Are Bigger Banks Better? Firm-Level Evidence from Germany

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The effects of large banks on the real economy are theoretically ambiguous and politically controversial. I identify quasi-exogenous increases in bank size in postwar Germany. I show that firms did not grow faster after their relationship banks became bigger. In fact, opaque borrowers grew more slowly. The enlarged banks did not increase profits or efficiency but worked with riskier borrowers. Bank managers benefited through higher salaries and media attention. The results are based on newly digitized microdata on German firms and their banks. Overall, the findings reveal that bigger banks do not always raise real growth and can actually harm some borrowers and the real economy.

I. Introduction

The impact of large firms on the economy has recently received significant attention (Zingales 2017; Philippon 2019). The topic is particularly

salient in banking. The continued growth of banks in past decades and failures of large banks in recent crises have kept the issue of bank size at the forefront of policy debates. Some policy makers argue that limits on bank size could reduce financial instability and excessive risk taking.[1] But if larger banks generate economies of scale, then discouraging banks from growing bigger could decrease the quality of financial services and harm real economic growth.

In this paper, I identify a setting where increases in bank size were exogenous to the performance of banks and their borrowers. This rare occurrence allows me to estimate how changes in bank size causally affected firms in the real economy. The setting is postwar Germany. Reforms in 1952 and 1957 determined when several institutions were allowed to consolidate from state-level banks into national banks. I find that, on average, firms did not grow faster after their banks became larger. The consolidations did not make the banks more cost-efficient or profitable. These findings show that increased bank size does not always generate improvements in bank efficiency or firm growth, in contrast to leading theories. Furthermore, opaque (small, young, low-collateral) firms grew more slowly after their banks got bigger, consistent with the view that bigger banks are worse at processing soft information.

The effect of bank size on borrower growth depends on the strength of several mechanisms that have been analyzed theoretically. On the one hand, theory suggests that bigger banks might be more efficient and stable. For instance, bigger banks spread fixed costs over more borrowers (Humphrey 1990), are more diversified (Diamond 1984), issue large loans without syndication (Holmstrom and Tirole 1997), and generate operational synergies (Kanatas and Qi 2003). Bigger banks might also improve screening and the allocation of capital across borrowers (Stein 1997, 2003). These channels could raise the average growth of bank borrowers. On the other hand, diversification might not be beneficial once banks exceed a certain size (Krasa and Villamil 1992b; Cerasi and Daltung 2000). Large organizations might entail complex agency and information problems (Rajan, Servaes, and Zingales 2000; Scharfstein and Stein 2000; Berger and Udell 2002; Stein 2002). These channels could harm borrower growth.[2]

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[1] The market share of the 10 biggest US banks increased from around 25% in 1990 to over 60% in 2014 (McCord and Prescott 2014). Recent regulatory proposals include outright caps on bank size as well as indirect incentives for banks to remain small, e.g., higher capital requirements and stress tests for big banks (Stern and Feldman 2004; Stein 2013; Greenwood et al. 2017).

[2] The banking reforms in postwar Germany increased bank size without directly deregulating local competition or entry, so I do not analyze the effects of bank size on local competition in this paper.

Given the ambiguous theoretical predictions, the effect of bank size on firm growth is an empirical question. I first estimate the overall effect of the banking consolidations on borrower growth. The results capture how the various theoretical mechanisms, on net, affected firm growth. Thereafter, I turn to assessing individual theories about bank operations. I provide evidence on which theoretical mechanisms were important determinants of bank efficiency and firm performance after banks got bigger.

The empirical challenge in estimating the effects of bigger banks is that banks do not become big randomly. For example, a bank might strategically consolidate with another bank if it expects the borrowers of the other bank to grow faster in the future. In such cases, one would observe a positive correlation between bank size and the growth of borrowers even if bank size has no causal effect on borrowers. Two features of the postwar German banking system allow me to overcome this challenge. The first feature is the reliance of German firms on relationship banking. As a result of asymmetric information, bank-borrower relationships were sticky. This means that shocks to a given bank affected the cost of banking services for its relationship borrowers.

The second feature is the banking policy of the Allied occupiers in postwar Germany. The Allies believed that three banks with nationwide branch networks (Commerzbank, Deutsche Bank, and Dresdner Bank) had contributed to the Nazi war effort. These banks constitute the treatment group for the purposes of this paper. In 1947–48, the Allies broke up the treated banks into 30 independent state-level organizations and prohibited the new banks from branching outside state borders. A first reform in 1952 permitted some of the state-level banks to consolidate with other state-level banks within three banking zones. This meant that the 30 state-level banks merged to become nine treated institutions, one for each former national bank in each banking zone. A second reform in 1957 permitted the reconsolidation of the three original, national banks. Hence, borrowers with a treated relationship bank experienced sharp increases in the size of their relationship banks in 1952 and 1957.[3]

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

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

bank representatives, and the Allies then determined specifically when the consolidations happened.

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

Policy makers often consider a bank systemically important if its assets exceed roughly 1%–2% of GDP (gross domestic product). During the breakup, all of the treated state-level 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% of GDP. Hence, the repeal of the Allied legislation transformed the treated banks from 30 state-level lenders into three banks of systemic importance.

One contribution of this paper is to present the first digital microdata set on firms in postwar Germany. The data were hand-digitized from historical firm records and cover the postwar period until 1970. The new data set allows researchers to study the corporate side of Germany's "economic miracle" after World War II. It includes the bank relationships of around 5,900 firms, the employment growth of around 2,300 firms, and the balance sheet variables of around 400 firms.

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

The main results show that firms did not experience faster growth rates of bank debt, employment, or revenue per worker if they had a relationship bank that was treated by a reform. The estimates are similar for firms that were more dependent on banking services. Firms that were newly

added as relationship borrowers by the treated banks after the consolidations also did not grow faster than comparable firms. The treated banks themselves did not lend more or add more relationship borrowers after consolidating, relative to other banks.

I separately examine a subsample of firms that were small, young, or in industries with a low share of easily collateralizable assets. These firms were "opaque" because when they applied for loans they relied on their banks to process hard-to-verify, soft information. For example, opaque firms were more likely to receive unsecured "character loans." Opaque firms replaced bank debt with other sources of financing after their relationship banks grew in size, indicating an increase in their relative cost of bank debt. Opaque firms with no access to stock market funding reduced employment growth. There were no corresponding effects on large or nonopaque firms with a treated relationship bank. These results are consistent with theories arguing that big banks are worse at processing soft information.

In the final set of main results, I examine the effects of the consolidations at a higher level of aggregation, on municipalities. The municipality-level results capture not only the effects on firm growth but also other potential channels, such as local general equilibrium effects or changes in the municipal banking market. I find that municipalities with a treated bank branch experienced lower employment growth after the reforms. The negative effect on municipalities is consistent with the firm-level results, because opaque firms grew more slowly and other firms did not benefit from the consolidations.

A range of theories predict how changes in bank size affect bank and borrower outcomes. I discuss the theoretical mechanisms that may have played a role. Some theories suggest that bank profitability and efficiency always rise with size. However, I find that the treated banks did not become more profitable or efficient after consolidating, relative to comparable untreated banks. Diversification theory predicts that bigger banks are more stable and experience larger deposit inflows. I find that fluctuations and deposit inflows of the treated banking groups did not improve after the consolidations. This suggests that the benefits of diversification apply only to relatively small banks.

There is also no evidence that the enlarged banks' ability to issue large loans or cross-sell products affected borrowers. Allowing banks to use internal capital markets did not significantly alter the allocation of capital, likely because interbank markets were adequate substitutes for internal capital markets (Horstmann 1991). Finally, I show that the treated banks were more likely to establish new relationships with risky firms (firms with high volatility or high leverage) after consolidating, relative to other banks. This suggests that the treated banks were willing to take greater risks, consistent with theories that emphasize "too-big-to-fail" incentives.

If the consolidations did not improve profits or efficiency, why did the treated bank managers favor consolidating? I report that the pay of treated bank managers rose after the consolidations, relative to that of untreated managers. While the literature has documented that firm size and executive pay are correlated (Edmans, Gabaix, and Jenter 2017), this finding suggests that increases in size cause greater pay. Furthermore, I find that the media mentions of the treated managers increased. Managers might enjoy media coverage as an end in itself. Media coverage might also have tangible benefits to firms because it can affect consumer choices and political decisions (Enikolopov and Petrova 2015; Bursztyn and Cantoni 2016). Overall, greater salaries and media mentions might explain why managers enjoy building corporate "empires" (Stein 2003).

The results from different data sets and analyses all paint a consistent picture: bigger banks did not improve the growth of borrowers. Whether bigger banks benefit or harm borrowers depends on the net impact of efficiency-enhancing and harmful mechanisms. Leading theories imply that certain mechanisms, such as improved cost structures or diversification, should boost bank and borrower outcomes after banks get bigger. The experience from postwar Germany highlights that the efficiency-enhancing mechanisms do not always outweigh the harmful effects.

II.   Related Empirical Literature

Existing evidence about the impact of bank size on bank efficiency is mixed (Berger and Mester 1997; Rhoades 1998; Berger, Demsetz, and Strahan 1999).[4] Most existing work is based on cross-sectional comparisons or bank consolidations. It is generally difficult to estimate causal effects using cross-sectional data or consolidations. Cross-sectional differences in bank size might be driven by reverse causality; that is, banks might have first improved their efficiency and then become bigger as a result. Bank size might also be correlated with unobservable variables, such as the quality of managers (Kovner, Vickery, and Zhou 2014). Moreover, consolidating banks are often systematically different from other banks, and control groups are hard to find (Calomiris 1999; Calomiris and Karceski 2000; Focarelli, Panetta, and Salleo 2002).

This paper contributes to the literature by identifying bank consolidations whose timing was exogenous to the growth of banks and their borrowers. This allows me to estimate how a shock to bank size causally affected firms, municipalities, and banks. I analyze not only the effects on financial

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[4] Some recent papers using cross-sectional data suggest that returns to scale might have increased over time (Feng and Serletis 2010; Wheelock and Wilson 2012, 2018; Hughes and Mester 2013; Davies and Tracey 2014; Kovner, Vickery, and Zhou 2014; Biswas, Gómez, and Zhai 2017; Hughes et al. 2019).

outcomes of banks but also how the real growth of firms and municipalities changed.

A literature has analyzed how big banks interact with small borrowers. Cross-sectional evidence suggests that big banks lend proportionally less to small firms (Berger, Kashyap, and Scalise 1995, Berger et al. 2005). The evidence from bank consolidations is mixed (Berger et al. 1998; Peek and Rosengren 1998; Strahan and Weston 1998; Berger, Klapper, and Udell 2001; Sapienza 2002; Jagtiani, Kotliar, and Maingi 2016). I contribute to this literature in three ways: by showing that increases in bank size can have negative effects on employment, by highlighting negative effects not just for small firms but also for young and low-collateral firms, and through cleaner identification, by exploiting quasi-exogenous variation in the size of the same bank serving the same firm.[5]

An influential literature shows that banking deregulation affected the US economy, starting with seminal papers by Hubbard and Palia (1995) and Jayaratne and Strahan (1996) and recently reviewed by Berger, Molyneux, and Wilson (2020). Several papers emphasize that increases in the number of banks in local markets and the threat of new bank entry were chiefly responsible for the effects of deregulation.[6] In contrast, the banking reforms in postwar Germany increased bank size without directly deregulating local competition or entry.[7] A further difference is that I analyze state-level banks that consolidated into national banks, while many US banks operated at the city level before deregulation. The results in this paper therefore speak to consolidations among relatively larger banks.

In related historical work, Eichengreen and Ritschl (2009) analyze the German postwar economy, and Hoshi and Kashyap (2004) describe how the US occupiers reformed Japanese corporate finance.[8]

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[5] A related literature shows that banks with longer hierarchies are less reliant on soft information (Cole, Goldberg, and White 2004; Liberti and Mian 2009; Cerqueiro, Degryse, and Ongena 2011; Canales and Nanda 2012) and use internal information differently (Qian, Strahan, and Yang 2015). Adding hierarchical layers in Indian bank branches, keeping overall bank size constant, reduced lending and loan performance (Skrastins and Vig 2019).

[6] Both intrastate branching and interstate banking allowed "banks to enter new markets and threaten incumbent banks" (Stiroh and Strahan 2003, 801; see also Jayaratne and Strahan 1998 and Evanoff and Ors 2008). Intrastate branching increased the actual number of competitors in local markets and the threat of new banks entering, which raised competitive pressures on incumbents even if there was no actual entry (Jayaratne and Strahan 1996). Interstate banking raised competitive pressures by affecting the number of participants in the market for corporate control (Hubbard and Palia 1995).

[7] Banking was a local business in postwar Germany (Eicke 1952), so state-level treated banks that had belonged to the same national banking group did not compete with each other across states (Horstmann 1991). Consistent with these facts, the data on bank-firm relationships in 1951 show that 99% of firms had treated relationship banks only in the state of their headquarters. The exceptions may be explained by firms operating multiple establishments.

[8] By analyzing a unique historical event to test whether influential theoretical predictions always hold in the data, this paper is inspired by a long tradition at the intersection

III. Relationship Banking and Reforms in Postwar Germany

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

A. Relationship Banking

Three types of banks operated in postwar Germany: commercial, cooperative, and public banks. Commercial banks, including the treated banks, operated for profit and offered the full range of banking services, including lending, deposit taking, payment transactions, and underwriting of securities.

Relationship banking is an integral part of the German banking system. Firms of all sizes have formed close and durable ties to their relationship banks since the late nineteenth century (Calomiris 1995; Elsas and Krahnen 1998; Fohlin 1998; Harhoff and Körting 1998; Guinnane 2002). In postwar Germany, relationship banks had specialized knowledge that allowed them to assess the creditworthiness of their relationship borrowers (Menzel 1960; Pohl 1973). They managed all banking business for their borrowers, including satisfying credit demand (Eicke 1952).[9]

Long-standing ties between banks and firms improve banks' screening and monitoring capabilities (Sharpe 1990; Boot 2000). At the same, firm-bank relationships become sticky, so that idiosyncratic shocks to relationship banks affect the growth of borrowers.[10] Firms' demand for banking services was particularly high in the fast-growing economy of postwar Germany, so banking shocks had the potential to affect real outcomes (Holtfrerich 1995, 544).

B. Banking Reforms

Three Allied military governments ruled over West Germany after World War II. The British were in charge of northern and western Germany,

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of macroeconomics and finance. Influential examples include Gorton (1988) and Richardson and Troost (2009) on banking panics, Calomiris (1990) on deposit insurance, Jayaratne and Strahan (1996) on banking deregulation, and Calomiris and Mason (2003) on consequences of bank failures.

[9] Banking relationships rarely ended in postwar Germany: 96% of firms that had a treated relationship bank in 1951 still had one in 1960.

[10] A large literature has shown this in many periods and countries, including Doerr et al. (2020) for the 1931 German banking crisis, Benmelech, Frydman, and Papanikolaou (2019) for the US Great Depression, Amiti and Weinstein (2011) for Japan from 1990 to 2010, and Bentolila et al. (2018) and Huber (2018) for the 2008–9 crisis in Spain and Germany, respectively.

most of the south was under American control, and the French governed two small regions in the southwest. The military government of the American zone was the driving force behind banking policy (Horstmann 1991).

1.   Phase 1: State-Level Breakup 1947/48–52

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

In March 1946, the American and French military governments prohibited branches in their zones from coordinating business with managers in other zones. This effectively ended the national operations of the treated banks (Wolf 1993, 28). In May 1947, the Americans formally created new state-level banks in their zone. A state-level bank was not allowed to operate a branch in another federal state. 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. The directors of the state-level banks 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 that the state-level banks operated independently and formally administered the banks' property (Adler 1949; Horstmann 1991, 169). The relationship between individual branches and their customers, the local market share of branches, and the financial services they offered remained unchanged (Adler 1949). What changed was that each branch now belonged to a much smaller state-level bank, rather than to a national organization.

The French military government issued an identical decree for its zone in September 1947. The British were initially against the breakup but gave in to US pressure and applied a similar regulation in their zone in April 1948. Allied legislation meant that there were now 30 separate state-level banks, whose branches had belonged to three national banks before the war (fig. 1A).[11] The Allied laws did not directly affect the other commercial, cooperative, or public banks.

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[11] To be clear, consider the example of Dresdner Bank. Instead of one national Dresdner Bank, as before the war, there were 11 state-level successor banks in 1948, one in each state. Each state-level bank was composed of the former Dresdner Bank branches in the relevant state. Deutsche Bank had not previously operated branches in Schleswig-Holstein, so there
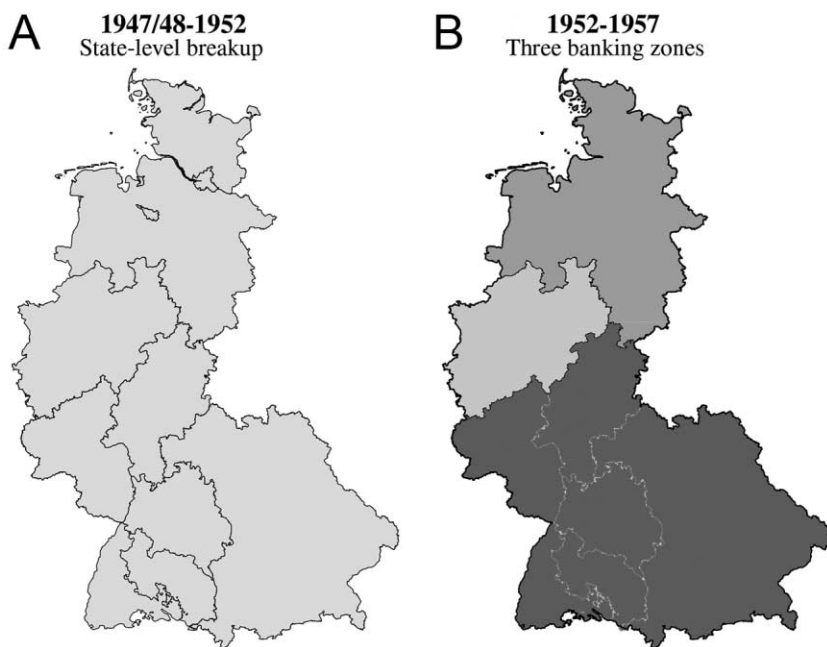
Fɪɢ. 1.—Maps of the postwar banking zones. The figure shows the two phases of the breakup. The postwar breakup limited banks to operating branches only within state borders (*A*). The 1952 reform lifted the state-level restrictions and allowed banks to operate in three regional zones (*B*). The northern zone comprised the northern states, which were under British control. The combined American and French territories formed the southern zone. The western zone was the state of North Rhine–Westphalia, also under British control. The 1957 reform removed all restrictions. A color version of this figure is available online.

The treated state-level banks acted "with autonomous management, independent custodians, and distinct business policies" and "appeared unrelated to the former national banks" (Horstmann 1991, 151). Deutsche Bank, for instance, "was decentralized after April 1, 1948, for all practical purposes" (Holtfrerich 1995, 484). The Allies intended the breakup to the state level to be permanent (*Der Spiegel* 1951). Several other firms that were broken up after the war were not allowed to reconsolidate in future years.[12] Any hopes for national reconsolidation that may have existed among the bank managers in 1949 were "wishful thinking" (Horstmann 1991, 181).

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were 10 Deutsche Bank successors. Commerzbank had not previously operated branches in Baden and Württemberg-Hohenzollern, so there were nine Commerzbank successors.

[12] To reduce the likelihood of a future war, the Allies also broke up the chemical manufacturer IG Farben, the steel corporation Vereinigte Stahlwerke, and the movie producer Universum Film. Unlike in the case of banking, German politicians did not believe that these other industries would generate significant economies of scale. Hence, these organizations were not allowed to reconsolidate to their former structures in sovereign Germany, despite the wishes of their management (Kreikamp 1977).

2. Phase 2: Three Banking Zones 1952–57

By the early 1950s, the American diplomatic stance toward West Germany had mellowed (Scholtyseck 2006). Hoping that a stable German economy could stop the spread of communism, the Americans became open to German suggestions about how to facilitate growth. The German federal government and managers of treated banks had always maintained that operating as separate banks harmed the banks and their borrowers (Ahrens 2007). The Americans made it clear that they were willing to consider some form of partial reconsolidation among the treated banks but would veto full reconsolidation at the national level (Holtfrerich 1995).

The sides reached a compromise that was passed as law in March 1952. The law defined three banking zones (fig. 1*B*). 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. Other consolidations and out-of-zone branching was prohibited. Because the borders of the state of North Rhine–Westphalia were identical to the borders of the new western zone, banks operating in the western zone remained unaffected by the 1952 reform.

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

The 1952 reform affected the operations of the treated banks. They were able to spread fixed costs over a larger base, to fund large loans on their own instead of through syndicates, to use internal capital markets to transfer capital across states, and to work with a more diversified lending portfolio and depositor base (Lanner 1951; Wandel 1980; Holtfrerich 1995). Historians disagree to what extent these operational changes affected the performance of banks and their borrowers (Pohl 1986; Horstmann 1991). The rules of the breakup that had applied to the state-level banks remained in place for the zonal banks, but the Allies did not enforce the rules as strictly as before 1952.[13] As a result, my analysis below emphasizes the effects of the 1952 reform and compares bank and firm outcomes before and after 1952.

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[13] For example, the directors of the successor institutes of Dresdner Bank met around six times a year, starting in late 1952, as did the directors of the Deutsche Bank successor banks (Holtfrerich 1995; Ahrens 2007).

3.   Phase 3: National Banks from 1957

During the 1950s, the Cold War made Germany a key ally of the West. The Allies granted the German government full sovereignty in the Paris Agreement of 1955. One condition for sovereignty set by the Allies was that the treated banks would remain separated at least until 1956. The German federal government passed a law that allowed national reconsolidation of the treated banks from January 1957 (Scholtyseck 2006). The banks consolidated soon thereafter. By 1958, there were once again three large banks with a national branch network, operating under their old, prewar names.

IV.   Empirical Strategy

The treated banks favored consolidation throughout the postwar period. This makes them comparable to many other banks in the past and present that want to merge. The key difference is that government reforms, not endogenous shocks to banks and their borrowers, determined how and when the treated banks consolidated. This means that the reforms generate quasi experiments that allow estimating how changes in bank size causally affect banks and their borrowers. A formal model in appendix B (apps. A–L are available online) illustrates how the reforms help to overcome the usual empirical challenges in estimating the effects of bank size on firm growth.

   The main empirical analysis of the paper studies whether firms with a treated relationship bank grew differently from firms with other relationship banks after the reforms. Specifically, I regress measures of firm growth on an indicator for whether one of the firm's relationship banks was treated by a reform. The coefficient on the indicator captures all the channels through which a change in bank size could affect firms. Potential channels include cheaper access to loans and deposits, improved underwriting and payment services, and less uncertainty about future credit access. I investigate the effects on both firms that already had a treated relationship in 1951 and firms that newly added a treated relationship bank after a reform.

   The empirical strategy estimates the causal effect of having a treated relationship bank if a parallel-trends assumption holds. This assumption requires that, had it not been for the reforms, firms with a treated relationship bank would have grown in parallel to other firms. The results sections below present evidence in support of the identification assumption, for instance by showing that there were parallel pretrends and that the results are robust to controlling for other shocks.

   The main regressions use all firms with available data in the sample. To create a tighter empirical test of the effects of the 1952 reform, I additionally create a more restrictive, "focused" sample. The focused sample includes

only firms whose relationship bank was broken up by the Allies after the war. This restriction addresses the concern that firms with a treated relationship bank were on different growth paths than firms with banks that were never treated. Furthermore, the focused sample contains only firms located in the western zone (i.e., the state of North Rhine–Westphalia) or in states bordering the western zone. The western zone was a hasty postwar creation, based on the British desire to institutionalize its control over western Germany. Many subregions of the western zone were culturally more similar to the states they bordered than to the other subregions in the western zone (von Alemann 2001). Finally, I drop firms located in the Ruhr area, an urban region in the western zone based on heavy industry, as well as coal and steel producers, from the focused sample. These restrictions ensure that the formation of the European Coal and Steel Community in 1952 does not bias the results.

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

V.   Firm Data and Summary Statistics

At the heart of the paper lies a newly digitized data set on German firms in the 1950s. The data are from two publication series by the publisher Hoppenstedt. Supported by the German National Library of Economics, I was able to access the 1941, 1952, 1958–59, and 1970 volumes of *Handbuch der Grossunternehmen* and the 1952–53, 1961–62, and 1970–71 volumes of *Handbuch der deutschen Aktiengesellschaften* in various German archives.[14] The poor print quality of the historical volumes makes automatic character recognition difficult, so the data had to be digitized by hand. I describe the data construction and summary statistics in more detail in appendix C.

The series on *Aktiengesellschaften* provides information on the universe of German stock corporations.[15] The available data allow me to calculate the growth of stock corporations' employment, revenue per worker, total assets, liabilities, and bank debt from 1951 to 1960. The series on *Grossunternehmen*

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[14] The historical volumes of these series are difficult to locate. Hoppenstedt destroyed its entire archive a few years ago. Library catalogs do not always report holdings accurately because historical volumes are often misplaced or destroyed.

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

reports a subset of firms of other legal forms, for which I can calculate employment growth from 1951 to 1956. I also carry out supplementary analyses using longer periods of firm growth based on the 1941 and 1970 *Grossunternehmen* and the 1970–71 *Aktiengesellschaften* volumes. To measure firm growth, I use the symmetric growth rate.[16] To accommodate comparisons across periods of different lengths, I calculate all firm growth rates as average annual growth rates, by dividing the symmetric growth over the entire period by the number of years in the period.[17]

The sample firms are fairly representative for aggregate growth during this period. Aggregate employment growth in West Germany was 4.2 log points per year (on average) from 1951 to 1956 and 3.5 log points from 1951 to 1960. Employment of the average sample firm grew by 4.3 log points annually from 1951 to 1956 and by 3.2 log points from 1951 to 1960. Sample firms with nonmissing employment data cover 15% of West Germany's 14.6 million employees in 1951 (Bundesministerium für Arbeit 1951).

Both publication series provide the names of firms' relationship banks. I calculate two main treatment indicators. The first, called "relationship bank treated in 1952/57," indicates whether one of the firm's relationship banks in 1951 was treated by at least one of the banking reforms in 1952 or 1957. The second treatment indicator, called "relationship bank treated in 1952," measures whether one of the firm's relationship banks was treated by the 1952 reform, that is, whether a relationship bank belonged to a treated banking group and was located outside of the western zone. There is no information on which financial services or how much lending a firm received from a particular relationship bank. Forty-three percent of firms had a relationship bank treated in 1952, while 69% of firms had a relationship bank treated in either 1952 or 1957 (table A.I; tables A.I–A.XXIV are available online).

To test whether firms with a treated relationship bank were different, I regress each treatment indicator on firm characteristics. In the full sample, firms with a relationship bank treated in 1952 or 1957 were larger and older (table A.II, col. 1), but these firms did not finance themselves with more stock capital or bank debt, conditional on size and age (col. 2). Correlations between firm characteristics and treatment indicators do not invalidate the empirical strategy, because it assumes only parallel trends and because I can control for observable differences. In the focused sample, there is no significant association between having a bank

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[16] Formally, the symmetric growth of y from $t-1$ to t is $g^y = 2 \cdot [(y_t - y_{t-1})/(y_t + y_{t-1})]$. It is a second-order approximation to the growth rate of the natural logarithm and is bounded in the interval $[-2, 2]$ (Davis, Haltiwanger, and Schuh 1998). It naturally limits the influence of outliers and accommodates zeros in the outcome variable, e.g., because of firm exits.

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

treated in 1952 and size, age, stock capital, or bank debt financing (cols. 3, 4). This confirms that, in the focused sample, observationally equivalent firms were exposed to different bank size shocks.

VI.   Main Results on the Growth of Banks, Firms, and Municipalities

This section presents the main results of the paper. I analyze how the banking reforms of 1952 and 1957 affected the growth of treated banks, of firms with a treated relationship bank, and of municipalities with treated branches.

A.   The Growth of Treated Banks

I first examine the growth of bank lending and deposits. The Deutsche Bundesbank provides data aggregated at the level of different groups of banks. One group includes all treated banks. The most comparable untreated group in terms of structure and business strategy includes other commercial banks.[18] Before 1952, total lending to firms and households of treated and untreated commercial banks evolved in parallel (fig. 2*A*). This suggests that the treated and untreated banks were on parallel growth paths and that the postwar breakup did not have persistent effects. After the 1952 reform, the loan growth of the treated banks slowed relative to that of the untreated group, and it continued to do so after the 1957 reform. The growth pattern of deposits mirrored that of lending (fig. 2*B*).[19] The results suggest that the treated banks did not expand their loan supply and deposit base after consolidating, relative to other banks. Moreover, one key aim of the treated banks in the postwar period was to gain market share.[20] The results in figure 2 imply that the consolidations did not facilitate this aim.

[18] There were 131 commercial banks in 1951 (excluding single-branch private banks; Deutsche Bundesbank 1976). Most were active within only one state, although a few had branches in several states. Their market share in lending to German nonbanks was 14% in 1951, while the treated banks' market share was 21%.
[19] The pattern remains similar until 1970, suggesting that the relative decline of the treated banks cannot be explained by temporary adjustment costs (fig. A.I; figs. A.I–A.III are available online). The pattern is also similar when all other German banks are in the untreated group, including the cooperative and public banks (fig. A.II).
[20] The banks openly expressed their desire to expand market share both before and after 1952 (Ahrens 2007, 227; Dresdner Bank report 1958). For example, the directorate of the Rhein-Main-Bank (located in Hesse) encouraged its staff to "poach customers" from other banks, by carefully "working through incoming mail to scout for new business opportunities (e.g., by reading between the lines)" (circular to all branches from August 1948, in Ahrens 2007, 227). The Nazis had instituted a ceiling on deposit rates in 1936 that was officially still in place after the war. There was only a nonbinding "recommendation" for the lending rate (Herlan 1952, 656). In practice, banks in postwar Germany largely ignored rate regulation and recommendations. They paid competitive deposit rates and outbid each other on fees and other costs of financial services (*Die Zeit* 1954; Wolf 1998, 70).
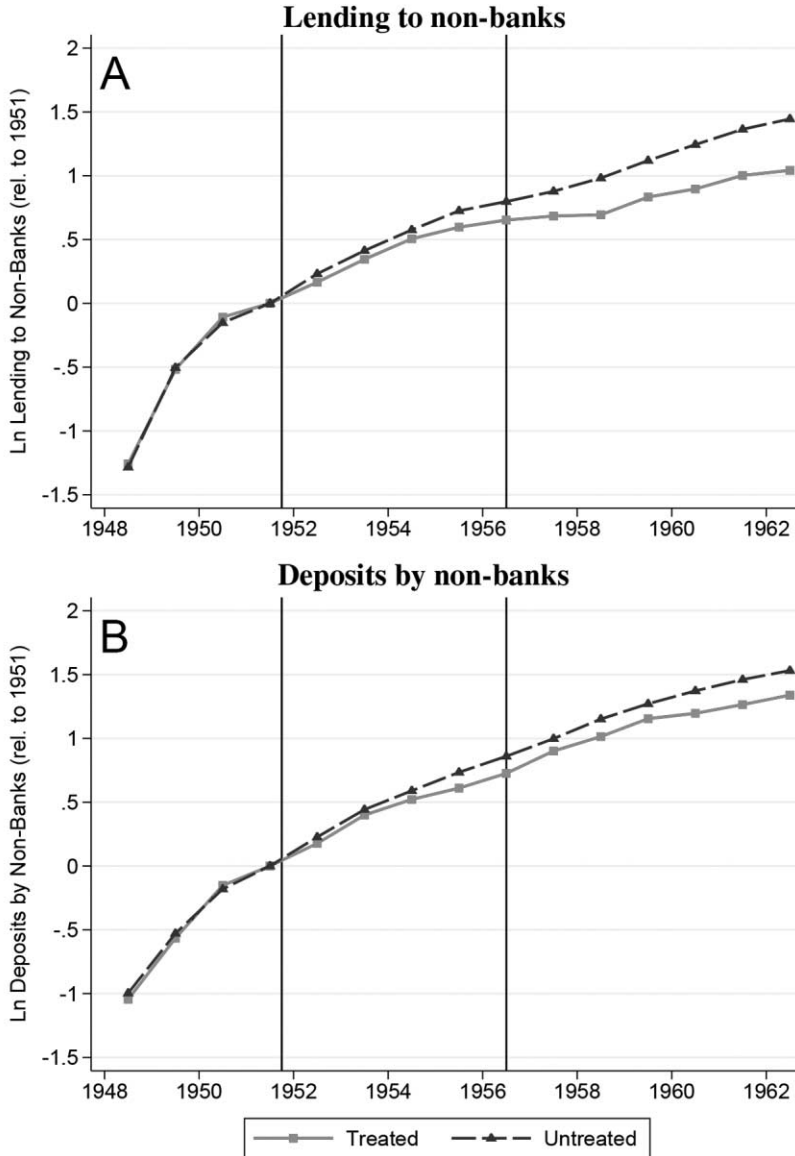
Fig. 2.—Lending to nonbanks and deposits from nonbanks. The data are in real terms, for December of the given year, and are provided by the Deutsche Bundesbank. The treated group includes banks affected by the breakup and subsequent reforms. The untreated group includes all untreated commercial banks. The 1952 reform lifted the state-level restrictions and introduced zonal restrictions. The 1957 reform removed all restrictions. A color version of this figure is available online.

B.   The Growth of Nonstock Firms with a Treated Relationship Bank

I turn to investigating how firms borrowing from the treated banks were affected by the consolidations. Since the data for nonstock firms and stock corporations are from separate sources and for different years, I analyze the firm types separately. To begin, I examine the employment growth of nonstock firms from 1951 to 1956. I regress average annual employment growth from 1951 to 1956 on an indicator for whether a bank treated in 1952 was among the firm's relationship banks in 1951.[21] If the 1952 reform allowed bank borrowers to grow faster, the coefficient on the indicator should be positive. However, the estimate is statistically not different from zero (col. 1 of table 1). It implies that the growth of firms with a treated relationship bank was 0.1 percentage points lower per year. The 95% confidence interval ranges from −0.8 to 0.6 percentage points.[22]

The coefficient hardly changes when I add control variables (col. 2). Since shocks to certain industries might have differed across regions, I include fixed effects for industries interacted with fixed effects for the northern, western, and southern regions of Germany.[23] I also control for ln firm age and for four bins of firm employment in 1951 (0–49, 50–249, 250–999, and 1,000+ employees), all interacted with the three zonal fixed effects.

A remaining concern is that firms with a treated relationship bank would have been on different growth paths during the 1950s had it not been for the reforms, for example, because they were persistently affected by the banking breakup after World War II. To address this concern, I show that firms with a treated relationship bank grew in parallel to other firms from 1949 to 1951 (table A.III). Moreover, I restrict the analysis to the "focused" sample. The main feature of the focused sample is that it contains only firms that had a treated relationship bank that was broken up after the war. Some firms in the focused sample were treated in 1952 and some were not, because of the exclusion of the western zone from

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[21]   To be clear, the outcome in table 1 is the symmetric growth rate of bank debt from 1951 to 1956 divided by 5 (the number of years between 1951 and 1956). This transformation makes it easier to compare the point estimates to later results, which use data for periods of different lengths.

[22]   Standard errors in the baseline are clustered at the level of the firm's county, but they are similar when clustering is by state-level banks and the cluster correction of Young (2016) is used (cols. 4, 5 of table A.IV).

[23]   The industries are agriculture and mining, food and drink, clothes and textiles, wooden products, chemicals and pharmaceuticals, rubber and glass, metals manufacturing, electric and electronics, production of machinery, repair and research, energy supply, water and waste management, construction and real estate, trade and retail, transport, gastronomy and art, information and communication, and finance and insurance. The regions are equivalent to the three banking zones of the 1952–57 period.

TABLE 1
EFFECTS ON THE GROWTH OF NONSTOCK FIRMS: EMPLOYMENT GROWTH, 1951–56

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rel. bank treated in 1952	−.001	−.001	.001	−.001	.003	.002	−.001	.000
	(.004)	(.005)	(.006)	(.006)	(.006)	(.010)	(.006)	(.006)
Observations	1,521	1,472	353	342	473	464	687	664
R^2	.000	.063	.000	.110	.000	.168	.000	.064
Industry FE × zone FE	No	Yes	No	No	No	Yes	No	Yes
ln age × zone FE	No	Yes	No	No	No	Yes	No	Yes
Size bin FE × zone FE	No	Yes	No	No	No	Yes	No	Yes
Industry FE	No	No	No	Yes	No	No	No	No
ln age	No	No	No	Yes	No	No	No	No
Size bin FE	No	No	No	Yes	No	No	No	No
Sample	Full		Focused		Exporters		Strict states	

NOTE.—Estimates of the effect of having a relationship bank (rel. 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. "Rel. bank treated in 1952" is an indicator for whether the firm had a bank treated in 1952 among its relationship banks in 1951. The controls include fixed effects (FE) for four bins of firm employment in 1951 (1–49, 50–249, 250–999, and 1,000+ employees), since firm assets are unavailable in the data for nonstock firms. The other controls are explained in table 2. Standard errors (in parentheses) are clustered at the level of the firm's county. The sample in cols. 1 and 2 includes all nonstock firms with available employment data. The "focused" sample in cols. 3 and 4 includes only firms that fulfill all of the following criteria: had a relationship bank that was treated in either 1952 or 1957, is in the western zone (North Rhine–Westphalia) or in states bordering the western zone, is outside the Ruhr region, and is not in the coal and steel industry. The sample in cols. 5 and 6 includes only firms that exported any of their products. The sample in cols. 7 and 8 includes only firms located in the southern states of Baden, Bavaria, Hesse, Rhineland-Palatine, Württemberg-Baden, and Württemberg-Hohenzollern, where the breakup was enforced most strictly. The samples include only nonstock corporations.

the 1952 reform.[24] The coefficients in the focused sample remain close to the baseline (cols. 3, 4 of table 1). Hence, differences between firms with a treated relationship bank and other firms do not explain the results.

Exporters are more likely to be affected by banking shocks because they have higher default risk and working-capital requirements (Amiti and Weinstein 2011). The stable coefficient for exporters suggests that even firms with higher bank dependence did not benefit from the consolidations (cols. 5, 6). The 1952 reform had the largest effects on bank operations in a few southern states that had enforced the rules of the breakup relatively strictly: Baden, Bavaria, Hesse, Rhineland-Palatine, Württemberg-Baden, and Württemberg-Hohenzollern. The southern state governments were in favor of state-level banks (Horstmann 1991, 231, 250) and considered the 1952 reform to be problematic (Wolf 1993). But even in the strict states, the effect was small and insignificant (cols. 7, 8). In additional tests, I show that the results are robust to different treatment measures and sample cuts (app. D).

[24] For the full set of restrictions in the focused sample, see sec. IV. The focused sample identifies the effect by comparing across zones, so I do not control for zonal fixed effects in cols. 3 and 4 of table 1.

*C. The Growth of Stock Corporations
with a Treated Relationship Bank*

For stock corporations, I analyze the growth of employment, bank debt, the ratio of bank debt to total assets, and revenue per worker from 1951 to 1960. The regressor of interest is an indicator for whether a bank treated in 1952 or 1957 was among the firm's relationship banks in 1951. The effects on stock corporations' employment are small and insignificant, without and with control variables (cols. 1, 2 in panel A of table 2).[25] The literature has shown that firms with high bank debt and high leverage react more strongly to shocks to their relationship banks (Bentolila et al. 2018; Huber 2018). I find no evidence that firms with high bank debt or low stock capital (relative to assets) in 1951 grew faster if they had a treated relationship bank (cols. 3, 4). This confirms that even bank-dependent firms did not benefit from the bank consolidations.

Moreover, there is no evidence of an effect on bank debt (panel B of table 2), the ratio of bank debt to assets (panel C), or revenue per worker (panel D) from 1951 to 1960. The small effect on bank debt confirms that the treated banks did not expand loan supply to their relationship borrowers. By using the ratio of bank debt to assets as an outcome, I implicitly control for changes in firms' total demand for funding in a way that is conceptually similar to using firm fixed effects. If firms with a treated relationship bank had access to cheaper bank debt, they should have funded themselves with more bank debt relative to other funding sources, raising the ratio. Finally, revenue per worker is a measure of labor productivity. The small effect suggests that firms did not make their workers more productive after their banks consolidated.

*D. The Growth of Opaque Firms
with a Treated Relationship Bank*

Big banks might be worse at dealing with opaque firms because it requires collecting and processing soft information. To identify opaque firms, I follow previous work and restrict the sample to firms that in 1951 had fewer than 50 employees, were younger than 10 years old, or were unlikely to have easily collateralizable assets (firms where the share of fixed tangible assets in their industry was in the bottom 10%).[26]

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[25] Since employment is not available for all stock corporations, I use ln assets to control for firm size in table 2. Results are similar when controlling for employment as in table 1.

[26] Firms with fewer than 50 employees face more idiosyncratic risk, have lower savings, and are hard for lenders to assess (Gertler and Gilchrist 1994). Firms under the age of 10 are less likely to have an established reputation and paper trail to prove creditworthiness (Rajan and Zingales 1998; Hurst and Pugsley 2011). It is difficult to unambiguously secure loans to firms that have relatively few collateralizable assets, so lenders are more likely to rely on soft information when dealing with these firms (Braun 2005).

TABLE 2
EFFECTS ON THE GROWTH OF STOCK CORPORATIONS

	(1)	(2)	(3)	(4)
	A. Employment Growth, 1951–60			
Relationship bank treated in 1952/57	.001	−.001	−.007	−.004
	(.003)	(.004)	(.010)	(.006)
Observations	821	685	225	356
R^2	.000	.112	.251	.177
	B. Bank Debt Growth, 1951–60			
Relationship bank treated in 1952/57	−.001	.006	.000	.019
	(.013)	(.017)	(.021)	(.026)
Observations	421	421	240	235
R^2	.000	.152	.248	.239
	C. Δ(Bank Debt/Assets), 1951–60			
Relationship bank treated in 1952/57	.001	.002	.004	.004
	(.002)	(.002)	(.003)	(.003)
Observations	421	421	240	235
R^2	.002	.125	.185	.247
	D. Revenue per Worker Growth, 1951–60			
Relationship bank treated in 1952/57	.004	−.000	.007	−.000
	(.007)	(.007)	(.016)	(.010)
Observations	345	293	86	165
R^2	.002	.303	.516	.369
	Specifications			
Industry FE × zone FE	No	Yes	Yes	Yes
ln age × zone FE	No	Yes	Yes	Yes
ln assets × zone FE	No	Yes	Yes	Yes
Sample	Full	Full	High bank debt	High leverage

NOTE.—Estimates of the effect of having a treated relationship bank on the growth of stock corporations. Growth in panels A, B, 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 Δ(Bank debt/Assets) is the difference in the ratio of bank debt to assets between 1951 and 1960, divided by 9. "Relationship bank treated in 1952/57" is an indicator for whether the firm had a bank treated in 1952 or 1957 among its relationship banks in 1951. The controls include 18 industry fixed effects (FE), the natural logarithm of firm age, and the natural logarithm of firm assets in 1951. All controls are fully interacted with fixed effects for the northern, western, and southern banking zones that were in existence from 1952 to 1957. Column 3 restricts the sample to stock corporations with above-median ratio of bank debt to assets in 1951. Column 4 includes only stock corporations with a below-median ratio of stock capital to total assets in the sample. Standard errors (in parentheses) are clustered at the level of the firm's county. The samples include only stock corporations.

Opaque stock corporations with a treated relationship bank reduced the ratio of bank debt to assets by 1.4 percentage points from 1951 to 1960, relative to other opaque stock corporations (significant at 5%; col. 1 of table 3). Their ratio of stock capital to assets increased by 0.6 percentage points, although the effect is imprecisely estimated (col. 2). These findings

TABLE 3
Effects on the Growth of Opaque Firms

	1951–60				1951–56	
	Δ(Bank Debt/ Assets)	Δ(Capital/ Assets)	Employment Growth	Δ(Bank Debt/ Assets)	Employment Growth	
	(1)	(2)	(3)	(4)	(5)	(6)
Rel. bank treated in 1952/57	−.014 (.005)	.006 (.004)	.000 (.015)			
0 < fraction rel. banks treated in 1952/57 ≤ .5				−.013 (.005)		
.5 < fraction rel. banks treated in 1952/57 ≤ 1				−.018 (.006)		
0 < fraction rel. banks treated in 1952 ≤ .5					−.016 (.011)	−.030 (.022)
.5 < fraction rel. banks treated in 1952 ≤ 1					−.029 (.015)	−.037 (.020)
Observations	74	74	160	74	295	65
R^2	.561	.775	.341	.567	.229	.366
Controls × zone FE	Yes	Yes	Yes	Yes	Yes	No
Controls	No	No	No	No	No	Yes
Firm type	Stock	Stock	Stock	Stock	Nonstock	Nonstock
Sample	Opaque	Opaque	Opaque	Opaque	Opaque	Focused and Opaque

Note.—The outcomes, regressors, and controls for cols. 1–4 are explained in table 2 and those for cols. 5 and 6 in table 1. Standard errors (in parentheses) are clustered at the level of the firm's county. The sample in every column includes only opaque firms. A firm is opaque if in 1951 it had fewer than 50 employees, was younger than 10 years old, or was in the bottom 10% of industry asset tangibility (ratio of fixed tangible to total assets). Rel. bank = relationship bank; FE = fixed effects.

suggest that opaque stock corporations suffered lower bank loan supply but were able to raise stock capital financing in response. There was no effect on stock corporations' employment (col. 3). If treated banks caused the decline in bank loan supply, firms with few alternative banks should have suffered the largest decrease in bank debt. In line with this view, I find a significant effect on the ratio of bank debt to assets only if more than half of a firm's relationship banks were treated (col. 4).

The employment of nonstock firms is more vulnerable to banking shocks because they cannot finance themselves by issuing stock capital. I find that the employment growth of opaque nonstock firms was 2.9 percentage points lower when more than half of relationship banks were treated

(significant at 10%; col. 5), relative to that of opaque nonstock firms without a treated relationship bank. The effect remains of similar magnitude and significant when I use the focused sample (col. 6). In summary, the results suggest that the treated banks were worse at dealing with opaque borrowers after the consolidations.[27]

E. The Growth of Newly Added Relationship Borrowers of Treated Banks

I have so far focused on firms that had a treated relationship bank in 1951. Next, I analyze the growth of firms that were newly added as relationship borrowers by the treated banks after 1951. The treated banks might have provided better financial services to new borrowers and thereby raised their growth. I regress firm employment growth on indicators for whether the firm added a treated bank as relationship bank between 1951 and 1958, in the case of nonstock firms, or between 1951 and 1960, in the case of stock corporations.

   Firms that added new relationship banks likely had higher loan demand than other firms. To ensure that this does not bias the results, I begin by restricting the sample to firms that increased the number of their relationship banks after 1951. The results suggest that nonstock firms (cols. 1, 2 of table 4) and stock corporations (cols. 4, 5) that newly added a treated relationship bank did not grow faster than other firms.

   As an additional check, I use all firms in the sample and include indicators for firms that added a treated bank after 1951, dropped a treated bank after 1951, or never had a treated bank (in or after 1951). The omitted category includes firms that had a treated bank both at the beginning and at the end of the sample period. The three coefficients are small and insignificant (cols. 3, 6).[28] There is also no significant difference between the growth of firms that added a treated bank and that of firms that never had a treated bank. These results confirm that the treated banks did not raise the growth of newly added borrowers, relative to firms that already had a treated bank in 1951 and relative to firms that never had a treated bank.

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[27] In contrast, the effects on nonopaque firms were insignificant and statistically different from the effects on opaque firms (table A.V). Moreover, opaque firms were less likely to add a treated bank as a new relationship bank between 1952 and 1970 (table A.VI). Opaque firms that already had a treated relationship bank in 1951 were not less likely to keep the treated bank, probably because banking relationships in postwar Germany rarely ended (table A.VII).

[28] The coefficients on firms that added a treated bank are slightly larger in cols. 3 and 6 of table 4, compared to the other columns. This is consistent with a small upward bias stemming from endogenously higher loan demand.

TABLE 4
EFFECTS ON THE GROWTH OF NEWLY ADDED RELATIONSHIP BORROWERS

	EMPLOYMENT GROWTH, 1951–56			EMPLOYMENT GROWTH, 1951–60		
	(1)	(2)	(3)	(4)	(5)	(6)
Added a bank treated in 1952 as rel. bank	.003 (.010)	−.001 (.011)	.011 (.008)			
Dropped all banks treated in 1952 as rel. banks			−.012 (.010)			
Never had a bank treated in 1952 as rel. bank			−.002 (.006)			
Added a bank treated in 1952/57 as rel. bank				.001 (.007)	−.001 (.008)	.001 (.007)
Dropped all banks treated in 1952/57 as rel. banks						−.005 (.008)
Never had a bank treated in 1952/57 as rel. bank						−.001 (.005)
Observations	320	308	1,472	370	308	681
R^2	.000	.231	.065	.000	.227	.127
Controls × zone FE	No	Yes	Yes	No	Yes	Yes
Firm type	Nonstock	Nonstock	Nonstock	Stock	Stock	Stock
Sample	Firms increased no. of rel. banks		Full	Firms increased no. of rel. banks		Full

NOTE.—"Added a bank treated in 1952/57 as rel. bank" is an indicator for whether a bank treated in either 1952 or 1957 was among the firm's relationship banks in 1958 (for nonstock firms) or 1960 (for stock firms), but no such bank was among the firm's relationship banks in 1951. "Dropped all banks treated in 1952/57 as rel. banks" is an indicator for whether such a bank was among the firm's relationship banks in 1951 but no such bank was among the firm's relationship banks in 1958/60. "Never had a bank treated in 1952/57 as rel. bank" is an indicator for whether such a bank was among the firm's relationship banks neither in 1951 nor in 1958/60. The remaining regressors are defined analogously, but with reference to a bank treated in 1952. The outcome and controls are explained in table 1 (for cols. 1–3) and table 2 (for cols. 4–6). The samples in cols. 1, 2, 4, and 5 include only firms that increased the number of their relationship banks between 1951 and 1958–60. Standard errors (in parentheses) are clustered at the level of the firm's county.

F. The Growth of Municipalities with Treated-Bank Branches

The final set of main results studies the effect of the reforms at a higher level of aggregation, on municipal employment. The municipality-level analysis includes potential channels of the reforms that the firm-level analysis cannot capture, such as local general equilibrium effects, the reallocation of lending across borrowers, changes in the local banking market, and effects on households. The data sources are described in appendix E.

TABLE 5
EFFECTS ON MUNICIPAL EMPLOYMENT GROWTH

	EMPLOYMENT GROWTH					
	1951–60 (1)	1951–60 (2)	1951–60 (3)	1951–56 (4)	1947–51 (5)	1951–60 (6)
Treated bank branch	−.013 (.005)	−.013 (.005)			.019 (.017)	−.014 (.004)
Fraction of firms with a treated rel. bank			−.014 (.008)			
Treated bank branch outside western zone				−.012 (.007)		
Treated bank branch in western zone				−.004 (.009)		
Observations	79	79	74	91	83	66
R^2	.340	.350	.303	.202	.441	.668
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

NOTE.—Estimates of the effect of exposure to treated banks on municipal employment growth. The outcomes are average annual symmetric growth rates of employment in the given period. Treated bank branches were treated in 1952, in 1957, or in both years. Treated bank branches not in the western zone (North Rhine–Westphalia) were treated in 1952 and 1957, while treated bank branches in the western zone were treated only in 1957. The fraction of firms with a treated relationship bank (rel. bank) is calculated from the Hoppenstedt firm data for 1951. Size bins are five quantiles of total employees in the municipality. The detailed controls include the full interaction of zonal fixed effects (FE) with the following variables: the growth rate from 1947 to 1951, five quantiles for the total number of employees, the share of employment in manufacturing, the share of employment in the primary sector, the share of employment in the public sector, and the employment share of wartime displaced. Standard errors (in parentheses) are robust.

I regress annual employment growth in the municipality on an indicator for whether the municipality had a treated bank branch in 1952. Employment growth of municipalities with a treated bank branch was 1.3 percentage points lower between 1951 and 1960 (significant at 5%; col. 1 of table 5). The coefficient remains stable when controlling for federal states, five quantiles of total employment, and the Ruhr area (col. 2). As an alternative treatment, I use the fraction of firms with a treated relationship bank in the municipality, calculated from the firm data (col. 3). The point estimate implies that employment growth was 1.4 percentage points lower in a municipality where every firm had a treated relationship bank (significant at 10%).[29]

[29] To compare the municipality effects to the firm-level results, I calculate the effect on municipal employment growth that is implied by the firm-level estimates. For nonopaque firms, there was no effect on employment growth. For the average opaque firm, employment growth was 1.6 percentage points lower (col. 5 of table 3). Roughly 67% of employees in the population worked in opaque firms. Thus, in a municipality where every firm had a treated

Between 1951 and 1956, only banks outside the western zone were treated by the 1952 reform. Employment growth from 1951 to 1956 was 1.2 percentage points lower for municipalities outside the western zone with a treated branch (significant at 10%; col. 4). The coefficient on municipalities in the western zone is less than one-third of the magnitude and statistically not different from zero. However, it is also not statistically different from the effect on municipalities outside the western zone. There were no pretrends, as municipality growth from 1947 to 1951 was not associated with having a treated branch (col. 5). The baseline result is robust to controlling for a more extensive set of controls (col. 6).[30]

The small sample sizes suggest that caution is warranted in interpreting the municipality-level results. Nonetheless, the evidence is consistent with the firm- and bank-level results, providing no evidence of a positive employment effect from the bank consolidations.

VII. Investigating Potential Mechanisms

Changes in bank size could, in principle, affect the performance of banks and their borrowers through several theoretical mechanisms. In this section, I present suggestive evidence on which mechanisms played an important role after the treated banks consolidated.

A. Banking as Natural Monopoly: Bank Efficiency and Costs

Several theories imply that banking is a natural monopoly (see reviews in Dowd 1992, Bhattacharya and Thakor 1993, and Freixas and Rochet 2008). One proposed explanation is that fixed costs are a large part of banks' total costs (Humphrey 1990; Pulley and Humphrey 1993; Hughes and Mester 2013; Kovner, Vickery, and Zhou 2014). In line with this view, managers of treated banks expressed concerns before the reforms about high fixed costs from employing separate legal departments, separate payment systems, and specialized credit experts for each industry (*Der Spiegel* 1951; Horstmann 1991). Other explanations for banking as a natural monopoly include diversification, the ability to issue large loans, and synergies between lending and underwriting (discussed in detail below).

All these theories predict that the efficiency of banks should improve as banks get bigger. I test the theories by estimating profit and cost efficiency

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relationship bank, the firm-level estimates imply that employment grew by $1.6 \times 0.67 = 1.1$ percentage points less. This amounts to $1.1/1.4 = 79\%$ of the municipality-level effect in col. 3 of table 5. The remaining 21% may be due to the effects on households or local spillover effects that harm firm growth (Huber 2018).

[30] I consider the role of local banking competition in app. F. Further robustness checks are in app. G.

ratios for banks, using the methodology of Berger and Mester (1997). The ratios measure how efficiently a bank generates profits or minimizes costs, conditional on outputs, inputs, and input prices. The closer the efficiency ratio is to one, the closer a bank is to the best-performing bank in the sample (for details, see app. H). I also calculate noninterest costs scaled by assets and revenue. Noninterest costs include fixed costs and other operational expenses. If banking is a natural monopoly, scaled noninterest costs should fall as banks become bigger.

I present financial statistics for the treated banking groups and nine comparable, untreated banks in table 6.[31] Average assets before the 1952 reform (col. 1), profit and cost efficiency ratios in 1952 (cols. 4, 6), and scaled noninterest costs in 1952 (cols. 8, 10) were similar for treated and untreated banks. This indicates that the treated and untreated banks were comparable in terms of size, efficiency, and costs in 1952.

Between 1952 and 1960, average lending and profits of the treated banks grew more slowly than those of the untreated banks (cols. 2, 3). Average profit and cost efficiency evolved roughly in parallel for treated and untreated banks (cols. 4–7). Scaled noninterest costs also improved in parallel (cols. 8–11).[32] Taken together, the results show that the treated banks did not have profit and cost functions of a natural monopoly and that they did not become more profitable and efficient after consolidating.

B. Diversification

The postwar reforms led to exogenous increases in the number of borrowers served by one treated bank. This increased the diversification of treated banks across states. The canonical theory of bank diversification is by Diamond (1984), and similar predictions appear in Boyd and Prescott (1986), Williamson (1986), and Krasa and Villamil (1992a). Banks diversify by raising the number of borrowers, as long as the growth of borrowers is not perfectly correlated. Diversified banks are less likely to experience simultaneous defaults of a large proportion of borrowers. As a result, diversified banks are less likely to hit capital or liquidity thresholds and less likely to default. Diversified banks also attract more deposits because they are safer (Diamond 2020).[33]

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[31] Apart from the treated banks, 16 universal, commercial banks with a branch network existed in 1949 (Hofmann 1949). I was able to locate the 1952 and 1960 annual reports of nine of these untreated banks and of the treated banks. The pre-1952 reports of many treated and untreated banks have not been preserved. The treated banks consolidated in September 1952, so the effect of the 1952 reform on the figures from December 1952 is likely small.

[32] In unreported results, I find that differences between treated and untreated banks in the growth of all the financial statistics were statistically insignificant.

[33] German deposit insurance started only in 1976, so this applies to both bank and nonbank depositors.

TABLE 6
FINANCIAL STATISTICS BY BANKING GROUP

BANKING GROUP	ASSETS PER BANK before 1952 Reform (M. DM)	LENDING GROWTH 1952–60	PROFIT GROWTH 1952–60	PROFIT EFFICIENCY RATIO		COST EFFICIENCY RATIO		NONINTEREST COST/ASSETS (%)		NONINTEREST COST/REVENUE (%)	
				1952	1960	1952	1960	1952	1960	1952	1960
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Deutsche Bank (treated)	448.8	.70	1.46	.31	.88	.82	.92	2.89	2.63	62.82	55.53
Dresdner Bank (treated)	297.6	.56	1.38	.19	.93	1.00	1.00	2.64	2.54	74.77	54.85
Commerzbank (treated)	212.8	1.09	1.62	.23	.89	.86	.92	2.85	2.17	72.47	57.15
Mean of 9 untreated banks	330.2	.93	1.64	.29	.91	.97	.98	3.36	2.11	65.24	50.82
Mean difference: treated − 9 untreated	−10.5	−.15	−.16	−.04	−.01	−.08	−.04	−.56	.34	4.79	5.03
	(152.4)	(.18)	(.15)	(.11)	(.02)	(.05)	(.03)	(.43)	(.26)	(4.45)	(6.01)

NOTE.—The data are from banks' annual reports. Column 1 lists the total assets in 1952 (in millions of Deutsche Marks [DM]) of all banks in the banking group divided by the number of individual banks in the banking group before the 1952 reform. Growth in cols. 2 and 3 is the symmetric growth rate from 1952 to 1960. Profit efficiency is estimated with a translog specification and the stochastic frontier approach, assuming a normally distributed error term and a half-normally distributed inefficiency term. The outcome in the translog specification is profits, and the regressors include input prices (prices of deposits and labor), outputs (loans and securities), and fixed inputs and outputs (financial capital and physical capital). Cost efficiency is estimated with a specification similar to that for profit efficiency, but with variable profits (plus minimum profits in the sample plus 1) as outcome. The higher the profit and cost efficiency ratios in cols. 4–7, the closer a given bank was to the best-practices bank in the sample. The cost ratios in cols. 8–11 are expressed as the percentage of the banking group's aggregated assets (8, 9) or aggregated revenue (10, 11). The nine untreated banks include commercial banks with a branch network in 1949: Badische Bank, Bayerische Hypotheken- und Wechsel-Bank, Bayerische Vereinsbank, Handels- und Gewerbebank Heilbronn, Handelsbank Lübeck, Norddeutsche Kreditbank, Oldenburgische Landesbank, Vereinsbank Hamburg, and Württembergische Bank. The reported mean difference is the mean of the three treated banking groups minus the mean of the nine untreated banks. Standard errors (in parentheses) are robust.

In canonical models, both small banks (with few preexisting borrowers) and large banks benefit from adding more borrowers. Consistent with this view, postwar academics and bank managers argued that the postwar reforms would "allow greater diversification of banking risks" and improve banking services to the real economy (Lanner 1951, 179; Holtfrerich 1995, 500, 505).

In contrast, some theories argue that increasing diversification might help only relatively small banks. There are two reasons. First, bank managers might struggle to optimally coordinate the monitoring of many borrowers within their organization. This could mean that large banks experience more disruptions to lending and deposit taking (Cerasi and Daltung 2000). Second, depositors might find it costly to monitor diversified banks. This could mean that large banks receive fewer deposits (Krasa and Villamil 1992b). In sum, theory is ambiguous about how diversification affects bank stability and deposit inflows.[34]

I find no evidence that fluctuations in assets, lending, or deposits of the treated banking groups were lower after 1952 (app. I). This suggests that diversification did not meaningfully stabilize operations of the treated banks. Moreover, the treated banks were not able to raise more deposits after 1952 (fig. 2), even though this was their explicit aim (Ahrens 2007). The findings suggest that diversification did not lead to first-order improvements in the performance of the treated banks. The negative effects of further diversification seem to have at least offset any positive effects.[35]

Ultimately, the effects of diversification depend on the comovement of new and existing borrowers, relative to the magnitude of coordination and monitoring frictions.[36] There was meaningful variation in expected and realized state growth in postwar Germany, including a recession in 1966–67 (Lanner 1951, 177; app. I). This suggests that there was at least some potential to diversify state-level shocks through a national bank. Of course, the effects of diversification might differ in other settings if comovement or frictions are significantly lower.

[34] Ramakrishnan and Thakor (1984) and Millon and Thakor (1985) present theories for nondepository financial institutions with similar conclusions.

[35] Evidence from the United States suggests that diversification reduced bank market valuations (Goetz, Laeven, and Levine 2013) and increased organizational complexity (Correa and Goldberg 2020), even though it lowered stock volatility (Goetz, Laeven, and Levine 2016) and funding costs (Levine, Lin, and Xie 2019). This suggests that diversification does not always benefit bank performance, in line with my findings.

[36] To be precise, deposit inflows depend on depositors' expectations about future borrower comovement and frictions after they deposit. In contrast, bank fluctuations depend on realized shocks to borrowers and realized frictions.

C. Avoiding Syndicates for Large Loans

Before 1952, the treated banks had to form syndicates with other banks to fund large loans (Wolf 1994). After the reforms, a bigger capital base allowed them to fund large loans on their own.

Theory is ambiguous about how the independent funding of large loans affects borrowers. On the one hand, moral hazard problems are inherent to syndicates, so loan supply might rise once banks can fund loans independently (Holmstrom and Tirole 1997). On the other hand, reputable banks overcome moral hazard problems (as in Diamond 1991), and syndicates offer opportunities for risk sharing (Wilson 1968), so loan supply might not be affected.

I find no evidence that the growth of large borrowers (nonopaque firms or firms above 500 and 1,000 employees) benefited from the consolidations (cols. 1–6 of table A.VIII). This suggests that the treated banks did not expand loan supply to large borrowers once banks could fund large loans independently. A likely reason is that moral hazard problems for the treated banks were small, consistent with evidence on syndication by reputable banks in Sufi (2007).

D. Synergies in Cross Selling

The treated banks and most commercial banks operated as universal banks before and after the reforms. This means that banks simultaneously lent to firms and underwrote corporate securities (cross selling). In many models, the availability and effects of cross selling do not explicitly depend on bank size (Boot and Thakor 1997; Puri 1999; Rajan 2002). However, in some models, synergies from cross selling are more beneficial when borrowers are large (Kanatas and Qi 2003) or when banks' decision-making structure is centralized (Lóránth and Morrison 2012). The treated banks were more likely to work with large borrowers after the reforms (table A.VI), and their decision-making structure became more centralized (Horstmann 1991, 170). As a result, there could have been a positive effect of size on borrowers that relied on cross selling. I find that firms with both stock capital and bank debt in 1951 did not grow faster after the consolidations (col. 7 of table A.VIII). This finding is consistent with models that suggest that cross-selling synergies do not change with bank size.

E. Screening Technologies

Bigger banks might use more sophisticated technologies to screen borrowers. For instance, a larger database of customer records might improve statistical analyses, and adopting advanced technologies might entail large fixed costs. Improved screening changes bank and borrower outcomes through various mechanisms, which I examine in turn below.

First, improved screening allows banks to issue new loans primarily to borrowers that do not default. This, in turn, reduces banks' loan losses and allows banks to offer cheaper interest rates. As a result of better targeting and cheaper rates, borrowers should be less likely to face liquidation. However, I find that firms with a treated relationship bank in 1951 were not more likely to avoid liquidation than other firms (table A.IX). The treated banks also did not reduce their loss reserves by more than untreated banks.[37] These results suggest that the treated banks did not improve their screening of new borrowers.

Second, better screening helps banks to avoid relationships with underperforming borrowers. If this mechanism was important, firms should have been more likely to avoid liquidation after being newly added as relationship borrowers by a treated bank, relative to firms not added. Moreover, firms should have been more likely to face liquidation after being dropped by a treated bank, relative to continuing borrowers. I find that the liquidation rates of newly added, dropped, and continuing borrowers were not different from each other (app. K). They were also not different from those of firms that never had a relationship with a treated bank. These results imply that the treated banks did not avoid borrowers with high liquidation rates.

Third, improved screening helps banks to identify firms with high future growth or high productivity. Such high-quality borrowers are more likely to generate positive returns for banks, so banks with better screening technologies should establish more relationships with them. However, labor productivity growth, employment growth, and the level of labor productivity were similar for newly added, dropped, and continuing borrowers of treated banks (app. K). This suggests that the treated banks did not become better at identifying high-quality borrowers.

F. Internal Capital Markets

After consolidating, the treated banks were able to shift capital across states through their internal capital markets, rather than relying on interbank markets and central bank clearing (Adler 1949; Wandel 1980; Wolf 1994). But it is not clear whether access to internal capital markets actually changed capital flows. In 1951, Deutsche Bank lawyer Fritz Kempner claimed that internal capital markets would alter cross-state capital flows because internal flows were significantly cheaper (Holtfrerich 1995, 505). On the contrary, Horstmann (1991) argues that German interbank markets were efficient and that capital flows did not change after 1952.

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[37] From 1952 to 1960, the treated banks increased reserves per unit of lending by 2.8 pennies, on average, and the untreated banks by 2.5 pennies, on average.

According to theory in Morgan, Rime, and Strahan (2004), changes in capital flows affect the volatility of firm and municipality growth. In several analyses, I show that the volatility and correlation of growth across firms and municipalities did not change after the reforms (app. J). This suggests that capital flows were unaltered.

In an additional test, I compare German states with a persistent capital account surplus to states with a deficit (Pohl 1971, 40). If internal capital markets made cross-state capital transfers cheaper, more capital would have flown into surplus states and out of deficit states. As a result, firms with a treated relationship bank in surplus states would have experienced an increase in capital supply and grown more quickly, with the opposite effect in deficit states. However, I find no evidence that the effect of exposure to treated banks differed in deficit states (tables A.X, A.XI).

The results suggest that cross-state capital flows did not significantly change after the reforms. A likely reason is that interbank markets were well developed and an adequate substitute for internal capital markets before 1952.[38]

G.   *Capital Allocation*

Theory is ambiguous about whether large banks improve the allocation of capital across borrowers. On the positive side, managers of large banks control a larger stock of capital, relative to managers of state-level banks. This gives them authority to allocate a greater stock of capital to borrowers with the highest marginal product of capital (Stein 1997). In addition, local branch managers in large banks might have greater incentives to produce high-quality information about borrowers, as long as information can be unambiguously recorded and passed on to superiors (Stein 2002).

But there might also be a dark side to capital allocation in large organizations. Managers of large banks might receive incomplete information on borrowers (Williamson 1967), engage in rent seeking (Scharfstein and Stein 2000), or wage internal power battles (Rajan, Servaes, and Zingales 2000). Problems of asymmetric information and agency might generally be worse in large organizations (Stein 2003). As a result, bigger banks might allocate capital less efficiently.

Theory suggests that capital allocation is optimal when marginal products of capital are equalized across firms. Hence, better capital allocation

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[38] US banking deregulation made state fluctuations smaller and more alike, likely because of increased cross-state capital flows after consolidations (Morgan, Rime, and Strahan 2004; Demyanyk, Ostergaard, and Sørensen 2007; Landier, Sraer, and Thesmar 2017). A reason for the different finding could be that many US banks were very small before deregulation, often operating at the city level. The city-level US banks might have found it harder to use interbank markets than the state-level banks in postwar Germany. Gilje, Loutskina, and Strahan (2016) and Cortés and Strahan (2017) also argue that larger banks can access interbank markets more easily, which makes them less dependent on internal capital flows.

means that firms with high marginal product receive relatively more capital (Restuccia and Rogerson 2008; Hsieh and Klenow 2009). Better capital allocation raises the average growth of firms, because the growth increase of firms receiving capital is greater than the growth decrease of firms deprived of capital. If credit allocation improved after the reforms, average growth among firms borrowing from the treated banks should have increased. I find that borrowers of treated banks did not grow faster on average, which implies that capital allocation did not meaningfully improve (sec. VI).

At the municipality level, theory suggests that a better allocation of capital generates higher average total factor productivity. As long as municipal labor supply is not perfectly inelastic, the productivity gain should also raise municipal employment growth. Existing evidence suggests that labor supply in German municipalities is not inelastic (Decressin and Fatás 1995; Helm 2020; Braun and Weber 2021). The negative effect on municipal employment is therefore consistent with the view that capital allocation did not improve (table 5).

H.   Processing Soft Information

Opaque borrowers often rely on their banks to incorporate soft information in lending decisions, for example, by assessing the character of business owners. In large hierarchies, it is difficult to incentivize employees to generate high-quality soft information and difficult to transfer soft information to decision makers (Berger and Udell 2002; Stein 2002; Brickley, Linck, and Smith 2003). As a result, opaque borrowers of large banks might face lower loan supply.

Indeed, the decision-making procedure on loans likely changed in the treated banks. Before 1952, the state-level banks made decisions about loan applications independently in regionally specialized credit councils (Horstmann 1991, 170). After the reforms, a more centralized structure took over. The results on opaque firms suggest that opaque borrowers became credit constrained after the consolidations (table 3).

I.   Relationships to Risky Firms

I find that treated banks were more likely to add risky firms (with high volatility or high leverage) as new relationship borrowers between 1952 and 1970, relative to less risky firms (table 7).[39] What explains greater risk taking? Bigger banks might take more risks because it allows them to raise

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[39] Risky firms that already had a treated relationship bank in 1951 were not more likely to keep the treated bank, probably because banking relationships in postwar Germany rarely ended (table A.XII). Another way to assess risk taking would be to examine bank leverage. However, changes to accounting regulations in the postwar period make it difficult to construct a consistent series for bank capital (Horstmann 1991).

TABLE 7
New Banking Relationships with Risky Firms:
Fraction of Treated Relationship Banks in 1970

	(1)	(2)	(3)	(4)
Low volatility	−.059	−.058		
	(.035)	(.035)		
Low leverage ((Cap/Assets) ≥ .75)			−.087	−.094
			(.027)	(.054)
Medium leverage (.75 > (Cap/Assets) ≥ .25)			.027	.029
			(.033)	(.031)
Observations	266	263	164	148
R^2	.033	.279	.020	.545
Opaque firm FE	Yes	Yes	Yes	Yes
Controls × zone FE	No	Yes	No	Yes
Sample	Firms without a treated relationship bank in 1951			

Note.—The outcome is the ratio of the number of treated relationship banks divided by the total number of relationship banks in 1970. A firm has low volatility if the standard deviation of its employment growth from 1946 to 1951 is below the median. Cap/Assets is the ratio of stock capital to total assets. A firm is opaque if in 1951 it had fewer than 50 employees or was in the bottom 10% of industry asset tangibility (ratio of fixed tangible to total assets). The remaining controls are explained in table 1. Standard errors (in parentheses) are clustered at the level of the firm's county. FE = fixed effects.

profits (Demsetz and Strahan 1997). However, the treated banks did not become more profitable after the reforms (table 6).

An alternative explanation is that bigger banks face an improved risk-return trade-off (Hughes and Mester 2013). To assess this theory, I study the growth of newly added, risky relationship borrowers of the treated banks. These borrowers did not grow more quickly than other firms after 1952 (table A.XIII). If borrower growth is correlated with the bank's return to lending, these results suggest that the banks took more risk without gaining a higher return.

A remaining possible explanation is that banks took greater risks because they became "too big to fail" (Freixas 1999; Pais and Stork 2013; Dávila and Walther 2019). Results are consistent with this view. Moreover, anecdotal evidence suggests that the treated banks influenced policy in favor of the financial sector as a whole after the consolidations (*Der Spiegel* 1971). It is, however, hard to test this theory rigorously, given that there were no serious banking crises in the postwar period.

J.   Private Benefits to Bank Managers

If the treated banks did not become more profitable or efficient, why did the bank managers favor consolidating? Several theories argue that managers enjoy private benefits from running large firms (Stein 2003). I show that managers of the treated banks indeed benefited in two ways: through greater salaries and media presence.

TABLE 8
Number of Media Mentions of Treated Banks and Their Managers

	Phase 1: June 30, 1947–March 29, 1952 (1)	Phase 2: March 30, 1952–December 24, 1956 (2)	Phase 3: December 25, 1956–September 24, 1961 (3)
	A. *Der Spiegel* (German Weekly News Magazine)		
Name of a treated bank	15	46	121
Name of a treated bank manager	6	12	20
The word "Bank"	487	407	479
The word "Deutschland"	3,145	3,086	3,062
	B. *Financial Times* (British Daily Newspaper)		
Name of a treated bank	3	261	779
Name of a treated bank manager	2	36	143
The word "bank"	22,160	30,035	37,168
The word "Germany"	4,065	8,129	10,311

Note.—The number of times that the word in the leftmost column was mentioned in an article in the given period. The data are based on the author's calculations from the online archives of *Der Spiegel* and the *Financial Times*, accessed August 29, 2017.

Between 1952 and 1960, the average annual salary of executives at treated banks increased by 251%, compared to 102% at untreated banks. The difference is statistically significant at 5%. Existing work documents that firm size is correlated with executive pay (Edmans, Gabaix, and Jenter 2017). The finding here suggests that increases in firm size can causally affect executive pay, even without improvements in firm profitability or efficiency.

Moreover, the media coverage of treated banks and their executives increased strongly after the reforms. I calculate the number of times that the name of a treated bank or of a treated bank executive appeared in the German magazine *Der Spiegel* and the British *Financial Times*, relative to mentions of the word "bank" or "Germany" (table 8).[40] Figuratively speaking, the results suggest that one bank of size 10 receives more media mentions than 10 banks of size one combined. Managers might enjoy media presence as an end in itself. Media coverage might also have tangible benefits, as it is correlated with influence on consumer choices and political decisions (Enikolopov and Petrova 2015; Bursztyn and Cantoni 2016).

Taken together, the increases in salaries and media coverage could account for the desire of bank managers to run large firms. The findings are consistent with theories that suggest that managers benefit from building corporate "empires."

[40] 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.

VIII. Discussion of the Results

In this final section, I discuss what we can learn from the postwar consolidations about the effects of bigger banks.

A. Interpreting the Magnitude of the Firm-Level Estimates

Estimates from other studies of bank consolidations are large in absolute terms and lie outside the confidence intervals of my paper. For instance, consolidations of relationship banks raised the debt growth of German firms by 3.3 percentage points (Marsch, Schmieder, and Forster 2007) and lowered the bank debt growth of Italian firms by 7.9 percentage points per year (Bonaccorsi di Patti and Gobbi 2007).[41] This suggests that my quasi-experimental approach leads to meaningfully different conclusions relative to observational studies of bank consolidations. Reforms of the banking sector can, in principle, generate large gains in borrower employment, as examples from other countries reveal.[42] The German postwar reforms were a comparatively ineffective piece of banking policy in terms of raising the growth of borrowers.

To further gauge the magnitude of the effects, I estimate an elasticity of firm employment with respect to the size of the firm's relationship banks of -0.0009, with a 95% confidence interval from -0.0024 to 0.0005 (app. L). The elasticity presents the firm-level effect scaled by the initial shock to bank size.[43] This might be useful because some models imply that a bigger shock to bank size raises firm growth by more (e.g., Diamond 1984). To put the elasticity into context, the reforms increased the size of German firms' relationship banks by 372%, on average. In comparison, average real assets of FDIC (Federal Deposit Insurance Corporation)-insured US banks increased by 314% from 1950 to 1990.[44]

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[41] I find that bank debt growth fell by 0.1 percentage points per year, with a 95% confidence interval from -2.7 to 2.6 (col. 1, panel B of table 2). Several studies of bank consolidations find negative effects on small borrowers, consistent with my findings on opaque firms, but do not estimate average employment effects on all firms, as I do.

[42] For instance, banking deregulation increased the employment growth of US states by 0.7 percentage points per year (Boustanifar 2014) and total employment in bank-dependent French industries by 23% (Bertrand, Schoar, and Thesmar 2007). I find that firm employment growth fell by 0.1 percentage points per year, with a 95% confidence interval from -0.8 to 0.6 (col. 1 of table 1).

[43] The true elasticity might, of course, be heterogeneous, so that doubling the size of a single-branch lender might have different effects than doubling the size of a large bank. In this regard, recall that the estimates here are about state-level banks that became national banks. After 1957, total assets of every treated bank exceeded 1% of German GDP.

[44] Data are from the FDIC. Between 1990 and 2016, average assets of FDIC-insured banks increased by 518.2%, and acquisitions raised the average growth per year of Citigroup by 4%, of JP Morgan Chase by 10%, of Wells Fargo by 13%, and of Bank of America by 13% (Adams and Driscoll 2018).

B. Insights from the Postwar Reforms

The main conclusion of this paper is that increases in bank size do not always improve bank efficiency and borrower growth. This finding stands in contrast to some leading models, where scale economies are the reason why banks exist and where bigger banks are always more efficient. In Diamond (1984), for instance, a banking system with a single monopoly bank is socially optimal.

Whether bigger banks benefit or harm borrowers depends on the net impact of several theoretical mechanisms that can affect bank performance. The results of this paper shed light on the net importance of the mechanisms. Some mechanisms might have positively affected borrower growth (spreading fixed costs, issuing large loans, developing synergies, using internal capital markets, more diversification). On the other hand, some mechanisms might have harmed borrower growth (greater complexity, worse processing of soft information). For the average borrower in the data, on net, the mechanisms did not shift growth, and no mechanism dominated. However, for opaque borrowers and municipalities, the harmful mechanisms relating to complexity and soft-information processing dominated, leading to lower employment growth.

More recent data suggest that the mechanisms determining outcomes of opaque borrowers were not relevant only in the 1950s. Just to name a few examples: relationship banking still influenced real outcomes during the 2008–9 crisis (Chodorow-Reich 2014; Bentolila et al. 2018), local bankers and branch closures continue to determine the loan supply of firms today (Degryse and Ongena 2005; Nguyen 2019), and US banking deregulation harmed financially constrained borrowers while it benefited large firms (Berger et al. 2020).

The findings of this paper do not imply that increases in bank size can never improve bank efficiency and borrower growth. Returns to scale might have increased since the 1950s because banks have adopted modern information technologies, such as credit scoring (Berger and Mester 1997; Petersen and Rajan 2002; Berger 2003). Moreover, it could be that the operations of a branch remain persistently influenced by having been part of a large institution at some point. The results in this paper would not capture such an effect, since all branches were already part of large banks before the war. While the results do not account for these specific channels, they capture several other mechanisms that are commonly associated with increases in bank size, as discussed above.

IX.   Conclusion

Banking reforms in postwar Germany determined when certain state-level banks were allowed to consolidate into national banks. The reforms

provide a rare opportunity to analyze exogenous changes in bank size. I digitize new microdata on German firms and their relationship banks and examine how the bank consolidations affected the growth of banks and their borrowers.

I find no evidence that increases in bank size raised the growth of borrowers. Firms and municipalities with higher exposure to the consolidating banks did not grow faster after their banks consolidated. Opaque (small, young, low-collateral) borrowers of the banks actually experienced lower employment growth after the consolidations. The consolidating banks themselves did not increase lending, profits, or cost efficiency, relative to comparable other banks.

The results show that increases in bank size do not always generate improvements in the performance of banks and their borrowers and might even harm some firms. The impact of bigger banks is a complex question that depends on the net impact of several mechanisms. Some mechanisms are beneficial and some harmful to borrower growth. The experience from postwar Germany highlights that the beneficial mechanisms are not always powerful enough to outweigh the harmful effects.

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