Geoscientific Model Development Discussions

Interactive comment on "Historical reconstruction of the Aral Sea shrinking by a full 3-D wetting and drying model ECOSMO" *by* I. Alekseeva and C. Schrum

Anonymous Referee #1

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Review of the manuscript: Historical reconstruction of the Aral Sea shrinking by a full 3-D wetting and drying model ECOSMO By: I. Alekseeva and C. Schrum

General ——-

As I understand it the main objective of the paper is to give a historical reconstruction of the water level and fresh water budget of the Aral Sea. This is done as an application of a 3D ocean-ice model. The model is a state-of-the-art regional ice-ocean model. In the manuscript the authors extend the model by a wetting and drying scheme. The authors apply the model in a technical skilful way.

However, I am not convinced that this is an appropriate tool for the task. Using a full 3D ocean-ice model with wetting and drying may be overkill in this situation. A simpler model would be a non-spatial resolving "box-model" or a 1D vertical model with an empirical relation between area and water level as in Figure 4. Such a model can work directly with the balance equation (3). It can treat the average heat and water exchange with the atmosphere, modified by observed sea ice fraction.

As I understand the authors argument, the main advantage of a full 3D model is an improved evaporation estimate. This may be correct, but it is only substantiated with general arguments. Except for the first years the model and Mamatov estimates of evaporation is quite close (Figure 6), and the difference may be of less importance than the tuning of the run-off and precipitation.

A weakness of the results is that they are presented without estimates of uncertainty. The modelled balance terms presented in Figure 6 would be much more useful if they had error bars attached. I am not sure if a complicated model makes it more difficult to perform that kind of statistical analysis.

Answers to questions ——————

1) Does the paper address relevant scientific questions within the scope of GMD? Yes, as far as I know the journal

2) Does the paper present novel concepts, ideas, tools, or data? Yes, using an ocean model for Aral level sinking is absolutely new. The paper also presents new model development.

3) Are substantial conclusions reached? Conclusions are reached. A substantial conclusion is that it is possible to do this kind of analysis with a 3D ocean model. I am more uncertain if the results obtained substantially improves previous estimates

4) Are the scientific methods and assumptions valid and clearly outlined? As mentioned above the model may be an overkill. This does not make it invalid. The presentation

can be approved, see remarks below.

5) Are the results sufficient to support the interpretations and conclusions? Partly. The arguments for the use of a complicated 3D model should be strengthened

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Almost. Some minor uncertainties are described below.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

8) Does the title clearly reflect the contents of the paper? Yes

9) Does the abstract provide a concise and complete summary? Yes

10) Is the overall presentation well structured and clear? This can be improved with better separation between the method, data sources for comparison, data sources used and the results obtained. I also miss a separate (sub)section containing an discussion.

11) Is the language fluent and precise? The language has room for improvement

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Not so much of this. I believe equation (2) is wrong or oversimplified, see remark below.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Section 2.3 could be shortened and moved to an appendix. The description of the ice results could be shortened.

14) Are the number and quality of references appropriate? Yes, as far as I can see

15) Is the amount and quality of supplementary material appropriate? I have not found any supplementary material, and that is appropriate.

More specific comments ———————-

The introduction is good. It describes the problem with relevant references to the literature. For readers unfamiliar with the region the map in Figure 1 could be improved. In particular paths of major rivers entering the lake would be useful. It should also be made clear at what time zero depth correspond to the shore line.

2.1 Model description

The description is mostly OK with references to more details. A special problem for the Aral Sea is the high salinities mentioned in the introduction. A realistic 3D simulation must develop similar bottom salinities which aremuch higher than usually considered in ocean models. Is the equation of state used in the model valid with such extreme salinity vlaues?

2.2 Initial and boundary

Is "impulse flux" the same as "wind stress"? If this is the case, I would recommend the more common term.

2.3 Wetting and drying

I understand that this section represent an essential improvement of the model necessary for the application. However the description is long and represent a major digression from the main objective of the paper. I would suggest moving a shortened version to an appendix.

The section also has an unclear disposition. First the tresholds are given as 10 and 15 cm. Thereafter in the middle of a description of the method, some sensitivity experiments are performed and other values are chosen. No need for figure 2 as the sensitivity experiments are not described properly anyway.

Equation (2) looks a bit funny. A dry cell can easily be surrounded by some other dry cells and less than 4 wet cells.

To accomplish the systematic change in water level, the vertical grid is adjusted yearly.

How is this done after the separation in two basins? As the subsequent water level is very different, can a common grid adjustemet work for both basins, or are the grids for the two domains treated separately.

3 Model strategy ————————-

This section is not clearly written. It contains a description of data sources that should be described in 2.4. This makes the actual model strategy more difficult to understand.

I understand that the model is run several time to determine some parameters. These parameters are reduction factors for ERA precipitation and river run-off (two rivers?). What algorithm is used for parameter estimation? Or is it simply trial and error?

Parameter estimation is a major statistical subject, and when done properly can also provide estimates of uncertainty in the parameters and the results.

4 Results ————

4.1

This could be written more clearly, separating the model results from other estimates and moving the discussion to a separate discussion section.

The presentation of the results is cluttered with comparison with data

4.2

The subsection on ice is a distraction from the main objective of the paper. Of course, a good ice coverage is necessary to estimate heat and water exchange with the atmosphere. But a more detailed discussion on ice is not necessary here, or should be treated in a separate paper together with 3D distribution of temperature and salinity of the lake, for which the present model should be an excellent choice.

5 Conclusions ————————-

This section could take in more of the discussion that is spread out over several sec-

tions. The section summarizes the results. As mentioned initially in this review, it is not obvious to me that the "model implementation is a clear improvement for water budget". I am not convinced that further complication with a non-hydrostatic 3D model will give significant better results for the simulation of water level and fresh water input. If more complication is wanted, hydrological modelling of the river and groundwater forcing with irrigation systems and evaporating may be more relevant for the uncertainties in the data.

Interactive comment on Geosci. Model Dev. Discuss., 1, 243, 2008.

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