

Reply to: “Response to: Long-Term Mortality in Patients Transferred by Emergency Medical Services: Prospective Cohort Study”

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EMS: Emergency Medical Services

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To the Editor,

We have read with great interest the comment from Gobeil Odai, et al¹ about our previous work.² In their letter, the authors stated some concerns and doubts about our work. We understand that the existing confounding factors are multiple and difficult to control, but this has not invalidated the development of predictive models in the past. In this sense, there are patterns that defy the current knowledge, and investigating their possibility is fundamental, which is what our work was looking for. Therefore, we would like to clarify those doubts and discuss the concerns.

The first concern of the authors’ comment is regarding the outcome. To do that, the authors stated that “the data to support the short-term clinical outcomes using [Emergency Medical Services] EMS data are weak;” this is somehow surprising, and could be explained by the novelty of the field as will be explained below. For instance, there are several randomized clinical trials that have shown the predictive validity of scores resulting from EMS data.^{3,4} In fact, a search in clinicaltrials.gov (National Center for Biotechnology Information, National Institutes of Health; Bethesda, Maryland USA) using “prehospital” and “predictive” revealed 28 trials. Regarding the long-term outcome, as occurred with the previous point, there is much evidence of the predictive power of EMS data for long-term outcomes.^{5,6} We are aware of the enormous number of interventions, confounding, among other factors, that are involved in patients attended by the EMS; in fact, we have dealt with some of them^{7,8} and we are developing several studies that assess this point. In fact, we found that despite all these confounding factors, the decrease in the predictions did not discard the potential of the EMS data prediction validity.⁷

The second argument concerns selection bias. There was no selection bias. All patients attended activated the 1-1-2 emergency system, and through a regulatory center, they were assigned to an advanced care resource consisting of two medical transport technicians, a nurse, and a specialist physician (family physician or internist). This doctor decided on the basis of the patient’s characteristics whether to transfer to his or her reference hospital, to the primary care center, or to home. We understand the doubts about the capacity of each hospital. However, the hospital network of the Health System of the community of Castilla y León (Spain) assumes that the total number of transfers managed is 1-1-2, and the referring centers depend on the localization of the emergency. All hospitals receiving patients have critical care units and are authorized to perform major surgery. If a patient needs to be transferred because the hospital lacks a specialty, this is assumed by the health system. It is important to highlight that no difference has ever been found when comparing the different locations. In fact, these geographical differences have also been used as different cohorts to validate our results in previous works.⁹

The third concern is about the lack of identification of diseases, which shows that our work has been interpreted from the hospital perspective. This should not be used when considering prehospital studies, in that settings there are not 100% diagnostics, and instead syndromic categories should be used; this is the syndromic category of each participant is assigned by a professional according to the main diagnosis assigned to each patient, which, as mentioned above, was always made by a specialist physician.

Regarding the point-of-care testing (POCT) values, again, the readers fell in the mistake, interpreting the results, in this case markers intervals, under the hospital perspective. We have observed that the marker ranges in this hyperacute situation did not resemble those observed in the emergency department.

Regarding the Charlson (not Charles as stated in the comment) Comorbidity Index, we again faced the limitation of a prehospital setting in which full access to the medical history of the patient is not guaranteed.

Clearly, the nursing homes category encompasses a large number of possibilities. However, it is clear that living in an institution with caregivers is associated, at least, with a high level of dependency, which is a life-threatening factor, as has been observed recently in the setting of coronavirus disease 2019/COVID-19 infection.⁹ Hence, to simplify the variable, it has been considered as “yes” or “no” without considering information on the type of residence or care that each patient requires.

Finally, we would like to add that prehospital emergency medicine has always been an under-rated discipline in health services. We are unaware of the knowledge of the authors about this field; we understand that there are differences between the different health systems, but it seems evident that the development of these predictive models substantially improves health care systems. This is evident from the exponential increase in scientific works found in PubMed (National Center for Biotechnology Information, National Institutes of Health; Bethesda, Maryland USA) by searching, for instance, the term “prehospital.” In fact, this field is becoming one of the most promising areas of medicine, perhaps due to the lack of research associated with this aforementioned underestimation. Therefore, with this comment and the work in which it is based, we would like to show the potential of prehospital care, not only in the short term, but also when considering long-term outcomes.

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