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Letter to the Editor

Reply to: Intraosseous Versus Intravenous Resuscitation During In-Hospital Cardiac Arrest



To the Editor,

We appreciate the interest shown by Shaw et al. in our manuscript. The authors raise several important points that we hope to address.

First, the authors ask us to define what is meant by the term “injection” used in the primary aim statement of our *Introduction*. By this we mean generally “injection of medications” through either a peripheral intravenous (PIV) or intraosseous (IO) device during cardiac arrest. We provide data only on epinephrine administration as this is the most consistently used medication during cardiac arrest. Additional medications were given to patients in this study with some frequency. However, information regarding their relative use was incomplete. Given this incomplete documentation, in combination with their mostly undetermined or neutral effects on our study outcomes, we felt their comparative utilization would be difficult to interpret in the context of our study.¹

Second, we are asked to clarify the patient population in our study. We use the term “acute care” to mean all those on the hospital floor, step-down unit (i.e. intermediate severity of illness between ICU and floor), and observation unit (e.g. those patients expected to stay inpatient for <72 hrs). As such, when included with the intensive care and procedural patients, we feel our study population reasonably includes those thought of as being “in-hospital.”

Third, we are asked to differentiate between those patients in the “PIV group” who had pre-existing PIV access and those who had PIV placed after IHCA. Patients with pre-existing PIV access could have shorter times-to-epinephrine (TTE) and shorter times-to-ROSC (TTR). Differentiating between these groups may also more accurately reflect the entirety of the in-hospital resuscitation process. The authors additionally request information regarding time-to-access between PIV and IO. Unfortunately, this information was not available with robust presence in our dataset. As a surrogate for venous access times, we used TTE. This variable has been associated with differences in survival and neurologic outcomes.^{2,3} As discussed in our paper, the TTE was statistically longer in the IO group compared to the PIV population and we adjusted for this variable during multivariate logistic regression.

Fourth, health care providers at our institution follow ACLS guidelines regarding IO placement during code events, which states IO access should be considered when PIV is not available.¹ No additional, institutional specific guideline exists to direct the use of IO during cardiac arrest. Contraindications to placing an IO include active infection at insertion site, ipsilateral long bone fracture or vascular injury. Although we can provide no definite numbers, these contraindications occur with such low frequency that they are unlikely to affect

outcomes in any meaningful way. We have no information regarding frequency of IO placement at humeral vs. tibial sites, although the results would undoubtedly heavily favor tibial insertion as this was the standard of care during the years of study. We agree that IO location would be important information and may confound our results as pharmacokinetics are known to differ between these two options as noted in our *Discussion*.⁴

Finally, we are asked whether patients who had an unwitnessed cardiac arrest were also included in the study analysis. We did include these individuals in our analysis. The authors note that there is a statistically significant difference in the number of witnessed cardiac arrest events between the PIV and IO group in favor of the PIV group. In fact, several studies have shown an association between witnessed status and increased odds of survival during IHCA.⁵ Thus, having a higher percentage of witnessed events in the PIV group may skew towards more favourable outcomes. We have attempted to correct for such a difference by adjusting for this variable using multivariate logistic regression, rather than performing a subgroup analysis as suggested.^{6,7}

Many of the limitations addressed here and previously should be considered when interpreting the outcomes of our paper. Nevertheless, we feel our analysis serves to add meaningful knowledge to IHCA resuscitation literature, in addition to serving as a basis for further prospective studies that would be able to address this question more fully.

CRediT authorship contribution statement

Kevin T. Schwalbach: Conceptualization. **R. Chad Wade:** Conceptualization. **Joseph Barney:** Writing-review & editing.

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All named authors meet the International Committee of Medical Journal Editors (ICMJE) criteria for authorship for this article, take responsibility for the integrity of the work and have given their approval for this version to be published.

Conflict of interest

Authors declared that they have no personal or financial conflict of interest.

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