Reviewer's report

Title: Clinical characteristics and risk factors for enterococcal infections in Nagasaki, Japan: a retrospective case-control study

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Reviewer: Theodoros Kelesidis

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The clinical significance of this study is still unclear. As mentioned earlier this research question has been previously addressed in other studies.

"Hospitalized patients in Japan has unique characteristics compared to Europe and United States, for instance, a large amount of elderly patients and the duration of hospitalization is long, and additionally the isolation rate of VRE is quite low. “ These are not unique characteristics since there are many elderly patients in Europe and USA with prolonged hospitalization. If indeed these are unique characteristics the authors should cite an epidemiological study that has directly compared these characteristics in Japan vs Europe vs USA before making these conclusions. Rather than providing specific references they just make vague conclusions. To my knowledge there is no such epidemiological study. In any case this study is more targeted towards a Japanese Journal if this is why this study is so important.

As mentioned previously there is no attempt from the authors to compare their study with others including comparison of study design, methods and statistical considerations such as sample size. The authors do not address in the revised manuscript why their study and sample size is superior to other numerous similar studies.

The methods should be revised to clarify the definition of infection with the appropriate references as outlined by the reviewers. In addition the authors did not address the question of the reviewer about selection and misclassification biases. There are statistical methods to address this e.g. a score to quantify agreement between the reviewers of the charts etc. In addition many factors that can affect white cell counts, medications, liver disease or other comorbidities are not considered.

The matched controlled design is poorly described and there are no power sample size calculations. In the revised manuscript there are still no power sample size calculations.

The colinearity between predictors is not considered. Did the authors look for interactions? For example there is colinearity between diabetes, steroids, kidney disease etc

The authors do not address the question of the reviewer by saying “It was difficult to analyze the collinearity because there are many predictors.”. In addition multivariate logistic analysis may have biases if they did not take into account the
interactions and the colinearity between parameters.

Overall poor design, the study lacks novelty, incomplete review of the literature. The authors claim that “Structural abnormalities of the urinary tract are unique in comparison with the risk factors of VRE infections”. There is no clear pathophysiologic mechanism why the structural abnormalities of the urinary tract is a unique risk factor for VSE and not for VRE. Again this is probably a random “association” in the setting of suboptimal statistical models that do not account for colinearity, interactions and selections biases especially in the setting of a retrospective study. This is not a clinically relevant important finding and the authors claim this is a unique finding of their study. Again compared to other studies this study lacks novelty and is more appropriate for a Japanese Journal with a focus on local epidemiological data.

**Level of interest:** An article of limited interest

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests