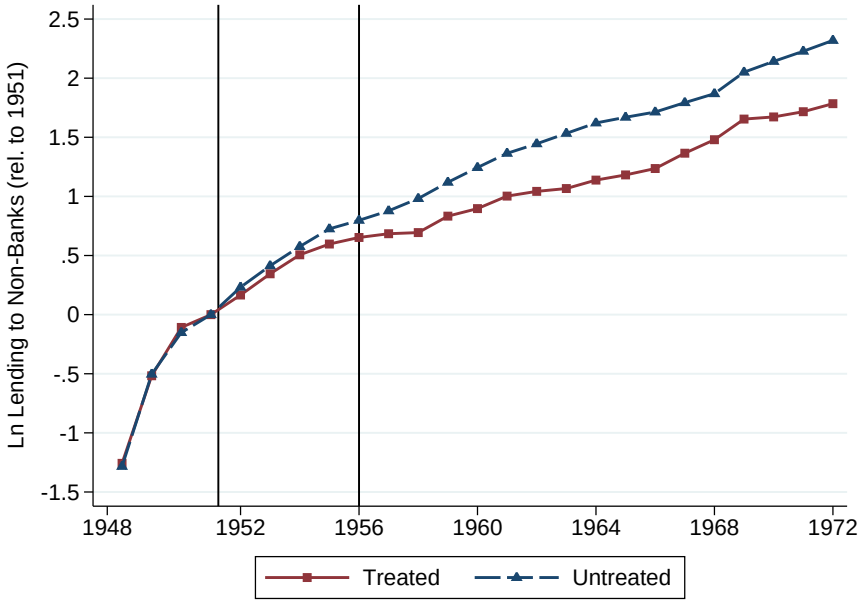
Supplemental Material (not copyedited or formatted) for: Kilian Huber. 2021. "Are Bigger Banks Better? Firm-Level Evidence from Germany." Journal of Political Economy 129(7). DOI: https://doi.org/10.1086/714120.

Online Appendix for "Are Bigger Banks Better? Firm-Level Evidence from Germany"

Appendix A Figures and Tables

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



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

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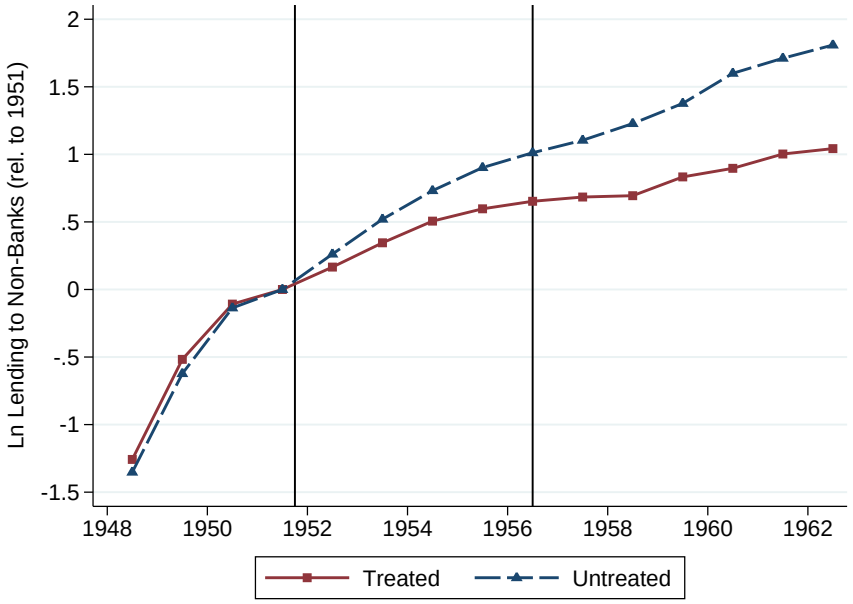
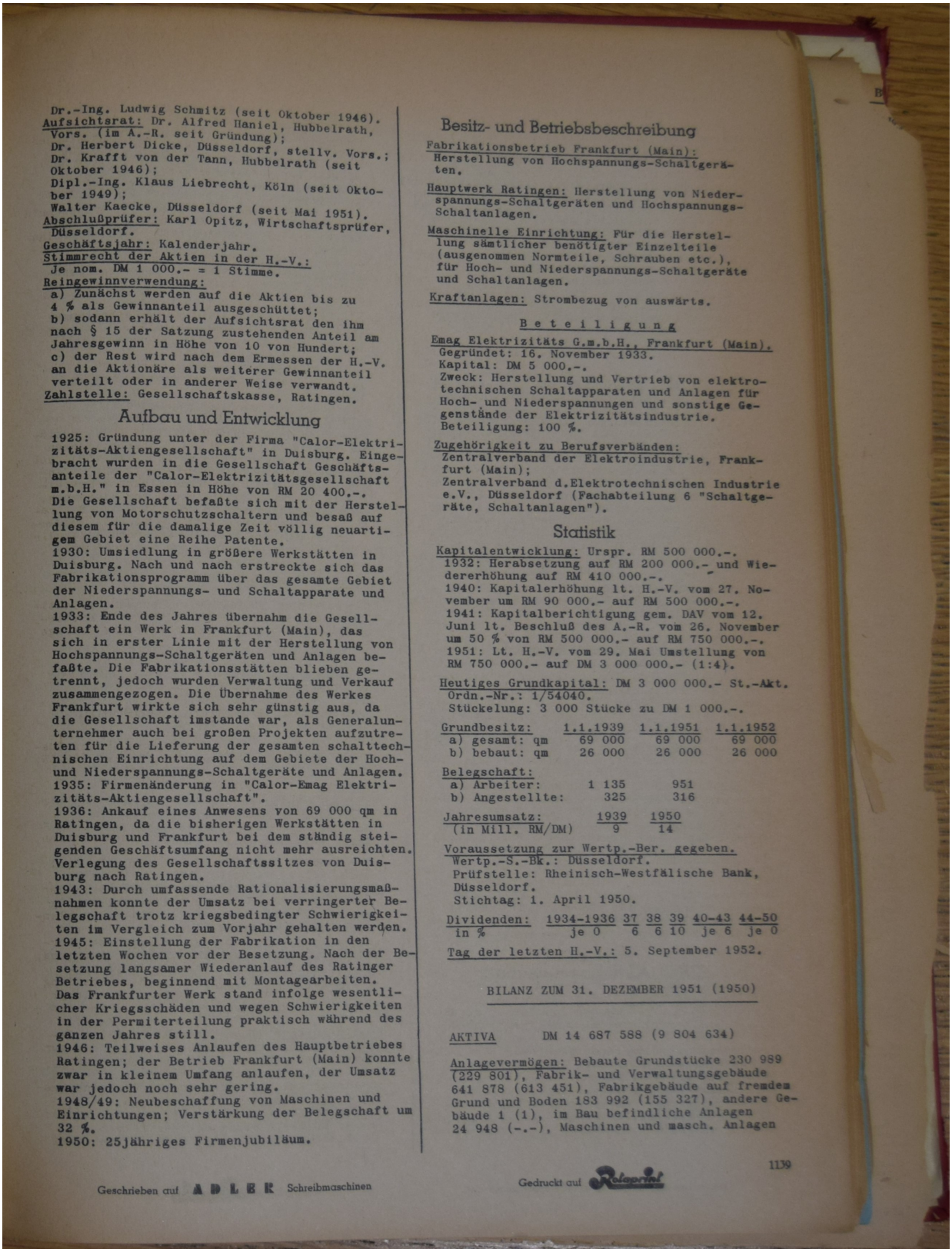
Figure A.II: Lending to non-banks (treated banks compared to all other banks)



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

Figure A.III: Photograph of a page from the 1952 *Handbuch der deutschen Aktiengesellschaften*



Dr.-Ing. Ludwig Schmitz (seit Oktober 1946).
Aufsichtsrat: Dr. Alfred Haniel, Hubbelrath, Vors. (im A.-R. seit Gründung);
Dr. Herbert Dicke, Düsseldorf, stellv. Vors.;
Dr. Krafft von der Tann, Hubbelrath (seit Oktober 1946);
Dipl.-Ing. Klaus Liebrecht, Köln (seit Oktober 1949);
Walter Kaecke, Düsseldorf (seit Mai 1951).
Abschlußprüfer: Karl Opitz, Wirtschaftsprüfer, Düsseldorf.
Geschäftsjahr: Kalenderjahr.
Stimmrecht der Aktien in der H.-V.:
Je nom. DM 1 000.- = 1 Stimme.
Reingewinnverwendung:
a) Zunächst werden auf die Aktien bis zu 4 % als Gewinnanteil ausgeschüttet;
b) sodann erhält der Aufsichtsrat den ihm nach § 15 der Satzung zustehenden Anteil am Jahresgewinn in Höhe von 10 von Hundert;
c) der Rest wird nach dem Ermessen der H.-V. an die Aktionäre als weiterer Gewinnanteil verteilt oder in anderer Weise verwandt.
Zahlstelle: Gesellschaftskasse, Ratingen.

Aufbau und Entwicklung

1925: Gründung unter der Firma "Calor-Elektrizitäts-Aktiengesellschaft" in Duisburg. Eingebracht wurden in die Gesellschaft Geschäftsanteile der "Calor-Elektrizitätsgesellschaft m.b.H." in Essen in Höhe von RM 20 400.-. Die Gesellschaft befaßte sich mit der Herstellung von Motorschutzschaltern und besaß auf diesem für die damalige Zeit völlig neuartigem Gebiet eine Reihe Patente.
1930: Umsiedlung in größere Werkstätten in Duisburg. Nach und nach erstreckte sich das Fabrikationsprogramm über das gesamte Gebiet der Niederspannungs- und Schaltapparate und Anlagen.
1933: Ende des Jahres übernahm die Gesellschaft ein Werk in Frankfurt (Main), das sich in erster Linie mit der Herstellung von Hochspannungs-Schaltgeräten und Anlagen befaßte. Die Fabrikationsstätten blieben getrennt, jedoch wurden Verwaltung und Verkauf zusammengezogen. Die Übernahme des Werkes Frankfurt wirkte sich sehr günstig aus, da die Gesellschaft imstande war, als Generalunternehmer auch bei großen Projekten aufzutreten für die Lieferung der gesamten schalttechnischen Einrichtung auf dem Gebiete der Hoch- und Niederspannungs-Schaltgeräte und Anlagen.
1935: Firmenänderung in "Calor-Emag Elektrizitäts-Aktiengesellschaft".
1936: Ankauf eines Anwesens von 69 000 qm in Ratingen, da die bisherigen Werkstätten in Duisburg und Frankfurt bei dem ständig steigenden Geschäftsumfang nicht mehr ausreichten. Verlegung des Gesellschaftssitzes von Duisburg nach Ratingen.
1943: Durch umfassende Rationalisierungsmaßnahmen konnte der Umsatz bei verringerter Belegschaft trotz kriegsbedingter Schwierigkeiten im Vergleich zum Vorjahr gehalten werden.
1945: Einstellung der Fabrikation in den letzten Wochen vor der Besetzung. Nach der Besetzung langsamer Wiederanlauf des Ratinger Betriebes, beginnend mit Montagearbeiten. Das Frankfurter Werk stand infolge wesentlicher Kriegsschäden und wegen Schwierigkeiten in der Permiterteilung praktisch während des ganzen Jahres still.
1946: Teilweises Anlaufen des Hauptbetriebes Ratingen; der Betrieb Frankfurt (Main) konnte zwar in kleinem Umfang anlaufen, der Umsatz war jedoch noch sehr gering.
1948/49: Neubeschaffung von Maschinen und Einrichtungen; Verstärkung der Belegschaft um 32 %.
1950: 25jähriges Firmenjubiläum.

Geschrieben auf ADLER Schreibmaschinen

Besitz- und Betriebsbeschreibung

Fabrikationsbetrieb Frankfurt (Main): Herstellung von Hochspannungs-Schaltgeräten.

Hauptwerk Ratingen: Herstellung von Niederspannungs-Schaltgeräten und Hochspannungs-Schaltanlagen.

Maschinelle Einrichtung: Für die Herstellung sämtlicher benötigter Einzelteile (ausgenommen Normteile, Schrauben etc.), für Hoch- und Niederspannungs-Schaltgeräte und Schaltanlagen.

Kraftanlagen: Strombezug von auswärts.

B e t e i l i g u n g

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

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

Statistik

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

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

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

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

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

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

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

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

BILANZ ZUM 31. DEZEMBER 1951 (1950)

AKTIVA DM 14 687 588 (9 804 634)

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

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Gedruckt auf Rotaprint

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Table A.I: Firm summary statistics

	Observations	Mean	Std. Dev.	p10	p50	p90
Panel A: Stock corporations						
Employment	1,251	1,625	5,488	23	354	3,405
Age	2,182	67	52	26	57	111
Assets	1,948	23.1	132.9	0.6	3.9	37.8
Stock capital / assets	1,872	0.37	0.20	0.14	0.34	0.63
Bank debt / assets	1,208	0.10	0.11	0	0.06	0.23
Number of relationship banks	2,188	3.18	2.08	1	3	6
Relationship bank treated in 1952/57	2,188	0.68	0.47	0	1	1
Relationship bank treated in 1952	2,188	0.46	0.50	0	0	1
Bank debt growth 1951-60	421	0.01	0.15	-0.22	0.03	0.21
$\Delta\frac{100 \cdot Bank\,debt}{Assets}$ 1951-60	421	-0.11	1.39	-1.77	-0.11	1.79
Employment growth 1951-60	815	0.03	0.05	-0.03	0.03	0.09
Revenue per worker growth 1951-60	344	0.05	0.05	0.00	0.04	0.10
Panel B: Non-stock firms						
Employment	1,800	559	1121	91	344	1,017
Age	3,494	63	51	16	54	112
Exporter	2,593	0.39	0.49	0	0	1
Number of relationship banks	3,706	2.54	1.29	1	2	4
Relationship bank treated in 1952/57	3,706	0.69	0.46	0	1	1
Relationship bank treated in 1952	3,706	0.41	0.49	0	0	1
Employment growth 1951-56	1,521	0.04	0.07	-0.01	0.03	0.13

Notes: The data are digitized by hand from Hoppenstedt volumes. The variables in levels are for the year 1951. Assets are in million Deutsche Mark. Growth is the average annual symmetric growth rate, i.e., the symmetric growth rate over the entire period divided by the number of years in the period. $\Delta\frac{100 \cdot Bank\,debt}{Assets}$ is the change in the percent ratio of bank debt over assets from 1951 to 1960, divided by 9, the number of years between 1951 and 1960. "Relationship bank treated in 1952/57" is an indicator for whether a bank treated in 1952 or 1957 was among the firm's relationship banks in 1951. "Relationship bank treated in 1952" is an indicator for whether a bank treated in 1952 was among the firm's relationship banks in 1951. Exporter is an indicator for whether the firm exported any of its products.

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Table A.II: Characteristics of firms with a treated relationship bank

	(1)	(2)	(3)	
	Rel. bank		Rel. bank	
Outcome	treated in 1952/57		treated in 1952	
Employment	0.063	0.064	0.001	-0.002
	(0.009)	(0.015)	(0.014)	(0.023)
Age	0.043	0.101	-0.012	0.045
	(0.011)	(0.040)	(0.019)	(0.056)
Stock capital / assets		-0.008		-0.022
		(0.050)		(0.050)
Bank debt / assets		-0.002		0.014
		(0.018)		(0.021)
Observations	3,515	492	811	158
R^2	0.040	0.078	0.000	0.010
Sample	Full		Focused	

Notes: The data are for the year 1951. The outcome in columns 1 and 2 is an indicator for whether a bank treated in 1952 or 1957 was among the firm's relationship banks in 1951. The outcome in columns 3 and 4 is an indicator for whether a bank treated in 1952 was among the firm's relationship banks in 1951. All regressors are in natural logarithms. Standard errors are clustered at the level of the firm's county.

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

	(1)	(2)	(3)
Outcome	Employment Growth 1949-51		
Rel. bank treated	0.005	0.004	0.004
in 1952/57	(0.011)	(0.023)	(0.023)
Rel. bank treated		0.001	-0.001
in 1952		(0.024)	(0.024)
Rel. bank treated			0.001
in 1952/57 * Stock Corporation FE			(0.044)
Rel. bank treated			0.028
in 1952 * Stock Corporation FE			(0.032)
Observations	1,147	1,147	1,147
R^2	0.146	0.146	0.147
Controls*zone FE	Yes	Yes	Yes
Sample	Full	Full	Full

Notes: The outcome is the average annual symmetric growth rate of employment, i.e., the symmetric growth rate from 1949 to 1951 divided by 2, the number of years between 1949 and 1951. Stock corporation FE is an indicator for stock corporations. The remaining regressors and controls are explained in Table I. The sample contains all stock corporations and non-stock firms with employment data in 1949 and 1951. Standard errors are clustered at the level of the firm's county.

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

	(1)	(2)	(3)	(4)	(5)
Outcome	Employment Growth 1951-56				
Rel. bank treated	-0.008			-0.001	-0.001
in 1952	(0.006)			(0.004)	(0.005)
0 < Fraction rel. banks		-0.003			
treated in 1952 $\leq$ 0.5		(0.005)			
0.5 < Fraction rel. banks		0.002			
treated in 1952 $\leq$ 1		(0.007)			
Commerzbank rel.			-0.001		
bank treated in 1952			(0.006)		
Deutsche Bank rel.			-0.004		
bank treated in 1952			(0.004)		
Dresdner Bank rel.			0.002		
bank treated in 1952			(0.004)		
Observations	889	1,472	1,472	1,472	1,472
R^2	0.062	0.063	0.063	0.063	0.063
Controls*zone FE	Yes	Yes	Yes	Yes	Yes
Robustness test	Excluding war-related producers	Treatment: intensive margin	Treatment: three banking groups	Cluster at bank level	Young's (2016) cluster correction

Notes: The controls are explained in Table I. Column 1 excludes from the sample firms that the Reichswehr identified as important for armament production (Hansen 1978) and firms in industries that produced war-related products (mining, clothes & textiles, chemicals & pharmaceuticals, metals manufacturing, electric & electronics, production of machinery). Column 2 tests for heterogeneity by firms' intensive margin dependence on treated banks. The two regressors are dummies based on the "fraction relationship banks treated", which is the firm's number of treated relationship banks divided by the firm's total number of relationship banks in 1951. Column 3 tests for heterogeneity by the three treated banking groups. Standard errors in columns 1 to 3 are clustered at the level of the firm's county. Column 4 clusters standard errors at the level of the state-level treated banks (i.e., 31 categories, one for each of the 30 treated state-level banks and one for firms with no treated relationship bank). Column 5 uses the effective degrees of freedom correction for clustered standard errors proposed by Young (2016).

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Table A.V: Effects on opaque firms are statistically different from effects on non-opaque firms

	(1)	(2)	(3)	(4)
Outcome	$\Delta\frac{Bk\ debt}{Assets}$ 1951-60		Empl. growth 1951-56	
Rel. bank treated	0.001	0.001		
in 1952/57	(0.002)	(0.002)		
Rel. bank treated	-0.009			
in 1952/57 * Opaque firm	(0.005)			
0 < Fraction rel. banks		-0.008		
treated in 1952/57$\leq$ 0.5 * Opaque firm		(0.005)		
0.5 < Fraction rel. banks		-0.012		
treated in 1952/57 $\leq$ 1 * Opaque firm		(0.007)		
Rel. bank treated			0.002	0.002
in 1952			(0.005)	(0.006)
0 < Fraction rel. banks			-0.013	-0.013
treated in 1952 $\leq$ 0.5 * Opaque firm			(0.009)	(0.020)
0.5 < Fraction rel. banks			-0.021	-0.038
treated in 1952 $\leq$ 1 * Opaque firm			(0.012)	(0.016)
Opaque firm	0.001	0.001	0.009	0.043
	(0.004)	(0.004)	(0.008)	(0.015)
Observations	288	288	1,472	342
R^2	0.207	0.209	0.065	0.125
Controls*zone FE	Yes	Yes	Yes	No
Controls	No	No	No	Yes
Firm type	Stock	Stock	Non-Stock	Non-Stock
Sample	Full	Full	Full	Focused

Notes: The outcomes, regressors, and controls are explained in Table II (for columns 1 and 2) and to Table I (for columns 3 and 4). Standard errors are clustered at the level of the firm's county. A firm is opaque if in 1951 it had fewer than 50 employees, was younger than 10 years old, or was in the bottom 10 percent of industry asset tangibility (fixed tangible over total assets).

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Table A.VI: Opaque firms were less likely to add treated relationship banks

	(1)	(2)	(3)
Outcome	Fraction of treated rel. banks in 1970		
Opaque firm	-0.056	-0.054	
	(0.018)	(0.020)	
$0 <$ Employees < 20			-0.072
			(0.021)
$20 \leq$ Employees < 50			-0.086
			(0.027)
$0 <$ Ind. Tangibility < 0.15			-0.030
			(0.032)
$0.15 \leq$ Ind. Tangibility < 0.2			-0.012
			(0.043)
Observations	720	720	720
R^2	0.010	0.068	0.013
Zone and industry FE	No	Yes	No
Sample	Firms without treated rel. bank in 1951		

Notes: The outcome is the number of treated relationship banks divided by the total number of relationship banks in 1970. Standard errors are clustered at the level of the firm's county. For the purpose of this table, I classify firms as opaque if in 1951 they had fewer than 50 employees or were in the bottom 10 percent of industry asset tangibility. This definition does not include firms younger than 10 years, because these firms were at least 18 years old in 1970. But in an unreported robustness check, I find that firms founded after 1965 had a significantly lower fraction of treated relationship banks in 1970. Similarly, firms with fewer than 50 employees in 1970 had a significantly lower fraction of treated relationship banks in 1970.

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Table A.VII: No effect on the bank relationships of opaque firms that already had a treated relationship bank

	(1)	(2)
	Fraction of treated rel. banks in	
Outcome	1970	1951
Opaque firm	0.014	0.023
	(0.018)	(0.014)
Observations	1,647	2,285
R^2	0.321	0.001
Fraction of treated rel. banks in 1951	Yes	No
Sample	Firms with treated rel. bank in 1951	

Notes: The outcome is the number of treated relationship banks divided by the total number of relationship banks in the given year. A firm is opaque if it has fewer than 50 employees in 1951 or is in the bottom 10 percent of industry asset tangibility (fixed tangible over total assets). Standard errors are clustered at the level of the firm's county.

Table A.VIII: Effects on non-opaque firms, large firms, and firms with both stock capital and bank debt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Employment Growth						
Outcome	1951-56	1951-60	1951-56	1951-60	1951-56	1951-60	1951-60
Rel. bank treated in 1952/57	0.002		-0.009		-0.003		-0.000
	(0.005)		(0.009)		(0.016)		(0.008)
Rel. bank treated in 1952		-0.002		-0.002		0.006	
		(0.005)		(0.005)		(0.007)	
Observations	1,177	525	382	293	125	181	312
R^2	0.057	0.146	0.275	0.352	0.605	0.422	0.185
Controls*zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm type	Non-Stock	Stock	Non-Stock	Stock	Non-Stock	Stock	Stock
Sample	Not opaque		Empl. > 500		Empl. > 1,000		Both stock capital & bank debt in 1951

Notes: The outcomes, regressors, and controls are explained in Table I (for odd-numbered columns) and Table II (for even-numbered columns). A firm is opaque if in 1951 it had fewer than 50 employees, was younger than 10 years old, or was in the bottom 10 percent of industry asset tangibility (fixed tangible over total assets). The samples in columns 1 and 2 include only non-opaque firms; in columns 3 and 4 firms that had over 500 employees in at least one year between 1951 and 1960; in columns 5 and 6 firms that had over 2,000 employees in at least one year between 1951 and 1960; and in column 7 firms that financed themselves with both stock capital and bank debt in 1951. Standard errors are clustered at the level of the firm's county.

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Table A.IX: Effects on liquidation of firms

	(1)	(2)
Outcome	Liquidation 1951-60	Liquidation 1960-68
Rel. bank treated in 1952/57	0.007	0.010
	(0.006)	(0.017)
Observations	1,346	908
R^2	0.072	0.072
Controls*zone FE	Yes	Yes

Notes: The outcomes are indicators for whether the firm went into liquidation between 1951 and 1960 (column 1) and between 1960 and 1968 (column 2). The regressor and controls are explained in Table II. Standard errors are clustered at the level of the firm's county.

Table A.X: Effects on firms did not differ in federal states with a persistent capital account deficit

	(1)	(2)
	Employment Growth	
Outcome	1951-56	1951-60
Rel. bank treated in 1952	-0.002	
	(0.007)	
Rel. bank treated in 1952 * Cap. acc. deficit	0.002	
	(0.006)	
Rel. bank treated in 1952/57		0.003
		(0.005)
Rel. bank treated in 1952/57 * Cap. acc. deficit		-0.007
		(0.006)
Observations	1,472	685
R^2	0.063	0.115
Controls*zone FE	Yes	Yes
Firm type	Non-Stock	Stock

Notes: The table tests whether effects were heterogeneous for firms located in German federal states that regularly ran a capital account deficit (indicated by "Cap. acc. deficit," i.e., states that had a persistent net capital outflow, as reported in Pohl 1971, page 40). The other regressors and controls are explained in Table I for column 1 and Table II for column 2. Standard errors are clustered at the level of the firm's county.

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Table A.XI: Effects on municipalities did not differ in federal states with a persistent capital account deficit

	(2)
Outcome	Empl. growth 1951-60
Treated bank branch	-0.017
	(0.006)
Treated bank branch	0.007
* Cap. acc. deficit	(0.011)
Observations	79
R^2	0.357
Federal state FE	Yes
Size bin FE	Yes
Ruhr FE	Yes

Notes: The table tests whether effects were heterogeneous for municipalities located in German federal states that regularly ran a capital account deficit (indicated by "Cap. acc. deficit," i.e., states that had a persistent net capital outflow, as reported in Pohl 1971, page 40). The other regressors and controls are explained in Table V. Standard errors are robust.

Table A.XII: No effect on the bank relationships of risky firms that already had a treated relationship bank

	(1)	(2)	(3)	(4)
	Fraction of treated rel. banks in			
Outcome	1970	1970	1951	1951
Low volatility	0.017		-0.018	
	(0.018)		(0.016)	
Low leverage ($\frac{Cap}{Assets} \geq 0.75$)		0.171		0.117
		(0.124)		(0.087)
Medium leverage ($0.75 > \frac{Cap}{Assets} \geq 0.25$)		0.034		0.025
		(0.024)		(0.023)
Observations	569	377	767	534
R^2	0.398	0.462	0.141	0.146
Controls*zone FE	Yes	Yes	Yes	Yes
Opaque firm FE	Yes	Yes	Yes	Yes
Fraction of treated rel. banks in 1951	Yes	Yes	No	No
Sample	Firms with treated rel. bank in 1951			

Notes: The outcome is the ratio of the number of treated relationship banks divided by the total number of relationship banks in the given year. A firm has low volatility if the standard deviation of its employment growth from 1946 to 1951 is below the median. Cap / assets is the ratio of stock capital over total assets. A firm is opaque if it has fewer than 50 employees in 1951 or is in the bottom 10 percent of industry asset tangibility (fixed tangible over total assets). A firm is opaque if it has fewer than 50 employees in 1951, is younger than 10 years old in 1952, or is in the bottom 10 percent of industry asset tangibility (fixed tangible over total assets). The controls are explained in Table I. Standard errors are clustered at the level of the firm's county.

Table A.XIII: Risky firms that added a treated relationship bank did not grow faster

	(1)	(2)	(3)	(4)
Outcome	Empl. growth 1951-68	Revenue per worker growth 1951-68	Empl. growth 1951-68	Revenue per worker growth 1951-68
Risky firm that added a bank treated in 1952/57 as rel. bank	-0.002	0.003	0.001	-0.008
	(0.024)	(0.009)	(0.010)	(0.006)
Observations	125	61	345	144
R^2	0.486	0.633	0.242	0.482
Controls*zone FE	Yes	Yes	Yes	Yes
Sample	Firms without treated rel. bank in 1951		Full	

Notes: The outcomes are the average annual symmetric growth rates of employment between 1951 and 1968 (columns 1 and 3) and the average annual symmetric growth rates of revenue per worker between 1951 and 1968 (columns 2 and 4). "Risky firm that added a bank treated in 1952/57 as rel. bank" is an indicator for whether the firm fulfilled two criteria: (1) the firm was risky (i.e., the ratio of stock capital over total assets in 1951 was below 0.25 or the standard deviation of employment growth from 1946 to 1951 was below the median); and (2) the firm had no treated relationship bank in 1951 but had a treated relationship bank in 1970. The controls are explained in Table I. Standard errors are clustered at the level of the firm's county.

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

	(1)	(2)	(3)	(4)
	Employment Growth			
Outcome	1949-51	1951-56	1949-51	1951-56
Rel. bank (as of 1940)	0.001	-0.001	0.027	-0.061
treated in 1952	(0.031)	(0.010)	(0.076)	(0.013)
Observations	182	370	25	51
R^2	0.374	0.157	0.175	0.338
Controls*zone FE	Yes	Yes	No	No
Basic Controls	No	No	Yes	Yes
Sample	Full		Opaque	

Notes: The outcomes are the average annual symmetric growth rates of employment in the given period. "Relationship bank (as of 1940) treated in 1952" is an indicator for whether one of the firm's 1940 relationship banks was treated in the first reform of 1952. A firm is opaque if in 1951 it had fewer than 50 employees, was younger than 10 years old, or was in the bottom 10 percent of industry asset tangibility (fixed tangible over total assets). The controls*zone FE correspond to the standard controls from Table I. The small sample sizes in columns 3 and 4 necessitate the use of a reduced set of controls. The controls in columns 3 and 4 include a fixed effect for manufacturing firms, fixed effects for four bins of firm employment in 1951 (1-49, 50-249, 250-999, 1000+ employees), and the natural logarithm of the firm's age. Standard errors are clustered at the level of the firm's county.

Table A.XV: Were the firm-level effects stronger in municipalities where few firms had a treated relationship bank?

	(1)	(2)	(3)
Outcome	Employment Growth 1951-56		
Definition of low-treatment municipality	<10%	<20%	<30%
Rel. bank treated	-0.002	0.002	0.002
in 1952	(0.006)	(0.006)	(0.006)
Low-treatment municipality	-0.001	0.008	0.010
	(0.008)	(0.009)	(0.008)
Rel. bank treated *	0.006	-0.009	0.004
Low-treatment municipality	(0.017)	(0.014)	(0.014)
Observations	1,472	1,472	1,472
R^2	0.063	0.064	0.065
Controls*zone FE	Yes	Yes	Yes

Notes: Low-treatment municipality is an indicator for firms located in municipalities where the fraction of firms with a treated relationship bank in 1951 is below the percentage indicated in the first row of the table. The outcome, remaining regressors, and controls are explained in Table I. Standard errors are clustered at the level of the firm's county.

Table A.XVI: Characteristics of municipalities with treated bank branches

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment share in municipality of				Municipality located in			
Outcome	manu-facturing	public sector	primary sector	war-time displaced	northern zone	western zone	southern zone	Ruhr region
Treated bank branch	0.007	0.017	0.003	-0.023	-0.045	-0.030	0.076	0.030
	(0.034)	(0.026)	(0.026)	(0.024)	(0.160)	(0.216)	(0.205)	(0.162)
Constant	0.230	0.175	0.283	0.121	0.167	0.500	0.333	0.167
	(0.029)	(0.025)	(0.074)	(0.022)	(0.154)	(0.207)	(0.195)	(0.154)
Observations	72	72	72	72	72	72	72	72
R^2	0.000	0.004	0.000	0.009	0.001	0.000	0.002	0.000

Notes: The table reports the relationship between municipality observables and having a treated bank branch. The outcome is listed in the top row of the table and refers to the year 1950. The regressors are an indicator for whether the municipality had a treated bank branch in 1952 and a constant. Standard errors are robust.

Table A.XVII: The municipality results are not driven by differences in municipality size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample of municipalities	Full Sample		Smaller than largest municipality without treated bank branch		Smaller than median		Full Sample		
Outcome	Ln size	Empl. growth 1951-60	Ln size	Empl. growth 1951-60	Ln size	Empl. growth 1951-60	Empl. growth 1951-60		
Treated bank branch	0.471	-0.013	0.099	-0.015	-0.027	-0.015	-0.010	-0.013	-0.011
	(0.166)	(0.005)	(0.148)	(0.006)	(0.100)	(0.010)	(0.005)	(0.005)	(0.005)
Observations	79	79	69	69	40	40	79	79	79
R^2	0.028	0.350	0.005	0.363	0.001	0.328	0.210	0.272	0.390
Federal state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ruhr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size bin FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Ln size	No	No	No	No	No	No	Yes	No	No
Size tercile FE	No	No	No	No	No	No	No	Yes	No
Size decile FE	No	No	No	No	No	No	No	No	Yes

Notes: Columns 1, 3, and 5 report the relationship between municipality size (measured as ln number of employees in 1950) and having a treated bank branch. Columns, 2, 4, 6, 7, 8, and 9 report the effect of having a treated bank branch on the employment growth in the municipality from 1951 to 1960 (measured as average annual symmetric growth rates). The sample in columns 3 and 4 includes only municipalities where total employment in 1950 was below total employment of the largest municipality that did not have a treated bank branch. The sample in columns 5 and 6 includes only municipalities where total employment in 1950 was below the median. The regressors are explained in Table V. Standard errors are robust.

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Table A.XVIII: Effects on the volatility of bank balance sheet figures

	(1)	(2)	(3)	(4)	(5)	(6)
	Conditional absolute deviation in growth of					
Outcome	Assets	Lending	Deposits	Assets	Lending	Deposits
Treated banking group *	0.010	0.026	0.038	0.010	0.027	0.040
Post 1952	(0.009)	(0.019)	(0.045)	(0.009)	(0.020)	(0.046)
Treated banking group *	-0.002	0.010	-0.014	-0.003	0.009	-0.015
Post 1957	(0.005)	(0.007)	(0.012)	(0.005)	(0.008)	(0.012)
Treated banking group *				0.002	0.004	0.013
Recession 1966/67				(0.007)	(0.008)	(0.010)
Observations	287	308	308	287	308	308
R^2	0.380	0.416	0.463	0.380	0.416	0.464
Banking group FE and time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The data are from the Deutsche Bundesbank and aggregated at the level of seven different banking groups. The groups are: the treated banks, untreated commercial banks, private banks, savings banks, cross-regional cooperative banks, regional cooperative banks, and mortgage banks. The data include two observations per year from 1948 to 1970 for each banking group (asset data from 1949). To calculate the outcome, I regress the change in the given financial variable C_{gt} (growth in assets, lending, or deposits for banking group g in year t) on a full set of fixed effects for banking groups (α_g) and for time (α_t): $C_{gt} = \alpha_g + \alpha_t + \varepsilon_{gt}$. The absolute value of the residual ($|\varepsilon_{gt}|$) is the outcome variable, a measure of the fluctuation in growth. Intuitively, it is the absolute deviation of a given banking group's growth relative to the average growth of all banking groups in that year and relative to the given banking group's average growth over all years. In columns 1 to 3, I regress the fluctuation measure on indicators for the periods after the 1952 reform (post 1952) and after the 1957 reform (post 1957), interacted with an indicator for the treated banking group. In columns 4 to 6, I add an indicator for the recession years 1966/67, interacted with the treated banking group indicator. All specifications control for time fixed effects and banking group fixed effects. Standard errors are clustered at the level of the banking group.

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Table A.XIX: Effects on the volatility of firm growth

	(1)	(2)	(3)	(4s)
Outcome	Conditional absolute deviation in growth of firm Employment		Revenue	
Rel. bank treated in 1952 * post 1952	0.006	0.006	0.006	-0.000
	(0.008)	(0.008)	(0.008)	(0.012)
Rel. bank treated in 1957 * post 1957	-0.005	-0.007	0.008	0.014
	(0.007)	(0.007)	(0.010)	(0.011)
Observations	11,750	8,628	7,034	4,717
R^2 (within firm)	0.000	0.050	0.055	0.107
Mean (std. dev.) of outcome	0.073 (0.106)		0.111 (0.140)	
Firm FE and year FE	Yes	Yes	Yes	Yes
Industry FE*zone FE*(post 1952, post 1957)	No	Yes	No	Yes
ln age*zone FE*(post 1952, post 1957)	No	Yes	No	Yes
Size bin FE*zone FE*(post 1952, post 1957)	No	Yes	No	Yes

Notes: To calculate the outcome, I use a first stage regression of the annual symmetric growth rate on firm and year fixed effects. The absolute value of the residual from the first stage regression is the outcome variable in the specifications reported in the table. Intuitively, it measures the absolute deviation of a given firm's growth relative to the average growth of all firms in that year and relative to the given firm's average growth over all years. I regress the fluctuation measure on indicators for the periods after the 1952 reform (post 1952) and after the 1957 reform (post 1957), interacted with indicators for whether a firm's relationship bank was treated in the given year. The remaining regressors and controls are explained in Table I. The data are from an unbalanced firm panel from 1946 to 1970. Standard errors are clustered at the level of the firm's county.

Table A.XX: Effects on the standard deviation of firm growth

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Difference in std. dev. of empl. growth				Difference in std. dev. of revenue growth	
Rel. bank treated	0.016	0.014	0.009	0.025	0.042	0.020
in 1952/57	(0.018)	(0.017)	(0.017)	(0.017)	(0.026)	(0.035)
Observations	372	349	329	311	301	236
R^2	0.001	0.145	0.001	0.197	0.008	0.293
Mean (std. dev.) of outcome	-0.006 (0.201)		-0.013 (0.159)		-0.080 (0.225)	
Industry FE*zone FE	No	Yes	No	Yes	No	Yes
ln age*zone FE	No	Yes	No	Yes	No	Yes
Size bin FE*zone FE	No	Yes	No	Yes	No	Yes
Excludes 1952-56	No	No	Yes	Yes	No	No

Notes: The table estimates the effect of having a relationship bank treated in 1952 on changes in the volatility of firm employment growth. The outcome is the difference in the standard deviation of firm employment or sales growth for the years after the 1952 reform versus before the 1952 reform. Columns 1 and 2 analyze the standard deviation of employment growth using all years with available employment data, while columns 3 and 4 exclude the years from 1952 to 1956, i.e., the years between the first and the second reform. Columns 5 and 6 analyze the standard deviation of revenue growth using all years with available sales data. The regressors and controls are explained in Table I. The data are from an unbalanced firm panel from 1946 to 1970. Standard errors are clustered at the level of the firm's county.

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Table A.XXI: Effects on the volatility of municipality growth

	(1)	(2)
Outcome	Conditional absolute deviation in growth of municipal employment	
Bank branches joint in 1952 * post 1952	0.005	-0.002
	(0.003)	(0.009)
Bank branches joint in 1957 * post 1957	-0.006	-0.005
	(0.004)	(0.005)
Observations	1,665	1,665
R^2 (within municipality)	0.348	0.357
Mean (std. dev.) of outcome	0.027 (0.033)	
Municipality FE and time FE	Yes	Yes
Zone FE*(post 1952, post 1957)	No	Yes
Size bin FE*(post 1952, post 1957)	No	Yes
Ruhr FE*(post 1952, post 1957)	No	Yes

Notes: To calculate the outcome, I use a first stage regression of the annual symmetric employment growth rate on municipality and time fixed effects. The absolute value of the residual from the first stage regression is the outcome variable in the specifications reported in the table. Intuitively, it measures the absolute deviation of a given municipality's growth relative to the average growth of all municipalities in that time and relative to the given municipality's average growth over all time periods. I regress the fluctuation measure on indicators for the periods after the 1952 reform (post 1952) and after the 1957 reform (post 1957), interacted with indicators for whether the municipality had a bank branch treated in the given time. The remaining regressors and controls are explained in Table V. The data are from an unbalanced panel of municipalities with two observations per year from 1948 to 1961. Standard errors are clustered at the level of the municipality.

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Table A.XXII: Effects on the synchronization of firm growth

	(1)	(2)	(3)	
	Absolute difference between firms in conditional growth of			
Outcome	Employment		Revenue	
Rel. banks consolidated in 1952 * post 1952	-0.001	-0.003	-0.007	-0.003
	(0.006)	(0.005)	(0.011)	(0.010)
Rel. banks consolidated in 1957 * post 1957	-0.000	-0.000	-0.008	-0.012
	(0.003)	(0.004)	(0.007)	(0.007)
Observations	5,276,448	3,086,033	1,250,952	556,437
R^2 (within firm pair)	0.014	0.019	0.009	0.014
Mean (std. dev.) of outcome	0.114 (0.143)		0.151 (0.166)	
Firm pair FE and year FE	Yes	Yes	Yes	Yes
Same zone FE*(post 1952, post 1957)	No	Yes	No	Yes
Same industry FE*(post 1952, post 1957)	No	Yes	No	Yes
Same age FE*(post 1952, post 1957)	No	Yes	No	Yes
Same size bin FE*(post 1952, post 1957)	No	Yes	No	Yes

Notes: Each observation in the dataset is for a pair of firms a and b. The outcome is the absolute difference in conditional growth between a and b, where conditional growth is the residual from a first stage regression of a firm's annual symmetric growth rate on firm and time fixed effects. Intuitively, the outcome measures the absolute difference in growth between two firms, accounting for permanent differences in growth rates across firms and for common differences in growth rates across years. The reported regressors are indicators for whether the firms had relationship banks in 1951 that were not part of the same bank before the given year, but consolidated because of the banking reform in the given year, interacted with indicators for the periods after the 1952 reform and 1957 reform, respectively. All specifications include fixed effects for each pair of firms and fixed effects for each year. The additional controls are indicators for whether the municipalities in the pair were in the same zone, the same industry, of the same age, and in the same size bin, interacted with indicators for the periods after the 1952 and 1957 reforms. The data are from an unbalanced firm panel from 1946 to 1970. Standard errors are two-way clustered at the level of the counties of the two firms.

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Table A.XXIII: Effects on the synchronization of municipality growth

	(1)	(2)
Outcome	Absolute difference between municipalities in conditional growth of employment	
Branches consolidated in 1952 * post 1952	-0.0005	-0.0003
	(0.0020)	(0.0022)
Branches consolidated in 1957 * post 1957	-0.0001	-0.0002
	(0.0012)	(0.0017)
Observations	52,534	52,534
R^2 (within municipality pair)	0.107	0.108
Mean (std. dev.) of outcome	0.033 (0.042)	
Municipality pair FE and time FE	Yes	Yes
Same zone FE*(post 1952, post 1957)	No	Yes
Same size bin FE*(post 1952, post 1957)	No	Yes
Both in Ruhr FE*(post 1952, post 1957)	No	Yes

Notes: Each observation in the dataset is for a pair of municipalities, i and j. The outcome is the absolute difference in the conditional growth of employment between i and j, where conditional growth is the residual from a first stage regression of a municipality's symmetric employment growth rate on municipality and time fixed effects. Intuitively, the outcome measures the absolute difference in growth between two municipalities, accounting for permanent differences in growth rates across municipalities and for common differences in growth rates across time. The reported regressors are indicators for whether the municipalities had branches of banks that were not part of the same bank before the given year, but became part of the same bank because of the banking reform in the given year, interacted with indicators for the periods after the 1952 reform and 1957 reform, respectively. All specifications include fixed effects for each pair of municipalities and fixed effects for each year. The additional controls are indicators for whether the municipalities in the pair were in the same zone, in the same size bin, and both in the Ruhr region, interacted with indicators for the periods after the 1952 and 1957 reforms. The data are from an unbalanced panel with two observations per year from 1948 to 1961. Standard errors are two-way clustered at the level of the two municipalities.

Table A.XXIV: The growth and productivity of firms that added or dropped treated relationship banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Liquidation 1960-68	Liquidation 1951-60	Revenue per worker growth 1960-68	Revenue per worker growth 1951-60	Empl. growth 1960-68	Empl. growth 1951-60	Revenue per worker 1968	Revenue per worker 1951
Added a bank treated	-0.001	-0.004	0.006	-0.011	-0.010	0.001	-0.153	-0.019
in 1952/57 as rel. bank	(0.028)	(0.003)	(0.010)	(0.010)	(0.009)	(0.007)	(0.210)	(0.039)
Dropped all banks treated	-0.030	0.018	0.011	0.003	0.007	-0.005	0.013	-0.006
in 1952/57 as rel. banks	(0.014)	(0.023)	(0.014)	(0.017)	(0.011)	(0.008)	(0.071)	(0.038)
Never had a bank treated	-0.003	-0.002	0.004	0.004	-0.000	-0.001	-0.124	0.086
in 1952/57 as rel. bank	(0.018)	(0.005)	(0.007)	(0.007)	(0.005)	(0.005)	(0.149)	(0.080)
Observations	865	1,336	313	293	674	681	1,009	1,258
R^2	0.064	0.022	0.177	0.259	0.131	0.127	0.041	0.061
Controls*zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The outcomes are: indicators for whether the firm went into liquidation between 1960 and 1968 (column 1) and between 1951 and 1960 (column 2); the average annual symmetric growth rates of employment between 1960 and 1968 (column 3) and between 1951 and 1960 (column 4); the average annual symmetric growth rates of revenue per worker between 1960 and 1968 (column 5) and between 1951 and 1960 (column 6); and revenue per worker in 1951 (column 7) and in 1960 (column 8). "Added a bank treated in 1952/57 as rel. bank" is an indicator for whether the firm had no treated relationship bank in 1951 but had a treated relationship bank in 1960. "Dropped all banks treated in 1952/57 as rel. banks" is an indicator for whether the firm had a treated relationship bank in 1951 but no treated relationship bank in 1960. "Never had a bank treated in 1952/57 as rel. bank" is an indicator for whether the firm did not have a treated relationship bank in both 1951 and in 1960. The controls are explained in Table II (columns 1 to 6 since they contain only stock corporations) and Table I (columns 7 and 8 since they contain mainly non-stock firms). Standard errors are clustered at the level of the firm's county.

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Appendix B Theoretical Model

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

Firms I assume that a firm can only borrow from one relationship bank. The motivation for this assumption is that German firms rely on one or a few relationship banks for financial services. This implies that a relationship bank holds a "bilateral monopoly" over each relationship borrower (Boot 2000). Firm ib maximizes profits π_{ib}:

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

Capital K_{ib} is the sole input, which the firm borrows at an interest rate r_{ib} from its relationship bank. The firm takes the interest rate as given. A_{ib} captures all exogenous factors shifting the firm's demand for capital, such as productivity or demand for the firm's products. The returns-to-scale production parameter is α, where α is time-invariant and $0 < \alpha < 1$. The firm's optimal demand for capital is given by:

$$\alpha A_{ib}K_{ib}^{(\alpha-1)} = r_{ib}. \tag{A1}$$

Banks Bank b lends to a total of n_b relationship borrowers. The bank takes the total number of borrowers as given. It earns interest income, which is the interest rate charged to each borrower, r_{ib}, multiplied by the amount of capital lent to that firm, K_{ib}, summed over all firms. The bank also takes as given the capital demand of each relationship borrower (equation A1).

Banks pay a constant marginal cost for each unit of lent capital, $c(n_b, \beta_b)$. This marginal cost includes expenditures on risk management, employees, and deposits. The marginal cost is a function of bank efficiency parameter β_b and the total number of borrowers n_b. The marginal cost is decreasing in bank efficiency β_b. An increase in the number of borrowers n_b can affect bank operations through multiple channels, as outlined in Section VII. The net effect of bank size on bank costs is theoretically ambiguous. Each bank maximizes profits π_b:

$$\pi_b = \sum_{i=1}^{n_b} [r_{ib}K_{ib} - c(n_b, \beta_b)K_{ib}], \tag{A2}$$

where the first term in the bracket is the interest income from lending to firm ib and the second term in the bracket is the total cost of lending to firm ib.

Equilibrium Combining equations A1 and A2 and taking the first-order condition gives the optimal amount of K_{ib}. This amount increases with the exogenous capital demand shock A_{ib} and

decreases with the marginal cost of lending $c(n_b, \beta_b)$:

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

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

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

where ϕ is either positive or negative (since the effect of size on marginal cost is ambiguous) and κ is strictly positive (since the effect of efficiency on cost is strictly positive). Under this assumption, the change in capital lent to firm ib from period t to period t' is given by:

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

The operator $\Delta^{t,t'}[.]$ indicates the growth of the variable in square brackets from t to t'. Changes in firm capital demand A_{ib}, the number of the bank's relationship borrowers n_b (i.e., bank size), and bank efficiency β_b determine the growth in capital lent. The coefficient $\frac{\phi}{1-\alpha}$ measures the causal effect of changes in bank size on firm growth, the key object of interest in this paper.

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

$$\Delta^{t,t'}[ln(K_{ib})] = \frac{\phi}{1-\alpha} \times \Delta^{t,t'}[n_b] + \upsilon_{ib}, \tag{A3}$$

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

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

A regression based on equation A3 estimates the true causal coefficient $\frac{\phi}{1-\alpha}$ if changes in firm capital demand and bank efficiency are uncorrelated with changes in bank size. However, banks do not become big randomly. For example, banks might strategically consolidate with other banks because they expect increases in the future efficiency of the other banks that are unrelated to size. Alternatively, a random shock to regional productivity can simultaneously increase the growth of incumbent bank borrowers and raise the size of banks operating in that region via higher firm entry. In both these examples, the observed correlation between bank size and firm growth would be positive, even if the true causal coefficient $\frac{\phi}{1-\alpha}$ is zero. Therefore, correlations between bank size and firm growth are not informative about how changes in bank size causally affect firm growth. A suitable empirical approach needs to identify a change in bank size that did not simultaneously affect firm capital

demand, bank efficiency, and other unobservable components of firm and bank performance.

I estimate the effect of the reforms by adapting equation A3. I replace the regressor with an indicator for whether one of the firm's relationship banks increased in size due to a postwar reform between the years t and t'. This regressor serves as proxy for an increase in the number of the bank's borrowers, i.e., the term $\Delta^{t,t'}[n_b]$ from equation A3.[A1] The outcome is still the growth of firm ib from period t to period t':

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

Since the error term is uncorrelated with the treatment indicator, θ measures the causal impact of having a treated relationship bank.

Appendix C Firm Data Construction and Summary Statistics

The main dataset builds on the 1952 and 1958/59 *Grossunternehmen* and the 1952/53 and 1961/62 *Aktiengesellschaften* volumes. Figure A.III displays a photograph of a page in the 1952/53 volume on *Aktiengesellschaften.* From these volumes, I digitize the records of all non-financial firms that, at a minimum, contain the names of the firm's relationship banks. There are 2,882 such stock corporations and 4,589 such non-stock firms in the 1952/53 volumes.

Using the firm name and address as identifiers, I perform a fuzzy match procedure (Stata command "reclink") to connect firm entries from the 1952/53 volumes to the 1958/59 and 1961/62 volumes. I check all matches by hand to ensure there are no errors. Firm exits, mergers, and liquidation proceedings are reported in individual firm records and in lists at the end of the Hoppenstedt volumes. To account for the mergers, I aggregate the employment and balance sheet values of all firms participating in the merger, record all their relationship banks, and keep only the aggregated observation in the dataset for the years before the merger. Overall, the match leaves 2,188 stock corporations and 3,706 non-stock firms in the dataset.

A Hoppenstedt volume reports data for one to three years prior to the release year of the volume. For instance, the 1952 volume mostly reports data for 1951, while the 1958/59 volume mostly covers 1956. For the firms in *Aktiengesellschaften*, I can calculate the growth of employment, revenue per worker, total assets, liabilities, and bank debt from 1951 to 1960. For the firms in *Grossunternehmen*, it is possible to calculate employment growth from 1951 to 1956. Some firm entries in the 1952/53 volumes report employment in 1949, so I can calculate the pre-reform growth of these firms from 1949 to 1951.

To supplement the main analyses, I collect additional data on relationship banks, employment, and revenue from the 1941 and 1970 *Grossunternehmen* and the 1970/71 *Aktiengesellschaften* vol-

[A1] In Tables III and A.IV, I also use treatment indicators based on the fraction of the firm's relationship banks that was treated.

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umes. No data on relationship banks exist in the *Aktiengesellschaften* volumes prior to 1952. I match 373 firms between the 1941 and 1952/53 volumes and 4,191 firms between the 1952/53 volumes and 1970/71 volumes.

The median stock corporation in the sample was of a similar size and age to the median non-stock firm (Table A.I). Both had close to 350 employees in 1951. The very largest firms were stock corporations, which means that the average stock corporation was larger than the average non-stock firm. Both stock capital and bank debt were important sources of stock corporations' financing, amounting to an average of 37 percent and 10 percent of total assets, respectively.

In the sample, 14 percent of stock corporations and 6 percent of non-stock firms have fewer than 50 employees. As a rough comparison, the fraction of establishments in the population with fewer than 50 employees was 98 percent in 1951 (Statistisches Bundesamt 1952). 70 percent of firms in the sample are in the manufacturing sector, compared to 32 percent of establishments in the population. All specifications in the results section control for firm size and industry, to ensure differences in size and industry do not drive the findings. I also explore heterogeneity related to size and industry.

Appendix D Robustness Checks for the Analysis of Firm Growth

The effect of having a treated relationship bank is similar when I exclude firms that might have participated in war-related production during World War II (column 1, Table A.IV). War-related firms might have grown more slowly in the postwar period, because they had to restructure their business model or because of Allied punishment. I exclude firms listed in Anlage Nr. 6 and 10 in Hansen (1978), which contributed to armaments production in the 1930s. I also exclude all war-related industries: mining, clothes & textiles, chemicals & pharmaceuticals, metals manufacturing, electric & electronics, and production of machinery.

The coefficients are stable when analyzing firms that had more than half of their relationship banks treated (column 2). All the firm-level results are similar across the three treated banking groups. This suggests that the results are not specific to one individual institution, but instead driven by more general mechanisms (column 3). Finally, I find similar results when I use relationship banks from 1940 to define the treatment indicators (Table A.XIV). This is not surprising, given that 87 percent of firms with a treated relationship bank in 1940 still had a treated relationship bank in 1951.

Appendix E Municipality Data

The municipality data are hand-digitized from the publication *Statistisches Jahrbuch deutscher Gemeinden.* I digitize employment data for 1951, 1956, and 1960, matching the years for which I have firm employment data. The annual bank reports identify whether a municipality had a treated bank branch. Sectoral employment shares are from the 1950 *Betriebszählung* (census of enterprises).

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Average employment in the municipalities in the sample was 64,992 in 1951. 86 percent of municipalities had a bank branch treated in either 1952 or 1957. 52 percent had a bank branch treated in the 1952 reform.

Appendix F Competitive Changes in Local Banking Markets

The competitive environment in local banking markets might have changed after the consolidations. For example, if the treated banks became more efficient after 1952, untreated banks might have been forced to raise their efficiency as well. This would imply that there was no difference between firms with a treated relationship bank and other firms in the same local banking market, even though all banks' efficiency improved after the consolidations.

To investigate this possibility, I test whether the effect of having a treated relationship bank differed in municipalities where treated branches only formed a small part of the market. The impact of the consolidations on competitive pressures was likely weaker in such municipalities. I find no evidence that treated firms grew more quickly than untreated firms in municipalities with low exposure to the treated banks (Table A.XV). This suggests that even in municipalities where competitive pressures were relatively weak there was no benefit to having a treated relationship bank.

Appendix G Robustness Checks for the Analysis of Municipality Growth

I regress municipality characteristics on a constant and an indicator for whether the municipality had a treated bank branch. I analyze all municipality characteristics that are reported in my municipal data sources *Statistisches Jahrbuch deutscher Gemeinden* and the 1950 *Betriebszählung* (census of enterprises). I find that municipalities with a treated branch were similar in terms of sectoral shares and location to other municipalities (Table A.XVI).

Next, I explore whether differences in municipality size can explain the results. There is a mechanical relationship between municipality size and having a treated bank branch: the larger a municipality, the greater the total number of bank branches, the more likely that a treated bank has a branch in the municipality. I confirm the relationship between municipality size and having a treated branch by regressing log total employment on the indicator for a treated branch. The coefficient is positive and statistically significant (column 1, Table A.XVII).

I investigate whether the municipality-level results are driven by this relationship using several strategies. I first restrict the sample to municipalities where total employment was at most as large as total employment in the largest untreated municipality. The idea is to only use municipalities where we know that size did not mechanically guarantee that there would be a treated branch. In this sample, the association between size and having a treated branch is small and insignificant (column 3), while the effect of having a treated branch remains close to the baseline and significant (column 4). Next, I restrict the sample to municipalities with below-median total employment. In this sample,

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the relationship between size and having a treated branch is also small and insignificant (column 5). The estimated effect of having a treated branch on employment growth remains close to the baseline although the small sample renders the coefficient insignificant (column 6).

The baseline estimate (column 2) controls for fixed effects for five quantiles of total employment. To test the robustness of the result, I instead control for log size (column 7), fixed effects for terciles of size (column 8), and fixed effects for deciles (column 9). The point estimates in all these specifications remain close to the baseline estimate and statistically significant. In summary, the effect of having a treated bank branch survives in samples where there is no association between municipality size and having a treated branch. Furthermore, the results are not sensitive to how I control for size. This implies that the effect of having a treated branch cannot be explained by the fact that larger municipalities are more likely to have a treated branch.

Appendix H Estimation of Cost and Profit Efficiency Ratios

I follow the procedures outlined in Sections 2.1 (cost efficiency) and 2.3 (alternative profit efficiency) of Berger and Mester 1997 to construct the efficiency measures. Cost efficiency is estimated by specifying a cost function where variable costs depend on prices of variable inputs, quantities of variable outputs, quantities of fixed inputs and outputs, other potential determinants of bank performance, and an error term composed of a random term and bank-specific inefficiency. The cost efficiency ratio of a given bank is an estimate of the costs needed to produce the bank's output if the bank were as efficient as the best-practice bank, divided by the predicted costs of the given bank, net of random error. The higher the ratio, the closer a given bank's efficiency is to the best-practice bank in the sample.

Alternative profit efficiency is estimated using a similar specification as the cost function, but with variable profits (plus minimum profits in the sample plus 1) as dependent variable. The profit efficiency ratio of a given bank is an estimate of the predicted profits of the bank divided by predicted profits of the bank if the bank were as efficient as the best-practice bank in the sample, net of random error. I report alternative profit efficiency instead of standard profit efficiency because alternative efficiency allows for the possibility that banks have market power and for unmeasured quality differences in bank outputs (Berger and Mester 1997).

The baseline results in columns 4 to 7 of Table VI are from a translog specification. The estimation method follows the stochastic frontier approach and assumes that the random error is normally distributed and that the inefficiency term is half-normally distributed, as in a study of European banks by Vander Vennet (2002). The baseline regressors are input prices (prices of deposits and labor), outputs (loans and securities), and fixed inputs and outputs (financial capital and physical capital). In robustness checks, I alternatively assumed a truncated normal and exponential distributions for the inefficiency term; I included the unemployment rate of the state in which the bank operated and its square among the regressors, to account for heterogeneity in the banks' economic environments; and

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I added Fourier trigonometric terms to the specification. The results of these robustness checks are similar to the ones reported in Table VI.

Appendix I Volatility of Banking Groups' Financial Figures

If the treated banks became less exposed to idiosyncratic shocks after consolidating, their aggregated financial figures should have become less volatile compared to untreated banks. For example, imagine that a given number of borrowers default every year for idiosyncratic reasons. If the treated banks became less exposed to these defaults, they would have experienced proportionally smaller hits to their capital stock and lending capabilities after consolidating. This, in turn, would have allowed them to limit the subsequent reductions in new lending and total assets. In addition, depositors would have been less concerned about bank failure and less likely to withdraw their funds. (German deposit insurance only started in 1976, so this applies to both bank and non-bank depositors.)

I use data from the Deutsche Bundesbank to analyze the volatility of the treated banks. The data are aggregated at the level of seven different banking groups. One group are the treated banks. Other groups include untreated commercial banks, private banks, savings banks, cross-regional cooperative banks, regional cooperative banks, and mortgage banks. The data include two observations per year from 1948 to 1970.[A2]

To analyze bank volatility, I adopt the procedure introduced by Morgan et al. (2004). In particular, I regress the log change in a given financial variable C_{gt} (growth in assets, lending, or deposits for banking group g at time t) on a full set of fixed effects for banking groups (α_g) and for time (α_t):

$$C_{gt} = \alpha_g + \alpha_t + \varepsilon_{gt}. \tag{A4}$$

The absolute value of the residual ($|\varepsilon_{gt}|$) is a measure of the fluctuation in growth for a given banking group in a given year. Intuitively, it is the absolute deviation of a banking group's growth relative to the average growth of all banking groups in that year and relative to the given banking group's average growth over all years. I regress the fluctuation measure on indicators for the years after 1952 ($post52_t$) and after 1957 ($post57_t$), interacted with an indicator for the treated banking group ($treated_g$). I control for time fixed effects and banking group fixed effects. The latter absorb all time-invariant differences across the banking groups:

$$|\varepsilon_{gt}| = \beta_1 \cdot treated_g \cdot post52_t + \beta_2 \cdot treated_g \cdot post57_t + \mu_g + \mu_t + \nu_{gt}.$$

The results are in columns 1 to 3 of Table A.XVIII. The three post-1952 coefficients are positive and statistically insignificant. This means, if anything, the fluctuations for the treated banking group marginally increased after 1952, relative to other banking groups. All three post-1957 coefficients

[A2]The annual reports of many treated and untreated banks from the 1940s and 1950s have not been preserved, so I rely on the Bundesbank data on banking groups instead. The data do not report items from the income statement.

are smaller in absolute terms than the post-1952 coefficients. This implies that after 1957, fluctuations of the treated banks still remained larger than before 1952, relative to other banking groups.

Diversification might have been particularly effective during recessions. In columns 4 to 6, I test whether fluctuations differed during the 1966/67 recession, by adding an indicator for treated banking groups during the recession. The coefficients are positive, small, and insignificant. This suggests that fluctuations of treated banks were not lower during the recession.

I carry out several robustness checks: I use squared residuals (ε_{gt}^2) for the outcome; I exclude from the sample all government-owned and cooperative banking groups to make the banking groups in the sample more comparable; and I include $treated_g \cdot post52_t$ and $treated_g \cdot post57_t$ as regressors in equation A4 to control for level effects on the growth rate. In all specifications, there is no evidence that volatility of assets, lending, or deposits fell by more for the treated banks.

Appendix J Volatility and Synchronization of Growth

Volatility of Growth: Potential Mechanisms How could the postwar bank consolidations affect the volatility of borrower growth? The answer depends on whether the consolidations affected capital flows and on whether shocks to bank lending capabilities or shocks to loan demand predominated in postwar Germany (Morgan et al. 2004).

First, consider an exogenous reduction in the lending capabilities of branches of one treated bank in one state. Before 1952, the branches formed a separate bank. They could stabilize their loan supply by borrowing funds from other banks through interbank markets. After 1957, the branches were part of a national bank. They had the additional option of receiving funds from out-of-state branches through an internal capital market. If the availability of internal funds after 1957 stabilized loan supply, exogenous shocks to lending capabilities would have affected borrowers by less. But if interbank funds after 1957 were as readily available as internal funds before 1952, volatility would have remained unchanged after 1957.

Second, consider an exogenous drop in loan demand in one state. Before 1952, treated branches in that state could respond by reducing loan supply to in-state borrowers and lending more to other banks through interbank markets. After 1957, treated branches had the additional option of sending funds to out-of-state branches through their internal capital market. If the flow of internal funds after 1957 was larger than the flow of interbank funds before 1952, exogenous shocks to loan demand would have led to greater volatility in the supply of funds in the state and, ultimately, greater volatility of borrower growth. But if internal flows were as large as interbank flows, there would have been no change in volatility after 1957.

Volatility of Firm Growth: Method 1 I use two methods to measure firm volatility. The first follows Morgan et al. (2004). In a first stage, I regress the change in annual firm growth (in employment

or revenue) for firm f in year t on fixed effects for firms (θ_g) and years (θ_t):

$$Growth_{ft} = \theta_f + \theta_t + \varepsilon_{ft}. \tag{A5}$$

The absolute value of the residual ($|\varepsilon_{ft}|$) measures the fluctuation in growth relative to the average growth of all firms in year t and relative to the average growth of firm f in all years. I regress $|\varepsilon_{ft}|$ on indicators for having a relationship bank treated in 1952 and 1957 (*rel bank treated in* 1952_f and *rel bank treated in* 1957_f), interacted with indicators for the years after 1952 ($post52_t$) and after 1957 ($post57_t$), respectively. I control for firm and year fixed effects:

$$|\varepsilon_{ft}| = \beta_1 \cdot \textit{rel bank treated in } 1952_f \cdot post52_t + \beta_2 \cdot \textit{rel bank treated in } 1957_f \cdot post57_t + \lambda_f + \lambda_t + \xi_{ft}.$$

The estimates of β_1 and β_2 are small and statistically insignificant, using either employment fluctuations (column 1, Table A.XIX) or revenue fluctuations (column 3) as outcome. Adding the controls, interacted with $post52_t$ and $post57_t$, makes little difference to the estimates (columns 2 and 4). The results imply that firm volatility did not change significantly after a relationship bank consolidated. The results are similar when using the squared residual to construct the outcome (ε_{ft}^2) and when including *rel bank treated in* $1952_f \cdot post52_t$ and *rel bank treated in* $1957_f \cdot post57_t$ as regressors in first stage equation A5 to control for potential level effects (which turn out to be small and insignificant).

Volatility of Firm Growth: Method 2 The second method analyzes the standard deviation of annual employment and annual revenue growth. I measure the change in volatility as the difference in the standard deviation of growth after versus before the 1952 reform. In Table A.XX, I regress the change in volatility on an indicator for whether a firm had a relationship bank treated in 1952 or 1957. The point estimates are statistically insignificant and relatively small, without controls (column 1) and with controls (column 2). The results are similar when I exclude the years 1952 to 1956 from the calculation of the standard deviation, to ensure that the period between the two reforms does not drive the results (columns 3 and 4). I find similar results for the standard deviation of revenue growth (columns 5 and 6).

Volatility of Municipality Growth I also analyze the volatility of municipal employment growth. I use data from the publication series *Statistisches Jahrbuch deutscher Gemeinden* for the years from 1947 to 1961, two observations per year. I use a similar method as for the firm-level analysis (based on Morgan et al. 2004). The outcome is the absolute deviation of municipality growth from average growth of the municipality over all years and average growth of all municipalities in the year. The estimates are small, of varying signs, and unaffected by adding the controls (Table A.XXI). The results are robust to using squared residuals as outcome and including the treatment indicators in the first stage regression.

Summary of Results on Volatility Taken together, the evidence from firms and municipalities indicates that the volatility of growth was not affected. The findings are consistent with the earlier results, which suggested that internal capital markets did not significantly alter capital flows across states and the level of firm and municipality growth.

Synchronization of Growth: Potential Mechanisms The banking consolidations could have affected the synchronization of real growth across firms and municipalities. The potential mechanisms are similar to the ones driving volatility (Morgan et al. 2004). If cross-state capital flows changed significantly, synchronization should have increased after loan supply shocks and decreased after loan demand shocks. Since the earlier evidence suggested that volatility did not change, I also expect no change in synchronization.

Synchronization of Firm Growth: Specification The outcome is the absolute value of the difference between the residuals from the first stage equation A5. That means, for a pair consisting of firms a and b, the outcome is $|\varepsilon_{at} - \varepsilon_{bt}|$. For every pair of firms in the data, I construct two variables that indicate whether the relationship banks of the two firms consolidated as a result of the 1952 and 1957 reforms, respectively. The regressors of interest are these two variables, interacted with indicators for the periods after the 1952 and 1957 reforms, respectively. All specifications include fixed effects for each firm pair, to account for pre-existing differences across pairs, and fixed effects for each year.

Synchronization of Firm Growth: Results I find no evidence that the consolidation of two firms' relationship banks had any effect on the synchronization of firms' growth, using either employment (column 1, Table A.XXII) or revenue (column 3) to define the outcome. The results are robust to controlling for whether firms in each pair were in the same zone, the same industry, of the same age, or in the same size bin (columns 2 and 4). By including the controls, I ensure that common shocks to firms in a certain zone, industry, age, or size bin do not bias the result.

Synchronization of Municipality Growth: Specification I follow an analogous method to analyze the synchronization of municipality growth, using a dataset of municipality pairs. The regressors of interest indicate whether the municipalities had bank branches that became part of the same bank as a result of a reform, interacted with an indicator for the period after the given reform. The outcome is the absolute difference in conditional employment growth between the two municipalities, where conditional growth is the residual from a regression of growth on year and municipality fixed effects.

Synchronization of Municipality Growth: Results The coefficients of interest are all small and statistically insignificant (column 1, Table A.XXIII). Adding controls for whether municipalities

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were in the same zone, the same size bin, and both in the Ruhr region hardly changes the coefficients (column 2). Overall, there is no evidence that synchronization changed as a result of the banking reforms.

Appendix K Firm Performance After Being Added as Relationship Borrower by a Treated Bank

Did the treated banks establish relationships with low-default borrowers? Column 1 of Table A.XXIV uses as outcome an indicator for whether the firm went into liquidation between 1960 and 1968. The regressors are indicators for whether the firm had no treated relationship bank in 1951 but one in 1960 ("added a bank treated..."); a treated relationship bank in 1951 but not in 1960 ("dropped all banks treated..."); and no treated relationship bank in either 1951 or 1960 ("never had a bank treated..."). The omitted category contains firms that had a treated relationship bank in both 1951 and 1960. The categories are mutually exclusive, so the coefficients can be interpreted as the average difference in the propensity to face liquidation between 1960 and 1968 for the different types of firms.

The coefficients on the first and third regressors in column 1 are close to zero and statistically insignificant. Hence, newly added relationship borrowers of treated banks as well as firms that were never relationship borrowers of treated banks were not significantly less likely to avoid liquidation, relative to continuing borrowers of treated banks. The second coefficient is negative and significantly different from zero at the 5 percent level. It implies that firms dropped by the treated banks as relationship borrowers were 3 percentage points less likely to avoid liquidation afterward, relative to continuing borrowers of treated banks.[A3] Overall, column 1 suggests that treated banks did not become better at identifying borrowers with lower likelihood to default.

It could be that the banks added and dropped borrowers between 1951 and 1960 based on the borrowers' default likelihood between 1951 and 1960. To explore this possibility, column 2 uses as outcome an indicator for whether the firm went into liquidation between 1951 and 1960. The coefficients are all insignificant and relatively small. The specification in column 2 potentially suffers from reverse causality bias, unlike column 1, since firms in liquidation are likely to be dropped as relationship borrowers at the same time. This might explain the (insignificant) positive point estimate on firms dropped between 1951 and 1960 in column 2, relative to the (significant) negative estimate in column 1. Most important, however, the results once again confirm that firms added or dropped by treated banks were not significantly less likely to avoid default.

[A3]The unconditional mean propensity to face liquidation was 3 percent. Interestingly, the point estimate is negative 6 percentage points when restricting the sample to opaque firms, although not significantly different from the baseline effect of 3 percentage points. A larger effect on opaque firms would be consistent with the finding that treated banks became worse at dealing with opaque borrowers after consolidating.

Did the treated banks establish relationships with fast-growing borrowers? Columns 3 and 4 of Table A.XXIV use as outcome the growth of revenue per worker, a measure of labor productivity. Columns 5 and 6 use as outcome the growth of employment. There is no evidence that firms added or dropped by the treated banks had differential growth rates. Taken together, the results in columns 1 to 6 are inconsistent with the view that that treated banks added firms with better performance as relationship borrowers, or dropped firms with worse performance.

Did the treated banks establish relationships with more productive borrowers? It could be that the treated banks reallocated credit from low to high productivity firms by adding or dropping relationship borrowers. In that case, firms added as relationship borrowers by the treated banks should have been more productive than other firms, and firms dropped as relationship borrowers less productive. I use revenue per worker as proxy for labor productivity. The results show that revenue per worker in 1968 (column 7) and in 1951 (column 8) was not different for firms added or dropped by the treated banks. This implies that the treated banks did not improve the average labor productivity of their relationship borrowers by selectively adding or dropping firms.

Appendix L The Elasticity of Firm Growth with Respect to Bank Size

The elasticity is the percentage change in borrower growth that results from a one percent increase in bank size. The first step in calculating the elasticity is measuring by how much each treated bank grew because of the reforms. I use the size of banks before the 1952 reform as basis for the calculation. Using only the pre-1952 assets instead of actual post-1952 assets ensures that the measure of bank size growth is exogenous to any potential effects of the reforms on bank assets.

The method is best explained using an example: before 1952, assets of Suedwestbank (Deutsche Bank successor located in the southern state of Wuerttemberg-Baden) totaled 301 million Deutsche Mark; the aggregated assets of all Deutsche Bank successors in the southern zone totaled 976 million; and aggregated assets of all Deutsche Bank successors in West Germany totaled 2,308 million. For a firm whose only relationship bank in 1951 was Suedwestbank, I calculate the reform-induced change in the size of the firm's banks from 1951 to 1956 as (976-301)/301 = 224.3 percent and from 1951 to 1960 as (2,308-301)/301 = 666.8 percent. For firms with only untreated relationship banks, the measure of reform-induced bank size growth is zero, since the reforms had no impact on the size of untreated banks. For firms with multiple relationship banks, the measure is the average change in bank size over all relationship banks.

To estimate the elasticity, I regress the annual percentage change in firm employment on the percentage change in the size of the firm's relationship banks. The sample and specification for this analysis are based on column 1 of Table I. The only difference between Table I and the result here is that the outcome and treatment variables here are in percentage changes, so that the point estimate can be directly interpreted as the elasticity. The estimated elasticity is -0.0009 (95 percent

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confidence interval: -0.0024, 0.0005) using the 1952 reform.[A4]

[A4] I find similar results when using symmetric or log growth rates instead of percentage changes. I also estimated the elasticity based on both reforms of 1952 and 1957, using a sample and specification based on column 1, panel A of Table II. The elasticity of employment with respect to bank size is -0.002 (95 CI: -.007, 0.003) and the elasticity of bank debt is 0.0004 (95 CI: -0.0007, 0.0016) using both reforms.

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