

The State Does Not Count the Same Dead: The Politics of Homicide Measurement Across Criminal Justice and Public Health Systems

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ABSTRACT

Homicide statistics were often treated as unusually comparable because lethal events depended less on victim reporting than many non-lethal offences. This methodological study examined whether that assumption survived differences between criminal justice and public-health recording systems. Official WHO and UNODC documentation was audited for source provenance, counting units, classification rules, and time-series breaks. A symmetric System Recording Differential was then stress-tested through negative-binomial Monte Carlo simulations, followed by a persistence experiment and a tipping-point analysis for deaths of undetermined intent. The audit showed that WHO mortality observations had a clear civil-registration origin, whereas UNODC national homicide data could originate in either criminal justice or public-health systems, making database labels insufficient proxies for provenance. Under a fixed 25 percent recording ratio, the simulated differential remained centred near its true log ratio across baseline homicide rates from 0.25 to 40 per 100,000, while absolute rate differences expanded mechanically. Observed annual ratios also sharply attenuated latent temporal persistence. The findings supported provenance verification, symmetric modelling, and explicit classification sensitivity before cross-system homicide gaps were given institutional explanations.



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INTRODUCTION

Homicide occupies an unusual place in comparative criminology because it is less dependent on victim reporting than most non-lethal offences. That advantage is real, but it does not make homicide statistics self-producing. A death must still be discovered, classified, assigned to a reference period, and retained in an official series. Vigderhous (1978) identified definitional and operational non-equivalence as a basic problem in cross-national crime research. The problem remains visible in contemporary homicide data because legal and medical systems do not observe or classify lethal events through the same institutional route.

Police data are administrative traces rather than direct observations of violence. Recording decisions can alter official crime statistics even when offending does not change (Levitt, 1998). MacDonald (2002) and Wellford (2017) therefore caution against treating recorded crime as transparent behaviour. Geographic work reaches the same conclusion when police data bias changes spatial patterns (Buil-Gil et al., 2022), while sensitivity analysis shows that measurement error can affect inference (Pina-Sánchez et al., 2023). Administrative choices can accumulate before the annual total is published. Indonesian homicide mapping accordingly treats the recorded count as territorial visibility rather than an exhaustive inventory of lethal events (Kadir, 2026g). Figure 1 summarises these parallel routes.

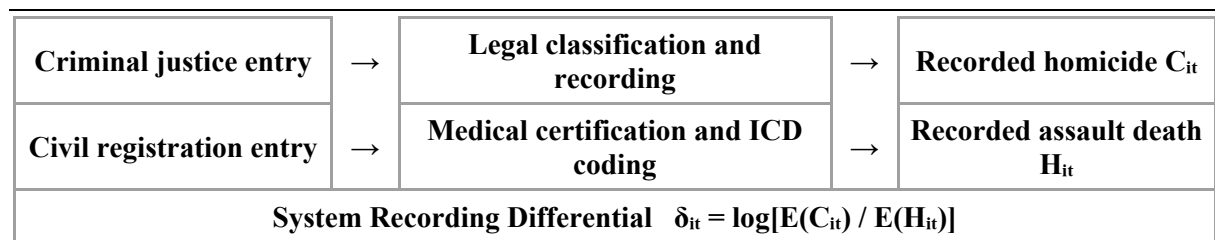


Figure 1 Dual Institutional Production of Homicide Statistics and the Cross-System Differential

Mortality statistics are produced through a different chain. Civil registration may be incomplete, and cause-of-death coding varies across countries even when deaths are registered (Mahapatra et al., 2007). Mikkelsen et al. (2015) document large differences in national civil registration and vital statistics capacity. Certification studies show that errors can alter the underlying cause assigned to a death (McGivern et al., 2017) (Gamage et al., 2021). The resulting health series is therefore not a neutral benchmark against which criminal justice data can simply be corrected.

Cause-of-death quality also cannot be reduced to a single completeness score. AbouZahr et al. (2015) distinguish counting deaths from assigning useful causes. Adair and Lopez (2018) estimate death-registration completeness, while Iburg et al. (2020) show that cause data quality varies independently of broad development levels. Naghavi et al. (2020) demonstrate why ill-defined causes remain consequential. For violent mortality, deaths of undetermined intent are especially important because they remain external-cause deaths without necessarily entering the assault category (Lachaud et al., 2018). These coding decisions may be internally consistent and still remain non-equivalent to legal homicide categories. A system may therefore register most deaths and still classify lethal violence differently from criminal justice authorities.

Direct comparisons confirm that source choice can change what a homicide series appears to show. Andersson and Kazemian (2018) found substantial correspondence between United Nations and World Health Organization series, but levels and treatment of unclassified deaths differed. Rogers and Pridemore (2023) showed that source selection can alter trends and structural conclusions. Buil-Gil et al. (2026) linked part of the European variation to police counting rules and legal definitions. Kadir (2026d, 2026e) similarly treats recorded lethal violence as an ecological property and cautions against assuming that fatal outcome creates a wholly distinct causal domain.

Jiang, Santos, and LaFree (2026) move closer to the present problem by treating the discrepancy between criminal justice and health homicide data as an outcome across 99 countries. Their Bayesian quantile models associate smaller gaps with stronger institutional capacity and greater global integration. That work establishes that the homicide gap can itself be analysed. It leaves a prior measurement problem unresolved, however. International database labels do not guarantee independent national origins, and two series may disagree because their constructs or recording stages differ before broader institutional capacity enters the explanation.

Homicide visibility is defined here as the institutional process through which a lethal death becomes recognisable as a statistical homicide. The idea extends the homicide visibility gap developed in earlier exploratory work (Kadir, 2026f), but narrows the claim. Criminal justice and public-health counts are compared only after source lineage and construct boundaries are audited. A symmetric recording differential is then tested across different homicide intensities and under classification uncertainty. The analysis combines documentary source auditing with Monte Carlo stress tests without pretending that two aggregate series reveal the true number of homicides.

RESEARCH METHODS

The study uses a methodological design with two linked components. The first is a documentary audit of the data-generating architecture behind international homicide series. Official documentation from the World Health Organization and the United Nations Office on Drugs and Crime is read alongside the comparative homicide literature. The audit records the originating institution, observation unit, reference period, classification rule, territorial coverage, revision status, and any documented break in source or method. The purpose is not to score countries. It is to determine whether two values presented under different international labels are actually distinct institutional observations of a sufficiently similar construct.

The public-health benchmark is the WHO Mortality Database because it compiles deaths transmitted by national civil registration systems and retains the underlying cause coded by national authorities (World Health Organization, 2026). UNODC intentional-homicide data are treated differently because repository identity alone does not establish provenance. UNODC states that national homicide data may originate in either criminal justice or public-health systems (United Nations Office on Drugs and Crime, 2026). An apparently cross-system comparison may therefore reproduce the same national source twice. A country-year enters the criminal-justice side only when its source can be verified as criminal justice in origin.

Construct comparability is assessed before any difference is calculated. Criminal justice observations must count victim deaths rather than cases, suspects, or convictions, while the public-health series must represent assault deaths under the relevant ICD framework. Legal categories still require separate attention because murder and intentional homicide do not preserve identical thresholds across legal systems (Kadir, 2026c). Homicide is also narrower than lethal violence in general (Kadir, 2026h). Boundary categories are therefore coded rather than silently absorbed into the comparison. Table 1 operationalises these rules.

Table 1 Provenance and Construct Comparability Rules

Dimension	Criminal justice series	Public-health series	Primary rule
Source origin	Police, prosecutor, or criminal justice statistical office	Civil registry, health ministry, or mortality statistical office	National origin must be traceable and independent
Observation unit	Victim death	Registered death	Case, offence, and offender counts are excluded
Classification	National homicide definition with documented mapping	Assault death under the relevant ICD framework	Boundary categories are recorded before estimation
Coverage	National or clearly bounded population	Same population and territory	Population denominator must match both series
Reference time	Occurrence or recording year as documented	Death or registration year as documented	Temporal mismatch is coded and assessed
Comparability grade	A or documented B for primary analysis	A or documented B for primary analysis	C and D remain in the audit but not the main model

Cross-system divergence is represented by the System Recording Differential. For country i and year t , δ_{it} equals $\log[E(C_{it}) \text{ divided by } E(H_{it})]$. Zero denotes parity. Positive values indicate higher expected criminal justice recording and negative values indicate higher expected public-health recording. Exponentiating δ_{it} gives the expected count ratio. The measure is directional rather than normative. It does not identify which system is correct, and rate differences per 100,000 are kept only as a descriptive companion.

The proposed estimation model treats the paired counts symmetrically. Each series follows a system-specific negative-binomial likelihood with the common population as an offset. A shared country-year component λ_{it} captures variation visible to both systems, while δ_{it} moves the expected counts in opposite directions around that shared component. The model therefore avoids designating either series as ground truth. Temporal persistence is represented by an autoregressive term ρ on δ_{it} . This separation is important when the common level of lethal violence changes faster than the institutional relation between the two recording systems. Weakly regularising priors, prior predictive checks, posterior predictive checks, and comparison with a static model are retained because they address model behaviour rather than add separate substantive claims (Carpenter et al., 2017) (Vehtari et al., 2017). Figure 2 shows this symmetric decomposition.

The System Recording Differential was stress-tested before it was used as the centre of the proposed model. The expected criminal justice to public-health count ratio was fixed at 1.25, giving a

true differential of log 1.25 or 0.223. Public-health homicide rates ranged from 0.25 to 40 per 100,000 in a population of ten million. Counts followed negative-binomial distributions with dispersion 40. Ten thousand replications were run at each baseline rate. Mean differential, bias, root mean squared error, direction accuracy, and absolute rate difference were recorded. Table 2 summarises the simulation design.

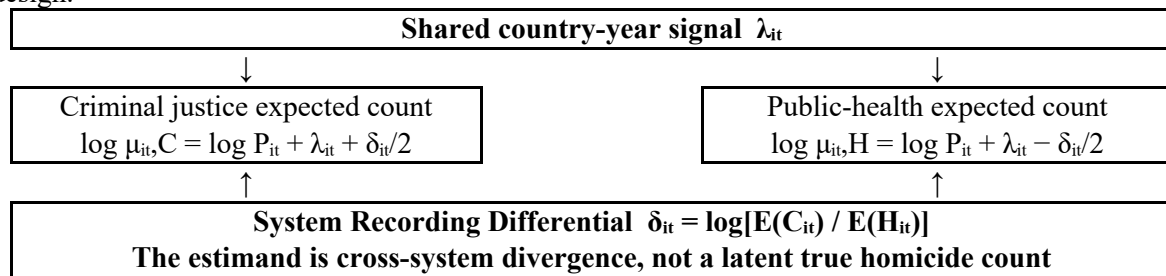
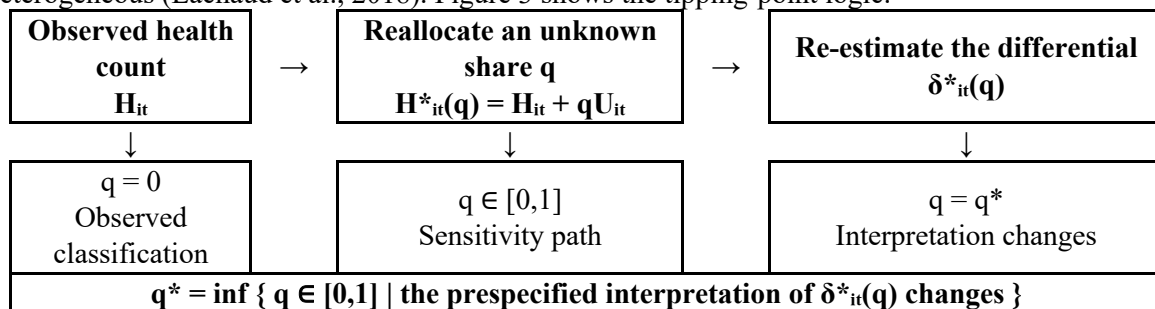


Figure 2 Symmetric Bayesian Paired-Count Model and the Cross-System Differential

Temporal persistence was examined under noisy paired counts. Thirty simulated countries were followed for twenty years. Country-specific average divergence varied around zero, while the latent yearly differential followed autoregressive processes with true ρ values of 0, 0.4, 0.7, and 0.9. Populations and baseline homicide rates varied across countries, and both observed systems were overdispersed. Three hundred panels were generated for each persistence condition. A country-demeaned lag regression on observed log ratios served only as a naive benchmark. Because annual ratios inherit noise from two count processes, this benchmark was expected to attenuate serial dependence rather than recover the latent parameter without error. It was therefore not treated as a substitute for the dynamic model.

Classification sensitivity was examined analytically through deaths of undetermined intent. If U_{it} is the number of such deaths in the mortality system, the alternative health count is $H^*_{it}(q) = H_{it} + qU_{it}$ for q between zero and one. The tipping point q^* is the smallest feasible reallocation at which the prespecified interpretation changes. The analysis does not select a preferred value of q . It asks whether a conclusion is fragile to small reallocations or survives even the complete reassignment of undetermined deaths. This is consistent with evidence that undetermined violent deaths are heterogeneous (Lachaud et al., 2018). Figure 3 shows the tipping-point logic.



q is a robustness boundary. It is not an estimate of the true share of undetermined-intent deaths that were homicide.*

Figure 3 Tipping-Point Sensitivity as a Partial-Identification Boundary

Table 2 Monte Carlo Stress-Test Design

Component	Specification	Analytical purpose
Source audit	WHO Mortality Database and UNODC intentional-homicide metadata	Test whether repository labels reveal institutional provenance
Scale stress test	Eight public-health baseline rates from 0.25 to 40 per 100,000	Assess whether the recording differential changes mechanically with baseline homicide intensity

Component	Specification	Analytical purpose
Count generation	Population 10 million, negative-binomial dispersion 40, 10,000 replications per baseline	Retain count noise and overdispersion
True recording ratio	Expected criminal justice count divided by health count equals 1.25	Set true differential to log 1.25 or 0.223
Persistence stress test	30 countries, 20 years, 300 panels per condition	Assess attenuation when persistence is estimated from noisy observed ratios
True persistence	0, 0.4, 0.7, and 0.9	Represent absent through strong temporal continuity
Tipping-point grid	Recording ratios 1.10, 1.25, 1.50 and U/H shares 0.10, 0.25, 0.50, 1.00	Measure how much reclassification is needed to change interpretation

RESULTS AND DISCUSSION

WHO and UNODC metadata do not describe parallel data architectures. The WHO Mortality Database identifies its observations as national vital-registration deaths with underlying causes coded by the competent national authority, although coverage and completeness can still vary (World Health Organization, 2026). UNODC uses the ICCS definition of intentional homicide and normally obtains national information through the UN Survey of Crime Trends and Operations of Criminal Justice Systems. Its own metadata, however, state that national homicide data may be sourced from either criminal justice or public-health systems (United Nations Office on Drugs and Crime, 2026). A nominal UNODC versus WHO comparison can therefore contain two health-origin series unless provenance is checked.

A mislabeled source changes the estimand rather than merely adding noise. If the UNODC label is assumed to denote police or prosecution data, agreement may be interpreted as cross-system convergence even when the values share a national origin. The opposite error is possible when repositories preserve different revisions or reference rules. Marshall and Block (2004) described the long-standing trade-off between cross-national coverage and comparability. The present audit makes that trade-off operational. Losing observations after provenance screening is preferable to retaining a larger panel whose apparent institutional contrast is partly manufactured by source duplication.

UNODC metadata show why construct harmonisation cannot be postponed until modelling. For India, the international series aggregates several Penal Code categories to approximate ICCS intentional homicide, while the metadata note that only one section is treated domestically as intentional homicide in the stricter legal sense (United Nations Office on Drugs and Crime, 2026). Kadir (2026c, 2026k) reaches the same issue from comparative criminal law and socio-legal homicide analysis. Statistical harmonisation does not make legal categories doctrinally identical, which matters when a numerical gap is interpreted as institutional visibility.

The United States metadata expose a different problem. UNODC records a break in 2021 for some disaggregated homicide measures because values shifted to National Incident Based Reporting System estimates (United Nations Office on Drugs and Crime, 2026). A sharp movement around such a year cannot be read as a behavioural discontinuity without first examining the statistical regime. Recorded violence is shaped by institutional stages and data ownership (Gonçalves & Quijano, 2026). Investigative selection can concentrate information in later case stages (Kadir, 2026b), while forensic evidence still depends on institutional conversion into attributable cases (Kadir, 2026a). Temporal irregularity may therefore carry administrative meaning, as Table 3 summarises.

Table 3 Documentary Audit Findings and Measurement Consequences

Finding	Documentary evidence	Measurement consequence
WHO observations have a defined health-system origin	Deaths are transmitted from national civil registration and vital statistics systems with causes coded by national authorities	WHO Mortality Database can serve as a public-health institutional output, subject to coverage and coding quality
UNODC repository identity does not guarantee criminal-justice origin	UNODC metadata state that national homicide data may come from criminal justice or public-health systems	Country-year provenance must be verified before calling a comparison cross-system
India requires legal-to-statistical mapping	UNODC metadata aggregate several Penal Code categories for ICCS reporting while noting a narrower domestic intentional-homicide interpretation	Statistical harmonisation does not produce doctrinal equivalence
United States has a documented 2021 break for some disaggregations	UNODC records a move to NIBRS estimates	Abrupt movement around the break cannot be presumed to reflect violence alone
Regional and global UNODC estimates use imputation	UNODC fills missing national values before aggregation	Observed national series and imputed aggregates should not be mixed in the primary paired analysis

The scale stress test kept the System Recording Differential centred on the intended relative difference across very different homicide intensities. The true differential was 0.223. Across baseline public-health rates from 0.25 to 40 per 100,000, the simulated mean differential ranged only from 0.219 to 0.227. Absolute bias never exceeded 0.0041, even though overdispersion was retained in both series. The ordinary rate difference behaved differently. Because the criminal justice count was fixed at 25 percent above the health count, the mean absolute gap rose mechanically from about 0.064 to 10.066 deaths per 100,000 as the baseline rate increased. Table 4 reports the full stress test.

Table 4 Monte Carlo Results for a Constant 25 Percent Recording Differential

Public-health rate	Mean SRD	Absolute bias	Direction accuracy	Mean absolute rate gap
0.25	0.227	0.0040	72.5%	0.064
0.5	0.219	0.0041	75.8%	0.122
1	0.221	0.0017	79.6%	0.248
2	0.225	0.0023	81.9%	0.504
5	0.223	0.0004	82.8%	1.253
10	0.222	0.0010	83.5%	2.489
20	0.220	0.0028	84.0%	4.937
40	0.224	0.0009	84.2%	10.066

Uncertainty was widest where homicide counts were smallest. The point estimates in Figure 4 remain close to the true log ratio over the entire baseline range, but direction accuracy rose from 72.5 percent at a public-health rate of 0.25 per 100,000 to 84.2 percent at a rate of 40. That increase does not mean the differential changed with homicide intensity. Random count variation can overwhelm a modest relative difference when events are rare. The result supports partial pooling and uncertainty intervals rather than deterministic country labels based on the sign of one observed annual gap.

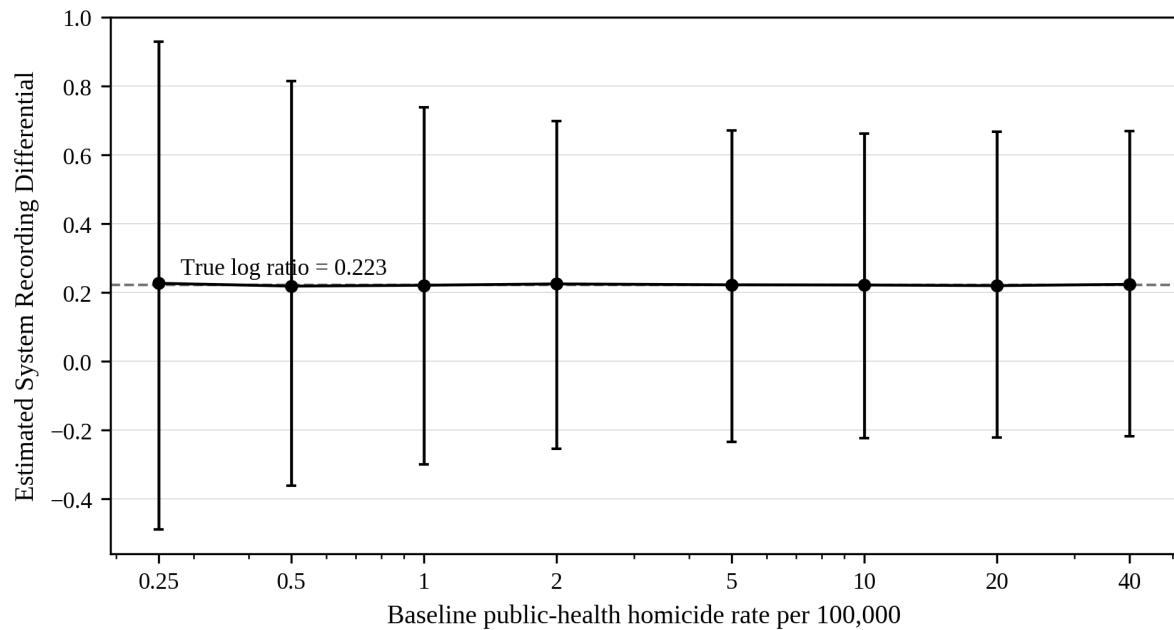


Figure 4 Stability of the System Recording Differential Across Baseline Homicide Rates

Relative and absolute gaps answer different questions when homicide levels vary sharply across countries. A fixed absolute difference does not have the same meaning in a low-homicide and a high-homicide setting, while a fixed relative difference can generate very different absolute gaps. Rogers and Pridemore (2023) show that measurement choices can alter cross-national inference, and Buil-Gil et al. (2026) demonstrate that counting rules contribute to international differences. The simulation does not prove that a log ratio is universally superior. It shows that the proposed differential answers the question posed here without mechanically scaling with baseline homicide intensity.

The temporal stress test exposed a second measurement problem. When latent persistence was zero, the naive country-demeaned lag coefficient on observed ratios averaged about -0.05. At true persistence values of 0.4, 0.7, and 0.9, the corresponding naive estimates were only 0.06, 0.19, and 0.33. Table 5 and Figure 5 show the systematic attenuation away from the identity line. The observed ratio remained correlated with the latent differential, but yearly count noise weakened the apparent serial dependence. A raw lag analysis would therefore make durable institutional divergence look substantially less persistent than it really was in the data-generating process.

Table 5 Temporal Persistence Stress Test Using Noisy Observed Log Ratios

True persistence	Naive mean	95 percent simulation interval	Latent-observed correlation	Sign accuracy
0.0	-0.05	-0.14 to 0.03	0.75	77.7%
0.4	0.06	-0.03 to 0.15	0.76	78.1%
0.7	0.19	0.09 to 0.28	0.79	79.4%
0.9	0.33	0.22 to 0.44	0.86	83.0%

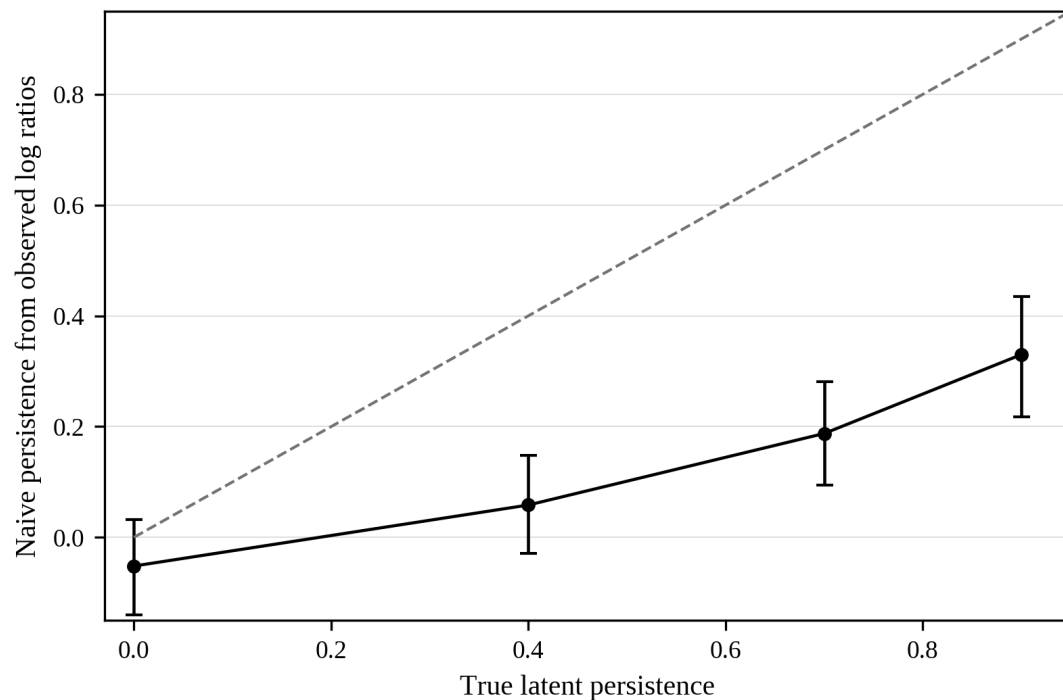


Figure 5 Attenuation of Persistence in Noisy Observed Log Ratios

The attenuation gives the autoregressive component a clear job. It is not included merely because longitudinal Bayesian models appear more sophisticated. The latent formulation separates persistence in the recording differential from noise in annual counts. A durable difference suggests a stable recording relationship worthy of documentary explanation, while an isolated reversal should be treated cautiously. The logic resembles the distinction between clustering and event dependence in homicide-contagion research, where recurring proximity does not by itself establish transmission (Kadir, 2026i). Statistical persistence remains a pattern to explain rather than a mechanism already discovered.

The tipping-point calculation produced sharp boundaries. If the criminal justice count is 25 percent higher than the health count and undetermined-intent deaths equal 10 percent of recorded health homicides, parity requires $q^* = 2.5$. The value is infeasible, so even complete reallocation would not erase the gap. When undetermined deaths equal 50 percent of health homicides, the same ratio reaches parity at $q^* = 0.50$. A 10 percent recording excess is more fragile and reaches parity at $q^* = 0.40$ when undetermined deaths equal one quarter of health homicides.

Classification uncertainty matters through its size relative to the observed gap. A large undetermined category does not automatically invalidate the health series, and a small category does not automatically make the result robust. The informative quantity is how much reallocation is required to cross the interpretive boundary. A q^* close to zero marks a fragile conclusion because only limited reclassification is needed. A value above one means that even complete reassignment of the observed undetermined category cannot reverse the result. Andersson and Kazemian (2018) warned that unclassified deaths can affect homicide comparisons, while Lachaud et al. (2018) show that undetermined violent deaths are heterogeneous. The boundary approach respects both findings without inventing a corrected homicide count.

The documentary and simulation results narrow the meaning of politics of measurement. A lower count does not prove concealment. The term refers instead to institutional authority over classification, revision, publication, and the rules that decide which agency owns the authoritative statistical version of a death. India shows how international harmonisation can cross domestic legal categories, while the United States metadata show how a reporting-system transition can create a temporal break. WHO documentation places mortality counts inside national certification and registration processes. These arrangements can alter statistical visibility without any change in lethal behaviour. They are politically consequential because they distribute classificatory authority, but claims about deliberate suppression still require evidence independent of the numerical discrepancy.

The framework differs from the nearest recent literature in where it places the identification problem. Jiang et al. (2026) explain a broad criminal justice and health homicide gap through political capacity, health capacity, and globalisation. Buil-Gil et al. (2026) show that counting rules and legal definitions matter in Europe. The present study asks first whether the observations are independent institutional products, whether their constructs are sufficiently comparable, and whether the divergence measure behaves coherently under count noise and classification uncertainty. The answer is conditional rather than universal. That restraint parallels Indonesian work using recorded homicide as an administrative indicator rather than a direct measure of all lethal events (Kadir, 2026j).

The simulations do not estimate the real distribution of homicide undercount across countries. They test properties of the proposed measurement strategy under known data-generating conditions. The documentary audit shows that provenance contamination and time-series breaks are possible and observable, but it does not replace country-level source reconstruction. Aggregate equality cannot establish victim-level concordance, and two systems can miss the same deaths. The framework is therefore a disciplined route to a later provenance-verified longitudinal analysis rather than a claim that the true number of homicides has already been recovered.

CONCLUSION

Homicide statistics do not become comparable merely because they count deaths. The official source audit showed that WHO mortality observations have a clear civil-registration origin, whereas UNODC national homicide data can originate in criminal justice or public-health systems. That finding makes provenance a substantive part of cross-system comparison. Official metadata also document definitional harmonisation and time-series breaks that can alter interpretation before any regression is fitted. The state may therefore count the same lethal event through different institutional rules, and international repositories do not automatically remove those differences.

The Monte Carlo results supported the System Recording Differential as a relative measure of that divergence. Its mean remained close to the imposed log ratio across a wide range of baseline homicide rates, while the ordinary absolute rate gap expanded mechanically with the baseline. The persistence experiment showed that noisy observed ratios can sharply attenuate serial dependence, giving a specific reason to model the differential as a latent dynamic quantity. Tipping-point analysis added a separate check by showing exactly how much reclassification of undetermined deaths would be needed before an interpretation changes.

These results do not identify a true homicide count, and they do not establish political manipulation. They provide a stricter measurement architecture for asking when criminal justice and public-health statistics are independent, comparable, and robust enough to support institutional explanation. That restraint matters because homicide is frequently used as an outcome in research on inequality, governance, culture, and state capacity. Explanations of lethal violence are stronger when the statistical translation from death to homicide is examined before the resulting number is treated as a stable fact.

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