

## ***Interactive comment on “Predictability of soil moisture and river flows over France for the spring season” by S. Singla et al.***

### **Anonymous Referee #2**

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The authors performed modeling experiments to estimate the contributions of the initial state (i.e. Soil moisture, snow and aquifer storage) as of January 31 and atmospheric forcings in the soil moisture (SWI) and river flow prediction skill during MAM, over France. Although the method used in this study is similar to a few previous studies, the findings of this work are very valuable. I especially liked that the authors considered the contributions of aquifer storage as well, besides soil moisture, snow cover and atmospheric forcings. I would recommend the publication of this manuscript however there are some major issues and some minor comments which needs to be addressed by the authors before the publication.

Major Issues:

(1) The authors used 9 randomly selected initial states and atmospheric forcings for RIS and RAF experiments, respectively. The objective of choosing 9 random members only is to keep those experiments consistent with Hydro-SF experiment that uses 9 members of the ENSEMBLES ARPEGE meteorological seasonal forecasts. However I think that conducting these experiments (RIS and RAF) with 9 ensemble members only, may not be appropriate. The RIS and RAF experiments (somewhat similar to Reverse-ESP and ESP experiments described in Wood and Lettenmaier (2008)) are conducted to partition the hydrologic (both SWI and river flow) prediction skill coming solely from the atmospheric forcings and the initial state respectively. In order to make sure that the experiments are doing what they are supposed to do it is essential that the distribution of the atmospheric forcings (in RAF experiment) and the initial state (in RIS experiment) is unconditional (random). I am not sure if 9 ensemble members can represent the climatological uncertainty (i.e. unconditional distribution) in the initial state and atmospheric forcings in the experiments. I would recommend authors to consider using about 20 ensembles or more for the experiments (\* Li et al., (2009) used 19 members and Wood and Lettenmaier (2008) used 21 members) or demonstrate that the process the authors used to randomly select years for both experiments did not end up selecting years, which are biased towards wetter or drier than normal climatology. (For example, the authors can possibly use the same process to select 2-3 different sets of 9 random years and redo the analysis and compare the results among each set of the experiments). Furthermore it is not clear if the authors used the same randomly selected set of 9 years for both RIS and RAF experiments or different set of years for each experiments.

(2) It is not clear how the percentage of groundwater contribution to spring discharge (Fig. 4) was calculated. Please provide the relevant equations. Also how was the contribution of groundwater separated from the contribution of soil moisture and snow?

Minor comments:

(1) P 7948: Line 16-18: The value was added in seasonal hydrologic forecasts (SWI

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and river flow) not seasonal meteorological forecasts. Please revise this sentence.

(2) P7950. L2-5: “For instance. . . . . (Liang et al., 1994)”. This sentence is not clear and not entirely accurate. Not sure if the authors are referring to the hydrologic forecasts systems or the studies conducted to analyze seasonal hydrologic forecasts, but operational hydrologic forecasts system in the United States also often use Noah or SAC models. Please revise this sentence.

(3) Page 7957: Line 17-18. Please cite those studies where the Brier score has been used for the skill evaluation of hydrologic forecasts.

(4) Page 7959: Line 3-5: “Most areas . . . . . Southern Alps.” I think the authors mean that most areas where SM prediction skill came from the initial SM state did not exhibit any skill for river flow. Since the study looks at the prediction skill of both SWI and river flow sometimes the use of the term “prediction skill” or “the skill” is confusing. Please consider clearly stating if the prediction skill refers to SWI or river flow prediction skill in this sentence and several other places in the manuscript.

(5) Page 7959: Line 24: “The time correlation. . . . .” Please include the figure number associated with this finding.

(6) Please define acronyms such as SAFRAN, ISBA, MODCOU, ENSEMBLES and ARPEGE for those readers who may not be familiar with those acronyms.

(7) Page 7960: Line 5-6: “Conversely to the RAF. . . . .” Not sure what authors mean by “worsening of scores”. Please make it clear.

(8) Page 7960 and Table 2. Please explain the contents of Table 2 before discussing it. Also the conclusion based on this table “This means that, in some years, predictability appeared to come from the atmospheric forcing whereas, in other years, predictability came from land surface initial states” was hard for me to follow. Does this mean that the contributions of the initial state and atmospheric forcings in the prediction skill of river flow vary? Please specify how those years when atmospheric forcings showed higher

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contributions were different than the years when the initial state played a dominant role in the prediction of river flow. Also another comment related to the same paragraph, please consider using either the term “river flow” or “river discharge” in lines 20-21 on the same page for consistency.

(9) Page 7961: Line 4 and 25: Please consider rephrasing these sentences. For example line 4 could be rephrased as “Figure 6 (left) shows the temporal correlation between forecasted SWI using Hydro-SF experiment and its reference value obtained from SIM reanalysis.”

(10) Page 7963: Line: 10-11: “Theoretical experiments . . . . .the system”. What does the authors mean by the term “internal predictive skill”?

(11) I found it hard to read the labels on color scale of every figure except Fig 1 and 4. Also labels in Fig. 10 are hard to read. Please use bigger fonts.

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