Banking Without Branches*

Niklas Amberg[†] Bo Becker[‡]

August 2024

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 rapidly following branch closures, mainly via reduced lending to small, collateral-poor, and risky firms. The reduced credit supply has substantial real effects: local firms experience a decline in employment and sales and an increase in exit risk after branch closures. Our results thus demonstrate that the disappearance of bank branches have far-reaching implications for the economy.

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

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

*We thank Diana Bonfim, Marieke Bos, Johannes Breckenfelder, Dominik Damast (discussant), Olivier De Jonghe, Tim Eisert (discussant), Viktor Elliot, Marie Hoerova, Christopher James (discussant), Luc Laeven, Klaas Mulier, Marco Pagano, Kasper Roszbach, Enrico Sette, Jagadeesh Sivadasan, Christophe Spaenjers (discussant), Per Strömberg, and Jonas Söderberg, as well as seminar and conference participants at the Chicago Federal Reserve, CSEF - University of Naples Federico II, the 11th EFI Workshop (National Bank of Belgium), the European Central Bank, the Conference on Frontier Risks, Financial Innovation and Prudential Regulation of Banks (University of Gothenburg), the 5th International Research Conference on Management & Business (Jakarta), ICFAB 2024, LUISS Finance Workshop (Rome), STIE YKPN Business School (Yogyakarta), Stockholm Business School, the Stockholm School of Economics, and Sveriges Riksbank for valuable comments and suggestions. Joakim Jigling provided excellent research assistance.

[†]Sveriges Riksbank, Research Division. E-mail: niklas.amberg@riksbank.se.

[‡]Stockholm School of Economics, CEPR and ECGI. E-mail: bo.becker@hhs.se.

1 Introduction

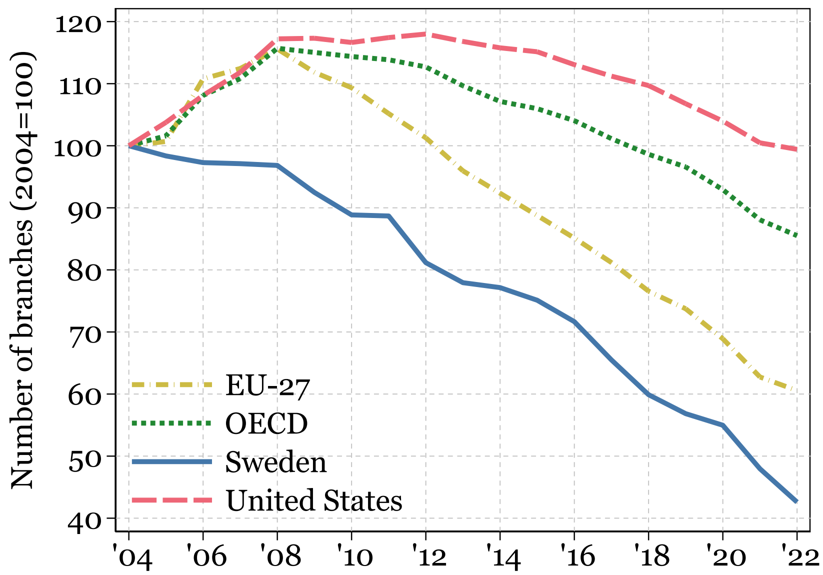
In the last twenty years, technological change has made possible the digital distribution of many financial services, reducing or eliminating the need for in-person interactions. Retail banking has been dramatically affected by the move to digital distribution: the share of European households using 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 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]

Disappearing bank branches may have a negative impact on business lending—and in particular on SME lending—a segment of the bank market where digital advances have not kept pace with those in retail banking. 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 (Bank Director, 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 several reasons. First, branch closures have been dramatic in the past decades: almost two thirds of Swedish bank branches disappeared between 2001 and 2023; as of 2023, 43 out of

[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, cash outstanding peaked in 2005 in real terms, and fell by 56 percent to 2023.

Figure 1: Bank branches across the world, 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 is 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.

Sweden's 290 municipalities no longer have a single bank branch.[2] Second, the market is dominated by a small number of large banks, which have all closed branches extensively, but at different times. This offers 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 large-scale branch closures. We provide extensive empirical evidence showing that the LATE conditions (Imbens and Angrist, 1994) are likely to be satisfied in our setting, and thus that the shift-share instrument 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

[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). The Nordic countries and the Netherlands have seen the largest declines.

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 consider effects up to a three-year horizon, and use the three-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 (results are similar if we disregard undrawn credit). 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 six percent over a three-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 4.8 percentage points following a 30-percent branch closure. Cross-sectional heterogeneity analyses show, moreover, that the credit-supply effect of branch closures is significantly larger for firms that are small (whether measured by sales or assets), risky (high ex-ante probability of default), and collateral-poor (low asset tangibility)—i.e., firms for which banks' lending decisions are likely to be particularly reliant on soft information. We do not find a significant effect of branch closures on loan entry, perhaps because the loan-entry variable only captures new lending to existing firms—not the arrival of new firms.

Having documented that branch closures lead to contractions in credit-supply, we next examine which financial margins firms use to adjust to the contractions. We find that firms do not change their cash holdings or the amount of trade credit they extend to customers, but draw more liquidity from suppliers by increasing accounts payable (i.e., paying later). Firm equity declines as a consequence of decreases in retained earnings. Taken together, our findings suggest that firms are able to offset at most a minor part of the credit-supply effects of branch closures by adjusting other financial positions.

In the last part of our empirical analysis, we assess whether the credit-supply shocks induced by branch closures have negative effects on firms' real economic activity. We begin by estimating the *direct* real effects of branch closures, i.e., the effects on firms that rely on banks for funding. We consider five real outcome variables: employment, sales, fixed assets (property, plant and equipment), working capital (accounts receivable and inventory), and firm exit. Our estimates imply that the closure of 30 percent of the bank branches in a municipality leads to declines in sales, employment, and working capital of 4-5 percent for local bank-dependent firms over a three-year period; the probability of exit increases by 1.6 percentage points over the same period. We do not find statistically significant effects of branch closures on fixed investments, however. This is consistent with the soft-information hypothesis: since loans for fixed investments are typically secured by physical collateral, they should be less dependent on soft information and therefore less sensitive to branch closures.

Finally, several papers have documented that credit-supply shocks can have substantial indirect effects by spreading either locally (Huber, 2018) or through supply chains (Alfaro, García-Santana and Moral-Benito, 2021). Motivated by this literature, we assess whether the credit-supply shocks induced by branch closures have indirect effects in the form of aggregate-demand spillovers—i.e., whether the decline in the real activity of directly affected firms leads to declines in the sales and employment of other local firms via lower aggregate demand. We find that the closure of 30 percent of the bank branches in a municipality leads to an average decline of four percent in sales for local firms that (i) do not rely on banks for funding and (ii) operate in non-tradable sectors, whereas the sales of non-bank firms in tradable sectors do not respond to branch closures (employment is not significantly affected in either group). This suggests that branch closures have some real effects through aggregate demand spillovers.

Related literature. The central contribution of our paper is to demonstrate that bank branches still remain economically important through their connection to local firms, and that the large-scale closure of a country's bank-branch network therefore has substantial negative effects on firms' credit access and real economic activity. Our findings are thus consistent with the view that soft information is critical for business lending (Agarwal and Hauswald, 2010) and that branches continue to play a key role in collecting and processing it.

The paper most closely related to ours is Nguyen (2019), which studies the effect of branch closures on local small-business lending using a merger-based instrument and data at the level of U.S. census tracts. She finds that small-business lending declines substantially and persistently following merger-induced branch closures, but that the real effects of the closures are fairly small and limited to entering establishments. We expand and improve on Nguyen's (2019) findings in several ways. First, our rich firm-level data allow us to document more detailed results regarding the effects of branch closures on firms, for example heterogeneity in the credit-supply effects across firm types, how firms adjust their financial positions in response to branch closures, and how firms' real activity is affected by the credit-supply contractions induced by closures. Second, we study the effects of large-scale, permanent branch closures, whereas Nguyen (2019) and most other papers in the existing literature (e.g., Ashcraft, 2005, and Garmaise and Moskowitz, 2006) examine individual branch closures—such as those induced by bank mergers, which tend to be highly local, selected specifically because there are other nearby branches, and possibly temporary. A potential explanation for why our estimates of the real effects of branch closures differ from those of Nguyen (2019) is that large-scale branch closures are more consequential than isolated branch closures that occur in the context of a stable or expanding surrounding branch network. Our findings may thus improve external validity for the effects of the large-scale winding down of countries' entire bank-branch networks, as is happening or starting to happen across developed countries.[3]

Another closely related paper is Bonfim, Nogueira and Ongena (2020), which uses 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 because of branch closures obtain worse loan rates from the new bank than do firms that change bank voluntarily, even though the former have lower default rates that the latter. We view our findings as consistent with and complementary to those of Bonfim, Nogueira and Ongena (2020), as we do not examine the effect of branch closures on loan pricing.

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

[3]A few contemporaneous papers study the effects of bank-branch closures on various firm outcomes, including Garrì (2019), Jiménez et al. (2022), and Ranish, Stella and Zhang (2024). The effects of bank branch closures in Sweden has previously been examined by Kärnä, Manduchi and Stephan (2021) and Ho and Berggren (2020).

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 results suggest, however, that traditional banks with brick-and-mortar branches continue to fulfill an important role that fintechs and other non-bank lenders cannot fully take over. Whether this will change in the future is an open question.

Moreover, several recent papers demonstrate that within business lending, new technology often serves as a complement, rather than a substitute, to bank branches. 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. Given this, it is not surprising that new information technology cannot fully substitute for physical bank branches.

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 firms.[4] Accelerated growth in the fintech sector may compensate (Gopal and Schnabl, 2022), perhaps by using new information sources (Liberti and Petersen, 2018), but to what extent it can do so remains unclear. Third, more broadly, our results point to the mixed blessings of technological disruption: large gains often come at the expense of some losses (e.g., Becker and Ivashina, 2023).

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

[4]Whether the gains outweigh the costs—i.e. whether the aggregate welfare effects of digital banking are positive or negative—is a question beyond the scope of this paper.

2 Institutional Background: The Great Bank-Branch Closure Wave

2.1 The Swedish bank market

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

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

2.2 The shrinking Swedish bank branch network

The number of bank branches in Sweden has declined rapidly and steadily since the early 2000s, going from almost 1,900 in 2001 to around 750 in 2023.[5] The decline has been particularly pronounced in recent years: the average annual decline in the number of bank

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

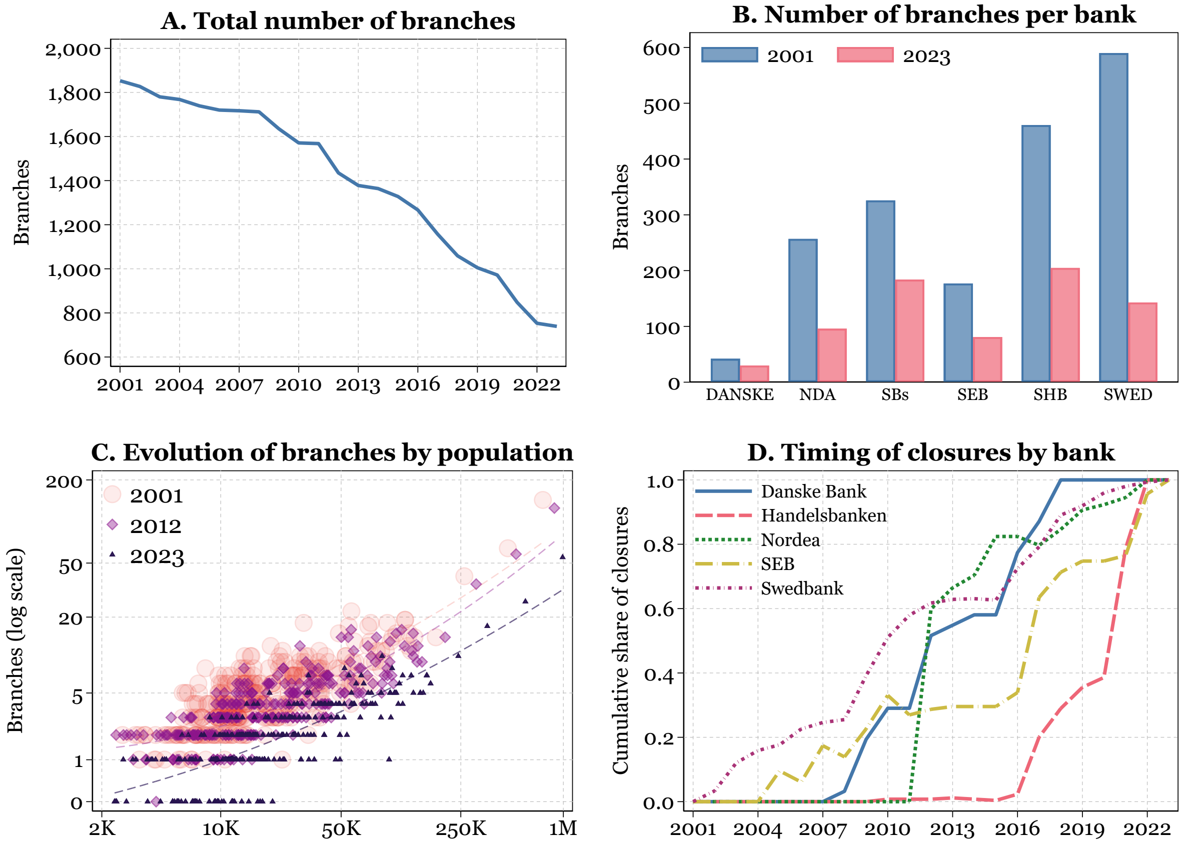
branches was 1.1 percent during 2002-2008, and then accelerated to 5.4 percent from 2009 and onwards. In both 2021 and 2022, more than 10 percent of all branches were closed. All four major banks (Handelsbanken, Nordea, SEB, and Swedbank) have contributed to the decline, with reductions in the number of branches ranging from 54 percent (SEB) to 76 percent (Swedbank) between 2001 and 2023. We illustrate this development in Figure 2, which shows the number of bank branches nationwide for all banks over the period 2001-2023 (Panel A) as well as for each bank separately in 2001 and 2023 (Panel B). Panel C—which plots the evolution of the number of branches in each municipality against population—shows that the closures have affected small and large municipalities alike, and accelerated after 2012.

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

While this broad trend is technological and affects all banks, the timing of the reductions in branch networks have been bank-specific.[6] This has meant that branch closures have been concentrated over a short time span for each bank and that the timing of the closures differ substantially: the largest reduction in branches in a single year occurred in 2012 for Nordea (–39 percent), in 2017 for SEB (–22 percent), in 2018 for Swedbank (–19 percent), and in 2021 for Handelsbanken (–29 percent). We plot the complete time profile of each bank's branch closures in Panel D of Figure 2: the figure demonstrates the varied timing, with Swedbank beginning large-scale branch closures early, SEB and Handelsbanken late, and Nordea and Danske Bank in between. Hence, the smooth decline in the total number of branches evident in Figure 2 masks substantial lumpiness in branch closures at the bank level, as well as heterogeneity across banks in the timing of the closures. The timing difference combined with the differing geographical coverage of the respective banks forms the basis of our shift-

[6]Kundu, Muir and Zhang (2024) identify significant divergence across U.S. banks, where some attract deposits with the convenience of a wider branch network. We observe no such heterogeneity.

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).

share identification strategy, which we describe in detail in Section 3.

2.3 Econometric evidence on the determinants of branch closures

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

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

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

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

In column (3), we add the percent change in the nationwide number of branches of a bank, which adds considerable explanatory power. The estimate shows that the probability that a bank reduces the number of branches in a municipality increases by one percentage point for every percentage point decrease in nationwide branch growth. In column (4), we include an interaction term between nationwide branch growth and the lagged number of branches a bank has in a municipality. The estimate shows that the effect of nationwide branch growth on closure probability is strongly increasing in the number of branches the bank has in the municipality and contributes to a further increase in R^2.

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

Table 1: Determinants of bank-branch closures

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

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

for large banks.

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

3 Empirical Framework: A Shift-Share IV Analysis

3.1 Data

The empirical analysis is based on two main data sources. The first is an annual panel data set comprising the number of bank branches per municipality and bank over the period 2001–2023 that we create based on data from two different sources. One source is *Bankplatser i Sverige* (Bank Locations in Sweden), a print publication containing the address of every bank branch in Sweden, issued annually by the Swedish Bankers' Association until 2008. This publication was replaced by a web page with the same name, which is regularly updated but where no historical records are maintained; the web page can therefore not be used to reconstruct historical series of bank branches by municipality and bank after 2008. We also use the administrative database Pipos from the Swedish Agency for Economic and Regional Growth, which provides the exact location—down to latitude and longitude—of every bank branch in Sweden from 2011 and onwards.[7]

Combining the two data sources with branch data, we construct an annual panel with the number of branches per municipality, bank, and year for the periods 2001–2008 and 2011–2023. The panel comprises branches belonging to Danske Bank, Handelsbanken, Nordea, SEB, Swedbank, and savings banks—that is, the main lenders on the Swedish corporate loan market during our sample period (see section 2).[8] For practical purposes, we define a bank

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

[8]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,

branch as a combination of bank and postal code. Hence, if a bank reports several branches for the same postal code—which occasionally happens, for example, when a branch office is split across several numbers of the same street—we count one branch. We impute observations for the years 2009–2010 by linearly interpolating between the number of branches for each municipality-bank cell in 2008 and 2011, respectively. By doing so, we obtain a complete municipality-level panel spanning the period 2001–2022.

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

In addition to the main data sets, we use data from two additional sources to construct control variables for the estimations. First, we build a municipality-year panel comprising, among other things, population size, employment, labor earnings, and population density using publicly available information from Statistics Sweden, the official Swedish statistics agency. Second, we obtain credit-score data from Upplysningscentralen AB (UC), a leading Swedish credit bureau co-owned by the major banks. More specifically, we use the firm-specific probability of default that UC assigns to each Swedish firm on the basis of a rich scoring model.[9]

3.2 Empirical model and instrument

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

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

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.

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

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

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.[10] 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 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.[11] 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 (2) is β^h, which measures the effect of a change in the number of bank branches in a municipality on real and financial outcomes for local

[10]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.

[11]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 $t+h$.

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

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

where $Branches_{b,j,t-1}/Branches_{j,t-1}$ is bank b's share in the total number of bank branches in municipality j in year $t-1$, and $\Delta Branches_{b,t}$ is the percent change in the number of bank branches nationwide for bank b between years $t-1$ and t. That is, the instrument combines variation in the exposure of a given municipality to the respective banks (the shares) with the nationwide change in the number of branches of each bank (the shifts), where the former is pre-determined at time t and the latter is plausibly orthogonal to economic conditions in a given municipality.[12]

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

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

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

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

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

3.3 Assessing the validity of the research design

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

3.3.1 Internal validity

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

1. **Instrument strength:** $Z_{j,t}$ strongly affects the endogenous regressor—the decision of a bank to close branches across the country has a strong effect on branch growth in the municipalities in which the bank has a large presence.

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

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

Instrument strength. The first assumption can be verified by inspecting the first-stage F-statistic. When estimating the first-stage regression (4) with the baseline set of control variables, we obtain an effective first-stage F-statistic of 151.7 (Montiel Olea and Pflueger, 2013). This is well above any 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 banks' headquarters.

Instrument independence. The independence assumption requires that firms' exposure to nationwide branch-closure waves are unrelated to other factors affecting their development. We assess the plausibility of this (formally untestable) assumption by comparing observations where the instrument is negative and non-negative, respectively, across a set of firm- and municipality-level covariates that are likely to be correlated with a firm's current and prospective economic condition. We assess the magnitude of the differences in the covariates using the normalized difference in means, a comparison metric proposed by Imbens and Rubin (2015) that measures the difference in means expressed in terms of standard deviations.

Table 2: Covariate balance

	$Z_{i,t} < 0$			$Z_{i,t} \geq 0$			Normalized
	Mean	SD	N	Mean	SD	N	difference
A. Firm-level characteristics							
Assets (MSEK)	28.6	98.7	665,961	23.5	83.5	147,300	0.06
Sales (MSEK)	32.4	84.1	665,961	28.6	76.4	147,300	0.05
Number of employees	14.1	27.8	665,961	13.3	26.3	147,300	0.03
Age (years)	19.88	14.30	665,961	19.36	13.92	147,300	0.04
Debt/Assets	0.75	0.18	665,961	0.76	0.18	147,300	–0.06
EBIT/Assets	0.07	0.13	665,961	0.07	0.12	147,300	–0.01
Cash/Assets	0.12	0.14	665,961	0.11	0.14	147,300	0.04
Probability of default	1.95	4.79	665,961	2.01	4.78	147,300	–0.01
B. Municipality-level characteristics							
Population (1000s)	36.1	70.5	3,528	23.4	51.3	1,112	0.21
Five-year population growth (%)	1.33	4.29	3,528	0.28	3.61	1,112	0.26
Population density	150	515	3,528	113	435	1,112	0.08
Branches per 1,000 inhabitants	0.22	0.14	3,528	0.24	0.13	1,112	–0.16
Employment ratio	0.68	0.04	3,528	0.68	0.04	1,112	0.07
Relative labor income	0.95	0.12	3,528	0.93	0.11	1,112	0.16
Manufacturing share	0.33	0.18	3,528	0.37	0.18	1,112	–0.20

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

The main benefit of using normalized differences instead of t-tests for such comparisons is that the normalized difference is scale-free, in the sense that the likelihood of rejecting similarity does not increase mechanically with sample size.

The results of the comparison exercise are reported in Table 2. Panel A demonstrates that firm-years highly exposed to banks closing branches nationwide do not differ meaningfully from less exposed firm-years across the characteristics under consideration. To see this, note that the magnitude of the largest normalized difference in means is only 0.06—for comparison, in an analysis of the data from an experiment with random treatment assignment, Imbens and Rubin (2015) observe a maximum normalized difference of 0.30 and judge this to be evidence of strong covariate balance. Panel B shows that the same holds true for municipalities—the largest normalized difference across the seven municipality-level characteristics under consideration is 0.26, which implies that municipality-years with high exposure to banks closing branches nationwide are similar to municipality-years with low exposure. Taken together, the results in Table 2 provide strong evidence in favor of the instrument-independence assumption.

Exclusion restriction. The exclusion restriction is—like the independence assumption and the monotonicity requirement—formally untestable. Recall that the exclusion restriction would be violated if the exposure of a municipality to a bank that closes branches nationwide affects economic outcomes in the municipality via channels other than the branch-closure channel. Importantly, such a mechanism would have to affect all municipalities in proportion to their exposure to the bank in question, and thus independently of the actual rates of branch closures that the nationwide closures induce in each municipality. For example, the municipality of Stockholm would have to be affected by the nationwide branch closures of the bank Alpha as long as Alpha has branches in Stockholm, even if Alpha does not in fact close any of its Stockholm branches.

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

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

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

3.3.2 External validity

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

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

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

Table 3: Complier characteristics

	Marginal effect at 10th pct	Marginal effect at 90th pct	Ratio of marginal effects (90/10)	p-value
Population (1000s)	1.26	1.13	0.90	0.025
Five-year population growth (%)	0.90	1.50	1.67	0.089
Population density	1.18	1.22	1.03	0.092
Branches per 1,000 inhabitants	0.97	1.51	1.56	0.023
Employment ratio	1.39	1.01	0.72	0.150
Relative labor income	1.28	1.09	0.85	0.321
Manufacturing share	1.02	1.42	1.40	0.089

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

The estimation results, reported in Table 3, show that the degree of compliance does not vary significantly with most of the municipality characteristics under consideration. To see this, note that the estimated interaction terms from equation (5) are statistically insignificant at the five-percent level for five out of the seven municipality characteristics. The two characteristics for which the interaction is significant is municipality population and branch density. The magnitude of the difference is modest when it comes to population: the effect of a one percentage point decrease in the instrument, $Z_{j,t}$, on the growth rate of bank branches is –1.3 percentage points in municipalities with small populations and –1.1 percentage points in municipalities with large populations. The difference is somewhat larger in terms of branch density: a one percentage point decrease in the instrument is associated with a reduction in the branch growth rate of 1.5 percentage points in high-density municipalities and 1.0 percentage points in low-density municipalities.

That the heterogeneity in instrument strength across observable municipality characteristics in general is small speaks against the concern that the local average treatment effects identified by our 2SLS estimates are uninformative about the average causal effect of branch closures in the population of municipalities.

4 Empirical Results

This section presents the results of the empirical analysis. 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 bank branches in a municipality, which is approximately the average branch growth rate in the municipality-years in which branch closures take place.

4.1 The effect of bank branch closures on local credit supply

4.1.1 Baseline estimates

The baseline estimates of the effect of bank branch closures on the supply of credit to local firms are reported in Table 4. The first column reports 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 5.8 percent over a three-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 4.5 percentage points following the closure of 30 percent of

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

	(1) Overall effect	(2) Loan exit	(3) Loan entry
$\Delta Branches_{j,t}$	0.201***	–0.159***	–0.010
	(0.077)	(0.036)	(0.021)
Scaled effect $(-0.3 \cdot \hat{\beta})$	–0.060	0.048	0.003
Weak IV statistic	164.868	164.763	153.984
Number of observations	813,261	801,311	468,095
Number of firms	107,441	106,992	77,575

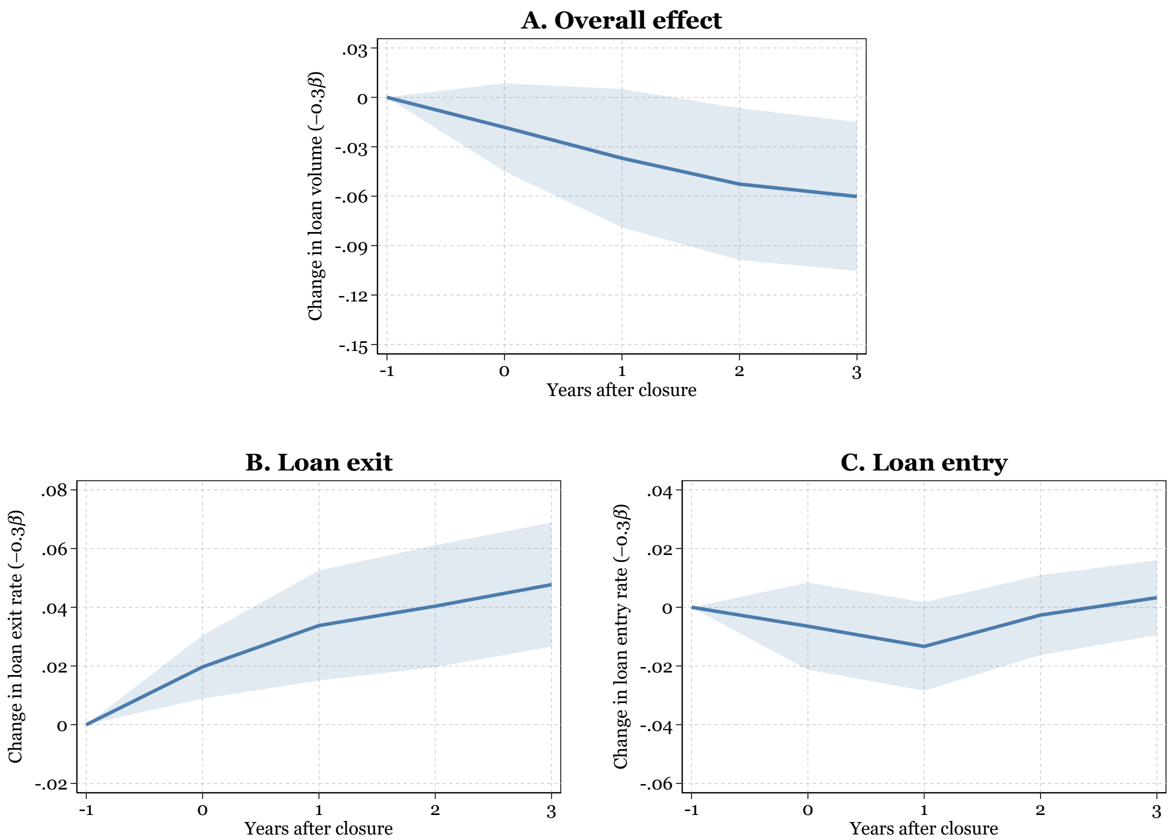
This table reports the baseline two-stage least squares estimates of the effect of bank branch closures on credit supply to local firms over a three-year period ($h = 3$). The dependent variable is $\Delta Loans_{i,t+3}$ in column (1), $LoanExit_{i,t+3}$ in column (2), and $LoanEntry_{i,t+3}$ in column (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.

the bank branches in a municipality.

The corresponding estimate for the entry margin, reported in column (3), is small 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 show how the effect of branch closures on credit supply evolves over time by plotting the 2SLS estimate of β^h for each estimation horizon h, scaled to correspond to the effect of closing 30 percent of the bank branches in a municipality. The estimates of the overall effect, reported in Panel A, show that bank lending to local firms starts to decline immediately after branch closures and then continues to decline for several years; the trough of the response is reached around two years after a closure. The picture is similar when we consider the exit margin (Panel B): loan exit rates increase immediately after branch closures and

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



This figure plots how the effect of bank branch closures on local credit supply evolves over time. The lines correspond to the respective two-stage least squares estimates of β^h obtained from the estimation of equations (4) and (2) for estimation horizons $h = 0, 1, 2, 3$. The dependent variable is $\Delta Loans_{i,t+h}$ in Panel A, $LoanExit_{i,t+h}$ in Panel B, and $LoanEntry_{i,t+h}$ in Panel C. 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 shaded areas represent 95-percent confidence intervals.

then continue to increase until the effect reaches its peak around two years after a closure. The loan-entry effect, meanwhile, is statistically insignificant at all estimation horizons.

That the credit-supply effect of branch closures 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 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.

4.1.2 Comparing the 2SLS estimates with OLS estimates

How do the baseline 2SLS estimates just documented compare to the corresponding OLS estimates? To answer this question, we report more detailed IV diagnostics in Table 5. More specifically, the table provides estimation results for the first-stage regression (first column), the reduced form regression in which the dependent variable is regressed directly on $Z_{j,t}$ and $\mathbf{X_{i,t}}$ (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 $\mathbf{X_{i,t}}$ (fourth column). We focus on the overall credit-supply effect over a three-year period ($h = 3$) 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 1.18 percentage point decrease in actual branch growth—the estimate is not statistically different from 1. The reduced form estimate is therefore by necessity close to the 2SLS estimate, since it is given by the product of the first-stage estimate and the 2SLS estimate. The OLS estimate, on the other hand, is close to zero and statistically insignificant. What accounts for this difference? Provided that our instrument is valid, the difference between the 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, omitted variable bias is unlikely to be the main driver of the difference between the 2SLS and OLS estimates. The reason is that the probable direction of any omitted variable bias is upward, not downward, since branch closures if anything should be more likely to occur in municipalities with worse economic prospects. The likely explanation for the larger 2SLS estimate is instead heterogeneous treatment effects, in the sense that the branch closures captured by our instrument are more consequential than the average branch closure. This is because when banks undertake nationwide closure waves—the closures primarily captured by our instrument—they will by necessity have to include many large and more active branches, whereas the closures undertaken outside of the nationwide closures

Table 5: Further IV diagnostics

	First stage	Reduced form	2SLS	OLS
		Dependent variable: $\Delta Loans_{i,t+3}$		
$Z_{j,t}$	1.169***	0.236***		
	(0.091)	(0.089)		
$\Delta Branches_{j,t}$			0.201***	0.014
			(0.077)	(0.015)
Number of observations	813,261	813,261	813,261	813,261
Number of firms	107,441	107,441	107,441	107,441

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

waves are likely to be focused on less active branches that can be closed with smaller consequences for the local economy (see Nguyen, 2019, for a similar argument). A corollary of this argument is that the adverse effects of branch closures are likely to grow more severe over time: whereas banks can initially scale down their branch networks by closing down relatively inconsequential branches, they will over time have to target more and more consequential branches to continue the downscaling.

4.1.3 Heterogeneity in the credit-supply effects of branch closures

Theory predicts that the effects of branch closures are heterogeneous in the population of firms—for example, small firms are predicted to be more sensitive to branch closures than large firms, as small-business lending is more reliant on soft information (e.g., Agarwal and Hauswald, 2010). In what follows, we undertake a cross-sectional heterogeneity analysis to assess whether this is indeed the case. We do so by estimating the baseline specification on various subsamples of firms—obtained by splitting the sample at given cutoffs of theoretically relevant firm characteristics—and testing whether the effect of branch closures on credit supply differs across the subsamples. We consider four firm characteristics in the heterogeneity analysis: size, age, asset tangibility, and riskiness. Depending on what is appropriate in each case, we compare either the top quartile with the bottom three quartiles, or the bottom quartile with the top three quartiles. All split variables are measured as of year $t-1$ to ensure that we do not split the sample based on measures possibly affected by the treatment.

We begin with firm size, for which our preferred measure is sales. We classify firms in the top quartile of the sales distribution as large and firms in the bottom three quartiles as small.[13] The results of the size-split, reported in Panel A of Table 6, 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 estimates in the two groups are, moreover, statistically significant in both cases. We obtain qualitatively similar results when measuring firm size by assets instead of sales (Panel B). This shows that the negative effect of branch closures on credit supply is primarily a small-firm phenomenon.

Next, lending to young firms is likely to be more dependent on soft information produced in local branches than is lending to older firms, due to the scarcity of hard information about young firms (Aretz, Campello and Marchica, 2020; Black and Strahan, 2002). We therefore expect the credit-supply effect of branch closures to be particularly strong for young firms. To test whether this is the case, we split the sample into younger firms (bottom quartile of the firm age distribution) and older firms (top three quartiles).[14] The results, reported in Panel B of Table 6, show that the point estimate of the overall credit-supply effect of branch closures is twice as large among young firms as among old firms. The difference across the two groups is not statistically significant, however. The point estimates for the loan exit response, meanwhile, are of similar magnitude and statistically significant in both cases. Hence, to the extent that the overall effect differs between young and old firms, it is accounted for by the intensive margin—i.e., by young firms receiving relatively smaller loans following

[13]We drop firms that are classified as small on their own, but are subsidiaries in corporate groups (*koncerner*) that also comprise large firms, since the appropriate size classification of such firms is difficult to determine.

[14]We drop firms that are classified as young on their own, but are subsidiaries in corporate groups that also comprise old firms, since the appropriate age classification of such firms is difficult to determine.

Table 6: 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 ($\geq P_{75}$)	–0.139	(0.141)	0.015	(0.052)	0.066	(0.084)
Small ($< P_{75}$)	0.229***	(0.074)	–0.149***	(0.034)	–0.029	(0.026)
Difference	–0.368**	(0.147)	0.163***	(0.055)	0.095	(0.088)
B. Firm size (assets)						
Large ($\geq P_{75}$)	0.001	(0.125)	–0.030	(0.047)	0.033	(0.063)
Small ($< P_{75}$)	0.191**	(0.076)	–0.147***	(0.035)	–0.025	(0.027)
Difference	–0.190	(0.139)	0.117**	(0.051)	0.058	(0.066)
C. Firm age						
Old ($\geq P_{25}$)	0.134*	(0.078)	–0.122***	(0.035)	0.001	(0.029)
Young ($< P_{25}$)	0.270**	(0.108)	–0.095**	(0.038)	–0.061	(0.047)
Difference	–0.136	(0.128)	–0.027	(0.049)	0.061	(0.055)
D. Asset tangibility						
High ($\geq P_{75}$)	0.010	(0.088)	–0.051	(0.034)	–0.027	(0.094)
Low ($< P_{75}$)	0.212**	(0.089)	–0.157***	(0.040)	–0.008	(0.022)
Difference	–0.202*	(0.111)	0.106**	(0.046)	–0.019	(0.096)
E. Probability of default						
Low PD ($< P_{25}$)	–0.010	(0.101)	–0.073*	(0.041)	0.004	(0.025)
High PD ($\geq P_{25}$)	0.205**	(0.081)	–0.164***	(0.038)	–0.024	(0.035)
Difference	–0.215**	(0.109)	0.091**	(0.045)	0.028	(0.042)

This table reports two-stage least squares estimates from estimations of equations (4) and (2) in various subsamples of the population. The dependent variable is $\Delta Loans_{i,t+3}$ in columns (1) and (2), $LoanExit_{i,t+3}$ in columns (3) and (4), and $LoanEntry_{i,t+3}$ in columns (5) and (6). The subsamples are constructed by splitting the sample at either the 25th percentile (P_{25}) or 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.

branch closures (the loan entry response is statistically insignificant in both groups). That the difference in the credit-supply response of branch closures across young and old firms is not more pronounced is somewhat surprising, in particular when it comes to the entry margin. One possible explanation is that the age difference that primarily matters is entrants versus encumbents, and, as explained in section 4.1.1 above, our estimates do not capture the effect of branch closures on banks' willingness to provide loans to entrants.

Third, we consider the role of asset tangibility: firms with more tangible assets are better able to pledge collateral when borrowing (e.g., Almeida and Campello, 2007) and should therefore be less sensitive to bank-branch closures, because the importance of soft information declines as loans become better collateralized. We split the sample based on the tangibility of firms' assets—defined as the ratio of tangible assets to total assets—classifying firms in the bottom three quartiles as low-tangibility firms and firms in the top quartile as high-tangibility firms. The results, provided in Panel C of Table 6, show that branch closures have a large and statistically significant effect on credit supply to firms with low asset tangibility, but no significant effect on firms with high asset tangibility. This is true in terms of the overall response as well as the loan exit response, and the difference between the respective estimates are statistically significant in both cases. As before, the loan entry response is insignificant in both groups of firms. These findings suggest that soft information collected via local branches is particularly important when pledgeable assets are scarce.

Finally, we test whether the credit-supply effects of branch closures vary depending on the borrower's credit risk, as measured by its probability of default (PD). We classify firms in the bottom quartile of the PD distribution as low-risk firms and firms in the top three quartiles as high-risk firms. The results of the risk-based sample split, reported in Panel D of Table 6, show that the overall effect as well as the loan exit response is significantly larger for high-risk firms than for low-risk firms; the overall response is, moreover, statistically insignificant in the group of low-risk firms (the loan entry response is statistically insignificant among low-risk as well as high-risk firms). Hence, banks primarily cut credit supply to riskier firms following branch closures.

4.1.4 Robustness checks

Our final empirical exercise concerning the credit-supply effects of branch closures is to assess the robustness of the baseline results. We do so by reporting estimation results for various alternative model specifications. The results are reported in Table 7, which also includes the baseline estimates for comparison (row A). For brevity, we only report results for the three-year estimation horizon ($h = 3$).

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

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

where $Branches_{b,j,2001}$ and $Branches_{j,2001}$ are the number of branches of bank b in municipality j in 2001 and the total number of branches in municipality j in 2001, respectively. The effect estimates obtained when instrumenting branch closures with $Z_{j,t}^{Initial}$, reported in row B, are essentially identical to the baseline estimates, except that the point estimate for the overall effect is no longer statistically significant. That the standard errors are somewhat larger in these estimations is unsurprising given the lower precision of the alternative instrument (the first-stage F-statistic is about two thirds smaller with the alternative instrument than with the main instrument). The similarity of the point estimates nevertheless suggests that endogenously evolving market shares do not bias the baseline estimates.

Second, we estimate the baseline regressions excluding municipality-years where the number of branches in year $t-1$ is zero or one. The results—reported in row C of Table 7—are very similar to the baseline results, which demonstrates that our findings are not primarily driven by municipalities with very few branches.

Third, we augment the baseline specification with a set of municipality-level controls comprising population size, five-year population growth, population density, 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 quite similar to the baseline estimates. The same is true when we augment the

Table 7: Specification checks for baseline credit-supply estimates

	(1) Overall effect	(2) Loan exit	(3) Loan entry
A. Baseline specification	0.201***	–0.159***	–0.010
	(0.077)	(0.036)	(0.021)
B. Instrumenting with $Z_{j,t}^{Initial}$	0.200	–0.157**	–0.031
	(0.138)	(0.061)	(0.039)
C. Dropping if $Branches_{j,t-1} \leq 1$	0.230***	–0.165***	–0.014
	(0.082)	(0.038)	(0.023)
D. Including municipality controls	0.189**	–0.149***	–0.012
	(0.074)	(0.034)	(0.021)
E. Including non-linear firm controls	0.192**	–0.151***	–0.011
	(0.075)	(0.035)	(0.021)
F. Excluding all control variables	0.202***	–0.146***	–0.011
	(0.074)	(0.034)	(0.020)

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 $\Delta Loans_{i,t+3}$ in column (1), $LoanExit_{i,t+3}$ in column (2), and $LoanEntry_{i,t+3}$ 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.

baseline specification with squares of the firm-level control variables (row E). Importantly, we also find that excluding all control variables do not meaningfully alter the baseline estimates (row F). These findings corroborate the instrument-independence assumption.

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 three-year period as dependent variables: cash over assets, receivables over sales times (receivable days), payables over input costs (payable days), and equity over assets. More specifically, the dependent variable in the cash regression is defined as

$$\Delta Cash/Assets_{i,t+3} = Cash_{i,t+3}/Assets_{i,t+3} - Cash_{i,t-1}/Assets_{i,t-1}, \tag{7}$$

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 8. Consider first column (1), in which we report the effect of branch closures on cash holdings. The estimated effect is precisely zero, which indicates that firms do not adjust their cash holdings in response to the credit-supply contraction 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 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 firms' choice between cash and credit lines). 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 estimates show that firms do not contract the trade credit extended to their customers following branch closures (column 2), but they do significantly increase the amount of trade credit that they obtain from their suppliers (column 3). The estimate implies that firms' payable days increase by 1.2 days on average following the closure of 30 percent of the bank branches in a municipality. This corresponds to four percent of the typical 30-day maturity of trade credit contracts in Sweden. That firms adjust their trade-credit positions in response to credit-supply shocks and other liquidity shocks is consistent with the findings in the previous literature (see, e.g.,

Table 8: Margins of adjustment to the credit-supply contraction

	(1) Cash holdings	(2) Receivable days	(3) Payable days	(4) Total equity	(5) Retained earnings	(6) Other equity
$\Delta Branches_{j,t}$	0.002	–0.228	–4.065**	0.020***	0.025***	–0.006
	(0.006)	(1.236)	(1.704)	(0.007)	(0.008)	(0.004)
Scaled effect $(0.3 \cdot \hat{\beta})$	–0.000	0.068	1.219	–0.006	–0.008	0.002
Weak IV statistic	164.2	164.2	163.1	164.3	164.3	164.3
Number of obs.	642,688	643,060	604,842	639,547	639,547	639,547
Number of firms	82,347	82,375	78,524	81,972	81,972	81,972

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 $t+3$ 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). 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.

Garcia-Appendini and Montoriol-Garriga, 2013, and Amberg et al., 2021). Note also that the increase in the payable days of bank borrowers is one channel through which the effects of bank branch closures may spread to firms that do not rely on banks for financing, and who are therefore not directly impacted by branch closures. We will return to the question of the indirect effects of branch closures below.

Finally, equity declines mildly but significantly following branch closures (column 4). The decline in overall equity is due to a statistically significant decrease in retained earnings (column 5) and a smaller, statistically insignificant increase in the other components of equity, which comprises, among other things, the proceeds from new share issues (column 6).[15] Hence, firms are not able to offset the decline in bank credit by raising more equity following branch closures. In sum, the results reported in Table 8 suggest that firms are able to offset at most a minor part of the credit-supply contraction induced by branch closures by adjusting other financial positions. We would therefore expect branch closures to have a negative impact on firms real activity. In the next section, we assess whether this is indeed the case.

[15]The coefficients in columns (5) and (6) do not add up to the coefficient in column (4) due to rounding error.

4.3 Real effects of branch closures on local firms

We assess the real effects of branch closures by estimating the baseline model with the symmetric growth rate of, in turn, sales, employment, fixed assets (PPE), and accounts receivable and inventory (AR&I) as dependent variables. We also test for effects on firms' survival rates by estimating the model with an indicator for firm exit as dependent variable.[16]

4.3.1 Direct effects

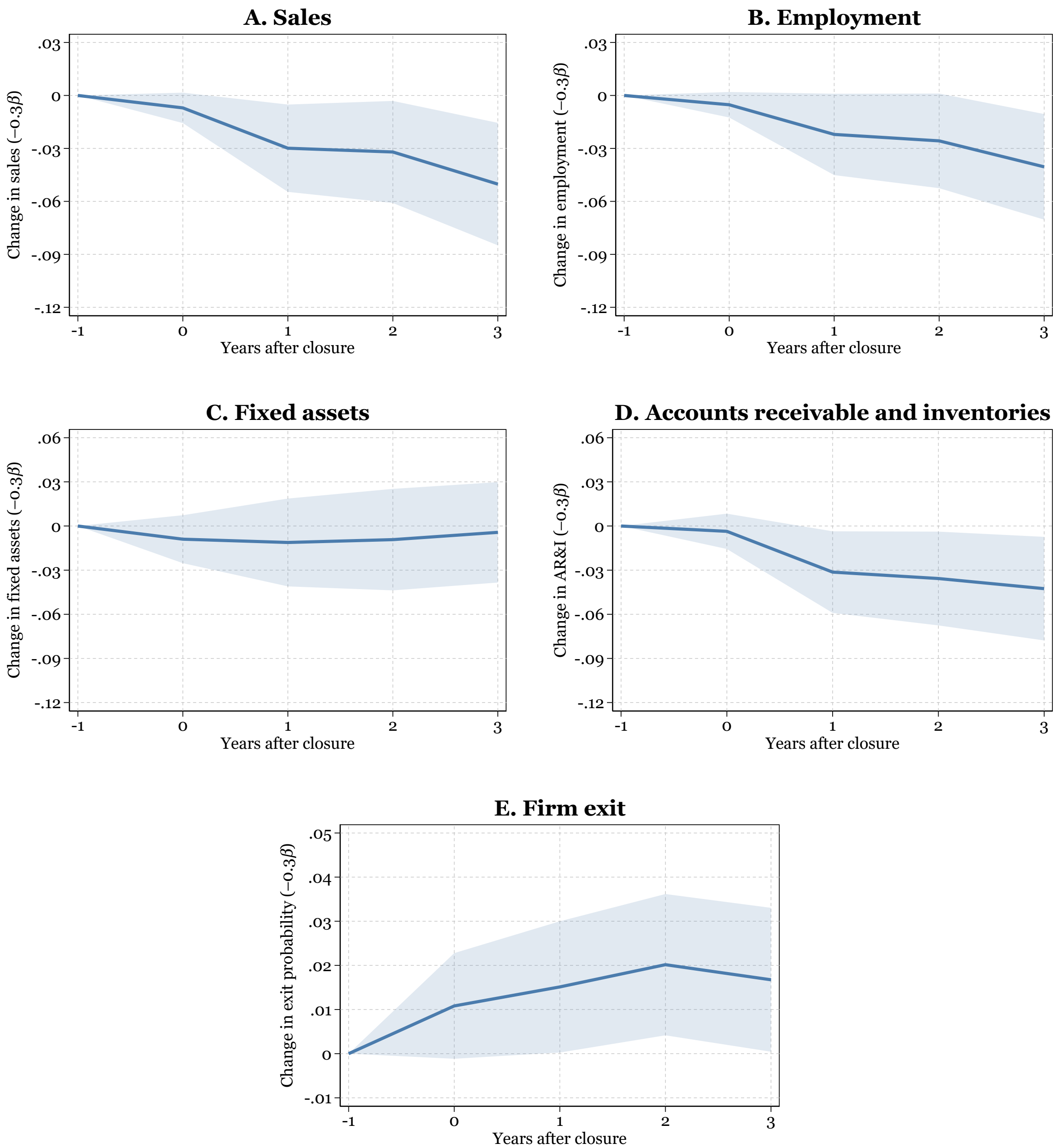
We begin by considering the direct real effects of branch closures, i.e., the effects of branch closures on real outcomes for firms that have loans and therefore are directly impacted by branch closures. The results, reported in Table 9, show that branch closures have significant negative effects on local firms' sales, employment, working-capital investments (as captured by accounts receivable and inventory), and survival probability. The magnitude of the estimates imply that the closure of 30 percent of the bank branches in a municipality causes local firms to experience a 5.1 percent decline in sales, a 4.2 percent decline in employment, a 4.9 percent decline in the stock of accounts receivable and inventories, and, finally, a 1.6 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.

The estimated effect of branch closures on fixed assets, on the other hand, is small and statistically insignificant (column 3). Our findings are thus consistent with the hypothesis that working-capital investments should be particularly affected by branch closures, as working-capital funding is frequently unsecured (or secured by low-quality collateral) and therefore more dependent on the soft information that local bank branches are meant to collect (Lian and Ma, 2020; Ivashina, Laeven and Moral-Benito, 2022). Fixed investments, on the other hand, should be less sensitive to branch closures since they are typically secured by physical collateral, which reduces the need for soft information. The previously documented result that branch closures only have a significant effect on credit supply to firms with few tangible assets corroborates this hypothesis further.

In Figure 4, we show how the real effects of branch closures evolve over time by plotting the 2SLS estimates of the respective β^h for each estimation horizon h, scaled to correspond

[16]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 $t+h$.

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 lines correspond to the respective two-stage least squares estimates of β^h obtained from the estimation of equations (4) and (2) for estimation horizons $h = 0, 1, 2, 3$. The estimates are scaled to correspond to the effect of closing 30 percent of the bank branches in a municipality. The estimation samples are restricted to firms that have a positive amount of loans in year $t-1$. Standard errors are clustered at the municipality-year level and the shaded areas represent 95-percent confidence intervals.

Table 9: Real effects of branch closures on local firms (direct effects)

	(1)	(2)	(3)	(4)	(5)
	Sales	Employ- ment	Fixed assets	AR&I	Firm exit
$\Delta Branches_{j,t}$	0.167***	0.135***	0.015	0.142**	–0.054**
	(0.059)	(0.051)	(0.058)	(0.060)	(0.022)
Scaled effect ($-0.3 \cdot \hat{\beta}$)	–0.050	–0.040	–0.004	–0.043	0.016
Weak IV statistic	165.023	165.019	164.193	164.963	165.440
Number of obs.	832,307	832,307	801,573	831,960	752,295
Number of firms	111,358	111,358	107,673	111,320	102,903

This table reports the two-stage least squares estimates of the effect of bank branch closures on local firms' sales, employment, fixed assets, accounts receivable and inventory (AR&I), and exit probability. The dependent variables are the symmetric growth rates of the respective variables between years $t-1$ and $t+3$ in columns (1)-(4) and the firm exit indicator, $FirmExit_{i,t+3}$, in column (5). The estimation sample is restricted to firms that have a positive amount of loans 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.

to the effect of closing 30 percent of the bank branches in a municipality. The estimates show that the sales, employment, and accounts receivable and inventory of local firms begin to decline about a year after branch closures, after which they keep on declining throughout the estimation period. The exit probability begins to decline immediately after branch closures, but the response only becomes statistically significant after one year. Hence, the real effects of branch closures lag the credit-supply effects by around one year. The response of fixed assets, meanwhile, is statistically insignificant at all estimation horizons.

4.3.2 Indirect effects

Having documented that bank-branch closures significantly reduce the real activity of directly affected local firms, we now assess whether branch closures in addition have indirect effects, i.e., whether they affect the real activity of local firms that do not rely on banks for funding. The motivation for considering the indirect effects of branch closures is that several papers have documented that credit-supply shocks can have substantial impacts on firms not directly affected by them, for example via lower aggregate demand and agglomeration spillovers in affected regions (Huber, 2018) and through supply chains via upstream changes in trade credit provision and relative prices (Alfaro, García-Santana and Moral-Benito, 2021).

We focus our test of indirect real effects on the aggregate-demand channel, i.e., spillovers via local aggregate demand declines that occur as a consequence of the decline in real economic activity at local firms directly affected by branch closures.[17] We implement the test by estimating the effects of branch closures on sales and employment for firms without loans that operate in non-tradable sectors and thereby depend on local demand. We also report results for firms without loans operating in tradable sectors, which amounts to a placebo test since these firms should be neither directly nor indirectly affected by branch closures. Our classification of non-tradable sectors is based on firms' primary NACE codes and follow Besley, Fontana and Limodio (2021): tradable sectors are agriculture, forestry, and fishing (A), mining and quarrying (B), manufacturing (C), and information and communication (J); all other sectors are classified as non-tradable.

The results are reported in Table 10. Column (1) shows that the effect of branch closures on the sales of non-bank firms in non-tradable sectors is statistically significant at the ten-percent level; the magnitude of the effect estimate is, moreover, almost as large as for directly affected firms. The employment response, on the other hand, is statistically insignificant (column 2). The estimates for non-bank firms operating in tradable sectors, reported in columns (3) and (4), are statistically insignificant for both sales and employment, which is what one would expect given that firms in tradable sectors do not depend on local demand conditions. Taken together, these findings are consistent with the hypothesis that branch closures have indirect effects by causing reductions in local aggregate demand, although the spillovers do not appear to go beyond the immediate impact on sales.

[17] We do not consider propagation through supply chains as we do not observe the structure of input-output linkages at the firm level, which is necessary to test for such effects.

Table 10: Real effects of branch closures on local firms (indirect effects)

	A. Non-tradable sectors		**B. Tradable sectors**	
	(1)	(2)	(3)	(4)
	Sales	Employment	Sales	Employment
$\Delta Branches_{j,t}$	0.147*	0.052	–0.086	–0.004
	(0.085)	(0.071)	(0.104)	(0.105)
Scaled effect $(-0.3 \cdot \hat{\beta})$	–0.044	–0.016	0.026	0.001
Weak IV statistic	150.296	150.307	135.901	135.911
Number of obs.	364,922	364,922	102,177	102,177
Number of firms	62,280	62,280	16,193	16,193

This table reports the two-stage least squares estimates of the effect of bank branch closures on local firms' sales and employment. The dependent variables are the symmetric growth rates of the respective variables between years $t-1$ and $t+3$. The estimation sample is restricted to firms with zero loans in year $t-1$ operating in *non-tradable* sectors in columns (1) and (2), and to firms with zero loans in year $t-1$ operating in *tradable* sectors in columns (3) and (4). 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.

5 Concluding remarks

We examine the impact of large reductions of banks' local branch networks on firms' access to credit and real economic activity. Our 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), which exploits spatial variation in the market shares of banks across municipalities and 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.

Our results suggest that the accelerating trend toward digital delivery of retail bank services—visible across the OECD and beyond—may harm credit supply to small and medium-sized firms, where lending decisions traditionally involve soft information collected through branches. Without large branch networks, banks' credit supply may tilt toward asset-backed loans (Lian and Ma, 2020) and secured credit contracts (Benmelech, Kumar and Rajan, 2022), potentially restricting new firm formation and entrepreneurship (Black and Strahan, 2002). More generally, large-scale, technology-driven disruption, even if it is beneficial overall, may be harmful to some activities and to some firms. The findings in this paper suggest that in the case of banking, 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, there may be important implications for economic growth, employment, and monetary policy of shrinking branch networks.

References

Acharya, Viral, Heitor Almeida, Filippo Ippolito, and Ander Perez. 2014. "Credit lines as monitored liquidity insurance: Theory and evidence." *Journal of Financial Economics*, 112(3): 287–319.

Acharya, Viral, Heitor Almeida, Filippo Ippolito, and Ander Perez. 2021. "Credit Lines and the Liquidity Insurance Channel." *Journal of Money, Credit and Banking*, 53(5): 901–938.

Agarwal, Sumit, and Robert Hauswald. 2010. "Distance and Private Information in Lending." *Review of Financial Studies*, 23(7): 2757–2788.

Alfaro, Laura, Manuel García-Santana, and Enrique Moral-Benito. 2021. "On the direct and indirect real effects of credit supply shocks." *Journal of Financial Economics*, 139(3): 895–921.

Almeida, Heitor, and Murillo Campello. 2007. "Financial Constraints, Asset Tangibility, and Corporate Investment." *Review of Financial Studies*, 20(5): 1429–1460.

Amberg, Niklas, Tor Jacobson, Erik von Schedvin, and Robert Townsend. 2021. "Curbing Shocks to Corporate Liquidity: The Role of Trade Credit." *Journal of Political Economy*, 129(1): 182–242.

Aretz, Kevin, Murillo Campello, and Maria-Teresa Marchica. 2020. "Access to collateral and the democratization of credit: France's reform of the Napoleonic Security Code." *The Journal of Finance*, 75(1): 45–90.

Ashcraft, Adam B. 2005. "Are Banks Really Special? New Evidence from the FDIC-Induced Failure of Healthy Banks." *American Economic Review*, 95(5): 1712–1730.

Bank Director. 2022. "2022 Technology Survey." Technical report.

Bartik, Timothy J. 1991. *Who Benefits from State and Local Economic Development Policies?* W.E. Upjohn Institute for Employment Research.

Becker, Bo, and Victoria Ivashina. 2023. "Disruption and Credit Markets." *The Journal of Finance*, 78(1): 105–139.

Benmelech, Efraim, Nitish Kumar, and Raghuram Rajan. 2022. "The secured credit premium and the issuance of secured debt." *Journal of Financial Economics*, 146(1): 143–171.

Berger, Allen. 2003. "The Economic Effects of Technological Progress: Evidence from the Banking Industry." *Journal of Money, Credit and Banking*, 35(2): 141–76.

Berger, Allen, Nathan H. Miller, Mitchell Petersen, Raghuram Rajan, and Jeremy Stein. 2005. "Does function follow organizational form? Evidence from the lending practices of large and small banks." *Journal of Financial Economics*, 76(2): 237–269.

Besley, Timothy, Nicola Fontana, and Nicola Limodio. 2021. "Antitrust Policies and Profitability in Nontradable Sectors." *American Economic Review: Insights,* 3(2): 251–65.

Black, Sandra E, and Philip E Strahan. 2002. "Entrepreneurship and bank credit availability." *The Journal of Finance*, 57(6): 2807–2833.

Bonfim, Diana, Gil Nogueira, and Steven Ongena. 2020. ""Sorry, We're Closed" Bank Branch Closures, Loan Pricing, and Information Asymmetries." *Review of Finance*, 25(4): 1211–1259.

Caggese, Andrea, Vicente Cuñat, and Daniel Metzger. 2019. "Firing the wrong workers: Financing constraints and labor misallocation." *Journal of Financial Economics*, 133(3): 589–607.

Dahl, Gordon B., Andreas Ravndal Kostøl, and Magne Mogstad. 2014. "Family Welfare Cultures." *Quarterly Journal of Economics*, 129(4): 1711–1752.

D'Andrea, Angelo, Marco Pelosi, and Enrico Sette. 2023. "When broadband comes to banks: credit supply, market structure, and information acquisition." *Market Structure, and Information Acquisition (February 8, 2023).*

Eurostat. 2023. "Individuals who used the internet, frequency of use and activities. Internet use: internet banking." Digital Society Statistics at the Regional Level.

FDIC. 2023. "Total branches." BankFind Suite, The Federal Deposit Insurance Corporation.

Garcia-Appendini, Emilia, and Judit Montoriol-Garriga. 2013. "Firms as Liquidity Providers: Evidence from the 2007-2008 Financial Crisis." *Journal of Financial Economics*, 109(1): 272–291.

Garmaise, Mark J., and Tobias J. Moskowitz. 2006. "Bank Mergers and Crime: The Real and Social Effects of Credit Market Competition." *Journal of Finance*, 61(2): 495–538.

Garrì, Iconio. 2019. "The effects of bank branch closures on credit relationships." Banca d'Italia, Working Paper No. 1254.

Gopal, Manasa, and Philipp Schnabl. 2022. "The Rise of Finance Companies and FinTech Lenders in Small Business Lending." *The Review of Financial Studies*, 35(11): 4859–4901.

Granja, João, Christian Leuz, and Raghuram G Rajan. 2022. "Going the extra mile: Distant lending and credit cycles." *The Journal of Finance*, 77(2): 1259–1324.

He, Zhiguo, Sheila Jiang, Douglas Xu, and Xiao Yin. 2022. "Investing in Bank Lending Technology: IT Spending in Banking." National Bureau of Economic Research.

Ho, Cynthia Sin Tian, and Björn Berggren. 2020. "The effect of bank branch closures on new firm formation: the Swedish case." *The Annals of Regional Science*, 65: 319–350.

Huber, Kilian. 2018. "Disentangling the Effects of a Banking Crisis: Evidence from German Firms and Counties." *American Economic Review*, 108(3): 868–98.

Imbens, Guido W., and Donald B. Rubin. 2015. *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction.* Cambridge: Cambridge University Press.

Imbens, Guido W., and Joshua D. Angrist. 1994. "Identification and Estimation of Local Average Treatment Effects." *Econometrica*, 62(2): 467–475.

IMF. 2023. "Branches of commercial banks." Financial Access Survey, The International Monetary Fund.

Ivashina, Victoria, Luc Laeven, and Enrique Moral-Benito. 2022. "Loan types and the bank lending channel." *Journal of Monetary Economics*, 126: 171–187.

Jiménez, Gabriel, Alfredo Martín-Oliver, José-Luís Peydró, Anna Toldrà-Simats, and Sergio Vicente. 2022. "Do Bank Branches Matter? Evidence From Mandatory Branch Closings." Unpublished manuscript.

Kundu, Shohini, Tyler Muir, and Jinyuan Zhang. 2024. "Diverging Banking Sector: New Facts and Macro Implications." UCLA Working Paper.

Kärnä, Anders, Agostino Manduchi, and Andreas Stephan. 2021. "Distance still matters: Local bank closures and credit availability." *International Review of Finance*, 21(4): 1503–1510.

Lewellen, Stefan, and Emily Williams. 2021. "Did technology contribute to the housing boom? Evidence from MERS." *Journal of Financial Economics*, 141(3): 1244–1261.

Lian, Chen, and Yueran Ma. 2020. "Anatomy of Corporate Borrowing Constraints*." *The Quarterly Journal of Economics*, 136(1): 229–291.

Liberti, José María, and Mitchell A Petersen. 2018. "Information: Hard and Soft." *Review of Corporate Finance Studies*, 8(1): 1–41.

Lin, Chen, Chicheng Ma, Yuchen Sun, and Yuchen Xu. 2021. "The telegraph and modern banking development, 1881–1936." *Journal of Financial Economics*, 141(2): 730–749.

Montiel Olea, José Luis, and Carolin Pflueger. 2013. "A Robust Test for Weak Instruments." *Journal of Business & Economic Statistics*, 31(3): 358–369.

Nguyen, Hoai-Luu Q. 2019. "Are Credit Markets Still Local? Evidence from Bank Branch Closings." *American Economic Journal: Applied Economics*, 11(1): 1–32.

Petersen, Mitchell A., and Raghuram G. Rajan. 1994. "The Benefits of Lending Relationships: Evidence from Small Business Data." *Journal of Finance*, 49(1): 3–37.

Petersen, Mitchell A., and Raghuram G. Rajan. 2002. "Does Distance Still Matter? The Information Revolution in Small Business Lending." *Journal of Finance*, 57(6): 2533–2570.

Ranish, Ben, Andrea Stella, and Jeffery Zhang. 2024. "Out of Sight, Out of Mind: Nearby Branch Closures and Small Business Growth." Unpublished manuscript.

Swedish Bankers' Association. 2023. "Bank and finance statistics 2022." Technical report.

Appendix A. Additional tables and figures

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

	Below median		Above median	
	$\hat{\xi}$	se($\hat{\xi}$)	$\hat{\xi}$	se($\hat{\xi}$)
A. Firm-level characteristics				
Assets (MSEK)	1.194***	0.091	1.245***	0.097
Sales (MSEK)	1.178***	0.090	1.259***	0.098
Number of employees	1.188***	0.091	1.247***	0.096
Age (years)	1.254***	0.095	1.205***	0.094
Debt/Assets	1.245***	0.095	1.193***	0.092
EBIT/Assets	1.188***	0.090	1.244***	0.095
Cash/Assets	1.184***	0.091	1.260***	0.096
Probability of default	1.253***	0.096	1.183***	0.090
B. Municipality-level characteristics				
Population (1000s)	1.086***	0.103	1.922***	0.294
Five-year population growth (%)	1.076***	0.113	1.584***	0.202
Population density	1.045***	0.109	1.892***	0.226
Branches per 1,000 inhabitants	1.629***	0.212	0.984***	0.095
Employment ratio	1.287***	0.130	1.274***	0.147
Relative labor income	1.146***	0.114	1.489***	0.202
Manufacturing share	1.445***	0.166	1.106***	0.114

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