Prehospital notification for major trauma patients requiring emergency hospital transport: A systematic review

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Abstract

Objective: This systematic review aimed to determine the effect of prehospital notification systems for major trauma patients on overall (<30 days) and early (<24 hours) mortality, hospital reception, and trauma team presence (or equivalent) on arrival, time to critical interventions, and length of hospital stay.

Methods: Experimental and observational studies of prehospital notification compared with no notification or another type of notification in major trauma patients requiring emergency transport were included. Risk of bias was assessed using the Cochrane ACROBAT-NRSI tool. A narrative synthesis was conducted and evidence quality rated using the GRADE criteria.

Results: Three observational studies of 72,423 major trauma patients were included. All were conducted in high-income countries in hospitals with established trauma services, with two studies undertaking retrospective analysis of registry data. Two studies reported overall mortality, one demonstrating a reduction in mortality; (adjusted odds ratio (OR) 0.61, 95% confidence interval (CI) 0.39 to 0.94, 72,073 participants); and the other demonstrating a nonsignificant change (OR 0.61, 95% CI 0.23 to 1.64, 81 participants). The quality of this evidence was rated as very low.
1 | INTRODUCTION

Trauma systems, incorporating centralized trauma centers have helped address the injury burden by providing prompt, specialist trauma care. In such systems, effective therapy for the severely injured is facilitated by an interdisciplinary and integrated (horizontal) approach to undifferentiated trauma with input from prehospital and in-hospital resuscitation teams. Improvements in prehospital triage, management and priority transport of the most severe cases to the highest level treatment centers have been provided as likely explanations for the reduced risk of death. Such inclusive systems of trauma care should be regarded as optimal standards for health jurisdictions, but require substantial resources.

Prehospital notification of the impending arrival of a patient requiring emergency care, is seen as an integral component of an advanced prehospital care system. The effect of prehospital notification has been evaluated in many areas of health care including stroke, coronary heart disease and, to a lesser degree, trauma with beneficial effects on mortality, and other outcomes, when included as part of an advanced emergency care system. Prehospital notification provides the opportunity for the receiving hospital to improve preparedness for reception and resuscitation of a critically injured or unwell patient. The information communicated may include the estimated arrival time, patient demographics, suspected injuries, vital signs, symptoms and any treatments provided in the prehospital setting. However, the evidence base for this seemingly fundamental component of the trauma care system is limited. Concurrently, many settings are lacking organized systems for prehospital care and if present, limited by absence of prehospital notification.

This objective of this systematic review was to determine the effect of a prehospital notification system for major trauma patients requiring emergency transport to a healthcare facility on overall (<30 days) and early (<24 hours) mortality, hospital reception and trauma team presence (or equivalent) on arrival, time to critical interventions and hospital length of stay. A secondary objective was to draw upon this evidence to document existing approaches to prehospital major trauma notification in both high-income and low- and middle-income countries.

2 | METHODS

This systematic review was conducted and reported in line with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) Statement. The protocol is available from PROSPERO (http://www.crd.york.ac.uk/PROSPERO/display_record.asp?ID=CRD42015020180).

2.1 | Literature searching and search strategy

The following databases were searched in June 2015: MEDLINE (Ovid) and EMBASE (Ovid) (Appendix A); and the following databases were searched in March 2015 (Appendix B): Cochrane CENTRAL Database of Controlled Trials (Wiley), CINAHL Plus (EBSCOhost), and Web of Science Conference Proceedings. Search strategies used a combination of controlled vocabulary and text words. Included studies were limited to those in English but no publication date restrictions were applied. Google Scholar (terms) were also searched and the first 100 citations identified and screened.

Grey literature was incorporated from two trial registries (ClinicalTrials.gov; World Health Organisation International Clinical Trials Registry Platform (ICTRP)); conference proceedings (Web of Science Conference Proceedings Citation Index–Science (1990 to present) and Embase); and the trauma.org website. Other search methods included contacting experts known to the authors, and via a traumatology email list. Reference lists of studies identified for inclusion in the review and citation tracking of these studies and other relevant references were also conducted. Search results were downloaded into Endnote (endnote.com) and then into Covidence (covidence.org), a web-based software platform for the production of systematic reviews. Duplicates were removed using both EndNote and Covidence.

Two pairs of authors (AK, LB, AS, MF, or MC) independently screened citations on title and abstract, selecting agreed citations in full text. Results were compared and any disagreement resolved by discussion or consultation with a third author. The same process was repeated with full-text citations, with agreed citations included in the review. The study selection process was documented according to the PRISMA flowchart.
2.2 Inclusion criteria

2.2.1 Study design

Eligible studies included those that investigated the effect of prehospital notification for major trauma patients requiring emergency transport to a healthcare facility. The comparison group could either be no prehospital notification, or the study could compare two different methods of prehospital notification. Included study designs were randomized controlled trials (RCTs), cluster-randomized controlled trials (C-RCTs), controlled before-after studies (CBAs) and interrupted time series (ITS) in line with Cochrane recommendations. Given that we expected few experimental studies, observational studies with a control group were also included.

2.2.2 Participants

To be included in the review, a study needed to have at least 80% of participants with major traumatic injury requiring emergency transport to a healthcare facility. The inclusion was limited to major trauma patients as this is the population that is expected to benefit most from prehospital notification. As there is no agreed definition of major trauma in the literature, a revised version of the Victorian State Trauma Registry definition was used to guide inclusion decisions. This includes death after injury, admission to an intensive care unit (ICU) or high dependency area for more than 24 hours and mechanically ventilated after admission, significant injury to two or more Injury Severity Score (ISS) body regions or an ISS greater than 12 and emergency surgery for intracranial, intrathoracic, or intra-abdominal injury, or for fixation of pelvic or spinal fractures. Emergency transport was defined as any type of vehicle assigned to transporting patients to a healthcare facility, including public and private ambulances, police or military vehicles, and airborne transport.

2.2.3 Intervention

Prehospital notification was defined as any type of communication between emergency personnel and a receiving healthcare provider with the purpose of preparing the receiving hospital for an incoming patient. There were no exclusion criteria about the method of communication (eg, two-way radio, text message, phone call, electronic system, etc), or the type of information that was shared. However, studies were excluded if the primary purpose of the communication was to provide specialist advice from the hospital to emergency personnel.

2.2.4 Outcomes

The primary outcome of interest was overall mortality (<30 days) and secondary outcomes were early mortality (<24 hours), hospital reception and trauma team presence (or equivalent) on arrival, time to intervention (as defined by the included study), and hospital and ICU length of stay. Details regarding the cost of implementing prehospital notification systems were also sought, without intention to conduct a formal analysis of this data. Included studies had to report at least one of the outcomes of interest to be included.

2.3 Data collection and assessment of risk of bias

Two pairs of authors (AK, AS, LB, or MC) independently extracted data and assessed risk of bias, and compared results. Any disagreements were resolved by discussion or consultation with a third author.

A detailed data extraction form was used, collecting information about the study design and source of funding, setting (including geographical location, and prehospital care system characteristics), participant characteristics (including demographics, potential confounders, and inclusion criteria), intervention characteristics (including type of technology, information communication, personnel involved, and hospital response) and outcomes measured (including timing, definitions and dichotomous or continuous raw data and raw and/or adjusted summary scores with accompanying 95% confidence intervals (CI) or P values).

Risk of bias was assessed using the Cochrane Risk of Bias Assessment Tool: for Non-Randomized Studies of Interventions (ACROBAT-NSRI). This tool considers the risk of bias either within, or due to, the following seven areas: Confounding, selection of participants into the study, measurement of interventions, departures from intended interventions, missing data, measurement of outcomes and selection of the reported result, resulting in an overall risk of bias assessment (Fig. 1). Each domain, and the resulting overall assessment, is judged as being at low, moderate, serious or critical risk of bias, or NI no information). The ACROBAT-NSRI tool requires review authors to prespecify the ‘critical confounders’ that should be controlled for in the included studies. Critical confounders were prespecified as age, injury severity (as measured by the ISS), mechanism of injury (ie, blunt or penetrating), time from injury to hospital and existence of a trauma team at receiving hospital.

2.4 Data synthesis

To synthesize the data, studies were described in tables and text, with results grouped by outcome. Meta-analysis, and formal assessment of publication bias via funnel plots was initially planned, but subsequently precluded due the heterogeneity of studies and paucity of consistent outcome data. In the setting of significant clinical and methodological heterogeneity among included studies, a narrative synthesis was conducted.

The GRADE approach for evidence synthesis was used to undertake a structured assessment of evidence quality, allowing for the results to be interpreted in light of the quality of the evidence underpinning them. The quality of the evidence for each outcome was considered according to the eight GRADE criteria, which generates an overall rating of high, moderate, low or very low quality. According to the GRADE approach, outcomes with non-RCT evidence start as low quality due to the inherent limitations in observational study designs, and can then be upgraded (but are more likely downgraded) on a number of factors.

2.5 Differences between the protocol and review methods

We made three post hoc changes to the methods, while undertaking the review. We excluded non-English language studies due to a lack of resources within the author team. We refined the inclusion criteria...
related to the population, to include only studies with greater than 80% major trauma patients for reasons of applicability, after assessing studies for inclusion with mixed populations in which trauma patients made only a very small percentage. Finally, we replaced the planned risk of bias tool with the newly developed ACROBAT-NRSI tool, as it takes into account the latest methodological research about risk of bias in observational studies of intervention effectiveness.13

3 | RESULTS

3.1 | Search results

A total of 10,083 deduplicated citations were identified through database searches and additional sources. After screening on title and abstract, 9033 citations were excluded and 1050 were obtained in full text. After full-text screening, a further 1047 citations were excluded. The main reasons for exclusion included ineligible study design (usually no comparison group), or where the study did not pertain to prehospital notification systems specifically (Fig. 1). Key studies that came close to being included (usually meeting all bar one of the inclusion criteria) are described in the excluded studies table (Appendix 3).

3.2 | Description of included studies

Three studies7,15,16 including a total of 72,423 major trauma patients, were included in the review (see Table 1). The vast majority of participants (n = 72,073) were included in a single study.7 Two studies7,16 used a retrospective cohort design (analyzing registry data) and one used a mix of both prospectively and retrospectively collected data (comparing the time periods immediately before and after the implementation of an enhanced trauma system).15

All studies were conducted in hospitals in high-income countries, with one study collecting data from 59 hospitals over a five-year period in Quebec, Canada,7 whereas the two smaller studies collected data from single hospitals in the UK15 and Norway.16 Of note is that the Canadian7 and Norwegian16 studies examined the effect of prehospital notification within the context of an advanced trauma system (ie, where the comparison group also received advanced trauma care,
<table>
<thead>
<tr>
<th>Study</th>
<th>Author, year</th>
<th>Design</th>
<th>Data source/ years</th>
<th>Setting</th>
<th>No. of participants (n)</th>
<th>Gender and age (% male; M (SD))</th>
<th>Injury severity/mekanism (ISS, % blunt)</th>
<th>Method of PHN</th>
<th>Comparison group</th>
<th>Prehospital Notification</th>
<th>Outcome measured (Outcome; time)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lieberman (2005)</td>
<td>Retro. Cohort</td>
<td>Trauma registry (1997–2002)</td>
<td>Canada (HIC)</td>
<td>59 hospitals Trauma service: Y</td>
<td>72,073 (total only)</td>
<td>Male: 57% Age (&lt;65 yrs): 57% (total only)</td>
<td>ISS: M 11.9 (SD 9.7) (total only) Blunt: 96% (total only)</td>
<td>NR</td>
<td>No PHN (but other ATS components)</td>
<td>-Mortality (discharge) -Hospital LoS–ICU LoS</td>
<td></td>
</tr>
</tbody>
</table>

*The hospital implemented prehospital notification as part of a number of improvements in the management of trauma patients at the hospital in 1989. Postimplementation data (ie, the prehospital notification group) was collected prospectively in 1989 from patients admitted in that year. Pre-implementation data (ie, the comparison group) was collected retrospectively, using data from patients admitted in 1987.

*Median ISS not reported by study authors, data presented as it is reported in the paper.

*Researchers retrospectively analyzed registry data to compare the outcomes for patients who did and did not receive prehospital notification.

*Intervention (PHN) and control group (no PHN) numbers add up to 268, not 269 (as reported in the study).

Abbreviations: % = percentage, ATS = Advanced trauma system, HIC = high-income country, ICU = Intensive Care Unit, ISS = Injury Severity Score, LoS = length of stay, M = mean, n = number, No. = number, NR = not reported, PHN = Prehospital notification, pros. = prospective, retro. = retrospective, SD = standard deviation, TTA = trauma team activation, UK = United Kingdom, Y = yes, yrs = years.
### TABLE 2
Critical confounders, domain-level and overall risk of bias assessments of included studies using the ACROBAT-NRSI tool for cohort studies

<table>
<thead>
<tr>
<th>Study</th>
<th>Critical confounders⁴</th>
<th>Measured</th>
<th>Confounding</th>
<th>Participant selection</th>
<th>Intervention measurement</th>
<th>Intervention departures</th>
<th>Missing data</th>
<th>Outcome measurement</th>
<th>Reported results selection</th>
<th>Overall</th>
</tr>
</thead>
<tbody>
<tr>
<td>Maimaris (1990)</td>
<td>Age, ISS</td>
<td>- Age</td>
<td>Serious</td>
<td>Low</td>
<td>Mod.</td>
<td>Mod.</td>
<td>Low</td>
<td>Low</td>
<td>Low</td>
<td>Serious</td>
</tr>
<tr>
<td></td>
<td></td>
<td>- ISS</td>
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<tr>
<td></td>
<td></td>
<td>- Mechanism of injury</td>
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<tr>
<td></td>
<td></td>
<td>- Time from injury to hospital</td>
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<tr>
<td></td>
<td></td>
<td>- Existence of trauma team</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Lieberman (2005)</td>
<td>Age, mechanism of injury, existence of trauma team, ISS</td>
<td>- Age</td>
<td>Serious</td>
<td>Low</td>
<td>Mod.</td>
<td>Low</td>
<td>NI</td>
<td>Low</td>
<td>Mod.</td>
<td>Serious</td>
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<td></td>
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<td>- Mechanism of injury</td>
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<td></td>
<td>- Existence of trauma team</td>
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<tr>
<td></td>
<td></td>
<td>- ISS</td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lillebo (2012)</td>
<td>Time from injury to hospital, existence of trauma team, age, mechanism of injury - ISS</td>
<td>- Age</td>
<td>Serious</td>
<td>Low</td>
<td>Low</td>
<td>Low</td>
<td>Low</td>
<td>Mod.</td>
<td>Low</td>
<td>Serious</td>
</tr>
<tr>
<td></td>
<td></td>
<td>- Mechanism of injury - ISS</td>
<td></td>
<td></td>
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</table>

⁴As determined by the review authors. Some studies measured additional confounders that we did not judge as critical to the risk of bias assessment.

Abbreviations: ISS = Injury Severity Score, Mod. = moderate, NI = no information.
but without the prehospital notification) whereas in the UK study\textsuperscript{15} a range of improvements were made to trauma care concurrent with the introduction of prehospital notification (ie, care for the comparison group differed in more ways than just the absence of prehospital notification). Demographic details were scant, but approximately 60\% of participants were aged less than 60 years, and male. Most outcomes of interest in this review were measured by one or more study, with the exception of early mortality and hospital reception and trauma team presence (or equivalent) on arrival. Only one study\textsuperscript{7} identified their funding source (provincial government).

### 3.3 Prehospital notification approaches used

With the exception of one study\textsuperscript{16} very little information was reported about the personnel who made and received the notification, how it was delivered to staff at the hospital, nor the preparations made as a result. As such, very little can be concluded from these studies about the range of prehospital notification approaches used in high-income countries, and there is no information about the approaches used in low and middle-income settings.

In Lillebo,\textsuperscript{16} a one-tiered trauma team, the regional emergency communication center sent two alerts via pagers to all members of the trauma team—an initial alert when prehospital staff had assessed the patient at the scene and a second alert approximately 10 minutes prior to arrival. The second page was designed to mandate immediate attendance to the resuscitation room.

### 3.4 Risk of bias

All studies were rated as being at ‘serious’ risk of bias overall (Table 2). Testing interventions with observational study designs requires critical confounders to be measured and accounted for in the design or analysis.\textsuperscript{13} While each study measured and accounted for some confounders, none accounted for all the critical confounders prespecified by the review team. In addition, two studies\textsuperscript{7,15} were identified as being at moderate risk of bias for limitations relating to the measurement of the intervention, as it was not clear that they could accurately determine whether participants truly did or not receive prehospital notification.

### 3.5 Effect of prehospital notification

#### 3.5.1 Overall mortality (<30 days)

Two studies examined the association between prehospital notification and mortality (See Table 3). Liberman\textsuperscript{7} reported that prehospital notification was associated with reduced mortality (adjusted odds ratio (OR) 0.61, 95\% CI 0.39 to 0.94; 72,073 participants). Maimaris\textsuperscript{15} also concluded that there was an improvement in mortality after the introduction of prehospital notification (from 32.4\% in 1987 to 22.7\%, 1989). A re-analysis of Maimaris\textsuperscript{15} data showed a very similar, but not statistically significant, reduction in mortality between studies (OR 0.61, 95\% CI: 0.23 to 1.64, 81 participants). Results from these studies were not pooled as different types of study designs were involved, and the particularly weak nature of a repeated measures, post cross-sectional design to assign causality as used by Maimaris.\textsuperscript{15} While these effect estimates are consistent with improved mortality, they must be interpreted within the context of the quality of evidence for this outcome, which was judged as very low (see Table 3 for reasons). This means the effect estimate is very uncertain and precludes firm conclusions about the effect of prehospital notification on mortality.

#### 3.5.2 Time to intervention

One study\textsuperscript{16} measured time to intervention (in this case, time to chest x-ray (CXR)) in 247 major trauma patients (see Table 3). Using their dichotomized raw outcome data (reported as ≤5 minutes or >5 minutes to CXR) an effect measure of RR 0.91 (95\% CI 0.71 to 1.17) was calculated. Again, the quality of the evidence for this outcome was judged as very low (see Table 3 for reasons), which precludes firm conclusions about the effect of prehospital notification on time to intervention.

#### 3.5.3 Hospital length of stay

Two studies\textsuperscript{7,15} measured hospital length of stay (See Table 3). Both found hospital length of stay to be similar between the intervention and control groups (ie, 29 days versus 31 days; 81 participants (Maimaris\textsuperscript{15}); adjusted mean differences 0.53 fewer days in hospital (95\% CI 3.84 days to 2.77 days; 72,073 participants) with prehospital notification (Liberman\textsuperscript{7}) and concluded there was no association with prehospital notification. Despite the fact that both studies conclude there was no association between prehospital notification and hospital length of stay, no firm conclusions could be drawn owing to the very low quality of the evidence (see Table 3).

#### 3.5.4 Intensive care length of stay

Two studies\textsuperscript{7,15} measured intensive care length of stay, with both concluding there was no association between prehospital notification and ICU length of stay. Maimaris\textsuperscript{15} reported there was ‘no significant difference’ between length of stay in the 81 participants in the intervention (mean 9 days, range 1 to 62 days) and control groups (mean 27 days, 1 to 116 days) and Liberman\textsuperscript{7} reported an adjusted mean difference of 3.69 fewer days in ICU (95\% CI 9.69 to 2.31 days; 72,073 participants) with prehospital notification. However, the quality of this evidence was judged to be very low (see Table 3) meaning uncertainty about this estimate and again precluding firm conclusions about the effect of prehospital notification on intensive care length of stay.

### 4 DISCUSSION

Two cohort studies and one repeated measures cross-sectional study including 72,423 major trauma patients were included in the review. All were conducted in high-income countries in hospitals with an established trauma service, with two studies undertaking retrospective analysis of registry data. Scant information was provided about the nature of the notification systems, such that very little can be concluded about the nature of prehospital notification systems.
TABLE 3  Outcome-level results and evidence quality

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Evidence quality (GRADE)</th>
<th>Study</th>
<th>Outcome measure/ timing</th>
<th>N =</th>
<th>Summary measures (where reported, or calculable by review team)</th>
<th>Study conclusions</th>
</tr>
</thead>
<tbody>
<tr>
<td>Overall mortality (&lt;30 days)</td>
<td>Very low¹</td>
<td>Maimaris (1990)</td>
<td>Mortality (time point not specified)</td>
<td>81</td>
<td>OR 0.61¹ (95% CI 0.23 to 1.64)</td>
<td>Mortality improved; 32.4% (no PHN) to 22.7% (PHN), Z statistic fell from +2.3 to +1.46 (PHN).</td>
</tr>
<tr>
<td>Early mortality (&lt;24 hours)</td>
<td>No study measured this outcome</td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Hospital reception and trauma team presence on arrival</td>
<td>No study measured this outcome</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Time to intervention</td>
<td>Very low²</td>
<td>Lillebo (2012)</td>
<td>Time to CXR (≤5 vs. &gt;5 mins)</td>
<td>247</td>
<td>RR 0.91² (95% CI 0.71 to 1.17)</td>
<td>'No effect of preactivation notification on time to chest X-ray (P = 0.474)'</td>
</tr>
<tr>
<td>Hospital length of stay</td>
<td>Very low³</td>
<td>Maimaris (1990)</td>
<td>Hospital LoS (days)</td>
<td>81</td>
<td>PHN: M 29 days (range: 1 to 100 days) No PHN: M 31 days (range: 2 to 126 days)</td>
<td>'No significant difference' (summary scores not calculable)</td>
</tr>
<tr>
<td>ICU length of stay</td>
<td>Very low³</td>
<td>Maimaris (2005)</td>
<td>ICU LoS (days)</td>
<td>81</td>
<td>PHN: M 9 days (range: 1 to 62 days) No PHN: M 27 days (range: 1 to 116 days)</td>
<td>'No significant difference' (summary scores not calculable)</td>
</tr>
<tr>
<td></td>
<td>Liberman (2005)</td>
<td>Hospital LoS (days)</td>
<td></td>
<td>72,073</td>
<td>Adj. MD –0.53 days (95% CI: 3.84 to 2.77)¹,²</td>
<td>Not associated with prehospital notification</td>
</tr>
<tr>
<td></td>
<td>Liberman (2005)</td>
<td>ICU LoS (days)</td>
<td></td>
<td>72,073</td>
<td>Adj. MD –3.69 days (95% CI: 9.69 to 2.31)¹,²</td>
<td>Not associated with prehospital notification</td>
</tr>
</tbody>
</table>

¹GRADE rating started at low (observational studies), further downgraded to both studies being judged as being at ‘serious’ risk of bias and high likelihood that unpublished data exists on this topic, given the number of trauma registries that exist internationally but the paucity of published studies.²Calculated by review team in RevMan software using raw data supplied in the paper.³Calculated by study authors using linear regression (adjusted for: age, gender, Glasgow Coma Score, Injury Severity Score, Revised Trauma Score, and Abbreviated Injury Scale category).⁴GRADE rating started at low (observational study), further downgraded as the study was rated as being at ‘serious’ risk of bias, and due to the imprecision of the estimate (due to it being a small, single study), and the likelihood that unpublished data exists on this topic.⁵GRADE rating started at low (observational studies), further downgraded due to the risk of bias ratings being judged as ‘serious,’ concerns with imprecision (the confidence intervals include a possible increase and decrease in length of stay) and the likelihood that unpublished data exists on this topic.⁶Confidence interval (CI) calculated by the review team in RevMan software using the standard error (SE) supplied in the paper, using the following formula: CI = MD ± 1.96 x SE. Abbreviations: Adj. = adjusted, CI = confidence interval, CXR = chest x-ray, ED = Emergency department, ICU = intensive care unit, LoS = length of stay, M = mean, MD = mean difference, mins = minutes, N = number of participants, PHN = prehospital notification, RR = relative risk, SD = standard deviation, SE = standard error, unadj. = unadjusted. Evidence quality interpretation: HIGH = Further research is very unlikely to change our confidence in the estimate of effect or accuracy, MODERATE = Further research is likely to have an important impact on our confidence in the estimate of effect or accuracy and may change the estimate, LOW = Further research is very likely to have an important impact on our confidence in the estimate of effect or accuracy and is likely to change the estimate, VERY LOW = Any estimate of effect or accuracy is very uncertain.

in use. Of the two studies that measured the primary review outcome of overall mortality, one found that prehospital notification was associated with a statistically significant reduction in mortality (Adjusted OR 0.61, 95% CI: 0.39 to 0.94, 72,073 participants) and the other, a non-significant reduction (OR 0.61, 95% CI: 0.23 to 1.64, 81 participants). One study measured time to intervention and two studies measured hospital and intensive care length of stay; none demonstrated convincing evidence of an effect. No study measured early mortality or hospital reception and trauma team presence (or equivalent) on arrival. Considerable uncertainty exists about these estimates as the quality of the evidence for all outcomes judged as very low. Rather than being a critique on the studies themselves, this reflects the fact that the few studies that exist on this topic are observational and had some risk of bias limitations, and two of them had very few participants (with correspondingly wide CIs). Additionally, it is likely that unpublished data exists on this topic, given the number of trauma registries internationally. Many studies assessed for inclusion in this review were feasibility studies, focused more on technical aspects of
development and implementation of prehospital notification, rather than evaluation of clinical outcome.\textsuperscript{18-20} The uncertainty is such that further studies could substantially alter the review conclusions.\textsuperscript{14}

Searches for this review were challenging, given the variability in subject headings and keywords used to describe this complex intervention. So while it is possible that studies were missed, the comprehensive search methods used make it extremely unlikely that experimental or robust observational studies that would have changed the conclusions of the review were missed. There were a number of studies excluded due to mixed populations (including less than 80% major trauma patients) or they didn’t include the pre-specified outcomes of interest. Including these studies may have strengthened the review conclusions and/or the quality of the evidence, but would have reduced the degree to which the findings applied to major trauma patients. Finally, excluding 46 studies due to non-English language might mean some potentially included studies were omitted.

Given the absence of included studies from low- and middle-income countries, the findings of this review may not be applicable to all trauma systems. In a recent systematic review of core concepts in the development of trauma systems in low- and middle-income countries,\textsuperscript{21} the authors found that trauma systems in place in high-income countries may not be transferable due to cost and resource limitations. This is particularly true for prehospital notification, where first responders and those providing transport to hospital are often commercial drivers, volunteers or bystanders.\textsuperscript{22,23}

Modern evidence-based resuscitation strategies lower mortality, making outcomes studies increasingly difficult to demonstrate significant improvements. Future studies may therefore be required to examine composite interventions. During the initial phases of trauma care, such interventions include not only prehospital notification, but also the immediate effect of such notification such as the availability of a resuscitation bay on patient arrival, assimilation of a trauma team and timely availability of investigative modalities and management of critical injuries.

5 | CONCLUSION

Improved outcomes from prehospital notification alone may be possible, but evidence is limited by its very low quality and analysis from high-income countries only. The suggestion of improved outcomes based on this single intervention should prompt more widespread analysis of prehospital notification of injured patients and include the effect of such notification on trauma reception and resuscitation on arrival to hospital.

CONFLICT OF INTEREST

None.

REFERENCES


SUPPORTING INFORMATION

Additional Supporting Information may be found online in the supporting information tab for this article.