

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

### **Anonymous Referee #1**

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This manuscript presents a relevant study of value to the scientific community, in particular regarding conclusions of the relative contributions of initial hydrological conditions (here defined as only snow and soil moisture) and forecast skill on seasonal hydrological predictions on the global scale. It is a timely contribution to this field and the overall presentation is well written and clear. The length of the paper is appropriate. It is recommended to be accepted with major reviews, as I believe the authors need to do more to make clear the limitations of the study and to present the results in a manner which more strongly reflects their usefulness (to reach more substantial conclusions). It would also be useful with equations for the variables discussed in the results.

### General Comments

- The statement that only initial hydrological conditions (IHC) and forecast skill FS con-

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tribute to hydrological forecast skill is incomplete. What is the contribution of the hydrological model or LSS itself? Would results have been different using a different LSS or a global hydrological model (GHM)? For example, the SWE results should be dependent on the scheme to calculate SWE. The statement (Pp 1989, line26) should be reworded to reflect that you consider only the contribution of FS and IHC (where IHC considers only snow and soil moisture, not surface water accumulation). The relative contributions of other factors should also be mentioned in the introduction and taken up in the discussion.

- In this light, perhaps the title of the paper could be reconsidered? (to make clear that the paper mainly talks about 'Contribution of IHC and FS to seasonal forecasts at the global scale')

- IHC are dependent on the model used to predict them and the predictability of the global forcing. For real forecasting, can it be shown that sufficient estimates of IHCs can be made? This should at least be discussed to show the context of the usefulness of the results

- Are the results only relative to VIC as no truthing against observations was made? This relates to the previous statements regarding effect of model's calculation of soil moisture and snow water equivalent. If a model poorly simulates soil moisture or snow accumulation, how would this affect results?

- Why use runoff accumulated over the lead times (1 month, 3 months, 6 months)? – This may have significance for droughts, but not necessarily flooding. For flooding an instantaneous runoff may be more significant. Please discuss in paper.

- What compromises are made at global scale which could be addressed for smaller scale forecasting? Where are these compromises most likely to affect results? (For example, where lakes/regulation dominate hydrology, where global atmospheric models perform poorly, such as for the monsoon in Asia). Please include in discussion

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- Anthropogenic impacts, irrigation/extractions, regulation etc – might totally mitigate (or exacerbate) flood and drought effects – important to mention!

The presentation of results could be better. Because the forecasts across a row have same the initialisation start but different lead times, it is hard to see the effect of lead time (e.g. Fig 1a gives forecasts for January, April and June). It would be more useful to compare forecasts to the same month, with different lead times (and therefore different initialisation starts). This would ensure better comparability of the effects of lead times. If I understand correctly, Fig 1a Lead 3 (April) would therefore give a forecast for the same period as Fig 1b Lead 1 (May). At these different lead times for the Spring melt season in the northern part of the northern hemisphere, its seems there are substantial differences in the contribution of IHC and FS.

- Many generalisations about the northern and southern hemisphere are made. The generalisations seem hardly useful and often inaccurate. Results should be related to climate, geographical characteristics or at least continents to be more useful. Referring to high latitude northern hemisphere is better than just northern hemisphere. In general, I think you would make the results much stronger by relating variation in results with variations in climate (perhaps Köppen regions) and physiography.

- I suggest to present the evaluation of Soil moisture and snow equivalent first, because these in turn influence runoff (CR) and kappa. It should also be mentioned that you are using soil moisture initialisation to predict soil moisture, so it is clear that at short lead times IHC should dominate. Here you are looking at ‘drift’ away from model initialisation. Perhaps regions that are wet have low IHC effect, indicating that change in soil moisture conditions is faster here?

- Why show both kappa and CR if they are related first order. Where do the results differ and why?

- In generally, the results and discussion sections are somewhat mixed up. Try make a clearer division between them. As shown above, there is a lot more to be discussed.

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## Content Review

Abstract, line 16 – The statement "Northern (Southern) hemisphere" – is confusing. (In general the use of parentheses to indicate opposite relationships throughout the paper is rather confusing. Suggest rewriting.)

Pp 1989, line 26. – Forecasting skill is only attributed to initial conditions and the forcing forecast skill. What about the hydrological model skill? I think it is only fair to state that you look at the relative contributions of these aspects. It should be clear that many other aspects are important (as indeed is mentioned in the discussion)

Pp 1990, line 3 – Do you mean "where snow dominated the WINTER runoff predictability"?

Pp 1990, line 20- The study of Koster et al 2010 and Manahama et al 2011 is mentioned, but what did it contribute? What was effect of the hydrological model used (given that they used an ensemble)?

Pp 1991, line 13- "... a method widely used for seasonal hydrologic prediction that runs a physically based hydrology model up to the time of forecast using observation-based atmospheric forcings, then resamples ensemble forcing members from sequences of past observations so as to form ensemble based hydrologic forecasts that are based solely on IHCs (no FS)." - Please make this statement clearer. Where do the resampled ensemble forcing members come from? Is the ensemble made up of single historical years?

Pp 1993, line 5 – I think you need to make clear the limitations in doing this! (see general comments)

Pp 1995, line 1 – Kappa: do you mean the standard deviation from the spatial variability of soil moisture or the temporal variability? Is it the standard deviation of precipitation the ratio is calculated with or total (not clear from text), perhaps an equation would be better (I see this is somewhat resolved in line 10 of pp 1996, but it should be better

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explained from the beginning)

Pp 1995, line 15 to 20 – it seems rather broad to attribute precipitation seasonality to hemisphere!

Pp 1997, lines 5 to 10 – Why? Why is FS more important at very high latitudes than IHC when in the next paragraph it is stated that snow dominant areas usually show that IHC are more important? Is it because there is no snow melt until later in the year at these latitudes? What about the non-snow dominated regions that are red? What is role of soil moisture?

Pp1997, line 23 – “That comes as no surprise” Please reword! Also smaller effect of soil moisture variability might be relevant in areas where soil moistures stays near to saturated?

Pp 1998, line 1-4 – “Overall the RMSE ratio for the CR forecasts over the Southern Hemisphere regions is around or greater than 1” . I don't agree at all. (Fig 2c for example. Also southern tip of south America and southwest Australia in Fig 2d)

Pp 1999. Line 22+ - This should be in the discussion

Technical Review Pp 1989, line 7, climate Change, small c for change Pp1990, line 26, initiation, should be initialisation? Pp 1995, lines 8-9: Writing the opposite in parentheses is rather confusing. Please rewrite. Pp 1997, line 6, the second figure reference should be to Fig 2a

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

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