

## Disentangling the Effects of a Banking Crisis: Evidence from German Firms and Counties<sup>†</sup>

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*Lending cuts by banks directly affect the firms borrowing from them, but also indirectly depress economic activity in the regions in which they operate. This paper moves beyond firm-level studies by estimating the effects of an exogenous lending cut by a large German bank on firms and counties. I construct an instrument for regional exposure to the lending cut based on a historic, postwar breakup of the bank. I present evidence that the lending cut affected firms independently of their banking relationships, through lower aggregate demand and agglomeration spillovers in counties exposed to the lending cut. Output and employment remained persistently low even after bank lending had normalized. Innovation and productivity fell, consistent with the persistent effects. (JEL E32, E44, G01, G21, G32, R11, R23)*

The Great Recession followed a common pattern in many developed economies. There was a systemic banking crisis in the years 2008 and 2009, during which bank lending fell. Subsequently, there were two years of negative output growth and a slow recovery, during which output failed to return to its pre-crisis trend. This persistence is unusual in the postwar history of developed economies (Friedman 1993). Is there a causal link between the reduction in bank lending and this growth pattern? Do bank lending cuts lead to deep and persistent recessions?

Motivated by these questions, this paper delivers causal evidence on the effects of bank lending on the real economy. I analyze a lending cut by Commerzbank, a large German bank. During the financial crisis, Commerzbank suffered significant

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losses on its international trading book. These losses were unrelated to its domestic loan portfolio, but forced it to reduce its loan supply to German borrowers. I study the effects of the lending cut using variation across German counties and firms in their dependence on Commerzbank.<sup>1</sup> The analysis produces two main findings. First, the lending cut not only reduced the growth of firms that directly relied on Commerzbank's loan supply. There were also significant indirect effects on firms with undisturbed loan supply, through reductions in local aggregate demand and agglomeration spillovers. The second main finding is that the lending cut had persistent effects. Output and employment remained low even after lending had normalized.

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

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

The first set of results shows that the lending cut had real effects on firms. Following the lending cut, firms dependent on Commerzbank reduced their capital stock and employment, relative to similar firms located in the same county, but with no pre-crisis Commerzbank relationship. Employment at a firm fully dependent on Commerzbank was on average 5.3 percent lower than at a firm with no Commerzbank relationship. I call these firm-level responses the *direct* effects of the lending cut, because they were driven by firms' immediate financial connections to Commerzbank. They are a partial equilibrium response, keeping constant other aggregate factors that affected firms independently of their banking relationships. The findings on the direct effects confirm the results of Almeida et al. (2012) and

<sup>1</sup>Commerzbank refers to all branches that were part of the Commerzbank network in 2009, including Dresdner Bank.

Chodorow-Reich (2014).<sup>2</sup> I estimate effects of similar magnitude to the existing literature, which suggests that Commerzbank's lending cut has external relevance to the United States and other countries.

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

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

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

The second aspect I discuss is that the effects on both firms and counties were persistent. The causal effects resemble the growth pattern of developed economies

<sup>2</sup>Gan (2007); Khwaja and Mian (2008); Amiti and Weinstein (2011); Schnabl (2012); Paravisini et al. (2015); Garicano and Steinwender (2016); Cingano, Manaresi, and Sette (2016); and Bentolila et al. (forthcoming) present further evidence.

during and after the Great Recession. During the years of the lending cut, growth was significantly lower. In the subsequent two years, affected firms and counties remained on a lower, roughly parallel trend, without any sign of convergence to the level of unaffected firms and counties. This implies that a temporary bank lending cut can persistently keep output and employment low even after bank loan supply has normalized. The dynamics of the estimated effects suggest that the bank lending cuts during the financial crisis of 2008 and 2009 may have contributed to the sluggish recovery from the Great Recession, even though the banking sector had stabilized by 2010 (Hall 2010).

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

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

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

I contribute to the literature by clearly differentiating between the contemporaneous effects of a lending cut and the effects after lending has stabilized. I present evidence that productivity is affected. Furthermore, the existing literature has had to rely

on strong assumptions about the indirect effects. The findings of large indirect effects are of interest to researchers studying the aggregate implications of a range of shocks, not just banking crises. It is a general problem in empirical work that well-identified, partial equilibrium effects may not be informative about the aggregate implications of a given shock (Acemoglu 2010). While the effects I estimate do not easily aggregate into national effects (Nakamura and Steinsson 2014; Beraja, Hurst and Ospina 2015; Chodorow-Reich 2017), the combination of firm and county data is sufficient to establish the two main findings of indirect county-level effects and persistence.

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

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

## I. Identification and Institutional Background

### A. Identification Strategy

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

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

A lending cut can affect firms through multiple channels. It can reduce access to bank loans, affect the interest rate on loans and deposits, reduce the length of loans, and increase uncertainty regarding future credit access. Using just one of these variables as regressor would overestimate the effect of this particular variable.

Identifying the causal impact of each channel would require one separate instrument per channel (Chodorow-Reich 2014). I do not pursue such approaches here. Instead, I estimate the reduced-form impact, where Commerzbank dependence serves as proxy for exposure to a lending cut. This strategy overcomes the problem of reverse causality because Commerzbank's lending cut was exogenous to the performance of its German loan portfolio, as shown in Section IB. To address possible bias due to omitted unobservable variables at the regional level, I propose an instrument for county Commerzbank dependence in Section IC.

### *B. The Origin of Commerzbank's Lending Cut*

This section argues that Commerzbank's lending cut during the financial crisis of 2008–2009 was an exogenous shock to its German borrowers. Commerzbank was responsible for around 9 percent of total bank lending to German nonfinancial customers in 2006. Its lending stock developed in parallel to that of the other banks until 2007, as shown in Figure 1. In 2008 and 2009, lending by Commerzbank fell sharply. Subsequently, it returned to a parallel trend relative to its peer group of other commercial banks.<sup>3</sup>

Why did lending decrease? Commerzbank is a universal bank, which means it earns both interest income from lending and non-interest income from trading and investing in international financial markets. During the financial crisis, Commerzbank suffered significant losses and write-downs on its trading portfolio. The trading losses led to a fall in Commerzbank's equity capital in every year between 2007 and 2009, decreasing it by 68 percent during this period. Commerzbank responded by cutting its loan supply to the German economy for two reasons. First, the Basel II regulations require a bank to hold at least 4 percent of its risk-weighted assets in equity. When equity falls, banks have to reduce assets (and start raising new equity). Second, the equity losses raised Commerzbank's cost of external funds, so it needed to lower risk exposure to be able to access funding markets.

The changes in Commerzbank's equity capital were entirely driven by write-downs on financial instruments and profits, as shown in panel A of Figure 2. Write-downs on financial instruments included, for example, changes in the valuation of derivatives the bank held and were unconnected to the firm and household loan portfolio. The change in profits was also unrelated to firms and households. Panel B illustrates that trading and investment income was entirely responsible for the negative profits. Interest income, on the other hand, which includes what Commerzbank earns from lending to firms and households, remained on an upward trend up to 2009.

The trading losses were due to Commerzbank's investments in asset-backed securities related to the United States subprime mortgage market and its exposure to the insolvencies of Lehman Brothers and the large Icelandic banks. In 2008, Commerzbank had wrongly forecast the duration of the financial crisis and the

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

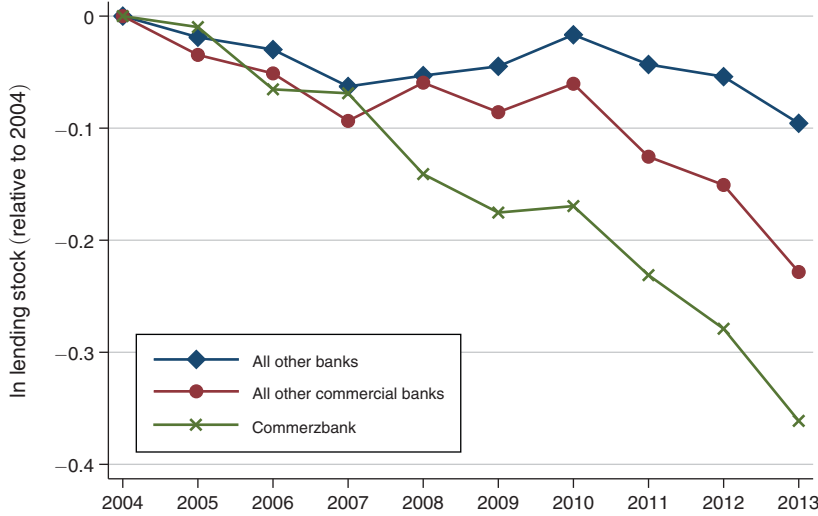


FIGURE 1. THE LENDING STOCK OF GERMAN BANKS

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

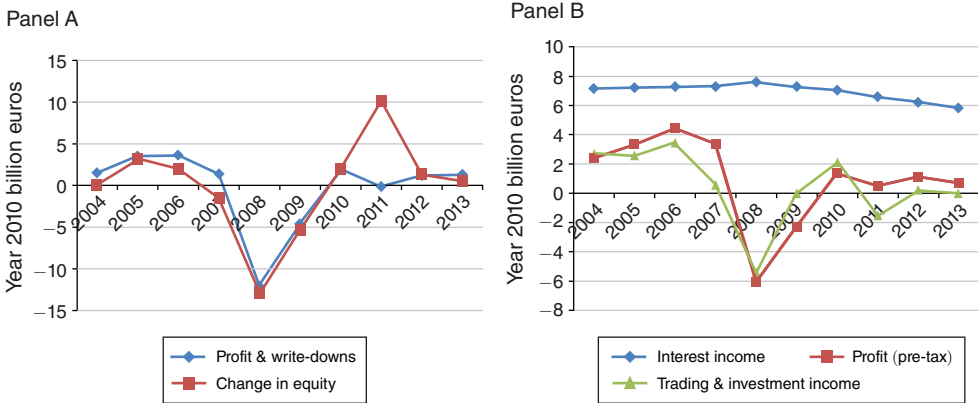


FIGURE 2. COMMERZBANK'S EQUITY CAPITAL, WRITE-DOWNS, AND PROFITS

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

likelihood of institutional failures. Commerzbank head Martin Blessing later admitted that his bank had reduced its exposure to asset-backed securities too late and had

believed that the United States government would not let Lehman Brothers fail. In comparison, Deutsche Bank avoided damage by hedging against a persistent drop in the United States housing market early on. Overall, the evidence shows that reverse causality is not a concern when I analyze the effects of Commerzbank's lending cut.

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

### *C. An Instrument for County Commerzbank Dependence*

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

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

The literature has established that banks prefer to form relationships with geographically close customers (Guiso, Sapienza, and Zingales 2004; Degryse and Ongena 2005). Indeed, in the years after the breakup, Commerzbank was



significantly more likely to establish a new branch in counties close to its temporary head offices, as shown in online Appendix Table A.I. The association between county Commerzbank dependence and distance to a postwar head office has survived until today, allowing me to construct a distance instrument based on how far a county is located from the postwar head offices. This distance instrument is calculated as the minimum of the linear (geodesic) distances to Düsseldorf, Frankfurt, and Hamburg. None of the three linear distances are perfectly correlated with the distance instrument. That means I can control for each of the linear distances to Düsseldorf, Frankfurt, and Hamburg in the IV specifications. In addition, I control for the linear distances to Berlin and Dresden, because historic, prewar head offices of Commerzbank were located there.

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

## II. Data

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

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

I obtain proprietary data from the year 2006 on the names of the relationship banks (*Hausbanken*) of 112,344 German firms, recorded by the credit rating agency *Creditreform*. The agency collects information on the relationship banks from firm surveys and financial statements. In all three firm datasets, I link firms to their banks in 2006 using a unique firm identifier (*Crefonummer*). The pre-crisis timing avoids endogeneity from weak banks getting matched with weak firms during the Great Recession. I drop firms in the financial and public sectors. This leaves 2,011

matched firms in the panel, 48,101 in the employment cross section, and 1,032 in the survey. I construct a variable to measure a firm's dependence on Commerzbank in 2006, called  $CB\ dep_{fc}$  for firm  $f$  in county  $c$ . It equals the fraction of the firm's relationship banks that were Commerzbank branches out of the firm's total number of relationship banks:

$$(1) \quad CB\ dep_{fc} = \frac{\text{number of relationship banks that are Commerzbank branches}_{fc}}{\text{total number of relationship banks}_{fc}}$$

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

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

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

Table 1 summarizes the firm panel. Firms have an average of three relationship banks. German firms traditionally form close and durable ties to their relationship banks. Dwenger, Fossen, and Simmler (2015) report that only 1.7 percent of firms find a new relationship bank per year. There is no information in my data on what services exactly a firm receives from a particular bank. In a separate survey, Elsas (2005) finds that relationship banks mostly finance bank loans, both long- and short-term, and provide payment transactions. A histogram of firm Commerzbank dependence is in panel A of Figure 3. Just under one-half of firms have a Commerzbank branch among their relationship banks. The average value of firm Commerzbank dependence is 0.16.

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

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

TABLE 1—SUMMARY STATISTICS FOR THE FIRM PANEL

	Mean	SD	p5	p50	p95
Firm <i>CB dep</i>	0.16	0.23	0	0	0.5
Number of relationship banks	3.00	1.54	1	3	6
Employment	913.71	11,592.54	19	132	2,030
Wage	32.04	47.15	15.51	29.46	46.37
Capital	57,711.61	544,582.57	225.75	5,467.81	196,539.06
Liabilities	152,628.46	3,657,557.10	1,552.79	8,848.93	213,144.20
Export share	11.02	21.31	0	0	64
Import share	5.24	16.73	0	0	40
Age	47.60	45.90	13.00	31.00	126.00
Bank debt/liabilities	0.48	0.26	0.05	0.49	0.90
Liabilities/assets	0.66	0.21	0.26	0.68	0.98
Firms	2,011				

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

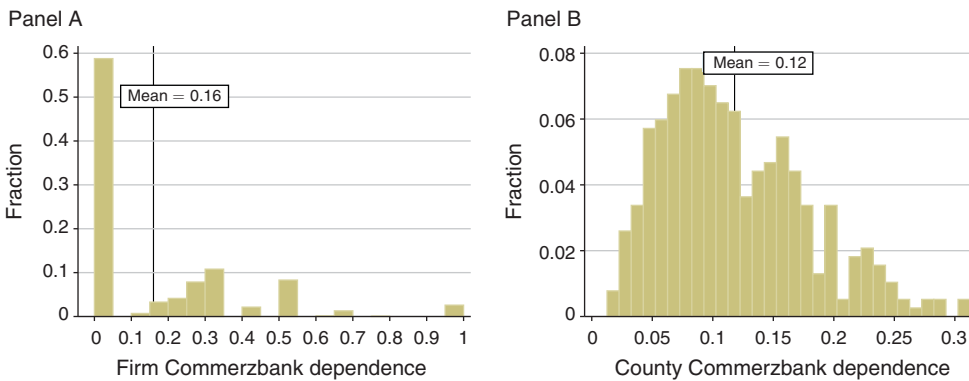


FIGURE 3. FIRM AND COUNTY COMMERZBANK DEPENDENCE

*Note:* The figure shows histograms of firm Commerzbank dependence for the 2,011 firms in the firm panel (panel A) and of county Commerzbank dependence for the 385 counties in the dataset (panel B).

and industry. An analysis of firm summary statistics by bins of Commerzbank dependence is in online Appendix A.

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

County summary statistics are in Table 2. The mean population of a county in 2000 was 203,280 and mean county Commerzbank dependence is 0.12. There is

TABLE 2—SUMMARY STATISTICS FOR THE COUNTY DATASET

	Mean	SD	p5	p50	p95
County <i>CB dep</i>	0.12	0.06	0.04	0.11	0.23
2000 GDP (year 2010 bn euros)	6.01	9.12	1.46	3.63	14.31
2000 population (1,000s)	203.28	229.39	52.68	147.12	487.13
2000 employment (1,000s)	98.27	126.49	29.90	64.50	220.40
Former GDR	0.16	0.37	0	0	1
Landesbank in crisis	0.67	0.47	0	1	1
Distance instrument	-1.63	0.97	-3.43	-1.51	-0.28
GDP growth, 2008–2012	2.66	6.18	-7.25	2.73	11.76
Employment growth, 2008–2012	2.79	3.22	-1.98	2.77	7.21
Observations	385				

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

significant variation in county Commerzbank dependence, as shown in the panel B of Figure 3 and in the map in online Appendix Figure A.I.

### III. The Effect of the Lending Cut on Bank Debt

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

#### A. Firm Survey Evidence on Commerzbank's Lending Cut

I examine whether firms dependent on Commerzbank perceived their banks to lend more restrictively. The results are in Table 3. The outcome variable is the answer to the question: "How do you evaluate the current willingness of banks to grant loans to businesses: cooperative, normal, or restrictive?" All the specifications control for firm industry, federal state, size, and age. A lagged dependent variable from 2006 accounts for preexisting, time-invariant differences in bank loan supply.

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

TABLE 3—FIRM SURVEY ON BANKS' WILLINGNESS TO GRANT LOANS

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

*Notes:* This table reports estimates from cross-sectional firm regressions for different years. The outcome variable is the answer to the question: "How do you evaluate the current willingness of banks to grant loans to businesses: cooperative (coded as 1), normal (0), or restrictive (-1)?" It is standardized to have zero mean and unit variance. The coefficients are interpreted as the standard deviation increase in banks' willingness to grant loans from increasing Commerzbank dependence by one. The control variables include fixed effects for 36 industries, 16 federal states, 4 size bins (1–49, 50–249, 250–999, and over 1,000 employees in the year 2006), and the ln of firm age. Standard errors are clustered at the level of the county.

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

### B. *The Effect of Commerzbank's Lending Cut on Firms' Bank Debt*

Having established that firms dependent on Commerzbank reported reduced loan supply, I test whether the lending cut actually reduced bank debt. The outcome is the natural logarithm of firm bank loans. I run specifications using the firm panel dataset, including year and firm fixed effects. Table 4 presents the results. The regressor of interest is firm Commerzbank dependence interacted with  $d$ , a dummy for the years following the lending cut, 2009 to 2012.

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

These results imply that Commerzbank dependence is a valid proxy for exposure to Commerzbank's lending cut. Firms dependent on Commerzbank were unable to

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

TABLE 4—FIRM BANK LOANS AND COMMERZBANK DEPENDENCE

	(1)	(2)	(3)
Firm <i>CB dep</i> × <i>d</i>	−0.101 (0.079)	−0.166 (0.080)	−0.205 (0.078)
Observations	12,066	12,066	12,066
$R^2$	0.009	0.078	0.094
Number of firms	2,011	2,011	2,011
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
County fixed effects × <i>d</i>	No	Yes	Yes
ln age × <i>d</i>	No	Yes	Yes
Size bin fixed effects × <i>d</i>	No	Yes	Yes
Industry fixed effects × <i>d</i>	No	No	Yes
Import and export share × <i>d</i>	No	No	Yes

*Notes:* This table reports estimates from firm OLS panel regressions. The outcome in all columns is firm ln bank loans. Firm *CB dep* is the fraction of the firm's relationship banks that were Commerzbank branches in 2006. *d* is a dummy for the years following the lending cut, 2009 to 2012. The following time-invariant control variables are calculated for the year 2006 and interacted with *d*: fixed effects for 70 industries, 357 counties, and 4 firm size bins (1–49, 50–249, 250–999, and over 1,000 employees); the ln of firm age; the export share (fraction of exports out of total revenue); and the import share (fraction of imports out of total costs). The data include the years 2007 to 2012.  $R^2$  is the within-firm  $R^2$ . Standard errors are two-way clustered at the level of the county and the industry.

substitute other lenders for Commerzbank. This was the case even though all firms were located in regions where other healthy lenders operated, as county Commerzbank dependence ranged from 1 to 31 percent. The results therefore suggest an important role for credit market frictions, even in the presence of alternative healthy lenders.

### C. The Effect of Commerzbank's Lending Cut on Household Debt

I investigate whether Commerzbank's lending cut also affected households' access to bank loans. Thirty-two percent of Commerzbank's interest income in 2006 stemmed from households. Table 5 analyzes the household panel GSOEP. The outcome in the first three columns is the symmetric growth rate of debt. The effect of county Commerzbank dependence is small and statistically insignificant in all specifications. The estimate in column 2 controls for county characteristics and pre-determined individual debt holdings. It implies that households in a county entirely dependent on Commerzbank experienced an increase in their growth rate of debt between 2007 and 2012 by 0.7 percentage points. Adding individual control variables in column 3 raises the coefficient, but it remains insignificant. The outcomes in columns 4 to 8 are dummies for whether an individual has any outstanding debt in the given year. There is no significant effect of county Commerzbank dependence in any year between 2008 and 2012.

These results can be explained by features of the German financial system that facilitate bank-switching for households. For example, the government-owned development bank KfW cofinances nationally standardized mortgage contracts in cooperation with private and public banks. This is important because mortgage debt comprised 91 percent of German household debt. Households can apply for these mortgages through any bank, regardless of whether they have a preexisting

TABLE 5—HOUSEHOLD DEBT AND COUNTY COMMERZBANK DEPENDENCE

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

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

relationship bank or not. KfW raised its mortgage commitments to households by 26.5 percent during the crisis. Aggregate lending to private customers by commercial banks actually rose slightly between 2007 and 2010, which suggests that other commercial banks were able to compensate households for Commerzbank's lending cut. In contrast, aggregate lending to corporate borrowers by commercial banks fell, which implies firms were not able to turn to other lenders. Consistent with these findings, a recent paper by Jensen and Johannesen (2017) shows that when bank-switching costs are low, there is no effect of lending cuts by individual banks on household debt.

#### IV. The Direct Effect on Firms

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

### A. Firm Specification

I use the firm panel to estimate equation (2), for firm  $f$  in county  $c$  at time  $t$ ;  $\beta$  is the direct effect;  $d_t^{post}$  is a dummy for the years following the lending cut, 2009 to 2012:

$$(2) y_{fct} = \zeta + \beta CBdep_{fc} \times d_t^{post} + \kappa_c \times d_t^{post} + \Gamma' X_{fc} \times d_t^{post} + \gamma_{fc} + \lambda_t + \varepsilon_{fct}.$$

The specification includes county fixed effects interacted with the post-lending cut dummy,  $\kappa_c \times d_t^{post}$ . This is an important step in isolating the direct effect. It keeps constant any county-specific shocks associated with the Commerzbank dependence of other firms in the county. Firm fixed effects  $\gamma_{fc}$  account for time-invariant, firm-specific differences in the outcome. Year fixed effects  $\lambda_t$  control for changes in the outcome that are common to all firms in a year, for example due to macroeconomic fluctuations. Finally,  $X_{fc}$  is a vector of further control variables, listed in Table 6. The standard errors are two-way clustered at the level of the county and the industry.

The identifying assumption in this section is that there were no unobservable shocks within counties correlated with firm Commerzbank dependence. The evidence supports this assumption. Figure 4 shows that firms with and without a relationship to Commerzbank followed parallel employment trends before the lending cut. The firm panel shows no strong correlation between Commerzbank dependence and firm observables in 2006 (online Appendix A). There was no effect of Commerzbank dependence on perceived product demand in any year before the lending cut, and an effect on perceived credit constraints only during the lending cut (online Appendix C).

### B. Firm Results

Table 6 reports the main result of this section in column 3. The point estimate implies that, following the lending cut, employment at a firm fully dependent on Commerzbank was on average 5.3 percent lower than at a firm with no Commerzbank relationship. The modest impact of the control variables across the first three columns of Table 6 strengthens the argument that Commerzbank dependence was not significantly correlated with other determinants of firm growth. The existing literature estimates direct effects of a similar magnitude, suggesting that Commerzbank's lending cut has external relevance. For instance, Chodorow-Reich (2014) for the United States and Bentolila et al. (forthcoming) for Spain find that firms connected to distressed banks reduced employment growth by 4 to 5 percentage points.

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



TABLE 6—FIRM EMPLOYMENT AND COMMERZBANK DEPENDENCE

	(1)	(2)	(3)	(4)	(5)
Firm <i>CB dep</i> × <i>d</i>	−0.044 (0.021)	−0.047 (0.016)	−0.053 (0.015)		
Low bank debt dep. × firm <i>CB dep</i> × <i>d</i>				−0.035 (0.032)	
High bank debt dep. × firm <i>CB dep</i> × <i>d</i>				−0.071 (0.020)	
(0 < firm <i>CB dep</i> ≤ 0.25) × <i>d</i>					0.007 (0.016)
(0.25 < firm <i>CB dep</i> ≤ 0.5) × <i>d</i>					−0.017 (0.008)
(0.5 < firm <i>CB dep</i> ≤ 1) × <i>d</i>					−0.065 (0.018)
Observations	12,066	12,066	12,066	12,066	12,066
<i>R</i> <sup>2</sup>	0.026	0.098	0.124	0.125	0.125
Number of firms	2,011	2,011	2,011	2,011	2,011
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
County fixed effects × <i>d</i>	No	Yes	Yes	Yes	Yes
Size bin fixed effects × <i>d</i>	No	Yes	Yes	Yes	Yes
ln age × <i>d</i>	No	Yes	Yes	Yes	Yes
Industry fixed effects × <i>d</i>	No	No	Yes	Yes	Yes
Import and export share × <i>d</i>	No	No	Yes	Yes	Yes

Notes: This table reports estimates from firm OLS panel regressions. The outcome in all columns is firm ln employment. Firms with low (high) bank debt dependence have up to (over) 50 percent of their liabilities with banks. The control variables, the standard error calculations, the years covered by the data, and the definition of *R*<sup>2</sup> are explained in Table 4.

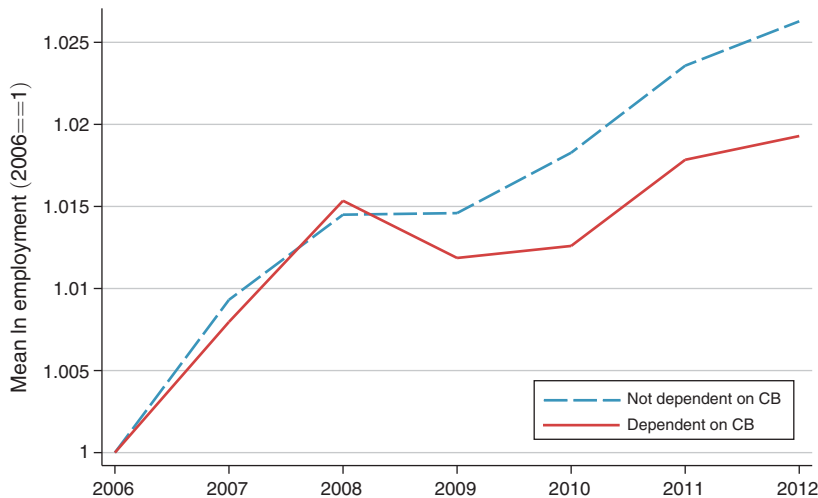


FIGURE 4. FIRM EMPLOYMENT EFFECTS

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

TABLE 7—FURTHER FIRM OUTCOMES AND COMMERZBANK DEPENDENCE

Outcome:	Capital (1)	Val add (2)	Val add/capital (3)	Val add/empl (4)	Wage (5)	Int rate (6)
Firm <i>CB dep</i> × <i>d</i>	−0.130 (0.038)	−0.061 (0.028)	0.069 (0.038)	−0.008 (0.024)	0.001 (0.011)	−0.003 (0.003)
Observations	12,066	12,066	12,066	12,066	12,066	12,024
<i>R</i> <sup>2</sup>	0.131	0.116	0.116	0.091	0.069	0.073
Number of firms	2,011	2,011	2,011	2,011	2,011	2,004
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls × <i>d</i>	Yes	Yes	Yes	Yes	Yes	Yes

*Notes:* This table reports estimates from firm OLS panel regressions. The respective outcome is given in the column title. Capital is the ln book value of fixed tangible assets. Value added (val add) is the ln of revenue minus expenditure on intermediates. Value added per worker is ln(val add/empl) and per unit of capital is ln(val add/cap). The wage is the ln of the wage bill divided by the number of employees. The interest rate is the interest paid over total liabilities. The control variables, the standard error calculations, the years covered by the data, and the definition of *R*<sup>2</sup> are explained in Table 4.

with Commerzbank dependence over one-half, which had few alternative options to access bank loans.<sup>6</sup>

Table 7 analyzes other outcomes and thereby sheds light on how firms adjust to a lending cut. The capital stock decreased by an average of 13 percent. Therefore, the capital-labor ratio fell, which suggests firms use bank loans primarily to finance capital investment. Firms dependent on Commerzbank were capital-constrained, which increased their average product of capital, measured as value added per capital in column 3. On the contrary, the lending cut did not affect the average product of labor and the average wage, relative to other firms in the same county, as shown in columns 4 and 5 respectively. This is consistent with a competitive county labor market. Column 6 reports no effect on the interest rate, in line with evidence from the United States credit card market (Ausubel 1991).

## V. The Effect on Counties

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

### A. County Specification

I estimate equation (3) for county *c* at time *t*:

$$(3) \quad y_{ct} = \zeta + \rho \overline{CB dep_c} \times d_t^{post} + \Gamma' X_c \times d_t^{post} + \gamma_c + \lambda_t + \varepsilon_{ct}$$

<sup>6</sup>In unreported results, I find no heterogeneity in the effect on capital-intensive industries (consistent with Paravisini et al. 2015), on large firms (consistent with Bentolila et al. forthcoming), on firms in counties with relatively high county Commerzbank dependence, or on firms dependent on Dresdner Bank before the 2009 acquisition. Online Appendix D shows firms dependent on Commerzbank did not suffer higher losses on the value of their financial assets during the financial crisis.

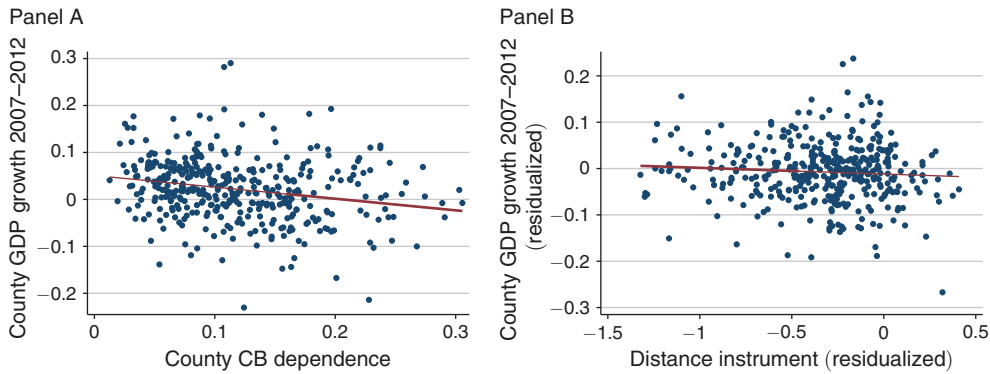


FIGURE 5. COUNTY GDP GROWTH, COMMERZBANK DEPENDENCE, AND THE DISTANCE INSTRUMENT

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

The coefficient on  $\overline{CB\ dep_c} \times d_t^{post}$ , scaled by 100, measures the average percentage change in the outcome following the lending cut in a county fully dependent on Commerzbank. Here,  $\gamma_c$  is a county fixed effect and  $\lambda_t$  a year fixed effect;  $X_c$  is a vector of time-invariant control variables, described in the notes of Table 5. The standard errors are clustered at the level of 42 quantiles of the county's industrial production share (GDP share of mining, manufacturing, utilities, recycling, construction). This is a more general method than clustering at the level of the county. It allows for arbitrary correlations of the errors across counties of similar industrial structure.

### B. County OLS Results

Panel A of Figure 5 plots the growth rate of county GDP from 2007 to 2012 against Commerzbank dependence. The line of best fit shows a statistically significant negative relationship, suggesting that the lending cut lowered GDP growth.

Table 8 reports the results of the corresponding OLS specifications. The key result of this section is in column 2. The point estimate implies that a standard deviation increase in Commerzbank dependence (6 percentage points) lowered county GDP by an average of 1 percent after Commerzbank's lending cut. This specification controls for the two main identification concerns. The first concern is that idiosyncratic shocks to certain industries and exposure to the trade collapse during the Great Recession may be correlated with Commerzbank dependence. I control for the share of 17 industries among the county's firms in 2006 as well as the average export and import shares of firms in the county. The second main concern is that some regions fared worse because they were in the former GDR or because their Landesbank suffered losses in the financial crisis (Puri, Rocholl, and Steffen 2011). I add dummies for counties in these regions to the specification. Column 3 tests the robustness of the result further, by controlling for population density,  $\ln$  population,  $\ln$  GDP per capita, and household leverage. The coefficient remains stable, suggesting that the results are not driven by preexisting differences in county characteristics.

TABLE 8—COUNTY OUTCOMES AND COMMERZBANK DEPENDENCE (OLS)

Outcome:	GDP (1)	GDP (2)	GDP (3)	Empl. (4)	Net migr. (5)
County <i>CB dep</i> × <i>d</i>	−0.132 (0.063)	−0.165 (0.066)	−0.141 (0.077)	−0.138 (0.042)	0.003 (0.006)
Observations	5,005	5,005	5,005	5,005	1,925
<i>R</i> <sup>2</sup>	0.301	0.341	0.350	0.494	0.592
Number of counties	385	385	385	385	385
County fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Former GDR fixed effects × <i>d</i>	No	Yes	Yes	Yes	Yes
Industry shares × <i>d</i>	No	Yes	Yes	Yes	Yes
Export and import shares × <i>d</i>	No	Yes	Yes	Yes	Yes
Landesbank in crisis × <i>d</i>	No	Yes	Yes	Yes	Yes
Population × <i>d</i>	No	No	Yes	No	No
Population density × <i>d</i>	No	No	Yes	No	No
GDP per capita × <i>d</i>	No	No	Yes	No	No
Debt index × <i>d</i>	No	No	Yes	No	No
Estimator	OLS	OLS	OLS	OLS	OLS

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

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

### C. County IV Results

I use the distance instrument to test whether there is any evidence for bias in the OLS estimates. Panel B of Figure 5 plots the growth rate of GDP from 2007 to

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

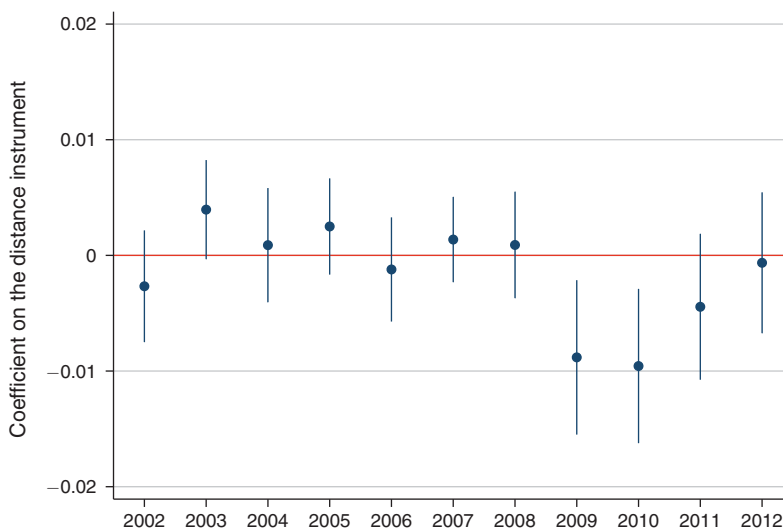


FIGURE 6. REDUCED-FORM IMPACT OF THE INSTRUMENT ON THE COUNTY GDP GROWTH RATE

*Notes:* This figure is based on a single regression, in which the dependent variable is the county's annual GDP growth rate. The plotted point estimates are the coefficients on the instrument, interacted with annual dummy variables. The vertical lines are 90 percent confidence intervals. The regression includes year and county fixed effects and the full set of control variables from Table 9, including the linear distances. The standard errors are calculated as in Table 8.

2012 against the distance instrument. There is a negative and statistically significant reduced-form relationship. Figure 6 confirms that the growth rate of GDP was lower only during the years of Commerzbank's lending cut. In the figures and in all IV specifications, I add five separate linear distance control variables, measuring the distances to five former head offices in Düsseldorf, Frankfurt, Hamburg, Berlin, and Dresden. This ensures that the effect is identified only through the distance to the closest of Commerzbank's postwar head offices. I also include a dummy for the former GDR to account for the postwar breakup of Germany.

Table 9 reports the regression results. Columns 1 and 2 show a strong first-stage relationship between the distance instrument and Commerzbank dependence. The IV second-stage coefficients in columns 3 to 7 report negative and significant effects on county GDP and employment and no effect on migration, consistent with the OLS results. Adding the list of control variables hardly affects the point estimates, strengthening the argument that the distance instrument is exogenous to county growth.<sup>8</sup>

In general, the IV point estimates imply larger effects than the OLS estimates. The coefficient in column 4 implies a GDP loss of 2.2 percent from a standard deviation

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

TABLE 9—COUNTY OUTCOMES AND COMMERZBANK DEPENDENCE (IV)

Outcome:	CB dep (1)	CB dep (2)	GDP (3)	GDP (4)	GDP (5)	Empl (6)	Net migr (7)
Distance instrument $\times d$	0.028 (0.005)	0.042 (0.006)					
County CB dep $\times d$			-0.335 (0.118)	-0.367 (0.182)	-0.345 (0.173)	-0.208 (0.113)	0.026 (0.020)
Observations	5,005	5,005	5,005	5,005	5,005	5,005	1,925
$R^2$	0.876	0.941	0.322	0.348	0.355	0.504	0.590
Number of counties	385	385	385	385	385	385	385
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Former GDR fixed effects $\times d$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear distances $\times d$	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry shares $\times d$	No	Yes	No	Yes	Yes	Yes	Yes
Export and import shares $\times d$	No	Yes	No	Yes	Yes	Yes	Yes
Landesbank in crisis $\times d$	No	Yes	No	Yes	Yes	Yes	Yes
Population $\times d$	No	Yes	No	No	Yes	No	No
Population density $\times d$	No	Yes	No	No	Yes	No	No
GDP per capita $\times d$	No	Yes	No	No	Yes	No	No
Debt index $\times d$	No	Yes	No	No	Yes	No	No
Estimator	OLS	OLS	IV	IV	IV	IV	IV

Notes: This table reports estimates from county panel regressions. Columns 1 and 2 report the first stage and columns 3 to 7 the IV regressions. The distance instrument is the negative of the county's distance to the closest post-war Commerzbank head office, in 100 kilometers. The linear distances include the county's distances to Düsseldorf, Frankfurt, Hamburg, Berlin, and Dresden. The outcomes, other control variables, weights, standard error calculations, the years covered by the data, and the definition of  $R^2$  are explained in Table 8.

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

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

TABLE 10—THE DIRECT AND INDIRECT EFFECTS ON FIRM EMPLOYMENT GROWTH

	(1)	(2)
Firm <i>CB dep</i>	−0.030 (0.009)	−0.036 (0.009)
<i>CB dep</i> of other firms in county	−0.166 (0.076)	−0.170 (0.082)
Observations	48,101	48,101
$R^2$	0.012	0.017
Firm controls	Yes	Yes
County controls	No	Yes

*Notes:* This table reports estimates from cross-sectional firm OLS regressions. The outcome is the symmetric growth rate of firm employment from 2008 to 2012. *CB dep* of other firms in county is the average firm Commerzbank dependence of all the other firms in the county. The firm control variables are the same as in Table 4, except there are no county fixed effects. The county controls and the standard error calculations are the same as in Table 8.

## VI. Discussion of the Results

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

### A. The Indirect Effect

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

I use the employment cross section dataset to estimate equation (4). The larger sample size of 48,101 firms enables me to estimate the direct effect  $\beta$  and the indirect effect  $\sigma$  in the same specification. The outcome is the symmetric growth rate of firm employment between 2008 and 2012:

$$(4) \quad \text{employment growth}_{fc} = \zeta + \beta \text{CB dep}_{fc} + \sigma \overline{\text{CB dep}_{fc}} + \Gamma' X_{fc} + \xi_{fc}.$$

Table 10 presents the results. The main object of interest in this section is the indirect effect, that is the coefficient on the average Commerzbank dependence of other firms in the county. I include firm control variables in column 1. The point estimate is negative and statistically significant at the 5 percent level. Adding the county controls in column 2 hardly affects the estimate. To illustrate the size of the indirect effect implied by the point estimates, consider a firm fully dependent on Commerzbank, operating in a county where no other firm had Commerzbank among their relationship banks. This

TABLE 11—THE IMPLIED COUNTY EMPLOYMENT CHANGE BASED ON DIFFERENT ESTIMATES

	Estimate from section	Estimator	Dataset	Estimated effect	Point estimate	95 percent CI	
						Lower	Upper
1.	IVB	OLS	Firm panel	Only direct	−0.32	−0.49	−0.14
2.	VB	OLS	County panel	Direct & indirect	−0.83	−1.31	−0.34
3.	VC	IV	County panel	Direct & indirect	−1.25	−2.58	−0.09
4.	VIA	OLS	Firm cross section	Direct & indirect	−1.24	−2.17	−0.29

Notes: This table reports different estimates of the county employment loss from increasing county Commerzbank dependence by a standard deviation (6 percentage points). Row 1 uses the estimate of the direct effect from column 3 of Table 6. Row 2 uses the county OLS estimate from Table 8, column 4. Row 3 uses the county IV estimate from Table 9, column 6. Row 4 uses the sum of direct and indirect effects from column 2 of Table 10.

firm reduced employment growth between 2008 and 2012 by 3.6 percentage points, the direct effect.<sup>9</sup> If the same firm had operated in a county where the Commerzbank dependence of the other firms had been 1 standard deviation (6 percentage points) greater, employment growth would have fallen by 4.6 percentage points. In this latter county, firms with no direct relationship to Commerzbank would have reduced employment growth by 1 percentage point, solely due to the indirect effect.

Table 11 gives an overview of the county employment change implied by the different estimates in the paper. The estimate in row 1, based solely on the direct effect, underestimates the county employment loss, because it ignores the indirect effect. The average county Commerzbank dependence is 0.12, so the direct effects harm only a relatively small fraction of firms. It is the indirect effect that amplifies the effects of the lending cut throughout the county economy. The estimates of the sum of direct and indirect effects are larger than the estimate in row 1, whether I use the county data (rows 2 and 3) or the firm data (row 4). The IV estimate based on the county dataset is close to the OLS estimate based on the firm employment cross section dataset, supporting the view that there is no significant bias in the OLS estimates.

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

The second theoretical channel argues that household consumption fell due to employment losses at firms dependent on Commerzbank, reducing aggregate demand

<sup>9</sup>This point estimate of the direct effect is slightly smaller than in Table 6, because I use a different outcome, the symmetric growth rate. Using the ln difference as outcome renders the point estimates almost identical.



in the county. Producers of non-tradables rely strongly on local demand. Producers of tradables, on the other hand, mainly depend on national and global demand. Following the methodology of Mian and Sufi (2014), I classify an industry as tradable if the sum of its exports is at least US\$10,000 per worker or US\$500 million in total (using industry data from the United States). The retail and restaurant sector are classified as non-tradable. In addition, firms with a Herfindahl index in the top quartile produce tradables and firms in the bottom quartile produce non-tradables. This uses the fact that non-tradable industries are highly dispersed, because they need to produce locally in the markets they serve, while tradable industries tend to be concentrated. If industries remain unclassified, I call them producers of part-tradables.

The interaction of innovation and tradability leaves me with seven industry types.<sup>10</sup> I estimate a separate indirect effect for each industry type, by interacting the variable  $\overline{CB dep}_{fc}$  in equation (4) with a full set of industry type dummies. The specification controls for the direct effect, by including the variable  $CB dep_{fc}$ . In addition to the full set of firm and county control variables, the specification also includes fixed effects for the categories of tradability and innovation, to ensure that the coefficients are not biased by common shocks to firms in these categories.

Figure 7 plots estimates of the indirect effect by industry type. There is a statistically significant indirect effect for high-innovation producers of tradables and producers of non-tradables.<sup>11</sup> The effect on high-innovation firms is consistent with agglomeration spillovers particular to these industries. In unreported results, I find that the Commerzbank dependence of other high-innovation firms in the county drives the indirect effect on high-innovation firms. There is no significant indirect effect from the Commerzbank dependence of low- and medium-innovation firms. Furthermore, the indirect effect is larger in counties with a high, above-median density of high-innovation firms. This suggests agglomeration spillovers are more important in innovation clusters.

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

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

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

<sup>12</sup>Changes in household debt cannot explain the non-tradable indirect effect. Di Maggio and Kermani (2017) estimate an elasticity of non-tradable employment with respect to household debt of 0.2. Using their estimate, the lower bound of the 90 percent confidence interval of the household debt effect from column 1 of Table 5 can only explain 15 percent of the indirect effect on non-tradable, low-innovation firms' employment.

<sup>13</sup>To get this figure, I first calculate the effect of the lending cut on tradable employment in a county, in which the tradable sector is fully dependent on Commerzbank. The direct effect leads to an employment loss of 3.5 percent for all tradable producers (estimated in the regression for Figure 7). In addition, 21 percent of tradable producers are high-innovators, so they also suffer the indirect effect of 39.9 percent. Overall, tradable employment declines by

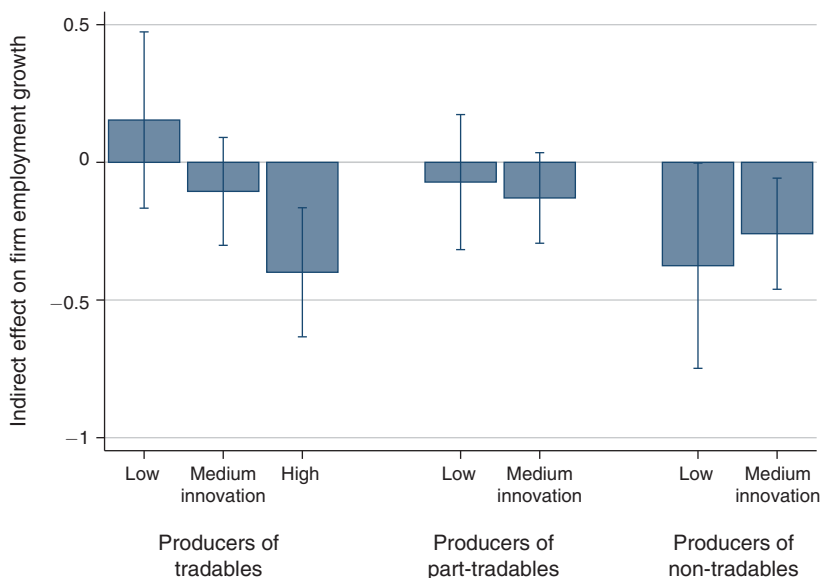


FIGURE 7. THE SIZE OF THE INDIRECT EFFECT BY INDUSTRY TYPE

Notes: This figure illustrates heterogeneity in the indirect effect by industry type. The plotted point estimates are the effect of the Commerzbank dependence of all other firms in the county on the symmetric growth rate of firm employment between 2008 and 2012. The estimates are from a single regression that controls for the firm’s direct Commerzbank dependence and the other control variables from Table 10. The vertical lines are 90 percent confidence intervals.

The two theories predict no indirect effect on producers of tradables with low innovation activities. Indeed, the coefficient on these firms in Figure 7 is positive and statistically insignificant. In an unreported test, I also find no indirect effect for low- and medium-innovation tradables producers located in an industrial cluster, unlike for high-innovation firms. Furthermore, I find no heterogeneity in the direct effect by county Commerzbank dependence. This implies that potential increases in the difficulty of finding new lenders cannot explain the indirect effect.

*B. The Persistence of the Effects*

Firms dependent on Commerzbank reported restrictive bank loan supply in 2009 and 2010, but not in any year before or after (Section IIIA). Figure 4 shows that employment at firms with Commerzbank among their relationship banks developed in parallel to other firms before the lending cut. In 2009 and 2010, firms dependent on Commerzbank grew more slowly. Afterward, they remained on a lower, parallel trend for two years. Figure 6 illustrates the same pattern for counties. Counties close to the postwar head offices, with greater Commerzbank dependence, grew more slowly during the years of the lending cut and did not recover afterward.

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approximately  $3.5 + 0.21 \times 39.9 = 11.9$  percent. The indirect effect on the average non-tradable firm is 25.9 percent. Twenty-three percent of firms produce tradables. Therefore, the indirect effect reduces non-tradable employment by  $0.23 \times 25.9 = 6$  percent. Multiplying the elasticity of non-tradable to tradable employment by 3.33, the ratio of non-tradable jobs to tradable jobs, gives the figure of 1.7. Further evidence on the local demand channel can be found in Bernstein et al. (forthcoming), Charles et al. (2017), and Giroud and Mueller (2017).

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

Table 12 examines the effect of the lending cut on firms' innovation activities, proxied by patents. The outcome in column 1 is the symmetric growth rate of the number of patents between the periods before (2005–2008) and after Commerzbank's lending cut (2009–2012). If a firm produced no patents in either period, the growth rate is set to zero. If a firm produced at least one patent from 1990 to 2004, I call it a patenting firm. The effect on these patenting firms is large. The growth rate of the number of patents was approximately 55 percentage points lower at patenting firms entirely dependent on Commerzbank. There is no effect on non-patenting firms. It is possible that many non-patenting firms are structurally unsuited to ever issue patents, independent of credit supply, or that in a period of low global growth, few firms choose to commence patenting. Negative binomial count models in columns 2 and 3 confirm that after the lending cut, patenting firms dependent on Commerzbank issued significantly fewer patents. There was no significant difference before the lending cut.<sup>14</sup>

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

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

<sup>15</sup>I carry out two robustness checks. First, the estimate of TFP growth remains negative when I use adjustment factors larger than any value observed two years after a recession in Fernald's data. Second, to explain the output

TABLE 12—FIRM PATENTS AND COMMERZBANK DEPENDENCE

Outcome:	Growth rate of patents (1)	Patents post lending cut (2)	Patents pre lending cut (3)
Patenting $\times$ firm <i>CB dep</i>	-0.548 (0.245)	-0.770 (0.409)	0.206 (0.409)
Non-patenting $\times$ firm <i>CB dep</i>	0.037 (0.065)		
In patents, 1990–2004		0.671 (0.088)	0.687 (0.116)
Observations	2,011	382	382
$R^2$	0.251		
In age	Yes	Yes	Yes
Size bin fixed effects	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes
County fixed effects	Yes	No	No
State fixed effects	No	Yes	Yes
Import and export share	Yes	Yes	Yes
Only patenting firms in sample	No	Yes	Yes
Estimator	OLS	Neg bin	Neg bin

*Notes:* A patenting firm is defined as a firm that has produced at least one patent from 1990 to 2004. The outcome in column 1 is the symmetric growth rate of the number of patents between the periods before (2005–2008) and after Commerzbank's lending cut (2009–2012). If a firm produces no patents in either period, the growth rate is set to zero. The control variables and the standard error calculations in column 1 are the same as in Table 4. Standard errors in columns 2 and 3 are clustered at the level of the industry.

paint a consistent picture. The results suggest innovation and productivity fell after the lending cut, which could explain the persistent losses.

## VII. Conclusion

This paper presents new evidence on the causal effects of bank lending on economic activity. It analyzes a lending cut by Commerzbank, a large German bank. The lending cut was not caused by domestic factors, but it was imported to Germany through Commerzbank's trading losses on international financial markets during the financial crisis of 2008–2009. The results show that the lending cut lowered the output and employment of firms and counties dependent on Commerzbank. Employment at a firm fully dependent on Commerzbank fell by 5.3 percent, while a standard deviation increase in county Commerzbank dependence reduced county employment by 0.8 percent.

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

loss while keeping TFP constant, capital would have had to fall by 5.6 percent. This equals 1.9 times the output loss, which is implausibly large given historic movements.

to the unaffected levels. This pattern resembles the growth experience of the United States and other developed economies following the financial crisis of 2008 and 2009.

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

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