SIMURGA
Journal of Islamic Studies and Muslim Societies
Simurga: Journal of Islamic Studies and Muslim Societies
Vol. 1, No. 1, 2026
E-ISSN: XXXX-XXXX
punggawajournal.com/simurga

Does Islam Explain Homicide? A Paired Cross-National Comparison of Religion, State Capacity, and Lethal Violence

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

Article Info

Article history:
Received July 10th, 2026
Revised July 25th, 2026
Accepted July 29th, 2026

Keyword:
Homicide; Islam; Muslim-majority countries; State capacity; Statistical visibility.

ABSTRACT

Existing cross-national studies have not resolved whether lower homicide rates reported in some Muslim-majority countries reflect religion, institutional conditions, the composition of comparison groups, or uneven death registration. This study examined whether Muslim-majority status provided a stable explanation for recorded homicide when countries were compared with broadly similar cases and administrative capacity was considered. It used descriptive comparative research based on secondary data and a focused literature review. Six countries were organised into two core pairs and one sensitivity pair and examined through homicide rates, Muslim population share, Government Effectiveness, conflict status, and death-registration completeness. The evidence produced no uniform religious pattern. Albania recorded a higher homicide rate than North Macedonia, whereas the Kyrgyz Republic recorded a lower rate than Moldova. The Jordan-Armenia comparison was too affected by conflict to support a firm inference. Government Effectiveness did not follow homicide consistently across the core pairs, while mortality-registration data could not show how accurately killings entered national records. The findings did not establish Islam as an independent explanation of homicide. Muslim-majority status remained dependent on the selected comparator and on the institutions through which violent deaths were classified and recorded.

INTRODUCTION

Global homicide does not follow a clear religious boundary. The United Nations Office on Drugs and Crime estimated that about 458,000 people were killed intentionally in 2021, yet the burden was concentrated in particular regions rather than divided neatly between Muslim and non-Muslim societies (United Nations Office on Drugs and Crime [UNODC], 2023). Several Muslim-majority countries report comparatively low homicide, while others face political fragmentation or weak public administration. Cross-national evidence also shows that broad cultural categories do not map onto lethal violence in a uniform way (Lin & Mancik, 2020). The comparison is complicated further because homicide statistics are produced under different institutional conditions. A low recorded rate can indicate less lethal violence, but it can also occur where deaths are poorly investigated or incompletely registered. Religious composition therefore cannot be read apart from the countries used as comparators or from the institutions that make violent death visible in official data.

Religion remains relevant because religious institutions can shape social regulation, although their influence varies with local context (Thomson, 2021). Alcohol offers one concrete example. Stronger alcohol-control policies have been associated with lower alcohol-attributable homicide across a large international sample (Trangenstein et al., 2021), but neither alcohol regulation nor religious norms operate in the same way across Muslim-majority societies (Kadir, 2025a). Religious authority

may be closely connected to public institutions in one country and largely social in another. A Muslim majority is therefore better treated as a demographic condition than as a direct cause of violence.

Muslim population share also measures something narrower than religiosity. Research distinguishes belief, practice, and religious social environment rather than treating Muslim religiosity as one attribute (Sahin & Unlu, 2021). Pew Research Center provides comparable estimates of national religious composition for 2010 and 2020, which makes cross-national classification possible (Hackett et al., 2025). Even within Muslim populations, education, religiosity, and development are associated with different attitudes toward violent practices (Gullickson & Ahmed, 2021), while private practice, social practice, and fundamentalism can relate differently to support for lethal violence (Beller et al., 2021). This study therefore uses Muslim population share and a 50 percent majority threshold only as national demographic indicators. The unit of inference remains the country, not the individual believer.

Recent research provides little basis for a single religious account of lethal violence. Religion can support restraint in some settings and become connected with violence under different moral and group conditions (Cousar et al., 2021). Homicide also varies with inequality and other structural conditions that do not sit at the same analytical level as religion (Kuznar & Day, 2021). The national label can therefore carry information that partly belongs to the wider setting in which it is measured. This problem is especially relevant for Muslim-majority countries because they are concentrated in particular regions and reflect different histories of legal and state development (Kadir, 2017, 2025b). The non-Muslim category is even more heterogeneous. A global contrast may consequently say as much about the selected comparison cases as about religion itself.

The difficulty begins before an outcome is interpreted. Countries can be placed in the same regression even when their income, population structure, regional history, or exposure to conflict differ sharply. Statistical adjustment does not necessarily make such comparisons substantively plausible. The present study therefore uses paired descriptive comparison rather than treating the entire non-Muslim world as a single reference category. Regional proximity and broad structural similarity are used to reduce obviously distant comparisons, while the analysis remains explicit about cases that cannot support a firm inference. The purpose is not to reproduce statistical matching, but to make the comparison itself visible and contestable.

Administrative capacity offers a more concrete institutional explanation than a broad appeal to culture. Public agencies must be able to implement policy and deliver public goods beyond formal legal commitments, which is why state capacity is commonly treated as a substantive feature of governance (Shen, 2023). Hanson and Sigman (2021) further distinguish administrative capacity from extractive and coercive capacity. This study focuses on the administrative dimension because both homicide prevention and the routine production of official records depend on organisational competence. Government Effectiveness is used as a broad indicator of that context, but it is not treated as a direct measure of police performance, forensic capacity, or homicide investigation.

Recorded homicide is itself partly an institutional product. Comparative work using Indonesian and global homicide data has shown how official patterns depend on the way lethal events enter administrative datasets (Kadir, 2026a, 2026b). Even established mortality sources contain linkage and classification problems that matter for criminological inference (Tostlebe et al., 2021), and medicolegal death-investigation systems differ in their ability to produce reliable cause and manner-of-death statistics (Joos et al., 2021). Recent cross-national evidence also finds systematic discrepancies between criminal-justice and health homicide counts in relation to institutional capacity (Jiang et al., 2026). Strong institutions may therefore reduce violence while also producing more complete records of the violence that remains. A low official rate cannot automatically be treated as evidence of a successful religious or institutional restraint.

The dependent variable is consequently understood as recorded intentional homicide rather than the full amount of lethal violence that occurred. A killing becomes an observation only after it reaches institutions that identify and classify the death (Kadir, 2026c). Mortality research shows that cause-of-death data remain sensitive to completeness and diagnostic quality even when deaths are formally registered (Pandey & Adair, 2022). Registration completeness is itself an estimated quantity that varies across countries and years (Fúquene-Patiño & Adair, 2025), while global assessments distinguish the completeness of registration from the usability of cause-of-death information (Adair et al., 2023). Death-registration completeness is therefore used only as a contextual indicator of the mortality-data environment, not as a direct estimate of homicide underreporting. The same caution applies to culturally

embedded forms of lethal violence whose social visibility may differ from their formal legal classification (Kadir, 2026d).

Case selection is kept deliberately narrow. Economic position matters because homicide varies with economic conditions alongside other structural factors (Ferguson & Smith, 2021), so countries in each core pair are drawn from broadly similar World Bank income groups. Population size and urbanisation are also considered to avoid comparisons between cases with very different social scales. Urbanisation is not assumed to raise or reduce homicide uniformly because its relationship with lethal violence changes across time and national context (Clement et al., 2023). Armed conflict is treated separately from ordinary intentional homicide. Uppsala Conflict Data Program records are used to identify conflict exposure during 2013 to 2019, which is why Armenia is retained only in a sensitivity comparison with Jordan. Regional proximity serves the same practical purpose by reducing, without eliminating, large contextual differences.

The analytical model remains simple. Muslim-majority status is the principal exposure and the mean recorded homicide rate for 2013 to 2019 is the outcome. Income group, population size, urbanisation, regional location, and conflict status guide the selection or qualification of the country pairs. Government Effectiveness is examined as a broad institutional correlate, while death-registration completeness indicates the general strength of the mortality-data environment. Other plausible mechanisms, including firearm regulation or family structure, are not added when comparable data are unavailable. The design therefore asks a limited question rather than attempting to explain every source of cross-national homicide variation.

The urgency of the inquiry lies in the ease with which religious labels can shape interpretations of crime even when the underlying comparisons are weak. Research on religious perceptions of crime shows that religious affiliation and fundamentalism can influence attribution and punitive attitudes (Seto & Said, 2022). The same risk appears when low homicide is treated as evidence that Islam restrains violence or when higher homicide is read as evidence of religious violence. This study therefore aims to assess whether Muslim-majority status remains a meaningful explanation of recorded homicide when countries are compared with broadly similar cases, administrative effectiveness is considered, and mortality-data quality is examined. Its contribution is deliberately limited. It does not decide whether Islam is peaceful or violent. It identifies the point at which a religious explanation becomes weaker than the institutional and comparative conditions surrounding the data.

RESEARCH METHODS

This study uses descriptive comparative research and a focused literature review. The country is the unit of analysis. Comparative case research is useful when the objective is to examine the contextual limits and applicability of criminological claims rather than estimate a global average effect (Light & Singh, 2022). Six countries are selected through purposive sampling, a strategy appropriate when case selection is deliberately aligned with the research aim (Campbell et al., 2020). Albania and North Macedonia form the first core pair, while the Kyrgyz Republic and Moldova form the second. Jordan and Armenia are retained as a sensitivity pair because conflict affects Armenia during the observation period. The core countries are selected from the same broad region and from broadly similar World Bank income groups. Population size and urbanisation are also considered so that the cases are not too different in social scale. Homicide rates are not used as a criterion when the cases are selected. The design remains descriptive and comparative and does not estimate a causal effect of religion.

Data are collected through document analysis of open international datasets and academic literature. Muslim population share is obtained from Pew Research Center estimates for 2010. Intentional homicide is measured through the UNODC series available through the World Bank and calculated as the mean annual rate per 100,000 inhabitants from 2013 to 2019. Administrative effectiveness is represented by the average Government Effectiveness score from 2010 to 2012. The completeness of death registration with cause-of-death information is used to assess the general quality of mortality reporting. Population, income group, and urbanisation are taken from the World Development Indicators, while conflict status is checked through UCDP. Relevant literature is identified through Google Scholar and OpenAlex.

The analysis is conducted in two stages. Each core pair is first compared descriptively through Muslim population share, average homicide rate, Government Effectiveness, and death-registration completeness. The comparison considers whether differences in homicide are accompanied by

differences in administrative effectiveness or data quality. The sensitivity pair is used only to examine how the pattern changes when conflict complicates the observation period. Findings across the cases are then compared through thematic analysis of the supporting literature, an approach suited to identifying patterned meaning without reducing interpretation to frequency counts (Braun & Clarke, 2021). Numerical differences are used only to describe the selected countries and are not treated as statistically significant effects. The conclusions are limited to patterns found in the selected cases and do not generalise about Islam or individual Muslim behaviour.

RESULTS AND DISCUSSION

1. Muslim-Majority Status and the Limits of Religious Explanation

Albania unsettles the expectation that Muslim-majority status should coincide with lower homicide. Its calculated mean recorded rate for 2013 to 2019 was about 2.91 per 100,000 people, compared with 1.31 in North Macedonia. The absolute difference was 1.60, while Albania's rate was a little more than twice that of its neighbour. North Macedonia also complicates a clean religious contrast because Muslims formed about 39.3 percent of its population in 2010. It was not a Muslim-majority country under the study's threshold, although Islam was hardly peripheral to its social setting. Albania, by comparison, had a clear Muslim majority. The pair therefore does not contrast a Muslim-majority setting with a society in which Islam is socially absent. Recent review evidence on Muslim religiosity and delinquency similarly shows that measurement and national context complicate simple Muslim/non-Muslim comparisons (Mohammad & Banse, 2023). Even under this narrower comparison, demographic predominance did not produce the lower homicide rate.

Nothing in that result establishes that Muslim predominance raised homicide in Albania. The data identify a national difference, not the process that produced it. Religious practice is not measured, and the figures do not reveal how much authority religious organisations exercised in either country. Albania is useful for a more limited reason in this study. A large Muslim majority can coexist with the higher recorded rate inside a plausible regional pair. A demographic label cannot carry the explanation on its own.

The Kyrgyz Republic and Moldova move in the other direction. Their calculated means were about 3.89 and 5.29 per 100,000 respectively, leaving an absolute difference of 1.40 and a Moldovan rate around 36 percent higher. The comparison does not show whether religion mattered through religious practice or informal social control. Evidence for either pathway is absent from the national indicators used here. Nor does it identify how either mechanism might operate. The pair establishes only that the lower rate occurred on the Muslim-majority side. It gives that proposition some descriptive support, although the Albanian comparison prevents it from becoming a stable cross-case pattern.

Reading the three comparisons as a vote count would be misleading. Jordan and Armenia initially appeared to add another lower-rate case for the Muslim-majority country, but Armenia cannot be treated as an uncomplicated core comparison under the article's own design. UCDP records an escalation of the Nagorno-Karabakh conflict in 2016, which falls inside the observation period. That exposure may affect ordinary violence. It can also complicate the classification and reporting of deaths. The pair is therefore retained only as a sensitivity case and does not determine the main inference. Its calculated means were roughly 1.53 in Jordan and 2.40 in Armenia. The absolute difference was 0.87 per 100,000, while Armenia's rate was about 57 percent higher. Reporting both measures avoids calling the same gap small in one place and large in another. It also prevents the comparison from being judged through whichever statistic best suits the argument. The conflict qualification matters more. A convenient third case should not be used as clean confirmation of a religious pattern when it fails one of the study's own selection rules.

Table 1 Pairwise Homicide Patterns in Selected Countries

Pair	Muslim-majority country	Comparator	Observed direction
Balkan core pair	Albania 2.91	North Macedonia 1.31	Higher in the Muslim-majority country

Pair	Muslim-majority country	Comparator	Observed direction
Post-Soviet core pair	Kyrgyz Republic 3.89	Moldova 5.29	Lower in the Muslim-majority country
Sensitivity pair	Jordan 1.53	Armenia 2.40	Lower in the Muslim-majority country, but conflict limits inference

Note. Homicide figures are mean annual intentional homicide rates per 100,000 people for 2013–2019. The Jordan–Armenia comparison is retained only as a sensitivity case.

As summarised in Table 1, the two core pairs move in opposite directions. Muslim-majority status therefore provides no stable ordering of recorded homicide in the selected cases, although this does not make religious context irrelevant. Albania and the Kyrgyz Republic differ despite both having large Muslim majorities. Their counterparts are not equivalent either, especially because North Macedonia contains a substantial Muslim minority. What remains is a descriptive pattern without an identified religious mechanism. The lower Kyrgyz rate deserves explanation, but so does the higher Albanian rate. Neither can be assigned to Islam from population shares alone. Administrative effectiveness and the quality of homicide recording now become necessary parts of the comparison.

2. State Capacity and the Visibility of Homicide

Moldova complicates any simple institutional account. Its average Government Effectiveness score for 2010 to 2012 was higher than that of the Kyrgyz Republic, yet Moldova also recorded the higher mean homicide rate during 2013 to 2019. Albania and North Macedonia produced a different problem. Their administrative effectiveness scores were almost the same, while their homicide rates were not. North Macedonia recorded the lower rate despite having no clear advantage in the WGI estimate. Broader cross-national research finds that economic governance is related to homicide, but the relationship is not reducible to one institutional indicator (de Soysa, 2021). Government Effectiveness is a broad national measure, and similar aggregate scores may conceal differences in policing or local implementation. The paired evidence is therefore more limited. Administrative effectiveness, as measured here, did not move consistently with recorded homicide.

That limitation belongs partly to the measure itself. Government Effectiveness captures perceptions of public services, bureaucratic quality, and policy implementation. It does not directly observe homicide investigation, forensic practice, or coordination between police and health agencies. Comparative work on state-capacity measurement warns that indicators that appear similar at the aggregate level can disagree substantially for particular countries (Vaccaro, 2023). Treating WGI as a complete measure of state capacity would therefore stretch the indicator beyond its design. In this study, it is better read as a broad administrative context that may shape homicide control, not as a direct measure of the institutions that respond to lethal violence.

Mortality registration adds another layer of uncertainty. Albania had much lower registration completeness than North Macedonia, yet Albania still reported the higher homicide rate. In the Kyrgyz Republic and Moldova, registration coverage was relatively high in both countries, while the lower homicide rate appeared in Kyrgyzstan. These patterns do not support the simple assumption that weaker mortality systems automatically produce lower homicide figures. The registration figures also do not prove that homicide was equally well recorded in every country. Death registration measures whether deaths and their causes enter the civil registration system. Homicide statistics may also depend on police records and legal classification, which follow a different administrative route. The indicator therefore says more about the general quality of mortality data than about the number of killings that may have been missed. Where registration is relatively complete, uncertainty remains because completeness alone says little about the accuracy of classifying intent.

Armenia cannot serve as a clean test of statistical visibility because armed conflict affected the observation period. Its mortality registration was more complete than Jordan's, and its recorded homicide rate was also higher. That pattern could fit an observability argument, but conflict offers another explanation and may disrupt the institutions that classify deaths. The pair is therefore useful

only as a warning against easy inference. It cannot confirm that better recording produced the higher rate, nor can it show how much conflict altered ordinary homicide statistics.

Table 2 Administrative Effectiveness and Mortality-Data Context

Country pair	Government Effectiveness	Death-registration completeness	Interpretive point
Albania / North Macedonia	−0.29 / −0.32	53% (2010) / 100% (2010 and 2013)	Similar administrative scores accompanied different homicide rates
Kyrgyz Republic / Moldova	−0.84 / −0.58	96% (2012) / 90% (2012)	The stronger administrative score accompanied the higher homicide rate in Moldova
Jordan / Armenia	0.14 / −0.04	56% (2015) / 100% (2016)	Conflict prevents a clean inference from the sensitivity pair

Note. Government Effectiveness values are three-year averages for 2010–2012.

Table 2 helps explain why the observability question remains unresolved. Administrative effectiveness did not follow the same direction as homicide across the two core pairs, while mortality registration could not identify how many killings entered criminal justice records correctly. The comparison exposes a measurement limit. Recorded homicide depends on institutions only partly captured by broad international indicators. The proposed state-capacity observability problem should therefore remain an interpretive caution rather than a finding. National homicide rates may reflect both violence and the ability to document it, but the present data cannot separate those processes with confidence. The stronger conclusion is narrower. Muslim-majority status and administrative effectiveness provide incomplete accounts unless the institutions producing homicide statistics are examined more directly.

3. What Paired Comparison Changes in Cross-National Homicide Research

Albania and the Kyrgyz Republic point in different directions. Albania recorded the higher homicide rate in its pair, while the Kyrgyz Republic recorded the lower one. Muslim-majority status was therefore not associated with the same pattern across the selected cases. North Macedonia and Moldova also represent very different alternatives to Muslim predominance. One is a religiously mixed Balkan society with a substantial Muslim minority. The other emerged from a different post-Soviet institutional history. Treating both countries as members of a single non-Muslim category hides those differences before any comparison begins. The labels still help classify cases, but they do not describe a common institutional setting or a shared route into lethal violence. The paired cases do not show that a narrower comparison changes a previously estimated global result, because no global estimate was calculated here. They show something more limited. The direction of the homicide difference depended on which country served as the comparator. Albania weakens a broad lower-homicide claim. Kyrgyzstan prevents the opposite conclusion. Jordan and Armenia cannot resolve the issue because armed conflict makes that comparison less secure.

Choice of comparator shapes how the observed difference is read. A country may appear relatively safe against one reference group and less distinctive against another. This is not a defect unique to studies of religion. Comparative findings always depend partly on the cases included in the contrast. The problem becomes sharper when the reference category combines countries with very different institutions and histories. Muslim-majority status can then appear more informative than it really is, simply because the countries on the other side of the comparison are too diverse to form a meaningful alternative. The contrast may remain visible in the data while being difficult to interpret in social terms.

Albania alone is enough to block a universal claim that Muslim-majority countries have lower homicide. It cannot establish the reverse proposition. The Kyrgyz case works in much the same way.

Small-N comparison is useful here because disagreement between cases remains visible rather than being absorbed into one average. That function is limited, but it matters. A case that refuses the expected pattern often reveals more about the weakness of a general claim than several cases that seem to confirm it.

Religious composition is easy to measure, while the historical and institutional setting behind it is not. A national percentage can therefore stand in for conditions that the dataset does not observe. Long-run state formation is one such condition and has been linked to contemporary homicide differences across countries (Gerring & Knutsen, 2022). Even administrative effectiveness did not produce a stable ordering in the present study. Government Effectiveness moved differently from homicide across the two core pairs, and mortality registration could not show how accurately killings entered national records. The difficulty is not solved by replacing religion with one broad institutional indicator. Similar scores may coexist with different homicide levels. Those unresolved differences should remain unresolved. Careful comparison narrows the claim, but it does not justify assigning what the data fail to explain to religion. An unexplained gap is still an unexplained gap.

The six countries examined here do not represent Muslim-majority societies as a whole. Their value lies in showing how quickly a broad religious conclusion becomes unstable once the cases are read more closely. A larger study could test whether the same instability persists under stricter case comparability and alternative reference groups. For the present cases, neither Muslim-majority status nor broad administrative effectiveness arranged homicide in a consistent order. The comparison itself also remained constrained by uneven mortality data. Religious composition may still matter, but these findings do not establish how. It should be treated as an explanation only after the comparator and the institutions recording violent death have been examined.

CONCLUSION

Muslim-majority status did not provide a stable explanation for recorded homicide across the selected country pairs. Albania and the Kyrgyz Republic moved in opposite directions, while the Jordan and Armenia comparison remained too affected by conflict to support a firm inference. Government Effectiveness also failed to follow homicide consistently, and the available mortality-registration data could not show whether differences in recorded killing reflected violence, reporting quality, or both. Islam, understood here only as Muslim demographic predominance, therefore did not explain homicide in a uniform way. The findings do not make religion irrelevant, but they show that its meaning cannot be separated from the institutional setting in which violence is prevented, classified, and recorded.

The study also shows why the choice of comparator matters in cross-national homicide research. A religious category can appear more or less important depending on the country placed beside it, while broad administrative indicators may hide important differences in policing and death classification. These conclusions are limited to six countries and do not estimate a causal effect of religion. Their value lies in showing that an unexplained difference should not be assigned to Islam simply because other measured factors are incomplete. Religious composition can be treated as a serious explanation only after countries are compared on plausible terms and the institutions producing homicide data are examined.

REFERENCES

Adair, T., Mikkelsen, L., Hooper, J., Badr, A., & Lopez, A. (2023). Assessing the policy utility of routine mortality statistics: a global classification of countries. *Bulletin of the World Health Organization*, *101*(12), 777–785. https://doi.org/10.2471/BLT.22.289036

Beller, J., Kröger, C., & Hosser, D. (2021). Disentangling Honor-Based Violence and Religion: The Differential Influence of Individual and Social Religious Practices and Fundamentalism on Support for Honor Killings in a Cross-National Sample of Muslims. *Journal of Interpersonal Violence*, *36*(19–20), 9770–9789. https://doi.org/10.1177/0886260519869071

Braun, V., & Clarke, V. (2021). One size fits all? What counts as quality practice in (reflexive) thematic analysis? *Qualitative Research in Psychology*, *18*(3), 328–352. https://doi.org/10.1080/14780887.2020.1769238

Campbell, S., Greenwood, M., Prior, S., Shearer, T., Walkem, K., Young, S., Bywaters, D., & Walker, K. (2020). Purposive sampling: complex or simple? Research case examples. *Journal of Research in Nursing*, *25*(8), 652–661. https://doi.org/10.1177/1744987120927206

Cousar, K. A., Carnes, N. C., & Kimel, S. Y. (2021). Morality as Fuel for Violence? Disentangling the Role of Religion in Violent Conflict. *Social Cognition*, *39*(1), 166–182. https://doi.org/10.1521/soco.2021.39.1.166

de Soysa, I. (2021). Economic governance and homicide: Some theory and empirics, 1990–2017. *Journal of Peace Research*, *58*(5), 1004–1017. https://doi.org/10.1177/0022343320962566

Ferguson, C. J., & Smith, S. (2021). Examining homicides and suicides c <scp>ross-nationally</scp> : Economic factors, guns and video games. *International Journal of Psychology*, *56*(5), 812–823. https://doi.org/10.1002/ijop.12760

Fúquene-Patiño, J., & Adair, T. (2025). A Bayesian approach to estimate the completeness of death registration. *Statistical Journal of the IAOS*, *41*(2), 421–428. https://doi.org/10.1177/18747655251335762

Gerring, J., & Knutsen, C. H. (2022). Homicide and State History. *American Sociological Review*, *87*(6), 950–980. https://doi.org/10.1177/00031224221131758

Gullickson, A., & Ahmed, S. (2021). The role of education, religiosity and development on support for violent practices among Muslims in thirty-five countries. *PLOS ONE*, *16*(11), e0260429. https://doi.org/10.1371/journal.pone.0260429

Hackett, C., Stonawski, M., Tong, Y., Kramer, S., Shi, A. F., & Zanetti, N. (2025). Religious composition by country, 2010–2020 [Data set]. *Pew Research Center*.

Hanson, J. K., & Sigman, R. (2021). Leviathan's Latent Dimensions: Measuring State Capacity for Comparative Political Research. *The Journal of Politics*, *83*(4), 1495–1510. https://doi.org/10.1086/715066

Jiang, B., Santos, M. R., & LaFree, G. (2026). Mind the Gap: Institutional Capacity, Globalization, and the Discrepancy Between Criminal Justice and Health Homicide Data. *Justice Quarterly*, 1–28. https://doi.org/10.1080/07418825.2026.2623928

Joos, O., Mrkic, S., & Sferrazza, L. (2021). Legal Frameworks: A Starting Point for Strengthening Medicolegal Death Investigation Systems and Improving Cause and Manner of Death Statistics in Civil Registration and Vital Statistics Systems. *Academic Forensic Pathology*, *11*(2), 103–111.

Kadir, Z. K. (2017). Honor Killing dan Modernisasi Hukum Pidana di Berbagai Negara Muslim. *Jurnal Pusaka*, *5*(2), 269–279.

Kadir, Z. K. (2025a). Kejahatan atau Kebijakan? Membongkar Bias Ideologis dalam Realisme Kanan dan Kiri di Era Neo-Liberalisme. *Jurnal Intelek Dan Cendikiawan Nusantara*, *1*(6), 11146–11161.

Kadir, Z. K. (2025b). Kriminalisasi Nilai Moral: Konstruksi Kebijakan Kriminal di Negara Muslim Modern. *Jurnal El-Thawalib*, *6*(1), 71–84.

Kadir, Z. K. (2026a). Beyond Firearms: Mapping Global Patterns of Non-Firearm Homicide in Comparative Criminological Perspective. *Punggawa Global Research: Jurnal Multidisiplin*, *1*(2), 1–10.

Kadir, Z. K. (2026b). Mapping Recorded Homicide in Indonesia: An Ecological Criminological Analysis Using BPS Data. *Punggawa Law Review*, *1*(2), 9–18.

Kadir, Z. K. (2026c). Narrative Closure in Honor killing Cases: How Judgments Stabilise Meaning, Eliminate Ambiguity, and Produce Sentencing Certainty. *Punggawa Law Review*, *1*(1), 1–10.

Kadir, Z. K. (2026d). Rural Criminology and the Cultural Authorization of Homicide: How Lethal Violence Becomes Socially Permissible. *Punggawa Social Inquiry: Journal of Crime, Culture, and Social Dynamics*, *1*(1), 1–10.

Kuznar, L. A., & Day, J. (2021). Homicide, Inequality, and Climate: Untangling the Relationships. *Frontiers in Psychology*, *12*. https://doi.org/10.3389/fpsyg.2021.697126

Light, M., & Singh, A.-M. (2022). Introduction to Special Issue on comparative criminology: Context, scope and applicability in critical criminological research. *Theoretical Criminology*, *26*(4), 525–536. https://doi.org/10.1177/13624806221134310

Lin, K., & Mancik, A. M. (2020). National Culture on the Cross-National Variation of Homicide: An Empirical Application of the Inglehart–Welzel Cultural Map. *Sociological Forum*, *35*(4), 1114–1134. https://doi.org/10.1111/socf.12640

Mohammad, T., & Banse, R. (2023). Muslim Religiosity and Juvenile Delinquency: A Systematic Review. *Adolescent Research Review*, *8*(4), 507–520. https://doi.org/10.1007/s40894-023-00206-y

Pandey, S. P., & Adair, T. (2022). Assessment of the national and subnational completeness of death registration in Nepal. *BMC Public Health*, *22*(1), 429. https://doi.org/10.1186/s12889-022-12767-z

Sahin, I., & Unlu, A. (2021). The Effect of Muslim Religiosity on Youth Delinquency. *Criminal Justice Review*, *46*(3), 361–381. https://doi.org/10.1177/0734016820966038

Seto, C. H., & Said, I. (2022). Religious perceptions of crime and implications for punitiveness. *Punishment & Society*, *24*(1), 46–68. https://doi.org/10.1177/1462474520960038

Shen, J. (2023). State capacity, political competition and public goods provision: A cross-national comparative study. *Asian Journal of Comparative Politics*, *8*(2), 620–636. https://doi.org/10.1177/20578911231161248

Thomson, R. A. (2021). Religion in Civil Society: The Influence of Black Religious Ecology on Crime in the South. *Journal of Quantitative Criminology*, *37*(1), 73–99. https://doi.org/10.1007/s10940-019-09444-7

Tostlebe, J. J., Pyrooz, D. C., Rogers, R. G., & Masters, R. K. (2021). The National Death Index as a Source of Homicide Data: A Methodological Exposition of Promises and Pitfalls for Criminologists. *Homicide Studies*, *25*(1), 5–36. https://doi.org/10.1177/1088767920924450

Trangenstein, P. J., Peddireddy, S. R., Cook, W. K., Rossheim, M. E., Monteiro, M. G., & Jernigan, D. H. (2021). Alcohol Policy Scores and Alcohol-Attributable Homicide Rates in 150 Countries. *American Journal of Preventive Medicine*, *61*(3), 311–319. https://doi.org/10.1016/j.amepre.2021.03.020

Vaccaro, A. (2023). Measures of state capacity: so similar, yet so different. *Quality & Quantity*, *57*(3), 2281–2302. https://doi.org/10.1007/s11135-022-01466-x