Are Bigger Banks Better?
Firm-Level Evidence from Germany

Kilian Huber*

December 2019

Abstract

This paper studies how increases in bank size affect real outcomes. I analyze two quasi-experiments from postwar Germany. Two reforms exogenously determined when certain banks were allowed to become larger by consolidating. I find that, on average, firms did not grow faster after their banks became bigger. Opaque firms even grew more slowly. The banks did not become more profitable or cost efficient after consolidating, but started lending to riskier firms. The results indicate that increases in bank size do not always raise the growth of firms or bank efficiency, but may actually harm some firms. (JEL E24, E44, G21, G28)

*University of Chicago, kilianhuber@uchicago.edu. I thank Philippe Aghion, Saleem Bahaj, Francesco Caselli, Nicola Cetorelli, Melissa Dell, Douglas Diamond, Thomas Drechsel, Erik Hurst, Anil Kashyap, Randall Kroszner, Ross Levine, Alan Manning, Stephan Maurer, Ana McDowall, Emi Nakamura, Brent Neiman, Daniel Paravisini, Thomas Philippon, Steve Pischke, Raghuram Rajan, Oliver Rehbein, Ricardo Reis, Albrecht Ritschl, Christina Romer, David Romer, Isabel Schnabel, Robert Shimer, Jeremy Stein, Jón Steinsson, Philip Strahan, Amir Sufi, Harald Uhlig, Fabian Waldinger, and Alwyn Young. Archivists and librarians at the Economic Archive Hohenheim, the German National Library of Economics, the Historical Archives of Commerzbank, Deutsche Bundesbank, and Deutsche Bank, the University Libraries of Cologne, Tübingen, and Munich, and the Württembergische Landesbibliothek generously supported the data collection. The research was funded by grants from the Centre for Economic Performance, Centre for Macroeconomics, LSE Institute of Global Affairs, Paul Woolley Centre, Sticerd, and Stiftung Familienunternehmen. Kenan Jusufovic and Stefan Wies provided excellent research assistance.

I Introduction

How do increases in bank size affect firms in the real economy? The growth of large banks since the 1990s and failures of large banks during the crisis 2008/09 have kept this question at the forefront of policy debates.[1] Some policymakers argue that limiting further increases in bank size could improve financial stability and reduce excessive risk-taking (Stern and Feldman 2004; Johnson 2016). But there could be significant costs to regulation. If larger banks generate economies of scale, then discouraging banks from growing bigger could reduce efficiency gains in the financial system, restrict credit supply, and harm real economic growth (French et al. 2010; Stein 2013).

In this paper, I analyze two quasi-experiments from postwar Germany. Reforms in 1952 and 1957 determined when several institutions were allowed to consolidate from state-level banks into national banks. The resulting bank consolidations led to increases in bank size that were exogenous to the growth of banks and their borrowers. I find that, on average, firms did not grow faster when their banks became larger. The consolidations did not make the banks more cost efficient or profitable. The banks began lending to riskier firms after consolidating, without generating higher growth among their new borrowers. These findings indicate that increased bank size does not always generate improvements in bank efficiency or firm growth, in contrast to some leading theories. Furthermore, opaque (small, young, low-collateral) firms grew more slowly after their banks got bigger, consistent with the view that bigger banks are worse at processing soft information.

Economic theory suggests that big banks may be more efficient because they are more diversified (Leland and Pyle 1977; Diamond 1984; Boyd and Prescott 1986; Williamson 1986), can use internal capital markets (Stein 1997), and can rely on a large capital base to spread fixed costs. On the other hand, large organizations may raise complex challenges for managers (Williamson 1967; Krasa and Villamil 1992a,b; Cerasi and Daltung 2000) and divisional rent-seeking (Scharfstein and Stein 2000). Large banks may find it difficult to process soft information, which can hurt small and opaque borrowers (Stein 2002; Berger and Udell 2002; Brickley et al. 2003). Big banks may also take excessive risks, due to implicit "too-big-to-fail" subsidies by governments (Freixas 1999; Dávila and Walther 2017) or internal agency problems (Rajan 2005). The net impact of increases in bank size on firms is an empirical question.

The empirical challenge in estimating the effects of bigger banks is that banks do not become big randomly. For example, banks strategically consolidate with other banks because they expect the borrowers of the other banks to grow faster in future. In such cases, one would observe a positive correlation between bank size and the growth of borrowers even in the absence of causal effects of bank size. Two features of the postwar German banking system combined provide quasi-experiments that overcome the challenge. The first feature is the reliance of German firms on relationship banking.

[1] The market share of the 10 biggest banks in the United States increased from around 25 percent in 1990 to over 60 percent today (McCord and Prescott 2014). Regulatory proposals include outright caps on bank size as well as indirect incentives for banks to remain small, for example higher capital requirements and stress tests for big banks (Greenwood et al. 2017).

Due to asymmetric information, bank-borrower relationships were sticky. This means that shocks to a given bank affected the cost of banking services for its relationship borrowers. Demand for banking services was high in postwar Germany, so shocks to the efficiency of banks were likely to have significant real effects (Holtfrerich 1995, page 544).

The second feature is the banking policy of the Allied occupiers in postwar Germany. The Allies believed that three banks with nationwide branch networks (Commerzbank, Deutsche Bank, and Dresdner Bank) had contributed to the Nazi war effort. These banks constitute the treatment group for the purposes of this paper. In 1947/48, the Allies broke up the treated banks into 30 independent state-level organizations and prohibited the new banks from branching outside state borders. A first reform in 1952 permitted some of the state-level banks to consolidate with other state-level banks within three banking zones. This meant that the 30 state-level banks merged to become 9 treated institutions, one for each former national bank in each banking zone. A second reform in 1957 lifted the restrictions entirely and led to the reconsolidation of the three original, national banks. Hence, borrowers with a treated relationship bank experienced sharp increases in the size of their relationship banks in 1952 and 1957.[2]

The history of banking is replete with cases of banks that have sought to consolidate. The prevailing sentiment among the managers of treated banks in postwar Germany was no different. They had wanted to reconsolidate from the moment of their initial breakup. The key advantage of this setting is that when the banks were allowed to consolidate was determined by the reforms. Improvements in the attitude of the Allies toward Germany, mainly due to the emergence of the Cold War, made the reforms possible. Negotiations among German politicians, central bankers, bank representatives, and the Allies then determined specifically when the consolidations happened.

Historical records suggest that the consolidations affected how the banks operated. They increased diversification, organizational complexity, and hierarchical decision-making; enabled the banks to use internal capital markets and to spread out fixed costs; and reduced the need for loan syndicates. Importantly, the reforms did not directly affect determinants of bank efficiency unrelated to size, such as the kinds of services offered by the banks or the number of branches operating in each local market. This allows me to to examine how increases in bank size affected banks and borrowers while keeping constant spurious confounders that are usually correlated with changes in bank size.

Policymakers and commentators often consider a bank systemically important if its assets exceed roughly 1-2 percent of GDP.[3] During the breakup, all of the state-level treated banks were below this

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

[3]The Fed carries out a review of bank mergers that create institutions above 100 billion USD (around 0.5 percent of U.S. GDP). Smaller mergers are "not likely to create institutions that pose systemic risks" (Federal Reserve System 2017). Germany sets higher capital requirements on banks with assets greater than 2.2 percent of German GDP and the United Kingdom does the same for banks with assets above 1.7 of U.K. GDP. The Minneapolis Fed (2016) suggests a 23.5 percent capital ratio for banks above 1.9 percent of GDP. Johnson (2016) proposes capping bank size at 2 percent of GDP.

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 regional lenders into three banks of systemic importance.

One contribution of this paper is to hand-digitize the first digital micro-dataset on German firms in the postwar period. The new dataset allows researchers to study the corporate side of Germany's "economic miracle" after World War II. It includes the bank relationships of around 5,900 firms, balance sheet variables of around 400 firms, and employment growth of around 2,300 firms.

In the main analysis of the paper, I examine whether the bank consolidations, induced by the banking reforms, affected the growth of firms. I compare the growth of firms with a treated relationship bank to firms with untreated banks. In the cleanest empirical test, I analyze the growth of firms around 1952 and I use the fact that the 1952 reform did not affect any banks located in the state of North-Rhine Westphalia. That means, I can compare firms located in North-Rhine Westphalia, whose banks were only treated in 1957, to firms in states bordering North-Rhine Westphalia, whose banks were treated in both 1952 and 1957. This test overcomes the concern that firms with a relationship bank treated by any banking reform were on different growth paths than other firms. I find that treated banks and firms grew in parallel to untreated banks and firms before 1952, suggesting that they would have continued to grow in parallel in the absence of the banking reforms.

The main results show that, after 1952, firms with a treated relationship bank did not experience faster growth rates of bank debt, employment, or revenue per worker after their relationship bank was treated by a reform. The treated banks did not lend more and did not add more relationship borrowers compared to other banks. The new relationship borrowers of treated banks also did not grow faster than comparable firms. The estimates are similar for firms that were more dependent on banking services and for firms located in states where the 1952 reform affected banks' internal operations most strongly.

I separately examine a subsample of firms that were 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 unsecured "character loans." Opaque firms substituted bank debt with other sources of financing after their relationship banks grew in size, indicating an increase in their relative cost of bank debt. Opaque firms with no access to stock market funding reduced employment growth. The results on opaque firms are consistent with theories arguing that big banks are worse at processing soft information. All the firm-level results are similar across the three treated banking groups. This suggests that the results are not specific to one individual institution, but instead are driven by more general mechanisms.

The second set of results analyzes bank profitability and cost efficiency. I find that the treated banks did not earn higher profits after consolidating, relative to a set of comparable untreated banks. Simple cost efficiency ratios (for example, non-interest expenses over assets) of the treated banks improved slightly less than the ratios of comparable banks. Following Berger and Mester (1997), I

also estimate efficiency measures based on alternative profit functions and cost functions. I find no evidence that treated banks improved these efficiency measures by more. Overall, the findings are inconsistent with theories emphasizing that big banks are generally more profitable or cost efficient.

The third set of results examines the new banking relationships formed between treated banks and firms. I find that opaque firms were less likely to establish new relationships with treated banks after the reforms, consistent with the lower 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 1952, and the volatility of revenue growth before 1952. Along all three dimensions, I find evidence that risky firms were more likely to establish new relationships with treated banks after the reforms. There is no evidence that the new risky borrowers of treated banks grew faster than comparable firms. If borrower growth is a measure of the expected return to lending, the banks took more risk without gaining a higher return.

An additional bank-level analysis shows that the treated banks and their executives were mentioned in the media more frequently after the reforms. Reports about the reforms do not explain this finding. It implies that many, small banks appear in the media less frequently than one big bank, even when the aggregated size of the small banks is identical to the size of the big bank. Media presence can benefit firms: It can affect consumer choices, political opinions, and voting (Enikolopov and Petrova 2015; Bursztyn and Cantoni 2016), and may 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 final step of the empirical analysis examines the effects of the consolidations at a higher level of economic aggregation, on municipalities. The municipality-level results capture not only the effects on firm growth, but also other potential channels, such as local general equilibrium effects, changes in the municipal banking market, and the responses of households. I find that municipalities with higher exposure to a treated bank experienced lower employment growth after the reforms. The municipality-level results are based on a small sample of around 80 municipalities, so caution is warranted in interpreting these results. Nonetheless, the negative effect on municipalities is consistent with the firm-level and bank-level analyses because opaque firms grew more slowly and other firms did not benefit from the consolidations.

II Related Empirical Literature

A large literature has analyzed the cross-sectional relationship between bank size and bank efficiency. Some papers find 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 et al. 2014; Biswas et al. 2017; Hughes et al. 2019). In general, however, the cross-sectional evidence is mixed (as reviewed by Berger and Mester 1997). The possibility of reverse causality, that is

banks first experience improvements in their efficiency and then become bigger as a result, makes a causal interpretation of the cross-sectional data difficult. In addition, Kovner et al. (2014) argue that bank size may be correlated with omitted variables, such as the quality of bank management.

The evidence from bank consolidations (mergers and acquisitions) is also mixed (Rhoades 1998; Berger et al. 1999; Calomiris 1999; Focarelli and Panetta 2003). A challenge for this literature is that consolidations are not random. For instance, Focarelli et al. (2002) find that consolidating banks and the quality of their loan portfolios differ systematically from other banks. Calomiris and Karceski (2000) argue that it is difficult to find appropriate control groups when analyzing bank mergers. These considerations make it hard to draw conclusions from consolidations about whether bigger banks are more efficient.

This paper contributes to the literature by identifying bank consolidations whose timing was exogenous to the growth of banks and their borrowers. This allows me to estimate how a shock to bank size causally affected bank efficiency and real outcomes. I study how bank size influenced several outcomes that are relevant to the real economy, including the growth of firms and municipalities, bank efficiency, and bank risk-taking. The results on media mentions provide new evidence about the benefits to managers of large firms.

The findings on opaque firms in this paper relate to the literature on how big banks interact with small firms. Berger et al. (1995) show that, in the cross-section, big banks lend proportionally less to small firms. Berger et al. (2005) find that firms located in markets with larger banks rely more on trade credit, indicating that these firms are credit constrained. The evidence from bank consolidations is mixed, possibly because bank consolidations do not occur randomly (Berger et al. 1998; Peek and Rosengren 1998; Strahan and Weston 1998; Berger et al. 2001; Sapienza 2002; Jagtiani et al. 2016). I demonstrate that negative effects appear not just for small firms, but also for young and low-collateral firms. My empirical strategy uses exogenous variation in the size of the same bank serving the same firm. This strategy overcomes concerns that the non-randomness of bank consolidations or unobservable cross-sectional differences across regions, firms, and banks bias the estimated effects. Furthermore, I go beyond lending outcomes and show that there are real effects on the employment growth of opaque firms when banks get bigger.[4]

Other papers investigate specific channels, through which bank size can affect efficiency. Houston et al. (1997), Gilje et al. (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 et al. 2013), reduces bank risk (Goetz et al. 2016), and lowers funding costs (Levine et al. 2016). In contrast to these papers, I do not focus on specific channels but estimate the overall effects of bank size, which may be partially driven by these channels.

An important literature studies banking deregulation in the United States, starting with seminal

[4]A related literature focuses on bank hierarchies. Hierarchical organizations rely less on soft information (Cole et al. 2004; Liberti and Mian 2009; Cerqueiro et al. 2011; Canales and Nanda 2012). Internal communication costs affect the quality of information produced by bank employees (Qian et al. 2015). Adding hierarchical layers to Indian bank branches, keeping overall bank size constant, reduced lending and loan performance (Skrastins and Vig 2018).

papers by Hubbard and Palia (1995) and Jayaratne and Strahan (1996). This literature has crucially shaped our understanding of how banks can affect the real economy.[5] Several papers emphasize that changes in local competition (i.e., increases in the number of banks in a market and the threat of new bank entry) were chiefly responsible for the effects of deregulation (Jayaratne and Strahan 1998; Stiroh and Strahan 2003; Evanoff and Ors 2008).[6] In contrast, the banking reforms in postwar Germany increased bank size without directly deregulating the number of banks in each market or the threat of entry.[7]

In related historical work, Eichengreen and Ritschl (2009) analyze the German postwar economic miracle, while Hoshi and Kashyap (2004) describe how the U.S. occupiers restructured corporate financing in Japan without breaking up the banks.

III Institutional Details and Theoretical Mechanisms

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

III.A Relationship Banking in Germany

Three types of banks operated in postwar Germany: commercial banks, cooperative credit unions, and public banks (Landesbanken and savings banks). Banks offered their customers the range of universal banking services. Most important were lending, deposit taking, payment transactions, and the underwriting of corporate bonds and stocks. Commercial banks all operated for profit, unlike the cooperatives and public banks. At the end of 1951, there were 131 commercial banks (excluding small, single-branch private banks, Deutsche Bundesbank 1976). Most were active within only

[5]Deregulation raised managers' performance incentives and pay (Hubbard and Palia 1995), 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 et al. 2017). It lowered growth volatility (Morgan et al. 2004), income volatility (Demyanyk et al. 2007), and income inequality (Beck et al. 2010).

[6]Beginning in the 1970s, U.S. states allowed banks to operate multiple branches in one state (intrastate branching) or to own banks in another state (interstate banking). Both provisions had direct effects on competition in local banking markets "by allowing banks to enter new markets and threaten incumbent banks" (Stiroh and Strahan 2003). In particular, intrastate branching raised the actual number of competitors in local markets. It also increased the threat of new banks entering, which put competitive pressures on incumbent banks even with no actual entry (Jayaratne and Strahan 1996). Interstate banking raised competitive pressures by affecting the number of participants in the market for corporate control (Hubbard and Palia 1995).

[7]Banking was a local business in postwar Germany. Horstmann (1991) explains that, in general, state-level treated banks that had belonged to the same national banking group did not compete with each other across state borders. In line with this historical narrative, the data on bank-firm relationships in 1951 show that 99 percent of firms had treated relationship banks only in the state of their headquarters. The exceptions may be explained by firms operating multiple establishments. While local competition was unaffected, the treated banks may have gained market power in cross-state interbank markets. I explore this possibility below, by testing whether the effects differed in states where interbank markets were important.

one state, although a few had branches in two or three states. Firms had high demand for the banking services of commercial banks in postwar Germany, in particular for loans, bond and stock underwriting, and export financing (Holtfrerich 1995, page 544).

Economic history (Jeidels 1905; Calomiris 1995), case studies (summarized in Guinnane 2002), and recent evidence (Harhoff and Körting 1998b; Elsas and Krahnen 1998; Elsas 2005) suggest that relationship banking has played an important role in German corporate finance since the start of the 19th century. Firms of all sizes formed close and durable business ties to their banks. These ties reduced asymmetric information and improved banks' monitoring capabilities (Sharpe 1990; Boot 2000). An important feature of a relationship-based system is that firm-bank relationships are sticky. As a result, idiosyncratic shocks to banks have real effects on their relationship borrowers.[8]

III.B The Timeline of 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 (Horstmann 1991).

Phase 1: State-level Breakup 1947/48-52 During the initial years of the occupation, the American objective was to reorganize the German economy, so that it would not be able to support another war in future. The Dodge Plan of 1945 argued that the centralized banking system and the cooperation between politicians and banks had helped the Nazis to fund the war. As a result, the Americans set out to break the economic and political influence of large centralized banks (Adler 1949). They focused their regulatory efforts on three banks with nationwide branch networks that were still active after the war: Commerzbank, Deutsche Bank, and Dresdner Bank. I refer to these three banking groups as "treated."

In March 1946, the American and French military governments prohibited the treated branch managers in their zones from exchanging business orders and cooperating with branches in other zones. This measure meant that in practice the treated banks did not operate a nationwide branch network from 1946 (Wolf 1993, page 28). In May 1947, an American military law formalized the breakup in the American zone. The law created new state-level banks, composed of the treated bank branches. The state-level banks were not allowed to operate a branch in another federal state. Their directors were regional and national managers of the former national banks. The names of the new institutions were unrecognizable from the former national names, to underscore that the

[8] A large empirical literature has shown this for many periods and countries, for instance Doerr et al. (2018) for the German banking crisis of 1931, Benmelech et al. (2017) for the U.S. Great Depression, Amiti and Weinstein (2011) for Japan from 1990 to 2010, and Chodorow-Reich (2014), Bentolila et al. (2018), and Huber (2018) for the 2008-09 crisis in the United States, Spain, and Germany. 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.

newly formed entities were separate from each other (Der Spiegel 1951). Government-appointed custodians, independent and unconnected to the former banks and their managers, were in charge of ensuring that the state-level banks operated independently (Adler 1949). The custodians supervised the management of the banks and formally administered the banks' property (Horstmann 1991, page 169). The internal structure of individual bank branches was not directly affected by the breakup. The relationship between branches and their customers, the financial services offered by the banks, and the market share of branches in their local banking markets also remained unchanged (Adler 1949). What changed was that each branch now belonged to a much smaller state-level bank organization, rather than to a national bank.

The French military government issued an identical decree for its zone in September 1947. The British were initially against the breakup because 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 a similar regulation in their zone. Allied legislation meant that there were now 30 separate state-level banks, whose branches had belonged to three national banking groups before the war.[9] Panel A of Figure I shows a map of the state-level banking zones. The Allied laws did not directly affect the other commercial, cooperative, or public banks.

The treated state-level banks acted as "companies, with autonomous management, independent custodians, and distinct business policies" and were "institutes that appeared unrelated to the former national banks" (Horstmann 1991, page 151). Analyzing the case of Deutsche Bank, Holtfrerich (1995, page 484) concludes that the bank "was decentralized after April 1, 1948, for all practical purposes." Below, I outline in more detail the different channels through which the banking reforms affected the operations of the treated banks.

The Allies initially intended the breakup to the state level 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. The motivation for these breakups was similar to the philosophy behind the banking breakup: to reduce the likelihood of a future war by decentralizing companies that had been linked to the Nazi government. Unlike in the case of banking, German politicians did not believe these other industries would generate significant economies of scale. Hence, these organizations were not allowed to reconsolidate to their former structures in sovereign Germany, despite the wishes of their management (Kreikamp 1977). The examples of these permanent breakups show that the reconsolidation of the treated banks was not a foregone conclusion. Assessing the situation in 1949, Horstmann (1991, page 181) calls any hopes for national reconsolidation that may have existed among the bank managers "wishful thinking."

[9]To be clear, consider 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. Deutsche Bank had not previously operated branches in Schleswig-Holstein, so there were 10 Deutsche Bank successors. Commerzbank had not previously operated branches in Baden and Württemberg-Hohenzollern, so there were 9 Commerzbank successors.

Phase 2: Three Banking Zones 1952-57 By the early 1950s, the American diplomatic stance toward West Germany had changed, with Germans being considered "friendly foes" (Scholtyseck 2006). In line with the view that a stable German economy could stop the spread of Communism through Europe, the American military government became more open to German suggestions about how to facilitate economic growth (Holtfrerich 1995, page 496; Ahrens 2007). Managers of treated banks had always maintained that operating as separate banks harmed the efficiency of their banks, reduced credit supply, and had negative real effects (Ahrens 2007). The German federal government and the British largely concurred (Horstmann 1991). But there was opposition from some German authorities. Leading politicians in the southern states and the presidents of the state central banks believed that state-level banks supplied credit more efficiently and that they were easier to regulate (Horstmann 1991). The Americans made it clear that they were willing to consider some form of partial reconsolidation among the treated banks, but would veto complete reconsolidation at the national level (Holtfrerich 1995, page 503).

German politicians, Allied military governments, treated bank representatives, and central bankers negotiated for several months. Their compromise, reached in 1951 and passed as law in March 1952, set precise rules stipulating how the state-level banks would be allowed to consolidate. The new law defined three banking zones, shown in Panel B of Figure I. 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. Because the borders of the state were identical to the borders of the new zone, the treated banks operating in the state of North-Rhine Westphalia remained unaffected by the 1952 reform. The empirical strategy outlined below exploits the particular treatment of the banks in North-Rhine Westphalia to construct a control group for the 1952 reform.

The treated banks were not forced to reconsolidate. But the vast majority of treated bank directors believed that they would benefit from reconsolidation. Hence, all the state-level banks in the northern and southern zones decided to consolidate in September 1952. Instead of 30 state-level banks, there were now nine treated banks, one for each former national bank in each banking zone (Wolf 1993). The directors of the former state-level banks became the board members of the nine new banks.

The rules of the breakup that had applied to the state-level banks remained in place for the zonal banks, but the Allies did not enforce the rules as strictly as before 1952. For example, the directors of the successor institutes of Dresdner Bank met formally around six times a year, starting in late 1952, and so did the directors of the Deutsche Bank successor banks (Ahrens 2007; Holtfrerich 1995). To a large extent, these meetings were devoted to coordinating the banks' lobbying with the Allies to achieve reconsolidation at the national level. As a result of the weaker enforcement of the breakup after 1952, my analysis below focuses on the effects of the 1952 reform, generally comparing bank and firm outcomes before and after 1952.

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 made Germany a key ally of the West. The Allies granted the German government full sovereignty in the Paris Agreement of 1955. One condition for sovereignty set by the Allies was that the treated banks would remain separated at least until 1956. The German federal government had always believed in the efficiency of large banks, so it lifted all restrictions as soon as permitted, from January 1957 (Scholtyseck 2006). And since the managers of the treated banks had maintained their conviction that reconsolidation was optimal, the banks soon consolidated. By 1958, there were once again three large banks with a national branch network, operating under their old, prewar names. All directors of the former, zonal banks joined the boards of the new national banks (Horstmann 1991; Holtfrerich 1995).

III.C The Effect of the Reforms on Bank Operations and Theoretical Predictions

The consolidations of the treated banks, caused by the banking reforms of 1952 and 1957, affected how the treated banks operated through various channels. Economic theory suggests that some channels may have improved bank efficiency, while others may have reduced it. The overall effect is theoretically ambiguous. In what follows, I describe how each channel changed bank operations and the theoretical predictions for each channel.

Diversification The first effect of the bank consolidations was to sharply increase the number of borrowers served by one treated institution. Models by Leland and Pyle (1977), Diamond (1984), Boyd and Prescott (1986), and Williamson (1986) show that banks with a larger number of borrowers can diversify more cheaply and therefore operate more efficiently. According to these models, the treated banks should have become more efficient after the reforms. Contemporary observers made similar predictions. Holtfrerich (1995, pages 500, 505) quotes several branch managers of treated banks who argued that increases in the number of borrowers per bank would improve risk diversification. Before the 1952 reform, the academic Lanner (1951) wrote that treated bank consolidations would "allow greater diversification of banking risks."

Internal Capital Markets A second potential benefit of big banks is that they can use large internal capital markets to allocate funds. Before 1952, the treated banks were allowed to operate interbank accounts, but had to settle their monthly balances through the central banking system, just like the other commercial banks (Adler 1949; Wandel 1980). Historical records show that treated banks with a strong deposit base regularly lent through interbank markets before 1952 (Wolf 1994). After consolidating, they were able to allocate capital across states internally. Horstmann (1991) argues that interbank markets and central clearing were well-developed in postwar Germany, although he leaves open the possibility that external refinancing was more expensive than internal capital mar-

kets. Bank lawyer Fritz Kempner argued in 1950 that central bank clearing made capital transfers more expensive (Holtfrerich 1995, page 505).

Stein (1997) shows that the use of internal capital markets is optimal when external financial markets are underdeveloped. If internal capital markets created significant gains, the treated banks should have become more efficient after the reforms. 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, access to larger internal capital markets could actually have been detrimental to the efficiency of the treated banks.

There continued to be some informal communication between the managers of the broken-up banks before 1952 (Horstmann 1991, page 179; Holtfrerich 1995, page 486). This might have facilitated interbank lending. Thus, the findings of this paper need to be interpreted as moving from a situation where interbank market participants knew each other well to a situation where they worked in the same institution. The German Ministry of Economics noted that the successor institutes of the Dresdner Bank communicated among each other most frequently. In addition, communication was more common among banks in the British occupation zone because the British enforced the breakup rules less strictly (Ahrens 2007, page 247). In the analysis below, I explore heterogeneity across the three treated banking groups and across occupation zones to test whether the intensity of communication across treated banks affected the effects of the reforms on borrowers.

Large Capital Base and Fixed Costs The third benefit concerns the larger capital base of big banks. Big banks can spread fixed costs across more borrowers and fund larger loans on their own. Treated branch managers expressed concerns before the reforms about high overhead costs from operating separate legal departments and separate payment transactions systems, and from employing specialized credit experts for each industry (Der Spiegel 1951; 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 fixed costs and contracting frictions for loan syndicates are high, the cost of lending should have fallen after the reforms.

Organizational Complexity The reforms increased the organizational complexity and the number of hierarchical levels of the treated banks. For instance, during the first phase of the breakup, each treated state-level bank made decisions about loan applications independently in regionally specialized credit councils (Horstmann 1991, page 170). After the reforms, a centralized decision-making structure took over. Williamson (1967) argues generally that transmitting accurate information to decision-makers is difficult in large organizations. 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), individual bankers have limited time. This means that the marginal cost of lending to an additional borrower is increasing.

Processing Soft Information Models by Stein (2002), Berger and Udell (2002), and Brickley et al. (2003) suggest that 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, such as those whose small size, youth and scant collateral make documenting creditworthiness difficult. Such firms rely on their banks to assess soft criteria and to issue "character loans", for example. The more centralized 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 the loan supply to opaque relationship borrowers of the treated banks (as in Stein 2002).

Risk-taking A final theoretical consideration is not about the efficiency of big banks, but about their risk-taking. Big banks may take socially inefficient, excessive risks. One cause of excessive risk-taking could be moral hazard. Big banks are more systemically important (Pais and Stork 2013; Adrian and Brunnermeier 2016). As a result, governments may be more likely to bail out big banks when they become insolvent (Freixas 1999). Dávila and Walther (2017) argue theoretically that big banks internalize the increased probability of a bailout and take more risk than small banks.

Another cause of excessive risk-taking may be agency problems. The hierarchical distance between bank directors and local branch managers is larger in big organizations. Directors of big organizations find it more difficult to directly monitor local bank managers and to understand local risks. Instead, they may reward local managers based on outcomes. Such outcome-based reward schemes can distort incentives. If the risk pays off, bank managers may reap the benefits, for example by earning promotions. They may not suffer severe 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 caused losses. If the upside benefits outweigh the downside risks in such a manner, the local managers in big organizations have an incentive to take excessive risks (Rajan 2005; Kashyap et al. 2008).

Greater risk-taking could imply that banks lend more to all firms or that banks lend disproportionately to risky firms. I explore both possibilities in the empirical analysis below.

IV Empirical Strategy

The banking reforms of 1952 and 1957 provide suitable quasi-experiments that allow estimating the causal effects of bank size. The treated banks favored consolidation throughout the postwar period. This makes them comparable to many other banks in the past and present that want to merge. The key difference is: Government reforms determined how and when the treated banks consolidated. The reforms led to increases in bank size independent of other shocks to borrower growth or bank efficiency. A theoretical model in Appendix B illustrates how the reforms help to overcome the usual empirical challenge in estimating the effects of bank size on firms.

To assess how the reforms and the resulting increases in bank size affected firms, I regress measures of firm growth on an indicator for whether one of the firm's relationship banks was treated by a reform. The coefficient on this indicator captures all the channels, through which a change in bank size could affect firms.[10] The coefficient estimates the causal effect of having a treated relationship bank on firm growth if a parallel-trends assumption holds. This assumption requires that, had it not been for the reforms, firms with a treated relationship bank would have grown in parallel to other firms. The results sections below present evidence in support of the identification assumption, including parallel pre-trends and balancing tests of firm and bank observables. To further strengthen the assumption, the regressions condition on control variables, described in the relevant results section. Throughout the paper, I use robust standard errors.[11]

Three additional analyses supplement the main analysis 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 firm-level analysis, namely that the treated banks and municipalities with a treated bank branch would have evolved in parallel to other banks and municipalities in the absence of the reforms.

V Data on Firms

V.A Firm Data Collection

At the heart of the paper lies a newly digitized dataset on the relationship banks and the growth of German firms in the 1950s. The sources for the firm data are two publication series by the commercial information provider Hoppenstedt. The historic volumes of these series are difficult to locate.[12] 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 makes automatic digital character recognition impossible. Instead, I photographed all pages from these publications, around 15,000 photographs in total. The firm data were then digitized by hand. Figure A.I displays a photograph of a page in the 1952/53 volume on *Aktiengesellschaften.*

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

[11] In robustness checks, I used three alternative methods to calculate standard errors: clustering at the level of the federal state; clustering at the level of the state-level treated banks (i.e. 31 categories, one for each of the 30 treated state-level banks and one for firms with no treated relationship bank); and using the correction for clustered standard errors from Young (2016). All these methods produce similar standard errors to the baseline method (as shown in columns 5 and 6 of Table A.IV.)

[12] Hoppenstedt destroyed its entire archive a few years ago when the company moved buildings. Online library catalogs do not always report the holdings accurately because historic volumes are often misplaced or destroyed. Based on my experience, I recommend that researchers interested in accessing volumes in a far-away library ensure they are still in place before traveling.

The publication on *Aktiengesellschaften* reports data on the universe of German stock corporations, while *Grossunternehmen* includes a subset of firms of other legal forms.[13] In the postwar years, both publications list the firms' names, addresses, number of employees, and names of relationship banks. There is no information on which financial services or how much lending 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 some 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 the records of 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 fuzzy match procedure (Stata command "reclink") to connect firm entries from the 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 the aggregated observation 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 employment in 1949, so I can calculate the pre-reform growth of these firms from 1949 to 1951. As measure of growth, I use 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 exits (Davis et al. 1998).[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*

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

[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.

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 et al. 2015). I match 373 firms between the 1941 and 1952/53 volumes and 4,191 firms between the 1952/53 volumes and 1970/71 volumes.

V.B Summary Statistics on Firms

Table A.I 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 had close to 350 employees in 1951. The very largest firms were stock corporations, which means that the average stock corporation was larger than the average non-stock firm. Both stock capital and bank debt were important sources of stock corporations' financing, amounting to an average of 37 percent and 10 percent of total assets, respectively. 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. As a rough comparison, the fraction of establishments in the population with fewer than 50 employees was 98 percent in 1951 (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, to ensure differences in size and industry do not drive the findings. I also explore heterogeneity related to size and industry.

In 1951, stock corporations had 3.2 relationship banks on average. Non-stock firms had 2.5 on average. 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 had a relationship bank treated in 1952 or 1957. 46 percent and 41 percent had a relationship bank treated in 1952.

V.C Sample Balancing Test and the "Focused" Sample

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 A.II. 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. Being an exporter was uncorrelated with having a treated bank.

I additionally create a more restrictive, "focused" sample. There are four restrictions for the focused sample. First, the focused sample only includes firms that had a relationship bank that was treated in either 1952 or 1957. This restriction addresses the concern that firms with a treated relationship bank fundamentally differed from firms with banks that were never treated. Second, 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). Third, I drop from the sample firms located in the Ruhr area, an urban region within NRW traditionally based on heavy industry that was potentially exposed to different economic shocks than the rest of the country. Fourth, 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.

Regressions using the focused sample identify the effect by comparing relationship borrowers of banks treated in both 1952 and 1957 (located in states bordering NRW) to borrowers of banks treated only in 1957 (located in NRW).[16] The use of the focused sample strengthens the parallel-trends assumption because the restrictions make it likely that all firms in the focused sample were affected by similar unobservable shocks.

The regressions in columns 5 and 6 use the focused sample. The outcome of interest in the focused sample is whether a relationship bank was treated in 1952. There is no significant correlation between having a bank treated in 1952 and size or age, for either stock corporations or non-stock firms. These results strengthen the argument that the focused sample provides a credible quasi-experiment, since observationally equivalent firms were exposed to differential bank size shocks.[17]

VI Results on the Growth of Banks and Firms

This section presents the main results of the paper. I begin by analyzing the effect of the consolidations on the treated banks' aggregate lending and deposits. I then examine the growth of firms that had a treated relationship bank.

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

[17] Unreported additional tests also reveal no correlation between treatment and firm stock capital financing, bank debt financing, and export status in the focused sample. The improved sample balance in the focused sample is mainly due to the first restriction of only using firms with a relationship bank treated in either 1952 or 1957. The results on firm growth presented below similarly hold when using only the first restriction.

VI.A Effects on Aggregate Bank Loans and Deposits

I first examine total lending and deposits of the treated banks over time. The Deutsche Bundesbank reports data aggregated at the level of different groups of banks, starting in 1948. Figure II plots the aggregate stock of lending and deposits to firms and households (non-banks) for two groups of banks. The treated group includes all banks treated by either the 1952 or the 1957 reform. The untreated group includes lending by other German commercial banks (not including small, single-branch private banks). These other commercial banks are the most comparable group to the treated banks in terms of structure and business policy. They all operated for profit. The treated banks were responsible for 21 percent of lending to non-banks in Germany in 1951, the other commercial banks for 14 percent.

Panel A of Figure II shows that before the 1952 reform, total lending by treated and untreated banks evolved in parallel. This implies that the treated banks and their borrowers were not exposed to different shocks than the untreated banks and borrowers. In addition, it suggests that the treated banks had adjusted to their state-level operations by 1948 and that the breakup did not have persistent effects on the treated banks. The parallel pre-trend strengthens the identification assumption, which requires that, had the reforms not taken place, treated and untreated banks and firms would have evolved in parallel.

After the 1952 reform, the loan growth of the treated banks slowed relative to that of the untreated group, and continued to do so after the 1957 reform. Panel B of Figure II shows that the growth pattern of deposits mirrored that of lending. Deposits of the treated banks grew in parallel to the untreated group before the 1952 reform and more slowly thereafter. One key aim of the treated banks in the postwar period was to increase their market share in lending and deposit taking (Ahrens 2007). They competed for market share with the untreated commercial banks in local banking markets.[18] 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 loan terms. Figure II provides no evidence that the treated banks were able to do this.

Figure A.II extends the data for an additional 10 years. The gap between lending by treated and untreated banks remains roughly constant. This suggests that the treated banks did not suffer from temporary adjustment costs, but that they persistently did not lend more than the untreated banks after the reforms. As robustness check, Figure A.III uses all other banks in the untreated group, including the credit unions and public banks (1951 market share in lending of 79 percent). The

[18] Statements of bank directors support this view. For example, the directorate of the Rhein-Main-Bank (the Dresdner Bank successor in the state of Hesse) encouraged its staff to "poach customers" from other banks, by carefully "working through incoming mail to scout for new business opportunities (for example, by reading between the lines)" (circular to all branches from August 23, 1948, quoted in Ahrens 2007, page 227). The Nazis had believed that competition in the banking sector led to instability. They instituted a ceiling on deposit interests rates in 1936 (the *Zinsabkommen*), which was officially still in place after the war. There was no such regulation for the lending rate, only a "recommendation" (Herlan 1952, page 656). In practice, banks in postwar Germany largely ignored rate regulation and recommendations. They paid competitive deposit rates and outbid each other on provisions and other costs of financial services (Die Zeit 1954; Wolf 1998, page 70). Interest rate regulation was formally abolished in 1967.

relative growth of treated and untreated groups is similar.

VI.B Effects on the Growth of Stock Corporations

Data on aggregated lending and deposits cannot fully reveal whether the consolidations affected firm growth. For example, the treated banks may have improved the terms on loans and deposits, the quality of payment services, underwriting, and financial advice. To investigate this possibility, I turn to firm data.

I begin by examining the growth of stock corporations. I observe data for stock corporations in 1949, 1951, and 1960. In Table I, I use four outcome variables: the growth of bank debt, bank debt over total assets, employment, and revenue per worker between 1951 and 1960. The key 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.

The outcome variable in Panel A of Table I is the average annual growth rate of bank debt between 1951 and 1960.[19] If the reforms led to an increase in firms' bank loan supply, the coefficient on the treated bank dummy 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 point 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 concern is that broad regional differences or heterogeneous shocks to certain industries may bias the estimate. Column 2 includes the full interaction of industry fixed effects with fixed effects for the northern, western, and southern regions of Germany, equivalent to the banking zones of the 1952-57 period.[20] To account for variation in growth due to firm size and age (Haltiwanger et al. 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 that region-specific shocks to firms in certain industries, of certain sizes, or of certain ages do not affect the results. The coefficients remain close to zero and statistically insignificant.

Column 4 restricts the sample to stock corporations with a high (above-median) ratio of bank debt over assets in 1951. These firms particularly depended on bank debt for financing. Column 5 includes only stock corporations with a low ratio of stock capital over total assets in the sample, i.e., firms with high leverage that required more outside financing in general. The literature shows that both types of firms react more strongly to shocks to their banks (Bentolila et al. 2018; Huber 2018). If one believes that the insignificant coefficients for the full sample in columns 1 to 3 are

[19] To be clear, the outcome in Panel A of Table I is: the symmetric growth rate of bank debt from 1951 to 1960 divided by 9 (the number of years between 1951 and 1960). This transformation makes it easier to compare the point estimates to later results, which use data for periods of different lengths.

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

due to noise, which masks a positive effect, the point estimates in columns 4 and 5 should be larger. However, the estimates remain small and insignificant. Overall, there is no evidence that firms with treated banks took out more bank loans than other firms.

The outcome in Panel B is the average annual change in the ratio of bank debt over total assets. Using the change in the ratio as outcome is conceptually similar to controlling for changes in firms' total funding by using firm fixed effects. If firms with a treated relationship bank had access to cheaper bank debt, they should have funded themselves with more bank debt relative to other funding sources. This would raise the ratio. 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 is equal 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 (a measure of firm productivity). The 95 percent confidence intervals exclude growth increases greater than 0.9 and 1.4 percentage points, respectively.

Table A.III examines employment growth of firms from 1949 to 1951. (Data on other balance sheet outcomes are not available for this period.) There is no evidence that firms with a treated relationship bank grew differently to other firms before the 1952 reform. The point estimates in specifications without controls (column 1) and with controls (columns 2 and 3) are small and insignificant. This holds for both non-stock firms and stock corporations (column 5). Consistent with the data on aggregate lending and deposits, these results suggest that firms with a treated relationship bank would have grown on parallel trends to other firms, had the 1952 reform not happened.

Other papers studying the effects of banking shocks on firms report large estimates compared to the coefficients in this paper. For instance, Liberti et al. (2016) find that the introduction of a credit registry in Argentina increased lending to affected firms by 61 percent within two years. Bertrand et al. (2007) find that the 1980s deregulation of the French banking sector increased employment in bank-dependent industries by 23 percent relative to other industries (moving from the 25th to 75th percentile of the industry bank debt-to-assets ratio). 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 et al. 2016). Among Spanish firms attached to weak banks, annual bank debt growth declined by 1.3 percentage points and annual employment growth fell by 0.7 percentage points from 2006 to 2010 (Bentolila et al. 2018). The large magnitude of these effects, 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.

VI.C Effects on the Growth of Non-Stock Firms

The data allow me to observe employment of non-stock firms in 1949, 1951, and 1956. Table II uses the average annual employment growth from 1951 to 1956 as outcome variable. The regressor of interest is a dummy for whether the firm had a relationship bank that was treated in 1952 (i.e., a treated bank outside of the western state of North-Rhine Westphalia, NRW).[21]

The sample in columns 1 and 2 of Table II contains all non-stock firms with available employment data. The untreated group in columns 1 and 2 includes firms with banks that were neither treated in 1952 nor in 1957, as well as firms with banks that were only treated in 1957. The point estimate in column 2, using the full sample with all controls, implies that 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 sample in columns 3 and 4 uses the more restrictive, "focused" sample, which provides a cleaner quasi-experiment, as explained in Section V.C. The untreated group in columns 3 and 4 includes only firms with banks treated in 1957 (located in NRW), while the treated group includes only firms in states bordering NRW. The focused sample does not include any firms located in the Ruhr region or coal and steel producers. The point estimate in column 4 also implies an insignificant growth decrease of 0.1 percentage points. The 95 percent confidence interval rejects improvements above 1.2 percentage points. The coefficients in the full and the focused samples are similar. This suggests that unobservable shocks are not correlated with the treatment indicator in the full sample, in line with the identification assumption. Table A.III shows that firms with a relationship bank that was treated in 1952 grew in parallel to other firms from 1949 to 1951 (column 4), further strengthening the identification assumption.

Columns 5 and 6 restrict the sample to firms that export some of their products, as reported in the *Grossunternehmen* volumes for non-stock firms. Due to higher default risk and working capital requirements, exporters are likely to respond strongly to financial shocks (Amiti and Weinstein 2011). The coefficients in columns 5 and 6 remain small and statistically insignificant, suggesting that exporters did not grow faster after their bank consolidated in 1952.

The rules of the breakup from 1947 to 1952 were enforced most strictly in a few southern states: Baden, Bavaria, Hesse, Rhineland-Palatine, Württemberg-Baden, and Württemberg-Hohenzollern. One reason was that the states were occupied by the Americans and the French, the originators of the breakup plan. Furthermore, the southern state governments generally were in favor of state-level banks (Horstmann 1991, pages 231, 250) and considered the 1952 reform to be problematic (Wolf 1993). An order from 1947 suggests that the state government of Württemberg-Baden enforced the rules of the breakup "religiously" (Wolf 1994). This means that the 1952 reform likely had the largest effects on bank operations in the strict southern states. In columns 7 and 8 of Table II, I use

[21] Because data on assets do not exist for non-stock firms, I control for size using fixed effects for four bins of firm employment (0-49, 50-249, 250-999, and 1000+). I do not use the zonal fixed effects in columns 3 and 4 because the focused sample identifies the effect using only cross-zonal variation.

only the strict states in the sample. The coefficients are small and insignificant. The point estimate in the specification with all controls is exactly zero, suggesting the consolidations had no effect on firm growth in the strict states.

VI.D Additional Tests Using the Growth of Non-Stock Firms

I carry out additional tests using the employment growth of non-stock firms as outcome.[22] The first test examines the role of internal capital markets. One theory underpinning the view that bigger banks are more efficient is that big banks can use internal capital markets instead of interbank markets. This is a potentially important channel since the treated banks financed 33 percent of their assets with deposits from other banks in 1951 (Deutsche Bundesbank 1976). Figure A.IV examines the evolution of interbank lending and deposits. After the 1952 reform, the treated banks increased interbank lending and deposits by less than the untreated group.[23] To test whether firms benefited from the increased ability of their relationship banks to use interbank loans, I analyze whether firms in federal states that had a persistent capital account deficit grew faster after the bank consolidations (Pohl 1971, page 40). Banks and firms in capital account deficit states were more dependent on capital inflows from other parts of Germany. Hence, they were most likely to benefit from internal capital markets. The point estimates in column 1 of Table A.IV show that firms in capital account deficit states did not grow faster than firms in capital surplus states. The effect for both types of states is small, negative, and insignificant. This suggests that any potential gains from internal capital markets did not have differential real effects on borrowers in states with capital account deficits.

A potential concern with the identification strategy is about firms that participated heavily in war-related production during World War II or firms that were punished by the Allies after the war. These war-related firms may have grown more slowly in the postwar period, because they had to restructure their business model or because of Allied punishment. To address this concern, I exclude from the sample firms that the Reichswehr had identified as important for armament production.[24] I also exclude all industries that produced war-related products: mining, clothes & textiles, chemicals & pharmaceuticals, metals manufacturing, electric & electronics, and production of machinery. Column 2 of Table A.IV reports the result. The point estimate is marginally smaller than the baseline effect, negative, and statistically insignificant. This indicates that war-related firms do not affect the results.

Columns 3 and 4 of Table A.IV use different treatment variables. Column 3 shows that firms,

[22] In unreported results, I find similar results when I carry out the same tests on the sample of stock corporations.

[23] Two mechanisms could have been at play. First, the treated banks may have substituted from interbank market transactions with untreated banks to internal transactions within their enlarged organization. Second, capital flows might have remained the same, but their classification might have changed. (I.e., a cross-state deposit among treated bank branches of the same prewar banking group was an interbank deposit before the reforms but an internal deposit after 1957.) Both mechanisms could have lowered costs if interbank markets were more expensive, for example due to central bank clearing.

[24] Anlage Nr. 6, page 217 in Hansen (1978) lists firms that prepared for the production of armament material. Anlage Nr. 10, page 226 lists firms that were important providers of inputs for armaments production.

for whom more than half of their relationship banks were treated, did not experience significantly faster employment growth. Column 4 shows that there was no heterogeneity in the treatment effect by whether the firm had a relationship bank belonging to the former Commerzbank, Deutsche Bank, or Dresdner Bank. The German Ministry of Economics noted that the successor institutes of the Dresdner Bank communicated among each other most frequently (Ahrens 2007, page 247). The absence of significant heterogeneous effects across the three treated banking groups suggests differences in communication among treated bank managers did not affect the growth of their relationship borrowers.

Table A.V 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. Opaque firms grew more slowly after the reforms. The effects are similar for all three treated banking groups.

VI.E Effects on the Growth of Newly Added Relationship Borrowers

The analysis so far has focused on the existing relationship borrowers of treated banks. Next, I examine whether the consolidation of the treated banks improved the growth of firms that were newly added as relationship borrowers from 1952 onward. There are now two regressors of interest: a dummy for firms with a treated relationship bank in 1951 plus a new regressor for firms that had no treated relationship bank in 1951 but added a treated bank as relationship bank between 1951 and 1960. Firms that added a new relationship bank are likely to have higher loan demand than other firms, which might introduce bias in the analysis. To overcome this endogeneity problem, I restrict the sample to firms that increased the number of their relationship banks from 1951 to 1960. The idea is to only compare firms with increased loan demand. The sample contains firms that had a treated relationship bank before 1952, firms that added a treated bank as relationship bank after 1952, and firms with no treated relationship bank at any point.

The results are in Table III. The sample in columns 1 and 2 contains only stock corporations, while the sample in columns 3 and 4 contains only non-stock firms. The point estimates in all specifications are small and statistically insignificant. There is no evidence that newly added relationship borrowers of treated banks grew faster than other comparable firms.

VI.F Effects on the Growth of Opaque Firms

A potential disadvantage of big banks is that they may be worse at dealing with opaque firms, which requires collecting and processing soft information. To create a systematic classification of opaque firms, I use 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 for lenders to assess. Studies typically use a cut-off of 50 employees to identify small firms (Gertler and Gilchrist 1994;

Chodorow-Reich 2014). 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 had fewer than 50 employees, were younger than 10 years old in 1952, or were in the bottom 10 percent by industry asset tangibility.

Table IV 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 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 fewer than half of relationship banks 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 regressor 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 focused sample in column 7. The effect on firms, for which fewer than half of relationship banks were treated, is negative, but smaller and insignificant in columns 6 and 7. These estimates suggest that the employment of non-stock firms is more vulnerable to a banking shock than the employment of stock corporations. A likely reason is that non-stock firms cannot fund themselves by issuing additional stock capital.[25]

[25]The literature has most frequently used firm size as a proxy for opacity. Table A.VII 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

In summary, the results in Table IV indicate that opaque firms experienced decreased bank loan supply after the reforms, with negative consequences for the employment of opaque non-stock firms. Table A.VI shows that non-opaque firms were not affected. The coefficients in the sample of non-opaque firms are all close to zero and insignificant (columns 1 and 2). There were no heterogeneous effects by banking group, as the effects on opaque firms are negative and economically significant (columns 3 and 4).

VII Results on the Profitability and Cost Efficiency of Banks

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

The treated banks were universal, commercial, branching banks. To find a set of comparable institutions, I use the banking handbook by Hofmann (1949). Apart from the treated banks, 16 universal, commercial banks with a branch network existed in 1949. I located the 1952 and 1960 annual reports of 9 of these untreated banks (in addition to the reports of the treated banks) in German libraries and archives. I then hand-digitized financial figures from the reports. The annual reports of many treated and untreated banks for the years before 1952 have not been preserved. The treated banks consolidated in September 1952, so the effect of the 1952 reform on the figures from December 1952 is likely small.

Table V compares summary statistics for the treated and the 9 untreated banks. I aggregate figures for the treated banks at the level of the three treated banking groups. The three banks with the largest branch networks apart from the treated banks were Bayerische Hypotheken- & Wechsel-Bank, Bayerische Vereinsbank, and Oldenburgische Landesbank (Hofmann 1949). These three banks had a similar number of branches to the treated banks between 1952 and 1957. The table reports figures for the treated banks, the three comparison banks, and the average for all 9 untreated banks (including 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 groups rose. The average size of the untreated banks naturally remained unaffected. Column 1 shows that average total assets of treated banks in 1952 were 323 million Deutsche Mark, while average total assets of untreated banks were 330 million. Columns 4 to 6 present three cost ratios commonly used to measure bank efficiency. The 1952 values for all banks are relatively close. These numbers indicate that, in terms of size and cost efficiency,

coefficients for the smallest firm size bins of 0-9, 10-19, 20-29, and 30-39 employees are all negative. The point estimates for the larger firms are of smaller magnitude and insignificant. 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).

the untreated banks are a suitable control group for the broken-up treated banks before 1952.

Table VI reports the growth of financial variables from 1952 to 1960. Panel A examines lending and profit growth. Both measures of treated banks lie well below those of the three comparison banks. Commerzbank, which pursued an aggressive policy of expansion after 1952 (Ahrens 2007), had the relatively strongest lending and profit growth among the treated banks. Nonetheless, it grew more slowly than the three comparison banks. Column 7 reports the average difference between the growth of the treated banks and the 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 over the entire period.

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 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.[26]

Overall, there is no evidence that the treated banks became more efficient or grew faster 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.

VIII Results on the New Relationship Banks of Firms

This section analyzes new banking relationships between firms and treated banks. For each firm, I calculate the fraction of relationship banks that were treated by one of the postwar banking reforms.[27] In 1951, the fraction of treated relationship banks was 36.4 percent for the average firm in the sample. It was 37.4 percent in 1960 and 36.9 percent in 1970. Thus, the difference between 1951 and 1970 is small and not statistically significant. This suggest that, on average, the treated banks did not become more prevalent as relationship banks after consolidating. This is consistent with the

[26] In unreported results, I follow Berger and Mester (1997) and estimate measures of alternative profit efficiency and cost efficiency. These measures indicate how efficiently a bank generates profits or minimizes costs, respectively, conditional on a bank's output choices and relative to the most efficient bank in the sample. Treated and untreated banks displayed similar levels of alternative profit efficiency and cost efficiency in 1952 and improved them similarly until 1960.

[27] For example, if a firm had two relationship banks and one was with a treated bank, the firm-level fraction of treated relationship banks is 0.5. Firms in the sample increased the average number of relationship banks from 2.8 in 1951 to 3.5 in 1970. By using the fraction of treated relationship banks, I account for this increase in the average number of banks. I analyze whether treated banks were more likely than other, untreated banks to be added as relationship bank.

earlier finding that the market share in lending and deposits of the treated banks did not increase.[28]

While there is no significant change for the average firm, there may be heterogeneity in the types of firms that added treated banks as new relationship banks. I test whether opaque and risky firms were more likely to add treated banks as relationship banks. Since the establishment of new relationships takes time (Dwenger et al. 2015), I use firms' relationship banks in 1970 as outcome variable in this section.

VIII.A The New Relationship Banks of Opaque Firms

For the purpose of this section, I define opaque firms as firms with fewer than 50 employees in 1951 or firms in the bottom 10 percent by industry asset tangibility.[29] I begin by focusing on the establishment of new relationships because 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 initially restrict the sample to firms without a treated relationship bank in 1951.

The point estimate in column 1 of Table VII 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 coefficient 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 asset tangibility from 0.15 to 0.2. All four coefficients are negative, indicating that all dimensions of opacity were relevant. Column 3 adds industry and zonal fixed effects to the specification. The coefficient remains robust. This implies that the effect cannot be explained by the treated banks specializing in certain industries and zones.

Column 4 reveals that there was no pre-trend while the banks were small. I restrict the sample to firms that either had no treated relationship bank in 1940 or firms that were founded after 1940. The outcome is the 1951 fraction of treated relationship banks. The coefficient on opaque firms is close to zero and insignificant. This result implies that from 1940 to 1951 the fraction of treated relationship banks did not grow more slowly among opaque firms compared to non-opaque firms.

The analysis so far has focused on the establishment of new banking relationships. Columns 5 and 6 instead restrict the sample to firms with a treated relationship bank in 1951. Column 5 uses the 1951 fraction of treated relationship banks as outcome. The coefficient on opaque firms is positive and statistically insignificant. This suggests that opaque borrowers were not significantly more

[28]The change for all three treated banking groups is relatively small. From 1951 to 1970, the average fraction of relationships with Deutsche Bank group fell by 2.3 percentage points, the fraction with Dresdner Bank fell by 0.1 percentage points, and the fraction with Commerzbank rose by 2.9 percentage points.

[29]This definition differs from the previous one of Section VI.F because it does not include firms younger than 10 years of age in 1952. By 1970, these firms were at least 18 years old, invalidating the argument that they were opaque because they could not have an established reputation and paper trail. In a robustness check, I find that firms founded after 1965 had a lower fraction of treated relationship banks in 1970. 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. A robustness check using firms with fewer than 50 employees in 1970 produces similar results as using 1951 employees.

likely to have a treated relationship bank in 1951. Column 6 uses the 1970 fraction of treated relationship banks as outcome. The point estimate is also positive, insignificant, and almost identical to column 5. This implies that the fraction of treated relationship banks did not change deferentially for existing opaque relationship borrowers relative to existing non-opaque borrowers. This is somewhat surprising since the evidence above suggests that existing opaque borrowers experienced lower credit supply after 1952, while non-opaque borrowers did not. One possible explanation is that credit market frictions make it hard for opaque firms to switch banks when they face reduced bank loan supply.

VIII.B The New Relationship Banks of Risky Firms

Table VIII examines whether risky firms increased the fraction of treated relationship banks following the reforms. 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 measure of firm risk in column 1 is the ratio of stock capital over total assets in 1951. This ratio proxies for funding stability and risk absorption capacity. The higher the ratio, the less likely that the firm will become bankrupt or default on its loans.

The specification in column 1 contains dummies for three bins of the ratio. The bins are for firms with a ratio between 0.25 and 0.5, between 0.5 and 0.75, and between 0.75 and 1. I also include a dummy for opaque firms in the specification, to ensure the results cannot be explained by the effects of firm opacity. 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. The estimates for firms in the second and third bins are statistically insignificant. These estimates suggest 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 reduction in safe relationship borrowers of the treated banks took place within zones and industries.

The third column examines the existing relationship borrowers of the treated banks, by restricting 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 1952, safe borrowers 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 borrowers of the treated banks, consistent with the previous results on opaque firms.

Column 5 uses 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. The coefficient is positive, but imprecisely estimated. The analysis in column 6 is 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 toward risky firms, the treated banks started lending to more productive firms. Column 7 tests 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 that there is no evidence for a move toward productive firms. Alternatively, one might wonder whether the treated banks took more risk in return for serving high-growth borrowers. In unreported tests, I find no evidence that the newly added, risky relationship borrowers of the treated banks grew more quickly than other firms after 1952 (in terms of bank debt, employment, or revenue per worker). If borrower growth is a measure of the expected return to lending for banks, this result suggests that the banks took more risk without gaining a higher return.[30] Under this interpretation, the findings are consistent with theories that suggest big banks take more risks due to moral hazard or bank-internal agency problems.

IX Results on Media Mentions of the Treated Banks

The results presented so far suggest that the treated banks did not become more efficient after the reforms and that their relationship borrowers did not grow faster. So, why were most managers of the treated banks in favor of reconsolidating? The literature on empire-building suggests that managers benefit from running big firms, independent of whether big firms are economically more efficient (Jensen 1986; Stein 2003). One non-economic 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.

Table IX 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 that the name of a treated bank or of a treated bank executive were mentioned in these publications, separately for three periods of equal length

[30] Another way to assess risk-taking is to examine bank leverage (equity capital relative to total assets). Several changes to accounting regulations in the postwar period make it impossible to construct a consistent series for bank equity capital (Horstmann 1991; Ahrens 2007).

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 was 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 times increase) and "Germany" (2.5 times increase) cannot explain this effect. These figures suggest that consolidations can raise the media presence of the involved organizations. One bank of size 10 receives more media mentions than 10 banks of size one combined.

A simple explanation of the results is that the media only report 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 relevant to the entire nation. Independent of the explanation, the causal effect of size on media presence could account for the desire of managers to increase the size of their banks.[31]

X Results on Municipal Employment Growth

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 municipality-level analysis includes potential channels of the reforms that the firm-level analysis could not capture, such as local general equilibrium effects, changes in the municipal banking market after the reforms, and the effects on households. The municipal employment data are hand-digitized from the publication series *Statistisches Jahrbuch deutscher Gemeinden.* I digitize employment data for 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.[32]

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 X implies that the annual employment growth of municipalities with a treated bank branch was 1.3 percentage points lower between 1951 and 1960. The effect

[31]The treated banks were not able to use the increased media presence to grow faster or to become more efficient than other German banks, as the earlier results show. But there is anecdotal evidence that leaders of the treated banks influenced federal economic policy in favor of the financial sector as a whole after they consolidated (Der Spiegel 1971).

[32]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 1952 reform.

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. Column 3 uses a different regressor, the fraction of firms with a treated relationship bank in the municipality, calculated using the Hoppenstedt firm data. The point estimate implies that in a municipality where every firm had a treated relationship bank annual employment growth was 1.4 percentage points lower (significant at 10 percent).

The outcome in column 4 is the average annual employment growth rate from 1951 to 1956. In this period, only the treated banks outside NRW were affected by the 1952 reform. The coefficient implies a 1.2 percentage point decrease in the employment growth rate (significant at 10 percent). The coefficient on municipalities with treated bank branches in NRW is less than one-third of the magnitude and insignificant. The difference between the two coefficients is not statistically significant, however. Column 5 reports a positive and insignificant relationship between growth from 1947 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 with additional controls, 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 1947 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 workers displaced during and after the war. The coefficient is close to the baseline specification in column 1.[33]

The evidence suggests that significant employment losses occurred in municipalities that were more exposed to the treated banks. The small sample sizes in the specifications, ranging from 72 to 91 municipalities, suggest that 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.

XI Conclusion

Banking reforms in postwar Germany permitted certain state-level banks to reconsolidate into national banks. I find no evidence that the resulting increases in bank size benefited real economic growth. Firms and municipalities with higher exposure to the treated banks did not grow faster after the reforms. The treated banks did not increase lending, profits, or cost efficiency, relative to comparable untreated banks. Other findings indicate that there can be real costs to bigger banks. Opaque

[33] I can compare the estimated slowdown in municipal employment growth to the results from firm data. To do so, I calculate the effect on municipal employment growth that is implied by the firm-level estimates. For non-opaque firms, there was no effect on employment growth. For the average opaque firm, employment growth was 1.6 percentage points lower (Table IV, column 6). Roughly 67 percent of employees in the population worked in opaque firms. Thus, in a municipality where every firm had a treated relationship bank, the firm-level estimates imply that employment grew by 1.6*0.67 = 1.1 fewer percentage points. This amounts to 1.1/1.4 = 79 percent of the municipality-level effect in column 3 of Table X. The remaining 21 percent may be due to local general equilibrium effects or due to the effects of the consolidations on households.

(small, young, low-collateral) borrowers of the treated banks experienced lower employment growth after the reforms. This suggests that big banks are worse than their smaller counterparts at processing soft information. Treated banks established more relationships with risky firms after the reforms, but these new risky borrowers were not more productive and did not grow any faster. This finding is consistent with theories emphasizing moral hazard or internal agency problems in big banks.

Some leading models of banking imply that a system with one big monopoly bank is socially efficient (for example, Diamond 1984). In the same vein, opponents of strict size regulation sometimes argue that there is a universally positive relationship between bank size, bank efficiency, and growth in the real economy. The results of this paper throw into question such arguments against size regulation, as far as traditional banking activities (lending, deposit taking, payment services, and security underwriting) are concerned. Traditional activities continue to represent a key link between banks and the real economy, and shocks to local banking relationships still affect real outcomes today (Degryse and Ongena 2005; Bentolila et al. 2018; Nguyen forthcoming). One challenge for future research is to understand whether non-traditional banking technologies that have been developed since the 1950s affect the relationship between bank size and real growth (Petersen and Rajan 2002; Berger 2003).

References

Adler, Hans A., "The Post-War Reorganization of the German Banking System," *Quarterly Journal of Economics*, 1949, *63* (3), 322–341.

Adrian, Tobias and Markus K. Brunnermeier, "CoVaR," *American Economic Review*, 2016, *106* (7), 1705–1741.

Ahrens, Ralf, *Die Dresdner Bank 1945-1957*, Oldenbourg, 2007.

Amiti, Mary and David E. Weinstein, "Exports and Financial Shocks," *Quarterly Journal of Economics*, 2011, *126* (4), 1841–1877.

Beck, Thorsten, Ross Levine, and Alexey Levkov, "Big Bad Banks? The Winners and Losers from Bank Deregulation in the United States," *Journal of Finance*, 2010, *65* (5), 1637–1667.

Benmelech, Efraim, Carola Frydman, and Dimitris Papanikolaou, "Financial Frictions and Employment During the Great Depression," 2017. NBER Working Paper 23216.

Bentolila, Samuel, Marcel Jansen, Gabriel Jiménez, and Sonia Ruano, "When Credit Dries Up: Job Losses in the Great Recession," *Journal of the European Economic Association*, 2018, *16* (3), 650–695.

Berger, Allen N., "The Economic Effects of Technological Progress: Evidence from the Banking Industry," *Journal of Money, Credit, and Banking*, 2003, *35* (2), 141–176.

__ **and Gregory F. Udell**, "Small Business Credit Availability and Relationship Lending: The Importance of Bank Organisational Structure," *The Economic Journal*, 2002, *112* (477), F32–F53.

__ **and Loretta J. Mester**, "Inside the Black Box: What Explains Differences in the Efficiencies of Financial Institutions?," *Journal of Banking & Finance*, 1997, *21* (7), 895–947.

__ **, Anil K. Kashyap, and Joseph M. Scalise**, "The Transformation of the US Banking Industry: What a Long, Strange Trip It's Been," *Brookings Papers on Economic Activity*, 1995, (2), 55–218.

__ **, Anthony Saunders, Joseph M. Scalise, and Gregory F. Udell**, "The Effects of Bank Mergers and Acquisitions on Small Business Lending," *Journal of Financial Economics*, 1998, *50* (2), 187–229.

__ **, Leora F. Klapper, and Gregory F. Udell**, "The Ability of Banks to Lend to Informationally Opaque Small Businesses," *Journal of Banking & Finance*, 2001, *25* (12), 2127–2167.

__ , **Nathan H. Miller, Mitchell A. Petersen, Raghuram G. Rajan, and Jeremy C. Stein**, "Does Function Follow Organizational Form? Evidence from the Lending Practices of Large and Small Banks," *Journal of Financial Economics*, 2005, *76* (2), 237–269.

__ , **Rebecca S. Demsetz, and Philip E. Strahan**, "The Consolidation of the Financial Services Industry: Causes, Consequences, and Implications for the Future," *Journal of Banking & Finance*, 1999, *23* (2), 135–194.

Bertrand, Marianne, Antoinette Schoar, and David Thesmar, "Banking Deregulation and Industry Structure: Evidence from the French Banking Reforms of 1985," *Journal of Finance*, 2007, *62* (2), 597–628.

Biswas, Swarnava Sonny, Fabiana Gómez, and Wei Zhai, "Who Needs Big Banks? The Real Effects of Bank Size on Outcomes of Large US Borrowers," *Journal of Corporate Finance*, 2017, *46*, 170–185.

Black, Sandra E. and Philip E. Strahan, "Entrepreneurship and Bank Credit Availability," *Journal of Finance*, 2002, *57* (6), 2807–2833.

Boot, Arnoud W. A., "Relationship Banking: What Do We Know?," *Journal of Financial Intermediation*, 2000, *9* (1), 7–25.

Boyd, John H. and Edward C. Prescott, "Financial Intermediary-Coalitions," *Journal of Economic Theory*, 1986, *38* (2), 211–232.

Braun, Matias, "Financial Contractability and Asset Hardness," 2005.

Brickley, James A., James S. Linck, and Clifford W. Smith, "Boundaries of the Firm: Evidence from the Banking Industry," *Journal of Financial Economics*, 2003, *70* (3), 351–383.

Bundesministerium für Arbeit, "Jahreszahlen zur Arbeitsstatistik," 1951. Bonn.

Bursztyn, Leonardo and Davide Cantoni, "A Tear in the Iron Curtain: The Impact of Western Television on Consumption Behavior," *Review of Economics and Statistics*, 2016, *98* (1), 25–41.

Calomiris, Charles W., "The Costs of Rejecting Universal Banking: American Finance in the German Mirror, 1870-1914," in Naomi R. Lamoreaux and Daniel M. G. Raff, eds., *Coordination and Information: Historical Perspectives on the Organization of Enterprise*, NBER, 1995, pp. 257–322.

__ , "Gauging the Efficiency of Bank Consolidation During a Merger Wave," *Journal of Banking & Finance*, 1999, *23* (2), 615–621.

__ **and Jason Karceski**, "Is the Bank Merger Wave of the 1990s Efficient? Lessons from Nine Case Studies," in Steven N. Kaplan, ed., *Mergers and Productivity*, NBER, 2000, pp. 93–178.

Canales, Rodrigo and Ramana Nanda, "A Darker Side to Decentralized Banks: Market Power and Credit Rationing in SME Lending," *Journal of Financial Economics*, 2012, *105* (2), 353–366.

Cerasi, Vittoria and Sonja Daltung, "The Optimal Size of a Bank: Costs and Benefits of Diversification," *European Economic Review*, 2000, *44* (9), 1701–1726.

Cerqueiro, Geraldo, Hans Degryse, and Steven Ongena, "Rules Versus Discretion in Loan Rate Setting," *Journal of Financial Intermediation*, 2011, *20* (4), 503–529.

Cetorelli, Nicola and Philip E. Strahan, "Finance as a Barrier to Entry: Bank Competition and Industry Structure in Local US Markets," *Journal of Finance*, 2006, *61* (1), 437–461.

Chodorow-Reich, Gabriel, "The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008-9 Financial Crisis," *Quarterly Journal of Economics*, 2014, *129* (1), 1–59.

Cingano, Federico, Francesco Manaresi, and Enrico Sette, "Does Credit Crunch Investment Down? New Evidence on the Real Effects of the Bank-Lending Channel," *Review of Financial Studies*, 2016, *29* (10), 2737–2773.

Cole, Rebel A., Lawrence G. Goldberg, and Lawrence J. White, "Cookie Cutter vs. Character: The Micro Structure of Small Business Lending by Large and Small Banks," *Journal of Financial and Quantitative Analysis*, 2004, *39* (2), 227–251.

Cortés, Kristle Romero and Philip E. Strahan, "Tracing out Capital Flows: How Financially Integrated Banks Respond to Natural Disasters," *Journal of Financial Economics*, 2017, *125* (1), 182–199.

Davies, Richard and Belinda Tracey, "Too Big to Be Efficient? The Impact of Implicit Subsidies on Estimates of Scale Economies for Banks," *Journal of Money, Credit and Banking*, 2014, *46* (s1), 219–253.

Dávila, Eduardo and Ansgar Walther, "Does Size Matter? Bailouts with Large and Small Banks," 2017.

Davis, Steven J., John C. Haltiwanger, and Scott Schuh, *Job Creation and Destruction*, MIT Press, 1998.

Degryse, Hans and Steven Ongena, "Distance, Lending Relationships, and Competition," *Journal of Finance*, 2005, *60* (1), 231–266.

Demyanyk, Yuliya, Charlotte Ostergaard, and Bent E. Sørensen, "US Banking Deregulation, Small Businesses, and Interstate Insurance of Personal Income," *Journal of Finance*, 2007, *62* (6), 2763–2801.

Der Spiegel, "Zerschlagen bis zur Dorfkasse," 4 July 1951.

_ , "Die Omnipotenten," 18 January 1971.

Deutsche Bundesbank, *Deutsches Geld- und Bankwesen in Zahlen 1876-1975*, Fritz Knapp Verlag, 1976.

Diamond, Douglas W., "Financial Intermediation and Delegated Monitoring," *Review of Economic Studies*, 1984, *51* (3), 393–414.

Die Zeit, "Graue Zinsen vor dem Ehrengericht," 7 January 1954.

Doerr, Sebastian, Stefan Gissler, José-Luis Peydró, and Hans-Joachim Voth, "From Finance to Extremism: The Real Effects of Germany's 1931 Banking Crisis," 2018.

Dwenger, Nadja, Frank M. Fossen, and Martin Simmler, "From Financial to Real Economic Crisis: Evidence from Individual Firm-Bank Relationships in Germany," 2015. DIW Berlin Discussion Paper 1510.

Eichengreen, Barry and Albrecht Ritschl, "Understanding West German Economic Growth in the 1950s," *Cliometrica*, 2009, *3* (3), 191–219.

Elsas, Ralf, "Empirical Determinants of Relationship Lending," *Journal of Financial Intermediation*, 2005, *14* (1), 32–57.

_ **and Jan Pieter Krahnen**, "Is Relationship Lending Special? Evidence from Credit-file Data in Germany," *Journal of Banking & Finance*, 1998, *22* (10), 1283–1316.

Enikolopov, Ruben and Maria Petrova, "Media Capture: Empirical Evidence," in Simon P. Anderson, Joel Waldfogel, and David Strömberg, eds., *Handbook of Media Economics*, 2015, pp. 687–700.

Evanoff, Douglas D. and Evren Ors, "The Competitive Dynamics of Geographic Deregulation in Banking: Implications for Productive Efficiency," *Journal of Money, Credit and Banking*, 2008, *40* (5), 897–928.

Federal Reserve System, "Order Approving the Merger of Bank Holding Companies: People's United Financial, Inc, Bridgeport, Connecticut," 16 March 2017. FRB Order No. 2017-08.

Feng, Guohua and Apostolos Serletis, "Efficiency, Technical Change, and Returns to Scale in Large US Banks: Panel Data Evidence from an Output Distance Function Satisfying Theoretical Regularity," *Journal of Banking & Finance*, 2010, *34* (1), 127–138.

Focarelli, Dario and Fabio Panetta, "Are Mergers Beneficial to Consumers? Evidence from the Market for Bank Deposits," *American Economic Review*, 2003, *93* (4), 1152–1172.

_ **,** _ **, and Carmelo Salleo**, "Why Do Banks Merge?," *Journal of Money, Credit, and Banking*, 2002, *34* (4), 1047–1066.

Fohlin, Caroline, "Relationship Banking, Liquidity, and Investment in the German Industrialization," *Journal of Finance*, 1998, *53* (5), 1737–1758.

_ , "The Rise of Interlocking Directorates in Imperial Germany," *The Economic History Review*, 1999, *52* (2), 307–333.

Freixas, Xavier, "Optimal Bail Out Policy, Conditionality and Constructive Ambiguity," 1999.

French, Kenneth R., Martin N. Baily, John Y. Campbell, John H. Cochrane, Douglas W. Diamond, Darrell Duffie, Anil K. Kashyap, Frederic S. Mishkin, Raghuram G. Rajan, David S. Scharfstein, Robert J. Shiller, Hyung Song Shin, Matthew J. Slaughter, Jeremy C. Stein, and René M. Stulz, *The Squam Lake Report: Fixing the Financial System*, Princeton University Press, 2010.

Gerschenkron, Alexander, *Economic Backwardness in Historical Perspective*, Harvard University Press, 1962.

Gertler, Mark and Simon Gilchrist, "Monetary Policy, Business Cycles, and the Behavior of Small Manufacturing Firms," *Quarterly Journal of Economics*, 1994, *109* (2), 309–40.

Gilje, Erik P., Elena Loutskina, and Philip E. Strahan, "Exporting Liquidity: Branch Banking and Financial Integration," *Journal of Finance*, 2016, *71* (3), 1159–1184.

Goetz, Martin, Luc Laeven, and Ross Levine, "Does the Geographic Expansion of Bank Assets Reduce Risk?," *Journal of Financial Economics*, 2016, *120* (2), 346–362.

Goetz, Martin R., Luc Laeven, and Ross Levine, "Identifying the Valuation Effects and Agency Costs of Corporate Diversification: Evidence from the Geographic Diversification of US Banks," *Review of Financial Studies*, 2013, *26* (7), 1787–1823.

Greenwood, Robin, Samuel G. Hanson, Jeremy C. Stein, and Adi Sunderam, "The Financial Regulatory Reform Agenda in 2017," 2017.

Guinnane, Timothy W., "Delegated Monitors, Large and Small: Germany's Banking System, 1800–1914," *Journal of Economic Literature*, 2002, *40* (1), 73–124.

Haltiwanger, John, Ron S. Jarmin, and Javier Miranda, "Who Creates Jobs? Small Versus Large Versus Young," *Review of Economics and Statistics*, 2013, *95* (2), 347–361.

Hansen, Ernst Willi, *Reichswehr und Industrie*, Vol. 24 of *Wehrwissenschaftliche Forschungen*, Harald Boldt Verlag, 1978.

_ and _, "Lending Relationships in Germany: Empirical Evidence from Survey Data," *Journal of Banking & Finance*, 1998, *22* (10), 1317–1353.

Herlan, Fritz, "Zinsen und Gebühren," in Hans Pohl, ed., *Die Bank: Lehrbuch und Nachschlagewerk des Bank-und Sparkassenwesens, Zweiter Band*, Springer Fachmedien, 1952, pp. 654–672.

Hofmann, Walter, *Handbuch des Gesamten Kreditwesens*, Knapp, 1949.

Holtfrerich, Carl-Ludwig, "Die Deutsche Bank 1945-1957," in Lothar Gall, Gerald D. Feldman, Harold James, Carl-Ludwig Holtfrerich, and Hans E. Büschgen, eds., *Die Deutsche Bank: 1870-1995*, C.H. Beck, 1995.

Horstmann, Theo, *Die Alliierten und die deutschen Großbanken*, Bouvier, 1991.

Hoshi, Takeo and Anil Kashyap, *Corporate Financing and Governance in Japan: The Road to the Future*, MIT Press, 2004.

Houston, Joel, Christopher James, and David Marcus, "Capital Market Frictions and the Role of Internal Capital Markets in Banking," *Journal of Financial Economics*, 1997, *46* (2), 135–164.

Hubbard, R. Glenn and Darius Palia, "Executive Pay and Performance: Evidence from the US Banking Industry," *Journal of Financial Economics*, 1995, *39* (1), 105–130.

Huber, Kilian, "Disentangling the Effects of a Banking Crisis: Evidence from German Firms and Counties," *American Economic Review*, 2018, *103* (3), 868–898.

Hughes, Joseph P. and Loretta J. Mester, "Who Said Large Banks Don't Experience Scale Economies? Evidence from a Risk-return-driven Cost Function," *Journal of Financial Intermediation*, 2013, *22* (4), 559–585.

_ , Julapa Jagtiani, Loretta J. Mester, and Choon-Geol Moon, "Does Scale Matter in Community Bank Performance? Evidence Obtained by Applying Several New Measures of Performance," *Journal of Banking & Finance*, 2019, *106*, 471–499.

Hurst, Erik and Benjamin Wild Pugsley, "What Do Small Businesses Do?," *Brookings Papers on Economic Activity*, 2011, (2), 73–142.

Jagtiani, Julapa, Ian Kotliar, and Raman Quinn Maingi, "Community Bank Mergers and Their Impact on Small Business Lending," *Journal of Financial Stability*, 2016, *27*, 106–121.

Jayaratne, Jith and Philip E. Strahan, "The Finance-Growth Nexus: Evidence from Bank Branch Deregulation," *Quarterly Journal of Economics*, 1996, *111* (3), 639–670.

_ and _, "Entry Restrictions, Industry Evolution, and Dynamic Efficiency: Evidence from Commercial Banking," *Journal of Law and Economics*, 1998, *41* (1), 239–274.

Jeidels, Otto, *Das Verhältnis der deutschen Großbanken zur Industrie*, Schmollers Forschung, 1905.

Jensen, Michael C., "Agency Costs of Free Cash Flow, Corporate Finance, and Takeovers," *American Economic Review*, 1986, *76* (2), 323–329.

Johnson, Simon, "A Size Cap for the Largest U.S. Banks," 2016. Speech at the Federal Reserve Bank of Minneapolis.

Kashyap, Anil, Raghuram G. Rajan, and Jeremy C. Stein, "Rethinking Capital Regulation," *Proceedings - Economic Policy Symposium - Jackson Hole, Federal Reserve Bank of Kansas City*, 2008, pp. 431–471.

Kerr, William R. and Ramana Nanda, "Democratizing Entry: Banking Deregulations, Financing Constraints, and Entrepreneurship," *Journal of Financial Economics*, 2009, *94* (1), 124–149.

Kovner, Anna, James Vickery, and Lily Zhou, "Do Big Banks Have Lower Operating Costs?," 2014. FRBNY Economic Policy Review.

Krasa, Stefan and Anne P. Villamil, "Monitoring the Monitor: An Incentive Structure for a Financial Intermediary," *Journal of Economic Theory*, 1992, *57* (1), 197–221.

__ and __ , "A Theory of Optimal Bank Size," *Oxford Economic Papers*, 1992, *44* (4), 725–749.

Kreikamp, Hans-Dieter, "Die Entflechtung der I.G. Farbenindustrie A.G. und die Gründung der Nachfolgegesellschaften," *Vierteljahreshefte für Zeitgeschichte*, 1977, *25* (2).

Landier, Augustin, David Sraer, and David Thesmar, "Banking Integration and House Price Comovement," *Journal of Financial Economics*, 2017, *125* (1), 1–25.

Lanner, J., "Changes in the Structure of the German Banking System," *Economica*, 1951, *18* (70), 169–183.

Leland, Hayne E. and David H. Pyle, "Informational Asymmetries, Financial Structure, and Financial Intermediation," *The Journal of Finance*, 1977, *32* (2), 371–387.

Levine, Ross, Chen Lin, and Wensi Xie, "Geographic Diversification and Banks' Funding Costs," 2016. NBER Working Paper 22544.

Liberti, Jose M. and Atif R. Mian, "Estimating the Effect of Hierarchies on Information Use," *Review of Financial Studies*, 2009, *22* (10), 4057–4090.

Liberti, Jose Maria, Amit Seru, and Vikrant Vig, "Information, Credit, and Organization," 2016.

Manova, Kalina, "Credit Constraints, Heterogeneous Firms, and International Trade," *Review of Economic Studies*, 2012, *80* (2), 711–744.

McCord, Roisin and Edward S. Prescott, "The Financial Crisis, the Collapse of Bank Entry, and Changes in the Size Distribution of Banks," *Federal Reserve Bank of Richmond Economic Quarterly*, 2014, *100* (1), 23–50.

Minneapolis Fed, *The Minneapolis Plan to End Too Big to Fail* 2016.

Morgan, Donald P., Bertrand Rime, and Philip E. Strahan, "Bank Integration and State Business Cycles," *Quarterly Journal of Economics*, 2004, *119* (4), 1555–1584.

Nguyen, Hoai-Luu Q., "Are Credit Markets Still Local? Evidence from Bank Branch Closings," *American Economic Journal: Applied Economics*, forthcoming.

Pais, Amelia and Philip A. Stork, "Bank Size and Systemic Risk," *European Financial Management*, 2013, *19* (3), 429–451.

Peek, Joe and Eric S. Rosengren, "Bank Consolidation and Small Business Lending: It's Not Just Bank Size that Matters," *Journal of Banking & Finance*, 1998, *22* (6), 799–819.

Petersen, Mitchell A. and Raghuram G. Rajan, "Does Distance Still Matter? The Information Revolution in Small Business Lending," *Journal of Finance*, 2002, *57* (6), 2533–2570.

Pohl, Rüdiger, *Regionale Leistungsbilanzsalden in integrierten Wirtschaftsräumen*, Duncker & Humblot, 1971.

Qian, Jun QJ, Philip E. Strahan, and Zhishu Yang, "The Impact of Incentives and Communication Costs on Information Production and Use: Evidence from Bank Lending," *The Journal of Finance*, 2015, *70* (4), 1457–1493.

Rajan, Raghuram G., "Has Financial Development Made the World Riskier?," *Proceedings - Economic Policy Symposium - Jackson Hole, Federal Reserve Bank of Kansas City*, 2005, pp. 313–369.

__ and Luigi Zingales, "Financial Dependence and Growth," *American Economic Review*, 1998, *88* (3), 559–586.

Rhoades, Stephen A., "The Efficiency Effects of Bank Mergers: An Overview of Case Studies of Nine Mergers," *Journal of Banking & Finance*, 1998, *22* (3), 273–291.

Sapienza, Paola, "The Effects of Banking Mergers on Loan Contracts," *Journal of Finance*, 2002, *57* (1), 329–367.

Scharfstein, David S. and Jeremy C. Stein, "The Dark Side of Internal Capital Markets: Divisional Rent-seeking and Inefficient Investment," *Journal of Finance*, 2000, *55* (6), 2537–2564.

Scholtyseck, Joachim, "Die Wiedervereinigung der deutschen Großbanken und das Ende der Nachkriegszeit im Epochenjahr 1957," *Bankhistorisches Archiv*, 2006, *32* (2), 137–145.

Sharpe, Steven A., "Asymmetric Information, Bank Lending, and Implicit Contracts: A Stylized Model of Customer Relationships," *Journal of Finance*, 1990, *45* (4), 1069–87.

Skrastins, Janis and Vikrant Vig, "How Organizational Hierarchy Affects Information Production," Technical Report 2018.

Statistisches Bundesamt, "Ergebnisse der nichtlandwirtschaftlichen Arbeitsstättenzählung vom 13.9.1950, Teil II," *Statistische Berichte*, 14 June 1952, *IV/15/2*. Wiesbaden.

Stein, Jeremy C., "Internal Capital Markets and the Competition for Corporate Resources," *Journal of Finance*, 1997, *52* (1), 111–133.

_ , "Information Production and Capital Allocation: Decentralized Versus Hierarchical Firms," *Journal of Finance*, 2002, *57* (5), 1891–1921.

_ , "Agency, Information and Corporate Investment," in G.M. Constantinides, M. Harris, and R. M. Stulz, eds., *Handbook of the Economics of Finance*, Vol. 1 2003, chapter 2, pp. 111–165.

_ , "Regulating Large Financial Institutions," 2013. Speech at the IMF Conference "Rethinking Macro Policy II", 17 April.

Stern, Gary H. and Ron J. Feldman, *Too Big to Fail: The Hazards of Bank Bailouts*, Brookings Institution Press, 2004.

Stiroh, Kevin J. and Philip E. Strahan, "Competitive Dynamics of Deregulation: Evidence from US Banking," *Journal of Money, Credit, and Banking*, 2003, *35* (5), 801–828.

Strahan, Philip E. and James P. Weston, "Small Business Lending and the Changing Structure of the Banking Industry," *Journal of Banking & Finance*, 1998, *22* (6), 821–845.

von Alemann, Ulrich, "Modell Montana: Was Hält NRW Zusammen?," 2000. Universität Düsseldorf.

Wandel, Eckhard, *Die Entstehung der Bank deutscher Länder*, Oldenbourg, 1980.

Wheelock, David C. and Paul W. Wilson, "Do Large Banks Have Lower Costs? New Estimates of Returns to Scale for US Banks," *Journal of Money, Credit and Banking*, 2012, *44* (1), 171–199.

Williamson, Oliver E., "Hierarchical Control and Optimum Firm Size," *Journal of Political Economy*, 1967, *75* (2), 123–138.

Williamson, Stephen D., "Costly Monitoring, Financial Intermediation, and Equilibrium Credit Rationing," *Journal of Monetary Economics*, 1986, *18* (2), 159–179.

Wolf, Herbert, "Die Dreier-Lösung," *Bankhistorisches Archiv*, 1993, *19* (1), 26–42.

_ , "Nicht Fisch noch Fleisch," *Bankhistorisches Archiv*, 1994, *20* (1), 28–44.

_ , "Vom Großbankengesetz bis zur "Normalisierung" (1953-1958)," in Hans Pohl, ed., *Geschichte der deutschen Kreditwirtschaft seit 1945*, Fritz Knapp Verlag, 1998, pp. 111–148.

Young, Alwyn, "Improved, Nearly Exact, Statistical Inference with Robust and Clustered Covariance Matrices using Effective Degrees of Freedom Corrections," 2016.

Zingales, Luigi, "Towards a Political Theory of the Firm," *Journal of Economic Perspectives*, 2017, *31* (3), 113–130.

Tables

Table I: Effects on the growth of stock corporations

	(1)	(2)	(3)	(4)	(5)
Panel A: Bank debt growth 1951-60					
Rel. bank treated in 1952/57	-0.001	-0.005	0.006	0.000	0.022
	(0.016)	(0.017)	(0.018)	(0.020)	(0.025)
Observations	421	421	421	240	219
R^2	0.000	0.134	0.152	0.248	0.252
Panel B: $\Delta\frac{Bk\ debt}{Assets}$ 1951-60					
Rel. bank treated in 1952/57	0.001	0.001	0.002	0.004	0.005
	(0.171)	(0.193)	(0.188)	(0.003)	(0.003)
Observations	421	421	421	240	219
R^2	0.002	0.095	0.125	0.185	0.259
Panel C: Employment growth 1951-60					
Rel. bank treated in 1952/57	0.001	0.000	-0.001	-0.007	-0.004
	(0.004)	(0.004)	(0.005)	(0.010)	(0.007)
Observations	821	734	685	225	338
R^2	0.000	0.107	0.112	0.251	0.178
Panel D: Revenue per worker growth 1951-60					
Rel. bank treated in 1952/57	0.004	0.002	-0.000	0.007	-0.000
	(0.005)	(0.006)	(0.006)	(0.017)	(0.010)
Observations	345	299	293	86	160
R^2	0.002	0.195	0.303	0.516	0.372
Industry FE*Zone FE	No	Yes	Yes	Yes	Yes
ln age*Zone FE	No	No	Yes	Yes	Yes
ln assets*Zone FE	No	No	Yes	Yes	Yes
Firm Type	Stock	Stock	Stock	Stock	Stock
Sample	Full	Full	Full	High bank debt	High leverage

Notes: The table reports estimates of the effect of having a treated relationship bank on the growth of stock corporations. 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 change in $\frac{Bank\,debt}{Assets}$ is the difference in the ratio of bank debt over assets between 1951 and 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, the natural logarithm of firm age, and the natural logarithm of firm assets in 1951. All are 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 stock corporations.

Table II: Effects on the growth of non-stock firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Employment growth 1951-56							
Rel. bank treated in 1952	-0.001	-0.001	0.001	-0.001	0.003	0.002	-0.001	0.000
	(0.003)	(0.004)	(0.007)	(0.007)	(0.006)	(0.009)	(0.005)	(0.005)
Observations	1,521	1,472	353	342	473	464	687	664
R^2	0.000	0.063	0.000	0.110	0.000	0.168	0.000	0.064
Industry FE*Zone FE	No	Yes	No	No	No	Yes	No	Yes
ln age*Zone FE	No	Yes	No	No	No	Yes	No	Yes
Size bin FE*Zone FE	No	Yes	No	No	No	Yes	No	Yes
Industry FE	No	No	No	Yes	No	No	No	No
ln age	No	No	No	Yes	No	No	No	No
Size bin FE	No	No	No	Yes	No	No	No	No
Firm Type	Non-Stock		Non-Stock		Non-Stock		Non-Stock	
Sample	Full		Focused		Exporters		Strict states	

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 the firm had a bank treated in 1952 among its relationship banks in 1951. The control variables include fixed effects for four bins of firm employment in 1951 (1-49, 50-249, 250-999, 1000+ employees), since firm assets are unavailable in the data for non-stock firms. The other control variables are identical to Table I. Standard errors are robust. The sample in columns 1 and 2 includes all non-stock firms with available employment data. The "focused" sample in columns 3 and 4 includes only firms that fulfill all of the following criteria: had a relationship bank that was treated in either 1952 or 1957; in the state of NRW or in states bordering NRW; outside the Ruhr region; not in the coal and steel industry. The sample in columns 5 and 6 includes only firms that exported any of their products. The sample in columns 7 and 8 includes only firms located in the southern states of Baden, Bavaria, Hesse, Rhineland-Palatine, Württemberg-Baden, and Württemberg-Hohenzollern, where the breakup was enforced most strictly.

Table III: Effects on the growth of newly added relationship borrowers

	(1)	(2)	(3)	(4)
	Employment Growth			
Outcome	1951-60		1951-56	
Rel. bank treated	-0.002	-0.005		
in 1952/57	(0.007)	(0.008)		
Added a bank treated	-0.000	-0.005		
in 1952/57 as rel. bank	(0.011)	(0.011)		
Rel. bank treated			0.005	0.004
in 1952			(0.008)	(0.014)
Added a bank treated			0.005	0.002
in 1952 as rel. bank			(0.010)	(0.015)
Observations	370	308	320	308
R^2	0.000	0.228	0.002	0.231
Controls*zone FE	No	Yes	No	Yes
Firm type	Stock	Stock	Non-Stock	Non-Stock
Sample	Firm increased the number of its rel. banks after 1951			

Notes: "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. "Added a bank treated in 1952/57 as relationship bank" is a dummy for whether the firm did not have a treated relationship bank in 1951, but added a bank treated in 1952 or 1957 as its relationship bank between 1951 and 1960. The sample in columns 1 and 2 includes only stock corporations that increased their number of relationship banks between 1951 and 1960. The outcome and controls in columns 1 and 2 are identical to Table I.
"Relationship bank treated in 1952" is a dummy for whether the firm had a bank treated in 1952 among its relationship banks in 1951. "Added a bank treated in 1952 as relationship bank" is a dummy for whether the firm did not have a treated relationship bank in 1951, but added a bank treated in 1952 as its relationship bank between 1951 and 1956. The sample in columns 3 and 4 includes only non-stock firms that increased their number of relationship banks from 1951 to 1956. The outcome and controls in columns 3 and 4 are identical to Table II. Standard errors are robust.

Table IV: Effects on the growth of opaque firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\Delta\frac{Bk\ debt}{Assets}$	$\Delta\frac{Cap}{Assets}$	Asset growth	Empl growth	$\Delta\frac{Bk\ debt}{Assets}$	Employment growth	
Outcome	1951-60		1951-60		1951-60	1951-56	
Rel. bank treated in 1952/57	-0.014	0.006	-0.011	0.000			
	(0.007)	(0.004)	(0.012)	(0.017)			
0 < Fraction rel. banks treated in 1952/57 $\leq$ 0.5					-0.013		
					(0.007)		
0.5 < Fraction rel. banks treated in 1952/57 $\leq$ 1					-0.018		
					(0.007)		
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
Firm type	Stock	Stock	Stock	Stock	Stock	Non-Stock	Non-Stock
Sample	Opaque	Opaque	Opaque	Opaque	Opaque	Opaque	Focused & Opaque

Notes: The outcomes, regressors, and control variables are identical to Table I (for columns 1 to 5) and to Table II (for columns 6 and 7). 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 10 percent of industry asset tangibility (fixed tangible over total assets).

Table V: Summary statistics by banking group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Treated group	$\frac{Assets\ in\ 1952}{No\ of\ banks\ in\ banking\ group\ in\ given\ years}$			Cost ratios in 1952 (in %)		
Banking group		1947/48-52	1952-57	From 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-Bk.	No	1,268	No change	No change	2.92	58.19	2.22
Bay. Vereinsbank	No	700			3.04	69.68	2.31
Oldenburgische Landesbk.	No	82			4.43	74.43	3.72
Avg. of 9 untreated banks	No	330			3.17	64.23	2.23

Notes: The data are from the annual bank reports. Assets are in million Deutsche Mark. The average of 9 untreated banks in the bottom row includes commercial banks with a branch network in 1949. 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 VI: 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 Landes-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)							
$\Delta\frac{Non-int\ cost}{Assets}$	-0.27	-0.10	-0.68	-1.05	-1.53	-1.54	0.80
							(0.31)
$\Delta\frac{Non-int\ cost}{Revenue}$	-7.29	-19.92	-15.32	-25.19	-38.99	-10.23	1.53
							(6.79)
$\Delta\frac{Empl\ comp}{Assets}$	0.00	-0.16	-0.41	-0.76	-1.17	-1.62	0.45
							(0.28)

Notes: Panel A reports the symmetric growth rate from 1952 to 1960. Panel B reports the change in the ratio from 1952 to 1960 in percentage points (i.e. the change in the percent ratio). Columns 1 to 6 report growth rates for the given banks. Column 7 reports the average difference (in the growth rate) between the three treated banking groups and 9 untreated banks. Robust standard errors are in parentheses. 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 VII: New banking relationships with opaque firms

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	1970	1970	1970	1951	1951	1970
Opaque firm	-0.056		-0.054	-0.001	0.022	0.023
	(0.019)		(0.021)	(0.046)	(0.020)	(0.015)
0 < Employees < 20		-0.072				
		(0.030)				
20 $\leq$ Employees < 50		-0.086				
		(0.026)				
0 < Ind. Tangibility < 0.15		-0.030				
		(0.033)				
0.15 $\leq$ Ind. Tangibility < 0.2		-0.012				
		(0.053)				
Observations	720	720	720	317	1,647	2,285
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 divided by 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 10 percent of industry asset tangibility (fixed tangible over total assets). The control variables are explained in Table II. Standard errors are robust.

Table VIII: 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
$0.25 \leq \frac{Cap}{Assets} < 0.5$	0.034	0.039	0.037	0.013			
	(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	0.162	0.251			
	(0.031)	(0.071)	(0.077)	(0.104)			
Volatile employment firm					0.058		
					(0.027)		
Volatile revenue firm						0.085	
						(0.065)	
High productivity firm							-0.038
							(0.029)
Observations	159	156	580	401	265	75	295
R^2	0.028	0.203	0.057	0.112	0.109	0.257	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 divided by 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 10 percent of industry asset tangibility (fixed tangible over total assets). The control variables are explained in Table II. Standard errors are robust.

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

	(1)	(2)	(3)
	Phase 1	Phase 2	Phase 3
	Jun 30, 1947 - Mar 29, 1952	Mar 30, 1952 - Dec 24, 1956	Dec 25, 1956 - Sep 24, 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 word 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 August 29, 2017.

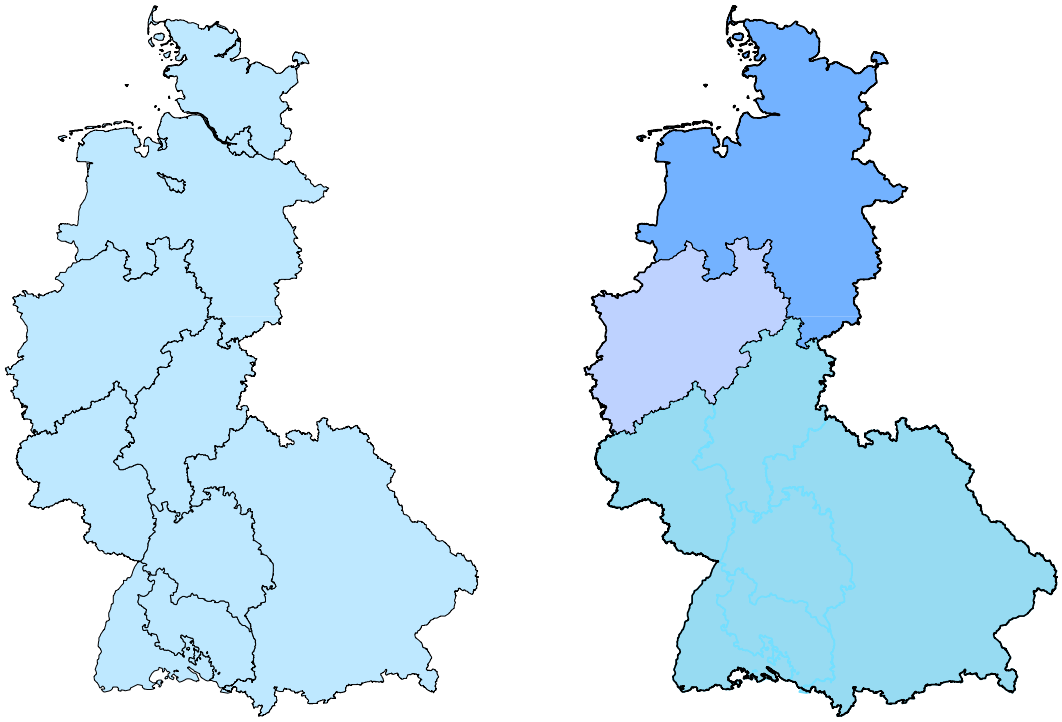
Table X: Effects on municipal employment growth

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment growth					
Outcome	1951-60	1951-60	1951-60	1951-56	1947-51	1951-60
Treated bank branch	-0.013	-0.013			0.019	-0.013
	(0.005)	(0.005)			(0.017)	(0.006)
Fraction of firms with a treated rel. bank			-0.014			
			(0.008)			
Treated bank branch not in NRW				-0.012		
				(0.007)		
Treated bank branch in NRW				-0.004		
				(0.009)		
Observations	79	79	74	91	83	72
R^2	0.340	0.350	0.303	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 estimates the effect of exposure to treated banks on municipal employment growth. The outcomes are annual average 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 fraction of firms with a treated relationship bank is calculated from the Hoppenstedt firm data for 1951. 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 1947 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.

Figures

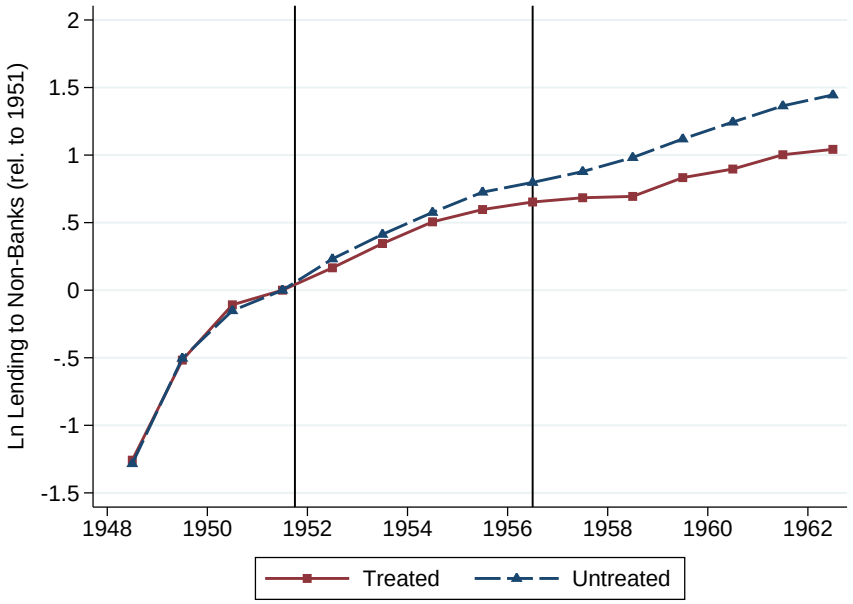
Figure I: Maps of the postwar banking zones



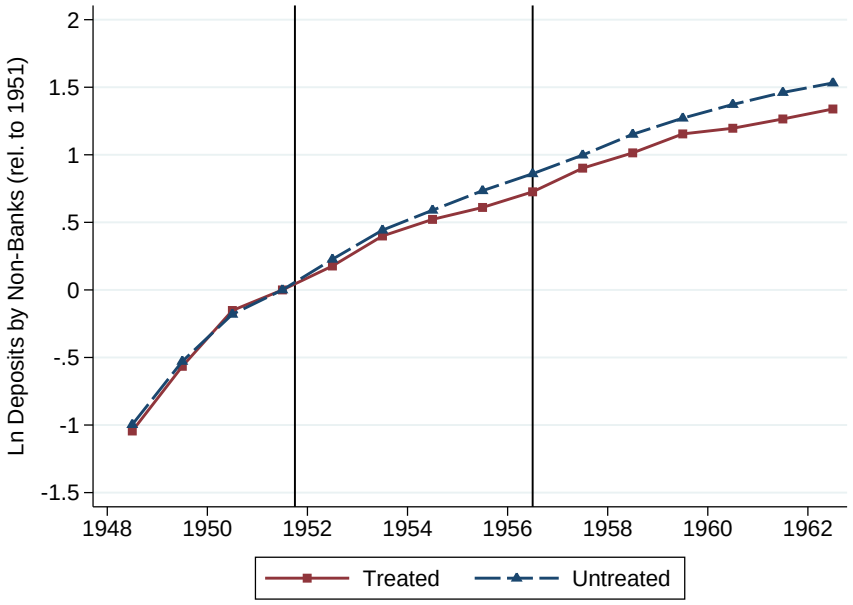
Notes: The figure shows the two phases of the breakup. The first reform in 1952 lifted the state-level restrictions and allowed banks to operate in three regional zones. The reform in 1957 removed all restrictions.

Figure II: Lending to non-banks and deposits from non-banks

A: Lending to non-banks



B: Deposits by non-banks



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 all untreated commercial banks. The 1952 reform lifted the state-level restrictions and introduced zonal restrictions. The 1957 reform removed all restrictions.

Online Appendix

Online Appendix A: Tables and Figures

Table A.I: 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
$\Delta\frac{100 \cdot Bank\,debt}{Assets}$ 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 digitized by hand from Hoppenstedt volumes, as described in Section V. The variables in levels are for the year 1951. 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. $\Delta\frac{100 \cdot Bank\,debt}{Assets}$ is the change in the percent ratio of bank debt over assets from 1951 to 1960, divided by 9, the number of years between 1951 and 1960. "Relationship bank treated in 1952/57" is a dummy for whether a bank treated in 1952 or 1957 was among the firm's relationship banks in 1951. "Relationship bank treated in 1952" is a dummy for whether a bank treated in 1952 was among the firm's relationship banks in 1951. Exporter is a dummy for whether the firm exported any of its products.

Table A.II: Firms with a treated relationship bank and firm observables in 1951

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Rel. bank treated in 1952/57				Rel. bank 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
Firm type	Stock	Stock	Non-stock	Non-stock	Stock	Non-stock
Sample	Full	Full	Full	Full	Focused	Focused

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

Table A.III: Testing for pre-trends in firm growth 1949-51

	(1)	(2)	(3)	(4)	(5)
Outcome	Employment Growth 1949-51				
Rel. bank treated	0.009	0.008	0.005	0.004	0.004
in 1952/57	(0.010)	(0.010)	(0.011)	(0.023)	(0.023)
Rel. bank treated				0.001	-0.001
in 1952				(0.024)	(0.024)
Rel. bank treated					0.001
in 1952/57 * Stock Corporation FE					(0.046)
Rel. bank treated					0.028
in 1952 * Stock Corporation FE					(0.039)
Observations	1,211	1,159	1,147	1,147	1,147
R^2	0.001	0.081	0.146	0.146	0.147
Industry FE*Zone FE	No	Yes	Yes	Yes	Yes
ln age*Zone FE	No	No	Yes	Yes	Yes
Size bin FE*Zone FE	No	No	Yes	Yes	Yes
Stock Corporation FE	No	No	Yes	Yes	Yes

Notes: The outcome variable is the average annual symmetric growth rate of employment, i.e. the symmetric growth rate from 1949 to 1951 divided by 2, the number of years between 1949 and 1951. Stock corporation FE is a dummy for stock corporations. The remaining regressors, control variables, and standard errors are explained in Table II. The sample contains all stock corporations and non-stock firms with available employment data in 1949 and 1951.

Table A.IV: Robustness tests for the effect on firm growth

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Employment Growth 1951-56					
Rel. bank treated	-0.002				-0.001	-0.001
in 1952 * Cap. acc. deficit	(0.005)				(0.004)	(0.004)
Rel. bank treated	-0.000					
in 1952 * Cap. acc. surplus	(0.005)					
Rel. bank treated		-0.008				
in 1952		(0.006)				
0 < Fraction rel. banks			-0.003			
treated in 1952 $\leq$ 0.5			(0.005)			
0.5 < Fraction rel. banks			0.002			
treated in 1952 $\leq$ 1			(0.007)			
Commerzbank rel.				-0.001		
bank treated in 1952				(0.006)		
Deutsche Bank rel.				-0.004		
bank treated in 1952				(0.004)		
Dresdner Bank rel.				0.002		
bank treated in 1952				(0.005)		
Observations	1,472	889	1,472	1,472	1,472	1,472
R^2	0.063	0.062	0.063	0.063	0.063	0.063
Controls*zone FE	Yes	Yes	Yes	Yes	Yes	Yes
Robustness test	Capital account deficit states	No war-related firms	Intensive margin banks	Three banking groups	Cluster at bank level	Cluster correction from Young (2016)

Notes: Column 1 tests for heterogeneity between firms in German federal states that regularly ran a capital account deficit (indicated by "Cap. acc. deficit", i.e. states that relied on capital from outside, Pohl 1971, page 40) and firms in states that regularly ran a capital account surplus (indicated by "Cap. acc. surplus").
Column 2 excludes from the sample firms that the Reichswehr identified as important for armament production (Hansen 1978) and firms in industries that produced war-related products (mining, clothes & textiles, chemicals & pharmaceuticals, metals manufacturing, electric & electronics, production of machinery).
Column 3 tests for heterogeneity by firms' intensive margin dependence on treated banks. The two regressors are dummies based on the "fraction relationship banks treated", which is the firm's number of treated relationship banks divided by the firm's total number of relationship banks in 1951.
Column 4 tests for heterogeneity by the three treated banking groups.
Standard errors in columns 1 to 4 are robust. Column 5 clusters standard errors at the level of the state-level treated banks (i.e. 31 categories, one for each of the 30 treated state-level banks and one for firms with no treated relationship bank). Column 6 uses the effective degrees of freedom correction for clustered standard errors suggested by Young (2016). The standard errors in columns 5 and 6 are identical to the baseline robust standard error.

Table A.V: 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
Firm Type	Non-Stock	Non-Stock	Non-Stock	Non-Stock
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 10 percent of industry asset tangibility (fixed tangible over total assets). The small sample sizes in columns 3 and 4 necessitate the use of a reduced set of controls. The controls in columns 3 and 4 are include a fixed effect for manufacturing firms, fixed effects for four bins of firm 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 II. 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.

Table A.VI: 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
Firm Type	Non-Stock	Non-Stock	Non-Stock	Non-Stock
Sample	Not opaque		Opaque	

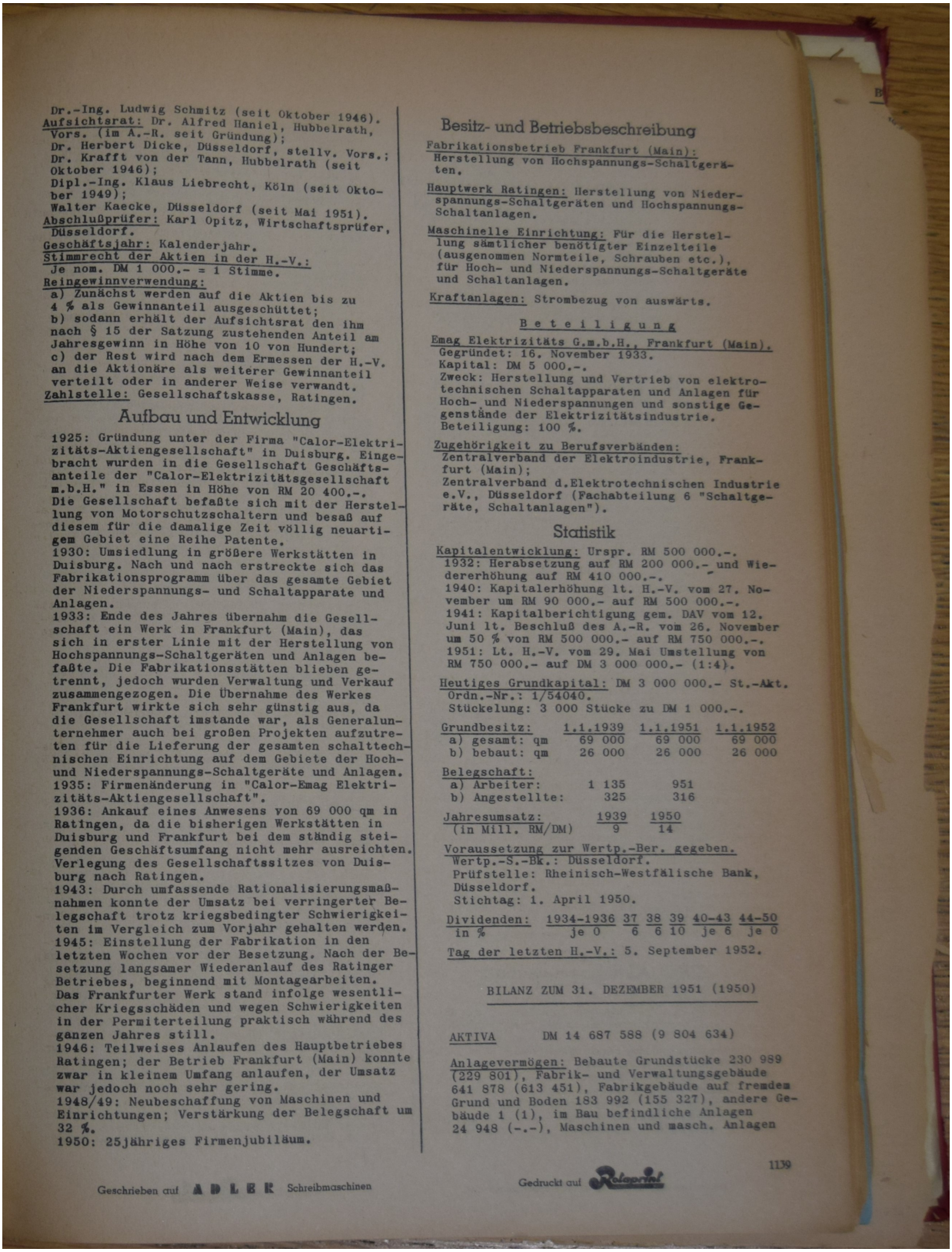
Notes: The outcome variables, regressors, control variables, and standard errors are identical to Table II. A firm is opaque if it has fewer than 50 employees in 1951, is younger than 10 years old in 1952, or is in the bottom 10 percent of industry asset tangibility (fixed tangible over total assets).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

Table A.VII: Effects on employment growth 1951-56, by firm size

Number of Employees	Coefficient	Std. Err.	Observations
0 - 9	-0.034	(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	-0.014	(0.030)	24
60 - 499	0.000	(0.004)	1,064
$\geq$ 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 additional control variables. Standard errors are robust. The samples include only non-stock firms.

Figure A.I: 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.

B e t e i l i g u n g

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

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

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

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

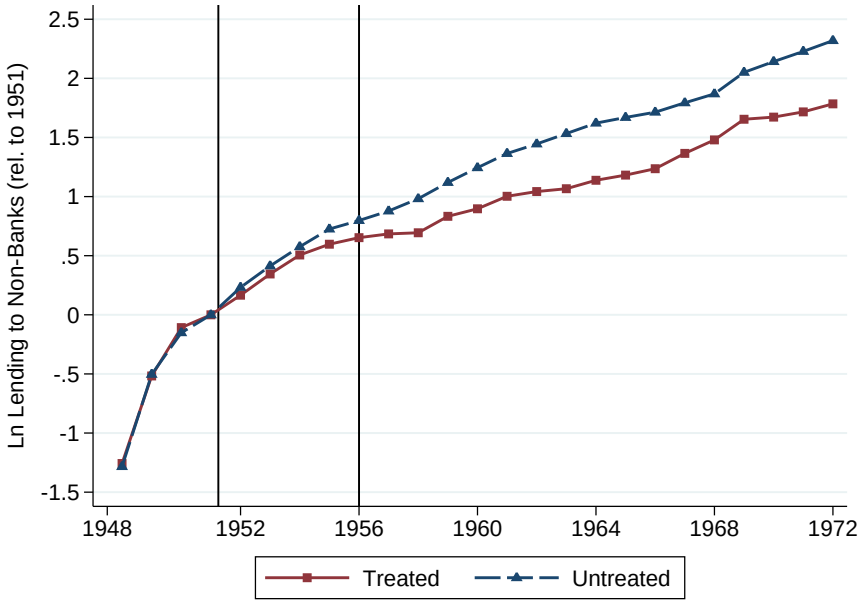
BILANZ ZUM 31. DEZEMBER 1951 (1950)

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

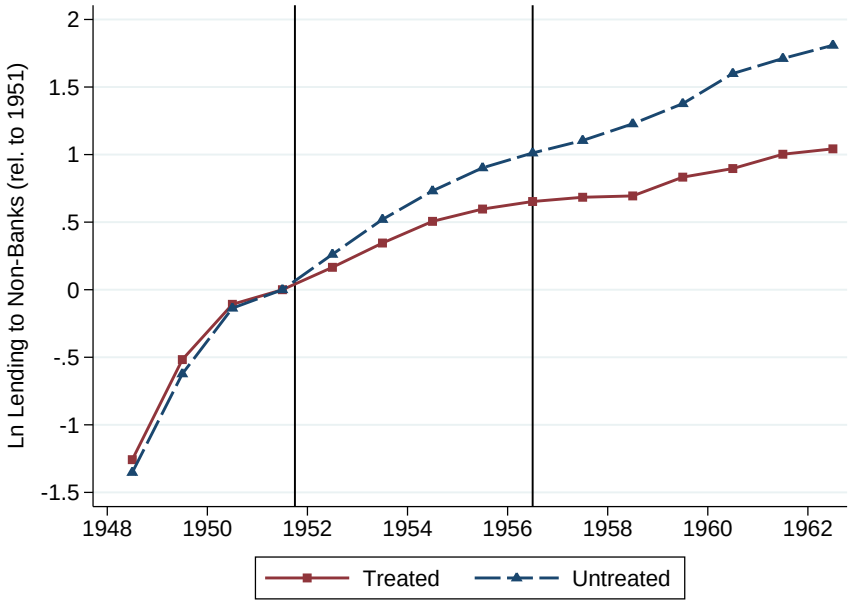
1139

Figure A.II: Lending to non-banks (data until 1972)



Notes: The figure extends the data in Figure II by 10 years. The treated group includes banks affected by the breakup and subsequent reforms. The untreated group includes the untreated commercial banks.uses the other commercial banks as untreated group. The data are for the December of the given year and provided by the Deutsche Bundesbank.

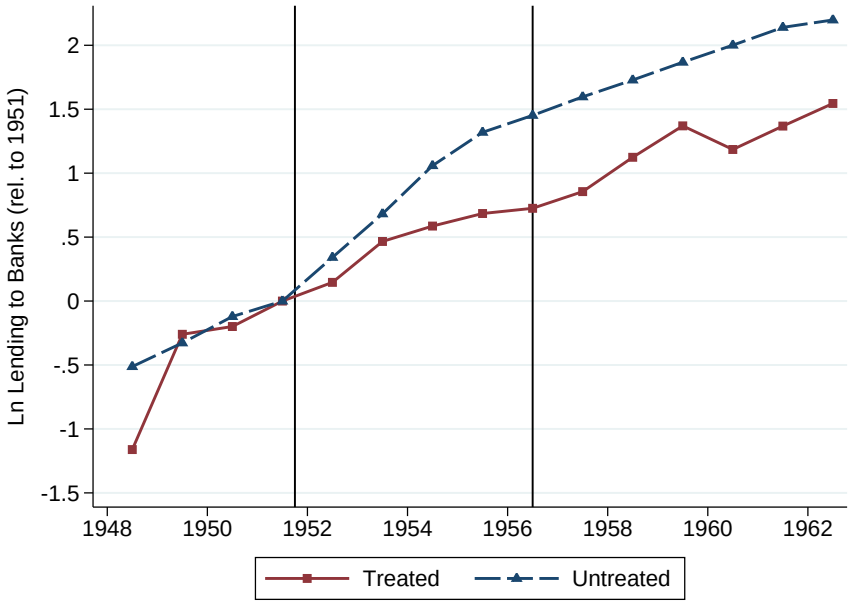
Figure A.III: Lending to non-banks (treated banks compared to all other banks)



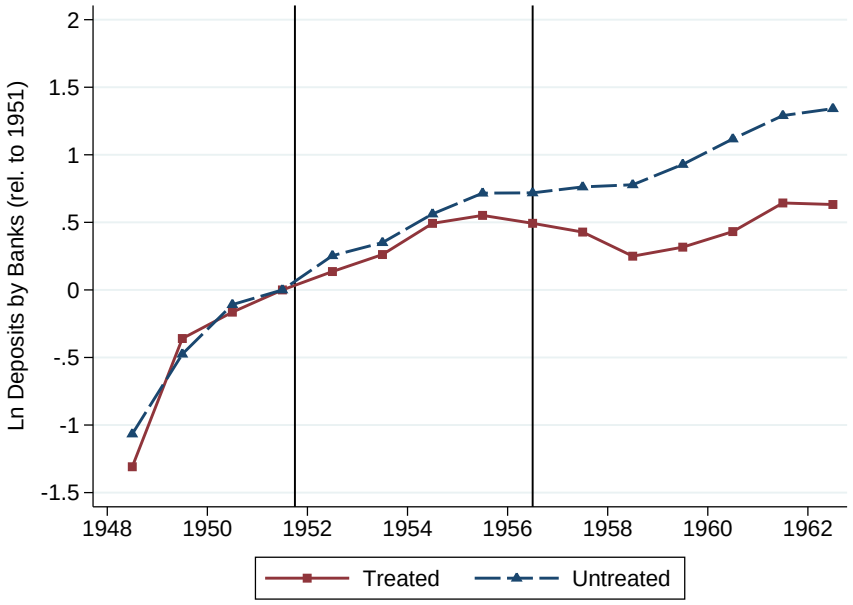
Notes: The figure differs from Figure II by including all other German banks in the untreated group. Figure II uses the other commercial banks as untreated group. The data are for the December of the given year and provided by the Deutsche Bundesbank.

Figure A.IV: Lending and deposits in interbank markets

A: Lending to banks



B: Deposits from banks



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 all untreated commercial banks. The first reform in 1952 lifted the state-level restrictions and introduced zonal restrictions. The reform in 1957 removed all restrictions.

Online Appendix B: Theoretical Model

I present a simple model of a firm borrowing from its relationship bank. The model illustrates how size-induced changes to bank efficiency can affect firms. The model also highlights the empirical challenge in identifying the causal effects of bank size.

The model makes two key assumptions. The motivation for the first assumption is that German firms rely on a few relationship banks for financial services. This implies that banks hold a "bilateral monopoly" over each relationship borrower (Boot 2000). This system can be explained by asymmetric information in credit markets that generates frictions for firms switching lenders (Sharpe 1990). In the model, I make the simplifying assumption that a firm can only borrow from one relationship bank.

The second assumption is that a bank's cost function depends on the number of its borrowers. The theory documented in the previous section suggests that an increase in the size of a bank can affect bank costs through multiple channel. The net effect of an increase in size on costs is theoretically ambiguous. An appropriate measure of size in these models is the number of borrowers served by a bank.[34] Henceforth, I refer to increases in bank size and increases in the number of borrowers interchangeably.

Firms Firm ib maximizes profits π_{ib}:

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

Capital K_{ib} is the sole input, which the firm borrows at an 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 or demand for the firm's products. The returns-to-scale production parameter is α, where $0 < \alpha < 1$. The firm's optimal demand for capital is given by:

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

Banks Bank b lends to a total of n_b relationship borrowers. The bank takes the total number of its relationship borrowers as given. (I discuss reasons for why this number may change when discussing equilibrium below.) It earns interest income, which is the interest rate charged to each relationship borrower multiplied by the amount of capital lent to that firm, summed over all firms. The bank also takes as given the capital demand function of each relationship borrower, as reported in equation 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 borrowers n_b. The marginal cost is decreasing in bank efficiency β_b. As discussed in the previous section, theory is ambiguous about the effect of the

[34] For example, by adding new borrowers with imperfectly correlated default risk, the bank becomes more diversified. This is not true when the bank simply expands lending to a single borrower.

number of relationship borrowers n_b on marginal cost. The bank maximizes profits π_b:

$$\pi_b = \sum_{i=1}^{n_b} [r_{ib} K_{ib} - c(n_b, \beta_b) K_{ib}], \tag{2}$$

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 1 and 2 and taking the first-order condition gives the optimal amount of capital lent by bank b to firm ib, K_{ib}. This amount increases with the exogenous capital demand shock A_{ib} and decreases with the marginal cost of lending $c(n_b, \beta_b)$:

$$ln(k_{ib}) = \frac{1}{1-\alpha}[ln(A_{ib}) - ln(c(n_b^{'}, \beta_b^{'}) + ln(\alpha^2)].$$

For concreteness, I assume a simple parametric specification for the marginal cost for each unit of lent capital:

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

where ϕ is either positive or negative (since the effect of size on marginal cost is ambiguous) and κ is strictly positive (since the effect of efficiency on cost is strictly positive). Under this assumption, the change in capital lent to firm ib from period t to period t' is given by equation 3. The operator $\Delta^{t,t'}[.]$ indicates the growth of the variable in square brackets from t to t':

$$\Delta^{t,t'}[ln(K_{ib})] = \frac{1}{1-\alpha} \times \Delta^{t,t'}[ln(A_{ib})] + \frac{\phi}{1-\alpha} \times \Delta^{t,t'}[n_b] + \frac{\kappa}{1-\alpha} \times \Delta^{t,t'}[\beta_b]. \tag{3}$$

Changes in firm capital demand A_{ib}, the number of the bank's relationship borrowers 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.[35]

Empirical Challenge The empirical challenge in estimating the causal effect of bank size is that changes in firm capital demand A_{ib} and bank efficiency β_b are typically unobservable in the data. This means that the regression specification one can actually estimate is:

$$\Delta^{t,t'}[ln(K_{ib})] = \frac{\phi}{1-\alpha} \times \Delta^{t,t'}[n_b] + \upsilon_{ib}, \tag{4}$$

[35]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.

Firm capital demand and bank efficiency enter the unobservable error term υ_{ib} :

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

A regression based on equation 4 estimates the true causal coefficient $\frac{\phi}{1-\alpha}$ if changes in firm capital demand and bank efficiency are uncorrelated with changes in bank size. However, banks do not become big randomly. For example, banks may strategically consolidate with other banks because they expect increases in the future efficiency of the other banks that are unrelated to size. Alternatively, a random shock to regional productivity can simultaneously increase the growth of incumbent bank borrowers and raise the size of banks operating in that region via higher firm entry. In both these examples, the observed correlation between bank size and firm growth would be positive, even if the true causal coefficient $\frac{\phi}{1-\alpha}$ is zero. Therefore, correlations between bank size and firm growth are not informative about how changes in bank size causally affect firm growth. A suitable empirical approach needs to identify a change in bank size that did not simultaneously affect firm capital demand, bank efficiency, and other unobservable components of firm and bank performance.

I estimate the effect of the reforms by adapting equation 4. I replace the regressor with 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 borrowers, i.e., the term $\Delta^{t,t'}[n_b]$ from equation 4.[36] The outcome is still the growth of firm ib from period t to period t':

$$\Delta^{t,t'}[ln(K_{ib})] = \theta \cdot (relationship\ bank\ treated\ between\ t\ and\ t')_b + \eta \cdot X_{ib} + \varepsilon_{ib}. \tag{5}$$

[36] In robustness checks, I also use regressors based on the fraction of the firm's relationship banks that was treated.