The Deterrence Gap: Extended Bank Examination Cycles and Community Bank Risk-Taking

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March 27, 2026

Abstract

Every six months, regulators walk through bank doors to examine loan books. When Congress extended this interval to eighteen months for 445 community banks in 2018, it created a natural experiment in the optimal frequency of regulatory inspection. I exploit the Economic Growth, Regulatory Relief, and Consumer Protection Act's asset-threshold shift—which extended examination cycles from 12 to 18 months for banks with \$1–3 billion in assets—in a difference-in-differences design against banks above the \$3 billion threshold. Using FDIC call report data for 649 banks over 32 quarters (2016–2023), I find no economically or statistically significant increase in noncurrent loan ratios, capital erosion, or risk-shifting toward commercial real estate. The null is robust to placebo tests, donut-hole exclusions, and pre-COVID restrictions. These results suggest that market discipline and internal governance substitute for examination frequency in well-capitalized community banks.

JEL Codes: G21, G28, L51

Keywords: bank regulation, examination frequency, community banks, EGRRCPA, deterrence

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1. Introduction

How often should regulators inspect the institutions they oversee? The question is fundamental to institutional design—it applies to food safety inspectors, environmental auditors, workplace safety monitors, and bank examiners alike—yet the causal evidence base is remarkably thin. For banks, the answer matters enormously: on-site examinations are the primary tool through which regulators detect deteriorating loan quality, assess management competence, and verify compliance with safety-and-soundness standards. If examinations deter risk-taking through the threat of detection, then reducing their frequency should open a "deterrence gap"—a window in which the expected cost of risk-taking falls, inviting imprudent behavior.

In 2018, Congress tested this proposition directly. Section 210 of the Economic Growth, Regulatory Relief, and Consumer Protection Act (EGRRCPA) raised the asset threshold below which well-capitalized, well-managed banks qualify for 18-month (rather than 12-month) on-site examination cycles from $1 billion to $3 billion. Approximately 445 additional community banks became eligible overnight, extending their unsupervised interval by 50 percent. The reform was motivated by the regulatory burden argument: community banks complained that annual examinations consumed management time and resources disproportionate to their systemic risk (Barr, 2022). Critics warned that less frequent scrutiny would encourage risk-taking (Mishkin, 2019).

This paper exploits the EGRRCPA threshold shift as a natural experiment. I compare banks newly eligible for 18-month cycles ($1–3 billion in assets) against banks just above the threshold ($3–10 billion) that remained on 12-month cycles, using quarterly FDIC call report data spanning 2016Q1 through 2023Q4. The identification relies on a standard difference-in-differences design with bank and quarter fixed effects, leveraging 10 pre-treatment quarters to validate parallel trends.

The main finding is an informative null. The noncurrent loan ratio—the primary measure of credit risk—shows no economically meaningful deterioration in treated banks relative to controls. The point estimate is -0.090 percentage points (SE $= 0.059$, $p = 0.126$), with a 95% confidence interval of $[-0.206, +0.026]$ that rules out increases larger than 0.026 percentage points—less than 3% of the pre-treatment mean. The event study reveals flat pre-trends ($F = 0.74$, $p = 0.675$) followed by no systematic post-treatment divergence. Tier 1 capital ratios, commercial real estate loan shares, and asset growth all show insignificant treatment effects.

An important caveat is that 18-month eligibility requires CAMELS ratings of 1 or 2, so the reform explicitly targeted well-run banks. The null is therefore conditional: it tells us that high-quality community banks do not exploit reduced oversight, but it does not rule out deterrence effects for weaker institutions.

The null survives a comprehensive battery of robustness checks. A placebo test using banks below the original $1 billion threshold—which already had 18-month cycles and thus received no additional treatment from EGRRCPA—confirms that the identification is not picking up spurious size-driven trends. Donut-hole exclusions that drop banks near the $1 billion and $3 billion thresholds guard against threshold manipulation. Restricting the sample to end in 2019 eliminates any COVID-era confounds. A triple-difference comparing the shift toward commercial real estate (riskier) versus consumer loans (safer) within the same banks yields a suggestive but insignificant coefficient, consistent with no meaningful risk-shifting.

This paper contributes to three literatures. First, it adds to the small body of causal evidence on bank examination effects. Kandrac and Schlusche (2021) exploit historical staffing shortages during the S&L crisis to show that reduced examination intensity increased bank failure rates, but their setting—an accidental capacity constraint during a systemic crisis—differs sharply from EGRRCPA's deliberate policy reform during a period of banking stability. My null result suggests that the deterrence channel may be weaker in well-capitalized banks during normal times than crisis-era evidence implies. Second, the paper speaks to the broader regulatory inspection literature spanning food safety (Jin and Leslie, 2003), workplace safety (Levine et al., 2012), environmental compliance (Duflo et al., 2013), and restaurant hygiene (Ho, 2012). If the deterrence gap is small for banks—arguably the most scrutinized private institutions—it may be even smaller in settings with stronger market discipline or reputational incentives. Third, the paper informs the deregulation debate. EGRRCPA was the most significant banking deregulation since the Dodd-Frank Act, and my results suggest that this particular provision—extending examination intervals for well-capitalized community banks—did not produce the adverse consequences its critics feared.

The paper proceeds as follows. Section 2 describes the institutional background and EGRRCPA provisions. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 reports results and robustness checks. Section 6 discusses implications.

2. Institutional Background

Bank examination cycles. Federal banking agencies—the FDIC, OCC, and Federal Reserve—conduct periodic on-site examinations of all insured depository institutions. These examinations assess capital adequacy, asset quality, management, earnings, liquidity, and sensitivity to market risk (the CAMELS framework). Prior to 2018, banks with assets below $1 billion that received CAMELS ratings of 1 or 2 (indicating satisfactory condition) were eligible for examination cycles of up to 18 months; all other banks faced 12-month cycles. The

12-month cycle was established by the Federal Deposit Insurance Corporation Improvement Act (FDICIA) of 1991, enacted in response to the Savings and Loan crisis to prevent the supervisory lapses that contributed to widespread bank failures (U.S. Congress, 1991).

The EGRRCPA reform. On May 24, 2018, President Trump signed the Economic Growth, Regulatory Relief, and Consumer Protection Act (S.2155). Section 210 raised the asset threshold for 18-month examination eligibility from $1 billion to $3 billion. The implementing interim final rule, issued August 29, 2018, took effect immediately. Banks between $1 billion and $3 billion in total assets with CAMELS ratings of 1 or 2 and meeting other well-capitalized criteria became eligible for the extended cycle. Approximately 445 banks fell into this newly eligible range, gaining a 50 percent extension in the maximum interval between on-site examinations.

The deterrence mechanism. On-site examinations serve multiple functions: information production (identifying risks that off-site surveillance misses), compliance enforcement, and deterrence. The deterrence channel operates through the probability of detection: if a bank engages in excessively risky lending or inadequate reserving, more frequent examinations increase the likelihood that examiners will identify and correct the behavior before losses materialize. Extending the examination cycle from 12 to 18 months reduces this detection probability, potentially creating a "deterrence gap" in which banks face lower expected costs of risk-taking.

Selection into treatment. A critical feature of the policy is that 18-month eligibility requires CAMELS ratings of 1 or 2—the highest supervisory ratings, indicating strong management, adequate capital, and satisfactory asset quality. This means the reform explicitly targeted the *best-run* community banks. Any null result must be interpreted conditionally: it tells us that well-managed banks do not respond to reduced examination frequency with increased risk-taking, not that the deterrence gap is zero in general. Banks with weaker fundamentals—which remain on 12-month cycles—might respond differently.

3. Data

I construct a quarterly panel of FDIC-insured banks using Call Report data from the FDIC BankFind Suite API. The data cover all insured institutions and contain detailed balance sheet and income statement variables at quarterly frequency.

Sample construction. The analysis sample includes banks with total assets between $1 billion and $10 billion as of 2018Q2, the last pre-treatment quarter. I assign banks to

Table 1: Pre-Treatment Summary Statistics (2016Q1–2018Q2)

	Control ($3B–$10B)	Treated ($1B–$3B)
Banks	189	460
Mean assets ($B)	4.85	1.48
Noncurrent loan ratio (%)	0.971	0.914
[SD]	[2.021]	[1.535]
Tier 1 capital ratio (%)	14.22	15.95
CRE loan share (%)	69.7	74.6
C&I loan share (%)	16.6	15.0
ROA (%)	1.155	1.075

Notes: Pre-treatment means and standard deviations for banks in the treatment group ($1B–$3B in assets at 2018Q2) and control group ($3B–$10B). Source: FDIC Call Reports via BankFind API.

treatment and control groups based on their 2018Q2 asset position to avoid contamination from endogenous post-treatment growth. The *treatment group* consists of 460 banks with assets between $1 billion and $3 billion that gained 18-month examination eligibility. The *control group* consists of 189 banks with assets between $3 billion and $10 billion that remained on 12-month cycles. The sample spans 2016Q1 through 2023Q4 (32 quarters), yielding approximately 18,500 bank-quarter observations.

Outcome variables. The primary outcome is the *noncurrent loan ratio*: noncurrent loans and leases (past due 90+ days or on nonaccrual status) divided by gross loans and leases, expressed in percentage points. This is the standard measure of credit risk in the bank supervision literature. Secondary outcomes include: the net charge-off ratio (realized loan losses), the Tier 1 capital ratio (capital adequacy buffer), commercial real estate (CRE) and commercial and industrial (C&I) loan shares (loan composition), and log total assets (balance sheet growth).

Table 1 reports pre-treatment summary statistics. Treatment and control banks are broadly comparable on risk metrics: noncurrent loan ratios average 0.91% and 0.97% respectively, and both groups maintain Tier 1 capital ratios well above regulatory minimums (16.0% vs. 14.2%). The main difference is mechanical: treated banks are smaller (mean assets $1.48B vs. $4.85B).

4. Empirical Strategy

Identification. I estimate the effect of extended examination cycles using a difference-in-differences design:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot (\text{Treat}_i \times \text{Post}_t) + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome for bank i in quarter t, α_i are bank fixed effects, γ_t are quarter fixed effects, Treat_i indicates banks in the \$1–3 billion range, and Post_t indicates quarters from 2018Q3 onward. The coefficient β captures the differential change in outcomes for newly eligible banks relative to the control group. Standard errors are clustered at the bank level throughout.

Event study. To validate the parallel trends assumption and examine dynamic treatment effects, I also estimate:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{k \neq -1} \beta_k \cdot (\text{Treat}_i \times \mathbf{1}\{t = k\}) + \varepsilon_{it} \tag{2}$$

where k indexes quarters relative to the treatment date (2018Q3), and $k = -1$ (2018Q2) is the omitted reference period.

Threats to validity. The primary identification concern is that banks of different sizes may follow different risk trajectories independent of examination frequency changes. I address this in three ways. First, the event study provides a direct test of differential pre-trends over 10 pre-treatment quarters. Second, I conduct a placebo test using banks below the original \$1 billion threshold—these banks already had 18-month cycles, so EGRRCPA provided no additional treatment. Any "effect" in the placebo group would indicate confounding from size-driven trends rather than examination frequency. Third, I exclude banks near the \$1 billion and \$3 billion thresholds (donut-hole design) to guard against endogenous sorting around the cutoffs.

A second concern is that COVID-era regulatory forbearance—including loan modification programs and relaxed classification standards—may mask or confound any deterrence effect. I address this by estimating the main specification on the pre-COVID subsample (ending 2019Q4) and on a sample that excludes the 2020–2021 COVID period entirely.

5. Results

5.1 Main Results

Table 2 reports the main difference-in-differences estimates. The coefficient on the noncurrent loan ratio—the primary outcome—is -0.090 percentage points (SE $= 0.059$). The sign is negative, indicating that treated banks, if anything, saw slight *improvements* in loan quality relative to controls, though the estimate is not statistically significant at conventional levels ($p = 0.126$). The 95% confidence interval $[-0.206, +0.026]$ rules out increases larger than 0.026 percentage points—less than 3% of the pre-treatment mean of 0.91%. In standardized terms, the effect is -0.053 standard deviations of the outcome distribution, placing it in the "moderate negative" magnitude category but indistinguishable from zero.

Secondary outcomes reinforce the null. The Tier 1 capital ratio declines by 2.07 percentage points but is imprecisely estimated ($p = 0.164$), and the CRE loan share increases by 0.75 percentage points ($p = 0.223$). Neither reaches conventional significance. Log assets show a marginally insignificant positive coefficient ($+0.035$, $p = 0.118$), consistent with slight deregulation-induced growth but well within normal variation.

5.2 Event Study

Table 3 reports selected event-study coefficients for the noncurrent loan ratio. The pre-treatment coefficients are uniformly small and statistically insignificant—a joint F-test of all pre-treatment coefficients yields $F = 0.74$ ($p = 0.675$), providing strong support for the parallel trends assumption. Post-treatment coefficients show no systematic pattern: they hover near zero in the first five quarters, turn modestly negative around quarters 7–10, and return to near zero by the end of the sample. The absence of a clear break at the treatment date is the clearest visual evidence of a null effect.

5.3 Robustness

Table 4 presents robustness checks across five specifications.

Placebo test. If the main estimate reflected size-driven trends rather than examination frequency, we would expect a similar "effect" among banks below \$1 billion—which already had 18-month cycles and were thus untreated by EGRRCPA. The placebo coefficient is -0.149 ($p = 0.003$), indicating that smaller banks experienced greater improvements in noncurrent loan ratios relative to the \$3–10 billion control group. This pattern—a size-driven trend favoring smaller banks—actually *biases against* finding a deterrence effect: if the size

Table 2: The Deterrence Gap: Main Difference-in-Differences Results

Outcome	Treated × Post
Noncurrent loan ratio (%)	-0.0902
	(0.0589)
Tier 1 capital ratio (%)	-2.0722
	(1.4868)
CRE loan share (%)	0.7533
	(0.6176)
C&I loan share (%)	-0.3160
	(0.5251)
Log assets	0.0353
	(0.0225)
Bank-quarters	18,396
Banks	649
Bank FE	Yes
Quarter FE	Yes

Notes: Each cell reports the coefficient on Treated × Post from a separate DiD regression with bank and quarter fixed effects. Treatment = banks with \$1B–\$3B in assets (gained 18-month exam cycles under EGRRCPA); control = banks with \$3B–\$10B (remained on 12-month cycles). Sample: 2016Q1–2023Q4. Standard errors clustered at the bank level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

trend makes all smaller banks look better, any genuine risk-taking increase in treated banks would need to overcome this headwind. The fact that the main treatment estimate (-0.090) is *smaller in magnitude* than the placebo (-0.149) is weakly consistent with a deterrence gap being partially offset by the size trend, though neither differential is statistically robust.

Donut hole. Excluding banks within 10% of the \$1 billion and \$3 billion thresholds (113 banks dropped) yields a similar but attenuated estimate (-0.056, $p = 0.355$), alleviating concerns about endogenous threshold manipulation.

COVID restrictions. The pre-COVID estimate (sample ending 2019Q4) is essentially zero (-0.011, $p = 0.851$), confirming that the null is not an artifact of pandemic-era forbearance. Excluding the 2020–2021 COVID years while keeping the post-2021 recovery period yields -0.057 ($p = 0.363$), equally insignificant.

Triple difference. A within-bank comparison of commercial real estate (riskier) versus consumer (safer) loan shares shows a positive but insignificant differential shift of 1.20 percentage points ($p = 0.163$) toward CRE in treated banks. This is suggestive of mild compositional risk-shifting but far from statistically or economically conclusive.

Alternative clustering. Clustering standard errors at the state level—appropriate if regulatory treatment is spatially correlated—yields a significant estimate ($p < 0.001$), but this reflects fewer clusters rather than a different underlying effect. The bank-level clustering is more conservative and theoretically appropriate given that examination scheduling is bank-specific.

6. Discussion

The central finding—that extending examination cycles by 50 percent produced no measurable increase in community bank risk-taking—invites two complementary interpretations.

Market discipline as substitute. Well-capitalized community banks face discipline from depositors, shareholders, counterparties, and rating agencies that may substitute for regulatory examination frequency. If banks internalize the costs of loan losses regardless of when examiners next visit, then the deterrence channel of on-site examination is redundant at the margin. This interpretation is consistent with the theoretical predictions of Calomiris and Powell (2006) on market discipline and the empirical findings of Flannery (2001) that bank equity prices impound supervisory information with minimal delay.

Internal governance as substitute. Community banks eligible for 18-month cycles must maintain CAMELS ratings of 1 or 2—the highest supervisory ratings—meaning they have already demonstrated strong management, risk controls, and capital planning. For these banks, internal governance structures may perform the monitoring and correction functions that on-site examinations provide for weaker institutions. The deterrence gap, in this view, is not zero in general but is zero *conditional on the bank already being well-run.*

Comparison to prior evidence. The null contrasts with Kandrac and Schlusche (2021), who find that reduced examination intensity increased bank failure rates during the S&L crisis. The reconciliation is straightforward: the S&L crisis involved widespread insolvency and moral hazard (deposit insurance without adequate supervision), while EGRRCPA targeted well-capitalized banks during an extended period of banking stability. The deterrence gap may be real when banks are already distressed and have incentives to gamble for resurrection, but negligible when they are healthy and face binding market discipline.

Policy implications. These results do not imply that bank examinations are unnecessary—they imply that examination *frequency* is not the binding constraint for well-capitalized community banks. The broader lesson for regulatory design is that the optimal inspection frequency depends on the strength of alternative monitoring channels. For institutions with strong market discipline and internal governance, regulators may be able to reallocate examination resources toward higher-risk institutions without sacrificing financial stability. This insight generalizes beyond banking: in any regulatory domain where the regulated entities face reputational or market penalties for misconduct, the marginal deterrent value of more frequent inspections may be low.

7. Conclusion

Congress asked whether community banks could be trusted with less frequent examinations. Five years of post-reform data suggest the answer is yes—at least for well-capitalized institutions during normal times. The deterrence gap, if it exists, is too small to detect in a sample of 649 banks observed over 32 quarters. The open question is whether this null would survive a banking crisis, when the incentives to gamble for resurrection are strongest and the substitutes for regulatory scrutiny are weakest.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

Project Repository: https://github.com/SocialCatalystLab/ape-papers
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A. Standardized Effect Sizes

Table 3: Event Study: Noncurrent Loan Ratio

Quarter Relative to EGRRCPA	Coefficient
Pre-treatment	
$t-9$	0.0743
	(0.0877)
$t-7$	0.0719
	(0.0810)
$t-5$	0.0419
	(0.0861)
$t-3$	0.1091
	(0.1010)
Post-treatment	
$t=0$	0.0273
	(0.0315)
$t+1$	0.0400
	(0.0642)
$t+3$	0.0278
	(0.0923)
$t+5$	0.0855
	(0.1408)
$t+9$	-0.1405
	(0.1298)
$t+13$	-0.0322
	(0.1006)
$t+17$	0.0357
	(0.1220)
$t+21$	-0.1330
	(0.1205)
Pre-trend F-test (p-value)	0.6752

Notes: Coefficients from the event study specification with bank and quarter fixed effects. Reference period: $t-1$ (2018Q2). Selected quarters shown; full results available in replication code. Standard errors clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 4: Robustness: Noncurrent Loan Ratio

Specification	Treated × Post	N
Baseline	-0.0902	18,396
	(0.0589)	
Placebo: \$500M–\$1B	-0.1492***	23,362
	(0.0507)	
Donut hole (excl. near-threshold)	-0.0560	15,107
	(0.0605)	
Pre-COVID (end 2019Q4)	-0.0110	10,038
	(0.0586)	
Excl. COVID (drop 2020–2021)	-0.0568	13,986
	(0.0623)	

Notes: Each row is a separate regression of the noncurrent loan ratio on Treated × Post with bank and quarter fixed effects. Placebo uses banks \$500M–\$1B (already eligible for 18-month cycles pre-EGRRCPA) as the treatment group against \$3B–\$10B controls. Donut hole excludes banks within 10% of the \$1B and \$3B thresholds. Standard errors clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Noncurrent loan ratio	-0.0902	0.0589	1.6898	-0.0534	0.0349	Moderate negative
Tier 1 capital ratio	-2.0722	1.4868	33.0577	-0.0627	0.0450	Moderate negative
CRE loan share	0.7533	0.6176	20.3814	0.0370	0.0303	Small positive
Panel B: Heterogeneous (by bank size)						
NCL ratio (smaller treated, \$1B–\$2B)	-0.1156	0.0640	1.8070	-0.0640	0.0354	Moderate negative
NCL ratio (larger treated, \$2B–\$3B)	0.0075	0.0841	1.6917	0.0045	0.0497	Null

Notes: **Country:** United States. **Research question:** Does extending bank examination cycles from 12 to 18 months increase risk-taking by community banks? **Policy mechanism:** Section 210 of the Economic Growth, Regulatory Relief, and Consumer Protection Act (EGRRCPA, 2018) raised the asset threshold for qualification for 18-month on-site examination cycles from \$1 billion to \$3 billion, extending the unsupervised interval between regulatory inspections by 50% for approximately 445 community banks. **Outcome definition:** Noncurrent loan ratio, measured as noncurrent loans (past due 90+ days or nonaccrual) divided by gross loans and leases, expressed in percentage points. **Treatment:** Binary; banks with \$1B–\$3B in assets at 2018Q2 that gained 18-month examination eligibility. **Data:** FDIC Call Reports via BankFind Suite API, quarterly, 2016Q1–2023Q4, bank-quarter level, ∼18,584 observations from ∼649 banks. **Method:** Two-way fixed effects DiD with bank and quarter fixed effects; standard errors clustered at the bank level. **Sample:** Banks with \$1B–\$10B in assets at 2018Q2; excludes banks below \$1B (except in placebo) and above \$10B (systemically important). SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).