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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.

Anonymous Referee #1

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Many of the references in the introduction are quite old, both those that refer to environmental flow requirement methods, as well as those referring to rainfall-runoff modelling approaches. I would have expected to see more references to the uncertainties inherent in hydrological modelling in a paper where there are limited gauging data to calibrate, assess and validate the model. The paper refers to Veloronvlei as both a lake and an estuary, which is it? How often is this water body linked to the sea and therefore how often is the water level influenced by sea water inputs? This is not mentioned in the paper at all apart from a passing reference to a sand bar. The paper also

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makes no mention of whether the lake/estuary receives any direct inputs from groundwater and while this may not be the case, this issue should at least be addressed as part of the simulation of the water balance. The introduction refers to setting the ecological reserve, but it only becomes clear later that the paper is focused on the reserve for the lake/estuary and not for the rivers themselves. Equation 1 provides, what appears to be, the overall water balance equation for the model but makes no reference to a groundwater component. The way in which the model is described is incomplete and yet a lot of detail is given. A flow diagram would have helped and I had to go to the journal of hydrology paper to get a real sense of how the model actually works. Why is slope considered to be a major forcing parameter of recharge? Recharge is largely a vertical drainage process and would be influenced much more by the drainage characteristics of the material in the unsaturated zone than the surface slope of the topography. Section 4.1.2 indicates that the recharge estimates of the rainfall/runoff model were based on the estimates from MODFLOW – this is equivalent of calibrating one model against another and the validity of this approach needs to be further supported and the inherent uncertainties discussed. Line 372 suggest that the MODFLOW recharge values were 'validated' with J2000 recharge estimates. You cannot validate one model against another, all you can say is that the two models were in broad agreement. Page 16 (and elsewhere) refers to an apparently non-standard use of the Nash-Sutcliffe efficiency statistic and refers to Watson et al., 2018. However, in neither of these papers could I find a definition of these (E2 and E1) statistics. If they are not the standard statistic then they need to be defined. Nowhere in the paper could I find mention of how water use and its impacts on the gauge data (and the inflows into the lake from the ungauged catchment) have been taken into account in the model. At the same time, Google Earth clearly indicates that there is extensive water use in the catchment (centre pivots, farm dams etc.) that are likely to affect both surface water and groundwater dynamics. The indications are therefore that the model has been setup to represent natural conditions (i.e. ignoring water use), while it has been calibrated against an observed record that reflects water use (the same comment

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applies to the earlier paper published in Journal of Hydrology). The presentation and discussion of the streamflow and baseflow results and other results (5.1 to 5.4) would have been clearer if presented in a table(s) supported by some explanatory text. I am surprised that the authors did not do some time series water balance modelling of the lake using the simulated inflows and reduced inflows to represent a 'reserve'. This would have avoided all the simplifications about an average evaporation loss. This would have been simple to do using a reservoir model. The reservoir model outputs using the simulated inputs could then have been converted to depths and compared with the observed depth data offering an additional method of assessing the model results. While some bathymetry data would be needed, I am sure some estimates could have been made, even if detailed bathymetry data are not available. Page 32 suggests that the 95th percentile is the ecological reserve percentile. This is simply not true. In South Africa (and most other countries) the reserve (or EWR) is expressed as a variable flow regime and never as a fixed FDC percentile. I am afraid that the whole discussion about the reserve indicates that the authors have little understanding of how reserves are estimated in a South African context.

Overall, the stated context of this paper is the determination of the ecological reserve, as supported by rainfall/runoff modelling. However, the environmental flow parts of the paper are far too simplified to justify publication. The paper therefore ends up being mostly focused on the hydrological model. However, there are not enough details provided in this paper to really assess the model or the results and heavy reliance is made on references to an earlier paper published in the Journal of Hydrology. I therefore do not see that this paper submitted to HESS adds anything new that is of scientific or practical relevance. Given that that are many other deficiencies in the manner in which this study has been conducted and presented, I have to recommend that this paper should not be published.

Other specific comments: Page 6: The paper refers to the catchment as a sub-catchment of the Olifants/Doorn quaternary catchment, but actually it is not in the

Olifants/Doorn catchment at all. Page 6: Reference is made to the lake supporting Karroid and Fynbos biomes, but these are terrestrial biomes that have no connection to any aquatic requirements. The paper also attributes the dual support of these biomes to the intermittent connection between salt and fresh water, which is clearly not correct and the salinity regime of the lake has nothing to do with the terrestrial biomes prevalent in the catchment. Page 6 mentions something about the salt and freshwater regimes of the lake, which will be critical to any environmental assessment, but no details are given and later in the paper this issue is totally ignored. Page 18: How do gauging station limitations result in good objective functions? This makes no sense to me. A casual glance at Fig 4 does not seem to support the conclusion that there were more gauge exceedance in the calibration period relative to the validation period. It would have been better to state how many were in each period. It is also not clear what the modellers did with these periods (set the observed data to missing values perhaps?). I could not find Table 1 in the submission. The title of Figure 6 does not seem to make grammatical sense? Page 28 says 'BFI values are generally below 1'. In fact BFI values are defined by baseflow/total flow and therefore are ALWAYS less than or equal to 1.

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