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Discussions

Interactive comment on "Potential evaporation estimation through an unstressed surface energy balance and its sensitivity to climate change" *by* A. Barella-Ortiz et al.

Anonymous Referee #2

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The paper "Potential evaporation estimation through an unstressed surface energy balance and its sensitivity to climate change" by Barella-Ortiz et al. presents an interesting comparison of different estimates of potential evaporation, and the implications for ETp projections. I do think the paper is worth publishing. However, I have some comments to the paper, see below.

General comments

My first major comment is on the method and analyses performed. If I understand this right, you use the FAO method directly in Case 1, i.e. you assume that the land

is covered by crops of height 0.12m. Since your Orchidee modeled ETp estimates (bulk, milly and useb) apparently use a more real representation of the land surface (vegetation), this comparison seems somewhat strange to me. I think you need to discuss this approach. There are many definitions of ETp, and some of them do not even aim at being comparable. I am not sure I fully understand the motivation behind the direct comparison of these numbers, apart from just showing that they are different (as would be expected). Also, your FAO case 4 is an attempt to make the USEB and FAO methods more comparable, so why do you not include FAO case 4 in Fig 3?

Possibly somewhat contrary to the previous paragraph, but still: I do not fully understand why you do not include all ET schemes in both your "validation" part (section 3.1) and your "climate sensitivity" part (section 3.2). The content of the paper would be more coherent if the same schemes were included in both sections. You say that the bulk method is not included in the estimates since it overestimates ETp (p 8203), but it is still found "good enough" to include in the climate change analyses. You also say that some of the schemes are not included in the validation part because site specific parameters need to be calibrated (Section 2.3), but since you later assume that the trends are mostly independent of these site specific parameters I still think it would be valuable to present the reference period results (at least in form of numbers in Table 3) for these methods as a backdrop to the trend analyses. Also, are the consequences of these site specific parameters much different from the consequences of the assumptions of your FAO case 1 compared to the USEB method?

Another major comment to the paper is the way the study and its results are presented. I currently find it challenging to grasp the main content and background of the analyses the paper is based on. Many details are included, e.g. on the various methods of estimating ET, and this contributes to the main points being somewhat lost. Many of the equations are actually presented in both the text and in Tables 1 and 2, which seems redundant. I recommend putting some of the details behind the ET methods (parts of Sections 2.1 and 2.2) in an appendix. On the other hand, there is some information I am looking for that I do not think is in there. Some times it is hard to understand why the information is included at all, i.e. why it is important for the study presented here. Some sentences I simply do not understand the meaning of, which further contributes to me being somewhat confused.

Specific comments

Clarification issue: My understanding is that Orchidee is run when estimating bulk, milly and useb, whereas the other estimates are calculated directly based on the given equations. In order to be clear on this, I suggest you say this directly, e.g. in the introduction of Section 2 or as a footnote to one of the tables.

Forcings: You are using WFD for the reference period and IPSL for the projection period. As far as I understand, there is no direct comparison between the projection period results and the reference period results. I assume you use WFD for the reference period because this dataset is based on observations. However, since you never compare your results to any observations, I am thinking that it would have been more consistent to use IPSL for the entire period. Also, regarding the trends in ETp: How do you think these are influenced by the choice of climate model output? Both wind and radiation values vary tremendously among climate models, and although I do not know how different the trends in these variables are, I assume there are some differences. Any thoughts on how this might influence the results? That is, how much more must the ra or vpd trend have been in order to have the radiation trend outweigh the vpd trend?

Tables: Table 1 and 2: I recommend combining these two tables into one, which I think would be beneficial to the readers of the paper.

Figure 3: Why isn't your FAO case 4 included in this figure/these analyses? It seems to me that the case 4 is the FAO version that you say is the most similar to the USEB method for the reference period, so how does it compare when looking at trends?

Wording: You talk about validation. As far as I understand, this "validation" is actually a comparison to your Orchidee estimates, and hence I find it slightly odd to talk about validation. I suggest rephrasing, and also point out that you consider your Orcidee estimates as the "ground truth" in this study, although I assume that you also agree that these ETp numbers are still estimates, and that the ETp estimates of another LSM might be somewhat different.

In general, I recommend the authors to go thoroughly through the way the paper is written. Here are some examples:

P 8198, line 1: "ETp is a basic input for . . . models" is maybe a bastant expression? Some of these models do the ETp calculations internally, although often based on much simpler approaches than a land surface model. However, I suggest writing "ETp can be" or ETp is a basic input for many . . . models" instead.

P 8198, line 14: "both formulations differ". "both" refers to what? And I assume you mean the resulting ETp estimates based on these two formulations, and not the formulations themselves?

P 8198, line 20-21: "USEB method shows higher sensitivity". Higher than what?

P 8199, line 22-23: "each one focuses on . . .". Weird sentence.

P 8203, lines 11-12: "Actual evaporation" is mentioned. What is the purpose of including information on actual evaporation when the focus of the paper is on potential evaporation? If included, maybe it would fit better in the discussion part, e.g. a discussion around the second-order effects of the various ETp schemes and underlying assumptions.

Page 8218, line 19: "more arid regions than humid ones". Do you mean "the arid regions more than the humid ones" or "larger areas"?

Page 8221, lines 2-5. This part I think fits better in the discussion than in the summary/conclusions, since it is never mentioned before.

P 8221, lines 15-19. I agree that having the climate models report some ETp estimates is a good idea. However, since your results are still model estimates, maybe the word “determine” is slightly ambitious when talking about the quality of estimates for current climate and sensitivities of ETp to climate change?

Page 8221, last sentence: I do not understand what you mean. “we recommend to unbias ET estimates”?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 8197, 2013.