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Outcomes after a Digital Behavior Change Intervention to Improve Trauma Triage: An Analysis of Medicare Claims



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ABSTRACT

Background: Under-triage in trauma remains prevalent, in part because of decisions made by physicians at non-trauma centers. We developed two digital behavior change interventions to recalibrate physician heuristics (pattern recognition), and randomized 688 emergency medicine physicians to use the interventions or to a control. In this observational follow-up, we evaluated whether exposure to the interventions changed physician performance in practice.

Methods: We obtained 2016–2018 Medicare claims for severely injured patients, linked the names of trial participants to National Provider Identifiers (NPIs), and identified claims filed by trial participants for injured patients presenting to non-trauma centers in the year before and after their trial. The primary outcome measure was the triage status of severely injured patients.

Results: We linked 670 (97%) participants to NPIs, identified claims filed for severely injured patients by 520 (76%) participants, and claims filed at non-trauma centers by 228 (33%). Most participants were white (64%), male (67%), and had more than three years of experience (91%). Patients had a median Injury Severity Score of 16 (IQR 16–17), and primarily sustained neuro-trauma. After adjustment, patients treated by physicians randomized to the interven-

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tions experienced less under-triage in the year after the trial than before (41% versus 58% [-17%], $P = 0.015$); patients treated by physicians randomized to the control experienced no difference in under-triage (49% versus 56% [-7%], $P = 0.35$). The difference-in-the-difference was non-significant (10%, $P = 0.18$).

Conclusions: It was feasible to track trial participants' performance in national claims. Sample size limitations constrained causal inference about the effect of the interventions.

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Introduction

Half of all injured patients are taken initially to a non-trauma center, where a physician must evaluate their injuries and decide whether they would benefit from transfer to a trauma center (*triage*).¹ Despite 40 years of performance improvement initiatives in trauma, under-triage (failure to transfer those for whom transfer is indicated based on clinical guidelines) remains common.^{2,3} Patient factors (age, insurance status) and institutional factors (for-profit status, distance to a trauma center) contribute to under-triage.^{2,4,5} However, physician judgment also plays a key role.

Physicians use pattern recognition “short-cuts” (*heuristics*) to triage patients. When well calibrated, heuristics produce rapid, cognitively-efficient solutions to complex problems. However, when calibrated poorly, heuristics can produce diagnostic errors, which can result (in this case) in under-triage.⁶ Professionals develop well-calibrated heuristics through an experience-feedback loop that hones pattern recognition and improves recognition of relevant contextual cues.⁷ Given the difficulty of replicating that loop outside of extended formal training programs (e.g., residency), initiatives for reducing diagnostic error focus on constraining the use of heuristics.⁸ Unfortunately, these initiatives have had limited impact.

In prior work, we developed customized, theory-based digital behavior change interventions (DCBIs) to recalibrate physician heuristics, as a novel method of reducing under-triage in trauma. In 2 clinical trials, exposure to the interventions reduced under-triage on an experimental task.^{10,11} In this follow-up study, we assessed whether the interventions affected the participating physicians' real-life practice using Medicare claims. Using a difference-in-difference design, we compared performance in the years before and after the trial for physicians in the treatment and control groups. We anticipated that it would be feasible to use claims to track physicians longitudinally, and hypothesized that greater improvement would occur for the intervention than for the control group.

Methods

Overview of prior studies and present analysis

In 2016 and 2017, we conducted two discrete trials using DCBIs to improve physicians' heuristic judgment.^{9,10} In the first trial, we tested the effect of a DCBI that attempted to recalibrate heuristics through stories (narrative engagement). We randomized participants to spend an hour using the DCBI or com-

pleting a text-based educational program modeled on the Advanced Trauma Life Support (ATLS) course (active control).⁹ In the second trial, we tested the effect of a DCBI that attempted to recalibrate heuristics through structured case comparison (analogical encoding). We randomized participants to spend two hours playing 1 of the 2 DCBIs or using the text-based educational program (active control), or to do nothing at all (passive control).¹⁰ In both trials, we measured the effect of the interventions on performance with a validated virtual simulation.¹¹ The main results from these studies have been previously reported, and we describe the interventions (both DCBIs and the text-based educational applications) in more detail in the **Appendix**.

The current study reports follow-up of trial participants, which we performed by linking trial data to claims records from the Centers for Medicare & Medicaid Services (CMS). We used Medicare claims because they: a) include physician and institutional identifiers; c) allow a national sample of patients; c) represent a population (i.e. those over the age of 65) disproportionately affected by under-triage. We identified patients treated by trial participants in the year before and the year after their trial, and compared the triage practices of those exposed to the DCBIs and those in the control (active/passive) groups. The University of Pittsburgh Institutional Review Board approved this study and permitted a waiver of consent given the retrospective nature and minimal risk to participants. We performed all analyses using Stata 15 (Statacorp, TX) with the per-comparison alpha set at 0.017 (using a Bonferroni adjustment for multiple comparisons). We registered the study on ClinicalTrials.gov (NCT04516044).

Participants

We included all physicians recruited for the 2 previous studies. Using the National Plan & Provider Enumeration System (NPPES), we identified each physician's unique National Provider Identifier (NPI). For physicians who participated in both trials, we restricted analysis to patients they treated during the first trial. We excluded physicians for whom we could not definitively identify an NPI, who did not file claims for severely injured beneficiaries, and who did not treat beneficiaries at a non-trauma center during the first visit of the first episode of care after a severe injury.

Data sources and variables

We obtained patient-level data from the 2016 – 2018 Medicare Beneficiary Summary Files, Inpatient and Outpatient Standard Analytic Files, and Carrier Files. We requested claims that involved a moderate to severe traumatic injury (defined as

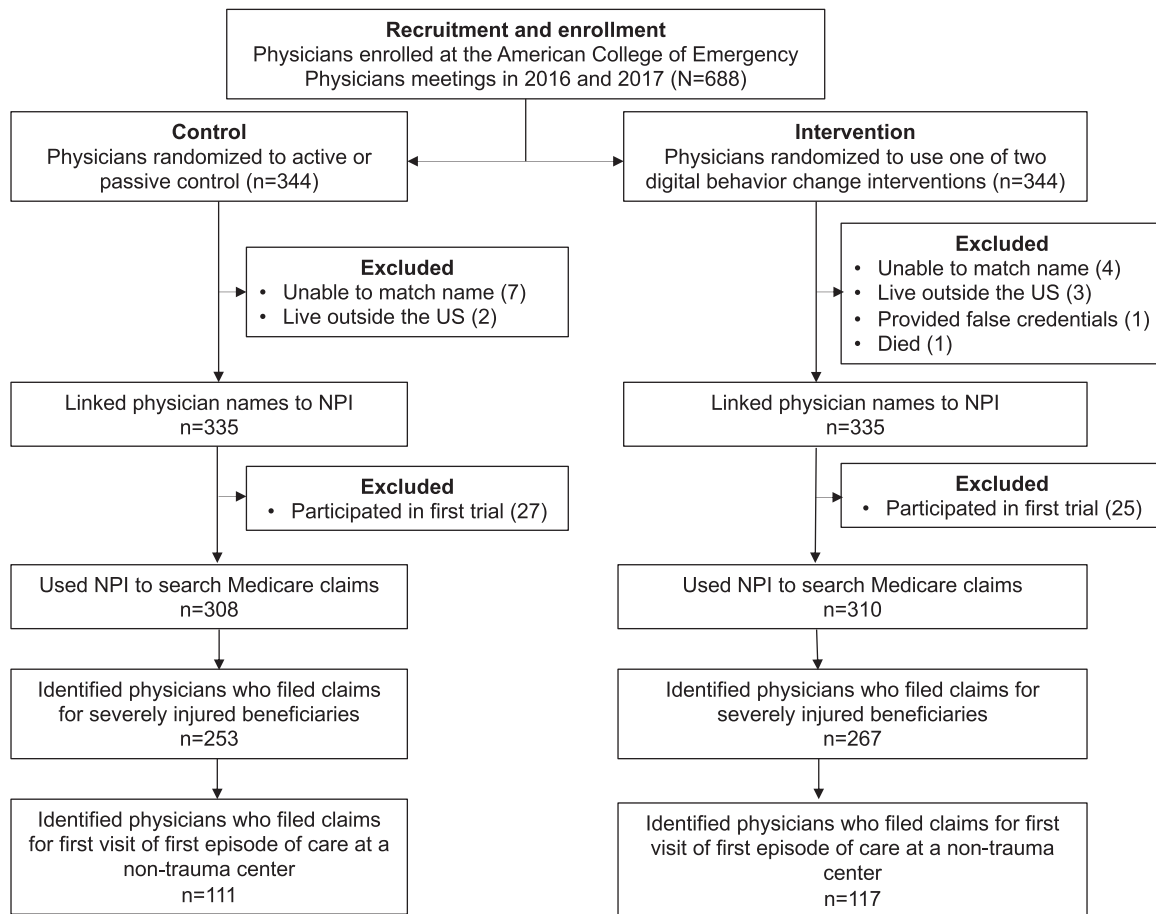


Fig. 1 – Sampling frame for the study.

an abbreviated injury score [AIS] ≥ 3), using validated International Classification of Diseases, version 10, Clinical Modification (ICD-10-CM) diagnostic codes.¹² We abstracted patient demographics and vital status from the Medicare Beneficiary Summary File. Information on co-morbid conditions and injury characteristics came from the ICD-10-CM diagnosis codes in the Inpatient and Outpatient claims. We calculated ISS by mapping ICD-10 codes to AIS using a well-validated program (ICDPIC).¹³ We derived variables on outcomes (e.g. triage status, readmission within 30 days) by linking claims into episodes of care (see below). We classified patient claims as occurring in the pre-trial or the post-trial period (see Appendix).

Physician-level data came from the NPPES and demographic surveys completed as part of trial tasks. We abstracted information on physician characteristics, including sex, board certification, and experience (see Appendix). We categorized participants as those who completed all study tasks, including the post-intervention assessment, and those who did not.

The trauma center designation of hospitals came from the Trauma Information Exchange Program (TIEP). We categorized hospitals as trauma centers (Levels I-V) and non-trauma centers (non-designated hospitals).

Identifying patients treated by trial participants

We identified patients treated by these physicians by searching the Outpatient and Inpatient Analytic Files and the

Carrier Files for the NPI numbers of trial participants (see Appendix).

Constructing episodes of care

We used the Inpatient and Outpatient Standard Analytic files to identify visits to acute care, non-federal hospitals. We constructed episodes of care by ordering claims by day and linking visits that occurred within one day of each other into a single episode. For episodes with multiple claims from the same day, we ordered them under the assumption that patients would move from non-trauma centers to trauma centers, and from low-volume hospitals to high-volume hospitals (see Appendix).

Statistical analyses

We excluded episodes of care that began at Level I-IV trauma centers because the American College of Surgeons focuses on reducing under-triage at non-trauma centers, and excluded patients younger than 65 years. We categorized episodes as involving minimal-to-moderate (ISS < 15) or severe (ISS > 15) injuries, and restricted the analyses to those with severe injuries. We categorized episodes as occurring during the pre-trial or post-trial period.

We defined *under-triage* as failure to transfer a patient with severe injuries to a higher level of care, either directly from the

Table 1 – Characteristics of trial participants.

Characteristics	Participants (n = 618)
Sex – male (n, %)	446 (67)
Race (n, %)*	
White	361 (64)
Black	20 (4)
Asian	100 (18)
Hispanic	33 (6)
Pacific Islander, Native American	19 (3)
Other	29 (5)
Specialty (n, %)	
Emergency medicine	631 (94)
Family practice	18 (3)
Internal medicine	7 (1)
Other (e.g. general surgery)	14 (2)
Years in practice based on date of NPI issuance	
≤3 years	61 (9)
4-9 years	269 (40)
≥10 years	340 (51)
Practice environment – non-trauma center only (n, %)	324 (53)
Self-reported use of intervention – min (median, IQR)*	90 (60–120)
Number of first visit/episode claims filed for severely injured patients who presented initially to non-trauma centers (median, IQR)	2 (1–3)
Pre-trial duration between evaluation of patients and start of trial – days (median, IQR)	233 (300–130)
Post-trial duration between completion of trial and evaluation of patients – days (median, IQR)	144 (83–271)

* Data abstracted only from trial records, and therefore represents those physicians who completed the demographic survey (n = 584)

ED or within one day of admission, as recommended by American College of Surgeons' guidelines. We therefore categorized those who were admitted as under-triaged. We excluded patients who died on the day of admission, as this could reflect either an error in triage decision-making or an assessment of clinical instability that precluded transfer.

We summarized outcomes for all severely injured patients including: triage status, in-patient mortality, disposition status at the time of discharge from their episode of care, readmission within 30 days, and 30-day mortality.

Effect of the intervention

To determine the difference-in-difference effect of the intervention on participants in the intervention and control arms, we fit a mixed effects logistic regression model with under-triage as the dependent variable and physicians' exposure as the primary independent variable, using an intention-to-treat approach (ignoring whether participants completed all study tasks). We adjusted for the year in which the patient presented, and clustered at the physician-level. We included the trial period (before or after) as an interaction term. We analyzed only the first episode of care, as we could not assess whether subsequent episodes reflected follow-up care or new injuries. We analyzed only the first visit within the first episode, to avoid confounding introduced by prior physicians' decisions. In secondary analyses, we tested the effect of the exposure on other outcomes (e.g. 30-day mortality). In sensitivity analyses, we restricted the cohort to physicians who completed all the study tasks, to strengthen our ability to make causal inferences about the role of the intervention

in any changes in performance. We also tested an alternative definition of non-trauma centers, reclassifying Level III-V hospitals as non-trauma centers.

Power

We estimated the sample size for the two parent trials based on Cohen's method of estimating power for behavioral trials, to test the primary hypothesis that exposure to the DCBIs would improve performance on a validated virtual simulation compared to exposure to the control.^{9,10} Since this study represented a pragmatic, observational follow-up of those trials, we did not estimate a sample size.

Results

Among 688 physicians from two clinical trials, we identified an NPI number for 670 (97%). We restricted data collection for 52 (8%) physicians, who participated in both Trial 1 and 2, to the first trial. We excluded: 1) 98 (14%) physicians who did not file a claim in fee-for-service Medicare for a severely injured patient; 2) 292 (42%) physicians who did not file a claim for an eligible visit (i.e. a first visit of a first episode at a non-trauma center). See (Fig. 1) for a schematic of the sampling framework.

Trial participant characteristics

Overall, the physician cohort was white (64%), male (67%), and had greater than 3 years of experience (91%). (Table 1) shows a more detailed characterization of the participants. Physicians

Table 2 – Characteristics of patients treated by trial participants, stratified by exposure and period.

	Control		Digital Behavior Change Interventions	
	Pre-trial(n = 113)	Post-trial (n = 156)	Pre-trial(n = 154)	Post-trial(n = 134)
Age – years (mean [SD])	80 (11.3)	81 (9.4)	79 (13.0)	79 (10.8)
Sex – male (n, %)	52 (46)	82 (53)	66 (43)	73 (54)
Race (n, %)				
White	89 (79)	133 (85)	131 (85)	105 (78)
Black	11 (10)	8 (5)	7 (5)	11 (8)
Asian	6 (5)	5 (3)	9 (6)	7 (5)
Hispanic	4 (4)	5 (3)	0 (0)	6 (4)
Native American, Pacific Islander	0 (0)	1 (0.6)	0 (0)	0 (0)
Other	3 (3)	4 (3)	7 (5)	5 (4)
Number of comorbidities, median (IQR)	3 (1–6)	4 (2–5)	4 (3–6)	4 (2–6)
Selected comorbidities				
Congestive heart failure	25 (22)	28 (18)	21 (14)	17 (13)
Cardiac arrhythmias	41 (36)	59 (38)	56 (36)	39 (29)
Renal failure	25 (22)	30 (19)	38 (25)	25 (19)
Liver disease	2 (2)	8 (5)	8 (5)	3 (2)
Lymphoma	2 (2)	3 (2)	3 (2)	1 (1)
Metastatic cancer	2 (2)	2 (1)	4 (3)	4 (3)
Solid tumor without metastasis	7 (6)	7 (4)	4 (3)	4 (3)
Coagulopathy	20 (18)	14 (9)	19 (12)	15 (11)
Weight loss	11 (10)	16 (10)	16 (10)	16 (12)
Fluid/electrolyte disorders	34 (30)	58 (37)	69 (45)	55 (41)
ISS (median, IQR)	16 (16-20)	17 (16–17)	16 (16–17)	16 (16–17)
Injury patterns				
AIS head \geq 3 (n, %)	88 (78)	122 (78)	120 (78)	107 (80)
AIS face \geq 3 (n, %)	0 (0)	0 (0)	0 (0)	1 (1)
AIS chest \geq 3 (n, %)	3 (3)	7 (4)	7 (5)	3 (2)
AIS abdomen \geq 3 (n, %)	2 (2)	1 (1)	5 (3)	0 (0)
AIS extremities/pelvis \geq 3 (n, %)	5 (4)	8 (4)	12 (8)	3 (2)
AIS external \geq 3 (n, %)	0 (0)	0 (0)	1 (1)	0 (0)

filed a median of 2 (IQR 1-3) claims for eligible visits. The median duration between the conclusion of the trial period and the evaluation of patients was 144 days (IQR 83-271).

Patient characteristics

We identified 557 patients over the age of 65 treated by trial participants during an eligible visit. Patients had a mean age of 80 (SD 11), were mostly white (82%) and male (51%). They had a median ISS of 16 (IQR 16-17), and primarily experienced single-system neurotrauma (see Table 2). (Table 3) summarizes unadjusted patient outcomes.

Effect of the interventions

After adjustment for the year of the patient's presentation and clustering at the physician-level, severely injured patients treated by physicians randomized to the interventions were less likely to be under-triaged in the year after the trial than in the year before the trial (58% versus 41%, $P = 0.015$). Pa-

tients treated by physicians randomized to a control arm (active or passive) had no difference in under-triage before and after their trial (56% versus 49%, $P = 0.35$). There was no difference-in-the-difference between the 2 arms of the trials (10%, $P = 0.18$). (Table 4) shows the adjusted probabilities of other outcomes and sensitivity analyses.

Discussion

In this longitudinal follow-up of 2 clinical trials of DBCIs designed to improve trauma triage at non-trauma centers, we used Medicare claims to assess sustained clinical behavior change. The first key finding is that it was feasible to track trial participants' clinical performance in national claims by linking physician trial participants' names to publicly-available NPI numbers. Physicians randomized to the intervention under-triaged fewer patients after the trial, while physicians randomized to the control arm made similar triage decisions across the 2 time periods. However, we lacked the

Table 3 – Unadjusted outcomes of patients treated by trial participants, stratified by exposure and trial period.

Outcomes	Control		Digital behavior change interventions	
	Pre-trial (n = 113)	Post-trial (n = 156)	Pre-trial (n = 154)	Post-trial (n = 134)
Under-triaged	59 (52)	77 (49)	85 (55)	56 (42)
Discharge status, n (%)				
Home	50 (44)	72 (46)	77 (51)	65 (49)
Continued acute care	13 (12)	9 (6)	10 (7)	9 (7)
Discharge to SNF or to rehabilitation center	43 (38)	55 (35)	52 (34)	53 (40)
Died	6 (5)	20 (13)	12 (8)	7 (5)
Other (e.g. discharged to jail)	1 (1)	0 (0)	1 (1)	0 (0)
Case fatality, n (%)				
Death on day of admission	0 (0)	3 (2)	2 (1)	0 (0)
Death during hospitalization	7 (6)	20 (13)	12 (8)	8 (6)
Death within 30-days of index hospitalization	26 (23)	40 (26)	26 (17)	18 (13)
30-day readmission if alive at time of discharge	10 (9)	27 (20)	28 (19)	24 (19)

Table 4 – rPredicted probabilities of outcomes of patients treated by trial participants*.

Outcomes	Game			Control			Difference-in-difference	
	Pre-trial	Post-trial	P	Pre-trial	Post-trial	P	Probability	P
	Probability (95% CI)	Probability (95% CI)		Probability (95% CI)	Probability (95% CI)			
Primary outcome								
Under-triage	0.58 (0.48–0.68)	0.41 (0.31–0.50)	0.015	0.56 (0.46–0.67)	0.49 (0.39–0.59)	0.35	0.10	0.18
Secondary outcomes								
Death during first episode of care	0.08 (0.04–0.13)	0.06 (0.02–0.10)	0.45	0.07 (0.02–0.12)	0.12 (0.07–0.17)	0.27	0.07	0.15
Death within 30 days	0.19 (0.12–0.25)	0.12 (0.07–0.18)	0.17	0.27 (0.17–0.37)	0.23 (0.16–0.30)	0.58	0.03	0.48
Readmission within 30 days	0.19 (0.12–0.26)	0.17 (0.10–0.24)	0.73	0.09 (0.03–0.15)	0.17 (0.10–0.23)	0.13	0.10	0.10
Sensitivity analyses								
Analysis restricted to physicians who completed study tasks								
Under-triage	0.61 (0.51–0.72)	0.40 (0.30–0.51)	<0.001	0.58 (0.46–0.71)	0.49 (0.38–0.60)	0.26	0.12	0.17
Level III-V trauma centers reclassified as non-trauma centers								
Under-triage	0.55 (0.46–0.63)	0.47 (0.38–0.56)	0.20	0.58 (0.49–0.68)	0.5 (0.41–0.58)	0.16	0	0.83

* Predicted probabilities of outcomes from a series of mixed effects logistic regression models, clustered at the physician-level. The independent variable was the exposure of the physician. We adjusted for the year of the patient's presentation and included the trial period as an interaction term. We did not include the trial (i.e. first or second) because of collinearity with the year of presentation.

sample size to reject the null hypothesis for the difference-in-difference between the two arms, precluding any causal inferences about the effect of the game on physician triage practices.

Our study exploits the richness of Medicare claims, which include physician and hospital identifiers, allowing the tracking of patients across space and time.¹⁴ Others have described the use of claims to measure healthcare utilization after behavioral trials.¹⁵ However, they have done so by paying third-party intermediaries to link patients' social security numbers to claims records, with the objective of minimizing privacy breaches. However, this introduces logistical barriers to the method. In contrast, we relied on publicly-available data to create the linkages, and demonstrated the feasibility of using claims to supplement primary data collection (performance in simulation). Using real-world performance data contributes to the generalizability and validity of our initial findings. It is more convincing than simulation performance because it suggests behavior change despite facilitators and barriers that affect physician performance in practice.¹⁶

Our finding that physicians randomized to the DCBIs under-triaged fewer severely injured, older patients after the trial compared with before the trial has several possible explanations. First, the DCBIs, grounded in the well-accepted dual process model of decision making, may have achieved their objective of changing physician behavior.^{17,18} By treating physicians' heuristic judgment as an asset that could be improved, instead of as a liability that should be curtailed, we may have improved transference and the durability of the treatment effect.^{19,8} The observation that (on average) 144 days elapsed between completion of the intervention and the clinical encounter corroborates the durability of the knowledge transfer. Second, the games may simply have increased physicians' willingness to transfer all patients (i.e., over-triage) instead of improving their recognition of the severely injured. To test this hypothesis, we would have needed to purchase claims filed for patients with minimal injuries, which was beyond the budget of the current study. Third, the difference may reflect secular trends and may be unrelated to the game intervention. We could not reject this hypothesis because the difference-in-differences between the intervention and control groups was not statistically significant. The study was underpowered to test the significance of the observed 10 percentage point difference in performance between the 2 groups, both because severe injuries at non-trauma centers are relatively rare events and because of our enrollment criteria. A large proportion of the trial participants worked at both trauma and non-trauma centers, which resulted in the exclusion of many of the patients that they treated.

Therefore, our findings regarding the effect of the DCBI on real-world clinical practice must be regarded as exploratory. Nevertheless, taken together with the practice improvement in simulation, there is promise that theoretically-based behavioral interventions may be a method of addressing a refractory clinical problem – under-triage in trauma. Severely injured patients treated at trauma centers have a 10 – 25% reduction in mortality, increased rates of return to independent living, and less pain at 1 year.²¹⁻²⁴ Half of all injured patients present initially to non-trauma centers, making physician

judgment central to efforts to eliminate preventable deaths and morbidity after injury.²⁵ If effective, our interventions could reduce under-triage by as much as 10%, when layered on existing quality improvement interventions. This effect is comparable with the impact of Advanced Cardiac Life Support on return of spontaneous circulation after cardiac arrest (risk difference 10%), widely accepted as an effective method of continuing medical education.²⁶

Our study has several limitations. First, we cannot assess the completeness of our patient sample. As many as 11% of institutional records lack matching professional claims, reflecting both administrative errors and procedures for bundling payments.²⁷ As a result we may not have captured all the patients treated by our trial participants. We cannot predict the effects of these omissions. Second, we recruited physician volunteers at a national meeting of emergency medicine physicians, also with unknown effects on the generalizability of our observations. The pre-trial under-triage rates for these volunteers (54% across all groups) were lower than those reported in observational studies (70% – 80%).^{1,3} Third, we used fee-for-service Medicare data to evaluate physician behavior. Our observations may not be generalizable to younger patients. However, patients over the age of 65 make up an increasing proportion of trauma patients (35%), are less likely to be triaged appropriately (odds ratio 0.48), and are more likely to experience adverse consequences after injury.^{5,28,29} Fourth, we use ICD10-derived ISS to identify our cohort, an imperfect, albeit well-validated, process. However, we have no reason to believe that this misspecification varies systematically. Fifth, our use of Medicare claims to assess performance limited our ability to measure formally the fidelity and sustainability of the intervention.

In summary, we present preliminary evidence that theoretically-based DCBIs may improve triage at non-trauma centers. These observations support experimental observations about the effect of behavioral interventions on diagnostic error and warrant additional investigation.

Access to data and data analysis

Dr. Mohan had full access to all the data in the study and takes responsibility for the integrity of the data and the accuracy of the data analysis.

Authorship contributions

Study concept, design, analysis, interpretation: dm, ccc, bf, mrr, dca, dmy, aeb

Drafting of the manuscript: dm

Critical revision of the manuscript for important intellectual content: ccc, bf, mrr, dca, dmy, aeb

All authors read and approved the final manuscript.

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reviewed the study but played no role in the design, collection, analysis or interpretation.

Data sharing

De-identified data will be shared on request, conditional on approval by the Institutional IRB.

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Conflict of interest

DM, CCC, BF, MRR, DCA, DMY, AEB report no conflict of interest.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at [doi:10.1016/j.jss.2021.07.029](https://doi.org/10.1016/j.jss.2021.07.029).

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