In this study, the authors attempt to calibrate the offline WRF-Hydro model for extremely low water levels and to test its performance during the 2018 drought event in the Rhine River basin, based on ERA5 reanalysis dataset and daily observed discharge data. The calibration process involved experimenting with various hydrological and lake parameters. Notably, the authors made the decision to disable the lake scheme due to its excessively dampening effect on streamflow downstream of the lake.

Overall I find that this manuscript present a nice piece of complete research that is clear and very well-written. The authors have demonstrated a comprehensive understanding of both the model and the complexities of hydrological dynamics in drought conditions. Their efforts to adjust the model to better reflect the observed conditions are commendable.

However, I would like to provide several comments and suggestions aimed at further enhancing the manuscript. These remarks are intended to foster constructive discussion and refinement rather than serve as critiques.

Major comments:

1) The abstract predominantly offers qualitative descriptions without accompanying quantitative analyses. I recommend incorporating quantitative statistical scores to enhance clarity and precision.

2) The utilization of ERA5 and ERA5-Land reanalysis datasets as both forcing and validation data for the WRF-Hydro model necessitates prior validation of their applicability within the study area. This validation is crucial to ensure the reliability and accuracy of subsequent simulations.

3) There is ambiguity regarding the estimation methodology for hydrological parameters such as REFKDT and SLOPE, derived from a land cover dataset. It would strengthen the study if the authors clarify the specific procedures used to determine these parameters.

4) The authors note unexpected model performance during calibration and validation periods. Given the focus on extreme low water events, consideration should be given to selecting a low-flow year for parameter calibration to better align with the study objectives.

5) The paper explores the impact of lake scheme variations on streamflow simulation through parameter testing and scheme deactivation. However, there lacks a detailed physical process analysis of this scheme. Furthermore, the authors should elucidate why adjustments to lake model parameters yield divergent results during calibration and validation.

6) The study attributes inaccuracies in simulated spring streamflow solely to snowmelt overestimation, neglecting to discuss other potential influences such as forcing data quality. Figure S1 indicates a consistent underestimation of snow depth throughout the simulation period, necessitating further exploration beyond seasonal dynamics.

7) While the objective of this study is to demonstrate the ability of the hydrological model WRF-Hydro-offline to simulate low streamflow observed values during the drought events, all analyses and metrics are presented for the entire year. To align with the study's focus, I recommend emphasizing discussions and analyses specific to extreme drought events in 2018.

8) Figure 7 and 8: Comparing the ERA5-Land soil moisture data with the simulation results indicates that there are obvious dry biases, especially during the low water year. However, in the previous analysis of this study, the simulation of streamflow in low water year is better than in high water years, what are the reasons for this difference? Does this mean that the model can not characterize both land surface and hydrological process parameters well?

9) Figure 8: It seems that there are some phase difference between the simulated soil moisture and ERA5-Land data. I suggest that the authors should add an explanation for these discrepancies.

Minor comments:

1) L135: The analysis of soil temperature can not be found in this paper.

2) L210: The term ‘Slope’ is identified as a soil drainage parameter; however, this is not explicitly stated in the manuscript.

3) L254: How the model's spin-up time time is set?

4) L255: There is a reference to Equation 1, which appears to be absent from the manuscript.

5) L260 and L285: The units for the variables within the equations presented on these lines are missing.

6) Line 320: Can not make sense, please rephrase it.

7) L360: It is stated that the third layer of soil moisture in the Noah-MP model is 40-100 cm. Please verify this information.

8) L370: The time period indicated in Figure 7(a) should be corrected to “2016-2017”.