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Discussions

Interactive comment on "The benefit of seamless forecasts for hydrological predictions over Europe" *by* Fredrik Wetterhall and Francesca Di Giuseppe

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This paper has a good (not new) idea, but is disappointing as it just skims over results without proper analysis. It currently does not have a proper scientific discussion and reads like it was rushed. In addition, the paper seems to have written for a different journal, it is extremely short (which is good in theory), but simply lacks depth and proper analysis. Results are not properly explained and leave many questions. This is best illustrated by the use of a single score, which does only measure one property of an ensemble forecast - I would have at least expected some de-compositions.

Detailed comments: Acronym ENS-ER appears in introduction first and needs to be

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Discussion paper

defined in introduction not only abstract. I could not find that acronym on ECWMF's websites which makes me wonder what the authors have actually used.

The introduction defeats most of the paper. I clearly states: "This implies that the skill of SYS4 is lower relative to ENS-ER in the overlapping first six weeks (Di Giuseppe et al., 2013)", which is obviously a result that has been already published by one of the authors earlier

L34 it is unclear why the extension leads to benefit. Point (ii) - that has been possible before, what is better and why? There are no references stated for the hypothesis listed in (i) to (iii) - a more detailed in depth discussion and reasoning (or supporting results) are needed

"The extended lead time provided by running EFAS forced by weather prediction across different time scales could potentially provide added benefit in terms of very early planning, for example for agriculture, energy and transport sectors as well as water resources management." - where is the evidence for that statement? references? Studies - this unsubstantiated and symptomatic for the rest of the paper - many claims or statements which are not backed up.

"often model implementation is segmented for practical reasons. Still major efforts have been made to create unified systems" - it is completely unclear what is meant - clarify

"Similarly, the UK Met Office has in the past twenty-five years worked to create a unified model that could work across all scales (Brown et al., 2012). Also the climate community has moved in the same direction. For example, the EC-Earth project shows that a bridge can be made between weather, seasonal forecasting and beyond (Hazeleger et al., 2010, 2012)." this is not relevant for the paper. I am unsure what point the authors are trying to make with respect to the hypothesis tested in this paper. Introduction needs significant shortening.

"avoiding the complications of new developments while generating forecast products to meet different types of users (Pappenberger et al., 2013)." Pappenberger is clearly wrong - one will always need different products for different applications.

"diverge over time, only re-converging when the seasonal system" That assumes that the seasonal system is very close to the system from which it is derived from. I just googled ECMWF System 5 and it seems to come from an older model cycle, hence this statement is clearly incorrect

"final products should be provided in terms of anomalies calculated against the model climate" that assumes that the model universe behaves similarly to the real universe in terms of anomalies - can the authors provide any prove and evidence?

"What is the gain of using a more recent model version in the first 46 days provided by the use of the ENS-ER?" I don't understand that question cause according to the authors this has been already answered in a paper cited by the authors themselves, (Di Giuseppe et al., 2013). It demonstrate that the paper currently only presents a very very incremental step.

It is unclear how the authors come to 786 reference points - how have they been choosen - the claims made by the authors are not substantiated by the results presented. Can the authors please add the analysis which lead to those points? this is a clear example where the paper has been cutting corners rather than explaining properly what has been done.

" (referred to as tuning in the NWP nomenclature)" This is a hydrology journal, why do you explain that?

"Using the WB run as proxy observation simplifies the interpretation of the skill scores as it avoids the complication of having to assess the bias against observed discharge." This maybe convenient to do, but then the analysis could have been done against all grid points or far more ($\sim$700 is pretty low given the size of that Grid). The authors need to elaborate on the limitations this analysis places on the results of the study. I am also thoroughly confused, the authors said that they had real observations for the calibration. I would expect at least some analysis against those real observations. Far more detail needs to be provided.

“The hindcast period can together with observations be employed to calibrate the forecast in an operational setting (Di Giuseppe et al., 2013).” I am unsure about what the authors mean with that statement and find the reference strange and forced (deliberate self citation?). Can the authors please cite references from others too?

Figure 1 is unclear - how do different ensemble number play a role. Did you only merge 11?

2.3. Experimental set-up - you are comparing apples with pears. One system has clearly a much larger sample size and the authors do not explain how the adjust for that fact. Results cannot be robust unless this is taken into account. Please revise your method thoroughly.

CRPS is equalised by randomly drawing from the distributions - that is at odds with the statistical literature. Check for example this presentation: http://empslocal.ex.ac.uk/people/staff/ferro/Presentations/ems2013ferro-fair.pdf

The authors need to present more scores and analysis. They talk explicitly about droughts in the introduction - this scores does not analyse. To understand skill, one needs to look at least at the decompositions of the CRPS. The analysis needs to be extended significantly and far better discussed.

“then some points show a benefit of using the SYS4 instead of SEAM.” - why? explain

“In the above example, a decision maker would have to make a decision based on a forecast that was issued 2.5 weeks earlier, which would inherently make the decision more uncertain if you only had the seasonal forecast. With the seamless system available a decision maker would gain the same early indication of a hazardous event and also have the benefit of frequent updates.” Can the authors please test their hypothesis and provide prove for such unsubstantiated statements? where is the social scientific evidence?

I do not understand the point of section 3.3. - it presents a single case and then makes some wild statements. Please assemble a larger number of cases or simply cut.

the analysis overall falls short for more details. It simply skims over results without really going into them and properly analysing them. Many hydro aspects are ignore.

Please explain how your results are driven by spatial variations of the weather forecasts.

Conclusions are not comprehensive enough and a proper scientific discussion is missing.

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