provides empirical evidence from a number of countries and episodes that idiosyncratic shocks to relationship banks have real effects on firms, for instance Benmelech, Frydman, and Papanikolaou (2017) for the US Great Depression, Amiti and Weinstein (2011a) for Japan in the 1990s and 2000s, Chodorow-Reich (2014a) for the 2008-09 US financial crisis, and Bentolila, Jansen, Jiménez, and Ruano (forthcoming) for the Great Recession in Spain.

Three types of banks operated in postwar Germany: commercial banks, cooperative credit unions, and public banks (Landesbanken and savings banks). The banks offered their relationship customers the range of universal banking services. Most important were lending, deposit-taking, payment transactions, and the underwriting of corporate bonds and stocks.

3.2.2 The Allied Banking Reforms

Three Allied military governments ruled over occupied West Germany after World War II. The British were in charge of Northern and Western Germany, most of the South was under American control, and the French governed two small regions in the South-West. The military government of the American zone was the driving force behind banking policy.

Phase 1: State-level Breakup 1947/48-52 During the initial years of the occupation, the American aim was to weaken the German economy, so that it would not be able to support another war in the future (as laid out in the doctrine of the Morgenthau Plan). American policymakers were convinced that one reason for the Nazis' ability to wage a destructive war had been the centralized banking system. They wanted to break the

[5] In an influential essay, Gerschenkron (1962) argues that the direct involvement of large banks in corporate governance was crucial for German industrialization in the late 19th century. Fohlin (1998, 1999) challenges this theory, but does not argue against the view that firms depended on their relationship banks for financial services.

political power of the large banks and punish them for cooperating with the Nazis. Three large banks with a national branch network remained active at the end of the war: Commerzbank, Deutsche Bank, and Dresdner Bank. All had cooperated with the Nazi regime. I refer to these banks as "treated" (Adler 1949; Horstmann 1991).

The first step towards the breakup of the treated banks came in early 1946, when the Americans prohibited the treated bank managers in their zone from coordinating business with branches in other zones (Wolf 1994). In May 1947, an American military law institutionalized the breakup. The law created new state-level banks, composed of the branches of the treated, former national banks. The new banks were not allowed to operate a branch in another federal state. Their directors were the regional and national managers of the former national banks. Government-appointed custodians, independent and unconnected to the former banks, were in charge of ensuring the new state-level banks operated separate financial accounts from each other. The names of the new institutions were unrecognizable from the former national names, to underscore that the newly-formed entities were separate from each other and their former national structure (Der Spiegel 1951). The financial services offered by the treated banks remained unchanged. The law did not introduce new regulations for the untreated commercial, cooperative, or public banks.

The French military government issued an identical decree for their zone in 1947. The British were initially against the breakup, since they worried that foregoing the efficiency benefits of big banks would harm German economic recovery. In April 1948, however, they gave in to American pressure and applied the American regulations in their zone. This first phase of Allied legislation completely changed the structure of the treated banks. Instead of three treated national banks, as before the war, there were now 30 independent state-level banks (Holtfrerich 1995; Ahrens 2007).[6] Panel A of Figure 5.23 shows a map of the state-level banking zones.

Phase 2: Three Banking Zones 1952-57 In the early 1950s, the Allied attitude towards West Germany changed. Instead of weakening the German economy, the Allies now wanted it to serve as buffer against the Communist threat from Eastern Europe. There was disagreement among the Allies, German politicians, and bankers on how to optimally reorganize the banking system. The Americans, leading Southern state politicians, and most central bankers believed that the state-level banks supplied financial services efficiently. On the other hand, the British, the federal German government, and most of the treated bank directors argued that bigger banks would be more

[6] To be clear, take the example of Dresdner Bank: Instead of one national Dresdner Bank, as before the war, there were 11 state-level successor banks in 1948, one in each state. Each state-level bank was composed of the former Dresdner Bank branches in the relevant state. No Deutsche Bank branches existed in Schleswig-Holstein, so there were 10 Deutsche Bank successors. No Commerzbank branches existed in Baden and Württemberg-Hohenzollern, so there were 9 Commerzbank successors.

efficient (Horstmann 1991).

The Allies and the federal German government reached a compromise in September 1952. They created three banking zones, shown in Panel B of Figure 5.23. There were precise rules on how the treated banks were allowed to partially reconsolidate within these zones. The state-level banks were allowed to consolidate with other state-level banks belonging to the same former national bank and located within the same banking zone. Out-of-zone branching was prohibited. The first zone comprised the Northern states, which were under British control. The American and French territories were combined to form the Southern zone. The third zone was the state of North-Rhine Westphalia, also under British control. Since the state and zonal borders were identical, the treated banks operating in the state of North-Rhine Westphalia remained unaffected by the 1952 reform. The empirical strategy outlined in the subsequent section exploits the particular treatment of the banks in North-Rhine Westphalia to construct a plausible control group for the 1952 reform.

The majority of treated bank directors believed their banks would benefit from being larger. Hence, the Northern and Southern state-level banks had consolidated by the end of 1952, soon after the reform. Instead of 30 state-level banks, there were now nine treated banks, one for each former national bank in each banking zone (Wolf 1993). Most of the directors of the nine banks had been directors of the former state-level banks. The reform did not directly affect the bank staff and the total number of branches (Holtfrerich 1995).

Phase 3: National Banks from 1957 Five years later, international political developments affected the structure of the treated banks once more. The emergence of the Cold War had made Germany a key ally of the West. The Allies granted the German government full sovereignty in the Paris Agreement of 1955. Since the federal government had always believed in the efficiency of large banks, it lifted all restrictions from January 1957 (Scholtyseck 2006). The treated banks subsequently consolidated. By 1958, there were once again three large banks with a national branch network, operating under their old, pre-war names. All directors of the former, zonal banks joined the boards of the new national banks, while staff and branches remained unchanged (Horstmann 1991; Holtfrerich 1995).

The reconsolidation of the treated banks was not a foregone conclusion. The Americans had intended the breakup to be permanent (Der Spiegel 1951). Apart from the treated banks, the Allies broke up three other large corporations into small, independent organizations: the chemical manufacturer I.G. Farbenindustrie, the steel corporation Vereinigte Stahlwerke, and the movie producer Universum Film. Unlike in the case of banking, German politicians did not believe these industries benefited from significant economies of scale. Hence, these organizations remained broken up in sovereign Germany, against the wishes of their management (Kreikamp 1977).

3.3 Theory and Identification

Economic theory suggests that changes in the size of a bank can affect its efficiency. This section reviews the theoretical advantages and disadvantages of big banks. I explain how the banking reforms of 1952 and 1957 affected the organization of the treated banks with respect to each theoretical advantage and disadvantage. A model of lending under relationship banking then illustrates how size-induced changes to bank efficiency can affect firms. The final part of this section argues that the postwar banking reforms provide a suitable natural experiment that identifies the causal effects of bank size on firms.

3.3.1 Advantages of Big Banks

The first theoretical benefit of big banks is that they are more diversified and therefore have lower funding costs. Under the assumption that there are fixed costs to monitoring borrowers, models by Diamond (1984), Boyd and Prescott (1986), and Williamson (1986) show that banks with a larger number of customers attract cheaper deposits, because they can diversify more cheaply. A monopoly bank is socially efficient in these models. The postwar banking reforms sharply increased the number of customers served by one treated institution. If indeed there are consequential fixed costs to monitoring, the treated banks should have benefited from cheaper funding after the reforms. Holtfrerich (1995) quotes a number of treated branch managers that argued during the breakup period that the reforms would lower funding costs.

A second theoretical benefit of big banks is that they use internal capital markets to allocate funds. During the breakup, the treated banks were allowed to hold interbank accounts, but had to settle their mutual balances through the central banking system, just like the other commercial banks (Adler 1949). After consolidating, they were able to allocate capital internally. Horstmann (1991) explains that interbank markets and central clearing were well-developed in postwar Germany. Accordingly, treated banks with a strong deposit base regularly lent through interbank markets before the reforms (Wolf 1994). If there were significant benefits to internal capital markets, the treated banks should have become more efficient after the reforms. Stein (1997) argues that the use of internal capital markets is optimal when external financial markets are underdeveloped. On the other hand, Scharfstein and Stein (2000) show that rent-seeking behavior by division managers can lead to an inefficient allocation of funds through internal capital markets. If such rent-seeking is widespread, the access to larger internal capital markets could actually have been detrimental to the efficiency of the treated banks.[7]

The third benefit concerns the larger capital base of big banks. Big banks can spread fixed costs over more customers and fund larger loans on their own. Treated branch managers expressed concerns about high overhead costs from operating separate payment transactions systems and from employing specialized credit experts for each industry before the reforms (Horstmann 1991). Wolf (1994) documents that during the first phase of the breakup, the treated banks formed loan syndicates with other treated and untreated banks to fund large loans. If contracting frictions are high for loan syndicates, the cost of large loans should have fallen after the reforms.

3.3.2 Disadvantages of Big Banks

The first theoretical disadvantage of big banks arises from the complexity of managing a large number of customers. Williamson (1967) argues generally that transmitting accurate information to decision-makers is difficult in large organizations. If banks cannot fully diversify their risk, Krasa and Villamil (1992a,b) show that the costs of monitoring big institutions can outweigh the benefits of diversification, raising the cost of deposits. In the model by Cerasi and Daltung (2000), limited resources of individual bankers mean that the marginal cost of lending to an additional borrower is increasing. The reforms increased the number of hierarchical levels and the organizational complexity of the treated banks. For instance, during the first phase of the breakup, each treated state-level bank decided on loan applications independently in regionally specialized credit councils (Horstmann 1991). After the reforms, a centralized decision-making structure took over.

Models by Stein (2002), Berger and Udell (2002), and Brickley, Linck, and Smith (2003) suggest a second disadvantage. Institutions with many hierarchical levels may be less suited to processing soft, difficult-to-verify information. Soft information is important when banks deal with opaque firms, where it is difficult to objectively document creditworthiness. In such cases, bank managers may rely on soft information to issue character loans, for example. The centralization of decision-making after the reforms may have reduced the incentives for regional managers to collect soft information, lowered the availability of soft information to the responsible bank managers, and ultimately decreased loan supply to opaque relationship customers of the treated banks.

[7] One hypothesis is that the benefits to internal capital markets depend on how closely the treated banks cooperated during the breakup period. Reports by the German Federal Ministry of Economics, Ahrens (2007), and Horstmann (1991) suggest that the successor banks of the Dresdner Bank cooperated most closely with each other, for example by organizing meetings of all the heads of the successor banks. The Commerzbank and Deutsche Bank successors cooperated less. In the result below, I report no differential effects for the corporate customers of the Dresdner Bank successors, suggesting the degree of cooperation did not significantly influence the effects of the postwar reforms.

A third theoretical disadvantage is that big banks may take more risks, because of moral hazard or agency problems. The cause of moral hazard is that big banks carry higher systemic risk (Pais and Stork 2013; Adrian and Brunnermeier 2016). As a result, Freixas (1999) argues, governments are more likely to bail out big banks when they become insolvent. Dávila and Walther (2017) show theoretically that big banks internalize the increased probability of a bailout and take more risk. The postwar reforms increased the probability that the treated banks would experience a bail-out. One reason is their increased size and hence systemic importance. A second reason is that the German government, which became sovereign before the second reform, believed in the economic necessity of big banks. In contrast, the Allied governments, that had been in charge before the second reform, had actively tried to break the large banks' influence.

The cause of agency problems is the increased hierarchical distance between bank directors and local branch managers in big organizations. Directors of big organizations find it more difficult to directly monitor the local bank managers and understand the local risks. Instead, they may reward the local managers based on outcomes. Many such outcome-based reward schemes distort incentives. Bank managers may reap the benefits if the risk pays off, for example by earning promotions. They may not suffer sever consequences in the downside scenario, for example because they can easily find a job at another bank or because it cannot be unambiguously documented that their increased risk-taking is to blame for losses. If the upside benefits outweigh the downside risks in such a manner, the local managers in big organizations have a greater incentive to take risks (Rajan 2005; Kashyap, Rajan, and Stein 2008).

A theoretical social cost of bank consolidation is a decrease in competition. Importantly, the Allied banking reforms changed the size of the treated banks without affecting competition in the regional banking markets. The reason is that the state-level institutions did not compete with each other, as they were not allowed to open branches in other federal states.[8] Hence, the number of banks operating in each regional banking market remained unaffected by the bank breakup and the reforms. This allows me to isolate the pure effects of bank size from the effects of competition.

3.3.3 Model

The theoretical considerations documented above suggest an increase in the size of a bank can affect bank efficiency. The appropriate measure of size in these models is the number of customers served by a bank.[9] Henceforth, I refer to increases in bank

[8]The data on bank-firm relationships show that 99 percent of firms did not have a treated relationship bank outside the state of their headquarters in 1951. The exceptions may be explained by firms operating multiple establishments.

[9]The reason is that by adding new customers with imperfectly correlated default risk, the bank becomes more diversified. This is not true when the bank simply expands lending to a single customer.

size and increases in the number of customers interchangeably. The following model illustrates how shocks to the number of customers can affect the loan supply of firms, if the cost function of the bank depends on the number of its customers. The key assumption of the model is that a firm can only borrow from its relationship bank, due to asymmetric information in credit markets (Sharpe 1990a). This implies that banks hold a "bilateral monopoly" (Boot 2000) over each relationship customer.

Firms Firm *ib* maximizes profits π_{ib}. Capital K_{ib} is the sole input, which the firm borrows at a interest rate r_{ib} from its relationship bank. The firm takes the interest rate as given. A_{ib} captures all exogenous factors shifting the firm's demand for capital, such as productivity shocks or demand shocks in the product market. The returns-to-scale production parameter is α, where $0 < \alpha < 1$:

$$\pi_{ib} = A_{ib}K_{ib}^{\alpha} - r_{ib}K_{ib}.$$

The firm's optimal demand for capital is given by:

$$\alpha A_{ib}K_{ib}^{(\alpha-1)} = r_{ib}. \tag{3.1}$$

Banks Bank *b* lends to a total of n_b relationship customers. For now, assume the bank takes as given the total number of relationship customers. I discuss reasons for why this number may change when discussing equilibrium below. Banks earn interest income, which is the interest rate charged to each relationship customer multiplied by the amount of capital lent to that firm, summed over all firms. The bank faces the capital demand function of each relationship customer, as reported in equation 3.1.

Banks pay a constant marginal cost for each unit of lent capital, $c(n_b, \beta_b)$. This marginal cost includes expenditures on risk management, employees, and deposits. The marginal cost is a function of a bank efficiency parameter β_b and the total number of relationship customers n_b. The marginal cost is decreasing in bank efficiency β_b. As discussed in the previous subsection, theory is ambiguous about the effect of the number of relationship customers n_b on marginal cost. The bank maximizes profits π_b:

$$\pi_b = \sum_{i=1}^{n_b} [r_{ib}K_{ib} - c(n_b, \beta_b)K_{ib}], \tag{3.2}$$

where the first term in the bracket is the interest income from lending to firm *ib* and the second term in the bracket is the total cost from lending to firm *ib*.

Equilibrium Combining equations 3.1 and 3.2 and taking the first-order condition gives the optimal amount of capital lent to firm *ib*, K_{ib}. This amount increases with the exogenous capital demand shock A_{ib} and decreases with the marginal cost of lending $c(n_b, \beta_b)$:

$$ln(k_{ib}) = \frac{1}{1-\alpha}[ln(A_{ib}) - ln(c(n_b^{'}, \beta_b^{'})].$$

A simple specification of the marginal cost for each unit of lent capital is:

$$ln(c(n_b, \beta_b)) = -\phi n_b - \kappa \beta_b,$$

where ϕ is either positive or negative and κ is strictly positive. Under this specification, the change in capital lent to firm ib from period t to period t' is given by equation 3.3, where the operator $\Delta^{t,t'}[.]$ indicates the growth of the variable in square brackets from t to t':

$$\Delta^{t,t'}[ln(K_{ib})] = \frac{1}{1-\alpha} \cdot \Delta^{t,t'}[ln(A_{ib})] + \frac{\phi}{1-\alpha} \cdot \Delta^{t,t'}[n_b] + \frac{\kappa}{1-\alpha} \cdot \Delta^{t,t'}[\beta_b]. \tag{3.3}$$

Changes in firm capital demand A_{ib}, the number of the bank's relationship customers n_b (i.e. bank size), and bank efficiency β_b determine the growth in capital lent. The coefficient $\frac{\phi}{1-\alpha}$ measures the causal effect of changes in bank size on firm growth, the key object of interest in this paper. The model can be extended to include other factors of production complementary to capital, such as employment. These factors would depend on firm capital demand, bank size, and bank efficiency in a qualitatively similar manner to capital.

Empirical Implication The empirical challenge in estimating the causal effect of bank size arises from the fact that changes in firm capital demand A_{ib} and bank efficiency β_b are typically unobservable in the data. This means the estimable specification is:

$$\Delta^{t,t'}[ln(K_{ib})] = \frac{\phi}{1-\alpha} \cdot \Delta^{t,t'}[n_b] + \upsilon_{ib}, \tag{3.4}$$

where firm capital demand and bank efficiency enter the unobservable error term:

$$\upsilon_{ib} = \frac{1}{1-\alpha} \cdot \Delta^{t,t'}[ln(A_{ib})] + \frac{\kappa}{1-\alpha} \cdot \Delta^{t,t'}[\beta_b].$$

A regression based on equation 3.4 estimates the true causal coefficient $\frac{\phi}{1-\alpha}$ if changes in firm capital demand and bank efficiency are generally uncorrelated with changes in bank size. However, banks do not become big randomly. For example, a random shock to regional productivity will lead to firm entry, raising the size of banks operating in that region, and simultaneously increase the capital demand of incumbent bank customers. In addition, banks strategically consolidate with other banks, for example because they expect increases in the efficiency of the other banks that are unrelated to size. These considerations imply that changes in bank size are likely to be correlated with changes in firm capital demand and bank efficiency. Hence, the observed, unconditional correlation between bank size and firm growth can be positive, even if the true causal coefficient $\frac{\phi}{1-\alpha}$ is zero.

To estimate the causal effects of bank size on firms, a suitable experiment needs to manipulate the number of a bank's relationship customers, without directly affecting firm capital demand, bank efficiency, and other unobservable components of firm and bank performance.

3.3.4 Identification Strategy

The postwar reforms of 1952 and 1957 provide suitable natural experiments that allow estimating the causal effects of bank size. Equation 3.4 motivates the regression specification, which is given by equation 3.5. The outcome is the growth in the total amount of capital borrowed by firm ib from period t to period t':

$$\Delta^{t,t'}[ln(K_{ib})] = \theta \cdot (relationship\, bank\, treated\, between\, t\, and\, t')_b + \eta \cdot X_{ib} + \varepsilon_{ib}. \quad (3.5)$$

Alternative specifications use firm employment growth and revenue productivity growth as outcomes.

The key regressor is an indicator for whether one of the firm's relationship banks increased in size due to a postwar reform between the years t and t'. This regressor serves as proxy for an increase in the number of the bank's customers, i.e. the term $\Delta^{t,t'}[n_b]$ from equation 3.4.[10]

Equation 3.5 can be thought of as reduced-form specification. The coefficient θ captures all the channels, through which a bank size shock could affect firms.[11] θ estimates the causal effect of a change in bank size on firm growth if a parallel-trends assumption holds. This assumption requires that firms with a treated relationship bank would have grown in parallel to other firms, had it not been for the reforms.

The parallel-trends assumption is equivalent to the assumption that the components of the error term, including changes to firm capital demand and bank efficiency, are uncorrelated with the treatment indicator. Changes in firm capital demand, $\Delta^{t,t'}[ln(A_{ib})]$, and bank efficiency, $\Delta^{t,t'}[\beta_b]$, enter the error term ε_{ib}, as in equation 3.4. Importantly, the reforms increased the size of the banks independent of changes in the firms' capital demand or other determinants of bank efficiency. The results sections below present evidence in support of the parallel-trends assumption, including parallel pre-trends and

[10]In robustness checks, I also use regressors based on the fraction of the firm's relationship banks that were treated. The baseline specification uses the dummy, since it is not clear theoretically whether the treated banks would extend the benefits of size equally to all their relationship customers or more to firms with a higher fraction of treated relationship banks.

[11]Apart from the interest rate on loans, the return on deposits, the cost of payment services, and expectations about future credit access could all be affected.

balancing tests of firm and bank observables. To further strengthen the assumption, the regressions condition on a vector of control variables X_{ib}, described in the relevant results section.

The data allow me to analyze two periods. First, I can calculate the growth of stock corporations from 1951 to 1960. The two reforms were in 1952 and 1957, so the reforms affected all stock corporations with a treated relationship bank during this period. The second period I analyze is from 1951 to 1956. I have data on the growth of non-stock firms for this period. The 1952 reform only affected treated banks outside the state of North-Rhine Westphalia (NRW). Hence, the treatment dummy in the specifications analyzing growth from 1951 to 1956 indicates whether firms had a relationship bank that was treated in 1952. The samples in the baseline regressions include all firms, for which I have data for the given period.

For the period from 1951 to 1956, I additionally create a more restrictive, "matched" sample. There are four restrictions for the matched sample. First, it only includes firms that had a relationship bank treated in either 1952 or 1957. This restriction addresses the concern that firms with a relationship to a bank treated in either 1952 or 1957 differed from firms with banks that were never treated. Second, I drop from the sample firms located in the Ruhr area, an urban region within NRW traditionally based on heavy industry, which was potentially exposed to different economic shocks than the rest of the country. Third, to address the concern that the formation of the European Coal and Steel Community in 1952 may bias the results, I drop firms producing coal and steel. Fourth, from the remaining sample, I only use firms in NRW or in states bordering NRW. The state of NRW was a hasty postwar creation, based on the British desire to institutionalize its control over Western Germany. The subregions composing NRW were culturally heterogeneous. Many were more similar to the states they bordered than to the other subregions in NRW (von Alemann 2000). Regressions using the matched sample identify the effect by comparing relationship customers of banks treated in both 1952 and 1957 (located in states bordering NRW) to customers of banks treated only in 1957 (located in NRW).[12] The use of the matched sample strengthens the parallel-trends assumption because the restrictions make it likely that all firms in the matched sample were affected by similar unobservable shocks.

Three additional analyses supplement the main results on firm growth. I study the financial figures and media mentions of banks, the establishment of new banking relationships, and municipal employment growth. All analyses require a similar parallel-trends assumption as the main analysis, namely that the treated banks and municipalities with a treated bank branch would have evolved in parallel to other banks

[12] In additional tests, I apply only the first restriction, comparing relationship customers of banks treated in both 1952 and 1957 (located in any state except NRW) to customers of banks treated only in 1952 (located in NRW). The results are similar.

and municipalities in the absence of the reforms.

3.4 Data

3.4.1 Data on Firms

At the heart of the paper lies a newly digitized dataset on the relationship banks and the growth of German firms in the 1950s. To my knowledge, this is the first digital micro-dataset on German firms in the postwar period. The data source are two publication series by the commercial information provider Hoppenstedt. The historic volumes of these series are difficult to locate.[13] Supported by the German National Library of Economics, I was able to access the 1941, 1952, 1958/59, and 1970 volumes of the publication *Handbuch der Grossunternehmen* and the 1952/53, 1961/62, and 1970/71 volumes of the publication *Handbuch der deutschen Aktiengesellschaften* in various German archives. The poor print quality of the older volumes does not allow automatic digital character recognition. Instead, I photographed all pages from these publications, around 15,000 photographs in total. Appendix Figure 6.9 displays a photograph of a page from a firm entry in the 1952/53 volume on *Aktiengesellschaften.* The firm data were then digitized by hand.

The publication on *Aktiengesellschaften* reports data on all German stock corporations, while *Grossunternehmen* includes a subset of firms of other legal forms. In the postwar years, both publications list the firms' names, addresses, names of relationship banks, and employment. There is no information on what financial services or how much credit a firm received from a particular relationship bank. *Aktiengesellschaften* additionally reports revenue, total assets, liabilities, and bank debt, while *Grossunternehmen* indicates whether the firm exported any of its products. A significant number of firms in both publications have missing data on many of these variables.

The main dataset builds on the 1952 and 1958/59 *Grossunternehmen* and the 1952/53 and 1961/62 *Aktiengesellschaften* volumes. From these volumes, I digitize data for all non-financial firms that, at a minimum, contain the names of the firm's relationship banks. There are 2,882 such stock corporations and 4,589 such non-stock firms in the 1952/53 volumes. Using the firm name and address as identifiers, I perform a Stata fuzzy match (reclink) procedure to connect firm entries from 1952/53 volumes to the 1958/59 and 1961/62 volumes. I check all matches by hand to ensure there are no errors. Additionally, I identify 43 cases of firm exit, which are reported at the end of the Hoppenstedt volumes. There are also six reported mergers of firms in the dataset. To account for the mergers, I aggregate the employment and balance sheet values of all firms participating in the merger, record all their relationship banks, and keep only

[13] Hoppenstedt destroyed its entire paper archive a few years ago. Online library catalogs do not always report the holdings accurately because historic volumes get misplaced or break.

one firm in the dataset for the years before the merger. Overall, the match leaves 2,188 stock corporations and 3,706 non-stock firms in the dataset.

A Hoppenstedt volume reports data for one to three years prior to the release year of the volume. For instance, the 1952 volume mostly reports data for 1951, while the 1958/59 volume mostly covers 1956. For the firms in *Aktiengesellschaften*, I can calculate the growth of employment, revenue per worker, total assets, liabilities, and bank debt from 1951 to 1960. For the firms in *Grossunternehmen*, it is possible to calculate employment growth from 1951 to 1956. Some firm entries in the 1952/53 volumes report 1949 employment values, so I can calculate the pre-reform growth of these firms from 1949 to 1951. The measure of growth is the symmetric growth rate, a second-order approximation to the growth rate of the natural logarithm. It naturally limits the influence of outliers and accommodates zeros in the outcome variable, for example due to firm exit (Davis, Haltiwanger, and Schuh 1998a).[14] To accommodate comparisons of growth rates across periods of different lengths, I calculate all the firm growth rates as average annual growth rates, by dividing the symmetric growth over the whole period by the number of years in the period.[15]

From the 1941 and 1970 *Grossunternehmen* and the 1970/71 *Aktiengesellschaften* volumes, I record only the relationship banks. No data on relationship banks exist in the *Aktiengesellschaften* volumes prior to 1952. Recording relationship banks over a longer time horizon is helpful in identifying changes in relationships, because few German firms add new relationship banks every year (Dwenger, Fossen, and Simmler 2015). There is a successful match for 373 firms between the 1941 and 1952 volumes. From the 1970/71 volumes, I match the relationship banks of 4,191 firms to the 1952/53 volumes.

3.4.2 Summary Statistics on Firms

Table 5.16 summarizes the main firm dataset. The median stock corporation in the sample was of a similar size and age to the median non-stock firm, both having close to 350 employees in 1951.[16] The very largest firms, however, were stock corporations, which means the average stock corporation was larger than the average non-stock firm. Both stock capital and bank debt were important parts of stock corporations' financing, amounting to an average of 37 percent and 10 percent of total assets, respectively. The percent ratio of bank debt over assets changed by an annual average of -0.11 percentage points from 1951 to 1960, which suggests bank debt grew at a marginally lower rate

[14]Formally, the symmetric growth of y from t-1 to t is $g^y = 2 \cdot \frac{(y_t - y_{t-1})}{(y_t + y_{t-1})}$. It is bounded in the interval [-2,2].

[15]For example, the total symmetric growth rate from 1951 to 1960 is divided by 9, the number of years between the base and final year. This gives the average annual growth rate.

[16]To be registered as stock corporation, firms had to hold at least 100,000 Deutsche Mark in stock capital. The advantage of stock corporations is that they could raise funds by issuing new stock capital.

than assets. The average annual symmetric growth rate of aggregate employment in West Germany was 0.04 from 1951 to 1956 and 0.03 from 1951 to 1960. The average growth rates of firms in the sample were identical to these aggregate growth rates, suggesting the firms are fairly representative for the period.

In total, the firms with non-missing employment data in the sample cover 15 percent of West Germany's 14.6 million employees in 1951 (Bundesministerium für Arbeit 1951). In the sample, 14 percent of stock corporations and 6 percent of non-stock firms have fewer than 50 employees. The number of firms in the 1951 population is unavailable, but as rough guide, the fraction of establishments with fewer than 50 employees was above 98 percent (Statistisches Bundesamt 1952). 70 percent of firms in the sample are in the manufacturing sector, compared to 32 percent of establishments in the population. All specifications in the results section control for firm size and industry when estimating average effects, to ensure the findings depend on variation in exposure among firms of similar size and industry. I also explore heterogeneity related to size and industry.

In 1951, stock corporations had on average 3.2 relationship banks. Non-stock firms had on average 2.5. I calculate two main treatment dummies. The first, called "relationship bank treated in 1952/57", indicates whether one of the firm's relationship banks in 1951 was treated by the postwar banking reforms, either in 1952 or 1957. The second, called "relationship bank treated in 1952", measures whether a 1951 relationship bank was treated by the 1952 reform, i.e. whether the firm had a relationship to a treated bank outside of North-Rhine Westphalia. 68 percent of stock corporations and 69 percent of non-stock firms have a relationship bank treated in 1952 or 1957, while 46 percent and 41 percent have a relationship bank treated in 1952.

To test whether firms with a treated relationship bank differed from other firms, I regress the two main treatment dummies on firm observables in Table 5.17. Column (1) shows that larger and older stock corporations were more likely to have a relationship bank that was treated in 1952 or 1957. The coefficients on the balance sheet variables in column (2) are small and insignificant, indicating that stock corporations with a treated bank were not more reliant on stock capital financing or bank debt financing, conditional on size and age. Columns (3) and (4) similarly reveal that larger and older non-stock firms were more likely to have a bank treated in 1952 or 1957, but that being an exporter was conditionally uncorrelated with having a treated bank.

The regressions in columns (5) and (6) use the matched sample. The outcome of interest in the matched sample is whether a relationship bank was treated in 1952. There is no correlation between having a bank treated in 1952 and size or age, for either stock corporations or non-stock firms. Unreported additional tests also reveal no correlation with firm stock capital financing, bank debt financing, and export status. These results strengthen the argument that the matched sample provides a credible natural experiment, since observationally equivalent firms were exposed to differential bank size shocks.[17]

3.4.3 Data and Summary Statistics on Banks

Data on banks supplement the firm-level analysis. The Deutsche Bundesbank reports lending and deposits aggregated at the level of groups of banks, starting in 1948. One of the groups includes all the treated banks. Most similar to the treated banks is the group of other commercial banks. These other commercial banks all operated for profit. Most were active within only one state, although a handful had branches in two or three states. The group of other commercial banks does not include small, single-branch private banks (*Privatbanken*).

The data by the Deutsche Bundesbank do not include banks' cost statements and information on individual banks. I therefore additionally hand-digitize financial figures from annual bank reports. The treated banks were universal, commercial, branching banks. To find a set of comparable institutions, I use the banking handbook by Hofmann (1949). There were 16 universal, commercial banks with a branch network in operation in 1949, apart from the treated banks. I was able to locate the 1952 and 1960 annual reports of 9 of these untreated banks in German libraries and archives, in addition to the reports of the treated banks. The reports of many treated and untreated banks for the years before 1952 have not been preserved. The treated banks consolidated after the first reform in September 1952, so the effect of the reforms on the December 1952 figures is likely small.

Table 5.18 compares the treated to the 9 untreated banks. I aggregate figures for the treated banks at the level of the three national banks that were formed after the second reform in 1957. Hofmann (1949) lists the three banks with the largest branch network apart from the treated banks: Bayerische Hypotheken- & Wechsel-Bank, Bayerische Vereinsbank, and Oldenburgische Landesbank. These three banks serve as a suitable direct comparison to the treated banks, since they had a similar number of branches to the treated banks during the first and second phases of Allied policy. The table reports figures for these three comparison banks and also the average value of all the 9 untreated banks, which includes the three comparison banks.

The first three columns show the mechanical impact of the reforms on bank size. Total assets for each banking group are fixed at their 1952 values and then divided by the number of individual banks in the relevant period. As the reforms lowered the number of banks in the treated groups, the average size of each institution in the treated

[17]The improved sample balance in the matched sample is mainly due to the first restriction of only using firms with a relationship bank treated in either 1952 or 1957 in the sample. Applying only the first restriction, the data also reject the hypotheses that firms with a relationship bank treated in 1952 and 1957 (outside NRW) were older or larger than firms with a relationship bank treated only in 1957 (in NRW). The results on firm growth presented below similarly hold when using only the first restriction.

groups rose. For instance, the two reforms increased average bank size in the Deutsche Bank group by a factor of ten, since there were 10 state-level banks during the breakup. The untreated banks naturally remained unaffected.

Column (1) show that the average total assets of a treated bank in 1952 were 323 million Deutsche Mark, while for the average untreated bank total assets were 330 million. Columns (4) to (6) present three cost ratios commonly used to measure bank efficiency, discussed in more detail in the results section. The 1952 values for all banks are relatively close. These numbers indicate that the untreated banks are a suitable control group for the treated banks.

3.4.4 Data and Summary Statistics on Municipalities

The municipal employment data are hand-digitized from the annual publication *Statistisches Jahrbuch deutscher Gemeinden.* I digitize employment data for 1949, 1951, 1956, and 1960, matching the years for which I have firm employment data. The annual bank reports identify whether a municipality had a treated bank branch. Sectoral employment shares are from the 1950 *Betriebszählung* (census of enterprises). Average employment in the municipalities in the sample was 64,992 in 1951. 86 percent of municipalities had a bank branch treated in either 1952 or 1957. 52 percent had a bank branch treated in the first reform of 1952.

3.5 Results on the Growth of Firms

This section presents the main results of the paper. It analyzes the effect of the postwar banking reforms on the growth of firms, separately for stock corporations and non-stock firms.

3.5.1 The Effect on the Growth of Stock Corporations

Table 5.19 estimates the effect of the reforms on stock corporations. The specifications are based on equation 3.5. The outcome in Panel A is the average annual growth rate of bank debt. The regressor of interest is a dummy for whether a bank treated in 1952 or 1957 was among the firm's relationship banks in 1951. The untreated group includes firms with relationship banks that were neither treated in 1952 nor in 1957. If the reforms led to an increase in firms' bank loan supply, the coefficient should be positive. The point estimate in column (1) implies that the growth of bank debt of firms with a treated bank was approximately 0.1 percentage points lower per year, compared to firms with no treated relationship bank. The 95 percent confidence interval excludes growth differences greater than 3 percentage points.

One potential concern is that broad regional differences or heterogeneous shocks to certain industries may mask the true effect. Column (2) includes the full interaction of 18 industry fixed effects[18] with fixed effects for the Northern, Western, and Southern regions of Germany, equivalent to the banking zones 1952-57. To account for variation in growth due to firm size and age (Haltiwanger, Jarmin, and Miranda 2013), column (3) adds controls for ln firm age and ln firm assets in 1951, again interacted with three zonal fixed effects. These control variables ensure region-specific shocks to firms in certain industries, of certain sizes, or certain ages do not affect the results. The coefficients remain close to zero and statistically insignificant. There is no evidence that firms with treated banks experienced an improvement in their bank loan supply relative to other firms.

The outcome in Panel B is the average annual change in the percent ratio of bank debt over total assets. If firms with a treated relationship bank had access to cheaper bank debt, they should have funded themselves with more bank debt, increasing the ratio. The use of the ratio of bank debt over total assets as outcome is conceptually equivalent to controlling for changes in the firms' total demand for funding, for example by using firm fixed effects. The coefficient in column (1) implies that firms with treated banks raised their ratio of bank debt over assets by a statistically insignificant 0.14 percentage points. This point estimate is small, as it amounts to 10 percent of a standard deviation of the outcome variable. The 95 percent confidence interval excludes increases in the ratio greater than 0.5 percentage points.

Panels C and D similarly report small and insignificant effects on employment and revenue per worker, respectively. The 95 percent confidence intervals exclude growth increases greater than 0.9 and 1.4 percentage points, respectively. There is no evidence that the reforms led firms to hire more workers or improve revenue productivity.

3.5.2 The Effect on the Growth of Non-Stock Firms

Table 5.20 analyzes non-stock firms. The outcome variables is the average annual employment growth from 1951 to 1956. The regressor of interest in this table is a dummy for whether the firm had a relationship bank that was treated in 1952 (i.e. a treated bank outside of NRW). The untreated group includes firms with relationship banks that were neither treated in 1952 nor in 1957. It also includes firms with relationship banks that were only treated in 1957. The sample in columns (1) and (2) includes all firms with available employment data. Since data on assets do not exist for this sample, I control for size using fixed effects for four bins of firm employment (0-49, 50-249, 250-999,

[18]The industries are agriculture & mining, food & drink, clothes & textiles, wooden products, chemicals & pharmaceuticals, rubber & glass, metals manufacturing, production of machinery, repair & research, energy supply, water & waste management, construction & real estate, trade & retail, transport, gastronomy & art, information & communication, and finance & insurance.

1000+). The sample in columns (3) and (4) use the more restrictive, "matched" sample. I do not use the zonal fixed effects in columns (3) and (4) because the matched sample identifies the effect using only cross-zonal variation.

The results in Table 5.20 present no evidence that the bank reforms affected employment growth, in either the full or the matched sample. For instance, the point estimate in column (2), using the full sample with all controls, implies employment growth at firms with a treated relationship bank was 0.1 percentage points lower per year. The 95 percent confidence interval excludes growth improvements above 0.7 percentage points. The point estimate in the matched sample in column (4) also implies an insignificant growth decrease of 0.1 percentage points and the 95 percent confidence interval rejects improvements above 1.2 percentage points. The similar coefficients in the full and the matched samples suggest no unobservable shocks are correlated with the treatment indicator in the full sample, strengthening the identification assumption.

Other papers studying the effects of banking shocks on firms report large point estimates compared to the coefficients reported in this paper so far. For instance, Liberti, Seru, and Vig (2016) find that the introduction of a credit registry in Argentina improved the efficiency of bank credit allocation, increasing lending to firms by 61 percent within two years. Bertrand, Schoar, and Thesmar (2007) study the effects of the 1980s deregulation of the French banking sector on bank-dependent firms, analyzing heterogeneity by firm profitability. More profitable firms (firms with a one standard deviation higher pre-reform return on assets) experienced a relative increase in the ratio of bank debt over total assets by 2.3 percentage points in the decade following the deregulation. There was a 23 percent increase in employment in bank-dependent industries relative to other industries (moving from the 25th to 75th percentile of the industry bank debt-to-assets ratio). Other papers study bank lending cuts. Due to the interbank liquidity freeze in 2007, the annual bank debt growth of the average Italian firm was 2.9 percentage points lower and employment growth was 0.5 percentage points lower from 2006 to 2010 (Cingano, Manaresi, and Sette 2016a). Spanish firms attached to weak banks experienced a reduction in the annual growth of bank debt by 1.3 percentage points and of employment by 0.7 percentage points from 2006 to 2010 (Bentolila, Jansen, Jiménez, and Ruano forthcoming). The large magnitude of the effects in the other studies, relative to the estimates of this paper, strengthens the conclusion that the postwar reforms had no economically significant impact on the growth of the average firm. The analysis of bank financial figures further below also supports this conclusion.

3.5.3 The Effect on the Growth of Opaque Firms

A theoretical disadvantage of big banks is that they may be worse at dealing with opaque firms, which requires collecting and processing soft information. The literature has traditionally used firm size as a proxy for opacity (Berger, Miller, Petersen, Rajan, and Stein 2005). Table 5.21 estimates the effect of having a relationship bank treated in 1952 on firm employment growth from 1951 to 1956, for different bins of firm size. The coefficients for the smallest firm size bins of 0-9, 10-19, 20-29, and 30-39 employees are all negative. While they are statistically insignificant due to the small sample sizes, they imply economically significant decreases in employment growth between 2.3 and 6.9 percentage points. The point estimates for the larger firms are of smaller magnitude and insignificant.

To create a more systematic classification of opaque firms, I identify three indicators for opacity: size, age, and asset tangibility. First, a literature argues that small firms face more idiosyncratic risk, have lower savings, and are difficult to assess for lenders. Studies typically use a cut-off of 50 employees to identify small firms (Gertler and Gilchrist 1994; Chodorow-Reich 2014a). Second, young firms are less likely to have an established reputation and paper trail to prove their creditworthiness. The literature usually defines young firms as firms under the age of 10 (Rajan and Zingales 1998; Hurst and Pugsley 2011). Third, technological differences across industries lead to variation in the share of assets that can be easily used as collateral. Firms with a low fraction of collateralizable assets are more likely to rely on their banks to use soft information, since it is difficult to unambiguously value and document their assets. Following Braun (2005) and Manova (2012), I use an industry measure of asset tangibility (industry average of fixed tangible assets over total assets) to identify firms with low collateral value. I classify firms as opaque if they have fewer than 50 employees, are younger than 10 years old in 1952, or are in the bottom ten percent by industry asset tangibility.

Table 5.22 restricts the sample to opaque firms. In columns (1) to (5), the various outcome variables measure growth from 1951 to 1960, so the regressor of interest indicates firms with relationship banks that were treated in 1952 or 1957. Column (1) reports that for opaque stock corporations with a treated relationship bank, the ratio of bank debt over assets fell by an annual average of 1.4 percentage points from 1951 to 1960. The effect is significantly different from zero at the 5 percent level. This suggests that opaque stock corporations suffered a decrease in their bank loan supply. Column (2) finds that the ratio of stock capital to assets increased by 0.6 percentage points for firms with a treated bank, although the effect is imprecisely estimated. The effect on the growth of total assets in column (3) implies a reduction of 1.1 percentage points, but the coefficient is statistically insignificant. This leaves open the possibility that stock corporations were not able to close all of the funding gap by issuing new stock capital. However, there was no effect on employment growth, as column (4) reports a point estimate of zero.

Opaque firms with few alternative sources of bank debt should have suffered the largest decrease in their bank loan supply. In line with this hypothesis, column (5) reports that opaque firms with higher intensive-margin dependence on the treated banks were more affected. There was a significant and economically large effect on the ratio of bank debt over assets on firms, for which more than half of relationship banks were treated. For firms where less than half were treated, the effect was smaller and statistically insignificant.

Columns (6) and (7) estimate the employment effects on opaque, non-stock firms. The outcome variables measure growth from 1951 to 1956, so the regressors of interest indicates whether firms had relationship banks that were treated in 1952. Column (6) shows that the employment growth of opaque firms was 2.9 percentage points lower, when more than half of relationship banks were treated. The coefficient is statistically significant at the 10 percent level. The effect remains of similar magnitude and significant when I use only the matched sample in column (7). The effect on firms, for which less than half of their relationship banks were treated, is negative, but smaller and insignificant in columns (6) and (7). These estimates suggest the employment outcomes of non-stock firms are more vulnerable to bank loan supply than stock corporations. A likely reason is that non-stock firms cannot fund themselves by issuing additional stock capital.[19]

In summary, the results in Table 5.22 indicate that opaque firms experienced decreased bank loan supply after the reforms, with negative consequences for the employment of opaque non-stock firms.[20]

3.5.4 Robustness Checks on the Growth of Firms

The robustness checks in Table 5.23 provide further evidence that firms with a treated relationship bank did not benefit from the reforms. Column (1) uses both stock and non-stock firms in the sample to test whether there was a pre-trend in employment growth from 1949 to 1951. The coefficient on firms with a treated bank is small, positive, and statistically insignificant. There is also no difference in the growth of firms with a bank treated in 1952 (i.e. with a treated bank not in NRW).

[19] I also examined age and asset tangibility separately. The effect of having a relationship bank treated in 1952 on employment growth from 1951 to 1956 is -0.020 (0.017) for firms under 10 years old and 0.001 (0.003) for firms at least 10 years old. The effect on firms in the bottom 10 percent by industry asset tangibility is -0.011 (0.011) and in the top 90 percent is 0.000 (0.004).

[20] Appendix Table 6.21 shows that non-opaque firms were not affected. The coefficients in the sample of non-opaque firms in columns (1) and (2) are all close to zero and insignificant. There were no heterogeneous effects by banking group, as the effects on opaque firms in columns (3) and (4) of Appendix Table 6.21 are negative and economically significant.

Columns (2) to (4) restrict the sample to firms that are particularly likely to benefit from shocks to the efficiency of their banks. Column (2) analyzes stock corporations with a high (above-median) ratio of bank debt over total liabilities in 1951. These firms particularly depend on bank debt for financing. Column (3) analyzes stock corporations with a low ratio of stock capital over total assets. These firms require more outside financing in general. Column (4) restricts the sample to firms that export some of their products, as reported in the 1952 *Grossunternehmen* volume for non-stock firms. Due to the high default risk and working capital requirements, many exporters rely on outside financing (Amiti and Weinstein 2011a). The coefficients in columns (2) to (4) are all small and statistically insignificant. If the reforms had any impact on the financial services offered by banks, the groups of bank-dependent firms in columns (2) to (4) should have been most strongly affected. The absence of a significant effect suggests that firms did not benefit from the reforms.

Columns (5) and (6) of Table 5.23 test the robustness by using different treatment variables. Column (5) shows that firms, for which more than half of their relationship banks were treated, did not experience faster employment growth. Column (6) shows that there was no heterogeneity in the treatment effect by whether the firm had a relationship bank belonging to the former Commerzbank, Deutsche, or Dresdner Bank. The coefficients in column (6) are all close to zero and insignificant.

Column (7) explores the possibility that the treated banks improved the growth of the relationship customers that they newly added after the reform of 1952. I create a dummy for whether a firm had no treated relationship bank in 1951, but had added a treated bank as relationship bank by 1956. An endogeneity problem arises in the interpretation of the coefficient on this dummy because firms that add new relationship banks are also likely to have higher loan demand. To correct for this, I restrict the sample to only firms that increased the number of their relationship banks from 1951 to 1956. The idea is to only compare firms with increased loan demand. Using this sample, the point estimate implies a 0.2 percentage point increase in growth, which is statistically and economically insignificant. The reforms did not improve the employment growth of their existing nor of their new relationship customers.

As additional robustness check, Appendix Table 6.22 uses the 1940 relationship banks to define the treatment indicators. 87 percent of firms with a treated relationship bank in 1940 still had a treated relationship bank in 1952. Given this stability, it is not surprising that the results remain unchanged. There is no differential growth before the reforms, non-opaque firms were unaffected by the reforms, and opaque firms grew more slowly after the reforms.

3.6 Results Using Bank Data

This section uses bank-level data to investigate the effects of the banking reforms on the treated banks. The findings confirm and supplement the firm-level results established in the previous section.

3.6.1 Financial Figures of Banks

Figure 5.24 uses data from the Deutsche Bundesbank. Panel A plots the lending stock to firms and households (non-banks) for two groups of banks. The treated group includes the sum of lending by all treated banks. The untreated group includes lending by the other commercial banks.[21] Before the first reform in 1952, the two lines evolved in parallel. This suggests the treated banks and their customers were not exposed to different shocks than the untreated banks, in line with the parallel-trends assumption. After the first reform, the loan growth of the treated banks slowed relative to the untreated group and continued to do so after the second reform. Panel B shows the growth of deposits by non-banks. Deposits by non-banks funded the majority of new bank lending (Ahrens 2007).[22] Accordingly, the relative growth pattern of non-bank deposits in Panel B mirrors that of lending. Deposits of the treated banks grew in parallel to the untreated group before the 1952 reform and more slowly thereafter.

Panel C examines interbank lending. The reforms in 1952 and 1957 forced a change in how the treated banks reported loans among each other. A cross-state loan among treated bank branches of the same pre-war banking group was an interbank loan before the reforms, and an internal loan after 1957. Hence, after the first reform, the treated banks reported a lower increase in interbank lending than the untreated group. The pattern for interbank deposits, shown in Panel D, was similar, as interbank deposits of the treated banks grew more slowly after the reforms.

One key aim of the treated banks in the postwar period was to increase their market share in lending and deposit-taking (Ahrens 2007). If the consolidations led to efficiency gains, the treated banks should have been able to increase lending and deposits relative to the other commercial banks, for example by offering more favorable interest rates. Figure 5.24 provides no evidence that the treated banks were able to do this.

Table 5.24 reports the growth of financial statistics from 1952 to 1960 for the treated banks, three comparison banks, and the mean difference between the treated and 9 untreated banks.[23] Panel A examines lending and profit growth. For both mea-

[21] As robustness check, Appendix Figure 6.10 uses all other banks as control group, including the not-for-profit credit unions and public banks. The relative growth of treated and control group is similar.

[22] The alternative source of funding is to issue new equity capital. Several changes to accounting regulations in the postwar period make it impossible to construct a consistent series for bank equity capital, and hence for bank leverage (Horstmann 1991; Ahrens 2007).

[23] Section 3.4.3 explains the selection of the years 1952 and 1960, the three comparison banks, and the

sures, the treated banks lie well below the three comparison banks. Commerzbank had the relatively strongest lending and profit growth among the treated banks, since it pursued an aggressive policy of branch expansion after 1952 (Ahrens 2007). Nonetheless, it grew more slowly than the three comparison banks. Column (7) reports the difference between the mean growth of the treated banks and the mean growth of 9 untreated, commercial banks. Lending by treated banks grew approximately 27.7 percentage points more slowly and profits approximately 5.7 percentage points more slowly. These findings confirm that the treated banks did not increase lending after the reforms and that they did not become more profitable.

Panel B analyzes the change in banks' cost efficiency. The ratio of non-interest expenses over total assets is a common measure of cost efficiency. Non-interest expenses include a variety of operating costs, including the cost of employees, office materials, and maintenance. If there are significant fixed costs to banking, as some theories suggest, the ratio should fall with bank size. The data show that the treated banks experienced lower improvements in the ratio, relative to the three comparison banks and also relative to all 9 untreated banks. To test the robustness of the result, I calculate two additional ratios: non-interest expenses scaled by revenue, a measure of the average cost required to earn one unit of revenue, and employee compensation scaled by total assets. The ratios of the treated banks fell by less than the ratios of the three comparison banks and the 9 untreated banks. The results suggest that the consolidations did not improve cost efficiency.

Panel C examines whether more firms had the treated banks as relationship banks after the reforms, relative to other banks. At the firm-level, I calculate the fraction of relationship banks that were part of a given banking group. The figures in Panel C report the average fraction of all firms in the dataset, by banking group and years. In general, the magnitude of changes in the average fraction is small for all banks. From 1951 to 1970, Deutsche Bank saw the strongest decrease at around 2.3 percentage points and Commerzbank the largest increase at around 2.9 percentage points. Overall, there is no evidence that the treated banks became more prevalent as relationship banks after the reforms.

The data on banking groups from the Deutsche Bundesbank and the bank-level financial statistics paint a consistent picture. There is no evidence that the treated banks grew faster or became more efficient after the reforms. These results are consistent with the firm-level evidence from the previous section, which found that firms with a treated relationship bank did not benefit from the reforms.

9 untreated banks. It also shows that the financial statistics of the treated, the three comparison banks, and the 9 untreated banks were similar in 1952.

3.6.2 Media Mentions of the Treated Banks

The results presented so far suggest the treated banks did not become more efficient after the reforms and that their relationship customers did not grow faster. So why were most treated bank managers in favor of reconsolidating? A literature on empire-building has suggested that managers benefit from running big firms, independent of whether big firms are more efficient (Jensen 1986; Stein 2003). One benefit of size may be that big banks and their managers are more present in the media. An empirical literature shows that media presence affects consumer choices, political opinions, and voting (Enikolopov and Petrova 2015; Bursztyn and Cantoni 2016). Furthermore, as argued by Zingales (2017), firms with high media presence may be able to influence politicians and regulators more effectively.

Table 5.25 examines the effect of the reforms on media presence. The data are from the archives of two influential publications, the German weekly magazine *Der Spiegel* and the British daily newspaper Financial Times. I calculate the number of times the name of a treated bank or the name of a treated bank executive were mentioned in articles in these publications, separately for three periods of equal length before, between, and after the reforms. I exclude articles from the count that directly report on the postwar banking reforms. Most counted articles either discuss the financial figures of the treated banks or cite the opinion of a bank executive on a particular political or economic issue.

The mentions of treated banks and executives increased strongly after both reforms. There were over 8 times as many mentions of a treated bank after the second reform than before the first reform in *Der Spiegel*, and over 3 times as many mentions of a treated bank executive. There is hardly any difference in the number of mentions of the word "bank" or "Deutschland" between the two periods, indicating that an increase in the number of articles about banks or Germany cannot explain the effect. Mentions of the banks and executives in the Financial Times increased by over 259 times and 71 times, respectively. Changes in the mentions of "bank" (1.7) and "Germany" (2.5) cannot explain this increase. These figures suggest that one bank of size 10 receives more mentions in the media than do 10 banks of size one combined. Hence, consolidations can raise the overall media presence of the involved organizations.

A simple explanation of the results is that the media only reports on firms whose actions can potentially affect a large number of readers. Banks operating at the state-level can affect only the population of one state. The actions of a national bank are potentially relevant to the entire nation. The consolidation of several state-level banks could move the resulting national bank beyond the threshold required for the media to mention it regularly. Independent of the explanation for the results, the causal effect of bank size on media presence found in this section could account for the desire of mangers to increase the size of their banks.

3.7 Results on the New Relationship Banks of Firms

This section tests whether there is heterogeneity in the types of firms that added the treated banks as new relationship banks. The dimensions of heterogeneity I examine are firm opacity and riskiness.

The outcome in this section is the fraction of a firm's relationship banks in 1970 that were treated in one of the reforms. This variable is preferable to a dummy because it takes into account that firms in the sample increased the average number of relationship banks from 2.8 to 3.5 from 1951 to 1970. The analyses test whether opaque and risky firms were more likely to add the treated banks as relationship banks, relative to the other, untreated banks. Since the establishment of new relationships takes time (Dwenger, Fossen, and Simmler 2015), I define the outcome variables using the 1970 relationship banks.

3.7.1 The New Relationship Banks of Opaque Firms

Opaque firms, as defined for the purpose of this section, had fewer than 50 employees in 1951 or were in the bottom ten percent by industry asset tangibility.[24] This definition differs from the previous one of Section 3.5.3 because it does not include firms younger than 10 years in 1952. By 1970, these firms were at least 18 years old, invalidating the argument that they are opaque because they could not have an established reputation and paper trail.[25]

Banking relationships in Germany rarely end. For instance, 94 percent of firms with a treated relationship bank in 1951 still had a treated relationship bank in 1970. Therefore, I begin by focusing on the establishment of new relationships, which is more common. To do so, I restrict the samples in columns (1) to (3) of Table 5.26 to only firms without a treated relationship bank in 1951. The point estimate in column (1) of Table 5.26 implies that the fraction of treated relationship banks was 5.6 percentage points lower among opaque firms in 1970, compared to non-opaque firms. The point estimate is statistically significant at the 1 percent level. Column (2) splits the treatment indicator into four subcategories, for firms with fewer than 20 employees, between 20 and 49 employees, asset tangibility below 0.15, and from 0.15 to 0.2. All four coefficients are negative, indicating that all dimensions of opacity were relevant.

[24]The use of pre-reform size to define opacity ensures that opacity is not endogenous to the causal effects of the reform. For instance, the addition of a treated relationship bank could have restricted firm employment growth for some opaque firms, keeping these firms under 50 employees. An unreported robustness check using firms with fewer than 50 employees in 1970 confirms the results found below.

[25]In an unreported robustness check, I find that firms founded after 1965 had a lower fraction of treated relationship banks in 1970, confirming the results below that opaque firms were less likely to add treated relationship banks.

Column (3) adds industry and zonal fixed effects to the specification. The coefficient remains robust. This implies the effect cannot be explained by the treated banks specializing in certain industries and zones.

The result in column (4) reveals there was no pre-trend. I restrict the sample to firms with no treated relationship bank in 1940 or firms founded after 1940. The coefficient on opaque firms is close to zero and insignificant. Apart from the war, the period from 1940 to 1951 includes the first phase of the breakup. The result implies that during this period, the fraction of treated relationship banks among opaque firms did not grow more slowly than at non-opaque firms.

The analysis so far has focused on the establishment of new banking relationships, by restricting the samples to firms without a treated relationship bank in 1951. Columns (5) and (6) instead restrict the sample to firms with a treated relationship bank in 1951. Column (5) uses the 1970 fraction of treated relationship banks as outcome, while column (6) uses the 1951 fraction. The coefficient on opaque firms estimates the difference in the fraction of relationship banks that were treated between opaque and non-opaque firms, conditional on the firm having a relationship to a treated bank. The point estimates in columns (5) and (6) are identical. Both are statistically not different from zero. This suggests there was no differential change in the fraction of treated relationship banks from 1951 to 1970 between opaque and non-opaque relationship customers of the treated banks. This conclusion implies that the reforms did not affect the banking relationships of existing customers of the treated banks. The finding suggests that opaque firms found it difficult to switch banks even when they faced reduced bank loan supply after the reforms.

3.7.2 The New Relationship Banks of Risky Firms

Theories of moral hazard and bank-internal agency problems suggest that big banks may be more willing to lend to risky firms. Table 5.27 examines whether risky firms were more likely to increase the fraction of treated relationship banks following the reforms. The first measure of firm risk is the ratio of stock capital over total assets in 1951. This ratio is a measure of funding stability and risk absorption capacity. The higher the ratio, the less likely that the firm will become bankrupt or default on its loans.

As before in the analysis of opaque firms, I begin by studying the establishment of new relationships. The sample in column (1) includes only firms without a treated relationship bank in 1951. The specification contains dummies for four quarterly bins of the ratio. I control for a dummy for opaque firms, to ensure the results cannot be explained by the effects of opacity. The estimates show no significant difference in the fraction of treated relationship banks in 1970 for firms with a ratio below 0.75. However, the coefficient on firms in the highest category, with a ratio above 0.75, is negative and statistically significant at the 10 percent level. It implies that the fraction of treated relationship banks was 8.7 percentage points lower for firms in the top quarter of the capital-to-assets ratio, compared to firms in the lowest quarter. This result suggests that low-risk firms were less likely than medium- and high-risk firms to establish new relationships with the treated banks. Column (2) adds zonal and industry fixed effects to the specification. The coefficient on the highest bin grows more negative and remains significant. This suggests that the increase in risky relationship customers of the treated banks took place within zones and industries.

The third column restricts the sample to firms with a treated relationship bank in 1951. The coefficients on the bins of the ratio are all positive and increase with the ratio. This implies that before the reforms, firms with high capital-to-assets ratio had a higher fraction of treated relationship banks, conditional on having a treated relationship bank. Column (4) reveals that these findings still held in 1970. The point estimates in column (4) are close in magnitude to the estimates in column (3) and lie well within their 95 percent confidence intervals. These results confirm that the reforms did not affect the banking relationships of existing customers of the treated banks, consistent with the results on opaque firms above. The decrease in exposure to low-risk firms came through the selective formation of new relationships, rather than the selective continuation of existing relationships.

Column (5) tests the robustness of the finding by using the volatility of employment growth as measure of risk. I calculate the standard deviation of the annual employment growth rates from 1949 to 1951. Firms in the top half of the distribution are called "volatile employment" firms. The sample in column (5) includes only firms with no treated relationship bank in 1951. The point estimate implies that firms with volatile employment increased the fraction of treated relationship banks by a statistically significant 5.8 percentage points, relative to other firms. The regressor of interest in column (6) is a dummy for "volatile revenue" firms, calculated the same way as volatile employment above. The coefficient is positive, but imprecisely estimated. The analysis in column (6) adds new information based on new firms, because only 13 percent of firms used in column (6) are also in the sample of column (5).

One might wonder whether by moving away from opaque and towards risky firms, the treated banks started lending to more productive firms. Column (7) test this hypothesis. The coefficient on a dummy for firms in the top half of the distribution of revenue per worker is negative and insignificant, suggesting there is no evidence for a move towards productive firms.

Summary statistics on the relationship customers of the treated banks confirm that the treated banks became more exposed to risky firms and less exposed to opaque firms. The fraction of firms with high capital-to-assets ratio and the fraction of opaque firms among the relationship customers of the treated banks decreased, while the fractions among the untreated banks increased.[26]

3.8 Results on Municipalities

The final step of the empirical analysis studies the effect of the reforms at a higher level of economic aggregation, on municipal employment growth. The firm-level analysis revealed that firms with a treated relationship bank did not benefit from the reforms and that opaque firms suffered. The municipal-level analysis includes other potential channels of the reforms that the firm-level analysis could not directly capture, including for example local general equilibrium effects or the effects on households.

The specifications regress municipal employment growth on measures of dependence on the treated banks. The first measure is whether the municipality had a treated bank branch in 1952. The coefficient in column (1) of table 5.28 implies that the employment growth of municipalities with a treated bank branch was 11.7 percentage points lower from 1951 to 1960. The effect is statistically significant at the 5 percent level. Column (2) adds fixed effects for federal states, five quantiles of total employment, and the Ruhr area. The coefficient remains stable and significant. Column (3) uses a different regressor, the average fraction of treated relationship banks for firms in the municipality, calculated using the Hoppenstedt firm data. The point estimate implies that in a municipality served exclusively by the treated banks, employment growth was 28.5 percentage points lower from 1951 to 1960. The coefficient is significant at the 1 percent level.

The outcome in column (4) is the employment growth rate from 1951 to 1956. In this period, only the treated banks outside NRW were affected by the first reform of 1952. The coefficient for municipalities with one of these branches implies a 6.2 percentage point decrease in the employment growth rate. The effect is significant at the 10 percent level. The coefficient on municipalities with treated bank branches in NRW is less than one-third of the magnitude and insignificant. Column (5) reports a positive and insignificant correlation between growth from 1949 to 1951 and a dummy for municipalities with a treated bank branch, suggesting there was no negative pre-trend before the reform. Column (6) performs a robustness check using the growth rate from 1951 to 1960 as outcome. The specification includes the full interaction of zonal fixed effects with the following controls: the employment growth rate from 1949 to 1951, five quantiles of total employment, the share of employment in manufacturing, the share of employment in the primary sector, and the employment share of war-time dis-

[26] Another valid test of whether the treated banks took more risks would be to analyze the growth of bank debt at risky firms with a treated relationship bank. Small sample sizes do not permit such a test. For instance, a regression akin to column (1) of table 5.22, using only volatile employment firms in the sample, has only 5 observations.

placed. The coefficient is of similar magnitude to the one in the baseline specification of column (1) and significant at the 10 percent level.

The evidence points towards significant employment losses for municipalities that were more exposed to the treated banks. The small sample sizes in the specifications, ranging from 72 to 91 municipalities, suggest caution is warranted in interpreting the municipality-level results. Nonetheless, the evidence is consistent with the firm- and bank-level results, providing no evidence of a positive employment effect from the banking reforms.

3.9 Conclusion

This paper studies the effects of two Allied banking reforms in postwar Germany. The reforms permitted treated state-level banks, formerly belonging to three national institutions, to reconsolidate into national banks. Firms with a treated relationship bank did not use more bank debt and did not grow faster after the reforms. The treated banks did not increase total lending, were not able to attract more deposits, and did not achieve lower cost efficiency ratios, relative to comparable, untreated banks. The results are inconsistent with theories that argue the real economy benefits from increases in bank size.

The evidence supports theories that suggests there are real costs to increases in bank size. Opaque (small, young, low-collateral) relationship customers of the treated banks experienced lower employment growth after the reforms. Other opaque firms were less likely to establish new banking relationships with the treated banks. These findings are in line with theories that suggest big banks are worse at processing soft information and dealing with opaque firms. Treated banks were more likely to establish new relationships with risky firms after the reforms, which is consistent with theories emphasizing moral hazard or internal agency problems in big banks. Consistent with the firm-level results, the employment growth of municipalities with a treated bank branch did not improve after the reforms. Taken together, the results of this paper find no evidence that increases in bank size benefit real economic growth. The results throw into question the standard arguments against size-dependent banking regulation.

Chapter 4

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Chapter 5

Tables and Figures

5.1 Tables

Table 5.1: Summary statistics for the firm panel

	mean	sd	p5	p50	p95
Firm CB dep	0.16	0.23	0	0	0.50
No of relationship banks	3	1.54	1	3	6
Employment	913.71	11,592.54	19	132	2,030
Wage	32.04	47.15	15.51	29.46	46.37
Capital	57,711.61	544,582.57	225.75	5,467.81	196,539.06
Liabilities	152,628.46	3,657,557.10	1,552.79	8,848.93	213,144.20
Export share	11.02	21.31	0	0	64
Import share	5.24	16.73	0	0	40
Age	47.60	45.90	13	31	126
Bank debt/liabilities	0.48	0.26	0.05	0.49	0.90
Liabilities/assets	0.66	0.21	0.26	0.68	0.98
Firms	2,011				

Notes: The data are from the firm panel for the year 2006. Monetary values are in year 2000 thousands of Euro. Capital is the book value of fixed tangible assets. The wage is the total wage bill divided by the number of employees. The export share is the percentage of exports out of total revenue, and the import share is the percentage of imports out of total costs.

Table 5.2: Summary statistics for the county dataset

	mean	sd	p5	p50	p95
County CB dep	0.12	0.06	0.04	0.11	0.23
2000 GDP (in year 2010 bn Euro)	6.01	9.12	1.46	3.63	14.31
2000 Population (in 1000s)	203.28	229.39	52.68	147.12	487.13
2000 Employment (in 1000s)	98.27	126.49	29.90	64.50	220.40
Former GDR	0.16	0.37	0	0	1
Landesbank in crisis	0.67	0.47	0	1	1
Distance instrument	-1.63	0.97	-3.43	-1.51	-0.28
GDP Growth 2008-12	2.66	6.18	-7.25	2.73	11.76
Employment Growth 2008-12	2.79	3.22	-1.98	2.77	7.21
Observations	385				

Notes: The data are from the Federal Statistical Office of Germany. The distance instrument is the negative of the county's distance to the closest postwar Commerzbank head office, in 100 kilometers. Landesbank in crisis is a dummy for whether the county's Landesbank suffered losses in the financial crisis (Puri, Rocholl, and Steffen 2011). Growth rates are in percent.

Table 5.3: Firm survey on banks' willingness to grant loans

	(1)	(2)	(3)	(4)	(5)	(6)
YEAR	2007	2008	2009	2010	2011	2012
Firm CB dep	-0.111	-0.095	-0.473	-0.316	0.059	0.379
	(0.157)	(0.140)	(0.190)	(0.182)	(0.197)	(0.184)
Dep var from 2006	0.631	0.522	0.380	0.365	0.335	0.206
	(0.041)	(0.047)	(0.051)	(0.055)	(0.055)	(0.050)
Observations	856	988	1,032	946	898	503
R^2	0.460	0.371	0.204	0.213	0.207	0.199
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Size Bin FE	Yes	Yes	Yes	Yes	Yes	Yes
ln age	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from cross-sectional firm regressions for different years. The outcome variable is the answer to the question: "How do you evaluate the current willingness of banks to grant loans to businesses: cooperative (coded as 1), normal (0), or restrictive (-1)?" It is standardized to have zero mean and unit variance. The coefficients are interpreted as the standard deviation increase in banks' willingness to grant loans from increasing Commerzbank dependence by one. The control variables include fixed effects for 36 industries, 16 federal states, 4 size bins (1-49, 50-249, 250-999, and over 1000 employees in the year 2006), and the ln of firm age. Standard errors are clustered at the level of the county.

Table 5.4: Firm bank loans and Commerzbank dependence

	(1)	(2)	(3)
Firm CB dep*d	-0.101	-0.166	-0.205
	(0.079)	(0.080)	(0.078)
Observations	12,066	12,066	12,066
R^2	0.009	0.078	0.094
Number of firms	2,011	2,011	2,011
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
County FE*d	No	Yes	Yes
ln age*d	No	Yes	Yes
Size Bin FE*d	No	Yes	Yes
Industry FE*d	No	No	Yes
Import and Export Share*d	No	No	Yes

Notes: This table reports estimates from firm OLS panel regressions. The outcome in all columns is firm ln bank loans. Firm CB dep is the fraction of the firm's relationship banks that were Commerzbank branches in 2006. d is a dummy for the years following the lending cut, 2009 to 2012. The following time-invariant control variables are calculated for the year 2006 and interacted with d: fixed effects for 70 industries, 357 counties, and 4 firm size bins (1-49, 50-249, 250-999, and over 1000 employees); the ln of firm age; the export share (fraction of exports out of total revenue); and the import share (fraction of imports out of total costs). The data include the years 2007 to 2012. R^2 is the within-firm R^2. Standard errors are two-way clustered at the level of the county and the industry.

Table 5.5: Household debt and county Commerzbank dependence

OUTCOME	(1) Total debt growth 2007-12	(2) Total debt growth 2007-12	(3) Total debt growth 2007-12	(4) Debtor 2008	(5) Debtor 2009	(6) Debtor 2010	(7) Debtor 2011	(8) Debtor 2012
County CB dep	0.107	0.007	0.112	0.027	0.080	-0.052	0.050	0.104
	(0.234)	(0.272)	(0.300)	(0.126)	(0.124)	(0.114)	(0.131)	(0.165)
Ln mortgage debt 2002	-0.042	-0.042	-0.036	0.018	0.018	0.015	0.015	0.011
‘	(0.003)	(0.005)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Ln other debt 2002	-0.006	-0.006	-0.005	0.008	0.008	0.008	0.009	0.010
	(0.006)	(0.006)	(0.006)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Debtor in 2002 FE	-0.028	-0.028	-0.088	0.278	0.253	0.231	0.208	0.202
	(0.039)	(0.039)	(0.040)	(0.015)	(0.015)	(0.017)	(0.016)	(0.018)
Observations	6,423	6,423	6,423	10,829	9,992	9,206	8,520	7,409
R^2	0.048	0.053	0.069	0.395	0.399	0.404	0.289	0.288
County controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from cross-sectional OLS regressions using data on individuals over 16 years of age from the GSOEP. The outcome in columns (1) to (3) is the symmetric growth rate of total debt from 2007 to 2012. If an individual has no debt in either year, the growth rate is set to zero. The outcomes in columns (4) to (8) are dummy variables for any outstanding debt in the given year. The mean value of the outcome in 2007 is 0.4. To avoid dropping observations with zero debt in 2002 from the sample, I add one Euro to the 2002 debt levels before transforming them into the ln control variables. The county controls include 17 industry shares, population density, population (in ln), GDP per capita (in ln), and the Schufa 2003 debt index, as described in Table 5.8. The individual controls are all measured in 2007. They include dummies for sex, the individual's employment status (unemployed, full-time, part-time, not in labor force), the employment status of household members (at least one full-time employed, at least one part-time employed, none employed), the former GDR, the number of children in the household, the number of adults in the household, the number of years in education of the most-educated household member (<10, 10, 11, 12, 13, >13), ten dummies for the deciles of the age distribution, and ten dummies for the deciles of the household income distribution. Standard errors are clustered at the level of the county.

Table 5.6: Firm employment and Commerzbank dependence

	(1)	(2)	(3)	(4)	(5)
Firm CB dep*d	-0.044	-0.047	-0.053		
	(0.021)	(0.016)	(0.015)		
Low bank debt dep*Firm CB dep*d				-0.035	
				(0.032)	
High bank debt dep*Firm CB dep*d				-0.071	
				(0.020)	
(0 < Firm CB dep $\leq$ 0.25)*d					0.007
					(0.016)
(0.25 < Firm CB dep $\leq$ 0.5)*d					-0.017
					(0.008)
(0.5 < Firm CB dep $\leq$ 1)*d					-0.065
					(0.018)
Observations	12,066	12,066	12,066	12,066	12,066
R^2	0.026	0.098	0.124	0.125	0.125
Number of firms	2,011	2,011	2,011	2,011	2,011
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
County FE*d	No	Yes	Yes	Yes	Yes
Size Bin FE*d	No	Yes	Yes	Yes	Yes
ln age*d	No	Yes	Yes	Yes	Yes
Industry FE*d	No	No	Yes	Yes	Yes
Import and Export Share*d	No	No	Yes	Yes	Yes

Notes: This table reports estimates from firm OLS panel regressions. The outcome in all columns is firm ln employment. Firms with low (high) bank debt dependence have up to (over) 50 percent of their liabilities with banks. The control variables, the standard error calculations, the years covered by the data, and the definition of R^2 are explained in Table 5.4.

Table 5.7: Further firm outcomes and Commerzbank dependence

	(1)	(2)	(3)	(4)	(5)	(6)
OUTCOME	Capital	Val add	Val add/capital	Val add/empl	Wage	Int rate
Firm CB dep*d	-0.130	-0.061	0.069	-0.008	0.001	-0.003
	(0.038)	(0.028)	(0.038)	(0.024)	(0.011)	(0.003)
Observations	12,066	12,066	12,066	12,066	12,066	12,024
R^2	0.131	0.116	0.116	0.091	0.069	0.073
Number of firms	2,011	2,011	2,011	2,011	2,011	2,004
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls*d	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from firm OLS panel regressions. The respective outcome is given in the column title. Capital is the ln book value of fixed tangible assets. Value added (val add) is the ln of revenue minus expenditure on intermediates. Value added per worker is ln(val add/empl) and per unit of capital is ln(val add/cap). The wage is the ln of the wage bill divided by the number of employees. The interest rate is the interest paid over total liabilities. The control variables, the standard error calculations, the years covered by the data, and the definition of R^2 are explained in Table 5.4.

Table 5.8: County outcomes and Commerzbank dependence (OLS)

OUTCOME	(1) GDP	(2) GDP	(3) GDP	(4) Empl	(5) Net migr
County CB dep*d	-0.132	-0.165	-0.141	-0.138	0.003
	(0.063)	(0.066)	(0.077)	(0.042)	(0.006)
Observations	5,005	5,005	5,005	5,005	1,925
R^2	0.301	0.341	0.350	0.494	0.592
Number of counties	385	385	385	385	385
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Former GDR FE*d	No	Yes	Yes	Yes	Yes
Industry Shares*d	No	Yes	Yes	Yes	Yes
Export and Import Shares*d	No	Yes	Yes	Yes	Yes
Landesbank in crisis*d	No	Yes	Yes	Yes	Yes
Population*d	No	No	Yes	No	No
Pop density*d	No	No	Yes	No	No
GDP per capita*d	No	No	Yes	No	No
Debt Index*d	No	No	Yes	No	No
Estimator	OLS	OLS	OLS	OLS	OLS

Notes: This table reports estimates from county OLS panel regressions of county outcomes on Commerzbank dependence (CB dep) interacted with d, a dummy for the years following the lending cut, 2009 to 2012. The outcome in columns (1) to (3) is ln GDP, in column (4) ln employment, and in column (5) net migration (immigration - out-migration) normalized by 2006 employment. The industry shares are 17 variables, giving the fraction of firms in each of the 17 industries in 2006 (agriculture, mining, manufacturing, utilities, recycling, construction, retail trade and vehicle repairs, transportation and storage, hospitality, information, finance, real estate, business services, other services, public sector, education, health). The export share is the fraction of exports out of total revenue and the import share is the fraction of imports out of total costs, both averaged across firms in the county for 2006. Landesbank in crisis is a dummy for whether the county's Landesbank suffered losses in the financial crisis. Population density, total population (in ln) and GDP per capita (in ln) are from 2000. Debt index is a 2003 measure of county household leverage, calculated by credit rating agency Schufa (Privatverschuldungsindex). The regressions are weighted by year 2000 population. Standard errors are clustered at the level of 42 quantiles of the county's industrial production share (GDP share of mining, manufacturing, utilities, recycling, construction). The GDP and employment data include the years 2000 to 2012. Migration data for all counties are only available for the years 2008 to 2012. R^2 is the within-county R^2.

Table 5.9: County outcomes and Commerzbank dependence (IV)

OUTCOME	(1) CB dep	(2) CB dep	(3) GDP	(4) GDP	(5) GDP	(6) Empl	(7) Net migr
Distance instrument*d	0.028	0.042					
	(0.005)	(0.006)					
County CB dep*d			-0.335	-0.367	-0.345	-0.208	0.026
			(0.118)	(0.182)	(0.173)	(0.113)	(0.020)
Observations	5,005	5,005	5,005	5,005	5,005	5,005	1,925
R^2	0.876	0.941	0.322	0.348	0.355	0.504	0.590
Number of counties	385	385	385	385	385	385	385
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Former GDR FE*d	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Distances*d	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry Shares*d	No	Yes	No	Yes	Yes	Yes	Yes
Export and Import Shares*d	No	Yes	No	Yes	Yes	Yes	Yes
Landesbank in crisis*d	No	Yes	No	Yes	Yes	Yes	Yes
Population*d	No	Yes	No	No	Yes	No	No
Pop density*d	No	Yes	No	No	Yes	No	No
GDP per capita*d	No	Yes	No	No	Yes	No	No
Debt Index*d	No	Yes	No	No	Yes	No	No
Estimator	OLS	OLS	IV	IV	IV	IV	IV

Notes: This table reports estimates from county panel regressions. Columns (1) and (2) report the first stage and columns (3) to (7) the IV regressions. The distance instrument is the negative of the county's distance to the closest postwar Commerzbank head office, in 100 kilometers. The linear distances include the county's distances to Düsseldorf, Frankfurt, Hamburg, Berlin, and Dresden. The outcomes, other control variables, weights, standard error calculations, the years covered by the data, and the definition of R^2 are explained in Table 5.8.

Table 5.10: The direct and indirect effects on firm employment growth

	(1)	(2)
Firm CB dep	-0.030	-0.036
	(0.009)	(0.009)
CB dep of other firms in county	-0.166	-0.170
	(0.076)	(0.082)
Observations	48,101	48,101
R^2	0.012	0.017
Firm Controls	Yes	Yes
County Controls	No	Yes

Notes: This table reports estimates from cross-sectional firm OLS regressions. The outcome is the symmetric growth rate of firm employment from 2008 to 2012. CB dep of other firms in county is the average firm Commerzbank dependence of all the other firms in the county. The firm control variables are the same as in Table 5.4, except there are no county fixed effects. The county controls and the standard error calculations are the same as in Table 5.8.

Table 5.11: The implied county employment change based on different estimates

Row	Estimate from section	Estimator	Dataset	Estimated effect	Point estimate	95 percent CI Lower	Upper
1	1.5.2	OLS	Firm Panel	Only Direct	-0.32	-0.49	-0.14
2	1.6.2	OLS	County Panel	Direct & Indirect	-0.83	-1.31	-0.34
3	1.6.3	IV	County Panel	Direct & Indirect	-1.25	-2.58	-0.09
4	1.7.1	OLS	Firm Cross-section	Direct & Indirect	-1.24	-2.17	-0.29

Notes: This table reports different estimates of the county employment loss from increasing county Commerzbank dependence by a standard deviation (6 percentage points). Row 1 uses the estimate of the direct effect from column (3) of Table 5.6. Row 2 uses the county OLS estimate from Table 5.8, column (4). Row 3 uses the county IV estimate from Table 5.9, column (6). Row 4 uses the sum of direct and indirect effects from column (2) of Table 5.10.

Table 5.12: Firm patents and Commerzbank dependence

OUTCOME	(1) Growth rate of patents	(2) Patents post lending cut	(3) Patents pre lending cut
Patenting*Firm CB dep	-0.548	-0.770	0.206
	(0.245)	(0.409)	(0.409)
Non-patenting*Firm CB dep	0.037		
	(0.065)		
Ln Patents 1990-2004		0.671	0.687
		(0.088)	(0.116)
Observations	2,011	382	382
R^2	0.251		
ln age	Yes	Yes	Yes
Size Bin FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
County FE	Yes	No	No
State FE	No	Yes	Yes
Import and Export Share	Yes	Yes	Yes
Only patenting firms in sample	No	Yes	Yes
Estimator	OLS	Neg bin	Neg bin

Notes: A patenting firm is defined as a firm that has produced at least one patent from 1990 to 2004. The outcome in column (1) is the symmetric growth rate of the number of patents between the periods before (2005-08) and after Commerzbank's lending cut (2009-12). If a firm produces no patents in either period, the growth rate is set to zero. The control variables and the standard error calculations in column (1) are the same as in Table 5.4. Standard errors in columns (2) and (3) are clustered at the level of the industry.

Table 5.13: Descriptive Statistics

	(1)	(2)	(3)
Panel A: Buyers vs Refinancers			
	Buyers	**All Refinancers**	**Refinancers in our Estimation Sample**
Age	36.47	42.08	40.85
	(10.13)	(9.77)	(8.90)
Couple	0.52	0.54	0.54
	(0.50)	(0.50)	(0.50)
Income	55,282.17	55,949.83	57,602.96
	(556,583.42)	(145,816.42)	(81,440.65)
Income Change (logs)		0.08	0.08
		(0.36)	(0.35)
Interest Rate	4.39	4.51	3.98
	(1.40)	(1.40)	(1.50)
House Price	229,375.32	248,328.76	256,517.10
	(326,209.46)	(361,735.65)	(187,020.25)
LTV	70.72	56.53	61.50
	(21.67)	(21.80)	(18.96)
Observations	7,119,807	5,935,441	1,384,346
Panel B: Refinancers in our Estimation Sample			
	Refinance On-Time	**Refinance Off-Time**	**Missing Duration**
Age	39.77	41.58	41.37
	(8.69)	(8.79)	(9.04)
Couple	0.55	0.53	0.54
	(0.50)	(0.50)	(0.50)
Income	54,516.32	53,442.66	62,005.61
	(48,424.02)	(52,355.65)	(108,733.95)
Income Change (logs)	0.08	0.11	0.07
	(0.31)	(0.38)	(0.37)
Interest Rate	4.22	3.60	3.97
	(1.51)	(1.33)	(1.53)
House Price	245,030.89	233,110.00	276,638.16
	(163,127.94)	(158,358.87)	(213,289.69)
LTV	61.56	63.04	60.72
	(18.30)	(19.27)	(19.27)
Observations	483,852	288,578	611,916

Notes: The table reports means and standard deviations (in parentheses) for different samples. Panel A compares descriptive statistics for home buyers (column 1), all refinancers (column 2), and refinancers in our estimation sample (column 3). Our estimation sample includes homeowners who we observe refinancing at least twice, and for whom we have sufficient information to precisely measure equity extraction. Panel B compares descriptive statistics for three subsamples of the refinancers in our estimation sample: households who refinance on-time (between two months before and six months after the onset of their reset rate), households who refinance off-time, and households where we do not observe the exact onset of the reset rate.

Table 5.14: Equity Extraction Elasticities by Refinance Timing

	(1)	(2)	(3)	(4)
Panel A: Full Sample				
Equity Extraction Elasticity	0.234	0.208	0.204	0.197
	(0.002)	(0.005)	(0.006)	(0.006)
Observations	1,384,346	1,384,346	1,311,734	1,173,626
Panel B: On-Time Sample				
Equity Extraction Elasticity	0.245	0.183	0.175	0.166
	(0.002)	(0.006)	(0.007)	(0.007)
Observations	483,852	483,852	460,077	459,571
Panel C: Off-Time Sample				
Equity Extraction Elasticity	0.317	0.269	0.263	0.252
	(0.004)	(0.011)	(0.012)	(0.013)
Observations	288,578	288,578	274,600	273,727
Panel D: Sample With Missing Durations				
Equity Extraction Elasticity	0.188	0.201	0.202	0.197
	(0.003)	(0.007)	(0.007)	(0.009)
Observations	611,916	611,916	577,057	440,328
Control Variables:				
Month FE		×	×	×
Household FE		×	×	×
County x Year FE			×	×
Household Controls				×

Notes: The table reports estimates of the equity extraction elasticity across different specifications and samples. Panel A considers the full sample (summarizing the results of the preceding figures), panel B considers the sample of on-time refinancers (defined as those who refinance between 2 months before and 6 months after reset rate onset), panel C considers the sample of off-time refinancers (defined as those who refinance more than 2 months before or more than 6 months after reset rate onset), and panel D considers the sample of refinancers with missing duration information. Standard errors are clustered by household and shown in parentheses. The household controls included in column (4) are income level, income growth, the last mortgage interest rate, the age of the borrower, a dummy for couples, and dummies for a range of self-reported reasons for the current and the last refinance (pure refinance / home improvement / debt consolidation / other).

Table 5.15: Equity Extraction Elasticities Using Instrumental Variables

	(1)	(2)	(3)	(4)	(5)
IV Equity Extraction Elasticity	0.150	0.163	0.284	0.295	0.283
	(0.004)	(0.004)	(0.026)	(0.056)	(0.056)
Observations	772,430	772,430	772,430	737,168	733,614
Control Variables:					
Contract Duration FE		×	×	×	×
Month FE			×	×	×
Household FE			×	×	×
County x Year FE				×	×
Household Controls					×

Notes: The table reports estimates of the equity extraction elasticity using instrumental variables. The instruments are interactions of dummies for the last mortgage contract duration (time until reset), year and region. The table shows IV elasticities from five different specifications, with the richest specification in column (5) corresponding to equations (2.5)-(2.6). The household controls included in column (5) are income level, income growth, the last mortgage interest rate, the age of the borrower, a dummy for couples, and dummies for a range of self-reported reasons for the current and the last refinance (pure refinance / home improvement / debt consolidation / other). Standard errors are clustered by household and given in parentheses. The IV elasticities with fixed effects (equal to 0.28-0.29) are slightly higher than the OLS elasticities, but overall the table confirms the qualitative results.

Table 5.16: Firm summary statistics for 1951

	Observations	Mean	Std. Dev.	p10	p50	p90
Panel A: Stock corporations						
Employment	1,251	1,625	5,488	23	354	3,405
Age	2,182	67	52	26	57	111
Assets	1,948	23.1	132.9	0.6	3.9	37.8
Stock capital / assets	1,872	0.37	0.20	0.14	0.34	0.63
Bank debt / assets	1,208	0.10	0.11	0	0.06	0.23
Number of relationship banks	2,188	3.18	2.08	1	3	6
Relationship bank treated in 1952/57	2,188	0.68	0.47	0	1	1
Relationship bank treated in 1952	2,188	0.46	0.50	0	0	1
Bank debt growth 1951-60	421	0.01	0.15	-0.22	0.03	0.21
$\frac{100 \cdot Bank\,debt}{Assets}$ difference 1951-60	421	-0.11	1.39	-1.77	-0.11	1.79
Employment growth 1951-60	815	0.03	0.05	-0.03	0.03	0.09
Revenue per worker growth 1951-60	344	0.05	0.05	0.00	0.04	0.10
Panel B: Non-stock firms						
Employment	1,800	559	1121	91	344	1,017
Age	3,494	63	51	16	54	112
Exporter	2,593	0.39	0.49	0	0	1
Number of relationship banks	3,706	2.54	1.29	1	2	4
Relationship bank treated in 1952/57	3,706	0.69	0.46	0	1	1
Relationship bank treated in 1952	3,706	0.41	0.49	0	0	1
Employment growth 1951-56	1,521	0.04	0.07	-0.01	0.03	0.13

Notes: The data are for the year 1951 for firms from the Hoppenstedt volumes 1952, 1952/53, 1958/59, and 1961/62, as described in Section 1.3. Assets are in million Deutsche Mark. Growth is the average annual symmetric growth rate, i.e. the symmetric growth rate over the entire period divided by the number of years in the period. The 1951-60 difference in $\frac{100 \cdot Bank\,debt}{Assets}$ is the change in the percent ratio of bank debt over assets, divided by 9, the number of years between 1951 and 1960. Relationship bank treated in 1952/57 is a dummy for whether the firm had a bank treated in 1952 or 1957 among its relationship banks in 1951. Relationship bank treated in 1952 is a dummy for whether a 1951 relationship bank was treated in 1952. Exporter is a dummy for whether the firm exported any of its products.

Table 5.17: Firms with a treated relationship bank and firm observables in 1951

	(1)	(2)	(3)	(4)	(5)	(6)
					Rel. bank	
Outcome			Rel. bank treated in 1952/57		treated in 1952	
Employment	0.063	0.047	0.061	0.068	-0.001	0.005
	(0.008)	(0.024)	(0.009)	(0.012)	(0.017)	(0.021)
Age	0.055	0.099	0.038	0.043	0.016	-0.032
	(0.023)	(0.037)	(0.011)	(0.013)	(0.042)	(0.025)
Assets		0.024				
		(0.026)				
Stock capital / assets		0.007				
		(0.043)				
Bank debt / assets		0.000				
		(0.014)				
Exporter				-0.013		
				(0.023)		
Observations	1,170	480	2,226	1,675	279	501
R^2	0.070	0.079	0.026	0.026	0.001	0.003
Sample	All	All	All	All	Matched	Matched
Firm type	Stock	Stock	Non-stock	Non-stock	Stock	Non-stock

Notes: The data are for the year 1951. The outcome in columns (1) to (4) is a dummy for whether whether the firm had a bank treated in 1952 or 1957 among its relationship banks in 1951. In columns (5) and (6), the outcome is a dummy for whether a 1951 relationship bank was treated in 1952. All regressors are in natural logarithms. Standard errors are robust.

Table 5.18: Summary statistics by banking group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Assets in 1952 / no of banks in			Cost ratios in 1952 (in %)		
Banking group	Treated	1947/48-52	1952-57	1957-	$\frac{Non-int\ cost}{Assets}$	$\frac{Non-int\ cost}{Revenue}$	$\frac{Empl\ comp}{Assets}$
Deutsche Bank	Yes	449	1,496	4,488	2.89	62.82	2.27
Dresdner Bank	Yes	298	1,091	3,273	2.64	74.77	1.93
Commerzbank	Yes	213	638	1,915	2.85	72.47	2.09
Bay. Hyp.- & Wechsel-Bank	No	1,268	No change	No change	2.92	58.19	2.22
Bay. Vereinsbank	No	700			3.04	69.68	2.31
Oldenburgische Landesbank	No	82			4.43	74.43	3.72
Average of 9 untreated banks	No	330			3.17	64.23	2.23

Notes: Assets are in million Deutsche Mark. The data are hand-digitized from the annual reports of the banks. The 9 untreated banks are: Badische Bank, Bay. Hyp.- & Wechsel-Bank, Bay. Vereinsbank, Handels- und Gewerbebank Heilbronn, Handelsbank Lübeck, Norddeutsche Kreditbank, Oldenburgische Landesbank, Vereinsbank Hamburg, Württembergische Bank.

Table 5.19: The effect on the growth of stock corporations

	(1)	(2)	(3)
Panel A: Bank debt growth 1951-60			
Rel. bank treated	-0.001	-0.005	0.006
in 1952/57	(0.016)	(0.017)	(0.018)
Observations	421	421	421
R^2	0.000	0.134	0.152
Panel B: $\frac{100 \cdot Bk debt}{Assets}$ difference 1951-60			
Rel. bank treated	0.144	0.085	0.226
in 1952/57	(0.171)	(0.193)	(0.188)
Observations	421	421	421
R^2	0.002	0.095	0.125
Panel C: Employment growth 1951-60			
Rel. bank treated	0.001	0.000	-0.001
in 1952/57	(0.004)	(0.004)	(0.005)
Observations	821	734	685
R^2	0.000	0.107	0.112
Panel D: Revenue per worker growth 1951-60			
Rel. bank treated	0.004	0.002	-0.000
in 1952/57	(0.005)	(0.006)	(0.006)
Observations	345	299	293
R^2	0.002	0.195	0.303
Industry FE*Zone FE	No	Yes	Yes
ln age*Zone FE	No	No	Yes
Size control*Zone FE	No	No	Yes

Notes: The table reports estimates of the effect of having a treated relationship bank on the growth of firm variables. Growth in panels A, C, and D is the average annual symmetric growth rate, i.e. the symmetric growth rate from 1951 to 1960 divided by 9, the number of years between 1951 and 1960. The 1951-60 difference in $\frac{100 \cdot Bank debt}{Assets}$ is the change in the percent ratio of bank debt over assets from 1951 to 1960, divided by 9. Relationship bank treated in 1952/57 is a dummy for whether the firm had a bank treated in 1952 or 1957 among its relationship banks in 1951. The control variables include 18 industry fixed effects and the natural logarithm of the firm's age. Both are fully interacted with fixed effects for the Northern, Western, and Southern banking zones that were in existence from 1952 to 1957. The size control in this table is the natural logarithm of 1951 firm assets, also interacted with the zonal fixed effects. Standard errors are robust. The samples include only stock corporations.

Table 5.20: The effect on the growth of non-stock firms

	(1)	(2)	(3)	(4)
Outcome	Employment growth 1951-56			
Rel. bank treated	-0.001	-0.001	0.001	-0.001
in 1952	(0.003)	(0.004)	(0.007)	(0.007)
Observations	1,521	1,472	353	342
R^2	0.000	0.063	0.000	0.110
Industry FE*Zone FE	No	Yes	No	No
ln age*Zone FE	No	Yes	No	No
Size control*Zone FE	No	Yes	No	No
Industry FE	No	No	No	Yes
ln age	No	No	No	Yes
Size control	No	No	No	Yes
Sample	All		Matched	

Notes: The table reports estimates of the effect of having a relationship bank treated in 1952 on the average annual symmetric growth rate of employment, i.e. the symmetric growth rate from 1951 to 1956 divided by 5, the number of years between 1951 and 1956. Relationship bank treated in 1952 is a dummy for whether a 1951 relationship bank was treated in 1952. The size control variables in this table are four fixed effects for the firm's employment in 1951 (1-49, 50-249, 250-999, 1000+ employees). The other control variables are explained in Table 5.19. Standard errors are robust. The samples include only non-stock firms.

Table 5.21: The effect on employment growth 1951-56, by firm size

Number of Employees	Coefficient	Std. Err.	Observations
0 - 9	-0.035	(0.029)	8
10 - 19	-0.040	(0.035)	15
20 - 29	-0.069	(0.029)	19
30 - 39	-0.023	(0.042)	27
40 - 49	0.008	(0.025)	19
50 - 59	-0.015	(0.030)	24
60 - 499	0.000	(0.004)	1,064
≥ 500	0.005	(0.007)	345

Notes: The table reports estimates of the effect of having a relationship bank treated in 1952 on the average annual symmetric growth rate of employment 1951-56, i.e. the symmetric growth rate from 1951 to 1956 divided by 5, the number of years between 1951 and 1956. Each row reports a different regression, limiting the sample to only firms in the given range of employment. The specifications include no control variables. Standard errors are robust. The samples include only non-stock firms.

Table 5.22: The effect on the growth of opaque firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\Delta\frac{100 \cdot Bk\,debt}{Assets}$	$\Delta\frac{100 \cdot Cap}{Assets}$	Asset growth	Empl growth	$\Delta\frac{100 \cdot Bk\,debt}{Assets}$	Employment growth	
Outcome	1951-60		1951-60		1951-60	1951-56	
Rel. bank treated in 1952/57	-1.413	0.598	-0.011	0.000			
	(0.674)	(0.370)	(0.012)	(0.017)			
0 < Fraction rel. banks treated in 1952/57 $\leq$ 0.5					-1.289		
					(0.716)		
0.5 < Fraction rel. banks treated in 1952/57 $\leq$ 1					-1.831		
					(0.710)		
0 < Fraction rel. banks treated in 1952 $\leq$ 0.5						-0.016	-0.030
						(0.012)	(0.023)
0.5 < Fraction rel. banks treated in 1952 $\leq$ 1						-0.029	-0.037
						(0.015)	(0.019)
Observations	74	74	168	160	74	295	65
R^2	0.561	0.775	0.526	0.341	0.567	0.229	0.366
Controls*Zone FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Controls	No	No	No	No	No	No	Yes
Sample	All	All	All	All	All	All	Matched
Firm type	Stock	Stock	Stock	Stock	Stock	Non-Stock	Non-Stock

Notes: The outcome variables, regressors, and control variables are explained in Tables 5.19 and 5.20. Standard errors are robust. The sample in every column includes only opaque firms. A firm is opaque if it has fewer than 50 employees in 1951, is younger than 10 years old in 1952, or is in the bottom ten percent of industry asset tangibility (fixed tangible over total assets).

Table 5.23: Robustness checks for the effect on firm growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Employment growth						
Outcome	1949-51	1951-60		1951-56			
Rel. bank treated in 1952/57	0.005	-0.007	-0.005				
	(0.023)	(0.011)	(0.008)				
Rel. bank treated in 1952	-0.000			0.002			0.005
	(0.023)			(0.009)			(0.013)
0 < Fraction rel. banks treated in 1952 $\leq$ 0.5					-0.002		
					(0.004)		
0.5 < Fraction rel. banks treated in 1952 $\leq$ 1					0.002		
					(0.007)		
Commerzbank rel. bank treated in 1952						-0.000	
						(0.006)	
Deutsche Bank rel. bank treated in 1952						-0.004	
						(0.004)	
Dresdner Bank rel. bank treated in 1952						0.002	
						(0.005)	
Added a bank treated in 1952 as rel. bank							0.002
							(0.015)
Observations	1,147	225	338	464	1,472	1,472	308
R^2	0.139	0.297	0.198	0.168	0.063	0.063	0.231
Controls*zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	High bk. debt	Low cap.	Exporters	All	All	Added banks
Firm type	Both	Stock	Stock	Non-Stock	Non-Stock	Non-Stock	Non-Stock

Notes: Fraction of relationship banks treated is the ratio of the number of treated relationship banks over the total number of relationship banks. The sample includes only firms: with a ratio of bank debt over total liabilities above the median in 1951 in column (1); with a ratio of stock capital over assets below the median in column (2); that export some of their products in 1951 in column (4); that increased their total number of relationship banks from 1951 to 1956 in column (7). The outcome variables, regressors, and control variables are explained in Tables 5.19 and 5.20. Standard errors are robust.

Table 5.24: Financial statistics by banking group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Deutsche Bank	Dresdner Bank	Commerz-bank	Bay. Hyp.- & Wechsel-Bank	Bay. Vereins-bank	Olden-burgische Lan-des-bank	Mean Difference: Treated - 9 Untreated (Std. Err.)
Panel A: Growth of lending and profits 1952-60 (symmetric growth)							
Lending	0.70	0.56	1.09	1.23	1.29	1.36	-0.277
							(0.172)
Profits	1.38	1.46	1.62	2	1.70	1.89	-0.057
							(0.126)
Panel B: Change in cost efficiency ratios 1952-60 (in percentage points)							
$\frac{Non-int\ cost}{Assets}$	-0.27	-0.10	-0.68	-1.05	-1.53	-1.54	0.80
							(0.31)
$\frac{Non-int\ cost}{Revenue}$	-7.29	-19.92	-15.32	-25.19	-38.99	-10.23	1.53
							(6.79)
$\frac{Empl\ comp}{Assets}$	0.00	-0.16	-0.41	-0.76	-1.17	-1.62	0.45
							(0.28)
Panel C: The average fraction among firms' relationship banks by period (in percent)							
1951	18.87	10.84	6.66	2.48	1.53	0.04	
1958-61	18.56	10.87	7.97	2.47	1.42	0.03	
1970	16.58	10.79	9.57	2.46	1.78	0.02	

Notes: The growth in Panel A is the symmetric growth rate from 1952 to 1960. The change in Panel B is the difference of the given percent ratios, i.e. the difference in percentage points. The fractions in Panel C are the average values (using all firms in the Hoppenstedt volumes from the given period) of the number of treated relationship banks over the total number of relationship banks, in percent. Column (7) reports the difference in the mean growth of the three treated banking groups relative to 9 untreated banks. The 9 untreated banks are: Badische Bank, Bay. Hyp.- & Wechsel-Bank, Bay. Vereinsbank, Handels- und Gewerbebank Heilbronn, Handelsbank Lübeck, Norddeutsche Kreditbank, Oldenburgische Landesbank, Vereinsbank Hamburg, Württembergische Bank.

Table 5.25: The number of media mentions of treated banks and their executives

	(1)	(2)	(3)
	Phase 1 30/06/1947 - 29/03/1952	Phase 2 30/03/1952 - 24/12/1956	Phase 3 25/12/1956 - 24/09/1961
Panel A: Der Spiegel (German weekly news magazine)			
Name of a treated bank	15	46	121
Name of a treated bank executive	6	12	20
The word "bank"	487	407	479
The word "Deutschland"	3,145	3,086	3,062
Panel B: Financial Times (British daily newspaper)			
Name of a treated bank	3	261	779
Name of a treated bank executive	2	36	143
The word "bank"	22,160	30,035	37,168
The word "Germany"	4,065	8,129	10,311

Notes: The table reports the number of times that the item listed in the left column was mentioned in an article in the given period. The data are based on the author's calculations from the online archives of Der Spiegel and the Financial Times, accessed 29 August 2017.

Table 5.26: New banking relationships with opaque firms

	(1)	(2)	(3)	(4)	(5)	(6)
	Fraction of treated rel. banks in					
Outcome	1970	1970	1970	1951	1970	1951
Opaque firm	-0.056		-0.054	-0.001	0.023	0.023
	(0.019)		(0.021)	(0.046)	(0.015)	(0.020)
0 < Employees < 20		-0.072				
		(0.030)				
20 ≤ Employees < 50		-0.086				
		(0.026)				
0 < Ind. Tangibility < 0.15		-0.030				
		(0.033)				
0.15 ≤ Ind. Tangibility < 0.2		-0.012				
		(0.053)				
Observations	719	719	719	317	2,286	1,648
R^2	0.010	0.013	0.068	0.000	0.001	0.001
Zone FE	No	No	Yes	No	No	No
Industry FE	No	No	Yes	No	No	No
Sample restricted to only firms with:						
No treated rel. bank in 1951	Yes	Yes	Yes	No	No	No
No treated rel. bank in 1940	No	No	No	Yes	No	No
Treated rel. bank in 1951	No	No	No	No	Yes	Yes

Notes: The outcome is the number of treated relationship banks over the total number of relationship banks in the given year. A firm is opaque if it has fewer than 50 employees in 1951 or is in the bottom ten percent of industry asset tangibility (fixed tangible over total assets). The control variables are explained in Table 5.20. Standard errors are robust.

Table 5.27: New banking relationships with risky firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Fraction of treated rel. banks in						
Outcome	1970	1970	1951	1970	1970	1970	1970
$0.25 \leq \frac{Cap}{Assets} < 0.5$	0.035	0.040	0.036	0.012			
	(0.035)	(0.038)	(0.019)	(0.027)			
$0.5 \leq \frac{Cap}{Assets} < 0.75$	-0.007	0.017	0.078	0.081			
	(0.052)	(0.058)	(0.034)	(0.046)			
$0.75 \leq \frac{Cap}{Assets} \leq 1$	-0.087	-0.138	0.161	0.250			
	(0.031)	(0.071)	(0.077)	(0.105)			
Volatile employment firm					0.058		
					(0.027)		
Volatile revenue firm						0.084	
						(0.065)	
High productivity firm							-0.038
							(0.029)
Observations	158	155	581	402	74	265	294
R^2	0.028	0.203	0.056	0.111	0.257	0.109	0.118
Opaque firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zone FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Sample restricted to only firms with:							
No treated rel. bank in 1951	Yes	Yes	No	No	Yes	Yes	Yes
Treated rel. bank in 1951	No	No	Yes	Yes	No	No	No

Notes: The outcome is the ratio of the number of treated relationship banks over the total number of relationship banks in the given year. Cap / assets is the ratio of stock capital over total assets. The standard deviation of the annual employment (or revenue) growth in the period 1949 to 1951 is above the median for a volatile employment (or revenue) firm. High productivity firms have revenue per worker above the median. A firm is opaque if it has fewer than 50 employees in 1951 or is in the bottom ten percent of industry asset tangibility (fixed tangible over total assets). The control variables are explained in Table 5.20. Standard errors are robust.

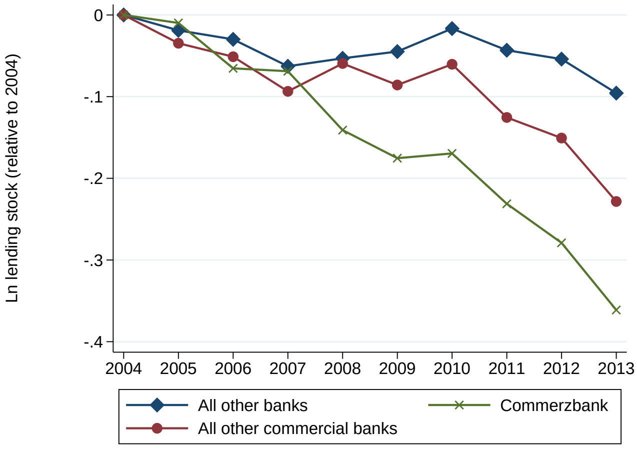
Table 5.28: The effect on municipal employment

	(1)	(2)	(3)	(4)	(5)	(6)
			Employment growth			
Outcome	1951-60	1951-60	1951-60	1951-56	1949-51	1951-60
Treated bank branch	-0.117	-0.118			0.075	-0.116
	(0.045)	(0.049)			(0.068)	(0.053)
Avg fraction of treated banks among firms' rel. banks			-0.285			
			(0.104)			
Treated bank branch not in NRW				-0.062		
				(0.033)		
Treated bank branch in NRW				-0.019		
				(0.044)		
Observations	79	79	74	91	83	72
R^2	0.340	0.350	0.344	0.202	0.441	0.508
Federal state FE	Yes	Yes	Yes	Yes	Yes	No
Size bin FE	Yes	Yes	Yes	Yes	Yes	No
Ruhr FE	No	Yes	Yes	Yes	Yes	No
Detailed controls*zone FE	No	No	No	No	No	Yes

Notes: The table reports estimates of the effect of having a treated bank branch in the municipality (as measured in 1952) on municipal employment. The outcomes are symmetric growth rates of employment in the given period. Treated bank branches belong to banks treated by the first reform of 1952, the second reform of 1957, or both. Treated bank branches not in NRW (North-Rhine Westphalia) were treated in 1952 and 1957, while treated bank branches in NRW were only treated in 1957. The average fraction of treated banks among firms' relationship banks is the average, over firms located in the municipality, of the firms' fraction of treated relationship banks out of the all relationship banks. Size bins are five quantiles of total employment in the municipality. The detailed controls include the full interaction of zonal fixed effects with the following variables: the growth rate from 1949 to 1951, five quantiles of total employment, the share of employment in manufacturing, the share of employment in the primary sector, and the employment share of war-time displaced. Standard errors are robust.

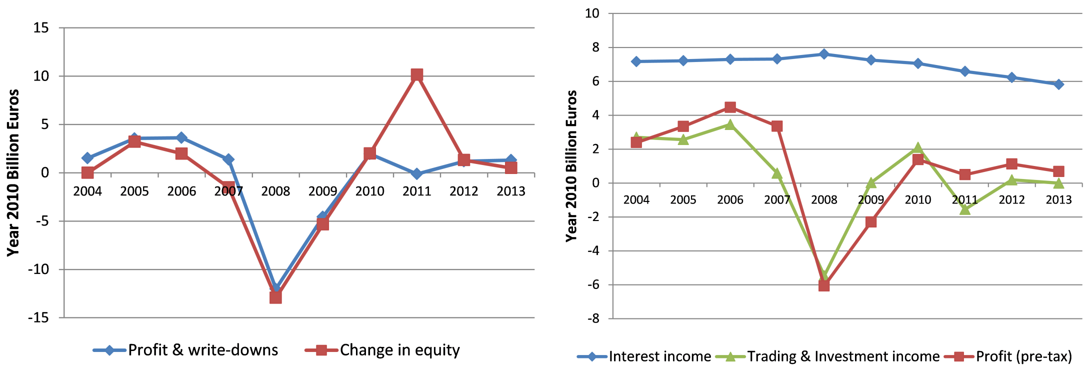
5.2 Figures

Figure 5.1: The lending stock of German banks



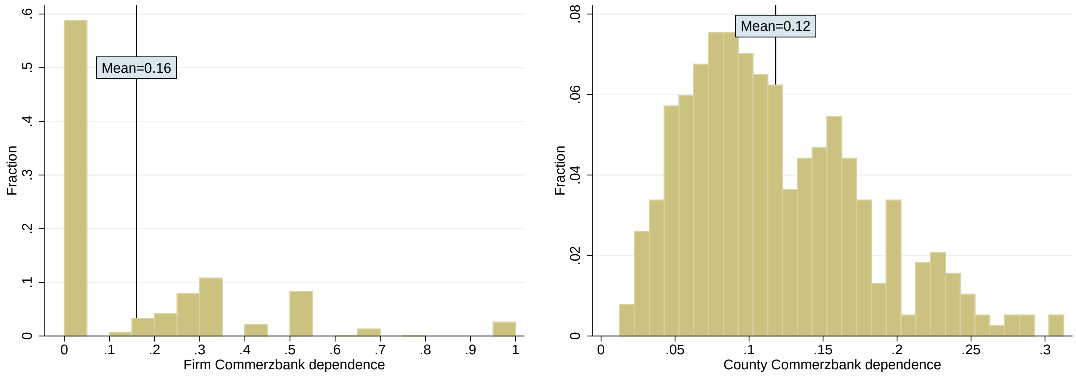
Notes: This figure plots the ln lending stock to German non-financial customers, relative to the year 2004, in 2010 billion Euro. The data for Commerzbank include lending by branches of Commerzbank and Dresdner Bank. I sum their lending stock for the years before the 2009 take-over, using data from the annual reports. For "all other banks", I use aggregated data from the Deutsche Bundesbank on German banks and subtract lending by Commerzbank. For "all other commercial banks", I subtract lending by Commerzbank, the savings banks, the Landesbanken, and the cooperative banks.

Figure 5.2: Commerzbank's equity capital, write-downs, and profits



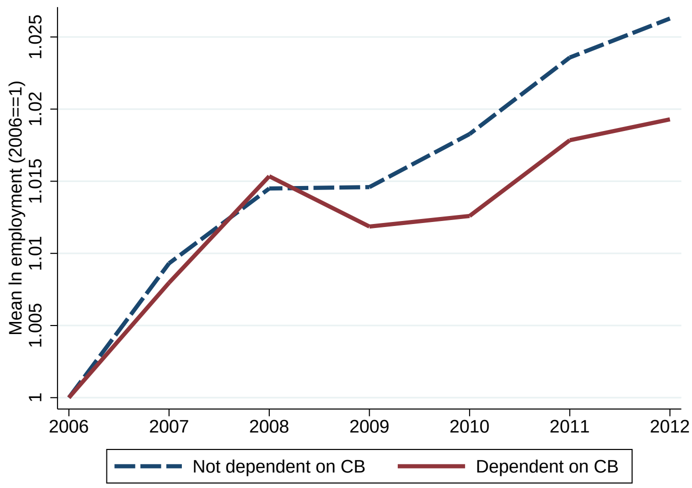
Notes: The left panel shows Commerzbank's profits & write-downs and equity capital. Write-downs arise from changes in revaluation reserves, cash flow hedges and currency reserves. The right panel shows the composition of Commerzbank's profits. Interest income is interest received from loans and securities minus interest paid on deposits. Trading & investment income is the sum of net trading income, net income on hedge accounting, and net investment income. Pre-tax profit is interest income plus trading & investment income minus costs. The values are in year 2010 billion Euro. I aggregate the positions of Commerzbank and Dresdner Bank for the years before the 2009 take-over. The data are from the annual bank reports.

Figure 5.3: Firm and county Commerzbank dependence



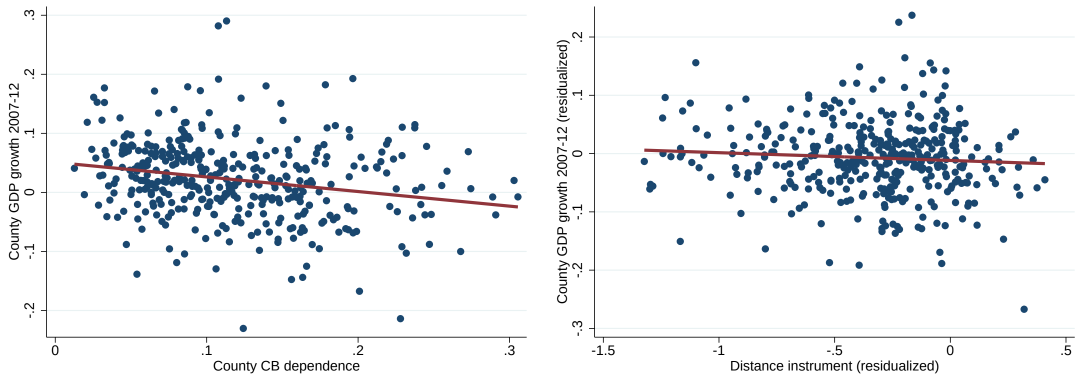
Notes: The figure shows histograms of firm Commerzbank dependence for the 2,011 firms in the firm panel (on the left) and of county Commerzbank dependence for the 385 counties in the dataset (on the right).

Figure 5.4: Firm employment effects



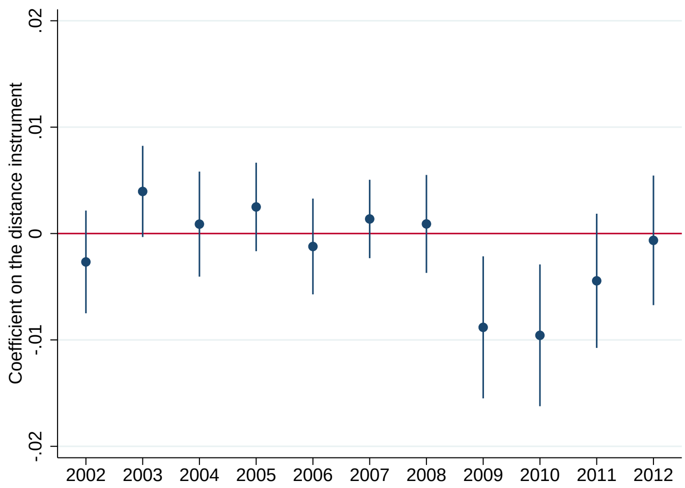
Notes: This figure plots the time series of the mean ln employment of firms with and without Commerzbank as one of their relationship banks. The time series are divided by their 2006 value. The data are from the firm panel.

Figure 5.5: County GDP growth, Commerzbank dependence, and the distance instrument



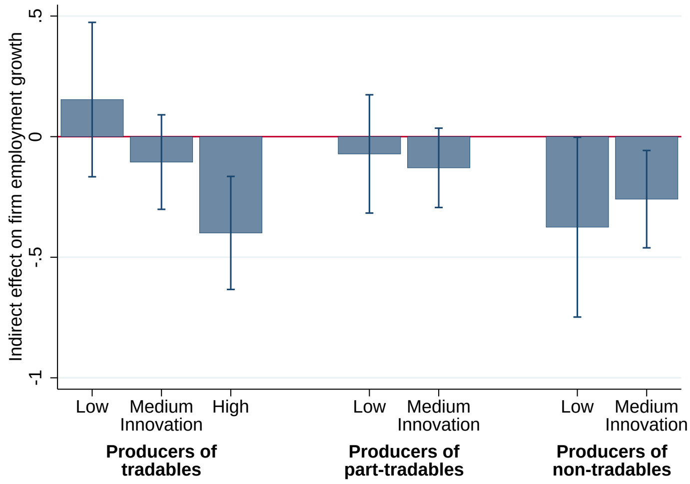
Notes: The left figure plots county GDP growth from 2007 to 2012 against county Commerzbank dependence. The right figure plots county GDP growth against the distance instrument, where both variables are residualized of the linear distances to Düsseldorf, Frankfurt, Hamburg, Berlin, and Dresden, and of a dummy for the former GDR. Both linear slope coefficients are negative and significant at the 1 percent level.

Figure 5.6: Reduced-form impact of the instrument on the county GDP growth rate



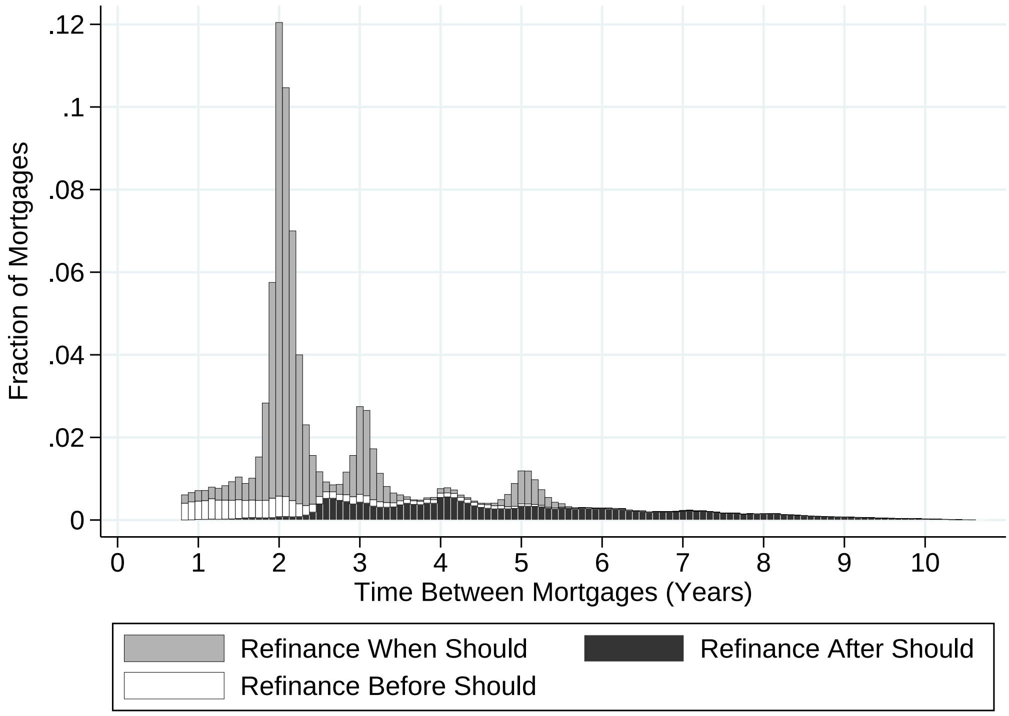
Notes: This figure is based on a single regression, in which the dependent variable is the county's annual GDP growth rate. The plotted point estimates are the coefficients on the instrument, interacted with annual dummy variables. The vertical lines are 90 percent confidence intervals. The regression includes year and county fixed effects and the full set of control variables from Table 5.9, including the linear distances. The standard errors are calculated as in Table 5.8.

Figure 5.7: The size of the indirect effect by industry type



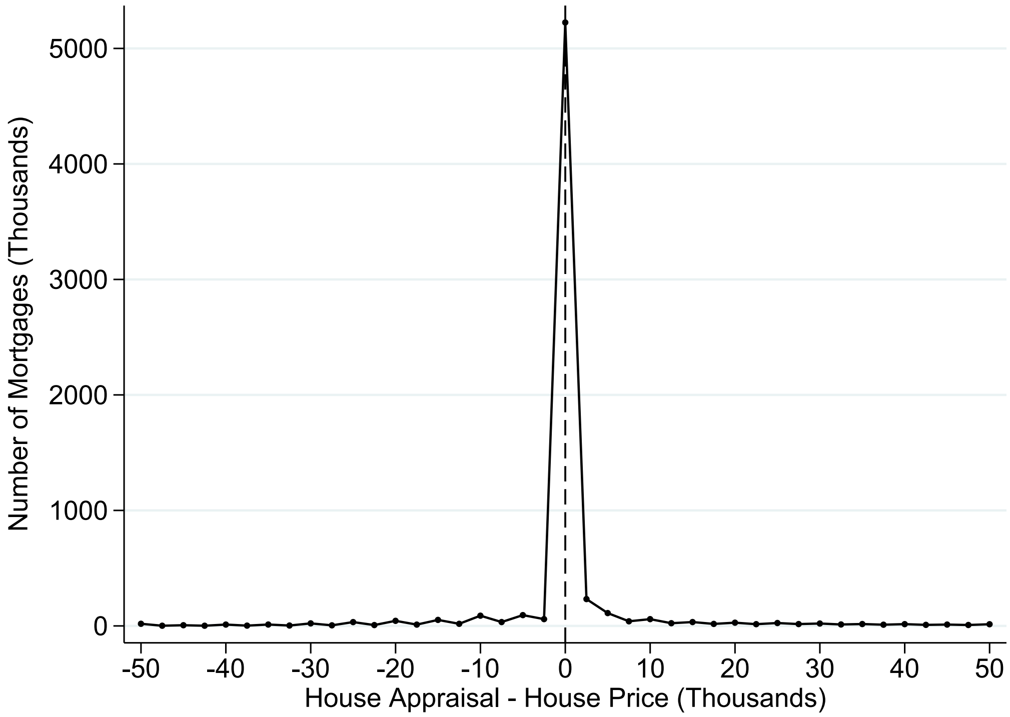
Notes: This figure illustrates heterogeneity in the indirect effect by industry type. The plotted point estimates are the effect of the Commerzbank dependence of all other firms in the county on the symmetric growth rate of firm employment between 2008 and 2012. The estimates are from a single regression that controls for the firm's direct Commerzbank dependence and the other control variables from Table 5.10. The vertical lines are 90 percent confidence intervals.

Figure 5.8: Homeowners Refinance Around the Onset of the Reset Rate



Notes: The figure shows the distribution of the time between mortgage financing events. Households who refinance between 2 months before and 6 months after the onset of their reset rate are shown in light gray, households who refinance more than 6 months after the onset of their reset rate are shown in black, and households who refinance more than 2 months before the onset of their reset rate are shown in white. The data in this figure exclude households for whom we do not observe the date of reset rate onset.

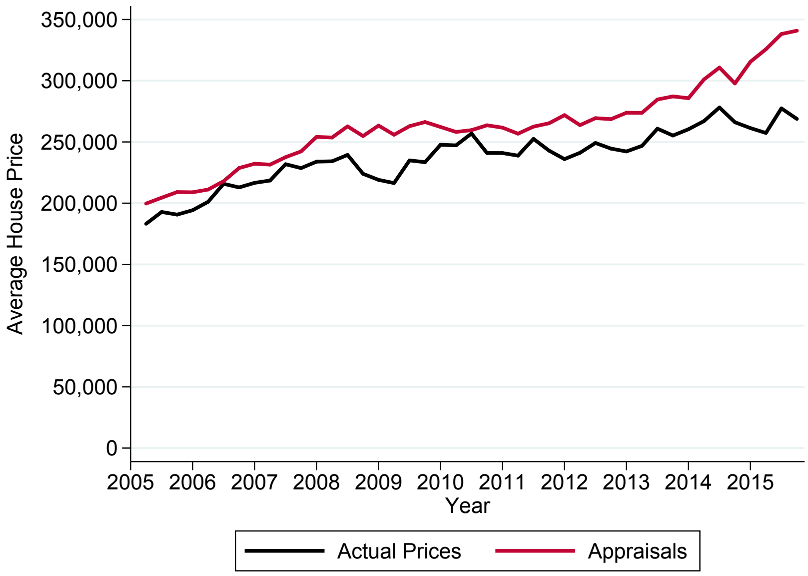
Figure 5.9: House Prices vs Appraisals (New Purchases)



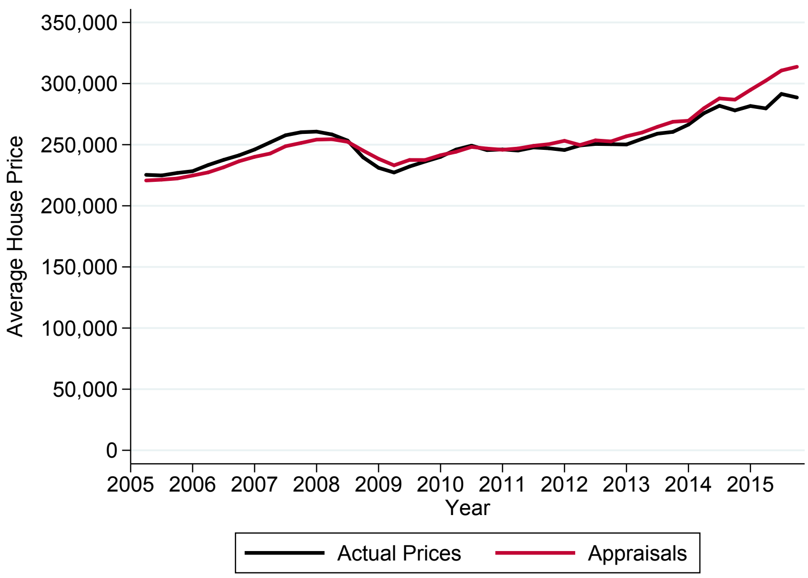
Notes: The figure shows the distribution of within-house differences between the actual house price and the appraisal price for transacted properties. This includes both first time buyers and home movers, but not refinancers.

Figure 5.10: House Prices vs Appraisals (Refinanced Homes)

A: No Other Controls



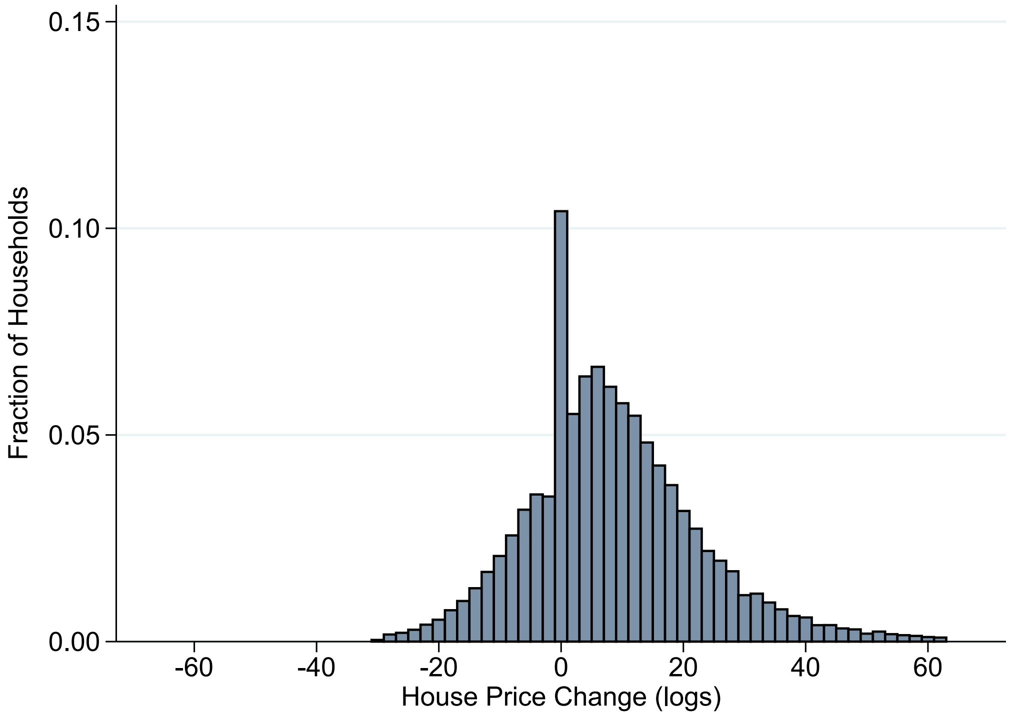
B: With Controls



Notes: The figure compares actual house prices (for transacted properties) with appraisal prices (for refinanced properties) over time. Panel A plots the raw time series of actual and appraised prices, obtained by regressing each of the price series on a full set of quarter dummies and plotting the estimated coefficients. Panel B augments the price regressions on quarter dummies with controls for twenty quantiles of the age distribution as well as twenty quantiles of the postcode-level price distribution (see equation (2.1)). The panel plots the coefficients on the quarter dummies, plus a constant equal to the effect of the average age and the average postcode. This panel shows that, once we correct for compositional differences in age and postcode, there is no significant appraisal bias.

Figure 5.11: Distribution of Raw House Price Growth

Households With $\geq$ 2 Mortgage Observations

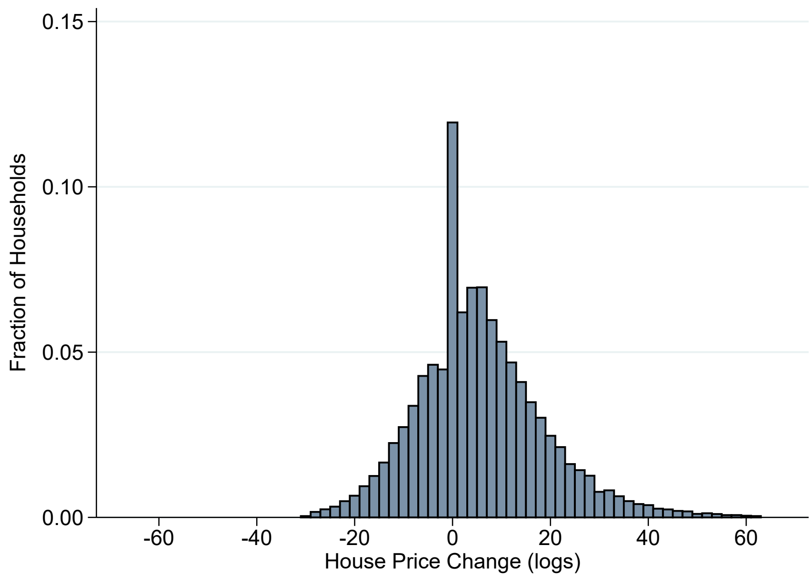


Notes: The figure shows the distribution of raw house price growth among households for whom we observe at least two mortgage financing events. House price growth is measured as the log change in house prices between the current and the last mortgage event, multiplied by 100 (i.e., approximately percentage house price growth).

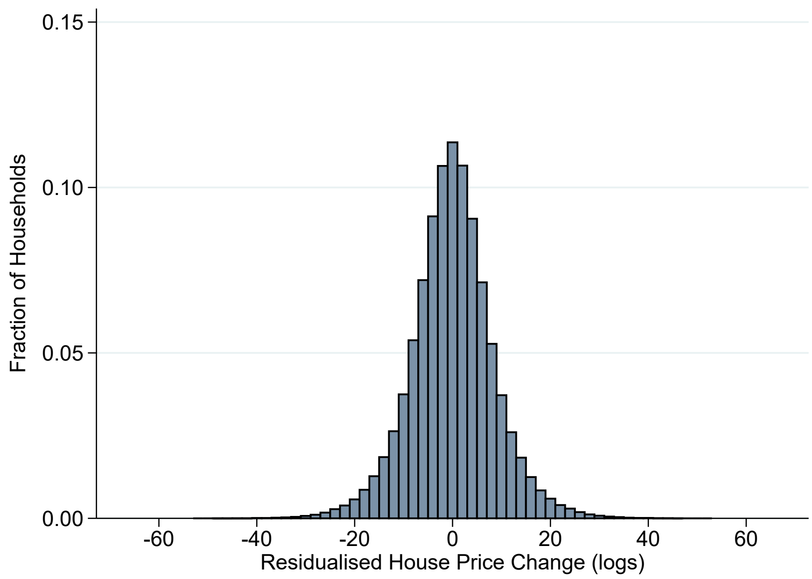
Figure 5.12: Distribution of Raw vs Residualized House Price Growth

Households With $\geq$ 3 Mortgage Observations

A: Raw Price Growth



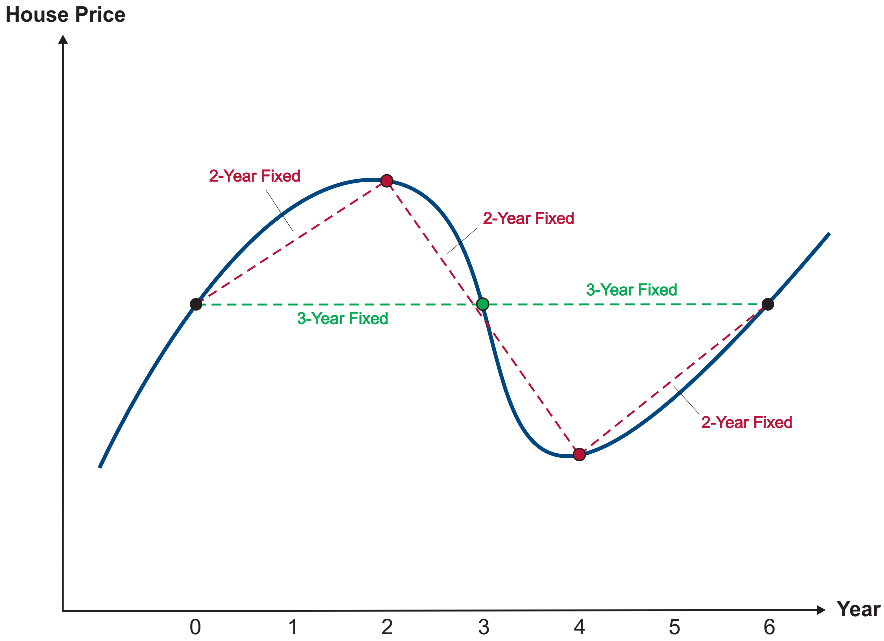
B: Residualized Price Growth After Absorbing Fixed Effects



Notes: The figure shows distributions of house price growth among households for whom we observe at least three mortgage financing events. Panel A shows the distribution of raw house price growth, while Panel B shows the distribution of residualized house price growth after absorbing household fixed effects, month fixed effects, and county-by-year fixed effects. In both panels, house price growth is measured as the log change in house prices between the current and the last mortgage event, multiplied by 100 (i.e., approximately percentage house price growth).

Figure 5.13: The Timing of Refinance Events and House Price Changes

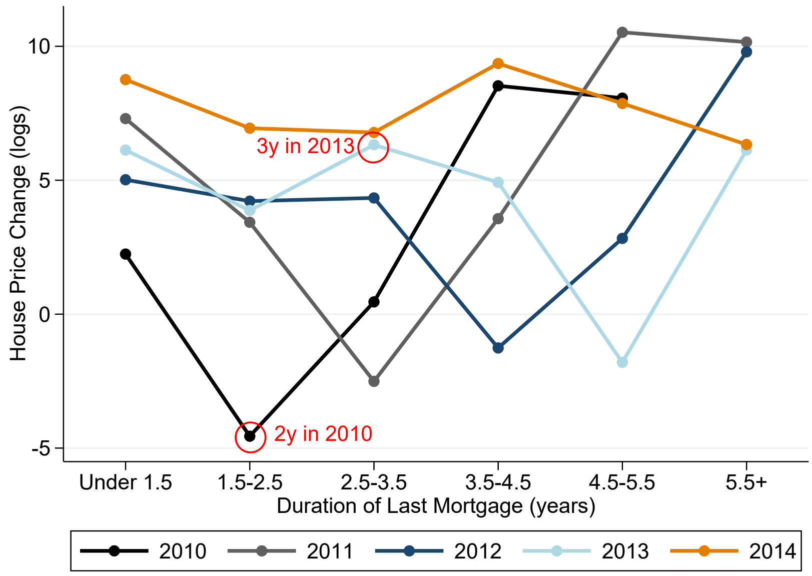
Conceptual Example



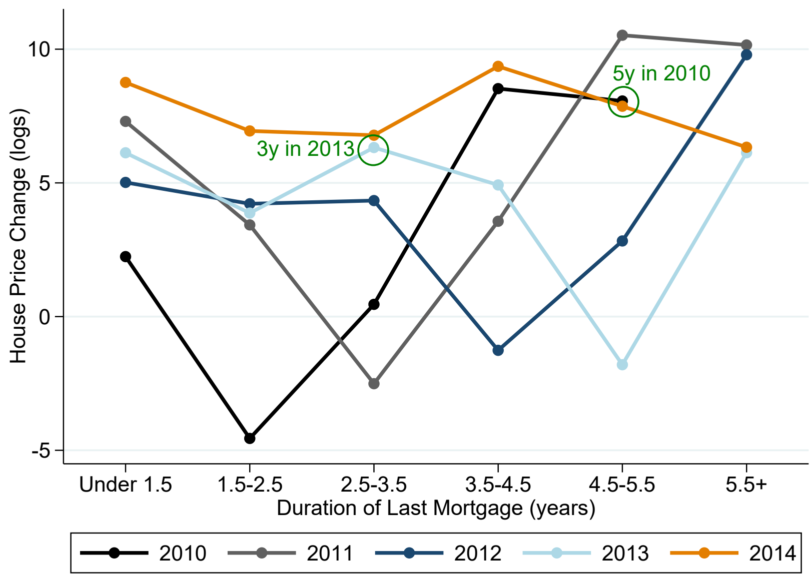
Notes: The figure illustrates, in a conceptual example, how differences in contract duration choices create variation in house price changes across households. The graph compares two homeowners who start out at the same time (time 0), live in houses with the same price cycle (the solid blue line), but have different preferences over low-interest rate durations. One homeowner prefers 2-year fixed interest rate loans, while the other prefers 3-year fixed interest loans. The homeowner in two-year contracts refinances three times over a 6-year period, facing either positive or negative price shocks at each event, whereas the homeowner in 3-year contracts refinances only two times facing a zero price shock each time. Our empirical strategy exploits such within-person variation in price growth driven by duration choices.

Figure 5.14: House Price Changes vs Last Duration x Time of Refinance

A: Large Difference in Price Change



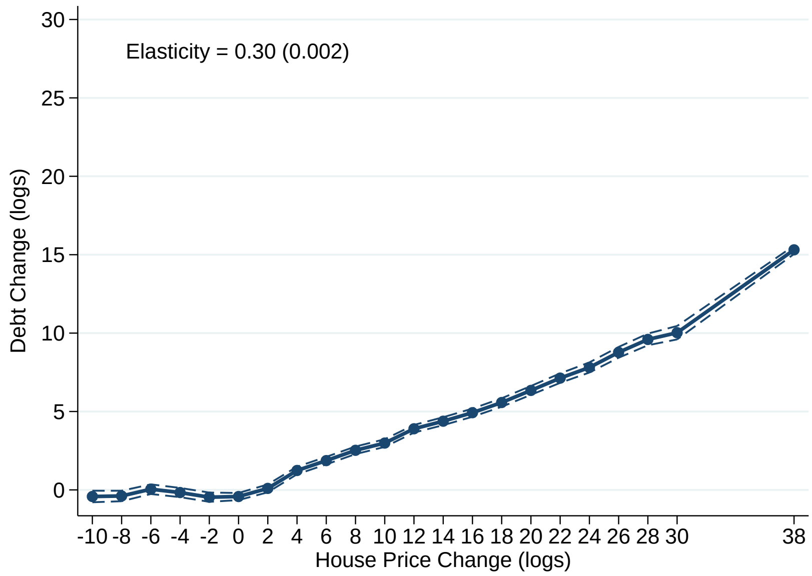
B: Small Difference in Price Change



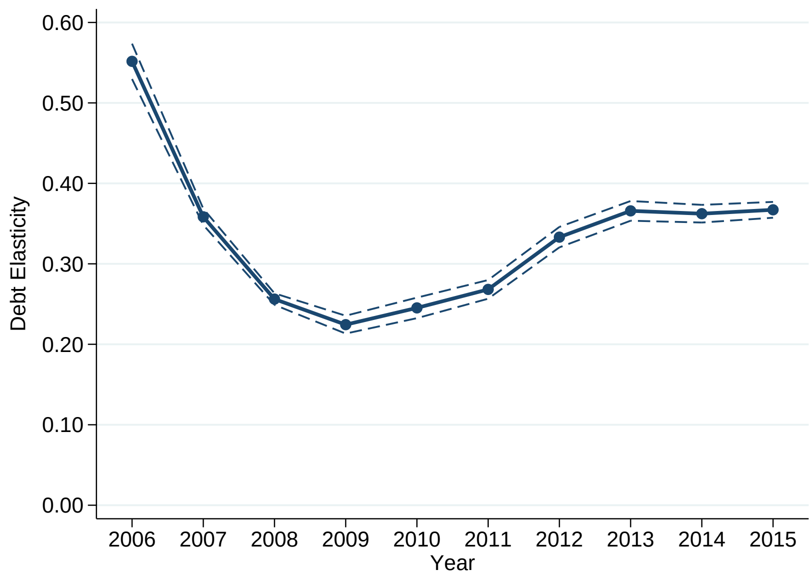
Notes: This figure is the empirical counterpart to the preceding conceptual figure. It plots average house price growth between refinance events for homeowners who refinance at different points in time (in January of different years) by bins of the duration of their last mortgage (number of years between the current and the last refinance events). The two panels show the same graphs, but highlight two different homeowners who experience very different within-person price patterns due to past duration choices. The homeowner in Panel A experiences a large negative price change in January 2010, followed by a large positive change in January 2013. The homeowner in Panel B also refinances in January 2010 and January 2013, but experiences similar price changes in the two events. Our empirical approach uses such within-person variation for identification.

Figure 5.15: Mortgage Debt and House Prices

A: Debt Growth vs House Price Growth



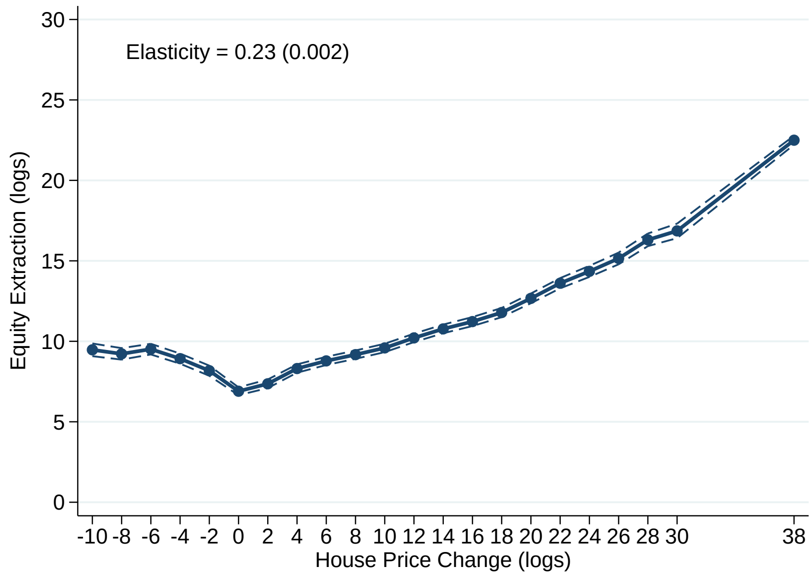
B: Debt Elasticity by Year



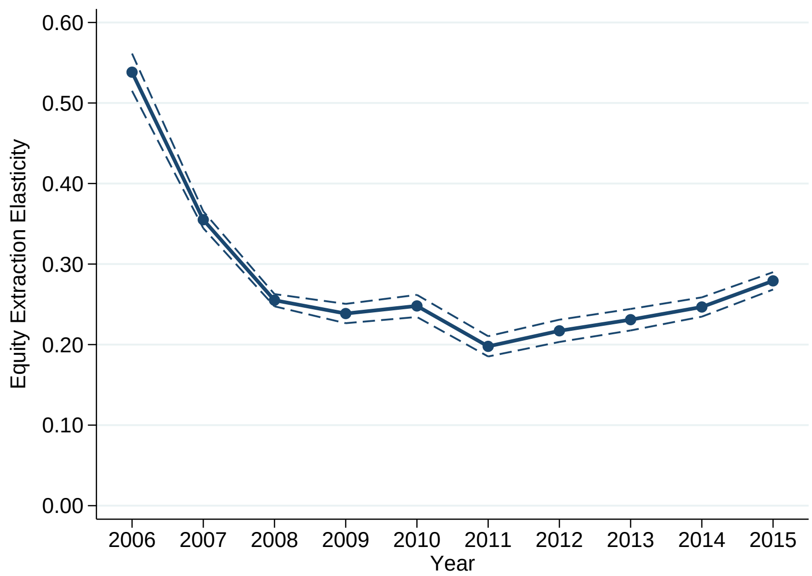
Notes: Panel A plots the average mortgage debt growth in different bins of house price growth, pooling all years 2005-15. Debt growth and house price growth are measured as log changes between refinance events multiplied by 100 (i.e., approximately percentage changes). The dashed lines represent 95% confidence intervals based on standard errors clustered at the household level. Panel A also shows the average elasticity of mortgage debt with respect to house prices across all years. Panel B reports the elasticity for each year separately, showing that the elasticity is pro-cyclical.

Figure 5.16: Equity Extraction and House Prices

A: Equity Extraction vs House Price Growth



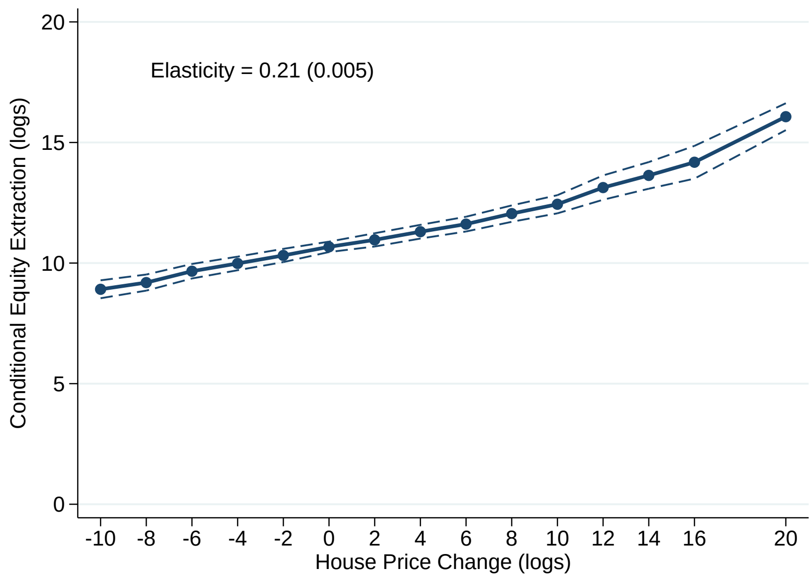
B: Equity Extraction Elasticity by Year



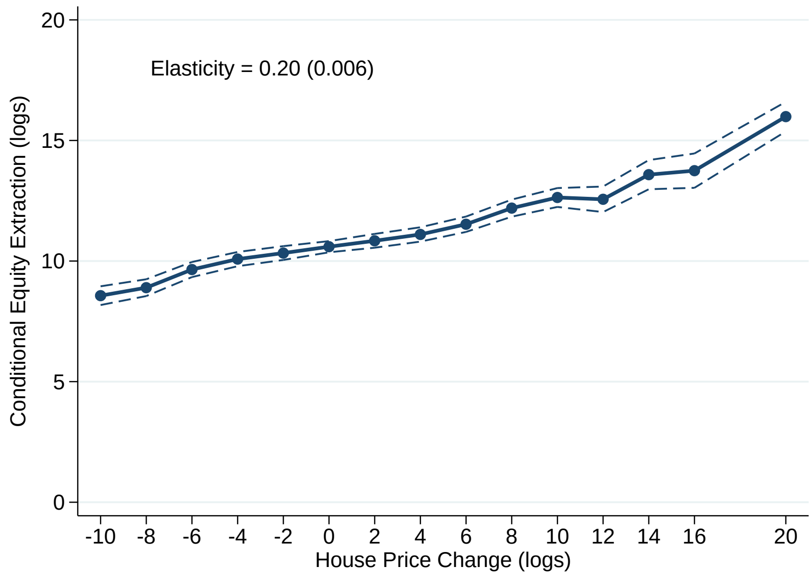
Notes: This figure corresponds to the previous figure, but considers equity extraction when refinancing (as opposed to total debt growth between refinance events) as the outcome variable. Equity extraction is measured as the log difference between mortgage debt after refinancing and the outstanding mortgage debt just before refinancing (i.e., the debt the household would hold if she simply rolled over the existing mortgage debt at the time of refinancing), multiplied by 100. Panel A plots average equity extraction in different bins of house price growth, pooling all years 2005-15. The dashed lines represent 95% confidence intervals, with standard errors clustered by household. Panel A also reports the average equity extraction elasticity across all years, while Panel B shows the equity extraction elasticity for each year separately.

Figure 5.17: Equity Extraction and House Prices With Fixed Effects

A: Household and Month Fixed Effects



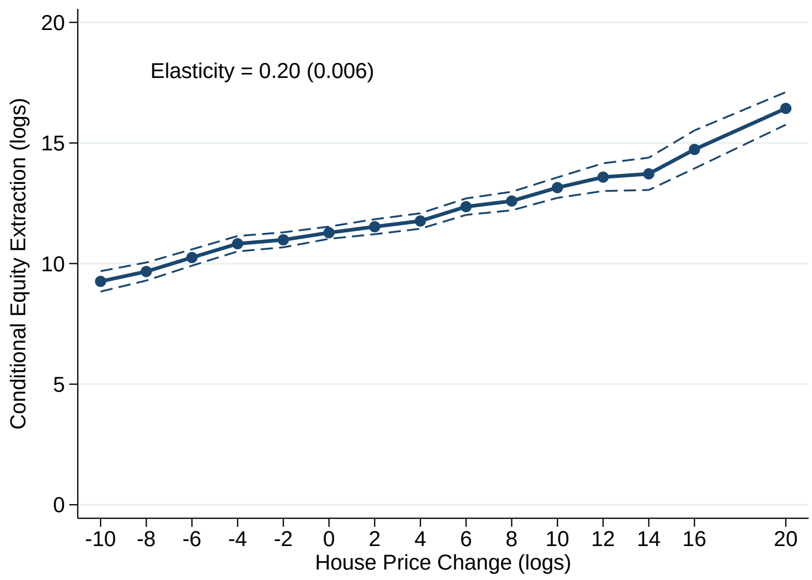
B: Household, Month, and County × Year Fixed Effects



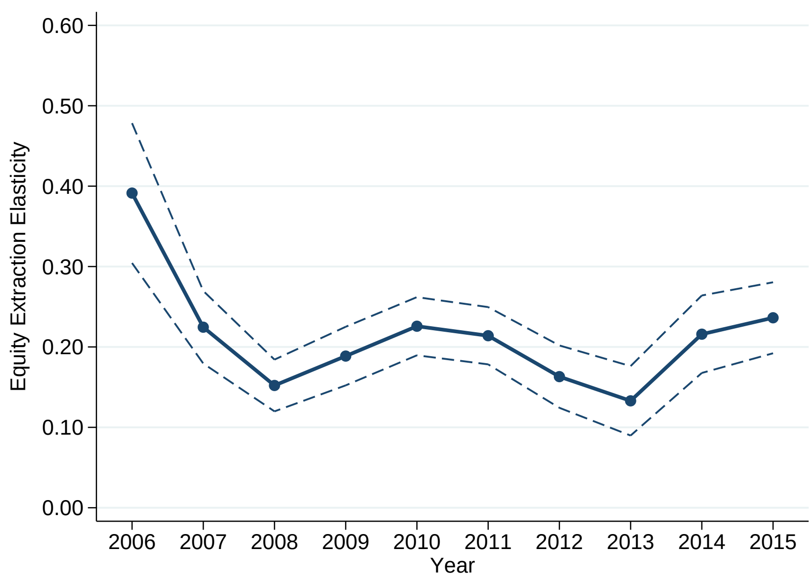
Notes: These panels plot conditional equity extraction in different bins of house price growth based on the fixed effects specification (2.3), pooling all years 2005-15. The plotted points are the estimated coefficients on house price growth dummies, adding a constant equal to the mean predicted value of equity extraction from all the other covariates. In Panel A, the other covariates are fixed effects for household and month. In Panel B, the other covariates are fixed effects for household, month, and county x year. The dashed lines represent 95% confidence intervals based on standard errors clustered by household. Each panel reports the average equity extraction elasticity based on a log-linear specification. The figure shows an almost perfectly log-linear relationship between equity extraction and house prices, and it shows that the relationship is unaffected by county x year (conditional on the other fixed effects).

Figure 5.18: Equity Extraction and House Prices With Fixed Effects and Household Controls

A: Equity Extraction vs House Price Growth



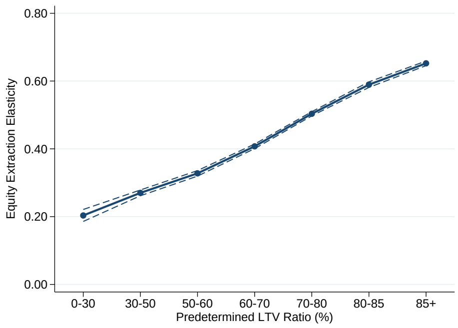
B: Equity Extraction Elasticity by Year



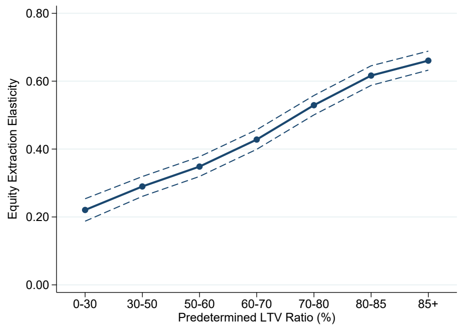
Notes: Panel A plots conditional equity extraction in different bins of house price growth based on the specification with fixed effects and household-level controls in equation (2.4). The panel is constructed exactly like the previous figure that is based on specifications without household-level controls. The household controls included here are income level, income growth, mortgage interest rate, age, a dummy for couples, and dummies for a range of self-reported reasons for the current and the last refinances. The figure shows that the inclusion of such rich controls makes no difference to the results. While Panel A is based on all years 2005-15, Panel B shows the equity extraction elasticity for each year separately. The rich specification considered here has reduced, but not eliminated, the cyclicality in the equity extraction elasticity.

Figure 5.19: Heterogeneity in Borrowing Elasticity by LTV and Age

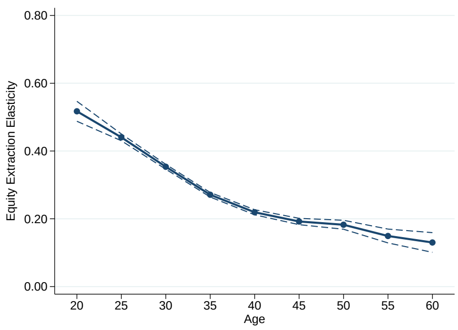
A: Pre-LTV
No Other Controls



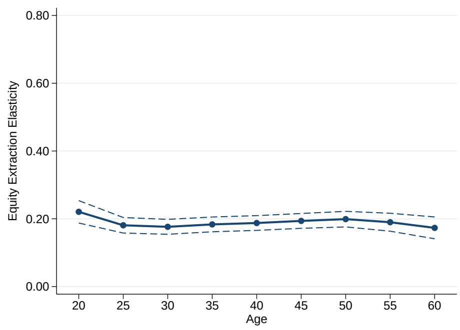
B: Pre-LTV
Controls for Age, Income, and Income Growth



C: Age
No Other Controls

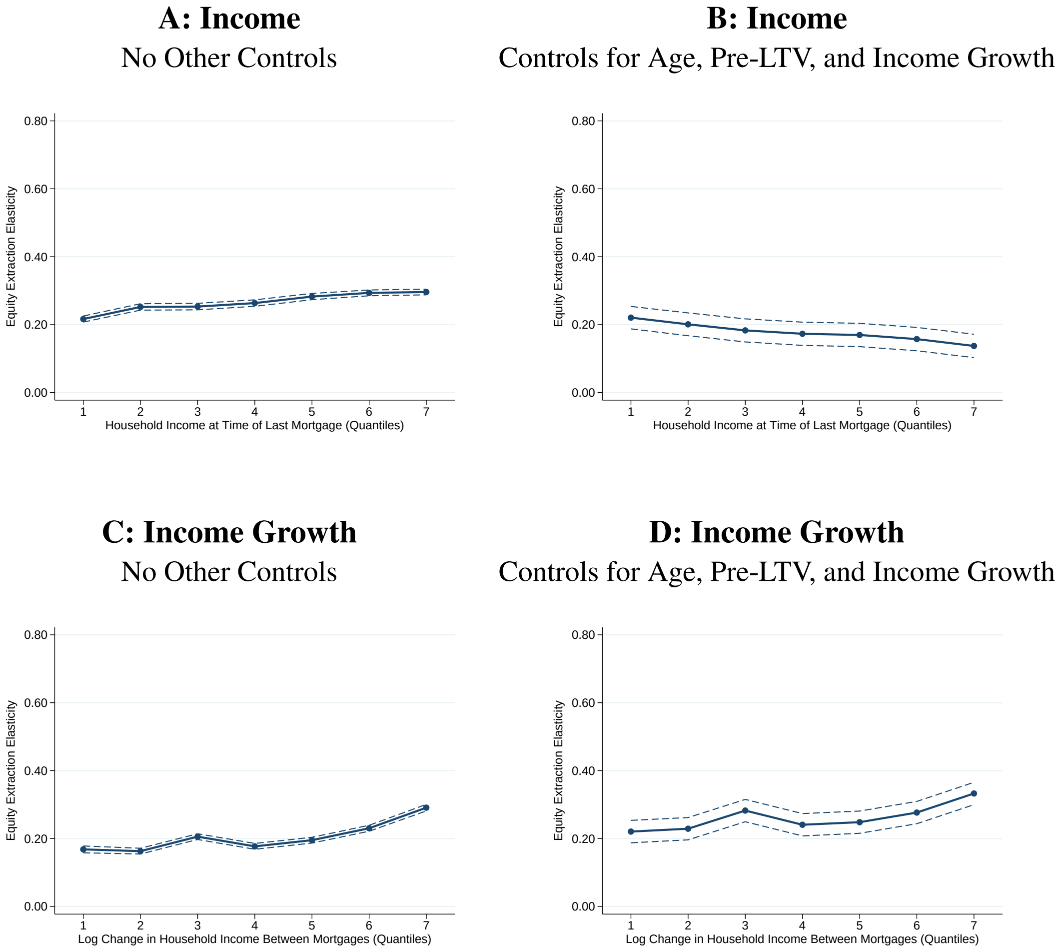


D: Age
Controls for Pre-LTV, Income, and Income Growth



Notes: The figure shows heterogeneity in the equity extraction elasticity by LTV (top panels) and by age (bottom panels). The heterogeneity analysis is based on a pre-determined LTV ratio, namely the LTV ratio at time t absent any equity extraction/injection at time t and absent any house price growth between t and $t-1$. The left panels are based on univariate specifications that investigate each dimension of heterogeneity on its own, while the right panels are based on multivariate specifications allowing for heterogeneity in four dimensions simultaneously: LTV, age, income level, and income growth. The multivariate specification is shown in equation (2.7). The dashed lines give 95% confidence intervals based on standard errors clustered by household. The top panels show a strong increasing relationship between LTV and the borrowing elasticity, consistent with collateral effects. The bottom panels show a negative or flat relationship between age and the borrowing elasticity, inconsistent with wealth effects.

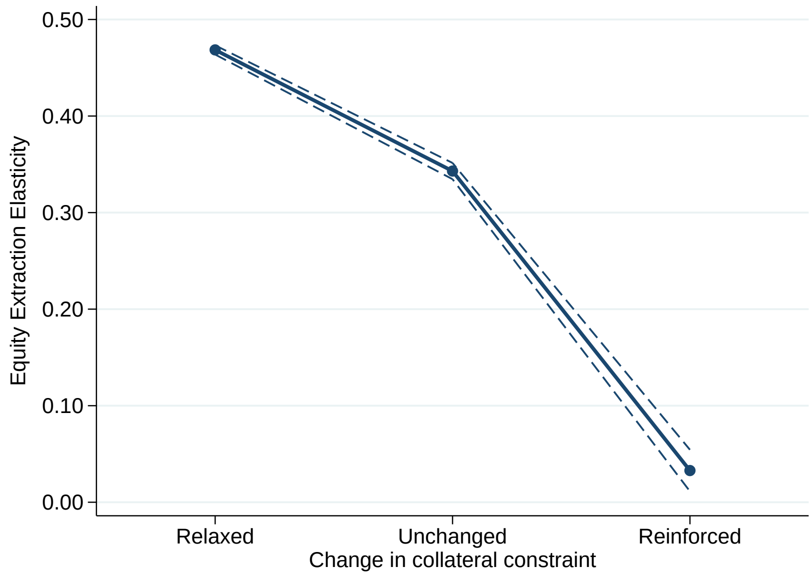
Figure 5.20: Heterogeneity in Borrowing Elasticity by Income and Income Growth



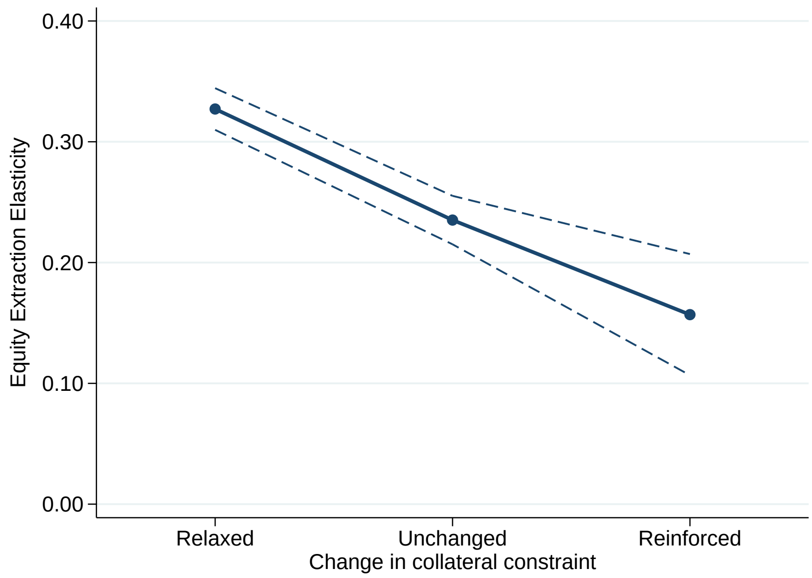
Notes: The figure shows heterogeneity in the equity extraction elasticity by income level (top panels) and by income growth (bottom panels). The income level is measured at the time of the last refinance event, while income growth is measured as the log-change since the last refinance event. The left panels are based on univariate specifications that investigate each dimension of heterogeneity on its own, while the right panels are based on multivariate specifications allowing for heterogeneity in four dimensions simultaneously: LTV, age, income level, and income growth. The multivariate specification is shown in equation (2.7). The dashed lines give 95% confidence intervals based on standard errors clustered by household. The figure shows that there is relatively little heterogeneity in the borrowing elasticity by either income level or income growth.

Figure 5.21: Heterogeneity in Borrowing Elasticity by Notches Moved

A: No Other Controls



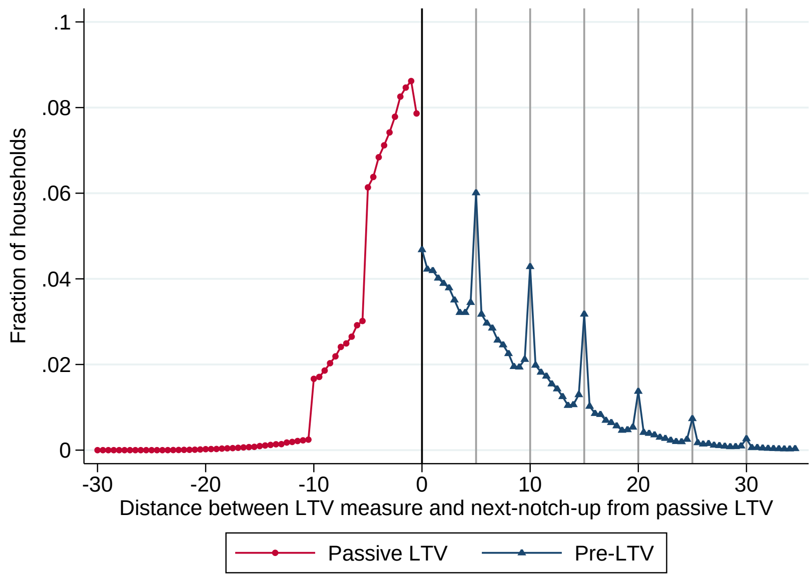
B: Household and Month Fixed Effects



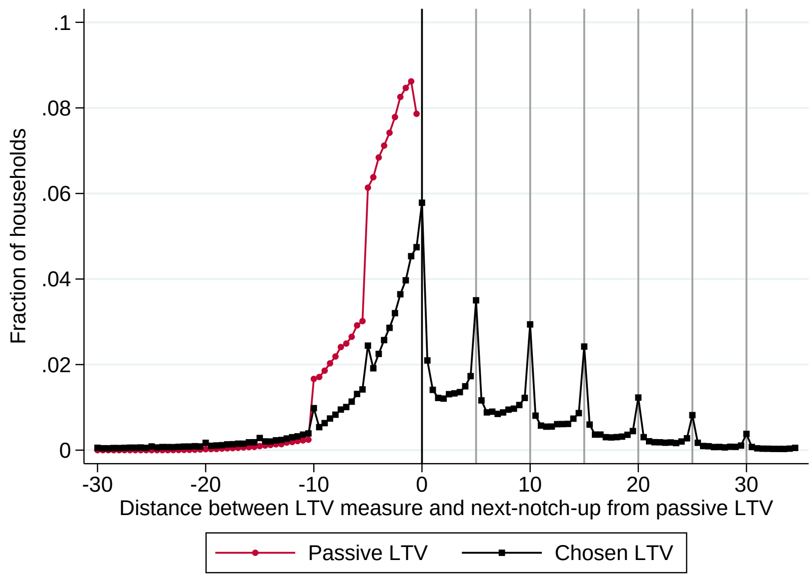
Notes: The figure shows heterogeneity in the equity extraction elasticity by notches moved due to house price changes. There are interest rate notches at LTV thresholds of 50%, 60%, 70%, 75%, 80%, 85%, and 90%. We define the collateral constraint as being relaxed (reinforced) when house price variation moves the homeowner at least one notch down (up) and thus reduces (increases) the interest rate on borrowing. Otherwise, the collateral constraint is defined as "unchanged." Panel A shows elasticity estimates when including no other controls, while Panel B allows for household and month fixed effects. The dashed lines give 95% confidence intervals based on standard errors clustered by household.

Figure 5.22: House Price Growth and Bunching at Collateral Notches

A: Effect of House Price Growth on LTV Distribution



B: Effect of Equity Extraction on LTV Distribution

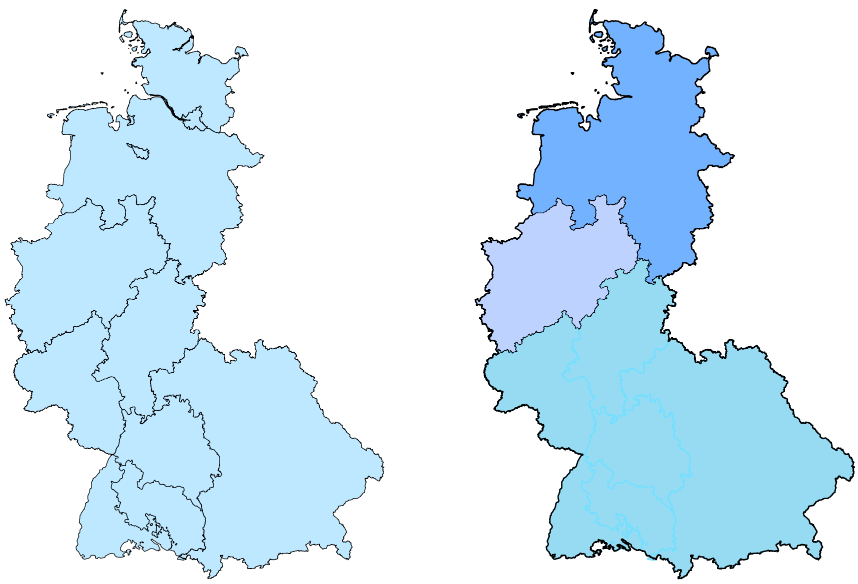


Notes: The figure is based on a sample of households who are pulled down to a lower notch by house price growth. The two panels show density distributions of three different LTV measures. The pre-LTV = D^P_{it}/P_{it-1} is the homeowner's LTV at time t given past mortgage choices (i.e., the debt level and amortization schedule chosen at time $t-1$, not including equity extraction at time t) and the old house price. The passive LTV = D^P_{it}/P_{it} is the homeowner's LTV given past mortgage choices and the new house price. The chosen LTV = D_{it}/P_{it} includes any equity extraction made at time t. The x-axis in each panel represents the distance between a given LTV measure and the next-notch-up from the passive LTV. Panel A illustrates the effects of house price growth by comparing the distributions of pre-LTV and passive LTV. This panel shows that house price growth moves homeowners from the positive to the negative range and eliminates bunching at interest rate notches. Panel B illustrates the effects of borrowing responses by comparing the distributions of the passive LTV and the chosen LTV. This panel shows that equity extraction largely recreates the qualitative pattern that existed before house price growth.

Figure 5.23: Maps of the postwar banking zones

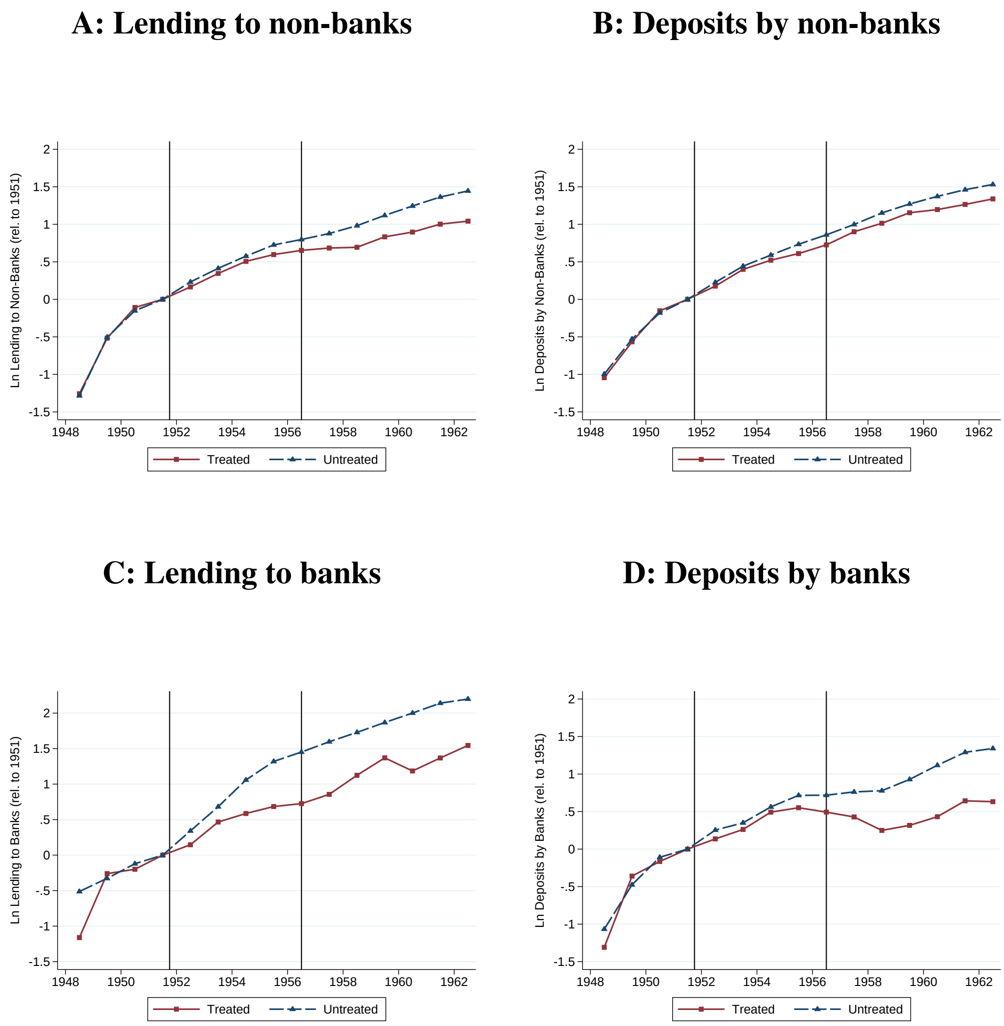
A: 1947/48-1952
State-level breakup

B: 1952-1957
Three banking zones



Notes: The figure shows the two phases of the breakup. The first reform in 1952 lifted the state-level restrictions and allowed banks to operate in three regional zones. The reform in 1957 removed all restrictions.

Figure 5.24: Lending and deposits



Notes: The data are for the December of the given year and provided by the Deutsche Bundesbank. The treated group includes banks affected by the breakup and subsequent reforms. The untreated group includes the untreated commercial banks. The first reform in 1952 lifted the state-level restrictions and introduced zonal restrictions. The reform in 1957 removed all restrictions.

Chapter 6

Appendix

6.1 Firm Summary Statistics

I present summary statistics for the firm panel by six bins of Commerzbank dependence in Appendix Table 6.7. In general, the table shows no linear relationship between Commerzbank dependence and firm characteristics. For instance, mean employment is less than 800 in the top two bins, for firms with Commerzbank dependence over 0.4. Employment is largest for firms in the mid-category, while the bins with low Commerzbank dependence have mean employment between 800 and 1,000. The average wage is fairly stable across the bins. The mean of total liabilities behaves similarly to employment. Firms with no Commerzbank dependence are somewhat of an outlier as they hold a large stock of liabilities given their employment and capital stock. The standard errors are large, however, indicating that the differences between the bins are not statistically significant. To conduct a test with greater statistical power, I pool all firms with a Commerzbank relationship and compare them to firms with zero Commerzbank dependence. I find no statistically significant difference between the two groups (t-statistic: 0.31). Bank loans over total liabilities are similar across bins. This suggests that the degree of Commerzbank dependence is not correlated with firms' dependence on banks.

Appendix Table 6.2 carries out a regression-based test of whether Commerzbank dependence is correlated with firm observables before the lending cut. I regress firm Commerzbank dependence ($CB\,dep_f$) on a cross-section of firm observables from 2006. The coefficients have the interpretation of the approximate change in Commerzbank dependence following a 100 percent increase in the regressor. Only the coefficient on ln capital has a coefficient that is statistically significantly different from zero. The estimate implies that a 100 percent increase in the capital stock is associated with a 0.014 decrease in Commerzbank dependence. There is no difference in the value of financial assets or the amount of bank loans. I therefore conclude that while there are slight differences between firms dependent on Commerzbank and other firms in the firm panel, they are not large.

6.2 Commerzbank's Trading Losses

This section provides more institutional detail on the trading losses that forced Commerzbank to cut lending.

6.2.1 Interpreting Financial Analyst Research Reports

Understanding the details of Commerzbank's trading losses is not trivial, because almost no bank publishes its detailed financial asset holdings. A more promising resource are research reports by financial analysts. I use the investment database Thomson Reuters Investext to extract relevant research reports on Commerzbank before and during the financial crisis. I focus on the period from 2008 to 2009, as these were the loss-making years, extracting all the available reports from Thomson Reuters Investext for this period. I also consider the most relevant reports from the years before and after, to understand the build-up of Commerzbank's trading portfolio and the years after the lending cut. Overall, I analyze the 110 research reports listed at the end of the references section of the Appendix.

I formulate nine questions in Table 6.8 that relate to the origin and nature of Commerzbank's trading losses. For each question, I begin by counting the number of reports that can provide any relevant information to a question. I then categorize the reports into three categories. Either they offer a clear conclusion (Answer yes/no) or they give information without committing either way (Answer unclear).

To illustrate my method, consider question 1 of Table 6.8. This questions asks whether trading income was more volatile at Commerzbank than at other German banks. One report mentions that Commerzbank's trading portfolio remained "resilient when even the large investment banks were struggling", so it gets classified as answering no to question 1 (Kepler Cheuvreux 6/11/2006). Many reports analyze movements in trading income, describing strengths and weaknesses, but do not make an explicit judgment on the relative volatility of the trading portfolio. These get classified as providing an unclear answer to question 1.

Questions 2, 8, and 9 are categorized in the same manner as question 1. Questions 3 to 7 are of a different style, asking whether a certain factor is mentioned explicitly as cause of Commerzbank's losses during the financial crisis of 2008/09. There are no unclear answers for these questions.

Commerzbank announced its acquisition of Dresdner Bank in 2008 and completed it in January 2009. From mid-2008 onward, there are few reports that analyze Dresdner Bank separately, so I report results combining the information for the new, enlarged

Commerzbank for the period after 2008. When I generally refer to Commerzbank, this includes Dresdner Bank.

In what follows, I describe the narrative of Commerzbank's trading losses, drawing on the reports of Table 6.8, financial statements, and additional secondary sources.

6.2.2 The Expansion Into Trading During the Early 2000s

From the early 2000s onward, German banks began increasing their international activities. The main actors were the large commercial banks, Commerzbank, Deutsche Bank, and Dresdner Bank (which was acquired by Commerzbank in January 2009), as well as the publicly owned Landesbanken. Unlike their competitors from France, Spain, and Italy, this internationalization was not driven by retail branching into foreign countries. Instead, German banks focused on trading on international financial markets (Hardie and Howarth 2013).

There was political support for this expansion, as Germany was suffering from anemic growth and a recession in 2003. Politicians hoped trading profits would allow banks to raise credit supply. For example, the federal 2003 *Kleinunternehmerförderungsgesetz* (law for the promotion of small businesses) introduced tax benefits for financial institutions involved in securitization, and the 2005 coalition agreement mentioned the development of securitization markets as a policy goal. The securitization of German assets had only been legally regulated from 1997, so these markets were small and unimportant before and during the financial crisis of 2008/09.

Commerzbank took part in this trading expansion, but not to an extraordinary degree relative to the other banks. The share of trading assets out of total assets at Commerzbank rose from 12 percent in 1999 to 22 percent in 2005, the eve of the United States subprime mortgage crisis. The other two large commercial banks had a bigger trading division than Commerzbank already in the 1990s, because Commerzbank's historic focus had been corporate credit. Dresdner Bank's share of trading assets out of total assets was 35 percent in 2005 (1999 data unavailable), and Deutsche Bank went from 27 percent in 1999 to 45 percent in 2005 (source: bank annual reports). For the Landesbanken, there was a similar range, with HSH Nordbank at 13.4 percent in 2006 and WestLB at 32.5 in 2007 (Hardie and Howarth 2013).

Commerzbank's and Dresdner Bank's increased trading activities coincided with two developments on financial markets. First, the rise of subprime mortgage lending in the United States, which peaked in 2006. German banks invested heavily in investment-grade-rated asset-backed securities based on the United States mortgage market and sold by American investment banks. Second, the expansion of the Icelandic banking sector. The total assets of Icelandic banks increased more than sixfold (in real terms) between 2003 and 2007 and their total assets grew to 10 times the value of Icelandic GDP. The Icelandic banks relied on financing from European bond markets, interbank credit lines, and wholesale market funding (Flannery 2009). By lending to the Icelandic banks, Commerzbank became more exposed to Iceland than the other German banks. However, this was not considered a risky strategy by the analysts at the time.

For the period 2004 to 2007, the research reports relevant to question 1 of Table 6.8 do not suggest that Commerzbank's and Dresdner Bank's trading income was more volatile or riskier than trading income of Deutsche Bank or the Landesbanken. Nine reports describe the year-by-year changes in trading income at different banks without identifying which banks were more volatile. I classify reports of this kind as giving no clear answer. If indeed there was excess volatility in trading incomes or if analysts believed that the trading portfolio was riskier, one would have expected the analysts to mention this in the reports. The lack of a clear statement can therefore be interpreted as evidence against higher volatility at Commerzbank and Dresdner Bank. Two of the reports mention that Commerzbank's trading income was stable relative to the other banks ("normal trading profit" Deutsche Bank Equity Research 7/02/2006; "trading result continued its remarkable stability" Kepler Cheuvreux 6/11/2006).

The capital ratios of German banks strengthen the impression that Commerzbank did not take on more risk than other German banks before the crisis. In 2005, the tier 1 capital ratio at Commerzbank was at 8 percent, Dresdner Bank at 10 percent, Deutsche Bank at 8.7 percent, and the aggregate of German banks at 7.8 percent.

6.2.3 The Relation Between Trading and Loan Portfolios

Question 2 of Table 6.8 asks whether the loan portfolios of Commerzbank and Dresdner Bank were riskier or more cyclical than other banks'. The answer is no. The research reports considered the loan portfolios of Commerzbank and Dresdner Bank a source of income stability and strength. The reports argue that the banks' long-term banking relationships to firms and households were reliable sources of income, because the German market is based on relationship lending and because the German economy is relatively stable. (For example: "We like Commerzbank, which benefits from relatively high exposure to German corporate lending." Deutsche Bank Equity Research 16/01/2004; Commerzbank's "strong progression in Mittelstand" JPMorgan 10/08/2007; Dresdner Bank's "retail client base is an important lever for revenues" Natixis 22/11/2006). In particular, Commerzbank was known for its strong position in the Mittelstand, the German group of small and medium-sized firms ("firmly established relationships with this client group, which is not easily penetrated by the large international banks, but has demand for a broad range of lucrative products." Bear Stearns & Co. Inc. 5/09/2005). Figure 5.2 confirms the remarkable stability of interest income before the lending cut.

There is no evidence in any of the reports that Commerzbank's or Dresdner Bank's trading portfolios were supposed to hedge the loan portfolio (question 3 of Table 6.8). The reports analyze the income streams for the lending division entirely separately from the trading and investment banking divisions ("conceptually separate Commerzbank into three banks" CA Cheuvreux 13/11/2008). One would have expected the bank management to point out cross-hedges between the lending and the trading portfolios in their communication to the analysts, in order to convince them that overall income was relatively stable. The fact that they did not suggests there were no such hedges.

Figure 5.2 shows that trading income varied in every year between 2004 to 2008, while net interest income remained on a gentle upward trend throughout the period. Following the trading losses in 2008, we would have expected the performance of firms dependent on Commerzbank and net interest income to improve, if there had been a hedging relationship. Instead, there was initially no change in 2008, followed by the firms underperforming and net interest income slowly declining in the following years. Thus the behavior of trading and net interest income confirms that there was no hedging relationship.

6.2.4 The Trading Losses 2007-09

Why did Commerzbank suffer severe losses during the financial crisis? None of the 83 relevant reports I examined blame the losses on the German loan portfolio (question 4 in Table 6.8). Given the discussion in the previous subsection on the nature of the loan portfolio and the stability of net interest income, this is not surprising. Several reports praise the income generated by the corporate loan and retail divisions from 2007 until the final quarter of 2008, even as trading losses were unfolding. (For example: "Mittelstand once again with a strong performance" ESN/equinet Bank 4/11/2008; Dresdner's "retail business continues to generate healthy returns" Deutsche Bank Equity Research 28/02/2008).

87 percent of reports explicitly mention losses and write-downs in asset-backed securities (ABS) related to the United States subprime mortgage crisis as loss drivers at Commerzbank and Dresdner Bank. These ABS include collateralized debt obligations, residential mortgage-backed securities, and credit default swaps. As the price of the ABS fell, the banks had to write down their values and sell at a loss. The research reports cite figures released by the banks to financial analysts to underscore the influence of the ABS on the banks. Dresdner Bank lost 1.3 billion Euro on its ABS trading portfolio in 2007, which on its own can explain around 75 percent of the difference in its trading income to the previous year. The remainder is accounted for by spill-

over effects from the subprime mortgage crisis to other financial markets, as liquidity and confidence in trading markets declined (breakdown of figures in CA Cheuvreux 24/04/2008). The story for Commerzbank is similar, as around 84 percent of its 2007 trading losses are due to losses in subprime ABS (Credit Suisse - Europe 25/03/2008).

By mid-2008, Commerzbank and Dresdner Bank were severely weakened, but there were no acute fears of bankruptcy. They were in a similar position to the other German banks (Commerzbank "handled the financial crisis relatively well" Kepler Cheuvreux 7/08/2008; "Dresdner has not done worse than other banks" Deutsche Bank Equity Research 28/02/2008). This changed when Lehman Brothers declared insolvency on 15 September 2008. As wholesale funding markets froze, the three large Icelandic banks were taken into government custody in October 2008, and their international creditors lost their deposits. Figures released to analysts by Commerzbank and Dresdner Bank confirm that the bulk of the losses in 2008 and 2009 can be explained by the ABS trading portfolios and items that had to be written down because of Lehman Brothers' and the Icelandic banks' insolvency (see, for instance, ESN 1/12/2009 and Credit Suisse - Europe 26/02/2009). These were the main factors behind the equity capital shortages at Commerzbank and Dresdner Bank (questions 5 to 7 in Table 6.8).

The importance of the insolvency of Lehman Brothers and the Icelandic banks can be seen in the timing of the 2008 quarterly results. Both Commerzbank and Dresdner Bank achieved positive earnings in the first and second quarters. The significant 2008 losses that we see in Figure 5.2 are entirely driven by third and fourth quarter trading losses and write-downs. Losses related to ABS write-downs continued throughout 2009.

The German bond markets did not deteriorate in this period, so Commerzbank's and Dresdner Bank's ABS losses were unrelated to the German economy. Germany saw a low default rate of around 0.3 percent for securitized transactions issued between 2005 and 2007, while in the United States subprime mortgage market the default rate was around 20 percent (International Monetary Fund 2011). The index for German mortgage covered bonds (iBoxx Euro Hypothekenpfandbriefe) rose by 18 percent between the end of 2006 and 2009. The index for German corporate bonds (RDAX) gained 17 percent in the same period. In comparison, the index for US AAA-rated subprime ABS (ABX.HE-AAA 07-1) fell by around 65 percent and the index for A-rated subprime ABS (ABX.HE-A 07-1) by over 95 percent.

The reason for the trading losses was the failure of the management of Commerzbank and Dresdner Bank to recognize the institutional instability that the financial crisis had caused in other institutions. Commerzbank wrote in its 2008 annual report: "We were encouraged by the US Treasury Department's rescue of Bear Stearns and for too long shared the market's mistaken belief that Lehman was too big to fail." Similarly, it had been too tentative in reducing its exposure to the Icelandic banks.

This is what differentiated it from Deutsche Bank, which profited from consequently hedging its ABS portfolio and shorting the subprime mortgage market, after the first signs of distress became apparent in 2007 (Fox-Pitt Kelton Cochran Caronia Waller 2/01/2008; O'Donnell and Nann 2008; Landler 2008). A number of Landesbanken followed a similar trading strategy as Commerzbank, for example Bayern LB, Sachsen LB, and West LB. However, they were publicly owned, and could rely on quick government funding at all stages of the crisis, preventing equity capital shortages and hence a lending cut (see 6.5 for details on the Landesbanken).

6.2.5 Commerzbank's 2009 Acquisition of Dresdner Bank

The insurance company Allianz had acquired Dresdner Bank in 2001. The aim was to exploit economies of scale and build a nationwide branch network offering "bankassurance", the combined retail of banking and insurance products. By 2007, it became clear that the plan had failed. The research reports and the media blamed management errors and the complexity of the task of merging the world's largest insurer with Germany's third-largest bank (CA Cheuvreux 24/04/2008). In late 2007 Allianz decided to give up the plan of "bankassurance", sell Dresdner, and refocus on its core business of insurance.

Commerzbank's management had first expressed interest in expanding in 2007. Commerzbank wanted to enlarge its German retail banking customer base and it was worried about being a takeover target itself (Schultz 2008). Dresdner Bank, with its solid and traditional retail banking division, was a natural option. The proposed acquisition got much political support, as German politicians were fond of the idea of a second "national banking champion", next to Deutsche Bank. German finance minister Steinbrück and Commerzbank head Blessing appeared on national television together to explain the deal.

Commerzbank and Dresdner Bank had got relatively well through the first two quarters of 2008. The acquisition plan was announced on 31 August 2008 and to be completed on 12 January of 2009. The analyst reports welcomed the deal. Out of eleven reports released around the time of the announcement, nine were explicitly positive (question 8 in Table 6.8). Morgan Stanley, for instance, welcomed the deal as "making perfect strategic sense" (Morgan Stanley 1/09/2008). One report delivered no clear judgment, and one argued the purchase price Commerzbank had to pay was too high.

The unexpected Lehman Brothers bankruptcy threw both banks into severe financial distress. Given their similar trading strategy discussed in the previous subsection, it is not surprising that the Commerzbank and Dresdner Bank contributed approximately evenly to the 12 billion Euro in negative profits and write-downs of the combined, enlarged Commerzbank in 2008 (based on my own calculations using the banks' annual reports). 48 percent of the12 billion Euro were due to operations at the "old" Commerzbank and 52 percent due to the "old" Dresdner Bank. It is thus likely that both banks would have had to cut lending even if it had not been for the acquisition. Testing for heterogeneity, I find that the lending cut affected firms and counties similarly, independent of whether they were initially served by Commerzbank or Dresdner Bank.

6.2.6 Recovery by 2011

The German government fund Soffin supported Commerzbank twice, on 3 November 2008 and on 8 January 2009, but was unable to entirely prevent a lending cut. Overall, Soffin provided Commerzbank with 18.2 billion Euro in equity and bought a 25 percent stake in the bank, around two-thirds of Soffin's total engagement. Commerzbank was the only large lender in Germany to be subsidized by Soffin. Only three other, specialized banks received capital from Soffin (two smaller real estate banks, Aareal Bank and Hypo Real Estate Group, and the former Landesbank West LB/Portigon), which shows that Commerzbank was uniquely affected.

The equity capital losses had forced Commerzbank to shrink its assets, in order to improve the tier 1 capital ratio, reduce risk exposure, and gain the trust of investors. This resulted in a lending cut to its customers in 2009 and 2010. The Commerzbank management subsequently refocused the bank on its core business of lending to German firms and households, whilst downsizing the trading and investment banking division. The research reports generally comment favorably on the success of the new strategy (question 9 in Table 6.8). Losses due to the subprime mortgage crisis are not mentioned anymore from 2011. One key piece of evidence for Commerzbank's recovery is that around 14.3 billion of the 18.2 billion in equity had been repaid by Commerzbank to the government by mid-2011. From 2010 onward, lending by Commerzbank moved in parallel to other commercial banks once again (Figure 5.1).

6.3 Further Firm Survey Results

Appendix Table 6.9 reports robustness checks on the survey results of Section 1.4.1. Column (1) shows that the effect in 2009 is not driven by the inclusion of the lagged dependent variable from 2006. The effect also remains stable and statistically significant at the 10 percent level when including county fixed effects in column (2). The year 2003 is an interesting comparison to 2009, because it was also a recessionary year. It is the first year, in which the question on bank loans was asked in the survey. The results in columns (3) to (6) of Appendix Table 6.9 show no association between Commerzbank dependence and bank loan supply or firms' product demand conditions in 2003. This implies that Commerzbank's loan supply was not more cyclical than other banks'. It also suggests that firms dependent on Commerzbank did not face different demand conditions in recessions.

I examine three survey questions on demand conditions, to test whether differences in product demand might affect the performance of firms dependent on Commerzbank. Appendix Table 6.10 analyzes responses to the question "Are your business activities constrained by low demand or too few orders: yes or no?", Table 6.11 to "Currently we perceive our backlog of orders to be: comparatively large, sufficient / typical for the season, or too small?", and Table 6.12 to "Tendencies in the previous month - The demand situation has: improved, remained unchanged, or deteriorated?". Firms are asked these questions at multiple times during the year, so I use the annual average of responses as outcome variable in the regressions. For these demand questions, none of the coefficients on Commerzbank dependence are statistically significant in any year, and most are of small magnitude. This indicates that neither before, during, or after Commerzbank's lending cut were there differences in the product demand for firms dependent on Commerzbank.

6.4 Firm Financial Assets

The bulk of Commerzbank's trading losses occurred between 2007 and 2009. I test whether firms dependent on Commerzbank experienced a decrease in the value of their financial assets at the same time. If Commerzbank gave firms investment advice correlated with the strategy of its own trading division, one would expect such an effect.

Appendix Table 6.13 presents the results. The outcome is the symmetric growth rate of the value of the firm's financial assets in the given period. If a firm begins and ends the period with no financial assets, the growth rate is set to zero. There is no association between Commerzbank dependence and the change in financial assets from 2007 to 2009. The insignificant point estimate in column (2) implies that the growth of financial assets from 2007 to 2009 at a firm fully dependent on Commerzbank was 3.6 percentage points higher than at a firm with no Commerzbank relationship. This result makes sense, given that the analyst reports presented in 6.2 suggest there was little coordination across the trading and corporate lending divisions at Commerzbank. Columns (1) analyzes the year before 2007, column (3) the year after 2009, column (4) a bivariate specification without controls, and column (5) adds county fixed effects. There is no significant effect in any specification.

6.5 An Identification Strategy Based on Savings Banks' Support to the Landesbanken

6.5.1 The Literature Analyzing Affected Savings Banks

Germany has eleven Landesbanken. Each operates in a restricted region, either one federal state or a group of states. The Landesbanken are jointly owned by the federal states and the savings banks of their region. During the financial crisis, five Landesbanken announced significant losses in their trading portfolios: Sachsen LB, HSH Nordbank, WestLB, Bayern LB, and Landesbank Baden-Württemberg. Following Popov and Rocholl (2015), I define a savings bank to be "affected" if it owns one of the five Landesbanken with trading losses during the crisis.

Puri, Rocholl, and Steffen (2011), Hochfellner, Montes, Schmalz, and Sosyura (2015), and Popov and Rocholl (2015) argue that the affected savings banks financially supported the Landesbanken they owned, and that this led the savings banks to cut lending. Below, I add further evidence to their analysis. First, I find little evidence that affected savings banks contributed significantly to the support measures to the Landesbanken, lost equity capital, or reduced lending following losses at their Landesbanken.[1] Second, I replicate the findings in Popov and Rocholl (2015) (henceforth PR). I show that the correlation between firm performance and affected savings banks disappears once I add the firm-level controls I use in my paper. There is also no association between firm growth and having an affected Landesbank as relationship bank, and there is no effect on counties.

6.5.2 The Public Support Measures to the Landesbanken

6.5.2.1 Support to Sachsen LB

A detailed narrative for the case of Sachsen LB, the first Landesbank to announce losses, is available from the European Commission investigation report on whether the public support given to Sachsen LB constituted illegal state aid (Kroes 2008). In the middle of August 2007, financial markets became suspicious that Sachsen LB was heavily affected by the subprime mortgage crisis. The bank was unable to finance itself on wholesale markets as a result.

On 17 August, the funding problems were publicly announced. On the same day, German banking regulators, the state government of Saxony, and representatives of the savings banks and other Landesbanken agreed that the other Landesbanken and Deka-Bank (jointly owned by all the German Landesbanken and all German savings banks) would purchase a set of subprime assets from Sachsen LB. On 26 August, the Landes-

[1] A research report by Fitch confirms this: "Sparkassen-Finanzgruppe Vollständiger Ratingbericht", 15 July 2014, page 16

bank Baden-Württemberg agreed to take over Sachsen LB and immediately injected capital. When further unexpected losses arose in late 2007, the state government of Saxony provided a guarantee for losses from Sachsen LB's securities portfolio of 2.75 billion Euro to Landesbank Baden-Württemberg, in addition to financing a separate investment vehicle that contained troubled assets with 8.75 billion. Sachsen LB and Landesbank Baden-Württemberg were not required to pay back the public funding. Because Sachsen LB was publicly owned, the public support measures were decided within days after it ran into difficulties. There was only a very short period of distress, during which Sachsen LB and the associated savings would have had time to cut lending.

The European Commission does not mention any capital injections or guarantees by the regional savings banks of Saxony to Sachsen LB. The annual report of the savings banks that partially owned Sachsen LB (Sachsen Finanzgruppe Geschäftsbericht 2007, page 4) reports "the sale of Sachsen LB produced no financial burden for the savings banks." The average equity capital of the savings banks that partially owned Sachsen LB grew by 8 percent in 2007, the year of Sachsen LB's distress and subsequent sale. As comparison, Commerzbank lost 68 percent of its equity capital from 2007 to 2009. The aggregate equity capital of German banks except Commerzbank rose by seven percent from 2007 to 2009. Overall, there is little evidence to suggests that the savings banks were strongly affected by the losses at Sachsen LB.

6.5.2.2 Support to HSH Nordbank

In 2008, the owners of HSH Nordbank provided 2 billion Euro of equity capital to the bank (Almunia 2011a). The savings bank association of Schleswig-Holstein contributed 78 million Euro of this in the form of silent participation and 170 million Euro in the form of a convertible bond. Following further losses, a second rescue package in 2009 included 3 billion Euro in equity capital and liquidity guarantees totaling 27 billion. The savings banks did not participate in this second package. The contribution of the savings banks to the support measures to HSH Nordbank amounted to less than one percent of the total package and to 0.7 percent of the savings banks' 2008 total assets. Lending to businesses by the savings banks of Schleswig-Holstein rose by 3.8 percent and new mortgage issuance rose by 17 percent in 2008 (data from the annual reports).

6.5.2.3 Support to West LB

The European Commission (Almunia 2011b) reports two support measures for WestLB from 2007 to 2010. The first measure in January 2008 was a guarantee to secure toxic assets held in WestLB's subsidiary Phoenix Light. The savings banks association of North-Rhine Westphalia guaranteed 1 billion Euro. The federal state and municipal governments guaranteed 4 billion Euro.

The second measure in November 2009 involved a 3 billion Euro capital injection by Soffin, the German government fund. In addition, it was agreed that the savings banks would only be responsible for 4.5 billion Euro of losses, independent of what the actual requirements of WestLB would be. These 4.5 billion Euro would have to be paid only after 25 years. In the meantime, the government would guarantee for the amount. Under standard financial regulations, the savings banks would have been responsible for 50 percent of losses immediately, as they held a 50 percent stake in WestLB. The combined equity capital of savings banks in 2008 was 14.4 billion Euro. This capital buffer and the possibility to accrue earnings over 25 years before paying for losses ensured the savings banks would not become insolvent due to their involvement with WestLB. The support measures for WestLB occurred in 2008 and 2009. Between the end of 2007 and 2009, the aggregate equity capital of savings banks in North-Rhine Westphalia rose by 11 percent.

6.5.2.4 Support to Bayern LB

Bayern LB reported losses from its exposure to asset-backed securities starting in February 2008. In December 2008, Bayern LB received 10 billion Euro in equity capital and a guarantee for losses of 4.8 billion from the federal state government of Bavaria. The savings bank association of Bavaria did not contribute to these measures (Almunia 2013). The losses at Bayern LB led to write-downs of a moderate size at the Bavarian savings banks, a total of 0.5 billion Euro in the year 2008, relative to total assets of 160 billion Euro (Krämer 2009). All Bavarian savings banks recorded a positive profit for 2008. The annual reports of Bayern LB state that aggregate loans by the savings banks in Bavaria rose by 4 percent between the end of 2007 and 2009.

6.5.2.5 Support to Landesbank Baden-Württemberg

Until late 2008, Landesbank Baden-Württemberg had not recorded serious losses. It was perceived strong enough by its management to take over Sachsen LB in 2007 (Kroes 2009). But after the Lehman Brothers insolvency, Landesbank Baden-Württemberg urgently required funding due to write-downs and trading losses on securities. On 21 November, Landesbank Baden-Württemberg announced that it would receive 5 billion Euro in equity capital from its owners. The contribution was in proportion to the ownership share (Gubitz 2013). The state's savings banks association owned 35.6 percent of Landesbank Baden-Württemberg and therefore contributed 1.8 billion Euro. This is not a negligible amount, considering the aggregate equity capital of the savings banks in Baden-Württemberg was 7.1 billion Euro at the end of 2007.

Nevertheless, between the end of 2007 and 2009, the aggregate equity capital of savings banks in Baden-Württemberg rose by 6 percent. Lending to non-banks increased by 5 percent (data from the annual reports).

6.5.2.6 Lending by the Affected Savings Banks

I analyze the Bureau van Dijk database Bankscope, which reports the lending stock for over 90 percent of the German savings banks.[2] I find that the affected savings banks, on average, increased their lending to non-financial customers by 2 percent between 2006 and 2008, and by 7 percent from 2006 and 2010. This suggests they did not cut lending. To test this conclusion further, I run bank-level regressions of the growth of lending on a dummy for affected savings banks. I use the change in lending between 2006 and 2010 as outcome.

The results are in Appendix Table 6.14. Column (1) compares the affected to unaffected savings banks. Savings banks across Germany are similar in structure, scope, and customer type, so this is a natural comparison. Affected savings banks grew their lending by 8 percent more relative to the unaffected.[3] Column (2) compares the affected savings banks to all similar banks, by adding dummies for bank size, federal state, cooperative banks, real estate banks, and commercial banks. Column (3) controls for the pre-trend. The outcome in column (4) is the change in lending between 2006 and 2008. Column (5) uses the symmetric growth of lending between 2006 and 2010 as outcome to limit the influence of outliers. There is no evidence in any specification that affected savings banks reduced their lending relative to other banks.

The savings banks that owned WestLB and Landesbank Baden-Württemberg contributed more to the rescue of their respective Landesbanken than the other affected savings banks, as I describe above. I add a dummy for affected savings banks in these two regions in column (6). The point estimate is positive, small, and insignificant, which indicates no difference in loan growth.

6.5.3 The Relationship Between Affected Savings Banks and Firm Employment

The results on equity capital and lending in the previous subsection raise the question whether the correlation between relationship to an affected savings bank and firm employment losses in PR can be interpreted as a causal effect. I extend the analysis in PR to examine this question. I replicate the sample in PR using the description in their paper. I use my Creditreform dataset to identify firms' relationship banks in the year 2006. The treatment variable is a dummy for whether a firm has an affected savings

[2]Bankscope also includes information on the history of the banks, including bank mergers. I hand-code all mergers since 2006 based on this information. For the years before a merger, I sum the lending stock of the merging banks, and keep one observation per institution, as of 2012.

[3]The results are unchanged when I weight regressions by the banks' lending stock in 2006.

bank among its relationship banks, interacted with a dummy for the treatment period in PR, the years 2009 to 2012.

PR present their main results in Table 3 of their paper. They find that firms with an affected savings bank among their relationship banks reduced employment by an average of 1.1 percent in the period 2009 to 2012. The results of my replication exercise are in Appendix Table 6.15. In all the regressions, standard errors are clustered at the level of the firm. Columns (3) to (7) estimate panel specifications identical to PR. The point estimate in column (3) implies an employment loss of 0.5 percent at firms with an affected savings bank among their relationship banks. Columns (1) to (2) of Table 6.15 estimate cross-sectional specifications, using my large employment cross-section dataset. The outcome is the ln employment difference between 2008 and 2012, which corresponds to the ln outcome variable in PR. The estimate in column (1) implies that firms with an affected savings bank among their relationship banks experienced an employment loss of 1.5 percent. The coefficient is statistically significant at the 1 percent level. Hence, I can replicate their findings.

I propose two additional control variables. These are the age and industry of the firm, measured in the year 2006. Firm age is important because the literature has frequently found correlations between age and growth (Haltiwanger, Jarmin, and Miranda 2013). In my data, dependence on an affected savings bank is positively and significantly correlated with age, even when conditioning on firm size. The reason is that savings banks traditionally have a public mandate to lend to business startups. I control for industry at the two-digit level of the German classification scheme WZ2008. Since savings banks only operate in their municipality, differences in the industrial composition of the municipal economy will lead to differences in the exposure of banks to industries. Controlling for ln age and industry shrinks the estimate in the employment cross-section dataset in column (2) towards zero, and it becomes statistically insignificant. Similarly, the point estimate in the panel specification of column (4) switches sign to positive, is of small magnitude, and insignificant. The 95 percent confidence interval in column (4) excludes employment losses greater than 0.5 percent. The coefficient on age has the expected negative sign and is significant.

Column (5) uses fixed effects for age bins, rather than ln age, to control for age-related differences in employment growth. The three age bins are for firms founded before 1990, from 1990 to 2000, and after 2000. The coefficient on savings banks remains small, positive, and statistically insignificant. Column (6) adds a number of controls that PR propose: the natural logarithm of firm assets, the capital-to-assets ratio, the profit-to-assets ratio, and the cash flow-to-assets ratio. To measure profits, I use the German balance sheet item Betriebsergebnis and to measure cash-flow I use Jahresüberschuss. PR control for the annual, time-varying value of these variables. This could be problematic, because assets, capital, profit, and cash-flow are likely to be outcomes of a credit shock. The coefficient on the affected savings banks in column (6) remains positive, but becomes statistically significant, suggesting the estimates are biased.

In column (7), I add a dummy to the specification that indicates whether the firm has a Commerzbank branch among its relationship banks, interacted with a post-treatment dummy. This measures a firm's relationship to Commerzbank the same way that PR measure a firm's relationship to an affected savings bank. The coefficient is significant at the 1 percent level. It implies that firms with Commerzbank as one of their relationship banks reduced employment by 1.9 percent. I also test whether firms that had one of the affected Landesbanken as relationship bank reduced employment. The coefficient is close to zero and statistically insignificant.

6.5.4 The Relationship Between Affected Savings Banks, Regional Growth, and Household Debt

I call a county "affected" if it is served by one of the affected savings banks. I test if affected counties grew more slowly using a county panel specification, such as the one in Table 5.8, column (1). The coefficient on the dummy for affected counties is 0.009 (standard error: 0.008). Thus, there is no effect of dependence on affected savings banks on county growth.

I examine the relationship between household debt and affected savings banks by using the nationally representative GSOEP. Around one-third of total bank loans to German households are issued by the savings banks and Landesbanken, so changes in their household loan supply may have significant consequences. The regressions I run are equivalent to the ones I report in Table 5.5 of my paper. The outcome is the symmetric growth rate of private debt from 2007 to 2012. 97 percent of GSOEP respondents entered the information before August 2007, so the observation for 2007 represents the state before the losses at the Landesbanken were announced. The regressor of interest is a dummy for individuals in affected counties. The coefficient on the dummy is small and insignificant at -0.01 (standard error: 0.03). Controlling for ln mortgage debt in 2002, ln other debt in 2002, and a dummy for any debt in 2002, the coefficient on the dummy becomes positive, but remains insignificant and small (point estimate: 0.01, standard error: 0.03). This suggests that household debt in the affected counties did not change.

6.6 An Identification Strategy Based on Other Banks' Trading Losses

6.6.1 The Literature on Other Banks with Trading Losses

A recent paper by Dwenger, Fossen, and Simmler (2015) (henceforth DFS) uses two instruments to identify exogenous variation in German firms' bank loan supply in the recent crisis. The first is a firm's dependence on an affected savings bank, which is the same variation PR use. I discuss this in detail in 6.5. The second instrument in DFS is the average of the trading losses of the firm's relationship banks. In their Table 1, DFS list the main German banks affected by trading losses. The table includes a number of Landesbanken, IKB, Deutsche Bank, HypoVereinsbank, DZ Bank, KfW, and Commerzbank (including Dresdner Bank).

Below, I extend the analysis in DFS by showing that their results are entirely driven by Commerzbank's lending cut. I find no evidence for a lending cut by any other bank. I then explain why the trading losses did not force other banks to cut lending. A number of institutional details played a role, such as a banks' hedging strategies, ownership structures, and pre-crisis capital buffers.

6.6.2 Replicating the Dataset of DFS

I follow Section 3 and Footnote 27 of DFS to replicate their dataset. Their sample spans the years 2006 to 2010. As first regressor, I calculate the firm's fraction of relationship banks that had trading losses, out of all the firm's relationship banks. I call this the firm's dependence on banks with trading losses. I define banks with trading losses as the banks listed in Table 1 of DFS. As an example: If a firm has two relationship banks, one being IKB and the other Commerzbank, the dependence on banks with trading losses would be 1. I also calculate the firm's dependence on all the other banks with trading losses, except Commerzbank. The firm from the previous example would have a value of 0.5 for this measure. DFS use two outcome variables, the ln annual growth rates of employment and fixed assets.

6.6.3 The Relationship Between Banks with Trading Losses and Firm Employment

Appendix Table 6.16 presents results for the type of specification used by DFS. Column (1) shows a negative and statistically significant effect on employment of dependence on a bank with trading losses. It implies that the annual growth rate of employment at a firm fully dependent on banks with trading losses was 1.2 percentage points lower in the years 2006 to 2010. This is the reduced-form effect that DFS capture in their IV specification of their Table 5. Column (2) tests the robustness of the coefficient by adding the firm controls from my paper. These controls are not in DFS. The coefficient falls to one-third of its value and becomes statistically insignificant.

In column (3), I split the regressor into two. I include my measure of firm Commerzbank dependence and the measure of dependence on all the other banks with trading losses, except Commerzbank. The coefficient on Commerzbank is negative and statistically significant. It implies a reduction in the annual employment growth of firms entirely dependent on Commerzbank by 1.1 percentage points.[4] The point estimate on the measure of dependence on the other banks with trading losses is positive, small, and insignificant. Columns (4) and (5) replace the interaction dummy d with a dummy for the years 2008 to 2010 and a dummy for 2007 to 2010, respectively. This tests whether the other banks had an effect in the early years of the financial crisis. I find no effect. In column (6), I add the lagged growth rate of sales to the specification, as suggested by DFS. I also add county fixed effects interacted with d. This controls for cross-regional differences, for example due to regional demand shocks or differences in business regulation. The coefficients remain similar.

I investigate whether the zero coefficient on the other banks with trading losses masks heterogeneous effects across the individual banks. I have already examined the affected Landesbanken in 6.5, so here I focus on the other banks mentioned in Table 1 of DFS. I add measures of dependence on each of these banks to the regression in column (7). None of the point estimates are statistically significant and they all imply smaller losses than the coefficient on Commerzbank dependence. In column (8), I use the annual growth rate of fixed assets as the outcome variable and run the same specification. The results confirm that there was no significant effect of dependence on these banks on firm growth.

The first three columns of Appendix Table 6.17 re-examine the employment effect of dependence on banks with trading losses using the sample and specification of my large employment cross-section. The results are similar to what I find when I use the sample and specification of DFS.

As a final check, I run county-level regressions analogous to the ones reported in Table 5.8. The outcome is ln county GDP. The regressor of interest is the average dependence of firms in the county on other banks with trading losses, except Commerzbank, interacted with a dummy for the years 2009 to 2012. I find a small and insignificant coefficient on the county dependence on these other banks with trading losses, in unreported results. The effect of county Commerzbank dependence in the same regression remains robust.

[4]The coefficients in Table 5.6 refer to the employment loss over four years, while this point estimate refers to the annual loss. Therefore, both types of regression estimate an employment loss between 4 to 5 percent from Commerzbank's lending cut, despite the considerable differences in sampling design and specification.

6.6.4 Institutional Details on the Other Banks With Trading Losses

I briefly explain why trading losses at these other banks did not have effects on firms. The case of KfW is similar to the Landesbanken discussed in 6.5. It is the national development bank, jointly owned by the government of Germany and the federal states. When trading losses at KfW became apparent, the government immediately stepped in. In fact, KfW was charged with several public credit extension programs to help households during the financial crisis. For example, KfW raised its mortgage commitments to households by 26.5 percent during the crisis.

IKB does not play an important role in the loan supply of German firms. In my Creditreform sample of relationship banks, only 0.1 percent of firms list IKB as one of their relationship banks. For the firms that do have an IKB relationship, over 90 percent have at least two other relationship banks. Therefore, when IKB became financially affected, firms were able to switch to their other relationship lenders. Similarly, in Table 5.6 I find that firms with positive, but low Commerzbank dependent did not cut employment following Commerzbank's lending cut.

DZ Bank and HypoVereinsbank had large equity capital buffers, so they were able to absorb trading losses relatively well. The tier 1 capital ratio at DZ Bank was 14 percent in 2006. DZ Bank is the central bank of the cooperative sector and owned by the cooperative banks, which were not generally affected by the crisis and would have been able to provide support in the hypothetical scenario of a capital shortage. Similarly, the tier 1 capital ratio of HypoVereinsbank was 15.7 percent in 2006. HypoVereinsbank is part of the international UniCredit Group, which eased its access to funding.

Deutsche Bank profited from consequently hedging its ABS portfolio and shorting the subprime mortgage market, after the first signs of distress became apparent in 2007 (see the research report by Fox-Pitt Kelton Cochran Caronia Waller, "European Banks: Credit Crisis - Stock Impact", 2 January 2008). While it made losses on the ABS trading portfolio, these were evened out by its hedging strategy. This enabled Deutsche Bank to expand its lending in Germany during the financial crisis. For example, mortgage lending in its private customer division rose by 21.7 percent between 2007 and 2010.[5]

[5] The point estimates on Deutsche Bank dependence in columns (7) and (8) Appendix Table 6.16 are both negative and statistically insignificant. In column (4) of Appendix Table 6.17, I show that this is not a general pattern. The sample is the large employment cross-section and the outcome is the ln employment growth rate. The coefficient on Deutsche Bank dependence is small, statistically insignificant, and positive.

6.7 A Proxy for the Change in Bank Loans

Data on county-level loans are not available in Germany. This section proposes a proxy to measure by how much county-level bank loans fell due to Commerzbank's lending cut.

6.7.1 Constructing a Proxy for the Change in Bank Loans due to Commerzbank's Lending Cut

The proxy for county-level bank loans is based on two quantities. First, the aggregate reduction in bank loans by Commerzbank. I calculate this as the difference between Commerzbank's lending stock to German customers in 2007 and a counterfactual value for 2010. To calculate the counterfactual value, I assume that in the absence of the trading losses, Commerzbank's lending stock would have developed in parallel to the other banks from 2007 to 2010.

The second quantity aims to measure the share that loans to each county took in Commerzbank's loan portfolio before the lending cut. I use the Creditreform dataset of relationship banks to measure this. For each firm, I calculate how many Commerzbank branches are among its relationship banks. I sum the number of Commerzbank relationships in each county. Similarly, I sum the number of Commerzbank relationship in the whole dataset. The second quantity is then the number of Commerzbank relationships in each county divided of Commerzbank relationships in the whole dataset. I call this second quantity the "Commerzbank loan share of the county."

The product of the two quantities is a proxy for how much bank loans fell in a county because of Commerzbank's lending cut. The accuracy of this proxy relies on two assumptions. The first assumption is that the Commerzbank loan share of the county (the second quantity) can be accurately measured using the method described above. This requires that the number of Commerzbank relationships in the Creditreform dataset is proportional to the true number of relationship for each county. To gauge how likely this assumption is to hold, I use the German Business Register as benchmark. There are some differences between the Creditreform dataset and the Business Register. For example, in the Business Register, 13.9 percent of firms are located in the former GDR (excluding Berlin). In the Creditreform dataset, it is 17.2 percent. If this represents a consistent bias towards the former GDR, the proxy would overestimate the lending cut to counties in the former GDR.

The second assumption states that Commerzbank reduced its lending to a county in proportion to the Commerzbank loan share of the county (the second quantity). Figure 6.2 shows that the effect of Commerzbank dependence on bank loans is stable across different dimensions of firm heterogeneity, which supports this assumption.

6.7.2 Result Using the Proxy

I turn to estimating the effects of changes in bank loans on GDP growth, using the proxy calculated above. The outcome is county GDP growth between 2008 and 2012, normalized by the level of county GDP in 2007. The regressor of interest is the proxy, also normalized by county GDP in 2007. This eases the interpretation of the coefficient as the effect of a one Euro increase in bank loans on the level of GDP. The control variables, weights, and standard error calculations are identical to Table 5.8. The (unreported) results imply a one Euro decrease in bank loans leads to a 1.58 Euro fall in GDP, with a standard error of 0.53. In comparison, Peek and Rosengren (2000) find that a one USD drop in bank loans corresponds to a loss of USD 1.11 in construction activity. The regression using the proxy therefore confirms that the lending cut lowered county growth. It is important to recall that the estimate is likely to overstate the causal effect of bank loans, because there are multiple other channels through which a lending cut affects firm and county growth (see Section 1.2.1).

6.8 The Effect of Export Dependence on Counties and Firms

Section 1.7.2 shows that the effects of Commerzbank's temporary lending cut persisted beyond the duration of the lending cut. Are such persistent effects a general response to economic shocks? In this section, I use the fall in export demand during the Great Recession to investigate whether the effects of export demand shocks persist (Eaton, Kortum, Neiman, and Romalis 2016; Behrens, Corcos, and Mion 2013).

I exploit heterogeneity across firms and counties in export dependence. Aggregate trade statistics show that German real exports fell by 14.3 percent from 2008 to 2009. By 2011, exports had recovered, as they grew by 24 percent from 2009 to 2011. If export demand shocks only have transitory effects, then counties and firms with high export dependence should have experienced lower growth during the years of the export demand shock, but by 2011 they should have recovered.

For both firms and counties, I construct a dummy variable for being in the top quartile of the distribution of the export share. Appendix Table 6.18 reports that GDP in export-dependent counties was on average 1.1 percent lower in 2009 and 2010. The point estimate for 2011, however, is of the opposite sign, larger in absolute terms, and statistically different. This means that export-dependent counties entirely made up the output shortfall in under two years. The dynamics are similar for firms, as shown in Appendix Table 6.19. Employment at export-dependent firms was on average 1.8 percent lower in 2009 and 2010. But by 2011, they had recovered to the level of the other firms, outgrowing them by 2 percent in 2011. Hence, export-dependent firms and counties converged to the growth path of unaffected firms and counties in under two years.

6.9 Appendix References

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6.10 Appendix Tables

Table 6.1: Establishment of Commerzbank branches in West Germany

	(1) 1948-1970	(2) 1948-1970	(3) 1948-1970	(4) 1925-1948	(5) Pre-1925
Distance instrument	0.094	0.090	0.077	0.021	0.010
	(0.031)	(0.032)	(0.033)	(0.020)	(0.017)
Observations	324	324	324	324	324
R^2	0.122	0.122	0.136	0.088	0.359
Zonal FE	Yes	Yes	Yes	Yes	Yes
Urban FE	No	Yes	Yes	Yes	Yes
Ln population	No	No	Yes	Yes	Yes
Population density	No	No	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS	OLS

Notes: This table examines the effect of Commerzbank's postwar break-up on its branch network. It reports regressions using a cross-section of West German counties. The data are hand-collected from the historic annual reports of Commerzbank. The outcome variable is a dummy for whether Commerzbank established a branch in the county during the respective period given in the column title. The regressor of interest is the distance instrument, the negative of the county's distance to the closest postwar Commerzbank head office, in 100 kilometers. The zonal fixed effects are dummies for the three postwar banking zones of North Rhine-Westphalia, Northern, and Southern Germany. The urban fixed effect is a dummy for counties with a year 2000 population density greater than 1,000 inhabitants per square kilometer. The ln population and population density are continuous variables from the year 2000. Standard errors are robust. Columns (1) to (3) show that from 1948 to 1970, Commerzbank was more likely to establish a new branch in counties close to its temporary, postwar head offices. Columns (4) and (5) report no significant association in the period before or after.

Table 6.2: Commerzbank dependence and firm variables in 2006

	(1)	(2)
ln age	-0.015	-0.011
	(0.009)	(0.010)
ln value added	0.018	0.022
	(0.015)	(0.020)
ln capital	-0.014	-0.024
	(0.006)	(0.008)
Investment rate	0.009	-0.009
	(0.016)	(0.020)
ln employment	0.011	0.010
	(0.012)	(0.016)
ln liabilites	0.008	0.009
	(0.012)	(0.012)
ln bank loans	0.002	0.000
	(0.007)	(0.007)
ln financial assets		0.001
		(0.002)
Observations	2,011	1,618
R^2	0.307	0.340
Industry FE	Yes	Yes
County FE	Yes	Yes
Estimator	OLS	OLS

Notes: This table reports estimates from cross-sectional firm regressions of CB dep on firm variables. The data are from the firm panel for the year 2006. The variables are defined as in Table 5.1. The regression includes fixed effects for 70 industries and 357 counties. Standard errors are two-way clustered at the level of the industry and the county.

Table 6.3: County GDP and the distance to cities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Distance instrument*d	-18.309	-14.493	-18.165	-17.279	-17.950	-17.420	-16.857	-19.595	-19.310	-17.328	-19.294	-18.167	-17.298
	(3.253)	(4.205)	(4.050)	(3.850)	(3.635)	(3.932)	(3.605)	(3.017)	(2.916)	(3.839)	(3.826)	(3.795)	(3.856)
Dist to Düsseldorf*d	0.845												
	(2.618)												
Dist to Frankfurt*d		-4.218											
		(3.111)											
Dist to Hamburg*d			1.166										
			(1.821)										
Dist to Berlin*d				3.016									
				(2.510)									
Dist to Dresden*d					-2.071								
					(2.795)								
Dist to Munich*d						-0.146							
						(1.858)							
Dist to Cologne*d							-0.385						
							(2.789)						
Dist to Essen*d								1.945					
								(2.458)					
Dist to Dortmund*d									1.806				
									(2.364)				
Dist to Stuttgart*d										-1.864			
										(1.872)			
Dist to Bremen*d											2.371		
											(1.940)		
Dist to Hannover*d												1.322	
												(2.670)	
Dist to Leipzig*d													-0.580
													(3.320)
Observations	5,005	5,005	5,005	5,005	5,005	5,005	5,005	5,005	5,005	5,005	5,005	5,005	5,005
R^2	0.361	0.362	0.361	0.362	0.361	0.361	0.361	0.361	0.361	0.362	0.362	0.361	0.361
Number of counties	385	385	385	385	385	385	385	385	385	385	385	385	385
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls*d	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS

Notes: This table reports the effect of the distance instrument and the linear distances to other cities on county GDP. The distance instrument is the negative of the county's distance to the closest postwar Commerzbank head office, in 100,000 kilometers. All regressions include the full set of control variables from Table 5.8, but not the linear distances. The data include the years 2000 to 2012. The regressions are weighted by year 2000 population, and the standard errors are calculated as in Table 5.8.

Table 6.4: The distance instrument and county characteristics

		(1)	(2)
OUTCOMES			
(1) GDP Growth 2005-08	Coeff	-0.005	-0.005
	Std Err	(0.004)	(0.006)
	R^2	0.008	0.035
(2) GDP Growth 2000-05	Coeff	-0.004	0.000
	Std Err	(0.004)	(0.008)
	R^2	0.011	0.030
(3) GDP Growth 2002-03 (recession year)	Coeff	0.001	-0.003
	Std Err	(0.002)	(0.004)
	R^2	0.004	0.019
(4) Empl Growth 2005-08	Coeff	-0.003	0.004
	Std Err	(0.002)	(0.003)
	R^2	0.010	0.049
(5) Professional services share	Coeff	0.028	-0.001
	Std Err	(0.017)	(0.043)
	R^2	0.098	0.111
(6) Shipping share	Coeff	0.000	0.001
	Std Err	(0.000)	(0.001)
	R^2	0.001	0.072
(7) Metal manufacturing share	Coeff	-0.052	-0.021
	Std Err	(0.012)	(0.023)
	R^2	0.068	0.128
(8) Other manufacturing share	Coeff	-0.008	-0.032
	Std Err	(0.009)	(0.024)
	R^2	0.009	0.061
(9) Non-tradable share	Coeff	0.006	-0.005
	Std Err	(0.010)	(0.022)
	R^2	0.014	0.033
(10) Unemployment rate	Coeff	0.015	0.000
	Std Err	(0.002)	(0.004)
	R^2	0.526	0.644
(11) Debt index	Coeff	0.086	0.026
	Std Err	(0.012)	(0.034)
	R^2	0.154	0.299
CONTROLS			
Linear distances to postwar head offices		No	Yes
Former GDR FE		Yes	Yes

Notes: The reported estimates are coefficients on the distance instrument from cross-sectional OLS county regressions. Each coefficient is from a different regression. A positive coefficient implies the outcome value is greater for counties close to a postwar head office. Rows (1) to (4) show that the distance instrument is not correlated with county growth before Commerzbank's lending cut. Rows (5), (7), (10), and (11) show statistically significant raw correlations between the distance instrument and the county employment shares of professional services, the metal manufacturing share, the unemployment rate, and the household debt index. These correlations disappear once one conditions on the three linear distances to Commerzbank's three postwar head offices Düsseldorf, Frankfurt, and Hamburg. There are no statistically significant correlations between the distance instrument and the other industry shares. The distance instrument is the negative of the county's distance to the closest postwar head office, in 100 kilometers. The growth rates are in natural logarithms. The industry shares are employment shares in 2006. Professional services include WZ2008 industry categories 69-75; shipping 50; metal manufacturing 23-29; other manufacturing 9-22 and 30-32; and non-tradables are defined in Section 1.7.1. The unemployment rate is from 2006. Debt index is a 2003 measure of county household leverage, calculated by credit rating agency Schufa (Privatverschuldungsindex). The weights and standard error calculations are explained in Table 5.8.

Table 6.5: High-innovation industries

WZ2008 Code	Industry
20.2	Manufacture of pesticides and other agrochemical products
21	Manufacture of basic pharmaceutical products and preparations
25.4	Manufacture of weapons and ammunition
26	Manufacture of computer, electronic and optical products
30.3	Manufacture of air and spacecraft and related machinery
30.4	Manufacture of military fighting vehicles
20.1	Manufacture of basic chemicals, fertilisers and nitrogen compounds, plastics and synthetic rubber in primary forms
20.4	Manufacture of soap and detergents, cleaning and polishing preparations, perfumes and toilet preparations
20.5	Manufacture of other chemical products (explosives, glues, essential oils, man-made fibres)
27	Manufacture of electrical equipment (electric motors, generators, transformers and electricity distribution and control apparatus)
28	Manufacture of machinery and equipment (e.g. engines, turbines, fluid power equipment, gears, furnaces, solar heat collectors, lifting and handling equipment, power-driven hand tools, non-domestic cooling and ventilation equipment, machinery for mining, quarrying and construction)
29.1	Manufacture of motor vehicles
29.3	Manufacture of parts and accessories for motor vehicles
30.2	Manufacture of railway locomotives and rolling stock
33.2	Installation of industrial machinery and equipment

Notes: This table reports the industries with an internal share of R&D spending over revenue above 2.5 percent (OECD cut-off), classified by Gehrke, Frietsch, Neuhäusler, Rammer, and Leidmann (2010).

Table 6.6: Low-innovation industries

WZ2008 Code	Industry
8.1	Quarrying of stone, sand and clay
9	Mining support service activities (for petroleoum, natural gas and other mining and quarrying)
16.1	Sawmilling and planing of wood
23.7	Cutting, shaping and finishing of stone
25.1	Manufacture of structural metal products
35.3	Steam and air conditioning supply
36	Water collection, treatment and supply
37	Sewerage
38.2	Waste treatment and disposal
39	Remediation activities and other waste management services
41.1	Development of building projects
43.9	Other specialised construction activities
45.1	Sale of motor vehicles
46.5	Wholesale of information and communication equipment
46.9	Non-specialised wholesale trade
47.3	Retail sale of automotive fuel in specialised stores
49.3	Other passenger land transport
49.4	Freight transport by road and removal services
50	Water transport (passenger and freight)
52.1	Warehousing and storage
53.2	Other postal and courier activities
56.1	Restaurants and mobile food service activities
59.2	Sound recording and music publishing activities
68.1	Buying and selling of own real estate
70.1	Activities of head offices
74.1	Specialised design activities
74.2	Photographic activities
78	Employment activities (employment placement and agency)
80	Security and investigation activities
81.1	Combined facilities support activities
81.3	Landscape service activities
82	Office administration, office support, and other business support

Notes: This table reports the industries with the lowest innovation activities, classified by Gehrke, Frietsch, Neuhäusler, Rammer, and Leidmann (2013) using data from the Mannheim Innovation Panel.

Table 6.7: Summary statistics by bins of Commerzbank dependence

	Range of Commerzbank dependence						
	0	0.01-0.24	0.25-0.32	0.33-0.4	0.41-0.75	0.75-1	Total
Commerzbank dep	0	0.182	0.250	0.332	0.502	1	0.156
	(0)	(0.0199)	(0)	(0.00830)	(0.0720)	(0)	(0.228)
No of relationship banks	2.433	5.577	4	3.768	3.059	1.192	2.997
	(1.311)	(0.647)	(0)	(1.359)	(1.222)	(0.398)	(1.544)
Employment	831.9	982.8	840.8	1,567.4	729.3	799.9	913.7
	(14,674.6)	(2,587.4)	(4,502.6)	(6,603.1)	(2,699.2)	(1,411.5)	(11,592.5)
Wage	32.50	32.08	30.81	31.72	30.57	33.11	32.04
	(60.92)	(7.777)	(9.429)	(9.484)	(10.98)	(15.54)	(47.15)
Capital	44,699.8	86,334.2	29,697.3	145,522.9	36,887.9	62,554.4	57,711.6
	(258,037.3)	(255,992.6)	(108,208.6)	(1,496,140.7)	(106,876.7)	(134,632.3)	(544,582.6)
Investment rate	0.258	0.205	0.280	0.298	0.328	0.368	0.271
	(0.366)	(0.220)	(0.317)	(0.378)	(0.410)	(0.415)	(0.363)
Liabilities	172,542.4	84,362.6	93,348.5	217,748.4	93,014.3	79,574.5	152,628.5
	(4,653,805.1)	(278,210.9)	(788,451.4)	(2,254,805.0)	(528,174.0)	(169,250.5)	(3,657,557.1)
Bank debt/liabilities	0.501	0.483	0.477	0.434	0.448	0.449	0.483
	(0.266)	(0.246)	(0.241)	(0.242)	(0.262)	(0.281)	(0.261)
Firms	1,182	163	151	224	238	53	2,011

Notes: The range of Commerzbank dependence in the relevant bin is given in the top row. The data are from the firm panel for the year 2006. The variables are defined as in Table 5.1.

Table 6.8: Insights from the research reports

Question	Number of relevant reports	Answer yes	Answer no	Answer unclear
1) Was the trading income more volatile than at other German banks from 2004 to 2007?				
- at Commerzbank	11	0	2	9
- at Dresdner Bank	4	0	0	4
2) Was the loan portfolio to German firms and households riskier than at other German banks from 2004 to 2007?				
- at Commerzbank	11	0	11	0
- at Dresdner Bank	5	0	5	0
3) Does the report mention that the trading and lending divisions cross-hedged risk from 2004 to 2009?				
- at Commerzbank	85	0	85	0
- at Dresdner Bank	42	0	42	0
4) Does the report mention that the German loan portfolio contributed to Commerzbank's losses from 2008 to 2009?	83	0	83	0
5) Does the report mention that exposure to Iceland contributed to Commerzbank's losses from 2008 to 2009?	83	8	75	0
6) Does the report mention that exposure to asset-backed securities or the subprime mortgage crisis contributed to Commerzbank's losses from 2008 to 2009?	83	72	11	0
7) Does the report mention that exposure to Lehman Brothers contributed to Commerzbank's losses in 2009?	83	8	75	0
8) Judging in 2008, is Commerzbank's acquistion of Dresdner Bank a strategically sound move?	11	9	1	1
9) Did Commerzbank stabilise after 2010?	10	8	0	2

Notes: This table summarizes insights from the research reports listed after the reference section of the Appendix. A relevant report is a research report from the given period that contains information relevant to the given question. Reports either offer a clear conclusion (Answer yes/no) or give information in support of both sides, without committing either way (Answer unclear). For interpretation and illustrative examples, see 6.2.

Table 6.9: Robustness checks for the firm survey results

	(1)	(2)	(3)	(4)	(5)	(6)
OUTCOME	Bank loans	Bank loans	Bank loans	Demand constraint	Orders backlog	Demand change
YEAR	2009	2009	2003	2003	2003	2003
Firm CB dep	-0.393	-0.381	0.040	-0.119	0.184	-0.080
	(0.185)	(0.232)	(0.367)	(0.350)	(0.292)	(0.317)
Dep var from 2006		0.376				
		(0.084)				
Observations	1,032	1.032	642	756	768	768
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	No	Yes	Yes	Yes	Yes
Size Bin FE	Yes	Yes	Yes	Yes	Yes	Yes
ln age	Yes	Yes	Yes	Yes	Yes	Yes
County FE	No	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from OLS cross-sectional firm regressions for different years, using data from the confidential ifo Business Expectations Panel. The outcome variables, the interpretation of the coefficients, and standard error calculations are explained in Tables 5.3, 6.10, 6.11, and 6.12.

Table 6.10: Firm survey on product demand constraints

	(1)	(2)	(3)	(4)	(5)	(6)
YEAR	2007	2008	2009	2010	2011	2012
Firm CB dep	-0.191	-0.196	-0.076	-0.121	0.281	0.194
	(0.121)	(0.133)	(0.148)	(0.156)	(0.175)	(0.197)
Dep var from 2006	0.655	0.561	0.409	0.450	0.503	0.421
	(0.032)	(0.035)	(0.034)	(0.037)	(0.044)	(0.045)
Observations	980	991	1,032	945	856	808
R^2	0.482	0.370	0.262	0.287	0.304	0.259
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Size Bin FE	Yes	Yes	Yes	Yes	Yes	Yes
ln age	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from OLS cross-sectional firm regressions for different years, using data from the confidential ifo Business Expectations Panel. The outcome variable is the answer to the question: “Are your business activities constrained by low demand or too few orders: yes or no?” It is standardized to have zero mean and unit variance. The coefficients are interpreted as the standard deviation increase in demand constraints from increasing Commerzbank dependence by one. The variables are defined and the standard errors calculated as in Table 5.3.

Table 6.11: Firm survey on the backlog of product orders

YEAR	(1) 2007	(2) 2008	(3) 2009	(4) 2010	(5) 2011	(6) 2012
Firm CB dep	0.108	0.119	0.025	0.051	0.048	-0.304
	(0.105)	(0.140)	(0.155)	(0.186)	(0.160)	(0.223)
Dep var from 2006	0.662	0.527	0.416	0.453	0.489	0.390
	(0.028)	(0.039)	(0.045)	(0.043)	(0.041)	(0.050)
Observations	914	910	919	852	802	737
R^2	0.632	0.412	0.273	0.312	0.342	0.230
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Size Bin FE	Yes	Yes	Yes	Yes	Yes	Yes
ln age	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from OLS cross-sectional firm regressions for different years, using data from the confidential ifo Business Expectations Panel. The outcome variable is the answer to the question: "Currently we perceive our backlog of orders to be: comparatively large, sufficient / typical for the season, or too small?" It is standardized to have zero mean and unit variance. The coefficients are interpreted as the standard deviation increase in the backlog of orders from increasing Commerzbank dependence by one. The variables are defined and the standard errors calculated as in Table 5.3.

Table 6.12: Firm survey on product demand changes

YEAR	(1) 2007	(2) 2008	(3) 2009	(4) 2010	(5) 2011	(6) 2012
Firm CB dep	0.130	0.014	-0.008	-0.243	-0.050	-0.042
	(0.151)	(0.155)	(0.192)	(0.177)	(0.169)	(0.222)
Dep var from 2006	0.549	0.437	0.376	0.455	0.486	0.328
	(0.054)	(0.056)	(0.057)	(0.059)	(0.064)	(0.079)
Observations	914	910	919	852	802	736
R^2	0.424	0.278	0.227	0.324	0.317	0.181
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Size Bin FE	Yes	Yes	Yes	Yes	Yes	Yes
ln age	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from OLS cross-sectional firm regressions for different years, using data from the confidential ifo Business Expectations Panel. The outcome variable is the answer to the question: "Tendencies in the previous month - The demand situation has: improved, remained unchanged, or deteriorated?" The coefficients are interpreted as the standard deviation improvement in the demand situation from increasing Commerzbank dependence by one. The variables are defined and the standard errors calculated as in Table 5.3.

Table 6.13: Firm financial assets and Commerzbank dependence

VARIABLES	(1) 2006-07	(2) 2007-09	(3) 2009-10	(4) 2007-09	(5) 2007-09
Firm CB dep	-0.022	0.036	0.022	0.018	-0.040
	(0.094)	(0.092)	(0.084)	(0.068)	(0.112)
Observations	1,816	1,816	1,816	1,816	1,816
R^2	0.062	0.060	0.059	0.000	0.219
ln age	Yes	Yes	Yes	No	Yes
Size Bin FE	Yes	Yes	Yes	No	Yes
Industry FE	Yes	Yes	Yes	No	Yes
State FE	Yes	Yes	Yes	No	No
County FE	No	No	No	No	Yes
Import and Export Share	Yes	Yes	Yes	No	Yes
Estimator	OLS	OLS	OLS	OLS	OLS

Notes: This table reports estimates from cross-sectional firm regressions. The outcome is the symmetric growth rate of the value of the firm's financial assets in the given period. If a firm begins and ends the period with no financial assets, the growth rate is set to zero. The control variables and the standard error calculations are the same as in Table 5.6.

Table 6.14: Loan growth and affected savings banks

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Affected savings bank	0.080	0.031	0.083	0.045	0.080	0.078
	(0.014)	(0.066)	(0.077)	(0.009)	(0.014)	(0.017)
Savings bank	-0.116			-0.088	-0.115	-0.116
	(0.019)			(0.013)	(0.015)	(0.019)
Loan growth 2003-05			0.015			
			(0.112)			
Savings bank in BW or NRW						0.005
						(0.016)
Observations	1,284	1,284	953	1,528	1,513	1,284
R^2	0.005	0.023	0.025	0.005	0.008	0.005
State FE	No	Yes	Yes	Yes	No	No
Bank Type FE	No	Yes	Yes	Yes	No	No
Bank Size FE	No	Yes	Yes	Yes	No	No
Estimator	OLS	OLS	OLS	OLS	OLS	OLS

Notes: This table reports estimates from cross-sectional regressions of bank loan growth on a dummy for affected savings banks. All outcomes are ln differences, except for column (5), which is the symmetric growth rate. Affected is defined as owning a Landesbank with trading losses during the financial crisis. Savings bank is a dummy for savings banks. Bank type FE are dummies for cooperative banks, real estate banks, and commercial banks. Bank size FE are ten dummies for the deciles of the distribution of the bank's lending stock in 2006. The data are from Bankscope. Standard errors are robust.

Table 6.15: Firm employment and affected savings banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OUTCOME	Ln employment growth 2008-12		Ln employment (panel specification)				
Firm relation to affected savings bank	-0.015	-0.003					
	(0.005)	(0.005)					
Firm ln age		-0.080					
		(0.004)					
Firm relation to affected savings bank*d			-0.005	0.003	0.001	0.012	0.001
			(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Firm ln age*d				-0.064		-0.046	-0.062
				(0.003)		(0.004)	(0.003)
Firm ln assets						0.208	
						(0.008)	
Firm equity / assets						-0.011	
						(0.016)	
Firm cash-flow / assets						-0.000	
						(0.000)	
Firm profit / assets						0.000	
						(0.000)	
Firm relation to CB*d							-0.019
							(0.004)
Firm relation to affected Landesbank*d							-0.001
							(0.008)
Observations	45,537	45,537	438,254	438,254	438,254	173,072	438,254
R^2	0.000	0.000	0.005	0.012	0.013	0.064	0.012
Number of firms	45,537	45,537	83,410	83,410	83,410	39,659	83,410
Firm FE	No	No	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes	Yes	Yes
Industry FE	No	Yes	No	No	No	No	No
Industry FE*d	No	No	Yes	Yes	Yes	Yes	Yes
Age Bin*d	No	No	No	No	Yes	No	No
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS

Notes: This table reports tests the effect of having an affected savings bank as relationship bank on employment. Affected savings banks own a Landesbank with trading losses during the financial crisis. For details, see 6.5. The outcome in columns (1) and (2) is the ln difference in employment between 2008 and 2012. Columns (3) to (7) replicate the sample and panel specification in Popov and Rocholl (2015), where the outcome is ln employment. Standard errors are clustered at the level of the firm.

Table 6.16: Firm employment and other banks with trading losses (1)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm dep on banks with trading losses*d	-0.012	-0.004						
	(0.003)	(0.003)						
Firm CB dep*d			-0.011	-0.010	-0.009	-0.010	-0.011	-0.011
			(0.004)	(0.004)	(0.004)	(0.006)	(0.004)	(0.007)
Firm dep on other banks with trading losses (except CB)*d			0.001			0.001		
			(0.004)			(0.006)		
Firm dep on other banks with trading losses (except CB)*d(2008-10)				0.004				
				(0.004)				
Firm dep on other banks with trading losses (except CB)*d(2007-10)					0.005			
					(0.003)			
Firm DtB dep*d							-0.006	-0.009
							(0.005)	(0.008)
Firm KfW dep*d							0.058	0.042
							(0.047)	(0.063)
Firm IKB dep*d							0.058	0.004
							(0.066)	(0.063)
Firm HypoVereinsbank dep*d							0.001	-0.010
							(0.007)	(0.010)
Firm DZ Bank dep*d							0.016	0.083
							(0.021)	(0.046)
Firm dep on affected savings banks	0.008	0.006	0.006	0.007	0.008	0.009	0.006	0.012
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)
Firm lagged change in sales						0.069		
						(0.007)		
Observations	188,233	188,233	188,233	188,233	188,233	73,851	188,233	314,803
R^2	0.001	0.004	0.004	0.004	0.004	0.017	0.004	0.004
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ln Age*d	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Bin*d	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE*d	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Import and Export Share*d	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE*d	No	No	No	No	No	Yes	No	No
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS

Notes: This table reports firm regressions, replicating the specification and sample of Dwenger, Fossen, and Simmler (2015). The outcome in columns (1) to (7) is the firm's annual ln employment growth rate. In column (8), it is the annual ln growth rate of fixed assets. The sample includes the years 2006 to 2010. The other banks with trading losses banks are the German banks, except Commerzbank, that held a large share of loss-making assets during the financial crisis, as listed in Table 1 of Dwenger, Fossen, and Simmler (2015). For details, see 6.6. Standard errors are clustered at the level of the firm.

Table 6.17: Firm employment and other banks with trading losses (2)

VARIABLES	(1)	(2)	(3)	(4)
Firm dep on banks with trading losses	-0.028	-0.010		
	(0.011)	(0.011)		
Firm CB dep			-0.050	-0.054
			(0.016)	(0.016)
Firm dep on other banks with trading losses (except CB)			0.019	
			(0.013)	
Firm DtB dep				0.005
				(0.018)
Observations	48,101	48,101	48,101	48,101
R^2	0.000	0.019	0.019	0.019
Ln Age	No	Yes	Yes	Yes
Size Bin	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Import and Export Share	No	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS

Notes: This table reports cross-sectional firm regressions. The outcome is ln employment growth between 2008 and 2012. The other banks with trading losses banks are the German banks, except Commerzbank, that held a large share of loss-making assets during the financial crisis, as listed in Table 1 of Dwenger, Fossen, and Simmler (2015). For details, see 6.6. Standard errors are clustered at the level of the firm.

Table 6.18: County GDP and export dependence

Export-dependent*d	-0.011
	(0.008)
Export-dependent*d(2011)	0.012
	(0.006)
Export-dependent*d(2012)	0.009
	(0.007)
CB dep*d	-0.138
	(0.065)
Observations	5,005
R^2	0.360
Number of counties	385
County FE	Yes
Year FE	Yes
Former GDR FE*d	Yes
Industry Shares*d	Yes
Population*d	Yes
Pop density*d	Yes
GDP per capita*d	Yes
Debt Index*d	Yes
Import Share*d	Yes
Export Share*Linear Trend	Yes
Landesbank in crisis*d	Yes
Estimator	OLS

Notes: This table reports estimates from county panel regressions. The outcome is ln GDP. Export-dependent is a dummy variable for counties in the top quartile of the distribution of the average export share (fraction of exports out of total revenue, averaged across firms in the county). d is a dummy for the years following the lending cut, 2009 to 2012. d(2011) and d(2012) are dummies for the years 2011 and 2012 respectively. The control variables, weights, standard error calculations, the years covered by the data, and the definition of R^2 are explained in Table 5.8.

Table 6.19: Firm employment and export dependence

Export-dependent*d	-0.018
	(0.012)
Export-dependent*d(2011)	0.020
	(0.007)
Export-dependent*d(2012)	0.041
	(0.010)
CB dep*d	-0.052
	(0.015)
Observations	12,066
R^2	0.126
Number of firms	2,011
Firm FE	Yes
Year FE	Yes
ln age*d	Yes
Size Bin FE*d	Yes
Industry FE*d	Yes
County FE*d	Yes
Import Share*d	Yes
Estimator	OLS

Notes: This table reports estimates from firm panel regressions. The outcomes is ln employment. Export-dependent is a dummy variable for firms in the top quartile of the distribution of the export share. d is a dummy for the years following the lending cut, 2009 to 2012. d(2011) and d(2012) are dummies for the years 2011 and 2012 respectively. The data include the years 2007 to 2012. The control variables and the standard error calculations are the same as in Table 5.6.

Table 6.20: Equity Extraction Elasticities by Home Improvement

	(1)	(2)	(3)	(4)
Panel A: Last Mortgage for Home Improvement				
Equity Extraction Elasticity	0.191	0.183	0.171	0.162
	(0.006)	(0.011)	(0.013)	(0.014)
Observations	114,566	114,566	108,237	96,613
Panel B: Last Mortgage Not for Home Improvement				
Equity Extraction Elasticity	0.213	0.198	0.189	0.184
	(0.003)	(0.006)	(0.007)	(0.007)
Observations	553,200	553,200	524,513	470,038
Panel C: Purpose of Last Mortgage Unknown				
Equity Extraction Elasticity	0.337	0.240	0.250	0.235
	(0.002)	(0.008)	(0.008)	(0.009)
Observations	716,580	716,580	678,984	606,975
Control Variables:				
Month FE		×	×	×
Household FE		×	×	×
County x Year FE			×	×
Household Controls				×

Notes: The table reports estimates of the equity extraction elasticity, splitting the estimation sample by whether the last equity extraction decision was made for home improvements or not. Panel A considers homeowners whose last refinance was for home improvements, Panel B considers homeowners whose last refinance was not for home improvements, while panel C considers homeowners whose last refinance purpose is missing in the data. Standard errors are clustered by household and shown in parentheses. The household controls included in column (4) are income level, income growth, the last mortgage interest rate, the age of the borrower, a dummy for couples, and dummies for the various reasons for both the last and current refinance (pure refinance / home improvement / debt consolidation / other). The table shows that, across the different fixed effects specifications, the estimated elasticity is quite stable across samples.

Table 6.21: Further tests by firm opacity

	(1)	(2)	(3)	(4)
Outcome	Employment Growth 1951-56			
Commerzbank rel.	0.003	0.003	-0.020	-0.009
bank treated in 1952	(0.006)	(0.007)	(0.012)	(0.017)
Deutsche Bank rel.	0.001	-0.000	-0.025	-0.022
bank treated in 1952	(0.004)	(0.005)	(0.010)	(0.011)
Dresdner Bank rel.	0.004	0.006	-0.003	-0.024
bank treated in 1952	(0.005)	(0.005)	(0.012)	(0.011)
Observations	1,177	1,177	301	295
R^2	0.001	0.058	0.028	0.241
Controls*zone FE	No	Yes	No	Yes
Sample	Not opaque		Opaque	

Notes: The outcome variables, regressors, control variables, and standard errors are explained in Tables 5.19 and 5.20. A firm is opaque if it has fewer than 50 employees in 1951, is younger than 10 years old in 1952, or is in the bottom ten percent of industry asset tangibility (fixed tangible over total assets). Standard errors are robust. The samples include only non-stock firms.

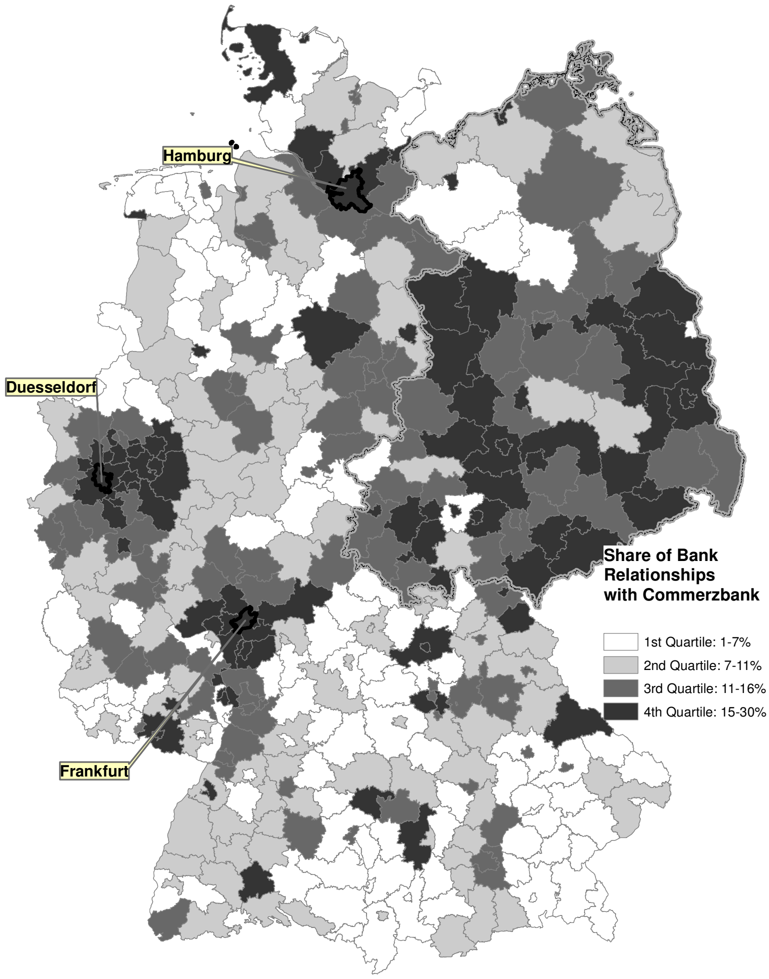
Table 6.22: Using 1940 relationship banks as treatment indicators

	(1)	(2)	(3)	(4)
	Employment Growth			
Outcome	1949-51	1951-56	1949-51	1951-56
Rel. bank (as of 1940)	0.001	-0.001	0.027	-0.061
treated in 1952	(0.027)	(0.010)	(0.076)	(0.014)
Observations	182	370	25	51
R^2	0.374	0.157	0.175	0.338
Controls*zone FE	Yes	Yes	No	No
Basic Controls	No	No	Yes	Yes
Sample	Not opaque		Opaque	

Notes: The outcomes are the average annual symmetric growth rates of employment in the given period. (For instance, in column (1), the outcome is the symmetric growth rate from 1949 to 1951 divided by 2.) Relationship bank (as of 1940) treated in 1952 is a dummy for whether one of the firm's 1940 relationship banks was treated in the first reform of 1952. A firm is opaque if it has fewer than 50 employees in 1951, is younger than 10 years old in 1952, or is in the bottom ten percent of industry asset tangibility (fixed tangible over total assets). The small samples in columns (3) and (4) necessitate the use of a basic set of controls, including a fixed effect for manufacturing firms, four fixed effects for the firm's employment in 1951 (1-49, 50-249, 250-999, 1000+ employees), and the natural logarithm of the firm's age. The controls*zone FE correspond to the standard control variables from Table 5.20. They include the four employment bin fixed effects, 18 industry fixed effects, and the natural logarithm of the firm's age, all fully interacted with fixed effects for the Northern, Western, and Southern banking zones that were in existence from 1952 to 1957. Standard errors are robust. The samples include only non-stock firms.

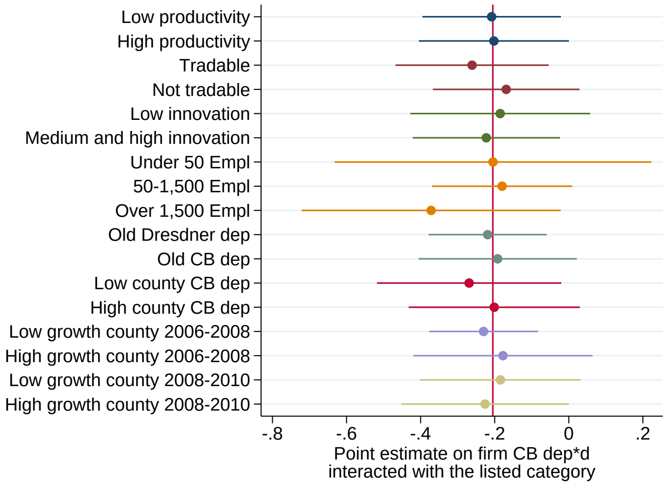
6.11 Appendix Figures

Figure 6.1: Commerzbank dependence across German counties in 2006



Notes: This map illustrates the Commerzbank dependence of German counties in the year 2006. I measure Commerzbank dependence using a dataset of the year 2006 relationship banks of 112,344 German firms. County Commerzbank dependence is the average of firm Commerzbank dependence for firms with their head office in the county. Two insights emerge from the map. First, counties around the post-war head offices Düsseldorf, Frankfurt, and Hamburg are more likely to depend on Commerzbank. Second, the former GDR is more dependent on Commerzbank. The reason is that Commerzbank followed a unique branch expansion strategy in the former GDR after German reunification in 1990 (Klein 1993). The other German banks simply took over the pre-existing branch networks of the former GDR state banks, while Commerzbank built up its own. The potential endogeneity resulting from Commerzbank's expansion in the former GDR is one of the motivations for the distance instrument.

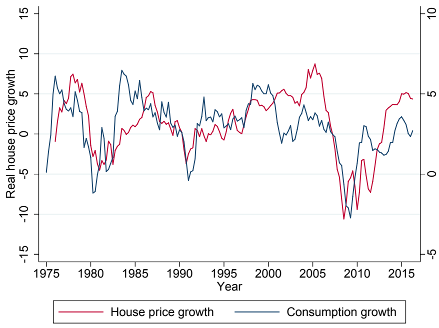
Figure 6.2: The lending cut to different categories of firms



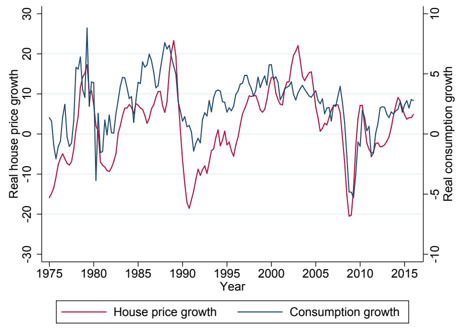
Notes: This figure plots coefficients from several firm panel regressions. The outcome is firm ln bank loans. Each color represents a different regression. The plotted point estimates are the coefficients on dummies for the category listed on the left, interacted with firm CB dep*d. The horizontal lines are 95 percent confidence intervals. The red, vertical line represents the average effect of CB dep*d on ln bank loans of -0.205. High (low) labor productivity is above (below) median 2006 valued added divided by employment. Tradability and innovation intensity are defined in Section 1.7.1. Old Dresdner dep refers to dependence on Dresdner Bank branches, which were then acquired and rebranded by Commerzbank. High (low) county CB dep and county growth are defined as above (below) the median. The control variables and the standard error calculations are the same as in column (4) of Table 5.4.

Figure 6.3: Aggregate House Prices, Consumption, and Mortgage Debt

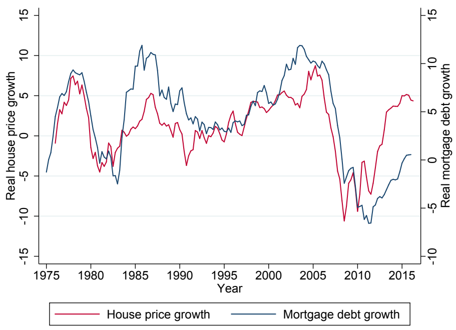
A: US House Prices vs Consumption



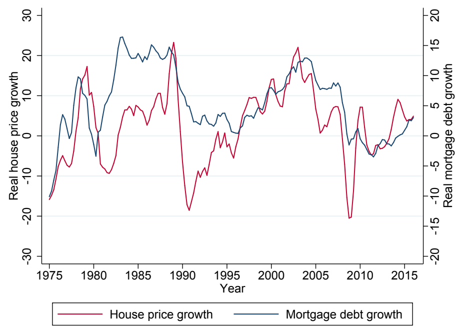
B: UK House Prices vs Consumption



C: US House Prices vs Mortgage Debt

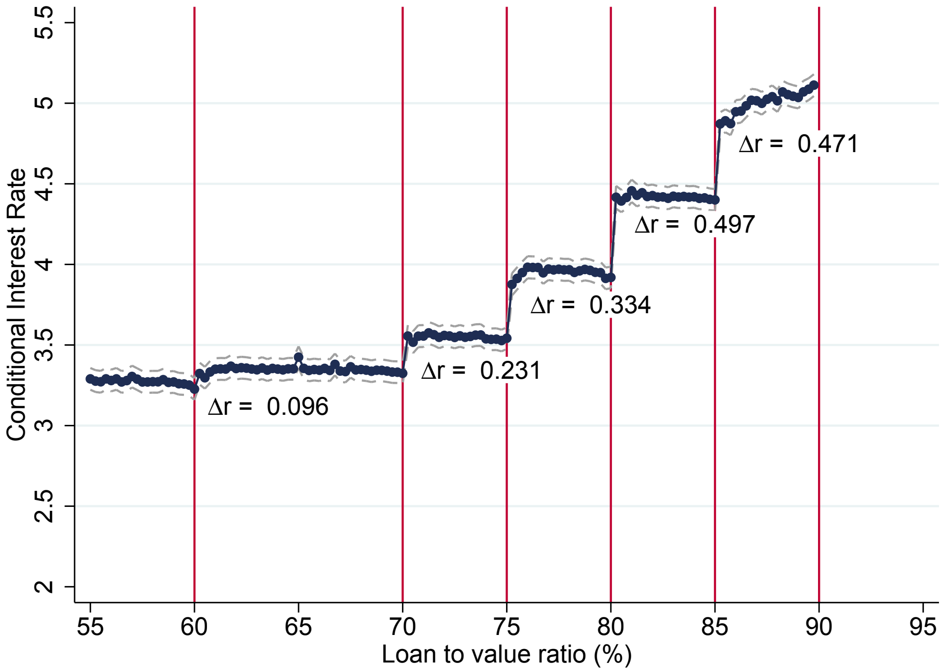


D: UK House Prices vs Mortgage Debt



Notes: US house price data are from the Federal Reserve Economic Data, US consumption data are from the BEA National Income and Product Accounts, and US mortgage debt data are from the US Flow of Funds. UK house price data are from the Nationwide Index, UK consumption data are from the ONS National Accounts, and UK mortgage debt data are from the Bank of England. All growth rates are log differences multiplied by 100.

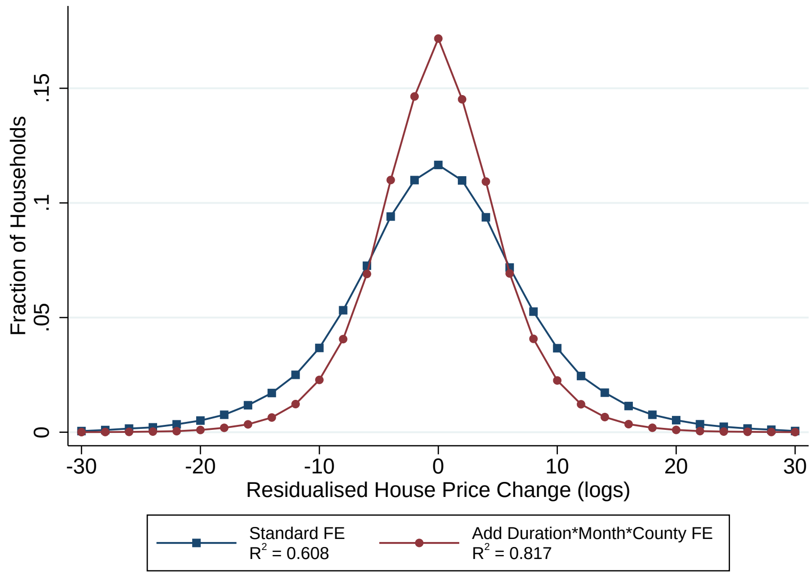
Figure 6.4: Average Interest Rate Schedule in the UK (Notches)



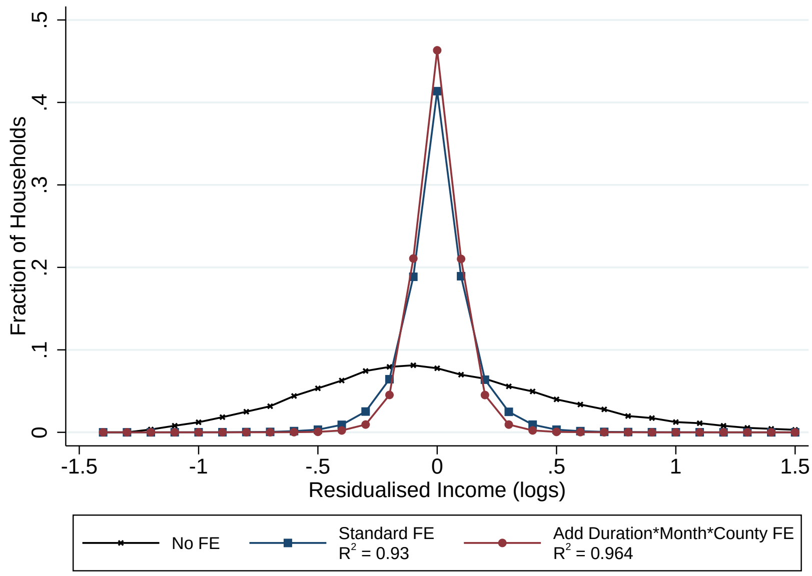
Notes: The figure shows the average mortgage interest rate in the UK (in %) as a step function of the LTV ratio, with sharp jumps (notches) at LTVs of 60%, 70%, 75%, 80%, and 85%. The figure plots coefficients (and confidence intervals) from a regression of the mortgage interest rate on dummies for each 0.25%-bin of the LTV distribution. To each coefficient, we add a constant equal to the mean predicted value of the interest rate from all the other covariates. The other covariates include non-parametric controls for lender, contract duration (time until reset), month of refinance, mortgage type (fixed interest rate / variable interest rate / capped interest rate / other), repayment type (interest only / capital and interest / other), term length, reason for refinance, age, couple indicator, and income. The figure is taken from Best, Cloyne, Ilzetzki, and Kleven (2015) and further details are provided there.

Figure 6.5: The Explanatory Power of Mortgage Duration

A: Duration Explains Future Price Change



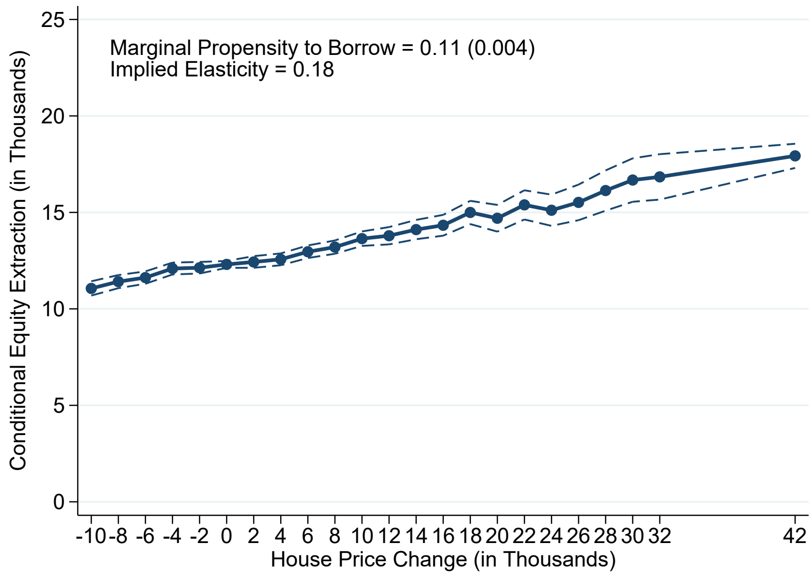
B: Duration Does Not Explain Future Income



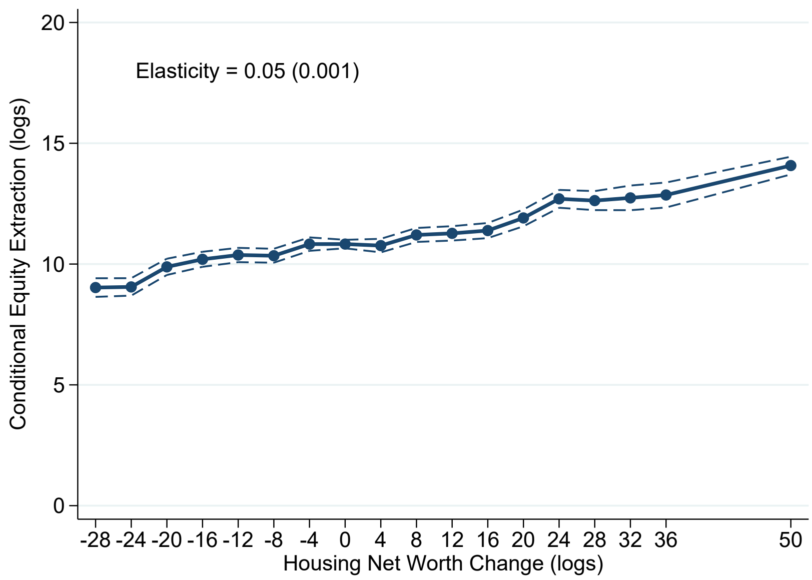
Notes: Panel A plots distributions of residualized house price growth, with and without fixed effects for the last contract duration choice (time until reset) interacted with month and county dummies. The panel shows that past duration choices can explain a large part of the residual price variation (having already absorbed fixed effect for household, month, and county x year). Panel B investigates if past duration choices can also explain residual income variation and shows that it cannot. The fact that past duration is able to predict house price growth, but not other determinants of borrowing such as income, makes it useful for identifying the effects of house prices on borrowing.

Figure 6.6: Alternative Specifications

A: From Logs to Levels

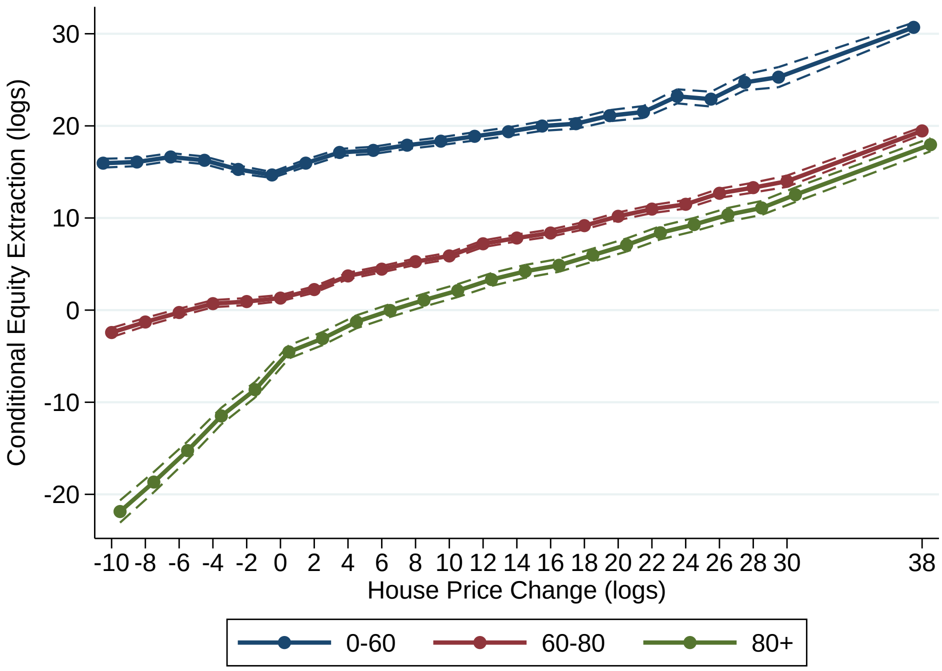


B: From House Prices to Housing Net Worth



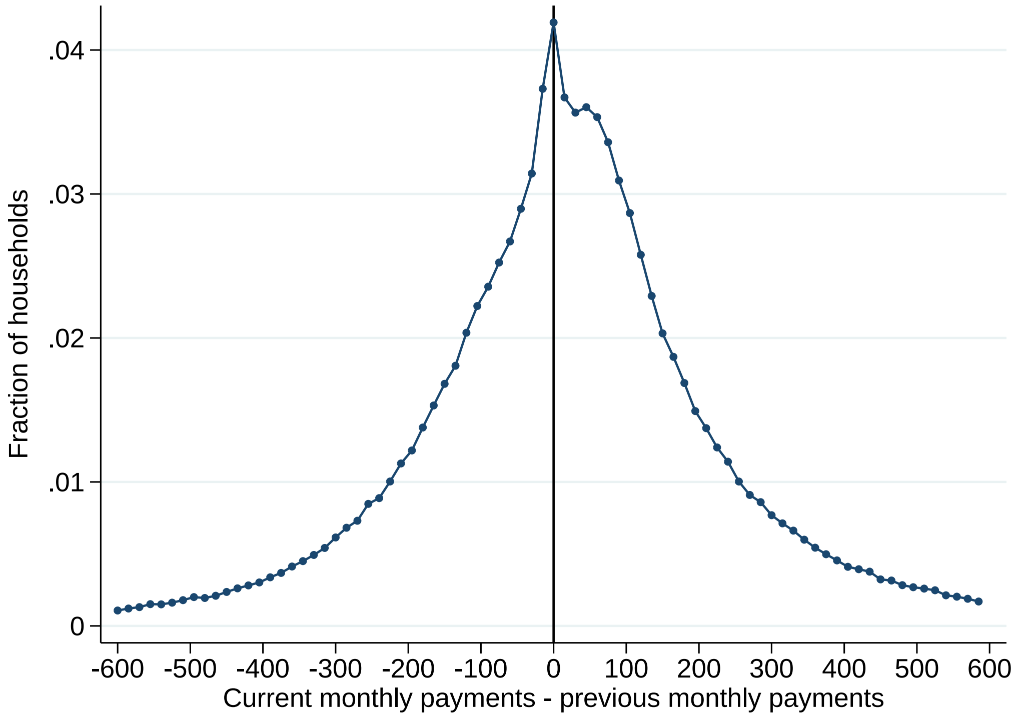
Notes: The figure investigates if the previous results are affected by moving from a log-specification to a level-specification (Panel A) and by moving from house prices to housing net worth as the explanatory variable (Panel B). Apart from these changes, the figure is based on the previous fixed effects specification (2.3) and the panels are constructed in the same way as Figure 5.17. The results are qualitatively unaffected by the changes, but the alternative specifications are useful for obtaining different parameters. Panel A yields an estimate of the marginal propensity to borrow (equal to 0.11), while Panel B yields an estimate of the equity extraction elasticity with respect to housing net worth (equal to 0.05).

Figure 6.7: Heterogeneity by LTV Non-Parametrically



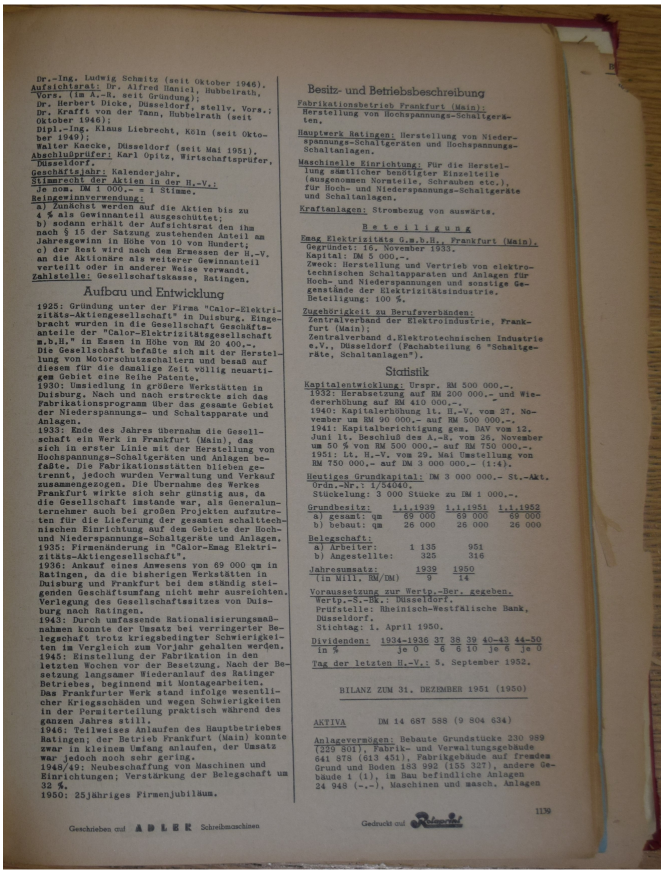
Notes: The figure plots average equity extraction in different bins of house price growth and in different bins of pre-determined LTV. Pre-LTV is defined as the LTV ratio at time t absent any equity extraction/injection at time t and absent any house price growth between t and $t-1$. The figure considers three bins of pre-LTV: low leverage (0-60%), intermediate leverage (60-80%), and high leverage (above 80%). The dashed lines represent 95% confidence intervals based on standard errors clustered by household. The figure shows that the *level* of equity extraction decreases with leverage, while the *slope* of equity extraction with respect to house price growth increases with leverage. This is consistent with the collateral channel.

Figure 6.8: Distribution of the Change in Monthly Mortgage Payments



Notes: The figure is based on a sample of households who are pulled down to a lower notch by house price growth. It shows the distribution of the difference (in GBP) between the household's current monthly mortgage payments and previous monthly payments.

Figure 6.9: Photograph of a page from the 1952 *Handbuch der deutschen Aktiengesellschaften*



Dr.-Ing. Ludwig Schmitz (seit Oktober 1946).
Aufsichtsrat: Dr. Alfred Haniel, Hubbelrath, Vors. (im A.-R. seit Gründung);
Dr. Herbert Dicke, Düsseldorf, stellv. Vors.;
Dr. Krafft von der Tann, Hubbelrath (seit Oktober 1946);
Dipl.-Ing. Klaus Liebrecht, Köln (seit Oktober 1949);
Walter Kaecke, Düsseldorf (seit Mai 1951).
Abschlußprüfer: Karl Opitz, Wirtschaftsprüfer, Düsseldorf.
Geschäftsjahr: Kalenderjahr.
Stimmrecht der Aktien in der H.-V.:
Je nom. DM 1 000.- = 1 Stimme.
Reingewinnverwendung:
a) Zunächst werden auf die Aktien bis zu 4 % als Gewinnanteil ausgeschüttet;
b) sodann erhält der Aufsichtsrat den ihm nach § 15 der Satzung zustehenden Anteil am Jahresgewinn in Höhe von 10 von Hundert;
c) der Rest wird nach dem Ermessen der H.-V. an die Aktionäre als weiterer Gewinnanteil verteilt oder in anderer Weise verwandt.
Zahlstelle: Gesellschaftskasse, Ratingen.

Aufbau und Entwicklung

1925: Gründung unter der Firma "Calor-Elektrizitäts-Aktiengesellschaft" in Duisburg. Eingebracht wurden in die Gesellschaft Geschäftsanteile der "Calor-Elektrizitätsgesellschaft m.b.H." in Essen in Höhe von RM 20 400.-.
Die Gesellschaft befaßte sich mit der Herstellung von Motorschutzschaltern und besaß auf diesem für die damalige Zeit völlig neuartigem Gebiet eine Reihe Patente.
1930: Umsiedlung in größere Werkstätten in Duisburg. Nach und nach erstreckte sich das Fabrikationsprogramm über das gesamte Gebiet der Niederspannungs- und Schaltapparate und Anlagen.
1933: Ende des Jahres übernahm die Gesellschaft ein Werk in Frankfurt (Main), das sich in erster Linie mit der Herstellung von Hochspannungs-Schaltgeräten und Anlagen befaßte. Die Fabrikationsstätten blieben getrennt, jedoch wurden Verwaltung und Verkauf zusammengezogen. Die Übernahme des Werkes Frankfurt wirkte sich sehr günstig aus, da die Gesellschaft imstande war, als Generalunternehmer auch bei großen Projekten aufzutreten für die Lieferung der gesamten schalttechnischen Einrichtung auf dem Gebiete der Hoch- und Niederspannungs-Schaltgeräte und Anlagen.
1935: Firmenänderung in "Calor-Emag Elektrizitäts-Aktiengesellschaft".
1936: Ankauf eines Anwesens von 69 000 qm in Ratingen, da die bisherigen Werkstätten in Duisburg und Frankfurt bei dem ständig steigenden Geschäftsumfang nicht mehr ausreichten. Verlegung des Gesellschaftssitzes von Duisburg nach Ratingen.
1943: Durch umfassende Rationalisierungsmaßnahmen konnte der Umsatz bei verringerter Belegschaft trotz kriegsbedingter Schwierigkeiten im Vergleich zum Vorjahr gehalten werden.
1945: Einstellung der Fabrikation in den letzten Wochen vor der Besetzung. Nach der Besetzung langsamer Wiederanlauf des Ratinger Betriebes, beginnend mit Montagearbeiten. Das Frankfurter Werk stand infolge wesentlicher Kriegsschäden und wegen Schwierigkeiten in der Permiterteilung praktisch während des ganzen Jahres still.
1946: Teilweises Anlaufen des Hauptbetriebes Ratingen; der Betrieb Frankfurt (Main) konnte zwar in kleinem Umfang anlaufen, der Umsatz war jedoch noch sehr gering.
1948/49: Neubeschaffung von Maschinen und Einrichtungen; Verstärkung der Belegschaft um 32 %.
1950: 25jähriges Firmenjubiläum.

Besitz- und Betriebsbeschreibung

Fabrikationsbetrieb Frankfurt (Main): Herstellung von Hochspannungs-Schaltgeräten.
Hauptwerk Ratingen: Herstellung von Niederspannungs-Schaltgeräten und Hochspannungs-Schaltanlagen.
Maschinelle Einrichtung: Für die Herstellung sämtlicher benötigter Einzelteile (ausgenommen Normteile, Schrauben etc.), für Hoch- und Niederspannungs-Schaltgeräte und Schaltanlagen.
Kraftanlagen: Strombezug von auswärts.

Beteiligung

Emag Elektrizitäts G.m.b.H., Frankfurt (Main).
Gegründet: 16. November 1933.
Kapital: DM 5 000.-.
Zweck: Herstellung und Vertrieb von elektrotechnischen Schaltapparaten und Anlagen für Hoch- und Niederspannungen und sonstige Gegenstände der Elektrizitätsindustrie.
Beteiligung: 100 %.
Zugehörigkeit zu Berufsverbänden:
Zentralverband der Elektroindustrie, Frankfurt (Main);
Zentralverband d.Elektrotechnischen Industrie e.V., Düsseldorf (Fachabteilung 6 "Schaltgeräte, Schaltanlagen").

Statistik

Kapitalentwicklung: Urspr. RM 500 000.-.
1932: Herabsetzung auf RM 200 000.- und Wiedererhöhung auf RM 410 000.-.
1940: Kapitalerhöhung lt. H.-V. vom 27. November um RM 90 000.- auf RM 500 000.-.
1941: Kapitalberichtigung gem. DAV vom 12. Juni lt. Beschluß des A.-R. vom 26. November um 50 % von RM 500 000.- auf RM 750 000.-.
1951: Lt. H.-V. vom 29. Mai Umstellung von RM 750 000.- auf DM 3 000 000.- (1:4).
Heutiges Grundkapital: DM 3 000 000.- St.-Akt.
Ordn.-Nr.: 1/54040.
Stückelung: 3 000 Stücke zu DM 1 000.-.

Grundbesitz:	1.1.1939	1.1.1951	1.1.1952
a) gesamt: qm	69 000	69 000	69 000
b) bebaut: qm	26 000	26 000	26 000

Belegschaft:		
a) Arbeiter:	1 135	951
b) Angestellte:	325	316

Jahresumsatz:	1939	1950
(in Mill. RM/DM)	9	14

Voraussetzung zur Wertp.-Ber. gegeben.
Wertp.-S.-Bk.: Düsseldorf.
Prüfstelle: Rheinisch-Westfälische Bank, Düsseldorf.
Stichtag: 1. April 1950.

Dividenden:	1934-1936	37	38	39	40-43	44-50
in %	je 0	6	6	10	je 6	je 0

Tag der letzten H.-V.: 5. September 1952.

BILANZ ZUM 31. DEZEMBER 1951 (1950)

AKTIVA DM 14 687 588 (9 804 634)

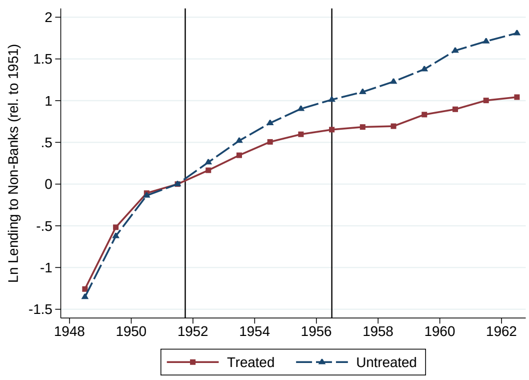
Anlagevermögen: Bebaute Grundstücke 230 989 (229 801), Fabrik- und Verwaltungsgebäude 641 878 (613 451), Fabrikgebäude auf fremdem Grund und Boden 183 992 (155 327), andere Gebäude 1 (1), im Bau befindliche Anlagen 24 948 (-.-), Maschinen und masch. Anlagen

Geschrieben auf ADLER Schreibmaschinen

1139

Gedruckt auf Rotaprint

Figure 6.10: Lending by the treated banks compared to all other banks



Notes: The figure shows lending using all other German banks as untreated group. Figure 5.24 uses the other commercial banks as untreated group. The data are for the December of the given year and provided by the Deutsche Bundesbank.