Comment on esd-2021-45
Anonymous Referee #2

Referee comment on "SSensitivity of land-atmosphere coupling strength to changing atmospheric temperature and moisture over Europe" by Lisa Lea Jach et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-45-RC2, 2021

Summary/overall impression:

This article uses regional climate simulations to test the sensitivity of land-atmosphere coupling in Europe to changes in atmospheric moisture and temperature profiles, by applying a well-known land-atmosphere coupling metric. I genuinely enjoyed reading this article and feel that the results have important implications for considering the influence of L-A coupling in a changing climate. The article is well-organized and uses novel methods that are based upon previous studies to address the authors’ hypotheses. I feel that this study will make a valuable contribution to the scientific literature. Moreover, the authors do an excellent job with the use of visuals to tell their story. As the authors’ results are so clearly valuable, I have a few suggestions that I hope will help ensure the authors’ main points are communicated clearly.

Specific comments:

L24-25: I may suggest rearranging the first two sentences, leading off with what L-A coupling is. i.e. “Land-atmosphere coupling describes the covariability between land and atmospheric states, and plays a key role for understanding....” An additional suggestion here may be to specify which states in the climate system.

L26: Schumacher et al. 2019 would be another relevant source for the influence of coupling on heat waves:

When you say, “The approach is based on the hypothesis...” it implies (to me) that this hypothesis was presented by numerous members of the broader scientific community and thus, supporting references should be provided. Though I am guessing you mean the hypothesis you are presenting specifically in this paper, in which case I might reword this to read “The approach is based on our hypothesis...”

It’s not entirely clear what you mean by “the differences in the mean and vertical distribution.” An extra sentence or two could improve clarity so that the reader knows what to expect in the rest of the analysis, especially if they are the type of reader that skips around sections and doesn’t necessarily read the methods in depth. Are you meaning to say that you are considering how the mean changes, or how moisture and temperature deviate from the mean? Additionally, it may be helpful to clarify whether the change in vertical distribution going to be considered separately from your analysis of changes in the mean state (or deviation from the mean state) here.

Section 2.1.1: This is well-organized, concise and easy to understand. Nice job.

Figure 1: I also really like how this figure is presented and summarizes your past work with respect to the metric you are using.

Figure 5: Once again... great use of visuals.

Forgive me if this is beyond the scope of the current study, or if I missed something here. I find it interesting that the hot and dry divergence factors increase CTP, but also increase the surface inversion. While we generally associate higher CTP with dry soil advantage, could a greater inversion strength over wet soils also lead to more moisture buildup in the PBL, and thus a lowering of the LCL to the PBLH, that may also trigger convection? In this case, would we expect the CTP-HI metric to be sufficient for diagnosing coupling potential? Papers by Ek et al. (1994 and 2004) may be relevant to a discussion of impact on surface inversion here. You may ignore this comment if I’m missing the point.


L355-357: Since you are discussing the influence of temperature in the “hot case,” it can be a bit confusing when you then say “fraction of nAC-days within the hotspot” as my mind first thought hotspot in a literal, temperature sense. I would suggest changing this to read: “within the L-A coupling hotspot.”

L360-362: Is this where CTP is increased, but so is the temperature inversion? So perhaps the likelihood of convective triggering over wet soils could be tied to the comments associated with L343-346 above?

Broader comment regarding discussion: How might overall warming of the climate impact the length of the season in which we consider L-A coupling to be most influential? Your results imply that warming enhances coupling strength, so would that also mean that L-A coupling might be an important driver of hydroclimatic variability over a longer warm season? For example, instead of JJA, perhaps the “coupling season” would now be MJJAS?

L466-468: I wholeheartedly agree that we need more vertical resolution, everywhere, however, I do believe you can argue that while Wakefield et al. (2021) shows that vertical resolution is a limiting factor, you can still get representative estimates of the L-A coupling pre-conditioning even when vertical resolution is unfortunately limited. Therefore, I think you can use this reference to argue both points… your limitation in vertical resolution introduces uncertainty, but that uncertainty is not so large that it substantially impacts the validity of your results.

L479: I’m not sure about the use of the word “reliable.” My mind immediately jumps to an operational use of the word and thinking about model reliability. I do like that you say the feedback class is insensitive to changes though. Maybe “wherefore a consistent regime can be expected,” if that’s the message you are trying to convey.

Technical:

L123: Change “but maybe limit investigations” to “may limit investigations.”

L142: Change “deep convection is inhibited by an inversion, only shallow clouds…” to “deep convection is inhibited by an inversion and only shallow clouds…” or separate into two sentences.

L382: Typo “Please not that…” should say “Please note that…”
L483-484: I might suggest rewording "cases reduced the coupling so that..." to "cases reduced the coupling sufficiently to cause disappearance of the strong positively. Coupled region over the Eastern European plain."