Auditing as a Response to Bank Branch Closures

Abstract

This study examines the effect of bank branch closures on small firms' voluntary audit decisions in Sweden. As local bank branches vanish, firms lose an important source of soft information exchange, potentially prompting them to substitute with audited financial statements. Using comprehensive Swedish firm-level data, we find that firms located in areas without a bank branch are significantly more likely to adopt voluntary external audits. This suggests that auditing serves as an independent channel to convey credible information to distant lenders when local banking relationships deteriorate.

Background and Literature

Bank branch networks have contracted sharply in many countries, driven by technological change and competition. In Sweden, for example, the number of bank branches fell from 1,948 in 2003 to just 959 by 2023. Globally, branch counts per adult have also declined. These closures increase geographic distance between small firms and lenders, impeding the exchange of soft information (Agarwal & Hauswald, 2010; Liberti & Petersen, 2019). We define soft information as the local, qualitative knowledge that bank officers gather from face-to-face meetings. This type of information is especially important for small opaque firms.

Empirical studies have documented the credit supply consequences of branch closures. For instance, Agarwal and Hauswald (2010) show that greater physical distance decreases a lender's ability to gather proprietary information, leading to higher borrowing costs for distant firms. Degryse & Ongena, (2005) similarly find that loan rates rise with distance between firm and competing banks, reflecting spatial pricing in credit. More recently, Amberg & Becker, (2024) use Swedish data to demonstrate that branch closures substantially reduce corporate lending to small, collateral-poor firms. Taken together, we see that branch closures tend to diminish access to bank credit for smaller firms, prompting them to seek alternative assurances of creditworthiness, i.e. we hypothesize that firms will respond to the decline in soft information exchange by moving to more hard information.

Auditors can provide one such alternative of creating hard information for the firms (Kano et al., 2011). An external audit is a costly mechanism that bolsters the credibility of a firm's financial reports. For small private firms, where auditing is often optional, choosing to hire an auditor is a way to signal reliability and transparency to external stakeholders. Prior literature emphasizes that voluntary auditing is driven by agency and information considerations. For example, Weik et al. (2018) find that in Germany voluntary audit adoption is higher when owners are uninvolved in management or when financial statements are valued for decision-making, and lower when firms outsource accounting work. Patel & Dahlin (2021) similarly consider Swedish small firms and ask whether voluntary auditing improves venture outcomes and find that opting out of audits slightly improves survival odds except when firms are highly volatile, whereas voluntary auditing allows firms to take on higher debt ratios at the cost of lower sales and profits. In other words, auditors can facilitate financing but might not necessarily boost short-run profitability.

None of the existing studies, however, have linked bank proximity and audit choice directly. Given that branch closures are largely exogenous to any one firm, they offer a suitable natural experiment. In our context, Swedish law made auditing optional for small firms starting in 2010, meaning that some firms could forego audits without penalty. We use this setting to identify whether increased distance to banks due to branch closures prompts those firms to choose audits they otherwise might skip. Intuitively, when a local bank branch disappears, the soft information channel is severed. Auditors can partially substitute by independently verifying financial information, making it "trustworthy" to remote banks.

Research Question and Contribution

In this paper, we examine whether bank branch closures lead small firms to adopt voluntary audits. This extends the debate on relationship lending and competition. Theoretical models predict that banks may underinvest in soft-information lending under intense competition (Boot & Thakor, 2000), and

empirical work confirms that credit to opaque firms declines with distance. We contribute by showing that firms actively respond to this loss of soft links.

Specifically, we test whether the probability of voluntary audit adoption increases when the last bank branch in the municipality closes. Our setting satisfies key identification criteria: branch closures are plausibly exogenous to individual firms' audit choices, and the voluntary audit law provides variation in auditing requirements across firm sizes.

Conceptually, our study situates itself at the intersection of two literatures. From the financial intermediation side, it aligns with research on the importance of geographic proximity and soft information in bank-firm relationships. From the accounting side, it builds on voluntary audit choice research (e.g., Weik et al., 2018; Patel & Dahlin, 2021) by introducing a new demand-side driver: banking infrastructure. We thus address a gap: prior work looks at firm-internal determinants of auditing or its outcomes, but not at external financial environment factors like branch proximity.

Our empirical findings confirm the hypothesis: absence of a local bank branch is positively associated with audit adoption. This result is robust to various specifications (linear probability and logit models, with firm fixed effects). As Table 5 shows, the results complement prior studies. For example, Weik et al. (2018) report that audit adoption depends on ownership and accounting outsourcing, whereas we find it also depends on banking market structure. Patel and Dahlin (2021) find that voluntary audits allow firms to raise more debt; our results suggest one mechanism for that debt increase is firms proactively using audits to reassure distant lenders.

Data

The empirical analysis draws on a panel dataset of Swedish private firms covering the period 2012–2023. The dataset is constructed by linking firm-level financial and organizational data with information on bank branch presence at the municipality level. After applying standard sample restrictions, the final sample comprises 1,787,595 firm-year observations from 340,247 unique firms, with firms observed for an average of 5.25 years (Table 1).

Table 1: Sample Composition and Panel Structure

Panel A: Overall Sample Characteristics	
Total firm-year observations	1,787,595
Unique firms	340,247
Average years per firm	5.25
Minimum years per firm	1
Maximum years per firm	12
Sample period	2012 – 2023
Panel B: Bank branch Status	
Observations with bank branch	1,768,429
Observations without bank branch	19,166
Share without bank branch	1.07%
Share with bank branch	98.93%

Notes: This table reports the composition of the sample used in the empirical analysis. The dataset consists of 1,787,595 firm-year observations from 340,247 unique firms over the period 2012–2023. Panel A describes the structure of the panel dataset, including the average number of years each firm appears in the sample. Panel B reports the distribution of observations by bank branch status.

The main explanatory variable, *No Bank Branch*, is an indicator equal to one if the firm is located in a municipality in which all bank branches have closed — that is, the municipality has no remaining physical bank presence. Of the 1,787,595 observations in the sample, 19,166 (1.07%) correspond to firms operating in such municipalities (Table 1).

The primary outcome of interest is voluntary audit adoption. Following the abolition of mandatory audit requirements for small private firms in Sweden, firms below statutory thresholds may choose whether to engage an external auditor. The dependent variable, *Adopt Audit*, is an indicator equal to one in the year a firm voluntarily adopts an external audit, and zero otherwise. Approximately 0.59% of firm-year observations record an audit adoption event, while 21.07% of all observations reflect firms that hold an active audit in a given year (Table 2).

Table 2: Descriptive Statistics

Variable	Mean	SD	Min	Median	Max	N
Adopt Audit	0.0059	0.0764	0	0	1	1,787,595
Audit	0.2107	0.4078	0	0	1	1,787,595
No Bank branch	0.0107	0.1030	0	0	1	1,787,595
Log Firm Age	2.1961	0.8005	0.693	2.079	5.063	1,787,595
Log Employees	0.7296	0.4705	0	0.693	7.561	1,787,595
Log Sales	6.8588	1.0017	4.625	6.964	15.212	1,787,595
Log Assets	6.6384	1.1079	0	6.633	19.317	1,787,595
Bank Loan	0.1947	0.3960	0	0	1	1,787,595
Equity Injection	0.6640	0.4723	0	1	1	1,787,595

Notes: This table reports descriptive statistics for the main variables used in the analysis. *Adopt Audit* is an indicator variable equal to one if a firm voluntarily adopts an external audit in a given year. *Audit* equals one if the firm has an audit in that year. *No Bank branch* equals one if the firm is located in no bank branch municipality. *Log Firm Age* is the natural logarithm of firm age measured in years. *Log Employees*, *Log Sales*, and *Log Assets* are the natural logarithms of the number of employees, total sales, and total assets, respectively. *Bank Loan* is an indicator variable equal to one if the firm has an outstanding bank loan. *Equity Injection* The variable equal to one if the firm records a positive owner capital contribution in year t. The indicator is constructed using several balance sheet items that capture equity injections from shareholders, including shareholders' contributions, share premium reserve, conditional shareholders' contributions, and subscribed capital unpaid. The sample includes 1,787,595 firm-year observations.

Firm-level controls are drawn from annual financial statements and include the natural logarithm of firm age, number of employees, total sales, and total assets. Two additional indicators capture financing activity: *Bank Loan*, equal to one if the firm carries an outstanding bank loan, and *Equity Injection*, equal to one if the firm records a positive owner capital contribution in year *t*, constructed from balance sheet items including shareholders' contributions, share premium reserve, conditional shareholders' contributions, and subscribed capital unpaid (Table 2). Pairwise Pearson correlations among the main variables are reported in Table 3. The correlations are modest in magnitude, suggesting that multicollinearity is unlikely to pose a concern in the regression analysis.

Table 3: Pairwise Correlation Matrix

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Adopt Audit	1							
(2) No Bank branch	-0.002*	1						
(3) Log Firm Age	-0.029*	0.013*	1					
(4) Log Employees	0.056*	-0.005*	-0.010*	1				
(5) Log Sales	0.055*	0.002*	-0.066*	0.552*	1			
(6) Log Assets	0.046*	-0.001	0.153*	0.172*	0.469*	1		
(7) Bank Loan	0.019*	0.018*	0.033*	0.132*	0.188*	0.127*	1	
(8) Equity Injection	0.012*	0.039*	-0.049*	-0.067*	-0.017*	-0.026*	-0.031*	1

Notes: This table reports pairwise Pearson correlations between the main variables used in the analysis. Variable definitions follow Table 2. An asterisk (*) indicates statistical significance at the 10% level. The correlations are calculated using the full sample of 1,787,595 firm-year observations.

Method

Identification exploits cross-municipal variation in the timing of complete bank branch exit. A municipality is classified as *treated* once all of its bank branches have closed, generating a binary treatment indicator that switches on permanently at the time of complete exit. Firms located in municipalities that retained at least one bank branch throughout the sample period constitute the control group.

A key challenge in this setting is that treated and control firms may differ systematically along pre-treatment observable characteristics, potentially confounding the estimated treatment effect. To address this, we apply *entropy balancing*, a pre-processing reweighting procedure that assigns weights to control observations such that the first and second moments of the covariate distribution in the reweighted control group exactly match those of the treated group (Hainmueller, 2012). The balancing covariates include log assets, log age, log sales, log employees, equity injection, and bank loan status.

Table 4 reports covariate balance before and after entropy balancing. Panel A reveals meaningful pre-weighting differences between treated and control firms: treated firms are, on average, smaller in terms of assets, slightly younger, less likely to have received an equity injection, and more likely to carry a bank loan. After applying entropy balancing weights, Panel B shows that all differences in means and variances are reduced to negligible magnitudes, confirming that the reweighted control group closely resembles the treated group on all observed dimensions.

Table 4: Covariate Balance Before and After Entropy Balancing

	Treated			Control	
Variable	Mean	Variance	Diff.	Mean	Variance
Panel A: Before weighting					
Log assets	6.501	0.8906	0.1396***	6.640	1.026
Log age	2.163	0.6345	0.0311***	2.195	0.5483
Log sales	6.853	0.6460	0.0056	6.859	0.7708
Log employees	0.7347	0.1487	-0.0049**	0.7298	0.1725
Equity injection	0.5835	0.1400	0.0795***	0.6630	0.1114
Bank loan	0.2970	0.1605	-0.1044***	0.1926	0.1101
Panel B: After weighting (entropy balancing)					
Log assets	6.501	0.8906	-0.0007	6.501	0.8922
Log age	2.163	0.6345	-0.0000	2.163	0.6345
Log sales	6.853	0.6460	-0.0001	6.853	0.6467
Log employees	0.7347	0.1487	-0.0000	0.7347	0.1489
Equity injection	0.5835	0.1400	-0.0001	0.5836	0.1400
Bank loan	0.2970	0.1605	0.0001	0.2969	0.1604
Treated units			35,819		
Control units			1,745,474		

Notes: This table reports covariate balance between treated and control firms before and after entropy balancing. A firm is considered as treated if it is located in a municipality that experienced all bank branch closures in the municipality. Panel A reports unweighted sample statistics. Panel B reports statistics after applying entropy balancing weights (`_webal`) to the control group. The table reports the mean and variance of each covariate for treated and control firms. The column "Diff." reports the difference in means between control and treated units. In Panel A, differences are based on two–sample t-tests assuming equal variances. In Panel B, differences are obtained from weighted regressions of each covariate on the treatment indicator using entropy balancing weights. Entropy balancing reweights control observations so that the first and second moments of the covariates match those of the treated group. Significance levels are denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

The baseline estimating equation takes the form:

$$Adopt_{it} = \beta_1 NoBankBranch_{tm} + \gamma X_{it} + \alpha_i + \delta_t + \mu_m + \varepsilon_{it}$$

where i indexes firms, t indexes years, and m indexes municipalities. The coefficient of interest, β_1 captures the effect of complete bank branch exit on the probability of voluntary audit adoption. X_{it} is a vector of time-varying firm-level controls. α_i, δ_t, and μ_m denote firm, year, and municipality fixed effects. Firm fixed effects absorb time-invariant heterogeneity across firms, year fixed effects control for aggregate time trends, and municipality fixed effects account for time-invariant local characteristics that may affect both bank exit and auditing decisions.

Result

The main specifications are reported in Table 5. Columns (1) and (2) present linear probability models (LPM) estimated via reghdfe, with standard errors clustered at the municipality level to account for within-municipality correlation in treatment exposure. The LPM is weighted using entropy balancing weights.

Columns (3) and (4) report conditional fixed-effects logistic regressions estimated via xtlogit, fe. Because the conditional logit estimator conditions on within-firm variation, it automatically drops firms that never adopt an audit, the effective sample is accordingly restricted to 57,305 observations from 10,038 firms. An average marginal effect (AME) for the *No Bank Branch* indicator is computed in column (3) to facilitate the interpretation of the logit coefficient in probability terms. Singleton observations, which do not contribute to identifying the fixed-effects regression, are dropped throughout to prevent overstated statistical significance and incorrect inference (Correia, 2016).

Table 5: Bank branchs and Voluntary Audit Adoption

	Linear Probability Model		FE Logit	
	(1) Adopt Audit	(2) Adopt Audit	(3) Adopt Audit	(4) Adopt Audit
No Bank branch	0.00401***	0.00391***	2.718***	2.717***
	(0.00082)	(0.00082)	(0.457)	(0.461)
Log Firm Age		0.00053		0.010
		(0.00108)		(0.076)
Log Employees		0.00281**		0.496***
		(0.00117)		(0.040)
Log Sales		0.00098**		0.065***
		(0.00043)		(0.022)
Bank Loan		-0.00016		0.090**
		(0.00075)		(0.044)
Equity Injection		0.00458***		1.060***
		(0.00079)		(0.041)
Average Marginal Effect (No Bank)			0.504***	
			(0.109)	
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Observations	1,750,970	1,750,970	57,305	57,305
Number of Firms			10,038	10,038
Clusters (Municipalities)	290	290		
R-squared	0.228	0.228		
Within R^2	0.0003	0.0010		
Log Likelihood			-15,656.50	-15,166.75
LR χ^2			597.63	1,577.13

Notes: Columns (1) and (2) report linear probability models estimated using `reghdfe`. Columns (3) and (4) report conditional fixed-effects logistic regressions estimated using `xtlogit, fe`. The dependent variable equals one if a firm voluntarily adopts an audit. All specifications include firm, year, and municipality fixed effects. Linear models are weighted using entropy balancing weights (`_webal`) and standard errors are clustered at the municipality level. Standard errors are reported in parentheses. The average marginal effect (AME) in column (3) is computed using `margins`. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The estimation drops singleton observations that do not contribute to identifying the fixed-effects regression.

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