Are Bigger Banks Better?

Firm-Level Evidence from Germany

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Abstract

How changes in bank size affect the real economy is an important question in the design of financial regulation. This paper studies a natural experiment from postwar West Germany. Reforms by the Allied occupiers led to exogenous increases in the size of a number of banks. I estimate the effect of increased bank size on the growth of firms, using newly digitized data on firms and their banks. The results suggest that firms did not benefit when their banks became larger. The findings are inconsistent with theories that argue the real economy benefits from increases in bank size. There is evidence that big banks are worse at processing soft information and take more risks. Big banks receive more mentions in the media, which could be an incentive for banks to become big. (JEL E24, E44, G21, G28)

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

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

Economic theory suggests that big banks may be more efficient, because they are more diversified (Diamond 1984; Boyd and Prescott 1986; Williamson 1986), can use internal capital markets (Stein 1997; Scharfstein and Stein 2000), and rely on a large capital base to fund loans and spread fixed costs. On the other hand, large organizations may be complex to manage (Williamson 1967; Krasa and Villamil 1992a,b; Cerasi and Daltung 2000) and worse at

processing soft information, which may hurt small and opaque borrowers (Stein 2002; Berger and Udell 2002; Brickley et al. 2003). They may also take more risk, due to implicit "too-big-to-fail" subsidies by governments (Freixas 1999; Dávila and Walther 2017) and more severe agency problems (Rajan 2005). The net impact of increases in bank size on the real economy is an empirical question.[1]

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

Two features of the postwar German banking system combined provide a natural experiment that overcomes the empirical challenge. The first feature is the reliance of German firms on relationship banking. Due to asymmetric information, firm-bank relationships were sticky, so that shocks to specific banks affected the relationship customers of the shocked banks more strongly. The second feature is the banking policy of the Allied occupiers in postwar Germany. The Allies wanted to punish three national banks (Commerzbank, Deutsche Bank, and Dresdner Bank) for their cooperation with the Nazis and to break their political power. In 1947 and 1948, the Allies broke up the treated banks into 30 independent state-level organizations, prohibiting the new banks from branching outside state borders. A first reform in 1952 permitted the state-level banks to consolidate with other state-level banks within three banking zones. Instead of 30 state-level banks, there were now 9 treated institutions, one for each former na-

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

tional bank in each banking zone. A second reform in 1957, after Germany became a sovereign nation, lifted the restrictions entirely and led to the reconsolidation of the treated banks into three national banks.[2]

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

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

The main analysis examines whether the increases in bank size, induced by the Allied banking reforms, affected the growth of firms. The firm-level identification strategy compares the growth of firms with a treated relationship bank to firms that borrowed from other, untreated banks. The implementation of the identification strategy requires information on the relationship banks and the growth of firms in postwar Germany. Historic volumes by the commercial

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

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

information provider Hoppenstedt provide such information. Due to the poor print quality of the paper volumes, the data needed to be hand-digitized. The resulting new dataset includes the names of the relationship banks of around 5,900 firms, the growth of balance sheet variables for around 400 firms, and employment growth for around 2,300 firms.

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

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

An additional bank-level analysis examines the number of times the treated banks and their

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

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

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

based on a small sample of around 80 municipalities, so caution is warranted in interpreting these results. Nonetheless, the results support the conclusion that there is no evidence of a beneficial effect of the reforms on employment growth.

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

This paper proceeds in the following section by describing institutional details about relationship banking and the postwar banking reforms. Section II reviews the theoretical channels of bank size, presents a simple model of how bank size can affect firm growth, and explains the identification strategy. Section III describes the data. The main results on the growth of firms are in Section IV. Section V presents the results based on bank data, Section VI analyses new banking relationships, and Section VII studies the effect on municipal employment growth. Section VIII concludes.

Related Literature A number of recent cross-sectional papers argue that big banks face increasing returns or are more efficient (Feng and Serletis 2010; Wheelock and Wilson 2012; Hughes and Mester 2013; Davies and Tracey 2014; Kovner et al. 2014; Biswas et al. 2017). In general, however, the cross-sectional evidence is mixed (Berger and Mester 1997; Berger et al. 1999). The possibility of reverse causality, that is banks becoming bigger because they first experienced improvements in their efficiency, makes a causal interpretation of the cross-sectional data difficult. The evidence based on banking consolidations (mergers and acquisitions) is similarly ambiguous (Rhoades 1998; Berger 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 this makes it difficult to find appropriate control groups and causally interpret bank performance after consolidations.

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

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

The findings on opaque firms contribute to an existing literature on how big banks interact with small firms. Berger et al. (1995) show that, in the cross-section, big banks lend proportionally less to small firms. The evidence from consolidations is mixed, which may be explained by the non-randomness of consolidations (Berger et al. 1998; Peek and Rosengren 1998; Strahan and Weston 1998; Berger et al. 2001; Sapienza 2002; Jagtiani et al. 2016). Cole et al. (2004) analyze a firm survey and report that big banks are less likely to use soft information. Berger et al. (2005) find that firms located in markets with larger banks rely more on trade credit, which indicates credit constraints. My identification strategy uses exogenous variation in the size of the same bank serving the same firm. This strategy overcomes concerns that the non-randomness of consolidations or underlying, cross-sectional differences across regions, firms,

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

and banks bias the estimated effects. An additional innovation relative to the small-firm literature is that I focus more broadly on opaque firms, rather than just small firms, and estimate real effects on employment, rather than just lending.

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

Other papers investigate specific channels, through which bank size can affect efficiency. Houston 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). Unlike these papers, I do not focus on the effects of internal capital markets or diversification. Instead, I estimate the causal effects of bank size, which may be partially driven by these channels.

I Institutional Details

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

I.A Relationship Banking

Economic history (Jeidels 1905; Calomiris 1995), case studies (summarized in Guinnane 2002), and recent evidence (Harhoff and Körting 1998; Elsas and Krahnen 1998; Elsas 2005) suggest that relationship banking has played an important role in German corporate finance from the start of the 19th century until today. Firms of all sizes formed close and durable business ties to their banks, which reduced asymmetric information and improved banks' monitoring capabilities (Sharpe 1990; Boot 2000).[5] The literature provides empirical evidence from a number of countries and episodes that idiosyncratic shocks to relationship banks have real effects on firms, for instance Benmelech et al. (2017) for the US Great Depression, Amiti and Weinstein (2011) for Japan in the 1990s and 2000s, Chodorow-Reich (2014) for the 2008-09 US financial crisis, Bentolila et al. (forthcoming) for the Great Recession in Spain, and Huber (2018) for Germany.

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

I.B The Allied Banking Reforms

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

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

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

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

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

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

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

Phase 2: Three Banking Zones 1952-57 In the early 1950s, the Allied attitude towards West Germany changed. Instead of weakening the German economy, the Allies now wanted it to serve as buffer against the Communist threat from Eastern Europe. There was disagreement among the Allies, German politicians, and bankers on how to optimally reorganize the banking system. The Americans, leading Southern state politicians, and most central bankers believed that the state-level banks supplied financial services efficiently. On the other hand, the British, the federal German government, and most of the treated bank directors argued that bigger banks would be more efficient (Horstmann 1991).

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

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

Phase 3: National Banks from 1957 Five years later, international political developments affected the structure of the treated banks once more. The emergence of the Cold War had made Germany a key ally of the West. The Allies granted the German government full sovereignty

in the Paris Agreement of 1955. Since the federal government had always believed in the efficiency of large banks, it lifted all restrictions on the treated banks from January 1957 (Scholtyseck 2006). The treated banks subsequently consolidated. By 1958, there were once again three large banks with a national branch network, operating under their old, pre-war names. All directors of the former, zonal banks joined the boards of the new national banks, while staff and branches remained unchanged (Horstmann 1991; Holtfrerich 1995).

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

II Theory and Identification

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

II.A Advantages of Big Banks

The first theoretical benefit of big banks is that they are more diversified and therefore have lower funding costs. Under the assumption that there are fixed costs to monitoring borrowers, models by Diamond (1984), Boyd and Prescott (1986), and Williamson (1986) show that banks with a larger number of customers attract cheaper deposits, because they can diversify more

cheaply. A monopoly bank is socially efficient in these models. The postwar banking reforms sharply increased the number of customers served by one treated institution. If indeed there are consequential fixed costs to monitoring, the treated banks should have benefited from cheaper funding after the reforms. Holtfrerich (1995) quotes a number of treated branch managers that argued during the breakup period that the reforms would lower funding costs.

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

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

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

loans should have fallen after the reforms.

II.B Disadvantages of Big Banks

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

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

A third theoretical disadvantage is that big banks may take more risks, because of moral hazard or agency problems. The cause of moral hazard is that big banks carry higher systemic risk (Pais and Stork 2013; Adrian and Brunnermeier 2016). As a result, Freixas (1999) argues, governments are more likely to bail out big banks when they become insolvent. Dávila and Walther (2017) show theoretically that big banks internalize the increased probability of a bailout and take more risk. The postwar reforms increased the probability that the treated banks

would experience a bail-out. One reason is their increased size and hence systemic importance. A second reason is that the German government, which became sovereign before the second reform, believed in the economic necessity of big banks. In contrast, the Allied governments, that had been in charge before the second reform, had actively tried to break the large banks' influence.

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

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

II.C Model

The theoretical considerations documented above suggest an increase in the size of a bank can affect bank efficiency. The appropriate measure of size in these models is the number of

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

customers served by a bank.[9] Henceforth, I refer to increases in bank size and increases in the number of customers interchangeably. The following model illustrates how shocks to the number of customers can affect the loan supply of firms, if the cost function of the bank depends on the number of its customers. The key assumption of the model is that a firm can only borrow from its relationship bank, due to asymmetric information in credit markets (Sharpe 1990). This implies that banks hold a "bilateral monopoly" (Boot 2000) over each relationship customer.

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

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

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

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

Banks Bank b lends to a total of n_b relationship customers. For now, assume the bank takes as given the total number of relationship customers. I discuss reasons for why this number may change when discussing equilibrium below. Banks earn interest income, which is the interest rate charged to each relationship customer multiplied by the amount of capital lent to that firm, summed over all firms. The bank faces the capital demand function of each relationship customer, as reported in equation 1.

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

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

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 to firm *ib*, K_{ib}. This amount increases with the exogenous capital demand shock A_{ib} and decreases with the marginal cost of lending $c(n_b, \beta_b)$:

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

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

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

where ϕ is either positive or negative and κ is strictly positive. Under this specification, the change in capital lent to firm *ib* from period t to period t' is given by equation 3, where the operator $\Delta^{t,t'}[.]$ indicates the growth of the variable in square brackets from t to t':

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

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

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

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

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

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

A regression based on equation 4 estimates the true causal coefficient $\frac{\phi}{1-\alpha}$ if changes in firm capital demand and bank efficiency are generally uncorrelated with changes in bank size. However, banks do not become big randomly. For example, a random shock to regional productivity will lead to firm entry, raising the size of banks operating in that region, and simultaneously increase the capital demand of incumbent bank customers. In addition, banks strategically consolidate with other banks, for example because they expect increases in the efficiency of the other banks that are unrelated to size. These considerations imply that changes in bank size are likely to be correlated with changes in firm capital demand and bank efficiency. Hence, the observed, unconditional correlation between bank size and firm growth can be positive, even if the true causal coefficient $\frac{\phi}{1-\alpha}$ is zero.

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

II.D Identification Strategy

The postwar reforms of 1952 and 1957 provide suitable natural experiments that allow estimating the causal effects of bank size. Equation 4 motivates the regression specification, which is given by equation 5. The outcome is the growth in the total amount of capital borrowed by firm

ib from period t to period t':

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

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

The key regressor is an indicator for whether one of the firm's relationship banks increased in size due to a postwar reform between the years t and t'. This regressor serves as proxy for an increase in the number of the bank's customers, i.e. the term $\Delta^{t,t'}[n_b]$ from equation 4.[10]

Equation 5 can be thought of as reduced-form specification. The coefficient θ captures all the channels, through which a bank size shock could affect firms.[11] θ estimates the causal effect of a change in bank size on firm growth if a parallel-trends assumption holds. This assumption requires that firms with a treated relationship bank would have grown in parallel to other firms, had it not been for the reforms. The parallel-trends assumption is equivalent to the assumption that the components of the error term, including changes to firm capital demand and bank efficiency, are uncorrelated with the treatment indicator. Changes in firm capital demand, $\Delta^{t,t'}[ln(A_{ib})]$, and bank efficiency, $\Delta^{t,t'}[\beta_b]$, enter the error term ε_{ib}, as in equation 4. Importantly, the reforms increased the size of the banks independent of changes in the firms' capital demand or other determinants of bank efficiency. The results sections below present evidence in support of the identification assumption, including parallel pre-trends and balancing tests of firm and bank observables. To further strengthen the assumption, the regressions condition on a vector of control variables X_{ib}, described in the relevant results section.

The data allow me to analyze two periods. First, I can calculate the growth of stock corporations from 1951 to 1960. The two reforms were in 1952 and 1957, so the reforms affected all stock corporations with a treated relationship bank during this period. The second period I analyze is from 1951 to 1956. I have data on the growth of non-stock firms for this pe-

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

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

riod. The 1952 reform only affected treated banks outside the state of North-Rhine Westphalia (NRW). Hence, the treatment dummy in the specifications analyzing growth from 1951 to 1956 indicates whether firms had a relationship bank that was treated in 1952. The samples in the baseline regressions include all firms, for which I have data for the given period.

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

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

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

III Data

III.A Data on Firms

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

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

The main dataset builds on the 1952 and 1958/59 *Grossunternehmen* and the 1952/53 and 1961/62 *Aktiengesellschaften* volumes. From these volumes, I digitize data for all non-financial firms that, at a minimum, contain the names of the firm's relationship banks. There are 2,882 such stock corporations and 4,589 such non-stock firms in the 1952/53 volumes. Using the firm name and address as identifiers, I perform a Stata fuzzy match (reclink) procedure to connect

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

firm entries from 1952/53 volumes to the 1958/59 and 1961/62 volumes. I check all matches by hand to ensure there are no errors. Additionally, I identify 43 cases of firm exit, which are reported at the end of the Hoppenstedt volumes. There are also six reported mergers of firms in the dataset. To account for the mergers, I aggregate the employment and balance sheet values of all firms participating in the merger, record all their relationship banks, and keep only one firm in the dataset for the years before the merger. Overall, the match leaves 2,188 stock corporations and 3,706 non-stock firms in the dataset.

A Hoppenstedt volume reports data for one to three years prior to the release year of the volume. For instance, the 1952 volume mostly reports data for 1951, while the 1958/59 volume mostly covers 1956. For the firms in *Aktiengesellschaften*, I can calculate the growth of employment, revenue per worker, total assets, liabilities, and bank debt from 1951 to 1960. For the firms in *Grossunternehmen*, it is possible to calculate employment growth from 1951 to 1956. Some firm entries in the 1952/53 volumes report 1949 employment values, so I can calculate the pre-reform growth of these firms from 1949 to 1951. The measure of growth is the symmetric growth rate, a second-order approximation to the growth rate of the natural logarithm. It naturally limits the influence of outliers and accommodates zeros in the outcome variable, for example due to firm exit (Davis 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* 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). There is a successful match for 373 firms between the 1941 and 1952 volumes. From the 1970/71 volumes, I match the relationship banks of 4,191 firms to the 1952/53 volumes.

[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.

III.B Summary Statistics on Firms

Table 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 having close to 350 employees in 1951.[16] The very largest firms, however, were stock corporations, which means the average stock corporation was larger than the average non-stock firm. Both stock capital and bank debt were important parts of stock corporations' financing, amounting to an average of 37 percent and 10 percent of total assets, respectively. The percent ratio of bank debt over assets changed by an annual average of -0.11 percentage points from 1951 to 1960, which suggests bank debt grew at a marginally lower rate than assets. The average annual symmetric growth rate of aggregate employment in West Germany was 0.04 from 1951 to 1956 and 0.03 from 1951 to 1960. The average growth rates of firms in the sample were identical to these aggregate growth rates, suggesting the firms are fairly representative for the period.

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

In 1951, stock corporations had on average 3.2 relationship banks. Non-stock firms had on average 2.5. I calculate two main treatment dummies. The first, called "relationship bank treated in 1952/57", indicates whether one of the firm's relationship banks in 1951 was treated by the postwar banking reforms, either in 1952 or 1957. The second, called "relationship bank treated in 1952", measures whether a 1951 relationship bank was treated by the 1952 reform,

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

i.e. whether the firm had a relationship to a treated bank outside of North-Rhine Westphalia. 68 percent of stock corporations and 69 percent of non-stock firms have a relationship bank treated in 1952 or 1957, while 46 percent and 41 percent have a relationship bank treated in 1952.

To test whether firms with a treated relationship bank differed from other firms, I regress the two main treatment dummies on firm observables in Table 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, but that being an exporter was conditionally uncorrelated with having a treated bank.

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

III.C Data and Summary Statistics on Banks

Data on banks supplement the firm-level analysis. The Deutsche Bundesbank reports lending and deposits aggregated at the level of groups of banks, starting in 1948. One of the groups includes all the treated banks. Most similar to the treated banks is the group of other commercial banks. These other commercial banks all operated for profit. Most were active within only one

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

state, although a handful had branches in two or three states. The group of other commercial banks does not include small, single-branch private banks (*Privatbanken*).

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

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

The first three columns show the mechanical impact of the reforms on bank size. Total assets for each banking group are fixed at their 1952 values and then divided by the number of individual banks in the relevant period. As the reforms lowered the number of banks in the treated groups, the average size of each institution in the treated groups rose. For instance, the two reforms increased average bank size in the Deutsche Bank group by a factor of ten, since there were 10 state-level banks during the breakup. The untreated banks naturally remained unaffected.

Column (1) show that the average total assets of a treated bank in 1952 were 323 million Deutsche Mark, while for the average untreated bank total assets were 330 million. Columns

(4) to (6) present three cost ratios commonly used to measure bank efficiency, discussed in more detail in the results section. The 1952 values for all banks are relatively close. These numbers indicate that the untreated banks are a suitable control group for the treated banks.

III.D Data and Summary Statistics on Municipalities

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

IV Results on the Growth of Firms

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

IV.A The Effect on the Growth of Stock Corporations

Table IV estimates the effect of the reforms on stock corporations. The specifications are based on equation 5. The outcome in Panel A is the average annual growth rate of bank debt. The regressor of interest is a dummy for whether a bank treated in 1952 or 1957 was among the firm's relationship banks in 1951. The untreated group includes firms with relationship banks that were neither treated in 1952 nor in 1957. If the reforms led to an increase in firms' bank loan supply, the coefficient should be positive. The point estimate in column (1) implies that the growth of bank debt of firms with a treated bank was approximately 0.1 percentage points lower per year, compared to firms with no treated relationship bank. The 95 percent confidence interval excludes growth differences greater than 3 percentage points.

One potential concern is that broad regional differences or heterogeneous shocks to certain industries may mask the true effect. Column (2) includes the full interaction of 18 industry

fixed effects[18] with fixed effects for the Northern, Western, and Southern regions of Germany, equivalent to the banking zones 1952-57. To account for variation in growth due to firm size and age (Haltiwanger 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 region-specific shocks to firms in certain industries, of certain sizes, or certain ages do not affect the results. The coefficients remain close to zero and statistically insignificant. The coefficients remain close to zero and statistically insignificant. There is no evidence that firms with treated banks experienced an improvement in their bank loan supply relative to other firms.

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

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

IV.B The Effect on the Growth of Non-Stock Firms

Table V analyzes non-stock firms. The outcome variables is the average annual employment growth from 1951 to 1956. The regressor of interest in this table is a dummy for whether the firm had a relationship bank that was treated in 1952 (i.e. a treated bank outside of NRW). The untreated group includes firms with relationship banks that were neither treated in 1952

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

nor in 1957. It also includes firms with relationship banks that were only treated in 1957. The sample in columns (1) and (2) includes all firms with available employment data. Since data on assets do not exist for this sample, I control for size using fixed effects for four bins of firm employment (0-49, 50-249, 250-999, 1000+). The sample in columns (3) and (4) use the more restrictive, "matched" sample. I do not use the zonal fixed effects in columns (3) and (4) because the matched sample identifies the effect using only cross-zonal variation.

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

Other papers studying the effects of banking shocks on firms report large point estimates compared to the coefficients reported in this paper so far. For instance, Liberti et al. (2016) find that the introduction of a credit registry in Argentina improved the efficiency of bank credit allocation, increasing lending to firms by 61 percent within two years. Bertrand et al. (2007) study the effects of the 1980s deregulation of the French banking sector on bank-dependent firms, analyzing heterogeneity by firm profitability. More profitable firms (firms with a one standard deviation higher pre-reform return on assets) experienced a relative increase in the ratio of bank debt over total assets by 2.3 percentage points in the decade following the deregulation. There was a 23 percent increase in employment in bank-dependent industries relative to other industries (moving from the 25th to 75th percentile of the industry bank debt-to-assets ratio). Other papers study bank lending cuts. Due to the interbank liquidity freeze in 2007, the annual bank debt growth of the average Italian firm was 2.9 percentage points lower and employment growth was 0.5 percentage points lower from 2006 to 2010 (Cingano et al. 2016).

Spanish firms attached to weak banks experienced a reduction in the annual growth of bank debt by 1.3 percentage points and of employment by 0.7 percentage points from 2006 to 2010 (Bentolila et al. forthcoming). The large magnitude of the effects in the other studies, relative to the estimates of this paper, strengthens the conclusion that the postwar reforms had no economically significant impact on the growth of the average firm. The analysis of bank financial figures further below also supports this conclusion.

IV.C The Effect on the Growth of Opaque Firms

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

To create a more systematic classification of opaque firms, I identify three indicators for opacity: size, age, and asset tangibility. First, a literature argues that small firms face more idiosyncratic risk, have lower savings, and are difficult to assess for lenders. Studies typically use a cut-off of 50 employees to identify small firms (Gertler and Gilchrist 1994; Chodorow-Reich 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 have fewer than 50 employees, are younger than 10 years old in 1952, or are in the bottom ten percent by industry asset tangibility.

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

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

Columns (6) and (7) estimate the employment effects on opaque, non-stock firms. The outcome variables measure growth from 1951 to 1956, so the regressors of interest indicates whether firms had relationship banks that were treated in 1952. Column (6) shows that the employment growth of opaque firms was 2.9 percentage points lower, when more than half of relationship banks were treated. The coefficient is statistically significant at the 10 percent level. The effect remains of similar magnitude and significant when I use only the matched sample in column (7). The effect on firms, for which less than half of their relationship banks were treated, is negative, but smaller and insignificant in columns (6) and (7). These estimates

suggest the employment outcomes of non-stock firms are more vulnerable to bank loan supply than stock corporations. A likely reason is that non-stock firms cannot fund themselves by issuing additional stock capital.[19]

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

IV.D Robustness Checks on the Growth of Firms

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

Columns (2) to (4) restrict the sample to firms that are particularly likely to benefit from shocks to the efficiency of their banks. Column (2) analyzes stock corporations with a high (above-median) ratio of bank debt over total liabilities in 1951. These firms particularly depend on bank debt for financing. Column (3) analyzes stock corporations with a low ratio of stock capital over total assets. These firms require more outside financing in general. Column (4) restricts the sample to firms that export some of their products, as reported in the 1952 *Grossunternehmen* volume for non-stock firms. Due to the high default risk and working capital requirements, many exporters rely on outside financing (Amiti and Weinstein 2011). The coefficients in columns (2) to (4) are all small and statistically insignificant. If the reforms had any impact on the financial services offered by banks, the groups of bank-dependent firms in columns (2) to (4) should have been most strongly affected. The absence of a significant effect

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

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

suggests that firms did not benefit from the reforms.

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

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

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

V Results Using Bank Data

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

V.A Financial Figures of Banks

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

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

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

Table IX reports the growth of financial statistics from 1952 to 1960 for the treated banks, three comparison banks, and the mean difference between the treated and 9 untreated banks.[23]

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

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

[23] Section III.C explains the selection of the years 1952 and 1960, the three comparison banks, and the 9 un-

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

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

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

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

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

V.B Media Mentions of the Treated Banks

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

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

The mentions of treated banks and executives increased strongly after both reforms. There were over 8 times as many mentions of a treated bank after the second reform than before the first reform in *Der Spiegel*, and over 3 times as many mentions of a treated bank executive. There is hardly any difference in the number of mentions of the word "bank" or "Deutschland" between the two periods, indicating that an increase in the number of articles about banks or

Germany cannot explain the effect. Mentions of the banks and executives in the Financial Times increased by over 259 times and 71 times, respectively. Changes in the mentions of "bank" (1.7) and "Germany" (2.5) cannot explain this increase. These figures suggest that one bank of size 10 receives more mentions in the media than do 10 banks of size one combined. Hence, consolidations can raise the overall media presence of the involved organizations.

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

VI Results on the New Relationship Banks of Firms

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

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

VI.A The New Relationship Banks of Opaque Firms

Opaque firms, as defined for the purpose of this section, had fewer than 50 employees in 1951 or were in the bottom ten percent by industry asset tangibility.[24] This definition differs from

[24]The use of pre-reform size to define opacity ensures that opacity is not endogenous to the causal effects of the reform. For instance, the addition of a treated relationship bank could have restricted firm employment growth for

the previous one of Section IV.C because it does not include firms younger than 10 years in 1952. By 1970, these firms were at least 18 years old, invalidating the argument that they are opaque because they could not have an established reputation and paper trail.[25]

Banking relationships in Germany rarely end. For instance, 94 percent of firms with a treated relationship bank in 1951 still had a treated relationship bank in 1970. Therefore, I begin by focusing on the establishment of new relationships, which is more common. To do so, I restrict the samples in columns (1) to (3) of Table XI to only firms without a treated relationship bank in 1951. The point estimate in column (1) of Table XI implies that the fraction of treated relationship banks was 5.6 percentage points lower among opaque firms in 1970, compared to non-opaque firms. The point estimate is statistically significant at the 1 percent level. Column (2) splits the treatment indicator into four subcategories, for firms with fewer than 20 employees, between 20 and 49 employees, asset tangibility below 0.15, and from 0.15 to 0.2. All four coefficients are negative, indicating that all dimensions of opacity were relevant. Column (3) adds industry and zonal fixed effects to the specification. The coefficient remains robust. This implies the effect cannot be explained by the treated banks specializing in certain industries and zones.

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

The analysis so far has focused on the establishment of new banking relationships, by restricting the samples to firms without a treated relationship bank in 1951. Columns (5) and (6) instead restrict the sample to firms with a treated relationship bank in 1951. Column (5) uses the 1970 fraction of treated relationship banks as outcome, while column (6) uses the 1951 fraction. The coefficient on opaque firms estimates the difference in the fraction of relationship

some opaque firms, keeping these firms under 50 employees. An unreported robustness check using firms with fewer than 50 employees in 1970 confirms the results found below.

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

banks that were treated between opaque and non-opaque firms, conditional on the firm having a relationship to a treated bank. The point estimates in columns (5) and (6) are identical. Both are statistically not different from zero. This suggests there was no differential change in the fraction of treated relationship banks from 1951 to 1970 between opaque and non-opaque relationship customers of the treated banks. This conclusion implies that the reforms did not affect the banking relationships of existing customers of the treated banks. The finding suggests that opaque firms found it difficult to switch banks even when they faced reduced bank loan supply after the reforms.

VI.B The New Relationship Banks of Risky Firms

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

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

within zones and industries.

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

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

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

Summary statistics on the relationship customers of the treated banks confirm that the treated banks became more exposed to risky firms and less exposed to opaque firms. The fraction of firms with high capital-to-assets ratio and the fraction of opaque firms among the

relationship customers of the treated banks decreased, while the fractions among the untreated banks increased.[26]

VII Results on Municipalities

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

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

The outcome in column (4) is the employment growth rate from 1951 to 1956. In this period, only the treated banks outside NRW were affected by the first reform of 1952. The coefficient for municipalities with one of these branches implies a 6.2 percentage point decrease in the employment growth rate. The effect is significant at the 10 percent level. The coefficient on municipalities with treated bank branches in NRW is less than one-third of the magnitude and insignificant. Column (5) reports a positive and insignificant correlation between growth

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

from 1949 to 1951 and a dummy for municipalities with a treated bank branch, suggesting there was no negative pre-trend before the reform. Column (6) performs a robustness check using the growth rate from 1951 to 1960 as outcome. The specification includes the full interaction of zonal fixed effects with the following controls: the employment growth rate from 1949 to 1951, five quantiles of total employment, the share of employment in manufacturing, the share of employment in the primary sector, and the employment share of war-time displaced. The coefficient is of similar magnitude to the one in the baseline specification of column (1) and significant at the 10 percent level.

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

VIII Conclusion

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

The evidence supports theories that suggests there are real costs to increases in bank size. Opaque (small, young, low-collateral) relationship customers of the treated banks experienced lower employment growth after the reforms. Other opaque firms were less likely to establish new banking relationships with the treated banks. These findings are in line with theories that suggest big banks are worse at processing soft information and dealing with opaque firms. Treated banks were more likely to establish new relationships with risky firms after the reforms, which is consistent with theories emphasizing moral hazard or internal agency problems in big

banks. Consistent with the firm-level results, the employment growth of municipalities with a treated bank branch did not improve after the reforms. Taken together, the results of this paper find no evidence that increases in bank size benefit real economic growth. The results throw into question the standard arguments against size-dependent banking regulation.

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Tables

Table 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
$\frac{100 \cdot Bank\,debt}{Assets}$ difference 1951-60	421	-0.11	1.39	-1.77	-0.11	1.79
Employment growth 1951-60	815	0.03	0.05	-0.03	0.03	0.09
Revenue per worker growth 1951-60	344	0.05	0.05	0.00	0.04	0.10
Panel B: Non-stock firms						
Employment	1,800	559	1121	91	344	1,017
Age	3,494	63	51	16	54	112
Exporter	2,593	0.39	0.49	0	0	1
Number of relationship banks	3,706	2.54	1.29	1	2	4
Relationship bank treated in 1952/57	3,706	0.69	0.46	0	1	1
Relationship bank treated in 1952	3,706	0.41	0.49	0	0	1
Employment growth 1951-56	1,521	0.04	0.07	-0.01	0.03	0.13

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

Table 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
Sample	All	All	All	All	Matched	Matched
Firm type	Stock	Stock	Non-stock	Non-stock	Stock	Non-stock

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

Table III: Summary statistics by banking group

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

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

Table IV: The effect on the growth of stock corporations

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

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

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

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

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

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

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

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

Table VII: The effect on the growth of opaque firms

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

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

Table VIII: Robustness checks for the effect on firm growth

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

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

Table IX: 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)							
$\frac{Non-int\ cost}{Assets}$	-0.27	-0.10	-0.68	-1.05	-1.53	-1.54	0.80
							(0.31)
$\frac{Non-int\ cost}{Revenue}$	-7.29	-19.92	-15.32	-25.19	-38.99	-10.23	1.53
							(6.79)
$\frac{Empl\ comp}{Assets}$	0.00	-0.16	-0.41	-0.76	-1.17	-1.62	0.45
							(0.28)
Panel C: The average fraction among firms' relationship banks by period (in percent)							
1951	18.87	10.84	6.66	2.48	1.53	0.04	
1958-61	18.56	10.87	7.97	2.47	1.42	0.03	
1970	16.58	10.79	9.57	2.46	1.78	0.02	

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

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

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

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

Table XI: New banking relationships with opaque firms

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

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

Table XII: New banking relationships with risky firms

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

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

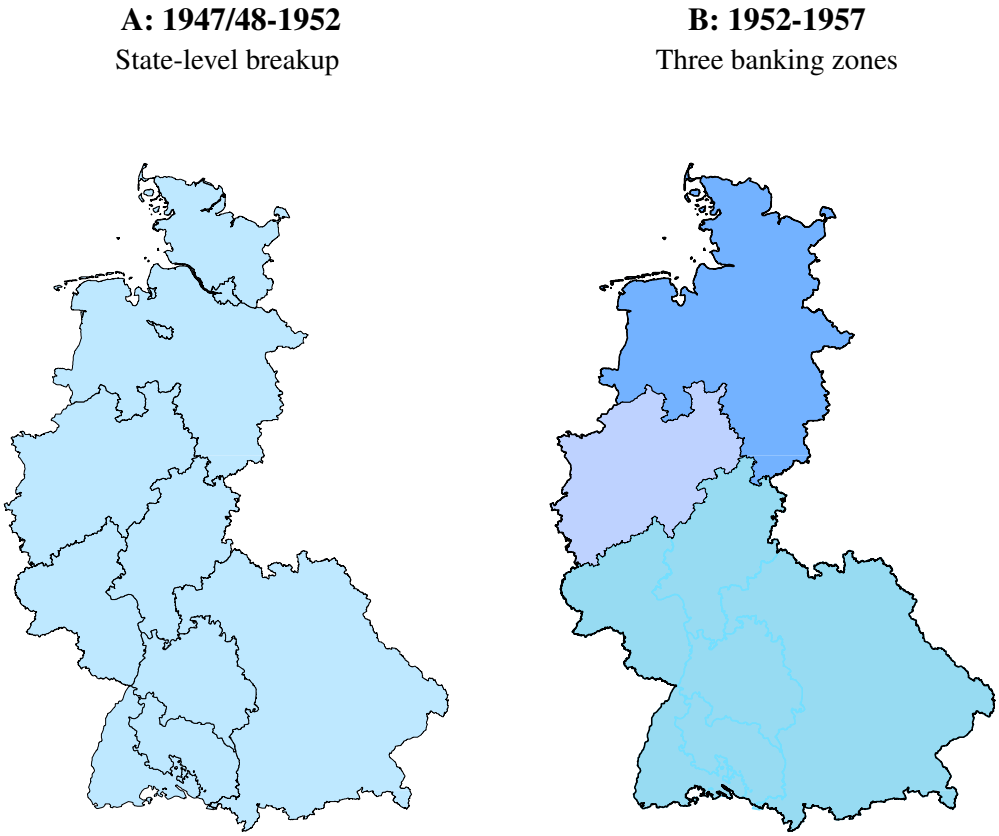
Table XIII: The effect on municipal employment

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

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

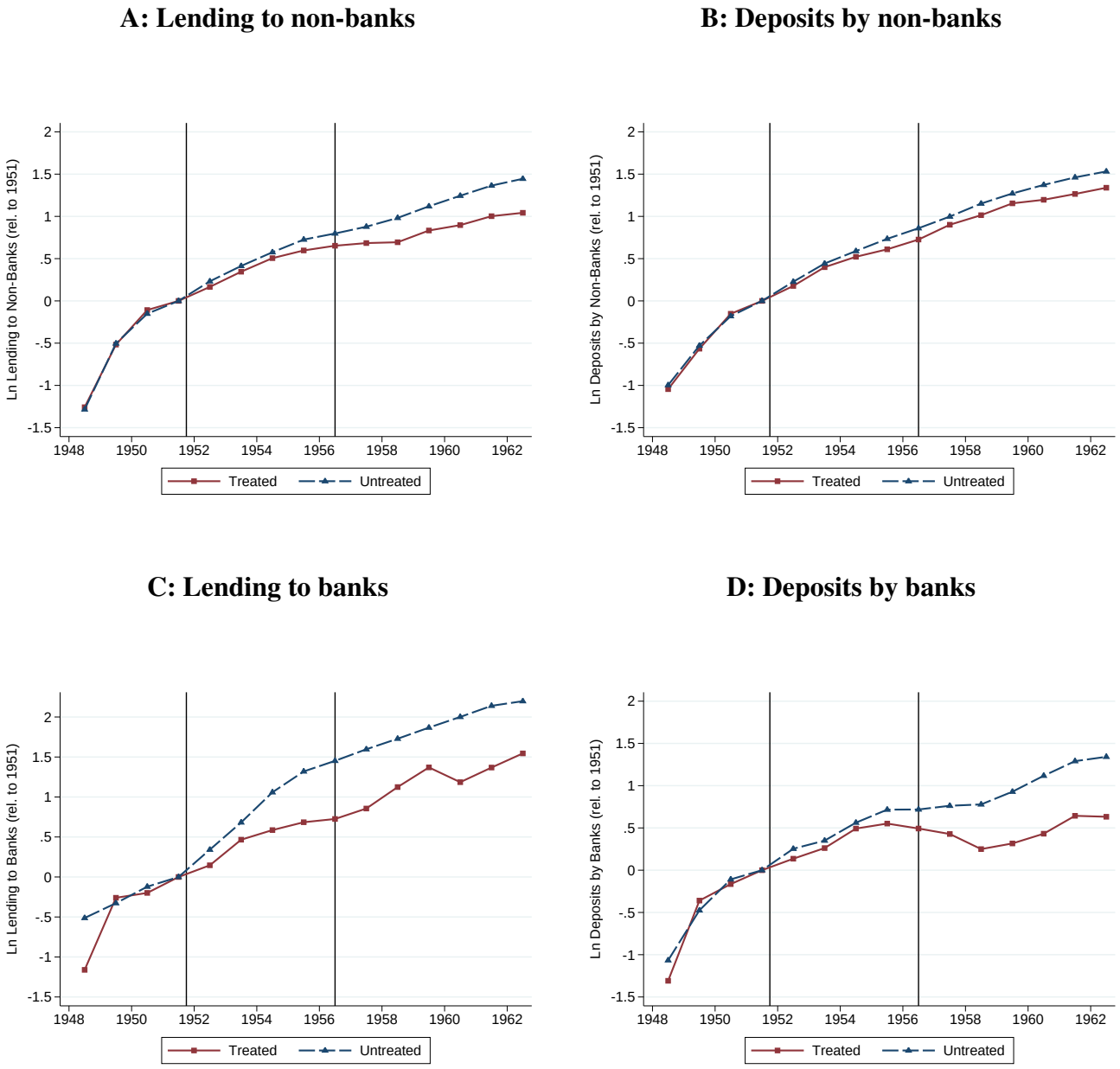
Figures

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 and deposits



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

Appendix Tables

Table A.I: Further tests by firm opacity

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

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

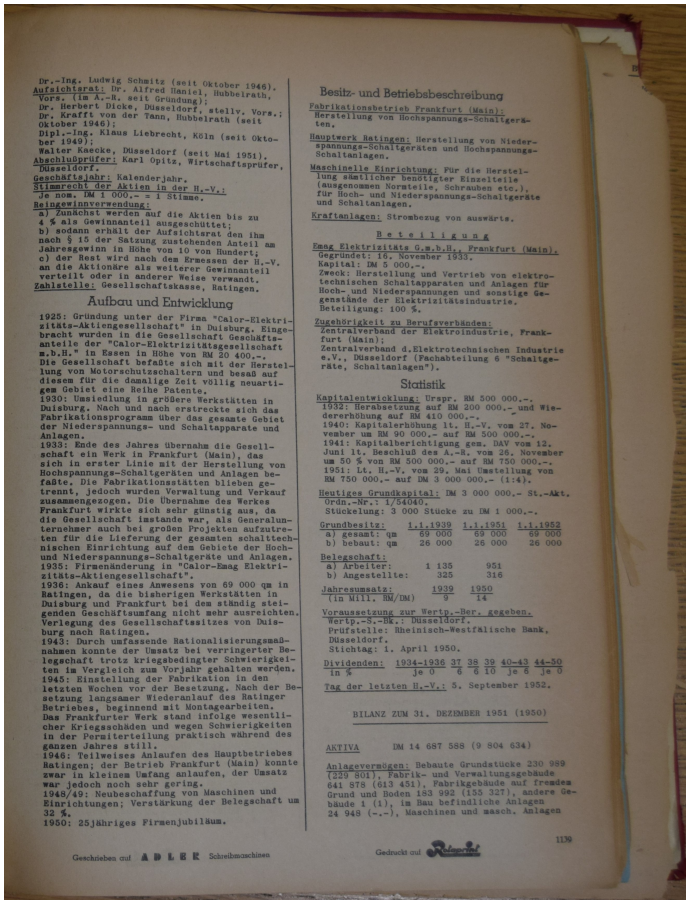
Table A.II: Using 1940 relationship banks as treatment indicators

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

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

Appendix Figures

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 bezog 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 Persilerteilung 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.

Geschrieben auf ADLER Schreibmaschinen

Besitz- und Betriebsbeschreibung

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

Beteiligung

Emag Elektrizitäts G.m.b.H., Frankfurt (Main).
Gegründet: 16. November 1933.
Kapital: DM 5 000.-.
Zweck: Herstellung und Vertrieb von elektrotechnischen Schaltapparaten und Anlagen für Hoch- und Niederspannungen und sonstige Gegenstände der Elektrizitätsindustrie.
Beteiligung: 100 %.

Zugehörigkeit zu Berufsverbänden:
Zentralverband der Elektroindustrie, Frankfurt (Main);
Zentralverband d.Elektrotechnischen Industrie e.V., Düsseldorf (Fachabteilung 6 "Schaltgeräte, Schaltanlagen").

Statistik

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

Heutiges Grundkapital: DM 3 000 000.- St.-Akt. Ordn.-Nr.: 1/54040.
Stückelung: 3 000 Stücke zu DM 1 000.-.

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

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

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

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

BILANZ ZUM 31. DEZEMBER 1951 (1950)

AKTIVA DM 14 687 588 (9 804 634)

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

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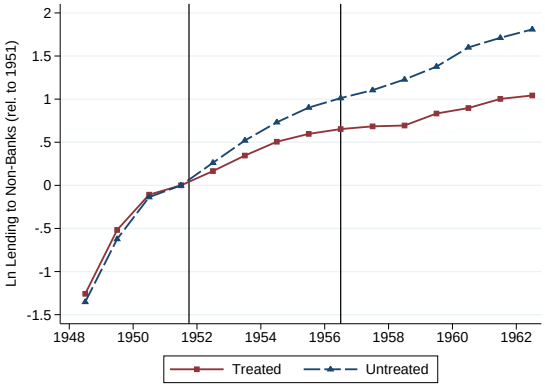
Gedruckt auf Rotaprint

Figure A.II: Lending by the treated banks compared to all other banks



Notes: The figure shows lending using all other German banks as 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.