Review 5: "Seroprevalence of SARS-COV-2 Antibodies in Scottish Healthcare Workers"

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**RR:C19 Evidence Scale** rating by reviewer:

- **Potentially informative.** The main claims made are not strongly justified by the methods and data, but may yield some insight. The results and conclusions of the study may resemble those from the hypothetical ideal study, but there is substantial room for doubt. Decision-makers should consider this evidence only with a thorough understanding of its weaknesses, alongside other evidence and theory. Decision-makers should not consider this actionable, unless the weaknesses are clearly understood and there is other theory and evidence to further support it.

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**Review:**

The article from Abo-Leyah, Gallant1, Cassidy et al. deals with the level of immunity in the healthcare workers population of a few hospitals in Tayside, a district of Scotland around the city of Dundee and compares it to a ‘control’ population.

The background and justification for the study is really light—it doesn’t provide (even a rapid) literature review or a good description of the added value of their work.

They decided to run an observational survey, which they called a prospective study but it is decidedly not so. It was more a cross-sectional survey with prospective recruitment, since they didn’t collect longitudinal data or follow the participants up over time. There is a clear selection bias in the way they organized the recruitment—participants had to volunteer and register online (no randomization, only people who had access to the advertising could apply). Globally, the methods section is really lacking—no definitions, no sample size calculation, no hypothesis, no justification for the design. They decided to recruit in the global population to have a matched comparable arm, which is a good thing, but the selection was from an hospital blood bank (definitely not a good representation of the global population, clear selection bias again). We don’t have details on the questionnaire or at least type of data collected, apart from blood for serology testing and demographic, which we have to guess by reading the results. A statistical analysis plan is also absent.

The results section is fairly detailed. Tables are clear but are lacking key information (no confidence intervals), and the text is the same way. We find confidence intervals for some statements only. They could have analyzed risk factors associated with level of exposure through seropositivity, through multivariable analysis, but they only report univariate results. They at least provide a slightly deeper analysis of symptoms correlated to seropositivity. The same goes for the use of the matched control population: they don’t perform conditional logistic regression, the only comparison is on the overall seroprevalence, nothing more! What was the point then? This section is really disappointing considering the sample and the design.
Finally, the best part of the article is the discussion. They compare their results to the existing literature, try to explain the differences, and put their results into context. They also provide a few recommendations based on their findings—the need to implement IPC measures in non-medical areas being a fair and clear one.

They address the limitations of the study, but completely forget to mention the clear selection biases of their survey (or any bias, for that matter).

The overall feeling about this article is disappointment. They could have done so much more and so much better. The English is not up to scientific literature standards (bear in mind that I am not a native) and the structure of the paper could definitely be improved.

Therefore, my recommendation for the paper is **Major Revise**. This does not imply acceptance, but rather indicates that the revised manuscript will likely require re-review by the original Reviewers.