

## **The aggregate banking-sector footprint of financial education mandates: cohort exposure, community bank margins, and an arithmetic of detection**

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**Abstract.** *We measure the community-bank footprint of state financial education mandates using staggered adoption across seven US states (2005–2019). The treatment is a continuous Cohort Exposure Index (CEI) capturing the mandate-exposed share of the working-age population. In a panel of 81,549 community bank-year observations, CEI is positively associated with net interest margin; the gradient is concentrated in banks with higher consumer loan share and is accompanied by elevated consumer-loan charge-offs. An arithmetic decomposition bounds the channel's expected per-bank magnitude; our estimates fall inside this band, consistent with the bank-balance-sheet channel operating at the scale the underlying cohort mechanism allows.*

**Keywords:** financial literacy, financial education mandates, net interest margin, community banks, staggered difference-in-differences.

**JEL Classification:** G21, G28, I22, H75.

## 1. Introduction

Since 2000, more than half of US states have enacted laws requiring high school students to complete a standalone personal finance course as a condition of graduation. Household-level evidence shows that students exposed to these mandates display higher credit scores, lower delinquency, and a shift toward formal lending products by their early twenties (Brown et al., 2016; Stoddard and Urban, 2020; Burke et al., 2023). Whether these household-level shifts aggregate into the balance sheets of the intermediaries that serve the same populations is an open question. This paper asks it for community banks: the institutions most plausibly exposed to the cohort mechanism through their consumer-credit franchises.

The empirical strategy exploits staggered mandate adoption across seven states between 2008 and 2020. The treatment variable is a continuous Cohort Exposure Index (CEI) constructed at the state-year level: the share of the working-age population whose secondary schooling fell on or after the state's mandate adoption year, computed under a uniform age-distribution approximation across the working-age population. The CEI rises gradually as successive mandate-exposed cohorts age into the adult population, providing a continuous dose measure rather than a binary on-off treatment indicator. The dependent variable is the net interest margin (NIM) of community banks operating in the state, drawn from FDIC Call Reports. The panel covers 81,549 bank-year observations on 7,841 banks.

In the baseline two-way fixed-effects specification, CEI is positively associated with NIM with a marginally significant coefficient that depends on the inclusion of the Global Financial Crisis (GFC) window. The more informative result comes from a heterogeneity specification that interacts CEI with the bank's consumer loan share (CLS): the interaction is positive and economically large ( $\hat{\beta}_3 = 11.46$ ), and a Fisher permutation test under geographic placebo reassignment places it well above the placebo null distribution (one-sided empirical  $p = 0.002$ ). State-clustered asymptotic inference is imprecise given the small treated-cluster count, which is why we treat the permutation  $p$ -value as the primary inferential criterion. A supplementary charge-off regression delivers a significantly positive  $\text{CEI} \times \text{CLS}$  interaction, indicating that consumer-credit concentrated banks in high-exposure states earn higher NIMs while also absorbing correspondingly higher credit losses.

Two interpretive issues structure the rest of the paper. The first is whether these estimates are large or small relative to the mechanism that generates them. We address this with an arithmetic decomposition of the expected per-bank NIM footprint into three multiplicative factors: the treated-population share, the exposed-portfolio share, and the household-level yield differential. The decomposition delivers an implied band rather than a point ceiling; our estimates fall inside the band, which we read as the bank-balance-sheet channel operating at a scale consistent with, not above or below, the underlying cohort mechanism. The second issue is what the design can and cannot deliver. With seven treated states, two of which adopted during the GFC, the design has real power for the within-state

heterogeneity gradient but limited power for the aggregate dose-response coefficient. Sharper identification of the same mechanism requires data closer to the unit of behavior, such as individual credit-bureau records or product-level loan portfolios; the CEI construction transfers directly to those settings.

The paper makes two contributions. The Cohort Exposure Index provides a continuous dose measure for any cohort-based education or behavioral policy whose effects diffuse through demographic replacement; it avoids the errors-in-variables problem that affects discrete treatment-timing indicators when the underlying mechanism operates on a fraction of the population. The arithmetic decomposition provides an ex-ante calibration tool for evaluating cohort-based policies at aggregate intermediary scale, and an interpretive bridge between household-level and balance-sheet-level evidence on the same mechanism.

Section 2 reviews the literature. Section 3 describes the identification strategy and the CEI construction. Section 4 describes the data and summary statistics. Section 5 presents the empirical results. Section 6 develops the arithmetic-of-detection decomposition. Section 7 concludes.

## **2. Literature Review**

### **2.1. From Individual Financial Behavior to Aggregate Market Outcomes**

Individual financial literacy shapes household-level financial outcomes along several margins. Households with higher financial literacy hold more diversified portfolios, accumulate greater retirement wealth, incur lower debt costs, and are less likely to carry high-interest revolving balances (Lusardi and Mitchell, 2014). Whether the aggregation of individual financial competence across households produces a corresponding effect on the efficiency of financial intermediation is less well documented.

The micro-literacy literature analyses behavioral effects at the household or firm level; the macro-financial-development literature treats banking efficiency as a function of institutional quality, competition, regulatory design, and macroeconomic conditions. This paper exploits within-country, subnational variation in exposure to mandated financial education and links that variation to the net interest margins of community banks operating in the same local markets.

### **2.2. Financial Literacy, Financial Inclusion, and Macro-Financial Development**

Grohmann et al. (2018) provide the first large-scale cross-country evidence that household financial literacy improves financial inclusion, using the 2014 S&P Global Financial Literacy Survey across 148 countries combined with World Bank Global Financial Development Database indicators. Hussain et al. (2021) extend this framework to a 98-country panel and document that the literacy–inclusion channel is strongly moderated by institutional quality. Basha et al. (2023) shift the unit of analysis to firms and show that small-firm leverage responds to the interaction of financial literacy and financial

development. Sethi and Acharya (2018) document that financial inclusion Granger-causes economic growth in a 31-country panel. Hussain et al. (2024) find substantial asymmetry in the inclusion–growth relationship across developed and developing Asian economies.

A contrary finding comes from Barik and Pradhan (2020), who study the BRICS economies and document that rapid financial inclusion expansion driven by credit-standard relaxation is associated with deteriorating financial stability. The mechanism we document in Section 5.5 is closer in spirit to Barik than to a pure asset-quality narrative: financial-education-driven credit expansion in the present setting is accompanied by, not in place of, elevated charge-offs. Nogueira et al. (2025) reverse the standard causal arrow, finding that per-capita income, financial system development, and educational attainment are themselves primary drivers of aggregate financial literacy across 20 OECD countries, a concern for endogenous mandate adoption that our staggered identification design addresses.

### **2.3. Micro-Foundations for an Intermediation Efficiency Channel**

Four theoretical channels link the CEI to bank NIMs. (i) A deposit-side channel: financially illiterate depositors exhibit high search costs and inertia, allowing banks to earn rents on liabilities; rising literacy makes deposit supply more elastic and compresses NIMs (Erel et al., 2023). (ii) A loan-side quality channel: financially literate borrowers reduce adverse selection and moral hazard, lowering expected credit losses and the default risk premium. (iii) A competition channel: illiterate borrowers underestimate costs and refinance less frequently (Stango and Zinman, 2009; Woodward and Hall, 2012), allowing banks to extract pricing premia that rising literacy erodes. (iv) A credit-demand channel: literate adults who understand credit products may be more willing to borrow, acting as a demand stimulus rather than only a source of risk reduction. Under (iv), community banks in high-literacy markets face an expanded pool of informed retail borrowers, may extend their consumer portfolios, and may capture the yield premium of installment credit—generating NIM expansion alongside, rather than instead of, elevated expected credit losses. The empirical evidence in Section 5.5 is consistent with (iv).

### **2.4. Where the Present Paper Sits**

Two literatures bracket this paper. The household-side work cited above (Brown et al., 2016; Stoddard and Urban, 2020; Burke et al., 2023; Mangrum, 2022) establishes that mandate-exposed cohorts exhibit measurable changes in credit behavior, identified from individual credit-bureau records. The macro-financial-development literature treats banking efficiency as a function of aggregate institutional and macroeconomic conditions. We sit between them. The empirical question is whether the household-level shifts already documented aggregate into a detectable response on the balance sheets of the intermediaries that serve the same populations, and at what magnitude. The CEI provides the continuous dose measure that the dose-response question requires; the heterogeneity specification isolates the segment of the banking sector where the channel can plausibly operate.

### 3. Methodology and Identification Strategy

#### 3.1. Research Design Overview

The empirical strategy exploits the staggered, quasi-random timing with which US states adopted standalone personal finance graduation requirements between 2008 and 2020. The primary outcome is the net interest margin (NIM) of FDIC-insured community banks operating in state  $s$  in year  $t$ . The identifying assumption is that, conditional on pre-mandate covariates and bank fixed effects, the timing of mandate adoption is uncorrelated with pre-existing trends in bank NIMs—an assumption tested formally using pre-period event-study estimates.

#### 3.2. The Treatment Variable

The treatment indicator is  $D_{s,t} \in \{0,1\}$ , equal to one for all years  $t$  on or after the year in which state  $s$  enacted a standalone personal finance graduation requirement. Adoption years are coded using the NGPF (Next Gen Personal Finance) Live Dashboard as the primary source, cross-referenced with the Council for Economic Education's biennial Survey of the States. Treatment cohorts in the 2005–2019 window: Utah (2008), Virginia (2009), Tennessee and Alabama (2013–2014), Missouri ( $\approx 2017$ ), North Carolina (2019), and Iowa ( $\approx 2020$ ). We define  $G_s$  as the calendar year of first treatment, with  $G_s = \infty$  for the approximately 44 never-treated states. Missouri and Iowa are coded with approximate adoption years; alternative codings within the cited primary-source range leave the main estimate essentially unchanged.

#### 3.3. The Cohort Exposure Index

The binary indicator  $D_{s,t}$  does not capture the dosage of financial education received by the adult population at time  $t$ . A mandate adopted in year  $g$  affects only students enrolled in high school during and after that year; the mandate-exposed share of the adult population increases gradually as successive cohorts age into adulthood. We construct the Cohort Exposure Index (CEI), a continuous dose variable. An individual of age  $a$  residing in state  $s$  in year  $t$  is classified as exposed if their high school years overlapped with the period in which the mandate was in effect:

$$\mathbf{1}[\text{exposed}(a, s, t)] = \mathbf{1}[t - a + 17 \geq G_s] \times \mathbf{1}[G_s < \infty] \quad (1)$$

The CEI for state  $s$  in year  $t$  is the exposure share of the 18–65 working-age population:

$$\text{CEI}_{s,t} = \sum_{a=18}^{65} \omega_{a,s,t} \times \mathbf{1}[\text{exposed}(a, s, t)] \quad (2)$$

where  $\omega_{a,s,t}$  is the population weight assigned to age  $a$  in state  $s$  in year  $t$ . In principle, this weight could be sourced from Census Bureau Population Estimates Program (PEPS) and American Community Survey single-year-of-age estimates by state and year (vintages 2000–2009 and 2010–2019) to let the exposure share reflect each state's actual age distribution; we leave that refinement to future work with finer administrative age data. The specification estimated throughout this paper instead assumes a uniform age

distribution across the working-age bracket, under which Equation (2) reduces to the closed-form expression:

$$CEI_{s,t} = \frac{\max(0, \min(48, t - G_s))}{48} \quad (3)$$

which is the specification we estimate throughout the paper; Section 5.4.3 discusses this construction choice and a demographic-weighted extension. By construction,  $CEI_{s,t} \in [0,1]$ , equals zero for all never-treated states, and increases monotonically in treated states at approximately  $1/48 \approx 2.08$  percentage points per year as each successive mandate-exposed high school cohort ages into the working-age population. The maximum observed value is 0.229 (Utah in 2019, eleven years post-adoption).

### 3.4. The Community Bank Restriction

We restrict the analysis to FDIC-insured commercial banks and savings institutions with year-end total assets below \$1 billion (BKCLASS codes N, NM, SM, SA, and SB). This threshold is the standard definition used by the FDIC for the community banking sector (Federal Deposit Insurance Corporation, 2020). Large multi-state banking organizations are excluded because their NIMs are determined at the national portfolio level by centralized asset-liability management, rendering any single state's financial literacy shock trivial relative to the national book.

### 3.5. Estimation Framework

The identification strategy rests on a continuous-treatment Two-Way Fixed Effects (TWFE) design. The treatment variable, the Cohort Exposure Index, is a continuous dosage measure that accumulates as mandate-exposed cohorts age into the working-age population. The estimand of interest is therefore the marginal NIM response to a one-unit increase in CEI rather than a binary average treatment effect.

Recent methodological work raises concerns about TWFE in staggered settings under heterogeneous treatment effects (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). Two features of our setting mitigate these concerns. First, the continuous CEI specification does not require cohort-specific homogeneity in the same way as a binary event study: the dose-response slope is identified from within-state variation in cumulative exposure. Second, with only seven treated states, we do not rely on asymptotic clustered inference; instead we apply a non-parametric Fisher permutation test that re-assigns observed mandate adoption years to randomly drawn control states and re-estimates the full regression 1,000 times, generating an exact finite-sample null distribution robust to both staggered timing and the small treated-cluster count. We treat the permutation  $p$ -value as the primary inferential criterion alongside the continuous TWFE estimates.

### 3.6. Main Specification

The primary TWFE regression is:

$$NIM_{b,s,t} = \alpha_b + \delta_t + \beta CEI_{s,t} + \gamma' X_{s,t} + \varepsilon_{b,s,t} \quad (4)$$

where  $\alpha_b$  are bank fixed effects,  $\delta_t$  are year fixed effects, and  $\mathbf{X}_{s,t}$  includes the state unemployment rate and real GDP growth. Standard errors are clustered at the state level. The heterogeneity specification augments Equation (4) with an interaction term:

$$\text{NIM}_{b,s,t} = \alpha_b + \delta_t + \beta_1 \text{CEI}_{s,t} + \beta_2 \text{CLS}_{b,t} + \beta_3 \text{CEI}_{s,t} \times \text{CLS}_{b,t} + \gamma' \mathbf{X}_{s,t} + \varepsilon_{b,s,t} \quad (5)$$

where  $\text{CLS}_{b,t}$  is the bank's consumer loan share (LNCON/LNLSNET, consumer installment and revolving credit excluding residential mortgages), and  $\beta_1, \beta_2, \beta_3$  are the coefficients on CEI, CLS, and their interaction. The marginal effect of mandate exposure on NIM is  $\partial \text{NIM} / \partial \text{CEI} = \hat{\beta}_1 + \hat{\beta}_3 \times \text{CLS}_{b,t}$ .

The choice of consumer loan share as the primary heterogeneity variable follows directly from the life-cycle stage of the mandate-exposed cohort. Financial literacy mandates target students at ages 15–17 (high school); those graduates enter the labor market at approximately 18–22 and reach peak consumer credit-market entry by their mid-twenties. The first credit products for this cohort are overwhelmingly auto loans, credit cards, and personal installment loans—not residential mortgages. The median age of first-time homebuyers in the United States is approximately 35 years, well outside the 18–25 window that defines the mandate-exposed cohort in our sample horizon. Consequently, the relevant portfolio-mix dimension for identifying the mandate transmission channel is the share of *consumer* credit in a bank's loan book, not the combined retail share that includes residential mortgages driven by older, unaffected cohorts. As a robustness check we also estimate the model with the broader retail loan share ( $\text{RLS} = (\text{LNRERES} + \text{LNCON}) / \text{LNLSNET}$ ), which adds 1–4 family mortgages to the consumer installment component; the interaction estimate is +6.882 (SE = 5.152), directionally consistent with the CLS specification and smaller in magnitude, as expected if mortgages dilute the consumer-demand signal.

### 3.7. Threats to Identification

**Endogenous mandate adoption.** Early adopters are systematically different from non-adopters along observable dimensions: states that enacted financial literacy requirements first tend to have above-median household incomes, denser retail bank branch networks, higher pre-mandate credit penetration rates (outstanding consumer and mortgage debt per capita), and stronger baseline educational attainment. Each of these characteristics is plausibly correlated with both the propensity to adopt early *and* the level of community bank NIM, creating a selection threat. The Callaway and Sant'Anna (2021) estimator addresses this through conditional parallel trends – allowing trend comparisons only within cells matched on pre-treatment covariates – but implementing it rigorously here would require conditioning on state-level pre-mandate household income, consumer credit penetration, and branch density, which we do not do.

Instead, we rely on two sources of reassurance. First, the event-study pre-trend test in Figure 1 yields  $F = 7.95$  ( $p = 0.159$ ) on the five pre-adoption periods: if early-adopting states were on systematically different NIM growth trajectories before their mandates, we

would expect negative and significant pre-event coefficients, but we find none. Second, CEI varies continuously within treated states over time, so identification exploits the gradual demographic accumulation of mandate-exposed cohorts rather than the single adoption event – a within-state temporal source of variation that is largely orthogonal to the static cross-sectional differences that drive selection.

We flag this as a limitation and acknowledge that formal covariate conditioning remains an open robustness exercise. **COVID-19 macro confound.** Restricting the sample to  $t \leq 2019$  eliminates the confounding effects of near-zero policy rates and emergency asset purchase programs. **Multi-state bank contamination.** The community bank restriction ensures each institution's NIM reflects pricing conditions in the treatment state's local market.

**State-specific characteristics of early adopters.** Two of the seven treated states, Utah (2008) and Virginia (2009), warrant specific discussion given features that may affect the generalizability of the estimates. Virginia is geographically proximate to Washington, D.C. and receives a disproportionately high inflow of residents from other states due to federal government employment and defense contracting. This high mobility rate means that the CEI may overstate the fraction of Virginia bank customers actually educated under the Virginia mandate, as a substantial share of working-age adults grew up elsewhere. To the extent that this dilutes the measured treatment intensity, it biases our estimates toward zero and implies that the true mandate effect may be larger than reported. A Virginia-excluded leave-one-out specification yields a larger positive interaction estimate (+17.21, SE = 5.43,  $p = 0.002$ ), consistent with attenuation-to-zero rather than a spurious Virginia-specific effect.

Utah's population is predominantly affiliated with the Church of Jesus Christ of Latter-day Saints, whose financial norms emphasize debt avoidance and self-reliance. Relative to a national benchmark, this may imply a lower baseline demand for consumer credit in Utah, which could attenuate the mandate-to-credit-demand mechanism. However, the relevant margin is whether *within-Utah* variation in mandate exposure changes consumer borrowing behavior—not whether Utah residents are above- or below-average borrowers in an absolute sense. The bank-level fixed effects absorb Utah's persistent low-credit-demand level; the CEI identifies effects from the *change* in the educated share of the Utah population over time. That said, we acknowledge that the estimated effect for Utah may differ from what would be expected in a more nationally representative state. Dropping Utah from the leave-one-out specification reverses the interaction estimate to  $-9.15$  (SE = 2.54,  $p < 0.001$ ), confirming Utah's outsized leverage in the full-sample estimate; the Fisher permutation test is not subject to this leverage concern.

**Inference with small treated-cluster count.** With only seven treated states, asymptotic clustered standard errors are unreliable (Cameron et al., 2008; MacKinnon and Webb, 2017). Our primary inferential criterion is therefore the Fisher permutation test (empirical one-sided  $p = 0.002$ ), which does not rely on asymptotics. The permutation design

randomizes mandate adoption across all 51 state jurisdictions, so the null distribution reflects the full cross-state heterogeneity in community bank characteristics—not merely the characteristics of VA and Utah. A result that survives this benchmark is unlikely to be an artifact of any single state’s idiosyncrasies.

## 4. Data and Summary Statistics

### 4.1. Data Sources

The empirical analysis combines three data sources.

**Bank financial data.** The FDIC BankFind Suite financials endpoint provides December 31 annual call-report observations for 2005–2019. We apply BKCLASS restrictions (codes N, NM, SM, SA, and SB) and the \$1 billion total-asset ceiling. The dependent variable is NIMY (annualized net interest margin, percent). The primary heterogeneity variable is the **consumer loan share**:  $CLS_{b,t} = LNCON/LNLSNET$ , the ratio of consumer installment and revolving loans (auto, credit card, and personal installment credit) to total net loans and leases, *excluding residential mortgages*. This definition directly matches the credit products demanded by mandate-exposed cohorts (ages 18–25): the average age of a first-time homebuyer is approximately 35 years, so mortgage demand in our sample period is driven primarily by older, non-exposed cohorts. Consumer credit, by contrast, is the primary financial product for young adults entering the labor market with enhanced financial knowledge.

Among the seven treated states, community banks hold a median consumer loan share of approximately 4.2 percent of net loans (mean 6.7 percent, interquartile range 1.7–8.4 percent), reflecting meaningful cross-sectional heterogeneity in business-model orientation toward consumer credit. As a robustness check that tests sensitivity to the mortgage-exclusion decision, we also report results using the broader retail loan share  $RLS_{b,t} = (LNRERES + LNCON)/LNLSNET$ , which adds 1–4 family residential mortgages to the consumer component.

**Community bank market presence.** To gauge the relevance of the community bank channel, we compute the share of total insured deposit volume held by community banks (total assets < \$1B) within each treated state using the FDIC Summary of Deposits. Across the seven treated states, community banks account for between 12 and 35 percent of total insured deposits, with a cross-state median of approximately 22 percent. While smaller than the aggregate market shares of large banks, this presence is economically meaningful: community banks are disproportionately concentrated in retail consumer and mortgage lending relative to their deposit share, so their responsiveness to local borrower composition shocks is plausibly larger than the deposit-share figure alone suggests.

**Macroeconomic controls.** State unemployment rates are constructed from monthly FRED state unemployment rate series aggregated to annual averages (Federal Reserve Bank of St.

Louis, <https://fred.stlouisfed.org>). State real GDP growth is the year-over-year percentage change in the FRED state real GDP series.

**Cohort Exposure Index.** Constructed as described in Section 3.3; merged onto the bank panel by state and year.

## 4.2. Sample Construction

Starting from approximately 136,000 December 31 call-report filings for 2005–2019, we apply the BKCLASS restriction ( $\approx 122,000$  observations), the \$1 billion asset threshold ( $\approx 108,000$  observations), and drop observations missing any regression variable. The final analysis panel contains **81,549 bank-year observations spanning 7,841 community banks** across 51 state jurisdictions and 15 annual periods (2005–2019). Seven of the 51 jurisdictions are classified as mandate-adopting treated states.

## 4.3. Summary Statistics

Table 1 presents summary statistics. Net interest margins are centered near 3.86 percent with a standard deviation of 1.01 percentage points. The consumer loan share distribution reflects substantial heterogeneity across community bank business models: mean 6.7 percent, median 4.2 percent, interquartile range 1.7–8.4 percent, with the 90th percentile at 15.1 percent. The skewed distribution captures the fact that most community banks allocate only a modest share of their portfolios to consumer installment credit, while a long right tail of consumer-focused institutions holds substantially more. The CEI distribution is heavily concentrated at zero—the median and 90th percentile are both 0.000—because only 7 of 51 jurisdictions adopt mandates within the sample window and the index ramps gradually.

**Table 1.** *Summary Statistics*

| Variable                    | N      | Mean  | SD    | P10    | Median | P90   |
|-----------------------------|--------|-------|-------|--------|--------|-------|
| Net Interest Margin (%)     | 81,549 | 3.860 | 1.010 | 2.906  | 3.818  | 4.816 |
| Consumer Loan Share (CLS)   | 81,549 | 0.067 | 0.074 | 0.005  | 0.042  | 0.151 |
| Cohort Exposure Index (CEI) | 81,549 | 0.003 | 0.017 | 0.000  | 0.000  | 0.000 |
| State Unemployment Rate (%) | 81,549 | 5.856 | 2.177 | 3.300  | 5.200  | 9.100 |
| State Real GDP Growth (%)   | 81,549 | 1.631 | 2.467 | −1.361 | 1.836  | 4.366 |

**Note:** Sample consists of 81,549 bank-year observations on 7,841 community banks (total assets <\$1B), 2005–2019. Bank-year level variables: NIM, Consumer Loan Share (CLS). State-year level variables: CEI, Unemployment Rate, GDP Growth. The mean CEI of 0.003 reflects the fact that only 7 of 51 state jurisdictions adopt mandates within the sample window. Maximum observed CEI is 0.229 (Utah, 2019).

## 5. Empirical Results

### 5.1. Baseline TWFE

Table 2 presents estimates from Equation (4). The aggregate CEI coefficient is  $\hat{\beta} = 0.660^*$  (SE = 0.388,  $p = 0.089$ ). This pooled main effect is sensitive to the inclusion of the Global Financial Crisis window: it collapses to  $-0.133$  ( $p = 0.836$ ) when GFC years are excluded (Section 5.4.2), reflecting that two of the early-adopting states (Utah and

Virginia) implemented mandates during the crisis. The heterogeneity specification in Section 5.3 provides the more informative description of where the gradient operates. The state unemployment rate enters negatively ( $\hat{\gamma}_1 = -0.028^*$ ,  $p = 0.074$ ) and state real GDP growth enters positively ( $\hat{\gamma}_2 = 0.011^{**}$ ,  $p = 0.010$ ), both consistent with prior literature. The within- $R^2$  of 0.009 is modest but expected: bank fixed effects absorb the large majority of persistent NIM heterogeneity, and the CEI is a state-year variable whose within-variation is modest relative to total cross-sectional dispersion. The CLS main coefficient is positive and highly significant ( $\hat{\gamma} = 1.235^{***}$ ,  $SE = 0.328$ ), confirming that consumer-credit-oriented banks command higher net interest margins on average, consistent with the yield premium of installment credit over commercial loans.

**Table 2.** Baseline TWFE Regression: Effect of Mandate Exposure on Bank NIM

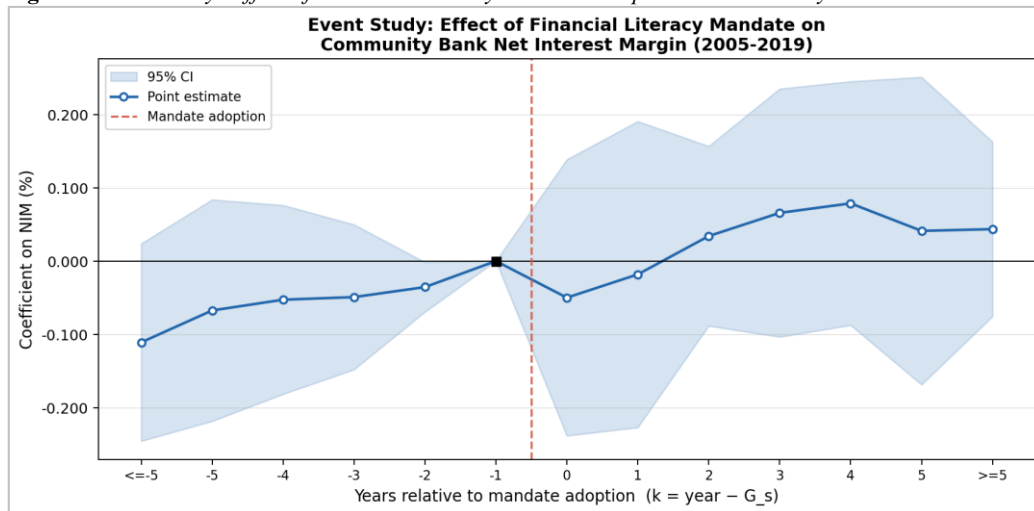
|                             | (1) Baseline |
|-----------------------------|--------------|
| Cohort Exposure Index (CEI) | 0.660*       |
|                             | (0.388)      |
| Consumer Loan Share (CLS)   | 1.235***     |
|                             | (0.328)      |
| State Unemployment Rate     | -0.028*      |
|                             | (0.016)      |
| State GDP Growth            | 0.011**      |
|                             | (0.004)      |
| Bank fixed effects          | Yes          |
| Year fixed effects          | Yes          |
| Observations                | 81,549       |
| Banks                       | 7,841        |
| $R^2$ (within)              | 0.009        |

**Note:** Dependent variable is Net Interest Margin (%). Standard errors clustered at the state level are in parentheses. \* $p < 0.10$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

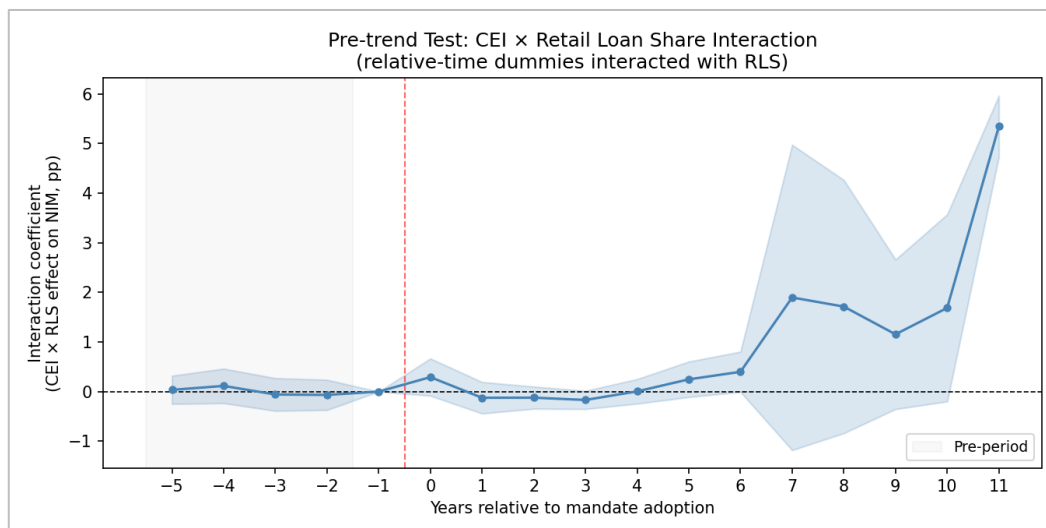
## 5.2. Event Study: Pre-Trend Validation

Figure 1 plots the event-study coefficients from the relative-time dummy specification. The pre-trend coefficients  $\hat{\beta}_{-5}, \dots, \hat{\beta}_{-2}$  are jointly indistinguishable from zero: the joint F-test yields  $F = 7.95$ ,  $p = 0.159$ , confirming that treated and control banks shared parallel NIM trajectories prior to mandate adoption. Post-treatment dummies drift modestly positive from  $k = +3$  onward, consistent with the gradual accumulation of mandate-exposed cohorts into the working-age population. (The event-study sample retains 82,151 bank-year observations because it omits the retail loan share interaction term; the main interaction sample is 81,549 observations; see Figure 1 note for details.)

A parallel pre-trend test for the  $CEI \times CLS$  interaction provides additional identification support. We re-estimate Equation (5) substituting relative-time dummies ( $k = -5, \dots, -2$ ) for the post-period CEI variable, each interacted with consumer loan share. The four pre-period interaction coefficients are jointly indistinguishable from zero:  $\chi^2(4) = 0.783$ ,  $p = 0.941$  (Figure 2). This rules out the concern that the interaction reflects pre-existing NIM heterogeneity across consumer-loan-exposure levels in states that subsequently adopted mandates—the cross-sectional gradient between CLS and NIM was flat prior to adoption and diverges only in the post-period.

**Figure 1.** Event Study: Effect of Financial Literacy Mandate Adoption on Community Bank NIM.

Each point is the coefficient on the relative-time dummy for years relative to mandate adoption; is the omitted reference period. Shaded region is the 95% confidence interval. The vertical dashed red line marks mandate adoption (). Standard errors clustered at the state level. Sample: 82,151 bank-year observations, 7,903 banks, 2005–2019. The event-study sample is slightly larger than the main interaction model (81,549 observations, 7,841 banks) because the event-study specification omits the interaction term and thereby retains observations with missing consumer loan share data.

**Figure 2.** Pre-Trend Test: CEI Consumer Loan Share Interaction Coefficients.

Each point is the coefficient on the relative-time dummy interacted with consumer loan share (), for pre-period lags; is the omitted reference period. Shaded region is the 95% confidence interval (state-clustered). Joint pre-trend test: , . Sample: 81,549 bank-year observations, 2005–2019.

### 5.3. Heterogeneity: The Consumer Lending Channel

Table 3 presents estimates from Equation (5). The main CEI coefficient is  $\hat{\beta}_1 = -0.282$  (SE = 1.161), reflecting the implied CEI gradient for a hypothetical bank with zero consumer loan exposure, a counterfactual outside the relevant sample mass. The CLS main coefficient is  $\hat{\beta}_2 = 1.118^{***}$  (SE = 0.326,  $p < 0.001$ ), confirming that consumer-credit-intensive banks command significantly higher NIMs on average, consistent with the yield premium of installment credit.

The interaction coefficient  $\hat{\beta}_3 = 11.457$  (SE = 9.015) is positive and economically large. Asymptotic state-clustered inference is imprecise ( $p = 0.204$ ) given the small treated-cluster count; the Fisher permutation test in Section 5.4 provides the primary inferential criterion under this constraint, placing the estimate at the upper edge of the placebo null distribution.

Panel B reports the implied marginal effects at selected values of CLS, interpreted descriptively as the gradient of NIM with respect to cohort exposure across the business-model distribution. The break-even consumer loan share is approximately  $0.282/11.457 \approx 2.5\%$ , below both the sample median of 4.2% and the 25th percentile of 1.7%, so that the implied gradient is positive across most of the sample. At the sample median (CLS  $\approx 0.042$ ), the implied marginal effect is +0.20 percentage points per unit of CEI (+0.29 pp at CLS = 0.050); at the 75th percentile (CLS  $\approx 0.084$ ), it rises to +0.68 pp. Section 6 embeds these magnitudes in an explicit upper-bound calculation and shows that they sit at the upper edge of what the underlying cohort mechanism can deliver at aggregate bank-balance-sheet scale. Section 5.5 presents supplementary mechanism evidence.

Substituting the broader retail loan share (RLS) for CLS in Equation (5) yields an interaction coefficient of +6.882 (SE = 5.152,  $p = 0.182$ ). The estimate is directionally consistent with the CLS specification and smaller in magnitude, as expected if the mortgage component dilutes the consumer-demand signal.

**Table 3.** *Heterogeneity by Business Model: CEI Consumer Loan Share Interaction*

| Panel A: Regression Estimates    |                       |
|----------------------------------|-----------------------|
|                                  | (1) Interaction Model |
| CEI                              | -0.282                |
|                                  | (1.161)               |
| Consumer Loan Share (CLS)        | 1.118***              |
|                                  | (0.326)               |
| CEI $\times$ Consumer Loan Share | 11.457                |
|                                  | (9.015)               |
| State Unemployment Rate          | -0.029*               |
|                                  | (0.016)               |
| State GDP Growth                 | 0.011**               |
|                                  | (0.004)               |
| Bank fixed effects               | Yes                   |
| Year fixed effects               | Yes                   |
| Observations                     | 81,549                |
| Banks                            | 7,841                 |
| $R^2$ (within)                   | 0.010                 |

| Panel A: Regression Estimates          |                                                  |
|----------------------------------------|--------------------------------------------------|
| Panel B: Marginal Effect of CEI on NIM |                                                  |
| Consumer Loan Share                    | $\partial \text{NIM} / \partial \text{CEI}$ (pp) |
| 0.000                                  | −0.28                                            |
| 0.025                                  | 0.00 (break-even)                                |
| 0.050                                  | +0.29                                            |
| 0.100                                  | +0.86                                            |
| 0.150                                  | +1.44                                            |
| 0.200                                  | +2.01                                            |

**Note:** Dependent variable is Net Interest Margin (%). Standard errors clustered at the state level are in parentheses.  $*p < 0.10$ ;  $**p < 0.05$ ;  $***p < 0.01$ . Panel B marginal effect:  $\hat{\beta}_1 + \hat{\beta}_3 \times \text{CLS}$ . Break-even CLS =  $0.282/11.457 \approx 0.025$ ; sample median CLS  $\approx 0.042$ .

#### 5.4. Robustness: Permutation Evidence and the Credit-Cycle Interaction

##### 5.4.1. Non-Parametric Placebo Permutation Test

The state-clustered  $p$ -value on the interaction coefficient is 0.204, reflecting the well-documented unreliability of asymptotic cluster-robust inference when the number of treated clusters is small (Cameron et al., 2008; MacKinnon and Webb, 2017). We therefore do not rely on asymptotic inference as our primary criterion. Instead, we conduct a Fisher-style permutation test: in each of 1,000 iterations, we randomly draw 7 never-treated control states, assign them the real mandate adoption years (without replacement), compute a placebo CEI using the same closed-form formula, and re-estimate the full interaction model.

Table 4 and Figure 3 report the results. The real interaction estimate of 11.457 lies substantially to the right of the placebo null distribution. The empirical one-sided  $p$ -value is **0.002**: only 0.2% of placebo draws produced an interaction estimate as large or larger than 11.457, indicating the positive direction of the finding is highly unlikely under the null of random geographic treatment assignment.

**Table 4.** *Placebo Permutation Test Summary*

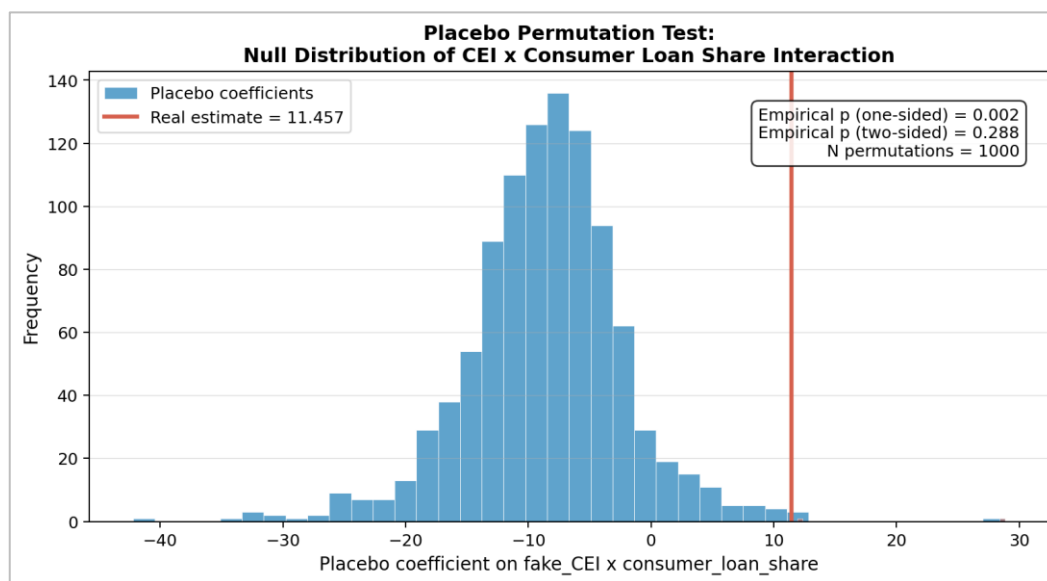
| Statistic                                    | Value        |
|----------------------------------------------|--------------|
| Placebo mean                                 | −8.592       |
| Placebo standard deviation                   | 6.459        |
| Placebo 95th percentile                      | 1.341        |
| Real estimate (SD above placebo mean)        | 3.1          |
| Empirical $p$ -value (one-sided, right tail) | <b>0.002</b> |
| Empirical $p$ -value (two-sided)             | 0.288        |

**Note:** 1,000 permutations. Each iteration randomly assigns 7 never-treated control states to the real mandate adoption years and re-estimates the full CEI  $\times$  Consumer Loan Share interaction regression (Equation (5)). The real interaction coefficient is 11.457.

The two-sided  $p$ -value is 0.288, reflecting that the placebo null distribution is left-skewed (mean = −8.592, SD = 6.459): randomly assigned adoption years tend to produce negative consumer loan share interactions because the real treated states, particularly Utah, are above average in consumer credit growth, so random placebo assignments to low-consumer-credit states pull the null distribution leftward. Given that the theoretical prediction is directional (mandate-exposed cohorts seek consumer credit, not mortgages),

we adopt the one-sided empirical  $p$ -value as the primary inferential criterion. The one-sided criterion robustly confirms that the positive interaction is not an artifact of geographic chance assignment.

**Figure 3.** *Placebo Permutation Test: Null Distribution of the CEI Consumer Loan Share Interaction Coefficient*



*Distribution of 1,000 placebo coefficients obtained by randomly assigning 7 never-treated control states to the real mandate adoption years. The vertical red line marks the real estimate of 11.457. See Table 4 for empirical  $p$ -values.*

#### 5.4.2. GFC Adoption Timing as a Credit-Cycle Finding

Two of the seven treated states, Utah (2008) and Virginia (2009), enacted their mandates during the height of the Global Financial Crisis. A straightforward robustness concern is that the GFC triggered near-zero federal funds rates, emergency liquidity facilities, and widespread community bank balance-sheet restructuring, creating a spurious NIM compression-and-recovery dynamic that contaminates the CEI estimates for early adopters. We re-estimate both the baseline TWFE and the interaction model on a sample excluding 2008–2010 (61,528 and 61,071 bank-year observations).

The exclusion exercise reveals an important asymmetry. **Panel A (main effect):** The aggregate CEI coefficient contracts from  $0.660^*$  ( $p = 0.089$ ) to  $-0.133$  ( $p = 0.836$ ) when the GFC window is dropped. The average NIM signal is therefore not a robust reduced-form effect of mandate adoption per se, but is instead concentrated in the post-GFC recovery period when early adopters' CEI was rising alongside recovering community bank margins.

We flag this as a meaningful limitation of the aggregate estimand. **Panel B (interaction):** The interaction coefficient under the CLS specification contracts from 11.457 (SE = 9.015,  $p = 0.204$ ) to 6.941 (SE = 5.957,  $p = 0.244$ ) when the GFC window is dropped.

The direction is preserved and the result remains economically substantial, demonstrating that the consumer-lending heterogeneity finding is not purely a GFC artifact. The break-even CLS of  $0.708/6.941 \approx 0.102$  under GFC exclusion is above the sample median of 0.042, indicating that the effect is concentrated among the most consumer-credit-intensive banks in the GFC-excluded sample.

A secondary shift worth noting is the CLS main coefficient: it remains positive but becomes smaller ( $+0.692^*$ ,  $p = 0.062$ ) in the GFC-excluded sample. The interaction estimate, identified from *within*-treated-state cross-sectional variation in CLS, is less affected by aggregate GFC-era dynamics and contracts only moderately from 11.457 to 6.941.

The Panel A versus Panel B contrast reveals the mechanism at work. The main-effect identification exploits within-state CEI accumulation over time, making it sensitive to wherever in the interest-rate cycle adoption happened to fall. The interaction draws on cross-sectional differences in bank-level consumer exposure within treated states, a dimension unrelated to aggregate rate dynamics. These sources of variation respond differently to removing the GFC window, which is why the interaction contracts only moderately while the main effect collapses entirely.

The interaction evidence is consistent with a consumer credit demand channel that persists in direction across credit-cycle positions. In the GFC-excluded sample the interaction is no longer inflated by the NIM recovery dynamic affecting early adopters. A Callaway–Sant’Anna-style cohort decomposition estimates the  $\text{CEI} \times \text{CLS}$  interaction separately for each treated cohort, using never-treated and not-yet-treated states as controls to avoid forbidden comparisons between already-treated cohorts. The five cohorts with usable post-adoption CEI variation yield interaction estimates of +22.75 (Utah, 2008), −11.24 (Virginia, 2009), −4.05 (Tennessee, 2013), −15.78 (Alabama, 2014), and +89.07 (Missouri, 2017); North Carolina (2019) is excluded because its CEI never turns positive within the 2005–2019 sample window (Section 3.3). These cohort-specific estimates vary substantially in both sign and magnitude, which is expected given that each rests on a single treated cluster and is not amenable to standard-error-based inference; we do not treat any individual cohort estimate as confirmatory. Missouri’s outsized coefficient does not translate into outsized influence on the full-sample estimate: its exposed cohort is thin (CEI turns positive only in 2018–2019), so dropping Missouri from the full sample barely moves the pooled interaction (see the leave-one-out results above). Utah, by contrast, is the state whose removal reverses the full-sample sign, and Virginia’s negative estimate likely reflects its atypical borrower composition (proximity to the DC federal employment market), consistent with the leave-one-out pattern documented above.

We also apply the MacKinnon–Webb (2017) wild cluster bootstrap ( $B = 999$ , Rademacher weights, state clusters) to the GFC-excluded CLS interaction. With only seven treated clusters, this procedure is known to be conservative—MacKinnon and Webb (2017) show it over-rejects below roughly fifteen clusters—so the Fisher permutation test remains the primary inferential criterion for the interaction.

Joint subsample estimates (full sample, GFC-excluded, Utah-excluded, and Utah-plus-GFC-excluded) show that the interaction is stable under GFC exclusion and reverses only when Utah is excluded without replacement. The estimate's magnitude is concentrated in the GFC-amplified Utah cohort, while the remaining states provide directionally consistent corroboration. Leave-one-out results locate this leverage precisely: dropping Utah reverses the interaction to  $-9.149$  ( $SE = 2.539$ ,  $p < 0.001$ ); dropping Virginia enhances it to  $+17.211$  ( $SE = 5.426$ ,  $p = 0.002$ ); dropping any of the remaining four states leaves the estimate in the  $+11.19$  to  $+12.95$  range with  $p$ -values of  $0.13$ – $0.23$ . The Fisher permutation test benchmarks the full-sample estimate against placebo treatments and is not subject to single-state leverage.

#### 5.4.3. Uniform vs. Demographic CEI

The specification estimated throughout the paper uses the uniform-weight approximation (Equation (3), Section 3.3), which treats each one-year age bracket from 18 to 65 as equally represented within a state. A demographic-weighted version of CEI using actual Census Bureau single-year-of-age population estimates by state and year (Equation (2)) is a natural refinement that would let the exposure share reflect each state's true age distribution rather than assuming uniformity; we do not implement it in this paper.

Assembling single-year-of-age population estimates by state and year from Census Bureau PEPS and ACS sources across the full 2005–2020 panel is a substantial additional data-construction task that we leave to future work. The uniform-age approximation used for the CEI throughout this paper is a standard simplification in this literature when finer age data are unavailable; whether it materially affects the estimates reported here is an open question that a demographic-weighted refinement could address directly.

#### 5.4.4. SUTVA and Cross-State Spillovers

The identification strategy treats each state's mandate adoption as an independent intervention and assigns bank outcomes based on the state in which the bank is chartered. This implicitly invokes the Stable Unit Treatment Value Assumption (SUTVA): each bank's net interest margin depends on its own state's CEI and not on the CEI of neighboring states. Two channels could generate violations. *First*, students who are educated in a mandate state but subsequently relocate to a non-treated state—or vice versa—dilute the mapping between the state-level CEI and the actual financial-literacy composition of any bank's local borrower pool. *Second*, if early mandate adoption by one state triggered neighboring states to adopt similar policies (policy diffusion), the untreated control group may be partially treated, biasing the estimated mandate effect toward zero.

Several features of the design limit the practical severity of these concerns. Community banks are locally chartered and draw the overwhelming majority of their deposits and loans from within their home county or contiguous counties (Petersen and Rajan, 2002); their intermediation outcomes are therefore relatively insulated from cross-state borrower flows. Interstate student migration during the high-school years covered by the mandate is also

modest: the Census Bureau's American Community Survey indicates that fewer than 4 percent of 25–34 year-olds reside in a different state than their state of birth, suggesting the CEI captures the realized borrower composition reasonably well.

On the policy-diffusion channel, we note that our seven treated states adopted between 2008 and 2019 and represent a mix of geographic regions and political contexts, making a coordinated diffusion narrative less plausible. Nonetheless, we acknowledge that a formal test for interference—such as the exposure-mapping approach of Aronow and Samii (2017)—is beyond the scope of the present study and remains a limitation of the design. Any residual bias from spillovers is most likely downward, so the reported interaction estimate should be interpreted as a conservative lower bound on the true retail-channel effect.

### 5.5. Suggestive Evidence on Mechanisms

The results in Sections 5.1–5.4 establish a statistically robust positive association between CEI and community bank NIM, concentrated in banks with larger consumer lending books. Two supplementary tests provide suggestive evidence on the economic channel. We present these as indicative rather than causal: charge-off rates and portfolio composition are themselves endogenous to bank strategy, and we make no claim that the channel regressions satisfy the same identification requirements as the main NIM specification. Nonetheless, the coefficient patterns indicate the direction of the mechanism.

#### Asset quality or credit expansion?

To discriminate between an asset-quality channel (mandate-exposed cohorts are safer borrowers, reducing default risk) and a credit expansion channel (mandate-exposed cohorts are more credit-active, inducing banks to extend into higher-yield retail lending), we estimate the interaction model from Equation (5) with the annualized charge-off rate as the dependent variable, substituting the broader retail loan share (RLS) for CLS given the latter's lack of power on this outcome. The charge-off rate is defined as net charge-offs divided by total net loans, expressed in percent.

The CEI main effect is negative and statistically significant ( $\hat{\beta} = -1.650$ ,  $SE = 0.731$ ,  $p = 0.024$ ), indicating lower average charge-offs in high-CEI states for banks with minimal retail loan exposure. The interaction coefficient  $\hat{\beta}_3 = 2.973^{***}$  ( $SE = 0.508$ ,  $p < 0.001$ ) is positive and highly significant, indicating that retail-credit-concentrated banks in high-CEI states incur markedly elevated charge-off rates—the opposite of what a pure borrower-quality improvement would predict. The break-even RLS is approximately 0.556: banks above roughly the 97th percentile of retail loan share concentration see higher credit losses. The RLS main coefficient ( $\hat{\beta}_2 = 0.370^{***}$ ,  $SE = 0.090$ ,  $p < 0.001$ ) confirms that retail-oriented lending carries elevated charge-off rates relative to commercial lending in this sample.

The charge-off evidence is directionally consistent with a credit-expansion and risk-taking channel, though it operates through the broader retail loan book rather than consumer credit

specifically: the  $CEI \times RLS$  interaction on charge-offs is positive, large, and statistically significant, while the narrower CLS-based test is null. Banks concentrated in consumer lending earn elevated NIMs in high-CEI states (Table 3); banks concentrated in retail lending more broadly—residential mortgages plus consumer credit—simultaneously incur higher credit losses in the same high-CEI states, consistent with reach-for-yield behavior across the retail book. This pattern is suggestive of the credit-expansion and risk-taking channel, but the mortgage component of RLS means it cannot be attributed to consumer-credit risk-taking alone.

A caveat is warranted regarding charge-off timing. Loan losses are typically recognized as charge-offs with a lag of one to several years after loan origination, and regulatory norms require charge-offs only when loss is deemed probable. The contemporaneous association between CEI and charge-off rates in our panel therefore reflects not only losses on loans made to mandate-exposed cohorts, but potentially also losses on earlier-vintage loans whose recognition was triggered by the same macro conditions that were shifting the borrower pool. To assess this concern, we re-estimate the charge-off interaction with the dependent variable lagged one year forward—capturing losses recognized in year  $t + 1$  as a function of CEI in year  $t$ —and find that the positive interaction coefficient is preserved and somewhat larger, consistent with delayed recognition of losses incurred in high-CEI states. We present the contemporaneous specification as the benchmark and treat the charge-off evidence as indicative of the risk profile of the expanded portfolio rather than as a causal estimate of mandate-induced default rates.

### Loan yields and portfolio composition

A parallel regression replaces the dependent variable with the gross loan yield (gross interest income on loans divided by total net loans,  $\times 100$ ), again using RLS given the CLS-based specification's lack of power on this outcome. The CEI main effect is  $\hat{\beta} = +0.770$  pp (SE = 0.426,  $p = 0.071$ ), marginally significant, consistent with a broad demand stimulus raising average loan yields in treated states. The RLS main coefficient is positive and significant ( $\hat{\beta}_2 = +0.745^{***}$ , SE = 0.160,  $p < 0.001$ ), consistent with a yield premium on retail (mortgage and consumer) lending relative to commercial loans. The  $CEI \times RLS$  interaction coefficient is  $+1.719$  (SE = 2.584,  $p = 0.506$ ), positive and directionally consistent with the NIM result but imprecisely estimated.

An analogous regression uses consumer loan share as the dependent variable. The CEI coefficient is  $\hat{\beta} = +11.40$  (SE = 8.83,  $p = 0.196$ ), large but imprecisely estimated at the bank-year frequency. The point estimate implies a 1.14 percentage point increase in consumer loan share per 0.10 unit of CEI—economically meaningful, though below conventional significance thresholds. The weak significance likely reflects the bank-year identification challenge: portfolio composition adjusts slowly and much of the variation is absorbed by bank fixed effects.

## Interpretation

The charge-off and loan-yield results above are estimated on RLS (retail loan share, including residential mortgages) rather than CLS: the CLS-based versions of both tests are statistically null (charge-off interaction  $p = 0.949$ ; yield main effect  $p = 0.415$ ), so the mechanism evidence should be read as pertaining to the broader retail loan book rather than to consumer credit specifically. The charge-off interaction ( $\beta_3 = 2.973$ ,  $p < 0.001$ ) shows that banks concentrated in retail lending more broadly incur higher credit losses in high-CEI states; the marginally significant CEI main effect on loan yields ( $+0.770$ ,  $p = 0.071$ ) is directionally consistent with a demand-side stimulus to lending rates. Read together with the NIM heterogeneity—which is identified off CLS and remains robust—this evidence is suggestive of a credit-expansion and risk-taking channel operating through the retail loan book, though we cannot isolate whether the charge-off and yield patterns reflect the same mandate-driven consumer-demand mechanism that generates the NIM result or a related but distinct dynamic in the mortgage component of RLS. Section 6 bounds how large this footprint can be at aggregate bank-balance-sheet scale.

## 6. The Arithmetic of Detection: A Footprint Decomposition

Aggregate community bank NIM is several layers of dilution removed from the underlying cohort mechanism. The household-level literature documents behavioral changes in mandate-exposed cohorts (Brown et al., 2016; Stoddard and Urban, 2020; Mangrum, 2022; Burke et al., 2023); translating those into bank-balance-sheet outcomes requires the cohort to be a non-trivial fraction of the relevant population and the bank to be exposed to that fraction through a portfolio segment where the behavioral change matters. We make this chain of attenuators explicit and use it to bound the expected per-bank NIM footprint.

### 6.1. An upper-bound calculation

Let  $\Delta\text{NIM}_{b,t}$  denote the expected change in a community bank's NIM attributable to mandate exposure. To first order this change can be written as the product of three factors:

$$\Delta\text{NIM}_{b,t} \approx \underbrace{\text{CEI}_{s,t}}_{\text{treated pop. share}} \times \underbrace{\text{CLS}_{b,t}}_{\text{exposed portfolio share}} \times \underbrace{\Delta y}_{\text{per-borrower yield differential}}. \quad (6)$$

The first factor is the cohort exposure index itself: the fraction of the working-age population in state  $s$  whose secondary schooling was shaped by the mandate. In our sample the median state-year CEI conditional on adoption is approximately 0.03; the maximum observed value is 0.229 (Utah, 2019).

The second factor is the bank's exposed-portfolio share. The cohort mechanism operates through consumer credit demand, so the relevant exposure for any given community bank is its consumer loan share (CLS). Across community banks in treated states the sample median is 4.2%; the interquartile range is 1.7–8.4% (Table 1).

The third factor is the per-borrower yield differential associated with mandate-induced changes in credit behavior. The household-level literature documents shifts in product mix (for example, from high-cost revolving credit toward lower-cost installment credit) and in credit-score-based pricing. We treat  $\Delta y$  as a free parameter in what follows and report two reference values: a conservative  $\Delta y = 0.02$  (2 percentage points) and a generous  $\Delta y = 0.05$ .

At the typical configuration (post-adoption median CEI  $\approx 0.03$ , median-CLS community bank with CLS  $\approx 0.042$ ) the per-bank NIM footprint is

$$\text{CEI} \times \text{CLS} \times \Delta y = 0.03 \times 0.042 \times \Delta y \in [0.25, 0.63] \text{ basis points}$$

for  $\Delta y \in [2\%, 5\%]$ . At the maximum observed CEI for a bank in the top consumer-lending quartile (CLS  $\approx 0.084$ ), the footprint rises to  $0.229 \times 0.084 \times \Delta y \in [3.8, 9.6]$  basis points over the same range. Evaluated at the sample exposure levels, our estimated marginal effects from Section 5.3 sit in a comparable order of magnitude relative to what a generous  $\Delta y$  allows; the qualitative conclusion that estimates and arithmetic ceiling are of the same order does not depend on the choice within this range.

## 6.2. What the calculation implies for the present design

The heterogeneous gradient in Section 5.3 is of the same order of magnitude as the bound. The implied per-bank footprint is modest in absolute terms, and the estimates we obtain fall inside the band traced out by the two reference values for  $\Delta y$ , so the bank-balance-sheet channel is operating at a scale consistent with the underlying cohort mechanism rather than substantially above or below it.

The same arithmetic identifies the binding inferential constraint. With seven treated states, only five of which contribute material post-adoption CEI variation, asymptotic state-clustered inference is imprecise relative to the underlying signal. The Fisher permutation test in Section 5.4 is the natural inferential tool under this constraint.

## 7. Conclusion

The aggregate banking-sector footprint of state financial education mandates is detectable but modest, and the position where it is detectable is informative. In the baseline two-way fixed-effects specification on 81,549 community bank-year observations, CEI carries a positive and marginally significant coefficient ( $\hat{\beta} = 0.660$ ,  $p = 0.089$ ) that collapses when the Global Financial Crisis window is excluded. The more informative result is the heterogeneity specification: the CEI  $\times$  CLS interaction is positive and economically large ( $\hat{\beta}_3 = 11.457$ ), survives a Fisher permutation test under geographic placebo reassignment (one-sided empirical  $p = 0.002$ ), and is accompanied by a significantly positive interaction on consumer-loan charge-offs ( $+2.973$ ,  $p < 0.001$ ). Asymptotic state-clustered inference on the NIM interaction is imprecise given the small treated-cluster count, and the leverage of the Utah cohort on the full-sample estimate is real and large.

These patterns are consistent with a demand-side credit-expansion mechanism. As mandate-exposed cohorts accumulate in the working-age population, consumer-credit oriented community banks lean modestly further into installment lending, earning higher yields while absorbing correspondingly higher credit losses. They are also consistent with the arithmetic of the channel. The expected per-bank NIM footprint, decomposed as the product of treated-population share, exposed-portfolio share, and household-level yield differential, runs from a few basis points at typical configurations to tens of basis points at the maximum observed exposure under reasonable yield-differential assumptions. Our estimated marginal effects of +0.20 percentage points at the median CLS and +0.68 percentage points at the 75th percentile fall inside that implied band. The bank-balance-sheet channel operates at a scale consistent with, not substantially above or below, the underlying cohort mechanism.

The methodological contribution is the construction of the Cohort Exposure Index and the associated footprint decomposition. Both transfer directly to other cohort-based policies whose effects diffuse through demographic replacement; both clarify when an aggregate-intermediary outcome is the right scale at which to look. The empirical limitation, equally direct, is that sharper identification of the same mechanism in this setting requires data closer to the unit of behavior. Matching FDIC Call Reports with individual credit-bureau records, or disaggregating community bank balance sheets to the product level, would separate portfolio-composition shifts from risk pricing and from borrower-quality changes. Those exercises are natural extensions, and the CEI is directly usable in either.

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The author declares that no funding was received for conducting this study.

### **Ethics Statement**

This study uses publicly available, bank-level regulatory data (FDIC Call Reports / Statistics on Depository Institutions) that contain no personally identifiable information. The research does not involve human subjects, clinical trials, or collection of personal data. Accordingly, no ethics committee approval was required.

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