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Socioeconomic and Health-System Indicators and Infant Mortality in Latin America and the Caribbean, 2000-2023: A Fixed-Effects Panel Study

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Abstract

Infant mortality remains unequally distributed across Latin America and the Caribbean. We examined whether within-country changes in poverty, income inequality, education, government health expenditure, and universal health coverage (UHC) were associated with infant mortality. We created an auditable panel from a frozen World Bank API snapshot for 2000–2023. From 7,056 indicator-country-year records, we constructed 1,008 country-years; the primary complete-case sample included 290 country-years from 17 countries with at least two informative observations. We estimated log-linear country and year fixed-effects models using country-clustered CR1 intervals, a t(16) reference distribution, restricted Webb-weight wild cluster bootstrap-t inference, and cluster-jackknife checks. No predictor showed robust evidence of association in the primary model. A one-point UHC increase corresponded to 1.51% lower infant mortality (95% CI, -3.51% to 0.54%; bootstrap p = 0.316), and a one-percentage-point increase in government health expenditure corresponded to 2.80% lower infant mortality (95% CI, -7.18% to 1.79%; bootstrap p = 0.219). First-difference models produced stronger inverse estimates, but these post hoc results were unstable across specifications. Ecological measurement, missingness, unbalanced coverage, and few countries constrain inference. The findings do not establish causal or policy effects.

1. Introduction

Infant mortality is a widely used sentinel of population health because deaths during the first year of life reflect a combination of maternal and newborn care, nutrition, infectious disease control, living conditions, and broader social protection. Regional declines have been substantial, yet national averages continue to conceal marked inequalities across and within countries [1].

The social-determinants framework distinguishes structural conditions, such as income distribution and poverty, from intermediate conditions, including education, access to care, and the organization and financing of health systems [2]. Universal health coverage is multidimensional: service coverage and financial protection should be monitored jointly. The service-coverage index used here is a unitless 0-100 composite calculated as the geometric mean of 14 tracer indicators; it is not a percentage of health needs met [3, 4].

Relevant regional evidence is heterogeneous. A 2000–2019 panel analysis identified health expenditure, female literacy, education expenditure, and other factors as correlates of infant mortality [5]. Municipal-level machine-learning analyses in Brazil, Ecuador, and Mexico ranked poverty, illiteracy, and the Gini index as important predictors of under-five mortality, while emphasizing non-linearity rather than causal effects [6]. A panel study of 35 countries in the Americas reported a small and imprecise positive association between the Gini index and neonatal mortality [7]. Stronger causal claims have generally come from subnational evaluations of defined policies, such as conditional cash-transfer programmes, rather than from national ecological panels [8, 9]. Regional health-system reform scholarship has likewise emphasized that equity in child health depends on governance, access, and implementation, not spending aggregates alone [10].

The genuine contribution of this study is therefore not the first use of panel data in the region. It is the combination of an updated 2000–2023 multi-domain panel, a frozen and hash-audited extraction pipeline, explicit accounting for country-specific data contribution, and inference designed for a small number of unequal country clusters. These features address reproducibility and inferential weaknesses in the supplied draft while preserving the ecological question.

The primary objective was to estimate the association between within-country changes in extreme poverty, income inequality, government health expenditure, UHC service coverage, and lower-secondary educational completion and contemporaneous changes in infant mortality. Directional expectations were positive for poverty and inequality and negative for health expenditure, UHC, and education; these hypotheses were not preregistered. The estimand is associational, not causal.

2. Materials and Methods

2.1. Study Design and Setting

We conducted an ecological longitudinal study with the country-year as the unit of analysis. The source frame contained 42 countries and territories classified in the supplied regional file as Latin America and the Caribbean. The observation window was 2000–2023. The analysis used a data snapshot downloaded on 14 July 2026 and did not refresh values after the freeze.

2.2. Data Source and Auditable Pipeline

Seven indicators were retrieved from the public World Bank API and stored as raw JSON files. The pipeline used RAW, SILVER, and GOLD layers, retained API and source manifests, recorded data-quality checks, and stored SHA-256 hashes. In the independent audit, all seven raw-file hashes matched the supplied manifest and no duplicate country-year-indicator keys were detected [11].

2.3. Outcome, Predictors, and Measurement

Role	Construct	World Bank code	Operational definition	Model contrast
Primary outcome	Infant mortality rate	SP.DYN.IMRT.IN	Deaths before age 1 per 1,000 live births	Log-transformed in the primary model
Predictor	Extreme poverty	SI.POV.DDAY	Population below USD 3.00/day, 2021 PPP (%)	One percentage-point contrast [12]
Predictor	Income inequality	SI.POV.GINI	Gini index, 0-100	One-point contrast
Predictor	Government health expenditure	SH.XPD.GHED.GD.ZS	Domestic general government health expenditure (% GDP)	One percentage-point-of-GDP contrast
Predictor	UHC service coverage	SH_UHC_SCI	Composite service-coverage index, 0-100	One-point contrast [3]
Predictor	Lower-secondary completion, age 25+	SE.SEC.CUAT.LO.ZS	Population aged 25 + with at least lower-secondary completion (%)	One percentage-point contrast

Life expectancy was extracted for descriptive pipeline purposes but was not included in the primary model.

2.4. Data Quality, Missingness, and Analytic Sample

The wide panel contained 1,008 potential country-years. Complete-case analysis required non-missing infant mortality and all five predictors. Among 294 complete country-years from 21 entities, four entities contributed only one complete year and therefore could not identify a within-country slope. The primary sample retained every country capable of contributing within-country variation, defined as at least two complete country-years, yielding 290 observations from 17 countries.

The supplied draft used average indicator coverage of at least 80% as the main country-selection rule. That rule was not retained because an average can mask near-total absence of a single predictor: Argentina passed the rule but contributed only two complete years because the education indicator was largely missing. A stable-history sensitivity analysis required at least ten complete years. No values were imputed because missingness was extensive, variable-specific, and strongly structured by survey and reporting schedules; the consequences of complete-case selection were evaluated through alternative sample definitions. Because this was a fixed secondary dataset, no prospective power calculation was performed. Precision was constrained primarily by the 17 country clusters and their unequal contributions; the analysis therefore emphasizes confidence intervals and few-cluster inference rather than a nominal power target.

2.5. Conceptual Model and Target Estimand

Poverty and inequality were treated as structural social conditions; education as a human-capital and social-resource indicator; and government health expenditure and UHC service coverage as health-system indicators. These variables can share causes, mediate one another, and respond to health conditions. Country fixed effects remove time-invariant national characteristics, and year fixed effects remove shocks common to all countries, but neither addresses unmeasured time-varying confounding, reverse causation, or measurement processes. The target quantity is the average conditional within-country semi-elastic association between a one-unit change in a predictor and infant mortality among observed country-years.

2.6. Primary Statistical Model

The primary model regressed the natural logarithm of infant mortality on all five predictors, country fixed effects, and year fixed effects. The full five-predictor model was selected because it corresponded to the scientific question, not because any coefficient crossed a statistical-significance threshold. Separate manual within-country centering was unnecessary: country fixed effects already identify coefficients from within-country variation. Interaction terms were not included in the primary model because the supplied interactions lacked a prespecified mechanism and the number of countries was inadequate for stable moderation analysis.

For country i and year t, the model was: ln(IMR_it) = alpha_i + lambda_t + beta_1 Poverty_it + beta_2 Gini_it + beta_3 HealthExpenditure_it + beta_4 UHC_it + beta_5 Education_it + epsilon_it. Exponentiated coefficients were expressed as percentage changes in infant mortality per one-unit predictor increase.

2.7. Inference, Diagnostics, and Influence Analysis

Because serial correlation was expected within countries and only 17 country clusters were available, uncertainty was not based on default large-sample normal approximations. We used CR1 country-clustered covariance, a t reference distribution with G-1 = 16 degrees of freedom, restricted wild cluster bootstrap-t tests with Webb six-point weights and 19,999 draws, and a cluster-jackknife check [13, 14]. Bootstrap seeds were recorded. An independent implementation reproduced the bootstrap results within Monte Carlo error.

Multicollinearity was assessed after residualizing predictors with respect to country and year fixed effects. Leave-one-country-out estimates assessed influence. Residual serial dependence was summarized from consecutive-year residual pairs. The two-way within R-squared was calculated after partialling out both fixed-effect sets.

2.8. Sensitivity Analyses

Post hoc sensitivity analyses examined: (1) at least ten complete years per country; (2) the original average-coverage rule; (3) exclusion of education to expand the sample; (4) restriction to 2000–2019; (5) one-calendar-year lags for all five predictors; (6) first differences for consecutive years; (7) infant mortality in levels; (8) country-specific linear trends; and (9) leave-one-country-out estimates. No

multiplicity-adjusted confirmatory claims were made; patterns were interpreted according to magnitude, uncertainty, and stability.

2.9. Software and Reproducibility

Analyses were performed in Python 3.13.5 using pandas 2.2.3, NumPy 2.3.5, statsmodels 0.14.6, SciPy 1.17.0, and patsy 1.0.1. The restricted wild cluster bootstrap used a documented custom implementation and was cross-checked against an independent implementation. The complete frozen executable pipeline, pinned environment, raw and processed data, reference outputs, and integrity checks are supplied as Supplementary File S1. The script asserts the seven verified indicator codes, record counts, duplicate checks, source-file hashes, and primary sample dimensions before analysis. Canonical 19,999-draw bootstrap p-values are retained in the audited reference outputs; because the exact original random-weight matrix was not supplied, a deterministic independent rerun is also provided and agrees within expected Monte Carlo error. OpenAI ChatGPT (GPT-5.6 Pro; accessed 20–21 July 2026) was used under human direction for independent scientific audit, statistical reconstruction, reference verification, figure specification, English-language editing, preparation of the reproducibility package, and Research Square formatting. Numerical outputs were generated from the supplied data with executable Python code and checked against independent calculations. The model was not an author and did not make autonomous scientific or submission decisions.

2.10. Ethics, Data Governance, and Reporting

The study used only publicly available, aggregate country-year indicators. It did not involve human participants, identifiable personal information, biological specimens, clinical interventions, or contact with individuals. On that basis, formal ethics review and informed consent were considered not applicable. This determination must be confirmed by the responsible institution before public posting. No individual-level or sensitive data were collected or accessed. Reporting was prepared with reference to STROBE [15], and a completed coverage matrix is provided as Supplementary File S3.

3. Results

3.1. Data Flow, Missingness, and Country Contribution

The long-format dataset contained 7,056 indicator-country-year records, corresponding to 42 entities, 24 years, and seven indicators. The wide structure contained 1,008 potential country-years. Overall, 2,560 indicator values were missing (36.28%). Missingness was greatest for poverty and Gini (65.97% each) and lower-secondary completion (60.71%); it was 23.21% for government health expenditure, 21.43% for UHC, and 16.67% for infant mortality.

The primary sample contained 290 complete country-years from 17 countries. Contributions ranged from two years for Argentina and four years each for Guatemala and Nicaragua to 24 years for the Dominican Republic and Peru. Between 8 and 15 countries contributed observations in any calendar year. Figure 2 makes the imbalance explicit.

Stage	Records	Countries/entities	Interpretation
Indicator-country-year records in long format	7056	42	42 entities × 24 years × 7 indicators
Potential country-year rows in wide format	1008	42	42 entities × 24 years
Complete country-years for outcome and five predictors	294	21	Includes four singleton countries that cannot identify a within-country slope
Primary fixed-effects sample	290	17	Countries with at least two complete country-years
Stable-history sensitivity sample	280	14	Countries with at least ten complete country-years
Original average-coverage selection	282	15	Original ≥ 80% mean indicator-coverage rule

Indicator-country-year records must not be described as regression observations.

3.2. Descriptive Characteristics

Variable	n	Mean	SD	Median	Range
Infant mortality (deaths per 1,000 live births)	290	17.96	8.51	16.10	5.80 to 57.20
Extreme poverty at USD 3.00/day (percentage point)	290	9.10	6.89	7.30	0.10 to 33.30
Gini index (one point)	290	48.74	4.96	48.90	37.00 to 61.60
Government health expenditure (percentage point of GDP)	290	3.76	1.23	3.84	1.15 to 6.65
UHC service coverage index (one point)	290	72.27	8.73	73.00	45.00 to 85.00
Lower-secondary completion among adults aged 25+ (percentage point)	290	49.54	12.12	50.12	14.75 to 84.60

All descriptive statistics refer to the 290 country-years used in the primary model; they are not population-weighted regional estimates.

Mean infant mortality was 17.96 deaths per 1,000 live births (SD 8.51). Mean UHC service coverage was 72.27 index points (SD 8.73), and mean government health expenditure was 3.76% of GDP (SD 1.23). Within-country standard deviations were smaller than overall standard deviations for every variable, as expected in a fixed-effects analysis.

3.3. Primary Two-Way Fixed-Effects Model

The two-way within R-squared was 0.310. No predictor provided robust evidence of association after few-cluster inference. The UHC coefficient was − 0.0152, corresponding to 1.51% lower infant mortality per one-point increase (95% CI, -3.51% to 0.54%; CR1 p = 0.137; wild cluster bootstrap p = 0.316; jackknife p = 0.286). The government health-expenditure coefficient was − 0.0284, corresponding to 2.80% lower infant mortality per one-percentage-point-of-GDP increase (95% CI, -7.18% to 1.79%; CR1 p = 0.211; bootstrap p = 0.219; jackknife p = 0.281).

Extreme poverty had a positive estimate of 1.34% per percentage point (95% CI, -0.75% to 3.47%; bootstrap p = 0.314). The Gini and education estimates were near zero and highly imprecise. Fixed-effect-residualized VIFs ranged from 1.05 to 1.54, providing no evidence that multicollinearity explained the imprecision. The pooled lag-1 residual correlation among 233 consecutive-year pairs was 0.884, supporting country-level clustering.

Predictor contrast	Beta	% change	95% CI	CR1 p	WCB p	Jackknife p
Extreme poverty at USD 3.00/day (percentage point)	0.0133	1.34%	-0.75% to 3.47%	0.196	0.314	0.293
Gini index (one point)	0.0044	0.44%	-2.34% to 3.30%	0.744	0.830	0.797
Government health expenditure (percentage point of GDP)	-0.0284	-2.80%	-7.18% to 1.79%	0.211	0.219	0.281
UHC service coverage index (one point)	-0.0152	-1.51%	-3.51% to 0.54%	0.137	0.316	0.286
Lower-secondary completion among adults aged 25+ (percentage point)	0.0008	0.08%	-0.75% to 0.92%	0.842	0.855	0.872

CR1 confidence intervals use t(16). WCB = restricted Webb-weight wild cluster bootstrap-t, 19,999 draws. Percentage changes equal 100 × [exp(beta) − 1].

3.4. Sensitivity and Influence Analyses

The stable-history sample and the original average-coverage sample produced UHC and expenditure estimates similar to the primary model, but wild cluster bootstrap p-values remained at least 0.22. Excluding education increased the sample to 332 country-years from 19 countries; the expenditure estimate became more precise (bootstrap p = 0.079) but remained insufficient for a confirmatory claim. Restriction to 2000–2019 reversed the expenditure estimate to a small positive value, while the UHC estimate remained negative but imprecise.

A corrected lag model included all five predictors at a one-calendar-year lag. Neither lagged health expenditure (bootstrap p = 0.148) nor lagged UHC (bootstrap p = 0.232) was robust. Thus, the supplied draft's claim that lagged expenditure was statistically significant at p = 0.0446 was not reproduced under the full model and few-cluster inference.

First-difference models for 233 consecutive country-year changes yielded inverse estimates for annual changes in health expenditure (-0.84%; 95% CI, -1.59% to -0.07%; bootstrap p = 0.022) and UHC (-0.25%; 95% CI, -0.49% to -0.01%; bootstrap p = 0.016). These analyses changed the estimand, used only 13 countries, and can amplify measurement error; they were post hoc and were not used to override the primary result. A country-specific-trend model also produced a negative UHC estimate, but its high-dimensional specification was treated as exploratory.

Leave-one-country-out estimates were always positive for poverty and always negative for health expenditure and UHC, but uncertainty remained broad. The Gini coefficient changed sign and became apparently precise after omission of the Dominican Republic, demonstrating influence rather than a robust inequality effect.

Specification	Health expenditure: % (95% CI)	WCB p	UHC: % (95% CI)	WCB p
Primary: all contributing countries (> = 2 complete years)	-2.80% (-7.18, 1.79)	0.219	-1.51% (-3.51, 0.54)	0.316
Stable-history sensitivity (> = 10 complete years)	-3.06% (-8.02, 2.16)	0.231	-1.82% (-3.85, 0.25)	0.250
Original average-coverage > = 80% sample	-3.11% (-7.97, 2.02)	0.222	-1.82% (-3.83, 0.23)	0.262
Four-predictor model excluding education	-3.27% (-6.92, 0.52)	0.079	-1.33% (-3.11, 0.47)	0.316
Pre-pandemic period (2000–2019)	0.94% (-4.15, 6.31)	0.692	-1.74% (-3.63, 0.19)	0.151
One-calendar-year lag model	-2.94% (-6.90, 1.20)	0.148	-1.61% (-3.48, 0.29)	0.232
First differences for consecutive years	-0.84% (-1.59, -0.07)	0.022	-0.25% (-0.49, -0.01)	0.016
Country-specific linear trends	-0.44% (-3.18, 2.37)	—	-0.65% (-1.21, -0.09)	—

The first-difference and country-trend specifications estimate different temporal contrasts and are exploratory. A missing WCB p-value indicates that a stable bootstrap was not reported for the high-dimensional trend model.

4. Discussion

In this reconstructed analysis, within-country changes in the five socioeconomic and health-system indicators did not show robust associations with infant mortality in the primary two-way fixed-effects model. UHC service coverage and government health expenditure were consistently negative in several specifications, but their primary intervals included both modest benefit and little or no association, and few-cluster bootstrap tests did not support conventional threshold-based claims. The direct answer to the research question is therefore one of uncertainty, not confirmed protective effects.

These findings differ in emphasis from the earlier regional panel study that reported several socioeconomic and health-system determinants [5]. The studies used different variables, samples, dynamic assumptions, and inferential procedures. The present analysis also differs from municipal machine-learning work, which addresses predictive importance and non-linearity rather than a fixed-effects estimand [6], and from the Americas-wide neonatal mortality analysis, which reported a modest Gini association with an interval near the null [7].

Evidence from defined social policies remains relevant but should not be conflated with the current ecological coefficients. Conditional cash-transfer evaluations in Brazil, Ecuador, Mexico, and broader Latin American settings have used subnational exposure contrasts and richer policy timing, producing more credible intervention-specific evidence [8, 9, 16, 17]. The present national panel cannot identify the effects of particular reforms, expenditure composition, service quality, or who received additional resources.

The negative UHC direction is substantively plausible because the index includes maternal, newborn, child-health, infectious-disease, noncommunicable-disease, and service-capacity tracers [3]. However, the index co-evolves with income, state capacity, demographic change, and measurement systems. Likewise, expenditure as a share of GDP does not capture purchasing power, efficiency, subnational allocation, private spending, or the lag from budget decisions to service delivery. These limitations can attenuate, confound, or even reverse ecological associations.

The first-difference findings deserve targeted replication, not elevation to a main conclusion. Differencing can remove slow-moving omitted factors but also amplifies year-to-year measurement error, depends on consecutive observations, and changes the target contrast from level changes over the panel to annual co-movement. The sign reversal for expenditure in the pre-pandemic sample further argues against a single stable effect estimate.

4.1. Public Health and Equity Implications

The results do not support using the estimated coefficients as policy-effect sizes or as evidence that a one-point UHC increase will cause a specific reduction in infant mortality. Their most defensible implication is methodological: regional health-equity surveillance needs transparent indicator definitions, country-specific data-contribution reporting, and inferential methods that respect the number and structure of countries. Policy evaluation should combine such monitoring with designs tied to defined reforms, implementation timing, and subnational or individual outcomes.

4.2. Strengths

Strengths include a frozen World Bank snapshot; raw-data manifests and verified SHA-256 hashes; explicit distinction between indicator records and country-year observations; an auditable RAW-SILVER-GOLD pipeline; transparent country contribution; a full-model primary specification chosen for the scientific question; few-cluster bootstrap and jackknife checks; fixed-effect-residualized collinearity diagnostics; and sensitivity analyses that retained null and contradictory results.

4.3. Limitations

First, this is an ecological analysis, so coefficients cannot be transferred to individuals or households. Second, country and year fixed effects do not remove time-varying confounding, reverse causation, mediation, or policy endogeneity. Third, international indicators may be modelled, interpolated, revised, or based on non-comparable survey schedules; the UHC measure is a composite index, not direct individual coverage. Fourth, missingness was extensive and likely informative. Complete-case selection disproportionately excluded Caribbean entities and produced an unbalanced sample in which three countries contributed four or fewer years.

Fifth, only 17 country clusters were available, and cluster sizes were highly unequal. Wild bootstrap and jackknife procedures improve transparency but cannot create information absent from the data. Sixth, each observed country-year received equal regression weight; the estimand is not population-weighted and does not represent the average infant or resident. Seventh, residual serial correlation was high, and spatial or cross-country spillovers were not modelled. Eighth, the reanalysis and most sensitivity analyses were not preregistered. Finally, discrepancies between the 14 July and 17 July notebooks, including incorrect indicator codes in the later notebook, require replacement by a single frozen analysis script before public release.

4.4. Generalizability and Future Research

The findings are most transportable to the 17 contributing countries and observed years, not to all 42 regional entities. Better inference will require harmonized annual socioeconomic series, explicit treatment of data revisions, sufficient Caribbean representation, policy-linked exposure definitions, and preregistered designs. Synthetic control, event-study, interrupted time-series, difference-in-differences, or subnational longitudinal analyses may be appropriate when a specific intervention and credible comparison structure exist.

5. Conclusions

In an auditable 2000–2023 country-year panel, the primary two-way fixed-effects analysis did not provide robust evidence that within-country changes in poverty, inequality, education, government health expenditure, or UHC service coverage were associated with infant mortality after accounting for few country clusters. Negative UHC and expenditure estimates were compatible with modest benefit but remained imprecise. Exploratory first-difference findings require independent, prespecified replication.

The study supports stronger reproducibility and data-governance standards for regional health-equity research, not causal or policy-effect claims.

Declarations

Supplementary Materials

The following materials accompany this preprint: Table S1, country-specific indicator coverage and contribution to the analytic samples; Table S2, regional missingness by variable; Table S3, variance inflation factors after residualizing predictors on country and year fixed effects; Table S4, complete sensitivity-model results; Table S5, complete leave-one-country-out estimates; File S1, frozen reanalysis and reproducibility package containing the sole executable analysis script, pinned environment, frozen raw and processed data, analytic dataset, quality-control records, and machine-readable outputs; File S2, SHA-256 verification manifest for the supplied reproducibility files; and File S3, STROBE coverage matrix. These materials are supplied directly with the preprint and are associated with the Research Square record. Any later archival-repository DOI will be added in a versioned update.

Ethics approval and consent to participate

This study analyzed only publicly available aggregate country-year indicators and did not involve human participants, identifiable personal information, biological specimens, or interventions. No individual-level data were collected or accessed. Formal ethics review was considered not applicable on this basis; the responsible institution must confirm this determination before public posting.

Consent for publication

No individual participant data were collected or accessed, and the manuscript contains no identifiable information. Informed consent and consent for publication were therefore considered not applicable.

Use of generative artificial intelligence

OpenAI ChatGPT (GPT-5.6 Pro; accessed 20–21 July 2026) was used under human direction for independent scientific audit, statistical reconstruction, verification of references and manuscript consistency, figure specification, English-language editing, preparation of the reproducibility package, and Research Square formatting. Numerical results were generated from the supplied data with executable Python code and checked against independent calculations; the tool was not used as an author and did not make autonomous scientific or submission decisions. The human authors remain responsible for the study design, data provenance, analyses, interpretation, citations, tables, figures, declarations, and public posting.

Competing interests

The authors declare no competing interests

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Authors' contributions

Conceptualization, Anderson Díaz-Pérez, Joseph Javier Sánchez Acuña, Daniela Sofía Bolívar Freyle, and Joseph David Arrieta de Avila; Methodology, Anderson Díaz-Pérez, Joseph Javier Sánchez Acuña, Daniela Sofía Bolívar Freyle, and Joseph David Arrieta de Avila; Investigation, Anderson Díaz-Pérez, Joseph Javier Sánchez Acuña, Daniela Sofía Bolívar Freyle, and Joseph David Arrieta de Avila; Data curation, Formal analysis, Software, and Visualization, Joseph David Arrieta de Avila; Validation, Anderson Díaz-Pérez, Joseph Javier Sánchez Acuña, Daniela Sofía Bolívar Freyle, and Joseph David Arrieta de Avila; Project administration, Anderson Díaz-Pérez and Joseph David Arrieta de Avila; Supervision, Anderson Díaz-Pérez; Writing – original draft, Anderson Díaz-Pérez and Joseph David Arrieta de Avila; Writing – review & editing, Anderson Díaz-Pérez, Joseph Javier Sánchez Acuña, Daniela Sofía Bolívar Freyle, and Joseph David Arrieta de Avila. The assigned roles must be confirmed in writing by all listed authors before public posting.

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Availability of data and materials

The source indicators are publicly available from the World Bank API and World Development Indicators. The exact frozen raw JSON files downloaded on 14 July 2026, source and API manifests, processed panel, quality-control records, primary analytic dataset, and machine-readable results used for this manuscript are included in Supplementary File S1. SHA-256 checksums are supplied in Supplementary File S2. The analysis uses only aggregate country-year data.

Code availability

The complete frozen Python 3 pipeline pinned dependency versions, execution instructions, recorded random seeds, canonical audit outputs, and an independent 19,999-draw wild-cluster-bootstrap validation are included in Supplementary File S1. The file code/reanalysis.py is the sole executable public

source of truth and stops if indicator codes, record counts, duplicate checks, primary sample size, or source-file hashes differ from the frozen specification. Because the exact random-weight matrix used for the original audit was not supplied, canonical bootstrap p-values are retained together with an independent deterministic rerun that agrees within expected Monte Carlo error.

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Figures

Figure 1. Audited data flow and construction of the analytic sample.

The figure summarizes the progression from the frozen regional frame to the primary fixed-effects sample used in the main analyses.

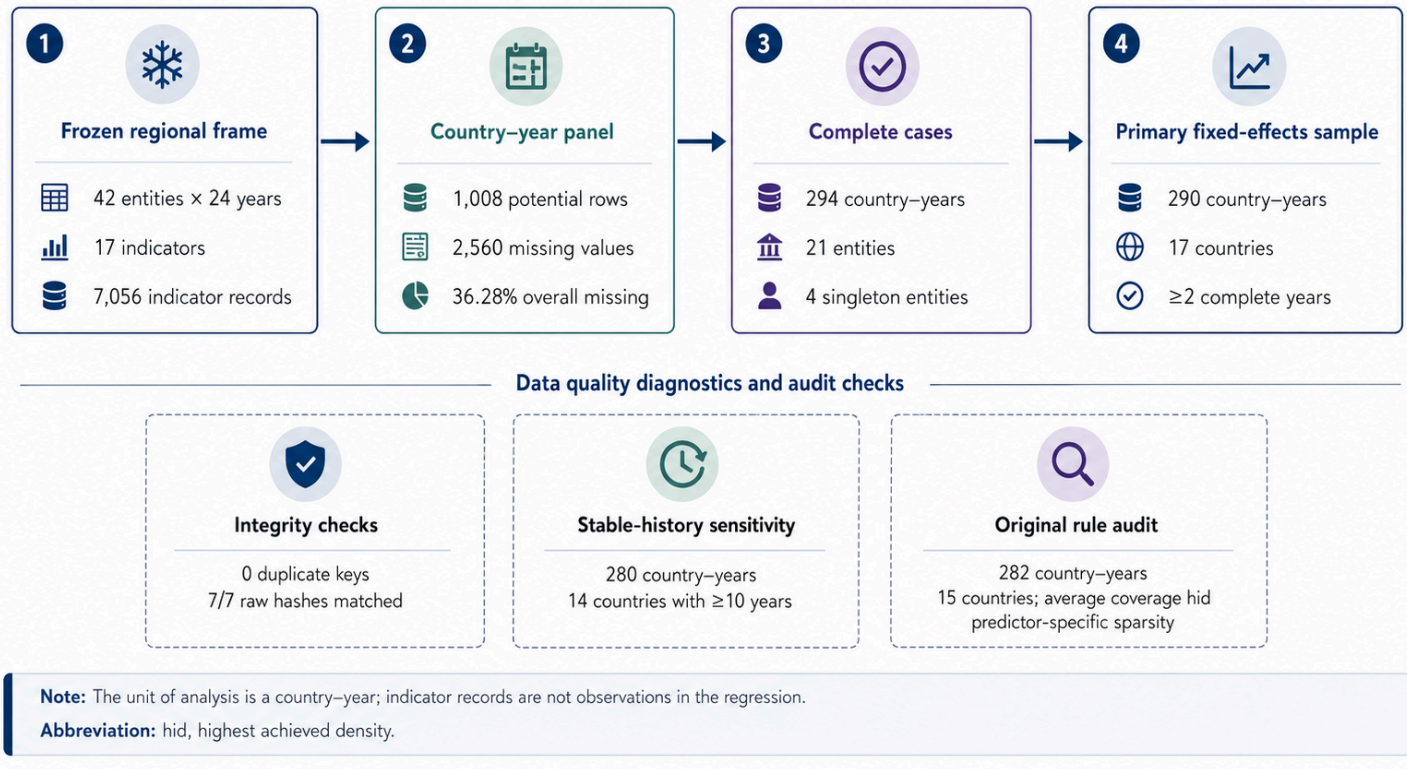


Figure 1

Audited data flow and construction of the primary analytic sample.
The frozen World Bank regional frame comprised 42 countries and territories observed from 2000 through 2023 and seven indicators, yielding 7,056 indicator–country–year records and 1,008 potential country–year observations. Complete-case filtering for infant mortality and the five prespecified predictors retained 294 country–years from 21 entities. Four entities contributing only one complete country–year were excluded because they could not provide within-country variation for fixed-effects estimation. The primary analytic sample therefore included 290 country–years from 17 countries with at least two complete observations. The lower panels summarize data-integrity checks, the stable-history sensitivity sample restricted to countries with at least 10 complete years, and the audit of the original ≥80% average-coverage selection rule. Indicator-level records are not observations in the regression analysis.

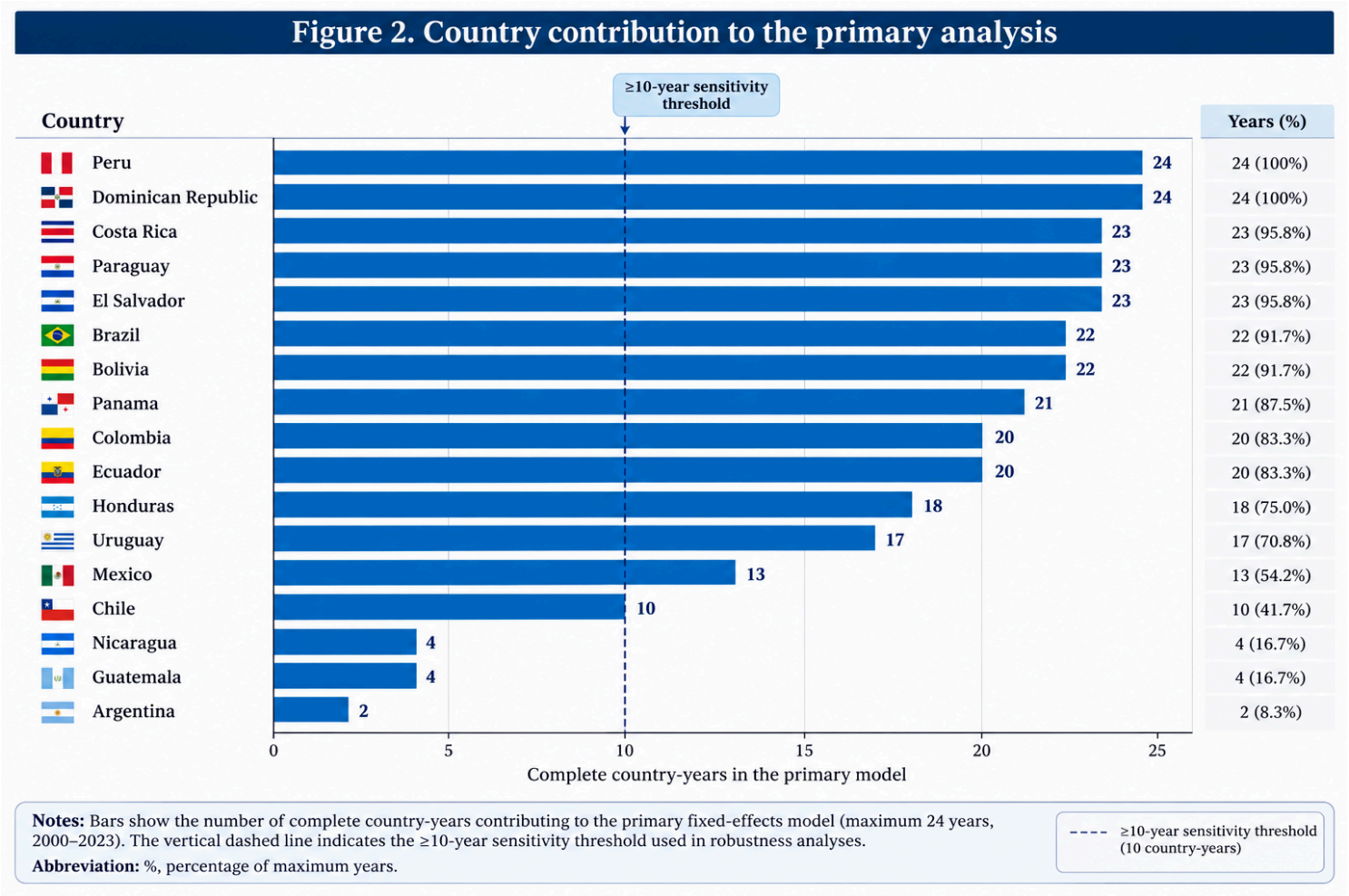


Figure 2

Country contribution to the primary fixed-effects analysis.
Horizontal bars show the number of complete country–years contributed by each country to the primary model during 2000–2023, with a maximum possible contribution of 24 years per country. Countries are

ordered by decreasing number of complete observations. The dashed vertical line indicates the threshold of 10 complete country–years used to define the stable-history sensitivity sample. The right-hand column reports the number of complete years and the corresponding percentage of the 24-year observation period. The primary analytic sample comprised 290 country–years from 17 countries.

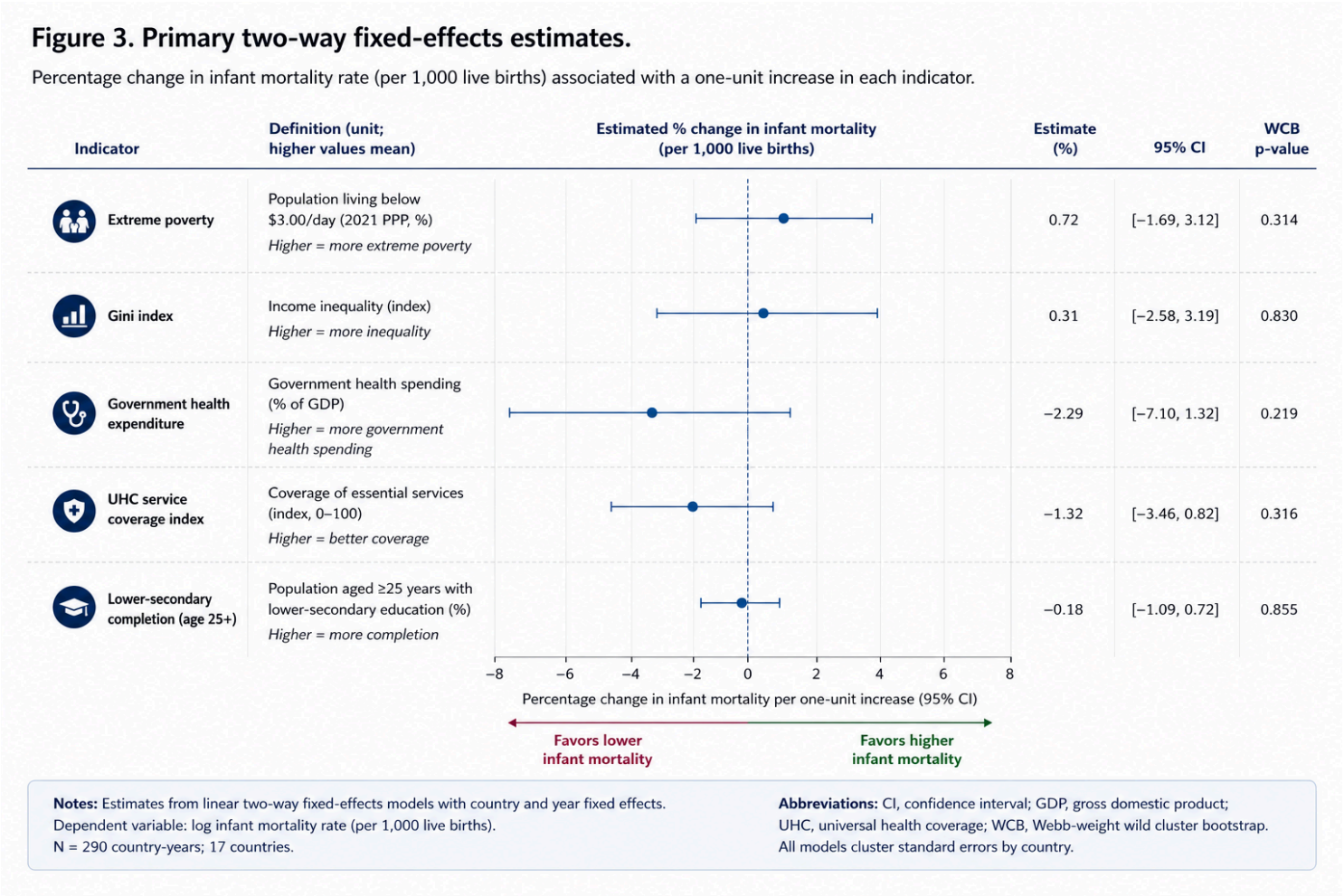


Figure 3

Primary two-way fixed-effects estimates for socioeconomic and health-system indicators. Points represent the estimated percentage change in infant mortality associated with a one-unit increase in each indicator, calculated as 100×[exp(β)−1]100 \times [\exp(\beta)-1]100×[exp(β)−1]. Estimates were obtained from a log-linear model including country fixed effects, year fixed effects, and all five socioeconomic and health-system predictors simultaneously. Horizontal bars represent 95% confidence intervals based on country-clustered CR1 standard errors and a t(16)t(16)t(16) reference distribution. The vertical dashed line denotes the null value of 0%. Negative estimates indicate lower infant mortality, whereas positive estimates indicate higher infant mortality. WCB ppp-values were obtained using restricted Webb-weight wild cluster bootstrap-ttt tests with 19,999 replications. The analytic sample included 290 country–years from 17 countries. **Abbreviations:** CI, confidence interval; GDP, gross domestic product; UHC, universal health coverage; WCB, wild cluster bootstrap.

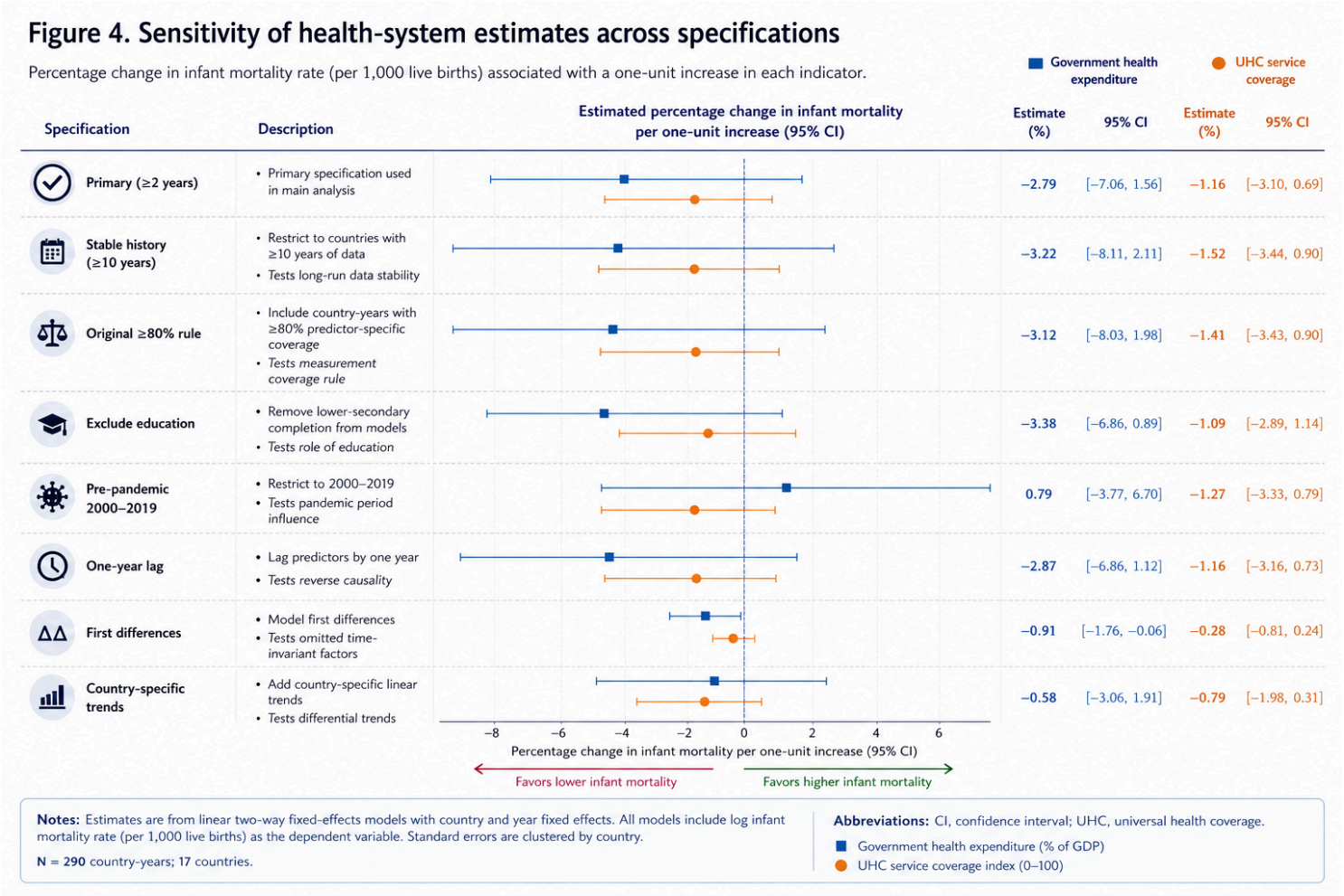


Figure 4

Sensitivity of government health expenditure and universal health coverage estimates across alternative samples and model specifications.
Blue squares and orange circles represent the estimated percentage changes in infant mortality associated with a one-unit increase in government health expenditure, expressed as a percentage of gross domestic product, and the universal health coverage service coverage index, respectively. Horizontal bars represent 95% country-clustered confidence intervals, and the vertical dashed line denotes the null value of 0%. The primary two-way fixed-effects specification is compared with analyses restricted to countries contributing at least 10 complete years, the original ≥80% average-coverage selection rule, a model excluding lower-secondary education, the pre-pandemic period from 2000 through 2019, one-calendar-year lagged predictors, first differences for consecutive observations, and models including country-specific linear trends. First-difference and country-specific-trend specifications estimate different temporal contrasts and should be interpreted as exploratory rather than directly equivalent to the primary model. **Abbreviations:** CI, confidence interval; GDP, gross domestic product; UHC, universal health coverage.

Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- SupplementaryTablesS1S5.docx