

## ***Interactive comment on “Impact of climate forecasts on the microbial quality of a drinking water source in Norway using hydrodynamic modelling” by Hadi Mohammed et al.***

### **Anonymous Referee #1**

Received and published: 25 October 2018

The authors are commended for trying to use hydrodynamic models to forecast impacts of climate change on drinking water quality. The paper has several serious issues as shown below in the comments:

1. P. 1 Line 15 – the most important point is not a model result but why the model result is occurring. For example, why is there a 2-3 fold increase in E. Coli?
2. Line 64: “When properly calibrated” – what does that mean? What is the proper calibration for hydrodynamic models?
3. Line 71,72: English grammar

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4. Section 2.2.1 – If I understood, there was never any calibration of the SWAT model for this watershed – only scaling the hydrologic parameters to the drainage basin areas. This may be OK for a first approximation, but the error doing this must be quantified in order to see the impact on the overall model.

5. Line 138-141: So the overall model was only run for 3 separate years? The detention time of the lake needs to be adequately evaluated – I did not see the volume of the lake. If the lake volume shows that the detention time is over 1 year – then runs must be done for more than just 1 year.

6. Line 169: How was hourly cloud cover measured – this is very unusual!

7. Line 173: This equation 1 is totally unclear – is D the depth of 99 m or variable over the lake? How was this equation used? This does not make any sense – there is important information missing. How was the computation of T from this equation used? It cannot be the temperature model for the lake. This is totally confusing and incorrect.

8. Line 201-203: These equations are incorrectly written.

9. Line 206-207:  $A_x$ ,  $A_y$ ,  $A_z$  are NOT constituent dispersion coefficients.

10. Line 214: Equation is written incorrectly.

11. Line 219: Show the equation of state from Gill.

12. Line 222-225: Upwinding for constituent transport is the worst possible numerical scheme – use a model that has higher order scheme or else show that this scheme was ‘fixed’ by using an appropriately small grid.

13. Line 229: “square grids of 1 m” – Are you sure? 1m!

14. Line 257: “Accordingly, the model outputs may reflect the actual water temperature”. There is nothing presented that gives confidence that the model reflects the actual temperature. Just saying so does not mean that it is true. There needs to be a rigorous analysis of model error – which in this case is too large and shows that the

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model is not yet adequately calibrated.

15. Line 261: "the model closely predicted the profiles' – I disagree – the errors for temperature are systematic and large. The model is not adequately calibrated.

16. Line 262-273: What are error bars in measurement of E coli? There was no attempt to quantify the error in the E. Coli and relate that to model predictions which are very low. Why would the authors say the model agrees more with 2015 than 2017 for which they have data – that shows something is wrong.

17. Line 275-293 – does the model predict ice cover? If it does, how well does it model ice? The model is not useful unless ice cover is being predicted. There was no mention of ice cover in the paper.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2018-488>, 2018.