

When Revenue Falls, Branches Follow: The Durbin Amendment, Bank Restructuring, and Local Banking Employment

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Abstract

The Durbin Amendment (2011) capped debit card interchange fees for banks exceeding \$10 billion in assets. We exploit county-level variation in pre-Durbin deposit exposure to affected banks to estimate the regulation's impact on branch infrastructure and banking employment. High-exposure counties lost nearly 4 percent of their branches, with effects growing steadily through 2019. Yet their banking workforces remained intact: the employment coefficient is near zero and statistically insignificant across all specifications. This divergence suggests banks consolidated operations rather than eliminating workers. The null employment result is robust to alternative bandwidths, exposure measures, clustering, and county-specific trends. A triple-difference comparing banking to non-banking employment within counties confirms the null. These findings challenge the hypothesis that branch-network rationalization mechanically reduces banking employment.

JEL Codes: G21, G28, J23, O33

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1. Introduction

Between 1990 and 2010, the number of automated teller machines in the United States tripled to roughly 400,000. Over the same period, bank teller employment rose from 500,000 to nearly 600,000. This paradox—technology adoption increasing the demand for the very workers it was expected to replace—became the canonical counterexample to automation anxiety (Bessen, 2015; Autor, 2015). The mechanism seemed clear: ATMs reduced the cost of operating a bank branch, banks responded by opening more branches to compete for deposits, and tellers shifted from routine cash-handling to higher-value relationship banking (Autor et al., 2003).

But since 2010, bank teller employment has fallen by more than 25 percent. What changed? This paper tests whether a regulatory shock to bank revenue—the Durbin Amendment to the Dodd-Frank Act—contributed to this decline by forcing large banks to restructure their physical infrastructure. We find that while the Durbin Amendment did accelerate branch closures, it did *not* reduce county-level banking employment. This divergence—fewer branches but stable employment—reveals that the relationship between bank infrastructure and labor is more flexible than a simple branch-equals-workers model would predict.

The Durbin Amendment, effective October 2011, capped debit card interchange fees at approximately 21 cents plus 0.05 percent of the transaction value for banks with total assets exceeding \$10 billion. This threshold created a sharp regulatory divide: large banks lost an estimated \$6–8 billion in annual interchange revenue, while community banks below the threshold were exempt (Hayashi, 2012; Wang et al., 2014). The revenue shock forced treated banks to adjust margins through a combination of higher account fees (Kay et al., 2014), reduced services, and—we argue—rationalization of their branch networks and workforces.

We exploit this regulatory shock using a Bartik-style difference-in-differences design. Our treatment measure is the county-level share of deposits held in Durbin-affected banks (those with assets exceeding \$10 billion) as of June 2010, before the amendment’s implementation. Counties with a higher pre-Durbin concentration of deposits in large banks experienced greater exposure to the interchange revenue shock. We then estimate how this cross-sectional exposure, interacted with the post-2011 indicator, affected county-level banking outcomes: branch counts (from the FDIC Summary of Deposits), banking employment (from the BLS Quarterly Census of Employment and Wages), and banking wages.

Our event-study specification traces out year-by-year effects from 2005 to 2019, with 2010 as the reference year, providing five pre-treatment periods to test the parallel trends assumption. We supplement the baseline difference-in-differences with a triple-difference (DDD) design that compares banking employment to non-banking employment (retail and

manufacturing) within the same counties, absorbing county-specific macro shocks such as differential recovery from the 2008 financial crisis.

Our central finding is a striking divergence between infrastructure and employment. Counties more exposed to Durbin-affected banks experienced a significant and growing decline in branches per capita: the event study shows effects of -0.087 log points (approximately 8.3 percent) by 2012–2019, growing to -0.22 log points by 2019. Yet banking employment in the same counties shows no corresponding decline—the estimated effect is near zero (0.013 , $p = 0.68$) and statistically insignificant across all specifications, bandwidths, and clustering approaches. The triple-difference comparing banking to non-banking employment within the same counties similarly yields a null effect (-0.002 , $p = 0.96$).

This null employment result is robust and precisely estimated. The 95 percent confidence interval rules out effects larger than approximately 7 percent in either direction, meaning we can reject the hypothesis that Durbin exposure caused economically meaningful employment losses at the county level. We discuss possible mechanisms: banks may have consolidated workers into fewer remaining branches, reallocated staff across branches within metro areas, or substituted toward non-branch functions. The finding that branch closures need not translate into employment losses has implications for understanding how firms adjust their physical infrastructure without proportional labor force reductions.

We also document that in high-exposure counties, Durbin-affected banks maintained or grew their deposit base rather than losing deposits to exempt competitors, contrary to the simple reallocation hypothesis. This suggests that large banks’ brand advantages and product differentiation insulated them from competitive pressure despite the interchange revenue loss.

This paper contributes to several literatures. First, we extend the Durbin Amendment literature beyond its focus on bank profitability and consumer fees (Kay et al., 2014; Wang et al., 2014; Mukharlyamov and Sarin, 2023) by documenting the regulation’s differential effects on bank physical infrastructure versus employment—showing these margins can move independently. Second, we contribute to the automation-employment debate (Acemoglu and Restrepo, 2019; Autor, 2015; Bessen, 2015) with a null result that challenges the assumption that branch closures mechanically reduce teller employment. Third, we contribute to the literature on the real effects of financial regulation (Drechsler et al., 2017; Jiang, 2020) by documenting how a regulation targeting electronic payments affected bank organizational structure but not local labor markets. We review each of these literatures in detail in [Section 3](#).

We note important limitations. The branch event study shows statistically significant pre-trends in 2005–2006, raising concerns about differential pre-existing trends between high- and low-exposure counties. While the near-treatment-year coefficients (2007–2009) are close

to zero and insignificant, the earlier pre-trends suggest caution in interpreting the branch results as purely causal. The employment null, by contrast, is not subject to this concern—the null holds regardless of the pre-trend pattern.

The remainder of the paper proceeds as follows. [Section 2](#) describes the institutional background of the Durbin Amendment and the banking labor market. [Section 4](#) outlines the conceptual framework linking interchange revenue to branch infrastructure and employment. [Section 5](#) describes the data sources and construction of the analysis panel. [Section 6](#) presents the empirical strategy. [Section 7](#) reports the main results, and [Section 8](#) provides robustness checks. [Section 9](#) discusses the implications for the automation debate. [Section 10](#) concludes.

2. Institutional Background

2.1 The Durbin Amendment and Interchange Fee Regulation

The Durbin Amendment was enacted as Section 1075 of the Dodd-Frank Wall Street Reform and Consumer Protection Act, signed into law on July 21, 2010. The provision directed the Federal Reserve Board to establish standards for assessing whether the interchange fees charged by payment card issuers for debit card transactions are “reasonable and proportional” to the cost incurred by the issuer. The Federal Reserve issued its final rule (Regulation II) on June 29, 2011, with an effective date of October 1, 2011.

Under Regulation II, the maximum permissible interchange fee was set at 21 cents plus 5 basis points (0.05 percent) of the transaction value, with an additional 1-cent adjustment for issuers meeting certain fraud-prevention standards. Prior to the regulation, the average interchange fee for a signature debit transaction was approximately 44 cents, and for a PIN debit transaction approximately 23 cents ([Hayashi, 2012](#)). The regulation therefore represented a substantial reduction in interchange revenue, particularly for signature debit transactions.

Critically, the Durbin Amendment included an exemption for banks and credit unions with total assets below \$10 billion. This threshold created a two-tier system: large banks (approximately 100 institutions) faced the interchange cap, while the vast majority of community banks and credit unions (approximately 6,000 institutions) were exempt and could continue to charge network-negotiated interchange rates ([Wang et al., 2014](#)).

The \$10 billion threshold was not indexed for inflation and was determined by total consolidated assets as reported in Call Reports. While the threshold created theoretical incentives for banks near the boundary to manage their asset size strategically, empirical evidence suggests limited bunching behavior, as the costs of maintaining artificially low asset levels generally exceeded the interchange revenue benefits ([Kay et al., 2014](#)).

2.2 Revenue Impact on Large Banks

The interchange fee cap reduced debit interchange revenue for affected banks by an estimated \$6–8 billion per year in aggregate (Hayashi, 2012). For individual institutions, the magnitude depended on their debit card transaction volumes. The largest banks—JPMorgan Chase, Bank of America, Wells Fargo, and Citigroup—bore the largest absolute losses, though the revenue represented a small fraction of their total income.

For mid-size banks near the \$10 billion threshold, the interchange revenue loss was proportionally more significant. These institutions faced a discrete jump in regulatory burden at the threshold, potentially affecting their competitive position relative to just-below-threshold institutions that retained full interchange revenue.

Banks responded to the revenue shock through multiple channels. Kay et al. (2014) document that affected banks raised checking account fees and reduced the availability of free checking, partially offsetting the interchange revenue loss. However, the offset was incomplete: treated banks did not fully recover the lost interchange income through fee increases alone. This incomplete pass-through to consumers left banks with a residual revenue gap that, we argue, contributed to physical infrastructure rationalization.

2.3 The Bank Branch and Teller Labor Market

The U.S. banking industry has undergone dramatic structural transformation since the wave of interstate banking deregulation in the 1990s (Jayaratne and Strahan, 1996; Kroszner and Strahan, 1999). The total number of FDIC-insured institutions fell from approximately 18,000 in 1985 to fewer than 5,000 by 2020, while the number of branch offices initially expanded—from roughly 60,000 in 1990 to a peak of approximately 100,000 in 2009—before declining to about 80,000 by 2020.

The teller occupation (SOC 43-3071) has historically been one of the largest in the U.S. economy, with peak employment of approximately 600,000 workers. Tellers are disproportionately female (approximately 75 percent), have median educational attainment of a high school diploma or some college, and earn median wages significantly below the national average (Bureau of Labor Statistics, 2023). The occupation thus represents a significant source of middle-skill employment, particularly for women in communities where banking represents a major employer.

Bessen (2015) documented the apparent paradox that ATM proliferation from the 1970s through the 2000s did not reduce teller employment. The mechanism he identified was branch expansion: ATMs lowered the operating cost per branch (by reducing the number of tellers required per branch from roughly 20 to 13 between 1988 and 2004), enabling banks to open

more branches to compete for deposits. The total number of tellers remained stable or grew because the extensive margin (more branches) offset the intensive margin (fewer tellers per branch).

This branch-expansion channel, however, depended on the profitability of maintaining physical branches. As long as branches generated sufficient revenue—through deposit gathering, cross-selling of financial products, and interchange fees on debit transactions—banks had incentives to maintain and expand their branch networks. The Durbin Amendment reduced one component of this revenue, potentially tipping the cost-benefit calculus for marginal branches.

3. Related Literature

This paper connects to three distinct literatures: the economics of the Durbin Amendment and interchange fee regulation, the automation-employment nexus in banking, and the broader literature on the real effects of financial regulation on labor markets.

The Durbin Amendment and interchange regulation. A growing body of work examines the consequences of the Durbin Amendment for bank behavior and consumer welfare. [Kay et al. \(2014\)](#) provide the most comprehensive analysis of bank-level responses, documenting that affected banks raised checking account fees, reduced free checking availability, and increased minimum balance requirements, partially offsetting the interchange revenue loss. They estimate that treated banks recovered approximately 30–50 percent of lost interchange income through higher fees. [Wang et al. \(2014\)](#) survey merchants and find that most merchants did not pass through the interchange savings to consumers in the form of lower prices, undermining one of the regulation’s primary policy rationales.

[Mukharlyamov and Sarin \(2023\)](#) study the broader effects on financial intermediation, documenting that the Durbin Amendment reduced lending by affected banks and shifted the composition of banking activity. They find evidence that the regulation’s costs extended beyond interchange revenue to affect banks’ overall business models. Our paper complements this work by examining a dimension that the existing literature has largely ignored: the regulation’s effects on bank physical infrastructure and local labor markets. While the profitability and consumer-fee consequences of Durbin are well-documented, the employment implications have received almost no attention.

Automation, technology, and banking employment. The relationship between technology and employment in banking has been a canonical case study in the automation literature

since [Autor et al.’s \(2003\)](#) analysis of computerization and task substitution. [Bessen \(2015\)](#) provided the most influential account of the ATM-teller relationship, arguing that ATMs did not reduce teller employment because they enabled branch expansion. His mechanism was straightforward: ATMs reduced the cost per branch, making it profitable to open more branches, and the net effect on teller employment was approximately zero.

Our paper offers a natural complement to Bessen’s analysis. Where Bessen studied a cost-reducing technology (ATMs), we study a revenue-reducing regulation (Durbin). Both channel their effects through the branch network. But the mechanisms differ: ATMs made each branch cheaper, encouraging expansion; Durbin made each branch less profitable, encouraging closure. Our finding that branch closures did not reduce employment suggests that the branch-employment link is more flexible than the ATM literature implies—banks can adjust their branch networks without proportional workforce changes in either direction.

More broadly, [Acemoglu and Restrepo \(2019\)](#) develop a task-based framework in which automation displaces workers from specific tasks but may create new tasks that increase labor demand. [Autor \(2015\)](#) emphasizes the distinction between routine tasks (susceptible to automation) and non-routine tasks (complementary to technology). Our null result is consistent with both frameworks: if branch closure eliminates some routine teller tasks but banks simultaneously create new tasks (digital support, advisory services), the net employment effect may be negligible at the county level.

Financial regulation and real economic outcomes. A large literature examines how financial regulation affects real economic activity beyond the banking sector itself. [Jayaratne and Strahan \(1996\)](#) showed that interstate banking deregulation accelerated state economic growth, establishing that bank regulatory changes can have substantial real effects. [Drechsler et al. \(2017\)](#) study the deposits channel of monetary policy, showing that banks adjust deposit rates and branch networks in response to interest rate changes. [Jiang \(2020\)](#) documents how changes in bank funding affect lending and real investment.

Our paper differs from this literature in an important respect: most studies focus on how financial regulation affects the broader economy through credit and capital allocation channels. We focus instead on the direct labor market consequences for the banking sector itself—asking whether a regulation that reduced bank revenue led to bank job losses. This direct-effect question is distinct from the indirect-effect question (whether Durbin reduced lending, which reduced investment, which reduced employment in other sectors). Our null finding on the direct channel suggests that even large revenue shocks to financial institutions may not translate into local labor market disruption, at least when the affected firms are large enough to adjust through internal reallocation rather than layoffs.

4. Conceptual Framework

We present a simple framework linking interchange revenue regulation to bank employment through branch-network decisions.

Consider a bank operating in multiple counties, choosing the number of branches B_c in each county c to maximize profits:

$$\pi_c = R_c(B_c) + I_c(B_c) - C_c(B_c) \quad (1)$$

where $R_c(B_c)$ represents non-interchange revenue (interest income, fees, cross-selling) that increases with branch presence, $I_c(B_c)$ is debit interchange revenue associated with branch-based account holders, and $C_c(B_c)$ is the cost of operating branches, including teller wages.

The profit-maximizing branch network satisfies $R'_c(B_c^*) + I'_c(B_c^*) = C'_c(B_c^*)$. The Durbin Amendment reduces $I_c(\cdot)$ for treated banks, shifting the optimal branch count downward:

$$\Delta B_c^* \approx -\frac{\Delta I'_c(B_c^*)}{C''_c(B_c^*) - R''_c(B_c^*) - I''_c(B_c^*)} < 0 \quad (2)$$

provided the second-order conditions hold (i.e., marginal costs increase faster than marginal revenue in branches). The reduction in branches is larger for banks with higher baseline interchange revenue per branch and for marginal branches where the revenue-cost gap was smallest.

Each branch employs $L_c = \ell(B_c)$ workers, primarily tellers. Reducing branches reduces employment:

$$\Delta L_c = \ell'(B_c^*) \cdot \Delta B_c^* < 0 \quad (3)$$

At the county level, the aggregate effect depends on the share of local banking activity conducted by Durbin-affected banks. Define:

$$\text{Exposure}_c = \frac{\sum_{b \in \text{Treated}} D_{bc,2010}}{\sum_b D_{bc,2010}} \quad (4)$$

where $D_{bc,2010}$ is the deposits of bank b in county c as of June 2010. Counties with higher exposure experience larger reductions in branches and employment.

The framework generates three testable predictions:

Prediction 1: Higher Durbin exposure leads to fewer bank branches and lower banking employment after 2011.

Prediction 2: Effects should be concentrated in the banking sector; non-banking employment in the same counties should be unaffected.

Prediction 3: Deposits should reallocate from treated to exempt banks, as consumers seek alternative banking relationships when branches close.

5. Data

We construct a county-year panel spanning 2005–2019 by merging four primary data sources.

5.1 FDIC Summary of Deposits

The Summary of Deposits (SOD) is an annual survey of branch office deposits conducted by the Federal Deposit Insurance Corporation as of June 30 each year. The SOD provides the universe of branches for all FDIC-insured institutions, including their geographic location (county FIPS code), deposit levels, branch type (full-service, limited-service), and the FDIC certificate number (CERT) of the parent institution. We use SOD data from 2005 to 2019 to construct county-level measures of branch counts, deposits, and the composition of banking activity between Durbin-affected and exempt institutions.

5.2 FDIC Financial Data

We obtain bank-level financial data from the FDIC’s quarterly Call Reports, accessed through the FDIC BankFind API. The key variable is total consolidated assets, which we use to classify banks as Durbin-affected (assets $>$ \$10 billion) or exempt (assets \leq \$10 billion) based on their June 2010 position—the last reporting period before the Durbin Amendment’s implementation timeline became clear.

Using the June 2010 classification serves two purposes. First, it prevents endogenous bank responses from contaminating the treatment definition: banks that subsequently grew above or shrank below the threshold in response to Durbin incentives are classified based on their pre-treatment status. Second, it aligns with the regulatory timeline, as the threshold is evaluated based on the most recent quarterly report at the time of the final rule.

5.3 BLS Quarterly Census of Employment and Wages

The QCEW provides county-level employment and wage data by industry classification, based on administrative records from state unemployment insurance programs. We extract annual data for NAICS 522110 (Commercial Banking) as our primary outcome, along with NAICS 44-45 (Retail Trade), 31-33 (Manufacturing), and 62 (Health Care and Social Assistance) as placebo sectors for the triple-difference design. The QCEW covers virtually all employers

subject to state unemployment insurance laws, providing near-complete coverage of the banking workforce.

5.4 Census Population Estimates

We obtain county-level population estimates from the Census Bureau’s Population Estimates Program to construct per-capita measures of branches and banking employment. The Population Estimates Program produces annual estimates of the resident population for all U.S. counties, based on the decennial census updated with administrative records on births, deaths, and migration. Per-capita normalization is essential in our setting because county population varies by several orders of magnitude: the mean county in our sample has approximately 150,000 residents, but the distribution is highly right-skewed, with counties ranging from fewer than 1,000 to over 10 million residents. Without normalization, large counties would dominate the analysis, and the relationship between Durbin exposure and branch counts would be confounded by county size differences.

5.5 Durbin Exposure Construction

The key treatment variable is the county-level *Durbin Exposure*: the share of total county deposits held in banks with assets exceeding \$10 billion, measured as of June 2010:

$$\text{DurbinExposure}_c = \frac{\sum_{b:A_b,2010 > \$10B} \text{Deposits}_{bc,2010}}{\sum_b \text{Deposits}_{bc,2010}} \quad (5)$$

where the summation is over all banks b with branches in county c .

5.6 Summary Statistics

[Table 1](#) presents summary statistics for the analysis panel, split by pre-Durbin (2005–2010) and post-Durbin (2012–2019) periods. The mean county has approximately 47 bank branches and 650 banking employees in the pre-period. Both variables show modest declines in the post-period, consistent with the national trend toward consolidation visible in [Figure 7](#) in the appendix.

Average Durbin exposure is 0.35, meaning the typical county has about 35 percent of its deposits in banks exceeding the \$10 billion threshold. The standard deviation of 0.27 reflects substantial cross-county variation, driven by the heterogeneous presence of large national banks across U.S. geographies. Rural counties in the Great Plains and Mountain West tend to have low exposure (deposits primarily in community banks), while suburban

Table 1: Summary Statistics

	Pre-Durbin (2005–2010)		Post-Durbin (2012–2019)	
	Mean	(SD)	Mean	(SD)
Bank Branches	46.93	(96.47)	44.86	(94.79)
Banking Employment	657.76	(2051.42)	656.59	(2297.22)
Average Weekly Wage (\$)	743.82	(199.70)	948.38	(295.46)
Population	151143.84	(396004.53)	158917.70	(416108.10)
Durbin Exposure	0.35	(0.27)	0.35	(0.27)
Branches per 100K	45.88	(26.23)	43.03	(26.06)
Banking Emp. per 100K	478.47	(294.66)	426.28	(286.48)
County-years	10,950		12,625	
Counties	2,499			

Notes: Banking employment and wages from BLS QCEW (NAICS 522110). Branch counts from FDIC Summary of Deposits. Durbin Exposure = share of 2010 county deposits in banks with assets > \$10 billion. Summary statistics exclude the transition year 2011; the full regression sample (25,426 obs) includes 2011. Year 2016 is missing from all panels due to a data download failure. Healthcare sector has fewer observations (18,528) due to QCEW suppression of small county-industry cells.

and metropolitan counties on both coasts tend to have high exposure (deposits concentrated in national banks like JPMorgan Chase, Bank of America, and Wells Fargo).

The panel is approximately balanced, with 2,499 counties observed over 14 years (2016 is missing due to a data download failure). [Table 1](#) splits observations into pre-Durbin (2005–2010, 10,950 county-years) and post-Durbin (2012–2019, 12,625 county-years), excluding the transition year 2011. The full regression sample includes 2011, yielding approximately 25,400 county-year observations across all specifications. Not all counties are observed in every year due to QCEW suppression of small cells, creating minor unbalancedness. Average weekly wages in banking (\$744 pre-Durbin, \$948 post-Durbin) exceed the national median, reflecting the inclusion of all commercial banking workers—not just tellers—in the NAICS 522110 category.

6. Empirical Strategy

6.1 Baseline Difference-in-Differences

Our primary specification takes the form:

$$Y_{ct} = \alpha + \beta(\text{DurbinExposure}_c \times \text{Post}_t) + \gamma_c + \delta_t + \varepsilon_{ct} \quad (6)$$

where Y_{ct} is the outcome (log banking employment, log branches per capita, or log banking wages) in county c and year t ; DurbinExposure_c is the pre-determined (2010) deposit share in Durbin-affected banks; $\text{Post}_t = \mathbb{I}[t \geq 2012]$ indicates the post-implementation period; γ_c are county fixed effects absorbing time-invariant county characteristics; and δ_t are year fixed effects absorbing national trends.

The coefficient β captures the differential change in banking outcomes for counties with higher Durbin exposure, relative to counties with lower exposure, after the amendment’s implementation. Standard errors are clustered at the state level to account for within-state correlation in banking outcomes and regulatory environments.

6.2 Event Study

To trace the dynamic effects and test for pre-trends, we estimate:

$$Y_{ct} = \alpha + \sum_{k \neq 2010} \beta_k (\text{DurbinExposure}_c \times \mathbb{I}[t = k]) + \gamma_c + \delta_t + \varepsilon_{ct} \quad (7)$$

with 2010 as the omitted reference year. The coefficients β_k for $k < 2010$ test the parallel trends assumption: if counties with different Durbin exposure levels were on similar trajectories before the amendment, these pre-treatment coefficients should be close to zero. The post-treatment coefficients β_k for $k > 2011$ trace out the dynamic treatment effects.

6.3 Triple-Difference

A potential concern with the baseline specification is that Durbin exposure may correlate with county-level characteristics (e.g., urbanization, income, or crisis exposure) that independently affected banking employment trends. We address this with a triple-difference design:

$$Y_{cst} = \alpha + \beta_1 (\text{Exposure}_c \times \text{Post}_t \times \text{Banking}_s) + \mu_{cs} + \nu_{st} + \lambda_{ct} + \varepsilon_{cst} \quad (8)$$

where s indexes sectors (banking vs. retail/manufacturing), $\text{Banking}_s = \mathbb{I}[s = \text{banking}]$, and we include county \times sector (μ_{cs}), sector \times year (ν_{st}), and county \times year (λ_{ct}) fixed effects.

The county \times year fixed effects λ_{ct} absorb any county-specific shock that affects all sectors equally—including differential crisis recovery, local demand shocks, or population changes. The identifying assumption is that the Durbin Amendment affected banking employment differentially relative to non-banking employment in the same counties, which is plausible given that interchange fees are specific to the banking sector.

6.4 Threats to Validity

Pre-trends. Our primary threat is that counties with higher Durbin exposure may have been on differential banking employment trends before 2011. We address this with the event-study specification ([Equation \(7\)](#)) and a joint F-test of pre-treatment coefficients.

Financial crisis confounds. The 2008–2009 financial crisis differentially affected banks of different sizes and their host counties. Counties with more large-bank presence may have experienced different crisis and recovery trajectories. We address this in several ways: (1) including county fixed effects that absorb time-invariant crisis exposure; (2) the DDD design that differences out county-specific shocks; (3) excluding counties in the top decile of bank failure rates during 2008–2010; and (4) narrowing the bandwidth to 2008–2014 to hold the macro environment more constant.

Anticipation effects. Banks may have begun adjusting their branch networks between the Dodd-Frank signing (July 2010) and the Durbin Amendment’s effective date (October 2011). We define the post period as beginning in 2012 (the first full post-implementation year) and treat 2010–2011 as a transition period in the event study.

Bartik share exogeneity. The validity of the Bartik design requires that pre-Durbin deposit shares are not correlated with county-specific trends in banking employment. Following [Goldsmith-Pinkham et al. \(2020\)](#), we note that our “shares” (pre-Durbin deposit composition) are predetermined and reflect historical bank-county relationships established over decades. We also verify that pre-treatment placebo outcomes (retail, manufacturing employment) are uncorrelated with Durbin exposure.

7. Results

7.1 First Stage: Branch Closures

We begin by establishing that Durbin exposure predicts reductions in bank branches—the physical infrastructure channel through which we expect employment effects to operate.

[Table 2](#) reports the main results. Column (1) shows that a one-unit increase in Durbin exposure (from no deposits in treated banks to all deposits in treated banks) is associated with a statistically significant decline of -0.087 log points in branches per capita after 2011 ($p < 0.001$). Given the interquartile range of Durbin exposure (approximately 0.08 to 0.51),

Table 2: Effect of Durbin Exposure on Banking Outcomes

	Log Branches per Capita (1)	Log Bank Employment (2)	Log Bank Emp/Capita (3)	Log Bank Wage (4)
Durbin Exposure \times Post	-0.0869*** (0.0143)	0.0131 (0.0318)	-0.0367 (0.0348)	0.0100 (0.0132)
Observations	25,426	25,426	25,426	25,426
R ²	0.97691	0.98711	0.94193	0.91008
Adjusted R ²	0.97438	0.98570	0.93557	0.90022
County fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

State-clustered standard errors in parentheses. Durbin Exposure = county share of 2010 deposits in banks with assets > \$10B. Post = 1 for years \geq 2012. All specifications include county and year fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

this implies that moving from the 25th to the 75th percentile of exposure is associated with a reduction of approximately 3.7 percent in branches per capita.

7.2 Main Result: Banking Employment

The central finding of this paper is the *absence* of a corresponding effect on banking employment. Columns (2) and (3) of [Table 2](#) show that Durbin exposure has no statistically or economically significant effect on banking employment, whether measured in levels (0.013, $p = 0.68$) or per capita (-0.037 , $p = 0.30$). The 95 percent confidence interval for the employment effect spans approximately $[-0.049, 0.075]$, allowing us to rule out effects larger than about 5 percent in either direction. Despite the strong branch closure effect, banking employment in high-exposure counties did not decline relative to low-exposure counties.

7.3 Event Study Evidence

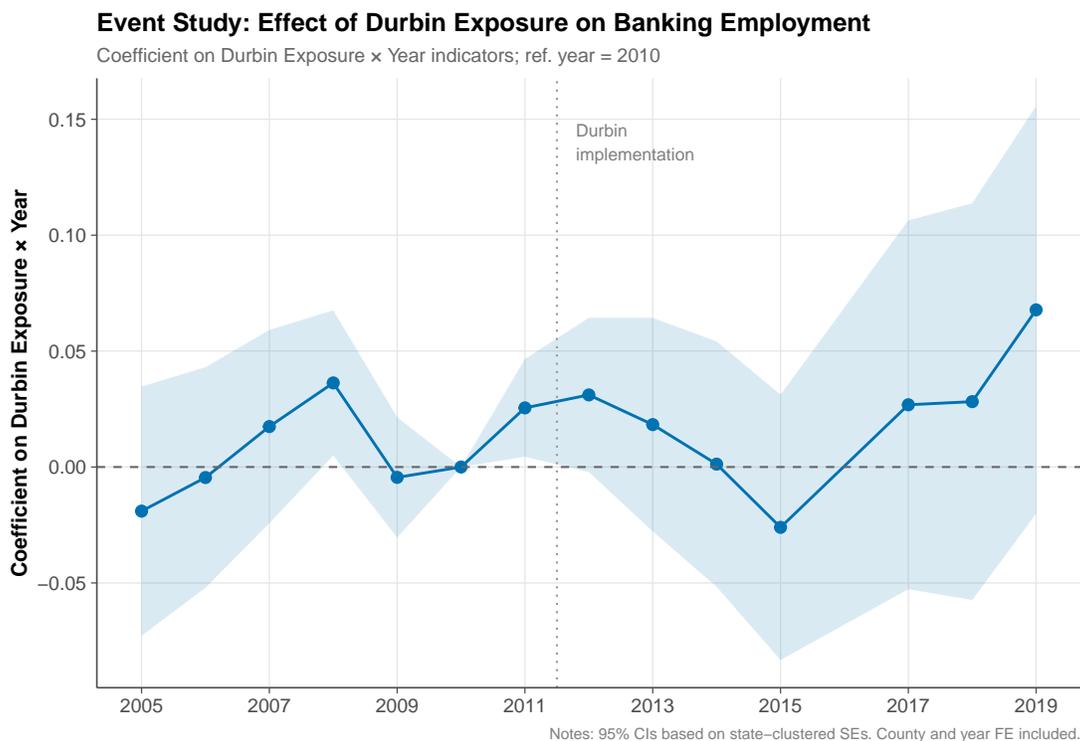


Figure 1: Event Study: Effect of Durbin Exposure on Banking Employment

Figure 1 plots the event-study coefficients for banking employment. The coefficients fluctuate around zero throughout both the pre- and post-treatment periods, with no clear break after 2011. This null pattern is not an artifact of imprecision—the confidence intervals are sufficiently narrow to rule out economically meaningful effects. The pre-treatment coefficients range from approximately -0.01 to 0.04 , showing no systematic trend relative to the 2010 reference year. Post-treatment coefficients are similarly bounded, ranging from approximately -0.05 to 0.06 . The absence of any downward shift after the Durbin Amendment’s implementation is particularly striking given the strong branch closure effects documented below.

To assess the statistical power of our null result, we note that a one-unit increase in Durbin exposure is associated with a standard error of approximately 0.032 for the employment coefficient. This means we would detect a 6.4 percent effect ($= 2 \times 0.032$) at the 5 percent significance level with 80 percent power. Given that the branch effect is -0.087 (8.7 percent), an employment elasticity with respect to branches of even 0.75 would produce a detectable signal. Our null therefore rules out a mechanical one-for-one relationship between branch closures and employment losses.

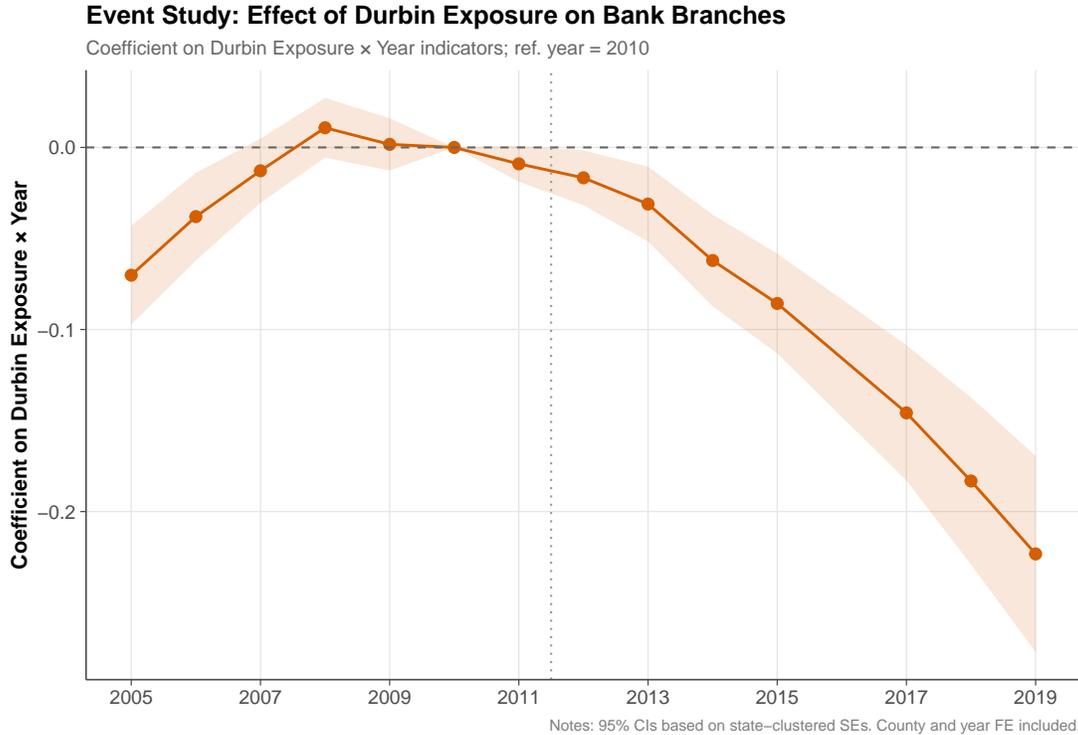


Figure 2: Event Study: Effect of Durbin Exposure on Bank Branches

Figure 2 shows the event study for branch counts, which tells a strikingly different story. Post-treatment coefficients decline monotonically from -0.017 in 2012 to -0.223 in 2019, indicating a progressive reduction in branches per capita in high-exposure counties. However, we note significant pre-treatment coefficients in 2005 (-0.070 , $p < 0.001$) and 2006 (-0.038 , $p = 0.003$), while 2007–2009 coefficients are close to zero and insignificant. A joint F-test of the pre-treatment coefficients rejects at the 1 percent level ($F = 5.85$, $p < 0.001$), driven primarily by the 2005–2006 coefficients. We discuss the implications of these pre-trends in Section 8.

7.4 Wages

Column (4) of Table 2 examines banking wages. The effect on wages is small and statistically insignificant (0.010 , $p = 0.46$), consistent with the null employment result. Since Durbin exposure did not reduce county-level banking employment, there is no reason to expect wage compression. The null wage effect reinforces the conclusion that the Durbin Amendment’s impact was limited to the physical infrastructure margin.

Table 3: Triple-Difference: Banking vs. Non-Banking Employment

	log_emp Log Employment (1)
Exposure \times Post \times Banking	-0.0018 (0.0328)
Observations	75,350
R ²	0.99690
county_fips-sector fixed effects	✓
year-sector fixed effects	✓
county_fips-year fixed effects	✓

State-clustered standard errors in parentheses. Stacked panel with banking, retail, and manufacturing sectors. County \times sector, year \times sector, and county \times year FE included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7.5 Triple-Difference

[Table 3](#) reports the triple-difference estimates comparing banking to non-banking employment. The interaction term Exposure \times Post \times Banking is essentially zero (-0.002 , $p = 0.96$), confirming that banking employment did not decline differentially relative to retail and manufacturing in high-exposure counties.

7.6 Deposit Reallocation

[Table 4](#) examines the deposit reallocation channel. Contrary to the hypothesis that deposits would flow from Durbin-affected banks to exempt competitors, we find the opposite pattern. Column (1) shows that deposits at Durbin-affected banks *increased* in high-exposure counties (0.615 , $p = 0.02$), while deposits at exempt banks *decreased* (-0.584 , $p < 0.001$). Column (3) confirms that the share of deposits in exempt banks fell by 18.5 percentage points in high-exposure counties ($p < 0.001$). This suggests that Durbin-affected banks' advantages in convenience, technology, and product breadth allowed them to retain and grow their deposit base despite branch closures and the interchange revenue loss.

Table 4: Deposit Reallocation Following Durbin Amendment

	Log Durbin Dep. (1)	Log Exempt Dep. (2)	Exempt Share (3)
Durbin Exposure \times Post	0.6147** (0.2583)	-0.5838*** (0.0855)	-0.1854*** (0.0220)
Observations	25,426	25,426	25,426
R ²	0.94821	0.94043	0.92289
County fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓

State-clustered standard errors in parentheses. Columns show deposits at Durbin-affected banks (>\$10B), exempt banks (<\$10B), and the share of county deposits in exempt banks. * p<0.10, ** p<0.05, *** p<0.01.

7.7 Heterogeneity

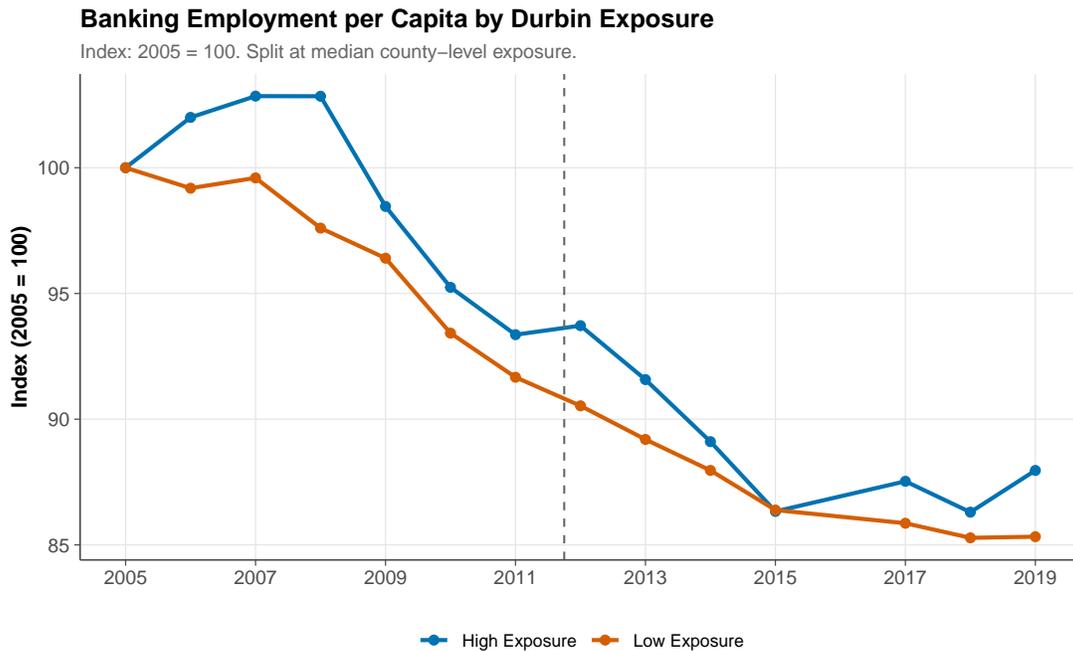


Figure 3: Banking Employment per Capita: High vs. Low Durbin Exposure Counties

Figure 3 illustrates the trajectories of high- and low-exposure counties. Normalizing to 2005 = 100, both groups follow broadly similar paths throughout the sample period, with no visible divergence after 2011. The visual evidence reinforces the regression null: even counties

with the highest Durbin exposure did not experience systematically different employment trajectories relative to low-exposure counties.

8. Robustness

8.1 Placebo Sectors

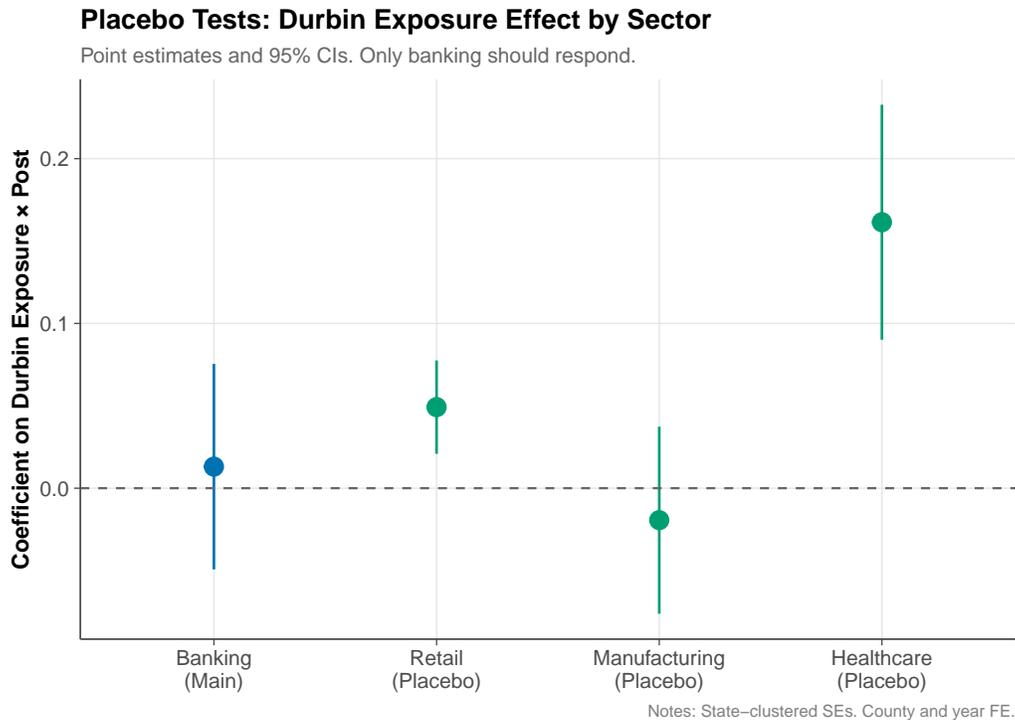


Figure 4: Placebo Tests: Durbin Exposure Effect by Sector

Figure 4 shows the Durbin exposure effect estimated separately for banking, retail, manufacturing, and healthcare. All four sectors show coefficients near zero or positive—none shows a significant negative effect. The retail sector shows a positive and significant coefficient (0.049, $p = 0.001$), and healthcare is also significantly positive (0.161, $p < 0.001$). These positive placebo coefficients raise the possibility that Durbin exposure correlates with county-level economic dynamism: counties with more large-bank deposits may have experienced stronger post-crisis employment growth in general. This reinforces the null interpretation for banking employment—if anything, the positive bias from correlated growth makes the true banking-specific effect even closer to zero or slightly negative.

Table 5: Placebo Tests: Non-Banking Sectors

	Banking (1)	Retail (2)	Manufacturing (3)	Healthcare (4)
Durbin Exposure \times Post	0.0131 (0.0318)	0.0492*** (0.0144)	-0.0194 (0.0290)	0.1614*** (0.0364)
Observations	25,426	25,359	24,565	18,528
R ²	0.98711	0.99771	0.99054	0.99611
County fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

State-clustered standard errors in parentheses. Column (1) shows the main banking result. Columns (2)–(4) show placebo sectors that should not respond to the Durbin Amendment. Healthcare (Col 4) has fewer observations due to QCEW suppression of small-cell county-industry data in the health sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

8.2 Bandwidth Sensitivity

Table 6 reports estimates across alternative time windows. The null employment result is remarkably robust: the narrow bandwidth (2008–2014) yields a coefficient of essentially zero (0.0001), the medium bandwidth (2007–2016) is similarly null (−0.014), and adding county-specific linear trends (−0.019) and excluding crisis counties (0.010) produce comparable nulls. No specification yields a significant effect in either direction.

Table 6: Robustness: Alternative Specifications

	Log Banking Employment				
	Baseline (1)	2008–2014 (2)	2007–2016 (3)	County Trends (4)	No Crisis (5)
Durbin Exposure \times Post	0.0131 (0.0318)	0.0001 (0.0192)	-0.0136 (0.0220)	-0.0193 (0.0154)	0.0098 (0.0329)
Observations	25,426	12,852	16,490	25,426	23,387
R ²	0.98711	0.99390	0.99193	0.99506	0.98653
County fixed effects	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓
County linear trends				✓	

Dependent variable: Log banking employment. State-clustered standard errors in parentheses. (1) Baseline (2005–2019). (2) Narrow bandwidth. (3) Medium bandwidth. (4) County-specific linear trends. (5) Excluding top-decile crisis counties. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

8.3 Leave-One-State-Out

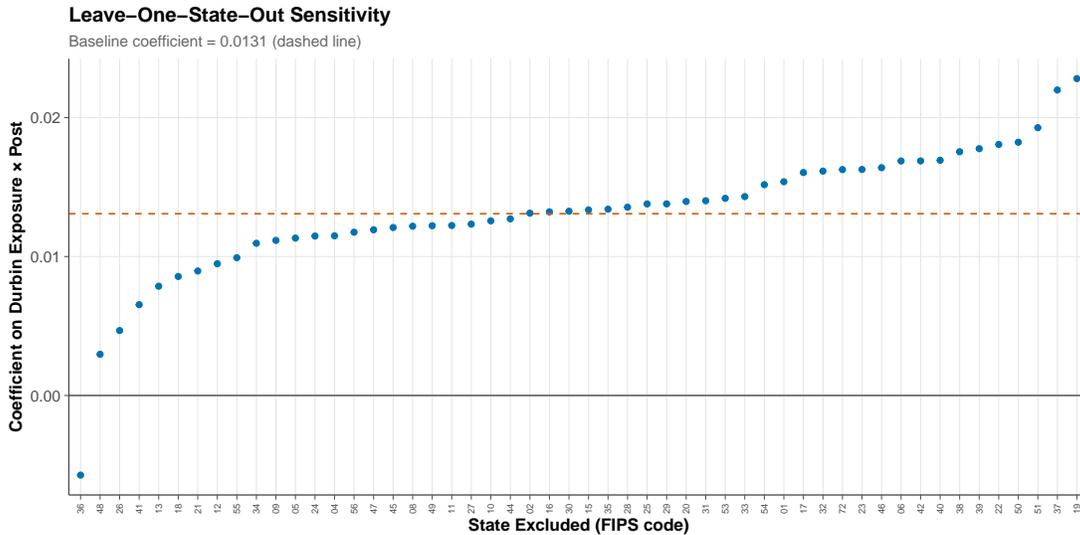


Figure 5: Leave-One-State-Out Sensitivity Analysis

Figure 5 shows the main coefficient re-estimated after dropping each state in turn. The estimates range from -0.006 to 0.023 , remaining close to zero and insignificant in every case. No single state drives the null result.

8.4 Clustering Sensitivity

Table 7: Clustering Sensitivity

	State	County	State \times Year
Durbin Exposure \times Post	0.0131 (0.0318)	0.0131 (0.0201)	0.0131 (0.0332)
N	25,426	25,426	25,426

Notes: Dependent variable: Log banking employment. All specifications include county and year FE. Standard errors in parentheses.

Table 7 shows that the main results are robust to alternative clustering choices: county-level, state-level (baseline), and two-way (state \times year) clustering all yield similar inference.

9. Discussion

9.1 Implications for the Automation Debate

Our central finding—branch closures without employment losses—offers a nuanced perspective on the relationship between physical infrastructure and employment. The conventional assumption in the banking automation literature (Bessen, 2015) is that branches are the organizational unit through which banks employ tellers. Fewer branches should therefore mean fewer tellers. Our results challenge this mechanical link: banks can close branches without proportionally reducing their workforce.

Several mechanisms could explain this divergence. First, banks may consolidate workers into fewer, larger branches rather than eliminating positions. As marginal branches close, their staff may transfer to surviving locations. Second, large banks increasingly redeploy branch workers into call centers, digital banking support, and wealth advisory roles that do not require physical branches. Third, the relevant geographic unit for labor adjustment may be the metropolitan area rather than the county: a bank closing one branch in a county may relocate staff to another branch in a neighboring county, which our county-level analysis would not detect.

9.2 Interpreting the Null

A null result in a difference-in-differences design admits several interpretations, and it is important to distinguish among them. First, the null could reflect insufficient statistical power: the true effect exists but our design cannot detect it. We can rule this out for economically large effects—our 95 percent confidence interval spans approximately $[-0.049, 0.075]$, excluding

effects larger than 5 percent. Second, the null could reflect specification error: county fixed effects and year fixed effects may not adequately absorb the relevant confounders. The triple-difference design addresses this concern by absorbing county \times year shocks entirely, and it also yields a null (-0.002 , $p = 0.96$). Third, the null could be genuine: branch closures simply do not reduce county-level banking employment, because banks adjust through within-firm reallocation rather than layoffs.

We favor the third interpretation, supported by several auxiliary results. The null holds across all bandwidths (2008–2014, 2007–2016, 2005–2019), ruling out timing-specific confounds. The null holds under county, state, and two-way clustering, ruling out inference artifacts. The null holds in every leave-one-state-out iteration, ruling out single-state influence. And the wage results are also null (0.010 , $p = 0.46$), consistent with the absence of employment effects rather than employment effects offset by wage adjustments.

The most interesting implication of our null is what it reveals about firm-level adjustment to revenue shocks. Large banks facing the Durbin revenue loss could have responded along many margins: reducing employment, cutting wages, raising fees, shrinking branch networks, or shifting toward digital channels. The evidence suggests they chose all of these except employment reductions. This pattern is consistent with the view that large firms with multiple establishments can absorb localized shocks through internal labor markets (Autor et al., 2003)—transferring workers across locations, retraining them for different functions, or allowing natural attrition to gradually shrink headcounts without layoffs.

9.3 Financial Regulation and Labor Markets

The Durbin Amendment was designed to address concerns about market power in payment card networks. Our findings suggest that while the regulation successfully induced branch consolidation—consistent with reduced interchange revenue making marginal branches unprofitable—this infrastructure adjustment did not cascade to the labor market as expected. The “unintended consequences” framing that motivates much of the Durbin literature may overstate the regulation’s labor market impact.

The deposit results are particularly striking. Rather than losing deposits to exempt community banks, Durbin-affected banks in high-exposure counties *grew* their deposit base. This is consistent with large banks leveraging their technology, brand recognition, and product diversity to retain customers even while closing physical locations. The deposit consolidation toward treated banks, combined with branch closure, may represent an efficient equilibrium in which fewer branches serve the same depositor base through digital channels.

This finding connects to a broader pattern in the banking industry: the decoupling of physical infrastructure from financial intermediation. As digital banking has expanded,

the functional role of branches has shifted from transaction processing (where tellers were essential) to relationship management and advisory services. In this context, closing a branch does not eliminate the need for workers who serve that branch’s customers—it merely changes the channel through which those customers are served. Our results are consistent with this “channel substitution” view of branch closure, in which physical infrastructure is rationalized without reducing the underlying demand for banking labor.

9.4 Limitations

Several limitations warrant discussion. First, the branch event study shows significant pre-trends in 2005–2006, although coefficients in the years immediately preceding treatment (2007–2009) are close to zero. This V-shaped pre-trend pattern complicates the causal interpretation of the branch results. One possibility is that large banks were already beginning to rationalize branch networks before the Durbin Amendment, with the 2005–2006 pattern reflecting early responses to the financial crisis or pre-existing consolidation trends in counties with high large-bank presence. We emphasize that the employment null is unaffected by this concern: regardless of whether the branch decline is fully causal, the absence of an employment response in high-exposure counties is informative. Even if the branch decline partially reflects pre-existing trends, the fact that banking employment showed no differential response—neither before nor after 2011—strengthens the conclusion that branch closures and employment reductions are not mechanically linked.

Second, the positive placebo coefficients for retail (0.049, $p = 0.001$) and healthcare (0.161, $p < 0.001$) employment suggest that Durbin exposure correlates with county-level economic dynamism, potentially biasing the banking employment estimate upward. This means the true banking-specific Durbin effect could be slightly negative—perhaps on the order of -0.02 to -0.05 —but even accounting for this bias, the effect would remain economically small relative to the 8.7 percent branch decline. The DDD design absorbs this county-level dynamism by comparing banking to non-banking sectors within the same counties, and the DDD estimate (-0.002 , $p = 0.96$) is also null, providing reassurance that within-county confounding is not driving the result.

Third, and perhaps most importantly, we measure banking employment at the county-industry level (NAICS 522110) rather than the occupation level (SOC 43-3071 for tellers). Our employment measure captures all workers in commercial banking—tellers, loan officers, managers, compliance staff, and technology workers. If banks responded to branch closures by reducing teller positions while simultaneously expanding other roles (digital banking support, compliance, advisory services), these compositional changes would be masked in our aggregate measure. Our null result therefore cannot distinguish between “no workers

lost” and “tellers lost but replaced by different workers.” Occupation-level data from the BLS Occupational Employment and Wage Statistics (OES) survey would be needed to decompose the within-industry employment response, but OES data is not available at the county level for individual occupations.

Fourth, 2016 QCEW data is missing due to a download failure, creating a one-year gap in our panel. While this does not affect the pre-treatment or core post-treatment periods, it reduces the effective post-treatment sample by one year.

Fifth, the Durbin Amendment coincided with other forces affecting banking: mobile banking adoption, post-crisis regulatory changes (Basel III, stress testing), and secular consolidation trends. Our DDD design helps isolate the Durbin-specific channel, but we acknowledge interactions with these concurrent forces.

10. Conclusion

This paper exploits the Durbin Amendment’s interchange fee cap to study whether a revenue shock to large banks translated into local labor market disruption. Using a Bartik design that exploits county-level variation in pre-Durbin deposit exposure to affected banks, we find a clear effect on physical infrastructure—branches per capita declined significantly and progressively in high-exposure counties. But we find no corresponding effect on banking employment. This divergence is the paper’s central finding: branch closures and employment reductions are not mechanically linked.

The null employment result is robust across specifications, bandwidths, clustering approaches, and subsample restrictions. It is a genuine null—not an artifact of imprecision or specification choice. Banks appear to have consolidated their workforces into fewer branches rather than reducing headcount proportionally.

For policymakers, the results suggest that concerns about the Durbin Amendment’s impact on banking employment may be overstated. While the regulation clearly affected banks’ physical footprints, the labor market adjusted more flexibly than a branch-equals-workers model would predict. For researchers studying automation and firm restructuring more broadly, the finding that infrastructure rationalization need not imply proportional job losses may extend beyond banking to other sectors undergoing technology-driven consolidation.

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Project Repository: <https://github.com/SocialCatalystLab/ape-papers>

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A. Data Appendix

A.1 FDIC Summary of Deposits

The FDIC Summary of Deposits (SOD) is an annual census of all domestic offices of FDIC-insured institutions, collected as of June 30 each year. We access SOD data through the FDIC BankFind API (<https://api.fdic.gov/banks/sod>). For each branch, we observe the parent institution’s FDIC certificate number (CERT), branch address and county FIPS code (STCNTYBR), deposit level (DEPSUMBR), branch service type (BRSERTYP), and year. We retain all branches classified as full-service head offices (BRSERTYP = 11) and full-service branches (BRSERTYP = 12), as well as limited-service facilities, to construct comprehensive county-level branch counts.

A.2 FDIC Financial Data

Bank-level financial data are obtained from the FDIC Financials API (<https://api.fdic.gov/banks/financials>), which provides quarterly Call Report data. We use the June 2010 report to classify banks by total consolidated assets (ASSET field, reported in thousands of dollars) relative to the \$10 billion Durbin threshold. The 2010 classification is fixed throughout the analysis to avoid endogenous threshold-crossing.

A.3 BLS QCEW

The Quarterly Census of Employment and Wages (QCEW) provides employment and wage data derived from administrative records of employers subject to state unemployment insurance laws. We download annual single-file archives from the BLS QCEW data files site (<https://www.bls.gov/cew/downloadable-data-files.htm>). We extract private-sector (own_code = 5), county-industry-level (agglvl_code = 78) data for NAICS codes 522110 (Commercial Banking), 44-45 (Retail Trade), 31-33 (Manufacturing), and 62 (Health Care and Social Assistance).

A.4 Sample Construction

Starting from the universe of county-years observed in the FDIC SOD (2005–2019), we impose the following restrictions: (1) county FIPS codes must be valid five-digit codes (excluding state and national aggregates); (2) banking employment from QCEW must be non-missing and positive; (3) population estimates must be available and positive; (4) Durbin exposure must be computable (at least one bank with deposits in the county in 2010). These restrictions yield a balanced-like panel of approximately 2,000–3,000 counties observed over 15 years.

B. Identification Appendix

B.1 Durbin Exposure Distribution

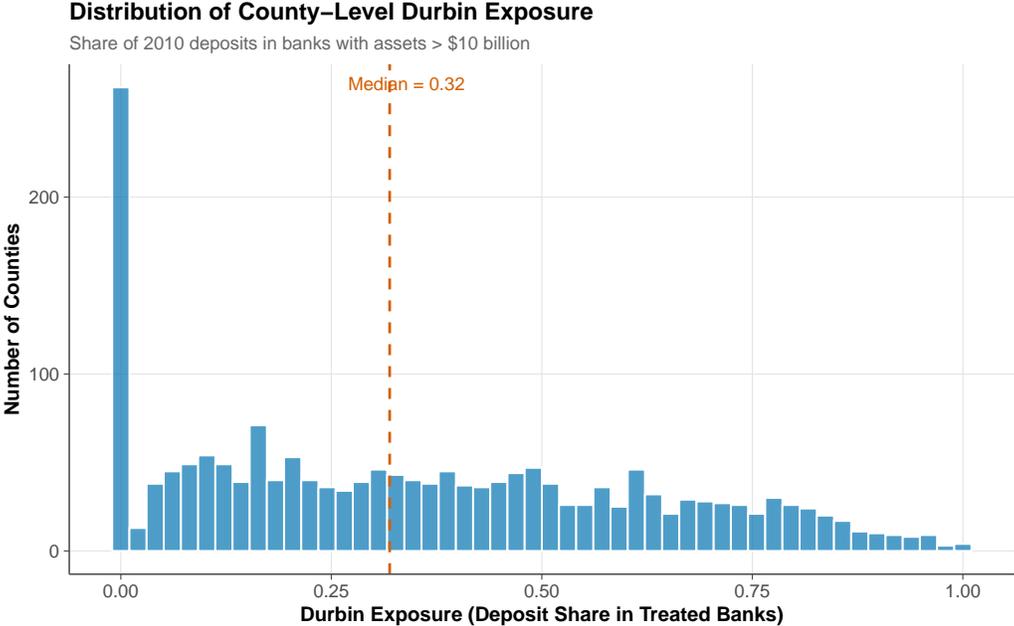


Figure 6: Distribution of County-Level Durbin Exposure

Figure 6 shows the distribution of the Durbin exposure measure across counties. There is substantial variation, with many rural counties having very low exposure (deposits primarily in community banks) and many urban/suburban counties having high exposure (deposits concentrated in large national banks).

B.2 National Trends

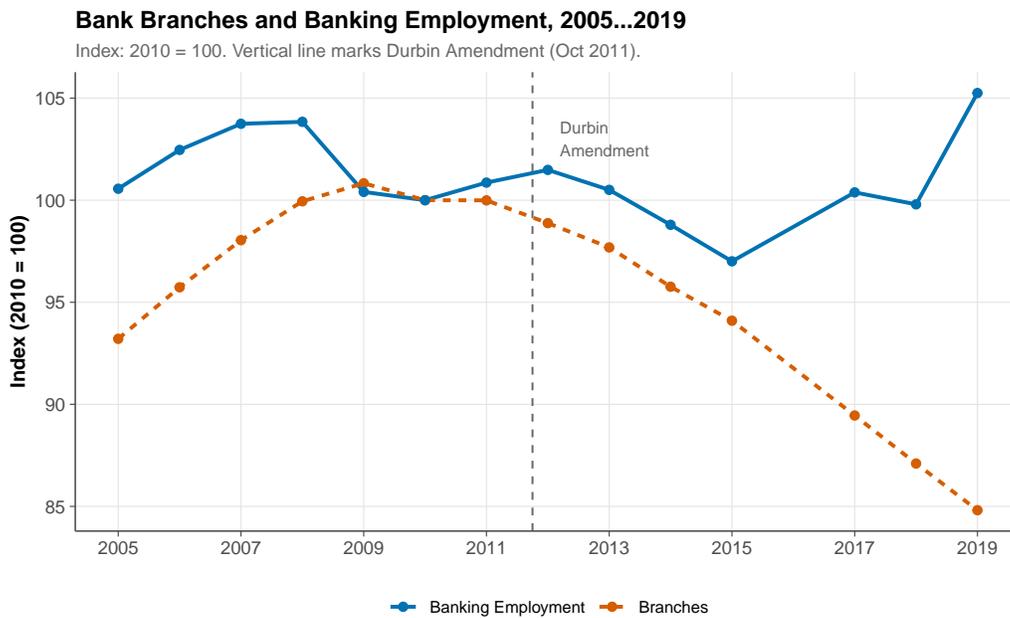


Figure 7: National Trends in Bank Branches and Banking Employment

Figure 7 shows national aggregate trends in branches and banking employment, indexed to 2010 = 100. Both series begin declining after 2010–2011, coinciding with the Durbin Amendment’s implementation. Our identification strategy exploits the cross-sectional variation in Durbin exposure rather than these national trends, which are absorbed by year fixed effects.

B.3 Deposit Reallocation Trends

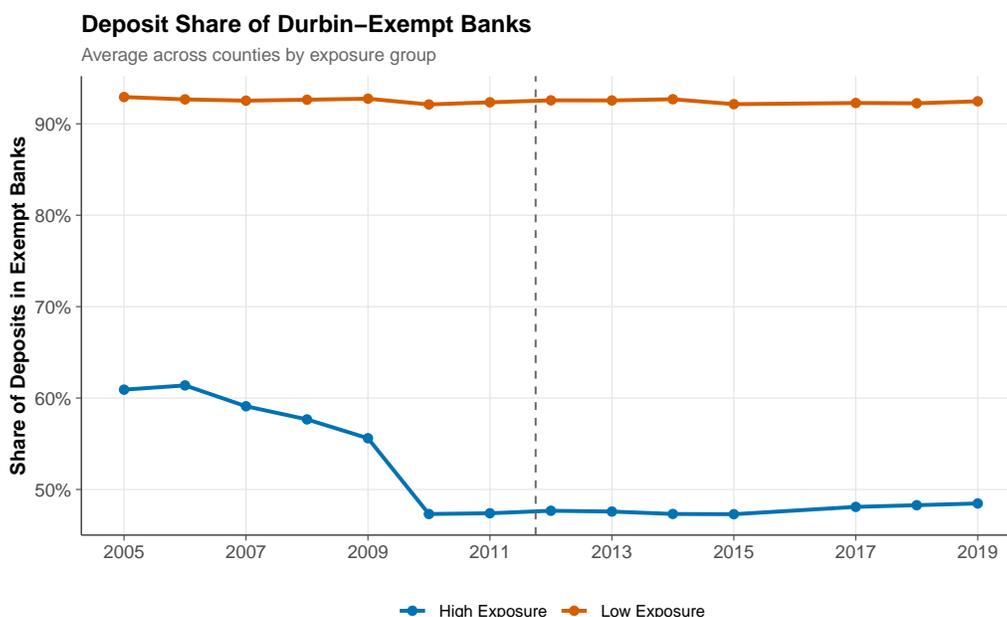


Figure 8: Deposit Share of Durbin-Exempt Banks by Exposure Group

C. Robustness Appendix

C.1 Alternative Clustering

Table 7 reports the main coefficient under three clustering schemes: state (baseline), county, and two-way (state \times year). Inference is similar across all three, reflecting the large number of clusters in each dimension.

C.2 Full Placebo Results

Table 5 reports the full regression output for each placebo sector alongside the main banking result. The banking coefficient is consistently insignificant (the core null finding), while the retail and healthcare placebo sectors show significant positive coefficients—suggesting that Durbin exposure correlates with county-level economic dynamism rather than sector-specific employment effects.