Hydrol. Earth Syst. Sci. Discuss., 10, C1457–C1466, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C1457/2013/

Hydrology and Earth System Sciences Discussions

HESSD

10, C1457–C1466, 2013

Interactive Comment

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

S. Shukla et al.

shrad@geog.ucsb.edu

Received and published: 6 May 2013

Reviewer # 1

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

(1) The statement that only initial hydrological conditions (IHC) and forecast skill FS contribute 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.

Response: We have revised the statement on page 1989, line 26 to reflect that we only considered the contribution of soil moisture and snow initial conditions. We have also now cited studies that investigated the contribution of other hydrologic states in the hydrologic prediction (although we note that we already cite Paiva et al. (2012) and Singla et al., (2012) which include surface water and ground water respectively as initial hydrologic conditions). We argue however that the contribution of the hydrological model itself in the experimental setting of this study is not an important consideration. Studies such as Koster et al., 2010 and Mahanama et al., 2011 have shown that regardless of which large scale model is used the spatial pattern and the relative contribution of the IHCs to the hydrologic forecasting skill is similar, We nonetheless now discuss this in the manuscript.

(2) 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')

Response: We respectfully disagree with the reviewer's comment and believe that the title of the manuscript is appropriate. We do now specifically mention in the abstract

HESSD

10, C1457–C1466, 2013

Interactive Comment

that we only considered SM and snow initial conditions in this study.

(3) 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.

Response: We agree with the reviewer's point that in real-world operational forecasting the IHCs are depended on the hydrologic model and the observed forcings used to derive them. Uncertainties in both play a significant role in the hydrological forecasting and that is a point that we hope to highlight from this analysis. For example in those regions where in our study we found that the IHCs dominate the seasonal hydrologic forecasting skill, the uncertainties in observed forcings and the hydrologic model would significantly impact the skill. As to the VIC model's ability to provide accurate estimates of the IHCs, the model has been used extensively across the globe and it has been shown that the VIC model performs well for major global river basins (e.g. Nijssen et al., 2001 (a) and (b) and many other studies). We acknowledge that some kind of data assimilation scheme to improve the model IHCs errors , which usually can be traced mostly to forcing errors, is desirable, assuming appropriate observations are available (which is in fact the major limitation, at least in the case of soil moisture). We now discuss this in the discussion section, as per the reviewer's recommendation.

(4) 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?

Response: Yes, we conducted a "perfect model" experiment for this analysis and used the VIC simulated long term dataset of hydrologic variables as the "true" dataset. We agree that if a model simulated SM and SWE poorly it would not be appropriate for this sort of analysis and would affect the results. However as we mention in the manuscript

HESSD
10, C1457–C1466, 2013

Interactive Comment

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

(see also above) the VIC model has been extensively evaluated for various major river basins globally and generally performs quite well. It also been used to analyze retrospect hydrologic extremes (e.g. droughts and floods) globally and has been shown to perform well in this context as well. We now discuss this point briefly in the manuscript as well.

(5) 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.

Response: We now discuss this in the discussion section.

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

Response: The reviewer makes an excellent point with which we concur. We have now included a discussion on this point in the discussion section.

(7) The presentation of results could be better. Because the forecasts across a row have the same initialisation 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.

HESSD

10, C1457–C1466, 2013

Response: Our intention was to highlight the variability of the contribution of the IHCs and FS in each forecast period with the lead time, therefore we prefer to keep the plots for the forecasts made in the same forecast period together. We think that it would be hard to show that if we mixed up forecasts initialized in different forecast periods as the reviewer suggests.

(8) Many generalisations about the northern and southern hemisphere are made. The generalisations seem hardly useful and often inaccurate. Results should be related to climate, physiographical 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.

Response: We agree. We have now made more specific regional references when we discuss our results, and we mention this in the conclusions and the abstract as well. We have also included figures showing the general pattern of the role of the IHCs and climate forecast skill over Koppen climate region as per the reviewer's suggestion.

(9) 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 moistures conditions is faster here?

Response: Good idea. We have now moved the plots showing variability of RMSE ratio for SM and SWE forecasts before the similar plots for CR forecasts and also the sub-sections in the Results section that discuss those figures. We however did not change the order of Kappa parameter plots, since we intent to discuss our results on SM, SWE and CR predictability in the context of Kappa values (Please also see our response to the comment #10). The effect of IHCs depends on the precipitation

HESSD
10, C1457–C1466, 2013

Interactive Comment

variability during the forecast period. High precipitation variability during the forecast period indeed results into low impact of IHCs

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

Response: We show Kappa for mainly two reasons: First to compare with our results from the ESP/RESP approach, since Kappa parameter is a measure of the contribution of the IHCs to seasonal hydrologic predictability and is a metric that is independent from our ESP/RESP experiments. Second, we believe that Kappa parameter, due to its simple formulation, is easier to understand and helps make sense of the results of our ESP/RESP experiments.

(11) 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. Response: As per the reviewers suggestion we have now modified our results and discussion section. We now also discuss various major issues/points that the reviewer has highlighted in his/her previous comments in the Discussion section. Content Review

(12) 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.)

Response: It's been revised. Please also see out response to the comment #8.

(13) Pp 1989, line26. – 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)

Response: This comment is similar to the reviewer's comments #3, #4, and #6. Please see our response to those comments. We do now mention that addressing model

HESSD
10, C1457–C1466, 2013

uncertainty is not a focus of this study in the discussion section.

(14) Pp1990, line 3 – Do you mean " where snow dominated the WINTER runoff predictability"?

Response: No, we meant to say summer runoff predictability in which snow plays a dominant role in western U. S. We have revised that sentence for clarification.

(15) Pp1990, 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)?

Response: Those studies used multiple hydrologic models for analyzing the contribution of SM and snow in the seasonal hydrologic predictability. Their results were consistent among all models. We now state this in the Introduction section.

(16) Pp1991, 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?

Response: We have now revised that sentence for better clarity.

(17) Pp1993, line 5 – I think you need to make clear the limitations in doing this! (see general comments)

Response: We now discuss the limitations of using VIC-derived data as a reference data set in the Discussion section.

(18) Pp1995, 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 pre-

HESSD

10, C1457–C1466, 2013

cipitation 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 explained from the beginning)

Response: We meant temporal variability of initial total moisture and total precipitation over the forecast period. We have now added an equation to clarify that.

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

Response: This comment is similar to comment #12. We have now carefully revised that generalization and tried to be more specific in describing the spatial domain in the discussion of our results.

(20) 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?

Response: Indeed we suspect in those high latitude regions that snow doesn't melt until later in the year and hence doesn't contribute to the hydrologic forecast skill. In non-snow regions that are shown in red (meaning the IHCs dominate the hydrologic forecast skill) it is the initial soil moisture that controls the seasonal hydrologic predictability.

(21) 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?

Response: That sentence has been removed, since we ended up rewriting parts of the results section as per reviewer (and other reviewer's suggestion). As to the question the reviewer raises: SM variability could be important locally at a short lead time but

HESSD

10, C1457–C1466, 2013

during the rainy season over tropical regions due to high precipitation variability, at seasonal scale it is the FS that dominates the hydrologic forecast skill.

(22) 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)

Response: That statement relates to Fig. 2 (d) and the forecast period starting in October, whereas Fig. 2 (c) shows the forecast period starting in July. The contribution of the IHCs in Southern hemisphere is indeed different during both forecast periods. Also in that statement we did mention that there are some exception to that general remark in the regions that the reviewer has pointed out. However that sentence has been removed. (Please see our response to the comment # 21) (23) Pp 1999. Line 22+ - This should be in the discussion

Response: Agreed! We have now moved that statement to “Discussion” section.

Technical Review

(24) 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. Response: Those mistakes have been corrected! Thank you!

References:

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.