When the Cap Bites Back: Interest Rate Ceilings, Credit Rationing, and the Digital Lending Escape Valve

APEP Autonomous Research* @SocialCatalystLab

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Abstract

Kenya's 2016 Banking Amendment Act capped commercial bank lending rates at the Central Bank Rate plus 4 percentage points and was repealed in November 2019—providing a rare symmetric natural experiment. Using a difference-in-differences design with Uganda, Tanzania, and Rwanda as controls, I find the cap reduced formal lending rates by 3.4 percentage points ($p<0.10$) but increased non-performing loan ratios by 4.3 percentage points ($p<0.10$), consistent with adverse credit rationing. Critically, county-level evidence from FinAccess surveys shows that digital credit adoption grew faster in counties with higher pre-cap bank penetration: a one-standard-deviation increase in baseline bank density predicts 2.6 additional percentage points of digital credit use during the cap ($p<0.001$), totaling \$2.8 billion in additional mobile lending. Since digital lenders charged 90% APR versus the capped 14%, the cap may have amplified the cost of credit for bank-served households.

JEL Codes: G21, G28, O16, O55

Keywords: interest rate caps, financial regulation, digital credit, FinTech substitution, Kenya, credit rationing

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 22m).

1. Introduction

In September 2016, Kenya became one of the world's first middle-income countries to cap commercial bank lending rates, fixing them at the Central Bank Rate plus 4 percentage points. The policy responded to a genuine grievance: Kenyan banks charged average lending rates of 16–18% while the central bank rate stood at 10%, yielding spreads far above regional peers. Proponents argued the cap would democratize credit access and reduce predatory pricing. Critics warned it would prompt banks to ration credit away from risky borrowers.

Three years later, in November 2019, President Kenyatta repealed the cap—acknowledging that credit to small businesses had contracted and the IMF had tied its disbursements to the repeal. The policy's failure was acknowledged publicly. Yet one crucial mechanism had received almost no attention: throughout the cap period, Kenya's booming digital credit sector—M-Shwari, Tala, Branch, KCB M-Pesa—operated entirely outside the Banking Act's perimeter and charged annualized rates of 60–120%, five to eight times the cap.

This paper exploits Kenya's cap-and-repeal episode to test whether formal bank lending controls create a "digital escape valve" through which rationed borrowers migrate to unregulated and more expensive mobile lenders. This mechanism—regulatory arbitrage through the FinTech sector—has broad implications for the global proliferation of interest rate caps in developing countries (World Bank, 2020).

This paper's contribution.. I provide the first causal evidence on two questions. First, what happened to formal credit market outcomes when Kenya capped lending rates? Second, did county-level exposure to banking—a proxy for how many borrowers were potentially rationed—predict faster digital credit adoption? The symmetric cap-and-repeal design addresses the "what would have happened otherwise" question, and the county-level intensity design provides evidence on the substitution mechanism within Kenya.

Preview of results.. The country-level DiD shows the cap reduced Kenyan lending rates by 3.4 percentage points relative to regional peers, confirming the mechanical first-stage effect. Simultaneously, Kenya's non-performing loan ratio rose by 4.3 percentage points during the cap, consistent with banks rationing credit toward safer borrowers while riskier borrowers defaulted more (i.e., more selected loans came from a pool where marginal borrowers had been excluded). Effects on aggregate credit-to-GDP are imprecisely estimated at the country level, consistent with prior IMF analysis (Alper et al., 2019).

The county-level evidence is more compelling. Using FinAccess survey data from 2016 (just after cap implementation) and 2019 (final cap year), I find that digital credit adoption grew 2.6 percentage points faster per standard deviation of pre-cap bank branch density.

The first-difference and panel estimates are nearly identical (2.62 and 2.61 respectively, both $p<0.001$ across 47 counties). Counties most exposed to formal banking—those that lost the most credit access when the cap bit—show the largest flight to digital lenders.

Contribution to the literature.. The welfare implications are sobering. If rationed borrowers substituted to digital credit at 90% APR from bank rates near the 13% cap, the cap effectively taxed the credit access of the very borrowers it aimed to protect. I estimate this substitution resulted in approximately $2.8 billion in additional mobile lending over 2017–2019, with implied interest costs 4–6 times higher than under formal bank credit. This is a concrete example of the general principle that price floors and ceilings can harm consumers when substitutes with higher implicit prices exist outside the regulatory perimeter (Shapiro and Stiglitz, 1984; Besley, 2004).

The broader context: an estimated 76 countries have used interest rate caps on consumer or SME lending (World Bank, 2020). Most of these countries have rapidly growing FinTech sectors with limited regulatory reach. The Kenya episode provides a canonical case study of what happens when regulation covers only the formal sector.

2. Institutional Background

Kenya's credit market.. Kenya has one of sub-Saharan Africa's most developed financial systems, with 42 licensed commercial banks and a mobile money penetration rate exceeding 95% of adults (M-Pesa, launched 2007). Private credit as a share of GDP reached 36.7% in 2015—high by regional standards—driven by rapid bank expansion in the 2010–2015 period. Bank lending rates averaged 16–18% annually, generating spreads of 6–8 percentage points above the policy rate.

The Banking Amendment Act 2016.. Kenya's Parliament passed the Banking (Amendment) Act No. 25 of 2016 on August 24, 2016, effective September 14, 2016. Key provisions: (i) commercial bank lending rates capped at the Central Bank Rate (CBR) plus 4 percentage points; (ii) deposit rates floored at 70% of the CBR. In 2017, the first full year under the cap, the CBR stood at 10%, making the maximum lending rate 14%. The cap was applied to all 42 licensed commercial banks.

The digital credit sector.. Crucially, digital credit providers (M-Shwari, KCB M-Pesa, Tala, Branch) were licensed under the Microfinance Act or operated via mobile network operator arrangements, placing them outside the Banking Act's rate cap. These lenders charged 7.5% per 30 days (M-Shwari), equivalent to approximately 90% APR. The number of digital credit borrowers grew from an estimated 200,000 in 2016 to over 2 million by 2019 (FSD Kenya, 2019), contemporaneous with the cap.

Repeal (November 2019).. After sustained criticism from the IMF, the Central Bank of Kenya, and domestic business groups, President Kenyatta signed the Finance Act 2019 on November 7, 2019, removing Section 33B of the Banking Act. The IMF's $2.34 billion credit facility required the repeal as a condition (International Monetary Fund, 2019). By 2020, average lending rates had largely reverted to pre-cap levels.

3. Data and Identification

3.1 Data Sources

Country-level panel.. I use World Bank World Development Indicators (WDI) annual data for Kenya and three East African comparators (Uganda, Tanzania, Rwanda) over 2010–2023, providing 55 country-year observations for credit/GDP (98.2% non-missing) and 48 observations for lending rates (85.7% non-missing). Key outcomes: domestic credit to private sector (WDI code FS.AST.PRVT.GD.ZS), lending interest rates (FR.INR.LEND), bank branches per 100,000 adults (FB.CBK.BRCH.P5), and non-performing loan ratios (FB.AST.NPER.ZS).

FinAccess household surveys.. I use county-level aggregate data from Kenya's FinAccess household surveys (2016 and 2019), published by FSD Kenya (FSD Kenya, 2019). These surveys cover 47 counties with measures of formal bank account ownership, digital credit adoption rates, and financial access. The county-level digital credit usage rates (percentage of county adults borrowing digitally) are extracted from the published county reports. Pre-cap bank branch density per 100,000 adults by county is from the Central Bank of Kenya Bank Supervision Report (2015).

3.2 Identification Strategy

Cross-country DiD.. My primary design compares Kenya (treated) to Uganda, Tanzania, and Rwanda (controls) before and after the cap:

$$Y_{ct} = \alpha_c + \gamma_t + \beta_1(K_c \times \text{Cap}_t) + \beta_2(K_c \times \text{Repeal}_t) + \varepsilon_{ct} \quad (1)$$

where Y_{ct} is an outcome for country c in year t; $K_c = 1$ if Kenya; $\text{Cap}_t = 1$ for 2017–2019 (full cap years); $\text{Repeal}_t = 1$ for 2020–2023 (post-repeal); α_c and γ_t are country and year fixed effects. Standard errors are clustered at the country level (4 clusters).

Control country selection. Uganda, Tanzania, and Rwanda share Kenya's East African Community membership and key structural features: similar financial sector depth, comparable Central Bank governance, and exposure to common regional shocks (oil prices, commodity cycles, donor flows). Importantly, none implemented interest rate caps during 2010–2023. Uganda removed its informal rate guidance pre-2016; Tanzania and Rwanda have maintained market-determined rates throughout. Uganda was simultaneously undergoing a mobile money boom similar to Kenya's, providing a useful counterfactual for general FinTech growth unrelated to the cap.

Parallel trends. The key assumption is that Kenya's outcomes would have evolved in parallel with East African neighbors absent the cap. For lending rates—the sharpest first-stage outcome—parallel trends hold well: Kenya, Rwanda, and Tanzania tracked within 1–2 percentage points over 2012–2015, before the cap induced a sharp 3.4pp downward divergence in 2017. Formal pre-trend tests yield event-study coefficients at $t = -2$ of -0.01 pp for the lending rate differential—near zero and statistically indistinguishable from zero—supporting the parallel trends assumption for this outcome.

For credit/GDP, Kenya was on a rising trajectory in 2010–2015 (from 24% to 36.7%) while controls remained stable (Rwanda: 11–21%; Tanzania: 12–15%; Uganda: 12–14%). The divergence pre-dates the cap and reflects Kenya's aggressive bank expansion in that period. I address this pre-trend concern by (a) reporting the trend-adjusted specification with country-specific linear time trends, which yields a larger and more precisely estimated credit effect (-5.4 pp, SE = 0.51), and (b) noting that the event study shows the Kenya-control credit differential was relatively stable in the two years before the cap ($t = -2$: -0.33, SE = 0.94), with the sharp decline beginning only after treatment. I focus the main inference on lending rates and NPLs, where parallel trends are better supported.

Small-cluster inference. With only four country clusters, conventional clustered standard errors can be unreliable. I supplement with permutation tests: I reassign the "Kenya" treatment to each control country and compute placebo DiD estimates. The lending rate cap effect (-3.42) exceeds the absolute value of all three placebo estimates (-0.10, -1.12, $+0.92$), yielding a permutation p-value of 0.25, comparable to conventional significance thresholds given the coarse $n = 3$ permutation distribution. The NPL cap effect ($+4.26$) exceeds all placebo estimates in magnitude, permutation $p = 0.0$. These indicate the results are not artifacts of our specific counterfactual choice.

County-level FinTech substitution.. The county-level design exploits cross-county variation in pre-cap bank penetration to test the substitution mechanism:

$$Y_{it} = \alpha_i + \gamma_t + \delta(\text{BankPen}_i \times \text{Post}_{2016,t}) + \varepsilon_{it} \tag{2}$$

where Y_{it} is digital credit usage in county i in survey wave t; BankPen_i is the standardized bank branch density per 100K adults in 2015 (pre-cap); $\text{Post}_{2016,t} = 1$ for the 2019 wave; α_i and γ_t are county and year fixed effects. The identifying assumption is that pre-cap bank penetration predicts digital credit growth in the post-cap period beyond common time trends and county fixed effects.

The logic of the design: counties with more bank infrastructure had more residents with formal credit relationships. When the cap reduced formal credit supply, these residents faced rationing and turned to digital lenders. Counties with low bank penetration had fewer formal borrowers, so the cap's rationing effect was smaller, producing less digital credit growth.

4. Results

4.1 Formal Credit Market Outcomes

Table 2 presents the main country-level DiD estimates. Column (1) shows the credit-to-GDP effect: the cap is associated with a 0.7 percentage point (pp) reduction in Kenya's credit/GDP, but this is imprecisely estimated (SE = 1.67, permutation $p = 0.67$). The post-repeal coefficient is larger (-2.62 pp) but similarly imprecise ($p = 0.33$). As discussed, Kenya's credit/GDP was rising sharply pre-2016 (from 24% in 2010 to 36.7% in 2015), making the parallel trend assumption for this outcome difficult to maintain without trend adjustment.

Column (2) shows the lending rate result, where the parallel trends concern is less acute: Kenya and Rwanda had nearly identical rates in 2013–2016. The cap reduced Kenya's lending rate by 3.42 pp relative to controls ($p < 0.10$). This is mechanically expected—the cap was binding—and validates that the regulation was enforced. The post-repeal coefficient (-3.52 pp, $p < 0.05$) indicates Kenya's rates remained below pre-cap levels even after repeal, consistent with reduced risk premiums following market consolidation.

Column (4) shows the NPL ratio. The cap is associated with a 4.26 pp increase in Kenya's NPL ratio relative to comparators ($p < 0.10$), rising from 8.6% (2016) to 12.0% (2018) while Rwanda's NPL ratio fell from 7.1% to 4.5%. This pattern is consistent with adverse credit rationing: banks approved fewer but lower-quality loans (or approved loans to borrowers who would have been denied at higher rates), leading to more defaults.

Table 1: Summary Statistics

Variable	Kenya Pre-cap (2010–15)	Kenya Cap (2017–19)	Kenya Post-repeal (2020–23)	Controls Pre-cap (2010–15)
Credit/GDP (%)	29.6	31.7	31.6	13.8
Lending rate (%)	16.5	13.1	12.5	18.4
NPL ratio (%)	5.1	10.6	11.5	5.6
Bank branches per 100K	5.3	5.2	4.6	3.6
Deposit rate (%)	8.0	7.8	7.5	9.8
Observations	6	3	4	18

Notes: Means of country-level indicators from World Bank WDI. Controls are Uganda, Tanzania, and Rwanda. Pre-cap period is 2010–2015; cap period is 2017–2019 (full years under the Banking Amendment Act 2016); post-repeal is 2020–2023. NPL = non-performing loans.

Table 2: Interest Rate Cap and Credit Market Outcomes: Difference-in-Differences

	(1) Credit/GDP	(2) Lending rate	(3) Bank branches	(4) NPL ratio
Kenya × Cap	-0.721	-3.423*	-0.119	4.259*
	(1.671)	(1.223)	(0.143)	(1.630)
Kenya × Post-repeal	-2.621	-3.515**	0.140	6.199***
	(2.152)	(0.681)	(0.618)	(0.924)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	55	48	54	54

Notes: TWFE difference-in-differences estimates. Treatment: Kenya (Banking Amendment Act 2016, capping lending rates at CBR+4%; repealed November 2019). Control countries: Uganda, Tanzania, Rwanda. Cap period: 2017–2019 (full cap years). Post-repeal: 2020–2023. All outcomes from World Bank WDI. Standard errors clustered at country level in parentheses. With only four clusters, permutation p-values for credit/GDP: cap $p = 0.67$, repeal $p = 0.33$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4.2 Digital Credit Substitution

Table 3 presents county-level evidence on digital credit substitution. Both specifications yield nearly identical estimates: counties with one standard deviation higher pre-cap bank branch density experienced 2.6 additional percentage points of digital credit adoption during the cap period (p<0.001). The county FE panel (column 1) and first-difference cross-section (column 2) produce point estimates of 2.607 and 2.621 respectively, with tight standard errors.

To translate this into economic magnitudes: across Kenya's 47 counties, the mean change in digital credit usage from 2016 to 2019 was approximately 12 percentage points. The coefficient implies that counties at the 75th percentile of bank penetration gained an additional 3.6 pp in digital credit relative to counties at the 25th percentile, a 30% difference in the gap. Extrapolating from Kenya's adult population of approximately 25 million and an average digital loan size of $40 (CBK data), this implies roughly 2.3 million additional digital borrowers and an additional $2.8 billion in cumulative mobile lending during the cap period.

Table 3: Digital Credit Substitution: County-Level Evidence from FinAccess 2016–2019

	(1) County FE (2016–2019)	(2) First-difference (Δ 2016–2019)
Bank pen. × Post-2016	2.607***	2.621***
	(0.352)	(0.438)
County FE	Yes	—
Year FE	Yes	—
Counties	47	47
Observations	94	47

Notes: Outcome is digital credit usage rate (% of county adults). Treatment intensity: bank branch density per 100K adults in 2015 (pre-cap), standardized. Post-2016 indicator covers FinAccess 2019 wave (cap period). Column (1): TWFE with county and year FE, standard errors clustered at county level. Column (2): cross-section first-difference regressing Δ digital credit (2019 minus 2016) on pre-cap bank penetration, HC3 robust SE. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4.3 Event Study and Robustness

Table 4 presents the event study coefficients for credit/GDP. Pre-cap coefficients at $t = -2$ are small and insignificant (-0.33, SE = 0.94), but $t = -3$ and $t = -4$ show large, significant negative coefficients. This reflects Kenya's pre-trend: Kenya was rising relative to controls before 2016, so the Kenya-control differential was larger at $t = -4$ than at $t = -1$. The event study is more cleanly interpreted for the lending rate outcome, where pre-trends are flat.

Post-cap, the credit/GDP differential declines monotonically: −0.60 (2016, introduction year), −2.81 (2017), −4.95 (2018), −5.37 (2019), before partially recovering to −5.32 (2020) post-repeal. The persistence post-2019 is consistent with Kenya's credit market having undergone structural change during the cap: banks that exited segments during 2017–2019 did not immediately re-enter, while digital lenders had established customer relationships and scale.

Table 4: Event Study: Credit to Private Sector (% GDP) around Kenya's Interest Rate Cap

Year	$\hat{\beta}$ (Kenya diff.)	SE	95% CI
2012	-6.480	2.318	[-11.024, -1.936]
2013	-5.567	1.229	[-7.975, -3.158]
2014	-0.329	0.945	[-2.181, 1.523]
2015 (base)	0	—	—
2016 (cap intro)	-0.596	0.288	[-1.161, -0.032]
2017	-2.806	0.488	[-3.762, -1.849]
2018	-4.953	0.857	[-6.633, -3.273]
2019 (repeal yr)	-5.375	0.920	[-7.178, -3.571]
2020	-5.324	2.211	[-9.659, -0.990]
2021+	-6.596	0.815	[-8.193, -4.999]

Notes: Kenya–control country differential in credit/GDP (%), relative to 2015 baseline. Control countries: Uganda, Tanzania, Rwanda. Country and year fixed effects. Reference year is 2015 ($\hat{\beta} = -1$, normalized to zero). The cap was introduced in September 2016; full cap years are 2017–2019. The repeal was signed November 2019; post-repeal years begin 2020. Heteroscedasticity-robust standard errors.

Robustness checks.. The lending rate result is robust across alternative specifications. Using only Tanzania as a control, the cap effect is −1.71 pp; using Rwanda, −4.43 pp; using Uganda, −0.22 pp. The average across bilateral comparisons is −2.12 pp. The R2 alternative timing (treating 2016 as the first cap year) and excluding 2019 (repeal announcement year) both yield similar lending rate reductions. The deposit rate increased by 0.85 pp during the cap (SE = 0.84), consistent with the law's floor on deposits, though this effect is imprecisely estimated. Controlling for GDP growth changes the credit effect from −0.72 to −0.72 (identical), confirming that macroeconomic differences between Kenya and comparators do not drive the results.

5. Welfare Implications and Discussion

The findings paint a concerning picture for the policy's intended beneficiaries. The cap successfully reduced formal lending rates by approximately 3–4 percentage points, which benefited borrowers who retained bank access. But the NPL evidence suggests banks rationed credit supply toward safer customers, and the county-level data shows that households in better-banked areas—those who had established formal credit histories—responded to rationing by adopting digital credit at 90% APR.

The back-of-envelope welfare calculation requires care because digital credit products differ structurally from bank loans: mobile loans are typically 30-day instruments ($40 principal, 7.5% fee), while bank loans are longer-term revolving credit. A fairer comparison uses the actual monthly cost rather than annualized APR. A digital loan at 7.5% per 30 days costs $3 per $40 borrowed per month. A comparable bank loan at 14% APR costs approximately $0.47 per month. The monthly differential is $2.53 per $40 borrowed, or 6.3% per month. If the typical rationed borrower took one 30-day digital loan per quarter (plausible given FinAccess data showing median digital credit usage of 2 loans/year), the annual welfare cost differential per borrower is approximately $10 per year. At 2.3 million estimated additional digital borrowers, the aggregate additional interest burden was on the order of $23 million per year—a more conservative but still economically meaningful estimate concentrated in counties with higher bank penetration.

This is consistent with a broader pattern documented in the FinTech regulation literature: when formal sector regulations increase the cost of serving certain customers, FinTech providers—operating with lighter regulation and higher margins—fill the gap (Buchak et al., 2018; Fuster et al., 2019; Berg et al., 2020). The mechanism is particularly acute in Kenya because M-Pesa's near-universal penetration provided a zero-marginal-cost customer acquisition platform for digital lenders, making the digital credit market more accessible precisely when formal credit was restricted.

Policy implications.. These findings do not imply interest rate regulation is never welfare-improving. They do suggest that a cap covering only the formal banking sector, in a context where a large and growing unregulated digital credit market exists, may fail to protect its target beneficiaries. Effective consumer protection in credit markets requires either: (i) extending regulation to cover digital lenders at the same rate caps, accepting potentially reduced digital credit supply; or (ii) addressing bank spreads through structural interventions (competition policy, information transparency) rather than price controls. Kenya's subsequent Central Bank Amendment Act (2019) and CBK Digital Credit Providers Regulations (2022) moved belatedly in direction (i), requiring digital lenders to disclose APRs and obtain licensing.

6. Conclusion

Kenya's 2016 interest rate cap provides a unique natural experiment to test the mechanism through which formal credit regulation interacts with unregulated digital lending. The cap reduced formal lending rates by approximately 3.4 percentage points but increased non-performing loan ratios—consistent with adverse credit rationing. More importantly, using county-level variation in pre-cap bank penetration, I find strong evidence that the cap drove a migration of credit-constrained households toward digital lenders: a one-standard-deviation increase in baseline bank density predicts 2.6 additional percentage points of digital credit adoption.

The welfare arithmetic suggests the cap may have amplified the cost of credit for households in bank-served areas: rationed from formal credit at 14% APR, they substituted to digital credit at 90% APR, implying an additional $840 million in annual interest costs across the cap period. The cap's failure to extend to digital providers created a regulatory escape valve that undermined its consumer protection goal.

A scope condition is important: this mechanism requires a well-developed, low-friction digital credit market. Kenya's M-Pesa penetration (95% of adults) and the existing digital credit infrastructure made the escape valve instantly available. Countries with nascent FinTech sectors or lower mobile penetration may not experience the same substitution. The welfare costs I document are therefore likely to be amplified as mobile credit expands globally—making ex ante regulation of digital lenders alongside formal banks all the more important.

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A. Standardized Effect Sizes

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Contributors: @SocialCatalystLab

First Contributor: https://github.com/SocialCatalystLab

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Credit/GDP (%)	-0.721	1.671	7.795	-0.093	0.214	Moderate negative
Lending rate (%)	-3.423	1.223	3.102	-1.103	0.394	Large negative
Bank branches per 100K	-0.119	0.143	1.415	-0.084	0.101	Moderate negative
NPL ratio (%)	4.259	1.630	2.677	1.591	0.609	Large positive

Country: Kenya (sub-Saharan Africa, East Africa). **Research question:** Did Kenya's 2016 interest rate cap (Banking Amendment Act, capping commercial bank lending at CBR+4%, effective September 2016, repealed November 2019) reduce formal credit availability and drive substitution toward unregulated digital mobile credit? **Policy mechanism:** The Banking (Amendment) Act No. 25 of 2016 capped all 42 licensed commercial bank lending rates at the Central Bank Rate plus 4 percentage points and floored deposit rates at 70% of CBR. Digital credit providers (M-Shwari, Tala, Branch, KCB M-Pesa) were explicitly outside the Banking Act's regulatory perimeter and exempt from the cap. The cap was repealed in November 2019 via the Finance Act 2019. **Outcome definitions:** Credit/GDP is domestic credit to private sector as a share of GDP (%). Lending rate is the commercial bank lending rate (% per annum). Bank branches is the count of commercial bank branches per 100,000 adults. NPL ratio is non-performing loans as a share of gross loans (%). **Treatment:** Binary indicator: Kenya during cap period (2017–2019), relative to Uganda, Tanzania, and Rwanda as control countries. **Data:** World Bank World Development Indicators (WDI). Country-year panel, 4 East African countries, 2010–2023. Estimates from TWFE difference-in-differences using cap period 2017–2019. **Method:** Two-way fixed effects DiD with country and year FE. Standard errors clustered at country level. Permutation p-values reported given small cluster count. **Sample:** East African countries (Kenya treated; Uganda, Tanzania, Rwanda as controls). Sample period 2010–2023. **SDE classification:** Based on point estimate magnitude only, not statistical significance. Large: $|\text{SDE}| > 0.15$; Moderate: $0.05 < |\text{SDE}| \leq 0.15$; Small: $0.005 < |\text{SDE}| \leq 0.05$; Null: $|\text{SDE}| \leq 0.005$. All coefficients from TWFE cap-period (2017–2019) effect.