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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Initial assessment of a multi-model approach to spring flood forecasting in Sweden" *by* J. Olsson et al.

R. Marty (Referee)

renaud.marty@gmail.com

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See attached file.

General comments

The paper presents dynamical and statistical approaches calibrated and used in order to provide more useful and more skillful seasonal hydrological forecasts, compared with the current operational system based on climatology. The evaluation focuses on 3 basins in Sweden and 3 forecast issue dates prior to spring flood. This study is an interesting contribution, particularly because of the analysis of negative and positive results, according to the target basin. However, there are major concerns that I think need to be addressed. They are: A number of assumption and details about dynamical

and statistical approaches need to be justified and clarified. Some of them came from literature. Thus it is quite difficult to judge the validity and the contribution of the authors. To my opinion, the verification part is subject to sample effect and the manuscript is confused on this topic. Especially it is not clear what are the ensemble sizes provided by the different procedures. The authors should explain why a spring flood forecast is reduced to a deterministic forecast from an ensemble of possibilities. Why the authors kept only 3 forecast issue dates? The results will be more conclusive with a greater sample.

Furthermore they are specific comments that the authors should consider to improve their manuscript. Note that I don't have a wide access to the majority of papers cited in this article. This review does not concern the references.

Specific comments 1. In page 6083, line 6: "Applying the model calibration of a number of free parameters, generally aout 10." This part requires more details, including the explanation of having several parametrization of the same model. Do the 3 basins have the same parametrization, i.e. the same number of free parameters? 2. In page 6084: why do you describe the ECMWF's model in details ("Cy31r1") and not the Arpege one? This level of details is unnecessary in your manuscript 3. In page 6084: Are ECMWF and ARPEGE models used in the same time or as two configurations of SD approach? 4. In page 6085: The description of the CE procedure is clear. Nevertheless, could you give the CE ensemble size? Please explain why the simulations are not made from the start of the last hydrogical year? 5. In page 6086: The relation between indices and hydro-meteorological trends is clearly described but deserves some figures to facilitate the analysis 6. In page 6087: Some information seems to be implicit in TCI procedure. Please clarify that the period length (1 to 6 months) is a parameter. The configuration TC1 determines the analogue year with the comparison of 3 value per year, while TC6 needs the concordance over 18 values per year. The interpretation of the ongoing results may be submitted to sample effect. Especially when you say "If not analogue years can be identified among the historical ones by comparison of the

state of the three climate indices, analogue years are sought using an agreement with two of them.". Is there a indice that is always remove from the comparison sample? How many TCI members make your sample? Or the analogue year is the input to provide your median SFV? Please clarify. 7. In page 6088: Why the CP procedure is calibrated only for the Vindelälven basin? Why CP is not set for the other ones? 8. In page 6089: In you Eqn (3), d seems to be a parameter that can be optimized in CP procedure. Do you use 0.1mm as mentionned in your manuscript to be the general case or did you use another threshold? 9. In page 6089, line 12: "... are determined subjectively to adjust for differences in magnitude as well as importance" sound not clear. What do you mean by magnitude? Spread? Please clarify. 10. In page 6089, lines 16-17: "CP catalogue" is not defined. Is there a set of weather types? 11. In page 6089, lines 20-22: "The two mos frequently occuring Cps within a period of 1 up to 6 months prior to the forecast issue date are used as a criterion to select the analogue historical years" Do you mean that historical CP series are compared to these two CPs? Finally, how many analogue years constitute your sample? 12. In page 6090, lines 10-11: Your assumption is strong (see e.g. Buizza and Palmer, 2008, Monthly Weather Review, 126), especially for precipitation ensemble forecast. Please justify that the ensemble median is not influenced by the ensemble size, increasing from 11 up to 41 in your dataset. 13. In page 6091: As the SD method is stochastic, how is defined the ensemble median? 14. In page 6092: The evaluation is only based on 3 forecast issue dates. The paper should present more forecasts. e.g. one per decade, to outline the influence of the lead time. 15. In page 6092, lines 8-9: "They were selected by an initial screening period based on previous literature..." You must give the reference of this previous literature. 16. In page 6092, lines 20-21: You could indicate that an approach is perfect if RI is 100%. 17. In page 6094, line 10: Which sample do your refer by "limited sample"? 18. In page 6095, line 4: According to the results shown in table 2, the first statement is wrong. Please clarify. 19. In pages 6096-6097: To be more robust your analysis deserves to include CP optimized basin by basin, and globally. Or please justify your decision to optimiwe CP only on

Vindelälveln basin. 20. In page 6097, lines 23-27: It would be interesting to include the new version of ECMWF seasonal forecasting in your results 21. In page 6098: Is there a reason to justify a SD configuration per forecast issue date? Do a global configuration decrease the performance of SD approach? In Section 2 the meteorological dataset is made of two atmospheric and oceanic combination. Is SD procedure applied on both combinations or only on the one including the ECMWF model? Is SD equally sensitive to the ECMWF or ARPEGE ? 22. In page 6099, lines 9-12: "If any of the new methods could not generate any forecast, it was replaced by CE [...]" Does it mean that previous results shown is Section 5 are based on heterogeneous sample of deterministic forecasts? If that is the case, it isn't mentioned anywhere and limit the scope of the previous conclusions. Please clarify and please give the importance of such replacements. 23. In page 6099, lines 14-17: The statement sounds explain that the median is less sensitive to sample issue. This aspect is too implicit in the manuscript and need to be addressed more clearly in Sections 3 and 5.

Technical corrections 1. In page 6078, line 2: what means "useful" in your context? 2. In page 6080, lines 11 and 15: "teleconnections" is not widely used in statistics. Could you give a technical definition of this term? 3. In page 6081, line 19: "development" could have a double meaning here. Do you refer to meteorological situations? 4. In page 6082, Eqn. 1: what don't you use a symbol to represent "lakes"? 5. In page 6083, line 14 and hereafter: you should use NSE instead of R2 to represent the Nash-Sutcliffe efficiency. R2 is more commonly used as the determination criteria in statistics. 6. In page 6083, line 24: you should put the CPC's domain name in your references and, here, give only the reference (e.g. CPC) 7. In page 6085, lines 24-25: typo "A collection of . . . constitutes the historical data" 8. In page 6086, line 1: "better" refers to a comparison. Here, the term is incorrect. "accurately" or "correctly" are more appropriate. 9. In page 6086, lines 16-18: Please, move ", which is part of . . . (NOAA)" in page 6083. The internet link is already given in Page 6083. 10. In page 6087, line 22: you should replace "normally" by "generally" 11. In page 6091, lines 24-25: In hydrological forecasting, lead time is sometimes defined as the period between the

forecast issue date and the validation date of the current forecast. 12. In page 6092, line 17: type “MAECE” 13. In page 6094, lines15-19: This part has to be moved in conclusion of Section 3 14. In pages 6094-6095, lines 27-28 and 1-2: theses results need to be added to a Table 15. In page 6098, last paragraph: The statement would be more comprehensible with an illustration 16. In page 6102, line 5: “and will be reported elsewhere” is unnecessary 17. In page 6113, Fig.1 : Is Vindeln the same basin known in the manuscript as Vindelälven ? Please keep one denomination.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C3068/2015/hessd-12-C3068-2015-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 6077, 2015.