

Comment on hess-2021-361

Anonymous Referee #2

Referee 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-RC2>, 2021

This study systematically investigates how future climate uncertainty, coming from RCP scenarios, GCMs and RCMs, transfers to uncertainty in potential evaporation (PE) modelling under inclusion of the uncertainty coming from different PE formulations. Uncertainty contribution is computed by using ANOVA on an ample dataset containing timeseries of climate variables obtained from the different RCPs, GCMs and RCMs, and the obtained PE resulting by feeding different PE formulations with the (future) timeseries of climate variables. The authors conclude that the choice of PE formulation does "only" make up about 10% of total uncertainty by the end of the century. It is mainly the consequence of the relatively low variability coming from PE formulations compared to the variability coming from the projected climate variables.

The study is timely since it addresses an important question concerning hydrological modelling under climate change: "How will future climate impact PE?" Overall it is a well framed and written manuscript. However, considering PE in a purely "theoretical" framework, without a bridge to the "real-world" (actual ET), limits the applicability of the results. The authors should be more careful not to confuse readers throughout the text and explicitly discuss this aspect in the discussion.

There are some inaccuracies or statements that lack explanations on top of some inadequacy in the manuscript structure, however overall I'd consider them to be rather minor. My specific comments are:

* Inaccuracies, need revision:

** line 17

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

** 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.

** 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?

** 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.

** line 64

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

** line 75

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

** 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.

** 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.

** line 144

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

** line 150

"covariance" instead of "interdependence"

** line 157

"uncertainty contributors" instead of "factors"

** line 159

this modelling chain needs to be defined, see comment above

** figure 2 caption

Instead of "absolute anomalies" I would use "expected increase"

** 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.

** figure 4

A further panel with total uncertainty would be good.

** line 179

This belongs to the results

** line 180 to 185

This should be placed in the methods section

** 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.

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

** line 189

This belongs to the results

** line 216

I would not use the word "globally" here

** 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?

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

** 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.

** line 254

Which usage?

** 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)

** 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.

** 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.

* Recomendations related to style

** line 12

Delete "Finally"

** line 17

I would replace "modifications" with "changes"

** line 28

empirical temperature methods

** line 29

delete "some", replace "relatively to" with "other than"

** line 30

delete "and possible feedbacks"

** line 35

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

** line 49

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

** line 51

uncertainty of

** line 62

results appear to be..

** line 71

since outputs become inputs for PE I suggest chosing another word

** line 95

Be more specific here and mention the variables R_n and T_a .

** line 98

equilibrium temperature, which better represents the..

** line 99

what radiations? Solar radiation?

** line 103

since feedbacks between climate variables exist

** line 113

respective variance contributions

** line 144/146

on the selected forcing variables

** line 145

probably being

** 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.

** 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?

** line 233

future trends

** line 253

relative insensitivity ..

.. study, compared to other sources ..

** line 260

are near the average