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Banking Without Branches*

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Abstract

Banks' branch networks are contracting rapidly in many countries. We study the effects of these large-scale branch closures on firms' access to credit and real economic activity. Our empirical setting is Sweden, where two thirds of all bank branches have closed in the past two decades. Using a shift-share instrument and micro data comprising the near-universe of Swedish firms and bank branches, we document that corporate lending declines substantially following branch closures, mainly via reduced lending to small, collateral-poor, and less productive firms. The reduced credit supply in turn causes contractions in the real activity of incumbent firms, as well as lower entry of new firms. The disappearance of bank branches thus has far-reaching implications for the economy.

Keywords: Banks; branch closures; credit supply; soft information.

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

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1 Introduction

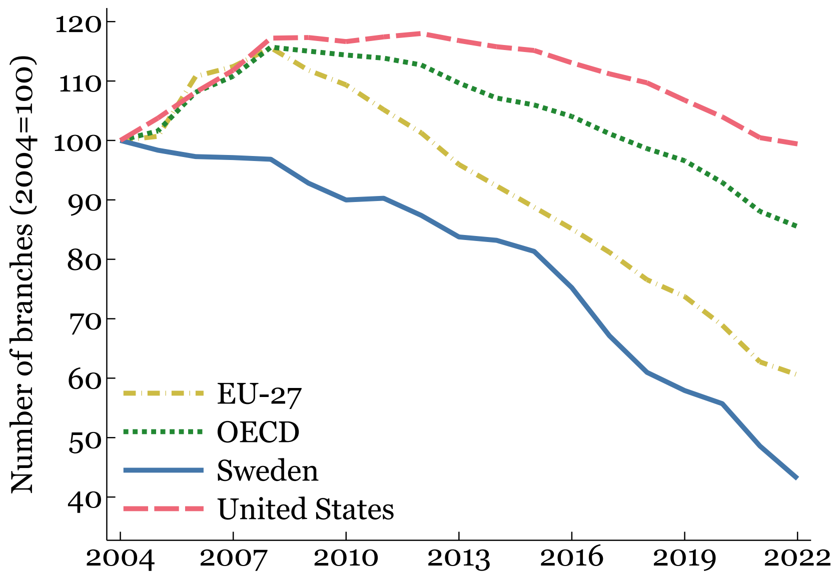
In the last twenty years, technological change has made possible the digital distribution of many financial services, reducing or eliminating the need for in-person interactions. Retail banking has been dramatically affected by the move to digital distribution: the share of European households using internet banking services almost tripled between 2007 and 2023, going from 24 to 64 percent; in some parts of Europe, such as the Nordic countries and the Netherlands, nearly every household uses internet banking (Eurostat, 2023). The rise of digital banking has fundamentally changed the economics of maintaining a physical branch network and has consequently led many banks to close branches rapidly in recent years: in the OECD countries, for example, the number of bank branches fell by almost 30 percent between its 2008 peak and 2022 (Figure 1).[1]

In some segments of the bank market, however, digital advances have not kept pace with those in retail banking. This is particularly true for SME lending: according to a recent survey of managers of U.S. medium sized banks, for example, SME services are only half as likely as retail services to be delivered digitally (Alix, 2022). Indeed, close physical proximity between lenders and borrowers is a well-known and pervasive feature of commercial lending (Petersen and Rajan, 1994, 2002), which reflects the importance of soft information for banks' ability to screen and monitor borrowers (Agarwal and Hauswald, 2010) and thereby develop relationships (Berger et al., 2005) and grant high-quality loans (Granja, Leuz and Rajan, 2022). The disappearance of bank branches—and the increased distance between borrowers and lenders that it entails—may therefore reduce the supply of credit to firms, with negative consequences for the real economy.

We investigate the hypothesis that large-scale branch closures reduce the supply of credit to firms using detailed micro-level data comprising the universe of Swedish bank branches and firms between 2001 and 2023. Sweden offers a suitable empirical setting for two reasons. First, branch closures have been rapid in the past decades: almost two thirds of Swedish bank branches disappeared between 2001 and 2023; as of 2023, 43 out of Sweden's 290 municipalities no longer have a single bank branch. Sweden is not unique in seeing a drop, but the shift started earlier and has proceeded more rapidly than in most countries, making this a valuable

[1] For evidence of technology's impact on retail banking, see Lewellen and Williams (2021) and He et al. (2022). Moreover, cash use has declined in line with the rise of digital payments, further reducing the need for bank branches. In Sweden, the setting of our empirical study, real cash outstanding peaked in 2005, and fell by 56 percent to 2023.

Figure 1: Bank branches across regions, 2004-2022



This figure plots the number of bank branches in each year between 2004 and 2022 for Sweden, the United States, the European Union, and the OECD. The numbers are indexed with 2008 as base year. The Swedish data are from this paper (see section 3.1), the OECD and EU data from the IMF's Financial Access Survey (IMF, 2023), and the US data from the FDIC's historical bank database (FDIC, 2023). The EU series excludes Romania, and the OECD series Norway and the UK, due to large numbers of missing observations in the IMF database.

opportunity to understand the consequences of the shift away from branches.[2]

Second, the market is dominated by four large banks, all of which have closed branches extensively, but at different times. This underpins an identification strategy based on a shift-share instrument in the spirit of Bartik (1991), which combines (i) spatial variation in the market shares of the respective banks across municipalities and (ii) variation in the timing of each bank's branch closures. Our identification strategy treats the shifts (the banks' nationwide branch growth rates) as exogenous, but does not make any assumption about the exogeneity of the shares (the banks' market shares in different municipalities). We therefore follow Borusyak, Hull and Jaravel's (2022, 2025) guidelines regarding model specification and diagnostic tests closely, and provide extensive empirical evidence showing that key identifying assumptions are likely to be satisfied in our setting. The shift-share instrument is thus plausibly valid and therefore

[2]Comparing Sweden to other OECD countries, the closure rate is high but not an outlier: between 2007 and 2022, the number of bank branches grew in two countries (Mexico and Turkey), did not change in two countries (Austria and Japan) and shrank in the rest (for example, by 22 percent in France, 42 percent in Italy and 65 percent in Spain). Other Nordic countries and the Netherlands have seen large declines.

enables us to identify the local average treatment effect of branch closures.

We implement our empirical tests by means of firm-level local-projections regressions in which the endogenous regressor (the percent change in the number of bank branches in a firm's municipality in a given year) is instrumented using the shift-share instrument (the weighted average nationwide branch growth rate of the banks that operate in the municipality). The local-projections specification enables us to trace out the dynamics of the effect of branch closures: we estimate effects up to a four-year horizon, and use the four-year effects as our focal estimates. We begin by estimating the effect of branch closures on credit supply. The outcome variables are constructed on the basis of the committed amount of bank credit to a firm, which comprises the amount of loans on the firm's balance sheet as well as any undrawn credit-line commitments. We use three outcome variables for assessing the credit-supply effects of branch closures: the symmetric growth rate of credit (capturing both extensive and intensive margin effects) and indicator variables for loan exit and entry (capturing the respective extensive margins).

Our baseline estimate implies that closing 30 percent of the bank branches in a municipality leads to an average decline in the outstanding credit stock of local firms of 12 percent over a four-year period. The loan-exit margin plays an important role in the response: the probability that a firm loses access to loans altogether increases by 2.8 percentage points following a 30-percent branch closure. Cross-sectional heterogeneity analyses show, moreover, that the credit-supply effect of branch closures is larger for firms that are small, collateral-poor (low asset tangibility), and less productive—i.e., firms for which banks' lending decisions are likely to be particularly reliant on soft information. We also find that firms have limited ability to counteract the credit-supply contractions induced by branch closures by adjusting on other financial margins, like cash holdings and trade-credit positions.

Having shown that branch closures cause credit-supply contractions that firms are unable to offset by other means, we assess whether the closures have negative effects on firms' real economic activity. We do so in two steps. First, we estimate the effects of branch closures on the real activity of incumbent firms with bank loans. We consider five outcomes: sales, employment, fixed assets (property, plant and equipment), working capital (accounts receivable and inventory), and exit risk. Our estimates imply that the closure of 30 percent of the bank branches in a municipality causes local firms to experience a 4.6 percent decline in sales, a 4.3 percent decline in the stock of working capital, and a 0.8 percentage point increase in exit risk over a four-year period. Employment and fixed assets, on the other hand, do not decline significantly in the full sample of firms. The explanation is that different firms use bank loans for different purposes. In the subsample of firms holding credit lines, branch closures cause declines in sales, employment, and working capital, as well as increases in exit risk, but do not affect fixed assets. In the subsample of firms holding term loans, on the other hand, branch closures cause declines in sales and fixed assets, but do not significantly affect employment, working capital, or the risk of exit. The real effects of branch closures thus depend on the nature of financial contracts.

In a final empirical exercise, we examine how branch closures affect business dynamism—i.e., their impact on firm entry and exit, and consequently on the overall number of firms in the economy. We find that the closure of 30 percent of the bank branches in a municipality lowers the growth rate of firms in a municipality by 3.7 percentage points over a four-year period. This decline is fully accounted for by a decrease in firm entry, which implies that branch closures do not only have negative effects on incumbent firms with bank loans, but also on entrepreneurship and new firm formation. Branch closures may in this way have long-lasting negative effects on the local economic environment.

Related literature. The central finding of our paper is that the large-scale closure of a country's bank-branch network has substantial negative effects on firms' credit access and real economic activity. This is consistent with the view that soft information is critical for business lending (Agarwal and Hauswald, 2010) and that branches continue to play an important role in collecting and processing it.

Several studies have examined the effect of branch closures on local credit supply in the context of individual branch closures—such as those induced by bank mergers, or by regulators in response to concerns about bank holding company performance (e.g., Ashcraft, 2005, Garmaise and Moskowitz, 2006, Nguyen, 2019, and Ranish, Stella and Zhang, 2024). By their nature, these branch closures tend to be isolated and done in locations where branch networks are dense.[3] While such empirical settings often enable credible causal identification, it is not clear that the effect estimates obtained from them are informative about the effects of large-

[3] Merger-related regulatory closures tend to be selected specifically because there are other nearby branches of the merged entity (e.g., Nguyen, 2019).

scale, permanent branch closures.

Consistent with this difference in settings, the previous literature on isolated closures typically finds that small-business lending declines following merger-induced branch closures, but that the real effects are relatively limited, whereas we find large and persistent negative effects on the real activity of existing borrowers as well as on new firm formation.[4] Our key contribution to the previous literature thus consists in studying branch closures in an empirical setting that allows us to estimate effects that are not only plausibly causal, but also relevant for understanding the consequences of the large-scale, permanent branch closures that are currently taking place across the developed world.

An additional benefit of our setting is that our rich firm-level data allow us to document granular results, such as heterogeneity in the credit-supply effects across firm types, how firms adjust their financial positions in response to branch closures, and details on how firms' real activity is affected by the credit-supply contractions induced by closures. Bonfim, Nogueira and Ongena (2020) use Portuguese data to examine how firms' loan conditions are affected when they switch bank following the closure of the nearest branch of their bank. They find that firms that change bank following branch closures obtain worse loan terms from the new lender than do firms that change bank at other times (i.e., voluntarily), even though the former have lower default rates. We do not examine the effect of branch closures on loan pricing, but the negative real effects that we document are consistent with Bonfim, Nogueira and Ongena's (2020) finding that it is costly to substitute to new credit sources when a branch closes.

Our findings have implications in several areas. To begin with, our results suggest that the rapid developments in information technology that have occurred over the past few decades have not made soft information and physical proximity between borrowers and lenders redundant (cf. Petersen and Rajan, 2002). This is not to say that technology does not matter—on the contrary, a growing literature documents that recent technological advances have had a profound impact on individual banks as well as the corporate loan market more broadly. For example, the increased availability of hard information, as well as improved tools for processing it (Liberti and Petersen, 2018), has allowed fintech lenders to enter the corporate loan market and compete with banks within certain market segments (Gopal and Schnabl, 2022). Our re-

[4]Quincy (2024) documents positive effects of the growth of branch networks in depression-era America. For different reasons, this is also a setting where the expected impact of closures may be larger.

sults suggest, however, that traditional banks with brick-and-mortar branches continue to fulfill an important role that fintechs and other non-bank lenders cannot yet. This may change in the future, for example through the use of artificial intelligence.

Several recent papers demonstrate that new technology in fact often serves as a complement to bank branches in the domain of business lending. For example, He et al. (2022) show that IT investments enhance bank branches' capacity for producing and transmitting soft information, which strengthens their ability to make business loans. Relatedly, D'Andrea, Pelosi and Sette (2023) find that the introduction of broadband internet in Italy enabled bank branches to become more efficient in terms of labor productivity and loan quality. In a historical context, Lin et al. (2021) document that the introduction of the telegraph—a major advance in information technology in the 19th century—led Chinese banks to substantially *expand* their branch networks, again highlighting the potential complementarity of bank branches and new information technology. Technology and branch networks thus have a complex relationship.

A second implication of our findings is that while the spread of digital banking in advanced economies generates large efficiency gains (e.g., Berger, 2003), it also has a cost in the form of worse credit access for some firms.[5] Accelerated growth in the fintech sector may compensate for lower credit supply from banks (Gopal and Schnabl, 2022), perhaps by using new information sources (Liberti and Petersen, 2018). Thirdly, 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).

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. Sections 4 and 5 present the empirical results on credit supply and real economic activity, respectively. Section 6 concludes.

[5] Whether the gains outweigh the costs in the aggregate is a question beyond the scope of this paper. Realistically, the cost savings of reduced branch networks are considerable.

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.

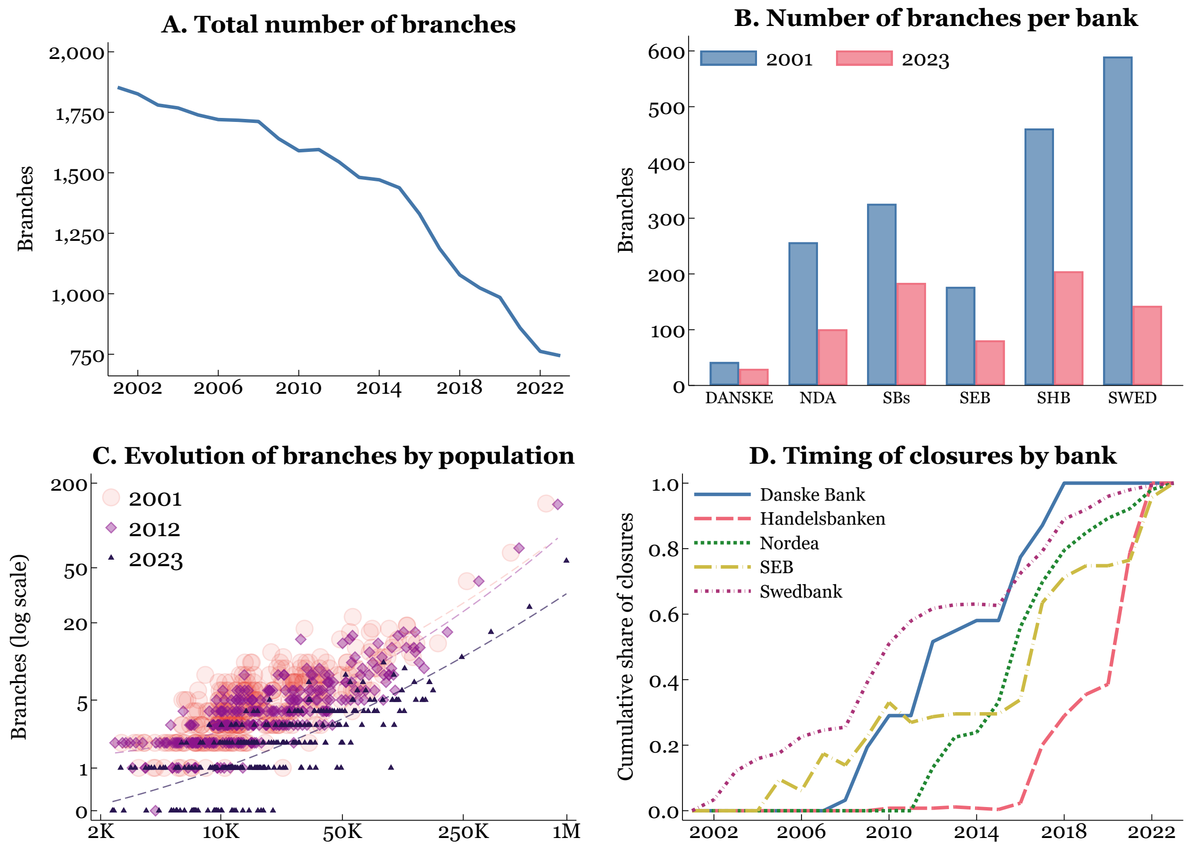
In 2023, at the end of our sample period, the market share of the four major banks has declined to around two thirds in terms of household lending, non-financial corporate loans, as well as branches. The decline in the market share of the four major banks is mainly due to the growth of two other banks. The first is the Danish bank Danske Bank, which entered the Swedish market by acquiring and growing an established but relatively small Swedish bank (Östgöta Enskilda Bank); as of 2023, Danske Bank is an important actor in both retail and corporate lending. The second is Länsförsäkringar Bank, which has grown organically over the 2000s and is especially large in retail loans; for corporate lending, Länsförsäkringar remains minor. 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.[6] 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 5.3 percent from 2009 and onwards. In

[6] All data sources are 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).

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, medium-sized and large municipalities alike, especially after 2012.

The background to the reduction in branch networks is new technology, which has drastically reduced the need for retail locations. For example, a 2022 Riksbank survey of households found that only 34 percent of respondents had used cash in the last 30 days (Sveriges Riksbank, 2022). Bank services apart from payments, such as financial advice, are also increasingly provided online. 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 reduction in the number of branch visits. When there is no longer any real need for a branch, it is time to close the doors for good"* (p. 4).

While the broad trend away from branches is driven by technology and affects all banks, the timing of the reductions in branch networks have been bank-specific.[7] This has meant that branch closures have been concentrated over a short time span for each bank and that the timing of the closures differs: the largest reduction in branches in a single year occurred in 2016 for Nordea (–20 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 beginning large-scale branch closures early, Handelsbanken late, and Nordea, SEB, 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. This heterogeneous timing combined with the different geography of the initial branch networks forms the basis of our shift-share identification strategy, which we describe in detail in the next section.

3 Empirical Framework: A Shift-Share IV Design

3.1 Data

Our 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

[7] Kundu, Muir and Zhang (2024) identify significant divergence across U.S. banks in the reliance on branch networks vs. online banking. We observe no such heterogeneity among large Swedish banks, perhaps reflecting underlying demographics.

that we create based on data from two different sources. One source is *Bankplatser i Sverige*, a print publication containing the address of every bank branch in Sweden, issued annually by the Swedish Bankers' Association until 2008. This publication was then 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. The other source is 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.[8]

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 (see Online Appendix B for additional details on the construction of the branch panel). The panel comprises branches belonging to Danske Bank, Handelsbanken, Nordea, SEB, Swedbank, and savings banks—that is, the main lenders on the Swedish corporate loan market during our sample period (see section 2).[9] 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

[8]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 confirm in Online Appendix B that the number of *betalningsförmedlingsplatser* per bank in the Pipos data correspond closely to the number of branches per bank reported in the publication *Bank and finance statistics* from the Swedish Bankers' Association (2024).

[9]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.

industry and location. We can thus link the Serrano data to the bank-branch data by means of the municipality code in each data set.

We complement the two main data sets with annual municipality-level data from Statistics Sweden and annual bank-level data from CapitalIQ.

3.2 Empirical model

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:

$$\Delta Y_{i,t+h} = \alpha_i^h + \gamma_t^h + \beta^h \cdot \Delta Branches_{j,t} + \xi^h \cdot \gamma_t \cdot s_{j,t} + \theta^{\mathbf{h}} \cdot \mathbf{X_{i,t}} + \varepsilon_{i,t}^h, \tag{1}$$

where the dependent variable is the change in outcome Y for firm i between years $t-1$ and $t+h$, and h denotes the estimation horizon. 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 is located. The baseline set of controls comprise firm fixed effects (α_i^h), year fixed effects (γ_t^h), a "sum of shares" control interacted with year fixed effects ($\gamma_t^h \cdot s_{j,t}$) that we define and motivate in section 3.4, and two lags of $\Delta Branches_{j,t}$ (collected in the vector $\mathbf{X_{i,t}}$) to ensure that any serial correlation in the shifts do not bias the estimates of interest (Jaeger, Ruist and Stuhler, 2018; Borusyak, Hull and Jaravel, 2025). We restrict the sample to incorporated firms with at least two full-time equivalent employees and one million SEK (approximately $100,000) in sales and net assets to make sure that we only include economically active enterprises in the estimations.[10]

We use three dependent variables to assess the effects of bank-branch closures on local credit supply. The first is $\Delta Loans_{i,t+h}$, the change in the stock of firm i's loans between years $t-1$ and $t+h$, where the change is measured as the symmetric growth rate and loans comprise all outstanding loans plus any undrawn credit-line commitments.[11] Note that the loan variable comprises loans from all sources—including, for example, fintech lenders—which implies that any substitution towards loans from non-bank lenders following branch closures are captured

[10] We also exclude financial firms and utilities (SNI/NACE codes 64–66 and 34–39, respectively), firms owned by central or local governments, and firms that are subsidiaries in foreign business groups, as well as firms that disappear from the sample for unknown reasons (i.e., without being deregistered as incorporated firms).

[11] 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.

by our estimates. We refer to the estimates from regressions with $\Delta Loans_{i,t+h}$ as dependent variable as the overall effects of branch closures, since $\Delta Loans_{i,t+h}$ captures both extensive and intensive margins effects. The second and third dependent variables are $LoanExit_{i,t+h}$ and $LoanEntry_{i,t+h}$, which capture the respective extensive margin responses of credit supply to branch closures.[12] The dependent variables used to assess the effects of branch closures on other financial and real outcomes will be described as we proceed with the analysis.

The coefficient of interest in equation (1) 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, including 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. We trace out the effects of branch closures over a nine-year window around the closures by estimating equation (1) for $h = -4, -3, -2, 0, 1, 2, 3, 4$ ($h = -1$ being the base period). The estimates for the negative horizons capture any difference in the trends of the outcome variable in the years before branch closures, which allows us to assess whether the pre-treatment trends in the outcome variables are parallel.

3.3 The identification problem

The empirical challenge we face is that estimating (1) by OLS may yield biased estimates of β^h, since $\Delta Branches_{j,t}$ is likely correlated with $\varepsilon^h_{i,t}$ due to the non-randomness of banks' choices about when and where to close branches. More specifically, since equilibrium loan volumes are the result of both loan demand and credit supply shocks, the model errors $\varepsilon^h_{i,t}$ will contain unobserved local loan demand shocks; if these are correlated with the growth rate of branches, OLS estimates of β^h will be biased.

The available empirical evidence on the determinants of branch closures does not give any clear indication of the direction in which any omitted variable bias is likely to go. One might surmise that banks close branches in areas where future loan demand is likely to be low, but Narayanan, Ratnadiwakara and Strahan (2025) show that lending in fact has small impact

[12] More specifically, $LoanExit_{i,t+h}$ is an indicator variable equal to one if the stock of firm i's loans is positive in year $t-1$ but not in year $t+h$, and zero if it is positive in both $t-1$ and $t+h$. $LoanEntry_{i,t+h}$, on the other hand, is an indicator variable equal to one if the stock of firm i's loans is zero in year $t-1$ but positive in year $t+h$, and zero if firm i doesn't have loans in either $t-1$ or $t+h$.

on U.S. banks' decisions about where to open and close branches. What matters is instead deposit franchise value: incumbent banks tend to close branches in locations with rate-sensitive depositors. These locations are usually economically vibrant, since rate-sensitive depositors are more educated and more exposed to the stock market than rate-insensitive depositors. This would imply that OLS estimates of the effects of branch closures are biased towards zero (if the true effect is negative), since closures are more likely to happen in vibrant areas where loan demand is presumably higher.[13]

Keil and Ongena (2024), on the other hand, document that banks that use more technology for internal purposes (collecting, processing, and storing information) are more prone to close branches. This type of selection would lead to omitted variable bias in OLS estimates if technology-intensive banks are more active in areas where loan demand is unusually high or low; whether this is the case is not immediately obvious.

Thus, while we can be reasonably sure that OLS estimates of the effects of branch closures suffer from multiple sources of omitted variable bias, we cannot confidently say whether the net effect of the different sources of bias is likely to be large or small, or even whether it is positive or negative. Estimating equation (1) by OLS would therefore not give us a reliable estimate of the true effect of branch closures on local credit supply, nor would it allow us to put a plausible upper or lower bound on the true effect.

3.4 A shift-share instrument

To address the identification problem, we instrument the change in bank branches in a municipality with the following shift-share instrument in the spirit of Bartik (1991):

$$Z_{j,t} = \sum_b \underbrace{\frac{Branches_{b,j,t-1}}{Branches_{j,t-1}}}_{\text{Shares } (s_{b,j,t})} \cdot \underbrace{\Delta Branches_{b,t}}_{\text{Shifts}}, \tag{2}$$

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

[13] This pattern may be weaker or absent in Sweden, where deposit collection is more independent of branches.

nationwide change in the number of branches of each bank (the shifts), where the former is predetermined at time t and the latter is—as we argue below—plausibly orthogonal to economic conditions in a given municipality-year.

We do not include savings banks in the construction of $Z_{i,t}$. The reason is that most 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. The shares going into the instrument do therefore not sum to one in municipalities where savings banks are present. Borusyak, Hull and Jaravel (2022) demonstrate that it is necessary to include a "sum of shares" control in such cases. In our setting, the sum of shares in question is the combined market share of the non-savings banks in municipality j in year $t-1$, $s_{j,t} = \sum_b s_{b,j,t}$, where $s_{b,j,t} \equiv \frac{Branches_{b,j,t-1}}{Branches_{j,t-1}}$. Moreover, in a panel setting like ours, the sum of shares control needs to be interacted with year fixed effects (Borusyak, Hull and Jaravel, 2022), which motivates the inclusion of the interaction term $s_{j,t} \cdot \gamma_t^h$ as a control in equation (1).[14]

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

$$\Delta Branches_{j,t} = \phi_i^h + \psi_t^h + \eta^h \cdot Z_{j,t} + \nu^h \cdot \psi_t \cdot s_{j,t} + \mu^{\mathbf{h}} \cdot \mathbf{X_{i,t}} + u_{i,t}, \tag{3}$$

where ϕ_i^h and ψ_t^h 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 (3) is the first stage and equation (1) is the second stage. Standard errors are clustered at the municipality-year level to account for the fact that the endogenous regressor and the instrument vary across but not within municipality-year cells.

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 is likely in our setting. For example, a branch closure could be 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 estimates should

[14]The mean sum of shares across all municipality-years in the sample is 0.79. The standard deviation is 0.30.

be interpreted as the effect of branch closures in municipalities where the change in the number of branches is affected by banks' nationwide closure decisions ("complier" municipalities).

3.5 Assessing the validity of the instrument

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 to what extent the local average treatment effects captured by our 2SLS estimates are representative of the causal effects of branch closures in the population of municipalities by means of Hull's (2025) method for characterizing compliers (external validity).

3.5.1 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.[15]

1. **Instrument independence:** $Z_{j,t}$ is not correlated with factors that affect the outcomes of interest—firms located in municipalities 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.
2. **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 firm 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.
3. **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.
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

[15]Notice that randomness in banks' decisions about which branches they target when undertaking branch closures is not among the assumptions required for internal validity. Non-randomness in this dimension does, however, matter for the *external* validity of the results, since it affects the effective population. We address this issue in section 3.5.2.

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 independence. The recent econometrics literature on shift-share instruments has clarified that it is sufficient for the validity of a shift-share instrument that either the shifts or the shares are exogeneous (Goldsmith-Pinkham, Sorkin and Swift, 2020; Borusyak, Hull and Jaravel, 2022). In our setting, it is most plausible to think of the shifts ($\Delta Branches_{b,t}$) as being exogeneous. The key identifying assumption is therefore that the banks' nationwide branch growth rates are uncorrelated with the market-share weighted average of the unobserved loan demand shocks in the municipalities in which they are present—that is, $\Delta Branches_{b,t}$ must be uncorrelated with an average of $\varepsilon_{j,t}$ taken across municipalities j with weights $s_{b,j,t}$, where $\varepsilon_{j,t}$ are the unobserved loan demand shocks in municipality j in year t.

The most plausible potential violation of this assumption would be that banks (i) can forecast local loan demand accurately and (ii) base their branch closure decisions predominantly on these forecasts. If so, the banks' nationwide branch growth rates $\Delta Branches_{b,t}$ would be correlated with changes in local loan demand and the instrument would consequently fail to satisfy the exogeneity condition. There are, however, several reasons for thinking that this is in fact not how branch closures are determined. First, there is, as discussed above, little in the empirical literature on bank branches to suggest that lending opportunities are an important determinant of closures. Second, the anecdotal evidence presented in section 2.2 indicate that the large-scale branch closures that have taken place in Sweden during our sample period are mainly driven by technological factors and declining cash use—in line with Keil and Ongena (2024)—and that the differences in the timing of the banks' large-scale closures are not due to differential developments in the local markets that they operate in. If so, our instrument is plausibly orthogonal to unobserved local loan demand shocks.

To corroborate the instrument-independence assumption, we compare 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.[16] The results are reported in Table 1. Panel A demonstrates that

[16]We assess the magnitude of the differences in the covariates using the normalized difference in means, a com-

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 0.08.[17] Panel B shows that the same holds true for municipalities—the largest normalized difference across the seven municipality-level characteristics under consideration is 0.33, which implies that municipality-years with high exposure to banks closing branches nationwide are similar to municipality-years with low exposure.

The covariate balance tests in Table 1 thus provide strong evidence in favor of the instrument-independence assumption. The other key piece of evidence in support of the independence assumption—to be presented along with the main results in sections 4 and 5—is that the pre-treatment trends are parallel for all outcome variables in the analysis.

Exclusion restriction. The exclusion restriction fails if the exposure of a municipality to a bank that closes branches nationwide affects credit supply to firms in the municipality for reasons other than the branch closures. The most plausible violation of the exclusion restriction would be that branch closures are driven by factors that also affect credit supply independently of the closures. Suppose, for example, that poor profitability causes banks to both close branches and to reduce lending across the country. If so, our instrument would be correlated with the lending cut driven by the poor profitability, because a nationwide credit-supply contraction of a bank is on average felt more in municipalities in which the bank has a larger market share. Our estimates of the effects of branch closures would then also capture the effects of the profitability-driven lending cut.

To assess whether branch closures are likely to be correlated with bank shocks that affect credit supply, we test for covariate balance across observable shift-level variables (Borusyak, Hull and Jaravel, 2025). Shift-level variables are measures of bank characteristics at the level of municipalities, constructed in the same way as the instrument—that is, by taking the weighted

parison metric proposed by Imbens and Rubin (2015) that measures the difference in means expressed in terms of standard deviations. The benefit of using normalized differences instead of t-tests is that the normalized difference is scale-free, in the sense that the likelihood of rejecting similarity does not increase mechanically with sample size.

[17]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 strong covariate balance.

Table 1: Covariate balance across firm, municipality. and bank characteristics

	$Z_{i,t} < 0$			$Z_{i,t} \geq 0$			Normalized
	Mean	SD	N	Mean	SD	N	difference
A. Firm characteristics							
Assets (MSEK)	11.19	20.23	293,382	11.64	21.84	293,059	–0.02
Sales (MSEK)	17.68	27.70	293,382	18.64	30.44	293,059	–0.03
Number of employees	11.78	14.50	293,382	11.06	13.65	293,059	0.05
Liabilities/Assets	0.73	0.20	293,328	0.75	0.19	293,055	–0.08
EBIT/Assets	0.09	0.15	293,353	0.08	0.13	293,006	0.03
Cash/Assets	0.14	0.16	293,373	0.13	0.15	293,048	0.07
Receivable days	40.28	35.61	283,603	42.92	37.89	281,481	–0.07
Payable days	36.88	33.66	293,382	38.29	34.19	293,059	–0.04
B. Municipality characteristics							
Log population (1000s)	9.90	0.98	2,021	9.78	0.91	2,619	0.12
Five-year population growth (%)	1.50	4.30	2,021	0.13	3.97	2,619	0.33
Population per square km	168	573	2,021	117	407	2,619	0.10
Elderly population share	0.22	0.04	2,021	0.21	0.04	2,619	0.16
Employment ratio (ages 20–74)	0.68	0.04	2,021	0.68	0.04	2,618	0.15
Relative labor income	0.95	0.13	2,021	0.94	0.11	2,619	0.08
Manufacturing share of employment	0.26	0.16	2,021	0.30	0.16	2,619	–0.23
C. Municipality-level bank characteristics							
Return on equity (%)	11.14	4.67	2,021	13.50	4.45	2,539	–0.52
Total assets (SEK billion)	2,285	585	2,021	2,224	867	2,539	0.08
Asset growth (%)	–1.10	9.18	2,021	3.91	12.32	2,539	–0.46
Loans/Deposits	2.00	0.38	2,021	1.94	0.73	2,539	0.10
Common equity/Assets (%)	5.03	0.54	2,021	4.14	1.14	2,539	0.99
Net interest income/Loans (%)	1.58	0.16	2,021	1.54	0.43	2,539	0.12
Credit losses (basis points)	19.3	32.4	2,021	7.4	9.8	2,539	0.50

This table compares firm-years (Panel A) and municipality-years (Panels B and C) with negative and non-negative values, respectively, of the instrument $Z_{j,t}$ across a set of firm, municipality, and bank covariates. Relative labor income is the ratio of the average labor income in a municipality to the national average, while the elderly population share is the share of people 65 years or older in a municipality's population. All other variables are self-explanatory. Nominal values are converted to 2012 SEK by means of the GDP deflator. The normalized difference in means is 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.

average of bank characteristic $x_{b,t}$ across the banks that are present in a municipality:

$$\bar{x}_{j,t} = \sum_{b} \frac{Branches_{b,j,t-1}}{Branches_{j,t-1}} \cdot x_{b,t}. \tag{4}$$

We conduct the covariate balance test by comparing municipalities with negative and non-negative values of the instrument across seven municipality-level bank variables: return on equity, total assets, asset growth, and the ratios of loans to deposits, common equity to assets, net interest income to outstanding loans, and credit losses to outstanding loans. The results, reported in Panel C of Table 1, show that the normalized differences in bank characteristics in some cases are economically meaningful.[18] These differences are, however, primarily due to time trends common to all banks. They are therefore not a concern for our empirical approach, since our regressions include year fixed effects that absorb any time trends and year-specific shocks common to all banks.

What we need to be concerned about is rather *within-year* differences across banks in terms of characteristics that are correlated with branch closures and may affect their lending behavior independently of the closures. Reassuringly, the economically significant differences in bank characteristics all but vanish when we cross-sectionally demean the data (see Table A1 in Online Appendix A). More specifically, the largest difference after demeaning is in terms of the ratio of common equity to total assets, which is 4.8 percent in municipality-years with negative values of the instrument and 4.3 percent in municipality-years with non-negative values. Moreover, we show below that including the seven municipality-level bank characteristics as controls in our regressions do not meaningfully change the coefficients of interest. Taken together, these findings suggest that the exclusion restriction is likely satisfied.

Instrument strength. The strength of the instrument can be assessed with the first-stage F-statistic. When estimating the first-stage regression (3) with the baseline set of control variables, we obtain a robust first-stage F-statistic of 62.1. This is well above any reasonable weak-instrument threshold and thus demonstrates that our instrument is strong. The strength of the instrument confirms that bank-branch closures in Sweden during our sample period to a large extent are driven by bank-specific closure waves decided on centrally by the respective

[18]We cannot compute the municipality-level bank characteristics for municipality-years in which only savings banks are present. The number of observations is therefore slightly lower in panel C than in panel B.

banks' headquarters.

Monotonicity. The 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 large bank consistently 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 (3) 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 1. The resulting first-stage estimates, reported in Table A2 in Online Appendix A, are consistently positive and large in magnitude, which supports the monotonicity assumption.

3.5.2 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 characterize how the effective population (the compliers) differs from the overall population in various dimensions by means of Hull's (2025) "one weird trick."

To begin with, note that the coefficient of interest in a 2SLS specification—under the standard IV assumptions and certain regularity conditions—can be expressed as a convexly weighted average of heterogeneous treatment effects:

$$\beta = E\left[\int \omega_i(x)\frac{\partial y_i}{\partial x}(x)dx\right], \tag{5}$$

where $\frac{\partial y_{i,t}}{\partial x}$ denotes the marginal effect of observation i at $x_i = x$ and $\omega_i(x)$ is a non-negative weight, such that $E\left[\int \omega_{i,t}(x)dx\right] = 1$. Hull's (2025) trick consists in replacing the outcome in such a 2SLS specification with the interaction of the treatment variable and some characteristic of interest c_i. The estimated treatment effect is then equivalent to the statistic

$$\tau = E\left[\int \omega_i(x)c_i dx\right], \tag{6}$$

i.e., the weighted average of c_i across observations i, where the weights are the same as those in (5). By comparing τ with the unweighted sample average (μ), we thus get a sense of how the weight of each observation i in the estimate of β varies with characteristic c: if τ is larger than μ, then observations with larger values of c on average have higher weight in the estimate of β, and vice versa.

We characterize the effective population by computing the τ and the μ (as well as the ratio between the two) in terms of the same seven municipality characteristics that we used for the covariate balance check in Table 1. The results, reported in Table 2, show that the effective population is very similar to the overall population in terms of almost all municipality characteristics: for six out of seven characteristics, the ratio τ/μ falls in the narrow range 0.93–1.03. For the seventh, the five-year population growth rate, the ratio is 1.48, which indicates that municipalities with high population growth receive slightly higher weight in our 2SLS estimates of the effects of branch closures. The overall picture that emerges from Table 2 is nevertheless that the external validity of the estimates is likely to be high.

4 The Effect of Bank Branch Closures on Local Credit Supply

This section presents the results on the credit-supply effects of branch closures. We quantify the economic magnitude of the effect estimates by reporting scaled coefficients that correspond to the effects of closing down 30 percent of the branches in a municipality, which is approximately the average branch growth rate in the municipality-years in which branch closures take place.

Table 2: Characterizing the effective population

	Weighted average (τ)	Sample average (μ)	τ/μ
Log population (1000s)	10.84	10.94	0.99
Five-year population growth (%)	4.48	3.02	1.48
Population density	639	621	1.03
Elderly population share	0.19	0.19	0.99
Employment ratio (ages 20–74)	0.69	0.68	1.01
Relative labor income	1.01	0.99	1.03
Manufacturing share	0.21	0.22	0.93

The τ are the treatment coefficients obtained when we estimate our 2SLS specification with the interaction of the treatment variable and the municipality characteristic in the leftmost column ($\Delta Branches_{j,t} \cdot c_i$) as dependent variable. The μ are the unweighted sample averages of the municipality characteristics. Relative labor income is the ratio of the average labor income in a municipality to the national average, while the elderly population share is the share of people 65 years or older in a municipality's population. All other variables are self-explanatory.

4.1 Baseline estimates

The baseline estimates of the effect of bank branch closures on the supply of credit to local firms are reported in Table 3. The first column shows the overall effect of closures on credit supply: the coefficient estimate is statistically significant and implies that closing 30 percent of the bank branches in a municipality on average causes a decline in the loan balances of local firms by 12.2 percent over a four-year period. The closing of bank branches thus has an economically important negative impact on local firms' access to credit.

The estimate of the overall effect reported in column (1) captures both the extensive and the intensive margins of the credit-supply response to branch closures. In the second and third columns, we consider the extensive margin separately by reporting the effect of branch closures on loan exit and entry. To begin with, the estimate in column (2) shows that branch closures significantly increase the probability of loan exit, i.e., the probability that a firm loses access to loans altogether. The magnitude of the estimate implies that the probability of loan exit for local firms increases by 2.8 percentage points over a four-year period following the closure of 30 percent of the bank branches in a municipality.

The corresponding estimate for the entry margin, reported in column (3), is close to zero

Table 3: The effect of bank branch closures on local credit supply

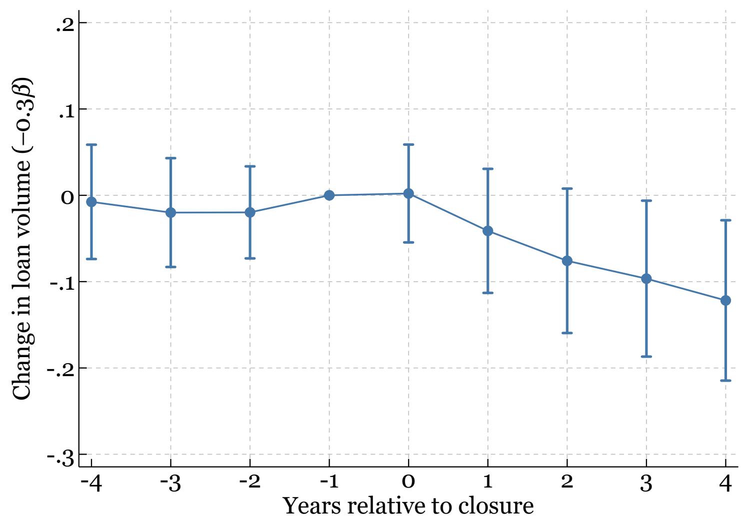
	(1) Overall effect	(2) Loan exit	(3) Loan entry
$\Delta Branches_{j,t}$	0.406**	–0.093**	–0.004
	(0.158)	(0.044)	(0.033)
Scaled effect $(-0.3 \cdot \hat{\beta})$	–0.122	0.028	0.001
Weak IV statistic	54.9	52.7	68.3
Number of observations	586,441	531,499	278,900
Number of firms	66,247	62,424	45,037

This table reports the baseline two-stage least squares estimates of the effect of bank branch closures on credit supply to local firms over a four-year period ($h = 4$). The dependent variable is $\Delta Loans_{i,t+4}$ in column (1), $LoanExit_{i,t+4}$ in column (2), and $LoanEntry_{i,t+4}$ in column (3). Standard errors clustered at the municipality-year level are reported in parentheses. The weak IV statistic is the Kleibergen-Paap rk Wald F-statistic. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

and statistically insignificant. Hence, increased loan exit rates contribute to the overall effect of branch closures on credit supply, but decreased loan entry rates do not. Note, however, that our loan entry variable only captures new lending to already existing firms. The estimate reported in column (3) does therefore not capture the effect that branch closures may have on banks' willingness to provide loans to entrants, and should thus be considered a lower bound on the effect of branch closures on loan entry.

In Figure 3, we assess how the overall effect of branch closures on credit supply evolves over time by plotting the scaled 2SLS estimate of β^h for each estimation horizon h. Lending to local firms starts to decline immediately following closures and then continues down throughout the four-year estimation period, but the effect only becomes significant after around 2–3 years. That the credit-supply effect of branch closures thus evolves gradually over time is expected, for at least two reasons. First, the average remaining maturity of outstanding corporate loans is typically several years. This creates a natural delay in the credit-supply response to branch closures, since loans cannot be revoked prior to maturity unless the borrower breaches a covenant. Second, the soft information that a bank has collected about its existing local borrowers likely

Figure 3: The dynamics of the credit-supply effect of branch closures



This figure plots how the overall effect of bank branch closures on local credit supply evolves over time. The circles correspond to the two-stage least squares estimates of β^h obtained from the estimation of equations (1) and (3) with $\Delta Loans_{i,t+h}$ as outcome variable and for horizons $h = -4, ..., 4$. The estimates are scaled to correspond to the effect of closing 30 percent of the bank branches in a municipality. Standard errors are clustered at the municipality-year level and the vertical bars represent 95-percent confidence intervals. The estimation sample in the pre-treatment periods ($h \leq -2$) is restricted to firms that also appear in the post-treatment period ($h \geq 0$).

remains relevant for some time after the closure of its local branches. If so, the bank's ability to make informed local lending decisions deteriorates gradually in the years after a branch closure, which may explain why the credit-supply response to branch closures also evolves gradually.

Importantly, the estimates for the pre-treatment periods are all close to zero and statistically insignificant, which shows that firms more and less exposed to branch closures follow parallel pre-treatment trends in terms of loan outcomes.

4.2 Margins of adjustment to the credit-supply contraction

A firm that experiences a contractionary credit-supply shock has several potential margins of adjustment that it can use to counter the shock. In this section, we examine to what extent firms use the following four adjustment margins to curb the consequences of branch closures: cash holdings, downstream trade credit (accounts receivable), upstream trade credit (accounts payable), and equity. We do so by estimating the baseline model with the change in the following ratios over a four-year period as dependent variables: cash over assets, receivables over sales (receivable days), payables over input expenditures (payable days), and equity over assets. More specifically, the dependent variable in the cash regression is defined as

$$\Delta Cash/Assets_{i,t+4} = Cash_{i,t+4}/Assets_{i,t+4} - Cash_{i,t-1}/Assets_{i,t-1}, \tag{7}$$

while the other three dependent variables are constructed analogously. The ratios of receivables to sales and payables to input costs are multiplied by 365 so that we can interpret the estimation results in terms of days. We also decompose the equity result by examining retained earnings and other equity separately; the dependent variables in these regressions are defined as in (7), i.e., as the three-year changes in retained earnings over assets and other equity over assets, respectively.

The estimation results are reported in Table 4. The effect of branch closures on cash holdings is precisely zero (column 1), which indicates that firms do not adjust their liquidity positions in response to the credit-supply contractions induced by branch closures. To understand this finding, note that the net effect of credit-supply contractions on cash holdings is ambiguous a priori. On the one hand, a firm may use its cash reserves as a substitute for bank credit, which would lead cash holdings to decline after branch closures. On the other hand, a firm that has its bank credit lines revoked may need to increase its cash holdings to maintain the desired size of its overall liquidity buffer (see Acharya et al., 2014, 2021, for analyses of how firms use cash and credit lines for liquidity management purposes). We conjecture that the zero effect on cash is the net effect of these countervailing forces.

We turn next to the response of trade credit to branch closures. The point estimates are close to zero for both receivable days (column 2) and payable days (column 3), which indicates that firms do not adjust their trade credit positions following branch closures. At first sight, this appears to contrast with the findings in the previous literature on the effects of liquidity and credit-supply disturbances. What most of the previous literature shows, however, is that firms use trade credit to handle liquidity shocks (e.g., Garcia-Appendini and Montoriol-Garriga, 2013, and Amberg et al., 2021), not permanent changes in the financing environment. To deviate from the standard payment periods in one's industry over longer periods is likely difficult, especially

Table 4: Margins of adjustment to the credit-supply contraction

	(1)	(2)	(3)	(4)	(5)	(6)
	Cash holdings	Receivable days	Payable days	Total equity	Retained earnings	Other equity
$\Delta Branches_{j,t}$	0.011	0.137	–0.013	0.017	0.047**	–0.027***
	(0.014)	(1.119)	(1.320)	(0.017)	(0.021)	(0.010)
Scaled effect ($-0.3 \cdot \hat{\beta}$)	–0.003	–0.041	0.004	–0.005	–0.014	0.008
Weak IV statistic	50.3	53.3	52.2	50.3	50.3	50.3
Number of obs.	430,871	314,251	274,143	430,871	430,871	430,871
Number of firms	54,619	46,754	44,213	54,619	54,619	54,619

This table reports the two-stage least squares estimates of the effect of bank branch closures on local firms' cash holdings, trade credit positions, and equity. The dependent variables are the respective changes between years $t-1$ and $t+4$ in the following ratios: cash to assets (column 1), receivables to sales times 365 (column 2), payables to input costs times 365 (column 3), equity to assets (column 4), retained earnings to assets (column 5), and other equity to assets (column 6). The estimation samples are restricted to firms that have a positive amount of loans outstanding in year $t-1$. Standard errors clustered at the municipality-year level are reported in parentheses. The weak IV statistic is the Kleibergen-Paap rk Wald F-statistic. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

for small firms without strong bargaining positions towards customers and suppliers.[19]

Finally, there is no statistically significant effect of branch closures on firms' total equity (column 4). The null effect on total equity masks underlying effects on the components of equity, however. Following branch closures, firms raise new equity capital, as evidenced by the statistically significant effect on "other equity" (column 6), which among other things comprise the proceeds from new share issues and owners' contributions. This is more than offset, however, by a slightly larger, statistically significant decrease in retained earnings (column 5), presumably due to a reduction in profits. The net effect of branch closures on equity is consequently close to zero and statistically insignificant.

In sum, the results reported in Table 4 suggest that firms are unable to offset the credit-supply contractions induced by branch closures by adjusting other financial positions.

[19]A caveat of the results on receivable and payable days is that accounting-based proxies for payable days and receivable days are well-known to suffer from measurement error and to give rise to many outliers. To mitigate the influence of such outliers, our estimations only include observations for which the absolute change in receivable days or payable days over the four-year estimation horizon is smaller than 20 (the mode payment period in business-to-business transactions in Sweden is 30 days). A fair amount of measurement error is nevertheless likely to remain.

4.3 Heterogeneity in the credit-supply effects of branch closures

The central hypothesis of this paper is that branches matter for banks' lending decisions, for example because they facilitate the collection of soft information about local firms. If this is the case, the credit-supply effects of branch closures should vary across firms with the importance of soft information. If the sensitivity of lending decisions to soft information depends on borrower characteristics, we can test this. In what follows, we undertake a cross-sectional heterogeneity analysis to assess this by estimating the baseline specification on various subsamples of firms—obtained by splitting the sample at given cutoffs of theoretically relevant firm characteristics—and testing whether the effect of branch closures on credit supply differs across the subsamples. We consider four firm characteristics—size, age, asset tangibility, and labor productivity—and form subsamples comprising firms in the top quartile and the bottom three quartiles, respectively. All split variables are measured as averages over the years $t-3$ to $t-1$. The lagging ensures that we do not split the sample based on measures possibly affected by the treatment, while the averaging reduces the influence of temporary shocks on the sorting.

Firm size. A large literature argues that small firms are more informationally opaque than large firms and that small-business lending therefore is particularly reliant on soft information (see, e.g., Petersen and Rajan, 1994; Berger and Udell, 2002; and Agarwal and Hauswald, 2010). A corollary of this is that the credit-supply effects of branch closures should be stronger for small firms than for large firms. To test whether this is the case, we sort firms based on sales and classify those in the top quartile of the distribution as large, and those in the bottom three quartiles as small. We then estimate the credit-supply effects of branch closures separately for the two groups.

The results of the sales-based heterogeneity tests, reported in Panel A of Table 5, show that branch closures have a large and statistically significant effect on credit supply to small firms—in terms of the overall response as well as the loan exit response—but no significant effect on large firms (the loan entry response is insignificant in both groups). The differences between the point estimates in the two groups are only statistically significant for the loan exit response. This shows that the negative effect of branch closures on credit supply is primarily a small-firm phenomenon.

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

	Overall effect		**Loan exit**		**Loan entry**	
	(1)	(2)	(3)	(4)	(5)	(6)
	$\hat{\beta}$	$se(\hat{\beta})$	$\hat{\beta}$	$se(\hat{\beta})$	$\hat{\beta}$	$se(\hat{\beta})$
A. Firm size (sales)						
Large ($\geq P_{75}$)	0.262	(0.214)	0.020	(0.057)	–0.027	(0.064)
Small ($< P_{75}$)	0.470***	(0.163)	–0.135***	(0.049)	0.002	(0.035)
Difference	–0.208	(0.204)	0.155**	(0.062)	–0.030	(0.070)
B. Firm size (assets)						
Large ($\geq P_{75}$)	0.269	(0.206)	0.003	(0.053)	–0.024	(0.060)
Small ($< P_{75}$)	0.454***	(0.163)	–0.146***	(0.050)	0.000	(0.036)
Difference	–0.184	(0.205)	0.149**	(0.061)	–0.025	(0.066)
C. Firm age						
Older than five years	0.394**	(0.155)	–0.101**	(0.046)	0.013	(0.035)
Five years or younger	0.345*	(0.179)	–0.172***	(0.063)	0.082	(0.066)
Difference	0.049	(0.227)	0.071	(0.073)	–0.070	(0.076)
D. Asset tangibility						
High tangibility ($\geq P_{75}$)	0.302*	(0.174)	0.015	(0.049)	–0.053	(0.086)
Low tangibility ($< P_{75}$)	0.348**	(0.172)	–0.101**	(0.047)	0.018	(0.034)
Difference	–0.046	(0.211)	0.116*	(0.061)	–0.071	(0.091)
E. Labor productivity						
High LP ($\geq P_{75}$)	0.109	(0.200)	0.041	(0.057)	–0.088	(0.060)
Low LP ($< P_{75}$)	0.483***	(0.175)	–0.129**	(0.051)	0.008	(0.039)
Difference	–0.374*	(0.218)	0.169**	(0.066)	–0.095	(0.071)

This table reports two-stage least squares estimates from estimations of equations (3) and (1) in various subsamples of the population. The dependent variable is $\Delta Loans_{i,t+4}$ in columns (1) and (2), $LoanExit_{i,t+4}$ in columns (3) and (4), and $LoanEntry_{i,t+4}$ in columns (5) and (6). The subsamples are constructed by splitting the sample at the 75th percentile (P_{75}) of the respective firm characteristics. Standard errors clustered at the municipality-year level are reported in parentheses. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

As a robustness check, we redo the size-based heterogeneity tests, but use assets instead of sales as size measure. The respective coefficient estimates, reported in Panel B, are quite similar to those from the sales-based tests; hence, the conclusion that small firms are more affected by branch closures than large firms is not sensitive to the exact choice of size measure.

Firm age. A related strand of the literature posits that informational asymmetries are also worse for young firms, since there is little hard information—in the form of, for example, established operational records and long credit histories—pertaining to them (see, e.g., Diamond, 1991; Petersen and Rajan, 1994; Black and Strahan, 2002; and Hadlock and Pierce, 2010). We therefore expect the credit-supply effect of branch closures to be particularly strong for young firms. To test this hypothesis, we split the sample into younger firms (five years or younger) and older firms (older than five years) and estimate the credit-supply effects of branch closures separately for the two groups.[20]

The results, reported in Panel C of Table 5, are not in line with expectations. The magnitude of the point estimate for the overall effects of branch closures is somewhat *larger* for old firms than for young firms, while the reverse is true for the loan exit response. The differences are fairly small in magnitude, though, and not statistically significant in either case. Hence, the results do not reveal any clear heterogeneity across younger and older firms. This finding is surprising given the importance attached to age in the previous literature on firms' access to credit. We conjecture that the distinction that primarily matters is entrants versus incumbents, and, as explained in section 4.1 above, our estimates do not capture the effect of branch closures on banks' willingness to provide loans to entrants. We will present empirical evidence corroborating this conjecture in section 5.2 below.

Asset tangibility. Firms with more tangible assets are better able to pledge collateral when borrowing (see, e.g., Almeida and Campello, 2007). They should therefore be less sensitive to bank-branch closures, because the importance of soft information declines as loans become bet-

[20]The reason for not splitting the sample at the 25th or 75th percentile for the age-based heterogeneity tests is that this would result in a cutoff that is arguably too high. The 25th percentile of the sample age distribution is eight years, which is a rather mature age in this context: an eight-year old firm does have a fairly long credit history and an established operational record and need therefore not be especially informationally opaque. One may argue that even five years is too high in this regard, but the lower we set the cutoff, the more statistical power we lose (only 15 percent of sample firms are five years or younger). Five years seems an appropriate compromise given this trade-off.

ter collateralized. All tangible assets do not constitute high-quality collateral, however. Canales and Nanda (2012) argue, for example, that specialized machinery and equipment often have low liquidation value for banks; consistent with this idea, Degryse et al. (2025) document that it is far more common for borrowers of European banks to pledge real estate than movable physical assets as collateral. We therefore focus on real estate and land when testing whether the credit-supply effects of branch closures vary across firms more and less able to pledge collateral.

We sort firms based on the ratio of real estate and land to net assets—classifying firms in the bottom three quartiles as low-tangibility firms and firms in the top quartile as high-tangibility firms—and report the coefficient estimates for the respective groups in Panel D of Table 5. The point estimate for the overall effects of branch closures is somewhat larger for low-tangibility firms than for high-tangibility firms (0.35 versus 0.30), but the difference is not statistically significant. In terms of loan exit, however, the response is large and statistically significant for low-tangibility firms but small and insignificant for high-tangibility firms, and the difference between the two is itself statistically significant at the ten percent level. This indicates that low-tangibility firms are more likely to lose access to bank loans altogether following branch closures, but that banks do not reduce the size of loans to the low-tangibility firms that still obtain loans after branch closures. In sum, soft information collected via local branches appears particularly important when pledgeable assets are scarce.

Labor productivity. Finally, we test for heterogeneity in the credit-supply effects across more and less productive firms, measured in terms of labor productivity (sales per employee). Productive firms may be less affected by a supply contractions simply because they have more resources, so can afford to pay more, or can find alternative funding sources. Bank branch closures should, consequently, affect highly productive firms' access to credit relatively less. Furthermore, the aggregate welfare effects of a credit supply contraction depends on such heterogeneity: if productive firms are more affected, the aggregate welfare effects are larger, if less affected, effects are smaller.

We classify firms in the top quartile of the labor-productivity distribution as high-productivity firms and those in the bottom three quartiles as low-productivity firms, and report the coefficient estimates for the respective groups in Panel E. The credit-supply effects of branch closures are large and statistically significant for low-productivity firms—in terms of the overall response as well as the loan exit response—but small and statistically insignificant for high-productivity firms. The difference between the effect estimates in the two groups is statistically significant for the exit response, but not for the overall effect (as before, the loan entry response is insignificant in both groups). High productivity thus insulates firms from the adverse credit-supply effects of branch closures.

4.4 Robustness checks

We assess the robustness of the baseline credit-supply results by estimating various alternative model specifications. The results are reported in Table 6, which also includes the baseline estimates for comparison (row A). For brevity, we only report results for the four-year estimation horizon ($h = 4$).

First, we estimate the baseline specification with a lagged-shares instrument to assess whether endogenously evolving market shares during the sample period may bias the baseline estimates. This alternative instrument uses the $t-3$ market shares of the banks and is consequently defined as:

$$Z_{j,t}^{LaggedShares} = \sum_{b} \frac{Branches_{b,j,t-3}}{Branches_{j,t-3}} \cdot \Delta Branches_{b,t}, \tag{8}$$

where $Branches_{b,j,t-3}/Branches_{j,t-3}$ is bank b's share in the total number of bank branches in municipality j in year $t-3$, 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.[21] The effect estimates obtained when instrumenting branch closures with the lagged-shares instrument, reported in row B, are statistically significant and around one third larger in magnitude than the baseline estimates, which shows that the baseline effects are not driven by endogenously evolving market shares.

Second, we estimate the baseline specification with a leave-one-out instrument to assess whether the own-observation information going into the instrument may bias the baseline estimates. More specifically, the leave-one-out instrument is defined as:

$$Z_{j,t}^{LeaveOneOut} = \sum_{b} \frac{Branches_{b,j,t-1}}{Branches_{j,t-1}} \cdot \Delta Branches_{b,t,-j}, \tag{9}$$

[21] The sum of shares control in the estimations with $Z_{j,t}^{LaggedShares}$ as instrument also uses the $t-3$ market shares.

Table 6: Specification checks for baseline credit-supply estimates

	(1) Overall effect	(2) Loan exit	(3) Loan entry
A. Baseline specification	0.406**	–0.093**	–0.004
	(0.158)	(0.044)	(0.033)
B. Instrumenting with $Z_{j,t}^{LaggedShares}$	0.543***	–0.131**	–0.004
	(0.191)	(0.055)	(0.038)
C. Instrumenting with $Z_{j,t}^{LeaveOneOut}$	0.545**	–0.130**	–0.023
	(0.226)	(0.063)	(0.052)
D. Dropping if $Branches_{j,t-1} \leq 1$	0.373**	–0.086**	0.001
	(0.150)	(0.042)	(0.032)
E. Including municipality-level controls	0.375***	–0.081**	–0.005
	(0.145)	(0.040)	(0.033)
F. Including firm-level controls	0.299**	–0.105**	0.004
	(0.137)	(0.045)	(0.033)
G. Including municipality-level bank controls	0.361**	–0.084*	0.020
	(0.169)	(0.047)	(0.035)

This table reports two-stage least squares estimates of the effect of bank branch closures on credit supply to local firms for several alternative model specifications. The dependent variable is $\Delta Loans_{i,t+4}$ in column (1), $LoanExit_{i,t+4}$ in column (2), and $LoanEntry_{i,t+4}$ in column (3). Standard errors clustered at the municipality-year level are reported in parentheses. The weak IV statistic is the Kleibergen-Paap rk Wald F-statistic. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

where $\Delta Branches_{b,t,-j}$ is the percent change in the number of bank branches for bank b across all municipalities except j between years $t-1$ and t. The effect estimates we obtain when using the leave-one-out instrument are quite similar to the ones we obtain when using the lagged-shares instrument: the overall effect and the exit response are statistically significant and around one third larger than the baseline estimates. The baseline estimates are thus not driven by the own-observation information going into the construction of the instrument.

Third, we estimate the baseline regressions excluding municipality-years with fewer than two branches in year $t-1$. The resulting effect estimates—reported in row C of Table 6—are quite similar to the baseline estimates, which demonstrates that our findings are not primarily driven by municipalities with very few branches.

Fourth, we augment the baseline specification with seven municipality-level control variables: log population size, the five-year population growth rate, population density, the share of inhabitants that are 65 years or older, the employment ratio, average labor income (measured relative to the national average), and the manufacturing share of employment, all measured as of year $t-1$. The resulting estimates, reported in the row D, are close to the baseline estimates.

Fifth, we estimate the baseline specification with seven firm-level control variables: log assets, log sales, log employment, debt-to-assets, cash-to-assets, EBIT-to-assets, payable days, and receivable days (all measured as of year $t-1$), as well as an indicator variable for whether the firm is five years or younger at time t. The coefficient estimate for the overall effect decreases somewhat while the estimate for loan exit increases slightly when the firm-level controls are included (row E); both estimates remain statistically significant. The reason for not including the firm-level variables as controls in the baseline specification is that doing so risks generating Nickell (1981) bias in the estimates. It is nevertheless reassuring to see that our results are robust to the inclusion of a broad set of firm-level controls.

Finally, we include seven municipality-level bank variables as controls: return on equity, log total assets, asset growth, the loans-to-deposits ratio, common equity to total assets, net interest income over outstanding loans, and credit losses as a share of outstanding loans (details on the construction of these variables are provided in section 3.5.1). The coefficient estimates, reported in row F, are again similar to the baseline estimates, although the loan-exit estimate is only statistically significant at the ten-percent level in this specification. Taken together, the robustness of the results to the inclusion of a broad set of firm, municipality, and bank controls corroborate the instrument independence and exclusion restriction assumptions.

4.5 Comparing the 2SLS estimates with the OLS estimates

Our final robustness exercise is to compare 2SLS estimates with the corresponding OLS estimates to get a better sense of how the instrument works. To this end, Table 7 provides more detailed IV diagnostics in the form of estimation results for the first-stage regression (first col-

Table 7: OLS and 2SLS estimates of the credit-supply effects

		Dependent variable: $\Delta Loans_{i,t+3}$		
	First stage	Reduced form	2SLS	OLS
$Z_{j,t}$	1.332***	0.541***		
	(0.180)	(0.203)		
$\Delta Branches_{j,t}$			0.406**	0.049**
			(0.158)	(0.021)
Number of observations	586,441	586,441	586,441	586,441
Number of firms	66,247	66,247	66,247	66,247

The reported coefficients correspond to the first-stage, reduced form, two-stage least squares, and OLS estimates, respectively, from estimations with $\Delta Loans_{i,t+4}$ as dependent variable. More specifically, the first-stage coefficient is obtained from OLS estimation of equation (3); the 2SLS coefficient from the two-stage least squares estimation of equations (3) and (1); the OLS coefficient from OLS estimation of equation (1); 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.

umn), the reduced form regression in which the dependent variable is regressed directly on $Z_{j,t}$ and the controls (second column), the baseline 2SLS specification (third column), and the OLS regression of the dependent variable on the endogenous regressor, $\Delta Branches_{j,t}$, and the controls (fourth column). We focus on the overall credit-supply effect over a four-year period ($h = 4$) in all regressions.

To begin with, the first-stage coefficient implies that a one percentage point decrease in predicted branch growth (the instrument) is associated with a 1.33 percentage point decrease in actual branch growth (the reduced form estimate is given by the product of the first-stage estimate and the 2SLS estimate), and is statistically significant. The OLS estimate is also statistically significant, but substantially smaller in magnitude than the 2SLS estimate: 0.049 versus 0.406. What accounts for this difference? Provided that an instrument is valid, the difference between 2SLS and OLS estimates is due to some combination of omitted variable bias in the OLS estimate and heterogeneous treatment effects in the population (see, e.g., Dahl, Kostøl and Mogstad, 2014).

In our case, the substantial difference is likely due to a combination of omitted variable bias and heterogeneous treatment effects. On the one hand, if branch closures are more likely to occur in areas with highly educated and financially active households (as suggested by Narayanan, Ratnadiwakara and Strahan, 2025)—i.e., in economically vibrant areas where loan demand presumably grows at a faster pace than elsewhere—the bias in OLS regressions will be positive (towards zero). On the other hand, the branch closures captured by our instrument may be more consequential (than the average branch closure), because in a nationwide wave—the closures primarily captured by our instrument—closures include many of the larger and more active branches in a municipality, whereas branches closed at other times are likely to be less active (see Nguyen, 2019, for a similar argument). In this case, the bias is toward a larger effect. Both omitted variable bias and heterogeneous treatment effects provide plausible explanations for why the OLS estimates are smaller than the 2SLS estimates—we cannot quantify their relative importance.

5 The Effects of Branch Closures on Economic Activity

We now turn to the effects of bank branch closures on real activity. We quantify the economic magnitude of the effect estimates by reporting the effects of closing 30 percent of the branches in a municipality.

5.1 Effects on incumbent firms

We begin by examining the effects of branch closures on the real activity of firms that have bank loans outstanding and can be directly affected by closures. We estimate the baseline model with the symmetric growth rate of, in turn, sales, employment, fixed assets (property, plant, and equipment), and working capital (accounts receivable and inventory) as dependent variables for this sample of firms. We also test for effects on firms' exit risk by estimating the model with an indicator for exit as dependent variable.[22]

The results, reported in Panel A of Table 8, show that branch closures have significant negative effects on local firms' sales, working-capital investments, and survival. The magnitude

[22] The indicator variable, $FirmExit_{i,t+h}$, is equal to one if firm i reports positive sales in year $t-1$ but not in year $t+h$, and zero if firm i reports positive sales in both $t-1$ and $t+h$.

Table 8: Real effects of branch closures on local firms

	(1)	(2)	(3)	(4)	(5)
	Sales	Employment	Fixed assets	Working capital	Firm exit
A. All firms with outstanding bank loans					
$\Delta Branches_{j,t}$	0.155**	0.072	0.083	0.142**	–0.025**
	(0.075)	(0.057)	(0.090)	(0.069)	(0.011)
Scaled effect ($-0.3 \cdot \hat{\beta}$)	–0.046	–0.022	–0.025	–0.043	0.008
Weak IV statistic	50.5	50.5	49.8	50.5	47.1
Number of observations	442,642	442,644	435,552	442,585	405,226
B. Firms with credit lines					
$\Delta Branches_{j,t}$	0.222***	0.144**	0.104	0.151**	–0.042***
	(0.083)	(0.067)	(0.117)	(0.077)	(0.015)
Scaled effect ($-0.3 \cdot \hat{\beta}$)	–0.067	–0.043	–0.031	–0.045	0.013
Weak IV statistic	46.6	46.6	45.8	46.7	43.4
Number of observations	308,651	308,651	303,155	308,624	289,003
C. Firms with other loans					
$\Delta Branches_{j,t}$	0.129*	0.051	0.212**	0.091	–0.014
	(0.073)	(0.055)	(0.095)	(0.069)	(0.011)
Scaled effect ($-0.3 \cdot \hat{\beta}$)	–0.039	–0.015	–0.064	–0.027	0.004
Weak IV statistic	47.7	47.7	47.2	47.8	44.3
Number of observations	383,372	383,374	379,263	383,323	350,540

This table reports the two-stage least squares estimates of the effect of bank branch closures on local firms' sales, employment, fixed assets, working capital, and exit probability. The dependent variables are the symmetric growth rates of the respective variables between years $t-1$ and $t+4$ in columns 1-4 and the firm exit indicator, $FirmExit_{i,t+4}$, in column 5. The estimation sample is restricted to firms that have a positive amount of loans outstanding in year $t-1$ in all panels; in Panel B, we further restrict the sample to firms that had a credit line outstanding in year $t-1$, and in Panel C to firms that had some other type of loan outstanding in year $t-1$. Standard errors clustered at the municipality-year level are reported in parentheses. The weak IV statistic is the Kleibergen-Paap rk Wald F-statistic. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

of the estimates imply that the closure of 30 percent of the bank branches in a municipality causes local firms to experience a 4.6 percent decline in sales, a 4.3 percent decline in the stock of working capital, and a 0.8 percentage points increase in exit probability. Hence, the credit-supply contractions induced by branch closures may become severe enough to drive some firms out of the market altogether (though the magnitude of the effect is fairly small). The effect estimates for employment and fixed assets, on the other hand, have the expected sign but are statistically insignificant.

That there is no significant effect on employment and fixed investment may seem surprising, but the full sample estimates mask some heterogeneity. Firms use loans for different purposes: credit lines are mainly used for working-capital financing and as insurance against liquidity shocks (see, e.g., Kashyap, Rajan and Stein, 2002; Campello et al., 2011; Brown, Gustafson and Ivanov, 2021; Berrospide and Meisenzahl, 2022; and Chodorow-Reich et al., 2022), whereas term loans, on the other hand, are predominantly used for fixed investments. We expect different real effects of branch closures depending on the types of loans that firms use: firms reliant on credit lines should be more affected in terms of employment and working capital, whereas firms reliant on term loans should be more affected in terms of fixed investments.

To test whether this is the case, we split the sample into groups of firms that have credit lines and other loans, respectively, where the latter category predominantly consists of term loans but also includes, for example, financial leasing.[23] We then estimate the real effects of branch closures separately for these two samples (note that the samples overlap). The results, reported in Panels B and C of Table 8, show that the real effects of branch closures indeed vary depending on the type of loan held by a firm: following a 30-percent branch closure, firms with credit lines experience reductions in sales, employment, and working capital of 6.7, 4.3, and 4.8 percent, respectively. Interestingly, their exit risk also increases more markedly, perhaps because they are more vulnerable liquidity shocks. For firms holding term loans and other loans, on the other hand, the effects are the reverse: a significant decline in fixed assets of 6.4 percent, but no significant changes in employment, working capital, or exit risk. Naturally enough, then, branch closures affect the production inputs that firms finance with bank loans.

[23]This is the finest possible decomposition of a firm's outstanding credit in our data. Some firms of course use both credit lines and term loans; these will appear in both subsamples.

In Figure 4, we show how the real effects of branch closures evolve over time by plotting the 2SLS estimates of the respective scaled β^h for each estimation horizon h (the estimation sample is the same as in Panel A of Table 8: all firms with outstanding bank loans). The real effects in terms of sales and working capital investments—where we observe significant effects of branch closures in the full sample—track the credit-supply response closely over time: both start to decline immediately following closures, but it takes 2–3 years before the effects become statistically significant. Moreover, the estimates for the pre-treatment periods are statistically insignificant for all outcomes, which shows that firms more and less exposed to branch closures follow parallel pre-treatment trends also in terms of their real activity.

5.2 Effects on the entry and exit of firms

Our final empirical exercise is to study the effect of branch closures on business dynamism—i.e., how closures affect the entry and exit of firms, and thereby the number of firms in the economy. We do so using the municipality-level analog of our baseline 2SLS model, which comprises the following two equations:

$$\Delta Branches_{j,t} = \phi_j^h + \psi_t^h + \eta^h \cdot Z_{j,t} + \nu^h \cdot \psi_t \cdot s_{j,t} + \mu^{\mathbf{h}} \cdot \mathbf{X_{j,t}} + u_{j,t}, \tag{10}$$

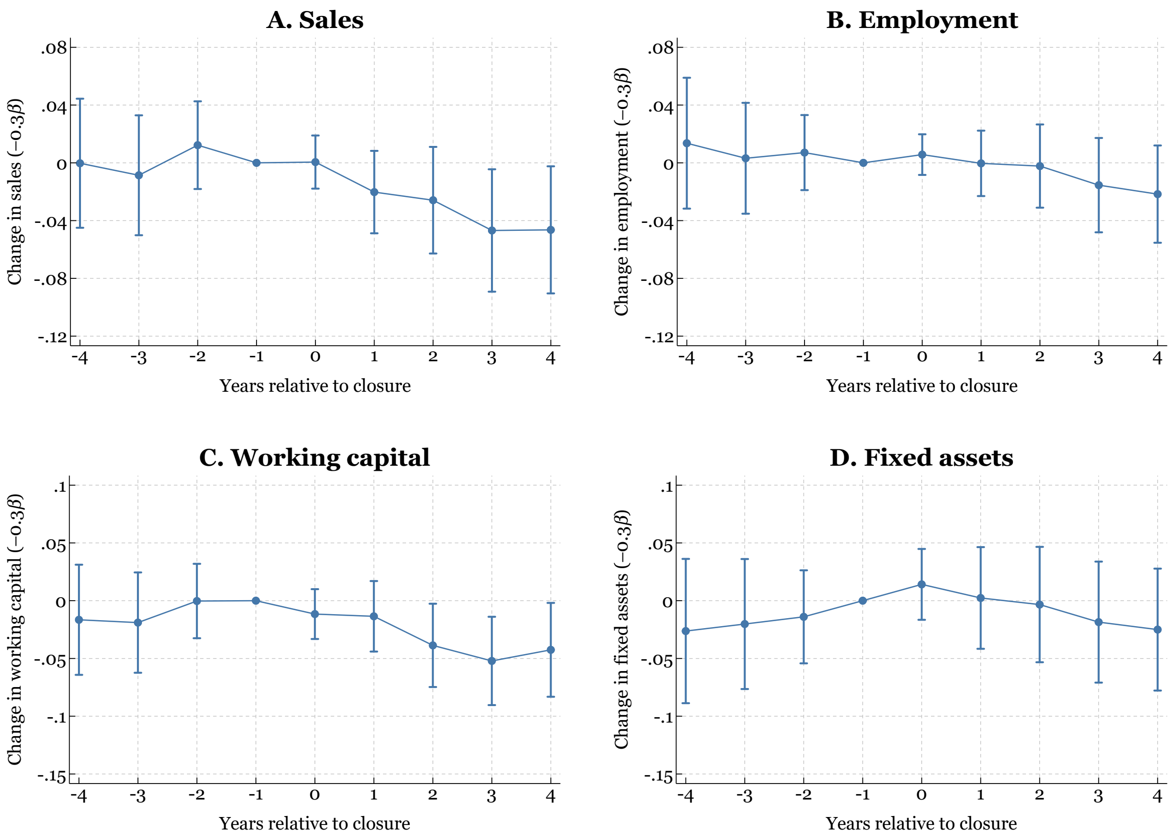
$$\Delta Y_{i,t+h} = \alpha_j^h + \gamma_t^h + \beta^h \cdot \Delta Branches_{j,t} + \xi^h \cdot \gamma_t \cdot s_{j,t} + \theta^{\mathbf{h}} \cdot \mathbf{X_{j,t}} + \varepsilon_{j,t}^h, \tag{11}$$

where (10) is the first stage and (11) is the second stage. ϕ_j and α_j^h are municipality fixed effects, ϕ_t and γ_t^h are year fixed effects, and $\mathbf{X_{j,t}}$ a vector of controls comprising two lags of $\Delta Branches_{j,t}$ and the seven municipality variables listed in Panel B of Table 1. The motivation for having the municipality control variables in the model is that the pre-treatment trends are parallel only conditional on including them (unlike in the firm-level estimations, where the pre-treatment trends are parallel irrespective of whether controls are included or not). We estimate the model with the $t-1$ municipality populations as weights to account for the large differences in the size of the municipalities.[24] Standard errors are clustered at the municipality level.

We use three dependent variables in the estimations: the percent change in the number of firms in municipality j between years $t-1$ and $t+h$, as well as the respective firm entry and exit rates in municipality j over the same period. The entry rate is defined as the number of entrants

[24]The results do not change if we instead weigh by the municipality population in year t.

Figure 4: The dynamics of the real effects of branch closures



This figure plots how the effects of bank branch closures on the real activity of local firms evolve over time. The circles correspond to the respective two-stage least squares estimates of β^h obtained from the estimation of equations (1) and (3) for horizons $h = -4, ..., 4$. The estimates are scaled to correspond to the effect of closing 30 percent of the bank branches in a municipality. The estimation samples are restricted to firms that have a positive amount of loans outstanding in year $t-1$. Standard errors are clustered at the municipality-year level and the vertical bars represent 95-percent confidence intervals.

between years $t-1$ and $t+h$ divided by the number of firms in year $t-1$; the exit rate is defined analogously. We construct the dependent variables in three steps: we begin by classifying each firm-year observation in the data as continuer, entrant, or exiter; we then sum up the number of firms in each category by municipality and year; finally, we compute the overall growth rate as well as the entry and exit rates based on the aggregated municipality-level data.[25]

Note that the percent change in the number of firms between years $t-1$ and $t+h$ is equal

[25]We apply the same sample screens as in the firm-level analysis, except that we do not impose any lower size threshold. This is to ensure that we do not exclude the many entrants that start out on a very small scale (say, as one-person businesses) and then grow from there.

Table 9: The effect of branch closures on firm entry and exit

	(1) Growth in number of firms	(2) Contribution from entry	(3) Contribution from exit
$\Delta Branches_{j,t}$	0.124*	0.150**	0.025
	(0.069)	(0.070)	(0.031)
Scaled effect ($-0.3 \cdot \hat{\beta}$)	–0.037	–0.045	–0.008
Weak IV statistic	36.923	36.923	36.923
Number of observations	4,639	4,639	4,639
Number of municipalities	290	290	290

This table reports the effects of bank branch closures on the number of firms in a municipality over a four-year period, as estimated using the municipality-level 2SLS model specified in equations (10) and (11). The regressions are weighted by the number of firms in each municipality in year $t-1$. The dependent variable is $(Firms_{j,t+4} - Firms_{j,t-1})/Firms_{j,t-1}$ in column (1), $Entrants_{t+4}/Firms_{j,t-1}$ in column (2), and $Exiters_{t+4}/Firms_{j,t-1}$ in column (3). Standard errors clustered by municipality are reported in parentheses. The weak IV statistic is the Kleibergen-Paap rk Wald F-statistic. *, **, and *** denote statistical significance at the ten, five, and one percent levels, respectively.

to the entry rate minus the exit rate:

$$\frac{Firms_{j,t+h} - Firms_{j,t-1}}{Firms_{j,t-1}} = \frac{Entrants_{t+h}}{Firms_{j,t-1}} - \frac{Exiters_{t+h}}{Firms_{j,t-1}}. \tag{12}$$

We can therefore decompose the overall effect of branch closures on the number of firms into the respective contributions of entry and exit by estimating the 2SLS model with the entry and exit rates, respectively, as dependent variables.

The estimation results for the four-year horizon ($h = 4$) are reported in Table 9. The coefficient estimate for the growth in the number of firms implies that the closing of 30 percent of the bank branches in a municipality on average is associated with a reduction in the growth rate of firms by 3.7 percentage points over a four-year period (column 1). The magnitude of the effect is thus large—for comparison, the average four-year growth in the number of firms in a municipality is 14.2 percent (17.2 percent when weighted by municipality population)—but the estimate is not statistically significant at the five-percent level ($p = 0.073$). Figure A1 in Online

Appendix A verifies that the pre-treatment trends are parallel (conditional on the municipality-level controls).

The overall effect of branches on the number of firms is entirely driven by a statistically and economically significant decline in the entry rate of 4.5 percentage points (column 2). The slight difference between the overall effect and the entry response is due to a small, statistically insignificant decline in the exit rate of 0.8 percentage points (column 3). The exit response estimated with the municipality-level model thus differs from the firm-level estimate reported in Table 8. The likely explanation for this discrepancy is that the municipality-level model has insufficient power to detect small effect magnitudes, like the firm-level exit response. That branch closures primarily affect the number of firms via the entry margin may reflect a larger role of soft information for this lending.

6 Concluding remarks

We examine how the dramatic downsizing of banks' local branch networks in recent years affects firms' access to credit and real economic activity. The empirical setting is Sweden, where almost two thirds of all bank branches have been closed in the past two decades. Our empirical analysis combines detailed data on the universe of Swedish firms and bank branches with a shift-share instrument in the spirit of Bartik (1991), exploiting spatial variation across municipalities in bank market shares, together with variation in the timing of each bank's branch-network downsizing. 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 firms' real economic activity and on business dynamism more broadly.

Our results suggest that the accelerating trend toward digital delivery of bank services—visible in both developed and emerging markets—may harm credit supply to small and medium-sized firms, where lending decisions traditionally involve soft information collected through branches. Without large branch networks, banks' credit supply may tilt towards asset-backed loans (Lian and Ma, 2020) and secured credit (Benmelech, Kumar and Rajan, 2022) to large, well-established borrowers, with negative consequences for new firm formation and entrepreneurship (Black and Strahan, 2002, and Ho and Berggren, 2020). One way to interpret our findings is that the digital transformation of SME lending has been slower than that of retail banking services; it may catch up in the future but until then, there may be a drop in the credit supply. More generally, large-scale, technology-driven disruption, even if it is beneficial overall and for many groups, may be harmful to some activities and to some firms. The findings in this paper suggest that this is the case in banking, where technology-driven retail banking efficiencies come at the expense of SME lending. Perhaps this creates an opportunity for innovation in the provision of SME loans. In the meantime, shrinking branch networks may have important implications for economic growth, employment, and monetary policy.

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Online Appendix for “Banking Without Branches”

Niklas Amberg and Bo Becker

Appendix A. Additional Tables and Figures

This appendix provides the additional tables and figures referred to in the main text of the paper. Table A1 reports the results of the covariate balance check for cross-sectionally demeaned municipality-level bank characteristics. Table A2 reports estimates of the first-stage regression for 30 subsamples of the data as an assessment of the monotonicity assumption.

Table A1: Covariate balance, cross-sectionally demeaned bank characteristics

	$Z_{i,t} < 0$			$Z_{i,t} \geq 0$			Normalized
	Mean	SD	N	Mean	SD	N	difference
Return on equity (%)	12.70	1.90	2,021	12.26	3.23	2,539	0.16
Total assets (SEK billion)	2,326	504	2,021	2,192	724	2,539	0.22
Asset growth (%)	1.49	1.87	2,021	1.85	3.49	2,539	-0.13
Loans/Deposits	2.01	0.28	2,021	1.93	0.60	2,539	0.17
Common equity/Assets (%)	4.82	0.32	2,021	4.31	1.14	2,539	0.61
Net interest income/Loans (%)	1.64	0.09	2,021	1.49	0.36	2,539	0.54
Credit losses (basis points)	15.0	10.7	2,021	10.8	10.2	2,539	0.40

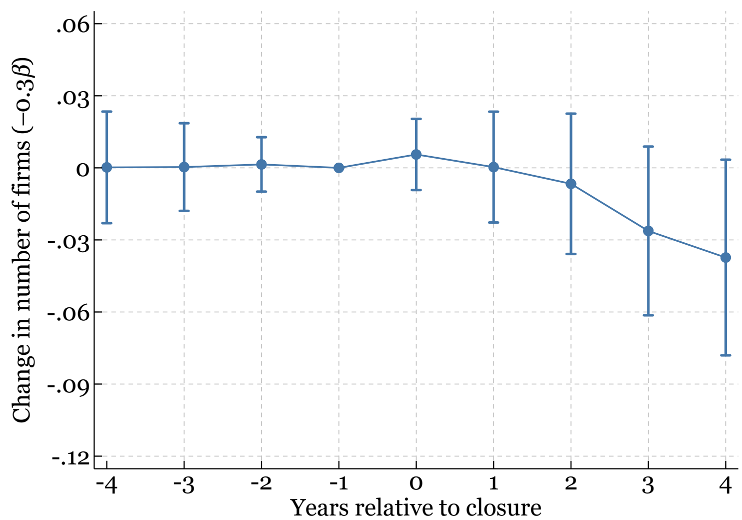
This table presents the same covariate balance check as in Panel C of Table 1, but for cross-sectionally demeaned covariates. The demeaned variables are constructed by subtracting the difference between the yearly mean and the overall mean from each variable.

Table A2: 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)	1.371***	0.177	1.495***	0.196
Sales (MSEK)	1.336***	0.177	1.545***	0.197
Number of employees	1.401***	0.179	1.462***	0.192
Liabilities/Assets	1.515***	0.193	1.376***	0.180
EBIT/Assets	1.347***	0.176	1.517***	0.193
Cash/Assets	1.312***	0.179	1.587***	0.194
Receivable days	1.517***	0.186	1.354***	0.185
Payable days	1.444***	0.185	1.431***	0.187
B. Municipality-level characteristics				
Log population (1000s)	1.065***	0.197	2.511***	0.437
Five-year population growth (%)	0.796***	0.205	2.630***	0.403
Population density	0.987***	0.214	2.395***	0.365
Elderly population share	1.887***	0.323	1.159***	0.226
Employment ratio (ages 20–74)	1.183***	0.238	1.765***	0.308
Relative labor income	0.985***	0.220	2.222***	0.328
Manufacturing share	1.994***	0.334	1.049***	0.225

This table reports estimates of the first-stage regression (3) for 30 subsamples of the data. The subsamples are constructed by splitting the sample at the median of each of the firm and municipality characteristics in the table. Relative labor income is the ratio of the average labor income in a municipality to the national average, while the elderly population share is the share of people 65 years or older in a municipality's population. All other variables are self-explanatory. 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.

Figure A1: The effect of branch closures on the number of firms in a municipality



This figure plots how the effect of bank branch closures on the number of firms in a municipality evolves over time. The circles correspond to the two-stage least squares estimates of β^h obtained from the estimation of equations (10) and (11) with $(Firms_{j,t+h} - Firms_{j,t-1})\,/Firms_{j,t-1}$ as outcome variable and for horizons $h = -4, ..., 4$. The estimates are scaled to correspond to the effect of closing 30 percent of the bank branches in a municipality. Standard errors are clustered at the municipality level and the vertical bars represent 95-percent confidence intervals.

Appendix B. Details on the Construction of the Branch Data

As described in section 3.1 in the main text, we construct our bank-branch panel using data from two sources: the Swedish Bankers' Association's publication *Bankplatser i Sverige* and the administrative dataset Pipos from the Swedish Agency for Economic and Regional Growth. In what follows, we provide further details on how we clean and adjust these data to obtain the branch panel that we use in the empirical analysis. We also validate our data by comparing the number of branches by bank in our data with the corresponding numbers reported in the publication *Bank and finance statistics 2023* from the Swedish Bankers' Association (2024).[B1]

B1 Data cleaning

The Pipos data has two shortcomings that we address as part of our cleaning of the data. First, it occasionally happens that a specific bank branch disappears from the data for one or a few years and then reappears again in the exact same place. These occurrences are in all likelihood due to errors in the data rather than to actual closings and reopenings. We therefore assume that such branches have existed in the intervening years and fill in the gaps in the panel accordingly.

Second, the information on Nordea branches in Pipos is reliable for 2011 as well as from 2017 and onwards, but there are severe reporting errors during the years 2012–16. We draw this conclusion on the basis of a comparison of the total number of Nordea branches in Pipos with the corresponding numbers reported in Swedish Bankers' Association (2024). We therefore need to reconstruct which Nordea branches that existed in the years between 2012 and 2016. To do so, we start from the set of Nordea branches that appear in Pipos in 2011. We assume that any branch that appears in the data in both 2011 and 2017 also existed in the intervening years and fill in the gaps in the panel accordingly. For the branches that appear in the data at some point during the years 2012–16 but not in 2017 or later, we take the last year in which the branch is observed in the data as its last year of existence.

After implementing these steps, we are left with 131 Nordea branches that appear in Pipos in 2011, but not in any later year. We are able to determine the closure year of 61 of these branches by means of local newspapers, which typically report on the closures of local

[B1] The publication *Bank and finance statistics* from the Swedish Bankers' Association only reports the aggregate number of branches per bank and year—not the location of individual branches. It is therefore not a potential alternative data source for our analysis.

bank branches. Another 21 branch closure years can be determined by means of the historical Street View function in Google Maps: if a branch office is visible in a Street View snapshot dated year t but not in the corresponding snapshot dated year $t + 1$, we set the closure year to t. This identification method requires frequent updates of the Street View, however, which are often not available. For the remaining 48 branches we are therefore unable to pin down an exact closure year with certainty. We know from external sources, however, that Nordea undertook substantial branch closures in 2016 and therefore assign 2016 as closure year for the remaining branches. When doing so, we obtain an overall branch growth rate of –20 percent for Nordea in 2016, which is close to the corresponding –22 percent branch growth rate reported in the Swedish Bankers' Association (2024).

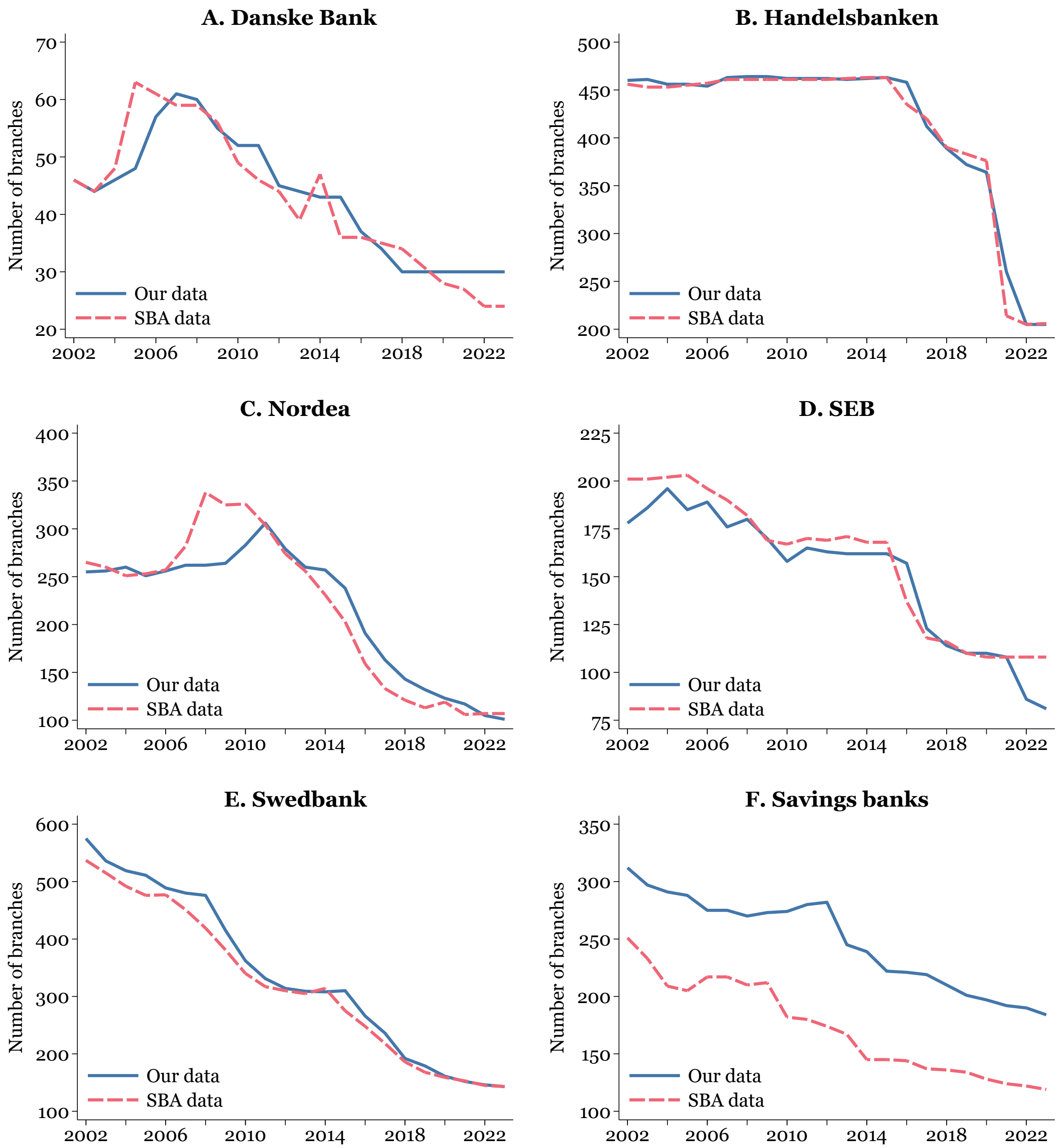
B2 Validating the cleaned branch data

To validate our cleaned branch panel, we compare the number of branches by bank and year in our data with the corresponding numbers reported in the publication *Bank and finance statistics* (Swedish Bankers' Association, 2024; henceforth SBA). We want to stress that small discrepancies in the number of branches in these two data sources are not necessarily an indication that our data contains errors. There are two reasons for this. First, the branch data in *Bank and finance statistics* is incomplete in certain dimensions (more on this below) and may contain errors of its own. Second, differences in the definition of what constitutes a branch may lead to some discrepancies, in particular the fact that we count several branches as one if they are located in the same postal code. That said, it would be a cause for concern if our data deviated substantially from the SBA data.

The results of the comparison are plotted in Figure B1. The overall picture that emerges is that the number of branches for each bank in our data closely tracks the corresponding number of branches in the SBA data. Two discrepancies are worth commenting on, however. First, the number of branches belonging to savings banks is substantially higher in our data than in the SBA data. This is because not all savings banks report branch information to the SBA; their data is therefore incomplete in this regard.

Second, the number of Nordea branches is substantially higher in the SBA data than in our data during the years 2008–10. The explanation is that Nordea in 2008 acquired about 70 branches from Svensk Kassaservice, a subsidiary of the government-owned Swedish postal ser-

Figure B1: Comparing the number of branches by bank in different datasets



This figure compares the number of branches per bank and year in our data with the corresponding numbers reported in the Swedish Bankers' Association (2024).

vice (Posten AB) that served retail customers with various payment and cash handling services until it was closed down in 2008. Since this acquisition occurred in between the periods covered by our two data sources, the resulting increase in the number of Nordea branches does not show up immediately in our branch panel. However, the acquired branches were not proper commercial bank branches at the time of the acquisition; the process of turning them into such and integrating them into Nordea's branch network unfolded over the years following the acquisition. Thus, while our data may understate the number of proper Nordea branches somewhat in the years 2008-10, the SBA data likely overstates it. We do therefore not deem this discrepancy a major concern.

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