

May the force be with you:  
Do political consolidation barriers depress bank profitability?\*

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**Abstract**

We test whether the removal of implicit political consolidation barriers causally enhances bank profitability. We exploit an exogenous shock through which selected regional banks in Germany are forced to merge: the unification of counties. County mergers legally force government-owned, but not privately owned banks to merge. Forced bank mergers cause both profitability to hike relative to voluntary mergers. Bank risk responds only mildly. Corporate borrowers exposed to forced bank mergers borrow more at lower cost, increase investment, and exhibit higher employment. The removal of implicit bank consolidation barriers thus seems to spark profit dynamics while enhancing simultaneously corporate performance.

**JEL Classification:** G21, G29, O16

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# I. Introduction

European bank profitability remains low since the financial crisis (ECB, 2016; EBA, 2017; Detragiache et al., 2018), which may eventually harm financial stability (IMF, 2016; ECB, 2017a, 2018). Frail profitability is largely attributable to structural causes, such as the need for downsizing and restructuring due to overcapacities (ESRB, 2014; IMF, 2016). Policymakers, therefore, call for more consolidation to restore banks' profitability and foster the resilience of the financial system (ECB, 2017b). However, why do banks not merge?

A prime candidate to rationalize lame merger activities across jurisdictions in the banking industry are implicit political consolidation barriers. The German economy minister, for example, announced publicly to block unwanted foreign takeovers and to promote national champions (The Economist, 2019). More generally, the market for corporate control in banking is imperfect (de Haan and Vlahu, 2016) and vested national political interests paired with intensive regulation complicate hostile and cross-border bank mergers (Morgan, 2002; Buch and DeLong, 2004; DeYoung et al., 2009). These impediments allow inefficient incumbents to continue operations (Porter, 1976; Harrigan, 1981; Brown and Dinç, 2011). Such (national) banking systems accumulate excess capacity and adopt technologies too slow (Jensen, 1993; Tinn, 2010; Titman, 2013).

We test if the removal of implicit political consolidation barriers causes performance to improve by exploiting an exogenous shock to bank merger activity in local German banking markets. Specifically, government-owned banks are *de jure* forced to merge if their host county ("Kreis") is unified with another one during regional reforms launched by state ("Bundesland") governments. Regional cooperative banks are, in turn, not required but may choose to merge voluntarily.<sup>1</sup> First, we test if the post-merger performance of

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<sup>1</sup>Cooperative banks are owned by their members. In contrast to the government-owned savings banks, we refer to them as privately owned.

the quasi-randomly selected banks forced to merge is significantly better compared to voluntarily merging banks. Second, we isolate the channels of how bank profits improve. Third, we test the implications for corporate customers of banks that are forced to merge.

The differential post-merger profitability hikes of merged local savings banks triggered by a county reform are economically and statistically significant. Depending on the reference group – local banks’ mergers in reformed counties, any merging bank, or all non-merging banks – we estimate an increase of the return on gross equity (RoE) between 3.8 and 5.7 percentage points. Against the backdrop of mean RoE on the order of 8% in our sample, this effect is economically substantial. We further find only mild increases in idiosyncratic bank risk after forced mergers. Thereby, our results suggest that implicit political transaction barriers prevent performance-enhancing bank consolidation.

The estimated effects of forced bank mergers exploit the political specificities of German federalism. German federalism provides a handy setting towards precise isolation of causal performance responses but prohibits to conclude that identical results apply necessarily outside our quasi-experimental setting. However, it is useful to note with regards to external validity that the German federal setting of state politics and county bank ownership resembles quite closely the vested national interests that subdue cross-border bank merger activities at the European level. After all, the keen interest of local German politicians to maintain their local banks wither good economic reasons to merge already before being forced by state politics closely resembles national politics geared towards the creation of national champions in each European economy.

Forced mergers emerge in this quasi-experimental setting because local savings banks are the property of the regional government where they reside. The residence is usually in one of the 402 counties (“Kreise”) nested in the 16 federal states of the Republic of Germany. The state issues the savings bank laws (“Sparkassengesetze”). These laws stipulate, in addition to county ownership, that local savings banks are *de jure* not allowed

to operate outside “their” regional market. During our sample period from 1993 until 2015, the number of counties declined drastically from 542 to 402, as illustrated in Figure 1.

– Figure 1 around here –

Two aspects of regional reforms are important. First, they are decided on at the level of the *state* for administrative efficiency reasons and do not depend on the financial health of local government-owned banks. The goal is to reduce the excessive fragmentation of public services. New administrative entities should be of sufficient size in terms of the population to pursue public tasks, like schooling, tax collection, and the like, efficiently (see, for example Stür and Landgraf, 1998; Büchner and Franzke, 2001; Wehner, 2015; Landtag von Sachsen-Anhalt, 2016). Second, neither savings bank owners nor managers can (“cherry-”)pick merger partners. Reforms affect all counties within a state at the same time, and solely the geographical scope of new, unified counties determines which savings banks have to merge. Therefore, regional reforms represent an exogenous shock to the *counties* that own local savings banks.<sup>2</sup> The latter is required by law to merge after the unification of counties. As such, they represent state government-mandated consolidation events to overcome perceived (sub-state) political merger barriers.

We use a difference-in-differences model to explain post-merger bank performance. Forced mergers occur to government-owned savings banks in counties that were subject to regional reforms conducted in different states at different times. Savings banks in counties of non-reforming states, as well as cooperative banks located in reformed and non-reformed counties, represent the control groups to conduct three main comparisons.<sup>3</sup> First, we compare merging savings banks to merging cooperative banks within reformed counties. This specification provides the most conservative test because we hold all unobserved factors that may trigger merger activity in a county constant. Second, we compare

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<sup>2</sup>County consolidation is also not a gerrymandering process pursued by incumbent state governments.

<sup>3</sup>Cooperatives are very comparable in terms of size, regional, and business model orientation.

forced savings banks mergers with mergers of cooperative banks and non-affected savings banks in both reformed and unreformed counties. This comparison encompasses almost all bank mergers that occurred in these banking groups.<sup>4</sup> Third, we compare forced mergers to both merging and non-merging banks across reformed and unreformed counties. All tests consistently indicate that forced mergers after county reforms outperform the respective control group.

The decomposition of the profitability hike reveals that the RoE improvements come from both a decline in net capital as well as enhanced profits due to post-merger cost contractions. At the same time, post-merger risk developments do not raise financial stability concerns. Whereas net equity levels decline, Tier 1 capital ratios are unaffected by forced mergers. Likewise, the total effect of county reforms is not a higher credit risk. Profits improve primarily due to cost reductions rather than revenue hikes, which is consistent with the absence of significant market power responses. Cost reductions result from better (re-)financing conditions rather than large-scale restructuring. Put differently, we do not find empirical evidence that forced mergers induce large-scale layoff waves or branch closures to realize performance potential. The headline result is robust to alternative evaluation windows around mergers, robust estimation methods accounting for potential serial correlation of performance, randomized treatment of mergers with placebo county reforms, and explicitly accounting for distressed mergers and observable differences in the strengths of political ties.

Regarding real effects, we find that corporate and consumer lending volumes by local banks increase significantly after forced mergers. So at least in the German banking system, the elimination of regional lenders did not constrain credit access. Similarly, we do not find a reduction in deposits, a crude measure of retail customer access to financial services. Another potential social cost inflicted by forced mergers could be that

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<sup>4</sup>The mergers of our sample account for 98% of banks exiting the market during the sample period.

post-merger banks return political favors by increasing (local) government lending. We find no support for this type of undesirable credit allocation. To shed more direct light on the real implications, we then use detailed information about a sample of corporate clients of local banks. We demonstrate that corporations that are connected to savings banks that were forced to merge after county reforms incur lower external financing costs. Connected corporations also increase investment and employment after forced bank mergers. So besides enhanced bank profitability at mildly riskier financial profiles, these results indicate also better financing conditions and performance realized by non-financial firms that are exposed to savings banks forced to merge.

Our study of government-enforced consolidation effects complements the literature on how the removal of government-imposed bank merger barriers affected market structure and performance by unleashing market forces. Kroszner and Strahan (1999) started a rich literature on how inter- and intra-state merger deregulation shaped the U.S. banking industry: enhanced technology adoption, more competition, increased bank efficiency, and ultimately a more concentrated and profitable banking system (Berger and Mester, 2003; Stiroh and Strahan, 2003). German bank merger analyses yield more mixed results regarding post-merger performance developments, often failing to report efficiency or profitability gains (Lang and Welzel, 1999; Behr and Heid, 2011). These studies, however, do not identify the causal reasons for bank mergers. If past bank performance co-determined a merger in the first place, any post-merger comparison of performance is subject to selection bias and possibly reverse causality. Our paper sharpens the insights into the bank merger literature because we exploit an exogenous, government-mandated call for consolidation. Thereby, we can isolate performance differences compared with an otherwise identical set of merging banks.

We also contribute to the literature investigating the performance implications of government ownership in banking. In contrast to abundant literature about the role of

political ties that might impede creative destruction (Brown and Dinç, 2005; Duchin and Sosyura, 2012; Dam and Koetter, 2012; Behn et al., 2015), our identification strategy relies on exogenous shifts in government ownership during non-crisis times that reveal the conventionally missing counterfactual of banks leaving the market. Many studies using pre-crisis data on the effect of government ownership report undesirable effects, such as preferred bailout treatment (Behn et al., 2015); political lending (Sapienza, 2004; Halling et al., 2016), especially around elections (Gropp and Saadi, 2015; Englmaier and Stowasser, 2017); inferior risk-management skills of management (Hau and Thum, 2009; Cuñat and Garicano, 2010); and ultimately, inadequate fulfillment of banks' roles as delegated monitors of corporate lending and guardians of managerial discipline (Berger et al., 2005; Ivashina et al., 2009), which deters economic growth (La Porta et al., 2002).

After the Great Financial Crisis, governments around the globe systematically supported their national banking systems with equity (Duchin and Sosyura, 2012), which impeded “natural” forces of competition to guide entry and exit into the industry.<sup>5</sup> The extensive and quick U.S. support of banks was followed by an equally rapid retreat of the government from its banking system (Hoshi and Kashyap, 2010; Calomiris and Khan, 2015). In contrast, the German system remains characterized by substantial government involvement. Rather than focusing on the effect of government interventions and ownership on bank performance as such, we directly test if the elimination of implicit political consolidation barriers utilizing state-government enforced mergers enhances bank performance.

We extend our analysis to identify real effects of the forced savings banks mergers to address concerns regarding harmful consolidation. Regional banks might fulfill essential functions that bigger banks fail to provide. Berger et al. (2005) provide evidence that

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<sup>5</sup>Gropp et al. (2011) and Berger and Roman (2015) study competition responses due to bank bailouts in Europe and the United States, respectively. Duchin and Sosyura (2014) and Dam and Koetter (2012) additional document risk-taking due to the moral hazard exerted by government bailouts of banks.

the monitoring techniques of small banks are better suited for lending to opaque small and medium-sized enterprises (SMEs). Hakenes et al. (2014) show theoretically that small regional banks foster local economic growth and confirm this prediction empirically for German savings banks. Berger et al. (2017) demonstrate that small banks possess a comparative advantage to provide liquidity insurance to SMEs, thereby helping to alleviate financing constraints, especially for those firms that conventionally depend on the most on bank credit. Importantly, Degryse et al. (2011) show that small bank mergers have the worst implications for SMEs with only a single relationship. Their banking contact is usually dropped and not replaced if their relationship lender turns out to be the target in a bank merger and acquisition, a result similar to one documented before in the United States (Berger et al., 1998). Thus, it is a priori unclear whether forced savings banks mergers induced by county reforms only unlock previously unrealized profitability potential or whether they generate worse conditions for an important group of these banks' customers. In contrast, our results show that firms connected to savings banks profited if they merge after county reforms.

## II. Institutional background and identification

### A. *Local savings banks*

In 2015, the German government-owned banking sector comprised 413 local savings banks (SB) that managed an aggregate balance sheet of EUR 2,119 billion assets, a 24% share of the entire market. The average SB has a balance sheet of EUR 2.7 billion and serves a regional market approximately the size of one county. Jointly, these banks cater to every region in Germany. They operate an extensive network of branches and are owned by regional municipalities or counties.<sup>6</sup>

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<sup>6</sup>The legal concept of government ownership (*Trägerschaft*) shares key features of private ownership but is not identical. We discuss the relevant differences below. We refer henceforth to local politicians



In addition to national regulation governing all credit institutions, they are subject to federal law regulating ownership, governance structure, and their business model.<sup>7</sup> These laws impose institutional frictions on competition and consolidation in the government-owned banking sector. The geographical scope of business is confined to the territory of the owning locality, also known as regional demarcation (*Regionalprinzip*), *de facto* eliminating competition with other SB in credit and deposit markets. Likewise, a free market for corporate control does not exist. Mergers are only permitted between neighboring SB and only within the government-owned banking sector. Decisions about closure and mergers are neither taken by the management nor the supervisory board but by the local governing politicians of the owning county or municipality, to whom we refer henceforth as local politicians. Decisions are subject to approval by the savings bank association and the federal regulator, which is one of the federal ministries. The savings bank association sometimes recommends mergers between distressed and healthy banks as a measure of last resort in order to avoid closure (Koetter et al., 2007; Behn et al., 2015).

The important aspect regarding our identification is that each region must not own more than one SB after county reforms. Federal laws or the reform bills themselves state that in case any of the newly formed counties owns more than one SB, these banks have to merge.<sup>8</sup> Often, the reform bills contain a deadline of two or three years within which this consolidation process has to be completed (see Table AI in the Appendix). Importantly, it is federal and not local politicians who vote on county reforms. The reform-induced mergers are therefore forced on local governments and their SB.

In addition to the decision about mergers and closures, local politicians exercise control

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who represent the relevant region over the election cycle as the owners of the SB.

<sup>7</sup>We distinguish between the local, federal, and national levels. The federal level refers to the 16 German states.

<sup>8</sup>See Mecklenburg-Vorpommern: §28 Abs.1a SpkG of Mecklenburg-Vorpommern, §25 LNOG from July 1, 1993, and §41 LNOG from July 12, 2010; Saxony-Anhalt: §30 Gesetz zur Kreisgebietsreform from July 20, 1993, and §18 LKGebNRG from November 11, 2005; Saxony: §22 SächsKrGebRefG from June 24, 1993, and §25 SächsKrGebNG January 29, 2008; Thuringia: §11 ThürMaßnG; Brandenburg §35 BbgSpkG, and §26 KNGBbg December 24, 1992.

over SB via the supervisory board. The composition of the supervisory board is regulated in detail. The chairman has to be the elected governor of the region. The remaining board seats are distributed among other local politicians, bureaucrats, and representatives of employees. The degree of influence by local politicians is sufficient to alter lending behavior, merger patterns, the dismissal of employees, in addition to whether and whom to bail out around elections (Hackethal et al., 2012; Behn et al., 2015; Englmaier and Stowasser, 2017). The timing of these phenomena around elections stresses that local politicians that control SB pursue vested interests. These interests could also pertain to social and welfare benefits due to owning and managing a bank on behalf and in the interest of the region itself. By constitution, SB serve the public by providing banking services to all regions and promoting the local economy. Often, they engage in charity and foster cultural and sports events.

At the same time, the institutional setting allows for the extraction of commercial rents on behalf of the county. Since 2002, regional owners do not participate in the losses of the bank anymore by issuing guarantees or bailouts because the EU commission ruled it to be a distortion of competition. However, counties are allowed to participate in the profits, which at times give rise to conflicts between savings bank managers and politicians (Correctiv Recherchen für die Gesellschaft gGmbH, 2015). The federal laws prescribe a maximum share of distributable profits. The management board proposes the allocation of earnings to the supervisory board, which has to affirm it. If the supervisory board is split between representatives of more than one county after a merger, extracting rents for one group of owners becomes increasingly tricky. In conclusion, the institutional background sets incentives for local politicians to prevent mergers in their private interests in addition to the genuine public interest.

## *B. German county reforms*

Spatial reforms change how the national territory is divided among federal and local political entities. In Germany, they occur only at the local level within federal states. In these reforms, the entire area of a federal state is reshaped at once. New county borders are set such that each county administration caters to a minimum population while limiting the geographical distance between county borders and the central administrative city (Stür and Landgraf, 1998; Büchner and Franzke, 2001; Wehner, 2015; Landtag von Sachsen-Anhalt, 2016). Counties are usually unified and only rarely split. Eventually, borders are determined by socioeconomic, cultural, and historical factors.<sup>9</sup>

The local governmental layer is divided into counties and municipalities. In 2015, there existed 11,168 municipalities that formed 402 counties instead of the 543 counties that existed after reunification in 1990 (Statistisches Bundesamt, 2015). We focus on county-level reforms, which are initiated and decided on by the federal states' parliaments, not by local politicians on the county level. They are usually linked to functional reforms of the state's administration and accompanied by municipality-level spatial reforms. The main motives are to increase the efficiency of administration and to ease fiscal budgets by forming fewer and consequently larger counties out of comparable regional entities regarding their socioeconomic structure (BBSR, 2010).

Since German reunification, eight major reforms occurred in five Eastern German states, each of which reduced the number of counties on average by half.<sup>10</sup> Appendix Table AI reports the number of counties, savings, and cooperative banks before and after each reform. In West Germany, two metropolitan areas were created: Aachen in North

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<sup>9</sup>The county Anhalt-Zerbst in Saxony-Anhalt is such an example. It existed between 1994 and 2007. It was created in the first reform of 1994 by unifying the counties Zerbst and Roßlau with a combined population of about 80,000. Borders thereby roughly restored the ancient territory of the principality of Anhalt-Zerbst, dating back to the 13th century. After reforms in 2007 it was split into three parts joining three different new counties with 100,000-190,000 inhabitants per county.

<sup>10</sup>Most reforms restored pre-GDR county borders to undo legislation passed in 1952, which almost doubled the number of counties in the former GDR Stür and Landgraf (1998).

Rhine-Westphalia and Hanover in Lower Saxony. Both county-level reforms implied that two cities were combined with their surrounding counties. These ten county reforms serve to identify treated savings banks.

Local politicians usually oppose reform plans since they lose their autonomy. Therefore, reforms are heatedly discussed both before and after their legislative passage. A majority vote of federal politicians issues reform bills. In light of our identification strategy, it is noteworthy that the allocation mechanism of seats in state parliaments implies that a dominant role of federal politicians with the same local interests as local politicians is improbable. Only approximately half of the seats of the state parliaments are allocated to politicians who directly represent voting districts. These voting districts are not equal to counties. They are set in such a manner that they represent a defined population (approximately 60,000 voters). Therefore, less-populated rural counties are combined into voting districts, and large cities are divided into several voting districts. Since large cities usually maintain their status even after county reforms, treated rural counties are under-represented in state parliaments. The other half of the seats are allocated to politicians that are chosen from a ranked list compiled by each political party. These members of state parliaments, therefore, do not have to represent any particular local interest per se. They are often “professional” politicians, and parties assign better ranks to these experts – or long-serving party members – to increase their odds of becoming a member of parliament.

Regarding SB, politicians can lobby upfront for an exemption ruling, which led, for example, to the suspension of forced mergers in Saxony in 2008 and Mecklenburg-Vorpommern in 2011. We observe two counties in Saxony and two counties in Mecklenburg-Vorpommern that own more than one bank after the reforms. The Saxonian banks merged eventually (in 2010 and 2012). In contrast, the Pomeranian banks have not.<sup>11</sup>

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<sup>11</sup>We treat these two Saxonian mergers as treated by reform, which can only harm our results. As a robustness check, we split the sample in the year 2000 and use only the early reforms.

### C. Identification

We illustrate the baseline and alternative identification strategies in Figure 2. In the baseline specification, we focus only on merging banks from either the cooperative or the savings bank sector, which are shown in the left-hand panel.

– Figure 2 around here –

We start by considering only merging banks  $i$ , which reside in (pre-reform) counties  $k'_1$  and  $k'_2$ . That is, we disregard both non-merging banks and those that merge but do so in non-reforming counties. Our focus is thus on those counties that form a single geographical entity  $k$  – and hence the owner of local SB – after county reforms. Observed savings bank ( $SB_i$ ) mergers are therefore forced upon the management and owners of either pre-reform, independent banking entity  $i'$  as a result of the legal requirements of the savings bank laws of the respective state. In contrast, observed cooperative bank ( $CB_i$ ) mergers occur voluntarily. This identification approach therefore compares post-merger performance of the four pre-reform banks  $i' = 1, 2, 3, 4$  in the upper-left panel of Figure 2, which merge into banks  $i = 1, 2$  in the lower-left panel. These two banks face otherwise identical, unobserved regional conditions, such as sluggish demand for banking products that might fuel consolidation pressures. Consequently, we attribute any significant performance difference to the abandoning of having separate SB per county.<sup>12</sup>

The second identification strategy acknowledges the abundant literature regarding conflicting merger motives, such as cherry-picking versus the “silent” resolution of bank distress via pre-emptive mergers. Therefore, we also sample merging banks in non-reforming counties:  $i' = 5, 6, 7, 8$  in the upper-right panel depicting the non-reformed counties  $k = 2$  and  $k = 3$ . These mergers then give rise to a new SB  $i = 3$  and a new

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<sup>12</sup>We demonstrate in Table III that the sampled SB and CB are for the most part not statistically different in terms of the level of observable financial traits and exhibit no statistically discernible trend in any of the controls we specify and discuss in more detail below.

CB  $i = 4$ , each of which catering to both counties simultaneously. The post-merger performance comparison between banks  $i = 1, 2, 3, 4$  relies now on both the within-county variation between SB and CB, as in the baseline identification, and the between-county, between-merged bank variation of regions  $k = 2, 3$  and  $k = 1$ .

In the third identification strategy, we add non-merging banks from both reformed and unreformed counties to the post-reform control group, that is banks  $i = 5, 6$  in terms of Figure 2. Thereby, we gauge if SB that are forced to merge also unleash profitability potential relative to incumbent competitors that maintain the size of their operations.

We deliberately choose not to pursue a conceivable fourth strategy: to compare only voluntary versus forced savings bank mergers. We do so because county reforms always apply to *all* counties within a reforming state. Therefore, such a setting would require a between-state comparison. Such a comparison precludes the saturation of the empirical model with the time-by-state fixed effect shown below. Controlling for unobservable regional macroeconomic conditions is, however, crucial in our case to avoid confounding drivers of post-merger bank performance other than reform-induced ownership changes. Such confounders are very likely to exist since such a between-state comparison essentially gauges differential effects between forced mergers in East Germany versus voluntarily mergers in West German states (see also Figure 1).

### III. Methodology and data

#### A. Methodology

To test whether government-forced mergers enhance profitability, we compare post-merger banks to synthetic pre-merger entities that we construct as follows. All banks in the sample leave the market via mergers and acquisitions. Thus, the assets of exiting banks remain within the (savings or cooperative) banking sector and end up with one surviving

bank at the end of the sample period in 2015. We identify acquiring banks and any subsequent acquirers up to four layers of acquisition history for each merging bank until we identify this ultimate survivor. For each surviving bank, we construct a synthetic pre-merger bank. We aggregate the assets, liabilities, and income statement positions from the first until the last available report before the M&As of all merging banks whose acquisition history leads to the ultimate survivor bank. We then specify a difference-in-differences model to test whether forced M&As unleash profitability potential:

$$\begin{aligned}
Profitability_{i,t} = & \alpha_i + \delta_{s,t} + \gamma \mathbf{X}_{(i,c),t-1} + \beta_1 (Merger_{i,t}) + \beta_2 (Reform_{i,t}) \\
& + \beta_3 (Merger_{i,t} \times Reform_{i,t}) + \beta_4 (Merger_{i,t} \times SB_i) \\
& + \beta_5 (Reform_{i,t} \times SB_i) + \beta_6 (Merger_{i,t} \times Reform_{i,t} \times SB_i) + \epsilon_{i,t}
\end{aligned} \tag{1}$$

The primary dependent variable  $Profitability_{i,t}$  is measured as the return on equity of synthetic bank  $i$  in year  $t$  residing in county  $c$  in state  $s$ , and it equals operating profits before taxes over gross book-value equity.

$Merger_{i,t}$  is an indicator variable equal to one in all years after a merger and acquisition. Since events occur at different points in time for each unit under observation,  $Merger_{i,t}$  is defined in terms of event time, which is zero for all merging banks in the year of the merger. The year of the merger is the first year in which the acquiring bank issued accounts incorporating the target, and the target stopped reporting. We exclude the merger year itself from the estimation. The indicator variable equals zero up to four years before the transaction, and it equals one up to four years after the event.

On average, synthetic banks merge more than once, and cooperative banks merge even more than twice. Consequently, the treatment dummy  $Reform_{i,t}$  is defined per transaction and bank, and it is equal to one in the pre- and post-periods if the merger occurred within three years after a county reform. For example, for banks headquartered in a county in Saxony-Anhalt, which was reformed in 1994, any deal in 1994, 1995, or

1996 would be treated. By using a three-year window, we account for the deadlines fixed in the reform bill (Table AI indicates that in the case of Saxony-Anhalt 1994, this was 1<sup>st</sup> January 1997) and the fact that we use end-of-year bank data.

$SB_i$  is a dummy variable indicating whether the bank is a government-owned savings bank (as opposed to  $CB_i$ ). The coefficient of interest is  $\beta_6$  of the triple interaction term, and it measures the difference in the effect of merging with or without reform on profitability for savings relative to cooperative banks.

## *B. Data*

We use bank-level data from annual accounts and regulatory statements, supplemented with event data regarding mergers and distress events provided by Deutsche Bundesbank, for the period from 1993 to 2015.<sup>13</sup> We observe the whole universe of savings and cooperative banks in Germany. The private banking sector is excluded because we cannot attribute financial data of nationwide operating private banks to local banking markets. The sample comprises 714 reporting savings banks and 2,782 reporting cooperative banks, resulting in 80,868 bank-year observations. We complement these data with macroeconomic information at the county level provided by the Federal Statistical Office of Germany and spatial data provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR), which we use to construct a reform indicator on the county level. We match this regional information based on the location of banks' headquarters using a county-level identifier.

We estimate Equation (1) with a sample of transactions, that is, each bank included in the sample merges eventually. We accumulate all transactions of an acquirer during a year and treat them as one transaction with multiple targets. All in all, we observe 1,820 deals. These deals involve 286 savings and 1,740 cooperative banks as targets and 182

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<sup>13</sup>The database regarding distress events is available from 1995 to 2013.



savings and 889 cooperative banks as acquirers.<sup>14</sup> By considering these transactions, we capture 98.5% of all exits in the population.<sup>15</sup> We discard 193 transactions because of missing covariates, reducing the sample to 1,627 transactions, of which 233 occurred in the savings bank sector. We observe 48 forced mergers of savings banks and 26 reform-induced mergers of cooperative banks. Table I depicts the dynamics over time.

– Table I around here –

A possible concern is that savings and cooperative banks may be too different to constitute suited comparison groups. Moreover, acquirers are different from targets (Hannan and Rhoades, 1987), and stressed savings banks are merged rather than closed (Koetter et al., 2007). Hence, banks that merge voluntarily – cooperatives – might be different from savings banks that are forced to merge due to a county reform. Four features of our setting alleviate concerns about spurious comparisons.

– Figure 3 around here –

First, Figure 3 corroborates that the average profitability of treated and untreated banks within a banking group evolves similarly in the pre-merger time window, but differs starkly for savings banks. This pattern also holds for return on assets, see Figure A1 in the appendix.

– Table II around here –

Table II compares mean levels and first differences in the profitability measure in the pre-merger period over treatment and ownership status. The difference-in-differences of

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<sup>14</sup>Approximately 24% of the acquiring savings banks and 46% of the acquiring cooperative banks merge more than once. However, some acquirers are themselves targets later on.

<sup>15</sup>Bank exit is defined as stopping to report total assets to Deutsche Bundesbank. Only 30 exits of regional banks over the sample period cannot be attributed to a merger. However, an Internet search reveals that all seven savings banks that exited without record were also acquired despite the transactions not being listed in the merger data.

means are significant neither in levels nor in changes before the event occurs (last row in Columns (3) and (6)). Both savings and cooperatives that are treated and untreated and treated cooperative banks do not differ significantly before the merger. The profitability differences between cooperative and savings banks that are untreated and between treated and untreated savings banks are significant. Note, however, that the latter differences only appear in levels; thus, the fixed effects and covariates control for the difference.

Second, the use of synthetic pre-merger bank-entities levels out some of the performance differences between target and acquiring banks. Third, we exclude and control for mergers if a party was distressed. Fourth, we are interested in the effect of the reform as alleviation of frictions, not the effect of merging per se. Therefore, any potential selection bias between non-merging and merging banks is less likely to bias our test.

The matrix  $\mathbf{X}$  in Equation (1) gauges macroeconomic and bank-specific conditions, which are defined in Appendix Table AIX. Bank-level fixed effects account for unobserved time-invariant heterogeneity across banks. To address time-varying variation between banks, we add CAMEL financial ratios, proxies for banks' business models, and size (Wheelock and Wilson, 2000). The summary statistics reported in Table III demonstrate that despite some significant differences in the differences of levels (Column (9) upper part), the difference-in-differences of all covariates' changes are insignificant (Column (9) lower part) except for loan loss provisions.

– Table III around here –

We measure financial profiles with (i) the equity to total assets ratio to gauge capital adequacy (*Equity*), (ii) loan loss provisions to total loans for asset quality (*LLP*), (iii) cost-to-income ratio for management quality (*CIR*), and (iv) liquid to total assets for liquidity profile (*Liquidity*). In the baseline estimation, we exclude proxies for earnings because these are strongly correlated with the dependent variable. To capture the business model, we add (v) consumer loans to total assets ratio (*Loans*) and (vi) non-interest-income to

total income ( $NII$ ). Finally, we specify (vii) size as an annual decile indicator of the total asset distribution ( $Size$ ). All covariates lag by one year. To account for macroeconomic differences, which affect business opportunities and the demand for banking services, we add year  $\times$  state fixed effects. Besides, we control for GDP at the county level, which is one of the few macroeconomic measures also available at granular regional levels in Eastern Germany since the early 1990s.

## IV. Effects of forced mergers on bank performance

### A. Profitability effects

Table IV reports our baseline regression results from estimating Equation (1). We start in Column (1) with a sample of merging banks that resided only in reformed counties.<sup>16</sup> In terms of the illustration in Figure 2, we thus consider banks  $i' = 1, 2, 3, 4$  in the upper-left panel. The results in Column (1) show that the coefficient of interest, the triple interaction term  $\beta_6$  between the savings bank indicator, the occurrence of a merger, and a spatial reform affecting banks' home counties, is positive and statistically significant.

– Table IV around here –

The economic magnitude of this forced merger effect is large. Government-owned savings banks that merge after a county reform exhibit a positive differential return of equity (RoE) effect of 5.7 percentage points relative to the comparison group of not-yet-merged savings and cooperative banks before the reform, which compares to a sample mean RoE of 7.9%.<sup>17</sup>

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<sup>16</sup>Note that even here, the consecutive terms Reform and Merger and their interactions do not drop from the regression since both vary over time for an individual bank. Furthermore, reforms happened at different points in time. For more details, we refer to the definitions in the previous section.

<sup>17</sup>Whereas we focus throughout on the differential effect of forced mergers between savings and cooperative banks, note that the total relative effect of the reform on savings bank profitability is still positive and amounts to one-third of a percentage point ( $-0.024 + 0.057$ ).

These results are unlikely to reflect fundamentally different business models between savings and cooperative banks, which are absorbed by bank-fixed effects. Also, recall that we specify time-varying control variables at both the bank and county levels, which limits the danger that other (time-variant) unobserved effects bias our estimate. Another concern is that county reforms may not occur randomly but correlate, for example, with electoral and budgetary cycles at the national and sub-national levels of the states.<sup>18</sup> Dire state-specific macro and credit demand conditions could ignite both county reforms and bank mergers. Because of this valid potential reservation, we specify state-by-year fixed effects. Thereby, the coefficients in Table IV result from a within state-year comparison of banks that controls for between-state differences in terms of economic surroundings, political influences, and other unobservable demand effects. Given this encompassing saturation of the model with fixed effects to gauge unobservable drivers of post-merger bank profitability, it is remarkable that the within-county variation in covariates identifies approximately one-third of the total variation in bank RoE.<sup>19</sup>

The tight specification in Column (1) provides a very clean identification of the RoE differential effect. However, it does not permit any inference beyond locally merging banks in counties that actually experienced a spatial reform at some stage.<sup>20</sup> Since the majority of reforms – and hence forced mergers – pertain to Eastern German states (see Figure 1), we expand the control group in Column (2) by merging savings and cooperative banks from non-reforming counties. This specification adds bank mergers that occurred without an exogenous change forced upon the local politicians that own savings banks. This sample is almost three times as large. The results are virtually identical concerning the statistical significance, the direction of effects, and economic magnitudes.

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<sup>18</sup>See, for example, Seitz (2000) and Galli and Rossi (2002) for evidence at the sub-national level of German states and Katsimi and Sarantides (2012) or Efthyvoulou (2012) for national evidence in Europe.

<sup>19</sup>Appendix Table AII shows that results are insensitive to the specification of the lagged natural logarithm of public county debt ( $L(\text{Debt})$ ), which are manually collected from state statistical offices.

<sup>20</sup>We provide details about alternative samples in Tables AVI and AVII.

Alternatively, savings bank performance could be unleashed because county reforms themselves enhance profitability. County reforms in pursuit of unrealized administrative efficiency gains may extend in particular to banks supervised and owned by that very government. As such, any profitability gains from ceased politically motivated consolidation frictions would apply to non-merging savings banks, also. Confining the sample to merging banks might then give rise to spurious RoE effects of enforced consolidation. To test whether RoE effects are at work through the elimination of excess capacities due to enforced mergers, we therefore also include banks that did not merge at all in Column (3).<sup>21</sup> This specification corresponds to banks  $i = 5, 6$  in Figure 2. The main effects remain qualitatively intact for this sample. Only the economic magnitudes of both the total effect of reforms and the triple differential effect reflected by  $\beta_6$  are smaller. Overall, these results corroborate the robustness of the main findings: savings banks are significantly more profitable after a merger that was forced upon them by a county reform. Henceforth, we focus on the specification in Column (2), which compares only merging savings and cooperative banks, but from both reformed and non-reformed counties.

The headline result implies that a reduction of implicit political consolidation barriers induced by the unification of counties increases the profitability of savings banks by spurring consolidation in this part of the banking sector.

The policy implications of this main finding partly depend on the duration of how long forced mergers yield profitability improvements compared to other merging banks that did not experience a consolidation shock. Therefore, we specify increasingly long post-merger reform periods to assess if and for how long forced M&As enhance RoE. Figure 4 plots these effects for post-reform periods of up to eight years.<sup>22</sup>

– Figure 4 around here –

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<sup>21</sup>Note that this specification based on all banks also gauges potential competitive pressure by other (non-merging banks) in the same regions, which might also affect the merging banks' profitability.

<sup>22</sup>These dynamics are practically identical for return on assets, see Figure A2.

The left panel depicts the estimated double and triple interaction effects and corresponding 95% confidence intervals based on estimations of Equation (1) for the primary sample (Column (2) in Table IV) across increasing lag lengths that are depicted on the x-axis. The differential RoE effect between savings and cooperative banks remains significant for up to eight years after a forced merger. The right panel plots the overall effect of county reforms on the profitability of savings bank, which is also significantly positive for the entire period. Thus, the profitability improvements of savings banks do not vanish quickly. Instead, profitability gains are statistically significant and economically meaningful for a considerable period.

### *B. Robustness of profitability effects*

We subject our baseline results to a series of robustness checks, which are generally provided in the Appendix. First, Table AIII presents regression results for different bank profitability measures and alternative samples. For comparison, Column (1) provides the regression results for the sample of merging banks in all counties from Table IV.

Columns (2) and (3) test the importance of choice in the baseline regression to use gross equity in the denominator of bank profitability. Gross equity contains some reserve positions that allow for reasonably particular valuation treatments under German accounting rules according to the commercial code (*Handelsgesetzbuch*). Therefore, we also gauge profitability relative to net equity or total assets. In both cases, the triple interaction term remains positive and significant, which confirms the positive differential effect of forced mergers on savings banks' profitability.

Most of the county reforms occurred in the 1990s. Columns (4) and (5) test whether the headline results are driven by a particular period. The results are qualitatively almost identical regarding significance and magnitude compared to the baseline case for the years from 1994 until 2000, but insignificant during the years between 2000 and 2009. The latter

feature reflects that much fewer county reforms that affected much fewer banks occurred after the turn of the century.

Column (6) excludes distressed banks from the sample. Supervisory orders to restructure might be a confounding channel to unlock profitability potential after the successful recovery of the merged entity. The size of the triple interaction term declines to an increase of RoE by 4.6 percentage points. This result still indicates economically relevant implicit political consolidation barriers that hold back bank profitability.

In Column (7), we acknowledge that savings banks might be connected to local politicians through credit connections. We, therefore, exclude banks with a municipality lending share of total loans above the average of their banking groups to account for possibly very close political ties in Column (7). This specification leaves the main results untouched.

We sample then as in Huang (2008) only banks from reforming counties and banks from adjacent non-reforming counties. This contiguous county specification ensures that those unobservable factors possibly not captured by the fixed effects are muted. Column (8) shows that savings banks still exhibit higher profitability after forced mergers.

In Column (9), we address possible concerns related to the time-series correlation of bank mergers and profitability in our sample. A typical concern with difference-in-differences regressions applied to panel data with many periods is the correlation of the dependent variable. In such a case, standard errors may be low enough to imply a systematic over-rejection of the null hypothesis of differential effects after the treatment (Bertrand et al., 2004). Note that the merger events analyzed here do not occur for all banks in one particular year. Therefore, the pre- and post-periods are not equal for each treated and control bank. Consequently, a standard OLS regression on the collapsed sample is inadequate. We follow Bertrand et al. (2004) and regress the dependent variable RoE on the covariates, fixed effects, and the reform indicator, which defines the treatment

status. Only the residuals of the treated banks are then distinguished into two groups, thereby eliminating the time dimension: residuals from the pre-reform years and residuals from post-reform years. Column (9) reports the results when we estimate the impact of the reform on the treated banks in this two-period panel. The interaction effect of the merger indicator and the indicator that separates savings from cooperative banks are both significant. Consequently, this procedure to eliminate potential concerns regarding serial dependence contaminating our estimates does leave our main effect of interest intact.

To verify whether the differential effect in returns was induced by reform or chance, Table AIV provides results from placebo reform treatments. We run two simulations with 1,000 replications and extract the probabilities to be treated by reform for each banking group separately. We separate by the banking group because the probability of being treated for savings banks is significantly higher than for cooperative banks. The reason is that most of the reforms occurred in Eastern Germany. However, there exist disproportionately more cooperative banks in Western Germany, especially in the south of Germany. Failure to account for these differences would over-sample cooperative banks. We assign reform treatment randomly for all years to other merger events, re-estimate the baseline specification (Column (2) in Table IV), and test in each repetition the hypothesis that the coefficient on the triple interaction between reform, post-merger, and savings bank is equal to 0. The rejection rates of this test at 1%, 5%, and 10% are reported in Table AIV. Treatments are assigned randomly for all reporting banks, also actually treated ones. Overall, Table AIV indicates that for these random placebo treatments, our main effect is only significant within the range of statistical noise. This outcome thus strongly supports our results from Table IV.



### *C. Accounting decomposition and channels*

At first sight, profitability improvements after forced mergers bode well to enhance the resilience of a banking system that exhibits notoriously low profitability developments since the Great Financial Crisis. In this section, we seek to shed light on the channels of positive bank RoE effects. We begin by decomposing return on equity from an accounting perspective to identify the source of profitability hikes: equity, profits, and cost. Then, we test for the economic drivers of post-merger performance documented in previous literature: risk, efficiency, and market power.

**Accounting decomposition** Table V decomposes of RoE components to identify the main drivers of profitability hikes after forced mergers from a naive accounting perspective. To this end, we replace RoE with the log level of each of its (sub)component as the dependent variable. Thereby, we accommodate the heterogeneous distribution in absolute sizes and ease the interpretation of the coefficients as semi-elasticities.

– Table V around here –

Consider first the numerator. To improve profitability in terms of RoE, management boards may increase leverage. Such an approach is undesirable from a financial stability perspective if this risk-taking turns excessive. Recall that the numerator of the performance metric RoE is gross equity, which comprises net equity, accruals, and other equity. Column (1) of Table V shows that forced mergers decrease savings banks' net equity position by approximately 8.6% relative to cooperative banks. In unreported results, we further decompose net equity based on detailed microprudential reports. For none of these sub-categories, we obtain sufficiently precise response estimates to forced mergers. But while statistically insignificant, the negative coefficient for retained earnings might indicate that the merged entity can now employ accumulated historical earnings in an

expanded business area of formerly multiple smaller counties which were not accessible because of the principles of regional demarcation.<sup>23</sup> Alternatively, this contraction of net equity is potentially hazardous if savings banks forced to merge exhibit in particular lower core capital ratios. This is not the case as we show later.

Beforehand, we turn to the numerator of RoE and decompose operating profits. If county reforms are the (positive) shock to eliminate implicit political consolidation barriers that we conjecture them to be, we expect profit hikes and cost cuts as a consequence of rectifying previously amassed operational slack. Alternatively, a substantial reduction in geographically diversified bank portfolios might aggravate agency conflicts and thereby reduce the value of surviving banks, as pointed out by Goetz et al. (2013) for the case of U.S. banks. To test whether one of these possibilities is at play in our sample, Columns (2) through (4) specify the log of profits, revenues, and total operating cost as the dependent variable.

Column (2) shows that savings banks forced to merge also increase profits before taxes by approximately 330% compared to cooperative banks. This increase in profits is not due to an increase in revenues (Column (3)). Instead, savings banks incur less total operating costs relative to their cooperative counterparts after forced mergers (Column (4)). More granular decompositions of revenue components confirm that no specific revenue type of treated banks is significantly affected by the county reforms.

Therefore, we focus on a further decomposition of costs that explained the profitability hike. Columns (5) and (6) report the two more granular cost categories that exhibit significant responses to forced mergers: reduced interest expenses and other operating costs. Importantly, personnel cost did not respond significantly to county reform shocks, which challenges the intuition that mergers are used as a vehicle to implement large-scale restructuring programs involving layoffs and branch closures. Instead, the contraction

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<sup>23</sup>All unreported results are available upon request.

of interest expenses and other operating costs, such as IT and other overhead expenses, suggests that forced savings bank mergers allowed the combined entities to operate at more efficient scales and manage to refund more efficiently, possibly through a broadened depositor base in combined counties.

**Bank risk** In addition to the somewhat mechanistic decomposition of bank profitability so far, we test three economic channels proposed in previous literature as determinants of post-merger performance. Against the background of well-known risk-taking incentives associated with higher banking market concentration (for example, Keeley, 1990), the first important question is whether the improved profitability of savings banks after forced mergers also bears implications for overall bank risk.

– Table VI around here –

Column (1) in Table VI shows that forced mergers have no significant differential effect on banks' z-scores, which curbs concerns that forced consolidation to impair financial stability. Related, while higher profitability is associated with significantly more volatile return on assets (Column (2)), Tier 1 capital ratios (Column (3)) remain unchanged. Paired with the insignificant response of core equity ratios, the decline in net equity levels reported in Table V corroborate that reduced retained earnings are no imminent threat to financial stability. This interpretation is also in line with mild increases in credit risk shown in Columns (4) and (5). The triple interaction effect of forced mergers on loan loss provision shares is significantly negative. The share of non-performing loans of savings banks exhibits, in turn, a weakly significant increase in comparison to cooperative banks. At the same time, county reforms reduce the NPL share directly by 4.6 percentage points. Thus, the triple differential effect of forced mergers of savings banks in reformed counties has to be put into perspective. Overall, and consistent with the mild reduction of net equity without compromising core capital ratios, the hike in credit risk is mild. Savings

banks' responses to the unification of fragmented small markets seem instead to be the realization of profitability potential in larger markets compared to pre-merger conditions.

**Bank efficiency** The second channel relates to the role of cost efficiency as a driver of consolidation (see, for example, Lang and Welzel, 1999; Berger et al., 1999).

Table VII accordingly reports the effects of forced mergers on the number of branches and the number of employees (both in relation to total assets), the ratio of employees per branch, wages per employee, and the cost-income ratio.

– Table VII around here –

Column (1) indicates that there is no significant reduction in the number of branches relative to bank size for savings and cooperative banks. Furthermore, savings banks have more staff relative to bank size than cooperative banks after forced mergers (Column (2)). However, when we contrast employees with branches, we find that savings banks manage to reduce the number of employees per branch by approximately 80% compared to the group of cooperative banks (Column (3)). This reduction is cost-neutral since the overall effect on labor costs (wages per employee) for savings banks is zero (Column (4)). Finally, Column (5) of Table VII indicates that the differential effect on the cost-to-income ratio between savings and cooperative banks is negative but insignificant. Thus, cost reductions do not seem to result in significantly higher efficiency of savings banks after forced mergers. This result is consistent with the absence of significant responses of personnel expenditure and the relevance of reduced funding expenditures and other operating expenses reported before.

**Bank market power** The third economic channel of potential importance is that banks merge to gain market power, thereby permitting them to extract rents, either from mere monopoly power (Canales and Nanda, 2012) or enhanced abilities to generate and use

private information from larger average customer pools per bank (Hauswald and Marquez, 2006). To test for any post-merger market power implications, we, therefore, explore net interest margins and their components and the market share of banks in terms of loans to customers of a bank within its business area. We provide the results in Table VIII.

– Table VIII around here –

We infer net interest margins (NIM) by subtracting interest expenses scaled by interest-bearing assets from interest revenues scaled analogously. Column (1) documents that forced mergers of savings banks increase these markups reflected by NIM by 0.3 percentage points, which is a large effect relative to mean net interest margins on the order of 3 percentage points. Columns (2) and (3) indicate that higher average prices charged on assets indeed drive the increase in NIM. In contrast, mean funding factor prices do not respond significantly. The finding that savings banks decrease their interest-bearing assets (IBA, Column (4)) aligns these results on (implied) interest rates with those on (log-) levels of revenues, profits, and cost in Table V. Whereas savings banks realized more pricing potential per interest-bearing asset, they reduced IBA, thereby explaining the insignificant response of the log-level of revenues. On the cost side, in turn, the reduction of total operating cost that was primarily due to reduced log-levels of interest expenses is not due to lower interest rates paid by the merged bank on borrowed funds. Instead, savings banks appear to have been able to re-finance their operations with lower levels of costly funding, possibly due to their ability to tap into a broader base of local depositor funding in the new, enlarged county. Here, we conclude that increased markups do not appear to indicate market power or rent-seeking, but rather seem to reflect more efficient pricing of credit customers paired with more efficient funding management. This interpretation is also in line with the finding in Column (5) of Table VIII that forced merges do not enable savings banks to gain market shares compared to cooperative banks.

## V. Bank and firm responses to forced mergers

So far, the evidence suggests a positive differential effect of forced mergers on bank profitability while increasing risk-profiles only mildly. However, the implication of eliminating consolidation hurdles on the real economy remains an open question. To this end, we consider next both banks' and non-financial firms' responses in greater detail.

### A. *Bank responses*

First, we analyze if and how the hike in the profitability of forced savings bank mergers is associated with some frequently voiced concerns that such a consolidation brings along: the limited provision of access to financial (retail) services in non-urban areas, support of local economic policymakers, and constrained credit access, especially for SMEs. Therefore, we specify alternative dependent variables in baseline Equation (1).

– Table IX around here –

Column (1) of Table IX specifies retail deposits of savings banks as the dependent variable. Due to the lack of more-direct measures of providing financial services to retail customers, we want to gauge whether forced savings bank mergers entail fewer retail customer accounts and lead instead to more wholesale-oriented sources of funding that do not require administering many relatively small denomination accounts. We do not find any such tendency. The triple interaction term of the merger indicator, the county reform dummy, and the savings bank indicator exhibits no significant difference relative to the comparison group of cooperative banks.

Next, we test for the possibility that savings banks either reduce or grant more municipality or state loans after their forced mergers. A reduction in lending to the local municipality or the host state of savings banks would support concerns that the statutory obligation of savings banks to serve their local community might be undermined. Expand-

ing local government lending, in turn, could give rise to entrenchment concerns between local politicians and bankers. Both outcomes would indicate some economic costs that would juxtapose the benefits of enhanced bank profitability after forced mergers. The empirical evidence, however, bears no indication for such concerns. The triple interaction terms for both forms of government lending (Columns (2) and (3)) are not significant. As such, the absence of a significant differential effect bodes well.

A third potential concern regarding undesirable real effect could be an overall credit restriction to the local business or at a politically motivated allocation to potentially less productive sectors of the economy. Columns (4) through (8), therefore, specify loans to different sectors. The only category that exhibits a significant effect is industrial loans (Column (6)), that is, loans to firms in the industrial sector. The triple interaction coefficient is positive and highly significant. Savings banks that experienced a forced merger increase their industrial loans by approximately 2% in comparison to cooperative banks. In contrast, the merged cooperative banks reduced their lending in this category by approximately 0.9% compared to the time before the reform, which leads to a gross increase of 0.9% in industrial lending by savings banks after forced mergers. Thereby, our results suggest a positive spillover effect of county reforms on the real sector in the form that savings banks use the improvements in their profitability to encourage firm lending after being forced to merge.

### *B. Non-financial firm responses*

To further zoom into such positive externalities of forced mergers to the real economy, we mobilize detailed firm-level data regarding corporations connected to savings banks. Specifically, we use detailed balance sheet and profit and loss data for firms that held a credit relationship with a savings bank between 1995 and 2006. These data have been used before (Puri et al., 2011; Gropp et al., 2013; Behr et al., 2013; Inklaar et al., 2015) and

feature an important link between savings banks and firms: the share of loans provided by savings banks (relative to total loans)  $SB$ . In comparison to the other studies, we restrict our data in two dimensions. First, we only use regions in Eastern Germany because these were subject to county reforms between 1995 and 2006. Second, we delete all firms with missing information for the main variables, which leads to a sample of 51,792 observations for 18,664 firms. With this data at hand, we estimate the following:

$$Outcome_{j,t} = \alpha_j + \gamma_{r,t} + \alpha_1 (SB_{j,t}) + \alpha_2 (RM_{i,r,t-h} \times SB_{j,t}) + \epsilon_{j,t}. \quad (2)$$

Equation (2) measures the impact of a forced merger of a savings banks  $RM$  in region  $r$  on firm  $j$  conditional on the share of savings bank loans  $SB$  that a firms holds in year  $t$ .  $RM$  is an indicator variable equal to one in the year when a savings bank in a firm's region merges due to a reform. We specify different post-merger spells that are indicated by the subscript  $h$ .

We specify four outcome variables to assess the real effects of forced bank mergers: firms' external financing cost measured as total interest expense over total liabilities, the natural logarithm of firms' gross real investments, the natural logarithm of firms' number of employees, and firms' leverage ratio measured as total liabilities over total assets. We use firm fixed effects  $\alpha_j$  and region-year fixed effects  $\gamma_{r,t}$  to control for constant factors on the firm level and for regional effects that vary over time. The coefficient of interest is  $\alpha_2$ . It gauges the differential effects on the outcome variables for firms located in regions that exhibit a forced savings bank merger in a given year with respect to the closeness of the firm's credit relation to this savings bank. We present our results in Figure 5. The associated (detailed) regression results and descriptive statistics for all variables are reported in Table AVIII in the Appendix.

– Figure 5 around here –



Each graph in Figure 5 shows the marginal effect of  $SB \times RM$  from Equation (2) for realizations of  $SB$  between 0.1 and 1. For each value of  $SB$ , we provide the marginal effect and the 95% confidence interval pertaining to four different post-merger spells: (i) the contemporaneous year (solid black dot), (ii) the contemporaneous and the subsequent year (black circle), (iii) the contemporaneous and subsequent two years (solid gray dot), and (iv) the contemporaneous and subsequent three years (gray circle).

The upper-left graph shows the marginal effects of forced savings bank mergers on the external financing costs of firms. Across the entire distribution of values for  $SB$ , we estimate a negative and significant marginal effect for the two specifications of short-term effects, that is, up to the first subsequent post-merger year. Our results are in line with for example Sapienza (2002) who also find decreasing loan rates for firms after bank mergers. Note that this result does not contradict the increase in NIM due to increasing interest rates charged on assets reported in Table VIII. There, we identify the differential effect between forced savings bank mergers and voluntarily merging cooperative banks. Here, we compare East German non-financial firms tied to a savings bank that is forced to merge to those firms connected to non-merging savings banks. This effect ranges between 10 and 25 basis points, which represents a contraction of approximately 5.5% compared to the average external funding cost in the sample of 4.6 percentage points. The marginal effects turn insignificant for spells up and until the second and third year after forced mergers. We further find that the reduction of external financing costs is more substantial for those firms that borrow larger loan shares from savings banks. As such, these results provide strong evidence against concerns that the exit of local banks after the elimination of consolidation frictions embodied in government ownership impose tighter credit conditions, especially on those SMEs that are very dependent on local savings banks. Importantly, this result does not necessarily contradict those of Berger et al. (1998), Degryse et al. (2011), or Berger et al. (2017), who emphasize the importance of

small, local lenders to provide credit and liquidity insurance to SMEs. Instead, our result provides important indications that government-owned local lenders that are shielded from market forces incur unrealized profitability potential, which in turn also benefits SMEs when released after the elimination of political frictions.

The upper-right graph reveals that more intensive users of savings bank loans invest significantly more after a forced savings bank mergers in the firm's region. This effect is long-lived, exhibiting a significantly positive response during the entire three-year spell after the merger. In economic terms, firms that borrow 50% of their loans from a savings bank increase their investments by around 50% in the years after a forced merger. Thus, this result corroborates the notion that county reforms unleash the potential in the local financial sector that was held back by additional frictions associated with fragmented local governments' interests of many counties. The results indicate that post-reform merged savings banks lend more to industry customers at a lower cost of credit, which is channeled by these corporations into additional investment in fixed assets.

The first two graphs of Figure 5 show that savings bank mergers due to reform are beneficial for connected firms. Forced consolidation seems to increase the supply of resources by banks that fuel investment by non-financial firms (Amiti and Weinstein, 2018). Significant differential effects thus indicate that the elimination of political barriers to bank consolidation in Germany also sparked meaningful real economic spillovers.

The lower-left graph of Figure 5 signals mildly positive employment effects in the range of 1% to 2% for the period three years after the mergers. More extended adjustment responses are commensurate with the notion of labor market frictions that are more binding compared to physical capital markets, for example, because of more restrictive labor laws that limit the ability of corporations to adjust wages downward or to layoff staff in economic downturns. The lower-right graph finally shows that these real expansionary effects are, at the same time, not associated with any significant effects on firms' leverage

ratios.

In sum, the factor market for physical capital – and with some delay, also labor markets – respond significantly positive to improved local financial market development. In contrast, we find no support for concerns of larger banks fueling an over-indebtedness of local firms. Thus, reforms that force savings banks into mergers appear to be beneficial because connected firms can increase investments and employment due to lower financing costs. At the same time, these real expansionary effects do not increase firms' leverage ratios in the years after the mergers.

## VI. Conclusion

We show that implicit political consolidation barriers are a cause for notoriously poor profitability in the banking industry. These frictions explain why too few bank mergers occur, which fuels excess capacities and depresses profits after the Great Financial Crisis in many banking markets. To identify any causal effect of consolidation barriers on frail profitability due to subdued merger dynamics, we exploit a quasi-random shock that forces government-owned savings banks in Germany to merge between 1993 and 2015, but leaves otherwise comparable cooperative banks with the choice to do so – or not.

Empirical results confirm that savings bank profitability increased significantly relative to cooperative banks in reformed and non-reformed counties. Return on equity increased by 5 to 6 percentage points up to 8 years after forced mergers, which is substantial in light of mean profitability on the order of 8 percentage points. These improvements are associated with a reduction in net equity, but no contraction of core capital ratios. Related, the total effect of reforms on non-performing loans is negative. Overall, risk profile responses of banks that are forced to merge do not spark financial instability concerns, whereas profitability gains are both statistically and economically substantial.

The primary mechanism of how forced mergers enhance profitability is increased profits due to lower operating costs. The latter reflects significantly lower interest expenditures rather than large-scale layoffs of staff or branch closures. Other operating costs, such as IT and other non-staff expenses, contribute to improved profitability, too. We find no evidence of rent-seeking and market power.

Detailed non-financial firm data of savings bank customers further shows that affected banks increase their corporate lending. SME connected to banks that are forced to merge exhibit lower external financing costs. These incumbent corporate borrowers also invest more and employ more staff after forced savings bank mergers.

In sum, our results suggest that tearing down (politically motivated) consolidation barriers unleashes not only the profitability potential of banks left idle before such consolidation shocks but also enhances non-financial firm performance.

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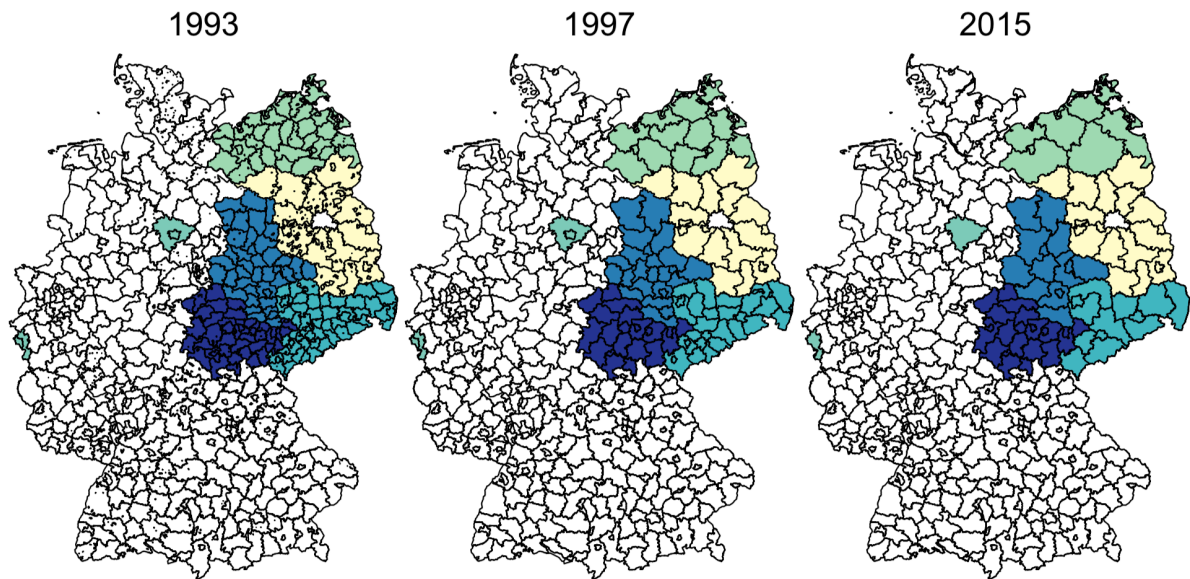
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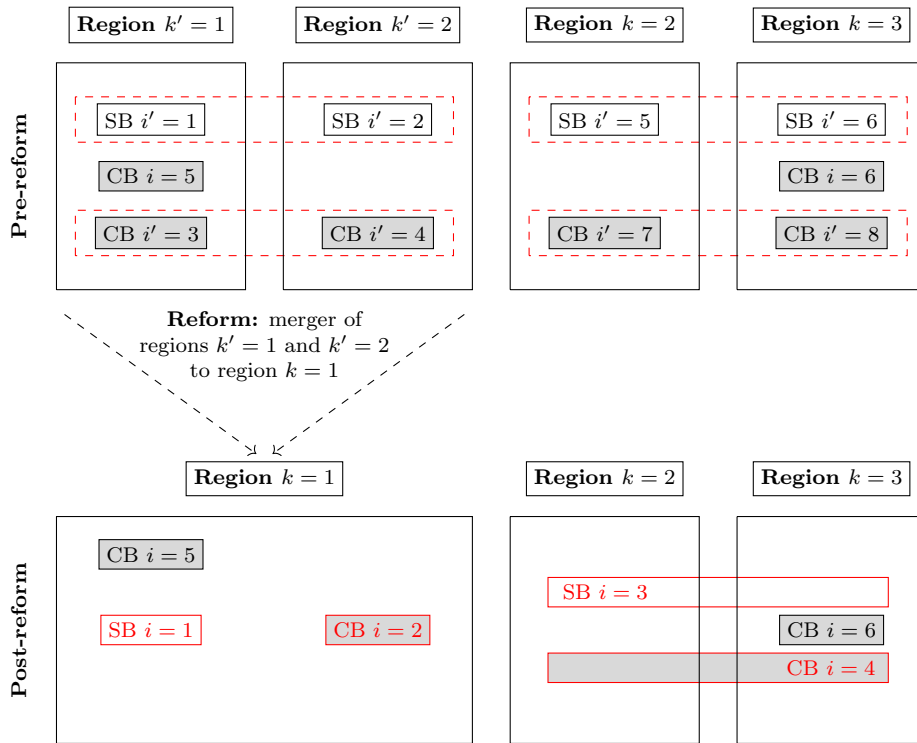
## Figures

Figure 1: Illustration of county unifications in Germany over time.



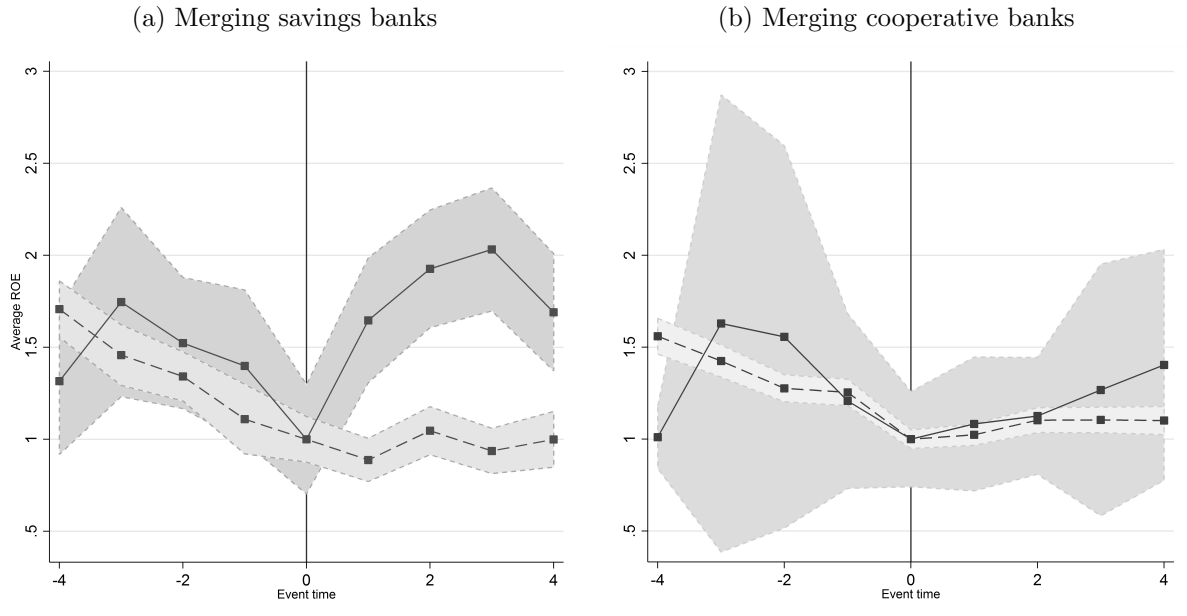
*Notes:* This figure shows the map of all counties (“Kreise”) in Germany in three respective years that span the estimation sample: 1993, 1997, and 2015. The evolution of regional vividly illustrates the sizeable contraction in sub-federal jurisdictions that act as owners of local savings banks. We assign different colors to each federal state in East Germany and dye the two regions in West Germany where county reforms happened, too, as well.

Figure 2: Identification illustrated – county reforms and bank mergers.



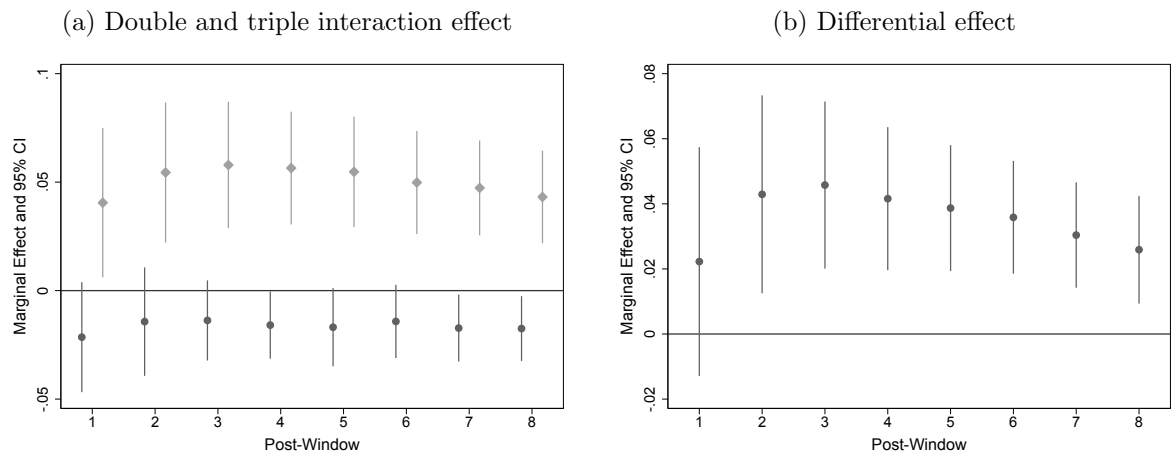
*Notes:* This figure shows savings banks (white squares) and cooperative banks (gray squares). The banks are active in regions  $k' = 1, k' = 2, k = 2, k = 3$  before a regional reform. Through a regional reform, the two regions  $k' = 1, 2$  merge to region  $k = 1$ , whereas the regions  $k = 2, 3$  are not reformed. The savings banks  $i' = 1, 2$  and cooperative banks  $i' = 3, 4$  merge into savings bank  $i = 3$  and cooperative bank  $i = 4$  in the non-reforming regions. However, the savings banks  $i' = 5, 6$  and cooperative banks  $i' = 7, 8$  merge into savings bank  $i = 3$  and cooperative bank  $i = 4$  in the reforming regions. The dashed areas that span around the savings and cooperative banks before the regional reform indicated that for the analysis, the banks are synthetically combined already before their mergers. The two cooperative banks  $i = 5, 6$  active in reforming region  $k' = 1$  and non-reforming region  $k = 2$  do not merge.

Figure 3: Bank profitability around merger events by ownership and treatment status.



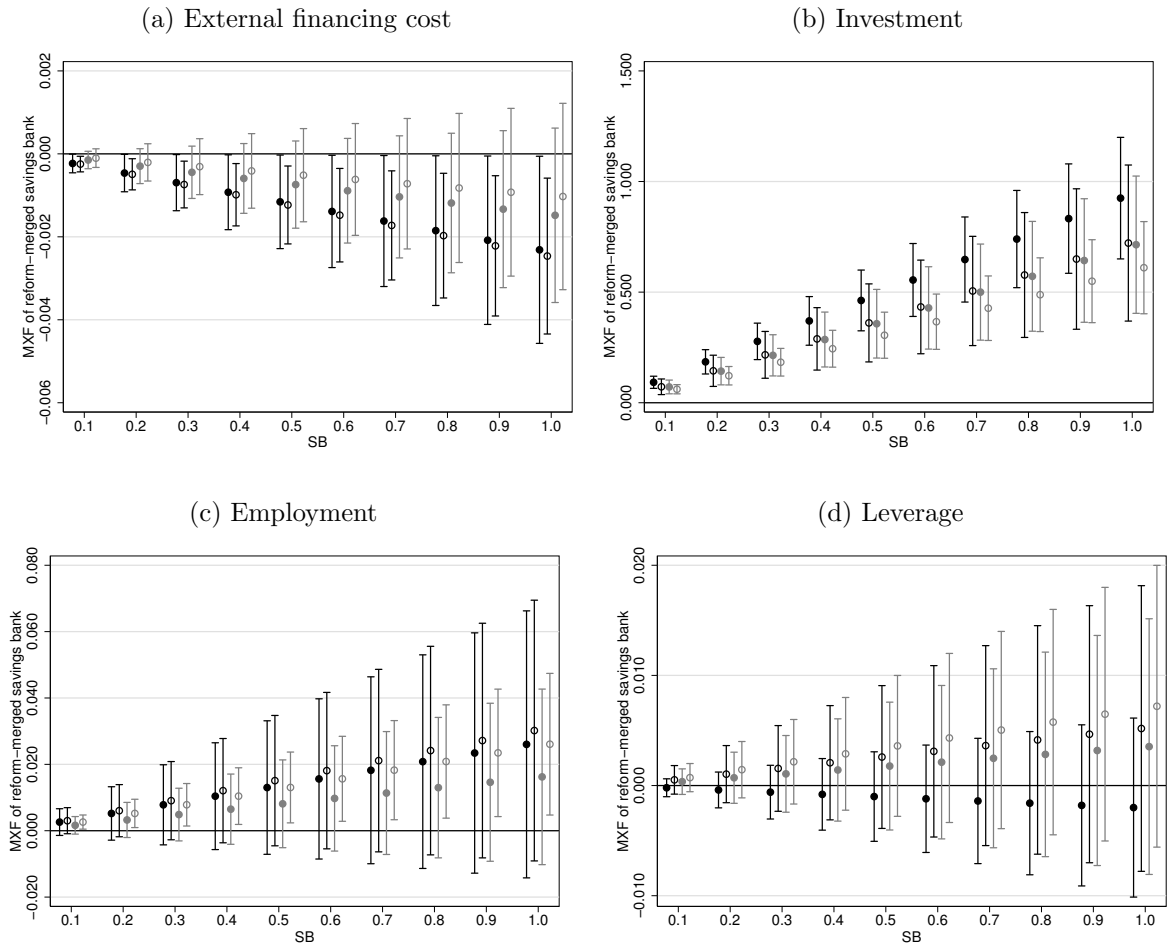
Notes: This figure shows average return on gross equity (lines)  $\pm 2$  standard errors (shaded area) in the 4 years before and after a merger in event time for the sample of merging banks by ownership status. The return on gross equity is rescaled to 1 at event time 0. The solid line represents treated banks, and the dashed line depicts non-treated banks.

Figure 4: Long-term effects on profitability



Notes: This figure shows coefficients and 95% confidence intervals of the effect of reform on merging savings banks for different time windows (0-8). The left graph displays the double and triple interaction effect, that is,  $\beta_3$  (dark gray) and  $\beta_6$  (light gray) in Equation (1). The right graph shows the differential effect of reform on the effect of merging for savings banks, that is,  $\beta_3 + \beta_6$  in Equation (1).

Figure 5: Real effects of forced savings bank mergers



*Notes:* The graphs depict the marginal effects of a forced savings bank merger (in a region) on firm outcomes (of firms in that region) conditional on the firms' share of savings banks' loans to total loans. The dots represents the marginal effects and the solid line the 95% confidence interval. We show the effects for shares of savings banks' loans between 0.1 and 1. For each level, we show four marginal effects: first, the marginal effect from the contemporaneous year (solid black dot); second, the marginal effect from the contemporaneous and the subsequent year (black, unfilled dot); third, the marginal effect from the contemporaneous and the subsequent two years (solid gray dot); and fourth, the marginal effect from the contemporaneous and the subsequent three years (gray, unfilled dot). We calculate the effects from regressions of Equation (2) and provide the detailed results in Table AVIII.

## Tables

Table I: Frequency distribution of banks and M&A transactions over years according to treatment and ownership status

	Observations				Banks	Transactions			
	Savings		Cooperatives		Total	Savings		Cooperatives	
	Non Treated	Treated	Non Treated	Treated		Non Treated	Treated	Non Treated	Treated
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
1993						13	2	74	2
1994	26	18	239	7	290	7	11	62	7
1995	37	18	322	10	387	1	19	43	8
1996	43	31	362	12	448	6	6	62	4
1997	56	37	389	14	496	6	1	68	0
1998	57	35	361	4	457	4	0	110	0
1999	67	25	343	0	435	11	0	126	0
2000	73	6	321	0	400	15	0	175	0
2001	83	2	408	1	494	19	0	125	0
2002	85	1	420	2	508	17	0	102	0
2003	75	0	412	0	487	27	1	83	2
2004	84	5	402	2	493	13	0	53	0
2005	74	6	346	2	428	14	0	42	0
2006	68	7	285	3	363	7	0	31	0
2007	58	8	231	3	300	4	0	21	0
2008	43	3	175	2	223	2	4	33	0
2009	35	5	152	1	193	5	2	36	1
2010	26	7	165	2	200	1	1	17	1
2011	19	8	162	3	192	3	0	17	0
2012	16	8	143	3	170	2	0	18	0
2013	14	3	122	2	141	4	1	19	1
2014	12	2	90	1	105	1	0	29	0
2015	9	1	80	0	90	3	0	22	0
Total	1,060	236	5,930	74	7,300	185	48	1,368	26

*Notes:* This table reports observations, number of banks, and number of M&A transactions in each year for the sample of merging banks according to treatment and ownership status. In Columns (1) to (4), observations of synthetic or original banks are counted. In Column (5), observations are summed up per year, giving the number of banks (original and synthetic) each year. In Columns (6) to (9), mergers are counted in the year in which they occurred.

Table II: Pre-merger tests for return on gross equity

	Untreated by Reform (1)	Treated by Reform (2)	Diff. in Treatment (3)	Untreated by Reform (4)	Treated by Reform (5)	Diff. in Treatment (6)
	<i>Levels</i>			<i>First-Differences</i>		
Savings	0.075 (0.057)	0.058 (0.045)	0.016 (0.019)	-0.010 (0.045)	-0.017 (0.055)	0.007 (0.368)
Cooperative	0.080 (0.063)	0.068 (0.050)	0.011 (0.325)	-0.004 (0.052)	0.007 (0.055)	-0.012 (0.364)
Diff. in Ownership	0.005 (0.087)	0.010 (0.448)	-0.005 (0.707)	0.006 (0.016)	0.024 (0.104)	-0.019 (0.195)

*Notes:* This table reports the summary statistics for return on equity by ownership and treatment in the pre-merger period of merging banks. Columns (1), (2), (4), and (5) present the mean and standard deviation in parentheses. Columns (3) and (6) report the difference in means and the p-value of a difference-in-means test in parentheses.

Table III: Summary statistics of explanatory variables

	Savings			Cooperative			Diff.	Diff.	Diff.
	NT (1)	T (2)	Diff. (3)	NT (4)	T (5)	Diff. (6)	NT (7)	T (8)	Diff. (9)
<i>Levels</i>									
Equity	0.046 (0.009)	0.039 (0.009)	0.007 (0.000)	0.053 (0.011)	0.048 (0.009)	0.005 (0.016)	-0.008 (0.000)	-0.009 (0.000)	-0.002 (0.445)
LLP	0.009 (0.007)	0.024 (0.014)	-0.016 (0.000)	0.007 (0.009)	0.010 (0.007)	-0.003 (0.070)	0.001 (0.000)	0.014 (0.000)	0.013 (0.000)
CIR	0.669 (0.068)	0.630 (0.068)	0.039 (0.000)	0.739 (0.139)	0.737 (0.080)	0.002 (0.900)	-0.070 (0.000)	-0.107 (0.000)	-0.037 (0.067)
Liquidity	0.043 (0.024)	0.067 (0.022)	-0.023 (0.000)	0.064 (0.028)	0.097 (0.028)	-0.033 (0.000)	-0.021 (0.000)	-0.031 (0.000)	-0.010 (0.151)
Loans	0.607 (0.107)	0.365 (0.093)	0.242 (0.000)	0.596 (0.093)	0.415 (0.120)	0.180 (0.000)	0.012 (0.030)	-0.050 (0.105)	-0.062 (0.038)
NII	0.172 (0.034)	0.177 (0.052)	-0.005 (0.481)	0.184 (0.058)	0.232 (0.074)	-0.048 (0.009)	-0.012 (0.000)	-0.055 (0.005)	-0.043 (0.015)
Size	4.052 (1.104)	3.509 (0.973)	0.542 (0.000)	3.833 (1.091)	3.850 (1.089)	-0.017 (0.946)	0.218 (0.000)	-0.341 (0.230)	-0.559 (0.044)
Log(GDP)	8.594 (0.902)	8.161 (0.667)	0.433 (0.000)	8.405 (0.778)	8.467 (0.818)	-0.062 (0.740)	0.190 (0.000)	-0.306 (0.146)	-0.495 (0.016)
<i>First-Differences</i>									
Equity	0.001 (0.002)	0.000 (0.002)	0.000 (0.137)	0.001 (0.002)	0.000 (0.003)	0.001 (0.332)	-0.000 (0.000)	-0.000 (0.636)	0.000 (0.841)
LLP	0.000 (0.007)	0.004 (0.015)	-0.003 (0.102)	-0.000 (0.009)	-0.002 (0.009)	0.002 (0.300)	0.000 (0.260)	0.006 (0.040)	0.006 (0.049)
CIR	0.007 (0.057)	-0.031 (0.094)	0.039 (0.005)	0.004 (0.141)	-0.027 (0.058)	0.030 (0.033)	0.004 (0.356)	-0.005 (0.794)	-0.008 (0.648)
Liquidity	0.002 (0.019)	-0.003 (0.020)	0.005 (0.119)	0.000 (0.024)	-0.007 (0.033)	0.007 (0.338)	0.001 (0.152)	0.004 (0.602)	0.003 (0.723)
Loans	0.001 (0.019)	0.009 (0.023)	-0.008 (0.022)	0.002 (0.023)	0.010 (0.024)	-0.008 (0.156)	-0.001 (0.193)	-0.002 (0.811)	-0.000 (0.969)
NII	0.005 (0.017)	0.007 (0.016)	-0.002 (0.382)	0.006 (0.045)	-0.001 (0.025)	0.006 (0.271)	-0.000 (0.759)	0.008 (0.188)	0.008 (0.156)
Size	-0.002 (0.213)	-0.057 (0.305)	0.054 (0.210)	-0.002 (0.188)	0.050 (0.394)	-0.052 (0.565)	-0.000 (0.966)	-0.107 (0.284)	-0.106 (0.269)
Log(GDP)	0.020 (0.033)	0.073 (0.065)	-0.054 (0.000)	0.027 (0.035)	0.062 (0.072)	-0.035 (0.045)	-0.007 (0.000)	0.012 (0.530)	0.019 (0.304)

*Notes:* This table reports the summary statistics of explanatory variables by ownership and treatment in the period before the merger. Columns (1), (2), (4), and (5) present the means and standard-deviation in parentheses by treatment and ownership. Columns (3) and (6) report the difference in means by treatment with p-value of t-test in parentheses within each banking sector. Columns (7) and (8) report the difference in means by ownership with p-value of t-test in parentheses within treatment status. Column (9) presents the difference-in-differences with p-value of t-test in parentheses. Equity, loan loss provisions (LLP), liquidity, and loans are defined as ratios to total assets. Non-interest income (NII) is defined as the ratio relative to interest-bearing assets. Size is a categorical variable indicating the quintile of the banking groups size distribution in terms of total assets. Cost-to-income ratio (CIR) is defined as administrative costs to total income. L(GDP) is the logarithm of GDP at the county level of the bank's headquarters.



Table IV: Baseline results: Effect of forced mergers on RoE

	Merging Reformed (1)	Merging (2)	Incl. Non-merging (3)
Merger	0.001 (0.002)	-0.003* (0.001)	0.000 (0.001)
Reform	0.011* (0.007)	0.007 (0.007)	-0.003 (0.007)
Merger*Reform	-0.024*** (0.008)	-0.016** (0.008)	-0.016** (0.008)
Merger*SB	-0.014** (0.006)	-0.014*** (0.004)	-0.011*** (0.003)
Reform*SB	-0.006 (0.013)	-0.008 (0.012)	0.005 (0.008)
Merger*Reform*SB	0.057*** (0.015)	0.056*** (0.013)	0.038*** (0.011)
Observations	2,441	7,300	20,893
Banks	291	788	1,438
Savings Banks	85	163	414
Cooperative Banks	206	625	1,024
Treated Deals	74	74	74
Non-treated Deals	466	1,553	1,553
Mean	0.079	0.078	0.083
Median	0.075	0.078	0.078
Standard Deviation	0.056	0.062	0.067
Bank & County Controls	yes	yes	yes
Bank, Year-State FE	yes	yes	yes
R-squared (within)	0.415	0.324	0.322

*Notes:* Clustered standard errors at the bank level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Difference-in-differences estimation with a 4-year event window (pre- and post-merger), where all available observations within the window are included. Merger is a dummy indicating the post-period. Reform is a dummy indicating the treatment status constant over event time for any transaction. In Column (1), only banks merging in Eastern Germany, Lower Saxony, and North-Rhine-Westphalia are included. In Column (2), all merging banks are included. In Column (3), all banks are included, and the treatment status of the Reform dummy lasts 8 years before and after a reform for non-merging banks. Bank controls are lagged by one year and comprise LLP, CIR, liquidity, loans, NII, size, and L(GDP) at the county level. Equity is excluded due to collinearity.

Table V: Reform effects on equity, profits, and cost

	L(Net Eq) (1)	L(Profit) (2)	L(Total Rev) (3)	L(Total Cost) (4)	L(Int Cost) (5)	L(Other Cost) (6)
Merger	-0.006* (0.004)	-0.102 (0.091)	-0.007** (0.004)	-0.005 (0.004)	-0.015*** (0.005)	0.005 (0.027)
Reform	0.037 (0.024)	0.971 (0.916)	0.022 (0.032)	0.017 (0.031)	0.019 (0.037)	-0.251* (0.152)
Merger*Reform	0.026 (0.023)	-0.174 (0.856)	0.043 (0.030)	0.049 (0.030)	0.114** (0.046)	0.293 (0.180)
Merger*SB	-0.014 (0.010)	-0.319 (0.203)	-0.032*** (0.008)	-0.014 (0.009)	0.027** (0.011)	0.048 (0.050)
Reform*SB	-0.039 (0.046)	-2.749** (1.249)	-0.094** (0.038)	-0.071* (0.039)	-0.147*** (0.053)	0.174 (0.182)
Merger*Reform*SB	-0.086** (0.034)	3.285*** (1.223)	-0.027 (0.038)	-0.077** (0.038)	-0.134** (0.055)	-0.449** (0.200)
Observations	7,300	7,300	7,300	7,300	7,300	7,300
Banks	788	788	788	788	788	788
Mean	17.32	14.26	17.57	17.48	16.72	12.46
Median	17.25	14.99	17.49	17.39	16.62	12.45
Standard Deviation	1.08	3.6	1.08	1.08	1.14	1.78
Bank & County Controls	yes	yes	yes	yes	yes	yes
Bank, Year-State FE	yes	yes	yes	yes	yes	yes
R-squared (within)	0.818	0.150	0.420	0.549	0.831	0.300

*Notes:* Clustered standard errors at the bank level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Difference-in-differences estimation with a 4-year event window (pre- and post-merger), where all available observations within the window are included. Merger is a dummy indicating the post-period. Reform is a dummy indicating the treatment status constant over event time. Controls are lagged by one year and comprise LLP, CIR, liquidity, loans, NII, size, and L(GDP). The dependent variables are logarithms and defined as follows: *Roe* is return over gross equity. *Net Eq* is nominal equity plus retained earnings. *Profit* is profit before taxes. *Total Rev* is total revenue, and *Total Cost* is total costs.

Table VI: Reform effects on financial stability of merging banks

	L(zscore) (1)	SD(RoA) (2)	Tier1 (3)	LLP (4)	NPL (5)
Merger	0.014 (0.033)	-0.000 (0.000)	0.000** (0.000)	0.000 (0.000)	0.000 (0.001)
Reform	0.460 (0.300)	-0.000 (0.001)	-0.001 (0.002)	-0.004 (0.003)	-0.046** (0.023)
Merger*Reform	-0.123 (0.274)	-0.000 (0.000)	-0.001 (0.002)	0.006* (0.004)	-0.011 (0.017)
Merger*SB	0.285*** (0.088)	-0.000** (0.000)	0.001** (0.000)	-0.001 (0.000)	0.001 (0.002)
Reform*SB	-0.197 (0.333)	-0.001 (0.001)	0.002 (0.002)	0.008** (0.004)	0.034 (0.025)
Merger*Reform*SB	-0.187 (0.292)	0.001** (0.001)	-0.003 (0.002)	-0.012*** (0.004)	0.030* (0.018)
Observations	7,206	7,206	7,300	7,300	5,153
Banks	788	788	788	788	748
Mean	3.65	0.00	0.05	0.01	0.06
Median	3.60	0.00	0.05	0.01	0.05
Standard Deviation	0.84	0.00	0.01	0.01	0.05
Bank & County Controls	yes	yes	yes	yes	yes
Bank, Year-State FE	yes	yes	yes	yes	yes
R-squared (within)	0.127	0.169	0.751	0.235	0.426

*Notes:* Clustered standard errors at the bank level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Lagged covariates are L(GDP) at the county level and CIR, liquidity, NII, loans, and size at the bank level. In Columns (4) to (5), equity is added as a control, whereas in Columns (3) to (5), RoA is used. LLP is excluded as a control due to endogeneity. The dependent variables are the following: *zscore* is defined as return on assets plus the *Tier 1* ratio over *SD(RoA)*. *SD(RoA)* is the standard deviation of return on assets calculated with a rolling window of three years, which results in a decrease in observations in Column (1) and (2). *Tier 1* is the ratio of regulatory tier 1 equity to total assets. *LLP* are loan-loss provisions. *NPL* are non-performing loans over total loans. *NPL* are available from 1999-2015, which causes the decrease in observations and reduces the number of treated deals to 39 and the number of non-treated deals to 1,245.

Table VII: Reform effects on efficiency of merging banks

	Branch	Empl	Empl/ Branch	Wages/ Empl	CIR
	(1)	(2)	(3)	(4)	(5)
Merger	-0.003 (0.003)	0.008 (0.005)	-0.218 (0.441)	0.001 (0.001)	-0.009*** (0.003)
Reform	-0.011 (0.062)	0.001 (0.010)	1.102 (1.750)	-0.002 (0.002)	-0.019 (0.014)
Merger*Reform	0.035 (0.041)	-0.017 (0.012)	-1.040 (1.659)	-0.008** (0.004)	0.004 (0.020)
Merger*SB	0.031*** (0.006)	-0.017* (0.009)	19.527** (9.880)	-0.001 (0.001)	0.026*** (0.005)
Reform*SB	-0.084 (0.059)	-0.021* (0.012)	8.103* (4.557)	0.007* (0.004)	0.035* (0.019)
Merger*Reform*SB	0.007 (0.045)	0.050*** (0.015)	-18.130* (9.475)	0.008* (0.004)	-0.021 (0.024)
Observations	6,958	7,228	6,958	7,228	7,300
Banks	788	788	788	788	788
Mean	0.43	0.3	10.5	0.11	0.73
Median	0.38	0.29	8.11	0.07	0.71
Standard Deviation	0.27	0.08	19.22	0.13	0.13
Bank & County Controls	yes	yes	yes	yes	yes
Bank, Year-State FE	yes	yes	yes	yes	yes
R-squared (within)	0.127	0.169	0.751	0.235	0.426

*Notes:* Clustered standard errors at the bank level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Lagged covariates are L(GDP) at the county level and equity, LLP, RoA, liquidity, NII, loans, and size at the bank level. In Columns (1) to (4), CIR is added as a control. Dependent variables are as follows. *Branch* is the ratio of number of branches to total assets in millions. *Branch* is available from 1993-2012, resulting in a decrease in observations in Columns (1) and (3). *Empl* is the ratio of number of employees over total assets in millions. *Empl* is missing for many banks in 2015, resulting in a decrease in observations in Columns (2) and (4). *Empl/Branch* is the average number of employees per branch. *Wages/Empl* is the average personnel costs spend per employee. *CIR* is the cost-to-income ratio.

Table VIII: Reform effects on market power of merging banks

	NIM (1)	Int. earned (2)	Int. paid (3)	L(IBA) (4)	Market share (5)
Merger	0.000*** (0.000)	0.000*** (0.000)	0.000 (0.000)	-0.011*** (0.004)	-0.000 (0.001)
Reform	-0.001 (0.001)	-0.001 (0.001)	-0.000 (0.001)	0.034 (0.029)	0.015 (0.017)
Merger*Reform	-0.001 (0.001)	0.001 (0.001)	0.001* (0.001)	0.060* (0.032)	0.013 (0.014)
Merger*SB	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.009)	0.002 (0.005)
Reform*SB	0.002 (0.001)	0.000 (0.001)	-0.002* (0.001)	-0.102*** (0.039)	-0.142*** (0.046)
Merger*Reform*SB	0.003*** (0.001)	0.003** (0.001)	-0.000 (0.001)	-0.101*** (0.039)	-0.004 (0.031)
Observations	7,300	7,300	7,300	7,300	6,965
Banks	788	788	788	788	788
Mean	0.03	0.06	0.03	20.21	0.15
Median	0.03	0.06	0.03	20.13	0.08
Standard Deviation	0.01	0.01	0.01	1.1	0.18
Bank & County Controls	yes	yes	yes	yes	yes
Bank, Year-State FE	yes	yes	yes	yes	yes
R-squared (within)	0.687	0.949	0.949	0.602	0.194

*Notes:* Clustered standard errors at the bank level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Lagged covariates are L(GDP) at the county level and equity, LLP, CIR, liquidity, and size at the bank level. In Column (5), RoA and NII are added as control variables. Dependent variables are the following. *NIM* is the net-interest margin, defined as *Int earned* minus *Int paid* over *IBA*. *Int earned* are interest revenues over *IBA*. *Int paid* are interest costs over total income. *IBA* are interest bearing assets consisting of loans to customers and banks and securities. *Market share* is the market share of loans to customers of a bank within its business area. Business area is defined by aggregating all counties where a bank has branches. Total loans on the bank level are split among counties according to the share of own branches located in that county. Branch data are available from 1993-2012, resulting in a decrease in observations in Column (5).

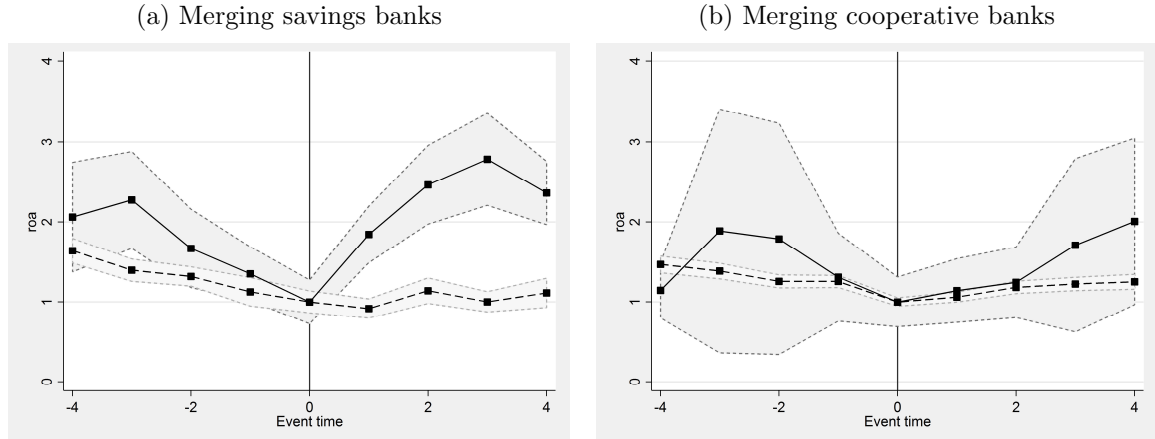
Table IX: Reform effects on deposits and credit provision of merging banks

	L(Deposits)	Public Loans			Private Sector Loans			L(Real estate)
	(1)	L(Municipal)	L(State)	L(Consumer)	L(Comm)	L(Industrial)	L(Agri)	(8)
Merger	0.001 (0.001)	-0.000 (0.000)	-0.000 (0.000)	0.007*** (0.002)	0.011*** (0.002)	0.004*** (0.001)	0.002*** (0.001)	0.004* (0.002)
Reform	-0.001 (0.006)	0.010 (0.008)	0.002 (0.010)	-0.011 (0.014)	-0.007 (0.025)	0.008 (0.005)	0.008 (0.006)	-0.003 (0.009)
Merger*Reform	-0.003 (0.009)	-0.004 (0.008)	0.001 (0.009)	0.003 (0.015)	0.015 (0.017)	-0.009* (0.005)	-0.006 (0.006)	-0.001 (0.011)
Merger*SB	0.013*** (0.004)	0.005*** (0.002)	-0.001 (0.001)	0.014*** (0.004)	0.009 (0.006)	0.004** (0.002)	-0.001 (0.002)	-0.005 (0.005)
Reform*SB	0.032** (0.016)	-0.024 (0.017)	0.018 (0.014)	0.038** (0.019)	0.016 (0.025)	-0.023*** (0.008)	-0.007 (0.010)	0.002 (0.014)
Merger*Reform*SB	-0.005 (0.012)	0.009 (0.011)	0.006 (0.010)	-0.005 (0.015)	0.012 (0.024)	0.018*** (0.007)	0.009 (0.007)	0.012 (0.016)
Observations	7,300	7,300	7,300	7,300	7,300	7,300	7,300	7,300
Banks	788	788	788	788	788	788	788	788
Mean	0.74	0.02	0	0.13	0.23	0.06	0.04	0.12
Median	0.75	0.01	0	0.12	0.22	0.05	0.02	0.11
Standard Deviation	0.08	0.03	0.01	0.07	0.09	0.03	0.04	0.09
Bank & County Controls	yes	yes	yes	yes	yes	yes	yes	yes
Bank, Year-State FE	yes	yes	yes	yes	yes	yes	yes	yes
R-squared (within)	0.331	0.347	0.181	0.469	0.546	0.550	0.455	0.599

*Notes:* Clustered standard errors at the bank level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Lagged covariates are L(GDP) at the county level and equity, LLP, CIR, liquidity, and size at the bank level. In Column, (5) RoA and NII are added as control variables. The dependent variables are the following:  $L(Deposit)$  is the logarithm of deposits to costumers;  $L(Loans)$ , the logarithm of total loans to non-bank customers;  $L(Consumer)$ , the logarithm of loans to private households (excl. real estate);  $L(Comm)$ , the logarithm of loans to firms and private businesses (excl. the industrial and agricultural sector);  $L(Industrial)$ , the logarithm of loans to firms in the industrial sector;  $L(Agri)$ , the logarithm of loans to firms in the agricultural sector;  $L(Real Estate)$ , the logarithm of loans to private households for the purpose of real estate;  $L(Municipal)$ , the logarithm of loans to the public sector on the municipal level; and  $L(State)$ , the logarithm of loans to the public sector on the state level.

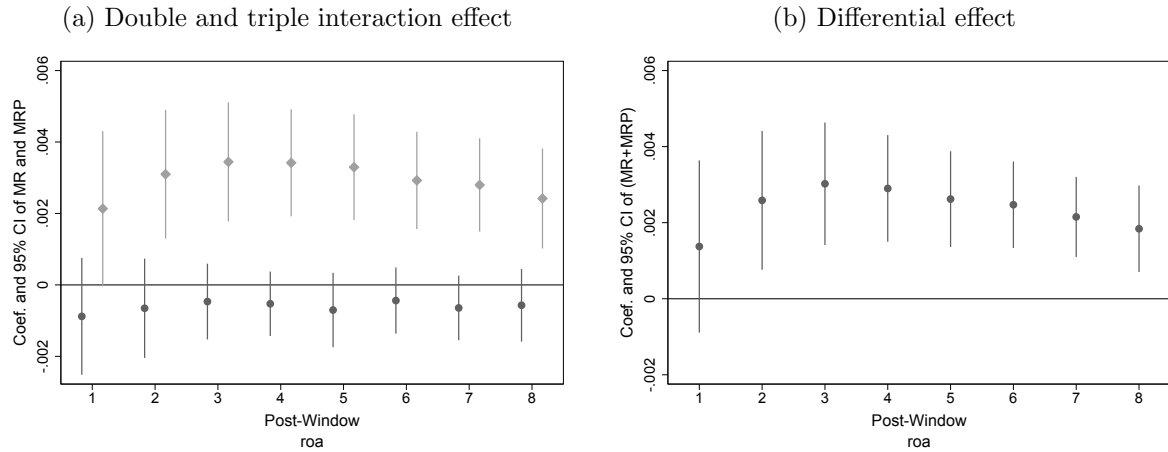
## Online Appendix

Figure A1: Bank profitability (RoA) around merger events by ownership and treatment status.



Notes: This figure shows average return on assets (lines)  $\pm 2$  standard errors (shaded area) in event time for the sample of merging banks by ownership status rescaled to 1 at event time 0. The solid line represents treated banks, and the dashed line depicts non-treated banks.

Figure A2: Long-term effects on profitability (RoA)



Notes: This figure shows coefficients and 95% confidence intervals of the effect of reform on merging savings banks for different time windows (0-8). The left graph displays the double and triple interaction effect, that is,  $\beta_3$  (dark gray) and  $\beta_6$  (light gray) in Equation (1). The right graph shows the differential effect of reform on the effect of merging for savings banks, that is,  $\beta_3 + \beta_6$  in Equation (1). The dependent variable used in these regressions is *Return on Assets*.

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Table AI: Overview of county reforms

Date	Federal State			Dead-line	Counties		Savings		Cooperatives	
					N	$\Delta$	N	$\Delta$	N	$\Delta$
12/06/1993	Brandenburg	<i>pre</i>	1992	2	44	-59%	30	-30%	36	-14%
		<i>post</i>	1995		18		21		31	
06/12/1994	Mecklenburg-Vorpommern	<i>pre</i>	1993	3	37	-51%	26	-38%	32	-19%
		<i>post</i>	1997		18		16		26	
07/01/1994	Saxony-Anhalt	<i>pre</i>	1993	3	40	-40%	36	-31%	41	-20%
		<i>post</i>	1997		24		25		33	
07/01/1994	Thuringia	<i>pre</i>	1993	-	40	-45%	33	-45%	50	-18%
		<i>post</i>	1996		22		18		41	
08/01/1994, 06/16/1996	Saxony	<i>pre</i>	1993	2-3	54	-46%	45	-47%	53	-15%
		<i>post</i>	1997		29		24		45	
11/01/2001	Lower Saxony	<i>pre</i>	2000	-	46	-2%	61	-20%	228	-32%
		<i>post</i>	2003		45		49		156	
07/01/2007	Saxony-Anhalt	<i>pre</i>	2006	2	24	-42%	22	-32%	17	0%
		<i>post</i>	2009		14		15		17	
08/01/2008	Saxony	<i>pre</i>	2007	-	29	-55%	15	0%	25	-4%
		<i>post</i>	2010		13		15		24	
10/21/2009	North-Rhine Westphalia	<i>pre</i>	2008	-	54	-2%	110	-2%	195	-7%
		<i>post</i>	2011		53		108		181	
09/04/2011	Mecklenburg-Vorpommern	<i>pre</i>	2010	-	18	-56%	10	0%	11	0%
		<i>post</i>	2013		8		10		11	

**Notes:** This table reports an overview of county-reforms since German reunification with the number of counties, savings and cooperative banks before and after the reform. Date refers to the date of enactment. The numbers of counties are presented before and after this date. Deadline states whether there was a deadline in years. Pre-year is the last year before a reform and post-year marks the year after the deadline expired or – if no deadline was given – two years after the reform. The numbers of banks are counted in these years. The reductions in counties and banks between respective pre- and post-years are given as percentages. In Saxony, most counties were reformed on the 1st of August, 1994. Lawsuits were filed, which resulted in three amendments to the original reform bill, the last of which was on the 16th of June, 1996. The ordinary deadline in Saxony was two years, but banks located in counties involved in the lawsuits were exempted.



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Table AII: Effect of forced mergers on RoE including county debt

	Merging Reformed (1)	Merging (2)	Incl. Non-merging (3)
Merger	0.001 (0.002)	-0.003* (0.002)	0.001 (0.001)
Reform	0.012* (0.007)	0.007 (0.007)	-0.003 (0.007)
Merger*Reform	-0.024*** (0.008)	-0.016** (0.008)	-0.016** (0.008)
Merger*SB	-0.014** (0.006)	-0.014*** (0.004)	-0.011*** (0.003)
Reform*SB	-0.006 (0.013)	-0.008 (0.012)	0.005 (0.008)
Merger*Reform*SB	0.058*** (0.015)	0.056*** (0.013)	0.037*** (0.011)
L(Debt)	0.001* (0.001)	0.003* (0.001)	0.000 (0.001)
Observations	2,441	7,300	20,893
Banks	291	788	1,438
Savings Banks	85	163	414
Cooperative Banks	206	625	1,024
Treated Deals	74	74	74
Non-treated Deals	466	1,553	1,553
Mean	0.079	0.078	0.083
Median	0.075	0.078	0.078
Standard Deviation	0.056	0.062	0.067
Bank & County Controls	yes	yes	yes
Bank, Year-State FE	yes	yes	yes
R-squared (within)	0.415	0.324	0.320

*Notes:* Clustered standard errors at the bank level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Difference-in-differences estimation with a 4-year event window (pre- and post-merger), where all available observations within the window are included. Merger is a dummy indicating the post-period. Reform is a dummy indicating the treatment status constant over event time for any transaction. In Column (1), only banks merging in Eastern Germany, Lower Saxony, and North-Rhine-Westphalia are included. In Column (2), all merging banks are included. In Column (3), all banks are included, and the treatment status of the Reform dummy lasts 8 years before and after a reform for non-merging banks. Bank controls are lagged by one year and comprise LLP, CIR, liquidity, loans, NII, size, L(GDP), and L(Debt) at the county level. Equity is excluded due to collinearity.

Table AIII: Robustness checks for return on gross equity

	Baseline (1)	Baseline RoNE (2)	Baseline RoA (3)	90s (4)	00s (5)	Excl. Distress (6)	Excl. Ties (7)	Cont. Counties (8)	Collapse Time Dim. (9)
SB									-0.015 (0.009)
Merger	-0.003* (0.001)	-0.004* (0.002)	-0.000 (0.000)	-0.002 (0.002)	-0.004* (0.002)	-0.002 (0.002)	-0.005** (0.002)	-0.010** (0.005)	-0.020** (0.009)
Reform	0.007 (0.007)	0.014 (0.010)	0.000 (0.000)	-0.003 (0.019)	0.003 (0.010)	0.002 (0.011)	0.002 (0.010)	0.004 (0.015)	
Merger*Reform	-0.016** (0.008)	-0.028** (0.013)	-0.001 (0.000)	-0.007 (0.017)	-0.006 (0.013)	-0.005 (0.010)	-0.012 (0.010)	-0.017 (0.021)	
Merger*SB	-0.014*** (0.004)	-0.021*** (0.006)	-0.001*** (0.000)	-0.013 (0.009)	-0.010** (0.005)	-0.021*** (0.004)	-0.012** (0.005)	-0.031 (0.026)	0.032*** (0.011)
Reform*SB	-0.008 (0.012)	-0.035* (0.020)	-0.001** (0.001)	0.001 (0.030)	-0.052*** (0.010)	-0.002 (0.017)	-0.006 (0.029)		
Merger*Reform*SB	0.056*** (0.013)	0.103*** (0.022)	0.003*** (0.001)	0.060*** (0.021)	-0.011 (0.013)	0.046*** (0.017)	0.078*** (0.017)	0.076** (0.036)	
Observations	7,300	7,300	7,300	2,513	4,787	4,220	5,428	485	310
Banks	788	788	788	632	724	501	591	63	67
Govern. Banks	163	163	163	124	128	123	121	19	43
Mutual Banks	625	625	625	508	596	378	470	44	24
Treated Deals	74	74	74	60	20	44	46	20	74
Non-treated Deals	1,553	1,553	1,553	801	1,162	800	1,173	90	0
Mean	0.078	0.11	0.006	0.089	0.067	0.085	0.08	0.062	0.061
Median	0.078	0.11	0.006	0.093	0.065	0.085	0.079	0.065	0.061
Standard Deviation	0.062	0.089	0.005	0.059	0.063	0.056	0.064	0.072	0.047
Bank & County Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Bank & Year*State FE	yes	yes	yes	yes	yes	yes	yes	no	no
R-squared (within/[overall])	0.324	0.326	0.326	0.354	0.260	0.403	0.328	0.467	[0.039]

Notes: Clustered standard errors at the bank level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Column (1) reproduces the baseline results. Column (2) specifies *net equity* instead of *gross equity* as the dependent variable. *Net Eq* is nominal equity plus retained earnings. In Column (3), the dependent variable is return on gross total assets. In Column (4), the sample period is from 1994 to 2000. In Column (5), the sample period is from 2001 to 2015. In Column (6), all banks that once reported a distress event are excluded. In Column (7), all banks with a ratio of loans to municipalities to total loans above their banking groups' average ratio are excluded. In Column (8), only banks on the borders between reformed and non-reformed states are included. Fixed effects for each neighboring county-pair are added. In Column (9), the residuals of a regression of RoE on reform treatment, year\*state fixed effects, and the main covariates are regressed on the post-dummy for treated deals only, following Bertrand et al. (2004). The controls are lagged by one year and comprise LLP, CIR, liquidity, loans, NII, and size at the bank level and L(GDP) at the county level.

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Table AIV: Placebo treatments for the effect on RoE

Rejection rate at 1%	at 5%	at 10%
0.013	0.069	0.114

*Notes:* This table reports the average rejection rates for 1,000 repetitions of placebo-treatments over the cross-section and time. In each repetition, *Reform* was randomly assigned to other mergers among all mergers including the actually treated tests  $H_0 : \beta_6 = 0$  using the baseline specification.

Table AV: Summary statistics of dependent variables by treatment and ownership status

<i>Levels</i>	Savings			Cooperatives			Diff.	Diff.	Diff.
	Non-T (1)	Treat (2)	Diff. (3)	Non-T (4)	Treat (5)	Diff. (6)	Non-T (7)	T (8)	Diff. (9)
<i>Equity Decomposition</i>									
L(Gross Eq)	19.166	18.585	0.581	17.347	17.352	-0.005	1.820	1.233	-0.586
	0.771	0.823	0.000	0.961	0.968	0.980	0.000	0.000	0.016
L(Net Eq)	18.665	18.038	0.628	17.037	17.029	0.008	1.629	1.009	-0.620
	0.780	0.743	0.000	0.912	0.978	0.972	0.000	0.000	0.010
L(Accruals)	17.036	16.248	0.789	15.292	15.432	-0.141	1.744	0.815	-0.929
	0.757	0.920	0.000	1.106	0.983	0.532	0.000	0.003	0.000
L(Other Eq)	17.387	17.287	0.100	13.762	11.529	2.233	3.625	5.758	2.133
	2.283	1.236	0.619	4.468	6.891	0.164	0.000	0.001	0.160
<i>Profit Decomposition</i>									
L(Profit)	15.941	13.503	2.437	13.952	13.873	0.079	1.989	-0.369	-2.358
	2.967	5.819	0.004	3.566	3.514	0.921	0.000	0.743	0.034
L(Total Rev)	19.021	18.514	0.507	17.265	17.333	-0.067	1.756	1.181	-0.575
	0.760	0.603	0.000	0.887	0.908	0.745	0.000	0.000	0.008
L(Total Cost)	18.931	18.431	0.500	17.170	17.246	-0.076	1.761	1.184	-0.577
	0.761	0.608	0.000	0.880	0.886	0.705	0.000	0.000	0.007
L(Op Cost)	18.829	18.262	0.566	17.087	17.121	-0.034	1.741	1.141	-0.600
	0.759	0.627	0.000	0.875	0.895	0.867	0.000	0.000	0.006
<i>Risk Channel</i>									
L(zscore)	3.217	3.165	0.053	3.364	3.652	-0.288	-0.147	-0.488	-0.341
	0.655	0.453	0.517	0.638	0.969	0.269	0.000	0.080	0.182
SD(RoA)	0.002	0.002	0.000	0.002	0.002	0.000	0.000	-0.000	-0.000
	0.002	0.001	0.400	0.002	0.002	0.920	0.651	0.935	0.877
Tier1	0.044	0.038	0.005	0.050	0.045	0.005	-0.006	-0.006	-0.001
	0.010	0.011	0.001	0.012	0.010	0.043	0.000	0.020	0.802
LLP	0.009	0.024	-0.016	0.007	0.010	-0.003	0.001	0.014	0.013
	0.007	0.014	0.000	0.009	0.007	0.070	0.000	0.000	0.000
NPL	0.063	0.100	-0.037	0.061	0.097	-0.036	0.002	0.002	0.000
	0.039	0.045	0.000	0.046	0.073	0.088	0.452	0.911	0.981
<i>Efficiency Channel</i>									
Branch	0.213	0.305	-0.092	0.480	0.656	-0.176	-0.268	-0.352	-0.084
	0.113	0.117	0.000	0.273	0.343	0.033	0.000	0.000	0.273
Empl	0.252	0.304	-0.052	0.305	0.359	-0.053	-0.053	-0.055	-0.002
	0.047	0.088	0.000	0.083	0.097	0.023	0.000	0.035	0.950
Empl/Branch	22.665	10.641	12.024	8.093	6.394	1.699	14.572	4.247	-10.325
	44.738	3.148	0.000	4.424	2.111	0.002	0.000	0.000	0.000
Wages/Empl	0.017	0.020	-0.003	0.128	0.087	0.041	-0.111	-0.067	0.043
	0.014	0.010	0.103	0.130	0.063	0.011	0.000	0.000	0.002
CIR	0.669	0.630	0.039	0.739	0.737	0.002	-0.070	-0.107	-0.037
	0.068	0.068	0.000	0.139	0.080	0.900	0.000	0.000	0.067
<i>Market Power Channel</i>									
NIM	0.024	0.031	-0.006	0.029	0.031	-0.002	-0.005	-0.000	0.005
	0.004	0.009	0.000	0.005	0.006	0.251	0.000	0.997	0.010
Int earned	0.060	0.061	-0.001	0.061	0.059	0.002	-0.001	0.002	0.003
	0.009	0.015	0.767	0.011	0.015	0.567	0.267	0.603	0.507
Int paid	0.036	0.030	0.006	0.032	0.028	0.004	0.004	0.002	-0.002
	0.007	0.009	0.000	0.009	0.010	0.136	0.000	0.440	0.407
L(IBA)	21.651	21.121	0.530	19.903	19.882	0.021	1.748	1.239	-0.509
	0.776	0.646	0.000	0.906	0.903	0.918	0.000	0.000	0.020
Market share	0.442	0.481	-0.039	0.081	0.091	-0.010	0.360	0.390	0.029
	0.210	0.210	0.202	0.061	0.044	0.327	0.000	0.000	0.354

continued.

<i>First-Differences</i>	Savings			Cooperatives			Diff.	Diff.	Diff.
	Non-T (1)	Treat (2)	Diff. (3)	Non-T (4)	Treat (5)	Diff. (6)	Non-T (7)	T (8)	Diff. (9)
<i>Equity Decomposition</i>									
L(Gross Eq)	0.056	0.092	-0.036	0.060	0.071	-0.011	-0.004	0.021	0.025
	0.071	0.113	0.028	0.058	0.087	0.592	0.230	0.406	0.310
L(Net Eq)	0.050	0.035	0.016	0.056	0.054	0.002	-0.006	-0.019	-0.014
	0.056	0.029	0.001	0.042	0.047	0.842	0.034	0.096	0.230
L(Accruals)	0.044	0.107	-0.063	0.044	0.057	-0.013	-0.000	0.050	0.050
	0.151	0.370	0.227	0.195	0.239	0.814	0.985	0.503	0.494
L(Other Eq)	0.068	1.406	-1.338	0.230	0.030	0.199	-0.162	1.375	1.537
	1.588	3.859	0.015	2.374	0.415	0.067	0.067	0.013	0.004
<i>Equity Decomposition</i>									
L(Profit)	-0.350	-1.423	1.073	-0.007	0.148	-0.154	-0.344	-1.570	-1.227
	2.970	6.879	0.266	3.081	0.638	0.332	0.024	0.106	0.201
L(Total Rev)	0.012	0.011	0.001	0.002	0.027	-0.025	0.010	-0.016	-0.025
	0.073	0.060	0.946	0.078	0.093	0.248	0.010	0.489	0.251
L(total Cost)	0.017	0.015	0.002	-0.000	0.013	-0.013	0.017	0.002	-0.015
	0.086	0.084	0.876	0.079	0.104	0.575	0.000	0.934	0.555
L(Op Cost)	0.011	-0.012	0.023	-0.001	-0.006	0.005	0.012	-0.007	-0.019
	0.073	0.076	0.038	0.060	0.061	0.744	0.001	0.707	0.281
<i>Risk Channel</i>									
L(zscore)	-0.058	-0.041	-0.018	-0.003	-0.066	0.063	-0.055	0.026	0.081
	0.430	0.458	0.832	0.466	0.414	0.580	0.018	0.852	0.550
SD(RoA)	0.000	0.000	-0.000	0.000	0.000	0.000	0.000	0.000	0.000
	0.001	0.001	0.718	0.001	0.001	0.753	0.079	0.344	0.621
Tier1	0.001	0.002	-0.001	0.002	0.001	0.001	-0.001	0.002	0.002
	0.003	0.003	0.003	0.003	0.004	0.439	0.000	0.179	0.045
LLP	0.000	0.004	-0.003	-0.000	-0.002	0.002	0.000	0.006	0.006
	0.007	0.015	0.102	0.009	0.009	0.300	0.260	0.040	0.049
NPL	0.001	-0.009	0.010	-0.002	-0.027	0.025	0.003	0.019	0.015
	0.013	0.020	0.009	0.024	0.033	0.013	0.002	0.065	0.098
<i>Efficiency Channel</i>									
Branch	-0.012	-0.017	0.004	-0.028	-0.058	0.031	0.015	0.042	0.026
	0.016	0.030	0.314	0.045	0.083	0.117	0.000	0.040	0.157
Empl	-0.010	-0.005	-0.005	-0.006	0.015	-0.021	-0.004	-0.020	-0.016
	0.016	0.054	0.543	0.063	0.109	0.435	0.008	0.476	0.550
Empl/Branch	2.067	0.249	1.818	0.208	0.303	-0.095	1.859	-0.054	-1.913
	14.005	1.450	0.009	1.758	0.998	0.702	0.005	0.871	0.009
Wages/Empl	-0.000	-0.001	0.000	-0.016	-0.066	0.051	0.016	0.066	0.050
	0.001	0.005	0.706	0.176	0.270	0.452	0.000	0.330	0.431
CIR	0.007	-0.031	0.039	0.004	-0.027	0.030	0.004	-0.005	-0.008
	0.057	0.094	0.005	0.141	0.058	0.033	0.356	0.794	0.648
<i>Market Power Channel</i>									
NIM	-0.001	-0.001	0.000	-0.001	-0.001	0.000	-0.000	0.000	0.000
	0.002	0.002	0.856	0.002	0.003	0.417	0.056	0.709	0.504
Int earned	-0.002	-0.004	0.002	-0.003	-0.004	0.002	0.000	-0.000	-0.001
	0.003	0.006	0.011	0.003	0.006	0.262	0.001	0.935	0.701
Int paid	-0.001	-0.003	0.002	-0.002	-0.003	0.001	0.001	-0.000	-0.001
	0.003	0.005	0.009	0.003	0.005	0.358	0.000	0.776	0.431
L(IBA)	0.035	0.053	-0.018	0.034	0.058	-0.024	0.001	-0.005	-0.006
	0.060	0.076	0.095	0.045	0.071	0.148	0.812	0.790	0.759
Market share	0.003	0.003	-0.000	-0.000	0.000	-0.001	0.003	0.003	-0.000
	0.025	0.049	0.953	0.007	0.007	0.647	0.005	0.657	0.966

*Notes:* This table reports the summary statistics of dependent variables in the pre-period by ownership and treatment status. Tier1, NPL, Branch, Empl, Salaries, and Admin are defined as ratios to total assets. NIM, I-Inc., and I-Cost are defined as ratios relative to interest-bearing assets. NI-Inc. and NI-Cost are defined relative to total income.

Table AVI: Frequency distribution of banks and M&A transactions over years according to treatment and ownership status for the full sample, including non-merging banks

	Non-Merging					Merging								
	Observations				Banks	Observations				Banks	Deals			
	Savings		Cooperatives		Total	Savings		Cooperatives		Total	Savings		Cooperatives	
	Non-T	Treat	Non-T	Treat		Non-T	Treat	Non-T	Treat		Non-T	Treat	Non-T	Treat
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	
1993											13	2	74	2
1994	204	6	342	15	567	26	18	239	7	290	7	11	62	7
1995	204	6	345	15	570	37	18	322	10	387	1	19	43	8
1996	204	6	345	15	570	43	31	362	12	448	6	6	62	4
1997	210	6	344	16	576	56	37	389	14	496	6	1	68	0
1998	210	6	343	16	575	57	35	361	4	457	4	0	110	0
1999	210	6	340	18	574	67	25	343	0	435	11	0	126	0
2000	209	7	339	19	574	73	6	321	0	400	15	0	175	0
2001	239	12	368	24	643	83	2	408	1	494	19	0	125	0
2002	239	12	367	25	643	85	1	420	2	508	17	0	102	0
2003	242	9	375	17	643	75	0	412	0	487	27	1	83	2
2004	242	9	375	17	643	84	5	402	2	493	13	0	53	0
2005	242	9	376	17	644	74	6	346	2	428	14	0	42	0
2006	242	9	374	17	642	68	7	285	3	363	7	0	31	0
2007	237	9	374	17	637	58	8	231	3	300	4	0	21	0
2008	237	9	374	17	637	43	3	175	2	223	2	4	33	0
2009	237	9	374	17	637	35	5	152	1	193	5	2	36	1
2010	240	6	377	14	637	26	7	165	2	200	1	1	17	1
2011	240	6	377	14	637	19	8	162	3	192	3	0	17	0
2012	240	6	377	14	637	16	8	143	3	170	2	0	18	0
2013	240	6	377	14	637	14	3	122	2	141	4	1	19	1
2014	240	6	376	14	636	12	2	90	1	105	1	0	29	0
2015	240	6	374	14	634	9	1	80	0	90	3	0	22	0
Total	5,048	166	8,013	366	13,593	1,060	236	5,930	74	7,300	185	48	1,368	26

*Notes:* This table reports the observations, number of banks, and deals each year for the full sample of banks according to treatment and ownership status. In Columns (1) to (4) and (6) to (9), observations of synthetic or original banks are counted. In Columns (5) and (10), observations are summed up per year. In Columns (11) to (14), mergers are counted in the year in which they occurred.

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Table AVII: Frequency distribution of banks and M&A transactions over years according to treatment and ownership status for the sample merging banks in reformed states only

	Observations				Banks	Deals			
	Savings		Cooperatives		Total	Savings		Cooperatives	
	Non Treated	Treated	Non Treated	Treated		Non Treated	Treated	Non Treated	Treated
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
1993						7	2	13	2
1994	12	18	48	7	85	0	11	8	7
1995	10	18	63	10	101	0	19	6	8
1996	8	31	71	12	122	3	6	10	4
1997	10	37	73	14	134	4	1	18	0
1998	13	35	77	4	129			31	0
1999	22	25	82	0	129	3	0	33	0
2000	28	6	81	0	115	5	0	59	0
2001	40	2	138	1	181	4	0	44	0
2002	32	1	146	2	181	10	0	31	0
2003	28	0	139	0	167	15	1	31	2
2004	34	5	143	2	184	8	0	17	0
2005	33	6	119	2	160	7	0	14	0
2006	34	7	100	3	144	3	0	6	0
2007	29	8	80	3	120	1	0	8	0
2008	20	3	61	2	86	0	4	10	0
2009	15	5	58	1	79	2	2	7	1
2010	10	7	52	2	71	0	1	7	1
2011	7	8	50	3	68	1	0	8	0
2012	5	8	44	3	60	2	0	6	0
2013	6	3	37	2	48	1	1	7	1
2014	5	2	34	1	42			9	0
2015	3	1	31	0	35	2	0	5	0
Total	404	236	1,727	74	2,441	78	48	388	26

*Notes:* This table reports the observations, number of banks, and deals each year for the sample of merging banks in reformed states according to treatment and ownership status. In Columns (1) to (4), observations of synthetic or original banks are counted. In Column (5), observations are summed up per year. In Columns (6) to (9), mergers are counted in the year in which they occurred.

Table AVIII: Real effects on related firms.

	Panel A							
		External financing cost			Investment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SB	0.0046*** (0.0011)	0.0048*** (0.0011)	0.0048*** (0.0011)	0.0048*** (0.0012)	-0.6931*** (0.1062)	-0.7318*** (0.1196)	-0.7810*** (0.1272)	-0.8153*** (0.1199)
RM (t=0) × SB	-0.0023* (0.0012)				0.9249*** (0.1402)			
RM (t=0,1) × SB		-0.0025** (0.0010)				0.7218*** (0.1800)		
RM (t=0,1,2) × SB			-0.0015 (0.0011)				0.7144*** (0.1583)	
RM (t=0,1,2,3) × SB				-0.0010 (0.0011)				0.6105*** (0.1063)
Observations	51,792	51,792	51,792	51,792	51,792	51,792	51,792	51,792
Firms	18,664	18,664	18,664	18,664	18,664	18,664	18,664	18,664
Mean			0.0460				10.5330	
Median			0.0451				10.5330	
Standard Deviation			0.0314				10.5330	
Firm, Year-Region FE	yes	yes	yes	yes	yes	yes	yes	yes
R-squared (within)	0.0020	0.0021	0.0020	0.0020	0.0034	0.0035	0.0039	0.0038
R-squared (adjusted)	0.6862	0.6862	0.6862	0.6862	0.5700	0.5700	0.5702	0.5701



continued.

	Panel B							
	Employment			Leverage				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SB	-0.0511*** (0.0158)	-0.0535*** (0.0166)	-0.0528*** (0.0166)	-0.0571*** (0.0162)	0.0092 (0.0062)	0.0084 (0.0059)	0.0083 (0.0059)	0.0070 (0.0063)
RM (t=0) × SB	0.0260 (0.0205)				-0.0020 (0.0041)			
RM (t=0,1) × SB		0.0302 (0.0200)				0.0052 (0.0066)		
RM (t=0,1,2) × SB			0.0162 (0.0135)				0.0035 (0.0059)	
RM (t=0,1,2,3) × SB				0.0261** (0.0109)				0.0072 (0.0065)
Observations	51,792	51,792	51,792	51,792	51,792	51,792	51,792	51,792
Firms	18,664	18,664	18,664	18,664	18,664	18,664	18,664	18,664
Mean		2.9304				0.7178		
Median		2.8904				0.7621		
Standard Deviation		2.8904				0.2242		
Firm, Year-Region FE	yes	yes	yes	yes	yes	yes	yes	yes
R-squared (within)	0.0008	0.0009	0.0008	0.0009	0.0003	0.0003	0.0003	0.0004
R-squared (adjusted)	0.9532	0.9532	0.9532	0.9532	0.8398	0.8398	0.8398	0.8399

*Notes:* Clustered standard errors at the bank level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . The table reports results for regressions of Equation (2). We use four dependent variables: firms' (average) external financing cost calculated as total interest expenses over total liabilities; firms' investment, which is the logarithm of total gross real investment; employment as the logarithm of the number of firms' employees; and leverage, which is the ratio of total liabilities to total assets. The regression results for the first two sets are presented in Panel A, and the other two sets, in Panel B. Standard errors in parentheses are clustered on the regional level.

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Table AIX: Description of the main variables.

Variable	Description
<i>Main dependent variables</i>	
RoE	Return on Gross Equity: Profit before Taxes to Total Gross Equity (See also <i>Profit, Equity Decomposition</i> )
RoNE	Return on Net Equity: Profit before Taxes to Total Net Equity (See also <i>Profit, Equity Decomposition</i> )
RoA	Return on Assets: Profit before Taxes to Total Assets
<i>Main independent variables</i>	
L(GDP)	Log (county GDP): Logarithm of GDP per county
Equity	Net County Ratio: Net Equity to Total Assets
LLP	Loan Loss Provisions: Loan Loss Provisions to Total Loans
CIR	Cost-to-income Ratio: Administrative Costs to Operating Income
Liquidity	Liquidity Ratio: Liquid Assets (Cash, Accounts receivable of banks with daily maturity) to Total Assets
Loans	Loans Ratio: Total Loans to Non-Bank Costumers to Total Assets
NII	Non-Interest-Income Ratio: Non-Interest Income to Operating Income
Size	Quintile of Total Asset Distribution of resp. banking group
L(Debt)	Regional public debt: Logarithm of public debt per county
<i>Equity Decomposition</i>	
L(Gross Eq)	Log (Gross Equity): Sum of Net Equity, Total Accruals, and Other Equity
L(Net Eq)	Log (Net Equity): Sum of Nominal Equity, Retained Earnings, Current Earnings, and Other Retained Profits
L(Accruals)	Log (Total Accruals): Sum of Accruals for Pensions, Taxes, and Other Accruals incl. for Risks
L(Other Equity)	Log (Total Other Equity): Sum of Subordinated Debt, Participating Debt Obligations, and Equity-like Special Items
<i>Profit Decomposition</i>	
L(Profits)	Log (Profits before taxes): Operating and Non-operating Result
L(Total Rev)	Log (Total Revenues): Operating and Non-operating Revenues
L(Total Cost)	Log (Total Costs): Operating and Non-operating Costs
L(Op Cost)	Log (Operating Costs): Costs paid on IBA, on Commissions, on the Trading Book, Other Operating, and Administrative Costs
<i>Risk Channel</i>	
L(zscore)	Log (z-score): Profits minus Tier 1 equity over assets divided by Standard deviation of RoA based on a 5-year window
SD(RoA)	Standard Deviation of RoA: Standard Deviation of RoA based on a 5-year rolling window (min. 3 years available)
Tier1	Tier 1 Capital Ratio: Tier1 to Total Assets
LLP	Loan Loss Provisions Ratio: Loan Loss Provisions to Total Loans
NPL	Non-Performing-Loans Ratio: Non-Performing-Loans to Gross Loans to Costumers

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continued.

Variable	Description
<i>Cost Channel</i>	
Branch	Branch Ratio: Number of Branches to Total Assets (in Mil.)
Empl	Employees Ratio: Number of Employees to Total Assets (in Mil.)
Empl/Branch	Employees per Branch: Number of Employees per Branch
Wages/Empl	Wage Costs per Employee Ratio: Personnel Costs per Employee to Total Assets
CIR	Cost-Income-Ratio: Administrative Costs to Operating Income
<i>Market Power Channel</i>	
NIM	Net Interest Margin: Net Interest Income to Interest bearing Assets
Int. Earned	Average Interest earned on IBA: Interest Income to Interest bearing Assets
Int. Paid	Average Interest paid on IBA: Interest Costs to Interest bearing Assets
L(IBA)	Log (Interest Bearing Assets): Interbank Loans, Customer Loans, and Bonds and Securities
Market share	Market share of loans: Average share over all counties of banks' business area of average loans per branch of all branches in one county
<i>Deposits and loans</i>	
L(Deposit)	Log (Deposits): Logarithm of Deposits to Costumers
L(Loans)	Log (Loans): Logarithm of Total Loans to (Non-Bank) Costumers
L(Consumer)	Log (Consumer Loans): Loans to private households (excl. real estate)
L(Comm)	Log (Commercial Loans): Loans to firms and private businesses (excl. the industrial and agricultural sector)
L(Industrial)	Log (Industrial Loans): Loans to firms in the industrial sector
L(Agri)	Log (Agricultural Loans): Loans to firms in the agricultural sector
L(Real Estate)	Log (Real Estate Loans): Loans to private households for the purpose of real estate
L(Municipal)	Log (Municipal Loans): Loans to the public sector on the municipal level
L(State)	Log (State Loans): Loans to the public sector on the state level
<i>Decomposition of Total Costs</i>	
L(Int Cost)	Log (Interest Costs): Costs of Interest-Bearing Assets
L(Other Cost)	Log (Other Costs): Other operating costs