

# Assessment of Hydrology Estimates from ERA5 Reanalyses in Benin (West Africa)

By : Bodjrènou et al. (reference: hess-2023-311)

## Responses to Anonymous Referee #1

*We deeply thank the anonymous reviewer for his time and constructive revisions, comments and questions on the manuscript. In the revised version of the manuscript, we will consider all the comments and suggestions made by the reviewer. We strongly believe that the paper will be greatly improved and hope that the reviewer will agree with changes made.*

Before revising the article, we would like to briefly clarify the fundamental comments released by the reviewer

**RC1 - Comment 1)** The first concern is the novelty of the research. The primary approach of comparing ERA5 and ERA5-Land data at a regional scale with point station data is a method that has been extensively and comprehensively explored in the literature (e.g., <https://doi.org/10.5194/hess-25-17-2021>, <https://doi.org/10.1016/j.jhydrol.2024.130649>). The manuscript does not provide a sufficiently novel contribution to the body of knowledge that would justify its publication. The study follows a very straightforward assessment methodology without introducing new insights into the assessment process, analytical techniques, or applications that could significantly benefit the scientific community or advance the field of hydrological modeling in West Africa. For example, the main conclusion "This study does not identify any reanalysis that is better than another, both spatially and monthly scale. Nevertheless, this study indicated that the choice of reanalyses must rely on their performance and the given water cycle element. Correcting the variables of these reanalysis could also improve their performance." in the abstract is somehow well-known principle in hydrology science.

**Answer of authors:** Firstly, the reviewer states that the method used has been thoroughly and exhaustively explored in the literature and that this publication should introduce new knowledge about the assessment process, analysis techniques or applications that could significantly benefit the scientific community or advance the field of hydrological modelling in West Africa.

We believe that the novelty of an article cannot be judged solely by introducing a new method. It is possible to use an existing method given the data available and the objectives of the study. In the case of our study, the main aim is not to compare methods, nor to invent a method of comparison. However, we are open to any other proposal for evaluating and analyzing the data that the reviewer may put to us. We would also like to point out that, unlike the studies that deal with the evaluation of meteorological data from reanalyses, the evaluation of hydrological data, and in particular all the terms of the hydrological balance, is very rare and very little explored. Even less so, since this is the first time the study has been carried out in the study area.

With regard to the main conclusion cited by the reviewer as an example, we agree that this set of sentences can be a little confusing. We will make sure care to reformulate these sentences in the revised version. In advance, we would like to share with you the table below, which shows the reanalysis judged best according to the scale. In light of this result, we cannot describe a re-analysis as better if we are on a spatial scale. Similarly, we cannot conclude that ERA5 is better or worse at the temporal scale for all the variables. However, as indicated above, we will make sure to rephrase these sentences.

We might suggest using ERA5 to calibrate models on ETR and Runoff. But we suggest LAND for SWS and WTD, as indicated in the following summary: <https://doi.org/10.5194/egusphere-egu24-12160>

Scale	Runoff	ETR	WTD	SWC
Spatial	ERA5	ERA5	LAND	LAND
Temporal	ERA5	ERA5	LAND	LAND

**RC1 - Comment 2)** My second concern or curiosity is about the method of directly comparing ERA5(-LAND) runoff data, which does not include routing, with observed discharge data. The conclusion such as "Analysis of the spatial variability of runoff over the whole country gives the impression that there are no rivers in Benin" is weird because fundamentally, the runoff products are unrouted data that do not consider the lateral connectivity of grid cells through the river channel. To make a more meaningful comparison between the routed runoff and observed "discharge/streamflow," I recommend the authors refer to <https://doi.org/10.5194/essd-12-2043-2020>.

**Answer of authors:** We're flexible about using another method to assess throughput, but we think it's ideal to use the present method, which is based on the principle of data comparison. In this way, a unique method is used for the analysis of all variables. The second reason which justifies the choice of this method for the evaluation of the Runoff is as follows. We discovered in the data source that:

« The units of runoff are depth in meters of water. This is the depth the water would have if it were spread evenly over the grid box. Care should be taken when comparing model parameters with observations, because observations are often local to a particular point rather than averaged over a grid box. Observations are also often taken in different units, such as mm/day, rather than the accumulated meters produced here ».

This description led us to reduce the observation values to the same unit as the reanalysis data. In the case of our study, the data presented for each basin outlet is in m<sup>3</sup>/day. They are converted to m/h by dividing by the surface area of the basin and the corresponding conversion factors.

The description of the reanalysis flow is available on the following link:

<https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5-single-levels?tab=overview>

The reviewer also indicated that he had concerns about certain parts of the article. He referred to section 3.5. In his opinion, the annual verification of the balance is insufficient. First of all, we want to stress the importance of presenting the closure of the water balance. In fact, it is necessary in our study to decide on the choice of all the variables in any reanalysis. In the case of our study, we noted that neither the ERA5 nor the LAND balance is closed. Consequently, in future hydrological studies in this environment, we will be able to calibrate our model with the Runoff and ETR data from ERA5. However, the model can only be calibrated with LAND's water table depth (WTD) and Soil Water Content (SWC) data, as LAND appears to be the best performing reanalysis on both the spatial and temporal scales (see table above).

What's more, as the balance is not closed, there are no consequences in terms of improving the performance of the reanalyses on a specific variable by applying correction methods.

It should be pointed out that we have verified closure on an annual scale, and we feel that this is already essential if we are to draw the right conclusions. This is the scale on which many research projects focus

in order to verify the water stock. If not, we would have chosen to present the balance on a monthly scale, especially as our study evaluated the reanalyses on a monthly time scale. We are always flexible your decisions/recommendations.

The reviewer also wonders about the nature of the water stock in the soil. The word "stock" used here is a misuse of language. We have presented the soil water content and not the stock. This word will be corrected in the next submission.

Finally, the reviewer would like to know whether layer 4 is considered in the table or whether it has been excluded as in other parts of the article. Although we have excluded this layer in the other parts of the article, we have considered it here because there are no constraints. In the case of the other parts, it is because of insufficient data that we have excluded it.