Author’s response to reviews

Title: Crowding in the emergency department in the absence of boarding – A transition regression model to predict departures and waiting time

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Author’s response to reviews:

Dear editor,

We are grateful for the opportunity to resubmit our manuscript after careful revision. We appreciate the reviewers comments and believe they have greatly improved our manuscript.

Below we answer each comment in detail and point to the resulting changes in the manuscript.

Reviewer 1 (R1)

R1.1: Wait time or length-of-stay: The authors use the term wait time for the time spent in the ED. That will likely be confusing to the casual reader. It is acknowledged that the authors define that term but substitution of a term such as ED LOS (ED length of stay) or even 'dwell' time might be more fitting

A1.1: We agree that the latter term is more common in the ED literature. In the manuscript we chose 'wait time' since this is the appropriate term in queuing modelling theory upon which our model is build. We believe this is an important point to underline since it is a theoretically well-established framework that we show can be extended to the ED. To clarify our use of "wait time" we have added the following, ll. 132-133: "synonymously with the term “length-of-stay” (ED LOS)"
R1.2: Boarding in the studied ED: What is missing is the presence or absence of significant boarding. From your data, it appears there is no boarding of inpatients in the ED (LOS maximum was 5 hours). As boarding of inpatients is a major factor in ED LOS in some countries such as the US, Canada and Australia, the authors should comment on their own experience. If indeed there is no boarding of inpatients (also known as access block), then this paper is of greater interest as it defines factors in the absence of boarding.

A1.2: We thank the reviewer for raising our attention to this point and agree it is important. Indeed, boarding was not a concern in the studied ED with LOS >5 hours for only 5% of patients. We have made several modifications to accommodate this:

Title: We ask the editors for permission to the addition: “in the absence of boarding”

l. 30 (abstract): added “Boarding played a negligible role in the studied ED.”

l. 48: added “risk”

l. 49: added “ high proportion of complex or critically ill patients and restricted access to general practice”

l. 53: added “the input”, deleted “arrivals”

ll. 242-242: “With only 5% of patients spending more than 5 hours in the ED boarding was not a concern in the studied ED.”

l. 315: “in the absence of boarding”

added two citations: Morley et al. 2018 (ref 7) and Hoot and Aronsky 2008 (ref 9)

Reviewer 2 (R2)

R2.1: Line 81 - the number for 2013 (N= 17520) is different from the estimated annual attendance (40000) in line 66. Over a year, conceivably ΣA(t) should = ΣD(t) = N ≈ annual attendance.

A2.1: The number of arrivals to the studied ED in 2013 was 41693. This is given in the Additional file 1 and in ref 10. We created a data set dividing the study period into 30 minutes intervals. We have made an effort to be detailed about our method. To clarify we have added to line 83: ”The total number of arrivals was 41693” and to l. 85: "24 * 2 * 365 ="
R2.2: Additional File 1: It might be helpful to include on-duty physician shifts as physician numbers should also affect departure time.

A2.2: We agree this is both interesting and possibly important data to take into account. Unfortunately we do not have data on this as stated in ll. 310-311.

R2.3: Line 128 - the definition of "waiting time" seems to be very close to turnaround time or length-of-stay, which may be better understood by emergency department readers. 

A2.3: We appreciate the reviewer's point and refer to our answer 1.1

R2.4: Lines 177 and 253-261 - "number of new arrivals" is not an "actionable" predictor unless you can turn away new arrivals or divert ambulance arrivals, both of which are undesirable actions. Likewise length of queue in itself is not particularly helpful to inform mitigation measures. Length of queue is predicated on ability of staff to turn over patients in the ED, which in a labor-intensive environment like the ED, essentially depends on staffing numbers. Would the authors consider modelling changes to staffing numbers at different times of the day to see how this would impact waiting time?

A2.4: We thank the reviewer for the very relevant points. We discuss the difficulties of predicting arrivals in ll. 260-263. While we agree that arrivals is not an actionable predictor, it is an independent predictor that is not affected by the immediate state in the ED. As such “new arrivals” as a predictor tells us something about the vulnerability of “the system” (the ED). Furthermore, we believe - and discuss in ll. 263-268 - that queue length and change in queue length is easy to monitor and we show that this is a strong predictor for increasing waiting time/LOS in the studied ED. We go on to discuss how this information can be used to take action to counter crowding. While staffing may have effect at a general level (it is approximately static within broad time spans), everything else being equal, the immediate “expedition-time” in the ED must be predicted by the immediate state in the ED which in turn is to a large degree captured by the queue length and the change in queue length and would not be captured by staffing. To clarify that we model the immediate waiting time within 30-minutes intervals (a dynamic model) and not the average waiting time (a static model) we have added to l. 64: “dynamically” and to l. 141: “immediate”. Finally, nurse staffing in the studied ED was determined by shift (day, evening, night) and weekday/weekend as detailed in Additional file 1 and ref. 10, both of which we model. Thus, modelling staffing would introduce collinearity.
R2.5: The use of binomial regression to model ED length of stay is novel to me, I have not come across this in the EM literature as yet. The authors made no arguments as to whether it is a valid model to apply to ED arrivals and departures.

A2.5: We appreciate the reviewer's comment and respectfully point to the section "A statistical model for the number of departures" Il. 98-130. We believe that a very detailed elucidation of the theoretical framework for the transition model is out of the scope for Medical Research Methodology but reference the seminal work by Diggle et al. "Analysis of Longitudinal Data" (ref. 18) on the topic. In brief (Il. 103-107) "The transition model is formulated as a binomial regression model for each time step t, where the number of trials is equal to the maximum number of possible departures (n(t) = Q(t) + A(t)) and the number of successes corresponds to the actual number of departures D(t), i.e. D(t) ~ Bin(n(t), p(t | t-1)), where p(t | t-1) is the probability of a departure in time step t dependent on past values". We have made an effort to be transparent about assumptions and present thorough diagnostics that discussed among others in Il. 292-296.

R2.6: Typos: Line 68, Line 105, Line 239?, Line 258

A2.6: We appreciate the reviewer pointing out attention to this and have corrected accordingly.