



Letters to the Editor

Infection prevention: is copper the new gold?



Sir,

The article by Butler, 'Effect of copper-impregnated composite bed linens and patient gowns on healthcare-associated infection rates in six hospitals' has caught our attention [1]. It is a multicentre study in six US hospitals of the Sentara Healthcare group that assessed the effect of replacing regular linen by copper-impregnated linens on incidence rates of *Clostridium difficile* infections and healthcare-associated infections (HCAIs) due to multidrug-resistant organisms (MDROs). The study's conclusion is that the intervention 'resulted in a significant reduction of both HCAI caused by *C. difficile*, and the combined metric of *C. difficile* or MDRO infection' [1].

Although the study question is certainly interesting, we believe that there are several major flaws in the study design, analytical methodology, and scientific reporting, which call the results and conclusion into question. First, we are surprised that, even now, multicentre studies can be published with just one author listed, raising the suspicion that other contributors have not been disclosed. Further, Sentara states on its website that it partnered with Cupron for a previous study [2,3]. None of the Sentara studies on copper, including the present one, disclose this relationship, or any funding [1,3,4].

With regards to the design, we are disappointed to see the author apply an uncontrolled before-and-after design, the lowest standard possible, susceptible to multiple types of bias [5]. Already in 2007, the ORION statement indicated that before-and-after studies should, at the very least, have a contemporary control group or sufficient observations for analysis as an interrupted time-series [5]. Unfortunately, the statistical analysis is also suboptimal. Comparing rates aggregated over the before-and-after period yields biased effect estimates – it would have been of higher standard to conduct a segmented Poisson regression or an interrupted time-series analysis in order to control for bias and random time effects [6].

Then, there are other puzzling issues. The author states that replacement of the linen was rolled out across 12 hospitals in the Sentara network – yet only six centres were part of the current study without providing a rationale for this choice, leading to risk of selection bias.

The selection of periods of analysis is not entirely clear. May to December 2016 are compared to the same months in 2017, to take into account seasonality. Why were January to March 2017

ignored in the 'before' periods? Seasonality could have been covered better by evaluating 12 months before and after the intervention [6].

The choice of the outcomes is confusing – why were no data reported on HCAI due to susceptible bacteria? Surely, the biocidal activity of copper would extend to those organisms too. In addition, infections were counted irrespective of the causal pathway; endogenous contamination versus cross-transmission [7]. It would have been more interesting, and more biologically plausible, if the study had focused on possible cross-transmission events. It is difficult to understand how copper linens would be able to prevent infection in an already colonized patient.

Other infection prevention and control (IPC) practices in the participating hospitals were reported, but the data are puzzling. It is unclear when the IPC practices mentioned were actually implemented. Importantly, an educational programme aiming to reinforce environmental cleaning was implemented in 2016/2017, but no data are provided on the content of the programme or its timing. Furthermore, in the Discussion, the author states that, in addition to replacing the linens, Sentara had decided to 'replace hard surfaces, such as bed rails, patient and staff countertops, with copper oxide biocidal composite surfaces'. This was neither mentioned in the Methods, nor considered in the analysis. Both the educational programme on environmental cleaning and the presence of copper surfaces may bias the reported relationship between the intervention and the outcome.

The near-perfect compliance with hand hygiene measures is laudable, but questionable, especially since the quality and methodology of the monitoring were not disclosed.

The Results section is scant, and further detail could have been provided, such as the types of HCAI identified or demographics of the patient population. Table I, which belongs in the Results, contains some unfamiliar terminology, such as 'Adjusted patient-days' and 'Co-morbidity and medical complication rate', which are not further explained.

Finally, there is no mention of the cost of the copper-impregnated linens. Although the objective is not to provide a health-economic analysis, this information is of relevance to the readers.

In conclusion, it is disheartening to see that a manuscript with features suggestive of a 'marketing trial', with a weak design and suboptimal reporting, raising more questions than are answered, ends with such a strong conclusion [8].

Conflict of interest statement and funding sources

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Reply to Abbas et al. "Infection prevention: Is copper the new gold?"



Sir,

I am grateful for the opportunity to respond to the letter by Abbas et al. *Infection Prevention: Is copper the new gold?* [1] concerning my original paper [2].

First, Abbas et al. [1] comment that they are surprised to see a single author reporting a multi-centre study. In fact, the Acknowledgement section of the paper thanks the entire Sentara Healthcare Infection Prevention Team for their contribution to the study. However, preparation of the manuscript was solely this author's responsibility.

Second, it is my opinion that Abbas et al. have taken out of context the single term 'partnered' from a Sentara Healthcare 2016 press release [3] to imply a lack of disclosure. The term 'partnered' was used in the press release to describe a commercial relationship between Sentara Healthcare and a vendor company Cupron. To avoid any doubt, this author and Sentara Healthcare have no disclosures of interest.

Some of the other points raised by Abbas et al. are already addressed in the original paper [2], but this letter can provide some further clarification. The reason why this study did not include all 12 Sentara Healthcare facilities was purely that the study was focused on a subset of the hospitals that were geographically co-located, had similar patient populations, and most importantly implemented the intervention at the same time-point. As such, the inference that the selection of units was inappropriate or biased is refuted. It is agreed that longer time periods for almost any study are better, but pragmatically studies have to be performed over a defined time period. The timeframes chosen for analysis in the study were from the date of deployment of the intervention to the end of the calendar year, and the same period in the preceding year; these times were chosen to minimize seasonal impacts and any impacts of NHSN definition changes. The choice of outcomes was also based upon the National Healthcare Safety Network standardized reporting, [4] which at this time does not include susceptible organism reporting.

The additional IPC measures that Abbas et al. were unclear about, including an educational effort, were all executed as referenced in the paper at the same time as the intervention. Further the hand hygiene figures disclosed in the paper and questioned by Abbas et al. did not form part of the main study, and were provided to the reader to simply demonstrate that no

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