Reviewer’s report

Title: The potential impact of expanding target age groups for polio immunization campaigns

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Reviewer: Bryan Mayer

Reviewer’s report:

Tebbens et al. use a previously developed mathematical model of polio transmission to evaluate the role of changing polio vaccination target age-groups. The model incorporates important facets of polio transmission such as waning of immunity, OPV reversion, vaccination specificity, and SIA use. Furthermore, the model has undergone previous scrutiny and validation as described in previously published works by the authors.

I generally think the strengths of the model reside in the ability to evaluate interventions based on alternative outcomes in the given historic scenarios. I think the manuscript could be improved substantially if the authors focused more on specifically contrasting reinfection and subpopulation dynamics to support their main conclusions. My primary concerns with the research focus on the model calibration and interpretation of the model results. A limitation of the model is that generalizations to future scenarios are difficult because the parameter fitting was conducted heuristically.

Major Compulsory Revisions

1) My biggest concern with the manuscript comes from the model calibration process. The description of the model fitting is vague in this manuscript. According to the methods paragraph 4 (2nd sentence), the authors simulate the model using a set of parameter boundaries for a given scenario and calibrate the fits visually based on the incidence data. Is this process similar to their previous work, where they used an iterative process to update their parameter fits based on recapitulating key components of the data?

1.1) How would the authors propose that this process be replicated by other scientists who programmed the same model?

1.2) There is not a good sense of which of the data features they mention were given the most weight. For example, was it more important to match total cases over peak incidence? What explains the discrepancy of the reference case and the actual incident data in figures 2a and 2c?

1.3) Are all the data features that the authors use to visually fit the model listed in this paper (methods, paragraph 4, 2nd sentence)?

2) The results generated from the model are presented as absolute expectations. Besides natural stochasticity, there is also some level of uncertainty in the
parameter values that would give these results variability. For this reason, it is
difficult to compare two alternative interventions that differ by outcome measures
with a small magnitude (e.g., two day difference in elimination time). It seems two
days of variability in elimination time could occur simply due to the stochasticity
of outbreaks or by a small misspecification of the parameters. Without
incorporating statistical techniques, alternative ways to deal with this could be to
use units with larger time frames (weeks, months) or to focus on relative
measures (see 2.2).

2.1) It is difficult to assess how these results scale. For example, if 50 more
cases were observed in Tajikistan, would we expect the authors’ alternative
intervention strategies to produce the same magnitude of difference, the same
proportion of difference, or different results entirely? How would the calibration
change?

2.2) To compare results across scenarios, the authors should focus more on
relative measures (e.g., percent changes) rather than absolute measures (e.g.,
difference in days) because outbreak sizes and durations are different. These
measures are included in the tables but not in the text.

2.3) The authors predict WPV1 elimination in northwestern Nigeria in late 2014.
Is this prediction robust to realistic changes in their non-vaccine parameters
based on the uncertainties of our knowledge?

3) One important conclusion from the paper comes from expanded coverage in
certain under-covered subpopulations. What specifically could be done to identify
and target these populations?

4) It seems that pSIAs could be fairly effective to prevent outbreaks. Are there
measures based on surveillance that could be used to identify susceptible
populations that are in danger of an outbreak? Are there general features of the
Tajikistan population prior to the 2010 outbreak that could’ve been utilized to
predict an outbreak? Can the model be used to identify these features?

4.1) Is it more cost-effective to conduct yearly SIAs at optimal times based on
high transmission seasonality rather than developing an immunity surveillance
program?

5) I find the conclusions from the Tajikistan and Northwestern Nigeria scenarios
are clear but I am not sure what the inferences are from the India scenario.
Transmission in India was unique in that transmission conditions were high and
vaccination was high in certain subpopulations. How could the results from this
analysis be applied to the current polio experience in endemic countries or
countries at risk for outbreak?

6) The main conclusion of this research focuses on the interplay between
under-vaccinated subpopulations and waning immunity. Is there a way for the
authors to present the contribution of these components to the
outbreak/transmission in their three scenarios? For example, a time series plot of
the force of infection using two lines grouped by “no immunity” and “partial
immunity/reinfections.”
6.1) In Results: Tajikistan (last sentence), I intuitively agree with the authors’ assertion that the combination of boosting immunity and targeting fully susceptible individuals has important implications on the outbreak dynamics. However, I would like to see more evidence from the model supporting this result because it is possible only one of those groups significantly drives the dynamics.

Minor Essential Revisions

7) Methods. Paragraph 2. The authors should elaborate on “exponential expiry process.” Should I take it that they used the same exponential rates used in their previous work?

8) From section A2, it is not entirely clear to me how SIAs are conducted during the computational simulation of this version of the model. Is this a pulse vaccination process or further parameterization of a constant effective vaccination rate? Is the vaccination rate time-dependent to incorporate historic data of SIA dates? Because this is a new realization of the vaccination process from the previous work, it would be useful to see the updated equations even if it’s just the vaccination portion.

9) Typo (Methods: Tajikistan, 7th sentence of paragraph 1): “comparators” used incorrectly.

10) Clarity (Results: Tajikistan Last sentence of section): use of “impacts” multiple times.

Discretionary Revisions

11) The authors’ effort to link the previous publication to the current publication occasionally detracts from clarity. For example, the parameter differences between the country scenarios are described in tables in the previous manuscript but they have also been updated in table 4 of this paper. I suggest including the updated tables in the main text or in the supplement.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests