IRJEMS International Research Journal of Economics and Management Studies
ISSN: 2583 – 5238 / Volume 5 Issue 11 November 2026 / Pg. No: 117-132
Paper Id: IRJEMS-V5I11P113, Doi: 10.56472/25835238/IRJEMS-V5I11P113

Original Article

Digital Payments, Mobile Money and National Innovation in Developing Economies: Standing Differences Rather Than Within-Country Change

[1]Afina Hasya
[1]Department of Management, Faculty of Economics and Business, Universitas Diponegoro, Indonesia.

Received Date: 25 October 2026 Revised Date: 17 November 2026 Accepted Date: 18 November 2026 Published Date: 20 November 2026

Abstract: *Digital payments and mobile money are often expected to support innovation in developing economies. This study tests that expectation with annual mobile money data from the IMF Financial Access Survey (2004–2025) and four Global Findex waves (2014–2024), relating them to resident patents, trademarks, high-technology exports, new business density and Global Innovation Index scores. Within countries, growth in registered mobile money accounts shows no detectable association with patents or trademarks; per doubling of accounts, the upper 95% bounds are 10–20% more patents and 6–17% more trademarks. Across countries, wider digital payment use goes with more patents and new firms, but these gradients were already present in 2008–2011 outcome levels and disappear once those levels are controlled. The study also shows that Indonesia's mobile money series in the Financial Access Survey counts all electronic money instruments, cards included. Payment infrastructure should therefore be judged by its payment and inclusion aims rather than by national innovation outcomes.*

Keywords: *Developing Economies, Digital Payments, Financial Inclusion, Innovation, Mobile Money, Patents.*

I. INTRODUCTION

Indonesia's QRIS and BI-FAST, India's UPI and the mobile money systems that now cover much of Sub-Saharan Africa represent large public and private investment in digital payment infrastructure. The case for that investment rests mainly on cheaper transactions and wider access. Bank Indonesia's payment system blueprints, for example, place QRIS and BI-FAST at the center of a digitalization strategy whose stated aims are stability, growth and inclusion [1, 2], and the 2030 blueprint reports 32.7 million QRIS merchants, 30.2 million of them micro, small and medium enterprises [2]. Indonesia's national financial inclusion strategy, set by Presidential Regulation No. 114/2020, lists digital financial services among its priorities [3]. A growing literature goes further and treats digital finance as a driver of innovation: studies of China find that digital financial inclusion is associated with more firm and regional innovation [4, 5], and firm-level evidence from Sub-Saharan Africa leads Osei-Tutu and Taylor [6] to suggest that policy could stimulate innovation by encouraging firms to adopt mobile money. This paper asks whether that expectation holds at the level that matters for national policy. Is the spread of digital payments and mobile money associated with national innovation outputs in developing economies, and does any association reflect change within countries or standing differences between them?

Theory gives reasons to expect a link. Finance relaxes the constraints that keep firms from funding risky, long-horizon projects. Cross-country evidence links financial development to growth and technological progress [7, 8], with equity markets in particular favoring innovation-intensive industries [9]. Financial constraints are a binding obstacle to innovation in emerging economies in particular [10]. Mobile money and digital payments reach households and small firms that banks never served, and they have improved consumption smoothing and saving [11–13]. But they are payment technologies. Whether they also relax the financing constraints on innovation is an empirical question.

The existing evidence answers it at a different level or with a different measure. The Chinese studies are sub-national and rely on the Peking University Digital Financial Inclusion Index, which bundles payments with credit, insurance and investment services [4, 5, 14]. The closest cross-country study links fintech and big-tech credit, not payments, to innovation in 89 countries [15]. Firm-level studies in Africa find that mobile money adoption is associated with firm innovation [6] and with higher productivity mainly when combined with traditional financial services [16]. Other cross-country work studies entrepreneurship and fintech start-ups [17, 18], the link between inclusion and growth [19] or the determinants of inclusion [20]. We are not aware of a study that relates internationally comparable payment and mobile money measures to national innovation outputs in developing economies and separates levels from changes.

This is an open access article under the CC BY-NC-ND license (https://creativecommons.org/licenses/by-nc-nd/2.0/)

We combine two sources: the IMF Financial Access Survey (FAS), which reports registered mobile money accounts annually for 75 developing economies, and the Global Findex, which measures digital payment use in about 90 developing economies in four waves (2014–2024). Outcomes are resident patents and trademarks per capita, high-technology exports, new business density and Global Innovation Index (GII) scores.

The main result concerns change within countries. With annual mobile money data, we find no detectable association with patents or trademarks across lags, estimators, control sets and alternative mobile money measures (Sections V-A and VI). Per doubling of registered accounts, the upper 95% bounds are 10–20% more patents and 6–17% more trademarks; the narrower figures come from log-linear models with country and year effects, the wider ones from first differences, country-specific trends and Poisson models. The bounds describe growth after launch, not the launch itself. The regressor has documented problems: Indonesia's FAS series counts all electronic money instruments, cards included, and other economies report inflated or broken series. Dropping these economies leaves the estimates insignificant, although the smallest screened samples (13 to 18 countries) give much wider bounds, and active accounts give negative rather than positive coefficients.

Across countries, a 10 percentage point higher share of adults using digital payments is associated with about 0.18 log points more resident patents per capita, which survives a wild cluster bootstrap, a Holm correction, region fixed effects and the exclusion of China and India. It disappears once each country's patent level in 2008–2011 is controlled (from +0.018 to −0.002), and the gradient for new business density behaves the same way (from +0.055 to zero). Both were already present before 2014; the data cannot say what produced them. The trademark gradient among reporters is negative but likewise indistinguishable from zero once earlier levels are controlled, and the GII association does not survive a multiple-testing correction (Section V-C).

On mechanisms, private credit is flat within countries as mobile money grows, a credit-complementarity interaction is insignificant, and the negative within-country association of registered accounts with new business density does not appear for active accounts on the same sample. An instrument based on pre-period infrastructure is too weak to use [21, 22], so all estimates are associational.

This paper relates payment-based digital inclusion, rather than bundled indices or fintech credit, to national innovation outputs across developing economies, reports explicit bounds on the within-country association instead of a bare null, and shows that countries with wider payment use were already patenting more, and registering more new firms, before 2012. A third, smaller contribution is to measurement: the FAS mobile money series is not comparable across all reporting countries, and we show how much this matters for the estimates. The first two results qualify policy arguments that treat payment infrastructure as an innovation instrument. They do not overturn the Chinese and firm-level evidence, which measures different constructs at a different level; Section VII discusses how the results fit together.

II. LITERATURE AND HYPOTHESES

A) Finance and Innovation

Endogenous growth models place financial intermediation at the center of technological progress [7, 23, 24]. Firms with better access to external finance patent more, and reforms that expand credit or equity supply raise innovation [25–28]. The type of finance matters. Across countries, Hsu et al. [9] find that equity-market development favors innovation in industries that depend on external finance and are high-tech, while credit-market development discourages it, and Kerr and Nanda [30] stress that innovation needs finance willing to bear uncertainty. In emerging economies, financial constraints reduce firms' innovation and slow catch-up with the frontier [10]. Finance and growth are closely linked in cross-country data [29], and financial innovation is itself part of the growth process [8].

B) Financial Inclusion and the Digital Turn

Financial inclusion extends access to households and firms that formal systems leave out [31–33]. It has several margins (access, usage and quality), and composite indices weight them differently [34, 35]. The Global Findex made cross-country measurement possible [36–38]. Financial development is associated with lower inequality and poverty [39], inclusion is less firmly linked to growth [19], and financial literacy raises inclusion [20].

Mobile phones lowered the cost of reaching the unbanked [40], and mobile money became the signature financial innovation of Africa and South Asia [13, 41]. In Kenya, mobile money cut transaction costs, improved risk sharing and moved households out of poverty [11, 12]. Payment instruments also interact with firm finance: in a general equilibrium model calibrated to Kenyan firm data, Beck et al. [46] find that more productive entrepreneurs with access to trade credit are more likely to adopt mobile money to pay their suppliers. Digital finance and fintech have changed how financial services are produced and delivered [42, 43]. At the macro level, digital financial innovation, including mobile payments and mobile banking, is associated with financial system development in Eastern and Southern Africa [44], and recent frameworks stress that inclusion outcomes depend on several interacting factors, of which technology is only one [45].

C) Digital Finance and Innovation

Most evidence that digital finance raises innovation comes from China and uses the Peking University index. Studies report positive associations with firm technological innovation, urban innovation, supply-chain-financed innovation and green innovation, usually through relaxed financing constraints [4, 5, 14, 47], with related work on digital finance, manufacturing R&D and patent output in emerging economies [48] and on environmental outcomes [49]. The Chinese work is sub-national, and its index combines payments with credit, insurance and investment.

Cross-country and firm-level evidence outside China is thinner. Pal et al. [15] relate fintech and big-tech credit to innovation in 89 countries over 2013–2019 and find that the association depends on institutional quality. Osei-Tutu and Taylor [6] use firm surveys from 16 Sub-Saharan African countries and find that mobile money adoption is associated with firm innovation, through better liquidity and access to trade credit and bank loans. Konte and Tetteh [16] find no robust direct link between mobile money and firm productivity in 14 Sub-Saharan African countries, but a positive link when mobile money is used alongside traditional financial services. Other studies link mobile money to entrepreneurship and business performance in East Africa [50, 51], to participation in global value chains [52] and to female business activity [53], and relate venture capital and credit to fintech start-up formation across countries [17], financial inclusion to entrepreneurial activity across countries [18] and digital financial inclusion to household entrepreneurship [54]. Work on measurement and trust continues [35, 55].

Two gaps follow. None of these studies relates payment-based inclusion measures to national innovation outputs across developing economies, and none separates the cross-country association from within-country change. National outputs matter in their own right: firm-level innovation in developing economies is mostly adoption of existing products and processes [56], so a firm-level association need not show up in national patenting.

D) Hypotheses

We state the hypotheses together with how they arose. Our initial Findex analysis, which took two-way fixed effects as the main specification, found positive pooled associations that vanished within countries. That pattern led us to treat levels and changes as distinct questions. We then fixed the annual FAS design (regressor, lag, controls, outcomes) in a specification log before estimating it, to test the within-country question with more variation. The comparison of reporters and non-reporters, and the controls for pre-period outcome levels, were added after the main analysis.

The theoretical case for distinguishing levels from changes is straightforward. Countries with wider digital payment use tend to have deeper credit markets, more human capital and a larger research base, and these inputs produce innovation. Standard controls measure these inputs imperfectly, so a pooled regression can attribute some of their effect to digital payments. Within a country, by contrast, the spread of a payment technology need not move those inputs, and a payment rail is not itself a source of risk-bearing finance.

- H1a (between): Across developing economies, digital payment use is positively correlated with national innovation outputs, and the correlation reflects standing differences between countries rather than recent change.
- H1b (within): Within a country over time, growth in digital payments and mobile money has no systematic association with national innovation outputs.
- H2 (credit complementarity): Where private credit is deeper, lenders may be able to use payment records in credit decisions, and mobile money appears to work alongside traditional financial services at the firm level [16], so the within-country association should be more positive. Because credit-market development can also discourage innovation in high-tech industries [9], we expect the more positive association to be small.
- H3 (digital versus conventional finance): Conventional banking and mobile money relate differently to formal firm creation. One argument predicts that mobile money relates less positively, because it lowers the cost of transacting informally and does not provide credit; another predicts the reverse, because digital payment trails can encourage formalization. We test for a difference without signing it.

III. DATA

A) Sources and Measures

The sample consists of developing economies under the current World Bank income classification (low-, lower-middle- and upper-middle-income). Regressors come from two sources.

The IMF Financial Access Survey (FAS) supplies annual supply-side series from 2004 to 2025: registered and active mobile money accounts per 1,000 adults, the number and value (as a share of GDP) of mobile money transactions, active agents per 100,000 adults, internet and mobile banking transactions, and conventional banking series (deposit accounts per 1,000 adults, branches and ATMs per 100,000 adults). The main FAS regressor is the log of one plus registered mobile money accounts per 1,000 adults. Appendix E reports coverage.

The Global Findex supplies demand-side measures for four waves (2014, 2017, 2021, 2024). The core regressor is the share of adults who made or received a digital payment, in percentage points; account ownership, mobile money account ownership and the share making digital payments are alternatives.

The two regressors measure different things. Findex digital payments cover any digital payment, including card payments and government transfers. FAS registered accounts measure the reach of one technology and include dormant sign-ups. The FAS defines a registered mobile money account as one held with a resident provider and "primarily accessed using a mobile phone", and an active account as one used in the past 90 days [57]. Not every country reports on that basis. Indonesia's FAS series matches, within rounding, in every year from 2020 to 2025, Bank Indonesia's count of all electronic money instruments in circulation (1,065 million at the end of 2025) [58]. That count includes chip-based cards (12% of instruments) and instruments recorded by issuers as unregistered (77%), so it is a count of e-money instruments rather than of mobile money accounts. Bank Indonesia's count of instruments recorded as registered alone implies about 1,130 per 1,000 adults in 2025, against 4,923 in the FAS (both use the 216 million adults implied by the FAS series). We verified the match only for 2020–2025, the years in the Bank Indonesia table, and Indonesia reports no active-account series to the FAS. We cannot check other countries against national statistics in the same way, but some show related symptoms (Appendix E). Eight economies have a median ratio of registered to active accounts above 5, against a median of 2.5 across the 65 economies that report both; five report identical registered and active figures; fifteen show a break in at least one mobile money series; and ten, Indonesia among them, report no active series and cannot be screened by the ratio. Section VI drops Indonesia and the eight high-ratio economies, drops the economies with breaks and also uses active accounts, transaction counts, transaction value and agents. Neither regressor measures credit, savings or insurance, which is why we speak of digital payments and mobile money rather than digital financial inclusion in general.

Outcomes and controls come from the World Development Indicators and WIPO. Outcomes are resident patent applications and resident trademark applications per million population, entered as the log of one plus the rate; high-technology exports as a share of manufactured exports; new business density, the number of newly registered limited liability companies per 1,000 people aged 15–64, which does not count sole proprietorships; and GII scores and sub-indices. Patent counts are an imperfect indicator of innovation [59], and in developing economies they capture frontier rather than adoptive innovation [56], so we pair them with the other outcomes. Controls are log GDP per capita, private credit to GDP, internet users, tertiary enrollment, and the Rule of Law and Regulatory Quality indicators (the Findex models omit internet users). Findex outcomes and controls are matched to the wave year or the nearest year within two years. Raw series, download scripts and file hashes are recorded in the replication package.

B) The Mobile Money Sample

Only countries whose authorities report mobile money statistics appear in these FAS series, so the mobile money sample has 58 to 75 economies depending on the indicator, concentrated in Sub-Saharan Africa and South and Southeast Asia. We treat unreported values as missing rather than zero. Two consequences follow. A country enters the FAS panel only after its mobile money market starts reporting, and the main specification uses a two-year lag, so within-country estimates describe growth in adoption after launch, not the launch itself. And the reporting economies differ from other developing economies (Table 1): they are poorer, have lower internet use and slightly shallower credit, and start from lower patent and trademark levels.

Table 1: Mobile Money Reporters Versus Non-Reporters, Developing Economies (Country Means)

Variable	Reporters (N=75)	Non-Reporters (N=56)
ln GDP per capita	7.55	8.25
Private credit / GDP (%)	33.1	38.9
Internet users (%)	27.3	45.9
ln resident patents per capita	1.15	2.44
ln resident trademarks per capita	4.54	5.65

Reporters are developing economies with at least one non-missing FAS observation on registered mobile money accounts.
Source: *Authors' calculations*.

Attrition from the full reporter group to the estimation samples is large. Of the 75 reporters, 30 have resident patent data in years when the regressor and all controls are available, and 24 contribute within-country variation. Indonesia is among them. It is richer than the average reporter (log GDP per capita 8.13 against 7.55), has similar credit depth (36% against 34% of GDP, averaged over country-years) and a lower patent rate (4.6 against 5.5 per million).

The two data sources complement each other. FAS is narrow but long. In the main patent estimation sample (176 country-years, 24 countries), the median within-country standard deviation of log mobile money accounts is 1.22, and the

standard deviation left after country and year effects and controls is 0.76. Findex is wide but short: digital payments average 40.6% of adults across the developing sample and rise from about 27% in 2014 to about 50% in 2024 (unweighted country means; Fig. 1). Each country figure comes from a survey of about 1,000 adults, so wave-to-wave changes include sampling error, which biases within-country Findex estimates toward zero.

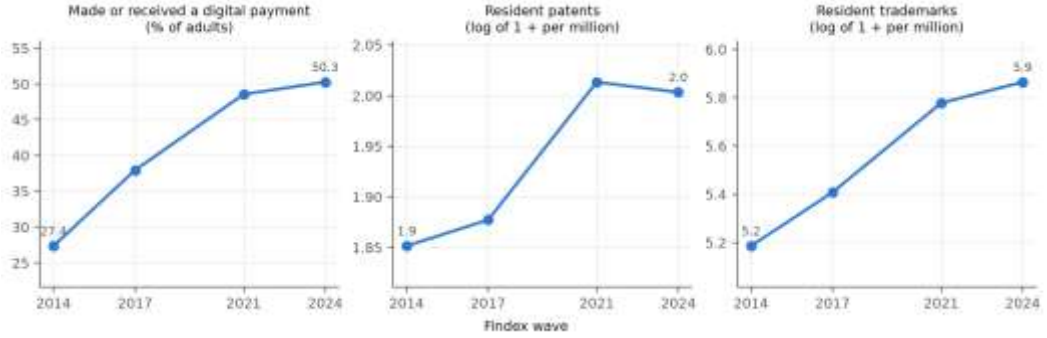


Figure 1. Digital Payment Use, Resident Patents and Resident Trademarks across Findex Waves, Developing Economies (Unweighted Country Means; One Scale Per Panel)

Raw cross-country correlations already show that the two measures behave differently. Findex digital payments correlate with log patents at 0.47, with log trademarks at 0.49, with new business density at 0.51 and with high-technology exports at 0.15. Findex mobile money account ownership correlates with log patents at −0.08, and across FAS country-years, log registered mobile money accounts correlate with log patents at −0.18.

IV. EMPIRICAL STRATEGY

A) Within and Between Estimates

The main FAS model is two-way fixed effects (TWFE):

$$y_{it} = \beta M_{i,t-k} + \gamma' X_{it} + \alpha_i + \tau_t + \varepsilon_{it}$$

Where y_{it} is the outcome for country i in year t, $M_{i,t-k}$ is log mobile money accounts lagged k years and matched by calendar year, X_{it} are controls, and α_i and τ_t are country and year effects. The main lag, $k = 2$, was fixed before estimation on the reasoning that innovation outputs respond with delay and that a lag reduces simultaneity; we report k = 1,2,3. With a continuous regressor, the TWFE coefficient is a weighted average of country-specific slopes and relies on parallel trends in the level of the outcome; if slopes differ across countries, positive and negative slopes can offset [60, 61]; with a continuous treatment, interpreting the TWFE coefficient as an average effect requires a stronger form of parallel trends [61], and related problems arise with binary treatments adopted at different times [62, 63]. We therefore also report first-difference estimates and models with country-specific linear trends.

The outcome is the log of one plus the patent or trademark rate. With zeros and low rates, coefficients on such a variable are not percentage effects and depend on the units of the outcome [64], so we also estimate Poisson pseudo-maximum likelihood (PPML) models on the rates with the same fixed effects, whose coefficients are semi-elasticities. Private credit and internet use may be channels as well as confounders, and income may respond to mobile money as well; we therefore report a reduced control set, a minimal set with log GDP per capita only, and a model without controls.

For the cross-country question, we estimate pooled models with period effects,

$$y_{it} = \beta D_{it} + \gamma' X_{it} + \tau_t + \varepsilon_{it}$$

Where D_{it} is Findex digital payments (or FAS mobile money). This model uses mostly cross-country variation; we call its coefficient the between-country gradient, while noting that it also contains some within-country variation. To test whether the gradient reflects standing differences, we add each country's average log outcome in 2008–2011, before the 2014 Findex wave. This lagged-outcome control absorbs any persistent factor already reflected in earlier outcomes, including any earlier effect of finance itself, so it shows whether the gradient predates the sample period, not why. To test whether it differs between mobile money economies and others, we interact D_{it} and every control with a reporter indicator. For Findex, we also estimate TWFE and a long difference between 2014 and 2024.

B) Dynamics, Moderation and Channels

We estimate local projections [65] of each outcome at horizons h = 0 to 4 on contemporaneous mobile money with country and year effects and controls. To test H2, we interact the lagged regressor with centered private credit and centered internet use in the TWFE model; the four outcomes and two moderators form a family of eight tests. To test H3, we re-estimate

the main model with conventional banking regressors and in a horse race with mobile money. To examine the bank-credit link of the financing channel, we regress private credit on lagged mobile money within countries.

C) Inference

Standard errors are clustered by country [66]. With 24 to 49 clusters in most FAS models, asymptotic tests can over-reject, so we report wild cluster bootstrap p-values with Rademacher weights, the null imposed and 9,999 replications [67–70]. The bootstrap uses the estimation sample of each reported coefficient. In fixed-effects cells, it retains countries with a single observation, which the estimation routine drops; their residuals are zero, so the coefficient and the bootstrap p-value are unchanged. Confidence intervals and the p-values of PPML models use $t(G-1)$ critical values, where G is the number of clusters. For the main within-country estimates, we also report a minimum detectable effect (MDE) at 80% power and a 5% two-sided level, $(c_{0.975} + t_{0.80,\ G-1})\times SE$, with $c_{0.975}$ from either $t(G-1)$ or the bootstrap distribution of the t statistic. We read a precisely estimated insignificant coefficient as informative about the range of plausible effects [71]. We do not have a smallest effect size of interest from comparable studies, because the Chinese and firm-level estimates use different regressors and outcomes, so we report bounds across specifications rather than a test of equivalence.

We apply Holm corrections within families defined by table rows or panels: the outcomes in each row of between-country estimates, the four GII measures, and the eight moderation tests. The same standard applies to positive and negative coefficients.

V. RESULTS

A) Within Countries: Annual Mobile Money Data (H1b)

Table 2 reports the FAS TWFE estimates. At every lag, the coefficients on patents and trademarks are insignificant. At the main lag patents are +0.0138 (p = 0.742; bootstrap p = 0.78) and trademarks +0.0018 (p = 0.961; bootstrap p = 0.96). High-technology exports are positive and marginal at the two-year lag (+0.409, p = 0.093). New business density is negative at the two-year lag (−0.169, p = 0.041), which we return to under H3; with four outcomes, neither survives a Holm correction (adjusted p = 0.28 and 0.17, using analytic p-values; 0.08 for new business with bootstrap p-values).

Table 2: Mobile Money and Innovation Outputs, IMF FAS, Two-Way Fixed Effects (Coefficient on Log Registered Mobile Money Accounts; P-Values In Parentheses)

Outcome	T−1	T−2 (Main)	T−3
ln patents per capita	+0.0363 (0.368)	+0.0138 (0.742)	−0.0574 (0.131)
ln trademarks per capita	−0.0088 (0.810)	+0.0018 (0.961)	−0.0085 (0.802)
High-tech exports (%)	+0.3027 (0.204)	+0.4085 (0.093)	+0.0578 (0.864)
New business density	−0.0706 (0.390)	−0.1688 (0.041)	−0.1662 (0.065)

Country and year fixed effects; controls: log GDP per capita, private credit, internet use, tertiary enrollment, Rule of Law, Regulatory Quality. N at t−2: patents 176 (24 countries), trademarks 195 (27), high-tech 261 (42), new business 259 (41). High-technology exports are under-powered (minimum detectable effect 0.67 percentage points at t−2). Bold marks p < 0.05.
Source: *Authors' calculations.*

Table 3 shows how much these zeros rule out. In the main specification, the 95% confidence interval is [−0.072, +0.100] for patents and [−0.074, +0.078] for trademarks, and the minimum detectable effect is 0.121–0.129 for patents and 0.107–0.117 for trademarks (t and bootstrap critical values). Reduced, minimal, and no controls give intervals of similar or smaller width. Three specifications are less precise. First differences widen the patent interval to [−0.097, +0.186]. Country-specific trends raise the standard errors by a factor of 1.7 for patents and 1.5 for trademarks. PPML estimates, which weight countries with many filings more heavily, are positive for both outcomes and marginal for patents (+0.088, p = 0.089).

Table 3: Bounds on the Within-Country Association, FAS, Mobile Money At T−2 (Coefficient, P-Value And 95% Confidence Interval)

Estimator and Controls	Patents	Trademarks
Log-linear TWFE, full controls (main)	+0.014 (0.742) [−0.072, +0.100]	+0.002 (0.961) [−0.074, +0.078]
Log-linear TWFE, reduced controls	+0.005 (0.893) [−0.067, +0.077]	+0.013 (0.726) [−0.060, +0.085]
Log-linear TWFE, minimal controls	−0.010 (0.772) [−0.077, +0.057]	−0.003 (0.928) [−0.063, +0.058]
Log-linear TWFE, no controls	−0.005 (0.892) [−0.080, +0.070]	−0.013 (0.652) [−0.068, +0.043]
Log-linear TWFE, country-specific trends	−0.018 (0.801) [−0.163, +0.127]	+0.038 (0.512) [−0.079, +0.154]
First differences, full controls	+0.045 (0.519) [−0.097, +0.186]	+0.026 (0.499) [−0.051, +0.102]
PPML, full controls	+0.088 (0.089) [−0.014, +0.190]	+0.099 (0.121) [−0.028, +0.226]
PPML, minimal controls	+0.069 (0.096) [−0.013, +0.151]	+0.062 (0.174) [−0.028, +0.152]

Confidence intervals and PPML p-values use t(G−1). Full controls as in Table 2; reduced: log GDP per capita, private credit, internet use; minimal: log GDP per capita. Countries: patents 24 (full, first differences), 34 (reduced), 37 (minimal, none);

trademarks 27, 42, 45. Minimum detectable effects (80% power, 5% level) in the main specification: patents 0.121 (t) and 0.129 (bootstrap), trademarks 0.107 (t) and 0.117 (bootstrap). PPML coefficients are semi-elasticities of the patent or trademark rate.
Source: *Authors' calculations*.

To translate the bounds, consider a doubling of registered accounts (a change of 0.69 in the log regressor, close to the 0.76 standard deviation of the identifying variation). We convert log points into percentage changes at the median rate in each estimation sample (2.4 patents and 190 trademarks per million). For patents, the upper bounds imply at most about 10% more patents in the main specification, 13% with country trends, 14% under PPML and 20% in first differences. For trademarks, the corresponding figures are 6% in the main specification, 7% in first differences, 11% with country trends and 17% under PPML. The data cannot rule out associations of this size, but they do rule out larger ones for the growth in registered accounts observed in the sample. Section VI shows two ways in which these figures widen: the strictest sample screens (up to 40% for patents and 24% for trademarks, converted at each sample's median rate) and an allowance for attenuation (about 18% for patents). Over the full rise from launch to maturity, which spans several doublings, the bounds would be correspondingly larger.

The local projections in Fig. 2 show no dynamic response for patents or trademarks. The patent response peaks at $h = 1$ (+0.057, p = 0.108) and returns to zero by $h = 3$; the 95% bands cover zero at every horizon, and trademarks are flat throughout. H1b is not rejected for patents and trademarks. For high-technology exports, the response is positive on impact (+0.73 percentage points at $h = 0$, p = 0.022) and insignificant at later horizons except a marginal $h = 2$; with an under-powered outcome and five horizons, we do not read this as evidence of an effect.

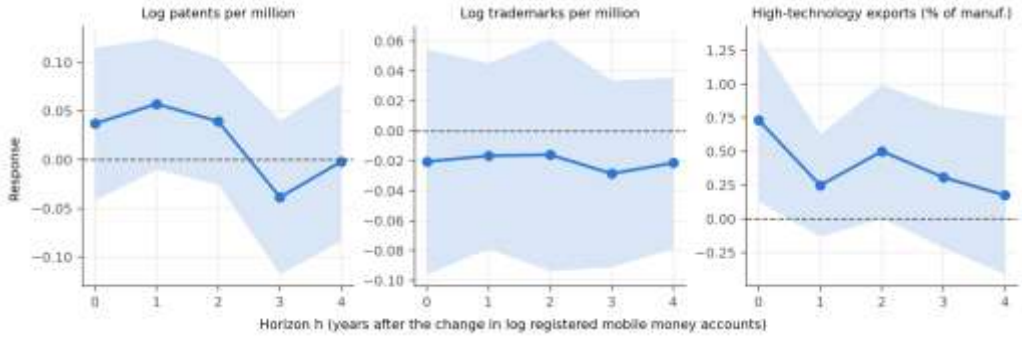


Figure 2. Local Projections of Patents, Trademarks and High-Technology Exports on Mobile Money, Horizons 0–4, FAS, With 95% Bands

B) Within Countries: Findex

The Findex panel gives the same answer with less precision (Table 4, Panel B; Fig. 3). With country and wave effects, trademarks, high-technology exports and new business density are insignificant. The patent coefficient is negative (−0.0105, analytic p = 0.044), and the 2014–2024 long difference agrees in sign (−0.0207, p = 0.012; bootstrap p = 0.017). The estimate is fragile. Its bootstrap p-value is 0.051; the 2017–2024 long difference is insignificant; the lead placebo is marginal, it is close to zero among reporters, and neither it nor the long difference survives a Holm correction across the four outcomes (Table 4 note). With four waves, we read it as reflecting trends that differ across countries in a short panel; it does not indicate harm. Differing trends are a caution for every within-country estimate here; the FAS null is included, hence the trend-adjusted and first-difference bounds.

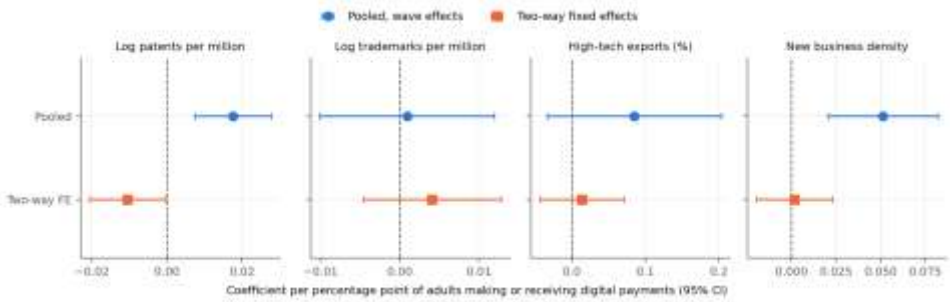


Figure 3. Pooled Versus Two-Way Fixed Effects Coefficients On Digital Payments, Findex, With 95% Confidence Intervals

Table 4: Digital Payments and Innovation Outputs, Findex, All Developing Economies (Coefficient Per Percentage Point; P-Values in Parentheses; Country-Clustered SE)

Outcome	A. Pooled + Wave FE	B. Two-Way FE	C. Long Difference 2014–2024
ln patents per capita	+0.0175 (0.001)	−0.0105 (0.044)	−0.0207 (0.012)
ln trademarks per capita	+0.0009 (0.871)	+0.0041 (0.362)	−0.0003 (0.955)
High-tech exports (%)	+0.0849 (0.164)	+0.0139 (0.636)	+0.0401 (0.484)
New business density	+0.0513 (0.002)	+0.0017 (0.874)	−0.0041 (0.749)

Controls: log GDP per capita, tertiary enrollment, private credit, Rule of Law, Regulatory Quality. N (A / B): patents 206 / 197, trademarks 210 / 196, high-tech 255 / 243, new business 243 / 232. Bold marks p < 0.05. Wild cluster bootstrap p-values: Panel A patents 0.005, new business 0.005; Panel B patents 0.051. Panel C is one observation per country (patents and trademarks: 45 countries, high-tech 52, new business: 50), with heteroskedasticity-robust standard errors and wild bootstrap p-values of 0.017 (patents), 0.959, 0.489 and 0.731. Holm-adjusted p-values for patents across the four outcomes: Panel B 0.18 (analytic), Panel C 0.07 (bootstrap). Panel B patents, further checks: lead placebo −0.0071 (p = 0.095); reporters only −0.0019 (p = 0.608); 2017–2024 long difference −0.0161 (bootstrap p = 0.133).

Source: *Authors' calculations.*

C) Between-Country Gradients (H1a)

In the pooled Findex model (Table 4, Panel A), digital payments are positively associated with patents (+0.0175 per percentage point, bootstrap p = 0.005) and new business density (+0.0513, bootstrap p = 0.005), and both survive a Holm correction across the four outcomes (adjusted p = 0.02). Dropping the 2021 wave, which was fielded by telephone in many economies during the pandemic, leaves both gradients unchanged (+0.0178 and +0.0484, bootstrap p = 0.005 and 0.003). Table 5 tests what lies behind these gradients.

Table 5: Between-Country Gradients (Pooled With Period Effects; P-Values in Parentheses, Wild Cluster Bootstrap P-Values in Brackets)

Specification	Patents	Trademarks	New Business
All developing economies, Findex digital payments			
(1) Baseline	+0.0175 (0.001) [0.005]	+0.0009 (0.871) [0.886]	+0.0513 (0.002) [0.005]
(2) Row 1 with region fixed effects	+0.0206 (<0.001) [0.004]	+0.0087 (0.163) [0.270]	+0.0262 (0.176) [0.306]
(3) Row 1, sample with 2008–2011 outcome data	+0.0184 (<0.001) [0.003]	+0.0056 (0.426) [0.600]	+0.0545 (0.001) [0.006]
(4) Row 3 plus 2008–2011 level of the outcome	−0.0019 (0.667) [0.697]	+0.0030 (0.531) [0.651]	+0.0002 (0.982) [0.981]
Mobile money reporters, Findex digital payments			
(5) Baseline	+0.0042 (0.613) [0.659]	−0.0189 (0.030) [0.048]	+0.0173 (0.353) [0.386]
(6) Row 5, sample with 2008–2011 outcome data	+0.0026 (0.747) [0.778]	−0.0150 (0.033) [0.041]	+0.0203 (0.349) [0.393]
(7) Row 6 plus 2008–2011 level of the outcome	+0.0063 (0.252) [0.426]	−0.0103 (0.070) [0.132]	−0.0030 (0.708) [0.728]
Difference, reporters minus non-reporters (fully interacted)			
(8) Baseline	−0.0137 (0.124) [0.144]	−0.0314 (0.002) [0.005]	−0.0240 (0.235) [0.272]
(9) Row 8 plus digital payments × ln GDP per capita	−0.0129 (0.177) [0.220]	−0.0334 (0.001) [0.003]	−0.0061 (0.802) [0.821]
(10) Row 8, row 3 sample, plus interacted 2008–2011 level	+0.0118 (0.034) [0.080]	−0.0170 (0.017) [0.049]	−0.0044 (0.730) [0.748]
Mobile money reporters, FAS mobile money accounts (t−2)			
(11) Baseline	+0.064 (0.358) [0.371]	−0.178 (0.003) [0.006]	−0.041 (0.850) [0.866]
(12) Row 11, sample with 2008–2011 outcome data	+0.100 (0.353) [0.442]	−0.100 (0.045) [0.062]	−0.035 (0.882) [0.902]
(13) Row 12 plus 2008–2011 level of the outcome	+0.048 (0.271) [0.340]	−0.050 (0.187) [0.272]	−0.014 (0.818) [0.829]
(14) Row 11 without Indonesia	+0.064 (0.388) [0.418]	−0.176 (0.004) [0.012]	+0.038 (0.857) [0.874]
(15) Row 11 without the nine economies flagged in Section III-A	+0.061 (0.384) [0.393]	−0.171 (0.002) [0.009]	+0.107 (0.612) [0.671]
(16) Row 11 with active instead of registered accounts	−0.024 (0.792) [0.825]	−0.180 (0.025) [0.036]	+0.035 (0.884) [0.899]

Rows 1–10 use the Findex controls of Table 4; rows 11–16 use the FAS controls (adding internet users). The 2008–2011 level is each country's mean of the same outcome (log patents, log trademarks or new business density) over 2008–2011. N (clusters), rows 3–4: patents 174 (52), trademarks 163 (51), new business 221 (70); row 5: 108 (37), 116 (40), 148 (51); rows 6–7: 79 (25), 81 (28), 131 (43); row 11: 182 (30), 202 (34), 267 (49); rows 12–13: 149 (19), 147 (22), 235 (41); row 15: 155 (28), 177 (31), 233 (42). Rows 8–10 interact digital payments and all controls (in row 10 also the 2008–2011 level) with a reporter indicator. Holm-adjusted bootstrap p-values across the three outcomes: row 5, patents 0.77, trademarks 0.14, new

business 0.77; row 8, 0.29, 0.014 and 0.29; row 9, 0.44, 0.008 and 0.82; row 10, 0.16, 0.15 and 0.75; row 11, 0.74, 0.017 and 0.87; row 15, 0.79, 0.027 and 0.79; row 16, 1.00, 0.11 and 1.00. Row 16: 116 (22), 142 (26), 194 (39). Bootstrap: Rademacher weights, null imposed, 9,999 replications, same sample as the coefficient. Bold marks bootstrap p < 0.05.
Source: *Authors' calculations.*

The patent gradient is robust to region fixed effects (+0.0206, bootstrap p = 0.004) and to excluding China and India (+0.0171, p = 0.002), but not to earlier patent levels. When each country's average patent rate in 2008–2011 is added, the coefficient on digital payments falls from +0.0184 to −0.0019 (bootstrap p = 0.697) on the same sample (rows 3–4). Countries with wider digital payment use file more patents, but they already filed more patents before 2012, and digital payments carry no information beyond that. The result is consistent with the reading in H1a, which we sharpened after seeing this test. It does not identify the source of the earlier differences, which could include innovation capacity, earlier financial development or reverse causation from innovative economies to faster digital adoption. The new business gradient follows the same pattern. It fails with region fixed effects (+0.0262, bootstrap p = 0.306), and on the sample with earlier data it falls from +0.0545 to +0.0002 once 2008–2011 new business density is controlled (rows 3–4).

Among economies that report mobile money, the patent gradient is close to zero (rows 5–7 and 11–13), and the difference from other developing economies is not significant (row 8, bootstrap p = 0.144). Trademarks differ. Among reporters, both digital payments and mobile money accounts are negatively associated with trademarks across countries (rows 5 and 11), and the difference from non-reporters is significant (row 8, bootstrap p = 0.005). The difference survives an interaction of digital payments with income (row 9), so reporter status is not simply a proxy for lower income. The FAS gradient survives a Holm correction across the three outcomes (adjusted p = 0.017), the Findex gradient does not (0.14), and the FAS gradient is not driven by Indonesia or by the economies with inflated account series (rows 14–15). Because the level of registered accounts is not comparable across countries (Section III-A), we also use active accounts, which follow one 90-day definition: the FAS trademark gradient is then −0.180 (bootstrap p = 0.036; row 16), close to the registered-account estimate, although with three outcomes it fails a Holm correction (adjusted p = 0.11). Earlier trademark levels explain part of the pattern. Restricting the sample to countries with 2008–2011 data already shrinks the FAS gradient from −0.178 to −0.100 and makes it marginal (bootstrap p = 0.062; row 12); adding the earlier level halves it again, to −0.050 (row 13). With Findex, the reporter gradient on the restricted sample shrinks from −0.0150 to −0.0103 once the level is added (bootstrap p = 0.132; rows 6–7). The difference from non-reporters, estimated with interacted 2008–2011 levels, is −0.0170 (bootstrap p = 0.049), but it does not survive a Holm correction across the three outcomes in its row (adjusted p = 0.15; row 10). In the same row, the patent difference turns positive (+0.0118, bootstrap p = 0.080), which we do not interpret. Rows 5–16 were added after the main analysis, and rows 14–15 were specified before they were run; the trademark difference is a pattern to be explained, not a test of a hypothesis.

The GII gives a further, smaller check. Across 28 mobile money economies observed in the 2022–2025 editions, mobile money is positively associated with the overall GII (+1.58, bootstrap p = 0.031) and with knowledge and technology outputs (+2.71, bootstrap p = 0.025), but neither survives a Holm correction across the four GII measures (adjusted p = 0.10 for both). The overall GII also averages input pillars, including ICT infrastructure, with output pillars, so part of its association with mobile money may be mechanical. Appendix F and Fig. 4 report the GII estimates; within economies, all GII coefficients are insignificant.

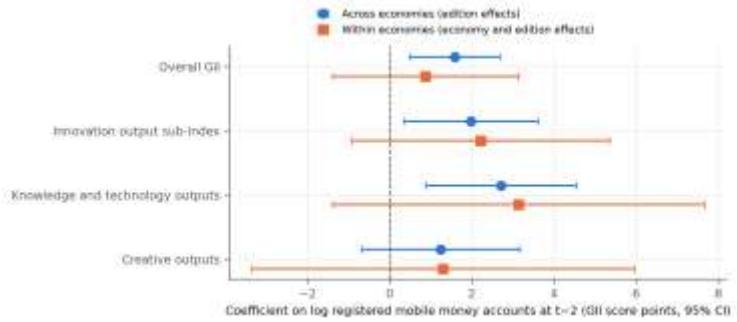


Figure 4. Across-Economy Versus Within-Economy Coefficients of Mobile Money (T−2) On GII Scores, Editions 2022–2025, With 95% Confidence Intervals

D) Digital Versus Conventional Finance, and the Bank-Credit Channel (H3)

Table 6 compares mobile money with conventional banking within countries. For new business density, registered mobile money accounts have a negative coefficient (−0.169, p = 0.041), and bank deposit accounts have a positive one

(+0.516, p = 0.020) that does not survive the bootstrap (p = 0.61), which suggests that a few influential countries drive it. In a horse race on the common sample, mobile money remains negative (−0.209, p = 0.011) and bank deposits fall to zero (+0.110, p = 0.741). The test that separates measurement from use is on the economies that report both series. There, registered accounts give −0.221 (p = 0.059) while active accounts give +0.029 (p = 0.814), and instrumenting registered with active accounts gives +0.037 (p = 0.814). For patents and trademarks, the same comparison gives coefficients of the same sign and similar size for all three measures (Section VI). For new business density, the negative association is therefore consistent with registrations that are not used, such as dormant or inflated accounts, rather than with mobile money use; but the active-account and IV estimates are imprecise (IV 95% interval [−0.28, +0.35]) and cannot exclude a negative effect of use. The registered-account coefficient also does not survive a Holm correction across the four outcomes of Table 2 (adjusted p = 0.17 with analytic p-values, 0.08 with bootstrap p-values). The outcome counts newly registered limited liability companies, not informal firms. We treat H3 as unresolved. For patents, both kinds of finance are insignificant. For trademarks, bank deposit accounts are positive (+0.080, bootstrap p = 0.016) while mobile money is zero.

Table 6: Digital Versus Conventional Finance Within Countries (Coefficient At T−2, FAS; P-Values in Parentheses)

Regressor (T−2)	Patents	Trademarks	High-Tech (%)	New Business
Bank deposit accounts /1,000 (log)	+0.0659 (0.168)	+0.0799 (0.015)	−0.6969 (0.179)	+0.5162 (0.020)
Bank branches /100,000 (log)	+0.1254 (0.417)	+0.1029 (0.329)	−0.9927 (0.548)	+0.0866 (0.762)
Registered mobile money accounts (log)	+0.0138 (0.742)	+0.0018 (0.961)	+0.4085 (0.093)	−0.1688 (0.041)

Country and year fixed effects; full controls. N for new business: bank deposits 683, branches 935, mobile money 259. Bootstrap p-values: new business, bank deposits 0.61 and mobile money 0.019; trademarks, bank deposits 0.016. Bold marks p < 0.05.

Source: *Authors' calculations.*

Private credit shows no response to mobile money growth within countries (−0.21 percentage points of GDP per log unit, p = 0.772, 65 countries; −0.16, p = 0.888, in the 30 countries of the patent sample). This bears only on the bank-credit link of the financing channel: private credit to GDP does not capture supplier and trade credit, which firm-level and model-based studies link to mobile money [6, 46], and the interval is wide. The result fits the view that a payment rail alone does not deepen bank finance, but it does not prove it.

E) Credit Complementarity (H2) and the Failed Instrument

In the Findex TWFE model, the interaction of digital payments with private credit is positive for patents (+0.00008, p = 0.024). In the annual FAS data, the corresponding interaction is close to zero (+0.00067, p = 0.670), and after Holm correction across the eight moderation tests, none of the interactions is significant (all adjusted p = 1.00; Appendix D). H2 is not supported.

We also tried an instrumental-variables design using pre-period (2008–2011) mobile and internet penetration. First-stage F statistics are about 0.02 (mobile) and 0.12 (internet) in the pooled Findex design and between 0.1 and 3.1 in long differences, far below the thresholds required for reliable inference [21, 22]. Once income and period are controlled, pre-period infrastructure no longer predicts digital adoption, and we draw no conclusion from the instrument. We did not estimate system GMM: with a short, unevenly spaced panel and few countries, lag instruments would quickly outnumber clusters.

VI. ROBUSTNESS

Table 7 reports further checks on the FAS within-country estimates; Appendix A summarizes the Findex checks.

Table 7: Robustness of the Within-Country Estimates, FAS (Coefficient On Mobile Money At T−2; P-Values in Parentheses, Bootstrap P-Values In Brackets)

Specification	Patents	Trademarks
Baseline	+0.0138 (0.742) [0.783]	+0.0018 (0.961) [0.961]
Drop India (China does not report)	+0.0010 (0.983)	+0.0011 (0.981)
Drop Indonesia	+0.0291 (0.486)	+0.0068 (0.861)
Drop Indonesia and the eight economies with registered/active ratio above 5	+0.0317 (0.476) [0.573]	+0.0081 (0.843) [0.847]
Drop the six economies with breaks in registered accounts (three in the patent sample)	+0.0227 (0.721) [0.778]	+0.0136 (0.785) [0.801]
Drop economies with breaks in any mobile money series	+0.0513 (0.584) [0.704]	−0.0110 (0.897) [0.910]
Drop breaks and the nine flagged economies	+0.1152 (0.266) [0.458]	−0.0241 (0.807) [0.824]
PPML without the nine flagged economies and those with no active series	+0.1193 (0.083)	+0.1438 (0.091)
Drop country-years with registered/active ratio above 5 at t−2	+0.0275 (0.574) [0.675]	+0.0187 (0.632) [0.629]
Registered accounts instrumented by active accounts (IV)	−0.0544 (0.175)	−0.0271 (0.616)
Winsorize regressor and outcome at 1%	+0.0149 (0.730)	+0.0024 (0.950)
Countries with at least 8 years of data	−0.0604 (0.118)	+0.0250 (0.650)

Lead t+2, conditional on lag t−2 (lead coefficient)	−0.0305 (0.725) [0.857]	−0.0132 (0.858) [0.887]
Active mobile money accounts	−0.0487 (0.160)	−0.0238 (0.616)
Mobile money transaction value (% GDP)	−0.0009 (0.648) [0.695]	−0.0018 (0.490)
ln patent count	+0.0159 (0.743)	
Inverse hyperbolic sine of patent rate	+0.0124 (0.811)	

The transaction-value regressor is in percent of GDP, so its coefficient is not comparable in size to the log regressors. The IV row uses the economies that report both series (patents: 110 observations from 16 countries with within-country variation; trademarks: 136, 20); first-stage F statistics are 279 and 200, and OLS on the same samples gives −0.0333 (p = 0.186) and −0.0253 (p = 0.566). Without the five economies with identical registered and active series, the IV coefficients are −0.0764 (p = 0.133) and −0.0233 (p = 0.751). The strictest break screen leaves 13 countries for patents and 15 for trademarks, with 95% intervals of [−0.100, +0.330] and [−0.232, +0.183]; the restricted PPML rows have 15 and 18 countries, with intervals of [−0.018, +0.256] and [−0.025, +0.313]. The at-least-8-years row implements the balanced-sample check listed in the specification log, which the unbalanced FAS panel does not allow in literal form. Thresholds of 4.4 and 6 for the registered/active ratio give patent coefficients of +0.027 and +0.029 and trademark coefficients of +0.008 and +0.007, all with p > 0.48. With a separate slope for the 16 patent-sample economies that fail at least one screen (common year effects and controls), the slope for the 14 that pass every screen is −0.048 (p = 0.517), and the difference is +0.108 (bootstrap p = 0.22); for trademarks, +0.008 (p = 0.912) and −0.010 (bootstrap p = 0.90). A pre-specified check that drops countries whose registered accounts never exceed one per 1,000 adults is identical to the baseline, because no such country remains in the estimation sample.

Source: *Authors' calculations.*

The estimates stay insignificant across sample restrictions, winsorizing, alternative outcome transformations and alternative mobile money measures. Dropping Indonesia alone gives +0.029 for patents (p = 0.486); dropping it together with the eight high-ratio economies (Section III-A) gives +0.032 (95% interval [−0.059, +0.123]) and +0.008 for trademarks, raising the patent bound from about 10% to about 12% per doubling. Dropping economies with breaks, using other ratio thresholds and dropping country-years with extreme ratios also leave the estimates insignificant. These screens remove up to half of the patent sample, however, and the intervals widen accordingly: in the smallest sample the upper bound is about 40% more patents per doubling of accounts, and the restricted PPML bound for trademarks is about 24%. The patent point estimates also rise as the screens tighten, to +0.115 in the smallest sample and +0.119 (p = 0.083) under the restricted PPML, with +0.144 (p = 0.091) for trademarks. None is significant at 5%. Letting the slope differ between economies that pass every screen and those that fail one, with common year effects and controls, gives no sign that cleaner series show a more positive association (Table 7 note). The screens show no significant positive association once suspect series are removed, but they do not keep the upper bound at 10%.

Measurement error in the regressor would bias these coefficients toward zero, and fixed effects can make the bias worse because they remove much of the true variation. We cannot rule this out, and the drift just described is what attenuation would produce. The evidence does not, however, point to a hidden positive association. To check, we use the economies that report both series, without the five with identical registered and active series. There, registered and active accounts are strongly correlated within countries once fixed effects and controls are removed (0.87 and 0.83 in the patent and trademark samples). Active accounts give coefficients of the same sign as registered accounts, and instrumenting registered accounts with active accounts, which is the active-account estimate rescaled by the first stage (F = 404 for patents), makes the patent coefficient more negative (−0.076 against −0.044 by OLS on the same sample). Read as attenuation, that ratio implies a bias of about 42%; scaling the main patent bound by it gives about 18% more patents per doubling, still within the range in Section V-A. Errors common to both series, such as a definition that counts all e-money instruments, are not corrected this way; Indonesia, the one case we could verify, is dropped in the rows above. Appendix C reports all 24 combinations of six FAS digital measures (including internet and mobile banking) and four outcomes; three coefficients are significant at 5%, against 1.2 expected by chance, and two of them are for high-technology exports with opposite signs.

The placebo results need care. Entered together with the lagged regressor, the lead is insignificant for both outcomes, but its standard error is about 2.1 times that of the main coefficient for patents and 2.0 times for trademarks, so the test has little power. Entered alone (Appendix G), the lead is also insignificant (patents −0.0349, bootstrap p = 0.328). Because the placebo has little power, we treat the wider intervals in Table 3, not the main specification alone, as the relevant bounds.

VII. DISCUSSION

Three considerations help interpret the bounded within-country null and the gradients that predate the sample.

Payment rails alone do not supply risk-bearing credit. Mobile money and digital payments move money cheaply and help households smooth consumption and share risk [11, 12]. Innovation needs risk-bearing capital [9, 30], and in emerging

economies financial constraints hold innovation back [10]. A payment rail can relax those constraints only if it leads to more credit. In our data, bank credit does not rise with mobile money, although supplier and trade credit are not measured. This reading fits the firm-level finding that mobile money is associated with higher productivity when used alongside traditional financial services [16], and cross-country evidence that relates fintech credit, not payments, to innovation, with a sign that depends on institutional quality [15].

Horizon and measurement also matter. Technology diffuses with long and variable lags [72], and patents come at the end of a long pipeline. We see no response at lags of one to three years or local-projection horizons up to four years, but effects that take a decade would fall outside what the data can show. Patents also measure frontier innovation, whereas most firm innovation in developing economies is adoption of existing technology [56]. This may reconcile our national null with the firm-level association reported by Osei-Tutu and Taylor [6]. Their measures are binary indicators from the World Bank Enterprise Surveys for product innovation, process innovation and R&D activity, none of which requires frontier innovation, so mobile money could raise such innovation without raising national patent or trademark filings.

Absorptive capacity may also matter [73]. The Chinese studies find within-region effects in an economy with deep credit markets and a large research base, while the mobile money economies in our sample are poorer and less financially developed. The cross-country results neither confirm nor rule out this reading, because controlling for earlier patent levels cannot tell us what produced them. The credit interaction that would test the reading within countries (H2) is not significant.

The negative cross-country trademark gradient among reporters has no ready explanation in the hypotheses, and once trademark levels before 2012 are controlled, it is no longer distinguishable from zero. A conjecture is that mobile money spreads most where economic activity is informal, and informal firms rarely register trademarks.

VIII. CONCLUSION

Growth in mobile money after launch is not associated with national patenting or trademarking: the main samples rule out associations larger than 10–20% more patents and 6–17% more trademarks per doubling of registered accounts, and screened samples give wider bounds. The cross-country differences in patenting and new firm registration that lie behind the digital payment gradients were already in place in 2008–2011.

Payment infrastructure should be judged by its payment and inclusion aims, not by innovation outcomes. The welfare and access case for digital payment infrastructure is untouched by our results. They do suggest that mobile money growth alone should not be expected to raise patenting or trademarking. For high-technology exports and firm creation, our estimates are not precise enough to say.

A) Implications for Indonesia

Indonesia is in the estimation sample, with a series reported to the FAS annually from 2007 to 2025, and dropping it does not change the conclusions. Three qualifications apply. First, the Indonesian series counts all electronic money instruments (1,065 million at the end of 2025, of which 12% are chip-based cards and 77% are recorded as unregistered) [58], and Indonesia reports no active-account series to the FAS. Second, QRIS payments can be funded from bank accounts as well as from e-money, and BI-FAST transfers between bank accounts are not counted at all, so the paper cannot evaluate QRIS or BI-FAST; their case rests on the payment and inclusion aims set out in the blueprints. Third, Indonesia is richer than the average mobile money economy in our sample and has a lower patent rate. Users of the FAS should read the Indonesian registered series as an e-money instrument count, and inclusion figures differ widely by source: the 2030 blueprint reports 88.7% in the national socio-economic survey for 2023 [2], while Global Findex account ownership is 52% in 2021 and 56% in 2024. A within-country Indonesian study linking digital payment adoption to firm-level innovation would test the question directly.

B) Limitations

- The FAS sample covers developing economies that report mobile money, which are poorer than other developing economies, and the within-country estimates describe growth after launch rather than the launch itself; a country enters the panel when its industry starts reporting, so part of the selection varies over time.
- The developing-country sample uses the current World Bank income classification, which excludes economies that have since graduated to high income.
- Resident patents are rare in many developing economies, which keeps the patent sample at 24 countries in the main specification; filings through regional patent offices may also be missing from national counts, which we have not been able to check.
- Registered accounts overstate active use, and the series definition differs across countries; for Indonesia, we verified that it counts all e-money instruments. We checked the definition against national statistics only for Indonesia. Dropping economies with extreme registered-to-active ratios, identical or broken series, and using active accounts reduces but

does not remove this concern; the samples that remain after the strictest screens are small and give wide bounds.

- The cross-country comparisons by reporter status and the controls for earlier outcome levels were added after the main analysis, and the GII results cover four editions and 28 economies.
- There is no credible causal identification: the infrastructure instrument is weak, and the local projections and placebos describe dynamics rather than establish causation.

IX. REFERENCES

[1] Bank Indonesia, Indonesia Payment Systems Blueprint 2025. Bank Indonesia: Navigating the National Payment Systems in the Digital Era, Bank Indonesia, Jakarta, 2019. [Online]. Available: https://www.bi.go.id/en/publikasi/kajian/Documents/Indonesia-Payment-Systems-Blueprint-2025.pdf

[2] Bank Indonesia, Indonesia Payment System Blueprint 2030. Bank Indonesia: Accelerating National Digital Economy for Future Generation, Bank Indonesia, Jakarta, 2024. [Online]. Available: https://www.bi.go.id/en/publikasi/kajian/Documents/Blueprint-Sistem-Pembayaran-Indonesia-2030-EN.pdf

[3] Republic of Indonesia, Peraturan Presiden Nomor 114 Tahun 2020 tentang Strategi Nasional Keuangan Inklusif [Presidential Regulation No. 114 of 2020 on the National Strategy for Financial Inclusion], 2020. [Online]. Available: https://peraturan.bpk.go.id/Details/154566/perpres-no-114-tahun-2020

[4] Z. Li, J. Zhu, and J. He, The effects of digital financial inclusion on innovation and entrepreneurship: a network perspective, Electronic Research Archive 30(12) (2022) 4697-4715. https://doi.org/10.3934/era.2022238

[5] J. Sun, and J. Zhang, Digital financial inclusion and innovation of MSMEs, Sustainability 16(4) (2024) 1404. https://doi.org/10.3390/su16041404

[6] F. Osei-Tutu, and D. Taylor, The impact of mobile money adoption on firm innovation, Economics Letters 244 (2024) 112006. https://doi.org/10.1016/j.econlet.2024.112006

[7] R.G. King, and R. Levine, Finance and growth: Schumpeter might be right, The Quarterly Journal of Economics 108(3) (1993) 717-737. https://doi.org/10.2307/2118406

[8] L. Laeven, R. Levine, and S. Michalopoulos, Financial innovation and endogenous growth, Journal of Financial Intermediation 24(1) (2015) 1-24. https://doi.org/10.1016/j.jfi.2014.04.001

[9] P.H. Hsu, X. Tian, and Y. Xu, Financial development and innovation: cross-country evidence, Journal of Financial Economics 112(1) (2014) 116-135. https://doi.org/10.1016/j.jfineco.2013.12.002

[10] Y. Gorodnichenko, and M. Schnitzer, Financial constraints and innovation: why poor countries don't catch up, Journal of the European Economic Association 11(5) (2013) 1115-1152. https://doi.org/10.1111/jeea.12033

[11] W. Jack, and T. Suri, Risk sharing and transactions costs: evidence from Kenya's mobile money revolution, American Economic Review 104(1) (2014) 183-223. https://doi.org/10.1257/aer.104.1.183

[12] T. Suri, and W. Jack, The long-run poverty and gender impacts of mobile money, Science 354(6317) (2016) 1288-1292. https://doi.org/10.1126/science.aah5309

[13] T. Suri, Mobile money, Annual Review of Economics 9(1) (2017) 497-520. https://doi.org/10.1146/annurev-economics-063016-103638

[14] H. Bai, L. Huang, and Z. Wang, Supply chain financing, digital financial inclusion and enterprise innovation: evidence from China, International Review of Financial Analysis 91 (2024) 103044. https://doi.org/10.1016/j.irfa.2023.103044

[15] A. Pal, A.D. Wedajo, M.A. Bhat, C.K. Tiwari, and S.T. Khan, Innovation on credit: the role of fintech and institutional quality in global technological advancements, Journal of Innovation and Entrepreneurship 14(1) (2025) 97. https://doi.org/10.1186/s13731-025-00568-8

[16] M. Konte, and G.K. Tetteh, Mobile money, traditional financial services and firm productivity in Africa, Small Business Economics 60(2) (2023) 745-769. https://doi.org/10.1007/s11187-022-00613-w

[17] D. Kolokas, T. Vanacker, D. Veredas, and S.A. Zahra, Venture capital, credit, and FinTech start-up formation: a cross-country study, Entrepreneurship Theory and Practice 46(5) (2022) 1198-1230. https://doi.org/10.1177/1042258720972652

[18] T.T.T. Pham, T. Bui Duy, T. Chu, and T. Nguyen, Financial inclusion, entrepreneurial activity and the moderating role of human capital: new cross-country evidence, Journal of Entrepreneurship in Emerging Economies 17(7) (2025) 187-204. https://doi.org/10.1108/jeee-09-2024-0415

[19] D. Sethi, and D. Acharya, Financial inclusion and economic growth linkage: some cross country evidence, Journal of Financial Economic Policy 10(3) (2018) 369-385. https://doi.org/10.1108/jfep-11-2016-0073

[20] A. Grohmann, T. Klühs, and L. Menkhoff, Does financial literacy improve financial inclusion? Cross country evidence, World Development 111 (2018) 84-96. https://doi.org/10.1016/j.worlddev.2018.06.020

[21] J.L. Montiel Olea, and C. Pflueger, A robust test for weak instruments, Journal of Business & Economic Statistics 31(3) (2013) 358-369. https://doi.org/10.1080/00401706.2013.806694

[22] D.S. Lee, J. McCrary, M.J. Moreira, and J. Porter, Valid t-ratio inference for IV, American Economic Review 112(10) (2022) 3260-3290. https://doi.org/10.1257/aer.20211063

[23] P. Aghion, and P. Howitt, A model of growth through creative destruction, Econometrica 60(2) (1992) 323-351. https://doi.org/10.2307/2951599

[24] R. Levine, Finance and growth: theory and evidence, in: P. Aghion, and S.N. Durlauf (Eds.), Handbook of Economic Growth, Vol. 1A, Elsevier, Amsterdam, (2005) 865-934. https://doi.org/10.1016/S1574-0684(05)01012-9

[25] J.R. Brown, S.M. Fazzari, and B.C. Petersen, Financing innovation and growth: cash flow, external equity, and the 1990s R&D boom, The Journal of Finance 64(1) (2009) 151-185. https://doi.org/10.1111/j.1540-6261.2008.01431.x

[26] M.D. Amore, C. Schneider, and A. Žaldokas, Credit supply and corporate innovation, Journal of Financial Economics 109(3) (2013) 835-855. https://doi.org/10.1016/j.jfineco.2013.04.006

[27] S. Chava, A. Oettl, A. Subramanian, and K.V. Subramanian, Banking deregulation and innovation, Journal of Financial Economics 109(3) (2013) 759-774. https://doi.org/10.1016/j.jfineco.2013.03.015

[28] J. Cornaggia, Y. Mao, X. Tian, and B. Wolfe, Does banking competition affect innovation?, Journal of Financial Economics 115(1) (2015) 189-209. https://doi.org/10.1016/j.jfineco.2014.09.001

[29] T. Beck, R. Levine, and N. Loayza, Finance and the sources of growth, Journal of Financial Economics 58(1-2) (2000) 261-300. https://doi.org/10.1016/S0304-405X(00)00072-6

[30] W.R. Kerr, and R. Nanda, Financing innovation, Annual Review of Financial Economics 7(1) (2015) 445-462. https://doi.org/10.1146/annurev-financial-111914-041825

[31] P. Honohan, Cross-country variation in household access to financial services, Journal of Banking & Finance 32(11) (2008) 2493-2500. https://doi.org/10.1016/j.jbankfin.2008.05.004

[32] F. Allen, A. Demirguc-Kunt, L. Klapper, and M.S. Martinez Peria, The foundations of financial inclusion: understanding ownership and use of formal

accounts, Journal of Financial Intermediation 27 (2016) 1-30. https://doi.org/10.1016/j.jfi.2015.12.003
[33] C.Y. Park, and R. Mercado, Financial inclusion, poverty, and income inequality, The Singapore Economic Review 63(1) (2018) 185-206. https://doi.org/10.1142/S0217590818410059
[34] M. Sarma, and J. Pais, Financial inclusion and development, Journal of International Development 23(5) (2011) 613-628. https://doi.org/10.1002/jid.1698
[35] B. Banik, and C.K. Roy, Measuring fintech-driven financial inclusion for developing countries: Comprehensive Digital Financial Inclusion Index (CDFII), Economic Journal of Emerging Markets 15(2) (2023) 143-159. https://doi.org/10.20885/ejem.vol15.iss2.art3
[36] A. Demirguc-Kunt, and L. Klapper, Measuring financial inclusion: the Global Findex database, Policy Research Working Paper No. 6025, World Bank, Washington, DC, (2012). https://doi.org/10.1596/1813-9450-6025
[37] A. Demirguc-Kunt, L. Klapper, and D. Singer, Financial inclusion and inclusive growth: a review of recent empirical evidence, Policy Research Working Paper No. 8040, World Bank, Washington, DC, (2017). https://doi.org/10.1596/1813-9450-8040
[38] A. Demirguc-Kunt, L. Klapper, D. Singer, S. Ansar, and J. Hess, The Global Findex Database 2017: Measuring Financial Inclusion and the Fintech Revolution, World Bank, Washington, DC, 2018. https://doi.org/10.1596/978-1-4648-1259-0
[39] T. Beck, A. Demirgüç-Kunt, and R. Levine, Finance, inequality and the poor, Journal of Economic Growth 12(1) (2007) 27-49. https://doi.org/10.1007/s10887-007-9010-6
[40] M. Andrianaivo, and K. Kpodar, Mobile phones, financial inclusion, and growth, Review of Economics and Institutions 3(2) (2012). https://doi.org/10.5202/rei.v3i2.75
[41] T. Beck, L. Senbet, and W. Simbanegavi, Financial inclusion and innovation in Africa: an overview, Journal of African Economies 24(Suppl. 1) (2015) i3-i11. https://doi.org/10.1093/jae/eju031
[42] P. Gomber, J.A. Koch, and M. Siering, Digital finance and FinTech: current research and future research directions, Journal of Business Economics 87(5) (2017) 537-580. https://doi.org/10.1007/s11573-017-0852-x
[43] P.K. Ozili, Impact of digital finance on financial inclusion and stability, Borsa Istanbul Review 18(4) (2018) 329-340. https://doi.org/10.1016/j.bir.2017.12.003
[44] C.O. Manasseh, I.C. Nwakoby, O.C. Okanya, N.G. Nwonye, O. Odidi, K.J. Thaddeus, K.K. Ede, and W. Nzidee, Impact of digital financial innovation on financial system development in Common Market for Eastern and Southern Africa (COMESA) countries, Asian Journal of Economics and Banking 8(1) (2024) 121-142. https://doi.org/10.1108/AJEB-04-2022-0041
[45] P.K. Ozili, The financial inclusion mix and the 7Ps: people, process-technology, product, policy, provider, place and price, Asian Journal of Economics and Banking 10(2) (2026) 257-282. https://doi.org/10.1108/AJEB-03-2026-0054
[46] T. Beck, H. Pamuk, R. Ramrattan, and B.R. Uras, Payment instruments, finance and development, Journal of Development Economics 133 (2018) 162-186. https://doi.org/10.1016/j.jdeveco.2018.01.005
[47] L. Guan, and Y. Xue, Digital financial inclusion and corporate green innovation, Finance Research Letters 85 (2025) 108112. https://doi.org/10.1016/j.frl.2025.108112
[48] W. Zhu, Digital finance and manufacturing innovation: evidence from R&D investment and patent output in emerging economies, Finance Research Letters 90 (2026) 109339. https://doi.org/10.1016/j.frl.2025.109339
[49] J. Cifuentes-Faura, K. Khan, and A. Khurshid, Does digital financial inclusion support carbon neutrality? A cross-regional study, Financial Innovation 12(1) (2026). https://doi.org/10.1186/s40854-025-00826-4
[50] I. Koomson, E. Martey, and P.M. Etwire, Mobile money and entrepreneurship in East Africa: the mediating roles of digital savings and access to digital credit, Information Technology & People 36(3) (2023) 996-1019. https://doi.org/10.1108/itp-11-2021-0906
[51] P. Tiwasing, J.A. Addae, F.Z. Naab, and G.Z. Naab, Do mobile money services enhance business performance? An empirical analysis of Kenyan businesses, The International Journal of Entrepreneurship and Innovation (2024). https://doi.org/10.1177/14657503241230946
[52] F.M. Ajide, R. Sakariyahu, R. Lawal, O.F. Etudaiye-Muhtar, and S. Johan, Mobile money innovation and global value chain participation: evidence from developing countries, Finance Research Letters 58 (2023) 104694. https://doi.org/10.1016/j.frl.2023.104694
[53] S.A. Asongu, and N.M. Odhiambo, Female unemployment, mobile money innovations and doing business by females, Journal of Innovation and Entrepreneurship 12(1) (2023). https://doi.org/10.1186/s13731-023-00319-7
[54] F. Mao, Y. Wang, and M. Zhu, Digital financial inclusion, traditional finance system and household entrepreneurship, Pacific-Basin Finance Journal 80 (2023) 102076. https://doi.org/10.1016/j.pacfin.2023.102076
[55] J. Rubio, and A.B. Tulcanaza-Prieto, Digital payments trust in Latin America and the Caribbean, Economies 13(5) (2025) 140. https://doi.org/10.3390/economies13050140
[56] X. Cirera, and W.F. Maloney, The Innovation Paradox: Developing-Country Capabilities and the Unrealized Promise of Technological Catch-Up, World Bank, Washington, DC, 2017. https://doi.org/10.1596/978-1-4648-1160-9
[57] International Monetary Fund, Financial Access Survey: dataset and indicator code list CL_FAS_INDICATOR (SDMX dataflow IMF.STA:FAS 9.0.0), IMF, Washington, DC, 2026. [Online]. Available: https://api.imf.org/external/sdmx/2.1 (accessed 9 November 2026).
[58] Bank Indonesia, Statistik Sistem Pembayaran dan Infrastruktur Pasar Keuangan Indonesia, Tabel 5e: Uang Elektronik [Payment system and financial market infrastructure statistics, Table 5e: Electronic money], 2026. [Online]. Available: https://www.bi.go.id/id/statistik/ekonomi-keuangan/spip/Documents/TABEL_5e.pdf (accessed 9 November 2026).
[59] Z. Griliches, Patent statistics as economic indicators: a survey, NBER Working Paper No. 3301, National Bureau of Economic Research, Cambridge, MA, (1990). https://doi.org/10.3386/w3301
[60] C. de Chaisemartin, and X. D'Haultfœuille, Two-way fixed effects estimators with heterogeneous treatment effects, American Economic Review 110(9) (2020) 2964-2996. https://doi.org/10.1257/aer.20181169
[61] B. Callaway, A. Goodman-Bacon, and P.H.C. Sant'Anna, Difference-in-differences with a continuous treatment, NBER Working Paper No. 32117, National Bureau of Economic Research, Cambridge, MA, (2024). https://doi.org/10.3386/w32117
[62] B. Callaway, and P.H. Sant'Anna, Difference-in-differences with multiple time periods, Journal of Econometrics 225(2) (2021) 200-230. https://doi.org/10.1016/j.jeconom.2020.12.001
[63] A. Goodman-Bacon, Difference-in-differences with variation in treatment timing, Journal of Econometrics 225(2) (2021) 254-277. https://doi.org/10.1016/j.jeconom.2021.03.014
[64] J. Chen, and J. Roth, Logs with zeros? Some problems and solutions, The Quarterly Journal of Economics 139(2) (2024) 891-936. https://doi.org/10.1093/qje/qjad054
[65] Ò. Jordà, Estimation and inference of impulse responses by local projections, American Economic Review 95(1) (2005) 161-182. https://doi.org/10.1257/0002828053828518
[66] M. Bertrand, E. Duflo, and S. Mullainathan, How much should we trust differences-in-differences estimates?, The Quarterly Journal of Economics

119(1) (2004) 249-275. https://doi.org/10.1162/003355304772839588
[67] A.C. Cameron, J.B. Gelbach, and D.L. Miller, Bootstrap-based improvements for inference with clustered errors, Review of Economics and Statistics 90(3) (2008) 414-427. https://doi.org/10.1162/rest.90.3.414
[68] J.G. MacKinnon, and M.D. Webb, Wild bootstrap inference for wildly different cluster sizes, Journal of Applied Econometrics 32(2) (2017) 233-254. https://doi.org/10.1002/jae.2508
[69] D. Roodman, M.Ø. Nielsen, J.G. MacKinnon, and M.D. Webb, Fast and wild: bootstrap inference in Stata using boottest, The Stata Journal 19(1) (2019) 4-60. https://doi.org/10.1177/1536867X19830877
[70] J.G. MacKinnon, M.Ø. Nielsen, and M.D. Webb, Cluster-robust inference: a guide to empirical practice, Journal of Econometrics 232(2) (2023) 272-299. https://doi.org/10.1016/j.jeconom.2022.04.001
[71] A. Abadie, Statistical nonsignificance in empirical economics, American Economic Review: Insights 2(2) (2020) 193-208. https://doi.org/10.1257/aeri.20190252
[72] D. Comin, and B. Hobijn, An exploration of technology diffusion, American Economic Review 100(5) (2010) 2031-2059. https://doi.org/10.1257/aer.100.5.2031
[73] W.M. Cohen, and D.A. Levinthal, Absorptive capacity: a new perspective on learning and innovation, Administrative Science Quarterly 35(1) (1990) 128-152. https://doi.org/10.2307/2393553

APPENDIX

Appendix A. Findex within-country robustness

The Findex two-way fixed effects patent estimate keeps its negative sign across the baseline (−0.0105, p = 0.044), dropping China and India (−0.0111, p = 0.037), alternative patent transformations (log count −0.0129, p = 0.040; inverse hyperbolic sine −0.0116, p = 0.051), additional controls (−0.0103, p = 0.052) and a balanced sample (−0.0129, p = 0.052). With alternative digital regressors, it ranges from −0.0121 (account ownership, p = 0.062) to −0.0024 (mobile money account ownership, p = 0.436; making digital payments, −0.0043, p = 0.178). The wild cluster bootstrap p-value of the baseline is 0.051, and the lead placebo is −0.0071 (p = 0.095). The Findex trademark estimate is insignificant in all specifications except one: with the outcome taken from the following wave, it is −0.0113 (p = 0.028).

Appendix B. Instrument diagnostics (Findex)

First-stage F statistics for the pre-period infrastructure instruments are far below conventional thresholds in every specification (mobile ≈ 0.02 and internet ≈ 0.12 in the pooled design; between 0.1 and 3.1 in long differences). Reduced-form and Anderson–Rubin tests are insignificant.

Appendix C. Alternative FAS Digital Regressors (T−2, Full Controls; P-Values In Parentheses)

Regressor	Patents	Trademarks	High-Tech (%)	New Business
Registered mobile money accounts /1,000 (log; main)	+0.014 (0.742)	+0.002 (0.961)	+0.409 (0.093)	−0.169 (0.041)
Active mobile money accounts /1,000 (log)	−0.049 (0.160)	−0.024 (0.616)	−0.322 (0.339)	+0.029 (0.814)
Mobile money transaction value (% GDP)	−0.001 (0.648)	−0.002 (0.490)	−0.033 (0.009)	−0.000 (0.957)
Number of mobile money transactions (log millions)	−0.040 (0.204)	−0.007 (0.907)	−0.142 (0.523)	−0.099 (0.094)
Active mobile money agents /100,000 (log)	−0.043 (0.399)	+0.034 (0.703)	+0.177 (0.668)	−0.051 (0.806)
Internet and mobile banking transactions (log millions)	+0.085 (0.241)	+0.011 (0.782)	+0.881 (0.015)	−0.074 (0.666)

Source: *Authors' calculations.*

All six measures were listed in the specification log before estimation. Three of 24 coefficients are significant at 5% (bold). Countries per cell range from 15 (agents, patents) to 49 (internet and mobile banking, high-tech); cells with fewer than 20 countries rely on analytic standard errors only.

Appendix D. Moderation Tests (FAS, Interaction of Mobile Money At T−2 With Centered Moderators)

Interaction	Coefficient	Raw P	Holm P	Romano–Wolf P
Patents × private credit (H2)	+0.00067	0.670	1.000	0.958
High-tech × internet use	−0.00965	0.219	1.000	0.958
New business × internet use	−0.00557	0.231	1.000	0.847
Remaining five interactions		0.26–0.93	1.000	≥ 0.76

Source: *Authors' calculations.*

Country and year fixed effects; full controls. Romano–Wolf step-down p-values use 1,999 joint wild bootstrap draws and order tests by their t statistics, so they need not be monotone in the raw p-values; under both methods no interaction survives correction.

Appendix E. IMF FAS Coverage, Developing Sample

Indicator	Observations	Countries	Years
Registered mobile money accounts /1,000 (main)	744	75	2007–2025
Active mobile money accounts /1,000	595	67	2009–2025
Mobile money transaction value (% GDP)	693	72	2007–2025
Active mobile money agents /100,000	509	58	2009–2025

Bank deposit accounts /1,000	1,733	102	2004–2025
Bank branches /100,000	2,481	122	2004–2025
ATMs /100,000	2,318	122	2004–2025

Source: *Authors' calculations.*

Economies with extreme ratios of registered to active mobile money accounts. Across the 65 developing economies that report both series, the median of each country's median ratio is 2.5, and the 75th percentile is 4.1. Eight economies exceed 5 (country median, number of years in parentheses): Papua New Guinea 16.7 (2), Niger 16.6 (11), Afghanistan 16.3 (8), Republic of Congo 15.5 (11), Chad 11.9 (8), Angola 7.0 (7), South Sudan 5.2 (2) and Madagascar 5.0 (16). The threshold of 5 was set before the affected regressions were run, when the ratio distribution was already known. Ten reporters have no active series: Belize, Ecuador, Ethiopia, Indonesia, Kenya, Morocco, Mozambique, Malaysia, Thailand and Tunisia. Two economies (Cabo Verde and Nicaragua) report active but not registered accounts, so 65 of the 67 economies with an active series report both. Five economies report active accounts equal to registered accounts in every year: Albania, Gabon, Honduras, Mauritania and Timor-Leste. Breaks, defined as a fall of more than one log point in log(1 + accounts) between consecutive observations: registered accounts in Afghanistan, Albania, the Republic of Congo, Ecuador, Jamaica and Jordan; active accounts in Albania, the Republic of Congo, India, Jamaica, the Philippines and El Salvador. Transaction value moves by more than 50 percentage points of GDP between consecutive observations in Burkina Faso, Ghana, Iraq, Cambodia, Namibia and Senegal. For Indonesia, the FAS registered series (FA67) equals Bank Indonesia's count of all electronic money instruments within rounding in every year from 2020 to 2025: 432.28, 575.32, 730.70, 809.78, 922.07 and 1,065.44 million instruments.

A note for users of the FAS SDMX files. The IMF data service writes a country with no observations as an empty, self-closing series element. A text-based parser that does not handle such elements assigns the next country's observations to the empty one (for example, India's active accounts to Indonesia, which reports none). Our data build parses the files as XML and checks that every non-empty series in the raw text is assigned to its own country and that no two countries share an identical series.

Appendix F. Mobile Money (T−2) and GII Scores, Editions 2022–2025 (P-Values in Parentheses; Bootstrap P-Values in Brackets)

GII Outcome	Across Economies (Edition FE)	Within Economy (Economy + Edition FE)
Overall GII	+1.580 (0.009) [0.031]	+0.870 (0.460)
Innovation output sub-index	+1.977 (0.025) [0.078]	+2.218 (0.179)
Knowledge and technology outputs	+2.713 (0.007) [0.025]	+3.136 (0.188)
Creative outputs	+1.241 (0.217) [0.366]	+1.299 (0.590)

Source: *Authors' calculations.*

28 economies; N = 69 (across) and 65 (within). Holm-adjusted bootstrap p-values across the four measures: 0.10, 0.16, 0.10 and 0.37.

Appendix G. Unconditional lead placebo

Regressing current outcomes on mobile money at $t + 2$ alone, without the lagged regressor, gives −0.0349 (p = 0.288; bootstrap p = 0.328) for patents and +0.0101 (p = 0.760) for trademarks. Neither indicates that outcomes move ahead of mobile money. The conditional version is in Table 7.

Data Availability Statement

The data that support the findings of this study are publicly available from the World Bank (Global Findex, World Development Indicators), the International Monetary Fund (Financial Access Survey), WIPO (Global Innovation Index) and Bank Indonesia (payment system statistics). The download scripts, intermediate datasets, estimation code, codebook and time-stamped specification log are available from the corresponding author upon reasonable request.