**International Journal of Economics, Commerce and Management**
United Kingdom ISSN 2348 0386 Vol. 14, Issue 10, Oct 2026

***https://ijecm.co.uk/***

# DOES MOBILE DIGITAL CREDIT PAY OFF FOR BANKS? STAGGERED-ADOPTION EVIDENCE FROM NSE-LISTED COMMERCIAL BANKS IN KENYA, 2010-2025

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**Abstract**

*This study examines whether the launch of mobile-phone-delivered credit, the signature financial innovation in Kenyan banking since 2014, improved the performance of commercial banks listed on the Nairobi Securities Exchange (NSE), and whether any gains were immediate, delayed or transitory. The study combined a 16-year sector record (2010–2025) from the Central Bank of Kenya (CBK) with a bank-level panel covering all ten NSE-listed Kenyan banks for FY2014–FY2025 (118 bank-years). Product launches are dated from public records and staggered between 2014 and 2024, with one bank never adopting. Because two-way fixed-effects (TWFE) estimators are biased under staggered adoption with heterogeneous effects, we estimate the Callaway–Sant'Anna group-time average treatment effects and the Sun–Abraham interaction-weighted event study. With ten clusters, inference relies on the wild cluster restricted bootstrap and Fisher randomisation inference. Findings suggested that adoption has no detectable average effect on return on average equity (ROaE). The heterogeneity-robust estimate is +0.21 percentage points (randomisation p = 0.90), and the 95% confidence interval for the TWFE effect excludes effects larger than about ±5 percentage points. There is no evidence of pre-adoption divergence. The dynamic profile is J-shaped: small, insignificant shortfalls in the first three years are followed by positive but imprecise effects in years three and four (+3.7 points, p = 0.18). TWFE suggests significant gains in net interest margin and declines in non-performing loans, but neither effect survives the heterogeneity-robust estimators. This is the first Kenyan study to treat financial innovation as a dated, staggered event and to apply modern difference-in-*

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*differences and few-cluster inference. It shows that conventional TWFE results in this literature can be artefacts of estimator bias. For listed banks, mobile credit has become a competitive necessity rather than a source of excess returns.*
*Keywords: Financial innovation; digital credit; mobile banking; bank performance; staggered difference-in-differences; Kenya*

## INTRODUCTION

Kenya is one of the world's most closely studied laboratories for financial technology. The launch of M-Pesa in 2007 created a payments rail that, by December 2025, ran through more than 473,000 active agents and 89 million registered accounts (CBK, 2026b). Mobile money lowered transfer costs and improved households' ability to smooth shocks (Jack & Suri, 2014; Suri & Jack, 2016). Commercial banks responded by moving lending onto mobile phones. Co-operative Bank launched MCo-op Cash loans in 2014; KCB launched KCB M-Pesa in 2015; and Equity Bank launched loans through its Equitel network around the same time. Later adopters included Barclays (now Absa) with Timiza in 2018 and Standard Chartered with SC Juza in 2024. Today, almost every listed bank prices and disburses small loans through algorithms rather than branches, and digital credit has become one of the fastest-growing segments of the financial system (CBK, 2026a).

Whether this innovation has improved bank performance is less clear than its welfare effects on households. Theory points both ways. Schumpeterian and constraint-induced accounts predict rents for innovators (Schumpeter, 1934; Silber, 1983). Diffusion and competition arguments predict that such rents are competed away once rivals imitate (Rogers, 2003; Vives, 2019). Mobile credit could raise margins by pricing short-tenor loans at high yields and widen fee income, but it could also raise credit risk and technology costs (Beck et al., 2016). Sector-level data hint at the tension. Between 2010 and 2025, mobile-money agents grew twelvefold, yet the pre-tax return on assets (ROA) of Kenyan banks fell from 4.4% to 3.7%, and the gross non-performing loan (NPL) ratio rose from 6.3% to 16.0% (Figure 1).

A large Kenyan literature, much of it in postgraduate theses, reports that mobile, agency and internet banking raise bank profitability (e.g., Kagema, 2020; Ananga et al., 2024). These studies share three problems that limit causal interpretation. First, innovation is usually measured by sector-wide transaction volumes or survey perceptions, which cannot distinguish banks that innovate from those that grow. Second, cross-sectional or pooled ordinary least squares designs over short windows confound innovation with the interest rate cap of 2016–2019, the adoption of IFRS 9 in 2018 and the COVID-19 shock of 2020. Third, these designs ignore a now well-documented

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property of TWFE regressions: when adoption is staggered and effects vary over time, TWFE compares newly treated banks with already-treated ones and can return estimates of the wrong sign (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfœuille, 2020; Roth et al., 2023).

This paper addresses these limitations. We date, for each of the ten NSE-listed Kenyan banks, the first public launch of a mobile-phone-delivered credit product. This yields a staggered adoption design with cohorts in 2014, 2015, 2018, 2019, 2022 and 2024, and one bank that had not adopted by 2025. We then estimate group-time average treatment effects with not-yet-treated controls (Callaway & Sant'Anna, 2021) and an interaction-weighted event study (Sun & Abraham, 2021). Ten clusters are too few for conventional cluster-robust inference, so we use the wild cluster restricted bootstrap with Webb weights (Cameron et al., 2008; MacKinnon & Webb, 2018; Webb, 2023) and randomisation inference over the assignment of adoption dates (Young, 2019). A 16-year sector record from CBK places the bank-level evidence within the longer arc of Kenyan banking since 2010.

The findings are threefold. First, adoption of mobile credit has no detectable average effect on ROaE. The point estimates are close to zero, pre-adoption trends are flat, and the confidence interval rules out effects larger than about five percentage points in either direction. Second, the dynamic profile is consistent with an adjustment-cost or "J-curve" account: profitability dips slightly in the first three years and turns positive thereafter. The later gains are imprecisely estimated and should be read as suggestive. Third, the apparently significant TWFE effects on net interest margin (NIM) and asset quality do not survive heterogeneity-robust estimation. This is a methodological warning for a literature that relies almost exclusively on TWFE and pooled regressions.

The paper contributes in three ways. To our knowledge, it is the first study of Kenyan banking to treat financial innovation as a dated, bank-specific event and to apply modern staggered difference-in-differences with few-cluster inference. It provides evidence on the supply side of the digital-credit revolution, complementing household-level studies (Suri et al., 2021) and cross-country evidence on alternative digital lending (Cuadros-Solas et al., 2023). It also reframes the managerial and policy question. Mobile credit appears to have become a competitive necessity, "table stakes" for listed banks, rather than a durable source of excess return, so performance differences must be sought in complementary capabilities rather than in adoption itself.

The rest of the paper is organised as follows. Section 2 describes the institutional setting. Section 3 develops the theoretical framework and hypotheses. Section 4 summarises the empirical literature. Section 5 sets out the research design, data and identification strategy. Section 6 reports the results. Section 7 presents the robustness checks. Section 8 discusses the findings. Section 9 concludes.

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## INSTITUTIONAL BACKGROUND: SIXTEEN YEARS OF KENYAN BANKING, 2010–2025

Figure 1 summarises the sector between 2010 and 2025. Three phases are visible. From 2010 to 2014 rapid balance-sheet growth coincided with high and stable profitability: pre-tax ROA was between 4.4% and 4.7% and the gross NPL ratio below 6%. Agency banking, permitted under CBK guidelines from 2010, expanded quickly, from 9,748 active bank agents in 2011 to 40,592 in 2015 (CBK, 2012, 2016).

The second phase, 2015–2019, combined the first wave of bank-led mobile credit with three shocks: the failure of three banks in 2015-2016, the statutory cap on lending rates from September 2016 to November 2019, and the adoption of IFRS 9 in January 2018. The rate cap compressed margins and pushed banks towards government securities and away from small borrowers (Alper et al., 2020). Pre-tax ROA fell to about 3.3%, and the NPL ratio doubled to 12.5%.

The third phase, 2020–2025, began with the COVID-19 shock, which drove sector ROA to a low of 2.1% in 2020. It then saw a recovery accompanied by consolidation, rapid growth in non-bank digital lenders, and an NPL ratio that peaked at 17.1% in 2024. CBK reports that licensed digital credit providers held KSh 110.1 billion in loans by the end of 2025, almost double the 2024 level (CBK, 2026a). By 2025, the leading banks processed the large majority of transactions outside the branch. Bank performance has therefore become more volatile, and its link to innovation harder to see, over the same years in which digital channels became dominant.

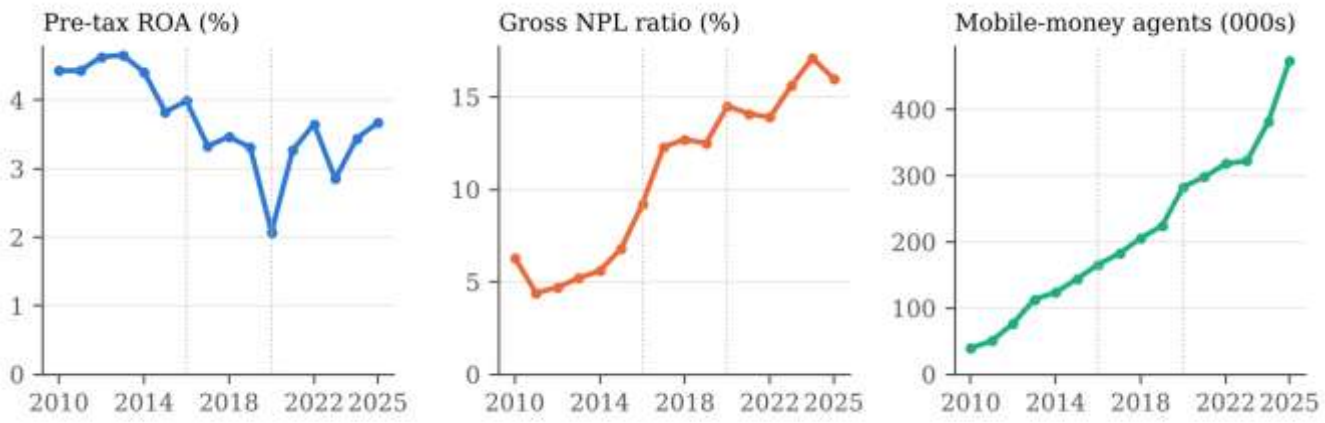



Figure 1. The Kenyan banking sector, 2010–2025.

Pre-tax ROA = profit before tax / total net assets; gross NPL ratio = gross non-performing loans / gross loans; mobile-money agents = active agents in December. Dotted lines mark the introduction of the interest rate cap (2016) and the COVID-19 shock (2020).

Source: CBK Bank Supervision Annual Reports 2011–2025; CBK National Payments System statistics.

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## THEORETICAL FRAMEWORK AND HYPOTHESES

### Why innovation could raise bank performance

Financial innovation theories explain new products as responses to frictions, regulation and technology. In Schumpeter's (1934) account, the innovator earns temporary rents by recombining resources in new ways. Silber's (1983) constraint-induced theory predicts that banks innovate to relax binding constraints on their balance sheets or pricing. Kenya's interest rate cap, which made small unsecured branch loans unprofitable, is such a constraint. Mobile credit lowers the marginal cost of originating and servicing small loans, uses transaction data from mobile-money and bank accounts to reduce information asymmetry, and can be priced through fees rather than nominal interest. These channels predict higher NIM, higher non-funded income, and, if credit scoring is effective, lower credit losses. Empirical work on earlier delivery technologies supports this logic. Internet adoption raised the profitability of Spanish and US community banks, mainly by lowering overheads and raising service-charge income (Hernando & Nieto, 2007; DeYoung et al., 2007), and technological progress improved bank productivity more generally (Berger, 2003).

### Why gains could be absent, delayed or transitory

Three arguments cut the other way. First, diffusion erodes rents. When a technology is available to all competitors through vendors or platform partnerships, as with M-Pesa-linked lending, first-mover advantages are short-lived and margins are competed away (Rogers, 2003; Vives, 2019; Stulz, 2019). Second, the resource-based and dynamic-capability views hold that technology yields sustained advantage only when combined with complementary organisational resources that rivals cannot easily replicate (Teece et al., 1997; Milgrom & Roberts, 1990). The ability to absorb and exploit new knowledge is itself a capability (Cohen & Levinthal, 1990), and complementarities between technological and organisational innovation drive performance (Vahter & Vadi, 2024). Banks whose lending relies on relationships and established routines adopt and exploit algorithmic credit more slowly (Mishra et al., 2022). Third, digital credit carries costs: investment in platforms, revenue-sharing with mobile network operators, fraud and cyber risk, and losses on thin-file borrowers. Financial innovation is associated with both faster growth and greater bank fragility (Beck et al., 2016), and non-bank digital lending is associated with lower bank ROA and NIM across countries (Cuadros-Solas et al., 2023). Fintech competition for payment flows can also erode the information advantage banks derive from observing customers' transactions (Parlour et al., 2022).

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These arguments imply an adjustment-cost or J-curve path. Profitability first falls as banks invest, learn, and absorb early credit losses, then recovers and may exceed its pre-adoption level once scoring models and processes mature. Hernando and Nieto (2007) find exactly such lags for internet banking: profitability gains appeared only after one and a half to three years.

**Hypotheses**

**H1 (average effect).** Launching a mobile-phone-delivered credit product increases a bank's return on average equity relative to banks that have not yet adopted.

**H2 (dynamics).** The effect of adoption on profitability is negative or zero in the first years after launch and positive in later years (adjustment-cost account), rather than immediately and permanently positive (sustained-gain account).

**H3 (mechanisms).** Adoption increases NIM (H3a) and the share of non-funded income (H3b), and changes the gross NPL ratio (H3c, sign ambiguous: better scoring versus riskier borrowers).

**H4 (heterogeneity).** Gains are larger for early adopters, which are also the largest banks with the deepest customer bases and complementary infrastructure.

## SYNTHESIS OF THE EMPIRICAL LITERATURE

### International evidence on bank technology and performance

Frame and White (2004) observed two decades ago that financial innovation attracted "lots of talk, little action" in empirical research. The evidence has since grown but remains mixed. Studies of earlier delivery technologies find performance gains that arrive with lags and work through cost and fee channels (Berger, 2003; DeYoung et al., 2007; Hernando & Nieto, 2007). Studies of the recent fintech wave find that effects depend on bank type and market structure. For Chinese banks, Chen et al. (2021) report that the impact of FinTech on performance varies markedly across bank types, consistent with "survival of the fittest" rather than a uniform leap forward. Cross-country evidence shows that alternative digital lending by non-banks is associated with lower bank profitability, especially where regulation is lax (Cuadros-Solas et al., 2023). Digital transformation is associated with lower bank risk-taking in Vietnam (Hoque et al., 2024), and digital finance and inclusion with greater banking stability internationally (Anton & Nucu, 2024). Conceptual reviews stress that fintech both threatens bank franchise values and offers banks the option to partner rather than compete (Philippon, 2016; Thakor, 2020; Murinde et al., 2022; Anagnostopoulos et al., 2025), and that technological change reshapes bank business models over time (Gupta et al., 2023; Bueno et al., 2024).

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### Evidence from Kenya and Africa

The best-identified Kenyan studies focus on households rather than banks. M-Pesa improved risk sharing and reduced poverty (Jack & Suri, 2014; Suri & Jack, 2016), and access to M-Shwari digital loans improved household resilience to shocks (Suri et al., 2021). Payment innovation can also ease firms' financing constraints (Beck et al., 2018), and mobile money is a central driver of digital financial inclusion (Shaikh et al., 2022; Kajol et al., 2022). On the bank side, Kenyan evidence comes mainly from postgraduate studies and survey-based articles. These typically regress ROA or ROE on sector-level volumes of mobile, agency and internet banking, or on managers' perceptions, and almost uniformly report positive associations (Kagema, 2020; Ananga et al., 2024). Some studies covering 2007–2017 report mixed effects, with some innovations improving performance and others being loss-making or insignificant.

### Synthesis and gap

Three conclusions follow. First, the international evidence does not support an unconditional positive effect of innovation on bank profitability. Effects are lagged, conditional on complementary capabilities, and eroded by competition. Second, the Kenyan bank-level evidence is dominated by designs that cannot disentangle the effect of innovation from bank growth, macro-financial shocks, or regulatory regimes, and that measure innovation using aggregates common to all banks. Third, no Kenyan study has exploited the staggered timing of bank-specific digital credit launches or used estimators that remain valid when effects vary across adoption cohorts and over time. The present study fills this gap.

## RESEARCH METHODOLOGY

### Research design

The study adopts a quantitative, longitudinal and explanatory research design grounded in a positivist philosophy. It proceeds deductively: hypotheses H1–H4 are derived from financial innovation, diffusion and dynamic-capability theory (Section 3) and tested against observed bank outcomes. The design is explanatory rather than descriptive or correlational, because the research question asks whether launching mobile digital credit caused a change in bank performance, not only whether the two move together.

Specifically, the study uses a quasi-experimental, staggered-adoption difference-in-differences design applied to a bank-level panel of secondary data. The launch of a mobile-phone credit product is treated as an event that occurred at different dates across banks. Banks that launched in a given year form a treatment cohort, while banks that had not yet launched, or never launched, form the comparison group. The design compares each cohort's post-launch

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performance with the change over the same years among comparison banks, thereby differencing out common shocks and stable differences between banks. This design suits the Kenyan setting for three reasons: launches were spread across 2014–2024 rather than occurring at a single date; random assignment of the innovation is impossible in a live banking system; and the cross-sectional and pooled correlational designs used in earlier Kenyan studies cannot separate the innovation from bank growth, regulatory regimes and macroeconomic shocks (Section 4.3).

The design has two tiers. The first is a descriptive longitudinal analysis of the Kenyan banking sector over sixteen years (2010–2025), documenting trends in profitability, asset quality and digital financial infrastructure against which the bank-level evidence is interpreted. The second is the causal analysis: a bank-level panel covering FY2014–FY2025. The unit of analysis is the bank-year. The study takes a census of the target population (all commercial banks continuously listed on the NSE over the panel period), so no sampling procedure is required and sampling error does not arise. All data are secondary, drawn from published audited financial results, CBK supervisory publications and contemporaneous public records of product launches. The study therefore involves no human participants and raises no confidentiality concerns.

**Target population, sample and data sources**

The population comprises the commercial banks listed on the NSE. We include all ten banks continuously listed from 2014 to 2025: Absa Bank Kenya (formerly Barclays Bank of Kenya), Co-operative Bank of Kenya, Diamond Trust Bank Kenya (DTB), Equity Group, HF Group, I&M Group, KCB Group, NCBA Group (NIC Group before the 2019 merger with CBA), Stanbic Holdings (formerly CfC Stanbic) and Standard Chartered Bank Kenya (SCBK). National Bank of Kenya is excluded because it was delisted after its acquisition by KCB in 2019. The ten banks held a majority of the sector's assets throughout the period.

Bank-level ratios are drawn from Cytonn Investments' annual Kenya Listed Banks reports for FY2015–FY2025. These reports compile each bank's audited, CBK-format published financial statements; FY2014 values are taken from the comparatives in the FY2015 report. Cytonn revises some figures across releases. We therefore apply a fixed hierarchy: values computed from the bank-level financial statement extracts take precedence, followed by the contemporaneous full-year table. Every value is stored with its source (Appendix A). Where releases disagree, alternative values are retained and used in a robustness check. As an internal consistency check, we recomputed ROaE from reported profit after tax and average shareholders' funds for the 21 bank-years (2022–2025, six banks) in which both are reported;

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every recomputed value is within 0.1 percentage points of the reported one. Sector data for 2010–2025 come from the CBK Bank Supervision Annual Reports and the CBK National Payments System statistics.

The 16-year horizon is covered at sector level (2010–2025). Comparable bank-level ratios for all ten listed banks are available from FY2014, so the causal analysis uses 2014–2025, which brackets every listed-bank launch of mobile credit.

**Variables and measurement**

**Outcome.** The primary outcome is return on average equity (ROaE, %), defined as profit after tax divided by the average of opening and closing shareholders' funds. ROaE is available for 118 of 120 bank-years (I&M and HF are missing for 2023). Secondary outcomes, used to test mechanisms, are NIM (%), non-funded income as a share of total operating income (NFI, %), and the gross NPL ratio (%). These are available consistently for 2017–2025 (NIM and NFI exclude 2023).

**Treatment.** The treatment indicator D_it equals one from the first year in which bank i offers a credit product applied for and disbursed via a mobile phone (USSD, SIM toolkit, mobile money wallet, or smartphone application) to retail or small-business customers, and zero before then. Launch dates are drawn from contemporaneous press and company announcements (Table 1). Treatment is absorbing: no bank withdrew mobile credit once launched. Equity Bank's date is coded as 2015, when Equitel loans became available; an alternative coding of 2016 is used in robustness. NCBA's 2019 date coincides with the merger that brought CBA's M-Shwari and Fuliza franchises into the group, so we also report results without NCBA.

Table 1. Launch of mobile-phone-delivered credit by NSE-listed Kenyan banks

| Bank | Product | Cohort | Evidence (date of public launch) |
|---|---|---|---|
| Co-operative Bank | MCo-op Cash loans | 2014 | MCo-op Cash mobile loans, launched Aug 2014 (Business Daily, 14 Aug 2014) |
| Equity Group | Equitel / Eazzy loans | 2015 | Equitel (MVNO, SIMs from Oct 2014) mobile loans by 2015; loan limit raised Apr 2016 (Business Daily, 3 Apr 2016); alt coding 2016 |
| KCB Group | KCB M-Pesa | 2015 | KCB M-Pesa with Safaricom, launched 10 Mar 2015 (Business Daily, 10 Mar 2015) |
| Absa Bank Kenya | Timiza (app) | 2018 | Timiza (Barclays Kenya) launched Mar 2018 (Capital FM / HapaKenya, Mar 2018) |

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| Bank | Product | Cohort | Evidence (date of public launch) |
|---|---|---|---|
| HF Group | HF Whizz | 2018 | HF Whizz digital platform incl. mobile loans, launched 24 Jul 2018 (Business Daily) |
| NCBA Group (NIC) | NIC NOW loans; M-Shwari & Fuliza via CBA merger | 2019 | NIC mobile loans (NIC NOW app) by 2019 and CBA merger (M-Shwari, Fuliza) Oct 2019; merger confound |
| Stanbic Holdings | M-Jeki (SME loans via mobile money) | 2019 | M-Jeki mobile-money-delivered SME loans, May 2019 (HapaKenya, 29 May 2019) |
| I&M Group | Unsecured digital loans (On-the-Go app) | 2022 | Automated unsecured digital lending via On-the-Go app, Jan 2022 (Capital FM, 18 Jan 2022) |
| Standard Chartered Kenya | SC Juza (app) | 2024 | SC Juza mobile lending app, Jun 2024 (HapaKenya/Khusoko, 4 Jun 2024) |
| Diamond Trust Bank | — | Never (to 2025) | No mobile-delivered credit product documented in public sources through 2025 (never-treated) |

Notes: Cohort = first fiscal year in which the product was available. Evidence from contemporaneous press and company announcements (Business Daily, Capital FM, HapaKenya, Khusoko). Alternative coding for Equity (2016) used in Table 5.

**Identification strategy and estimation**

The estimand is the average treatment effect on the treated (ATT) of launching mobile credit, overall and by year since launch. Identification rests on two assumptions. The first is parallel trends: in the absence of adoption, the profitability of adopting banks would have evolved like that of banks not yet treated. The second is no anticipation: profitability does not respond before launch. Year fixed effects absorb all shocks common to listed banks, including the interest rate cap, IFRS 9, COVID-19, and changes in the policy rate and exchange rate. Bank fixed effects absorb time-invariant differences in size, ownership, and business model. The main threat is that adoption timing is correlated with bank-specific trends. We assess this by testing for pre-adoption differences in event time, using a placebo that shifts launch dates two years earlier, and by allowing bank-specific linear trends.

**Benchmark TWFE.** We first estimate the conventional two-way fixed-effects model

$$ROaE_{it} = \alpha_i + \lambda_t + \beta D_{it} + \varepsilon_{it}, \quad (1)$$

where $\alpha_i$ and $\lambda_t$ are bank and year fixed effects. Under staggered adoption and heterogeneous effects, $\beta$ is a weighted average of all two-by-two comparisons. Some of these

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use already-treated banks as controls and can receive negative weights (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfœuille, 2020). In our panel this is a real concern, because the 2014–2015 adopters serve as "controls" for the 2018–2024 adopters throughout the period.

**Callaway–Sant'Anna (CS) estimator.** Our preferred estimator computes group-time effects for each adoption cohort g and year t,

$$ATT(g,t) = E[Y_t - Y_{g-1} \mid G = g] - E[Y_t - Y_{g-1} \mid G > \max(t, g-1)], \quad (2)$$

using as controls only banks that are never treated or not yet treated at t, with the year before launch as the base period (Callaway & Sant'Anna, 2021). The ATT(g,t) are aggregated into an overall effect (post-launch cells weighted by cohort size) and into an event-study profile θ(e) for event times e = t − g from −4 to +4. Co-operative Bank (cohort 2014) has no pre-adoption year in the panel and therefore does not contribute to the CS estimates.

**Sun–Abraham (SA) estimator.** As a second heterogeneity-robust estimator we fit a saturated regression of ROaE on bank and year fixed effects and cohort-by-relative-period interactions, using the never-treated bank (DTB) and the last-treated cohort (SCBK, 2024) as controls. The sample is truncated before 2024, and the coefficients are aggregated with cohort-share weights (Sun & Abraham, 2021).

**Inference with few clusters**

With ten banks, cluster-robust standard errors are unreliable and t-tests over-reject (Cameron et al., 2008). We therefore report two procedures that remain valid with few clusters. The first is the wild cluster restricted bootstrap with Webb six-point weights and 9,999 replications, which imposes the null hypothesis when generating bootstrap samples (MacKinnon & Webb, 2018; Webb, 2023). The second is Fisher randomisation inference (RI), which re-assigns the observed vector of adoption years across banks 20,000 times (5,000 for the CS estimator) and compares the estimate with its permutation distribution (Young, 2019). RI tests the sharp null of no effect for any bank and does not rely on asymptotic approximations. CR1 cluster-robust standard errors are reported for reference only.

**Robustness and sensitivity**

We re-estimate the models (i) with the alternative ROaE values from earlier Cytonn releases; (ii) with Equity's launch dated 2016; (iii) excluding NCBA (merger), HF Group (persistent losses), Co-operative Bank (no pre-period) and DTB (the only never-treated bank); (iv) excluding the COVID-19 year 2020; (v) with bank-specific linear trends; (vi) with a placebo that shifts every launch two years earlier, estimated on pre-launch observations only; and (vii) leaving out one bank at a time. We also test for heterogeneity between early (2014–2015) and later adopters.

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## RESULTS

### Descriptive statistics

Table 2 reports summary statistics. Mean ROaE over 2014–2025 is 17.0%, with wide dispersion: HF Group averaged 1.6% and recorded losses in four consecutive years (2018–2021), while Equity Group averaged 23.8%. NIM averages 7.0%, non-funded income one-third of operating income, and the gross NPL ratio 12.6%. Figure 2 plots mean ROaE by adoption cohort. Early adopters were already the most profitable banks before 2014 and remained so. All cohorts fell together during the rate-cap years and COVID-19 and recovered together after 2021. No cohort's profitability visibly breaks from the others in the years after its launch.

Table 2. Descriptive statistics, ten NSE-listed Kenyan banks

| Variable | Period | N | Mean | SD | Min | Median | Max |
|---|---|---|---|---|---|---|---|
| ROaE (%) | 2014–2025 | 118 | 16.98 | 7.71 | -23.3 | 17.75 | 30.1 |
| Net interest margin (%) | 2017–2025* | 80 | 6.96 | 1.59 | 3.3 | 7.00 | 10.1 |
| Non-funded income / operating income (%) | 2017–2025* | 80 | 34.07 | 7.24 | 21.1 | 32.35 | 60.4 |
| Gross NPL ratio (%) | 2017–2025 | 90 | 12.58 | 4.83 | 5.5 | 11.55 | 27.8 |

Notes: * excludes 2023 (not reported consistently for all banks). ROaE missing for I&M and HF Group in 2023.
Source: authors' panel compiled from banks' audited results as reported by Cytonn Investments (Appendix A).

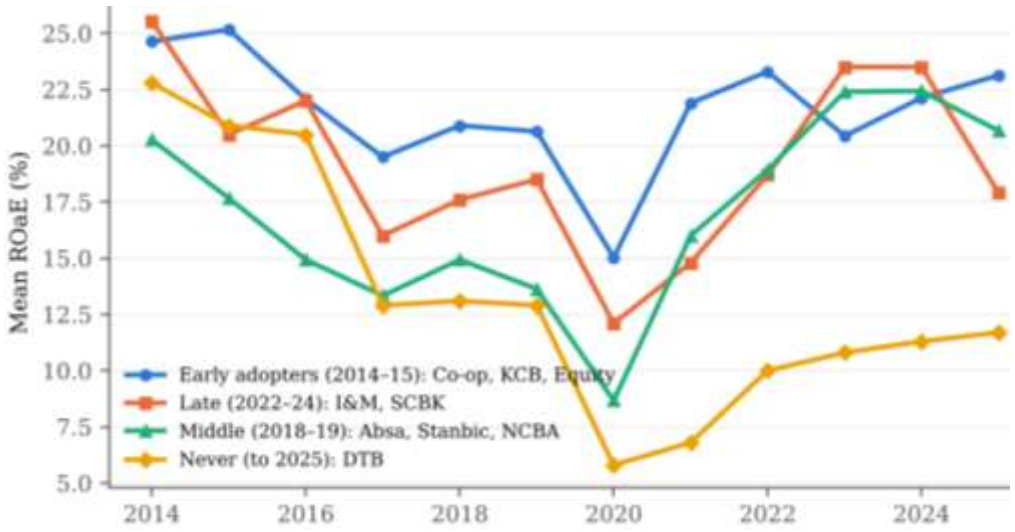



Figure 2. Mean ROaE by adoption cohort, 2014–2025.

Source: authors' panel (Appendix A)

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HF Group (2018 cohort) is omitted from the figure because its persistent losses dominate the cohort mean; it is included in all estimates. In 2023 the late cohort is represented by SCBK only because I&M's value is unavailable.

**Average effect of mobile credit on profitability (H1)**

Table 3 presents the main estimates. The TWFE coefficient is 0.06 percentage points, with a CR1 standard error of 2.64. Both the wild bootstrap and randomisation inference give p-values of about 0.98. The heterogeneity-robust CS overall ATT is similar, at +0.21 points (RI p = 0.90). The CS estimate for the pre-period is −0.22 points (RI p = 0.91), which supports the parallel-trends assumption. The confidence interval implied by the TWFE estimate, roughly −5.1 to +5.2 points, excludes effects as large as a third of the mean ROaE. H1 is not supported: launching mobile credit did not measurably raise the profitability of listed banks relative to banks that had not yet launched.

Table 3. Effect of launching mobile digital credit on ROaE (percentage points)

| | (1) TWFE | (2) CS overall ATT | (3) CS pre-period placebo | (4) CS, excl. HF Group |
|---|---|---|---|---|
| Estimate | 0.06 | 0.21 | −0.22 | 1.39 |
| CR1 cluster s.e. | (2.64) | — | — | — |
| Wild cluster bootstrap p | 0.983 | — | — | — |
| Randomisation inference p | 0.978 | 0.896 | 0.913 | 0.289 |
| 95% CI (CR1) | [−5.11, 5.22] | — | — | — |
| Bank and year FE / controls | Yes / — | Never- & not-yet-treated | Never- & not-yet-treated | Never- & not-yet-treated |
| Banks; bank-years | 10; 118 | 9; 106 | 9; 106 | 8; 95 |

Notes: TWFE = equation (1). CS = Callaway–Sant'Anna (2021) group-time ATTs aggregated over post-launch event times 0–4 (column 2) or pre-launch event times −4 to −2 (column 3), weighted by cohort size; base period e = −1. Co-operative Bank (cohort 2014) has no pre-period and does not enter CS. Wild cluster restricted bootstrap with Webb weights, 9,999 replications. Randomisation inference permutes adoption years across banks (20,000 draws for TWFE; 5,000 for CS; 3,000 for column 4).

**Dynamics: adjustment cost or sustained gain? (H2)**

Figure 3 plots the event-study estimates. The pre-launch coefficients from both estimators are small and statistically indistinguishable from zero (CS: −0.88, −0.21 and +0.43

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points at e = −4, −3 and −2), so adopting banks were not on different profitability paths before launch. After launch, the CS estimates are negative for three years (−0.70, −1.59 and −1.25 points at e = 0, 1 and 2) and then turn positive (+1.50 at e = 3 and +3.70 at e = 4; RI p = 0.57 and 0.18). The Sun–Abraham estimates trace the same shape (+2.30 and +4.30 at e = 3 and 4). This J-shaped profile matches the adjustment-cost account in H2 and the lags documented for internet banking (Hernando & Nieto, 2007). However, none of the post-launch coefficients is individually significant, and the later event times are identified from fewer cohorts. The evidence is therefore consistent with H2 but does not establish a lasting gain.

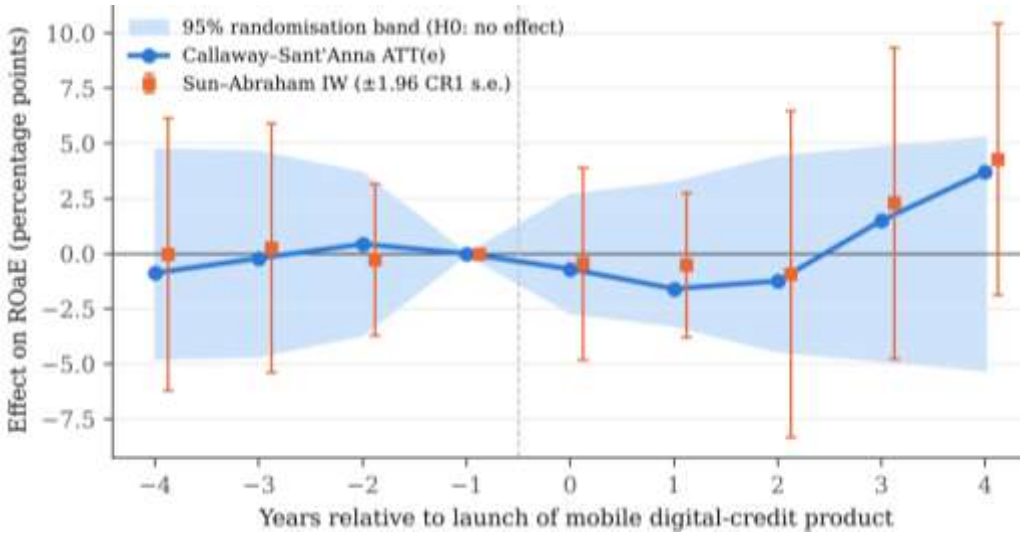



Figure 3. Event-study estimates of the effect of launching mobile digital credit on ROaE.

Blue: Callaway–Sant'Anna ATT by event time, never- and not-yet-treated controls, base period e = −1. The shaded band is the 95% randomisation band under the sharp null of no effect (5,000 permutations of adoption dates). Orange: Sun–Abraham interaction-weighted estimates with ±1.96 CR1 standard errors (controls: DTB and SCBK; sample 2014–2023).

**Mechanisms (H3)**

Table 4 reports the mechanism outcomes. The TWFE estimates appear to support H3a and H3c: NIM rises by 0.70 points (RI p = 0.04; wild bootstrap p = 0.07) and the gross NPL ratio falls by 3.3 points (RI p = 0.04; wild bootstrap p = 0.08). These results disappear when heterogeneity-robust estimators are used. The CS effect on NIM is +0.28 points (RI p = 0.47), and the CS effect on the NPL ratio reverses sign to +1.71 points (RI p = 0.35). The non-funded income share does not respond under either estimator (TWFE +1.58, CS +1.75; both p > 0.5).

The divergence has a clear source. In the 2017–2025 mechanism sample, the 2014–2015 adopters (Co-op, KCB and Equity) are treated throughout and act as implicit controls in TWFE.

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These banks' NPL ratios rose sharply after 2017, especially KCB's and Co-op's. TWFE subtracts that deterioration from the later adopters' outcomes and produces a spurious "improvement" in asset quality. This is the forbidden comparison described by Goodman-Bacon (2021). The CS estimator excludes such comparisons by construction. We therefore conclude that there is no robust evidence for H3 among listed banks, and that TWFE-based mechanism findings in this literature should be treated with caution.

Table 4. Mechanisms: TWFE versus heterogeneity-robust (CS) estimates, 2017–2025

| Outcome | TWFE β (s.e.) | WCB p | RI p | CS ATT | RI p | CS pre-period | RI p | N |
|---|---|---|---|---|---|---|---|---|
| NIM (%) | 0.70 (0.31) | 0.072 | 0.042 | 0.28 | 0.47 | −0.68 | 0.15 | 80 |
| Non-funded income share (%) | 1.58 (3.99) | 0.857 | 0.555 | 1.75 | 0.746 | 4.20 | 0.134 | 80 |
| Gross NPL ratio (%) | −3.33 (1.81) | 0.079 | 0.044 | 1.71 | 0.352 | 3.44 | 0.097 | 90 |

Notes: TWFE with bank and year FE; WCB = wild cluster restricted bootstrap (Webb weights, 9,999 draws); RI = randomisation inference (10,000 draws for TWFE; 3,000 for CS). CS overall ATT aggregates event times 0–3; CS pre-period averages event times −3 and −2; base period e = −1. In 2017–2025 the 2014–2015 adopters are always treated and serve as implicit controls in TWFE but not in CS.

**Heterogeneity (H4)**

Allowing separate effects for early (2014–2015) and later adopters gives +1.52 points (CR1 s.e. 2.16) for early adopters and −0.11 points (2.90) for later ones. The direction is consistent with H4, since the earliest movers are the largest banks with the deepest deposit and mobile-money bases, but the difference is far from significant. H4 is not supported at conventional levels.

**ROBUSTNESS**

Table 5 shows that the null average effect is stable. Using alternative ROaE values from earlier Cytonn releases (0.02 points), dating Equity's launch to 2016 (0.26), excluding NCBA (−0.27), Co-operative Bank (0.14) or DTB (−1.27), or dropping 2020 (0.76) leaves the estimate small and insignificant, with all bootstrap and RI p-values above 0.5. Excluding HF Group, whose losses after its 2018 launch reflect its mortgage book rather than digital lending, raises the TWFE estimate to 2.10 points ($p \approx 0.21$–$0.24$) and the CS estimate to 1.39 points (RI $p = 0.29$). Here the J-shaped profile becomes sharper (+4.42 points at $e = 4$, RI $p = 0.09$). Adding bank-specific trends gives −2.67 (CR1 s.e. 2.37). The two-year placebo is insignificant (1.99,

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wild bootstrap p = 0.44). Leave-one-out estimates range from −1.27 (without DTB) to +2.10 (without HF), so no single bank drives the result.

Table 5. Robustness of the TWFE estimate of the effect on ROaE

| Specification | β | CR1 s.e. | Wild bootstrap p | RI p | N |
|---|---|---|---|---|---|
| Baseline (Table 3, col. 1) | 0.06 | (2.64) | 0.983 | 0.978 | 118 |
| 1. Alternative ROaE values (earlier releases) | 0.02 | (2.72) | 0.994 | 0.991 | 118 |
| 2. Equity launch dated 2016 | 0.26 | (2.50) | 0.924 | 0.898 | 118 |
| 3. Excluding NCBA (2019 merger) | −0.27 | (3.10) | 0.938 | 0.911 | 106 |
| 4. Excluding HF Group | 2.10 | (1.77) | 0.238 | 0.212 | 107 |
| 5. Excluding Co-operative Bank | 0.14 | (2.87) | 0.963 | 0.952 | 106 |
| 6. Excluding DTB (never-treated) | −1.27 | (2.37) | 0.692 | 0.571 | 106 |
| 7. Excluding 2020 (COVID-19) | 0.76 | (2.34) | 0.751 | 0.686 | 108 |
| 8. Bank-specific linear trends | −2.67 | (2.37) | — | — | 118 |
| 9. Placebo: launch shifted 2 years earlier (pre-launch obs.) | 1.99 | (2.00) | 0.441 | — | 50 |
| 10. Leave-one-bank-out, range | −1.27 to 2.10 | — | — | — | 105–107 |
| 11. Early adopters (2014–15) | 1.52 | (2.16) | — | — | 118 |
| Later adopters (2018–24) | −0.11 | (2.90) | — | — | 118 |

Notes: All specifications include bank and year fixed effects. Bootstrap and RI as in Table 3. Row 9 restricts the sample to observations before each bank's actual launch and assigns a fictitious launch two years earlier. Rows 11 estimate separate effects for the two groups in one regression.

## DISCUSSION

The evidence points to one main conclusion. Among Kenya's listed banks, mobile digital credit has become a competitive necessity rather than a source of excess profitability. Every listed bank except one adopted it by 2024. Early adopters were already the most profitable banks and remained so, but launching the product did not shift any bank's profitability trajectory relative to banks that had not yet launched. This is what the diffusion and competition arguments predict. When the core technology is accessible to all incumbents through the mobile-money rail and vendor platforms, the act of adoption cannot be a lasting source of

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advantage (Rogers, 2003; Vives, 2019; Stulz, 2019). It is also consistent with the dynamic-capability view that returns accrue to complementary resources, such as data, scoring models, customer franchises and risk processes, rather than to the channel itself (Teece et al., 1997; Vahter & Vadi, 2024).

The J-shaped dynamics deserve attention, even though they are imprecise. A shortfall of one to one and a half points in the first three years, followed by gains in years three and four, matches the adjustment-cost account developed in the original framing of this research and documented for earlier banking technologies (Hernando & Nieto, 2007). If this pattern holds in larger samples, short-window studies, which dominate the Kenyan literature, will tend to understate or misstate the payoff to innovation. Banks and investors should evaluate digital credit over horizons of at least four to five years.

The mechanism results carry a methodological lesson. Conventional TWFE regressions would have reported statistically significant improvements in margins and asset quality. Both disappear under estimators that avoid comparisons with already-treated banks. Because nearly all of Kenya's large banks adopted mobile channels early, TWFE regressions on Kenyan bank panels are especially exposed to this bias. Reported positive associations in the existing literature may partly reflect it, alongside the confounding of innovation with bank growth.

For policy, the results caution against expecting digital credit to strengthen bank balance sheets by itself. The sector's NPL ratio rose throughout the period in which mobile credit spread, and our estimates do not attribute that deterioration to bank-led digital lending. Supervisors should continue to treat digital lending as a conduct and operational-risk issue, an emphasis visible in CBK's recent innovation surveys (CBK, 2026c), rather than as a profitability buffer. Managers should treat mobile credit as table stakes and invest in the complementary capabilities that are more likely to differentiate performance.

## LIMITATIONS

Several limitations qualify these conclusions. First, with ten banks and six adoption cohorts, statistical power is limited. The design can rule out large effects but cannot detect effects of one or two percentage points. Second, the bank-level ratios come from a secondary compilation of audited statements, with occasional revisions across releases. We mitigate this with a documented source hierarchy, internal recomputation for recent years and robustness to alternative values, but primary statements should be used to confirm the series. Third, the treatment is binary and captures the launch of a product, not its scale. Disclosed digital loan volumes, available for only a few banks, would allow an intensity design. Fourth, group-level ratios for holding companies include regional subsidiaries. Finally, bank-level data before 2014

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were not available on a comparable basis, so the earliest adopters contribute little to identifying pre-trends.

## CONCLUSION

This study examined whether financial innovation-measured by the introduction of credit delivered via mobile phones- improved the performance of commercial banks listed on the Nairobi Securities Exchange, and whether any effect was immediate, delayed, or temporary. Over the sixteen years from 2010 to 2025, Kenyan banking shifted from branches to phones: mobile-money agents increased twelve-fold, while sector pre-tax ROA fell from 4.4% to 3.7% and the gross NPL ratio rose from 6.3% to 16.0%. Against this backdrop, the study draws five conclusions.

First, launching mobile credit did not raise the profitability of listed banks on average (H1 not supported). The heterogeneity-robust estimate of the effect on ROaE is +0.21 percentage points, and the evidence rules out effects larger than about five percentage points in either direction. Second, the effect evolves over time in a way consistent with an adjustment-cost path (H2): profitability dips slightly in the three years after launch and turns positive in years three and four. This later gain is not statistically significant, so the payoff to digital credit, if it exists, is delayed rather than immediate. Third, there is no robust evidence that mobile credit works through net interest margins, fee income or asset quality (H3 not supported). The apparent margin and asset-quality gains under conventional TWFE regressions are artefacts of comparing later adopters with banks that had already adopted. Fourth, early adopters, which are also the largest banks, show somewhat larger effects than later adopters, but the difference is not significant (H4 not supported). Fifth, taken together, these results show that in Kenyan banking, mobile credit has become a competitive necessity, adopted by nine of ten listed banks by 2024, rather than a durable source of superior returns.

**Theoretical contribution.** The findings support diffusion and competition accounts of financial innovation over Schumpeterian accounts of lasting innovator rents. When a technology is available to all incumbents via a shared mobile-money infrastructure, adopting it cannot, by itself, produce lasting advantage. Consistent with the resource-based and dynamic-capability views, any durable performance gains must stem from complementary capabilities, such as credit-scoring data, risk processes and customer franchises, that rivals cannot easily replicate.

**Methodological contribution.** The study shows that the estimator matters for the conclusion. Conventional TWFE regressions, the standard tool in the Kenyan literature, would have reported statistically significant improvements in margins and loan quality, but these effects disappear under heterogeneity-robust estimators. Future studies of staggered innovation in

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banking should therefore use cohort-based difference-in-differences estimators and inference procedures valid with few clusters.

**Implications for practice and policy.** For bank boards and managers, the evidence suggests treating mobile credit as a baseline requirement and assessing its returns over horizons of at least four to five years. Investment should focus on complementary capabilities, above all credit-risk analytics, that determine whether digital lending adds value. For investors and analysts at the NSE, a bank's digital-lending announcement on its own is not a reliable signal of higher future returns on equity. For the Central Bank of Kenya, the results caution against expecting digital credit to strengthen bank balance sheets. Supervision of digital lending should continue to focus on credit, conduct and operational risk, particularly given the sector's rising NPL ratio. For the Capital Markets Authority and listed banks, more consistent disclosure of digital loan volumes, revenues and losses would enable investors and researchers to assess the true contribution of digital channels.

**Future research.** The conclusions are constrained by a sample of ten banks, a binary measure of innovation that captures launch rather than scale, group-level ratios for holding companies, and bank-level data that begin in 2014. Future research should extend the bank panel to all licensed commercial banks and back to 2010 using CBK bank-level returns, measure innovation intensity using disclosed digital loan volumes and revenues, examine the costs of digital lending directly, including technology spending, fraud losses and provisions on digital loans, and compare Kenya with other mobile-money economies such as Tanzania, Uganda and Ghana. Such work would test whether the delayed gains suggested here become statistically detectable when samples are larger and innovation is measured more finely.

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## APPENDICES

### Appendix A. Data sources and construction

Table A1 lists the source of the ROaE values for each fiscal year. NIM, non-funded income share and gross NPL ratios for 2017–2025 are taken from the per-bank performance and asset-quality tables of the corresponding Cytonn FY reports. FY2023 NIM and NFI are omitted because they are not reported for all banks on a consistent basis. Ratios for holding companies (Equity Group, KCB Group, I&M Group, NCBA Group, Stanbic Holdings, HF Group) are group-level. Alternative values reported in other Cytonn releases are retained in the replication file (column roae_alt) and used in Table 5, row 1. The full bank-year panel with a source note for each year, the adoption-date evidence and the sector series are provided as supplementary files.

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**Table A1. Sources of ROaE and sector data by year**

| Year(s) | Source |
|---|---|
| 2014 | Cytonn FY2015 Banking Report (FY2014 comparatives) |
| 2015–2016 | Cytonn Q3'2017 Banking Sector Report, bank financial statement extracts |
| 2017–2018 | Cytonn FY2018 Banking Sector Report, bank financial statement extracts |
| 2019 | Cytonn Kenya Listed Banks FY2019 Report |
| 2020 | Cytonn FY2020 Report; Absa and Co-op from FY2024 full-report extracts |
| 2021–2022 | Cytonn Kenya Listed Banks FY2021 and FY2022 Reports |
| 2023 | Cytonn FY2024 full-report extracts (I&M and HF not reported) |
| 2024–2025 | Cytonn Kenya Listed Banks FY2024 and FY2025 Reports |
| Sector, 2010–2025 | CBK Bank Supervision Annual Reports 2011–2025; CBK NPS mobile payments statistics |

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