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Are We Safer, or Just Older? Testing the Demographic Trap Through Cross-National Homicide Rates and Age-Conditional Ranking Instability

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ABSTRAK

This study examined whether older national populations were associated with lower homicide rates and how demographic adjustment affected comparative rankings. A matched sample of 78 countries and territories was assembled for 2023 using intentional homicide rates from World Bank–UNODC sources and old-age dependency ratios from United Nations demographic data. The association was summarized using Spearman correlation. An additional rank-sensitivity exercise compared observed homicide rankings with an age-conditioned descriptive model. Old-age dependency was negatively associated with reported homicide rates, with a Spearman coefficient of −0.599. The conditioned index produced rank concordance of 0.731 with the observed ordering and a median absolute displacement of 10 positions. Rank changes varied substantially with the assumed strength of adjustment. These model outputs were not direct age-standardized mortality rates, and the ecological association did not establish a causal effect of population ageing. Differences between offender age and victim age, together with uneven national reporting practices, further restricted inference. The findings identified a risk of overinterpreting homicide rankings while establishing why population composition, victim-specific mortality, and data provenance needed to be distinguished in comparative research.

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INTRODUCTION

A country with fewer homicide deaths per 100,000 residents is usually described as safer than one with a higher rate. The statement sounds reasonable because the rate records a serious outcome rather than an opinion about crime. It is also incomplete. People are not evenly exposed to lethal violence, and the populations used to calculate national rates do not have the same age structure. A crude rate may fall while the risk faced by a particular age group remains unchanged. In such circumstances, the national decline is real, although its meaning is narrower than the claim often attached to it. Comparisons of homicide have long faced problems of data quality and definition, even where deaths are counted more reliably than other offenses (Andersson & Kazemian, 2018).

The distinction becomes harder to ignore as populations grow older. Fertility decline and improvements in survival change the proportions of people in successive age groups. If homicide victimization differs by age, those shifting proportions can affect the national average without making any age group less exposed. A population can also become substantially older while lethal risk increases among some young adults. The familiar age–crime curve offers limited help here. It concerns patterns of offending, whereas a homicide mortality rate counts the age of people killed. Treating these as equivalent obscures the very problem that demographic comparison needs to resolve (Hirschi & Gottfredson, 1983).

A strong demographic explanation would predict a reasonably stable link between age structure and homicide across settings. The evidence is less accommodating. Gartner and Parker (1990) examined five national time series and found no consistent effect of youth population share. Pampel and Gartner (1995) later argued that collective social protection changes the relationship. Phillips (2006) found an association across United States counties, but its strength varied with local socioeconomic conditions. The findings point to conditional relationships rather than a general law of ageing and safety. A demographic transition may coincide with fewer killings in one setting and little change in another. Population composition cannot, by itself, explain the social circumstances that produce lethal violence.

The disagreement becomes harder to ignore in Rennó Santos et al. (2019). Their analysis of 126 countries since 1990 found no consistent relationship between age structure and homicide. A longer series covering 26 countries since 1960 produced a different result, linking changes in the youth population share to homicide trends. They raise a question about how samples and periods affect an explanation, but also a more basic issue. A demographic shift can change the average rate arithmetically without establishing that the shift caused the risk within age groups to change. The two claims use different forms of evidence and should not be presented as interchangeable.

The numerator can be as troublesome as the population denominator. Police records depend on offense definitions and the point at which an event enters the criminal justice system. Death registration relies on the medical classification of an underlying cause of death. An incident may be known to both systems and still appear in different categories or periods. Buil-Gil et al. (2026) examined 41 European countries and documented the effect of counting rules on the correspondence between police and mortality records. Age standardization cannot solve this discrepancy.

Recent research has made both dimensions more visible. Robey and Vogel (2026) show that different ways of reconstructing homicide rates for birth cohorts can yield substantially different estimates. Work on American counties reports considerable differences in homicide mortality by age and sex (GBD US Health Disparities Collaborators, 2025). Cross-national studies of the age–crime curve likewise question the idea of a single adolescent peak across societies (Steffensmeier et al., 2025). These advances address related questions, but they do not authorize one simple conclusion about national safety.

A national homicide ranking can be arithmetically correct and still invite a weak claim about safety. The relation between age composition and a crude rate is familiar to demographers. The problems of inconsistent homicide recording are familiar to comparative criminologists. Less attention has been given to how these established observations should constrain an ordinary claim that one country is safer than another. The proposed term demographic trap refers to that particular error. It occurs when a difference in aggregate mortality is treated as evidence of lower risk for comparable people without inspecting the population weights and the way deaths were counted.

A sharper empirical question is whether older populations appear safer in the available cross-national data and how much a ranking changes when an explicitly stated demographic adjustment is imposed. This study uses 2023 data on recorded intentional homicide and old-age dependency to examine those issues without claiming to conduct direct age standardization. The distinction is decisive. A country-level demographic covariate cannot reconstruct a set of victim-age-specific death rates. We first examine the observed association and the distribution of homicide across age-structure quartiles. A rank stress test then asks how the order of countries responds to a deliberately simple adjustment model and to changes in its strength. A stable association would not identify a demographic cause of violence. Instability in an age-conditioned ranking would not prove that a properly standardized WHO ranking would behave similarly. Both outcomes are valuable precisely because they narrow what can be inferred from comparative statistics.

RESEARCH METHODS

The study uses a quantitative descriptive-comparative design based entirely on published secondary information. The unit of analysis is a country or territory in 2023. Intentional homicide rates per 100,000 residents were transcribed from the year-specific World Bank and UNODC ranking for 2023. No value from another year was substituted for a missing observation. The demographic indicator is the number of residents aged 65 or older per 100 residents aged 15–64, drawn from the UN World Population Prospects 2024 data as disseminated by Our World in Data. It is an old-age dependency ratio, not the percentage of the whole population aged 65 or above. After matching names and excluding

observations for which both published values could not be independently captured, the analytical table contains 78 matched cases from the 83 jurisdictions shown with homicide records for 2023. The set is a data-availability sample rather than a random representation of all states.

The observed association was described using Spearman rank correlation and a simple least-squares line fitted to the natural logarithm of the homicide rate plus 0.1. Adding 0.1 avoids undefined logarithms at very low rates and is not a substantive criminological threshold. The slope of the fitted line measures the cross-sectional association between old-age dependency and log homicide, without any claim that the coefficient is causal. Homicide distributions were also compared across four old-age dependency quartiles. Counts, medians, and scatterplots were used to retain the large differences among countries within each demographic group. All quantitative summaries refer to the matched sample and use 2023 measurements only.

A separate sensitivity exercise constructed an age-conditioned index by subtracting the fitted demographic component from each logged homicide rate and centering it at the sample median old-age dependency ratio. The exponential transformation returns that index to the original numeric scale. This procedure is statistical residualization. It does not estimate an age-standardized homicide mortality rate and cannot replace direct standardization using age-specific deaths. Rankings were calculated from the published homicide rates and from the conditional index using descending order, with first rank assigned to the highest rate. Absolute rank displacement and Spearman concordance were computed. Adjustment strength was then varied from zero to one and a half times the descriptive slope to check whether the resulting conclusion depended on an arbitrary coefficient choice.

The article also draws on peer-reviewed research to interpret the numbers. Existing work on the age–crime relationship was separated from research on victim-age homicide mortality. Earlier cross-national studies, measurement research, and UNODC and WHO documentation inform the discussion of possible confounding and data quality. Neither the fitted conditional index nor the resulting positions are presented as actual counterfactual homicide rates. There is no new collection of individual-level data, no interview, and no attempt to identify policy effects. Data were transcribed from the linked public tables, calculations were performed with Python, and the matched country table is retained as a reproducibility supplement.

Table 1 Data architecture and scope of the secondary-data comparison

Dimension	Definition	Coverage
Homicide rate	UNODC intentional homicide per 100,000, as distributed by World Bank	2023, 83 available source cases
Demographic indicator	Old-age dependency, ages 65+ divided by ages 15–64, per 100	UN WPP 2024, year 2023
Main analytical set	Matched cases with both figures independently captured	78 jurisdictions
Descriptive tests	Spearman association and homicide quartiles	Cross-sectional, 2023
Sensitivity exercise	Fitted age-conditioned index and rank displacement	Not direct age standardization

Note. Public data provenance is reported in References. No mortality microdata or age-specific homicide series were reconstructed.

RESULTS AND DISCUSSION

3.1. Demographic Ageing and the Observed Homicide Gradient

Homicide mortality and homicide offending have different age profiles because they count different people. Hirschi and Gottfredson (1983) argued that criminal involvement peaks early in life, while Steffensmeier et al. (1989) documented differences across recorded offense types. Neither finding establishes the age of the people who die in homicide incidents. A young offender can kill an older victim. The offender enters an arrest or suspect series, while the victim enters a death series. The distinction is elementary but consequential. If the purpose is to examine whether population ageing changes a national homicide mortality rate, the age weights must apply to people at risk of being killed. Offending data address a separate question about the people who commit the violence.

More recent international work has strengthened that objection further. Steffensmeier et al. (2025) examined international and historical patterns and found substantial variation in the ages at which offending peaks. Steffensmeier and Schwartz (2025) further question explanations that assign the curve chiefly to a common timetable of brain development. A wider distribution of adult offending is not an exception that can simply be ignored in cross-national comparison. It challenges the assumption that fewer adolescents must produce fewer lethal offenses everywhere. Yet none of these studies directly measures whether older national populations experience lower homicide victimization. Their relevance is to caution against overconfident explanations, not to supply the missing victim data across national populations.

United States trends illustrate the danger of carrying results across measures. Tuttle (2024) reported changes in the historical age profile of male arrests. In a subsequent homicide study, Tuttle (2026) found that the age pattern of people accused of homicide differed across racial and ethnic groups. These are findings about recorded involvement. They are useful for questioning a universal offending pattern, but they cannot be inserted into the denominator of a victim mortality rate. Age composition must be linked to the outcome being measured. The limitation grows sharper in case-based homicide research. Offender profiles drawn from cleared investigations may exclude deaths for which no suspect was identified, as Kadir (2026a) cautions.

Earlier studies of population structure also resist a mechanical explanation. Gartner and Parker (1990) found that the share of younger residents did not have a stable relationship with homicide across five countries. The cross-national analysis by Pampel and Gartner (1995) associated the relationship with institutional protection. Local circumstances mattered in Phillips's (2006) study of United States counties as well. The same demographic shift can occur under very different arrangements of social protection and exposure to violence. Age composition has an arithmetic role in a population rate, but the level of risk within an age group remains a criminological question. The question is therefore not whether population ageing occurs, but how particular risks are distributed.

The 78 matched observations span widely different demographic conditions and levels of lethal violence. Old-age dependency ranges from 5.5 in Nigeria to 50.1 in Japan, while the published homicide values range from 0.07 per 100,000 in Singapore to 64.16 in Saint Kitts and Nevis. The scale alone makes a simple linear graph difficult to read. Figure 1 therefore uses a logarithmic-like vertical scale while retaining a direct label for the homicide rate. The fitted curve has a negative slope, but several cases sit far from it. That dispersion matters more than the apparent smoothness of the line. The same demographic ratio is compatible with very different levels of lethal violence, which rules out the use of the bivariate fit as a general theory of homicide.

The Spearman coefficient is −0.599 for the matched sample. Its conventional significance level is below 0.001, although sampling-based inference is not the principal aim here because these observations are an availability-selected set of countries. The fitted coefficient for the logarithm of the homicide rate plus 0.1 is −0.0827 for a one-point difference in old-age dependency. The strong negative association is consistent with the possibility of a demographic contribution, but it is also consistent with unmeasured differences in economic development, criminal markets, firearms, institutional recording, and the distribution of violence. None of these alternatives is removed by a two-variable cross-section. Treating the slope as a causal effect would be a serious inferential error.

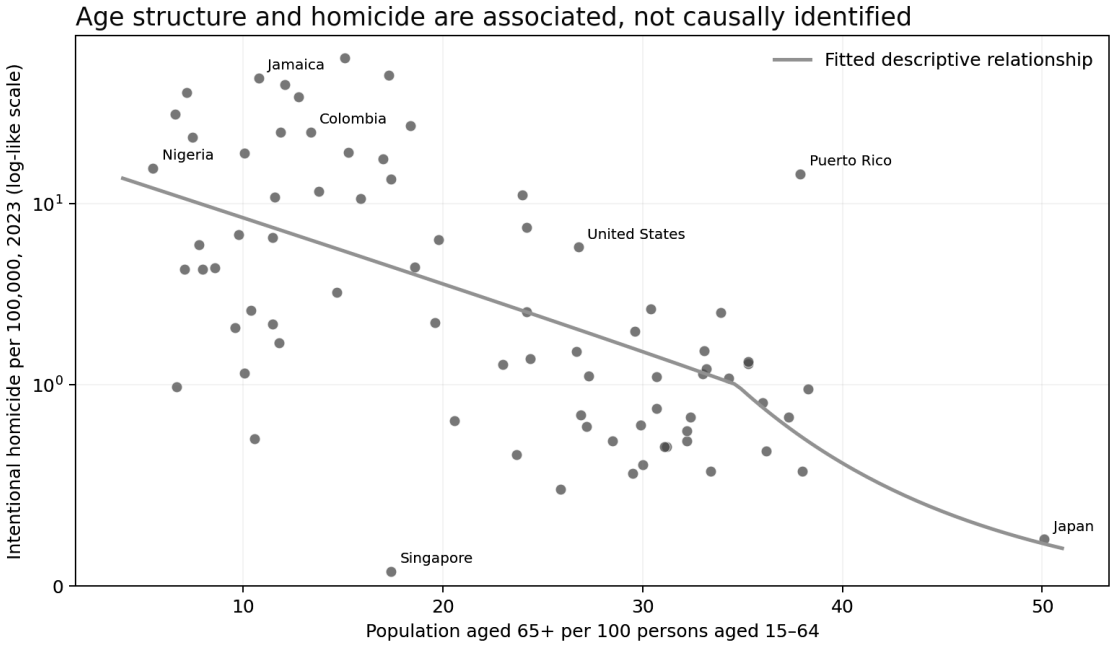


Figure 1 Old-age dependency and observed homicide rates in 78 matched countries and territories

Note. UNODC and World Bank homicide rates and UN WPP demographic values, 2023. The fitted relationship is descriptive.

Four age-structure groups provide another view of the data. The median homicide rate is 5.17 in the youngest-dependency quartile and 0.875 in the oldest quartile. Yet the second quartile has the highest median, 13.67, with extreme cases concentrated in a number of Latin American and Caribbean jurisdictions. This nonmonotonic group result is a warning against treating the age gradient as a universal ordering rule. Mean homicide values are 11.75, 19.40, 2.27, and 1.66 across successive age-dependency quartiles. The distribution, rather than a regression coefficient alone, reveals the combination of geographic clustering and within-group variation.

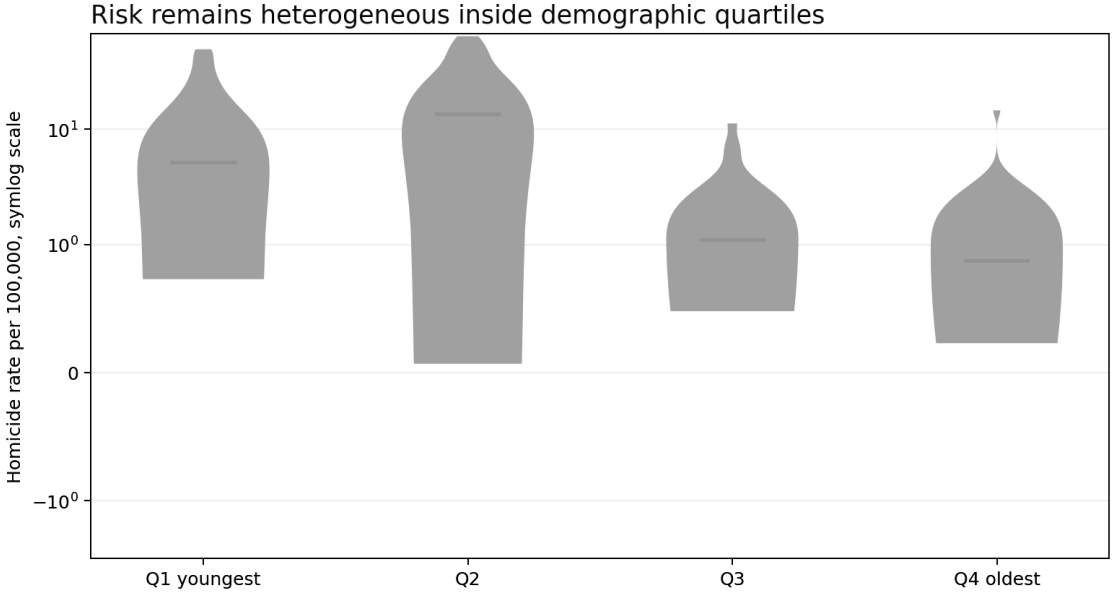


Figure 2 Homicide distributions across four old-age dependency quartiles

Note. Violin shapes display within-quartile dispersion. Horizontal gold markers indicate median rates. Quartile sizes are 20, 19, 19, and 20 cases.

A national homicide decline can therefore emerge through different processes. Lethal risk might fall among people of every age. It might fall only for some age groups, while others experience no

improvement. The share of people concentrated in high-risk ages might also shrink. These are different developments with the same possible effect on a crude rate. They should not be merged into a single story of successful prevention, or into the opposite story that ageing alone produced the decline. That distinction follows from the construction of the rate, not from an estimated causal model. Kadir (2026d) makes a related distinction in a comparison of recorded homicide and serious assault. The lethal share of recorded violence is an aggregate measure, not the probability that a particular assault ends in death.

Rennó Santos et al. (2019) provide a useful test of intellectual restraint. Their 126-country analysis and longer 26-country series reached different conclusions about the importance of age composition. The authors also found that the demographic association was less evident where other risk factors for violence were more prominent. That qualification matters. Social arrangements can change the risks attached to particular ages, while a change in population weights can alter the overall mean. Neither process cancels the other. A serious interpretation must allow them to coexist, even when that makes the explanation less tidy.

Victim-based studies show why the issue cannot be settled through offender age. Friche et al. (2023) found marked variation in youth and young adult homicide rates across Latin American cities. Canudas-Romo and Aburto (2019) documented substantial survival losses from homicide in Latin America and the Caribbean. The United States study by the GBD US Health Disparities Collaborators (2025) also identified major differences in homicide mortality across age groups and places. These findings concern people killed, rather than a general theory of youthful offending. They offer the type of evidence that demographic interpretation actually needs. An Indonesian documentary study identifies another limitation. Victim–offender relationships and the immediate circumstances of killings are often absent from aggregate homicide totals (Kadir, 2026i).

The crude homicide rate can be written as a population-weighted average of victim-age rates. Each age group contributes according to its share of residents and its recorded risk of homicide death. A shift in the shares can change the overall rate even if every age-specific rate stays fixed. The effect may be small or substantial, depending on how risk is distributed. This identity differs from the descriptive country-level association in Figure 1. The fitted association cannot identify the contribution of age structure to any particular national homicide decline.

The arithmetic relation also guards against an ecological mistake. A country with many older residents does not reveal the individual probability that an older person will kill or be killed. O'Brien (1989) separated cohort size from age-specific offending, while Levitt (1999) found that changing age structure had a limited role in the change in aggregate crime rates in the United States. Their outcomes are not identical to homicide mortality, but the analytical warning remains useful. A national percentage cannot replace an observation about risk within the relevant population group. The inference needs age-specific information that a crude aggregate has already concealed. This is particularly relevant when the outcome is homicide mortality rather than a general arrest series.

The present results cannot adjudicate the distinction between offender age and victim age. The outcome is recorded intentional homicide per 100,000 people, and the independent indicator concerns the overall population. Neither series describes the age of the victim in an incident. A lower aggregate homicide rate in an older country could arise from a demographic composition effect, lower age-specific victimization risks, or other conditions that covary with demographic transition. The observed negative association establishes no more than a patterned cross-sectional difference. It may be a useful reason to seek age-specific mortality data, but it cannot substitute for them. The tendency to promote this association into an explanation would reproduce the very error that motivates the article.

3.2. What the Observed Homicide Ranking Actually Measures

A comparative rate depends on the deaths entering its numerator and on the residents entering its denominator. Problems on either side can distort the meaning of a country ranking. Bisogno et al. (2015) explained why an international crime classification is needed, while Andersson and Kazemian (2018) documented limits in the consistency of international homicide figures. These are not minor technical qualifications. If countries count different events, the same label and population denominator do not establish that the indicators are equivalent. National classification also determines which deaths qualify as homicide victims in the first place, a boundary examined in a comparative legal study by Kadir (2026h).

Buil-Gil et al. (2026) compared police homicide records with mortality sources across 41 European countries. The association between the records varied with legal definitions and national counting practices. Some differences concern the stage at which an incident is recorded. Others arise from how a death is classified. The finding challenges the claim that homicide is automatically a fully comparable crime simply because a victim has died. The death may be known while its treatment in an official series differs. A demographic correction applied to such data can produce a precise-looking comparison of unlike quantities. The provenance problem can persist under a shared database label. Kadir (2026j) found that observations reported to UNODC may originate in criminal justice or public-health systems.

A crude rate is not inferior in every setting. It describes homicide mortality for the population that actually lives in the country. This is useful when the question concerns the current aggregate burden of death. An age-standardized rate serves a different purpose by comparing rates under a common age distribution. It is more informative when population composition is suspected of shaping a difference between countries. The choice depends on the question. Treating the adjusted rate as the real number and the crude rate as an illusion would reproduce the same error in another form. Both rates are meaningful if the question they answer is clear and the underlying deaths have been counted consistently. Prison homicide offers a concrete illustration of why the denominator matters. Kadir (2026g) compared custodial death rates using prisoner populations and distinguished reported zero deaths from missing observations.

The reference population also deserves attention. Different standard age distributions can produce different standardized rates when the observed age-specific patterns vary. Standardization removes one source of compositional difference by construction, but it does not repair missing deaths or different homicide definitions. Nor does it hold sex composition or local exposure conditions constant. The procedure is valuable because its assumptions are explicit, not because it makes every other difference disappear. Those limits matter.

Table 2 Quantitative distribution and model diagnostics in the matched sample

Measure	Observed result	Interpretation
Matched country and territory observations	78	Availability sample with 2023 matched indicators
Spearman age-dependency vs homicide	-0.599	Descriptive ecological correlation
Log-rate fit slope	-0.0827	Not a demographic causal effect
Median absolute conditional rank shift	10 ranks	Model-dependent displacement
Spearman observed vs conditional ranks	0.731	Concordance, not validation of an adjusted rate
Countries shifting at least ten places	42 of 78	Under full fitted-strength assumption

Note. Rates are per 100,000 people. Figures are reproducible from the accompanying matched data. The adjusted index is not an age-standardized homicide rate.

Homicide statistics come from institutions with different purposes. Police data classify suspected offenses through criminal procedures. Vital registration records causes of death. Their figures may diverge even where the underlying incident is the same. Cook (2025) argues that evidence about homicide is fragmented across systems of knowledge production. Kivivuori et al. (2024) show the analytical value of incident-level detail when comparing homicide in Finland and a South African setting. Both studies support examining the provenance of a number rather than treating its publication by an official agency as sufficient proof of equivalence. In a comparison of UNODC and WHO figures from 81 countries, Kadir (2026e) found strong correlation but weaker numerical agreement. Neither source was treated as an unquestioned account of the true number of killings.

International categories can reduce some of the confusion. The International Classification of Crime for Statistical Purposes creates a common frame for recording intentional homicide (Bisogno et al., 2015). Yet a shared definition does not guarantee identical implementation or complete reporting.

A mortality series may exclude deaths awaiting classification, while a police series can change after investigation. Comparing rates from different observation periods makes the problem worse. Statistical consistency requires knowing which events entered a dataset and when they did so. An Indonesian mapping study likewise distinguishes national homicide totals from police-region and village-based measures, which cover different administrative units (Kadir, 2026f).

The evidentiary reach of a homicide indicator depends on the population and event it actually measures. The distinction between rates is methodological, but its consequence is substantive. A government may accurately report fewer homicide deaths per resident and still lack evidence that the risk faced by comparable age groups has fallen. Conversely, an adjustment that changes a country's position in a ranking does not prove that ageing produced the difference. Each measure answers a particular question. The argument begins to fail once one answer is used as evidence for another.

The difference between legitimate demographic adjustment and residual ranking requires careful language. WHO age-standardized mortality compares deaths within the same age groups and combines those rates using a fixed population distribution. Our data do not contain the necessary country-by-age homicide rates. We instead estimate a conditional index using one aggregate demographic covariate. The coefficient captures every relevant and irrelevant national characteristic that happens to correlate with old-age dependency in this sample. The exercise can assess the sensitivity of comparative ranks to a stated demographic model. It cannot measure the share of homicide attributable to demography or tell us the correct standardized position of Japan, Brazil, or any other country.

A comparison of homicide rates should not rely on a single country's statistical profile. Singapore has a very low published rate while Japan combines a low homicide rate with extremely high old-age dependency. Puerto Rico is older than many jurisdictions in the sample and nevertheless has a substantially higher homicide rate. These cases establish that the demographic ordering is neither deterministic nor a substitute for criminological explanation. The broader association can arise alongside national exceptions, although such exceptions are not themselves proof that age composition is irrelevant. The proper question is how much of the observable contrast survives a consistent examination of victim-age mortality, which this source pair cannot answer.

3.3. Age-Conditional Rank Instability and the Demographic Trap

The conditional exercise asks what happens to an ordering when the observed homicide rates are expressed relative to the sample-wide association with old-age dependency. The index is centered at the sample median age-dependency ratio of 23.35. The fitted coefficient therefore increases the conditional value of an older country and decreases the value of a younger one. That change follows directly from the negative empirical slope. Its presence is not evidence that such countries would truly exchange places under WHO age standardization. The distinction is most visible in large rank movements. Japan moves from 77th to 33rd under this specific index, whereas Pakistan moves from 31st to 66th. Those are consequences of the model and should be interpreted as stress-test outcomes.

Figure 3 identifies the most displaced cases. Japan, Italy, Finland, and Croatia move toward the higher-homicide end of the model-conditioned ordering, while several younger countries move in the opposite direction. The graph is deliberately shown beside an explicit warning that the conditional values are not actual age-standardized rates. Its role is diagnostic. A model that uses the observed cross-country age gradient to redraw the ranking can create dramatic movements even if the underlying association is substantially confounded. The exercise is therefore an argument for more stringent data requirements, rather than evidence for a radically revised list of the world's safest countries.

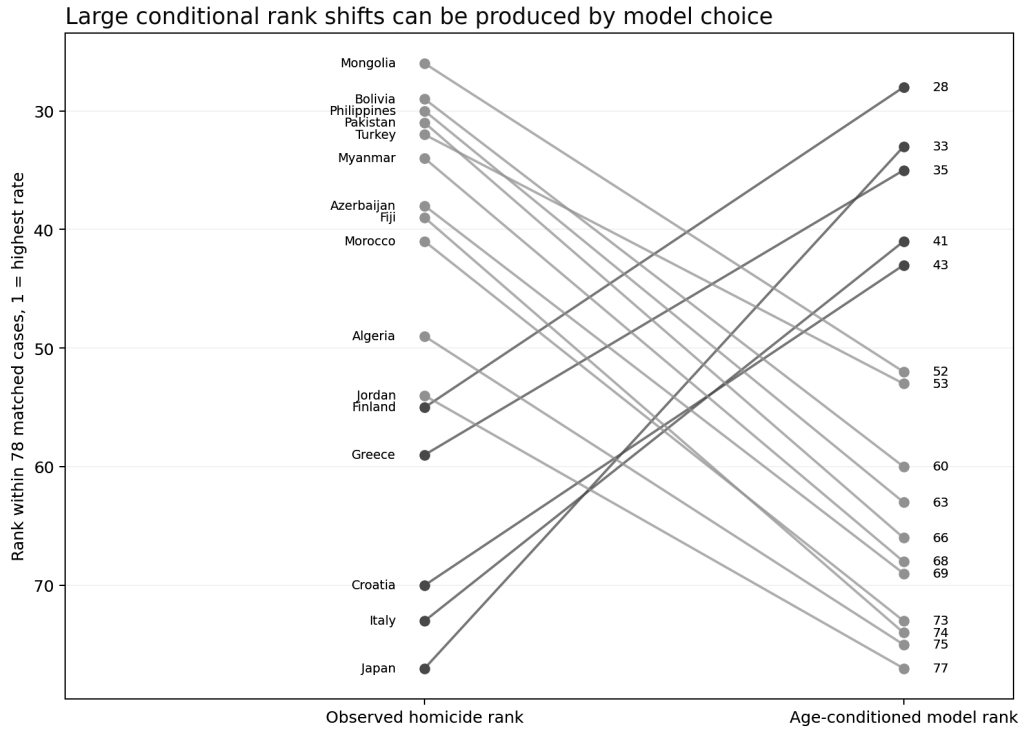


Figure 3 Largest observed-to-conditioned homicide rank displacements

Note. The 16 cases with the greatest absolute rank displacement are shown. The conditioned positions are model-based, not WHO age-standardized rankings.

Table 3 Largest absolute conditional ranking shifts under the fitted index

Country	Crude rate	Old-age ratio	Observed rank	Model rank	Change
Japan	0.23	50.1	77	33	-44
Fiji	2.06	9.6	39	74	+35
Pakistan	4.33	7.1	31	66	+35
Myanmar	2.58	10.4	34	68	+34
Philippines	4.35	8.0	30	63	+33
Italy	0.57	38.0	73	41	-32
Morocco	1.71	11.8	41	73	+32
Bolivia	4.42	8.6	29	60	+31
Azerbaijan	2.16	11.5	38	69	+31
Finland	0.98	38.3	55	28	-27
Croatia	0.67	36.2	70	43	-27
Mongolia	5.92	7.8	26	52	+26

Note. Rankings order the 78 sampled rates from highest to lowest. A negative change moves a country toward higher modeled homicide rank. Model rank is not a directly standardized mortality estimate.

The aggregate results also require restraint. Rank concordance between the observed rates and the fitted conditional index is 0.731. The median absolute displacement is 10 positions and 42 of the 78 countries move at least ten places. These values quantify the fragility of a single fitted ordering, not the impact of biological ageing. If a reviewer interpreted them as the consequences of direct age standardization, the analysis would be mislabeled. They have a different use. They reveal how readily a persuasive story about a demographic correction can be manufactured through an ecological slope unless the source of adjustment is fully specified. The statistical result is real, while the causal meaning attached to it remains unestablished.

Figure 4 compares the published and conditional values among a subset of jurisdictions with high homicide rates. The figure makes visible that the exercise shifts a numerical index, not the underlying

observed mortality. The values cannot be treated as corrected country rates for policy evaluation. That caveat matters because a reduction in the conditioned value of a young population might be mistaken for evidence that the country's actual homicide risk has been overstated. The index is an answer to a model-based question. It is not a measurement of how many deaths would have occurred if the population had a different age distribution. Such a claim would require age-specific fatal victimization data.

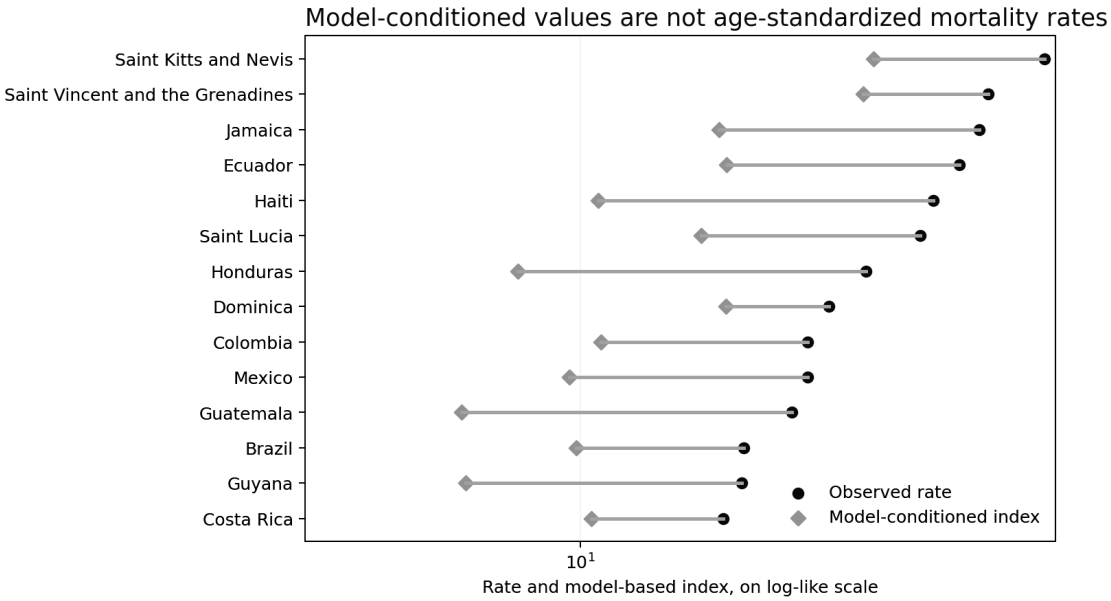


Figure 4 Observed homicide rates and conditional-model indices for high-rate cases

Note. These modeled values are counterfactual only in a statistical descriptive sense. They are not estimates of deaths under another population age distribution.

Rank sensitivity changes considerably with the strength imposed on the adjustment. At half the fitted demographic coefficient, Spearman concordance between observed and conditional ranks is approximately 0.937 and only 16 countries move at least ten places. Using the full fitted slope reduces concordance to 0.731 and raises that count to 42. Increasing the slope by half again brings the correlation down to about 0.474 and the count of large shifts to 57. The substantive conclusion therefore depends on an analytical choice. A model that is less aggressive produces rankings much closer to the observed data, and an amplified model produces a more spectacular picture of instability. Neither choice is uniquely justified by age structure alone.

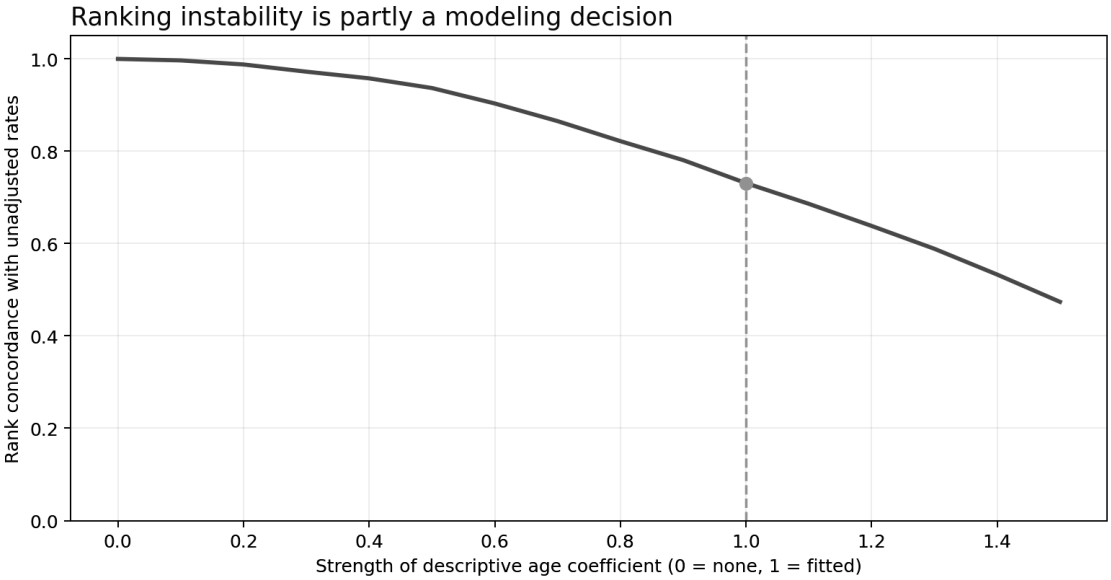


Figure 5 Ranking concordance under alternative demographic adjustment strengths

Note. Adjustment strength ranges from none to 1.5 times the fitted coefficient. This is a model sensitivity analysis, not an age-standardization comparison.

The demographic trap refers to an interpretive error that can arise from an otherwise legitimate statistic. Country A may have a lower crude homicide rate than country B. If both sources count comparable deaths over the same period, it is reasonable to say that residents of A experienced fewer recorded homicide deaths per person. It is not yet reasonable to say that a person of the same age faces less lethal risk in A. That second judgment requires age-specific evidence. The national average has already combined different groups, and their weights may not be the same across countries. It is possible for the lower national average to coexist with a higher risk among younger residents, which is why the aggregate difference alone is not enough. A paired-country comparison found that Muslim-majority status did not provide a stable explanation of recorded homicide across the selected cases (Kadir, 2026c). The lesson concerns the limits of national inference, not an effect of religion on age-specific mortality.

The first difficulty is deciding what the homicide number measures. Police counts and mortality data cannot be exchanged casually. Buil-Gil et al. (2026) show that recording practices affect their correspondence in European comparisons. The time period matters as well. A recent annual rate from one country is not directly equivalent to a modeled multi-year estimate from another. Unless the events and periods are sufficiently comparable, any later adjustment for age operates on uncertain ground. The quartile comparison in Figure 2 cannot resolve these differences in the classification of deaths. A clearer denominator will not make incompatible deaths equivalent.

The age of the person killed is central to an analysis of homicide mortality. The young age profile of some offenders does not show that younger victims account for the same share of deaths. Tuttle's (2026) findings on the ages of recorded homicide suspects illustrate variation in offending, not a universal distribution of deaths. Kivivuori et al. (2024) describe differing incident patterns across two national settings. Such variation makes it even harder to infer victim exposure from an offender-age curve. The relevant groups must be identified before an age comparison is interpreted. Kadir (2026b) also identifies differences in how national legal systems define intentional homicide. That doctrinal comparison cannot supply victim-age rates, but it cautions against treating offence labels as equivalent across jurisdictions.

Age standardization becomes useful once comparable age-specific rates are available. It answers whether the national contrast persists when both countries are assessed under the same age weights. If a ranking changes, the original contrast was sensitive to population composition. If it remains stable, the comparison is less sensitive under that particular standard. Neither outcome establishes why the countries differed historically. A standardized ranking is a comparison under stated assumptions, not a natural experiment in demographic change. The choice of standard population should be stated, since it is part of how the adjusted rate is constructed.

Comparative claims should also remain sensitive to sex and the circumstances of death. Dunaiski et al. (2024) estimate the burden of killings of women and girls by intimate partners or family members. Those deaths cannot be adequately characterized through a generic assumption about young male offenders. The composition of homicide varies across places and types of relationship. A single national age distribution may be relevant, but it is not sufficient to explain the forms of violence represented by the aggregate rate. This difference in the types of lethal violence matters for the ages at which victims are exposed to risk.

Age is therefore not an alternative explanation that makes social conditions irrelevant. Nivette (2011) found that the evidence on national homicide predictors was sensitive to research design and sample composition. Rivera (2016) examined the role of broader social conditions in Latin American homicide. These studies do not supply a fixed adjustment that can be added after age standardization. They show why exposure to lethal violence differs within age groups. Demography describes the number of people who occupy those groups. Social processes help explain the risks they face.

Claims about crime control inherit the same difficulty. A decline in homicide may coincide with a new policy, altered local conditions, and a changing population. Cook and Laub (2002) situated the American decline in youth violence within broader historical conditions. LaFree et al. (2015) studied declines beyond one national setting, while Weiss et al. (2016) questioned whether the 1990s trend was confined to Western countries. These studies discourage attributing a national change to one factor

merely because the events occurred together. Ageing is neither a universal competing cause nor a reason to dismiss crime prevention.

The demographic trap can also work in the opposite direction. A country with a comparatively young population may have a high crude homicide rate partly because more residents fall into groups with higher victimization rates. That possibility does not make the deaths less serious or establish that the country would become safe through ageing. Its age-specific risks may remain high. Using an older country as the sole benchmark could exaggerate what the difference says about institutions or prevention. A comparison based on age-specific mortality would answer a narrower and potentially fairer question.

Comparative safety claims need limits that match the evidence used to support them. Researchers must check the provenance of recorded deaths and the available age-specific information before describing a country as safer. This is not a new statistical estimator. It is a discipline for avoiding an interpretive leap that crude rates invite. Where comparable age-specific data are unavailable, a responsible conclusion may be limited to the aggregate homicide burden rather than a ranking of underlying safety.

Our empirical findings sharpen rather than dissolve the original theoretical distinction. The national homicide rate is a population average, and the observed association with age structure is substantial in the matched 2023 sample. But a crude cross-country regression cannot allocate homicide differences between compositional exposure and age-specific victimization. Other explanations remain open, particularly where organized crime and different forms of recording dominate national patterns. The World Bank homicide measure is also an administrative indicator that may combine national police and health inputs. Its comparability is not secured by matching a calendar year. The value of the present exercise lies in demonstrating how statistical adjustment choices alter a ranking and how easily such changes can be given a causal interpretation they have not earned.

A direct standardization study should use a matched set of victim-age homicide mortality rates and population age weights drawn from the same observation years. Its reference population must be identified in advance. Countries whose underlying mortality information is too incomplete to support age-specific estimates should be distinguished from those with high-quality vital registration. The results should be accompanied by sensitivity tests for small populations, uncertain death classification, and the age bands used in standardization. Only then can a numerical ranking shift be interpreted as the effect of replacing each country's age composition with a common distribution. Our present figures should not be cited as if that more demanding exercise had already been undertaken.

CONCLUSION

Older populations in the matched 2023 data tend to have lower reported rates of intentional homicide. That association is clear, but its causal meaning is not. The analysis covers 78 countries and territories and finds a Spearman coefficient of −0.599 between old-age dependency and recorded homicide. The second empirical result is more conditional. A rank index formed from the fitted demographic relationship displaces many countries substantially, although the size of those movements changes with the strength assigned to the adjustment. The exercise confirms that some cross-national comparisons are sensitive to how demographic information is incorporated. It does not show that genuine age standardization would move the same countries in the same direction or by similar amounts. Large modeled movements without victim-age rates should be treated as evidence of analytical fragility, not a correction to official international homicide rankings.

The demographic trap is best understood as a restriction on inference. A low crude homicide rate is meaningful for describing the number of deaths relative to the population actually living in a country. It is less adequate for comparing age-specific exposure to lethal violence across countries with different population structures. Both questions are legitimate, but they are not interchangeable. The present quantitative exercise gives the argument an observable empirical basis while also locating its principal weakness. Age composition is correlated with other national conditions, and a model built from one demographic ratio cannot isolate their effects. A stronger test requires directly comparable homicide mortality by victim age and a common standard population. Until such data are analysed, claims that some states are merely older rather than safer should remain hypotheses rather than conclusions.

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