



Debt Overhang, Investment and Economic Growth in the Philippines (Revisiting La Década Perdida 1970 to 2024)

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Abstract

This study examines whether annual changes in external debt and actual debt-service payments are associated with economic growth and investment in the Philippines, and whether these relationships change around the end of the Lost Decade. Independently retrieved World Bank data cover 1970–2024. Historical comparisons are combined with dynamic regressions, heteroskedasticity and autocorrelation consistent inference, an exploratory autoregressive distributed lag levels assessment, and recursive wild-bootstrap stability tests. The principal growth and capital formation growth equations use 54 and 53 observations, respectively. Higher debt burdens coincide with negative average per capita growth during 1983–1992, followed by improved average performance from 1993. Debt and service change coefficients are not statistically distinguishable from zero in the principal equations, and their confidence intervals remain broad. Bootstrap tests do not establish a robust 1993 parameter shift at the 5% level. Significant interaction and nonlinear alternatives fail specification checks, while weak output adjustment limits the long-run interpretation. Inflation and depreciation have negative conditional associations with growth. These findings qualify the evidence available from annual aggregate data without disproving persistent debt overhang or identifying a causal restructuring effect or borrowing threshold. Numerical replication confirms the estimates, and recent official-source substitutions leave the principal findings unchanged. Policy implications emphasize distinguishing outstanding obligations from realized payments and evaluating sustained recovery. Full historical source harmonization remains incomplete.

Keywords: *debt overhang; external debt; debt service; capital formation; Brady Plan; Philippines; parameter stability*

1 Introduction

The Philippine debt crisis of the 1980s raises a question that extends beyond the management of missed payments: when does relief from immediate financing pressure become a durable recovery in investment and output? In Chapter 2 of *Risk and Resilience in the Philippine Financial System*, Fernandez and Remolona (2026) interpret *La Década Perdida* as a prolonged debt overhang that repeated rescheduling did not adequately resolve. Their account distinguishes the restoration of payment capacity from the removal of obligations that discouraged recovery. This distinction provides the starting point for our study, while leaving its empirical implications open to examination.

External indebtedness can support productive expenditure, but its economic consequences depend on repayment prospects, financing conditions, and the uses of borrowed resources. A high outstanding stock, heavy payments in a particular year, and exposure to refinancing are related but different conditions. Kose et al. (2020) emphasize both the potential benefits of borrowing and the costs associated with rollover pressure, reduced policy space, and excessive accumulation. For the Philippines, these distinctions matter because the debt crisis coincided with exchange rate instability, inflation, political disruption, and a contraction in investment. Attributing the subsequent growth path to debt alone would disregard competing explanations. Comparative WDI research has likewise situated macroeconomic indicators, including inflation, alongside broader developmental outcomes in ASEAN economies, including the Philippines (Quinto & Atento, 2025).

Philippine debt relationships have already received empirical attention. Borensztein (1990) examined foreign debt and private investment, while Akram (2015) estimated debt, output, and investment relationships over 1975–2010. Our contribution is to assess whether an extended, independently assembled annual series supports a stable debt association and a parameter change around the end of the Lost Decade, with explicit attention to measurement, dynamics, and diagnostic evidence.

The historical argument also requires a distinction between the timing of recovery and its cause. Fernandez and Remolona (2026) document the completion of the Philippine Brady restructuring in December 1992. We therefore use 1993 as the first full subsequent year in the principal regime comparison. However, this date also separates periods with different domestic institutions and international conditions. Evidence from a single national series can assess parameter stability around that transition; it cannot isolate the restructuring from accompanying reforms or establish the counterfactual growth path without it.

Our central research question is whether changes in external debt and actual debt-service payments are associated with real gross domestic product (GDP) per capita growth and real capital formation growth in the Philippines, and whether those associations are stable across historically defined periods. We examine annual observations for 1970–2024, distinguishing 1970–1982, 1983–1992, and 1993–2024 for descriptive analysis. Dynamic regressions assess contemporaneous and lagged relationships, while fixed-date tests and recursive wild-bootstrap checks examine the sensitivity of apparent regime changes. A separate assessment considers whether the data justify a long-run levels relationship.

The study connects the Bangko Sentral ng Pilipinas (BSP) chapter's historical interpretation to an auditable empirical exercise. The extended sample distinguishes debt stocks from payment flows and subjects apparent breaks to alternative specifications and finite-sample checks. Investment equations examine a proposed transmission channel, although aggregate capital formation cannot isolate private investment behavior. The interpretation separates conditional associations involving annual changes from claims about persistent overhang and causal restructuring effects.

2 Review of Related Literature

2.1 Debt overhang and the distinction between stocks and payments

Debt overhang theory concerns the incentives associated with inherited claims on future resources. Krugman (1988) analyzes the choice between financing an indebted country and forgiving part of its obligations when debt obstructs voluntary new lending. In this framework, the expected transfer of gains to existing creditors can weaken the incentive to expand the resources available for repayment. The theoretical object is therefore the expected burden of outstanding claims, rather than the amount repaid in a single year. Our analysis draws on this incentive mechanism without assuming that any high debt ratio necessarily constitutes an overhang.

Debt service represents a different margin. Actual payments may absorb resources that would otherwise finance consumption, investment, or public expenditure. Yet a reduction in observed payments may also reflect arrears, suspension, or rescheduling, while an increase may accompany improved repayment capacity. The World Bank defines total debt service as payments actually made, including principal and interest on long-term debt, interest on short-term debt, and specified International Monetary Fund (IMF) repayments (World Bank, n.d.-b). It does not measure every contractual payment falling due, unmet financing need, or perceived future liability. Consequently, a weak association involving actual service cannot establish that financing constraints were unimportant.

The same caution applies to changes in the debt-to-income ratio. This ratio varies with the debt numerator and the income denominator, and its annual change need not represent new borrowing. Valuation changes, repayment, restructuring, and income movements can all enter its evolution. A regression of annual growth on a change in this ratio examines a short-run statistical relationship. It does not directly estimate the investment disincentive arising from a persistent inherited stock. This distinction determines how we interpret both statistically significant and statistically insignificant coefficients.

2.2 Investment financing conditions and heterogeneous debt relationships

The evidence reviewed here does not support a single debt relationship that applies regardless of institutional or financial conditions. Turan and Yanikkaya (2021) examine total, public, and private external debt alongside growth and several investment measures in developing countries. They report negative debt associations with investment and a growth relationship that depends on government effectiveness and ethnic fractionalization. Their results do not support a general nonlinear or threshold relationship. These findings support attention to investment and debt composition, while cautioning against transporting a cross-country coefficient or threshold directly to the Philippines.

Chudik et al. (2017) likewise find no universally applicable public debt threshold after accounting for global factors and spillovers, although they report adverse long-run relationships involving debt accumulation. Their analysis concerns public debt in a heterogeneous international panel, so its estimates are not interchangeable with a Philippine total external debt equation. The implication for our design is methodological: a turning point obtained from a quadratic

term requires more than an individually significant coefficient. It must also rest on a defensible treatment of integration, specification, and parameter stability.

Kose et al. (2020) explain why borrowing benefits and costs vary with productive use, the economic cycle, and financial development. Their review identifies refinancing risk and the loss of room to respond to downturns as relevant costs alongside repayment expenditure. In our framework, inflation and peso depreciation describe observable macroeconomic conditions that may accompany debt distress. We include them as controls, while recognizing that they may also lie along debt-related transmission paths. The resulting debt coefficients are partial associations conditional on these variables, rather than estimates of the total economic consequences of indebtedness.

2.3 Rescheduling debt reduction and recovery

The design of debt relief is central to the distinction between liquidity relief and a durable resolution of debt distress. Reinhart and Trebesch (2016) compare historical relief episodes involving official and private external creditors. Their evidence associates stronger subsequent economic performance with debt write-offs; maturity extensions and interest reductions do not show the same general pattern. The study also discusses endogenous timing and competing explanations for recovery. Its comparative results motivate a distinction between forms of relief, but they do not establish that every rescheduling fails or that a particular national recovery is attributable to debt reduction.

Forni et al. (2016) distinguish restructurings that conclude a default episode from those followed by further negotiations. Their analysis links recovery to the resolution of distress and the scale of debt stock reduction. This distinction is relevant to repeated Philippine rescheduling: an agreement need not alter expected future obligations sufficiently to support investment. Our dataset lacks transaction-level terms and a complete creditor-loss series, so the regimes provide historical comparisons rather than estimates of returns to particular restructuring provisions.

Shenai and Bolhuis (2023) compare ten Brady countries with forty other emerging market and developing economies. They document improvements in debt burdens, output, and productivity alongside stronger structural reform efforts. Their comparative design provides evidence that extends beyond a simple before-and-after comparison. It also highlights the importance of complementary reforms. We use this study to motivate a reassessment of the Philippine transition, while acknowledging that a post-1993 indicator in a single-country regression cannot reproduce its comparative identification or separate debt relief from reform.

2.4 Philippine evidence and the scope of the contribution

Philippine investment evidence predates the Brady restructuring. Borensztein (1990) tests whether foreign debt discouraged private investment and reports support for the hypothesis, particularly after 1982. This earlier work rules out a claim that the investment channel has not been investigated in the Philippines. It also concerns a different empirical outcome from our real gross capital formation series, which includes public investment and changes in inventories. The two measures should not be treated as interchangeable.

Akram (2015) uses annual observations for 1975–2010 and an autoregressive distributed lag approach to examine real GDP and investment. The study includes public and publicly guaranteed external debt relative to GDP, debt service relative to exports, and domestic debt relative to GDP; investment is gross capital formation relative to GDP. It reports a negative long-run external debt relationship with real GDP and an insignificant debt-service relationship. Our use of GDP per capita growth, total external debt relative to gross national income (GNI), and real capital formation growth changes both the estimand and the measurement basis. The present exercise consequently extends and reassesses related evidence; it is not a strict replication of Akram's model.

Fernandez and Remolona (2026) provide the immediate institutional foundation. Their chapter interprets the Lost Decade through persistent obligations after repeated refinancing and the eventual transition to debt restructuring. It supplies the chronology and proposed mechanism, while independently retrieved official observations supply the empirical series. This separation permits an assessment of the historical interpretation without presuming that aggregate annual data identify its mechanism.

The research gap concerns the sensitivity of Philippine debt-related growth and investment associations, and apparent changes around the end of the Lost Decade, to an extended sample, alternative timing, and diagnostic scrutiny. The cited studies establish theory and prior national evidence; they do not determine the answer for our variables and 1970–2024 sample.

2.5 Analytical framework and empirical questions

Our analytical framework distinguishes an expected future burden associated with the debt stock from the current resource burden associated with actual payments. Both can be related to investment, with implications for subsequent

output, while exchange rate movements and inflation can reflect financing stress and other disturbances. Domestic policy changes and international shocks can alter these relationships. Because expectations, investment returns, and restructuring terms are not observed comprehensively, the framework organizes interpretation rather than constituting a fully identified structural model.

We operationalize three questions. First, do annual changes in the debt and service ratios have statistically distinguishable conditional associations with per capita output growth? Second, do lagged changes in those ratios have corresponding associations with real capital formation growth? Third, are the baseline coefficients stable around 1983 and the transition beginning in 1993? The null hypotheses concern zero individual debt coefficients and stable regression parameters. Debt overhang theory motivates adverse relationships under distress, but does not justify imposing a negative sign on every annual debt change. Failure to reject these nulls would therefore limit the short-run evidence without disproving the historical existence of an overhang.

3 Methodology

3.1 Research design and sample

We conduct an observational study of Philippine annual macroeconomic data. The dataset contains 55 years from 1970 through 2024. This period provides observations before the 1983 moratorium, during the Lost Decade, and after the December 1992 Brady restructuring. The principal growth regression uses 54 observations for 1971–2024 after constructing differences and a growth lag. Lagged-predictor and investment regressions use 53 observations for 1972–2024. Each estimate is reported with its actual sample rather than the nominal dataset length.

The design combines historical description, dynamic short-run regressions, and stability analysis. It neither assigns restructuring exogenously nor constructs an untreated Philippine counterfactual. Weak activity can raise debt ratios or prompt renegotiation, while recovery can support repayment. Lagged predictors and crisis indicators do not resolve these identification problems; the estimates are interpreted as conditional associations.

3.2 Data sources construction and operational definitions

We retrieved Philippine observations directly from the World Bank indicator application programming interface (API) on 5 October 2026 and retained the original responses, series identifiers, source update dates, and checksums (World Bank, 2026). The debt indicators originate in International Debt Statistics and are distributed through the World Development Indicators interface. The extraction includes 1969 for the exchange rate transformation. No missing observations are interpolated, and no values are copied from the BSP chapter's charts. Country, indicator, and year identifiers are checked for uniqueness and correspondence to the requested series.

Real GDP per capita growth, denoted g , is NY.GDP.PCAP.KD.ZG, measured in percent per year. The direct real GDP per capita series, NY.GDP.PCAP.KD, in constant 2015 US dollars, supplies the logarithmic outcome for the separate levels assessment. Total external debt relative to gross national income (GNI), D , is DT.DOD.DECT.GN.ZS; actual total debt service relative to GNI, S , is DT.TDS.DECT.GN.ZS. Total external debt includes public, publicly guaranteed, private nonguaranteed long-term obligations, International Monetary Fund (IMF) credit, and short-term debt (World Bank, n.d.-a). It is consequently broader than sovereign external debt and is not total public debt.

The principal investment outcome, k , is real gross capital formation growth, NE.GDI.TOTL.KD.ZG. Gross capital formation as a share of GDP, NE.GDI.TOTL.ZS, is used in additional investment and levels specifications. Its Philippine coverage in the retained snapshot begins in 1981, so the pre-crisis share average includes only 1981–1982. Consumer price inflation, π , is FP.CPI.TOTL.ZG. Peso depreciation, e , is calculated as 100 times the change in PA.NUS.FCRF relative to its previous-year value; the underlying rate is the annual average number of pesos per US dollar. Positive values denote depreciation. WDI-based ASEAN research has likewise included inflation alongside other macroeconomic indicators as covariates (R. G. Atento, 2026).

Debt and service first differences are expressed in percentage points, as are inflation changes where used. For a debt-composition sensitivity check, public and publicly guaranteed external debt in current US dollars, DT.DOD.DPPG.CD, is divided by current-US-dollar GNI, NY.GNP.MKTP.CD, and multiplied by 100. This constructed ratio omits domestic public debt. Short-term debt as a share of total external debt, DT.DOD.DSTC.ZS, provides descriptive information about composition; it does not measure a complete refinancing requirement.

We check growth reconstructed from direct real GDP per capita and debt ratios reconstructed from dollar values. We also compare recent observations with official BSP tables and a dated Philippine Statistics Authority (PSA) release. BSP debt stocks for 2017–2024 and debt-service burden and GNI for 2022–2024 provide numerical benchmarks; the PSA release of 22 April 2025 provides rounded national growth observations for 2024 (Bangko Sentral ng Pilipinas,

n.d.-a, n.d.-b; Philippine Statistics Authority, 2025). This comparison documents recent discrepancies rather than establishing complete historical harmonization. The retained World Bank vintage supplies the main estimates; explicitly hybrid endpoint substitutions assess measurement sensitivity.

3.3 Empirical models and interpretation

The principal short-run growth specification includes one growth lag, contemporaneous changes in the debt and service ratios, inflation in levels, depreciation, and three crisis indicators:

$$g_t = \alpha + \rho g_{t-1} + \beta \Delta D_t + \gamma \Delta S_t + \delta \pi_t + \eta e_t + \theta' C_t + u_t \quad (1)$$

Here, Δ denotes an annual first difference and C contains indicators for 1997–1998, 2008–2009, and 2020. The intercept is α and the disturbance is u . A one-percentage-point change in the debt ratio is associated with β percentage points of growth, conditional on the other regressors. This coefficient is not an effect of one percentage point of additional borrowing. We also estimate an equation with the debt changes, service changes, inflation, and depreciation lagged by one year, and a variant with two growth lags. Separating year-to-year variation from shared secular trends through first-differenced specifications has precedent in single-country Philippine annual research (R. G. Atento, Quinto, et al., 2026).

The complementary investment equation uses lagged debt and service changes to examine their association with subsequent real capital formation growth:

$$k_t = a + \phi k_{t-1} + b \Delta D_{t-1} + c \Delta S_{t-1} + d \Delta \pi_{t-1} + f e_{t-1} + \psi' C_t + v_t \quad (2)$$

This specification uses the lagged inflation change, whereas Equation 1 uses the inflation level. Both choices are disclosed and evaluated in the specification checks. The investment equation assesses compatibility with a proposed channel; it does not identify a causal indirect effect. Investment-share specifications are retained as sensitivity exercises because their shorter coverage and alternative interpretation affect comparability. Specifications were evaluated after examining the data and diagnostics, so the analysis is exploratory rather than preregistered.

3.4 Estimation stationarity and the long run assessment

We estimate the dynamic short-run equations by ordinary least squares (OLS) and use Bartlett-kernel heteroskedasticity and autocorrelation consistent (HAC) covariance estimates with two annual lags, following the approach of Newey and West (1987). The covariance calculation includes a sample-size correction of n divided by n minus the number of estimated coefficients. Individual inference uses a Student t reference distribution with residual degrees of freedom. These corrections improve the treatment of disturbance variation, but do not eliminate endogeneity or finite-sample uncertainty. Philippine annual WDI studies have likewise paired Newey–West standard errors with unit-root pretesting and first-differenced estimation of integrated series (R. G. Atento, Feng, et al., 2026).

We use packaged augmented Dickey–Fuller (ADF) tests with candidate augmentation lags from zero to three, selected by the Bayesian information criterion (BIC) on a common comparison sample, and MacKinnon probabilities. Phillips–Perron tests use a two-lag Newey–West bandwidth, with the package default as a sensitivity. Zivot–Andrews tests allow a single endogenous break, use 15% endpoint trimming, and select up to three lags by BIC. The statsmodels implementation uses one preliminary lag selection for all candidate break dates. Log real GDP per capita in levels includes a constant and trend; the other tests include a constant. These are unit-root screens, with limited power in short samples; they do not date a causal restructuring effect (ARCH Developers, n.d.; Statsmodels Developers, n.d.-b). The original Gaussian-simulation ADF screens remain in the reproducibility package for comparison.

For the long-run question, we estimate an autoregressive distributed lag (ARDL) specification with one lag of the outcome and one lag of each included predictor, expressed as an equivalent unrestricted error-correction model (UECM). The outcome is log real GDP per capita; the levels regressors comprise the debt ratio, investment share, inflation, and depreciation, with debt service added in alternative models. Investment coverage produces a principal estimation window of 1982–2024 with 43 observations. We also retain the shorter service specification ending in 2020 for comparison.

The joint test of lagged levels follows Pesaran et al. (2001), with an unrestricted intercept and no trend, designated case 3. We replicate the UECM equations in statsmodels 0.15.0 and report both packaged asymptotic bounds and finite-sample bounds simulated with 10,000 replications and a fixed seed. Table 7 uses the finite-sample bounds; the package retains both reference procedures and the original custom screens. The simulated and asymptotic bounds rely on a homoskedastic null distribution and do not validate the test under arbitrary heteroskedasticity or structural change (Statsmodels Developers, n.d.-a). A long-run interpretation also requires support for output adjustment, admissible integration orders, and specification stability. We do not derive long-run multipliers from a joint F statistic alone.

Bounds-testing ARDL analysis of Philippine annual data has been reported with a similar caution that evidence of a long-run relationship and an unusually large error-correction coefficient should be read carefully in a small sample (A. G. Atento, Teodosio, et al., 2026).

3.5 Diagnostics stability and robustness

We assess serial correlation with the packaged two-lag Breusch–Godfrey Lagrange multiplier (LM) test, using zeros for unavailable initial residual lags; heteroskedasticity with the studentized Breusch–Pagan test; functional form with Ramsey regression specification error test (RESET) using squared and cubed fitted values; residual normality with Jarque–Bera; and collinearity with variance inflation factors. The original serial-correlation implementation discards the first two observations, so its probabilities differ slightly. Both conventions are retained. An approximate OLS-residual cumulative sum (CUSUM) is descriptive. We also compute cumulative sum of squared recursive residuals (CUSUMSQ) for a pre-1997 core equation without future crisis indicators; its Gaussian simulation is exploratory for a dynamic equation. The full principal design first becomes full rank in 2020, making its four subsequent recursive predictions inadequate for a useful historical stability assessment. Diagnostic failures remain visible.

Fixed-date comparisons use 1983 and 1993 as historically motivated split points, with 1992 and 1994 as timing sensitivities. The joint tests permit the intercept and baseline macroeconomic coefficients to change. Later crisis coefficients are held common because their indicators occur only after the historical split, making unrestricted crisis interactions redundant. The early split has relatively few pre-crisis observations, which limits precision. A separate unknown-date scan is exploratory: its pointwise probabilities are not corrected for searching across dates and do not establish a confirmed break.

We supplement conventional fixed-date tests with 1,999 recursive wild-bootstrap replications using Rademacher signs applied individually and in three-year blocks. Centered residuals receive a global degrees-of-freedom correction. Simulated growth is generated recursively so that its lagged values respond to the simulated path, while macroeconomic predictors and initial conditions are held fixed. The fitted pandemic observation is also conditioned on because the singleton 2020 indicator has leverage one. Bootstrap exceedance probabilities include a one-count correction. The comparison tests a conditional parameter shift, not the causal effect of restructuring.

Robustness exercises alter predictor timing, growth lags, inflation transformation, the debt measure, the endpoint, and the inclusion of later crisis years. We examine exclusions of individual influential historical years while preserving the calendar definitions of retained lags. Leverage-adjusted heteroskedasticity-consistent (HC3) covariance comparisons are restricted to specifications without the singleton pandemic indicator, for which the full-model correction is undefined. Quadratic debt terms and regime interactions are exploratory and must satisfy the same specification requirements as the linear models. We assess economic magnitude and confidence intervals alongside statistical significance.

3.6 Reproducibility ethical considerations and limits

The analysis uses public aggregate indicators and contains no personal records or human-subject observations. The reproducibility package retains the data vintage, transformations, model definitions, numerical results, source snapshots, and simulation seeds. The original NumPy and SciPy estimates are independently replicated with statsmodels 0.15.0 and ARCH 8.0.0 for unit-root validation. Across 45 short-run specifications and 407 coefficients, maximum absolute differences in coefficients, HAC standard errors, and probabilities are below 5×10^{-14} . The three UECM equations also replicate to numerical precision. Simulation generates reference distributions rather than replacement empirical observations.

Numerical replication, additional unit-root and levels checks, and recent official-source comparisons support computational consistency and the retained transformations. Full historical harmonization and a quantitative bridge between debt and service coverage remain incomplete. The restricted recursive stability screen and unobserved expectations, creditor terms, and policy changes also limit inference. These qualifications are developed in Section 8.

4. Results

The results distinguish historical co-movement from conditional annual relationships. We report period comparisons, dynamic estimates, diagnostics, levels tests, and stability and sensitivity checks in that order.

4.1 Data coverage and historical patterns

The retained dataset contains 55 annual observations for 1970–2024. The core growth, debt, debt-service, inflation, and exchange rate series have no missing values within that period. Gross capital formation as a share of GDP begins in 1981, whereas real capital formation growth covers the longer period. No missing investment-share

observations are interpolated. Internal construction checks reproduce per capita growth from the direct real GDP per capita series and show a maximum discrepancy of approximately 0.009 percentage points between the reported debt ratio and the corresponding dollar-value reconstruction.

Recent reconciliation identifies small stock discrepancies and larger proportional payment differences. In 2024, BSP reports external debt of US\$137.628 billion, compared with US\$137.018 billion in the retained World Bank series. The retained GNI agrees with BSP at the published million-dollar precision. BSP debt-service burden calculated from its published numerator and denominator is 3.258% of GNI, compared with 3.451% for World Bank actual service. These are differing statistical series, not interchangeable observations; aggregate metadata do not quantitatively explain the gap. PSA's 2025 release reports 2024 per capita growth of 4.8% and capital formation growth of 7.7%, matching the retained values at one decimal (Bangko Sentral ng Pilipinas, n.d.-a, n.d.-b; Philippine Statistics Authority, 2025). The PSA 2026 annual-revision notice does not establish a complete latest-vintage comparison for these two series (Philippine Statistics Authority, 2026).

Table 1. Period averages of economic performance and debt burdens

Measure	1970–1982	1983–1992	1993–2024
Real GDP per capita growth (%)	2.76	−1.44	2.85
External debt (% of GNI)	36.30	69.25	38.77
Actual debt service (% of GNI)	4.76	7.78	5.38
Gross capital formation (% of GDP)	26.54	19.75	21.00
Consumer price inflation (%)	14.38	15.49	4.70
Peso depreciation (%)	6.88	12.56	2.90
Short-term debt (% of external debt)	32.84	21.66	14.76

Note. Unweighted annual averages. Period counts are 13, 10, and 32, respectively. The 1970–1982 investment-share mean uses only two observations, 1981–1982. Depreciation includes the 1970 change calculated from the 1969 exchange rate. Source: authors' calculations from the retained World Bank data.

Mean real GDP per capita growth declined from 2.76% before the crisis to −1.44% during the Lost Decade, before averaging 2.85% in 1993–2024. The corresponding external debt ratio rose from 36.30% to 69.25% of GNI, then averaged 38.77%. Actual debt service averaged 7.78% of GNI during the Lost Decade, compared with 4.76% before it and 5.38% afterward. These comparisons document coincident movements; the longer post-1993 period contains several distinct crises and policy environments.

The post-1993 investment share averaged 21.00% of GDP, modestly above the Lost Decade average of 19.75%. Its comparison with the earlier 26.54% average must remain qualified because that earlier value covers only 1981–1982. Figure 1 shows that neither debt burdens nor investment followed a uniform path after the Brady transition. Positive average growth therefore does not establish that earlier output losses were fully recovered relative to a counterfactual trend.

4.2 Preliminary stationarity assessment

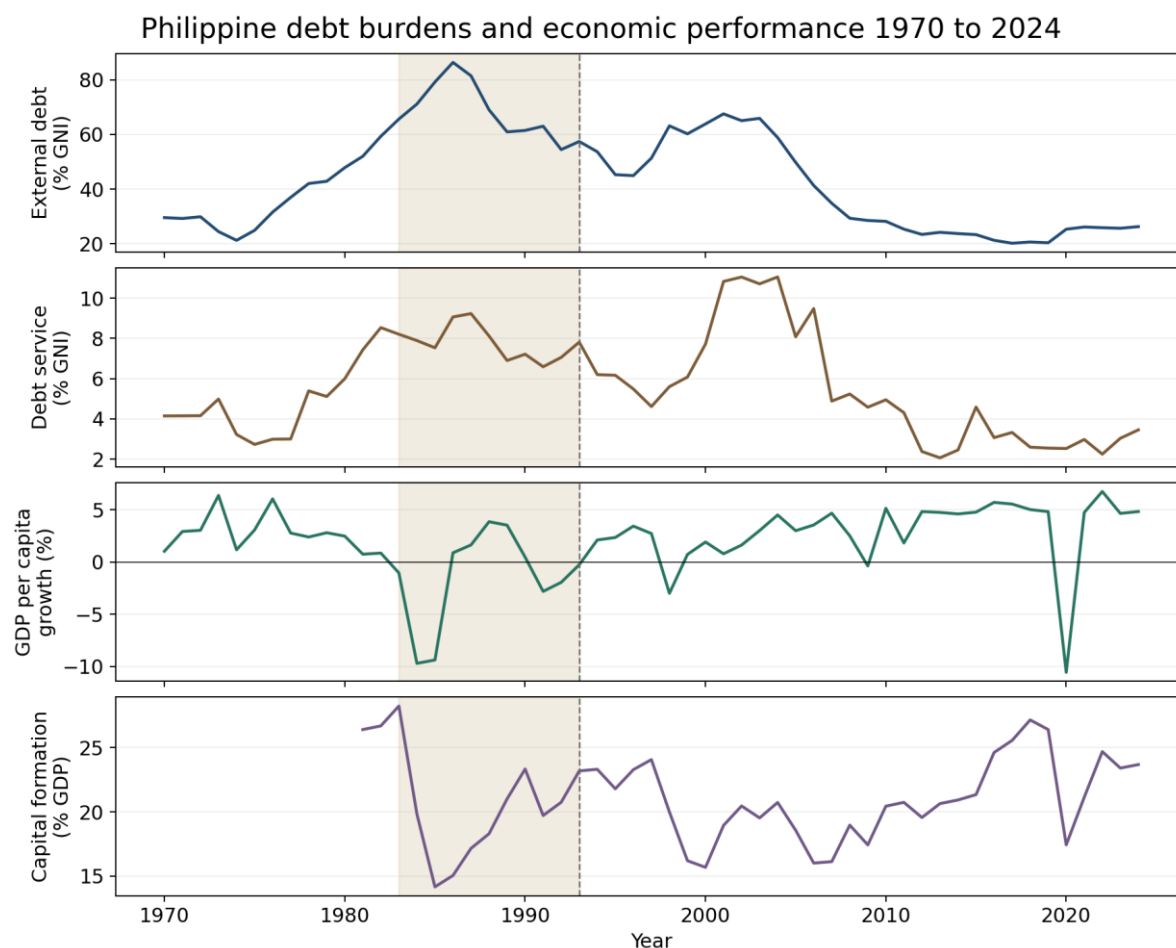
The packaged ADF tests reject a unit root for per capita growth, inflation, depreciation, and the investment measures in levels. Debt and service do not reject in levels but reject after differencing; log real GDP per capita follows the same pattern with a trend in its level test (Table 2). Phillips–Perron and Zivot–Andrews screens retain these 5% classifications, including the default-bandwidth Phillips–Perron sensitivity. For debt levels, the two-lag Phillips–Perron and Zivot–Andrews probabilities are .642 and .670; for service levels they are .343 and .256. All first-difference screens reject at 5%. These results support the retained transformations without proving integration orders or a unique historical break.

4.3 Short run associations with per capita growth

Table 3 reports the principal contemporaneous specification for 1971–2024. The debt-change coefficient is −0.013, with a 95% confidence interval from −0.165 to 0.139 and HAC $p = .867$. The service-change coefficient is −0.011, with an interval from −0.439 to 0.416 and $p = .958$. Neither individual null of a zero coefficient is rejected at the 5% level.

The intervals permit negative and positive associations, so these estimates should not be interpreted as demonstrating economically negligible effects.

Figure 1 Debt burdens and economic performance over time



Note. Shading identifies 1983–1992; the dashed line identifies 1993. Investment-share observations begin in 1981. The marked dates are historical reference points rather than estimated break dates. Source: authors' calculations from World Bank indicators.

Table 2 Packaged ADF statistics and MacKinnon probabilities

Series	Level statistic	Level p	Difference statistic	Difference p
Per capita growth	-4.837	< .001	-7.475	< .001
Debt ratio	-1.724	.419	-4.069	.001
Service ratio	-1.772	.394	-7.932	< .001
Investment share	-3.949	.002	-5.939	< .001
Inflation	-4.783	< .001	-7.350	< .001
Depreciation	-6.348	< .001	-10.155	< .001
Capital formation growth	-6.129	< .001	-8.118	< .001
Log real GDP per capita	-0.868	.959	-4.821	< .001

Note. statsmodels 0.15.0; BIC selects augmentation lags from 0–3 on a common comparison sample. Probabilities are MacKinnon approximations. Level log GDP per capita includes a constant and trend; other regressions include a constant. Statistics reproduce the original ADF calculations. The supplementary package retains the original custom probabilities and full Phillips–Perron, bandwidth sensitivity, and Zivot–Andrews results, including selected lags and candidate break years.

Table 3 Principal growth regression

Regressor	Coefficient	HAC SE	95% interval	HAC p
Intercept	3.642	0.584	[2.466, 4.817]	< .001
Previous-year per capita growth	0.364	0.137	[0.087, 0.640]	.011
Change in debt ratio	−0.013	0.076	[−0.165, 0.139]	.867
Change in service ratio	−0.011	0.212	[−0.439, 0.416]	.958
Inflation level	−0.157	0.044	[−0.245, −0.070]	< .001
Peso depreciation	−0.100	0.027	[−0.154, −0.045]	< .001
Asian crisis indicator 1997–1998	−1.058	0.895	[−2.860, 0.745]	.243
Global crisis indicator 2008–2009	−2.754	0.481	[−3.723, −1.784]	< .001
Pandemic indicator 2020	−15.926	0.504	[−16.942, −14.911]	< .001

Note. Dependent variable: real GDP per capita growth in percent. N = 54, 1971–2024; $R^2 = .749$. Debt and service changes are in percentage points. Bartlett HAC uses two annual lags, a degrees-of-freedom correction, and a Student t reference distribution. Model identity: R1_inflation_level. Crisis terms are controls rather than independently identified event effects; the 2020 indicator represents one observation.

Inflation and depreciation have negative contemporaneous coefficients of −0.157 and −0.100, respectively, with HAC $p < .001$ in both cases. Previous-year growth is positively associated with current growth. Since the controls may lie along debt-related transmission paths, the debt coefficients represent partial associations rather than total effects.

Lagging the macroeconomic predictors does not produce a statistically distinguishable debt association (Table 4). With one growth lag, the debt-change coefficient is 0.063 ($p = .390$), while service is −0.302 ($p = .159$). Adding a second growth lag gives corresponding estimates of 0.066 ($p = .376$) and −0.285 ($p = .173$). The change in signs across timing choices underscores the limited evidence for a stable short-run debt coefficient.

Table 4 Debt coefficients under alternative predictor timing

Growth specification	N	Debt coefficient	Debt p	Service coefficient	Service p
Contemporaneous predictors	54	−0.013	.867	−0.011	.958
Lagged predictors one growth lag	53	0.063	.390	−0.302	.159
Lagged predictors two growth lags	53	0.066	.376	−0.285	.173

Note. All equations include later crisis indicators. Inflation is in levels, contemporaneously or with a one-year lag as indicated. The principal sample is 1971–2024; the lagged samples are 1972–2024. Probabilities use two-lag HAC inference.

4.4 Associations with real capital formation growth

The principal investment regression uses 53 observations for 1972–2024 (Table 5). Lagged debt changes have a coefficient of −0.053 ($p = .911$), and lagged service changes have a coefficient of −0.244 ($p = .880$). Their respective 95% intervals, [−1.011, 0.904] and [−3.494, 3.006], are wide. The estimates do not provide statistically distinguishable short-run evidence for the proposed debt–investment relationship, but they also do not rule out substantial associations in either direction.

Table 5 Principal capital formation growth regression

Regressor	Coefficient	HAC SE	95% interval	HAC p
Intercept	9.821	2.038	[5.715, 13.928]	< .001
Previous-year capital formation growth	−0.005	0.127	[−0.261, 0.251]	.969
Previous-year change in debt ratio	−0.053	0.475	[−1.011, 0.904]	.911
Previous-year change in service ratio	−0.244	1.613	[−3.494, 3.006]	.880
Previous-year change in inflation	−0.270	0.184	[−0.640, 0.100]	.148
Previous-year peso depreciation	−0.592	0.270	[−1.136, −0.048]	.034
Asian crisis indicator 1997–1998	−7.603	5.535	[−18.758, 3.552]	.177
Global crisis indicator 2008–2009	−3.962	7.998	[−20.081, 12.156]	.623
Pandemic indicator 2020	−45.817	1.956	[−49.758, −41.876]	< .001

Note. Dependent variable: real gross capital formation growth in percent. $N = 53$, 1972–2024; $R^2 = .454$. Model identity: `I2_invest_growth_lagged`. Inflation enters as a lagged first difference. Capital formation includes public investment and inventories and is not private investment. Inference follows the two-lag HAC procedure; the pandemic coefficient is a control fitted to a singleton year.

Lagged depreciation is negatively associated with capital formation growth (−0.592; $p = .034$), consistent with an adverse financing environment. The aggregate outcome cannot distinguish private investment incentives from public investment or inventories, or identify a causal mediation effect.

4.5 Diagnostics and the limits of inference

Table 6. Diagnostics for the principal and timing specifications

Specification	BG p	BP p	RESET p	JB p	Max VIF
Principal growth	.269	.008	.119	.077	2.37
Lagged growth one lag	.218	.206	.728	< .001	2.05
Lagged growth two lags	.227	.266	.758	< .001	2.06
Capital formation growth	.708	.594	.084	.695	2.09

Note. Packaged diagnostics from `statsmodels 0.15.0`. BG: two-lag Breusch–Godfrey LM test, padding unavailable initial residual lags with zero. BP: studentized Breusch–Pagan heteroskedasticity test. RESET: squared and cubed fitted values, F reference. JB: Jarque–Bera test. VIF excludes the intercept. BG probabilities differ from the original convention that discards two initial observations; other displayed diagnostics reproduce to numerical precision. Probabilities below .001 are displayed as such. Non-rejection does not establish that an assumption holds.

The principal growth model rejects homoskedasticity (BP $p = .008$), supporting HAC inference. Packaged serial-correlation and RESET probabilities are .269 and .119, with maximum VIF 2.37. Lagged growth models reject residual normality (JB $p < .001$). The investment model does not reject at 5% on the displayed tests, although RESET $p = .084$ indicates sensitivity at 10%. The original approximate OLS-residual CUSUM has $p = .400$. A separate pre-1997 core recursive CUSUMSQ screen gives a Gaussian simulated $p = .719$, but this shorter dynamic equation is not validation of stability in the full model. Its future crisis indicators leave only four recursive predictions after full rank is achieved in 2020.

4.6 Assessment of a long run levels relationship

Table 7. ARDL levels screens and output adjustment

Specification	N	Joint F	5% lower / upper bound	Adjustment coefficient	Adjustment HAC p
Debt stock through 2024	43	4.995	3.168 / 4.464	−0.0042	.894
With service through 2020	39	3.409	2.943 / 4.324	−0.0410	.246
With service through 2024	43	4.023	2.945 / 4.298	−0.0019	.953

Note. Log real GDP per capita outcome; ARDL with one lag of the outcome and each predictor, expressed as a UECM; case 3. Packaged finite-sample bounds from statsmodels 0.15.0, 10,000 Gaussian-null simulations, seed 20261005. Windows: 1982–2024 (N = 43) and 1982–2020 (N = 39). Adjustment inference uses two-lag HAC. The asymptotic bounds and probabilities are retained separately. Both bounds procedures rely on homoskedastic reference distributions; packaged replication does not establish robust cointegration.

The stock specification yields $F = 4.995$, above the packaged finite-sample 5% upper bound of 4.464 (upper-bound $p = .029$). Output adjustment is nevertheless -0.0042 with HAC $p = .894$. Adding service through 2024 gives $F = 4.023$, between the finite-sample bounds of 2.945 and 4.298 (upper-bound $p = .070$), with adjustment -0.0019 ($p = .953$). This service model exceeds the asymptotic upper bound of 3.627 (upper-bound $p = .024$), so its inference changes with the reference distribution. The shorter service model remains between both sets of 5% bounds and has adjustment $p = .246$. The stock joint test supplies a levels signal, but weak adjustment and reference-distribution sensitivity preclude stable long-run multipliers. These screens do not prove the absence of cointegration.

4.7 Historical splits and finite sample stability checks

Table 8. Recursive wild bootstrap checks of fixed date parameter shifts

Growth specification	Post starts period	Observed F	One-year block p	Three-year block p
Current inflation level	1983	1.995	.286	.380
Current inflation level	1992	1.634	.425	.400
Current inflation level	1993	1.126	.568	.536
Current inflation level	1994	1.395	.466	.448
Current inflation change	1983	0.944	.681	.589
Current inflation change	1992	3.225	.125	.076
Current inflation change	1993	2.067	.254	.210
Current inflation change	1994	2.467	.177	.127
Lagged predictors two growth lags	1983	0.841	.536	.501
Lagged predictors two growth lags	1992	3.474	.109	.074
Lagged predictors two growth lags	1993	3.376	.120	.107
Lagged predictors two growth lags	1994	2.897	.149	.164

Note. Each probability uses 1,999 replications with a one-count correction. Tests allow the intercept and baseline macroeconomic coefficients to shift, holding later crisis coefficients common. Growth lags are generated recursively; macroeconomic paths, initial conditions, and the fitted singleton pandemic observation are conditioned on. The split year is included in the post period. The 1983 comparison has few pre-crisis observations.

For the principal 1993 split, the current inflation-level model gives bootstrap $p = .568$ with individual-year signs and .536 with three-year blocks. Corresponding values are .254 and .210 for the inflation-change model and .120 and

.107 for the lagged model with two growth lags. None rejects parameter stability at 5%. The 1992 split produces weaker signals in two alternatives under three-year blocks ($p = .076$ and $.074$), but these depend on the date, specification, and reference procedure. The 1983 and 1994 comparisons also fail to reject at 5% in the displayed bootstrap tests.

Conventional and HAC tests are not uniformly consistent with these bootstrap checks. For example, the inflation-change model at the 1983 split has a conventional $p = .475$ but HAC $p = .019$; its bootstrap probabilities are .681 and .589. The contrast prevents treating a favorable reference test as sufficient evidence of a historical break. The unknown-date scan is also exploratory because its pointwise probabilities do not account for searching across candidate years.

4.8 Sensitivity checks and unsuccessful alternative specifications

The debt-change coefficient remains statistically indistinguishable from zero when selected historical years are omitted, when later crisis years are excluded, and in the retained pre-pandemic specification. One- and three-lag HAC comparisons for the inflation-change model yield debt p -values of .855 and .849. These bandwidth checks apply to that alternative model rather than the principal inflation-level model. HC3 estimates in designs without the singleton pandemic indicator likewise do not establish a debt association; in the principal specification excluding 2020, debt $p = .899$ and service $p = .964$.

Hybrid source sensitivities replace 2017–2024 debt numerators with BSP stocks and, in a second variant, 2022–2024 service numerators with BSP debt-service burden, retaining the World Bank GNI denominator and recalculating changes and lags. With both substitutions, principal growth debt and service coefficients are -0.011 ($p = .886$) and -0.027 ($p = .901$); investment coefficients are -0.051 ($p = .916$) and -0.259 ($p = .871$). The individual 5% decisions are unchanged. These substitutions test recent measurement sensitivity; they do not create a harmonized historical dataset.

Other alternatives contain significant terms but diagnostic failures. The post-1993 interaction with lagged debt changes has $p = .002$ in the inflation-change model, whose RESET $p = .003$. With two growth lags and inflation levels, the interaction has $p = .032$, while RESET $p = .021$. The squared lagged debt term has $p = .006$ in a quadratic levels screen, but RESET $p < .001$ and integrated debt levels complicate interpretation. These estimates do not establish a robust slope change or a usable debt threshold.

The public and publicly guaranteed debt alternative has debt $p = .641$ and rejects RESET ($p = .022$). The investment-share change model rejects no serial correlation under the packaged BG convention ($p = .009$) and rejects RESET ($p = .005$). The original truncated-sample BG $p = .001$ gives the same 5% decision. The dynamic investment-share levels model has maximum VIF 53.35. These failures limit interpretation of the alternatives.

5. Discussion

5.1 Annual debt changes and persistent debt overhang

Debt burdens were higher and average per capita growth was negative during the Lost Decade, but annual debt and service changes lack statistically distinguishable coefficients in the principal equations. Historical period differences and conditional annual variation therefore provide distinct forms of evidence.

The results allow two interpretations: a weak conditional annual relationship, or inadequate measurement of the expected future burden central to debt overhang theory. Debt ratios incorporate income and valuation movements, while actual service can decline when payments are deferred or suspended. Inflation and depreciation controls may absorb variation along debt-related channels. The estimates do not distinguish these explanations, and their wide intervals rule out equating non-significance with economic irrelevance.

Akram (2015) reports insignificant short-run external debt and service terms alongside a negative long-run external debt relationship with real GDP. Our findings are compatible with the short-run result but do not establish the long-run relationship. Differences in sample, outcome, controls, debt coverage, and denominators prevent a direct replication claim: Akram uses public and publicly guaranteed external debt relative to GDP and service relative to exports, whereas our principal equation uses changes in total external debt and service relative to GNI and per capita growth.

5.2 Investment as a proposed transmission mechanism

The investment equation offers limited support for the proposed channel. Its broad coefficient intervals permit substantial associations, while aggregate capital formation growth does not isolate private investment incentives. Persistent effects on expected returns or private investment levels may not appear as stable annual-change coefficients. Philippine WDI time-series evidence on inward investment is similarly limited: lagged FDI intensity was not

statistically associated with manufacturing outcomes once macroeconomic dynamics and shocks were controlled (Refozar et al., 2026).

Turan and Yanikkaya (2021) report negative external-debt relationships with investment and heterogeneous growth relationships in developing countries. Their evidence supports investigating the channel, but does not require an equivalent Philippine aggregate coefficient. Our result qualifies its measurement and specification without establishing that the channel was absent during the crisis. Aggregate investment also depends on financing channels that these series do not separate; Philippine and ASEAN comparisons treat FDI and portfolio flows as analytically distinct, with portfolio investment showing high volatility and reversal risk (Chen et al., 2026).

5.3 The Brady transition and the interpretation of recovery

Improved growth and lower debt burdens after 1993 are compatible with Fernandez and Remolona's (2026) account of recovery from prolonged distress. The bootstrap results and unstable interaction evidence nevertheless do not establish a robust 1993 change in the estimated debt–growth relationship. Weaker 1992 signals remain sensitive to timing and specification.

Shenai and Bolhuis (2023) document improvements in debt, output, and productivity among Brady countries alongside stronger structural reforms. Their comparison economies support a counterfactual assessment that our single-country split cannot reproduce. The present findings do not separate restructuring from reform or establish equivalence between rescheduling and debt reduction. Related thematic review work links institutional quality and the business environment to investment attraction in ASEAN+3 economies (R. G. Atento, Espelita, et al., 2026), which is relevant to the concurrent institutional changes that this design cannot separate.

Failure to reject a break does not establish institutional continuity. Low power, overlapping shocks, and an unsuitable functional form can weaken a stability test. Historical chronology guides the choice of split dates but cannot replace identification. Regional growth research likewise uses a post-crisis estimation window to reduce sensitivity to late-1990s shock dynamics (Balingit et al., 2026).

5.4 Macroeconomic conditions and the strength of the evidence

The negative inflation and depreciation associations indicate that debt ratios alone provide an incomplete account of macroeconomic performance. They do not establish the effect of stabilization policies, because policy responses, debt distress, and activity may be jointly determined. Philippine evidence on inflation and per capita growth likewise indicates a negative association in each regime examined, while cautioning that inflation regimes are country-specific and context-dependent (Chen & Atento, 2026).

Numerical replication confirms the calculations; it does not remove the diagnostic constraints. Heteroskedasticity, lagged-model non-normality, specification failures in significant alternatives, and weak output adjustment limit inference. Recent source substitutions preserve the principal decisions, but coverage differences and the full-model recursive rank problem remain unresolved.

6. Conclusion

Philippine annual data for 1970–2024 show higher debt burdens and negative average per capita growth during 1983–1992, followed by lower burdens and improved average growth from 1993. This pattern is consistent with a transition out of prolonged distress, but does not identify the contribution of debt relief relative to other economic changes.

The principal dynamic regressions provide limited evidence for a stable short-run debt relationship. Annual changes in total external debt and actual service relative to GNI have individually insignificant coefficients in the growth and capital formation equations. Alternative timing and selected exclusions do not establish consistent debt associations. Broad confidence intervals nevertheless permit economically meaningful relationships.

The recursive wild-bootstrap tests do not establish a robust 1993 parameter shift at 5%; weaker 1992 signals depend on specification and block length. Significant interaction and quadratic alternatives fail specification checks. A stock-model joint levels signal, accompanied by weak output adjustment, does not support a reliable long-run multiplier. The findings therefore establish neither a post-Brady slope change nor a borrowing threshold.

The study qualifies the empirical implications of Fernandez and Remolona's (2026) historical account. Annual changes and realized payments capture a narrower mechanism than persistent expected obligations. Weak conditional relationships and inadequate proxies remain plausible explanations. The extended, reproducible series clarifies this

distinction, while incomplete historical harmonization and constrained recursive inference prevent stronger causal or long-run claims.

7. Policy Implications

The policy implications follow from measurement and diagnostic findings. They guide assessment of debt distress and recovery; the study does not estimate the effects of particular interventions.

7.1 Distinguishing payments from outstanding obligations

Debt assessment should distinguish outstanding stocks, actual payments, and obligations falling due. A lower realized-payment ratio need not indicate a smaller future burden. Comparable contractual schedules, arrears, and financing terms should therefore accompany observed debt ratios where records permit. This recommendation addresses measurement limits; the regressions do not rank indicators by their usefulness for predicting growth or distress.

7.2 Evaluating relief through sustained economic outcomes

Debt-relief evaluation should examine investment and output paths over an explicit horizon together with repayment obligations. Improved post-1993 averages do not establish full recovery from earlier output losses or isolate the Brady restructuring. Fernandez and Remolona (2026) motivate the distinction between payment relief and resolving persistent overhang; Shenai and Bolhuis (2023) emphasize the accompanying reforms. A Philippine evaluation therefore requires a defensible comparison path and attention to concurrent changes. Philippine crisis-response research similarly distinguishes consumption-side relief from production-side stabilization and considers the conditions under which each instrument is appropriate (A. G. Atento, Quinto, et al., 2026). Comparative analysis of Philippine investment attraction adds a focus on how the country positions itself for capital relative to ASEAN peers, beyond the macroeconomic fundamentals that dominate determinants research (R. G. Atento & Espelita, 2026).

7.3 Considering macroeconomic conditions jointly

Debt surveillance should consider exchange rate exposure and domestic price conditions alongside debt and investment indicators. Their negative conditional associations in the principal equations warrant joint assessment, but do not identify independent benefits from stabilization policies. Policy responses may be endogenous, and these controls may absorb part of a debt-related transmission channel.

7.4 Avoiding unsupported borrowing thresholds

Insignificant principal debt coefficients do not justify unrestricted borrowing, while the significant quadratic alternative does not justify a numerical ceiling. The former remain imprecise; the latter fails specification checks and uses debt levels with unresolved integration concerns. Decisions about debt limits or restructuring instruments require additional evidence and transparent reporting of unsuccessful alternatives.

8. Limitations and Directions for Future Research

8.1 Data coverage and measurement

The 55-year dataset yields 54 observations in the principal growth equation and 53 in the lagged growth and investment equations. Investment-share coverage begins in 1981, leaving only two pre-1983 observations. Annual frequency combines developments within each year, limiting the study of anticipation, immediate adjustment, and delayed responses.

Total external debt includes private liabilities, and the public and publicly guaranteed alternative omits domestic public borrowing. GNI ratios incorporate denominator and valuation movements. Actual service does not measure contractual obligations, arrears, or expected transfers; capital formation includes public investment and inventories. These measures provide a partial test of the proposed debt-overhang mechanism.

The retained World Bank vintage supplies the principal observations without interpolation. Source comparisons cover BSP stocks for 2017–2024, service burden and GNI for 2022–2024, and two rounded PSA growth observations for 2024. Hybrid substitutions preserve the principal coefficient decisions, but do not harmonize the full history. A quantitative coverage and revision bridge is still required.

8.2 Identification and omitted influences

The observational design does not identify exogenous debt changes. Growth can alter debt denominators, borrowing, and repayment capacity, while debt distress, inflation, depreciation, and policy responses may develop jointly. Controls cannot resolve this simultaneity and may remove debt-related transmission variation; the coefficients should not be interpreted as total effects.

Historical splits cannot separate restructuring from domestic reforms, political changes, and international conditions. Creditor composition, financing terms, institutions, and expectations are also incompletely observed. Parameter stability or instability alone cannot identify the contribution of debt relief.

8.3 Statistical inference and validation

Small samples constrain precision and the power of early-period break tests. HAC inference addresses disturbance heteroskedasticity and dependence, not omitted-variable bias or functional-form error. Principal-model heteroskedasticity, lagged-model non-normality, RESET failures in significant alternatives, and collinearity in an investment-share levels model limit the weight of individual significant terms.

MacKinnon ADF probabilities replace the original Gaussian screening probabilities, which remain archived. Phillips–Perron and Zivot–Andrews tests agree at 5%, subject to small-sample power and single-break assumptions. ARDL bounds use homoskedastic reference distributions; asymptotic and finite-sample inference differ for service through 2024. Weak output adjustment prevents a confident long-run interpretation. Bootstrap stability tests condition on macroeconomic paths and the fitted pandemic observation and depend on block length and date. The unknown-date scan remains pointwise and search-unadjusted.

Replication covers 45 short-run models and three UECM equations. Recursive CUSUMSQ is confined to an exploratory pre-1997 core equation because the full design first attains initial full rank in 2020, leaving four predictions. The singleton pandemic indicator also makes full-sample HC3 undefined, so its comparisons exclude that indicator. Computational agreement does not resolve these design limitations.

8.4 Priorities for further research and external validity

Further work should prioritize historical source harmonization, explicit coverage bridges, and stability procedures suited to the crisis design. Material measurement or specification differences would require reassessment. If evidence remains weak, contemporaneous historical records may be more informative than adding complexity to regressions on the same limited sample.

Research could separate private fixed investment from public investment and inventories, and reconstruct contractual service, arrears, and restructuring terms. Comparative designs should select economies for substantive comparability and adequate pre-transition fit, account for common shocks and reforms, and test sensitivity to the comparison group. These extensions address measurement and identification gaps rather than completed findings.

The results apply to the Philippine historical sample and retained definitions. Changing institutions and financing environments limit transfer to other countries, current borrowing arrangements, or different debt instruments. Comparability of mechanisms and measures should be established before pooling periods or generalizing coefficients.

Data Availability Statement

The source indicators are publicly accessible through the World Bank API and the official BSP and PSA links cited here. A supplementary data and code package retains the retrieved source responses, processed dataset, construction rules, analysis scripts, validation results, and simulation settings needed to reproduce the reported estimates. It distinguishes the retained World Bank vintage from the hybrid official-source sensitivity exercises.

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