

GENERAL COMMENTS

I have the feeling that most of my comments have been properly tackled and that the article is close to publication-proof. I still have the feeling that the authors miss some chances to truly make this a high-impact paper. For instance, I think that elaborating more on the differences between the “current” and “future” impact of climate modes on evaporation is very interesting. However, I do see that this would need substantially more work: building up in-depth knowledge of the different climate models, running extra analyses, and physically interpreting the results. Below, I list some final comments that need to be tackled before the paper can be published.

SPECIFIC COMMENTS

1. It is still not clear to me from the description at P2-L61–64 why the historical period and future period need to cover the same number of years. As the methods used in this study have a statistical nature, I would say that more data gives more robust results (and conclusions). The authors also mention that “periods with similar lengths do not alter the results”. Why is the latter statement of importance then?
2. It is not clear why the authors de-trend their data. At P4-L103, the authors claim that this processing step does not change the results. When this step has no impact on the analysis, why is it performed then? I think a motivation should be given in the manuscript to avoid confusing the reader.
3. The description of the temporal resolution at P4-L125–130 is very vague and unclear to me. Please revise this description to make it more readable.
4. I am still surprised about the low impact of the climate modes on evaporation dynamics over land areas. Previous numbers reported in literature are generally higher than the ones reported in this study (P8-L253–266). I can agree that different methods give different results, but the authors should be able to explain these differences when they are that substantial. This difference should at least be acknowledged and better discussed in the manuscript by contrasting the numbers against similar values reported in literature.
5. The statement at P9-L259 is interesting: “We observe an increase in land area affected by ENSO to ...”. Why do the authors not elaborate a bit more on this? Why do they think they see this increase? The nice feature of this study is that a separate analysis is performed for a “current” and “future” period. In my opinion, elaborating a bit more on the different results between both periods would really make this a stronger paper.
6. The discussion at P9-L267–274 is interesting as well. I think another reason might be that the modes are affecting meteorological variables that (during that period) are not driving evaporation. E.g. NAO might affect precipitation (water availability) in northern Europe during winter, but in wintertime, evaporation is mainly driven by radiation in this region, so NAO will apparently only have a small (or no) impact on the dynamics of evaporation in that case.

TECHNICAL CORRECTIONS

1. P2-L49: this is the first use of “CMIP” in the main text, the abbreviation should thus be defined here (and not at P2-L54).
2. P3-L73–74 : this is a repetition of P2-L31.

3. P3-L68–69: this description can be easily miss-interpreted as this is not how the authors deal with the multi-model ensemble (details are described at P4-L123–124) in this study. I would suggest rephrasing this statement.
4. P4-L134: "... influence on evaporation of different regions ...": please rephrase because it is not clear what the authors mean.
5. P4-L140: "... which is the main contributor of change in ...": the authors use "change" multiple times along these lines, but to me, it is not exactly clear what the authors mean with "change". I guess they mean that dynamics of evaporation are affected/changed by something else, but it sounds vague to me.

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