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Reply on RC2

Thibault Lemaitre-Basset et al.

Author comment on "Unraveling the contribution of potential evaporation formulation to uncertainty under climate change" by Thibault Lemaitre-Basset et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-361-AC2, 2021

Responses to comments of Referee 2:

We would like to thank the referee for the useful comments and his/her interest for our study. We will include all the specific comments mentioned by the referee, clarify the vocabulary, and correct figures directly in the manuscript.

My specific comments are:

* Inacuracies, need revision:

** line 17
This is debatable, but to me "climate change" is more of a "consequence" than a "cause".

Thank you for this comment, we will modify to “climate change results in”.

** line 20-24
This part I would avoid in the introduction, since it leads the reader to think that the study will cover actual evaporation, which is not the case. But it could be material to place in the discussion, where potential consequences of this study on AET could be discussed.

We agree with the referee, moving the consequences of PE on AET from the introduction to the discussion part will enhance clarity to the manuscript.

** line 32
I don't get this sentence. What do you mean by "through calibration"? What do you mean by model sensitivity here? Sensitivity of model output or of PE? And sensitivity towards what?

Here we refer to the fact that PE generally feeds an impact model (for instance hydrological models), and the calibration step of the impact model compensates to a certain extent potential bias from PE. We will rephrase as reviewer 1 also asked clarifications on this point.

** line 36

You cannot say "a more comprehensive way" because you are not aiming at the same things. Above you write about validity of assumptions, whereas here you write about uncertainty of modelling results.

We propose the following reformulation: “Here, we propose to assess the contribution of PE formulations to the overall uncertainty of projections by testing several formulations under several climate projections.”

** line 64
Why do you say that all PE formulations are questionable over mountainous areas?

Because physical processes involved for these areas are different from the rest of the study area. Indeed, part of the snow cover disappears by sublimation without melting. The tested PE formulations do not represent this process. We agree on the misunderstanding, so we propose to detail the difficulties of PE estimation under cold and mountainous regions in the revised version of the manuscript.

** line 75
Revise this sentence. I don't like it. Write something that you partitioned the total uncertainty on projected PE among...

We propose a new sentence: “Second, the total uncertainty on projected PE will be partitioned and quantified among all uncertainty sources (RCPs, GCMs, RCMs, and PE formulations)”

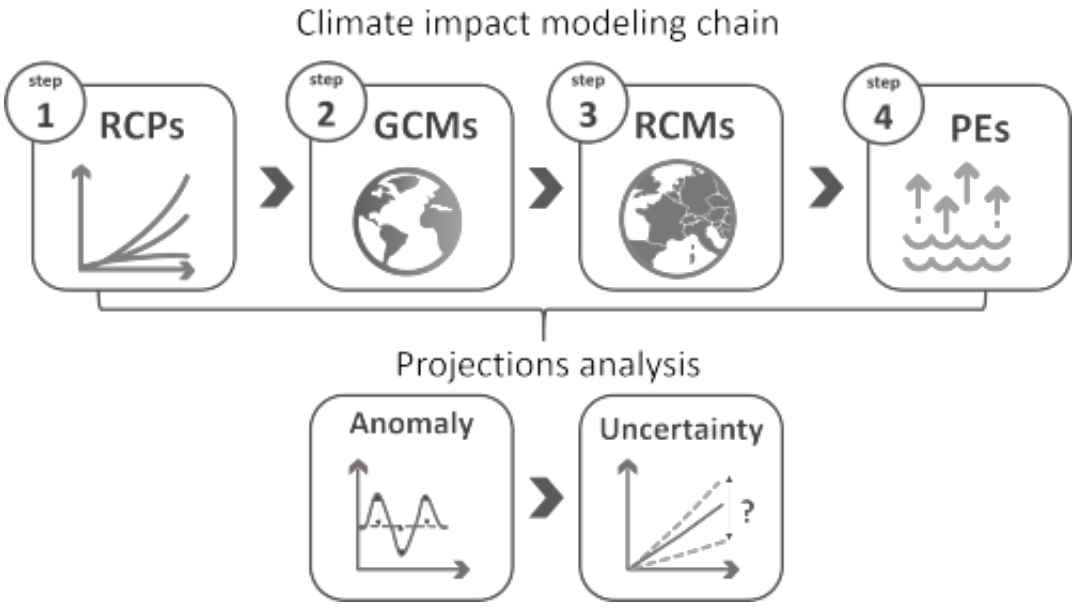
** line 84
For all RCP and GCMs/RCMs combinations (3*6*9) you get only 1 realization of the climate variables? This is a limitation of the study since you will not be able to reproduce uncertainty coming from internal climate variability (stochasticity). Indeed later in the text you write that you took a running mean on climate variables in order to reduce fluctuations. Not including internal climate variability and the running mean choice, both need better explanation/rational behind it.

We fully agree that using only a single realization for each combination is a limitation for the total uncertainty quantification, and hinders the quantification of an additional uncertainty source (namely internal climate variability). The main problem here is the access to several realizations of climate projections for the different GCMs / RCMs used in the study. However, as we focused on anomalies over long time slices, we assume that the natural climate variability may have a limited impact. We will add a sentence in the manuscript to improve explanations.

** line 121
See my comment above.

** line 124
Overall I find the methods section a bit difficult to follow. I think it would highly benefit from some schematics where the procedure is drawn out. Also, later in the text you would refer multiple times to "modelling steps/chain", and those steps were never explicitly defined in the text.

We agree that it could be difficult to understand the modeling chain used for the climate impact study, especially if the reader is not familiar with this type of study. We propose the following diagram to improve the understanding of the method. We will refer to this scheme in the new version of the manuscript.



** line 144
What exactly do you mean by "trend slopes"? I think you should be more specific.

In this case we mean “growth rate”, we will change this in the manuscript.

** line 150
"covariance" instead of "interdependence"

Thank you for this recommendation, we will modify the manuscript accordingly.

** line 157
"uncertainty contributors" instead of "factors"

Thank you for this recommendation, we will modify the manuscript accordingly.

** line 159
this modelling chain needs to be defined, see comment above

In order to clarify the manuscript, we propose to define the modelling chain at the beginning of the method section (line 82). We will add the following sentence, which completes the information given by the new scheme added in the same sub-section. “The modelling chain is a pathway from different RCPs to an impact model (here PE formulations), with a succession of models, whose simulations outputs feed the next model. The figure 1 represents the modelling chain used for this study with each modelling step, namely RCPs, GCMs, RCMs and PE formulations.”

** figure 2 caption
Instead of "absolute anomalies" I would use "expected increase"

Thank you for this recommendation, we will modify the manuscript accordingly.

** line 169
But why does it increase for RCPs? This could be the consequence of the total uncertainty being lower for locations in the south and RCPs uncertainty being equal throughout France, leading to higher relative contribution of uncertainty of RCPs.

We agree with the referee. This comment will be clarified by adding a panel on figure 4

with the total uncertainty, as suggested by the referee below. Besides, we will analyze the uncertainty contribution for one RCP (namely RCP 8.5) in order to address this issue that was also highlighted by referee # 1.

** figure 4
A further panel with total uncertainty would be good.

We agree (see previous comment).

** line 179
This belongs to the results

** line 180 to 185
This should be placed in the methods section

We agree with this comment and we will move these explanations in the method section (2.3), and provide more details.

** figure 5
Since 1 is the threshold here modify the lower row in order that it has 2 different colormaps, e.g. reddish for values above 1 and greyish for values below 1.

Thank you for this recommendation, we will modify the Figure accordingly.

Insert the equation of the signal-to-noise ratio in the figure caption.

Agreed, we will modify the figure caption.

** line 189
This belongs to the results

We agree, this sub-section will be moved to the Results section, to remain consistent with the previous change.

** line 216
I would not use the word "globally" here

We agree, we will remove the term “globally”. The sentence will become “*Moreover, the uncertainty spread also increases with time, which adds up to the total uncertainty, despite the fact that the relative contribution of this factor to the total uncertainty decreases with time*”

** figure 6
What are the distributions exactly? To my understanding looking at one boxplot in one panel gives information on the variability in delta PE given that one modeling step is chosen and "fixed"(specified by the color) and all the other modeling steps (all but the colored specified one) vary across their PE output range. Is this correct?

Yes, the referee’s interpretation is correct. We will add details in the Figure caption to avoid misunderstandings.

This figure needs time ticks to show the 3 periods distinction. Then you get it at first glance.

Thanks for this comment; we will add markers for the 3 periods to improve the readability of the figure for the reader.

** line 241
I don't think this is true.

To me, this is more the consequence of the higher variability (and therefore uncertainty), which is introduced when more RCP scenarios are considered. The relative contribution of PE formulations to total uncertainty is thereby reduced, but PE formulation would not vary more among one RCP.
The referee's interpretation, as well as the one from referee #1, is completely plausible. Therefore, we will present the results for a single RCP to quantify the impact of multi-RCPs analysis on our results (see answer to referee #1). We wanted to explain that by focusing on a single emission scenario, the relative contribution of the PE formulas to the overall uncertainty is mathematically larger. However, indeed in terms of absolute uncertainty it is the same. We propose deleting the following sentence from the manuscript: "…due to the higher future air temperature".

** line 254
Which usage?

We agree that the term "usage" can be confusing. We propose the following reformulation by changing "usage" with "practice".

** line 262
This is only valid for the give dataset. It would be interesting to see whether the same conclusion can be drawn if the dataset would contain also stochasticity of climate variables (multiple realizations of climate variables for the same RCP-GCM-RGM modelling chain)

We agree with the referee's comment; however, we did not have access to multiple runs for each RCP-GCM-RCM. This lack is common to many climate impact studies. It could be an improvement for future works about the uncertainty quantification in climate impact studies but this is beyond the scope of this study.

** line 265
Personally, I would have appreciated some qualitative statements on how future PET uncertainty might transfer to AET uncertainty, since finally AET is the variable we care about, PET being only a "modelling"-byproduct.

We agree with this suggestion, and we propose to add a discussion section dedicated to this issue. The discussion section will address two related issues:

- The spatial heterogeneity of the consequences of PE uncertainty on AE that may be limited over "water limited" regions, such as Mediterranean regions but more important over "energy limited" regions, at higher latitudes.
- The sensitivity of the impact model to PE inputs variability, which was already addressed in the introduction but following the referee suggestion, this will be moved (and detailed/clarified) in the discussion section.

** line 267
Since AET is never mentioned in results or discussion I don't think mentioning it in conclusions is justified. I would delete the whole sentence.

As suggested previously by the referee, we will address this issue in a new discussion section.

* Recomendations related to style

** line 12

Delete "Finally"

Thank you for this suggestion, we will modify the manuscript.

** line 17
I would replace "modifications" with "changes"

Thank you for this suggestion, we will modify the manuscript.

** line 28
empirical temperature methods

Thank you for this suggestion, we will modify the manuscript.

** line 29
delete "some", replace "relatively to" with "other than"

Thank you for this suggestion, we will modify the manuscript.

** line 30
delete "and possible feedbacks"

Thank you for this suggestion, we will modify the manuscript.

** line 35
.. but assuming that models may represent past and future climates equally well is difficult to verify

Thank you for this suggestion, we will modify the manuscript.

** line 49
how future streamflow anomalies can be dependent on the choice of PE formulation

We mentioned the fact that for an energy-limited environment, the increase in PE leads to a decrease in runoff, if the changes in precipitation do not offset the increase in PE. But, if the increase is small, the changes in precipitation can offset the increase in PE, and lead to an increase in runoff.

** line 51
uncertainty of

Thank you for this suggestion, we will modify the manuscript.

** line 62
results appear to be..
Thank you for this suggestion, we will modify the manuscript.

** line 71
since outputs become inputs for PE I suggest choosing another word
Thank you for this suggestion, we will modify the manuscript.

** line 95
Be more specific here and mention the variables Rn and Ta.

Thank you for this recommendation, we will mention Rn and Ta variables.

** line 98
equilibrium temperature, which better represents the..

Thank you for this recommendation, we will change the sentence.

** line 99
what radiations? Solar radiation?

Yes, “radiation” will be replaced by “solar radiation”.

** line 103
since feedbacks between climate variables exist

Thank you for this recommendation.

** line 113
respective variance contributions

Thank you for this recommendation.

** line 144/146
on the selected forcing variables

Thank you for this recommendation.

** line 145
probably being

Thank you for this recommendation.

** line 150
this suggests "covariance" instead of interdependence.

I would stick to the term "covariance" throughout the text when you write relationships between climate variables.

Thank you for this suggestion, we will change “independence” for “covariance”.

I would stick to the term "covariance" throughout the text when you write relationships between climate variables.

We agree with the referee.

** line 192
I would not use the word significant since it induces the reader to think about statistical significance, but here you mean only higher vs lower, correct?

Yes. We will changed the expression “more significant” for “higher”.

** line 233
future trends

Thank you for this recommendation.

** line 253
relative insensitivity ..

.. study, compared to other sources ..
Thank you for this recommendation.

** line 260

are near the average

Thank you for this recommendation.