

Safer Outside than Inside? The Prison Homicide Paradox and the Problem of Reported Zero

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ABSTRACT

Deaths caused by intentional violence are a particularly serious test of safety in prisons, yet comparative prison-homicide statistics remain uneven. Using two United Nations Office on Drugs and Crime workbooks, the study compared recorded prison homicide with national homicide rates from 2018 to 2023. It distinguished explicit zero counts from missing reports and calculated custodial homicide rates using prisoner stocks rather than national populations. The files contained 370 reported prison-homicide country-years, including 229 explicit zeros. Of these, 347 country-years from 72 countries and territories could be paired with the required denominators. In the main sample of 63 areas with at least three matched years, 24 had a pooled prison-to-national homicide rate ratio above one, while 31 had no recorded prison homicides in their matched years. Among 34 areas with all six years available, 19 had a ratio above one. An exploratory comparison of 57 areas found a weak association between prison occupancy and custodial homicide rates, with Spearman rho of 0.164 and a two-sided p value of 0.221. The estimates varied substantially with reporting coverage. The findings indicated that cross-national comparisons required explicit reporting of zero cases, missing observations and population denominators.



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INTRODUCTION

A person killed in prison dies within a setting in which the state controls movement and the conditions of everyday life. Prison authorities regulate access to protection, but they also record deaths and assign their causes. A reported homicide therefore concerns both violence and the reliability of custodial records. The distinction between legal murder and statistical intentional homicide also matters, since offence categories are not interchangeable across legal systems (Kadir, 2026b). A small number of deaths can produce an unusually high annual rate in a prison population, while an empty entry in an international database may reflect missing information rather than the absence of a killing.

Prisoners are not a random sample of the population with whom their mortality is usually compared. Many enter custody with different histories of violence exposure and health disadvantage. Fazel and Baillargeon (2011) documented serious health problems in incarcerated populations, and Favril et al. (2024) reviewed the extent of mental and physical morbidity across prison settings. These studies explain why crude mortality rates need careful interpretation, although neither provides a basis for treating every custodial death as homicide. Incarceration also carries risks that extend beyond a prison sentence, as Wildeman and Wang (2017) observed in their discussion of health inequality. A comparison of recorded homicide rates cannot remove those differences in population composition. It describes the rates observed in two populations, not the risk faced by otherwise identical individuals.

Institutional violence has been studied through explanations centred on conditions inside prisons and characteristics brought into custody. Cao et al. (1997) tested deprivation and importation

approaches in relation to disciplinary misconduct. Jiang and Fisher-Giorlando (2002) found reasons to consider situational influences alongside those accounts. Huebner (2003) added evidence about prison administration, while Hochstetler and DeLisi (2005) examined prison offending through a wider model of individual exposure. Most of these studies addressed misconduct rather than lethal victimisation. Their relevance to homicide is therefore explanatory rather than evidential. As Kadir (2026d) argues in relation to lethal and nonlethal violence, evidence restricted to fatal outcomes also cannot establish what uniquely made those events fatal. A theory developed from nonfatal aggression does not establish the rate at which prisoners are killed.

Confinement affects the ways prisoners encounter threats. Moving away from a dangerous person may be difficult when housing assignments and daily routines are controlled by others. Yet reported prison misconduct also reflects conflicts that existed before imprisonment. The systematic review by Ireland et al. (2026) identifies institutional conditions and relationships among prisoners as relevant to intragroup aggression. Kuo et al. (2023) examined the overlap between victimisation and misconduct, while Lahm (2016) studied the distinction between victims and perpetrators in a women's prison. Such findings caution against interpreting homicide as the simple result of one institutional factor. Administrative control may influence opportunities for violence, but the present data contain no information about specific encounters, offenders, or individual victims. Aggregate mortality cannot reconstruct how a particular killing occurred.

Crowding remains a plausible institutional concern, though the available evidence is not confined to fatal violence. Gaes and McGuire (1985) related crowding to assault rates in federal prisons. Baggio et al. (2020) studied assaults requiring medical attention in a Swiss pre-trial facility and examined turnover alongside overcrowding. The outcome in both studies was assault, not homicide. National occupancy rates also reduce very different living conditions to one administrative percentage. A prison system may be close to its official capacity while particular facilities hold far more people than intended. An association between national occupancy and custodial homicide therefore requires cautious interpretation.

Deaths in custody have several causes, and evidence about one cause cannot be used as a substitute for evidence about another. Wolff et al. (2016) examined overcrowding and self-harm in a Swiss prison. Van Ginneken et al. (2017) investigated prison overcrowding and suicide rates in England and Wales, while Rabe (2012) studied institutional structure and mortality across European prison systems. Aon et al. (2025) found that quantitative studies of overcrowding and health were uneven in both design and outcome. Those findings make overcrowding worth examining, without establishing that it causes prisoner homicide. The same restraint applies to broader research on incarceration and health (Dumont et al., 2012). Homicide should remain a separate outcome, with a numerator that includes only deaths classified as intentional killings in prison.

UNODC's Prison Matters 2025 reports elevated custodial homicide rates among many countries recording at least one prison homicide between 2018 and 2023. The restriction to positive-reporting countries is important. A country reporting zero deaths differs from a country that supplies no prison homicide count, and neither can be treated in the same way as a positive-reporting system. A comparison that excludes explicitly recorded zeros describes only a selected set of prison systems. Published rate fields also require attention to their denominators. A rate calculated against the national resident population is not a homicide rate per prisoner, even when its numerator consists of deaths inside prison. Comparing those figures without reconstructing the prisoner-based rate would produce a misleading measure of custodial lethality.

Matched national records for 2018–2023 permit a narrower examination of this problem. The analysis compares prison homicide counts with prisoner stocks and national homicide rates for the same areas and years. It retains explicit numerical zeros, then tests how the distribution of the prison-to-national homicide ratio changes when reporting requirements become stricter. National occupancy is considered separately as an exploratory association rather than a proposed cause of homicide. The study cannot measure the danger of imprisonment for comparable individuals or establish how many killings went unreported. Its contribution concerns the interpretation of cross-national administrative evidence, especially when claims about prison safety depend on which reporting systems enter the comparison.

RESEARCH METHODS

The study uses a descriptive cross-national ecological comparison of UNODC observations for 2018–2023. Two source workbooks were supplied for analysis. The Intentional Homicide file was dated 12 July 2026, and the Prisons and Prisoners file was dated 30 September 2026. The different update dates are recorded because future extracts may revise earlier observations. Only country or territory records were taken from the indicator sheets. Modelled regional estimates were not combined with the reported national values. The design compares aggregate risks across administrative systems and does not estimate what would have happened to a person had imprisonment been avoided (Wakefield, 2008).

Prison homicide was measured from the count of deaths attributed to intentional homicide in custody. Prison exposure was approximated by the reported number of persons held, and the national comparison used homicide counts and the corresponding rate per 100,000 residents. Official capacity provided a measure of prison occupancy. Only observations marked as totals for sex, age and other applicable dimensions were selected. Country and year were matched using three-letter area codes. Subnational codes for the constituent UK jurisdictions and central Iraq were excluded because they could not be paired consistently with the national comparison.

A usable country-year needed a reported prison homicide count, a positive prisoner stock and a positive national homicide rate. The count could be zero, but only when the source cell contained an explicit numerical zero. Blank entries and missing rows were left missing. The main country-level sample required at least three matched years during 2018–2023. Areas with one or more matched years and those with all six years were also retained for sensitivity checks.

For each matched year, the custodial homicide rate was calculated by dividing recorded prison homicides by the reported prisoner stock and multiplying by 100,000. The penal lethality multiplier, or PLM, divides this custodial rate by the matched national homicide rate. Country-level pooled custodial rates use the sum of recorded deaths divided by the sum of prisoner stocks over matched years. Pooled national rates use the sum of national homicide deaths over corresponding population exposures reconstructed from national counts and rates. Values above one indicate higher crude recorded homicide among prisoners. A value of zero indicates no recorded prison homicide in the matched years, rather than verified freedom from lethal violence.

The prison homicide workbook contains a rate field that could be mistaken for a prisoner-based rate. Checking 353 comparable records against homicide counts and national population measures showed that this field uses the general resident population as its denominator. It was therefore excluded from the custodial rate calculation. Annual prisoner stock is another imperfect denominator because the count is taken at a particular date, while deaths occur throughout the year. Where consecutive stocks were available, the mean of the two was used to check how much the pooled ratio changed.

Prison occupancy was computed as the prisoner stock divided by official capacity and multiplied by 100. We compared each area's median occupancy with its pooled custodial homicide rate, requiring at least three matched homicide years and two years with capacity information. Spearman rank correlation was appropriate for an exploratory description because many countries recorded no prison homicides and the positive rates were strongly skewed. We also compared systems whose median occupancy exceeded official capacity with those at or below it. No regression coefficient is presented. The available material does not support credible adjustment for prison-level conditions, prisoner composition or the quality of death reporting.

Sensitivity checks repeated the pooled comparison for areas with at least one matched year and for areas with complete six-year records. A positive-death-only subset illustrates what happens if recorded zero series are excluded. Unknown underreporting cannot be corrected with these data, and the comparison cannot estimate what happened in countries lacking suitable records. Ecuador was omitted separately because its high custodial death rate could influence the distribution. We then compared same-year prisoner stocks with the mean of consecutive-year stocks where both were available.

RESULTS AND DISCUSSION

Coverage, Reporting, and the Status of Zero

The two workbooks yielded 1,296 potential country-years across the 216 country and territory codes in the internal frame. A prison homicide count was reported for 370 of them, representing 77

areas. Explicit zeros accounted for 229 observations, or 61.89 per cent of reported counts, and 141 entries were positive. After matching prisoner stocks and national homicide rates, 347 country-years from 72 areas remained. Among these matched observations, 216 recorded zero prison homicides, representing 62.25 per cent. The numbers refer to values present in the UNODC files. They were not checked against separate death investigations in each jurisdiction.

Table 1 Annual availability and zero reporting in the matched UNODC panel, 2018–2023

Year	Prison-homicide counts	Explicit zeros	Strict pairs	Pairs with capacity
2018	58	37	55	39
2019	62	40	58	46
2020	67	43	64	47
2021	64	38	61	47
2022	68	43	63	55
2023	51	28	46	37
Total	370	229	347	271

A strict match requires a prison homicide count, a positive prisoner stock and a positive national homicide rate in the same area-year. Capacity is not a condition for inclusion in the main comparison. Source — authors' calculations from the two UNODC workbooks.

The number of matched observations varies across years. There were 64 strict matches in 2020 but only 46 in 2023. The fall does not imply that custodial homicide declined because the set of areas reporting had changed. Capacity information was available in 271 of the 347 matched observations. Prison homicide was unreported in 926 potential country-years from the original frame. Those cells were retained as missing rather than replaced with zero. Such distinctions are important in comparative mortality research, where routine administrative recording may differ across prison systems (Pereira, 2025). Incomplete reporting affects what can be estimated before any substantive theory of violence is considered. Evidence from prison suicide research in Italy further illustrates why the recorded cause of death must be distinguished from overall prison mortality (Bailo et al., 2023).

A zero over one year is not strong evidence of effective prison protection, especially when the prisoner population is small. Repeated zeros are more informative about the reported series but still depend on accurate classification of deaths. The original UNODC records contain different source labels and reporting channels. Kadir (2026e) documented disagreement between homicide figures derived from UNODC and WHO records, cautioning against equating source concordance with completeness. A positive count may also coexist with stronger investigation and disclosure practices. Reading all positive counts as institutional failure and all zeros as successful prevention would therefore go beyond the evidence.

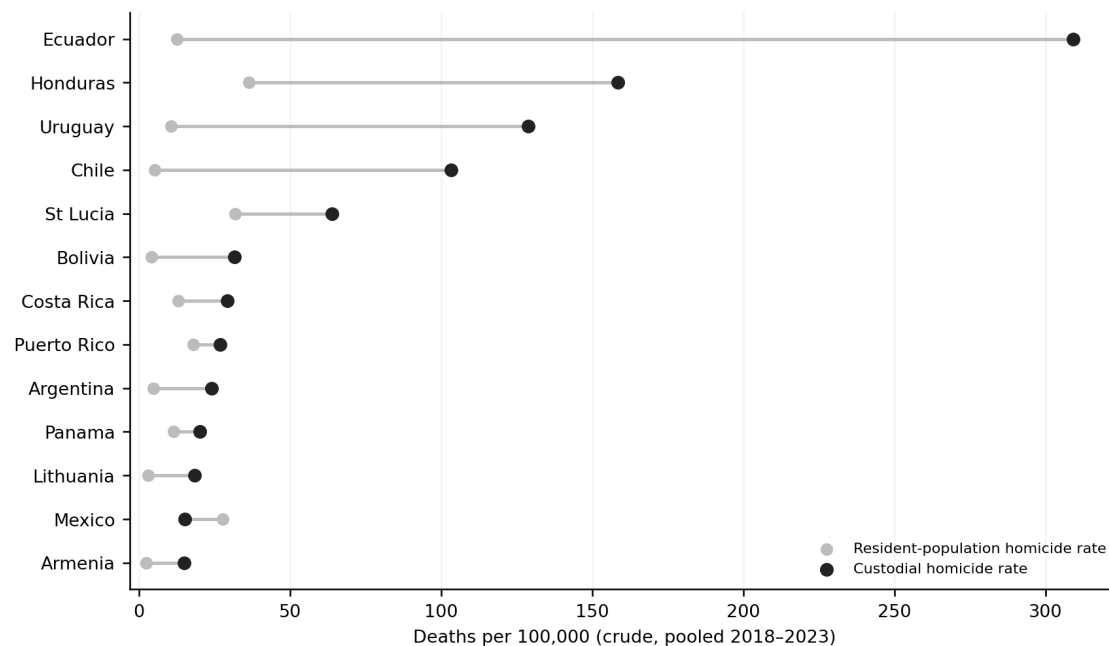
The Geography of Penal Lethality

The distribution of pooled PLM values differs across reporting-eligibility rules. Of 72 areas with at least one complete match, 28 have a ratio above one, nine have a positive ratio below one and 35 record zero. The principal sample of 63 areas with at least three matched years contains 24 above one, eight between zero and one, and 31 at zero. Its median PLM is 0.106. Among the 34 areas reporting all six years, however, 19 exceed one and the median rises to 1.708. These values describe different administrative samples. They should not be treated as competing estimates of a single worldwide parameter.

Table 2 Penal lethality across alternative longitudinal eligibility rules

Analytical sample	Areas	PLM = 0	0 < PLM < 1	PLM > 1	Median PLM
≥1 paired year	72	35	9	28	0.136
≥3 paired years	63	31	8	24	0.106
All six paired years	34	10	5	19	1.708

Each country or territory contributes one ratio pooled over its matched years. Values are crude and use annual prisoner stocks as exposure proxies. The eligibility groups are nested but non-random.

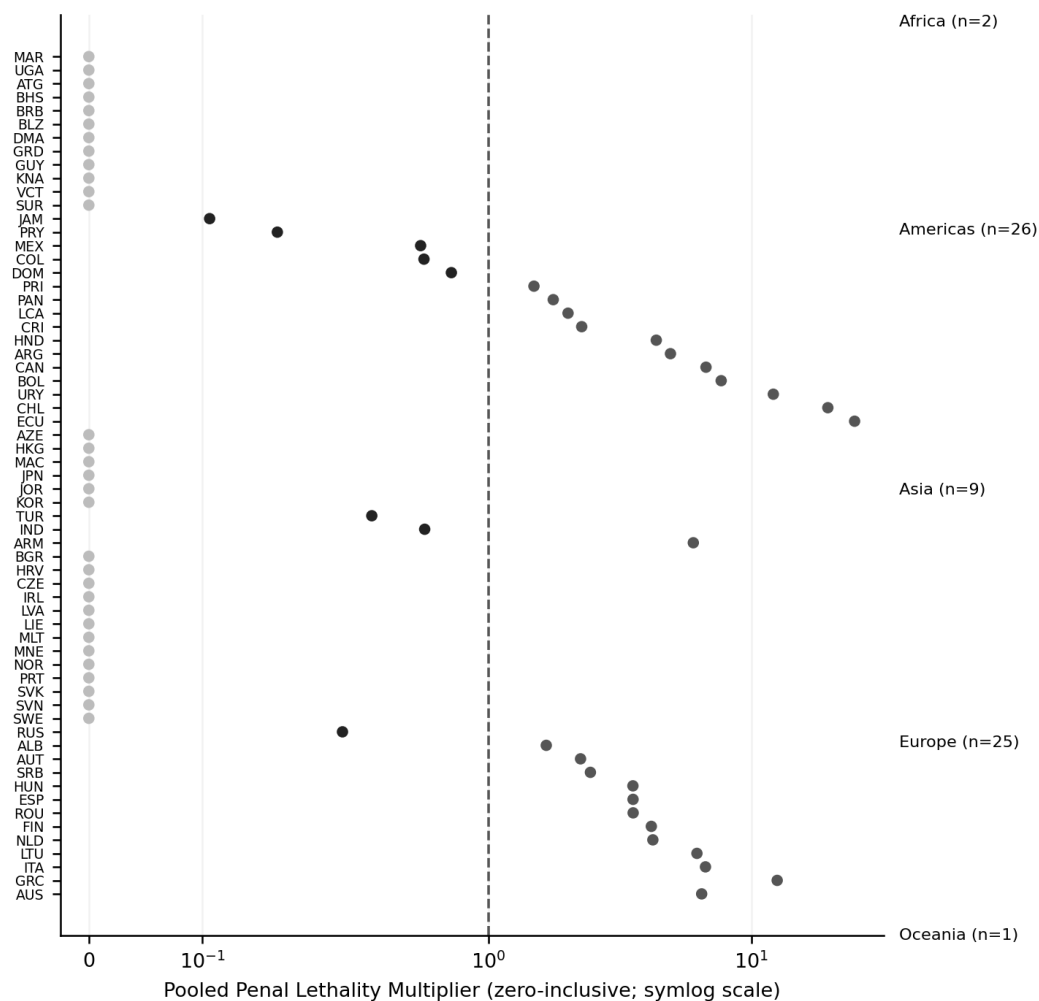


Picture 1 Prison and general-population homicide rates for the 13 areas with the highest custodial rates in the eligible three-year sample.

The figure ranks areas by custodial homicide rate rather than by representativeness. Annual counts are pooled within each area. Neither rate has been standardised by age or sex.

Ecuador reported 574 prison homicide deaths across five matched years. Its custodial rate was approximately 309 per 100,000 prisoner-stock exposure units, giving a PLM of 24.71. Chile reported 229 deaths across five matched years and had a custodial rate of 103.3 with a PLM of 19.54. Honduras and Uruguay also appear among the high-rate systems in Picture 1. Such differences deserve attention, but the rate alone does not identify how the deaths occurred or which institutional practices preceded them. Some years may contain concentrated lethal incidents. Our aggregate data cannot separate sustained danger from brief episodes of exceptional violence.

The rate ratio also depends on the homicide rate in the national resident population. Mexico recorded 196 custodial homicide deaths over six matched years, yet its PLM was approximately 0.55 because the national resident-population homicide rate was much higher than the pooled custodial rate. Greece reported seven prison homicides over six years and produced a PLM around 12.53 against a low national homicide rate. Seven deaths can yield a larger ratio than almost two hundred deaths when the denominators differ. Kadir (2026c) similarly treated cross-national lethal violence indicators as measured compositions, rather than individual probabilities of fatal escalation.



Picture 2 Zero-inclusive distribution of pooled penal lethality multipliers across the 63 areas with at least three matched years.

The display retains zero values and marks a PLM of one for comparison. A symmetric logarithmic scale is used only for presentation. No transformation enters the underlying calculation.

Zero-valued ratios are common in the matched sample, which changes the visual distribution compared with a chart that reports only positive deaths. Geographic coverage also limits interpretation. Of the 63 eligible areas, 26 were in the Americas and 25 in Europe. Asia contributed nine, Africa two and Oceania one. These are reporting locations, not a sample drawn to represent the world. The ecological study of recorded homicide in Indonesia by Kadir (2026f) likewise distinguished the geography of official records from the distribution of unobserved violence. The lack of observations from several regions prevents any credible estimate of the global share of prison systems with excess custodial homicide. More complete coverage could change the median, the proportion above one and the apparent geographic pattern in either direction.

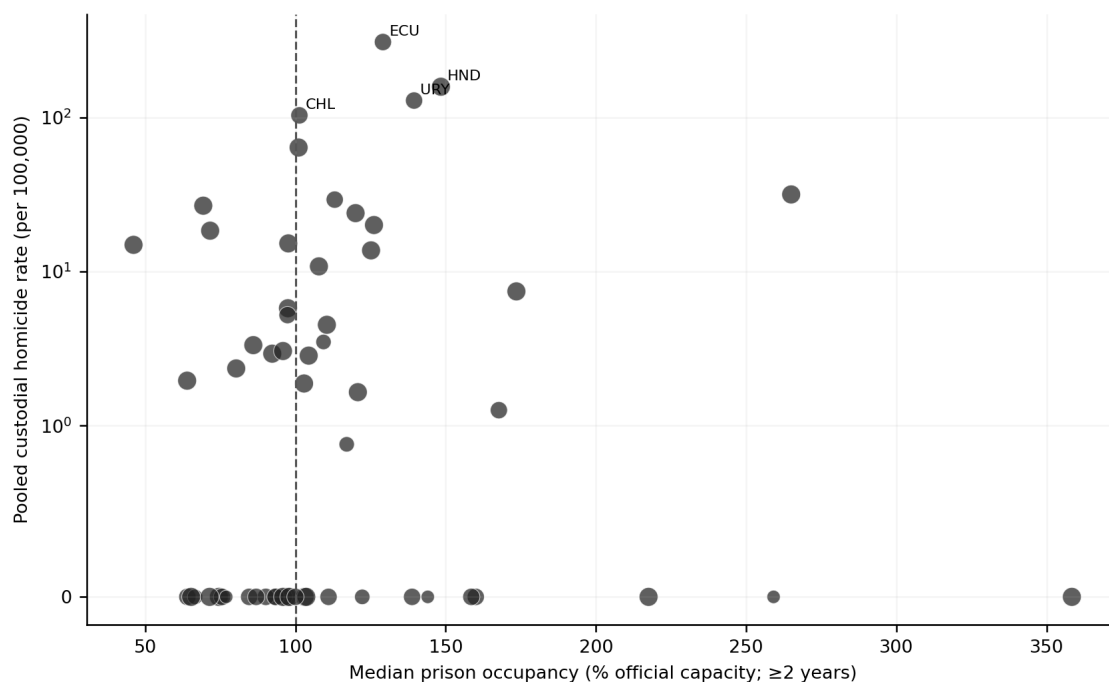
Overcrowding and the Institutional Distribution of Risk

Prison occupancy and homicide were available together for 57 areas under the minimum coverage rules. Thirty had a median occupancy above official capacity, compared with 27 at or below it. The pooled custodial homicide median was 2.36 per 100,000 in the over-capacity group and zero in the other. Positive prison homicide counts occurred in 19 of the 30 over-capacity systems and 11 of the 27 others. Across all 57 areas, however, the rank correlation between median occupancy and custodial homicide was only 0.164, with a two-sided p value of 0.221. This provides little evidence of a consistent monotonic relationship in the available sample.

Table 3 Prison occupancy and custodial homicide at the country/area level

Median occupancy	Areas	Positive death count	Median pooled CHR	PLM > 1
≤100% official capacity	27	11	0.00	8
>100% official capacity	30	19	2.36	14
Overall analytic set	57	30	—	22

Occupancy groups use the area's median across at least two years with matching prisoner-stock and capacity reports. The exploratory Spearman coefficient is 0.164 with a two-sided p value of 0.221, based on 57 areas. No causal estimate is implied.



Picture 3 National prison occupancy and pooled custodial homicide rates in the 57 areas with sufficient capacity observations.

The vertical line marks occupancy of 100 per cent of official capacity. The symmetric logarithmic vertical display retains areas with zero recorded prison homicides.

Findings from individual prisons help explain why a national occupancy figure might have little connection with recorded homicide rates. Gaes and McGuire (1985) linked crowding with assaults in a set of US federal facilities. Baggio et al. (2020) found associations between overcrowding, turnover and medically attended assault in one Swiss institution. Neither study estimated a national custodial homicide rate. Work on prison suicide has produced results that depend on institutional composition and management (Huey & McNulty, 2005). Van Ginneken et al. (2017) reported no independent overcrowding association after accounting for other prison characteristics. These findings justify examining occupancy, but not expecting a single cross-national homicide coefficient to capture local mechanisms. Leese et al. (2006) also studied self-inflicted deaths through an ecological design, but the dependent variable was not homicide.

Official prison capacity is an administrative number that can vary with building standards and counting practices. Simpson et al. (2019) also distinguished institutional occupancy from the spatial conditions experienced by prisoners. One national figure can hide severe crowding in particular wings while other facilities have unused beds. Prison-level violence may also depend on staff practices or conflicts between groups that national totals cannot identify. Kadir (2026h) described a similar loss of relational and situational information when homicide is reduced to aggregate legal categories. Because the present data contain neither victim characteristics nor institutional conditions, the difference between occupancy groups remains descriptive.

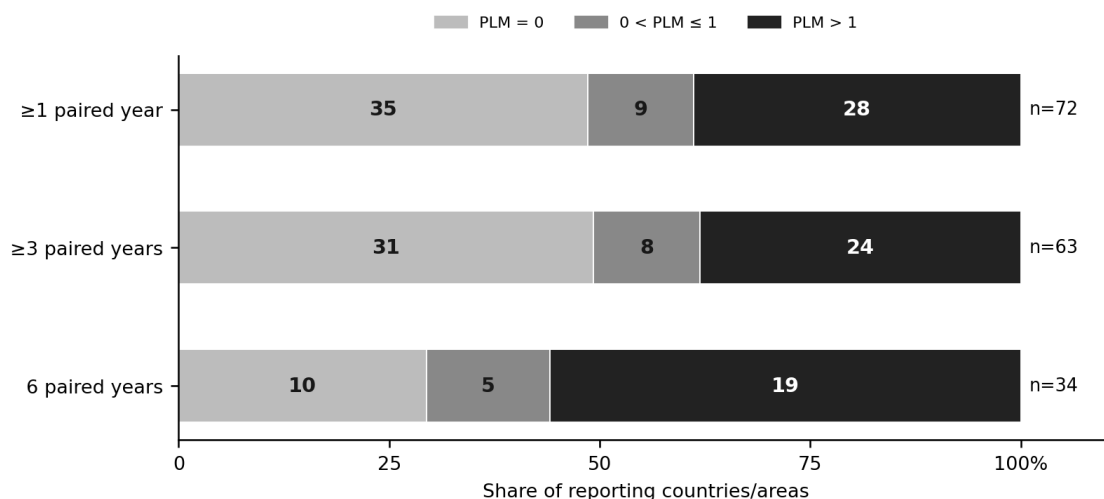
Does the Penal Lethality Paradox Survive Alternative Specifications?

Excluding areas with only recorded zeros changes the sample distribution without changing the calculation. Of the 63 areas in the main sample, 31 have no recorded custodial homicide in their matched years. Restricting the analysis to the other 32 leaves 24 areas with PLM above one, or 75 per cent, and raises the median ratio from 0.106 to 3.540. The calculation is correct for the selected subset. Its significance lies in what has been excluded. A comparison built around countries with a recorded custodial death cannot be treated as evidence that a similar excess characterises all reporting countries, much less countries without usable records. The selection problem differs from the cleared-case bias examined by Kadir (2026a), but both restrict which cases can support general claims.

Table 4 Sensitivity of the apparent prison homicide excess to case selection

Specification	Areas	PLM > 1	PLM = 0	Median PLM
≥3 pairs, zeros retained	63	24 (38.1%)	31	0.106
≥3 pairs, positive deaths only	32	24 (75.0%)	0	3.540
≥3 pairs, exclude Ecuador	62	23 (37.1%)	31	0.053
Complete six-year pairs	34	19 (55.9%)	10	1.708

All proportions refer to the countries and territories covered by the corresponding specification. Removing zero-only series changes the eligible sample and does not estimate the number of deaths missed by national reporting systems.



Picture 4 Penal lethality classifications under alternative minimum reporting requirements.

Bars retain exact counts of areas with ratios above one, between zero and one, and equal to zero. The groups differ in their reporting completeness and are not random national samples.

A single extreme system can also affect the apparent distribution. Removing Ecuador from the main three-year sample leaves 23 of 62 areas with PLM above one and lowers the sample median to approximately 0.053. At the other end, retaining only the 34 complete six-year panels yields 19 areas above one. More consistent reporting is therefore associated with a different observed composition, but completeness itself does not prove greater danger. This difference reflects reporting composition, not an established change in prison safety.

A further check used the mean of consecutive annual prisoner stocks as the exposure denominator. This was possible for 289 matched country-years across 68 areas. Among 33 areas recording positive prison homicide in that restricted comparison, the ratio of the revised PLM to the original PLM had a median of 1.000. Its observed range was approximately 0.955 to 1.093. That degree of variation was small relative to the changes produced by country selection. It still cannot resolve the effect of admissions and releases during the year. Adler and Chen (2023) found jail turnover relevant to mortality, although their study examined US jails rather than the prison systems compared here.

A high custodial rate does not mean that prison caused an equivalent increase in an individual person's chance of being killed. People entering prisons differ from the national population in age, sex, previous violence exposure and other characteristics. Deprivation and importation research has long identified this mixture of institutional and pre-existing conditions (Jiang & Fisher-Giorlando, 2002). The data used here do not allow those influences to be disentangled. Reported homicide remains a narrower outcome than custodial safety as a whole. Kadir (2026j) also distinguished general violence risk from the circumstances under which an encounter becomes lethal. Neither component can be recovered from these national prison counts. Suicide, illness and deaths after release have separate risk structures and should not be merged into the homicide numerator (Fazel et al., 2017).

International administrative statistics can support careful comparison even when they cannot support causal explanation. The useful distinction is between a reported positive count, a reported zero and a year for which no count can be found. Each carries different information about observed mortality. A small prisoner stock also means that one fatal event can move the rate sharply. The PLM makes the relative rate visible but cannot resolve uncertainty about the underlying investigation or the completeness of reporting. As Kadir (2026i) cautioned, the institutional origin of homicide statistics needs checking before differences between recording systems receive substantive explanations. Guidance on ecological studies of violence calls for clarity about unit selection and measurement (Zeoli et al., 2019). Those concerns are especially pressing when cross-national death counts are sparse. Poorly reported systems may have very different mortality patterns, and the present findings supply no defensible basis for imputing their experience.

The present comparison cannot identify specific unsafe institutions or recommend a particular capacity threshold. Crowding may be experienced differently across facilities even within the same national system, while turnover and institutional practice may alter exposure to violence (Baggio et al., 2020). Surveillance that distinguishes homicide from other causes of death would allow more meaningful comparisons over time. Research on the health of people in custody makes clear that homicide represents only one part of custodial risk (Favril et al., 2024). An ecological analysis of Indonesian provincial spending by Kadir (2026g) likewise found that recorded homicide need not align neatly with the administrative distribution of protective resources. Wider safety claims need more than a single mortality category.

The limits of these findings arise principally from the source material. Reporting areas are concentrated in Europe and the Americas. Case definitions and prison coverage cannot be independently harmonised for every jurisdiction. The prison stock does not measure actual person-time, and the national homicide denominator includes prisoners rather than a strictly separate non-prison population. Sex and age standardisation was not possible. Data for 2020 and 2021 may also reflect changes in admissions and prison conditions during the COVID-19 pandemic (Saloner et al., 2020). Finally, the source workbooks were updated at different points in 2026. Replication against future releases may therefore produce revisions even when the analytical rules stay unchanged.

CONCLUSION

Recorded prison homicide does not exceed national homicide in every country that reports data. Among 347 matched observations across 72 countries and territories, zero prison deaths were explicitly recorded in 216 country-years. In the principal sample of 63 areas with at least three matched years, 24 had a pooled PLM above one and 31 had a ratio of zero. The balanced six-year panel had a different distribution, with 19 of 34 areas above one.

The descriptive link between prison occupancy and custodial homicide was weak in the 57 areas with sufficient data. More importantly, the high share of explicitly recorded zeros changes the interpretation of a comparison limited to positive-death systems. More complete and comparable reporting would support broader comparisons of recorded custodial homicide. Prison-level death counts, comparable person-time denominators and documented investigative standards would permit much stronger tests. For the available UNODC material, the defensible contribution is a zero-inclusive and reporting-sensitive assessment of custodial homicide, not a claim that imprisonment or overcrowding has been shown to cause lethal violence.

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