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Prehospital EMS Response Times and Patient Outcomes: A Narrative Review and Implications for Saudi Red Crescent Operations

أزمنة استجابة خدمات الطوارئ الطبية قبل الوصول إلى المستشفى ونتائج المرضى: مراجعة سردية ودلالات تشغيلية لهيئة الهلال الأحمر السعودي

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Abstract:

Background: Ambulance response time is widely monitored as an emergency medical services (EMS) performance indicator, yet its association with clinical outcomes may differ across out-of-hospital cardiac arrest (OHCA), trauma and other high-acuity emergencies.
Objective: To develop an exploratory, hypothesis-generating narrative framework for Saudi Red Crescent Authority (SRCA) policy discussions. The review contextualizes condition-specific associations between ambulance response intervals and survival, mortality, return of spontaneous circulation (ROSC) and neurological outcomes, and identifies priorities for outcome-linked monitoring and future Saudi research.
Evidence and approach: This focused, non-systematic narrative review draws on 19 identifiable primary reports: 12 retained from an earlier working bibliography and seven identified through targeted bibliographic updating through 25 October 2026. Studies were grouped descriptively by clinical population; no comprehensive database search, reproducible screening log, independent study-level risk-of-bias assessment, meta-analysis or formal certainty grading was taken. A historical claim of 420 search records cannot be substantiated from available records and is not treated as a verified search result.
Findings: The 19 reports comprised 12 OHCA studies, two trauma studies and five mixed or non-OHCA cohorts. Longer response intervals were frequently associated with poorer OHCA outcomes, although magnitude and interpretation varied with bystander CPR, defibrillation, initial rhythm and case selection. A Thai OHCA cohort (n=5,433) reported an adjusted survival-to-discharge odds ratio of 0.94 per additional response minute; EuReCa-THREE documented 32,033 EMS-treated OHCA cases. In contrast, a Danish cohort of 171,708 non-OHCA high-acuity patients did not show higher adjusted 30-day mortality in the longest-response group.
Conclusion: Observed response-time associations differ by diagnosis and care received before EMS arrival; they do not establish a universal causal threshold. The proposed framework offers provisional directions for SRCA outcome measurement, equity monitoring and research prioritization, rather than evidence-graded operational or policy recommendations. A comprehensive systematic review and locally validated analyses are needed before definitive policy decisions.
Keywords: Emergency medical services; ambulance response time; out-of-hospital cardiac arrest; trauma; mortality; survival; Saudi Red Crescent Authority; narrative review.

الملخص

الخلفية :يُعدّ زمن استجابة سيارات الإسعاف من المؤشرات التي تُراقَب على نطاق واسع لتقييم أداء خدمات الطوارئ الطبية (EMS)، إلا أن ارتباطه بالنتائج السريرية قد يختلف باختلاف الحالة الطبية، سواء تعلّق الأمر بتوقف القلب خارج المستشفى (OHCA)، أو الإصابات، أو غيرها من الحالات الطارئة ذات الخطورة العالية.
الهدف :تطوير إطار سردي استكشافي مولّد للفرضيات، يُمكن الاستفادة منه في مناقشات السياسات لدى هيئة الهلال الأحمر السعودي .(SRCA)وتتناول هذه المراجعة الارتباطات الخاصة بكل حالة مرضية بين الفترات الزمنية لاستجابة سيارات الإسعاف وكل من النجاة والوفيات وعودة الدورة الدموية التلقائية (ROSC)والنتائج العصبية، كما تحدد الأولويات المتعلقة بالرصد المرتبط بالنتائج السريرية والبحوث المستقبلية في المملكة العربية السعودية.
الأدلة والمنهجية :تستند هذه المراجعة السردية المركّزة، غير المنهجية، إلى 19تقريرًا بحثيًا أوليًا أمكن تحديدها، منها 12دراسة احتُفظ بها من قائمة مراجع عمل سابقة، وسبع دراسات جرى تحديدها من خلال تحديث ببليوغرافي موجّه حتى 25أكتوبر 2026.وصُنّفت الدراسات وصفيًا وفقًا للفئات السريرية، دون إجراء بحث شامل في قواعد البيانات، أو إعداد سجل قابل لإعادة التحقق لعملية الفرز، أو تقييم مستقل لمخاطر التحيز على مستوى الدراسات، أو إجراء تحليل تلوي، أو تصنيف رسمي لمستوى اليقين في الأدلة .كما أن الادعاء التاريخي بوجود 420سجلًا ناتجًا عن البحث لا يمكن التحقق منه استنادًا إلى السجلات المتاحة، ولذلك لم يُتعامل معه بوصفه نتيجة بحث موثقة.
النتائج :اشتملت التقارير البحثية التسعة عشر على 12دراسة تناولت حالات توقف القلب خارج المستشفى (OHCA)، ودراستين تناولتا الإصابات، وخمس دراسات تناولت مجموعات مختلطة أو حالات غير مرتبطة بتوقف القلب خارج المستشفى .وقد ارتبطت

الفترات الأطول لاستجابة سيارات الإسعاف، في كثير من الأحيان، بنتائج أسوأ لدى مرضى توقف القلب خارج المستشفى، إلا أن حجم هذا الارتباط وتفسيره اختلفا تبعًا لعوامل منها الإنعاش القلبي الرئوي الذي يقدمه المارة، وإزالة الرجفان، والنظم القلبي الأولي، وآلية اختيار الحالات .وأظهرت دراسة أترابية تايلاندية لحالات توقف القلب خارج المستشفى (n=5,433)أن نسبة الأرجحية المعدّلة للبقاء على قيد الحياة حتى الخروج من المستشفى بلغت 0.94مقابل كل دقيقة إضافية في زمن الاستجابة .كما وثّقت دراسة EuReCa-THREEعدد 32,033حالة توقف قلب خارج المستشفى عولجت بواسطة خدمات الطوارئ الطبية . وفي المقابل، لم تُظهر دراسة أترابية دنماركية شملت 171,708مرضى من غير المصابين بتوقف القلب، ممن يعانون حالات طارئة عالية الخطورة، ارتفاعًا في معدل الوفيات خلال 30يومًا بعد ضبط العوامل المؤثرة، وذلك لدى المجموعة ذات أطول زمن استجابة.
الخلاصة :تختلف الارتباطات المرصودة بين زمن استجابة سيارات الإسعاف والنتائج السريرية باختلاف التشخيص والرعاية المقدمة قبل وصول خدمات الطوارئ الطبية؛ ولا تثبت هذه الارتباطات وجود عتبة زمنية موحّدة ذات تأثير سببي على جميع الحالات .ويوفر الإطار المقترح توجهات أولية لتطوير قياس النتائج السريرية لدى هيئة الهلال الأحمر السعودي، ورصد أوجه التفاوت والإنصاف في تقديم الخدمات، وتحديد أولويات البحث العلمي، دون أن يشكّل توصيات تشغيلية أو سياساتية نهائية قائمة على تقييم منهجي لدرجة قوة الأدلة .وتظل هناك حاجة إلى إجراء مراجعة منهجية شاملة وتحليلات محلية مُعتمدة للتحقق من صلاحيتها قبل اتخاذ قرارات سياساتية نهائية.

الكلمات المفتاحية :خدمات الطوارئ الطبية؛ زمن استجابة سيارات الإسعاف؛ توقف القلب خارج المستشفى؛ الإصابات؛ الوفيات؛ النجاة؛ هيئة الهلال الأحمر السعودي؛ مراجعة سردية.

1. Emergency Medical Services – Technician, Saudi Red Crescent Authority;
2. Emergency Medical Services – Technician, Saudi Red Crescent Authority;
3. Emergency Medical Services – Technician, Saudi Red Crescent Authority;
4. Emergency Medical Services – Technician, Saudi Red Crescent Authority;
5. Emergency Medical Services – Technician, Saudi Red Crescent Authority;
6. Emergency Medical Services – Specialist, Saudi Red Crescent Authority;
7. Emergency Medical Services – Technician, Saudi Red Crescent Authority;
8. Emergency Medical Services – Technician, Saudi Red Crescent Authority.

1. Introduction

Prehospital emergency medical services bridge the interval between an emergency call and definitive clinical care. Response time is therefore a prominent operational indicator, but the interval may be defined differently between studies and systems: from call receipt to dispatch, dispatch to arrival at the scene, or the time until a clinician reaches the patient. Combining these definitions can obscure the clinical meaning of the observed associations [1–3].

Clinical interpretation is particularly challenging because an ambulance response interval reflects not only vehicle travel, but also how an emergency is recognized, how calls are classified, where resources are stationed, and whether care has already begun. A shorter measured interval does not necessarily indicate a comparable intervention across countries or diagnoses. Endpoints must also be distinguished: initial return of spontaneous circulation is not equivalent to survival to discharge, and crude mortality differs from risk-adjusted mortality [1–3,15].

For OHCA, delayed access to chest compressions and defibrillation has biological plausibility as a contributor to worse survival. Nevertheless, the ambulance interval cannot be evaluated independently of witnessed status, presenting rhythm, bystander cardiopulmonary resuscitation (CPR), public access to automated external defibrillators (AEDs), and variation in whether resuscitation is attempted [4–10]. In trauma and undifferentiated emergencies, the relationship is further complicated because dispatch algorithms may prioritize the most severely ill patients; the same patients may have higher baseline mortality despite receiving a faster response [2,3,11,12].

These limitations are relevant to Saudi Arabia, where urban congestion, geographic dispersion, rural access and differences in resource deployment can affect time to care. Published Saudi evidence

describes both clinical OHCA outcomes and regional variations in EMS response intervals, but does not yet establish a transferable causal response-time effect across the SRCA system [8,23,24].

1.1. Review objective

The aim is to provide an exploratory, hypothesis-generating narrative framework that can inform SRCA policy dialogue, measurement design and research priorities. It describes how ambulance response intervals have been associated with survival, mortality, ROSC and neurological outcomes across OHCA, trauma and mixed emergency populations, while distinguishing observational patterns from causal effects. It does not seek to replace a comprehensive systematic review or establish validated ambulance deployment standards.

A practical rationale for focusing on the Saudi Red Crescent Authority (SRCA) is the need to move from isolated operational time targets toward clinically meaningful, equitable evaluation. International findings are useful for identifying hypotheses, but Saudi-specific causal estimates require linked local dispatch, clinical and follow-up records. Consequently, this manuscript emphasizes the boundaries of what is known, as well as the implications for data design.

2. Methods: Exploratory Narrative Framework

Purpose and scope. This review is designed as an exploratory narrative framework to organize existing observations, identify evidence gaps and suggest testable directions for Saudi Red Crescent Authority (SRCA) performance measurement and policy deliberation. It is hypothesis-generating rather than confirmatory. The findings should not be interpreted as a comprehensive or systematically selected evidence base, a formal assessment of effect certainty, or sufficient justification for a particular response-time target or system-wide policy change. A separate protocol-driven systematic review, supplemented by context-specific Saudi outcome data, remains necessary for definitive guidance.

2.1. Design and provenance of the evidence

This exploratory narrative review uses a purposively assembled evidence set rather than a prospectively registered, exhaustive systematic review. The source set comprises 12 reports from a prior working manuscript (H01–H12; published 2002–2024) and seven subsequently identified primary reports (U01–U07; first published online during 2025–2026). The earlier manuscript referred to a search conducted between 10 January and 20 February 2025 and to 420 records; however, no database exports, duplicate logs, screening decisions or full-text exclusion records were available for audit. Accordingly, 420 is an unverified historical figure, 19 is the number of identifiable reports discussed here rather than a PRISMA-validated inclusion count, and no missing selection numbers have been reconstructed.

2.2. Sources and identification process

The recent evidence was assembled through targeted checks of identifiable bibliographic records and accessible publisher or indexing pages through 25 October 2026, rather than a fully reproducible multi-database search. The reports were retained to discuss complementary clinical populations and newer estimates. This purposive approach can omit eligible publications and is not presented as an exhaustive search of MEDLINE, Embase, Scopus, Web of Science or CENTRAL.

2.3. Relevance criteria and evidence boundaries

Studies were considered relevant when they involved persons receiving prehospital EMS care, a measurable emergency response interval and a patient-centred clinical endpoint such as survival, death, ROSC or neurological status. Operational timing studies without patient outcomes and non-primary reviews were used, where relevant, only for context. Populations were grouped descriptively as OHCA, trauma or mixed/non-OHCA. Study selection could not be independently recreated from the prior search, and these criteria should not be confused with prospectively applied eligibility screening.

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2.4. Evidence provenance and accounting

The following source-accounting summary describes only report provenance. It must not be interpreted as a study-selection flowchart: the historical 420-record claim has no auditable database or screening log, so no removal, exclusion or final eligibility counts can be confirmed.

Box 1. Documented provenance of the 19 primary reports. This is not a PRISMA study-selection diagram; the number of records screened or excluded cannot be verified.

Evidence source	Reports (n)	What can be verified
Earlier working bibliography	12	Named citations H01–H12; original database search and screening records unavailable
Targeted bibliographic updating	7	Identifiable primary articles U01–U07, with selected bibliographic and abstract checks through 25 October 2026
Total reports discussed	19	Curated narrative evidence set; not a systematically determined inclusion count

2.5. Data reporting, critical appraisal and synthesis

For each report, the evidence table records author, clinical setting or population, analytic denominator, response-interval contrast and an available outcome estimate or summary. Some historical estimates were inherited from the source manuscript and must be checked against the full published text before a definitive systematic synthesis. References are numbered to match the original manuscript, enabling a direct audit of the 19 primary reports [1–12,16–22].

No independently adjudicated, study-level critical appraisal has been completed for this curated set. Consequently, individual studies are not assigned validated JBI risk-of-bias categories, and the findings are interpreted with explicit attention to selection, confounding by indication, measurement differences and residual bias. The JBI approach [14] is identified as a possible design-appropriate tool for a future protocol-driven systematic review, not as an assessment completed here. Differences in exposure intervals, populations, outcomes and adjustment sets also preclude defensible pooling from the present source collection.

2.6. Handling source verification and numerical estimates

Study identities and selected outcomes were compared with available bibliographic records, publisher pages or indexed abstracts for key sources, including the Japanese registry [5], Saudi Jubail cohort [8], recent broader synthesis [15], Thai cohort [16], EuReCa-THREE [17], Norwegian ambulance-availability study [18], Danish, South Korean and Italian reports [19–22], and Saudi regional service study [23]. These targeted checks do not amount to an independent full-text audit of every one of the 19 primary studies. Other inherited estimates must be verified against original articles and supplementary tables before journal submission.

2.7. Reporting standard and eligibility statement

This manuscript is reported as an exploratory narrative review and does not claim to meet PRISMA 2020 requirements for a completed systematic review [13]. Box 1 reports only the identifiable provenance of reports (12 inherited and seven newly identified). It does not present unverified record-screening, duplicate-removal or exclusion counts. Formal independent screening, study-level critical appraisal [14] and certainty assessment were not undertaken. A future systematic review would require a protocol, reproducible searches, reviewer-level screening records, justified exclusions and design-appropriate appraisal before making evidence-graded conclusions.

3. Characteristics of the Evidence

The 19 distinct primary reports comprise 12 OHCA-focused cohorts, two trauma cohorts and five mixed/non-OHCA groups. Across the reports, response time appears as a continuous interval, a dichotomous threshold or multiple interval bands. The study counts describe this curated evidence

set and do not establish the number of studies that would be included after a complete systematic search and independent eligibility review.

Table 1. Characteristics and principal findings of the 19 primary studies. Source reference numbers follow the original manuscript.

Study / source	Setting / sample	Response interval	Main reported association
OHCA			
H04 Bürger et al. 2018 [4]	Germany / OHCA (n=10,853)	Continuous / system comparison	Longer intervals associated with lower survival
H05 Goto et al. 2018 [5]	Japan / OHCA (n=553,426)	Per additional min	Adjusted neurological survival OR 0.89 (95% CI 0.89–0.90)
H06 Holmén et al. 2020 [6]	Sweden / OHCA (n=20,420)	0–6, 7–9, 10–15, >15 min	Survival declined with longer response
H07 Moon et al. 2020 [7]	Korea / OHCA (n=13,245)	3-min intervals by CPR	Apparent time-outcome relation varied by bystander CPR
H08 Alumran et al. 2020 [8]	Saudi Arabia / OHCA (n=108)	>8 vs <=8 min	Death OR 2.4 for >8 vs ≤8 min (95% CI 0.81–7.1); not significant [8]
H09 Huang et al. 2021 [9]	Taiwan / OHCA (n=6,742)	<6.2 vs >=6.2 min	Adjusted survival OR 1.992 (95% CI 1.496–2.653)
H10 Stoesser et al. 2021 [10]	US/Canada / OHCA (n=102,216)	Continuous + interactions	Delay-survival association modified by AED and circumstances
U01 Damdin et al. 2025 [16]	Thailand / OHCA (n=5,433)	Per additional min	Discharge survival adjusted OR 0.94 per minute (95% CI 0.89–0.98; p=0.006) [16]
U02 Gräsner et al. 2025 online [17]	28 countries / OHCA (n=32,033 EMS-treated)	International response variation	Mean 12.2 min; overall survival 7.5%; reported time link
U04 Hindborg et al. 2025 online [20]	Denmark / witnessed shockable OHCA (n=3,924)	Time bands + bystander AED	Public-location 30-day survival ratio 1.24 (95% CI 1.03–1.49) with bystander AED at 7–9 min [20]
U05 Kwon & Shin 2026 [21]	Korea / OHCA (n=2,268 analyzed; 2,388 eligible)	Minute-category model	Shorter response associated with prehospital ROSC [21]
U06 Marjin et al. 2026 [22]	Italy / OHCA (n=12,532)	Minutes; system efficiency	Longer time associated with higher prehospital mortality, OR 1.03/min
TRAUMA			
H02 Pons & Markovchick 2002 [2]	USA / trauma (n=3,490)	>8 vs <=8 min	Adjusted mortality OR 0.81 (95% CI 0.43–1.52); null
H11 Björkman et al. 2022 [11]	Finland / HEMS trauma (n=4,803)	Multiple EMS/HEMS intervals	No overall adjusted association with 30-day death
MIXED/NON-OHCA			

Study / source	Setting / sample	Response interval	Main reported association
H01 Blackwell & Kaufman 2002 [1]	USA / mixed (n=5,424 transports)	<5 vs >5 min	0.51% vs 1.58% mortality (unadjusted); confounding substantial
H03 Blanchard et al. 2012 [3]	Canada / mixed high-priority (n=7,760 responses)	>=8 vs <8 min	Adjusted mortality OR 1.19 (95% CI 0.97–1.47); null
H12 Mills et al. 2024 [12]	Denmark / mixed (n=182,895 people)	Response interval	Adjusted overall OR 1.00 (95% CI 0.99–1.00)
U03 Asheim et al. 2025 [18]	Norway / medical emergencies (n=239,320)	Busy-ambulance-linked delay	5-min delay: 7-day death RD +0.10 pp (95% CI -0.17 to 0.36)
U07 Bitzer et al. 2026 [19]	Denmark / non-OHCA (n=171,708)	Longest vs shorter quartiles	Longest Q4 vs Q1–Q3: 30-day mortality OR 0.87 (95% CI 0.83–0.91); non-causal [19]

Abbreviations: AED, automated external defibrillator; EMS, emergency medical services; HEMS, helicopter EMS; OHCA, out-of-hospital cardiac arrest; OR, odds ratio; ROSC, return of spontaneous circulation. H01–H12 are inherited citations; U01–U07 are targeted additions. Sample sizes are not summed because studies, outcomes and registries are heterogeneous.

3.1. Composition and geographic coverage

The curated evidence collection is predominantly focused on OHCA (12 of 19 primary reports), compared with two trauma studies and five mixed or high-acuity non-OHCA reports. Settings span North America, Europe and Asia. One small Saudi hospital-based OHCA cohort was included [8]; Saudi operational-response evidence [23] and qualitative organizational research [24] are discussed as contextual sources, not as additional studies in the 19-report patient-outcome set. Consequently, OHCA estimates cannot be assumed to represent most SRCA missions, and Saudi outcome-linked evidence remains limited.

3.2. Outcome heterogeneity

Studies used different response-time definitions and comparisons: per-minute associations, cut-offs such as eight minutes, data-derived thresholds, and quartiles or wider categorical intervals. Endpoints ranged from prehospital ROSC to hospital discharge, 7-day mortality, 30-day mortality, and favorable neurological status. Effect estimates therefore cannot be compared as though they are estimates of one underlying response-time parameter. Associations from separate registries were not pooled, and overlapping populations were not quantified.

3.3. Interpreting the evidence table

Table 1 summarizes one principal estimate or finding for each report, not all outcomes or adjustments in the publication. Values labeled as odds ratios refer to different endpoints and comparison groups. Reported sample sizes are study-specific analytic denominators and should not be summed to describe independent patients, particularly across national registries with possible overlap. The accompanying provenance appendix distinguishes limited verification from the full-text review that would be required in a systematic assessment.

4. Narrative Findings

4.1. Out-of-hospital cardiac arrest

In the historical evidence, OHCA cohorts generally reported that longer professional arrival intervals were associated with less favorable survival or neurological outcomes. The large Japanese cohort by Goto et al. (n=553,426) reported lower adjusted odds of neurologically favorable one-month survival per additional minute (OR 0.89; 95% CI 0.89–0.90) [5]. Reports from German and Swedish settings

described directionally similar survival gradients [4,6]. However, these observational associations must not be interpreted as independent treatment effects of reducing every ambulance response by one minute.

Variation in community response is crucial to interpretation. The Korean, Taiwanese and North American OHCA studies highlighted interactions with bystander CPR, location, witnessed status and AED availability [7,9,10]. A Taiwanese 6.2-minute discriminative cut-off was derived from one dataset and is not, by itself, a validated deployment standard [9]. Conversely, a small Saudi OHCA cohort from Jubail (n=108) did not show a statistically robust bivariate survival difference across an eight-minute threshold, although the precision was limited and the finding should not be interpreted as proof that response interval is unimportant [8].

The recent Thai five-year cohort (n=5,433) reported an adjusted odds ratio for survival to discharge of 0.94 for each additional minute of EMS response (p=0.006) [16]. EuReCa-THREE, a prospective multinational registry study, included 32,033 EMS-treated OHCA cases from 28 countries; mean response time was 12.2 minutes and overall survival was 7.5%. The authors described response-time associations across settings, underscoring the relevance of regional and system variation [17]. These findings remain associations within their respective study designs and should not be transferred unadjusted to Saudi Arabia.

More recent analyses further emphasize effect modification. Hindborg et al. studied 3,924 bystander-witnessed Danish OHCA cases with shockable initial rhythm and bystander CPR; for public-location arrests at EMS intervals of 7–9 minutes, bystander defibrillation was associated with higher adjusted 30-day survival than no bystander defibrillation (survival ratio 1.24; 95% CI 1.03–1.49) [20]. A Busan cohort included 2,388 eligible adults, of whom 2,268 were retained in response-minute regression categories, and reported stronger associations between shorter responses and prehospital ROSC [21]. A Lombardy observational cohort (n=12,532) found higher odds of prehospital death with longer intervals (OR 1.03 per minute), while its system-efficiency framework also considered community response [22].

In the Bangkok cohort, the reported 5,433 OHCA cases included approximately 1% survival to hospital discharge, showing that statistically observed per-minute changes arose against a low absolute discharge-survival rate. The investigators estimated an adjusted discharge-survival odds ratio of 0.94 (95% CI 0.89–0.98) per additional response minute and an odds ratio of 2.09 (95% CI 1.00–4.35) for responses at or below eight minutes versus longer intervals [16]. These are observational associations rather than a transferable effect of introducing an eight-minute policy.

The EuReCa-THREE registry identified 45,251 confirmed OHCA episodes, of which 32,033 were treated by EMS. The present narrative denominator is the EMS-treated subset rather than all confirmed cases, illustrating the importance of recording the analytic cohort when contrasting emergency systems [17]. The recent Busan study likewise distinguishes 2,388 eligible cases from the 2,268 included in its response-minute regression categories [21].

4.2. Trauma

Evidence within the two trauma reports does not substantiate a universal eight-minute response threshold for mortality. Pons and Markovchick studied 3,490 patients and reported an adjusted mortality OR of 0.81 (95% CI 0.43–1.52) for response intervals exceeding eight minutes, a confidence interval consistent with uncertainty rather than a convincing protective effect [2]. A Finnish HEMS analysis of 4,803 trauma patients found no clear overall adjusted relationship between the considered prehospital intervals and 30-day mortality [11]. Case severity, dispatch selection, geographic distance and rescue-resource allocation may obscure effects in particular injury subgroups.

The narrow trauma evidence should not be framed as demonstrating that minutes never matter: neither study can exclude clinically important relationships within hemorrhage, traumatic brain

injury or patients needing rapid lifesaving procedures. Mixed dispatch populations may hide such subgroups even when their overall adjusted effect is null. Future work should consider injury severity and treatment opportunity alongside operational intervals, without choosing thresholds from retrospective fit alone.

4.3. Mixed and non-OHCA high-acuity emergencies

Mixed-population studies frequently attenuated toward null associations after case-mix adjustment. In an urban high-priority cohort, Blanchard et al. reported an adjusted mortality OR of 1.19 (95% CI 0.97–1.47) for responses of at least eight minutes compared with shorter responses [3]. Other observational registry studies likewise cautioned against interpreting unadjusted interval differences as a universal survival gradient [1,12].

Asheim et al. analysed 239,320 Norwegian medical emergencies using variation in ambulance availability to assess the possible consequences of response delay. A five-minute delay was associated with a seven-day mortality risk difference of +0.10 percentage points (95% CI −0.17 to +0.36), with no clear substantial overall mortality increase, although other aspects of hospital use differed [18]. In a recent Danish matched priority-A cohort excluding OHCA (n=171,708), the longest-response quartile was not associated with increased 30-day mortality compared with shorter quartiles (OR 0.87; 95% CI 0.83–0.91) [19]. This seemingly inverse association must not be interpreted as evidence that delaying ambulances improves outcomes: residual severity, geography, dispatch priority and selection processes remain plausible explanations.

4.4. Cross-condition comparison

Across diagnoses, the association is strongest and most biologically interpretable in OHCA, especially when response time is evaluated jointly with bystander CPR and defibrillation. Trauma studies in this set are comparatively few and imprecise for universal thresholds. Mixed and non-OHCA studies using risk adjustment or quasi-experimental approaches often find attenuated or absent overall mortality gradients. The synthesis thus supports stratified performance assessment rather than one system-wide time-to-survival conversion factor.

5. Interpretation and Sources of Uncertainty

5.1. Why clinical context matters

A single response-time target cannot adequately represent the clinical complexity of prehospital emergencies. In OHCA, the window before professional arrival intersects with immediate bystander action, rhythm-dependent defibrillation and the probability of successful resuscitation [4–10,16,17,20–22]. In trauma or heterogeneous dispatch populations, waiting time may matter for selected time-critical conditions, while overall mortality models can be diluted by case mix and triage selection [1–3,11,12,18,19]. It is therefore more defensible to estimate condition-specific and severity-adjusted relationships than to impose one cross-diagnostic effect estimate.

5.2. Confounding and measurement

Confounding by indication is a major methodological concern. EMS dispatchers allocate resources according to reported urgency, producing shorter observed intervals for patients who are already at greater risk of death. Other important variables include distance, rurality, vehicle availability, first-responder activation, witness status, CPR and AED use, comorbidity and hospital destination. Differences in time-stamp definitions, missing data, exposure categorization and outcome horizons limit comparability between cohorts.

5.3. Position relative to broader synthesis

A broader systematic review and meta-analysis published in 2025 by Hansen and colleagues included 115 studies involving 691,056 patients and judged the certainty of the overall response-time/survival evidence to be very low [15]. Its documented search and appraisal methods are fundamentally different from this purposive narrative collection. Accordingly, the present framework adds context for SRCA-oriented measurement questions but does not replace that systematic synthesis or justify evidence-graded operational thresholds.

5.4. Absolute and relative effect interpretation

An odds ratio such as 0.94 per minute does not mean that an additional minute causes a fixed six-percentage-point fall in survival probability. It expresses a relative change in the odds under a specific regression model, and the corresponding absolute difference depends on the starting outcome risk. Similarly, an adjusted OR of 0.87 for a later response quartile must not be interpreted as evidence that delay is beneficial. Such comparisons may retain residual differences in patient severity, call coding, location and resource selection [16,19].

5.5. Limits of transferability to Saudi EMS

The studies span urban registries, helicopter-supported trauma systems and international OHCA networks with different dispatch definitions, staffing, first-responder systems and distances. Transporting their point estimates to the SRCA without local validation could misstate the likely benefit of operational changes. Locally linked clinical outcomes, patient acuity, time stamps and community interventions are necessary to evaluate which parts of ambulance response delay are modifiable and clinically consequential [8,23,24].

6. Implications for the Saudi Red Crescent Authority

For SRCA evaluation, ambulance response time is a useful operational metric, but patient-outcome analyses should distinguish diagnosis, dispatch priority and baseline acuity. The time record should differentiate call receipt, triage, dispatch, crew mobilization, travel, scene arrival and first patient contact. Reports should include medians and upper percentiles and be stratified by region, urban/rural setting and dispatch priority. A 2026 Saudi cross-sectional analysis of 3,398 time-critical EMS cases across five regions found that more than 88% of responses exceeded eight minutes; it investigated operational variation, not causal effects on mortality [23]. Therefore, locally linked clinical outcome analyses are needed before interpreting these service intervals as survival effects.

A linked OHCA registry would be particularly informative if it captured witnessed status, first monitored rhythm, bystander CPR, AED use, ROSC, survival to hospital discharge and neurological outcome. Trauma and non-OHCA pathways should use clinically meaningful outcomes rather than treating all high-priority cases as a single patient group. Operational planning should also account for organizational factors such as staffing, coordination and rural service delivery reported in Saudi qualitative research [24].

Future Saudi analyses should prespecify response-time definitions, important confounders, subgroup comparisons and missing-data methods. Where feasible, prospective or quasi-experimental evaluations of station placement, dispatch practices or resource changes may help separate effects of operational intervention from underlying case severity. International estimates can inform hypotheses and monitoring frameworks but should not be represented as proven SRCA-wide causal effects.

6.1. Operational measurement priorities

SRCA operational reporting should separate five events: emergency call receipt, dispatch decision, resource mobilization, on-scene arrival and first patient contact. Clinical registries should also capture initiation of bystander CPR, first defibrillation and subsequent patient outcomes when applicable. Quality dashboards can then compare the median, 90th percentile and geographically stratified distributions for clinically similar call categories rather than treating the arithmetic mean as a sufficient index.

6.2. Clinical linkage and analytic design

For OHCA, an Utstein-aligned minimum dataset should include witnessed status, initial rhythm, bystander CPR, AED use, EMS attempted resuscitation, ROSC, survival to discharge and neurological status. For trauma, potential measures include injury severity, major hemorrhage, access to time-critical intervention and 30-day death. For mixed emergencies, dispatch category, baseline comorbidity, presenting acuity and reason for non-transport should be captured. Analyses should prespecify potential confounders and sensitivity analyses addressing time-stamp quality and missingness.

6.3. Suggested SRCA reporting data dictionary

Table 2. Illustrative minimum reporting elements proposed for future consideration in SRCA-specific evaluation. These are exploratory measurement options, not documented current SRCA data fields or evidence-graded policy recommendations.

Variable	Operational definition	Reason to capture
Call-to-dispatch	Call receipt to confirmed dispatch decision	Separates triage and dispatch processing
Dispatch-to-en-route	Dispatch assignment to crew departure	Measures mobilization rather than road travel
En-route-to-scene	Crew departure to arrival on scene	Travel and geographic access
Scene-to-patient	Scene arrival to first patient contact	Building/access and patient access delay
Dispatch acuity	Predefined priority and complaint code	Supports case-mix comparability
Clinical category	OHCA; major trauma; other acute diagnosis	Permits clinically relevant subgroup analysis
Bystander intervention	CPR and/or AED before EMS arrival	Potential effect modifier
Patient outcome	ROSC; discharge survival; 30-day mortality; neurological outcome	Links operational time to meaningful endpoints
Geographic context	Region; urban/rural; distance to response station	Equity and confounding assessment

6.4. Illustrative roadmap for future evaluation

An illustrative evaluation roadmap, subject to SRCA feasibility and governance review, is to: standardize timestamp definitions and audit missing values; link dispatch records with appropriately governed, de-identified patient outcomes; pilot condition-specific risk-adjusted monitoring; examine geographic and dispatch-priority variation; and formally evaluate a prespecified operational change. This sequence is a research and measurement proposal rather than an implementation directive. Neither local causal benefits nor a universal response-time threshold should be claimed before supporting analysis.

7. Limitations and Boundaries of Policy Interpretation

This work was intentionally developed as an exploratory narrative framework, not a substitute for a comprehensive systematic review. Its purposively assembled evidence set is not exhaustive and is potentially affected by selection, retrieval and publication bias. The 12 historical references cannot be reconciled with the asserted 420-record search through an auditable selection log; the seven newer reports came from targeted, non-systematic identification. Thus, 19 represents the number of identifiable publications discussed, not the final inclusion counts from reproducible PRISMA screening. Complete full-text verification was not available for every study, the preliminary JBI observations were not independently adjudicated, and some inherited numerical findings still require confirmation against source tables. Registry overlap, heterogeneity of timing definitions, outcomes, confounder adjustment and follow-up periods further limit synthesis. We have not derived a pooled effect, evaluated formal certainty or estimated an SRCA-specific causal response-time benefit.

These limitations define the scope of the policy implications. The proposed monitoring indicators and research priorities are conceptual options for SRCA consideration, not validated effectiveness claims or evidence-graded recommendations for changes in dispatch, staffing, station location or response-time thresholds. Findings from international systems cannot be assumed to generalize to Saudi geography, clinical case mix, first-responder capacity, resource allocation or equity

conditions. Definitive decisions should rely on a prospectively specified, fully documented systematic review where appropriate, together with analyses of linked Saudi prehospital and patient-outcome data, feasibility, costs and equity. The framework should therefore be used to formulate and prioritize questions for evaluation, not to resolve them conclusively.

8. Conclusion

This exploratory narrative framework draws on 19 identifiable primary reports to illustrate how associations between EMS response intervals and patient outcomes depend on clinical condition, dispatch decisions and care before ambulance arrival. Although longer response times often correlate with less favorable OHCA outcomes, the observed patterns in trauma and heterogeneous emergencies are less consistent. The value for the SRCA is to identify candidate outcomes, confounders, equity measures and research priorities for local validation—not to establish a universal target or offer definitive evidence-graded policy recommendations. This non-systematic synthesis does not replace a comprehensive systematic review; rigorous, reproducible searching and study appraisal, combined with linked Saudi outcome analyses, are required before strong causal or policy claims.

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Appendix A: Critical Appraisal Using the JBI Eight-Item Checklist

Ambulance Response Time and Patient Outcomes in Prehospital EMS — Group E

Table 1. Preliminary appraisal of the 19 primary reports

Group	Study	Q 1	Q 2	Q 3	Q 4	Q 5	Q 6	Q 7	Q 8	% Score
OHCA	**Bürger et al. (2018) [4]**	✔	✔	✔	✔	✔	✔	✔	✔	**100.0%**
OHCA	**Goto et al. (2018) [5]**	✔	✔	U	U	✔	✔	U	✔	**62.5 %**
OHCA	**Holmén et al. (2020) [6]**	✔	✔	✔	✔	✔	✔	✔	✔	**100.0%**
OHCA	**Moon et al. (2020) [7]**	✔	✔	U	✔	✔	✔	✔	✔	**87.5 %**
OHCA	**Alumran et al. (2020) [8]**	U	✔	U	U	✔	✗	U	✗	**25.0 %**
OHCA	**Huang et al. (2021) [9]**	✔	✔	U	U	✔	✔	✔	✔	**75.0 %**
OHCA	**Stoesser et al. (2021) [10]**	✔	✔	U	✔	✔	✔	✔	✔	**87.5 %**
OHCA	**Damdin et al. (2025) [16]**	✔	✔	U	U	✔	✔	✔	✔	**75.0 %**
OHCA	**Gräsner et al. (2025) [17]**	✔	✔	✗	✗	✗	✗	✔	✗	**37.5 %**
OHCA	**Hindborg et al. (2025) [20]**	✔	✔	✗	✔	✔	✔	✔	✔	**87.5 %**
OHCA	**Kwon & Shin (2026) [21]**	✔	✔	✗	✗	✔	✔	✔	✔	**75.0 %**
OHCA	**Marjin et al. (2026) [22]**	✔	✔	✗	✗	✔	✔	✔	✔	**75.0 %**
Trauma	**Pons & Markovchick (2002) [2]**	✔	✔	✔	✔	✔	✔	✔	✔	**100.0%**
Trauma	**Björkman et al. (2022) [11]**	✔	✔	✔	✔	✔	✔	✔	✔	**100.0%**
Mixed / non-OHCA	**Blackwell & Kaufman (2002) [1]**	✔	✔	✔	✗	✔	✗	✗	✗	**50.0 %**
Mixed / non-OHCA	**Blanchard et al. (2012) [3]**	✔	✔	✗	✗	✔	✔	✔	✔	**75.0 %**
Mixed / non-OHCA	**Mills et al. (2024) [12]**	✔	✔	✗	✗	✔	✔	✔	✔	**75.0 %**

Group	Study	Q 1	Q 2	Q 3	Q 4	Q 5	Q 6	Q 7	Q 8	% Score
Mixed / non-OHCA	**Asheim et al. (2025) [18]**	✔	✔	✔	✗	✔	✔	✔	✔	**87.5 %**
Mixed / non-OHCA	**Bitzer et al. (2026) [19]**	✔	✔	✔	✗	✔	✔	✔	✔	**87.5 %**

Legend: ✔ = Yes; ✗ = No; Score = number of Yes judgments ÷ 8 × 100. No not awarded points. A low score may reflect unavailable reporting rather than demonstrably poor study conduct; these percentages must not be interpreted as validated risk-of-bias probabilities or evidence-certainty ratings.

Questions Q1–Q8

Q1. Were the criteria for inclusion in the sample clearly defined?
Q2. Were the study subjects and the setting described in detail?
Q3. Was the exposure measured in a valid and reliable way?
Q4. Were objective, standard criteria used for measurement of the condition?
Q5. Were confounding factors identified?
Q6. Were strategies to deal with confounding factors stated?
Q7. Were the outcomes measured in a valid and reliable way?
Q8. Was appropriate statistical analysis used?

Important methodological qualifications

The supplied review states that no independently adjudicated JBI appraisal was undertaken. This worksheet is therefore an additional, provisional assessment, not evidence that the manuscript's methods already included formal JBI appraisal.

The eight questions are from JBI's analytical cross-sectional checklist. Applying them uniformly to mainly retrospective/prospective cohorts does not substitute for the official JBI cohort checklist, including follow-up, baseline outcome status and loss-to-follow-up items.

Alumran et al. (2020) reports inconsistent participant numbers (e.g., a 108-patient table alongside a 63-patient statement in the Results section). Its unadjusted bivariate approach also leaves important potential confounding unaddressed. The judgments shown are deliberately conservative.

The percentages are descriptive counts of positive judgments within a preliminary worksheet. Formal publication-grade appraisal requires access to every complete original study, prespecified decision rules, independent reviewers, and reconciliation of disagreements.

Methodological source notes

JBI Manual: eight-item analytical cross-sectional checklist — https://jbi-global.atlassian.net/wiki/spaces/MANUAL/pages/355599424/Appendix+7.5+Critical+appraisal+checklist+for+analytical+cross-sectional+studies

JBI Manual: cohort-study checklist (11 questions) — https://jbi-global.atlassian.net/wiki/spaces/MANUAL/pages/355599324/Appendix+7.1+Critical+appraisal+checklist+for+cohort+studies

Source manuscript: "Ambulance Response Time and Patient Outcomes…" (Group E); primary study descriptions and references [1–12, 16–22] included therein.

Additional verification included selected full-text or indexed study pages, particularly Bürger (2018), Holmén (2020), Alumran (2020), Pons & Markovchick (2002), Asheim (2025), and Bitzer (2026).