

Interactive comment on “Evaluation of five hydrological models across Europe and their suitability for making projections under climate change” by W. Greuell et al.

Anonymous Referee #2

Received and published: 30 December 2015

This is a well-written manuscript with two stated goals (in both the title and the introduction): a) a comparison of the performance of five hydrological models for selected basins in Europe; and b) an assessment of the suitability of these models for climate changes assessment. While the manuscript meets its first goal, it does not adequately address the second and as such major revisions are required before it can be considered for publication.

One of the stated goals of the paper is to evaluate model suitability for climate change assessment. The main argument made by the authors is that if the models can reproduce the spatial variability of runoff production as well as the interannual variability

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in runoff production, then they are likely able to model the changes that may occur in runoff generation under climate change. In the current form, the assessment of climate change suitability is rather perfunctory and includes no specific analysis of model sensitivities to changes in precipitation and temperature (other than to evaluate the different forcing datasets). At the very least, the authors will need to evaluate model sensitivities more explicitly, either through analyzing model performance in different climatic regions or through targeted sensitivity experiments.

Major comments:

1) As the authors state, most of the interannual variability in runoff generation stems from interannual variability in precipitation. Precipitation changes are not the only ones that are likely to occur under climate change. Significant changes in temperature are expected as well, with accompanying changes in evapotranspiration (ET) as well as snow accumulation and melt. The authors should analyze model sensitivity to changes in both precipitation and temperature (and separately). Because the models have different ET formulations, this may provide important insights that will significantly strengthen this part of the paper. The same is true for the snow simulations, which play an important role in a large subset of the study basins.

2) While the manuscript is explicit in its evaluation of model performance (which models perform better in what regard and where), it is rather non-descript in its discussion of the climate sensitivity, presumably because of the lack of targeted experiments (see previous comment). A more in-depth evaluation and discussion are required.

3) The manuscript lacks a clear conclusions section.

Minor comments:

a) p.10293 l.17-18: "a dry and a wet year" or "dry and wet years" (same for cold/warm).

b) p.10296 l.26: "two version" should read "two versions".

c) p.10300 eq.1: Explain what is being evaluated with this statistic. This is done later,

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but would be helpful right here (as is done for the other metrics).

d) p.10302: Move first paragraph of section 6.1 to section 5.

e) p.10305 l.20: Unclear what is meant by "reference of no flow".

f) p.10306 l.16: "Fig. 4" should read "Fig. 6".

g) p.10306 l.24-25: What is the sensitivity experiment that was performed to evaluate the frozen soil formulation in VIC? Do any of the other models include frozen soil representations?

h) p.10307 l.22-24: Low flows do not equal droughts since the latter are a deviation from climatology, while low flows can be simply a seasonal phenomenon. I suggest replacing "droughts" with "low flows".

i) Fig.8: Please include all models, since they are all discussed in the text and it is unclear why only a subset is shown.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 10289, 2015.

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