

Interactive comment on “Distributed modeling of land surface water and energy budgets in the inland Heihe river basin of China” by Y. Jia et al.

Anonymous Referee #1

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The paper presents the application of a land surface model to a river basin in China. I appreciate the effort the authors have made in the preparation of this paper. However, there are a number of problems with the paper.

The first problem is that it provides no advancement to our current knowledge about land surface modeling. This type of models has been applied to basins worldwide.

The second problem with the paper is that it has been calibrated and validated with discharge data alone. This means that the internal states and fluxes of the model can be quite wrong, but that the model still provides reasonable discharge results. This relates to the well-known equifinality problem, about which enough papers have been published. This has not been discussed at all in the paper.

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A third problem with the paper is that no explanation at all has been provided regarding the parameter calibration. This is going to have a strong impact on the results. In the same context, it is unclear whether a distinction is made between a calibration and validation period.

A fourth and final problem with the paper is that there are a large number of linguistic mistakes, which at some points make the paper very difficult to read.

My specific comments are:

- Abstract, line 6: hydrological and energy cycle. This should be referred to either as the hydrologic cycle, or the water and energy cycle. The hydrologic cycle encompasses both the water and energy cycle.
- Page 2191, line 5-8. There are statements that really should be substantiated by references.
- Same page, line 16-18: same remark, references are needed.
- Page 2192, line 3: Please spell HBV in full the first time it is used and provide a reference.
- This is a remark that can be made regarding almost all equations: when the variables are explained, units or dimensions should ALWAYS be provided. For most equations this is not the case.
- Page 2194, line 13: "heat conduction" should be "heat flux".
- Same page, line 15: lambda is the latent heat of vaporization!
- Page 2196, line 12-14: "The net radiation and soil heat flux corresponding to the saturated vapor pressure are used in the Penman equation while the actual soil may be unsaturated". This statement is not true. The Penman equation uses measured values of the net radiation and soil heat flux, which are consistent with reality. Maybe I understand it wrong, but then more explanation is needed.

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- Page 2197, line 7-8: "In the water body group, surface runoff is estimated as precipitation minus evaporation". This means that, if it rains on a lake, and if the rain rate is higher than the evaporation rate, there is no increase in storage in the lake, but the excess rainfall is removed as surface runoff. This makes no sense. Again, maybe I am understanding it wrong, but in that case, please provide a better explanation.
- Same page, line 24: what is a "left period"?
- Page 2200, equation 23. Where does this equation come from? What is "slope"? This does not look like the Darcy equation, which is the basic equation for groundwater flow. Please provide more explanation.
- Page 2202, line 6: What is the amount of water "transferred" from the snow?
- Same page, line 8: What is a model "debugging" parameter? Does this mean a calibration parameter? This also comes back on page 2211, line 15.
- Same page, line 20: Hydraulic "conduction" should be hydraulic "conductivity".
- Page 2209, line 6-8: Please provide the equation for rainfall.
- Page 2210, line 3-10: We really need more explanation on how the parameters are tuned. Are they tuned manually, or is some automatic calibration algorithm used? The authors perform a scenario analysis with their model, so this is going to be a very important issue!
- Same page, section 3.2.1. More explanation is needed on the calculation of the soil moisture characteristics, and on the development of the empirical formula.
- Sections 3.2.2. and 3.2.3. also need more explanation, they are unclear.
- Page 2212, line 1-10. Again, it is unclear how the parameters were adjusted. Also, it is not clear what exactly the calibration and validation periods were.
- Page 2213, line 15. Figure 5 does not show a discharge comparison, it shows a land

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use map.

- Same page, line 24-26. If the physical processes in the model are wrong, they are wrong as well on a daily as on a monthly time scale.
- Page 2215, line 5: is water "assumption" the same as water "consumption"?
- Page 2216, line 10-15: indeed, these results may be totally wrong, since they have not been validated!
- Same page and the page after: As stated above, good discharge simulations may have been obtained with totally erroneous internal states and fluxes. This has not been discussed at all.
- Page 2218, line 13-15: I have a big problem with the statement that this type of model can be applied to an ungauged basin. If one looks at the amount data needed to run this model, it is fair to say that it cannot be applied to an ungauged basin at all!
- Are all the figures from figure 11 on needed? They have not been validated at all.

Based on all these comments, I do not think that the paper is publishable.

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