

Review of: "SARS-CoV-2 Infection and Blood Group Associations in the United Arab Emirates"

Ahmed Bouhouche^{1,2}

¹ Laboratory of Human Genetics, Medical School and Pharmacy, Mohammed V University, Rabat, Morocco

² Department of Neurology and Neurogenetics, Hôpital Ibn Sina-Rabat, Rabat, Morocco

Potential competing interests: No potential competing interests to declare.

Review comments

Guella et al. present a study on the association of ABO groups with COVID-19 outcomes in the AUE population. The paper was poorly documented and poorly written, and all sections were very short. So it does not reach the level required to be accepted. The authors should make major revisions to improve their paper.

Introduction:

- This section contains only one short paragraph which does not provide background and conclusions on the current state of the subject at the international level. Also, the authors say that severity and mortality were analyzed but do not give any conclusions or references. They should further develop this section based on recent references, including systematic review and meta-analysis studies.

Methods:

- The authors did not provide demographic data and comorbidities for both patients and controls. These data are important to determine causal factors of susceptibility and severity to COVID-19.
- The criteria for inclusion of patients in the study are not specified, namely age limits, ethnic origin, vaccination, etc.
- There are international definitions for clinical spectrum levels of COVID-19 based on several parameters. Were those guidelines carefully considered in the classification of cases? Furthermore, in the cohort studied, there is no asymptomatic group (people positive for SARS-CoV-2 but without symptoms), only mild, moderate, and severe groups.

Statistical analysis:

- Did the authors perform logistic regressions? If so, they should specify.

Results:

- In this section, the authors speak of a frequency of type 2 diabetes of 42.8% in their cohort, which is too high in my opinion, but do not give the values by group (mild, moderate, and severe) nor in the group of 500 healthy controls. It would be good to also add other comorbidities as already noted.

- It is not clear why the authors put two subheadings 4.1 and 4.2 knowing that they are the same thing referring to the same table 2. Susceptibility and risk mean the same thing. And the statistical comparison values must be given in full (i.e.: $p < 0.001$, $OR = 2.19$ [1.53-3.13]), even when it is not significant.
- It is not possible that the 12% difference for group B between patients and controls is significant at $p < 0.0001$ and that the 10% difference for group O is not significant with a $p = 0.22$. There must be an error, and the authors are requested to check.
- In 4.3, the text is too short and does not correspond to the results of Table 3. The authors do not study severity with blood group but with Rhesus, as indicated in the legend of Table 3. To do this, they must do as in Table 2 and perform a logistic regression.
- It is necessary to mention in the results whether it was deviations from Hardy-Weinberg equilibrium or not.

Discussion:

- The discussion is too short and confusing and needs to be seriously rewritten taking into account the previous remarks.
- The authors claim that their results are consistent with those of Jawdat et al. in Saudi Arabia, but do not discuss the work of Hafez et al. 2022 in the same UAE population, which is contradictory. They need to explain this difference in results in the same population.
- It is suggested to add a paragraph at the end to mention the major limitations of this study.
- Authors are invited to specify if their study received approval from their local ethics committee; if yes, they must indicate its reference in the text.