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Original article

Clinical impact of a prehospital trauma shock bundle of care in South Africa

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ABSTRACT

Introduction: Patients experiencing traumatic shock are at a higher risk for death and complications. We previously designed a bundle of emergency medical services traumatic shock care ("EMS-TruShoC") for prehospital providers in resource-limited settings. We assess how EMS-TruShoC changes clinical outcomes of critically injured prehospital patients.

Methods: This is a quasi-experimental educational implementation of a simplified bundle of care using a pre-post design with a control group. The intervention was delivered to EMS providers in Western Cape, South Africa. Delta shock index (heart rate divided by systolic blood pressure, reported as change from the scene to facility arrival) from the 13 months preceding intervention were compared to the 13 months post-implementation. A difference-in-differences analysis examined the difference in mean shock index change between the groups.

Results: Data were collected from 198 providers who treated 770 severe trauma patients. The patient groups had similar demographic and clinical characteristics at baseline. Over all time-points, both groups had an increase in mean delta shock index (worsening shock), with the largest difference occurring 4-months post-implementation (0.047 change in control arm, 0.004 change in intervention arm; −0.043 difference-in-differences, $P = 0.27$). In pre-specified subgroup analyses, there was a statistically significant improvement in delta shock index in the intervention arm in patients with penetrating trauma cared for by basic providers immediately post-implementation (−0.372 difference-in-differences, $P = 0.02$).

Discussion: Overall, there was no significant difference in delta shock index between the EMS-TruShoC intervention versus control groups. However, significant improvement in shock index in one subgroup suggests the intervention may be more likely to benefit penetrating trauma patients and basic providers.

African relevance

- In resource-limited settings, simplified bundles of care that promote performance of basic evidence-based interventions are needed.
- Prehospital recognition and management of shock is critical in Africa, where a paucity of trauma centres and under-resourced hospitals contribute to delays in care and adverse outcomes.
- Patients transported with severe shock or penetrating injuries had modest, clinically-relevant improvements in shock indices if the EMS provider received weekly in-ambulance training on traumatic shock care within 4 months of the clinical encounter.

Introduction

Traumatic injuries are the leading cause of mortality in persons

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under 45-years worldwide, and trauma causes significant long-term morbidity [1–5]. Further, injured people in low- and middle-income countries (LMICs) experience disproportionately worse outcomes compared to those in high-income countries [6–8]. South Africa, for example, has an age-standardized mortality rate from interpersonal violence that is seven-times higher than the global mean rate [7,9,10].

Among trauma patients, haemorrhage is the most common reason for death, and shock contributes to organ failure [11–14]. Yet, early death from traumatic shock is preventable through early and quality resuscitation, beginning with prehospital providers, and rapid transportation to definitive surgical care [15]. The need for prehospital recognition and management of shock is critical in LMICs where few trauma centres and under-resourced hospitals contribute to delays in care and adverse outcomes [16,17].

Management of traumatic shock is an ideal target for intervention in the prehospital setting because shock is often identifiable, interventions are mostly basic and can be life-saving, and providers' skills can be quickly improved with effective training [11,12,18]. In 2016, we pilot tested an evidence-based, expert-informed, essential bundle of traumatic shock care (Emergency Medical Services Traumatic Shock Care – EMS-TruShoC) for prehospital care in resource-limited settings [18]. The core interventions within the EMS-TruShoC bundle of care include: early haemorrhage control (if applicable), maintaining short scene times (preferably, <10 min), direct transport to a trauma centre, establish a large bore intravenous (IV) catheter, deliver of oxygen (see Appendix A for EMS-TruShoC algorithm and bundle of care). Prior pilot testing of EMS-TruShoC in South Africa demonstrated high implementation effectiveness and improved providers' knowledge, attitudes, and skills in traumatic shock care [18].

The objective of this study was to assess how implementation of EMS-TruShoC bundled care among EMS providers influences clinical outcomes of critically injured patients in shock in a resource-limited, high-trauma international setting. We expected to find a larger improvement in patients' shock indices, measured between the scene and facility arrival, in the intervention cohort compared to the control cohort.

Methods

We performed a prehospital, quasi-experimental, pragmatic study using a pre-post design with a contemporaneous control group. The study settings were ambulance bases located in Khayelitsha and Mitchells Plain, two densely populated, high-trauma suburbs, within Cape Town, in the Western Cape Province of South Africa. These communities experience a high incidence of inter-personal and non-intentional trauma, and have among the world's highest burden of morbidity and mortality from trauma [7,10,19].

The organizational setting was a state-wide government-operated EMS system – Western Cape Government (WCG) Department of Health EMS [18,20]. Study-eligible providers were 120 clinically-active EMS providers at each of the two participating bases with national qualifications of basic-, intermediate-, or advanced-life support (BLS, ILS, ALS, respectively). At the time of this study, foundational education for WCG EMS providers from across the Western Cape Province included a 6-week certificate courses for BLS, a 12-week course for ILS, and a 4-year (degree-earning) training for ALS providers [21]. Khayelitsha was selected as the intervention site, primarily due to the administrative readiness and capacity at the ambulance station to host the educational intervention, as determined by study investigators who were WCG EMS staff.

The intervention was EMS-TruShoC bundled care, which was pragmatically implemented by trained peers (paramedics, called "facilitators") at Khayelitsha using a low-dose (15-minutes), high-frequency (once-weekly) structured program taught in the back of ambulances at the start of shifts (see Appendix B for learning objectives and Appendix C for full training materials). This training program has been previously described and proven to have high implementation effectiveness and strong educational outcomes [18]. Implementation at Khayelitsha occurred from August to November 2018. The Mitchells Plain ambulance base served as a concurrent control arm, where EMS providers, patient population, and trauma caseloads were similar to Khayelitsha. There were no implementation activities at Mitchell Plain except usual classroom-based training with similar learning objectives as EMS-TruShoC. There was a 1-month period in December 2018, during which no training or clinical outcomes data were collected. Pre- and post-implementation data were collected for the 13 consecutive months preceding (i.e., August 2017 through August 2018) and following (i.e., January 2019 through January 2020) implementation, respectively.

Data were collected from EMS providers and patients at both sites using a previously validated standardized chart review and abstraction methodology [22]. Providers' demographics, qualifications, years of practice, and number of training sessions attended were collected. EMS clinical outcome data was collected for any patient who was ≥18 years old, traumatically injured excluding burns, electrocutions and isolated severe traumatic brain injuries, received care by a provider at either the intervention or control site, alive or attempted resuscitation upon ambulance arrival, and had at least two sets of vital signs documented (which was critical for calculating the primary outcome). Clinical data for each patient included mechanism of injury, vital signs, time from scene to hospital and prehospital interventions and was limited to data available from EMS clinical charts.

The primary outcome was delta shock index (i.e., the change in a patient's shock index at the scene versus their shock index upon hospital arrival) in the intervention group compared to the control group. Shock index is heart rate divided by systolic blood pressure, and is validated to predict trauma outcomes, including the early need for blood products and mortality [23,24]. A shock index of <0.7 is normal, between 0.7 to <1.0 is intermediate, and ≥1.0 is considered high [25,26]. In this study, a negative delta shock index represents improved shock upon facility arrival. The target effect of the study was the difference between the intervention and control groups in mean change of delta shock index from pre- to post-implementation (i.e., difference-in- differences) [27]. A more negative difference-in-differences indicates that the intervention is performing better than the control.

The power calculation was based on an assumed sample size of 600 patients (300 per intervention and control arms each, and 150 pre- and 150 post-implementation) collected over a two-year period. Based on prior data, we assumed a mean delta shock index of −0.05 in the control and pre-implementation group and a standard deviation of 0.025 for both treatment groups in the pre- and post-implementation time periods [24,25,27]. With 90% power, we could detect an effect size (difference-in-differences) of −0.013 (corresponding to a standardized effect size of 0.53). Thus, assuming that there was no change in the mean delta shock index in the control group (−0.05 both pre- and post-implementation), we could detect a decrease in the mean delta shock index in the intervention group pre- vs. post-implementation from −0.05 to −0.063.

Comparisons between the intervention and control groups for both provider and patient characteristics, pre- and post-implementation, were performed using Wilcoxon, chi-squared, and *t*-tests, based on the type and distribution of the variable. The primary analysis was a difference-in-differences analysis to examine the difference between the control and intervention groups in changes in delta shock index over time [27]. This analysis was performed using a mixed effects model with a random effect for the provider to account for clustering of outcomes for patients cared for by the same (primary) provider. Due to lack of variability between providers, as suggested by an estimated random intercept variance closer to zero, a regression model assuming independence within providers was used. To estimate the difference-in-differences, an interaction between study period and group (Intervention/Control) was of primary interest. Study period for trauma cases was classified as pre-implementation, 0–4 months post-implementation, 5–8 months post-implementation, or 9–13 months post-implementation. We divided the

study period into intervals to study the change in intervention effect over time. All models also adjusted for the predictors: provider qualification (BLS, ILS, ALS), patient sex, injury mechanism (blunt or penetrating), cause of shock (i.e., haemorrhagic or other), patient age in years, initial shock index, and pre-arrival minutes (time from injury to ambulance arrival). All statistical analyses were conducted using SAS version 9.4 (SAS Institute Inc., Cary, N.C.).

Ethics approval was granted by the University of Cape Town Human Research Ethics Committee (ref 077/2018), the primary oversight ethics board, with a single-IRB reliance agreement with the Colorado Multiple Institutional Review Board. A waiver of informed consent for patients was approved, and EMS providers' written informed consent was obtained for all participating providers.

Results

Data were collected from a total of 198 of 240 eligible providers, who treated 770 trauma patients (Fig. 1). Each provider cared for a median of 3 (IQR: 1–4) traumatic shock patients during the study, and 150 (76%) of providers cared for fewer than 5 traumatic shock patients during the study. There were no significant demographic differences in EMS providers' age, sex, or years of experience between the cohorts of EMS providers in the pre-implementation period. There was a significant difference in EMS qualification between the pre-implementation cohorts, with the control group having a significantly higher proportion of BLS providers than the intervention group (Table 1).

There were no significant differences in pre- or post-implementation patient demographic and physiologic characteristics between the control and intervention cohorts with respect to age, sex, blunt versus penetrating injury mechanism, initial systolic blood pressure, and initial heart rate (Table 2a and 2b). In both pre- and post-implementation periods, there were similar proportions of patients with severe shock (i.e., shock index >1.0) and intermediate shock (i.e., shock index 0.7- < 1.0) in both the intervention and control groups. Providers spent a similar amount of time on scene 23-minutes (standard deviation 13–35) and delivered similar volumes of intravenous (IV) fluids in the intervention and control groups (500 mL; IQR, 200–500), although 73% of patients received no IV fluids.

Overall, both the control and the intervention groups had an increase in mean delta shock index (i.e., worsening shock) in the 4 months post-implementation compared to pre-implementation (Fig. 2 and Table 3); although the increase in mean delta shock index was smaller in the intervention group compared to the control group, the difference in the change between the two groups was not statistically significantly different (0.047 change in control arm, 0.004 change in intervention arm; −0.043 difference-in-differences, $P = 0.27$). There was no significant difference in change over time between the groups for any of the other time intervals (5–8 months: difference-in-differences 0.008, $P = 0.86$; 9–13 months: difference-in-differences −0.021, $P = 0.59$).

In pre-specified subgroup analyses, there was no statistically

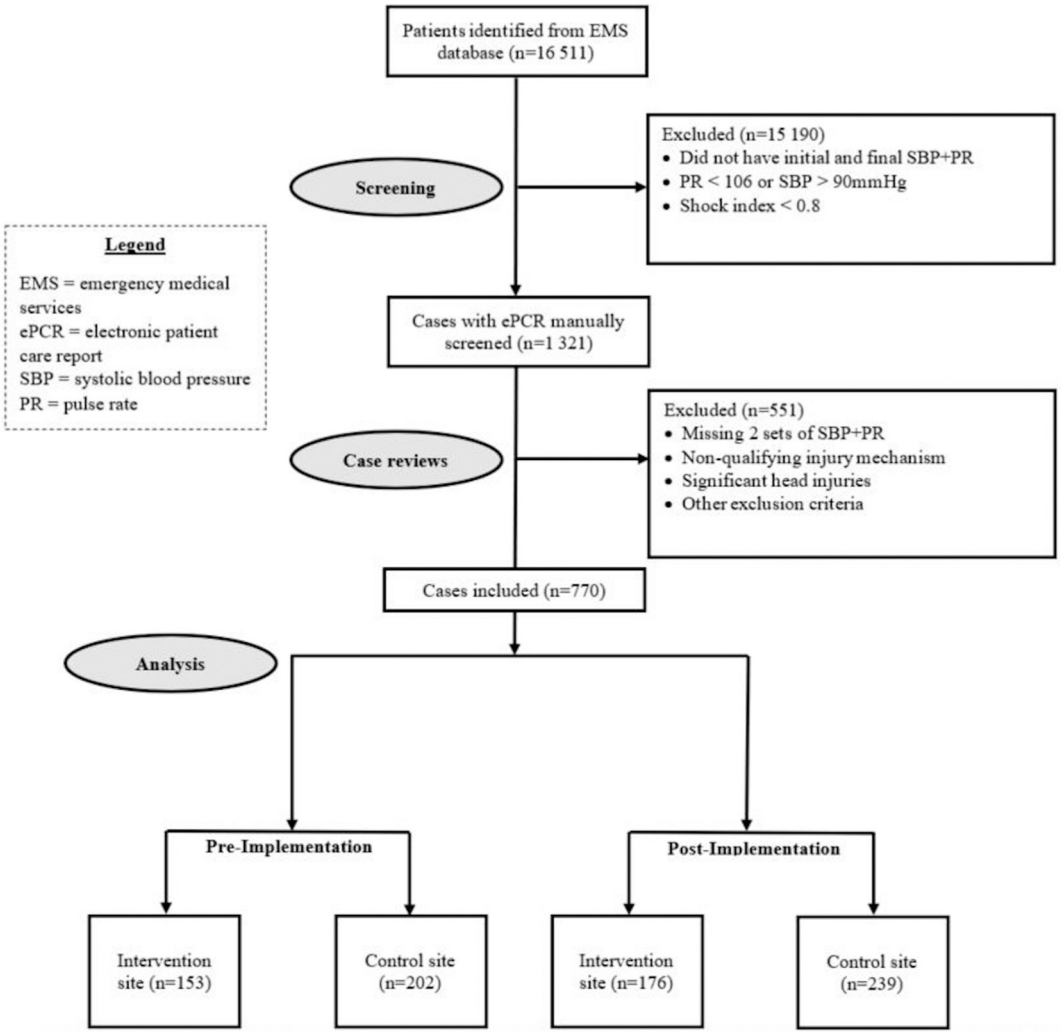

Fig. 1. Study flow diagram.

Table 1
Providers' demographics and characteristics.

Variable	Category	Overall (*N* = 198)	Study group		*P*-value
			Control (*N* = 105)	Intervention (*N* = 93)	
Provider Sex	Male	107 (54%)	60 (57%)	47 (51%)	0.35
	Female	91 (46%)	45 (43%)	46 (49%)	
Provider Qualification	BLS[b]	83 (42%)	57 (54%)	26 (28%)	<0.001
	ILS	83 (42%)	36 (34%)	47 (51%)	
	ALS	32 (16%)	12 (11%)	20 (22%)	
Mean (SD) age in years		37.2 (7.3)	37.6 (7.9)	36.6 (6.5)	0.38
Median (IQR) years of experience		8.0 (5.0–11.0)	8.0 (5.0–12.0)	8.0 (5.0–11.0)	0.56[a]

[a] Wilcoxon Test.
[b] The differing proportions of BLS providers are taken into account in the modelling procedures by adjusting for provider type as a fixed effect in all of the multivariable mode.

Table 2a
Patients' pre-intervention demographic and physiologic characteristics.

Pre-implementation (*n* = 355)					
Variable	Category	Overall (N = 355)	Control (*N* = 202)	Intervention (*N* = 153)	P-value
Median (IQR) patient age in years		30 (25–37)	30 (25–39)	30 (25–36)	0.34[a]
Patient sex	Female	84 (24%)	44 (22%)	40 (26%)	0.34
	Male	271 (76%)	158 (78%)	113 (74%)	
Primary injury mechanism	Blunt	166 (47%)	96 (48%)	70 (46%)	0.74
	Penetrating	189 (53%)	106 (52%)	83 (54%)	
Median (IQR) initial heart rate (BPM)		111 (102–118)	112 (104–118)	110 (98–119)	0.17[a]
Median (IQR) initial SBP (mm Hg)		112 (90–130)	114 (94–130)	110 (90–129)	0.12[a]
Median (IQR) Initial Shock Index		0.96 (0.85–1.10)	0.96 (0.85–1.11)	0.96 (0.87–1.09)	0.84[a]
Shock stage defined by initial Shock Index	High (≥1.0)	149 (42%)	87 (43%)	62 (41%)	0.18
	Intermediate (0.7- < 1.0)	189 (53%)	109 (54%)	80 (52%)	
	Normal (<0.7)	17 (5%)	6 (3%)	11 (7%)	
Median (IQR) change in Shock Index from initial to final		−0.05 (−0.19–0.02)	−0.04 (−0.16–0.01)	−0.06 (−0.23–0.02)	0.24[a]
Median (IQR) minutes from incident to scene arrival (*n* = 4, 1% missing)		16 (10−33)	17 (10–34)	15 (10−32)	0.93[a]
Median (IQR) minutes from scene arrival to scene departure		23 (13–35)	24 (12–36)	22 (14–32)	0.93[a]
Median (IQR) minutes from scene departure to hospital arrival		18 (10–27)	21 (12–29)	13 (9–22)	<0.0001[a]

SBP = systolic blood pressure.
[a] Wilcoxon Test.

Table 2b
Patients' post-intervention demographic and physiologic characteristics.

Post-implementation (*n* = 415)					
Variable	Category	Overall (N = 415)	Control (*N* = 239)	Intervention (*N* = 176)	P-value
Median (IQR) patient age in years		30 (24–36)	30 (24–36)	30 (25–37)	0.42[a]
Patient sex (n = 4, 1% missing)	Female	85 (21%)	53 (22%)	32 (18%)	0.35
	Male	326 (79%)	185 (78%)	141 (82%)	
Primary injury mechanism	Blunt	191 (46%)	109 (46%)	82 (47%)	0.84
	Penetrating	224 (54%)	103 (54%)	94 (53%)	
Median (IQR) initial heart rate (BPM)		111 (104–119)	111 (106–120)	110 (97–119)	0.06[a]
Median (IQR) initial SBP (mm Hg)		114 (91–130)	115 (100−130)	110 (90–129)	0.10[a]
Median (IQR) Initial Shock Index		0.96 (0.85–1.11)	0.95 (0.85–1.11)	0.97 (0.85–1.12)	0.96[a]
Shock stage defined by initial Shock Index	High (≥1.0)	176 (42%)	100 (42%)	76 (43%)	0.12
	Intermediate (0.7- < 1.0)	226 (54%)	135 (56%)	91 (52%)	
	Normal (<0.7)	13 (3%)	4 (2%)	9 (5%)	
Median (IQR) change in Shock Index from initial to final		−0.03 (−0.14–0.05)	−0.03 (−0.12–0.04)	−0.04 (−0.18–0.06)	0.53[a]
Median (IQR) minutes from incident to scene arrival (*n* = 7, 2% missing)		23 (13–47)	25 (15–51)	18 (12–41)	0.003[a]
Median (IQR) minutes from scene arrival to scene departure		18 (9–27)	17 (7–28)	19 (10–26)	0.25[a]
Median (IQR) minutes from scene departure to hospital arrival		15 (9–27)	16 (10–28)	14 (9–25)	0.43[a]

SBP = systolic blood pressure.
[a] Wilcoxon Test.

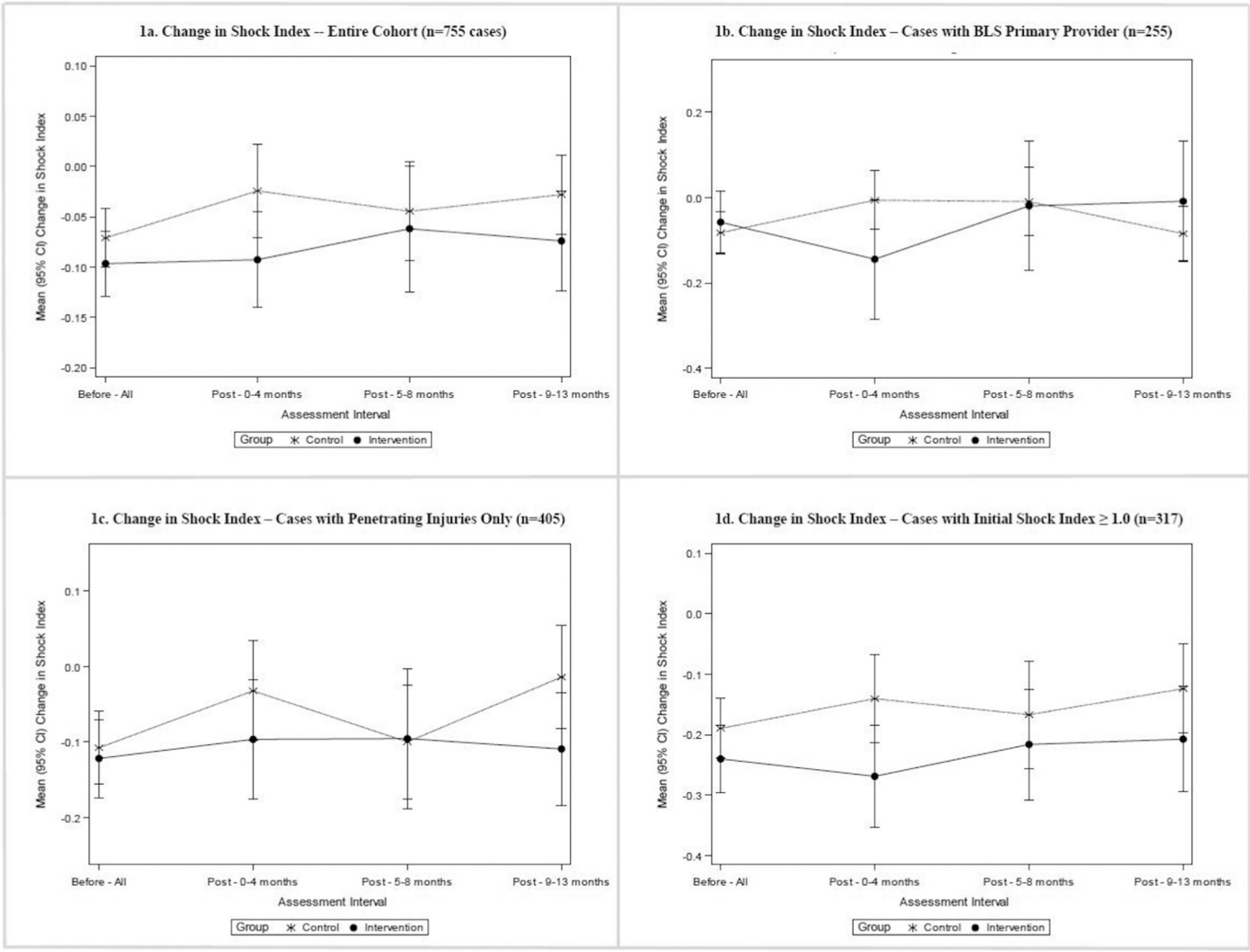

Fig. 2. Mean change in shock between EMS arrival at the scene of injury to hospital arrival by whole cohort (1a), cases with BLS providers (1b), penetrating injury (1c) and severe shock (1d). The more negative the change in shock index value is, the more improved the shock.

Table 3
Delta shock index by time interval and study group, for entire cohort ($N = 755$)[a].

	Control		Intervention			
Time Interval	Frequency	Estimated Delta SI (95% CI)	Frequency	Estimated Delta SI (95% CI)	Difference in Differences (95% CI) (Intervention-Control)	P-value
Before - All	200	−0.071 (−0.101, −0.042)	151	−0.097 (−0.129, −0.064)		
Post - 0-4 months	73	−0.024 (−0.070, 0.022)	69	−0.093 (−0.140, −0.045)	−0.043 (−0.119, 0.033)	0.27
Post - 5-8 months	62	−0.044 (−0.094, 0.005)	39	−0.062 (−0.124, 0.001)	0.008 (−0.080, 0.097)	0.86
Post - 9-13 months	98	−0.028 (−0.067, 0.011)	63	−0.074 (−0.124, −0.025)	−0.021 (−0.095, 0.054)	0.59

SI = shock index. A more negative delta SI represents more improved shock.
[a] 15 cases from the original sample of $N = 770$ were excluded from this analysis due to missing data.

significant difference between cohorts of patients based on the following individual characteristics: mechanism of injury (i.e., penetrating versus blunt); patients in severe shock at the scene (i.e., initial shock index ≥1.0); EMS provider qualification (i.e., BLS, ILS, BLS + ILS); mechanism of injury (i.e., penetrating versus blunt); or combinations of provider and patient factors (i.e., BLS +/− ILS with severe shock; BLS +/− ILS with penetrating injury).

In the following four pre-specified groups, we observed a clinically relevant, but not statistically significant, improvement of delta shock indices in the intervention arm compared to control arm: entire cohort, cases with BLS providers, penetrating injury cases, and severe initial shock (Table 4 and Fig. 2). Notably, the greatest clinical improvement in delta shock index in the intervention versus control arm consistently occurred at the immediate post-implementation (i.e., 0–4-month) period, and decreased with later time periods (i.e., 5–8 months and 9–13 months). Further, the largest (and most clinically significant)

Table 4
Difference-in-differences by time interval and study group.

Time interval	N Control	N Intervention	Difference in Differences (95% CI) (Intervention-Control)	P-value
Group=Overall[a]				
Before - All	200	151		
Post - 0-4 months	73	69	−0.043 (−0.119, 0.033)	0.27
Post - 5-8 months	62	39	0.008 (−0.080, 0.097)	0.86
Post - 9-13 months	98	63	−0.021 (−0.095, 0.054)	0.59
Cases with BLS Provider				
Before – All	81	37		
Post - 0-4 months	39	9	−0.163 (−0.336, 0.011)	0.07
Post - 5-8 months	28	8	−0.035 (−0.219, 0.149)	0.71
Post - 9-13 months	44	9	0.051 (−0.120, 0.222)	0.56
Cases with ILS provider				
Before – All	97	70		
Post - 0-4 months	20	39	0.023 (−0.091, 0.136)	0.70
Post - 5-8 months	15	23	0.090 (−0.042, 0.221)	0.18
Post - 9-13 months	32	36	−0.074 (−0.178, 0.029)	0.16
Cases with ALS Provider				
Before – All	22	44		
Post - 0-4 months	14	21	−0.009 (−0.158, 0.139)	0.90
Post - 5-8 months	19	8	−0.044 (−0.214, 0.127)	0.62
Post - 9-13 months	22	18	0.008 (−0.133, 0.149)	0.91
Penetrating injury only				
Before – All	104	81		
Post - 0-4 months	47	34	−0.050 (−0.165, 0.065)	0.39
Post - 5-8 months	35	23	0.019 (−0.112, 0.150)	0.78
Post - 9-13 months	45	36	−0.081 (−0.195, 0.033)	0.16
Initial Shock Index $\geq$ 1.0				
Before - All	86	60		
Post - 0-4 months	37	26	−0.078 (−0.208, 0.051)	0.24
Post - 5-8 months	24	23	0.001 (−0.141, 0.144)	0.99
Post - 9-13 months	36	25	−0.033 (−0.164, 0.097)	0.62
Penetrating Injury with BLS Providers				
Before - All	44	23		
Post - 0-4 months	26	3	−0.372 (−0.674, −0.070)	0.02
Post - 5-8 months	18	3	0.029 (−0.283, 0.341)	0.86
Post - 9-13 months	20	7	−0.015 (−0.247, 0.218)	0.90

[a] 15 cases from the original cohort of N = 770 were excluded from this analysis due to missing data.

relative improvement of delta shock index in the intervention arm occurred in the subgroup of penetrating trauma cases cared for by BLS providers in the 0–4 month post-implementation phase (−0.163 difference-in-differences, $P = 0.07$) (Table 4).

Discussion

To our knowledge, this is the first prehospital traumatic shock clinical study conducted in a low- or middle-income country. We implemented a simplified bundle of traumatic shock care, EMS-TruShoC, among 240 EMS providers in South Africa and assessed 770 patient's delta shock index at the scene versus upon hospital arrival, and compared the pre- versus post-implementation delta shock index to a control arm. Overall, there was no statistically significant difference between arms. In pre-planned exploratory analyses, we did, however, observe clinically relevant and statistically significant improvements in shock index in specific EMS-TruShoC intervention subgroups consisting of patients with severe initial shock, cases with BLS providers, and penetrating injuries, and we noted consistently superior improvements in shock indices in the immediate post-implementation phase (i.e., 0–4 months).

There is plausibility to support the four subgroups in which we measured the most significant improvements in shock index in our intervention cohort. A-priori, based on opinion and experience, we hypothesized that BLS providers were likely to benefit most from our bundle of care intervention, compared to ILS and ALS providers, due to limited baseline BLS provider training in recognizing and managing haemorrhagic shock commensurate with their narrow training and scope of practice [21]. Next, it is widely reported that penetrating trauma is more likely than blunt trauma to cause haemorrhage, promulgate shock and increase mortality [28]. Coupled with the fact that penetrating injury is more amenable to EMS management compared to blunt trauma, it is not surprising that EMS-TruShoC improved shock physiology in the penetrating trauma subgroup [28]. Similarly, patients with severe initial shock (i.e., those who had the most deranged systolic blood pressure and/or heart rate at the scene) experienced physiologic improvements, which is also the expected effect of the bundle of care. Last, we noted a consistent trend across all subgroups that delta shock indices were more improved in the intervention cohort during the immediate post-implementation period compared to later periods. This may be explained by decay in EMS-TruShoC knowledge, attitudes, and skills with advancing time, which has been well-described in prior emergency care literature [29]. As an aggregated effect, we noted that BLS providers who cared for penetrating trauma cases in the immediate post-implementation phase experienced the largest improved shock indices (median improvement of 0.37) which was statistically significant.

Notwithstanding these clinically modest improvements in selected subgroups, most findings in this study did not reach statistical nor clinical significance which warrant further exploration via a contextual understanding of our clinical bundle, the sensitivity of shock index, prehospital resources and provider capabilities.

First, three core components of our EMS-TruShoC shock bundle may confer no immediate prehospital physiologic advantage, namely: large IV catheter, scene time <10 min, and transport to trauma centre. The expert panel purposefully included large bore IV catheter insertion in the bundle, in lieu of prescribing an IV fluid regimen, as both a patient safety measure and to help enhance implementation feasibility across diverse EMS systems [30]. Additional studies have demonstrated that short scene times and rapid transport to trauma centres have been strongly correlated with improved survival in severe trauma and are considered ‘best practices’, but neither core component directly influences prehospital patient physiology [28]. However, delivery of oxygen and haemorrhage control may be more likely to directly improve prehospital shock index by dampening tachycardia and hypotension [23,25].

Second, despite several advantages, delta shock index may be limited in its ability to detect early and subtle physiologic changes in haemorrhagic shock. Shock index outperforms traditional vital signs in predicting adverse trauma outcomes; and shock index is non-invasive and more practical to collect than laboratory markers, such as lactic acid [23–25,31]. Yet, the shock index area under the receiver-operated curve (AUROC) is modest (0.63 to 0.68) for predicting 48-hour mortality in undifferentiated trauma patients [23,31,32]. Further, despite being

intuitive to interpret, there is sparse data regarding the utility of the delta value of shock index. The median delta shock index in our study ranged between 0.03 and 0.05 (post- and pre-implementation, respectively) which may be too small of a physiologic change compared to the discriminative ability of delta shock index reported by Cannon et al. [25].

Third, it is conceivable that prehospital haemorrhage control resources and variability in provider care (i.e., factors beyond our control) may have influenced our results and prevented us from measuring a difference in physiology between cohorts. It is also possible that EMS providers implemented our bundle variably, as is typical in prehospital practice [33].

Overall, this study contributes valuable preliminary evidence to the under-researched field of prehospital resuscitation in trauma, especially in resource-limited settings globally. All prior prehospital traumatic shock studies published in peer-reviewed journals were conducted in high-income countries, mostly in North America and Europe, [34–39] and mostly published in the past decade with a predominant focus on the effect of fluid resuscitation (often, blood products or crystalloids) on patient outcomes e.g., PAMPer, PROMMTT, PROPPR, and COMBAT trials [34–36,38,39]. However, in resource-limited settings, simplified bundles of care that promote performance of basic evidence-based interventions are needed.

This study was limited to analysis of data available in standard prehospital documentation, therefore detailed information about the final injuries necessary to calculate ISS or AIS were not available, and SI was selected as the best surrogate for injury severity. Additionally, the final hospital outcome could not be assessed. A primary outcome of delta SI was selected due to the more consistently available vital signs throughout the prehospital course and correlation with outcomes in other trauma studies. In our study setting providers work in pairs, frequently changing work partners; it would not have been possible to randomly assign providers at a given base to the intervention without contamination and patients receiving a mix of intervention and control providers throughout the course of the study. Therefore, a quasi-experimental study design was used which allows for assessment of the impact of the intervention but not causality. Consequently, data collectors could not be blinded to intervention.

In conclusion, educationally implementing a prehospital bundle of care, EMS-TruShoC, for treatment of patients with traumatic haemorrhagic shock did not result in a statistically significant improvement in shock indices from scene to hospital arrival. However, we observed modest clinically-relevant improvements in shock index in patients receiving care from EMS-TruShoC-trained BLS providers in the first four months after the intervention was implemented. Patients with initial severe shock and those with penetrating injuries, in the EMS-TruShoC arm, also experienced small clinical improvement in their shock index following implementation of the intervention. Additional work is needed to identify which components of the bundle of care had the most impact on shock index and to identify critical prehospital interventions associated with hospital morbidity and mortality.

Dissemination of results

Results from this study were disseminated via email reports and in-person presentations to the various leaders of the relevant units that contributed to the data collection.

Authorship contribution statement

Authors contributed as follow to the conception or design of the work; the acquisition, analysis, or interpretation of data for the work; and drafting the work or revising it critically for important intellectual content: NM contributed 35%; JD contributed 20%; BBes contributed 10%; BBea, KS, BvS, FM, and CC contributed 4% each; AG, LW, SS, JM, SdV, and VB contributed 2.5% each. All authors approved the version to be published and agreed to be accountable for all aspects of the work.

Declaration of competing interest

Opinions, interpretations, conclusions, and recommendations are those of the authors and are not necessarily endorsed by, or the official opinions of, the NIH, the U.S. Department of Defense, the Western Cape Government Department of Health, or the University of Colorado.

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Appendices. Supplementary data

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