

Article

Credit Deepening and Bank Asset Quality: Dynamic Early-Warning Evidence from 58 Countries

Marco Antonio Ledesma Munive ^{1,*} , Alejandro Anibal Aguirre-Rojas ² , Graciela Soledad Verastegui Velasquez ³, William Huanca ² , Pilar Zevallos ¹  and Nivaneth Valencia ⁴ 

¹ Faculty of Business Sciences, Universidad Continental, Huancayo 12001, Peru; pzevallos@continental.edu.pe

² Faculty of Administrative Sciences, Universidad Nacional del Centro del Perú, Huancayo 12006, Peru; aaguirre@uncp.edu.pe (A.A.A.-R.); whuanca@uncp.edu.pe (W.H.)

³ Faculty of Administrative and Accounting Sciences, Universidad Peruana Los Andes, Huancayo 12002, Peru; d.gverastegui@ms.upla.edu.pe

⁴ Faculty of Business, Universidad Tecnológica del Perú, Lima 15046, Peru; nvalencia@utp.edu.pe

* Correspondence: mledesma@continental.edu.pe

Abstract

This study examines whether the accumulated stock of private credit provides early-warning information for subsequent deterioration in banking-sector asset quality. It combines annual Passport banking indicators with World Development Indicators for 58 countries over 2010–2024; the preferred sample contains 746 country–year observations. A second-order dynamic fixed-effects model links $\log(1 + \text{NPL})$, where NPL denotes the non-performing loan ratio, to lagged private credit to gross domestic product (GDP), real credit growth, lending rates, bank capital, GDP growth, inflation, and unemployment. Its preferred credit-depth coefficient is 0.00377, implying that a 10-percentage-point increase is associated with approximately 0.15 percentage points more NPLs one year later at the sample median. To operationalize early-warning calibration without claiming a universal cutoff, the paper reports the sample credit-depth quartiles and estimates a country fixed-effects linear probability model using the European Banking Authority’s 5% gross-NPL supervisory trigger. In that alternative outcome, a 10-percentage-point increase in credit depth is associated with a 2.78-percentage-point higher conditional probability of NPLs reaching 5% or more ($p = 0.002$). On a strictly common 609-observation sample, the credit-depth coefficients at one-, two-, and three-year horizons are 0.00501, 0.00960, and 0.01266. Lending rates and unemployment are positive, whereas annual credit growth and capital ratios are not robust predictors. Pooled interactions do not reject equal slopes across broad country partitions. System generalized method of moments (GMM) passes conventional tests but violates a persistence-bound credibility check. The evidence supports an early-warning interpretation, not a causal claim.

Keywords: non-performing loans; credit deepening; bank asset quality; early warning; dynamic panel; lending rates

JEL Classification: C23; E44; G21; G28



Academic Editors: Maria-Eleni K. Agoraki, Konstantina K. Agoraki, Nicholas Belesis and Christos Kampouris

Received: 23 June 2026

Revised: 21 July 2026

Accepted: 30 July 2026

Published: 6 August 2026

Copyright: © 2026 by the authors.

Licensee MDPI, Basel, Switzerland.

This article is an open access article distributed under the terms and conditions of the [Creative Commons Attribution \(CC BY\)](https://creativecommons.org/licenses/by/4.0/) license.

1. Introduction

Banks expand access to credit, transform maturities, screen borrowers, and allocate savings toward productive uses. These functions are central to the finance–growth relationship (King & Levine, 1993; Levine, 2005). Yet a large stock of private credit can also

represent accumulated leverage, relaxed underwriting, and sensitivity to income, collateral, or interest-rate shocks. Historical evidence shows that credit-intensive expansions are disproportionately associated with financial crises and deeper subsequent contractions (Jordà et al., 2013; Mian et al., 2017; Schularick & Taylor, 2012). The policy problem is therefore not whether financial deepening is inherently desirable or dangerous, but when it begins to contain information about future deterioration in credit portfolios.

Non-performing loans (NPLs) provide a direct measure of realized asset-quality deterioration. They are not a comprehensive measure of bank fragility: they do not by themselves capture liquidity risk, market risk, insolvency, interconnectedness, or systemic spillovers. However, NPLs are economically consequential because they require provisions, weaken profitability, consume managerial and capital capacity, and can reduce new lending. The classic bad-management and bad-luck hypotheses connect problem loans to bank efficiency and managerial quality (Berger & DeYoung, 1997). Recent evidence continues to link NPLs to lower efficiency, weaker lending behavior, and impaired balance-sheet intermediation (Gjeçi et al., 2023; Quang et al., 2022; Takahashi & Vasconcelos, 2024). For this reason, the present paper uses the more precise term bank asset quality, reserving “fragility” for the wider systemic setting.

The empirical relationship between credit and NPLs is difficult to interpret because it runs in both directions. Rapid or prolonged credit expansion may reduce marginal screening standards and generate future loan losses (Altunbaş et al., 2022; Foos et al., 2010). Conversely, recognized NPLs can tie up capital, raise funding costs, and induce subsequent credit retrenchment (Gjeçi et al., 2023; Park & Shin, 2021). A regression that treats annual credit growth as an exogenous driver of NPLs can therefore mix the build-up phase with the defensive response phase. The distinction between a stock measure—private credit relative to gross domestic product (GDP)—and a flow measure—annual real credit growth—is consequently central. A high stock may persist after the flow has slowed or turned negative.

The COVID-19 episode amplified this identification problem. The shock combined borrower cash-flow disruption with guarantees, moratoria, supervisory flexibility, capital-buffer releases, and extraordinary liquidity provision. These policies supported viable borrowers and bank lending, but they also changed the timing of default recognition and the relation between reported NPLs and underlying borrower distress (Bitar & Tarazi, 2022; Çolak & Öztekin, 2021; Demirgüç-Kunt et al., 2021; Duncan et al., 2022). Evidence from China and cross-border lending confirms that pandemic exposure affected NPLs and credit transmission in ways conditioned by bank ownership, capitalization, and national policy (Kryzanowski et al., 2023; Park & Shin, 2021). Income diversification and uncertainty also changed credit-risk behavior during the pandemic (Bilgin et al., 2021; Mehmood & De Luca, 2023). A cross-country model spanning 2020 must therefore absorb common shocks and test whether slope relationships changed, rather than simply insert a pandemic dummy and interpret it causally.

Bank capital introduces a second ambiguity. In theory, capital absorbs losses, improves incentives, and supports continued lending. In practice, observed capital ratios are endogenous to provisions, risk-weighted assets, recapitalization, regulatory forbearance, business models, and past portfolio quality. European evidence shows that capitalization matters for coverage decisions and resilience (Alessi et al., 2021; Gehrig & Iannino, 2021), while recent work on counter-cyclical buffers and pandemic resilience emphasizes state-contingent effects (Anani & Owusu, 2023; Benbouzid et al., 2022). Markets react to tighter requirements, and recapitalization policies can change credit allocation (Couaillier & Henricot, 2023; Kasahara et al., 2024). A null direct coefficient on capital is therefore not equivalent to a finding that capital regulation is irrelevant.

The NPL literature has documented roles for real activity, labor markets, interest rates, uncertainty, institutional quality, competition, liquidity, and sovereign risk (Baselga-Pascual et al., 2025; Beck et al., 2015; Karadima & Louri, 2021; Louzis et al., 2012; Mdaghri, 2022; Pozo & Rojas, 2023; Saliba et al., 2023). Recent international and regional studies nevertheless leave three gaps. First, credit depth and annual credit growth are frequently treated as interchangeable indicators of financial expansion. Second, dynamic estimators are sometimes selected mechanically: system generalized method of moments (GMM) may pass conventional Hansen and serial-correlation tests even when internal instruments are weak or persistence is implausible (Roodman, 2009). Third, most studies estimate a single average horizon and devote limited attention to whether a credit variable predicts NPLs one, two, or three years ahead.

This paper addresses these gaps with an explicitly predictive design. It combines a country-level banking panel from Passport with real GDP growth, inflation, and unemployment from World Development Indicators. The preferred specification contains 746 country-year observations for 58 countries over 2010–2024. It uses two lags of the dependent variable, country and year fixed effects, one-year lags of private credit to GDP, real credit growth, lending rates, capital to assets, and macroeconomic controls. The dynamic order is selected from residual diagnostics: a first-order specification leaves serial correlation, whereas the second-order model does not.

The contribution is fourfold. First, the paper distinguishes the accumulated stock of private credit from the annual flow of credit and shows that the stock measure has substantially more stable predictive content for NPLs. Second, it demonstrates that the result survives stepped macro-control models, alternative covariance estimators, balanced and stable panels, alternative definitions, outlier treatment, and country exclusions. Third, it evaluates one-, two-, and three-year horizons on both conventional and strictly common samples and formally tests whether horizon-specific coefficients differ. Fourth, it replaces significance-based subgroup comparisons with pooled interaction tests and subjects system GMM to credibility checks beyond Hansen and second-order serial-correlation [AR(2)] tests.

The main result is a positive association between lagged private credit to GDP and subsequent NPLs. In the preferred macro-controlled model, the coefficient is 0.00377. At the sample median, a 10-percentage-point increase in private credit to GDP is associated with approximately 0.15 percentage points more NPLs one year later. The coefficient remains positive through three-year forecast horizons. Lending rates and unemployment are also positive, whereas annual real credit growth and capital ratios are not robust. Separate subgroup regressions differ in precision, but pooled interaction tests do not reject equal credit-depth slopes across average depth, capitalization, initial NPL, or broad regional partitions. The paper therefore avoids describing the relationship as concentrated in any one group.

The paper does not claim that private credit causes NPLs. Country effects remove time-invariant heterogeneity and year effects absorb common shocks, but the available design does not supply an external credit-supply instrument or quasi-experimental policy discontinuity. Moreover, lead-placebo evidence indicates reverse feedback from current NPLs to future credit growth. The paper is therefore positioned as an early-warning and predictive-risk study. This narrower interpretation is more credible and more useful than a causal claim unsupported by the design.

Operationally, the paper distinguishes a monitoring reference from a structural threshold. Within the estimation sample, the 25th percentile, median, and 75th percentile of lagged private credit to GDP are 32.43%, 51.95%, and 83.05%, respectively. The 75th percentile is used only as an elevated, sample-based screening reference. Separately, the European Banking Authority (EBA) uses a 5% gross NPL ratio as a trigger for an enhanced

non-performing exposure (NPE) strategy, governance, and monitoring, while explicitly stating that it is not an optimal target (European Banking Authority, 2018). The revision therefore evaluates an alternative indicator, $1(NPL \geq 5\%)$, but does not present either 83.05% credit depth or 5% NPLs as a universal safe/unsafe boundary.

The remainder of the paper proceeds as follows. Section 2 reviews the literature and develops the hypotheses. Section 3 presents the data and methods. Section 4 reports the stepped models, preferred estimates, diagnostics, horizon tests, heterogeneity, and robustness. Section 5 discusses interpretation and policy implications. Section 6 concludes.

2. Literature Review and Hypothesis Development

2.1. NPLs as an Asset-Quality Outcome

The NPL ratio is one of the most common ex post measures of credit-portfolio quality. It reflects the share of gross loans that have entered non-performing status and thus links borrower repayment capacity to bank balance sheets. Its exact definition varies across jurisdictions, and regulatory recognition can be delayed by restructuring or forbearance. Nevertheless, it remains a central indicator in both supervisory and empirical work (Erdaş & Ezanoğlu, 2022; Ferreira, 2022). Stress-test outcomes are also sensitive to the selected credit-risk variable and the level of data aggregation (Ferrari et al., 2021).

NPLs display persistence because recognition, provisioning, collateral enforcement, restructuring, write-offs, and asset disposal take time. Banking-crisis evidence shows that NPL peaks may occur several years after the initiating shock (Ari et al., 2021). Expected-loss accounting can shift the timing of recognition and market reactions (Onali et al., 2024), while securitization and asset-management companies change where risk is held and how quickly balance sheets are repaired (Brei et al., 2023; Wengerek et al., 2022). These features justify a dynamic model rather than a contemporaneous static regression.

NPLs also affect the real economy. High impaired-loan stocks can constrain bank efficiency and new credit supply (Gjeçi et al., 2023; Quang et al., 2022). They may interact with liquidity creation and funding stability (Fungáčová et al., 2021; Mdaghri, 2022). The dependent variable in this paper is therefore interpreted as an asset-quality outcome with wider financial-stability consequences, not as a complete proxy for systemic fragility.

2.2. Credit Depth, Annual Growth, and Accumulated Leverage

Financial deepening can improve access to finance and support growth (King & Levine, 1993; Levine, 2005). Yet the risk consequences depend on how credit expands. A rapid flow of new lending can stretch screening and monitoring capacity, attract marginal borrowers, and generate future loan losses (Foos et al., 2010). Excess lending can also interact with market power, securitization, and capital in the build-up of systemic risk (Altunbaş et al., 2022).

Private credit to GDP captures a different concept. It is a stock relative to the economy's income base and may proxy for accumulated borrower leverage, the scale of banking claims, and the volume of assets exposed to a downturn. Historical studies find that high leverage before recessions predicts weaker subsequent performance (Jordà et al., 2013; Mian et al., 2017; Schularick & Taylor, 2012). Recent NPL studies likewise show that aggregate financial structure, total credit, and country-level conditions retain explanatory content after bank-specific controls are introduced (Erdaş & Ezanoğlu, 2022; Ferreira, 2022; Saliba et al., 2023). The stock can remain high after banks have already tightened credit, which means it may retain predictive information when annual credit growth does not.

This distinction leads to two hypotheses:

H1. Higher private credit to GDP in year $t - 1$ is positively associated with the NPL ratio in year t .

H2. *Faster real bank-credit growth in year $t - 1$ is positively associated with the NPL ratio in year t .*

H2 is more fragile theoretically because the observed flow may already incorporate a response to current or anticipated NPLs. The paper therefore tests future credit growth as a lead placebo and estimates longer predictive horizons.

2.3. Lending Costs and Macroeconomic Repayment Capacity

Borrowers service loans from income and cash flow. Higher lending rates increase debt-service burdens on floating-rate loans and refinanced credit, while weaker real activity and rising unemployment reduce repayment capacity. Global and portfolio-level NPL studies repeatedly identify output, labor-market, and interest-rate channels (Beck et al., 2015; Ferreira, 2022; Foglia, 2022; Louzis et al., 2012). Country risk and uncertainty can further change borrower behavior, collateral valuation, and banks' willingness to recognize losses (Bilgin et al., 2021; Karadima & Louri, 2021; Saliba et al., 2023).

Inflation is theoretically ambiguous. Moderate inflation can reduce the real value of nominal debt, but high or volatile inflation can raise borrowing costs, weaken real income, and coincide with macroeconomic instability. Accordingly, the paper imposes no directional hypothesis on inflation.

H3. *A higher lending rate in year $t - 1$ is positively associated with the NPL ratio in year t .*

H4a. *Higher real GDP growth in year $t - 1$ is negatively associated with the NPL ratio in year t .*

H4b. *Higher unemployment in year $t - 1$ is positively associated with the NPL ratio in year t .*

2.4. Capitalization as a Conditional Mechanism

The direct sign of capital is not unambiguous. Higher capital can absorb unexpected losses and strengthen monitoring incentives. It may also permit banks to recognize impaired assets without threatening solvency. However, capital ratios react to past losses, risk-weighted assets, recapitalization, and regulatory decisions. Pandemic research suggests that stronger capital improved risk resilience, but policy relaxation also supported lending and affected measured risk (Anani & Owusu, 2023; Bitar & Tarazi, 2022; Duncan et al., 2022).

Capital may matter more as a buffer or moderator than as a stable determinant of whether borrowers default. Capital requirements can alter market-assessed default risk (Couaillier & Henricot, 2023), recapitalization can change allocation and productivity (Kasahara et al., 2024), and countercyclical buffers are explicitly designed to act over the financial cycle (Benbouzid et al., 2022). The hypotheses are therefore conditional rather than mechanically negative:

H5a. *Higher bank capital is associated with lower subsequent NPLs, although the sign may be attenuated by endogenous adjustment.*

H5b. *Bank capital weakens the positive association between credit expansion and subsequent NPLs.*

2.5. Heterogeneity and the Early-Warning Interpretation

Average international coefficients can conceal important heterogeneity. Competition changes the incentives for screening and risk taking (Pozo & Rojas, 2023). Sovereign exposure and country risk can alter bank credit risk (Baselga-Pascual et al., 2025; Saliba et al., 2023). Internationalization and bank-specific characteristics affect the credit channel (Cantú et al., 2022), post-crisis European default risk reflects both regulation and sovereign

conditions (Soenen & Vander Vennet, 2022), and policy support and pandemic exposure vary across countries (Çolak & Öztekin, 2021; Demirgüç-Kunt et al., 2021; Kryzanowski et al., 2023).

The expected credit-depth association may vary across banking systems with different levels of financial depth, capitalization, initial asset quality, or regional institutions. These comparisons are exploratory because group membership is not exogenous. Importantly, statistical significance within one subgroup and insignificance within another do not establish a difference between them. The main heterogeneity analysis therefore relies on pooled interaction tests and reports formal slope-difference statistics.

3. Materials and Methods

3.1. Data Sources and Coverage

Banking and financial indicators come from Euromonitor International's Passport Statistics export dated 22 June 2026 (Euromonitor International, 2026). The original file contains 100 economies and annual observations from 1990 through 2025, but coverage differs substantially by variable. The NPL ratio has 2457 observations across 98 countries; private credit to GDP has 2990 observations across 92 countries; bank claims on the private sector have 2850 observations across 86 countries; lending rates have 2311 observations across 71 countries; and capital to assets has 1449 usable observations across 87 countries.

Real GDP growth, consumer-price inflation, and modeled International Labour Organization (ILO) unemployment are merged from World Development Indicators (World Bank, 2026a, 2026b, 2026c). The principal estimation window is 2010–2024. The starting date improves joint coverage of NPLs, rates, and capital. The 2025 vintage is reserved for sensitivity analysis because recent observations can be incomplete or revised.

The Passport-only baseline contains 754 country-year observations for 58 countries. Requiring the three macroeconomic controls yields 746 observations for the same 58 countries. No missing value is imputed. The paper therefore presents the Passport-only estimate and a macro-augmented restricted sample rather than silently changing the estimand.

The revision prioritizes the highest-coverage macroeconomic determinants. Exchange-rate depreciation, real interest rates, per-capita income, property prices, and governance indicators remain important extensions, but their joint inclusion would produce a materially smaller and compositionally different panel. This limitation is stated explicitly rather than obscured by an unstable complete-case sample.

Figure 1 displays the geographical coverage. The preferred sample comprises 23 European economies, 14 Asian economies, 13 Latin American and Caribbean economies, six African economies, the United States, and Australia. Appendix C reports the complete country list by region.

3.2. Variable Construction

The dependent variable, y_{it} , is defined as

$$y_{it} = \ln(1 + NPL_{it}),$$

where NPL_{it} is bank non-performing loans as a percentage of total gross loans. The transformation reduces right skewness and accommodates values close to zero. NPLs in percentage points are used as a robustness outcome.

Credit depth is domestic credit to the private sector as a percentage of GDP. Real credit growth is calculated as

$$CreditGrowth_{it} = 100[\ln(RealClaims_{it}) - \ln(RealClaims_{i,t-1})],$$

where real claims are expressed in constant 2025 U.S. dollars. The transformed lending-rate measure is

$$Rate_{it}^* = \ln(1 + LendingRate_{it}).$$

Capitalization is capital as a percentage of total assets; Tier 1 capital relative to risk-weighted assets is used as an alternative.

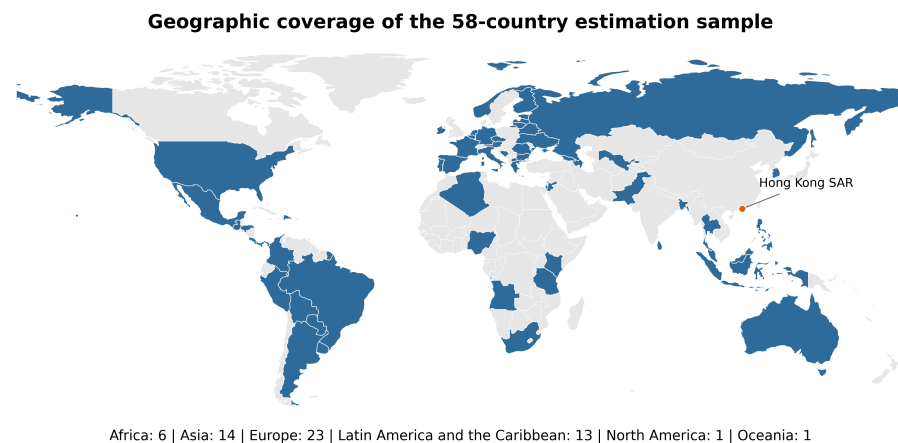


Figure 1. Geographic coverage of the 58-country estimation sample. Highlighted economies are included in the preferred analysis; Hong Kong SAR is indicated by a point because it is not separately represented in the low-resolution boundary file. Region counts are Africa (6), Asia (14), Europe (23), Latin America and the Caribbean (13), North America (1), and Oceania (1). Base boundaries: Natural Earth.

All core explanatory variables enter with one lag. The macro-augmented model additionally includes lagged GDP growth, inflation, and unemployment. A contemporaneous macro specification is reported to evaluate timing sensitivity.

A data-quality audit identified a capital-to-assets value of 266.554% for Jordan in 2015, compared with adjacent observed values close to 10–11%. The preferred data set treats this observation as missing before lag construction. The raw observation and 1/99 winsorization are used as sensitivity tests. This decision is consequential because the raw outlier creates a statistically significant capital coefficient that is absent under defensible treatments.

Table 1 summarizes the variable definitions, theoretical mechanisms, timing choices, expected signs, and representative evidence.

Table 1. Variable definitions, theoretical mechanisms, timing choices, expected relationships, and representative evidence. The dependent-variable lags capture persistence in recognition, provisioning, restructuring, and resolution; financial and macroeconomic regressors are lagged one year so that the predictor precedes the annual asset-quality outcome.

Construct	Operational Measure and Timing	Theory, Mechanism, and Lag Rationale	Expected Sign	Representative Evidence
Asset-quality deterioration	NPLs/total gross loans; $\ln(1 + NPL_{it})$; lags $t - 1$ and $t - 2$	NPL recognition, provisioning, restructuring, collateral recovery, and write-off are persistent and may peak after the initiating shock; two lags are retained because AR(1) leaves residual serial correlation whereas AR(2) does not.	Outcome	Berger and DeYoung (1997); Ari et al. (2021); Quang et al. (2022)
Credit depth (H1)	Domestic credit to private sector/GDP, lagged one year	Accumulated leverage and a large stock of bank claims can increase the volume exposed to income, collateral, and interest-rate shocks.	+	Schularick and Taylor (2012); Jordà et al. (2013); Mian et al. (2017); Beck et al. (2015); Ferreira (2022)

Table 1. Cont.

Construct	Operational Measure and Timing	Theory, Mechanism, and Lag Rationale	Expected Sign	Representative Evidence
Credit expansion (H2)	Annual log change in real bank claims $\times 100$, lagged one year	Fast expansion can stretch screening and monitoring, but observed growth can fall after NPL recognition; the expected ex ante sign is positive, with reverse feedback tested by leads.	+ /ambiguous	Foos et al. (2010); Altunbaş et al. (2022); Gjeçi et al. (2023)
Lending costs (H3)	$\ln(1 + \text{lending rate})$, lagged one year	Higher debt-service costs weaken borrower cash flow and can worsen selection and refinancing risk before delinquency is recognized.	+	Louzis et al. (2012); Beck et al. (2015); Foglia (2022)
Real activity (H4a)	Real GDP growth, lagged one year; contemporaneous timing check	Stronger activity supports borrower revenue and repayment capacity; the concurrent model checks whether this channel operates within the year.	−	Beck et al. (2015); Foglia (2022); Ferreira (2022)
Price environment	CPI inflation, lagged one year	Inflation can erode real debt burdens but also real income, raise rates, and signal instability; theory does not imply a unique sign.	Ambiguous	Foglia (2022); Ferreira (2022)
Labor-market stress (H4b)	Unemployment rate, lagged one year	Employment loss reduces household repayment capacity and weakens aggregate demand and firm cash flow.	+	Louzis et al. (2012); Foglia (2022); Erdaş and Ezanoğlu (2022)
Bank capital (H5)	Capital/total assets, lagged one year; Tier 1/RWA alternative	Capital absorbs loss and may improve monitoring, but observed ratios adjust endogenously to provisions, risk weights, recapitalization, and past asset quality.	− /conditional	Alessi et al. (2021); Benbouzid et al. (2022); Anani and Owusu (2023); Couaillier and Henricot (2023)

Abbreviations: NPL, non-performing loan; GDP, gross domestic product; AR, autoregressive; CPI, consumer price index; RWA, risk-weighted assets.

3.3. Operational Early-Warning Calibration and Alternative Outcome

The early-warning interpretation requires an operational reference but does not imply that the model estimates a universal regulatory threshold. Two calibrations are therefore reported. First, the sample distribution of lagged private credit to GDP provides internal reference points: the 25th percentile is 32.43%, the median is 51.95%, and the 75th percentile is 83.05% of GDP. These values describe the estimation sample and are not policy cutoffs.

Second, the European Banking Authority identifies a 5% gross NPL ratio as a trigger for developing an NPE strategy and related governance and operational arrangements, while explicitly noting that 5% is not an optimal target (European Banking Authority, 2018). The paper therefore defines the alternative binary outcome as

$$HighNPL_{it} = \mathbf{1}(NPL_{it} \geq 5\%)$$

and estimates a country- and year-fixed-effects linear probability model on the 754-observation Passport core sample, with the same financial predictors and country-clustered inference. This is an alternative operationalization of material asset-quality deterioration, not an independent measure and not a causal threshold model. NPLs in levels provide a second alternative outcome in the robustness section. Comparable loan-loss-provision and charge-off series are not available across the 58 countries in Passport.

3.4. Stepped Specification Strategy

To show how the principal coefficient changes as controls are added, the paper reports a sequence of models:

1. Persistence and Passport financial variables;

2. The baseline plus lagged GDP growth;
3. The baseline plus lagged GDP growth and inflation;
4. The preferred model adding lagged unemployment;
5. A timing check with contemporaneous macroeconomic controls.

This stepped presentation prevents the main result from appearing only in a saturated specification and reveals how much of the raw association is absorbed by macroeconomic conditions.

3.5. Preferred Dynamic Fixed-Effects Model

The preferred model is

$$y_{it} = \rho_1 y_{i,t-1} + \rho_2 y_{i,t-2} + \beta_1 \text{Depth}_{i,t-1} + \beta_2 \text{CreditGrowth}_{i,t-1} \\ + \beta_3 \text{Rate}_{i,t-1}^* + \beta_4 \text{Capital}_{i,t-1} + \gamma_1 \text{GDPGrowth}_{i,t-1} \\ + \gamma_2 \text{Inflation}_{i,t-1} + \gamma_3 \text{Unemployment}_{i,t-1} + \alpha_i + \lambda_t + \varepsilon_{it}.$$

Here, α_i and λ_t denote country and year fixed effects. Country effects absorb time-invariant legal, supervisory, institutional, and structural differences. Year effects absorb common global shocks.

A first-order dynamic model was estimated initially, but its residuals remained serially correlated. Adding the second lag eliminated detectable first- and second-order residual autocorrelation. The AR(2) specification is therefore selected from diagnostics rather than from coefficient significance.

3.6. Inference and Diagnostic Hierarchy

The primary covariance estimator clusters by country. With 58 clusters, the analysis also reports two-way country–year clustering and a Rademacher wild cluster bootstrap, following current guidance on cluster-robust inference (MacKinnon et al., 2023). Driscoll–Kraay covariance estimates are reported as a robustness check for broad temporal and cross-sectional dependence (Driscoll & Kraay, 1998), but they are not treated as the sole basis of inference because the time dimension is 15 years.

The diagnostic set includes a poolability test, a Hausman comparison of fixed and random effects, residual AR(1) and AR(2) tests, the Brown–Forsythe heteroskedasticity test, within transformed variance-inflation factors, and the Pesaran CD test (Pesaran, 2015). Dynamic stability is evaluated using the roots implied by the two dependent-variable lags.

3.7. Predictive Horizons and Alternative Timing Measures

The early-warning interpretation is evaluated beyond a one-year horizon. For $h \in \{1, 2, 3\}$, the direct predictive specification is

$$y_{i,t+h} = \theta_h y_{it} + \phi_h y_{i,t-1} + \beta_h \text{Depth}_{it} + \delta_h' \mathbf{X}_{it} \\ + \alpha_{i,h} + \lambda_{t,h} + u_{i,t,h},$$

where $\mathbf{X}_{it}$ contains credit growth, the transformed lending rate, and bank capital. The coefficients are horizon-specific predictive projections, not cumulative impulse responses.

The primary comparison uses a strictly common origin sample with complete outcomes at all three horizons, producing 609 observations from 57 countries. Separate horizon equations retain the same country and origin-year fixed effects. A stacked model allows all slopes and fixed effects to vary by horizon and uses country-clustered covariance to test the joint null $\beta_1 = \beta_2 = \beta_3$. Conventional varying-sample estimates are retained in the supplement.

Three additional timing checks are estimated. First, private credit to GDP enters with lags of one, two, and three years in separate dynamic models. Second, the lagged three-year

moving average of credit depth replaces the single-year measure. Third, a descriptive event profile identifies non-overlapping episodes in which credit to GDP increases by at least 15 percentage points over three years. Because those episodes are not randomly assigned and no parallel-trends assumption is imposed, the event profile is placed in Appendix B and is not used as primary inferential evidence.

3.8. Heterogeneity Analysis

Separate subgroup regressions are reported in the supplement for descriptive comparison. The main heterogeneity inference instead uses pooled interaction models on the same 754-observation, 58-country Passport sample. For a binary country group G_i , the relevant component is

$$y_{it} = \dots + \beta \text{Depth}_{i,t-1} + \delta (\text{Depth}_{i,t-1} \times G_i) + \dots$$

The lower-group slope is β , the higher-group slope is $\beta + \delta$, and the interaction test evaluates whether the two slopes differ. Country groups are defined by median country-level average credit depth, median country-level average capitalization, and initial NPLs. Broad regional interactions compare Europe, Latin America and the Caribbean, and the remaining economies; a joint Wald test evaluates equality across the three regional slopes. All models retain country and year fixed effects and country-clustered inference.

A separate pooled model tests whether the slopes of credit depth, credit growth, lending rates, and capital differ after 2020. Because group membership and the pandemic period are not exogenous treatments, all heterogeneity results are descriptive rather than structural.

3.9. Dynamic-Panel Bias and Endogeneity Checks

Fixed effects with lagged dependent variables can exhibit Nickell bias in short panels (Nickell, 1981). The analysis therefore estimates a half-panel jackknife on the balanced sample (Dhaene & Jochmans, 2015) and reports a parsimonious system-GMM model based on the Arellano–Bond, Arellano–Bover, and Blundell–Bond framework (Arellano & Bond, 1991; Arellano & Bover, 1995; Blundell & Bond, 1998).

GMM instruments are collapsed and restricted so that the instrument count remains below the number of countries. The model is evaluated using Hansen, AR(1), AR(2), and persistence-bound diagnostics. The last check is important because instrument proliferation and weak internal instruments can coexist with reassuring Hansen statistics (Roodman, 2009). System GMM is treated as a robustness exercise, not as automatic causal identification.

3.10. Why a Difference-in-Differences Design Is Not Imposed

A valid difference-in-differences design requires a clearly dated and comparably defined treatment, defensible untreated or not-yet-treated comparators, and a credible parallel-trends assumption. Modern staggered adoption methods additionally require care when treatment timing and effects are heterogeneous (Callaway & Sant’Anna, 2021; Sun & Abraham, 2021). The present database does not contain harmonized country-year adoption dates for a single prudential policy. A post-2020 indicator would combine moratoria, guarantees, provisioning relief, capital-buffer releases, monetary easing, and fiscal support and would therefore not identify one treatment. The paper consequently reports post-2020 slope interactions as descriptive robustness but does not label them as difference-in-differences (DiD) or assign them a causal interpretation. A policy-based DiD is reserved for future work with verified legal adoption dates and pre-trend tests.

4. Results

4.1. Descriptive Evidence

The preferred macro-controlled sample has a mean NPL ratio of 5.31% and a median of 2.92%, confirming substantial right skewness. Private credit averages 62.35% of GDP, with a median of 52.0% and a range from 7.5% to 264.4%. Real credit growth averages 2.49% and is volatile. The median lending rate is 7.0%, while unemployment averages 7.98%.

Table 2 reports the descriptive statistics, and Figure 2 displays the annual median and interquartile range of NPL ratios.

Table 2. Descriptive statistics for the 746-observation preferred macro-controlled sample, 2010–2024.

Variable	N	Mean	SD	Median	Minimum	Maximum
NPL ratio (%)	746	5.314	6.819	2.916	0.233	54.823
Private credit/GDP, $t - 1$ (%)	746	62.350	40.741	52.000	7.500	264.400
Real credit growth, $t - 1$ (%)	746	2.487	8.155	2.833	−42.343	57.745
Lending rate, $t - 1$ (%)	746	9.099	8.296	7.000	0.900	95.900
Capital/assets, $t - 1$ (%)	746	8.529	2.409	8.525	−1.262	18.313
Real GDP growth, $t - 1$ (%)	746	2.555	3.998	2.854	−17.821	24.624
Inflation, $t - 1$ (%)	746	5.125	8.338	3.332	−1.736	133.489
Unemployment, $t - 1$ (%)	746	7.981	6.137	5.851	0.249	34.007

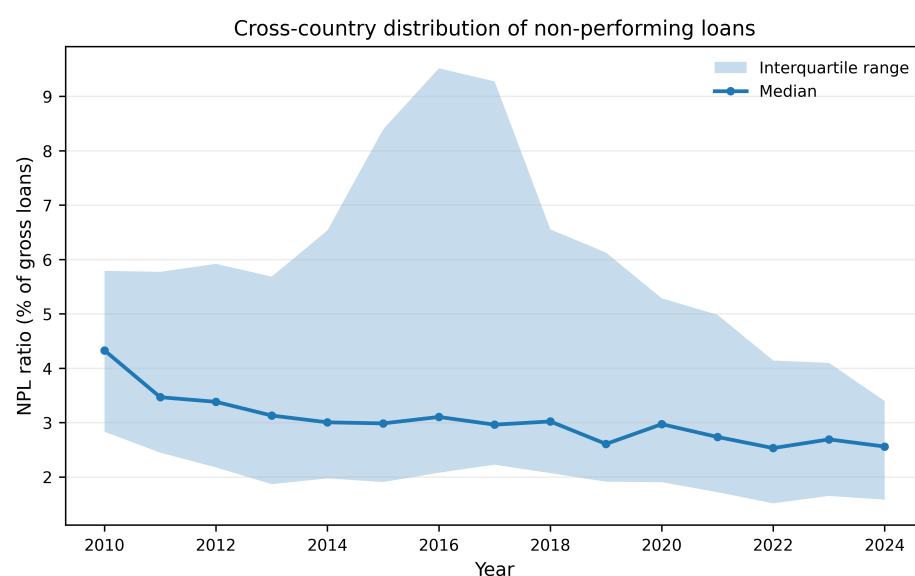


Figure 2. Annual distribution of NPL ratios in the preferred-sample economies, 2010–2024. The solid line is the cross-country median and the shaded band is the interquartile range; the number of available countries can vary by year because the panel is unbalanced. Source: authors' calculations from Passport.

4.2. Early-Warning Calibration and Alternative Asset-Quality Outcome

Table 3 separates descriptive calibration from external supervisory monitoring. The 83.05% credit-depth value is the sample's 75th percentile and is used only to describe an elevated inherited credit stock. The 5% NPL value is the EBA trigger for enhanced NPE strategy and governance, not a universal optimal target. In the core sample, 28.91% of country-year observations meet or exceed 5% NPLs.

Table 3. Descriptive early-warning calibration and external NPL monitoring reference. The credit-depth values are sample quantiles, whereas the 5% NPL value is an EBA supervisory trigger; none is estimated as a causal threshold.

Reference	Value	Interpretation
Private credit/GDP, 25th percentile	32.43%	Lower-quartile sample reference; descriptive only
Private credit/GDP, median	51.95%	Sample midpoint used for economic-magnitude calculations
Private credit/GDP, 75th percentile	83.05%	Elevated sample-based monitoring reference; not a prudential cutoff
Gross NPL monitoring trigger	5.00%	EBA trigger for enhanced NPE strategy and governance, not an optimal NPL target (European Banking Authority, 2018)
Country-years with $NPL \geq 5\%$	28.91%	Observed incidence in the 754-observation Passport core sample

The alternative linear probability model confirms that credit depth predicts a material asset-quality state. The coefficient on lagged private credit to GDP is 0.00278 (clustered SE = 0.00090, $p = 0.002$). Thus, a 10-percentage-point increase is associated with a 2.78-percentage-point higher conditional probability that NPLs reach 5% or more; the approximate 95% interval is 1.02 to 4.54 percentage points. This result complements the continuous log-NPL and level-NPL specifications; it does not transform the 5% reference into a causal or universal threshold.

Table 4 reports the full alternative-outcome estimates.

Table 4. Alternative asset-quality outcome: country and year fixed-effects linear probability model for $1(NPL \geq 5\%)$, 2010–2024. The model uses the 754-observation Passport core sample and country-clustered standard errors.

Variable	Coefficient	Clustered SE	p-Value	95% CI
$\ln(1 + NPL), t - 1$	0.46824	0.07156	<0.001	[0.32797, 0.60850]
$\ln(1 + NPL), t - 2$	−0.10940	0.05078	0.031	[−0.20892, −0.00988]
Private credit/GDP, $t - 1$	0.00278	0.00090	0.002	[0.00102, 0.00454]
Real credit growth, $t - 1$	−0.00026	0.00195	0.896	[−0.00408, 0.00357]
$\ln(1 + \text{lending rate}), t - 1$	0.12358	0.06511	0.058	[−0.00403, 0.25119]
Capital/assets, $t - 1$	−0.00440	0.01053	0.676	[−0.02505, 0.01625]

4.3. Stepped Macro-Control Models

Figure 3 visualizes the credit-depth coefficient as controls are added, and Table 5 reports the corresponding sequential estimates. The coefficient on lagged private credit to GDP declines from 0.00477 in the Passport-only model to 0.00377 in the preferred lagged-control model, but it remains positive and statistically significant at every step. The attenuation indicates that macroeconomic composition explains part, but not all, of the association.

The lending-rate coefficient remains positive. Its precision improves when inflation is included. Lagged unemployment is positive and strongly significant. Lagged GDP growth is negative but imprecise; when GDP growth enters contemporaneously, it becomes negative and significant. This timing pattern is consistent with an immediate borrower cash-flow channel rather than a delayed one.

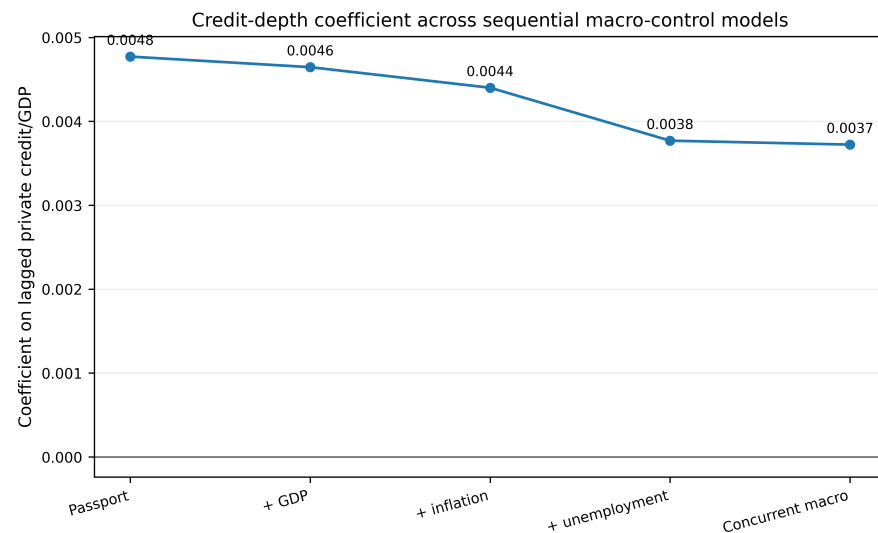


Figure 3. Point estimate of the lagged private-credit-to-GDP coefficient across sequential dynamic fixed-effects models. Every model includes two NPL lags and country and year fixed effects; exact coefficients and country-clustered inference are reported in Table 5.

Table 5. Sequential second-order dynamic fixed-effects specifications for $\ln(1 + NPL)$, with country and year effects and country-clustered inference.

Variable	M1 Passport	M2 + GDP	M3 + Inflation	M4 Preferred	M5 Concurrent
NPL, $t - 1$	0.955 ***	0.950 ***	0.952 ***	0.918 ***	0.911 ***
NPL, $t - 2$	−0.183 ***	−0.176 ***	−0.188 ***	−0.195 ***	−0.163 ***
Private credit/GDP, $t - 1$	0.00477 ***	0.00465 ***	0.00440 ***	0.00377 ***	0.00372 ***
Real credit growth, $t - 1$	−0.00024	−0.00005	−0.00099	−0.00112	−0.00004
$\ln(1 + \text{lending rate})$, $t - 1$	0.125 **	0.122 **	0.147 ***	0.118 **	0.066
Capital/assets, $t - 1$	0.002	0.003	0.004	0.004	0.008
GDP growth	—	−0.002	−0.003	−0.002	−0.014 ***
Inflation	—	—	−0.004 *	−0.004	−0.001
Unemployment	—	—	—	0.014 ***	0.015 ***
Observations	754	754	748	746	746
Countries	58	58	58	58	58
Within R^2	0.793	0.791	0.792	0.798	0.810

Notes: The dependent variable is $\ln(1 + NPL)$. All columns include country and year fixed effects and use country-clustered inference. M1 contains the Passport financial variables; M2–M4 add lagged macroeconomic controls sequentially; M5 uses contemporaneous macroeconomic controls as a timing check. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.4. Preferred Macro-Controlled Model

The NPL process is persistent but stable. The coefficients on the first and second lags are 0.918 and -0.195 ; their sum is 0.722. Private credit to GDP has a coefficient of 0.00377 with a country-clustered p -value below 0.001. It remains significant under two-way clustering, Driscoll–Kraay inference, and wild cluster bootstrap.

The 95% country-clustered interval for the credit-depth coefficient is approximately [0.00175, 0.00579]. At the sample median, a 10-percentage-point increase changes the log outcome by approximately 0.0377 and is associated with about 0.15 percentage points more NPLs in the next year. Moving from the sample's 25th to 75th percentile of credit depth—a 50.62-percentage-point difference—corresponds to approximately 0.82 additional NPL percentage points at the median. This interquartile comparison is an economic-magnitude calculation, not a policy treatment effect. The long-run multiplier is 0.01358 per percentage point of credit depth, although long-run effects should be interpreted cautiously because persistence magnifies residual endogeneity.

The lending-rate coefficient is positive and significant under all four inferential procedures. At the median lending rate, a one-percentage-point increase corresponds to approximately 0.054 additional NPL percentage points at the median outcome. Unemployment is also positive and robust: one additional unemployment percentage point corresponds to approximately 0.054 additional NPL percentage points at the median. These translations hold the other regressors and fixed effects constant and remain predictive rather than causal. Annual credit growth and capital to assets are statistically insignificant. Inflation is sensitive to the covariance estimator and is not treated as a robust substantive result.

Table 6 reports the preferred estimates under the four inference procedures.

Table 6. Preferred second-order dynamic fixed-effects model for $\ln(1 + NPL)$ under country clustering, two-way clustering, Driscoll–Kraay covariance, and wild cluster bootstrap.

Variable	Coef.	SE	<i>p</i> (Country)	<i>p</i> (Two-Way)	<i>p</i> (DK)	<i>p</i> (Wild)
NPL, $t - 1$	0.91777	0.04486	<0.001	<0.001	<0.001	—
NPL, $t - 2$	−0.19530	0.05286	<0.001	0.001	<0.001	—
Private credit/GDP, $t - 1$	0.00377	0.00103	<0.001	<0.001	<0.001	0.008
Real credit growth, $t - 1$	−0.00112	0.00150	0.456	0.400	0.463	0.492
$\ln(1 + \text{lending rate})$, $t - 1$	0.11830	0.05363	0.028	0.044	0.011	0.040
Capital/assets, $t - 1$	0.00386	0.01156	0.738	0.723	0.543	0.657
GDP growth, $t - 1$	−0.00202	0.00253	0.426	0.425	0.484	0.408
Inflation, $t - 1$	−0.00360	0.00226	0.112	0.028	<0.001	0.410
Unemployment, $t - 1$	0.01382	0.00401	<0.001	<0.001	<0.001	0.012

Notes: The dependent variable is $\ln(1 + NPL)$. All estimates include country and year fixed effects. DK denotes Driscoll–Kraay covariance. Wild-bootstrap *p*-values use country clusters and are reported for the substantive regressors. $N = 746$; countries = 58; within $R^2 = 0.798$.

The evidence supports H1, H3, and H4b. H2 is not supported. H4a receives only partial support because contemporaneous, rather than lagged, GDP growth is significant. Neither H5a nor H5b is supported in the aggregate data.

4.5. Diagnostics

The poolability test rejects a common-intercept model, and the Hausman statistic rejects the orthogonality condition required for random effects. Heteroskedasticity is present, which validates the use of robust covariance estimators. The maximum within variance inflation factor (VIF) is 4.26.

The first-order dynamic model leaves residual autocorrelation. In the preferred second-order model, residual AR(1) and AR(2) are not statistically detected. The Pesaran CD statistic is −1.103 with a *p*-value of 0.270, so residual cross-sectional dependence is not detected after year effects.

Table 7 summarizes the specification and residual diagnostics.

Table 7. Specification and residual diagnostics for the preferred macro-controlled dynamic fixed-effects model.

Diagnostic	Statistic	<i>p</i> -Value	Decision
Poolability F	4.149	<0.001	Reject pooled model
Hausman FE vs. RE	73.505	<0.001	Prefer fixed effects
Residual AR(1)	0.027	0.674	Do not reject
Residual AR(2)	0.071	0.105	Do not reject
Pesaran CD	−1.103	0.270	No residual dependence detected
Brown–Forsythe/Levene	2.783	<0.001	Robust inference required
Maximum within VIF	4.260	—	No material collinearity

4.6. Predictive Horizons on a Common Sample

The common-sample exercise retains 609 origin observations from 57 countries for all three forecast horizons. Private credit to GDP remains positive and statistically significant at each horizon. The coefficient is 0.00501 at one year, 0.00960 at two years, and 0.01266 at three years. A stacked country-clustered Wald test rejects equality of the three credit-depth coefficients ($p < 0.001$); all pairwise equality tests also reject at 1%.

Table 8 reports the common-sample horizon estimates, and Figure 4 displays their country-clustered confidence intervals.

Table 8. One-, two-, and three-year predictive regressions estimated on the identical 609-observation origin sample with horizon-specific country and origin-year effects.

<i>h</i>	<i>N</i>	Countries	Depth Coef.	SE	<i>p</i> (Depth)	<i>p</i> (Growth)	<i>p</i> (Rate)
1 year	609	57	0.00501	0.00123	<0.001	0.952	0.002
2 years	609	57	0.00960	0.00211	<0.001	0.369	0.001
3 years	609	57	0.01266	0.00243	<0.001	0.211	<0.001

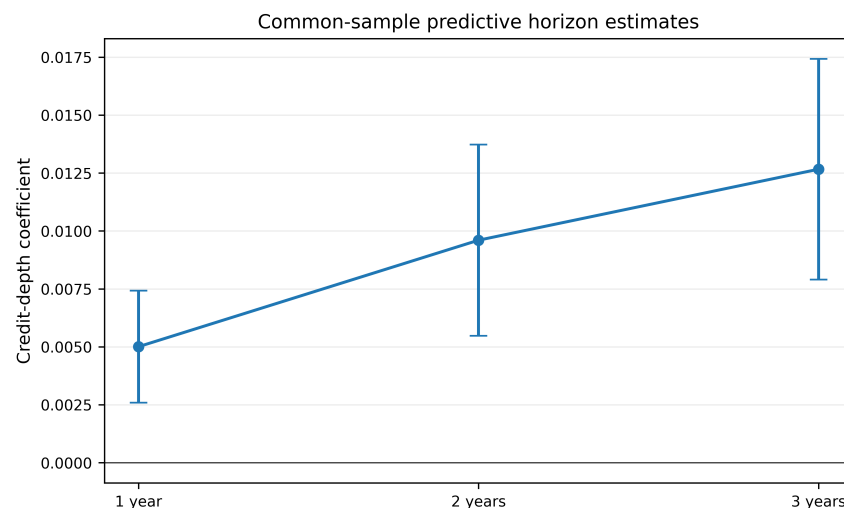


Figure 4. Credit-depth coefficients and 95% country-clustered confidence intervals from one-, two-, and three-year predictive regressions estimated on the same 609 origin observations from 57 countries. These are horizon-specific projections, not cumulative causal effects.

The larger coefficients at longer horizons should not be read as accumulated causal effects. Each regression has a different future dependent variable, and the coefficient therefore summarizes horizon-specific predictive association. The common-sample result establishes that the pattern is not generated by changing sample composition, but it does not identify a structural propagation mechanism.

Separate lag models produce coefficients of 0.00477, 0.00441, and 0.00394 for credit depth lagged one, two, and three years, respectively; each is significant below 1%. Replacing the one-year measure with a lagged three-year moving average yields a coefficient of 0.00460 ($p < 0.001$).

4.7. Formal Heterogeneity Tests

Point estimates differ across descriptive groups, but the pooled interaction tests do not establish statistically different credit-depth slopes. For average financial depth, the lower- and higher-group slopes are 0.00447 and 0.00484, and the interaction p -value is 0.903. For average capitalization, the slopes are 0.00418 and 0.00543, with an interaction p -value of 0.491. For initial NPL burden, the slopes are 0.00379 and 0.00526, with an interaction p -value of 0.370.

Broad regional slopes are positive in the pooled model: 0.00380 for the residual regional group, 0.00500 for Europe, and 0.00605 for Latin America and the Caribbean. Neither regional interaction is individually significant, and the joint equality test does not reject a common regional slope ($p = 0.418$). These results show why significance in one separate subgroup and insignificance in another cannot be interpreted as evidence that the relationship is concentrated in the first group.

Table 9 reports the pooled interaction estimates, and Figure 5 displays the implied group-specific slopes and confidence intervals.

Table 9. Pooled fixed-effects interaction estimates of credit-depth slope heterogeneity across country groups and broad regions.

Dimension	Group	Depth Coef.	SE	p (Slope)	p (Difference)
Average credit depth	Lower	0.00447	0.00277	0.107	—
Average credit depth	Higher	0.00484	0.00109	<0.001	0.903
Average capitalization	Lower	0.00418	0.00153	0.006	—
Average capitalization	Higher	0.00543	0.00108	<0.001	0.491
Initial NPL burden	Lower	0.00379	0.00107	<0.001	—
Initial NPL burden	Higher	0.00526	0.00133	<0.001	0.370
Broad region	Other regions	0.00380	0.00133	0.004	Reference
Broad region	Europe	0.00500	0.00136	<0.001	0.529
Broad region	Latin America & Caribbean	0.00605	0.00160	<0.001	0.190

Notes: The difference p -value tests the interaction relative to the lower or reference group. The joint regional equality test has $p = 0.418$. $N = 754$ and countries = 58 in every pooled interaction model.

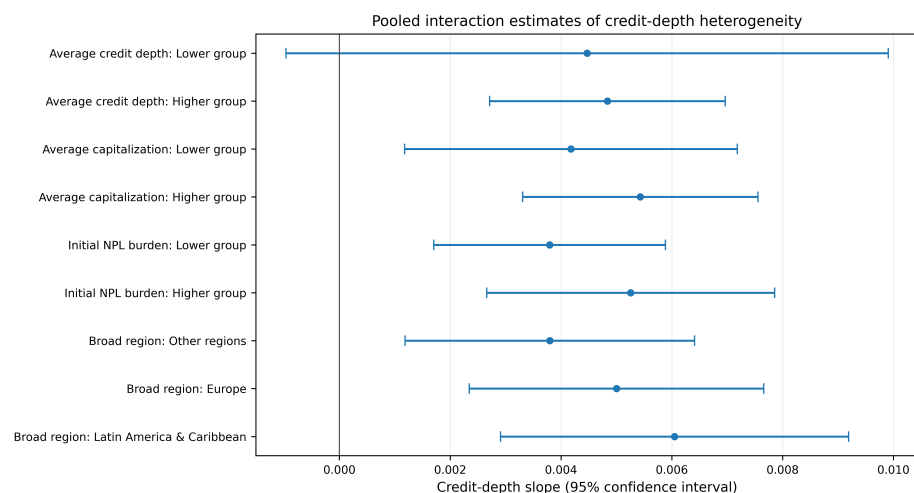


Figure 5. Group-specific credit-depth slopes and 95% country-clustered confidence intervals derived from pooled interaction models with country and year fixed effects. Formal interaction tests, rather than within-group significance, determine whether slopes differ.

A pooled interaction model also finds no statistically significant post-2020 change in the credit-depth slope. The pre-2020 coefficient is 0.00487; the post-2020 difference is -0.00035 ($p = 0.348$). Interactions for credit growth, lending rates, and capital are likewise insignificant. The common pandemic level effect is absorbed by year fixed effects.

4.8. Sample, Scale, and Influence Robustness

The credit-depth result survives the principal robustness tests. It remains positive and statistically significant when 2025 is included, when the sample starts in 2008 or 2015, when countries require at least 10 observations, and in the balanced 2010–2024 panel. It is also robust to excluding private-credit-to-GDP values above 200%, replacing capital with Tier 1 capital, estimating NPLs in levels, and using log credit depth.

In leave-one-country-out estimates, the credit-depth coefficient ranges from 0.00321 to 0.00442 and remains significant after every exclusion. The lending-rate coefficient remains positive, although its least favorable p -value is 0.057. The unemployment coefficient remains positive and significant throughout.

Table 10 reports selected sample, scale, outcome, and capital-measure robustness checks.

Table 10. Selected robustness tests for the credit-depth result under alternative samples, scales, outcomes, and capital measures.

Specification	N	Countries	Depth Coef.	p (Depth)	p (Rate)	p (Unemp.)
Preferred macro-controlled	746	58	0.00377	<0.001	0.028	<0.001
Countries with ≥ 10 observations	684	49	0.00394	<0.001	0.024	<0.001
Balanced 2010–2024	420	28	0.00350	0.005	0.147	0.001
Credit depth $\leq 200\%$ GDP	735	58	0.00394	<0.001	0.024	<0.001
Tier 1 capital alternative	773	59	0.00363	<0.001	0.011	<0.001
NPL ratio in levels	746	58	0.03174	0.018	0.152	0.012
Log credit depth	746	58	0.24557	<0.001	0.016	<0.001

4.9. Bias Correction and System GMM

The half-panel jackknife on the balanced panel preserves a positive credit-depth coefficient, with wider uncertainty due to the smaller sample. This result is consistent with the fixed-effects estimate but does not eliminate all finite-sample concerns.

The parsimonious system-GMM model uses 26 instruments for 55 countries. The Hansen test ($p = 0.491$) and AR(2) test ($p = 0.417$) do not reject the specification. However, the sum of the dynamic coefficients is 0.994, above the pooled-ordinary least squares (OLS) persistence bound of 0.961. The credit-depth coefficient becomes negative and imprecise. This combination is a warning that internal instruments are weakly informative in a highly persistent process. System GMM is reported, but it is not used to claim causal identification.

This finding illustrates why overidentification tests cannot be interpreted mechanically. A model can pass Hansen and AR(2) tests while producing economically implausible persistence. Instrument discipline and coefficient bounds are therefore integral to the credibility assessment (Roodman, 2009).

4.10. Reverse-Feedback Placebo and Secondary Descriptive Evidence

Lead-placebo tests sharpen the timing interpretation. Future credit depth, future lending rates, and future capital do not predict current NPLs. Future credit growth, however, enters negatively and significantly, whereas lagged credit growth is null. This pattern is consistent with reverse feedback from current NPLs to subsequent credit retrenchment. It explains why a high credit stock can predict later asset-quality deterioration even when annual credit growth does not.

Appendix B reports the descriptive profile around 27 strong credit-deepening episodes. Mean NPLs rise after the event, but confidence intervals are wide and medians remain near zero. The event profile is therefore treated as contextual evidence only and carries no weight in the paper's primary inference.

5. Discussion

5.1. Stock Versus Flow and Forecast Horizon

The central finding is not simply that “more credit is risky.” It is that the stock of private credit relative to GDP is more informative than the annual flow of new credit once persistence and macroeconomic conditions are controlled. This distinction is consistent with the leverage-cycle literature (Jordà et al., 2013; Schularick & Taylor, 2012) and with

evidence that high NPLs constrain subsequent lending (Gjeçi et al., 2023). A low current growth rate may therefore signal either prudence or post-deterioration retrenchment; it cannot be interpreted without the inherited credit stock.

The common-sample horizon exercise strengthens the early-warning interpretation without turning it into a causal impulse response. Credit depth predicts NPLs one, two, and three years ahead, and the stacked test rejects equality across horizons. Because the future outcome changes with h , however, the larger longer-horizon coefficients are not directly comparable to accumulated treatment effects. Their defensible interpretation is narrower: the information in current credit depth remains relevant, and becomes statistically stronger, for more distant asset-quality outcomes.

5.2. Macroeconomic Channels

The positive lending-rate coefficient supports a debt-service mechanism. Higher borrowing costs can reduce repayment capacity, particularly where leverage is already high. The result remains after inflation is controlled, suggesting that nominal lending costs contain information beyond the general price environment. Unemployment is the strongest lagged macroeconomic predictor, consistent with household income loss, weaker demand, and corporate cash-flow pressure.

Lagged GDP growth is imprecise, while contemporaneous GDP growth is negative and significant. This timing difference suggests that output shocks may reach repayment performance more quickly than labor-market adjustment. It also demonstrates why the timing of macro controls should be disclosed rather than selected opportunistically.

5.3. Interpreting the Capital and Heterogeneity Results

The direct capital coefficient is null, and the credit-growth-by-capital interaction is also insignificant. These findings do not contradict the role of capital in resilience. Capital absorbs losses after borrower distress and supports confidence and credit supply; it need not prevent loans from becoming non-performing. Capital ratios also react endogenously to risk recognition, provisions, and balance-sheet adjustment (Alessi et al., 2021; Anani & Owusu, 2023; Gehrig & Iannino, 2021).

The pooled heterogeneity tests change the substantive interpretation of the separate subgroup estimates. Although the point estimate is somewhat larger in highly capitalized and high-initial-NPL systems, neither difference is statistically established. The same is true for average credit depth and broad regions. The evidence therefore does not justify claims that the association is concentrated in one group. At most, some partitions are estimated more precisely, and contextual differences remain a hypothesis for future bank-level or quasi-experimental work.

5.4. Formal Assessment of H1–H5

Table 11 evaluates every hypothesis against the preferred model, timing checks, interaction tests, and relevant theory. Statistical support is not inferred from isolated subgroup significance; it requires the coefficient or formal interaction specified by the hypothesis.

5.5. Regulatory Implications

The first implication is operational: supervisory dashboards should track the level and multi-year average of private credit to GDP alongside annual credit growth. A deceleration in the flow does not necessarily mean that vulnerability is falling; it may follow a long build-up of leverage or already-deteriorating asset quality.

Table 11. Assessment of the pre-specified hypotheses using the preferred model and targeted robustness tests. The assessment is based on the coefficient or formal interaction specified ex ante, not on isolated subgroup significance.

H	Prediction	Evidence and Interpretation	Assessment
H1	Credit depth predicts higher subsequent NPLs.	Preferred coefficient 0.00377 ($p < 0.001$), robust across inference procedures, samples, forecast horizons, and the high-NPL outcome; consistent with accumulated-leverage and credit-cycle evidence (Beck et al., 2015; Jordà et al., 2013; Schularick & Taylor, 2012).	Supported
H2	Faster annual real credit growth predicts higher NPLs.	Lagged growth is null, while future growth is negative in the lead placebo, consistent with post-NPL credit retrenchment and two-way lending–NPL feedback (Foos et al., 2010; Gjeçi et al., 2023).	Not supported
H3	Higher lending rates predict higher NPLs.	Preferred coefficient 0.1183 and significant under country, two-way, DK, and wild inference; consistent with a debt-service channel (Beck et al., 2015; Foglia, 2022; Louzis et al., 2012).	Supported
H4a	Higher lagged GDP growth predicts lower NPLs.	Lagged GDP growth is negative but imprecise; contemporaneous GDP growth is negative and significant, suggesting a within-year repayment-capacity channel.	Partially supported
H4b	Higher lagged unemployment predicts higher NPLs.	Coefficient 0.01382 and robust under all reported inference procedures; consistent with household-income and firm-demand channels (Foglia, 2022; Louzis et al., 2012).	Supported
H5a	Higher capital directly predicts lower NPLs.	Direct capital and the Tier 1 alternative are statistically null. Capital may absorb losses and sustain lending without preventing borrower default, and observed ratios adjust endogenously (Alessi et al., 2021; Anani & Owusu, 2023).	Not supported
H5b	Capital attenuates the credit-growth/NPL relation.	The credit-growth-by-capital interaction is statistically insignificant; the aggregate data do not identify a moderation mechanism.	Not supported

Second, the preferred model supports a tiered escalation rule rather than a universal numerical threshold. A system above the sample’s 75th percentile of credit depth (83.05% of GDP) warrants closer monitoring as a descriptive high-stock state. If that state is accompanied by higher lending rates and worsening unemployment, supervisors should intensify loan-vintage reviews, sectoral concentration analysis, collateral revaluation, and adverse-scenario testing. If the gross NPL ratio also approaches or exceeds the EBA’s 5% monitoring trigger, NPE strategy and governance should receive additional attention (European Banking Authority, 2018). These are combined screening signals, not a model-estimated safe/unsafe frontier, and the formal heterogeneity tests do not support region-specific cutoffs.

Third, stress tests should distinguish borrower-default mechanisms from bank-resilience mechanisms. Lending rates and labor-market stress help identify repayment pressure, whereas capital buffers primarily determine the capacity to absorb losses and maintain intermediation. Countercyclical buffers are therefore most useful before vulnerabilities become embedded (Benbouzid et al., 2022); after impairment emerges, resolution mechanisms, securitization, and asset-management companies may accelerate balance-sheet repair (Brei et al., 2023; Wengerek et al., 2022).

Fourth, model governance should preserve the distinction between forecasting and identification. Early-warning models can justify enhanced monitoring even when they do not identify a causal effect. Their outputs should be combined with underwriting standards, debt-service ratios, property-price indicators, and supervisory judgment rather than used as automatic policy triggers.

5.6. Methodological Contribution

The paper's methodological contribution is transparency rather than estimator novelty. The dynamic order is selected from residual diagnostics. Multiple inferential procedures are reported. Outliers are audited rather than silently winsorized. System GMM is not promoted after it fails a persistence-bound check. These practices matter because sophisticated estimators can create an appearance of identification without strong instruments or credible timing.

The lead-placebo result is equally important. It reveals that credit growth is endogenous to asset quality. A positive lagged coefficient alone would not establish a one-way lending-to-default mechanism. Early-warning models should therefore distinguish forecasting performance from causal interpretation.

5.7. Limitations and Research Agenda

The study has five main limitations. First, it uses aggregate country-level data and cannot observe bank-specific underwriting, borrower composition, collateral, loan vintage, or sectoral concentration. Second, NPL definitions and regulatory recognition differ across countries. Third, the panel is unbalanced and conditional on joint data availability. Fourth, the macro-augmented model includes the highest-coverage controls but not a complete structural set. Exchange-rate exposure, real policy conditions, property prices, institutional quality, and macroprudential intensity should be integrated where coverage permits. Fifth, the design lacks an external source of exogenous credit variation.

Future research should combine the present architecture with bank-level credit registers and policy discontinuities. Staggered macroprudential reforms, loan-to-value or debt-service limits, foreign funding shocks, or carefully justified exposure-based instruments could support event-study or difference-in-differences designs. Pre-trend tests, treatment heterogeneity, and exclusion restrictions would be required before making causal claims.

6. Conclusions

This paper evaluates whether the accumulated scale of private credit contains early-warning information for banking-sector asset quality. It combines Passport banking indicators with macroeconomic controls for 58 countries over 2010–2024 and estimates a second-order dynamic fixed-effects model.

The principal result is stable: higher private credit to GDP in the preceding year is associated with a higher NPL ratio. The preferred coefficient is 0.00377, implying approximately 0.15 additional NPL percentage points after a 10-percentage-point increase in credit depth at the sample median. In the alternative high-NPL outcome, the same 10-percentage-point increase predicts a 2.78-percentage-point higher conditional probability of reaching the 5% monitoring reference. On a strictly common sample, credit depth remains positive at one-, two-, and three-year horizons, and a stacked Wald test rejects equal horizon-specific coefficients. These estimates are predictive projections, not cumulative causal effects.

Lending rates and unemployment are also positive predictors. Annual real credit growth and capital ratios are not robust. Formal pooled interactions do not reject equal credit-depth slopes across average-depth, capitalization, initial-NPL, or broad regional partitions; differences in separate subgroup significance therefore mainly reflect precision and composition. Lead-placebo evidence shows that higher current NPLs are followed by lower future credit growth, confirming feedback from asset quality to lending. System GMM passes conventional tests but fails a persistence-bound credibility check.

The appropriate conclusion is predictive, not causal. Private credit depth is a slow-moving vulnerability indicator whose information content exceeds that of annual credit

growth in this panel. Regulators should monitor inherited leverage jointly with lending costs, labor-market stress, underwriting quality, loan vintages, and sectoral exposure. Researchers should distinguish forecasting from identification and should not treat conventional GMM diagnostics or subgroup significance patterns as sufficient evidence of causality or heterogeneity.

The replication scripts, country crosswalk, derived regression outputs, and figure-generation materials are documented in Supplementary Materials S1–S5.

Supplementary Materials: The following supporting information can be downloaded at <https://www.mdpi.com/article/10.3390/jrfm19080594/s1>: S1, a script for constructing the licensed Passport panel; S2, a World Bank API script for downloading, harmonizing, and lagging GDP growth, inflation, and unemployment; S3, scripts for stepped macroeconomic specifications, common-sample horizon tests, pooled heterogeneity interactions, robustness checks, and the descriptive event profile; S4, the benchmark calibration, alternative high-NPL outcome, and country-region crosswalk; S5, high-resolution figure generation, derived regression outputs and figures, and the Scite reference-audit file. The proprietary Passport source file is not redistributed, and reproduction requires lawful access to the original export.

Author Contributions: Conceptualization, M.A.L.M.; methodology, M.A.L.M.; software, A.A.A.-R.; validation, A.A.A.-R.; formal analysis, G.S.V.V.; investigation, G.S.V.V.; resources, W.H.; data curation, W.H.; writing—original draft preparation, P.Z.; writing—review and editing, P.Z.; visualization, N.V.; supervision, N.V. All authors have read and agreed to the published version of the manuscript.

Funding: This research received no external funding.

Institutional Review Board Statement: Not applicable.

Informed Consent Statement: Not applicable.

Data Availability Statement: The Passport source data are proprietary and cannot be redistributed without permission from Euromonitor International. World Development Indicators are publicly available through the World Bank API. The supplementary package provides scripts to reconstruct the WDI controls, merge them with a lawfully obtained Passport export, and reproduce the reported derived outputs. Requests concerning access to licensed source data should be directed to the data provider.

Acknowledgments: The authors acknowledge Euromonitor International and the World Bank as data providers. Scite was used for literature discovery and citation-context screening. During preparation of this manuscript, the authors used OpenAI ChatGPT (GPT-5.6 Pro) for language refinement, document structuring, LaTeX adaptation, and formatting. The authors reviewed and edited the output and take full responsibility for the content of this publication.

Conflicts of Interest: The authors declare no conflicts of interest.

Abbreviations

The following abbreviations are used in this manuscript:

NPL	Non-performing loan
WDI	World Development Indicators
GMM	Generalized method of moments
DK	Driscoll–Kraay
GDP	Gross domestic product
CPI	Consumer price index
VIF	Variance inflation factor

Appendix A. Additional Timing Evidence

Table A1 reports the alternative lag and moving-average timing measures.

Table A1. Alternative credit-depth timing measures in second-order dynamic country and year fixed-effects models.

Timing Measure	N	Countries	Depth Coef.	SE	p-Value
Credit depth, $t - 1$	754	58	0.00477	0.00102	<0.001
Credit depth, $t - 2$	754	58	0.00441	0.00094	<0.001
Credit depth, $t - 3$	753	58	0.00394	0.00086	<0.001
Lagged 3-year moving average	753	58	0.00460	0.00098	<0.001

The conventional varying-sample horizon estimates are 0.00496, 0.00945, and 0.01266 at one, two, and three years, respectively. They closely match the common-sample estimates reported in the main text.

Appendix B. Descriptive Credit-Deepening Event Profile

The descriptive event exercise identifies 27 non-overlapping episodes in which private credit to GDP rises by at least 15 percentage points over three years. Relative to the year before the episode, the mean NPL change is 0.27 percentage points in the event year, 0.81 after one year, 0.75 after two years, and 1.07 after three years. Confidence intervals are wide and medians remain close to zero. The profile is therefore suggestive, not causal, and is not used to support the principal conclusions. Figure A1 visualizes this descriptive profile.

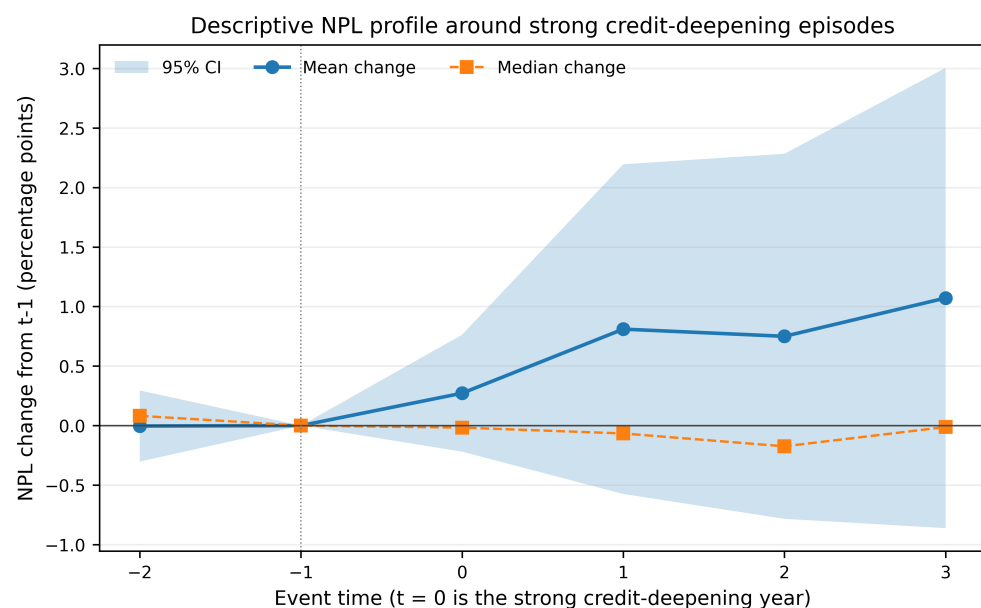


Figure A1. Descriptive mean and median NPL changes, relative to $t - 1$, around 27 non-overlapping episodes in which private credit to GDP rose by at least 15 percentage points over three years. Shading denotes the 95% confidence interval for the mean. No untreated counterfactual or parallel-trends assumption is imposed.

Appendix C. Economies in the Preferred Sample

Table A2 lists all 58 economies by geographical region.

Table A2. Countries in the preferred estimation sample, grouped by geographical region. Region assignments are descriptive and are not used as causal treatments.

Region	N	Countries
Africa	6	Algeria; Angola; Kenya; Nigeria; South Africa; Tanzania
Asia	14	Azerbaijan; Bangladesh; Georgia; Hong Kong SAR, China; Indonesia; Israel; Jordan; Malaysia; Pakistan; Philippines; South Korea; Sri Lanka; Thailand; Uzbekistan
Europe	23	Austria; Belarus; Belgium; Bosnia and Herzegovina; Bulgaria; Czech Republic; Estonia; Finland; France; Germany; Greece; Ireland; Italy; Latvia; Lithuania; Netherlands; North Macedonia; Norway; Portugal; Romania; Russia; Spain; Ukraine
Latin America and the Caribbean	13	Argentina; Bolivia; Brazil; Colombia; Costa Rica; Dominican Republic; Guatemala; Honduras; Mexico; Panama; Paraguay; Peru; Uruguay
North America	1	United States
Oceania	1	Australia

Appendix D. Representative Regulatory and Accounting Definitions of Problem Loans

Table A3 documents representative frameworks covering the international benchmark and major regulatory settings in the sample. It is not an exhaustive 58-country concordance. The purpose is to make explicit why cross-country NPL ratios may differ even when underlying borrower risk is similar.

Table A3. Representative cross-jurisdictional differences in problem-loan definitions and reporting. The exhibit summarizes official guidance and illustrates comparability limitations; it does not recode the proprietary Passport series.

Framework/Jurisdiction	Primary Operational Rule or Reporting Bucket	Restructuring/Scope Feature	Comparability Implication
IMF FSI Guide	Normally 90 days or more past due or earlier when evidence indicates non-performance; the gross loan amount is recorded.	Replacement or restructured loans remain non-performing until performance or write-off criteria are met.	International benchmark, but national rules may be stricter and exit criteria can differ (International Monetary Fund, 2019).
European Union/EEA	Harmonized NPE and forbore-exposure framework; a 5% gross NPL ratio triggers enhanced NPE strategy and governance.	The 5% value is a supervisory trigger, not an optimal target or universal definition.	Useful monitoring reference but not a safe/unsafe cutoff (European Banking Authority, 2018).
Australia	Default when the borrower is unlikely to pay in full and/or is 90 days or more past due; the whole exposure is classified non-performing.	Restructured exposures have probation and return-to-performance criteria; collateral does not prevent NPE status.	Combines days-past-due and unlikeliness-to-pay tests (Australian Prudential Regulation Authority, 2022).
United States	Call Reports separate 30–89 days past due, 90+ days past due and still accruing, and nonaccrual loans.	Past-due and nonaccrual are distinct reporting buckets rather than one universal NPL category.	Cross-country NPL ratios do not map one-for-one to U.S. Call Report categories (Federal Deposit Insurance Corporation, 2025).

Table A3. Cont.

Framework/ Jurisdiction	Primary Operational Rule or Reporting Bucket	Restructuring/Scope Feature	Comparability Implication
Peru	SBS reports <i>cartera atrasada</i> as overdue loans and judicial-collection loans; a broader high-risk portfolio also includes refinanced and restructured loans.	The broader prudential measure exceeds a narrow past-due-only concept.	Published delinquency and high-risk ratios differ according to the inclusion of refinanced and restructured credit (Superintendencia de Banca, Seguros y AFP, 2025).
Hong Kong SAR	HKMA classifies loans as pass, special mention, substandard, doubtful, or loss; the last three form classified loans.	Delinquency statistics may additionally use more-than-three-month overdue and rescheduled-loan concepts.	Classification-based and days-past-due measures coexist and need not equal a single NPL ratio (Hong Kong Monetary Authority, 2021).

References

- Alessi, L., Bruno, B., & Carletti, E. (2021). Cover your assets: Non-performing loans and coverage ratios in Europe. *Economic Policy*, 36(108), 685–733. [\[CrossRef\]](#)
- Altunbaş, Y., Marqués-Ibáñez, D., & van Leuvensteijn, M. (2022). Market power and bank systemic risk: Role of securitization and bank capital. *Journal of Banking & Finance*, 138, 106451. [\[CrossRef\]](#)
- Anani, M., & Owusu, F. (2023). Regulatory capital and bank risk-resilience amid the COVID-19 pandemic: How are the Basel reforms faring? *Finance Research Letters*, 52, 103591. [\[CrossRef\]](#) [\[PubMed\]](#)
- Arellano, M., & Bond, S. (1991). Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. *Review of Economic Studies*, 58(2), 277–297. [\[CrossRef\]](#)
- Arellano, M., & Bover, O. (1995). Another look at the instrumental variable estimation of error-components models. *Journal of Econometrics*, 68(1), 29–51. [\[CrossRef\]](#)
- Ari, A., Chen, S., & Ratnovski, L. (2021). The dynamics of non-performing loans during banking crises: A new database with post-COVID-19 implications. *Journal of Banking & Finance*, 133, 106140. [\[CrossRef\]](#)
- Australian Prudential Regulation Authority. (2022). *APS 220 credit risk management*. Prudential Standard. Available online: <https://www.apra.gov.au/standards/aps-220> (accessed on 21 July 2026).
- Baselga-Pascual, L., Lobán, L., & Myllymäki, E.-R. (2025). Bank credit risk and sovereign debt exposure: Moral hazard or hedging? *Finance Research Letters*, 71, 106454. [\[CrossRef\]](#)
- Beck, R., Jakubík, P., & Piloju, A. (2015). Key determinants of non-performing loans: New evidence from a global sample. *Open Economies Review*, 26(3), 525–550. [\[CrossRef\]](#)
- Benbouzid, N., Kumar, A., & Mallick, S. (2022). Bank credit risk and macro-prudential policies: Role of counter-cyclical capital buffer. *Journal of Financial Stability*, 63, 101084. [\[CrossRef\]](#)
- Berger, A. N., & DeYoung, R. (1997). Problem loans and cost efficiency in commercial banks. *Journal of Banking & Finance*, 21(6), 849–870. [\[CrossRef\]](#)
- Bilgin, M. H., Demir, E., & Tarazi, A. (2021). Economic uncertainty and bank stability: Conventional vs. Islamic banking. *Journal of Financial Stability*, 56, 100911. [\[CrossRef\]](#)
- Bitar, M., & Tarazi, A. (2022). A note on regulatory responses to COVID-19 pandemic: Balancing banks' solvency and contribution to recovery. *Journal of Financial Stability*, 60, 101009. [\[CrossRef\]](#) [\[PubMed\]](#)
- Blundell, R., & Bond, S. (1998). Initial conditions and moment restrictions in dynamic panel data models. *Journal of Econometrics*, 87(1), 115–143. [\[CrossRef\]](#)
- Brei, M., Gambacorta, L., & Lucchetta, M. (2023). How effective are bad bank resolutions? New evidence from Europe. *Journal of Financial Stability*, 67, 101153. [\[CrossRef\]](#)
- Callaway, B., & Sant'Anna, P. H. C. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2), 200–230. [\[CrossRef\]](#)
- Cantú, C., Claessens, S., & Gambacorta, L. (2022). How do bank-specific characteristics affect lending? New evidence based on credit registry data from Latin America. *Journal of Banking & Finance*, 135, 105818. [\[CrossRef\]](#)
- Couaillier, C., & Henricot, D. (2023). How do markets react to tighter bank capital requirements? *Journal of Banking & Finance*, 151, 106832. [\[CrossRef\]](#)

- Çolak, G., & Öztekin, Ö. (2021). The impact of COVID-19 pandemic on bank lending around the world. *Journal of Banking & Finance*, 133, 106207. [CrossRef]
- Demirgüç-Kunt, A., Pedraza, Á., & Ruiz-Ortega, C. (2021). Banking sector performance during the COVID-19 crisis. *Journal of Banking & Finance*, 133, 106305. [CrossRef] [PubMed]
- Dhaene, G., & Jochmans, K. (2015). Split-panel jackknife estimation of fixed-effect models. *Review of Economic Studies*, 82(3), 991–1030. [CrossRef]
- Driscoll, J. C., & Kraay, A. (1998). Consistent covariance matrix estimation with spatially dependent panel data. *Review of Economics and Statistics*, 80(4), 549–560. [CrossRef]
- Duncan, E., Horváth, Á., & Iercosan, D. (2022). COVID-19 as a stress test: Assessing the bank regulatory framework. *Journal of Financial Stability*, 61, 101016. [CrossRef] [PubMed]
- Erdaş, M. L., & Ezanoğlu, Z. (2022). How do bank-specific factors impact non-performing loans: Evidence from G20 countries. *Journal of Central Banking Theory and Practice*, 11(2), 97–122. [CrossRef]
- Euromonitor International. (2026). *Passport statistics: Banking and credit indicators, 1990–2025* [data set]. Available online: <https://www.euromonitor.com/passport> (accessed on 22 June 2026).
- European Banking Authority. (2018). *Final guidelines on management of non-performing and forborne exposures*. European Banking Authority. Available online: <https://www.eba.europa.eu/publications-and-media/press-releases/eba-publishes-final-guidance-management-non-performing-and-forborne-exposures> (accessed on 21 July 2026).
- Federal Deposit Insurance Corporation. (2025). *FFIEC 051 reports of condition and income instructions: Schedule RC-N, past due and nonaccrual loans, leases, and other assets*. Federal Deposit Insurance Corporation and Federal Financial Institutions Examination Council. Available online: <https://www.fdic.gov/bank-financial-reports/ffiec-reports-condition-and-income-instructions-ffiec-051-report-form-0> (accessed on 21 July 2026).
- Ferrari, S., Roy, P. V., & Vespro, C. (2021). Sensitivity of credit risk stress test results: Modelling issues with an application to Belgium. *Journal of Financial Stability*, 52, 100805. [CrossRef]
- Ferreira, C. (2022). Determinants of non-performing loans: A panel data approach. *International Advances in Economic Research*, 28(3–4), 133–153. [CrossRef]
- Foglia, M. (2022). Non-performing loans and macroeconomics factors: The Italian case. *Risks*, 10(1), 21. [CrossRef]
- Foos, D., Nordén, L., & Weber, M. (2010). Loan growth and riskiness of banks. *Journal of Banking & Finance*, 34(12), 2929–2940. [CrossRef]
- Fungáčová, Z., Turk, R., & Weill, L. (2021). High liquidity creation and bank failures. *Journal of Financial Stability*, 57, 100937. [CrossRef]
- Gehrig, T., & Iannino, M. C. (2021). Did the Basel process of capital regulation enhance the resiliency of European banks? *Journal of Financial Stability*, 55, 100904. [CrossRef]
- Gjeçi, A., Marinč, M., & Rant, V. (2023). Non-performing loans and bank lending behaviour. *Risk Management*, 25(1), 7. [CrossRef]
- Hong Kong Monetary Authority. (2021). *Loan classification system*. Guide to Hong Kong Monetary, Banking and Financial Terms. Available online: https://www.hkma.gov.hk/eng/data-publications-and-research/guide-to-monetary-banking-and-financial-terms/loan_classificat_sys/ (accessed on 21 July 2026).
- International Monetary Fund. (2019). *Financial soundness indicators compilation guide 2019*. International Monetary Fund. [CrossRef]
- Jordà, Ö., Schularick, M., & Taylor, A. M. (2013). When credit bites back. *Journal of Money, Credit and Banking*, 45(S2), 3–28. [CrossRef]
- Karadima, M., & Louri, H. (2021). Economic policy uncertainty and non-performing loans: The moderating role of bank concentration. *Finance Research Letters*, 38, 101458. [CrossRef]
- Kasahara, H., Sawada, Y., & Suzuki, M. (2024). The effect of bank recapitalization policy on credit allocation, investment, and productivity: Evidence from a banking crisis in Japan. *Journal of Banking & Finance*, 158, 107047. [CrossRef]
- King, R. G., & Levine, R. (1993). Finance and growth: Schumpeter might be right. *Quarterly Journal of Economics*, 108(3), 717–737. [CrossRef]
- Kryzanowski, L., Liu, J., & Zhang, J. (2023). Effect of COVID-19 on non-performing loans in China. *Finance Research Letters*, 52, 103372. [CrossRef] [PubMed]
- Levine, R. (2005). Finance and growth: Theory and evidence. *Handbook of Economic Growth*, 1A, 865–934. [CrossRef]
- Louzis, D. P., Vouldis, A. T., & Metaxas, V. L. (2012). Macroeconomic and bank-specific determinants of non-performing loans in Greece: A comparative study of mortgage, business and consumer loan portfolios. *Journal of Banking & Finance*, 36(4), 1012–1027. [CrossRef]
- MacKinnon, J. G., Nielsen, M. Ø., & Webb, M. D. (2023). Cluster-robust inference: A guide to empirical practice. *Journal of Econometrics*, 232(2), 272–299. [CrossRef]
- Mdaghri, A. A. (2022). How does bank liquidity creation affect non-performing loans in the MENA region? *International Journal of Emerging Markets*, 17(7), 1635–1658. [CrossRef]
- Mehmood, A., & De Luca, F. (2023). How does non-interest income affect bank credit risk? Evidence before and during the COVID-19 pandemic. *Finance Research Letters*, 53, 103657. [CrossRef] [PubMed]

- Mian, A., Sufi, A., & Verner, E. (2017). Household debt and business cycles worldwide. *Quarterly Journal of Economics*, 132(4), 1755–1817. [CrossRef]
- Nickell, S. (1981). Biases in dynamic models with fixed effects. *Econometrica*, 49(6), 1417–1426. [CrossRef]
- Onali, E., Ginesti, G., & Cardillo, G. (2024). Market reaction to the expected loss model in banks. *Journal of Financial Stability*, 74, 100884. [CrossRef]
- Park, C.-Y., & Shin, K. (2021). COVID-19, nonperforming loans, and cross-border bank lending. *Journal of Banking & Finance*, 133, 106233. [CrossRef]
- Pesaran, M. H. (2015). Testing weak cross-sectional dependence in large panels. *Econometric Reviews*, 34(6–10), 1089–1117. [CrossRef]
- Pozo, J., & Rojas, Y. (2023). Bank competition and credit risk: The case of Peru. *Journal of Financial Stability*, 66, 101119. [CrossRef]
- Quang, P. T., Vu, H. V., & Tran, H. P. (2022). Do non-performing loans impact bank efficiency? *Finance Research Letters*, 46, 102393. [CrossRef]
- Roodman, D. (2009). A note on the theme of too many instruments. *Oxford Bulletin of Economics and Statistics*, 71(1), 135–158. [CrossRef]
- Saliba, C., Farmanesh, P., & Athari, S. A. (2023). Does country risk impact the banking sectors' non-performing loans? Evidence from BRICS emerging economies. *Financial Innovation*, 9(1), 86. [CrossRef] [PubMed]
- Schularick, M., & Taylor, A. M. (2012). Credit booms gone bust: Monetary policy, leverage cycles, and financial crises, 1870–2008. *American Economic Review*, 102(2), 1029–1061. [CrossRef]
- Soenen, N., & Vander Vennet, R. (2022). Determinants of European banks' default risk. *Finance Research Letters*, 47, 102557. [CrossRef]
- Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2), 175–199. [CrossRef]
- Superintendencia de Banca, Seguros y AFP. (2025). *Sistema financiero: El crédito se recupera y mejora la calidad de cartera a marzo 2025*. Informe de Estabilidad del Sistema Financiero, Resumen Institucional. Available online: <https://www.sbs.gob.pe/boletin/detalleboletin/idbulletin/2363> (accessed on 21 July 2026).
- Takahashi, F. L., & Vasconcelos, M. R. (2024). Bank efficiency and undesirable output: An analysis of non-performing loans in the Brazilian banking sector. *Finance Research Letters*, 59, 104651. [CrossRef]
- Wengerek, S. T., Hippert, B., & Uhde, A. (2022). Risk allocation through securitization: Evidence from non-performing loans. *Quarterly Review of Economics and Finance*, 86, 48–64. [CrossRef]
- World Bank. (2026a). *Gdp growth (annual %)* [data set]. World Development Indicators. Available online: <https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG> (accessed on 23 June 2026).
- World Bank. (2026b). *Inflation, consumer prices (annual %)* [data set]. World Development Indicators. Available online: <https://data.worldbank.org/indicator/FP.CPI.TOTL.ZG> (accessed on 23 June 2026).
- World Bank. (2026c). *Unemployment, total (% of total labor force), modeled ilo estimate* [data set]. World Development Indicators. Available online: <https://data.worldbank.org/indicator/SL.UEM.TOTL.ZS> (accessed on 23 June 2026).

Disclaimer/Publisher's Note: The statements, opinions and data contained in all publications are solely those of the individual author(s) and contributor(s) and not of MDPI and/or the editor(s). MDPI and/or the editor(s) disclaim responsibility for any injury to people or property resulting from any ideas, methods, instructions or products referred to in the content.