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Hydrology and Earth System Sciences Discussions

HESSD

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Interactive Comment

Interactive comment on "On the sources of global land surface hydrologic predictability" *by* S. Shukla et al.

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This paper assesses the sources of hydrologic predictability on a global scale. Essentially they are comparing the contributions of the initial (soil moisture and snow) conditions versus the atmospheric forcing. I like the paper, it is very well written and the message is very clear. Also, the topic the authors address is highly relevant and currently an active ïňĄeld of research. I have some minor remarks regarding the methodology, which should be addressed before the paper is ready for submission:

1. with the ESP and rESP methodology you test the importance of the initial conditions versus a *perfect* forcing forecast because you use observed forcings; however you should be aware of the fact that seasonal climate forecasts are far from perfect in many

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regions of the world. I think, in comparison with these non-prefect forecasts the initial conditions would be even more important.

Response: We agree with the reviewer that seasonal climate forecasts are indeed far from perfect. In general precipitation forecast skill for example is limited at best beyond 1 month lead time (and nonexistent at even 1 month scale in some cases, such as monsoon season). The experimental setup that we used for this study makes sure that the uncertainty in the hydrologic forecast skill in the ESP experiment is derived from the uncertainties in atmospheric forcings (i.e. climate forecast skill) only and vice versa for the Reverse ESP experiment. In other words this experimental set up helps us clearly partition the relative contributions of the IHCs and climate forecast skill, in the seasonal hydrologic forecast skill, which is the intend of this study. In the past we have looked at the contribution of the uncertainty in the seasonal climate forecast skill in other studies (e.g. Mo et. al., 2012; Shukla and Lettenmaier 2013 (in review)) but addressing this is beyond the scope of this study.

2. the importance of the initial soil moisture depends strongly on the soil moisture variability which in turn depends on the water holding capacity. Given that the underlying water holding capacities used in the VIC model are probably poorly validated in many regions of the world, you should include a sensitivity experiment to assess the impact of the water holding capacities on your results.

Response: Although the VIC model has indeed been calibrated over all major global basins and it's been shown to capture the water balance of major global basins reasonably well in Nijssen et al., 2001(a) and (b), we performed a sensitivity analysis as per the reviewer suggestion. Our results indicate that the impact of change in soil depth (+/- 20 % in this case) on RMSE ratio is minimal at best.

3. you validate your forecasts against a control run, the necessarily imperfect representation of hydrology in the VIC model will therefore inevitably impact your results. At least for runoff in in some place(s) in the world you should try to validate your ïňĄndings

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with observations.

Response: As mentioned in the response to comment #2 we used the same parameters as in Nijssen et al., 2001 (a) and (b). Those studies have indeed compared VIC simulated runoff, soil moisture and snow cover with the observations, so we believe that for this study we do not need to re-validate the VIC model.

4. the ratio between kappa(SM) and kappa(SWE) could be replaced by the ratio of SWE variability and SM variability that would be more straightforward.

Response: Kappa (SM) is the ratio of initial soil moisture variability and total precipitation variability, whereas Kappa (SWE) is the ratio of initial SWE variability and total precipitation variability, therefore the ratio of Kappa(SM) and Kappa(SWE) is indeed equivalent to the variability of initial SM and initial SWE.

5. Whereas the initial conditions are more important for soil moisture forecasts than for runoff forecasts at a lag of 1 month (which makes intuitive sense), they seem to contribute less skill at longer lags. Why is that?

Response: At longer lags the influence of the IHCs dissipates and climate forecast skill starts to dominate the seasonal hydrologic forecast skill, which is why we see a decrease in relative contribution of the IHCs with the lag.

6. sections 3.1-3.3 are a bit lengthy, maybe you could deïňĄne regions and summarize all information (different start dates, different lag times) as regional means in a table

Response: We have reworded a major portion of the results section as per reviewer's suggestion. We have also now included figures showing the general pattern of the role of the IHCs and climate forecast skill over Koppen climate region, which we believe takes care of the reviewer's second suggestion as well. .

Minor comments: 7. page 1991, line 11: references are not in chronologic order line 13 & 17: change 'up to the time of forecast ...' to 'up to forecast initialization' line 26: change '... as in (...' to '(e.g. ...)'

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Response: Corrected. 8. page 1997, line 27: southern instead of eastern Europe, I think

Response: It should be western Europe. We have revised it.

9. page 1999, line 11: change 'half of US' to 'half of the US'

Response: Changed.

Reference:

Mo, K. C., S. Shukla, D. P. Lettenmaier, and L.-C. Chen, 2012: Do Climate Forecast System (CFSv2) forecasts improve seasonal soil moisture prediction?, Geophys. Res. Lett., 39, L23703, doi:10.1029/2012GL053598.

Nijssen, B., G. M. O'Donnell, D. P. Lettenmaier, D. Lohmann, E. F. Wood, 2001 (a): Predicting the Discharge of Global Rivers . J. Climate, 14, 3307–3323.

Nijssen, B., R. Schnur, and D.P. Lettenmaier, 2001 (b): Global retrospective estimation of soil moisture using the Variable Infiltration Capacity land surface model, 1980-1993. J. Climate, 14, 1790-1808.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 1987, 2013.

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