DOES LOAN-TO-DEPOSIT RATIO MEASUREMENT CHANGE BANK-LEVEL INFERENCE? EVIDENCE FROM NIGERIA'S 2019 LENDING-FLOOR TIGHTENING

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Abstract
This study examines the sensitivity of bank-level annual loan-to-deposit ratio (LDR) associations to public exposure definitions around Nigeria's 2019 lending-floor changes. End-Q3 2019 positions classify eight of eleven banks below, and three at or above, the 65% threshold, although those positions were already affected by the July 60% directive. A bank- and year-fixed-effects comparison estimates a 10.46 percentage-point differential (bank-clustered SE = 10.52; 95% CI = -12.98 to 33.90); the zero-bounded threshold-gap estimate is 0.825 per percentage-point gap (95% CI = -0.215 to 1.864). A five-bank subset with issuer-reported or issuer-derived Q3 evidence gives 2.34 percentage points (95% CI = −18.58 to 23.26), a less informative and still imprecise association. The end-Q3 and outcome-defined 2018 groupings yield 10.46 and 28.90 on the same eleven-bank sample; the latter is susceptible to endogenous stratification. The results document sensitivity to exposure construction, not a precise heterogeneous association or a causal regulatory effect.

Keywords: Bank heterogeneity, construct validity, loan-to-deposit ratio, panel data, regulatory exposure.
JEL Classification: G21, G28, E58, C23

1.0 Introduction

The central difficulty in evaluating threshold regulation at bank level is not only estimation but construct validity. Nigeria's 2019 lending-floor regime created a common policy benchmark, yet a bank's reported position can differ across issuer releases, market summaries and the Central Bank of Nigeria's weighted supervisory calculation. Treating these measures as interchangeable can move banks across the regulatory threshold and materially change estimated heterogeneous differentials. This study therefore treats measurement disagreement as part of the empirical problem rather than as a routine robustness detail.

The Central Bank of Nigeria introduced a minimum LDR of 60 per cent in July 2019 and, on 30 September 2019, increased the minimum to 65 per cent for compliance by 31 December 2019. The regulatory calculation assigned a 150 per cent weight to SME, retail, mortgage and consumer lending, while non-compliance attracted an additional cash-reserve requirement equal to 50 per cent of the lending shortfall (Central Bank of Nigeria [CBN], 2019). A conventional financial-statement LDR should therefore not automatically be described as the supervisory compliance ratio.

Existing Nigerian research has mainly examined LDR as a determinant of bank liquidity, performance, stability or credit outcomes rather than as a measurement construct (Adenuga et al., 2021; Danmulki et al., 2022; Nwani&Adukwu, 2026; Ibrahim et al., 2026). Those questions remain important, but they leave a separate issue unresolved: when researchers classify individual banks as more or less exposed to a lending floor, do the conclusions survive a different credible exposure source? The issue is especially important when treatment status is constructed from the same outcome later used in estimation because regression to the mean and endogenous stratification can exaggerate apparent heterogeneity.

The primary design avoids defining exposure from the 2018 value of the annual outcome. It uses an end-Q3 2019 public bank position reconciled across direct issuer releases and contemporaneous

results-based sources. The measure is independent of the annual outcome dataset, but it is not an untreated baseline for the full 2019 policy regime, the July 60 per cent directive was already in force and the 65 per cent tightening was announced at the end of Q3. The estimand is therefore a post-tightening annual LDR differential conditional on the bank's end-Q3 position, not a causal effect of the July intervention.

The study contributes a transparent reconciliation of publicly reported bank-ratio constructs; a replicable set of contrasts and sensitivity tests under an unusually small comparison group; and a clear distinction between policy-affected end-Q3 positions and genuinely untreated exposures. The comparisons document how numerical estimates vary across definitions. They do not establish that these estimates differ statistically in a generalizable population, nor that the 2019 regime caused a bank-level change.

2.0 Literature Review

Loan-to-deposit ratio relates a bank's lending book to its deposit funding base and, at the institution level, reflects lending strategy, deposit mobilisation, liquidity management and risk appetite. Banks facing the same policy floor may therefore face different adjustment pressure when their starting positions differ. This heterogeneity makes the distance between each institution's measured position and the regulatory benchmark empirically relevant.

The analytical framework is one of heterogeneous regulatory pressure under measurement uncertainty. A conventional accounting LDR, a gross-loans-to-funding ratio reported by management and the CBN's weighted supervisory LDR can differ because of numerator definitions, funding scope, regulatory weights and timing. Threshold classification converts those continuous differences into a discrete group assignment, so even modest definitional variation can alter treatment composition and the resulting estimate.

Operational Definitions and Comparability Rule

Accounting LDR is defined for this paper as gross customer loans (or the issuer-specified loan figure) divided by customer deposits, multiplied by 100; the precise gross/net and group/bank scope is retained from each source. A management gross-loans-to-funding ratio can use a broader denominator than customer deposits and cannot be treated as an equivalent accounting LDR without component-level reconciliation. Fidelity labels its Q3 2019 68.4% figure as gross loans to funding (Fidelity Bank Plc, 2019).

The CBN supervisory ratio, by contrast, applies the regulator’s eligible-loan categories and a 150% weighting to qualifying SME, retail, mortgage and consumer lending for compliance with the minimum. Public financial-statement figures lack sufficient numerator, denominator, and classification details to reconstruct that measure (CBN, 2019). End-Q3 public ratios are therefore proxy classifications only, not verified supervisory compliance statuses.

For the binary threshold screen, each published ratio is compared with 65%, but the sources do not establish a fully common numerator, denominator or supervisory measurement basis. Direct issuer LDRs, issuer-derived loan/deposit ratios and third-party summaries are distinguished by provenance. The six secondary-source observations all fall below 65%, so source quality is confounded with group membership. A five-bank issuer-reported or issuer-derived subsample checks this dependency; an additional check removes Fidelity's differently denominated funding measure. Neither exercise recovers supervisory treatment status or resolves selection into the higher-quality group.

Official CBN material establishes the 60-to-65 per cent chronology and the weighted regulatory construction (CBN, 2019). Adenuga et al. (2021) examine the relationship between LDR and bank liquidity in Nigeria using a factor-augmented VAR framework. The present study addresses a different question: whether bank-level conclusions remain stable across alternative exposure measurements. This focus also connects to broader econometric concerns. Roth (2022) cautions that conventional pre-trend tests can have low power, while MacKinnon and Webb (2018) document the fragility of cluster-robust inference when treated clusters are few. These concerns motivate emphasis on confidence intervals, source sensitivity and descriptive transparency rather than binary significance claims.

Recent studies in the Gusau Journal of Accounting and Finance further show how LDR is used in applied Nigerian banking research. Danmulki et al. (2022) use LDR as one component of liquidity management in explaining listed-bank financial performance; Nwani and Adukwu (2026) include LDR among monetary-policy variables associated with financing of small-scale enterprises; and Ibrahim et al. (2026) relate LDR to bank stability through liquidity outcomes. These studies address different dependent variables, but they share an empirical convention in which an observed LDR enters analysis as a stable construct. The present study complements that literature by asking whether the bank-level conclusion changes when the exposure measure itself is reconstructed from sources that differ in definition, timing and regulatory weighting.

A bank-level fixed-effects comparison can describe whether institutions with different measured exposure positions subsequently move differently, but fixed effects alone do not establish causality. Time-varying bank shocks, mergers, asset-quality events, denominator changes and exposure-source choice can all affect the observed path. The estimates are therefore interpreted as conditional panel evidence.

H01 states that banks below 65 per cent according to the independently sourced end-Q3 2019 measure do not exhibit a statistically detectable differential annual LDR path during 2020-2023 relative to banks already at or above 65 per cent. A continuous specification tests whether the size of the end-Q3 gap to 65 per cent is associated with the subsequent differential path. Secondary specifications assess how the conclusion changes when exposure is defined differently.

H_{01}: End-Q3 2019 exposure is not associated with a statistically detectable differential post-2019 annual LDR path among the sampled banks.

End-Q3 Balance-Sheet Position and Expected Post-Tightening Adjustment

The end of Q3 2019 is useful but policy affected. The 65 per cent benchmark was announced on 30 September with year-end compliance, while banks had already been responding to the July 60 per cent directive with a 30 September deadline (CBN, 2019). Direct issuer material reports an Access Bank LDR of 67.4 per cent, a Fidelity Bank gross-loans-to-funding ratio of 68.4 per cent, a Sterling Bank LDR of 74.4 per cent, gross loans and deposits for Zenith that imply approximately 55.7 per cent, and an FBN gross-loans-to-deposits ratio of 52.8 per cent (Access Bank Plc, 2019; Fidelity Bank Plc, 2019; FBN Holdings Plc, 2019; Sterling Bank Plc, 2019; Zenith Bank Plc, 2019). Values for banks without a directly extracted issuer ratio are retained as lower-tier evidence from contemporaneous results reporting (Olagunju, 2019).

The primary comparison contains eight banks with public end-Q3 ratios below 65% and three at or above 65%. The continuous measure records the non-negative distance below 65%; banks already above the threshold receive a zero gap, rather than negative treatment intensity. The

construction intentionally captures one-sided shortfall pressure but discards information about how far above the threshold other banks lie. The small and unbalanced control group limits power, precision and reliable t approximations.

Measurement independence from the annual outcome dataset does not remove all identification concerns. End-Q3 2019 positions may already embody responses to the July directive, and banks below 65 per cent may differ systematically in risk appetite, asset quality, franchise structure or funding strategy. Bank fixed effects remove time-invariant differences but not bank-specific time-varying shocks. The estimates are therefore interpreted as post-tightening differentials conditional on end-Q3 position rather than causal treatment effects.

Alternative Exposure Definitions and the Identification Problem

An alternative classification based on each bank's 2018 annual LDR is retained only as a secondary diagnostic because the grouping variable is drawn from the same outcome series used in estimation. That design is inherently vulnerable to regression to the mean and endogenous stratification. The independently sourced end-Q3 measure therefore serves as the primary exposure definition.

Source provenance is equally important. Access Bank's direct 9M material reports 67.4 per cent rather than the 69.4 per cent appearing in the contemporaneous newspaper aggregation, although the threshold class is unchanged (Access Bank Plc, 2019; Olagunju, 2019). Fidelity's 68.4 per cent figure is explicitly labelled by the bank as a gross-loans-to-funding ratio, not simply assumed to be the same construct as a conventional accounting LDR (Fidelity Bank Plc, 2019). These discrepancies illustrate why source labels and construct definitions should remain visible in threshold-based research.

The resulting research question is deliberately narrow: how stable is the evidence of heterogeneous post-2019 bank adjustment when exposure is measured independently and when alternative credible LDR sources are used? This measurement-sensitivity framing is distinct from an aggregate policy-effect study and does not require the observed differential to be interpreted as a clean causal effect.

3.0 Methodology

The study adopts an ex-post facto archival panel design. The annual outcome panel contains thirteen listed Nigerian deposit money banks observed from 2015 to 2023, with 2019 treated as a transition year. The primary estimation sample is restricted to the eleven banks for which a comparable contemporaneous end-Q3 2019 exposure measure could be reconciled with the annual panel, yielding 88 bank-year observations across 2015-2018 and 2020-2023. Annual LDR and bank-size values use the published bank-year appendix in Hassan (2026) as their immediate source. Unity Bank's 2016-2018 LDR entries were checked against the bank's annual reports and corrected where necessary (Unity Bank Plc, 2018, 2019).

The annual outcome series is not presented as a complete primary re-extraction from every bank report. To make the validation boundary explicit, each annual observation in the data package is tagged as either published-panel-derived or directly verified/corrected. Only Unity Bank's 2016-2018 LDR values receive direct annual-report verification in this study; the remaining annual observations retain Hassan (2026) as their immediate source. The independently assembled end-Q3 exposure series is therefore a separate evidence layer from the annual outcome panel. This separation prevents the Q3 source audit from being interpreted as full primary validation of the annual series.

Verification Scope and Selection Uncertainty

The annual outcome panel contains 117 potential bank-years for thirteen banks during 2015–2023; the primary balanced subset has eleven banks observed in four pre- and four post-period years. Union Bank bank-size information is missing for 2017 in the source panel; bank size is descriptive, not a control entered into the reported fixed-effects equations. No observation is imputed. The presence of a reported LDR should not be equated with independent checking against an issuer statement.

Exposure is constructed from end-Q3 2019 public bank figures, separately from Hassan's annual outcome series but after the July policy phase began. Below65_Q3 equals one for a public end-Q3 measure strictly less than 65% and zero otherwise; Gap_to_65 = max (0, 65 − end-Q3 percentage). The source hierarchy distinguishes (i) directly reported issuer LDR; (ii) issuer-derived gross-loans/deposits ratios; (iii) Fidelity's issuer-reported gross-loans-to-funding ratio; and (iv) ratios reported by a contemporaneous secondary source. These are not assumed to measure the CBN's weighted supervisory compliance ratio. FCMB and Jaiz lack exposure measures that meet this limited source rule and are excluded only from primary models.

The preferred fixed-effects model is:

$$LDR_{it} = \alpha_i + \gamma_t + \beta(Below65_Q3_i \times Post_t) + \varepsilon_{it} \quad (1)$$

where bank fixed effects (α_i) absorb time-invariant bank characteristics, year fixed effects (γ_t) absorb common shocks, and Post equals one for 2020-2023. The continuous specification replaces the binary interaction with the end-Q3 distance to 65 per cent:

$$LDR_{it} = \alpha_i + \gamma_t + \beta(Q3_Gap_to_65_i \times Post_t) + \varepsilon_{it} \quad (2)$$

Standard errors are clustered by bank. With eleven clusters and only three comparison banks, finite-cluster t p-values are treated as approximate descriptive aids rather than dispositive tests, consistent with the caution in MacKinnon and Webb (2018). Headline interpretation therefore emphasises 95 per cent confidence intervals and stability across exposure definitions. For the primary binary specification, an additional exhaustive same-size bank-label permutation diagnostic re-estimates all 165 possible assignments of eight below-threshold labels across the eleven banks. Because the observed exposure labels were not randomly assigned, the resulting tail proportion is reported only as a finite-sample sensitivity diagnostic rather than as causal randomization inference.

Potential Confounding and Bounded-Gap Estimand

The fixed-effects coefficient compares the average pre/post changes of the two bank groups after subtracting common year effects. It does not separately identify lending-floor enforcement from evolving bank-specific loan demand, funding shocks, asset impairment, capital actions or sectoral mix. No bank-specific contemporaneous controls, instruments, untreated policy benchmark or defensible random exposure assignment is available here; causal language is therefore excluded.

The zero-bounded shortfall s_i = max (0, 65 − m_i) sets s_i = 0 for banks at or above 65%, not a negative value representing headroom. Its interaction coefficient represents the slope among measured shortfalls under a single assumed linear association; the model does not estimate a response to distance above 65%. Because m_i mixes published proxy constructs, even this interpretation is conditional on the public-data classification architecture.

Event-time interactions relative to 2018 describe individual lead and post-period estimates, not validated parallel trends. The joint 2015–2017 lead test and all three individual confidence intervals are reported. The outcome-defined 2018 and ARM 2018 classifications are secondary

diagnostics, and bank-label permutations are not causal inference. Further analyses remove Fidelity, restrict Q3 measurement to five issuer-reported or issuer-derived banks, exclude Unity/outlying LDR observations, and vary post-period windows. Identical-sample exposure differences use a paired, stratified bank-bootstrap interval, interpreted as exploratory. None of these specifications establishes a causal lending mechanism.

4.0 Results and Discussion

Descriptive Statistics

The primary sample has 88 bank-years for eleven banks. FCMB and Jaiz are omitted solely because comparable end-Q3 exposure measurements were not recovered within the defined evidence hierarchy, not because either bank was known to have a particular regulatory compliance status. Included banks' average 2015-2018 annual LDR is 75.76% versus 78.19% for the excluded banks, and average log bank size is 6.17 versus 5.49. Similar LDR averages cannot eliminate selection bias: exclusion may covary with disclosure transparency, bank business model, financing mix, governance, regulatory treatment, or unobserved adjustment pathways. The estimated differential therefore generalizes only to the selected eleven banks, not automatically to all Nigerian deposit money banks. Table 1 reports the data distribution; source-level limitations are also given in Appendix A.

Table 1:
Descriptive Statistics

Variable	Mean	Median	Std. Dev.	Minimum	Maximum	N
Annual LDR (%)	69.677	69.345	19.555	3.550	134.760	88
Bank size (log assets)	6.360	6.394	0.446	5.195	7.315	87
Q3-2019 measure (%)	59.676	60.200	9.835	34.950	74.400	88
Gap to 65 (pp)	6.706	4.800	8.379	0.000	30.050	88
Below 65 at Q3	0.727	1.000	0.448	0.000	1.000	88

Source: Authors' computation from the Hassan (2026) annual panel with documented Unity Bank corrections and the separately reconciled Q3 exposure data.

Correlation Analysis

The correlation matrix in Table 2 is descriptive rather than causal. Annual LDR is negatively correlated with the post indicator and with the binary exposure interaction in the unadjusted panel. The continuous end-Q3 gap has only a small unconditional correlation with annual LDR. These correlations do not adjust for bank heterogeneity or common time shocks.

Table 2:
Correlation Statistics

Variable	LDR	Bank size	Below65_Q3	Post	Below65×Post	Q3 gap
LDR	1.000	-0.155	-0.197	-0.313	-0.272	-0.058
Bank size	-0.155	1.000	-0.085	0.415	0.254	-0.314
Below65_Q3	-0.197	-0.085	1.000	0.000	0.463	0.493
Post	-0.313	0.415	0.000	1.000	0.756	0.000
Below65×Post	-0.272	0.254	0.463	0.756	1.000	0.228

Variable	LDR	Bank size	Below65_Q3	Post	Below65×Post	Q3 gap
Q3 gap	-0.058	-0.314	0.493	0.000	0.228	1.000

Source: Authors' computation from the primary 88-observation estimation sample.

Regression Analysis and Hypothesis Testing

The bank- and year-fixed-effects model estimates a 10.46 percentage-point differential in annual LDR for banks classified below 65% at end-Q3 (clustered SE = 10.52, 95% CI = −12.98 to 33.90). The zero-truncated continuous gap specification estimates 0.825 percentage points for each extra percentage point of shortfall (SE = 0.467; 95% CI = −0.215 to 1.864). Table 3 reports both. The intervals include zero and sizeable opposite-signed associations, consequently H01 is not rejected. The positive coefficient alone supplies no reliable evidence of an adjusted group difference. The estimates are conditional contrasts, not effects of regulatory tightening.

Table 3:
Fixed-Effects Regression Results

Statistic	Q3 binary exposure	Q3 continuous gap	Secondary 2018 grouping
Exposure coefficient	10.459	0.825	27.951
Clustered standard error	10.521	0.467	9.854
Finite-cluster p-value	0.344	0.108	0.015
95% CI lower	-12.984	-0.215	6.480
95% CI upper	33.901	1.864	49.422
Banks / clusters	11	11	13
Below-65 / treated banks	8	Continuous	4
Observations	88	88	104
Bank & year fixed effects	Yes	Yes	Yes

Source: Authors' computation from the published annual panel, documented Unity corrections and reconciled Q3 exposure data.

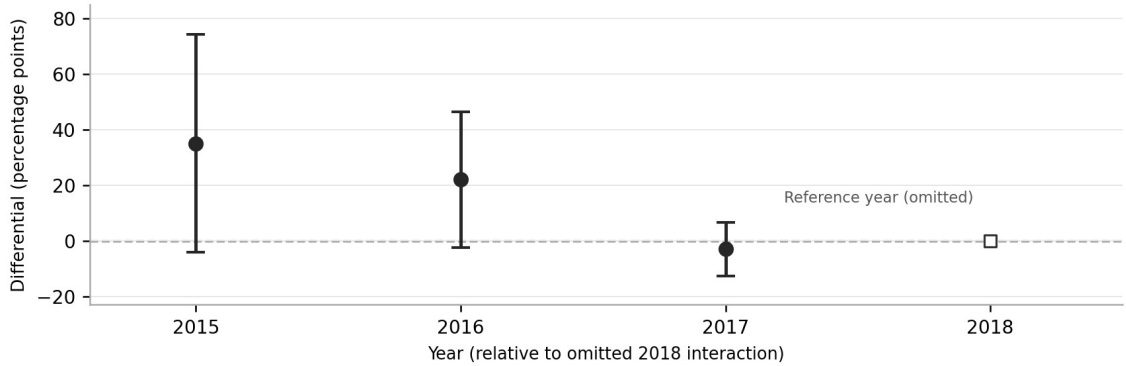
Note: The binary model compares eight banks below 65 per cent at end-Q3 2019 with three banks at or above the threshold. Standard errors are clustered by bank. End-Q3 exposure is policy affected and is not interpreted as an untreated pre-policy measure.

The primary eleven-bank estimate is uncertain, with only three banks in the comparison group. Clustered t-based intervals are approximate with so few clusters. Exhaustively enumerating 165 eight-of-eleven exposure assignments produces a two-sided absolute-coefficient tail frequency of 0.479. Because banks are not exchangeable and labels were not randomly assigned, this is a descriptive sensitivity statistic, not a causal p-value. The exact frequency is the number of equally extreme assignments divided by 165, without an additional add-one correction.

Robustness and Identification Diagnostics

Individual pre-period interactions with 2018 omitted are 35.09 pp in 2015 (95% CI −4.22 to 74.39), 22.00 pp in 2016 (−2.46 to 46.47) and −3.04 pp in 2017 (−12.76 to 6.69). Their uneven magnitudes and wide intervals do not support an assertion of parallel trends. The joint lead statistic is F = 1.55 (p = .263), but non-rejection is uninformative about the validity of the identifying assumption in a sample this small (Roth, 2022). Figure 1 and Table 5 show the individual estimates and intervals.

Figure 1: ***Event-Study Diagnostic Using Independent End-Q3 2019 Exposure***



Source: Authors' computation. The 2018 interaction is the omitted reference period.

Table 4 summarises the diagnostics. The primary permutation examines all 165 allocations of eight below-threshold labels across eleven banks; 79 allocations are at least as extreme in absolute coefficient, giving 79/165 = 0.479. Exposure-label exchangeability is not justified, so this is not randomization inference. The 2018 outcome-defined grouping produces a 27.95 pp coefficient in thirteen banks and a low exhaustive-label tail share, but categorising banks using their own earlier outcome creates regression-to-the-mean and endogenous-stratification risk. ARM-based estimates are imprecise. No model is preferred because its p-value happens to fall below a conventional threshold.

Table 4:
Robustness and Identification Diagnostics

Test	Estimate / statistic	SE	p-value
Q3-exposure joint pretrend	F = 1.547	-	0.263
Primary Q3 label-permutation diagnostic	10.459	-	0.479
Secondary 2018 grouping	27.951	9.854	0.015
Secondary 2018 label-permutation diagnostic	27.951	-	0.006
ARM-2018 classification	12.311	13.549	0.394

Source: Authors' computation; ARM classification values are based on ARM Securities Limited (2019).

Table 5:
Individual Event-Study Pre-Period Differentials (Reference Year: 2018)

Year	Estimate (pp)	Clustered SE	95% CI (pp)
2015	35.09	17.64	−4.22 to 74.39
2016	22.00	10.98	−2.46 to 46.47
2017	−3.04	4.36	−12.76 to 6.69

Source: Authors' calculation from the eleven-bank panel; bank-clustered t intervals (10 df). Non-significant leads do not establish parallel trends.

Table 6:
Exposure, Extreme-Value and Post-Period Sensitivities

Specification	Estimate (pp)	95% CI (pp)	Banks	Bank-years
Exclude Fidelity	15.98	-6.35 to 38.32	10	80
Issuer-evidence subset (5 banks)	2.34	-18.58 to 23.26	5	40
Exclude Unity Bank	6.97	-17.38 to 31.32	10	80
Exclude Unity LDR outside 20–120%	6.66	-16.41 to 29.73	11	85
Winsorize LDR 5th/95th	9.51	-13.62 to 32.64	11	88
Post: 2020–2021	5.78	-14.44 to 25.99	11	66
Post: 2022–2023	15.14	-13.22 to 43.50	11	66
Post: 2021–2023	10.99	-14.90 to 36.88	11	77
Post: 2020–2022	9.62	-12.77 to 32.01	11	77

Source: Computation using the bank panel; bank-clustered intervals use the G−1 t reference. The issuer-based restriction leaves five banks (two below 65%); all confidence intervals cross zero. Winsorization cutoffs are 41.952% and 96.583%.

Table 7:
Direct Comparison on Identical Eleven-Bank Sample

Exposure classification	Estimate (pp)	95% CI / exploratory interval (pp)
End-Q3 2019 below 65%	10.46	−12.98 to 33.90
2018 annual LDR below 65%	28.90	6.01 to 51.79
Difference: 2018 minus Q3	18.44	1.19 to 40.24 (exploratory bootstrap)

Note: Exactly the same 11 banks (88 observations) enter both models. Difference interval uses 20,000 stratified bank resamples, with 19,924 valid draws; it is not a formally calibrated causal test with three comparison banks. 2018 labels use the annual outcome itself, introducing endogenous stratification and possible mean reversion.

Discussion of Heterogeneous Bank Adjustment

The evidence supports a narrow construct-validity conclusion. The preferred eleven-bank end-Q3 comparison is imprecise, and a restricted analysis using five issuer-reported or issuer-derived observations yields a coefficient of 2.34 pp (95% CI −18.58 to 23.26). The restriction changes both sample composition and the estimate: it cannot isolate measurement error from sample selection or demonstrate statistically different effects. The same-sample alternative grouping comparison in Table 7 is exploratory, with the 2018 grouping mechanically linked to the outcome. Differences in statistical significance alone do not prove that estimates differ.

This finding should not be read as evidence that the policy had no effect. The confidence interval around the primary binary estimate includes economically meaningful negative and positive differentials, and the sample includes only three banks already at or above 65 per cent. The defensible conclusion is limited precision combined with substantial measurement sensitivity.

With the same eleven banks and annual observations, the end-Q3 grouping gives 10.46 pp and the 2018 outcome-defined grouping 28.90 pp; their direct difference is 18.44 pp. A stratified bank-resampling interval of approximately 1.19 to 40.24 pp is descriptive and unstable with only three comparison banks. It is not definitive evidence of construct effects, because the alternative 2018

label is endogenous to the annual outcome and the exposure constructs do not define randomized interventions. The thirteen-bank 2018 result of 27.95 pp is not directly comparable without controlling for the different sample. This distinction is central to the study's contribution.

Magnitude, Mechanisms and Regulatory Interpretation

The preferred binary estimate of 10.46 pp is uncertain by more than 23 pp on either side; effects of both signs remain plausible. A ten-percentage-point increase in the nonnegative distance below 65% corresponds to 8.25 pp in the continuous model, but its interval covers zero. Interpretation of that distance applies to banks below the cutoff only. For banks above 65%, further headroom is coded zero; the model cannot estimate a symmetric slope above the regulatory floor.

Unity Bank contributes the extreme annual LDR observations: 134.76% in 2015, 104.93% in 2016, 3.55% in 2017 and 18.05% in 2018. Its 2016–2018 published ratios were checked against issuer annual reports; the 2015 value remains secondary-panel derived. Ratios may move sharply when loan numerators, deposit denominators, balance-sheet classification or reporting scope changes, but the supplied issuer component tables do not permit a verified attribution of these extremes to any one mechanism. Excluding Unity yields 6.97 pp (95% CI −17.38 to 31.32), excluding three Unity observations outside 20-120% yields 6.66 pp (−16.41 to 29.73), and 5%/95% winsorization yields 9.51 pp (−13.62 to 32.64); no interval excludes zero (Table 6).

The regulatory implication is methodological rather than directional: bank-level evaluation of threshold regulation should use the regulator's own compliance metric wherever possible and should distinguish conventional accounting LDR from weighted supervisory LDR. Where supervisory microdata are unavailable, researchers should report alternative public exposure definitions rather than treating one proxy as definitive.

Sensitivity of the Bank-Level Inference

Three identification concerns must be separated. First, the end-Q3 position was observed after banks were exposed to the July 60% directive and at the time the 65% target was announced; it is not a pre-policy treatment. Second, bank fixed effects do not account for bank-specific time-varying shocks to loan demand, funding, capitalization, credit risk, merger activity or deposit pricing, while year fixed effects remove only common annual shocks. Third, the 2020–2023 window potentially absorbs pandemic-era disruption and subsequent monetary, foreign-exchange and bank-specific developments unrelated to lending-floor rules. Comparing early and later post-periods changes point estimates but leaves them imprecise (Table 6). Taken together, these limitations rule out an identified tightening effect.

Few-bank inference is a second structural limitation. The primary sample contains eleven bank clusters but only three comparison banks. MacKinnon and Webb (2018) show why conventional cluster inference can be unreliable when treated clusters are few. Accordingly, p-values are given limited weight, confidence intervals are reported prominently, and the primary binary model is supplemented with an exhaustive same-size bank-label permutation diagnostic. The diagnostic does not create random assignment; it is used only to show how unusual the observed coefficient is relative to alternative label allocations in this small bank panel.

Interpretation of Source Levels and The Permutation Diagnostic

Direct issuer LDR, management funding ratios, ratios derived from issuer loan/deposit components, and news aggregation differ in assurance and often economic meaning. An observation can be placed on one side of the public 65% benchmark without proving its

supervisory compliance class. The key risk is not just random noise: a numerator or denominator change near 65% can switch a label, and source availability may be correlated with bank behavior.

The exhaustive permutation holds the outcome trajectories and number of assigned exposure labels fixed while relabelling banks. The resulting empirical coefficient distribution answers a narrow fragility question about alternative label allocations. It cannot justify an exchangeability assumption, correct nonrandom treatment selection, validate the parallel-trends condition or replace a cluster-robust causal test; the reported exact tail frequency is only descriptive.

Finally, annual LDR is a ratio and cannot reveal whether adjustment came from gross loan growth, deposit contraction, write-offs, asset sales or changes in qualifying loan composition. A future study using quarterly supervisory LDR, gross loans, deposits, sectoral lending and asset-quality data could test those mechanisms directly.

5.0 Conclusion and Recommendations

This study examines whether bank-level conclusions around Nigeria's 2019 LDR tightening are robust to how exposure is measured and sourced. The primary end-Q3 2019 classification is reconciled against direct issuer material where available and is explicitly treated as policy affected rather than pre-policy. The binary differential is 10.46 percentage points with a wide 95 per cent confidence interval of -12.98 to 33.90, while the continuous exposure estimate is also imprecise. These results do not support a precise heterogeneous post-tightening association.

Estimates vary with exposure source, sample composition, group assignment and post-period window, while the preferred intervals remain wide. An issuer-reported or issuer-derived five-bank sample yields 2.34 pp (95% CI −18.58 to 23.26) versus 10.46 pp (−12.98 to 33.90) in the larger sample. That difference cannot distinguish sample selection from construct error. The paired eleven-bank comparison with an outcome-defined 2018 classification is also exploratory. The analysis therefore neither identifies a causal policy effect nor establishes a population-level difference between constructs; it documents the practical fragility of public-data threshold comparisons.

For stronger evaluation, the regulator could release anonymized or bank-identified quarterly supervisory ratios alongside: (i) the qualifying gross loan numerator disaggregated by product category and each 150% weight; (ii) the eligible deposit base and any exclusion rules; (iii) compliance gaps in both ratio points and naira lending shortfalls; and (iv) reserve-requirement penalties, dates and policy changes. This would allow researchers to distinguish new lending from deposit-base contractions, reclassification and denominator management, reconstruct regulatory exposure on a pre-announcement date, examine mechanisms and test whether bank-specific shocks explain later outcomes. Disclosure would need appropriate confidentiality protection.

Data Availability and Reproducibility

The supplied code reproduces the descriptive statistics, fixed-effects estimates, interval calculations, five-bank issuer-evidence restriction, outlier and window checks, event-study figure, and same-sample grouping contrasts. The 2015–2023 outcome panel uses Hassan (2026) as immediate source; issuer verification covers Unity Bank's 2016–2018 LDR values only. End-Q3 measures mix issuer and secondary reporting and do not recreate weighted supervisory compliance ratios. The workbook provides observation-level provenance, measurement tiers, estimated specifications and source sensitivity. Further bank-year issuer verification and component reconciliation remain outside the evidentiary scope of these data; no causal claim is made.

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