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ASSESSMENT OF THE IMPACT OF DIGITIZATION ON INFLATION PROCESSES AND FINANCIAL STABILITY IN AZERBAIJAN

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Abstract

This study examines whether payment digitization is associated with inflation and selected financial vulnerabilities in Azerbaijan using 96 monthly observations from January 2018 to December 2025. The cashless share of domestic card transaction value is the main payment indicator; transaction counts and point of sale infrastructure provide alternatives. Persistence tests, exploratory ARDL bounds screening and dynamic change regressions are combined with seasonal and dated controls, heteroskedasticity and autocorrelation consistent inference and sensitivity analysis. The cashless value share increased from 17.375% to 67.754%, while overdue lending and deposit dollarization declined. These trends do not identify a causal relationship. In the principal inflation equation, the lagged change in the cashless share has a coefficient of 0.105 ($p = 0.085$; 95% confidence interval −0.015 to 0.226). Payment lags are jointly significant in the untrended log-risk equation ($p = 0.046$), but their signed sum is imprecise ($p = 0.092$); adding a trend reverses its sign. The principal dollarization estimate is also imprecise ($p = 0.832$). Results vary across proxies, covariance bandwidths and samples. Inference is conditional on exploratory model selection, and the bounds screens do not justify an equilibrium interpretation. The findings document payment transformation while leaving its independent macrofinancial benefits unresolved, supporting statistical harmonization and targeted policy evaluation.

Keywords: digital payments; inflation dynamics; overdue lending; deposit dollarization; financial vulnerability; time-series analysis; Azerbaijan

1 INTRODUCTION

Digital payments are changing how households transact, how firms receive revenue and how financial institutions obtain information. Their macroeconomic consequences, however, depend on how these operational changes interact with spending, lending and monetary conditions. Thakor (2020) and Boot et al. (2021) distinguish technological innovation in payments and distribution from changes in the information and balance-sheet functions of financial intermediation. This distinction is central to evaluating whether growing digital payment use is associated with price stability and lower financial vulnerability.

The relationship with inflation has competing mechanisms. More efficient transactions could reduce some business costs, while easier payments and access to finance could support expenditure. Agarwal et al. (2024) provide evidence that switching to digital payments increases consumer spending, although a spending response does not by itself establish an aggregate price effect. Hasan et al. (2024) find that fintech adoption can weaken monetary policy transmission, including its effects on consumer prices and lending. Payment digitization therefore cannot be assigned a uniformly disinflationary role without examining the economic setting and the outcome being measured.

The evidence on financial stability is similarly heterogeneous. Daud et al. (2022) find a stabilizing association between fintech development and banking outcomes across countries, whereas Khan et al. (2023) identify reduced stability among GCC banks, with important differences across bank characteristics and financial environments. Currency choice creates another source of ambiguity. Cong and Mayer (2025) show theoretically that the consequences of payment and currency digitization depend on competition between monetary arrangements. These findings motivate examining credit vulnerability and deposit dollarization separately, since payment convenience, loan performance and preferences for foreign-currency saving need not move through the same mechanism.

Azerbaijan offers a relevant setting because substantial payment digitization has coincided with changes in financial vulnerability. In the monthly dataset assembled for this study, the cashless share of domestic card transaction value increases from 17.375% in January 2018 to 67.754% in December 2025. Across the same

endpoints, the overdue lending ratio falls from 14.549% to 1.642%, and deposit dollarization declines from 73.668% to 36.832%. These patterns establish the scale of the observed changes, but their concurrence leaves open whether digitization has an independent association with the financial outcomes. Inflation also varies over the period, providing a further outcome against which to assess the payment transition.

The research gap concerns the connection between evidence obtained at different levels of observation and the interpretation of national time-series relationships. International studies identify behavioral mechanisms, bank-level responses and cross-country regularities, but these do not determine the direction or stability of an Azerbaijan-specific association. Persistent trends, reporting changes and concurrent economic developments can produce strong correlations in levels. A useful national assessment must therefore establish whether the apparent relationships remain informative after allowing for dynamics, seasonality and observable monetary conditions, and whether a long-run equilibrium interpretation is supported.

This study asks whether changes in payment digitization are associated with changes in inflation, overdue lending and deposit dollarization, and whether the conclusions survive alternative measures and specifications. Digitization is represented primarily by the cashless share of domestic card transaction value, supplemented by transaction counts and point of sale (POS) infrastructure. Financial vulnerability is examined through two indicators: the overdue lending ratio for credit institutions and the foreign-currency share of deposits. These indicators permit a focused assessment of credit and currency vulnerability; they do not provide a comprehensive measure of systemic resilience.

The empirical analysis uses 96 monthly observations from January 2018 to December 2025, with official payment, monetary, credit, deposit and consumer price information. Stationarity and break tests precede preliminary autoregressive distributed lag screening. Dynamic change models incorporate seasonal and dated controls, monetary variables, residual diagnostics and heteroskedasticity and autocorrelation consistent (HAC) inference. Quarterly estimates and alternative payment proxies assess sensitivity. This sequence allows the treatment of persistence to follow the evidence, while recognizing that lagged regressors and statistical controls do not independently establish causality.

The contribution is an integrated assessment of three outcomes within a common national framework, with explicit attention to measurement comparability and specification uncertainty. The results document rapid payment digitization but do not establish a robust disinflationary relationship, a stable net reduction in overdue lending or a consistently significant association with deposit dollarization. They also do not support a sufficiently defensible equilibrium relationship across specifications. The findings consequently help delimit the macrofinancial claims that can be drawn from payment adoption data and identify priorities for improved measurement and policy evaluation. The article proceeds through the literature, data and methodology, empirical results and discussion, and their implications, limitations and conclusions.

2 LITERATURE REVIEW

Research published during 2019–2026 connects digital finance to payment efficiency, household spending, credit allocation and the transmission of monetary policy. Its findings do not imply a uniform relationship between digitization, inflation and financial stability. The reviewed evidence concerns different technologies and levels of observation, ranging from individual transactions and loans to banks and national financial systems. This critical narrative review synthesizes 40 journal articles, distinguishing direct evidence on payments and prices from evidence on mechanisms and related dimensions of financial vulnerability.

2.1 Defining digitization and its economic mechanisms

A recurring distinction is between digitizing the delivery of a financial service and changing the underlying intermediation function. Thakor (2020) reviews payment, credit and other financial innovations within their interaction with banking, while Boot et al. (2021) separate innovations in information production from innovations in communication and distribution. Broby (2021) similarly places technological change within the banking business model. These frameworks imply that a payment indicator cannot be interpreted as a complete measure of financial transformation: faster settlement, borrower screening and maturity transformation are related but analytically distinct activities.

Competition further complicates the expected outcomes. Vives (2019) identifies efficiency opportunities alongside uncertainty about the eventual market structure and the influence of regulation. Stulz (2019) emphasizes banks' deposit-related advantages and the distinctive competitive position of large technology platforms. Goldstein et al. (2019) frame fintech as a research field requiring careful empirical designs, including the registered-report approach used in their special issue. Together, these contributions support studying specific technologies and outcomes rather than assuming that a broad label such as fintech identifies a single, consistently stabilizing economic force.

2.2 Digital payments and expenditure behavior

Household spending provides one possible connection between payment adoption and inflation. Agarwal et al. (2024) use variation in the switch to digital payments induced by India's demonetization and find a persistent increase in consumer spending. Their analyses distinguish the spending response from explanations involving price changes, credit supply and income shocks. This evidence supports an expenditure channel, but an increase in household expenditure is not itself an estimate of aggregate inflation: its price consequences depend on supply responses, substitution across purchases and monetary conditions.

Schomburgk et al. (2024) synthesize 392 effect sizes from 71 studies and find a small but significant cashless spending effect, with substantial contextual variation and a tendency for the effect to weaken over time. This heterogeneity cautions against treating a fixed behavioral response as universal. Chodorow-Reich et al. (2020), examining India's demonetization, find that more severe cash shortages coincided with faster adoption of alternative payments but lower economic activity and credit growth. The two Indian studies therefore address different margins: the spending consequences of payment substitution and the disruption caused by withdrawing cash. Their identification strategies do not justify equating every observed rise in cashless transactions with an expansionary aggregate demand shock.

2.3 Inflation and the transmission of monetary policy

Direct macroeconomic evidence challenges the expectation that payment digitization must be disinflationary. Prabheesh et al. (2024) apply a panel VAR to monthly observations from ten emerging market economies during the COVID-19 period and report positive responses of inflation to cashless transactions. Their setting includes substantial fiscal and pandemic-related changes, making the separation of payment developments from concurrent conditions particularly important. Ajayi and Onah (2026) report short- and long-run inflationary associations in a Nigerian ARDL analysis. This country-specific result adds direct time-series evidence, but neither it nor a pandemic-period panel establishes that the same equilibrium or transmission mechanism holds in Azerbaijan.

Rooj and Mehta (2025) connect Indian retail digital payment data with household inflation expectations and report higher expectations as digital payments increase. Expectations and realized consumer price inflation are distinct outcomes, so their findings support examining an additional mechanism rather than substituting for a price equation. In a different setting, Wiafe et al. (2022) use a structural VAR for Ghana and conclude that mobile money growth weakens monetary policy effectiveness. Hasan et al. (2024) also find that fintech adoption attenuates the transmission of monetary policy to economic activity, consumer prices and lending in an interacted panel VAR, with the strongest weakening in the bank lending response. These studies concern how shocks propagate, which is different from estimating an unconditional effect of digitization on the inflation rate.

The composition of financial inclusion helps explain conflicting results. Kebede et al. (2024) find that overall financial inclusion shocks temporarily increase inflation in the West African Economic and Monetary Union, while deposit and credit shocks produce different responses. Oanh et al. (2023) likewise identify contrasting relationships among inclusion, monetary policy and financial stability across countries with high and low financial development. Conversely, Miano et al. (2026) find limited short-run effects of mobile money on broad money and interbank rates in Tanzania, with some change following the post-2021 policy environment. The collective implication is conditionality: mobile money, digital card payments, deposit access and digital lending should not be treated as equivalent explanatory variables.

For an Azerbaijan time-series assessment, this literature motivates controlling for monetary conditions and retaining both positive and negative associations as empirical possibilities. It also distinguishes an inflation-rate change from monthly inflation, and a relationship with inflation from a change in monetary transmission. A single-equation model with lagged payment indicators can examine conditional dynamics, but it does not reproduce the shock identification of a structurally identified VAR or directly test the expectations channel when expectations data are absent.

Evidence specific to Azerbaijan also motivates a broader set of potential inflation determinants. Using annual observations for 1995–2023 and OLS and ARDL specifications, Gulaliyev et al. (2025) examine government expenditure, minimum wages and unemployment, reporting associations that depend on wage measurement and lag structure. This identifies relevant contextual covariates for the present study, whose monthly controls do not comprehensively represent fiscal or labour-market conditions. Their estimates concern different regressors and frequency and do not establish an effect of digital payments.

2.4 Credit information and lending outcomes

A separate literature provides mechanisms through which digitization could improve credit allocation. Berg et al. (2020) show that digital footprints contain information useful for predicting consumer default and complement credit bureau information. Jagtiani and Lemieux (2019) find that alternative information in LendingClub ratings helps predict loan performance and can improve credit pricing for borrowers classified unfavorably by conventional criteria. Fuster et al. (2019) find faster mortgage processing by technology-based lenders without a corresponding increase in defaults. These findings establish that particular technologies can improve screening or service delivery, but they do not show that a growing national cashless share necessarily lowers aggregate overdue lending.

Other studies qualify the favorable mechanism. Di Maggio and Yao (2021) find higher default among fintech borrowers than comparable borrowers at traditional institutions and an increase in indebtedness following origination. Tang (2019) shows that peer-to-peer lending can substitute for bank credit for some borrowers while complementing it for small loans. Vallée and Zeng (2019) identify a trade-off between information production, sophisticated investor participation and adverse selection in marketplace lending. Thus, greater digital credit availability can change both the composition of borrowers and the distribution of risk, rather than simply improve the quality of an unchanged loan portfolio.

Distributional and competitive effects also matter. Fuster et al. (2022) show that more accurate machine-learning predictions need not distribute credit-market gains evenly across borrower groups. Chu and Wei (2024) demonstrate theoretically that fintech competition can reduce interest rates while worsening credit allocation and welfare under some conditions. Berg et al. (2022), reviewing fintech lending, conclude that convenience and speed have been more central to its growth than demonstrated improvements in screening or monitoring. Taken together, these studies justify treating improved credit assessment as a plausible mechanism to investigate, rather than an automatic consequence of payment adoption.

2.5 Bank stability and the role of institutions

Evidence using bank and country panels often supports a stabilizing association, although the relevant measurements differ. Daud et al. (2022) find a positive relationship between fintech development and bank stability in a 63-country panel. Safiullah and Paramati (2024) report a stabilizing relationship for Malaysian banks, while Yudaruddin et al. (2023) find improved stability among Indonesian banks, with stronger benefits for smaller and non-listed institutions. Haddad and Hornuf (2023) associate fintech start-up formation with improved incumbent performance, lower stock return volatility and reduced systemic risk exposure across a broad international sample. These studies measure different aspects of stability; their estimates are not directly comparable to a national overdue lending ratio.

Institutional conditions and nonlinearities help explain why favorable results are not universal. Banna et al. (2021) find that fintech-based inclusion reduces bank risk-taking across OIC countries. Ni et al. (2023), using Chinese urban bank data, report an inverted U-shaped relationship in which risks initially increase and later decline, with regulation moderating the association. Khan et al. (2023), by contrast, find that fintech adoption reduces stability in GCC banks, with substantial variation by capitalization, size, ownership and the banking environment. Antwi and Kong (2023) obtain opposing stability relationships for mobile subscriptions and internet use in developing economies. Such differences may reflect both institutional heterogeneity and the fact that connectivity indicators capture something different from the adoption of a specific financial service.

The competitive environment is also endogenous to digital development. Frost et al. (2019) show how platform information and banking market structure shape big-tech intermediation. Cornelli et al. (2023) identify income, banking mark-ups, regulation and institutional conditions as correlates of fintech and big-tech lending across 79 countries. Hodula (2023) associates alternative credit expansion with narrower bank interest margins, reflecting changes in lending and deposit rates. These findings suggest that digitization and financial structure evolve jointly. An observed decline in aggregate arrears could therefore arise from portfolio growth, restructuring or changes in the lenders serving particular borrowers, rather than solely from better screening enabled by digital transactions.

The measurement of stability is particularly relevant to this national application. Gulaliyev et al. (2019) construct a composite banking stability indicator for 29 countries using capital adequacy, asset quality, profitability and liquidity. Their framework illustrates the broader dimensions omitted by an overdue lending ratio or deposit currency share alone. Gulaliyev, Rahimov, et al. (2021) combine composite indices and linear regressions to compare financial institutions in Azerbaijan, Turkey and Iran, reporting differences across countries in the relationships between financial indicators and growth. These studies motivate distinguishing financial access, depth, efficiency and stability when interpreting payment adoption; neither directly tests the macrofinancial consequences of digital payments.

Social outcomes provide a further distinction. Gulaliyev, Gurbanova, et al. (2021) compare financial development and poverty in Azerbaijan, Turkey and Iran. They report weak poverty relationships for Azerbaijan and Turkey, alongside negative associations of efficiency and stability indices with poverty in Iran. This contextual evidence cautions against equating financial development with uniform welfare gains. Poverty and distributional outcomes are outside the present dataset, so the payment regressions cannot evaluate those relationships.

2.6 Currency choice and deposit dollarization

Currency denomination introduces a further distinction between the medium used to execute a payment and the currency in which wealth is held. Gopinath and Stein (2021) explain how trade invoicing and the demand for safe assets reinforce the use of a dominant currency. Their theory provides a reason why improved payment convenience need not independently overturn established currency preferences. Brunnermeier and Niepelt (2019) derive conditions under which substitutions between private and public money leave allocations and prices unchanged. Their equivalence result is conditional and does not imply that all changes in payment technology are neutral.

Cong and Mayer (2025) explicitly model digitization as a strategic response to competition among fiat and private digital monies. The consequences depend on currency dominance, timing and interoperability. This perspective allows digital infrastructure to strengthen a domestic currency in some settings while facilitating competing currencies in others. However, the theory concerns currency and payment competition, including digital money arrangements, whereas the present empirical indicator measures domestic card payment use. It therefore motivates examining dollarization without treating a cashless share as a measure of stablecoin use, CBDC adoption or foreign digital currency exposure.

2.7 Synthesis and the Azerbaijan research gap

The reviewed evidence yields three implications for research design. First, payment adoption, digital financial inclusion and fintech lending represent different processes; a proxy should be interpreted within its actual coverage. Second, the relationships with inflation and risk depend on behavioral responses, financial structure and institutional conditions. Third, much of the stronger evidence on mechanisms comes from loan-level data or identifiable policy shocks, whereas aggregate time-series regressions face simultaneity, omitted influences and common trends. Panel estimates and microeconomic causal results can inform an Azerbaijan study without determining its coefficient signs or resolving its identification problems.

The resulting gap is a measurement-aware assessment of Azerbaijan's aggregate payment transition across inflation, overdue lending and deposit currency composition. The empirical contribution is a documented national dataset and an exploratory comparison of conditional dynamic associations. Evidence about digital lending, financial inclusion or digital currencies motivates possible channels, but those constructs differ from the card-payment share measured here.

3 DATA AND VARIABLES

3.1 Sources coverage and comparability

The assembled dataset draws on the Central Bank of Azerbaijan (CBA) statistical bulletins, monetary and payment statistics and consumer price information published by the State Statistical Committee of the Republic of Azerbaijan (SSC). The workbook spans 2014–2025, but the principal analysis uses January 2018–December 2025, providing 96 complete monthly observations for the core variables. Earlier payment tables do not consistently report electronic-commerce coverage and are not concatenated with the principal digitization series. Internet and mobile banking counts are available only from 2022 and do not determine the main sample. The quarterly sensitivity sample covers 2018Q1–2025Q4, with 32 observations before transformation and lag losses.

Monthly source changes are chained into a CPI series with December 2013 assigned a reference value of 100. The choice of base leaves growth rates unchanged; rounding in the published monthly changes introduces measurement uncertainty. Cumulative flow statistics, where encountered, are converted into individual periods before aggregation. Missing observations are not replaced by zero or automatically interpolated. Reporting changes remain relevant within the principal sample, so dated indicators and restricted samples provide sensitivity checks rather than proof of full harmonization.

3.2 Definitions and transformations

Table 1 Variables definitions and sources

Symbol	Indicator	Construction and empirical use	Source
CPI	Reconstructed consumer price index	$CPI_t = CPI_{t-1}(1 + m_t/100)$, where m_t is the official monthly percentage change; December 2013 = 100.	SSC price and tariff statistics

Symbol	Indicator	Construction and empirical use	Source
INF; INFM	Annual and monthly log inflation	$INF_t = 100\ln(CPI_t/CPI_{t-12})$; $INFM_t = 100\ln(CPI_t/CPI_{t-1})$. Main inflation outcome: ΔINF.	SSC; author calculations
ODL; LOGODL	Overdue lending and its logarithmic transformation	ODL = 100 × overdue loans / total credit of credit institutions. LOGODL = 100ln(ODL); principal risk outcome: ΔLOGODL.	CBA statistical bulletins
DOLL	Deposit dollarization	100 × (foreign-currency demand deposits + foreign-currency time deposits, in AZN equivalent) / total deposits. Outcome: ΔDOLL.	CBA deposit statistics
DIG	Cashless value share in domestic card operations	100 × NC/(NC + CW), where NC is cashless transaction value and CW is card cash withdrawals. Main payment regressor: ΔDIG.	CBA payment statistics
DIG_N; DIGN	Cashless count share	100 × NNC/(NNC + NCW), using cashless and withdrawal transaction counts. Alternative proxy; labeled DIGN in result tables.	CBA payment statistics
M2G	Annual nominal M2 growth	$100\ln(M2_t/M2_{t-12})$, using month-end M2. Enters the inflation change model in first differences.	CBA monetary statistics
CREDG	Annual nominal credit growth	$100\ln(CR_t/CR_{t-12})$, where CR is total credit of credit institutions. First difference used in the extended risk equation.	CBA credit statistics
RATE	Policy rate	Official month-end refinancing rate, in annual percent. Enters change equations in first differences; not a monthly average.	CBA policy-rate statistics
POS; LNPOS	Payment acceptance infrastructure	Month-end number of POS terminals; LNPOS = ln(POS). The first difference of LNPOS is an alternative payment regressor.	CBA terminal statistics
Z	Seasonal and dated controls	Eleven monthly indicators; COVID = 1 during 2020M03–2020M12; step indicators from 2021M01 and 2022M01. A trend is added only in the risk sensitivity model.	Calendar and author coding

Note: created by the author

Shares and rates are stored on a 0–100 scale. A one-unit change in DIG, DOLL or ODL is one percentage point, whereas inflation and growth measures use log percentages. NC combines separately reported domestic cashless channels, including ATM, POS, electronic commerce and self-service payments when separately disclosed. Contactless POS operations are a subset and are not added again. Withdrawals include the reported ATM and POS components. DIG therefore measures the payment-card segment rather than all cashless activity in the economy. ODL covers credit institutions, with bank and non-bank credit summed where separately reported; it is not equated with the banking-sector nonperforming loan ratio.

Quarterly transaction flows are summed, while loans, deposits, the policy rate and terminal counts use quarter-end observations. Quarterly payment shares are ratios of quarterly transaction totals. The quarterly CPI is the mean of monthly index levels, and annual quarterly inflation is $100\ln(CPIq_t/CPIq_{t-4})$. Nominal M2 and credit growth use quarter-end stocks and four-quarter comparisons. Population-normalized payment measures, real non-oil activity and a standardized nonperforming loan alternative are excluded because sufficiently consistent series were not obtained. This limits the controls and the scope of financial vulnerability measured by the study.

Institutional source access points are the CBA statistical bulletin, monetary indicators and payment system indicators (Central Bank of Azerbaijan, n.d.-a, n.d.-b, n.d.-c) and SSC price and tariff indices (State Statistical Committee of the Republic of Azerbaijan, n.d.). The accompanying workbook retains source links, reporting

coverage and construction notes. The analytical sample ends in December 2025; subsequent observations are not used to select or validate the reported models.

4 METHODOLOGY

4.1 Research design and preliminary assessment

The study estimates conditional dynamic associations using observational time-series data. Lagged payment indicators reduce reliance on same-month co-movement but do not eliminate reverse causality, policy endogeneity or omitted influences. Separate equations examine inflation, overdue lending and deposit dollarization. Monetary controls comprise annual nominal M2 growth and the policy rate in the inflation equation, and inflation and the policy rate in the financial vulnerability equations. An extended credit risk specification additionally includes annual nominal credit growth, which may itself represent a transmission channel. The nominal AZN/USD rate has little variation in the principal sample and is not included as an uninformative additional regressor.

Descriptive statistics, time plots and level correlations precede estimation. Augmented Dickey–Fuller (ADF), Phillips–Perron (PP) and Kwiatkowski–Phillips–Schmidt–Shin (KPSS) tests examine persistence in levels and first differences. ADF uses a Bayesian information criterion (BIC) search up to 12 lags, PP uses its automatic bandwidth, and KPSS uses an automatic bandwidth. Intercept specifications are supplemented by intercept-and-trend specifications. Zivot–Andrews tests allow one endogenously located intercept and trend break. ADF and PP test the unit-root null, whereas KPSS tests stationarity. Conflicting outcomes are reported rather than resolved by selecting only the favorable test. Projection on seasonal and intervention indicators provides an additional sensitivity check, whose conventional test probabilities are not interpreted as formal integration-order evidence.

4.2 Preliminary levels specifications

The economic specifications used to screen levels relationships are:

$$INF_t = f(DIG_t, M2G_t, RATE_t) \quad (1)$$

$$ODL_t = f(DIG_t, INF_t, RATE_t\,[, CREDG_t]) \quad (2)$$

$$DOLL_t = f(DIG_t, INF_t, RATE_t) \quad (3)$$

The bracketed variable in (2) belongs only to the extended risk specification. Preliminary ARDL screening uses an unrestricted intercept without a trend, seasonal indicators or dated controls. BIC selects own and explanatory lag orders from one to two on a common holdback of two. The chosen lag orders are then used to fit the corresponding unrestricted error correction model (UECM) with its natural lag loss. Its F-statistic and lagged-outcome t-statistic are obtained from the same specification and sample. The F-statistic is compared with case-3 asymptotic bounds (Pesaran et al., 2001; Kripfganz & Schneider, 2023). These deliberately limited screens are distinct from the seasonally controlled change models and are not treated as definitive cointegration tests.

Bounds inference requires no I(2) variables and an adequate deterministic and stochastic specification. The short sample, conflicting persistence tests and omission of seasonal and dated controls from these screens limit their inferential value. The lagged-outcome t-statistic is descriptive; ordinary t critical values do not constitute a bounds adjustment test. No complete set of adjustment or degeneracy tests, or finite-sample calibration, is supplied. An upper-bound F rejection alone therefore does not justify an equilibrium interpretation. The study reports no cointegrating coefficients or error correction speeds; this choice does not prove that an equilibrium relationship is absent.

4.3 Dynamic change models and exploratory selection

The principal outcome transformations are $y_t = \Delta INF_t$ for inflation, $y_t = 100\Delta\ln(ODL_t)$ for credit risk, and $y_t = \Delta DOLL_t$ for dollarization. An initial ΔODL equation is retained in the diagnostic record but fails the functional-form check. The logarithmic risk transformation is examined in response. Let x_t contain the outcome-specific controls in (1)–(3). Because annual log inflation sums twelve monthly log inflation rates, $\Delta INF_t = INFM_t - INFM_{t-12}$. This overlapping construction can generate seasonal dependence and motivates both the lag-12 diagnostic refinement and the separate monthly-inflation sensitivity equation. The working change equation is:

$$\boldsymbol{y}_t = \boldsymbol{\alpha} + \boldsymbol{\Sigma}_i \in \boldsymbol{L}\, \boldsymbol{\varphi}_i \boldsymbol{y}_{t-i} + \boldsymbol{\Sigma}_{j=1}\boldsymbol{9}\boldsymbol{\beta}_j \boldsymbol{\Delta DIG}_{t-j} + \boldsymbol{\Sigma}_k \boldsymbol{\Sigma}_{j=1}{}^{rk} \boldsymbol{\gamma}_{kj} \boldsymbol{\Delta x}_{k,\,t-j} + \boldsymbol{\delta}' \boldsymbol{Z}_t + \boldsymbol{u}_t \qquad \textbf{(4)}$$

Z contains eleven month indicators, a COVID indicator equal to one during March–December 2020, and step indicators beginning in January 2021 and January 2022. The dated steps are specification sensitivities, not independently established break dates. They absorb intercept shifts in the change equations and therefore allow changes in the slope of the corresponding level paths. They neither identify policy effects nor harmonize reporting coverage. A linear trend in the log-risk change equation defines a separate sensitivity specification; it implies a quadratic component in the log-risk level and is a strong assumption rather than an automatic correction.

Initial BIC selection uses one to two own and explanatory lags on 93 observations. Residual dependence in the inflation equation motivates a seasonal outcome lag at 12 months and an expanded own-lag ceiling of six. For the diagnostic comparison, models share 89 observations and explanatory lag ceilings of two. Within each of the three searches for inflation and log risk with and without a trend, 48 candidates are evaluated. Minimum BIC is selected among candidates with Breusch–Godfrey (BG) LM p-values above 0.05 at lags 1, 4 and 12 and a Ramsey regression specification error test (RESET) p-value using heteroskedasticity-consistent HC3 covariance above 0.05. The dollarization equation retains its initial 93-observation specification. The chosen models and unsuccessful intermediate models are reported. Selection is exploratory; conditional standard errors do not incorporate repeated model-search uncertainty.

4.4 Inference and interpretation

Coefficients are estimated by ordinary least squares. Monthly inference uses Bartlett heteroskedasticity and autocorrelation consistent covariance with bandwidth 12, a small-sample correction and Student t/F reference distributions. Confidence intervals and signed lag-sum tests complement the joint test that all payment coefficients are zero. The latter can reject even if the net signed association is imprecisely estimated. A one-percentage-point increase in DIG is one unit; ΔINF is the monthly change in annual log inflation, not monthly inflation itself. All tests are two-sided unless a diagnostic has its conventional upper-tail rejection rule. Reported p-values and confidence intervals are nominal and conditional on model selection; no multiplicity adjustment is applied. The 5% threshold is descriptive rather than a selection-adjusted measure of evidential strength.

$$\boldsymbol{B} = \boldsymbol{\Sigma}_{\mathrm{j}}\, \boldsymbol{\beta}_{\mathrm{j}}\,; \qquad \boldsymbol{M} = \boldsymbol{B} \,/\, (\mathbf{1} - \boldsymbol{\Sigma}_{\mathrm{i}} \in \boldsymbol{L}\, \boldsymbol{\varphi}_{\mathrm{i}}) \tag{5}$$

B is the signed sum of the payment lags. Under stable fitted dynamics, M is the sum of the fitted response of y to a one-unit pulse in ΔDIG, holding other inputs fixed. Equivalently, it is the steady-state response of y to a sustained one-unit increase in ΔDIG under the maintained equation. A permanent one-percentage-point increase in the level of DIG produces the pulse interpretation. Because y is a change, its accumulated response concerns the corresponding level of INF, 100ln(ODL) or DOLL; for Model I this is the annual inflation rate, not the price level. The own-lag sum includes lag 12 where retained. Delta-method confidence intervals use the HAC covariance and a Student t critical value. These conditional summaries are neither cointegrating coefficients nor causal policy effects, and a sustained rise in a bounded payment share is only a local analytical scenario. Companion-matrix eigenvalue moduli assess fitted dynamic stability.

4.5 Diagnostics and stability

Breusch–Godfrey tests assess residual dependence, Breusch–Pagan tests assess heteroskedasticity, autoregressive conditional heteroskedasticity (ARCH) LM tests at lags 4 and 12 assess conditional variance dependence, and HC3 RESET evaluates functional form. Jarque–Bera and OLS residual CUSUM provide distributional and stability diagnostics. HAC covariance does not substitute for an adequate dynamic specification. Supplementary recursive cumulative sum (CUSUM) and cumulative sum of squares (CUSUMSQ) checks use a fixed-design Gaussian Monte Carlo calibration with 1,999 draws and seed 20261024. The earliest full-rank prefix must contain all dated indicators, leaving 47 recursive innovations from February 2022 to December 2025. These conditional checks are not conventional full-sample recursive bands or a dynamic bootstrap.

4.6 Robustness and reproducibility

Sensitivity analysis substitutes the cashless count share or ln(POS), examines monthly rather than annual inflation, adds credit growth to the log-risk equation, compares the Akaike information criterion (AIC) with BIC, restricts the sample after 2022, and excludes 2020-dependent observations. Lags are constructed before exclusions to preserve calendar spacing. The strict inflation exclusion also removes observations whose twelfth outcome lag falls in 2020. Coincident dated controls are reduced where needed for full rank. Most alternatives re-run the initial two-lag search, so their different samples and diagnostic failures are explicitly disclosed. Monthly HAC bandwidths 4, 8 and 12 are compared.

Quarterly sensitivity models use one outcome and explanatory lag, quarter indicators and HAC bandwidth 4, with 30 usable observations. Their limited information content restricts interpretation. Computation uses Python 3.12, statsmodels 0.15.0, arch 8.0.0 and NumPy/SciPy. Workbook cached values are checked against the raw payment construction, and independent least-squares calculations verify the selected coefficients. Appendix C records the computational scope and the supplementary replication files. The replication script verifies the four selected regressions and the corrected preliminary bounds screen; it does not independently reproduce the entire exploratory search. No simulated observations are inserted into the estimation dataset.

5 Results and Discussion

The estimates below were calculated from the assembled Excel workbook. They provide evidence of rapid payment digitization and declining observed financial vulnerability, but do not establish a stable causal effect of digitization. Long-run cointegration is not claimed. Model adjustments prompted by stationarity and diagnostic results are disclosed explicitly.

5.1 Sample coverage and estimation route

The principal sample contains 96 monthly observations from January 2018 to December 2025. All core variables used in the monthly models are observed throughout this period. The earlier 2014–2017 cashless payment series is not concatenated with DIG because electronic-commerce coverage is not consistently reported in those tables. Internet and mobile banking transaction counts begin later and are not used to shorten the principal sample.

The analysis first evaluates integration properties and preliminary levels relationships. Because unit-root evidence is mixed and an acceptable equilibrium relationship is not established, the main regressions use changes rather than interpreting levels coefficients as long-run effects. In the annual-inflation equations, the outcome is ΔINF, the month-to-month change in the twelve-month log inflation rate. Credit risk is examined both as ΔODL and, after a functional-form failure, as 100Δln(ODL). Dollarization is modeled as ΔDOLL.

Table 2 Sample and implementation

Item	Implementation
Original monthly sample	2018M01–2025M12; 96 observations
Initial differenced models	2018M04–2025M12; 93 observations
Diagnostic monthly comparison	2018M08–2025M12; 89 observations; common lag holdback of six
Quarterly sensitivity	2018Q1–2025Q4; 32 observations, 30 after differencing and one lag
Monetary controls	Changes in annual M2 growth and/or the end-of-month policy rate
Fixed controls	Eleven month indicators; COVID = 2020M03–2020M12; steps from 2021M01 and 2022M01
Inference	OLS point estimates; Bartlett HAC covariance, bandwidth 12, small-sample correction and t/F reference distributions
Selection	Initial BIC search over own and explanatory lags 1–2; subsequent diagnostic search over own lags 1–6 and explanatory lags 1–2

Note: created by the author

The reported model refinements are data-informed rather than a preregistered confirmatory exercise. Conditional standard errors do not account for lag selection or repeated specification checks. Statistical findings are consequently interpreted cautiously, especially where signs or significance change across alternatives.

5.2 Descriptive patterns

The cashless share by value rose from 17.375% in January 2018 to 67.754% in December 2025. Over the same endpoints, ODL declined from 14.549% to 1.642%, while deposit dollarization fell from 73.668% to 36.832%. These common time patterns motivate the study but cannot distinguish digital adoption from banking-sector restructuring, policy changes or other concurrent developments. Inflation followed a different, nonmonotonic path.

Table 3 Descriptive statistics for the 96-month principal sample

Variable	Mean	SD	Minimum	Median	Maximum
INF	5.371	4.069	-0.214	3.867	14.479
ODL	5.920	4.359	1.533	4.150	14.712
DOLL	52.237	10.609	36.832	51.124	73.668
DIG	40.022	19.227	14.125	37.374	68.530
DIGN	69.870	18.952	35.017	73.048	93.225
M2G	14.561	6.255	1.278	16.065	25.105
CREDG	9.932	12.124	-35.989	13.643	21.905
RATE	7.984	1.520	6.250	7.500	15.000

Note: created by the author

All entries are in percent or percentage points as defined in Methodology. INF, M2G and CREDG are log percentage changes. DIG_N is labeled DIGN in the calculation scripts. The annual inflation peak is 14.479 log percent; this is not numerically identical to a conventional percentage change.

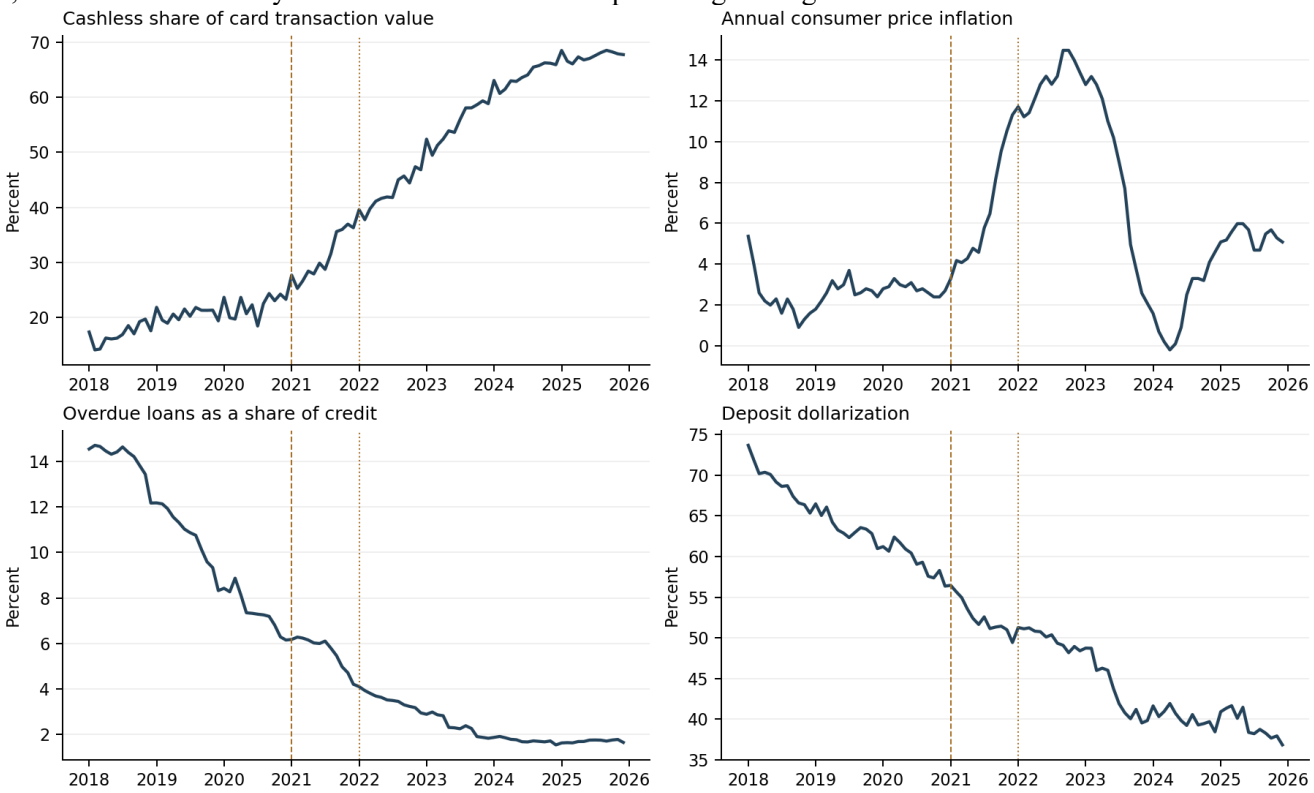


Figure 1 Payment digitization and macrofinancial outcomes

Monthly observations, 2018–2025. Dashed and dotted lines mark January 2021 and January 2022, the dates used for step indicators. These are sensitivity dates, not independently established reporting or causal breaks. Source: author calculations from the CBA and SSC series in the accompanying workbook.

Table 4 Correlations in levels

Variable	INF	ODL	DOLL	DIG	M2G	CREDG	RATE
INF	1.000	-0.411	-0.281	0.240	0.346	0.297	-0.070
ODL	-0.411	1.000	0.963	-0.891	0.194	-0.661	0.593
DOLL	-0.281	0.963	1.000	-0.961	0.276	-0.605	0.504
DIG	0.240	-0.891	-0.961	1.000	-0.377	0.547	-0.317
M2G	0.346	0.194	0.276	-0.377	1.000	0.065	0.134
CREDG	0.297	-0.661	-0.605	0.547	0.065	1.000	-0.549
RATE	-0.070	0.593	0.504	-0.317	0.134	-0.549	1.000

Note: created by the author

The level correlation of DIG with ODL is -0.891, and its correlation with DOLL is -0.961. These correlations are not interpreted as marginal effects. The stationarity results below show why inference from a static levels regression would be unreliable.

5.3 Stationarity and breaks

ADF tests use a BIC lag search up to 12 months. PP tests use the default automatic bandwidth, which equals 12 in these samples; KPSS uses an automatic bandwidth. Table 5 reports intercept specifications in levels and first differences. Appendix A provides trend specifications and the selected ADF lags.

Table 5 Unit-root and stationarity tests

Variable	ADF level	ADF p	PP p level	KPSS level	ADF Δ	ADF p Δ	KPSS Δ
INF	-1.666	0.449	0.380	0.360	-5.270	<0.001	0.123
ODL	-3.051	0.030	0.032	1.567	-8.145	<0.001	0.861
DOLL	-1.526	0.521	0.354	1.685	-12.732	<0.001	0.183

DIG	-1.274	0.641	0.968	1.664	-1.042	0.738	0.230
DIGN	-2.809	0.057	0.734	1.673	0.144	0.969	0.206
M2G	-2.345	0.158	0.144	0.460	-8.719	<0.001	0.058
CREDG	-3.836	0.003	0.001	0.710	-3.499	0.008	0.425
RATE	-1.599	0.484	<0.001	0.558	-6.629	<0.001	0.567
INFM	-4.457	<0.001	<0.001	0.214	-10.883	<0.001	0.028
LNPOS	4.045	1.000	1.000	1.280	-7.068	<0.001	1.079
LOGODL	-0.911	0.784	0.789	1.681	-9.164	<0.001	0.227

Note: created by the author

ADF/PP H_0: unit root; KPSS H_0: stationarity. KPSS 5% critical value for an intercept is 0.463; the lookup p-values are bounded between 0.01 and 0.10. Δ denotes first difference. INFM is monthly log inflation; LNPOS is ln(POS); LOGODL is 100ln(ODL), so its first difference is the modeled log-risk growth rate.

Inflation in levels does not reject a unit root in the intercept ADF test ($p = 0.449$), whereas KPSS does not reject level stationarity at 5%. ODL rejects a unit root with an intercept (ADF $p = 0.030$), but KPSS strongly rejects stationarity and the trend ADF specification does not reject a unit root. DOLL and DIG also display substantial persistence. These results do not support an unqualified classification of every variable as I(0) or I(1).

First differences of INF, DOLL and M2G reject the ADF unit-root null strongly. For ΔDIG and ΔDIG_N, the twelve-lag ADF search retains long seasonal dynamics and fails to reject, while PP rejects strongly and KPSS does not reject stationarity. For ΔODL, ΔRATE and Δln(POS), KPSS remains above its 5% threshold. Deterministic seasonality and changing means therefore remain relevant after differencing; differencing alone is not treated as proof of stationarity.

Table 6 Zivot–Andrews break tests with intercept and trend

Variable	Statistic	p-value	ADF lags	Estimated break
INF	-3.542	0.791	1	2023-06
ODL	-2.796	0.984	0	2020-03
DOLL	-5.579	0.010	0	2023-05
DIG	-3.225	0.917	12	2024-10
DIGN	-1.982	0.999	11	2022-12
M2G	-4.457	0.232	0	2021-02
RATE	-2.934	0.973	3	2021-08
LNPOS	-3.604	0.758	0	2020-03

Note: created by the author

The Zivot–Andrews test rejects the unit-root null for DOLL at approximately 1% and locates a break in May 2023; it does not reject for INF, ODL or DIG. This result is compatible with dollarization being stationary around a changing deterministic path, but one test with an estimated break does not settle its integration order. The break is a statistical estimate and is not an identified policy effect.

Projecting differenced variables on monthly and dated indicators yields lower KPSS statistics for several series (Appendix A). Because these deterministic components are estimated before testing, ordinary reference probabilities are not formally valid for the resulting residual series. These calculations describe sensitivity to deterministic adjustment; they do not establish stationarity or validate the change equations.

5.4 Preliminary bounds screening

Preliminary ARDL screening uses lagged explanatory variables and an unrestricted intercept, with lag orders one to two selected by BIC on a common holdback of two. The reported case-3 F-statistic, descriptive t-statistic and sample size then refer to the same fitted UECM with natural lag loss. Seasonal and dated controls are omitted from these deliberately restricted screens, limiting their inferential value relative to the later change specifications.

Table 7 Preliminary levels relationship screening

Outcome	p; q orders	N	F	5% I(0)	5% I(1)	t on Y_{t-1}	F decision
INF	2; 1/1/1	94	2.102	2.877	4.010	-1.119	Below lower bound
ODL	1; 1/1/1	95	3.920	2.877	4.010	-2.032	Inconclusive
ODL_EXT	1; 1/1/1/1	95	4.511	2.637	3.791	-2.163	Above upper bound

DOLL	1; 1/1/1	95	2.279	2.877	4.010	-2.506	Below lower bound

Note: created by the author

Explanatory order: INF: DIG, M2G, RATE; ODL/DOLL: DIG, INF, RATE; ODL_EXT additionally CREDG. Lag selection uses an intercept and holdback two; reported F, t and N use the same final UECM with its natural holdback and no seasonal or dated indicators. Bounds are asymptotic case-3 reference values. The descriptive t-statistic is not compared with ordinary t critical values to establish adjustment.

The corrected INF and DOLL F-statistics fall below the 5% lower bound, ODL lies between the bounds, and ODL_EXT exceeds the upper bound. The extended-risk equation's lagged-outcome t-statistic is −2.163, obtained from the same unseasonal specification as its F-statistic. This value is descriptive because supplementary bounds adjustment and degeneracy tests are not provided. The restricted deterministic specification, conflicting persistence evidence and absence of corroboration across equations prevent a defensible equilibrium claim. These screens are not evidence that no equilibrium exists; the study confines its reported estimates to change equations.

5.5 Monthly change regressions

The initial BIC-selected ΔINF model exhibits residual dependence at 12 months (BG LM $p = 0.007$). Adding the twelfth lag of ΔINF is not sufficient on its own ($p = 0.011$). The ΔODL model fails RESET strongly ($p < 0.001$), so an additional logarithmic risk specification is examined. These unsuccessful models are retained in the diagnostic record rather than silently discarded.

For the expanded monthly comparison, all candidates use 89 observations, August 2018–December 2025. The own lag ceiling is expanded to six, while explanatory lag ceilings remain at two. Among models with BG LM $p > 0.05$ at lags 1, 4 and 12 and HC3 RESET $p > 0.05$, the minimum-BIC model is selected. This yields 3 admissible inflation candidates, 19 log-risk candidates without a trend and 5 with a trend, out of 48 in each search. The inflation model retains own lags 1–5, a seasonal lag at 12, and two policy-rate lags.

Model I explains ΔINF; Model R explains 100Δln(ODL); Model RT adds a linear trend to that change equation; Model D explains ΔDOLL and retains the original two-lag selection on 93 observations. RT is a sensitivity specification: a trend in changes is a strong deterministic assumption and is not presented as an automatic correction.

Table 8 Dynamic coefficients of the monthly change models

Regressor	I Inflation	R Credit risk	RT Risk with trend	D Dollarization
Constant	-0.194 (0.289)	-0.711 (1.406)	-1.743 (1.369)	0.631 (0.323)
Linear trend	—	—	0.094 (0.029)	—
Own dependent variable L1	0.319 (0.124)	-0.046 (0.116)	-0.075 (0.096)	-0.234 (0.105)
Own dependent variable L2	0.067 (0.138)	—	—	—
Own dependent variable L3	0.091 (0.133)	—	—	—
Own dependent variable L4	0.126 (0.095)	—	—	—
Own dependent variable L5	0.229 (0.093)	—	—	—
ΔDIG L1	0.105 (0.060)	-0.214 (0.506)	0.316 (0.518)	-0.021 (0.098)
ΔDIG L2	—	-0.902 (0.356)	—	—
ΔM2G L1	0.006 (0.025)	—	—	—
ΔINF L1	—	1.260 (0.796)	1.086 (0.865)	-0.195 (0.160)
ΔINF L2	—	—	—	0.391 (0.203)
ΔRATE L1	-0.418	-3.049	-2.456	-0.612

	(0.305)	(1.975)	(2.244)	(0.460)
ΔRATE L2	-1.079 (0.344)	—	—	—
ΔINF L12	-0.286 (0.085)	—	—	—
COVID indicator	0.236 (0.137)	0.861 (0.796)	-0.570 (0.956)	0.221 (0.207)
Step from 2021M01	0.667 (0.157)	0.514 (1.265)	-3.113 (1.761)	-0.155 (0.229)
Step from 2022M01	-0.406 (0.135)	1.639 (1.135)	-0.690 (1.489)	0.471 (0.226)
Observations	89	89	89	93
Parameters including fixed controls	25	20	20	20
R^2	0.555	0.419	0.423	0.417
Adjusted R^2	0.389	0.259	0.264	0.265
BIC	232.070	559.003	558.487	308.406
Maximum root modulus	0.958	0.046	0.075	0.234

Note: created by the author

HAC standard errors in parentheses, bandwidth 12 and finite-sample correction. All models include eleven monthly indicators; their coefficients are reported in Appendix B. “Own dependent” refers to the transformed outcome named in the text. Model D has 93 observations; I, R and RT share 89. BIC values must not be compared across different dependent variables or sample sizes.

In Model I, a one-percentage-point increase in the previous month’s change in the cashless share is associated with a 0.105 percentage-point increase in the current change in annual log inflation, conditional on the included regressors (HAC p = 0.085; nominal 95% CI −0.015 to 0.226). This positive estimate is imprecise and provides no robust evidence of a disinflationary association. The interval is conditional on the exploratory specification search and is not a selection-adjusted confidence interval.

Table 9 Tests and magnitudes for digitization terms

Statistic	**I Inflation**	**R Credit risk**	**RT Risk with trend**	**D Dollarization**
Sum of DIG lag coefficients	0.105	-1.116	0.316	-0.021
95% CI for lag sum	-0.015 to 0.226	-2.419 to 0.186	-0.716 to 1.349	-0.217 to 0.175
p-value for lag sum	0.085	0.092	0.543	0.832
Joint F-test p for all DIG lags	0.085	0.046	0.543	0.832
Cumulative dynamic multiplier	0.232	-1.067	0.294	-0.017
95% CI for multiplier	-0.133 to 0.597	-2.362 to 0.227	-0.661 to 1.249	-0.177 to 0.143

Note: created by the author

The signed lag sum describes the payment coefficients before feedback through the outcome’s own lags. The multiplier additionally divides that sum by one minus the sum of all own-lag coefficients, including lag 12 in Model I. It summarizes the accumulated fitted response of the change outcome to a one-unit ΔDIG pulse under the maintained model. Its definition and units follow Section 4.4. It does not estimate a cointegrating coefficient or a permanent causal price-level effect.

In Model R, the two digitization lags are jointly significant at 5% (p = 0.046), but their sum is -1.116 with a confidence interval of -2.419 to 0.186. The signed lag sum is not significant at 5% (p = 0.092). Joint significance of individual lags must therefore not be equated with a precisely estimated net reduction in credit risk.

Adding a trend changes the risk estimate to 0.316 (p = 0.543) and reverses its sign. The trend specification has a slightly lower BIC than R on the same sample. This sensitivity substantially weakens a claim that digitization consistently improves credit quality. In Model D, the cashless-value-share terms are not jointly significant (p = 0.832).

5.6 Diagnostics and stability

Table 10 Diagnostic p-values

Test	I- Inflation	R- Credit risk	RT- Risk with trend	D- Dollarization
BG LM lag 1	0.058	0.074	0.403	0.585
BG LM lag 4	0.160	0.393	0.760	0.875
BG LM lag 12	0.087	0.123	0.052	0.530
Breusch–Pagan LM	0.031	0.225	0.133	0.819
ARCH LM lag 4	0.934	0.926	0.844	0.784
ARCH LM lag 12	0.322	0.895	0.936	0.954
HC3 RESET squared fitted value	0.414	0.249	0.532	0.630
Jarque–Bera	0.278	<0.001	<0.001	0.821
OLS residual CUSUM	0.982	0.715	0.962	0.959

Note: created by the author

The selected inflation and log-risk models do not reject the stated BG and RESET nulls at 5%; these same tests were used during selection, so this outcome is not independent confirmation of model adequacy. Several BG probabilities remain close to 0.05. The inflation residuals reject homoskedasticity (Breusch–Pagan p = 0.031), and the log-risk equations strongly reject normality. HAC covariance accommodates some heteroskedasticity and dependence but does not remedy misspecification, selection bias or unreliable small-sample reference distributions. Jarque–Bera probabilities are asymptotic distributional diagnostics.

Table 11 Supplementary recursive stability checks

Model	Recursive period	CUSUM MC p	CUSUMSQ MC p
I Inflation	2022M02–2025M12	0.433	0.196
R Credit risk	2022M02–2025M12	0.105	0.304
RT Risk with trend	2022M02–2025M12	0.049	0.406
D Dollarization	2022M02–2025M12	0.265	0.500

Note: created by the author

These are conditional fixed-design Gaussian Monte Carlo checks, 1,999 draws, seed 20261024. They are not the usual tabulated recursive-CUSUM critical bands. The initial prefix must contain all dated indicators at full rank, leaving 47 recursive innovations from February 2022. They cannot establish stability before that date; lagged endogenous regressors and heteroskedasticity further limit the conditional calibration.

OLS residual CUSUM does not reject at 5% in the four models. The supplementary recursive exercise is less reassuring for RT, whose CUSUM Monte Carlo p-value is 0.049. No general claim that all coefficients are stable across the entire sample is made.

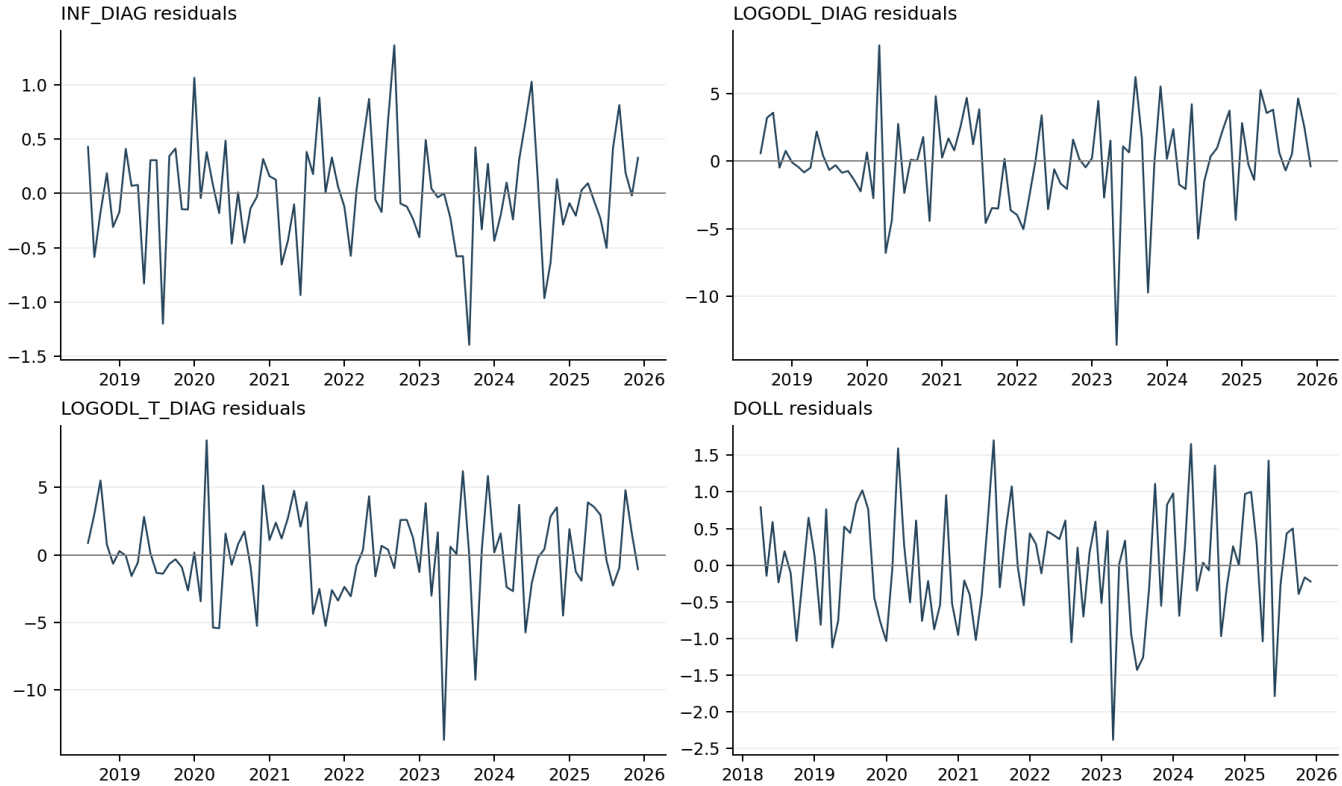


Figure 2 Residuals from the principal monthly regressions

Panels correspond to I, R, RT and D. Risk-model residuals are in units of 100Δln(ODL), whereas inflation and dollarization residuals are in percentage points.

5.7 Robustness and sensitivity

Sensitivity estimates below re-run the stated BIC or AIC selection under each alteration. Except where noted, they use the initial two-lag search and the corresponding 93-observation sample. They are not all estimated on the diagnostic 89-observation sample, so differences reflect both specification and sample changes. The log-risk checks use the untrended risk specification; the trend comparison remains a central countercheck.

Table 12 Alternative specifications

Specification	N	Proxy lag sum	Joint proxy p	BG12 p	RESET p
INF_DIGN	93	0.058	0.251	0.017	0.329
INF_LNPOS	93	2.717	0.270	0.005	0.363
INF_AIC	93	0.099	0.154	0.003	0.396
INF_POST2022	48	0.112	0.411	0.016	0.774
INF exclude all 2020 inputs	69	-0.154	0.002	0.020	0.561
LOGODL_DIGN	93	-0.203	0.406	0.137	0.138
LOGODL_LNPOS	93	45.533	<0.001	0.521	0.988
LOGODL_AIC	93	-1.081	0.038	0.094	0.164
LOGODL_NO2020	79	-0.953	0.021	0.014	0.495
LOGODL_POST2022	48	1.414	0.116	0.008	0.221
LOGODL_LAG1	93	0.197	0.688	0.155	0.191
LOGODL_EXT	93	-1.060	0.040	0.059	0.195
DOLL_DIGN	93	-0.131	0.025	0.428	0.767
DOLL_LNPOS	93	2.870	0.276	0.453	0.535
DOLL_AIC	93	-0.017	0.866	0.598	0.184
DOLL_NO2020	79	0.100	0.190	0.672	0.917
DOLL_POST2022	48	0.165	0.273	0.135	0.907
INFM	93	0.059	0.311	0.549	0.194

Note: created by the author

DIGN = cashless transaction count share; LNPOS = ln(POS). The POS coefficients are not on the same scale as DIG coefficients. “NO2020” excludes 2020 outcomes and rows whose short lags use 2020 observations. The strict inflation exclusion also removes observations requiring its twelfth lag from 2020; coincident step controls are reduced to retain full rank. POST2022 is a short 48-month sensitivity, not proof of harmonization. INFM uses monthly log inflation in levels as the outcome, with differenced explanatory variables.

The monthly-inflation sensitivity equation gives an imprecise digital-value-share association ($p = 0.311$). Excluding all 2020-dependent inputs from the annual-inflation equation produces a negative estimate ($p = 0.002$), but its BG12 test rejects no residual serial correlation ($p = 0.020$). Its smaller p-value therefore cannot establish a superior or robust disinflationary result. Both the sign reversal and diagnostic failure reinforce the dependence on sample and specification.

For credit risk, the transaction-count proxy is not significant, while the POS infrastructure proxy gives a positive association. The negative relationship also disappears with a one-lag ceiling and reverses in the short post-2022 sample. Adding credit growth retains joint significance in the initial untrended log-risk model, but does not resolve the trend, proxy and bandwidth sensitivity. The negative count-share coefficient in the dollarization equation (joint $p = 0.025$) is isolated among several alternatives and does not establish a general effect of digitization.

Table 13 HAC bandwidth sensitivity for selected diagnostic models

Model	Bandwidth 4 p	Bandwidth 8 p	Bandwidth 12 p
I- Inflation	0.042	0.074	0.085
R- Credit risk	0.059	0.055	0.046
RT- Risk with trend	0.528	0.552	0.543

Note: created by the author

The inflation digitization term is significant at 5% with bandwidth 4 but not with bandwidths 8 or 12. The untrended log-risk joint test becomes significant only at bandwidth 12. The trend specification remains insignificant across all three. These comparisons show that borderline significance should not anchor the substantive conclusion.

Table 14 Quarterly sensitivity estimates

Outcome	N	ΔDIG L1	HAC SE	p-value	BG4 p
ΔINF	30	-0.036	0.142	0.804	0.522
ΔLOGODL	30	-2.232	1.416	0.129	0.189
ΔDOLL	30	-0.041	0.133	0.760	0.015

Note: created by the author

Quarterly change models cover 2018Q3–2025Q4 and include one outcome lag, one lag of each explanatory change, quarterly indicators and HAC bandwidth 4. $\Delta LOGODL = 100\Delta\ln(ODL)$; ΔINF and ΔDOLL are first differences of the corresponding quarterly annual inflation rate and dollarization share. The short sample precludes a comparable set of dated controls.

None of the quarterly digitization coefficients is significant at 5%. The dollarization equation also fails its fourth-order BG test. Quarterly results therefore provide no strong corroboration of a stable digitization effect, while their limited power prevents a conclusion that the true effect must be zero.

5.8 Interpretation of the empirical evidence

Table 15 Evidence summary

Research proposition	Assessment from the estimates
Digitization is associated with lower inflation	Not robustly supported. The principal estimate is positive and insignificant at 5%; its sign changes in exclusion checks.
Digitization improves credit quality	Inconclusive. Some untrended lag terms are jointly significant, but the signed lag sum includes zero and the trend/proxy/lag checks do not support a stable negative effect.
Digitization reduces deposit dollarization	Not supported by the principal value-share model; an isolated count-share result requires caution.
A long-run equilibrium relationship exists	Not established across the relevant tests and specifications; ECM and cointegrating coefficients are not reported.
No economic effect exists	Not demonstrated. Sample length, measurement changes, omitted

	controls and model uncertainty limit power and identification.

The main empirical finding is therefore a distinction between strong descriptive co-movement and weakly identified dynamic effects. Azerbaijan experienced a substantial shift toward cashless payments while measured overdue lending and dollarization declined. Once persistence, seasonality, interventions and alternative specifications are considered, this dataset does not provide sufficiently stable evidence to attribute the improvement in those outcomes to digital payments. Policy conclusions should reflect the observed transition without assigning a quantified causal benefit that the estimates do not identify.

The inflation estimates do not establish a disinflationary payment channel. This is compatible with the ambiguity emphasized by the expenditure evidence of Agarwal et al. (2024) and the transmission results of Hasan et al. (2024), but the present regressions do not identify either mechanism directly. The weakly estimated positive association also cannot be treated as confirmation of the inflationary relationships reported by Prabheesh et al. (2024). Differences in outcomes, periods and identification prevent a direct causal comparison.

The Azerbaijan evidence in Gulaliyev et al. (2025) further suggests that fiscal and labour-market conditions deserve explicit consideration. Their annual inflation equations cannot be directly compared with the present monthly ΔINF equation, and their findings do not establish that the payment coefficient is unbiased. Incomplete coverage of those covariates remains a substantive limitation of the present estimates.

For financial vulnerability, the instability of the net credit-risk association qualifies the favorable bank-stability findings of Daud et al. (2022) and Safiullah and Paramati (2024), while reinforcing the importance of context visible in Khan et al. (2023). These studies measure bank-level stability rather than the same national overdue ratio. Similarly, the dollarization result does not identify the currency-competition mechanisms modeled by Cong and Mayer (2025). The common implication is that payment adoption and broader financial resilience require separate measurement and evaluation.

6 Policy Implications

The estimates support a measured approach to digital financial development in Azerbaijan. Observable progress in payment adoption should be distinguished from macroeconomic benefits that remain uncertain. The following recommendations combine implications of the results with proposed monitoring and evaluation measures; those proposals are not effects identified by the regressions.

6.1 Monetary policy and inflation monitoring

The principal inflation model provides no robust evidence that increasing the cashless payment share reduces inflation. Its digitization coefficient is positive and insignificant at the 5% level under the principal HAC specification ($p = 0.085$), while alternative samples and bandwidths change the inference. These findings do not justify treating cashless payment expansion as a substitute for monetary stabilization or assigning it a numerical disinflation target. Decisions on the policy rate and liquidity conditions should continue to be based on a broader assessment of inflation, economic activity and financial conditions.

Digital payment information can instead be evaluated as an additional source of timely economic information. Transaction values, counts and merchant activity could supplement conventional indicators of spending, provided their changing coverage is documented. The appropriate test is whether these data improve inflation forecasts beyond established benchmarks. A rolling forecasting exercise should compare models with and without payment indicators using information available at each forecast date and reserve later observations for evaluation. Until a consistent forecasting gain is demonstrated, the present association estimates offer insufficient grounds for changing the monetary policy response to a given inflation development.

6.2 Credit supervision alongside digital adoption

The credit risk results provide a reason for caution about automatic prudential benefits. Although the digitization lags are jointly significant in the untrended logarithmic risk model ($p = 0.046$), their signed sum is not significant at 5% ($p = 0.092$), and its confidence interval includes zero. Adding a trend reverses the sign and removes significance; the count-based and infrastructure proxies also produce different conclusions. Payment adoption therefore cannot be used as evidence that credit screening has improved sufficiently to relax underwriting or supervisory scrutiny.

Banks and supervisors should assess the relationship between digital use and loan performance at the borrower, product and institution levels. Proposed monitoring should distinguish payment activity from digital credit origination and track repayment performance by lending vintage. Changes in overdue ratios should also be decomposed into new arrears, recoveries, write-offs, portfolio growth and changes in reporting coverage. A declining aggregate overdue ratio can otherwise be mistaken for an improvement generated by digital services. Any use of transaction histories in credit assessment should be evaluated against realized repayment outcomes

and the quality and permitted use of the underlying information. The aggregate models do not establish the effectiveness of such scoring tools.

6.3 Dollarization and confidence in domestic currency

The principal value-share model does not identify a statistically reliable relationship between digitization and changes in deposit dollarization ($p = 0.832$). The negative result obtained with the transaction-count proxy is not consistently reproduced across specifications. Authorities should consequently avoid attributing the observed decline in dollarization to digital payment growth or assuming that a higher cashless share will independently sustain that decline.

Measures intended to support domestic-currency intermediation should be assessed through their effects on confidence, saving choices and currency risk, rather than through payment adoption alone. Dollarization monitoring should distinguish depositors and maturities and, where relevant, separate currency valuation effects from net deposit flows. The study does not quantify the effect of any specific deposit incentive or currency restriction.

6.4 Payment development and service quality

The large observed increase in cashless use supports evaluating how payment services are used and how well they function. It does not establish that further expansion will necessarily lower prices or improve financial stability. Performance assessment should extend beyond the number of cards and terminals to active use, transaction costs, service availability and the ability of households and small merchants to participate. These are proposed operational objectives, not benefits estimated by the regressions.

A practical evaluation framework could track active merchant acceptance, payment completion rates, outages, user complaints and reported fraud losses alongside transaction counts and values. Results should be disaggregated geographically and by merchant size where data permit. Financial literacy and merchant assistance initiatives can be piloted with explicit cost and participation measures before wider implementation. The present aggregate dataset does not measure access barriers, cybersecurity exposure or distributional effects; interventions in these areas require additional evidence rather than an assumption that a growing cashless share guarantees inclusive or safe use.

6.5 Statistical harmonization as an immediate priority

The most direct policy implication concerns the quality of the information used to assess digital transformation. Reporting changes limit comparability before 2018 and remain relevant within the principal sample. In addition, the overdue loan measure covers credit institutions and is not equivalent to the banking sector nonperforming loan ratio. Policy dashboards should preserve these distinctions so that changes in statistical definitions are not interpreted as improvements in behavior or risk.

A coordinated statistical agenda involving the Central Bank of Azerbaijan, the State Statistical Committee and reporting institutions should prioritize consistent historical definitions. Payment releases should identify domestic and cross-border operations, withdrawals and cashless payments, individual channels and changes in institutional coverage. Where feasible, revised historical series and overlap-period comparisons should be published with revision histories. Financial data should separately document overdue lending, nonperforming loans, write-offs and institutional coverage. Consistent real activity, external price and population series would also support a better specified assessment of the links between digitization, inflation and financial conditions.

6.6 Evaluation before scaling and claims about outcomes

The sensitivity of the estimates favors staged policy evaluation over ambitious claims based on aggregate correlations. Merchant support or payment adoption initiatives could be introduced as pilots with clearly specified outcomes and a credible comparison group. Where feasible, randomized assignment can identify effects; otherwise, staggered implementation may support a carefully designed comparison of treated and untreated groups. Such designs must examine pre-intervention trends, spillovers and concurrent policy changes. The current time-series results do not themselves validate any of these identification assumptions.

Adoption, prices and credit performance should be treated as separate outcomes. An initiative may improve access or payment convenience without materially affecting inflation or arrears. Evaluation should report implementation costs, uncertainty and unintended effects as well as take-up. Because the present evidence does not establish a stable long-run equilibrium, estimated change-model multipliers should not be converted into guaranteed permanent policy benefits. The appropriate policy position is to support useful payment development while requiring stronger evidence before claiming quantified improvements in price stability or financial resilience.

7 LIMITATIONS OF THE RESEARCH

This study documents payment digitization and its conditional associations with inflation, overdue lending and deposit dollarization in Azerbaijan. The findings are subject to limitations in sample coverage, measurement and econometric identification. These limitations constrain the precision of the estimates and the interpretation of digitization as a determinant of price stability or financial resilience.

7.1 Sample coverage and statistical power

The principal sample comprises 96 monthly observations from January 2018 to December 2025. Differencing and lag requirements reduce the expanded monthly comparison to 89 observations; the quarterly sensitivity analysis contains only 30 usable observations. Estimating dynamic coefficients alongside seasonal and intervention indicators leaves limited information for distinguishing small effects from sampling variation. Consequently, failure to reject a zero coefficient does not establish that the economic effect is absent. The sample also spans substantial economic and reporting changes, making it difficult to distinguish persistent relationships from developments specific to this period.

7.2 Measurement and comparability of the indicators

Digitization is proxied primarily by the cashless share of domestic card transaction value. This measure captures a particular payment channel rather than the full digital transformation of finance or the wider economy. It does not directly measure digital lending, institutional technology adoption, financial access or service quality. Changes in the composition and size of transactions can alter the value share without an equivalent change in participation. Differences between results using transaction counts and POS infrastructure indicate that the proxies are not interchangeable.

Reporting differences prevent a defensible concatenation of the pre-2018 payment series with the principal indicator, and dated controls cannot fully harmonize changes in coverage. Internet and mobile banking transaction counts are available only for 2022–2025 and therefore cannot provide a comparable full-sample alternative. The CPI series is reconstructed from rounded official monthly changes, introducing rounding uncertainty into calculated inflation. The policy rate is observed at month-end and may not represent the monetary conditions prevailing throughout the month.

The overdue lending ratio covers credit institutions and is not equivalent to a standardized banking-sector nonperforming loan ratio. Its movement can reflect loan growth, recoveries, write-offs and changes in institutional coverage as well as repayment performance. Deposit dollarization captures the currency composition of deposits rather than financial stability as a whole. Capital adequacy, liquidity resilience, bank profitability, payment outages and cyber losses are outside the empirical measurement framework. The findings therefore concern selected vulnerability indicators and cannot establish the overall stability of the financial system. The multidimensional banking stability framework of Gulaliyev et al. (2019), which also includes capital adequacy, profitability and liquidity, provides a concrete benchmark for the broader measurement needed in future work.

7.3 Identification and omitted influences

The models estimate conditional time-series associations without an instrument, a natural experiment or another source of plausibly exogenous variation in digitization. Lagged payment indicators do not eliminate reverse causality: inflation and financial conditions may themselves influence payment behavior. The dataset also lacks a reliable real activity control, while external prices, exchange-rate expectations, fiscal developments and banking-sector restructuring are not comprehensively represented. Their omission may affect the estimated digitization coefficients. The COVID and dated step indicators absorb broad shifts but do not identify their causes or fully control for simultaneous events.

7.4 Persistence and model uncertainty

Unit-root and stationarity tests provide mixed evidence and have limited power in a short sample with breaks and seasonality. Preliminary bounds tests do not establish a sufficiently defensible equilibrium relationship across specifications. The change models consequently describe short-run dynamics; they do not identify a stable long-run digitization coefficient or an error correction mechanism. Differencing may also remove information about persistent relationships that a longer and more consistent dataset could reveal. Differencing is a working transformation and does not by itself ensure that every regression input is stationary. The deliberately restricted preliminary bounds screens cannot resolve this uncertainty.

Lag orders, functional forms and diagnostic refinements were chosen using the observed data. Reported confidence intervals and p-values are conditional on those choices and do not incorporate uncertainty from repeated specification searches. HAC inference addresses certain forms of residual dependence and heteroskedasticity, but remains approximate in a small sample. Some sensitivity models fail residual diagnostics, while signs and significance vary across proxies, bandwidths and sample restrictions. In particular, joint

significance of the digitization lags in the untrended credit risk model does not establish a significant negative net effect. The supplementary stability simulations condition on the estimated design and cover only the later recursive evaluation period; they do not demonstrate stability over the entire sample. The diagnostic filter uses the same data as estimation, and the many reported comparisons have no family-wise error control. Borderline nominal significance should therefore receive little weight without independent replication or a prespecified validation sample.

7.5 Scope and further research

Aggregate national data conceal differences across banks, borrowers, merchants and regions. The findings cannot determine who benefits from digital adoption or whether effects differ by income, location or access to financial services. Their transferability to other countries or monetary settings is also uncertain. Future research should prioritize harmonized historical payment statistics, broader measures of financial resilience, real activity and external price controls, and institution- or borrower-level data. Designs exploiting independently justified variation in adoption would strengthen causal interpretation. Until such evidence is available, the present estimates should inform cautious monitoring and further investigation rather than quantified claims of permanent policy benefits.

8 CONCLUSION

This study assessed the relationship between payment digitization, inflation and selected indicators of financial vulnerability in Azerbaijan using monthly observations from January 2018 to December 2025. Its central finding is that rapid growth in cashless payments coexisted with declining overdue lending and deposit dollarization, but the estimated relationships do not establish a stable causal contribution of digitization to these developments. The evidence supports distinguishing changes in payment behavior from improvements in macroeconomic and financial outcomes.

Persistence tests yielded conflicting evidence. The corrected preliminary bounds screen produces an upper-bound F rejection for the extended-risk equation, but its limited deterministic specification and the absence of supplementary adjustment and degeneracy tests prevent a defensible equilibrium interpretation. The analysis consequently reports dynamic change associations, with uncertainty about persistence explicitly retained. This choice establishes neither the absence of an equilibrium relationship nor a permanent digitization effect.

For inflation, the principal diagnostic-selected model yielded a positive coefficient on the lagged change in the cashless value share, with a HAC p-value of 0.085. Its confidence interval included zero, and inference varied with the covariance bandwidth and alternative specifications. The findings therefore provide no robust support for a disinflationary effect of payment digitization. Nor do they justify concluding that digitization consistently raises inflation. The dependent variable in this model is the monthly change in annual inflation, which further limits direct interpretation as an effect on monthly price growth.

The credit risk findings were similarly conditional. In the logarithmic overdue lending model without a trend, the digitization lags were jointly significant at the 5% level, but their signed sum was not significant at that level and its confidence interval included zero. Adding a trend reversed the estimated direction and removed significance. Alternative proxies and sample restrictions also changed the conclusions. These results do not establish a reliable net reduction in overdue lending attributable to digitization. For deposit dollarization, the principal value-share model provided no statistically reliable association, while an isolated result using transaction counts was not consistently reproduced. Quarterly estimates did not supply statistically significant corroboration of the principal digitization relationships.

The contribution of the study is to document these patterns while showing why descriptive co-movement is insufficient evidence of a stable macrofinancial benefit. Explicit treatment of reporting differences, persistence, diagnostic failures and specification sensitivity provides a more qualified assessment than a regression of trending levels alone. At the same time, the short sample, incomplete controls and absence of exogenous variation limit identification. The use of overdue lending and deposit dollarization also restricts the conclusions to selected vulnerability measures rather than the resilience of the financial system as a whole.

For policy, the findings favor evaluating digital payment development through observable improvements in access, use and service quality while maintaining separate assessments of inflation and financial risk. Payment data may offer useful information for monitoring and forecasting, but that value requires direct evaluation. Digital adoption alone does not provide an empirical basis for relaxing credit oversight or assigning quantified disinflation and dedollarization benefits. Harmonizing historical payment definitions and documenting financial reporting coverage are immediate priorities for improving the evidence available to policymakers.

Further research should combine longer and more consistent time series with real activity and external price controls, broader financial resilience indicators, and data at the institution or borrower level. Credible variation in

the timing or intensity of digital adoption would help distinguish causal effects from concurrent reforms and economic changes. Azerbaijan's experience demonstrates a substantial shift toward cashless payments; whether that shift independently strengthens price stability and financial resilience remains an open empirical question.

Data and code availability

The accompanying supplementary replication package provides the assembled official-source data, variable construction notes, selected-model design matrices and code to verify the four reported principal regressions and corrected preliminary bounds screen. The scope and remaining replication limitations are documented in Appendix C and the package README. The underlying statistical releases are publicly accessible through the institutional sources listed in the references.

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Appendix A Additional stationarity information

Table A1 Trend ADF and KPSS tests in levels

Variable	ADF ct	ADF p	ADF lags	KPSS ct	KPSS p
INF	-1.581	0.800	1	0.227	≤0.010
ODL	-0.016	0.994	0	0.403	≤0.010
DOLL	-3.069	0.114	0	0.205	0.014
DIG	-3.180	0.088	12	0.278	≤0.010
DIGN	3.566	1.000	11	0.372	≤0.010
M2G	-2.676	0.246	0	0.250	≤0.010
CREDG	-3.705	0.022	2	0.131	0.079
RATE	-1.558	0.808	3	0.274	≤0.010
INFM	-4.470	0.002	0	0.161	0.037
LNPOS	0.823	1.000	0	0.398	≤0.010
LOGODL	-0.907	0.955	0	0.191	0.019

Note: created by the author

Note: ct denotes an intercept and linear trend. KPSS trend-stationarity 5% critical value: 0.146. KPSS lookup limits are explicitly marked.

Table A2 First-difference PP and KPSS details

Variable	PP statistic	PP p	ADF lags	KPSS p
INF	-5.741	<0.001	0	≥0.100
ODL	-8.629	<0.001	0	≤0.010
DOLL	-13.023	<0.001	0	≥0.100
DIG	-15.981	<0.001	11	≥0.100
DIGN	-20.560	<0.001	12	≥0.100
M2G	-9.009	<0.001	0	≥0.100
CREDG	-4.963	<0.001	1	0.066
RATE	-10.816	<0.001	2	0.027
INFM	-15.901	<0.001	0	≥0.100
LNPOS	-7.766	<0.001	0	≤0.010
LOGODL	-9.177	<0.001	0	≥0.100

Note: created by the author

Table A3 Deterministic-adjustment sensitivity for first differences

Variable	ADF statistic	ADF lags	KPSS statistic
INF	-5.784	0	0.077
ODL	-9.271	0	0.174
DOLL	-12.118	0	0.055
DIG	-15.031	0	0.230
DIGN	-13.000	1	0.176
M2G	-8.861	0	0.045
RATE	-4.034	1	0.107
LNPOS	-8.486	0	0.145

Note: created by the author

Series are first differenced, projected on eleven monthly indicators and the dated COVID/step controls, then tested as residual series. ADF uses a maximum of two lags. These residual-based sensitivity statistics do not have exactly the usual unadjusted test distributions and are not used to make definitive integration-order claims.

Appendix B Seasonal coefficients and unsuccessful specifications

Table B1 Monthly indicator coefficients in the main regressions

Month relative to January	I- Inflation	R- Credit risk	RT- Risk with trend	D- Dollarization
2	-0.538 (0.299)	-0.463 (2.295)	-1.343 (2.127)	-0.966 (0.685)
3	0.154 (0.306)	1.151 (1.583)	-0.657 (1.741)	-1.049 (0.579)
4	-0.270 (0.201)	-5.561 (1.328)	-3.288 (1.020)	-1.361 (0.466)
5	-0.388 (0.314)	-5.335 (3.414)	-6.657 (3.354)	-1.427 (0.520)
6	-0.041 (0.274)	-0.658 (1.513)	-2.026 (1.313)	-2.101 (0.546)
7	-0.150 (0.286)	-1.687 (1.370)	-1.662 (1.391)	-1.623 (0.562)
8	-0.204 (0.347)	-0.287 (1.375)	-0.781 (1.621)	-1.103 (0.462)
9	-0.414 (0.375)	-3.807 (1.936)	-4.732 (1.948)	-1.381 (0.550)
10	-0.256 (0.233)	-3.900 (2.453)	-6.427 (2.243)	-1.110 (0.482)
11	-0.098 (0.222)	-2.303 (1.020)	-3.364 (1.143)	-0.930 (0.449)
12	-0.151 (0.257)	-8.490 (1.664)	-9.327 (1.700)	-1.930 (0.378)

Note: created by the author

HAC standard errors in parentheses. All monthly indicators are included regardless of individual significance. The reference month is January.

Table B2 Diagnostic record of initial and intermediate models

Model	N	DIG lag sum	Joint p	BG1 p	BG12 p	RESET p
INF	93	0.089	0.229	0.701	0.007	0.772
ODL	93	-0.082	0.077	0.563	0.682	<0.001
ODL_EXT	93	-0.088	0.101	0.638	0.781	<0.001
DOLL	93	-0.021	0.832	0.585	0.530	0.630
INF_S12	93	0.092	0.146	0.471	0.011	0.438

Note: created by the author

INF, ODL, ODL_EXT and DOLL use differences and the initial maximum lag of two. INF_S12 adds the twelfth lag of the change in annual inflation. The failed diagnostics motivated the adjustments described in Section 5.5. Their p-values are not suppressed.

Appendix C Reproducibility and scope

The supplementary replication package contains Azerbaijan_Digitization_Inflation_Data.xlsx, the monthly analysis extract, recorded raw source values, four selected-model design matrices and a verification script with expected outputs. The matrices reproduce the fitted specifications, including calendar lags and the seasonal outcome lag. Re-estimation verifies the selected coefficients, HAC standard errors, signed lag sums, joint tests, multipliers and selected diagnostics. Independent NumPy least-squares estimates agree with the selected coefficients within 2×10^{-14}. The script also reconstructs and verifies the corrected preliminary bounds screen from the monthly extract. It is a targeted verification package, not a complete independent reconstruction of all exploratory searches, stationarity tests or source releases.

Software: Python 3.12; statsmodels 0.15.0 for ADF, KPSS, Zivot–Andrews, ARDL/UECM and regression diagnostics; arch 8.0.0 for Phillips–Perron tests; NumPy/SciPy for numerical verification and Monte Carlo calculations. HAC uses a Bartlett kernel, bandwidth 12 for monthly models and 4 for quarterly models, small-sample correction and Student t/F inference. For the corrected bounds screen, intercept-only ARDL lag selection and the final UECM use no seasonal indicators. The final F-statistic, t-statistic and observation count refer to that same UECM and its natural holdback. Case-3 asymptotic reference values are used; no finite-sample bootstrap cointegration claim is made.

Risk transformation: $100[\ln(ODL_t) - \ln(ODL_{t-1})]$. All other differenced annual series use arithmetic first differences, so ΔINF is an inflation-rate change, not monthly inflation. Reported root moduli are companion-matrix eigenvalue moduli and include Model I's twelfth endogenous lag. The monthly-inflation robustness outcome is $100 \ln(CPI_t/CPI_{t-1})$. Own-lag and explanatory-lag indices are calendar lags, constructed before any exclusion mask. Supplementary recursive Monte Carlo tests keep the estimated design fixed; they are conditional stability checks, not a full dynamic bootstrap.

The analysis does not supply an instrumental-variable design, a structural VAR, a causal experiment or a reliable real-activity control. Remaining reporting differences in the payment series are not fully removed by dummy variables. Results should be revisited after harmonizing the underlying source definitions and adding appropriate real activity and external price controls.