Proposal: Leverage Constraints and Small-Business Credit: Evidence from the 2014 U.S. Supplementary Leverage Ratio

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Introduction

After the 2008 Global Financial Crisis, U.S. regulators introduced a series of capital reforms to strengthen the resilience of large financial institutions. A key measure was the Supplementary Leverage Ratio (SLR), finalized in 2014 by the Basel Committee on Banking Supervision as part of the Basel III framework. Unlike risk-based capital (RBC) ratios that weight assets by perceived risk, the SLR constrains overall balance-sheet size through a simple, non-risk-weighted limit on leverage exposure. The introduction of the SLR aimed to improve transparency and limit banks' reliance on internal risk assessments (Miller, 2016; Basel Committee on Banking Supervision, 2014).

This regulatory shift had important implications for banks' balance sheet management. Under the earlier RBC regime, low-risk assets such as Treasuries and reserves were favored because they carried near-zero risk weights. However, once the SLR became binding, this uniform capital charge increased the return per unit of regulatory capital on higher-yield exposures, reducing incentives to hold low-yield safe assets (Acosta Smith et al., 2017; Choi et al., 2020). At the same time, quantitative-easing policies greatly expanded system reserves (Martin et al., 2016), making the leverage ratio more likely to bind for large institutions. Although individual banks may attempt to reduce low-yield reserve holdings when the leverage ratio binds, the aggregate supply of reserves is largely determined by the Federal Reserve's policy. Hence, leverage constraints primarily operate through adjustments in portfolio composition rather than through changes in aggregate reserves.

Building on this mechanism, this paper examines how the SLR-induced portfolio rebalancing affected credit supply, specifically small-business lending reported under the Community Reinvestment Act (CRA). The focus on CRA lending is motivated by two factors. First, when the leverage ratio binds, the uniform capital charge increases the relative return on higher-spread loans, thereby strengthening banks' incentives to extend CRA-eligible small-business credit. Second, small firms are important to local economies, accounting for nearly half of U.S. private-sector employment (U.S. Small Business Administration, Office of Advocacy, 2024), and higher CRA lending is associated with greater business formation and faster local growth (Rupasingha and Wang, 2017). These

considerations motivate an empirical test of whether the SLR altered banks' CRA small-business lending, with implications for local economic outcomes.

Because the SLR initially applied only to the largest U.S. bank-holding companies, its introduction constitutes an exogenous regulatory shock that created a quasi-experimental setting for identification (Choi et al., 2020). The empirical analysis employs a difference-in-differences framework comparing constrained and unconstrained banks before and after the 2014 implementation to test whether SLR-constrained institutions expanded CRA lending and whether counties more exposed to these banks experienced larger changes in small-business credit.

While existing research shows that the SLR reduced intermediation in Treasury and repo markets and pushed banks toward higher-yield securities, there is limited evidence on its effects on credit supply. In particular, no prior study has directly examined its effects on CRA small-business lending. This paper fills that gap by constructing a 2009–2019 bank–county panel linking FFIEC CRA disclosures with regulatory filings (FR Y-9C, FR Y-15, Call Reports) to trace both bank-level portfolio shifts and their local transmission to small-business credit. The analysis also extends to the COVID-19 period, when the temporary SLR relief provides an additional natural experiment to contrast the effects of tightening and subsequent relaxation.

This paper has the following research questions:

RQ1: Does the introduction of SLR have an impact on the count and amount of CRA-reported small-business loans?

RQ2: Do counties with greater exposure to SLR-constrained banks experience larger changes in CRA small-business lending after the rule was finalized?

RQ3: How did the temporary relaxation of the SLR during the COVID-19 period alter these credit-supply effects?

Literature

(1) Capital Regulation Background

Before 2014, U.S. banks were primarily constrained by risk-based capital (RBC) requirements. In 2014, the Basel Committee on Banking Supervision finalized the Supplementary Leverage Ratio (SLR) as part of the Basel III framework (Basel Committee on Banking Supervision, 2014). The two capital ratios are defined as

$$\text{RBC ratio} = \frac{\text{Tier 1 capital}}{\text{risk-weighted assets (RWA)}}, \tag{1a}$$

$$\text{SLR} = \frac{\text{Tier 1 capital}}{\text{total leverage exposure (TLE)}}. \tag{1b}$$

These ratios differ in the denominator. In Eq. (1a), the denominator is risk-weighted assets (RWA), which assign capital charges according to asset risk. In Eq. (1b), the denominator is total leverage exposure (TLE), which includes all on-balance-sheet assets and certain off-balance-sheet exposures such as derivatives and securities financing transactions (Basel Committee on Banking Supervision, 2014).

In the U.S., the SLR initially applied to bank holding companies with at least $250 billion in consolidated assets or more than $10 billion in foreign exposure, subject to a minimum SLR of 3%. U.S. Global Systemically Important Banks (G-SIBs) were subject to an enhanced SLR (eSLR) with an additional 2 percentage-point buffer at the holding-company level, raising their effective requirement to 5 percent. Given their size and market share, these institutions were the first to adjust their balance-sheet management under the new regime, providing an empirical reference for later adopters (Choi et al., 2020).

(2) From RBC to SLR: Implications for Bank Portfolios

Empirical studies show that the different capital regulations can alter banks' portfolio choices. Before 2014, risk-based capital (RBC) requirements served as the binding constraint for most U.S. banks. Because Treasuries and reserves carried near-zero risk weights while commercial loans faced higher weights, banks shifted portfolios toward low-risk assets, crowding out riskier lending such as commercial credit (Haubrich and Wachtel, 1993; Jacques and Nigro, 1997; Kashyap et al., 2010; Gambacorta and Mistrulli, 2004; Gropp et al., 2019).

The Supplementary Leverage Ratio (SLR), introduced in 2014 as a non-risk-weighted requirement, applies uniformly across all exposures (Basel Committee on Banking Supervision, 2014). This change altered bank balance-sheet management. Favara et al. (2022) show that the SLR compressed balance-sheet capacity and reduced intermediation in Treasuries and repos. Choi et al. (2020) find rebalancing away from low-yield safe assets toward higher-yield securities among SLR-constrained banks. Meanwhile, quantitative-easing policies significantly expanded reserve holdings (Martin et al., 2016), making the leverage ratio more likely to bind for large institutions. While individual banks facing binding constraints may attempt to shed reserves, the aggregate supply of reserves is policy-determined by the Federal Reserve. Consequently, the SLR primarily operates through portfolio composition rather than through changes in aggregate reserves.

These portfolio adjustments may also have implications for banks' external credit supply, particularly in small-business lending markets where spreads are higher and loans are more capital-intensive.

(3) Credit-Supply Effects of Leverage Constraints

By limiting balance-sheet capacity, capital and leverage requirements can influence banks' willingness and ability to extend credit. A growing body of research documents these effects. Using

Community Reinvestment Act (CRA) data, Cortés et al. (2020) find that stress-tested banks reduced small-business lending and increased loan rates, while Demyanyk (2019) provide a policy overview that reaches similar conclusions. Marodin (2021) further shows that tighter leverage limits induced higher mortgage spreads and riskier originations at constrained lenders. However, systematic evidence on how leverage-based constraints such as the SLR affect CRA small-business credit remains scarce.

(4) Research Gap and Hypotheses

In sum, prior research shows that the SLR reshaped portfolios in securities and repo markets, and that CRA lending responds to bank constraints. However, no study has directly linked the SLR to CRA small-business credit. This paper fills this gap and sets out the following hypotheses:

H1: After the SLR, constrained banks expanded total CRA loan counts and amounts.

H2: Counties with greater deposit shares held by SLR-constrained banks experienced larger increases in CRA small-business lending after the SLR.

H3: During the COVID-19 period, the temporary relaxation of the SLR reversed these effects, reducing high-spread small-business lending relative to the tightening period.

Data

Three main datasets are used for 2009–2019 to yield a symmetric five-year window before and after the 2014 SLR shock, which is consistent with the difference-in-differences and event-study specifications. To incorporate the COVID-period SLR relief, the sample will be extended to 2024 to form a symmetric four-year window around the 2020.

(1) CRA data

The primary data source is the FFIEC Community Reinvestment Act (CRA) Disclosure files, which report annual small-business loan originations at the bank–county level. For each reporting bank and year, loan originations are aggregated to borrowers' counties using FIPS codes.

The outcome variables include loan counts and dollar amounts in four size categories: $< \$100k$, \$100–250k, \$250k–\$1m, and total $< \$1m$. To address skewness while retaining zeros, variables are transformed as $\ln(x + 1)$.

Each CRA reporting institution is linked to its top-tier bank holding company through RSSD identifiers and merged with regulatory data from the FR Y-9C and FR Y-15 reports. This linkage enables the construction of consistent bank–county–year observations that can be matched to regulatory characteristics at the holding-company level.

(2) Bank data

The bank-level panel comes from the Federal Reserve Board's National Information Center (NIC) and uses FR Y–9C regulatory reports for top-tier U.S. bank holding companies. Entities are linked via RSSD identifiers.

Treatment status is assigned based on pre-policy information (2014Q2) and is held fixed over time. The treated group comprises the 15 largest BHCs subject to the 2014 SLR final rule, defined as institutions with consolidated assets $\geq$ \$250 billion or on-balance-sheet foreign exposure $>$ \$10 billion. The control group consists of the next 20 largest BHCs with total assets between \$50 and \$250 billion. The post-policy period starts in 2014, the year the SLR was finalized.

The key regulatory variable is the bank-level SLR_b^{2014} defined in Eq. (1b). It is measured in 2014 and kept constant for all years in bank b. I compute SLR_b^{2014} using Tier 1 capital from FR Y–9C and a proxy for total leverage exposure from Call Report data.

Following Choi et al. (2020), the bank-level control vector includes: (i) log total assets; (ii) the Tier–1–to–RWA (RBC) ratio; (iii) a liquidity-coverage proxy based on the New York Fed Liquidity Stress Ratio; and (iv) the noninterest-to-interest income ratio. All bank controls are lagged by one year.

(3) County-level data

County–year lending outcomes are constructed by aggregating FFIEC CRA small-business records to borrowers' counties using FIPS codes. Local economic conditions are drawn from standard federal sources. County GDP is obtained from the BEA Regional Economic Accounts; median household income from the Census Bureau's American Community Survey (ACS; five-year estimates to ensure coverage); unemployment rates from BLS Local Area Unemployment Statistics (LAUS); and poverty rates from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE). These controls enter as lagged values $W_{c,t-1}$, with levels (GDP, income) in logs and rates (unemployment, poverty) in percentages.

County exposure to SLR-covered banks is measured using the FDIC Summary of Deposits (SOD) as the baseline (2014) deposit share in county c:

$$SLRShare_c \;=\; \frac{\sum_{b\in\mathcal{T}} D_{bc,2014}}{\sum_{j} D_{jc,2014}},$$

where $D_{bc,t}$ denotes deposits of bank b in county c at year t, and $\mathcal{T}$ is the set of bank affiliated (via RSSD linkages) with holding companies subject to the 2014 SLR final rule. Since the SLR was finalized in 2014, $SLRShare_c$ is fixed at 2014 and treated as predetermined. It is a county-level exposure share, distinct from the bank-level leverage requirement SLR_b^{2014} defined in Eq. (1b).

Methodology

The 2014 SLR was introduced to strengthen bank capital and was not designed to target CRA lending. Therefore, it is treated as an exogenous policy shock for CRA small-business credit. Thus, the empirical strategy is based on a difference-in-differences (DiD) design.

(1) Bank-level baseline

The baseline specification is as follows:

$$\begin{aligned} Y_{bct} = \alpha + \beta(Treatment_b \times Post_t) \\ + \gamma X_{b,t-1} + \mu_b + \lambda_{ct} + \varepsilon_{bct}. \end{aligned} \tag{2}$$

where Y_{bct} denotes lending outcomes of bank b in county c at time t. The treatment indicator equals one for SLR banks and zero for control banks. $Post_t$ equals one for years 2014 and onward, and zero otherwise. Bank-level controls $X_{b,t-1}$ include log assets, Tier 1 capital to risk-weighted assets, liquidity, and noninterest income share. County–year fixed effects absorb all local shocks common to banks within a county–year, while bank fixed effects control for time-invariant bank heterogeneity. Standard errors are clustered at the bank level.

The coefficient of interest is β on $Treatment_b \times Post_t$. It equals the post-2014 change in Y_{bct} for treated banks minus the contemporaneous change for control banks, holding $X_{b,t-1}$, bank fixed effects μ_b, and county–year fixed effects λ_{ct} constant.

(2) Bank-level SLR

As an extension, I allow the treatment effect to vary with the bank's baseline leverage constraint by interacting the post period with the bank-level SLR measured in 2014:

$$\begin{aligned} Y_{bct} = \alpha + \beta\,(Treatment_b \times Post_t) \\ + \theta\,(Treatment_b \times Post_t \times SLR_b^{2014}) \\ + \phi\,(Post_t \times SLR_b^{2014}) \\ + \gamma X_{b,t-1} + \mu_b + \lambda_{ct} + \varepsilon_{bct}. \end{aligned} \tag{3}$$

where the variable definitions, control vectors, fixed-effects structure, and clustering of standard errors are as specified in Eq. (2). SLR_b^{2014} denotes bank b's SLR measured in 2014, computed according to the definition in Eq. (1b), and is held fixed over time in this specification.

The object of interest is the treatment effect conditional on the bank's baseline leverage constraint. The coefficient θ measures how this effect varies with the bank's SLR, so that the post-policy effect for bank b equals $\beta + \theta\, SLR_b^{2014}$, holding $X_{b,t-1}$, bank fixed effects μ_b, and county–year fixed effects λ_{ct} constant. Thus, θ captures how the treatment effect scales with the SLR constraint, while

ϕ absorbs any post-2014 change associated with SLR_b^{2014} that is common to treated and control banks.

(3) Heterogeneity by County Characteristics

To examine the heterogeneous effects of SLR on the CRA loan, I extend the baseline specification by interacting the treatment effect with county-level characteristics. The regression equation is:

$$\begin{aligned} Y_{bct} = \alpha + \beta(Treatment_b \times Post_t \times W_{ct}) \\ + \mu_{bt} + \lambda_{ct} + \varepsilon_{bct}. \end{aligned} \tag{4}$$

where W_{ct} denotes one county-level variable at a time. Specifically, I estimate separate regressions using county GDP, median household income, unemployment rate, and poverty rate as alternative measures of local demand and credit risk. Bank–year fixed effects absorb all time-varying bank-level factors, while county–year fixed effects capture local economic shocks. These specifications test whether the SLR effect is stronger in counties with different economic conditions.

(4) County-level SLR exposure

Finally, I implement a county-level design exploiting variation in local exposure to SLR banks:

$$\begin{aligned} Y_{ct} = \alpha + \beta(SLRShare_c \times Post_t) \\ + \gamma' W_{c,t-1} + \mu_c + \lambda_t + \varepsilon_{ct}. \end{aligned} \tag{5}$$

where Y_{ct} denotes aggregate lending outcomes in county c at year t, and $SLRShare_c$ is the share of county c deposits held by SLR-constrained banks in 2014. County and year fixed effects absorb time-invariant heterogeneity and common shocks. The coefficient β indicates whether counties more exposed to SLR banks experienced larger shifts in lending after 2014.

(5) Temporary SLR Relief during COVID-19

In April 2020, the Federal Reserve temporarily relaxed the SLR requirement for large bank holding companies, allowing them to exclude reserves and U.S. Treasury securities from total leverage exposure. The measure was in place until March 2021. This temporary relief constitutes a reversal of the 2014 tightening and provides an additional natural experiment to test whether the earlier effects on small-business lending were undone when leverage constraints were eased.

To capture this episode, I estimate a difference-in-differences specification similar to the baseline model, replacing the post-2014 indicator with a COVID-period indicator that equals one during the SLR relief window (2020–2021):

$$Y_{bct} = \alpha + \beta\,(Treatment_b \times COVID_t) + \gamma X_{b,t-1} + \mu_b + \lambda_{ct} + \varepsilon_{bct}. \tag{6}$$

where $COVID_t$ equals to one for the temporary SLR relief period (2020–2021) and zero otherwise, and $Treatment_b$ identifies banks subject to the original 2014 SLR rule. The coefficient β captures whether the easing of the leverage ratio reversed the earlier effects on small-business lending.

Preliminary Results

Bank-level baseline (Tables 1–2). For loan counts, *Treatment*×*Post* is positive and significant for loans under \$100k and for the total under \$1m. For example, in column (2), the estimate 0.527 implies $e^{0.527} - 1 \approx 69\%$ more loans for constrained banks after the SLR shock. Counts for \$100–250k and \$250k–\$1m are not significant. For loan amounts, coefficients are small and not statistically significant.

Heterogeneity by County Characteristics (Tables 3–6). Triple-interaction estimates show larger post-2014 effects in counties with higher *GDP* and *median income*. Interactions with *poverty* and *unemployment* are negative and significant, indicating weaker or negative responses in distressed areas. Overall, the treatment effect is heterogeneous and concentrated in economically advantaged locations.

County-level SLR exposure (Tables 7–8). Using pre-period deposit shares to measure local exposure to SLR banks, more-exposed counties have larger post-2014 increases in CRA lending. For counts, the exposure effect are positive and significant for loans below \$250k and insignificant for loans \$250k–\$1m. For amounts, coefficients of *SLRshare*×*Post* are all positive and statistically significant.

Table 1: Baseline DiD results: Loan counts

	(1) Total	(2) Total	(3) less100k	(4) less100k	(5) 100–250k	(6) 100–250k	(7) 250k–1m	(8) 250k–1m
Treatment×Post	0.499**	0.527**	0.528*	0.546**	0.0314	0.0670	0.0210	0.0566
	(0.020)	(0.013)	(0.050)	(0.032)	(0.711)	(0.465)	(0.813)	(0.495)
Ln(Assets)		-0.387		-0.567		0.162		0.208
		(0.414)		(0.330)		(0.231)		(0.155)
Capital		-3.062		-4.121		0.661		0.0117
		(0.522)		(0.439)		(0.633)		(0.991)
Non-interest Inc. Ratio		0.00253**		0.00282*		0.000269		0.000505
		(0.014)		(0.057)		(0.671)		(0.407)
Liquidity		-0.194***		-0.163**		-0.0794***		-0.0797***
		(0.007)		(0.033)		(0.002)		(0.000)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	261113	242805	261113	242805	261113	242805	261113	242805
R^2	0.499	0.515	0.492	0.509	0.358	0.373	0.371	0.385

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Baseline DiD results: Loan amounts

	(1) Total	(2) Total	(3) less100k	(4) less100k	(5) 100–250k	(6) 100–250k	(7) 250k–1m	(8) 250k–1m
Treatment×Post	0.220	0.219	0.337	0.294	0.0814	0.170	0.107	0.214
	(0.335)	(0.387)	(0.145)	(0.215)	(0.793)	(0.593)	(0.783)	(0.527)
Ln(Assets)		0.0505		-0.547		0.544		0.867
		(0.898)		(0.261)		(0.267)		(0.188)
Capital		1.355		-4.100		4.820		3.907
		(0.716)		(0.500)		(0.336)		(0.449)
Non-interest Inc. Ratio		0.00139		0.00251		-0.000723		0.00189
		(0.158)		(0.100)		(0.778)		(0.385)
Liquidity		-0.344***		-0.192**		-0.279***		-0.340***
		(0.000)		(0.040)		(0.000)		(0.000)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	261113	242805	261113	242805	261113	242805	261113	242805
R^2	0.551	0.562	0.461	0.478	0.361	0.374	0.383	0.395

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Heterogeneity by county GDP

	Log num of loans				Log loan amount			
	(1) All	(2) less100k	(3) 100–250k	(4) 250k–1m	(5) All	(6) less100k	(7) 100–250k	(8) 250k–1m
Treatment×Post×GDP	0.391***	0.400***	0.186**	0.166**	0.421***	0.413***	0.557***	0.528**
	(0.000)	(0.000)	(0.016)	(0.026)	(0.000)	(0.000)	(0.002)	(0.016)
Bank×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	255523	255523	255523	255523	255523	255523	255523	255523
R^2	0.600	0.605	0.394	0.400	0.596	0.524	0.396	0.414

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Heterogeneity by county median income

	Log num of loans				Log loan amount			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	less100k	100–250k	250k–1m	All	less100k	100–250k	250k–1m
interaction_medianincome	1.726***	1.738***	0.797***	0.736***	2.028***	1.821***	2.415***	2.517***
	(0.000)	(0.000)	(0.007)	(0.007)	(0.000)	(0.000)	(0.001)	(0.003)
Bank×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	260113	260113	260113	260113	260113	260113	260113	260113
R^2	0.588	0.593	0.385	0.393	0.589	0.517	0.389	0.410

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Heterogeneity by county poverty rate

	Log num of loans				Log loan amount			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	less100k	100–250k	250k–1m	All	less100k	100–250k	250k–1m
interaction_poverty	-0.0464***	-0.0462***	-0.0175**	-0.0164**	-0.0574***	-0.0484***	-0.0594***	-0.0653***
	(0.000)	(0.000)	(0.013)	(0.013)	(0.000)	(0.000)	(0.004)	(0.006)
Bank×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	260113	260113	260113	260113	260113	260113	260113	260113
R^2	0.582	0.587	0.381	0.389	0.585	0.514	0.385	0.407

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Heterogeneity by county unemployment rate

	Log num of loans				Log loan amount			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	less100k	100–250k	250k–1m	All	less100k	100–250k	250k–1m
interaction_unemploy	-0.0716**	-0.0702**	-0.0291**	-0.0288**	-0.103***	-0.0807**	-0.108**	-0.122**
	(0.015)	(0.018)	(0.037)	(0.046)	(0.005)	(0.016)	(0.014)	(0.031)
Bank×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	259642	259642	259642	259642	259642	259642	259642	259642
R^2	0.579	0.584	0.380	0.389	0.584	0.513	0.385	0.406

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: County-level exposure: Loan counts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total	Total	less100k	less100k	100–250k	100–250k	250k–1m	250k–1m
SLRshare×Post	0.522***	0.437***	0.619***	0.520***	0.196***	0.160***	0.0390**	0.00984
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.035)	(0.579)
Ln(GDP)		0.103***		0.110***		0.0692***		0.0116
		(0.000)		(0.000)		(0.000)		(0.244)
Ln(MedianHouseholdIncome)		0.0740		0.0551		0.132***		0.0691**
		(0.129)		(0.282)		(0.000)		(0.018)
UnemploymentRate		-0.00846***		-0.0145***		0.00552***		0.00818***
		(0.000)		(0.000)		(0.000)		(0.000)
PovertyPercent		-0.00140		-0.00190		0.000940		0.00168**
		(0.302)		(0.188)		(0.263)		(0.027)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	261290	239956	261290	239956	261290	239956	261290	239956
R^2	0.258	0.259	0.217	0.216	0.246	0.251	0.280	0.284

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: County-level exposure: Loan amounts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total	Total	less100k	less100k	100–250k	100–250k	250k–1m	250k–1m
SLRshare×Post	0.360***	0.290***	0.626***	0.520***	0.621***	0.549***	0.456***	0.369***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln(GDP)		0.150***		0.166***		0.229***		0.112**
		(0.000)		(0.000)		(0.000)		(0.045)
Ln(MedianHouseholdIncome)		0.0741		0.0708		0.253**		0.346**
		(0.325)		(0.396)		(0.031)		(0.013)
UnemploymentRate		0.0147***		-0.0165***		0.0266***		0.0437***
		(0.000)		(0.000)		(0.000)		(0.000)
PovertyPercent		-0.0000956		-0.00563**		0.000465		0.00870**
		(0.965)		(0.021)		(0.893)		(0.026)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	261290	239956	261290	239956	261290	239956	261290	239956
R^2	0.339	0.343	0.187	0.189	0.210	0.213	0.250	0.253

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Next Steps

Table 9: Timeline for Next Steps

Time	Plan	Potential risks	Feasibility / mitigation
2025.11.20	Build bank-level SLR SLR_b^{2014} from FR Y-15	Missing data, RSSD ID mismatches, measurement error in SLR_b^{2014}	Cross-validate across FR Y-15, FR Y-9C, and Call Reports via NIC/FDIC
2025.12.05	Estimate baseline and bank-level SLR specifications for loan counts and amounts	Small or insignificant effects; collinearity with fixed effects	Re-estimate with county and year FE as a check, cluster by bank and county
2025.12.25	Estimate COVID-period specification (temporary SLR relief 2020–2021)	Overlap with pandemic credit programs; limited variation	Include controls for pandemic shocks; restrict to CRA banks with consistent reporting
2026.01.05	Event study and parallel-trend tests, plot coefficients	Pre-trend violations, sparse or unstable event-time bins	Shift the base year, add some controls
2026.01.20	Placebo: fake policy years; identify assumptions	Spurious significance.	Randomize policy years and show the distribution of placebo estimates, report nulls.
2026.02.10	Draft figures and tables, finalize the RP text and appendix	Time constraints, version drift	Freeze a dated data snapshot and rebuild all outputs with one reproducible script

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