Review of: "A Mathematical Characterisation of COVID-19 in Mauritius"

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The author conducts a simple, epidemiological analysis of the COVID-19 outbreak in Mauritius (and other countries). After providing a short time line of the specific epidemic on the island of Mauritius, the author models the cumulative incidence using a straightforward 3-parameter logistic model, where t_0 is the onset, α is the initial growth rate and M is the carrying capacity (or ceiling value). The author studies two waves in Mauritius (and other countries), but more importantly he calibrates the logistic model for several countries and makes a surprising *statistical* finding (for the first wave, respectively): Namely, α and M appear to be anti-correlated (*cf.* Fig. 3). This suggests that the total number of infected individuals created during a wave decreases provided that the infectious disease spreads more rapidly through the population initially. Unfortunately, the author fails to rationalize, explain or discuss this observation convincingly. Furthermore, additional details and discussion are necessary: Could this be an artifact of the chosen procedure? Are there particular properties of the countries/outbreaks that support a negative correlation between the two parameters? Could there be an effect due to initial *stochastic* dynamics in the disease spread, *e.g.*, it may be possible that the conditioning on "the initial spread resulting in a (full-blown) wave" has an overestimate for α as a consequence provided that this phase is dominated by stochastic effects. I hypothesize, that this effect may affect less-densely populated areas (or localities with low α) stronger, however, causes may be more subtle (demography, *etc.*). Furthermore, a more detailed analysis of "immigration", *i.e.*, the introduction of the virus from outside the studied system, is recommended. Also: May there be another unobserved, correlated variable causing the effect?

Provided that the hypothesis - anti-correlation between α and M - can be reliably substantiated, a significant rationalization should be added. The reader expects to understand how this counter-intuitive phenomenon arises. Among others routes, this may utilize findings from outbreaks on social networks. The relationship may depend on the demographic structure (contact network) of the studied population.

Importantly, the author should address whether the results suggest that the initial outbreak shall not be suppressed (maximal α) in order to dampen M, and potentially decrease the total number of casualties.

The remaining analyses are mainly descriptive: Several arguments for the dynamics in the difficult-to-estimate case fatality rate (CFR) and crude mortality rate (CMR) are provided. The changes may be due to reporting, media, strain prevalence and changing contact patterns, *etc.*

All in all, the author describes an interesting finding, however, falls short of discussing and rationalizing it in detail.

Accordingly, the conclusions are not convincing. There are still several - not many - language mistakes. The cited sources and formatting were not assessed. The analysis is straightforward. Additional investigations (*e.g.*, more countries) and significantly more discussion to support and explain the findings may render this work into an interesting contribution to epidemiology.