Reviewer comments and changes by authors

# Changes not requested but deemed necessary

* We removed one entry from our dataset because it was a duplicate of another study with a different title. This only marginally impacted the results but did not lead to any significant changes. The results section has been updated to reflect this.
* We removed the question, objective, results and discussion of the impact of vaccination as a sole intervention. We think it is potentially confusing and its results can be misconstrued and taken out of context for propaganda.
* The first two sentences in the introduction of the abstract have been revised to read, “Outbreak response modelling often involves collaboration among academics, and experts from governmental and non-governmental organisations. We conducted a systematic review of modelling studies on human vaccine-preventable disease (VPD) outbreaks to identify patterns in modelling practices between two collaboration types.”

# Reviewer comments and author responses

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| **Reviewer** | **Authors** |
| 1. Do the authors explain the reason for writing a review article in this field?  Please provide suggestions to the author(s) on how to better justify their reasons. Please number each suggestion so that the author(s) can more easily respond. | |
| Reviewer #1:  1.1 Yes, somewhat. The title is overly general and may over-promise what the systematic review is about. From the title, it appears that the systematic review will be quite broad, whereas the outlined aims indicate that the manuscript is more narrowly focused on categorizing modeling papers as to whether they included non-academic authors (and if those that do utilize different modeling approaches). I believe the idea is to highlight that purely academic teams are utilizing different approaches than mixed teams, and this could be because they aren't well enough connected to the practical aspects of outbreak response. However, this could also be because of limitations in model availability (i.e., are mixed teams using more canned "black-box" modeling programs that academic teams). | We thank the reviewer for these comments. We have now changed the title to “Mechanistic modelling of outbreak response in human vaccine-preventable diseases: A systematic review of differences in practices between two collaboration types before COVID-19”. We believe this title now captures the aims of the paper but we are open to suggestions for revision. |
| 1.2. It would also be nice to see if mixed teams produce better outcomes (i.e., more impact on policy), as seems to be suggested by the authors. That being said, I understand that impact on policy is very hard to measure. | We thank the reviewer for this comment. It is difficult to tease out the impact that modelling alone makes in outbreak decision-making as modelling only forms part of a multi-facetted approach to decision-making. In this paper, we only suggest that mixed collaborations are potentially likely to lead to decision-making due to the assumed involvement of decision-makers in the modelling processes. |
| 2. Does the review article provide a good overview of the development of the field while providing insights on its future development?  Please list the historical developments of likely future scenarios that the author(s) should add or emphasize more. Please number each suggestion so that the author(s) can more easily respond. | |
| 2.1. Overall, I found this review interesting but also thought that the title over-promised the content (as described below). It focuses more on patterns of collaboration and how some modeling practices may differ between mixed and academic collaborations (and between human vaccine-preventable diseases and FMD). However, the review synthesized a number of aspects “modeling practices” that may be more informative for those embarking on modeling projects, such as outcomes measured, which are only briefly touched upon in the paper. | We thank the reviewer for these comments. We have now included the results of the outcomes measured in the main text (See “outcomes measured” section of the Results Section, lines 398-404) as it has been identified as a useful outcome of this review. We would be happy to get feedback on how to expand on what is already included to better inform the readers. |
| 3. Do the authors adequately represent the most relevant and recent advances in the field?  Please provide suggestions to the author(s) on how to improve their reference list to include the relevant topics and cover both historical references and recent developments. Please number each suggestion so that the author(s) can more easily respond. | |
| 3.1 I could not find a list of papers included in the review, so I was unable to assess whether their search terms adequately captured relevant literature. | We originally provided a link on lines 307-310 to a csv file of the included studies but we recognise that this might be hard to access, so we have now included a pdf of the database of the included studies and the data extracted from each study (Supplementary Table 4). The table could not be printed as one table because of the large number of columns, so we have split it across multiple tables, each containing the title and year of the paper for easy identifiability. |
| 4 Is the review reported in sufficient detail to allow for its replicability and/or reproducibility (e.g., search strategies disclosed, inclusion criteria and risk of bias assessment for individual studies stated, summary methods specified)?  Please provide suggestions to the author(s) on how to improve the replicability/reproducibility of their review. Please number each suggestion so that the author(s) can more easily respond. | |
| 4.1. Yes | We thank the reviewer for affirming this. |
| 5. Is the statistical summary method (e.g., meta-analysis, meta-regressions) and its reporting (e.g., P-values, 95% CIs, etc.) appropriate and well described?  Please clearly indicate if the review requires additional peer review by a statistician. Kindly provide suggestions to the author(s) on how to improve the statistical analyses, controls,  sampling mechanism, or statistical reporting. Please number each suggestion so that the author(s) can more easily respond. | |
| Reviewer #1:  5.1. N/A. No meta-analysis was performed. Results are purely descriptive in nature. | - |
| 6. Does the review structure, flow or writing need improving (e.g., the addition of subheadings, shortening of text, reorganization of sections, or moving details from one section to another, following [PRISMA](http://protect-eu.mimecast.com/s/g2TwCx1g4cPGlyQCvhX4h?domain=prisma-statement.org) guidelines)?  Please provide suggestions to authors on how to improve the review structure and flow. Please number each suggestion so that the author(s) can more easily respond. | |
| Reviewer #1:  6.1 Yes | We thank the reviewer for their comment. We have reworded most of the paper to improve clarity and would appreciate feedback on specific areas that can be improved. |
| 7. Could the manuscript benefit from language editing?  Reviewer #1: Yes | |
| Reviewer #1: 7.1. In line 546, the authors point out an interesting divergence between academic versus mixed collaborations in that mixed collaborations tend to use more complex models. While the authors suggest that this is because the need to model more fine-scale operational aspect of policy, they also state that they "make no value judgements." I am more familiar with the FMD literature, and one reason for more "complex" models utilized by mixed collaborations is that such mixed collaborations sometimes rely on "black-box" models such as Interspread+ and NAADSM that require less coding expertise. I tend to agree with the authors that these are often over-parameterized, and the user may not have a full understanding of the assumptions or how the many different parameters may interact to determine model behavior (let alone do a through sensitivity analysis). I think it would be useful for the authors to be a little bolder about best modeling practices. | We thank the reviewer for their comment. We have reworded this paragraph of the discussion for clarity and would appreciate feedback to improve it further. In terms of best practices, we have intentionally refrained from proposing any as it is out of the scope of this study. |
| 7.2. Similarly, in the following paragraph, the authors write that FMD models are more complex because the nature of FMD spread requires the inclusion of farm structure, connectivity, etc. While that is true, one could equally argue that human diseases are equally complex (complex contact structures within schools, workplaces, etc, and complex patterns of movement), but more simplifying assumptions are made. Would human VPD models benefit from incorporating more complexity, or alternatively, would FMD models benefit from more simplifying assumptions? On a side note, perhaps the need for complexity in FMD models is not due to the nature of FMD, but perhaps the complex nature of the responses (i.e., movement control, depopulations). | We have reworded the paragraph to say “The FMD models were generally more complex than the human disease models. This is not surprising because the nature of FMD spread often requires the inclusion of farm structure, farm connectivity, and demographics to capture the disease's dynamics accurately and to capture the complex nature of the responses to outbreaks in terms of movement control and depopulations of farms (Kinsley2018). These levels of detail and complexity are often required in human disease outbreak models, but we observed a more frequent use among the FMD studies. Future studies designed to explain the differences in modelling practices of human outbreak response modelling groups might help to explain what we have observed in this review.” We thank the reviewer for pointing this out and look forward to getting their feedback on this. |
| 7.3. In line 568, the authors write that most FMD models are not validated with observed data, which is a bit unfair to FMD modellers. In many cases, models are used to simulate outbreaks in regions that are free of the disease as part of preparedness planning, so not validating the model with observed data is not out of laziness or failure to adhere to best modelling practices, but rather a necessary constraint of modelling in those regions. | We did not intend to imply that FMD modellers were lazy or did not adhere to best practice. In fact, we included FMD studies in this review to point out the great and quality work that has been done with FMD. We have rephrased the paragraph to say “The FMD models were generally not validated with observed data. This is most likely due to the lack of data on FMD outbreaks as most areas in the world are free of FMD outbreaks. The lack of data seems to have constrained FMD outbreak model validation and might serve as an opportunity to use data-free model validation techniques. However, these techniques were not captured in this review.”. As this is an important point, we would be happy to get more feedback on improving this paragraph. |
| 7.4. Abstract: The initial reference to FMD should include that this is a veterinary disease that is also managed through vaccination to provide context for why FMD was chosen for comparison. | We have reworded the sentence to say “We complemented this with a mini review of foot-and-mouth disease (FMD), a veterinary disease that is controllable by vaccination.” |
| 7.5. L587: Change "forth" to "fourth" | We have made the requested change. |
| 7.6. The introduction should be improved grammatically | The introduction has been mostly reworded while keeping the original structure and flow. We would appreciate any specific feedback to improve it further. |
| Editor comments | |
| Editor | Authors |
| I think this is an interesting study that looks at an important area to help us understand how collaboration is working in modelling to answer policy questions.  I agree with the reviewer that the title and the content are mismatched. The abstract and highlights focuses on the types of groups and collaborations involved in the modelling- I would suggest retitling with something more specific to this. | We have now reworded the title to read, “Mechanistic modelling of outbreak response in human vaccine-preventable diseases: A systematic review of differences in practices between two collaboration types before COVID-19”. We believe this title now captures the aims of the paper but we are open to suggestions for revision. |
| 1. Abstract Methods:   “whether at least one author was affiliated to the country studied, interventions, and model characteristics” Are the three later parts of the sentence, “country studied etc, interventions and model charateristics” what it is looking to see whether the author is affiliated with? Suggest clarifying. If it is, how is one affiliated with model characteristics? If it is different what characteristics were you assessing? | We thank the reviewer for identifying this confusing statement. We have reworded it to say “We extracted data on author affiliation type (academic institution, governmental, and non-governmental organizations), location studied, and whether at least one author was affiliated to the studied location. We also extracted the outcomes and interventions studied, and model characteristics.” |
| 1. P2 line 42 ish: “is there a difference between the conclusions 44 drawn about the impact of interventions, especially, vaccination in comparison with 45 other non-vaccination interventions during outbreaks?” difference between what? “Ebola” appeared for the first time in P4 Line 78 for the first time, suggest justifying it’s inclusion earlier | We thank the reviewer for their comments. The first question has been removed as it is potentially confusing and its results can be misconstrued and taken out of context.   Concerning the comment about Ebola, the sentence has been reworded to say “Aside from the WHO list, we also included Ebola because at the time of conducting this review, a vaccine had been approved for outbreak response purposes. We also included foot-and-mouth disease for comparison as a candidate veterinary disease that is manageable by vaccination and has been well modelled in the past.” |
| 1. In the section entitled: “Patterns in FMD..” The beginning says you will not be comparing between the different collaboration types, but then later on there is some. Suggest aligning for consistency. | The section has now been reworded to remove all such comparisons. |
| 1. Lin 466: Suggest “and” instead of “with” as to me these are slightly different points. | This has been reworded to “Mixed collaborations dominated the FMD literature and almost all the papers had an author in the location studied.” |
| 1. Line 470- 499: Though I agree with the majority of the points raised here, I think for the discussion of this paper, this section should be more closely linked to the results of the study. Did you find evidence for any of the points made here in your study or what further studies would be needed? (You discuss some of this in the following paragraph but it would be helpful for it to be more integrated.) | We have integrated the two paragraphs and reworded them to say “We quantified the presence of mixed collaborations in outbreak response modelling between 1970 and 2019 (Fig 3). Our results show an absolute increase in the number of papers published by mixed collaborations during the period. Mixed or interdisciplinary collaborations have been reported to be strongly associated with research impact and translation to decision-making (Deelstra2003). We, however, did not measure the impact of mixed collaborations in outbreak response decision-making and therefore recommend that future studies take this into account. Outbreak response, which is broadly an operational field, should ideally have modelling groups/collaborations that impact decision-making. The increase in mixed collaborations could suggest an increase in the uptake or recognition of modelling by decision-makers in governmental and non-governmental organizations as an outbreak response decision-making tool. There are examples of the commissioning of modelling by organisations such as the US Centers for Disease Control and Prevention (CDC) and the World Health Organization (WHO) to inform decision-making during past outbreaks of Influenza, Ebola, Zika, and Dengue (Muscatello2017). Future research to investigate the impact of such collaborations could advance our understanding of the contribution of modelling to outbreak response decision-making.” |
| 1. Line 432-434, Suggest rephrasing the latter part of this sentence for clarity | We have reworded the paragraph to read “Slightly more than half of the 25 studies were authored by mixed collaborations (56.0%; 14/25). In terms of how connected the authors were to the locations studied, almost all the papers had at least one author affiliated to an institution in the location studied (92.0%; 23/25)”. |
| 1. Line 587: Typo, I think should be: “Fourth” | We thank the reviewer for identifying this. It has been fixed. |