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Efficacy of Hyperbaric Oxygen for Carbon Monoxide Poisoning



To the Editor:

Despite inaccuracies in Cowl's¹ recent editorial in *CHEST* (November 2017) on the use of hyperbaric oxygen treatment (HBOT) for carbon monoxide poisoning (Henshaw's 17th century domicile, if ever constructed according to his specification, with windows in brick or stone walls, could not have been pressurized; Cunningham's efforts in the 1920s used hyperbaric air equivalent to 50% to 60% normobaric oxygen, not

HBOT), he should be applauded for endorsing the conduct of a multicenter clinical trial. There is already strong evidence that HBOT is beneficial if delivered in a timely manner to patients who are seriously poisoned.² In the recent Huang et al³ database review, despite surprisingly high mortality in both groups, HBOT appears to offer a survival advantage, especially among those with respiratory failure. As Cowl points out, there is ethical concern about the equipoise of randomizing patients with severe carbon monoxide poisoning to normobaric oxygen vs HBOT, an issue that was central to the design of a published clinical trial.⁴ A new study should therefore not focus on patients with carbon monoxide poisoning who are critically ill, but rather on HBOT's effectiveness in subgroups of patients who are less severely poisoned, HBOT's efficacy as a function of time from end of carbon monoxide exposure until HBOT administration, and the optimal number of HBOT sessions.

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Response



To the Editor:

It is reassuring to be applauded by two giants in the field of hyperbaric medicine regarding my observation that there is a clear need for a definitive multicenter clinical trial to further delineate the efficacy of hyperbaric oxygen therapy in the treatment of carbon monoxide poisoning.¹ Collecting adequate trial numbers to

establish the statistical power to appropriately assess subsets of individuals with generally less severe symptoms as Moon and Hampson suggest will be the imperative moving forward. Regarding their allegation of subtle inaccuracies in my historical examples regarding what would be considered true hyperbaric oxygen therapy vs attempts at exposing humans to a pressurized environment without providing 100% oxygen, they would be technically correct given the principles of Socrates, who has been credited with describing the beginning of wisdom as starting with the definition of terms.² To clarify, the Undersea and Hyperbaric Medical Society defines hyperbaric oxygen therapy as an intervention in which an individual breaths near 100% oxygen intermittently while inside a hyperbaric chamber that is pressurized to greater than sea level pressure.³ However, my use of historical examples was not designed to nitpick the technical aspects of what precisely defines hyperbaric oxygen therapy but instead credit how far the field has advanced over the last several centuries. Although their inference may be technically correct, it thankfully does not change the point being made in the article.

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Morphine Use in Acute Heart Failure and Limitation of Therapeutic Effort



To the Editor:

Miró et al¹ should be congratulated for their multicenter study published in *CHEST* (October 2017) analyzing the impact of morphine use on mortality of patients admitted with acute heart failure in the ED. The

interesting approach by means of propensity score matching techniques allowed investigators to adjust the analysis when confounding variables were not balanced.

The main risk of bias of these techniques seems to be if the propensity score model does not include a variable strongly associated with outcome and classification variable.² In the study by Miró et al,¹ knowing the age and comorbidity of patients included in the register, the prompt decision of limitation of the therapeutic effort, which is both related to mortality and morphine administration, has not been included as a confounding variable.

The main difference observed in mortality in the study occurs in the ED and after discharge from emergency setting, and it could be related to decisions of limitation of the therapeutic effort. In a previous study with the Epidemiology of Acute Heart Failure in Emergency Department multicenter database,³ a predictive model of short-term mortality was developed in patients with acute heart failure, and morphine and limitation of the therapeutic effort were not considered. Moreover, another recent study using Cox regression by Dominguez-Rodriguez et al⁴ was conducted in 991 patients treated for heart failure in the ED. The study showed that morphine was associated with in-hospital mortality, but decisions of limitation were also not considered.

The results of these studies can help to modify the management of acute heart failure, but it is necessary to construct well-designed clinical trials with a clear definition of use for morphine.

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