

Response to Referee #4

(All referee comments are in Italics.)

This is my review of the paper "A framework for upscaling short-term process-level understanding to longer time scales" (Manuscript number: HESS-2012-206). The paper aims to provide insights about temporal up-scaling of model parameters (describing the physical relationship for pan evaporation) based on the covariance between model variables. The authors quantified the change in covariance between model parameters using the Taylor series expansion.

I have reviewed the manuscript, and I believe that overall it can sit well within the journal's objectives. However, drawing from my experience in hydrological modelling, I believe that the paper requires major revisions. The paper should be modified and clearly state its originality (though this is actually done in several points of the manuscript) and present/discuss its limitations. The previous 3 reviewers have identified several weak points in this analysis each from his/her own research background; however, there is also space to highlight mine from a modelling point of view. My detailed comments are tracked (each highlighted text has a pop-up note with my comments) in the "hess-2012-206-discussions-typeset_manuscript-version2_Reviewed_IP" pdf file.

Thank you. We noted the tracked comments and will address them accordingly in the revision.

- 1. Overall, I disagree with the title of the current manuscript. It is indeed very promising and could generally be misleading to the reader. Although, the authors believe that the approach has wider application in hydrology, this is not, in my opinion, presented properly (see my comments below). You could clear things up providing more details in the title, e.g. "Application using pan evaporation data". In addition, I disagree with the word "framework". In hydrologic science, we lack of robust frameworks, still though would you really call this approach a framework, hence supporting the approach's potential in other hydrologic areas? A framework means that it has wider applications, which is not the case in this paper (at least based on what the authors conclude given this study's limitations).*

We respect the referee's (and previous referees') viewpoints on the title. In response, we will likely modify it in the revision.

- 2. I think you should have a section just for the theoretical background. It is maybe better to combine Section 2 and 3 into one, and use sub-sections. In addition, Section 2 should be renamed e.g. "Statement of the problem: Evaporation hypothesis testing". Current title is very general. In addition, I like Section 3 and I believe that it links well with Appendix A. However the title is misleading. You could use e.g. "Theoretic expansion of ..."*

We agree. We will make the suggested changes. Thank you.

3. *The reader needs to know more about the selected dataset (Section 5). How was the aggregation conducted? I agree that the reader should follow the Lim et al., 2012 paper, however this section should, to a certain extent, stand on its own. I believe that this information is important to argue on the subjective selection of “minimum of 16 days to be considered valid” threshold for the data aggregation.*

This comment is more or less similar to Referee #1. In response, we will expand the Data and Method section to suit. We have previously described the data in the first response to Referee #1.

4. *You need to have a short paragraph right after Section 6 title introducing the reader to your methodology’s objective. I felt a bit lost when reading the Results section for the first time without prior understanding the reasons for each analysis/sub-section. In addition, I agree with Reviewer 1 that Figs. 5 and 7 are poorly discussed. There is enough information in both figures that could be argued by the authors (e.g. data scarcity in Fig. 7).*

Noted. We shall add a brief paragraph after Section 6 to guide readers through the subsections. We also plan to make further discussion on Figs. 5 and 7.

5. *I believe that a separate section just for Discussion is required. It should include a paragraph(s) on how this approach can be generalised using other hydrological fluxes or even in hydrological modelling. In addition, the discussion raises a question: how results would have change if we use 10’ or 15’ data? Or even, can this approach be used to down-scale information? Finally, I agree with Reviewer 1 regarding the robustness of the paper’s conclusions using for instance 10 or 100 days of high frequency data. Although this could be addressed presenting the corresponding results, it could also be presented in the Discussion section.*

Noted. In response, we shall separate the Discussion section from the Summary.

6. *There are 2 mistakes in the equations A1 and A2. Check the reviewed manuscript.*

Good point. We obviously missed that. Thank you.