

Interactive comment on “Optimizing the parameterization of deep mixing and internal seiches in one-dimensional hydrodynamic models: a case study with Simstrat” by Adrien Gaudard et al.

Anonymous Referee #1

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I have reviewed this manuscript, which is generally well written albeit with a certain level of grammatical awkwardness which the copy editors will hopefully deal with.

The content of the manuscript raises some issues, however:

1. The parametrisation of energy is introduced out of context of the original model. By reading the preceding paper I am able to see where these parameters fit in, and I understand the desire by the authors to be concise and not to repeat previously published work, however the paper should also stand on its own. I suggest that a more appropriate balance in this case would be to write out the full equation set being solved

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(which is not long) and then point readers at the previous paper for the details of the discretisation. This would make understanding the parametrisation presented much easier.

1.a. As a minor point, it is conventional to typeset subscripts and superscripts which are English words in upright text. f_{stab} conventionally means $f_{s^t a^b}$ while $f_{\text{\texttrm{stab}}}$ means what the authors intend here.

2. Verification.

There is no verification of the model: the manuscript jumps straight from explaining the parametrisation to validation (ie testing the model's effectiveness on a real problem). Given that the validation exercise involves significant calibration, it is not really possible for the reader to conclude from the paper that the implementation presented is actually correct. One would usually expect as an intermediate step, some very idealised cases where the model error is demonstrated to converge at the predicted rate. In the absence of such evidence, how is the reader supposed to conclude that the implementation is correct? Admittedly, the validation exercise provides evidence that the implementation is not disastrously wrong, but minor bugs which nonetheless affect solution quality are very common.

3. Independence of calibration and validation.

If I understand the last paragraph of page 9 correctly, the same data period was used to calibrate the models as is then used to assess the model performance. If I am mistaken in this, then the relationship between the data used for the two phases should be made much more explicit. If it is indeed the case that the same data was used for calibration and assessment, this would appear to significantly undermine the results in the paper. Using disjoint data sets for this purpose is surely necessary to demonstrate the predictive power of the model.

4. Model code availability and reproducibility.

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I set out to attempt to reproduce the results in this paper using the code provided. I failed completely. There is a litany of issues:

a. Plain GitHub URL. This is not a safe archive for model code. This is for two reasons. First, the code might disappear from GitHub tomorrow, second, it is not at all clear which version of the repository was used to create the results in this paper - and as the model is developed into the future, the current version of the code at any given time will no longer be the version in this paper. Please use the GitHub Zenodo integration, or similar, to archive the precise version of the code used in this paper in a permanent location with a DOI. See: <https://guides.github.com/activities/citable-code/>

b. Code does not build. The provided .exe file is clearly not going to be of any use on my Linux workstation, so I set out to build the code. The user manual provides no information on how to do this. The developer manual is clearly incomplete but does at least say something.

i. The first surprise is that I need FABM. This rather important dependency isn't cited in the paper. One would rather think it would be and that what FABM provides to the model would be documented somewhere. FABM did not build for me, this may be my fault although the amount of manual intervention in the build process which the instructions appear to require makes this inherently error-prone.

ii. Apparently there is a dependency on NetCDF and HDF5, however since this section is only a header, with a single sentence body indicating that the author failed at this point, I have no indication of what I am supposed to do.

iii. I then attempted to compile kepsmodel_2016.F90 using gfortran 5.4.0, knowing that there would probably be a linker problem due to the missing libraries. However, the code does not compile due to missing keps_utilities_clean.f90. Presumably this means that keps_utilities.f90 needs to be preprocessed in some way, but no instructions to end are provided.

c. Scripts are not provided.

None of the namelists, parameter files or Matlab scripts referred to in the user manual are actually present in the repository. This despite the manuscript explicitly claiming they are there. In particular, all the code required to drive PEST appears to be absent.

d. Data is not identifiable.

Neither the paper nor the repository provides enough information for the user to obtain the data used from the original sources, since no identification is provided for the datasets. The fact that the data is not publicly available further underlines the need for verification tests based on ideal synthetic data, so that a future user can establish that the code works without depending on data to which he or she may never have access.

In short, the model code does not appear to have been published in a usable way, and the information required to reproduce the results in this paper (even assuming access to data) does not appear to be present. Indeed, the GitHub commit record appears to suggest that some code was just thrown together and committed on one day in November. It seems unlikely that the code in the repository was actually used in its current form to produce the paper. This does not conform with either the spirit or the letter of GMDs guidance on code and data availability and needs to be fixed.

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