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Interactive comment on "Hydrological assessment of atmospheric forcing uncertainty in the Euro-Mediterranean area using a land surface model" *by* Emiliano Gelati et al.

Anonymous Referee #2

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In the manuscript entitled "Hydrological assessment of atmospheric forcing uncertainty in the Euro-Mediterranean area using a land surface model", the authors evaluate the sensitivity of simulated top layer soil moisture, leaf area index (LAI), and streamflow by the SURFEX-CTRIP model system over Europe given five meteorological forcing data sets. The SURFEX-CTRIP model system is based on the land-surface model ISBA-A-gs, which is based on a biochemical model to simulate the interaction between the soil, biosphere, and atmosphere. The goal of the study is to assess the uncertainty in model simulation that can be attributed to the forcings. The forcing data sets are ERA-Interim reanalysis (ERA-I), ERA-I with precipitation bias-corrected to monthly values by GPCP (P-ERA), WFDEI, PGF, and a reference data set based on several observational data

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sets. The authors report, in general, little impact by the different forcing data sets on bias and correlation of simulated values. Exceptions are PGF for simulated soil moisture and P-ERA for streamflow. The motivation of the study is to assess the modelling tool for planning human activities involving freshwater resources (i.e., integrated water resources management). I think this motivation is odd because normally, hydrologic models are used for this purpose instead of land surface models (LSMs). The former are designed for this purpose, they are more conceptual than the latter, and typically, parameters can be calibrated to achieve a satisfying representation of the terrestrial hydrologic cycle. On the contrary, LSMs are more physically-based, incorporate a wider range of processes (e.g., CO2-cycle), and have been developed to provide the lower boundary condition for coupled atmosphere-land-ocean models over land. Nevertheless, I think that the evaluation of a LSM modelling system by different forcing data sets is a welcome contribution to the field of hydrology, but there are several criteria that have to be met to provide a meaningful analysis. The most important criteria is that the model satisfactorilly reproduces the terrestrial hydrological cycle. Often, streamflow is used for this assessment and the authors also compare simulated streamflow against observations at 35 gauges (locations are shown in Figure 1). The median KGE over all gauges is at most 0.4 with at least 20\% of the gauges having a negative KGE, indicating a poor representation of the surface hydrology. This holds for all forcing data sets, which indicates that the poor performance is independent of these. As can be seen in the bottom row of Figure∼S1 in the supplements, most of the better performing gauges are nested sub-catchments of the Danube, Rhine, and Elbe river. These represent the same humid conditions and the good performance over the same area is "double counted". Other areas such as the cold region in North-Eastern Europe and catchments in the Mediterranean show significantly poorer performance. These also represent a large fraction of the European area. The North-Eastern part of Europe also experiences very poor agreement with soil moisture observations (Figure 3) and LAI (Figure 5). This poor agreement in this region does not allow any assessment of the forcing uncertainty because the model might lack important processes to reproduce the terrestrial hyrdological cycle there. This might also be due to the fact that the authors use the parameters of previous work that has been only validated in the Rhone catchment (Decharme and Douville, 2006). In conclusion, it is not demonstrated that the little differences seen among the forcing data sets are not due to the chosen model parameters (e.g., soil water parameters like porosity, saturated hydraulic conductivity and d_ice etc.), or lacking hydrologic processes in the model. In other words, it is very likely that the results are an artefact of poor model performance over large parts of Europe. The authors have to investigate the sensitivity of model parameters at the different hydro-climatic regimes in Europe and choose parameters that lead to a better representation of the terrestrial hydrologic cycle before they can assess the influence of different forcing data sets. This calibration exercise should be conducted using the observation-based reference data set.

Furthermore, main conclusions are not supported by the results. I would like to give two examples for these.

1.) p. 20, l. 21 ff: The authors conclude that LSMs may progressively integrate groundwater modelling to improve the simulation of river discharge. This does not relate to the findings of this study. All the runs presented in this study include a linear reservoir to represent groundwater storage (see p. 5, l. 32f). The authors would have to present results that do not use this groundwater component to make this conclusion.

2.) p. 20, l. 24f: The authors conclude that: "Due its relevance for crop yield and water demand prediction, large-scale irrigation schemes should be embedded into LSMs". This is contradicting the findings of this study. Section 6.3 shows that the model best reproduces correlations to LAI during the maximum phenological development phase (p. 17, l. 29f). The authors further state in this section that: "This is an encouraging result for the simulation of crop yield and, in general, of the primary production of land surface ecosystems." (p. 17, l. 30f). The authors also demonstrate that LAI is systematically overestimated during the maximum phenological development. While I do agree that it is important to include irrigation in LSMs, it is not supported by these

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findings. In the case of the ISBA-A-gs model, including irrigation would lower the water stress of plants during summer (i.e., the maximum phenological development phase) and lead to even more increased GPP and LAI. The authors have to conduct additional analysis and perform runs including an irrigation scheme to make this conclusion.

Overall, the authors have to either investigate which processes are lacking in ISBA-A-gs to better represent the hydrologic cycle in cold regions and semi-arid ones. Alternatively, they have to conduct a comprehensive parameter calibration study. Both of these avenues are beyond the scope of this study. Therefore, unfortunately, I have to recommend to reject this manuscript.

References:

Decharme, B., and Douville, H.: Introduction of a sub-grid hydrology in the ISBA land surface model, Climate Dynamics, 26, 1, 65–78, doi:10.1007/s00382-005-0059-7, 2005

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