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Letter to the Editor

Reply to: Comment on: “The effect of prehospital critical care on survival following out of hospital cardiac arrest: A prospective observational study”



To the Editor,

We thank ter Avest et al. for their interest in our research, which examined the effect of prehospital critical care on survival following out-of-hospital cardiac arrest (OHCA).¹

Ter Avest et al. rightly point out that our study population consisted predominantly of ‘usual’ adult out-of-hospital cardiac arrest (OHCA) patients. The reason we excluded children and patients with OHCA due to trauma is that the underlying pathophysiology and treatment are different,² and the adjustment methods commonly used for adult non-traumatic OHCA would not apply.³ Inclusion of these patient groups would have resulted in a loss of internal validity. Therefore, as we stated in our manuscript: ‘the findings of this research should not be extrapolated to other causes of OHCA, or patient populations.’

The second point raised is the possibility of better outcomes if the prehospital critical care included in our research had been provided by physicians exclusively. We argue that, since two thirds (68%) of prehospital critical care attendances in our study included at least one physician, any potential benefit of physicians would have likely been visible as at least a trend in the overall analysis. We also point readers to the subgroup analysis of primary dispatch of physician delivered prehospital critical care, which had an odds ratio of 1.00 (95%CI 0.57–1.76) for the primary outcome of survival to hospital discharge (n=881).¹

Third, we agree that the relatively long median response time of 28 min for prehospital critical care providers may have limited their effectiveness, as discussed in the manuscript. While this response time is similar to those published by other critical care services in the United Kingdom (UK),⁴ we agree that these results may not be applicable to critical care services if they achieve significantly shorter response times.

Ter Avest et al. state that it would have been more interesting to focus our study on subsets of patients with OHCA in which critical care teams would have the highest potential to improve survival. While we agree that prehospital critical care might be more beneficial in OHCA under special circumstances, we disagree with the notion that focusing on adult non-traumatic OHCA makes the research less interesting. Our research is applicable to the largest group of OHCA patients attended by critical care teams currently, and one where considerable equipoise exists regarding the potential benefit from prehospital critical care.⁵

Finally, we agree that our recommendation to ‘focus dispatch of critical care teams on patients with OHCA who might require critical care interventions and to those with a high likelihood of achieving return of spontaneous circulation (ROSC)’ is not directly supported by the data from our research. However, such focused dispatch in OHCA has been suggested for helicopter based critical care services before, including by co-authors of ter Avest et al’s comment.⁴ Our recommendation is based on the fact that prehospital critical care teams attend patients with OHCA in many parts of the UK and internationally. Given the lack of benefit for the group of adult non-traumatic OHCA patients included in our study, and the theoretical arguments for benefits in the smaller group of patients with ROSC or OHCA under special circumstances made by ter Avest et al., it would seem sensible to focus dispatch of critical care teams on the latter patient groups. We acknowledge that there is only limited evidence for critical care teams attending OHCA in the patient groups that were not included in our research, and would welcome further high-quality research in this important area.

Conflict of interest statement

Jonathan Bengler and Johannes von Vopelius-Feldt work for and with a prehospital critical care service in the UK.

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