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Criminological Doppelgängers? Similar Starting Homicide Rates and Divergent Change in Six Latin American Countries

Zul Khaidir Kadir[1]
[1] Universitas Muslim Indonesia, Indonesia
Correspondence:: zulkhaidir.kadir@umi.ac.id

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ABSTRACT

Cross-national homicide rates are commonly compared across countries with very different demographic and institutional settings. Two countries may begin with similar rates but subsequently move in opposite directions. Such a contrast is easy to describe yet difficult to interpret if the countries were chosen after their later trends became known. This article examines five annual homicide observations for six Latin American countries from the World Bank, covering 2019–2023. The comparison uses descriptive rate changes, annual slopes, and sensitivity checks against an earlier endpoint. Argentina and Chile began at 5.13 and 4.81 deaths per 100,000 people in 2019 but recorded changes of −0.64 and +1.54 rate points by 2023. Costa Rica experienced the largest increase within this selected group, although much of its movement occurred in the final year. These findings establish short-period descriptive contrasts, not statistically matched countries or causal explanations. The article also specifies the data that would be needed to test the broader claim that countries similar in homicide rates and structural conditions later follow different trajectories. The distinction between initial resemblance on a single indicator and multidimensional comparison is essential to a defensible interpretation of cross-national homicide statistics.

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INTRODUCTION

Intentional homicide is often used to compare serious violence between countries. Its consequences are difficult to overlook, but the figures do not come from identical reporting systems. A rate of five deaths per 100,000 residents can describe societies with quite different patterns of lethal violence. Differences also emerge over time. Argentina and Chile recorded similar homicide rates in 2019, yet their published rates moved apart by 2023. The figures raise a straightforward question about how much an initial similarity can tell us about what follows. Cross-national research has long associated homicide with demographic conditions and social organization. Those associations do not establish that countries at a similar starting level will experience the same changes in subsequent years (Land et al., 1990).

Homicide trends have been compared for decades. LaFree (2005) examined the convergence of national victimization rates, while Lappi-Seppälä and Lehti (2014) documented the uneven distribution of homicide across regions. The decline observed in several wealthy countries was not reproduced everywhere (Weiss et al., 2016). Baumer and Wolff (2014) also described considerable diversity in longer national series. These findings make uniform trajectories difficult to defend. They leave open a more specific comparison between countries that start with similar observed rates but change at different speeds. A comparison of this kind remains descriptive unless other starting conditions are measured and the cases are selected independently of later outcomes.

Tuttle et al. (2018) identified several homicide trajectory groups among 82 countries observed between 1980 and 2010. Their later study examined change within those groups (Tuttle et al., 2021). A paired comparison involving national religious composition also found that interpretations depended on which countries were paired, with no independent religious effect established (Kadir, 2026a). Both studies establish that national trajectories differ. Countries assigned to separate trajectory classes, however, have already been distinguished by the outcome used for classification. A country-pair comparison faces a related problem if the countries are chosen because their later lines move apart. Initial similarity would need to be assessed independently of those outcomes. The six-country observations used here cannot establish that stronger comparison.

Economic development is frequently associated with homicide variation, although the relationship differs across samples. The institutional-anomie framework places economic activity within a wider institutional setting (Messner & Rosenfeld, 1997). Cross-national studies have considered poverty and inequality alongside other structural conditions (Pridemore, 2008). Ouimet (2012) found that socioeconomic correlates did not operate uniformly across groups of countries. These results provide reasons to consider GDP per capita when forming comparison pairs. They also caution against assuming that two countries are equivalent because a small set of indicators happens to be close. The observed homicide rate describes a population-level outcome. It does not measure the organization of illicit markets, the credibility of public institutions, or the conditions in which individual killings occur.

Urbanization presents a similar difficulty. National urban population shares describe where people live, but they say little about the concentration of violent deaths within specific cities. McCall et al. (2011) demonstrated how urban homicide rates can follow different paths. Peres and Nivette (2017) examined geographic and temporal differences in homicide mortality within Brazil. A comparative account of homicide concentration at micro-places likewise distinguished national burden from the locations where fatal incidents cluster (Kadir, 2026e). A national series may decline while selected localities remain highly violent. The present figures cannot identify those local patterns. They nonetheless provide a useful check on claims that national homicide change follows one common regional direction. A comparison at the country level should remain at that level rather than importing explanations about neighborhoods that were not observed.

International homicide data are not fully interchangeable. Andersson and Kazemian (2018) compared UN and WHO figures, while Kanis et al. (2017) questioned the reliability of estimated rates. Buil-Gil et al. (2026) showed that counting rules can affect national comparisons. Cross-national comparisons of UNODC and mortality-based rates have also documented substantial source disagreement (Kadir, 2026d). Another audit found that a database label alone cannot identify the recording system behind a particular homicide figure (Kadir, 2026j). Similar recorded rates may therefore conceal differences in documentation as well as in the underlying deaths. Small baseline gaps warrant particular care because the recording differences could be larger than the observed gap. The label statistical twin cannot resolve this measurement problem.

The World Bank DataBank table contains consecutive homicide rates for six Latin American countries between 2019 and 2023 (World Bank, 2026). These observations were selected because they could be checked within one published view. They do not form a representative regional sample. Their main use is to describe rates and short-period changes. GDP and urbanization were not included in the observed panel. Any later matched-country study would need those measures for an earlier baseline before assessing the subsequent trajectories. The present comparison therefore has a narrower meaning than the original project title. Similarity concerns only the recorded homicide rate in 2019.

This article asks whether similar observed homicide rates in 2019 were followed by different changes through 2023 within the selected six-country panel. It also examines whether those contrasts are stable when 2022 is used as the terminal observation instead. The analysis is descriptive and avoids claims about why national homicide rates changed. The broader idea of criminological doppelgängers remains a research question rather than an empirical finding. A valid test would require countries to be selected from a complete set according to prespecified baseline variables, with the subsequent homicide outcomes kept separate from that selection. The present results identify what the shorter data series can establish and what remains unresolved.

RESEARCH METHODS

This descriptive comparative study uses publicly available secondary statistics. The source is the World Bank World Development Indicators series for intentional homicides per 100,000 people, derived from UNODC reporting. Six countries with five consecutive published observations for 2019–2023 were retained from the DataBank extract examined on 2 November 2026. The countries are Argentina, Bolivia, Brazil, Chile, Colombia, and Costa Rica. This is an availability-based diagnostic sample rather than a matched or representative sample of Latin America. No interviews, surveys, field observations, or primary-data collection were undertaken.

Annual homicide rates were placed in country-year tables and compared with simple line graphs. Absolute change was calculated by subtracting the 2019 rate from the 2023 rate. Percentage change used the 2019 value as the denominator. Descriptive linear slopes summarized movement across the five observations. A second endpoint comparison, using 2022, assessed whether the apparent direction or magnitude depended strongly on the final year. Initial proximity was described as the absolute difference between countries' homicide rates in 2019. It was not treated as a multidimensional matching score, and no inferential or causal model was estimated.

All reported figures use the published two-decimal rates and are subject to later revisions by the data provider. Differences between national counting systems may affect their comparability (Buil-Gil et al., 2026). The analysis accordingly describes observed rates rather than the underlying causes of homicide. A full statistical-twin study would require separate 2010–2012 observations for homicide, GDP per capita in purchasing-power-parity terms, and urbanization, followed by independently selected pairs and longer subsequent homicide series. Those data were not used to generate the results reported here, so no country is identified as a confirmed structural twin.

RESULTS AND DISCUSSION

Data Provenance and the Boundary of Observation

Six countries have directly checked homicide-rate observations for 2019–2023. The stronger statistical-twin hypothesis requires an earlier baseline covering economic and demographic conditions. Those measures were not retrieved in this analysis. Argentina and Bolivia cannot be called statistical twins merely because they recorded similar rates in one year. Table 1 identifies what was observed and what remains outside the present data. An ecological analysis of recorded homicide in Indonesia similarly distinguished official statistical visibility from individual offending risk (Kadir, 2026f). The analysis therefore concerns one-dimensional starting proximity and short-period changes in homicide rates. It does not establish structural equivalence between the selected countries. This restriction applies even if a pair happens to display strikingly different later changes. The figures cannot replace a missing baseline.

Table 1 Indicators, coverage, and limits of the observed data

Component	Analysed here	Not analysed
Homicide rate	Six countries, 2019–2023	Longer 2010–2023 panel
GDP per capita	No	2010–2012 matching baseline
Urban population share	No	2010–2012 matching baseline
Country selection	Available continuous series	Structural similarity selection
Comparison	Rate change and annual slope	Matched-country divergence
Sensitivity	2022 versus 2023 endpoint	Long-period three-year averages

Note. Six countries were selected from a World Bank DataBank view for descriptive comparison. No structural matching was conducted.

The six countries do not constitute a random sample of Latin America. They were retained because the same published DataBank table allowed their annual rates to be inspected from 2019 through 2023. The findings cannot estimate how common divergent trajectories are across the region. They also do not identify the determinants of national homicide change. Within this small panel, the figures can still establish whether rates rose, fell, or remained relatively stable over the period. The

temporal pattern matters, since a large change in the final year is different from a gradual increase across the full observation window.

GDP per capita and urbanization were part of the initial statistical-twin proposal, but neither variable enters the current calculations. Table 1 makes that distinction visible. Those indicators should be retrieved before an extended country-matching analysis is attempted. A published homicide table alone cannot support claims that two countries have the same socioeconomic conditions. Narrowly defined observations can still be useful. The limitation lies in what the statistics permit us to infer, not in the simplicity of descriptive analysis.

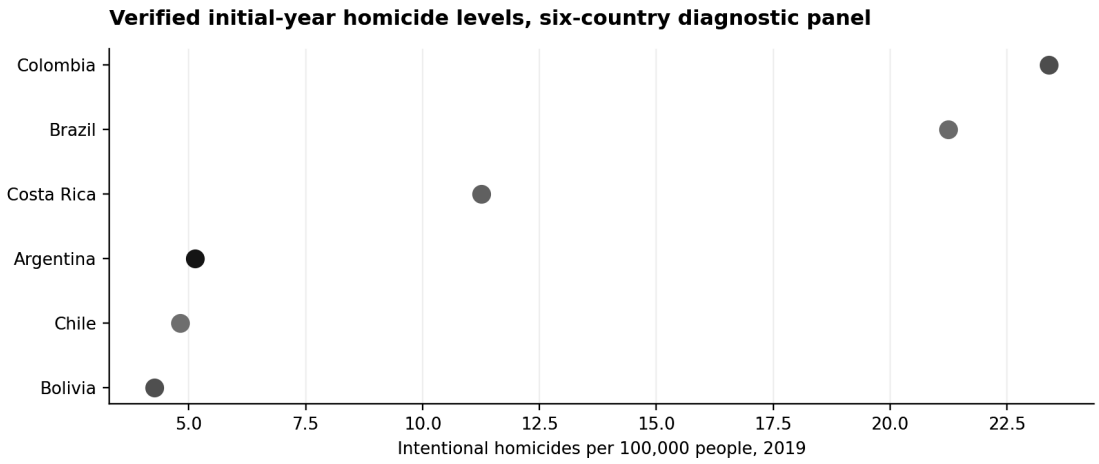
Observed Homicide Trajectories in the Verified Diagnostic Panel

The six countries began 2019 at very different homicide levels. Colombia recorded 23.40 deaths per 100,000 people and Brazil 21.24. Argentina stood at 5.13, Chile at 4.81, and Bolivia at 4.26. Costa Rica occupied an intermediate position at 11.26. Picture 1 displays these values on their observed scale. The plot cannot be treated as a ranking of Latin America because the remaining countries are absent. The initial ordering also says little about subsequent change. A country may have a relatively low rate and still experience a large proportional increase over the following years.

Table 2 Annual intentional homicide rates, 2019–2023

Country	2019	2020	2021	2022	2023
Argentina	5.13	5.35	4.62	4.31	4.49
Bolivia	4.26	3.62	3.50	4.02	4.42
Brazil	21.24	21.81	20.50	20.08	19.28
Chile	4.81	5.76	4.66	6.80	6.35
Colombia	23.40	22.64	25.58	24.95	24.91
Costa Rica	11.26	11.32	11.62	12.91	17.75

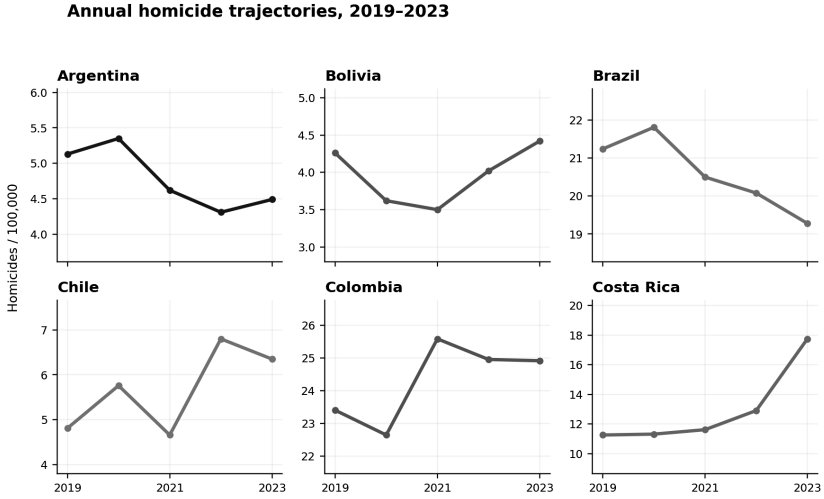
Note. Deaths per 100,000 population. Source — World Bank DataBank, World Development Indicators, displayed table accessed 2 November 2026. The six countries form a diagnostic set, not a matched or representative regional sample.



Picture 1 Initial homicide-rate distribution in the verified diagnostic panel, 2019. This figure does not establish structural similarity.

Annual changes differed across the countries in Picture 2. Brazil changed from 21.24 in 2019 to 19.28 in 2023, with intermediate levels of 21.81, 20.50, and 20.08. Colombia started at 23.40, declined to 22.64 in 2020, increased to 25.58 in 2021, and registered 24.91 in 2023. These two relatively high-rate countries did not exhibit the same shape, although both continued to record substantially higher homicide rates than Argentina or Bolivia in 2023. This illustrates why comparisons limited to two endpoints can obscure turning points. In Colombia, the 2021 observation is higher than both the first and final values. A descriptive label such as increasing does not capture that interruption in its temporal profile. A fuller dataset would make it possible to establish whether such deviations are temporary or part of longer changes in homicide production and recording.

Argentina's rate declined from 5.13 in 2019 to 4.49 in 2023, although it rose briefly in 2020. Bolivia fell from 4.26 to 3.50 by 2021 and returned to 4.42 in 2023. Chile increased from 4.81 to 6.35, after reaching 6.80 in 2022. Thus the three countries ended the short period with different net movements. Their rates do not indicate that one government or policing strategy was more effective. The available figures cannot isolate those explanations from other changes occurring during the same years.



Picture 2 Annual intentional homicide rates in six countries, 2019–2023. Panel-specific vertical scales assist shape comparison and should not be used for direct visual comparison of risk magnitudes.

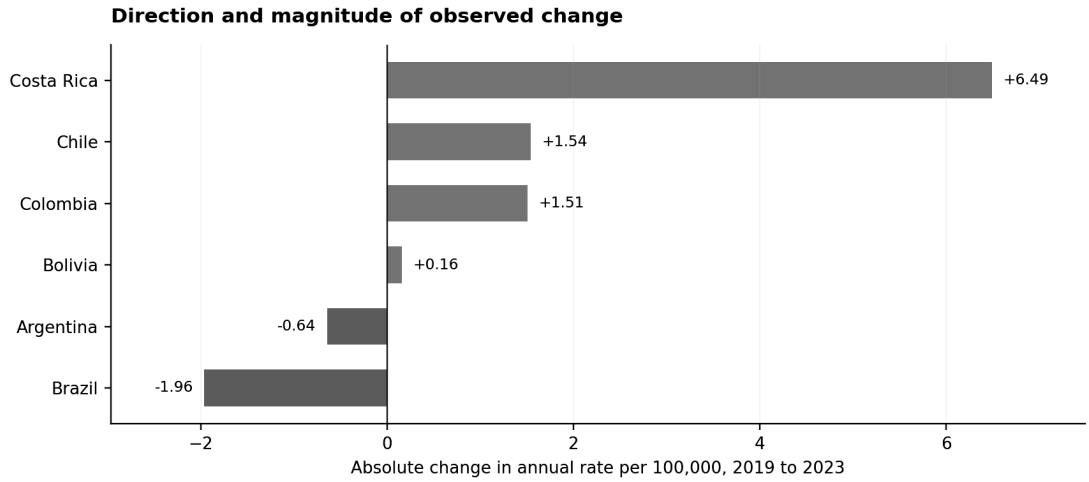
Costa Rica's recorded rate rose from 11.26 in 2019 to 17.75 in 2023, a change of 6.49 deaths per 100,000 people. Much of the increase occurred after 2022. The pattern is visible but the short series cannot establish a lasting structural break. A comparison should also distinguish rate-point changes from percentage changes. An increase of one homicide per 100,000 people represents a larger proportion of a low starting rate than of a high one. Reporting both measures avoids overstating the most visually striking change.

Costa Rica recorded the largest positive endpoint difference in the panel, followed by Chile and Colombia. Argentina and Brazil ended below their 2019 levels, while Bolivia changed little overall. These findings describe five years, not a durable category of countries that are inherently rising or declining. National homicide rates measure the frequency of deaths relative to population, whereas changes in those rates describe movement through time. Comparing the two requires care, particularly where initial levels differ widely (Baumer et al., 2018). Percentage differences are useful as a companion measure, but they answer a different question from the absolute change in deaths per 100,000 residents.

Table 3 Observed changes and descriptive annual slopes, 2019–2023

Country	2019	2023	Δ rate	Δ %	Slope / yr
Argentina	5.13	4.49	-0.64	-12.5%	-0.23
Bolivia	4.26	4.42	+0.16	+3.8%	+0.07
Brazil	21.24	19.28	-1.96	-9.2%	-0.56
Chile	4.81	6.35	+1.54	+32.0%	+0.41
Colombia	23.40	24.91	+1.51	+6.5%	+0.53
Costa Rica	11.26	17.75	+6.49	+57.6%	+1.46

Note. Delta rate is the 2023 rate minus the 2019 rate. Percentage change uses 2019 as denominator. Slope is ordinary least squares on five annual points and is descriptive, not a causal coefficient.

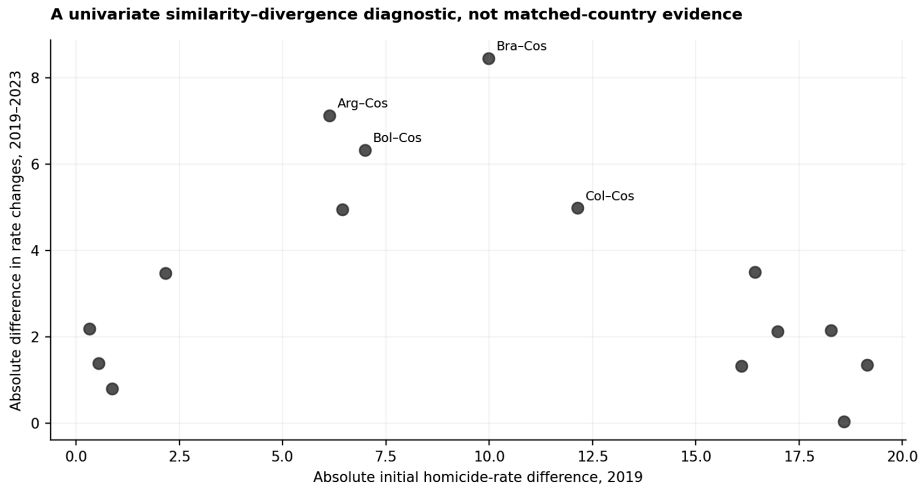


Picture 3 Net changes in annual intentional homicide rates between 2019 and 2023, in rate points per 100,000 population.

A descriptive slope uses all five annual observations, whereas an endpoint difference uses only two years. Neither measure provides a stable estimate of long-term direction from such a short series. A sharp movement in 2023 can influence the fitted line considerably. The study consequently reports annual rates alongside the slopes and checks the sensitivity of endpoint comparisons. It does not use the slopes to test causal hypotheses or make forecasts. The graph remains more informative than a single coefficient where intermediate years contain reversals.

Similarity, Divergence, and the Danger of Post-Outcome Selection

A statistical-twin interpretation would need evidence of initial similarity based on variables chosen before the outcome period. The comparisons in Picture 4 use a more limited approach. Every pair is located according to its 2019 homicide-rate difference and the gap between its subsequent rate changes. The plot identifies pairs whose observed starting levels were close, but it does not measure economic resemblance. It also cannot establish that the apparent differences would remain under a longer baseline. The figure is retained as a descriptive diagnostic rather than evidence that structural twins were found.



Picture 4 Pairwise 2019 homicide-only starting differences and differences in net change through 2023. Country dyads overlap, and no multidimensional baseline matching was carried out.

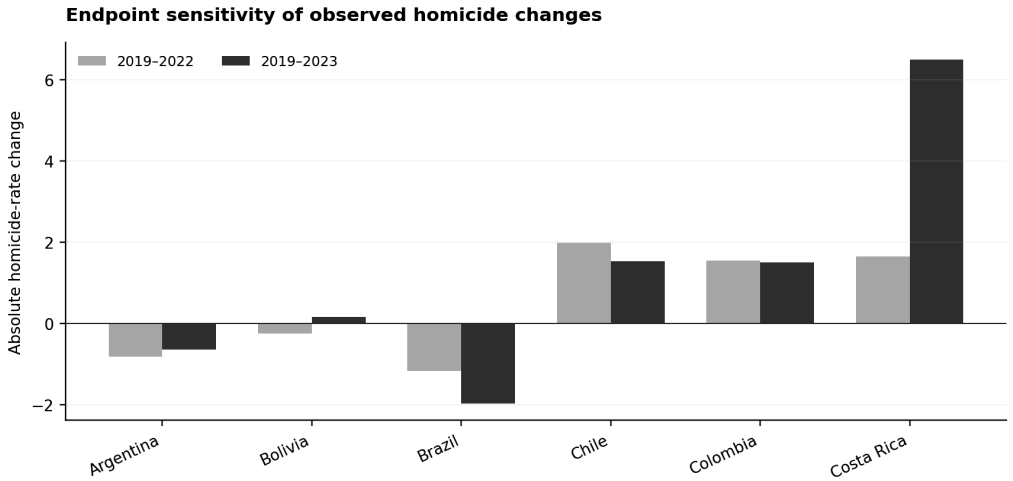
Argentina and Chile illustrate the central descriptive contrast. Their homicide rates differed by 0.32 per 100,000 people in 2019. By 2023, Argentina had declined by 0.64 rate points and Chile had increased by 1.54. The difference in their changes was 2.18 points. Those calculations are valid for the observed years. They do not establish equivalent economic conditions or justify calling the countries structural twins. Selecting this pair after inspecting its contrasting changes would also risk outcome-based selection. A longer design should choose pairs first and examine later rates separately.

Bolivia and Chile also began with relatively close recorded rates. Bolivia moved from 4.26 to 4.42 between 2019 and 2023, while Chile increased from 4.81 to 6.35. Bolivia's series fell before recovering, whereas Chile reached its highest observation in 2022. These intermediate movements matter because the same endpoint difference can arise from quite different annual paths. The word opposite should therefore be reserved for changes with different signs. The broader term divergence can also describe differences in magnitude without assuming that the directions are opposed. These distinctions keep the descriptive categories tied to what was actually observed rather than to an assumed long-run trend.

The fifteen pairwise differences in Picture 4 are not independent observations. Each country appears repeatedly in several dyads, so the plotted comparisons share observations and cannot support conventional independent-observation significance tests. The figure is exploratory and its labels identify illustrative contrasts rather than statistical outliers confirmed by formal inference. An expanded study could adopt non-overlapping pairing or employ dyadic dependence corrections, but neither intervention is necessary for this paper's restricted descriptive purpose. The central reporting requirement is clarity about the unit of analysis. Annual observations belong to countries, whereas similarity and divergence are defined at the country-pair level. Confusing these levels can inflate sample size and understate uncertainty. A transparent descriptive figure with acknowledged dependence is therefore preferable to a poorly justified regression with a larger-looking number of observations.

Robustness, Measurement, and Statistical Interpretation

The choice of endpoint changes the size of several observed differences. This simple exercise is unusually informative in short annual series. Costa Rica displays a considerably smaller net increase when the observation ends in 2022, demonstrating that its final-year value makes a substantial contribution to the five-year difference. Chile's 2022 level exceeds its 2023 level, so the apparent magnitude of its increase is smaller with the later endpoint even though the direction remains positive. Bolivia also illustrates endpoint sensitivity because its gradual recovery after 2021 changes the comparison with 2019. These findings are about the arithmetic of the published series. They do not reveal whether a shift resulted from offending patterns, economic shocks, policing, population estimates, or data recording. Their value is to make a potential source of overinterpretation immediately visible.



Picture 5 Sensitivity of net homicide-rate change to terminal year, comparing 2019–2022 and 2019–2023 windows. The figure uses only the published five-year diagnostic panel.

A planned extension to the full 2010–2023 panel would address endpoint sensitivity by comparing three-year averages rather than individual annual values. If both years' averages preserve a divergence, the conclusion is less dependent on a particular terminal observation. The selection of the averaging interval must be fixed before viewing which definition yields the strongest contrasts. Smoothing does not remove measurement error or establish a mechanism. Temporal clustering may arise from shared exposure rather than the transmission of violence (Kadir, 2026g). It can still prevent one year from dominating a comparison. It also changes the estimand. The difference between two period averages answers a question about typical rates across windows, whereas an endpoint difference describes two particular years. Both are legitimate if their interpretations remain distinct. A manuscript that treats one as a direct substitute for the other risks obscuring the underlying time structure of homicide.

The World Bank reports homicide rates to two decimal places, but that precision does not eliminate uncertainty in national counting practices. Police statistics and mortality registers may not classify every death in the same manner. Buil-Gil et al. (2026) demonstrated the importance of national counting rules. Andersson and Kazemian (2018) and Kanis et al. (2017) also questioned the direct comparability of international homicide sources. A starting gap of 0.32 rate points should therefore not be treated as proof that the countries experienced identical levels of lethal violence. That comparison would need an independent source check.

Reproducibility requires access to the exact data extract, the selection rule, and the calculations. The World Bank homicide view establishes the five observed annual values used here, but it does not supply the missing baseline for the proposed structural matching. GDP per capita and urbanization would also need consistent definitions and an explicit rule for handling unavailable years. Neither measure captures institutional capacity or the internal geography of violence. A provincial study in Indonesia found no consistent association between recorded homicide and the composition of government spending (Kadir, 2026h). Any future claim of national similarity must remain confined to the indicators actually used. The present analysis does not make that stronger claim.

Criminological Meaning and Limits of Comparison

The expression criminological doppelgängers should not be treated as a theory of homicide. It describes a potential comparative design in which measured starting conditions are close and later movements differ. Only one starting indicator is currently available. The national rates also omit information about the organization of lethal violence within each country. An Indonesian review likewise found that aggregate counts conceal distinctions in victim-offender relationships and immediate circumstances (Kadir, 2026i). No findings are presented about the effects of criminal networks or public institutions, since those conditions have not been measured. The available comparison is useful primarily because it exposes the difference between an intriguing paired-country question and evidence sufficient to answer it.

Different homicide trajectories do not refute broad structural explanations. A relationship between inequality or social conditions and homicide may hold across countries even where particular national rates move differently. Institutional-anomie arguments and poverty-related explanations also address different mechanisms (Messner & Rosenfeld, 1997, Pridemore, 2008). A cross-national study of recorded homicide relative to serious assault likewise reported substantial differences in the lethal composition of recorded violence across structural settings (Kadir, 2026b). The six-country series cannot decide among them. Assigning an account of national policy success to one of the observed curves would be speculation without independent evidence about those policies and their timing. The discussion is therefore restricted to the measurable changes and the assumptions behind their comparison.

The six series include declines, increases, and relatively small endpoint movements over five years. Such diversity is consistent with earlier work on international homicide trends (Baumer & Wolff, 2014, Weiss et al., 2016). A separate analysis of homicide persistence after economic contractions also found no uniform global pattern (Kadir, 2026c). It is less clear whether these short movements would persist across a longer observation window. Colombia reached a local peak in 2021, while Costa Rica experienced its largest jump near the end of the series. A five-year comparison can reveal that the paths differ, but it cannot establish a stable long-run national trajectory.

National homicide estimates serve two different purposes in a paired-country design. A global ranking of homicide rates typically treats a country-year estimate as an exchangeable indicator of risk. A baseline pairing procedure goes further because it uses those estimates to determine which countries should be compared. Measurement deviations may therefore influence both the sample construction and the outcome evaluation. This double use makes a robustness strategy particularly valuable. It could compare source-specific matches, report the proportion of stable pairs under alternative indicators, and identify cases whose apparent similarity disappears under small revisions. None of these analyses has been completed here. They define transparent extensions that would improve the evidential status of a future full-data paper. Treating their absence as a recognized limit is preferable to presenting an unverified pair as a striking discovery.

The World Bank observations show different short-period homicide movements in the selected countries. Argentina and Chile began with rates close to five deaths per 100,000 people and ended the period with changes in different directions. This does not establish that the countries were alike in their economic or demographic conditions. A claim about multidimensional statistical twins would require a complete earlier baseline and a rule for choosing pairs before inspecting the later rates. The current figures support the descriptive comparison, while the broader hypothesis remains untested.

CONCLUSION

The six-country World Bank series contains contrasting homicide movements between 2019 and 2023. Argentina's rate fell from 5.13 to 4.49 deaths per 100,000 people, whereas Chile's rate increased from 4.81 to 6.35. Costa Rica's increase was larger, though the sharpest movement appeared in the final year. These are observations about national rates rather than explanations of the killings behind them. Similarity in a single starting homicide rate does not establish comparable social conditions. The findings therefore justify a limited descriptive conclusion and no claim of a completed statistical-twin comparison.

A full test of criminological doppelgängers would require a predetermined set of eligible countries, complete baseline measures for 2010–2012, and homicide outcomes observed after that period. Pair selection would need to occur without using those later outcomes. The present analysis stops short of that test. Its value is in separating a visible descriptive contrast from an explanation that the available data cannot sustain. Future work may use GDP per capita and urbanization to establish measured proximity, but neither indicator would make countries equivalent in all respects. The term statistical twin should be reserved for comparisons whose selection and measurement can be independently reproduced.

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