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Interactive comment on "Seasonal streamflow forecasts for Europe – II. Explanation of the skill" *by* Wouter Greuell et al.

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We are very happy with the fact that the editor found three anonymous referees to give their highly informed opinions on our paper. We thank all three for their respective efforts to produce such extensive and constructive reviews.

We adopt most of the suggested textual improvements and specified our action to every remark in the annotated report hess-2016-604-RC3-author-reply.pdf1. RC3 raises concerns regarding a number of technical issues (RC3's numbering):

3. Streamflow persistence due to e.g. aquifer fed flow (e.g. for he UK: Svesson et al., 2015 ERL) is at least partially accounted for through soil moisture stores. It is one of the objectives of the current study to analyse whether this store is sufficiently large to attain the observed levels of streamflow persistence. We will improve the introduction

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and discussion sections on this aspect.

4. Wrt human impacted, regulated flows we concede that impacts of dams etc. on river flow have not been accounted for in our model nor in this paper. Thus, all results presented refer to naturalised flow. We will state that more explicitly. In paper I, a brief analysis has been made, partially attributing the reduction of skill between theoretical and actual to the effect of dams and other man-made channel modifications.

5. This relates to issue [4]. Actual skill here is defined against real station data, theoretical skill is measured against the reference simulation (pseudo observations, reanalysis data). This was extensively discussed in paper 1. However, we will make paper 2 more self-contained by restating such definitions and better explaining such statements, repeated from paper 1.

6. We (partially) address this issue in the discussion (fig 12 and ensuing texts). A more full exploration of these pure statistical relations between snow/soil moisture and streamflow will not inform us about the added value of using a model (since here they are model derived themselves), unless such analysis would be based on pure observational records of snow or soil moisture. That we consider beyond the scope of the present study.

7. Also raised by other RCs. We have the average R of all significant cells already available and will present them, probably as supplementary material.

8. Also RC2 made the same point. In paper 1 the other skill scores have been briefly presented, and its similarity in spatio-temporal patterns to those of R demonstrated. However we will better describe the differences observed between the various metrics. All metrics (R, RPSS, ROC AN, ROC BN) will be more fully presented in both the body text and in the supplementary material of paper 1. However, since paper 2 focuses on causes of skill, we believe it less relevant to show other metrics.

9. This part of the discussion focuses on the (potential) contribution of meteorological

forecast quality to stream flow forecast skill, showing that (partially) statistical meteorological forecasts seem to outperform those of purely dynamical state of art models, but that there is scope for improvement of the latter. A more extensive discussion on (potential progress in) meteorological forecast quality is beyond the scope of our paper (and of our expertise) . So we will try to explain better the implications of these findings, but not delve into meteorological forecast quality itself.

10. Assimilation of snow or soil moisture observations into our modelling framework is high on our wish list too, and will be future work. We can, however, try to better discuss its potential based on literature.

11. We recognize this and we will -also with help of suggestions by the other reviewers- make a serious attempt to improve and have a final check done by a native speaker.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-604/hess-2016-604-AC3-supplement.pdf

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