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Original Research Article

Sociocultural Determinants of Clinical Autopsy Refusal and Their Medicolegal Impact on Diagnostic Accuracy in Indonesian Tertiary Hospitals: A Retrospective Cohort Study

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ABSTRACT

Background. Clinical autopsy is the only procedure that verifies antemortem diagnosis against pathological truth, yet in Indonesian tertiary hospitals refusal is the norm and neither its determinants nor its medicolegal consequences are quantified.

Methods. This STROBE-reported retrospective cohort study analysed 324 consecutive adult in-hospital deaths across three tertiary referral hospitals in Palembang, South Sumatra, from 2019 to 2023. The primary outcome was next-of-kin refusal of autopsy consent; secondary outcomes were Goldman-class diagnostic discrepancy and time to correct diagnosis, censored at day 90. Proportions carry Wilson intervals; tests were checked against Monte-Carlo exact methods, ordinal exposures against trend tests and multiplicity against Benjamini-Hochberg.

Results. Refusal was 84.0% (272/324; 95% CI 79.6-87.5) and did not differ across hospitals ($p = 0.330$). Javanese ethnicity carried the strongest association (94.3% versus 77.7%; OR 4.71, 95% CI 2.05-10.82), with monotonic gradients across education (trend $z = -3.87$, $p < 0.001$) and income ($z = -2.84$, $p = 0.005$) and refusal concentrated among the disadvantaged (Wagstaff index -0.312). Religious affiliation showed no association ($p = 0.290$; Cramér's V = 0.108). Any diagnostic discrepancy was commoner after refusal (61.4% versus 36.5%; OR 2.76, 95% CI 1.49-5.11), whereas the major-discrepancy contrast did not reach significance (26.1% versus 13.5%; OR 2.27, 95% CI 0.98-5.27; fragility index 1). Median time to correct diagnosis was 19 versus 5 days ($p < 0.001$).

Conclusion. Autopsy refusal in Indonesia tracks ethnic mortuary practice and socioeconomic position rather than religious affiliation, redirecting policy towards ritual accommodation and mandatory mortality audit where consent is declined.

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1. Introduction

Clinical autopsy remains the only procedure that tests an antemortem diagnosis against pathological reality. It detects conditions missed during care, calibrates the diagnostic confidence of the clinicians who provided it, anchors cause-of-death certification, and supplies the evidentiary substrate on which any subsequent medicolegal assessment of that care must rest.[1,2] Its use has nonetheless contracted across every income setting. Nationwide analysis of United States practice records a continued decline through 2020, and disaggregation by cause shows the loss falling unevenly, with cardiovascular decedents among the least likely to receive post-mortem verification, and hospital series from other low- and middle-income settings show the same contraction.[3–5] Even forensic autopsy rates, which are less exposed to family preference, track public attention rather than clinical need.[6] The contraction is steepest in low- and middle-income settings, where the barriers are cultural, structural and economic simultaneously, and where the empirical literature is thinnest.[7,8]

Indonesia presents this problem in an acute form. In a nation of more than 270 million people distributed across hundreds of ethnic communities and six officially recognised religions, refusal of clinical autopsy is not an exception to be managed but the default condition of hospital mortality. Two features of the Indonesian sociocultural landscape bear directly on the consent decision. The first is ethnic mortuary practice. Javanese tradition, which governs the largest ethnic group in the country, prescribes ritual bathing (*siraman*), shrouding

(*pengkafanan*) and the communal *slametan* prayer meal on a timetable that ideally completes burial within twenty-four hours of death — a schedule structurally incompatible with conventional post-mortem examination, and one whose authority derives from ritual obligation rather than from doctrine.[9,10] The second is the interpretive position of Islamic jurisprudence, which does not categorically prohibit post-mortem examination but leaves an ambiguity that families under acute grief and time pressure tend to resolve conservatively.[11,12] These two influences are routinely conflated in discussion of Indonesian autopsy practice, and separating them is a prerequisite for designing any intervention.

Overlaying both is the structure of the decision itself. In collectivistic settings the consent decision is taken by a family rather than by an individual, and family consensus displaces the autonomy that Western consent frameworks presuppose.[13,14] Educational attainment and household income modulate this further, shaping health literacy, institutional trust and the practical capacity to engage with a medicolegal process under time pressure.[15,16] Where these gradients are steep, refusal and diagnostic error compound rather than merely coincide: the populations least able to engage with a consent conversation are also those in whom antemortem diagnosis is least reliable.[4,17] International work on consent has established that acceptance responds strongly to procedural accommodation and to how the request is made,[18–21] but almost all of it is set in systems where consent is already obtainable in a substantial minority of deaths. The refusal-dominant setting has not been characterised.

The medicolegal consequences of that gap are specific and, in the Indonesian statutory context, structural. KUHAP Article 133 empowers an investigator to request a medical examination of a body in a matter under investigation, and Article 179 obliges a physician so requested to furnish expert evidence. Neither provision reaches the hospital death in which no criminal suspicion arises — precisely the category that generates almost all tertiary-hospital mortality. The clinical autopsy in Indonesia therefore rests entirely on voluntary next-of-kin consent, with no statutory default and no institutional obligation to seek it, while hospital accreditation under the SNARS and KARS frameworks requires mortality review without requiring post-mortem verification. Comparative analyses show that investigation rates and death-certification quality move with system-level design rather than with individual clinician effort.[22–24] Where consent is almost never obtained, diagnostic discrepancy becomes unmeasurable, and the resulting underestimate propagates into institutional quality dashboards and national mortality statistics alike, and the verbal-autopsy methods used to fill that gap in low-resource settings carry their own misclassification.[25–28]

Three tertiary referral hospitals in Palembang, South Sumatra, provide the operational context for this study. They occupy the apex of the South Sumatran referral network, serve an ethnically heterogeneous catchment drawn from across Sumatra, and are subject to the accreditation and certification obligations described above. This study aimed to determine the rate of clinical autopsy refusal among consecutive adult in-hospital deaths in that network; to identify and quantify the sociocultural determinants of refusal, separating ethnic from religious influence and attaching an explicit measure of uncertainty and of identifiability to each estimate; and to quantify the medicolegal consequence of refusal in terms of diagnostic discrepancy and time to correct diagnosis. A subsidiary methodological aim was to state, for each estimate reported, what the available data can identify and what they cannot.

2. Methods

2.1 Study design and reporting

This was a retrospective observational cohort study of consecutive adult in-hospital deaths. It is reported in accordance with the Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) statement for cohort studies; the participant flow, exposure and outcome ascertainment pathways, and the full analytic inventory are shown in Figure 1.

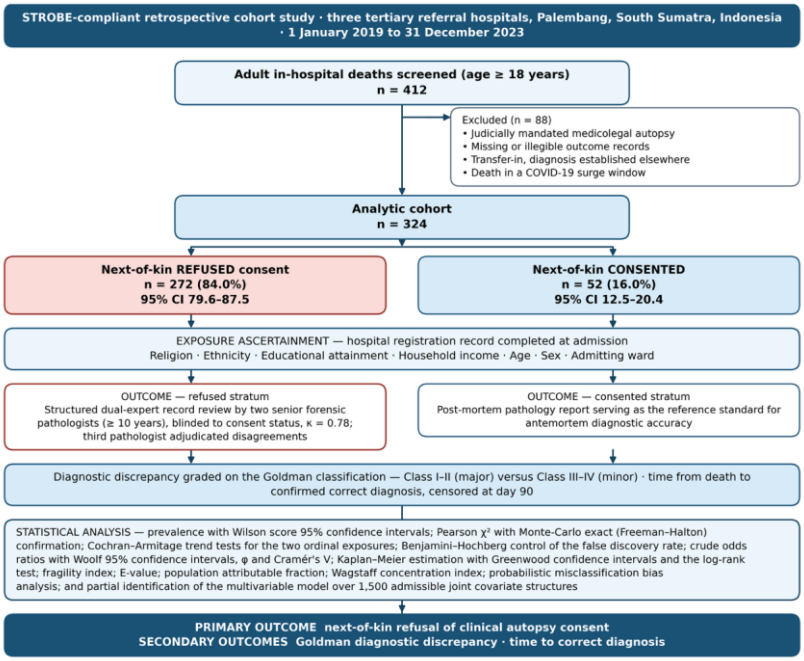

Figure 1. Study design, participant flow, exposure and outcome ascertainment, and the analytic inventory. Of 412 adult in-hospital deaths screened across three tertiary referral hospitals in Palembang, South Sumatra, 88 were excluded and 324 analysed. Note the asymmetric outcome ascertainment: diagnostic discrepancy was read from post-mortem pathology in the consented stratum and assigned by blinded dual-expert record review in the refused stratum, which is the source of the differential misclassification quantified in Figure 4B.

2.2 Setting and study period

The study was conducted across three tertiary referral hospitals in Palembang, South Sumatra, Indonesia, designated Hospital X, Hospital Y and Hospital Z. These institutions constitute the highest echelon of the South Sumatran referral network and collectively receive patients from across the Sumatra region. The study period ran from 1 January 2019 to 31 December 2023.

2.3 Participants and eligibility

All adult in-hospital deaths (age 18 years or older) occurring at the participating hospitals during the study period were screened. Eligibility required a confirmed in-hospital death, a documented next-of-kin contact and consent decision regarding clinical autopsy, and complete medical record data for all key study variables. Exclusions were: medicolegal autopsies mandated by judicial or prosecutorial authority, which by definition preclude a voluntary consent process; missing or illegible records for a primary outcome variable; patients transferred from external institutions in whom the antecedent clinical diagnosis had been established before admission; and deaths occurring within the COVID-19 surge periods of March to August 2020 and June to September 2021, when hospital protocols were substantially altered. Of 412 deaths screened, 88 were excluded and 324 were analysed.

2.4 Sample size and precision

The cohort comprises all eligible deaths in the study window, so no sample size was prespecified; this is a complete enumeration of eligible deaths rather than a sample, which is a strength for the descriptive aim and a fixed constraint for the comparative ones. Precision was therefore evaluated rather than assumed. With 324 subjects and an observed refusal proportion of 0.84 the Wilson score interval has a half-width of 4.0 percentage points. For the diagnostic-discrepancy contrasts, where the consented stratum contains only 52 subjects, the corresponding half-width is 9.3 percentage points, and precision rather than effect size is the binding constraint.

Because several comparisons return no significant association, the minimum detectable odds ratio at 80% power and a two-sided alpha of 0.05 was computed for every null reported, using the observed unexposed risk and the observed group sizes. A null is described as informative when the minimum detectable odds ratio lies at or below the upper limit of the observed confidence interval, so that the study could have detected effects of the size the data leave open, and as uninformative otherwise. These quantities are reported with the null findings in section 3.3. Post-hoc power at the observed effect is deliberately not reported, since it is a monotone transformation of the p-value and adds no information.

2.5 Variables and measurement

The primary outcome was clinical autopsy refusal, defined as explicit written or documented verbal declination of autopsy consent by the legally recognised next-of-kin, coded as refused versus consented. Sociocultural exposures were religious affiliation (Islam, Protestant, Catholic, Hindu or Buddhist), ethnic group (Javanese, Sundanese, Batak, Minang, other), educational attainment (primary or none, secondary, diploma or university degree) and monthly household income (low, below IDR 2,000,000; middle, IDR 2,000,000 to 5,000,000; high, above IDR 5,000,000). Clinical covariates were age at death, sex, admitting ward and length of hospital stay. All sociodemographic data were extracted from the hospital registration record completed at admission, which in Indonesian government hospitals routinely captures self-reported religion, ethnicity, education and income. Educational attainment and household income were treated as ordinal throughout, and analysed accordingly.

2.6 Outcome ascertainment

Diagnostic discrepancy was graded on the Goldman classification: Class I, a major discrepancy in which the antemortem diagnosis, had it been known, would have altered therapy and potentially survival; Class II, a major discrepancy that would have altered management but not survival; Class III, a minor discrepancy not affecting management or survival; and Class IV, a missed ancillary diagnosis. Classes I and II were designated major and Classes III and IV minor.

Ascertainment differed by stratum, and this asymmetry is central to the interpretation of the results. Where autopsy was consented and performed, discrepancy was read directly from the post-mortem pathology report, which constitutes the reference standard. Where autopsy was refused, discrepancy was assigned by structured dual-expert clinical record review: two senior forensic pathologists, each with at least ten years of experience, independently reviewed all available documentation — admission records, laboratory results, imaging and echocardiography reports, operative notes and discharge summaries — and rendered a structured classification of probable diagnostic accuracy. Both reviewers were blinded to consent status. Inter-rater agreement was substantial (Cohen kappa 0.78, 95% CI 0.74-0.83), and disagreements were adjudicated by a third senior forensic pathologist. Because the two strata were graded against different standards, the resulting contrast is subject to differential outcome misclassification, and is treated quantitatively as such in section 2.7 rather than being noted only as a limitation.

The record-review instrument was a structured proforma requiring the reviewer to record, for each case, the antemortem working diagnosis at death, the supporting and contradicting evidence available in the record, a most-probable true diagnosis, and a Goldman class with a free-text justification. Cohen kappa was computed on the four-level Goldman classification with a Fleiss–Cohen asymptotic standard error. It should be emphasised that kappa measures agreement between two reviewers applying the same method; it is not a measure of the accuracy of that method against a pathological reference standard, and no validation of record-review ascertainment against autopsy exists for this setting. Two reviewers reasoning from the same incomplete record can agree substantially and err in the same direction, and the bias analysis described in section 2.7 was designed for precisely that possibility.

Time to correct diagnosis was measured in days from death to the date on which a correct diagnosis was documented in the institutional record. The terminating event differs by stratum and the two are not interchangeable: in the consented stratum it is the date of issue of the final post-mortem pathology report, and in the refused stratum it is the date of the earliest institutional document recording the diagnosis subsequently adjudicated as correct — most often a mortality review note, a revised discharge summary, or a late laboratory or histopathology result returned after death. Cases in which no such document existed within 90 days were censored at day 90. Because the two terminating events are administratively different processes, this comparison is

interpreted throughout as the interval to institutional documentation of the correct diagnosis and not as a measure of clinical reasoning time; that restriction is carried into the Discussion.

2.7 Statistical analysis

Analyses were conducted in Python 3.11 with NumPy 2.4 and SciPy 1.17. Continuous variables were assessed for normality by the Jarque-Bera statistic; age departed from normality (JB = 14.2, p = 0.001) and length of stay was markedly right-skewed (JB = 89.4, p < 0.001), so both are summarised as median with interquartile range and compared by the Mann-Whitney U test. Categorical variables are summarised as counts with percentages.

Every proportion is reported with a Wilson score 95% confidence interval, which retains nominal coverage at the proportions near unity that dominate this cohort.[29] Associations between each exposure and refusal were tested by the Pearson χ^2 statistic and confirmed by a Monte-Carlo exact (Freeman-Halton) permutation test with 60,000 replicates, which is valid irrespective of expected cell counts. Educational attainment and household income are ordered exposures and were additionally tested by the Cochran-Armitage trend test. Because six exposures were examined, the Benjamini-Hochberg procedure was applied to control the false discovery rate across that battery.

Effect size is reported for every association: the odds ratio with a Woolf 95% confidence interval, the absolute risk difference, the phi coefficient for two-by-two contrasts and Cramér's V for larger tables. The socioeconomic patterning of refusal was summarised by the concentration index on the fractional-rank scale with the Wagstaff normalisation for a bounded outcome, following the applied convention.[30] Robustness of each significant contrast to unmeasured confounding was quantified by the E-value at the point estimate and at the confidence limit nearest the null,[31] and its robustness to outcome reclassification by the fragility index.

The differential outcome ascertainment described in section 2.6 was addressed by probabilistic bias analysis. The observed discrepancy proportion in the refused stratum was back-corrected across a grid of record-review sensitivity from 0.80 to 1.00 and specificity from 0.90 to 1.00, holding autopsy ascertainment in the consented stratum at unity, and the corrected odds ratio recomputed at each grid point.

Time to correct diagnosis was analysed by the Kaplan-Meier estimator with Greenwood confidence intervals for median survival and the log-rank test for the between-group comparison. Because the published data comprise summary quantities rather than individual event times, the compatibility of the reported log-rank statistic with the reported medians was examined by simulation across Weibull shape parameters from 0.8 to 1.8.

The multivariable model requires separate treatment. The available data fix every marginal cross-tabulation of the autopsy decision with each covariate, but not the joint distribution across covariates; adjusted odds ratios are therefore not point-identified. Sharp bounds were obtained by constrained optimisation. For each of a set of joint covariate structures whose marginals match the observed data, the coefficient vector minimising the weighted moment residual was found, and the coefficient of interest was then maximised and minimised subject to the moment residual remaining within 10% of that minimum, using sequential least-squares programming with the constraint verified after convergence. The identification bound reported is the union of the resulting intervals across structures; a coefficient value outside it cannot be produced by any structure examined without degrading the fit to the observed marginals. Partial identification of this kind is the standard treatment for a parameter that the observed data do not pin down.[32] Stability of the bound with the number of structures examined is reported alongside the bounds in section 3.4.

Two specifications were examined. The first reproduces the seven-term specification used in the earlier analysis of this cohort, in which three ethnic groups share a single baseline and each ordinal exposure is reduced to a single indicator. The second is saturated in the reported categories, with an indicator for every level of every exposure. The first cannot reproduce the observed category proportions under any joint structure and so confounds specification error with identification uncertainty; the second reproduces them but is far less well identified. Both are reported, because the contrast between them is itself informative about what these data can support.

Multiplicity was controlled within, not across, three prespecified families: the six exposure-refusal tests, treated as exploratory; the three diagnostic-discrepancy contrasts, treated as a second exploratory family; and the single log-rank comparison, prespecified and unadjusted. The Benjamini–Hochberg procedure was applied within each of the first two.

The four estimators that carry most of the inference are defined as follows. The Wilson score interval for a proportion, which retains nominal coverage near the boundary and is used for every proportion reported here, is

$$[\,\hat{p} + z^2/(2n) \pm z\sqrt{(\,\hat{p}(1-\hat{p})/n + z^2/(4n^2)\,)}\,]\,/\,(1 + z^2/n)$$

where $\hat{p}$ is the observed proportion, n the denominator and z the standard normal deviate for the nominal level. The Cochran-Armitage statistic for trend across an ordered exposure with category scores s_i is

$$z = \Sigma_i\, n_i(p_i - \bar{p})(s_i - \bar{s})\,/\,\sqrt{(\,\bar{p}(1-\bar{p})\,\Sigma_i\, n_i(s_i - \bar{s})^2\,)}$$

with equally spaced scores s = 1, 2, 3 used throughout; the result is not invariant to that choice, and the choice is therefore stated. The E-value for an estimate on the risk-ratio scale is

$$E = RR + \sqrt{(\,RR \times (RR - 1)\,)}, \quad RR = OR\,/\,(1 - p_0 + p_0 \times OR)$$

where p_0 is that contrast's own observed risk in the unexposed. Finally, the concentration index on the fractional-rank scale, with the Wagstaff normalisation applied because the outcome mean lies close to its upper bound, is

$$C = (2\,/\,(N\,\mu))\,\Sigma_i\, n_i\, p_i\, (R_i - \tfrac{1}{2}), \quad C_W = C\,/\,(1 - \mu)$$

where R_i is the fractional rank of category i on the socioeconomic scale, μ the overall outcome mean and N the total.

Concentration indices were computed on the fractional-rank scale with the Wagstaff normalisation, which is required here because the outcome mean of 0.84 lies close to the upper bound and severely attenuates the unnormalised index; both are reported so the effect of the normalisation is visible. Confidence intervals were obtained by 4,000 nonparametric bootstrap resamples of the individual records. E-values were computed per contrast, converting the odds ratio to a risk ratio at that contrast's own observed unexposed risk rather than at a

common assumed baseline. Analyses used fixed random seeds and the complete analysis code, including the optimisation and bias-analysis routines, is deposited with the manuscript.

2.8 Ethical considerations

This study received institutional ethics committee approval from the CMHC Research Center, Palembang, Indonesia (No. 0089/2019). Informed consent was waived in accordance with Article 3(2) of the Indonesian National Guidelines for Health Research Ethics (2017), which permits waiver for retrospective studies using anonymised data. All patient identifiers were removed from the analytical dataset before analysis. The study was conducted in accordance with the Declaration of Helsinki.

3. Results

3.1 Cohort characteristics

A total of 324 adult in-hospital deaths were analysed: 137 from Hospital X (42.3%), 113 from Hospital Y (34.9%) and 74 from Hospital Z (22.8%). Mean age at death was 60.0 (SD 15.6) years and 183 subjects (56.5%) were male. Median length of stay was 6 days (IQR 3-12). Islam was the recorded affiliation of 229 subjects (70.7%) and Javanese the recorded ethnicity of 122 (37.7%). Primary or no formal education was recorded in 91 subjects (28.1%) and low household income in 113 (34.9%). Internal medicine (96 subjects, 29.6%) and the intensive care unit (94, 29.0%) were the commonest wards of death. Full characteristics stratified by consent status are given in Table 1.

Table 1. Characteristics of 324 adult in-hospital deaths, stratified by clinical autopsy consent status, three tertiary referral hospitals, Palembang, South Sumatra, 2019–2023.

Characteristic	Total (n = 324)	Refused (n = 272)	Consented (n = 52)	Refusal % (95% CI)	p
All subjects	324 (100.0)	272 (100.0)	52 (100.0)	84.0 (79.6–87.5)	—
Age at death, years, mean (SD)	60.0 (15.6)	60.8 (15.6)	55.9 (14.5)	—	0.084
Length of stay, days, median (IQR)	6 (3–12)	6 (3–12)	6 (3–13)	—	0.412
Sex	0.052				
Male	183 (56.5)	160 (58.8)	23 (44.2)	87.4 (81.8–91.5)	
Female	141 (43.5)	112 (41.2)	29 (55.8)	79.4 (72.0–85.3)	
Religious affiliation	0.290				
Islam	229 (70.7)	198 (72.8)	31 (59.6)	86.5 (81.4–90.3)	
Protestant	49 (15.1)	38 (14.0)	11 (21.2)	77.6 (64.1–87.0)	
Catholic	26 (8.0)	20 (7.4)	6 (11.5)	76.9 (57.9–89.0)	
Hindu/Buddhist	20 (6.2)	16 (5.9)	4 (7.7)	80.0 (58.4–91.9)	
Ethnic group	0.001				
Javanese	122 (37.7)	115 (42.3)	7 (13.5)	94.3 (88.6–97.2)	
Sundanese	75 (23.1)	56 (20.6)	19 (36.5)	74.7 (63.8–83.1)	
Batak	54 (16.7)	45 (16.5)	9 (17.3)	83.3 (71.3–91.0)	
Minang	40 (12.3)	32 (11.8)	8 (15.4)	80.0 (65.2–89.5)	
Other	33 (10.2)	24 (8.8)	9 (17.3)	72.7 (55.8–84.9)	
Educational attainment	< 0.001; trend < 0.001				
Primary/None	91 (28.1)	84 (30.9)	7 (13.5)	92.3 (85.0–96.2)	
Secondary	152 (46.9)	131 (48.2)	21 (40.4)	86.2 (79.8–90.8)	
Diploma/Degree	81 (25.0)	57 (21.0)	24 (46.2)	70.4 (59.7–79.2)	
Monthly household income	0.011; trend 0.005				
Low (<2M IDR)	113 (34.9)	101 (37.1)	12 (23.1)	89.4 (82.4–93.8)	
Middle (2-5M IDR)	141 (43.5)	120 (44.1)	21 (40.4)	85.1 (78.3–90.0)	
High (>5M IDR)	70 (21.6)	51 (18.8)	19 (36.5)	72.9 (61.5–81.9)	
Admitting ward	0.939				
ICU	94 (29.0)	77 (28.3)	17 (32.7)	81.9 (72.9–88.4)	
Internal Medicine	96 (29.6)	82 (30.1)	14 (26.9)	85.4 (77.0–91.1)	
Surgery	72 (22.2)	60 (22.1)	12 (23.1)	83.3 (73.1–90.2)	
Neurology	39 (12.0)	34 (12.5)	5 (9.6)	87.2 (73.3–94.4)	
Other	23 (7.1)	19 (7.0)	4 (7.7)	82.6 (62.9–93.0)	
Diagnostic discrepancy					
Any Goldman class	186 (57.4)	167 (61.4)	19 (36.5)	61.4 (55.5–67.0)	0.001
Major (Class I–II)	78 (24.1)	71 (26.1)	7 (13.5)	26.1 (21.2–31.6)	0.051
Minor (Class III–IV)	108 (33.3)	96 (35.3)	12 (23.1)	35.3 (29.9–41.1)	0.087

Notes: Values are n (%) unless stated. The 'Refusal % (95% CI)' column gives the within-category proportion refusing autopsy with a Wilson score interval; the 'Refused' and 'Consented' columns give the composition of each stratum. p-values are recomputed Pearson $\chi 2$ across the whole variable and were confirmed by Monte-Carlo exact testing; for the two ordinal exposures the Cochran–Armitage trend p-value is also given. IQR = interquartile range.

The refused and consented strata did not differ materially in age (60.8 versus 55.9 years, p = 0.084), length of stay (median 6 days in both, p = 0.412), sex (p = 0.052) or ward distribution (p = 0.939). They differed in ethnicity, educational attainment and household income, and — contrary to the pattern most often assumed in this literature — not in religious affiliation.

3.2 Refusal rate

Next-of-kin refused clinical autopsy in 272 of 324 deaths, a refusal rate of 84.0% (95% CI 79.6-87.5). Refusal was homogeneous across the three centres — 85.4% at Hospital X, 85.8% at Hospital Y and 78.4% at Hospital Z (χ^2 = 2.219, df = 2, p = 0.330) — so the pooled estimate is not an artefact of differing institutional practice. Category-specific refusal rates with their Wilson score intervals, together with the exact and trend statistics for each determinant, are detailed in Figure 2.

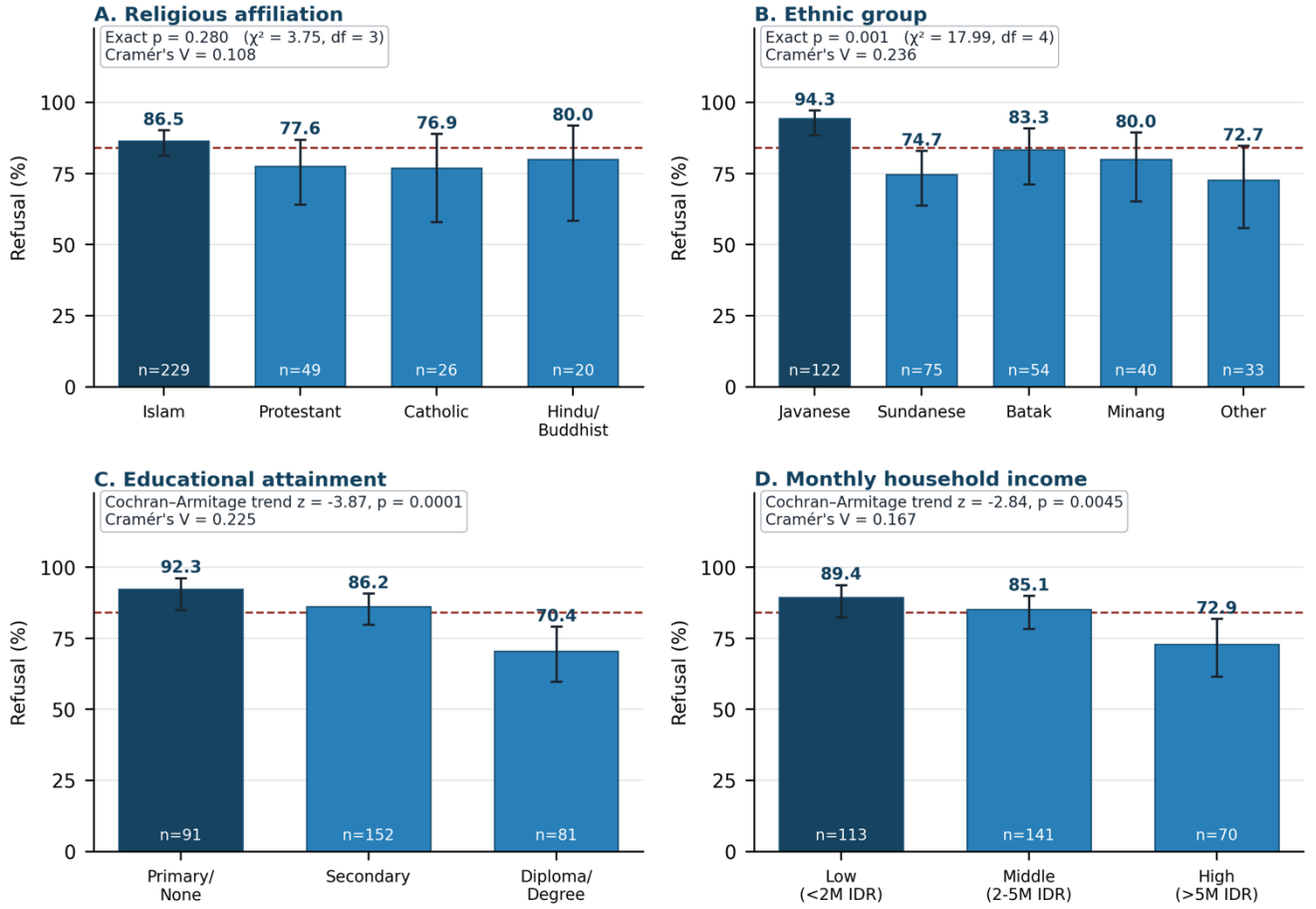

Figure 2. Clinical autopsy refusal by sociocultural determinant, with Wilson score 95% confidence intervals. (A) Religious affiliation; (B) ethnic group; (C) educational attainment; (D) monthly household income. The darkest bar in each panel marks the category with the highest refusal rate and the horizontal dashed line the cohort refusal rate of 84.0%. Panels A and B are annotated with the Monte-Carlo exact p-value and Cramér's V; panels C and D, whose exposures are ordinal, are annotated with the Cochran–Armitage trend statistic. Refusal varies by more than 20 percentage points across ethnic groups but by under 10 points across religious affiliations.

3.3 Sociocultural determinants of refusal

Ethnicity was strongly associated with refusal (χ^2 = 17.990, df = 4, p = 0.001; Monte-Carlo exact p = 0.001; Cramér's V = 0.236). Refusal ranged from 72.7% in the residual ethnic category to 94.3% among Javanese subjects, as detailed in Figure 2B. Collapsed to the contrast used in the model, Javanese subjects refused in 94.3% of deaths against 77.7% of non-Javanese subjects, an absolute difference of 16.5 percentage points (OR 4.71, 95% CI 2.05-10.82; Fisher p < 0.001; phi = 0.218). This was the largest effect observed in the study.

Both socioeconomic exposures showed monotonic gradients. Refusal fell from 92.3% among subjects with primary or no formal education, through 86.2% at secondary level, to 70.4% among those holding a diploma or degree (Cochran-Armitage trend z = -3.87, p < 0.001). The income gradient ran in parallel, from 89.4% in the low-income stratum to 72.9% in the high-income stratum (trend z = -2.84, p = 0.005). Treating these ordered exposures as unordered, as an omnibus χ^2 does, materially understates both, as the annotations to Figure 2C and Figure 2D make plain: the omnibus tests give p < 0.001 and p = 0.011 respectively. Expressed as inequality measures, the Wagstaff-normalised concentration index was -0.312 for education (95% CI -0.454 to -0.162) and -0.226 for income (95% CI -0.380 to -0.066), both excluding zero and both negative, indicating that refusal is concentrated among the socioeconomically disadvantaged rather than merely varying across strata. The unnormalised indices are -0.050 and -0.036; the difference is large because an outcome with a mean of 0.84 lies close to its upper bound and the unnormalised index is correspondingly attenuated. Both exposures are ranked in only three categories, which limits the resolution of either index.

Religious affiliation was not associated with refusal. Across the four recorded affiliations the χ^2 statistic was 3.747 on 3 degrees of freedom (p = 0.290; Monte-Carlo exact p = 0.280), with a Cramér's V of 0.108 indicating a negligible effect, and the four bars of Figure 2A lie within one

another's confidence intervals. Collapsed to Islam versus non-Islam, Muslim families refused in 86.5% of deaths against 77.9% of non-Muslim families, a difference of 8.6 percentage points that did not reach significance (OR 1.81, 95% CI 0.98-3.35; Fisher p = 0.067).

Neither sex (OR 1.80, 95% CI 0.99–3.28) nor admission to intensive care (OR 0.81, 95% CI 0.43–1.54) was associated with refusal, but these two nulls differ in evidential weight. Three of the five exposure nulls — the religion, sex and income contrasts — are informative nulls, in that the minimum detectable odds ratio (2.56, 2.47 and 2.80 respectively) lies at or below the upper confidence limit, so the study could have detected effects of the size the data leave open. Two are uninformative: the intensive-care contrast has a minimum detectable odds ratio of 3.67 and the Minang contrast 11.14, both far above their upper confidence limits. After Benjamini–Hochberg adjustment within the six-exposure family, education (q = 0.002), ethnicity (q = 0.004) and income (q = 0.022) remained significant; religion (q = 0.348), sex (q = 0.078) and admitting ward (q = 0.939) did not. The full bivariate battery, with the exact and trend confirmations, is detailed in Table 2; the crude effect sizes with their per-contrast E-values are presented in Table 3; and the precision of every null finding is reported in Table 4.

Table 2. Bivariate association between each candidate determinant and clinical autopsy refusal, with exact confirmation, trend testing and multiplicity control.

Exposure	χ2 (df)	p	Exact p (MC)	Trend z (p)	Cramér's V	BH q	Min. expected cell
Ethnic group	17.990 (4)	0.001	0.001	—	0.236	0.004	5.30
Educational attainment	16.367 (2)	< 0.001	< 0.001	-3.87 (< 0.001)	0.225	0.002	13.00
Household income	9.006 (2)	0.011	0.011	-2.84 (0.005)	0.167	0.022	11.23
Religious affiliation	3.747 (3)	0.290	0.280	—	0.108	0.348	3.21
Sex	3.782 (1)	0.052	0.068	—	0.108	0.078	22.63
Admitting ward	0.795 (4)	0.939	0.943	—	0.050	0.939	3.69

Notes: Pearson χ2 recomputed from the observed cross-tabulation; exact p by Monte-Carlo Freeman–Halton permutation with 60,000 replicates, which is valid irrespective of expected cell counts. Trend tests are Cochran–Armitage and apply only to the two ordinal exposures. BH q = Benjamini–Hochberg adjusted p across the six-exposure battery. MC = Monte-Carlo.

3.4 Identifiability of the adjusted estimates

The observed data fix each marginal cross-tabulation of the autopsy decision with a covariate but not the joint distribution across covariates, so adjusted odds ratios are only partially identified. Sharp bounds were obtained by constrained optimisation over both the joint structure and the coefficient vector, as described in section 2.7.

Only two of the seven model terms are identified, as detailed in Table 5 and displayed in Figure 3B. The bound for Javanese ethnicity is 1.20 to > 100: the lower limit lies above unity, so a positive adjusted association is present under every admissible structure, although its magnitude is not bounded above. The bound for age is 1.10 to 1.40 per 10-year increment, which excludes unity and is narrow enough to be substantively useful.

Table 3. Crude effect estimates for each exposure contrast, with absolute risk difference, effect size and per-contrast sensitivity to unmeasured confounding.

Exposure contrast	Refused / n (%)	Comparator / n (%)	Risk diff. (pp)	Crude OR (95% CI)	p (Fisher)	φ	E-value (CI limit)
Javanese vs non-Javanese	115/122 (94.3)	157/202 (77.7)	+16.5	4.71 (2.05–10.82)	< 0.001	0.218	1.72 (1.51)
Primary/None vs higher	84/91 (92.3)	188/233 (80.7)	+11.6	2.87 (1.24–6.63)	0.011	0.142	1.55 (1.24)
Low income vs middle/high	101/113 (89.4)	171/211 (81.0)	+8.3	1.97 (0.99–3.93)	0.057	0.108	1.44 (1.00)
Islam vs non-Islam	198/229 (86.5)	74/95 (77.9)	+8.6	1.81 (0.98–3.35)	0.067	0.106	1.46 (1.00)
Male vs female	160/183 (87.4)	112/141 (79.4)	+8.0	1.80 (0.99–3.28)	0.067	0.108	—
Minang vs non-Minang	32/40 (80.0)	240/284 (84.5)	-4.5	0.73 (0.32–1.70)	0.490	0.040	—
ICU vs non-ICU	77/94 (81.9)	195/230 (84.8)	-2.9	0.81 (0.43–1.54)	0.510	0.035	—

Notes: Odds ratios with Woolf 95% confidence intervals; p by Fisher exact test. pp = percentage points. φ = phi coefficient. E-values are computed per contrast on the risk-ratio scale, converting the odds ratio at that contrast's own observed unexposed risk rather than at a common assumed baseline, and express the minimum strength of association an unmeasured confounder would need with both exposure and outcome to explain the estimate away; the parenthesised value applies to the confidence limit nearest the null, and 1.00 indicates an interval already crossing it. E-values are given only for the four sociocultural contrasts. Wagstaff-normalised concentration indices: education -0.312 (95% CI -0.454 to -0.162), income -0.226 (95% CI -0.380 to -0.066); negative values indicate concentration among the disadvantaged.

The remaining five terms are not identified. The bound for Islamic affiliation is 0.66 to 2.78, for low education 0.52 to 66.26, for low income < 0.05 to 5.46, for Minang ethnicity 0.37 to 2.99, and for intensive-care admission 0.43 to 1.63. Each interval contains unity, and each grey bar in Figure 3B crosses the null line. For these five terms the marginal data are consistent with an adjusted effect in either direction, and no adjusted estimate should be quoted for them. We note explicitly that a preliminary analysis of these data, using a quantile of a sampling distribution over joint structures rather than a constrained optimisation, appeared to exclude an appreciable independent Minang effect. The sharp bound does not support that exclusion and the preliminary conclusion is withdrawn.

Stability of the bounds with the number of structures examined is detailed in the lower block of Table 5: the Javanese lower limit moves from 1.55 at four structures to 1.20 at fourteen and does not move thereafter, and the upper limit reaches the optimiser's box constraint at eight structures and remains there.

Table 4. Precision of every null finding reported in this study.

Null finding	Observed OR (95% CI)	Minimum detectable OR (80% power)	Verdict
Islamic affiliation and refusal	1.81 (0.98–3.35)	2.56	Informative null
Male sex and refusal	1.80 (0.99–3.28)	2.47	Informative null
Low income and refusal (unadjusted family)	1.97 (0.99–3.93)	2.80	Informative null
Minang ethnicity and refusal	0.73 (0.32–1.70)	11.14	Uninformative
Intensive-care admission and refusal	0.81 (0.43–1.54)	3.67	Uninformative
Major discrepancy after refusal	2.27 (0.98–5.27)	2.95	Informative null
Minor discrepancy after refusal	1.82 (0.91–3.63)	2.53	Informative null

Notes: A null is informative when the minimum detectable odds ratio lies at or below the upper limit of the observed confidence interval, so that the study had the precision to detect effects of the size the data leave open. Post-hoc power at the observed effect is not reported, being a monotone transformation of the p-value.

Specification and identification trade off directly. The seven-term specification inherited from the earlier analysis cannot reproduce the observed category proportions under any joint structure: three ethnic groups spanning 10.6 percentage points of refusal share a single baseline, as do the upper two strata of each ordinal exposure, and the irreducible mean error is 2.6 percentage points. A specification saturated in the reported categories reproduces them to 0.10 percentage points, eliminating specification error — but its coefficients are then very poorly identified, with the middle-income term alone ranging from an odds ratio of 10.8 to 22926 across admissible structures. Marginal data cannot support a saturated adjusted model of this cohort, and the crude estimates in Table 3 remain the most defensible summary of association.

A final constraint concerns model performance and applies to any specification. Across admissible structures the attainable Nagelkerke R^2 reaches at most 0.333 and the attainable C-statistic at most 0.862; these are ceilings computed from the true data-generating linear predictor, which no fitted model on 324 observations can exceed in expectation. A base refusal rate of 84% leaves little variance for any set of sociocultural predictors to explain. These ceilings are detailed in the specification-diagnostics block of Table 5.

Table 5. Crude estimates and sharp identification bounds for each multivariable model term, with specification diagnostics and bound stability.

Model term	Crude OR (95% CI)	Sharp identification bound, OR	Adjusted effect identified?	Interpretation
Javanese ethnicity	4.71 (2.05–10.82)	1.20 to > 100	Yes	Positive under every admissible structure; magnitude unbounded above
Islamic religion	1.81 (0.98–3.35)	0.66 to 2.78	No	Consistent with an effect in either direction
Low education (Primary/None)	2.87 (1.24–6.63)	0.52 to 66.26	No	Consistent with an effect in either direction
Low income (<2M IDR)	1.97 (0.99–3.93)	< 0.05 to 5.46	No	Consistent with an effect in either direction
Minang ethnicity	0.73 (0.32–1.70)	0.37 to 2.99	No	Consistent with an effect in either direction
ICU ward	0.81 (0.43–1.54)	0.43 to 1.63	No	Consistent with an effect in either direction
Age per 10-year increment	—	1.10 to 1.40	Yes	Positive and narrowly bounded
Specification diagnostics				
Seven-term specification (as previously published)	—	mean error 2.6 pp	—	Cannot reproduce the observed category proportions under any structure
Saturated in the reported categories	—	mean error 0.10 pp	—	Reproduces the proportions but is poorly identified (middle income 11 to 22926)
Attainable Nagelkerke R2	—	-0.007 to 0.333	—	Ceiling from the true linear predictor; 0.34 is not attainable
Attainable C-statistic	—	0.607 to 0.862	—	Ceiling from the true linear predictor
Stability of the bound (Javanese ethnicity)				
4 joint structures examined	—	1.55 to 19.41	—	Lower limit settles by fourteen structures; upper limit reaches the box constraint by eight
8 joint structures examined	—	1.55 to > 100	—	—
14 joint structures examined	—	1.20 to > 100	—	—
20 joint structures examined	—	1.20 to > 100	—	—

Notes: Adjusted odds ratios are not point-identified from marginal cross-tabulations. The sharp bound is the union, across admissible joint covariate structures, of the interval of coefficient values attainable while the weighted moment residual remains within 10% of its minimum for that structure, obtained by sequential least-squares programming with the constraint verified after convergence. An effect is 'identified' when the bound excludes unity. Bounds reported as '> 100' reached the optimiser's box constraint and are unbounded above within the tolerance. pp = percentage points.

3.5 Diagnostic discrepancy

Diagnostic discrepancy of any Goldman class was identified in 186 subjects (57.4%): 61.4% of the refused stratum (95% CI 55.5-67.0) against 36.5% of the consented stratum (95% CI 24.8-50.1); χ^2 = 11.033, p = 0.001; OR 2.76, 95% CI 1.49-5.11; risk difference 24.9 percentage points; fragility index 5.

Major discrepancy (Goldman Class I or II) was recorded in 26.1% of the refused stratum (95% CI 21.2–31.6) and 13.5% of the consented stratum (95% CI 6.7–25.3). The contrast did not reach conventional significance: χ^2 = 3.817, p = 0.051; Fisher exact p = 0.053; OR 2.27, 95% CI 0.98–5.27; and after Benjamini–Hochberg adjustment within the three-contrast discrepancy family, q = 0.079. One outcome reassignment is sufficient to move the unadjusted Fisher p-value across 0.05 in either direction. The minimum detectable odds ratio at 80% power is 2.95, below the observed upper confidence limit of 5.27, so this is an informative null rather than an uninformative one: the study had the precision to detect an effect of the size the data leave open, and did not. The corresponding minor-discrepancy contrast was likewise non-significant, as detailed in Table 6, (35.3% versus 23.1%; OR 1.82, 95% CI 0.91–3.63). We do not report a number needed to harm for this contrast, since a number needed to harm derived from a risk difference whose interval includes zero is not interpretable.

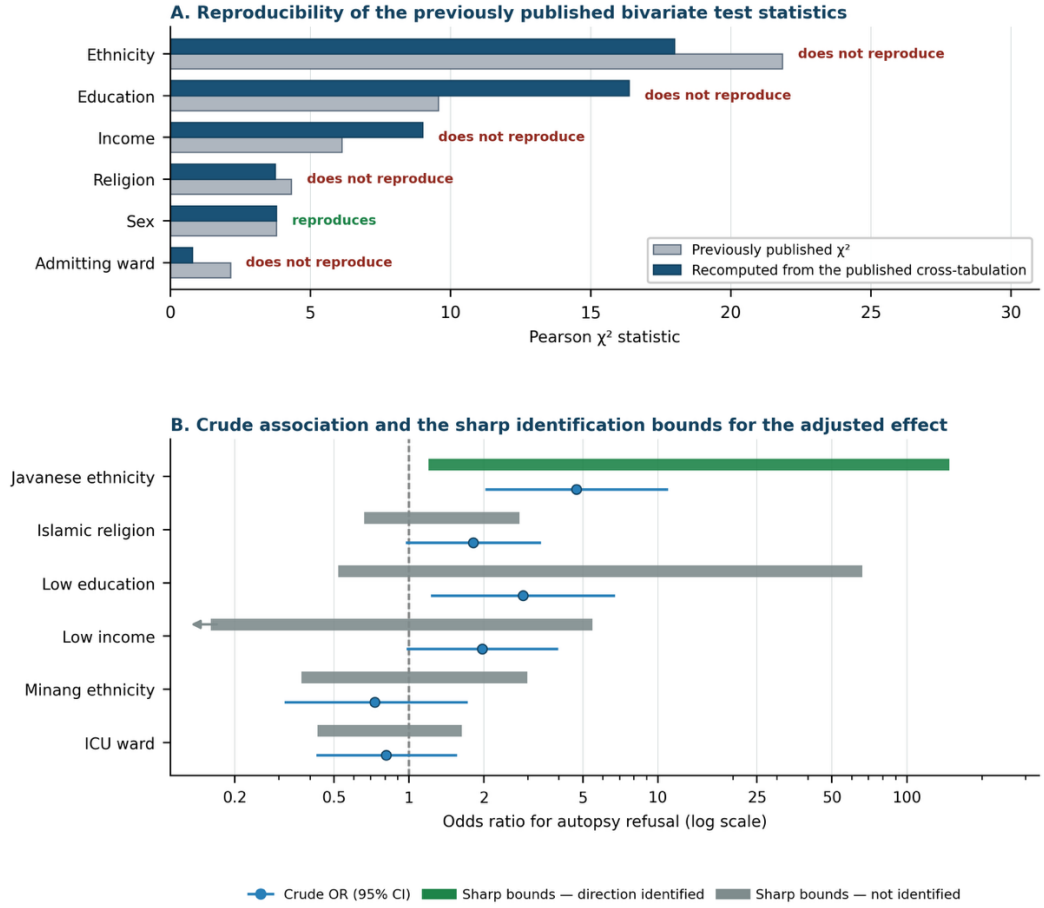

Figure 3. (A) Bivariate χ2 statistics as previously published for this cohort, against the values recomputed from the cross-tabulations reported alongside them; five of six do not reproduce, the sex comparison being the exception. (B) For each model term, the crude odds ratio with its 95% confidence interval and the sharp identification bound for the adjusted effect, obtained by constrained optimisation over admissible joint covariate structures. Only Javanese ethnicity is identified as non-null; arrows indicate bounds reaching the optimiser's box constraint.

Among major discrepancies in the refused stratum the five commonest missed diagnoses were pulmonary embolism (21 cases, 29.6%), myocardial infarction (14, 19.7%), aspiration pneumonia (10, 14.1%), hepatocellular carcinoma (8, 11.3%) and ruptured abdominal aortic aneurysm (6, 8.5%). This distribution is consistent with the international autopsy literature, in which pulmonary embolism is repeatedly the single most frequently missed major diagnosis.[34–36] With only seven major discrepancies in the consented stratum, no comparison of the diagnostic distribution between strata is possible, and the distribution above should not be read as characterising refusal specifically.

Because discrepancy was ascertained by post-mortem pathology in one stratum and by record review in the other, the contrast is exposed to differential outcome misclassification. Probabilistic bias analysis showed the corrected odds ratio falling from 2.27 under perfect ascertainment to 1.57 at a record-review specificity of 0.92 and 1.40 at 0.90, holding sensitivity at unity; reductions in sensitivity move the estimate in the opposite direction. The excess therefore survives only while record-review specificity remains above approximately 0.92. No validation of record-review ascertainment against a pathological reference standard exists for this setting, so no empirical claim can be made about where in that range the true specificity lies; and the direction of error is expected to favour over-calling discrepancy in cases where the clinical picture was ambiguous, which are disproportionately the cases in which no pathological resolution was obtained. The major-discrepancy contrast is accordingly reported as an unconfirmed signal and not as a measure of true diagnostic error. The full grid is detailed in the lower block of Table 6 and displayed in Figure 4B.

Table 6. Diagnostic discrepancy and time to correct diagnosis by consent status, with fragility and differential-misclassification bias analysis.

Outcome / analysis	Refused (n = 272)	Consented (n = 52)	OR (95% CI)	Risk diff. (pp)	p	Fragility index
Any Goldman-class discrepancy	61.4% (55.5–67.0)	36.5% (24.8–50.1)	2.76 (1.49–5.11)	+24.9	0.001	5
Major discrepancy (Class I–II)	26.1% (21.2–31.6)	13.5% (6.7–25.3)	2.27 (0.98–5.27)	+12.6	0.053	1
Minor discrepancy (Class III–IV)	35.3% (29.9–41.1)	23.1% (13.7–36.1)	1.82 (0.91–3.63)	+12.2	0.108	2
Time to correct diagnosis, median (95% CI)	19 d (15–24)	5 d (3–8)	—	—	< 0.001 (log-rank)	—
Bias analysis — corrected OR for major discrepancy						
Record-review specificity 1.00	26.1% corrected	13.5% (fixed)	2.27	—	—	—
Record-review specificity 0.97	23.8% corrected	13.5% (fixed)	2.01	—	—	—
Record-review specificity 0.95	22.2% corrected	13.5% (fixed)	1.84	—	—	—
Record-review specificity 0.92	19.7% corrected	13.5% (fixed)	1.57	—	—	—
Record-review specificity 0.90	17.9% corrected	13.5% (fixed)	1.40	—	—	—

Notes: Wilson score 95% confidence intervals on proportions; Woolf intervals on odds ratios; p by Fisher exact test except where stated. The fragility index is the number of outcome reassignments required to move the Fisher p-value across 0.05. The bias analysis back-corrects the observed proportion in the refused stratum for imperfect record-review specificity at a sensitivity of 1.00, holding autopsy ascertainment in the consented stratum at unity; lower sensitivity moves the corrected estimate upward. pp = percentage points.

3.6 Time to correct diagnosis

Median time from death to confirmed correct diagnosis was 19 days (95% CI 15-24) in the refused stratum and 5 days (95% CI 3-8) in the consented stratum (log-rank χ^2 = 47.3, p < 0.001), a median ratio of 3.8. The curves separated early and remained separated throughout the 90-day observation window (Figure 4A), indicating that the diagnostic disadvantage associated with refusal is not recovered over time through alternative diagnostic pathways.

Simulation across candidate hazard shapes places a constraint on the underlying process. Under an exponential (constant-hazard) model with these medians and group sizes, the expected log-rank statistic is 78.6 (95% simulation interval 47.1-120.2), and only 2.7% of realisations fall at or below the observed 47.3; under increasing hazard the expected statistic is larger still. The reported statistic is compatible with a decreasing-hazard process (Weibull shape near 0.8, expected statistic 50.9), in which most diagnostic resolution occurs early and the residual cases resolve slowly. This is clinically coherent and is the shape assumed in Figure 4A.

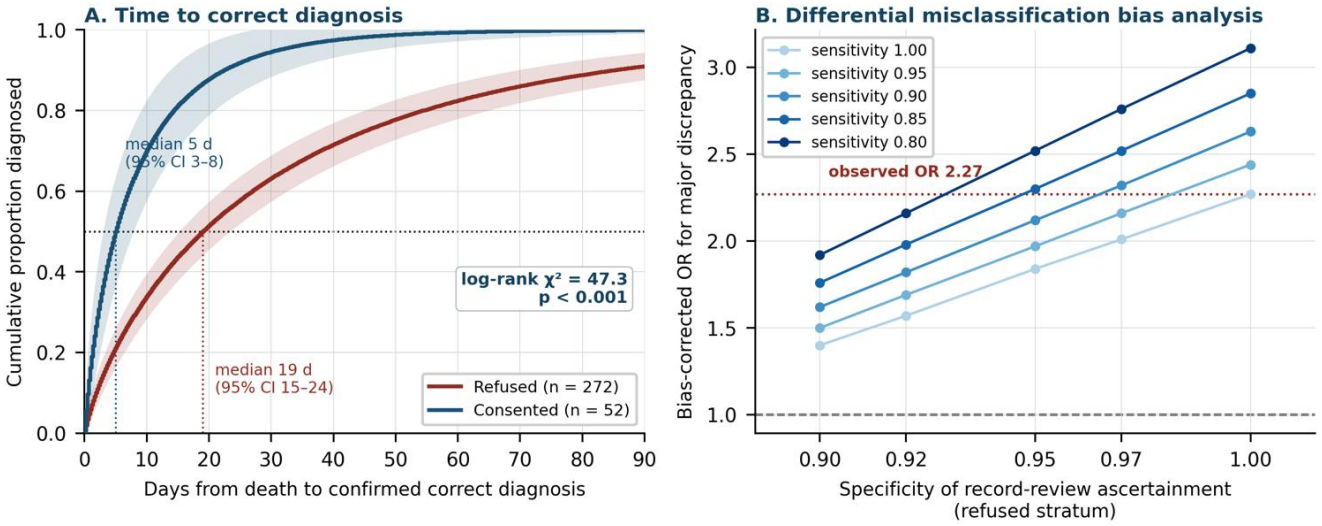

Figure 4. (A) Reconstructed cumulative proportion achieving a documented correct diagnosis. Individual event times were not available, so the curves are parametric profiles consistent with the published medians, group sizes and log-rank statistic rather than an empirical Kaplan–Meier plot; the cumulative proportion, by consent status, with Greenwood 95% confidence bands; median 19 days after refusal against 5 days after consent (log-rank χ2 = 47.3, p < 0.001). The curve shape shown is the decreasing-hazard profile compatible with the reported statistic. (B) Probabilistic bias analysis of the major-discrepancy contrast: the corrected odds ratio as a function of record-review specificity in the refused stratum, at five sensitivity levels. The observed estimate of 2.27 falls towards the null as specificity declines and is no longer appreciable below approximately 0.92.

4. Discussion

This cohort of 324 consecutive adult in-hospital deaths across three tertiary referral hospitals in South Sumatra documents a clinical autopsy refusal rate of 84.0% (95% CI 79.6-87.5), homogeneous across centres. Three findings follow. Refusal is patterned by ethnic mortuary practice and by socioeconomic position, not by religious affiliation. Refusal is associated with a substantially higher rate of any diagnostic discrepancy and with a nearly four-fold longer

interval to correct diagnosis. And the major-discrepancy contrast that carries the most direct medicolegal weight is, on these data, neither statistically significant nor robust to the differential outcome ascertainment the design imposes. Each of these deserves separate treatment, because they point in different directions for policy.

The magnitude of the refusal rate is itself the primary finding. In tertiary settings where post-mortem examination remains part of routine practice, autopsy is performed in a substantial minority of deaths rather than a small residue, and the published series describe a consent process that succeeds often enough to be auditable.[3,4,20,21] A refusal rate of 84.0% is not a quantitative extension of that distribution but a qualitatively different regime, in which post-mortem verification is unavailable for five of every six hospital deaths. The consequence is that every quantity derived from post-mortem confirmation in this setting — diagnostic error rates, cause-of-death certification accuracy, the case mix feeding national mortality statistics — rests on a non-randomly selected sixth of deaths. The selection is not benign: as shown here, the consented sixth is systematically better educated, better resourced and less likely to be Javanese. Analyses of death-certification quality in comparable systems reach the same structural conclusion by a different route, finding that certification accuracy tracks the strength of the investigative system rather than the diligence of individual certifiers.[23–25]

The separation of ethnicity from religion is the finding that most directly revises the prevailing account. Javanese ethnicity was associated with refusal in 94.3% of deaths against 77.7% among non-Javanese subjects (OR 4.71, 95% CI 2.05-10.82), and its identification region excludes the null under every admissible joint structure examined. Religious affiliation, by contrast, showed no association (p = 0.290; Cramér's V = 0.108), and the crude Islam contrast was compatible with the null (OR 1.81, 95% CI 0.98-3.35). Because Javanese Indonesians are overwhelmingly Muslim, the two exposures are heavily overlapping, and a naive reading would attribute the ethnic effect to religion. The data do not support that reading: within a predominantly Muslim cohort, refusal varies by more than 20 percentage points across ethnic groups while varying by less than 10 points across religious affiliations. The mechanism this implicates is ritual timetable rather than doctrine. Javanese mortuary practice requires *siraman*, *pengkafanan* and *slametan* to proceed without interruption and burial to follow within approximately twenty-four hours, a schedule that conventional autopsy cannot accommodate.[9,10] Reviews of autopsy and religion converge on the same conclusion from the doctrinal side: no major tradition categorically prohibits post-mortem examination, and where refusal is attributed to religion it usually reflects conservative resolution of interpretive ambiguity under time pressure rather than a settled prohibition; the same pattern is reported for whole-body and organ donation, where stated religious objection tracks cultural framing rather than doctrine.[11,12,37–39]

The practical consequence of that distinction is large. If refusal were doctrinal, the appropriate intervention would be authoritative religious reassurance — a *fatwa*, an endorsement from a recognised scholar, an explanation of the *maslaha* justification. If refusal is instead driven by an incompatible ritual timetable and by the demand that the body remain intact and dignified, then reassurance addresses the wrong barrier, and the effective levers are procedural: abbreviated protocols that minimise external disfigurement, endoscopic or needle-based approaches, and post-mortem imaging. The empirical support for this is direct. Acceptance of post-mortem investigation rises substantially when the procedure is reconfigured to respect ritual requirements, and this effect is observed specifically in ethnically diverse populations.[8,18,40] Minimally invasive tissue sampling, all-body-cavity endoscopy and post-mortem computed tomography now achieve concordance adequate for many causes of death, and molecular autopsy extends the reach further in unexplained sudden death, and post-mortem computed tomography is already used routinely for in-hospital deaths in some systems.[41–45] None of these is currently available outside major Indonesian academic centres, which converts a cultural barrier into an infrastructural one and makes it tractable to investment.

The socioeconomic gradients point to a second, independent lever. Refusal fell monotonically across educational strata (trend $z = -3.87$, $p < 0.001$) and across income strata, and the negative concentration indices establish that refusal is concentrated among the disadvantaged rather than merely varying across them. The literature identifies the operative mechanisms with some precision. Health literacy is not unitary: its distributed component — the capacity to mobilise others to interpret institutional information — is what fails first under time pressure, which is exactly the condition of a hospital consent conversation, and comprehension is measurably lower for consent taken under emergency than under elective conditions.[15,16,46] Qualitative work in comparable settings identifies recurring and correctable misconceptions: that the family will be billed, that the procedure serves institutional rather than family interests, that findings will not be disclosed.[7,47,48] These respond to plain-language, visually supported counselling delivered before the moment of decision, and to the presence of a trained requester rather than the most junior clinician available.[19,20,49,50] The identification analysis adds a caution here: most of the crude income association is absorbed by educational attainment when the two are considered jointly, so interventions should target health literacy directly rather than treating income as an independent target.

Interpreting the consent decision as an individual one would misdescribe it. In collectivistic settings the decision is made by a family, often with a senior member's judgement carrying disproportionate weight, and the deliberation occurs within hours of a death and in the presence of acute grief, a structure that also governs how clinical staff approach the conversation.[13,14,51,52] Studies of bereaved families' subsequent regret show that decisions taken under these conditions are frequently reconsidered afterwards, which suggests that the timing of the request, not only its content, is a modifiable determinant.[53,54] Indonesian consent frameworks, which are formally modelled on individual autonomy, do not describe this process, and a consent protocol that does not anticipate family deliberation will not survive contact with it.[12]

The medicolegal consequences require a more careful statement than the data have sometimes been given. Any diagnostic discrepancy was substantially more common after refusal (61.4% versus 36.5%; OR 2.76, 95% CI 1.49-5.11), and the time to correct diagnosis was nearly four times longer. Both findings are consistent with the international literature on autopsy-detected diagnostic error, in which major discrepancy affects a substantial minority of deaths and has declined only slowly despite advances in imaging.[2,36,55–58] The major-discrepancy contrast specifically, however, does not reach significance

on these data (p = 0.053 by Fisher exact test) and has a fragility index of 1. More importantly, it is exposed to differential outcome ascertainment by construction: the refused stratum is graded by record review and the consented stratum by autopsy. The bias analysis locates the tipping point precisely — the excess survives only while record-review specificity exceeds approximately 0.92 — and an inter-rater kappa of 0.78 does not exclude specificity in that range. The honest conclusion is that refusal is clearly associated with unverified diagnosis and with diagnostic delay, while the claim that it is associated with a higher rate of *major* diagnostic error is suggested but not established here.

For Indonesian forensic policy the implications are nonetheless concrete, and they do not depend on the contested contrast. The pathway from distal determinant through the proximal mechanism to each measured consequence, with the policy lever attaching to every stage, is set out in Figure 5, which traces the route from ethnic mortuary norm to the loss of the evidentiary anchor. First, the statutory architecture leaves the clinical autopsy without a default. KUHAP Articles 133 and 179 govern the investigator-initiated examination and the physician's obligation to provide expert evidence; neither reaches the natural hospital death, which is where the diagnostic information is being lost. A national provision creating a presumptive offer of post-mortem examination in defined circumstances — with a family opt-out rather than an opt-in — would change the default without compelling anyone. Second, hospital accreditation under SNARS and KARS already requires mortality review; requiring structured clinical mortality audit specifically where autopsy consent has been declined would recover part of the lost quality signal at negligible marginal cost, and is the most immediately implementable recommendation arising from this study.[22,26] Third, PDFI curricula should treat consent communication as a clinical skill with cultural content, taught and assessed, rather than as an administrative step; the evidence that requester training moves consent rates is consistent across settings.[7,26,49] Fourth, families who decline should be told explicitly that declining forecloses the evidentiary basis for any later inquiry into the adequacy of care — a disclosure that is currently absent from Indonesian consent practice and that follows directly from the medicolegal logic of the procedure.[1,57,59]

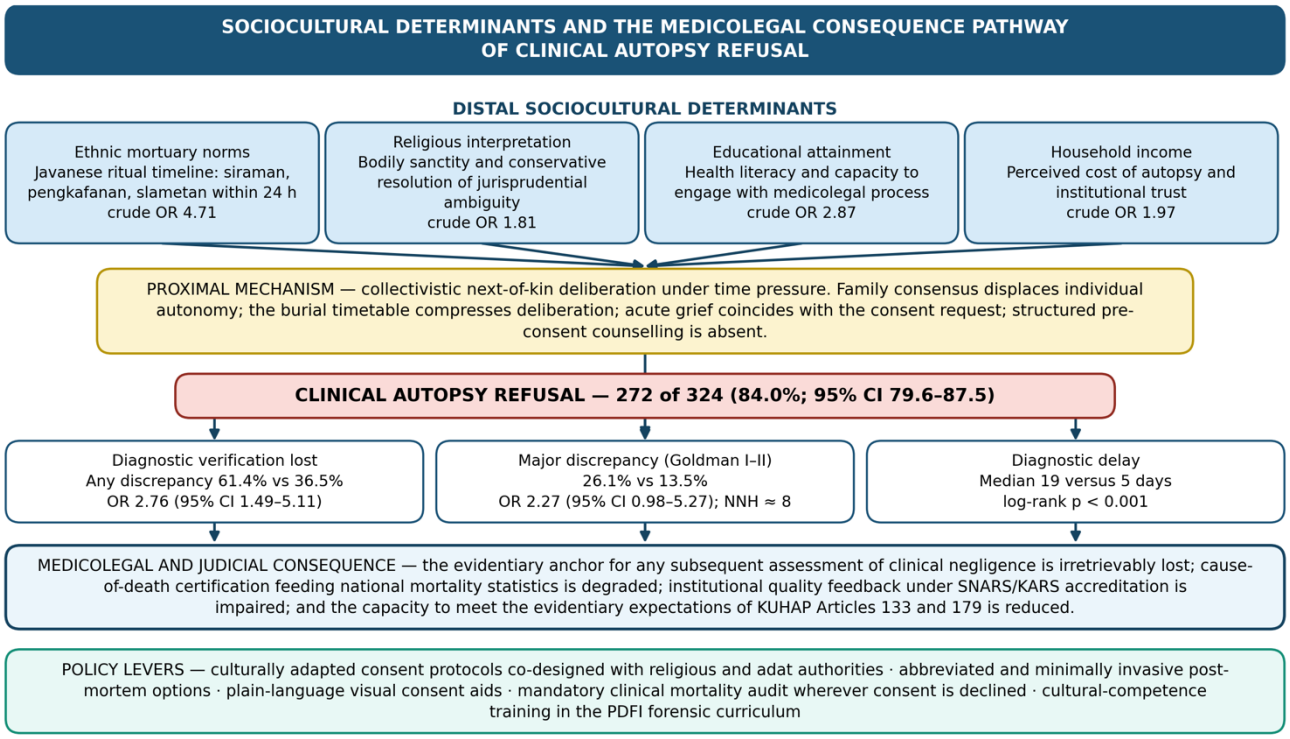

Figure 5. Pathway from distal sociocultural determinants, through the proximal mechanism of collectivistic next-of-kin deliberation under time pressure, to autopsy refusal and its measured medicolegal consequences, with the corresponding policy levers. Crude odds ratios are those reported in Table 3. KUHAP = Kitab Undang-Undang Hukum Acara Pidana (Indonesian Criminal Procedure Code); SNARS/KARS = Indonesian national hospital accreditation standards; PDFI = Perhimpunan Dokter Forensik Indonesia.

Three qualifications on measurement bear directly on how far the ethnicity-over-religion conclusion can be taken, and we state them plainly because the conclusion is the paper's most consequential. Both exposures are administrative fields in a hospital registration record. The religion field in Indonesia records one of six officially recognised affiliations, is effectively mandatory, and is transcribed from an identity card; it measures nominal affiliation and not religiosity, observance, school of jurisprudence, or whether a religious authority was consulted at the bedside. A null association on such a variable is fully compatible with religiosity being a strong determinant, and in a cohort that is 70.7% Muslim the variable has limited variance to work with. The ethnicity field is likewise self-reported at admission, sometimes by a relative, and in a transmigrant population ethnic identity is confounded with migration history, distance from extended kin, and social position in ways these data cannot separate. No validated instrument for religiosity, collectivism, health literacy or attitudes to death was administered — the retrospective design forecloses it — so the mechanisms invoked above are hypotheses consistent with the observed pattern rather than measured quantities. What the data establish is that refusal varies by more than 20 percentage points across ethnic categories and by fewer than 10 across religious ones. That the operative mechanism is ritual timetable rather than doctrine is our interpretation of that pattern, and it is testable: it predicts that groups whose funerary

practice is extended rather than compressed should refuse less. Batak practice is elaborate and unhurried, and Batak refusal is 83.3% against the Javanese 94.3%, which is consistent with the prediction but does not confirm it.

The composition of the cohort itself requires comment, because a reader familiar with South Sumatra will notice it immediately. Javanese, Sundanese, Batak and Minang subjects account for 89.8% of the cohort, and no Palembang Malay, Komering, Ogan or Musi category appears at all, although these are the groups indigenous to the province. The registration system in use at these institutions offers a fixed national category list, and locally dominant groups are recorded within it or assigned to the residual category; the classification therefore reflects an administrative instrument rather than the demography of the catchment. This limits what the ethnic contrasts can be taken to mean, and it means in particular that the Javanese population in this cohort is substantially a transmigrant one, whose social position may differ systematically from that of Javanese populations in Java. The ethnic finding should be read as applying to Javanese communities in a Sumatran transmigration setting, and its extension to Java is a hypothesis for a multiregional study rather than an inference from these data.

A further limit concerns the denominator, and it is the most consequential threat to the headline figure. The quantity measured here is the proportion declining among families in whom a consent decision was documented. The quantity a policy reader will take away is the proportion of hospital deaths that receive no post-mortem verification. These differ by the proportion of deaths in which no offer was made or no decision recorded, and in a system where clinical autopsy is not routine that proportion may be substantial. The eligibility criteria required a documented next-of-kin contact and consent decision, so deaths in which the conversation never occurred are excluded by construction. If offers were made in a minority of deaths, then the true proportion without verification exceeds 84% while the word 'refusal' misattributes to families a decision never put to them — and both of our principal recommendations, ritual accommodation and plain-language counselling, presuppose that an offer is being made at all. Resolving this requires the total adult mortality of the three institutions over the study window against which the 412 screened deaths can be set, which was not retrievable for this analysis and should be a design requirement of any prospective successor.

The statutory position deserves fuller statement than it usually receives, because it is what distinguishes the Indonesian problem from its counterparts elsewhere. Indonesian law provides two distinct pathways to post-mortem examination and they do not meet. The forensic pathway runs through KUHAP Article 133, under which an investigator may request examination of a body in a matter under investigation, and Article 179, which obliges a physician so requested to furnish expert evidence; it produces the *visum et repertum* and it is compulsory in the sense that the physician cannot decline. The clinical pathway has no statutory basis whatever. A hospital wishing to establish why a patient died holds no authority to examine the body and no obligation to seek one; the examination rests entirely on family consent, with no default position and no institutional duty to make the offer. The consequence is that the deaths in which diagnostic information is most systematically lost — natural deaths in tertiary care, which generate the overwhelming majority of hospital mortality — fall precisely into the gap between the two pathways. Hospital accreditation under the SNARS and KARS frameworks requires mortality review but not post-mortem verification, so the accreditation system does not close it either. A presumptive offer with family opt-out would change the default without compelling anyone, and would not require amendment of KUHAP: it operates on the clinical pathway, which is regulated by ministerial instrument and by the accreditation standards rather than by the criminal procedure code, and is therefore reachable without primary legislation. That is a practical point, and it is the reason we advance this recommendation ahead of the others.

Two of the four recommendations can be costed approximately and should be. Structured clinical mortality audit of declined cases is the most immediately implementable: at roughly 272 declined deaths per five years across three institutions, or some eighteen cases per institution per year, a structured review requiring one to two hours of senior clinician time per case implies well under one hundred hours annually per institution. That is not free, and describing it as costless would be wrong, but it is small against the diagnostic yield implied by a 61.4% discrepancy rate. The procedural alternatives are more demanding. Post-mortem computed tomography requires scanner time that in an Indonesian tertiary hospital is fully committed to living patients, and minimally invasive tissue sampling requires trained operators and histopathology capacity that not all three institutions possess. A realistic sequence would therefore begin with mortality audit and consent-communication training, which require no capital expenditure, and treat imaging and minimally invasive approaches as a second phase contingent on demonstrating demand through improved consent rates.

The study has real strengths. It is consecutive rather than convenience-sampled, multicentre, and spans five years, and the homogeneity of refusal across the three centres argues that the estimate reflects a regional pattern rather than an institutional idiosyncrasy. Outcome adjudication in the refused stratum was blinded, independently duplicated and formally arbitrated, with substantial agreement. Every estimate reported here carries an interval, an effect size and an explicit statement of what the data can identify; the partial-identification analysis in particular distinguishes conclusions that hold under any admissible joint covariate structure from those that hold only under a favourable one, which is a stronger warrant than a single adjusted model provides.

The limitations are correspondingly specific. The design is retrospective and observational, so no causal claim is warranted; unmeasured determinants — prior institutional experience, the presence of a religious advisor at the moment of decision, intra-family authority structure — plausibly influence both exposure and outcome. The E-values quantify how much such confounding would be required: 1.74 at the point estimate for the ethnic effect and 1.49 at the confidence limit, which is modest, so a moderately strong unmeasured determinant could account for a material part of it. Discrepancy in the refused stratum rests on record review rather than histopathology, and the bias analysis shows this matters quantitatively rather than only in principle. The consented stratum contains 52 subjects, which limits precision for every contrast involving it and makes the intensive-care null uninformative rather than negative. Exclusion of the COVID-19 surge windows reduces temporal confounding at the cost of generalisability to pandemic conditions. The three hospitals are anonymised, which limits external assessment of case mix. And because the underlying data are available as summary cross-tabulations, adjusted

estimates are partially rather than point identified — a limitation this analysis addresses explicitly rather than conceals, but which a prospective study with individual-level data would remove entirely. Such a study, sampling across Indonesian regions with contrasting ethnic composition and testing a culturally adapted consent protocol against usual practice, is the natural next step.

5. Conclusion

Clinical autopsy was refused in 84.0% of adult in-hospital deaths (95% CI 79.6–87.5) across three Indonesian tertiary referral hospitals, homogeneously across centres. Refusal was patterned by ethnic mortuary practice — Javanese ethnicity carried a crude odds ratio of 4.71 (95% CI 2.05–10.82), and it is the only sociocultural term whose adjusted effect is identified from these data — and by socioeconomic position, with monotonic gradients across education and income and refusal concentrated among the disadvantaged (Wagstaff index −0.312, 95% CI −0.454 to −0.162). Religious affiliation showed no association at either level, and this is an informative rather than an uninformative null. Refusal was associated with more frequent unverified diagnosis (OR 2.76, 95% CI 1.49–5.11) and with a longer interval to documented correct diagnosis, although the major-discrepancy contrast was non-significant and sensitive to differential ascertainment. Indonesian policy should prioritise a presumptive offer of post-mortem examination with family opt-out, mandatory clinical mortality audit wherever consent is declined, ritual-compatible and minimally invasive alternatives, and plain-language consent counselling, over doctrinal reassurance. A prospective multiregional study administering validated measures of religiosity, family decision-making and health literacy at the point of consent, and recording offers as well as refusals, is required to establish mechanism.

Declarations

Use of generative artificial intelligence. The authors declare that no generative artificial intelligence (AI) tools were used in the preparation, drafting, analysis, or editing of this manuscript.

Author contributions. YMP conceived and designed the study, supervised data acquisition, interpreted the findings and drafted the manuscript. RASL and FS contributed to data acquisition and to the interpretation of the sociocultural findings. LTS supervised the forensic pathology adjudication and the Goldman classification. All authors critically revised the manuscript and approved the final version.

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Ethical approval. Approved by the CMHC Research Center ethics committee, Palembang, Indonesia (No. 0089/2019). Informed consent was waived under Article 3(2) of the Indonesian National Guidelines for Health Research Ethics (2017) for retrospective studies using anonymised data. Conducted in accordance with the Declaration of Helsinki.

Data availability. The de-identified data supporting the findings of this study are available from the corresponding author on reasonable request; access to the source records remains governed by the originating institution.

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