Reviewer's report

Title: Current crisis or artifact of surveillance: insights into rebound chlamydia rates from dynamic modelling

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Reviewer: Robert Smith?

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I'm not sure I understand what this paper contributes. The authors showed that they could reproduce surveillance data with a simple model (without more complex phenomenon thought to play a role in chlamydia transmission). I agree simple models are more useful to compare different scenario outcomes but if parameters were chosen in such a way that it best reproduced the data. How confident can we be in the underlying assumptions. Being able to mimic data (by adjusting parameter values that “best fit”) does not make the model right. Just because the data can be reproduced by a very simple model doesn't mean more complex factors are not at play. So in the end, how does knowing that simple model is sufficient to reproduce the data trends help us in the fight against chlamydia?

Figure 3B compares proportions of positive tests to the model. The model predicts the proportion of infected individuals in the entire population. Presumably the population of those being tested for chlamydia is different than the general population. Is this a valid comparison. Appropriateness of this comparison is especially important since the model was built in order to reproduce the comparison data best.

p.5. Last paragraph. Do we know what percent of cases were based on clinical criteria vs lab methods? Do they change over time? Does it matter?


p.6. Lines 16-17. “This provided reliable testing volume (i.e., denominator) data over the entire reporting history”. This is true only post 1991 since “Between 1983 and 1991, chlamydia test volume data was combined with another category of viral testing. Because of this, chlamydia test volume rates prior to 1991 were estimated proportions within the broader testing category.” How does this estimation impact conclusions?

p.7. Line 6. What initial conditions were used and what were they based on? How “valid” are they? Were the models sensitive to initial conditions? If so, results can be due to the initial conditions more than the actual model.

p.7. Line 16. They used a deterministic, compartmental model. Is this really appropriate? It assumes random mixing and sexual relations (necessary to result
in chlamydia transmission) are anything BUT random.

p.7. Last paragraph describing the SITRS model. Assumptions are made and their effects could be important. 1) those who are treated are immediately not infectious. It could be that they are less infectious or are still infectious but change their behaviour while treated. 2) the rate of being treated because they sought medical care or because of contact tracing is assumed to be the same but is probably not. 3) rate or return from removed to susceptible is the same for those who return to risky behaviour and those whose immunity wanes. Is this valid? Is it important?

p.8. Line 10. “Instead, we focused on the segment of the sexually active population who are at high-risk or are efficient transmitters.” What section was this? Was only their data used? If model was built for this population but then parameters were chosen to mimic the data for the general population, is it biased?

p.8. Line 15. “Setting initial parameter values”. Parameters were estimated from “Key epidemiological or review articles...between 1997 and 2007”. Are these subjective? Are they the same as the parameter values from the 80s? Presumably not. But the same values are used for the 25 year period which is probably not valid.

p.11. 3rd paragraph. “...implies that changes to current test-and-treat protocol...have significantly impacted the epidemiology of chlamydia....Although initially appealing, our analyses suggest that chlamydia rates are governed by a simpler set of epidemiological processes that have been operating throughout the entire reporting history of this infection.” I don't agree. Judging by figure 4B, there was a major increase in the percent of infectives that recover via treatment in the 80s. This “increased success of treatment” could on its own explain the peak in prevalence in the 80s (seen figure 3D). If the number of new cases (incidence) remains the same but treatment is more successful, then the total number of individuals who are infected at any given time (prevalence) will decrease. (eg. A faucet drips into a slowly draining sink (i.e. so-so treatment) at a constant rate (i.e. constant incidence) accumulating water in the sink (i.e. increasing prevalence). If we unplug the sink and allow a faster drain (i.e. more successful treatment) then even with everything else remaining constant, the sink will empty (i.e decreasing prevalence).

More generally, is it valid to build a model that predicts prevalence in such a way that it mimics incidence data? Especially if treatment options and its success have varied over time.

p.12. Last 2 lines. “One viable explanation that does not require appealing to changes in risky sexual behaviour is that members of the population are engaging in earlier “debut” into, or later “retirement” from sexual activity.” The way I understand this is that the total population is increasing (susceptible population grows). But the model assumes a constant total population (additional files)...contradiction?
p.13. Last paragraph. “....they may offer poor resolution for investigating network-based interventions and will fail to capture relations that will be important in the future. As a result, one must be careful not to view these models, or any models of a complex phenomenon, as tools that promise accurate forecasts.”

Then what is the point of this model? It is true that models can help understand the way elements of the system interact but in this case, the model was kept as simple as possible, most potentially important elements have been left out of the model (therefore we can't study how each comes into play).

**Level of interest:** An article whose findings are important to those with closely related research interests

**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.