

Interactive comment on “Global meteorological drought – Part 2: Seasonal forecasts” by E. Dutra et al.

Anonymous Referee #1

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This paper evaluates the ECMWF seasonal forecast system (S4) on the global meteorological drought forecasts. The dynamical forecast shows advantage against climatological forecast. I am impressed by the detailed supplementary materials. The paper can be published in HESS after addressing a few comments below: 1) My big concern is the use of two datasets (GPCC and ERAI) to *quantify* the influence of initial conditions on the drought forecast. I agree such comparison can reflect the uncertainty to some extent, but it can not support the conclusions such as "The memory effect of initial conditions was found to be 1 month lead time for the SPI-3, 3 to 4 months for the SPI-6 and 5 months for the SPI-12". To rigorously investigate the effects of initial conditions, I suggest using climatological mean (GPCC) during the antecedent period (plus following S4 forecast) when calculating SPI, and compare it with the result from

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GPCC S4. Then we may have a sense how an initial anomaly affect the SPI forecast. Otherwise, I suggest the authors change their statement about the effects of initial condition throughout of the paper (e.g., P931 L1, P935 etc). The evidence shown in this paper only demonstrates the impacts of differences in the two datasets on the forecast. 2) Following the above comments, I think the statement like "ERA-Interim has higher RMS errors" (P929, L11), which using GPCC as the truth, is unfair for the ERA-Interim. How do we know there is no uncertainty in GPCC, especially for sparsely gauged area like Africa? It seems that the authors do not compare apples with apples. I mean, you may expect less RMS for ERA-Interim if using ERA-Interim as reference. Actually, global meteorological drought forecast has many issues, and the uncertainty of observation is one of them. I think the authors can make their points along this line. 3) P926, equations (3) and (4). Any reason using such ACC instead of the common one: correlation both in space and time? Any reference? 4) About the comments on whether global drought onset is a stochastic forecasting problem (last sentence in the paper), I think what Yuan and Wood (2013) want to illustrate is the low skill averaged globally. For example, if the GPCC S4 has hit rate as 0.3, but false alarm ratio as 0.5 (Table 2), that means if 10 droughts in the historical, the model give you 6, but 3 of them are false alarms. I do not think Yuan and Wood (2013) neglect the fact that over some region, the models are quite skillful.

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