Benefit of Helicopter Emergency Medical Services on trauma patient mortality in the Netherlands?

[Letter to the Editor of *Injury*]

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Recently we received the following publication in this Journal: *The effect of Helicopter Emergency Medical Services on trauma patient mortality in the Netherlands*, by de Jongh et al. [1]. We have read this article with a higher than normal level of interest as this directly appeals to our field of work and a few serious questions were raised at (all) the four HEMS centres in the Netherlands.
First of all, the title of the paper suggests that the study represents the national situation, while in fact it is a single center study and therefore these data cannot by definition be extrapolated to the whole country. This paper was submitted without consulting the two trauma centers with an HEMS which prehospitally treated the patients involved in this study. Furthermore, previous papers [2,3,4] from other trauma centers showed conclusions that are very different to this study. Since this article is based on research on the effectiveness of the HEMS in the Netherlands, and the research was performed neither with involvement, nor information of one of the four HEMS stations, we like to make some remarks on this article from the perspective of the four HEMS stations.

In their observational study, the authors conclude that '*HEMS treatment is associated with a non-significantly higher risk of in-hospital mortality for patients with TBI and a non-significantly lower risk for patients without TBI'.* The adjusted OR's are 1.3 (95% CI 0.6-2.7) and 0.9 (95% CI 0.3-2.5) and since both the 95% CI's contain 1 (as do all 95% CIs in their study), indeed there is no statistical significance. The authors use the words *non-significantly higher/lower risk* to indicate this in their statement. But when using statistics to test for significance of findings and no statistical significance is found, only speculative conclusions can be drawn.

Regarding methodology, we would also like to suggest that, to assess a possible effect of HEMS+EMS vs EMS-only on trauma patient mortality, investigators should stratify or adjust for EMS 'scoop and run' to regional hospitals (of a lower level of care) for primary stabilization and subsequent secondary transport to the definitive care facility (level I trauma centre with neurosurgical service). Also, in case of HEMS+EMS, delivery to regional hospitals can be omitted since primary stabilization takes place at the scene or *'en route'* and patients are directly delivered to the level 1 trauma centre.

Excluded from analysis, as described in methods, are patients who were directly transferred from the ED to another hospital. However, the authors do not provide information on the number of patients that have been excluded for this reason, and, if any, for what reason. As in the introduction it is stated that the regional hospitals in the county (Noord-Brabant) are capable of resolving acute problems. But it is unclear from the data provided what the level of care of these hospitals is, and whether or not they are adequately staffed for trauma care 24/7 within the time interval given. Selective transfer of patients that otherwise would have been eligible for inclusion might have impacted the outcome of the study. In addition to this: In this study patients who died on the scene are not included also, nor are patients presented primarily at regional hospitals. This was common praxis for the EMS in the examined period, especially in the rural areas, but can strongly bias the found results.

Table 1 showed us the dispatch criteria for HEMS, but the reality in the Netherlands is that only a few of the accidents present on the list result in an activation of the HEMS, as Ringburg etal. [3] showed earlier that only in 14% of the indications a HEMS has been activated. Lemson etal. [4] found large regional differences in activation frequency of the HEMS, which cannot be explained by differences in accident frequency. In this respect it is good to know that during the examined period the HEMS coverage in that specific area was expanded from Rotterdam-HEMS only to Rotterdam and Nijmegen-HEMS together. In 2003 the Nijmegen-HEMS presented only 10 patients in that specific hospital, while in 2011 more than 70 patients were presented there. This can be due of differences in protocol adherence, but can certainly influence the results. Furthermore the dispatch criteria used in this study (Table 1) are currently being updated and will be introduced in the dispatch centers. Herein, there is more focus on the vital parameters of the patient and less on the nature of the accident or the trauma mechanism. This will have a beneficial effect on the rate of overtriage and undertriage, and future research should indicate to what extent the added value of HEMS in the Netherlands will be positively affected under the new dispatch protocol. So adherence to dispatch criteria and regional differences do influence patient presentation, patient numbers and patient characteristics, and therefore the conclusions can be influenced.

In the methods only mortality as an outcome parameter has been examined. In specific situation it can be debated that morbidity or GOSE after a longer period (>6months) is a more sensitive parameter to measure differences in treatment. Ringburg etal.[6,7] already showed that health-related quality of life (QoL, measured as HUI and EQ-5D score) of severely injured patients treated by HEMS does not differ from the QoL of less-severely injured patients who received EMS care alone. This was the case both at one [6] and two years [7] after trauma, and clearly indicates that the severely injured patients who survive the accident do not perform less. This was true for patients with an AIS-score for head injury of <3 as well as for those with an AIS >=3. For the 2-year time point, HEMS was shown to be a cost-effective medical service [7]. Mortality is also of limited value since introduction of the HEMS made more treatment possibilities available in the pre-hospital situation and there may be a shift from patients who die on the place of the accident to those who die during treatment in the ER. This is a missing part in the article: no pre-hospital data about given treatments and time of activation (delay) of the HEMS were used and therefore the conclusions are weak.
Also, patients should be matched for different prehospital scenario's dictated by e.g. entrapment and confinement / difficult access or remoteness of location to minimize selection bias. As decisions in the field regarding triage and treatment are made on physiological derangements, patients should be primarily matched using the *un*coded Revised Trauma Score RTS (0-12) and/or Glascow Coma Scale (GCS) instead of anatomical criteria that can only be assessed retrospectively (such as Injury Severity Score (ISS) and/or Abbreviated Injury Score (AIS)).
The categories used for the matching should have been given and justified. Non-physiological or too broad categories may result in inadequate matching and/or residual confounding. Whether or not that has occurred cannot be judged from the information provided. The baseline data in Table 2 show a 3-point difference in ISS between the HEMS and EMS group, which implies that the matched EMS patients generally had a lower ISS than the HEMS patients. There's a fair chance that this causes residual confounding. Furthermore it is generally not advised to use unpaired analysis for matched data.
Another remark upon the data collection is about recording of vitals. The time of recording of vital parameters and GCS is a bit blurry. The methods section implies that these data were recorded when the patient entered or left the ED. It would have been preferable to choose either one.

Table 5 shows the mortality risks of patients treated by EMS and HEMS. There seems to be a miscalculation in the NNT related to preventing early trauma fatality in patients without TBI in the HEMS group. From the data given, the NNT should be 138 instead of 273 (the pc is 4 out of 107 EMS

patients, but it seems like the authors used the total number of patients in the denominator, resulting in a pc of 4/214).
In addition, given the differences in patient characteristics as shown in Table 2, the model should have corrected for residual confounding, at least for the matching parameters.
In Table 6, the authors correct for prehospital time. This may be artificially longer in the HEMS group. If a longer prehospital time has resulted in a reduced ED time, this might have affected the outcome. From the data it can be calculated that NNT by HEMS is 10 preventing one early fatality case, and 18 for preventing one patient to die during hospitalization, suggesting a relevant contribution of HEMS in this population. For the TBI patients the NNT is 138 and 129 for preventing one case of early and in-hospital mortality, respectively.

Finally, physician-staffed HEMS is part of the standard prehospital (trauma) care system in the Netherlands. Therefore, its effectiveness can only be investigated in observational studies that, unfortunately but inevitably, are subjected to bias and inappropriate or no adjustment for important (or unknown) confounders. Hopefully, the results of the Head Injury Retrieval Trial (HIRT) [5] , currently being performed in Australia, will shed more light on the topic of the effect of p-HEMS/EMS (primary response) vs. EMS on neurological outcome (and mortality) in head injury.
In the Netherlands we suggest that a nationwide study is done on performance of both the EMS and the HEMS care. This can be done by combining the data from the "regional Trauma databases" of all Level-1 trauma centres (also including data on patients presented not primarily in the trauma centre but in the regional hospitals). Data present at the four HEMS stations about medical treatment given and objective on-scene times should be included.

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