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Discussions

Interactive comment on "Using altimetry observations combined with GRACE to select parameter sets of a hydrological model in data scarce regions" *by* Petra Hulsman et al.

Anonymous Referee #2

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This paper by Hulsman et al., presents and compares several strategies for using remote sensing data for the calibration of a distributed hydrological model. It is an interesting contribution, as the poor availability of insitu data is certainly is a problem for the application of hydrological models in many regions in the world as the African test-case presented here. + the strategy of gradually reducing the parameter space of plausible simulations is also quite interesting. However in its present form the manuscript raises several questions that are listed below:

1/ My main comment is that the general strategy followed for the model calibration lacks legibility. The 7 (?) calibration strategies tested are explained at various places

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in the manuscript (3.1, 3.3.1-4, again in 4.1.1-4) with a lot of redundancy and at the same time partial information here and there. We don't really understand how the strategies interact (are they all independent from each other). For example it is not clear in section 3 whether the altimetry and water level strategies were applied after the GRACE strategy or independently. Is there a reference strategy to which all other strategies are compared? We lack also information about the objectives behind the technical setup of each strategy (what are the assumptions tested, why)? I think that a synthetic table presenting the strategies and how they are linked to each other would be very informative.

2/ I have doubts on the interest of the "water level" strategies presented in the paper. They don't correspond to the title of the paper that mentions only GRACE and altimetry data. If I understood correctly, these strategies correspond to using the water level time series of the gauging station instead of the discharge data. Since the discharge data are available, what is the interest of these strategies? Is it just about reconstructing a rating curve using Google Earth cross – sections? Why not, but there is really no need to involve a hydrological model in that case. I think that the authors should question the interest of presenting these strategies in the paper, and if yes explain how they relate to the other strategies and what they bring for the use of satellite altimetry data.

3/ About water level based calibration : as shown by the results (Altimetry strategies 1 and 2) and discussed by the authors (p 25, l. 620-625; p26 l. 649-653), calibration of models directly on water level data generates additional uncertainties associated to the level – discharge transformation. Have the authors considered separating the problems by 1/ tackling the altimetry water level – discharge transformation issue (without hydrological model) 2/ considering the model multi-station calibration on discharge. It would bring a clearer theoretical framework, by separating the uncertainty sources (see for example Renard et al., 2010). Moreover, there is already a rich literature corpus on each subject, to which the authors could relate. I think this could be worth a discussion.

Renard, B.; Kavetski, D.; Kuczera, G.; Thyer, M. & Franks, S. W. (2010), 'Understand-

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ing predictive uncertainty in hydrologic modeling: The challenge of identifying input and structural errors', Water Resources Research 46(5), W055521.

4/ More information should be provided in the paper about GRACE, for the readers not familiar with satellite products. In particular, readers need to understand how the GRACE water storage anomalies (what is it exactly)? can be compared to total water storage in the model (not even speaking about calibration).

5/ Many performance indicators are used in the paper and not always explained / justified. The use of NSE on variables like water storage of flow duration curve seems a bit strange, as these variables behave very differently from discharge time series for which NSE is defined. Similarly, the general performance indicator for signatures combines NSE values and relative error values. Again, it is not clear to me how this indicator can be interpreted. What is the added value of using such complex indicators instead of more direct relative errors?

6/ In the model presentation it is not clear how the flow routing in the hydrographic network is computed – or is there any channel routing at all? This is quite important to know in the context of calibration with water level data (see also remark 3/).

Minor comments: - A table of presenting the parameters (+ how many parameters and which ones were calibrated for each strategy) would be useful in the main text, instead of the detail of all model equations - Provide a table with a clear list of signatures + associated performance criteria – the reader is left to guess what goes with what when it comes to presentation and interpretation of results. - p 11 l 253: what are type II errors? - p 13 l345-350: the authors present a Distance as performance criterion like Eq 3, but there are only water levels in this strategy? Were signatures calculated here as well? - Table 4 is confusing. Why are the criteria different for each strategy in the "model efficiency" column?

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