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Title	Establishing consensus on emergency department interventions that could be conducted in sub-acute care settings for non-emergent paramedic transported visits: a RAND/UCLA modified Delphi study
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Reviewer 1	[Name withheld]
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Reviewer comments and author response	Patients selected from the database were only restricted by their triage category upon arriving at the ED. No consideration has been made about what their outcomes were. It is possible that some of these patients were subsequently admitted to the hospital, admitted to ICU or even died in the hospital. Perhaps a better data selection technique would have been if patient intervention information was selected based on patient discharge outcomes. *Thank you for your comment, we appreciate this standpoint. The objective of this study was to examine consensus of an expert physician panel regarding suitability of ED interventions to be performed in sub-acute care centres. To construct our list of recorded interventions to be assessed by experts, we selected the 150 most frequently interventions recorded on paramedic transported patients with a non-emergent acuity. We decided to not include other characteristics to define this cohort (ie age, ED visit outcome) as non-emergent medical acuity's already have a high discharge proportion (~70% in our dataset) and we did not want to incorporate reporting bias into the study by making this restriction (supressing select patient visits based on the outcome). In a forthcoming study that will test this study's results with additional patient characteristics, we intend to analyze validity of our classification with the ED visit disposition outcome. Thus, the outcome could not be incorporated into this cohort, and we relied on panellist judgements to decide if an intervention was associated with a higher probability of a patient being admitted.* Secondly, the distribution of physician demographics was not sufficient which will have biased the study. The majority were male, and more concerning, most were emergency physicians. Another issue I noted was that they did not necessarily need to be currently practicing in a clinical setting. The results section needs a breakdown of the consensus based on physician qualification to demonstrate this had no effect. *This is a fair comment. Our decision to include emergency department physicians primarily was based on their ability to make justified ratings of the ED interventions included in the study. Several interventions assessed may be outside of a primary care physicians' skills set, thus we concentrated on assembling a panel that would be comfortable with making these judgements. Of the primary care physicians included, all were adept in knowledge of the interventions to be rated prior to study initiation, confirmed by the authors. The panel is largely male, which is a limitation of purposive sampling in this circumstance; we have added this as a limitation to Limitations on page 18.* *All physicians included in the panel are currently practicing in a clinical*

	setting. We have added further clarity regarding this comment to page 5. Finally, and most importantly, no context is given for the interventions. I am unclear on how the physicians could have made a decision about where an intervention could occur without any context. I am assuming they were asked to consider the intervention alone, in which case, all of them and more can be carried out in the sub-acute setting. The most important question here, is should they be? This cannot be answered without knowing who and what is being treated. *Thank you for your comment, please allow us to clarify. You are correct to identify that further contextualization of patients is needed prior to informing clinical decisions or practices. This study contributes only an epidemiological contribution to the scientific community. As you stated in your comment, all the interventions of this study could be performed in sub-acute care, which was the very objective of this study – to identify, in a very broad sense, which ED interventions could be conducted in non-acute settings. We certainly agree that this study does not represent a patient classification on its own. This was highlighted in the Interpretation and Conclusion of the manuscript - this study is preliminary to any validation research. This study is ideally the first of three. We are currently undergoing research to add further contextualization with additional patient characteristics to formulate a patient classification, which will incorporate the result of this study as one aspect.* For example, an intravenous catheter can be placed in any setting, but if the patient is a 50 year old male with diabetes and a history of feeling general unwell it should be occurring either in the ambulance on the way to hospital or in the ED. This patient, who presents with vague symptoms of simply being generally unwell (and therefore quite likely to only appear as a potential category 3, 4 or 5 patient), may in fact be having a silent myocardial infarction hidden by his diabetes. *We concur that this specific patient case is an example of atypical care findings, and certainly warrants acute medical attention to rule out underlying syndromes. Our objective was to ascertain where ED interventions could be performed within alternative care, not to direct paramedics to treat on site and negate transport. This scenario illustrates the need for a patient classification that incorporates more characteristics beyond the presenting complaint and acuity. By studying ED interventions, an important element that is uncommonly included in known potentially preventable ED visit classifications can be incorporated to best describe potentially preventable ED visits. By no means can interventions alone dictate clinical guidance, emphasized through this patient case, and was written in the Interpretation and Conclusion of the manuscript.* If the Delphi questions were asked in the setting of a small scenario I think the responses could have been very different. It is for this reason that the findings don't add a great deal to the body of knowledge. If the results are to be presented as a classification of interventions alone, a very strong introduction and discussion need to be written to provide support as to why this was done in isolation and demonstrate the value of this study. *Our objective was to present results of physician consensus on ED interventions alone, not in specified scenarios. We determined that*

	specific scenarios would not be scalable for the panel to make judgements without fatigue, given each intervention could have had three to four scenarios and increased to request of the panel members significantly. We included 150 interventions for rating amongst 4 care centres, a time-consuming undertaking in itself. We deemed 150 interventions is an acceptable workload, would be manageable for each panel member level, and manageable for the study authors to ensure high-quality results. We have chosen to conduct this study as a first of three studies, in which the subsequent study will address additional patient characteristics more specifically, plausibly in a scenario format with this study's results (see comment above). We agree with your recommendation that additional details are needed in the Introduction to articulate our study objective alone, and done so on page 3.
Reviewer 2	Name withheld
Institution	
Reviewer comments and author response	Great initiative. This is the type of research that is required to rationalize and find efficiencies in health care. I think the process of expert panels and specifically finding areas of consensus is extremely important. I do have a couple of areas of concern in the reporting of the value of the data and conclusions, however. The authors did a good job of identifying the limitations of their study, with the main limitation for me in their words..."Some experts emphasized their ratings do not infer any direction for clinical guidance based solely on this study, as an intervention alone does not provide enough specific information to inform care planning without further contextualization." As a patient's clinical presentation (context) will be a much tougher question for consensus, I feel the above sentiment should be better reflected in the abstract. *Thank you for your comment, we appreciate this insight. A concluding statement on this sentiment has been added to the manuscript Abstract on page 2.* Secondly, the specificity of the data used, leads one to believe that almost 70% of ED interventions can be performed elsewhere. Looking at the data there are over 20 (14%) of "simple skin repair" procedures broken down by procedure (suture vs staple) and body location (arm vs leg). Most clinicians would likely expect this to be described as a few different interventions not as many as reported. There are other examples as well such as, ultrasound and x-ray. This makes the conclusion, while accurate, open to criticism. I would prefer to see a further analysis that group some of the "alike" interventions into single interventions. Also, if there is data describing the frequency of each procedure to reflect actual % workload that could be a very interesting table and supportive to the message. This point is optional, but if not described, it's absence should probably be identified in the studies limitations. *This is a valid comment. Our approach to examine consensus did not group alike interventions together as this may introduce bias into the study by making an assumption that all physicians would consider grouping interventions based on procedure with differing body locations in the same way. We report in this manuscript the exact findings of the modified*

	Delphi panelist ratings. However, we certainly agree that analyzing frequency and groupings of ED visits with similar intervention in valuable, and we are conducting this research right now, but is out of scope for this manuscript (i.e., derivation, description, and reporting of a population cohort for a related to different objective). Study Conclusion: "An overarching goal of our study was to determine if consensus on which ED interventions could be performed elsewhere, such that an epidemiological patient classification could be constructed to inform redirection by paramedics." If the above statement is accurate, I think the authors did a great job of research and proving the validity of their consensus building and health policy making process. I feel that the conclusions much beyond that, are not strong or necessarily valid, given their own stated weaknesses and limitations. "knowledge of interventions suitable for sub-acute healthcare centres WILL inform a patient classification mode" may be more reasonably stated as "has potential to..." or "With more research in clinical presentation... etc. *Thank you for your comment, we have amended the manuscript's concluding statements to reflect this modification. Adjustments made to the Interpretation on page 19.*