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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Distributive rainfall/runoff modelling to determine runoff to baseflow proportioning and its impact on the determination of the ecological reserve" *by* Andrew Watson et al.

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General comments:
The manuscript adapts a previously set-up surface water model by incorporating calibrated aquifer hydraulic conductivity values from a distributed groundwater model. The adapted model, representing the four main river tributaries upstream of a South African estuarine lake, is then used for calculating daily river- and base flows for each tributary during the period of 1987-2017. The model outputs are used for calculating tributary contributions, variability and flow exceedance percentiles – representing the

dynamics of this particular water system's flow regime. For instance, this information can be of use when determining the amount of water that should be reserved for ecological purposes (in the manuscript called 'the ecological reserve'). The results showed that the average streamflow influxes were not able to meet the approximated evaporative demand of the lake, highlighting the importance of high-flow events for regenerating the lake's water level and ecology.

I welcome model attempts to provide the data needed to maintain the ecological status of water systems. Indeed, this is particularly relevant for data scarce and ecologically sensitive areas. A comprehensive understanding of the flow dynamics, including both surface and subsurface flows, is needed to understand how to preserve ecological functions. To provide examples on how to combine results from two commonly used models (here MODFLOW and J2000) is also judged to be of value for the scientific community.

The intention of this manuscript is therefore relevant and important. In sum, I agree that the conclusions regarding the importance of high-flow events, illustrating the importance of wet cycles when maintaining biodiversity, is relevant. Nevertheless, I unfortunately have some major concerns with the manuscript. I would therefore like to encourage the authors to make som major revisions. This review will outline two main areas of improvement needs found with the manuscript, related to: 1) Methodology, and 2) Clarity.

Specific comments:
1) Methodology
1.1) The authors are encouraged to explain why a combination of these particular models are selected for this particular modelling challenge. What other options exists for combining distributed surface water modelling with distributed groundwater modelling?

1.2) The description of the study area states that agriculture is the dominant water user from the sub-catchment. But following this, the manuscript does not take agricultural expansion into account. Would not the increased irrigation in the area affect the streamflow data used for calibration, bringing non-stationary patterns? Is the water use taken into account in the model and how has water use developed during the simulation period? Also land-cover changes would be relevant to take into account, since the land cover data was only based on data from 2009. This is particularly important, to at least discuss, due to the fact that the manuscript presents agricultural expansion to be one of the major threats to the lake.
1.3) The estuarine nature of the lake is not taken into account or discussed. The evaporative demand is merely roughly approximated in the discussion. I wonder why this was not made with more care, since some of the main conclusions in the manuscript rely on this evaporative demand. The mix of salt and fresh water must mean that there is a dynamic flow exchange between the ocean and the lake. Please consider to explain/discuss why is this not taken into account in the modelling.
1.4) The authors are suggested to explain why the two areas for surface and sub-surface calibration was selected. The authors are also encouraged to discuss the implications of the calibration and validation limitations, especially in relation to the major calibration data gap and the fact that the measuring gauge had an upper measurement limitation.

2) Clarity
2.1) One of the main issues with this manuscript is its unclear aim. This is suggested to be written in a more clear and concise way. Following from this, the distinction between general information (e.g. model equations in the J2000 model), previously done work and the novelty of this particular manuscript becomes fuzzy. The authors are strongly recommended to make this clearer. For instance, when describing the water balance calculations in chapter 3.3, it is also necessary to clarify what information is general for the software used and what is specifically chosen for this study.

2.2) A majority of the chapters would benefit from being written more concise and to-the-point. General model information could be left out with reference to the model documentation, the model settings and the results would benefit from being presented in tables.
2.3) Chapter 4.2.2 is describing the surface water calibration. But the section describing the groundwater calibration is missing (or possibly it is just the headline that is missing). This is a gap that is suggested to be highly relevant for this manuscript.

Technical corrections:
-It is difficult to distinguish the colours in the hydrogeological map in Figure 2.
-The text description of Figure 4 has an error; the period of validation should be for 1994-2006.
-I would encourage the authors to more carefully describe why this particular lake system was selected for the case study. The manuscript states that the estuarine system is “under threat from climate change and agricultural expansion”. The term “under threat” is vague, and the statement is not referenced.
-The authors are referencing to their own unpublished work. This reference is furthermore not included in the reference list. This is problematic with regard to transparency, since no access to this source is given. The authors are encouraged to consider other ways of providing this information, for instance through supplementary materials (if possible).
-There is a general issue of missing references, for instance the geological data in chapter 2 and the parameter values in chapter 3.3.2. The reference list needs to be revised, at least one reference is missing (Sigidi, 2018).
-The appropriate number of significant figures should be revised. It is not reasonable to give exceedance percentiles with six significant figures (Table 3), due to uncertainties and limitations in input data and models.
-The headline for chapter 4 is missing.

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