In the following response, the original review is shown in black and our response in orange.

Comment on gmd-2021-250
Laura M. V. Soares (Referee)

Referee comment on "Effects of dimensionality on the performance of hydrodynamic models" by Mayra Ishikawa et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-250-RC1, 2021

General comments:

The authors applied 1D, 2D, and 3D numerical models running with identical initial and boundary conditions to simulate hydrodynamic processes in a medium-sized drinking water reservoir. The results of the models supported a further understanding of how dimensionality affects model performance and the authors highlighted which dimensions are better suited for representing different hydrodynamic processes.

I think the authors have created a very interesting manuscript, and that the findings presented here have the potential to make a good contribution to the literature. It should be well received by model users and by a broad audience of the Geoscientific Model Development journal. The manuscript is well-structured, includes valuable and useful figures, and is based on relevant and recent literature. The models are well-described, the authors indicated which version was applied, and the results are sufficient to support the interpretations and conclusions. I recommend the acceptance of the work for publication after a number of comments have been addressed. I have few scientific questions/issues about the methodological approach and additional pieces of information that I believe should be included in the manuscript, and a list of purely technical corrections. I have outlined my comments below as either specific comments (relating to the methods/findings) or technical corrections (relating to word choice or content organization).

We thank Laura M. V. Soares for her valuable comments to improve the study.

Specific comments:

- Introduction Page 2, Lines 49–50: I suggest the authors expand a bit further that the selection of a model's dimension is ultimately defined by the research question and must be bound by justifiable simplifications to balance the variable of interest, the ecosystem, and knowledge, not making the model more complex than the data set

can support. This would help the reader understand the recommendations listed by the authors in the conclusions regarding the suitability of 1D, 2D, and 3D models for representing different hydrodynamic variables.

We agree that the selection of the model should be defined according to the research question and environment. We will add an explanation to the revised introduction.

- P 6, L 178: How could the modeled discharge of Passaúna River be calibrated? Is there a discharge gauge station on the Passaúna River? If so, please indicate in Fig. 1.

  The hydrological model and its calibration was done, and is described in a companion publication using data from a gauging station upstream of the reservoir (Ishikawa et al., 2021b). We will add this information to the revised version of the manuscript and include the location of the gauging station to Fig. 1.

- P 7, L 213: I am wondering if your approach of manual calibration is somehow biased as the models require quite different efforts to this procedure, for instance, calibration of the 1D model demands much lower effort than for the 3D model. My reasoning is that if an automatic calibration was performed applying the same calibration range and the same number of iterations in each model, the results would be better comparable. Perhaps, a brief discussion should be included about how the manual calibration could affect the results.

  As both reviewers raised concerns about the calibration process, we added a more detailed description to the revised manuscript. Although automatic calibration is available for GLM we did not use it, therefore its calibration processes was similar to that of the 2 and 3 D models. Nevertheless, it is worth to mention that 1D models are more easy to calibrate and can complement models of higher dimensionality. We discuss this issue in line 530 ff. of the manuscript.

- Table 1: Why did the authors adopt different time steps for each model? Wouldn't it be possible to adopt the same time step for the 3 models? Do you envisage how this might influence model performance?

  In the 2D and 3D models, the numerical time steps were similar (1 and 12 seconds). In the 3D model, the time step was selected based on the Courant number in order to meet numerical stability. Shorter time steps increase computational costs, but do not change the results, once numerical stability is achieved. In the 1D model, we used the recommended time step of 1 hour, which is the default value of GLM and common practice, as observed in several other works using GLM (Farrell et al., 2020; Gal et al., 2020; Ward et al., 2020; Ladwig et al., 2021).

Due to the valuable reviewer comments we tested different time steps (1, 12, 30, 60, 1800, and 86400 seconds) with GLM. To our surprise, even not presenting numerical stability issues the results were quite different using different time steps.

According to Hipsey et al. (2019) at section 2.1: "*Surface mass fluxes operate on a sub-daily time step, dt, by impacting the surface layer thickness (described in Sect. 2.2), whereby the dynamics of inflows and outflows modify the overall lake water balance and layer structure on a daily time step, dt_d, by adding, merging, or removing layers (described in Sect. 2.7).*" Furthermore, on p. 487 some damping mechanisms are applied, depending on time-steps.

Therefore, we concluded that changing the time steps in GLM changed the number of layers and affected the mixing characteristics. Thus, time step changes become a calibration parameter. According to our tests the time step which best fits to the measurements was 1800 seconds (cRMSE = 0.83°C), and simulations with Δt = 3600 seconds was in the same order of magnitude (cRMSE = 0.84 °C), see figure below. Since the analysis in the manuscript was made with Δt = 3600 seconds and error difference with the best simulation is minor, we believe that it is not necessary to change time step for a revised version.

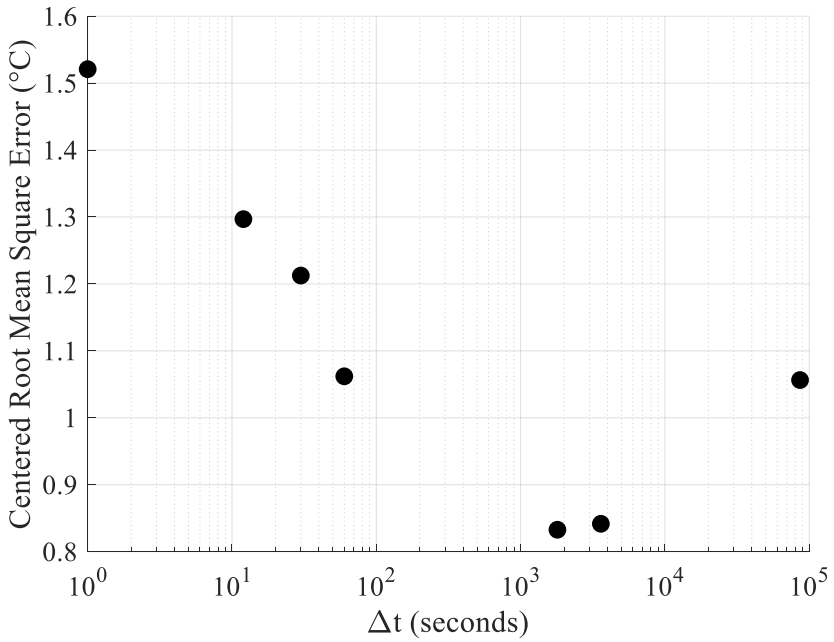

Figure 1: RMSE vs Δt (numerical time step) of simulated temperatures by GLM

The different time steps should not be a problem for the model comparison, once the models are stable and calibrated, for example the model intercomparison presented by Stepanenko et al. (2014) used time steps varying from 30 to 3600 seconds. In addition, if the input data does not have a better temporal resolution it is

not reasonable to decrease the time step, once the model is already stable and smaller time steps increase the computational time (GLM with dt = 1 sec took 55 minutes to run the simulation) and models usually interpolate linearly between the given measured time steps.

We can add this information over the calibration description that we already planned to include in the revised version. The figure can be presented at supplementary information.

- Section 5: The key point of the manuscript is the performance assessment of the hydrodynamic models based on statistical metrics calculated for the variables of interest. However, the description of the indices for comparison is somehow incomplete and must be clearly outlined. For example, in section 5 – indices for comparison, the authors present: stratification duration based on the ST, UML, temperature, and flow velocities. But the authors also analyzed other variables beyond the above-mentioned: water level, spillway discharge, evaporation rate, the formation of currents, and substance transport. Some of those variables appear in the Results section for the first time, but they should be introduced in section 5. Also, in section 5.1 – Statistics the authors present the following metrics: standard deviation, r, cRMSE, and MAE. However, not all statistics are present for the variables of interest. For example, I missed the standard deviation, r, and cRMSE for water level. In addition, other statistics were applied by the authors (coefficient of determination, percentiles, and percentage difference) and they should be stated in section 5.1. Hence, I recommend the authors describe all statistics and variables of interest in section 5 aiming at a better structure of the methods and thus the reader can better follow the results. Perhaps, adding a table synthesizing all statistic metrics for each hydrodynamic variable would help to visualize the results.

  The information about water level, spillway discharge and evaporation rate were not presented at section 5 because they are direct results (a simple time series) and not derived quantities (like stratification and currents). In the revised manuscript, we will mention all variables of interest in section 5 and add more specific information on the formation of currents. In addition, we will provide a complete description of the statistical indices that are used for comparison.

  A table presenting all variables with corresponding statistical metrics will be added to supplementary information.

- P 21, topic 6.3.3. In section 4, the authors explain that the tracers are implemented starting from 1 Aug 2018. Could you please explain the presence of the tracers in the intake region since the beginning of the simulation period (March 2018)?

It started on 1 Aug 2017, thank you for noticing the typo. It will be corrected.

Technical corrections:

- P 1, L 35: “land use” instead of “land usage”?
The wording will be changed accordingly.

- P 2, L 51: “as well as to the assessment” instead of “as well to assessment”?
The wording will be changed accordingly.

- P 2, L 67: This sentence is not a conclusion from the work of Polli and Blenninger, 2019, neither from Soares et al., 2019. I suggest removing these citations from here.
It will be removed.

- P 2, L 69-70: What do you mean by “good results”?
It will be changed to: “better agreement with measurements”.

- P 3, L 84-85: The sentence is disconnected from the idea of this paragraph and its content is more close to the idea of lines 49-50. I suggest the authors move these lines to be closer to line 50.
It will be changed accordingly.

- P 5, L 153: The format of the reference here is not correct. Could you correct it, please?
It will be corrected.

- Fig. 1: what is PPA?
The name of the monitoring point. A note will be added in the legend.

- Fig. 2: I missed the time-series of rainfall. Could you provide it, please?
Yes, this information can be provided.

- P 6, L 179: “beseflow” instead of “baseline”?
The wording will be changed accordingly.

- Table 1: What does the * mean in the second line of GLM column?
An indication that the thickness is not fixed. A note will be added at the end of the Table.

- P 9, L 226: The format of the reference here is not correct. Could you correct it, please?
It will be corrected.

- P 11, L 260: The format of equation 2 is not correct. Could you revise it, please?
It will be corrected.

- P 11, L 276: “only for the period” instead of “only the period”.
The wording will be changed accordingly.

- P 12, L 294: The format of the reference here is not correct. Could you correct it, please?
It will be corrected.

- P 13, L 334-335: Could you rephrase that line (“Persistent thermal stratification developed in spring, and retained over summer”), please?
It can be rewritten to: “Thermal stratification developed in spring and persisted throughout the summer”.

- Figure SI 1a: unit “m.a.s.l.” instead of “m”.
The wording will be changed accordingly.

- Fig. 4: Why simulation results of GLM is at 0.5 m depth rather than at 1 m depth to be at the same depth of measurements? It would provide a better comparison between (a) and (b). Also, by a visual inspection, it seems that temperature simulated by GLM is higher than the measured. If the authors use the simulated temperature by GLM at 1 m depth, the same depth of measurements, the contour plots would be better comparable.
Results from GLM were linearly interpolated to a fixed $\Delta z = 0.5$ m because the thickness of the cells change over time. This procedure should not have a great impact on the results. Indeed GLM simulated larger surface temperatures, the statement in line 355 was based on calculations of temperatures at the same depth of the measurements.

- Fig. 4 caption: “intake” instead of “Intake”.
The wording will be changed accordingly.

- P 15, L 335: “0.5 °C” instead of “0.5°C”.
The wording will be changed accordingly.

- P 15, L 359: “Schmidt stability” instead of “Schmidt number”.
The wording will be changed accordingly.

- P 15, L 363: The correlation coefficient (r) rather than the coefficient of determination (R2) is a better metric to provide a measure of the correlation between simulated and observed variables. The same is valid for P 15, L 366.
We will provide the correlation coefficient.

- Please review the citation of figures along the text. For instance, Fig SI 3 is cited in the text before Fig SI 2; Fig. 10 is cited in the text before Fig. 9; and Fig. 7 is not cited in the text.
It will be revised.

- P 19, L 419: “deviation” instead of “deviations”.
The wording will be changed accordingly.

- P 20, L 443: “was” instead of “were”.
The wording will be changed accordingly.

- P 23, L 491-492: The sentence presents results and should be placed on Results section rather than in Discussion section.
The results are just presented in another form, because they were estimated through water level presented in results section. We thought it could be good to present the result again so the reader does not need to go back in the paper and have a new perspective on it while reading discussion.

- P 23, L 492: “water level is similar” instead of “water level similar”.
The wording will be changed accordingly.

- P 24, L 529: “strength of vertical” instead of “strength if vertical”.
The wording will be changed accordingly.

- P 25, L 562: The format of the reference here (Zamani et al., 2020) is not correct. Could you correct it, please?
It will be corrected.

- P 26, L 628: The sentence lacks punctuation. Could you correct it, please?
It will be corrected.

- P 27, L 651: “large effects on subsequent simulations” instead of “large effects subsequent simulations”.
The wording will be changed accordingly.

- Could you please provide the DOI for the following references, please: Chung et al. 2014, Dai et al., 2013; Kobler et al., 2018; Lorke and Peeters, 2006.
They will be provided.

References:

Farrell, K. J., Ward, N. K., Krinos, A. I., Hanson, P. C., Daneshmand, V., Figueiredo, R. J., and Carey, C. C.: Ecosystem-scale nutrient cycling responses to increasing air temperatures vary with lake trophic state, Ecological Modelling, 430, 109134, https://doi.org/10.1016/j.ecolmodel.2020.109134, 2020.

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