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The Impact of Emergency Medical Services on Trauma Outcomes and Mortality Reduction: A Systematic Review

أثر خدمات الإسعاف والطوارئ الطبية على نتائج الإصابات الرضّية والحد من معدلات الوفيات: مراجعة منهجية

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Abstract:

Background: Trauma mortality is influenced by events occurring between injury and definitive hospital treatment. Emergency Medical Services (EMS) coordinate field assessment, resuscitation, triage and transport, yet the mortality effects of individual prehospital interventions and transport strategies are inconsistent across trauma systems.
Aim: To synthesize evidence on the association between EMS organization and interventions and survival, functional outcomes and mortality among patients with traumatic injuries.
Methods: This structured narrative synthesis follows PRISMA 2020 reporting conventions and preserves the source-provided selection counts: 235 database records, 120 screened records, 62 reports assessed for eligibility and 14 studies included. Fourteen identifiable primary studies published from 2016 to 2025 were mapped across transport pathways, prehospital time, resuscitation, brain injury and triage. Original search exports and study-level selection logs were unavailable, so linkage between the mapped evidence and historical screening cannot be confirmed.
Results: Associations varied by clinical context. Several national registry studies reported improved adjusted survival with helicopter transport, whereas a small physician-staffed helicopter comparison reported no statistically significant difference. In penetrating trauma, longer response and scene times correlated with higher mortality, and private-vehicle transport was associated with lower mortality than ground EMS in selected urban cohorts, although selection bias remains substantial. Prehospital plasma reduced 30-day mortality in one air-medical trial but showed no significant benefit in rapid urban ground transport or in other blood-product trials. Out-of-hospital tranexamic acid did not significantly improve functional outcome in moderate-to-severe traumatic brain injury.
Conclusion: EMS can influence trauma survival through time-sensitive access, destination decisions and appropriately selected interventions. The evidence does not support a uniform survival benefit for every advanced intervention or transport modality. Trauma systems should emphasize rapid assessment, evidence-based triage, efficient transport, targeted resuscitation and rigorous outcome monitoring.

Keywords
Emergency Medical Services; Trauma; Prehospital Care; Mortality; Survival; Helicopter Emergency Medical Services; Emergency Transport; Response Time; Hemorrhagic Shock; Traumatic Brain Injury; PRISMA 2020.

الملخص
الخلفية :تتأثر معدلات الوفيات الناجمة عن الإصابات الرضّية بالأحداث والتدخلات التي تقع خلال الفترة الممتدة بين حدوث الإصابة وتلقي العلاج النهائي في المستشفى. وتضطلع خدمات الإسعاف والطوارئ الطبية (EMS) بدور محوري في تنسيق التقييم الميداني، والإنعاش، والفرز الطبي، ونقل المصابين. ومع ذلك، لا تزال تأثيرات التدخلات الفردية المقدمة قبل الوصول إلى المستشفى، واستراتيجيات النقل الإسعافي، على معدلات الوفيات متفاوتة بين أنظمة رعاية الإصابات المختلفة.
الهدف :توليف الأدلة العلمية المتعلقة بالعلاقة بين تنظيم خدمات الإسعاف والطوارئ الطبية والتدخلات التي تقدمها، وبين معدلات البقاء على قيد الحياة، والنتائج الوظيفية، والوفيات لدى المرضى المصابين بإصابات رضّية.
المنهجية :يعتمد هذا التوليف السردي المنظم على إرشادات إعداد التقارير وفق بيانPRISMA 2020 ، مع الاحتفاظ بأعداد اختيار الدراسات الواردة في المصدر، وهي: 235 سجلاً مستخرجاً من قواعد البيانات، و120 سجلاً خضعت للفحص، و62 تقريراً جرى تقييمها للتحقق من أهليتها، و14 دراسة أُدرجت في المراجعة النهائية. وقد جرى تصنيف 14 دراسة أولية أمكن تحديدها، ونُشرت خلال الفترة من 2016 إلى 2025، وفق خمسة محاور رئيسية، شملت مسارات النقل الإسعافي، والوقت المستغرق في الرعاية قبل المستشفى، والإنعاش، وإصابات الدماغ، والفرز الطبي. ونظراً لعدم توفر ملفات البحث الأصلية المستخرجة من قواعد البيانات وسجلات اختيار الدراسات على مستوى كل دراسة، تعذّر التحقق من مدى تطابق الأدلة المصنفة مع عملية الفرز والاختيار التاريخية للدراسات.
النتائج :تباينت الارتباطات المرصودة تبعاً للسياق السريري ونوع الإصابة. فقد أظهرت عدة دراسات قائمة على السجلات الوطنية تحسناً في معدلات البقاء على قيد الحياة بعد ضبط المتغيرات المصاحبة لدى المرضى المنقولين بواسطة المروحيات الإسعافية، في حين لم تُظهر دراسة مقارنة محدودة شملت مروحيات إسعافية مزودة بأطقم طبية يقودها أطباء فروقاً ذات دلالة إحصائية. وفي حالات الإصابات النافذة، ارتبط طول زمن الاستجابة وزمن البقاء في موقع الحادث بارتفاع معدلات الوفيات، كما ارتبط

النقل بواسطة المركبات الخاصة بانخفاض الوفيات مقارنة بالنقل عبر خدمات الإسعاف الأرضية في بعض الدراسات الحضرية، رغم استمرار احتمالية وجود تحيز كبير في اختيار وسيلة النقل. وعلى صعيد الإنعاش، أسهم إعطاء البلازما قبل الوصول إلى المستشفى في خفض معدل الوفيات خلال 30 يوماً في إحدى التجارب الخاصة بالنقل الجوي الطبي، إلا أن هذا التأثير لم يظهر بصورة ذات دلالة إحصائية في حالات النقل الأرضي السريع داخل المدن، ولا في تجارب أخرى تناولت منتجات الدم. كذلك، لم يُظهر إعطاء حمض الترانيكساميك خارج المستشفى تحسناً ذا دلالة إحصائية في النتائج الوظيفية لدى المرضى المصابين بإصابات دماغية رضّية متوسطة إلى شديدة.

الخاتمة :يمكن لخدمات الإسعاف والطوارئ الطبية التأثير في فرص النجاة لدى المصابين بالإصابات الرضّية من خلال ضمان الوصول السريع إلى الرعاية، واتخاذ قرارات مناسبة بشأن الوجهة العلاجية، وتطبيق التدخلات الطبية الملائمة لكل حالة. ومع ذلك، لا تدعم الأدلة المتاحة وجود فائدة موحدة في تحسين معدلات البقاء على قيد الحياة لجميع التدخلات الطبية المتقدمة أو وسائل النقل الإسعافي. ومن ثم، ينبغي أن تركز أنظمة رعاية الإصابات على سرعة التقييم الأولي، وتطبيق بروتوكولات الفرز الطبي القائمة على الأدلة العلمية، وكفاءة النقل إلى المرافق العلاجية المناسبة، وتوجيه تدخلات الإنعاش وفقاً للحالة السريرية، إلى جانب المتابعة الدقيقة والمنهجية للنتائج العلاجية.

الكلمات المفتاحية

خدمات الإسعاف والطوارئ الطبية؛ الإصابات الرضّية؛ الرعاية قبل المستشفى؛ الوفيات؛ البقاء على قيد الحياة؛ خدمات الإسعاف الجوي بالمروحيات؛ النقل الإسعافي؛ زمن الاستجابة؛ الصدمة النزفية؛ إصابات الدماغ الرضّية؛ بيانPRISMA 2020 .

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Introduction

Emergency Medical Services (EMS) constitute the first organized clinical response for many individuals experiencing severe injury. At the scene, EMS professionals must identify immediate threats to life, maintain oxygenation and perfusion, recognize bleeding and brain injury, and select the most appropriate receiving hospital. These decisions frequently occur under conditions of incomplete information, environmental danger and limited time. The prehospital phase therefore functions as a critical link between injury and definitive trauma care.

Whether a particular EMS strategy reduces mortality is not simply a question of providing more care. The time required for advanced procedures can delay hemorrhage control or neurosurgical treatment, while rapid transport without necessary stabilization can expose patients to preventable deterioration. The relative value of treatment and transport depends on injury mechanism, physiology, local geography, personnel capability and the accessibility of trauma centers. In a nationwide Japanese analysis, Tsuchiya et al. (2016) found an adjusted survival advantage associated with helicopter EMS, while other transport comparisons have produced different findings. The literature illustrates the importance of considering the injury pattern. Patients with isolated penetrating injuries may have a compelling need for immediate operative treatment. A large American cohort found lower adjusted mortality among patients transported by private vehicles rather than ground EMS, but the authors cautioned that nonrandom assignment and differences in initial injury severity complicated interpretation (Wandling et al., 2018). A separate study identified a positive association between longer response and on-scene intervals and mortality in penetrating trauma (Nasser et al., 2020). These observations should inform locally tested protocols rather than lead to unqualified recommendations for private transport.

For patients with hemorrhagic shock, the question often concerns whether administering blood products before hospital arrival improves survival. The multicenter PAMPer trial reported lower 30-day mortality after prehospital plasma during air medical transport (Sperry et al., 2018). In contrast,

an urban ground ambulance trial with shorter transport intervals found no significant reduction in mortality with plasma-first resuscitation (Moore et al., 2018). The RePHILL trial similarly did not establish superiority of packed red cells plus lyophilized plasma over saline for its composite primary outcome (Crombie et al., 2022).

Traumatic brain injury introduces additional considerations, including airway support, ventilation, secondary brain injury and functional recovery. In Japan, helicopter transport was associated with lower adjusted in-hospital mortality among patients with traumatic brain injury (Hosomi et al., 2022). However, a major randomized trial of out-of-hospital tranexamic acid found no statistically significant improvement in favorable neurological outcomes at six months (Rowell et al., 2020). Patient-centered outcome measures are therefore essential in addition to mortality alone.

Although the global trauma literature is extensive, EMS effectiveness is evaluated using different exposures, study designs, populations and outcomes. A coherent synthesis is needed to distinguish practices consistently linked with favorable outcomes from those whose benefits depend on local circumstances or remain uncertain. The present review examines these issues through a structured assessment of primary prehospital trauma studies.

Problem Statement

Preventable trauma mortality remains a central concern for emergency care systems. Despite major investments in ambulance fleets, skilled responders, air medical transport and advanced prehospital treatments, uncertainty persists regarding which services confer measurable survival gains. Observational reports may be influenced by injury severity and dispatch selection, while randomized trials sometimes yield results that vary by transport duration and patient phenotype. As a consequence, service expansion or protocol intensification does not necessarily translate into reduced mortality across all trauma categories. An integrated appraisal is needed to understand where EMS is most likely to improve outcomes, where evidence is conflicting and which quality indicators should guide system improvement.

Aim of the Review

This review aims to examine the impact of prehospital emergency medical services on trauma outcomes and mortality, with particular emphasis on transport strategies, response and scene intervals, resuscitation interventions, traumatic brain injury and triage decisions.

Objectives

1. To identify patterns linking EMS transport modality and trauma survival.
2. To evaluate the association of response time, scene time and transport efficiency with mortality in patients with traumatic injuries.
3. To summarize trial evidence concerning prehospital plasma, blood products and tranexamic acid in trauma-related shock or brain injury.
4. To assess trauma triage and destination decisions as determinants of patient outcomes.
5. To describe methodological limitations and develop practical recommendations for EMS organizations and trauma systems.

Research Questions

1. How do helicopter, ground ambulance and other transport pathways relate to mortality among trauma patients?
2. What is the relationship between prehospital time intervals and survival, particularly in penetrating trauma?
3. Which prehospital resuscitation strategies demonstrate measurable benefit for mortality or functional outcomes?
4. How do injury severity and EMS triage influence the apparent effects of prehospital care?
5. Which evidence-informed operational priorities are appropriate for ambulance services and trauma networks?

Significance of the Review

The findings are relevant to EMS clinicians, medical directors, trauma-center leaders and regional health authorities who must allocate resources across ground ambulances, air-medical programs, training, dispatch and hospital networks. For services such as the Saudi Red Crescent Authority, an evidence-oriented understanding of when rapid transport, advanced interventions or specialized response makes the greatest difference can support effective protocol design and ongoing quality improvement. The review also identifies research priorities that can be evaluated using national EMS and trauma registry data.

Methodology

Study Design

This paper presents a PRISMA 2020-aligned, structured narrative synthesis of 14 traceable primary publications. The original numerical study-selection flow is retained as an unverified historical record, while the 14-article evidence map is independently bibliographically checked. Because the original search exports, screening decisions and excluded-report log were not supplied, the evidence map must not be construed as a prospectively selected or fully reproducible systematic-review sample. No meta-analysis was undertaken because of the clinical and methodological heterogeneity of the evidence.

Review Question Framework

The analytical framework comprises patients with traumatic injury (population); prehospital response intervals, transport pathways, field resuscitation and triage (exposures or interventions); alternative EMS pathways or control resuscitation approaches (comparators); and mortality, survival, neurological recovery and trauma-related patient outcomes (endpoints). This framework organizes the published evidence presented in Table 1; the original materials do not establish that it was a registered prospective protocol.

Protocol Registration

A registration identifier, dated protocol or prospective amendments were not included in the original Group K source file, the reference manuscript or the supplied PRISMA flowchart. Protocol registration is therefore not claimed.

Eligibility Criteria

Inclusion Criteria

1. Population: patients with traumatic injury managed or transported in the prehospital setting.
2. Exposure or intervention: EMS response intervals, transport modality, ambulance-based treatment, air-medical care, prehospital resuscitation or EMS triage.
3. Outcome: mortality, survival to discharge, 30-day survival, neurological or functional recovery, or a directly related trauma outcome.
4. Study design: peer-reviewed original randomized clinical trials, prospective cohorts and retrospective comparative observational studies.
5. Publication window for the mapped evidence set: 2016 through 2025; English-language research with identifiable bibliographic information.

Exclusion Criteria

1. Articles limited to non-traumatic illness or out-of-hospital cardiac arrest without a trauma cohort.
2. Editorials, opinion pieces, conference abstracts and non-original narrative reviews in the principal evidence table.
3. Publications lacking a relevant prehospital exposure or a patient-outcome endpoint.
4. Duplicate publications of the same analysis when a more informative report was selected.

Information Sources and Search Strategy

The historical selection counts were transcribed from the project-supplied PRISMA figure (235 database records; no register records). Database names, interfaces, dates, search strings, exports, the identity of any automation tool and de-duplication logs were absent. A distinct bibliographic

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verification exercise checked the authors, titles, years, study designs and PubMed identifiers of all 14 papers in Table 1 against PubMed and, where accessible, journal or full-text reports. These checks verify that the publications exist; they do not recreate the missing 235-record search or establish that the identified papers were present in that original export. An auditable article-level verification ledger is supplied in Appendix D.

For transparency, the conceptual search framework used to organize the mapped literature was: (trauma OR injury OR penetrating injury OR traumatic brain injury OR hemorrhagic shock) AND (prehospital OR emergency medical services OR ambulance OR helicopter emergency medical services) AND (mortality OR survival OR outcome OR neurologic recovery). Topic-specific concepts included response time, scene time, plasma, transfusion and tranexamic acid. This is a reproducible example for a future updated search, NOT a historical search strategy or a basis for back-calculating database-level yields (Appendix A).

Study Selection and Screening

Article-Level Reconciliation Procedure

The study ledger was reconstructed by matching first author, year, title, journal and DOI to PubMed records, then recording the PMID for each publication. Open-access full texts and journal pages were used when available to corroborate methods and outcome-specific appraisal statements. A publication was marked "bibliographically verified" only when it matched a unique source record. This procedure provides article-level provenance but does not establish presence in the historical 235-record search, because that file was not supplied. A crosswalk linking the source export identifier to each of the 14 PMIDs is therefore unresolved rather than assumed.

According to the supplied historical PRISMA counts, 70 duplicates, 30 records described only as "automation-ineligible", and 15 other records were removed before screening (235 - 115 = 120). Forty records were excluded at title/abstract screening; 80 reports were sought, 18 were not retrieved, and 62 were assessed. Forty-eight full-text reports were excluded in three historical count blocks of 20, 13 and 15, producing 14 included studies. The source does not identify the substantive reasons represented by those three blocks. They are therefore relabelled "unclassified historical block A/B/C" in Figure 1, not inaccurately described as specific eligibility reasons. No study-by-study decision or reason can be independently verified from the source material.

The source documents contain neither record-level screening decisions nor the number and identities of screeners, independent duplicate screening procedures, disagreement-resolution decisions or full-text exclusion reasons. Thus, none of these procedures is retrospectively asserted to have occurred. The 14 studies in Table 1 are a separately verified article map; matching these citations to the original screening set would require a DOI/PMID-level export and eligibility log. An audit and reconciliation protocol is provided in Appendix F.

Data Extraction

For each of the 14 identifiable publications summarized here, the evidence table records authorship, year, country, design, analyzed population, EMS exposure and reported outcome. Exact effect measures and confidence intervals were retained when supported by the published abstract or article. Mortality definitions and follow-up intervals were not assumed to be interchangeable; for example, 24-hour mortality, in-hospital mortality, 30-day mortality and functional recovery describe different clinical endpoints.

For comparability, effect measures were presented together with their study design, comparator and time horizon when available. This evidence-mapping extraction should not be interpreted as the historical extraction sheet used for the 235-record search, which was not present in the supplied source files.

Quality Appraisal and Risk of Bias

Study-level methodological appraisal was performed using the 11-question JBI Cohort Checklist for the nine observational studies and the five-domain Cochrane RoB 2 framework for the five randomized trials, with the cluster-specific considerations applied to PAMPer. Evaluations were

made for the outcomes emphasized in this review (mortality for the transport cohorts and four resuscitation trials; six-month neurological function for Rowell et al.). An item was coded Unclear where the accessible report or accompanying materials did not provide evidence sufficient for a defensible judgement. These are explicitly single-pass, publication-based assessments, not retrospectively claimed blinded or independently duplicated evaluations. Appendix E records the item/domain judgments, source availability, and supporting study-level rationales. The assessment does not infer a numerical quality percentage from JBI items or equate missing reporting with demonstrated bias.

Data Synthesis

A thematic narrative approach was used. The evidence was grouped into five themes: (1) transport modality and access to definitive care, (2) EMS time intervals, (3) blood-product resuscitation, (4) traumatic brain injury and neurologic outcomes, and (5) triage, generalizability and health-system performance. Reports of associations were distinguished from evidence of causal efficacy, and non-significant trial results were retained rather than excluded from interpretation.

Results

Study Selection

Figure 1 reconstructs the original numerical PRISMA pathway without changing its verified arithmetic: 235 - (70 + 30 + 15) = 120 screened; 120 - 40 = 80 reports sought; 80 - 18 = 62 full-text reports assessed; 62 - (20 + 13 + 15) = 14 included studies. The three exclusion-block labels are not substantive exclusion reasons. The historical diagram reported "reports of included studies = 0", which is incompatible with the 14 articles documented in Table 1. The revised display shows 14 mapped publications (one per listed study), explicitly labelled as a bibliographic reconciliation rather than a historically audited report count. The original value of zero remains documented in Appendix B as a source discrepancy.

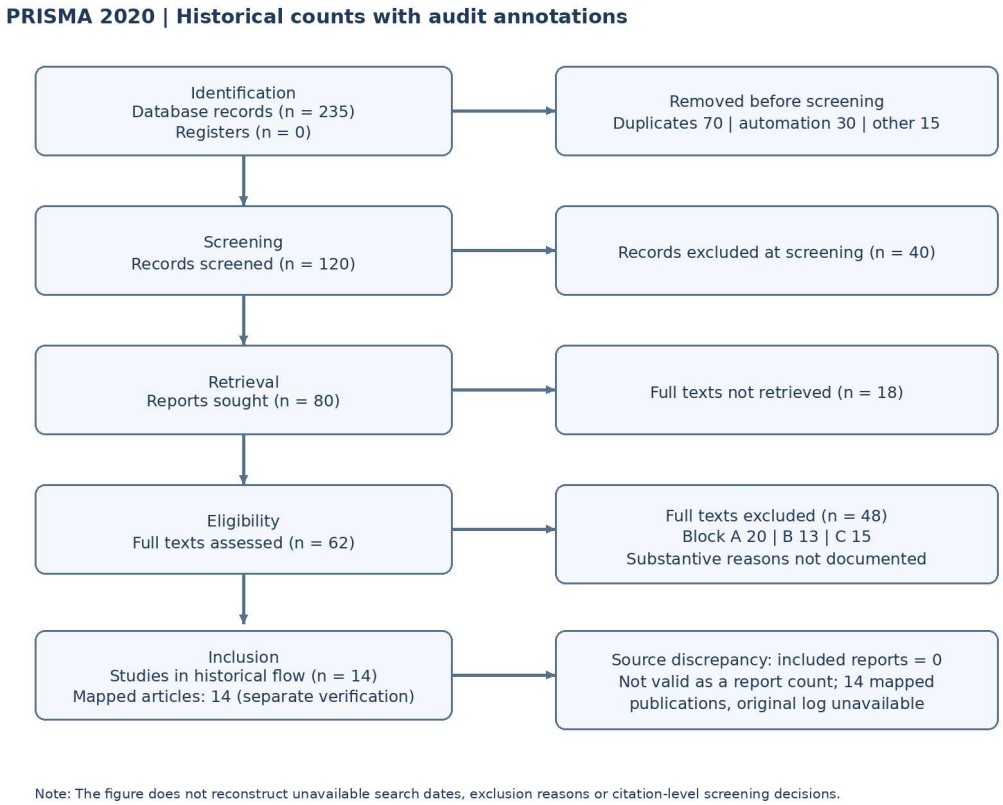


Figure 1. PRISMA 2020: numerical reconstruction with explicitly unresolved historical fields.

Characteristics of Included Evidence

The 14 mapped primary studies span publications from 2016 to 2025 and cover the United States, Japan, Sweden, the United Kingdom, France and three Latin American countries. They include randomized intervention trials and observational comparisons based on trauma registries or regional cohorts. The principal prehospital exposures were helicopter versus ground transport, EMS versus private transport, response and scene intervals, plasma or blood-product resuscitation, tranexamic acid and triage accuracy. Comparability is limited because the study populations range from selected patients with hemorrhagic shock or moderate-to-severe brain injury to much broader trauma registries.

Table 1. Characteristics and principal reported outcomes of 14 mapped primary studies.

Author (year)	Country	Study design	Sample	Main focus	Key reported finding
Tsuchiya et al. (2016)	Japan	Retrospective national cohort	21,286	HEMS vs ground EMS in major trauma	Mortality 22.2% vs 24.5% after matching; adjusted risk difference −2.3 percentage points.
Zhu et al. (2018)	USA (rural)	Propensity-matched cohort	1,049 identified	Rural helicopter vs ground transport	Adjusted survival OR 2.69 (95% CI 1.21–5.97) favoring helicopter transport.
Wandling et al. (2018)	USA (urban)	Retrospective multicenter cohort	103,029	Ground EMS vs private transport, penetrating injury	Private vehicle transport associated with lower adjusted mortality: OR 0.38 (95% CI 0.31–0.47).
Sperry et al. (2018)	USA	Cluster-randomized trial (PAMPer)	501	Prehospital thawed plasma during air transport	30-day mortality 23.2% with plasma vs 33.0% with standard care (P = .03).
Moore et al. (2018)	USA	Randomized ground-ambulance trial (COMBAT)	144 randomized	Plasma-first vs saline in short urban transport	28-day mortality 15% vs 10% (P = .37); no significant survival benefit.
Nasser et al. (2020)	USA	Retrospective TQIP analysis	43,467	Response time and scene time in penetrating trauma	Per-minute mortality associations: response OR 1.02; scene OR 1.01 after adjustment.
Rowell et al. (2020)	USA/Canada	Multicenter randomized trial	966 analyzed	Out-of-hospital TXA for moderate/severe TBI	No significant 6-month favorable neurological outcome difference (65% vs 62%; P = .16).
Nabeta et al. (2021)	Japan	Single-center propensity cohort	317 eligible	Physician-staffed HEMS vs ground transport	No significant matched mortality difference: 8.7% vs 5.8% (OR 1.55; 95% CI 0.53–4.51).

Author (year)	Country	Study design	Sample	Main focus	Key reported finding
Hosomi et al. (2022)	Japan	National retrospective cohort	58,532	HEMS vs ground ambulance for traumatic brain injury	Adjusted in-hospital mortality OR 0.83 (95% CI 0.74–0.92), favoring HEMS.
Crombie et al. (2022)	UK	Multicenter randomized trial (RePHILL)	432 randomized	Prehospital RBC + plasma vs saline	Composite adverse outcome 64% vs 65%; no significant benefit (P = .996).
Jost et al. (2022)	France	Randomized multicenter trial	150 randomized; 134 analyzed	Lyophilized plasma vs saline	No significant INR or 30-day survival difference; death HR 1.07 (95% CI 0.44–2.61).
Atkins et al. (2023)	USA	Retrospective national database	48,444	Transport mode in isolated penetrating torso trauma	Ambulance vs private transport mortality OR 1.81 (stabbing), 1.66 (firearm).
Lapidus et al. (2023)	Sweden	Retrospective national registry	74,032	HEMS vs ground transport, 30-day survival	Lower mortality in HEMS groups across ISS strata, but worse neurologic discharge outcomes.
Nogueira et al. (2025)	Argentina, Brazil, Colombia	Prospective multicenter cohort	1,193	Prehospital care level and triage accuracy	Neither level of prehospital care nor triage accuracy associated significantly with mortality.

Methodological Considerations Across Studies

The nine observational studies depend on nonrandom treatment allocation, EMS dispatch choices, location, severity and baseline physiologic differences. Although several authors used propensity methods or multivariable adjustment, residual confounding and uneven capture of prehospital factors remain important. The five randomized intervention studies offer stronger intervention comparisons but differ in masking, analysis sets, recruitment, outcome completeness and use of composite or functional outcomes. The study-level checklist results below concern the methodological credibility of the reported estimate, not the direction or statistical significance of its effect.

Table 2. Study-specific methodological concerns accompanying the structured appraisals.

Study	Study category	Principal methodological consideration
Tsuchiya et al. (2016)	Observational	Residual confounding and EMS dispatch selection.
Zhu et al. (2018)	Observational	Single trauma center; historic transport cohorts.
Wandling et al. (2018)	Observational	Strong selection differences by injury severity and transport mode.
Sperry et al. (2018)	Randomized trial	Air-medical setting and cluster allocation constrain transferability.
Moore et al. (2018)	Randomized trial	Small sample; trial stopped early for futility; rapid transport.
Nasser et al. (2020)	Observational	Time-dependent confounding and retrospective timing data.
Rowell et al. (2020)	Randomized trial	Outcome follow-up incomplete; TBI-specific eligibility.
Nabeta et al. (2021)	Observational	Small matched sample and wide confidence intervals.
Hosomi et al. (2022)	Observational	Registry confounding and clinical selection by transport mode.
Crombie et al. (2022)	Randomized trial	Stopped short of planned sample; open-label design.
Jost et al. (2022)	Randomized trial	Small number of events; primary endpoint was coagulation, not mortality.
Atkins et al. (2023)	Observational	Selection bias; unmeasured prehospital clinical factors.
Lapidus et al. (2023)	Observational	Possible indication confounding; longer transport and outcome differences.
Nogueira et al. (2025)	Observational	Short recruitment windows; heterogeneity between hospitals.

Table 3. Risk-of-bias appraisal framework applied to the included publications.

Study types	Number	Tool applied to publications	Scope / source qualification
Observational cohorts and comparative registry analyses	9	JBI Cohort Checklist (11 items)	Publication-supported Y/N/U item judgments in Appendix E; residual confounding prioritized.
Individually randomized clinical trials	4	Cochrane RoB 2 (five domains)	Outcome-specific judgments for mortality, coagulation or functional recovery; source citations in Appendix E.
Cluster-randomized trial (PAMPer)	1	RoB 2 with cluster randomization considerations	Explicit consideration of post-allocation recruitment, unmasked care, modified ITT and outcome follow-up.

Appraisal interpretation: the audit tables in Appendix E supplement the study cautions in Table 2 with explicit JBI items and RoB 2 domains. "Unclear" reflects unavailable or insufficiently reported information and must not be read as a "No". The collective evidence is less certain for helicopter-versus-ground transport and private-car comparisons, principally because of nonrandom assignment and residual confounding. Experimental evidence should be interpreted trial by trial, with particular attention to outcome-specific attrition, unmasked care and changes between randomization and analysis.

Risk of Bias: Structured Appraisal Findings

Across nine nonrandomized cohorts, treatment selection, geography and baseline injury severity frequently threatened exchangeability despite statistical adjustment. Item-level JBI observations and reporting limitations appear in Appendix E. Across the five randomized trials, the principal RoB 2 concerns were absence of treatment masking in field interventions, exclusions after randomization, and missing follow-up for a functional outcome. A domain concern is not equivalent to evidence that the direction of the published result is wrong. These limitations are considered when interpreting both statistically significant and null findings.

Theme 1: Transport Modality and Trauma Survival

1.1 Helicopter Emergency Medical Services

Several observational studies associated helicopter transport with improved survival among patients with major trauma, although the magnitude and certainty of benefit varied. In the Japanese national trauma database, Tsuchiya et al. (2016) reported matched in-hospital mortality of 22.2% for helicopter transport and 24.5% for ground transport, a difference that remained favorable across several adjustment methods. In a rural American cohort, Zhu et al. (2018) identified higher adjusted odds of survival after helicopter transport (OR 2.69, 95% CI 1.21–5.97), emphasizing that transport distance and resource geography are likely to influence effectiveness.

Other comparisons demonstrate why this association should not be treated as universal. Nabeta et al. (2021) reported no statistically significant difference in mortality between physician-staffed helicopter and ground EMS in a smaller Japanese cohort of 317 eligible patients. In Sweden, Lapidus et al. (2023) found lower mortality among helicopter-transported trauma patients within specified injury-severity strata, but also longer prehospital times and less favorable neurological outcomes at discharge. Taken together, these findings suggest that helicopter transport may contribute important clinical capabilities beyond transport speed, while selection and case-mix remain central interpretive challenges.

1.2 Ground EMS and Private Transportation

In penetrating trauma, comparisons of ambulance and private vehicle transport have yielded provocative findings. Wandling et al. (2018) analyzed 103,029 patients with gunshot or stab wounds and found substantially lower adjusted odds of in-hospital mortality with private transport than with ground EMS (OR 0.38, 95% CI 0.31–0.47). Atkins et al. (2023) reported higher adjusted mortality for ambulance transport compared with private vehicle transport among 48,444 patients with isolated penetrating torso injury. However, these nonrandom comparisons are vulnerable to major differences in injury patterns, transport availability, prehospital physiology and access to care. Their value lies in prompting careful assessment of avoidable delay and not in supporting wholesale replacement of ambulance services.

Theme 2: Response Time, Scene Time and Access to Definitive Care

Time-to-treatment was most directly studied for patients with penetrating injury. Nasser et al. (2020) analyzed 43,467 individuals and found that each additional minute of response time was associated with approximately 2% higher odds of mortality (OR 1.02), while each additional minute on scene was associated with approximately 1% higher odds (OR 1.01), after statistical adjustment. These are observational associations rather than proof that shortening every prehospital interval by one minute

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will cause an equivalent risk reduction. The findings nevertheless highlight the potential importance of prompt dispatch, efficient scene management and direct access to hemorrhage control.

The best time strategy is unlikely to be uniform across injuries. Hemorrhage requiring surgery, severe airway compromise and isolated traumatic brain injury demand different decisions. Operational measures such as ambulance turnout time, dispatch-to-scene arrival, on-scene interval, total prehospital time and destination accuracy should therefore be examined separately. Interventions that delay departure should be evaluated according to whether their expected benefit justifies the additional time and whether hospital notification can reduce the next care interval.

Theme 3: Prehospital Resuscitation for Hemorrhagic Shock

3.1 Prehospital Plasma and Blood Products

The strongest favorable randomized finding in the evidence set comes from the PAMPer trial. Sperry et al. (2018) randomized trauma patients at risk for hemorrhagic shock to prehospital plasma or usual resuscitation during air medical transport and found 30-day mortality of 23.2% and 33.0%, respectively (P = .03). This supports a clinically meaningful benefit in a particular air-medical system, but it does not establish that identical protocols will benefit all EMS settings.

In contrast, the COMBAT randomized trial studied rapid urban ground transportation and observed 28-day mortality of 15% with plasma and 10% with saline, a difference that was not statistically significant (P = .37) (Moore et al., 2018). The trial was relatively small and stopped early for futility. The RePHILL multicenter trial randomized 432 patients with traumatic hemorrhagic shock to packed red cells plus lyophilized plasma or saline; the composite of episode mortality and/or impaired lactate clearance did not differ significantly between groups (Crombie et al., 2022). Jost et al. (2022) likewise found no statistically significant improvement in their primary coagulation endpoint or 30-day survival with lyophilized plasma.

These studies should not be reduced to a simple positive-versus-negative tally. They varied in transport time, geography, blood-product formulation, injury severity, physiologic inclusion criteria and primary outcome. Heterogeneity is clinically plausible: early blood-product therapy may be more valuable during longer transport when hemorrhage control is delayed, while rapid access to a trauma center may reduce the incremental effect of prehospital transfusion. The available findings favor tailored protocols with close audit rather than indiscriminate treatment escalation.

3.2 Timing and Prioritization of Interventions

EMS clinicians must balance immediate physiologic stabilization against definitive treatment. Evidence from the included trials indicates that the value of intervention depends on patient selection, the operational burden of preparation and transport characteristics. High-value field care includes early recognition of hemorrhagic shock, hemorrhage control, avoidance of hypoxia and hypothermia, and rapid coordination with a trauma center. The precise contribution of any single procedure to mortality cannot reliably be inferred from broad observational estimates.

Theme 4: Traumatic Brain Injury and Functional Outcomes

Traumatic brain injury is a major source of death and long-term disability. Hosomi et al. (2022) studied 58,532 patients in a Japanese trauma registry and reported lower adjusted odds of in-hospital mortality among helicopter-transported patients (OR 0.83, 95% CI 0.74–0.92). This association supports further evaluation of specialized crews, triage, early intervention and trauma-center access, but remains susceptible to confounding by case selection and transport decisions.

The randomized evidence regarding out-of-hospital tranexamic acid is more specific. In a multicenter trial of moderate-to-severe TBI, Rowell et al. (2020) found favorable neurological outcomes at six months in 65% of patients receiving tranexamic acid and 62% receiving placebo; the difference was not statistically significant (P = .16). The study also reported no statistically significant overall difference in 28-day mortality. Thus, claims that routine prehospital tranexamic acid improves neurological recovery in the trial population would exceed the reported evidence.

Furthermore, mortality reduction alone may be insufficient to define success. Lapidus et al. (2023) described lower mortality in several helicopter-transported injury-severity groups but worse reported neurological outcome among those surviving to discharge. Trauma outcome evaluations should include survival, disability, neurologic recovery and quality-of-life endpoints whenever feasible, recognizing that different measures are not interchangeable.

Theme 5: Triage, EMS Care Level and System Performance

Correctly identifying patients who need a trauma center is a central EMS responsibility. In a prospective study spanning 14 hospitals in Argentina, Brazil and Colombia, Nogueira et al. (2025) analyzed 1,193 patients and found no statistically significant association between care level or triage classification and hospital mortality. Intermediate or advanced prehospital care was associated with longer length of stay, a finding that may reflect differences in severity or referral processes rather than a harmful effect of care itself. The results emphasize the necessity of system-specific evaluation and case-mix adjustment.

A high-performing trauma system depends on coordinated dispatch, standardized field triage, available ambulance resources, access to trauma centers, continuous clinical education and timely handover. Registry-based measures should include response time, duration on scene, undertriage, overtriage, intervention rates, clinical outcomes and the proportion of eligible patients delivered directly to definitive trauma care. Without linked prehospital and hospital data, apparent gains in operational speed cannot confidently be attributed to improved patient survival.

Discussion

Interpretation of the Main Findings

This synthesis indicates that EMS influence on trauma survival is real but conditional. Strongest evidence for benefit is seen in carefully defined settings, such as prehospital plasma during air medical transport in patients at risk for hemorrhagic shock. Large cohorts also identify favorable associations for helicopter transport and for shorter response or scene intervals in selected populations. At the same time, high-quality randomized trials failed to demonstrate benefits for several alternative plasma regimens or for out-of-hospital tranexamic acid in a defined TBI population. A balanced conclusion must incorporate both positive and null findings.

Transport comparisons reflect a combination of access, crew expertise and care-system structure. Helicopter programs can extend specialist capability to remote areas or facilitate access to definitive care, but they are also subject to dispatch selection, regional availability and weather restrictions. Some EMS ground transport comparisons in penetrating trauma appear less favorable than private transport; however, private transport cannot be assumed safe or superior for all patients because the groups differ clinically in ways that observational adjustment may not capture. An EMS policy response should focus on reducing avoidable time and improving patient selection rather than adopting a single transport strategy for every situation.

Mortality Reduction Versus Functional Recovery

Several studies selected hospital mortality or 30-day survival as their primary endpoint; others considered coagulation measures, lactate clearance or neurologic recovery. These are not direct substitutes. An intervention might increase short-term survival without restoring meaningful function, or improve physiology without a demonstrable mortality effect. Both the Rowell et al. (2020) and Lapidus et al. (2023) findings illustrate why trauma-service performance should be interpreted through a multidimensional outcome framework.

Implications for Trauma Care Systems

Operational priorities should be guided by injury-specific pathways. For penetrating hemorrhage, systems should evaluate dispatch delays, necessary scene interventions, immediate transport to a capable center and pre-arrival activation. For remote major trauma, the relative benefit of helicopter transport should be judged against distance, resources and patient severity. For suspected hemorrhagic shock, blood-product availability should be integrated with clinical indication and

projected transport time. For traumatic brain injury, prevention of secondary injury and reliable documentation of long-term outcomes should complement mortality reporting.

Evidence Consistency and Causal Interpretation

The observational evidence base includes multiple large databases with statistical adjustment, but residual confounding remains unavoidable in many EMS comparisons. In particular, sicker patients may be preferentially assigned advanced teams, longer scene care or helicopter transport. Conversely, patients taken by private car may differ from ambulance patients in injury severity, injury location and access. Randomization reduces these biases but is difficult for transport allocation in emergency conditions, and intervention trials may have limited power in clinically important subgroups. Therefore, the findings should be framed as associations except where randomized comparisons support causal interpretation within the trial population.

Comparison Across Settings

Geographic and organizational factors help explain different study conclusions. A longer rural or air-medical journey creates a different opportunity for advanced treatment than a short urban ambulance trip. Access to a comprehensive trauma center, availability of physicians and critical-care paramedics, destination policies and trauma network maturity may alter the apparent effect of a given intervention. Evidence should be adapted using local audit data rather than transferred unchanged between healthcare systems.

Summary of Findings

The 14 mapped studies show five recurring messages. First, helicopter EMS is frequently associated with improved survival in large observational cohorts but the benefit is not universal. Second, prolonged response and on-scene intervals in penetrating trauma correlate with higher mortality, although causality and optimal timing differ by patient. Third, prehospital plasma showed benefit in one major air-medical randomized trial but not in several other randomized resuscitation comparisons. Fourth, functional recovery in traumatic brain injury requires assessment independently of death rates. Fifth, triage accuracy, skill mix, transport geography and destination capabilities should be treated as integral components of EMS effectiveness.

Conclusion

Emergency Medical Services influence trauma outcomes through rapid recognition, stabilization, triage, transport and coordination with definitive trauma care. The reviewed evidence indicates that survival advantages are most plausible when an intervention solves a specific time-critical clinical or system problem. Helicopter transport and early plasma therapy may be beneficial in selected settings; however, observational transport comparisons are susceptible to confounding, and several randomized prehospital interventions have not demonstrated improved mortality or functional outcome. Trauma systems should pursue targeted, measurable improvements rather than assume that greater intervention intensity automatically lowers death rates.

Recommendations

1. Operational and Dispatch Improvements

Monitor dispatch, crew activation, response, scene and transport intervals separately; review outliers by injury mechanism and acuity.

Use evidence-informed, injury-specific scene-time and destination protocols rather than fixed rules for every trauma presentation.

Strengthen early trauma-center notification and coordinated activation to reduce delays after hospital arrival.

2. Clinical Practice and Training

Prioritize early recognition of hemorrhagic shock, airway threats and traumatic brain injury with appropriate stabilization and rapid escalation.

Apply blood-product protocols only with clear indications, appropriate storage, clinical governance and evaluation by transport setting.

Build continuing education around rapid assessment, hemorrhage control, communication, destination selection and safe handover.

3. EMS System and Leadership

Evaluate helicopter deployment using severity, geography, receiving-center capability and outcome data, not transport time alone.

Integrate ambulance electronic records with hospital trauma registries to permit risk-adjusted mortality and functional-outcome analysis.

Investigate undertriage and overtriage, and audit cases transported to facilities unable to deliver definitive trauma treatment.

4. Future Research

Conduct multicenter studies that separate penetrating trauma, blunt trauma, traumatic brain injury and hemorrhagic shock.

Compare transport and resuscitation strategies by predicted transport duration and availability of definitive trauma care.

Use prespecified outcomes including 24-hour, in-hospital and 30-day mortality alongside neurologic and functional recovery.

Report protocol registration, exact searches, screening decisions and full study-level risk-of-bias assessments to improve reproducibility.

Implications for Practice and Policy

For prehospital practice, the most defensible approach is timely, structured assessment and efficient transfer to appropriate definitive treatment, combined with interventions that have a favorable benefit-to-delay profile. For policymakers, investments in EMS personnel, air transport or advanced resuscitation should be accompanied by transparent indication criteria and linked outcome evaluation. For quality leaders, case review should examine whether every stage of the response contributed to the correct treatment at the correct time, rather than focusing exclusively on overall ambulance response targets.

Strengths and Limitations of the Review

Strengths

Preserves the original numerical PRISMA selection pathway supplied with the project without inventing alternate counts.

Uses 14 identifiable primary publications with traceable PubMed records and DOI links rather than placeholder studies.

Integrates both randomized and observational evidence and presents positive, negative and statistically inconclusive findings.

Examines survival, neurological recovery and EMS operational processes rather than treating all trauma endpoints as interchangeable.

Limitations

The principal limitation is the lack of a reproducible historical search: the 235-record export, database-specific search dates and strategies, DOI-linked screening ledger, automated exclusion criteria and original full-text reasons were not available. The 14 published studies are individually verifiable but cannot be certified as the same 14 historical inclusions. The original flowchart also reported zero included-study reports; the corrected schematic uses the 14 identifiable publications as a bibliographic reconciliation rather than claiming an independently audited historical count. The 48 excluded full texts remain without verifiable reasons, so PRISMA 2020 item 16b is only partially satisfied. This cannot be repaired reliably by inventing a distribution of reasons.

Substantial heterogeneity prevented defensible quantitative pooling. Nine nonrandom transport or service-level cohorts are vulnerable to residual confounding; the randomized comparisons involved different populations and outcomes. Publication-based JBI and RoB 2 assessments now specify item-level concerns, but a fully validated dual-reviewer appraisal and reconciliation to any registered

review protocol remain undocumented. The certainty of the synthesized evidence should not be represented as formally GRADE-rated without an additional outcome-specific certainty assessment.

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Appendix A. Reproducible Search Framework

The following is an operationally reproducible search template for an updated search, not a claim about which query produced the original 235 records. Records should be exported with database name and search date, deduplicated with a documented tool, and screened against prespecified eligibility rules.

Core search: ("Emergency Medical Services" OR "prehospital" OR "ambulance" OR "helicopter emergency medical services" OR "EMS") AND ("trauma" OR "injury" OR "hemorrhagic shock" OR "traumatic brain injury" OR "penetrating trauma") AND ("mortality" OR "survival" OR "functional outcome" OR "clinical outcome").

Suggested data sources: PubMed/MEDLINE; Scopus; Web of Science; CINAHL; Embase. The full search should record the interface, exact query string, date searched, any language or time filters, returned record count and retained export for each database.

Screening and extraction audit fields: database record ID; title; DOI; duplicate status; title/abstract decision; reviewer identifiers; full-text availability; final exclusion rationale; study characteristics; outcomes; risk-of-bias judgments; and resolution of reviewer disagreements.

Appendix B. PRISMA Count Audit

Stage	Source count	Arithmetic check
Records from databases / registers	235 / 0	Total = 235
Duplicates removed	70	Pre-screen exclusions
Ineligible via automation	30	Pre-screen exclusions
Other pre-screen removals	15	70 + 30 + 15 = 115
Records screened	120	235 − 115 = 120
Records excluded	40	120 − 40 = 80
Reports sought	80	As supplied
Reports not retrieved	18	80 − 18 = 62
Reports assessed	62	As supplied
Excluded: Reason 1	20	Label unspecified in source
Excluded: Reason 2	13	Label unspecified in source
Excluded: Reason 3	15	20 + 13 + 15 = 48
Studies included	14	62 − 48 = 14
Reports of included studies	0 in source; unresolved	Historical diagram conflict; 14 articles mapped, not an audited report count.

Appendix B documents a transcription of the source counts. The three excluded-report groups of 20, 13 and 15 cannot be relabelled with clinical eligibility reasons without the full-text screening log. The apparent value “0 reports of included studies” is treated as an undocumented inconsistency rather than evidence of absence of publications.

Appendix C. Bibliographic Verification Links

Each entry below links to the indexed article and corresponds to one row in the evidence characteristics table. Links enable verification of authorship, publication year, journal and the abstracted results.

1. Tsuchiya et al. (2016) — https://pubmed.ncbi.nlm.nih.gov/27899124/
2. Zhu et al. (2018) — https://pubmed.ncbi.nlm.nih.gov/28898557/
3. Wandling et al. (2018) — https://pubmed.ncbi.nlm.nih.gov/28975247/
4. Sperry et al. (2018) — https://pubmed.ncbi.nlm.nih.gov/30044935/
5. Moore et al. (2018) — https://pubmed.ncbi.nlm.nih.gov/30032977/
6. Nasser et al. (2020) — https://pubmed.ncbi.nlm.nih.gov/31761299/
7. Rowell et al. (2020) — https://pubmed.ncbi.nlm.nih.gov/32897344/
8. Nabeta et al. (2021) — https://pubmed.ncbi.nlm.nih.gov/33676079/
9. Hosomi et al. (2022) — https://pubmed.ncbi.nlm.nih.gov/34779275/
10. Crombie et al. (2022) — https://pubmed.ncbi.nlm.nih.gov/35271808/
11. Jost et al. (2022) — https://pubmed.ncbi.nlm.nih.gov/35881397/
12. Atkins et al. (2023) — https://pubmed.ncbi.nlm.nih.gov/37453802/
13. Lapidus et al. (2023) — https://pubmed.ncbi.nlm.nih.gov/38104083/
14. Nogueira et al. (2025) — https://pubmed.ncbi.nlm.nih.gov/40779777/

Appendix D. Article Identity and Search-to-Study Reconciliation Ledger

Bibliographic identity was checked for each mapped study using the provided DOI/PMID list and public journal/PubMed records. Verification of identity is different from verification of historical inclusion. Source-export record IDs and original decisions were not available, so the historical inclusion crosswalk remains unverified for all 14 papers.

Study	PMID	DOI	Accessible verification
Tsuchiya 2016	27899124	10.1186/s13049-016-0335-z	PMC full text
Zhu 2018	28898557	10.1111/acem.13307	PMC full text
Wandling 2018	28975247	10.1001/jamasurg.2017.3601	PMC full text
Sperry 2018	30044935	10.1056/NEJMoa1802345	Publisher full text
Moore 2018	30032977	10.1016/S0140-6736(18)31553-8	PMC / publisher
Nasser 2020	31761299	10.1016/j.amjsurg.2019.11.018	Publisher article
Rowell 2020	32897344	10.1001/jama.2020.8958	PMC full text
Nabeta 2021	33676079	10.1016/j.ajem.2021.02.062	Indexed abstract
Hosomi 2022	34779275	10.1089/neu.2021.0181	PMC full text
Crombie 2022	35271808	10.1016/S2352-3026(22)00040-0	PMC full text
Jost 2022	35881397	10.1001/jamanetworkopen.2022.23619	Publisher full text
Atkins 2023	37453802	10.1016/j.amjsurg.2023.06.033	PMC full text
Lapidus 2023	38104083	10.1186/s13049-023-01168-9	PMC full text; 2024 correction
Nogueira 2025	40779777	10.1097/JTN.0000000000000869	Indexed abstract

The entry "Indexed abstract" indicates that a complete full text was not authenticated in the available review materials; judgments that depend on unreported methods remain Unclear. DOI identity alone must not be described as independent screening verification. Lapidus et al. was followed by a 2024 correction of the axis label in Figure 3 (doi:10.1186/s13049-024-01173-6).

Appendix E. Structured Critical Appraisal Records

Assessment framework. Each publication was examined for the outcome emphasized in the narrative synthesis. The JBI 11-item cohort checklist was used for nine observational investigations; the Cochrane RoB 2 domains were used for four individually randomized trials and the cluster-specific extension was considered for PAMPer. Y = supported yes; N = supported no; U = unclear/insufficient publicly available information; NA = not applicable. Unclear judgments are preserved rather than imputed. No percentage quality scores or author-level consensus procedures are claimed.

E1. JBI Cohort Checklist (nine studies)

Items: Q1 comparable groups from same source; Q2 exposure measured similarly across groups; Q3 valid and reliable exposure measurement; Q4 confounders identified; Q5 strategies to address confounders stated; Q6 outcome absent at start; Q7 valid and reliable outcome measurement; Q8 adequate follow-up time; Q9 follow-up completeness and loss explained; Q10 strategies to address incomplete follow-up; Q11 appropriate statistical analysis. Interpret each question using the JBI explanatory manual.

Cohort (author/year)	Q1–Q11 (in order)	Study-specific appraisal rationale
Tsuchiya 2016	U Y Y Y Y Y Y Y U U Y	National registry with propensity/instrumental methods; nonrandom dispatch and completeness of follow-up not fully ascertainable.
Zhu 2018	U Y Y Y Y Y Y Y U U Y	Propensity matching addresses reported confounders, but one-centre referral and unmeasured selection remain.
Wandling 2018	U Y Y Y Y Y Y Y U U Y	National Trauma Data Bank comparisons with risk adjustment; patients choosing private transport differ systematically.
Nasser 2020	U Y U Y Y Y Y Y U U Y	Retrospective timing records and missing prehospital severity may affect measured time and association.
Nabeta 2021	U Y U Y Y Y U Y U U Y	Propensity approach described in indexed record; full methods and ascertainment require complete-text review.
Hosomi 2022	U Y Y Y Y Y Y Y U U Y	Registry-based adjusted comparison; dispatch, destination and residual clinical case-mix confounding.
Atkins 2023	U Y Y Y Y Y Y Y U U Y	NTDB multivariable transport contrast; unmeasured physiologic factors and selection remain.
Lapidus 2023	U Y Y Y Y Y Y Y U U Y	SweTrau registry and ISS stratification; HEMS/EMS baseline differences cannot be eliminated.
Nogueira 2025	U Y U Y Y Y Y Y U U Y	Prospective cohort/adjusted regression visible in indexed abstract; fuller exposure validity and attrition details unknown.

The table is deliberately not converted into a single percentage or a low/medium/high score: a sum of checklist answers may conceal the clinical seriousness of uncontrolled confounding. In this synthesis, comparison-group exchangeability is the dominant interpretive concern. Y on a statistical-analysis item does not demonstrate that adjustment removed unmeasured confounding.

E2. Cochrane RoB 2 Domain-Based Assessment (five randomized trials)

D1 randomization process; D2 deviations from intended interventions; D3 missing outcome data; D4 outcome measurement; D5 selection of reported results. L = low concern based on published evidence, SC = some concerns, U = insufficient evidence. Overall labels are conservative publication-based judgments for the specified outcome; they are not presented as completed independent duplicate assessments, and the relevant protocol/analysis-plan comparison should be checked during a formal review audit.

Study / outcome	D 1	D 2	D 3	D 4	D 5	Overall
Sperry 2018 (PAMPer) 30-day mortality	S C	S C	L	L	S C	Some concerns
Moore 2018 (COMBAT) 28-day mortality	L	S C	S C	L	S C	Some concerns
Rowell 2020 6-month GOSE>4	L	L	S C	L	L	Some concerns
Crombie 2022 (RePHILL) episode mortality / lactate composite	L	S C	S C	L	L	Some concerns
Jost 2022 (PREHO-PLYO) INR at hospital arrival	L	S C	S C	L	S C	Some concerns

Outcome-specific reasoning (in the same study order):
Sperry 2018 (PAMPer): Cluster/month-base allocation, unequal assignment and unmasked fluids; modified intention-to-treat not originally prespecified. 481/501 vital statuses observed; 20 imputed with sensitivity analyses.
Moore 2018 (COMBAT): Concealed coolers until enrollment, then semi-masked care; main as-treated cohort 125 of 144 randomized, with ITT analysis also published.
Rowell 2020: Computer-generated masked kits; neurological outcomes observed for 819/966 in analysis population (84.8%). Missing outcome may affect certainty.
Crombie 2022 (RePHILL): Concealed randomization, open-label postallocation care; primary composite outcome in 409 of 432 randomized; trial ended short of target.
Jost 2022 (PREHO-PLYO): Unblinded administration; 134 of 150 participants in key analysis; change to inclusion threshold/primary measurement context and missing-data analysis require sensitivity scrutiny.
For PAMPer, cluster randomization raises additional recruitment/timing questions beyond the usual individually randomized trial domains. Outcome assessors were masked, but prehospital clinicians were not. For COMBAT, the as-treated primary result must be interpreted alongside the randomized cohort and sensitivity analysis. For Rowell, the primary endpoint is functional recovery at six months, not mortality. For Jost, the primary endpoint is coagulation (INR), while mortality is secondary; these outcomes should not share a single RoB 2 decision without a distinct appraisal.

E3. Appraisal Source Index

JBI. Critical Appraisal Checklist for Cohort Studies (2017; current JBI Manual Appendix 7.1). https://jbi-global.atlassian.net/wiki/spaces/MANUAL/pages/355599324/Appendix+7.1+Critical+appraisal+checklist+for+cohort+studies
Cochrane Handbook for Systematic Reviews of Interventions, Chapter 8: Assessing risk of bias in a randomized trial. https://www.cochrane.org/authors/handbooks-and-manuals/handbook/current/chapter-08
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Jost et al. (2022). Full text: https://jamanetwork.com/journals/jamanetworkopen/fullarticle/2794640
Lapidus et al. (2024). Correction: https://doi.org/10.1186/s13049-024-01173-6

Appendix F. Screening Audit and PRISMA Reconciliation Protocol

This appendix distinguishes completed bibliographic checks from missing source evidence. It is intentionally not a fabricated full-text exclusion ledger.

Audit element	Current evidence	Evidence needed for full PRISMA verification
Historical database export	Not supplied	Obtain original 235-record file with database name, search date, full query, title, DOI/PMID and source ID.
Deduplication (n=70)	Count reported only	Retain match key, software version and cross-database duplicates if available.
Automation-ineligible (n=30)	Criteria/tool unreported	Retrieve exact rule and software log; do not assume machine-learning screening.
Other pre-screen removals (n=15)	Reason unreported	Attach reason to each source record.
Title/abstract exclusions (n=40)	Count only	Preserve decisions, reviewer identifiers and basis.
Full text unavailable (n=18)	Count only	Log citation and retrieval attempts; PRISMA distinguishes nonretrieval from ineligibility.
Excluded full-text reports (20/13/15)	Three historical blocks; labels unknown	Retrieve 48 full-text records and map a single primary eligibility reason to each.
Included 14 studies	14 independently identifiable publications	Join the source export on DOI/PMID, confirm 14 match original inclusion decisions and record associated reports.
Included reports shown as zero in original	Contradiction with 14 known publications	Use 14 as a transparent mapped-report reconciliation only; historical report count requires source-log check.
Reviewer protocol and independence	No evidence supplied	Do not claim preregistration, double screening or disagreements resolved without documentation.

Full-text exclusions: the only defensible retained quantitative information is Block A (20), Block B (13), Block C (15), total 48. A scientifically meaningful distribution into wrong population, wrong exposure, wrong outcome, wrong design, or duplicate analysis is not reconstructible without the 48 decisions. No replacement numerical reason breakdown is reported.

Final reporting interpretation. The article presents a PRISMA-informed narrative evidence map with systematic-review structure. Full compliance with PRISMA 2020 selection and reproducibility items remains conditional on access to the original search and screening records; journal submission requiring a reproducible systematic review should not describe this audit discrepancy as closed.