

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

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Points 1 and 2 In response, we first cite the motivation for our approach to assess the suitability of the different models to make climate projections from the paper: We propose that the skill of a hydrological model to simulate interannual variability in discharge is a good measure for its ability to make projections of climate changes in mean discharge. The reasoning for this proposal is that differences between e.g. drier versus wetter years and warmer versus colder years can be considered as an analogue for the projected climate changes. Hence, we assume that if a model is well capable of simulating the difference in discharge between a dry and a wet year (or a warm and

C6150

a cold year, etc.), it can be expected that the model is also well capable of simulating the climate change impacts on discharge. Thus, by evaluating the skill of the models to simulate interannual variability, we assess the relative skill of the different models to make projections of future change. So we clearly motivate our approach. Limitations are discussed in the second paragraph of Section 8.3. The approach also links in a fluent way to the evaluations that form the first goal of the paper, in the sense that part of the results of the evaluations have been used to assess the suitability of the models to make climate projections. Certainly, it would be interesting to perform experiments to determine the sensitivity of the models to perturbations in the various meteorological variables, as suggested by the reviewer. This would provide understanding of the differences between the models and one could indeed analyse the sensitivity not only for discharge but also for e.g. snow and evapotranspiration. Such an approach would, however, be fundamentally different from our approach and hence be outside the scope of this study. It would require performing new experiments and developing another method of translating the results (sensitivities) into suitability of making projections. Moreover, such an alternative approach would not fluently link to the validation that we have carried out. We will write in the discussion section that there are other conceivable ways of approaching the issue and briefly discuss advantages and limitations. We have analysed model performance for different climatic regions by showing different colors for different regions in the plots. Figure 5 (relevant for the issue raised here) shows no clear spatial pattern. Because of that lack and because regionalisation would yet add another layer of complexity to the analysis, we do not regionalize the results.

Point 3 We will add a short conclusions section

Minor points a-d we will change this e with “no flow” we mean “discharge = 0 m³/s”, which is an important reference in the figures, especially for those stations where streamflow approaches zero in parts of the year. We propose to replace the text by “Though the bias was intentionally eliminated in the calculation of CRMSR_{mth}, ab-

C6151

solute values are shown in the figures so that values can be related to the regime of no streamflow”? f ok g VIC can be run with and without frozen soil. LPJmL does not consider temporarily frozen soil (from above) but permafrost thawing and melting. We will later send a note about the treatment of frozen soils in the other 3 models. h-i ok

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C6152