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Banking Without Branches*

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Abstract

The decline in cash use and growing use of digital distribution for retail banking leads to a reduced need for bank branches. Lending to small and medium sized firms (SMEs) has not benefited as much from a digital transformation, and widespread branch closures may reduce their supply of credit. Using the closing of two thirds of Swedish branches as a laboratory, we document that corporate lending declines rapidly following branch closures, mainly via reduced lending to small and young firms. The reduced credit supply has real effects: local firms experience a decline in employment and sales and an increase in exit risk after branch closures. Our results thus suggest that the disappearance of bank branches have far-reaching implications for the economy.

Keywords: Banks; branch closures; credit supply.

JEL: D22, G21, G32, R12, R32.

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1 Introduction

Recent technological development has made digital distribution of many bank services possible, reducing or eliminating the need for in-person interactions. Retail banking has been most dramatically affected by this move to digital distribution. According to a recent Eurostat survey, 60 percent of European households reported using internet banking services in 2022, up from 24 percent in 2007 (Eurostat, 2022), and the fraction of households using online bank services is considerably higher in some countries and regions (e.g., 96 percent in Norway). The rise of digital banking has changed the economics of maintaining a branch network.[1] The total number of bank branches in the OECD countries fell by 30 percent from the 2008 peak to 2022 according to the IMF's Financial Access Survey. Given the persistence and magnitude of the underlying technological changes, we hypothesize that the trend away from bank branches will continue in the affected countries, and spread to others, perhaps also reaching developing markets.

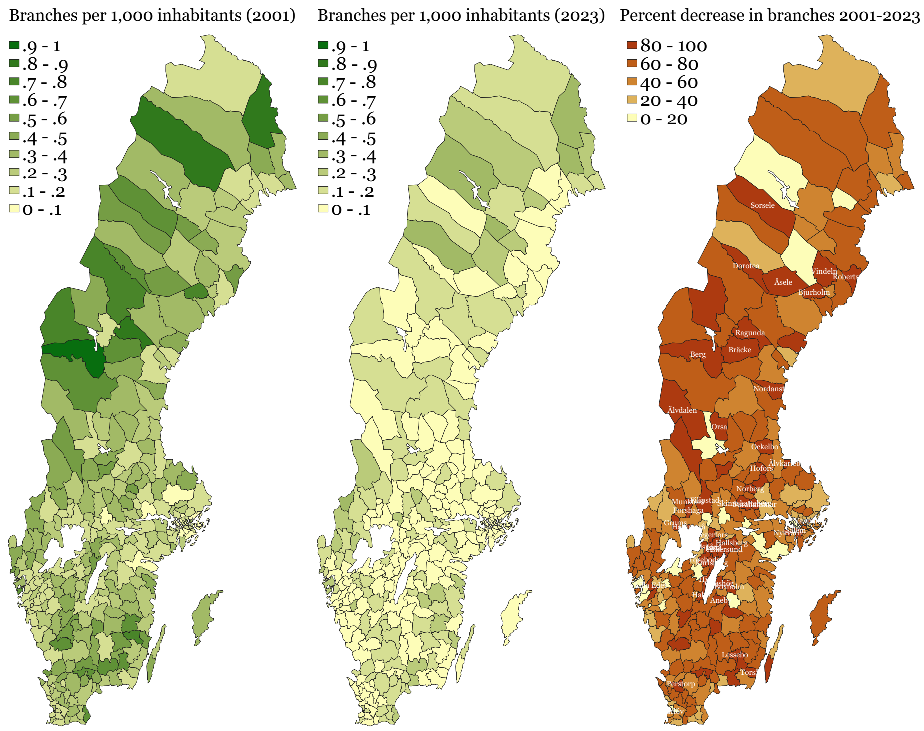
When bank branches disappear, a concern is the potential impact on business lending. Close proximity between lenders and borrowers is a well-known attribute of commercial lending relationships (Petersen and Rajan, 1994, 2002). Without branches, banks' ability to collect soft information and develop relationships may fall, impeding the flow of credit (Berger et al., 2005; Agarwal and Hauswald, 2010; Liberti and Petersen, 2018). Unfortunately, there is still little evidence that digital distribution of non-retail bank services has changed as much that for retail banking. For example, according to a recent survey of managers of U.S. medium sized banks, retail services are more than twice as likely to be delivered digitally as small and medium sized enterprise (SME) services (Bank Director, 2022). Because banks dominate SME credit, the result may be worse availability of financing for these firms.[2]

We aim to investigate the hypothesis that large scale branch closures reduce the supply

[1]Additionally, cash use has declined in line with the rise of digital payments, also reducing the need for bank branches. In Sweden, the setting of our empirical study, cash outstanding peaked in 2005 in real terms, and fell by 56 percent to 2023

[2]Gopal and Schnabl, 2022 point to Federal Reserve data on Finance Companies from Form G-20 (https://www.federalreserve.gov/releases/g20/current/g20.htm): by late 2023, non-bank lenders including but not limited to fintech firms, had loan to companies of $630 billion outstanding, dwarfed by commercial banks' C&I loans and commercial real estate loans, together $5.8 trillion.

Figure 1: The spatial distribution of bank branches in Sweden



This figure plots the number of bank branches per 1,000 inhabitants in Swedish municipalities in 2001 (left figure) and 2023 (middle figure), as well as the change in the number of branches between 2001 and 2023 (right figure). Municipality names in white indicate locations with no branches at the end of the sample.

of SME loans, using detailed micro level data from Swedish banks and firms. Sweden offers a suitable empirical setting for several reasons. First, branch closures have been dramatic in the last decades, with almost two thirds of branches closing; at the end of 2023, 43 out of 290 municipalities in Sweden do not have a single bank branch (see Figure 1). Such wide-ranging branch closures may generate effects that differ quantitatively and qualitatively from more isolated branch closures, which are known to have moderate negative effects (Ashcraft, 2005; Garmaise and Moskowitz, 2006; and Nguyen, 2019). Second, the market is dominated by four large banks, which have closed branches rapidly but at different times. This offers an identification strategy based on shift-shares—ex-ante similar locations with many Swedbank branches saw closures early, then those with Nordea and SEB branches, with Handelsbanken not closing branches on a large scale until 2019.[3]

[3]Comparing Sweden to other OECD countries, the rate of closing is high but not unique: in the 15 years to

We examine the impact of branch closures on financial and real outcomes using detailed firm-level data and bank-branch data comprising the location of nearly every bank branch in Sweden over the period 2001–2023. Our primary outcome variable for assessing credit-supply effects is loan balances. Using a shift-share instrumental variables (SSIV) strategy based on large banks' national branch closings, we estimate that local firms' loan balances decline by 1.8 percent following the closure of 10 percent of the bank branches in a municipality. The effect is larger for small firms—whether measured by employment, assets or sales—for younger firms, and for firms with fewer tangible assets. Short-term loans (such as credit lines) represent the bulk of the effect.

We examine real effects using local firm employment, sales, assets and exit rates. Branch closures are associated with significant negative effects on the employment, sales, and working capital of local firms, but have no (detectable) effect on fixed assets. Firm exits increase: the probability that a firm exits increases by around 0.6 percentage points over a three year period if 10 percent of local bank branches are closed.

Our findings have implications in several areas. First, our results confirm that branches are economically important through their connection to local firms (Petersen and Rajan, 1994). The evidence is consistent with a key role of bank branches in collecting soft information, and a correspondingly larger effect of branch closures for firms and loan types where soft information matters most (e.g., Agarwal and Hauswald, 2010; Almeida and Campello, 2007; Ivashina, Laeven and Moral-Benito, 2022).

A second implication is that while the spread of digital banking in advanced economies generates large efficiency gains (e.g., Berger, 2003), there are also potential concerns, including for SME lending. Accelerated growth in the fintech sector may compensate (Gopal and Schnabl, 2022), perhaps by using new information sources (Liberti and Petersen, 2018), but to what extent remains unclear. Third, more broadly, our results point to the mixed blessings of technological disruption: large gains often come at the expense of some losses (e.g., Becker and Ivashina, 2023).

2022, two countries saw growth (Mexico and Turkey), two saw no net growth (Austria and Japan) and the rest saw shrinking (e.g., France by 22 percent, Italy by 42 percent and Spain by 65 percent). The Nordic countries and the Netherlands have seen the most rapid changes.

The rest of the paper is organized as follows: the next section (2) briefly describes the Swedish banking system. The following section (3) introduces our empirical methodology and Section 4 presents results from municipality data. Section 5 concludes.

2 Institutional Background: The Great Bank-Branch Closure Wave

2.1 The Swedish bank market

In 2001, at the start of our sample period, the Swedish bank market was heavily dominated by four major banks—Handelsbanken, Nordea, SEB, and Swedbank—who jointly accounted for over 75 percent of bank lending to non-financial firms and households as well as of the number bank branches (the share of corporate lending was even larger). The remainder of the bank market consisted of 77 savings banks as well as various other lenders, including smaller banking groups, mortgage lenders, finance companies, and subsidiaries of foreign banks. The savings banks are noteworthy in that they jointly accounted for a fairly large share of bank branches, 17 percent, despite their small share in total lending. The four major banks and the savings banks thus accounted for around 95 percent of the number of bank branches in Sweden at the start of our sample period.

In 2023, at the end of our sample period, the market share of the four major banks has declined to around two thirds in terms of lending to households and non-financial firms as well as in terms of branches. The decline in the market share of the four major banks is mainly due to the growth of two other banks. The first is the Danish bank Danske Bank, who entered the Swedish market by acquiring and growing an established but small Swedish bank (Östgöta Enskilda Bank); as of 2023, Danske Bank is an important actor within retail as well as corporate lending. The second is Länsförsäkringar Bank, which has grown organically over the 2000s and established itself as an important actor on the retail loan market; on the corporate loan market, on the other hand, Länsförsäkringar remains a minor actor. The joint market share of the four major banks, Danske Bank, and Länsförsäkringar Bank was around 75 percent in 2023 measured in terms of lending as well as branches.

2.2 The shrinking Swedish bank branch network

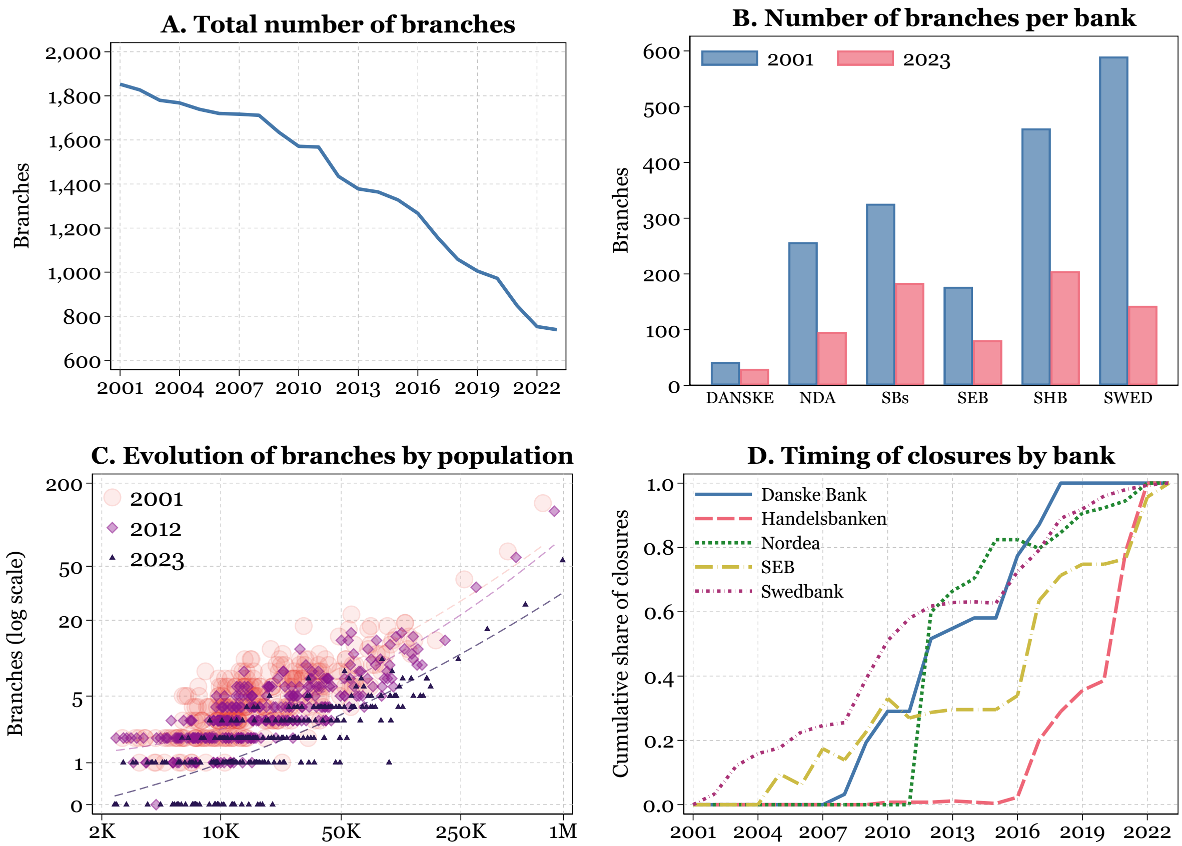
The number of bank branches in Sweden has declined rapidly and steadily since the early 2000s, going from almost 1,900 in 2001 to around 750 in 2023.[4] The decline has been particularly pronounced in recent years: the average annual decline in the number of bank branches was 1.1 percent during 2002-2008, and then accelerated to 5.4 percent from 2009 and onwards. In both 2021 and 2022, more than 10 percent of all branches were closed. All four major banks (Handelsbanken, Nordea, SEB, and Swedbank) have contributed to the decline, with reductions in the number of branches ranging from 54 percent (SEB) to 76 percent (Swedbank) between 2001 and 2023. We illustrate this development in Figure 2, which shows the number of bank branches nationwide for all banks over the period 2001-2023 (Panel A) as well as for each bank separately in 2001 and 2023 (Panel B). Panel C—which plots the evolution of the number of branches in each municipality against population—shows that the closures have affected small and large municipalities alike, and accelerated after 2012.

The background to the reduction in branch networks is new technology, which has drastically reduced the need for retail locations. For example, in 2022, the Riksbank reports that only 34 percent of survey respondents reported having used cash in the last 30 days. Bank services apart from payments are also increasingly provided online, and this has driven banks toward closing branches. Handelsbanken writes in its 2021 Annual Report: *"In places where almost all of our customers can manage their finances via their computer and smartphone, we have seen a marked downturn in the number of visits to our branches. When there is no longer any real need for a branch, it is time to close the doors for good"* (p. 4).

While this broad trend is technological and affects all banks, the timing of the reductions in branch networks have been bank-specific. This has meant that branch closures have been concentrated over a short time span for each bank and that the timing of the closures differ substantially: the largest reduction in branches in a single year occurred in 2012 for Nordea (–39 percent), in 2017 for SEB (–22 percent), in 2018 for Swedbank (–19 percent), and in 2021 for Handelsbanken (–29 percent). We plot the complete time profile of each bank's branch closures in Panel D of Figure 2: the figure demonstrates the varied timing, with Swedbank

[4] The data on which all numbers in this section are based is described in section 3.1 below.

Figure 2: Bank branches in Sweden 2001–2023



Panel A plots the total number of bank branches in Sweden over the period 2001–2023 and Panel B the number of branches per bank in 2001 and 2023. The banks included in the data are Danske Bank (DANSKE), Nordea (NDA), SEB (SEB), Handelsbanken (SHB), Swedbank (SWED), and all savings banks grouped together (SBs). Panel C plots the evolution of the number of bank branches by municipality between 2001 and 2023 against the adult population of each municipality (each dot in the figure corresponds to a municipality-year). Panel D plots the timing of the branch closures that each bank undertook between its peak year and 2023. The cumulative share is the share of closures that took place up to and including a given year (it is zero before the peak year).

beginning large-scale branch closures early, SEB and Handelsbanken late, and Nordea and Danske Bank in between. Hence, the smooth decline in the total number of branches evident in Figure 2 masks substantial lumpiness in branch closures at the bank level, as well as heterogeneity across banks in the timing of the closures. The timing difference combined with the differing geographical coverage of the respective banks forms the basis of our shift-share identification strategy, which we describe in detail in Section 3.

2.3 Econometric evidence on the determinants of branch closures

In order to provide more detailed evidence on the determinants of bank-branch closures, we estimate the following regression at the bank-municipality level:

$$\mathbb{1}\{\Delta Branches_{b,j,t} < 0\} = \alpha + \beta \cdot \mathbf{X_{b,j,t}} + \varepsilon_{b,j,t}, \tag{1}$$

where b indexes banks, j municipalities, and t years, and where the dependent variable is an indicator variable equal to one if bank b reduced the number of branches in municipality j during year t. The explanatory variables are collected in the vector $\mathbf{X_{b,j,t}}$. Standard errors are clustered at the municipality level. The coefficients β capture the effect of a one-unit change in the respective explanatory variables on the probability that a bank reduces the number of branches in a municipality in a given year.

The estimation results are reported in Table 1. The specification in the first column uses three explanatory variables: the logs of municipality population, area, and average labor income, respectively. The estimated effects of these variables are small and sensitive to specification, and their joint explanatory power is low, with an adjusted R^2 of less than 0.01. This confirms that bank-branch closures in Sweden is a nation-wide phenomenon and not, primarily about closing branches in poor or rural areas. In the second column, we add the log of the lagged number of branches of a bank in a given municipality to the set of explanatory variables. This increases R^2 substantially, and the effect is fairly large: the estimate indicates that a bank is 15 percent more likely to reduce the number of branches in a municipality where it has twice as many branches as in another municipality.

In column (3), we add the percent change in the nationwide number of branches of a bank, which adds considerable explanatory power. The estimate shows that the probability that a bank reduces the number of branches in a municipality increases by one percentage point for every percentage point decrease in nationwide branch growth. In column (4), we include an interaction term between nationwide branch growth and the lagged number of branches a bank has in a municipality. The estimate shows that the effect of nationwide branch growth on closure probability is strongly increasing in the number of branches the

Table 1: Determinants of bank-branch closures

	Dependent variable: $\mathbb{1}\{\Delta Branches_{b,j,t} < 0\}$					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
ln $Population_{j,t}$	0.025***	–0.014***	–0.006***	–0.008***	–0.014***	–0.025***
	[0.007]	[0.002]	[0.002]	[0.002]	[0.002]	[0.003]
ln $Area_j$	0.004**	0.001	–0.005***	–0.005***	–0.005***	–0.005**
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
ln $Labor\ income_{j,t}$	–0.019	0.081***	–0.047***	–0.040***	–0.030***	–0.03
	[0.012]	[0.012]	[0.010]	[0.011]	[0.011]	[0.019]
ln $Branches_{b,j,t-1}$		0.145***	0.139***	0.092***	0.102***	0.097***
		[0.005]	[0.005]	[0.007]	[0.007]	[0.008]
$\Delta Branches_{b,t}$			–1.080***	–0.722***	–0.714***	–0.769***
			[0.055]	[0.057]	[0.059]	[0.064]
ln $Branches_{b,j,t-1} \cdot \Delta Branches_{b,t}$				–1.444***	–1.458***	–1.539***
				[0.190]	[0.206]	[0.209]
Estimation period	2001-22	2001-22	2001-22	2001-22	2001-22	2009-22
Savings banks included?	Yes	Yes	Yes	Yes	No	No
Adjusted R^2	0.009	0.085	0.157	0.193	0.211	0.253
Number of observations	19,800	16,776	16,776	16,776	14,369	8,873
Number of municipalities	290	290	290	290	285	282

This table reports estimation results for the regression specified in (1). $Population_{j,t}$ measures the total number of inhabitants in municipality j in year t; $Area_j$ the size of municipality j in square kilometers; $Labor\ income_{j,t}$ the average labor income of inhabitants 20 years or older in municipality j in year t; $Branches_{b,j,t-1}$ the number of branches of bank b in municipality j in year $t-1$; and $\Delta Branches_{b,t}$ the percent change in the total (nationwide) number of branches of bank b between years $t-1$ and t. Standard errors are clustered at the municipality level in all regressions. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

bank has in the municipality and contributes to a further increase in R^2.

In column (5) we drop savings banks from the sample—savings banks do not operate nationwide and that the logic of studying the effects of nationwide branch growth therefore is less clear. The results turn out to be quite similar to those based on the full sample. In column (6), finally, we restrict the sample to the period 2009 to 2022: this is when the great majority of the branch closures took place. The R^2 in this specification is 0.25, which demonstrates that the lagged number of branches and the nationwide branch growth together explain a substantial part of the variation in the probability of branch closures at the municipal level for large banks.

In sum, two key insights emerge from the results reported in Table 1. First, branch closures are largely driven by institution-wide initiatives, as evidenced by the high explanatory power of the nationwide change in the number of branches of a bank. Second, when banks reduce the number of branches, they tend to concentrate the closures in municipalities where they have more branches.

3 Empirical Framework: A Shift-Share IV Analysis

3.1 Data

The empirical analysis is based on two main data sources. The first is an annual panel data set comprising the number of bank branches per municipality and bank over the period 2001–2023 that we create based on data from two different sources. One source is *Bankplatser i Sverige* (Bank Locations in Sweden), a print publication containing the address of every bank branch in Sweden, issued annually by the Swedish Bankers' Association until 2008. This publication was replaced by a web page with the same name, which is regularly updated but where no historical records are maintained; the web page can therefore not be used to reconstruct historical series of bank branches by municipality and bank after 2008. We also use the administrative database Pipos from the Swedish Agency for Economic and Regional Growth, which provides the exact location—down to latitude and longitude—of every bank branch in Sweden from 2011 and onwards.[5]

Combining the two data sources with branch data, we construct an annual panel with the number of branches per municipality, bank, and year for the periods 2001–2008 and 2011–2023. The panel comprises branches belonging to Danske Bank, Handelsbanken, Nordea,

[5]More specifically, we use as our measure of bank branches what in the Pipos data is referred to as *betalningsförmedlingsplatser* (locations providing payment services). To verify that these actually correspond to bank branches, we have confirmed that the number of *betalningsförmedlingsplatser* per bank in the Pipos data correspond closely to the number of branches per bank reported in the Swedish Bankers' Associations' annual statistics publication *Bank and finance statistics* (e.g., Swedish Bankers' Association, 2023).

SEB, Swedbank, and savings banks—that is, the main lenders on the Swedish corporate loan market during our sample period (see section 2).[6] For practical purposes, we define a bank branch as a combination of bank and postal code. Hence, if a bank reports several branches for the same postal code—which occasionally happens, for example, when a branch office is split across several numbers of the same street—we count one branch. We impute observations for the years 2009–2010 by linearly interpolating between the number of branches for each municipality-bank cell in 2008 and 2011, respectively. By doing so, we obtain a complete municipality-level panel spanning the period 2001–2022.

The second main data set used in the analysis is Serrano, an annual firm-level panel comprising the universe of incorporated firms in Sweden. The Serrano database is primarily based on data from the Swedish Companies Registrations Office—to which all Swedish corporations are required to submit annual financial accounting statements in accordance with EU standards—and contains detailed accounting data as well as demographic data, such as a firm's industry, age, and location (in case a firm is active in multiple locations, we observe the location of the firm's headquarters). We can thus link the Serrano data to the bank-branch data by means of the municipality code in each data set.

In addition to the main data sets, we use data from two additional sources to construct control variables for the estimations. First, we build a municipality-year panel comprising, among other things, population size, employment, labor earnings, and population density using publicly available information from Statistics Sweden, the official Swedish statistics agency. Second, we obtain credit-score data from Upplysningscentralen AB (UC), a leading Swedish credit bureau co-owned by the major banks. More specifically, we use the firm-specific probability of default that UC assigns to each Swedish firm on the basis of a rich scoring model.[7]

[6]Our current data sources do not enable us to construct municipality-level branch series spanning the entire sample period for banks other than these. We do not deem this a major concern for the empirical analysis. First, while Länsförsäkringar is a fairly large bank with many branches, it is primarily a retail bank—Länsförsäkringar's share of the corporate loan market is very small and it is therefore not important for our analysis. Second, the remaining banks not covered by our branch panel have very few branches or, in some cases, no branches at all.

[7]UC's credit-score data has previously been used in the corporate-finance literature by, for example, Caggese, Cuñat and Metzger (2019) and Amberg et al. (2021).

3.2 Empirical model and instrument

The structural relationship between bank-branch closures and firm-level outcomes that we are interested in can be described by the following local-projections model:

$$\Delta Y_{i,t+h} = \alpha_i^h + \theta_t^h + \beta^h \cdot \Delta Branches_{j,t} + \gamma^{\mathbf{h}} \cdot \mathbf{X_{i,t}} + \varepsilon_{i,t}^h, \tag{2}$$

where the dependent variable is the symmetric growth rate of outcome Y for firm i between years $t-1$ and $t+h$, and h denotes the estimation horizon.[8] The main explanatory variable, $\Delta Branches_{j,t}$, is the percent change in the number of bank branches in municipality j between years $t-1$ and t, where j is the municipality in which firm i has its headquarters. α_i^h is a firm fixed effect, θ_t^h a year fixed effect, and $\mathbf{X_{i,t}}$ a vector of control variables. The baseline set of controls comprises two lags each of the dependent variable and the main explanatory variable, as well as log assets, log employment, firm age in years, probability of default (as estimated by the credit bureau UC), and the ratios of debt to assets, EBIT to assets, and cash to assets, all measured at time $t-1$. We restrict the sample to non-financial corporations with at least two full-time equivalent employees and one million SEK (approximately $100,000) in sales and net assets. The lower size threshold is imposed to ensure that we only include economically active enterprises in the estimations.

The main dependent variable of interest is the total volume of bank loans to firm i in year t, $Loans_{i,t}$, which we use to estimate the effect of bank-branch closures on local credit supply. We include all outstanding bank loans to firm i when computing $Loans_{i,t}$, but not undrawn parts of credit lines—the reason is that we do not observe unused loan commitments in the firm data. When assessing the real effects of branch closures, we estimate (2) with employment growth, sales growth, asset growth, and firm exit, respectively, as dependent variables.

The coefficient of interest in equation (2) is β^h, which measures the effect of a change in the number of bank branches in a municipality on real and financial outcomes for local firms over an h-year horizon. Note that β^h captures any effect operating through branch closures,

[8] The symmetric growth rate is defined as $\Delta Y_{i,t+h} \equiv (Y_{i,t+h} - Y_{i,t-1}) / [(Y_{i,t+h} + Y_{i,t-1})/2]$ and is a commonly used alternative to percent and log changes, since it straightforwardly accommodates entry and exit.

including, for example, both direct effects such as reduced lending when information collection about local borrowers becomes more difficult and indirect effects such as changes in the competitiveness of the local bank market. The empirical challenge we face is that estimating (2) by OLS would yield biased estimates of β^h, since $\Delta Branches_{j,t}$ is almost certainly correlated with $\varepsilon^h_{j,t}$ due to the non-randomness of banks' choices about when and where to close branches, even after conditioning on observable firm and municipality characteristics. To address this problem, we instrument the change in bank branches in a municipality with the following shift-share instrument in the spirit of Bartik (1991):

$$Z_{j,t} = \sum_b \frac{Branches_{b,j,t-1}}{Branches_{j,t-1}} \cdot \Delta Branches_{b,t}, \tag{3}$$

where $Branches_{b,j,t-1}/Branches_{j,t-1}$ is bank b's share in the total number of bank branches in municipality j in year $t-1$, and $\Delta Branches_{b,t}$ is the percent change in the number of bank branches nationwide for bank b between years $t-1$ and t. That is, the instrument combines variation in the exposure of a given municipality to the respective banks (the shares) with the nationwide change in the number of branches of each bank (the shifts), where the former is pre-determined at time t and the latter is plausibly orthogonal to economic conditions in a given municipality.[9]

We introduce the instrument into the empirical model by supplementing the structural equation with the following regression:

$$\Delta Branches_{j,t} = \phi_i + \psi_t + \xi \cdot Z_{j,t} + \theta \cdot \mathbf{X_{i,t}} + u_{i,t}, \tag{4}$$

where ϕ_i and ψ_i are firm and year fixed effects, respectively, and all other variables are defined as before. We estimate the resulting two-equation system by two-stage least squares (2SLS), where equation (4) is the first stage and equation (2) is the second stage. Standard errors are clustered at the municipality-year level to account for the fact that the endogenous re-

[9]We do not include savings banks in the construction of $Z_{i,t}$. The reason is that savings banks operate with a small number of branches in a limited number of locations and that their decisions about whether to close branches therefore are unlikely to be independent of local economic conditions. To be precise, branches of savings banks are included in the denominator of $Z_{j,t}$ ($Branches_{j,t}$) but not in the set of banks b over which the summation is done.

gressor and the instrument vary across municipality-year cells. We also report reduced-form estimates from regressions of $\Delta Y_{i,t+h}$ directly on $Z_{j,t}$ and $\mathbf{X_{i,t}}$.

As demonstrated by Imbens and Angrist (1994), 2SLS estimation captures the local average treatment effect (LATE) when the treatment effect is heterogeneous in the population, which it is almost certain to be in our setting. For example, a branch closure is likely to be significantly less consequential in an area with dwindling economic activity and few fundamentally viable firms than in a high-growth area where firms have many profitable investment opportunities. Hence, our 2SLS estimates should be interpreted as the local average treatment effects, which here means the causal effect of branch closures in municipalities where the change in the number of branches is affected by banks' nationwide closure decisions ("complier" municipalities). The effects we estimate are therefore not immediately informative about the corresponding causal effects in municipalities that are non-compliers in terms of our instrument, nor in entirely different empirical settings. The external validity of our findings may be better since branch closures were such a widespread phenomenon during our sample period (see Figure 1 and Figure 2).

3.3 Assessing the validity of the research design

In what follows, we evaluate the internal as well as external validity of the SSIV research design. First, we discuss the assumptions necessary for our empirical model to identify the local average treatment effect and provide empirical evidence in support of them (internal validity). Second, we assess whether the local average treatment effects captured by our 2SLS estimates are likely to representative of the causal effects of branch closures in the population of municipalities by comparing the characteristics of municipalities in which the instrument has a large and small effect, respectively, on the growth rate of bank branches (external validity).

3.3.1 Internal validity

The following four conditions, specified by Imbens and Angrist (1994), are required for the 2SLS estimates to identify the local average treatment effect of branch closures:

1. **Instrument strength:** $Z_{j,t}$ strongly affects the endogenous regressor—the decision of a bank to close branches across the country has a strong effect on branch growth in the municipalities in which the bank has a large presence.
2. **Instrument independence:** $Z_{j,t}$ is not correlated with factors that affect the outcomes of interest—muncipalities more and less exposed to a bank that closes branches across the country would have developed similarly in the counterfactual scenario where the bank decided not to undertake nationwide branch closures.
3. **Exclusion restriction:** $Z_{j,t}$ does not affect the outcomes of interest except through the effect on the endogenous regressor—the decision of a bank to close branches across the country does not affect the outcomes of interest in the municipalities in which the bank has a large presence except through the effect it has on the number of bank branches in these municipalities.
4. **Monotonicity:** There are no "defier" municipalities in the sample—the decision of a bank to close branches across the country never causes an increase in the number of branches in which the bank has a large presence. Put differently, the derivative of $\Delta Branches_{j,t}$ with respect to $Z_{j,t}$ is non-negative for all municipality-year observations.

In what follows, we assess the plausibility of each assumption in turn.

Instrument strength. The first assumption can be verified by inspecting the first-stage F-statistic. When estimating the first-stage regression (4) with the baseline set of control variables, we obtain an effective first-stage F-statistic of 151.7 (Montiel Olea and Pflueger, 2013). This is well above any conventional rule-of-thumb threshold for weak instruments and thus demonstrates that our instrument is strong. The strength of the instrument confirms that bank-branch closures in Sweden during our sample period are largely driven by bank-specific closure waves decided on centrally by the respective banks' headquarters.

Instrument independence. The independence assumption requires that firms' exposure to nationwide branch-closure waves are unrelated to other factors affecting their development. We assess the plausibility of this (formally untestable) assumption by comparing observations where the instrument is negative and non-negative, respectively, across a set of firm- and municipality-level covariates that are likely to be correlated with a firm's current and prospective economic condition. We assess the magnitude of the differences in the covariates using the normalized difference in means, a comparison metric proposed by Imbens and Rubin (2015) that measures the difference in means expressed in terms of standard deviations. The main benefit of using normalized differences instead of t-tests for such comparisons is that the normalized difference is scale-free, in the sense that the likelihood of rejecting similarity does not increase mechanically with sample size.

The results of the comparison exercise are reported in Table 2. Panel A demonstrates that firm-years highly exposed to banks closing branches nationwide do not differ meaningfully from less exposed firm-years across the characteristics under consideration. To see this, note that the magnitude of the largest normalized difference in means is only 0.07—for comparison, in an analysis of the data from an experiment with random treatment assignment, Imbens and Rubin (2015) observe a maximum normalized difference of 0.30 and judge this to be evidence of strong covariate balance. Panel B shows that the same holds true for municipalities—the largest normalized difference across the seven municipality-level characteristics under consideration is 0.24, which implies that municipality-years with high exposure to banks closing branches nationwide are quite similar to municipality-years with low exposure. Taken together, the results in Table 2 provide strong evidence in favor of the instrument-independence assumption.

Exclusion restriction. The exclusion restriction is—like the independence assumption and the monotonicity requirement—formally untestable. Recall that the exclusion restriction would be violated if the exposure of a municipality to a bank that closes branches nationwide affects economic outcomes in the municipality via channels other than the branch-closure channel. Importantly, such a mechanism would have to affect all municipalities in proportion to their exposure to the bank in question, and thus independently of the actual rates of branch closures that the nationwide closures induce in each municipality. For example, the municipality of Stockholm would have to be affected by the nationwide branch closures of

Table 2: Covariate balance

	$Z_{i,t} < 0$			$Z_{i,t} \geq 0$			Normalized
	Mean	SD	N	Mean	SD	N	difference
A. Firm-level characteristics							
Assets (MSEK)	30.4	106.0	453,254	25.3	90.2	108,042	0.05
Sales (MSEK)	33.2	86.6	453,254	29.5	78.8	108,042	0.05
Number of employees	14.9	28.5	453,254	14.0	27.0	108,042	0.03
Age (years)	19.81	14.33	453,254	19.35	13.97	108,042	0.03
Debt/Assets	0.77	0.17	453,254	0.78	0.16	108,042	–0.07
EBIT/Assets	0.06	0.12	453,254	0.06	0.11	108,042	–0.02
Cash/Assets	0.09	0.13	453,254	0.09	0.12	108,042	0.05
Probability of default	2.25	5.22	453,254	2.26	5.11	108,042	0.00
B. Municipality-level characteristics							
Population (1000s)	35.8	69.8	3,278	23.9	52.2	1,072	0.19
Five-year population growth (%)	1.12	4.25	3,278	0.17	3.57	1,072	0.24
Population density	148	506	3,278	115	441	1,072	0.07
Branches per 1,000 inhabitants	0.23	0.14	3,278	0.25	0.13	1,072	–0.15
Employment ratio	0.68	0.04	3,278	0.68	0.04	1,072	0.03
Relative labor income	0.95	0.12	3,278	0.93	0.11	1,072	0.15
Manufacturing share	0.33	0.18	3,278	0.37	0.18	1,072	–0.18

This table compares firm-years (Panel A) and municipality-years (Panel B) with negative and non-negative values, respectively, of the instrument $Z_{j,t}$ across a set of covariates measured at time $t-1$. The five-year population growth is defined as the percent change in a municipality's population between years $t-6$ and $t-1$; population density as inhabitants per square kilometer; manufacturing share as the share of manufacturing firms in total employment at non-financial firms; relative labor income as the ratio of the average labor income in a municipality to the national average; and employment ratio as the share of inhabitants between the ages of 20 and 74 years that are employed. All other variables are self-explanatory. The normalized difference in means is, following Imbens and Rubin (2015), defined as $\left(\bar{X}_{Z<0} - \bar{X}_{Z\geq 0}\right) / \left[\left(S^2_{Z<0} + S^2_{Z\geq 0}\right)/2\right]^{0.5}$, where $\bar{X}$ and S are the means and standard deviations of the comparison variables in the respective groups.

the bank Alpha as long as Alpha has branches in Stockholm, even if Alpha does not in fact close any of its Stockholm branches.

One possible violation of the exclusion restriction is that widespread branch closures by Alpha may lead households and firms in municipalities where Alpha has a large presence to become concerned that additional branches will be closed in the coming years, including in municipalities where Alpha is active but has not yet closed any branches. Hence, real as well as financial uncertainty may increase as a consequence of Alpha's nationwide closures even in as-of-yet unaffected municipalities and thereby affect local credit demand and real economic activity. If so, our estimates will capture not only the direct effects of branch closures, but also any indirect effect working through the expectation of future branch closures.

Monotonicity. The fourth and final assumption necessary for interpreting the 2SLS estimates as the local average treatment effect is monotonicity, which requires that a higher nationwide branch-closure rate of a bank never causes a higher overall branch growth rate in the municipalities in which the bank has a large presence, and vice versa. The most plausible cause of non-monotonicity in our setting would be that outside banks see growth opportunities when incumbent banks close down branches in a municipality and therefore decide to open up new branches to such an extent that the overall number of branches in the municipality on net increases.

There are at least two reasons for thinking that the monotonicity requirement is fulfilled. First, our empirical setting is one in which the overall number of bank branches declined dramatically and in which no a large bank pursued a branch-growth strategy. Hence, the probability that the decision of one bank to close branches nationwide caused an increase in the overall number of branches in the municipalities in which it was active at the time—via an induced expansion of other banks' local branch networks—appears small a priori. Second, a testable implication of the monotonicity assumption is that the first-stage estimate is positive in all subsamples of the data. We evaluate this implication by estimating the first-stage regression (4) separately for 30 subsamples of the data, constructed by splitting the sample at the median of each of the firm and municipality characteristics in Table 2. The resulting first-stage estimates, reported in Table A1 in Appendix A, are consistently positive and large in magnitude, which supports the monotonicity assumption.

3.3.2 External validity

To assess whether the local average treatment effects captured by our 2SLS estimates are likely to be representative of the effects of branch closures more generally, we analyze if and how complier municipalities differ from other municipalities. Since our instrument and endogenous regressor are both continuous variables, we focus on how the *degree* of compliance varies with observable municipality characteristics. More specifically, we use the following variation on the first-stage regression, estimated at the municipality level:

$$\Delta Branches_{j,t} = \phi_j + \psi_t + \xi \cdot Z_{j,t} + \gamma \cdot C_{j,t-1} + \delta \cdot Z_{j,t} \cdot C_{j,t-1} + \theta \cdot \mathbf{X_{j,t}} + u_{j,t}, \tag{5}$$

where $C_{j,t}$ is a vector of municipality characteristics measured in year $t-1$, and $\mathbf{X}$ is a vector comprising the same firm-level variables as in the baseline specification, but aggregated to the municipality-year level by means of averaging. We then quantify how the degree of compliance varies with each municipality characteristic by comparing the estimated marginal effect of the instrument at the 10th and 90th percentile of the respective characteristics. Standard errors are clustered at the municipality level.

The estimation results, reported in Table 3, show that the degree of compliance does not vary significantly with most of the municipality characteristics under consideration. To see this, note that the estimated interaction terms from equation (5) are statistically insignificant at the five-percent level for all but one of the municipality characteristics. The one characteristic for which the interaction is significant is municipality population, but the magnitude of the difference is modest: the effect of a one percentage point decrease in the instrument, $Z_{j,t}$, on the growth rate of bank branches is –1.25 percentage points in municipalities with small populations and –1.12 percentage points in municipalities with large populations. That the heterogeneity in instrument strength across observable municipality characteristics in gen-

Table 3: Complier characteristics

	Marginal effect at 10th pct	Marginal effect at 90th pct	Ratio of marginal effects (90/10)	p-value
Population (1000s)	1.25	1.12	0.90	0.025
Five-year population growth (%)	0.89	1.50	1.67	0.088
Population density	1.17	1.21	1.03	0.090
Branches per 1,000 inhabitants	0.97	1.50	1.55	0.026
Employment ratio	1.38	1.00	0.72	0.153
Relative labor income	1.28	1.08	0.84	0.303
Manufacturing share	1.01	1.42	1.41	0.085

This table reports the marginal effect of the instrument on the growth rate of branches at the 10th and 90th percentiles, respectively, of the distribution of the various municipality characteristics in the leftmost column, as estimated using equation (5). More specifically, the marginal effect at percentile 10 (90) of municipality characteristic k is given by $\hat{\xi^k} + \hat{\delta^k} \cdot C^k_{j,t-1}$, where $C^k_{j,t-1}$ takes on the value at the 10th (90th) percentile of its distribution. The ratio is the marginal effect at the 90th percentile relative to the marginal effect at the 10th percentile. The p-value is for a two-sided test of the null hypothesis that the interaction term in (5) is zero. See the notes to Table 2 for variable definitions.

eral is small speaks against the concern that the local average treatment effects identified by our 2SLS estimates are uninformative about the average causal effect of branch closures in the population of municipalities.

4 Empirical Results

4.1 The effect of branch closures on local credit supply—Main estimates

The baseline estimates of the effect of bank-branch closures on the supply of credit to local firms are reported in Table 4. The first column reports the first-stage estimate: the coefficient implies that a one percentage point decrease in predicted branch growth (the instrument) is associated with a 1.16 percentage point decrease in actual branch growth—the estimate is not statistically different from 1. The third column reports the baseline 2SLS estimate of the effect of branch closures on local credit supply: the coefficient implies that closing 10 percent of the bank branches in a municipality leads to a decline of 1.8 percent in the volume of bank

Table 4: The effect of branch closures on local credit supply

		Estimation horizon $h = 2$		
	First stage	Reduced form	2SLS	OLS
$Z_{j,t}$	1.168***	0.214**		
	[0.091]	[0.102]		
$\Delta Branches_{j,t}$			0.183**	0.029*
			[0.089]	[0.017]
Number of observations	596,261	561,465	561,465	561,465

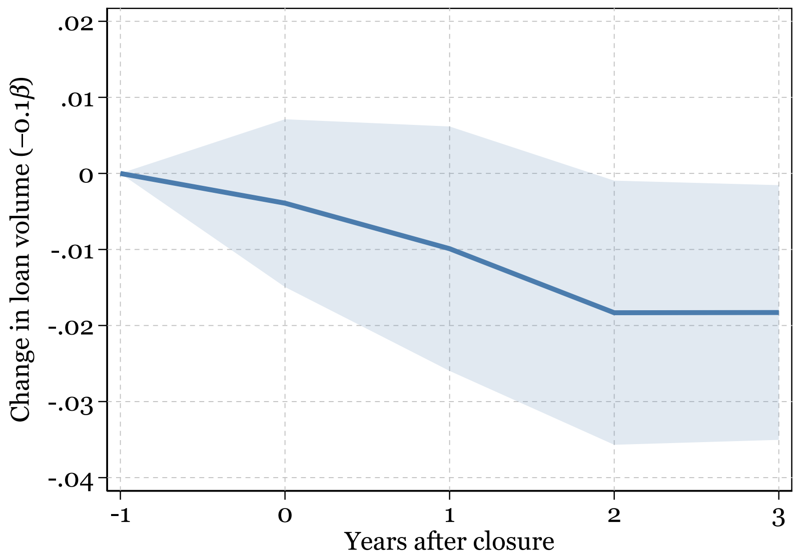
The reported coefficients correspond to the first-stage, reduced form, two-stage least squares, and OLS estimates, respectively, from estimations with $\Delta Loans_{i,t+3}$ as dependent variable. More specifically, the first-stage coefficient is obtained from OLS estimation of equation (4); the 2SLS coefficient from the two-stage least squares estimation of equations (4) and (2); the OLS coefficient from OLS estimation of equation (2); and the reduced form coefficient from the regression of the dependent variable on $Z_{j,t}$ and $\mathbf{X_{i,t}}$. All regressions include the baseline set of control variables listed in section 3.2. Standard errors cluster-adjusted at the municipality-year level are reported in square brackets. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

loans to non-financial firms in the municipality after two years. The estimate implies that the closing of bank branches has a statistically significant and ecnomically important negative impact on local credit outstanding to corporate borrowers.

The second and fourth columns report the reduced-form and OLS estimates, respectively, of the effect of branch closures on local credit supply. The reduced form estimate is close to the 2SLS estimate, which follows mechanically from the fact that the first-stage coefficient is close to one; the OLS estimate, on the other hand, is close to zero and statistically insignificant. Hence, OLS estimation of the effects of branch closures most likely suffers from severe omitted variable bias, although heterogeneous treatment effects in the population may also explain some part of the difference between the OLS and IV estimates (see, e.g., Dahl, Kostøl and Mogstad, 2014).

In Figure 3, we show how the effect of branch closures on credit supply evolves over time by plotting the 2SLS estimate of β^h for each estimation horizon h, scaled to correspond to

Figure 3: The evolution of the effect of branch closures on local credit supply



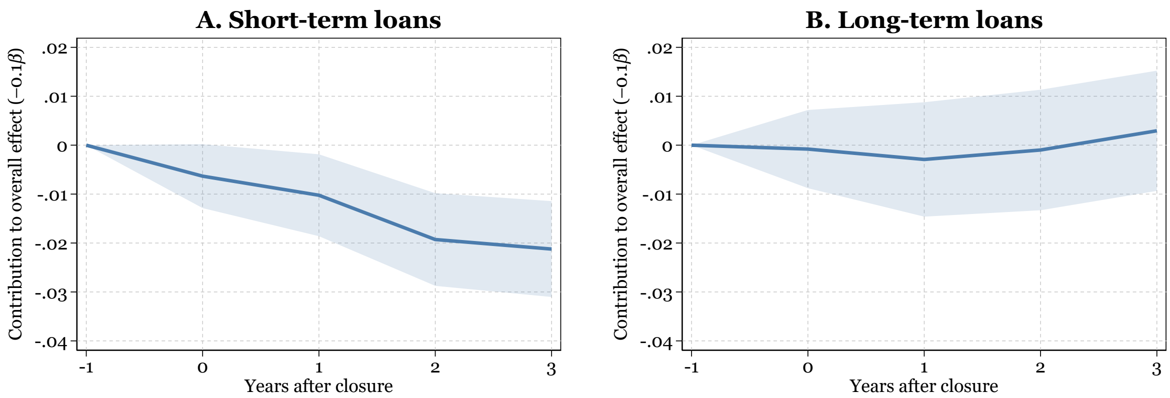
This figure plots the estimated effect of bank branch closures on local credit supply. The line corresponds to the respective two-stage least squares estimates of β^h obtained from the estimation of equations (4) and (2) for estimation horizons $h = 0, 1, 2, 3$. The estimates are scaled to correspond to the effect of closing 10 percent of the bank branches in a municipality. Standard errors are clustered at the municipality-year level and the shaded areas represent 95-percent confidence intervals.

the effect of closing 10 percent of the bank branches in a municipality. The results show that bank lending starts to decline immediately after branch closures: the volume of bank loans extended to local firms declines by around two percent percent within two year of a closure and then stabilizes at this level.

Next, we decompose the overall credit-supply effect of branch closures into the parts attributable to short-term and long-term loans, respectively, where short-term loans are loans with a remaining maturity of one year or less. We undertake the decomposition by additively decomposing the main dependent variable, $\Delta Y_{i,t+h}$, into a short-term loan component, $\Delta Y^{Short}_{i,t+h}$, and a long-term loan component, $\Delta Y^{Long}_{i,t+h}$.[10] We then estimate the 2SLS model with $\Delta Y^{Short}_{i,t+h}$ and $\Delta Y^{Long}_{i,t+h}$, respectively, as dependent variables in the second stage. Since $\Delta Y_{i,t+h} = \Delta Y^{Short}_{i,t+h} + \Delta Y^{Long}_{i,t+h}$, the coefficients obtained in the regressions on the two com-

[10] More specifically, $\Delta Y^{Short}_{i,t+h}$ is defined as $\left(Y^{Short}_{i,t+h} - Y^{Short}_{i,t-1}\right) / [(Y_{i,t+h} + Y_{i,t-1})/2]$—i.e., we replace the numerator in $\Delta Y_{i,t+h}$ with the change in short-term loans, but keep the denominator unchanged. $\Delta Y^{Long}_{i,t+h}$ is defined analogously.

Figure 4: Contribution of short- and long-term loans to the overall credit-supply effect



This figure plots the estimated effect of bank branch closures on short- and long-term loans. The lines corresponds to the respective two-stage least squares estimates of β^h obtained from the estimation of equations (4) and (2) for estimation horizons $h = 0, 1, 2, 3$ with $\Delta Y^{Short}_{i,t+h}$ and $\Delta Y^{Long}_{i,t+h}$, respectively, as dependent variables. The estimates are scaled to correspond to the effect of closing 10 percent of the bank branches in a municipality. See the main text for the definitions of $\Delta Y^{Short}_{i,t+h}$ and $\Delta Y^{Long}_{i,t+h}$. Standard errors are clustered at the municipality-year level and the shaded areas represent 95-percent confidence intervals.

ponents of total loans then add up to the coefficient from the baseline regression on total loans—i.e., $\beta^h = \beta^{h,Short} + \beta^{h,Long}$. This enables us to assess to what extent short-term and long-term loans, respectively, account for the overall credit-supply effect.

The results, plotted in Figure 4, demonstrate that the entire credit-supply response to branch closures is driven by a contraction in short-term lending. To see this, note that the response of short-term loans is negative and statistically significant, while the response of long-term loans is a series of fairly precisely estimated zeroes. Why does short-term lending respond more than long-term lending to branch closures? Our conjecture is the following. When it comes to business lending, the main purpose of a local bank branch is to facilitate the collection of soft information and thereby improve the bank's ability to engage in informationally sensitive lending. Short-term business lending—i.e., lending for operational and working-capital purposes—is frequently unsecured, or secured by assets with uncertain liquidation value, such as a firm's inventory or accounts receivable. Long-term business lending, on the other hand, is commonly secured by tangible assets with more transparent liquidation values and should therefore be less dependent on local, soft information. If so, one would expect precisely what we find, namely, that short-term lending is more sensitive to the disappearance of local bank branches than long-term lending. In what follows, we will assess other empirical implications of this conjecture.

4.2 The effect of branch closures on local credit supply—Cross-sectional heterogeneity

Theory predicts that the effects of branch closures will be heterogeneous in the population—for example, small firms are predicted to be more sensitive to branch closures than large firms, as small-business lending relies on soft information (Agarwal and Hauswald, 2010). In what follows, we undertake a cross-sectional heterogeneity analysis to assess whether this is indeed the case. We do so by estimating the baseline specification on various subsamples of firms—obtained by splitting the sample at given cutoffs of relevant firm characteristics, such as size or age—and testing whether the effect of branch closures on credit supply differs across the subsamples. The exact levels of the cutoffs are chosen to make the resulting subsamples differ in an economically meaningful way, while also ensuring that the subsamples are large enough to give the tests sufficient statistical power. All split variables are measured at time $t-1$ to ensure that we do not split based on characteristics possibly affected by the treatment.

We begin with firm size. Our preferred measure of size is employment, where we classify firms with fewer than 100 full-time equivalent (FTE) employees as small and firms with more than 100 employees as large.[11] The results of the employment-split, reported in row A of Table 5, show that while the point estimates are fairly similar in the two groups—a 10-percent branch closure causes lending to decline by 2.0 percent to small firms and by 1.6 percent to large firms—the estimate is only statistically significant for small firms. We obtain qualitatively very similar results when constructing the size-split based on total assets or sales instead of employment, as shown in rows B and C.[12] Hence, the negative effect of branch

[11]To avoid misclassifying firms that are part of corporate groups, we classify group firms based on the size of the corporate group to which they belong—i.e, we sum employment across all firms in the group and assign the total employment figure to all individual firms belonging to the group.

[12]As in the case of employment, we classify group firms based on group-level assets and sales.

Table 5: Cross-sectional heterogeneity in the credit-supply effects

	Dependent variable: $\Delta Loans_{i,t+2}$					
	Low (L)		High (H)		Test of equality	
	β^L	N^L	β^H	N^H	H_1	p-value
A. Employment (100 FTEs)	0.202**	394,154	0.164	156,458	$\beta^L > \beta^H$	0.396
	[0.089]		[0.143]			
B. Assets (150 MSEK)	0.198**	447,173	0.101	106,543	$\beta^L > \beta^H$	0.257
	[0.091]		[0.146]			
C. Sales (250 MSEK)	0.198**	445,372	0.153	107,759	$\beta^L > \beta^H$	0.385
	[0.090]		[0.153]			
D. Age (15 years)	0.504***	74,399	0.137	478,186	$\beta^L > \beta^H$	0.033
	[0.172]		[0.090]			
E. Asset tangibility (50%)	0.222**	368,629	0.084	184,859	$\beta^L > \beta^H$	0.142
	[0.111]		[0.095]			

This table reports two-stage least squares estimates from estimations of equations (4) and (2) in various subsamples of the population, where the second-stage dependent variable is $\Delta Loans_{i,t+2}$. All regressions include the baseline set of control variables listed in section 3.2. The subsamples are constructed by splitting the sample at the cutoff of each variable listed in the leftmost column. Low and High denote firms below and above the cutoff, respectively. β^L and β^H are the estimated effects, and N^L and N^H the number of observations, in the respective subsamples. The test of equality refers to one-sided t-tests of the null hypothesis that the coefficients in the subsamples are equal, against the alternative hypothesis provided in column H_1. FTE is short for full-time equivalents. Standard errors cluster-adjusted at the municipality-year level are reported in square brackets. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

closures on credit supply is primarily a small-firm phenomenon.

Next, we split the sample into younger firms (less than 15 years old) and older firms (more than 15 years old) to assess the role of firm age in the effect of branch closures on credit supply.[13] Younger firms are likely more sensitive to soft information produced in branches (Aretz, Campello and Marchica, 2020; Black and Strahan, 2002). The results are reported in row D of Table 5. We find that branch closures have a large, statistically significant effect on

[13]As with the size splits, we want to avoid misclassifying firms that belong to corporate groups. We therefore assign the age of the oldest firm in a group to all firms in the group when constructing the age-based subsamples. Fifteen years may seem a high threshold for identifying young firms, but in fact more than four fifths of the firms in our sample are fifteen years or older. The high average age of firms in our sample reflects the fact that the sample only comprises firms with bank loans, who are on average older than firms without bank loans.

credit supply for young firms and a considerably smaller, statistically insignificant effect for older firms; the difference is significant at the five-percent level in a one-sided test. The point estimate for young firms implies that closing 10 percent of the branches in a municipality leads loan balances of young firms to decline by 5.0 percent. Hence, the results suggest that age is an even more important characteristic than size when it comes to the credit-supply effects of branch closures.

Finally, we consider the role of asset tangibility: firms with more tangible assets are better able to pledge collateral when borrowing (e.g., Almeida and Campello, 2007) and should therefore be less sensitive to bank-branch closures, because the importance of soft information declines as a loan becomes better collateralized. We split the sample based on whether a firm's asset tangibility (defined as the ratio of tangible assets to total assets) is above or below 50 percent, which corresponds to comparing the top tercile with the bottom two terciles of the distribution of asset tangibility. The results, provided in the bottom row of Table 5, show that the point estimate of the effect of branch closures is more than twice as large for low-tangibility firms as for high-tangibility firms, although we cannot statistically reject equality of the point estimates in the two groups. Moreover, the estimated effect is statistically significant for low-tangibility firms but not for high-tangibility firms. These findings corroborate the view that soft information collected via local branches is particularly important when pledgeable assets are scarce.

Two further comments on the asset-tangibility results are in order. First, the importance of asset tangibility is consistent with the previously documented finding that the overall credit-supply effect of branch closures is accounted for by short-term loans, because short-term loans are less often secured by high-quality collateral than long-term loans. Second, it suggests that working capital should be more affected by branch closures than fixed investment, because working capital tends to be financed by short-term, uncollateralized loans (or loans secured by lower-quality collateral), whereas fixed investment tends to be funded by long-term loans secured by tangible assets. We explore this implication in the section on real effects below.

4.3 The effect of branch closures on local credit supply—Specification checks

Table 6 reports results for various various specification checks for the baseline credit-supply estimates. In each case, we report the first stage, reduced form, and 2SLS estimates for the two-year estimation horizon ($h = 2$). The baseline estimates are provided in the top row for comparison.

First, we augment the baseline specification with a set of municipality-level controls comprising population size, five-year population growth, population density, bank-branch density, employment ratio, average labor income (measured relative to the national average), and the manufacturing share of employment, all measured at time $t-1$. The resulting estimates, reported in the second row of Table 6, are quite similar to the baseline estimates, although now marginally insignificant ($p = 0.052$). The same is true when we add squares of the firm-level control variables to the baseline specification (third row). These findings are consistent with the instrument-independence assumption.

Second, we employ an alternative instrument, which uses the initial (2001) market shares of the banks instead of the $t-1$ market shares to ensure that the results are not biased by endogenously evolving market shares during the sample period. More specifically, the alternative instrument is defined as:

$$Z_{j,t}^{Initial} = \sum_{b} \frac{Branches_{b,j,2001}}{Branches_{j,2001}} \cdot \Delta Branches_{b,t}, \tag{6}$$

where $Branches_{b,j,2001}$ and $Branches_{j,2001}$ are the number of branches of bank b in municipality j in 2001 and the total number of branches in municipality j in 2001, respectively. The first-stage estimate is noticeably smaller with the alternative instrument—0.66 compared to 1.17 with the main instrument—which is unsurprising given that an instrument based on initial market shares becomes a progressively worse reflection of actual market shares as time passes. The 2SLS point estimate with $Z_{j,t}^{Initial}$ as instrument is close to the baseline estimate, but the standard error is almost twice as large—unsurprisingly given the lower precision of the alternative instrument—with the result that the estimate becomes statistically insignificant. The similarity of the point estimates nevertheless suggests that endogenously evolving

Table 6: Specification checks for credit-supply estimates

	Dependent variable: $\Delta Loans_{i,t+2}$			
	First stage	Reduced form	2SLS	N
1. Baseline specification	1.168***	0.214**	0.183**	561,465
	[0.091]	[0.102]	[0.089]	
2. Including municipality controls	1.112***	0.178*	0.160*	561,465
	[0.089]	[0.098]	[0.089]	
3. Including non-linear firm controls	1.168***	0.197*	0.169*	561,465
	[0.091]	[0.101]	[0.087]	
4. Instrumenting with $Z_{j,t}^{Initial}$	0.656***	0.143	0.217	561,465
	[0.095]	[0.102]	[0.157]	
5. Dropping if $Branches_{j,t-1} \leq 1$	1.174***	0.244**	0.209**	549,502
	[0.092]	[0.111]	[0.096]	

This table reports the results of various specification checks. The reported coefficients correspond to the first-stage, reduced form, and two-stage least squares estimates, respectively, from estimations with $\Delta Loans_{i,t+2}$ as dependent variable. The alternative instrument $Z_{j,t}^{Initial}$ is constructed based on the initial (2001) bank market shares, rather than the $t-1$ market shares used in the baseline. The municipality controls comprise the seven municipality characteristics included in Table 3. Standard errors are clustered at the municipality-year level in all regressions. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

market shares do not bias the baseline estimates.

Third, we estimate the baseline regressions excluding municipality-years where the number of branches in year $t-1$ is zero or one. The results—reported in the fourth row of Table 6—are very similar to the baseline results, which demonstrates that our findings are not primarily driven by municipalities with very few branches.

4.4 Real effects of branch closures on local firms

We now assess the real effects of reduced loan supply due to bank-branch closures. Estimates are reported in Figure 5, which plots the 2SLS estimates of β^h from regressions with employment, sales, assets (as well as two of its subcomponents, fixed assets and working capital), and firm exit as dependent variables.[14] As before, the coefficients are scaled to correspond to the effects of closing down 10 percent of the branches in a municipality.

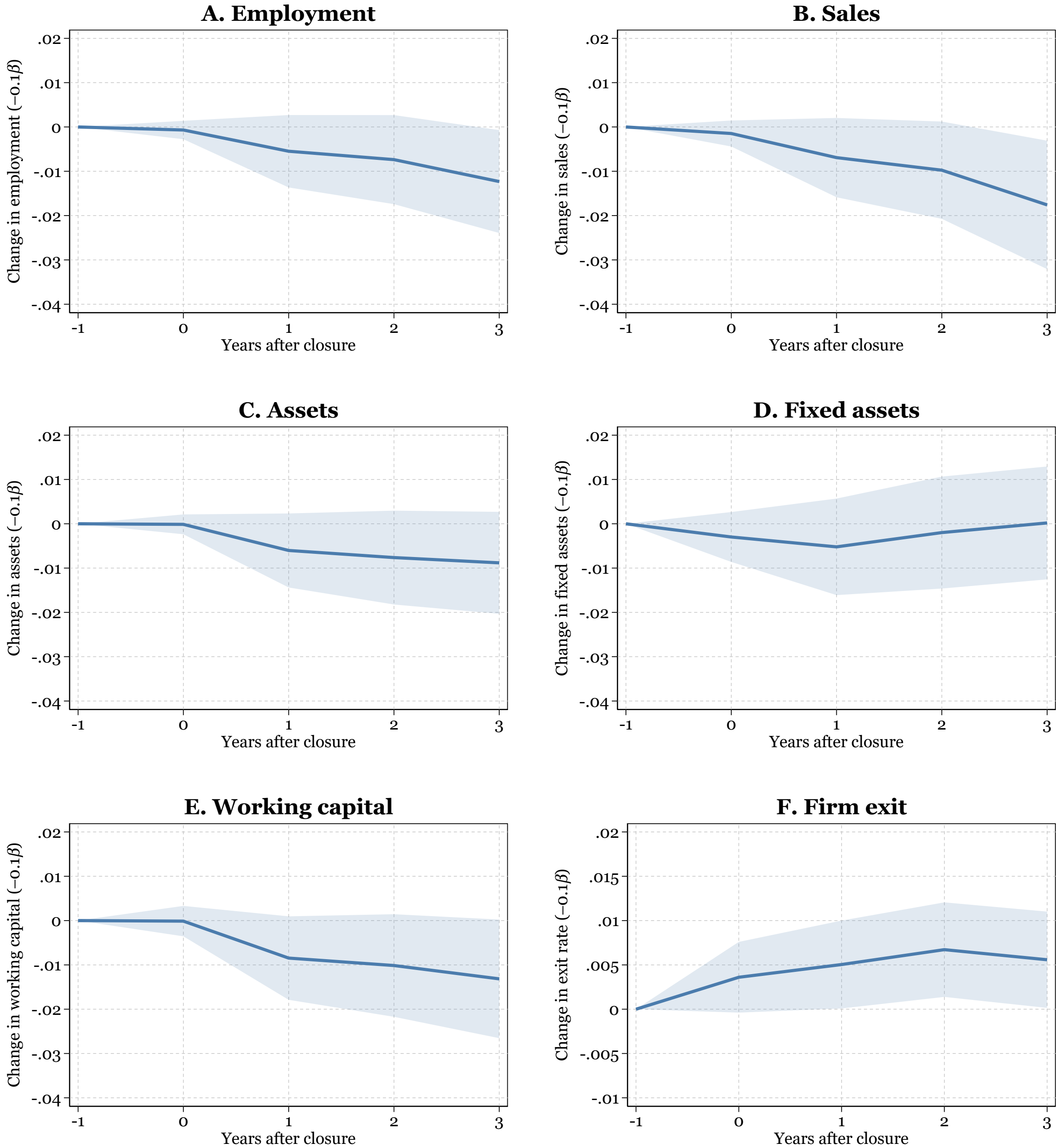
Estimates of the employment effect of branch closures are shown in Panel A. Closing 10 percent of the branches in a municipality leads to an average employment decline of 1.3 percent for firms operating in the municipality at the three-year horizon. The effect on sales, plotted in Panel B, is slightly larger in magnitude: closing 10 percent of the branches in a municipality leads the sales of local firms to decline by 1.8 percent. Unlike the credit-supply effect, which becomes significant after two years, the employment and sales effects only become significant after three years. The real responses thus lag the credit-supply response by about a year—i.e., the estimates suggest that it takes around a year for the credit-supply contraction induced by branch closures to reach its full impact on the real activity of firms, as measured by sales and employment.

Panel C displays how assets respond to branch closures. Estimates are negative but not significantly different from zero at any estimation horizon. When we consider the two most important sub-components of assets separately, however, we find that although fixed assets are not significantly affected by branch closures (Panel D), working capital is (Panel E). More specifically, a firm's stock of working capital (defined as inventories plus accounts receivable) declines by 1.3 percent over three years following a 10-percent reduction in the number of local bank branches.

Bank branch closures reduce employment, sales, and working capital of local firms, but have no (detectable) effect on fixed assets. This is consistent with the hypothesis that working-capital investments should be particularly affected by branch closures, as working-capital funding is frequently unsecured (or secured by low-quality collateral) and therefore more dependent on the soft information that local bank branches are meant to collect (Ivashina, Laeven and Moral-Benito, 2022; Lian and Ma, 2020). These results thus also agree with the findings which motivated the hypothesis about working capital, namely that (i) short-term loans fully account for the overall credit-supply effect of branch closures, and

[14]As in the case of loans, the regressions with employment, sales, and assets are estimated with the symmetric growth rate as dependent variable. When we estimate the effects on firm exit, the dependent variable is a dummy, $Exit_{i,t+h}$, equal to one if firm i exits between years t and $t+h$, and zero otherwise.

Figure 5: Real effects of bank-branch closures on local firms



This figure plots the two-stage least squares estimates of the effects of bank branch closures on employment (Panel A), sales (Panel B), assets (Panel C), fixed assets (Panel D), working capital (Panel E), and firm exit (Panel F). Working capital is the sum of inventories and accounts receivable. The estimates are scaled to correspond to the effect of closing 10 percent of the bank branches in a municipality. Standard errors are clustered at the municipality-year level and the shaded areas represent 95-percent confidence intervals.

(ii) branch closures only have a significant effect on firms with low asset tangibility.

Finally, the estimates of the firm-exit response, shown in Panel D, demonstrate that exit increases significantly after branch closures: the probability that a firm exits the market sometime during the coming three years increases by around 0.6 percentage points if 10 percent of the bank branches in the firm's municipality are closed. Hence, the credit-supply contractions induced by branch closures may become severe enough to drive some firms out of the market altogether.

5 Concluding remarks

We examine the impact of large reductions of banks' local branch networks on business lending. More than half of all bank branches have been closed in Sweden in the last two decades, with the most rapid decline taking place in recent years. Given the importance of distance in bank-borrower interactions, the potential impact on SME lending is negative and large.

In empirical tests on Swedish data, we employ a shift-share instrument—based on differences in the geographic dispersion of large banks and in the timing of their branch-network downsizing—to identify the impact of branch closures on local firms. Our main finding is that lending to local firms declines substantially and rapidly when branches are closed, and that this, in turn, has adverse effects on the real activity of firms.

Our results suggest that the accelerating trend toward digital delivery of retail bank services—visible across the OECD and beyond—may harm credit supply to small and medium-sized firms, where lending decisions traditionally involve soft information collected through branches. Without branches, the credit supply may tilt toward asset-backed loans (Lian and Ma, 2020) and secured credit contracts (Benmelech, Kumar and Rajan, 2022), potentially restricting new firm formation and entrepreneurship (Black and Strahan, 2002).

More generally, large-scale, technology-driven disruption, even if it is beneficial overall, is harmful to some activities and to some firms. In banking, technology-driven retail banking efficiencies may come at the expense of SME lending. Perhaps this creates an opportunity for innovation in the provision of SME loans. In the meantime, there may be important implications for economic growth, employment, and monetary policy of shrinking branch networks.

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Appendix A. Additional tables and figures

Table A1: First-stage estimates in subsamples of the data

	Below median		Above median	
	$\hat{\xi}$	se($\hat{\xi}$)	$\hat{\xi}$	se($\hat{\xi}$)
A. Firm-level characteristics				
Assets (MSEK)	1.248***	0.093	1.279***	0.098
Sales (MSEK)	1.232***	0.092	1.291***	0.100
Number of employees	1.233***	0.093	1.283***	0.098
Age (years)	1.306***	0.099	1.244***	0.094
Debt/Assets	1.287***	0.097	1.236***	0.093
EBIT/Assets	1.222***	0.091	1.279***	0.097
Cash/Assets	1.213***	0.092	1.312***	0.098
Probability of default	1.300***	0.097	1.216***	0.094
B. Municipality-level characteristics				
Population (1000s)	1.108***	0.103	2.080***	0.294
Five-year population growth (%)	1.109***	0.114	1.724***	0.205
Population density	1.031***	0.109	2.002***	0.224
Branches per 1,000 inhabitants	1.797***	0.206	1.019***	0.101
Employment ratio	1.290***	0.130	1.346***	0.149
Relative labor income	1.160***	0.114	1.642***	0.206
Manufacturing share	1.498***	0.173	1.124***	0.114

This table reports estimates of the first-stage regression (4) for 28 subsamples of the data. The respective subsamples are constructed by splitting the sample at the median of each of the firm and municipality characteristics in the table. See the notes to Table 2 for variable definitions. Standard errors are clustered at the municipality-year level in all regressions. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

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