MHJ

LUMORA JOURNALS

Multidisciplinary Healthcare Journal

Vol. 1, No. 4 (2025) · e-ISSN 3152-8775

Research article · DOI: not registered

Associated thoracic injury and live hospital discharge after traumatic spinal fracture in Saudi Arabia: a competing-risk secondary cohort analysis

Fatema Alawad[1],*, Aamir Kadri[2], Abdulkarim Abdullatif Alkhamisi[3], Abdullah Mushra Alghamdi[4], Abdulrahman Ahmed Almebki[5], Abdulrazaq Esin Issa[6]

Submission date	Acceptance date	Publication date
20 April 2025	25 April 2025	30 April 2025

[1] College of Medicine, King Faisal University, Al-Ahsa, Saudi Arabia

[2] University of Kentucky College of Medicine, Lexington, United States

[3] King Abdulaziz University, Saudi Arabia

[4] Al-Baha University, Saudi Arabia

[5] King Saud University, Riyadh, Saudi Arabia

[6] Gombe State University, Gombe, Nigeria

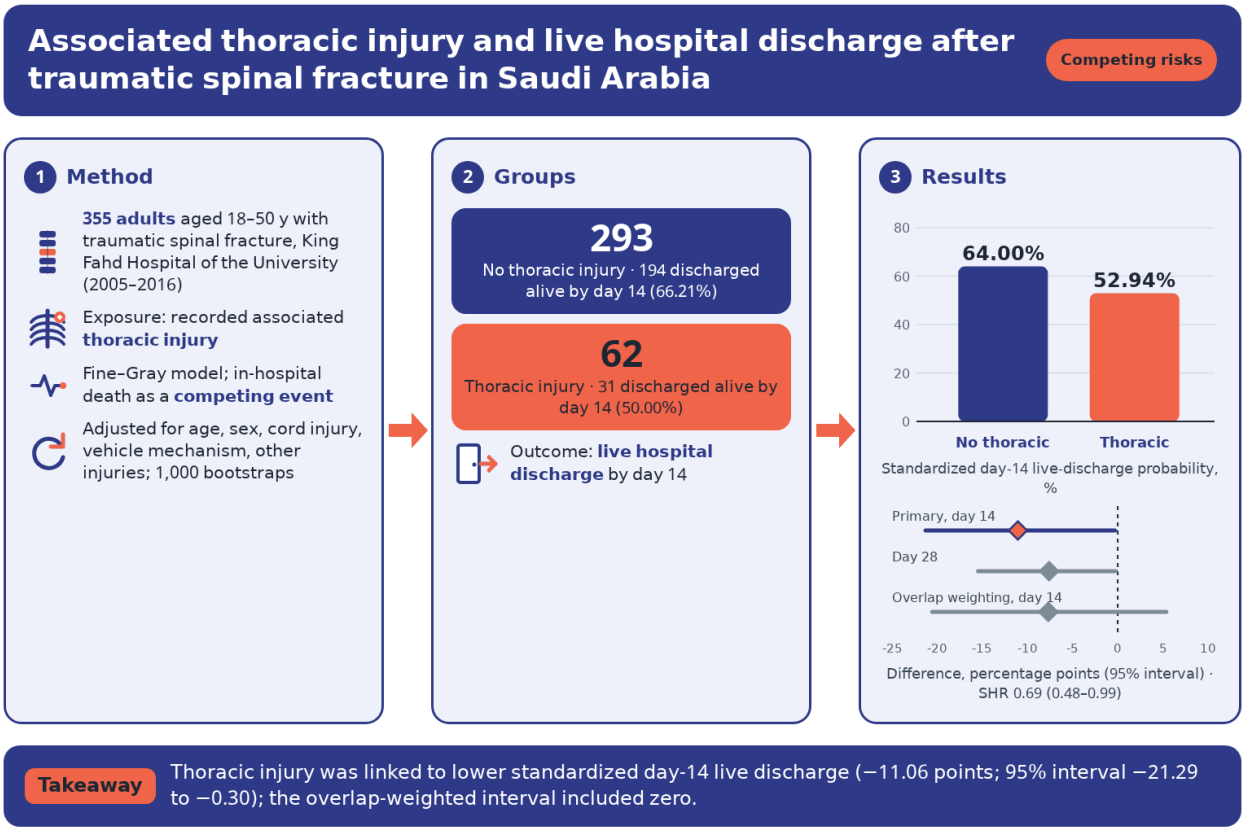
* Correspondence: Fatema Alawad, fatemalumora@gmail.com

English edition. An Arabic edition of this article is available.

Cite as: Alawad F, Kadri A, Alkhamisi AA, Alghamdi AM, Almebki AA, Issa AE. Associated thoracic injury and live hospital discharge after traumatic spinal fracture in Saudi Arabia: a competing-risk secondary cohort analysis. Multidisciplinary Healthcare Journal. 2025;1(4):MHJ-2026-000342. © The authors. Published open access under the Creative Commons Attribution 4.0 International licence (CC BY 4.0), https://creativecommons.org/licenses/by/4.0/.

Abstract

Graphical abstract



Background: Hospital duration after spinal trauma can end in live discharge or death. We examined recorded associated thoracic injury and day-14 live discharge in an openly released Saudi cohort, accounting for in-hospital death.

Methods: A locally frozen plan used 355 adults aged 18–50 years treated during 2005–2016. Fine–Gray regression adjusted for age, sex, spinal cord injury, motor vehicle mechanism and the number of other injured systems. Case-mix-standardized live-discharge probabilities were estimated through day 28, with day 14 primary. Patient bootstraps supplied intervals. Planned sensitivity analyses addressed injury burden, calendar year, missing cord status and contradictory death/follow-up codes; overlap weighting provided an alternative comparison.

Results: 62 patients had associated thoracic injury. Standardized day-14 discharge probabilities were 64.00% without and 52.94% with injury: difference -11.06 percentage points (95% bootstrap interval, -21.29 to -0.30). The subdistribution hazard ratio was 0.69 (0.48–0.99). Excluding two death-coded records with later follow-up codes gave a difference of -11.02 points (-22.32 to 0.06). The overlap-weighted difference was -7.69 points (-20.55 to 5.38). All 4,000 planned bootstrap fits succeeded. Published and released counts differed for several source fields.

Conclusions: Recorded thoracic injury was associated with lower modeled early live-discharge probability in this historical cohort. Alternative weighting and coding sensitivity show important uncertainty. Unmeasured severity, selected completed stays and unresolved source discrepancies prevent causal, treatment or discharge-readiness claims.

Keywords: spinal trauma; thoracic injury; Saudi Arabia; live discharge; competing risks; anesthesiology; trauma care

Introduction

Traumatic spinal injury in Saudi Arabia has been described across several hospital cohorts, with motor vehicle collisions and falls prominent among recorded mechanisms [1,2,3,4,5]. Those studies concern different hospitals, periods and selected patient groups; they are not interchangeable national incidence estimates. They nevertheless establish that associated injuries, neurological involvement and hospital outcomes are existing subjects of Saudi trauma research. An additional analysis must therefore specify what it adds beyond another description of injury frequency. For perioperative and critical-care teams, a useful question is how a readily recorded injury marker relates to the course of hospitalization while preserving the distinction between survival and discharge.

Associated thoracic injury is relevant to this question because the chest and spine can be injured in the same traumatic episode. In an operative spinal-fracture cohort, Weinberg and colleagues examined severe chest injury and spinal cord injury in relation to pulmonary complications; Childs and colleagues studied neurological impairment and respiratory-support outcomes after cervical injury [6,7]. These investigations used clinical severity or respiratory measurements that are absent from the present release. They motivate attention to associated chest trauma but do not establish the mechanism behind a discharge association in a less detailed dataset. In particular, a binary thoracic-injury field cannot distinguish pulmonary contusion, rib injury, pleural injury or their physiological consequences.

The source study by Aldakheel and colleagues described spinal fractures in patients aged 50 years or younger at King Fahd Hospital of the University, Khobar, during 2005–2016 [8]. It already reported longer hospitalization among patients with any associated injury. The present work is a secondary analysis of that same released cohort. It is not an independent replication or the first Saudi demonstration that patients with additional trauma may stay longer. Its narrower contribution is a defined associated-thoracic-injury comparison, an explicit admission time origin, separate handling of live discharge and in-hospital death, standardized absolute probabilities and an audit of the public coding.

Hospital length of stay is not a single-direction measure of recovery. A short stay may end because a patient improves and leaves alive or because the patient dies early. Analyses that treat both endings as equivalent short durations can obscure this distinction. Research on trauma-service outcomes has shown how time to live discharge can be analyzed with death as a competing event, while methodological work distinguishes cumulative incidence from a survival complement that removes competing events [9,10,11]. The relevant probability here is discharge alive by a specified day in the presence of the observed mortality process. It is not the hypothetical discharge probability in a world where death could not occur.

A relative hazard alone does not communicate that probability. A Fine–Gray subdistribution hazard ratio describes the modeled cumulative-incidence process and must not be interpreted as a ratio of discharge probabilities or as the instantaneous discharge rate among patients still alive in hospital [12,13]. We therefore designated the standardized day-14 probability difference as primary and used the subdistribution hazard ratio as a companion summary. The day-14 horizon was fixed before exposure–outcome associations were examined. Day 28 supplied a secondary horizon and a common administrative end for modeling; neither horizon defines a clinical requirement to discharge a patient by that day.

The release also provides an opportunity to examine what an apparently straightforward hospital dataset can and cannot support. Its first worksheet contains patient records and coded injury fields, while other sheets contain derivative subsets. Published counts do not agree perfectly with the canonical worksheet,

and two death-coded records also carry long-term follow-up codes. These features require transparent treatment rather than undocumented correction. We aimed to estimate one conditional injury-marker association, quantify its uncertainty under prespecified alternatives, and identify the clinical and data requirements for a more definitive contemporary Saudi investigation.

Methods

Design, source and reproducibility

This retrospective secondary cohort analysis used version 2 of the Figshare dataset deposited by Aldakheel, DOI 10.6084/m9.figshare.22960979.v2, with CC0 repository metadata [14]. The original workbook and its hash were retained. Only the first sheet, “Corrected 2005-2016<50,” was used. Its numeric patient-ID rows begin after the fourteenth spreadsheet row; header definitions, blank rows and derivative worksheets were not pooled as additional observations. The analysis preserved raw row numbers and identifiers in an exclusion log and a separate derived cohort.

The source publication remains version 1, published in 2023 with its displayed status awaiting peer review [8]. It describes a single trauma-center record review and excludes transfers to another hospital, refusal of medical advice, pathological or low-energy osteoporotic fractures, and isolated sacral fractures. These source-level exclusions cannot be re-created in full from the public worksheet. Our target population is therefore the released, selected cohort, with additional adult eligibility applied transparently. We did not access hospital charts, recruit patients or recover excluded source patients.

A statistical plan was frozen locally after reading the source, inspecting column definitions and reviewing univariate completeness and value ranges, but before thoracic-injury/discharge cross-tabulations or fitted associations. The frozen plan and timestamp are preserved; no prospective public registration is claimed. Analyses were implemented in Python with the raw extraction, matrix construction, model fitting, bootstrap draws and locked results supplied. STROBE and applicable RECORD items guided reporting [15,16]. These reporting frameworks do not constitute clinical validation or an institutional determination about secondary use.

Population, index and follow-up

The release contains 373 unique numeric patient records. We restricted the primary analysis to age 18–50 years, positive hospital-stay duration and complete prespecified model variables. Seventeen patients were younger than 18, and one adult lacked recorded spinal-cord-injury status. The resulting primary cohort contained 355 patients. The missing adult record was not assigned a status in the primary analysis. Two planned deterministic assignments later examined that specific uncertainty without implying a justified missing-at-random mechanism. No outcome-driven record exclusions or statistical outlier removals were made.

The index was hospital admission, and the time scale was the released hospital-stay duration in days. The workbook records positive integer durations, ranging from 1 to 156 days in the source cohort. It does not provide exact event clock times. A stay was treated as ending in in-hospital death when the complication code equaled four, consistent with the workbook legend and the source's statement that nine patients died in hospital. Other completed stays were interpreted as live discharges. This is an explicit reconstruction from the released coding, not independently verified vital-status or discharge documentation.

The analysis followed each recorded stay until its terminal event or administrative day 28, whichever came first. All source rows contain a positive terminal stay duration, and the source excluded transfers and refusal of advice before release. Thus no loss-to-follow-up time is recorded within the selected completed stays. The assumption needed for the risk-set calculation is that these durations represent complete terminal hospitalization times, rather than unrecorded interim observations. The audit does not prove that assumption from clinical records; it states the evidence and boundary on which the analysis depends.

Death before live discharge was a competing event. Deaths occurring after day 28 were not treated as events within the analysis horizon, and their recorded stays remained under observation through the common administrative end. We did not use the later follow-up code as the terminal hospital outcome or substitute it for admission-based time. We also did not estimate postdischarge mortality, readmission, functional recovery or destination after discharge because those outcomes cannot be reconstructed from the available fields.

Exposure and measured case mix

The exposure was the binary “Thoracic” associated-injury field, distinct from the columns describing the vertebral fracture region. It represents recorded associated chest-region injury and is not synonymous with a fracture of the thoracic spine. The public variable does not provide injury subtype, an anatomical severity grade, respiratory physiology or a diagnostic timestamp. We treated it as an injury marker from the presenting trauma episode, rather than a treatment or an incident postoperative complication. The exact timing of clinical recognition relative to admission cannot be established from the worksheet.

Five covariates accompanied the exposure: age in decades centered at 30, female sex, any recorded spinal cord injury, motor vehicle accident mechanism, and the number of other injured systems. Spinal cord injury combined complete and incomplete codes versus the normal code. The other-injury count summed six explicit binary fields: central nervous system, abdominal, pelvic, urological, upper-limb and lower-limb injury. It excluded the thoracic exposure. Each positive field contributed one irrespective of the number or severity of injuries within that system.

This count is a coarse description of recorded injury burden, not an Injury Severity Score or a substitute for validated anatomical and physiological severity assessment [17]. The workbook's mixed-format associated-injury string contains ambiguous numeric entries, so the prespecified explicit binary columns took precedence. Their domains were checked as zero or one. We did not reinterpret malformed strings after observing model results. The source's original total-injury field and treatment fields were retained for audit but did not replace the defined adjustment variables.

The adjustment set describes conditional comparisons within the measured case mix. Other injuries occur in the same traumatic episode and may share causes with thoracic injury; their inclusion does not establish a causal confounding structure or isolate the effect of preventing chest trauma. Surgery, surgical timing, approach and subsequent complications were excluded from baseline adjustment because they can lie after the injury and reflect later management. No automated variable selection, exposure cut-point search or interaction screening determined the primary model.

Competing-risk model and standardized probabilities

We fitted a Fine–Gray proportional subdistribution-hazard model for live discharge through day 28 with six slopes, including thoracic injury [12,13]. The model has no ordinary intercept; the unspecified baseline cumulative subdistribution hazard supplies the time component. At each live-discharge day, the

subdistribution risk set retained patients who had not yet had a terminal event and also retained those who had died earlier. With complete terminal stay times and only a common administrative end, competing-death records have retention weight one through the horizon. No estimated censoring-survival weights were necessary under this specific data structure.

Live discharges tied on the same integer day were handled using the Breslow partial-likelihood convention. Deaths and discharges at a common day were included in the appropriate simultaneous event processing for crude cumulative incidence. Exact ordering within a day was neither observed nor invented. The modeling convention is an approximation for grouped hospital days, not evidence that event times were recorded continuously. A discrete-time competing-risk model could address that measurement scale differently; it was not substituted after inspecting results [18].

We estimated Breslow baseline cumulative subdistribution-hazard increments at the observed live-discharge days. For each primary patient, the thoracic indicator was set in turn to zero and one while other recorded covariates were held at their observed values. Model-based cumulative incidence at day 14 was calculated as one minus the exponential of the negative cumulative subdistribution hazard, including the fitted covariate multiplier. Averaging these predictions over the primary cohort produced two standardized discharge probabilities. The primary difference was the probability with thoracic injury minus that without injury, expressed in percentage points.

Standardization places the two fitted comparisons over the same measured covariate distribution. It does not create exchangeability, establish positivity in every covariate combination or identify the outcome of an intervention that removes thoracic injury. We retained the observed age restriction and avoided extrapolation to older trauma patients. Day-28 probabilities and the subdistribution hazard ratio were secondary summaries from the same primary fit. A lower subdistribution hazard ratio was not described as a proportional reduction in the chance of discharge.

A patient-level nonparametric bootstrap resampled whole records 1,000 times, using a fixed seed and refitting coefficients and baseline hazard in each sample. The 2.5th and 97.5th percentiles defined intervals for standardized risks, risk differences and the hazard ratio. Every attempted draw and any failure were retained; no failed draw would be replaced. This interval captures sampling variability conditional on the chosen source coding and model. It does not represent uncertainty from unknown clinical misclassification, omitted severity measures or the choice of hospital.

Crude outcomes, sensitivities and diagnostics

Crude live-discharge and death cumulative incidence were calculated separately by exposure using Aalen–Johansen event accounting [9,10,11]. The analysis reports both endpoints and the number remaining in hospital at days 14 and 28. In this completed-stay dataset without pre-horizon censoring, each crude cumulative incidence equals the corresponding observed event proportion by that day. That identity provided an independent arithmetic check. One minus Kaplan–Meier with deaths censored was not used as the observed discharge probability.

A cause-specific Cox model used the identical covariates while ending the live-discharge risk interval at death [19]. Its hazard ratio describes live-discharge rates among patients still alive and hospitalized and is a different estimand from the Fine–Gray summary. Its companion Wald interval uses model-information covariance and is labeled separately from the primary patient-bootstrap intervals. We also added thoracic

injury multiplied by log(day/14) to the subdistribution model as a prespecified proportionality diagnostic. The likelihood comparison does not certify proportionality when the interaction is imprecisely estimated.

Five planned Fine–Gray sensitivity fits omitted the other-injury count, excluded the two death/follow-up contradictions, added calendar year per five years centered at 2010, or included the adult with missing cord status under either normal or injured assignment. Each used 400 patient bootstraps. The first changes the measured-case-mix question, the second changes the source cohort, the third supplies a limited calendar check, and the final two explore one coding uncertainty. None was used to replace the primary estimate because it produced a narrower interval. Calendar adjustment does not estimate the effect of traffic legislation or a change in trauma care.

The alternative overlap-weighted comparison used logistic regression for thoracic-injury status with an intercept and the same five baseline covariates [20]. Weights were one minus the fitted exposure probability for exposed patients and the fitted probability for unexposed patients. These weights emphasize covariate profiles shared by the two groups. The weighted outcome was live discharge by day 14, with death or continued hospitalization counted as absence of that event. All patients' status at this horizon was available under the terminal-stay reconstruction, so this fixed-horizon calculation did not require a survival-censoring adjustment.

We reported effective sample sizes, propensity ranges and standardized mean differences before and after weighting, and used 1,000 patient bootstraps for its risk-difference interval. The overlap comparison targets a different measured-case-mix population from full-cohort standardization; differences in estimates need not indicate a software error. Balance applies to the variables and moments actually fitted, not to unmeasured physiology or severity. This use of a propensity model does not convert an injury-marker analysis into a treatment-effect study.

Source audit and independent verification

The raw file was checked against the source article's reported counts and the workbook's embedded legend. The canonical sheet contains 63 thoracic-injury flags, 36 cord-injury codes and 128 operative-treatment codes, whereas the article reports 62, 35 and 129, respectively. These are examples of release-versus-publication differences before our adult restrictions; several other injury-system totals also differ. Thirteen rows have disagreement between the global any-injury indicator and whether an explicit system field is positive. Two of those rows carry a thoracic flag. The explicit system-count sum agrees with the released total-injury count for every row. We preserved the frozen field hierarchy rather than deciding after analysis which conflicting value was clinically correct. Two records combine the death code with a later follow-up category denoting more than 24 months. Their primary coding follows the prespecified hospital-death definition, and the exclusion sensitivity documents the impact of that choice.

An independent script reconstructed the raw first worksheet using a separate reader, built a dense time-by-patient risk-set matrix and optimized the likelihood without importing production functions. It checked all six Fine–Gray model points, cause-specific and time-interaction fits, standardized risks, crude event probabilities, overlap estimates and every saved bootstrap percentile. Twenty-one selected bootstrap samples were independently regenerated and refitted. Agreement verifies the specified numerical calculations conditional on source mapping and assumptions; it is not a clinical chart audit.

Results

Cohort and observed course

The primary cohort comprised 355 adults, including 62 with and 293 without associated thoracic injury. Median age was 28 years in the thoracic group and 29 years in the other group. Women accounted for 3/62 and 66/293, respectively. Motor vehicle mechanism and recorded cord injury were more common in the thoracic group, and its median other-injury count was higher (Table 1). These differences underscore why an unadjusted contrast and a standardized comparison address different measured case mixes.

By day 14, 31/62 patients with thoracic injury had been discharged alive, compared with 194/293 without it: crude cumulative incidence 50.00% versus 66.21%. There were 4 versus 0 in-hospital deaths by that day, and 27 versus 99 remained in hospital. At day 28, crude live-discharge incidence was 67.74% versus 86.35%, and death incidence was 6.45% versus 0.34%. Figure 1 and Table 2 preserve the distinction between the two terminal events.

Across the full primary terminal stays, eight patients had death codes, but only five deaths occurred by day 28. There were 295 live discharges through day 28 and 55 patients still hospitalized at that administrative endpoint. The later deaths remained outside the event horizon. The missing cord-status adult had a nine-day stay without a death code and no thoracic-injury flag. Source exclusions and publication-count discrepancies are summarized in Table 4.

Primary standardized comparison

The standardized day-14 live-discharge probability was 64.00% without and 52.94% with thoracic injury. The probability difference was -11.06 percentage points, with a 95% patient-bootstrap interval from -21.29 to -0.30. The subdistribution hazard ratio was 0.69, with bootstrap interval 0.48–0.99. The upper probability-difference limit was close to zero; the estimate should therefore be interpreted with its uncertainty rather than as a sharp clinical separation between groups.

At day 28, standardized probabilities were 83.83% and 76.23%, respectively, giving a difference of -7.60 points (-15.47 to -0.20). This is a secondary horizon from the same fitted model, not an independent replication. Figure 2 displays both horizons and the separate group intervals. The primary model's information-matrix condition number was 13.42, and all 1,000 primary bootstrap fits succeeded. These numerical properties do not resolve unmeasured injury severity or verify the clinical meaning of each source code.

Sensitivity analyses and competing estimands

Omitting the other-injury count gave a day-14 difference of -20.57 points (-32.01 to -9.87), more negative than the primary conditional contrast. Excluding the two death/follow-up contradictions gave -11.02 points (-22.32 to 0.06), with an interval that includes zero. Adding calendar year gave -10.36 points (-20.79 to -0.46). The missing cord-status assignments produced point differences of -11.05 and -11.14 points. All six Fine–Gray estimates and intervals appear in Table 3 and Figure 3.

The cause-specific live-discharge hazard ratio was 0.70 (information-based Wald interval, 0.50–0.97). The thoracic-by-log-time coefficient was -0.231, and the one-degree-of-freedom likelihood-ratio statistic was 1.032 (P=0.310). The diagnostic did not establish a clear time interaction, but it cannot exclude clinically important departures in a dataset with a limited exposed group. The cause-specific and subdistribution

estimates summarize related processes under different risk sets; numerical similarity does not make their interpretations identical.

The overlap-weighted day-14 probabilities were 62.54% without and 54.85% with thoracic injury, for a difference of -7.69 points (-20.55 to 5.38). Effective sample sizes were 199.3 and 59.0, and fitted exposure probabilities ranged from 0.014 to 0.750. The largest absolute fitted-covariate standardized mean difference was 0.617 before weighting and below 0.001 afterward. Despite measured balance, the interval includes both a substantial negative difference and a small positive difference. All 3,000 Fine–Gray and 1,000 overlap-bootstrap fits succeeded; no failed draws were discarded or replaced.

Discussion

Recorded associated thoracic injury was linked to a lower modeled probability of live hospital discharge by day 14 in this selected historical Saudi spinal-fracture cohort. The primary standardized difference was approximately -11.06 percentage points after conditioning on five recorded case-mix variables. The bootstrap interval extended from a materially lower probability to a value close to zero. The overlap-weighted analysis was less negative and more imprecise, while the exclusion of two contradictory death records retained a similar point estimate with an interval spanning zero. Taken together, these findings support a conditional association that deserves further study, rather than a definitive causal or individual prognostic claim.

The analysis builds on existing Saudi evidence rather than discovering the general importance of associated trauma. The source article already found longer stays with any associated injury [8]. Saudi cohorts reported by Alawad, AlEissa, Algahtany and Alkhathlan describe mechanisms, neurological involvement, associated injuries or hospital outcomes in different clinical populations [1,2,3,4,5]. The two Algahtany reports concern related Southern Saudi trauma populations and should not be counted as independent replications merely because they are separate publications. None of those contexts makes the present single-center estimate representative of all Saudi hospitals or of contemporary care.

Our focused estimand differs from a comparison of average hospital duration. Death changes the opportunity for live discharge, and early fatal stays can make a simple duration comparison difficult to interpret. Four primary patients with thoracic injury died by day 14, whereas no patient without the flag had a death coded by that day. Keeping those outcomes separate matters even though the overall number of deaths is small. The discharge cumulative incidence incorporates both the rate of live discharge and the competing mortality process; a lower value cannot be attributed entirely to slower recovery among survivors.

The relative and absolute summaries also answer different questions. The subdistribution hazard ratio of 0.69 is not a statement that the day-14 probability is reduced by one minus that ratio. The standardized probabilities provide the corresponding model-based absolute comparison over the measured cohort. The cause-specific estimate concerns discharge among patients currently alive and hospitalized and therefore excludes people after death from its risk set. Reporting both models can clarify the process, but neither hazard model automatically identifies a causal effect or supports treating a complication-free short stay as universally better care [12,13][19].

Associated thoracic injury may identify patients with a more complex traumatic episode. The primary groups differed in mechanism, neurological injury, sex and the burden of other injured systems. The stronger contrast after omitting the other-injury count shows that this conditioning choice materially affects

the descriptive question. It does not show that the remaining adjusted coefficient is a direct chest-injury effect. A common injury mechanism may influence several organ systems, and the coarse count cannot capture the anatomical severity within each system or the physiological consequences. Residual differences in severity could account for some or all of the association.

Respiratory complications are a plausible clinical concern, but they were not measured here. Weinberg and colleagues studied pulmonary complications after operative spinal-fracture fixation and included severe chest injury among the evaluated factors; Childs and colleagues incorporated neurological impairment and chest severity when studying tracheostomy [6,7]. Those richer measurements highlight the limitations of the current binary exposure. We cannot determine whether ventilatory support, chest procedures, pulmonary contusion, pain-limited breathing or another process contributed to delayed discharge. No ICU-admission, mechanical-ventilation or analgesia outcome should be inferred from this workbook.

The overlap analysis offers useful context precisely because it does not reproduce the same target population by construction. Full-cohort standardization averages model predictions over all included adults, including covariate patterns that may be poorly represented in one exposure group. Overlap weights emphasize patterns more common to both groups. Their nearly exact balance on the fitted covariate means is consistent with the logistic weighting construction, but it does not show balance on unmeasured characteristics [20]. The weighted interval crosses zero and is too broad to support equivalence. Treating that result as confirmation of a precise effect would ignore both its uncertainty and its different target.

The small number of women with recorded thoracic injury also limits detailed comparisons. A model with a female indicator can adjust a mean contrast under its functional form, but it cannot recover the missing clinical diversity of a sparsely represented subgroup. We did not fit exposure-by-sex or exposure-by-neurological-injury interactions because those were not part of the frozen question and the source does not support detailed effect-modification claims. A future study should examine covariate support explicitly and define whether its goal is a population-wide adjusted association or a comparison restricted to clinically comparable patients.

The source discrepancies are consequential even when sensitivity point estimates are stable. Two death-coded records have stays of two days and a follow-up category corresponding to more than 24 months. The release does not explain whether the death code, duration, follow-up field or another aspect of record linkage is wrong. Removing the records explores one plausible analytic boundary but does not identify the correct clinical outcome. Their exclusion does not authorize reclassifying them as survivors, and retaining them does not authenticate the recorded deaths. Reconciliation requires the source investigators or a permitted chart review.

Likewise, the differences between published and released counts cannot be repaired by selecting values that produce agreement with the paper. The public thoracic flag totals 63 rather than the source's 62; cord injury and operative-treatment totals also differ by one. The adult thoracic group happens to contain 62 records after the prespecified age restriction, but this numerical coincidence does not resolve the all-age source discrepancy. The explicit fields, frozen mapping and exclusion log make the result reproducible despite that uncertainty. They do not establish that the current analysis has recovered the exact original analytic dataset.

Completeness within the release should not be confused with completeness of the source population. The original study excluded patients transferred elsewhere and those who refused medical advice. Those

pathways may relate to injury severity, resource availability, patient preference and disposition. The absence of censoring in the released completed stays is what permits retention weights of one after competing death through the common administrative horizon; it does not imply that no patients were lost or selected out before the public cohort was formed. Transporting this calculation to an unselected admissions registry would require recording and handling additional exit processes explicitly.

The time scale imposes another boundary. Integer hospital days cannot distinguish the order of events occurring within the same day, and the analysis used prespecified Breslow ties rather than simulated clock times. The proportionality diagnostic did not show a clear exposure-by-log-time departure, but a nonsignificant test cannot certify a constant hazard ratio. Discrete-time methods provide a different framework for such grouped observations [18]. A future confirmatory study should choose its time scale and tie strategy prospectively, collect exact admission and exit times if clinically meaningful, and retain the standardized absolute-risk question even if a different model is selected.

Calendar adjustment was deliberately modest. The source period spans 2005–2016, and hospital organization, referral patterns, imaging, surgical practice and rehabilitation arrangements may have changed. A single linear year term cannot disentangle these influences. In particular, the present analysis does not estimate a transportation-policy effect or attribute changes in injury patterns to legislation. Current Saudi trauma services may care for a different population under different pathways, making contemporary multicenter data necessary before using the reported probabilities for planning or counseling.

The endpoint is hospital exit alive, not discharge readiness or functional recovery. A patient may remain hospitalized because of rehabilitation access, home support, discharge destination or administrative delay after acute problems have stabilized. Conversely, earlier discharge does not guarantee absence of pain, neurological disability, later complications or readmission. The release has no validated readiness assessment or destination field suitable for separating these possibilities. The results should therefore inform a research question about the observed hospital course, not a benchmark encouraging earlier discharge or a measure of the quality of an individual clinician's care.

Several features strengthen the transparency of this secondary analysis: an unchanged public workbook, a frozen estimand and adjustment set, preserved source restrictions, explicit competing-event handling, a complete record of resampling and an independent numerical reconstruction. Each has a limited role. Computational agreement reduces concern about arithmetic or software implementation; it cannot compensate for incomplete clinical variables, inconsistent coding or a selected historical sample. The article's awaiting-peer-review status and the need for source reconciliation remain relevant when interpreting the analysis.

A stronger follow-up investigation would begin with consecutive trauma admissions at several Saudi centers and retain transfers, deaths, live discharges and discharge destinations as distinct observed pathways. It should record validated anatomical and physiological severity measures, detailed chest-injury subtype, neurological status, respiratory support, perioperative management and readiness for discharge. Clinical definitions and coding checks should be agreed before database lock, and the role of surgery should follow an explicit temporal question rather than routine adjustment. Such a design could test whether the present marker association persists and whether it adds useful information beyond standard severity assessment. An intervention study would be required to establish whether modifying a particular management strategy improves outcomes.

Conclusion

Associated thoracic injury was linked to lower standardized day-14 live-discharge probability in this released Saudi spinal-trauma cohort. The point estimate was sensitive to the measured-case-mix comparison, and uncertainty included no difference in important alternative analyses. Competing mortality, incomplete severity measurement, selected completed stays and unresolved source discrepancies limit interpretation. These findings support carefully specified contemporary research, not treatment changes, individual prediction or a discharge target.

Tables

Table 1. Primary cohort characteristics by recorded associated thoracic injury.

Characteristic	No thoracic injury (n=293)	Thoracic injury (n=62)
Age, years	29 (24–37)	28 (22.25–35)
Recorded year	2011 (2008–2013)	2010 (2009–2012.75)
Other injured systems, count	0 (0–1)	1 (0–1)
Female	66 (22.5)	3 (4.8)
Any spinal cord injury	25 (8.5)	10 (16.1)
Motor vehicle mechanism	193 (65.9)	56 (90.3)
Central nervous system injury	24 (8.2)	14 (22.6)
Abdominal injury	7 (2.4)	14 (22.6)
Pelvic injury	14 (4.8)	7 (11.3)
Urological injury	2 (0.7)	0 (0.0)
Upper-limb injury	26 (8.9)	7 (11.3)
Lower-limb injury	55 (18.8)	9 (14.5)

Values are median (IQR) or count (%); quantiles use linear interpolation. The exposure is associated chest-region injury, not thoracic vertebral location. Other-injury count sums six explicit fields and excludes the thoracic flag; it is not an Injury Severity Score.

Table 2. Observed outcomes and standardized live-discharge probabilities.

Day	Quantity	No thoracic injury	Thoracic injury
14	Observed live discharge	194/293 (66.21%)	31/62 (50.00%)
14	Observed death	0/293 (0.00%)	4/62 (6.45%)
14	Remaining in hospital	99/293 (33.79%)	27/62 (43.55%)
14	Standardized live-discharge probability	64.00% (59.22–68.96)	52.94% (42.80–63.80)
28	Observed live discharge	253/293 (86.35%)	42/62 (67.74%)
28	Observed death	1/293 (0.34%)	4/62 (6.45%)

Day	Quantity	No thoracic injury	Thoracic injury
28	Remaining in hospital	39/293 (13.31%)	16/62 (25.81%)
28	Standardized live-discharge probability	83.83% (79.81–87.13)	76.23% (68.16–83.58)

Observed terminal-event proportions equal the crude Aalen–Johansen cumulative incidence because no pre-horizon loss to follow-up is recorded. Death and live discharge are distinct competing events. Standardized estimates average over the same primary case mix, with percentile 95% intervals from 1,000 patient bootstrap fits. Day 14 is primary; day 28 is secondary. Patients still hospitalized at day 28 are administratively censored.

Table 3. Primary and planned sensitivity estimates.

Analysis	Patients	Subdistribution HR (95% interval)	Day-14 difference, percentage points (95% interval)
Primary	355	0.685 (0.484–0.988)	-11.06 (-21.29 to -0.30)
Omit other-injury count	355	0.554 (0.377–0.759)	-20.57 (-32.01 to -9.87)
Exclude contradictory death follow-up	353	0.688 (0.462–1.002)	-11.02 (-22.32 to 0.06)
Add calendar year	355	0.700 (0.492–0.983)	-10.36 (-20.79 to -0.46)
Missing SCI assigned normal	356	0.686 (0.465–0.979)	-11.05 (-21.44 to -0.54)
Missing SCI assigned injured	356	0.684 (0.482–0.936)	-11.14 (-21.38 to -2.00)
Overlap-weighted comparison	355	—	-7.69 (-20.55 to 5.38)

Differences are thoracic minus no thoracic injury. Primary intervals use 1,000 bootstrap samples; each Fine–Gray sensitivity uses 400; overlap weighting uses 1,000. All attempted samples succeeded. Overlap weighting targets shared covariate profiles and does not estimate a subdistribution hazard ratio. Its effective sample sizes are 199.3 without and 59.0 with thoracic injury. These analyses alter the conditioning set, cohort or target population and are not independent replications.

Table 4. Source-population and coding audit.

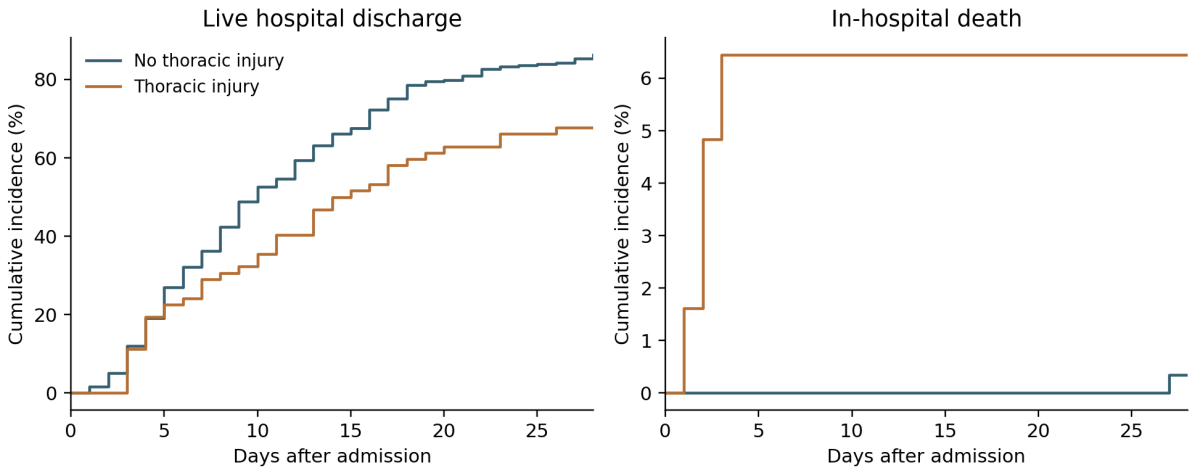
Audit item	Count or comparison	Interpretation
Released unique patient records	373	Canonical first worksheet only
Below age 18	17	Excluded from adult primary cohort
Adult missing cord-injury status	1	ID 207; primary exclusion; two assignment sensitivities
Primary adults	355	62 thoracic flags; 293 without
Deaths over complete primary stays	8	Five by day 28; three later
Death plus >24-month follow-up code	2	IDs 166 and 241; two-day stays; unresolved
Thoracic flags: release / article	63 / 62	Comparison before adult restriction
Cord injury: release / article	36 / 35	Complete plus incomplete codes
Operative treatment: release / article	128 / 129	Treatment not used in primary adjustment

Audit item	Count or comparison	Interpretation
Global any-injury versus explicit flags	13	IDs retained under frozen explicit-field rule; two have thoracic flags

The raw coding was preserved. Excluding contradictory records is a sensitivity analysis, not proof that either field is incorrect. The adult thoracic count of 62 does not resolve the different all-age publication/release totals. The source excluded interhospital transfers and refusal of advice before public release.

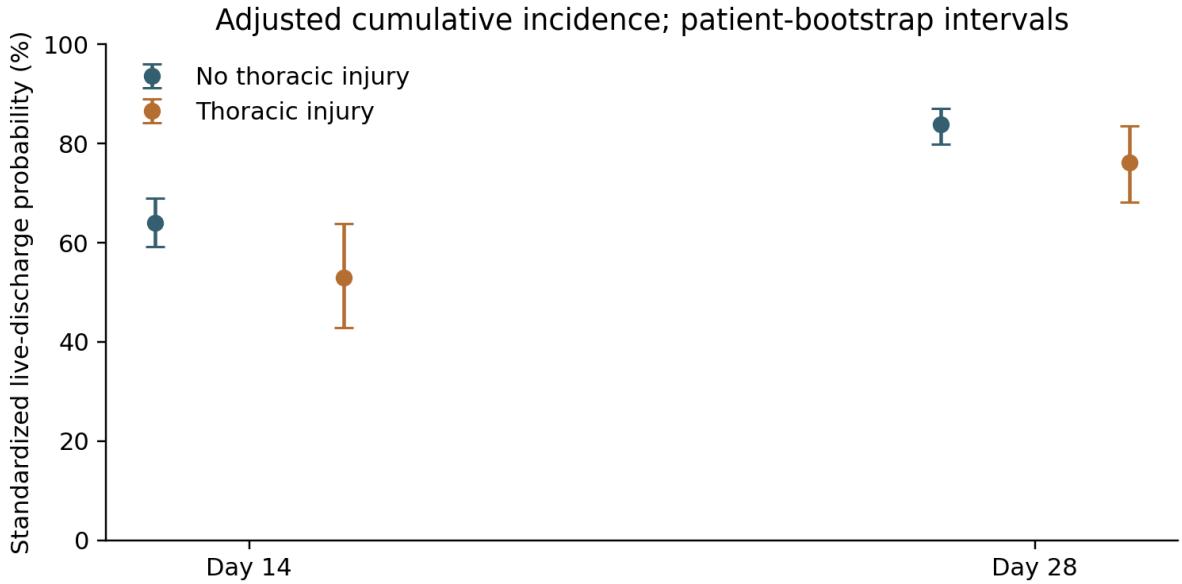
Figures

Figure 1. Crude cumulative incidence after admission.



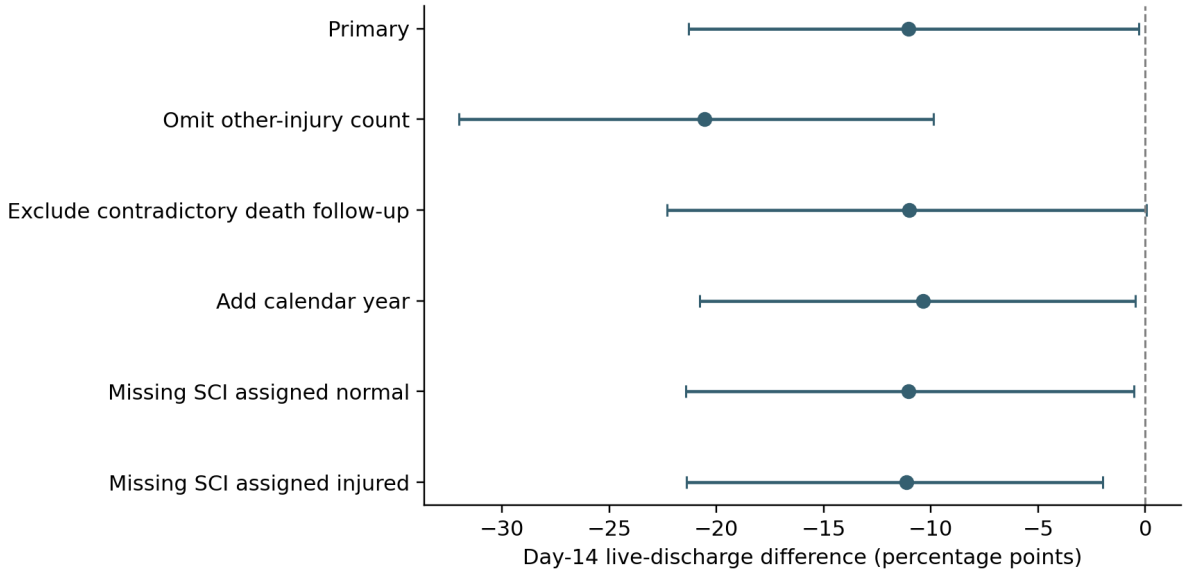
Live hospital discharge and in-hospital death are shown separately by the recorded associated-thoracic-injury flag. Death is a competing event for discharge. The two panels have separately labeled outcome scales, and the curves describe the primary adult cohort through day 28.

Figure 2. Standardized live-discharge probabilities.



Points show model-based probabilities at days 14 and 28 after averaging over the primary cohort's measured covariates. Bars are percentile intervals from 1,000 patient bootstrap fits. Day 14 is primary; day 28 is secondary. These are conditional injury-marker comparisons, not intervention effects.

Figure 3. Day-14 standardized probability differences across planned Fine–Gray analyses.



Differences are thoracic injury minus no thoracic injury in percentage points. Bars show patient-bootstrap intervals: 1,000 draws for the primary analysis and 400 for each sensitivity. The analyses alter covariates or source inclusion and do not represent independent replications. The distinct overlap-weighted comparison is reported in the tables and text.

Declarations and data availability

The public CC0 workbook is attributed to its depositor and Figshare version, with the source article cited separately. The package retains data provenance, a frozen analysis plan, code, exact cohort and matrix, bootstrap draws, results and independent numerical checks. The original publication reports an institutional ethics determination. The present secondary analysis was exempt from ethical review because the data are publicly available and de-identified. No patient contact or chart access occurred.

This research received no external funding. The authors declare no competing interests. Artificial intelligence was not used in the preparation of this manuscript.

References

1. Mohammad O Alawad, Nasser Alenezi, Bander S Alrashedan, et al. Traumatic spinal injuries in Saudi Arabia: a retrospective single-centre medical record review. BMJ open. 2020 Nov 14;10(11):e039768. doi:10.1136/bmjopen-2020-039768. PMID:33191261.

2. Sami AlEissa, Suhail Saad AlAssiri, Rayed Meshal AlJehani, et al. Neurological disability among adults following traumatic spinal fractures in Saudi Arabia: a retrospective single-center medical record review. Annals of Saudi medicine. 2019;39(1):8-12. doi:10.5144/0256-4947.2019.8. PMID:30712045.

3. M A Algahtany. Association between orthopedic fractures and traumatic spinal injury due to road traffic accidents. European review for medical and pharmacological sciences. 2021 Dec;25(24):7878-7886. doi:10.26355/eurrev_202112_27636. PMID:34982450.

4. Mubarak Ali Algahtany. Epidemiology, trend and in-hospital outcome of traumatic spinal injuries due to road traffic accidents. Pakistan journal of medical sciences. 2022;38(3Part-I):492-497. doi:10.12669/pjms.38.3.5288. PMID:35480551.

5. Khalid Mansour Alkhathlan, Mohammad Ghormallah Alzahrani, Khalid Hadi Aldosari, et al. Traumatic spinal injuries in the Kingdom of Saudi Arabia: a study of associated injuries, management and mortality. The Pan African medical journal. 2019;32:153. doi:10.11604/pamj.2019.32.153.18047. PMID:31303924.

6. Douglas S Weinberg, Brian Z Hedges, Jonathan E Belding, et al. Risk factors for pulmonary complication following fixation of spine fractures. The spine journal : official journal of the North American Spine Society. 2017 Oct;17(10):1449-1456. doi:10.1016/j.spinee.2017.05.008. PMID:28495240.

7. Benjamin R Childs, Timothy A Moore, John J Como, et al. American Spinal Injury Association Impairment Scale Predicts the Need for Tracheostomy After Cervical Spinal Cord Injury. Spine. 2015 Sep 15;40(18):1407-13. doi:10.1097/BRS.0000000000001008. PMID:26426711.

8. Aldakheel D, Ali MS, Alothman A, et al. Epidemiology of traumatic spinal fractures in patients aged 50 years or less at a center in Saudi Arabia from 2005 to 2016 [version 1; awaiting peer review]. F1000Research. 2023;12:819. doi:10.12688/f1000research.133527.1.

9. Brice Batomen, Lynne Moore, Erin Strumpf, et al. Addressing Competing Risks When Assessing the Impact of Health Services Interventions on Hospital Length of Stay. Epidemiology (Cambridge, Mass.). 2021 Mar 01;32(2):230-238. doi:10.1097/EDE.0000000000001307. PMID:33284168.

10. Peter C Austin, Douglas S Lee, Jason P Fine. Introduction to the Analysis of Survival Data in the Presence of Competing Risks. Circulation. 2016 Feb 09;133(6):601-9. doi:10.1161/CIRCULATIONAHA.115.017719. PMID:26858290.

11. T A Gooley, W Leisenring, J Crowley, et al. Estimation of failure probabilities in the presence of competing risks: new representations of old estimators. Statistics in medicine. 1999 Mar 30;18(6):695-706. doi:10.1002/(sici)1097-0258(19990330)18:6<695::aid-sim60>3.0.co;2-o. PMID:10204198.

12. Fine JP, Gray RJ. A proportional hazards model for the subdistribution of a competing risk. Journal of the American Statistical Association. 1999;94(446):496–509. doi:10.1080/01621459.1999.10474144.

13. Peter C Austin, Jason P Fine. Practical recommendations for reporting Fine-Gray model analyses for competing risk data. Statistics in medicine. 2017 Nov 30;36(27):4391-4400. doi:10.1002/sim.7501. PMID:28913837.

14. Aldakheel, Dakheel (2023). Spine Trauma 10 years KFHU, Saudi Arabia datasheet Coded.xlsx. figshare. Dataset. https://doi.org/10.6084/m9.figshare.22960979.v2. Version 2. CC0.

15. Erik von Elm, Douglas G Altman, Matthias Egger, et al. The Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) statement: guidelines for reporting observational studies. PLoS medicine. 2007 Oct 16;4(10):e296. doi:10.1371/journal.pmed.0040296. PMID:17941714.

16. Eric I Benchimol, Liam Smeeth, Astrid Guttmann, et al. The REporting of studies Conducted using Observational Routinely-collected health Data (RECORD) statement. PLoS medicine. 2015 Oct;12(10):e1001885. doi:10.1371/journal.pmed.1001885. PMID:26440803.

17. S P Baker, B O'Neill, W Haddon, et al. The injury severity score: a method for describing patients with multiple injuries and evaluating emergency care. The Journal of trauma. 1974 Mar;14(3):187-96. PMID:4814394.

18. Tomer Meir, Malka Gorfine. Discrete-time competing-risks regression with or without penalization. Biometrics. 2025 Apr 02;81(2):ujaf040. doi:10.1093/biomtc/ujaf040. PMID:40279120.

19. Cox DR. Regression models and life-tables. Journal of the Royal Statistical Society Series B. 1972;34(2):187–202. doi:10.1111/j.2517-6161.1972.tb00899.x.

20. Li F, Morgan KL, Zaslavsky AM. Balancing covariates via propensity score weighting. Journal of the American Statistical Association. 2018;113(521):390–400. doi:10.1080/01621459.2016.1260466.

Multidisciplinary Healthcare Journal · Vol. 1, No. 4 (2025) · CC BY 4.0 · https://creativecommons.org/licenses/by/4.0/