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Urban Precarity and Social Risk: A Cross-Country Panel Analysis of Informal Settlements, Youth Unemployment, Urbanization, and Homicide

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Abstract

Background: Urban housing precarity has been associated with socioeconomic exclusion and interpersonal violence, yet evidence remains inconclusive regarding whether these relationships persist across national contexts. In particular, the moderating role of youth unemployment in the relationship between informal-settlement prevalence and homicide remains insufficiently established.

Objective: This study examined the association between urban housing precarity and intentional homicide rates and determined whether youth unemployment moderates this relationship across countries.

Methods: A quantitative longitudinal panel design was employed using World Development Indicators data covering 2000–2020. The analytical sample comprised 1,061 country-year observations from 133 countries and economies across 11 biennial observation waves. Two-way fixed-effects models included slum prevalence, youth unemployment, urbanization, population density, and real GDP per capita. An interaction term assessed moderation, with country-clustered standard errors and multiple robustness specifications.

Results: Slum prevalence was not significantly associated with intentional homicide rates ($\beta = -0.00018$, $p = .953$). Youth unemployment exhibited a positive but statistically insignificant association ($\beta = 0.00472$, $p = .062$). The interaction was negative and statistically insignificant ($\beta = -0.000155$, $p = .229$). The moderation model achieved a within R^2 of .035. The nonsignificant interaction finding persisted in alternative temporal samples, outcome specifications, inequality controls, and lagged-variable models.

Conclusion: The findings provide insufficient evidence that national slum prevalence is independently associated with homicide rates or that youth unemployment moderates this relationship. They underscore the importance of geographic scale, context, and measurement in interpreting urban social risk and support further geographically disaggregated research.

Keywords: *urban precarity; informal settlements; slum prevalence; youth unemployment; intentional homicide; two-way fixed effects; cross-country panel analysis*

1. Introduction

Urbanization is among the most consequential demographic and socioeconomic transformations of the contemporary world. The expansion of cities generates opportunities for economic development, labor-market integration, infrastructure investment, and improvements in public services. Nevertheless, these advantages are unevenly distributed. Comparative evidence from ASEAN economies has similarly shown that aggregate economic growth does not translate uniformly into employment and child-nutrition outcomes across countries (Quinto & Atento, 2025). In many developing economies, urban population growth has outpaced the provision of affordable housing, secure employment, sanitation, transportation, and essential public services. Significant segments of urban populations consequently experience precarious living conditions within otherwise economically dynamic metropolitan environments. These disparities invite investigation of the connections between urban development, socioeconomic exclusion, and public safety.

The persistence of informal settlements illustrates the limitations of urban growth as an indicator of social progress. The United Nations (2026) estimated that approximately 1.16 billion individuals, or 24.8% of the global urban population, lived in slums or informal settlements in 2024. Although the proportion living in such conditions has declined in some regions, absolute numbers have continued to increase alongside urban expansion. Urbanization therefore does not necessarily entail corresponding improvements in housing security, infrastructure accessibility, and social inclusion. The concentration of socioeconomic vulnerability within cities presents an enduring challenge to inclusive development.

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Urban precarity may be conceptualized as insecurity associated with housing, employment, access to essential services, and participation in economic and civic life. Its measurable housing dimension includes the proportion of urban residents living in slum households. The World Bank (2026b), using UN-Habitat data, identifies slum households through deficiencies relating to improved water and sanitation, adequate living space, housing durability, and security of tenure. Local evidence from a densely populated district of Manila likewise shows that access to public toilets and handwashing facilities can remain poor even where household water availability is rated favorably (Marcos et al., 2026). Although this measure cannot represent all facets of urban vulnerability, it supplies a standardized indicator of material housing deprivation for international comparisons.

The relationship between precarious living conditions and violence has attracted sustained interest in criminology and development research. Structural perspectives propose that concentrated deprivation, limited economic opportunity, and institutional weakness may coexist with elevated social risk. Yet the relationships are neither automatic nor uniform. Informal settlements may also display strong community networks, reciprocal assistance, and informal protective institutions. Material deprivation should never be taken as evidence that residents are intrinsically predisposed to violence.

Winter et al. (2022), drawing on interviews with women in Nairobi's Mathare settlement, identified youth economic inactivity, inadequate protective infrastructure, policing problems, and institutional distrust as perceived contributors to violence. These findings locate insecurity in interlocking social and governance conditions. Their qualitative and localized design, however, cannot determine whether similar relationships prevail across countries. At the comparative level, Pridemore (2011) questioned conventional interpretations of inequality–homicide associations by showing that introducing poverty altered earlier results, illustrating the importance of omitted-variable bias and the distinction between economic inequality and material deprivation.

Urbanization likewise produces heterogeneous associations. In a longitudinal cross-national analysis, Clement et al. (2019) reported that urban population share and the share residing in large cities were associated with homicide in different directions. Such findings caution against treating urbanization as intrinsically criminogenic. Labor-market conditions may present an additional pathway: employment instability reduces opportunities for economic participation, but its association with violence differs among offenses and institutional contexts. Fougère et al. (2009), for example, found evidence linking youth unemployment to certain property crimes in France, a finding that should not be transferred uncritically to homicide.

Taken together, the literature suggests that urban development, material deprivation, and labor-market conditions should be examined as potentially interacting dimensions of socioeconomic vulnerability. Yet it remains uncertain whether higher national informal-settlement prevalence corresponds to higher national homicide rates after accounting for youth unemployment, urbanization, demographic concentration, and economic development. It is also uncertain whether unemployment changes the strength or direction of that relationship.

This study addresses those questions using country-level World Development Indicators (WDI) data and a two-way fixed-effects framework. The primary analysis focuses on the eleven common biennial observation waves from 2000 through 2020; an extension through 2022 assesses robustness. Slum prevalence is used as a proxy for urban housing precarity, youth unemployment as a measure of labor-market vulnerability, and intentional homicide per 100,000 residents as the outcome of severe interpersonal violence. The design distinguishes persistent country differences from within-country changes and explicitly tests the interaction between slum prevalence and youth unemployment.

The analysis contributes in three ways. First, it incorporates an internationally standardized housing-deprivation indicator into comparative homicide analysis. Second, it estimates a directly specified labor-market moderation term instead of assuming interactive effects from localized descriptive studies. Third, it evaluates the stability of the findings across alternative samples, transformations, and controls. As national indicators cannot identify the locations or perpetrators of particular crimes, the results are interpreted as country-level associations and not as individual- or neighborhood-level causal mechanisms. The policy context is the intersection of Sustainable Development Goal 11 on inclusive and sustainable cities and Goal 16 on peaceful societies.

1.1 Research Objectives

The general objective is to examine the relationship between urban housing precarity and intentional homicide rates across countries, with particular attention to youth unemployment and broader socioeconomic conditions. The specific objectives are to:

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- describe temporal and cross-country patterns in slum prevalence, homicide, youth unemployment, urbanization, and socioeconomic conditions during 2000–2020;
- estimate the association between changes in slum prevalence and national homicide rates after accounting for country and year effects and selected controls;
- examine the independent association between youth unemployment and homicide;
- evaluate whether youth unemployment moderates the association between slum prevalence and homicide; and
- assess stability across alternative sample periods, specifications, and data-quality restrictions.

1.2 Research Questions

RQ1. What patterns characterize housing precarity and intentional homicide across the observed country-years?

RQ2. Is slum prevalence associated with national homicide rates after accounting for youth unemployment, urbanization, density, and economic development?

RQ3. Is youth unemployment independently associated with national homicide rates?

RQ4. Does youth unemployment moderate the association between slum prevalence and homicide?

RQ5. Are estimated relationships robust to temporal, specification, and sample restrictions?

1.3 Research Hypotheses

H1. Higher slum prevalence is positively associated with national intentional homicide rates, conditional on socioeconomic and demographic controls.

H2. Higher youth unemployment is positively associated with national intentional homicide rates.

H3. Youth unemployment positively moderates the slum prevalence–homicide association, making the association more positive as unemployment increases.

2. Review of Related Literature

2.1 Urban Housing Precarity and Social Risk

Urban housing precarity has been associated with heightened exposure to crime and interpersonal violence, particularly in communities facing inadequate infrastructure, socioeconomic exclusion, and limited institutional protection. Ajom et al. (2022), examining slum prevalence and crime incidence in Calabar, Nigeria, linked deteriorating housing environments, poor accessibility, and restricted security services to greater local vulnerability. The study suggests that deprivation may interact with deficiencies in public governance and neighborhood safety rather than operate in isolation.

Herrera Giraldo et al. (2023) applied a Spatial Durbin Model to labor informality and homicide in Cali, Colombia. They identified a positive spatial association between labor informality and homicide, especially in hillside neighborhoods characterized by precarious socioeconomic conditions. The findings emphasize the analytical importance of employment insecurity alongside residential deprivation. Winter et al. (2022), through interviews with women in Nairobi's Mathare settlement, described youth economic inactivity, inadequate policing, corruption, and deficient protective infrastructure as perceived contributors to violence. These accounts situate urban insecurity within overlapping structural and institutional vulnerabilities.

Nevertheless, residential disadvantage does not predict violence uniformly. Nivette and Peres (2022) observed that social disorganization explained spatial differences in homicide across São Paulo districts, but changes in disorganization did not account for differences in declining homicide trajectories. Gray and Parker (2026) found that rent burden was associated with homicide in United States cities while several other measures of housing instability had limited associations with crime.

These studies position housing precarity as a plausible correlate of violence, but they also indicate substantial dependence on governance, offense type, and geographic scale. Because the literature is predominantly localized, a longitudinal cross-country analysis is needed to determine whether national slum prevalence is significantly related to national homicide after accounting for labor-market vulnerability and broader development conditions.

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2.2 Socioeconomic Deprivation and Homicide

Socioeconomic deprivation has been extensively investigated as a structural correlate of homicide, particularly through poverty, income inequality, and unequal economic opportunity. Kastle et al. (2026), examining 237 countries and territories, identified socioeconomic indicators—including inequality and poverty—among the variables strongly associated with homicide. Schargrodsky and Freira (2023) reported positive relationships between inequality and crime in international and Latin American data, while finding less consistent associations for some measures of economic deprivation. These observations are compatible with relative deprivation perspectives, in which social disparities can shape the context of interpersonal conflict.

Distinguishing absolute from relative deprivation is crucial. Dunn (2025), analyzing 105 countries, associated both absolute and relative poverty with crime indicators. At the subnational level, Aransiola et al. (2021) found that unemployment and inequality were associated with homicide across Brazilian municipalities, and that their combination helped explain differences in lethal violence. These findings motivate the examination of multiple deprivation dimensions rather than reliance on a single income variable.

Contradictory findings nevertheless challenge generalization. Aliyeva (2026), employing alternative cross-country panel estimators, did not identify statistically significant relationships of poverty or income inequality with national homicide. Vilalta and Fondevila (2026) found that the inequality–homicide association differed among Canada, the United States, and Mexico. Variation in measurement, institutional characteristics, sample coverage, and spatial aggregation appears central to these differences.

Accordingly, the present study includes economic development as a primary control and evaluates income inequality in a restricted-sample robustness model. This approach seeks to distinguish national patterns of housing deprivation from broader socioeconomic disadvantage without presuming that the latter has a uniformly positive effect on homicide.

2.3 Youth Unemployment and Violent Crime

Youth unemployment represents a dimension of socioeconomic vulnerability through restricted livelihood opportunities, financial insecurity, and difficulties in the transition to productive employment. Sézard et al. (2021), using dynamic generalized method of moments estimation across 40 Sub-Saharan African countries from 1990 to 2018, identified a positive association between youth unemployment and murder rates. Akinbobola (2022), analyzing Economic Community of West African States countries, similarly reported a positive unemployment–homicide correlation.

Evidence from cities illustrates the importance of offense-specific and temporal contexts. Schleimer et al. (2022) examined adult unemployment shocks in sixteen United States cities during the COVID-19 pandemic and found positive associations with firearm violence and homicide, while associations with other offenses were not uniform. That analysis concerns adult unemployment generally and therefore does not directly estimate a youth-specific effect. Oboi et al. (2025), analyzing security in Lira City, Uganda, associated youth unemployment with aggravated robbery but did not identify comparable relationships with all violent offenses.

Potential explanations include economic strain, weakened institutional attachment, and greater vulnerability to recruitment by violent organizations. Umanah et al. (2023) examined intersections of youth unemployment and violence in Nigeria's Niger Delta. Such mechanisms should not imply that unemployed people are inherently inclined toward criminality; they describe conditional social environments rather than individual inevitabilities. Conceptual work on adolescent aggression likewise treats proposed risk pathways as conditional mechanisms shaped by proximal social processes and buffering factors rather than as direct deterministic causes (Atento, Quinto, & Atento, 2026).

Taken together, the studies justify evaluating youth unemployment as a potential national-level correlate of homicide, while emphasizing that offense definitions, institutional context, and labor-market measurement can alter the direction and magnitude of estimated relationships. This study consequently evaluates youth unemployment both as an explanatory variable and as a proposed moderator.

2.4 Urbanization, Population Density, and Violence

Urbanization and population density are commonly examined as structural determinants of homicide, but evidence remains inconsistent across geographic contexts and methods. Clement et al. (2023), using longitudinal data for 83 countries, demonstrated that dimensions of urbanization can display countervailing and persistent associations with homicide. These results imply that urban development combines conditions potentially associated with higher social risk and characteristics that may reduce it.

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Subnational analyses illustrate additional heterogeneity. Wanzinack et al. (2022), studying 5,570 Brazilian municipalities, reported positive associations between homicide, urbanization, population size, and inequality, while human development exhibited an inverse association. By contrast, de Lima Friche et al. (2023), analyzing 315 Latin American cities, did not find a uniform positive association between city population density and youth homicide. Mohammadi et al. (2022), studying Toronto neighborhoods, identified spatial associations involving the built environment, density, and socioeconomic characteristics that varied geographically.

Cross-national longitudinal research also cautions against uniform developmental interpretations. Li et al. (2023) identified heterogeneous national homicide and suicide trajectories, while Rogers and Pridemore (2023) demonstrated that homicide measurement choices can alter estimated associations with structural covariates. The direction of urbanization–homicide relationships therefore depends partly on scale, definition, reporting systems, and model specification.

The present study incorporates urban population share and national population density as separate controls. The former measures the proportion of inhabitants residing in areas defined as urban, whereas the latter measures persons per unit of national land area. Neither variable directly measures neighborhood crowding. Their inclusion helps separate broad demographic patterns from the specific association attributed to slum prevalence.

2.5 Interaction Between Urban Precarity and Youth Unemployment

The possible interaction between urban housing precarity and youth unemployment provides a plausible conceptual bridge between residential disadvantage and labor-market exclusion. Aransiola et al. (2021), applying negative binomial models to Brazilian municipalities, identified associations of unemployment and inequality with homicide and examined how combined deprivation measures were related to lethal violence. Their analysis indicates the value of studying potentially interacting sources of disadvantage instead of assuming wholly independent effects.

Antunes and Manasse (2022), drawing on multilevel Chicago data, found that concentrated neighborhood disadvantage conditioned relationships involving individual strain and youth violence. Similarly, Ejlskov et al. (2023), using a Danish national cohort, reported that the socioeconomic characteristics of residential environments modified relationships between individual and family disadvantage and later violent criminality. These studies provide general support for contextual moderation but do not directly test youth unemployment as a moderator of national slum prevalence.

Qualitative and spatial evidence complements this perspective. Winter et al. (2022) reported that residents of a Nairobi informal settlement identified youth inactivity, deficient policing, and infrastructure problems among the circumstances linked to insecurity. Herrera Giraldo et al. (2023) found concentrations of labor informality and homicide in disadvantaged neighborhoods of Cali. Both studies suggest a shared spatial context for housing and labor vulnerability, although neither demonstrates the proposed cross-country interaction.

The empirical gap is therefore specific: existing findings do not establish whether the national youth unemployment rate statistically changes the within-country association between slum prevalence and homicide. The present study addresses this question by estimating and testing the product term between mean-centered slum prevalence and youth unemployment, while avoiding the claim that significant moderation is already established.

2.6 Synthesis of Related Literature

The literature identifies urban housing deprivation, poverty, inequality, youth unemployment, and urbanization as interconnected aspects of socioeconomic vulnerability, yet their relationships with homicide vary by location, institutional context, and measurement. Research in informal settlements links local insecurity to infrastructure deficits, labor informality, and governance problems (Winter et al., 2022; Herrera Giraldo et al., 2023). Cross-country investigations frequently identify inequality as a correlate of homicide, although alternative specifications generate mixed findings (Schargrodsky & Freira, 2023; Aliyeva, 2026). Youth unemployment has been associated with murder in some regional panels and with specific forms of violence in urban studies, but not consistently across all offenses (Sézard et al., 2021; Schleimer et al., 2022).

Urbanization and density offer still less uniform evidence. The outcomes observed by Clement et al. (2023) and de Lima Friche et al. (2023) indicate that demographic concentration alone is an inadequate explanation for lethal violence. Research on interacting disadvantages further suggests that socioeconomic and neighborhood contexts may jointly shape risk (Antunes & Manasse, 2022; Ejlskov et al., 2023), without resolving the particular role of national slum prevalence and youth unemployment.

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This synthesis supports an integrated panel analysis that estimates housing-deprivation and employment associations jointly while controlling for persistent country characteristics and common year effects. It also warns against uncritical transfer of neighborhood-level findings to aggregated national outcomes. The proposed interaction model offers a transparent test of an important but insufficiently examined proposition: whether youth unemployment systematically modifies the association between national slum prevalence and homicide.

2.7 Research Gaps

Four gaps emerge from the review. First, localized studies often examine informal settlements and crime without directly analyzing standardized national slum-prevalence indicators in longitudinal homicide models. Second, although unemployment and deprivation are frequently examined together, the specific interaction between national slum prevalence and youth unemployment has not been conclusively established. Third, the evidence is difficult to reconcile across countries because homicide definitions, reporting systems, spatial scale, and econometric estimators vary (Rogers & Pridemore, 2023). Fourth, cross-sectional research cannot distinguish persistent differences among countries from changes occurring within the same countries over time.

The present investigation addresses these gaps through an unbalanced cross-country panel, country and year fixed effects, explicit estimation of the unemployment–slum interaction, and sensitivity analyses covering data discontinuities and alternative specifications. The aim is not to infer neighborhood-level causal processes from national aggregates, but to identify whether hypothesized relationships remain statistically detectable under a transparent comparative longitudinal design.

3. Research Methodology

3.1 Research Design

The study employs a quantitative explanatory design based on longitudinal cross-country panel data. Two-way fixed-effects regression assesses whether within-country changes in slum prevalence and youth unemployment are associated with changes in intentional homicide, conditional on selected demographic and economic indicators. Country fixed effects absorb time-invariant national characteristics; year fixed effects account for common shocks and global temporal trends. The design remains associational because reverse causality, measurement error, and omitted time-varying determinants may affect the estimates.

3.2 Data Sources and Study Period

The primary source is the World Bank's World Development Indicators (WDI) dataset provided for this project. The intentional-homicide series is based on UNODC statistics (World Bank, 2026a); the slum-prevalence series draws on UN-Habitat; youth unemployment estimates originate in ILO statistical systems; and demographic and economic controls are obtained from WDI constituent sources.

The baseline period comprises eleven biennial waves: 2000, 2002, 2004, 2006, 2008, 2010, 2012, 2014, 2016, 2018, and 2020. This reflects the predominant reporting structure of slum prevalence in the available dataset. Three complete observations outside this grid, in 2009 and 2019, were retained for a sensitivity check. The extended-period analysis adds 2022, subject to a separate examination of large slum-prevalence discontinuities.

3.3 Study Population and Sampling

The initial extracted country/economy panel contains 217 units and 4,991 potential country-year observations for 2000–2022. A complete-case screen across the six core variables identified 1,076 observations in 146 countries/economies during 2000–2020. Restricting these data to the common biennial years produced 1,073 observations in 145 units. Twelve units contained only one eligible observation and were removed from the principal fixed-effects estimation, resulting in 1,061 country-year observations across 133 units.

The resulting dataset is unbalanced: countries are retained even when they lack observations for some years. Unbalanced WDI country–year panels with population density, GDP per capita, and unemployment covariates have similarly been used to examine persistent cross-country differences in development outcomes (Atento, 2026). The primary analysis does not interpolate or otherwise impute absent values. Inclusion therefore depends on data availability and the resulting sample cannot be assumed to represent all countries equally.

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3.4 Variables and Operational Definitions

Table 1. Operational Definitions of Study Variables

Variable	Operational definition	WDI code
Intentional homicide	Homicides per 100,000 population (dependent variable)	VC.IHR.PSRC.P5
Slum prevalence	Urban population living in slums (%)	EN.POP.SLUM.UR.ZS
Youth unemployment	Unemployed ages 15–24 (% of youth labor force)	SL.UEM.1524.ZS
Urbanization	Urban population (% of national total)	SP.URB.TOTL.IN.ZS
Population density	People per square kilometer of national land area	EN.POP.DNST
GDP per capita	Constant 2015 US dollars	NY.GDP.PCAP.KD
Income inequality	Gini index (supplementary only)	SI.POV.GINI

Note. Slum prevalence proxies material housing deprivation; it is not a comprehensive measure of urban precarity. National population density does not measure density within informal settlements.

Intentional homicide reflects severe lethal interpersonal violence and is not a proxy for all crime. Youth unemployment includes economically active persons aged 15–24 without work; it does not represent all young people outside employment. GDP per capita and national population density are logarithmically transformed in the regression models. Gini is reserved for sensitivity analysis because its missingness sharply reduces the available sample.

3.5 Data Preparation and Descriptive Analysis

Selected WDI indicator series were extracted and converted from country–indicator rows to a country–year panel. Regional and income aggregates were excluded so that individual country/economy units were not mixed with composite groups. Unique identifiers were checked, missing values were left unchanged, and model-specific complete cases were identified. Distributions were reviewed for extreme values, zero observations, and changes between consecutive slum-prevalence waves. Descriptive statistics and Pearson correlations were calculated on the primary estimation sample. Pooled correlations were treated as descriptive because they cannot distinguish within-country from between-country differences.

3.6 Econometric Specification

3.6.1 Baseline Fixed-Effects Model

$$\ln(1 + \text{Homicide}_{it}) = \alpha_i + \lambda_t + \beta_1 \text{Slum}_{it} + \beta_2 \text{Youth}_{it} + \beta_3 \text{Urban}_{it} + \beta_4 \ln(\text{Density}_{it}) + \beta_5 \ln(\text{GDPpc}_{it}) + \varepsilon_{it}$$

Here, i indexes country/economy and t indexes the observation wave. α_i denotes country fixed effects, λ_t year effects, and ε_{it} the residual. The outcome transformation ln(1 + homicide rate) retains reported zeros while reducing positive skewness. Coefficients are on a transformed outcome scale and are not exact percentage effects on the original homicide rate.

3.6.2 Interaction Model

$$\ln(1 + \text{Homicide}_{it}) = \alpha_i + \lambda_t + \beta_1 \text{Slum}^c_{it} + \beta_2 \text{Youth}^c_{it} + \beta_3 (\text{Slum}^c_{it} \times \text{Youth}^c_{it}) + \beta_4 \text{Urban}_{it} + \beta_5 \ln(\text{Density}_{it}) + \beta_6 \ln(\text{GDPpc}_{it}) + \varepsilon_{it}$$

The superscript c denotes grand-mean centering within the analytical sample. Centering makes the lower-order coefficient interpretable at the sample-average value of the other interacting variable. The parameter β_3 tests whether the conditional slum–homicide association changes with youth unemployment. Its conditional marginal association is $\beta_1 + \beta_3(\text{Youth}^c_{it})$. Statistical moderation does not establish a causal amplification mechanism.

3.7 Estimation and Diagnostic Procedures

Two-way fixed-effects regression was estimated with country and observation-year effects. Standard errors were clustered by country to accommodate heteroskedasticity and within-country dependence; two-sided tests used $\alpha = .05$. Comparable two-way fixed-effects specifications with economy-clustered standard errors have been applied to regional development panels examining SDG interlinkages (Atento & Quinto, 2026). The principal emphasis was on estimates, cluster-robust confidence intervals, and within R^2 rather than pooled explanatory performance. Multicollinearity was assessed after accounting for fixed effects. The observed interaction was supplemented with

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conditional marginal-effect estimates at selected youth-unemployment percentiles. The diagnostic outputs are exploratory; cross-sectional dependence and broader identification challenges remain relevant.

3.8 Robustness and Sensitivity Procedures

Robustness specifications excluded 2020, included 2022, excluded countries flagged for absolute 2020–2022 slum-prevalence changes exceeding 10 percentage points, incorporated off-cycle observations, omitted zero-slum observations, used untransformed homicide rates, added the Gini index on an available-data subset, and examined two-year lagged slum prevalence and youth unemployment. Lagged two-way fixed-effects specifications at multiple intervals have similarly been used to assess the temporal persistence of country-level panel associations (Patron et al., 2026). The Gini-adjusted specification was compared with a no-Gini model estimated on the same restricted sample. The discontinuity threshold was used as a diagnostic flag rather than evidence of data error; observed values were not silently corrected.

3.9 Measurement and Validity Limitations

The national aggregate design cannot establish where particular homicides occurred or infer individual behavior from country-level relationships. Slum definitions and reporting schedules vary, homicide registration differs across national systems, and youth unemployment omits informal and discouraged employment conditions. Platform-based delivery work, for example, combines active employment with cost, financial, and operational pressures that conventional unemployment rates do not register (Hinagpis & Mendoza, 2026). Complete-case selection may create nonrandom sample composition. Fixed effects account for stable heterogeneity but not reverse causality or omitted time-varying factors such as policing, organized crime, institutional change, and armed conflict. All estimated relationships are therefore reported as conditional associations.

3.10 Research Ethics and Reproducibility

The study relies exclusively on public, country-level secondary indicators and does not involve identifiable human participants. The supplied data extract and reproducible Python analysis files preserve the initial observations, sample rules, transformation choices, and model outputs. Final reporting identifies the estimation periods, country counts, exclusions, uncertainty estimates, and sensitivity analyses for independent assessment.

4. Results and Discussion

4.1 Descriptive Statistics

The primary analytic sample contains 1,061 complete country-year observations from 133 countries/economies across eleven biennial waves. Table 2 summarizes the distributions in the fixed-effects sample.

Table 2. Descriptive Statistics, 2000–2020 (N = 1,061)

Variable	Mean	SD	Min	Max
Slum prevalence (%)	20.14	22.28	0.00	86.58
Homicide rate (per 100,000)	7.49	11.45	0.00	85.09
Youth unemployment (%)	18.14	12.17	0.31	65.02
Urbanization (%)	60.18	21.52	12.22	100.00
Population density (per km²)	210.51	736.91	1.61	7,918.95
GDP per capita (2015 US$)	15,296.36	20,136.27	256.18	110,096.66

Source. Calculations from the supplied WDI dataset. Observation-weighted statistics; countries with more observed waves contribute more rows.

Homicide rates display pronounced dispersion: the mean is 7.49 per 100,000 and the standard deviation 11.45, with a maximum of 85.09. Slum prevalence averages 20.14% but ranges from 0 to 86.58%. Youth unemployment averages 18.14%. Economic development and national density also vary widely. Such between-country heterogeneity motivates attention to country fixed effects. High overall slum variation does not itself imply substantial within-country variation over time.

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4.2 Correlation Analysis

Table 3. Pearson Correlations Among Core Variables (N = 1,061)

Variable	Homicide	Slum %	Youth U	Urban %	Density	GDPpc
Homicide	1.000	0.164	0.037	-0.090	-0.087	-0.269
Slum %	0.164	1.000	-0.190	-0.608	-0.065	-0.522
Youth U	0.037	-0.190	1.000	0.036	-0.090	-0.154
Urban %	-0.090	-0.608	0.036	1.000	0.182	0.586
Density	-0.087	-0.065	-0.090	0.182	1.000	0.167
GDPpc	-0.269	-0.522	-0.154	0.586	0.167	1.000

Note. Descriptive pooled Pearson correlations. They do not account for country or year fixed effects.

Slum prevalence exhibits a weak positive pooled correlation with homicide (r = .164), whereas the youth unemployment–homicide correlation is near zero (r = .037). Urbanization and GDP per capita correlate negatively with homicide (r = −.090 and −.269, respectively). Slum prevalence correlates negatively with urbanization (r = −.608) and GDP per capita (r = −.522). Because pooled correlations combine cross-country differences and within-country change, they cannot be interpreted as estimates of the hypotheses tested in the fixed-effects models.

4.3 Two-Way Fixed-Effects Regressions

Table 4. Two-Way Fixed-Effects Regression Results

Regressor	Model 1 β (p)	Model 2 β (p)	Model 3 β (p)
Slum prevalence	-0.00002 (0.996)	-0.00018 (0.953)	-0.00054 (0.849)
Youth unemployment	—	0.00472 (0.062)	0.00296 (0.313)
Slum × Youth unemployment	—	—	-0.00016 (0.229)
Urbanization	0.00278 (0.632)	0.00172 (0.756)	0.00173 (0.752)
ln(Population density)	0.14125 (0.627)	0.12090 (0.671)	0.14769 (0.611)
ln(GDP per capita)	-0.25257 (0.121)	-0.20705 (0.195)	-0.20127 (0.198)
Country and year FE	Yes	Yes	Yes
Observations	1,061	1,061	1,061
Countries/economies	133	133	133
Within R^2	0.0230	0.0309	0.0350

Dependent variable: ln(1 + homicide per 100,000). Country-clustered standard errors; entries give coefficient and two-sided p-value. Model 3 mean-centers the interacting variables. No coefficient meets p < .05.

4.3.1 Slum Prevalence and Homicide

Model 1 yields a near-zero association between slum prevalence and homicide (β = −0.000016, p = .996). Model 2 produces a similarly insignificant estimate after introducing youth unemployment (β = −0.000176, p = .953). The positive pooled correlation is therefore not reproduced in the within-country specification with country and year effects. A comparable divergence has been documented in another WDI panel, where a significant pooled coefficient disappeared once country heterogeneity was controlled and within-country explanatory power was negligible (Tabujara et al., 2026). The finding does not prove that housing deprivation is unrelated to violence; it indicates that the proposed national-level association is not statistically distinguishable from zero under these controls and this panel.

4.3.2 Youth Unemployment and Homicide

In Model 2, youth unemployment has a positive coefficient (β = 0.004724) with p = .0615. The direction is consistent with H2, but the coefficient does not reach the pre-specified 5% significance threshold. In Model 3, its conditional association at mean slum prevalence is β = 0.002958 (p = .313). It would be inappropriate to characterize either coefficient as a confirmed positive effect. The data provide imprecise, model-dependent evidence of an unemployment–homicide association.

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4.3.3 Moderating Effect

Model 3 estimates a negative interaction coefficient of β = −0.0001555 (p = .2288), with a 95% confidence interval of [−0.0004099, 0.0000989]. The interval includes zero and plausible effects in both directions. Thus, the model does not support the proposition that higher youth unemployment strengthens the positive slum–homicide association. Although the point estimate is negative, this cannot be interpreted as evidence of a protective relationship.

4.3.4 Model Fit

Within R^2 increases from .0230 in Model 1 to .0309 in Model 2 and .0350 in Model 3. Hence, the measured variables explain only a modest fraction of variation remaining after removing country and year fixed effects. The very high overall fit attributable to country effects should not be conflated with explanatory power of the study regressors for changes within countries.

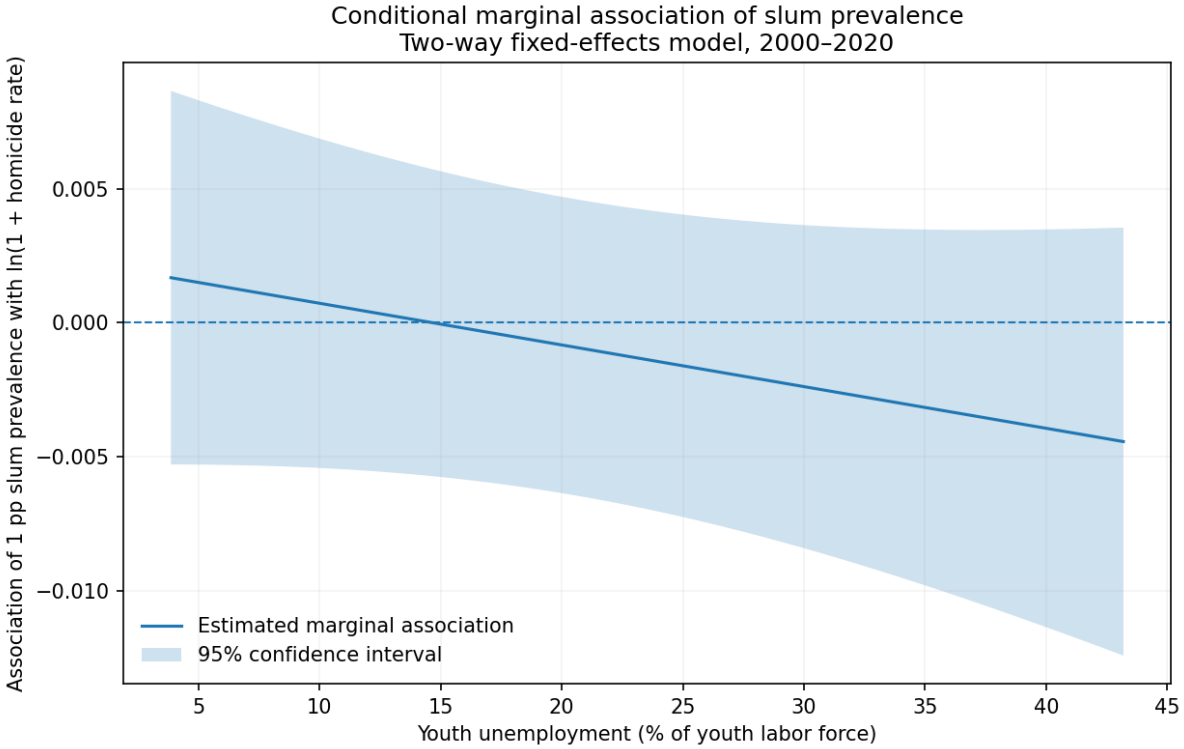
4.4 Conditional Marginal Effects

Table 5. Conditional Marginal Association of Slum Prevalence at Youth-Unemployment Percentiles

Percentile	Youth U (%)	Marginal β	95% CI	p
P10	5.69	0.00140	[-0.00529, 0.00809]	0.680
P25	9.29	0.00084	[-0.00538, 0.00706]	0.790
Median	15.44	-0.00012	[-0.00579, 0.00556]	0.967
P75	23.61	-0.00139	[-0.00697, 0.00419]	0.624
P90	35.61	-0.00325	[-0.00998, 0.00347]	0.340

Note. Effects are on the ln(1 + homicide) scale for a one-percentage-point difference in slum prevalence. All confidence intervals include zero.

At the 10th percentile of youth unemployment (5.69%), the estimated marginal association of slum prevalence with transformed homicide is β = .00140. It approaches zero at the median (15.44%) and becomes negative at the 90th percentile (35.61%; β = −.00325). None of these estimates is statistically different from zero. The apparent slope across unemployment levels is therefore descriptive of model point estimates rather than evidence of significant moderation.



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Figure 1. Conditional marginal effects of slum prevalence across youth unemployment levels. Shading shows 95% confidence intervals, which encompass zero throughout the plotted range.

4.5 Robustness and Sensitivity Analysis

Table 6. Robustness of the Slum Prevalence × Youth Unemployment Interaction

Specification	N	Interaction β	p
Baseline 2000–2020	1,061	-0.000155	0.229
Exclude 2020	973	-0.000128	0.332
Extend through 2022	1,138	-0.000152	0.205
Exclude flagged countries	1,083	-0.000170	0.168
Include off-cycle years	1,065	-0.000155	0.230
Positive-slum observations	940	-0.000174	0.189
Untransformed outcome	1,061	0.000780	0.643
Add Gini index	559	-0.000142	0.608
Lag slum and youth 2 years	979	-0.000082	0.599

Note. All models include country and year effects with country-clustered standard errors. The untransformed-outcome coefficient is on a different outcome scale; sample composition varies by specification.

The interaction remains statistically insignificant across all reported specifications. Excluding 2020 yields p = .332, extending the period through 2022 yields p = .205, and excluding countries flagged for exceptional 2020–2022 slum changes yields p = .168. Its insignificance also persists after incorporating off-cycle observations (p = .230), omitting zero-slum observations (p = .189), and using untransformed homicide rates (p = .643).

Including the Gini index restricts estimation to 559 observations in 77 countries/economies. In this sample the interaction remains nonsignificant (p = .608), as does Gini (p = .657). The corresponding restricted-sample model without Gini was also estimated to distinguish the consequences of sample loss from the additional variable. The two-year lagged interaction similarly remains nonsignificant (p = .599). These findings indicate that the hypothesis decision is not driven by any single examined modeling choice, although nonsignificance does not demonstrate the absence of an underlying effect.

The inclusion of off-cycle observations increases the fixed-effects estimation sample by four, from 1,061 to 1,065, even though only three new complete country-year records are added. Two countries that were previously singletons become eligible for the within-country model, explaining the difference. This reflects the sampling rule rather than a computational error.

4.6 Discussion of Findings

The central empirical result contrasts with localized studies documenting associations among informal settlements, employment exclusion, and violence (Winter et al., 2022; Herrera Giraldo et al., 2023). Geographic aggregation is a plausible source of divergence. Specific neighborhoods may simultaneously experience housing deprivation, poor infrastructure, and elevated crime, while national homicide rates combine incidents occurring inside and outside informal settlements. National slum prevalence may consequently be too coarse to register geographically concentrated risk.

The distinction between within-country and between-country variation is equally important. Slum prevalence varies substantially across the pooled sample (SD approximately 22.28 percentage points), but much less within countries over time (approximately 6.29 percentage points). Twenty-one countries/economies exhibit no reported within-country slum variation across their included observations. Fixed effects remove persistent cross-country differences and identify associations using the more limited remaining within-country changes. The absence of precise estimates may partly reflect that reduced information. Cross-country research on violence-accepting gender norms has reported a similar pattern: associations observed in cross-sectional models did not significantly explain within-country changes under country fixed effects (Quinto, Atento, Espelita, et al., 2026).

The results are compatible with literature emphasizing contextual heterogeneity. Clement et al. (2023) demonstrated countervailing dimensions of urbanization, while Rogers and Pridemore (2023) emphasized that data-source choices can alter cross-national homicide findings. Other two-way fixed-effects panels of WDI data have

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likewise found aggregate national indicators to be imprecise predictors of within-country change amid substantial and persistent cross-country heterogeneity (Dela Cruz & Atento, 2026b). Homicide is influenced by social and institutional processes not fully represented by the current model, including organized crime, political conflict, policing quality, firearm availability, and historical violence patterns. Youth unemployment also incompletely measures labor-market precarity where informal work and underemployment are pervasive.

The analysis therefore does not invalidate structural theories of deprivation and violence. Rather, it demonstrates that their hypothesized implications are not detectable as stable, statistically significant within-country national relationships under the selected indicators and model. Interpretation should avoid both overclaiming a deprivation–homicide effect and treating nonsignificant estimates as definitive evidence that no relationship exists.

4.7 Summary of Hypothesis Testing

Table 7. Research Hypotheses and Decisions

Hypothesis	Coefficient / p-value	Decision
H1: Slum prevalence positively associated with homicide	$\beta = -0.000176$; $p = .953$	Not supported
H2: Youth unemployment positively associated with homicide	$\beta = 0.004724$; $p = .0615$	Not supported at $\alpha = .05$
H3: Youth unemployment amplifies slum–homicide association	$\beta = -0.000155$; $p = .229$	Not supported

Note. H1 and H2 use the additive Model 2; H3 uses the interaction Model 3. A nonsignificant result is not a statistical demonstration of no effect.

5. Conclusions, Policy Implications, and Recommendations

5.1 Summary of Principal Findings

Using 1,061 observations from 133 countries/economies spanning the common biennial waves of 2000–2020, the study assessed national slum prevalence, youth unemployment, and their interaction in relation to intentional homicide. The main models controlled for urbanization, population density, GDP per capita, country-specific characteristics, and common year effects.

Three findings emerge. First, slum prevalence was not significantly associated with homicide in either the restricted or expanded additive models. Second, youth unemployment exhibited a positive but statistically insignificant coefficient in the additive model ($\beta = 0.004724$, $p = .0615$). Third, the centered interaction between slum prevalence and youth unemployment was negative and statistically insignificant ($\beta = -0.000155$, $p = .229$). Its nonsignificance was robust across alternative samples, model transformations, additional inequality controls, and lagged specifications.

5.2 Conclusions

The study concludes that the available national panel does not provide statistically sufficient evidence of a positive within-country association between slum prevalence and homicide or of a youth-unemployment moderation effect. The findings do not imply that urban deprivation or unemployment is irrelevant to public safety; instead, they identify limits to generalizing localized settlement–violence relationships to national aggregate homicide measures.

Persistent country characteristics, spatial aggregation, sparse slum-prevalence reporting, and important omitted institutional determinants are plausible reasons for the limited explanatory performance. Housing deprivation may coexist with local vulnerability and social exclusion even when national within-country associations are indistinguishable from zero. The contribution is consequently one of careful empirical qualification: structural vulnerability indicators alone explain only a small share of national homicide changes after accounting for fixed effects, and their hypothesized interaction is not supported under the examined specifications.

5.3 Policy Implications

5.3.1 Housing and Settlement Development

The absence of a significant national association should not be used to discount investment in housing durability, sanitation, water, tenure security, transport, and other basic services. Such improvements have intrinsic public-health

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and human-development value. Nevertheless, urban upgrading should not automatically be presented as a proven homicide-reduction intervention on the basis of the current evidence. Violence-focused programs should include location-specific safety indicators and appropriate evaluation designs.

5.3.2 Youth Employment and Social Inclusion

Youth employment and skills programs remain defensible on grounds of livelihood opportunity, household stability, and economic participation. The observed unemployment coefficient is insufficient to show that these programs will independently reduce homicide. Programs with explicit violence-prevention goals should evaluate crime and victimization outcomes rather than infer them from changes in employment alone.

5.3.3 Integrated Urban Safety

Public safety planning should integrate locally disaggregated crime statistics with information on neighborhood infrastructure, governance, and institutional capacity. The findings argue against stigmatizing informal-settlement residents by equating housing poverty with criminality. Qualitative evidence from homeless individuals in Batangas City similarly identifies stigma, informal labor, and perceived exclusion among the conditions restricting access to essential services for people without stable shelter (Macatangay & Magnaye, 2026). Housing disadvantage is a condition of vulnerability, not a measure of individual criminal responsibility. Related conceptual work cautions more generally against attributing structurally conditioned behavior among disadvantaged populations to fixed dispositional traits (Dela Cruz & Atento, 2026a).

5.3.4 Statistical Development

National statistical agencies and international repositories could strengthen evidence by improving the continuity, comparability, and subnational detail of slum, employment, and homicide indicators. Broader scholarship treats public data openness and coherent data governance as institutional policy instruments with consequences for the business and investment environment (Atento, Espelita, & Atento, 2026). Better linked neighborhood datasets could test whether relationships concealed by national aggregation are observable at the geographic level relevant to local policy.

5.4 Limitations

The study uses national aggregates and therefore cannot identify individual or neighborhood processes without ecological-inference error. Slum prevalence is an incomplete proxy for urban precarity, youth unemployment excludes important dimensions of informal and discouraged employment, and homicide registration varies across countries. The unbalanced complete-case panel may be affected by nonrandom data availability. The biennial observation grid also restricts temporal resolution, and fixed effects do not remove time-varying omitted variables or reverse causality. These limitations constrain the precision, scope, and causal interpretation of the results.

5.5 Recommendations for Future Research

Future research should use city, municipal, or neighborhood data with harmonized measures of housing conditions, employment, victimization, and institutional characteristics. Multilevel designs could distinguish individual and household vulnerability from contextual neighborhood associations. Broader measures of precarity—including overcrowding, sanitation, tenure security, underemployment, youth not in employment, education or training (NEET), and labor informality—may be more informative than slum prevalence and conventional unemployment alone.

Further analyses should investigate regional and income-group heterogeneity, nonlinearities, institution-related controls, and credible causal identification where suitable instruments or policy shocks exist. Threshold-based evidence that macroeconomic relationships can be regime-dependent and country-specific illustrates why nonlinear specifications merit attention (Chen & Atento, 2026). Improved time-series continuity would also permit stronger assessment of lag structures and dynamic relationships, as lagged panel models elsewhere have detected delayed associations not evident in contemporaneous specifications (Quinto, Atento, & Nemis, 2026). All such extensions should maintain a clear distinction between observed associations and causal effects.

5.6 Final Conclusion

Urban precarity is a major challenge to inclusive development, but its relationship with lethal interpersonal violence cannot be presumed direct or uniform. The present analysis found no statistically significant within-country relationship between national slum prevalence and homicide, and no significant moderating influence of youth unemployment, across the tested specifications. These results encourage greater attention to scale, measurement,

institutions, and local heterogeneity in comparative research on urban social risk. They do not eliminate the developmental imperative to improve precarious housing and employment conditions.

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